BETWEEN STATE AND TRANSNATIONAL COMMUNITY:
U.S.–JAPAN TECHNOSCIENTIFIC DIPLOMACY
IN EARTH OBSERVATION

A Dissertation

Presented to the Faculty of the Graduate School

of Cornell University

in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy

by

Daniel John Plafcan, Jr.

January 2007

© 2007 Daniel John Plafcan, Jr.

BETWEEN STATE AND TRANSNATIONAL COMMUNITY:

U.S.–JAPAN TECHNOSCIENTIFIC DIPLOMACY

IN EARTH OBSERVATION

Daniel John Plafcan, Jr., Ph.D.

Cornell University 2007

This dissertation examines intergovernmental collaboration in science and technology. In particular, it ventures into a new area of theoretical and empirical inquiry by investigating how scientists and engineers, working on behalf of different states, built an international remote-sensing system and created knowledge about the earth in the absence of the shared social, political, and cultural resources that would have been made available by a shared state or other shared authority, such as an international organization or treaty regime. In contrast to two prominent interpretations of scientific activity in the international arena, namely science as an idealist epistemic community and science as a Hobbesian imperial endeavor, the dissertation offers an explanatory interpretation that accounts for the creation of scientific knowledge and the development of international order by analyzing U.S.-Japan collaboration in remote sensing as a kind of international negotiation that I call "technoscientific diplomacy." Through this technoscientific diplomacy, U.S. and Japan scientists and engineers, working on behalf of their governments, accomplished something that had never been previously accomplished: they developed and operated in near-real time a space-based remote-sensing instrument, its ground data and information system, and an international political economy of scientific data, all under the management of two states.

BIOGRAPHICAL SKETCH

Dan Plafcan has had a long-standing interest in the international politics of science and technology, especially with respect to the politics of international security. As an undergraduate in the College of Engineering at Cornell University, many of his course electives were classes that brought together science, technology, and security issues. After graduating with a Bachelor of Science in Applied and Engineering Physics in 1994 and with a commission in the U.S. Navy Reserve, he briefly worked on nuclear proliferation and export control issues at the Department of Defense. That following fall, he returned to Cornell as a graduate student in Engineering Physics and conducted research on verification measures for the Comprehensive Test Ban Treaty. He also became more involved with Cornell's Peace Studies Program.

After receiving his Master of Engineering degree in 1995, Dan entered the doctoral program in the Department of Science and Technology Studies at Cornell. There, he extended his research on arms control by analyzing the politics of technical trust in the development of the verification system for the Comprehensive Test Ban Treaty. During his coursework, however, his attention turned to science and technology in the U.S.-Japan relationship. After completing a full-year of intensive Japanese-language training, Dan left Cornell for Japan and began in April 2000 what would turn out to be almost three years of language study, coursework, and field-work, all based out of the Research Center for Advanced Science and Technology (RCAST) at the University of Tokyo. While Dan was writing this dissertation, he returned to RCAST in February 2005 for a year as a fellow of the Japan Society for the Promotion of Science. In September 2006, he started as a post-doctoral fellow in science, technology, and public policy at the University of Michigan's Gerald R. Ford School of Public Policy.

For Pauline

ACKNOWLEDGEMENTS

Many people and institutions in the United States and Japan have supported the research, writing, and thinking that have produced this dissertation. I would like to acknowledge and thank them. In my second year as an undergraduate student in engineering, Judith V. Reppy introduced me to the social study of science and technology in what was then her seminar on the military and new technology. I have since benefited tremendously from her teaching, guidance, scholarship, and personal example. As the chair of my advising committee (i.e., my "special committee" in the parlance of Cornell), Judith has generously given me and my work more attention, energy, and patience than any graduate student could ever expect from an advisor. This dissertation—as well as the many drafts of chapters which came before it—is indebted in particular to Judith's close reading and countless, helpful suggestions, often heroically at the level of individual sentences. Judith has faithfully shepherded my work on this dissertation from the beginning until its submission, knowing when to let me roam and when to move me along.

The other members of my advising committee have also significantly shaped this dissertation, likely in ways in which they are unaware. Michael A. Dennis—with his breadth of knowledge, demanding standards, and empathetic and searching writing—has helped to teach me from the beginning of my time as a graduate student in the Department of Science and Technology Studies what it means to be a scholar as well as what it means to study science and technology as social, cultural, and political phenomena. In addition, Michael has done his best to ensure that the dissertation never lost sight of the forest for the trees. Sheila Jasanoff was a member of my advising committee through my candidacy exams, remaining on my committee for a time even after she had left Cornell for Harvard University. Sheila's expansive and holistic vision of the field of Science and Technology Studies and of what I could bring to it has provided unique encouragement. I was very fortunate that Peter J. Katzenstein returned to Cornell from sabbatical around the same time as Sheila's departure and was able to join my advising committee at a critical moment—while I was revising my thesis proposal before I left for Japan. This dissertation's ambitions, argumentation, and organization have benefited from Peter's penetrating questions, thoughtful insights, and astute advice. I also thank Peter for his unsurpassed professionalism as a scholar and a teacher which inspires those around him to perform at their best. Michael E. Lynch kindly provided valuable suggestions concerning a fellowship proposal during his first semester at Cornell, before he was even a member of my committee. I was delighted that he later accepted my invitation to join. Michael's careful and sensitive investigations of scientific and technical practice have served as a touchstone for my work, and his comments have helped me to recognize to a greater degree the dissertation's original contribution to the field of Science and Technology Studies. Finally, I thank Kathleen M. Vogel for serving as the field appointed reader for the defense of this dissertation (i.e., my "B-Exam"). Kathleen's suggestions were helpful for my revisions to the concluding chapter in particular and will be incorporated in future work.

Several groups of people outside of my formal advising committee have provided essential support. First, dozens of people involved with developing, operating, and managing the ASTER remote-sensing system have welcomed me to study their work, observe their meetings, and talk with them. Many, but not all of them, have been listed as interviewees in the references chapter. I thank them for their good will and openness. I would especially like to thank Anne Kahle, Tsu Hiroji, Michael Abrams, Kudoh Masahiko, and Watanabe Hiroshi. Yamaguchi Yasushi provided crucial assistance in locating documents. He was extraordinarily generous with his time. Were it not for Yamaguchi-san's openness at a key moment, the field work for this dissertation would have been much more difficult. I also would like to thank individuals at the U.S. National Aeronautics and Space Administration's Goddard Space Flight Center and at the Earth Remote Sensing Data Analysis Center under Japan's Ministry of Economy, Trade, and Industry who helped me obtain unclassified, non-proprietary "gray literature" through the filing of a Freedom of Information Act request. They know who they are, and I thank them for helping me with the bureaucracy. Russell Castonguay at the Jet Propulsion Laboratory Archives and Record Center assisted me with appealing to the Jet Propulsion Laboratory for the public release of two documents. After over three years of annual requests, no response of any kind has been received as far as I am aware.

Hashimoto Takehiko of the Department of History and Philosophy of Science at the University of Tokyo hosted me at the university's Research Center for Advanced Science and Technology for what amounted to, in total, over three years of field work and writing. Hashimoto-sensei's research group provided friendship as well as stimulating intellectual community. In many ways, Hashimoto-sensei has been an informal member of my advising committee. I would also like to thank my colleague and friend Fujita Yasumoto, who initially introduced me to the Hashimoto group and who helped me immensely in my first year, as well as other colleagues and friends who have been affiliated with the Hashimoto group, in particular, Itō Kenji, Kim Boumsoung, and Satō Yasushi. My work has benefited from frequent, informal discussions with these former members of the Hashimoto group. Choi Hyungsub, Kuo Wen-Hua, and Nishiyama Takashi also have provided comments and suggestions on my papers and presentations. Okamoto Takuji and Matsumoto Miwao, both of the University of Tokyo, welcomed me and provided assistance in a variety of ways. Finally, among my fellow foreign students and the "expatriate" community in Tokyo, I would like to thank in particular Per Wiggen for his encouragement and guidance as well as friendship.

A number of organizations and institutions have funded my conference travel and research. I thank them for their crucial support, and for, in some cases, the comments that I have received from their reviews of my work. These organizations, institutions, and fellowships are: Cornell University's Department of Science and Technology Studies; Cornell University's Graduate School; Cornell University's Peace Studies Program and its graduate student fellowships; Cornell University's East Asia Program and its Robert J. Smith Fellowship in Japanese Studies; the Government of Japan's Ministry of Education Scholarship; the Japan Society for the Promotion of Science's Short-Term Postdoctoral Fellowship; the Society for Social Studies of Science; the U.S. Social Science Research Council's Japan Dissertation Workshop; and the U.S. National Science Foundation's Dissertation Improvement Award (No. SES-0115829). Deborah Van Galder, Lillian Isacks, Stacey Sullivan, and Judy Yonkin—all from the Department of Science and Technology Studies—provided important administrative assistance concerning applying for and managing these funds as well as the processing of other essential paperwork related to the progress of this dissertation.

Colleagues and friends in the Department of Science and Technology Studies at Cornell University who have heard talks, read papers, and provided helpful suggestions or important encouragement are many, and they include: Javier Lezaun, Cyrus Mody, Shobita Parthasarathy, Miranda Paton, Sonja Schmid, and Jamey Wetmore. David Caruso and Carin Berkowitz have gone above and beyond many times, and I especially would like to thank them for their caring phone calls to Japan which helped to sustain me during my year of writing in Japan. Friends outside the department who have provided guidance that contributed to my academic work and this dissertation are also many, but I would like to thank especially Ed Chan, Taryn Mattice, Robb Newman, and Gil Toombes. My parents and family have been a constant source of love and support, even when they might have struggled to make sense of academia and my journey in it. Pauline Kusiak has been a true partner in the fullest sense of the word throughout my field work and the writing of this dissertation. I have learned from her doctoral work and from her gentle critiques of mine, always knowing that she was by—and on—my side, even when we were thousands of miles apart for many months at a time.

Despite all of this generous assistance, I have researched and written this dissertation. Its flaws are my responsibility alone.

TABLE OF CONTENTS

BIOGRAPHICAL SKETCH iii

DEDICATION iv

ACKNOWLEDGEMENTS v

TABLE OF CONTENTS x

LIST OF FIGURES xiv

LIST OF TABLES xv

LIST OF ABBREVIATIONS xvi

PREFACE xviii

CHAPTER ONE:

TECHNOSCIENTIFIC DIPLOMACY 1

Intergovernmental Collaboration in Science and Technology 5

Knowledge-Making and Technology-Building in International Affairs 10

State Goals 11

Explanatory Sketches of International Political Order 16

The Practice of Technoscientific Diplomacy 36

<u>Liminal State Actors</u> 38

<u>Enacting Technoscientific Diplomacy</u> 40

<u>Synthesizing Power and Knowledge</u> 41

Outline of the Dissertation 45

CHAPTER TWO:

SCIENCE AND TECHNOLOGY IN U.S.-JAPAN RELATIONS 48

The Centrality of Technoscience in U.S.-Japan Relations 49

The Building of Two U.S.-Japan Relationships 57

U.S.-Japan Collaboration in Defense 59

U.S.-Japan Collaboration in Space 70

Re-Thinking U.S.-Japan Technoscientific Relations 86

CHAPTER THREE:

TWO COMMUNITIES OF REMOTE-SENSING PRACTICE

AND STATES' GOALS 89

Two Geologic Remote-Sensing Communities 89

The Origins of States' Goals 111

EOS and the Origins of U.S. State Goals 112

Japan's State Goals and ITIR 133

Conclusion 146

CHAPTER FOUR:

TRYING TO SHARE SEPARATELY 148

The Politics of Boundary Objects 151

The TIGER Team 154

MITI's ITIR and Kahle's Two Proposals 154

Advancing Bilateralism as Liminal State Actors 165

The Practice of Bilateral Technoscientific Diplomacy:

The Thermal Infrared Radiometer, 1989 176

Technoscientific Diplomacy as an Explanation 200

CHAPTER FIVE:

PERSISTENCE AND TRANSCENDENCE 203

The Formation and Construction of the ASTER Team 207

The TIGER Team becomes the U.S. Team 208

The Elevation of Japanese Users, Researchers, and Scientists 215

The Japan ITIR Team 218

The Japan "Science Team" in the Japan Team 225

The Implications of Social Makeup for Negotiations 229

Persistence of Bilateralism:

The Shortwave Infrared Radiometer, 1990-1992 231

Japan Makes a Unilateral Selection 238

Negotiations in the Geology Working Group 243

State Power and Scientific Acquiescence 247

Transcendence of Bilateralism:

The ASTER Instrument's Operational Capabilities, 1991-1993 261

Operational Modes 263

Negotiating Operational Scenarios and Instrument Capabilities 266

Analytical Mixing and Nucleation Sites 276

Intersubjectivity 281

Breaking Out of Bilateral Diplomacy 289

CHAPTER SIX:

THE INTERNATIONAL POLITICAL ECONOMY

OF THE ASTER DATA AND INFORMATION SYSTEM 291

A Data and Information System as a Solution to

Problems of International Order 293

Designing the ASTER Data and Information System 302

The "ASTER Centric" End-to-End Data System Concept 302

Symmetry and Interoperability 309

The U.S.-Japan MoU and Clean Interfaces 317

Configuring an International Political Economy 322

A Partnership in Production 326

<u>The Level 1 Compromise: Product and Process</u> 327

<u>Creating a U.S.-Japan Hybrid: The TES Algorithm</u> 345

U.S.-Japan Equality in the Governance of Consumption 372

The Utility of a Normative Notion of a U.S.-Japan Partnership 377

Transnational Authority and Socio-Political Interdependency 383

CHAPTER SEVEN:

BETWEEN STATE AND TRANSNATIONAL COMMUNITY 390

APPENDIX:

NASA'S LETTER OF INTRODUCTION FOR KAHLE TO JAROS 403

REFERENCES 407

Note on Sources 407

Meetings Observed 408

Interviews and Personal Correspondence 409

Bibliography 411

LIST OF FIGURES

Figure 4.1: A Standard Depiction of the ASTER Instrument 150

Figure 5.1: The TIGER Team's Proposal 210

Figure 5.2: Proposed U.S. ITIR Team Tasks 211

Figure 5.3: Kahle's Copy of ITIR Management Structure 220

Figure 5.4: SWIR Sensor Band Configurations 233

Figure 5.5: ASTER Operational Sequence 269

Figure 5.6: Trade-Off between Number of Operations and Time 274

Figure 5.7: Miyazaki and the Tensions in Mission Operations 285

Figure 6.1: The EOSDIS Concept 296

Figure 6.2: The EOSDIS Architecture 298

Figure 6.3: The ASTER Data and Information System 307

LIST OF TABLES

Table 2.1: Dimensions for Comparing Collaborative Practices 59

Table 2.2: Comparing U.S.-Japan Technoscientific Relations 86

Table 7.1: The Interpretative Approach of Technoscientific Diplomacy 391

Table 7.2: Episodes of Technoscientific Diplomacy 393

Table 7.3: Outcomes Suggested by Alternative Interpretative Approaches 394

LIST OF ABBREVIATIONS

ADE	Alpha-Derived Emissivity
ASTER	Advanced Spaceborne Thermal Emission and Reflection radiometer
ATBD	Algorithm Theoretical Basis Document
DAAC	Distributed Active Archive Center
DAR	Data Acquisition Request (this is a request to acquire new data)
DDL	Direct Downlink
DPR	Data Product Request (this is a request to process existing data)
DRS	Data Receiving Station
EOS	Earth Observing System
EOSDIS	Earth Observing System Data and Information System
ERSDAC	Earth Resources Satellite Data Analysis Center (pre-1993)
	Earth Remote Sensing Data Analysis Center (post-1993)
GSFC	Goddard Space Flight Center
ITIR	Intermediate Thermal Infrared Radiometer
JAROS	Japan Resources Observation System organization
JAXA	Japan Aerospace Exploration Agency (post-2003)
JERS-1	Japan Earth Resources Satellite No. 1
JPL	Jet Propulsion Laboratory
MITI	Ministry of International Trade and Industry (pre-2001)
METI	Ministry of Economy, Trade, and Industry (post-2001)
MMD	Mean-Maximum Difference (for Dr. Matsunaga's method)
	Maximum-Minimum Difference (for revision of Matsunaga's method)
MoU	Memorandum of Understanding
NASA	National Aeronautics and Space Administration

NASDA	Japan's National Space Development Agency (pre-2003)
NEM	Normalized Emissivity Method
ODS	One Day Schedule
OMPWG	Operations and Mission Planning Working Group
PIP	Project Implementation Plan
SWIR	Shortwave Infrared Radiometer
TES	Temperature and Emissivity Separation (an algorithm)
TIGER	Thermal Infrared Ground Emission Radiometer
TIMS	Thermal Imaging Multispectral Scanner (the airborne instrument)
TIMS	Thermal Infrared Mapping Spectrometer (proposed with TIGER)
TIR	Thermal Infrared Radiometer
VNIR	Visible Near-Infrared Radiometer

PREFACE

This dissertation required me to learn in detail about the political and technical work of scientific and engineering communities in Japan and the United States. That involved drawing extensively upon my engineering background, upon critical-thinking skills and an interpretative sensibility that were developed through graduate-level coursework in the humanities and social sciences, and upon whatever skills I had in establishing rapport with individuals who were not accustomed to being studied. It also demanded becoming proficient in the Japanese language. If I was going to learn about these scientific and engineering communities—and especially if I was going to learn about crucial matters that were not published in any journal article or public technical report—I needed to be able to move around competently in their worlds and prove myself as someone who could comment intelligently and responsibly upon their work. To my surprise, however, that turned out to be the easy part.

What was difficult was learning about the world from which I came—the academy—so that eventually I could effectively convey in writing what I had thought I had learned about these scientific and engineering communities. Inspired by the cross-disciplinary discussion that took place in the dinner seminars of the Peace Studies Program at Cornell, I saw a common set of ideas being tossed around in the fields of Government, Anthropology, and Science and Technology Studies. All of these ideas went under the broad banner of "constructivism." In the naivety of a young graduate student, I at the time could not think of a compelling reason to get behind any particular corner of literature and join a particular camp when, it seemed to me, debating the differences between the camps—especially in the abstract, outside of the context of an empirical investigation—was like debating the proverbial "how many angels could dance on the point of a pin." Apparently, I had not yet taken to heart enough of the lessons from my own field about the politics of knowledge when that knowledge is expressed in particular social circumstances. Consequently, much of the time, work, and energy that went into this dissertation were spent not on learning about the work of geologic remote-sensing communities in the United States and Japan or about Japan and the Japanese language, but on learning how intellectual ideas that are very similar in the abstract could come to life very differently in the writing of different corners of the academy. However this dissertation comes to life in any discussion (if it does at all), I hope it is less in the spirit of specialization and alienation and more in the spirit of synthesis and engagement.

A note about the use and transliteration of Japanese in this dissertation: I have generally followed the style and guidelines recommended by the Society of Writers, Editors, and Translators (based out of Tokyo, Japan). Japanese names are written in their native order—family-name first and given-name second—except when citing a work in which the order has been reversed. Thus, consistency is sacrificed when it risks confusion. When the Japanese is written, it is transliterated into Roman letters using the Hepburn system. A note on sources precedes the bibliography in the references section.

CHAPTER ONE

TECHNOSCIENTIFIC DIPLOMACY

This dissertation investigates the politics of an international collaboration in science and technology. It explains in detail how scientists and engineers, who were working on behalf of the governments of the United States and Japan, developed an international remote-sensing system that included a space-based instrument, an internationally-integrated ground data and information system, and an international political economy of scientific data. These scientists and engineers developed this remote-sensing system, called the ASTER remote-sensing system, so that they and other users could acquire certain imagery data of the earth's surface and produce scientific knowledge about the earth. The ASTER remote-sensing system was the first land remote-sensing system in which two states shared responsibility for the system's design, development, data acquisition operations, and data processing.[1]

[1] Some readers, especially those with knowledge of the history of earth observing satellites, might want my claim of ASTER's exceptionality to be unpacked and clarified. Previous to ASTER, there had been intergovernmental collaborations on atmosphere-observing and ocean-observing satellites in which states contributed their own instruments to a common satellite platform (e.g., the UARS satellite that was launched in 1992 with seven instruments from the United States, one from the United Kingdom, and one from a Canadian-French consortium; the TOPEX-POSEIDON satellite that was launched in 1992 with instruments from the United States and France; and the TRMM satellite that was launched in 1997 with instruments from the United States and one from Japan). The focus of these intergovernmental collaborations was much more on balancing the satellite platform's complement of instruments and less on any single instrument. These collaborations did require coordination in instrument development, operations, and data processing, but this coordination was qualitatively different than the collaboration that came to be required for the ASTER instrument. Since these instruments observed the earth's atmosphere and ocean on set, periodic schedules that were established before launch, the complexity of their post-launch operations and the extent of collaboration required between each state's team(s) were substantially less than that of the ASTER collaboration. The ASTER instrument was tasked daily with different operations in order to meet the changing needs of different users. Furthermore, unlike for the ASTER collaboration, for the other collaborations mentioned above, two states did not share responsibility for a single instrument's data processing, in the sense that the processing was distributed and harmonized between two states. Some data processing for the TRMM satellite did require data to be collected and integrated from more than one of its instruments, but that data

This international achievement poses a question for scholars of the politics of science and technology who write within the field of Science and Technology Studies. The field of Science and Technology Studies generally holds as a central tenet that the development of science and technology draws upon, interacts with, and changes socio-political order.[2] Yet, the field to date has largely investigated the workings of science and technology within the context of established socio-political orders, such as personal relationships, a coherent community, an institution, a culture, or relations within a single state, especially those of liberal-democratic states.[3] The literature on the politics of science and technology in particular has emphasized, for instance, the importance of states' "political cultures" or national "technopolitical regimes" for the buttressing of scientific knowledge-making and technology-building.[4] The relatively

---

processing was centralized in one state (in the United States). Finally, because the ASTER instrument observed the earth's land at high-resolution, rather than the atmosphere or ocean at low-resolution, its data potentially had much more commercial value. The political significance of these differences for intergovernmental collaboration becomes clearer in the chapters ahead.

[2] Central texts of the field of Science and Technology Studies which make this point include Bloor (1991 [1976]), Collins (1992 [1985]), Shapin and Schaffer (1985), and Latour (1987). More recently, the point is emphasized by Jasanoff (2004) and the chapters therein. Surveys of, and introductions to, the field of Science and Technology Studies which approach the field from a variety of antecedent disciplinary concerns, such as Ethnomethodology, Sociology of Science, History of Science, and post-positivist Philosophy of Science, include, respectively, Lynch (1993), Shapin (1995), Golinski (1998), Biagoli (1999), and Zammito (2004). Jasanoff et al. (1995) and Sismondo (2004) perhaps do the most to frame the field of Science and Technology Studies as an emerging intellectual discipline.

[3] Literature in the History of Science does offer some illuminating exceptions to this generalization, such as Schaffer (1988). Yet, even in that study, the emphasis is on cross-national (or cross-institutional) diversity, rather than on the settlement of scientific and political difference across borders. All differences of course do not need to be settled. Whether or not (provisional) settlements of difference are demanded, and what form those settlements take if they have occurred, are empirical questions. Clearer exceptions to my generalization are a few of the chapters in Crawford et al. (1993), such as Abir-am (1993) and Elzinga (1993). The latest volume in the History of Science Society's *Osiris* annual series, which appeared too late for this dissertation to take into full consideration, takes as its theme science and technology in international affairs. See Krige and Barth (2006). Studies within the History of Science that have investigated the rhetoric, ideologies, and use of scientific internationalism in national activities include Forman (1973) and Hamblin (2002).

[4] See, respectively, Jasanoff (2005) and Hecht (1998). Other accounts of the politics of science and technology within the field of Science and Technology Studies, broadly defined, which focus on the activities of state-centered bureaucracies and institutions include Shapin and Schaffer (1985), Smith (1989), Ezrahi (1990), Jasanoff (1990), Mack (1990), MacKenzie (1990), Smith (1990),

small collection of recent studies that have investigated intergovernmental collaboration have turned to the context and resources offered by established intergovernmental organizations or treaty regimes to account for knowledge-making and technology-building.[5] What is the socio-political context or what are the socio-political resources for scientific and technological development that is undertaken without the benefit of a shared institution, a shared culture, a shared native language, or a shared state?[6] How did a U.S. team and a Japan team of scientists and

---

Leslie (1993), Dennis (1994), Samuels (1994), Bimber and Guston (1995), Gusterson (1996), Vaughan (1996), Alder (1997), Scott (1998), Guston (2000), Hilgartner (2000), Eden (2004), and Parthasarathy (2005). Not all of these studies completely lean on the workings of bureaucracies and the stability of state institutions in their accounts. For example, MacKenzie's historical sociology of the development of nuclear missile guidance attends to C.S. Draper as an entrepreneurial, "heterogeneous engineer" and to the members of the "guidance mafia" that passed through Draper's MIT Instrumentation Laboratory, in contrast to earlier accounts of the development of weapon systems which drew heavily upon logics of bureaucratic politics (1990: 7-12, 85-92, 118-119). Dennis (1994) describes in more detail Draper's pedagogy and argues that Draper's pedagogy and MIT's Instrumentation Laboratory both relied upon and helped to reconstitute the United States' national security establishment after World War II, especially the boundaries between the civil and the military.

[5] For instance, the institutional procedures of the Intergovernmental Panel on Climate Change and of United Nations-affiliated multilateral organizations play a crucial role in Miller's analysis of knowledge-making concerning climate change (2004). The new regulatory regime for genetically modified organisms closely analyzed by Lezaun (2006) was conditioned upon the shared bureaucratic practices of the European Union. Other examples include Elzinga (1993) on the Antarctic Treaty System; Bonnet and Manno (1994), Zabusky (1995), and Krige (1997) on the European Space Agency; Miller and Edwards (2001) on international environmental regimes; Thompson (2004) on the Convention of International Trade in Endangered Species of Fauna and Flora; and Waterton and Wynne (2004) on the European Environment Agency.

[6] This question is analogous to the "anarchy problematique" in the field of International Relations—that is, the political dilemmas posed by the absence of a central governing authority for states. This problematique undergirded many explanatory projects in the field of International Relations (e.g., Oye 1986), especially explanations of resolutions to problems of cooperation among states as those puzzles were posed most trenchantly within the field of International Relations in the 1980s and early 1990s by the schools of "neorealism" and "structural realism." For works that exemplify neorealism and structural realism, see Waltz (1979) and Buzan et al. (1993), respectively. For a canonical debate over cooperation under anarchy between the two "rationalist" schools of thought within the field of International Relations, called "(neo) realism" and "(neo) liberalism," see Grieco (1988, 1990). Ashley (1988) offers a critical theory commentary on the "anarchy problematique" as it has been articulated in the field of International Relations. In an early article in what was then International Relations' newly emerging "social constructivist" school of thought (a school of thought which is now established), Wendt (1992) questioned whether or not anarchy had any natural or inherent logic (i.e., an asocial logic). The article made clear that it was not questioning the empirical state of anarchy in contemporary world politics, just the use of the idea of anarchy in international relations theory. See also chapter six in Wendt (1999).

engineers—whose respective members had never previously met—develop an intricate international remote-sensing system? What was the politics of their collaborative work? How did they manage to arrive at their decisions? How did the two teams advance technical claims and together make scientific judgments, and how in that knowledge-making and technology-building did they assert, ascribe, and exercise power? Those are the questions concerning the politics of intergovernmental collaboration in science and technology which this dissertation seeks to answer.

I want to make clear that by saying that this dissertation investigates the "politics" of a U.S.-Japan collaboration in earth observation, I do not mean the "petty politics" of who personally got along with whom or of who said what about whom (although when carefully considered, that information can offer insights). Nor do I really mean just the "who got what" issues that typically came to mind as "politics" for many of the scientists and engineers who were involved in the ASTER remote-sensing system, when I had explained to them that I was interested in the history and politics of their international collaboration. Questions that concern gains and losses—for example, what government paid for what, whose requirements were designed into the system, who controlled the instrument's operations, and what data were produced for whom and at what cost—are indeed significant political questions. Explaining "why" gains and losses were distributed among parties in the way that they were is an important analytical task that is regularly taken up by studies of politics and

---

For fields of intellectual inquiry that are informed by "post-structuralist" or "constructivist" thought that interrogates or rejects strong structural assumptions, as does the vast bulk of the literature in Science and Technology Studies, the static problematique of anarchy can be explicated as a dynamic problem: the problem of creating socio-political order (on the rejection of strong structural assumptions in Science and Technology Studies, see in particular Latour (1987), Gieryn (1992b), and Lynch (1993)). Recent constructivist literature in International Relations has also studied the creation of socio-political order. See, for example, the account of Price (1997) which examined the emergence of a chemical weapons "taboo" in war among states, the explanation offered by Crawford (2002) for decolonization, and the argument of Katzenstein (2005) that "the American imperium"—as both an actor and a system—has shaped a world of regional orders and identities.

policy (such as studies of international bargaining or interest-group politics).[7] Yet, this dissertation suggests that by focusing on the "how" questions—by describing and explaining especially how particular decisions were made—it can better explain the often-asked "why" questions, as well as better account for the political and technical development of the ASTER remote-sensing system as a whole. Taking on directly the "why" questions can too easily have the ironic effect of diverting attention to the social and political conditions outside of the activity of the ASTER collaboration and thus neglect precisely what it is that this dissertation seeks to understand and explain: the politics of technical decision-making and scientific judgment in the development of a U.S.-Japan remote-sensing system and, more broadly, the politics of intergovernmental collaboration in science and technology.

Intergovernmental Collaboration in Science and Technology

In the past few decades, states around the world have pursued a myriad of diverse intergovernmental collaborations in science and technology to achieve ends that are scientific, technical, political, and economic in nature.[8] This point is best illustrated with several examples. Let us start in the Mediterranean and generally work our way east to the United States. The Abdus Salam International Centre for Theoretical Physics in Trieste, Italy was established in 1964 under the auspices of the United Nations Education, Scientific and Cultural Organization, the International

---

[7] See, for example, Moravcsik (1993, 1997), Schoppa (1997), and Fearon (1998).

[8] I take "intergovernmental collaboration" to mean collaboration between governments which is conducted by individuals who either work for governments as civil servants or who are under direct contract with governments. A given intergovernmental collaboration need not necessarily involve the participation of governments exclusively. Only some of the collaborating parties might fall under the category of "state actors," to use the vocabulary of international relations theory. That is, international organizations and non-state actors as well as state actors can be involved. I problematize below the membership categories of "state actor" and "non-state actor."

Atomic Energy Agency, and the Government of Italy. The center's primary mission has been to provide training and research facilities for scientists from developing countries, especially from Africa, the Middle-East, and South Asia, hosting over a hundred scientists each year for several months or longer for collaboration in research and training, and thousands of scientists each year for conferences and short-courses. Because "Italy views the center as an important foreign policy tool," Italy supports about 85% of the center's \$23 million annual budget.[9] In 1983, Italy added the secretariat of the Third World Academy of Sciences and the UN-affiliated International Centre for Genetic Engineering and Biotechnology to the Trieste research and development complex.[10] The biotechnology center has over 55 member states, which are, again, largely from developing countries. This biotechnology center has another facility hosted in New Delhi, India.

Further to the east, Japan's Ministry of International and Trade and Industry launched in 1989 what was hoped to be a 10-year, \$1 billion international research program on "Intelligent Manufacturing Systems" and invited U.S. and European firms to participate. According to the Government of Japan, one goal of the program was to develop global standards for factory automation that would encourage firms in Japan to build factories overseas, which would, in turn, reduce the trade imbalances that Japan was charged with having vis-à-vis the United States and European countries. The United States and the European Community reportedly feared, however, that the program was a Trojan horse designed for Japan to acquire expertise in U.S. software and European precision equipment. Eventually, after extensive consultations with the U.S. Department of Commerce, the European Commission, Australia, and Canada, the

9 Clery (2003). More detailed information about the International Centre for Theoretical Physics can be found at the center's website at http://www.ictp.it (last accessed July 20, 2006).

10 See the website of the International Centre for Genetic Engineering and Biotechnology at http://www.icgeb.org (last accessed July 20, 2006).

governments restructured the program into a "bottom-up," "industry-led," and "market-driven" program that was funded in part by the program's member governments to promote the "industrial competitiveness" of their own countries.[11]

Cooperation in science and technology in East Asia is not only about industrial competitiveness and trade. In April 2006, 200 researchers from South and North Korea met in secret in Pyongyang to discuss possibilities for international collaboration, perhaps leading to the establishment of an "Inter-Korean Science Center." The conference came at a time when the "six-party talks" over North Korea's nuclear program were stalled.[12] According to the conference organizer, who was a university president in South Korea, the long-term objective of the collaboration was to ameliorate disparities in science and technology between the North and the South and to facilitate Korea's reunification.[13]

Continuing east and across the Pacific, Japan and the United States have had extensive bilateral collaborations in science and technology, particularly in defense and space. For instance, the two states have co-developed fighter aircraft and are involved in the construction of the International Space Station. Canada, the European Space Agency, and especially Russia also contribute to the International Space Station. In addition to the ASTER remote-sensing system, U.S. and Japan space agencies have collaborated on several other earth observing systems, beginning with Japan receiving imagery data from the U.S. Landsat satellite in the late 1970s. The two states participate with China, the European Union, Russia, and South Korea in the $5 billion International Thermonuclear Experimental Reactor project. The reactor will be

---

[11] Garnett (1990), Wagstyl (1990), and Heaton (1991). The quotes are from the international website for the Intelligent Manufacturing System program, which can be found at: http://www.ims.org/index_introduction.html (last accessed July 20, 2006).

[12] The six parties were China, the Democratic Peoples Republic of Korea (i.e., North Korea), Japan, the Republic of Korea (i.e., South Korea), Russia, and the United States.

[13] Stone (2006).

constructed in France, and the project will be directed by a diplomat from the Government of Japan.[14] Since 1991, Canada, China, Korea, Japan, Russia, and the United States have promoted and coordinated marine research in the northern Pacific through an intergovernmental organization.[15] Since 1992, the United States has collaborated with Russia and other states of the former Soviet Union on protecting and controlling nuclear, chemical, and biological materials.[16] Although the United States' use of intergovernmental collaboration in science and technology as an instrument of foreign policy as well as a spur to the promotion of scientific and technological development is by no means unique, the United States is likely the largest supporter of intergovernmental scientific and technological collaboration in the world.

In one of the last accountings of its kind, the U.S. Department of State reported in 1995 that the United States had in force 26 general science and technology agreements (known as "umbrella" agreements, most of which were bilateral).[17] Over 850 agency-to-agency international agreements were also in effect under the Department of State's imprimatur.[18] Many other agreements are not included in that count because they do not have or do not need the Department of State's imprimatur. In fiscal year 1997, the U.S. Government spent approximately $4.4 billion on thousands of international collaborations that involved over 110 countries, according to a study by RAND.[19] U.S. Government agencies in that same year were engaged in over 490 international agreements that related to environmental remote sensing alone. Only about 10% of these 490 agreements had been approved through the formal

---

[14] Clery and Normile (2005) and Clery (2005).
[15] Perry et al. (2002).
[16] Stern (1996).
[17] U.S. Department of State (1996).
[18] U.S. Department of State (1996). See also U.S. General Accounting Office (1999).
[19] Wagner et al. (2001). $4.4 billion is about 6 percent of fiscal year 1997's total federal R&D spending (including defense R&D, which is a little more than half). Not all of these international collaborations are intergovernmental.

interagency process that would have brought them to the attention of the Department of State.[20] Even if this percentage was unusually low, it is reasonable to estimate that U.S. government agencies were—and likely still are—committed to thousands of agreements for international collaboration in science and technology, and most of these agreements are likely for intergovernmental collaborations.

The United States' purported goals for its collaborations in science and technology have been tailored to its foreign policy. The U.S. Department of State's last report on "Science, Technology, and American Diplomacy," covering fiscal year 1995, informs us that science and technology were "central to the goals of economic security, military strength, and diplomatic engagement—the vital elements of national security."[21] The United States undertook international collaborations in order to "advance U.S. R&D objectives, minimize duplicative research efforts, gain access to R&D programs worldwide, jointly address global problems, and support U.S. foreign policy."[22] The document described U.S. foreign policy objectives for that year as "building democracy," "promoting and maintaining peace," "promoting economic growth and sustainable development," "addressing global problems," and "providing humanitarian assistance."[23] Other states—such as those mentioned in the collaborations described above—have similarly tried to use collaborations in science and technology as occasions to support their foreign policies and advance state goals. States that, like the United States, have played or are now playing leading, dominant, or imperial roles in world affairs are the most conspicuous examples, such as China, France, Japan, the Netherlands, the Soviet Union, Spain, and the United Kingdom.[24]

---

[20] Wagner (1998). Thus, remote-sensing agreements do not represent half of the 850 agency-to-agency international agreements previously mentioned.
[21] U.S. Department of State (1996: iv).
[22] Ibid., p. 9.
[23] Ibid., p. 9-11.
[24] Articles in MacLeod (2000) describe the use of science and technology in colonial projects of the nineteenth and early twentieth century, and articles in Krige and Barth (2006) discuss the use of

Knowledge-Making and Technology-Building in International Affairs

My dissertation's account of a U.S.-Japan remote-sensing collaboration contributes to understanding scientific knowledge-making and technology-building in international affairs in three ways. First, it contributes to a general understanding of intergovernmental collaboration in science and technology as a tool of foreign policy by showing that state goals, while handed down from "higher" state authorities, were nevertheless (re)articulated in different, politically useful ways in the process of scientific and technical collaboration.

Second, the dissertation's methodology is itself a contribution to the fields of Science and Technology Studies and International Relations. The dissertation pushes common interpretative methods in Science and Technology Studies in two intertwined ways. First, while some of the literature in Science and Technology Studies has altogether avoided "why" questions, this dissertation proposes to address those questions in a way that is still in accordance with the interpretative sensibility of much of the field. Second, the dissertation incorporates alternative explanations (or alternative accounts, if one prefers) in analyses along with the author's preferred explanation. In addition, the dissertation demonstrates to the field of International Relations the value of describing in detail the mundane work of international relations and of taking into serious consideration the various political and technical interpretations that actors might have offered of their own quandaries, essentially acting as analysts themselves before other analysts (such as this author) have come along to examine those quandaries. Using the methodology that this dissertation

---

science and technology to promote foreign policy in the mid-twentieth century. Skolnikoff (1993) offers a broad discussion of the role science and technology in international affairs. National Research Council (1997) explores how the United States might "maximize" its interests in its science and technology relations with Japan.

advocates does not necessarily make an account more accurate, but it can help clarify what that account does and does not illuminate.

Third, the dissertation contributes to literature on the politics of science and technology in the field of Science and Technology Studies and to literature in the field of International Relations, particularly the constructivist literature, by explaining how knowledge and power came together in the two teams' technical decision-making and scientific judgment, especially through the two teams' enactment of their respective states and U.S.-Japan relations and through the teams' assertions and ascriptions of knowledge and power, particularly state power. The dissertation characterizes this technical decision-making, collective scientific judgment, and international relations as "technoscientific diplomacy." This account of the "how" explains the "why(s)" of the "who got what" issues that were noted above. These three contributions are elucidated below.

State Goals

Although intergovernmental collaborations in science and technology occupy a central role in many states' foreign policies, most discussions of the topic neglect the question of how the complexity of scientific and technical practice aligns itself with the advancement of states' foreign policy goals, if it does at all. A typical narrative of the founding of the U.S. National Science Foundation's overseas office in Tokyo exemplifies how commentators, especially those in policymaking circles, have long thought about scientific and technological collaboration in world affairs:

> In 1960 in the journal *Foreign Affairs*, Edwin Reischauer wrote about the "The Broken Dialogue" in U.S.–Japan relations. Reischauer was at the time a professor at Harvard University and a renowned scholar of Japan,

> and he would become the United States' most famous Ambassador to Japan.[25] Amid the massive protests in Japan over the signing of the Mutual Security and Cooperation Treaty of 1960, Reischauer wrote of a gap in understanding between the two nations' general publics and between each nation's elite intellectuals in particular. Friendship and mutual understanding between the United States and Japan could only be born from relationships of equality, he argued. Reischauer considered collaborations among intellectuals to be promising enterprises with which to lead the way in mending the U.S.–Japan alliance. Newly-elected President Kennedy took note of Reischauer's article and subsequently made him ambassador, replacing Ambassador Douglas MacArthur, who was the nephew of the former Supreme Commander of the Allied Powers in Japan during the postwar occupation, General MacArthur.
>
> One of Reischauer's and Kennedy's first foreign policy initiatives was to suggest a U.S.–Japan committee of scientific cooperation. After the idea was affirmed by Prime Minister Ikeda Hayato in a summit meeting with Kennedy in 1961, the U.S. Government signed its first formal agreement for cooperation in basic science, its first not only with Japan, but with any state. Its implementation as the "U.S.–Japan Cooperative Science Program" quickly led to the establishment in Tokyo of what is now the U.S. National Science Foundation's most senior overseas office. The National Science Foundation's second permanent overseas office, which was in Paris, wasn't opened until over twenty years later, in 1983.[26]

In this state-centric narrative, scientific and technological collaboration follows broad foreign policy goals—principally that of building alliances against communism. When the U.S.–Japan relationship was seen to be deeply troubled in 1960, intellectual collaboration was a safe way to foster good will, a good will that could supposedly be leveraged to pursue other state goals. According to this state-centric reasoning, a kind of reasoning assumed not only by the narrative but also likely by the principal actors in it, collaboration is typically a means to a foreign policy end, an end that is determined in part by stable national interests, whether those interests are scientific, economic, or military in nature. Those interests are presumably expressed in goals that

---

[25] Reischauer (1960).

[26] This standard narrative is a paraphrasing and blending of accounts in Bloom (1984: 91-92), Neureiter (1998: 30-32), Uyehara (2000: 25-26), and Blanpied (2002: 1-2). Although the United States' first formal bilateral agreement for cooperation in basic science was with Japan, the U.S. Government had previously cooperated with other governments in basic science, most famously with the United Kingdom during World War II.

guide collaboration in practice. This fledgling U.S.–Japan collaboration in science and technology could well be said then to have been an epiphenomenon of power politics, at least up to a certain point in its history.

State goals, however, do not float freely. How do we know whether or not state goals, even those publicly expressed, make their way from the supposed "summit" of the state to the collaborative practices of scientists and engineers? Even if these goals are passed on clearly and somehow make the trip through bureaucracy, are they in a form that is meaningful and useful? Wouldn't they need to be further specified and refined? And when during the course of collaboration are they actually referred to and used? To answer these questions, we need to follow state goals as they are deployed in practice. Then, we can capture and assess, for example, how the state is and is not brought to bear in intergovernmental collaborations in science and technology and how scientists and engineers may or may not re-present and realize state goals.[27] If there is something to these questions, the above story about the founding of the U.S. National Science Foundation's office in Tokyo and about the U.S.–Japan Cooperative Science Program is at best incomplete, if not deceptive in its simplicity. To understand what the goals of the United States and Japan were in practice for the U.S.-Japan Cooperative Science Program and whether or not they were realized, we need to follow their articulation in the U.S.–Japan Cooperative Science Program and in the subsequent U.S.–Japan programs in the environmental and medical

[27] National goals for international collaboration in science and technology are discussed in U.S. Congress, Office of Technology Assessment (1995), National Research Council (1997), National Research Council and European Science Foundation (1998), and National Research Council (1999). These studies on international collaboration in science and technology are of course not unaware that the distance between policy planning and implementation can be large. The last two studies in particular are very much attuned to possible differences between planning and practice (i.e., NRC and ESF 1998 and NRC 1999). But they do not know what happens to state goals in practice. One excellent study of an international collaboration in science and technology that did at least examine whether or not state goals can be said to have been realized is Lorell (1996).

sciences, nuclear energy, defense technology, and space technology, among other areas.[28]

This dissertation argues that states' goals were (re)articulated and (re)cast within the context of the U.S.-Japan collaboration on the ASTER remote-sensing system. To state this, however, is not to say that the U.S. and Japan teams articulated state goals that were in contradiction with the state goals that were set forth and possibly passed on by their respective national policymakers and bureaucracies. Rather, for state goals to be useful in their collaboration, members of the two teams adapted

---

[28] The science collaborations stemming from the U.S.–Japan Cooperative Science Program are briefly reviewed in Bloom (1984) and Uyehara (2000: 25-30). Let me offer an additional point that illustrates the almost necessarily deceptive simplicity of "high politics" narratives as well as of anecdotal "origin stories."

A close reading of Bloom (1984: 92), Uyehara (2000: 26), and Blanpied (2002: 2) reveals some ambiguity and disagreement over when and why the "NSF Tokyo Office" was exactly "established." As I did in the "typical narrative" offered above, Bloom and Uyehara state that the NSF Tokyo Office was established to administer the U.S.–Japan Cooperative Science Program in 1961. Yet, Blanpied states that the NSF Tokyo Office was in fact established 18 months earlier in June 1960 as a small information gathering and reporting office and that "the responsibilities of the Office increased significantly as a result" of the Cooperative Science Program. If Blanpied's details are correct, as seems quite likely, then the mistaken simplifications of Bloom's and Uyehara's narratives reinforce my point that practices "on the ground," including the bureaucratic act of setting up an overseas office, are often assumed and retold to follow "high politics" stories, even by knowledgeable commentators such as Bloom and Uyehara.

On similar grounds of skepticism, however, we can doubt Blanpied's narrative that the Tokyo office was established in June 1960. In agreement with Blanpied's story, the NSF's 1961 annual report notes that two personnel were assigned to the U.S. embassy in Tokyo in June 1960 (p. 133). At the time, however, those personnel could have been considered just short-term representatives rather than as the definitive beginning of the "NSF Tokyo Office." Blanpied seems to make elsewhere a general distinction between "short-term" activities and a long-term office. Blanpied's tenth endnote recognizes the seniority of the "establishment of the NSF Tokyo Office" by contrasting it with "short-term" NSF activities at overseas embassies, one of which was prior to June 1960. If Blanpied had judged the NSF's 1960 activities in Tokyo historically without the Tokyo office's long future in mind, he might have found those activities comparable with the NSF's other "short-term" international activities (this point is supported in part by NSF 1962: 153). Thus, even taking into account Blanpied's details, it can still be reasonably said that while NSF representatives were in Tokyo prior to the Cooperative Science Program, it was the Cooperative Science Program that gave those overseas assignments the permanency of an overseas "office." In any case, for Bloom and Uyehara, it was the intergovernmental collaboration that gave that office meaning for the U.S.–Japan relationship. Neureiter is also in agreement with this point (1998: 30).

My warning here about stories of "high politics" and the ambivalent status of the founding of the NSF Tokyo Office is analagous to the analysis that is discussed in Winner (1980), Joerges (1999), and Woolgar and Cooper (1999) regarding the "parable" of Langdon Winner's bridges and the ambivalent status of the bus schedule.

and modified state goals in ways that were politically significant, even if these state goals were not articulated "from scratch."[29] Thus, I am suggesting that each team was not a mechanical, talking intermediary for their respective state in their state's relations with its counterpart state. Their words and actions were not smoothly guided by the supposed goals of their state. The team members were, at the very least, active, competent, and creative mediators in the international arena.[30] As mediators, they

[29] This attention to state goals as they are articulated or re-articulated in the practice of scientific and technical collaboration is indebted most immediately to the insights of ethnomethodology, particularly to the canonical debate in Science and Technology Studies between Lynch and Bloor about the treatment of rules and rule-following in Wittgenstein's writings and in Science and Technology Studies more generally. The debate between Lynch and Bloor draws substantially on the writings of philosophers S. Kripke, S. Shanker, G.P. Baker, P.M.S. Hacker, and N. Malcolm, among others. See Lynch (1992), Bloor (1992), and Lynch's explication of rules in "ordinary action" in chapter five of Lynch (1993). Suchman's recommended approach to "plans" nicely summarizes the ethnomethodological touchstone for my approach to "state goals":

> For situated action, the vagueness of plans is not a fault, but is ideally suited to the fact that the detail of intent and action must be contingent on the circumstantial and interactional particulars of actual situations. Given this view of plans, namely as resources for action rather than as controlling structures, the outstanding problem is not to improve upon them, but to understand what kind of resource they are (1987: 185-186).

To be clear, however, it is neither this dissertation's goal nor plan to be an ethnomethodological account.

This focus on state goals as they are articulated in practice is also in agreement with Neumann's call for scholars in the constructivist and critical schools in the field of International Relations to better integrate their recent "linguistic turn" with practice (2002). Neumann highlights Wittgenstein as a "seminal theorist" to which the field of International Relations should "return" (p. 627). Neumann's article argues that an integration of discourse with practice can lead to a more distributed and localized understanding of diplomacy.

On a different point, the quotes around "from scratch" mark the word as an idiomatic expression. Those quotes can also be read as scare quotes, questioning when there ever might have been in a U.S.-Japan intergovernmental collaboration in the late twentieth century a *tabula rasa* that could be "scratched" for the first time. In this sense, because articulations *de novo* are arguably an historical impossibility, the qualification "even if these state goals were not articulated 'from scratch'" might not be as much as a qualification as some readers might think.

[30] Here, my use of the terms "intermediary" and "mediators" reflects a distinction recently defined by Latour (2005), a scholar whose writings have centrally shaped the field of Science and Technology Studies. In Latour's vocabulary,

> an *intermediary* is what transports meaning or force without transformation: defining inputs is enough to define its outputs. For all practical purposes, an intermediary can be taken not only as a black box, but also as a black box counting for one, even if it is internally made of many parts. *Mediators*, on the other hand, cannot be counted as just one; they might count for one, for nothing, for several, or for infinity. Their input is never

exercised judgment and selectively turned to state goals for guidance; they used state goals as rhetorical props on occasions that called for persuasion and justification; and they specified, refined, and recast state goals so that these goals would be more useful, politically and scientifically. The goals of the United States and Japan were substantial resources that the two teams used to shape the character and content of their collaboration, even if state goals were not dispositive of the two teams' development of a U.S.-Japan remote-sensing system.

Explanatory Sketches of International Political Order

This dissertation's central burden is to offer a convincing account of how a U.S. team and a Japan team of scientists and engineers worked together to build an international remote-sensing system. In particular, I seek to understand and explain how they made complex technical decisions and scientific judgments, since the two teams—initially at least—worked without the benefit of extensively-shared socio-political resources, such as shared authority. That is, in developing the ASTER remote-sensing system, the two teams faced the challenge of creating an international political order as well as the challenge of creating scientific and technical order. They worked on behalf of two states with different goals that in some cases diverged, and the two teams comprised members who were drawn from different communities of

---

> a good predictor of their output; their specificity has to be taken into account every time. Mediators transform, translate, distort, and modify the meaning or the elements they are supposed to carry (p. 39, italics in original).

As other scholars in Science and Technology Studies have done, I too am "sociologizing" to some extent the actor-network theory that is advanced by Latour in that my account of, and use of, Latourian actor-network theory: a) knowingly "misreads" the loose semiotics of Latour's analysis by conflating rhetorical signs and their referents, b) interprets actor-network theory as giving agency to nature and technologies, and then c) proceeds to reject that ontology and to privilege human agency in most of my use of actor-network theory. On "sociologizing" actor-network theory, see Lynch (1993: 107-113), Shapin (1995: 308, note 17), and Hilgartner (2000: 160, note 17).

remote-sensing practice. The depth and scope of the challenge that they confronted in creating order are left to the empirical chapters to substantiate. Yet, their achievement of order—to the extent that it was in fact achieved—presents a question for the literature in Science and Technology Studies. The question points to a gap in literature from International Relations as well. Both of those literatures to date have not explored to any great extent scientific knowledge-making and technology-building in international affairs outside of the realm of international organizations and regimes, especially scientific knowledge-making and technology-building in intergovernmental collaborations.

The most widely-cited literature in the field of International Relations which examines the politics of scientific and technical knowledge in international affairs is the literature on "epistemic communities." Peter Haas and others who have collaborated in and advanced that literature have defined an epistemic community as:

> a network of professionals with recognized expertise and competence in a particular domain and an authoritative claim to policy-relevant knowledge within that domain or issue-area. Although an epistemic community may consist of professionals from a variety of disciplines and backgrounds, they have (1) a shared set of normative and principled beliefs, which provide a value-based rationale for the social action of community members; (2) shared causal beliefs, which are derived from their analysis of practices leading or contributing to a central set of problems in their domain and which then serve as the basis for elucidating the multiple linkages between possible policy actions and desired outcomes; (3) shared notions of validity—that is, intersubjective, internally defined criteria for weighing and validating knowledge in the domain of their expertise; and (4) a common policy enterprise—that is, a set of common practices associated with a set of problems to which their professional competence is directed, presumably out of the conviction that human welfare will be enhanced as a consequence.[31]

[31] See P. Haas (1992: 3) and the articles in that special issue of *International Organization*, which are reprinted as P. Haas (1997). E. Haas (1990) and Adler (2005a) have also advanced the study of epistemic communities. While the above definition of epistemic communities likely arouses many skeptical questions from readers within the field of Science and Technology Studies—some of which are discussed below—it is commendable for Haas to lay out a tight definition and then subsequently an argument that is generally faithful to that definition in a manner that is clear enough to allow sharp critique. Other definitions of epistemic communities have been posited, as

For Haas and other students of epistemic communities in the field of International Relations, these epistemic communities serve as the backbone for an explanation of international coordination in policy questions such as global climate change.

The logic of the explanation is straightforward. Epistemic communities provide "decision makers" with "human interpretations of social and physical phenomena" in conditions of policy uncertainty, particularly "those which arise from the strong dependence of states on each other's policy choices for success in obtaining goals and those which involve multiple and only partly estimable consequences of action."[32] Epistemic communities may or may not be from the same profession or intellectual discipline, and they may or may not be "transnational" in nature (e.g., transnational coalitions).[33] Still, the emphasis in the literature is on transnational

---

Haas's article notes. For example, Ruggie (1975), borrowing Foucault's term episteme, defines epistemic communities as consisting of "interrelated roles which grow up around an *episteme*; they delimit, for their members, *the* proper construction of social reality" (p. 570, italics in original).

[32] Haas (1992: 3-4).

[33] The term "transnational" has circulated in the academic literature that is relevant to this dissertation with a variety of definitions. The Oxford English Dictionary defines "transnational" as "extending or having interests extending beyond national bounds or frontiers; multinational." Crawford et al. (1993) in their study of "the nationalization and denationalization of the sciences" defines "transnational science" as "activities involving persons, equipment or funds from more than one country" (p. 1). As Crawford et al. recognize, the term "transnational" can take on the connotation of "not-national" in contrast to "international" and "multinational." They chose, however, to not rely on the term "transnational" for that connotation and to make that meaning explicit in their use of the term "denationalizing" (which they do not concisely define). A well-known edited volume that explores "Bringing Transnational Relations Back In" the field of International Relations uses as its definition of transnational relations "regular interactions across national boundaries when at least one actor is a non-state agent or does not operate on behalf of a national government or an intergovernmental organization" (Risse-Kappen 1995: 3). Evangelista, for instance, explicitly adopted that definition in his study of a "transnational movement" among scientists and physicians which sought to shape the arms control policies of the United States and the Soviet Union (and later Russia) (1999: 6). In line with what I consider to be currently connoted by word "transnational," I take the term to mean not just "international" or "multinational" but as an extension across and outside of the "national" with respect to both geopolitical boundaries and political control, as is implied by the terms "denationalizing" and "non-state actor." Thus, in my taxonomy, I have the rough dichotomy of the "intergovernmental" (defined in note 8) and the "transnational," leaving the more general word "international" to mean either intergovernmental, transnational, or both. The distinctions in my dichotomy become problematized to some extent when the rhetorical practices of scientists and engineers are closely studied (see, in addition to this dissertation, Forman 1973).

epistemic communities of natural or social scientists who typically work in universities, think-tanks, or non-governmental organizations. Epistemic communities bring about international policy coordination in ways that suggest different kinds of political involvement and different kinds of influence, such as by "educating" decision makers, by "framing" issues for decision makers, by "identifying" particular interests and policies to decision makers, and by "formulating" specific policies.[34] For Haas and his collaborators, while "epistemic communities provide consensual knowledge, they do not necessarily generate truth."[35] Nevertheless, if epistemic communities were "confronted with anomalies that undermined their causal beliefs, they would withdraw from the policy debate, unlike interest groups."[36] Thus, in Haas's portrayal, although epistemic communities do not quite bring "truth to power," they are certainly not lobbyists. They are groups of true believers of particular theories who are looking to realize their policy preferences.

After being "informed by the causal beliefs and policy preferences of the epistemic community," decision makers of one state negotiate with the decision makers of other states, who may or may not have been similarly influenced by that same epistemic community.[37] If the decision makers of all of the states that were involved in negotiating a given international policy were similarly influenced by a transnational epistemic community, then it would be easier, "all things being equal," as the saying goes, to arrive at an international policy that was coordinated internationally. Empirical studies have argued that the degree to which international policies reflect

---

On a different point, with respect to the literature on epistemic communities, Adler (1992) makes clear that the term epistemic communities, according to the definition employed in the Haas (1992) collection, can be used to refer to groups that are exclusively national and to groups with members from different professions and disciplines.

[34] Different forms of these verbs are used throughout Haas (1992) to describe what epistemic communities do. Adler (1992) frequently uses "educate."

[35] Haas (1992: 23).

[36] Ibid., p. 18.

[37] Ibid., p. 4.

the "principled" and "causal" beliefs that were shared by an epistemic community can depend upon a variety of factors, including the "distribution of international power" and "domestic [institutional] structures."[38] In sum, the epistemic community approach that was articulated by Haas and his collaborators adopted what they called a "limited constructivist view"—a view that made intellectual space for ideas, knowledge, and practices within the "rationalist" orientation that dominated the field of International Relations at that time; the rationalist orientation emphasized the development of conditional hypotheses (i.e., "if . . . then . . ." statements) and the explication of "causal mechanisms" in theory building with the goal of generalization.[39] While the

[38] Ibid., p. 7. See, for example, the articles in the Haas (1992) collection. More recently, Evangelista (1999) has contributed to the literature on epistemic communities by investigating under what domestic structural conditions, such as "decentralized, fragmented states" (resembling the United States) or "centralized, hierarchical states" (resembling the Soviet Union), a transnational movement was and was not able to effect its preferred policy (p. 19). To be clear, Evangelista (1999) does not take on the full assumptions of the epistemic community research program, nor does he even call the transnational movement of scientists and physicians whom he studies an "epistemic community." While Evangelista does draw upon the literature on epistemic communities, such as Adler (1992) (e.g., p. 18, 195, 378), he generally situates his study in the literature that is organized around the theme of transnational social movements, ideas, and international norms in international relations (e.g., p. 6, 16-21). If the analyst drops any claims that the actors under study are in some way epistemologically-privileged in their beliefs, the intellectual move from the "epistemic communities" literature to, for example, the "transnational advocacy" literature is a small step. Keck and Sikkink (1998) define a "transnational advocacy network" as including "those relevant actors working internationally on an issue, who are bound together by shared values, a common discourse, and dense exchanges of information and services" (p. 2). Whereas epistemic communities "inform," transnational advocacy networks "persuade" as well as "inform."

[39] Haas (1992: 23). Katzenstein et al. (1998) reviews and explains the "International Political Economy" literature in International Relations in terms of "rationalist" and "constructivist" orientations. Fearon and Wendt (2002) offer a review and critique of the International Relations literature which also organize the literature in those terms. Dessler and Owen (2005) in their review of constructivism in the field of International Relations highlight the development of conditional statements as a central element of the structural and rational conception of theory building and as a piece of the puzzle of explanation which to date, they argue, constructivism has not provided. "Causal mechanisms" specify how the effects of a condition come into being. For example, a causal mechanism of the epistemic communities literature is the way that an epistemic community interacts with decision makers to bring about their preferred preferences (by "informing," "educating," etc.). In Hacking's explication and critique of constructivist literature writ large (especially that in Science and Technology Studies), which he calls "constructionism" in order to avoid confusion with constructivism in the philosophy of mathematics, he characterizes the literature partly by its attention to causal mechanisms (what he calls "causal routes"):

theoretical and metatheoretical landscape of the field of International Relations has changed significantly since the articulation of the epistemic communities approach, especially with the establishment of "constructivism" in various forms, no approach in the field of International Relations has examined the politics of scientific and technical knowledge in international affairs as thoroughly as the epistemic communities literature has.[40]

Whereas the epistemic communities literature and the more general literature in International Relations have used consensus on "principled" and "causal" beliefs concerning nature and technology to support explanations of international order (e.g.,

---

> Hence by *constructionism* (or social constructionism if we need, on occasion, to emphasize the social) I shall mean various sociological, historical, and philosophical projects that aim at displaying or analyzing actual, historically situated, social interactions or causal routes that led to, or were involved in, the coming into being or establishing of some present entity or fact (1999: 48).

[40] Haas and Haas (2002) and Adler (2005b) have integrated their work on epistemic communities into their preferred forms of constructivism, which they respectively call "pragmatic constructivism" and "communitarian constructivism." While the different versions of constructivism that have been recently staked out in theory in the field of International Relations are different in their theoretical positioning, differences in how various studies handle, in practice, the empirical material in their accounts are arguably much more revealing and often do not neatly correspond to the differences that were articulated in their respective theoretical statements. Consequently, I make no effort here to translate at the level of abstraction among the nuances of the various constructivisms in International Relations (or in Science and Technology Studies as well), especially when studies' handling of the empirical material and the content of their empirical material are usually much more interesting, often providing insight by showing how distinctions are ordered and then blurred in practice. More importantly, because this dissertation is focused on a particular question concerning intergovernmental collaboration in science and technology, its contribution is relevant to most of the constructivisms ("critical theory" or "critical constructivism" being the possible exception), and detailed parsing of the various constructivisms is unnecessary to communicate that contribution.

In his integration of "epistemic communities" into his preferred version of constructivism, P. Haas has preserved and made use of much of the original epistemic communities approach, particularly the rationalist divide between science and policy and between scientists and policymakers. In a recent article, P. Haas (2004) conceives of epistemic communities as "the transmission belts by which new knowledge is developed and transmitted to decision-makers" to influence policy (p. 587). According to Haas, the more "autonomous and independent science" and these epistemic communities are from policy "the greater its [i.e., science's] potential influence" (p. 576). Science and policy should be thought of as separate, and "it may be best if the two activities can be kept as separate as possible" (p. 580). Haas's empirical overview of the workings of the Intergovernmental Panel on Climate Change in that recent article draws heavily upon the original framework of epistemic communities (p. 580-584).

in the form of international policy coordination), the field of Science and Technology Studies has taken as one of the field's central projects the examination of that consensus and the creation (or the "production") of knowledge at the core of that consensus. The field of Science and Technology Studies has in general questioned any easy consensus on beliefs about nature and technology and has studied how consensus has in practice been achieved with difficulty—if it is achieved at all—especially with respect to beliefs or claims that are asserted or accepted as scientific knowledge. Much of the literature has emphasized that any consensus is not just an achievement, but a "social" achievement that could have been filled with contention at one point in time but that was (provisionally) settled within a socio-political context and with the assistance of particular socio-political resources.[41]

For example, studies in the field have argued that the successful replication of experimental results has often required personal relationships of trust and the person-to-person (or the person-to-instrument-to-person) passing of skill-based techniques.[42] If experimenters were to persuade their scientific community of the validity of their results, of the proper calibration of their experimental instrument, or of the significance of their results for scientific knowledge, then they had to conduct themselves as competent members of a scientific culture that comprised rhetorical and literary techniques, certain methodological practices, particular understandings of what did and did not count as evidence, and socio-political organization.[43] As described by work in the field of Science and Technology Studies, knowledge claims that arose

[41] Footnote 2 lists central texts and literature reviews from the field of Science and Technology Studies which substantiate this point. In writing here about a "difficult consensus," I am not implying that consensus in the sciences is inherently any more difficult to achieve than consensus in other human enterprises.
[42] For example, Shapin and Schaffer (1985) and Collins (1992 [1985]).
[43] Ibid., and also see Gilbert and Mulkay (1984), Dear (1991), Gieryn (1992a), Shapin (1994), Knorr Cetina (1999), Collins (2004), and Doing (2004).

from experiment were in principle open to challenge.[44] Debate over the validity of knowledge claims inherently embroiled socio-political resources in socio-political circumstances along with rational thinking, technical analysis, and factual assertions. Successful claims to knowledge were successful in part because socio-political resources were used in ways that helped to make an experimenter's thinking "rational," her analysis "technical," and her assertions "factual."[45]

Let me illustrate the point with a brief exemplar that does not require much background to be explained or discourse to be described, as many examples drawn from within the annals of science and technology would. As a member of the Guidance and Control Panel of the Air Force Scientific Advisory Board, the distinguished physicist George Gamow challenged the technical feasibility of inertial guidance in 1948. He did not challenge, however, just any inertial guidance system in the abstract. By drawing a cartoon depicting a confused Air Force pilot trying to understand the path that he had flown, Gamow literally caricatured the U.S. Air Force's future use of inertial guidance systems—guidance systems that were under development by Charles Draper at MIT's Instrumentation Laboratory. The "abstract argument" of the feasibility of inertial guidance was grounded in the political and in the personal. Likewise, the persuasiveness of Charles Draper's response to Gamow's challenge did not as much lay in academic papers that compellingly documented a feasible design but in the building and demonstration of the technology for the Air Force and in subsequent production contracts for the Air Force.[46] The socio-political

[44] See the "experimenter's regress" in chapter four of Collins (1992 [1985]) and in Collins (2004), especially chapter forty three. Chapter ten of Sismondo (2004) reviews "controversy studies" in Science and Technology Studies.

[45] The precise theoretical underpinnings of this "making," indeed what the word "make" means, are contested within the literature. See the debates in Pickering (1992) (particularly Collins and Yearly 1992), chapter three of Lynch (1993), Sismondo (1996), Bloor (1999), Hacking (1999), Latour (1999), and Zammito (2004).

[46] Dennis (1994: 447-450) and MacKenzie (1990: 66-74)

positioning, dynamics, and solidarity of this technical community were a far cry from that of an epistemic community. The definition of epistemic communities which is set forth by Haas and his collaborators could not be used to describe the community of scientists and engineers who researched, developed, and assessed inertial guidance systems at MIT's Instrumentation Laboratory and elsewhere, if Haas and his collaborators wanted to explain, hypothetically speaking, the role of these scientists and engineers in producing accuracy assessments that were used to negotiate an arms control measure.

The exclusion of these scientists and engineers from the epistemic community approach, however, raises the general question of how often epistemic communities, as defined by Haas and his collaborators, can convincingly be stipulated as existing in practice.[47] From the perspective of the literature in Science and Technology Studies, the epistemic communities approach seems to be inattentive to knowledge-making practice and consequently seems to be problematically apolitical.[48] Beyond debates

[47] Perhaps a more persuasive example with which to illustrate this point might have been drawn from a controversy in "basic science" which would initially be presumed to be deeply isolated inside the laboratory or the "ivory tower," without obvious connections to politics, especially national security politics. For those examples, see Pickering (1984), Shapin and Schaffer (1985), Latour and Woolgar (1986 [1979]), Pinch (1986), MacKenzie (2001), Doing (2004), and Collins (2004). Nevertheless, the moment these communities embark on a "common policy enterprise"—as described by the epistemic communities approach—they have an obvious connection to politics.

[48] Miller and Edwards (2001b: 4, 16) briefly allude to this critique, but Darst (2003: 86) argues that Miller and Edwards have unfairly taken the epistemic communities literature as a foil. In my reading, Darst is correct that Miller and Edwards (2001b: 4, 16) do not recognize in that essay how the epistemic communities literature has positioned the concept of epistemic communities in broader political contexts, such as in international regimes. Furthermore, in my reading, the critique of Miller and Edwards (2001b: 4, 16) overlooks the shared "principled beliefs" component of epistemic communities. Still, Darst misses Miller's and Edwards' fundamental point about what Miller and Edwards argue to be the inherent politics involved in the process of making knowledge. I write "seems to be" in the statement noted above because I would like to keep open the possibility that because the literature in International Relations and the literature in Science and Technology Studies generally emphasize different explanatory goals and generally have different metatheoretical assumptions, scholars in one field are not necessarily in the best position to take issue with the supposed "faults" of a work that is in another literature. I certainly do not mean to imply, however, that the sharing and critiquing of ideas across fields are not valuable to intellectual inquiry. This introduction to this dissertation plainly presumes otherwise.

about particular knowledge claims, work in Science and Technology Studies has argued that scientific cultures and particular ways of knowing the natural and social world are intertwined with and bolstered by the “wider polity” and the “wider society.”[49] Framing that argument more holistically, some scholars in Science and Technology Studies have proposed that natural and social orders are “co-produced,” that is, that the “ways in which we know and represent the world (both nature and society) are inseparable from the ways in which we choose to live in it.”[50]

Although the epistemic communities approach to explaining international coordination might stipulate too much solidarity, the literature in Science and Technology Studies suggests too few socio-political resources to account for international coordination outside of international organizations and treaty regimes.[51] In particular, the literature suggests too few socio-political resources to account for how a U.S. team and a Japan team of scientists and engineers collaborated to develop an international remote-sensing system. These scientists and engineers did not have pre-existing relationships of trust with each other. They did not even know each other before they embarked upon their collaboration. They did not share a laboratory or an organization, nor could they presume any shared bureaucratic or institutional procedures. Furthermore, they lacked a shared scientific or political culture in any meaningful sense of the word “culture” (previous to their collaboration, they had not even published in the same journals, with only very few exceptions). They did not share a native language, and it was not unusual, as it turned out, for members from one team to doubt whether their words were understood by members of the other. Finally,

---

[49] For “wider polity” and “wider society,” see Collins (1981), chapter eight of Shapin and Schaffer (1985), and chapter six of Collins (1992 [1985]). MacKenzie (1990) advanced the argument for technology and technical knowledge.
[50] The quote is from Jasanoff (2004: 2). On the theme of “co-production,” see the studies that are collected in that volume. Also see Shapin and Schaffer (1985), Latour (1990), and Latour (1993).
[51] For the literature that substantiates this point, see notes 3 through 6.

they did not share a centralized political authority. In principle, they answered to different states and to no particular international organization or treaty regime. Given the emphasis in Science and Technology Studies on socio-political context and on the use of socio-political resources to account for the jump starting (i.e., "bootstrapping") and subsequent buttressing of the development of science and technology, and considering the field's emphasis on the "local" creation of scientific and technical knowledge, what does the literature in Science and Technology Studies suggest can account for these two teams' achievement of international order, as that order was realized in their development of a U.S.-Japan remote sensing system?[52]

While many eclectic, historically-contingent answers to this question can be imagined in the abstract, only one systematic approach in the field of Science and Technology Studies could be used to sketch a compelling account of international knowledge-making and technology-building: the "actor-network theory" that was articulated in Latour's book *Science in Action* (and to be clear, throughout the

---

[52] Reviewing the literature concerning the sociology of scientific knowledge, Shapin (1995) asks similar, bur broader, questions:

> The localist thrust of recent SSK [i.e., sociology of scientific knowledge] has generated one of the central problems for future work. If, as empirical research securely establishes, science is a local product, how does it travel with what seems to be unique efficiency? One appeal to the modernist grand narratives of reason, reality, and method was the table-thumping response they offered to questions about the travel of science. If, however, universality can no longer be accepted as an assumption flowing from the very nature of the knowledge or the "method" for making it, then what are the mundane means that so powerfully effect the circulation of science? And is that travel, indeed, to be treated as real, or is what circulates yet another illusory grand narrative? (p. 307)

In my reading of the recent literature, work in Science and Technology Studies is still wrestling with these difficult questions, where they have not been skirted or else dismissed entirely or otherwise not grappled with because of more interest in other questions. Certainly no contingent generalizations have been offered for intergovernmental collaboration in knowledge-making and technology-building. In emphasizing a problem of international order, however, I am not adopting what Lynch (1993) sees as "the prevailing version of constructivism" which "tells us that scientists confront a primordial chaos from out of which they construct facts" (p. 286). As I suggested in note 29, the U.S. team and Japan team of scientists and engineers were not working "from scratch." They were working in time.

dissertation, when I refer to “actor-network theory” or “Latourian actor-network theory” I am referring not in a general way to Latour’s entire oeuvre but to actor-network theory as it is articulated in *Science in Action*).[53] The actor-network theory of *Science in Action* describes the development of science and technology by implicitly adopting a Hobbesian realism as a political sensibility, with Machiavellian, will-to-power actors for whom power is manifested in the imperialist building of “networks.”[54] It presumes minimal socio-political order. Scientific facts and technologies are created when a protagonist links together in “alliances” human and non-human entities such as statements, readings from instruments (“inscriptions”), material artifacts, engineers, and scientists with increasing density and cohesion, until the network collapses and hardens into uncontestable “black boxes.” The building of

---

[53] Before and since *Science in Action* (i.e., Latour 1987), actor-network theory has been set forth, developed, and used in different forms, rejecting some elements and revising and adding others, (e.g., Callon and Latour 1981; Callon 1986, Law 1986a and other essays in Law 1986b; Law 1987; Latour 1988a; Law and Callon 1988; Latour 1990; the essays in Law 1991; Latour 1993; Mol and Law 1994; Latour 1996; the essays in Law and Hassard 1999; Latour 2004; and Latour 2005). Many works in Science and Technology Studies have been informed by elements of actor-network theory without adopting its core assumptions or vocabulary (e.g., Hilgartner 2000). Latour himself has said that his work is a “moving target” (1999: 115). Despite the different interpretations of actor-network theory after *Science in Action* (including by Latour), I use *Science in Action* above and throughout the dissertation—rather than attempting to explicate and use later versions—because the motivating logic that drives the network building in *Science in Action* is explicit and because *Science in Action* is a well-known work that is indisputably within the canon of Science and Technology Studies. Furthermore, the later versions of actor-network theory sometimes lack the force of *Science in Action*. For example, although Latour (2005) generally remains true to the central methods and principles set forth in *Science in Action*, Latour (2005) does not have *Science in Action*’s explanatory suggestiveness owing to the attempt of Latour (2005) to position thoroughly actor-network theory as a generalized challenge to social theory in a manner that is frequently reminiscent of ethnomethodology. Whereas *Science in Action* is filled with well-developed empirical accounts of “science in action,” Latour (2005) rarely offers a sustained account or analysis, limiting itself instead to explicating vocabulary and method.

[54] In determinedly formal readings of *Science in Action*, these networks are rhetorical systems of signs, rather than networks of facts, material artifacts, and people. Much more work, however, is required of the reader to restrict *Science in Action* to a semiotic account of the development and circulation of scientific and technical texts than is required to read *Science in Action* as a guide for, and demonstration of, “how to follow scientists and engineers through society” (which is the book’s subtitle). Therefore, I knowingly “politicize” and “sociologize” *Science in Action*, as many other readers have done. For more on “sociologizing” Latourian actor-network theory, see note 30. Two penetrating reviews of *Science in Action* are Shapin (1988) and Amsterdamska (1990).

networks and the creation of black boxes are carried out by means of a power-laden, goal-oriented process. The "rules" of this process "are the oldest of politics":

> Weaken your enemies, paralyze those you cannot weaken . . . , help your allies if they are attacked, ensure safe communications with those who supply you with indisputable instruments . . . , oblige your enemies to fight one another; if you are not sure of winning, be humble and understated (p. 37-8).

According to *Science in Action*, black-boxed facts and objects are uncontestable and "objective," at least until a "dissenter" with another network of a longer length can, in a controversy, pull the black boxes apart, rendering the competing chain of "associations" as "subjective." In these "trials of strength," battling knowledge-makers and technology-builders might construct laboratories and "counter-laboratories," each trying to become an "obligatory passage point" through which other efforts at knowledge-making and technology-building must pass to achieve their interests, "betraying" their original "camp" and becoming "enrolled" into the network-building of the expanding laboratory. To fortify their "technoscience"—which is what Latour calls science and technology to emphasize their inseparability—competing knowledge-makers and technology-builders engage in "proof races." Latour explains that:

> The similarity between the proof race and the arms race is not a metaphor, it is literally the mutual problem of *winning*. Today no army is able to win without scientists, and only very few scientists and engineers are able to win their arguments without the army. It is only now that the reader can understand why I have been using so many expressions that have military connotations (trials of strength, controversy, struggle, winning and losing, strategy and tactics, balance of power, force, number, ally), expressions which, although constantly used by scientists, are rarely employed by philosophers to describe the peaceful world of pure science. I have used these terms because, by and large, technoscience is part of a war machine and should be studied as such (p. 172).

The outcome of these struggles not only settles what is factual and what is not, but it "stabilizes society," such as who is doing "science" and who is doing "politics."

Because the world, however, is not (yet) a single laboratory, to produce knowledge about the world sometimes requires going outside of laboratories and into the world and bringing back inscriptions that tell about the world. Here, the actor-network theory of *Science in Action* offers an imperialist vision in which knowing the world and dominating the world go hand-in-hand, both requiring the collection, sorting, and aggregation of regularly-formatted inscriptions at places where judgments can be made about the world and where action can be taken "at a distance." These places are "centers of calculation":

> After having followed expeditions, collections and enquiries, and observed the setting up of new observations, of new inscription devices and of new probes, we are now led back to the centres . . . inside these centres, specimens, maps, diagrams, logs, questionnaires and paper forms of all sorts are accumulated and are used by scientists and engineers to escalate the proof race (p. 232).

National history museums, census bureaus, geological surveys, tax authorities, and military command and control centers are all mentioned by Latour as centers of calculation. These centers of calculation are greatly aided in their imperialist knowledge-making by metrology. In the "gigantic enterprise" of metrology, long networks of standardized weights, instruments, and measurement systems become ubiquitous throughout much of the world and "pave the way" for the work of centers of calculation (p. 252). In Latour's vision of metrology, the world is slowly coming to resemble a laboratory, and a central question concerning dominance in the world is whose standards will be used and what advantage will be gained by the use of those standards. In sum, in the actor-network theory of *Science in Action*, the resources of knowledge-making and technology-building might be distributed, but they are not shared. Gathering them to make knowledge and to build technology is not an act of community or of even socio-cognitive understanding, but of material power.

To provide a convincing account of how a U.S. team and a Japan team of scientists and engineers worked together to build an international remote-sensing system, answers to "why" questions need to be provided. Why were certain technical claims challenged by some scientists and not by others? Why were particular design characteristics selected? Why did the two teams choose to calculate a measurement using the algorithm that they did? More broadly, why were the goals of one team realized in one area, but not in another? Why did a particular technical practice or decision-making process come to have the form that it did? Answering these "why" questions might not be sufficient to constitute an illuminating examination of the development of a techno-political system, but any account that did not offer answers to these questions would be lacking.

Each of the two approaches outlined above can be employed to draft an "explanatory sketch" that might account for these "why" questions. What I mean by "explanatory sketch" is an interpretation that provides a rough account of some specific event, activity, or observation which can be compared and contrasted with other sketches of that event, activity, or observation and which can also offer lessons that can be adapted to other empirical contexts.[55] The sketch for the epistemic

---

[55] My use of the term "explanatory sketch" is indebted to Katzenstein and Sil (2004: 13). I hope my use of their term facilitates cross-disciplinary (as well as intra-disciplinary) discussion about developing portable analyses that are suggestive but that do not presume too much about what it means to provide an empirically-sensitive, convincing account. I think the term "explanatory sketch" is especially well-suited for this purpose because of the tenuousness that the word "sketch" connotes. Moreover, if the adjective "explanatory" is not required to mean something more forceful than "accounts," then arguably most post-structuralists can also draft and submit explanatory sketches for discussion and comparison, provided that they do not deny that their accounts imply at least a weak sense of causality. According to Katzenstein and Sil (2004):

> Explanatory sketches broadly refer to any interpretation of a set of observations that is intended to generate a causally significant understanding of specific empirical outcomes, whether these are specific historical events, patterns of similarity and dissimilarity in

communities approach would first look for a core of principled and causal beliefs which is shared among the members of the U.S. and Japan teams. Although almost all of the members of these two teams had not met previous to their collaboration on the remote-sensing system, it could be argued, in line with the epistemic communities approach, that they were part of a "tacit" transnational community of geologic remote sensing.[56] Members of the Japan team had followed closely the work of many of the U.S. scientists who had published path-breaking work in remote sensing, particularly thermal remote-sensing. A few of the U.S. scientists had authored textbooks and chapters of handbooks that some of the scientists in the Japan team had read in their doctoral studies.

Based upon what had been published in these articles and texts, principled and causal beliefs could be assumed, such as the effectiveness of particular sensing bands in the thermal electromagnetic region for discriminating among different types of rocks. Using articles published in the English language (the U.S. scientists did not read Japanese), we could also map out standards of validity for technical claims, such

---

> broad configurations, or variations across comparable processes. As such, explanatory sketches are sufficiently open-ended to encompass a wide range of empirical claims (p. 13).

With this definition and with my definition given in the text above, I adopt a deflated, interpretative sense of causality which is not required to be statistical in nature. The causality claimed in this dissertation, I would argue, is in agreement with the causality claimed in Katzenstein and Okawara (2004) and Suh (2004). Those two essays are among those essays that Katzenstein and Sil (2004) introduce in the Suh et al. (2004) volume which they described as offering "explanatory sketches." For critiques in Science and Technology Studies of strong causality claims, see Woolgar (1981), Latour (1988), and Lynch (1993: 57-59, 75-76). Deflated, implicit causality claims can be readily found even in analyses that their authors describe as "story-telling." See, for example, Jasanoff (2005: 11).

[56] Evangelista (1999: 39-40) argues that Soviet scientists shared a "transnational scientific ethos" with activist scientists in the United States because the Soviet scientists read and were inspired by articles in journals such as *Bulletin of the Atomic Scientists*. Evangelista's use of "tacit" to describe this form of transnationalism is distinct from the use of the word in "tacit knowledge," a key concept in the field to Science and Technology Studies which describes skill-based knowledge that can not be well explained in writing and that is generally transferred through person-to-person interaction (for uses of "tacit knowledge" in the Science and Technology Studies literature, see, for example, Collins 2001 and Vogel 2006).

as signal-to-noise ratios, benchmarks for judging the effectiveness of remote-sensing systems, and typical comparisons involving well-established knowledge and methods (referring, for example, to the development and use of the United States' widely-known Landsat satellite or to France's SPOT satellite). Based upon the status of these scientists as *geologic* remote-sensing scientists, that is, as geologists and geophysicists, we might posit principled views that the group might have shared on issues such as the importance and utility of remote sensing for investigating a whole host of *geologic* concerns, including oil exploration, environmental pollution, and climate change. These principled views might then be reflected in their development of the remote-sensing system and its policies, such as what kinds of data would be collected and how these data would be distributed.

Finally, once this normative and epistemic solidarity had been established, we would expect most of the "political" decisions to be negotiated between the officials from each teams' sponsoring agency, informed by the united views of the two "science" teams. The sponsor of the U.S. team was the U.S. National Aeronautics and Space Administration (NASA). The sponsor of the Japan team was Japan's Ministry of International Trade and Industry (MITI). On matters about which the two science teams had expressed their united preferences, we would then trace how some of their preferences were accepted and implemented and why others were not. The outcomes of negotiations that concerned issues for which the two teams' preferences had not been accepted (i.e., the residual issues) would then be explained using well-developed analytical frameworks from bargaining theory, perhaps emphasizing the disparity in technical experience and financial resources between NASA and MITI and any differences in the hardware that the two agencies were bringing to the table for the collaboration. According to this reasoning, Japan's preferences would win the day in the design of the instrument, since Japan paid for and was principally responsible for the development of the ASTER remote-sensing instrument. The United States' preferences would dominate in the design of the ground data and information system, since the data and information system for the ASTER instrument had to be standardized with NASA's other data and information systems.

In contrast to the epistemic communities approach, an explanatory sketch that employed Latourian actor-network theory would explain the "why" questions by examining closely the politics of technical practice: which team mustered what resources in each debate and what networks were attached to those resources.[57] Importantly, these debates would be goal-oriented "trials of strength" in which the U.S. and Japan team's interests were posited in advance. The discussions of the two teams would be filled with measurements (i.e., "inscriptions") and chains of citations, as each side tried to increase their rhetorical strength. Measurements conducted in the laboratory would likely be most persuasive, because they would be difficult to challenge without a "counter-laboratory." When technical claims were disputed in the design of the remote-sensing system, the realpolitik that Latourian actor-network theory presumes would be conducted through tactics that did not as much investigate the reasoning behind the claims, as they dealt with the interests that those technical claims advanced and the "enrolling" of other scientists and instruments into those interests through the "translation" of their initial interests. These "translations" include: a team A "pushing" the interests of a team B to align them with those of team A; team A "displacing" team B's interests at a time in which team B thought their own interests were "cut off;" and team A appealing to team B to take a "short detour" in achieving team B's interests, through which these interests would then become entangled with the interests of team A. In the playing out of these and other tactics,

[57] Throughout the dissertation, whenever I refer to "actor-network theory" or "Latourian actor-network theory," I mean the actor-network theory of Latour's *Science in Action*. For more on this point and for other literature on actor-network theory, see footnote 53.

one team would try to become indispensable to the fact-making, the "obligatory passage point."[58]

Because the U.S. team and the Japan team had available to them different networks for these "trials of strength," we would expect that the team with the most developed network would generally win the contests. Unlike the explanatory sketch that employed the epistemic community approach, initial consensus would not be expected even regarding many "core" issues of knowledge. The Japan team would dominate the discussions—down to the slightest detail—about the instrument's hardware: since Japan's contractors were responsible for the actual building of the instrument, they could advance technical claims with much longer networks, claims that had attached to them laboratory inscriptions. Neither team would abandon their interests easily (remember, in Latourian actor-network theory, it is a Hobbesian world). The U.S. team would not hesitate to muster whatever leverage they could find in debates, including bringing in more rhetorical firepower by escalating issues up the chain of command of NASA's hierarchy, if they thought that NASA might be able to exercise more influence with MITI. The U.S. side would dominate the design of the ground data and information system from the very beginning and on almost all issues, because the networks and centers of calculation of NASA's systems were far larger and more established than MITI's networks. Metrology would be essential to the circulation of data, and standardization battles would be expected, with again, the U.S. most likely winning, given its much more pervasive network of standardization and calibration in remote sensing, in comparison to Japan's and especially MITI's network. Neither team would cede authority to the other or embrace any norms, unless the team could anticipate that their interests would be advanced as a consequence. Data would not be shared if it meant a loss of national, team, or personal control. In sum, with the

[58] Latour (1987: 108-120)

exception of issues that were tightly confined to instrument hardware, the ASTER remote-sensing system would be through and through an imperial extension of U.S. hegemony, according to the explanatory sketch of Latourian actor-network theory.

The explanatory sketches offered by Latourian actor-network theory and by the epistemic communities approach are competing interpretations of the creation of a specific international political order. Comparing how these interpretations do and do not account for the "why" questions of the design of an international remote-sensing system contributes differently to the literatures of International Relations and Science and Technology Studies. The literature in International Relations generally has not investigated the practice of technical activity to answer political questions, even for political questions about techno-political systems. To investigate which interpretation is better able to account for the "why" questions of the design of an international remote-sensing system, the technical discussions, practices, and decisions of the two teams need to be examined in detail. Although a line between the "technoscientific" and the "political" is drawn by the epistemic communities approach, no such distinction is recognized by Latourian actor-network theory, at least not until that line is drawn as an outcome of a "trial of strength" through which technoscience and the political are "co-produced" in one move.

In contrast to the literature from the field of International Relations, the literature of Science and Technology Studies generally does not have a practice of making "alternative accounts" explicit. These two explanatory sketches present different accounts of the development of science and technology in the international arena and lead us to expect different outcomes from the technical decisions and scientific judgments of the U.S. and Japan teams concerning the development of the international remote-sensing system. Focusing in on the "why" questions using these two explanatory sketches not only can help us better to explore the strengths and weaknesses of each sketch, but keeping in mind two sketches also checks one or the other from being the assumed interpretation. Furthermore, the sketches might displace implicit, typical narratives—such as that offered earlier about the beginning of U.S.-Japan cooperation in basic science—as well as other forms of conventional wisdom. This dissertation, however, does not use every decision or negotiation as an opportunity to test one explanatory sketch against the other, judging which sketch was superior in its capacity to answer the "why" questions. Rather, the dissertation develops its own explanatory sketch which, I argue, better answers those "why" questions by interpreting how the two teams conducted their joint work, describing in particular the socio-political conditions and the evolving form of their technical decision making and collective exercise of scientific judgment.[59]

The Practice of Technoscientific Diplomacy

This dissertation argues that the intergovernmental collaborative work of the U.S. and Japan teams to build an international remote-sensing system can best be explained by attending to what I call the "technoscientific diplomacy" that the two teams conducted. Here is what I mean by "technoscientific diplomacy." Following the general use of the term "technoscience" by Latour and by other scholars to refer to the

---

[59] Thus, I am in general agreement with Shapin (1995) when he writes in his review of the sociology of scientific knowledge:

> Latourian social order appears all natural fact and no moral fact. Therefore, the onus on those who suspect the adequacy of Hobbesian accounts of order would be to produce a post-Mertonian picture of the moral economies of science—the locally distributed conceptions of legitimacy, authority, and trust by which scientific knowledge comes to be a collective good, the moral-pragmatic preconditions for intersubjectivity, and the mundane means by which moral orders of scientific knowledge-making come to be distributed around the world (p. 309).

This dissertation takes on an onus that requires that kind of socio-political exploration.

activities and tools of scientific and technological development, “technoscientific diplomacy” suggests a diplomacy that is not only political, as we would expect from the term “diplomacy,” but that is also scientific and technical in its character.[60] I use the term “technoscientific diplomacy” to describe (and hence interpret) the negotiating practices that the U.S. and Japan teams employed in their technical decision making and in their exercise of collective scientific judgment.

In coining (to my knowledge) the term “technoscientific diplomacy,” I am arguing that the conceptual vocabulary and explanatory sketch offered by the epistemic communities approach and the conceptual vocabulary and explanatory sketch offered by Latourian actor-network theory are insufficient to describe the negotiating practices of the two teams. Furthermore, while the term “negotiating practices” might itself be accurate, it does not characterize these practices beyond the word “negotiating.” Because the term “negotiating” is commonly used throughout Science and Technology Studies to describe many activities, it has unfortunately lost some of its conventional meaning that connotes two or more parties knowingly working out the terms of an agreement or settlement (in the sense that not every act of communication, discussion, or debate is necessarily a negotiation). The negotiating activity that is studied in this dissertation was filled with relations that were explicitly international in a way that justifies the use of the term “diplomacy.” For these reasons, I use “technoscientific diplomacy” to describe the negotiations of the two teams. “Technoscientific diplomacy” highlights three qualities of the negotiating practices of

[60] Latour (1987: 29, 174-175). My use of the term “technoscience” and “technoscientific” avoids the repeated use of the words “science and technology,” as it did for Latour. I also use the term to remind us, as Latour did, that the activities of scientific and technological development are closely interrelated in practice. Histories of research and design in areas including aeronautics, semiconductor physics, laser engineering, and genomics demonstrate the difficulty of empirically distinguishing scientific and technological development (e.g.,Vincenti 1990). When science is distinguished from non-science, the boundary is drawn in particular circumstances (Latour 1987 and Gieryn 1995). Ronald Kline has, for example, documented how engineers and scientists created the category of "applied science" for purposes of professionalization (1995).

the two teams: team members' positioning as liminal state actors; the enactment of "the state" and of U.S.-Japan relations; and the synthesis of state power and scientific knowledge.

<u>Liminal State Actors</u>

Most of the team members were neither unambiguous state actors nor unambiguous non-state actors; to greater and lesser degrees, team members were on the threshold of their respective states, working on behalf of their state but carrying out work that they in part had proposed to their state. I call these team members "liminal state actors." In some cases, the U.S. and Japan teams included scientists and engineers who were civil servants who worked for federal agencies or national labs. More often, however, team members were primarily affiliated with, and working for, universities, corporate contractors, and themselves (as entrepreneurial private contractors). This latter characterization might lead us to call them "non-state actors." Yet, once their proposals for research and technical work were selected for funding by their respective governments, should their status in the eyes of an analyst change from "non-state actors" to "state actors," as the common definition of "state actor" would suggest? Or should they still be considered "non-state actors?" Moreover, what if, as is frequently the case, their salaries and research activities were only partially funded by their governments? These questions concerning membership category challenge the existence of any bright line around the state in their collaborative activity and argues for an interpretive sensibility.

Rather than force fitting these individuals into either the "state actor" or "non-state actor" membership category when it makes little sense to do so, that is, when their position bears little resemblance to others who are grouped into those categories (such as military officers in the former and activists in the latter), it is more analytically penetrating to describe how and when these actors were situated, and situated themselves, as working on behalf of their states. Thus, I use the term "liminal state actors" to provisionally label persons at the threshold of the state, who worked between what they and their colleagues saw as positions that were less ambiguously a part of either the government bureaucracy or the private sector.[61] The degree to which these liminal state actors could put on the cloak of the state as a socio-political resource or reject their association with the state in any particular instance was a question of strategy, pragmatism, context, and personal identity. Yet, on the whole, their liminal status and their ability to situate their personal agency in relation to the state is part of what characterized their technoscientific diplomacy and their development of the ASTER remote-sensing system and its international political economy. These liminal state actors asserted and ascribed "the state" and its politics, as well as "science" and scientific knowledge claims, in their negotiations with each other and in their collaboration more generally.[62]

---

[61] Evangelista (1989) confronted similar questions with respect to scientists involved in national security policymaking:

> Models that posit a sharp distinction between state apparatus and society (in order, for example, to evaluate state "autonomy") are not particularly useful for studying security policy. How, for example, should one characterize a scientist working for a private corporation that depends entirely on Pentagon contracts—as a societal or state actor? . . . The issue is perhaps more problematic for the Soviet Union, where one is hard pressed to identify any nonstate actors involved in the formulation of security policy.

As explained in note 56, Evangelista (1999) later addressed that last issue with the idea of "tacit transnationalism."

[62] Forman (1973) closely examines the rhetoric and ideology of scientific internationalism in Weimar Germany and suggests that:

> The scientist's *persona*, his image of himself, usually makes him most reluctant to admit that he has chosen between his science and his nation. He will often prefer to equate the interests of his nation with those of science (and, perhaps, humanity), most easily and most often when his nation is both a leading scientific power and a leading military power. At the same time, in order to maintain that *persona* intact, he must take care to avoid any direct repudiation of the ideology of scientific internationalism. And those who have the most to lose from a cancellation of international competition [in science] and recognition [from international science] are likely to take the greatest care (p. 156).

Enacting Technoscientific Diplomacy

The second quality of the U.S. and Japan teams' technoscientific diplomacy is that they "enacted" their technoscientific diplomacy as a matter of course in their negotiations and in their development of the ASTER remote-sensing system. The term "enact" has two meanings, "act out" (as in enacting a role in a play) and "establish" (as in enacting legislation). My use of the term "enact" in this dissertation generally means "act out." When I am using the term to mean also "establish," I make that additional meaning explicit. The U.S. and Japan teams enacted both the meaning and form of their technoscientific diplomacy.

They took on the roles and authorities not only of scientists and engineers but also those of diplomats conducting international relations. These roles and authorities initially were in principle delegated to them to some extent by their state sponsors. Just as they (re)articulated the state goals of their states in their use of them, they also as mediators between states developed the roles and authorities that their state sponsors had assigned to them. Team members, especially key members of each team such as the team leaders, often spoke for the "United States" and for "Japan" in their collaboration, reasoning through and asserting the goals, interests, and intentions of their states and reasoning through and ascribing goals, interests, and intentions to the state with which they were collaborating. When they reflected upon their roles in this

---

While Forman's attentiveness here and elsewhere to the tensions between universal ideals and human choice in the particular is unsurpassed (here in the instance of scientists trading on the ideal of scientific internationalism), my portrayal of scientists and engineers in this dissertation as liminal state actors suggests that the concepts and interests of "science" and the "nation" were less established at their boundaries than Forman's firm use of the terms connotes. In effect, while liminal state actors asserted and ascribed both "science" and the "nation" in their negotiations, it would be too presumptuous to argue with any definitiveness that either "science" or the "nation" was betrayed in those negotiations, owing to the changing boundaries of the "nation" and "science" and to the agency and authority of these liminal state actors. None of this is to say, however, that the rhetoric of scientific internationalism was not on occasion used instrumentally, as Forman (1973) explores.

intergovernmental collaboration, members of both the U.S. and Japan teams explicitly described themselves to me in interviews by using the word "geopolitician" as well as "geophysicist." This dissertation takes those descriptions seriously. When they were negotiating scientific and technological issues, they sometimes saw themselves as conducting U.S.-Japan relations and as establishing U.S.-Japan relations in the ASTER remote-sensing system and in the ASTER system's international political economy of scientific data.[63] They did indeed do those things, as this dissertation describes. For example, in their techno-political practices, they broke out of state-to-state bilateral diplomacy and established a normative transnational authority that they exercised in their configuration and governance of the ASTER remote-sensing system and its international political economy.

Synthesizing Power and Knowledge

In their enactment of technoscientific diplomacy, of their respective states, and of U.S.-Japan relations, the two teams synthesized power and knowledge, particularly state power and scientific knowledge, to develop the ASTER remote-sensing system. "Power" and "knowledge" are elevator words that can be especially slippery.[64] In this dissertation's account, "power" simply means the

---

[63] Scott (1998) interprets the (micro-)practices of the state and the (macro-)consequences of those practices for development projects. Mitchell (1991), Ferguson and Gupta (2002), and Jasanoff (2004) call for examining the practices of the state at its boundaries, such as in international spaces. Knorr-Cetina and Bruegger (2002) describe "global microstructures," but in that article at least, they do not study the development of global micro-structures. In the terminology of Knorr-Cetina and Bruegger, the ASTER remote-sensing system, with its space-based instrument and its trans-Pacific remote-sensing system, might be called a micro global-structure, and this dissertation does study its development.

[64] The meaning of "power" is problematized throughout the humanities and social sciences, and there is no need to review that expansive literature here (e.g., Lukes 1986). Lynch (1993: 76) critiques the contemporary literature in sociology of scientific knowledge, particularly the "strong program," for the breadth of that literature's use of the term "knowledge" and that use's lack of distinction with "belief" and "opinion." Jasanoff (2005), for example, explicitly and implicitly uses the term "knowledge" to refer to contested as well as uncontested scientific and technical claims that are asserted by scientists in scientific publications, by policymakers in administrative

capacity to influence. The dissertation usually does not try to describe power itself but rather interprets team members' explicit assertions and ascriptions of power, especially state power. In a few places, however, the dissertation does infer team members' assumptions about power in specific contexts, particularly power that arises from an asymmetry in interdependencies between states' technological capacities.[65]

Similarly, the dissertation generally describes contested and uncontested scientific and technical claims that are explicitly asserted and ascribed by team members, rather than making assumptions about "scientific knowledge" and then explaining what team members did or did not do because of that assumed "knowledge." I use the term "knowledge-making" to refer to the acceptance and rejection of scientific and technical claims and to the creation of scientific data that were intended to be taken-for-granted as scientific knowledge.

By investigating the enactment of technoscientific diplomacy and by tracing the intertwining of assertions and ascriptions of both state power and scientific claims, the dissertation provides an original and compelling account of how the U.S. and Japan teams worked together in an intergovernmental collaboration to build a U.S.-Japan remote-sensing system. This account offers a better understanding of the "how" of intergovernmental collaboration than either the epistemic community approach or Latourian actor-network theory. Because the dissertation examines closely the articulation of state goals and the enactment of state power within the context of

---

procedures, by legislators in law, and by activists in public campaigns. Jasanoff (2005) also uses the word to encompass scientific and technical understandings held by publics. While that breadth in the use of the term can be justified in an interpretative exploration, because the empirical scope of my investigation is not as ambitious as, for example, Jasanoff (2005), this dissertation in its empirical chapters generally tries to be more specific. Literature in the field of International Relations has distinguished among individual beliefs (e.g., Goldstein and Keohane 1993), consensual, intersubjective belief (e.g., Haas 1992), and intersubjective knowledge (e.g., Adler 2005a). Katzenstein et al. (1998) point to explorations of "common knowledge" as a site for productive exchange between rationalist and constructivist traditions in International Relations.

[65] Keohane and Nye (1977) is a seminal work for this understanding of power.

knowledge-making and technology-building, it is not required to stipulate—like the epistemic communities approach does—shared “principled” and “causal” beliefs among epistemic communities in order to understand how epistemic communities do and do not help effect international coordination. While investigating technoscientific diplomacy is more demanding and less straightforward in terms of the empirical research and description that is required, to justify the degree of solidarity and intersubjectivity assumed in the epistemic communities approach would also require extensive research and description for it to be compelling to audiences who are aware of the possibilities for contention in scientific and technological development.

Describing the synthesis of power and knowledge as technoscientific diplomacy also fills some gaps in the literature of Science and Technology Studies. It explores state goals, the state, and international politics as socio-political resources that are used in knowledge-making and technology-building in the practice of intergovernmental collaboration without being narrowly realist about state goals, the state, state power, or the creation of knowledge. This exploration, in turn, opens up the possibility of connecting power exercised by “the state” in knowledge-making, which is a well-explored phenomenon in Science and Technology Studies, to power exercised by “other” states through international politics, which is a phenomenon that has only recently come to be investigated in the Science and Technology Studies literature. None of these themes have been explored to date by the “dramaturgical” or “performance” literature in Science and Technology Studies, which has loosely informed my account of the enactment of technoscientific diplomacy.[66] In addition,

[66] Hilgartner (2000) examined the production of scientific credibility at the U.S. National Academy of Sciences through a well-developed dramaturgical framework that draws on the seminal work of Goffman (e.g., 1959, 1974). The metaphors of drama and performance are not as well-developed in my account of the enactment of technoscientific diplomacy as they are, for example, in Hilgartner (2000). Yet, because my account integrates other accounts from interviews and from the “audience” and because my account does not make assumptions about the “public identity” of characters, its eclecticism is better able to portray the relational dynamics of

unlike Latourian actor-network theory, examining the articulation and the intertwining of both state power and scientific claims as technoscientific diplomacy provides interpretative space to develop the dynamics of norms in international politics and knowledge-making without cynically assuming those norms to be only instrumental rhetoric.[67] In technoscientific diplomacy, rhetoric becomes constitutive of politics, especially through its role in persuasion, rather than only a tool of politics.

By offering a better account of the "how" than other well-known, potential approaches for examining intergovernmental collaboration in science and technology, this dissertation argues that it can develop an explanatory sketch for the two teams' technoscientific diplomacy which better explains the "why" questions that are often about "who got what" issues. I illustrate the value-added of this explanatory sketch by answering "why" questions throughout the empirical chapters of the dissertation. In that way, the dissertation incorporates both social science problem-solving (i.e., puzzle-solving) and the emphasis in Science and Technology Studies on understanding and describing knowledge-making and technology-building as those activities are conducted "in the field" (for this dissertation, the "field" is in part an international

---

intergovernmental collaboration, including the enactment of power. Other work in Science and Technology Studies which stress the performative aspects of producing scientific and technical knowledge include Pickering (1995), Law and Singleton (2000), Law (2002), Mol (2002), and MacKenzie (2003). Butler (1990) is canonical to the performance literature across the humanities and social sciences. Because I do not claim to be contributing to theories of dramaturgy and performativity, I do not review that literature in detail in the text.

[67] The literature in Science and Technology Studies has long emphasized normative assertions as strategic posturing (e.g., Gilbert and Mulkay (1984), Collins (1992 [1985]). In contrast, the literature on norms in International Relations, after having illustrated the significance of "idealist" norms in international relations, has recently worked to show how norms can be "bad" as well as "good," strategic as well as principled. For instance, Finnemore and Sikkink (1998) review theories about norms and norm construction in the International Relations literature. They explore how norm construction can be strategic as well as institutional or principled, and they highlight how theories of norms and norm construction do not need to side completely with either "rationalists" or "constructivists." Chapter six in Katzenstein (1996) explicates contrasting norms in U.S.-Japan relations.

field).[68] While the dissertation's account requires the reader to engage with the technical as well as the political details of the U.S. and Japan teams' intergovernmental collaboration, ultimately I hope readers will find this account of technoscientific diplomacy to be compelling because through the account readers are able to engage with the two teams' mundane work and the technical and political challenges that they faced and overcame.[69]

Outline of the Dissertation

The chapters ahead are as follows. Chapter two employs a comparative analysis of U.S.–Japan relations in defense and space to argue that an interpretative explanation of technoscience in international politics is needed. Science and technology—and scientific and technological collaboration—have been central to U.S.–Japan relations since the end of World War II. Yet, existing explanatory approaches in the fields of Science and Technology Studies and International Relations are unable to explain why U.S. and Japan scientists, engineers, and bureaucrats have negotiated crucial aspects of U.S.–Japan relations—such as reciprocity in knowledge sharing, equity in financial arrangements, and techno-political interdependencies—in

---

[68] Katzenstein presented an argument for a problem-solving focus in social science, even—and perhaps especially—if it involved eclectically hybridizing various theoretical approaches, in Kohli et al. (1995: 10) as well as with Sil in Katzenstein and Sil (2004). Dear (2001: 130) characterized science studies as being driven by describing "science 'in the field,' as it were," what he called "epistemography." Lynch (1993: 299-308) outlined a program for research in Science and Technology Studies for investigating "epistopics"—"discursive themes that so often come up in discussions of scientific and practical reasoning: observation, description, replication, measurement, rationality, representation, and explanation" (p. 299). While "epistopics" pervade this dissertation, the dissertation neither investigates them head-on nor fully adopts Lynch's admittedly appealing "post-analytic" orientation, owing to the dissertation's central burden of accounting for the development of the ASTER U.S.-Japan remote-sensing system.

[69] Readers can then understand acts of scientific and technological development. Lynch (1993: 287-299) discusses the value of demonstrating the transformation that is at the core of (re)discovery. Livingston's studies of geometrical proofs are exemplars (1999, 2006).

the way that they have. Explanations that are highly-structured are especially insufficient.

Drawing upon ethnographic observation, numerous interviews, and extensive collection of unpublished and unarchived documents (including letters, faxes, and e-mails), and using the Japanese language as well as English, chapters three through six empirically investigate the U.S.-Japan collaboration that developed and operated the ASTER space-based remote-sensing instrument, its ground data and information system, and its international political economy of scientific data. Chapter three describes in detail how the practices of the two geologic remote-sensing communities in the United States and Japan differed from each other. They were different communities of practice. The key participants in the U.S.-Japan collaboration had conducted their scientific research and engineering within their respective communities of practice for at least several years before their collaboration, in some cases for decades before. Chapter three also contextualizes the development of geologic remote-sensing instrumentation in the 1980s, especially as that development related to each state's goals.

Chapter four describes the emergence of U.S.-Japan bilateral diplomacy and how NASA and MITI positioned members of the U.S. and Japan teams as liminal state actors within that diplomacy. In addition, the chapter analyzes how—as liminal state actors—members of the U.S. and Japan teams tried, before the formal establishment of their collaboration, to negotiate through bilateral diplomacy a remote-sensing instrument that they could share separately as a politically-neutral U.S.-Japan "boundary object." This analysis accounts in particular for the design specifications of an instrument sensor in terms of technoscientific diplomacy, and it highlights how the explanatory sketches of epistemic communities and Latourian actor-network theory are inadequate. While this effort to share separately proved unsuccessful, the two teams nevertheless pushed forward in their collaboration.

Chapter five then compares two negotiations over ASTER's design, both of which were conducted in the same time period. Bringing together the concern in Science and Technology Studies for describing ways of knowing, and a social science emphasis on puzzle-solving, chapter five explains why, in one negotiation, the teams broke out of their bilateral diplomacy and subsequently began to forge a new international techno-political order and yet why, in another negotiation, they did not do so, even though the latter negotiation—like the former—was conducted after the formal establishment of the institutional workings of the ASTER collaboration.

Chapter six describes how the U.S. and Japan scientists' and engineers' technoscientific diplomacy constructed an international techno-political order, and more specifically an international political economy, in and around the ASTER ground data and information system. The chapter elucidates the workings of this international order by comparatively examining the politics of how different calculative routines, algorithms, and procedures became designed and embedded in the data and information system and were maintained as an international order. Chapter six also illustrates the use of normative notions of U.S.-Japan relations and how common political and normative commitments that were forged through the collaborative process held sway when managing the international political economy of the ASTER system, including its pricing arrangements. The ASTER remote-sensing system itself gradually became a stable political resource for the two teams, to an extent that fostered transnational authority and community. The concluding chapter articulates this dissertation's explanatory sketch of technoscientific diplomacy and suggests other activities that are between states and transnational communities which might be illuminated by this sketch's rethinking of science, technology, and international affairs.

CHAPTER TWO

SCIENCE AND TECHNOLOGY IN U.S.-JAPAN RELATIONS

Scholars and policymakers in the United States and in Japan have long recognized science and technology as an important issue in U.S.-Japan relations.[1] Technoscience is not just an important issue, however. It is a fundamental thread that has tied together the United States and Japan since World War II. Scientists, engineers, and policymakers have practiced and materialized U.S.-Japan relations through technoscience and have thus made U.S.-Japan relations a reality through technoscience. Throughout the last sixty years, conflict, cooperation, and competition between the United States and Japan in technoscientific enterprises have shaped what each state has wanted to achieve—politically, economically, and morally, as well as scientifically and technologically—in both the domestic and international arenas. The centrality of science and technology for these two states and for their bilateral relationship is not without cause. Perhaps more than in any other bilateral relationship, the governments of the United States and Japan have, as a matter of policy and practice, used technoscience to underpin and promote their inter-state relationship, even if the two governments have periodically contested since the end of World War II the qualities desired for that bilateral relationship.

This chapter argues for the need of an interpretative explanation of science and technology in international affairs by describing how the U.S. and Japan governments have used technoscience to build different U.S.-Japan relationships. Although it is common to talk about "the" U.S.-Japan relationship, the U.S.-Japan relationship has not

[1] For general discussions of science and technology in the U.S.-Japan relationship, see Bloom (1984), National Research Council (1997), Matsumura (1999), Chinworth (1999), and Vogel and Zysman (2002).

been monolithic.[2] During the same period of time, the governments of the United States and Japan have realized different versions of "the" U.S.-Japan relationship. These versions have neither been harmonized nor unified under "top down" policies. This chapter substantiates the disunity of the U.S.-Japan relationship post-1980 and the centrality of technoscience within that disunity by analyzing policies and technologies of U.S.-Japan collaborations in defense and space and by illustrating ways in which collaborations in defense and in space have differently materialized, operationalized, and practiced the U.S.-Japan relationship. I argue that the existence of different versions of U.S.-Japan relations, specifically in defense and in space, suggests that scholars need to re-think how to account for the politics of U.S.-Japan intergovernmental collaboration in technoscience.

The Centrality of Technoscience in U.S.-Japan Relations

The U.S.-Japan relationship was indelibly marked by the United States' atomic bombings of Hiroshima and Nagasaki.[3] The bombings, or more accurately, reports of the bombings, testified to the potential power of science and technology to governments around the world, foremost among them the Government of Japan.[4] In the shadow of the bombings, Japanese elites attributed their defeat not to the United States' military or

[2] For instance, Katzenstein (1996) has persuasively argued that the Japanese state has demonstrated policy "flexibility" in its military relations with the United States and "rigidity" in its economic relations with the United States (e.g., p. 15). I have argued in Plafcan (1999) that some technological issues have problematized policymaking boundaries between economic and military security. The governments of the United States and Japan have had great difficulty at times in distinguishing between economic and military security in technology policy. Stone (1999) has made a similar point regarding the boundary between trade and security in the U.S.-Japan relationship.
[3] The edited volume Hein and Seldon (1997) includes studies from a variety of perspectives which address this point. See also Hogan (1996).
[4] How and what the citizens of the United States and Japan learned about the atomic bombings is far from straightforward, especially given prolonged censorship and early "official" histories in both the United States and Japan. See Dower (1996b), Hogan (1996), and in particular within the latter volume, Dower (1996a).

industrial might, and not to the United States' strategy or determination, but to their enemy's science. Throughout Japan and to overseas troops, Emperor Hirohito broadcast via radio on August 15, 1945 that Japan was compelled to surrender under the threat of a "new and most cruel bomb."[5] A speech by Prime Minister Suzuki Kantarō followed Hirohito's address, and in it Suzuki urged the Japanese people to "strive for the progress of science and technology, which were our greatest deficiency in this war."[6]

Turning to science and technology for strength was certainly not new for Japan.[7] Nevertheless, during the chaotic and desperate times of the United States' seven-year occupation of Japan, science and technology in Japan took on additional meanings, meanings that had inflections of political and moral renewal. In the early years of the occupation, prominent speeches and government reports "embraced defeat" and pronounced that prerequisites for democratization and a free-thinking citizenry were technological advancement and a scientific spirit among the populous.[8] In the name of these ideals, Japan embarked on another era of what the Government of Japan called "building a nation through science and technology" (*kagaku gijutsu rikkoku*).[9] And over the next few decades, Japan indeed rebuilt itself spectacularly.[10]

---

[5] Butow (1954: 247-248). This taped radio address was the first radio address of Emperor Hirohito, and as such it was the first time that the people of Japan heard his voice. The address had been taped the previous day.

[6] Nihon kagakushi gakkai (1964: 44). I originally learned of this source from Morris-Suzuki (1994: 161), and I have used her translation. The speech was reported in *Asahi Shimbun*, 17 August 1945.

[7] Samuels (1994).

[8] While idealistic, these invocations of science and technology were also in some ways pragmatic and self-serving, implicitly and often explicitly blaming the war on "irrationality" and providing justification for society-wide "rationalization." See especially Dower (1999: 494-496) and the *Asahi Shimbun*, 17-20 August 1945. For examples of government commissions on such topics as economic reconstruction and education which promoted science and democracy as mutually interdependent, see Morris-Suzuki (1994: 161-164) and Hata (2001: 206-209). For a similar theme in American social and political thought about science, see Merton (1973[1942]) and Ezrahi (1990). The quote is adapted from Dower's title (1999).

[9] For more on *kagaku gijutsu rikkoku*, see Samuels (1994) and Nakayama (1995).

[10] Johnson (1982) and chapter seven of Morris-Suzuki (1994).

Japan did so with the United States close by and with the United States' technoscience readily available for use. The two states' most intimate of times was, unsurprisingly, the occupation. With respect to science and technology, the boundary between "domestic policy" and "foreign policy" was porous; the two states were entangled in each other's domestic policies, even if far from equally so. During the occupation, the General Headquarters of the Supreme Commander of the Allied Powers (SCAP) was the ultimate, but indirect, governing authority. SCAP supported scientists and engineers in Japan who would fashion a Japanese technoscientific infrastructure that would ameliorate the anxieties of U.S. policymakers. These anxieties were about the United States' national security and about the proper relationship between science and politics; they derived from circumstances within the United States as well as from those within and surrounding Japan. When New Dealers held key positions in the sections of SCAP during the first few years of the occupation, they worked to achieve what they could not in the United States: first, a complete rejection of the "garrison state," a state in which the military dominated politics through its appropriation of science and technology, and second, the establishment of governance structures for science and technology that fostered democracy and economic revitalization.[11] In these years, the projects of SCAP's "Economic and Scientific Section"—its name betrays its New Deal thrust—were eagerly supported by most scientists and engineers in Japan, above all by members of the thriving Marxist associations of scientists and engineers.[12]

But as the Cold War intensified in 1948, and with the arrival of the fiscally-conservative Detroit-banker Joseph Dodge as the finance advisor to SCAP in

---

[11] With regard to the circumstances in the United States, here I am thinking in particular of the plight of Senator Harley Kilgore's proposal for a National Science Foundation vis-à-vis the National Research Foundation which was later proposed by Vannevar Bush (1960 [1945]). See Kevles (1977) and Dennis (2004: 228).

[12] Nakayama (2001a: 41); Dees (1997), especially chapter eight; and Yoshikawa and Kauffman (1994).

early 1949, SCAP proceeded to quickly "reverse course" away from more comprehensive reform and moved toward conservative policies and practices.[13] Balanced budgets and increases in industrial production became immediate goals. The Scientific and Technical Division of SCAP was reorganized to assist more with the development of industry than with academic research.[14] The United States did not want Japan to be dependent upon the United States' largesse, and the United States needed Japan in the Cold War; more specifically, the United States needed Japan's geography, potential industrial capacity, and political support. The U.S. Government acquired these resources and solidified its long-term inter-state relationship with Japan through scientific and technological ties. Since the Government of Japan could also put this science and technology to other uses, to develop Japan's economy and to legitimize its rule, the Government of Japan gladly accommodated.[15]

During the Korean War, through technical assistance and technology licensing, Japan arguably became the preeminent forward-deployed arsenal of the United States. For the four years after the war's onset in June 1950, the United States infused about $2.4 billion into Japan's economy through special procurement (or roughly, $19 billion in 2005 dollars).[16] Between 1951 and 1953, special procurement for the war amounted to 60% of Japan's exports.[17] The United States provided military, economic, and technical aid to Japan under the United States' 1951 Mutual Security Act and the U.S.-Japan 1954 Mutual Defense Assistance Agreement. In particular, the United States

[13] This summary presents the "reverse course" through the lens of U.S.-Japan science and technology relations, but changes in SCAP's policies were felt throughout Japan's politics and society. See Gordon (1993), Schaller (1997), Dower (1999), Forsberg (2000), and Gordon (2003: 234-240).
[14] Dees (1997: 223). Academic organizations that SCAP's Scientific and Technical Division helped to promote, such as the Science Council of Japan, did continue on, but after the "reverse course" more technocratic organizations came to dominate Japan's science and technology policy. See Nakayama (2001b).
[15] Green (1995: 26).
[16] Johnson (1982: 228).
[17] Gordon (2003: 241).

encouraged Japan's companies to take advantage of technical assistance and buy technology licenses from U.S. firms in order to increase Japan's industrial capacity. Soon, hundreds of Japan's firms were manufacturing war materiel, including ordinance and vehicles, and they were also repairing aircraft and ships.[18] From 1950 to 1955, Japan's total industrial production almost doubled.[19] In the words of historian Laura Hein, "the Korean War, or more accurately, American use of the war, had an enormous impact on the Japanese economy in practical terms. It is hard to overestimate the importance of that timely influx of money, technology, and willing customers for Japanese industry."[20] Through U.S. economic and technical assistance, the war "permanently linked Japanese economic development to United States military priories in Asia."[21] This connection extended through the Vietnam War as well.

From the signing of the Mutual Defense Assistance Agreement in 1954 to the mid 1970s, the U.S.-Japan technoscientific relationship can be characterized in broad terms by four inter-state technoscientific practices and two political understandings that helped to sustain those practices. First, Japan willingly joined numerous intergovernmental science collaborations with the United States government (these collaborations also involved academics, often working as government advisors or consultants). Yet until the 1980s, only the United States crafted the initial offers to collaborate and the initial scope of collaboration.[22] Second, through the practice of licensing and coproduction administered by the U.S. Department of Defense's Defense Security Assistance Agency, the United States essentially paid U.S. contractors to develop the technological capability and industrial capacity of Japan's major

[18] Samuels (1994: 132-153).
[19] Johnson (1982: 4-5).
[20] Hein (1990: 217). I became aware of this statement from Samuels (1994: 133), who also quotes Hein.
[21] Hein (1990: 217).
[22] Bloom (1984: 108) and Uyehara (2000: 25).

corporations and their subcontractors.[23] A 1970 study of U.S. assistance to Japan's aerospace industry in the 1950s and 1960s notes that:

> In general [through coproduction], the Japanese received all product designs and specifications and all process specifications. In particular, they had the benefit in every case either of the tooling or of the tool design information used by the developer in his production activities. They also received a great deal of planning information. And since important data exist in the notes and black books of foremen and other production line personnel, these too were collected and made a part of the data package.[24]

One manager summed it up: "We were paid to put them in business, and we gave them everything we had."[25] Third, as Richard Samuels has compellingly argued, the Government of Japan and its contractors indigenized, diffused, and nurtured this technology throughout their industrial base with the United States' encouragement.[26] Fourth, industry-to-industry collaboration was integral to sustaining this patron-client relationship between the United States and Japan, and as paid agents of the state, industries profited from it.[27]

These inter-state practices did not exist in a political vacuum. The U.S. Government expected the Government of Japan to follow its lead in the Cold War in East Asia, and it regarded Japan's economic and military development as necessary to this endeavor. Japan viewed the "bilateral partnership" (during this time the term "alliance" was controversial in Japan) as a means to economic and technological growth, as well as a means to domestic stability and military security.[28]

---

[23] Licensing and coproduction are different. Licensing refers to a commercial agreement to rent or purchase intellectual property which is approved by a government export license. Coproduction is a collaborative activity that is authorized and supported by a government-to-government memorandum of understanding, and this agreement often includes specific licenses, among other arrangements.
[24] Hall and Johnson (1970: 317).
[25] Ibid.
[26] Samuels (1994: 42-56).
[27] Chinworth (1992).
[28] I am referring here to what Pyle calls the "Yoshida doctrine" (Pyle 1996).

By the mid 1980s, however, the United States' and Japan's relationship in science and technology was unraveling, and this unraveling was jeopardizing U.S.-Japan relations in general. Fueled in part by a series of acrimonious trade conflicts of increasing intensity,[29] governments, firms, and citizenry of both the United States and Japan were thoroughly interrogating the political assumptions and the inter-state technoscientific practices that undergirded the U.S.-Japan alliance. As a consequence of being confronted with Japan's spectacular economic growth and technological accomplishment as evidenced in reports and by commercial products,[30] the United States government questioned what it had previously presumed to be the United States' dominance in science and technology.[31] For the United States, defense technology assistance to Japan and intergovernmental technoscience collaborations could no longer be justified primarily as means for maintaining U.S.-Japan relations, despite the Reagan administration's defense build-up and its "new Cold War" with the Soviet Union.[32] In the United States—but also in Japan, even if to a lesser extent—science and technology policy and industrial policy were thrown much further into the foreground of national

---

[29] By 1985, trade conflicts had already arisen over agricultural products, steel, automobiles, machine tools, and consumer electronics, among other goods. Their acrimonious character was foreshadowed by the "Textile Wrangle" of 1969-1971. See Buckley (1992: 122-130) and Destler, Fukui, and Sato (1979). Numerous and more intense trade disputes were yet to come in the late 1980s and early 1990s. Prestowitz (1989) presents a forceful account of these times.

[30] Perhaps the most prominent public testimonial from an academic of Japan's success was Vogel (1979). Impressive products that testified to Japan's technological prowess by the mid 1980s included photographic equipment, audio and television equipment (e.g., VCRs), and microelectronics (e.g., DRAMS).

[31] In the U.S. Government, this questioning had started in the late 1970s in the context of U.S.-Japan "burden sharing" in defense technology. But for the United States government, these questions did not transform into full-blown anxieties until the late 1980s with the controversy over the FS-X fighter. See Rubinstein (1987) and Lorell (1996). Lorell (1996) is an excellent and comprehensive study of the history of the U.S.-Japan FS-X fighter collaboration from the perspective of policy analysis.

[32] For a general overview of the United States' concerns during this time regarding defense technological assistance, see U.S. Congress, Office of Technology Assessment (1990). Regarding U.S.-Japan international collaboration in technoscience during this time, see Bloom (1984) and NRC (1991).

politics and foreign policy, especially in relations between the two states.[33] The decade beginning in the early 1980s came to be characterized by what was then called "techno-nationalism"[34]: technoscience, particularly "high-technology" (as the "advanced" technology of times was called) became a critical measuring stick in U.S.-Japan competition, and—in an explicit bow to mercantilism by the United States—even the United States' technoscience outside of the military sphere became something that needed to be cautiously guided through industrial policies and held on to with managed trade policies.[35] High technology became high politics.

---

[33] See, for example, Zysman and Tyson (1983), Johnson (1984), Krugman (1986), Ergas (1987), and U.S. Congress, Office of Technology Assessment (1991).

[34] Many commentators in the United States and some also in Japan have used the term "techno-nationalism" to describe this era of U.S.-Japan relations (for the case of Japan, I am referring to the term "techno-nationalism" as it is written in *katakana*, a writing system that is a part of the Japanese language and is frequently used for phonetically importing terms from foreign languages. Here, I am not referring to *gijutsu rikkoku.* See note 9). The term "techno-nationalism" has been used in different ways. For example, Robert Reich—who would later become Secretary of Labor in the first Clinton administration—used the term in a well-known article in *The Atlantic Monthly* (1987). That article is sometimes said to have coined the term. Richard Samuels used the term much differently in a prominent academic study (1994). Samuels used "techno-nationalism" to describe a central ideology in Japan which gives primacy to technological development as an end to itself (thus, he aligned "techno-nationalism" with *gijutsu rikkoku*). Samuels regarded techno-nationalism as a successful strategy for Japan and as a strategy from which the United States could learn. For Reich, unlike for Samuels, techno-nationalism was a distortion. In Reich's view, states can not hold on to technology in what he regarded as a tightly interdependent world, nor do states leave political and social fingerprints on technology when they develop it: "The notion of 'American' technology has become a meaningless concept" (Reich 1987: 64). Techno-nationalism "misconstrues the problem and thus advances the wrong solutions," and it interferes with recognizing what Reich called "techno-globalism" (63). For more about techno-nationalism in U.S.-Japan relations, see Moritani (1981), Johnstone (1988), Tyson (1992), Ostry (1995), Simon (1997), and Yamada (2001).

[35] U.S. Congress, Office of Technology Assessment (1991); Committee on Science, Engineering, and Public Policy (1992); Harris and Moore (1992). In the U.S., the "managed trade" rhetoric and agenda moved most prominently into the center stage of policymaking with President Clinton's Democratic administration in 1993, but academic and policy debate about "managed trade" and "strategic trade" was very prominent in the 1980s as well, fueled by U.S.-Japan trade disputes in the 1970s and 1980s (see, e.g., Krugman 1986). In fact, "managed trade" policies were first pressed and implemented by the Reagan administration, most famously (or most notoriously) in the 1986 U.S.-Japan Semiconductor Agreement, in which Japan agreed to limit the number of DRAM semiconductor chips it exported to the United States and agreed to try to ensure U.S. semiconductor manufacturers a certain share of the semiconductor market in Japan. For one analysis of that managed trade agreement, see Tyson (1992).

The Building of Two U.S.-Japan Relationships

The United States' and Japan's techno-nationalism and each government's questioning of its science and technology relationship with the other were largely worked out and given substance through specific contestations over technoscientific enterprises and collaborations. That is, while the above-mentioned political assumptions and inter-state technoscientific practices from the 1950s through the 1970s were, by the late 1980s, generally open for questioning in most of the United States' and Japan's bureaucracies and legislatures, how these questions were framed, what their stakes were, and how resolutions were negotiated were contingent upon the circumstances of particular technoscientific enterprises and intergovernmental collaborations. As will be illustrated below, these circumstances included the political saliency of the technoscience for various governmental institutions,[36] the goals of the institutions promoting a given enterprise or collaboration, the technologies and technoscientific practices involved in the enterprise, and the previous experiences of the

[36] Tables in government reports that portrayed U.S. and Japan capabilities in "high-technology" and in "critical technologies" as either leading or lagging the other were straightforward rhetorical devices that cast a competitive, relative-gains edge to science and technology policies. During this time, both the governments of the United States and Japan widely circulated these tables to inform policy. For tables in the U.S. policy process, see for instance the multiple sets of critical technology reports, including the U.S. Department of Commerce's *Emerging Technologies* series (e.g., 1987), the U.S. Department of Defense's *Critical Technologies Plan* series (e.g., 1989), and, in the Executive Branch, the Office of Science and Technology Policy's *National Critical Technologies Report* series (e.g., National Critical Technologies Panel 1991), all of which were commissioned by the U.S. Congress, and all of which listed different technologies as "critical." Yet, what counted as "critical" depended upon the government entity that authored the list as well as the intended audience. For tables produced by the Government of Japan, see the Science and Technology Agency's white paper series (e.g., kagaku gijutsu chō 1987). Technologies usually became "critical" (or not) and "lagging" (or leading) on the basis of results of surveys of specific business or technical communities. These surveys represented judgments that were informed by a wide variety of factors and usually did not include direct comparisons of technologies at the lab bench. Even for the cases in which formal technology assessment teams were organized for the United States (e.g., to evaluate Japan's gallium arsenide microwave integrated circuits), it is likely that assessors were only privy to glimpses of technologies. See, for example, Defense Science Board (1984) and the account of Lorell (1996: 44).

institutions with the enterprise (i.e., the enterprise's "legacy"). There was no high-level, overarching, and unifying policy framework for all U.S.-Japan intergovernmental collaboration in science and technology. Conventional explanations of science and technology in international affairs—such as the sharing of science and technology according to balance-of-power alliance politics (e.g., Green 1995; Synder 1997), economic competition in science and technology according to industrial sectors (e.g., Destler, Fukui, and Sato 1979), the expansion of science and technology as a Hobbesian imperial enterprise (e.g., Latour 1987), and epistemic communities as sources of policy coordination (e.g., Haas 1992)—do not account for the policymaking, policies, and practices of U.S.-Japan technoscientific relations. For example, although state defense and space bureaucracies are often supported by overlapping industrial sectors that develop broadly similar aerospace, electronic, and communications systems, as has been the case both for the United States and for Japan, U.S.-Japan technoscientific collaborations in defense and in space have differed markedly in their history throughout the 1980s and 1990s.[37] During the same period of time, between the same two states, and supported by significantly overlapping industrial sectors, intergovernmental technoscientific collaborations realized U.S.-Japan relations differently. Collaborations in defense and space had different ways of negotiating: 1) common concerns about U.S.-Japan reciprocity in the sharing of knowledge and know-how, 2) the distribution of the project's financial costs and benefits between the governments of the United States and Japan, and 3) U.S.-Japan interdependency in political and technological arrangements (see table 2.1 below). The next two sub-sections substantiate these differences, presenting first an analysis of U.S.-Japan

[37] The industrial sectors of space and defense are similar and significantly overlapping in contrast to their lack of similarities and ties with, for example, the consumer electronics industry or the pharmaceutical industry (Bromberg 1999; Reppy 2000). For Japan's heavy industry firms, however, space and defense business has been a much smaller percentage of those firms' overall business than it has been for the less diversified defense and space firms in the United States (Samuels 1994).

technoscientific collaboration in defense and then an analysis of U.S.-Japan technoscientific collaboration in space.

Table 2.1: Dimensions for Comparing Collaborative Practices

1. U.S.-Japan reciprocity in knowledge and know-how
2. Distribution of financial costs and benefits
3. Techno-political interdependency

U.S.-Japan Collaboration in Defense:
Economically Explicit, Component-by-Component Negotiations

From about 1980 until 2000, U.S.-Japan collaborations in defense technology were plagued with disagreements over reciprocity and how to judge it. For defense collaborations, reciprocity was for the most part focused on “critical” technologies, was conceptualized as “technology flowback,” and was largely adjudicated on a case-by-case basis, rather than settled by policy memoranda written by each state’s foreign affairs or defense bureaucracies. In 1979, then U.S. Under Secretary of Defense for Research and Engineering, William Perry, began to implement recommendations from the Department of Defense’s first report on "military critical technologies."[38] The report called for the promotion of 15 critical technologies, and in many of those technologies, Japan was perceived to be a rival. In the face of what the Government of the United States considered to be a history of a one-way flow of technology from the

[38] This report was issued by the Department of Defense's advisory body, the Defense Science Board. See Lorell (1996: 14). While the discussion here on U.S.-Japan defense collaboration is informed by many other studies, such as Chinworth (1992), Noble (1992), Green (1995), Katzenstein (1996), and Matsumura (1999), many of the specific factual details here are drawn from Lorell (1996).

United States to Japan, the U.S. Government started to judge "reciprocity" in the alliance in terms of "technology flowback."[39] In order to facilitate flowback in defense technological collaboration and the exchange of technology and ideas related to defense research and development, the U.S. Department of Defense and the Japan Defense Agency established the Systems and Technology Forum in 1980. But that forum was soon crippled: Japan expanded its prohibition on the export of defense technology to include so-called "dual-use" technology in addition to weapons and equipment. The United States and Japan then wrestled with issues of categorization and debated over what would count as "dual-use" and "defense" technology.[40]

Despite these setbacks for managing U.S.-Japan technology exchange, "technology flowback" became only further entrenched as a U.S. Government standard for reciprocity in U.S.-Japan relations. In 1982, the U.S. Congress inquired into the details of U.S.-Japan technology issues, particularly reciprocity. Its General Accounting Office issued a widely-read (and widely cited) report that drew attention to what it called a "one-way street" of technology transfer and to the commercial implications for the Japan's aerospace industry of the technology transferred in the United States' F-15 fighter co-production program with Japan.[41] The Department of Defense also commissioned a study in 1983 on defense-related technology collaboration. That study found that the "prerequisite for continued transfer of U.S. advanced defense

---

[39] Use of the terms "reciprocity" and "technology flowback" was often politically-charged, value-laden, and contentious. How these concepts were defined and measured was quite contingent on the circumstances of their use. As far as this author is aware, no single metric was ever adopted to substantiate when "reciprocity" and "technology flowback" were achieved. For prominent and public charges of a one-way flow in technology, see U.S. General Accounting Office (1982).

[40] Lorell (1996: 9-31). The Japan Defense Agency and its contractors also lost interest in the Systems and Technology Forum as a mode of exchange because the U.S. Department of Defense was persistent in its refusal to license sensitive technologies to firms in Japan for Japan's co-production of the United States' F-15 fighter aircraft. See Green (1990: 40-41) and Lorell (1996: 78).

[41] U.S. General Accounting Office (1982: ii).

technologies [to Japan] will generally be reciprocal transfer of Japan's dual-use and military technologies."[42]

Only after high-level consultations were conducted under the pressure of President Reagan's impending state visit to Japan did the Japan Defense Agency adjust in 1983 its stance with regard to technology transfer to the United States. Japan exempted the United States from its arms export ban in an *Exchange of Notes on the Transfer of Japanese Military Technology*, noting that Japan "welcomes the transfer to the United States of America defense-related technologies." The *Exchange of Notes* established the Joint Military Technology Commission composed of government representatives to facilitate paperwork for technology transfer (representatives were from Japan's Ministry of International Trade and Industry, the Japan Defense Agency, the U.S. Mutual Defense Assistance Office, etc.).[43]

Even with this organizational apparatus, the Joint Military Technology Commission could not resolve which technologies were defense-related, civilian, dual-use, public, or private; all the problems of categorization from the Systems and Technology Forum of 1980 remained. These categories had implications. Technologies classified as "private" could be owned by Japan's firms and thus would require further negotiation, as well as funds, for licensing to the United States. This "private" category could cover "dual-use" technology too. What remained as "public" and "defense-related" technologies in the interpretation of the Government of Japan were only highly specific arms that excluded any potential civilian application, such as ammunition, explosives, and weapons. These arms were not necessarily the technologies the United States government or its contractors wanted. Prior agreements in the form of diplomatic notes, memoranda-of-understanding, or contracts could not

---

[42] Defense Science Board (1984: 5).
[43] Lorell (1996: 23-26).

delineate interpretations of these technologies in practice. For the U.S. Government, technological "reciprocity" remained elusive, and the Government of Japan cast the United States' efforts to achieve it as unwarranted foreign pressure.[44]

As the United States' and Japan's first joint effort to develop and produce a weapon system, their collaboration on the FS-X / F-2 multi-role support fighter forced questions about reciprocity to be addressed directly and answered concretely. The FS-X / F-2 fighter is undoubtedly the most intense and notorious example of conflict in the U.S.-Japan science and technology relationship since World War II. The history of the controversy-ridden, with all its complex twists and turns over the course of a decade, will no be recounted here. A brief overview suffices for our purposes.[45] The collaboration illustrates how questions of reciprocity in the U.S.-Japan relationship were adjudicated and settled in the context of technical practice, on a component-by-component basis in the thick of an on-going collaboration, rather than through the standard diplomatic channels that had proved so problematic in the past.

The Government of Japan did not intend for the FS-X to be a U.S.-Japan joint collaboration. By the end of 1985, after much disagreement among its ministries and agencies, the Government of Japan had reached an unofficial interagency consensus that Japan's next fighter would be developed and manufactured domestically, instead of

---

[44] For the U.S. Government, U.S.-Japan "reciprocity" in science was also a theme in the 1980s. For example, the Omnibus Trade and Competitiveness Act of 1988 (Public Law 100-48) required science and technology agreements to provide for reciprocal access (sec. 5171). See Uyehara (2000: 91).

[45] While under development, the fighter was called the "FS-X." When the fighter entered into the production phase, it was named the "F-2." For the sake of convenience, I will refer to the fighter hereafter as simply the FS-X fighter. Accounts and evaluations of the FS-X controversy differ in important ways, even considering just the English-language literature (e.g., contrast Green's and Lorell's assessments of the project's costs and benefits for the United States and Japan, in Green (1995) and Lorell (1996), respectively). For the purpose of illustrating the importance of collaboration as a means of defining and substantiating reciprocity, my account does not need to, nor does it attempt to, sort out those differences or present a comprehensive history of the FS-X co-development project. Lorell (1996) does a fine job of the latter task from a U.S. policy perspective.

purchased from the United States or produced under a co-production agreement.[46] Previously, the Government of Japan had consistently chosen co-production (e.g., for the United States' F-86, F-104, F-4, and F-15 fighter aircraft).[47] Although the Government of Japan had come to a consensus to develop their next fighter domestically, in order to jump through what some key individuals in Japan considered to be just a bureaucratic hoop, Japan's Ministry of Finance solicited import bids to help it frame and evaluate the cost of an indigenous program.[48]

This process provided an opening for the U.S. defense aerospace industry and the U.S. Department of Defense to advocate the benefits of previous co-production arrangements vis-à-vis indigenous development. The U.S. Department of Defense thought that for Japan to develop and produce a top-of-the-line indigenous fighter would be extremely expensive and would redirect funds from Japan's other defense programs that complemented the United States' military posture in northeast Asia. Moreover, the Department of Defense feared that an indigenous fighter would diminish interoperability in the alliance and possibly encourage Japan to distance itself from the United States' foreign policy.[49] Alliance managers in the Department of State and in the Department of Defense came to regard the FS-X collaboration as a bellwether of future U.S.-Japan relations more generally, and they communicated that political escalation to the Government of Japan.[50] After the U.S. Congress became involved[51] and after

[46] Kohno (1989: 459).
[47] U.S. General Accounting Office (1989) and Samuels (1994: 198-269).
[48] See, for example, the comments of the Director of Japan's Technical Research and Development Institute, as quoted in Samuels (1994: 238).
[49] These concerns were voiced by numerous policymakers throughout congressional hearings on FS-X. See, for instance, Secretary of Defense Richard Cheney's testimony in U.S. House, Committee on Foreign Policy (1989).
[50] Lorell (1996: 120-154).
[51] Congressional pressure intensified, in part, because the Toshiba Machine Company was found in 1987 to have secretly and knowingly breached export controls by selling the Soviet KGB high-precision computerized milling equipment that, it was alleged, could be used to manufacture submarine propeller blades. Indulging in political theatrics, some members in the U.S. Congress reacted to the "Toshiba Incident" by crushing Toshiba products on Capitol Hill using

several diplomatic miscues and missteps, in October 1987 Japan capitulated and agreed to co-develop with the United States a fighter loosely based on General Dynamic's F-16 C/D Block 40.

But the controversy had really just begun. Not until two years later was the U.S.-Japan agreement for the FS-X co-development project free from challenges from the U.S. Congress and U.S. Governmental agencies concerned with trade and industrial relations, such as the Department of Commerce. The agreement's opponents charged that the co-development project would be a boon for Japan's commercial aerospace industry, who, they said, would receive through co-development valuable technology and know-how from the United States.[52] As a result of these criticisms, in 1989 the Bush administration insisted on formally "clarifying" the 1987 agreement with Japan, to the outrage of the Government of Japan and Japanese commentators, an outrage that included charges that the United States was trying to clarify the agreement in order to shore up its techno-military hegemony.[53] Despite the FS-X agreement's ultimate survival, contestations over reciprocity in technology transfer continued years later, even after the signing of the production agreement in 1996.[54]

Technology sharing in the FS-X collaboration focused and gave material form to previously nebulous contestations over reciprocity in the U.S.-Japan relationship.[55] It exemplified how defense collaborations conceptualized reciprocity in the U.S.-Japan relationship in terms of "technology flowback" and how this flowback was eventually settled on a component-by-component basis by engineers, rather than by policy

---

sledgehammers. Uyehara (2000: 100-102) notes the hypocrisy and inconsistency in the U.S. Government's outrage.

[52] See, for example, a particularly influential op-ed, Prestowitz (1989).

[53] Noble (1992: 23).

[54] U.S. General Accounting Office (1995b); U.S. General Accounting Office (1997).

[55] The U.S. and Japan workshare in the development and production phases was also a significant component of reciprocity, but it was settled early on in the collaboration and was a much less troublesome issue.

memoranda of official alliance managers in each state's foreign affairs or defense bureaucracies. The "technology flowback" discourse characterized the U.S. congressional hearings on the FS-X.[56] Both the House and the Senate also expressed concern about the adequacy of the United States' controls on "critical technologies," such as the computer software source code for the F-16's flight-control and fire-control computer. A report by the Congressional Research Service, however, helped deflate how critical those technologies were for Japan's commercial aerospace aspirations.[57] Most Congressional attention was focused on the value of "technology flowback" from Japan to the United States for the technologies whose development would not derive from the F-16 technology package that would be licensed to Japan. Under the terms of the "clarified" memorandum of understanding between the governments of United States and Japan, the United States would have free and open access to all technologies "derived" from the F-16 licensing package, and the United States (and its defense contractors) could pay to acquire technologies that Japan's firms developed indigenously for the FS-X. These "non-derived" technologies included, among others, gallium arsenide monolithic microwave integrated circuits for the active phased array radar that Mitsubishi Electric Corporation was developing and the co-cured composite wings that Mitsubishi Heavy Industries was developing. At issue was not only the value of those "non-derived" hardware components, but just as important, the value of the techniques for manufacturing those components.

[56] See, for example, the testimony of Secretary of Defense Richard Cheney and General Dynamics Chief Executive Officer Herbert Rogers before the U.S. House's Committee on Foreign Affairs (1989: 63, 192, respectively). Also see the testimony before, and the discussion within, the U.S. House's Committee on Banking, Finance, and Urban Affairs (1989), the U.S. House's Committee on Science, Space, and Technology (1989), and the U.S. Senate's Committee on Foreign Affairs (1989).
[57] The Congressional Research Service report is Moteff (1989). Lorell's account suggests that the Congressional Research Service report, which questioned the value of FS-X technologies for commercial aerospace, was influential among Congressional staffers and members of Congress (1996: 249-254).

Congress was able to debate the nature and value of those technologies in large part because these issues were left open and unspecified in the intergovernmental memorandum of understanding. The U.S. and Japanese negotiating parties had decided to leave those details for lower-level working groups to resolve.[58] Yet, those details were hardly unimportant; they came up for debate in the U.S. Congress in the spring and summer of 1989. Despite extensive technical testimony, however, Congress did not resolve those matters. An effort in the Senate in September 1989 to override the President's veto of a last-minute Senate amendment that added further conditions to the U.S.-Japan intergovernmental agreement, conditions primarily addressing work share and technology transfer, came up one vote short. Later in 1990, the U.S. Congress was able to keep the FS-X agreement under the spotlight by tasking their General Accounting Office to periodically monitor and review the implementation of the FS-X agreement and, in particular, to assess reciprocity in technology transfer. [59]

For most of the 1990s, the General Accounting Office followed and reviewed how the U.S.-Japan technical steering committee for the FS-X, its expert working groups, and its supporting bureaucracies negotiated reciprocity in the U.S.-Japan relationship. These actors, especially the working groups of the technical steering committee, managed "technology flow," classified and re-classified key technologies as either "derived" or "non-derived," and negotiated the nature and extent of technology flowback.[60] It almost could not have been done any other way. The F-16 data package included over 10,000 documents and technical drawings.[61] In addition to that package, the U.S. Department of State granted over 500 FS-X related munitions export licenses.[62] By 1997, Japan had sent over 40,000 technical documents "back" to the United States

[58] Lorell (1996: 233).
[59] U.S. General Accounting Office (1992c: 2); Lorell (1996: 294).
[60] U.S. General Accounting Office (1995b); U.S. General Accounting Office (1997).
[61] U.S. General Accounting Office (1995b: 66).
[62] Ibid, p. 5.

which were related to technologies "derived" from the F-16 data package and from licenses acquired by Japan's firms in support of the collaboration.[63] U.S. technology assessment teams composed of experts from government and industry visited Japan's manufacturing facilities to evaluate "non-derived" technologies of interest for technology transfer, such as the active phased array radar, the mission computer, the integrated electronic warfare system, and the co-cured composite wing.[64]

The transfer of Mitsubishi Electronic Corporation's gallium arsenide devices for the active phased array radar paved new territory in U.S.-Japan arrangements for exchanging technologies, territory unspecified by the formal technology transfer protocols that were still under negotiation at the time. Side stepping other contentious official export control categories, the gallium arsenide devices were transferred from Japan to the United States under a new category, as simply "data" for the purpose of testing. In a way similar to that of the gallium arsenide devices, ad-hoc practices by expert working groups adjudicated "technology flow" for other technologies too, such as the licensing of lower-tier vendor technologies and the re-classification of several technologies from derived to non-derived status.[65] Consequently, expert working groups negotiated and settled U.S.-Japan reciprocity in high-level matters for which high-level political forums and agreements had previously proven to be ill equipped or insufficient.[66] They carried out these discussions on a component-by-component basis.

In addition to the negotiation of technological reciprocity, a second dimension in which defense and space collaborations generally manifested different U.S.-Japan relations is the way that they distributed projects' financial costs and benefits. In defense, the distribution of costs was a much more straightforward issue than

---

[63] U.S. General Accounting Office (1997: 12).
[64] Ibid., p. 14.
[65] Lorell (1996: 312-356).
[66] Plafcan (1999: 30), and the author's interview with a former bureaucrat in the Japan Defense Agency's Equipment Bureau (2000).

technological reciprocity.[67] Most U.S.-Japan co-production and co-development collaborations in defense evaluated financial costs and benefits of projects in terms of *ex ante* agreements. Funding and workshare percentages were agreed upon between governments, and licensing fees were agreed upon between firms. For the FS-X, the Government of Japan funded the financial costs of developing and producing the fighter. After considerable negotiation and after the "clarifications" in the spring of 1989, U.S. firms were guaranteed in the memorandum of understanding a research and development workshare of about 40 percent of the Government of Japan's research and development budget (about $1.2 billion). Most of that went to two firms: General Dynamics (later Lockheed Fort Worth), which was the U.S. sub-contractor for a portion of the aircraft's structure, and General Electric Aircraft Engines.[68] According to the memorandum of understanding signed for the production phase in 1996, U.S. firms were also guaranteed 40 percent of the workshare (or about $4.1 billion for the then-expected 130 aircraft production run). Although over 200 U.S. firms participated in the production of the FS-X (now called the F-2), Lockheed Martin and General Electric had 70 percent of the U.S. workshare. Which companies would produce which components was all stipulated up front in the set of production agreements. For example, Lockheed Martin was slated to produce four out of five of the fighter's left wings.[69] Judging from the successful experience of workshare arrangements in the development phase, the quotas in the production phase were expected to be realized.[70]

Concerning the third dimension of comparison, how collaborations negotiated U.S.-Japan interdependency, defense collaborations constrained interdependencies in

---

[67] Distributional issues do have some complications, such as taking into consideration fluctuating exchange rates and validating that the agreed upon distribution has been realized in practice (U.S. General Accounting Office 1997: 9).
[68] U.S. General Accounting Office (1997: 6); Lorell (1996: 288).
[69] U.S. General Accounting Office (1997: 8).
[70] Ibid., p. 8-10.

the U.S.-Japan relationship to issues about the supply of hardware, servicing agreements, and hardware development, rather than about operations. Negotiations in defense collaborations did not directly limit how the hardware would be used by the United States or Japan, once built. Matters of military cooperation and training were handled separately. Nevertheless, even for matters surrounding hardware development, balances between national autonomy and interdependency were contentious issues that required frequent negotiation, and those negotiations were often conducted over individual components.[71]

A debate about the development of a radar altimeter for the FS-X fighter illustrates how the United States and Japan technical teams negotiated a particular instance of U.S.-Japan inter-state dependency.[72] In late 1991, the FS-X design group in Japan had been considering producing under license a digital radar altimeter built by a U.S. firm, Teledyne Ryan. The Japan Defense Agency and its contractors had designated this hardware item among several dozen other components as "critical" for flight safety and flight testing, thus necessitating, they claimed, suppliers based in Japan. With that reasoning, they requested to the United States that they be allowed to license-produce these critical items rather than purchase them from vendors in the United States, as had been previously assumed by the U.S. technical steering committee. The U.S.-Japan memorandum of understanding and associated agreements did not address these emergent questions; it was up to the U.S. technical steering committee for the FS-X and more specifically, an ad-hoc working group of that committee, to decide this issue and negotiate any conditions with Japan.

[71] See Green (1995) for an account of U.S.-Japan relations which frames Japan's foreign policy in terms of Snyder's alliance dilemma of entrapment versus abandonment (1984).
[72] This account is drawn from the account given in Lorell (1996: 316), and Lorell's account is based entirely upon Lorell's interview work with officials in the United States and Japan.

The steering group as a whole found the Japan Defense Agency's reasoning to be unconvincing. In line with the U.S. Government's concerns about technology flow and thinking that Japan's contractors would have no other alternative, the steering committee decided that this component, along with many others, should not be co-produced but should be purchased as a hardware item from vendors in the United States. Nevertheless, the Japan Defense Agency opted to license-produce a different altimeter, an analog altimeter that met different specifications, using technology provided by a French firm. Officials at the U.S. Defense Security Assistance Agency objected to the Japan Defense Agency bringing in a third country, even if indirectly, into the FS-X program; they claimed that such action violated the spirit of the memorandum of understanding. The debate over how to interpret the memorandum of understanding was never resolved, but Japan went ahead anyway to license-produce from a French firm an analog radar altimeter that met their technical specifications and ensured some level of domestic production and national autonomy. For this FS-X component, the Japan Defense Agency bucked its dependency upon the United States.

U.S.-Japan Collaboration in Space:
Negotiating with and through "Clean Interfaces"

In the 1980s and 1990s, U.S.-Japan intergovernmental collaborations in both defense and space were confronted with expansive and unruly techno-political questions about reciprocity, distribution of project costs, and inter-state interdependency. Like U.S.-Japan collaboration in defense, U.S.-Japan collaboration in space in the last half of the twentieth century roughly moved from industry-to-industry collaborations approved by the two governments, in which Japan's firms licensed-produced technologies from the United States, to more complex arrangements in the 1980s and 1990s in which intergovernmental collaborations were supported by firms from each state. Although U.S.-Japan space collaboration has not been studied as extensively as the two states' collaboration in defense, given the extant literature, stark differences between defense and space can be documented in how U.S.-Japan relations were actualized in the 1980s and 1990s.

U.S.-Japan intergovernmental collaborations in defense negotiated reciprocity on a component-by-component basis in terms of "technology flowback." Financial costs and benefits were negotiated up front, in an explicit manner, in terms of government funding percentages and workshares for firms from each state. Negotiating interdependency between states was largely constrained to questions about the supply of hardware and servicing. In contrast, in U.S.-Japan collaborations in space, technological reciprocity and the distribution of financial costs and benefits were both generally subsumed into questions of division of labor/expertise and were negotiated primarily in the design of the technoscientific system. Questions of interdependency were straightforwardly and conspicuously managed by deliberately designing what were known as "clean interfaces" between compartmentalized modules, modules for which each government was solely responsible. Whereas in defense collaboration "technology flow" was conceptualized as needing to be tracked blueprint-by-blueprint or component-by-component, in space collaboration the two governments designed their joint technoscientific systems so as to discourage, if not eliminate, technology transfer altogether. Yet, in space collaboration, unlike in defense, inter-state dependency could—and often did—extend beyond hardware and into operations.

International collaboration has been a central element of the mission of the United States' National Aeronautics and Space Administration from the organization's beginnings. While NASA was founded as a consequence of the fear and competition provoked by the Soviet Union's launching of its Sputnik satellite, it was also founded amid idealism for international cooperation in science, an idealism inspired by the International Geophysical Year to which Sputnik had been a contribution. [73] The International Geophysical Year (IGY) was a comprehensive effort to study the earth and its atmosphere, involving over 60 national programs and tens of thousands of scientists who were coordinated through the International Council of Scientific Unions.[74] Arnold Frutkin, who directed NASA's Office of International Programs soon after NASA's establishment and had previously served at the National Academy of Sciences as Deputy Director of the U.S. Committee of the IGY, judged it to be an "arresting fact" that Congress "plung[ed] the nation into competition for preeminence" with the founding of NASA and yet "simultaneously" directed NASA to "establish cooperative ties with other countries."[75] Frutkin also noted that "the parallel prosecution of both was, of course, entirely conscious. NASA's Administrator, James E. Webb, said on more than one occasion that space, like Janus, looks in two directions."[76]

Aware of the potential complexities of "looking in two directions," NASA developed a set of guidelines for its international cooperation. First, NASA was to cooperate with other states through a single, central civilian agency designated by each state, instead of through universities, private contractors, or even ministries that might be assigned case-by-case. Second, commitments to cooperate were to be made on a

[73] Frutkin (1965: 17-30). The National Aeronautics and Space Act of 1958, in its "Declaration of Policy and Purpose" section stated among NASA's objectives were both "(5) The preservation of the role of the United States as a leader in aeronautical and space science and technology and in the application thereof to the conduct of peaceful activities within and outside the atmosphere" and "(7) Cooperation by the United States with other nations and groups of nations in work done pursuant to this Act and in the peaceful application of the results, thereof." Section 102, subsection c, paragraphs 5 and 7, respectively, of the "National Aeronautics and Space Act of 1958," Public Law #85-568, 72 Stat., 426.
[74] Sullivan (1961).
[75] Frutkin (1965: 8). Frutkin was NASA's senior negotiator for its major international programs and space agreements from when he was hired by NASA in 1959 as Director of International Programs to when he retired at NASA in 1979 as Associate Administrator for External Relations. "Arnold W. Frutkin," Biographical File, NASA Historical Reference Collection, NASA History Office, NASA Headquarters, Washington, D.C.
[76] Frutkin (1965: 8).

project-by-project basis, rather than through programmatic or more general agreements. Within each project, governments were to be encouraged to make contributions that, while in keeping with NASA's overall objectives, supported in particular the contributing government's scientific and technological interests. In other words, NASA was not to impose elements of a project onto a foreign government as that government's "contribution." Furthermore, each government was to be singularly responsible for its contribution to the joint project with NASA, technically and financially; neither technology nor money was to be exchanged between governments. Finally, scientific results of projects were to be openly published.

NASA's guidelines were explicit and widely circulated, and their language in pamphlets, policy statements, and Congressional testimony was relatively consistent throughout NASA's existence in the twentieth century.[77] This consistency is likely in part a result of Arnold Frutkin's twenty-year tenure in senior positions at NASA's international affairs office and external relations division. Kenneth Pedersen, who held the same positions with NASA in the 1980s as Frutkin did in the 1960s and 1970s, explained NASA's guidelines in a similar way to Frutkin. Yet, Pedersen did elaborate upon—and occasionally thoughtfully problematized—NASA's requirement that national contributions be compartmentalized and structured along what Pedersen called

[77] See Kash (1967: 40), who cites a 1963 pamphlet by NASA's Office of International Programs, and in addition, see Frutkin (1965: 35) and NASA International Affairs Office (1983). Frutkin contrasts NASA's approach to international cooperation with that of the purported failure of the Atomic Energy Commission's approach. Frutkin explains the "conservative values" behind NASA's philosophy for international cooperation in the following way:

> Experience in international programs of assistance, support, and cooperation, suggested that a new realism might be appropriate—at least in the special circumstances of space activity. A program philosophy was required to minimize both the warranted and the unwarranted criticism which had been directed at other cooperation ventures—for alleged waste and misdirection of dollars, for investment in training and facilities never used, for shoddy performance by cooperating foreign groups, for questionable motivation of our own and foreign personnel, and for promise outrunning performance. A program founded on conservative values, though not necessarily conservative in scope and objectives, was indicated (p. 32, and see more generally, p. 28-36).

"clean interfaces."[78] In the 1990s and through until 2004, the bureaucratic language of NASA's guidelines has been slightly revised and qualified.[79] Still, in less formal presentations, such as in presentations by NASA External Relations personnel to scientists interested in proposing international projects to NASA, the guidelines for cooperation have been expressed in much the same way as they have been for decades (the informal "clean interfaces" jargon that emphasizes the need for clear lines of technological and financial responsibility was most likely introduced by Pedersen in the 1980s and has seemed to have been in use since).[80]

NASA's guidelines were, for the most part, strongly suggested guidelines that needed to be taken into consideration, rather than absolute rules that resulted in strict adherence. For example, in the early 1960s, NASA first approached scientists in Japan to collaborate on space-related projects, reportedly in agreement with a request to do so from the U.S. Department of State.[81] But while an advisory body to the Prime Minister's office had been established in 1960 for Japan's space activities and their promotion (namely, the National Space Activities Council), Japan's space activities were not at that time managed by a central government agency as was called for by

---

[78] For Pedersen's articulation of the guidelines, see Pedersen (1987). For his analysis of the quandaries that might arise in their implementation, see Pedersen (1986).

[79] For instance, industrial competitiveness is now addressed explicitly in NASA's policy directives: "Arrangements for cooperative projects must take into consideration the need to protect against the unwarranted transfer of technology abroad, in accordance with U.S. export laws and regulations. Issues related to U.S. industrial competitiveness will be considered in developing cooperative projects." See NASA Policy Directive 1360.2A, "Initiation and Development of International Cooperation in Space and Aeronautics Programs," section 1, paragraph f. This directive can be found at:
http: //nodis.hq.nasa.gov/displayDir.cfm?Internal_ID=N_PD_1360_002A_&page_name=main, last accessed 3 August 2005.

[80] An example of one recent presentation to scientists who were interested in proposing international projects is Kirkham (2004). Slide four of Kirkham's presentation depicts clear lines of responsibility as "clean interfaces." Kirkham emphasized the importance of clean interfaces for NASA programs and for proposing programs to NASA, saying: "Clean interfaces is real important with international collaboration [*sic*]. The greater the technical complexity, it's harder to have clean interfaces, I understand, but I think that to the extent that you can have clean interfaces I think your proposal will be helped" (NASA 2004: 100).

[81] Kash (1967: 42).

NASA's guidelines for international cooperation.[82] It was academic scientists and their students, principally from the Institute of Industrial Science at the University of Tokyo, who had since 1955 been successfully developing and launching small sounding rockets that measured phenomena in the earth's upper atmosphere.[83] Because academic space scientists in Japan in the 1950s and early 1960s valued their independence from the Government of Japan and because these scientists lacked a working relationship with their government beyond receiving funds for their research, the U.S. Department of State reportedly interceded on their behalf with the Government of Japan so as to establish cooperative ties between NASA and those scientists on a project-by-project basis.[84] NASA scientists and these Japanese scientists in 1962, 1963, and 1964 launched a small number of sounding rockets that carried complementary devices, contributed by each party, to measure electron density and temperature in the ionosphere.[85] Despite these efforts, NASA scientists did not undertake any major intergovernmental collaboration in space technoscience with Japan, in small part owing to the changing organizational structure of Japan's governance of space activities during the 1960s.[86]

The lack of a central space agency in Japan with which NASA could collaborate in accordance with its guidelines was not the primary obstacle to U.S.-Japan collaboration in the 1960s. In the name of pragmatism, NASA's guidelines were serviceable and malleable, as the guidelines had been in the collaborative sounding rocket experiments and as they would be in the future when Japan would have two distinct space agencies. Nor did dramatic differences in the 1960s between each state's expertise and capacity in space technoscience pose insurmountable obstacles to collaboration (in the mid 1960s, NASA was in its prime with the Apollo program, while

[82] Hachifuji (1983: 13).
[83] Kagaku gijutsu chō (1978: 1).
[84] Kash (1967: 42).
[85] Frutkin (1965: 56).
[86] See generally, Hachifuji (1983) and Kagaku gijutsu chō (1978).

Japan was struggling to develop rockets to launch satellites). Rather, for most of the 1960s, Japan's dominant space scientists, engineers, and their supporting bureaucracies, such as the Institute of Space and Aeronautical Science and the National Space Development Center, all wanted Japan to develop its space capabilities independently without the crutch of U.S. assistance and without the constraints of licensing from the United States, including the prohibition of third-party transfer and sales.[87] Whereas, as described above, from the early 1950s Japan's firms had been license-producing numerous defense-related technologies from the United States, arrangements for U.S.-Japan industry-to-industry licensing and collaboration in space technology were not made until 1969, with the U.S.-Japan *Exchange of Notes Concerning Cooperation in Space Exploration*.[88]

The 1969 U.S.-Japan agreement was a turning point for Japan's space program in that it formally presented Japan's emerging space bureaucracies with the option of using licensing from U.S. firms to ultimately achieve domestic development and national autonomy in space. The agreement had been a few years in the making. It had been advanced largely at the initiative of President Johnson's administration and the U.S. Department of State, which wanted to stem the proliferation of solid-fueled rockets developed by Japan's Institute of Space and Aeronautical Science and the Institute's contractors, such as Nissan Motor. Solid-fueled rockets were viewed by the U.S. Department of State and the U.S. Arms Control and Disarmament Agency as more of a proliferation threat than the liquid-fueled rockets on which NASA's satellite launch systems were based, and Japan's firms had been selling solid-fueled rockets and licensing solid-fueled rocket technology.[89]

[87] Saitō (1992: 20-30); Logsdon (1997: 4); and Satō (2001: 180-182, 189).
[88] U.S. Department of State (1969).
[89] Logsdon (1997: 7) and Satō (2001: 184).

While the United States was seeking to collaborate, assist with, and shape Japan's space programs so as to realize U.S. foreign policy goals, Japan was in the midst of a major re-organization of its space development effort. Japan's re-organization of its domestic space institutions and the United States' collaborative proposals to the Government of Japan fed off each other. A particularly important and central goal that Japan's space bureaucracy was struggling to achieve in a timely manner was the development of rockets to launch meteorological, communication, and broadcast satellites. In the late 1960s, the Government of Japan—like the United States—maintained a policy that required all of its satellites to be domestically launched, thus encouraging rapid domestic development of launch vehicles, but there was a hitch in this policy: Japan had not yet successfully put a satellite into orbit.[90] To realize this goal and to establish space development as a national effort, three new organizations were established in Japan. In July of 1968, the Federation of Economic Organizations (Keidanren) founded its Space Activities Promotion Council; in August of 1968, Japan's National Space Activities Council—an advisory body to the Prime Minister's Office—was reconstituted into a supposedly more robust policy-making body, the Space Activities Commission; and in June 1969, the Japan's parliament passed legislation to create the National Space Development Agency.[91]

As a part of this renewed space effort, in July 1969 after the splashdown of Apollo 11 in the Pacific, the governments of Japan and the United States agreed to a governmental framework in which Japan's firms would be allowed to license liquid-fueled launch vehicle technology from U.S. firms, if the Japan and U.S. firms wanted to do so.[92] A few months after the signing of the intergovernmental framework

[90] Satō (2001: 192).
[91] Hachifuji (1983: 117).
[92] Logsdon (1997: 13-15) and Satō (2001: 190).

agreement, the head of Japan's new National Space Development Agency declared in a newspaper interview:

> Of course we must consider international cooperation. Insularism does not work in the space age, though when and how to engage in international cooperation should be decided by what Japanese people think and feel . . . Even if we choose to develop independently, I do not sympathize with the view that national projects must consist of purely domestic parts and components with Japanese flags on them. Necessary materials, components, and technologies should be of course introduced and absorbed.[93]

Following that philosophy, Japan's National Space Development Agency, as part of its mandate to build a commercial space launch vehicle, cancelled its solid-fueled rocket projects and began to license U.S. liquid-fueled technology at what were unprecedented levels of capability. While the U.S. National Aeronautics and Space Administration regarded this collaborative arrangement as unjustified according to both its guidelines for international cooperation and its organizational mission, the U.S. Department of State and the White House saw for a time such collaboration as advancing U.S. foreign policy goals.[94] The arrangement of unilateral transfer of space launch vehicle technology from U.S. firms to Japan's firms, in exchange for licensing fees, differed little in practice or rationale from the aerospace defense assistance agreements discussed earlier.

But that arrangement for U.S.-Japan space collaboration was short-lived. Almost as soon as the 1969 arrangement was used, it was contested. In other words, the parties still had to work out what they had agreed to. Which technologies the U.S. Government had agreed to allow Japan's firms to license came into dispute. A U.S. technical advisory group for launch vehicles noted in 1970 that

[93] Shima Hideo in *Nihon Keizai Shimbun*, October 27, 1969, as quoted in Satō (2001: 191).
[94] Logsdon (1997: 8).

> the task of generating an explicit, single faceted and easy to administer definition of the level of technology transfer authorized and/or intended under the United States/Japanese space agreement [of 1969] is not a simple one. . . . The most specific guidance provided [in the agreement] is that of paragraph B of the Attachment to the basic agreement which states that "This agreement will cover unclassified technology and equipment up to the level of the Thor-Delta vehicle systems exclusive of reentry and related technology." This statement does not limit the Thor-Delta level to a fixed configuration as of a specific point in time. The word "level" can be defined in a number of ways depending on the specific set of conditions surrounding a particular situation.[95]

As early as 1972, the U.S. National Aeronautics and Space Administration, partly through its role in the technical advisory group, re-asserted its position and its longstanding guidelines in the U.S.-Japan arrangement regarding space launch vehicle technology. NASA was repeatedly able to cast the 1969 agreement as a one-time deal for a static configuration of specific technology, rather than as a framework for a more general relationship structured along a particular line of technology. Licensing and transfer of U.S. technology precipitously gave way to sales of already assembled hardware rather than licensing and transfer of upgrades of a given model.[96] Although contractors for Japan's National Space Development Agency continued to be able to purchase hardware that U.S. firms were not permitted to sell to anyone else outside of the United States, U.S.-Japan exchanges of notes about space technology in 1976 and 1980 reinforced NASA's traditional guidelines for international cooperation as the guidelines relevant for U.S.-Japan collaboration in space.[97] Those guidelines were

[95] Letter from Vincent Johnson, National Aeronautics and Space Administration, to John Snipes, Director, Office of Munitions Control, Department of State, October 30, 1970, as quoted in Logsdon (1997: 29).
[96] See, for example, the discussions that are documented in the collection Kenkyū Chōsei Kyoku Uchū Kokusaika (1979), especially the U.S. Department of State "Talking Paper on the Sale of Launch Vehicle for Japanese Meteorological Satellite" (1979 [1972]:121-126) and the meeting between officials from NASA and Japan's National Space Development Agency (1979 [1974]: 167-171). This collection can only be found, as far as I am aware, at the libraries of Japan's National Space Development Agency in Tsukuba and Tokyo. I was not allowed to photocopy these documents, owing to uncertainty about their administrative declassification.
[97] Bloom (1984: 102) and the 1976 "Letter from Bloom to Osawa" in Kenkyū Chōsei Kyoku Uchū Kokusaika (1979: 200-201).

operative for numerous intergovernmental collaborations in space from the late 1970s onward.[98]

How those guidelines played out in U.S.-Japan collaboration in space from the 1980s to the present can be illustrated by their collaborative practices in the most expensive and largest intergovernmental technoscientific project in history: the NASA-led International Space Station. In addition to the United States, construction of the International Space Station has involved Russia, the European Space Agency, Japan, and Canada. Brazil and Italy are payload participants. The Government of Japan, through its National Space Development Agency, is contributing the Japanese Experimental Module, a pressurized and unpressurized (i.e., exposed) space laboratory in which material science and life science experiments can be conducted, with the ultimate goal of commercialization.[99] As of 1996, the module's projected budget was over $3 billion, with about seventy percent of it already spent at that time.[100] To describe U.S.-Japan collaboration in the space station, which included the integration of the Japanese Experimental Module with the space station, I will again turn to the three dimensions of technoscientific collaboration that I presented above for U.S.-Japan collaboration in defense.

First, U.S.-Japan collaboration in the space station managed concerns about technoscientific reciprocity by minimizing technology transfer and by implementing a philosophy of "clean interfaces." From exploratory talks in the mid 1980s, to the initial intergovernmental agreement and bilateral memorandum-of-understanding in 1988, through to the new set of agreements in 1998 and the hardware testing of the flight model delivered to the Kennedy Space Center in Florida in 2003, the Japanese Experimental Module that will attach to the core structure of the space station has been

---

[98] Logsdon (1997: 42-43).
[99] Ida, Murayama, and Horikawa (1992) and Kawamoto (2004).
[100] Covault (1996: 62).

designed, funded, managed, and built by Japan's National Space Development Agency with the support of Japan's contractors, such as the Japan Manned Space Systems consortium. [101] The module's equipment for crew operations, such as its communications system, did need to be standardized across the space station, and some of this equipment was consequently procured from U.S. manufacturers (but not as technology transferred from NASA). Still, the module's container, internal systems, power systems, environmental controls, and payload support mechanisms have all been designed and built by firms in Japan.[102] The overriding principle is that the Japanese module during its development will be managerially and technically independent from the other elements of the space station.[103] Eventually, it will plug into a space station "node" via a standardized berthing mechanism.[104]

Second, U.S.-Japan collaboration on the space station handled financial costs and benefits as matters to be negotiated and worked out through the design and use of the technology, rather than as based upon *ex ante* cost/benefit calculations that might possibly require intergovernmental cash payments in order to balance distributional issues. At one time in the planning for the space station, a payment of funds was thought necessary, so that NASA could recoup the costs of launching other states' modules and of providing common services onboard the space station. Eventually, in the case of

---

[101] [Intergovernmental Agreement] On the Cooperation in the Detailed Design, Development, Operation, and Utilization of the Permanently Manned Space Station, September 29, 1988; Agreement Among the Government of Canada, Governments of the Member States of the European Space Agency, the Government of Japan, the Government of the Russian Federation, and the Government of the United States of America Concerning Cooperation on the Civil International Space Station, January 29, 1998.

[102] The language in 1998 intergovernmental agreement allows exchange of technical data for "interface, integraton and safety" (article 19, paragraph 3).

[103] Kato (1993) describes how Japan's engineers contested NASA's standards for the space station's electrical system in an effort to establish "clean interfaces" (Kato's words) and ensure a stable power system for the module. Kobayashi (1986) discusses Japan's policy of "clean interfaces" vis-à-vis NASA for the space station (e.g., p. 11). Not surprisingly, the language of Frutkin's and Pedersen's guidelines for NASA's international cooperation made its way to Japan's engineers.

[104] Mayo, Bolton, and Laurini (1989) discuss this mechanism and other standards, such as those for payload racks.

Japan, instead of exchanging funds, bartering was employed for offsets. Japan agreed to provide the space station with a Centrifuge Accommodation Module, a Centrifuge Rotor, and a Life Sciences Glovebox, in exchange for NASA launching and transporting the Japanese Experimental Module to the space station. Operational allocations were worked out—at least on paper in the intergovernmental agreements and memorandum-of-understanding—in terms of "real estate," i.e., what modules were contributed and by whom. Under the terms of the negotiated agreements, Japan would have 51% of the use of its experimental module, or, in terms proportional to the entire space station, 12.8%. While the latter percentage is said to be roughly in the neighborhood of Japan's percentage of expected development costs for the space station (itself a highly uncertain figure), unlike for the manufacturing share of the FS-X fighter aircraft collaboration, intricate cost calculations did not drive this allocation of resources. If a single factor did, it was the "real estate" that had itself been based upon previous technoscientific ambitions and decisions.[105]

As with other U.S.-Japan collaborations in space, and unlike in their arrangements in defense collaboration, the United States and Japan in their collaboration for the space station once again used compartmentalization, made possible by the construction of "clean interfaces," to manage political and technical interdependencies. NASA did not consider the Japanese Experimental Module as a "critical path" element; if it were to explode (at launch), fail, or otherwise be eliminated, the other modules of the space station could be launched, assembled, and utilized without it.[106] Thus, the Japanese Experimental Module was non-essential to NASA. Of course, NASA's core space station elements, as well as Russia's contribution, were essential to the functioning of the Japanese module. The United States' and Japan's

[105] Cline and Gibbs (2003: 923-924). Lynn Cline was involved in the negotiations as a representative of NASA's Office for External Relations.
[106] Ibid., p. 918.

techno-political interdependency for the space station was then, for the most part, in one direction. This imbalance in power becomes particularly evident when NASA "proposes" design changes and delays to which Japan's National Space Development Agency really has no choice but to acquiesce. For the National Space Development Agency to cancel what is, at over $3 billion, its single most expensive space project would be catastrophic to the agency. Even in the project's early years, in 1991, when the U.S. Congress was threatening to cancel NASA's efforts in the space station, and thus the space station itself, Japan's foreign minister wrote to the U.S. Secretary of State that if the space station were cancelled, "major joint efforts which the international partners have made so far would be nullified, Japanese Space Development Programs would be significantly impaired, and furthermore, I fear that the credibility of the United States as a partner in any major big science effort would inevitably be damaged."[107]

In contrast to their defense collaboration, U.S.-Japan space station operations will breach compartmentalization. In operational matters, interpreting and assessing techno-political interdependencies becomes more complex and speculative, given the limited nature of the published literature on space station operations and the limited activity to describe to date. The Japanese Experimental Module has yet to be launched and attached to the space station structure. While compartmentalization was regarded as a workable strategy for dividing clear lines of technical and managerial (and thus political) responsibility during the space station's development on earth, once the modules are linked together in space via structural nodes and common berthing mechanisms, the modules free fall in orbit together. When the space station's orientation and orbital positioning are adjusted, all experiments on-board are potentially affected. Likewise, the crew on the space station when the Japanese module will be

[107] Letter from Minister for Foreign Affairs Nakayama Taro to Secretary of State James Baker III, 24 May 1991, as quoted in Logsdon (1992: 299).

attached is expected to be limited to an integrated group of seven personnel. One of those seven will be the formal commander of the other six. While crew members will no doubt take on different roles and functions, the crew will hardly be compartmentalized in any strict sense of the term. As an indication of the potential complexity of social life onboard, common procedures for prosecuting criminal conduct have even been formally agreed upon.[108] U.S.-Japan ground operations supporting the space shuttle and the Japanese Experimental Module are also interdependent. While ground operations and engineering support systems are distributed across Japan and the United States, with Japan controlling most of the systems for its module, a few elements—such as the data network system—are utterly dependent upon systems in the United States to pass along information to the space station.[109] Despite the United States' and Japan's longstanding military alliance, these operational interdependencies do not exist in U.S.-Japan scientific and technological collaboration in defense.[110]

So far I have argued that since World War II the governments of the United States and Japan have used technoscience to undergird and mold their inter-state relations, and consequently, technoscientific projects have become a central constitutive thread of U.S.-Japan relations. Through examples of collaboration in defense and space, I have suggested that important high-level negotiations about the character of the U.S.-Japan relationship—questions about reciprocity, partnership, interdependence, and power—and that negotiations about the techno-political roles and boundaries of

---

[108] Article 22 of the 1998 Intergovernmental Agreement.

[109] Fawacett (1996) and Outline of Kibo operations system, from the JAXA web site at: http://iss.sfo.jaxa.jp/iss/kibo/ctl_e.html, last accessed 4 August 2005.

[110] That could change, however, with Government of Japan's December 2003 decision to purchase ballistic missile defense systems from the United States which take advantage of U.S. missile tracking capabilities. Nevertheless, the Government of Japan has maintained that it will not become operationally tied up in U.S. systems. Japan is building its own radar systems, for instance, and it is not clear whether or not that information would be shared with a U.S. Navy Aegis cruiser equipped with missile defense capabilities which is deployed in the Sea of Japan. See Onishi (2004) and Boese (2004).

each state have often been conducted by working groups of scientists and engineers, rather than just by diplomats from each nation's foreign policy bureaucracies. In the case of the FS-X fighter aircraft, as well as other U.S.-Japan collaborative projects in defense, and in the case of the international space station, as well as for other U.S.-Japan collaborative projects in space, the U.S.-Japan relationship has been worked out in the building of technoscientific machines.

Furthermore, I have argued that "the" U.S.-Japan relationship has not been a single, unified relationship (see table 2.2 below, on page 86). While of course different collections of U.S.-Japan scientists, engineers, and bureaucrats living in contemporaneous times confronted vaguely similar questions about the U.S.-Japan relationship at a given historical moment and can have roughly common anxieties about the Cold War, trade, and so on, their particular circumstances and problems were not identical. As a result, in order to address these different circumstances and problems, they built and actualized U.S.-Japan relations around their particular circumstances and problems. This section has described how the practices of U.S.-Japan technoscientific collaboration in defense and space have consistently differed in important ways over decades of time, even though the building of systems in defense and space has been supported by overlapping industrial sectors that focus on aerospace, electronics, and communications systems. In defense collaboration in the 1980s and 1990s, issues of technoscientific reciprocity, financial costs, and techno-political interdependencies have been tracked component-by-component, with economically-explicit *ex ante* decisions. In space collaboration during the same period of time, practices have been guided by a general philosophy of "clean interfaces" in which questions of reciprocity, financial costs, and techno-political interdependences have been subsumed into and negotiated in technical development, and significantly, also in operations.

Table 2.2: Comparing U.S.-Japan Technoscientific Relations in Defense and Space

Dimensions	Defense Collaboration	Space Collaboration
1. Reciprocity	Component-by-component analysis	"Clean interfaces" system design
2. Distribution of financial costs	*Ex ante* contractual agreement in explicit financial terms	Implicitly follows "clean interfaces" and division-of-labor
3. Interdependency	Component-level, case-by-case hardware development	Operations of system in addition to hardware development

Most of the available literature that addresses political questions about U.S.-Japan technoscientific collaboration—literature from which this section has drawn in part to make its argument—has focused on intergovernmental agreements, corporate contracts, and such, rather than technoscientific practice, and has overlooked or implicitly minimized differences in technoscientific practice as merely lower-level bureaucratic differences that do not implicate or complicate U.S.-Japan relations. By using technical secondary source material and some primary source material, including Japanese-language sources, to describe the working group negotiations and technical arrangements regarding the FS-X's active phased array radar, the FS-X's radar altimeter, space launch vehicles, and the Japanese Experimental Module, I have gone beyond the formal documents and have described practices that have built U.S.-Japan relations. In sum, I have argued that scientists, engineers, and bureaucrats have negotiated and realized distinct U.S.-Japan relations in defense and space.

Re-thinking Collaboration and Explaining U.S.-Japan Technoscientific Relations

Describing how different collections of U.S. and Japan scientists, engineers, and bureaucrats have established different policies and practices for negotiating the

U.S.-Japan inter-state relationship in their technoscientific collaborations opens up intellectual space for new interpretative explanations of science and technology in international affairs and new interpretative explanations of the politics and policymaking of intergovernmental technoscientific collaboration in particular. Illustrating, as this chapter has done, how the inter-state relationship has been differently negotiated begs the question of why: why have different ways of negotiating the U.S.-Japan relationship arisen in defense and space?

To provide a rationale for this long-standing difference in policies and practices, explanations are unable to turn to geopolitics or balance-of-power alliance politics (e.g., Green 1995; Synder 1997), with their understandings of each state as a unified actor, since these collaborations have been conducted between the same two states in the same decades. The explicitness of *ex ante* economic calculations in defense collaborations might suggest an economic explanation for the difference in U.S.-Japan relations in defense and space, structured by competition on the basis of industrial sectors (e.g., Destler, Fukui, and Sato 1979). Yet, owing to Japan's ban on arms sales (albeit a weakening ban), Japan and the United State's economic rivalry was just as salient, if not more so, for space endeavors as it was for defense (see, for example, Pekkanen 2003). As the above description of U.S.-Japan collaboration in space launch vehicles indicates, NASA officials and their policies have been sensitive to competition with Japan since the 1970s. Nor was one project categorically more significant to either state's economy or its contractors. Both the development and production of the FS-X fighter aircraft and the development and operation of the space station were complex aerospace projects that demanded billions of dollars from the tax payers of each state. While the FS-X fighter aircraft was obviously a military weapon and Japan's space station module was a facility for experimentation in space, it is not clear how any differences between what might be called the "issue areas" of defense and space would lead deductively to the different ways of negotiating U.S.-Japan relations in technoscience.

Finally, explanations for differences in collaborative policies and practices between the United States and Japan which might rest on the involvement of different sets of domestic institutions in defense and space collaborations can not account for these differences on the sole basis of the structure of domestic institutions, since relations between the relevant bureaucracies and their contractors for defense and space were similar in each state and the relevant industrial sectors overlap. If an explanation that rests on domestic institutions is going to be persuasive, it must explain the differences in domestic institutions in terms of their particularistic histories, the meaning of their institutional enterprises, and their policymaking. Yet, if that is the case—if conventional explanations that place a premium on structure and deduction will not suffice—then the differences in U.S.-Japan relations which were described in this chapter can be taken as a call for an interpretative account of how the political, the technical, and the material are brought together in collaborations such that differences in inter-state relations and in policies for intergovernmental technoscientific collaborations can be explained. An interpretative account of inter-state relations in technoscience might include, but need not be limited to, interactions among institutions with particularistic histories and patterns of policymaking. This dissertation strives to do just that, using an approach that it argues is more compelling than two interpretative alternatives: the epistemic communities approach and actor-network theory. Without overlooking the character of various national institutions, the subsequent chapters of this dissertation explains the policies and practices of U.S.-Japan intergovernmental collaboration in science and technology as "technoscientific diplomacy" in which scientists, engineers, and bureaucrats in the United States and Japan effect particular U.S.-Japan inter-state relations in their knowledge-making and exercise of state power.

CHAPTER THREE

TWO COMMUNITIES OF REMOTE-SENSING PRACTICE AND STATES' GOALS

This chapter describes the technical practices, institutions, and state goals that informed the work of the U.S. and Japan teams of the intergovernmental collaboration on the ASTER remote-sensing system. The chapter first argues that geologic remote-sensing scientists and engineers in the United States and Japan were, before their collaboration, members of two distinct and separate communities, communities of practice which differed in their professional interests, in their technical practices, and in their institutional relationships to their respective states. The chapter then turns to the origins of U.S. and Japan state goals, describing how the governments of the United States and Japan wanted their forthcoming remote-sensing systems to achieve goals that potentially conflicted. As chapters four, five, and six illustrate, these differences in community and state goals were persistent sources of techno-political problems that U.S. and Japan scientists and engineers confronted, negotiated, set aside, and settled through their technoscientific diplomacy.

Two Geologic Remote-Sensing Communities: One in the United States, Another in Japan

The "U.S. science team" and the "Japan science team" for ASTER were constituted from two scientific communities that practiced geologic remote sensing quite differently. These two communities of practice not only had different visions of what they wanted to do, including different scientific and professional goals. They also differed with each other in the kind of scientific work that they had done and in how they typically went about doing that science, particularly with respect to their relationships with their government sponsors and with the developers of the instrumentation that they used in their scientific investigations. While not strictly representative of the deeply diverse remote-sensing, environmental, and earth science communities in the United States or Japan, the different perspectives and practices that the two teams brought to the negotiating table of their technoscientific diplomacy was nevertheless indicative of what it meant to do geologic remote sensing in the United States and Japan. They inhabited and sustained distinct forms of techno-political life that were bound to—but far from determined by—national bureaucracies that themselves were different in mission and in character.

Before they became members of the U.S. science team that helped develop the ASTER remote-sensing system, the members of the "U.S. science team" all shared an intellectual interest in the activity of developing techniques to acquire, to process, and to analyze remote-sensing data.[1] Most, but not all, of the U.S. science team had been a part of the same geology and geophysics group at the Jet Propulsion Laboratory in Pasadena, California for at least several years before their future collaboration with Japan's scientists and engineers on the ASTER remote-sensing system. Many had worked in the same building and on the same floor, literally down the hall from each other for well over a decade. The professional interests of this well-known and widely-respected group were principally in the "thermal infrared" region of the electromagnetic spectrum.[2] In the late 1970s these JPL researchers, who would

[1] The use of the term "U.S. science team" here is anachronistic for two reasons. First, the members of the U.S. team had not yet been collected together as a team, and second, the "U.S." label had not yet been attached to them. Nevertheless, for the sake of convenience, I will carefully use the term, usually with clarification and qualification nearby in the text. The same disclaimer applies to my anachronistic use of the term "Japan science team."

[2] I am using the term "thermal infrared" to refer to the general region of the electromagnetic spectrum with wavelengths from about 3 to 13 microns, particularly the 8-12 micron region.

become key members of the U.S. ASTER science team, were contracted by NASA's Office of Space Science and Applications to develop and calibrate a satellite instrument and to interpret the instrument's measurements of the thermal infrared temperature of the earth's surface.[3] In the early 1980s, these researchers were similarly contracted to develop and calibrate another thermal instrument and to interpret its data. This instrument, called the Thermal Infrared Multispectral Scanner, or TIMS, was an aircraft-based sensor that was able, for the first time, to discriminate among, and in some cases identify, important geologic constituents of the earth's surface, such as rocks and minerals. These geologic constituents had been indistinguishable when using previous instruments, which were sensitive only to other spectral regions (such as visible light and shortwave infrared regions).[4] Future members of the U.S. ASTER science team, including some members who were in JPL's geology and geophysics group, had also conducted research and development in the visible and shortwave infrared regions,[5] including assisting in the development of two aircraft systems for those regions.[6] But the core of the future ASTER science team had been focused for decades on developing instrumentation for acquiring thermal data, and they were interested in using that instrumentation mainly for such scientific applications as geologic analysis and volcanology.[7]

---

[3] I am referring here to the Heat Capacity Mapping Mission, a thermal satellite instrument launched in 1978. See, Kahle, Palluconi, LeVine, Abrams, Nash, Alley, and Schieldge (1982), Abrams, Kahle, Palluconi, and Schieldge (1984), and Kahle, Schieldge, and Alley (1984), as well as earlier foundational work, such as Gillespie and Kahle (1977).

[4] Kahle and Goetz (1983) and Palluconi and Meeks (1985).

[5] See, for instance, Abrams, Ashley, Rowan, Goetz, and Kahle (1977), and Kahle and Rowan (1980).

[6] Namely, the Airborne Imaging Spectrometer and the Airborne Visible and Infrared Imaging Spectrometer. For a general overview of these efforts, see NASA (1987a: 4-9).

[7] See, for instance, Abrams, Kahle, Palluconi, and Schieldge (1984), Kahle, Schieldge, and Alley (1984), and Kahle, Gillespie, Abbott, Abrams, Walker, Hoover, and Lockwood (1988). Occasionally, however, members of this community did involve itself more directly in the applications of remote sensing. A prominent example among this group was Michael Abrams, who was the principal investigator of the "Joint NASA/Geosat Test Case Project," a public/private cooperative investigation of remote sensing for mineral and petroleum exploration. See Abrams,

These researchers shared more than their scientific interest in thermal infrared instrumentation and data analysis, however. They shared practices and understandings that regarded remote-sensing research and development as a technical activity that demanded acumen for integrating scientific and technological aspects of remote sensing, a variety of project management skills, and entrepreneurship. An extensive history of collaborating together, a history that spanned over fifteen years in many cases and that included proposing and carrying out together "mission-oriented" projects for NASA such as those mentioned above, reinforced these shared practices and understandings among this community of geologic remote-sensing scientists.

For the remote-sensing missions outlined above, this community of scientists had taken on leadership positions that had involved them at one time or another both in scientific work, such as geological field work, and in instrument design and development. The term "technoscience" appropriately describes the hybrid nature of their work. When NASA awarded a contract to the researchers at JPL for a space-based or aircraft-based remote-sensing investigation, these researchers sometimes subcontracted outside of JPL for the engineering, instead of doing the work in-house. Nevertheless, the JPL scientists, as the "principal investigator," "co-principal investigator," or "instrument scientist," managed and were responsible for that work. In particular, Dr. Anne Kahle, who would become the team leader for the U.S. science team for the ASTER instrument had been both a principal investigator and an instrument scientist. Because all of the scientists who would become part of the U.S. ASTER team worked on a contract basis, often dividing their time among several contracts, and because for many of the scientists these contracts paid for a large portion of their positions, tasks such as proposal writing, project management, and

---

Conel, and Lang (1985). He and Alan Gillespie were also vice-presidents in the 1970s of GeoImages, Inc., of Altadena, California, a firm that provided mineral and petroleum exploration companies with computationally-enhanced satellite data and data interpretation services.

keeping patrons pleased were an integral part of their technoscientific practice, whether they liked it or not. The character of their work was, as some JPL scientists themselves put it, “entrepreneurial.”[8]

This way of carrying out their business of remote-sensing research and development brought with it a degree of organizational autonomy and professional independence. Michael Abrams was a geologic remote-sensing scientist at JPL and a core member of the U.S. science team for ASTER. When Anne Kahle retired in 2003, he became the team leader for the U.S. science team. In an interview in 2001, I asked Abrams if any of the re-organizations at JPL in the 1990s affected his work. He replied:

> We’re pretty much immune from any re-organizations that happen here [at JPL]. It doesn’t matter very much what goes on above us . . . this divison here, division 32, is the science division. And it’s different than the rest of the lab. Many of the people here are self-supporting, entrepreneurial types like myself. I am self-funded, for the most part. 90% of the lab is not that way. Ninety percent of the lab punch a time clock and know they are hired by somebody to do an engineering job, and they don’t go out and get the money. . . . so we are kind of an anomaly here. But because of that we’re sort of isolated from managerial changes at the top. Reorganizations [and such]—bla bla bla. And it’s been that way for the 30 years since I have been here.[9]

As Abrams’s use of “we” and as that last remark about 30 years both hint, while the way this community of scientists conducted and financially supported their research might have provided these scientists with some organizational distinction and autonomy vis-à-vis JPL, the individual researchers at JPL were not transient free agents who worked alone or relocated their office to wherever they might find work. While they did “go out and get the money,” they almost always got the money from NASA, albeit through a competitive, peer-reviewed process. In addition, they are part

---

8 Abrams (2001), Palluconi (2001), and Kahle (2003a).

9 See Abrams (2001). Abrams’s comments about the group’s autonomy vis-à-vis JPL were in agreement with comments made by Palluconi (2001) and Kahle (2003a).

of a long-standing community at JPL whose members know each other well and are professionally identified as members of the geologic remote-sensing community at JPL. Almost all of the U.S. ASTER science team—and not only the dozen or so scientists who were or who had been at JPL—previously worked together in some significant way at one point or another before the ASTER project. They had together authored well-regarded (and widely-cited) joint textbooks and publications. On occasion these articles made the cover of a prestigious journal, displaying the latest in remote-sensing imagery.[10]

In the late 1980s, when the ASTER collaboration was just beginning, the situation was different in Japan. While the central figure in the business of developing geologic remote sensing at JPL was the scientist serving as principal investigator,[11] the central "figure" of government-sponsored geologic remote-sensing development projects in Japan was the program manager—but in the plural. These program managers worked at non-profit corporate consortia (*zaidan hōjin)* that were established under the jurisdiction of a government ministry, Japan's Ministry of International Trade and Industry (MITI). The "centrality" of the program managers in Japan,

---

[10] Their tight professional relationships are of course no coincidence. As will be explained below, for the most part, Team Leader Anne Kahle and her Deputy Team Leader, Frank Palluconi, invited scientists with whom they had worked previously, and with whom they were personally familiar, to join what became the U.S. ASTER science team. See Palluconi (2001). For some conspicuous co-authored publications, see Abrams, Ashley, Rowan, and Goetz (1977); the textbook *Remote Sensing in Geology*, Siegal and Gillespie (1980); Kahle and Goetz (1983); and chapters 13 and 31 of *Manual of Remote Sensing*, Colwell (1983).

[11] This characterization of the central importance of the principal investigator in geologic remote sensing at JPL should not be taken too far. In particular, it should not be used to reduce the process of research and development to just that individual. Principal investigators could, and did, delegate their authority and responsibilities across a team and were, at times, institutionally required to do so. For example, principal investigators worked hand-in-hand with individuals who were the "project managers" employed by the responsible institutions such as JPL and Goddard Space Flight Center. The project manager often was counted upon to keep a closer eye on important organizational and administrative matters such as budgeting, documentation, scheduling, and compliance with institutional regulations. It is also important to note that not all remote-sensing instruments sponsored by NASA were instruments proposed by principal investigators. Some, like HIRIS and MODIS, were instruments proposed by NASA facilities which later came to have lead instrument scientists.

however, was of a much different kind than that of the principal investigator in the United States.[12] While a principal investigator in the United States was the central authority who shaped the character of his or her instrument and scientific investigation, program managers at non-profit corporate consortia in Japan were managing an enterprise that came to them largely not of their own making. Their authority was more circumscribed by the institutions and bureaucratic system of which they were a part.

The remote-sensing research and development project that the program manager in Japan found himself overseeing almost always came about through an extended committee-based bureaucratic process. This process involved the corporations that partly sponsored the relevant consortia, the government ministry that funded the project, informal advisors for the ministry or consortia, and the contractors who carried out the engineering of the project—contractors who may or may not have been members of a program manager's consortium. If scientists who were not employed by one of the consortia or corporations, such as academic scientists, participated in this development process, they served as advisors to the consortia on behalf of the government ministry. Since these non-corporate scientists were typically working at national institutes or national universities, they were already civil servants, and consequently, they were not given additional compensation for the significant time they spent advising the government's and corporate consortia's research and development project.[13] As civil servants, they were supporting a national project.[14]

---

[12] This introduction to the institutional circumstances of program managers in MITI's *zaidan hōjin* in the late 1980s and to how they were advised by scientists and engineers draws upon accounts from numerous interviews, mainly conducted in Japanese, including Yamaguchi (2002), Fujisada (2003), Ishii (2003), Kudoh (2003), Takenouchi (2003), Watanabe (2003), and Yokota (2004).

[13] This practice was apparently not standard across ministries in Japan. Like MITI, the Science and Technology Agency under the Ministry of Education during this time did not compensate researchers from national institutes for their support of government projects, but unlike MITI, the Science and Technology Agency did provide minimal compensation for consulting carried out by researchers at national universities. See, Satō (2005).

Program managers took into account the advice of these scientists to the extent that these scientists were able to influence the decisions of the formal committees of the program manager's consortium, committees in which these scientists might have sat as members or even perhaps chaired. In sum, a program manager for a remote-sensing research and development project sponsored by the Government of Japan faced a more complex collection of overlapping interests and users than the typical principal investigator in the United States, largely by virtue of the program manager being embedded in and working for a consortium that was an institutional meeting ground for the Government of Japan and various industry corporations.

One such public interest, non-profit industry consortium was the Earth Resources Satellite Data Analysis Center (ERSDAC), which was founded in 1981 under the jurisdiction of Japan's MITI.[15] It was one of many new organizations established in response to the oil shocks of the 1970s. ERSDAC came to play an important role in Japan's geologic remote-sensing community: the consortium articulated and advanced the concerns of industrial users in the development and operation of remote-sensing instrumentation in Japan. ERSDAC's founding board of directors included executives from oil and mining firms such as the Arabian Oil Company (a firm incorporated in Japan), the Dowa Mining Company, the Indonesia Oil Company (again, a firm incorporated in Japan), the Japan National Oil Corporation, the Japan Petroleum Exploration Company, the Mitsubishi Metal Corporation, the Nittetsu Mining Company, and the Sumitomo Metal Mining Company. The board included high-level representatives from industry associations such as the Japan

---

[14] See, Yamaguchi (2002), Fujisada (2003), and Yokota (2004).
[15] ERSDAC was legally founded as a *zaidan hōjin* (a juridical foundation), which itself was a type of *kōeki hōjin* (public-interest corporation). ERSDAC's Japanese name was *shigen kansoku kaiseki sentā.* See Sōritsu jūnenshi henshū iinkai (1993).

Machinery Federation, the Japan Petroleum Development Association, and the Society of Japanese Aerospace Companies, as well as from government agencies such as the Metal Mining Agency of Japan. At its founding, ERSDAC's board of twenty-three directors also included two university professors, both of whom were from the faculty of engineering at the University of Tokyo and had former students who worked for some of ERSDAC's corporate sponsors.[16] In the 1980s, ERSDAC received grants from MITI, government agencies, and industry associations to carry out studies on the use of remote-sensing technologies for exploiting non-renewable energy and mining resources as well as studies on the development of remote-sensing data processing and analysis systems.

Another public-interest, non-profit industry consortium under the jurisdiction of MITI was JAROS, or the Japan Resources Observation System organization.[17] Toshiba, NEC, Hitachi, Fujitsu, and Mitsubishi Electric together established JAROS in 1986 for the particular purpose of promoting and coordinating, supposedly on behalf of MITI's Space Industry Division, the development of remote-sensing sensors. JAROS's scientists and engineers, with the support of academic and government researchers in an advisory capacity, would manage the hardware engineering of ASTER.

ERSDAC and JAROS were both bolstered—in terms of their budgets, capabilities, and expertise—by their central involvement in the development and operation of Japan's first land remote-sensing satellite system, a system called the Japan Earth Resources Satellite number 1 (JERS-1). The development of JERS-1 in

---

[16] Sōritsu jūnenshi henshū iinkai (1993: 97).

[17] JAROS is an acronym for JApan Resources Observation System organization. A more literal translation of the Japanese name of the organization is "earth resource observation systems development organization" (the Japanese is *shigen tansa-yō kansoku sisutemu kenkyū kaihatsu kikō*). JAROS was preceded by the Technology Research Association for Resources Remote-Sensing Systems. See Kōkōgyō gijutsu kenkyū kumiai kondankai (1991: 102).

the early-to-mid 1980s illustrates how Japan's institutions tackled the development of geologic remote-sensing systems before the U.S.-Japan collaboration on ASTER came onto the scene in the late 1980s. The JERS-1 sensors were built by MITI with two mission objectives in mind: the development of remote-sensing sensor technology and the geologic investigation and exploitation of non-renewable resources.[18] The latter objective allowed MITI's Space Industry Division to fund its primary interest—the "nurturing" of remote-sensing technology—from a special account whose source of revenue was an import tax on petroleum.[19] The petroleum tax account was dedicated to expenditures that worked toward maintaining a stable reserve of oil for Japan and developing energy-saving technologies.[20]

By using funds from this special account to sponsor—through ERSDAC—the development of technologies and methods for utilizing data from JERS-1 (e.g., a data and information system), MITI was able to assist the same companies upon which the petroleum import tax was levied. More important for MITI's Space Industry Division, however, was that its sponsorship of the JERS-1 system also benefited the division's primary sector of industrial responsibility—aerospace and space electronics firms. Yokota Makoto, who was Deputy Director of MITI's Space Industry Division from 1989 to 1992, recalled that:

---

[18] See Ishizawa, Takamura, Saito, Niwa, Kuramasu, and Iwai (1986: 291), Jinkō eisei kaihatsu honbu chikyū kansoku eisei gurūpu (1991: 1-1), Kudo, Fujisada, Kato, Hashimoto, and Hino (1992: 319), and National Space Development Agency of Japan (1992: 11). The concept of the JERS-1 instrument was a union of a concept for a "land satellite" that was to be developed by the National Space Development Agency and a "mining and earth resources satellite" that was to be developed by MITI. The National Space Development Agency developed the JERS-1 launching system and satellite bus and MITI's consortia developed the JERS-1 mission instruments. For a brief account of the compromise that led to the JERS-1, see Nakayama's "prehistory" (1986: 3).

[19] The Japanese verb *sodateru* (to rear, to bring up, to raise, to nurture, to foster, to grow), as well as the orthographically related word *ikusei-suru* (to cultivate, to train, to nourish), was used overwhelming in primary source materials and by interviewees to describe MITI's relationship to the aerospace industry. See, for example, Fujisada (2003), Ishii (2003), Kudoh (2003), and Takenouchi (2003).

[20] See Ishii (2003), Watanabe (2003), and Yokota (2004).

> The direction of MITI during this time [the mid-to-late 1980s] emphasized the development of new instruments but without any adjustments to infrastructure. It was the way of thinking in the mid-1980s that MITI would develop new instruments, and provided their capabilities were validated, users (and here I [Yokota] am assuming private companies) would later probably manufacture and operate the earth observation system for themselves.[21]

If Japanese geologic remote-sensing scientists outside corporations were going to be a part of the development of remote-sensing sensors and systems in Japan, they would need to do so by involving themselves as advisors for projects that were carried out by consortia such as ERSDAC or JAROS. Otherwise, national institute and university scientists were confined to just using imagery produced by satellites over whose design and operation they had no influence, much as they had been previously confined in their use of the U.S. Geological Survey's Landsat satellites or the satellites of other states since the mid-1970s.[22] Japanese scientists who in the late 1980s became involved with the ERSDAC and JAROS consortia generally conducted research that focused on image analyses that were heavily dependent upon mathematical techniques and simulations. That is, during the early-to-mid 1980s, these scientists rarely carried out empirical investigations that involved intimate knowledge of, or coordination with, remote-sensing instrumentation.[23] Moreover, as long as Japanese scientists were only data users who did not exercise influence over the design, development, and operation of instrumentation, not only was their research limited in methodological form, but *what* they could research was also restricted by the design

---

[21] See Yokota (2004).

[22] For a brief of remote-sensing activities in Japan during that time, see Sakata (1980).

[23] See, for example, Tsuchiya, Arai, and Ishida (1982), Yasuoka and Miyazaki (1982), Ishii and Rokugawa (1982), Yamaguchi (1984), Arai (1985), Urai, Kōda, Satō, and Tsu (1985), and Ishii, Rokugawa, and Katō (1988). The very few exceptions to this generalization typically involved the use of aircraft-based sensors rather than space-based sensors. See Suyama, Ishii, Yamaguchi, Kamata, Hase, and Ogawa (1982).

choices of others—such as the engineers working for the manufacturers who might have different priorities for the instrument's capabilities.[24]

Regarding this point, the contrast in the early-to-mid-1980s between the practices of geologic remote-sensing scientists in Japan and the scientists at JPL could not have been starker. During this time, the remote-sensing scientists at JPL who were working under principal scientist arrangements exercised much more control over instrumentation, instrumentation that produced the data that they would use to create knowledge about the earth. In the 1980s, the JPL scientists were accustomed to being major players in deciding what phenomena their instruments could measure and when and where "their" instruments would measure the phenomena of interest. Their research not only used the instrument to characterize phenomena, but also, owing to their control of the instrumentation, they were able to use phenomena to characterize the performance of the instrument. Because the geologic remote-sensing scientists in Japan, until the late 1980s, had little control or influence over the development and operation of the remote-sensing instrumentation that provided the data that they used, they were generally relegated to the position of trying to put to use data that had been made available by others. Their research practices rarely opened the "black boxes" of remote-sensing sensors in the way that the work of the JPL scientists did.

Control over instrumentation in Japan in the early-to-mid-1980s was generally the province of the program managers and manufacturers who were at the helm of Japan's institutions for developing remote-sensing technology, namely Japan's

[24] See Yamaguchi (2002). The importance of integrating matters of instrumentation (presumably the domain of "engineering") into the work of remote-sensing analysis (which was considered a "science") was still an issue worth calling attention to for ERSDAC scientists as late as the early 1990s. See, for example, Arai Kōhei's recommendation to train what he called the "sciengineer" in ERSDAC News ("sciengineer" was alphabetically written in what otherwise was Japanese text) (1992).

National Space Development Agency (NASDA) and JAROS,[25] and the aerospace and electronic manufacturers, such as NEC and Mitsubishi Electric who were regular contractors for NASDA and who were founding members of the JAROS consortium.[26] For the JERS-1 satellite, NASDA had been in charge of the design and development of the launch vehicle and the satellite bus (the “bus” carries the remote-sensing mission instruments in orbit). NASDA had also initially researched JERS-1’s mission instruments—that is, the synthetic aperture radar, the visible and near infrared optical sensor, the shortwave infrared optical sensor, the data transmitter, and the data recorder. The development of the mission instruments was handed off to JAROS’s predecessor organization, the Technology Research Association of Resources Remote-Sensing System, when that organization was established in 1985, and subsequently to JAROS.[27] JERS-1’s preliminary design review was held in early 1987 and the critical design review was conducted in early 1989. ERSDAC, however, did not become involved with analyzing the utility of the instrument for its users until around that time, much too late in the development process to have an influence over the instrument’s fundamental design.[28] Potential industrial users of JERS-1 who were associated with ERSDAC and scientists at Japan’s national research institutes (such as at the

[25] Including JAROS’s predecessor, the Technology Research Association of Resources Remote-Sensing System. See Kōkōgyō gijutsu kenkyū kumiai kondankai (1991: 102).

[26] While firms such as NEC and Mitsubishi Electric were contractors for both NASDA and JAROS, NASDA and JAROS were differently involved in remote-sensing system development and actually differed in their legal status as institutions. Despite the official English translation of its name, NASDA was not a government agency. It was a quasi-public special corporation under the jurisdiction of Japan’s Science and Technology Agency and was involved in developing launch vehicles, satellite buses, and sometimes, remote-sensing sensors. As has been already discussed, JAROS was a private non-profit industry consortium under the jurisdiction of MITI which was specifically tasked with developing remote-sensing mission instruments, such as remote-sensing sensors.

[27] See Ishizawa, Takamura, Saito, Niwa, Kuramasu, and Iwai (1986: 297-8), Kōkōgyō gijutsu kenkyū kumiai kondankai (1991: 102), Kudo, Fujisada, Kato, Hashimoto, and Hino (1992: 319, 321), and National Space Development Agency (1992: 21).

[28] See Koizumi, Suzuki, Kobayashi, and Kakuichi (1989: 65), Sōritsu jūnenshi henshū iinkai (1993: 102), National Space Development Agency (1992: 21), Saitō (1992a: 27), Yamaguchi (2002), and Watanabe (2003).

Geological Survey of Japan) who were also potential users of JERS-1 remember considering JERS-1 in the late 1980s as a satellite heavily influenced by what they viewed as the technology development goals of NASDA and NASDA's engineering contractors.[29] Over a decade later, one prominent geologist charged that, throughout the 1980s, "NASDA was not user oriented" and was more interested in building rockets than in designing good remote-sensing satellites.[30]

In the mid-to-late 1980s, prominent geologic remote-sensing scientists at the Geological Survey of Japan and at national universities viewed ERSDAC as an institutional mechanism through which their voices could influence the development and operation of Japan's remote-sensing satellites.[31] They knew that ERSDAC had been striving to articulate and advance the voice of users in the development and operation of Japan's remote-sensing satellites. They also reasoned, owing to the fact that MITI used the petroleum import tax to fund the development of remote-sensing satellites, that the voices of the users from the oil and mining industry—the voices whom ERSDAC institutionally represented—would be difficult for MITI to ignore.[32] Thus, while realizing that the scientific and technological interests of oil and mining

[29] Yamaguchi (2002) and Watanabe (2003). A research engineer who advised JAROS on JERS-1's hardware also judged that the JERS-1 project had been focused on technology development. See Fujisada (2003).

[30] Yamaguchi (2002). While it is not relevant to my description of the geologic remote-sensing community in Japan whether or not Yamaguchi's assessment of NASDA was accurate or not, NASDA's fifty-nine page description of the JERS-1 satellite that was distributed to the press at the satellite's launch did dedicate far more pages to discussing the satellite's launch vehicle and the tracking and control system than it did to the satellite's mission instruments, such as the remote-sensing systems. See National Space Development Agency (1992). Yet, without access to the minutes of JERS-1's specifications committee(s), it is difficult to definitively say to what extent the design of JERS-1's mission instruments did and did not take into consideration the voice of users. JAROS officials, as would be expected, have written that "requests from the petroleum resource exploration community" were taken into consideration (e.g., Kudo, Fujisada, Kato, Hashimoto, and Hino 1992: 321). Still, the design parameters of JERS-1 were, without a doubt, established before most of ERSDAC's studies of the instrument. See, for example, Koizumi, Suzuki, Kobayashi, and Kakuichi (1989: 65) and the comments of Saitō (1992a: 27).

[31] Yamaguchi (2002), Yasuoka (2002), and Ishii (2003).

[32] This strategic assessment is most clear in Yamaguchi (2002) but see also Ishii (2003).

companies could very well at times diverge from their own[33] and that the interests of oil and mining companies would likely dominate the institutional environment of ERSDAC, many research scientists at Japan's national institutes and universities who had been asked by MITI to support its national projects by providing advice through ERSDAC chose to do so and thus to be a part of the system.

One scientist from the Geological Survey of Japan spoke of the excitement ("*shigeki"*) for him and his colleagues of possibly having the opportunity to contribute to the building of what they had hoped would be better remote-sensing systems for users.[34] What "better" meant at the time was a remote-sensing system that, in particular, took more seriously complementarities among multiple sensors, the selection of the sensors' bands of wavelength sensitivity, instrument calibration, and data processing and distribution, especially the distribution of calibrated data rather than requiring users to calibrate their own data.[35]

Although the preferences and practices of researchers and users affiliated with ERSDAC in the late 1980s did diverge, at times in specific ways, as a community of practice they had much in common, especially when contrasted with the community of geologic remote-sensing scientists at JPL during the same period of time. First, as has already been discussed, whereas the scientists at JPL had been considerably involved in matters of instrument design and operation, the community of geologic

[33] For example, some oil and mining firms expressed a preference on occasion to buy data from foreign instruments rather than domestically develop instruments that provide data of lesser quality. See Ishii (2003) and Watanabe (2003).
[34] The quote is from Yamaguchi (2002).
[35] For example, the three sensors of JERS-1 were, at one time, going to be launched separately into different orbits rather than orbit together on the same satellite bus, where they would make simultaneous observations. Such a choice, if it had been made, might have been advantageous for testing hardware, but it would have been tremendously disadvantageous for drawing conclusions about an area of interest which would make use of more than one instrument. Moreover, owing to the timing of the studies, the selection of bands of wavelength sensitivity for JERS-1 did not benefit from extensive simulations using aircraft based sensors. See Watanabe and Tsukada (1989), Watanabe and Tsukada (1992), Saitō (1992a: 27), Watanabe, Mills, Sano, and Tagawa (1993), Yamaguchi (2002), Ishii (2003), and Watanabe (2003).

remote-sensing scientists associated with ERSDAC generally had not been involved in instrument design and operation—although as a community they desired to be. Unlike the scientists at JPL in the 1980s, the researchers at ERSDAC in the mid-to-late 1980s did not identify with a specific research program or research tool (as many of the JPL scientists identified with thermal remote sensing). Rather, the researchers affiliated with ERSDAC identified with a particular community of users, users who were a part of the natural resources exploration and exploitation industry.

For a volume put together to commemorate ERSDAC's tenth-year anniversary celebration, a volume which was circulated only in-house to those who had been affiliated with ERSDAC during its first ten years, many contributors wrote of the importance of ERSDAC for bringing meaning and substance to remote-sensing technology by connecting the tool of remote sensing with users. Professor Nakayama of Hiroshima Institute of Technology, who served as the chair of an ERSDAC advisory committee for research from 1982 into the 1990s, wrote that:

> The only problem [with remote sensing in Japan before ERSDAC] concerned the various users of [remote-sensing] data. If [remote sensing] does not respond to high-level demands from the user, it is meaningless. That is, even if we distribute data as [free] allotments, as uniforms passed out to employees, I hear that it would not be welcomed by real users. Raising the level of users from various fields grows new technologies of earth observation and causes these new technologies to become established; furthermore, it has been recognized [at ERSDAC] that it is also an urgent policy and is the best policy for securing funds.[36]

Yamazaki Akira, the director of MITI's Space Industry Division in the early 1990s, wrote that ERSDAC was "a central pillar of MITI space policy;" that the consortium had raised "expectations for the utilization of remote sensing;" and that ERSDAC was "the center of a leap forward" in the use of remote sensing. He, like Professor

[36] See Sōritsu jūnenshi henshū iinkai (1993: 72).

Nakayama and many other contributors to the volume,[37] pushed for ERSDAC to have a future research agenda that connected with users:

> . . . these things will become necessary [for ERSDAC]: 1) digging up the needs of users in industry by accumulating remote-sensing research results that university and national research institutes have already come to perform and by making clear their effectiveness and availability in industry; 2) presenting all sorts of data that a user requires, such as acquisition times and acquisition areas concerning the upcoming JERS-1 remote-sensing data, together with also offering data related to methods for data use; and 3) to go and make a "user oriented" database built from the perspective of the industrial user, for the case the case of constructing a database for the purpose of the industrial use of future remote sensing.[38]

In sum, the geologic remote-sensing community of ERSDAC sought to shape the development of the remote-sensing instruments and systems that they used, and they did so to advance their usefulness to the industrial user community that supported ERSDAC.

A second commonality of the community of geologic remote sensing which was associated with ERSDAC was that this community regularly identified themselves as part of the state of Japan and as working together on national projects to advance Japan's state goals in the international arena, such as the exploitation of natural resources and the development of remote-sensing technology, which were ascribed with the explicit purpose of exploring for natural resources and exploiting them effectively. Unlike the community of geologic remote-sensing scientists based in JPL in the 1980s, the community in Japan during that time did not imagine themselves and their work as tied only tangentially to their state. Geologic remote-sensing

[37] See, for further example, the comments of Komatsu Kunio and Fukuhara Genichi at ERSDAC's tenth year anniversary party in the fall of 1991, as recorded in Sōritsu jūnenshi henshū iinkai (1993: 7-10). At the time, Komatsu was President of Japan's National Oil Corporation, and Fukuhara was the chairman of ERSDAC's board of directors and was from the Metal Mining Agency of Japan.
[38] See Sōritsu jūnenshi henshū iinkai (1993: 66).

scientists conceived of their relationship to the state in more intimate terms than contractual arrangements that merely enabled personal research programs. Furthermore, whereas before ASTER almost all of the geologic remote-sensing scientists at JPL had not worked on international projects on behalf of the United States and thus thought of "the international" in terms of geological field sites that happened to be abroad,[39] for the community of geologic remote-sensing scientists in Japan, the state and the international arena—in terms of politics, economics, and security—loomed larger in their conceptions of what it meant to do geologic remote sensing.

For example, despite Japan's economically heady days in the 1980s, the importance and legitimacy of calls to technologically "catch up" (*oitsuku)* and build a "nation founded on technology" (*kagaku-gijutsu rikkoku*) were both assumed. Professor Iijima Azuma of the University of Tokyo, who worked with ERSDAC in the 1980s and who chaired an advisory committee on remote-sensing technology in the early 1990s, recalled in his submission to ERSDAC's in-house tenth year anniversary commemorative volume that:

> I visited the U.S. for sixteen days in the fall of Showa 58 [1983], the third year of ERSDAC's establishment, and I cannot forget either having investigated many times the geology-related remote-sensing technical situation with Mr. Ofusa of Nittetsu Mining and with Mr. Sano of the center [i.e., ERSDAC]. . . . On an occasion when I visited [in the United States] a certain company and a TM image [that is, an image from the United States' Landsat Thematic Mapper remote-sensing sensor] which generally had not appeared on the market yet in Japan projected on a television screen, I was especially surprised at its clarity. I saw an image-processed picture and a partial magnification screen rapidly pop up in an instant, whenever the key of the computer was pushed. Although I thought that it would surely be interesting if I could do it with my own hand, it was very regrettable that the moment passed without a chance.
>
> My report of my visit to the U.S. nine years ago [in 1983] concluded with "Whether or not I feel heartened that our country is rapidly progressing

[39] See, for example, Abrams (2001) and Palluconi (2001).

> depends upon our country's development of the utilization of satellite remote-sensing technology for natural resource inquiry. As soon as possible, an independent resources observation satellite will be launched, and I just eagerly await its launch, so that analysis may be made with original data." *Fuy*ō-*1* [JERS-1][40] successfully launched on February 11th, 1992 and now my prayers have come true. ERSDAC has arrived at a new stage, and now I wish for more and more development.[41]

Professor Iijima's story was told as if it would be shared—as it almost certainly was shared—with an audience who would agree with the importance that his story placed on the development and use of remote-sensing technology for Japan's future. Moreover, the setting of a trip abroad to the United States, where technology was purportedly more advanced, was a circumstance that many in the audience would have readily recognized and with which they could have empathized.

Nagatani Hirokazu, who was at the time the head of the Geothermal Research Department at the Geological Survey of Japan and who had chaired ERSDAC advisory committees in the first half of the 1980s, told a similar story of progress as Professor Iijima. His tale emphasized not only catching up with the United States, but also the legitimacy and responsibilities that came to him and his colleagues who were associated with ERSDAC when state projects in which they participated through ERSDAC received international attention and involvement:

> Because the work that has constituted ERSDAC from the beginning, since its establishment, has been that of grasping and acquiring U.S. earth resource satellite applied technology that was ahead [of Japan], as it should have been, symposiums have often been held that have invited well-known scientists and specialists from universities and related organizations, such as the United States' NASA, Geological Survey, and JPL. On those occasions, we used to be deeply impressed, knowing that the use of satellite data founded on views of the earth were superior in the United States. In our country, in which space industry efforts were delayed for the purpose of establishing an early stage of independent technology, a developmental system that was led by the

[40] *Fuyō-1* is the Japanese name for the JERS-1 satellite. *Fuyō* is a lotus-like flower (*Hibiscus mutabilis*) commonly known as a "Confederate Rose" in the United States.
[41] See Sōritsu jūnenshi henshū iinkai (1993: 71).

> government was taken. . . . Now, the quality of the satellite image data offered by ERSDAC is wonderful. It could be said that we have already reached the world-class level in both image-processing and image-analysis technology, along with the printing technology which was excellent in our country. . . .
>
> At the Geological Survey of Japan with which I am affiliated, visits from earth resource specialists from developing countries have recently been rapidly increasing, and they place much expectation in our country's resource satellite plans. Moreover, the interest in ERSDAC's performance in data processing and analysis is similarly great. . . . How should we, who depend upon other countries for resources, contribute to the world, for the purpose of living together from now on? The role for an ERSDAC that seizes the precise key will no doubt become still larger. The next step [for ERSDAC] is very serious.[42]

Other geologic remote-sensing researchers who, in 1992 and 1993, submitted recollections to ERSDAC's in-house commemorative volume, including Japan's science team leader for the ASTER project,[43] told similar stories that lauded their efforts at ERSDAC during the 1980s in assisting with Japan's quest for natural resources, in advancing Japan's technological development *vis-à-vis* the United States, in bringing international recognition to Japan, and in contributing to the international community.[44]

A third commonality of the community of geologic remote-sensing scientists who were associated with ERSDAC was that much of this community's research in the mid-to-late 1980s focused on the shortwave infrared region of light, owing to its expected utility for oil and mining exploration and exploitation, which was after all the activity that ERSDAC had been funded to support.[45] The optical sensor of JERS-1, which was developed in the 1980s by NASDA and then JAROS, had three different

[42] Ibid., p. 73.
[43] Ibid., p. 74.
[44] For further examples including, but not exclusive of, geologic remote-sensing researchers, see Ibid., p. 5, 6, 10, 66, 72, 76, and 89.
[45] Yamaguchi (2002) and Watanabe (2003). And see Arai (1985), Yamaguchi (1987), Watanabe and Tsukada (1989), Urai, Satō, Ninomiya, Kōda, Miyazaki, and Yamaguchi (1992), and Satō (1992). ERSDAC researchers did sponsor hyperspectral work to simulate the bands that had already been selected for JERS-1. See, for example, Watanabe and Tsukada (1989), Watanaba and Tsukada (1992), Okada and Iwashita (1992), and Watanabe, Mills, Sano, and Tagawa (1993).

bands of sensitivity in the visible and near infrared region, four in the shortwave infrared region, and none in the thermal region of electromagnetic radiation. The visible bands, with stereo capability in one band, were anticipated to be used for geographic reference and mapping, and the shortwave infrared bands would be used for geologic analysis.[46] In contrast, as has been discussed, the research of the geologic remote-sensing scientists who were based out of JPL generally investigated remote sensing techniques in the thermal region.[47]

In sum, those scientists and engineers who would become members of either "U.S. science team" or the "Japan science team" shared in the mid-to-late 1980s with most of the other members of their future national team a coherent bundle of techno-political practices, understandings, and commitments which contrasted with those of the other team. These two forms of life were to some extent indicative of the different institutional arrangements for doing geologic remote-sensing science in each state. The two sets of future members generally sought to craft remote-sensing techniques in different electromagnetic regions, one in thermal infrared and the other in shortwave infrared, in order to make different types of remote-sensing knowledge, knowledge that would in turn draw recognition from and serve different users and government patrons. What they researched and how they went about their research reflected and maintained different techno-political arrangements. A contract-based, entrepreneurial principal investigator in the United States—even one who, after being vetted through peer-review was largely at the mercy of one set of administrators and one basic source of funding—has a form of life that allows for technoscientific practices that open up the "black box" of technology to redesign and tweak

[46] See Jinkō eisei kaihatsu honbu chikyū kansoku eisei gurūpu (1991: 1-1) and Kudo, Fujisada, Kato, Hashimoto, and Hino (1992: 319, 321). Also see note 18.

[47] This is not to say that geologic remote-sensing scientists associated with ERSDAC did not occasionally conduct research in the thermal region (see, e.g., Ninomiya and Satō 1992), just that as a community they had built up much greater expertise in the shortwave infrared region.

instrumentation and operations in the name of better results for “science.” These results in the name of science presumptively lead to, among other things, increased recognition from a scientific community and a funding agency which have particular (but changing) ideas of what is scientifically interesting, significant, and credible.

The form of life of a university or government scientist in Japan who sits in an advisory capacity to industry consortia that develop and operate instruments in the name of serving national goals allows for, and encourages, practices that either distance the scientist from instrumentation (such as calculation and simulation) or foster cooperation with those who are more immediate to the instrumentation (such as committee-based analyses and team investigations undertaken with consortia program managers and contractors). Given the long-standing, specific goals of MITI’s Space Industry Division, the particular funding source that it draws from to support remote sensing, and the mission of the industry consortia under its jurisdiction, a researcher in this latter form of life exercises and expands his influence over a project by working with program managers in the consortia, or perhaps in MITI, and appealing to and advancing their goals. Owing to the interdependencies associated with the authority of program managers, their goals typically take into account the national goals and the goals of corporate members. After all, while the expertise of the scientist from the university or national lab might be compelling for many reasons, the consortia and its member corporations employ many researchers who are integral to the development and operation of an instrument and who outnumber the academic and government lab researchers within a given remote-sensing project.

In contrast, in the form of techno-political life shared by the group centered around JPL, a principal investigator arrangement already has delegated to that research group considerable authority. How power within that group is exercised can be contingent upon factors that are very particular to the dynamics of that group and the management of the principal investigator. National goals and the goals of users who are not professionally (i.e., socially) connected to that group, although they typically need to be taken into consideration—especially when government funding agencies call the principal investigator to account for his or her work—are not necessarily strongly-held commitments shared by researchers within the group and do not necessarily serve as resources for action. These were the two forms of techno-political life that the members of the U.S. and Japan teams lived in the mid-to-late 1980s. When pushed together, how the members of these two teams would work as a joint team was far from determined by their past practices, understandings, and commitments. Yet, even if these team members desired as individuals to cast off their respective forms of techno-political life to single-mindedly serve some designated national interest or to effect international cooperation, they would likely find it difficult to do so. To achieve something new, they first needed to work with what they knew.

The Origins of States' Goals

To realize state goals, the United States' NASA and Japan's MITI enlisted the above two geologic remote-sensing communities into a collaboration. This collaboration would build and operate what would become the ASTER remote-sensing system. Before core members of these communities met together in Tokyo in March of 1989 and presented their instrument concepts to each other, these two communities had not worked together to forge common understandings about their instrument's purpose, design, and capabilities. The two communities had not even shared or discussed with each other in a general way their previous work (at, say, international scientific conferences). Before the ASTER collaboration, while the research of the U.S. scientists was very well-known and was almost canonical to many of the scientists who would constitute the Japan science team,[48] the Japanese scientists (and engineers) were simply unknowns to the U.S. scientists.[49] What the U.S. scientists did know, however, was that the Japanese organizations, on whose behalf the Japanese scientists worked, seemed to have had acquired a slot to fly an instrument on a NASA earth observing satellite,[50] a slot that they wished they had themselves received, but had not. The JPL scientists and other scientists from the United States would need to work with, what were to them, relatively unknown Japanese scientists if they were going to take advantage of the opportunity that NASA's plans for a massive earth observing system offered for advancing their research objectives and their individual careers. Likewise, by flying an instrument on a NASA earth observing satellite, the Japanese scientists—and the earth resource bureaucracies that they advised and served—would need to be integrated in some to-be-determined way with NASA's earth observing system. To carry out this international cooperation and integration in a politically justifiable way, the U.S. and Japan scientists addressed, re-articulated, and in some cases, re-defined the state goals that they would work to realize through their development of the ASTER system. The state goals with which the U.S. and Japan science teams had initially been charged were not necessarily complementary or mutually-supporting; in practice, the goals of each state at times clashed.

EOS and the Origins of U.S. State Goals

The United States' goals for the ASTER collaboration largely followed from the instrument's importance to the success of a national "big science" project, NASA's

[48] Interviews with Yamaguchi (2002) and Matsunaga (2003).
[49] Interviews with Abrams (2001), Biggar (2001), Hook (2001), Palluconi (2001), Thome (2001), Kahle (2003a), and Pieri (2003).
[50] At that time, the earth observing "satellite" was conceived to be of such a size that it was considered a "platform" rather than a satellite.

earth observing system (EOS). In the late 1980s, NASA's plan for an earth observing system was the centerpiece of NASA's proposal for a "Mission to Planet Earth." In 1990, both EOS and the Mission to Planet Earth became classified as part of the U.S. Global Change Research Program. In the U.S. Global Change Research Program's fiscal year 1990 budget, spending on EOS comprised about seventy percent of the $660 million that was "focused" on global change research.[51] Although in the early 1990s, EOS became associated with climate change research and was typically discussed within the context of umbrella programs such as the Mission to Planet Earth and the U.S. Global Change Research Program, to think of EOS as something driven by high-profile and high-level programs would be historically inaccurate and would risk misunderstanding the United States' goals for EOS and, ultimately, the United States' goals for ASTER.

Planning for EOS pre-dated ASTER, the Mission to Planet Earth, the U.S. Global Change Research Program, and the United States' public discussion and national policy formulation regarding global change. More specifically, EOS was conceived before the national debate in the United States about the causes and significance of ozone depletion and global warming, and before the administrations of President Reagan or Bush confronted the issue of global climate change in terms of policymaking. In brief, unlike the ASTER collaboration, EOS—of which ASTER would be just a part—was not originally conceived to fulfill specific, pre-determined, and explicit goals of the United States as a state. The ASTER collaboration was in fact initiated by NASA and MITI for specific scientific and political purposes, purposes

[51] U.S. Global Change Research Program, Committee on Earth Sciences (1990). The Committee on Earth Sciences' compilation of budgets distinguished between funds "focused on" global change research and funds that "contributed to" global change research. The Committee on Earth Sciences was an interagency coordinating body for global change research which was established in 1987 under the Executive Branch's Office of Science and Technology Policy's Federal Coordinating Committee on Science, Engineering, and Technology.

which—for the United States—were intimately tied to bolstering the Earth Observing System. In 1989, however, the EOS concept itself was still very much evolving scientifically, technically, and politically. To understand the meaning and significance of the ASTER project for the United States' goals, we must understand how and when ASTER was tied to the United States' goals for EOS and to the evolution of EOS from a concept to a project.

EOS had been in the sights of NASA scientists and engineers as early as 1981, when an ad hoc NASA study group met to discuss how the Earth—including its land, ocean, and atmospheric environments—could be measured and characterized in a systematic, integrated, and synergistic way.[52] At the time, NASA's new managers, who had come in with the Reagan administration, were actively searching for and examining new missions for NASA. Burton Edelson, the new director of NASA's Office of Space Science and Applications under whose auspices the 1981 ad hoc study had convened, was attracted to the study's idea of a space-based global monitoring system both for its research value and for its potential as a way to shore up and advance his office's and NASA's mission. His boss, James Beggs, the new NASA administrator, was interested too. The space shuttle was supposed to shift soon to operational use, and while the space station as a presidentially-approved program was still years away, it was anticipated that if the platforms for earth observing instruments were tied to the space station program, EOS could then be seen as part of the space station program rather than as competing with it for funding dollars. Tepid support from the scientific community for the costly space station program could be strengthened. The collections of earth observing instruments on these large platforms would also require the space shuttle in order to be serviced. Finally, EOS as a new

[52] This study group was known in NASA parlance as "System Z" (Greenstone 1989:2). Anne Kahle, a future leader of the United States' science team for ASTER, was a participant in this study group. See, Kahle (2003a).

multibillion dollar program might have a better chance of getting off the ground if it were integrated with existing programs such as the space station.[53]

Other study groups sponsored in the early-to-mid 1980s by NASA and its Office of Space Science and Applications—study groups that had been organized under diverse charges but which had overlapping membership and interrelated ideas—endorsed concepts of a space-based earth observation system which were similar to those discussed by NASA's ad hoc group in 1981. The cluster of these study groups included: a NASA-sponsored conference at Woods Hole in 1982 on the earth's "habitability;" a study of the National Academy of Sciences' Committee on Earth Sciences in 1982 that recommended an EOS concept in its "Strategy for Earth Science from Space;" an internal NASA study in 1983 and 1984 of the science and mission requirements for the EOS concept that had been proposed by the other study groups; a large National Academy of Sciences Space Science Board study commissioned by NASA in 1984 to define the future of space science disciplines, including planetary and earth sciences; a NASA advisory group formed in 1983 called the "Earth Systems Science Committee" that operated in parallel with the National Academy of Sciences – Space Science Board study; and a working group of NASA, called the "EOS science steering committee," that built upon the EOS concept advanced in NASA's earlier 1984 internal study and that also drew upon the work of the "Earth Systems Science Committee."[54] All of the "earth observing system" concepts endorsed by these studies—studies backed by the names of the many well-regarded scientists who had

[53] See Edelson (1988: 6-7), Stevens (1990: C1), Taubes (1993: 912), and Lambright (1994: 99).

[54] Those studies were, respectively, Goody (1982), National Research Council (1982), NASA (1984), NASA Advisory Council (1986, 1988), National Research Council (1988), and NASA (1987b). Working groups of the International Council of Scientific Unions also authored studies in the mid-1980s supporting EOS as a concept (e.g., International Council of Scientific Unions 1986, 1988).

participated in them—sought to globally measure, monitor, research, and assess the earth as a holistic system.

NASA's science steering committee for EOS described in 1987 the over-arching objective of EOS as helping scientists "to quantitatively characterize our planet and provide the basis for a scientific understanding of the global system which is the Earth." [55] The science steering committee further asserted that "this understanding will enable us to predict the future state of the environment and how it will respond to natural events and human activities."[56] In 1988, after global climate change and its relationship to "human activities" had become subjects of congressional hearings and were prominently reported in the news,[57] the National Academy of Sciences study that NASA had commissioned in 1984 opened its final report not by focusing on just climate change, but rather by declaring in the broadest terms of all the previous studies its hopes for EOS and the Mission to Planet Earth concepts:

> We now have the technology and the incentive to move boldly forward on a Mission to Planet Earth. We call on the nation to implement an integrated global program using both space-borne and earth-based instrumentation for fundamental research on the origin, evolution, and nature of our planet, its

---

[55] NASA (1987b: v).

[56] Ibid.

[57] NASA scientist James Hansen testified to a Senate committee in 1988 that "it is time to stop waffling so much and say that the evidence is pretty strong that the greenhouse effect is here" and that he was ninety-nine percent certain that global warming was not a natural occurrence but what was caused by human activity. See, U.S. Senate (1988) and Shabecoff (1988a, 1988b). The *New York Times* reported in the first sentence of their cover story of Hansen's testimony that "the earth has been warmer the first five months of this year than in any comparable period since measurements began 130 years ago . . ." (Shabecoff 1988a: A1). The spring and summer of 1988 also had been experiencing "record-breaking" temperatures and droughts; because of which, forty percent of the counties in the United States had been declared disaster areas. These circumstances were of course noted in the national news coverage about global change research (e.g., Shabecoff 1988b: 1). With less publicity, NASA scientists James Hansen and Robert Watson had testified about global warming in 1986 before a subcommittee of the Senate Committee on the Environment and Public Works in which Robert Watson stated that "I believe global warming is inevitable. It's only a question of magnitude and time." See, U.S. Senate (1986) and Peterson (1986: A1). Robert Watson would later lead the United States' delegation in the Intergovernmental Panel on Climate Change.

> place in our solar system, and its interaction with living things, including mankind.[58]

With this broad scientific objective of studying the earth as a system by using EOS to take simultaneous, continuous, and synergistic measurements over the course of approximately fifteen years, NASA distributed in early 1988 its call for proposals for instruments and investigators that would compose its earth observing system.[59]

Whereas the broad and ambitious goals of EOS as a concept had been thoughtfully developed and outlined for the better part of a decade, for EOS to become something real in the world required more than a loose consensus among a cluster of study groups, even study groups that had involved dozens of prominent NASA and academic scientists. NASA's Office of Space Science and Applications and that office's recently-established Earth Sciences and Applications Division had been nurturing EOS through the planning process. But, as late as March 1989—one year after the call for proposals for EOS and a month after instruments and teams had been selected for the "definition" phase of EOS—the head of NASA's Office of Space Science and Applications still described to NASA's Senate oversight committee the Mission to Planet Earth as well as EOS as "just a NASA internal concept study" that "is clearly not part of the current U.S. Global Change Research Program, nor is it clear that we would ever implement it in its current conceptual framework."[60] Would NASA as an agency, the President of the United States, and Congress, support the funding of EOS? What would *their* goals be for EOS and its associated components, such as ASTER, and in what form would they support EOS? Up to this point in the almost decade-long existence of EOS as an "internal concept," NASA officials and earth scientists had not been required to explicitly and formally explain why either

---

[58] National Research Council (1988: iii).
[59] NASA (1988b).
[60] Prepared statement of Dr. Lennard Fisk in U.S. Senate (1989a: 10).

EOS—or the Mission to Planet Earth proposal within which EOS was being wrapped—should be of interest to the nation, the national treasure, and American society, rather than just of interest to NASA and earth scientists. NASA officials, however, began to make the case for both EOS and the Mission to Planet Earth as national programs in 1989.[61]

In forums in which the United States' national interests and goals were addressed and were spoken for—such as in Congress, in interagency and White House working groups, and in Presidential speeches—throughout the first few years of EOS as a formal component of the U.S. Global Change Research Program (i.e., from 1990 to 1992)[62] policymakers and NASA officials consistently portrayed EOS as contributing to three state goals: 1) the national and international demonstration of U.S. leadership in space; 2) the understanding and prediction of natural and human induced global change (and eventually, global climate change more specifically, which was increasingly being emphasized); and 3) promotion of substantive international cooperation in space, when such cooperation was deemed to be in accordance with other U.S. national interests.

The first goal of demonstrating U.S. leadership in space to national and international audiences was largely an outgrowth of several major evaluations of the United States' civil space program in the mid-1980s which had been charged with proposing a future vision for NASA after the completion (or termination) of its post-Apollo infrastructure projects that included the development of space communications networks, the space shuttle, and the space station.[63] The most influential of these reports with the Bush Administration and the U.S. Congress,

[61] The concurrent development of EOS and Mission to Planet Earth are addressed below.
[62] How EOS comes to be counted as part of the U.S. Global Change Research Program is described below.
[63] These evaluations include: National Commission on Space (1986), American Institute of Aeronautics and Astronautics (1987), and Ride (1987).

judging from the degree to which it was cited and its formulations were employed,[64] was the "Ride Report" in 1987.[65] Ride and her staff wrote that:

> "For two decades, the United States was the undisputed leader in nearly all civilian space endeavors. However, over the last decade the United States has relinquished, or is relinquishing, its leadership in certain critical areas . . . Leadership does not require that the U.S. be preeminent in all areas and disciplines of space enterprise. In fact, the broad spectrum of space activities and the increasing number of spacefaring nations make it virtually impossible for any nation dominate in this way. Being an effective leader does mandate, however, that this country have capabilities which enable it to act independently and impressively when and where it chooses, and that its goals be capable of inspiring others—at home and abroad—to support them. . . . Leadership results from both the capabilities a country had acquired and the active demonstration of those capabilities."[66]

In the view of the Ride Report, "a U.S. space leadership program" must not only "contain a sound program of scientific research and technology development" but must also "incorporate visible and significant accomplishments."[67] The report further continued to explain that "the United States will not be perceived as a leader unless it accomplishes feats which demonstrate prowess, inspire national pride, and engender international respect and a worldwide desire to associate with U.S. space activities."[68] As one of its four "leadership initiatives," the report recommended the "Mission to Planet Earth" and EOS.

The demonstration of U.S. leadership in space was, of course, a state goal that had motivated and directed national projects and policymaking for decades previous to EOS, especially at NASA. In the United States' all-consuming competition with the Soviet Union, space technology had served as symbols of the two polities and social

---

[64] See, for example, U.S. Senate (1989a:1-6) and U.S. Senate (1992: 2).
[65] Ride (1987). Dr. Sally K. Ride was a physicist, a space shuttle mission specialist, and the first American woman in space.
[66] Ibid., p. 12.
[67] Ibid.
[68] Ibid.

systems, and space achievements had served as benchmarks.[69] The Ride Report itself noted that the National Space Policy of 1982 [the last national space policy document of the executive branch prior to the Ride Report], a document which "establish[ed] the basic goals of United States policy," included in it the mandate to "maintain United States space leadership."[70] In 1989, the National Space Council was reconstituted to invigorate the United States' space programs; the President tasked the Vice President with chairing the council. The national space policy document that that council authored, called "National Space Policy Directives and Executive Charter," reaffirmed that a "fundamental objective guiding United States space activities has been, and continues to be, space leadership."[71] While other national space policy directives of the 1980s had highlighted "leadership" as an objective,[72] this national space policy document used language that closely resembled that of the Ride Report of two years earlier. The national space policy document stipulated that the U.S. civil space activities "shall contribute significantly to enhancing the Nation's science, technology, economy, pride, sense of well-being and direction, as well as United States world prestige and leadership."[73] Like other NASA programs from the mid-1980s through the early 1990s, policymakers in the White House and in NASA, and to a lesser extent, in Congress, drew upon the rhetoric of U.S. leadership to explain and to justify the Mission to Planet Earth and EOS as programs that would bolster U.S. leadership. In his testimony before a Senate oversight committee in 1989, an associate administrator of NASA listed five benefits of the Mission to Planet Earth and EOS; two of those five

---

[69] McDougall (1985), and see also Ezrahi's interpretation of a presentation of a Saturn 5 rocket at the United States' Kennedy Space Center (1990: 41-2). As mentioned in chapter two, the maintenance of U.S. leadership in aeronautics and space was listed as an objective for NASA in its founding legislation.

[70] As quoted in Ride (1987: 12).

[71] National Space Policy Council (1989).

[72] In addition to the Presidential Directive on National Space Policy of 1982, noted in the Ride Report, see for example, Bush (1988: 1).

[73] National Space Policy Council (1989: 2).

were that they were an "international effort in space demonstrating U.S. leadership" and that they were an "idea which stirs popular ideals, continuing the inspirational tradition of space activities."[74]

---

[74] Prepared statement of Dr. Lennard Fisk in U.S. Senate (1989a: 11). Of the remaining three benefits that Associate Administrator Fisk listed, one spoke to "preserving the global environment," another to "investing in space research for a purpose which everyone can see and appreciated," and the third to the "demonstration of U.S. commitment to investment in a better global future." Taken as a set, the five benefits listed by Fisk both presume and justify the three state goals for EOS described above. Because in March 1989 Fisk was still emphasizing that EOS was an "internal concept" (along with the Mission to Planet Earth), the benefits of EOS which Fisk enumerated in his testimony were rhetorical trial balloons. Neither EOS nor the Mission to Planet Earth were yet Congressionally-approved programs. Fisk was stating, in effect, that "if EOS and the Mission to Planet Earth were official NASA programs, these would be their benefits for the nation." In this testimony, Fisk was laying a rhetorical and political foundation for EOS and the Mission to Planet Earth as future NASA programs.

At this point, although the matter is tangential to this chapter's argument, some readers might want to know in detail how EOS and Mission to Planet Earth—two closely-related ideas that were intertwined in the late 1980s and early 1990s—did and did not overlap in terms of the NASA bureaucracy and Congressional funding. Previous to Congressional deliberations in 1989 over NASA's budget for fiscal year 1990, neither EOS nor Mission to Planet Earth was recognized in the line items of Congressional budgets. They were considered as plans that were bureaucratically "internal" to NASA. As of March 1989, the development of EOS was subsumed within NASA's space station program, and funding for the development of EOS would draw from anticipated increases in that program's line item in the NASA authorization bill (in the fiscal year 1990 appropriations bill, that funding was specified as allocations to the "polar platform" within the space station program. See Title III of H.R. 2916 in U.S. House 1989a). On the other hand, science projects that would use the Earth Observing System to conduct the Mission to Planet Earth were provisionally slated to be managed by the Earth Sciences and Applications Division of the Office of Space Science and Applications, drawing upon expected increases in that office's budget, the "Space Science and Applications" line item in the authorization bill (in terms of the appropriations bill for fiscal year 1990, that funding was specified as allocations for "earth sciences," see U.S. House 1989a). Unlike EOS, which in 1989 had a specific bureaucratic home, the "Mission to Planet Earth" umbrella plan—a plan that included EOS—was a broad theme that cut across NASA's bureaucracy. While the Office of Space Science and Applications was generally responsible for articulating that theme for NASA (as part of, for instances, NASA's preparations for the International Space Year in 1992), the implementation of the Mission to Planet Earth was not planned to completely fall under the authority of that office.

It was not until the fall of 1989, after President Bush called for the Mission to Planet Earth "initiative" in July, that either EOS or the Mission to Planet Earth was identified in the budget outline of a NASA authorization act (authorization bills authorize the government and its activities, provide policy guidance, and set maximum limits on spending for subsequent appropriations bills). How EOS and the Mission to Planet Earth would be recognized in the NASA authorization act was a matter that was not resolved by Congress in 1989. Congress was ultimately unable to pass into law a NASA authorization act for the upcoming year. The names of the line items in the different versions of the NASA authorization act—versions which the House and Senate sent back and forth to each other in their unsuccessful effort to pass a bill in the fall of 1989—indicate how for Congress in 1989 EOS was the core of the broad "Mission to Planet Earth" initiative.

In suggesting to the Senate that Mission to Planet Earth and EOS were inspirational activities, the NASA associate administrator was not being naïve, blindly optimistic, or maudlin about saving what the cover of *Time* magazine had called "Endangered Earth."[75] The administrator's rhetoric was reflecting the politics of the time. He was reminding Senators of what they already knew: understanding and predicting global change was a fast emerging national goal of the United States. Before the administrator's remarks, the chairman of the Senate Committee on Commerce, Science, and Transportation, Senator Ernst Hollings of South Carolina, had framed the committee's hearing on what the administrator called "just a NASA

In the House's first version of the 1989 multi-year authorization act for NASA, an act that would outline NASA's funding for fiscal years 1990, 1991, and 1992, neither EOS nor Mission to Planet Earth was recognized in line items in the budget. As "internal" NASA plans, the funding for those activities were subsumed under the line items for "space station" and for "space applications" (see, section 4(1)(F) of H.R. 1759 in U.S. House 1989c). That act was passed in the House in September. On the other hand, in the Senate's first version of the NASA authorization act in November, funding for EOS and for the Mission to Planet Earth was listed. The two were named together as "Earth Observing System of Mission to Planet Earth, $24,200,000 in order to complete Phase B activities [i.e., the definition phase] and to initiate Phase C/D of this program in fiscal year 1990" (the Mission to Planet Earth was not listed in any other line items, see Title I, section 101(a)(7) of S. 916 in U.S. Senate 1989b). Responding to the Senate's version, the House discussed a second version of the NASA authorization act, which was passed in November. As it was discussed in the House, it initially did not mention Mission to Planet Earth, but it did list EOS, with a line item of $24.2 million for "Global Change/EOS." In the engrossed version of the bill, more formal language was inserted which referenced the Mission to Planet Earth (see section 4(1)(G) of H.R. 3729 in U.S. House 1989d). Finally, the Senate's second version, which for procedural reasons was offered as an amendment to the Senate's first version of the act, took the words "global change" from the House's second version and inserted them before the line item that was listed in the Senate's first version, to finally read "Global change, $24,200,000 for the Earth Observing System of Mission to Planet Earth in order to complete phase B activities and to initiate phase C/D of the program in fiscal year 1991" (see section 4(1)(G) of S. 916 as amended by Amendment No. 1208 in U.S. Senate 1989c). Although the Senate—in particular, Senators Hollings and Gore who offered the amendment—were able to have the last word and included the "Mission to Planet Earth" language, global change and EOS were at this point the common denominators for the Congress, in terms of the politics of naming. In the end, for reasons much more significant than these semantics, the House and the Senate were unable to reconcile their two versions of a NASA authorization act. An authorization act for NASA was not passed into law for fiscal year 1990.

[75] In place of its annual "person of the year" issue, for the year of 1988, *Time* named "Endangered Earth" the "planet of the year." See "Planet of the Year" (1989).

internal concept" by testifying to that concept's manifest importance and broad scientific and political support:

> For nearly twenty years, since the first American left his footprints in the dust of the moon, this Nation has been in search of major goals for its civil space program. Many goals have been proposed, but none has succeeded in capturing the imagination of the American people and the world community as much as the Mission to Planet Earth. Maybe that's because of the renewed worldwide interest in global change and our fragile planet Earth, or maybe it's because of our basic desire to know more about ourselves and the planet we inhabit. Be it the timing, the proposed goals and objectives of the Mission, or its basic appeal, the concept of a Mission to Planet Earth has been recognized as a goal worthy of pursuit and as a priority within the civil space program's agenda. The concept of the Mission to Planet Earth has been endorsed by a broad range of scientific organizations and advisory groups with domestic and international composition, and it was endorsed by President Bush and Governor Dukakis during the 1988 presidential campaign.[76]

Indeed, later that year in July 1989 on the 20th anniversary of the Apollo 11 moon landing and on the steps of the National Air and Space Museum, when President Bush declared that the United States should commit itself to manned missions to the Moon and Mars, the President did not overlook the Mission to Planet Earth, even on such an occasion celebrating manned space exploration:

> As I said in Europe just a few days ago [at the Paris G-7 Economic Summit], environmental destruction knows no borders. A major national and international initiative is needed to seek new solutions for ozone depletion and global warming and acid rain. And this initiative, "Mission to Planet Earth," is a critical part of our space program. And it reminds us of what the astronauts remember as the most stirring sight of all. It wasn't the Moon or the stars, as I remember. It was Earth—tiny, fragile, precious, blue orb—rising above the arid desert of Tranquility Base.[77]

The Moon and Mars manned exploration "leadership initiatives"—to reprise a term from the Ride Report—were ultimately not supported by Congress because of

[76] U.S. Senate (1989a: 1-2).
[77] Bush (1989). The Senate also endorsed the President's statement with a "sense of the Senate" resolution. See U.S. Senate (1989d).

numerous competing domestic spending priorities for fiscal year 1991, but Congress did follow through at that time and support the bulk of the Mission to Planet Earth and EOS.[78]

Congress was not only a supporter of research on global environmental change but, unlike the Reagan and Bush administrations, a clear majority of the Congress was also supportive of policymaking that addressed, and that aspired to respond to, what was already known about global change and what could be known about global change in the near future. Back in December 1987, Congress had passed the Global Climate Protection Act to try to spearhead the development of climate change policy by authorizing the State Department and the Environmental Protection Agency to develop such policy (the Reagan Administration had opposed, but signed, the bill).[79] When that proved ineffective at instigating executive action, in 1989 and 1990 the Congress deliberated upon and eventually passed the Global Change Research Act. The act required "the establishment of a United States Global Change Research Program aimed at understanding and responding to global change, including the cumulative effects of human activities and natural processes on the environment, to promote discussions toward international protocols in global change research, and for other purposes."[80] While it would have been inappropriate for this legislation to mention Mission to Planet Earth or EOS by name since those concepts were not yet official programs when the language was drafted, the act did require the new interagency coordinating body that it established to develop a "National Global Change Research Plan" that included "global measurements, establishing worldwide observations necessary to understand the physical, chemical, and biological processes

---

[78] See Rogers (1990), U.S. House (1990a), and U.S. Senate (1990b)
[79] U.S. General Accounting Office (1990a: 19).
[80] Preamble of U.S. House (1990b).

responsible for changes in the Earth system on all relevant spatial and time scales."[81] The Global Change Research Act of 1990 and other legislation, including appropriations, affirmed the concept of EOS as a system to study global change.

The White House, Congress, NASA, interagency coordinating committees, and prominent scientists who held positions on key advisory committees all agreed that NASA should deploy numerous space-based instruments to observe the earth for the purpose of learning about global change, especially global climate change. National Research Council Chairman Frank Press was reflecting, not building, a national consensus when he stated in a letter to the President's science advisor in September 1990 that "continuous, long-term, space-based observations of fundamental environmental parameters are essential."[82] In a June 1990 article that surveyed the EOS program, including the concerns of its critics, the *New York Times* was able to judge that "the project has attracted broad political support, including the strong backing of President Bush."[83] This national consensus of course did not preclude extensive debate on the technological design of EOS and funding for the system. The *New York Times* article went on to quote John Logsdon, the director of the Space Policy Institute at George Washington University, as saying that "In principle, I think it's beyond criticism. We have the tools to understand the planet on an earthwide basis as a system, and that's an understanding we need to have. The criticism focuses on how you go about doing that."[84]

---

[81] Section 104(c) of U.S. House (1990b).
[82] Asker (1990).
[83] Stevens (1990: C1). In December 1990, the Advisory Committee on the Future of the U.S. Space Program, more commonly known as the "Augustine Commission" after it chair, also endorsed in its final report the importance of the concept of Mission to Planet Earth and EOS for NASA and the United States. See, Advisory Committee on the Future of the U.S. Space Program (1990).
[84] Quoted in Stevens (1990: C1).

Aside from this debate among experts regarding the proper scientific and technological means to realize the national goal of studying global change,[85] there was the blunt matter of funding, a matter which was largely to be determined by Congress. Between the summer of 1990, when appropriations for fiscal year 1991 were debated, and September 1992, when NASA spending for 1993 was approved, EOS directly competed against—and lost a sizeable proportion of its budget to—specific domestic spending priorities that were under the purview of the same Senate appropriations subcommittee, such as the rising costs of veterans' health care. EOS also indirectly competed against general national spending during that time, such as that for the first Gulf War, Operation Desert Storm. Unlike many other high-profile NASA initiatives, EOS did survive the cost-cutting, yet it nevertheless became recognized by its NASA program office as a "cost-driven project" which was to be done "faster, better, cheaper," under the managerial philosophy of the new NASA administrator, Daniel Goldin, who had just joined NASA in the spring of 1992.

In about two years, the concept of EOS went from being two massive orbiting platforms carrying dozens of instruments charged with studying global change at its broadest, with a projected decadal cost of $17 billion, to an overlapping series of six satellites, each carrying several instruments, that, if considered on the whole, were focused on climate change, at a projected cost of $8 billion.[86] From the standpoint of specific scientific objectives, the focus on global climate change came at the expense of solid earth geophysics and stratospheric chemistry.[87] Amid this radical restructuring of EOS between 1990 and 1992, the state goal of understanding and predicting global change, particularly global climate change, proved itself rhetorically

---

[85] For the public face of those debates, see U.S. House (1991), U.S. Senate (1992), and Taubes (1993).
[86] See Rogers (1990), Dozier (1992), King (1992), and Taubes (1993).
[87] See the testimony of NASA Associate Administrator Lennard Fisk in U.S. Senate (1992: 5) and the EOS Payload Advisory Panel's recommendations in Moore and Dozier (1992a, 1992b).

as a national goal that directed EOS as a concept, and moreover as a goal of enough importance to justify a multi-billion dollar earth observing system, which was to be developed and operated over the course of a minimum of 15 years.[88]

The White House, Congress, and NASA were not planning to accomplish this goal alone. NASA officials as well as scientific advisory committee after scientific advisory committee conceived of EOS as a U.S.-led enterprise with a techno-political infrastructure that would extend beyond the borders of the United States and incorporate international participation. In 1983 the NASA Advisory Council appointed an "Earth System Sciences Committee" and charged it with, among other things, outlining and recommending how NASA should enable, promote, and support the study of the earth as a global system, particularly through the development and operation of an earth observation system. While the committee noted in their 1988 report that their report was "restricted to consideration of scientific issues" and that "discussion of economic, social, or political factors" was "explicitly excluded," they did nevertheless comment in their report, as other reports did,[89] that "the study of the Earth is inherently international" and that their recommended research program "will require increased international cooperation to further U.S. partnership in a worldwide research effort."[90] The committee also recommended, "beginning at once," the "strengthening of the international agreements and cooperation necessary for a truly

---

[88] President Bush also further affirmed EOS in his May 1992 National Space Policy Directive on Space-Based Global Change Observation. EOS not only inspired this White House program, but was incorporated into it as well. The directive appointed NASA as the lead agency and, among other things, directed NASA to "continue with the Mission to Planet Earth by conducting the ongoing development, operation, and scientific use of instruments and satellites designed to observe and monitor processes that govern key aspects of global environmental change." See Bush (1992).

[89] NASA (1987b).

[90] The Earth System Sciences Committee came out with two reports, an "overview" in 1986 and a "closer view" in 1988. The quotes are from the latter report, NASA Advisory Council (1988: 21, 171, respectively).

worldwide study of the Earth."[91] NASA management agreed with the committee's assessment of the importance of international cooperation.[92] In 1989, a NASA associate administrator explained to the Senate Committee on Commerce, Science, and Transportation that:

> Due to the global nature of the problem this research effort addresses and the interest of most of the world in the goals of this effort, Mission to Planet Earth would require cooperation and cost-sharing by many of the major spacefaring nations of the world under the leadership of the U.S.[93]

As that statement's allusion to the international arena suggests, by 1989 international cooperation was required to realize the Mission to Planet Earth and EOS for reasons beyond the asserted scientific and economic necessity of international cooperation. International cooperation had become, for the U.S. Government, an inherent aspect of the concept of EOS and Mission to Planet Earth as well as a necessary political goal for the EOS and Mission to Planet Earth programs.[94]

"International cooperation" had been a part of NASA's plans for the earth observing system from the beginning. The concepts of Mission to Planet Earth and EOS and the goal of international cooperation were never quite separable. NASA officials took for granted that the "Mission to Planet Earth," with all the inspiration

---

[91] NASA Advisory Council (1988: 5).

[92] For an example of how the conclusions of the Earth System Science Committee regarding international cooperation were explicitly used by NASA to justify its international efforts, see the prepared statement of Dr. Shelby G. Tilford in U.S. House (1990c: 2).

[93] Prepared statement of Dr. Lennard Fisk in U.S. Senate (1989a: 11).

[94] International cooperation had for centuries been an important aspect of the complex development and synthesis of a diverse set of fields which came to be called in the twentieth century the "earth sciences." The International Geophysical Year from July 1957 to December 1958 is regularly cited as an event that "began an era of international observation and regulation involving the Earth's environment" (Doel 1997: 404). See also Sullivan (1961). This internationalism in the earth sciences and more specifically in the environmental sciences should not be equated with an apolitical universalism. For instance, in the second half of the twentieth century, the earth sciences and their internationalism owe much of their development and synthesis to the Cold War. See Mukerji (1989), Elzinga (1993), Cloud (2000), Hamblin (2002), and the special issue of *Social Studies of Science* on "The Earth Sciences in the Cold War," edited by Cloud and Reppy (2003), particularly the commentary of Dennis (2003).

and appeal that such name might provoke, the program could not be identified as just the United States' Mission to Planet Earth. In the eyes of its advocates, if a Mission to Planet Earth and an EOS were to be politically credible concepts, they would need to be seen by NASA, the White House, Congress, and maybe at times by other nations, as no more than efforts in which NASA and the United States were leading the world on a Mission to Planet Earth, as opposed to unilaterally dictating to other nations knowledge about the globe.[95] It made little sense to NASA planners for NASA to have a Mission to Planet Earth and an Earth Observing System if other countries were not conspicuously involved, especially if any tangible drawbacks of such cooperation were years away from the positioning of Mission to Planet Earth and EOS as rhetorical and political concepts in the mid-1980s. If NASA had wanted not to involve other governments, it might have advocated a constellation of environmental satellites, but not a Mission to Planet Earth or an Earth Observing System. By the time the Mission to Planet Earth and the Earth Observing System concepts were approved by Congress in 1990, international cooperation had become so intertwined with those concepts that if NASA could not point to significant international cooperation in the implementation of those concepts, especially before Congress, then the program would be seen as at least a partial failure.[96]

---

[95] See, for instance, NASA Associate Administrator Edelson's emphasis on international cooperation in his unveiling of the "Mission to Planet Earth" concept in his commentary inside the cover of *Science* (1985). He explicitly used the rhetoric of "Mission to Planet Earth" in his announcement. Compare this rhetoric of internationalism with that of the response to the "Global Habitability" program proposed by NASA and the United States at the United Nations Conference on Peaceful Uses of Outer Space in Vienna in 1982, as reported in Edelson (1988: 6-7) and Lambright (1994: 99). As reported in Edelson (1988: 7), Associate Administrator Edelson recalled that the "Global Habitability" program "came across like NASA was trying to take over the world."

[96] See the discussion in U.S. House (1990c) and, in particular, the testimonies of Dr. J. Thomas Ratchford, Associate Director of the Office of Science and Technology Policy, and Dr. Shelby G. Tilford.

In the mid-1980s, when NASA was nurturing the concept of a Mission to Planet Earth and EOS, it would have been more difficult for NASA to have rejected the idea of international cooperation than to have worked to accommodate it, not just for rhetorical and political reasons, such as being seen by the White House and Congress as advancing a popular and promising program, but also for bureaucratic and logistical reasons as well. International cooperation had after all been an objective of NASA since NASA's founding, and that goal had been reflected in the designs of the programs and in the hardware in which EOS was being integrated. As was discussed in chapter two, NASA had designed the space station program and the space station's architecture as international endeavors. In the 1980s, NASA and its international partners had in fact managed the planning and coordination for EOS as an outgrowth of their cooperative efforts for the international space station.[97]

NASA's management in particular handled EOS as an "international program because the space station was an international program," and consequently, NASA's standard guidelines for international cooperation were used for EOS because they were used for the space station.[98] In accordance with the guidelines' philosophy of "clean interfaces" and clear lines of responsibility, it was provisionally thought that NASA would provide two large frames for EOS instruments, frames that were known as "platforms." The European Space Agency would provide an additional platform, and Japan's National Space Development Agency would provide another.[99] By the summer of 1989, representatives from the space agencies of the United States, Europe, and Japan had met together several times since 1986 as an "international coordination

---

[97] See their [Intergovernmental Agreement] On the Cooperation in the Detailed Design, Development, Operation, and Utilization of the Permanently Manned Space Station, September 29, 1988.

[98] U.S. Senate (1989a: 17-18, and quoted from 46). See also Shaffer (1989: 1). On NASA's standard guidelines for international cooperation, see chapter two.

[99] Prepared statement of Dr. Lennard Fisk in U.S. Senate (1989a: 9).

working group" for the purpose of coordinating and harmonizing calls for instrument and research proposals, data policies, and designs of platforms, although the international coordination working group had not yet agreed upon many hard decisions—for example, commitments concerning hardware interdependencies, the sharing of resources, or the allocation and scheduling of instruments.[100]

While NASA held out international cooperation as a goal for Mission to Planet Earth and EOS in a large part because Mission to Planet Earth and EOS had become rhetorically and institutionally tied to international cooperation, international cooperation became not just a NASA goal but a state goal for additional reasons. First, the White House, Congress, and NASA desired international cooperation for the significant scientific, technical, and economic benefits that cooperation seemed to promise, such as internationally calibrated data, internationally validated data, and especially, cost-sharing among states. International cooperation was recognized by most everyone as a mode of operation through which those benefits could be efficiently and effectively secured.[101] Second, Mission to Planet Earth, EOS, and the international cooperation that was to be a part of those programs enabled the Bush administration to respond to national and international criticism that they were exaggerating the uncertainty of scientists' knowledge of global climate change and using uncertainty as an excuse for inaction, rather than crafting policy that would mitigate the probable implications of climate change, as it was argued other nations were trying to do.

For example, in President Bush's opening remarks to a two-day international conference on the science and economics of global change held at the White House in

---

[100] Shaffer (1989: 1).
[101] See U.S. House (1990c) and, in particular, the testimonies of Dr. J. Thomas Ratchford, Associate Director of the Office of Science and Technology Policy, and Dr. Shelby G. Tilford before those subcommittees. Also see U.S. Senate (1989a: 17-18, 46).

April 1990, Bush told an anecdote about two scientists vehemently debating the extent of future global warming on a recent Sunday morning news show. Bush commented in his speech: "two scientists, two diametrically opposed points of view—now, where does that leave us?"[102] Bush's speech was subsequently criticized by delegates from other nations, particularly those from European nations; West Germany's Minister for the Environment released a statement later that day after Bush's speech that declared that "gaps in knowledge must not be used as an excuse for worldwide inaction."[103] In response to that kind of criticism, President Bush held up as his administration's action the U.S. Global Change Research Program and the increase in the program's spending, an increase that was largely the result of funding for Mission to Planet Earth and EOS.[104]

President Bush's plenary speech at the second session of the Intergovernmental Panel on Climate Change two months earlier also had emphasized the increase in the United States' spending on global change research. Furthermore, this speech had explicitly tied together Mission to Planet Earth, EOS, and international cooperation as part of the United States' commitment to "aggressive and thoughtful action on environmental issues":

> I have just submitted a budget to our Congress for fiscal 1991. It includes over a billion in new spending to protect the environment. And underscoring our commitment to your efforts, I am pleased to note that funding for the U.S. Global Change Research Program will increase by nearly 60 percent, to over a billion. That commitment, by far the largest ever made by any nation, reflects our determination to improve our understanding of the science of climate change. We are working with our neighbors around the world to enhance global monitoring and data management, improve analysis, reduce

[102] Bush (1990b). The conference was held several days before the twentieth anniversary of Earth Day.
[103] As quoted in Shabecoff (1990).
[104] Bush (1990b). His remarks at the closing session also emphasized that research was a form of action, even if not a substitute for policy. See Bush (1990a). Shabecoff (1990) reports that other administration officials noted the U.S. commitment to global change research in response to criticisms of inaction.

> the uncertainty of predictive models, and conduct regular reassessments of the state of science. Our program allows NASA and her sister agencies and all our international partners to move forward with the Mission to Planet Earth. That will initiate the U.S. Earth Observing System, in cooperation with Europe and Japan, to advance the state of knowledge about the planet we share.[105]

In sum, along with nationally and internationally demonstrating U.S. space leadership and helping scientists (and nations) around the world better understand global change, international cooperation was also a U.S. goal for its Mission to Planet Earth and Earth Observing System. For the White House, Congress, and NASA—that is, for the United States as a state—the EOS and the Mission to Planet Earth enterprise came into being in part because that enterprise was a form of international cooperation. In addition to the domestic and international political benefits that international cooperation could facilitate, the White House, Congress, and NASA feared that a Mission to Planet Earth and an EOS without a substantial and conspicuous component of international cooperation would be scientifically and politically vulnerable to charges of being an expensive but incomplete endeavor.

Japan's State Goals and ITIR

Despite their global ambitions, Mission to Planet Earth and EOS were programs initiated and sponsored by the United States, and the rhetoric, politics, and goals of the development of earth observing satellites in Japan can not be assumed to be those of the United States. In this chapter's earlier description of the community of geologic remote sensing which surrounded and partly constituted ERSDAC and JAROS in the 1980s, it was argued that the scientists and engineers of that community regularly identified themselves as a part of the state of Japan. They saw themselves as,

[105] Bush (1990c).

and indeed acted as if they were, working together on national projects to advance Japan's state goals in the international arena, such as the exploitation of natural resources and the development of remote-sensing technology to realize that goal. As Professor Iijima's words written in commemoration of ERSDAC's tenth-year anniversary especially make clear, Japan's first earth resources satellite, JERS-1/*Fuyō-1,* embodied for him, and for others who were associated with ERSDAC, the Japanese state's energy resource and developmental goals, even if many acknowledged that much work remained for the satellite to effectively support the industrial user.[106]

While undoubtedly the corporate members of the ERSDAC and JAROS consortia looked to those public-interest non-profit consortia as organizational devices to channel lucrative MITI contracts to corporations, contracts that those firms could perhaps leverage to develop their general technological capabilities for other business, ERSDAC and JAROS were founded under the jurisdiction of MITI with corporate charters that explicitly specified organizational missions to promote and develop remote-sensing analysis and remote-sensing technologies for the purpose of exploiting natural resources, especially non-renewable resources. As long as the MITI Space Industry Division supported the marriage of its technology development ambitions with that goal—which they did throughout the 1980s—no one on the "development" side of this institutional complex (vis-à-vis the industrial users of the technology) appeared to have contested that overarching goal in any concrete way.[107] JERS-1,

[106] Professor Iijima's words are on p. 106-107, from Sōritsu jūnenshi henshū iinkai (1993: 71). This is not to say that all geologic remote-sensing scientists during the 1980s supported, agreed with, or otherwise went along with, the developmental goals of MITI. My claim is merely about those scientists who were associated—and had associated themselves—with ERSDAC. Those scientists included prominent academic scientists (for instance, at the University of Tokyo) who also held key positions in Japan's remote-sensing professional societies (such as the presidency of the Remote Sensing Society of Japan).

[107] A developmental ideology with regard to technology extended in Japan to policymaking bodies outside of MITI's jurisdiction as well (for a history of a particularly "techno-national"

which was developed throughout the 1980s, was supposed to be, as the "1" signified, just the beginning of Japan's efforts in earth resource remote sensing.[108]

The MITI Space Industry Division looked at the United States' EOS as an opportunity to further its state goal of developing space-based remote-sensing technology for the exploitation of natural resources. But this opportunity was not the only path to realize that goal, nor was it self-evident that it was the best path. Whether or not even to propose an instrument to NASA for incorporation into EOS, and if an instrument were to be proposed, what instrument to propose, were questions that were thoroughly debated by the relevant parties in Japan. Even if one accepts the contention that Japan's industrial institutions were "techno-national" in their ideological outlook in the 1980s,[109] there was no technological imperative that eliminated the politics of technical decision-making. The goal of promoting the exploitation of natural resources through the development of remote-sensing technologies was definitely clear enough on paper. Moreover, that goal—which was the *raison de'tre* of JAROS and

---

developmental ideology in Japan's industrial policymaking, see Samuels (1994)). In the mid and late 1980s, Japan's Space Activities Commission, the intergovernmental body nominally responsible for guiding, harmonizing, and integrating the space policy and activities of Japan's various ministries and agencies, called for increases in expenditures both for developmental activities in remote-sensing technology and for commercialization activities. The commission also suggested that Japan engage in more international projects (see, for example, Uchū kaihatsu iinkai 1989). Partly in response to this suggestion, Japan's Federation of Economic Organizations (*Keidanren*), a central advisory and lobbying group of industry, issued a report in October 1988 that advocated international cooperation in space only as a part of an overall program for developing an "autonomous" position in space activities:

> As Japan seeks to contribute to the international community, it must exercise every opportunity for international cooperation in its space program. To this end, Japan must possess original, superior technology essential to the tools and methods for exploring and using space. Japan should upgrade its space program to a level high enough to establish genuine design authority—to invest its space effort, that is, with a technological validity independent of activity in other nations with advanced space programs (Federation of Economic Organizations (1988); here I have used their official translation).

[108] Sōritsu jūnenshi henshū iinkai (1993), Yamaguchi (2002), and Ishii (2003).
[109] I am referring to a developmental ideology of "techno-nationalism" such as that which was described in Samuels (1994).

ERSDAC—was intellectually and institutionally more focused than, say, the charge of the United States' EOS to study the earth as a system. Nevertheless, different technical decisions could all be construed as advancing Japan's technological capabilities. There were a variety of ways of realizing the goal of developing remote-sensing technologies that would be helpful for natural resource exploration and exploitation.

A debate arose among the relevant study groups about how to best realize that goal. How that state goal was to be pursued was critical for many parties in determining whether or not that goal was worth pursing in any given particular instance. A director of the MITI Space Industry Division in the mid-1980s had proposed developing a thermal infrared instrument, reasoning that MITI—together with the Science and Technology Agency's NASDA—had already sufficiently nurtured the technological development of Japan's firms in the visible and shortwave infrared regions through the development of JERS-1 and other remote-sensing instruments.[110] The MITI Space Industry Division at this time was interested in building "tools" for industry and wanted to develop a portfolio of various remote-sensing tools; a thermal sensor would fill out that portfolio.[111] The Space

110 Space-based remote-sensing sensors other than MITI's JERS-1 which were either operational, planned, or under development in Japan during the mid-1980s, include the MOS series of satellites and the planned ADEOS satellite.

111 My understanding of these mid-1980s deliberations is based almost entirely upon recollections and understandings recounted for me during interviews. See Yamaguchi (2002), Ishii (2003), Yokota (2004), and to a lesser extent, Fujisada (2003) and Watanabe (2003). I was not able to obtain access to any internal MITI planning documents from that time regarding these deliberations, assuming that such documents even existed (such as the minutes of meetings). The three key interviews concerning these deliberations noted above are in agreement with each other about the matters that I discuss, but two interviews offered only second-hand accounts—one from a bureaucrat, Yokota, and another from a scientist, Yamaguchi. Their accounts referenced similar sources for their knowledge of the deliberations. One of my interviews did offer a first-hand account. Ishii was a senior academic scientist at the time of these deliberations in the mid-1980s, was deeply involved in them, and was a key participant. In my interview with him, he greatly emphasized that during this time MITI was interested in supporting the development of remote-sensing technologies as "tools" for general industry and not only as technologies for natural resource exploitation. Yet, in his judgment, the MITI Space Industry Division recognized that natural resource exploitation was at that time its most compelling use. In the policymaking rhetoric of the MITI Space Industry Division, including but not limited to the rhetoric of its policymaking

Industry Division also had heard at some point—possibly directly from a NASA representative—that EOS would be in need of a thermal infrared sensor.[112] NASA's publicly-distributed planning documents for EOS in the mid-1980s noted that a high-resolution thermal instrument was desired to make measurements of surface temperature over land and to complement another high-resolution instrument that sensed in the visible and shortwave infrared regions but not in the thermal.[113] The success of the JPL group's airborne thermal infrared multispectral scanner (TIMS) in the early and mid-1980s had shown thermal remote sensing to be promising.[114] The capacity of Japan's firms to develop a useful thermal infrared earth observation sensor had yet to be demonstrated, however. In the assessment of the MITI Space Industry Division, Japan's firms—and the state of Japan—needed such a capability. If NASA would launch a thermal sensor as a part of EOS—freeing MITI of any need to collaborate with NASDA as MITI had needed to do for JERS-1—all the better.[115]

---

concerning remote-sensing development, "space utilization" (*uchū riyū*) and "space industrialization" (*uchū sangyōka*) were ubiquitous goals. See, for instance, the MITI Space Industry Division presentation, Obara, Yoshida, and Yokota (1990).

112 Yamaguchi (2002), Watanabe (2003), and Ishii (2003). While few commitments had been made, NASA's need for a thermal instrument and the potential opportunity for MITI to provide one were no doubt discussed in the meetings of the International Coordination Working Group for the EOS Space Station platform in late 1986 and in 1987. A slot for an intermediate thermal infrared instrument that was sponsored by the Government of Japan had already been included in the EOS baseline planning scenario by May 1987, which was several months before the description of the MITI facility instrument was released as a part of the Announcement of Opportunity for EOS instrument proposals. See the overhead slides of Fisk (1987).

113 The instrument that I am referring to here is the HIRIS instrument. See NASA (1987a) and Goetz and Herring (1989). The NASA planning documents are NASA (1984, 1987b: 13-20; 32-37; 56-64).

114 See Kahle and Goetz (1983) and see that article's and the TIMS instrument's citations in NASA (1987b: 13-20, 32-27; 56-64) and NASA Advisory Council (1988: 192-3). It is clear from my interviews as well as from numerous documentary sources that JAROS, ERSDAC, and the academic scientists and engineers associated with JAROS and ERSDAC followed the development of TIMS. And see references to TIMS in Ishii (1991) and the articles therein that special issue.

115 This MITI/NASDA rivalry was mentioned especially by Yamaguchi (2002). NASDA was responsible for the launch vehicle for JERS-1 and for the satellite bus. The mission instruments (i.e., the observation sensors) were funded and built by MITI, its consortia, and its contractors.

Study groups made up of representatives from the MITI Space Industry Division, JAROS, and to a lesser extent, ERSDAC, as well as from representatives who were directly from the firms that constituted those two consortia, debated whether or not MITI should fund sensors that would be successors, known as "follow ons," to those that were onboard JERS-1, in addition to a thermal sensor. If follow-on sensors were to be supported, as the makers of the previous sensors wanted, would it not make better sense, some study group members asked, to have those follow-on visible/near infrared and shortwave infrared sensors on the same platform as the proposed thermal sensor, so as to enable simultaneous measurements in diverse spectral bands?[116] A few scientists from the Geological Survey of Japan, a national lab under the jurisdiction of MITI, pressed this point in particular and were given the opportunity to assist the MITI Space Industry Division with the writing of its "facility instrument" proposal to NASA.[117] Building a visible/near infrared sensor capable of stereo imaging for topographic analysis and building a shortwave infrared sensor for mineral discrimination would also appease the primary intended users of instrument's data—the natural resource exploration and exploitation firms.[118]

Study groups also explored other options for sensor development that were not in line with the MITI Space Industry Division's original trial-balloon proposal of developing a thermal instrument for EOS. Some of the natural resource firms had already been pressing that "their" funds, funds that the Government of Japan had

---

[116] Yamaguchi (2002), Fujisada (2003), Ishii (2003), and Watanabe (2003).

[117] Tsu Hiroji from the Geological Survey of Japan's Department of Geothermal Research was purportedly an influential advocate for bands in the visible and shortwave infrared region. See Yamaguchi (2002) and Fujisada (2003). "Facility instruments" were EOS instruments that organizations themselves were offering to sponsor. They were proposed to the Office of Space Science and Applications at NASA Headquarters for the purpose of proposing their incorporation into the EOS system but not for the purpose of proposing that NASA fund the instrument's development (as principal investigators would). The "facilities" that proposed facility instruments for EOS included NASA centers (such as the Goddard Space Flight Center), the National Oceanic and Atmospheric Administration, and international (i.e., foreign) organizations.

[118] Ishii (2003) and Watanabe (2003).

collected by taxing oil and gas imports and placed into a special petroleum account, could be more wisely spent by contracting the building of MITI's future space-based remote-sensing instruments to General Electric in the United States, or by otherwise simply purchasing data that were acquired by foreign satellites, rather than by contracting with Mitsubishi Electric, NEC, and the other firms, as was being done at the time for JERS-1. The "domestic production faction," however, wanted nothing to do with contracting instruments to GE or purchasing data from abroad, and that faction included the MITI Space Industry Division among its ranks.[119] But if the MITI Space Industry Division wanted to fund technology development using funds from the petroleum tax—as they were doing at the time for JERS-1—it could not ignore the concerns of those firms who bore the brunt of this import tax.[120] In this debate, as was arguably characteristic of MITI industrial policymaking,[121] a compromise was reached which had a little bit in it for everyone, with no clear winner or loser. The MITI Space Industry Division would sponsor the domestic development of an instrument for launch with, and incorporation into, a NASA EOS platform, but the instrument would not be only a thermal instrument.

The instrument outlined as the MITI facility instrument in NASA's January 1988 Background Information Package for the EOS call-for-proposals reflected that compromise and also indicated what had yet to be settled.[122] The advocates of

---

[119] Ishii (2003) and Yokota (2004).

[120] According to legislation, funds in the special petroleum account were to be used to stabilize and develop Japan's supply of oil. Why firms whose oil and gas imports were taxed in order to maintain this account were apparently able to shape how MITI used funds from this governmental account, I do not seek to explain. According to numerous interviews, however, they were able to do so. What is important for the purposes of this chapter is outlining what was advocated by whom and how the goals of the state of Japan materialized.

[121] This apparent deference to those firms that bore the brunt of the importation tax, and likely this history of the special petroleum account itself, is yet another example of what Richard Samuels has called "reciprocal consent" between the government bureaucracy and industrial firms in Japan (1987).

[122] NASA (1988a: 180-87).

including follow-on sensors to those that would be onboard JERS-1 seemed to have prevailed to some extent.[123] The MITI facility instrument, called the Intermediate Thermal Infrared Radiometer (ITIR), was described in the Background Information Package as a "second generation" optical instrument that built upon the experience and instrument designs of the JERS-1 optical system (which had instruments for the visible/near infrared and shortwave infrared regions, but not for the thermal). The provisional description further noted that "to lower development cost, system designs will be derived from previous spacecraft programs (i.e., MOS, JERS) and available system hardware will be used with a minimum amount of modification."[124] The instrument was described as including one band in the near infrared, five bands in the shortwave infrared region, and bands in the thermal infrared region, but the description left unspecified the number of bands in the thermal region. A stereo optical system (i.e., two telescopes) for the near infrared band was also stated as being under consideration, which in addition to providing more work for the chosen contractor, was expected to be useful for surveying sites for drilling and mining operations and for laying pipeline.

But what came of the purported original interest of the MITI Space Industry Division—its (and NASA's) need for a sensor in the thermal region? The name of the proposed instrument, the Intermediate Thermal Infrared Radiometer, suggested that the thermal region was the proposed instrument's emphasis, as the MITI Space Division had originally suggested to its consortia and contractors. Yet, according to the instrument's provisional description, the expected performance of the instrument in the thermal region seemed questionable. The instrument's description noted that "the linear [sensing] array in the thermal band[s] and its heat rejection are major tradeoff

---

[123] My interpretation here is based upon my interviews with Yamaguchi (2002), Ishii (2003), and Yokota (2004), as well as the instrument's description in the EOS Background Information Package.

[124] NASA (1988a: 183).

items."[125] This "tradeoff" qualification meant two interrelated things, both of which did not bode well for the performance of the instrument in the thermal region. First, that qualification indicated that MITI, its science advisors, and its JAROS engineers expected an ambitious thermal-sensing array to produce lots of heat, and that level of heat would be difficult to manage. Consequently, the "tradeoff" between these competing performance characteristics was expected to be especially troublesome (hence, the description's highlighting of the tradeoff as "major"). Second, and more ominously, that qualification also signaled that if the instrument's performance needed to be decreased for any number of a variety of reasons (weight, cost, etc.), the performance of the thermal-sensing array (such as the number of sensors in the array) would be one of the first characteristics of the instrument as a whole to be "traded off." It was thought that building the thermal-sensing array would be more demanding than building the others, and thus, it was also projected to be relatively more costly. If the number of sensors in the charge-coupled device array for the thermal sensors was decreased either because of problems relating to their heat production or because of cost (or both), the instrument's spectral or spatial capability (or both) in the thermal region would decrease. Thus, although the instrument was identified in NASA's conceptual planning as a functional substitute for a space-based version of the JPL group's then-operational airborne thermal infrared multispectral scanner (TIMS),[126] and although the word "thermal" was prominent in the name of the MITI instrument, it was not at all clear in the provisional description of the instrument that the performance of the instrument in the thermal region was in fact a priority.

Given this potential mismatch between the instrument's name and the description of its proposed performance, the instrument description could be read as a

[125] Ibid., p. 187.
[126] NASA Advisory Council (1988: 194).

political document to suggest that the MITI Space Industry Division had been persuaded by its consortia or contractors to propose an instrument different than what the division had originally intended when it had named the instrument an "Intermediate Thermal Infrared Radiometer."[127] More cynically, the description could be read to suggest that MITI was trying to pull a bait and switch with NASA, advertising one thing but delivering another under the hood. If judged according to its projected performance, the instrument's emphasis did seem to be on the shortwave infrared region, in keeping with the emphasis of the optical system of the JERS-1 instrument and in keeping with the research interests of researchers at ERSDAC and of scientists at the Geological Survey of Japan.[128] But such a purely strategic interpretation of the motivations behind the instrument's design, based upon the instrument's expected performance and upon deductions of actors' supposed interests, misses an important point: for the MITI Space Industry Division, the salient point was the development of a thermal capability, and ITIR allowed for that development. That was probably why the instrument's title focused on the "thermal," even though to many observers, the instrument seemed to be focused on the shortwave infrared.[129]

What was not written about the instrument in its description was just as revealing—and intriguing—as what was written. ITIR's technical description was remarkably tentative, especially considering that the description was included in the Background Information Package for the expressed purpose of allowing other potential EOS scientists to use it to inform their proposals for EOS instruments and

[127] Judging from documentary sources, it is likely that the name of the MITI instrument had been submitted to NASA for reference in general planning discussions at least several months in advance of the instrument description, maybe as much as a year in advance. Compare Fisk (1987) and NASA Advisory Council (1988: 194).

[128] Yamaguchi (2001), Fujisada (2003), and Watanabe (2003).

[129] This interpretation is implicitly supported by, and alluded to, in Fujisada (2003) and Ishii (2003). The distinction between developmental goals and utility is a central point in the analysis of Samuels (1994). The "observers" I reference are U.S. scientists whom I will discuss below.

science investigations. The instrument's provisional description stated explicitly that much remained to be settled.[130] As has already been mentioned, the number of sensing bands in the thermal region went unspecified. Unlike the provisional descriptions of other facility instruments, such as HIRIS, the instrument description for ITIR did not include plans for how the various spectral region sub-instruments would be coordinated to take measurements (i.e., the instrument's "data acquisition modes"). It did not even include a brief description of the calibration of the instrument, which was a scientific matter that EOS's proponents argued was of crucial importance for EOS's long-term, synergistic approach.[131] The HIRIS instrument description specified the potential pointing angles of the instrument's telescope (and thus the width of the potential "swath" of the earth which it could view), the anticipated performance of its sensors' abilities to quantify light (such as signal to noise ratios and modulation transfer functions), and how the data were to be distributed to users.[132]

It is reasonable to assume that all of these specifications would be important, albeit to a varying degree, for scientists who would respond to the EOS call-for-proposals with proposals for complementary instruments or with scientific investigations that would use these baseline facility instruments. The ITIR description, however, included none of these specifications. To a trained eye, or to an untrained eye willing to engage in some comparison, even if the authors of the description had not described the status of the instrument design as "in a conceptual stage" and in need of "a more detailed study," which they indeed did, the design's tentativeness would have

---

[130] NASA (1988a: 187).
[131] The production of calibrated data was an EOS requirement for proposed instruments. See NASA (1988b: 7).
[132] NASA (1988a: 28-50).

likely been assumed, given its lack of specification.[133] The design's tentativeness, as we will see, implied for some that the design was open to being shaped.

While the description of the ITIR instrument in the EOS Background Information Package strongly suggested that much remained to be settled in its design, the objectives of the instrument's use were definitively expressed. The instrument was to be used to map the distribution of rocks and minerals, primarily—but not exclusively—for the purpose of mineral, petroleum, and geothermal exploration and exploitation.[134] The five bands of the shortwave infrared radiometer (SWIR) were stated as being "helpful" for studying "volcanic systems and mineralization mechanism[s]" and as "important for mineral and geothermal exploration."[135] "Carbonate rocks" were listed as "another possible target to be delineated by the SWIR bands, whose distribution may help to find petroleum reservoirs."[136] With the unspecified number of bands of the thermal infrared radiometer (TIR), the description asserted that it will be "possible to differentiate certain rock types," but the rock types of interest in this spectral region went unspecified, as well as did the bands. The description did note, however, that the thermal sensor "should be quite useful for mapping the thermal anomalies which sometimes show the location of a promising

[133] To be clear, I do not argued that, in comparison to the description of the HIRIS instrument, the ITIR instrument description was a poor or otherwise inadequate description. Given HIRIS's substantial, and perhaps exceptional, conceptual development early on, dating back to years before the beginning of the EOS definition process, the instrument description for HIRIS could perhaps be argued as being too high of a bar by which to judge the scientific merits of the description of the ITIR instrument. But I am not judging the scientific merit of the ITIR instrument description. My argument is that it was likely that the intended audience of the instrument description judged the ITIR description to be general and technically tentative, with many issues still to be settled. I compare ITIR's description with the description of the HIRIS instrument to demonstrate the possibility for, and the likely expectation of, thoroughness and specification in the description of EOS facility instruments. Later, I will describe how a particular reader interpreted the description of the ITIR instrument.

[134] Geothermal energy is energy, such as that contained in steam, which cycles to the earth's surface as a result of the earth's internal heating. This energy can be harnessed for producing electricity.

[135] NASA (1988a: 182).

[136] Ibid. Petroleum, commonly called a "fossil fuel," is a carbon-based substance.

geothermal area."[137] Under the heading "Description of the [imagery] Scenes to be Acquired," the authors of the ITIR instrument description wrote just two sentences: "The ITIR can provide detailed local maps of non-renewable resources. Specific goals will be determined by the Announcement of Opportunity (AO) selection [that is, by the selection of proposals which were submitted by potential investigators in response to this call for proposals]."[138]

While the MITI Space Industry Division, MITI's supporting consortia and advisory committees, and MITI's Geological Survey of Japan had left much to decide in terms of the design of ITIR, the MITI Space Industry Division's deliberative process had established that the expressed goal of its ITIR instrument was first and foremost to explore for and exploit natural resources as an instrument of the state of Japan. While neither that primary goal nor the less explicit secondary goal of technological development definitively determined the instrument's conceptual design, those goals largely gave the instrument's design meaning for the group of bureaucrats, scientists, and engineers who described and spoke for the instrument's design on behalf of the MITI Space Industry Division, its consortia, the Geological Survey of Japan, and ultimately, the state of Japan. Although MITI and the Government of Japan had proposed to NASA that ITIR become a part of NASA's Earth Observing System, which NASA was justifying as a scientific tool for understanding global change, the group of bureaucrats, scientists, and engineers clustered around the MITI Space Industry Division justified ITIR's tentative conceptual design not in those terms, but in terms of the instrument's ability to explore for and exploit natural resources while also allowing for the development of Japan's technological capacity in remote sensing.

---

137 NASA (1988a: 182).

138 Ibid., p. 187. Under this heading, the authors of other instrument descriptions had written paragraphs.

Conclusion

This chapter has described two communities of geologic remote-sensing practice in the 1980s and the goals of their respective states. The techno-political form of life of the community that was anchored around the research and technologies of the JPL group was accustomed to mission-oriented, NASA-sponsored investigations that were governed under a principal-investigator arrangement. In this community, principal investigators typically led scientific investigations that they had proposed to NASA which NASA had subsequently selected for contract. Consequently, this community usually presumed that the driving force behind their investigations would be the visions of the principal investigators, their co-investigators, and the members of their teams. By the mid-1980s, the vision and expertise of the group of geologic remote-sensing researchers at JPL was primarily, but not exclusively, aligned with the science and instrumentation of remote sensing in the thermal region. For many of the researchers at JPL, the science and the instrumentation went hand-in-hand; to do innovative science required pushing innovative instrumentation. In the 1980s, this community's primary patron, NASA, was in the process of planning for a "Mission to Planet Earth." The heart of this mission's concept was to use an "Earth Observing System" to understand global change, an endeavor that was also promoted to demonstrate U.S. leadership in space and the United States' commitment to international cooperation. By 1990, using the Earth Observing System to achieve these three broad goals was supported by Congress and the White House. These goals were state goals. As the next chapter will explain, to harness the potential of the Earth Observing System for their research and to take advantage of these three emerging state goals, the geologic remote-sensing group at JPL embarked upon a collaborative project with Japan's MITI and MITI's consortia.

But MITI, its consortia, and their scientific and engineering advisors had their own coherent bundle of professional interests, scientific and technical practices, and state goals. In response to the oil shocks of the 1970s and incited by the success of the U.S. Landsat civilian remote-sensing program, the MITI Space Industry Division started promoting the development and use of remote-sensing technologies for the exploration and exploitation of energy and mining resources. While the division's primary mission was to "nurture" the space industry by helping it to develop technologies for broader industry use, promoting the development of remote-sensing technologies for the purpose of exploiting energy and mining resources was an objective that served as a compelling justification for its primary mission. In addition, it would also allow the Space Industry Division to tap into a source of funding: the special petroleum tax. With plans for Japan's first earth resources satellite in the works, MITI and a dozen or so firms chartered two industry consortia, ERSDAC and JAROS (more precisely, JAROS's predecessor), which would help MITI achieve its goals and spend its money. Scientists and engineers at MITI's research institutes, such as the Electro-technical Laboratory and the Geological Survey of Japan, were enlisted to advise these consortia. Their research interests tended to complement MITI's goals, focusing for instance on the shortwave infrared region. Since the institutional arrangement of MITI and industry consortia was the only game in town for the development of geologic remote-sensing instrumentation, only those few university scientists who wanted to, and were allowed to, work in this institutional arrangement and support its goals acquired any experience with harmonizing the design and operation of geologic remote-sensing instrumentation with the requirements of scientific investigations. Even by the late 1980s, however, most of that experience was limited at best. The contrast between these two communities' techno-political forms of life was stark.

CHAPTER FOUR

TRYING TO SHARE SEPARATELY

A year after the National Aeronautical Space Administration's 1988 call for proposals for instruments for its Earth Observing System, NASA Headquarters wrote an acceptance letter to a proposing team based out of the geologic remote-sensing group at the Jet Propulsion Laboratory. The team leader and the deputy team leader later described the acceptance letter as the "strangest one [they] had ever seen."[1] NASA explained in the letter that NASA had selected the team leader and her proposed team as a provisional EOS team, but that NASA had *not* selected the thermal instrument that they had proposed to develop in support of EOS science goals. Instead, NASA Headquarters asked Dr. Anne Kahle and her team to somehow realize their thermal instrument and their proposed science "by influencing the Japanese design" of an instrument that had been proposed by Japan's Ministry of International Trade and Industry.[2] This instrument was MITI's Intermediate Thermal Infrared Radiometer (ITIR).

What was implicitly suggested by NASA's "partial selection" letter, and what was later confirmed explicitly, was that NASA wanted Kahle's team to serve as a means to NASA's end of cost-effective international scientific cooperation by helping to ensure that U.S. cooperation with Japan could be scientifically and technically justified as a legitimate and important element of EOS to potential detractors in Congress or NASA advisory committees.[3] In the contemporary words of NASA

---

[1] Palluconi (2001) and Kahle (2003a). The exact quote is from the interview with Palluconi, who was the deputy team leader, but the team leader, Kahle, voiced the same opinion in her interview and explicitly agreed with Palluconi's characterization of the letter, when asked.
[2] Fisk (1989: 1).
[3] See Kahle (2003a) and Butler (2005).

Headquarters' chief project scientist for EOS, the MITI instrument was "the essence of cooperation between Japan and NASA."[4] The stakes of effecting scientifically-credible international cooperation with Japan in the Earth Observing System became even more important for NASA Headquarters because there was less international cooperation elsewhere, such as with the European Space Agency.[5] What international cooperation with Japan meant for Kahle was that she needed to be, as she would later remark, not only a "geophysicist" but also a "geopolitician."[6]

The instrument that MITI's consortia ultimately developed with the guidance of Kahle's team and a team of scientists and engineers from Japan was not named ITIR, but ASTER, the Advanced Spaceborne Thermal Emission and Reflection radiometer (see figure 4.1 on the next page). The "U.S. ASTER team" and the "Japan ASTER team," as the two teams later came to be called, set out in the initial years of their collaboration to achieve the goals of their states by trying to build the ITIR/ASTER instrument as an instrument that would accomplish both teams' goals without either team having to embrace the other team's goals and their state's goals. The two teams—especially the U.S. team—wanted to share ITIR/ASTER separately so that the instrument would serve as what scholars in Science and Technology Studies call a "boundary object."[7]

---

[4] Butler (1989).
[5] Butler (2005). The history of another EOS instrument, the Advanced Microwave Scanning Radiometer, is especially relevant here. Around this time the contributor of the Advanced Microwave Scanning Radiometer shifted from Japan's National Space Development Agency to Italy and the European Space Agency. After some controversy with the European Space Agency, however, the instrument eventually was allocated back to Japan's National Space Development Agency. See also Butler (2005).
[6] Kahle (2005).
[7] What a "boundary object" is—and is not—is addressed below.

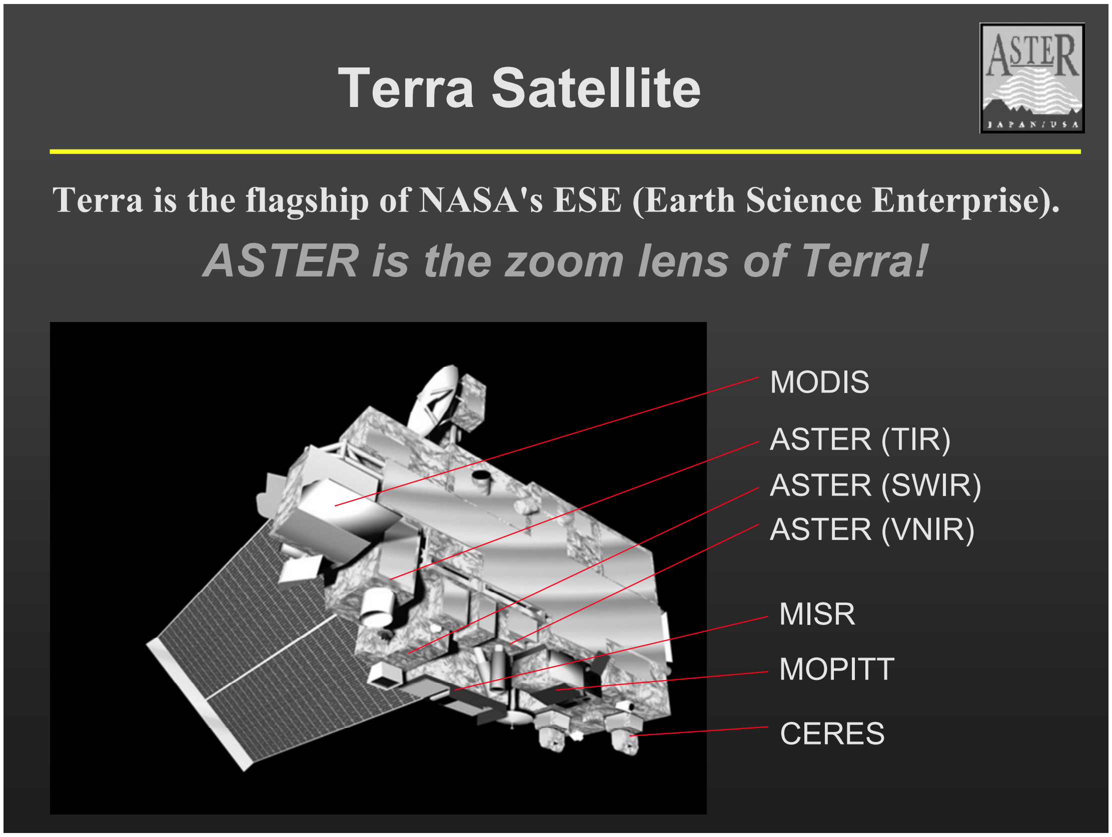


Figure 4.1: A Standard Depiction of the ASTER Instrument Onboard the First EOS Satellite (here from Yamaguchi 2003)

NASA's Terra satellite (which was formerly called EOS-AM1 and EOS-AM) hosted five instruments. MODIS was a MODerate-resolution Imaging Spectrometer, with spatial resolutions of 250 meters, 500 meters, and 1000 meters, depending upon the wavelength band. It was a facility instrument of NASA's Goddard Space Flight Center. MISR was a Multi-angle Imaging Spectrometer, with resolutions of 250 meters and 275 meters, depending upon the camera. It was a facility instrument of the Jet Propulsion Laboratory. MOPITT takes Measurements Of Pollution In The Troposphere, with a spatial resolution of 22 kilometers. The instrument was sponsored by the Government of Canada and was tested at the University of Toronto. The CERES instrument characterizes Clouds and the Earth's Radiant Energy System. It is a facility instrument of NASA's Langley Research Center. The ASTER instrument has three sub-instruments, a thermal infrared radiometer (TIR) with 90 meter resolution, a shortwave infrared radiometer (SWIR) with 60 meter resolution, and a visible near-infrared radiometer (VNIR) with 30 meter resolution. Because ASTER is a high-resolution instrument that could complement the other instruments in some ways, NASA described it as the "zoom lens" of the Terra satellite.

This chapter describes the emergence of U.S.-Japan bilateral diplomacy, how NASA and MITI positioned members of the U.S. and Japan teams as liminal state actors, and how these liminal state actors enacted technoscientific diplomacy and attempted to negotiate the design of the ITIR instrument so that it could serve as a "boundary object." In particular, the chapter accounts for the design specifications of an instrument sensor, and it highlights how the explanatory sketches of epistemic communities and Latourian actor-network theory offer inadequate accounts compared to interpreting the two teams' technoscientific diplomacy.

The Politics of Boundary Objects

The concept of "boundary objects" has been much used and abused in the literature of Science and Technology Studies, as well as in other fields in which the concept has been taken up. It was originally developed to account for "coherence" in "ecological" analyses of "social worlds."[8] Boundary objects were defined as:

> " . . . scientific objects which both inhabit several intersecting social worlds . . . *and* satisfy the informational requirements of each of them. Boundary objects are objects which are both plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites. . . . They have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable, a means of translation" (emphasis in original).[9]

Unfortunately, when employing the term "boundary object," much of the literature asserts the efficacy of an object as a boundary object without describing its creation. Even when an analysis does describe the creation of a boundary object, it generally neglects the diplomacy that led to the creation of the boundary object, including the

[8] See Star and Griesemer (1989) and Star (1989).
[9] Star and Griesemer (1989: 393).

possible pulling-and-hauling of politics among the coherent collectivities through which the boundary objects circulate, such as communities, organizations, states, or "social worlds."[10] In contrast to other examples of "boundary work" analyses in Science and Technology Studies, boundary objects, as well as their cognitive descendants such as "boundary organizations," have been assumed to be stable by virtue of their function as politically-neutral elements that serve as passage points, buffer devices, or common tokens among coherent collectivities. Coherent collectivities, however, may compete or conflict as well as collaborate. Allowing boundary objects to operate as quasi-functional *dei ex machina* misses an opportunity to understand and take seriously collaboration, competition, and conflict in the creation of techno-political order.

This chapter as well as the next will use the general notion of a boundary object in a more sophisticated manner by conceptualizing politics as endogenous to the construction and use of boundary objects.[11] Unlike the work to date on boundary

---

[10] See footnote 66 in Star and Griesmer (1989: 413 and that note's referenced text. That discussion called attention to the strong presumptions of cooperation in Star and Griesemer's account. For example, Star and Griesemer does not account for how Grinnell and Alexander were able to convince the amateur collectors and Berkeley's administrators to adopt Grinnell and Alexander's preferences regarding the boundary objects. Star and Griesemer allude that Grinnell's position of authority had, in their words, a "price," but they do not describe what that price was and why it was paid (see p. 407). Diplomacy over competing preferences was suggested by Star and Griesemer, but not described. In fact, Star and Griesemer's description actually implies that the amateur collectors and administrations initially had little or no objection to Grinnell's and Alexander's goals. In other words, there seemed to be no substantive disagreement. At most, Star and Griesemer show how actors who arguably lived in different social worlds, and aspired to different identities, could nevertheless agree—for reasons that Star and Griesemer do not explain—on the particulars of a boundary object and then were able to institutionally build upon those particulars. Judging from their empirical account, it seems that the boundary object as an historical entity had never itself actually been a site of contestation. Over all, Star and Griesemer's account is quasi-functional (a use "quasi" because the account does not incorporate the causality claims of strong functionalism). See Star and Griesemer (1989: 404-410).

[11] The types of boundary objects that Star and Griesemer (1989) call attention to—namely, repositories, ideal types, coincident boundaries, and standardized forms—all facilitate trading, translation, and coherence across boundaries because the boundary objects are abstracted from contestation in their analysis. Their assumption of political neutrality can also smuggle in historical stability without warrant: social worlds are presumed to be institutionalized from the beginning of analysis, much in the same way that uses of the concept of "trading zones" have presumed

objects, boundary organizations, and most recently, “boundary infrastructures,” this dissertation’s analysis does not make a presumption in favor of cooperation, political neutrality, and stability in its account of how the U.S.-Japan teams attempted to design ASTER as a U.S.-Japan boundary object. The previous chapter described the breadth and depth of difference between the two communities of practice from which the U.S. and Japan teams were assembled. It spelled out and substantiated the distinct state goals of the governments for whom they would work. This chapter and the next illustrate in detail that, mainly because of these differences, the U.S.-Japan team struggled over ASTER’s specifications as a potential boundary object, engaging in power-laden negotiating tactics that were predominantly bilateral in social form in an effort to crystallize their goals in the ASTER instrument itself. In this account, unlike in that of Star and Griesemer, if ASTER had been realized as a boundary object for the two teams, it would have not been the result of merely some functional consequence but rather the result of intentional strategy.

---

institutional stability (for the former point, see, for instance, Star and Griesemer’s discussion of the state of California as a boundary object, p. 407-410; for the latter point, see Galison 1996). Guston’s principal-agent explanation of “boundary organizations,” an explanation that characterizes a boundary organization as stable as far as it can serve two masters equally—especially equally vis-à-vis each other—highlights the importance of the assumption of political neutrality for boundary entities and also the assumption of stability for the institutions/social worlds being served (2001: 401-402). One “master” never gains an upper hand through the design or use of the boundary object or in other ways that implicate the boundary object. Earlier rhetorical and institutional ideas of “boundary work” in S&TS did not neglect the pulling-and-hauling of politics; they did not make problematic assumptions of equal power relations as the literature on boundary objects and boundary organizations has done (see, for instance, Gieryn 1983, 1995, 1999; and Jasanoff 1987).

The TIGER Team

MITI's ITIR and Kahle's Two Proposals

In response to the description of MITI's ITIR instrument which was included in the 1988 EOS call for proposals, Dr. Anne Kahle of the Jet Propulsion Laboratory's geologic remote-sensing group proposed along with two colleagues to develop and the instrument.[12] They proposed to become what the call for proposals called "team members" for the ITIR "research facility instrument."[13] This proposal, which I will call the "ITIR team member proposal," was the first of two proposals that groups led by Kahle submitted to constitute at least part of a team for an EOS instrument. The second proposal was the "TIGER team proposal." A comparison of the fates of these two proposals highlights the stakes of the organizational arrangements of international collaboration for NASA and Kahle's team. For NASA and Kahle's team, the ITIR team proposal did not offer the opportunity of turning ITIR into a boundary object. A revision of the TIGER team proposal did, provided that MITI and its consortia accepted it. The feasibility of designing the ITIR instrument as a boundary object hinged upon a key difference between the two proposals with respect to how a "team" of American and Japanese scientists and engineers would be politically constituted: the ITIR team member proposal did not allow for collaboration between a "U.S." team and a "Japan" team; the TIGER team proposal did.

[12] Her two colleagues were Frank Palluconi and Dr. Alan Gillespie. Kahle was principal investigator and Palluconi and Gillespie were co-investigators. Both Kahle and Palluconi worked in the Geologic Remote-Sensing Group at JPL, which was part of JPL's Geology Program. Kahle was supervisor of the group and manager of the program. Gillespie had worked for several years in the same program at the Jet Propulsion Laboratory as Kahle and Palluconi, but at this time he was at the University of Washington (Kahle et al. 1988: vi).
[13] NASA (1988b: 18) and Fisk (1987: slides 16-17).

In their "ITIR team member" proposal, Kahle and her two colleagues presumed a "team" organizational arrangement for governing the development of ITIR. This team arrangement was stipulated in the EOS call for proposals for all EOS instruments. It was a common organizational form for NASA and was almost taken-for-granted in the principal investigator system, with co-investigators (i.e., team members) organized under a principal investigator (i.e., a team leader). But ITIR was an instrument sponsored by Japan's MITI. Previous development projects of the MITI Space Industry Division and the geologic remote-sensing community surrounding MITI's consortia had no precedent for such a "team" arrangement, especially an international team. As discussed in the previous chapter, Japan's geologic remote-sensing community was not organized along the lines of principal investigator and co-investigator team. The "team" arrangement, however, was a condition for every agency that wanted to incorporate an instrument into NASA's EOS platforms, so if MITI wanted to "fly" ITIR as part of an EOS platform, it would have to create something it could call a "team."

How would the management of the MITI Space Industry Division form a team (either domestic or international) and integrate or not integrate it into its usual way of going about the business of geologic remote sensing? What would be the roles, the division of labor, and the allocation of responsibilities? How would disagreements be managed? Whose expertise would be trusted and who would have decision-making authority? In 1988, these questions were just starting to be discussed within MITI.[14] When Kahle and her colleagues proposed work to MITI in their ITIR team member proposal, which they described in terms of NASA's mundane bureaucratic categories such as working as a "team member," those NASA bureaucratic categories likely had

[14] This claim is based on the understanding of, but not the direct experience of, Yamaguchi (2002) and Yokota (2004).

little explanatory value for the MITI Space Industry Division and its contractors. The labels of those categories did not communicate the assumptions behind those categories, such as what the "team" concept meant, nor did they structure the roles of Kahle and her colleagues in relation to the development of ITIR. While advisory committees and study groups, along with the chairpersons for those committees and groups, were tried and true organizational forms for MITI and its consortia, a "team" as such had not been. What it meant to be a "team" and a "team member" needed to be worked out. But these questions were not worked out—nor arguably could they have been worked out—by the time the MITI Space Industry Division had proposed in 1987 to NASA that ITIR be incorporated into NASA's EOS. The international relations that were emerging among MITI, its consortia, and the Kahle team were not highly structured by diplomatic protocols or shared bureaucratized practices. A relatively small group of scientists and engineers were largely making up their "international relations" as they went along.

There is another reason to think that the bureaucrats of the MITI Space Industry Division and its consortia likely did not work through the possible implications that the "team" concept could suggest for their governance of remote-sensing development: MITI was not the lead agency in Japan's interagency process for soliciting and managing foreign EOS proposals. Kahle and her colleagues technically did not submit their "team member" proposal directly to the MITI Space Industry Division, but rather to NASA's Office of Space Science Applications and to the Research and Development Bureau of the Science and Technology Agency of Japan. The latter office was the designated point of coordination in Japan for foreign proposals to contribute to the design or utilization of instruments that were sponsored by agencies or ministries in Japan.[15]

As had been pre-arranged by the international cooperation working group for EOS, which was an international interagency working group convened by NASA Headquarters, if a researcher affiliated with an agency or ministry in one state wanted to apply to become a part of a future "team" for an instrument that was sponsored by an agency or ministry in another state, that scientist or engineer submitted a copy of his or her proposal to the agency of the "home state" as well as to the agency of the state that sponsored the instrument.[16] If the agency or ministry of the home state endorsed the proposal, the foreign agency or ministry that was sponsoring the development of the instrument would have the option of selecting the researcher's proposal to join the future team. The general arrangement was that the "home state" would then fund the work of those researchers.[17] Thus, by allowing international

---

[15] Kahle et al. (1988). Japan's Science and Technology Agency, and specifically its Research and Development Bureau, was the official point of contact for international science proposals because academic scientific research was in their bureaucratic jurisdiction. Moreover, Japan's Science and Technology Agency was the agency under whose jurisdiction the Japan's National Space Development Agency (NASDA) operated.

[16] "Home state" is my terminology, chosen for convenience and clarity. The international interagency working group, formerly called the "Earth Observation International Coordination Working Group on the Use of Polar Platforms and Space Station Elements," was established in 1986 and included the U.S., European, Japanese, and Canadian space and weather agencies. The working group met about a half dozen times before the release of the EOS announcement of opportunities (i.e., what I have called, for clarity, the EOS call for proposals). The group concentrated on issues such as the number, sponsorship, and general functionality and design of five earth observing platforms, which were planned to be sponsored by NASA (two platforms), the European Space Agency (two platforms), and Japan's space-related ministries and agencies (one platform). By early 1988, the group had not made any agreements about the configuration of the instruments onboard those platforms, although the group had a tentative baseline scenario that they were discussing. The group outlined its charter in June 1988, about the same time scientists were submitting proposals in response to each state's EOS announcements of opportunity. See Fisk (1987: slides 15-17). Charter of the Earth Observation International Coordination Working Group (EO-ICWG) on the Use of Polar Platforms and Space Station Elements, 8 June 1988. In 1990, the entire idea of polar platforms associated with the Space Shuttle program was thrown into question. Ultimately, the platform architecture was abandoned, but many of the constitutive instruments were not.

[17] Fisk (1987).

participation, Japan's Science and Technology Agency and MITI's Space Industry Division had the opportunity of taking onboard additional, and possibly different, expertise at NASA's expense. But to do this, MITI would need to have a "team" of investigators, whatever that might come to mean in practice.

In their ITIR team member proposal, Kahle and her co-investigators did explain what being a "team member" of a future ITIR team meant for them. They proposed, in accordance with the practices and expectations of the community of geologic remote sensing of which they were a part, to do much more development work than would academic scientists who just wanted to receive processed data from the instrument to use in investigations of particular geologic phenomena. They did not propose to be merely particularly influential science users who would shape what regions the instrument targeted or how the instrument's raw data would be processed into "high level" data products.[18] Their proposal was much more comprehensive, and it proposed to open the figurative "black-box" of instrument design and remote-sensing data production.

The overarching objective of their proposal was "to use our expertise gained with development of NASA's airborne Thermal Infrared Multispectral Scanner (TIMS) to maximize the capability and utility of the ITIR for meeting the goals of the EOS mission."[19] They broke down their proposed contribution "toward the wise development and utilization of the ITIR" into five major areas:

1. formulation and selection of the operation and functional requirements [of the instrument's hardware];
2. algorithm development, for the derivation of geophysical parameters and data utilization;
3. operational planning;

[18] "High level" data products are down stream in a processing chain and are the results of algorithmic calculations made with data that are closer to the measurements made by the instrument.
[19] Kahle et al. (1988: iv).

4. data verification; and
5. utilization of the data for geologic research.[20]

During the "definition phase" of the instrument, they expected to participate in the design of the instrument, deliberate along with counterparts from Japan about engineering and scientific trade-offs, and to use their substantial experience with the NASA/JPL TIMS instrument—for which Kahle had been the instrument scientist—to inform those discussions. They pitched this work to Japan's Science and Technology Agency and to NASA as a "significant contribution to the design of an ITIR which fulfills EOS science goals."[21] Borrowing from, editing, and adding to articulations of the science goals that had been already outlined in the "solid earth" section of the lists of major "EOS science goals" which circulated during 1987 and 1988,[22] Kahle and her colleagues set forth specific goals, which they called "EOS science tasks," which they would address, including mapping continental rock units, investigating changes in soils and rock in response to climate, and studying young volcanoes.[23] To MITI, Kahle and her two colleagues offered experience and expertise, especially with thermal instruments and thermal data. To NASA, they offered a way to integrate and leverage a Japan-sponsored instrument to advance NASA's science goals for EOS.

In addition to the ITIR team member proposal, Kahle and her two co-investigators submitted another proposal to NASA to become an EOS team for an instrument—the TIGER team proposal. Joined by about a dozen other co-investigators, they proposed as a team their own thermal instrument, which they called TIGER.[24]

---

[20] Ibid., p. iv, 8.
[21] Ibid., p. v, 12.
[22] E.g., NASA (1987a: 56-64); Fisk (1987: slides 7-8); and NASA Advisory Council (1988).
[23] Kahle et al. (1988: iv, 4-5).
[24] The acronym stands for Thermal Infrared Ground Emission Radiometer. Kahle and Palluconi (1988). The co-investigators were chosen by the core members of the team at JPL and by Kahle in particular. Most of the core members at JPL had worked previously with those members of the team who had never spent time in JPL's Geologic Remote Sensing Group or its Geology and Planetology Section. The proposed Deputy Project Scientist, Frank Palluconi, remembered that:

Kahle and her team, several of whom were or had been affiliated with the Geologic Remote Sensing Group and Geology and Planetology Section at JPL, wanted to push the field of thermal geologic remote sensing with instrumentation that went beyond the capability of the JPL airborne TIMS instrument. The instrument that they proposed was far more ambitious in the thermal region than the thermal sub-instrument of ITIR which had been tentatively outlined by MITI. While Kahle and her proposed TIGER team cast their TIGER instrument in service of the same EOS science tasks that they articulated in their ITIR team member proposal, the TIGER team's proposed means for accomplishing those goals were dramatically different. The TIGER instrument that they proposed was actually two instruments. One sub-instrument was a Thermal Infrared Mapping Spectrometer, which, although it had a different name, went by the same acronym as the airborne TIMS. The second sub-instrument was a profiler, which pin-pointed particular spots (rather than mapping an area) and measured the thermal electromagnetic emission over a wide spectrum of thermal wavelengths (rather than focusing on just several wavelength bands in that spectrum, as an imaging mapper would). "The synergism between an imager and a profiling spectrometer," the TIGER team argued, "is exceedingly powerful, greatly enhancing the value of either data set taken separately."[25]

Although Kahle and her co-investigators were not, and did not expect to be, directly involved in deliberations about whether to accept ITIR for a NASA EOS platform, these types of presumably "high level" techno-political decisions between states were not divorced from their world, and they apparently did not regard them as

> I had worked closely with Hugh Kieffer at USGS at Flagstaff and with Phil Christensen, and I suggested that they both would be useful complements to the team. So, [the team] was put together in a collaborative way starting with a core group here at JPL, and then looking at the range of problems we wanted to cover and people who we knew who did good work in those areas who might be interested (Palluconi 2001).

[25] Kahle and Palluconi (1988: iii).

beyond their influence. They judged it worthwhile to make their opinions about the matter known to NASA Headquarters. In their proposal, they stated that they "recognized that the TIGER appears to be in competition" with ITIR.[26] They argued that while both instruments took on the "thermal" label, underneath the hood were vastly different instruments, capabilities, and potential benefits for EOS science. Although the number of thermal bands that ITIR would propose to offer was still unclear, they were confident that MITI would be proposing fewer bands in the thermal region than the TIGER team had proposed. Their proposed TIGER instrument, through its TIMS sub-instrument, would offer ten thermal bands, whereas in comparison, ITIR's shortwave infrared sensor—which arguably was the focus of ITIR in terms of performance—proposed five bands. It was unlikely that ITIR would be offering many more bands than that for its thermal sensor, and it was much more likely that they would be offering fewer. Since TIGER would probably offer more bands in the same thermal spectral region than would ITIR, TIGER's spectral resolution was expected to be greater. Furthermore, the thermal noise in the TIGER instrument's TIMS was proposed to be one third of that proposed for ITIR. TIGER, having no bands in the shortwave infrared region, was a better complement to another proposed facility instrument for the EOS platform, the HIRIS instrument. ITIR's shortwave infrared sensor overlapped with the shortwave infrared capability that HIRIS was proposing, and therefore could be argued to be redundant (as it eventually was argued). In sum, the team argued, "only TIGER has been specifically designed to exploit . . . the [scientific] potential of the thermal infrared wavelength regions."[27]

In light of this apparent competition, and knowing that NASA would need to pay for the development of TIGER whereas the Government of Japan would be

---

[26] Ibid.
[27] Ibid.

funding the development of ITIR, the TIGER team appealed to EOS science objectives and suggested that:

> "both ITIR and TIGER proceed through the definition phase, during which time the Japanese be requested to consider upgrading the proposed ITIR to increase significantly the signal-to-noise and to include additional spectral bands. If these modifications prove feasible, JPL could then build only the profiling spectrometer part of TIGER as a significant and complementary adjunct to ITIR. If, however, the Japanese do not desire or are unable to achieve the signal-to-noise requirements, JPL could proceed to build the entire TIGER as proposed in order to reach the EOS science objectives."[28]

By suggesting to NASA that it select both instruments to better ensure the fulfillment of EOS science goals, Kahle and her colleagues opened the door for a response from NASA that they had not anticipated.

Over six months later, in early 1989, NASA came back with an offer for the TIGER team, which, if the team accepted, would thrust them into technoscientific diplomacy. The offer from NASA charged these researchers with conducting international relations on behalf of the state through activities that would be thoroughly scientific and technological in character. NASA Headquarters offered Kahle and the TIGER team "partial selection" of their proposal for the definition phase of EOS and EOS instruments. NASA "selected" the TIMS sensor of TIGER and declined the profiler. But, as NASA explained in its selection letter to Kahle, NASA did not actually select the TIMS sensor that Kahle and her team had proposed to NASA as a part of TIGER. NASA selected the TIMS sensor and the full TIGER team in the sense that NASA asked them "to try to implement the design advances in [their] TIMS concept by influencing the Japanese design for ITIR."[29] NASA wrote in its selection letter to Kahle that "TIMS is being considered . . . as an integral technology

[28] Ibid.
[29] Fisk (1989: 1).

improvement for ITIR."[30] That is, NASA would not be competing TIGER's TIMS against ITIR's thermal capabilities, as Kahle and her team had suggested in their TIGER proposal. Furthermore, the letter explained that, while ITIR and, separately, the TIGER team, would both still be subject to selection processes two years later when EOS was scheduled to enter into its execution phase, and at that time one or the other or both could possibly be dropped from EOS, ITIR would not be competing against any other specific instrument, as some other EOS instruments would be.[31] This whole arrangement came as a bizarre surprise to Kahle and her deputy team leader.[32]

Nevertheless, as was suggested in their TIGER proposal, Kahle and her core co-investigators had speculated—and maybe even had heard—that NASA Headquarters had concerns about the degree to which ITIR and its implementation would realize EOS science goals.[33] Both in their ITIR team member proposal to NASA and Japan's Science and Technology Agency and in their TIGER proposal to NASA, Kahle and her co-investigators had asserted that any MITI-appointed science and engineering group to support the ITIR instrument would likely need assistance to meet the EOS science objectives that Kahle and her co-investigators had enumerated and to which they had rhetorically appealed. NASA, as it turned out, agreed with their judgment.

Yet, NASA Headquarters did not indicate—nor even allude to—how Kahle and her team were to go about "influencing the Japanese design" to achieve what NASA Headquarters interpreted to be the TIGER proposal's goal of "significantly

30 Ibid., p. 2.
31 Ibid.
32 Kahle (2003a) and Palluconi (2001).
33 Kahle (2003a). An interview with a NASA official involved in the selection decision at headquarters, who wished to remain unnamed, confirmed for me in 2001 that the EOS program office did in fact have such concerns when they selected ITIR.

improved thermal infrared high spatial resolution imaging for Eos." [34] Nor did NASA Headquarters indicate in its partial selection letter what its particular goals were for this "challenging effort in international cooperation" which it hoped Kahle and the TIGER team would be willing to take on. What was implicitly suggested was that the TIGER team would serve as a means to NASA's end of enabling international cooperation with Japan in a way that could be scientifically and technically justified as a legitimate and important element of EOS. Consequently, for any renewal of funding for Kahle and her team, NASA would evaluate Kahle on how well she performed in that role. NASA Headquarters informed Kahle that:

> the key product of your definition phase study will be a cohesive and technically sound plan demonstrating the feasibility of improved ITIR capabilities and an approach whereby the Japanese agree to implement this improved approach to ITIR with your advice and U.S. hardware assistance only where absolutely necessary. Among the results of your definition phase effort will be improved science, management, data, calibration, and cost plans for your investigation.[35]

But the investigation that NASA Headquarters referred to as "your investigation" was not just Kahle's investigation, of course. The ITIR remote-sensing system was proposed by, and was being developed by, the MITI Space Industry Division and its consortia. NASA inserted the proposed ITIR instrument between, on one end, itself and the TIGER Team and, on the other, MITI and its consortia. This positioning was precarious, however, without the willingness of Kahle's team to develop the instrument as a boundary object between the United States and Japan and without the willingness of MITI and its consortia to incorporate a TIGER team into its team for the ITIR instrument.

---

[34] Fisk (1989: 4).
[35] Ibid., p. 3. This passage is punctuated as it was in the original.

Advancing Bilateralism as Liminal State Actors

Kahle and the TIGER team decided to make the most of any possibilities that they thought they might have for shaping the ITIR instrument through this revised TIGER team arrangement. The description of the ITIR instrument which MITI had submitted for the EOS Background Information Package had emphasized that the instrument design was in a "conceptual stage" and that, regarding data processing, "specific goals will be determined by" those scientists and engineers who were selected through the call for proposals.[36] The strategy that Kahle and her colleagues adopted to influence the ITIR instrument was to advocate that the ITIR instrument be designed in such a way that it would be shared separately between the United States and Japan and between the science goals of those two states. They sought to design ASTER so that it could serve as what scholars in Science and Technology Studies call a "boundary object."

But boundary objects do not come into being on their own accord. They are products of power, even if this aspect of their construction has often been overlooked in the literature. Kahle and her TIGER team strove to build ITIR, and later ASTER, as boundary objects by exercising power from the position of the state, as the "U.S. team," and not just as international members of a Japan team. They worked to enact a bilateralism in which they would be half of a dyad that shaped ITIR and ASTER. Nevertheless, they walked into bilateral negotiations not as state actors but as liminal state actors—working on behalf of their state, supported by their state, but without an authority that could be presumed to speak for their state. Without clear authority to speak for the United States, in their negotiations over ITIR Kahle and her team made the most of their authority as scientists to speak for science. This authority to speak for

[36] NASA (1988a: 187).

“science,” however, pushed up against the authority of others, such as that of NASA and MITI’s JAROS, to speak for “technology.”

The TIGER team’s strategy of designing ITIR as a boundary object was just a step from what they had recommended earlier to NASA in their TIGER team proposal in the summer of 1988. In that proposal, they had suggested to NASA that NASA ask “the Japanese” to “upgrade” the ITIR instrument so that it would better satisfy the TIGER team’s needs, as well as—the TIGER team had argued—better satisfy what they articulated to be NASA’s science goals for EOS.[37] NASA’s selection of the TIGER team, NASA’s charge to the team to try to implement an “improved approach to ITIR,” and NASA’s willingness to provide a minimal amount of hardware assistance all implied that NASA, at least provisionally, found the proposed goals of the TIGER team to be compatible with their own goals and also worth paying for.[38]

At this point then, judging from the selection letter that the TIGER team had received from NASA, NASA seemed ready to support the efforts of the TIGER team to make ITIR a boundary object. It would be the TIGER team, working as liminal state actors on behalf of NASA and the United States, and not NASA, who would negotiate and realize ITIR as a boundary object. Kahle and her colleagues from the TIGER team did not, of course, use the term “liminal state actors” to describe themselves or their role (they generally called themselves “scientists,” “engineers,” and “the TIGER team”). They did not use the term “boundary object” to describe what they wanted to build (they used the term “instrument,” “sensor,” and sometimes “international remote-sensing system”). Nor did they use Star’s and Griesemer’s language of “social worlds” to account for what they had then only strongly suspected to be different visions and goals between MITI and JPL for geologic remote sensing. Nevertheless,

[37] Kahle and Palluconi (1988: iii).
[38] Fisk (1989).

Star's and Griesemer's language of mutually satisfying "informational requirements" would have been quite intelligible and even familiar to them.[39] Kahle, Palluconi, and the rest of the TIGER team pushed for ITIR to satisfy their informational requirements, not as a substitute for MITI's requirements—i.e., not using a Latourian strategy of translation—but in support of and in addition to MITI's requirements—i.e., trying to share separately by making ITIR a boundary object. Yet, if the TIGER team was going to achieve this balance, they would not be able to dictate it; they would need instead to conduct technoscientific diplomacy.

Kahle's and the TIGER team's first opportunity for technoscientific diplomacy came almost immediately after they received their partial selection letter from NASA in February 1989. In an interview, the deputy team leader of the TIGER Team, Frank Palluconi, summarized how they explored what they could make of the hand that NASA had dealt them:

> When we got the letter of acceptance that suggested we cooperate with the ITIR experiment in Japan, we immediately asked how we should do that, and the idea that was settled upon was to contact JAROS as the implementing organization for the instrument and as the first point of contact. So we got a letter of reference from NASA Headquarters . . . which served as the introduction to JAROS and then we just got on a plane and went to Japan and knocked on their door. They knew we were coming but they did not know what to expect, and that first meeting was a little bit awkward, although both sides essentially described the instrument that they had proposed and were thinking of building. And then we went out to Tsukuba [which is a "science city" outside of Tokyo where many of MITI's national research institutes were] and talked to people out there at the metrology lab, among whom was Dr. Ono. He spoke English rather well, which maybe one of the reasons why we were referred to him, but he also was head of the section for radiation standards and had been an integral part of instrument development in Japan. And based on conversations there we realized that one of the principal scientific people involved was a fellow, a Professor Ishii. So at Tsukuba, we

[39] My judgment here draws upon my reading of the TIGER proposal, contemporary correspondence among MITI, JAROS, ERSDAC, the TIGER team, and NASA, all of which will be cited below, and retrospective interviews, particularly, Abrams (2003), Kahle (2003), and Palluconi (2001).

> arranged to meet with him [Professor Ishii was back in Tokyo]. And we did meet with him [in Tokyo] and talked about our interests, and talked about what benefits there would be for beefing up the thermal portion of ITIR and, ah, the benefits that a U.S. team might bring to this enterprise. And we felt when we left that we had made a good impression and things sort of grew from there.[40]

As was typical of Frank Palluconi's speaking style,[41] in this interview excerpt Palluconi's language is considered, cautious, and matter-of-fact. Others who tell this story first-hand, or who re-tell it second-hand as part of the lore of the team, have more colorfully emphasized that NASA purportedly provided very little guidance to the team, that NASA had done minimal advance work, and that, under the pressure of the EOS development schedule, it was a core group from the TIGER team who took the initiative and "knocked on the door" of MITI and JAROS without any detailed agenda planning between MITI and NASA, or for that matter, between MITI and the TIGER Team.[42]

Palluconi's account, as well as accounts of others, notes that the leadership of the TIGER team was not going over to Japan in March 1989 merely to see how they could be of assistance as members of some future international science team. The TIGER team had their own ideas about what kind of instrument they wanted to build and what kind of organizational arrangements might most effectively realize that instrument. That is, the TIGER team leadership was not going to Tokyo just to advocate for particular instrument specifications. They went there to lay the ground work for political arrangements as well. They wanted to make clear, as Palluconi put it, "the benefits that a U.S. team might bring to this enterprise"—a "U.S. team," not a few team members, but a "U.S. team." Although NASA had provisionally accepted the

---

40 Palluconi (2001).

41 I make this generalization based upon my interview with him and from my observations of his style of talking with Japanese and American colleagues at team meetings.

42 Abrams (2001) and Kahle (2003).

TIGER team (but without its proposed instrument), MITI, its consortia, and its key advisors had not yet agreed to incorporate the TIGER team members into their ITIR team, much less to call the TIGER team the "U.S. team." A "U.S. team" did not yet exist, and its creation was not assured. Demonstrating the benefits of a "U.S. team" was a significant reason for why it might have been important to make a "good impression."

There was also another, less important, reason to make a "good impression": the verdict was still out on the ITIR team member proposal, which was the proposal submitted before the TIGER team as a group had been charged with collaborating with Japan's future ITIR team. While the ITIR proposal had been endorsed by NASA, it had not yet been accepted by Japan's Science and Technology Agency and MITI.[43] It is reasonable to assume that, of the three members from TIGER team on this trip, the two members who also had proposed to be ITIR team members, Kahle and Palluconi, wanted their separate ITIR team member proposal to be accepted. Making a poor impression at this first meeting with MITI and its consortia might conceivably jeopardize that acceptance.

Yet, the "ITIR team member" arrangement by itself was not the preferred arrangement for collaboration. If the ITIR proposal had been accepted, its three proposers—with Kahle as the principal investigator for the three—would likely have become individual members of some future ITIR international science team, a team that had yet to be defined or constituted, but which Kahle would not have led. This team would have been populated largely by scientists and engineers from Japan whom Kahle and Palluconi did not expect to share the goals of the TIGER team. Under this "ITIR team member" arrangement, the American ITIR team members would not have the support of the numerous experts who were a part of the TIGER team effort but

[43] Kahle et al. (1988).

who were not ITIR team members, experts with whom they were already colleagues. Those non-ITIR team colleagues would only be accepted onto the ITIR team if the TIGER team in which they were members was, as a whole, incorporated into the ITIR science team.[44] Moreover, the three American scientists of the ITIR team member proposal, as individual ITIR team members, would not have been able to act with the weight that the label of a "U.S. team" might afford, assuming that the TIGER team became the "U.S. team" for ITIR.[45] Without the recognition that a "U.S. team" was involved in the development of ITIR, it would be more difficult for those team members to convincingly deploy the rhetoric of speaking for a state in order to influence decisions.[46] The "U.S. team" label might prove to be a useful resource, if they could get it.

For their first meeting in Japan, it was uncertain what political resources were available (or were going to be made available) to the three members of the provisional TIGER team to help them realize their preferred instrumentation, their science goals, and the TIGER team itself, possibly as a "U.S. team." There had been some correspondence prior to their visit which had introduced them to their counterparts. In the above interview excerpt, Palluconi mentioned that the managers at JAROS, as "the first point of contact," did not know what to expect. Anne Kahle had written the Executive Managing Director of JAROS a brief introductory letter, requesting that she and two colleagues visit to "discuss the developing plans for ITIR" and to familiarize

---

[44] One could argue that some ad-hoc arrangement for adding NASA-funded scientists to the ITIR team could have been worked out, but since it would have not gone through the standard review process that NASA had established years in advance, such an ad-hoc arrangement would have been highly exceptional. It is extremely doubtful that NASA Headquarters would have selected scientists through a bureaucratic procedure that circumvented their public "announcement of opportunity" and peer-review process.

[45] I write "might" because this weight can not be assumed. It needs to be demonstrated or, at least, illustrated. That is, Japan's project managers would need to interpret how this group of American scientists was or was not speaking for NASA and the United States.

[46] A "U.S. team" label would probably also confer more status, discretion, and funding to its members with respect to NASA, particularly in the case of Kahle as the "U.S. team leader."

themselves "with all technical aspects of the current design, calibration, and data utilization."[47] The letter parenthetically explained that "(NASA has accepted our proposal, TIGER, and requested that we collaborate with you)."[48]

The Director of NASA's Earth Science and Applications Division had also written a letter to introduce the team to the Executive Managing Director of JAROS (this letter of introduction is included in the Appendix). But Kahle and her two colleagues did not at the time find the letter of introduction to be particularly clear or helpful for laying out the terms of their cooperation with MITI and JAROS.[49] Neither, apparently, did their Japanese counterparts. In the first meeting of their visit to Japan, in a meeting with JAROS managers and with a MITI representative, the Executive Managing Director of JAROS explained to Kahle and her colleagues that the letter of introduction did not clarify for them "what form a cooperative effort could take."[50]

The letter of introduction from NASA Headquarters did seem to state that, if MITI and its consortia wanted to incorporate ITIR into NASA's EOS, MITI and its consortia would be required to cooperate to some extent with the TIGER science team for the definitional phase of the instrument, which was expected to be about two years.[51] Beyond this general understanding, the critical aspect of NASA's letter of introduction was how it cast the TIGER team—what kind of authority and importance

---

[47] Kahle (1989b).
[48] Ibid.
[49] See the two trip reports, Chase (1989a) and Kahle (1989c). The latter included a copy of Kahle and Palluconi (1989).
[50] Kahle and Palluconi (1989), which was written to Shelby G. Tilford and Dixon M. Butler. The last two individuals were, respectively, the Director of NASA's Earth Science and Applications Division and the EOS Program Scientist.
[51] I write "seem" because my interpretation of the subtle letter of introduction from NASA Headquarters is not informed by specific comments from its readers about how they read, or did not read, the letter. In interviews, I asked interviewees to discuss the first several meetings between the TIGER Team and ITIR's developers in Japan, but I did not ask those individuals who might have taken the letter into consideration and used it as a resource during those meetings how they specifically interpreted the letter of introduction (or, more accurately, how they remembered interpreting the letter of introduction, if they would have at all).

it granted the team and how decidedly it portrayed the TIGER team as expressing the will of NASA Headquarters. The TIGER team leadership might have been able to use a compelling portrayal of a tight relationship between NASA Headquarters and the TIGER team as a resource in their negotiations. In the circumstances of these initial meetings, with little or no history and previous interaction between the TIGER team on the one side and MITI and its advisors on the other, the more aligned NASA seemed to be with the TIGER team, the stronger the TIGER team's negotiating position vis-à-vis MITI was likely to be.[52] NASA, after all, was providing the rocket and the satellite bus for the ITIR instrument.

Nevertheless, the letter of introduction did not portray the TIGER team as representing NASA Headquarters. The letter explained matter-of-factly that "NASA has asked Ms. Kahle to try to implement the design advances of her TIMS concept by working with you, and your design for ITIR." Alone, such a statement did not indicate how much "backing" Dr. Kahle and her team had, or would have, from NASA in their dealings with MITI and its consortia. The statement certainly did not suggest that the TIGER team leadership represented NASA in this collaboration. Moreover, the letter of introduction's author, the Director of NASA's Earth Science and Application Division, used qualifying language such as "try" and conditional phrases such as "if her proposal if [*sic*] accepted by you" to suggest that the full acceptance of the TIGER team and all of its recommendations might not have been strict requirements for U.S.-Japan cooperation regarding ITIR.[53]

In particular, NASA cast the TIGER team as having less sway over the fate of the ITIR remote-sensing system on matters of "technology" (i.e., hardware), writing

---

[52] This judgment is loosely based upon after-the-fact interpretations of these initial meetings offered by Ishii (2003), Kahle (2003), and upon my general familiarity with these two communities. A closer analysis is below.

[53] Tilford (1989: 1).

that "NASA remains firmly committed to flight of ITIR on NPOP-1 even if technology improvements such as those proposed by Ms. Kahle cannot be achieved."[54] That statement would have made much less compelling any attempt in these meetings on the part of Kahle and the TIGER team leadership to rhetorically tie MITI's acceptance of the TIGER team's recommendations in the realm of technology to NASA's acceptance of ITIR for the execution phase of EOS, and thus, to the NASA's launch of ITIR (ITIR and the other instruments that would constitute EOS had so far only been selected for the definition phase). Especially in the realm of technology vis-à-vis science, the TIGER team was by no means in a rhetorical position to practice any kind of brinksmanship in which the team would insinuate that if their needs were not met they would withdraw from the collaboration and take ITIR out of EOS with them.

The letter suggested to its readers in Japan three deductions about the relationship between the TIGER team and NASA which likely influenced the weight that MITI and its consortia would give to the TIGER team and to the team's recommendations.[55] First, NASA Headquarters supported the goals of the TIGER team. Second, NASA's launching of ITIR was conditioned on MITI's, and its consortia's, cooperation with the TIGER team in some significant but vague way, at least in the short-term. Third, NASA was willing to go out of its way to help the TIGER team achieve its goals. Even if MITI ultimately did not need to implement the TIGER team's recommendations concerning "technology improvements" for the ITIR

[54] Ibid., p. 2.

[55] The key readers of the letter with whom the TIGER team leadership interacted during this first trip to Japan were mentioned or alluded to in the Palluconi interview excerpt: namely, the Executive Managing Director of JAROS (the formal recipient of the letter of introduction), the Director and Deputy Director of the MITI Space Industry Division, Dr. Ono Akira of the National Research Laboratory of Metrology, and Professor Ishii of the University of Tokyo. Also in attendance were Dr. Fujisada Hiroyuki from MITI's Electro-technical Laboratory, and Dr. Tsu Hiroji, Dr. Satō Isao, and Dr. Yamaguchi Yasushi from the Geological Survey of Japan. All of these individuals were named in trip reports (Chase 1989a and Kahle and Palluconi 1989)

instrument (whatever those recommendations might have turned out to be), the letter of introduction was a first step in convincing MITI and its consortia that NASA Headquarters needed the involvement of the TIGER team to ensure that NASA's cooperation with MITI was conducted in a manner that could be scientifically justified. Given that need, there was no reason for anyone to think that NASA Headquarters would not agree with changes to the ITIR remote-sensing system which stemmed from the recommendations of the TIGER team, even those in the realm of "technology" which might come at some inconvenience or cost to NASA. While NASA Headquarters conceded that it was their expectation that the TIGER team might have less sway over "technology," NASA still wanted "technology" on the negotiating table. NASA's Director of Earth Science and Application explained in the letter of introduction that, "in light of [NASA's] current desire to see the best possible ITIR developed under Japanese leadership, NASA is willing to reconsider [NASA's earlier stance concerning] the question of export licenses" and would also "consider a limited hardware contribution . . . to enable increases in ITIR performance."[56]

For matters other than technology, NASA cast the TIGER team with much more authority. Although the NASA Director had written only a paragraph earlier that NASA was "firmly committed" to the flight of ITIR, he took two paragraphs to emphasize that ITIR was still in a definition phase, that ITIR had not yet been selected for flight, and that "all instrument investigations will require confirmation for execution phase." The confirmation decision, the author wrote, "will depend on many factors including successful progress between Ms. Kahle and you on the ITIR effort . . . ." What constituted "successful progress" from the perspective of NASA Headquarters, however, went unspecified. Using language grafted from NASA's earlier partial acceptance letter to the TIGER team, the NASA Director noted that:

[56] Tilford (1989: 1-2).

> The key product of a combined definition phase study would be a cohesive and technically sound plan demonstrating the feasibility of improved ITIR capabilities and an approach to ITIR whereby the Japanese agree to implement this improved approach to ITIR with Ms. Kahle's advice . . . . Among the results of this definition phase effort will be improved science, management, data, calibration, and cost plans for the portion of the combined investigation.[57]

Whereas such a letter can always in theory be open to differing interpretations, and while different interpretations could have been offered at the time it was initially read and discussed, there is no evidence that differing interpretations were consequential. NASA's awkward beating around the bush seems to have suggested to its readers in Japan that NASA was not going to insist that MITI implement the TIGER team's hardware recommendations in order for ITIR to be incorporated into EOS, but that NASA did have some ill-defined yet substantive expectation—an expectation that was probably assumed to verge on a non-negotiable requirement—that MITI and its consortia allow the TIGER team to help "the Japanese" implement an "improved approach to ITIR." This was the negotiating position in which NASA placed the TIGER team. Although it was never made explicit, the letter of introduction conveyed that NASA had delegated some authority to the TIGER team as liminal state actors, but their authority to speak for NASA and the United States in bilateral negotiations with MITI and its consortia lessened as the issues under discussion came increasingly under the realm of "technology." Finally, in no way did NASA's letter of introduction label anywhere the TIGER team as a "U.S. team." The TIGER team eventually acquired that label themselves, formalizing the bilateral practices of their negotiations with Japan. As liminal state actors, they nurtured and advanced state vis-à-vis state bilateralism.

---

[57] Ibid., p. 3.

The Practice of Bilateral Technoscientific Diplomacy:

The Thermal Infrared Radiometer, 1989

Positioned by NASA as liminal state actors, the TIGER team set out in March 1989 in their first round of meetings with the MITI Space Industry Division, JAROS, and JAROS's advisors to negotiate ITIR so that they could share the instrument separately as a boundary object. The TIGER team pushed for ITIR, later named ASTER, to meet their "informational requirements" by working for changes in the instrument's technological design.[58] While hardware (i.e., what NASA called "technology") was precisely the realm in which the TIGER team had the weakest negotiating position, it was commonly understood that these matters of technological design needed to be settled first, owing to ITIR's and EOS's development schedule and because of the significance of the instrument's design for shaping the kind of science that the instrument could support. Negotiations about the design of the instrument continued from 1989 through 1994.

In these negotiations, the two teams—and especially the JPL-based team—worked to make the ITIR instrument a U.S.-Japan boundary object. As liminal state actors, they carried out scientific analyses of the instrument and attempted to design it to realize both states' goals as well as the goals of individual team members. This technoscientific diplomacy enacted negotiations that were generally bilateral in

---

[58] In April, Kahle's statement of work to NASA for the definition phase of ITIR (Kahle 1989e) included the following objectives:

> a. Attempt to integrate into the design of the ITIR the capability improvements proposed for TIMS.
> b. Develop a cohesive and technically sound plan demonstrating the feasibility of improved capabilities consistent with the Eos [i.e., EOS] mission and an approach which the Japanese agree to implement.
> c. Determine the level of technology readiness for the performance enhancements in the TIMS concept acceptable in ITIR. . . .

their socio-political form, pitting a "U.S." team and a "Japan" team against each other. Despite numerous and detailed discussions, both teams' "informational requirements" were not equally satisfied in the design of the ITIR/ASTER instrument. For these bilateral negotiations, neither solidarity among scientific expertise (per epistemic communities) nor the power of material networks (per Latourian actor-network theory) provide satisfying explanations for the outcomes of the negotiations, that is, for the complex weighing of which side's preferences would be built into the instrument. Through their technoscientific diplomacy, U.S. and Japan scientists and engineers crafted pragmatic compromises that took into account both how they asserted and ascribed "science," such as claims of scientific knowledge, and political power, particularly state power.

Discussions and debate generally emerged as members of the TIGER team were confronted with designs that had already been proposed by Japan's scientists, engineers, and managers. These designs usually represented, at a minimum, tentative decisions. Frequently, however, these decisions were much closer to already being settled rather than to being just tentative. By no means was the TIGER team presented with a "clean slate" from which to work. MITI was financing the ITIR instrument; a MITI consortium and its advisors had originally proposed the instrument to NASA and were managing its development; and Japanese contractors were building the instrument. Given this established division of responsibilities and the TIGER team's letter of introduction from NASA, the TIGER team could not simply demand design changes, assuming that if these changes were not accommodated the TIGER team would then be able to implement the changes themselves. Although the TIGER team could resign from the collaboration, its members had no clear-cut options for "going it alone" and building their own instrument. NASA had placed the TIGER team in a position vis-à-vis MITI that did not provide the team with the financial and material resources for pursuing alternatives to collaboration with Japan. Thus, the TIGER team needed to negotiate their recommendations by persuading their Japanese counterparts.

The number of thermal infrared bands that ITIR would measure was a specification of central importance to the TIGER team. Dr. Kahle and the TIGER team had proposed to push their field of geologic remote sensing by building a space-based instrument that would make thermal measurements to distinguish, and in some cases, identify, "geologically important materials" and to facilitate "global studies of volcanic provinces, sedimentary basins, and weathering history of soils and rocks related to paleoclimate."[59] The design of their proposed TIGER instrument promised a capability for mapping thermal radiation in fourteen different spectral bands.

The question of how many thermal infrared bands ITIR would have, and should have, was discussed in the first meeting of the first trip that the TIGER team leadership took to Japan, the March 1989 trip that Palluconi outlined in the interview excerpted above. On the morning of their first day in Japan, TIGER team members Kahle, Palluconi, and Chase, met in Tokyo with the Executive Director of JAROS and a MITI official to explore how they might cooperate as NASA had requested in its letter of introduction. According to trip reports written by the TIGER team delegation, the "key message" of the TIGER leadership in that first meeting was that "ITIR did not satisfy the requirements of the TIGER team because there were too few spectral bands in the thermal IR [infrared]."[60] The TIGER team leadership was under the impression that just five thermal bands had been planned for ITIR. That was not enough for the research that the TIGER team wanted to conduct. In response to the TIGER leadership's "key message," the representative at the meeting from MITI pointed out that MITI was "responsible for promoting the industrial use of space and,

---

[59] Kahle and Palluconi (1988: iii).
[60] Chase (1989a: 2).

while the thermal infrared was of interest to Japanese geology users and scientists, ITIR placed major emphasis in the visible and near infrared and short wavelength infrared . . . ."[61] Given the technology that was available to MITI's preferred contractors, it was simply too expensive to do everything, the MITI representative explained, and difficult choices had to be made. In this conversation, Kahle conceded that ITIR's thermal bands were "sufficient for operational assessment," but she still impressed upon JAROS management and MITI that ITIR's proposed thermal bands were not sufficient for the "scientific purposes" intended for the TIGER instrument, scientific purposes that Kahle and much of her team had been refining for years at JPL.[62]

To reprise here "boundary object" terminology, ITIR's five bands did not meet the TIGER team's "informational requirements." Kahle asserted, and MITI and JAROS management seemed to have agreed, that there were two different sets of goals proposed for the instrument—operational goals for natural resource exploration and the scientific goals of the TIGER team. These two sets of goals were each side's particular articulation of broader state goals that were pushed by their respective bureaucracies, such as MITI's interest in space industry development and energy security and NASA's interest in demonstrating U.S. leadership in global change research. How these two sets of goals would be brought together and even reconciled in ITIR's design concept was still far from apparent.

That afternoon the TIGER team leadership traveled to Tsukuba to meet with the scientists and engineers who worked at the national research institutes in the

[61] Kahle and Palluconi (1989: 1). Without a doubt, however, for the key geologists and scientists in Japan who had already been consulted about the design of ITIR, the shortwave infrared region was of much more interest than the thermal infrared region.
[62] Chase (1989a: 2).

"science city" and who had advised MITI and JAROS on ITIR's design.[63] During this meeting, the TIGER team leadership was presented with changes in the ITIR design that "came as a complete and unpleasant surprise;" these changes underscored the difference in preferences that had been expressed in the morning.[64] The Deputy Director of the MITI Space Industry Division provided a design update that took into consideration, he explained, a number of advisory consultations. These consultations included: discussions that were carried out in the fall among the user community in Japan (i.e., the natural resource industry); the work of other committees and instrument working groups; and conclusions from a research trip to the United States which had been made that past fall, during which, among other things, JAROS managers and scientific advisors consulted with Hughes Aircraft's Santa Barbara Research Center, the maker of the U.S.'s Landsat series of remote-sensing satellites, about remote-sensing technology.[65]

The design changes that the Deputy Director presented were the following: a decrease in the number of thermal infrared sensing bands from five to three; a relaxation of the spatial resolution of those bands from sixty to as much as ninety meters; an increase in the number of shortwave infrared bands from five to six; a decrease in the spatial resolution of those bands from fifteen to as much as thirty meters; and an increase in the number of visible / near infrared bands from one to three. Just as a head of state might say his or her hands were tied by parliamentary politics and that some less-than-ideal position would need to be accepted, MITI was saying to

---

[63] Included in this group were the Deputy Director of the MITI Space Industry Division (Mr. Tomita), Dr. Ono Akira who was the head of the thermal measurement section of Japan's National Research Laboratory of Metrology, Dr. Fujisada Hiroyuki from Japan's national Electro-technical Laboratory, and Dr. Satō Isao and Dr. Yamaguchi Yasushi from the Geological Survey of Japan. All of these individuals were named in trip reports (Kahle and Palluconi 1989: 2; Chase 1989a: 2).

[64] Kahle and Palluconi (1989: 2).

[65] Ibid., p. 2 and Chase (1989a: 1). The Landsat series of remote-sensing satellites was a workhorse for the geologic remote-sensing worldwide and was in many ways the de facto standard for geologic remote-sensing instruments, certainly in the United States (Mack 1990).

the TIGER team leadership that important discussions had already been completed, that these discussions had reached beyond MITI, and that the participants in these discussions had—as a group—decided what MITI's priorities were for ITIR's design concept.

The TIGER team leaders were disappointed, especially in the reduction of the number of thermal infrared bands, and their disappointment was "obvious" during the meeting, in Kahle's estimation. The Deputy Director of the MITI Space Division explained to the TIGER team that the reduction in thermal bands as well as the other changes was the result of a broad assessment that involved four factors: funding; other instrument requirements such as weight, size, and power; the state of sensor technology in Japan; and the needs of natural resource exploration firms in Japan. The Deputy Director also explained that at the top of the list of the requirements of the natural resource industry and their geologists were high-resolution stereo images and images in the shortwave infrared region.[66] When Kahle was asked by a senior scientific advisor to MITI and JAROS to list the most important differences between the revised ITIR design and TIGER, which was a question that basically invited Kahle to indicate what instrument characteristics she would prefer added or changed, Kahle noted five instrument characteristics. These characteristics included specifications such as the signal-to-noise ratio of the thermal radiometer and the spectral location of the sensing bands. At the top of Kahle's list was the number of bands in the thermal region.[67]

---

[66] In particular, in the 2.0-2.4 micrometer region (Kahle and Palluconi 1989: 2).

[67] Ibid., p. 3. To be more precise, at the top of her list was the number of bands in the 8-13 micrometer thermal region (and the spectral width of those bands) and the importance of at least one band in the 3-5 micrometer thermal region. The updated design of ITIR not only had fewer total bands in the thermal than the previous design had, but it also did not have a single band in the 3-5 micrometer region, which the TIGER team valued.

In subsequent meetings over the next two days, the TIGER team leadership met with key scientists who advised MITI and JAROS on the design of the ITIR instrument and explored how much leeway there was to implement design changes. Dr. Ono, who was head of the thermal measurement section of Japan's National Research Laboratory of Metrology, explained that the user committee had settled on two options, and a request for proposals had already gone out to contractors based upon those two options. The details of those two options were not shared with the TIGER team leadership. In her meeting with Ono, Kahle inquired "if it would be possible, in light of the additional goals of Eos [i.e., EOS] and the international community represented by the TIGER team, to convince [Japan's] user groups to add capability to ITIR in the thermal infrared and get that capability implemented."[68] With this rhetorical move, the TIGER team's goals were being asserted not only as the goals of the TIGER team, but also as the goals of EOS (and thus, by implication, the goals of NASA), and then, on top of that, as the goals of an international community. This was foreign pressure in the making—what politicians, diplomats, and political commentators in Japan commonly call *gaiatsu* (literally, outside pressure).[69] According to Kahle's trip report, Ono replied that "it was late, and strong user support would be needed."[70] With that response, Kahle, Palluconi, and Chase set up an appointment for the next day with a Dr. Ishii. The TIGER team leadership had learned from Ono that Dr. Ishii was the chair of JAROS's user committee and that he was a Professor of Geophysics at the University of Tokyo in the College of Engineering. So, in search of ways to influence the design of ITIR, the TIGER team went back to Tokyo.

While Kahle and the TIGER team had just come across Professor Ishii's name for the first time, Ishii was at that point a very prominent figure in Japan's geological

---

[68] Ibid.
[69] Schoppa (1993) and Johnson (1995).
[70] Kahle and Palluconi (1989: 3).

remote-sensing community. He had become an associate professor and then a professor of geophysics at the University of Tokyo after working over fifteen years in the oil industry.[71] When the TIGER team leadership met him, he was serving a second non-consecutive term as the President of the Society of Exploration Geophysics of Japan. He had already served a few terms as the Vice-President of the Remote Sensing Society of Japan and would become the society's President the next year. When the TIGER team leadership talked with him in 1989, he was a member of the Science Council of Japan, an advisory body to Japan's Prime Minister.[72] Kahle and her colleagues, however, likely knew none of this, other than that Ishii was chair of the user committee for ITIR and a professor at the University of Tokyo.[73]

The TIGER team leadership debated with Ishii the pros and cons of what the TIGER team thought was the too small number of thermal bands in ITIR.[74] According to Chase's trip report, Kahle "advanced several arguments for having at least five and preferably six bands while Prof. Ishii argued for four bands max. Dr. Kahle did admit that the ITIR probably would satisfy the needs of the operational community but not

---

[71] Professor Ishii's field at the University of Tokyo was "natural resources development engineering" (*shigen kaihatsu kōgaku*). His undergraduate degree was in physics, with a concentration in geophysics, and he preferred to describe himself in English as a "geophysicist." His doctorate was awarded by the department in which he was a professor at the University of Tokyo, natural resources development engineering.

[72] A brief biography of Ishii can be found at his personal website (in English, as well as in Japanese) which can be found at: http://ecosocio.tuins.ac.jp/ishii/myenvironmentalism/ishii.html, last accessed September 17, 2006. In the 1990s, Ishii became the Deputy Director General and then Director General of Japan's National Institute for Environmental Studies.

[73] Kahle and Palluconi noted in their trip report that "it was evident that Ishii was held in great respect," but they did not explain why he was held in such respect (1989).

[74] Tsu Hiroji, who at the time was the Chief of the Applied Geophysics Section of the Geological Survey of Japan, also attended this meeting, but judging from Kahle's and Palluconi's trip report and from Chase's trip report, he did not assume much of a role in the meeting. Again, while this was unknown to the TIGER team at the time, Tsu was then a Ph.D. student of Ishii's. Tsu and his colleagues at the Geological Survey of Japan were among those who had advised MITI on ITIR's conceptual design. Tsu would later become the chair of ERSDAC's advisory committee on the EOS science mission, and with that position, he would serve as Japan's science team leader for the ASTER instrument.

the international science community represented by her team."[75] This discussion could be interpreted as merely the deployment of haggling tactics over the number of bands, in which one side starts from a "low price" and the other from a "high price" in their attempts to arrive at a "middle price" that is closer to their preconceived preferences. That was certainly going on. Kahle had said on the first day of their trip that five bands would not satisfy the TIGER team's requirements. Now in day three, the TIGER team leadership was in the position of having to argue for, at a minimum, the restoration of five bands. Yet, this discussion was more than just an exchange of preferences about the desirable number of bands for the thermal infrared radiometer. After all, the relevant parties already knew the basics of each other's preferences by the first day. Ishii and Kahle were ascribing political meaning and stakes to their preferences, e.g., "operational" goals vis-à-vis "science" goals. Kahle asserted that the TIGER team's goals were not just science goals, or even just the goals of her team, but the science goals of an "international community" that her team represented. Ishii was advising an organization and users who needed to justify the instrument in terms of what it could do for Japan's pursuit of energy and mineral resources. Each of the two sides of these negotiations was politically explaining themselves and characterizing the other.

While reports of these particular discussions did not indicate in what way Kahle's team was said to have represented an international community, such representation was likely largely asserted by virtue of the international ambitions of EOS. Kahle's claim to representation can not be easily justified with reference to the national composition of the TIGER team. When Kahle had proposed her TIGER team, twelve of the fourteen investigators were of U.S. nationality. Before this meeting, one of the two who were not of U.S. nationality—an Australian national—had withdrawn

[75] Chase (1989a: 4).

from the TIGER team.[76] Was the anticipated participation of a single Frenchman among a dozen Americans enough for Kahle to say that the TIGER team represented an "international community?" Since Ishii, as well as MITI's other advisors, was perhaps already aware of the composition of the TIGER team—or at the very least was expected to soon become aware of its composition as consultations proceeded—Kahle's rhetorical framing of Japan's operational interests versus the interests of the international science community did not likely rest upon a mere willingness to strategically deploy the word "international" on the basis of one non-American among a dozen team members. Such a literal interpretation of "international" too narrowly captures this rhetoric. Amid the contemporary EOS rhetoric of "global" studies of the earth, and considering the explicitly "global" ambitions of the TIGER team's proposal,[77] it is just as conceivable, if not more so, that Kahle and the TIGER team leadership saw themselves as spokespersons for the international science community, especially in this situation of contrasting their "science" interests against the "operational" interests of their Japanese counterparts.

Neither the more exclusive interpretation, the one that accounts for Kahle's claim of international representation as only the deployment of strategic rhetoric, nor the more inclusive interpretation, the one that also allows for Kahle's own understanding of the identity of her and her team, can be conclusively justified over the other on the basis of this single exchange. The rhetorical framing of national operational goals versus international scientific goals, however, was persistent throughout the negotiations, even in circumstances in which it was obvious to all participants that the TIGER team was populated almost entirely of Americans and was sponsored only by NASA and the U. S. Government.

---

[76] Green (1988).
[77] NASA, Earth Observing System Science Steering Committee (1987b); Kahle and Palluconi (1988).

I suggest that to understand this rhetoric and its utility, the inclusive interpretation needs to be taken seriously. In the United States, “real” geologic remote-sensing science and especially “global” EOS science were considered to be endeavors of international scale, and if the TIGER team considered themselves to be doing real geologic remote-sensing science and to be a part of the EOS enterprise, which they certainly did, then their science was by association international, even if the science in particular was only sponsored by the U.S. Government. The TIGER team leadership was funded by NASA, and they could claim to work on behalf of NASA, but they were not unambiguous state actors. They were rather liminal state actors who carried with them ideals of scientific autonomy and universalism which could be recalled and deployed when useful. Such an interpretation can only be justified with the benefit of additional illustrations that will arise in subsequent chapters. Nevertheless, I suggest that the TIGER team’s identification of their science with the international was not merely a strategic enactment. It was an assumption that could be strategic in its articulation. To say that the rhetoric was deployed strategically, which it most certainly was, is not to also say that the sensibility was disingenuous.

When these Americans claiming to represent international science pressed Dr. Ishii to justify scientifically the rationale for ITIR’s thermal bands, he supported his position by moving outside the rhetorical realm of his domestic “operational” concerns and into the rhetorical realm of “international science.” He cited a study that was also “international,” in that the authors were not Japanese. The study, in fact, was of American origin. Ishii explained that the design of ITIR “was taken directly and exactly” from a public-private study paper on space-based thermal instrumentation which was jointly produced by NASA and the company EOSAT (EOSAT was a company that in the late 1980s had taken over developing and managing the U.S.’s Landsat series of satellites in an ultimately failed attempt to privatize Landsat’s development and operations). This study, Kahle learned from Ishii, "was treated as something of a 'bible' in Japan."[78]

Kahle herself had actually chaired the Geology Working Group of that NASA-EOSAT joint study. If it was "treated as something of a 'bible' in Japan," Kahle could provide an authoritative exegesis. In response to Ishii's referencing of that study, Kahle explained that "the Eosat [i.e., EOSAT] report was strongly conditioned and constrained to fit within the modest modifications permitted by working within an existing design for the Thematic Mapper" [a satellite in the Landsat series].[79] Kahle added that a Landsat with more extensive thermal capabilities "was viewed by at least some of the EOSAT Working Group members as a preliminary step to a more capable instrument we hoped would be a part of Eos."[80] According to Kahle's and Palluconi's trip report, in this discussion Ishii eventually conceded that "three bands in the thermal infrared represented a step back from current capability . . . and did not consider that three bands . . . could adequately represent the spectral information content in this region."[81] Ishii asked the TIGER team leadership for copies of the TIGER proposal and a white paper that would justify additional bands in the thermal infrared region. He said he would discuss the TIGER team's arguments with the ITIR user committee. In this meeting, while common understandings of the capabilities of various designs were worked out, Ishii made no commitments or promises about changing ITIR's design. In these discussions, which pitted the preferences of the TIGER team against those of MITI and JAROS, Kahle and Ishii debated and exchanged opinions about instrument characteristics. Still, Dr. Ishii made it clear that the decision was ultimately for MITI, JAROS, and the JAROS user committee to make.

78 Kahle and Palluconi (1989: 4). The study paper is Putnam (1986).
79 Kahle and Palluconi (1989: 4).
80 Ibid.
81 Ibid.

Kahle and Palluconi took away from their exploratory trip to Japan that what the TIGER team wanted out of the ITIR instrument (and presumably what they hoped NASA wanted out of the instrument) and what MITI and JAROS wanted out of the ITIR instrument were two very different things—different capabilities, different operations, different measurements. In their trip report, which was distributed to the other TIGER team members as well as to NASA, Kahle and Palluconi wrote that:

> From the Japanese point of view, ITIR, as currently specified, strongly supports commercial exploration applications by incorporation [*sic*] and, in some areas, extending the most attractive exploration features of SPOT (high resolution stereo, 15 meter resolution, same orbit stereo not available with SPOT) [SPOT is another remote-sensing satellite], Landsat (VNIR and SWIR bands) and TIMS (three TIR bands). From out [*sic*] point of view, ITIR, as currentlyh [*sic*] specified, duplicates the VNIR and SWIR spectral coverage of the Eos HIRIS instrument, is a step back in terms of spectral coverage and sensitivity from the aircraft TIMS, and does not have the spectral bands, spectral coverage, and sensitivity in the TIR to fully satisfy exploration requirements, let alone research requirements.[82]

Kahle and Palluconi did not seek to transform this techno-political fact by trying to mandate changes that directly competed with MITI's objectives for ITIR or by trying to convince the JAROS user committee to adopt as their interests those of the TIGER team or those of NASA (i.e., a move that would have been a Latourian translation). Rather, they conveyed to NASA Headquarters a "boundary object" strategy: the possibility of working with their Japanese counterparts to modify ITIR so that the instrument would be better able to accomplish the TIGER team's goals without trying to challenge or dominate those of MITI and JAROS:

> On the positive side, we found the Japanese to be very open, direct, communicative and quite willing to listen and consider what we had to say. There is a possibility (whose probability cannot be estimated) that the TIGER Science Team can influence the design and performance specification of ITIR; but the potential is likely limited to the addition of one or two bands,

[82] Ibid.

> extending the wavelength limit and improving the signal to noise ratio. It does not appear possible to incorporate the major design innovations of TIGER in ITIR nor to capture the full research potential of TIGER in some modification of ITIR. ITIR funding and motivation is provided by a Japanese user community, lacking first hand experience with multispectral thermal data, interested in a practical, bread and butter instrument that exploits existing remote sensing knowledge.[83]

Kahle's and Palluconi's assessment of the situation was not optimistic for fulfilling their research goals. The path they outlined for possibly influencing the instrument in order to achieve their goals (goals that might or might not have been NASA's goals) was not ambitious. They largely accepted at face value what they had been told about the realities of the situation by the Deputy Director of the MITI Space Industry Division, by Dr. Ono, and by Dr. Ishii. Even if they were able to modestly improve the instrument for their research goals—restoring the number of TIR bands back to five from three, increasing the instrument's sensitivity, etc.—the ITIR instrument would still be unsatisfactory for the research that the TIGER team members had conducted with the aircraft-based TIMS instrument and for the research that they had hoped to conduct with the TIGER instrument. ITIR was not going to meet their informational requirements.[84]

---

[83] Ibid., p. 5. This Kahle and Palluconi trip report should not be interpreted naively. They were, after all, writing to NASA Headquarters which was still considering how the TIGER team would or would not be incorporated into the ITIR enterprise and into EOS. For instance, Kahle and Palluconi might have shared with NASA Headquarters their counter-argument to Ishii's marshalling of the EOSAT study for the purpose of clarifying the value-added of the TIGER instrument to NASA Headquarters. For the arguments that I am advancing, however, the question of how Kahle and Palluconi crafted their report for the NASA Headquarters audience does not need to be answered, especially if another trip report, written for a completely different audience, is available for comparison and cross-referencing, in this case Chase (1989a). Kahle also distributed the Kahle-Palluconi trip report to her team members for their reference, so the trip report's audience can not be said to have consisted of only NASA Headquarters. Along with the Kahle-Palluconi trip report, Kahle sent a letter to the TIGER team (which was not copied to NASA Headquarters) which supports my description of the TIGER team as engaging "boundary object" negotiations to realize their goals: "I think that it is essential at the outset to play the game and try hard to make ITIR the instrument we require" (Kahle 1989c: 1).

[84] In addition to the fact that ITIR would have fewer thermal bands than required by the TIGER team's scientific goals, Kahle "steadfastly maintained" (Chase 1989b: 1) that to do their thermal

Yet, a cooperative arrangement with MITI on the ITIR instrument might be the best the ITIR team could do in terms of a thermal instrument. They proceeded with their "boundary object" strategy in an attempt to make the most of the situation. After their trip, Kahle wrote to her team members (a letter not copied to NASA) that "we need to consider what we should do if we fail with the Japanese. Quit?, [*sic*] Try to convince NASA to fly TIGER? Help the Japanese anyway? other options?"[85] Answers to these questions were months away. The TIGER team first had to find out what, if any, instrument modifications MITI and JAROS would accommodate and what NASA Headquarter's bottom-line requirements for ITIR were. Not knowing the answer to the second question, however, did not impede them from speaking for EOS's goals in order to accomplish the first. The TIGER team was not NASA, but as liminal state actors, they could translate and assert what EOS's goals meant for ITIR.

The white paper that Kahle and the TIGER team sent to Ishii in April 1989 to justify changes to the ITIR instrument illustrates two tactics of persuasion that Kahle and the TIGER team leadership used to try to ensure that ITIR would come as close as possible to satisfying their "informational requirements." The first tactic recognized and affirmed the natural resource exploration goals that MITI and the JAROS user committee had asserted for ITIR. This tactic argued that if ITIR were going to be used as a tool for resource exploration, ITIR would be much more effective if it incorporated certain technology improvements that the TIGER team also wanted—such as increasing the number of sensing bands in the thermal region. To make this point, Kahle compiled into the white paper arguments that were submitted by team members at Kahle's request.[86]

---

remote-sensing science, ITIR would need to be at least three times more sensitive than it was being designed to be (Kahle 1989d: 9).

85 Kahle (1989c: 1).

86 In contrast to the tactics and translations explicated by Latour, these improvements were not intended to function as a craftily designed "detour" (i.e., a bait and switch) to ultimately enlist

The white paper emphasized the evaluations of team members who had prominent reputations in the field of natural resource remote sensing. Kahle's presentation of these evaluations, which were listed under the heading of "user experience," endowed the evaluations with a testimonial quality. Kahle introduced team member Harold Lang as "a JPL expert in petroleum exploration and sedimentary basin analysis."[87] Lang's memo was titled "Comments on the Value of Thermal Infrared Capabilities of the ITIR Instrument as an Oil and Gas Exploration Tool;" Kahle selected from Lang's memo a passage that argued that ITIR's three proposed thermal bands "do not cover the appropriate wavelength interval," that "the spectral features of interest are on the wings of the proposed spectral bands" and that ITIR's thermal bands "will be of little value for oil and gas exploration."[88] Kahle also introduced Michael Abrams in her white paper as a "JPL geologist, and a world leader in remote sensing for mineral exploration." Abrams's comments concluded that "at least two bands long of the ozone absorption band are necessary to map these mafic and ultramafic rocks associated with gold deposits [ITIR only had one thermal band "long of" (i.e., beyond) the ozone band at this point]."[89]

In their discussions with the TIGER team leadership, Japanese geologists had used a certain published scientific article to argue that three thermal bands for ITIR would be sufficient. As it happened, much in the same way that it turned out that Kahle had been chairperson of a working group for the NASA-EOSAT study that Dr.

MITI and JAROS into NASA's center of calculation (see Latour 1987:113-5). Latour's analytical vocabulary and narrative form as exemplified in *Science in Action* can be arguably used to describe the making of technoscience as war, but it can not be used to describe the making of technoscience as diplomacy, in which the dynamic is one of achieving an advantageous compromise rather than a win at all costs.

87 In lay terms, sedimentary basins are depressed regions of the earth's surface where sediments have collected and become more compact. Most of the earth's hydrocarbon reserves are located in sedimentary basins (Kahle 1989d: 5).

88 Ibid., p. 6.

89 Ibid., p. 7.

Ishii referenced, one of the co-authors of the article that the Japanese geologists referenced was a member of the proposed TIGER team.[90] In her white paper, Kahle included the author's personal reply to the Japanese geologists' use of his article:

> Our enthusiasm in discussing the unexpected result has been misinterpreted as advocacy of few bands, but it is obvious from our work as summarized in Fig. 10 [of the paper at issue] that the larger the number of bands and the higher the resolution, the better.[91]

Kahle concluded her white paper by saying that the TIGER team had judged that five bands in the thermal region were necessary to achieve two objectives: the discrimination of rocks and minerals and the determination of the earth's surface temperature. The former objective for ITIR, which included the discrimination of natural resources, was one that had been emphasized by the Deputy Director of the MITI Space Industry Division and by Dr. Ishii (and also, presumably, the JAROS user committee); the latter objective for ITIR was a key measurement objective of the TIGER science team.[92] While those two objectives could be argued to be two sides of the same coin, that was not the rhetorical tactic that Kahle took in her white paper. As she had done elsewhere in her paper, Kahle recognized and accepted that her team and her Japanese counterparts had different goals for the ITIR instrument, and Kahle worked for ITIR to be an instrument that would meet everyone's needs. It was a "boundary object" strategy. Beyond the instrument itself, little, if any, "commonality of interest" was expected: you have your "operational" goals; I have my "science" goals; and let's make this instrument work for the both of us so we can each do our own thing our own way. Kahle wanted to share separately.

[90] Ibid., p. 8. It is unclear whether or not those Japanese geologists knew at the time that one of the authors of the paper that they cited was a member of the TIGER team.
[91] The words of Jack Salibury, quoted in Ibid., p. 8.
[92] Ibid., p. 10-11.

The second tactic of persuasion that Kahle used in her white paper was to portray the recommended improvements as not just significant to the TIGER team but also as significant to EOS (and thus, by implication, significant to NASA Headquarters, who would ultimately approve or reject ITIR for the developmental phase of EOS). Kahle's first tactic accepted MITI's and JAROS's operational goals and mustered scientific arguments for how design improvements could allow ITIR to better fulfill those goals. It rhetorically came at the political from the scientific. This second tactic assumed the necessity of the science and turned science into a political resource. In her cover letter for the white paper, Kahle listed in priority order the enhancements that she and her team recommended for ITIR's thermal instrument:

1. increase to 5 bands in the 8-12 μm region (three below the ozone band and two above)
2. NEΔT < 0.3 K [NEΔT is a noise specification, presumably at 300K]
3. 60-90 m spatial resolution
4. increase to 8+ bands in the 8-12 μm region
5. addition of 1 or more bands in the 3-5 μm region[93]

After the presentation of the list, Kahle commented that "The first three . . . are all important to achieving the goals of EOS."[94] Kahle did not write that those three recommendations were important for "the goals of the TIGER team." She wrote that they were important for "the goals of EOS." EOS was not mentioned anywhere else in the cover letter or in the eleven-page body of the white paper. But there it was, an extra rhetorical nudge, leveraging the state to advance her team's goals where possible. And at the bottom of the cover letter, among the names on the distribution list, were three individuals at NASA Headquarters: the Director of NASA's Earth Science and Applications Division, the EOS program scientist, and the special assistant for international programs at NASA. As a liminal state actor, Kahle could make claims of

93 Kahle (1989a).
94 Ibid.

representing an "international science community," and then in other circumstances, she could also credibly allude to working to achieve the technological preferences of the state that had, after all, asked her to influence ITIR's design on its behalf.

But what were the technological preferences of NASA, and more specifically, the EOS Project Office at NASA Headquarters to whom Kahle reported? Was NASA pushing for these improvements for ITIR's thermal infrared radiometer as well, and so, was Kahle merely passing on, rather than articulating, state preferences? Judging from NASA Headquarters' letter of introduction for the TIGER team, which was discussed above, it is reasonable to think that NASA might have been willing to go somewhat out of their way to support and accommodate an ITIR instrument that helped the TIGER team realize their research objectives. The goals and priorities of the TIGER team and those of NASA, however, were not identical. NASA's and the United States' goals for EOS in the late 1980s were to demonstrate U.S. leadership in space and in international cooperation and to build instruments that scientists could use to research global change, especially global climate change.

While NASA's handling of ITIR in 1989 supported these techno-political goals, NASA Headquarters did not independently press MITI or JAROS to adopt the TIGER team's preferences. In a "NASA mail" electronic message in November 1989, Dr. Dixon Butler, who was the EOS Project Scientist at NASA Headquarters and who had chaired NASA's EOS study groups throughout the 1980s, wrote to a team leader of a potential EOS instrument that:

> ITIR is quite secure on the EOS platform EOS-A because it is the *essence of cooperation* between Japan and NASA. The threats would only come from those whh [*sic*] don't like the Platform per se and want us to break up the payload into little pieces. The key selling point for ITIR on the first platform is actually the use of the thermal bands (*even if there were only 1*) to provide high spatial resolution surface temperature measurements to complement HIRIS and MODIS [other EOS instruments]. I know this must strike you as ironic. The geology [which had recently taken a back seat to climate change

> as a NASA priority] is really geting [*sic*] a free ride and if there is any need to augment the Japanese justifications and objectives of ITIR it will be with oceanography and land ecology aspects not better geology. The TIGER folk should work with and hopefully through the Japanese to get good communication going of the full motivations for ITIR (emphasis added).[95]

From Butler's e-mail, which was written several months after the TIGER team began their negotiations with MITI and JAROS, it can be inferred that NASA Headquarters would not have jeopardized its cooperation with "Japan" (or in this particular case, with MITI) in order to insist that ITIR have more than three sensing bands in the thermal region, an improvement that Kahle had judged to be important for achieving the goals of EOS. The TIGER team leadership had been pushing MITI and JAROS on their own for their own professional goals as much as for EOS's goals. For NASA Headquarters in 1989, the capacity of a couple of thermal bands to provide a rough estimate of land surface temperature and the capacity of ITIR to demonstrate U.S.-Japan cooperation in the Earth Observing System warranted the technological and economic cost of accommodating on the first EOS satellite ITIR's visible / near infrared radiometer, its shortwave infrared radiometer, and its thermal infrared radiometer. The stakes of demonstrating scientifically-credible international cooperation with Japan in EOS became even more important for NASA in 1989 and 1990 when NASA's prospects for international cooperation diminished elsewhere, specifically with the European Space Agency.[96] Consistent with the conclusion that the TIGER team pushed MITI and JAROS for instrument requirements that NASA had not defined as EOS requirements, and had done so without NASA guidance or assistance, neither Dr. Kahle nor Dr. Butler could remember any time in 1989 in which NASA Headquarters became involved with pressing MITI or JAROS to add bands to

[95] Butler (1989).
[96] Butler (2005), and see note 5.

the ITIR thermal infrared radiometer.[97] According to Butler, "we left it to Kahle and her team to make the scientific case to Japan [for their TIR requirements]."[98]

By no later than November 1989, and probably several months earlier in the spring of 1989, the Executive Managing Director of JAROS, the top official at JAROS, decided to follow the recommendation of the TIGER team. He stipulated that ITIR's thermal infrared radiometer should have five bands.[99] It was the Executive Managing Director himself who ultimately judged that, if Japan's contractor for the thermal sensor could build a thermal infrared radiometer with five bands, JAROS should direct them to make one (and by implication, that MITI should fund it), according to recollection of Dr. Fujisada Hiroyuki, the chair of JAROS's sensor committee for the ITIR instrument (i.e., the chair of the committee that had jurisdiction over matters relating to hardware and engineering for the instrument).[100]

Fujisada was at the time an engineer at Japan's national Electro-Technical Laboratory, which was an institute under the administrative jurisdiction of MITI. Before serving as an advisor to JAROS for the ITIR instrument, Fujisada had served as an advisor to JAROS for Japan's first earth resources satellite, JERS-1. He was well acquainted with MITI's efforts to nurture remote-sensing technology. In an interview

---

[97] Kahle (2005) and Butler (2005). When interviewed about this matter, neither Kahle nor Butler were still supported by, or working for, NASA. Kahle had retired, and Butler was working for the U.S. House of Representatives.

[98] Butler (2005).

[99] I have dated the decision to no later than November 1989 using the report of Stillman Chase, Santa Barbara Research Center (A Subsidiary of Hughes Aircraft Company) Internal Memorandum, December 5, 1989. The dating of the decision to the spring of 1989 is based upon Kahle's recollection of when she remembered the issue being settled (Kahle 2005).

[100] Fujisada (2003). The questions of at what cost—technological and financial—JAROS was willing to pay to support five bands in the thermal region was not addressed in the interview. Because potential contractors for the sub-instruments had already submitted proposals, and because those proposals and contractors had already been selected by JAROS by the time the decision was made to have five bands in the thermal sub-instrument, one can reasonably speculate that the cost of the two additional bands in the thermal region did not come at the expense of the two other sub-instruments or their contractors. More funds were very likely allocated to the ITIR instrument as a whole.

conducted over a decade after the Executive Managing Director's decision, Fujisada explained that, from his perspective, the decision to have five bands in the thermal was a "mission oriented" decision that came about from Kahle's request for at least five bands. He had debated with the thermal sensor's contractor, Fujitsu, whether or not the contractor could actually follow through and build a radiometer with five sensing bands (presumably within a certain set of time and budgetary conditions). Fujitsu convinced Fujisada that it could. According to the Executive Managing Director's earlier decision, it had already been decided that if Fujitsu thought they could build a five-band radiometer, then they should do it. For Fujisada, the meaning of the decision-making process suggested that JAROS was coming to do things under the direction of science leadership, in contrast to the "engineering first" approach used up to that date for satellites such as JERS-1.[101]

Dr. Kahle and the TIGER team had successfully "made the case" for significant design improvements for the thermal infrared radiometer. Those improvements, however, did not satisfy the science requirements of the TIGER team. Before the TIGER team leadership learned of MITI's and JAROS's scaling back of the thermal sensor's capabilities, they had wanted, at a minimum, five or six bands (they had proposed fourteen bands for TIGER). With respect to the signal-to-noise specification for the thermal infrared radiometer, Kahle had "steadfastly maintained that 0.1K [which was the signal-to-noise level of the 1980s TIMS instrument] is needed to do the science originally planned by the TIGER team," in the words of one contemporary report of the negotiations in 1989.[102] Yet, the TIGER team could only manage to negotiate a specification that allowed three times as much noise. For the sensor's spatial resolution, the TIGER team ended up with ninety-meter resolution in

[101] Ibid.
[102] Chase (1989b).

the thermal region, instead of the sixty meters that they had initially expected (they had proposed ninety meter resolution for the TIGER's mapping sensor, but TIGER also had a sensor that was a profiler). Although from Fujisada's perspective the ITIR instrument had taken a turn toward a "science mission," in Kahle and Palluconi's judgment, even with improvements, the ITIR's thermal infrared radiometer was designed to "exploit existing remote sensing knowledge," rather than to advance the science of thermal remote sensing.[103]

Several months into the negotiations in 1989, the TIGER team leadership had even persuaded NASA to allow the team to explore with their Japanese counterparts Japan's licensing of thermal remote-sensing technology from Hughes Santa Barbara Research Center (makers of Landsat), in order to boost ITIR's performance. MITI and JAROS, however, were not interested in exploring any licensing arrangements, preferring instead to develop the necessary technology on their own, provided this technology was judged to satisfy EOS requirements. As a result of Japan's rejection of any licensing arrangement, a TIGER team member—an instrument engineer—left the team. He worked for Hughes Santa Barbara Research and had hoped to use Landsat technology to improve the ITIR instrument (as well as, presumably, to improve Hughes's profits). From the perspective of one of the JPL scientists, Michael Abrams, who was involved in the meetings at this time, Japan's rejection of the Landsat technology "really emphasized the attitude that MITI/JAROS had, that they were going to build the technology from the ground up. . . . It was a constant struggle to try to get them to agree to improve the instrument."[104] Looking back, Abrams judged that improving the instrument's design was actually one of their significant achievements in the collaboration, but it was a mixed one:

---

103 Kahle and Palluconi (1989: 5).
104 Abrams (2001). See also Kahle (2005) and Chase (1989b).

> We can look at [the final design of the instrument] with some amount of pride at having partially succeeded. I'm not sure where I'd put the level of success compared to what we really wanted . . . 50%, 70%, I don't know. Certainly nowhere near 100%. If we had to design an instrument, it would be an entirely different instrument—different instrument, different capabilities, better capabilities. Given the constraints, the environment, we did have some significant impact upon improving the design. We did as much as we could. We pushed, pushed, and pushed.[105]

The improved design specifications for ITIR's thermal infrared radiometer did satisfy the requirements of EOS, both the version of those requirements which was privately stated by the chief EOS Project Scientist, Dr. Butler, in his e-mail in November 1989 described above and the version that Dr. Kahle articulated to MITI and JAROS in her April 1989 white paper. The two versions of requirements were significantly different. Dr. Butler required at most an instrument that demonstrated effective U.S.-Japan cooperation and an instrument that had a couple of thermal bands from which to calculate a rough estimate of land surface temperature. Dr. Kahle and her TIGER team were more demanding, and they were able to persuade their Japanese counterparts to provide much of what they requested. In fact, JAROS agreed to provide everything that Kahle called "important to achieving the goals of EOS."[106] Because Dr. Butler and NASA Headquarters did not intervene in the negotiations and maintained its bilateral form, leaving it to Dr. Kahle to be the "U.S." voice to work with "Japan's" MITI and JAROS to achieve the goals of both EOS and the TIGER team, MITI and JAROS were not able to distinguish between what Kahle and the TIGER team said EOS's requirements were for ITIR in the thermal region and what NASA Headquarters might have regarded as EOS's requirements for ITIR in the thermal region, if that office had indeed ever come to a decision on such a matter in the first place. MITI and JAROS were presented with only one U.S. position. To be

[105] Abrams (2001).
[106] Kahle (1989a).

sure, NASA's letter of introduction did not cast Kahle in the role of NASA's representative, and thus there was no reason for MITI and JAROS to presume that Kahle spoke with the official authority of the EOS Project and NASA Headquarters. Nevertheless, Kahle did speak for EOS by articulating to MITI and JAROS the specifics of EOS's goals for ITIR and what design specifications would satisfy EOS goals.[107]

Technoscientific Diplomacy as an Explanation

Using a strategy that conceived of ITIR as a boundary object and tactics that took advantage of assertions of both scientific knowledge and political power, Kahle persuaded her counterparts. The Executive Managing Director of JAROS became convinced that, first, Kahle's scientific argument was sound—that is, that these design improvements would help achieve science goals as well as MITI's operational goals. Second, he became convinced that these improvements were significant enough to the science goals of NASA's EOS that to brush them aside might risk jeopardizing ITIR's confirmation for the definition phase of EOS and along with it, ITIR's means of getting into space to orbit the earth. His conclusion was that these chances were not worth taking if Fujitsu, the contractor, could accommodate the improvements that Kahle had requested on behalf of EOS. While ITIR fell short of the TIGER team's ambitions in the thermal region and was therefore not an efficacious boundary object for them, Kahle had been able to "make the scientific case" for the specific improvements to ITIR's thermal infrared radiometer that she had herself named as being important for EOS, despite the fact that NASA's bottom line for improvements

[107] Such a role was a consistent with Kahle's statement of work to NASA, in which she stated she would review ITIR's "compatibility for the Eos [i.e., EOS] mission" (Kahle 1989e).

was not as demanding (which was a fact that was not communicated to MITI and JAROS). Thus, this outcome was not a self-evident meet-in-the-middle position between *a priori* preferences of each state. Nor was it a bargaining outcome, in the line with state-centered political realism. The outcome did not reflect each state's power—if power is conceptualized as an asocial material property of a state (whether located in state bureaucracies or Latourian centers of calculation which are marshaled in a "trial of strength"). Nor did the outcome straightforwardly follow from a common idea of what was best for the promotion of thermal infrared remote-sensing science, as an epistemic-community approach would suppose.

The outcome concerning the design of the thermal infrared radiometer was an outcome negotiated by liminal state actors who as mediators leveraged interdependencies between the two states to achieve their professional goals as well as state goals. NASA was relying upon MITI to provide an instrument with thermal remote-sensing capabilities, and MITI needed NASA to place its instrument into orbit. But what would the thermal capabilities of this instrument be? The TIGER team, in part to ensure the continuation of their work, negotiated and authorized a pragmatic compromise in the specifications for the thermal infrared radiometer. The compromise's specifications would allow MITI to fund the instrument; the specifications would provide justification for NASA's funding of the TIGER team to support the instrument; and this work would be carried out on behalf of achieving EOS goals—the meaning of which the TIGER team leadership themselves articulated. To effect this pragmatic compromise, the TIGER team leadership compellingly enacted both expert knowledge and state power, and MITI and JAROS took both of those enactments into account when they agreed to the compromise. Without the TIGER team leadership's assertion of scientific expertise and expert knowledge in the thermal infrared, MITI and JAROS would likely have not been "pushed," in Michael

Abrams's words, to have as many as five bands in the thermal infrared. Without MITI's and JAROS's reliance on NASA's EOS as a means to place ITIR into orbit, the TIGER team leadership would have had little with which to "push" MITI and JAROS to adopt the design specification for five bands. As is clear from which of the TIGER team's requests for changes in specifications were fulfilled and which were not (e.g., dramatic improvements were not made in the thermal sensor's signal-to-noise ratio), their scientific expertise and knowledge claims were alone not enough to effect their desired preferences. As liminal state actors, the TIGER team leadership convincingly asserted state power along lines of interdependency. Scientific knowledge and the techno-political power of states were used in concert. Only by understanding how they were used in technoscientific diplomacy can we convincingly account for the design of ITIR's (and subsequently, ASTER's) thermal infrared sensor.

CHAPTER FIVE

PERSISTENCE AND TRANSCENDENCE

Throughout the technoscientific diplomacy concerning the specifications for ITIR's thermal infrared radiometer, the future status of the TIGER team was unclear. NASA had accepted the team to assist with only the definition phase of the ITIR instrument which originally was planned to conclude in the fall of 1990. While the TIGER team was negotiating the specifications for the thermal infrared radiometer, they were working out a place for themselves in the project for the long term by, for instance, demonstrating "the benefits that a U.S. team might bring to this enterprise," to cite the words of the TIGER deputy team leader.[1] By January 1992, however, when the two teams were in the thick of their deliberations over the specifications concerning the ITIR instrument's shortwave infrared radiometer (which was another one of ITIR's three sensors along with the thermal infrared radiometer and the visible / near infrared radiometer), formal institutional arrangements for the development and execution phases of the ITIR project had been made. A "Japan team" for ITIR had been selected, and the TIGER team had become attached to the ITIR instrument as ITIR's "U.S. team." The ITIR instrument had been renamed the Advanced Spaceborne Thermal Emission and Reflection radiometer (ASTER), and, in the same move, the two teams together were beginning to be called the "ASTER team."

In early 1991, NASA Headquarters had also tentatively confirmed the ASTER instrument for flight on its flagship EOS satellite.[2] Although NASA's EOS suffered a series of dramatic cutbacks from late 1991 through 1992, the ASTER instrument was

[1] Palluconi (2001).
[2] Fisk (1991).

privileged to some extent throughout these "restructuring" and "rescoping" processes, since the instrument was perceived by NASA Headquarters as the lynchpin of U.S.-Japan collaboration in EOS. The instrument survived all of the progressively less-ambitious plans of NASA and its advisory committees which sorted out which EOS remote-sensing instruments would be onboard which satellites, satellites that were shrinking in capacity and number. In the discussions of these plans, ASTER's presumed importance for U.S.-Japan cooperation in EOS was explicitly noted.[3] In late 1992, when the negotiations over the specifications for the shortwave infrared

---

[3] See, for instance, the October 1991 and October 1992 Payload Advisory Panel reports (Moore 1991 and Moore and Dozier 1992b: 7,37), the March 1992 NASA Red/Blue team report, and the U.S.-Japan instrument trading scheme purported therein. Frank Palluconi made a presentation to the U.S. ASTER team in November 1992 in which he mentioned an instrument trading scheme that had been discussed in the September Payload Advisory Panel meeting, which was the meeting that produced the conclusions described in the Panel's October 1992 report. Palluconi noted in his presentation the importance of instrument trading for the entire EOS mission as well as for ASTER's slot on the U.S. satellite, EOS-AM 1 (later called "Terra"). According to this purported trading scheme, the United States would fly Japan's ASTER, and Japan would fly some U.S. instruments. In planning deliberations, NASA needed to make this trading scheme explicit because during this time ASTER's placement onboard the EOS-AM 1 satellite was being questioned by members of the EOS community, both on the basis of ASTER's value to EOS science (or lack thereof) and because it was possible that the seventh Landsat remote-sensing satellite might launch around the same time as ASTER. Since the Landsat satellite, as the name "Landsat" implied, would measure surface characteristics of the earth's land in a way broadly similar to ASTER, it was debated whether or not ASTER would be redundant (see U.S. ASTER Science Team 1992: 2 and Palluconi 1992).

It should be noted that Japan's space agencies, and more officially, Japan's high-level, interagency Space Activities Committee, might not have had the same understanding of this purported trading scheme as NASA did, or have any understanding of a trading scheme, for that matter. First, ASTER was being funded by Japan's MITI, and Japan's other instruments and satellites that were supposedly implicated in this scheme were being built by Japan's NASDA. It is far from clear that NASDA would view NASA's hosting of ASTER as benefiting NASDA. Secondly, neither a MITI project manager for ASTER in its early years nor a long-time participant in ASTER's development, both of whom attended meetings of the Space Activities Committee in the early 1990s, remembered any such trading scheme (Yokota 2004, Watanabe 2003). Furthermore, a trading scheme is not referenced in the U.S.-Japan Memorandum of Understanding for the ASTER instrument. Nevertheless, it is very clear not only from the 1991 and 1992 Payload Advisory Panel reports and Palluconi's report of the 1992 Payload Advisory Panel meeting, but also from the minutes of a 1991 Payload Advisory Panel meeting, that NASA and its advisory committees were under the impression that there was some sort of trading scheme in play (accounts differed, however, over exactly which instruments were involved in this scheme). The reports of the Payload Advisory Panel are cited above, but the minutes of a 1991 meeting of the Panel, a copy of which I read at the Jet Propulsion Laboratory's archives, have yet to be released after a request was initially made in 2001 (which has since been renewed yearly).

radiometer were concluding, the multi-year negotiations over a U.S.-Japan memorandum of understanding for the ASTER collaboration were beginning.

Despite these changes, which implied that the ASTER collaboration was becoming a distinct transnational, institutional entity unto itself, albeit one supported by national governments, the technoscientific diplomacy concerning most of the specifications for the shortwave infrared radiometer was still bilateral in its social form—like it was for the thermal infrared radiometer—and the artifact under negotiation was still approached as a boundary object. A joint ASTER science team had been officially established, but a group of American scientists presented a unified front and spoke for the "United States" and a group of Japanese scientists presented a unified front and spoke for "Japan." These two teams negotiated with each other how they could together design an instrument so that both of their goals could be satisfied, without requiring either team to necessarily buy into the goals of the other team and that team's state (i.e., they still attempted to design what scholars in Science and Technology Studies call a boundary object). As was the case with the two teams' design negotiations over the thermal infrared radiometer, their technoscientific diplomacy took into account both scientific knowledge claims and state power in their efforts to reach pragmatic compromises about the design of the shortwave infrared radiometer.

In the case of the thermal infrared radiometer, it had been members of the U.S. TIGER team who had enacted state power under the name of NASA's EOS in order to promote acceptance of their scientific judgment that at least five thermal bands were needed for EOS "science" goals, in addition to MITI's "operational" goals. In contrast, for most of the negotiations over the specifications for the shortwave infrared radiometer (which are described below), NASA had already accepted, if not strictly guaranteed, the ASTER instrument in its broad concept for flight onboard a NASA satellite. The U.S. team could no longer rhetorically exploit with much creditability Japan's dependence on the United States for the launch of the ITIR/ASTER instrument. After the U.S. team's incorporation into the ASTER enterprise, ASTER's placement onboard NASA's flagship EOS satellite became ballast for the ASTER collaboration, a shared weight of mutual dependency which stabilized the course of the collaboration for the Japan and U.S. teams. With the loss of leverage, enacting state power was now more difficult for the U.S. team. In the negotiations over the specifications of the shortwave infrared radiometer, the Japan team had the opportunity to wield their state's material advantage. Japan was physically building the shortwave infrared radiometer and was recognized as being ultimately responsible for its engineering. The Japan team did not bolster, however, their position using explicit assertions of state power in the two teams' negotiations over the shortwave infrared radiometer's design. Rather, it was the U.S. team's ascriptions of Japan's state power as well as each team's assertions and ascriptions of scientific knowledge which shaped the negotiations. Together the two teams enacted power, and in the enactment of Japan's state power, the U.S. team played a leading role.

This chapter describes the formation of the "ASTER team" as the "U.S." and "Japan" teams, detailing in particular the social makeup of the Japan team. It then compares two negotiations over the ASTER instrument's design, both of which were conducted in the same time period, soon after the formation of the "ASTER team." One concerned the design of the ASTER instrument's shortwave infrared radiometer and the other the design of the operational capabilities of the ASTER instrument's hardware. Bringing together the concern in Science and Technology Studies for accounting for ways of knowing and a social science emphasis on puzzle-solving, this chapter explains why, in the negotiation over the instrument's operational capability, the teams broke out of their bilateral diplomacy and subsequently began to forge a new international techno-political order, whereas in the negotiation over the shortwave infrared radiometer's design, they did not do so, even though both negotiations were conducted after the establishment of the formal institutional workings of the ASTER intergovernmental collaboration, unlike the prior bilateral negotiation over the thermal sensor. Thus, the chapter comparatively explains persistence and transcendence in the negotiating practices of the two teams' technical decision-making and collective scientific judgment.

The Formation and Constitution of the ASTER Team

The social makeup of the ASTER team circumscribed how the team's members took into account scientific knowledge and state power in their design of the shortwave infrared radiometer and the ASTER instrument's operational capabilities. Whereas the specifications of the thermal infrared radiometer were negotiated by the leadership of the TIGER team on the one side and the leadership of JAROS and its advisory committees on the other side, the negotiations between the sides over the shortwave infrared radiometer and the ASTER instrument's operational capabilities were conducted within and as a part of the "ASTER team." While the negotiations over the shortwave infrared radiometer were still as bilateral as the earlier negotiations over the thermal sensor, the socio-political context and process of this bilateral technoscientific diplomacy were more complex. The ASTER team's formation, constitution, and workings are critical to understanding how the U.S. team ascribed Japan's state power in the two teams' negotiations over the designs of both the shortwave infared radiometer and the ASTER instrument's operational capabilities, even when such power was not explicitly asserted by the Japan team. It is especially critical for explaining why, despite the formation of the joint ASTER team, bilateral diplomacy persisted in the negotiations over the shortwave infrared radiometer, and why the two teams transcended bilateral diplomacy in their negotiations over the operational capabilities of the ASTER instrument's hardware.

The TIGER Team becomes the U.S. Team

By early 1990, the U.S. TIGER team and the Japan ITIR team were discussing what role the TIGER team might play in the future ASTER instrument. As things stood, only a few TIGER team members had actually proposed to also be ITIR team members (for example, as a part of Kahle's ITIR team member proposal). If the ten or so other TIGER team members were to find homes on an EOS instrument team as NASA-funded researchers, the TIGER team would need to become integrated into the ITIR team. It was generally felt at the time that—true or not—the importance of the U.S.-Japan collaboration to NASA would provide some long-term security to the team's funding and work.[4]

In an interview, the TIGER deputy team leader, Frank Palluconi, explained to me the reasons why the ITIR/ASTER collaboration ultimately worked out for the TIGER team. He included among them that "in the long run, [the collaboration] gave us an instrument, which we might not have had otherwise." In response, I queried, "[but] you clearly didn't know all of this when you first decided to continue on with this [ITIR/ASTER] program." He replied:

> Well, we did know something of it because, uh, the Japanese made an offer at the time that everyone [i.e., all of the potential EOS instruments] was competing, and NASA made a commitment to the Japanese to fly their instrument. So, right from the time when we realized that ITIR had received that level of commitment from NASA, we knew that if we committed to this

[4] Kahle (1989c); Palluconi (2001); and Abrams (2001).

> that there was a different level of NASA commitment to this instrument than was true of any of the U.S. instruments.[5]

The TIGER team's incorporation into the ITIR team and their shaping of the design of ITIR were means through which NASA could have the collaboration that it wanted. They were also means through which the TIGER team could work with an instrument that might approach allowing them to conduct the research that they had proposed to do with TIGER.

Figure 5.1 (on the next page) is a diagram of the TIGER team leader's proposed reorganization of the ITIR team to accommodate the TIGER team (which had been accepted by NASA without the team's proposed TIGER instrument). "Phase B" was, in NASA terminology, the "definition phase" of EOS, which was scheduled to conclude at the end of September 1990, and "Phase C/D" was the "development and execution" phase. "EOS-A" refers to the satellite platform on which ITIR would orbit the earth, and it also refers to the EOS-A project office for the EOS-A satellite platform at the Goddard Space Flight Center. Figure 5.2 (on page 211) is a chart from the TIGER team's proposal to the ITIR team for how the TIGER team could help with the development and use of the ITIR instrument and the data and information system for the instrument. In addition to advising the ITIR team, the TIGER team was proposing to "help" with or "do" almost everything as well, with the exception of the physical construction and laboratory calibration of the instrument, two activities that the Japanese scientists themselves would not generally "do" either. Rather, MITI's and JAROS's contractors would.

NASA would be footing the bill to support the TIGER team's proposed work and the travel of the TIGER team members, if MITI accepted them into the ITIR fold. The TIGER team members would bring with to the ITIR project unequaled expertise

[5] Palluconi (2001).

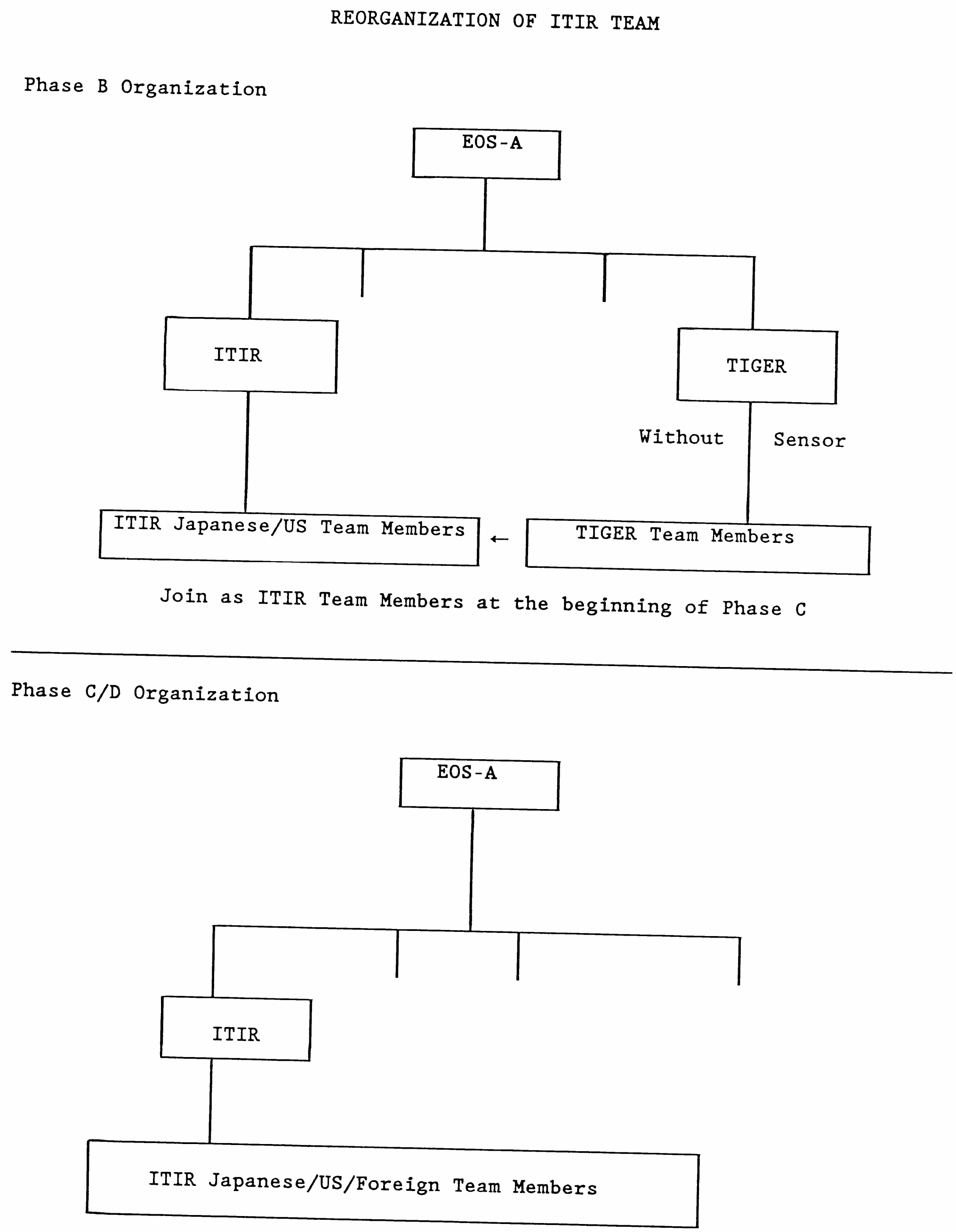


Figure 5.1: The TIGER Team's Proposal to be Incorporated into the ITIR Team (from Kahle 1990b)

"Phase B" was known as the "definition phase." Phase C and D were the "development and execution" phases. Phase C was planned to begin in October 1990.

TIGER TEAM / ITIR INTERACTIONS

	Advise	Help	Do
Functional Requirements	×	×	
Design	×	×	
Construction	×		
Calibr. -- Laboratory	×		
Calib. -- Inflight	×	×	
Integration		×	×
Testing/q.a.		?	
Operation (planning)	×	×	
Data System			
Standard Products		×	
Interface w/EOSDIS		×	
Atmos. Correct.		×	×
Data Verification (ground truth)	×	×	×
Data Utilization (analysis)	×	×	×

Figure 5.2: Proposed U.S. ITIR Team Tasks (from Kahle 1990b)

and experience in geologic remote-sensing. Yet, the Japanese scientists on the ITIR team were not quite sure that they could work with these Americans (nothing was said of the one Frenchman, a Frenchman who had yet to attend any of the meetings). Part of the problem was that the Japanese scientists considered the expertise and experience of the prominent TIGER team to be unequaled. In interviews, at least a few of the Japanese scientists recounted to me that when they first became involved in the collaboration they recognized the TIGER team researchers as among the top experts in the world in geologic remote sensing.[6] TIGER team scientists had written landmark papers and textbooks that Japanese scientists on the ITIR/ASTER team had studied in their education and had taken to be authoritative.[7] Dr. Yamaguchi Yasushi, who was at that time a geologic remote-sensing researcher in his mid-thirties at the Geological Survey of Japan, recalled that he and some of the other Japanese scientists wondered if they could work with such "big names." At the same time, the challenge of working with these big names was part of the excitement of the project for them.[8]

According to Kahle's recollection, MITI and the Japan team "out of the clear blue sky" invited the TIGER team to become not only ITIR team members but a "U.S. team," and they invited Kahle to become the "U.S. team leader" and not just the first among equals of the American members of an international ITIR team (as had been depicted at the bottom of figure 5.1).[9] Kahle recounted MITI's naming of a "U.S. team" and a "U.S. team leader" to me with the broad, knowing smile of the Cheshire cat. The TIGER team's transformation into the "U.S. team" and Kahle's appointment

---

[6] For some conspicuous co-authored publications, see Abrams, Ashley, Rowan, and Goetz (1977); the textbook *Remote Sensing in Geology*, Siegal and Gillespie (1980); Kahle and Goetz (1983); and chapters 13 and 31 of *Manual of Remote Sensing*, Colwell (1983). Not all of the team members became involved at the particular moment described above.

[7] The research of the members of the TIGER team is discussed in chapter three, p. 90-92.

[8] Yamaguchi (2002).

[9] Kahle (2003a).

as the leader of this "U.S. team" was evidently considered to be of good fortune, whether it came "out of the clear blue sky" or not.[10]

There is evidence to suggest that this propitious invitation might not have come "out of the clear blue sky." If MITI and the Japan team did initially broach the matter, Kahle certainly did not let MITI and the leadership of the Japan ITIR team forget it. In March 1990, Kahle proposed via fax to the leadership of the Japan ITIR team a list of tasks and responsibilities for the U.S. team leader position.[11] When Tsu Hiroji, a central figure in the Japan ITIR team and the Director of the Research Planning Office of the Geological Survey of Japan, replied in May with a formal invitation for Kahle to become the "ITIR Deputy Team Leader," Kahle on her copy of the fax crossed out "Deputy" and added "U.S." in front of "ITIR" to make "U.S. ITIR Team Leader."[12] The "U.S." signifier split, in name, the "ITIR team" into a "U.S. ITIR team" and a "Japan ITIR team," instead of just leaving it as the "ITIR team," which would have been a Japan-led international team (as had been depicted on the bottom of figure 5.1). The move also made room for Kahle to become a "team leader," which would be a position on par with the other team leaders of EOS instruments. It also would be on par with what Kahle's position would have been if her TIGER proposal had been fully accepted, rather than remaining a "co-investigator" on the ITIR team, which is how she had been accepted through her team member proposal for the ITIR instrument. This move to establish a "U.S. team" had been made possible by the TIGER team's diplomacy since the first Japan visit of the TIGER team

[10] The good fortune of this achievement was also hinted in documents from the time period which I collected, as will be explained below.

[11] Tsu (1990a). On March 6, Kahle also faxed to Tsu the proposals of TIGER team members to be ITIR team members (Kahle 1990b).

[12] Tsu (1990a). The Director of MITI's Space Industry Division, Obara Michio, later wrote Dr. Fisk, who was the NASA Associate Administrator for Space Science and Applications, and recommended that Kahle be appointed the "U.S. ITIR Team Leader." He listed a set of tasks and responsibilities that in all probability derived from the list that Kahle had forwarded Tsu in March, to which Tsu referred in his 1 May fax (Obara 1990).

leadership in March 1989, when discussions about the design of the thermal infrared radiometer had begun.

The stakes of the formal naming of a "U.S. ITIR team" and a "U.S. ITIR team leader" had just as much to do with Kahle's position vis-à-vis NASA and other American members of the ITIR team as it did with providing the American scientists with some collective bargaining power as a "U.S. team" in their negotiations with the Japanese scientists. In April, NASA's EOS Program Scientist had written to Kahle that:

> We [NASA] are very pleased that your Thermal Infrared Ground Emission Spectrometer (TIGER) Team has been able to effectively interact with the Japanese Intermediate Thermal Infrared Radiometer (ITIR) Team, helping to facilitate the design of the ITIR sensor. Considering this, and in response to the Japanese invitation for TIGER Team members to join the ITIR Team, we will need a proposal from each of your Team members to serve as a basis for supporting your involvement with ITIR Team activities. . . . In your capacity as *Lead U.S. Co-Investigators* [sic] *of the ITIR Teams*, we encourage you to work with your U.S. Team members to help coordinate their proposals.[13] [italics mine]

Now, with Kahle's change of "ITIR deputy team leader" to "U.S. ITIR team leader," as was written on her copy of the May 1st fax from Mr. Tsu, Kahle could answer this request from NASA for a proposal to be a "lead U.S. co-investigator" with a proposal to be an "ITIR team leader." Kahle submitted just such a "team leader" proposal to NASA Headquarters fifteen days after the fax from Tsu.[14] In that proposal, Kahle posited that her appointment as the "U.S. ITIR Team Leader" would keep "science high in the Japanese priorities as they proceed with the ITIR development."[15] The

[13] Wilson (1990).
[14] The other U.S. co-investigators had been members of the TIGER team. A few scientists who had not been part of the TIGER team had also proposed to be ITIR team members at the same time Kahle had submitted her ITIR team member proposal.
[15] Kahle (1990c: 1-2). The status of "team leader" was something that Kahle guarded not only in contractual language with NASA but also in her everyday work. Kahle held in her files a copy of the ITIR management organizational chart from 1990, a chart that was created by the Japan science

proposal's "overall objective" was "to maximize the capability and utility of ITIR for meeting the science goals of the EOS mission."[16] In that proposal, Kahle and her colleagues committed themselves to the ITIR instrument. When ITIR had been in competition with TIGER, Kahle and her colleagues had argued in their 1988 proposal that the two instruments were in fact dramatically different and so were their specific scientific objectives.[17] In the ITIR U.S. team leader proposal to NASA, however, Kahle now wrote that while her team members' "proposed approaches to using the modified ITIR instrument may be somewhat different from the original TIGER concept," "the science objectives are basically the same."[18] Kahle and her TIGER team members had lined up behind the ITIR enterprise.

The Elevation of Japanese Users, Researchers, and Scientists

This ITIR enterprise had been, until the TIGER team's involvement, primarily guided by JAROS management, the JAROS user committee that was chaired by Dr. Ishii, the JAROS sensor committee that was chaired by Dr. Ono, and industry hardware engineers (who comprised much of JAROS as well). These were the voices who had, in the mid-1980s, dominated the design of the MITI's mission instruments for JERS-1, Japan's first earth resources satellite. Users at ERSDAC, researchers at the Geological Survey of Japan, and academic scientists—all of whom were initially at the periphery of instrument design in 1988 but whose influence generally increased

---

team and faxed to her. She kept her undated copy along with her 1990 correspondence with MITI and the Japan science team leadership. The organizational chart included a box for the "US ITIR team," and it noted that Kahle was the "US Team Leader." Kahle had circled that box and wrote "note 'U.S. Team Leader.'" She was pointing that out to someone—to MITI, to NASA, or maybe to just herself. See figure 5.3.

16 Kahle (1990c: 3).

17 Kahle and Palluconi (1988: iii).

18 Kahle (1990c: 2).

through the early 1990s—were brought into the design process of the ITIR instrument by four changes. First, in a move similar to the United States' effort to privatize its Landsat series of satellites in the late 1980s (an effort that failed), the MITI Space Industry Division began talking about privatizing its satellite development and operations. According to this plan, the development of the JERS-1 and ITIR satellites were to nurture the remote-sensing industry, with the hope that the industry might eventually mature. For it to grow, MITI felt that it needed to demonstrate the utility of the technology to the natural resource industry that comprised ERSDAC and which was ERSDAC's main constituency.[19] Second, and related to the first, the MITI Space Industry Division, ERSDAC, and the MITI Space Industry Division's key advisors from its national institutes, recognized a requirement for satellite remote-sensing projects to take the needs of users (and potential users) into better consideration, whether or not satellite development was eventually privatized. The policy rationale of technology development as a good in and of itself was starting to run out of steam. This (contested) consideration of users authorized the voices of experts who were not driven by the technology development goals of the hardware makers.[20]

A third change that brought users at ERSDAC, researchers at the Geological Survey of Japan, and academic scientists further into instrument design was that MITI had proposed ITIR as an EOS instrument. Consequently, the MITI Space Industry Division required the expertise of scientists to justify scientifically to NASA that the ITIR instrument could be an EOS instrument. Japanese geologic remote-sensing scientists, in their own judgment, "had little power" and "little [project management] experience" up to the late 1980s compared with the hardware makers, but their

[19] Yokota (2004) and see chapter three, p. 97-99, 104-105.

[20] See the comments of Professor Nakayama and Yamazaki Akira in chapter three (p 104-105) and more generally, Ishii (2003), Yamaguchi (2002), Watanabe (2003), and the accounts in Sōritsu jūnenshi henshū iinkai (1993).

influence started to increase when the MITI Space Industry Division came to geologic remote-sensing scientists at the Geological Survey of Japan for their assistance with writing the ITIR proposal to NASA in the fall of 1987 and with that proposal's subsequent revisions in 1989 and 1990.[21]

The fourth change that moved Japanese remote-sensing users, researchers, and scientists closer to instrument design decisions in the late 1980s and early 1990s was that, according to the requirements for participation in EOS, MITI needed to constitute something that they could label a "team" of researchers to participate in the EOS enterprise, a team that would, as of spring of 1990, collaborate with a "U.S. ITIR team" composed of well-known geologic remote-sensing scientists. Over the next fifteen years, the collaboration between these two teams under the banner of EOS would ultimately be a socio-political mechanism and catalyst for opening up the development and operations of MITI's remote-sensing system to many kinds of users. This U.S.-Japan collaboration, however, was driven and circumscribed by the particular social makeup of the two teams. While the social makeup and organization of the U.S. side did not change substantially between the negotiations over ITIR's thermal infrared radiometer and the negotiations over ITIR's shortwave infrared radiometer, the social makeup and organization of the Japan side changed dramatically, becoming more complex and diverse. Remote-sensing users, researchers, and scientists became part of the Japan team for the EOS enterprise. When the U.S. team of scientists pushed for instrument design changes, it was these Japanese users, researchers, and scientists—who had previously been peripheral to instrument design—that negotiated with the U.S. team first, rather than JAROS engineers and JAROS's long-standing advisors.

[21] Yamaguchi (2002) and chapter three, p. 135-137.

The Japan ITIR Team

In the deliberations over the thermal infrared radiometer in 1989, the TIGER team did not negotiate with the collection of Japanese scientists who would constitute the future Japan ITIR team. The Japan team for the ITIR instrument had not been selected at that point. JAROS management and Dr. Ishii had been the TIGER team's primary points of contact, along with Dr. Ono of Japan's National Research Laboratory of Metrology. The MITI Space Industry Division, JAROS management, the chair of JAROS's user committee (Dr. Ishii), the chair of JAROS's sensor committee (Dr. Ono and then Dr. Fujisada), and JAROS's engineering contractors spoke for the design of the ITIR instrument. While Dr. Ono and Dr. Fujisada became part of the ITIR team, it was not until a meeting at the end of November 1989 that the TIGER team met the core group of the collection of scientists and engineers who would become part of the future Japan team.[22] The Japan team was composed of members who were drawn from relatively diverse institutions (e.g., national labs, government agencies, industry consortia, and universities), and it was a complex conglomerate in terms of its organizational structure as well as membership. The team's core leadership, however, knew each other well, just as was the case for the U.S. team.

According to Yokota Makoto, who at the time was MITI's newly-appointed project manager for ITIR, MITI turned in 1990 to its Geological Survey of Japan and the "established network" from the JERS-1 project to constitute the Japan team for EOS.[23] In keeping with the advisory committee system described in chapter three, Japan's "ITIR team" was in its administrative structure a set of committees and

[22] Chase (1989b); Abrams (2001); Palluconi (2001); and Yamaguchi (2002, 2005).
[23] The following paragraph draws from my interviews of Yokota (2004), Yamaguchi (2002), Fujisada (2003), and Rokugawa (2003).

sub-committees which served as advisors to MITI by advising project managers at JAROS and ERSDAC (see figure 5.3 on the next page). Yet, in a break with management practices from the development of JERS-1, Yokota wanted these advisory committees to be more independent of the hardware industry and to be more involved in project management.[24] MITI as a bureaucracy might have wanted to encourage a source of technical information and expertise other than its contractors which would still be at the service of the bureaucracy. In my interview with Yokota, however, he only alluded to this motivation in passing, commenting on contractors' conflict-of-interest in giving technical advice to the state. He did nevertheless matter-of-factly recall that he thought at the time that the state could make better use of its researchers at MITI's national institutes by directly involving them in project management, an activity that had in the past been dominated by the hardware industry, Japan's National Aerospace Development Agency, and the JAROS consortium:

> I recognized that researchers at the national institutes [collected under the umbrella of MITI's Agency of Industrial Science and Technology] were cognizant of their duty toward matters of confidentiality for the reason that they were national civil servants, the same as us bureaucrats. I also recognized that their interest in the success of state projects as a whole was strong. And, at the same time, I thought that, provided managers took industrial research and administrative collaborative projects in a good direction, these researchers at the national institutes should not just be conducting research themes one-by-one but should use their knowledge and ability in a practical way, as staff for the project management side. I had conveyed these thoughts to many of my senior managers [in previous MITI projects], but they were not used. . . . the [ITIR/ASTER] project had just began constructing the project system ["*taisei*"] exactly at the time that I assumed my post, so the project was at a stage where I could try and test my ideas. To the researchers at the national institutes (the core being Tsu Hiroji of the Geological Survey of Japan, Fujisada of the Electro-technical Laboratory, Ono Akira of the National Research Laboratory of Metrology), I entrusted important project management duties (such as the adoption or rejection of

[24] Yokota (2004).

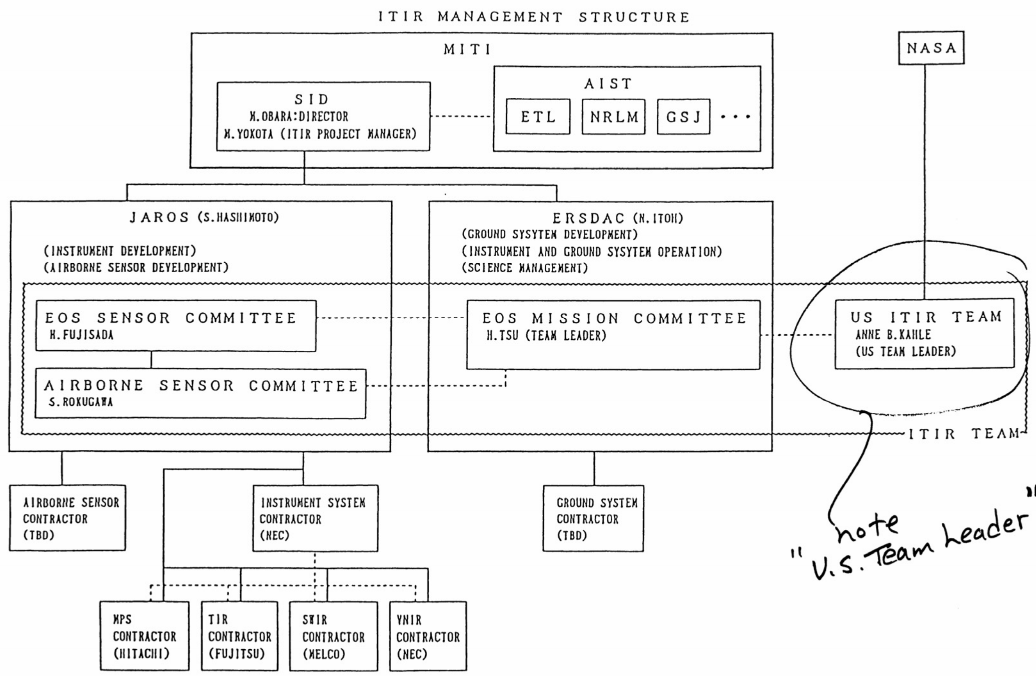


Figure 5.3: Kahle's Copy of ITIR Management Structure
(from Tsu 1990 and Kahle 1990b)

Acronym List

AIST: Agency of Industrial Science and Technology (a collection of MITI national research institutes)
ERSDAC: Earth Resources Satellite Data Analysis Center
ETL: Electro-technical Laboratory
ITIR: Intermediate Infrared Thermal Radiometer
GSJ: Geological Survey of Japan
JAROS: Japan Resources Observation System Research Organization
MITI: Ministry of International Trade and Industry
NASA: National Aeronautics and Space Administration
NRLM: National Research Laboratory of Metrology
SID: Space Industry Division (of MITI)

> contractor proposals concerning scheduling, development, and the technical sides of things), and I mostly made use of their judgment.[25]

The researchers at the national institutes who were named by Yokota in the above interview excerpt were from the JERS-1 "established network," but in the JERS-1 project, MITI had not "entrusted important project management duties" to them. For the ITIR/ASTER project, MITI and its consortia elevated these researchers from MITI's national institutes to project management. Dr. Fujisada Hiroyuki of MITI's Electro-technical Laboratory had advised the development of components for the JERS-1 sensor and had also worked on the development of electro-optical semi-conductors for military sensors. For the ITIR project, the MITI Space Industry Division, in consultation with JAROS, named him chair of the hardware advisory committee for ITIR, which is shown in figure 5.3 as the "EOS Sensor Committee." Dr. Ono of MITI's National Research Laboratory of Metrology had previously chaired the EOS Sensor Committee, and when Dr. Fujisada was appointed chair of the committee, Dr. Ono continued to play an important role in the committee and continued to chair the subcommittee for instrument calibration. Both Ono and Fujisada had been involved in the negotiations over the thermal infrared radiometer back in 1989. These two researchers were thought of, by MITI management as well as by themselves, as the "neutral parties" who were responsible for leading the committee and mediating "the exchange of ideas" between the hardware makers and users (the latter of whom MITI initially considered to be mainly natural resource exploration and exploitation firms).[26]

---

[25] Yokota (2004). Other interviewees, especially Yamaguchi (2002) and Fujisada (2003), collaborated Yokota's story, praised Yokota's managerial skills, and recognized his contribution as a key element of ITIR/ASTER's history. Interviewees were not so kind to other MITI bureaucrats.

[26] The first quote is from Fujisada (2003), but Yokota (2004) also cast Fujisada and Ono as neutral mediators between users and the hardware industry. The second quote is from Yokota (2004), who further commented that Fujisada and Ono carried out this responsibility with "great effort" and that this was an important reason for what he regarded as the committee's "smooth" workings and the

The EOS Sensor Committee was composed of about a dozen members who were drawn from ERSDAC, NASDA, and the national institutes that were under the umbrella of MITI's Agency of Industrial Science and Technology, such as the Electro-technical Laboratory, National Research Laboratory of Metrology, and the Geological Survey of Japan. A couple of members were university scientists.[27] The Sensor Committee itself did not include employees of JAROS (which was the organization that the committee was supposed to directly advise), hardware contractors (who had an obvious financial interest in the committee's decisions), or representatives from non-renewable resource firms (who were the users of immediate interest to MITI). Scientists and engineers who were affiliated with these excluded groups did, however, staff the sensor committee's four subcommittees—the ground system interface subcommittee, the calibration subcommittee, the cooler development subcommittee, and the platform interface subcommittee. While the groups that were deemed to be highly interested were excluded from the committee, the main committee nevertheless included scientists and engineers who had worked for non-renewable resource firms and hardware contractors for many years but who now worked for ERSDAC or other organizations as permanent employees (that is, employees who were not just on loan ("*shukkō*"), as some were).[28] MITI also delegated to Fujisada and Ono the selection of scientists outside of the JAROS and ERSDAC consortia who would advise on instrument issues, such as Dr. Sakuma Fumihiro, who was Dr. Ono's subordinate in the thermal measurement section at the National Research Laboratory of Metrology.

Larger and more diverse than the EOS Sensor Committee was the "EOS Mission Committee," also shown in figure 5.3. This committee was composed of

---

ultimate success of the ASTER instrument.

[27] See, for example, Shigen tansa-yō kansoku sisutemu kenkyū kaihatsu kikō (2000: 1-0-J-4).

[28] Ibid. and Fujisada (2003).

twenty-eight "user" representatives, including scientists from national institutes and organizations under the jurisdiction of MITI, such as the Geological Survey of Japan (6 committee members), the National Research Laboratory of Metrology (2), the Meteorological Research Institute (2), the National Institute of Environmental Pollution Research (2), the Electro-technical Laboratory, and JAROS. Moreover, the committee included university scientists (6) and representatives from oil and mining companies that were members of the ERSDAC consortium, namely, Japan Petroleum Exploration Company (JAPEX), JAPEX Geoscience Institute Inc., Mitsui Oil and Smelting Company, and Teikoku Oil Company. Finally, the EOS Mission Committee included representatives from government ministries, such as the Ministry of Construction, the Ministry of Agriculture, Forestry, and Fisheries, and public corporations (i.e., *tokushū hōjin*), such as the Institute of Physical and Chemical Research and NASDA.[29] As the EOS Sensor Committee was supposed to do for JAROS, this EOS Mission Committee was intended to serve as an advisory committee for ERSDAC and its ITIR/EOS program manager, and thus the committee's members did not include employees of ERSDAC, although the committee obviously did include representatives from companies that were corporate members of the ERSDAC consortium.

Note that this new "user committee" was formed under ERSDAC, rather than under JAROS, where the user committee chaired by Dr. Ishii had been located during the time of the U.S.-Japan negotiations over the thermal infrared sensor. The MITI Space Industry Division and, specifically, Mr. Yokota, pushed for the move of the user committee to ERSDAC, owing to the four changes that led to the elevation of users, researchers, and scientists which were noted above.[30] While this move certainly

[29] For a listing of the members of its first two years, see Sōritsu jūnenshi henshū iinkai (1993: 106-7). Also see MITI (1991: 2-15 – 2-17).
[30] Yokota (2004); Yamaguchi (2002); and Yokota (1990).

signaled new circumstances (e.g., involvement in EOS) and policy changes (e.g., the promotion of users)—both of which brought new opportunities for ERSDAC's user community, for research scientists at MITI's national institutes, and for university scientists—the move was implemented in a way that also allowed for a definite sense of continuity. Mr. Tsu Hiroji, who was at the time the Director of the Research Planning Office of the Geological Survey of Japan, was named in the spring of 1990 the chair of this new EOS Mission Committee. In the words of Mr. Yokota, Dr. Ishii had, in consultation with MITI, "passed the baton" of the user committee in the spring of 1990 to Mr. Tsu, whom Ishii held in "great trust."[31] Mr. Tsu was at the time a Ph.D. candidate who was advised by Ishii, in addition to being a senior scientist-administrator at the Geological Survey of Japan.

Tsu himself brought onboard to the EOS Mission Committee other scientists and researchers, such as Dr. Satō Isao, Dr. Miyazaki Yoshinori, and Dr. Yamaguchi Yasushi, all of whom had worked for Tsu when he had been the Chief of the Applied Geophysics Section of the Geological Survey of Japan in previous years.[32] Tsu, as the chair of this new user committee, also became the leader of the Japan ITIR team, because the MITI Space Industry Division had recently judged that "user scientists will play a more important role in [our] EOS project from now on."[33] The title of "team leader" had been held for a short while up to that point by Dr. Ono, who was a specialist in instrumentation. Dr. Rokugawa Shuichi was named the chair of the committee that would supervise the development of the aircraft model of the ITIR sensor. Like Tsu, he had been recommended by Dr. Ishii. Rokugawa had been another of Ishii's students, and he held the professorship at the University of Tokyo from which Ishii had retired. In sum, this constitution of the two EOS committees in figure

---

31 Yokota (2004).
32 Yamaguchi (2002) and Miyazaki (2003).
33 Yokota (1990).

5.3 integrated geologic remote-sensing users, researchers, and scientists more closely into instrument design than ever before in the history of the development of Japan's remote-sensing instruments, dramatically more so than they were in the development of JERS-1.

The Japan "Science Team" in the Japan Team

In the early 1990s, as NASA's EOS moved from the instrument "definition" phase and into the "execution" phase, the EOS "instrument" teams started to become called EOS "science" teams.[34] While the terms "U.S. ITIR team" and "U.S. ITIR science team" referred to the same collection of people, when that term was used by NASA, MITI, or members of the joint U.S.-Japan ITIR team, the Japan portion of the

---

[34] This change in terminology occurred throughout NASA's EOS teams in the early 1990s, but the change cannot be pinned down to a specific policy change at NASA Headquarters or to a uniform time (such as EOS's "new start," the start of the C/D phase). Different teams made the name change at different times and with different degrees of consistency. The MODIS team consistently called itself the "MODIS Science Team" as early as January 1990 (e.g., Salomonson 1990: 9), well before the beginning of phase C/D in October 1990. EOS Project Scientist Gerald Soffen, located at the newly-reorganized EOS Project Office that had moved from NASA Headquarters to the Goddard Space Flight Center in January 1990, referred in that same month to all of the "Science Teams" being formed (Soffen 1990: 1). Yet, a year later, NASA Associate Administrator Fisk in his official phase C/D selection letter to Kahle in January 1991, still referred to Kahle as an instrument team leader, not using the increasingly popular "science team leader" terminology (Fisk 1991). The U.S. ITIR/ASTER team made a clear change to the "science team" terminology in their documentation in the fall of 1992 (e.g., U.S. ASTER Science Team 1992), although the "science team" terminology began to be used here-and-there at team meetings over a year earlier (e.g., U.S. ASTER Science Team 1992: 50 and MITI 1991: 2-15). For some EOS teams, the name change was likely of little consequence, other than emphasizing that the teams were soon going to be "doing science" which they had proposed back when they were called instrument teams. Those instrument teams would have "done the science" whether they were called "instrument" teams or "science" teams (and the term did not imply a necessary change in personnel).

For the ITIR/ASTER joint team, however, as described below, the different terminology was sometimes used in ways that were consequential for the organization and workings of the collaboration, especially with respect to the management of boundaries vis-à-vis JAROS's EOS Sensor Committee and Goddard Space Flight Center's EOS data and information system personnel. I speculate that the workings of the ITIR/ASTER team were not unique in this regard. For example, the MODIS Science Team—based out of the Goddard Space Flight Center—recognized and distinguished themselves from a "MODIS engineering team" which was also at Goddard Space Flight Center (see, Salomonson 1990: 9).

"ITIR team" delineated in figure 5.3 was not synonymous with the "Japan ITIR science team." The combined membership of the EOS Sensor Committee and the EOS Mission Committee did not constitute the Japan "science team" for ITIR. When MITI, its consortia, and Japanese scientists did use the "science team" label to refer exclusively to a group of Japanese scientists, which was uncommon, they usually used it to describe a particular collection of Japanese scientists who contributed to what ERSDAC called its "science project." This science project came under the advisory purview of the EOS Mission Committee and had its own project manager at ERSDAC (which is not shown in figure 5.3 but which is alluded to with the "science management" duty that was listed in ERSDAC's box).[35] Most of the Japanese scientists who were members of this "science team" were also members of the larger EOS Mission Committee, but not all were (in 1990 as well as in the future).[36]

The members of this "science team," which MITI had constituted to fulfill the EOS requirement for a team of EOS researchers, did not so much work together as a distinct and autonomous functional group, but were rather integrated into the

[35] A reorganization of ERSDAC in 1993 made this "science project" distinction more explicit (Tsu 1993). Prior to 1993, Japanese-language documents did not refer to a "Japan science project" or a "Japan science team." Prior to 1993, if those terms were used by members of the Japan team, they were generally only spoken in English to English-language speakers, in an attempt to communicate smoothly in the categories of their audience or conversation partner. If the Japanese-language equivalent of "science team" was used (which was rendered in the Japanese language in *katakana*, which is a phonetic alphabet used for foreign loan words), it was typically used to refer to the U.S. science team for ITIR/ASTER or to the science teams of other EOS instruments, and not to a Japan science team. Consequently, members of the Japan ITIR team who spoke Japanese and English juggled various membership categories in their speech. On the other hand, the members of the U.S. ITIR team / U.S. ITIR science team regularly treated not only "U.S. ITIR team" and "U.S. ITIR science team" as terms that were interchangeable (or later, "U.S. ASTER team" and "U.S. ASTER science team"), but also the joint "ASTER team" and joint "ASTER science team" as interchangeable, and often even the "Japan team" and the "Japan science team" as interchangeable. Japanese-language speakers actually referred to the U.S.-Japan joint meetings in Japanese using the Japanese-language equivalent of "ITIR team meeting" or "ASTER team meeting." Not surprisingly, these different habits of speaking sometimes led to confusion regarding who authorized what, who was responsible for what, and who was doing what.

[36] For the list of ITIR science team members in 1990, I am using the list included in Kahle's ITIR U.S. Team Leader Proposal (1990c: 7). A different and more exclusive list that was provided to NASA is given in Kahle (1992: 14).

operations of JAROS, ERSDAC, and the larger Japan “ITIR team” (the Japan “ITIR team” that included the two advisory committees).[37] The “Japan science team” categorization was generally meant for NASA’s consumption or for those outside of the development and operations of the ITIR remote-sensing system, such as international audiences who were accustomed to talking in terms of EOS “science teams” (e.g., audiences at international scientific conferences and at EOS meetings that called for the participation of “science team” members, etc.). The term was used out of comfort and convenience in international situations, to avoid having to reference and explain the workings of MITI’s consortia and its advisory committees. Consequently, in addition to JAROS and ERSDAC, the salient group on the Japan side was, in general, not the “Japan science team,” but the much larger “Japan ITIR team,” which was more simply referred to as the “Japan team.”[38]

Kahle’s ITIR team leader proposal in May 1990 called the combination of the U.S. ITIR team and those individuals who were designated by MITI as the members of the Japan science team the “ITIR science steering committee.”[39] Kahle’s proposal implied that this “science steering committee” would be “steering” the development and operations of the ITIR remote-sensing system as a whole. The absence of this committee as a distinct entity in MITI’s and the Japan team’s diagram of ITIR’s management structure (figure 5.3) suggests that the “science steering committee” might have “steered” just “science,” however narrowly “science” might have been defined vis-à-vis technology and operations. In contrast, as was illustrated in the

[37] The ERSDAC science project did have working groups that met separately and regularly, and these mainly included scientists who were designated as members of the Japan “science team.”
[38] See, for example, the explanation of the workings of the Japan team offered in MITI (1991: 2-8 – 2-10). Also see discussions of the organization of the Japan team in the following interviews: Iwasaki (2002), Yamaguchi (2002), Maekawa, Muraoka, and Okada (2003), Watanabe (2003), and Yasuoka (2003).
[39] Kahle (1990c: 7). The same “science steering committee” formulation was used at the first ASTER team meeting in November 1990 (ASTER Science Team 1990: 125).

negotiations over the design of the thermal infrared radiometer, the TIGER instrument team, which was now the U.S. ITIR team, did not define the scope of their science narrowly, owing both to the long-standing practices of the geologic remote-sensing community from which its members were drawn and to the team's goal of improving the design of ITIR so that the instrument could more effectively support the scientific research that the U.S. team wanted to conduct.

Not only did the typical representation of the ITIR/ASTER management structure shown in figure 5.3 avoid any suggestion of something called "science" driving the ITIR project, the representation of the management structure also provided no indication of the institutional disparities among Japan's scientists in the ITIR team.[40] For example, university scientists in Japan were not compensated for the time and effort that they dedicated to the ITIR/ASTER enterprise, and they were not provided relief in any way of their usual university duties. All the other members of Japan's advisory committees who participated in the ITIR/ASTER enterprise did so in direct support of the organizations for which they worked (which were governmental organizations, national research institutes, or contractors billing the Government of Japan). Recognizing this disparity, Mr. Yokota, the MITI project manager for ITIR, broke new bureaucratic ground by allocating significant funds to JAROS and ERSDAC to support the travel of these scientists to international conferences to discuss the ITIR enterprise. International travel, at that time, was typically excluded from university funding or government research grants. Professors were known to pay for their international travel out of their own pockets. By funding international travel, Yokota—wanting the EOS collaboration with NASA to be successful and thinking that

[40] This representation would eventually be included in official planning documents exchanged between the NASA and MITI a few years later. For example, see the Project Implementations Plans (Goddard Space Flight Center 1995, 1997), and also JAROS's Nine Plans (MITI 1991: 8-47).

the participation of scientists was needed for that to happen—tried to encourage the involvement of select university scientists in the ITIR team, even while the ITIR enterprise in the late 1980s and early 1990s was, from the perspective of MITI, to support the needs of the oil and mining industry first and foremost.[41] In sum, the social makeup of the Japan "ITIR team" was quite diverse, much more diverse than the U.S. "ITIR team." Representatives of "science" from academia and research institutes were just one element of the complex conglomerate that was the Japan "ITIR team."

The Implications of Social Makeup for Negotiations

The social makeup of the Japan ITIR team, specifically the elevation into project management of scientists from MITI's national institutes and of scientist-users more generally, created an institutional division of responsibility and labor within the Japan team between user-scientists and engineer-makers which complicated the U.S. ITIR team's efforts to influence the design of the ITIR instrument. First, the members of the U.S. ITIR team were no longer the only scientist-users in the discussion. Japanese scientists could now readily speak for science, too, especially in areas of their expertise, such as shortwave infrared remote sensing. In addition, they were now in a better organizational position to effect their preferences than they were when the design of the thermal infrared sensor was being negotiated.

---

[41] Yokota (2004). In my interview with him, Yokota cited an annual travel budget of fifteen million Yen (in 1990) that he had allocated Fujisada and Tsu to distribute, as committee chairs, to the members of their committees and to other members of the Japan ITIR team whose travel expenses were not covered by their organization. At the time, fifteen million Yen was roughly one hundred thousand dollars. Compensation practices were apparently not standard across ministries in Japan. Like MITI, the Science and Technology Agency under the Ministry of Education during this time did not compensate researchers from national institutes for their support of government projects, but unlike MITI, the Science and Technology Agency did provide minimal compensation for consulting carried out by researchers at national universities. See Satō (2005).

A second reason why the social makeup of the Japan team complicated the efforts of U.S. scientists was that the scientists with whom the U.S. ITIR team would be dealing did not yet have at JAROS the seniority and influence of Dr. Ishii and Dr. Ono. Dr. Ishii and Dr. Ono were the two central individuals with whom the TIGER team had debated the specifications of the thermal infrared sensor in 1989. Dr. Fujisada (the chair of the EOS Sensor Committee), Dr. Tsu (the chair of the EOS Mission Committee and Japan's team leader), and the younger scientists who Fujisada and Tsu brought onboard the ITIR enterprise, all needed to work through the program managers and the bureaucratic process at JAROS and ERSDAC in order to develop and implement their technical preferences to a degree that the significantly more senior Ishii and Ono did not. Whereas the U.S. ITIR team expected that it would be easier to work with the scientists from the national institutes and universities than with the instrument engineers of JAROS and the hardware makers, the realization of this hope came at the cost of additional layers of bureaucracy.[42] The university scientists and the national institute scientists, the latter of whom were MITI's "neutral parties" who would supposedly span the divide between user-scientists and engineer-makers, were all liminal state actors. They were not unambiguous decision makers in terms of the state's bureaucratic delegation of authority.

The social complexity of Japan's project management structure and of the Japan ITIR team, encouraged, as we shall see, the U.S. ITIR team to ascribe moves of state power in the Japan team's knowledge-making, even when state power had not been explicitly asserted by the Japan team. Such an ascription was made in the two teams' negotiations over the specifications of the shortwave infrared radiometer. Despite the establishment of the joint team, these negotiations were bilateral in form

[42] This hope was suggested in the interviews of Kahle (2005), Abrams (2001), and Palluconi (2001).

and were approached as if the two teams were negotiating a boundary object in which everyone's "informational requirements" could be equally satisfied, as the U.S. team had approached negotiations over the thermal infrared radiometer. As in the design of the thermal infrared radiometer, a boundary object was not realized in the design of the shortwave infrared sensor. To understand why the two teams—negotiating under the name of a joint team—arrived at the specifications they did for the shortwave infrared radiometer, I trace how the social makeup of the U.S.-Japan ITIR team described above was implicated in the intertwining of the assertion of scientific knowledge and the enactment of state power.

Persistence of Bilateralism: The Shortwave Infrared Radiometer, 1990-1992

In November 1990, the first "ASTER team meeting" was held. JAROS had renamed the Intermediate Thermal Infrared Radiometer (ITIR) the Advanced Spaceborne Thermal Emission and Reflection radiometer (ASTER), and the joint U.S.-Japan team meeting was from then onward known as the "full" ASTER team meeting, dropping any national designation in favor of the solidarity afforded by the name of a shared instrument. At the beginning of the first meeting, Japan's team leader, Tsu Hiroji, explained the change in the instrument's name: the "ITIR" name emphasized the thermal bands, but the instrument's name should indicate that the instrument had sensing bands in the visible and shortwave infrared regions as well.[43] Because the light that the instrument would sense in the visible and shortwave infrared region would generally be sunlight that had been reflected off the earth's surface, rather than light that had been "emitted" from the earth as what is known as black-body radiation, the word "reflection" in the new name was supposed to indicate

[43] Tsu (1990b: 24).

that the so-called “reflection bands”—that is, the visible and shortwave infrared—were also sensed by the instrument. Years later, Kahle said that while she had given her approval to this new name, she did not select it herself.[44] Kudoh Masahiko, JAROS’s project manager for the ASTER instrument, explained in my interview with him that Dr. Ishii, who was a geophysicist, had actually chosen the name partly on the basis that “ASTER” supposedly sounded like English-language words for geologic minerals. Kudoh, on the other hand, said that he and the engineers who were working for JAROS liked the name “ASTER” because *asteracea* is the family name of the chrysanthemum, which is the seal and traditional symbol of Japan’s imperial throne.[45] Thus, at least the name of the ASTER instrument worked as a boundary object, not only between the U.S. and Japan teams but also between Japan’s scientists and engineers.

While everyone could agree on the instrument’s name, when it came to the specifications of the shortwave infrared radiometer, the technical preferences of the Japan and U.S. teams were not in agreement, nor were even Japan’s scientists and engineers initially in agreement. Figure 5.4 (on the next page) shows three alternative specifications for locations and widths of the bands for the shortwave infrared radiometer (SWIR). The band set at the top of the figure is not a band set for ASTER; it is the band set that was adopted for JERS-1’s shortwave infrared radiometer in the late 1980s.[46] JERS-1’s shortwave infrared radiometer was built by Mitsubishi Electric Corporation, and this contractor apparently preferred for the ASTER shortwave

[44] Author’s notes of Kahle’s remarks at a dinner reception for the Twenty-Fourth U.S.-Japan ASTER Science Team Meeting on May 21, 2003 at Nippon Seinenkan in Aoyama, Tokyo, Japan, and Kahle (2003a).
[45] Kudoh (2003). “Aster” means “star” in Latin, but this meaning was not mentioned as a reason for selecting ASTER as the name (acronym) of the instrument.
[46] Hino et al. (1991: 175).

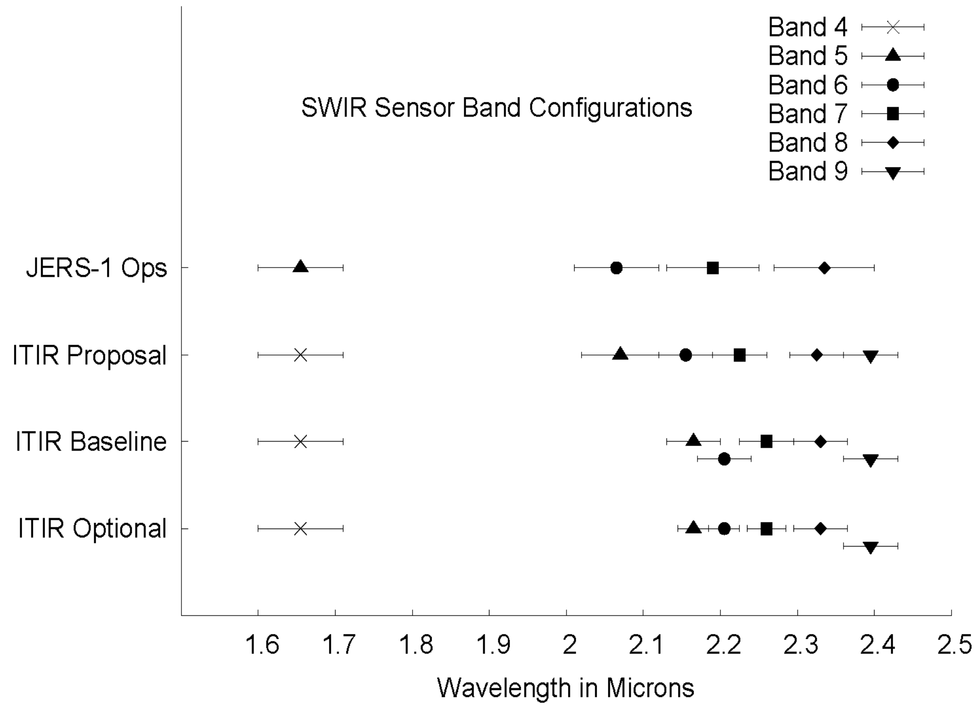


Figure 5.4: SWIR Sensor Band Configurations

The preliminary design review for the entire JERS-1 Optical Sensor (i.e., "JERS-1 Ops") was in 1987. The ITIR proposal for phase C/D, which was the development and execution phase, was submitted to NASA in May 1990. Both the ITIR "Baseline" and the ITIR "Optional" configurations for the SWIR sensor bands were presented at the first ASTER team meeting in November 1990. I have constructed this figure from sources that are noted in the text.

infrared sensor to refine the design that they had used for JERS-1.[47] Thus, Mitsubishi, in consultation with Dr. Ishii, Dr. Ono, and JAROS, proposed that ASTER feature nearly the same spectral coverage as JERS-1, but with JERS-1 bands seven and eight split into two bands each, for greater spectral resolution. This band set was what JAROS considered to be "officially accepted" through its decision-making process, and it was included by Tsu in MITI's rushed proposal to NASA. This band set, the "proposal" band set labeled in figure 5.4, remained in MITI's proposals to NASA through the C/D proposal in May 1990, where it was noted that the bands were still under discussion.[48] The "proposal" band set was also the band set that Kahle referenced in her U.S. ITIR team leader proposal to NASA in May 1990.[49]

Japan's user community working through ERSDAC did not regard the "proposal" band set as satisfactory for their oil and mineral exploration goals. ERSDAC had contracted Sumitomo Metal Mining Company to determine what band locations and widths in the shortwave infrared region might be most suitable for discriminating and identifying minerals that might arguably indicate the presence of mineral deposits.[50] The band centers that Sumitomo proposed, combined with the band width analysis of JAPEX Geoscience Institute, resulted in ERSDAC's advocacy of the band set listed as "optional" in figure 5.4.[51] The "optional" band set dropped band five from the "proposal" band set and split bands six and seven into three narrow bands (i.e., bands five, six, and seven in the "optional" band set).

---

[47] I was not able to interview the individual who was Mitsubishi Electronic Corporation's chief engineer for the ASTER shortwave infrared radiometer in 1989 and 1990. My account here of negotiations between engineers and users on the Japan side draws heavily upon my correspondence with, and the documentation of, Yamaguchi Yasushi, who was a key participant on the Japan side in the selection of the bands for the shortwave infrared radiometer (Yamaguchi 2002, 2005).

[48] Yamaguchi (2005) and MITI (1990:3-6).

[49] Kahle (1990c: 4).

[50] Sumitomo Metal Mining contractor report to ERSDAC, December 22, 1989, referenced in Yamaguchi (2005).

[51] Internal ERSDAC specification document for SWIR, January 26, 1990, referenced in Yamaguchi (2005).

What was unclear to the ERSDAC user community, however, was whether Mitsubishi could build such an instrument. Since it was considered inappropriate for the user community anchored around ERSDAC to interact directly with JAROS's engineering contractors, ERSDAC needed to re-open this issue with JAROS to find out whether or not their preferred "optional" band set was actually feasible. Instrument changes could not be officially spoken for without the approval of JAROS and the EOS Sensor Committee. Consequently, the difference in band preferences was a matter that needed to be formally negotiated between, on the one side, the ERSDAC project manager and key members of the EOS Mission Committee (such as Mr. Tsu, Dr. Yamaguchi, and a Dr. Watanabe Hiroshi, who at this time worked for JAPEX Geoscience Institute) and on the other side, the JAROS project manager and key members of the EOS Sensor Committee (e.g., Dr. Fujisada and Dr. Ono).[52] This was the institutional mechanism for arriving at a "Japan" consensus.

In the early spring of 1990, in a meeting between the user-scientist and engineer-maker sides, it was decided to stay with the Mitsubishi/JAROS "proposal" band set until signal-to-noise estimates were available from Mitsubishi for the "optional" band set with the narrower bands. The narrower the widths of the bands, the more difficult it would be to manufacture the bandpass filters and the less the amount of light that reached the sensors. No one knew at this point what the "optional" changes meant for the sensor's signal-to-noise ratio. Yet, signal-to-noise estimates were already available from Mitsubishi for the "proposal" band set, and because signal-to-noise estimates were required for the NASA C/D proposal, Tsu and MITI stuck with what they did know for use in the documentation for NASA.[53] Nevertheless, Yamaguchi, Satō, and Tsu—who were all at that time scientists from the

[52] Yamaguchi (2005).
[53] MITI (1990) and Yamaguchi (2005).

Geological Survey of Japan on the EOS Mission Committee—presented the "optional" band set as ASTER's shortwave infrared radiometer's preliminary band specifications in a 1990 presentation concerning the preliminary design of ASTER at a professional conference.[54] In contrast, Fujisada and Ono—who were instrumentation specialists from the EOS Sensor Committee—presented the temporarily-agreed upon "proposal" band set at a different professional conference in 1990 as ASTER's preliminary shortwave infrared band specifications.[55] The public professional literature at the time reflected the different preferences of the user-scientist and engineer-maker sides of the Japan team.

At the first ASTER team meeting in November 1990, the representative from Mitsubishi presented two band sets to the U.S. and Japan teams: a "baseline" band set that differed from the original "proposal" band set and the "optional" band set (see figure 5.4).[56] The "baseline" band set was a new band set in the mix. Fujisada, the chair of the EOS Sensor Committee, had requested that Mitsubishi report back Mitsubishi's signal-to-noise estimates for the "optional" band set that was preferred by ERSDAC users. At the same time, Fujisada also asked for Mitsubishi's performance estimates for a band set that covered almost the same total spectral range as the "optional" band set and which had the same band centers, but which had wider individual bands. This "baseline" band set required bands to overlap. Mitsubishi's performance estimates for what Fujisada rhetorically cast as the new "baseline" were reportedly intended to allow Fujisada the ability to isolate any difference in performance between the "baseline" and "optional" sets to the differences in their bandwidths, rather than to the spectral range or to band centers. This precise comparison was important for the Japan team's deliberations and negotiations with its

54 Yamaguchi, Satō, and Tsu (1992:303). This paper was presented in June of 1990.
55 Fujisada and Ono (1992:324). This paper was presented in September of 1990.
56 Ono (1990: 5).

contractor. Mitsubishi preferred to design a sensor with wider bands, because it was less difficult and because, reportedly, Mitsubishi's contract with JAROS was something closer to a fixed contract rather than a cost-plus contract. Consequently, Mitsubishi would bear some of the financial risk for any technological ambition on the part of MITI and the ASTER team.[57] Still, the user community in Japan had clear preferences that conflicted with those of Mitsubishi, and Fujisada was working to convince Mitsubishi to accept them.

In the November 1990 presentation to the ASTER team, months after Fujisada's request for performance estimates, Mitsubishi still did not present the requested performance estimates for either the "baseline" or "optional" band sets.[58] Because these performance estimates were not yet available, and because "recommendations are a part of the task of the ASTER working groups" that had been constituted at this first ASTER team meeting, the U.S. ASTER team members did not express a preference for one band set over the other. The U.S. ASTER team took the stance that they simply did not have enough information to even start to make a decision.[59] They planned to address the issue in the working groups once the performance estimates became available from Mitsubishi.

[57] Yamaguchi (2005).
[58] Ono (1990: 5).
[59] ASTER Science Team (1990: 3).

Japan Makes a Unilateral Selection

By the second ASTER team meeting six months later in May 1991, the Japan team had themselves chosen between the "baseline" and "optional" band sets, and consequently, by the second team meeting, the leadership of the U.S. ASTER team had concluded that they had been left out of the decision-making loop. Fujisada and Ono, who were both on the EOS Sensor Committee under JAROS, had presented the "optional" band set as ASTER's new specifications at a well-known professional society meeting in the United States the month before the second ASTER team meeting.[60] This list of the bands was a change from what they had publicly presented a year earlier, which had had the "still under discussion" disclaimer attached to it. Their new listing, however, had no such disclaimer attached, suggesting that the specification was no longer under discussion. Some of the U.S. team members attended this professional society meeting, and given that Fujisada chaired the session on the ASTER instrument, it would be very surprising if the U.S. ASTER team leadership were not aware of the Fujisada and Ono paper.[61] For that and other reasons, it can be concluded that the leadership of the U.S. team came to the second ASTER team meeting at least suspecting that the "optional" band set had been selected by the Japan team.[62] The U.S. team had had an opportunity to think through their approach to the situation.

In the opening plenary session of the second ASTER team meeting, the JAROS program manager for ASTER, Kudoh Masahiko, stated in his presentation of

[60] Fujisada and Ono (1991: 247). Mitsubishi, in their presentation at the professional society meeting was still holding out, however, and presented both alternatives as under discussion. See, Akasaka, Ono, and Sakurai (1991: 269).
[61] Documents exist that imply the U.S. ASTER team was aware. See, for example, Lang (1991).
[62] Michael Abrams, for instance, in advance of the meeting, had made tentative calculations of the disadvantages of the "optional" band set. ASTER Science Team (1991: 27).

the status of the ASTER instrument that detailed feasibility studies had been completed and that the shortwave infrared radiometer's "bands have been newly specified."[63] The Japan team had indeed chosen the "optional" band set. Kudoh's presentation of this decision surprised the U.S. team, perhaps because of the matter-of-fact way in which he presented a decision about which they had not been, and were not then, consulted. The presentation provoked a rare intervention by the representative from NASA Headquarters who was sitting in on this meeting, rare because representatives from NASA Headquarters typically left it to the U.S. team leadership to deal with complications that were regarded as just scientific or technical details. Maybe the U.S. team leadership had decided in advance that it would be helpful for their cause if they brought in bigger guns and if NASA were seen as raising the first objection to the unilateral decision. When the NASA Headquarters representative asked why the specification change had been made, Tsu, the Japan team leader, replied that "these specifications were changed by Japanese scientists' requirements."[64] The NASA representative retorted "NASA HQ is interested in this kind of specification change."[65] Discussion then broke out about having better communication, which might have relieved tension surrounding this sensitive topic of sharing design authority. Kahle commented that the U.S. team needed to work to achieve better communication, but the public nature of her comment—which was made in front of the entire ASTER team at the plenary meeting—implied that it was not the U.S. team alone who could make good communication happen.

Later in the meeting, in a working group, Kahle raised again the issue of the unilateral change in instrument specifications, but she made her point more directly. According to the working group's minutes, which in this case were taken by a staff

[63] Kudoh (1990: 3).
[64] ASTER Science Team (1991: 14).
[65] Ibid.

person working for ERSDAC who was also likely a translator, Kahle "made a comment that the U.S. ASTER team would like to be informed of any consideration of changes to the instrument that would affect on science [*sic*]."[66] Yokota, the MITI project manager for the ASTER instrument, also attended this working group meeting along with Kahle. His reply to Kahle's remarks suggests how entrenched in the Japan team was the approach of putting technology and engineer-makers first, especially given that Yokota himself had repeatedly pushed for greater user involvement. Yokota "explained the Japanese situation that equipment accommodation would be decided through consultation with [the] Goddard Space Flight Center [who was integrating ASTER into the NASA satellite], therefore, there was some time lag to inform such instrumental changes to [the] U.S. ASTER team."[67] The minutes recorded that the "*U.S. team* showed understanding toward [the] *Japanese* situation and stressed [the] importance of cooperation in information exchanges between [the] *two countries*" [italics mine].[68] Although it was not made clear how the changes to the bands of the radiometer related at all to the "accommodation" of the ASTER instrument into NASA's satellite (i.e., the instrument's integration), Yokota's explanation reveals a presumption that instrument personnel will sort matters out first and then report it to the science team. Even if Yokota's response is interpreted as merely a face-saving excuse, it still suggests that putting instrument personnel first was an appropriate practice. As the italicized words in the quotation suggest, the minutes also betray the bilateralism enacted in the meetings and the easy conflation of team, government, and nation.

The Japan side did not just make a decision that came across to the U.S. team as uncommunicative. It was unilateral. The Japan team skirted a decision-making

---

[66] ASTER Science Team (1991: 48).
[67] Ibid.
[68] Ibid.

process that had been previously agreed upon at the first team meeting—vetting this specific question about the bands of the shortwave infrared radiometer through the joint ASTER working groups (e.g., in the Geology Working Group).[69] But was not ASTER a Japanese instrument? Should not the Japan team have been the sovereign authority for determining the fundamental functional requirements of an instrument that the Government of Japan was funding? If those questions had been asked that way, in the abstract, the U.S. team leadership's answer to both of those questions would have been "yes" in the abstract. But in the particular, it was a qualified "yes"—that is, in effect, "no, not absolutely." According to Kahle's proposal from a year earlier of the U.S. team's tasks in the collaboration (see figure 5.2), and according to her U.S. ITIR team leader proposal and subsequent documentation, the U.S. team expected to "advise" and "help" with specifying the ASTER instrument's functional requirements.[70] In response, Tsu had written that he and MITI agreed with Kahle's task list.[71] Per the specification documents of both the Japan and U.S. teams, the "baseline" vs. "optional" question was undoubtedly a question that concerned a functional requirement.

The shared status of the responsibility for functional requirements, however, was not without ambiguity—not only in practice, but also on paper. In the official correspondence from MITI to NASA about Kahle's tasks as U.S. team leader, her tasks had been only summarized, and they had not been articulated as precisely as she had written them in her original task list. Providing advice on the instrument's functional requirements had not been specifically enumerated in MITI's

---

[69] ASTER Science Team (1990: 3). The minutes of the first ASTER team meeting had been written and compiled by the U.S. ASTER team, since the meeting was held at JPL. Japan team leader Tsu was on the distribution list of the minutes. The location and secretarial duties of the team meetings alternated between the United States and Japan.
[70] Kahle (1990c); Kahle (1992: 19).
[71] Tsu (1990a: 2).

correspondence with NASA.[72] Maybe the two teams had thought there was agreement when there was not. Or, put differently, maybe they had agreed on the broad principles, but the particulars of the situation were casting their agreement of broad principles as something rather narrow in terms of its practical applicability.

Clear agreement or not, NASA had charged the U.S. team with improving Japan's design basically as the U.S. team saw fit, provided that the improvements could be said to better support the broad goals of EOS. To carry out their charge and to fulfill their contracts, they needed to become part of the decision-making process. Being cut out of the decision-making loop could not become the norm if they were to succeed in "influencing the Japanese design." When I asked Kahle years later in an interview how the U.S. team fostered international collaboration with respect to an instrument that the Government of Japan was funding and that Japanese contractors were building, she replied "we [the U.S. team] just assumed [collaboration] and did it."[73] In keeping with that assertive and pragmatic outlook, in this instance the U.S. team did not refer back to the original two-page letter that MITI had sent NASA, which had invited the participation of the U.S. team, in order to try to sort out these matters of decision-making and authority in the terms of that official letter. Nor did they wait for some U.S.-Japan memorandum of understanding to guide them, a document that was not finalized until over four years later and which would, as it turned out, largely codify the terms of four years of collaboration.[74] What the U.S. team did was simply interrogate specific concerns in the meetings before them.

For the U.S. and Japan teams, U.S.-Japan collaboration was less something that was legislated or dictated from "higher-ups" than it was something that they

[72] Obara (1990). The description of roles and responsibilities provided in JAROS's Nine Plans (MITI 1991: 2-8 – 2-10) neither explicitly included nor excluded the U.S. Team from discussions concerning the ASTER's functional requirements.
[73] Kahle (2005).
[74] NASA and MITI (1996).

needed to forge in their practices when challenges arose. Such a challenge was designing the band specifications for the shortwave infrared radiometer. For the U.S. team, if they were going to stake out a claim to "have a say," then they could not neglect their responsibility to understand the design of the shortwave infrared radiometer. This was not a question of competing *a priori* preferences. At the first meeting, the U.S. team did not even know enough about the performance estimates of the design alternatives to form any firm preferences. On the other hand, the Japan team had formed their own preferences and had come to their own technical conclusions regarding an issue that was significant to them (i.e., the remote sensing of minerals), and their preferences were formed in a technical area in which they had expertise (i.e., shortwave infrared remote sensing). These kinds of discussions, such as the "baseline" vs. "optional" debate, while potentially of first order importance to the achievement of each team's goals, were not so much foreseen in advance of their collaboration as emergent through the process of collaboration and technological development. The socio-political dynamic was certainly not one of epistemic consensus, and nor was it one of "trials of strength" or bargaining. It was technoscientific diplomacy in which knowledge was being asserted, debated, and negotiated.

Negotiations in the Geology Working Group

The technical issue that subsequently arose in the Geology Working Group over the next few ASTER team meetings was the trade-off in the "baseline" and "optional" designs between the narrowness of the bandwidths for bands five, six, and seven and the effective signal-to-noise ratios for each of these bands.[75] The

[75] In their debates, the signal-to-noise ratio specification was usually expressed in terms of noise equivalent differential reflectance (i.e., NE$\Delta\rho$).

signal-to-noise ratios of these bands would also be affected by the diameter of the optics for the shortwave infrared radiometer, which was another issue that was under contention at this time. Fujisada's earlier introduction of the "baseline" band set into the planning discussions, which had been for the purpose of obtaining more information to use in the EOS Sensor Committee's and JAROS's negotiations with Mitsubishi, implied to the U.S. team that the baseline band set was actually a viable alternative for the Japan team (it had, after all, been named "baseline"), even while from the standpoint of the ERSDAC user community the baseline band set was not a viable alternative and did not merit consideration. For the ERSDAC user community, the baseline alternative was only supposed to be a bureaucratic device through which more information could be gathered from Mitsubishi about a "baseline" design, so that the EOS Sensor Committee and JAROS might improve their bargaining position vis-à-vis Mitsubishi. Mitsubishi, however, preferred the baseline design, since it was easier to manufacture and carried less risk on their part.

Then, in the middle of this negotiation within the Japan team, the U.S. team—likely unaware of the motivations behind the existence of the baseline band set—expressed a preference for the baseline alternative because its wider bands would ensure adequate signal-to-noise ratios, a specification that Michael Abrams, Simon Hook, and Harold Lang (all from JPL) and Lawrence Rowan (from the U.S. Geological Survey) had come to regard as critically important for discriminating minerals, based upon their experience with the Airborne Visible/Infrared Imaging Spectrometer (i.e., the AVIRIS instrument). These U.S. members of the Geology Working Group were not worried that band six in the baseline set overlapped with bands five and seven, as were the Japan members of the Geology Working Group, because the U.S. scientists said that they could use a mathematical technique to effectively separate (i.e., deconvolve) the two bands. In their reasoning, concern about spectral separation—which was the concern emphasized by Yamaguchi Yasushi of the Geological Survey of Japan and Watanabe Hiroshi of the JAPEX Geoscience Institute—was moot if there were not an adequate signal-to-noise ratio.

This U.S.-Japan debate was a scientific debate about means, not ends. In the debate, there was not any competition or exclusivity between the goals of the U.S. and Japan teams. Both teams acknowledged and addressed each other's goals in their bilateral debate (it was a bilateral debate in the sense that there were two sides, the U.S. side and the Japan side; it was not a debate among just team members). Both teams were out to design a shortwave infrared radiometer that worked as what scholars in Science and Technology Studies would call a boundary object.[76]

From the debate's beginning, when the U.S. team concluded by the second ASTER meeting that they had been cut out of the decision-making loop, this technical issue was inherently intertwined with the political issue of the scope of the U.S. team's role and authority in the design of an instrument sponsored by the Government of Japan. The politics of how the technical specification would be decided was just as important to the ASTER collaboration as the technical rationale of the decision. To understand why the ASTER team negotiated the particular design specification that they eventually did for the shortwave infrared radiometer's band set, and more specifically to understand why the U.S. team stopped pushing the issue when they did, we need to understand how scientific knowledge and state power were taken into account by the ASTER team, and in particular, how the U.S. team ascribed the exercise of state power to the Japan team, even if that power was not explicitly asserted.

---

[76] ASTER Science Team (1991: 27); ASTER Science Team (1992b: 20, 29, and 64); Yamaguchi (2002); Yamaguchi (2005); Abrams (2001); and Abrams (2005).

The issue under debate—the design of a sensor's band set—was similar to what the two teams had negotiated over two-years earlier in the case of the thermal infrared radiometer. Like in that case, in the negotiation over the shortwave infrared radiometer, scientific knowledge and state power were synthesized through technoscientific diplomacy by liminal state actors. Moreover, these liminal state actors—this collection of scientists—carried with them the practices, experiences, and concerns of two different communities of remote sensing, just as they had when they negotiated the thermal infrared radiometer's specifications.

Yet, in the case of the shortwave infrared radiometer, the socio-political context of the two teams' negotiations had changed significantly. The performances of these liminal state actors were more circumscribed by the constitution and workings of the ASTER team, especially the social makeup of the Japan team, with its scientist-user/engineer-maker institutional division of responsibility and labor. The "team" was a complex collection of scientists and engineers who worked for, overlapped with, and otherwise intersected the developmental institutions of Japan's space industry (figure 5.3). This team extended beyond the forum of the Geology Working Group where the negotiations about the radiometer's specifications took place, and the members of the Geology Working Group took into account the ASTER team's social makeup, workings, and collaborative arrangements. The negotiations differed from those concerning the thermal infrared radiometer in that NASA and its advisory panels had already expressed to Japan and the ASTER team their strong commitment toward launching the ASTER instrument. Bucking the U.S. team's preferences on a matter that could be written off as a scientific disagreement about means was much less likely to throw into jeopardy ASTER's placement on NASA's satellite than was the debate over whether or not the thermal infrared radiometer would satisfy what Kahle was able to label as the goals of EOS. Kahle and her colleagues were also now more invested in the ITIR/ASTER instrument and the ASTER team arrangement. Hence, not only were the scientists of the Geology Working Group positioned by their bureaucracies as liminal state actors, as Kahle and Ishii were in their negotiations over the thermal infrared radiometer, but their own collaborative arrangements circumscribed what enactments were feasible in their technoscientific diplomacy. Consequently, the dynamics and outcome of the two teams' negotiation over the shortwave infrared radiometer's bands were different than those of the negotiation over the thermal infrared radiometer's bands, even if the two negotiations were bilateral in their form.

State Power and Scientific Acquiescence

The ASTER team's ultimate decision about the band set of the shortwave infrared radiometer was the upshot of three socio-political conclusions drawn by the U.S. team members. Each of these conclusions implicated scientific knowledge and state power and was the consequence of enactments by liminal state actors which were circumscribed by the ASTER collaboration, particularly the social makeup and workings of the Japan team.

First, the social makeup and workings of the Japan team led the U.S. team to conclude again and again that the Japan team, and especially JAROS and its contractors, did not want to consult the U.S. team members in the instrument design process. Japan's unilateral decision initially set the stage for this conclusion. When questioned by the NASA representative at the second ASTER team meeting, JAROS Program Manager Kudoh and the Japan Team Leader Tsu responded firmly that the specifications of the shortwave infrared radiometer had been determined by the Japan team. Kudoh's presentation did not offer those specifications as a proposal or tentative plan to be discussed further. They had been formally approved by the EOS Sensor Committee and the EOS Mission Committee.

Nevertheless, U.S. team members did not let that decision stand without question. In order to understand the general specifications and performance estimates of the radiometer, U.S. team members specifically and repeatedly requested detailed technical information about the assumptions and calculations that justified the numbers that were given to them in presentations by Japanese instrumentation scientists and hardware contractors.[77] In particular, performance estimates of the signal-to-noise ratios of the bands were subject in the second and third biannual ASTER team meetings to robust and persistent questioning by U.S. team members, and these questions called for further information and documentation.[78] Judging from the many calls for "cooperation" in documents at the time, as well as from participants' recollections, the U.S. team members thought that answers to their questions were given only after much pressing.[79] Satisfying answers were particularly difficult to give and receive since it was JAROS's policy that U.S. team members were not allowed to communicate personally with JAROS's contractors, with the exception of the question-and-answer period after the contractors gave presentations at ASTER team meetings. Many of Japan's scientists operated under similar restrictions. At this point in the collaboration, all requests for technical information had to be handled by a member of the Japan team who would work the issue across the Japan team's institutional divide between the scientist-users and engineer-makers.[80]

---

[77] Kieffer (1990).

[78] ASTER Science Team (1991: 18, 27, 29, 48, 50); ASTER Science Team (1992b: 9, 20); and Yamaguchi (1992a: 2).

[79] For conspicuous calls for international cooperation, see ASTER Science Team (1991: 14, 48) and ASTER Science Team (1992b: 16). See also Abrams (2001), Palluconi (2001), and Biggar (2001).

[80] Biggar (2001), Thome (2001), Iwasaki (2002), and Yamaguchi (2002).

This activity, which can be described as “getting behind the power point slides,” is often present even in the least skeptical of scientific meetings and was in that sense not unusual. It was, however, much more socially marked and uncomfortable in the “thesis defense” type environment in which U.S. team members, as a matter of course, regularly asked Japan team members to defend instrument design decisions that the Japan team members had provisionally made without the consultation of U.S. team members, decisions with which the Japan team members were content and which they no longer questioned themselves.[81] Consequently, the U.S. team members generally did most of the questioning at the meetings. The repeated nature of these requests for information from U.S. team members, the frequent tardiness of the responses (which were almost always provided not at the meeting itself but weeks or months later), and the instability of the numbers that were eventually shared, all wore on the U.S. team.

The following contract report to NASA, written on behalf of a U.S. team member, suggests the degree of the U.S. team’s exasperation in their checking and interrogation of the Japan team’s calculations, calculations that were eventually provided after months of requests:

---

[81] Although his comments were not specifically about the negotiations concerning the shortwave infrared radiometer, Frank Palluconi, in my interview of him, alluded to how the U.S. team members were asking the Japan team and its contractors to demonstrate to the U.S. team that Japan’s measurements and calculations were valid:

> “Um, a lot of the discussion [concerning the instrument design] was about how you validate that you have met the performance in the specification. A good example of that, and a difficult measurement, is what is the out-of-band response. All of the radiometric and spectral characteristics have questions about how do you really measure them in the laboratory in a way that will completely satisfy the requirements or that will demonstrate that you have satisfied the requirements. The actual changes in the instrument [from U.S. team’s involvement] I think, um, were important, but they were not by any means wholesale. I think more of the discussion focused on issues that relate to the quality of the instrument and how you knew along the way that you were getting the quality built into the instrument that you wanted. So the discussions were pretty wide ranging, and I think that it was a different process for both the U.S. side and the Japanese side . . .” (Palluconi 2001).

> . . . [ASTER team member] Slater *re*calculated the [signal-to-noise ratios of the shortwave infrared radiometer] for all the gains presented by Dr. Fujisada and MELCO [Mitsubishi Electric Corporation] and for the *latest* values of array spectral quantum efficiencies, QEs, and system spectral transmittances. (Slater *points out that MELCO have presented at least five sets of QEs in the last six months* and therefore that the values used here are not likely to be those for the flight system. He anticipates that, for this and for several other reasons, his calculated SNRs may *only* be within +/- 20% of those of the flight system, although the relative changes of SNR with gain will be more precise, perhaps within +/- 5%).[82] (emphasis mine)

The parenthetical reference suggests that the patience of Dr. Philip Slater was wearing thin. Information gathering and the (re)checking of calculations were not fun work.[83] Michael Abrams, while not referring specifically to his experience in the deliberations over the shortwave infrared radiometer's bands in the Geology Working Group, was left with the general impression that during the negotiations over the design of the instrument, the instrumentation specialists and contractors "didn't want to hear from us, to be honest. They didn't want us there, I don't think. . . . They didn't want to be bothered by these scientists telling them that they were wrong."[84] During the first few years of their collaboration, the U.S. team found it awkward and tiresome to collaborate with the Japan team on issues concerning instrument design, including the specifications for the shortwave infrared radiometer, largely owing to the institutional distance between Japan's engineer-makers and the user-scientists. The U.S. team, however, judged this difficulty in collaboration to stem from a deep reluctance to consult with the U.S. team about the fundamental requirements of the shortwave infrared radiometer.

The ASTER team's decision about the band set of the shortwave infrared radiometer was a consequence of a second socio-political conclusion drawn by the U.S.

---

[82] Slater and Thome (1992: 2).

[83] An interview with the author of this contract report mentioned that Slater was often exasperated with the process of collaborating with the Japan team (Thome 2001). See also ASTER Science Team (1992a: 40).

[84] Abrams (2001).

team members concerning their collaboration: the U.S. and Japan team members who were negotiating this issue in the Geology Working Group were not seeing eye-to-eye and were not going to see eye-to-eye. Although the two sides were trying to create a boundary object that satisfied each other's "informational requirements," the two teams' scientific assessment of what specifications would satisfy those informational requirements were in disagreement. Their disagreement was sustained in part by the different methods the U.S. and Japan team members used to judge the adequacy of the shortwave infrared radiometer's signal-to-noise ratios. These different methods reflected differences in the practices and experiences of the two communities of geologic remote sensing from which the two states' teams were composed.

The adequacy of the shortwave infrared radiometer's signal-to-noise ratios came into question at the second ASTER team meeting, when a U.S. team member asked a representative from Mitsubishi about a key assumption that went into Mitsubishi's calculation of the estimates for the sensor's signal-to-noise ratios. U.S. Deputy Team Leader Frank Palluconi asked Mitsubishi what percentage of sunlight Mitsubishi had assumed would be reflected from the earth (i.e., the surface's reflectance). Mitsubishi's reply was considered to be unrealistically optimistic for the surfaces that the majority of users would want to target, unrealistically optimistic not only in the judgment of the U.S. team members but in the judgment of Japan team members as well (Mitsubishi had assumed a surface reflectance of seventy percent).[85]

Later in the ASTER team meeting—in the Geology Working Group the next day—Michael Abrams presented signal-to-noise calculations for the sensor's imagery based upon a surface reflectance of almost one third of what Mitsubishi had assumed

[85] The Mitsubishi presenter claimed that the EOS Sensor Committee of JAROS had given them the assumption. That the EOS Sensor Committee had passed on this inappropriate assumption was likely taken by the U.S. science team as yet another reason to be probing throughout the instrument design process (ASTER Science Team 1991: 16).

(Abrams used a surface reflectance of twenty-five percent). With that new assumption, the signal-to-noise estimates for even the baseline alternative, the one with the wider bands, promised just borderline performance, he argued.[86] The Geology Working Group resolved that "both Japanese and U.S. sides" would present "more realistic" calculations of signal-to-noise estimates at the next meeting. The members of the Geology Working Group who were from the Japan team emphasized, however, that the optional alternative for the sensor's band set was already "~~officially~~ formally accepted in the Japanese committees based on the study work during the past one year" (the double strikethrough is in the hand-written draft of the minutes, and the change presumably cast the Japan team's decision as slightly less unilateral and imposing).[87]

Despite Mitsubishi's use of what the Japan team recognized as a dramatically unrealistic assumption for estimating the shortwave infrared radiometer's performance, Yamaguchi and Watanabe in the Geology Working Group stood firm on their team's preference for the narrow band set specification (i.e., the optional alternative). Given their firm preference, either less-than-satisfactory performance would need to be accepted, or the signal-to-noise ratios of the sensor would need to be improved in ways that did not involve compromising on the narrow band set. The preference of the ERSDAC community had already passed through the Japan consensus process that included the EOS Mission Committee and the EOS Sensor Committee, and their preference was not going to budge, Yamaguchi and Watanabe asserted. These liminal state actors from the Japan team cast themselves as institutionally constrained. In this particular case, that constraint was fine with them, because it bolstered the position of their own scientific assessments. Unlike in the case of the negotiations over the

[86] Ibid.
[87] Abrams and Yamaguchi (1991).

number of bands in the thermal infrared radiometer, in which Kahle negotiated with Ishii and JAROS, Japan had its own geologic remote-sensing scientists at the table in this debate. If the U.S. team was going to have any chance at influencing this design specification, they would need to scientifically demonstrate that the specification would result in patently unacceptable instrument performance. Assertions of expertise and experience were not enough.

As a result of the U.S. team's outing of Mitsubishi's poor design assumption, JAROS pushed Mitsubishi to design a better sensor, a sensor that would at least meet the signal-to-noise ratio requirements that Mitsubishi had originally stated that they could meet before their poor design assumption had been exposed. Mitsubishi did come back with a better design, one that took advantage of the flexibility and willingness of NASA's satellite engineers to open up more space and cooling resources on the satellite itself in order to improve the instrument. At the third ASTER team meeting in January 1992, JAROS's ASTER Project Manager described Mitsubishi's shortwave infrared sensor as having "remarkably improve[d] signal-to-noise ratio[s]."[88] The diameter of the aperture of the sensor had been increased from 170 millimeters to 190 millimeters (to focus more light through the bands onto the actual detectors). The entire sensor had been placed horizontally on the satellite instead of vertically, to make use of a cold plate that NASA would provide on the satellite to stabilize the temperature of the sensor. Furthermore, the design of the coating for the bandpass filters had been improved (to allow more light to pass onto the detectors).[89] JAROS and Mitsubishi had not informed either the U.S. team or the user-scientists on the Japan team of the new performance estimates of this redesigned sensor before they announced the changes at the beginning of this third ASTER team

[88] ASTER Science Team (1992b: 73) and Kudoh and Koyama (1992: 6).
[89] ASTER Science Team (1992b: 9) and Michioka (1992: 2).

meeting. Consequently, the Geology Working Group at this meeting had little choice but to proceed to deliberate about whether or not the signal-to-noise ratios of this new design were satisfactory, using their own estimates of the new design's performance instead of Mitsubishi's estimates.[90]

Both the U.S. and Japan teams—that is, "both sides," in the words of the working group minutes—presented more realistic calculations of the shortwave infrared radiometer's performance, as they had said they would do at the last team meeting, but they presented different calculations. To be clear, they did not present different numbers for the same calculation; they presented different calculations. The two teams used different methods for assessing the adequacy of the shortwave infrared radiometer with the narrow band set (i.e., the optional alternative). Michael Abrams and Lawrence Rowan from the U.S. team showed a comfort with instrument design and used simulations of the signal-to-noise ratios for the sensor to argue for an increase of the sensor's aperture from what had been 170 millimeters to 200 millimeters (they had just learned at this meeting about Mitsubishi's new 190 millimeter design).[91] As was the case with the U.S. team's argumentation from the previous meeting, their calculations of signal-to-noise ratios assumed that everyone could recognize and agree upon sufficient signal-to-noise ratio numbers when they saw them. What the U.S. team members questioned was if the sensor would have the signal-to-noise ratio numbers that they desired. Thus, they did not present an assessment at this meeting of whether or not the numbers that they wanted were themselves adequate, or too demanding, or too loose. They thought that their preferred signal-to-noise ratio numbers would achieve everyone's measurement goals. The U.S. team members simply asserted that their preference for signal-to-noise ratios was

[90] ASTER Science Team (1992b: 20).
[91] Rowan (1992: 6). They also argued for a double gain option, which I will not detail.

based upon their expertise and prior experience as geologic remote-sensing researchers.

In contrast, Yamaguchi's and Watanabe's calculations took the tack of arguing that the narrow band set was fine for the task of spectrally discriminating the minerals that were of most interest to the ERSDAC user community (emphasizing spectral discrimination over signal-to-noise ratios, as the Japan team members of the Geology Working Group also did at the previous meeting).[92] The presumption of their method was that if the sensor were up to that empirical task, then the sensor would succeed elsewhere. Neither the assessment method of the U.S. team nor that of the Japan team was explicitly argued to be more flawed than the other in its assumptions. The two methods for assessing the adequacy of the narrow band set simply did not directly compete with each other. They were not mutually exclusive.[93] Michael Abrams was persuaded that maybe the baseline alternative's wide bands—which were 70 nanometer bands for bands five, six, and seven—were not as critical, given the sensor's new 190 millimeter aperture and the other design improvements that Mitsubishi had announced. He still, however, was not convinced that the optional alternative's narrow bands—which were 40 nanometer bands for bands five and six and a 50 nanometer band for band seven—would make a better sensor. For Abrams, without robust signal-to-noise ratios, the Japan team members' concerns about spectral separation were moot. He said he would study the advantages and disadvantages of 50 nanometer bands for bands five and six. This was a compromise position between the optional alternative's 40 nanometers and the baseline alternative's 70 nanometers.[94]

[92] Yamaguchi (1992a) and Watanabe (1992).

[93] I am not arguing that these methods were incommensurable. They were, in fact, interdependent and could be technically reconciled in theory. But they were not technically reconciled at this meeting.

[94] ASTER Science Team (1992b: 20).

Pressed by the U.S. team members, the Geology Working Group at this January 1992 ASTER team meeting recommended to the EOS Sensor Committee that a 200 millimeter aperture was needed, in order to distinguish minerals with low reflectance (e.g., an effective surface reflectance of twenty percent, taking into consideration variations in slope and vegetation of the earth's surface, etc.).[95] The two teams left the Geology Working Group with the "action item" to study again the shortwave infrared radiometer's signal-to-noise ratios, but this time using Mitsubishi's performance estimates of the new design, once Mitsubishi passed them along to the working group members through JAROS and the EOS Sensor Committee.[96] JAROS and the EOS Sensor Committee set a March 1992 deadline—less than two months away—for any recommended design changes. This deadline, they stated, would allow the fabrication of the engineering model of the instrument to begin with the start of the new Japanese fiscal year in April.[97]

After the U.S. team had recovered from being cut out of the loop of the radiometer design process a year earlier, after they had ferreted out Mitsubishi's inappropriate surface reflectance assumption, and after they had grounds for claiming some success in bringing about design improvements, the U.S. team decided that enough was enough. They were done with this issue. Even if JAROS and Mitsubishi were not forthcoming with a 200 millimeter aperture, which they were not, the U.S. team decided that it was time to move on. Mike Abrams decided to stop debating and not to continue pressing for bands that were wider than those in the optional band set, the band set which had been preferred by the ERSDAC user community from the very beginning. Although Yamaguchi came to the conclusion that he had successfully persuaded Abrams of the merits of the narrow band set, Abrams in fact remained

[95] ASTER Science Team (1992b: 29).
[96] Yamaguchi (1992a: 2).
[97] Fujisada (1992: 9) and ASTER Science Team (1992b: 29).

convinced that the wider bands, along with the use of a common mathematical technique to distinguish spectral information in overlapping bands (i.e., deconvolution), would have promised better results—perhaps even improvements of a few percentage points, which for some studies can be of real importance.[98] The U.S. team, and more specifically Michael Abrams, determined that the representatives of the two teams in the Geology Working Group were just not going to see eye-to-eye on this issue.[99] The two teams were approaching the question using different methods that reflected the different practices and experiences of each team's distinct community of geologic remote sensing. Still, one might wonder, could not the compromise position of 50 nanometer bands have been further explored? Might the shortwave infrared radiometer have become a boundary object after all?

These two questions bring us to the U.S. team's third socio-political conclusion. The instrument design process to date and the social makeup of the Japan team suggested to the U.S. team that even if they were able to persuade the members of the Japan team on the Geology Working Group of the merits of the U.S.'s preference for wider bands, changes to the band set of the shortwave infrared radiometer were quite unlikely because the Japan team and its bureaucratic institutions—that is, what the U.S. team called simply "Japan"—could not have been pushed any further, especially in such a short time.[100] The JAROS Program Manager, Kudoh, and the EOS Mission Committee Chair and Japan Team Leader, Tsu, appeared to give no ground on this issue at the second ASTER team meeting, nor was a

[98] Yamaguchi (2005) and Abrams (2005). Looking back on this debate with the hindsight of 2005, an uncharacterized phenomenon unexpectedly plagued the shortwave infrared radiometer, and this phenomenon overwhelmed the differences of a few percentage points which were at issue here. This phenomenon was "cross-talk."

[99] Abrams (2003) and Abrams (2005).

[100] As an aside, power can be expressed in classical physics as the amount of force used to push a mass a given distance in a given amount of time. In terms of this physical understanding of the concept of power, the U.S. team did not think that they had the power to push Japan.

compromise position suggested at the third ASTER team meeting by Yamaguchi and Watanabe, who were user-scientists no less. The latter two scientists had ensured that the "~~official~~ formal" decision of Japan's committees was noted in the minutes and reported to the ASTER team as a whole at the closing plenary session of the third team meeting. Maybe it was really true, as these enactments conveyed and as was commonly said, that once Japan's bureaucratic decision-making process arrived at a consensus decision, a decision that spoke for Japan as a state, that that decision was nearly impossible to change.[101] True or not, despite the lack of an explicit assertion of state power on the part of the Japan team to oppose the U.S. team's preferences—an action that the scientific disagreement in the Geology Working Group had rendered unnecessary—the U.S. team ascribed to the Japan team the power of a stubborn Japanese state (and, along with that ascription, a corresponding lack of power on the part of the U.S. team to do anything about it). The will of the Japan team was accepted as embedded in bureaucratic and committee-based decision-making and as advantageously strengthened by an unwieldy institutional divide between scientist-users and engineer-makers.[102] The preferences of ERSDAC's users became the Japan position, and that position was not going to budge. The enactments on the part of the Japan team members, in combination with the Japan team's social makeup, provided the U.S. team with a compelling rationale for that ascription, whether it was true or not.

Once this socio-political interpretation of Japan's power to resist change was accepted, more pragmatic reasons were nails in the coffin of further debate. The EOS Sensor Committee and Mitsubishi had already managed one set of design

[101] For example, the supposed importance of, and stubbornness of, consensus decision-making in Japan was described in a "survival guide" for negotiating with the "Japanese" which was pulled together for the use of the U.S. team (Avellar 1993).

[102] In my interview with her, Kahle emphasized this divide as a consideration in how much the U.S. team could push Japan to improve the shortwave infrared radiometer (2005).

improvements for the sensor, and given the pending deadline, there was not enough time to build a strong consensus among all of Japan's user-scientists to push for more design improvements, which would then need to make their way across the user-scientist/engineer-maker divide.[103] Furthermore, from the perspective of the U.S. team, many other issues about the ASTER instrument still needed to be assessed and negotiated, requiring the time and energy of the U.S. team, and more specifically, the U.S. members of the Geology Working Group.[104] And then there was the final nail of closure: it was, in the end, "Japan's" instrument, as was said.[105] When U.S. team members came to talk about ASTER as "Japan's" instrument, or as "their" instrument, as some did when recalling negotiations over the ASTER instrument's design, it indicated that U.S. team members had acquiesced to something and that that something did not meet their satisfaction. The boundary object ideal proved again to be elusive, as it had in the case of the thermal infrared radiometer, even if a boundary object strategy had been a sensible strategy to pursue.

The U.S. team's first socio-political conclusion was that JAROS and the contractors did not want the advice of the U.S. team. Their second socio-political conclusion was that the members of the Geology Working Group were not seeing eye-to-eye and were not going to see eye-to-eye. The U.S. team members then drew a third socio-political conclusion that closed the debate and settled the ASTER team's decision about the band set of the shortwave infrared radiometer: the Japan team and its bureaucratic institutions—the complex that U.S. team members referred to as

---

[103] Yamaguchi Yasushi, in correspondence with me, has suggested that design changes to the shortwave infrared radiometer were still possible to implement at this point, provided that the changes were compelling (Yamaguchi 2005). I think Yamaguchi is optimistic in his assessment. In any case, no design changes were found to be compelling, and that is what is of significance.

[104] Abrams (2005). Other issues in contention during this time that were judged to be of more significance, post Mitsubishi's design changes, include instrument calibration, the approval of common data products, and basic scenarios for instrument operations (ASTER Science Team 1992a, 1992b).

[105] Abrams (2003).

“Japan”—could not be moved, and the U.S. team could not do anything about it, since the ASTER instrument was, in the end, “their” instrument. If the U.S. team escalated the issue to the level of NASA and MITI, which was the ASTER team’s equivalent of escalating things to a trade war, that would have made the issue a “political matter,” and it would have been largely out of the control of their technoscientific diplomacy. The “political matter” would have been resolved through unscientific means, and the U.S. team had no reason to anticipate that the resolution would be resolved in their favor. The cost and risks of escalation were much too high, they judged. The U.S. team leader, Dr. Kahle, never seriously considered it. For her, the issue was up to the Geology Working Group to sort out scientifically based upon their expertise.[106] Escalation would have risked placing the integrity of the ASTER team and its cooperative spirit in jeopardy. The U.S. team had at least ten more years of collaboration to work out with the Japan team if the ASTER instrument was going to be a success.[107]

As was the case with the design specifications for the thermal infrared radiometer, the specifications for the bands of the shortwave infrared radiometer were pragmatic compromises that were negotiated through the technoscientific diplomacy of liminal state actors. The American and Japanese scientists, in their two years of negotiations over the shortwave infrared radiometer as a boundary object, consistently enacted bilateralism, with a U.S. side versus a Japan side, despite the establishment of the joint ASTER team which might imply (mistakenly) transnational community. Bilateralism was practiced within the joint team. Yet, the enactments of this

---

[106] Kahle (2005). The chair of the EOS Sensor Committee, Fujisada, stated as well at the third ASTER team meeting that the issue first needed to be worked out at the Geology Working Group. Fujisada (1992: 9).

[107] The importance of long-term relationships and of looking to the future was emphasized in a general way in my interviews with Fujisada (2003), Kahle (2003a), Palluconi (2001), Pniel (2001), and Yamaguchi (2002).

bilateralism by the U.S. and Japan sides in the Geology Working Group were circumscribed by the techno-political commitments that had brought them together in the first place and by the collaborative arrangements of the ASTER team beyond the Geology Working Group. The social makeup and workings of the Japan team, in particular, led U.S. members of the Geology Working Group to the above three socio-political conclusions, conclusions that implicated both scientific knowledge and state power. Those conclusions, taken together, ascribed a power to the Japanese state that could not be advantageously countered along lines of interdependency by the U.S. team, as the leadership of the U.S. team had done in the case of the thermal infrared radiometer. As a result, the U.S. team's conclusions led to the U.S. team's scientific acquiescence. The technical and political cost of their acceptance of the narrow band set had been moderated, however, by Mitsubishi's re-design of the shortwave infrared radiometer after the U.S. team had ferreted out the flawed design assumption.

Transcendence of Bilateralism:
The ASTER Instrument's Operational Capabilities, 1991-1993

At the fourth joint ASTER team meeting in June 1992, the Geology Working Group as its first order of business officially approved the "optional" band set as the specification for ASTER's shortwave infrared radiometer.[108] Many other issues concerning the design of the ASTER remote-sensing system remained to be settled in the Geology Working Group as well as in the seven other working groups of the joint team. Some of these outstanding issues still directly involved the design of the ASTER instrument. Most concerned the design and development of the ASTER ground data and information system, the calibration of the ASTER instrument, the governance of

[108] Yamaguchi (1992b).

the operations of the entire ASTER remote-sensing system, and the validation of the data that would be produced by the ASTER remote-sensing system. Because many of the team's design tasks were entangled with each other, a challenge of designing the ASTER remote-sensing system was often that of focusing a joint group on a defined task long enough to achieve closure on some issue, even if that achievement was incomplete. Other tasks that were intertwined with any given achievement often threatened to destabilize an achievement by redefining its assumptions and terms. Specifying the operational requirements for the hardware of the ASTER instrument presented just such a challenge.

While the U.S. and Japan teams initially approached this challenge in ways that were similar to their approach to the design of the band and signal-to-noise ratios for the thermal and shortwave infrared radiometers, the dynamics of the two teams' technoscientific diplomacy over the ASTER's instrument's operational specifications quickly became strikingly different. For both the negotiations over the thermal infrared radiometer and the shortwave infrared radiometer, the two teams enacted bilateralism, even despite—in the case of the shortwave infrared radiometer—the establishment of a formal institutional arrangement for the international ASTER team which might imply transnational community. In negotiating the operational requirements of the hardware for the ASTER instrument, although the two teams did not abandon their boundary object strategy, they broke out of their bilateral diplomacy and began mixing technical claims and political power in new ways that displaced the "two sides" premise of their boundary object strategy. To specify designs that the two sides each independently desired, the U.S. and Japan teams still occasionally pushed and recoiled along lines of U.S.-Japan interdependency, as they had in their negotiations over the band and signal-to-noise specifications for the thermal infrared and shortwave infrared sensors. Yet, in addition, the members of the two teams wove together technical claims and political power in intersubjective problem-solving. This weaving finessed mutual dependence between the U.S. and Japan teams into collaborative work that was driven, configured, and governed by transnational community and authority.

Operational Modes

The origins of this new international techno-political order were in the same ASTER team meetings that negotiated the specifications of the shortwave infrared radiometer. While the Geology Working Group was debating the band and signal-to-noise specifications of the shortwave infrared radiometer in 1991 and 1992, they were also discussing other ways that the functional requirements of the ASTER instrument might be changed or tweaked to be more responsive to what were sometimes asserted as "science requirements." As this dissertation has illustrated, these "science requirements" did not exist as *a priori* sources of authority for the joint team. Science requirements had not even been articulated for the ASTER instrument previous to its design, except in the broadest of terms. The science requirements for the ASTER instrument were decided along with the design of the instrument.

By the time of the first ASTER team meeting, the ASTER instrument had been designed so that it could be used in two "operational modes." A full mode would simultaneously use all three of ASTER's radiometers to characterize the earth's land. A night mode would use just the thermal infrared radiometer to measure the temperature of the earth's surface. The use of the ASTER instrument in these two modes was constrained by a variety of factors. The ASTER instrument relied upon the NASA host satellite for its power and data transmission. These resources were scarce, especially since they had to be shared with the other instruments onboard the host satellite (the ASTER instrument demanded far and away the most power and data resources). The

ASTER instrument was limited also by ASTER's own wear and tear (including its heating and cooling), whatever that wear and tear might ultimately turn out to be through the life of the instrument. The two teams looked to the engineer-makers to provide estimates of what engineering constraints the teams should use in their planning. NASA specified the power and data bandwidth allocated to ASTER in its interface documents with JAROS. For its part, JAROS and its contractors explained at ASTER team meetings the capabilities that were going to be built into the ASTER instrument (these capabilities would later be codified in documentation between JAROS and its contractors). As a result of these design specifications, ASTER was anticipated to acquire data for only about eight minutes of the daytime half of its orbit and for about eight minutes of the nighttime half of its orbit (which, given ASTER's orbit of approximately one hundred minutes, was a sixteen percent duty cycle for the full orbit for the thermal infrared radiometer and eight percent duty cycles for the other two radiometers).[109]

In May 1991, at the second ASTER team meeting, U.S. team members in the Geology Working Group—and a JPL volcanologist in particular—requested that the shortwave infrared radiometer be made available for acquiring data at night, along with the thermal infrared radiometer, for the purpose of monitoring volcanoes.[110] The U.S. team proposed, in effect, a new operational mode, a third operational mode, what they called a "volcano mode." In response, the Japan team said that they would study its feasibility. At this joint team meeting, which was the same meeting in which the Japan team made its unilateral decision regarding the band set for the shortwave infrared radiometer, the U.S. team "stressed that *science* operation scenarios should be fully recognized by both teams" [italics mine].[111] Strictly speaking, the operational

[109] Fujisada (1990: 7) and Chang et al. (1992: 3).
[110] ASTER Science Team (1991: 27) and Abrams and Yamaguchi (1991).
[111] ASTER Science Team (1991: 33).

scenarios outlined the operations of the *instrument*, and not the operations of the two teams' *science*, but the U.S. team was making a point about who they wanted to shape those operational scenarios, namely the user-scientists of both teams (per their boundary object strategy) and not just the engineer-makers of the Japan team. Instrument operations, including the proposed third operational mode, were thus asserted as science operations.

Over six months later, at the third joint team meeting, the U.S. team recommended a fourth observational mode: a daytime emergency mode that used the visible and near infrared radiometer to look perpendicular across its orbital path at much larger angles, up to twenty four degrees.[112] This large-angle pointing capability would allow the radiometer to make observations of emergencies and other time-urgent observations that could not wait up to sixteen days for the ASTER instrument to pass within the view of its regular pointing. Whereas the Japan team was still studying the U.S. team's request for the third observational mode, the volcano mode, at the time of the fourth ASTER meeting in June 1992, the request for the fourth observational mode was promptly accepted. The difference between the fates of the requests is partly explained by the different hardware engineering challenges that they supposedly posed. The third operational mode was said to require the development of new cooler technology for the shortwave infrared radiometer (to extend the radiometer's use), while the fourth mode just necessitated the redesign of the visible and near infrared radiometer but not the development of new technology. The difference between the fates of the two requests is also explained by who had made the requests. For the fourth mode, unlike for the third mode, NASA Headquarters and the EOS Project Office had directly asked MITI and JAROS to

[112] ASTER Science Team (1992b: 29) and Yamaguchi (1992a).

implement the large-angle “emergency mode” capability.[113] MITI and JAROS quickly agreed to the request. As for the third mode, however, the two teams’ liminal state actors were left to work it out.

Negotiating Operational Scenarios and Instrument Capabilities

While operational modes were agreed upon in concept, such as the first two operational modes and the fourth mode, they were not necessarily agreed upon in their specifics. At the fourth joint team meeting in June 1992, the meeting in which the Geology Working Group approved the narrow band set for the shortwave infrared radiometer, the U.S. and Japan teams started to try to specify just what was meant by eight minutes of full-mode observations in the day time and eight minutes of observations using the thermal infrared radiometer in the night time (i.e., respectively, the first and second operational modes). Could that block of observation time—and the power, data resources, and wear and tear that it represented—be broken down into smaller blocks of time for instrument operations, or were the eight minutes of observation time required to be consumed continuously? Separate from the question of what kinds of operations and sequences of operations might be held to be technically possible, what kinds of operations were desired? Was there any such thing as “optimal” operations, and if so, what did they look like? As of June 1992, no one on either team had hard answers to these questions.

The two teams did have vague preferences. More precisely, they had leanings. The U.S. team leaned toward flexibility in operations, given their team members’ professional interests in scientifically characterizing and monitoring targeted sites over time (e.g., certain volcanoes, glaciers, sedimentary basins, mountain ranges, cities,

[113] ASTER Science Team (1992b: 10).

etc.). The U.S. team members had been selected as EOS scientists at least in small part on the basis of their scientific research agendas. For instance, at least three U.S. team members had previously conducted remote-sensing investigations for the purpose of contributing to volcanology and had proposed to continue with this work using ASTER.[114] On the other hand, in the early 1990s, Japan's user-scientists leaned on the whole toward regional and global mapping. Regional and global mapping implied at the time that relatively large blocks of continuous observations might be operationally sufficient and would avoid the fragmentation that targeted observations might presumably risk. The user-scientists on the Japan team did not, in general, have well-defined scientific research programs that made intensive use of remote-sensing imagery. For instance, although the Japan team listed volcanic monitoring as one of its key areas of scientific interest in its "Science Management Plan," the Japan team itself did not include a volcanologist among its members until the U.S. team, at the second ASTER meeting, requested that the Japan team include at least one volcanologist.[115]

For the Japan team's user-scientists, global and regional mapping was an operations plan that covered many of their concerns. ERSDAC and the oil and mining firms associated with ERSDAC wanted to compile databases of imagery that mapped Asia, especially a database of three-dimensional topographic imagery constructed from the two telescopes of ASTER's visible and near infrared radiometer. Moreover, a high-resolution map of the earth's entire land surface had yet to be made using imagery from a satellite sponsored by the Government of Japan, and that conspicuous national goal was promoted to and by MITI early on. Finally, global and regional mapping suggested consistent and regular operations that ASTER's engineer-makers could model and could satisfy with confidence.[116] As of early 1992, the two teams'

[114] Kahle and Palluconi (1988) and Kahle (1990c).
[115] MITI (1991: 2-3) and ASTER Science Team (1991: 27).
[116] Chang et al. (1992: 3) and Yamaguchi (2002).

leanings, however, were just leanings and were not settled preferences. They had not been explored in much detail.

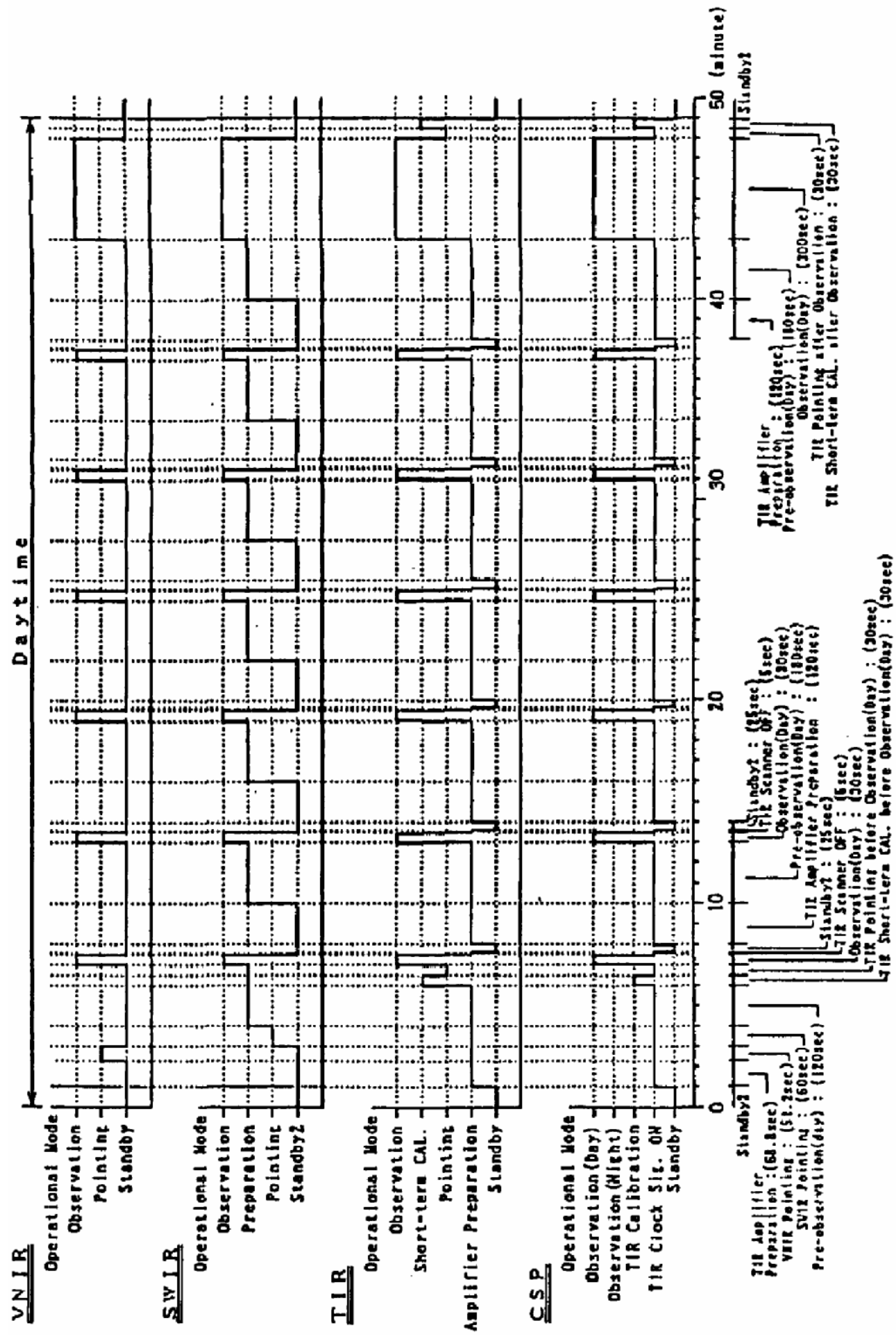
The first joint opportunity for the two teams to explore these leanings, arrive at specific preferences, and negotiate an operational approach that would satisfy both teams' goals, was when an NEC engineer presented an analysis of the ASTER instrument's operations in the first plenary meeting of the fourth ASTER team meeting in June 1992. NEC was the contractor for the systems engineering for the ASTER instrument, as well as the maker of the instrument's visible and near infrared radiometer. NEC's approach to ASTER's operations was very top-down and systematic. NEC took the five years of the instrument's planned lifetime and broke it down into groups of forty-eight days, which included three sixteen-day cycles around the earth. The orbital path of the ASTER instrument, projected on the earth's surface, retraced itself every sixteen days. NEC reasoned that for the first sixteen-day cycle, the ASTER instruments might primarily look directly down (i.e., at the nadir path). For the next sixteen days, ASTER might look to the right or to the left, and then, for the following sixteen days, to the left or to the right, depending upon which direction it had looked the previous cycle. That group of three cycles would roughly repeat itself throughout the instrument's five years, according to NEC's plan.[117]

Within this plan, how NEC proposed approaching the division of the eight minute blocks of continuous observation was illustrated by figure 5.5, which is one of the slides from NEC's presentation (below on the next two pages). It depicted an ASTER full-mode operational sequence in which the daytime half of the ASTER orbit was divided into six equal segments, accompanied by a seventh segment for instrument calibration. The first six of the highest bars in each radiometer's operational sequence represented the observations (the "CSP" at the bottom of the

[117] Takahashi (1992: 3).

Figure 5.5: ASTER Operational Sequence for Six Evenly Divided Observations during the Daytime Half of One Orbit (from Takahashi 1992: slide 10)

VNIR: Visible and Near Infrared Radiometer
SWIR: Shortwave Infrared Radiometer
TIR: Thermal Infrared Radiometer
CSP: Central Signal Processor



Daytime
VNIR
Operational Mode
Observation
Pointing
Standby
SWIR
Operational Mode
Observation
Preparation
Pointing
Standby2
TIR
Operational Mode
Observation
Short-term CAL.
Pointing
Amplifier Preparation
Standby
CSP
Operational Mode
Observation(Day)
Observation(Night)
TIR Calibration
TIR Clock Sig. ON
Standby
0
10
20
30
40
50 (minute)
Standby2

vertical axis does not denote a radiometer, but ASTER's central signal processor). Each of those six high bars marked observations of approximately thirty seconds. The seventh bar at the end, the widest bar, was the time that was required for the calibration of the radiometers. This calibration was planned to occur once every sixteen days (i.e., once every cycle of ASTER's path around the earth).[118]

As depicted in figure 5.5, NEC proposed an operational approach that, if the eight minute blocks needed to be divided, they would be divided into a set number of divisions, as opposed to allowing the number of divisions to vary. Moreover, according to the NEC plan, those divisions would be equal divisions, as opposed to allowing for the length of the divisions to vary. NEC suggested that the maximum possible observation time, in total, for a six division plan, of the type shown in figure 5.5, was too short to be practical. Power constraints, the time required for the radiometers to prepare to make an observation, and other issues such as cooling, took their toll on the total amount of observation time. In this calculation, the resources and constraints that allowed for a block of eight continuous minutes would amount to a total of less than three minutes when that time was divided equally into six divisions. In light of the cost of the constraints of a six division scenario, JAROS and NEC suggested dividing the eight minute block into a set number of divisions between three and five, and not six, which was their limiting case.[119]

The reactions of U.S. team members to NEC's presentation of ASTER's operations were deflated and somber. First, NEC did not present all the operational capabilities that the U.S. team had anticipated. For example, no mention had been made of the shortwave infrared radiometer operating at night along with the thermal

[118] In another calibration procedure, the thermal infrared radiometer was calibrated before and after each observation. This calibration, owing to its short duration, was only depicted once in figure 5.5, the rise just before the first observation of the thermal infrared radiometer.
[119] Takahashi (1992: 7) and ASTER Science Team (1992a: 26).

infrared radiometer, in what was known as volcano mode. After commenting on how comprehensive the presentation had been, a U.S. team member—recorded in the minutes as simply an “American”—remarked that “there is a science requirement for SWIR observations at night” which had not been taken into consideration.[120] In response, Dr. Yamaguchi explained that while the science requirement had been approved by the Geology Working Group, the requirement had been passed on to just the Operations and Missions Planning Working Group but had not yet been passed across the user-scientist/engineer-maker divide for consideration by JAROS, the EOS Sensor Committee, and the contractors, such as NEC. Dr. Ono from the EOS Sensor Committee explained further that the asserted science requirement was “still on the user side,” that there was “some kind of misunderstandings on both sides [i.e., between the user and instrument sides of the Japan team],” and that “sufficient discussions [had] not been made yet on the hardware side.”[121] It was far from clear that this “science requirement” would actually be fulfilled, and if it were to be, this exchange presaged that further negotiations would likely be required.

Other U.S. team members worried that the instrument was too constrained for effective operations. Dr. Lang from the U.S. team asked if the power and data transmission constraints of the host satellite were too restrictive to the entire ASTER operational scenario and if the constraints were “terribly detrimental to any science.”[122] He wondered if Dr. Tsu and Dr. Kahle should petition NASA for more power and data transmission resources, even contacting the team leaders of the other instruments slated to be onboard the NASA satellite to see if those team leaders wanted to join the petition. In sum, in this interpretation of NEC’s presentation,

[120] ASTER Science Team (1992a: 25).
[121] Ibid.
[122] Ibid.

dividing the eight minutes of continuous observation did not so much as offer flexibility as it reduced over all capability.

Dr. Kahle, however, had seen ASTER's potential operational capabilities presented differently a week earlier at a U.S.-Japan hardware interface meeting.[123] After Dr. Lang made his comments about the possibility of petitioning NASA for more power and data resources, Kahle remarked that once Lang heard an upcoming presentation he might "find that it is not quite as bleak as perhaps pictured by Takahashi-san [who was the NEC engineer]." For the U.S. team, figure 5.5 was inflexible, austere, and bleak. Maybe the figure's engineering aesthetic encouraged that impression, drawing the eye's attention to what little observational capabilities the instrument would possess in comparison to all of the housekeeping tasks the instrument's operations required (which amounted to overhead time). The next presentation, however, offered a much more serviceable picture of ASTER's operations.

In this same plenary session, Dr. Miyazaki Yoshinori, from the Geological Survey of Japan, presented an analysis of ASTER's operations and data acquisition which he qualified as his "personal idea."[124] Figure 5.6 (on the next page) was among the slides that Miyazaki showed. Rather than focusing on the housekeeping tasks and presuming equal divisions of observation time, the table did not display the amount of overhead time required for housekeeping, nor did it demand equal divisions of observations. The highlighted diagonal was set at the ASTER instrument's allotted maximum average power for two consecutive orbits (i.e., 424 watts). The number underneath the power, the instrument's data rate, was determined by the number of

[123] The meeting was one of a series of meeting in which JAROS engineers and Goddard Space Flight Center engineers met to discuss and negotiate the ASTER instrument's physical interface and integration with the NASA host satellite.
[124] ASTER Science Team (1992a: 26).

ASTER "TRADE NIGHT-FOR-DAY" OPERATION

POWER (W) & DATA RATE (Mbps)
(ORBIT AVERAGED)
TOTAL NUMBER OF OBSERVATION MINUTES

NUMBER OF OPERATIONS DAY	NUMBER OF OPERATIONS NIGHT					
		DAY	3.8	7.18	4.33	4.84
		NIGHT	3.8	1.9	1.9	0
2	2		424.0	431.8	423.0	406.5
			3.7	6.7	4.1	4.4
2	1		416.2	424.0	415.3	406.5
			3.7	6.6	4.1	4.4
3	1		425.0	432.8	424.0	415.2
			3.7	6.7	4.1	4.4
4	0		420.8	431.2	422.4	424.0
			3.6	6.6	4.0	4.5

Figure 5.6: Trade-Off between Number of Operations (i.e., Observations) and Total Observation Time for Full Orbit Scenarios (from Miyazaki 1992a: slide 24)

In this matrix, the shaded diagonal held constant the maximum average power that was allocated to ASTER for any two consecutive full orbits (i.e., 424 watts). By doing so, it showed how the number of day and night observations—the former of which would use all three of the radiometers and the latter of which would use only the thermal radiometer—would affect the total observation time of the instrument during the day and night and the average data rate of the instrument (which was not to exceed an average of 8.3 megabits per second over any two consecutive orbits).

observations (the table's vertical axis) and by the power. The two-orbit average for the data rate could be no more than 8.3 megabits per second (Mbps). Together, the instrument's power, the number of observations, and the data rate determined the horizontal axis at the top of the table, which is the total number of observation minutes for the night and day. With the table's diagonal line, vertical axis, and horizontal axis established, the reminder of the chart could then be filled with pairings of power and data rate. In addition to the plethora of combinations which the table displayed, the table also integrated into its calculations a trading scheme in which instrument resources that would not have been used at night could be "traded" into more instrument resources for daytime observations. Miyazaki's scheme rendered instrument resources as much more fungible and instrument operations as more flexible than NEC's proposal had done.[125] Not surprisingly, the U.S. team was much more enthusiastic and engaged with Miyazaki's "personal idea" than with NEC's proposal.[126]

While Miyazaki's analysis of ASTER's operations and data acquisition was no doubt his own, the table shown in figure 5.6 was actually not constructed by him. This table was what the U.S. team leader, Anne Kahle, had referenced in her earlier exchange with Dr. Lang. The table, along with others like it, had been presented by Scott Lambros, who was the chief engineer for the NASA satellite into which ASTER was to be integrated. Lambros worked at NASA's Goddard Space Flight Center, and the tables that he presented had actually been constructed by another engineer at NASA's Goddard Space Flight Center, Edward Chang, the operations manager for the NASA satellite. Aware that Miyazaki was working on his own analysis of ASTER's operations, either Chang or Lambros had passed on Chang's tables to Miyazaki for his

[125] Miyazaki (1992a: slide 24); ASTER Science Team (1992a: 30).
[126] ASTER Science Team (1992: 25-28).

potential use. Perhaps not wanting to redo the tedious calculations, Miyazaki pragmatically incorporated the tables into his own analysis, which was an analysis that went well beyond these tables. In his use of the tables for his larger project, Miyazaki took on board the tables' assumptions of resource interchangeability and instrument flexibility. Likely aware that these assumptions and other assumptions in his analysis had not yet been approved by Japan's "instrument side," Miyazaki went out of his way to label his analysis his "personal idea."[127]

Analytical Mixing and Nucleation Sites

Over the next few ASTER team meetings, "Miyazaki's Operations Scenario Analysis," as it was called, served as the primary resource for the two teams in their negotiations over ASTER's operations.[128] It was the analysis's tables in particular—constructed by the operations manager at the Goddard Space Flight Center but spoken for by a member of the Japan team—which were most valuable to the U.S. team and which were used in repeated deliberations between the two teams. These tables were not coerced into or sneakily slipped into the collection of operations documentation and then subsequently taken on face value by the two teams in some unreflective way. That is, they were not, in the vocabulary of actor-network theory, semiotic "immutable mobiles" that imperially extended the analytical reach of the U.S. team through the intrinsic ability of the table's representation to discipline and enroll. Furthermore, they were not an epistemic community's consensus representation of how a physical system naturally worked or should work. The trade-off tables were rather instances of analytical mixing that facilitated intersubjective problem-solving.

[127] ASTER Science Team (1992a: 26-27); Lambros (1992); and Chang (1992).
[128] ASTER Science Team (1992a: 6).

They were nucleation sites around which the teams negotiated how ASTER's engineering resources could best be used, and these nucleation sites carried with them starting assumptions that had at least the personal imprimaturs of both a Japanese team member and an American, if not that of their teams as a whole.

This analytical mixing and nucleation were steps toward breaking out of the two team's boundary object strategy. The joint Operations and Mission Planning Working Group did initially have an action item to "develop a scheme by which Japan and the U.S. decide operationally on the division of resources ([e.g.,] instrument bandwidth)."[129] In light of Miyazaki's analysis (and the tables that partly constituted it), the two teams did not follow through with that initial idea in line with engineering a boundary object. According to that strategy, they would have designed ASTER's operations in a manner that split instrument resources (such as bandwidth), avoided questions of community, and preserved two distinct communities of practice (which would have allowed for "coherence" while preserving separate "social worlds," in the vocabulary of Star and Griesemer). Instead, the two teams directly confronted questions concerning "informational requirements," and Miyazaki's analysis and pragmatic use of the passed-on tables facilitated the collective action of their broader problem-solving about ASTER's operations.

Both Miyazaki and Chang, the operations manager for the NASA satellite, incorporated the "trade night-for-day" table (i.e., figure 5.6) into the wide-ranging operations scenarios that they presented to the teams at the fourth joint team meeting. Chang's presentation in the plenary session followed Miyazaki's plenary session presentation. The discussion of Chang's presentation, as recorded in the minutes of the meeting, centered on the "trade night-for-day" scheme that was illustrated by the table, but it also touched upon the magnitude of the benefits of possibly extracting more

[129] ASTER Science Team (1992a: 58-59).

power from the host satellite, avoiding the operational demands of trading. With respect to the question of extracting more power from the host satellite, Scott Lambros, NASA's chief engineer for the NASA host satellite, jokingly commented that "the way to change your power allocation is to lock me in a closet for three days and don't feed me until I say yes!"[130] Given Lambros's firm stance about ASTER's power allocation, Chang's presentation and the discussion that followed reinforced the trading scheme as the main operational framework under joint consideration that might allow for an optimal solution to the various demands upon ASTER's operations.[131]

While Chang's presentation, coming after Miyazaki's, reinforced the utility and legitimacy of the trading scheme as a joint framework, it was Miyazaki's presentations at the team meeting that put the scheme to use. Miyazaki's presentations, both the presentation in the Operations and Mission Planning Working Group that was on a day after the plenary session as well as the presentation that was in the plenary session and previous to Chang's, integrated the table's flexible approach into a feasibility analysis of a plan to acquire different kinds of imagery. The plan distributed ASTER's acquisition of imagery across four categories, namely emergency observations, team requests, regional monitoring, and cloud-free global mapping. Miyazaki also used the "trade night-for-day" table (and other tables like it which had been originally constructed by Chang), along with the results of rough calculations of the desired volume of acquisitions within each category, to estimate how many times the telescopes of the ASTER radiometers might need to point in order to fulfill imagery acquisition objectives. The number of "pointing operations" that the telescopes for ASTER's three radiometers could perform was one of those engineering constraints that was limited by wear and tear, as well as by the possible impact of

[130] Ibid., p. 31.
[131] Chang (1992) and ASTER Science Team (1992a: 30-31).

pointing operations on the positional stability of the entire NASA satellite (when one telescope moves on a free-floating platform in space, that movement needs to be countered if the platform is going to conserve momentum and remain stable; pointing operations can also cause vibrations on the platform known as "jitter"). For planning purposes, NEC and the other contractors had cautiously estimated that approximately ten thousand pointing operations for both the visible and near infrared radiometer and the shortwave infrared radiometer were feasible over the life of the instrument, taking into consideration issues such as lubrication, oil pressure for the bearings, and quality control. Miyazaki's analysis of ASTER's operations—using figure 5.6—suggested that ten thousand might not be enough, given the global mapping and other data acquisition goals that Miyazaki proposed.[132] This issue of how many pointing operations would be designed into the instrument arose again in later team and working group meetings, and Miyazaki's analysis was an important resource in pushing for more point operations.

Miyazaki's use of figure 5.6 in his analysis—and his personal endorsement of that table's assumptions of flexibility—invited a discussion of shared goals, shared data acquisition methods, and shared instrument operations. At the same meeting where the U.S. team acquiesced to the Japan team's preferences regarding the band set and signal-to-noise ratios of the shortwave infrared radiometer, in which "Japan" was ascribed as monolithic and the U.S. team was not able to leverage mutual dependence between the U.S. and Japan teams to move "Japan" to their advantage, Miyazaki's analysis served as a resource with which the U.S. team could cast aside NEC's plan for ASTER's operations, breaking out of bilateral diplomacy by breaking down "Japan." The U.S. team's dependency on the ASTER instrument, and here more precisely the U.S. team's dependency on the Japan team to design the ASTER instrument's

[132] Miyazaki (1992a, 1992b) and Takahashi (1992).

operational capabilities, was gradually finessed into encouraging the design of the instrument's operational capabilities to be undertaken by the two teams as a community enterprise. Premised upon Miyazaki's use of the trade tables, Miyazaki's "personal idea" was eventually accepted into the two teams' design discourse as the joint baseline for ASTER's operation scenario, a scenario that had implications for the design of the ASTER instrument's hardware. Adjustments, and proposals for adjustments, were made to Miyazaki's baseline and not to NEC's proposal. For example, at the plenary session, Dr. Kahle said Miyazaki's presentation "left [her] a perfect opening to request another mode," and that mode—a fifth operational mode—was deliberated upon in terms of the Miyazaki's operations scenario.[133] The U.S. team's Dr. Kieffer and the Japan team's Dr. Fujisada and Dr. Ono, the last two of whom were members of the EOS Sensor Committee, discussed this fifth mode—which was a mode for monitoring glaciers—within the framework of Miyazaki's operations scenario, and not NEC's. Given the scenario's discursive ascendancy, NEC's engineer was left to say "for the ASTER system as a whole we have not looked into that possibility [i.e., of a fifth mode] but if there is such a request or requirement then we are ready to make a study on that."[134]

As was suggested by that diplomatic response from the NEC engineer, the intersubjective problem-solving that revolved around Miyazaki's operations scenario and the trading tables did not settle once and for all the design of the operational capabilities of the ASTER instrument. The problem-solving sessions in the plenary session, in the Operations and Mission Planning Working Group, and in the Geology Working Group did all conclude with general endorsements of Miyazaki's scenario, of its imported flexible trading scheme, and of the additional modes. While these

[133] ASTER Science Team (1992a: 27).
[134] Ibid., p. 28.

endorsements were assertive steps based upon the authority of agreement between both teams' user-scientists, NEC just said that it was "ready to make a study" of such requests. As members of both teams were well aware by this point in the ASTER collaboration, those words from Japan's contractors were far from a "we will do that." Dr. Ono closed the discussion of Chang's presentation of ASTER's operations by reminding everyone of "the general procedures for changing the design of the instrument . . . requests for changes should be made through JAROS which is the *other* team for ASTER to NASA's Goddard, formally [speaking]" (emphasis mine).[135] The institutional boundary in the Japan team between the user-scientists and the engineer-makers was not erased through one episode of intersubjective problem-solving. Dr. Ono's words even projected the user-scientist/engineer-maker boundary onto the U.S. team, distinguishing the engineers at NASA's Goddard Space Flight Center from the U.S. team. For Ono and for Kudoh, JAROS's project manager for ASTER, the instrument side of the Japan team was "the *other* team" in the ASTER collaboration, namely the "instrument team" and not the "science team."[136]

Intersubjectivity

Despite the persistent maintenance of a boundary between engineer-makers and user-scientists, Miyazaki's analysis—and the imprimatur of a member of the Japan team—was nevertheless a helpful resource for the purpose of negotiating a design for ASTER's operational capability that was not only accepted by both teams but that was jointly understood, and also understood to be jointly understood—in one word, intersubjective. Miyazaki's analysis was a way for the U.S. team to work itself into

[135] Ibid., p. 31.
[136] The distinction between "instrument team" and "science team" is made by Dr. Ono in Ibid., p. 43. See Kudoh's comments on p. 35.

instrument design discussions within the Japan team, breaking negotiations out of bilateral diplomacy first by breaking down "Japan" and then by engaging different Japanese liminal state actors in ways that did not enact the socio-political dynamics of the United States vis-à-vis Japan. Rather than the United States vis-à-vis Japan, it was the ASTER team that was coming to understand what operational capabilities would be best, and it was the ASTER team that was gaining confidence that Japanese and American interpretations of the operational design of the ASTER instrument were converging.

For example, after Miyazaki's presentation of his analysis in the Operations and Mission Planning Working Group, Dr. Ono presented the instrument side's proposal of what operational scenarios the instrument side of the Japan team would study in light of the discussion that had occurred among the two teams' user-scientists. Dr. Ono's proposal took an earlier comment made by the U.S. team members, which was that all eight minutes at night would likely not be needed on a regular basis, to mean that the eight-minute allocation at night could be cut back and fixed to an estimate of what would be needed on a regular basis, which was said to be approximately two minutes. In discussions earlier in the meeting, U.S. team members had said that the two middle columns of figure 5.6, which each offered 1.9 minutes at night, were, in their judgment, realistic day/night distributions.[137] Upon hearing Dr. Ono's proposal to fix the instrument to something like those distributions, Dr. Kahle realized that a point that she had made in support of the feasibility of flexibility (i.e., up to eight minutes, but usually two minutes) had been transformed into a new limitation (i.e., that really only two minutes would be needed). Kahle marshaled Miyazaki's analysis to explain why Ono's proposal was unsatisfactory:

137 Ibid., p. 27.

> I find this [i.e., Dr. Ono's proposal] to be not very acceptable in that there is probably less flexibility now than we had with eight minutes in the daytime and eight minutes at night. You are only allowing us eight minutes in the daytime and the total amount at night had been cut down to two minutes. So we've really lost the amount of data we can acquire with this type of scenario. I prefer the type of scenarios that we saw that Dr. Miyazaki presented. We had six scenarios [under Miyazaki's analysis], which sort of covered the entire ground from all of the data would be take in the daytime, [to] some of the data would be taken in the daytime and some at night and allow the complete flexibility between the amount of data that would be taken day or night within the constraints of the power and pointing [i.e., as in figure 5.6]. This [i.e., Dr. Ono's proposal] to me seems more limiting that what we had at the very outset [i.e., even before Miyazaki's analysis].[138]

Dr. Ono replied by giving an explanation of what Miyazaki's operational scenario had meant to him:

> But the only reason I cut down the nighttime operation from eight to two minutes was because I could not find any scientific reason [in Miyazaki's scenario] that would justify having the operation last in the nighttime for as long as eight minutes. So I think this two minutes is extremely reasonable.[139]

Although Dr. Miyazaki's analysis had been marshaled by Kahle, as it turned out, Miyazaki had also worked with Dr. Ono on Ono's proposal of what operational scheme the instrument side should study. Unavoidably embroiled in this debate, Miyazaki placed figure 5.6 on the overhead projector and interjected his interpretation of his operational scenario:

> This [figure 5.6] is the OHP [overhead project slide] that I showed you on the first day [in the plenary session]. And in my interpretation this one [and this] one [i.e., the two 1's on the vertical axis for night operations in figure 5.6] means eight minutes [and] eight minutes [i.e., potentially eight minutes at night for each of them] and therefore it is my personal opinion is [*sic*] to have as much as [*sic*] capability as possible and I do fully support Dr. Kahle's comment of eight minutes. But the reason why we dropped this to 1.9 minutes [in Dr. Ono's study proposal of what the instrument side should study] was to

[138] Ibid., p. 43.
[139] Ibid., p. 43-44.

> increase the number of divisions [that were possible in the eight minutes], so it was a strategic reason why we dropped it to 1.9 minutes.[140]

Miyazaki walked the fine line between, on the one side, standing by his analysis and agreeing with Kahle's interpretation of it, and, on the other side, coming to Dr. Ono's defense by explaining the rationale for the drop to 1.9 minutes, a defense which itself alluded to the need for more flexibility in the baseline operational scenario by increasing the number of feasible divisions. At the closing plenary session, in his report about his proposed operations, Miyazaki displayed figure 5.7 as his final slide (see the next page), conveying the tensions within, and the complexity of, designing ASTER's operational capability.

Miyazaki articulated an interpretation that was understood and accepted by both teams, and understood as such. It was an accomplishment of intersubjectivity among the team members, and especially, between the members of the U.S. and Japan teams. Both the political and the material made possible this intersubjectivity, which informed what "1" night operation and "1.9" minutes meant on an overhead slide, and, which, more broadly, helped to effect intersubjective problem-solving with respect to ASTER's operational capabilities. As the above discussion illustrates, Dr. Miyazaki's deft man-in-the-middle diplomacy was undoubtedly important to establishing an understanding of what the ASTER instrument's operational capabilities should be, and that diplomacy has been remembered by team members.[141] Nevertheless, the sharing and mixing of analytical material, such as the table of figure 5.6, forgotten as those quiet materials might be,[142] served as nucleation sites around which liminal state actors such as Kahle and Miyazaki finessed the techno-political interdependence of the

---

[140] ASTER Science Team (1992a: 44).
[141] E.g., Yamaguchi (2002).
[142] E.g., Ibid.; Miyazaki (2003); and Kahle (2005).

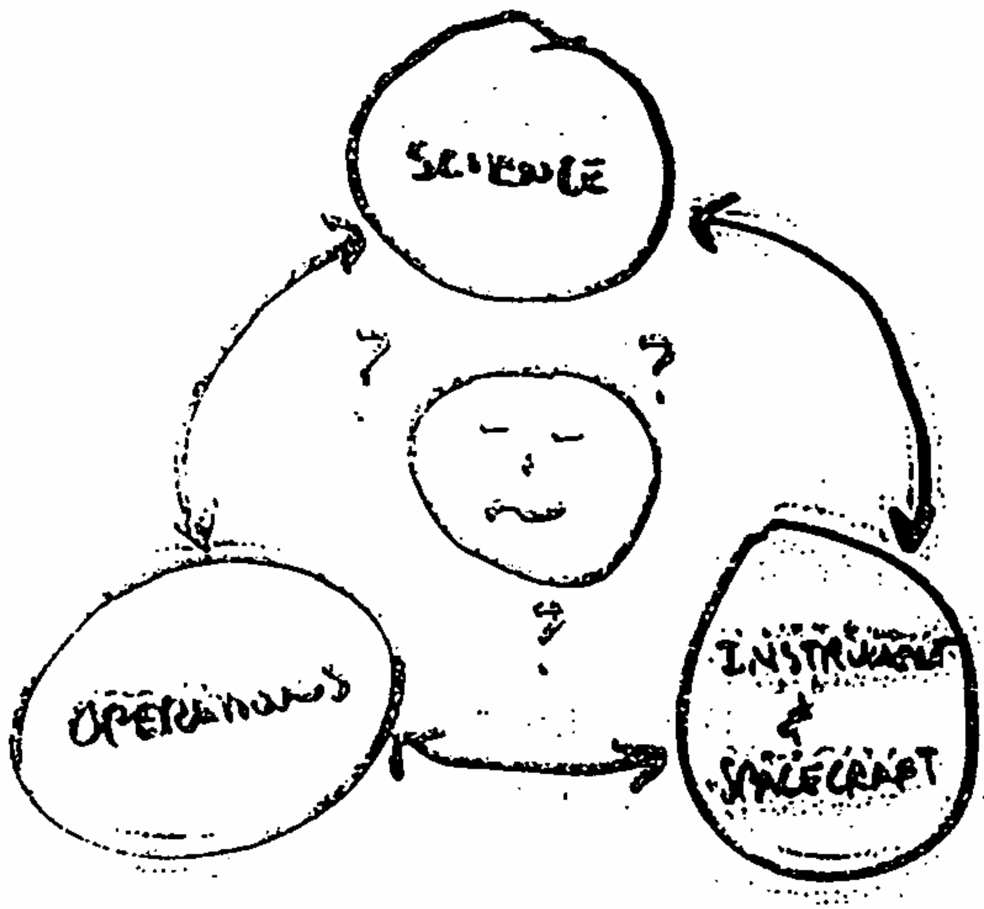


Figure 5.7: Miyazaki and the Tensions in Mission Operations

The slide was titled “Mission Operations Working Group” (Miyazaki 1992c: slide 7).

Japan and U.S. teams into politically-robust technical knowledge, knowledge that had the power to shape what the U.S. and Japan could do with the ASTER instrument.

The fourth joint team meeting concluded with a shared interpretation of what figure 5.6 and, more broadly, Miyazaki's operational analysis, meant for the design of the ASTER instrument and for the two teams' general operations scenario, which was understood to be a common operations scenario, with shared goals and shared methods. The third, fourth, and fifth operational modes (which were, respectively, the volcano mode, the large-angle emergency mode, and the mode for monitoring glaciers) were recommended as important for fulfilling the data acquisition objectives that were specified by Miyazaki's analysis, as articulated in that analysis's four data acquisition categories and its desired imagery volumes for those categories. The instrument side of Japan's team had already agreed to the fourth operational mode, which had been requested by NASA Headquarters, but the liminal state actors of the two teams' user-scientists managed to forge a qualified consensus of some strength which recognized benefits to the other two modes.[143] The acceptance of the third operational mode implied, however, that the shortwave infrared radiometer would need to be able to run at night as well as day, which would require the instrument's maker, Mitsubishi, to further develop its cooler technology and to add an additional radiator. The third and fifth modes were eventually accepted as design specifications (although the fifth in a different form), and Mitsubishi designed its shortwave infrared radiometer for the burden of operating at night as well as day, for a longer total observation time in each orbit.[144] The two teams also made headway by recognizing that their common operations for the ASTER instrument might significantly benefit from an increase in the pointing operations specification for the telescopes of the visible and near infrared

[143] Yamaguchi (1992c: 53).
[144] Kudoh (1993: 2) and Isoda (1993: 2).

radiometer and the shortwave infrared radiometer; 10,000 might possibly do, but this constraint definitely needed further study, in the judgment of members of both teams. Within the next year, the contractors for those two radiometers, NEC and Mitsubishi, reported that they were likely able to meet a design specification for 20,000 pointing operations, which did become the specification.[145]

The two teams' common understanding of Miyazaki's general analysis and their confidence in that common understanding also spurred more specific studies on the part of both teams which were implicitly and explicitly premised upon Miyazaki's operational scenario and the resource-trading tables that it incorporated. For instance, U.S. and Japan team members were assigned action items to "make multiple, typical 16 day scenarios and statistical [analyses of] five year mission characteristics" which were "based on" the two teams' discussion of Miyazaki's operational scenarios.[146] These studies were conducted over the next three years.[147] Most significantly, a clear and strong joint consensus formed around the resource-trading tables, a consensus that demanded that the ASTER instrument be designed to allow operations that assumed fungible instrument resources and operational flexibility, involving night/day trading, other forms of temporal trading (within over all maximum averages) and variable divisions of observations within orbits. NEC's plan for fixed and regular operations was deemed utterly inadequate and unacceptable.[148] A year later NEC described at a joint team meeting an instrument design that integrated many of the operational capabilities that had been requested by the user-scientists of both teams, and they worked to integrate more.[149]

---

[145] Miyazaki (1993).
[146] ASTER Science Team (1992a: 460).
[147] E.g., Morrison (1992); Hekl (1993); Miyazaki (1993); and Watanabe (1993).
[148] The chair of the EOS Sensor Committee, Dr. Fujisada, recognized the unacceptability of the NEC plan and of the initial specifications for the ASTER instrument's operational capabilities at a meeting in June 1993 (Fujisada 1993).
[149] NEC (1993).

Members of both teams came to remember their negotiations in the early to mid-1990s over ASTER's operations and the ASTER instrument's operational capabilities as different in some qualitative way from their negotiations over the band sets and the signal-to-noise ratios of ASTER's radiometers, even though in the case of the shortwave infrared radiometer these negotiations overlapped to some extent in time and in personnel. As recently as six months after the fourth joint team meeting, far before many of the operational issues were settled, Kahle reported at an internal JPL project management review that her team had successfully "identified and helped resolve operational constraints which were being 'built in' by Japanese contractors and/or General Electric."[150] Her team had, however, "made about as much progress as possible in [instrument] S/N [signal-to-noise] issues."[151] Also at this review, the JPL Project Manager for JPL's participation in the collaboration described "instrument operations scenario and constraints analysis" as a "recent accomplishment," but he named as a "concern" the team's "lack of insight into details of instrument design."[152]

While contemporary documentation of the perspectives of members of the Japan team are unavailable, both Miyazaki and Yamaguchi over a decade later recalled the negotiations over operations in the early years as intensely collaborative, cross-cutting, and complex, with many working group meetings outside of the joint team meetings. With the exception of the early difference in what kinds of operations the two teams initially leaned toward, targeted observations vis-à-vis global mapping, sweeping generalizations about "U.S. versus Japan" skirmishes in the early to mid-1990s over operational planning and over the design of the ASTER instrument's operational capabilities were non existent in interviews, in comparison with the easy recollection of sharp debates over the band sets and signal-to-noise ratios of the

[150] Kahle (1992: 4).
[151] Ibid.., p. 3.
[152] Nichols (1992b: 2, 4).

radiometers.[153] Although perhaps exaggerated in part to construct a history of the ASTER collaboration's success, Kahle remarked that in the two teams' negotiations of the ASTER instrument's operational capabilities and operational scenarios, "we had none of that U.S. versus Japan stuff, none of that."[154] It was in the two teams' negotiations over the ASTER instrument's operational capabilities that they first broke out of bilateral technoscientific diplomacy and began to use transnational authority to design an international techno-political order. Analytical mixing, the nucleation sites of Miyazaki's resource-trading tables, and intersubjective problem-solving had together enabled the two teams to transcend bilateralism.

Breaking Out of Bilateral Diplomacy

Although the U.S. and Japan teams set out to design ITIR, and then ASTER, using a boundary-object strategy that was to avoid the need for the negotiation of common goals, common methods, and other manifestations of transnational community, that strategy to share separately proved to be fraught with complications. The ASTER instrument did not become a boundary object. The politics of technoscientific diplomacy offered both less and more to the two teams than ASTER as a boundary object. It offered less, in that the design specifications of the ASTER instrument were outcomes that were negotiated by liminal state actors from distinct communities of practice who had different professional interests and who worked on behalf of states with interests and goals that were sometimes at odds. The designs of the thermal infrared radiometer, the shortwave infrared radiometer, and the instrument's operational capabilities, did not meet many of the "science requirements"

[153] Yamaguchi (2002); Miyazaki (2003); Nichols (2001); and Kahle (2003, 2005).
[154] Kahle (2005).

of the U.S. team. Conversely, in more than a few cases, MITI and its contractors were persuaded to spend more time, money, and resources to satisfy preferences of the U.S. team and of the joint ASTER team, especially for the additional bands for the thermal infrared radiometer and for the more capable design and hardware for ASTER's operations. Neither the epistemic communities approach nor Latourian actor-network theory can account for the diversity in these outcomes. This chapter has argued that a satisfactory explanation of these outcomes must take into account how liminal state actors assert scientific knowledge and enact state power in their technoscientific diplomacy. In particular, the contrast between the negotiation over the shortwave infrared radiometer and the negotiation over the ASTER instrument's operational capabilities illustrates how the mixing of knowledge and power is an interpretative undertaking, in which U.S.-Japan relations is enacted as much as it is dictated by states' goals or structured by states' material power.

The politics of technoscientific diplomacy offered more to the two teams than ASTER as a boundary object in that the two teams' negotiations—because they were enacted by liminal state actors—articulated not only joint specifications, but along with them, new and refined goals, more effective methods, and new techno-political arrangements. The TIGER team managed to negotiate its incorporation into the ITIR team and to design a vastly more capable thermal infrared radiometer. User-scientists in Japan's geologic remote-sensing community became more influential in instrument design in Japan as a consequence of their participation in the ASTER collaboration, determining key specifications for the shortwave infrared radiometer. By the mid-1990s, owing to the two teams' intersubjective problem-solving, the teams had broken out of their bilateral diplomacy and began facing the challenge of building and governing an international techno-political system to produce knowledge about the earth.

CHAPTER SIX

THE INTERNATIONAL POLITICAL ECONOMY OF THE ASTER DATA AND INFORMATION SYSTEM

In their negotiations over the operational design of the ASTER instrument in 1992 and 1993, the U.S. and Japan teams broke out of their bilateral diplomacy and into collaborative dynamics that fostered transnational authority. In contrast with their negotiations over the designs of the thermal and shortwave infrared sensing bands, in their negotiations over the instrument's operational design the two teams intersubjectively analyzed the advantages and disadvantages of different operational capabilities for the instrument. In their discussion, they used Miyazaki's operational scenario especially as a nucleation site around which they generated their preferred specifications for the instrument, and they successfully pushed contractors to build the instrument according to those specifications.

While these discussions over the ASTER instrument's operational design had implications for the instrument's basic data acquisition capabilities, particularly how flexibly the instrument and it sub-instruments could be turned on and off to acquire data in different observation modes at different times in an orbit, these fundamental design decisions did not determine how the instrument would in fact be used in practice. Recall from the last chapter that Kahle, in an internal JPL management review in December 1992, had reported that her team had successfully "identified and helped resolve operational constraints which were being 'built in' by Japanese contractors."[1] Kahle would later report at a JPL-NASA internal review in March 1994

[1] Kahle (1992b: 4).

that these discussions had "greatly increased data acquisition flexibility."[2] Nevertheless, as of March 1994, this flexibility was still only potential flexibility. As Kahle had explained in the 1992 review, this flexibility was the loosening of hardware "constraints" that would impede the possibility of flexibility in operations. It remained to be seen whether or not, for example, the design, implementation, and operation of the ASTER data and information system would take advantage of this hardware flexibility and would consequently facilitate an ASTER remote-sensing system that was satisfactorily flexible. Would the two teams' transnational authority that had arisen in their negotiations over ASTER's operational design ensure that flexibility was actually engineered into ASTER's remote-sensing system? This particular question about the implementation of flexibility begs a more general one. Did the transnational authority of the U.S.-Japan ASTER team extend beyond the negotiations over ASTER's operational flexibility and pervade negotiations over the design, operation, and maintenance of the ASTER remote-sensing system writ large?

This chapter explains how the two teams worked together as the ASTER team to design and maintain the ASTER data and information system. Furthermore, the chapter explains how, as the two teams designed and maintained the ASTER data and information system, they also configured and governed that system's organized interactions of consumption, production, and distribution of scarce scientific data among states, their researchers, and hundreds, if not thousands, of other consumers of ASTER's remote-sensing data. In other words, the chapter shows how when the two teams were developing a data and information system, they were also developing an international political economy of scientific data. The chapter contends that the transnational authority of the two teams did reach beyond the design of the ASTER instrument's operational capabilities and into the remote-sensing system and its

[2] Kahle (1994: 8).

international political economy writ large. In explaining the ASTER team's design and maintenance of their data and information system and their configuration and governance of that system's international political economy, the chapter illustrates the team's exercise of transnational authority and delineates the scope of their authority. I argue that the ASTER team underpinned their exercise of transnational authority and their construction of international order—order in the form of an international political economy—by using and enacting a normative notion of U.S.-Japan partnership and equality. By focusing on how the liminal state actors of the ASTER team used and enacted this normative notion as one line of prescriptive reasoning and action, I account for the dynamics of these liminal state actors' technoscientific diplomacy as they grappled with wide-ranging concerns that included economic value and efficiency, the integrity of scientific knowledge, national autonomy, state power, and U.S.-Japan relations. I suggest that the utility of a normative notion of U.S.-Japan partnership and equality for the team's technoscientific diplomacy was enhanced by team members' liminality with respect to their states and by socio-political interdependency in their joint work.

A Data and Information System as a Solution to Problems of International Order

For NASA, the ASTER data and information system was one system among many other data and information systems that were to be integrated together to form the EOS Data and Information System (EOSDIS). EOSDIS was a fundamental component of NASA's Earth Observing System. While EOS satellites orbited the earth, EOSDIS was intended to enable NASA to establish an international order on the ground, organizing an international community of scientists and engineers as well as organizing data production, distribution, and consumption across states. Latourian actor-network theory and the epistemic communities approach differently suggest how NASA and its scientists and engineers would use EOSDIS to establish and govern an international order and how they would integrate the ASTER data and information system into that order.

In the late 1980s and early 1990s, NASA endorsed a vision for EOS which extended beyond the United States and reached across the globe. NASA, Congress, and the White House looked to multinational cooperation with Japan and Europe to lessen EOS's unprecedented cost in particular. Of course, that rationale was not usually stated quite so bluntly. The 1987 strategy report of NASA's steering committee for EOS asserted a "commonality of interest and mutual benefit of shared cost" that "bind the national efforts in Earth science together and have led to significant ongoing cooperation in satellite programs and Earth remote sensing."[3] The report continued to explain how global investigations that "transcend national borders" required "various kinds" of multinational resources, such as intellectual and financial support and geographical access. In such an international effort, involving collaborations such as the ASTER collaboration, how would these "various kinds" of multinational resources be negotiated and coordinated? The report explained that the "Earth science research community," which is what NASA called the primary intended users of the EOS system, was "quite large and widely distributed throughout the United States and the world."[4] What kind of unity among these scientific investigators would actually be required to produce global knowledge about earth processes?

For NASA administrators and scientists, EOSDIS was essential to any solution to these questions about international coordination and unity. NASA's steering committee explained EOSDIS as "the key to the EOS concept and to its ultimate

[3] NASA, Earth Observing System Science Steering Committee (1987b: 29).
[4] Ibid., p. 27.

success in meeting the needs of the Earth science community."[5] NASA conceived of EOSDIS as an extensive system of satellite command and control centers, data processing facilities, data archive and distribution systems, and communications networks. This vision of EOSDIS also included numerous systems that were dedicated to specific remote-sensing instruments that, like ASTER, would sit onboard NASA's satellites.[6] In NASA's vision, international as well as national instruments and systems were all wrapped into EOSDIS.

When NASA planners in the late 1980s and early 1990s portrayed EOSDIS, however, they conceptualized their international system as coordinating and uniting not diverse governments, organizations, and communities of practice, but more reductively, individuals with instruments. The strategy report of the NASA steering committee declared that the "implementation of EOS should *start* with a data and information system that *unites* researchers and sources of data" (emphasis mine).[7] In the steering committee's concept of EOSDIS's architecture (see figure 6.1 on the next page), on top were rows of boxed users who submitted their requests for instrument observations to the data management and communications network, which then would pass those requests down (in the figure's frame of reference) to an instrument on the platform via the satellite platform's network. Once instruments acquired the data, the data would be passed back to the ground systems for processing, archiving, and distribution.[8] The text that accompanied figure 6.1 described EOSDIS's architecture as non-hierarchical and distributed but also as integrated. This massive data and

[5] Ibid., p. 25. Also see the earlier EOS Data Panel report on which this implementation strategy was based, NASA, Earth Observing System Data Panel (1986), and a later development of the EOSDIS vision, NASA (1992).
[6] In EOS planning in the late 1980s, the "satellites" were large platforms that would be serviced by space shuttle missions.
[7] NASA, Earth Observing System Science Steering Committee (1987b: 29).
[8] Ibid., p. 28. This architectural concept was initially proposed by NASA, Earth Observing System Data Panel (1986).

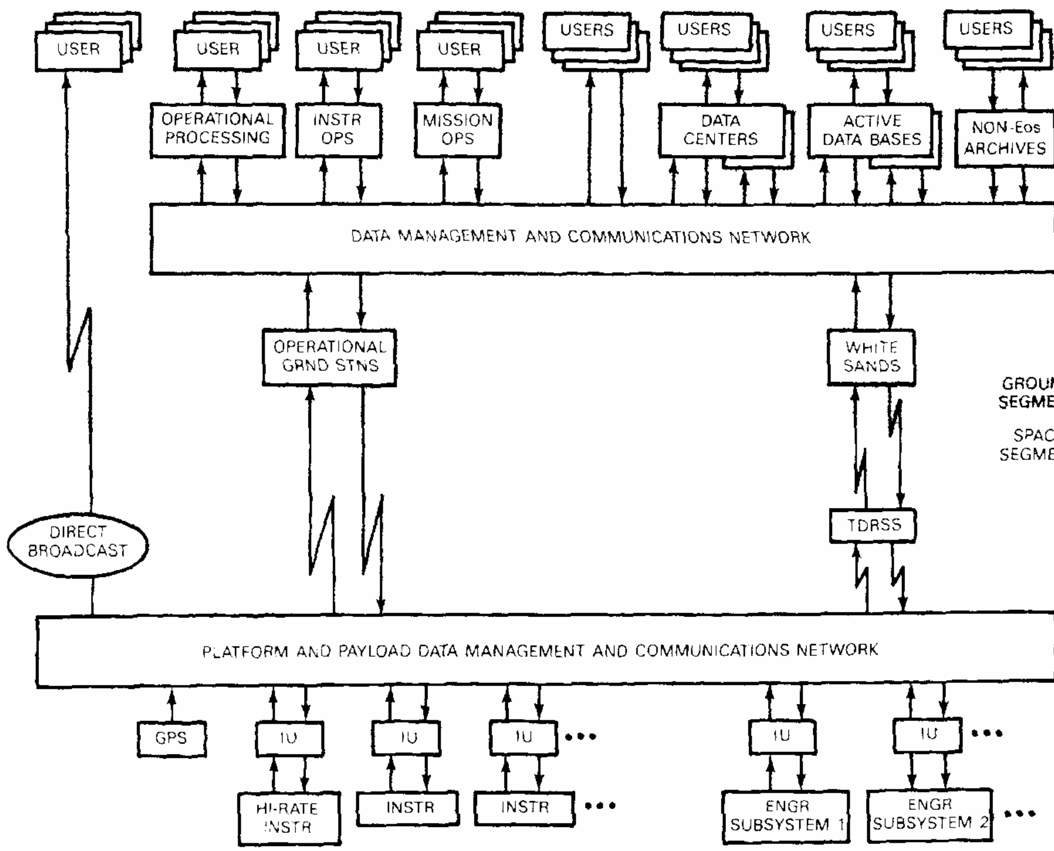


Figure 6.1: The EOSDIS Concept

information system was thought to be “the foundation upon which the rest of the [EOS] mission is built.”[9] NASA’s plan for EOSDIS to connect instruments with individual user-researchers cut out the middle man of politics between nations, and it envisioned a reordering of international politics around an integrated, yet distributed, techno-political system.

If Latourian actor-network theory were used to describe this vision of EOSDIS, EOSDIS would be NASA’s “center of calculation” in which “immutable” and “mobile” black-boxed users and instruments—national and international—were all “enrolled” into EOSDIS’s extensive imperialist networks, bringing virtual “inscriptions” of imagery back to EOSDIS’s home at NASA’s Goddard Space Flight Center (GSFC) or to one of the several EOSDIS distributed active archive centers. EOSDIS would likely be interpreted on the whole as an enterprise that eschewed concerns of transnational community and authority in favor of state-centered empire building.

In 1990, after further planning, NASA’s advisory panel for EOSDIS stressed many of the same features and objectives for EOSDIS’s system architecture as had been described in the 1987 strategy report, but with further specification (see figure 6.2 on the next page).[10] First, EOS was indeed going to be a large and extensive enterprise. In 1992, NASA estimated that it would spend over three billon dollars to develop EOSDIS through the year 2000, which at that time, was between a quarter and a third of the entire budget for EOS. NASA anticipated that as many as ten thousand earth science researchers would use the system and perhaps as many as two hundred

[9] NASA, Earth Observing System Science Steering Committee (1987b: 25-7).

[10] The figure is from Dozier (1990: 14). Dozier was the chair of the NASA’s advisory panel for EOSDIS. That figure of EOSDIS’s architecture was also used in NASA’s vision of EOSDIS as described in NASA (1992: 10) and in NASA (1993: 28). The figure was picked up and used to explain EOSDIS to international audiences, see for example, Tsu (1991: 141).

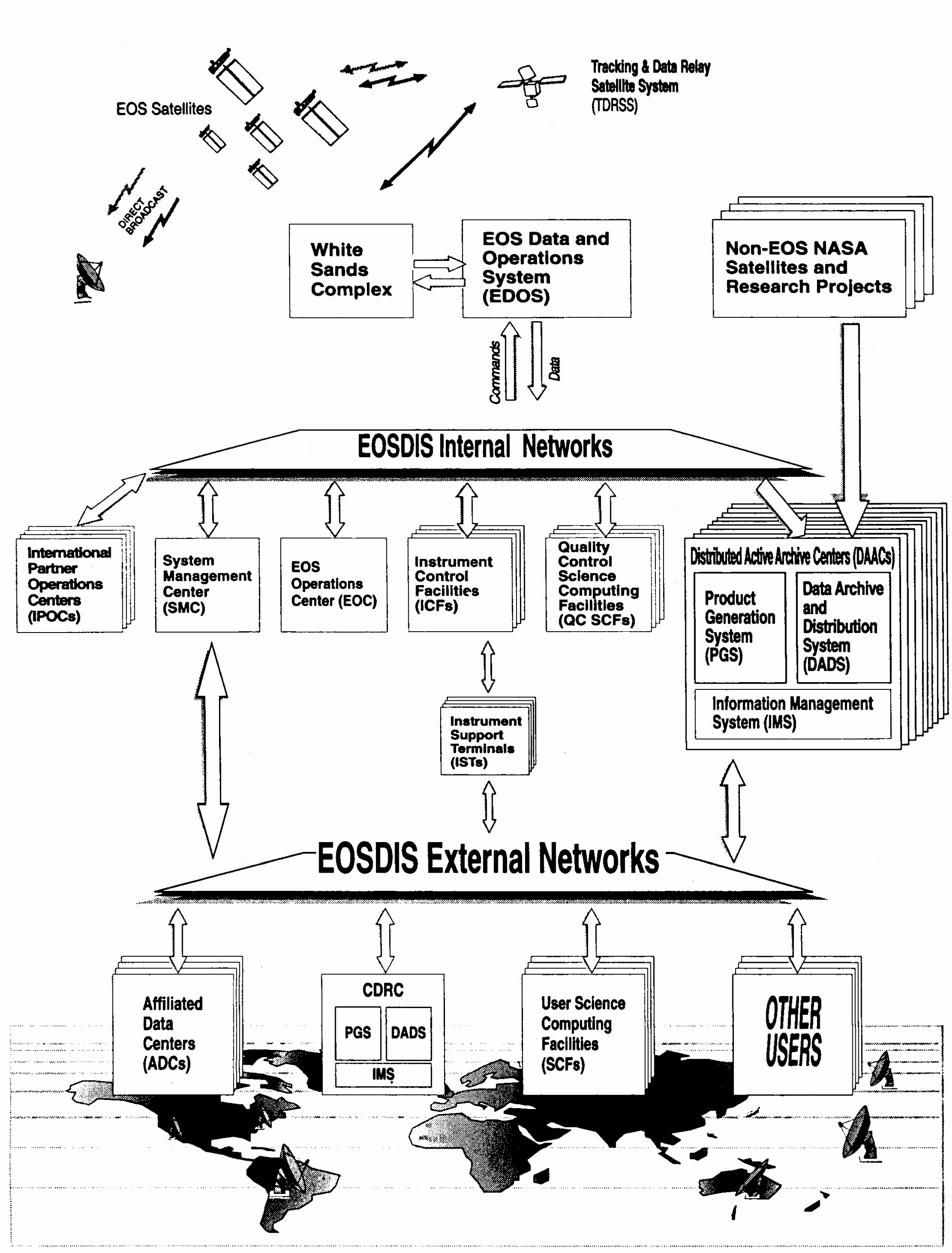


Figure 6.2: The EOSDIS Architecture

thousand other users.[11] In testimony to Congress in 1992, the U.S. General Accounting Office described EOSDIS as:

> a massive undertaking, both in terms of the system's scope as well as its cost. . . . The sheer size of EOSDIS is staggering; its intended scope far exceeds that of any previous civilian data management system. Over its lifetime, the system could accumulate a mass of data equivalent to more than 1,000 times the amount of text stored in the Library of Congress.[12]

The EOSDIS advisory panel struggled to find a balance between centralized hegemony and individualistic autonomy in their proposed architecture. They found their balance by casting EOSDIS not only as an infrastructure but also as a generalized socio-technical space in which scientists could produce knowledge. In contrast to a Latourian imperialist vision, EOSDIS planners made clear in their reports that they were not seeking to impose a top-down monopoly on the production of knowledge. "Individualized styles of research must be accommodated," the EOSDIS advisory panel said. Yet, they insisted that "EOSDIS must become the logical integrator" of facilities and researchers that are "not now closely knit."[13] To accomplish this integration without hegemonic centralization and bureaucratization, the chair of the EOSDIS advisory panel encouraged the adoption of a "philosophy" that envisioned EOSDIS as a "place":

> A view that EOSDIS is a *thing*, a piece of hardware supported by software, is fundamentally mistaken. EOSDIS is not a collection of hardware and software, it is a "place" where scientists communicate with each other and with the data they have collected with the help of engineering and operations disciplines. . . . It may even be correct to view EOSDIS as *the place where the*

[11] U.S. General Accounting Office (1992a: 12-15). The two hundred thousand figure is from U.S. General Accounting Office (1995a: 3).
[12] U.S. General Accounting Office (1992b: 1).
[13] Dozier (1990: 12). The amount of text stored in the Library of Congress is generally said to be around 20 terabytes of information, for the purpose of these comparisons, if the Library of Congress were to digitize its holdings.

> *scientists produce information to be used by other scientists*. EOSDIS must be run **by** scientists, **for** scientists.[14] (emphasis in original)

Scientists would both use and govern EOSDIS, and that, in this vision, would go a long way toward holding centralization and hegemony in check.

Yet, this vision seemed to stipulate, as does much of the literature on epistemic communities, that scientists from around the world would be able to gather as a community into a single town hall and come out agreeing on how to produce knowledge about the earth. In this vision, EOSDIS would be run by something akin to scientist-philosopher Michael Polanyi's "Republic of Science," a polity in which the judgments of individual scientists were spontaneously checked and coordinated throughout the world only by other scientists via "the invisible hand." [15] In NASA's vision, that invisible hand would be EOSDIS. Whatever its mechanism of governance, the EOSDIS advisory panel as well as subsequent NASA scientists and engineers who spoke for EOSDIS portrayed the system's "place" as not at NASA's GSFC, but as spanning across the globe (e.g., figure 6.2), creating a coherent system of systems.[16] EOSDIS and its associated data and information systems were to help bring about a global order of knowledge production.

In the late 1980s and early 1990s, when NASA Headquarters and the GSFC were planning EOSDIS and starting to define EOSDIS's scope and requirements, MITI's Earth Resources Satellite Data and Analysis Center (ERSDAC) had yet to begin a similar process for the ASTER instrument's data and information system. ERSDAC was focusing on developing and operating the data and information system

[14] Ibid., p. 10. This "overview and philosophy" of EOSDIS is also restated in NASA (1992: 29-30).

[15] Polanyi (1962). In the context of the times in which it was originally asserted, Polanyi's image of science was actually in reaction to populism.

[16] Dozier (1990: 14), NASA (1992: 10), and NASA (1993: 28). Recent architecture concept diagrams are also inclusive of systems funded and developed by other governments. See, for example, the work-in-progress architecture diagrams of NASA's Interface Control Working Group (2002).

for the first Japanese Earth Resources Satellite (JERS-1), which was the satellite program that had prompted the establishment of the ERSDAC industry consortia in the early 1980s. JERS-1 was planned to launch in 1992, and ERSDAC's development of JERS-1's data and information system continued up until that launch date. The system, which was called the Earth Resources Satellite Data Information System (in line with the name of the JERS-1 satellite), was a substantial enterprise that was designed to receive, process, and distribute data from JERS-1's radar and optical sensors.[17]

JERS-1's Earth Resources Satellite Data Information System required, however, only a small fraction of the resources that were anticipated to be required by EOSDIS—somewhere around ten managers, not a hundred; around a hundred scientists, engineers, and technicians, not a thousand; and less than a hundredth of the capital.[18] Assuming that both the JERS-1 and EOS satellite programs went as planned, the Earth Resources Satellite Data Information System for JERS-1 would have involved less than a hundredth of EOSDIS's users and would have handled well less than a *ten thousandth* of the information that EOSDIS was expected to process.[19] In

[17] Sōritsu jūnenshi henshū iinkai (1993: 140-149).

[18] Some readers might want to know the basis for this comparison. These estimates are rough and conservative, based upon the approximate workforce and equipment of NASA's GSFC, the nine EOSDIS data centers, and their contractors, in comparison to the workforce of ERSDAC and the equipment for the Earth Resources Data Information System (Ibid.).

[19] This last comparison is based upon a conservative back-of-the-envelope calculation that almost certainly underestimates the difference in scale of the two data and information systems. Drawing upon information from the JERS-1 handbook (Jinkō eisei kaihatsu honbu chikyū kansoku eisei gurūpu 1991), the calculation compares the maximum possible throughput of the planned JERS-1 satellite over the satellite's expected life-span with the amount of data that EOSDIS was expected to accumulate. It then allows for NASA to process and distribute a hundred times more data for users, which is certainly a significant underestimate given the difference in the size of the user populations and given that NASA's EOSDIS was expected to offer (and process) almost a hundred times the types of "data products" that JERS-1's data information was expected to process (by "types" I mean different kinds of data products which represented different geophysical measurements).

sum, EOSDIS would dwarf ERSDAC's system for JERS-1. Similarly, EOSDIS would dwarf ERSDAC's system for JERS-1's successor, the ASTER instrument.

If MITI, ERSDAC, and the Japan team were going to have any real power in their negotiations with NASA, GSFC, and the U.S. team over the design and operations of the data and information system for the ASTER instrument, the negotiations could not resemble "trials of strength" between Latourian "centers of calculation." According to Latour's account of the dynamics of technoscience, if Latour's use of the words "size," "scale," and "length" to characterize the strength of a network means anything, U.S.-Japan technoscientific diplomacy over ASTER's data and information system would necessarily end in NASA's dominance, barring extraordinarily weak "associations" in NASA's massive network. Yet, U.S.-Japan diplomacy did not end in NASA's hegemony. U.S.-Japan technoscientific diplomacy was, nevertheless, laden with the exercise of power to an extent that is unrecognized and unaccounted for by both the "Republic of Science" vision and the epistemic communities approach.

Designing the ASTER Data and Information System

The "ASTER Centric" End-to-End Data System Concept

The U.S. and Japan science teams played significant roles in the design and operation of the ASTER data and information system. The two teams did not, however, engineer and build that data and information system from the ground up themselves—selecting all the hardware, testing and implementing all the computer systems, and writing all the layers of software code. It was GSFC and ERSDAC that had immediate design and contractor responsibility for the U.S. and Japan sides of the

ASTER data and information systems. NASA Headquarters expected the ASTER data and information system to be integrated with GSFC's EOSDIS, which would have its own across-the-board requirements and standards for the dozens of data and information systems incorporated into its network. Yet, since the U.S. and Japan science teams were the ones who had initiated the ASTER collaboration at its broadest and were meeting regularly and extensively in the early 1990s, the two teams had the opportunity to become significantly involved in the process of developing the architecture of the ASTER remote-sensing system, including its data and information system. In the first half of the 1990s, many of the basic requirements for the ASTER data and information system were still being worked out. High-level documents such as the U.S.-Japan Memorandum of Understanding (MoU) and the GSFC-ERSDAC Project Implementation Plan (PIP) for the ASTER data and information system were still being negotiated, with contributions from key members of the two teams. In addition, the development of EOSDIS lagged the development of the ASTER instrument as well as the rest of the instruments on EOS's first satellite. Consequently, in the early 1990s the U.S. and Japan teams were asking questions and exploring solutions before GSFC and ERSDAC had settled upon their own answers, many of which would have in any case taken into consideration the preferences of the two teams.

The U.S.-Japan ASTER data and information system was to be a well integrated, continuous system that allowed comprehensive interoperability across the Pacific between the U.S. and Japan sides.[20] In 1992, David Nichols was appointed as JPL's project manager for the "ASTER Science Project." The "project" was JPL's support for project management and engineering which the JPL management had

[20] Remember that in the early 1990s, the hardware and software infrastructure for high-speed internet use was not yet available. The early, experimental versions of hypertext browsers such as Mosaic / Netscape Mosaic were not developed until 1994.

decided Anne Kahle needed in order to effectively carry out her work as the leader of the "U.S. ASTER Science Team."[21] The project included scientists, system engineers, and software engineers who were not themselves formal members of the U.S. ASTER science team, which was the team whose members had been directly designated and funded by NASA and/or MITI.[22] One of David Nichols's responsibilities as project manager was to help Kahle "ensure ASTER instrument applicability and contribution to EOS scientific objectives by influencing the development of instrument requirements and Ground Data System designs."[23] Almost from the beginning, the U.S. science team considered it to be their responsibility to be involved in the design work for the ASTER data and information system (i.e., the "Ground Data System" referenced in that last quote).

In carrying out this responsibility, Nichols decided in the fall of 1992 that a "System Concept Document" was needed that would take an "ASTER centric end-[to-]end view of the system."[24] The need for a data system concept document had been mentioned in the data system working group at the fourth U.S.-Japan ASTER team meeting the previous summer, and it had become an action item of the working group for the U.S. side to write such a document and present it "to Japan for discussion."[25] "ASTER centric" meant a perspective of the ASTER data and information system which looked outward from the ASTER instrument and the

---

[21] Several interviewees at JPL informed me that JPL management had pressured Kahle into accepting this JPL support. The funding for this organizational and technical support would come out of NASA's funding for the U.S. ASTER team, which were funds that were, once allotted, largely under the control of Kahle as the U.S. team leader. Although Kahle had reportedly expressed, initially at least, that she thought that such organizational support was excessive and would be a drain on NASA funds for the U.S. ASTER team, JPL management disagreed, and the JPL ASTER project came into being in early 1992. Interviewees preferred that this bit of information not be attributed to them.

[22] Nichols (1992b: 4).

[23] Nichols (1992d: 2).

[24] Nichols (1992a: 4).

[25] Data Receiving, Processing, Archiving and Distribution Working Group (1992a: 65). Data Receiving, Processing, Archiving and Distribution Working Group (1992b: 54).

ASTER team's work. This perspective was not necessarily the perspective that was presumed by the system engineers at either MITI's ERSDAC or GFSC's EOSDIS or by NASA Headquarters. Nevertheless, this ASTER centric concept document was intended to be, and was in practice, "developed in conjunction with" the GSFC-ERSDAC PIP as well as the NASA-MITI MoU during the early 1990s.[26] In the fall of 1992, the task of writing the concept document had landed in the lap of Gary Geller, a system engineer for the JPL ASTER project.[27] Below, I quote at length his explanation of the purpose of the *ASTER End-to-End Data System Concept Document* to convey the general political sensibility of "ASTER centric" planning and coordination, and in particular, how Geller understood his role as a system engineer in the ASTER project:

> You know, people would ask me, you know, "what do you do?" I say, I, I, argue with people. Oh so you're a lawyer! Oh, no, no, no, I'd say, I'm a system engineer in this project. Everyone has heard of that but people never have any idea of what that is, so I would explain with great simplification that there are all these different groups, some of them are at JPL, but within different cultures at JPL, some are somewhere else in the U.S., some are in Japan, some are at Goddard [i.e., GSFC], the funding agency, and so forth. And these groups don't really like to talk to each other. Um, but they are all building different parts of the system, so if they don't talk to each other, the system won't work, because this guy will build it this way, but this guy will expect it that way. So, I would tell them what I would do is try to argue with people, but in a way that they didn't know that we were having an argument. . . . Basically, since no one worked for me, I had to convince people to do the right thing, even though you had no real authority over them, especially if they were in Japan or were at Goddard or somewhere. So that's kind of arguing, but they don't know you are having an argument, um, to try to ensure that in the end you would have this continuity [in the data and information system]. So the purpose of this document [i.e., the End-To-End Data System Concept document] was not exactly to do this design, but to create a framework within which you could do the design, and also to figure out what you didn't know, so you could earmark that as a gap that you would then need to fill in one way or another.[28]

---

[26] Nichols (1992a: 4). See also Geller (2003) and Nichols (2001).
[27] Geller (1992).
[28] Geller (2003).

In sum, the document was a high-level engineering document that was written to help foster national and international consensus around, and win authority for, an ASTER centric data and information system.[29]

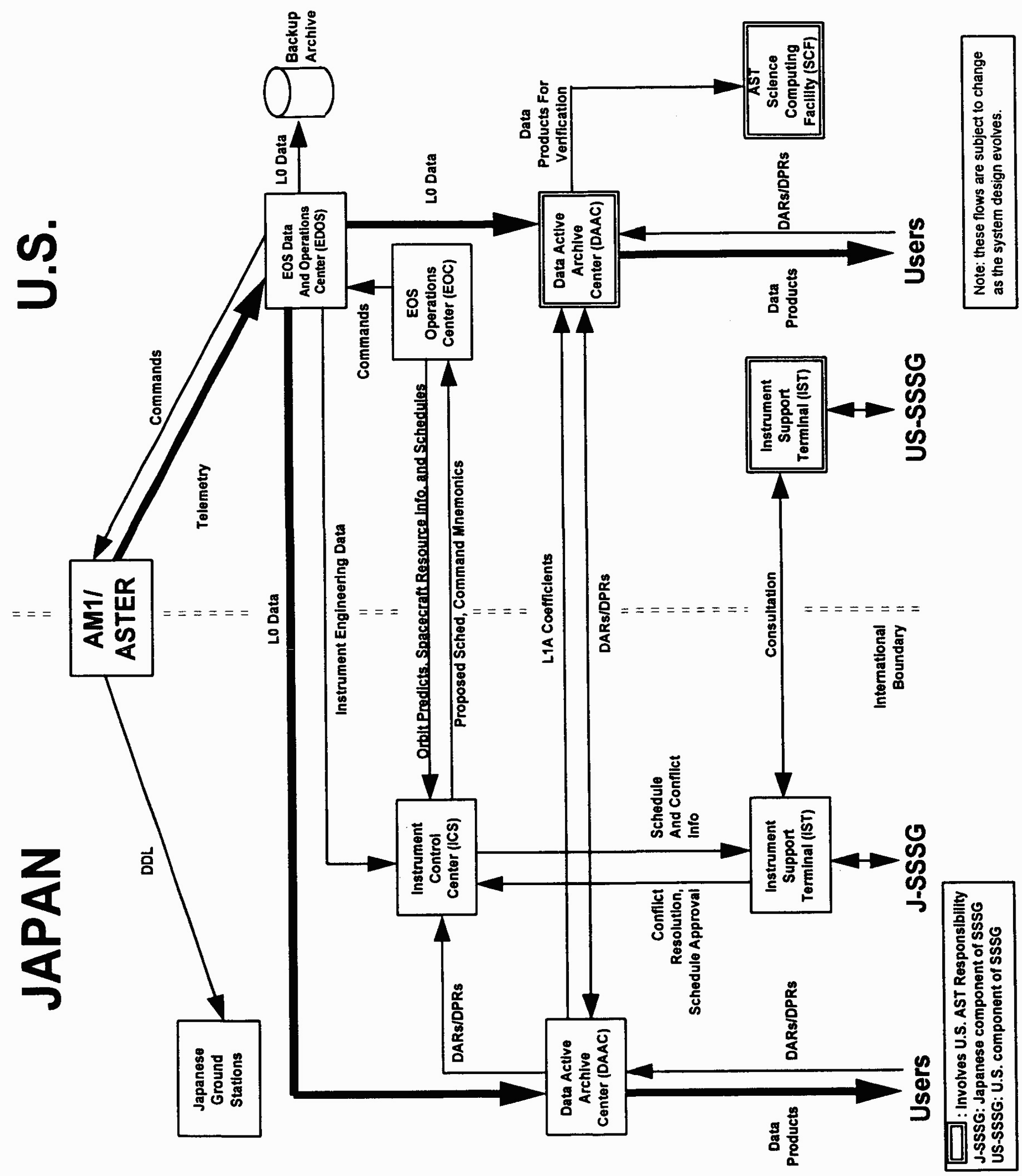
At the sixth U.S.-Japan team meeting in November 1993, Geller distributed the first draft of the *ASTER End-to-End Data System Concept Document*.[30] That document included a draft of figure 6.3 (on the next two pages).[31] This diagram of the ASTER data and information system, as well as the document's accompanying description, highlighted the connections and data flows between Japan on the left and the United States on the right, with the ASTER instrument positioned on the international boundary (as chapters four and five have argued, the ASTER instrument did not in fact become a "boundary object"). The diagram was the U.S. team's proposed architecture for the U.S.-Japan international relations that the ASTER data and information system would materialize. Both the Japan and U.S. teams eventually used later versions and variations of this diagram—typically a colorful Power Point version of it—to convey the basics of the U.S.-Japan relationship that they were building. They used versions and variations of this diagram not only in presentations to each other, but also in their presentations to broader audiences, such as to NASA

[29] The document itself said as much as well. The introduction to the document read: "The purpose of this document is to establish a common understanding (among the U.S. Science Team members and the EOS Program and Project) of the operational end-to-end data system for the Advanced Spaceborne Thermal Emission and Reflection Radiometer (ASTER). . . . It is the responsibility of the AST [i.e., the ASTER Science Team] to understand all aspects of this end-to-end system so that the Team can 1) influence, to the extent possible, the design of this system to maximize the scientific utility of the ASTER mission, and 2) characterize the team's responsibilities and interfaces with other organizations regarding the development and operations of particular pieces of this end-to-end system" (Geller 1994: 1). The draft of the document, which was distributed a year earlier, had an introduction with almost exactly the same wording.

[30] Geller (1993b).

[31] Figure 6.3 is actually a copy of the diagram from a later version of the concept document, Geller (1994). Because the first draft, version 1.0, included in it some connections and flows that this chapter does not discuss and which are of lesser importance to the overall development of the system's architecture, I have used here the later, more polished version of the diagram. Differences between the draft and the later version are noted in the above discussion when relevant.

Figure 6.3: The ASTER Data and Information System



JAPAN
U.S.
AM1/ ASTER
Commands
Telemetry
DDL
Japanese Ground Stations
EOS Data And Operations Center (EDOS)
L0 Data
Backup Archive
EOS Operations Center (EOC)
Commands
Instrument Engineering Data
Orbit Predicts, Spacecraft Resource Info. and Schedules
Proposed Sched, Command Mnemonics
Instrument Control Center (ICS)
Data Active Archive Center (DAAC)
L1A Coefficients
DARs/DPRs
Data Products For Verification
AST Science Computing Facility (SCF)
DARs/DPRs
Data Products
Users
Instrument Support Terminal (IST)
US-SSSG
Consultation
Schedule And Conflict Info
Conflict Resolution, Schedule Approval
J-SSSG
International Boundary
: Involves U.S. AST Responsibility
J-SSSG: Japanese component of SSSG
US-SSSG: U.S. component of SSSG
Note: these flows are subject to change as the system design evolves.

EOS project administrators, MITI officials, other earth scientists and space system engineers, and general users. Often the diagram was titled "U.S.-Japan Relations" in English or the equivalent in Japanese (*nichibei kankei*).[32]

Symmetry and Interoperability

The relationship between the United States and Japan in the design of the ASTER data and information system depicted in figure 6.3 was characterized by roughly symmetrical systems and capabilities and a high-level of interoperability, or—in the language of international politics—mirror imaging and interdependence. To show this mirror imaging and interdependence, I outline below how the end-to-end data and information system was basically supposed to work, according to the conceptual design of figure 6.3 and the *ASTER End-to-End Data System Concept Document* (the details of the system's design and construction is unpacked later in the chapter).

At the starting "end" of the end-to-end system at the bottom of the figure, users in either Japan or the United States place data acquisition requests ("DARs" in the figure) with their state's distributed active archive center, that is, with their "DAAC" (in the figure, the acronym is expanded in a non-standard way, as a "data" active archive center). The DAAC in Japan is physically located at ERSDAC, and the DAAC in the United States is located in South Dakota, at the site of the U.S. Geological Survey's Earth Resources Observation and Science Data Center.[33] The

[32] Nichols et al. (1995: 53); Watanabe et al. (1995: 35); Yamaguchi et al. (1999: 1420); Zaidan Hōjin Shigen Kankyō Kaisoku Kaiseki Sentā (1999: II-34); Kahle (2003b: 30); and Yamaguchi (2003: 12).

[33] While the U.S. DAAC for ASTER was located at the Earth Resources Observation and Science Data Center in South Dakota, the U.S. website for the data acquisition requests for ASTER as it turned out was hosted by NASA's Goddard Space Flight Center in Maryland.

data acquisition requests are then passed on to Japan's instrument control center at ERSDAC (via Japan's DAAC, for the case of data acquisition requests from the United States). The instrument control center in Japan creates an observation schedule for each orbit of the ASTER instrument, including where the instrument will look, when it will turn on and off, what observational modes it will use, etc.

If a scheduling conflict arises that calls for the attention of the U.S. and/or Japan team, their respective Science Scheduling Support Group ("SSSG" in the diagram) can intervene. The U.S. and Japan teams can interact with Japan's instrument control center through their instrument support terminals at JPL and ERSDAC. Breaking the symmetry in control capability, however, the U.S. team's terminal at JPL would go through Japan's terminal first. The U.S. team had initially wanted to be able to interact directly with the instrument control center in Japan from JPL, which would have preserved the symmetry in control relations between the two teams. When the request was made early on, however, NASA Headquarters and the GSFC made clear that they would not fund the extra cost of such an arrangement, given that the GSFC would have its own instrument support terminal for ASTER at the EOS Operations Center at the GSFC in Maryland. GSFC's terminal would thus preserve some symmetry in control at the level of states. Once ASTER's observation schedule is provisionally settled at Japan's instrument control center, it is sent to the EOS Operations Center at the GSFC for integration with the commands for other instruments onboard the NASA satellites. The commands are then sent to the EOS satellite ("AM1" in the diagram) through the EOS Data and Operations Center in West Virginia ("EDOS" in the diagram) and through NASA's tracking and data relay satellite system.

After the ASTER instrument acquires its imagery data, the data are stored on the satellite's data recorder and transmitted along with the rest of the satellite's data through NASA's tracking and data relay satellite system and to ground receiving stations at NASA's White Sands Complex in New Mexico. Japan also proposed that an alternative path be made available, using a direct downlink ("DDL" on the diagram) to Japan's ground receiving stations in Japan (or possibly elsewhere). This direct downlink would have the advantage for Japan of not going through NASA's systems, and thus would preserve another element of symmetry and national autonomy. The direct downlink would be more limited in the quantity of data that it could transmit than would the path through NASA's systems, however, because the direct downlink would not utilize any kind of tracking and data relay satellite system when ground receiving stations were out of the satellite's range during the satellite's orbit around the earth.

NASA's station at the White Sands Complex forwards the satellite's data to the Earth Data and Operations Center, which strips away data that are not relevant to the ASTER instrument and strings together the data that are relevant, generating what are called ASTER's "level 0" data. The level 0 data are archived in the United States and are sent to Japan electronically. Japan's DAAC at ERSDAC then uses the level 0 data to produce level 1A and level 1B scenes of imagery.

The distinction between level 1A and 1B is quite significant. In level 1B data, each of the thousands of pixels that compose an image scene is geometrically and radiometrically corrected. The image scene in level 1A data, however, is not geometrically and radiometrically corrected ("radiometric correction" refers both to the conversion of "raw data" into measurements that are related to standardized quantities and to the adjustment of "raw data" to compensate for changes in the sensor's sensitivity; to "radiometrically correct" an instrument, the instrument must be "calibrated" periodically). According to all of the parties involved in the ASTER collaboration, the generation of information that is used to correct any particular level

1A scene requires calculations that are computationally intensive and complex, especially for the geometric correction.[34] Furthermore, the generation of this correction information also requires the changing sensitivity of each of ASTER's three sensors to be characterized pre-flight and in-flight. Everyone involved expected that most users would prefer for the starting point of their use of the imagery data either the corrected level 1B "data product" or the "higher-level data products" that are calculated from the level 1 products. Because the level 1 data products are the staple of the ASTER data and information system, both the United States and Japan regarded level 1 processing as requiring significant computing resources.

Higher-level data products—level 2 and higher—are data products that use lower-level data to calculate estimates of particular measurements that scientifically describe the earth's surface. These measurements are often represented as an image. For example, one level 2 data product for the ASTER instrument was anticipated to be measurements of the earth's surface temperature with a resolution of ninety meters (which is the resolution of ASTER's thermal sensor). This difficult calculation involves, among other things, correcting level 1B data for "atmospheric effects"—that is, for what happens to electromagnetic waves as those waves travel from the earth's surface to the sensor. One level 3 data product that was planned by both the U.S. and Japan teams was a three-dimensional digital elevation map of a scene of the earth's surface. This data product would make use of data from the ASTER instrument's two telescopes for the visible region of the electromagnetic spectrum in order to obtain a stereo view of the earth's surface. From this stereo view an elevation map can be constructed.

[34] For the sake of simplicity, my use of the term "geometric correction" includes the processes for spatially correcting the shortwave infrared sensor's parallax and for spatially registering ASTER's telescopes with each other.

In the definitional scheme of data product "levels," a scheme that was based upon international practices in earth observation in the 1970s and 1980s and which NASA, its advisory groups, and EOSDIS tweaked in the early 1990s, each higher level of data is roughly another step away from the "raw data" that are produced by an instrument's electronic response to the electromagnetic waves that hit the sensor while the sensor is orbiting in space. Getting from the ASTER instrument's electronic response to meaningful and useful information about the earth's surface was a core responsibility of the two teams.

Once either or both of the states' DAACs generated data products using algorithms that the two teams developed, the DAACs catalogue the data products and make them available to users who are authorized by each state. These users could include users from third-party states. The *ASTER End-to-End Data System Concept Document* envisioned the two DAACs and their catalogues as interoperable: the DAAC in the United States could, on behalf of U.S. users, request products from the DAAC in Japan, and vice-versa. The distribution and exchange of data would be in accordance with a general policy and pricing arrangement that was to be negotiated by states that were participating in the Earth Observing System (i.e., in what was called the "International Earth Observing System"). Such an EOS-wide data exchange policy was in its final stages but was still under negotiation when Geller and the U.S. team authored and distributed both the first version and final version of the *ASTER End-to-End Data System Concept Document*.[35]

Although the concept document was, strictly speaking, just a JPL working document and not a document that the U.S. and Japan team leaders, or NASA's GSFC and MITI's ERSDAC, would finalize and jointly sign, it was nevertheless intended, according to its introduction, "to establish a common understanding (among the U.S.

[35] Geller (1993b).

Science Team members and the EOS Program and Project)" of the ASTER data and information system. Furthermore, the U.S. team distributed the document to the Japan team and to ERSDAC for discussion, as the joint data system working group had requested when they initially called for the document in an action item.[36] In my interview with Geller, I asked him what he and the U.S. team were hoping to accomplish by distributing the document to his Japanese counterparts, since according to the parenthetical in the above quote from the document's introduction (see footnote 29), the Japan team had not been included as the intended audience of the document:

> That's a good question. I think [the intended audience] was primarily the U.S., and I think . . . what you said reminded me that was the other reason [for creating the document]. We wanted other people to look at this and make sure that they knew what other people were thinking, to line them up for this end-to-end continuity thing. But we wanted Japan to know what we were doing, so that if there was some problem we could resolve, but we don't have any authority over them, and we didn't want to offend them. So I think that's why that parenthetical thing was in there, so that we could distribute it to them and make it clear that we were not telling them what to do, but if this doesn't make sense, could you please let us know.

Geller's response conveys how these kinds of informal and conceptual working documents were shared diplomatically as resources for building international as well as national consensus, in the same way as the "trade night-for-day" matrix described in the previous chapter was passed from GSFC engineers to Miyazaki and ultimately served as a nucleation site after Miyazaki incorporated it into his plan for ASTER's operational design.

At the prodding of Nichols, who according to Kahle and Geller "liked documents," the ASTER science project in the early 1990s authored several other JPL

[36] Ibid., p. 1-1. I know that the U.S. team distributed the document to the Japan team not only because the document was included in the minutes of the 6th team meeting, but also because members of the Japan team were listed on the document's distribution list and because I saw copies of the document in the offices of members of the Japan team.

working documents that informally were disseminated to the Japan team for their consideration.[37] These documents concerned the U.S. team's plans for the ASTER data and information system and were in addition to presentations that they exchanged during team and working group meetings. They included the *ASTER Team Member Algorithm Software Development Guidelines* (1992), the *Product Generation System Concept* (1993), and *DAR Generation and Processing Concepts* (1994).[38] In contrast, informal working documents that members of the Japan team or ERSDAC authored made their way over to the U.S. team much less commonly. One of the few examples was *User Interface Requirements Document Overview* (1993). The document was authored by Satō Isao, who was a scientist at the Geological Survey of Japan, a member of the Japan team, and the chair of ERSDAC's advisory committee for the ASTER Ground Data System Committee.[39] Satō's document, which was an English version of a Japanese original, was presented and distributed to the two teams at the sixth team meeting, just after the distribution of the first draft of Geller's document. The timing of the distribution of the two documents was almost as if their distribution were an exchange of think pieces.[40] Yet, whereas Geller's document presented a broad view of the ASTER data and information system, including an end-to-end architecture, data acquisition procedures, and geometric and radiometric calibration plans, thus staking out broad and substantial territory for the two teams' discussion and joint jurisdiction vis-à-vis their two respective data and information system bureaucracies, Satō's document focused more narrowly on what features ERSDAC should include in the user interface to ERSDAC's DAAC.

[37] Kahle (2003) and Geller (2003).
[38] Voge and Larson (1992); Voge (1993); and Borutzki (1994).
[39] Satō (1993).
[40] ASTER Science Team (1993a).

This contrast in scope was one of many contrasts that denoted the different demands placed upon, and ambitions of, the two teams. While both of these working documents were valuable to try to get everyone on the same page before the formal documents were settled, members of the U.S. science team and the JPL project could hold out these working documents to JPL and NASA management as work that had been accomplished in the early development stages of a project. In the parlance of project management, they could be pointed to as "deliverables."[41] Members of the Japan science team, as advisors to ERSDAC, had no pressing need to produce time-marking and place-holding deliverables, especially if those deliverables had to be written in a language that was not their native language. Moreover, members of the U.S. team, in creating an "ASTER centric" discussion of the ASTER data and information system, were opening issues for negotiation vis-à-vis Japan which ERSDAC might have otherwise assumed as its prerogative. Influencing the development of the ASTER remote-sensing system was, after all, what the U.S. team had been charged to do from the beginning by NASA Headquarters. The members of the Japan team were, rather, to serve as advisors to ERSDAC (and to JAROS, for issues that concerned the instrument), to act as a neutral party among ERSDAC's contractors, and to fulfill NASA's requirement for the participation of a "science team." As late as December 1992, a year before Geller's concept document was distributed, MITI had still preferred that team cooperation and science obligations be left to a more "flexible" agreement, rather than be included in the more "rigid" MoU that they felt was necessary for the instrument and data processing arrangements.[42]

---

[41] See, for example, Nichols (1994: 8) and Pniel (1994: 13).
[42] The quotes are the words of MITI's project manager for ASTER at the time. See Yokota (1991: 50), and see also Kahle (1992b: 13).

The U.S.-Japan Memorandum of Understanding and Clean Interfaces

While Geller's *End-to-End Data System Concept Document* raised many issues for discussion between the two teams as well as with other parties for the purpose of constructing the continuous and interoperable flow of the ASTER data and information system, the MoU that was under negotiation from 1992 to 1996 between the international office at NASA Headquarters and MITI emphasized "clean interfaces."[43] The MoU's writing of clean interfaces agreed with the symmetry and mirror imaging of the *ASTER End-to-End Data System Concept Document*. First, the form of the MoU highlighted the symmetry and mirror imaging of responsibility between the two states. For example, Article 3 listed the "Programmatic Responsibilities of NASA," and Article 4 listed the "Programmatic Responsibilities of MITI." Paragraph D of section 2 of Article 3 stated that NASA would use "its best efforts" to "designate a National Data Node in the United States to be responsible for international coordination, connection and transfer of data between the data systems of the Parties."[44] Likewise, paragraph D of section 2 of Article 4 stated that MITI would use "its best efforts" to "designate a National Data Node in Japan, the Earth Remote Sensing Data Analysis Center (ERSDAC), to be responsible for international coordination, connection and transfer of data between data systems of the Parties."[45]

The MoU was in this way generally symmetrical in both its organization and substance, but it was not completely symmetrical. For example, Japan was providing the instrument, and the United States was providing the satellite platform. The United States would be doing most of the level 0 processing (if the direct downlink was not

[43] Chapter two discusses a philosophy of "clean interfaces" as central to the NASA international office's management of international collaboration.
[44] Article 3, section 2, paragraph D in NASA and MITI (1996).
[45] Article 4, section 2, paragraph D in Ibid.

used), and Japan would be doing most of the level 1A processing, and these and other asymmetries would leave openings for the potential exercise of power. Yet, U.S.-Japan symmetry was a dominant organizing theme of the MoU as well as of the end-to-end document.

This organizing theme of symmetry in the discourse of these documents was also a principle that guided the design and development of the ASTER data and information system in practice. For example, when GSFC and ERSDAC met to discuss the system engineering of the ASTER data and information system, as they did regularly from 1993 onward, the GSFC regularly pushed for symmetry and mirror imaging in design. At one such meeting in 1993, GSFC's objective for the meeting was "to define (to the greatest extent possible) the system-level interfaces needed to ensure interoperability between EOSDIS and Japan's ASTER ground data system [i.e., Japan's data and information system for ASTER]."[46] In an internal talking points memo that was passed via e-mail around in preparation for the meeting, the GSFC's lead for the ASTER data and information system, Mathew Schwaller, who was also the lead U.S. negotiator for the data and information system's PIP, instructed participants to push for symmetry based upon NASA standards:

> At present, MITI is developing an RFP [i.e., Request For Proposals from contractors] for its ASTER GDS [i.e., Ground Data System]. In fact, a number of MITI's potential ASTER GDS contractors will be present at the meeting (see list). Thus, we have the opportunity to present EOSDIS design philosophy, architecture, functionality, standards, and operating procedures to a receptive audience at the earliest stages of their system design.
>
> In the effort to ensure compatibility between ASTER GDS and EOSDIS, I believe that we should use this meeting as to recommend and encourage MITI to adopt as much of the EOSDIS design as possible. Thus, I would like speakers to be explicit about defining EOSDIS standards, procedures and protocols, especially as they relate to the ASTER GDS. Please specify EOSDIS-wide standards, for example, in open systems (UNIX/POSIX),

[46] Schwaller (1993c).

> software generation (C, FORTRAN, use of the EOSDIS software toolkit, data distribution formats, media), network protocols, etc.[47]

In an e-mail to NASA Headquarters which alerted the EOS Project Scientists and others of his objective for the meeting, Schwaller was even more to the point: "<u>To the greatest extent possible, MITI's ASTER GDS shall be functionally identical to the ASTER GDS at EDC</u> [i.e., NASA's DAAC]. . . . MITI's ASTER GDS shall be as DAAC-like as possible" (emphasis in original).[48] In interviews, Schwaller and Glenn Iona, another key person for ASTER issues at the GSFC, both called my attention to how the architecture of the functional systems in Japan did indeed come to resemble that of EOSDIS, even down to the naming of the components of the system, which were named in English using the English alphabet (i.e., the Roman alphabet) before they were named in Japanese (in *katakana*) as loan words.[49] The architecture that ERSDAC adopted for their side of the ASTER data and information system was a departure from the data and information system that they had developed for their JERS-1 satellite in the early 1990s, even if not a radical one in terms of its functional organization.[50] The U.S.-Japan symmetry and mirror imaging—which often meant standardization—were central organizing themes of the design of the ASTER data and information system.

Unlike the end-to-end concept document, the MoU did not try to address the actual flows and interfaces between the United States and Japan, opting instead to leave those more technical questions for lower-level documents, such as the end-to-end concept document, the GSFC-ERSDAC PIP, interface requirement documents, and interface control documents. Thus, while the MoU certainly provided

---

[47] Schwaller (1993c).
[48] Schwaller (1993d).
[49] Schwaller (2001) and Iona (2001).
[50] This can be judged by comparing Sōritsu jūnenshi henshū iinkai (1993: 140-149) and ERDSIS in ASTER Science Team (1992a: 377) with Watanabe et al. (1995).

policy guidance on many issues (e.g., data exchange and access policy), it was silent on many significant issues as well, usually intentionally. For example, after examining figure 6.3 and reading its description in the end-to-end concept document, one might wonder if the data products distributed in the United States were going to be the same as the data products distributed in Japan. Would the United States and Japan distribute the same kinds of data products? And if the two states distributed the same kinds of data products, would their calculations need to agree? Or would the user get different scientific measurements depending upon whether or not the scene was processed in the United States or Japan? If the quality of the products were different, might then users prefer to order (and pay for) data products from one provider but not from the other?

The MoU was intentionally silent on the potentially consequential issue of higher-level data product standardization. The document explicitly endorsed symmetry and asserted clean interfaces, but it did not speak to international standardization (both NASA and MITI preferred international standardization, as long as they each could set the standard). So, rather than addressing the important details of the flows and interfaces, the MoU wrote symmetry and clean interfaces into the data and information system:

> [NASA would] make available to MITI the algorithm software (source code and documentation), compatible with EOSDIS standards, used by NASA to produce ASTER standard data products, according to a schedule defined in the jointly-developed Project Implementation Plan.[51]
>
> [MITI would] make available to NASA the algorithm software (source code and documentation), in accordance with EOSDIS standards, used by MITI to produce ASTER higher level standard data products, according to a schedule defined in the jointly-developed Project Implementation Plan.[52]

[51] Article 3, section 2, paragraph C(5) in NASA and MITI (1996).
[52] MoU, Article 4, section 2, paragraph C(5) in Ibid.

To be clear, the language of “standard” data products in the U.S.-Japan MoU *did not* mean products internationally standardized between the United States and Japan. A “standard” data product, as defined in the MoU, was a data product that was “generated as a part of a research investigation, of wide research utility, accepted by IWG [i.e., EOS’s Investigators Working Group, a high-level scientific advisory body to NASA] and the EOS Program Office [at NASA Headquarters], routinely produced, and in general, spatially and/or temporally extensive data products.”[53] Thus, when MITI signed the MoU, they signed on to the discursive constraint of being able to label a data product a “standard” data product only if NASA Headquarters and its EOS advisory bodies approved that product as a “standard” data product. Standard data products *were not* stipulated in the MoU as data products that are standardized between the United States and Japan, if corresponding products were produced by each state’s DAAC. Thus, according to the MoU, there could be U.S. “standard” products and Japan “standard” products, and these products would not necessarily be standardized with each other. Furthermore, if MITI or ERSDAC wanted to develop a data product and not run it through the NASA approval process to receive the “standard” label, then they could call that data product a “semi-standard” or “specialized” data product.

This silence in the MoU about standardization was not an oversight. The lead negotiator of the MoU for NASA Headquarters recalled that:

> Certainly NASA HQ did not REQUIRE the science team to standardize level 2 algorithms and products—that [would be] the science team's decision. My recollection is that the US side thought it could do a better job [on the data products] but wanted to be able to learn what the Japanese were doing as well in hopes of achieving a standard product [i.e., standardized internationally]. We would not have required [international] standardization

[53] Appendix 2 in Ibid.

> for fear of being "stuck" with a product the US community might not have been satisfied with.[54] (emphasis in original)

Thus, while the MoU, and to a lesser extent the GSFC-ERSDAC PIP that was negotiated between 1993 and 1997, mapped out general responsibilities for the United States and Japan in a symmetrical way and asserted clean interfaces for data sharing and exchange, it was left to the technoscientific diplomacy of the U.S. and Japan teams, as well as to GSFC and ERSDAC personnel and their contractors, to specify and build symmetry and interoperability into the U.S.-Japan relations of their ASTER-centric, end-to-end data and information system.

Configuring an International Political Economy

As the U.S. and Japan science teams, their respective centers (i.e., GSFC and ERSDAC), and those centers' contractors went about designing and building ASTER's data and information system, they also were at the same time bringing into existence an international order. In particular, they were configuring an international political economy. I use here the word "configuring" instead of "designing" for two reasons. First, "configure" can be used as a synonym for "design." In some ways the U.S. and Japan teams and their sponsors were intentionally and self-consciously designing an international political economy. Yet, second, an international political economy was not the system that the two teams saw themselves as designing during most of their work.

The word "configure" suggests an action that is less intimate, less total, and less fixed than "design," that is, a more provisional arranging of components and an adjusting of settings, perhaps so that someone can get on with what he or she really

[54] Shaffer (2006).

wants to do. Think of when the settings of a computer system or of a new software program such as an internet browser are configured and re-configured so that it can be interacted with and used as one wishes. Typical users are only occasionally interested in their computer's configuration, usually when it is causing them problems. That is not the stereotypical attitude of the engineer who "designed" the computer system. When users do become interested, it is not unusual for them to be intensely interested. That, I argue, captures how most of the members of the two teams and their colleagues were interacting with the international political economy that was emerging with the data and information system that they were designing. As they developed their ASTER data and information system, they confronted and solved problems of international order, and in particular, problems that concerned the international political economy of ASTER's scientific data. When they designed and built their data and information system, they configured that international political economy.

The term "international political economy" is not one that members of the U.S. or Japan team themselves used to describe what they were either designing or configuring. They did use, however, the terms "production" and "consumption" throughout their discussions of the ASTER data and information system.[55] Moreover, "data products" were often at the center of their attention. Despite the two teams' lack of use of the term "international political economy," they were configuring organized interactions of consumption, production, and distribution around scarcity, organized interactions that were among states, their researchers, and hundreds, if not thousands, of other consumers of ASTER's remote-sensing data.[56]

The two teams not only configured these interactions. As liminal state actors, they enacted an international political economy in both senses of the word "enact":

---

[55] For example, Tsu and Kahle (1997: 10); Okada (2003b: slide 37).
[56] Gilpin (2001) adopts a similar definition for "global" political economy (e.g., p. 17-18).

"establish" as well as "acted out." The configuration and enactment of this international political economy was a political as well as a technical achievement. Here is a brief outline of the order of this international political economy:[57]

> Scarcity: The number of ASTER's observations was limited. For each pass from north to south in its polar orbit around the earth, ASTER could acquire data for only about eight percent of the time. Furthermore, ASTER had three telescopes that could be operated independently, one each for the visible, shortwave infrared, and thermal regions. Each of these telescopes could point at an angle of differing degrees. Owing, however, to the design specifications for the ASTER instrument for "wear and tear" and to other operational specifications, some of which the previous chapter described, the two teams regarded the number of times that these telescopes could be pointed to be limited over a given orbit as well as over the instrument's life. So, not only were the number of ASTER's observations limited, but so were the number of times that it could point.
>
> Consumption: The consumers of ASTER's imagery could order data from either NASA's DAAC in the United States or from ERSDAC's DAAC in Japan (see figure 6.3). Many issues revolved around consumption: who would be ASTER's authorized users? (only researchers? what about industry?); how would these users go about ordering data? (could users in United States and in Japan order data from either NASA or ERSDAC?); how much data could users request? (could team members request more data than other users?); and what price would be charged? (would NASA and ERSDAC charge the same price?). All of these issues were negotiated through

[57] Morrison (1993); Geller (1994); Nichols et al. (1995); Watanabe et al. (1995); Yamaguchi et al. (1999); and Satō et al. (1999).

technoscientific diplomacy in some way, even after NASA and international EOS participants signed a high-level data exchange policy in 1996.

Production: According to the *ASTER End-to-End Data System Concept Document*, once a user requested data, either ERSDAC's DAAC or NASA's DAAC would handle the request. If the requested data were not already archived by those systems from previous observations, then the data and information system would need to generate a data acquisition request (i.e., a DAR). Because the ASTER team anticipated that users' requests would conflict, the requests would need to be evaluated and compared with each other before the instrument's observation schedule for a given day could be determined. The scheduling process was expected to be a massive daily calculation that would take into account, among other things, constructs of users' categories, the requirements of thousands of requests for data, and real-time operational conditions, such as what operation the instrument last performed and where clouds were predicted to be that day. After the requested scene was observed by the instrument, the imagery data would need to be processed. Which DAAC would do that processing? Would the types of higher-level data products offered by the two DAACs be similar? When both DAACs offered the same type of data product, would the data that they produced be identical? How much processing would be done by each DAAC?

Distribution: Finally, depending upon what scenes were requested and what type of processing had been requested for those scenes, the imagery data might need to be passed between NASA's and ERSDAC's DAACs for different kinds of processing before the data product was distributed to the user who had requested it. Would the two DAACs be able to do this sharing? That is, would the two DAACs be interoperable? The *End-to-End Data*

System Concept Document implied that they would, especially the draft version in 1993, but the MoU that was eventually signed in 1996 was silent about the particulars of the international flows. The MoU just connoted that the interfaces would be "clean interfaces."

A Partnership in Production

This section examines the two teams' technoscientific diplomacy of designing and developing symmetry, interoperability, and international standardization into two "production" flows: the production of the level 1A and 1B data products and the production of two closely-intertwined higher-level data products, namely the temperature and emissivity of the earth's surface, both of which are level 2 data products. It argues that the practice of technoscientific diplomacy in the design and development of these two production flows illustrates three of this chapter's contentions. First, the transnational authority of the ASTER team did in practice extend via technoscientific diplomacy from the two teams' negotiations over ASTER's operational design into the design of the ASTER data and information system writ large, as the "ASTER centric" *End-to-End Data System Concept Document* envisioned. The moves and strengths of the two states' "centers of calculation," ERSDAC and GSFC, explain neither the epistemic core of the production of the data products (e.g., their algorithms) nor the architecture of the ASTER data and information system, as Latourian actor-network theory implies that they should. The two teams' technoscientific diplomacy better accounts for the epistemic core and architecture of the ASTER data and information system, especially for the ways in which the ASTER data and information system extended outward from the two teams' negotiations rather than inward from the two states' centers of calculation.

Second, in contrast to the dynamics of epistemic communities which are assumed by the epistemic communities approach, the practice of the two teams' technoscientific diplomacy pragmatically, and sometimes strategically, took into account each state's political-economic power relative to the other state. Power and politics were (unavoidably) part of enacting a system for producing scientific data. Moreover, it is difficult to argue that the arrangements for these production flows and data products (as well as for other data products, I would argue) can be explained by a process in which members of a transnational epistemic community relayed consensus views to decisionmakers who then did the consequential negotiating. Rather, the designs of these two production flows were shaped in crucial ways by the two teams' technoscientific diplomacy that involved the assertion and ascription of state power as well as the consideration of scientific and technical claims.

Third, the two teams, and especially the U.S. team, advanced in their technoscientific diplomacy a normative notion of U.S.-Japan partnership, initially at least for strategic reasons. Nevertheless, they continued to enact it as a prescriptive principle for their collaboration when the professional interests of members of each team and the national interests of each team's state no longer presented compelling reasons for advancing such a normative notion.

The Level 1 Compromise: Product and Process

MITI's "baseline" configuration in 1991 for the ASTER level 1 data production flows stated that "*all* level 1 ASTER data" (emphasis mine) would be generated and archived in Japan and that "level 1 data" would be transferred to the United States. Note in that second quote the absence of an "all." The kind and quantity of level 1 data that would be transferred to the United States went unspecified in

MITI's baseline configuration.[58] The "all" in the first quote, and its absence in the second, were potentially very significant. The "all" in the first quote could mean a daily volume of as many as 780 scenes for level 1A and then another 780 scenes for level 1B—with each scene being sixty kilometers square, comprising imagery from all three sensors and their fourteen spectral bands, with a resolution that varied according to the particular ASTER sensor (fifteen meters for the visible sensor's three spectral bands, thirty meters for the shortwave infrared sensor's six spectral bands, and ninety meters for the thermal sensor's five spectral bands). That volume would amount to about eighty gigabytes of data to process each day for level 1A alone. The absence of the "all" in the second quote meant that MITI had not yet agreed to transfer to the United States any or all of the computationally-intensive, hardware-dependent, geometrically and radiometrically corrected level 1B data.

Taking on the primary responsibility for level 1 data production could bring both costs and benefits for MITI and ERSDAC, costs and benefits that were understood in their gist by those involved in the ASTER collaboration in the United States and Japan.[59] Although it is not unusual in 2006 for eighty gigabytes of data to fit onto the hard drive of a personal computer, in the mid-1990s to store that amount of data on a daily basis and run it through computationally complex processes each day required expensive parallel-processing machines and storage systems, with costs totaling well into the millions of dollars for the equipment and maintenance. In a 1998 article, ERSDAC described the volume of ASTER level 1 data to be processed each day as "huge" and as a major engineering challenge.[60] Any increase (or decrease) in

[58] MITI (1991: 10-3). When MITI originally proposed to NASA in 1987 to provide an instrument for EOS, they had also proposed to produce level 1A and 1B data. Producing level 1A and 1B data was an up-front requirement for every EOS instrument and its provider (NASA 1988b: 19).
[59] For example, the following interviews: Abrams (2003), Eng (2001, 2003), Iona (2001), Fujisada (2003), Schwaller (2001), Watanabe (2003), and Yamaguchi (2002).
[60] Watanabe et al. (1998: 45).

processing requirements would lead to an increase (or decrease) in the funding that was required.

Yet, some of the general benefits for MITI and ERSDAC of producing this level 1 data, while less tangible than the costs, were also widely agreed upon on both sides of the Pacific.[61] First, everyone involved thought it made good sense for the state that had built and provided the instrument to hold the primary responsibility of generating level 1 data, especially since the task of correcting the data required detailed pre-flight and in-orbit characterization of the sensitivity of the instrument's sensors. Unless exceptionally elaborate measures were taken well in advance of launch, the sensors' makers would need to assist in this characterization. Second, for MITI, ERSDAC, the corporate members of ERSDAC, and the Japan team, producing level 1 data would ensure that they could access and use ASTER data relatively unencumbered by the United States. Third, MITI and ERSDAC hoped to recoup some of their costs by selling ASTER data, not only to their domestic industry, but to everyone to whom they could sell it, worldwide. To effectively sell ASTER level 1 imagery, ERSDAC reasonably judged that it needed to be the primary provider of that imagery. I know of no one who disagreed with that business judgment. To what degree ERSDAC needed to be the exclusive provider of level 1 imagery to sell it effectively was up for debate. And that is where ERSDAC's level 1 production plans, particularly the question of what data would be transferred to the U.S. DAAC—which went unspecified in MITI's baseline configuration—intertwined with NASA's preferred data exchange policy and the plans of the U.S. team for producing level 1 data products.

NASA Headquarters, from the beginning of EOS planning, pushed for EOS data to be open for wide-spread research use at *no more than* the marginal cost of

[61] Again, the following interviews: Abrams (2003), Eng (2003), Iona (2001), Fujisada (2003), Schwaller (2001), Watanabe (2003), and Yamaguchi (2002).

producing and delivering the data product (my emphasis). Team members and data centers would have no period in which they would have exclusive access to the data. All "research users" who were authorized as such by states participating in EOS would have open and equal access to all archived data, according to the data exchange policy that NASA planned. A draft of this policy was circulated and discussed among members of the U.S. and Japan ASTER teams as early as their first joint meeting in 1990.[62] Policy for commercial users and for non-research users residing outside the states that were participating in EOS (namely, outside Canada, Europe, Japan, and the United States) went unsettled, however, throughout much of the 1990s. It was largely left in the hands of each state to decide in accordance with that state's national laws and policy.

In the United States, the Land Remote Sensing Policy Act of 1992 directed that all users, including commercial users, be charged "the cost of fulfilling user requests" (i.e., the data product's marginal cost) for the level 1 data that were to be received from the next Landsat satellite, which would be launched in the late 1990s around the same time as ASTER.[63] In addition, the Act declared that the "development of the remote sensing market and the provision of commercial value-added services based on remote sensing data should remain exclusively the function of the private sector."[64] While certainly judged as relevant to ASTER by key members of the U.S. and Japan teams, what these two statements meant precisely for NASA's preferred policy regarding the distribution of ASTER data to commercial

[62] ASTER Science Team (1990: 1, 15-16); IEOS Data Exchange Principles in NASA and MITI (1996).

[63] According to section 5602, paragraph 2, of the Act, "the term 'cost of fulfilling user requests' means the incremental costs associated with providing product generation, reproduction, and distribution of unenhanced data in response to user requests and shall not include any acquisition, amortization, or depreciation of capital assets originally paid for by the United States Government or other costs not specifically attributable to fulfilling user requests." Land Remote Sensing Policy Act of 1992, Pub. L. 102-555, at 15 U.S.C., section 5601 et seq.

[64] Land Remote Sensing Policy Act of 1992, Pub. L. 102-555, section 5601, paragraph 15.

users was not clear to Japan in the early 1990s.[65] By the time the MoU was signed in 1996, however, ERSDAC and members of the Japan team had read the Act as well as the language about marginal cost in the ASTER MoU and had expected that NASA's pricing policy for users who were not sponsored by, or affiliated with, the EOS programs of member states—including research users as well as commercial users—would be charged the marginal cost of the data product.[66] They expected that many EOS and other government-funded users, but no one else, might receive from NASA their ASTER data free of charge, taking advantage of the explicit "no more than" marginal cost wording that was eventually included in the MoU.

Given the goals of the EOS program and the U.S. Congress's emphasis as expressed in the Land Remote Sensing Policy Act on distributing level 1 data inexpensively and widely to foster a value-added services industry for remote sensing data, NASA and its DAACs had little or no reason to try to become the exclusive provider of ASTER level 1 imagery. NASA was not operating on the quasi-business model that the ERSDAC consortia was.[67] In fact, NASA's baseline configuration had been that ERSDAC would not only process all of the level 1A and 1B data but, in contradistinction to MITI's baseline, would also make all of data available for shipping to the U.S. DAAC.[68] What NASA wanted to ensure was that the U.S. DAAC would contain as much quality level 1 data product as was possible for the least possible cost, for the purpose of archiving it for the record and for distributing it promptly to EOS researchers. For NASA, the answer to the question of where that

---

65 Abrams (2001, 2003) and Yamaguchi (2002). As late as February 1993, NASA Headquarters and MITI had not discussed pricing policy with respect to commercial users (Shaffer 1993a).
66 Watanabe (2003) and Yamaguchi (2002).
67 Interviews: Abrams (2001), Iona (2001), Pneil (2001), and Schwaller (2001). As early as 1993, NASA and GSFC did discuss the possibility of charging for ASTER data, at no more than the cost of fulfilling the user's request. See, for example, Schwaller (1993a) and Shaffer (1993a). Throughout the 1990s, EOSDIS and its DAAC for ASTER neither designed nor implemented a billing and accounting system for charging for ASTER imagery. See, in particular, Iona (2001).
68 Geller (1993a: DD3).

level 1A and 1B imagery should be produced should be "based primarily on cost effectiveness."[69] In the spring of 1992, Daniel Goldin had become the new NASA Administrator under the slogan of "faster, better, cheaper;" NASA's EOS program was entering its "descoping" phase as a result of Congressional budget reductions; and the budgets for GSFC's EOSDIS, which included the U.S. DAAC for ASTER, were beginning to tighten. NASA and GSFC did not want to pay for more processing capacity than it really needed.

In light of these expressed preferences of MITI/ERSDAC and NASA/GSFC, everyone expected that ERSDAC could be counted on to produce at least all of the level 1A imagery (i.e., at a maximum of 780 scenes a day).[70] The questions in the early 1990s which concerned Japan's production activities were: whether or not ERSDAC was going to produce routinely the computationally-intensive, corrected level 1B data product (ERSDAC could, alternatively, produce level 1B "on-demand" as it was ordered); how much of the level 1B data would ERSDAC produce, either routinely or on-demand; and what kind of data and how much data would ERSDAC pass to the U.S. DAAC. The questions for the U.S. side in the early 1990s were: whether or not the U.S. DAAC would produce the level 1A and/or the corrected level 1B data (for the latter data, the U.S. DAAC would need to receive regularly from Japan information that was required to characterize the changing sensitivity of the instrument's sensors); if the U.S. DAAC were going to produce level 1A and/or 1B data, would the U.S. produce it routinely or as needed (i.e., "on-demand" when an order came in); and whether or not the U.S. DAAC would rely upon ERSDAC for any

---

[69] Geller (1992: N6).

[70] Japan's team leader announced Japan's intention to process all level 1A imagery at the first joint ASTER team meeting in November 1990 (ASTER Science Team 1990: 8). That level 1A processing was also included in MITI's "baseline" execution plan in 1991 (MITI 1991: 10-3).

data, and if so, for how much data. Each side's answers to these questions needed to be compatible with the other side's answers, if any data were going to be produced.[71]

It was left to the U.S. and Japan teams to sort out many of these crucial issues and to configure U.S.-Japan data production in the international political economy of ASTER data. The U.S. team in particular weighed in heavily on whether or not the United States should rely upon ERSDAC to produce quality level 1A and/or 1B data products. If the United States could not, the United States would need to produce level 1A and/or 1B data products, potentially threatening the realization of MITI's business and market-building ambitions, not to mention throwing a blow to the scientific credibility of ERSDAC's data products. The Japan team and ERSDAC needed to decide just what level 1 data it was willing to share with the United States (the corrected 1B or only 1A?) and how it was going to do so. Provided the United States deemed Japan's level 1 production reliable, the more data Japan shared with the United States, the less the United States would seem to need to produce level 1 data on

[71] Some kind of techno-political interdependency existed between the United States and Japan in every hypothetical configuration for data production. Take the highly-improbable and extreme "go it alone" configuration. The United States could not completely go it alone in data production. For geometric and radiometric correction, the U.S. DAAC required periodic information regarding the changing sensitivity of the sensors. Unless extraordinary measures were taken well in advance, only Japan could provide this information, since its contractors had physically built and characterized the instrument. In the extreme "go it alone" configuration, Japan would not provide that crucial information to the United States if the United States produced data of a kind and of a volume that clearly relegated Japan to being a secondary provider of the staple data products of the ASTER instrument, an instrument that the Government of Japan funded. On the other hand, Japan could not go it alone (and not share data with the United States), because—if push came to shove—the United States had the option of not sending the level 0 data to Japan. Only the U.S. received the level 0 data that were transmitted from the instrument by the NASA host satellite. U.S.-Japan interdependency becomes more complex as more cooperation is planned (for the purpose of realizing, for example, cost-savings through data sharing). Given the political and technical considerations that have been described so far in the chapter, the reader can construct many other configurations of interdependency for varying degrees of cooperation.

its own, unless it turned out that it was simply cheaper to process it in the United States than to ship it from Japan.[72]

The U.S. team in the fall of 1992 leaned toward producing in the United States some portion of, if not all of, ASTER's level 1 data. At a U.S. team meeting in November 1992, U.S. team members expressed three primary concerns about relying upon Japan for level 1 data. First, they were concerned that the coolers for the shortwave infrared and thermal sensors might cause the ASTER instrument to shake and vibrate to such an extent that the "jitter" would harm the geometric accuracy and precision of the instrument's observations.[73] If jitter were a real possibility—and the issue was being investigated by JAROS and GSFC instrument engineers and contractors—then the geometric correction in the level 1 processing would need to be well enough understood and flexible enough to allow some changes after the instrument was in orbit, if not before. U.S. team members were skeptical that ERSDAC would be that flexible in accommodating changes.[74] Second, in the opinion of the U.S. lead for geometric correction, Hugh Kieffer, "the influence of [the] U.S. side on level 1 processing is unknown."[75] Kieffer was particularly concerned about the designs of the level 1A and 1B data products that Japan would supposedly transfer to the United States (under NASA's baseline configuration, at least). Neither of the designs allowed the United States to reverse engineer the product to understand Japan's geometric corrections. In addition, the products' designs did not enable the United States (or anyone else) to undo the geometric correction for the purpose of

[72] The rate of the shipping and data transfer rate also needed to be considered—how fast a given volume of data could make the trip between Japan and the United States. Since the issue was not a dominant concern, I have omitted it in the above text for the sake of simplification and readability.
[73] "Jitter" was technically defined as "the short term variation of the attitude [of the satellite] around the actual position" (Scholz 1990: 156).
[74] U.S. ASTER Science Team (1992: 10, 13) and Lambros (1992b).
[75] Kieffer (1992: DD2).

producing higher-level data products of better quality or for troubleshooting.[76] Kieffer judged that "the U.S. will have little visibility into the Level 1 processing if it is done exclusively in Japan," and that "the fundamental impact is that any flaws, etc., introduced in the Level 1 processing by the Japanese will be there forever in all subsequent data products."[77] Third, it did not seem that Japan would be incorporating "ground control points" (i.e., "ground truth" anchor points for map projections) into their level 1 processing for the purpose of locating their scenes on the earth. The lack of ground control points was especially a concern for producing digital elevation maps, a widely-anticipated data product for the ASTER instrument. One of U.S. team's experts in producing digital elevation maps stated that, in contrast to what the Japan team had suggested, "there are no magic systems out there that do not require ground controls – period."[78] If the United States were to integrate ground control points into the ASTER production flow to create a digital elevation map, it would be best to do it in level 1 processing, assuming the U.S. team had "visibility" into that level 1 processing.

The writer of the meeting minutes commented that "the magnitude of the job of developing level 1 processing capability was a surprise to most of the attendees. Also the importance of having a U.S. capability was brought home."[79] The team agreed to push GSFC and NASA Headquarters to ensure that the United States would have all the level 1 software source code, correction information, and other hardware-dependent information that were required to have a level 1 processing capability in the United States. GSFC personnel were in attendance at that U.S. team

[76] The particular level 1 processing steps to which I am referring here are the re-sampling required for the inter-telescope registration and the application of geometric calibration coefficients to the data. Both were thought to be "irreversible" transformations (Ibid., p. DD1).
[77] U.S. ASTER Science Team (1992: 13).
[78] Ibid., p. 18.
[79] Ibid., p. 20.

meeting, and so was NASA Headquarters' point person for ASTER.[80] Two months later, in true bottom-up fashion, the U.S. team's preference for having a level 1 processing capability in the United States was passed on from GSFC to NASA Headquarters, in preparation for the U.S.-Japan negotiations over the MoU.[81] NASA Headquarters had not expressed a specific position on this issue prior to hearing of the U.S. team's desire to have a level 1 processing capability in the United States. Either NASA Headquarters was quickly convinced by the U.S. team, or alternatively, NASA Headquarters needed no convincing.

Within several months of this meeting there was no doubt among the U.S. team and other U.S. participants in the ASTER collaboration that having the capability to produce level 1 data in the United States was a top negotiating priority of the United States. In a worst-case catastrophe, the United States would want to be able to produce level 1 data independently. Reportedly, a prominent individual in EOS at NASA Headquarters declared that "we want to be able to produce ASTER data even if we go to war with Japan."[82] More often in my interviews, rationales for national autonomy would reference a different kind of catastrophe: instead of war breaking out, a devastating earthquake hits Japan. Having the capability to do something, however, is much different in its consequences than actually doing it.

It is in that difference that the United States and Japan were ultimately able to negotiate a compromise that did not threaten MITI's and ERSDAC's primacy in level 1 production but that still placed the U.S. team in a negotiating position that would help them to ensure the quality of ASTER's level 1 data. Technoscientific diplomacy

---

[80] Ibid., p. 5.
[81] Schwaller (1993b).
[82] Although three U.S. scientists and engineers, speaking separately, relayed this quote to me and told me the name of the individual who offered these words as guidance, since those scientists and engineers do not wish to go on the record, I do not think it is appropriate for me to name the speaker based upon what would be just anonymous hearsay, even if it is multiply-sourced hearsay.

between the two teams which synthesized the production of knowledge about the earth with assertions and ascriptions of each state's political-economic power was integral to achieving this compromise. This diplomacy proceeded in roughly four moves: the U.S. team's hedge toward national autonomy; Japan's proposal for a "limited partnership" that redefined the level 1 data products in a critical way; the U.S.'s counter-proposal that stuck with the U.S. team's hedge; and the grand compromise.

First, the U.S. side started to hedge what they judged to be the U.S.'s potential risk. In the fall of 1992 and winter of 1993, the U.S. team investigated the scientific, economic, and political advantages and disadvantages of various proposals for regularly producing different elements of level 1 data in the United States, although they realized that NASA's "formal position" was that Japan should produce and provide all the level 1 data, in line with NASA's cost-conscious baseline configuration.[83] They began to conduct what was known in systems engineering circles as a "trade study" that examined the "trade offs" of various level 1 processing configurations. With the approval of JPL management, they also sent GSFC a rough cost estimate of the software code required for both a prototype and production version of level 1 processing in the United States.[84] At the same time, the U.S. team requested that GSFC and NASA Headquarters make it a negotiating priority to receive from Japan the level 1 source code, correction information, hardware data, and other documentation required to have a level 1 processing capability at the U.S. DAAC. The U.S. team also decided to mitigate some of their "risk" (their language) by becoming more closely involved in the design of the level 1 processing algorithm and by "establishing and maintaining close communications" with those team members in Japan who would be doing the level 1 design work.[85]

[83] Kieffer (1992: DD1).
[84] Nichols (1993a).
[85] Voge (1992: 9) and Kieffer (1992: DD3).

Japan then made the second move. In February 1993, at the next U.S.-Japan ASTER team meeting (which was their fifth), Japan proposed a “limited partnership” for level 1 processing.[86] It is not clear to me what Japan did or did not know at this point about the U.S. team’s efforts in exploring the development of an independent level 1 processing capability in the United States, but at the opening plenary session, both team leaders listed the level 1 data processing at the top of their issues to be discussed at the meeting.[87] Because level 1 processing issues mixed “science” questions such as georeferencing, geometric correction, and radiometric correction with “engineering” questions such as level 1 data flows and the design of the architecture of the ASTER data and information system, it was awkward for the Japan team and ERSDAC to manage the design of level 1 processing. ERSDAC and the Japan team leader (who worked at both the U.S. Geological Survey of Japan and ERSDAC) preferred to draw a clear boundary between the responsibilities of Japan’s science advisors and ERSDAC (in a similar way to the science/engineering boundary that was asserted by the Japan team in the negotiations over the shortwave infrared bands discussed in the previous chapter).[88]

Owing to this concern of mixing but also for the purpose of focusing the efforts of the joint team on level 1 data production, Japan’s team leader, Tsu, proposed at the joint meeting a “restructuring” of the working groups in order to create a “Level 1 Data Products Working Group.” This new structure, which likely received the assent of the U.S. team leader, Anne Kahle, in advance of the meeting, was intended by the Japan side to ensure the “engineering” purity of the working group that handled the

[86] Geller (1993a: DD-5). Whether the Japan team or the U.S. team first described the Japan team’s proposal as a “limited partnership” is also unclear, but that was what it came to be called. Given how it is quoted in the document cited in this note, I suspect that the description initially came from the Japan team.
[87] ASTER Science Team (1993b: 4-5).
[88] See also Tsu’s comments in (Ground Data System Working Group 1993: PV1).

engineering of the architecture of the ASTER data and information, which was called the ground data system working group.[89] On the U.S. side, however, the mixing of science and engineering work and responsibilities was less of a concern, and the people who attended those two working groups overlapped more than they did for the Japan side. For example, it was common for the system engineers and software developers from the science project at JPL, such as Gary Geller, as well as Anne Kahle and U.S. team member Hugh Kieffer, to attend both groups—the newly formed, more "science" level 1 data products working group and the more "engineering" ground data system working group. The level 1 issues discussed in these two groups sometimes overlapped and were often closely connected. Although Japan's team leader and ERSDAC preferred a clear boundary between science and engineering work, because those distinctions had implications for their institutional hierarchy, the U.S. team did not leave the international flows for level 1 data production to ERSDAC to work out on their own with GSFC.[90] The U.S. team could only vouch for quality of the level 1 data product if they had a role in its design.

The Japan side's proposal at this joint team meeting for a "limited partnership" in level 1 data production addressed the concerns of both the U.S. and Japan sides. In line with MITI's original baseline configuration, the Japan side

---

[89] ASTER Science Team (1993b: 5). See also Tsu's comments in the "Minutes of the Level 1 Data Product Working Group" in ASTER Science Team (1993b) and Ground Data System Working Group (1993: PV-1). It was typical for the agendas of the joint meetings and working groups to be approved in advance by, respectively, the two team leaders and the two chairs of each working group.

[90] See, for example, the minutes of the Level 1 Data Products Working Group and of the Ground Data System Working Group (1993: PV-1) in ASTER Science Team (1993b), and in particular, Arai's, Kieffer's, and Geller's questions. When, at the request of ERSDAC, ERSDAC and GSFC formed after the fifth joint team meeting their own ground data system working group to handle engineering issues, which was a working group that Japan (but not the United States) considered to be at a "higher level" than the joint science team, the U.S. science team was "plugged into" that working group. That working group was called the "greater" (i.e., broader) ground data system working group. See the comments of Lambros in Ground Data System Working Group (1993: PV-1).

proposed to produce all of the level 1A data in Japan at ERSDAC. It also proposed to make available *all* level 1A data to EOSDIS for NASA's pick up, as had been expected by NASA's baseline configuration. The critical compromise, however, was that the Japan team also redefined the level 1A data product and the basic structure of the algorithm that would produce it. The file for the level 1A data product would now include *all* of the geometric and radiometric correction information that was needed for the scene to be corrected in future processing to level 1B, assuming that that processing used the appropriate software (the horizontal flow labeled "Level 1A Coefficients" in figure 6.3 reflected this new commitment). While all of the geometric and radiometric correction information would now be included in the 1A data file, it would not be applied to the imagery scene. The imagery scene would still correspond to the instrument's uncorrected electronic response.

That separation between the correction information and the uncorrected scene was the critical move that helped to bring, to use Kieffer's word, "visibility" to the level 1 data products. The U.S. team was reassured by this redesign of the level 1A data product because the separate provision of both the correction information and the instrument's response enabled the U.S. team to tweak and troubleshoot more easily the imagery data (because the raw measurements and the corrections could be independently "seen") and to produce better higher-level data products by going back to the least processed data that were still formatted as a scene for easy manipulation.[91] The provision of scene-specific correction information also allowed NASA to avoid the computationally-intensive and costly step of producing that information, but it still kept ERSDAC in control of the level 1 processing pipeline. Finally, by separating out the correction information, the Japan side encouraged a separation between the work

[91] Geller (1993a: DD-5). For the 1990-1992 working definition of level 1A, see Iwashita (1990: 182) and Bothwell (1990: 198).

of "science" (attending to how that correction information would be calculated and adjusted) and the work of "engineering" and "project management" (determining the volume of the 1A and 1B data that would be provided to the United States and the particulars of the data formats and software interfaces).

Japan's "limited partnership" configuration was not completely satisfactory for everyone, of course. To stock the U.S. DAAC with level 1B data, Japan proposed that the United States carry out the production from level 1A to 1B using Japan-supplied software.[92] This processing amounted to the application of the correction information embedded in the level 1A data file to the imagery scene included in the level 1A data file. While that step was certainly less demanding of processing resources than the initial calculation of the correction information, that processing would still demand resources that NASA Headquarters had not committed in its baseline configuration. Yet, MITI and ERSDAC would be giving the U.S. DAAC the go ahead to independently produce level 1B data from their level 1A data products. The details of how this production would be coordinated with ERSDAC were to be determined. Finally, the U.S. team (or other users) would still not have the independent capability to completely reverse engineer the correction calculations unless the U.S. DAAC also received all the level 1 algorithms, source code, correction information, and other necessary hardware-dependent information.

The third move in the sequence of the technoscientific diplomacy over level 1 processing was NASA's counter-proposal that would try to secure the U.S. team's hedge and then ask for more. The U.S. team assessed the Japan side's February proposal for a "limited partnership." Then, in an "April Fool's Meeting" with NASA Headquarters and EOSDIS managers at GSFC, the U.S. team recommended that the United States accept Japan's proposal, provided that a level 1 processing "testbed" was

[92] Geller (1993a: DD-4).

developed for the U.S. DAAC in which the United States—and specifically the U.S. team—would have the capability for complete level 1 testing and processing. NASA Headquarters "agreed that a U.S. L1 [level 1] testbed was a good idea."[93] A "testbed" at the U.S. DAAC meant that Japan would need to provide the software code and hardware-dependent information that was required for the United States to have an independent level 1 processing capability. That effectively was the hedge that the U.S. team had recommended earlier to NASA Headquarters (under the Japan proposal, the United States would now receive the correction coefficients in the level 1A data product). Although the language in the MoU had yet to be worked out, by July 1993 the "basic concept of [the] Japanese proposal seem[ed] to be informally accepted" by NASA Headquarters.[94] The catch, of course, was that Japan had yet to agree to turn over the software code and other hardware-dependent information required for the United States to have an independent level 1 processing capability. That item had not been part of Japan's initial "limited partnership" proposal.

In addition to the independent level 1 processing capability in the United States, NASA Headquarters wanted a little more from Japan. In line with their baseline configuration, NASA wanted Japan to make available to the U.S. DAAC at least some of the level 1B data products that ERSDAC would produce, provided that the United States decided not to produce the level 1B data products at the U.S. DAAC because of reasons of expense. With respect to the provision of level 1B data, NASA wanted then to have its cake and to eat it too. They wanted to have the option of independently producing level 1B data, but they wanted to receive the data from Japan if that option was less expensive than independent production. Under NASA's counter-proposal, Japan would—on top of providing the U.S. with an independent processing

[93] Ibid., p. DD-8.
[94] Ibid., p. DD-9.

capability—effectively be on the hook for providing level 1B data, but without pipeline control. The Japan side reported in the summer of 1993 that it was a "very big problem," for them to provide the level 1 processing source code because the source code would be the property of ERSDAC's contractor and not the property of the Government of Japan.[95] Japan supposedly could not turn over the source code even if it wanted.

The grand level 1 compromise in the fall of 1993 negotiated the tension between the U.S. side's desire for an independent level 1 processing capability and the Japan side's desire for primacy in level 1 processing, with financial expense also in mind for both parties. NASA did secure an independent level 1 processing capability for the United States, as the U.S. team had wanted. Yet, NASA informally assured Japan that that capability would likely be used only for testing and not for general production, thus preserving Japan's primacy in the international political economy of ASTER level 1 processing. After all, NASA from the beginning did not want to pay for the capacity required to independently produce level 1 data products on a daily basis. The Government of Japan bought a license to their contractor's level 1 source code, so it could provide NASA with the source code required for the U.S.'s independent level 1 processing capability. In return, NASA kicked in the capability for its satellite to direct downlink ASTER's data to Japan's ground receiving stations (see "DDL" in figure 6.3).[96] Japan would provide level 1B data to NASA, but only "in response to NASA's requests," in the wording of the MoU, at a volume that would be determined in future negotiations over the PIP (this volume was decided to be "limited to" 310 scenes per day, according to the 1997 PIP, which is about roughly forty

95 Nichols (1993b).
96 Article 4, section 2, paragraph A in the NASA and MITI (1996); Shaffer (1993b); Miyazaki and Schwaller (1993); Miyazaki (2003); and Schwaller (2001).

percent of the level 1A scenes that were expected to be produced).[97] Because Japan would be providing the level 1B data "in response to NASA's requests," Japan would still be in control of the level 1 production flows, and the level 1 production flows at least would be internationally standardized.

Crucially, however, the hedge that the U.S. team had initially insisted upon would allow the United States to ensure that Japan's control of the level 1 production flows, such as its provision of level 1A and 1B data products to the U.S. DAAC, went as expected. Moreover, the U.S.'s "full capability to produce level 1 data products" provided "a framework to interact with Japan during [further] level 1 algorithm and code development," according to the assessment of a U.S. level 1 working group meeting.[98] In other words, the U.S. team hoped that they could use their hedge to exercise power in future technoscientific diplomacy concerning level 1 processing and production.

Although the redefinition of the level 1 products to allow "visibility" and the laying down of the basic configuration of level 1 data processing were significant achievements, a few years worth of issues remained to be negotiated concerning just level 1 processing and production. After announcing that the two teams had committed themselves to an internationally-integrated and interoperable configuration for the ASTER data and information system, the two team leaders commenced the sixth joint ASTER team meeting in November 1993 with a call for "closer collaboration" between the two teams, particularly in algorithm development. It was "necessary to the sucess of ASTER Project [*sic*]," according to Tsu's overhead.[99] The team leaders promised to do their best to make more funding available for extra meetings, if needed. They referenced as an exemplar of close and productive cooperation the ad-hoc

---

[97] Article 4, section 2, paragraph C(1) in NASA and MITI (1996) and GSFC (1997: 6-3).
[98] Geller (1993c). See also Iona (2001) and Schwaller (2001).
[99] Tsu (1993b).

meeting of the Operations and Mission Planning Working Group which had been held that past summer.[100] At that meeting, the two teams had established a strong U.S.-Japan consensus on ASTER's operational design (which was discussed in the previous chapter). ERSDAC's project manager told the level 1 working group that he would "make more cooperation" and that "we will be cooperating more tightly in the future."[101] After he spoke, Geller distributed the first version of the *ASTER End-to-End Data System Concept Document,* calling attention to the international flows and how messy even clean interfaces can be.

Creating a U.S.-Japan Hybrid: The TES Algorithm

U.S.-Japan technoscientific diplomacy over the design of the ASTER remote-sensing system's level 1 data processing and production negotiated concerns of economic value and efficiency, the integrity of scientific knowledge, national autonomy, state power, and U.S.-Japan relations. The two teams' bilateral technoscientific diplomacy did not assert, ascribe, or significantly exercise transnational authority of the kind that had been fostered in the previous two years of negotiations over the ASTER instrument's operational design in the Operations and Mission Planning Working Group. While the two team leaders' call for "closer collaboration" cast the negotiations over the instrument's operational design as an exemplar of cooperation and urged more communication and community between the two teams, their call also pointed out that the ASTER collaboration was, for them, not yet close, or at least, not close enough, especially in the selection and development of the higher-level data products.

[100] Ibid.
[101] Level 1 Working Group (1993).

The two team leaders backed up their call for enacting "closer collaboration" with recommendations for organizational reforms that encouraged joint work and transnational authority. Just as Tsu and Kahle at the fifth ASTER team meeting had restructured working groups and had formed the "Level 1 Data Products Working Group" for the purpose of focusing the teams' joint efforts on defining the level 1 data products, at the sixth ASTER team meeting they recommended to their teams that an existing working group be restructured to focus on the coordination of the joint development of higher-level data products. According to the charter of the new "Higher-Level Data Products Working Group," the purpose of the working group was to "oversee all aspects of joint U.S.-Japan team collaboration and coordination with respect to the specification and development of ASTER [higher-level] data products."[102] Following Tsu's recommendations for closer collaboration which he had outlined at the opening plenary of the sixth team meeting, the working group discussed and adopted a set of principles for collaboration on higher-level data products. The Japan chair of the working group, Yamaguchi Yasushi, reported these principles to the two teams at the team meeting's closing plenary:

> The first one is that all algorithms [for higher-level data products] produced in both countries will be identical. The source code can be different. . . . Next, we will create one unified data product list, and one science data product specification (SDPS) document. So this means that we will prepare for the ATBD—what does ATBD stand for? Algorithm Theoretical Basis Document, ok—we will create one document for US and Japan data products. And for data products specification document, I propose this description, and the US side has already prepared a sort of documentation, so Geller-san and Hook-san will work to combine to create data products specification.[103]

Yamaguchi went on to explain that the Higher-Level Data Products Working Group had decided to leave it to each disciplinary working group to define when differences

---

[102] Higher Level Data Products Working Group (1993b).
[103] Data Products Working Group Reporting (1993).

in source code were substantial enough that the U.S. and Japan algorithms under each group's purview were in fact no longer identical in practice, although the algorithms might have purported to calculate the same geophysical information. He suggested "maybe one idea is maybe 1/10th of the absolute accuracy, or maybe 1/100th of the absolute accuracy, maybe something like that."[104]

Over the next few years, none of the disciplinary working groups—at least according to the minutes of their meetings—attempted to establish such performance benchmarks for similarity. In 1993, most of the working groups were busy selecting higher-level data products for production and were developing algorithms and Algorithm Theoretical Basis Documents (ATBDs) in preparation for NASA's March 1994 peer-review of proposed "standard" data products (as explained earlier in the chapter, "standard" data products were not, by definition, products that were internationally standardized; rather, they were data products that were NASA-approved, peer-reviewed, and slated to be routinely-produced by national data centers). As of November 1993, the U.S. and Japan teams each listed about ten "standard" data products that they would produce and another dozen of "specialized" data products that they would produce "in house" and not for public distribution. Most of each team's list of "standard" data products overlapped with the other's list. These overlapping data products purported to calculate the same geophysical information (e.g., surface reflectance, surface temperature, etc.).[105] Yet, as Yamaguchi's comments about establishing benchmarks for similarity suggested, two algorithms could both claim to calculate the same geophysical information, but in practice they might come up with different answers, maybe to such an extent that users could be left to wonder whether either the U.S. or Japan product was systematically superior, or even whether

---

[104] Ibid.

[105] "Japanese Data Products (Draft)" and "List of [U.S.] Data Products" in ASTER Science Team (1993a).

or not the two products were in fact—to use the language of remote sensing—"extracting" or "recovering" the same geophysical information. Rather than deciding at this point to proceed on the path of competition, the team leaders and the members of the Higher-Level Data Products Working Group chose to advance the international cooperation that had been realized in the designs of ASTER's operational capabilities and level 1 processing system and take "closer collaboration" one step further by enacting a normative notion of U.S.-Japan partnership.

Rhetorically acting out partnership is one thing; consistently enacting it in practice is another. As of 1993 most of the working groups that were proposing "standard' data products were still evaluating and debating between competing algorithms for calculating a particular type of geophysical information. Not all of these competitions were between one algorithm proposed by the U.S. side of a working group and another algorithm proposed by the Japan side. In certain cases, discussions within working groups also included debates over two or more competing algorithms that were proposed by one side. Nevertheless, for the competitions among U.S. algorithms or among Japan algorithms, either the U.S. or Japan chair of the relevant working group could weigh in with an influential recommendation, or if need be, the team leader could make the deciding call. For the debates between U.S. and Japan versions of the "same" data product, there was no single authority in an organizational hierarchy to make the ultimate decision, if discussions came down to needing one. The respective teams could choose to differently "recover" the "same" geophysical information using different "standard" data products. Still, the team leaders pressed the working groups to come to a U.S.-Japan consensus, even if the decisions within each side might not have been consensus decisions. For example, in their efforts to come to a U.S.-Japan consensus on the data products within their purview, the ASTER team's Temperature-Emissivity Working Group enacted U.S.-Japan partnership and established their group as a transnational authority over the design and maintenance of their data products. But this partnership and transnational authority did not come easy.

The working group's objectives for their temperature and emissivity data products were to map on a pixel-by-pixel basis the temperatures and emissivities of a scene that had been observed by ASTER's thermal infrared sensor. The term "temperature" has an intuitive meaning in everyday language which does not require further definition in order to understand the working group's efforts to design algorithms for their two data products. But the term "emissivity" requires brief elaboration.

The emissivity of a particular material is the extent to which a material's surface emits light in comparison to a theoretical standard that is characterized by Planck's law of black-body radiation. The emissivity of a material can vary, depending upon the wavelength of light (i.e., the wavelength of electromagnetic radiation). Planck's law describes the intensity of light (or, more precisely, the spectral radiance of electromagnetic radiation) that is emitted by a theoretical "black body." The law can be expressed as a function of temperature and wavelength, with intensity as the independent variable. At a given temperature and wavelength, real materials reflect more light and absorb less light in comparison to the ideal black body. When real materials are in thermal equilibrium in which the energy that is absorbed must also be emitted, because they absorb less light than an ideal black body, they also emit less light. This "emissivity" of real materials is a percentage of the perfect "1" that is a black body's emissivity at any temperature and wavelength, and the percentage is different for different wavelengths of light. Thus, if Planck's law of black-body radiation were used to predict the intensity of light emitted by a real material at a given temperature and wavelength, the result would need to be multiplied by the emissivity of that material at that particular wavelength (i.e., multiplied by the fraction to which that material looks like a black body at that wavelength). Water and water-filled materials such as vegetation have high emissivities across the thermal infrared region of light. Their emissivities are close to one. They come close to being "black-bodies" and are said to be "gray-bodies." Materials such as minerals and dry soils have lower emissivities, and their emissivities vary significantly according to the wavelength of light that is absorbed and emitted. That is, minerals and dry soils look more or less like black bodies depending upon the wavelength of light that is sensed.

Because the emissitivity of many minerals vary significantly and differently according to the wavelength of light that is emitted, particularly in the thermal infrared region (i.e., they are not all "black" and have, in effect, different "colors" in the thermal region), an instrument such as the ASTER instrument's multi-band thermal infrared radiometer can be used to discriminate among and possibly identify minerals on the earth's surface according to their characteristic emissivities at various wavelengths in the thermal region (i.e., according to their "colors"). The calculation of these emissivities and the discrimination and identification of materials are both easier tasks if the remote-sensing scientist has independent knowledge of—or can make good, educated guesses about—the kinds of materials that are likely to be found in the remotely-sensed scene under investigation.

The remote-sensing literature from the 1980s and through the 1990s presented the task of designing increasingly robust and general methods for "recovering" temperatures and emissivities of land surfaces from remote-sensing data as a formidable challenge worthy of further work. This challenge became more significant with the development of airborne and space-based instruments such as the ASTER instrument that offered, or would offer, greater capabilities around which these methods could be tested and refined.[106] The ASTER team described the ASTER

---

[106] For example: Price (1984); Gillespie (1985); Becker (1987); Wan and Dozier (1989); Becker

instrument as an instrument that could be used to map the temperature and emissivity of land surfaces without the benefit of extensive, independent knowledge of the particulars of the scene that was going to be mapped (e.g., whether the scene was, for example, a desert, a receding snow field, a volcano, a forest clearing, an irrigated agricultural field, or a city; what kinds of minerals were expected to be in the desert scene, etc.).[107]

Excellent performance for arid and semi-arid scenes was of particular interest to the U.S. team, especially since those scenes would best take advantage of the emissivity-mapping capabilities of ASTER's thermal infrared sensor. These capabilities derived, after all, from the sensor's multiple bands that the TIGER team had pushed for in negotiations in 1989 with the specific objective of mapping the emissivities of arid and semi-arid scenes, in distinction to the goals of the U.S. state and of NASA's EOS, which had emphasized the study of climate change by investigating biogeochemical cycles and which had required, at a minimum, only one or two bands in the thermal infrared region for mapping temperature. With five bands in the thermal infrared region, the capabilities of the ASTER instrument for recovering temperature and emissivity information were unlike any other (unclassified) space-based instrument that had been launched prior to ASTER or any instrument that has been launched to date since.

The ASTER team's Temperature-Emissivity Working Group took on the challenge of "recovering" temperature and emissivity information in a most ambitious way. The group sought to write an all-purpose, generalized algorithm that would generate temperature and emissivity data products for the so-called "general" user who, it was assumed, would not want to be bother with many details of the calculations and

---

and Li (1990a, 1990b); Kealy and Gabell (1990); Vidal (1991); Hook et al. (1992); and Watson (1992).

[107] See, for example, Hook et al. (1994: 1).

who would want temperature and emissivity maps of diverse land scenes. That is, the working group sought to design an algorithm that would not be limited to particular kinds of scenes and which would not know in advance what kind of land surface it was mapping. The widely-recognized, fundamental dilemma at the core of all efforts to recover temperature and emissivity information for land surfaces was that the contributions of temperature and emissivity were mathematically and physically indistinguishable in the intensities of light which remote-sensing instruments sensed.

Here was the particular form of that fundamental dilemma for the ASTER instrument: Each of the five bands of ASTER's thermal infrared radiometer was to sense, on a pixel-by-pixel basis, intensities of light at the particular band's wavelength. Using Planck's law, these intensities could be mathematically equated to an unknown emissivity at the wavelength of each of ASTER's bands, provided the temperature of the material was known (remember, emissivity was the fraction of light that was actually emitted in comparison to what was expected according to Planck's law, which was a function of wavelength and temperature). Yet, not only was the emissivity of the material at that pixel unknown, but the temperature was unknown too. Using only math and fundamental principles of physics, and without venturing any initial, educated guesses about the kind of scene that was being mapped, there was no way to derive emissivity by comparing the sensed intensity of light with the intensity of light that Planck's law predicted a black-body would emit, because temperature was also unknown, and it—like emissivity—also contributed to the intensity of light emitted by a material.

The contributions of emissivity and temperature were, in effect, mixed in the intensity information, and the analytical task was to somehow "separate" temperature and emissivity. Because the emissivity of the material varied for the wavelength of each sensing band, but the material's temperature did not, using Planck's law, a set of five equations, one for each wavelength band, could be written. These five equations, however, included six unknowns—the five different emissivities (a different emissivity for every band) and their shared unknown of temperature. If one more equation with no additional unknowns were formulated and introduced into this set of equations (i.e., more precisely, this set of simultaneous equations), the set of equations could have been, in theory, mathematically solved for the pixel's temperature and for the pixel's emissivities at five different wavelengths, which might have allowed that pixel to be matched to a material or at least contrasted with materials at other pixels on the image. Much of the debate in the Temperature and Emissivity Working Group was how that additional equation could be formulated and what assumptions could be wisely made to enable its formulation. Yet, because they sought to design a generalized algorithm for the general user, they could not formulate an additional equation that assumed too much independent information to "separate" temperature and emissivity, such as an equation that took into account the particular kind of land surface was being mapped.

The working group had discussed and evaluated over ten competing methods for deriving temperature and emissivity information, not all of which recovered absolute numbers for both temperature and emissivity over land surfaces. By the time of the sixth joint team meeting, they spent most of their time examining and debating the advantages and disadvantages of three. Each of these three methods was generally developed, refined, or used by different members of the working group. Alan Gillespie, the U.S. chair of the working group, had developed the "normalized emissivity method," which is commonly abbreviated NEM.[108] This method provided the required "additional equation" by assuming a middling emissivity value for one of the

[108] Gillespie (1985) and Hook et al. (1994). NEM was a refinement of the "reference-channel method" (or, alternatively, the "model emissivity method"). For the reference-channel method, see Lyon (1965).

five equations.[109] This value was then used, along with the intensities sensed at each of the five wavelength bands, to calculate the temperature of the land surface at that pixel and the emissivities of the material at that pixel at the other four wavelengths (the fifth emissivity being assumed from the beginning). The key flaw that the working group saw with this method was that it assumed the same, fixed emissivity for one of the wavelengths for every pixel of every ASTER scene. It was hoped that this flaw could be repaired by using ASTER's other sensors to "classify" the scene on a pixel-by-pixel basis (as water, soil, vegetation, etc.), and then use that classification to pick a better assumed emissivity on a pixel-by-pixel basis (whether the scene was water-filled vegetation or desert rock would make a significant difference in the assumed emissivity, that is, the initial educated guess of NEM). The repair of classifying scenes, however, would introduce much more complexity, the working group agreed, and the costs and benefits of this complexity were unclear.

Simon Hook, who was a geologic remote-sensing scientist at JPL and a member of Kahle's research group, had used and assessed the second method that was discussed by the working group. This method was called the "alpha-derived emissivity" method, commonly abbreviated ADE.[110] The ADE method provided the "additional equation" to enable the separation of temperature and emissivity by making, at the method's core, one mathematical approximation and one

[109] In NEM, the equation (i.e., wavelength band) that was selected for this assumption was the one that yielded the highest temperature using the assumed emissivity.

[110] The ADE method had been developed by Kealy and Gabell (1990). For Hook's use of the method, see Hook (1992: AA1), Hook et al. (1992), Kealy and Hook (1993), and Hook et al. (1994). Gabel, Hook, and Kealy all collaborated together on temperature and emissivity separation for geologic remote-sensing in the early 1990s (e.g., Kealy and Gabell 1990; Hook et al. 1992; Kealy and Hook 1993). As young scientists, they had taken similar career paths as well. Gabell had been a U.S. National Research Council postdoctoral fellow at JPL, advised by Kahle, from 1986 to 1988. Hook first came to JPL as a National Research Council postdoctoral fellow in 1989 and was also advised by Kahle. Kealy received his Ph.D. from the same department that Hook had, but two years later. Hook received his Ph.D. from the University of Durham in the United Kingdom in 1989. Kealy received his in 1991.

empirically-based statistical generalization. First, the method used a well-known approximation of Planck's law to equate mathematically, for each pixel of land surface, the intensities that would be sensed by each of the five wavelength bands of the instrument to mathematically-manipulated versions of their respective, unknown emissivities. The temperature variable was conveniently separated out from the equation in the mathematical approximation and manipulation. But in the mathematical manipulation, information concerning the mean of the five emissivities had also been subtracted from each of the mathematically-manipulated versions of the five emissivities. This subtraction of information about the mean of the five emissivities had left the emissivity information with only their respective variations around the mean. Thus, only half of the emissivity information was recovered through mathematical approximation (analogously, to reconstruct a student's absolute grade on an exam in a class requires knowing the mean grade of the class as well as how much better or worse he did relative to the mean).

The ADE method estimated the subtracted information concerning the mean by statistically matching the variation in the mathematically-manipulated versions of the five emissivities to an empirical mean, by generalizing from a laboratory-constructed database of the thermal-infrared emissivities of over eighty materials that are common on the earth's surface. With that statistically-matched information about the empirical mean emissivity (which was taken to be the "real" mean) the emissivities at each of the five wavelength bands could be calculated from their respective variations from the "real" mean. Once these emissivities were "recovered" for the five wavelength bands, so could the temperature that was shared by the equation for each wavelength band. The recognized flaws of the ADE method were that, first, the mathematical approximation introduced a systematic bias. Second, the method's statistical matching was limited by the inherent uncertainty of generalizing from a database that may or may not be representative of the material sensed in the pixel.

The third method that was still being discussed and debated by the Temperature-Emissivity Working Group around the time of the sixth ASTER team meeting was a “split window” method. Rokugawa Shuichi, the Japan chair of the working group, and Matsunaga Tsuneo, who was at the time Rokugawa’s graduate student at the University of Tokyo, were the two members of the working group that were proposing an algorithm that used a split-window method.[111] The split-window method had a long track record of use for recovering the temperature of sea-surfaces, but not for land surfaces. In this method, the emissivity of the sea’s surface was known in advance or could be reliably assumed, unlike for land surfaces. One advantage of the split-window method was that it compared the intensities that were sensed at two wavelength bands (which created the “split window”) and used that comparison to reduce atmospheric effects in the calculation of surface temperature. In contrast, the NEM and ADE methods both relied upon a separate algorithm that would have been run earlier in the data processing to remove atmospheric effects. Another advantage of adopting the split-window method for the standard data product for temperature was the possibility of comparing both the algorithm and its data products with the split-window algorithm and data products of another EOS instrument that was slotted to be on the same satellite as ASTER.[112] The working group invited a scientist from that instrument’s team to their meetings for the purpose of examining that possibility.[113]

---

[111] That method was explained in Matsunaga et al. (1993).

[112] Gillespie (1993b: 3) and the discussion in Gillespie et al. (1999: 35).

[113] That person was Zhengming Wan, from the University of California at Santa Barbara. He was writing the Algorithm Theoretical Basis Document for recovering temperature and emissivity of land surfaces for the moderate-resolution MODIS instrument, using classification in the visible and near infrared region and in the shortwave infrared region (1999). This algorithm, however, was not intended to be capable of mapping geologic materials in arid and semi-arid regions. Wan attended

Yet, the working group considered the split-window method to have significant disadvantages. First, it could not be used to map the emissivities of land surfaces. Emissivity-mapping was a central goal, however, of the U.S. ASTER team and had been, from the beginning, a central goal of the TIGER team. ASTER's emissivity-mapping was intended to build upon the work of Kahle's research group at JPL in the 1980s and early 1990s, which mapped emissivities using an airborne instrument called the Thermal Infrared Multispectral Scanner. Second, owing to the design of ASTER's thermal infrared bands, the split-window method's ability to recover temperature over land surfaces was limited to, at best, scenes that were well-characterized in advance on a scene-by-scene basis.[114] Consequently, U.S. team members did not consider the method to be an appropriate choice for a general-purpose standard data product of the kind that the working group had set out to design.

Nevertheless, the members of the working group from the Japan team proposed the split-window method for at least "a" U.S.-Japan standard data product for temperature (particularly for sea-surface temperatures), if not for "the" U.S.-Japan standard data product. U.S. support for this proposal, however, was not forthcoming—the decision was repeatedly deferred.[115] From the perspective of the U.S. team, many different algorithms—including one that used the split-window method—could be selected for "in-house" specialized products, but the U.S. team

at least the 5th, 6th, 7th, 9th, and 11th U.S.-Japan ASTER Team Meetings and the U.S. ASTER Team Meeting in November 1992 (respectively, ASTER Science Team 1993b, 1993a, 1994, 1995, and 1996 and U.S. ASTER Science Team 1992).

[114] Other disadvantages of the split-window method for the ASTER instrument are discussed in Gillespie et al. (1999: 48). In the working group's report to the ASTER team at the fifth team meeting, Gillespie relayed the idea of using the split-window method for only water surfaces (1993b: 18).

[115] Rokugawa and Hook (1993) and Rokugawa (1993). In the 7th meeting, Gillespie mentioned exploring an alternative algorithm for sea-surface temperature as a specialized data product and not as a standard data product (Gillespie 1994a: 1).

supported having only one standard data product for temperature and only one for emissivity (and presumably, the U.S. team also considered it to be advantageous if those two data products were produced from the same algorithm to ensure their agreement, since temperature and emissivity were highly intertwined in each other's calculation).[116] The U.S. team had proposed one standard data product for temperature and one for emissivity to NASA in their list of standard data products for ASTER. Furthermore, the U.S. team and a NASA review panel had been pruning that list, to keep it in line with projected funding cuts.[117] In these circumstances, it was unlikely that the U.S. team could have added more standard data products for temperature and emissivity even if they had wanted, which they did not.

Gillespie, Hook, and the other U.S. members of the Temperature-Emissivity Working Group planned to select the standard data product through a "play off" of the various methods.[118] The play off compared the performances of these methods by using the methods to map the temperatures and emissivities of the same simulated scenes and model targets. By the time of the sixth joint team meeting, the U.S. members had narrowed their preferred methods down to two primary contenders, NEM and ADE.[119] While the play off between the NEM and ADE methods had been partially completed, the results were mixed, and the U.S. members could not yet agree on one method over the other for the standard data products for temperature and emissivity. Several months before the sixth team meeting, the U.S. chair of the working group, Gillespie, reported to the U.S. team as a whole that "concurrence will be sought from Japanese counterparts" to provisionally select both NEM and ADE

[116] Gillespie (1992b: V1), Gillespie (1993b: 2), and Kahle's and Hook's comments in "Higher-Level Data Products Working Group (1993a: 2).
[117] List of [U.S.] Data Products (1993) and Kahle (2003).
[118] The quote is from Gillespie (1992a: V2). For the playoffs, see Hook (1992: AA1), Hook et al. (1992), Kealy and Hook (1993), and Hook et al. (1994).
[119] See Gillespie's comments in U.S. ASTER Science Team (1993: 14).

(but not split-window) for the U.S.-Japan common algorithm for the U.S. and Japan standard data products.[120]

At the sixth ASTER team meeting, however, where the two team leaders and the Higher-Level Data Products Working Group endorsed the idea of joint U.S.-Japan algorithms, the U.S. team received no such concurrence from the Japan team to select, on a provisional basis, both the NEM and ADE methods for the joint algorithm. No such concurrence was received for two reasons. First, as Rokugawa's minutes for the working group's meeting prominently noted, Gillespie was absent from the meeting. Second, Rokugawa and Matsunaga were not yet convinced that either NEM or ADE should be selected at that time as Japan's standard data product for emissivity as well as for temperature. The classification step that was important for refining the NEM method was still just a concept and not a working algorithm, and the flaws introduced by the ADE method's mathematical approximation and statistical generalization were still causes of concern for members of the Japan team.[121]

Furthermore, as the thinking reportedly went for the members of the Japan team, if the U.S. team could not yet agree on a single algorithm to propose as "the" U.S.-Japan standard data product, then the Japan team was not yet required to make a decision. At the time, the default position of the Japan team was, as Matsunaga recalled in an interview over a decade later, not to select any temperature or emissivity method for Japan's standard data product, but to relegate all of the contested methods to "in-house" specialized products, as the members of the U.S. team had proposed for the split-window method that was advocated by members of the Japan team.[122] But, if Japan would not have any single standard data product, then a single U.S.-Japan

[120] Gillespie (1993a: AA1).
[121] Rokugawa and Hook (1993: 2); Rokugawa (1993: 1); Rokugawa (2003); Matsunaga (2003); and Matsunaga (2006).
[122] Matsunaga (2006).

standard data product could not exist either. In March 1994, the U.S. team submitted for peer-review to NASA an ATBD for temperature and emissivity which had no co-authors from the Japan team. The document proposed, on a provisional basis, both NEM and ADE as standard data products. Presumably, the two methods were proposed as only "U.S." standard data products, but the document made no note of the issue, writing just that NEM and ADE were "the two" algorithms that were "under consideration."[123]

The scientific and technical work of the U.S.-Japan Temperature-Emissivity Working Group had so far involved: understanding the expected performance of the ASTER instrument, which was still under development; making educated guesses about the probable uncertainties of not-yet-written algorithms for atmosphere correction which would produce data that the temperature and emissivity algorithm—whatever it would turn out to be—would likely require for its calculations; characterizing the implications of different approximations and assumptions that were made by competing methods to provide the required "additional equation"; testing in "play offs" the performance of competing algorithms using model targets and remote-sensing scenes that had been modified to simulate the data that was expected to be acquired by the ASTER instrument; and statistically analyzing these tests. Each of these tasks, however, was not actually conducted by the working group as a whole but by individual members of the working group, who would report their results back to working group as a whole. It should not be that surprising that given a problem that everyone agreed from the beginning was mathematically and physically underdetermined, individual members proceeded down different evaluation paths, became familiar with and accustomed to different ways of providing the additional equation that was required, and had substantively different recommendations for what

[123] Hook et al. (1994:4).

additional equation and what algorithm would work best, recommendations that aligned with their own methods, practices, and, arguably, professional interests.

Amid these specific differences, the question is: how did the working group come to enact U.S.-Japan partnership and exercise transnational authority over a common algorithm? First, especially after the fifth and sixth joint team meetings, the two team leaders pressured the working group to stop fine-tuning methods, to stop testing methods against each other, and to start finding a middle ground.[124] Productive compromises were, after all, being made in other working groups and in other areas (e.g., in the Operations and Mission Planning Working Group, on the MoU, etc.). Hook remembered that the U.S. team members "were encouraged to settle on an approach and stop researching other methods."[125] The two team leaders pushed their respective team members on the working group to make good on the call for "closer collaboration" and on the normative notion of U.S.-Japan partnership that the team leaders had advanced.

In addition, U.S. team members, after not unifying around a single method for the ATBD, were not able to successfully act out state power and were more open to compromise. A contingency—the results of the review—arguably threw a spotlight on the weaknesses in the U.S. team members' enactment. The March 1994 NASA peer-review of version 1.0 of the U.S. team members' ATBD did not result in a strong endorsement for either NEM or ADE. While the reviewers did not split down the middle, they—like the members of the U.S. team—did not decisively endorse one method over the other. Neither method was found to be overwhelmingly compelling. According to an account given in a later version of the ATBD, "some reviewers" were satisfied with the core assumptions made by either the NEM or ADE methods to

---

[124] Matsunaga (2003) and Hook (2006).
[125] Hook (2006).

provide the necessary additional equation, noting that "sometimes nature is not kind" and that the indeterminacy was not likely to be settled through empiricism. "Others" were not satisfied with the core assumptions, approximations, or generalizations of at least one of the two methods. Reviewers commented that both methods were relatively untested. Furthermore, other critiques were numerous. A second review was deemed to be necessary after more work was completed.[126] Soon after the review, members of the Japan team became aware of the lack of a strong endorsement for either NEM or ADE.[127]

The Temperature-Emissivity Working Group's enactment of U.S.-Japan partnership and their exercise of transnational authority are most explained by analytical mixing and by the emergence of a nucleation site in their technoscientific diplomacy. The working group members' own explanations for their eventual consensus around a common algorithm emphasized that it was a mixing of methods that fostered agreement. For example, Rokugawa, the Japan chair of the working group, explained in an interview:

> Although what kind of assumption should be made [to provide the additional equation] was argued over exceedingly, we found a method, at least, that would become the base [of the algorithm]. I say "found." Well, this included combining (*kumi-awase*) along the way various proposals that came to be offered from the science team, such as Gillespie's and Hook's, in addition to the proposals that were available in the beginning [from the literature]. . . . We came to a decision of the form of "this method is probably fine."[128]

The methods that needed to be combined, I suggest, were not, for example, just Gillespie's NEM and Hook's ADE, but methods that had been proposed by members of both teams. The joint working group could have proceeded down the path of mixing or combining Gillespie's NEM and Hook's ADE the year before the review of the

[126] Gillespie et al. (1999:35).
[127] Gillespie (1994b: 3) and Matsunaga (2003).
[128] Rokugawa (2003). Hook (2006) used the word "blending."

ATBD. Yet, Rokugawa did not go along with the provisional selection of both NEM and ADE, which would have been a first step toward the eventual mixing of the two methods and the creation of a U.S.-Japan standard algorithm. A standard algorithm that was acceptable to all parties apparently needed to include methods that were proposed by members of both the U.S. and Japan teams. The gap was wide, however, between the split-window method that was advanced by members of the Japan team, and the NEM and ADE methods that were advanced by members of the U.S. team. No one offered a proposal for how the split-window method could be reconciled with either of the other two. Unlike both NEM and ADE, the split-window method did not attempt to "recover" emissivities. It assumed they were known.

At the ASTER team's seventh meeting in May 1994, Rokugawa's graduate student, Matsunaga, presented to the Temperature-Emissivity Working Group his recent research on what he called the "Mean-Maximum Difference (MMD) method."[129] Matsunaga had submitted a Japanese-language article about his new method to a remote-sensing journal in Japan in January of that year. A few months later and a couple of weeks before the joint meeting, the article had been accepted for publication.[130] As Matsunaga explained to the working group, his MMD method incorporated the NEM method and made use of the empirically-based statistical generalization that was at the heart of the ADE method. His MMD method first used the NEM method to generate emissivities for the sensed pixel at each of the five wavelength bands (with one of the five emissivities being assumed from the beginning). Rather than taking these emissivities to be the final emissivities, as the NEM method did, the MMD method took these emissivities as "first guesses" that would be provisionally used and then later improved.

[129] Gillespie (1994b: 3) and Matsunaga (2003).
[130] Matsunaga (1994).

The MMD method, like the ADE method, provided the additional equation by making an empirically-based statistical generalization between a kind of variation in the presumed emissivities for the five wavelength bands and a "real" mean emissivity. The kind of variation that MMD used, however, was different than the kind of variation used by the ADE method. Whereas the ADE method used a variation that was a standard deviation of mathematically-manipulated versions of emissivities, the MMD method used a variation that was simpler. It used the range of the emissivities: the difference between the maximum and minimum among the "first guesses" of the emissivities for the five wavelength bands. Once the presumed "real" mean was found from the statistical matching, it was taken as the mean for the emissivities of the five wavelength bands and the "first guesses" from the NEM method were subsequently adjusted around their new mean accordingly, preserving their original variance. The MMD method was able to avoid the convenient, but problematic, mathematical approximation that was used in the ADE method because the MMD method had used the NEM method, instead of mathematical approximation, to generate the initial emissivity information.

Like Miyazaki's trade-off table that initially enabled the two teams to break out of bilateral diplomacy, Matsunaga's MMD method carried with it starting assumptions that had the imprimatur of members of both the U.S. and Japan teams. The MMD method emerged as the nucleation site around which the Temperature-Emissivity Working Group negotiated their common algorithm. The working group generally produced minutes that were matter-of-fact, reserved, and sparse. For example, a typical description of work conducted at the meetings was this line from Gillespie's minutes from the meeting at the fifth ASTER team meeting: "discussion on data product definition (Japan, U.S.)—continued."[131] In contrast, here

[131] Gillespie (1993b: 1).

is the minutes' description, which was also written by Gillespie, of Matsunaga's presentation:

> Next, Matsunaga presented details on a new Japanese algorithm (MMD) that combines characteristics of the Normalized emissivity and Alpha emissivity methods. Gillespie notes this approach could also be used to make Watson's ratio method yield accurate temperatures and emissivities. After the session, Japanese members suggested combined application of Normalized and MMD emissivity. Wan reported on the MODIS SST [sea-surface temperature] algorithm, and the team considered how to adapt it to ASTER. This might be a separate algorithm, but if the Alpha or MMD approach was used, the algorithms could be combined.[132]

Not only did the minutes note that Matsunaga's MMD combined aspects of NEM and ADE, but Gillespie suggested enticingly that MMD could serve as the base for two other methods that the Japan team had previously expressed an interest in: Watson's ratio method (which has not been described in this chapter) and a split-window method for sea-surface temperature. The U.S. team had also been interested in both methods, but as specialized data products and not as standard data products. Finally, in what was a rare comment for any of the ASTER team's minutes, Gillespie's minutes took note of something that happened outside of the meeting itself: "Japanese members" (not only a particular individual) suggested using NEM along with MMD for the standard product. A U.S.-Japan hybrid algorithm was in the making, and MMD had served as a nucleation site for it.

Matsunaga shared in an interview that he considered that presentation and MMD more generally to be among his most significant personal contributions to the U.S.-Japan ASTER collaboration (and at the time of the interview, almost a decade after the presentation, Matsunaga was still heavily involved in the ASTER collaboration, which at that point largely concerned the operations and maintenance of

[132] Gillespie (1994b: 3).

the remote-sensing system).[133] Furthermore, Matsunaga said that his most vivid memory of the collaboration was something that Kahle said at the working group meeting during his presentation. Matsunaga expected that the U.S. and Japan teams would not agree on a U.S.-Japan standard algorithm for temperature and emissivity separation and that the Japan team would relegate all of the algorithms to "in house" specialized data products. Matsunaga recalled that, during his presentation, he said "I think it's too late for ATBD or ASTER standard algorithm." Kahle, however, authoritatively interjected "it's not too late," and she, along with Gillespie, suggested that perhaps a common algorithm could be agreed upon using MMD as the core of the new version of the ATBD. Kahle's comment, Matsunaga shared in the interview, made a deep impression on him. Kahle was "at the time like my father (*boku no chichioya-gurai*) . . . I mean, generally speaking . . . senior in years and having broad vision." He was astonished, Matsunaga said, that his MMD would be central to the standard temperature and emissivity algorithm for the ASTER remote-sensing system.[134]

By the ninth joint team meeting, in May 1995, the U.S. and Japan teams both expressed their clear support for a U.S.-Japan standard algorithm designed around MMD. It was called "TES" for "Temperature and Emissivity Separation."[135] The second version of the ATBD, which was the first version with TES, was labeled

---

[133] Matsunaga (2003).
[134] Matsunaga (2003). Matsunaga's use of "father" instead of "mother" might strike some readers as odd. Judging from my observations of joint team meetings that occurred between 2001 and 2004, Kahle's status as the U.S. team leader, her strong personality, her abrupt style of questioning, and her stereotypically American hard-charging principal-investigator disposition pushed aside any gender expectations that members of the Japan team might have had. A few members of the Japan team have confided in me that they found at times Kahle's habit of interrupting presentations with questions and comments to be impolite, but I suspect that they would have found those questions to be impolite if they had been asked by a man as well. Similar comments have been made to me about one or two other U.S. participants (who were men, like all the participants in the collaboration with the exception of Kahle).
[135] Gillespie, "T/E Working Group Meeting," in 9th AST Meeting, p. 1.

version 2.0 (as opposed to 1.1) in order to denote that the version was a major revision of the 1.0 document that had been produced for the March 1994 review. By June 1996, the working group reported to the ASTER team that the TES algorithm was “mature” and would “change little before launch.”[136] The abstract of the last version of the ATBD for TES, version 2.4, described TES as an algorithm that “hybridizes two established algorithms,” NEM and ADE, with MMD described as being an adaptation of ADE.[137] In the body of that document and in the body of a prominent 1998 English-language journal article that documented the TES algorithm and discussed its contribution to the field of remote sensing, TES was described as being “most closely related to the Mean-MMD method [among the many methods in the literature],” citing Matsunaga’s Japanese-language article for the “Mean-MMD method.”[138] In the U.S. team’s usage of the MMD acronym, MMD came to stand not for “Mean” and “Maximum Difference,” as Matsunaga had originally parsed it in his Japanese-language article, but “Maximum-Minimum Difference,” thus requiring an additional prefix for the “Mean.”

After Matsunaga’s presentation in May 1994, the basic MMD method underwent significant development, expansion, and fine-tuning in its transformation into the complex TES algorithm. The work of transforming MMD into the TES algorithm involved almost ten members of the Temperature and Emissivity Working Group, and the bulk of the work—judging from the action items listed in the working group’s minutes—was done by Gillespie, Matsunaga, Hook, and Rokugawa. The documentation shared at working group meetings shifted from the pre-MMD documentation of elaborations on concepts, outlines of methods, and analyses of play

[136] Gillespie (1996: 1).
[137] Gillespie et al. (1999).
[138] Gillespie et al. (1998: 1116) and Gillespie et al. (1999: 1,7,44). Gillespie et al. (1998) was part of a special issue dedicated to instruments that were aboard NASA’s flagship EOS satellite, EOS-AM (which was later re-named Terra).

offs, to post-MMD documentation of flow charts of a single algorithm's processes, statistical analyses of one algorithm's various components, considerations of potential trade-offs within the algorithm, and lists of changes made to design and of potential changes for the future. Individual members of the working group still reported to the group as a whole, but they took each other's work products and incorporated them into their own work (e.g., Gillespie incorporated Matsunaga's analysis of different options for the statistically-matched "additional equation"). While the Japan team was writing its own implementing code, and while the U.S. team was writing its own implementing code, they were turning to the same ATBD for guidance, and they returned to the working group meetings with issues that required further clarification (when they were not handled through e-mail). Occasionally, Gillespie's University of Washington group passed code directly to Rokugawa and Matsunaga (e.g., code for scene classification).[139]

The TES hybrid had five modules (here the description is specifically of version 2.4). The first module of TES was based upon NEM, and it established the best "first guesses." The second module used Watson's ratio method to construct ratio versions of the emissivities which were less sensitive to error than the first guesses. The third module, the core of TES, was the MMD module, which used a modified version of the additional equation that was used in Matsunaga's original MMD. Instead of statistically matching a difference between the maximum and minimum of the emissivities of the five wavelength bands to an empirical mean emissivity (which was then used to adjust each emissivity of the five wavelength bands), the additional equation statistically matched a difference between the maximum and minimum of

[139] For discussion of common flow charts, assessing together different options for the "additional equation," and for trading code, see Gillespie (1996). For joint problem-solving, see the discussion over the "bandlack" issue and quality assurance (in Gillespie 1997 and Gillespie and Rokugawa 1999, respectively).

ratio versions of the emissivities to an empirical minimum emissivity. Furthermore, this statistically-matched additional equation was no longer a linear equation, but a power law (i.e., the max-min difference was operated upon by a constant exponential). These significant changes to the additional equation that was provided by MMD were hashed out together by both teams and both teams agreed that these changes resulted in a more stable and robust statistical matching. The final module of the algorithm was a quality assurance module that estimated the accuracies and precisions of the data product.[140]

Many fine-tunings and optimizations were explored and implemented inside these five modules. For example, the use of a scene classification algorithm had been investigated, although ultimately one was not implemented as a part of TES. On the other hand, an iterative approach was implemented. In this approach, calculated temperatures and emissivities were used again and again as better "first guesses" for the next versions, until the difference in the two versions was less than the noise specification of the instrument. The iteration was first implemented in the MMD module, but then it was later moved up front to the NEM module. Throughout this development of the U.S.-Japan hybrid algorithm, MMD and TES were not competed against ever more fine-tuned competitors in play offs (such as with the traditional ADE). MMD served as the sole nucleation site for developing the U.S.-Japan TES hybrid.[141]

The Temperature and Emissivity Working Group made good on the two team leaders' call for "closer collaboration." They also fulfilled their share of the language in the GSFC-ERSDAC Project Implementation Plan that tasked "the ASTER Science Team" with "developing a common set of algorithms for ASTER standard data

[140] Gillespie et al. (1998, 1999).
[141] Ibid.

products to be generated both in Japan and in the U.S."[142] Kahle and Nichols had written that language into the draft PIP that had been under negotiation in the fall of 1993, a few months before Tsu and Kahle called for "closer collaboration" at the sixth ASTER team meeting.[143] The PIP was not signed, however, until the fall of 1997, after the Temperature and Emissivity Working Group had, in practice, already enacted U.S.-Japan partnership in the design of the TES hybrid and had exercised transnational authority over the development of multiple versions of that standard algorithm.

Nevertheless, the algorithm's implementation was ultimately not exactly the same in both countries. For example, where the U.S. team's code took ratios of emissivities in the second module, the Japan team's code took ratios of intensities.[144] Despite this difference, a 1997 joint comparison of the performances of the two codes, which was based on common target sites, showed only "very small differences."[145] At that time, however, the code's implementation and testing was just in its beginning. Once Rokugawa and Matsunaga handed off their code to Japan's DAAC, ERSDAC, in the fall of 1997, where ERSDAC's contractors—with the assistance of Matsunaga—further developed the code and implemented it into ERSDAC's systems, the code became much less open to change. After the launch of ASTER in December 1999, the joint Temperature-Emissivity Working Group fine-tuned the details of the TES algorithm, advancing the code's version number incrementally from 2.5 to 2.9. By this time, ERSDAC and its contractors had finished what they considered to be their engineering and development stage and were then into operations. ERSDAC did not want to open up the code that their contractors had supposedly completed, especially if the importance of the changes were relatively small. More money had to

[142] GSFC (1997: 2-10).
[143] Nichols (1993c).
[144] Rokugawa (1997: 124).
[145] Ibid.

be allocated to contractors for ERSDAC to make changes. ERSDAC made a management decision not to keep pace with the joint working group's recommended updates to the algorithm. Japan's code was not open to amendment to the same extent that the U.S.'s code was.[146]

In the United States, Gillespie's University of Washington group passed revisions to the software engineers at JPL, who then passed it on to the U.S. DAAC for implementation, usually in the form of consolidated packages that included updates for the other standard data products as well. Updates to TES were made in April 2001, October 2001, April 2002, and June 2002.[147] Every update to the algorithm might be an update that ERSDAC would not make. For example, the implementation of TES at the U.S. DAAC had an option that was said to improve the estimation of temperature over water surfaces and vegetation (i.e., over gray bodies). ERSDAC chose not to implement this option.[148] At this level of specificity, the question of whether or not the United States and Japan actually had a U.S.-Japan standardized data product was for users to decide. From the perspective of the two team leaders and the two chairs of the Temperature and Emissivity Working Group, the working group had agreed to an internationally standardized data product that was documented by an ATBD that had U.S. and Japan co-authors, and the working group was exercising a joint authority over the maintenance of that standard data product. The working group's transnational authority over the U.S.-Japan partnership in production was substantial, but the scope of that transnational authority was not unlimited.

---

[146] Matsunaga (2003).
[147] Eng (2003).
[148] ASTER GDS, "Release Note: Note on ASTER GDS Temperature and Emissivity," http://www.gds.aster.ersdac.or.jp/gds_www2002/service_e/release_e/set_release_e.html, last accessed 11 August 2006.

U.S.-Japan Equality in the Governance of Consumption

A normative notion of U.S.-Japan partnership underpinned the ASTER team's exercise of transnational authority in another area of the ASTER remote-sensing system's international political economy: consumption. In the realm of consumption, "partnership" also meant "equality." One early issue that the two teams' Operations and Mission Planning Working Group confronted was the fact that not all users were alike. Users could not be treated as interchangeable black-boxes, although they were often simplified that way in diagrams (e.g., see figure 6.1). The two teams decided that different categories of users were to have different rights to consumption of the ASTER instrument's resources, although it took a few years of work to define just what those user categories would be.[149] While user categories came to be hierarchical for users within each of the national groupings, a strict equality was observed and enacted, at least on paper, between the rights of "Japan" and "U.S." users. This equality was in agreement with, but was not specifically dictated by, a general philosophy of "non-discrimination" in data exchange which was expected to be forthcoming in the international data exchange policy for EOS. According to most descriptions of international political economy, states are not typically guaranteed an equal slice of consumption under a notion of international partnership, or even just a "right" to equal consumption in the abstract.[150] Nevertheless, in the configuration of the international political economy of ASTER data and information system, a right to equal consumption was indeed affirmed as a principle of U.S.-Japan partnership and equality.

---

[149] The two teams' Operations and Mission Planning Working Group started to explore differences in the identity of users in 1993 (Morrison 1993).

[150] Bhagwati and Patrick (1990); Frieden and Lake (2000); and Gilpin (2001). International trade theory posits equal benefits in equilibrium. "Benefits" does not necessarily correspond with "consumption," however.

For instance, the two teams granted individual members of each team the right to acquire more than four times the amount of observations that any single general user could acquire. The Japan and U.S. team leaders were each allocated ten times that of the general user. Likewise, acquisition requests from a "Special Priority Japan User" who was affiliated with a mineral or natural resource firm and those from a NASA-sponsored investigator who was associated with other EOS studies were both allowed twice as much as a general user.[151]

Yet, despite evidence of a wide difference in demand, the two teams established equal allocations of consumption for corresponding user categories between Japan and the United States. That is, a U.S. team member's individual allocation was in principle equal to a Japan team member's individual allocation, and the individual allocation for an EOS user was equal to the allocation of a Special Priority Japan User. This equality was emphasized and formally established in such planning documents as the Long-Term Instrument Plan, which was written and signed by the team leaders.[152] The Long-Term Instrument Plan was also signed by the program management at NASA and MITI. To give an indication of how much the demand could vary between the U.S. and Japan teams, after a preliminary survey of desired observations was conducted for planning and simulating the instrument's operations, U.S. scientists had requested more than three times the number of scenes as Japan's scientists had requested over the anticipated life of the instrument (the numbers of scientists on each team was close to equal in number); moreover, if the

---

[151] Tsu and Kahle (1997: 10) and Yamaguchi (1999). These ratios are for just the allocations of individual users. These allocations do not include, for instance, observation requests that were successfully proposed by members of each team as an observation (or, more likely, a set of observations) acquired under the transnational authority of the entire joint science team (called a "science team acquisition request"). Science team acquisition requests were vetted through a committee process that allowed them to be endorsed first by each national team and then by a joint working group (ASTER Operations and Mission Planning Working Group 2000).

[152] For example, Tsu and Kahle (1997).

"common ground" of a global land map is discounted, at this early date U.S. scientists had requested more than ten times the number of scenes that Japanese scientists had requested.[153] At the time, there was good reason to regard this survey of demand as an important but speculative exercise; this particular survey was conducted several years before the launch of the instrument, and interest among scientists in Japan was expected to increase.[154] Nevertheless, the U.S. team never considered trying to work out an arrangement in which the United States or U.S. users were formally allocated more observational resources in light of the significant difference in the sizes of the potential science user communities in the United States and Japan. In no way was this kind of equality—equality on the basis of states as well as users—mandated by the international data exchange policy that at the time was expected to be forthcoming. In practice, when the U.S. team leader saw an opportunity to work in more requests from U.S. colleagues, such as by funneling in more proposals from non-team members through the team acquisition request process, she generally seized it. Nevertheless, a norm of equality was enacted.[155]

This presumptive norm of equal consumption between similar user categories was not just held as a principle of equality that should govern allocations only between the two teams. It was held to extend to the use of ASTER by Japan's and the U.S.'s general users who were not associated with the teams in any way other than sharing a

[153] Hekl (1993b: D17-19).

[154] ASTER Operations and Mission Planning Working Group (1993: 3).

[155] For instance, just under a year before launch, over 250 acquisition requests that had originated from, or were funneled through, U.S. team members were approved through a joint process in the name of the U.S.-Japan ASTER science team. About 150 acquisition requests from Japan team members were approved through this same joint process (ASTER Science Team 1999: 7, 12, 162-166). While this comparison does suggest that U.S. scientists were able to work in more requests through the joint approval process, it does not compare the use of the instrument's observation resources (say, in terms of number of imagery scenes or bandwidth). One acquisition request through this process could easily have included a request for hundreds of imagery scenes. Furthermore, these requests were not strictly "zero sum." Requests overlapped, especially with the joint objective of producing a global map of the earth's land surface. Rather than pointing out these complexities, the two teams presumed equality.

national membership category. These users were expected to certainly number in the hundreds, possibly in the thousands. Moreover, the U.S.'s general users were not to "crowd out" Japan's general users. Because, however, observation requests from general users received relatively low priority in the scheduling of observations in comparison to the requests that came from members of each team as well as from other prominent users, this presumptive norm against crowding out was rarely tested.[156]

The presumptive notion of equality between the consumption of national general users extended into pricing as well, but not without significant contention. NASA Headquarters had in the late 1990s wanted the ASTER data that NASA distributed to be free to general research users, in accordance with their policy for other EOS instruments.[157] The U.S. DAAC had in fact distributed ASTER's data free of charge after the instrument's level 1 data products were released for user orders in November 2000, subsequent to the instrument's initial check out period in the first months of its launch. In NASA's view, such a policy promoted ASTER, promoted NASA, and promoted the value-added services industry for remote sensing in agreement with the Land Remote Sensing Act of 1992. Furthermore, the ASTER MoU allowed for such a policy, with its wording of "no more than" the marginal cost.

Yet, ERSDAC wanted to recover some of their costs by charging for ASTER data.[158] If NASA freely distributed ASTER data, users who were not otherwise committed to receiving imagery from ERSDAC (especially private firms outside of either Japan or the United States) would largely request their ASTER imagery from NASA. Given Japan's earlier expectations about pricing, Japan unsurprisingly

---

[156] This norm was discussed—usually when an imbalance was a matter of concern—in the meetings of the U.S.-Japan science scheduling support group (author's notes, particularly from the 24th ASTER science team meeting, May 21, 2003).
[157] Interview (2005).
[158] Yamaguchi (2002), Watanabe (2003), and Interview (2005)

protested NASA's free distribution of ASTER's data, dramatically reading a letter of objection at the opening plenary sessions of joint team meetings, often after the representative from NASA Headquarters spoke, if one attended.[159] The situation was "ugly, ugly, ugly," in the opinion of one U.S. team member.[160] A member of the Japan team said "we felt like Japan was being treated as a U.S. subsidiary."[161] After being lobbied by *both* the Japan *and* the U.S. teams, NASA Headquarters made an exception to their general EOS data policy for the case of ASTER imagery.[162] After years of diplomacy, MITI and NASA ultimately agreed to pricing policies that were roughly comparable (exchange rates fluctuate, after all).[163] The two teams had not only configured equality of consumption into the international political economy of the ASTER data and information system, but in this important issue at least, they governed as a transnational authority using a normative notion of partnership and equality.

As a consequence of the new pricing policy, NASA's monthly distribution of ASTER's basic data product dropped "in dramatic fashion" to one percent of what it had been before the pricing policy was implemented.[164] NASA, which had always been conscious of public outreach for the EOS program, regarded the massive reduction in its distribution of ASTER data to the public as a significant political cost. According to one individual involved in the negotiations from the U.S. side, NASA's equalizing of pricing policies was designed to emphasize "a basic notion of fairness" in order to open up the door for a future change to the data and information system which the U.S. team would propose to the Japan team and to ERSDAC; this change

---

[159] I observed a reading of the protest letter at the opening plenary session of 20th ASTER science team meeting, May 22, 2001.
[160] Abrams (2001).
[161] Yamaguchi (2002).
[162] Abrams (2001) and Yamaguchi (2002)
[163] Baily and Duda (2002)
[164] Bailey and Duda (2002: slide 10)

was a delicate matter that concerned the United States re-processing arguably flawed imagery that it had received from Japan.[165] A quid-pro-quo, however, had not been suggested, much less agreed upon, prior to NASA's decision to start charging for its imagery.[166] NASA's and the U.S. team's move to start charging for ASTER imagery distributed through the U.S. DAAC in the United States was normative as well as strategic. They considered it an important move to strengthen the U.S.-Japan relationship and the ASTER team's transnational authority by showing in a clear way that the United States valued a balanced partnership with Japan. While the Japan and the U.S. teams' notions of an equitable U.S.-Japan partnership did not always have such clear stakes, the use of that notion in their technoscientific diplomacy to configure and enact an equitable international political economy through their data and information system was not unusual.

The Utility of a Normative Notion of a U.S.-Japan Partnership

In addition to working out a scheme to specify how much data various categories of U.S. and Japan users could consume and in addition to agreeing upon the cost, the teams also had to negotiate issues concerning who could use various instrument capabilities and how much of those scarce capabilities they could consume. The capability of the ASTER instrument to point its three telescopes a limited number of times over its expected life was just such an issue. The two teams needed to decide the way in which the consumption of ASTER's scarce pointing maneuvers would be regulated. Their technoscientific diplomacy over this issue deployed technical reasoning and asserted and ascribed state power, and the closure of two teams'

[165] Abrams (2003) and Interview (2005)
[166] Interview (2005).

discussions concerning this issue was facilitated by the enactment of a normative notion of a U.S.-Japan partnership.

Questions about how to regulate ASTER's pointing capabilities came to a head in March of 1997, a couple of years before ASTER's launch, when the team was evaluating the (simulated) performance of their routines for scheduling ASTER's observations. At a joint Operations and Mission Planning Working Group meeting, a mission analyst working at and for ERSDAC as a contractor explained that the scheduling algorithm that his firm was developing for the system was expected to be too slow. Because the U.S. and Japan teams had not finished collecting what they considered to be "realistic" data requests with which to run scheduling simulations, no one knew how slow the scheduling software was, or even if it was actually slow.[167] But the two teams suspected that the scheduler was indeed slow, particularly because they had been experiencing other problems with the scheduler that ERSDAC's diligent but "overcommitted" contractor was developing.[168]

One way of speeding up the processing was to be especially strict about when the scheduler would consider observations that required the visible telescope to point at large angles.[169] Eliminating large-angles eliminated many more potential observation schedules that would need to be considered in the scheduler's search to find an optimal one-day schedule. The two teams had already agreed that large-angle pointing observations were appropriate only in emergencies, such as to monitor an erupting volcano or a forest fire. But now they had to specify and implement this general guideline. They left the meeting with this issue unresolved, and the Operations

[167] ASTER Operations and Mission Planning Working Group (1996: 1); ASTER Operations and Mission Planning Working Group (1997a: 4-6); and Ohno (1997).
[168] Cohen (1996) and the quote is from ASTER Operations and Mission Planning Working Group (1996: 2).
[169] Pniel (1997: 4) and Yamaguchi (1997: 1)

and Mission Planning Working Group posted it as their 116th action item.[170] This action item was an important one for the working group, but it was only one in a long list of action items that had been opened and closed over their years of collaboration.

A couple of months later before the next joint team meeting, the Japan co-chair of the joint working group, Yamaguchi Yasushi, e-mailed the U.S. co-chair of the working group, Moshe Pniel. To resolve the question about how to restrict the use of large-angle pointing, Yamaguchi had proposed that only U.S. and Japan team members should be able to submit requests to use the large-angle pointing capability and that they should only be able to do so in the case of a specific kind of observation request. His proposal seemed to make a sharp distinction between who could and who could not use the visible telescope's capability to look sideways at large angles.[171]

The co-chair of the Operations and Mission Planning Working Group, Pniel, in a response over e-mail to Yamaguchi, stated that it was Pniel's and Kahle's recommendation that they discuss this issue with the two teams as a whole during the plenary session of the upcoming meeting.[172] Everyone involved with the development of the ASTER remote-sensing system—the U.S. and Japan teams, ERSDAC and JAROS managers, even sometimes representatives from NASA Headquarters—typically attended the plenary session of team meetings. Kahle and Pniel desired to retain the flexibility of the large-angle viewing for users other than the two teams, and they suspected that the U.S. and Japan teams as a whole would agree with them. They also thought that the two teams as a group should determine a method by which the data and information system could calculate when to limit the use of the large-angle pointing. Kahle and Pniel wanted the above-mentioned scheduling program of the data and information system, which the ERSDAC contractor was

---

[170] ASTER Operations and Mission Planning Working Group (1997a: 8)
[171] Pniel (1997: 2-4)
[172] Ibid., p. 3.

developing, to integrate this calculation into its scheduling routines. This proposal for a technological fix was e-mailed to Yamaguchi.[173]

Yamaguchi e-mailed a response to Kahle and Pniel, reiterating his position on the matter, arguing that they should not ask the two teams about this question at the upcoming U.S.-Japan joint team meeting before the joint working group had come to a position among themselves. He had reasoned that it was a "very political matter" and that it was best for the working group to present a single rationale to the two teams together as a group.[174] Yamaguchi's hesitancy to broach the question of how to regulate the consumption of ASTER's pointing capabilities during a plenary session stemmed from a number of general concerns: 1) JAROS engineers had already requested that the ASTER instrument, for whose technical health they were directly responsible, not be needlessly taxed with pointing maneuvers; opening up pointing capabilities to general users was a risk in that regard; 2) industry users associated with ERSDAC had been concerned that emergency pointing maneuvers would detract from their more regular and less-time dependent observation goals; and 3) as was mentioned above, ERSDAC's contractor for the scheduling program for ASTER was already overcommitted and was having some problems developing the complex scheduler. Because Yamaguchi did not list in his e-mail these general concerns, concerns which had been explicitly voiced in other forums in which Yamaguchi was involved, the salience of these concerns is a matter of interpretive judgment. It is certainly reasonable to assume, however, that these concerns would have complicated any discussion of the issue during a plenary session. For different reasons, many parties had a stake in this issue. Wide-ranging discussions at the relatively large plenary

[173] Ibid, p. 4-6.
[174] Ibid., p. 5.

session risked a contentious scene in which Japan's two consortia would be pitted against scientists from both the Japan and U.S. teams.

In his e-mail to Kahle and Pniel, Yamaguchi suggested that he feared that any discussion at the plenary would highlight ERSDAC's and its contractor's inability to develop, given their tight budget allocations, a scheduler that could fulfill what would likely be a request from both teams to have the scheduling software manage the large-angle pointing issue.[175] Yamaguchi's alternative proposal, to decide at the working group level to restrict pointing maneuvers for everyone but team members, had the merit of ensuring that the general consumption of ASTER's limited pointing maneuvers would be strictly limited without burdening the scheduling software. He also proposed that exceptions to this rule would be allowed on a case-by-case basis, to be determined by a joint committee. If that was not going to be the procedure for regulating pointing maneuvers, however, Yamaguchi wanted to know in advance of any plenary session what the procedure would be.[176]

Yamaguchi's response pushed Kahle, Pniel, and others at JPL who were discussing the issue to name their specific objections to Yamaguchi's proposal. They disagreed with Yamaguchi's proposal on the grounds that it would appear to favor the two teams—in their words, "the insiders"—over the general users. And on that basis, they would expect to receive complaints. EOS teams were, in general, under consistent pressure by NASA Headquarters to level user hierarchies and avoid insider advantages. But as we have seen in the case of, for instance, data acquisition allocations, that demand for leveling only goes so far. In addition to exacerbating possible outsider complaints about a system that would be alleged as rigged for the insiders, the U.S. team argued that Yamaguchi's proposal would require a joint team working group to

---

175 Ibid.
176 Ibid., p. 7.

evaluate every urgent request, instead of leaving it to the computer processing system to calculate whether or not an urgent request would fit into the schedule.[177] The U.S. members of this scheduling committee did not want to have any direct agency in this scheduling calculation. Instead, they preferred to leave it to computer algorithms to sort out, algorithms which would of course need to be designed and agreed upon in advance.

Yamaguchi persisted with his position, pointing out that other insider/outsider discrepancies were already embedded into the U.S.-Japan team's user classification scheme.[178] He closed by writing "if you [Pniel and others] still want to nominate this issue [for the plenary], we agree, but please understand our feeling which were mentioned in this and previous mail."[179] Notes on the U.S. team's discussion show that the plea to "understand our feeling" solidified the issue as not just a request from Yamaguchi but as a request from Yamaguchi speaking for "Japan."[180] Yamaguchi, whose English was excellent, often served as a spokesperson for the Japan team in negotiations with the U.S. team. Pniel reported to the U.S. team that it was "Japan's STRONG feelings that this was a closed issue" (emphasis in original).[181] Yamaguchi's rhetoric had ascribed primacy to a U.S.-Japan partnership, arguing that that partnership should trump any domestic insider/outsider concerns that the U.S. team had. Yamaguchi's enactment of this norm did not allow the U.S. team to easily insist that ERSDAC fund the integration of a new calculating routine into a complex scheduling program that ERSDAC's contractor was already having problems developing. Would the U.S. team deny the importance of this U.S.-Japan partnership that buttressed their transnational authority? Ultimately, after the U.S. team members discussed the matter,

177 Ibid., p. 6.
178 Ibid., p. 7-8.
179 Ibid., p. 7.
180 Ibid., p. 9.
181 U.S. ASTER Science Team Meeting (1997: 3)

Kahle and Pniel decided that they would "concede" to what by now had moved from being just Yamaguchi's request to the "Japan team's position." The U.S. team wanted to avoid any further escalation of the issue. Nevertheless, they thought it amounted to what they characterized as a "loss of science." They strategized, however, that that concession would leave enough good will for them to "go for" another issue of importance to the U.S. team leader regarding the international political economy of the ASTER data and information system.[182] Yamaguchi's proposal became policy, and rather than going for a technological fix, the two teams further enacted partnership in the U.S.-Japan relationship and enhanced the ASTER team's transnational authority in the particular socio-political practices of the two teams: on a case-by-case basis, an ASTER team working group would evaluate requests for observations that required using the visible and near infrared telescope at large angles and through that practice the ASTER team would configure yet another aspect of consumption in the international political economy of the ASTER data and information system.

Transnational Authority and Socio-Political Interdependency

As the ASTER science team designed and maintained the ASTER data and information system, they also configured and governed that system's international political economy. The organization and substance of this international political economy was not primarily determined by the decisions of "high-level" senior officials from U.S. and Japan state bureaucracies such as NASA Headquarters or MITI, or even by managers in their respective offices of international affairs. The two science teams, using documents written by, for instance, Nichols, Geller, and Satō, negotiated

[182] U.S. ASTER Science Team (1997: 3) and ASTER Operations and Mission Planning Working Group (1997b).

an "ASTER centric" data and information system and sought to establish a "common understanding" about its design and purpose. Settling questions about the international political economy of the ASTER remote-sensing system was an inherent part of that technoscientific diplomacy. Neither the NASA-MITI MoU nor the GSFC-ERSDAC PIP set out, in advance of the two teams' diplomacy, a framework for their configuration of the ASTER system's international political economy.

While high-level documents asserted "clean interfaces" for the U.S.-Japan relationship in the ASTER remote-sensing system, the construction of clean interfaces was neither natural nor easy, and the prescription of clean interfaces left the two teams to work out the politically and technically significant issues of standardization and interoperability which might allow interfaces to be "clean." After discussing with the Japan team possible designs for the level 1 data products, the U.S. team was the driver behind the U.S.'s hedge during the negotiations over the MoU. They pushed the United States to have a level 1 processing capability, in part so the U.S. team could have "visibility" into the level 1 data products and vouch for the integrity of those data products that would flow between Japan and the United States. The two team leaders, Tsu and Kahle, and not the MoU or PIP, pushed the two teams to have "closer collaboration" and to enact U.S.-Japan partnership. The language in the PIP that tasked the ASTER team with negotiating common ATBDs had been in fact crafted by Kahle and Nichols before the call for closer collaboration at the sixth joint team meeting. Furthermore, the MoU and PIP were signed only after closer collaboration had been achieved in practice. In the Temperature-Emissivity Working Group, although members of the U.S. and Japan teams had competing preferences for how to deal with the indeterminancy of temperature and emissivity separation—preferences that aligned with their professional interests and which were embedded in their respective communities of practice—the working group was able to make good on Tsu's and

Kahle's call for "closer collaboration" by building on the nucleation site that was Matsunaga's MMD method. The working group also exercised transnational authority over successive versions of their U.S.-Japan hybrid algorithm, although the authority of their working group was more limited when it came to updating the algorithm's implementation.

U.S.-Japan transnational authority and the enactment of U.S.-Japan partnership extended as well into the configuration and governance of consumption in the ASTER systems' international political economy. In particular, the two teams successfully pushed for equality in the pricing arrangements for the U.S. and Japan data products, when the political and economic interests of NASA and MITI diverged. Even in the more mundane task of negotiating how the scarce pointing resources of the ASTER instrument would be distributed, a normative notion of U.S.-Japan partnership had utility for the two teams and buttressed their exercise of transnational authority.

Neither Latourian actor-network theory nor the epistemic communities approach can account for how the two teams' configured ASTER's international political economy or for why they negotiated the above outcomes. The Hobbesian realism of Latourian actor-network theory suggests that NASA and the U.S. team should have dominated most of these issues, given the overwhelming disparity between the center of calculation of GSFC and the U.S. DAAC for ASTER on one side and the center of calculation of ERSDAC and the Japan DAAC for ASTER on the other side. GSFC was able to impose many EOS software standards onto ERSDAC, especially since the development of ERSDAC's data and information lagged the development of GSFC's EOSDIS. Yet, for level 1 processing, ERSDAC retained flow of the production pipeline and agreed to produce only 40% of the level 1B products. Moreover, the U.S. team did not dictate the definition of the level 1 data products. In the design of the temperature and emissivity standard data products, neither Gillespie's

NEM nor Hook's ADE won out. TES was a thoroughly hybrid algorithm. From the perspective of Latourian actor-network theory, this outcome is especially surprising since the laboratory that created the empirical emissivity data for the statistical matching of the ADE method was at JPL. No counter-laboratory and counter-emissivity claims arose in Japan, and yet Matsunaga's MMD additional equation served as the nucleation site for TES. Finally, given the significant difference in the market power of the U.S. and Japan DAACs, and that the U.S. had a level 1 processing capability, the U.S.'s concession on pricing arrangements cannot be accounted for by Latourian actor-network theory.

The extensive technoscientific diplomacy described in this chapter suggests that the epistemic community approach is right to call attention to the work of epistemic communities in order to understand international policy coordination. While the chapter did not examine "policy coordination" outside the realm of the international earth observing system, we could imagine how ASTER remote-sensing data might feed into policymaking about such issues as climate change, urban planning, and even international security (reportedly, ASTER standard data products were used to assess the extent of bombing and fires in and around Baghdad during the Iraq War of 2003). Clearly, however, the depth of difference between the two teams' communities of practice as borne out in their technoscientific diplomacy belies the epistemic communities approach's core assumptions of shared "principled" and "causal" beliefs. Moreover, there is no reason to think that the ASTER team is exceptional in its initial lack of epistemic and normative solidarity. More importantly, if the epistemic communities approach were adopted to understand, for example, why NASA and MITI ultimately harmonized their pricing policies, the account would misunderstand why the two teams pushed in the first place for that issue to be resolved. It was not because they idealistically believed EOS data should be priced at the same “marginal cost” everywhere (overlooking for the moment that the calculation of “marginal cost” depends upon local arrangements), but because the U.S. team wanted to enact “a basic notion of fairness” so that they could continue to do business with the Japan team. The U.S. and Japan teams’ work was political as well as scientific.

In contrast to Latourian actor-network theory and the epistemic communities approach, this chapter’s analysis of the two teams’ technoscientific diplomacy accounts more compellingly for how the two teams designed the ASTER data and information system and configured that system’s international political economy. I also argue that this “how” account can better explain why the two teams reached the scientific, political, and technical settlements that they did. The explanatory sketch of technoscientific diplomacy emphasizes: the moves of liminal state actors; their enactment of international relations, especially their enactment of state power and normative notions; and the mixing of scientific claims and assertions of power. It can indeed offer “because” answers to “why” questions: The U.S.-Japan level 1 compromise was reached because, owing to the U.S. team’s push for “visibility” on the grounds of assuring scientific integrity and the Japan team’s redesign of the level 1 data products, NASA and MITI were able to negotiate a tension between the U.S. side’s desire for an independent level 1 processing capability and the Japan side’s desire for primacy in level 1 processing. The U.S. and Japan members of the temperature and emissivity working group developed the hybrid TES because Tsu and Kahle—perhaps for their own strategic reasons—pushed the working group to enact U.S.-Japan partnership. Yet, because Matsunaga’s MMD method served as a nucleation site with assumptions that had the imprimatur of members from both teams, the working group was eventually able to exercise transnational authority over the development of the TES hybrid. The U.S. team agreed with Yamaguchi’s proposal to limit large-angle pointing because his enactment of “Japan” and of a normative notion of U.S.-Japan partnership challenged the basis of the Operations and Mission Planning Working Group's transnational authority, which the U.S. team valued (in part, but not only, for strategic reasons).

The new "why" puzzle that this chapter leaves us with is why transnational authority was exercised in some negotiations and not others. Why did transnational authority and normative notions of U.S.-Japan partnership have persuasive force in, for instance, the Operations and Mission Planning Working Group and, eventually, the Temperature and Emissivity Working Group, but not in the negotiations over level 1 processing? I suggest that the persuasiveness and affirmation of transnational authority and of normative notions of U.S.-Japan partnership were predicated upon local socio-political interdependency in particular joint work.

This interdependency in joint work cannot be read alone from the arrangement of material artifacts, such as the interdependency in the architecture of the data and information system which shaped the negotiations over level 1 processing and production. While asymmetry in material interdependencies can give rise to power relations that enable coercion,[183] the kind of interdependency that fostered the use of normative notions of U.S.-Japan partnership and transnational authority in the two working groups noted above was a local interdependency. To some extent the two teams' enacted U.S.-Japan partnership in their joint work because conspicuous alternative paths were becoming more challenging (e.g., months after months of disagreement on those alternative paths; the mediocre peer-review of ATBD version 1.0 for temperature and emissivity). Yet, owing to joint work conducted around nucleation sites, normative notions of U.S.-Japan partnership and the exercise of transnational authority could become part of the work process. In the development of TES and in the negotiations over the regulation of pointing consumption, the two

[183] Keohane and Nye (1977) is the seminal work on this theme.

“sides” were problem-solving together, exchanging e-mails, code, faxes, and results that were salient to the task at hand of at least someone on the “other side” of an issue. Papers, diagrams, and presentations did not repeatedly drive home difference (such as in the “play offs” between NEM and ADE) as much as they were building an algorithm or object that was mutually-shared and mutually-owned. While this local socio-political interdependency does not of course guarantee the exercise of—much less the effectiveness of—transnational authority, they maintain its possibility rather than assuming the normality of state-centered, bilateral politics.

CHAPTER SEVEN

BETWEEN STATE AND TRANSNATIONAL COMMUNITY

This dissertation has sought to understand and explain the politics of technical decision-making and scientific judgment in the development of a U.S.-Japan remote-sensing system. It has argued that these politics can be best understood and explained using a new interpretative approach that the dissertation has developed, called "technoscientific diplomacy." This concluding chapter summarizes this new approach, argues the merits of its explanatory power in comparison to that of other approaches, and suggests the approach's broader applicability for investigating activities that are conducted in socio-political spaces between states and transnational communities.

The interpretative approach of technoscientific diplomacy has been used to highlight four qualities of the negotiating practices of the U.S. and Japan scientists and engineers who—on behalf of their respective states—developed, operated, and managed the ASTER remote-sensing system (see table 7.1 on the next page). First, the dissertation has conceptualized these scientists and engineers as neither state actors nor non-state actors, but as liminal state actors. As such, they often worked with the presumption of transnational community but at the threshold of the states of the United States and Japan. By situating their personal agency in relation to their respective states as well as transnational community, and by (re)articulating and deploying state goals in their decision-making and collective exercise of scientific judgment, they mediated international relations between the United States and Japan. Second, the approach of technoscientific diplomacy has been used to attend to how members of the ASTER team conducted international relations in and through their decision-making and collective exercise of scientific judgment. These scientists and engineers asserted scientific knowledge, ascribed and projected state power, and enacted U.S.-Japan relations in their decision-making. They also enacted U.S.-Japan relations in their work practices, in their design and operation of the ASTER remote-sensing system, and in the ASTER system's international political economy of scientific data.

Table 7.1: The Interpretative Approach of Technoscientific Diplomacy

1. Liminal state actors who (re)articulate state goals
2. Enactment of state power and international relations
3. Intertwining and synthesis of knowledge and power
4. Local conditions for transnational decision-making

The interpretative approach of technoscientific diplomacy has been used to highlight a third quality of the negotiating practices of the U.S. and Japan teams: interplay and interdependence between transnational knowledge production and politics among states. By closely tracing the intertwining of assertions and ascriptions of scientific knowledge that transcended states and assertions and ascriptions of state power in an international arena, the dissertation has used the approach to explain in detail the ASTER team's decision-making and collective exercise of scientific judgment. Finally, the approach of technoscientific diplomacy has been used to show how local socio-political interdependency between the U.S. and Japan teams could foster transnational authority and community, even if their realization was not in any way guaranteed. In particular, nucleation sites and normative notions of partnership and equality have been investigated as resources for the fostering of transnational authority and community. In sum, by using technoscientific diplomacy as an interpretative approach, the dissertation has provided an original and compelling account of how the U.S. and Japan teams worked together to build an international remote-sensing system. It has also contributed to our understanding of the politics of intergovernmental collaboration in science and technology more broadly.

Technoscientific diplomacy, however, is not just a new interpretative approach that has been used to persuasively account for the development of the ASTER remote-sensing system and illuminate interplay between knowledge production and politics among states. It is a better approach for explaining decision-making and the collective exercise of scientific judgment in intergovernmental collaborations in science and technology. This dissertation has used the approach of technoscientific diplomacy to provide detailed accounts of overlapping and interdependent episodes of decision-making and scientific judgment (see table 7.2 on the next page). Each of these episodes can be roughly captured by an explanatory sketch, and this explanatory sketch can then be compared with those that can be offered using other theoretical approaches. Recall from this dissertation's introductory chapter that an explanatory sketch is an interpretation that provides a rough account of some specific event, activity, or observation which can be compared and contrasted with other sketches of that event, activity, or observation and which can also offer lessons that can be adapted to other empirical contexts (p. 30). Table 7.3 (on p. 374) summarizes the expected outcomes of explanatory sketches for each episode for three different approaches: epistemic communities, actor-network theory, and technoscientific diplomacy.

The episodes of technoscientific diplomacy that have been described by this dissertation have had diverse outcomes, both in terms of the socio-political form of the decision-making that the two teams enacted in the episodes (table 7.2 and table 7.3) and in terms of the specifications that the two teams adopted for the design of the

Table 7.2: Episodes of Technoscientific Diplomacy

Chapter and Episode	Critical Years of Negotiation	Form of Decision-Making
Chapter Four: Thermal Infrared Radiometer (TIR)	1989	Bilateral
Chapter Five: Shortwave Infrared Radiometer (SWIR)	1990-1993	Bilateral
Chapter Five: ASTER Operational Capabilities (Ops)	1991-1993	Transnational
Chapter Six: Level 1 Product and Process (Level 1)	1992-1993	Bilateral
Chapter Six: Temperature – Emissivity Separation (TES)	1993-1995	Transnational
Chapter Six: User Consumption and Pricing (C & P)	1993-1997; 2001-2005	Bi/Transnational
Chapter Six: Regulation of Pointing Capabilities (Pointing)	1997	Transnational

Table 7.3: Outcomes Suggested by Alternative Interpretative Approaches

Episode	**Epistemic Community**	**Actor-Network Theory**	**Technoscientific Diplomacy (crafted to historical outcomes)**
TIR	EOS consensus, 1 or 2 bands	JAROS preference of 3 bands	Negotiated priority of 5 bands
SWIR	Working group consensus	Japan-proposed "optional" set	Modified "optional" band set
Ops	Engineered to EOS science	NEC's operational scenario	Engineering driven by science teams
Level 1	Open standards; joint distrib.	EOS standards; U.S. level 1 prod.	Negotiated standards; U.S. capability
TES	Either ADE or NEM	Either ADE or NEM (likely ADE)	TES hybrid (via MMD nucleation site)
C & P	Equal C; different P	U.S./Japan split, esp. in pricing	U.S.-Japan interdependency in C & P
Pointing	Consensus on algorithm	Consensus on algorithm	Jointly decided case-by-case
Satisfactory Explanatory Sketches?	**No**	**No**	**Yes**

ASTER remote-sensing system (table 7.3). A satisfactory explanatory sketch for each of these episodes must not only explain the "why" of these outcomes in a way that is consistent with its guiding interpretative approach, but it must also compellingly explain the "how": the dynamic, historical process that produced those outcomes. Thus, what it means to "compellingly explain the 'how'" is not simply the documentation of a plausible process that might provide the historically correct outcome (which is often the objective of using "process tracing" to construct "causal mechanisms"). Rather, an account of a process which is, itself, a history independent of the outcome contributes separately to the power of the explanatory sketch and to the adequacy of its guiding interpretative approach. In short, "how" as well as "why," and "process" as well as "outcome," need to be understood to explain persuasively the politics of decision-making and the collective exercise of scientific judgment in the development of the ASTER remote-sensing system.

According to this standard, the interpretative approach of technoscientific diplomacy is better able to explain the episodes listed in tables 7.2 and 7.3 than either the epistemic community approach or the Latourian actor-network theory of *Science in Action*. Chapters four through six of the dissertation have advanced this argument episode-by-episode and in detail. Here, in order to examine as a whole the general merits of technoscientific diplomacy vis-à-vis those alternative approaches, I compare just the explanatory sketches of a few of these episodes.

The negotiations over the thermal infrared radiometer perhaps illustrate best the merits of conceptualizing scientists and engineers as liminal state actors, of examining their (re)articulation and deployment of state goals, and of explaining outcomes in terms that account for the intertwining and synthesis of knowledge and power (i.e., qualities #1 and #3 in table 7.1). Only by accounting for how Dr. Kahle as a liminal state actor was able to articulate her team's goals as the goals of NASA

Headquarters as well as those of an international community can we understand why the thermal infrared radiometer has five bands, rather than one, two, three, or seven, and why neither the radiometer's signal-to-noise ratio nor its spatial resolution was improved. In particular, Kahle's cover page to her white paper for Dr. Ishii, which was copied to the EOS program office at NASA Headquarters, had listed increasing the number of thermal bands as the top priority for EOS regarding improvements to the radiometer, even when the EOS program office at NASA Headquarters would have apparently been satisfied with just two bands.

The epistemic community approach, however, would counterfactually presume solidarity among the EOS epistemic community, and this epistemic community would likely be represented by EOS advocates who circulated through the EOS program office at NASA Headquarters. Consequently, the approach would suggest that a NASA/EOS consensus of one or two bands would probably have been the agreed upon design. Like NASA Headquarters, MITI and MITI's JAROS also required fewer than five bands; they preferred no more than three. Five bands were expensive after all and were not needed to satisfy their "operational" goals of natural resource exploration and exploitation, according to both Dr. Ishii and Dr. Kahle. Given that MITI, JAROS, and their contractors were funding and building the instrument in their laboratories, Latourian actor-network theory would suggest that MITI and JAROS had all the material resources that they needed to counter Kahle's claims with a longer network of facts in a "trial of strength." And actor-network theory cannot account for Kahle's strategy to negotiate what scholars in Science and Technology Studies would call a boundary object. Latourian actor-network theory would counterfactually suggest that Kahle would have sought to "translate" MITI's interests into hers and "enroll" MITI in her project, but she did not. Neither the epistemic community approach nor the Latourian actor-network theory of *Science in Action* is suited for explaining the negotiations over the thermal infrared radiometer. In contrast, the interpretative approach of technoscientific diplomacy is much more able to explain the TIR episode persuasively. This is not, perhaps, surprising because it was in part inductively developed around this episode; its performance on the other episodes or on a different case entirely is a better test of its relative superiority.

The two teams' negotiations over the shortwave infrared radiometer and the ASTER instrument's operational capabilities illustrate the significance of the second and third qualities of technoscientific diplomacy listed in table 7.1. By examining how the teams' enactments of state power and assertions of scientific knowledge were circumscribed and bolstered by the social make-up of the ASTER team, technoscientific diplomacy is able to account for the difference in the outcomes of the two negotiations, both of which occurred in the same period of time. In the episode concerning the shortwave infrared radiometer, consensus arose not through U.S.-Japan agreement on the issue that was under debate, but through termination of the bilateral debate; the U.S. members of the Geology Working Group ascribed power to "Japan" owing to the institutional divide between engineers and scientists in the Japan team and the supposed stubbornness of decision-making processes in Japan. Agreement did not arise through consensual knowledge and principles among working group members or through a political solution on the part of ministry bureaucrats, as the epistemic community approach would suggest with its experts-then-policymakers framework for understanding the process of policy coordination. Latourian actor-network theory can more closely account for the episode, especially the Japan team's unilateral insistence on the "optional" band set and the Japan team's mustering of evidence from the laboratories of its contractors. Still, the approach's Hobbessian leanings misses the crucial importance of compromise and the U.S. team's acquiescence for the sake of future community, both of which account for why the "optional" band set was modified closer to the preferences of the U.S. team.

In the episode concerning the ASTER instrument's operational capabilities, the approach of technoscientific diplomacy can be used to illuminate why Dr. Miyazaki's trading tables served as a nucleation site and fostered transnational authority in decision-making and collective scientific judgment: Miyazaki borrowed visually-intelligible and flexible operational scenarios from the U.S. team and distanced himself from the cramped and rigid operational scenarios provided by NEC. In addition, Dr. Miyazaki consistently came to the support of the Japan team. Dr. Kahle and others on the U.S. team recognized the opportunity presented by this mix of technical assertions and political power and used it to advance transnational decision-making, which was in the U.S. team's immediate and, arguably, long-term interest. As indicated in table 7.3, both the epistemic community approach and Latourian actor-network theory can not capture this dynamic. An EOS epistemic community was neither posited nor important in the episode, and the two science teams together prevailed against the significant material and network advantages of the hardware maker, NEC. While the U.S. team still had to receive most of their facts about the hardware from JAROS and NEC, the U.S. team's preferences regarding this issue became the ASTER team's preferences. The negotiation process that brought about this outcome called attention to local conditions, such as nucleation sites, which effect transnational community.

The fourth quality of the technoscientific diplomacy approach—the focus on interdependency and the local conditions for transnational decision-making—can be used to capture well the design of the temperature-emissivity separation algorithm as a U.S.-Japan hybrid, and more generally, the configuration of an international political order through the development of the ASTER data and information system. Although members of the U.S. and Japan teams had competing preferences for how to deal with the indeterminacy of temperature and emissivity separation—preferences that aligned with their professional interests and which were embedded in their respective communities of practice—the working group was able to make good on Dr. Tsu's and Dr. Kahle's call for "closer collaboration": the working group built on the nucleation site that was Dr. Matsunaga's MMD method. The epistemic community approach would suggest that the method that was most prominent and well-documented among the relevant epistemic scientific community—i.e., either the ADE or NEM method—would have likely been adopted. Furthermore, since the U.S. advocates of the ADE and NEM methods had more rhetorical resources from the literature and from the laboratory, Latourian actor-network theory also would suggest that either ADE or NEM would have prevailed. Because, however, the approach of technoscientific diplomacy explicitly takes into account the benefits of a U.S.-Japan hybrid that would address concerns of international collaboration and the change in technical practices that allowed MMD to evolve as a hybrid (e.g., moving away from the playoffs among the methods), it—unlike the epistemic community approach and Latourian actor-network theory—does not need to be stretched to explain the process and outcome of the development of the temperature-emissivity separation algorithm.

The power of this dissertation's interpretative approach of technoscientific diplomacy does not question the general insights or intellectual utility of either the epistemic community approach or Latourian actor-network theory. This study affirms the central insight of the epistemic community literature that scientists can shape politics as well as science. The approach of technoscientific diplomacy conceives of the relationship between the two enterprises differently, however: as two sides of the same coin rather than as separate components in a systematic belief system. Indebted to Latourian actor-network theory, the dissertation has followed scientists and engineers as they make technoscience, and the interpretative approach of technoscientific diplomacy is attentive to their rhetoric. Yet, the approach of technoscientific diplomacy also presses for the details of technical practice to be investigated and suggests an international politics between states in the collaboration, in which power is enacted as state power rather than as power that has force through its self-evident materiality, as is the case with Latourian immutable mobiles. Furthermore, the approach of technoscientific diplomacy directly investigates international politics between states in the practice of technoscience. Consequently, because technoscientific diplomacy's explanatory sketches better explain process as well as outcomes in the development of the ASTER remote-sensing system than explanatory sketches guided by the epistemic community approach or Latourian actor-network theory, I argue that the approach of technoscientific diplomacy promises a better understanding and explanation of the politics of technical decision-making and scientific judgment in intergovernmental collaboration more generally.

Given the particular context, however, of the development of the interpretative approach of technoscientific diplomacy, is it portable? That is, is it open-ended enough that it—like the epistemic community approach and Latourian actor-network theory—can be learned from and adapted to other empirical contexts? Provided that, first, core participants in the intergovernmental collaboration are liminal state actors, in that they are ostensibly not employees of the state but yet are working in some capacity for the state in which they can, as members of a transnational community, also assert their independence (e.g., scientists, doctors, teachers, international development specialists, etc.) and, second, that these participants are engaged in the development of technoscience, then I suggest that intergovernmental collaboration in which they participate can be analyzed as technoscientific diplomacy. The (re)articulation and deployment of state goals in practice can be followed; participants' abilities to enact state power and assert scientific knowledge in varying international contexts and in an intertwining way can be analyzed; and the fostering of transnational decision-making and authority—through nucleation sites and the assertion of norms—can be interpreted.

For example, both of the U.S.-Japan collaborations that have been described in chapter two—the FS-X fighter co-development project and the international space station—could be analyzed using the approach of technoscientific diplomacy and, through comparison, could illuminate the dynamics of scientific and technical development in international affairs. In the case of the FS-X fighter, the "U.S." contractor (i.e., Lockheed Martin) and the "Japan" contractor (i.e., Mitsubishi Heavy Industries) were both intimately involved in the co-development project. Based upon the discussion of the case in chapter two, it is very reasonable to surmise that they had opportunities to deploy state goals and enact state power in their technical discussions. In the development of the international space station, contractors have mediated intergovernmental communication and shaped state goals, based upon anecdotal evidence.[1]

Another conspicuous example is intergovernmental collaborations in non-proliferation which have been coordinated by organizations such as the International Atomic Energy Agency or the U.S. Department of Defense's Cooperative Threat Reduction program (i.e., the Nunn-Lugar program). For these collaborations, the interpretative approach of technoscientific diplomacy as outlined in table 7.1 might prove useful for explaining the processes and outcomes of decision-making, the collective exercise of scientific judgment, and how those

[1] Interview with NASA Headquarters official who wished to remain anonymous. He related a story in which a contractor had helped both the U.S.'s NASA and Japan's NASDA establish a common bilateral direction in the development of an element of the International Space Station.

processes and outcomes entailed international politics. These collaborations involved experts in weapons of mass destruction who were employed by national laboratories and universities and who were ostensibly—but far from determinatively in practice—joined together by non-proliferation goals and non-proliferation protocols. They were likely situated in international contexts, however, in which they not only fostered transnational community, but acted as liminal state actors, advancing state goals. These examples suggest that the interpretative approach of technoscientific diplomacy promises to be of broad utility for understanding and explaining science and technology in international affairs and, in particular, the intertwining of the creation of knowledge and international politics. Yet, only further empirical research can substantiate this promise.

APPENDIX

NASA's Letter of Introduction for Kahle to JAROS

(Tilford 1989)

TO: T.Tanaka
NASDA

Fax: 3-432-3969

Shohichi Hashimoto, JAROS
Fax: 3-459-1670

As you are aware, Dr. Anne B. Kahle of the Jet Propulsion Laboratory has been selected for definiton for her proposal entitled "Thermal Infrared Ground Emission Spectrometer" submitted in response to the NASA Announcement of Opportunity for the Earth Observing System (Eos).

Specifically, NASA has selected the Thermal Infrared Mapping Spectrometer (TIMS) but did not select the Non-Imaging Thermal Infrared Profiling Spectrometer (TIPS). Furthermore, NASA has asked Ms. Kahle to try to implement the design advances of her TIMS concept by working with you, and your design for ITIR.

In addition Ms. Kahle's proposal entitled "Development and Utilization of the ITIR for Geologic Application" submitted in response to the Japanese AO has been endorsed for selection under terms to be negotiated between NASA and the Science and Technology Agency of Japan. If her proposal if accepted by you, NASA plans to provide funding for her investigation as part of its overall Eos mission. As we had previously agreed, no announcement will be made of NASA's decisions to endorse proposals submitted to foreign AO's until the foreign agency formally announces it decisions.

For definition phase, the TIMS is being considered in the context of flight on NASA Polar Orbiting Platform 1 (NPOP-1) only as a potential integral technology improvement for ITIR. NPOP-1 is planned for launch during the fourth quarter of 1996. This portion of the Eos mission is managed by the Eos Project Office at the Goddard Space Flight Center. Given the relatively tight schedule between now and the planned launch of NPOP-1, it is important that definition studies proceed as quickly as possible.

If efforts to cooperate in achieving the best possible ITIR are agreeable to you, the Eos Program Office at NASA Headquarters would like to work with you to integrate the proposed TIMS technology into ITIR. During the initial discussions of the provision of ITIR at the La Jolla ICWG meeting, Dr. Butler indicated that NASA could not support any hardware contribution from the U.S. for ITIR and that you should not anticipate NASA support of any efforts to obtain export licenses to assist in the development of ITIR. In light of our current desire to see the best possible ITIR developed under Japanese leadership, NASA is willing to reconsider the question of export liscenses. Furthermore, despite our limited resources to pursue instrument development in the thermal infrared imaging area, NASA would also consider a limited hardware contribution if this is absolutely necessary to enable increases in ITIR performance. NASA remains firmly committed to flight of ITIR on NPOP-1 even if technology improvements such as those proposed by Ms. Kahle cannot be achieved.

The key product of a combined definition phase study would be a cohesive and technically sound plan demonstrating the feasibility of improved ITIR capabilities and an approach whereby the Japanese agree to implement this improved approach to ITIR with Ms. Kalhe's advice and U.S. hardware assistance only where absolutely necessary. Among the results of this definition phase effort will be improved science, management, data, calibration, and cost plans for the portion of the combined investigation.

All instrument investigations will require confirmation for execution phase. At that time, NASA and Japan will jointly determine the most appropriate role for Ms. Kahle to play in execution phase. This confirmation decision is planned for September 1990. This decision will depend on many factors including successful progress between Ms. Kahle and you on the ITIR effort, the overall financial, orbiting, and data system resources of the Eos mission, and the value of the scientific contribution from any enhanced investigation that is developed. Final assignment of ITIR to NPOP-1 will be subject to the procedures established under the Space Station MOU and the conclusion of an Eos/Polar Platforms MOU between the Earth observations offices represented on the ICWG.

Ms. Kahle would very much like to visit with you as soon as possible and discuss the folllowing:

Functional Requirements
Design
Construction
Calibration / Laboratory
Calibration / Inflight
Integration
Testing & Quality Assurance
Operation planning
Data Systems
Interface with the Eos Data & Information System
Standard Data Products
Atmospheric Corrections
Data Verification
and
Data Utilization and Analysis.

Ms. Kahle's address is as follows:

Anne B. Kahle
Jet Propulsion Laboratory
Geology Group MS 183-50

California Institute of Technology
4800 Oak Grove Drive
Pasadena, CA 91109

Please direct your responses and any questions you may have in this matter to:

Dr. Dixon M. Butler
Eos Program Scientist
Earth Science and Applications Division
Mail Code EE
NASA Headquarters
Washington, DC 20546

Phone: (202) 453-1681
Telemail: DBUTLER on NASAMAIL
Fax: (202)497-9843

Thank you very much.

S. G. Tilford
Director, Earth Science and Application Division
NASA Headquarters

REFERENCES

Note on Sources

The bibliography below lists all of the sources that are cited in the footnotes of the text, with the exception of public laws and web pages. These exceptions are, however, cited fully in the footnotes. Japanese-language sources are listed together with English-language sources in the bibliography, and translations of titles and corporate authors are provided. Most of the primary sources that are referenced in the dissertation were personally sought and collected by the author (e.g., minutes of meetings, overhead presentations at working group meetings, specification documents and their draft versions, instrument performance reports, contractor project management documents, statements of work, letters, faxes, and e-mails). To date, most of these sources cannot be found in major research libraries or in archives that are open to the general public, either in Japan or the United States.

Several documents in the bibliography were gathered from the library of Japan's National Space Development Agency (now the Japan Aerospace Exploration Agency) in Tsukuba, Japan and from the library and archives of the Jet Propulsion Laboratory in Pasadena, California. Some documents were retrieved from NASA's Goddard Space Flight Center with the assistance of a Freedom of Information Act request. Many primary sources likely can be found in the private collections of Japan's Earth Remote Sensing Data Analysis Center (formerly the Earth Resources Satellite Data Analysis Center), Japan Resources Observation System organization, or the Jet Propulsion Laboratory (the documents that can still be found at JPL may or may not eventually be made available in JPL's archive). A few of the scientists involved in the ASTER collaboration might still retain personal copies of some documents. Finally, the author has in his possession either a hardcopy or scanned copy of every document that is listed in the bibliography which cannot be found through a major research library. These documents can be made available upon request.

Preceding the bibliography are a list of meetings observed by the author and a list of interviews and personal correspondence. The interview list does not include a handful of interviews with individuals from the Japan Defense Agency, NASA Headquarters, the U.S. Department of Defense, and the U.S. Embassy in Japan who preferred to remain anonymous. These anonymous interviews are not central to the dissertation. The interviews listed below ranged from 45 minutes to 3 hours in length, and they were conducted in English, in Japanese, or sometimes, in both English and Japanese, as is noted.

Meetings Observed

The 20th U.S.-Japan ASTER Science Team Meeting in Tokyo (Aoyama), Japan, May 22-24, 2001

U.S.-Japan ASTER Calibration Team [i.e., Working Group] Meeting in Tokyo (Kachidoki), Japan, October 2, 2001 (at ERSDAC)

The 24th U.S.-Japan ASTER Science Team Meeting in Tokyo (Aoyama), Japan, May 21-23, 2003

Earth Remote Sensing Data Analysis Center Annual Business Meeting in Tokyo (Aoyama), Japan, June 3, 2003

The 25th U.S.-Japan ASTER Science Team Meeting in Tokyo (Aoyama), Japan, June 14-17, 2003

The 27th U.S.-Japan ASTER Science Team Meeting in Tokyo (Yurakucho), Japan, June 6-9, 2005

Interviews and Personal Correspondence

Abrams, Michael J. 2001. Interview by author, Pasadena, CA, May 30.

———. 2003. Interview by author, Pasadena, CA, August 5.

———. 2005. Personal correspondence with author, September 9.

Alley, Ron E. 2001. Interview by author, Pasadena, CA, May 30.

Anderson, Tom. 2001. Interview by author, Greenbelt, MD, October 23.

Arai Kōhei. 2002. Interview by author in Japanese and English, Saga, Japan, October 1.

Biggar, Stuart F. 2001. Interview by author, Tokyo, Japan, October 3.

Butler, Dixon M. 2005. Personal correspondence with author, June 18.

Cripe, Richard. 2001. Interview by author, Tokyo, Japan, July 12.

Dehghani, Navid. 2001. Interview by author, Pasadena, CA, May 31.

Eng, Bjorn. 2001. Interview by author, Pasadena, CA, May 31.

———. 2003. Interview by author, Pasadena, CA, August 7.

Fielding, Eric. 2001. Interview by author, Pasadena, CA, June 1.

Fujisada Hiroyuki. 2003. Interview by author in Japanese, Tsukuba, Japan, March 27.

Geller, Gary N. 2003. Interview by author, Pasadena, CA, August 7.

Gubbels, Timothy. 2005. Interview by author, Tokyo, Japan, June 8.

Hook, Simon J. 2001. Interview by author, Pasadena, CA, June 4.

———. 2006. Personal correspondence with author, June 14.

Iona, Glenn. 2001. Interview by author, Greenbelt, MD, October 19.

Ishii Yoshinori. 2003. Interview by author in Japanese, Tokyo, Japan, June 2.

Iwasaki Akira. 2002. Interview by author in Japanese, Tsukuba, Japan, October 3.

Johnson, Carol. 2001. Interview by author, Gaithersburg, MD, October 22.

Kahle, Anne B. 2003. Interview by author, Pasadena, CA, August 4.

———. 2005. Interview by author, Phone call to Castle Rock, WA, August 26.

Kudoh Masahiko. 2003. Interview by author in Japanese, Tokyo, Japan, June 9.

Maekawa Masatoshi. 2003. Interview by author in Japanese, Tokyo, Japan, June 10.

Maldonado, Leon. 2003. Interview by author, Pasadena, CA, August 5.

Matsunaga Tsuneo. 2003. Interview by author in Japanese, Tsukuba, Japan, March 28.

———. 2006. Personal correspondence with author, June 8.

Miyazaki Yoshinori. 2003. Interview by author in English and Japanese, Tokyo, June 2.

Muraoka Hiroyasu. 2003. Interview by author in Japanese, Tokyo, Japan, June 10.

Nichols, David A. 2001. Interview by author, Pasadena, CA, June 4.

Okada Ken'ichi. 2003. Interview by author in Japanese, Tokyo, Japan, June 10.

Palluconi, Frank D. 2001. Interview by author, Pasadena, CA, May 30.

Pieri, David. 2003. Interview by author, Pasadena, CA, August 7.

Pniel, Moshe. 2001. Interview by author, Pasadena, CA, May 31 and June 1.

Rokugawa Shuichi. 2003. Interview by author in Japanese, Tokyo, June 6.

Sakuma Fumihiro. 2003. Interview by author in Japanese, Tsukuba, Japan, March 38.

Schieldge, John P. 2001. Interview by author, Pasadena, CA, June 1.

Schwaller, Matthew. 2001. Interview by author, Greenbelt, MD, October 23.

Shaffer, Lisa R. 2006. Personal correspondence with author, June 14.

Takenouchi. 2003. Interview by author in Japanese, Tokyo, Japan, May 30.

Thome, Kurtis J. 2001. Interview by author, Tokyo, Japan, October 2.

Tsuchida Satoshi. 2002. Interview by author in Japanese, Tsukuba, Japan, September 26.

Urai Minoru. 2002. Interview by author in Japanese, Tsukuba, Japan, September 26.

Watanabe Hiroshi. 2003. Interview by author in Japanese, Tokyo, Japan, June 3.

Yamaguchi Yasushi. 2002. Interview by author, Nagoya, Japan, September 17.

Yasuoka Yoshifumi. 2003. Interview by author in Japanese, Tokyo, Japan, March 26.

Yokota Makoto. 2004. Personal correspondence with author in Japanese, November 15.

Bibliography

Abir-am, Pnina. 1993. From Multidisciplinary Collaboratiion to Transnational Objectivity: International Space as Constitutive of Molecular Biology, 1930-1970. In *Denationalizing Science: The Contexts of International Scientific Practice*, edited by Elisabeth Crawford, Terry Shinn, and Sverker Sorlin, 153-86. Vol. 16, *Sociology of the Sciences*. Dordrecht: Kluwer Academic Publishers.

Abrams, Michael J. 2001. Interview by author, Pasadena, CA, May 30.

———. 2003. Interview by author, Pasadena, CA, August 5.

———. 2005. Personal correspondence with author, September 9.

Abrams, Michael J., Roger P. Ashley, Lawrence C. Rowan, Alexander F.H. Goetz, and Anne B. Kahle. 1977. Mapping of Hydrothermal Alternation in Cuprite Mining District, Nevada, Using Aircraft Scanner Images for Spectral Region 0.46 to 2.36 mu-m. *Geology* 5, no. 12: 713-18.

Abrams, Michael J., J. Conel, and Harold Lang. 1985. *The Joint NASA/Geosat Test Case Project*. 3 vols. Vol. 2. Tulsa, OK: American Association of Petroleum Geologists Bookstore.

Abrams, Michael J., Anne B. Kahle, Frank D. Palluconi, and John P. Schieldge. 1984. Geologic Mapping Using Thermal Images. *Remote Sensing of Environment* 16, no. 1: 13-33.

Abrams, Michael J., and Yamaguchi Yasushi. 1991. Geology W-G [handwritten version]. In *Minutes of the Second ASTER Team Meeting*, held at Tokyo, Japan, May 15-17: Attachment 43.

Adler, Emanuel. 1992. The Emergence of Cooperation: National Epistemic Communities and the International Evolution of the Idea of Nuclear Arms Control. *International Organization* 46, no. 1: 101-45.

———. 1997. Seizing the Middle Ground: Constructivism in World Politics. *European Journal of International Relations* 3, no. 3: 319-63.

———. 2005a. *Communitarian International Relations: The Epistemic Foundations of International Relations*. Edited by Barry Buzan and Richard Little. *The New International Relations*. London: Routledge.

———. 2005b. Communities of Practice in International Relations. In *Communitarian International Relations: The Epistemic Foundations of International Relations*, edited by Emanuel Adler, 3-28, *The New International Relations*. London: Routledge.

Advisory Committee on the Future of the U.S. Space Program. 1990. *Report of the Advisory Committee on the Future of the U.S. Space Program: Submitted to the Administrator of NASA*. Washington, D.C.: National Aeronautics and Space Agency. December 17.

Akasaka, Akira, Makoto Ono, Yasushi Sakurai, and Bun Hayashida. 1991. Short Wavelength Infrared (SWIR) Subsystem Design Status of ASTER. In *Future European and Japanese Remote-Sensing Sensors and Programs*, edited by Philip N. Slater, 269-77. Vol. 1490, *The Proceedings of SPIE*. Bellingham, WA: SPIE--The Society of Photo-Optical Instrumentation Engineers.

Alder, Ken. 1997. *Engineering the Revolution: Arms and Enlightment in France, 1763-1815*. Princeton: Princeton University Press.

Alley, Ron E. 2001. Interview by author, Pasadena, CA, May 30.

American Institute of Aeronautics and Astronautics. 1987. *The Civil Space Program: An Investment in America*. Washington, D.C.: American Institute of Aeronautics and Astronautics. December 23.

Amsterdamska, Olga. 1990. Surely You Are Joking, Monsieur Latour! *Science, Technology, & Human Values* 15, no. 4 (Fall): 495-504.

Anderson, Tom. 2001. Interview by author, Greenbelt, MD, October 23.

Arai Kōhei. 1985. Randosatto no shisei kettei seido kōshō [An Improvement of Attitude Determination Accuracy of Landsat]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 5, no. 2: 27-32.

———. 1992. Sciengineer no susume [A Recommendation for a Sciengineer]. *ERSDAC nyūsu [ERSDAC News]*, no. 32 (March): 4.

———. 2002. Interview by author in Japanese and English, Saga, Japan, October 1.

Ashley, Richard K. 1988. Untying the Sovereign State: A Double Reading of the Anarchy Problematique. *Millennium: Journal of International Studies* 17, no. 2: 227-62.

Asker, James R. 1990. Scientists Question NASA's Plans for Large Environmental Satellite. *Aviation Week and Space Technology* 133, no. 10 (September 3): 201.

ASTER Operations and Mission Planning Working Group. 1993. *Operations and Mission Planning Working Group Meeting Minutes*. Hilo, Hawaii. June 28-30.

———. 1996. *Very Brief OMPWG Meeting Narrative Notes*. Yokohama, Japan. December 5.

———. 1997a. *OMPWG Splinter Meeting Minutes*. Tokyo, Japan. March 5-7.

———. 1997b. *Operations and Mission Planning Working Group Narrative Minutes*. Sioux Falls, SD. May 22.

———. 2000. *STAR Guidelines*. October 20.

ASTER Science Team. 1990. *Minutes of the First ASTER Team Meeting*. Pasadena, CA. November 12-13.

———. 1991. *Minutes of the Second ASTER Team Meeting*. Tokyo, Japan. May 15-17.

———. 1992a. *Minutes of the Fourth ASTER Team Meeting*. Tsukuba, Japan. June 22-25.

———. 1992b. *Minutes of the Third ASTER Team Meeting*. Pasadena, CA. January 21-23.

———. 1993a. *Minutes of the 6th ASTER Science Team Meeting*. Tokyo, Japan. November 9-12.

———. 1993b. *Minutes of the Fifth ASTER Science Team Meeting*. Las Vegas, NV. February 2-5.

———. 1994. *Minutes of 7th ASTER Science Team Meeting*. Pasadena, CA. May 24-27.

———. 1995. *Minutes of the 9th ASTER Science Team Meeting*. Flagstaff, AZ. May 22-26.

———. 1996. *Minutes of the 11th ASTER Science Team Meeting*. Pasadena, CA. June 10-14.

———. 1997a. *13th ASTER Science Team Minutes*. Sioux Falls, SD. May 20-23.

———. 1997b. *Minutes of 14th ASTER Science Team Meeting*. Yurakucho, Japan. December 9-12.

———. 1999. *Narrative Minutes of the 16th ASTER Science Meeting*. Pasadena, CA. January 12-14.

Avellar, Dana Babette. 1993. *Japan: A Business Survival Guide*. Prepared for the U.S. ASTER Team. Jet Propulsion Laboratory.

Bailey, G. Bryan, and Ken Duda. 2002. Land Processes DAAC Report. Presented at *23rd ASTER Science Team Meeting*, December 3, held at Palm Springs, CA.

Barnes, Barry. 1981. On the 'Hows' and 'Whys' of Cultural Change (Response to Woolgar). *Social Studies of Science* 11, no. 4 (November): 481-98.

Becker, Francois. 1987. The Impact of Spectral Emissivity on the Measurement of Land Surface Temperatures from a Satellite. *International Journal of Remote Sensing* 8, no. 10: 1509-22.

Becker, Francois, and Zhao-Liang Li. 1990a. Temperature-Independent Spectral Indices in Thermal Infrared Bands. *Remote Sensing of Environment* 32: 17-33.

Becker, Francois, and Zhao-Liang Li. 1990b. Towards a Local Split Window Method Over Land Surfaces. *International Journal of Remote Sensing* 11, no. 3: 369-93.

Bhagwati, Jagdish, and Hugh T. Patrick, eds. 1990. *Aggressive Unilateralism: America's 301 Trade Policy and the World Trading System*. *Studies in International Economics*. Ann Arbor: University of Michigan Press.

Biagioli, Mario, ed. 1999. *The Science Studies Reader*. New York: Routledge.

Biggar, Stuart F. 2001. Interview by author, Tokyo, Japan, October 3.

Bimber, Bruce, and David H. Guston. 1995. Politics by the Same Means: Government and Science in the United States. In *Handbook of Science and Technology Studies*, edited by Sheila Jasanoff, Gerald E. Markle, James C. Petersen, and Trevor Pinch, 554-71. Thousand Oaks: Sage Publications.

Blanpied, William A. 2002. *A Brief History of the National Science Foundation's Tokyo Regional Office*. Tokyo: National Science Foundation Office, U.S. Embassy in Tokyo. August.

Bloom, Justin L. 1984. The U.S.-Japan Bilateral Science and Technology Relationship: A Personal Evaluation. In *Scientific and Technological*

Cooperation Among Industrialized Countries: The Role of the United States, edited by Mitchel B. Wallerstein, 84-110. Washington, D.C.: National Academy Press.

Bloor, David. 1991 [1976]. *Knowledge and Social Imagery*. 2nd ed. ed. Chicago: University of Chicago Press.

———. 1992. Left and Right Wittgensteinians. In *Science as Practice and Culture*, edited by Andrew Pickering, 266-82. Chicago: University of Chicago Press.

———. 1999. Anti-Latour. *Studies in History and Philosophy of Science* 30, no. 1: 81-112.

Boese, Wade. 2004. Japan Seeks Missile Defense Interceptors. *Arms Control Today* 34, no. 5 (June).

Bonnet, Roger M., and Vittorio Manno. 1994. *International Cooperation in Space: The Example of the European Space Agency*. Edited by Herbert Friedman. *Frontiers of Space*. Cambridge: Harvard University Press.

Borutzki, Suzan. 1994. *DAR Generation and Processing Concepts*. JPL D-11504. February.

Bromberg, Joan Lisa. 1999. *NASA and the Space Industry*. Edited by Roger D. Launius. *New Series in NASA History*. Baltimore: Johns Hopkins University Press.

Buckley, Roger. 1992. *U.S.-Japan Alliance Diplomacy, 1945-1990*. Edited by Steve Smith. Vol. 21. *Cambridge Studies in International Relations*. Cambridge: Cambridge University Press.

Bush, George H.W. 1988. *Presidential Directive on National Space Policy*. February 11.

———. 1989. *Remarks on the 20th Anniversary of the Apollo 11 Moon Landing*. July 20.

———. 1990a. *Remarks at the Closing Session of the White House Conference on Science and Economics Research Related to Global Change*. April 18.

———. 1990b. *Remarks at the Opening Session of the White House Conference on Science and Economics Research Related to Global Change*. April 17.

———. 1990c. *Remarks to the Intergovernmental Panel on Climate Change*. February 5.

———. 1992. *Space-Based Global Change Observation, National Space Policy Directive 7*. May 28.

Bush, Vannevar. 1960 (1945). *Science--The Endless Frontier: A Report to the President on a Program for Postwar Scientific Research*. Washington, D.C.: National Science Foundation.

Butler, Dixon M. 1989. NASA Mail message from the NASA EOS Project Scientist to L. Lincoln, Re: Message from Benn Martin, msg NJIJ-2847-2908, November 12.

———. 2005. Personal correspondence with author, June 18.

Butler, Judith. 1990. *Gender Trouble: Feminism and the Subversion of Identity*. *Thinking Gender*. London: Routledge.

Butow, Robert J.C. 1954. *Japan's Decision to Surrender*. Stanford: Stanford University Press.

Buzan, Barry, Charles Jones, and Richard Little. 1993. *The Logic of Anarchy*. New York: Columbia University Press.

Callon, Michel. 1986. Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St. Brieuc Bay. In *Power, Action and Belief: A New Sociology of Knowledge?* edited by John Law, 196-233. Vol. 32, *Sociological Review Monograph*. London: Routledge and Kegan Paul.

Callon, Michel, and Bruno Latour. 1981. Unscrewing the Big Leviathans: How Do Actors Macrostructure Reality? In *Advances in Social Theory and Methodology: Toward an Integration of Micro and Macro Sociologies*, edited by Karin Knorr and Aaron Victor Cicourel, 277-303. London: Routledge.

Chang, Edward. 1992. AM-1 Operations Concept. In *Minutes of the Fourth ASTER Team Meeting*, Plenary I, held at Tsukuba, Japan, June 22.

Chang, Edward, Debbie Ramey, and Rick Hatfield. 1992. *Earth Observing System (EOS)-AM1 Operations Concept Summary*. Greenbelt, MD: Goddard Space Flight Center. June 16.

Chase, Stillman. 1989a. Santa Barbara Research Center Internal Memorandum, Subject: report of trip to Japan to discuss the ITIR, March 7, 8, and 9, 1989, March 10.

———. 1989b. Santa Barbara Research Center Internal Memorandum, Subject: Trip to Japan: ITIR/TIGER Science Team Meeting; 11/25/89 to 11/30/89, December 5.

Chinworth, Michael. 1999. The Technology Factor in U.S.-Japan Security Relations. In *The U.S.-Japan Alliance: Past, Present, and Future*, edited by Michael J. Green and Patrick M. Cronin, 286-310. New York: Council on Foreign Relations Press.

Chinworth, Michael W. 1992. *Inside Japan's Defense: Technology, Economics & Strategy*. New York: Brassey's.

Clery, Daniel. 2003. Emerging From the Shadow of Abdus Salam. *Science* 300, no. 5617 (11 April): 241.

———. 2005. Fusion Leaders Make a Diplomatic Choice. *Science* 310, no. 5750 (11 November): 955.

Clery, Daniel, and Dennis Normile. 2005. ITER Finds a Home--With a Whopping Mortgage. *Science* 309, no. 5731 (1 July): 28-29.

Cline, Lynn F.H., and Graham Gibbs. 2003. Re-negotiation of the International Space Station Agreements--1993--1997. *Acta Astronautica* 53: 917-25.

Cloud, John. 2000. Crossing the Olentangy River: The Figure of the Earth and the Military-Industrial-Academic-Complex, 1947-1972. *Studies in the History and Philosophy of Modern Physics* 31, no. 3: 371-404.

Cloud, John, and Judith Reppy. 2003. Special Guest-Edited Issue on the Earth Sciences in the Cold War. *Social Studies of Science* 33, no. 5 (October): 629-33.

Cohen, Ron. 1996. Use of Cloud Simulation by Scheduler. In *Very Brief OMPWG Meeting Narrative Notes*, held at Yokohama, Japan, December 5.

Collins, H.M. 1981. Stages in the Empirical Programme of Relativism. *Social Studies of Science* 11, no. 1: 3-10.

———. 1992 [1985]. *Changing Order: Replication and Induction in Scientific Practice*. rev. ed. Chicago: University of Chicago Press.

Collins, Harry. 2004. *Gravity's Shadow: The Search for Gravitational Waves*. Chicago: The University of Chicago Press.

Collins, Harry M. 2001. Tacit Knowledge, Trust and the Q of Sapphire. *Social Studies of Science* 31, no. 1 (February): 71-85.

Collins, Harry M., and Steven Yearley. 1992. Epistemological Chicken. In *Science as Practice and Culture*, edited by Andrew Pickering, 301-26. Chicago: University of Chicago Press.

Colwell, Robert N., ed. 1983. *Manual of Remote Sensing*. 2nd ed. 2 vols. Falls Church, VA: American Society of Photogrammetry.

Committee on Science, Engineering, and Public Policy, Panel on the Government Role in Civilian Technology, National Academy of Sciences. 1992. *The Government Role in Civilian Technology: Building a New Alliance*. Washington, D.C.: National Academy Press.

Covault, Craig. 1996. Japan Accelerates Station Development and Promotion. *Aviation Week and Space Technology* 144, no. 5 (January 29): 62.

Crawford, Elisabeth, Terry Shinn, and Sverker Sorlin. 1993a. *Denationalizing Science: The Contexts of International Scientific Practice*. Edited by R.D. Whitley. Vol. 16. *Sociology of the Sciences*. Dordrecht: Kluwer Academic Publishers.

———. 1993b. The Nationalization and Denationalization of the Sciences: An Introductory Essay. In *Denationalizing Science: The Contexts of International Scientific Practice*, edited by Elisabeth Crawford, Terry Shinn, and Sverker Sorlin, 1-42. Vol. 16, *Sociology of the Sciences*. Dordrecht: Kluwer Academic Publishers.

Crawford, Neta C. 2002. *Argument and Change in World Politics: Ethics, Decolonization, and Humanitarian Intervention*. Vol. 81. *Cambridge Studies in International Relations*. Cambridge: Cambridge University Press.

Cripe, Richard. 2001. Interview by author, Tokyo, Japan, July 12.

Darst, Robert G. 2003. The Political Climate for Climate Science. *International Studies Review* 5, no. 1 (March): 83-86.

Data Products Working Group. 1993. Data Products Working Group Reporting Part in Plenary Session. In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 9-12.

Data Receiving, Processing, Archiving and Distribution Working Group. 1992a. Action Item List. In *Minutes of the Fourth ASTER Team Meeting*, held at Tsukuba, Japan, June 22-25: 65.

———. 1992b. Summary of the GDS Working Group [i.e., the Data Receiving, Processing, Archiving and Distribution Working Group]. In *Minutes of the Fourth ASTER Team Meeting*, held at Tsukuba, Japan, June 22-25: 54.

Dear, Peter. 2001. Science Studies as Epistemography. In *The One Culture? A Conversation about Science*, edited by Jay Labinger and Harry Collins, 128-41. Chicago: University of Chicago Press.

———, ed. 1991. *The Literary Structure of Scientific Argument: Historical Studies*. Philadelphia: University of Pennsylvania Press.

Dees, Bowen C. 1997. *The Allied Occupation and Japan's Economic Miracle: Building the Foundations of Japanese Science and Technology, 1945--52*. Richmond, Surrey: Japan Library, Curzon Press Ltd.

Defense Science Board Task Force. 1984. *Industry-to-Industry International Armaments Cooperation: Phase II - Japan*. Department of Defense, Office of the Under Secretary of Defense for Research and Engineering. June 26.

Dehghani, Navid. 2001. Interview by author, Pasadena, CA, May 31.

Dennis, Michael Aaron. 1994. "Our First Line of Defense": Two University Laboratories in the Postwar American State. *ISIS* 85, no. 3 (Sept.): 427-55.

———. 2003. Earthly Matters: On the Cold War and the Earth Sciences. *Social Studies of Science* 33, no. 5 (October): 809-19.

———. 2004. Reconstructing Sociotechnical Order: Vannevar Bush and US Science Policy. In *States of Knowledge: The Co-Production of Science and Social Order*, edited by Sheila Jasanoff, 225-53, *International Library of Sociology*. London: Routledge.

Dessler, David, and John Owen. 2005. Constructivism and the Problem of Explanation: A Review Article. *Perspectives on Politics* 3, no. 3 (September): 597-610.

Destler, I.M., Haruhiro Fukui, and Hideo Sato, eds. 1979. *The Textile Wrangle: Conflict in Japanese-American Relations, 1969-1971*. Ithaca, NY: Cornell University Press.

Doel, Ronald E. 1997. The Earth Sciences and Geophysics. In *Science in the Twentieth Century*, edited by John Krige and Dominique Pestre, 391-416. Amsterdam: Harwood Academic Publishers.

Doing, Park. 2004. 'Lab Hands' and the 'Scarlet O': Epistemic Politics and (Scientific) Labor. *Social Studies of Science* 34, no. 3 (June): 299-323.

Dower, John W. 1996a. The Bombed: Hiroshima and Nagasakis in Japanese Memory. In *Hiroshima in History and Memory*, edited by Michael J. Hogan, 116-42. Cambridge: Cambridge University Press.

———. 1996b. Three Narratives of Our Humanity. In *History Wars: The Enola Gay and Other Battles for the American Past*, edited by Edward T. Linenthal and Tom Engelhardt. New York: Metropolitan Books.

———. 1999. *Embracing Defeat: Japan in the Wake of World War II*. New York: W.W. Norton and Company.

Dozier, Jeff. 1990. EOSDIS. *The Earth Observer* 2, no. 1 (January): 10-14.

———. 1992. *The Earth Observer* 4, no. 3.

Edelson, Burton I. 1985. Mission to Planet Earth. *Science* 227, no. 4685 (January 25): 367.

Edelson, Edward. 1988. Laying the Foundation. *Mosaic* 19, no. 314 (Fall/Winter): 6-7.

Eden, Lynn. 2004. *Whole World on Fire*. Edited by Robert J. Art, Robert Jervis, and Stephen M. Walt. *Cornell Studies in Security Affairs*. Ithaca: Cornell University Press.

Elzinga, Aant. 1993. Antarctica: The Construction of a Continent by and for Science. In *Denationalizing Science: The Contexts of International Scientific Practice*, edited by Elisabeth Crawford, Terry Shinn, and Sverker Sorlin, 73-106. Vol. 16, *Sociology of the Sciences*. Dordrecht: Kluwer Academic Publishers.

Eng, Bjorn. 2001. Interview by author, Pasadena, CA, May 31.

———. 2003. Interview by author, Pasadena, CA, August 7.

Ergas, Henry. 1987. Does Technology Policy Matter? In *Technology and Global Industry: Companies and Nations in the World Economy*, edited by Bruce Guile and Harvey Brooks, 191-245. Washington, D.C.: National Academy Press.

Evangelista, Matthew. 1989. Issue-Area and Foreign Policy Revisited. *International Organization* 43, no. 1 (Winter): 147-71.

———. 1999. *Unarmed Forces: The Transnational Movement to End the Cold War*. Ithaca: Cornell University Press.

Evans, Peter B., Harold K. Jacobson, and Robert D. Putnam, eds. 1993. *Double-Edged Diplomacy: International Bargaining and Domestic Politics*. Vol. 25. *Studies in International Political Economy*. Berkeley: University of California Press.

Ezrahi, Yaron. 1990. *The Descent of Icarus: Science and the Transformation of Contemporary Democracy*. Cambridge: Harvard University Press.

Fawcett, Michael K. 1996. From Centralized to Distributed: The Evolution of Space Station Command and Control. *Acta Astronautica* 38, no. 4-8: 637-45.

Fearon, James D. 1998. Bargaining, Enforcement, and International Cooperation. *International Organization* 52, no. 2 (Spring): 269-305.

Fearon, James, and Alexander Wendt. 2002. Rationalism v. Constructivism: A Skeptical View. In *Handbook of International Relations*, edited by Walter Carlsnaes, Thomas Risse, and Beth A. Simmons, 52-72. Thousand Oaks, CA: Sage Publications.

Federation of Economic Organizations (Keidanren). 1988. *Ensuring a Viable and Productive Future for Japan's Space Program*. Tokyo. October 25.

Ferguson, James, and Akhil Gupta. 2002. Spatializing States: Toward an Ethnography of Neoliberal Governmentality. *American Ethnologist* 29, no. 4: 981-1002.

Fielding, Eric. 2001. Interview by author, Pasadena, CA, June 1.

Finnemore, Martha, and Kathryn Sikkink. 1998. International Norm Dynamics and Political Change. *International Organization* 52, no. 4 (Autumn): 887-917.

Fisk, Lennard A. 1987. Prepared Testimony, U.S. House of Representatives, Committee on Science, Space and Technology, Subcommittee on International Scientific Cooperation, Subcommittee on Natural Resources, Agriculture Research and Environment, One Hundredth Congress, First Session, held at Washington, D.C., September 30.

———. 1989. EOS Selection Letter from the NASA Associate Administrator to Anne B. Kahle, February 7.

———. 1991. ASTER Confirmation Letter to Anne B. Kahle, January 18.

Forman, Paul. 1973. Scientific Internationalism and the Weimar Physicists: The Ideology and Its Manipulation in Germany after World War I. *ISIS* 64, no. 2: 150-80.

Forsberg, Aaron. 2000. *America and the Japanese Miracle: The Cold War Context of Japan's Postwar Economic Revival, 1950-1960*. Chapel Hill: University of North Carolina Press.

Frieden, Jeffrey A., and David A. Lake. 2000. Introduction: International Politics and International Economics. In *International Political Economy: Perspectives on Global Power and Wealth*, edited by Jeffrey A. Frieden and David A. Lake. London: Routledge.

Frutkin, Arnold W. 1965. *International Cooperation in Space*. Englewood Cliffs, N.J.: Prentice-Hall.

Fujisada Hiroyuki. 1990. ASTER Instrument Overview. In *Minutes of the First ASTER Team Meeting*, held at Pasadena, CA, November 12.

———. 1992. Comments from Science Mission Team on ASTER Performance Specification. In *Minutes of the Third ASTER Science Team Meeting*, held at Pasadena, CA, January 21.

———. 1993. ASTER Instrument Operation Capability Upgrade. In *Operations and Mission Planning Working Group Meeting Minutes*, held at Hilo, Hawaii, June 28.

———. 2003. Interview by author in Japanese, Tsukuba, Japan, March 27.

Fujisada Hiroyuki, and Ono Akira. 1992. Preliminary Design Concept and Development Targets of Japanese Optical Sensor for EOS-A. *Advances In Space Research* 12, no. 7: 323-26.

Fujisada, Hiroyuki, and Akira Ono. 1991. Overview of ASTER Design Concept. In *Future European and Japanese Remote-Sensing Sensors and Programs*, edited by Philip N. Slater, 244-54. Vol. 1490, *Proceedings of SPIE*. Bellingham, WA: SPIE--The International Society for Optical Engineering.

Galison, Peter. 1996. Computer Simulations and the Trading Zone. In *The Disunity of Science: Boundaries, Contexts, and Power*, edited by Peter Galison and David J. Stump, 118-57, *Writing Science*. Stanford: Stanford University Press.

Garfinkel, Harold. 1967. *Studies in Ethnomethodology*. Englewood Cliffs, N.J.: Prentice-Hall.

Garnett, Nick. 1990. Suspicious of a Link-Up. *Financial Times*, January 4, 12.

Geller, Gary N. 1992. Ground Data Systems Working Group Action Item Status. In *U.S. ASTER Science Team Meeting*, held at Tuscon, AZ, November 9-13.

———. 1993a. ASTER Level 1 Processing Working Group. In *Minutes of the US ASTER Science Team Meeting*, held at Pasadena, CA, July 14-16.

———. 1993b. End-to-End Data System Concept Document, version 1.0 (September 10, 1993). In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 9-12.

———. 1993c. L1 Processing Meeting Highlights [e-mail], October 8.

———. 1994. *Advanced Spaceborne Thermal Emission and Reflection Radiometer End-to-End Data System Concept*. JPL D-11199. Jet Propulsion Laboratory. October 13.

———. 2003. Interview by author, Pasadena, CA, August 7.

Gieryn, Thomas F. 1983. Boundary Work and the Demarcation of Science for Non-science: Strains and Interests in Professional Ideologies of Scientists. *American Sociological Review* 48: 781-95.

———. 1992a. The Ballad of Pons and Fleischmann: Experiment and Narrative in the (Un)making of Cold Fusion. In *The Social Dimensions of Science*, edited by Ernan McMullin, 217-43. Nortre Dame, IN: Nortre Dame Press.

———. 1992b. Riding the Action/Structure Pendulum with those Swinging Sociologists of Science. In *The Outlook for STS: Report on an STS Symposium and Workshop*, edited by Sheila Jasanoff, 23-34. Ithaca: Department of Science and Technology Studies, Cornell University.

———. 1995. Boundaries of Science. In *Handbook of Science and Technology Studies*, edited by Sheila Jasanoff, Gerald E. Markle, James C. Petersen, and Trevor Pinch, 393-443. Thousand Oaks: Sage Publications.

———. 1999. *Cultural Boundaries of Science: Credibility on the Line*. Chicago: University of Chicago Press.

Gilbert, G. Nigel, and Michael Mulkay. 1984. *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*. Cambridge: University of Cambridge Press.

Gillespie, Alan R. 1985. Lithologic Mapping of Silicate Rocks Using TIMS. In *The TIMS Data User's Workshop, June 18 and 19*, 29-44. Pasadena, CA: JPL Publication 86-38.

———. 1992a. Choice of T/E Algorithm. In *Minutes of the Fourth ASTER Team Meeting*, held at Tsukuba, Japan, June 22-25.

———. 1992b. T/E. In *U.S. ASTER Science Team Meeting*, held at Tucson, AZ, November 9-13.

———. 1993a. Report from the Temperature/Emissivity Working Group. In *Minutes of the U.S. ASTER Science Team Meeting*, held at Pasadena, CA, July 14-16.

———. 1993b. T/E Working Group. In *Minutes of the Fifth ASTER Science Team Meeting*, held at Las Vegas, NV, February 2-5.

———. 1994a. Agenda for the ASTER T/E Working Group Meeting. In *Minutes of 7th ASTER Science Team Meeting*, held at Pasadena, CA, May 24-27.

———. 1994b. TEWG Meeting. In *Minutes of the 7th ASTER Science Team Meeting*, held at Pasadena, CA, May 24-27.

———. 1996. TEWG Report to the Plenary Session. In *Minutes of the 11th ASTER Science Team Meeting*, held at Flagstaff, AZ, May 22-26.

———. 1997. TE Session -- 22 May 1997. In *13th ASTER Science Team Meeting*, held at Sioux Falls, South Dakota, May 20-23.

Gillespie, Alan R., and Anne B. Kahle. 1977. Construction and Interpretation of a Digitial Thermal Inertia Image. *Photogrammetric Engineering and Remote Sensing* 43, no. 8: 983-1000.

Gillespie, Alan R., and Rokugawa Shuichi. 1999. AGENDA: TEWG Report to ASTER Plenary Session. In *Narrative Minutes of the 16th ASTER Science Team Meeting*, held at Pasadena, CA, January 12014.

Gillespie, Alan R., Shuichi Rokugawa, Simon J. Hook, Tsuneo Matsunaga, and Anne B. Kahle. 1999. *Temperature/Emissivity Separation Algorithm Theoretical Basis Document, Version 2.4*. NASA Contract: NAS5-31372. University of Washington, University of Tokyo, Jet Propulsion Laboratory, and Geological Survey of Japan. March 22.

Gillespie, Alan R., Shuichi Rokugawa, Tsuneo Matsunaga, J. Steven Cothern, Simon J. Hook, and Anne B. Kahle. 1998. A Temperature and Emissivity Separation Algorithm for Advanced Spaceborne Thermal Emission and Reflection Radiometer (ASTER) Images. *IEEE Transactions on Geoscience and Remote Sensing* 36, no. 4 (July): 1113-26.

Gilpin, Robert. 2001. *Global Political Economy: Understanding the International Economic Order*. Princeton: Princeton University Press.

Goddard Space Flight Center. 1995 [signed in 1996]. *Project Implementation Plan, Volume I: Advanced Spaceborne Thermal Emission and Reflection Radiometer and EOS-AM Projects*. GSFC 421-12-01-01. Greenbelt, MD. January.

———. 1997 [signed in 1997]. *Project Implementation Plan, Volume II -Ground Data System: Advanced Spaceborne Thermal Emission and Reflection Radiometer (ASTER) and ESDIS and EOS-AM Projects*. CCR 505-10-11-002-A. Greenbelt, MD. September.

Goetz, Alexander F.H., and Mark Herring. 1989. The High Resolution Imaging Spectrometer (HIRIS) for Eos. *IEEE Transactions on Geoscience and Remote Sensing* 27, no. 2 (March): 136-43.

Goffman, Erving. 1959. *The Presentation of Self in Everyday Life*. Garden City, N.Y.: Doubleday, Anchor Books.

———. 1974. *Frame Analysis: An Essay on the Organization of Experience*. New York: Harper & Row.

Goldstein, Judith, and Robert O. Keohane. 1993. Ideas and Foreign Policy: An Analytical Framework. In *Ideas and Foreign Policy: Beliefs, Institutions, and Political Change*, edited by Judith Goldstein and Robert O. Keohane, 3-30, *Cornell Studies in Political Economy*. Ithaca: Cornell University Press.

Golinski, Jan. 1998. *Making Natural Knowledge: Constructivism and the History of Science*. Cambridge: Cambridge University Press.

Goody, Richard M. 1982. *Global Change: Impacts on Habitability: A Scientific Basis for Assessment: A Report*. Washington, D.C.: National Aeronautics and Space Administration, Jet Propulsion Laboratory.

Gordon, Andrew. 2003. *A Modern History of Japan: From Tokugawa Times to the Present*. New York: Oxford University Press.

———, ed. 1993. *Postwar Japan as History*. Berkeley: University of California Press.

Green, Andrew. 1988. Letter to Anne B. Kahle, August 17.

Green, Michael J. 1990. *Kokusanka: FSX and Japan's Search for Autonomous Defense Production*. Cambridge, MA: The MIT Japan Program, Center for International Studies, Massachusetts Institute of Technology. May.

———. 1995. *Arming Japan: Defense Production, Alliance Politics, and the Postwar Search for Autonomy*. New York: Columbia University Press.

Greenstone, Renny. 1989. A Condensed History of the Earth Observing System (Eos). *The Earth Observer* 1, no. 3 (June): 2-3.

Grieco, Joseph M. 1988. Anarchy and the Limits of Cooperation: A Realist Critique of the Newest Liberal Institutionalism. *International Organization* 42, no. 3 (Summer): 485-507.

———. 1990. *Cooperation Among Nations: Europe, America, and Non-Tariff Barriers to Trade*. Edited by Peter J. Katzenstein. *Cornell Studies in Political Economy*. Ithaca: Cornell University Press.

Ground Data System Working Group. 1993. Minutes of the Ground Data System Working Group. In *Minutes of the Fifth ASTER Science Team Meeting*, held at Las Vegas, NV, February 2-5.

Gubbels, Timothy. 2005. Interview by author, Tokyo, Japan, June 8.

Gusterson, Hugh. 1996. *Nuclear Rites: A Weapons Laboratory at the End of the Cold War*. Berkeley: University of California Press.

Guston, David H. 2000. *Between Politics and Science: Assuring the Integrity and Productivity of Research*. Cambridge: Cambridge University Press.

———. 2001. Boundary Organizations in Environmental Policy and Science: An Introduction. *Science, Technology & Human Values* 26, no. 4 (October): 399-408.

Haas, Ernst B. 1990. *When Knowledge Is Power: Three Models of Change in International Organizations*. Edited by Stephen D. Krasner. *Studies in International Political Economy*. Berkeley: University of California Press.

Haas, Peter M. 1992. Introduction: Epistemic Communities and International Policy Coordination. *International Organization* 46, no. 1 (Winter): 1-36.

———. 2004. When Does Power Listen to Truth? A Constructivist Approach to the Policy Process. *Journal of European Public Policy* 11, no. 4 (August): 569-92.

———, ed. 1997. *Knowledge, Power, and International Policy Coordination*. *Studies in International Relations*. Columbia, S.C.: University of South Carolina Press.

Haas, Peter M., and Ernst B. Haas. 2002. Pragmatic Constructivism and the Study of International Institutions. *Millennium: Journal of International Studies* 31, no. 3: 573-601.

Hachifuji Tōki. 1983. *Uchū kaihatsu seisaku keisei no kiseki [The Track of the Formation of Space Development Policy]*. Tōkyō: Kokusai tsūshin bunka kyōkai.

Hacking, Ian. 1999. *The Social Construction of What?* Cambridge: Harvard University Press.

Hall, G.R., and R.E. Johnson. 1970. Transfers of United States Aerospace Technology to Japan. In *The Technology Factor in International Trade*, edited by Raymond Vernon, 305-58. New York: National Bureau of Economic Research.

Hamblin, Jacob Darwin. 2002. Undersea Warfare, the Limits of Internationalism, and the Utility of Basic Research, 1945-1956. *ISIS* 93, no. 1: 1-27.

Harris, Martha Caldwell, and Gordon E. Moore. 1992. *Linking Trade and Technology Policies: An International Comparison of the Policies of Industrialized Nations*. Washington, D.C.: National Academy Press.

Hata, Takashi. 2001. Science Engineering Education in Japanese Universities after World War II. In *A Social History of Science and Technology in Contemporary Japan, Volume I: The Occupation Period 1945--1952*, edited by Shigeru Nakayama, 206-23. Melbourne: Trans Pacific Press.

Heaton, George R. 1991. *International R&D Cooperation: Lessons from the Intelligent Manufacturing Systems Proposal, Manufacuring Forum Discussion Paper, No. 2*. Washington, D.C.: National Academy of Press.

Hecht, Gabrielle. 1998. *The Radiance of France: Nuclear Power and National Identity after World War II*. Edited by Wiebe E. Bijker, W. Bernard Carlson, and Trevor Pinch. *Inside Technology*. Cambridge: The MIT Press.

Hein, Laura. 1990. *Fueling Growth: The Energy Revolution and Economic Policy in Postwar Japan*. Cambridge: Harvard University Press.

Hein, Laura, and Mark Seldon, eds. 1997. *Living with the Bomb: American and Japanese Cultural Conflicts in the Nuclear Age*. Armonk, NY: M.E. Sharpe.

Hekl, Bob. 1993a. ASTER Operations Scenario. In *Operations and Mission Planning Working Group Meeting Minutes*, held at Hilo, Hawaii, June 28.

———. 1993b. Report on ASTER 16-Day Scenario. In *Operations and Mission Planning Working Group Meeting Minutes*, held at Las Vegas, NV, February 4: D1-D49.

Hekl, Bob, and Frank Mills. 1992. Mission Planning Action Items. In *Minutes of the Fourth ASTER Team Meeting*, held at Tsukuba, Japan, June 23.

Higher Level Data Products Working Group. 1993a. Higher Level Data Products Working Group. In *Minutes of the Fifth ASTER Science Team Meeting*, held at Las Vegas, NV, February 2-5.

———. 1993b. Higher Level Data Products Working Group Charter. In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 9-12.

Hilgartner, Stephen. 2000. *Science on Stage: Expert Advice as Public Drama*. Edited by Timothy Lenoir and Hans Ulrich Gumbrecht. *Writing Science*. Stanford: Stanford University Press.

Hino, Hideo, Mitsuru Takei, Hiromi Ono, and Riichi Nagura. 1991. Optical Sensor System for Japanese Earth Resources Satellite 1. In *Future European and Japanese Remote-Sensing Sensors and Programs*, edited by Philip N. Slater, 166-76. Vol. 1490, *Proceedings of SPIE*. Bellingham, WA: SPIE--The International Society for Optical Engineering.

Hogan, Michael J. 1996. *Hiroshima in History and Memory*. Cambridge: Cambridge University Press.

Hook, Simon J. 1992. T-E Separation Algorithm Evaluation. In *Minutes of the Fourth ASTER Team Meeting*, held at Tsukuba, Japan, June 22-25.

———. 2001. Interview by author, Pasadena, CA, June 4.

———. 2006. Personal correspondence with author, June 14.

Hook, Simon J., Andrew R. Gabell, Andrew A. Green, and Peter S. Kealy. 1992. A Comparison of Techniques for Extracting Emissivity Information from Thermal Infrared Data for Geologic Studies. *Remote Sensing of Environment* 42: 123-35.

Hook, Simon J., Anne B. Kahle, Alan R. Gillespie, and Marit Jentoft-Nilsen. 1994. *Temperature/Emissivity Separation, Version 1.0*. Jet Propulsion Laboratory. February 28.

Ida, H., H. Murayama, and Y. Horikawa. 1992. The Japanese Experimental Module for Space Station Freedom. *Acta Astronautica* 28: 43-47.

International Council of Scientific Unions. 1986. *The International Geosphere-Biosphere Programme: A Study of Global Change*. Paris: International Council of Scientific Unions.

———. 1988. *The International Geosphere-Biosphere Programme: A Study of Global Change, IGBP: A Plan for Action*. Paris: International Council of Scientific Unions.

Iona, Glenn. 2001. Interview by author, Greenbelt, MD, October 19.

Ishii Yoshinori. 1991. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 11, no. 1.

———. 2003. Interview by author in Japanese, Tokyo, Japan, June 2.

Ishii Yoshinori, and Rokugawa Shuichi. 1982. Gōsei kaikō rēda no dijitaru gazō sakusei ni kan suru kenkyū [A Study on Digital Image Reconstruction of SAR Data]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 2, no. 3: 63-78.

Ishii Yoshinori, Rokugawa Shuichi, and Katō Yoshishi. 1988. Shumirēshon ni yoru rēdā remōto senshingu no kenkyū [Basic Characteristics of Radar Remote Sensing --- A Simulation Study ---]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 8, no. 1: 15-26.

Ishizawa, Y., S. Takamura, N. Saito, S. Niwa, R. Kuramasu, and S. Iwai. 1986. Current Status of Japan's Earth Resources Satellite-1, AAS 85-633. In *Space Exploitation and Utilization*, edited by Gayle L. May, Peter M. Bainum, Kenji Ikeda, Tamiya Nomura, Tatsu Yamanaka, and Ryojiro Akiba, 291-98. Vol. 60, *Advances in the Astronautical Sciences*. San Diego, CA: American Astronautical Society.

Isoda M. 1993. ASTER SWIR Instrument Development (Mitsubishi Electric Corp.). In *Minutes of the Fifth ASTER Science Team Meeting*, Plenary I, held at Las Vegas, NV, November 8.

Iwasaki Akira. 2002. Interview by author in Japanese, Tsukuba, Japan, October 3.

Iwashita, Atsushi. 1990. ERSDAC Ground Data System Status. In *Minutes of the First ASTER Team Meeting*, held at Pasadena, CA, November 12-13.

Jasanoff, Sheila. 1987. Contested Boundaries in Policy-Relevant Science. *Social Studies of Science* 17, no. 2: 195-230.

———. 1990. *The Fifth Branch: Science Advisors as Policymakers*. Cambridge: Harvard University Press.

———. 1996. Beyond Epistemology: Relativism and Engagement in the Politics of Science. *Social Studies of Science* 26, no. 2 (May): 393-418.

———. 2005. *Designs on Nature: Science and Democracy in Europe and the United States*. Princeton: Princeton University Press.

———, ed. 2004. *States of Knowledge: The Co-Production of Science and Social Order*. Edited by John Urry. *International Library of Sociology*. London: Routledge.

Jasanoff, Sheila, Gerald E. Markle, James C. Petersen, and Trevor Pinch, eds. 1995. *Handbook of Science and Technology Studies*. Thousand Oaks: Sage Publications.

Jinkō eisei kaihatsu honbu chikyū kansoku eisei gurūpu [Artificial Satellite Development Head Office, Earth Observation Satellite Group]. 1991. *Chikyū shigen eisei 1 gō (ERS-1) handobūku [Earth Resources Satellite No. 1 (ERS-1) Handbook]*. Tōkyō: Uchū kaihatsu jigyōdan [National Space Development Agency].

Joerges, Bernward. 1999. Do Politics Have Artefacts? *Social Studies of Science* 29, no. 2 (April): 411-31.

Johnson, Carol. 2001. Interview by author, Gaithersburg, MD, October 22.

Johnson, Chalmers. 1982. *MITI and the Japanese Miracle: The Growth of Industrial Policy, 1925-1975*. Stanford: Stanford University Press.

———. 1995. *Omote* (Explicit) and *Ura* (Implicit): Translating Japanese Political Terms. In *Japan: Who Governs? The Rise of the Developmental State*, 157-82. New York: W.W. Norton and Company.

———, ed. 1984. *The Industrial Policy Debate*. San Francisco: ICS Press.

Kagaku gijutsu chō. 1987. *Kagaku gijutsu hakusho [White Paper on Science and Technology]*. Kagaku gijutsu chō.

Kagaku gijutsu chō [Science and Technology Agency]. 1978. *Wagakuni no uchū kaihatsu no ayumi [The Progress of Our Country's Space Development]*. Tōkyō: Ōkurashō insatsu kyoku.

Kahle, Anne B. 1989a. Letter to Ishii Yoshinori, Re: Rationale for Additions to the Proposed ITIR Instrument, April 4.

———. 1989b. Letter to Shoichi Hashimoto, Executive Managing Director, Japan Resources Observation System Organization, February 27.

———. 1989c. Letter to TIGER Team Members, March 17.

———. 1989d. *Rationale for Additions to the Proposed ITIR Instrument*. Pasadena, CA: Jet Propulsion Laboratory. April 4.

———. 1989e. *Statement of Work for the Development of the TIGER Investigation*. Pasadena, CA: Jet Propulsion Laboratory. April 21.

———. 1990a. ITIR Science Steering Committee. In *Minutes of the First ASTER Team Meeting*, held at Pasadena, CA, November 12-13: 124-25.

———. 1990b. Letter to Hiroji Tsu, Chief of Kyushu Center, Geological Survey of Japan, March 6.

———. 1990c. *U.S. Team Leader: Intermediate Thermal Infrared Radiometer (ITIR) for EOS Polar Orbiting Platform, A Proposal to the National Aeronautics and Space Administration Office of Space Science and Applications, Vol 1.* Jet Propulsion Laboratory. March 15.

———. 1992a. Advanced Spaceborne Thermal Emission and Reflection Radiometer. In *ASTER 1st Annual OSSI POP Review, Jet Propulsion Laboratory*, held at Pasadena, CA, December 11.

———. 1992b. Science and Instrument Overview. In *ASTER 1st Annual OSSI POP Review, Jet Propulsion Laboratory*, held at Pasadena, CA, December 11.

———. 1994. Science Team Objectives. In *Experiment Implementation Plan JPL Internal Review*, held at Pasadena, CA, March 28.

———. 2003a. Interview by author, Pasadena, CA, August 4.

———. 2003b. An Overview of ASTER. Presented at *ASTER Data Users Workshop*, April 26, held at Pasadena, CA.

———. 2005. Interview by author, Phone call to Castle Rock, WA, August 26.

Kahle, Anne B., Alan R. Gillespie, Elsa A. Abbott, Michael J. Abrams, Richard E. Walker, Gordon Hoover, and John P. Lockwood. 1988. Relative Dating of Hawaiian Lava Flows Using Multispectral Thermal Infrared Images: A New Tool for Geologic Mapping of Young Volcanic Terranes. *Journal of Geophysical Research - Solid Earth and Planets* 93, no. B12 (December 10): 15239-51.

Kahle, Anne B., and Alexander F.H. Goetz. 1983. Mineralogic Information from a New Airborne Thermal Infrared Multispectral Scanner. *Science* 222, no. 4619: 24-27.

Kahle, Anne B., and Frank D. Palluconi. 1988. *Thermal Infrared Ground Emission Radiometer (TIGER): A Proposal to the National Aeronautics and Space Administration Office of Space Science and Applications*. Jet Propulsion Laboratory. July 5.

———. 1989. Trip Report to Shelby G. Tilford and Dixon M. Butler, March 17.

Kahle, Anne B., Frank D. Palluconi, and Alan R. Gillespie. 1988. *Development and Utilization of the ITIR for Geologic Application: A Proposal to Science and Technology Agency of Japan, Research and Development Bureau*. Jet Propulsion Laboratory. June 21.

Kahle, Anne B., Frank Don Palluconi, Catherine J. LeVine, Michael J. Abrams, David B. Nash, Ronald E. Alley, and John P. Schieldge. 1982. *Evaluation of Thermal Data for Geologic Applications*. JPL Publication 83-56. Jet Propulsion Laboratory. December.

Kahle, Anne B., and Lawrence C. Rowan. 1980. Evaluation of Multispectral Middle Infrared Aircraft Images for Lithologic Mapping in the East Tintic Mountains, Utah. *Geology* 8, no. 5: 234-39.

Kahle, Anne B., John P. Schieldge, and Ronald E. Alley. 1984. Sensitivity of Thermal Inertia Calculations to Variations in Environmental Factors. *Remote Sensing of Environment* 16, no. 3: 211-32.

Kash, Don E. 1967. *The Politics of Space Cooperation*. West Lafayette, IN: Purdue University.

Kato, Takehiko. 1993. JEM Electrical Power System Architecture. *Acta Astronautica* 29, no. 4: 283-89.

Katzenstein, Peter J. 1996. *Cultural Norms and National Security: Police and Military in Postwar Japan*. Edited by Peter J. Katzenstein. *Cornell Studies in Political Economy*. Ithaca: Cornell University Press.

———. 2005. *A World of Regions: Asia and Europe in the American Imperium*. Edited by Peter J. Katzenstein. *Cornell Studies in Political Economy*. Ithaca: Cornell University Press.

Katzenstein, Peter J., Robert O. Keohane, and Stephen D. Krasner. 1998. *International Organization* and the Study of World Politics. *International Organization* 52, no. 4 (Autumn): 645-85.

Katzenstein, Peter J., and Nobuo Okawara. 2004. Japan and Asian-Pacific Security. In *Rethinking Security in East Asia: Identity, Power, and Efficiency*, edited by J.J. Suh, Peter J. Katzenstein, and Allen Carlson, 97-130, *Studies in Asian Security*. Stanford: Stanford University Press.

Katzenstein, Peter J., and Rudra Sil. 2004. Rethinking Asian Security: A Case for Analytical Eclecticism. In *Rethinking Security in East Asia: Identity, Power, and Efficiency*, edited by J.J. Suh, Peter J. Katzenstein, and Allen Carlson, 1-33, *Studies in Asian Security*. Stanford: Stanford University Press.

Kawamoto Chiyoshi. 2004. Towards the ISS Commercial Utilization. Presented at *The 10th Session of the Asia-Pacific Regional Space Agency Forum*, 14-16 January, held at University of Tokyo, Institute of Industrial Science, Tokyo.

Kealy, Peter S., and Andrew R. Gabell. 1990. Estimation of Emissivity and Temperature using Alpha Coefficients. In *Proceedings of the Second TIMS Workshop*. Pasadena, CA: JPL Publication 90-55.

Keck, Margaret, and Kathryn Sikkink. 1998. *Activists Beyond Borders: Advocacy Networks in International Politics*. Ithaca: Cornell University Press.

Kenkyū Chōsei Kyoku Uchū Kokusaika, ed. 1979. *Uchū gijutsu kiki dōnyū kankei shiryōshū*. Tsukuba, Japan: Uchū Kaihatsu Jigyōdan.

Keohane, Robert O., ed. 1986. *Neorealism and Its Critics*. Edited by Helen Milner and John Gerard Ruggie. *New Directions in World Politics*. New York: Columbia University Press.

Keohane, Robert O., and Joseph S. Nye. 1977. *Power and Interdependence: World Politics in Transition*. 2nd ed. Boston: Little, Brown and Company.

Kevles, Daniel J. 1977. The National Science Foundation and the Debate over Postwar Research Policy, 1942-1945: A Political Interpretation of *Science--The Endless Frontier*. *ISIS* 68, no. 241: 297-319.

Kieffer, Hugh H. 1990. Letter to Dr. Akira Ono, re: ASTER calibration. In *Minutes of the Second ASTER Team Meeting*, held at Tokyo, Japan, May 15-17, 1991 (November 21 letter).

———. 1992. Level 1 Processing. In *U.S. ASTER Science Team Meeting*, held at Tucson, AZ, November 9-13.

King, Michael D. 1992. *The Earth Observer* 4, no. 5.

Kirkham, Gilbert. 2004. International Participation in NASA's Discovery Program: Presentation to the NASA Discovery Program. In *Discovery Preproposal Conference*, held at Renaissance Hotel, Washington D.C., April 23, 2004.

Kline, Ronald. 1995. Construing 'Technology' as 'Applied Science': Public Rhetoric of Scientists and Engineers in the United States, 1880-1945. *ISIS* 86: 194-221.

Knorr Cetina, Karin. 1999. *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge: Harvard University Press.

Knorr Cetina, Karin, and Urs Bruegger. 2002. Global Microstructures: The Virtual Societies of Financial Markets. *American Journal of Sociology* 107, no. 4 (January): 905-50.

Kobayashi, Shigeo. 1986. Overview of Japanese Policy on Space Station. *Acta Astronautica* 14: 11-18.

Kohli, Atul, Peter Evans, Peter J. Katzenstein, Adam Przeworski, Susanne Hoeber Rudolph, James C. Scott, and Theda Skocpol. 1995. The Role of Theory in Comparative Politics: A Symposium. *World Politics* 48, no. 1 (October): 1-49.

Kohno, Masaru. 1989. Japanese Defense Policy Making: The FSX Selection, 1985-1987. *Asian Survey* 24, no. 5: 457-79.

Koizumi, Tamisuke, Takashi Suzuki, Masanori Kobayashi, and Osamu Kakuichi. 1990. Development of Earth Resources Satellite-1 (JERS-1), AAS 89-609. In *Space Utilization and Applications in the Pacific*, edited by Peter M. Bainum, Gayle L. May, Tatsuo Yamanaka, and Yang Jiachi, 61-74. Vol. 73. San Diego, CA: American Astronautical Society.

Kōkōgyō gijutsu kenkyū kumiai kondankai [Mining and Industrial Technology Research Associations Committee]. 1991. *Kōkōgyō gijutsu kenkyū kumiai 30 nen no ayumi: kōkōgyō gijutsu kenkyū kumiai hōshikō 30 syūnen kinen shuppan [A 30 Year History of Mining and Industrial Technology Research Associations: Mining and Industrial Technology Research Associations 30 year Anniversary Commemorative Publication]*. Tōkyō: Nihon kōgyō gijutsu shinkō kyōkai [Japan Industrial Technology Promotion Association].

Koyama Hideaki. 1992. ASTER SWIR Instrument Development Status (MELCO). In *Minutes of the Third ASTER Team Meeting*, held at Pasadena, CA, January 21.

Krige, John. 1997. The Politics of European Scientific Collaboration. In *Science in the Twentieth Century*, edited by John Krige and Dominique Pestre, 897-918. Amsterdam: Harwood Academic Publishers.

Krige, John, and Kai-Henrik Barth, eds. 2006. *Global Power Knowledge:Science and Technology in International Affairs*. Edited by Kathryn M. Olesko. Vol. 21. *Osiris*. Chicago: University of Chicago Press.

Krugman, Paul R., ed. 1986. *Strategic Trade Policy and the New International Economics*. Cambridge, MA: MIT Press.

Kudo, I., H. Fujisada, Y. Kato, S. Hashimoto, and H. Hino. 1992. Mission Instruments Development for Japanese ERS-1. *Advances In Space Research* 12, no. 7: 319.

Kudoh Masahiko. 1990. Platform/ASTER (ITIR) Interface (Current Status). In *Minutes of the First ASTER Team Meeting*, held at Pasadena, CA, November 12.

———. 1993. ASTER Instrument Development Status. In *Minutes of the Fifth ASTER Science Team Meeting*, held at Las Vegas, NV, February 2.

———. 2003. Interview by author in Japanese, Tokyo, Japan, June 9.

Kudoh Masahiko, and Koyama Hideaki. 1992. ASTER Instrument Development Status. In *Minutes of the Third ASTER Team Meeting*, held at Pasadena, CA, January 21.

Lambright, W. Henry. 1994. Administrative Entrepreneurship and Space Technology: The Ups and Down of "Mission to Planet Earth". *Public Administration Review* 54, no. 2 (March/April): 97-104.

Lambros, Scott. 1992a. ASTER Accommodation. In *Minutes of the Fourth ASTER Team Meeting*, Plenary I, held at Tsukuba, Japan, June 22.

———. 1992b. Spacecraft Pointing Capabilities. In *Minutes of the Fourth ASTER Team Meeting*, held at Tsukuba, Japan, June 22-25.

Lang, Harold. 1991. Memo to Watanabe Hiroshi, Re: Agenda for Petroleum Working Group. In *Minutes of the Second ASTER Team Meeting*, held at Tokyo, Japan, May 15-17 (April 9 Memo).

Latour, Bruno. 1987. *Science in Action: How to Follow Scientists and Engineers Through Society*. Cambridge: Harvard University Press.

———. 1988. The Politics of Explanation: An Alternative. In *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge*, edited by Stephen Woolgar, 155-76. London: Sage Publications.

———. 1990. Drawing Things Together. In *Representation in Scientific Practice*, edited by Michael Lynch and Steve Woolgar. Cambridge: MIT Press.

———. 1993. *We Have Never Been Modern*. Translated by Catherine Porter. Cambridge: Harvard University Press.

———. 1999. For David Bloor. and Beyond: A Reply to David Bloor's 'Anti-Latour'. *Studies in History and Philosophy of Science* 30, no. 1 (1999): 113-29.

———. 2004. *Politics of Nature: How to Bring the Sciences into Democracy*. Translated by Catherine Porter. Cambridge, MA: Harvard University Press.

———. 2005. *Reassembling the Social: An Introduction to Actor-Network-Theory*. *Clarendon Lectures in Management Studies*. New York: Oxford University Press.

Latour, Bruno, and Stephen Woolgar. 1986 [1979]. *Laboratory Life: The Construction of Scientific Facts*. 2nd ed. ed. Princeton: Princeton University Press.

Law, John. 1986a. On the Methods of Long Distance Control: Vessels, Navigation, and the Portuguese Route to India. In *Power, Action and Belief: A New Sociology of Knowledge?* edited by John Law, 234-63. Vol. 32, *Sociological Review Monograph*. London: Routledge.

———. 1987. Technology and Heterogeneous Engineering: The Case of Portuguese Expansion. In *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*, edited by Wiebe E. Bijker, Thomas P. Hughes, and Trevor Pinch, 111-34. Cambridge, MA: The MIT Press.

———. 2002. *Aircraft Stories: Decentering the Object in Technoscience*. Edited by Barbara Herrnstein Smith Smith and E. Roy Weintraub. *Science and Cultural Theory*. Durham, NC: Duke University Press.

———, ed. 1986b. *Power, Action and Belief: A New Sociology of Knowledge?* Vol. 32. *Sociological Review Monograph*. London: Routledge and Kegan Paul.

———, ed. 1991. *A Sociology of Monsters: Essays on Power, Technology and Domination*. London: Routledge.

Law, John, and Michel Callon. 1988. Engineering and Sociology in a Military Aircraft Project: A Network Analysis of Technical Change. *Social Problems* 35, no. 3 (June): 284-97.

Law, John, and John Hassard, eds. 1999. *Actor Network Theory and After*. *Sociological Review Monograph*. Oxford: Blackwell Publishers.

Law, John, and Vicky Singleton. 2000. Performing Technology's Stories: On Social Constructivism, Performance, and Performativity. *Technology and Culture* 41, no. 4 (October): 765-75.

Leslie, Stuart W. 1993. *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford*. New York: Columbia University Press.

Level 1 Working Group. 1993. Level 1 Working Group Report in Plenary Session. In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 12.

Lezaun, Javier. 2006. Creating a New Object of Government: Making Genetically Modified Organisms Traceable. *Social Studies of Science* 36, no. 4 (August): 499-531.

List of [U.S.] Data Products. 1993. In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 9-12.

Livingston, Eric. 1999. Cultures of Proving. *Social Studies of Science* 29, no. 6 (December): 867-88.

———. 2006. The Context of Proving. *Social Studies of Science* 36, no. 1 (February): 39-68.

Logsdon, John M. 1992. U.S.-Japanese Space Relations at a Crossroads. *Science* 255, no. 5042 (January 17): 294-300.

———. 1997. *Learning from the Leader: The Early Years of U.S.-Japanese Cooperation in Space*. Washington, D.C.: George Washington University Space Policy Institute.

Lorell, Mark. 1996. *Troubled Partnership: A History of U.S.-Japan Collaboration on the FS-X Fighter*. New Brunswick: Transaction Publishers.

Lukes, Steven, ed. 1986. *Power*. Edited by William Connolly and Steven Lukes. *Readings in Social and Political Theory*. New York: New York University Press.

Lynch, Michael. 1992. Extending Wittgenstein: The Pivotal Move from Epistemology to the Sociology of Science. In *Science as Practice and Culture*, edited by Andrew Pickering, 215-65. Chicago: University of Chicago Press.

———. 1993. *Scientific Practice and Ordinary Action: Ethnomethodological and Social Studies of Science*. New York: Cambride University Press.

Lyon, R.J.P. 1965. Analysis of Rocks by Spectral Infrared Emission (8 to 25 microns). *Economic Geology* 60: 715-36.

Mack, Pamela E. 1990. *Viewing the Earth: The Social Construction of the Landsat Satellite System*. Edited by Wiebe E. Bijker, W. Bernard Carlson, and Trevor Pinch. *Inside Technology*. Cambridge: MIT Press.

MacKenzie, Donald. 1990. *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance*. Edited by Wiebe E. Bijker, W. Bernard Carlson, and Trevor Pinch. *Inside Technology*. Cambridge: MIT Press.

———. 2001. *Mechanizing Proof: Computing, Risk, and Trust*. Edited by Wiebe E. Bijker, W. Bernard Carlson, and Trevor Pinch. *Inside Technology*. Cambridge: The MIT Press.

———. 2003. An Equation and its Worlds: Bricolage, Exemplars, Disunity and Performativity in Financial Economics. *Social Studies of Science* 33, no. 6 (December): 831-68.

MacLeod, Roy, ed. 2000. *Nature and Empire: Science and the Colonial Enterprise*. Vol. 15. *Osiris*. Chicago: University of Chicago Press.

Maekawa Masatoshi. 2003. Interview by author in Japanese, Tokyo, Japan, June 10.

Maekawa Masatoshi, Muraoka Hiroyasu, and Okada Ken'ichi. 2003. Interview by author in Japanese, Tokyo, Japan, June 10.

Maldonado, Leon. 2003. Interview by author, Pasadena, CA, August 5.

Matsumura Masahiro. 1999. *Nichibei dōmei to gunji gijutsu [The Japan-U.S. Alliance and Military Technology]*. Tōkyō: Keisō shobō.

Matsunaga Tsuneo. 1994. Netsuseki-gaibun hikari hōsharitsu no heikin oyobi saidai, saishōchi no keikenteki kankei wo mochita ondo -- hōsharitsu bunri shuhō [A Temperature-Emissivity Separation Method Using an Empirical Relationship between the Mean, the Maximum, and the Minimum of the Thermal Infrared Emissivity Spectrum]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 14, no. 3: 28-39.

———. 2003. Interview by author in Japanese, Tsukuba, Japan, March 28.

———. 2006. Personal correspondence with author, June 8.

Matsunaga Tsuneo, Rokugawa Shuichi, and Ishii Yoshinori. 1993. Supuritto uindō-hō TIMS dēta e no ōyō [Application of the Split Window Technique to Thermal Infrared Multispectral Scanner (TIMS) Data]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 13, no. 2: 1-13.

Mayo, Richard E., Gordon R. Bolton, and Daniele Laurini. 1989. International Interface Design for Space Station Freedom: Challenge and Solutions. *Acta Astronautica* 20: 29-38.

McDougall, Walter A. 1985. *The Heavens and the Earth: A Political History of the Space Age*. New York: Basic Books.

Merton, Robert K. 1973 (1942). The Normative Structure of Science. In *The Sociology of Science: Theoretical and Empirical Investigation*, edited by Robert K. Merton, 267-78. Chicago: University of Chicago Press.

Michioka Hidekazu. 1992. ASTER SWIR Instrument Development Status. In *Minutes of the Third ASTER Team Meeting*, held at Pasadena, CA, January 21-23.

Miller, Clark A. 2004. Climate Science and the Making of a Global Political Order. In *States of Knowledge: The Co-Production of Science and Social Order*, edited by Sheila Jasanoff, 46-66, *International Library of Sociology*. London: Routledge.

Miller, Clark A., and Paul N. Edwards. 2001a. *Changing the Atmosphere: Expert Knowledge and Global Environmental Governance*. Edited by Peter M. Haas, Sheila Jasanoff, and Gene Rochlin. *Politics, Science, and the Environment*. Cambridge: MIT Press.

———. 2001b. Introduction: The Globalization of Climate Science and Climate Politics. In *Changing the Atmosphere*, edited by Clark A. Miller and Paul N. Edwards, 1-30, *Politics, Science, and the Environment*. Cambridge: The MIT Press.

Ministry of International Trade and Industry. 1990. *A Phase C/D Proposal in Response to the Earth Observing System, Intermediate and Thermal Infrared Radiometer (ITIR) for Eos-A Polar Orbiting Platform: Investigation and Technical Plan, Data Plan, and Management Plan - Volume 1*. 90JS-POP-TS009. Tokyo, Japan. May 30.

———. 1991. *Earth Observing System-A, Advanced Spaceborne Thermal Emission and Reflection Radiometer (ASTER), The Nine Execution/Operations Phase Plans*. Version 2.0, 90JS-POP-TS010. Tokyo. March.

Mitchell, Timothy. 1991. The Limits of the State: Beyond Statist Approaches and their Critics. *American Political Science Review* 85, no. 1: 77-96.

Miyazaki Yoshinori. 1992a. ASTER Data Acquisition Scenario. In *Minutes of the Fourth ASTER Team Meeting*, Plenary I, held at Tsukuba, Japan, June 22.

———. 1992b. Data Acquisition Scenaro Proposed by Dr. Miyazaki. In *Minutes of the Fourth ASTER Team Meeting*, Plenary III, held at Tsukuba, Japan, June 25.

———. 1993. ASTER Operations Study. In *Operations and Mission Planning Working Group Meeting Minutes*, held at Hilo, Hawaii, June 28.

———. 2003. Interview by author in English and Japanese, Tokyo, June 2.

Miyazaki Yoshinori, and Matthew Schwaller. 1993. ASTER MOU, PIP, ICWG status. In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 9.

Mol, Annemarie. 2002. *The Body Multiple: Ontology in Medical Practice*. *Science and Cultural Theory*. Durham, N.C.: Duke University Press.

Mol, Annemarie, and John Law. 1994. Regions, Networks and Fluids: Anaemia and Social Topology. *Social Studies of Science* 24, no. 4: 641-71.

Moore, Berrien. 1991. *Recommended Instruments for the Restructured Earth Observing System (EOS)*. Washington, D.C.: National Aeronautics and Space Administration, EOS Payload Advisory Panel. October 24.

Moore, Berrien, and Jeff Dozier. 1992a. *The Earth Observer* 4, no. 5: 3-10.

———. 1992b. *Adapting the Earth Observing System to the Projected $8 Billion Budget: Recommendations from the EOS Investigators*. Washington, D.C.: National Aeronautics and Space Administration, EOS Payload Advisory Panel. October 14.

Moravcsik, Andrew. 1993. Introduction: Integrating International and Domestic Theories of International Bargaining. In *Double-Edged Diplomacy: International Bargaining and Domestic Politics*, edited by Peter B. Evans, Harold K. Jacobson, and Robert D. Putnam, 3-42. Vol. 25, *Studies in International Political Economy*. Berkeley: University of California Press.

———. 1997. Taking Preferences Seriously: A Liberal Theory of International Politics. *International Organization* 51, no. 4 (Autumn): 513-53.

Moritani Masanori. 1981. *Nichibeiō gijutsu kaihatsu sensō: gijutsu hūdo kara no hikaku kenkyū [Japan-U.S.-Europe Technology Development War: Comparative Research of Technology Milieus]*. Tōkyō: Tōyō keizai shimpōsha.

Morris-Suzuki, Tessa. 1994. *The Technological Transformation of Japan: From the Seventeenth to the Twenty-first Century*. Cambridge: Cambridge University Press.

Morrison, Andrew. 1992. ASTER 16-Day Operations Scenario. In *U.S. ASTER Science Team Meeting*, held at Tucson, AZ, November 11.

———. 1993. ASTER User Community Definition. In *Minutes of the Operations and Mission Planning Working Group Meeting*, held at Hilo, HI, June 28-30.

Moteff, John D. 1989. *FSX Technology: Its Relative Utility to the United States and Japanese Aerospace Industry*. Congressional Research Service, Library of Congress. April 12.

Mukerji, Chandra. 1989. *A Fragile Power: Scientists and the State*. Princeton: Princeton University Press.

Muraoka Hiroyasu. 2003. Interview by author in Japanese, Tokyo, Japan, June 10.

Nakayama Katsuya. 1986. Yūshi izen -- ERSDAC no tanjō made no hanashi -- [Pre-history: Discussions up until the Birth of ERSDAC]. *ERSDAC nyūsu [ERSDAC News]*, no. 11 (September): 3.

Nakayama, Shigeru. 2001a. Introduction: Occupation Period 1945-'52. In *A Social History of Science and Technology in Contemporary Japan: The Occupation Period, 1945-1952*, edited by Shigeru Nakayama, Kunio Gotō, and Hitoshi Yoshioka, 23-56. Vol. 1, *A Social History of Science and Technology in Contemporary Japan*. Melbourne: Trans Pacific Press.

———. 2001b. The Science Council of Japan and the Scientific and Technical Administration Committee. In *A Social History of Science and Technology in Contemporary Japan, Volume I: The Occupation Period, 1945-1952*, edited by Shigeru Nakayama, Kunio Gotō, and Hitoshi Yoshioka, 224-36. Vol. 1, *A Social History of Science and Technology in Contemporary Japan*. Melbourne: Trans Pacific Press.

Nakayama Shigeru. 1995. *Sengo kaikaku to sono isan [Post-war Reform and Its Legacy]*. Edited by Nakamura Masanori. Vol. 6. *Sengo nihon: senryō to sengo kaikaku [Post-war Japan: Occupation and Post-reform]*. Tōkyō: Iwanami Shoten.

NASA Interface Control Working Group. 2002. *EOSDIS Architecture and Interface Schematic*. Greenbelt, MD: Goddard Space Flight Center.

NASA International Affairs Office. 1983. NASA's Approach to International Cooperation. In *Unispace '82: A Context for International Cooperation and*

Competition, edited by U.S. Congress Office of Technology Assessment, 68-70. Washington, D.C.: U.S. Government Printing Office.

National Aeronautics and Space Administration. 1987a. *HIRIS: High-Resolution Imaging Spectrometer: Science Opportunities for the 1990s, Instrument Panel Report*. Edited by Dixon M. Butler. IIh vols. Vol. IIc. *Earth Observing System Reports*. Washington, D.C.: National Aeronautics and Space Administration.

———. 1988a. *Earth Observing System (Eos) Background Information Package (BIP) Announcement of Opportunity, Part Five: Research Facility and Operational Facility Instrument Descriptions*. OSSA-1-88. National Aeronautics and Space Administration. January 19.

———. 1988b. *Earth Observing System (Eos) Background Information Package (BIP) Announcement of Opportunity, Part One: Guidelines for Proposal Preparation*. OSSA-1-88. National Aeronautics and Space Administration. January 19.

———. 1992. *EOSDIS: EOS Data and Information System*. Washington, D.C.: National Aeronautics and Space Administration.

———. 1993. *Earth Observing System: EOS Reference Handbook*. Edited by Ghassem Asrar and David Jon Dokken. Greenbelt, Md.: National Aeronautics and Space Administration.

———. 2004. Transcript of Discovery Preproposal Conference, April 23, 2004, held at Renaissance Hotel, Washington D.C.

National Aeronautics and Space Administration, and Ministry of International Trade and Industry. 1996. *Implementing Arrangement between the National Aeronautics and Space Administration of the United States of America and the Ministry of International Trade and Industry of Japan concerning Cooperation on the Advanced Spaceborne Thermal Emission and Reflection Radiometer (ASTER) Program*. November 7.

National Aeronautics and Space Administration Advisory Council, Earth System Sciences Committee. 1986. *Earth System Science: Overview: A Program for Global Change*. Edited by Francis P. Bretherton. Washington, D.C.: National Aeronautics and Space Administration.

———. 1988. *Earth System Science: A Closer View*. Edited by Francis P. Bretherton. Washington, D.C.: National Aeronautics and Space Administration.

National Aeronautics and Space Administration, Earth Observing System Data Panel (Robert R.P. Chase, et al.). 1986. *Report of the Eos Data Panel*. Edited by Dixon M. Butler. IIh vols. Vol. IIa. *Earth Observing System Reports*. Washington, D.C.: National Aeronautics and Space Administration.

National Aeronautics and Space Administration, Earth Observing System Science Steering Committee. 1987b. *From Pattern to Process: The Strategy of the Earth Observing System, Eos Science Steering Committee Report*. Edited by Dixon M. Butler. IIh vols. Vol. II. *Earth Observing System Reports*. Washington, D.C.: National Aeronautics and Space Administration.

National Aeronautics and Space Administration, Science and Mission Requirements Working Group. 1984. *Earth Observing System: Science and Mission Requirements Working Group Report*. Edited by Dixon M. Butler. IIh vols. Vol. I. *Earth Observing System Reports*. Washington, D.C.: National Aeronautics and Space Administration.

National Commission on Space. 1986. *Pioneering the Space Frontier*. New York: Bantam Books.

National Critical Technologies Panel. 1991. *National Critical Technologies Plan*. U.S. Office of Science and Technology Policy.

National Research Council. 1997. *Maximizing U.S. Interests in Science and Technology Relations with Japan*. Washington, D.C.: National Academy Press.

———. 1999. *U.S.-European-Japanese Workshop on Space Cooperation*. Washington, D.C.: National Academy Press.

National Research Council, and European Science Foundation. 1998. *U.S. European Collaboration in Space Science*. Washington, D.C.: National Academy Press.

National Research Council, Committee on Earth Sciences, Space Science Board, Assembly of Mathematical and Physical Sciences. 1982. *A Strategy for Earth Science from Space in the 1980's, Part I: Solid Earth and Oceans*. Washington, D.C.: National Academy Press.

National Research Council, Task Group on Earth Sciences, Space Science Board, Commission on Physical Sciences, Mathematics, and Resources. 1988. *Space Science in the Twenty-First Century: Imperatives for the Decades 1995 to 2015: Mission to Planet Earth*. Edited by Don L. Anderson. Washington, D.C.: National Academy of Sciences.

National Science Foundation. 1961. *National Science Foundation: Eleventh Annual Report for the Fiscal Year Ended June 30, 1961*. Washington, D.C.: U.S. Government Printing Office.

———. 1962. *National Science Foundation: Twelfth Annual Report for the Fiscal Year Ended June 30, 1962*. Washington, DC.

National Space Development Agency of Japan. 1992. *JERS-1 Information for Press: Launch of Japanese Earth Resources Satellite-1 by H-1 Launch Vehicle (H24F)*. Tokyo: National Space Development Agency of Japan.

National Space Policy Council. 1989. *National Space Policy Directives and Executive Charter (NSPD-1)*. November 2.

NEC. 1993. Instrument Operations Update and Command Size Modeling / Estimation. In *Minutes of the 6th ASTER Science Team Meeting*, Operations and Mission Planning Working Group, held at Tokyo, Japan, November 9.

Neumann, Iver B. 2002. Returning Practice to the Linguistic Turn: The Case of Diplomacy. *Millennium: Journal of International Studies* 31, no. 3: 627-51.

Neureiter, Norman P. 1998. U.S.-Japan Cooperative Science Program: A Personal Retrospective. Presented at *Seventh U.S.-Japan Joint Science Policy Seminar, Science Policy in the 21st Century: Bilateral Cooperation in a Multilateral Context*, May 11-14, held at Hilo, HI.

Nichols, David A. 1992a. ASTER Project Management and System Engineering. In *ASTER 1st Annual OSSI POP Review, Jet Propulsion Laboratory*, held at Pasadena, CA, December 11.

———. 1992b. ASTER Science Project Overview. In *ASTER 1st Annual OSSI POP Review, Jet Propulsion Laboratory*, held at Pasadena, CA, December 11.

———. 1992c. Operations/Mission Planning Action Items. In *Minutes of the Fourth ASTER Team Meeting*, held at Tsukuba, Japan, June 22.

———. 1992d. Project Overview. In *ASTER 1st Annual OSSI POP Review, Jet Propulsion Laboratory*, held at Pasadena, CA, December 11.

———. 1992e. Summary. In *ASTER 1st Annual OSSI POP Review, Jet Propulsion Laboratory*, held at Pasadena, CA, December 11.

———. 1993a. ASTER Level 1 Processing [E-mail Msg: IPJD-5021-5965/20], January 22.

———. 1993b. Dave Nichols PIP Report [e-mail], June 23.

———. 1993c. Input to PIP Version 3 [e-mail], August.

———. 1994. Project Overview. In *Experiment Implementation Plan JPL Internal Review*, held at Pasadena, CA, March 28.

———. 2001. Interview by author, Pasadena, CA, June 4.

Nichols, David A., Matthew Schwaller, Moshe Pniel, and Gary N. Geller. 1995. EOSDIS Support for the Advanced Spaceborne Thermal Emission and Reflection Radiometer (ASTER). In *Advanced and Next-Generation Satellites, Proceedings of SPIE, Vol. 2583*, edited by Hiroyuki Fujisada and Martin N. Sweeting, 51-61. Vol. 2583. Bellingham, WA: SPIE--The International Society for Optical Engineerings.

Nihon kagakushi gakkai [History of Science Society of Japan]. 1964. *Tsūshi [General History]*. Edited by Nihon kagakushi gakkai [History of Science Society of Japan]. Vol. 5. *Nihon kagaku gijutsu taikei [A Compendium of Japanese History of Science and Technology]*. Tōkyō: Dai ichi hōki shuppan.

Ninomiya Yoshiki, and Satō Isao. 1992. Chishitsu chōsajo ganseki hyōjun shiryō "kaseigan shirīzu" no chūkan sekigai ryōiki hansha supekutoru [The Mid-Infrared Reflection Spectra of the "Volcanic Rock Series" Standard of the Geological Survey of Japan]. In *Chishitsu rimōto senshingu ni kansuru kenkyū [Research on Geologic Remote Sensing]*, edited by Tsu Hiroji, 27-38. Vol. 278, *Chishitsu chōsajo hōkoku [Reports of the Geological Survey of Japan]*. Ibaraki-ken Tsukuba-shi: Chishitsu chōsajo [Geological Survey of Japan].

Noble, Gregory W. 1992. *Flying Apart? Japanese-American Negotiations Over the FSX Fighter Plane*. Vol. 41. *Policy Papers in International Affairs*. Berkeley: Institute of International Studies, University of California.

Obara Michio. 1990. Letter to Lennard A. Fisk, Associate Administrator for Space Science and Applications, NASA Headquarters, [n.d., between May 1 and July 10].

Obara Michio, Yoshida, and Yokota Makoto. 1990. *Space Activities in Japan and MITI's Policies for Space Industry*. Tokyo: Ministry of International Trade and Industry, Space Industry Division.

Ohno Taijiro. 1997. Schedule on Performance-Check-Simulation. In *OMPWG Splinter Meeting*, held at Tokyo, Japan, March 5-7.

Okada Ken'ichi. 2003a. Interview by author in Japanese, Tokyo, Japan, June 10.

———. 2003b. SSSG Report for the Operations and Mission Planning Working Group. Presented at *24th ASTER Science Team Meeting*, May 22, held at Tokyo.

Okada, Kinya, and Atsushi Iwashita. 1992. Hyper-Multispectral Image Analysis Based on Waveform Characteristics of Spectral Curve. *Advances In Space Research* 12, no. 7: 433.

Onishi, Norimitsu. 2004. Japan Support of Missile Shield Could Tilt Asia Power Balance. *New York Times*, A1.

Ono Makoto. 1990. Status of SWIR Subsystem (MELCO). In *Minutes of the First ASTER Team Meeting*, held at Pasadena, CA, November 12.

———. 1993. ASTER SWIR Instrument Development Status (MELCO). In *Minutes of the Fifth ASTER Science Team Meeting*, held at Las Vegas, NV, February 2.

Ostry, Sylvia, and Richard Nelson. 1995. *Techno-Nationalism and Techno-Globalism: Conflict and Cooperation*. *Integrating National Economies: Promise and Pitfalls*. Washington, DC: The Brookings Institution.

Oye, Kenneth, ed. 1986. *Cooperation Under Anarchy*. Princeton: Princeton University Press.

Palluconi, Frank D. 1992. Payload Advisory Panel Recommendations. In *U.S. ASTER Science Team Meeting*, held at Tucson, November 17: B1-B3.

———. 2001. Interview by author, Pasadena, CA, May 30.

Palluconi, Frank Don, and Gerald R. Meeks. 1985. *Thermal Infrared Multispectral Scanner (TIMS): An Investigator's Guide to TIMS data*. NASA CR-175875. Jet Propulsion Laboratory, California Institute of Technology, National Aeronautics and Space Administration.

Parthasarathy, Shobita. 2005. Architectures of Genetic Medicine: Comparing Genetic Testing for Breast Cancer in the USA and the UK. *Social Studies of Science* 35, no. 1 (February): 5-40.

Pedersen, Kenneth S. 1986. The Changing Face of International Space Cooperation: One View of NASA. *Space Policy* 2, no. 2 (May): 120-37.

———. 1987. In *Economics and Technology in U.S. Space Policy*, edited by Molly K. Macauley, 173-98. Washington, DC: National Academy of Engineering.

Pekkanen, Saadia M. 2003. *Picking Winners? From Technology Catch-Up to the Space Race in Japan*. Stanford: Stanford University Press.

Peterson, Cass. 1986. A Dire Forecast for 'Greenhouse' Earth. *Washington Post*, June 11, A1.

Pickering, Andrew. 1984. *Constructing Quarks: A Sociological History of Particle Physics*. Chicago: University of Chicago Press.

———. 1992. *Science as Practice and Culture*. Chicago: University of Chicago Press.

———. 1995. *The Mangle of Practice: Time, Agency, and Science*. Chicago: University of Chicago Press.

Pieri, David. 2003. Interview by author, Pasadena, CA, August 7.

Pinch, Trevor. 1986. *Confronting Nature: The Sociology of Neutrino Detection*. Dordrecht, Netherlands: Reidel.

Plafcan, Dan. 1999. Technology Transfer and Security Shifts in the Japan-U.S. Alliance: Department of Science and Technology Studies, Cornell University.

Planet of the Year: Endangered Earth. 1989. *Time* 133, no. 1 (January 2).

Pniel, Moshe. 1994. Product Generation System. In *Experiment Implementation Plan JPL Internal Review*, held at Pasadena, CA, March 28.

———. 1997. Large Angle Pointing > 8.55 degrees. In *US ASTER Science Team Meeting Minutes*, held at Sioux Fall, SD, May 19.

———. 2001. Interview by author, Pasadena, CA, May 31 and June 1.

Polanyi, Michael. 1962. The Republic of Science: Its Political and Economic Theory. *Minerva* 1, no. 1: 54-73.

Prestowitz, Clyde. 1989a. Giving Japan a Handout: Why fork over $7 billion in aircraft technology? *Washington Post*, January 29.

Prestowitz, Clyde V. 1989b. *Trading Places: How We Are Giving Our Future to Japan and How to Reclaim It*. New York: Basic Books.

Price, J.C. 1984. Land Surface Temperature Measurements from the Split Window Channels of the NOAA 7 Advanced Very High Resolution Radiometer. *Journal of Geophysical Research* 89: 7231-37.

Price, Richard. 1997. *The Chemical Weapons Taboo*. Ithaca: Cornell University Press.

Putnam, Evelyn S. 1986. *Commercial Applications and Scientific Research Requirements for Thermal-Infrared Observations of Terrestrial Surfaces: A Report of the Joint EOSAT/NASA Thermal Infrared Working Group*. Lanham, MD: National Aeronautics and Space Administration, Earth Observation Satellite, Co.

Pyle, Kenneth B. 1996. *The Japanese Question: Power and Purpose in a New Era*. Washington, DC: AEI Press.

Reich, Robert B. 1987. The Rise of Techno-nationalism. *The Atlantic Monthly*, 63-9.

Reischauer, Edwin O. 1960. The Broken Dialogue with Japan. *Foreign Affairs* 39, no. 1 (October): 11-26.

Reppy, Judith, ed. 2000. *The Place of the Defense Industry in National Systems of Innovation*. Vol. 25. *Occasional Paper*. Ithaca: Cornell University Peace Studies Program.

Ride, Sally. 1987. *Leadership and America's Future in Space: A Report to the Administrator*. Washington, DC: National Aeronautical and Space Administration.

Risse-Kappen, Thomas, ed. 1995. *Bringing Transnational Relations Back In: Non-State Actors, Domestic Structures and International Institutions*. Edited by Steve Smith. Vol. 42. *Cambridge Studies in International Relations*. Cambridge: Cambridge University Press.

Rogers, David. 1990. Senate Panel Cuts Most New Funding for NASA Project. *The Wall Street Journal*, September 14, A16.

Rokugawa Shuichi. 1993. T/E & Atmosphere Working Group Reporting Part in Plenary Session. In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 9-12.

———. 1995. ASTER ni yoru chihyōmen-ondo oyobi hōsharitsu no dōshutsu [Derivation of Surface Temperature and Surface Emissivity from ASTER TIR Data]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 15, no. 2: 33-38.

———. 1997. Report of the Temperature - Emissivity Working Group Session. In *Minutes of 14th ASTER Science Team Meeting*, held at Yurakucho, Japan, December 9-12.

———. 2003. Interview by author in Japanese, Tokyo, June 6.

Rokugawa Shuichi, and Simon Hook. 1993. T/E & Atmosphere Working Groups Summary. In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 9-12.

Rokugawa, Shuichi, Tsuneo Matsunaga, Hiroji Tsu, and Yoshinori Ishii. 1994. Future Potential of Thermal Infrared Multispectral Data for Mapping Temperature and Emissivity Parameters. *Advances In Space Research* 14, no. 3: 71-80.

Rowan, Lawrence C. 1992. SWIR SNRs and NEdRs. In *Minutes of the Third ASTER Science Team Meeting*, Geology Working Group, held at Pasadena, CA, January 22.

Rubinstein, Gregg A. 1987. Emerging Bonds of U.S.-Japanese Defense Technology Cooperation. *Strategic Review* 15(Winter).

Ruggie, John Gerard. 1975. International Responses to Technology: Concepts and Trends. *International Organization* 29, no. 3 (Summer): 557-83.

Saitō Shigebumi. 1992a. Kichō kōen [Keynote Address]. In *Kokusai shinpojium--JERS-1, nijūisseiki e no kakehashi--kōenshū [International Symposium--JERS-1, A Bridge to the 21st Century--Proceedings]*, (Zai) shigen kansoku kaiseki sentā, held at Tōkyō: 21-39.

———. 1992b. *Nihon uchū kaihatsu monogatari: kokusan eisei ni kaketa senkushatachi no yume [The Story of Japan's Space Development: The Dreams of Pioneers Who Placed in Orbit an Indigenous Satellite]*. Tōkyō: Mita Shuppankai.

Sakata, Toshibumi. 1980. Remote Sensing Activities in Japan. Presented at *First Asian Conference on Remote Sensing*, November 5-7, held at Bangkok.

Sakuma Fumihiro. 2003. Interview by author in Japanese, Tsukuba, Japan, March 38.

Salomonson, Vince. 1990. Team Meetings: MODIS Team. *The Earth Observer* 2, no. 2 (February 28): 9.

Samuels, Richard J. 1987. *The Business of the Japanese State: Energy Markets in Comparative and Historical Perspective*. Edited by Peter J. Katzenstein. *Cornell Studies in Political Economy*. Ithaca: Cornell University Press.

———. 1994. *"Rich Nation, Strong Army": National Security and the Technological Transformation of Japan*. Edited by Peter J. Katzenstein. *Cornell Studies in Political Economy*. Ithaca: Cornell University Press.

Satō Isao. 1992. Shimyurēshon dēta ni yoru JERS-1 kōgaku sensa no ganseki shikibetsu no tame no bando kumiawase [Band Combinations for Discriminating Rock Based on JERS-1 Optical Sensor Simulated Data]. In *Chishitsu rimōto senshingu ni kansuru kenkyū [Research on Geologic Remote Sensing]*, edited by Tsu Hiroji, 133-44. Vol. 278, *Chishitsu Chōsajo Hōkoku [Reports of the Geological Survey of Japan]*. Ibaraki-ken Tsukuba-shi: Chishitsu chōsajo [Geological Survey of Japan].

———. 1993. User Interface Requirements Document. In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 9-12.

Satō, Isao, Hiroshi Watanabe, and Hiroji Tsu. 1999. Launch Ready Status of ASTER Ground Data System. In *Sensors, Systems, and Next-Generations Satellites III, Proceedings of SPIE, Vol. 3870*, edited by Hiroyuki Fujisada and Joan B. Lurie, 548-54. Vol. 3870. Bellingham, WA: SPIE--The International Society for Optical Engineering.

Satō Yasushi. 2001. A Contested Gift of Power: American Assistance to Japan's Space Launch Vehicle Technology, 1965-1975. *Historia Scientiarum* 11, no. 2: 176-204.

———. 2005. Personal communication. Personal correspondence with author, Tokyo, Japan, July 22.

Schaffer, Simon. 1988. Astronomers Mark Time: Discipline and the Personal Equation. *Science in Context* 2, no. 1: 115-45.

Schaller, Michael. 1997. *Altered States: The United States and Japan Since the Occupation*. New York: Oxford University Press.

Schieldge, John P. 2001. Interview by author, Pasadena, CA, June 1.

Scholz, Lawrence C. 1990. Point and Jitter Discussion. In *Minutes of the First ASTER Team Meeting*, held at Pasadena, CA, November 12-13.

Schoppa, Leonard J. 1993. Two Level Games and Bargaining Outcomes: Why Gaiatsu Succeeds in Japan in Some Cases and Not Others. *International Organization* 47, no. 3 (Summer): 353-86.

———. 1997. *Bargaining with Japan: What American Pressure Can and Cannot Do*. New York: Columbia University Press.

Schwaller, Matthew. 1993a. Action A2, Lisa's Scenarios #2 [attachment to e-mail], February 27.

———. 1993b. Action Items from PIP volume II meeting [e-mail], January 13.

———. 1993c. Meeting Notice for Speakers (not for general distribution): ASTER Ground Data System Meeting, June 17-18, 1993 [e-mail], June [n.d.].

———. 1993d. Memo on ASTER Ground Data System meeting with MITI [e-mailed memo to Dixon Butler and Ghassem Asrar at NASA Headquarters], June 2.

———. 2001. Interview by author, Greenbelt, MD, October 23.

Scott, James C. 1998. *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed*. Edited by James C. Scott. *Yale Agrarian Studies*. New Haven: Yale University Press.

Shabecoff, Philip. 1988a. Global Warming Has Begun, Expert Tells Senate. *New York Times*, June 24, A1.

———. 1988b. The Heat Is On: Calculating the Consequences on a Warmer Planet Earth. *New York Times*, June 26, D1.

———. 1990. European Officials Dispute Bush Over Global Warming. *New York Times*, April 18, B4.

Shaffer, Lisa R. 1989. International Coordination for the Eos Program. *The Earth Observer* 1, no. 3 (June): 1.

———. 1993a. ASTER User Scenarios [E-mail Msg: BGJD-5557-3174], February 14.

———. 1993b. RE: MITI/MTPE Meeting [E-mail Msg: MGJD-5828-4925], August 3.

———. 2006. Personal correspondence with author, June 14.

Shapin, Steven. 1988. Following Scientists Around. *Social Studies of Science* 18, no. 3 (August): 533-50.

———. 1994. *A Social History of Truth: Gentility, Credibility, and Scientific Knowledge In Seventeenth-Century England*. Chicago: University of Chicago Press.

———. 1995. Here and Everywhere: Sociology of Scientific Knowledge. *Annual Review of Sociology* 21: 289-321.

Shapin, Steven, and Simon Schaffer. 1985. *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*. Princeton: Princeton University Press.

Shigen tansa-yō kansoku sisutemu kenkyū kaihatsu kikō [Japan Resources Observation System Organization]. 2000. *Sekiyu shigen enkaku tanchi gijutsu kenkyū kaihatsu kenkyū hōkokusho [Petroleum resources remote-sensing technology research and development research report]*. Tokyo: Shigen tansa-yō kansoku sisutemu kenkyū kaihatsu kikō [Japan Resources Observation System organization].

Siegal, Barry S., and Alan R. Gillespie, eds. 1980. *Remote Sensing in Geology*. New York: Wiley.

Simon, Denis Fred, ed. 1997. *Techno-Security in an Age of Globalization: Perspectives from the Pacific Rim*. *Pacific Rim Business and Technology*. New York: M.E. Sharpe.

Sismondo, Sergio. 1996. *Science without Myth: On Constructions, Reality, and Social Knowledge*. Albany: State University of New York Press.

———. 2004. *An Introduction to Science and Technology Studies*. Malden, MA: Blackwell Publishing.

Skolnikoff, Eugene B. 1993. *The Elusive Transformation: Science, Technology, and the Evolution of International Politics*. *A Council on Foreign Relations Book*. Princeton: Princeton University Press.

Slater, Philip N., and Kurtis J. Thome. 1992. *Semi-annual Eos Contract Report -- Report #6*. Contract Number: NAS5-31717. Tucson, AZ: Remote Sensing Group, Optical Sciences Center, University of Arizona. January 15 - June 30.

Smith, Bruce L.R. 1990. *American Science Policy Since World War II*. Washington, DC: The Brookings Institution.

Smith, Robert W. 1989. *The Space Telescope: A Study of NASA, Science, Technology, and Politics*. New York: Cambridge University Press.

Snyder, Glenn. 1984. The Security Dilemma in Alliance Politics. *World Politics* 36, no. 4 (July): 461-95.

Snyder, Glenn H. 1997. *Alliance Politics*. Edited by Robert J. Art, Robert Jervis, and Stephen M. Walt. *Cornell Studies in Security Affairs*. Ithaca: Cornell University Press.

Soffen, Jerry. 1990. Editor's Corner: Phase C/D Here We Come! *The Earth Observer* 2, no. 1 (January 31): 1.

Sōritsu jūnenshi henshū iinkai [Editorial Committee for History of Ten Years after Establishment]. 1993. *Sōritsu jūnenshi, zaidan hōjin shigen kansoku kaiseki sentā [History of Ten Years after Establishment]*. Tōkyō: (Zai) shigen kansoku kaiseki sentā.

Star, Leigh Susan. 1989. The Structure of Ill-Structured Solutions: Boundary Objects and Heterogeneous Distributed Problem Solving. In *Readings in Distributed Artificial Intelligence*, edited by M. Huhs and L. Gasser. Vol. 3. Menlo Park, CA: Morgan Kaufmann.

Star, Leigh Susan, and James R. Griesemer. 1989. Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39. *Social Studies of Science* 19, no. 3: 387-420.

Stern, Jessica E. 1996. U.S. Assistance Programs for Improving MPC&A in the Former Soviet Union. *The Nonproliferation Review* 4, no. 2 (Winter): 17-32.

Stevens, William K. 1990. Huge Space Platforms Seen as Distorting Studies of Earth. *New York Times*, June 19, C1.

Stone, Laura. 1999. Whither Trade and Security? A Historical Perspective. In *The U.S.-Japan Alliance: Past, Present, and Future*, edited by Michael J. Green and Patrick M. Cronin, 247-67. New York: Council on Foreign Relations Press.

Stone, Richard. 2006. Secret Pyongyang Meeting Builds Science Ties Between Two Koreas. *Science* 312, no. 5771 (April 14): 172-73.

Suchman, Lucy A. 1987. *Plans and Situated Actions: The Problem of Human-Machine Communication*. Cambridge: Cambridge University Press.

Suh, J.J. 2004. Bound to Last? The U.S.-Korea Alliance and Analytical Eclecticism. In *Rethinking Security in East Asia: Identity, Power, and Efficiency*, edited by J.J. Suh, Peter J. Katzenstein, and Allen Carlson, 131-71, *Studies in Asian Security*. Stanford: Stanford University Press.

Suh, J.J., Peter J. Katzenstein, and Allen Carlson, eds. 2004. *Rethinking Security in East Asia: Identity, Power, and Efficiency*. Edited by Muthiah Alagappa. *Studies in Asian Security*. Stanford: Stanford University Press.

Sullivan, Walter. 1961. *Assault on the Unknown: The International Geophysical Year*. New York: McGraw-Hill.

Suyama Junji, Ishii Yoshinori, Yamaguchi Yasushi, Kamata Hiroki, Hase Hirokazu, and Ogawa Katsurō. 1982. Gōsei kaikō rēda ni yoru Nihon rettō no jinetsu shigen chōsa [Airborne SAR Survey of Japanese Islands---As a Part of The Nation-wide Geothermal Survey---]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 2, no. 4: 57-66.

Takahashi Fumiō. 1992. Instrument Operations (Constraints/Modes) (NEC). In *Minutes of the Fourth ASTER Team Meeting*, Plenary I, held at Tsukuba, Japan, June 22.

Takenouchi. 2003. Interview by author in Japanese, Tokyo, Japan, May 30.

Taubes, Gary. 1993. Earth Scientists Look NASA's Gift Horse in the Mouth. *Science* 259, no. 5097 (February 12): 912-14.

Thome, Kurtis J. 2001. Interview by author, Tokyo, Japan, October 2.

Thompson, Charis. 2004. Co-producing CITES and the African Elephant. In *States of Knowledge: The Co-Production of Science and Social Order*, edited by Sheila Jasanoff, 67-86, *International Library of Sociology*. London: Routledge.

Tilford, Shelby G. 1989. Letter from NASA Earth Science and Applications Division Director Shelby G. Tilford to JAROS Executive Managing Director Hashimoto Shoichi and to T. Tanaka of NASDA, March [3?].

Tsu Hiroji. 1990a. Letter to Anne B. Kahle, May 1.

———. 1990b. Status of ASTER (ITIR) Project. In *Minutes of the First ASTER Team Meeting*, held at Pasadena, CA, November 12-13: 23-29.

———. 1991. In *Dai 3 kai kokusai shinpojiumu: JERS-1 to kongo no rimōto senshingu kōenshū [Proceedings of the 3rd International Symposium: JERS-1 and Remote Sensing in the Future]*, (Zai) shigen kansoku kaiseki sentā, held at Tōkyō, March.

———. 1993a. Restructuring of Japanese ASTER Science Team. In *Minutes of the Fifth ASTER Science Team Meeting*, held at Las Vegas, NV, November 8-12: PA1-PA3.

———. 1993b. Team Leaders' Comment on Closer Collaboration between U.S.-Japan ASTER Science Teams. In *Minutes of the 6th ASTER Science Team Meeting*, held at Tokyo, Japan, November 9.

Tsu Hiroji, and Anne B. Kahle. 1997. *ASTER Long-Term Instrument Plan (LTIP)*. Version 1.0. JPL D-15109. Jet Propulsion Laboratory and Earth Remote Sensing Data Analysis Center. October 14.

Tsuchida Satoshi. 2002. Interview by author in Japanese, Tsukuba, Japan, September 26.

Tsuchiya Kiyoshi, Arai Kōhei, and Ishida Chū. 1982. Randosatto 3-gō RBV kamera no kando tokusei to shēdingu hoseihō [Shading Characteristics of Landsat-3 RBV Cameras and Correction Method of Shading Effects]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 2, no. 2: 39-48.

Tyson, Laura D'Andrea. 1992. *Who's Bashing Whom? Trade Conflict in High-Technology Industries*. Washington, DC: Institute for International Economics.

U.S. ASTER Science Team. 1992. *U.S. ASTER Science Team Meeting*. Tucson, AZ. November 9-13.

———. 1993. *Minutes of the US ASTER Science Team Meeting*. Pasadena, CA. July 14-16.

———. 1997. *US ASTER Science Team Meeting Narrative Minutes*. Sioux Falls, SD. May 19.

U.S. Congress, Office of Technology Assessment. 1990. *Arming Our Allies: Cooperation and Competition in Defense Technology*. OTA-ISC-449. U.S. Government Printing Office.

———. 1991. *Competing Economies: America, Europe, and the Pacific Rim*. U.S. Government Printing Office.

U.S. Department of Commerce. 1987. *The Status of Emerging Technologies: An Economic/Technological Assessment to the Year 2000*. National Bureau of Standards. June.

———. 1990. *Emerging Technologies: A Survey of Technical and Economic Opportunities*. Spring.

U.S. Department of Defense. 1989. *Critical Technologies Plan*. May.

U.S. Department of State. 1969. Exchange of Notes Concerning Cooperation in Space Exploration, TIAS 6735. In *United States Treaties and Other International Agreements--1969*, 2720-28. Vol. 20, part 2. U.S. Government Printing Office.

———. 1996. *Science, Technology, and American Diplomacy*. Washington, DC: U.S. Government Printing Office.

U.S. General Accounting Office. 1982. *U.S. Military Coproduction Programs Assist Japan in Developing Its Civil Aircraft Industry*. GAO/ID-82-23. Washington, DC: U.S. Government Printing Office. March 18.

———. 1989. *U.S.-Japan FS-X Codevelopment Program*. GAO/T-NSIAD-89-32. Washington, DC: U.S. Government Printing Office. May 16.

———. 1990. *Global Warming: Administration Approach Cautious Pending Validation of Threat*. GAO/NSIAD-90-63. January.

———. 1992a. *Earth Observing System: NASA's EOSDIS Development Approach Is Risky*. GAO/IMTEC-92-24. Washington, DC: U.S. Government Printing Office. February.

———. 1992b. *Earth Observing System: NASA Needs to Reassess Its EOSDIS Development Strategy*. GAO/T-IMTEC-92-8. Washington, DC: U.S. General Accounting Office. February 27.

———. 1992c. *U.S.-Japan Co-Development: Update of the FS-X Program*. GAO/NSIAD-92-165. Washington, DC: U.S. Government Printing Office. June.

———. 1995a. *Earth Observing System: Concentration on Near-term EOSDIS Development May Jeopardize Long-term Success*. GAO/T-AIMID-95-103. Washington, DC: U.S. Government Printing Office. March 16.

———. 1995b. *U.S.-Japan Cooperative Development*. GAO/NSIAD-95-145. Washington, DC: U.S. Government Printing Office. August.

———. 1997. *U.S.-Japan Fighter Aircraft: Agreement on F-2 Production*. GAO/NSIAD-97-76. Washington, DC: U.S. Government Printing Office. February.

———. 1999. *Federal Research: Information on International Science and Technology Agreements*. GAO/RCED-99-108. Washington, DC: U.S. Government Printing Office. April.

U.S. Global Change Research Program, Office of Science and Technology Policy, Federal Coordinating Council on Science, Engineering, and Technology, Committee on Earth Sciences. 1990. *Our Changing Planet: The FY 1990 Research Plan*. Washington, DC: Office of Science and Technology Policy.

U.S. House of Representatives, 1989a. *Department of Veterans Affairs and Housing and Urban Development, and Independent Agencies Appropriations Act, 1990 (H.R. 2916 ENR)*. One Hundred First Congress, First Session. January 3, Washington, DC: U.S. Government Printing Office.

U.S. House of Representatives, Committee on Science, Space, and Technology, Subcommittee on Investigations and Oversight. 1989b. *FSX: Hearings*. One Hundred First Congress, First Session. April 6, May 11, Washington, DC: U.S. Government Printing Office.

U.S. House of Representatives, 1989c. *National Aeronautics and Space Administration Multiyear Authorization Act of 1989 (H.R. 1759 EH)*. One Hundred First Congress, First Session.

U.S. House of Representatives, 1989d. *National Aeronautics and Space Administration Multiyear Authorization Act of 1989 (H.R. 3729 EH)*. One Hundred First Congress, First Session.

U.S. House of Representatives, Committee on Foreign Affairs, Subcommittee on Arms Control, International Security, and Science, Subcommittee on Asian and Pacific Affairs, Subcommittee on International Economic Policy and Trade. 1989e. *United States-Japanese Security Cooperation and the FSX Agreement, hearings and markup on S.J. Res. 113*. One Hundred First Congress, First Session. April 13, May 3, 9, 10, 18, and 23, Washington, DC: U.S. Government Printing Office.

U.S. House of Representatives, 1990a. *Departments of Veterans Affairs and Housing and Urban Development, and Independent Agencies Appropriations Act, 1991 (H.R. 5158 ENR)*. One Hundred First Congress, Second Session. January 23, Washington, DC: U.S. Government Printing Office.

U.S. House of Representatives, Committee on Science, Space, and Technology, Subcommittee on International Scientific Cooperation, Subcommittee on Space Science and Applications. 1990b. *International Cooperation in Earth*

Observing Systems. One Hundred First Congress, Second Session. July 24, Washington, DC: U.S. Government Printing Office.

U.S. House of Representatives, 1990c. *National Aeronautics and Space Administration Multiyear Authorization Act of 1990 (S. 916 EAH)*. One Hundred First Congress, Second Session. September 28, Washington, DC: U.S. Government Printing Office.

U.S. House of Representatives, Committee on Science, Space, and Technology, Subcommittee on Space. 1991. *Earth Observing System Engineering Review*. One Hundred First Congress, First Session. September 26, Washington, DC: U.S. Government Printing Office.

U.S. House of Representatives, Committee on Banking, Finance, and Urban Affairs, Subcommittee on Economic Stabilization. 1989f. *Proposed Joint Development of the FSX Fighter with Japan*. One Hundred First Congress, First Session. April 18, May 5, Washington, DC: U.S. Government Printing Office.

U.S. House of Representatives, Committee on Energy and Commerce, Subcommittee on Commerce, Consumer Protection, and Competitiveness. 1989g. *FSX Codevelopment Project*. One Hundred First Congress, First Session. February 23, Washington, DC: U.S. Government Printing Office.

U.S. Senate, Committee on Environment and Public Works, Subcommittee on Environmental Pollution. 1986. *Ozone Depletion, the Greenhouse Effect, and Climate Change*. Ninety-Ninth Congress, Second Session. June 10, 11, Washington, DC: U.S. Government Printing Office.

U.S. Senate, Committee on Energy and Natural Resources. 1988. *Greenhouse Effect and Global Climate Change*. One Hundredth Congress, First Session. June 23, Washington, DC: U.S. Government Printing Office.

U.S. Senate, Committee on Commerce, Science, and Transportation. 1989a. *Mission to Planet Earth*. One Hundred First Congress, First Session. March 8, Washington, DC: U.S. Government Printing Office.

U.S. Senate, 1989b. *National Aeronautics and Space Administration Authorization Act, Fiscal Year 1990 (S. 916 ES)*. One Hundred First Congress, First Session.

U.S. Senate, 1989c. *National Aeronautics and Space Administration Multiyear Authorization Act of 1989 (H.R. 3729 EAS)*. One Hundred First Congress, First Session.

U.S. Senate, 1989d. *Relative to the Apollo 11 Lunar Mission, the International Space Station Freedom Program and the 'Mission to Planet Earth' (S. Res. 159)*. One Hundred First Congress, First Session. July 20.

U.S. Senate, 1990. *National Aeronautics and Space Administration Multiyear Authorization Act of 1990 (S. 916 EAH)*. One Hundred First Congress, Second Session. 28 September.

U.S. Senate, Committee on Commerce, Science, and Transportation, Subcommittee on Science, Technology, and Space. 1992. *NASA's Earth Observing System*. One Hundred Second Congress, Second Session. February 26, 1992, Washington, DC: U.S. Government Printing Office.

Uchū Kaihatsu Iinkai [Space Activities Commission]. 1989. *Uchū kaihatsu seisaku taikō: heisei gannen 6 gatsu 28 nichi kaitei [Fundamental Outline of Space Policy: June 28, 1989 revision]*. Tokyo. June 28.

Urai Minoru. 2002. Interview by author in Japanese, Tsukuba, Japan, September 26.

Urai Minoru, Kōda Ryōichi, Satō Isao, and Tsu Hiroji. 1985. Randosatto MSS gazō no senjō moyō chūshutsu to karā hyoji [Linear Pattern Detection and Color Presentation for Landsat MSS Images]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 5, no. 4: 5-18.

Urai Minoru, Satō Isao, Ninomiya Yoshiki, Kōda Ryōichi, Miyazaki Yoshinori, and Yamaguchi Yasushi. 1992. Kashi tanpa osa sekigai iki ni okeru nendo tansan'en iwa hansha supekutoru to sono tokuchū [Reflection Spectra and Features of Clay Carbonate Rocks in the Visible Near Infrared Region]. In *Chishitsu rimōto senshingu ni kansuru kenkyū [Research on Geologic Remote Sensing]*, edited by Tsu Hiroji, 11-26. Vol. 278, *Chishitsu chōsajo hōkoku [Reports of the Geological Survey of Japan]*. Ibaraki-ken Tsukuba-shi: Chishitsu chōsajo [Geological Survey of Japan].

Uyehara, Cecil H. 2000. *The U.S.-Japan Science and Technology Agreement: A Drama in Five Acts*. Aldershot, Hampshire: Ashgate Publishing Limited.

Vaughan, Diane. 1996. *The Challenger Launch Decision: Risky Technology, Culture, and Deviance at NASA*. Chicago: University of Chicago Press.

Vidal, A. 1991. Atmospheric and Emissivity Correction of Land Surface Temperatures Measured from Satellite Using Ground Measurements or Satellite Data. *International Journal of Remote Sensing* 12, no. 12: 2449-60.

Vincenti, Walter G. 1990. *What Engineers Know and How They Know It: Analytical Studies from Aeronautical History*. Baltimore, MD: Johns Hopkins University Press.

Voge, Charles. 1992. Product Generation Subsystem. In *ASTER 1st Annual OSSI POP Review, Jet Propulsion Laboratory*, held at Pasadena, CA, December 11.

———. 1993. *Product Generation System Concept*. JPL D-11417. Jet Propulsion Laboratory. June 10.

Voge, Charles, and Steve Larson. 1992. *ASTER Team Member Algorithm Software Development Guidelines*. JPL D-11418. Jet Propulsion Laboratory. October 29.

Vogel, Ezra F. 1979. *Japan as Number One: Lessons for America*. Cambridge: Harvard University Press.

Vogel, Kathleen. 2006. Bioweapons Proliferation: Where Science Studies and Public Policy Collide. *Social Studies of Science* 36, no. 5 (October): 659-90.

Vogel, Steven K., and John Zysman. 2002. Technology. In *U.S.-Japan Relations in a Changing World*, edited by Steven K. Vogel. Washington, DC: Brookings Institution Press.

Wagstyl, Stefan. 1990. US and EC Force Japan to Delay High-Tech Plans. *Financial Times*, November 21, 6.

Waltz, Kenneth N. 1979. *Theory of International Politics*. New York: Random House.

Wan, Zhengming. 1999. *MODIS Land-Surface Temperature Algorithm Theoretical Basis Document (LST ATBD), Version 3.3*. NASA Contract Number: NAS5-31370. University of California Institute for Computational Earth System Science, Santa Barbara. April.

Wan, Zhengming, and Jeff Dozier. 1989. Land-Surface Temperature Measurement from Space: Physical Principles and Inverse Modeling. *Geoscience and Remote Sensing* 27, no. 3: 268-78.

Watanabe, H., F. Mills, M. Sano, and T. Tagawa. 1993. Recent Progress in Geologic Application of Airborne and Spaceborne Remote Sensing. *Advances In Space Research* 13, no. 11: 15.

Watanabe, H., and M. Tsukada. 1989. Geologic Interpretation Japanese ERDS-1 Simulation Data Merged with Geographic Information. Presented at *Tenth Asian Conference on Remote Sensing*, November 23-29, held at Kuala Lumpur.

Watanabe Hiroshi. 1991. Geology W-G. In *Minutes of the Second ASTER Team Meeting*, held at Tokyo, Japan, May 17: Attachment 43.

———. 1992. Evaluation of SWIR bands of ASTER by Mineral Response Simulation (Japex Geoscience Institute, Inc.). In *Minutes of the Third ASTER Science Team Meeting*, Geology Working Group, held at Pasadena, CA, January 22.

———. 1993. User Requirements for the ASTER Mission Operations. In *Minutes of the 6th ASTER Science Team Meeting*, Operations and Mission Planning Working Group, held at Tokyo, Japan, November 9.

———. 2003. Interview by author in Japanese, Tokyo, Japan, June 3.

Watanabe, Hiroshi, Hiroyuki Fujisada, and Isao Satō. 1995. Preliminary Design Concept of ASTER Ground Data System. In *Advanced and Next-Generation Satellites, Proceedings of SPIE, Vol. 2583*, edited by Hiroyuki Fujisada and Martin N. Sweeting, 26-40. Vol. 2583. Bellingham, WA: SPIE--The International Society for Optical Engineering.

Watanabe, Hiroshi, Hiroji Tsu, and Isao Satō. 1998. Technical Challenge of the ASTER Ground Data System. In *Sensors, Systems, and Next-Generation Satellites II*, edited by Hiroyuki Fujisada, 45-52. Vol. 3498, *Proceedings of SPIE*. Bellingham, WA: SPIE--The International Society for Optical Engineering.

Watanabe, Hiroshi, and Makoto Tsukada. 1992. Simulation of ERS-1 OPS by the use of GER Airborne imaging Spectrometer. *Advances In Space Research* 12, no. 7: 389.

Waterton, Claire, and Brian Wynne. 2004. Knowledge and Political Order in the European Environmental Agency. In *States of Knowledge: The Co-Production of Science and Social Order*, edited by Sheila Jasanoff, 87-108, *International Library of Sociology*. London: Routledge.

Watson, K. 1992. Spectral Ratio Method for Measuring Emissivity. *Remote Sensing of Environment* 42: 113-16.

Wendt, Alexander. 1992. Anarchy Is What States Make of It: The Social Construction of Power Politics. *International Organization* 46, no. 2 (Spring): 391-425.

———. 1999. *Social Theory of International Politics*. Edited by Steve Smith. Vol. 67. *Cambridge Series in International Relations*. Cambridge: Cambridge University Press.

Wilson, W. Stanley. 1990. Letter to Anne B. Kahle, April 3.

Winner, Langdon. 1980. Do Artifacts Have Politics? *Daedalus* 109, no. 1 (Winter): 121-36.

Woolgar, Steve. 1981. Interests and Explanation in the Social Study of Science. *Social Studies of Science* 11, no. 3 (August): 365-94.

Woolgar, Steve, and Geoff Cooper. 1999. Do Artefacts Have Ambivalence? Moses' Bridges, Winner's Bridges and Other Urban Legends in S&TS. *Social Studies of Science* 29, no. 3 (June): 433-49.

Yamada Atsushi. 2001. *Neo-tekuno-nashonarizumu: gurōkaru jidai no gijutsu to kokusai kankei [Neo-technonationalism: technology and international relations in a glocal era]*. Tōkyō: Yūhikaku.

Yamaguchi, Y., H. Fujisada, M. Kudoh, T. Kawakami, H. Tsu, A. B. Kahle, and M. Pniel. 1999. ASTER Instrument Characterization and Operation Scenario. *Advances In Space Research* 23, no. 8: 1415-24.

Yamaguchi, Yasushi. 1987. Possible Techniques for Lithologic Discrimination Using the Short-Wavelength-Infrared Bands of the Japanese ERS-1. *Remote Sensing of Environment* 23, no. 1: 117-29.

Yamaguchi Yasushi. 1984. Riniamento kaiseki ni okeru teihukaku SAR gazō kara no shōshahōkō [The Influence of Look-Direction on the Detectability of Lineaments in Low Depression Angle Images]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 4, no. 1: 5-16.

———. 1992a. Geology WG. In *Minutes of the Third ASTER Team Meeting*, Geology Working Group, held at Pasadena, CA, January 21-23.

———. 1992b. Performance of the SWIR Bands. In *Minutes of the Third ASTER Science Team Meeting*, Geology Working Group, held at Pasadena, CA, January 22.

———. 1992c. Summary of the Geology Working Group. In *Minutes of the Fourth ASTER Team Meeting*, held at Tokyo, Japan, June 24: 53.

———. 1997. Policy for VNIR > 8.55 pointing angles. In *Operations and Mission Planning Working Group Minutes*, held at Sioux Falls, SD, May 22.

———. 1999. STAR Categories and Criteria. In *Narrative Minutes of the 16th ASTER Science Team Meeting*, held at Pasadena, CA, January 12-14: 162-66.

———. 2002. Interview by author, Nagoya, Japan, September 17.

———. 2003. ASTER dēta shutoku hōshin [ASTER Data Acquisition Scenario]. Presented at *Dai 6 kai kokusai shinpojiumu - ASTER dēta riyō sokushin ni tuite [6th International Symposium on the Promotion of ASTER Data Utilization]*, held at Tokyo, Japan.

———. 2005. Personal correspondence with author, September 6.

Yamaguchi Yasushi, Satō Isao, and Tsu Hiroji. 1992. ITIR Design Concept and Science Missions. In *The Use of EOS for Studies of Atmospheric Physics*, edited by J.C. Gille and G. Visconti, 299-310. Vol. CXV, *Proceedings of the International School of Physics, "Enrico Fermi," 26 June - 6 July 1990*. Amsterdam: Italian Physical Society, North-Holland.

Yasuoka Yoshifumi. 2003. Interview by author in Japanese, Tokyo, Japan, March 26.

Yasuoka Yoshifumi, and Miyazaki Tadakuni. 1982. Remōto senshingu ni yoru kosui iki no suishitsu bunpu keisoku --kaikibunseki wo mochita taiki hosei to suishitsu suitei-- [Remote Sensing of Water Quality in the Lake --Atmospheric Correction and Water Quality Estimation by Regression Analysis--]. *Nihon Remōto Senshingu Gakkaishi [Journal of The Remote Sensing Society of Japan]* 2, no. 3: 51-62.

Yokota Makoto. 1990. Letter to Shelby G. Tilford, Director, Earth Science and Applications, Office of Space Science and Space Application, [n.d., spring].

———. 1991. Framework of Science Cooperation. In *Minutes of the Second ASTER Team Meeting*, held at Tokyo, Japan, May 15-17.

———. 2004. Personal correspondence with author in Japanese, November 15.

Yoshikawa, Hideo, and Joanne Kauffman. 1994. *Science Has No National Borders: Harry C. Kelly and the Reconstruction of Science and Technology in Postwar Japan*. Cambridge, MA: MIT Press.

Zabusky, Stacia E. 1995. *Launching Europe: An Ethnography of European Cooperation in Space Science*. Princeton: Princeton University Press.

Zaidan hōjin shigen kankyō kaisoku kaiseki sentā [Earth Remote Sensing Data Analysis Center]. 1999. *ASTER kenkyū kōba [ASTER Announcement of Opportunity]*. Kaitei dai ni ba [Version 2]. Zaidan hōjin shigen kankyō kaisoku kaiseki sentā [Earth Remote Sensing Data Analysis Center]. March.

Zammito, John H. 2004. *A Nice Derangement of Epistemes: Post-positivism in the Study of Science from Quine to Latour*. Chicago: University of Chicago Press.

Zysman, John, and Laura Tyson, eds. 1983. *American Industry in International Competition: Government Policies and Corporate Strategies*. Ithaca, NY: Cornell University Press.