

# **Agricultural Economics to the Dyson School A Personal Experience**

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## The Early Years, 1961-1970

I completed a PhD at the University of Minnesota and joined the faculty of the Department of Agricultural Economics at Cornell University in September 1961. The Minnesota graduate program in agricultural economics was essentially the same as the program in general economics; therefore, I had strong training in economic theory, theory of statistics and econometrics. This foundation influenced the courses I taught and my research program, as discussed below.

My choice of Cornell, rather than the University of Wisconsin-Madison, was based on the job description related to the behavior of agricultural prices that seemed a better fit to my professional interests and training. I was taking a risk, however, because the department's faculty was highly inbred, and some senior faculty, wedded to historical research practices, were skeptical of the value of developments in mathematical modeling and econometrics. Indeed, the faculty of about 30 had only five non-Cornell PhDs. The others were Randy Barker (Iowa State), Phil Baumol (Iowa State), Ken Robinson (Harvard), and Bud Stanton (Minnesota). The department head, Glenn Hedlund, must have known that a more diverse faculty was essential for the future well-being of the department, but he found making changes difficult.

My arrival at Cornell was a surprise to the faculty; Hedlund had not told them that I accepted an offer. Perhaps this reflected his concern that influential senior colleagues would be upset by my quantitative orientation. Also, my thesis adviser, Willard Cochrane, was an economic advisor to President Kennedy, and several Cornell faculty members were opponents of Cochrane's proposed farm policy. But Ken Robinson and Bud Stanton were most welcoming, and other younger faculty, though Cornell PhDs, also understood that recruiting faculty from other graduate programs was a good idea. I became friends with many, but especially Ken and Bud.

My initial teaching assignment, along with Ken Robinson, was an upper-division course in agricultural prices with emphasis on the factors influencing the behavior of prices in agricultural commodity markets. Ken, Bud, and I also added and (initially) co-taught a graduate-level course in price analysis. It was an applied econometrics course, which emphasized modeling commodity markets' supply-demand-price behavior. In about 1965, Hedlund gave me one month's notice that I was to teach the undergraduate statistics course. This was a major preparation, and the class had more than 100 students. It was probably not my finest hour in teaching, although I worked hard at it.

I received little guidance about expectations for a research program and publishing. The senior faculty emphasized departmental and college publications, often oriented toward NY State topics. But, it seemed important for me to have a portfolio of research. Thus, I published in professional journals as well as via experiment station bulletins and departmental research series. Of the six articles published in the *American Journal of Agricultural Economics* in the 1960s, one was from my PhD thesis and one was related to futures contract prices, an area emphasized in

much of my later research. The other papers were related to technical issues in econometric applications. These topics were more focused on a particular area than those of older professionals, but less focused than is current practice in applied economics. (An appendix discusses my research and provides references.)

The experiment station bulletins (monographs) contained econometric results that were journal material. For example, one bulletin, co-authored with a PhD student, estimated the seasonal demand for apples and used the results to estimate the storage capacity required in NY State to accommodate the demand for apples from storage. While a NY application, it used a fixed-effects analysis of a covariance model with interaction terms, a relatively unknown model in economics at that time. Department publications, like the revision of the *Index of Prices Received by New York State Farmers*, were of interest mainly within NY State.

There was no feedback about teaching or research, and I was not asked to prepare any material for promotion. However, in June 1966, I received a letter in the mail from the Secretary of the Board of Trustees that I had been promoted to associate professor with tenure effective July 1, 1966 (so different from today!). Promotion suggested that my professional work was acceptable to a majority of the tenured faculty, but I wished someone had told me in person.

From my perspective, two important things happened in the late 1960s. One, Bud Stanton became department chair (a change from "Headships"). He shifted recruiting faculty from an emphasis on Cornell PhDs to an emphasis on nationwide searches. Second, I took a sabbatical leave in 1968-69 at the Food Research Institute, Stanford University; it was the equivalent of a department of applied economics that specialized in international and development economics, and in the economics of commodity markets. Stanford faculty members Roger Gray and Holbrook Working were two of a tiny group of academics studying markets for contracts for future delivery of commodities, i.e., futures markets.

Ken Robinson and I had agreed to draft a text book for the upper-division course in agricultural prices, and I wrote a first draft of my assigned chapters while at Stanford. We found later that commercial publishers did not think a sufficient market existed for this specialized topic to justify publication. Thus, we submitted the manuscript to Cornell Press, and they treated it as a scholarly document subject to anonymous reviews. The comments were positive and helpful, and the Press accepted the book subject to deleting some of the mathematical notation, which at that time was costly to typeset. Thus, the first edition was published in 1972 to favorable reviews and to economic success for Cornell Press. It is now in a fifth edition. The last two editions are somewhat more advanced to serve better-prepared students.

A second outcome of my Stanford leave was a paper about the economic roles of futures contract prices, and the relationship of these prices to varying levels of information. An idea had occurred to me, and I talked to Roger Gray about it. He secured access to Stanford computing resources, and the regression analysis supported my idea. We co-authored a paper that was published in 1970. Both this article and the book received awards from the Agricultural and Applied Economics Association (AAEA) as publications of enduring quality in 1989 and 1997, respectively. I also published a note on the effects of futures markets on wheat prices. It was a productive leave.

While at Stanford, Stanton asked me to visit UC Berkeley to interview three PhD candidates, and this too was productive, as we ultimately recruited two (Tim Mount and Duane Chapman) to the Cornell faculty. Tim's position was explicitly related to econometrics, and soon after he arrived, Tim and I developed a two-semester, graduate-level sequence in applied econometrics. I typically taught the first semester and Tim the second.

As of the late 1960s, the graduate program had a poor reputation among academics. Cornell's system of graduate education permitted students to select their faculty guidance committee, and this committee had complete control over the course program and thesis research. No department-wide minimum standards existed. Thus, while the concept of tailoring programs to individual's interests and needs was potentially desirable (and produced some excellent graduates), a few students chose weak programs, devoid of graduate-level economic theory or quantitative methods. Such programs were permitted because some senior faculty were skeptical of the value of the growing emphasis on mathematics in economics.

It took a long time for the reputation to change, and it is a reminder of the lasting effects of a lack of diversity which stifles change. The increasingly diverse faculty resulted in generally stronger programs, and in this context, perhaps 1,000 students took the first semester of econometrics in the 30 years that I taught it. Additional positives from teaching included the opportunity to interact with many bright students and to keep up to date in economet-

rics as new topics entered the literature. Also, the development of computing hardware and software permitted use of estimators that were only theoretical concepts when I was in a PhD program. I like to think that these courses made an important contribution to the graduate program.

## **The Middle Years, 1970-1988**

I was promoted to professor effective July 1, 1970, and although I was not asked to prepare materials for promotion, Department Chair Stanton did provide a personal communication and congratulations. At that time, my papers (sometimes with coauthors) often had considerable econometric content. For example, a paper (Girao, Tomek and Mount 1974), published in the general economics journal, *Review of Economics and Statistics*, used data from a group of Minnesota farmers who were observable for a period of years. It was possible to partition the data into a group with relatively stable incomes and those with unstable incomes, and then estimate investment and consumption functions for the two groups. Estimating these functions required frontier (at that time) econometric methods that were appropriate for a cross section of farm firms observed over a period of years and that allowed for dynamic behavior. No similar results existed in the literature.

In 1974, I was asked to be the editor of the *American Journal of Agricultural Economics* (AJAE), which also required the selection of associate editors from the Cornell faculty. Stanton encouraged me to do it, and Dick Boisvert and Don Freebairn agreed to be associates. The three of us handled all of the submissions. This was billed as a one-half time appointment for me, and quarter time for the other two. We were supported by a full-time secretary and a technical editor housed at Cornell. Of course, the correspondence was typed and many physical files had to be maintained. We edited three volumes, 1975–1977. Definitive decision-making was perhaps a factor in the efficient handling of submissions. There were fewer postponed decisions with second-round reviews than we see now.

As the editorship responsibilities were completed, I arranged a sabbatical leave with the Economic Research Service, USDA, in 1978-79. This was a productive year, interacting with a small group of professionals with similar interests, headed by Allen Paul. Using data available through the USDA, an article on thin markets was judged the best article published in AJAE in 1980. An article on a method for estimating expected demand relations using futures market prices received little attention when published in 1979 but has been rediscovered.

Paul, Kandice Kahl, and I collaborated on a congressionally mandated analysis of the potato futures market that resulted in a 185-page U.S. Senate Print that was revised into an USDA Technical Bulletin. This study indicated the market was failing because of outdated contract provisions and it would fail on its own, without congressional action, unless the contract was revised. We made some novel suggestions for revising the delivery and settlement provisions, but the contract was not revised and the market went out of existence.

In 1976, I was named a Master Alumnus of the University of Nebraska, where I had earned BSc and MA degrees, and in 1985 a Distinguished Alumnus of the Department of Applied Economics at the University of Minnesota. I was director of graduate studies in the Graduate Field of Agricultural Economics 1979-82. This position administered the admissions process, recommended financial aid, and supervised the graduate program and was analogous to committee service.

In the mid-1980s, a major research effort was devoted to a commissioned book chapter on *Margins on Futures Contracts: Their Economic Roles and Regulation*. This research included conversations with Futures Commission Merchants (FCMs) to understand how the margining system worked in practice for their customers. (Margins are performance bonds intended to prevent defaults on contracts.) The margining system is not well understood by outside observers, including legislators who think margins ought to be set very high to reduce “unnecessary” speculation. My estimates suggested, however, that market places, particularly the Chicago Board of Trade, were already setting margins high relative to their main purpose of preventing defaults.

Futures markets had existed largely for agricultural and a few other commodities since the nineteenth century, but in the late 1960s, futures markets started to develop for other products, including government notes and bonds (interest rate futures), currencies, and stock market indexes. Moreover, markets developed for option contracts, often with the underlying asset being a futures contract. The number of academics doing research on these markets grew, as did the number of academic journals. Most of current research about futures and options markets is considered to be in the domain of financial economics, and in 1982, Kandice Kahl and I published in the new *Journal of Futures Markets*.

The department's faculty placed a high priority on undergraduate teaching, accentuated by the introduction of a program in food industry management in the 1950s. Since no undergraduate business program existed at Cornell at that time, the department began offering courses in accounting, business management, marketing, finance, and business law, as well as classes in applied economics. Many of these courses were very well-taught and therefore attracted many students. Teachers like Dan Sisler and Dick Aplin were legendary, and large, high-quality courses were offered on a "shoe string" in terms of the departmental budget.

In about 1980, I developed and taught a new course, "Introduction to Econometrics," intended for seniors and master's-level students. It required less background in mathematics and statistics than did the PhD-level courses taught by Tim Mount and me. As a consequence of my assuming administrative positions (see below), a new hire took over the introductory econometrics course, and my teaching at the undergraduate level ended. The new hire, Lois Willett, turned out to be a great teacher and adviser.

By the late 1970s, the department was offering both high-quality undergraduate and graduate programs. This likely provided complementarities, as the business students were exposed to analytical applied economics, statistics, and potentially other quantitative courses. Today, such a program is called "business analytics." Also, undergraduate teaching justified a number of TA-ships that helped fund the graduate program.

In 1984, I was asked to be a candidate for president of the American Agricultural Economics Association, now the Agricultural and Applied Economics Association (AAEA). The person elected serves on the AAEA Board for three years, as the president-elect, president, and immediate past-president. The other candidate was a faculty member at Stanford, Wally Falcon, and winning was a surprise. This meant preparing a presidential address for the summer 1985 meetings, and the responsibility of organizing the winter and summer meetings for 1986. The summer 1986 meetings were held at the MGM Grand Hotel in Reno; this was the start of holding meetings in commercial facilities, instead of at universities.

In the 1980s, the association was highly dependent on the president and secretary-treasurer's office for managing the association; it is now professionally managed. By 1985, we were moving rapidly into the personal computer age, and thus, with the help of a graduate student, I tried to use new software to organize the AAEA summer 1986 meetings. The sessions needed to be grouped by subjects and arranged to avoid various types of conflicts (e.g., not having similar topics in the same time slot, not assigning a person to more than one session at the same time). The software was helpful, but the overall planning was very time-consuming.

Presiding over the summer meetings was mentally and physically exhausting. The Directors of the Association meet before the professional meetings start; much business is conducted with a packed agenda. Next, the president hosts a reception for department chairs, association fellows, past presidents, etc. (I was given the presidential suite at the hotel, and it was large enough to hold the reception.) Then, during the professional meetings, the president introduces the principal speakers, talks with student and other groups, helps present awards and the new fellows of the association, as well as other miscellaneous tasks. The key to doing this is being prepared; I even had a few 3x5 cards for talks with small groups. The president also has to respond to complaints, including a few who didn't like the venue (with gambling) and/or preferred meeting at university locations. In some ways, my most memorable time occurred after the meetings were over. I was able to relax in my suite with my close friends Tim and Elizabeth Mount, enjoying champagne as we gazed out over Reno from the top floor of the hotel.

The AAEA Board decided to start publishing a magazine called *Choices*. The intent was to provide decision-makers with research results, evaluations of policies, etc., in readable, accessible articles. I spent much time as the immediate past-president in getting this publication started. (Lyle Schertz, the editor-designate, was extremely helpful in the process.) Ultimately, long after I had left the Board, *Choices* can be judged successful because it was possible to transform it from a print form to an electronic form, making it freely available to a wide audience.

### **Department Chair, 1988-1993**

My term on the AAEA Board was completed in August 1987, and I contemplated returning to a normal work schedule as a Cornell faculty member. But, this was not to be. In February 1988, Dean Call informed Professor Bob Kalter that he would not be reappointed as department chair. Bob was hurt by this decision. Consequently, he moved out of the chair's office almost immediately, and the department was effectively without a leader. After consulting the faculty, the Dean asked me to become the chair. I didn't want the job, but the pressures to do it were irresistible. Thus, I became department chair on April 1(!), 1988. Since I had a six-month sabbatical leave planned at the University of New England in Australia, I negotiated a change to a four-month leave, September – December

1988, and my colleague and friend, Bud Stanton, agreed to be the acting chair for that period. Also, it was understood that my appointment as chair would run to June 30, 1993.

The leave was short but valuable. Among other things, I met Bob Myers, a young Australian agricultural economist who was obviously bright and knowledgeable about the latest developments in time-series econometrics. I worked with him and another colleague on an application of a vector auto-regression (VAR) model to the Australian wool market. Also, this research was a precursor to co-authorships with Bob of a review article on commodity price analysis (1993), and with Bob and Rich Sexton, UC Davis, on a survey of 100 years of research on commodity markets (2010). The leave also provided me with opportunities to see some of Australia's agriculture and to visit a number of major cities. This included attending the opera at Sydney's famous opera house.

A large department inevitably has personnel issues, such as financial malfeasance, alcoholism, mental illness, interpersonal conflicts, etc., and in the 1988-1993 period, a major problem was a decrease in funding. I had to make recommendations and decisions that cut hundreds of thousands of dollars. This necessarily required cutting positions and hence programs and services. Leaving vacancies open helped, but not enough, and across-the-board cuts did not make sense. If you are going to have high-quality instruction, TAs are needed, especially for the large courses we were teaching. Thus, the Dean approved eliminating the local government program. I hand-carried letters to those losing their positions, not a pleasant experience for me or for them. They were given considerable time (90 days, I think) before they were dropped from the payroll. I also terminated a staff person who was not keeping up with modern computing technology and who had another problem. This person was well-liked by some of my colleagues, and I caught considerable static for that decision.

Department chairs had an annual budget review with the dean and his associates. It was somewhat like a combined written and oral PhD exam. One prepared documents about the department's programs and expenditures, and met with the deans to discuss current and future programs in resident instruction, cooperative extension, and research. There were sharp questions about particular projects and outcomes. A chair should be knowledgeable about "everything" going on in the department.

Another important task was making recommendations for salary increases. The chair was given a fixed-dollar amount for faculty (and a separate amount for staff), and the dollars had to be allocated throughout faculty and staff so they summed to the exact total allocation. One could make a case for an extra amount for particular faculty members based on performance. Clearly a chair had to prepare by evaluating each faculty member's work. I used quantitative measures like course evaluations and publications, but judgment was also involved. I used qualitative terms: stars, rising stars, solid citizens, under-performing, and "deadwood." Perhaps one faculty member was in this latter category, though he wouldn't have thought so. He was encouraged to retire by the Dean, and he did so.

A computerized system for salary allocations was introduced while I was chair. One entered the recommended increase, and the system accumulated the increases. Thus, it was possible to hit the target total increase given to the department. There was space for comments and to ask for extra amounts for individual faculty members. Unfortunately the program had an error, and all of the comments were lost (for all faculty). So, the entire process had to be redone after the bug was fixed. It was a loss of many hours of time for me and for the other chairs. Obviously, colleagues don't know the varied challenges of being a department chair.

At that time, administrative duties were centered on departments, and bill payments, record-keeping, and so forth were "paper based." Thus, the departmental office had an administrative manager, an accounts clerk, a building manager, and my private secretary. I felt that we were a team and that our objective was to support the faculty and their work. The administrative manager was the main interface with the faculty on business-related issues, such as research funding and account management, and this occasionally led to unhappiness when we couldn't accommodate a faculty member's wish. The administrative manager and I had the responsibility of approving the payment of bills. Before I went home, I could be found signing a large stack of documents. Today, many of these tasks are paperless and processed at the college level.

One morning a graduate student came to see me to report that someone was trying to kill him. It rather quickly became apparent that he had a serious mental health issue. Fortunately several small "miracles" happened. First, he agreed to walk with me to the student health center, Gannett Clinic. I asked my secretary to call ahead that we were coming. So, when we arrived at the clinic, we avoided the paperwork and went immediately to a doctor's office. He made the judgment that the student should be admitted to the hospital, but this required approval of a parent. The second "miracle" was his mother answered the phone in the middle of a weekday morning. He was

admitted to the hospital and dropped out of Cornell for more than a year. However, he returned and finished his master's degree. Moreover he wrote me a thank you note for my help. Of course, I really hadn't done anything, except escort him to Gannett Clinic.

One of my habits was to walk to and from campus, about 30 minutes each way. The walk home helped work off the frustrations of the day, and the walk to campus was sometimes used to formulate exam questions or to "solve" other problems. I had to be careful about the exam questions, because it was fun to think of challenging ones. On one occasion, I did use a question (not intentionally) that was so difficult only one of the 30-plus students answered it. I suppose it stimulated a lot of commentary amongst the students.

With declining income during the 1988–1993 period, only one assistant professor was hired while I was department chair (Greg Poe, a strong appointment). New endowments allowed the department to fill two senior positions. The position in environmental policy had two strong finalists, and the department faculty was split 50-50 on the choice. I knew one of the candidates personally and liked him, but the other candidate was favored by almost 100% of faculty outside of the department as well as the dean's office. Thus, I recommended that person, and while many of my colleagues were upset by the decision, it worked out well. The other position, in small business management, went to a faculty member from the University of Illinois who had an excellent external reputation in research and teaching. But, allegations of various kinds of malfeasance surfaced after he joined our faculty. The Dean's office was informed. I left the chair's position before any conclusions had been drawn, but the professor in question was fired and notably he did not contest the decision.

An opportunity to recruit another faculty member arose from observing the increasing prominence of an African-American faculty member at LSU, and I knew that his PhD adviser at Michigan State thought highly of him. So, I made a "pitch," with the faculty's approval, to the Dean's office to hire Ralph Christy through a directed search. From my perspective, we were recruiting a strong candidate who was also African-American. This was a source of funding not otherwise available. He was an excellent addition for the department and Cornell. Ralph is a terrific classroom teacher and role model, and he has dealt well with the many pressures that exist for a person from an under-represented minority group. He was asked to serve on numerous committees, take various administrative posts, etc. And he was elected president of AAEA. As this is written (2016), he is the director of CIIFAD (Cornell International Institute for Food, Agriculture and Development), a one-half time administrative appointment while still teaching.

Department chairs get invited to conferences and are expected to participate and "show the flag" at these events. Also, I provided leadership for considering a change in the name of the department. We ultimately settled on the Department of Agricultural, Resource and Managerial Economics (ARME). This name was a bit long, but specifically identified major components of the department's programs. We were reluctant, at that time, to use "applied economics," thinking that colleagues in labor economics and consumer economics would object.

I continued to teach the first semester of the econometrics sequence, and I also wrote pieces on the effects of futures and options markets on farm incomes and on the nature and value of replicating econometric results. The latter essay was published in an issue of AJAE that celebrated its 75<sup>th</sup> anniversary in 1993. The Tomek and Robinson textbook was revised for a third edition. Ken had retired before I became department chair, but contributed to the revision. Ken influenced my career in numerous ways. He and his wife, Jean, were incredibly generous with their time and friendship. Ken was an excellent teacher both on campus and in off-campus extension settings. He was, however, skeptical of the value of many journal articles. His guiding question was, is this research useful to society? He had high standards for himself and others and was a challenging co-author. (In addition to the book, we collaborated on a substantial review of the literature on price analysis and outlook research.) Although Ken was elected a fellow of AAEA, his contributions to students and society in general may not be fully appreciated.

My appointment had always included a small extension component, and starting in the 1970s, I collaborated with other faculty in preparing the monthly *Current Economic Situation*. It was a one-page document that covered the general economic situation on one side and the agricultural economic situation on the other. About one-third of each side was devoted to economic data, such as price indexes and commodity prices, including year-to-year comparisons as well as comparing the most recent month with the prior month. The remaining space was used for a discussion of the data or other relevant topics. Typically I prepared this document six months of the year, and Ken Robinson, Bud Stanton, or John Brake (after Ken retired) assumed the other six months. This was the era of mimeographing, maintaining mailing labels, and using surface mail. The internet and technological changes eventually overtook the need for such a publication.

While department chair, I was elected a Fellow of AAEA (1989), and received the Earle M. Combs Award from the Chicago Board of Trade Educational Research Foundation (1992) for my contributions to the body of knowledge concerning futures markets. The latter award was given at a black-tie dinner, and I mostly remember lack of sleep that night because of the food and wine.

## **On Achieving Faculty Diversity**

With Stanton and then Olan Forker and Bob Kalter as Chairs, the department made steady progress in diversifying the PhD training of its faculty. By the mid-1970s it also became clear that other types of diversity needed attention. The department granted its first PhD to a woman in the mid-1960s, and the number of women in PhD programs was growing slowly. However, graduate programs had very few students from under-represented minorities, even though there were, and still are, PhD candidates from many countries around the world, including Africa and South America.

Six women had been appointed to the faculty before I became chair in 1988. One was not promoted. Three did well and were promoted. But, in one case, the trailing spouse did not receive tenure, and in another case, she and her spouse (a business person) made a joint decision to move. The productivity of another woman stalled for reasons that are unclear to me (I had left the chair's position), and she took early retirement. A fifth appointment was to a lecturer position in business law, which was successful, and she was promoted to senior lecturer. The sixth was struggling with both teaching and research at the time I became chair. I decided to be her mentor, and it helped. She now holds the endowed chair in small business management that was vacated by the person from Illinois (mentioned earlier). In sum, only two of the original six appointments of women remained on the faculty in 2016. Currently there are 14 women on a faculty of about 60, still less than the proportion of women in PhD programs.

A Hispanic American had been appointed to an extension-research position before I became chair, and his research and extension efforts were "a mile wide and an inch deep," and were unlikely to result in a positive tenure decision. Thus, I counseled him on a number of occasions to do fewer things in greater depth, but my advice was not heeded. He did not receive a favorable tenure vote from the department faculty, and this resulted in accusations of bias and in a prolonged process of reviews. Ultimately, he left Cornell.

Into the late 1990s, the Department Chair was still doing annual reviews of junior faculty and periodic reviews of other faculty, and when I was the acting chair for a few months, while my successor was on a personal leave, I helped him by conducting a few reviews. In one case, it was clear to me that an assistant professor was an average teacher with low research productivity. I provided this assessment, and a few years later this assessment appeared correct to most tenured faculty. He did not receive a positive tenure decision, and appealed the decision, in part, on the basis of bias because he was gay. At the time of my review I had no idea he was gay. Ultimately, the decision was upheld.

The foregoing cases illustrate the challenges associated with taking account of diversity in making appointments and in tenure and promotion decisions. Another aspect of recruiting a diverse faculty is finding positions for spouses. During the 1988–1993 period, we had the opportunity to accept appointments of two trailing spouses. I favored these appointments, but the faculty tended to be suspicious of such appointments, as prefilling future vacancies, and thereby preempting their input on new hires. Nonetheless, the faculty agreed to accept a woman PhD in statistics as it met an important need in teaching undergraduate statistics. Cindy van Es preferred an appointment as a lecturer, because she had small children and did not want the publishing obligation required for a favorable tenure decision. She became a great teacher and adviser and was promoted to senior lecturer, which we treat as analogous to a tenured position. Cindy is now a "professor of practice," a new title for faculty whose responsibilities extend beyond classroom teaching, and who have strong professional experience, but few professional publications.

I was unsuccessful, however, in convincing the faculty to accept a woman PhD (from MIT) in business management. I was also unsuccessful in convincing an African American research associate to consider an assistant professor position. (Money would have been available for this appointment.) He did remain on our staff via grant funds.

When we were advertising to fill the endowed professorship in environmental policy (mentioned earlier), I called two distinguished women at other universities and encouraged them to apply. Both consulted their spouses and declined. In contrast, the ultimately successful candidate told me up front that if he was offered the job, we would

have to find a position for his wife. (She was also an economist.) We did hire him, and she accepted a position in consumer economics. **Achieving a diverse faculty has been and likely will continue to be a major challenge.**

## **The Later Years, 1993-2000**

On July 1, 1993, I returned to full-time teaching and research and, before retiring, I was the major adviser to six graduate students, four doctoral and two master's candidates. Five of the six were women, and all six were from countries other than the U.S. Although I secured a competitive grant for a project (and also a modest gift from the Chicago Board of Trade), their research involved a range of topics, but were all related to commodity markets. These students were intelligent and motivated. While making suggestions about specific research topics, I gave the PhDs considerable latitude in the choice of their research topic.

The principal model for the grant-supported research turned out to be difficult to build and calibrate. The student, Hikaru Hanawa Peterson from Japan, persisted, however, and did a great job. I very much appreciated her perseverance. She and her spouse, Jeff Peterson, are now on the faculty in Applied Economics at the University of Minnesota.

The Russian man's MS thesis won a national award; he returned to Moscow as a consultant. Incidentally, he was a wonderful pianist. The other master's candidate, from Argentina, went to the University of Illinois for a PhD and is now an academic in Canada. Other PhDs, both from earlier and more recent years, have had highly successful careers in the private sector and government. These include positions with a consulting firm (a principal of the firm); Ernest and Young; the Farm Service Agency, USDA; Government Accountability Office (GAO); Standard Chartered Bank, London; and the Bureau of Agricultural and Resource Economics, Australian government. Others hold or have held faculty positions in universities in the U.K., Taiwan, and Australia, as well as in North America. A common thread in most of these jobs is the individual's ability to do high-quality empirical analyses to address specific problems. Of course, the faculty is always pleased to see the success of our alumni.

In addition to coauthoring with graduate students, I continued to do some independent writing and to teach econometrics and a futures market-related course. My extension effort was limited to preparing the general economic outlook, and grain and feed outlook, for the department's annual outlook conference. After retiring I continued to do the grain and feed outlook with co-authors until 2010. A relatively simple model for predicting feed prices in NY State was published and took account of a new source of demand: the use for grains and oilseeds to make biofuels (Schmit, Verteramo and Tomek 2009).

I advised undergraduates throughout my entire career, but typically only four or five new ones each year (i.e., about 20 at any one point in time). I advised them to take a general program, including courses like history, government, etc., as I felt the requirements for the major provided sufficient basic training in economics, finance, and similar courses. I also encouraged students to follow their passions, even if they appeared unrealistic. These alumni are in a wide range of jobs, including (at this writing) general manager of the Texas Rangers baseball team, viola player in the Metropolitan Opera Orchestra, real estate broker, consultant, and bank president. It was rare for our program majors to go on to PhD programs, though many ultimately did MBA or law degrees.

Most academics do considerable traveling to attend conferences, professional meetings, etc. Among my most memorable trips in the 1990s were two to a university in Nitra, Slovakia. We had a teaching program located there, and on one trip Bill Lesser and I taught a short course on markets and price behavior. On the other, Tim Mount and I taught a course on applied econometrics. These experiences occurred after the collapse of the communist government. The objective of the Cornell program was to teach modern economics.

I'm not sure how much these Eastern European students learned, but little things made an impression on me, such as the realities of exchange rates. For example, Lesser and I went to the opera in Bratislava where a box seat was over 120 korona, but with an exchange rate of over 40 to 1, this was equivalent to \$3. Just 40 kilometers away in Vienna, opera ticket prices were like those at the Met in NY City, i.e., \$150+ for a similar seat location. Our Slovak hosts treated us well, and on one of the trips, they took us to a retreat (previously used by the communist elite) in the Carpathian Mountains on the Polish border. It appeared to have great potential as a ski resort. We hiked down a mountain, which I should not have done; I had walking shoes, but not hiking boots; it was exhausting.

For many years, I was a member of the education committee of the Chicago Board of Trade (CBOT). Among other things, we sponsored research symposia on futures markets-related topics. From 1986 to 1992, I was an associate editor of the proceedings of these conferences. Then, a decision was made to publish the papers as part of a new

journal, *Review of Futures Markets*, and to hold some of the conferences at international locations. I became a co-editor of this journal, 1993 to 2001, after which the journal was spun off to an academic publisher, and though little known, it still exists. From my perspective, the conferences were valuable as a way to interact with academics and traders, but the CBOT gradually reduced the number of conferences, most of which were held overseas. As a result, the interaction of academics with private sector people was reduced.

The 1990s and 2000s were a time of major changes for the former agricultural economics department. Having been renamed ARME (Agricultural, Resource and Managerial Economics), it next became the Department of Applied Economics and Management (AEM), then the School of Applied Economics and Management, and is now the Dyson School of Applied Economics and Management. As of 2015-2016, the school had more than 700 undergraduate majors, about 100 graduate students, and about 60 faculty.

Fourteen faculty hold named chairs, and eight assistant professors are faculty fellows. The school's faculty is likely one of the most diversified in the U.S. by some metrics. While it is difficult to recruit under-represented minorities, the Dyson School has faculty from India, China, Canada, Portugal, England, Finland, Lithuania, France, Colombia, and Venezuela. Their graduate training ranges from Oxford, Helsinki, Edinburgh to Harvard, Brown, Columbia, Duke to Chicago, Wisconsin, Minnesota, Illinois, on to UC Berkeley, UC Davis, Cal Tech—and this is not a complete list.

The graduate program's quality and reputation has improved immensely, and it is now among the best of its type anywhere in the world. Alumni of the program hold positions in distinguished universities, government and non-governmental agencies, and in the private sector. Since 2000, six alumni have been named fellows of AAEA and three elected president of AAEA. In 2016, Jan Low (PhD 1994) was a co-recipient of the World Food Prize.

From the viewpoint of empirical research, one of the biggest changes over my career was in computing power. My PhD thesis research used a desk calculator to invert matrices as large as five-by-five. With check columns and check rows, such computations took four or more hours. Next, one used a mainframe computer with 80 column punch cards, and by the early '70s, it was possible to estimate the parameters of a "system of simultaneous over-identified equations." Today, the answers to similar problems appear on one's personal computer screen immediately after you press the return key.

In addition, modern computing facilities allow analysis of very large data sets. A simulation model of corn prices was developed and used to generate monthly observations for 10,000, 40-year life-times. That is, it was possible to imagine a farmer producing corn (maize) over 40-year periods, facing 10,000 different price scenarios all based on the same market structure. What differed for each run of a 40-year "history" was the random events modeled in the various equations. Thus, probability distributions of prices were generated and used to examine the expected outcomes from alternative marketing strategies. It was possible to demonstrate, for example, the probabilities of farming in "lucky" and "unlucky" 40-year periods. None of this would have been possible in earlier years.

In my opinion, it is difficult for an academic to judge his/her effectiveness in the classroom and research as we age. So, I made a decision to retire before my 68<sup>th</sup> birthday; this implied a logical retirement date of June 30, 2000. But, an early retirement incentive was announced in the fall of 1999, and I took it, retiring January 1, 2000. The department rehired me to teach econometrics in the spring of 2000. Also, for a few years thereafter, I taught a master's level course on commodity futures markets, just for the fun of it. Ultimately, I reduced it to a two-credit class for seven weeks. I stopped teaching completely in the spring of 2012.

As one approaches retirement, it's natural to reflect on the past. For me, Cornell was a fortunate choice in many dimensions. There are wonderful colleagues and staff, many bright and motivated students, and superb supporting resources such as the library system. Indeed, I sometimes wondered if the various accolades about my research and writing were deserved. The self-doubt was accentuated by the many distinguished colleagues I have known.

Paradoxically, there were times, now passed, when I wondered if my Cornell colleagues appreciated my work. Although I had been elected president and a fellow of my professional society and received recognitions from my alma maters (Nebraska and Minnesota) and others, most of my Cornell colleagues did not seem to notice. However, at retirement, a departmental research award was established in my name (although now, some 15+ years later, it is being used to recognize the best paper by a second-year graduate student).

Alumni of the graduate program have made positive comments about the value of the econometrics sequence, but it is less clear to me and, I think, to Tim Mount that our colleagues understood the effort required to teach

high-quality courses in econometrics. Lectures need continuous updating, homework exercises and exams prepared, and term papers read and graded with comments. Econometrics is a dynamic subject as we gain experience from applications and as new ideas enter the literature. And the varied levels of preparation of students is another challenge for teachers.

I requested no retirement event, and this request was honored. Rather, John and Francille Firebaugh hosted a private retirement dinner. Francille was Dean of the College of Human Ecology, but she, John and I had met through service on the board of the Protestant Cooperative Ministry (PCM) at Cornell, and we also had mutual friends, including Ken and Jean Robinson (Jean was the Associate Dean in Human Ecology). Others at the dinner were Bud and Lara Stanton, Tim and Elizabeth Mount, the Robinsons, Terry and Taryn (Chaplain of PCM) Mattice, and my friend Mary Lou Kish. Conversation was not about past achievements but more like celebrating a birthday or anniversary. It was a good time with good friends. In addition, my church, Forest Home Chapel, held a retirement recognition at a fellowship time, which I appreciated.

In retirement, I received an alumni achievement award from the University of Nebraska's College of Agriculture and Natural Resources, an award of merit from Nebraska's College of Fine and Performing Arts, and also the first distinguished alumnus award made by the Department of Agricultural Economics at Nebraska. At Cornell, I continued some professional activities and enjoyed coffee on campus with other retirees and active faculty. The professional side included chairing the Senior Section of AAEEA and doing some writing. Coffee included wide-ranging conversations about many topics, from local sports to world-wide economic issues.

## **Other Cornell Experiences**

Tenured faculty are often involved in committees and activities outside their home department, and I served on a few ad hoc committees appointed by the Dean of the Faculty. A committee's charge typically was to review a specific problem and make a recommendation about how to address it. Often, no clear solution existed. For example, an assistant professor did not have a strong case for tenure, but his department chair had written misleading, favorable annual reviews. The committee recommended a year's extension of the appointment to allow the candidate to strengthen the case or find another position. (I do not know the actual outcome.)

I was elected to the Faculty Committee on Music, which provided advice to a concert manager about the classical music series sponsored by Cornell. I had, and still have, a strong interest in classical music, but my main contribution, along with president-emeritus Dale Corson, was to discuss the trade-offs among ticket prices, demand for particular performances, and the costs (fees) of the performers. Star performers command large fees, but a limit exists on price that could be charged if we wanted to fill Bailey Hall. The cost of a concert always exceeds ticket income, but an objective was to arrange a combination of performances and prices that minimized losses while achieving other objectives, like a diverse set of concerts.

A senior did an honors thesis under my direction that estimated the demand for classical music concerts at Cornell, and her analysis showed the obvious, namely that the demand for major stars is larger than for the lesser-known (though high-quality) performers. It also showed that ticket sales for the less well-known performers were more sensitive to prices than those for the major stars. This research, however, did not solve the issue of pricing individual concerts for varying types of performances (e.g., a major orchestra versus a recital). Also, a fairly simple pricing scheme was required, particularly for managing sales for an entire series.

I also served on the Board of the Protestant Cooperative Ministry (PCM) on two separate six-year occasions, 1992-98 and again for six years after I retired. I was the treasurer for a portion of those later years. While Cornell does not provide financial support for campus ministries, they are Cornell affiliates that can rent space in Anabel Taylor Hall. As affiliates, they also can receive gifts from alumni, place funds in Cornell endowment accounts, and use the Cornell accounting system to make payments from endowment income. As a former department chair, I was somewhat familiar with this system.

Campus ministries can play important roles on campus. The chaplains provide counseling that supplements the services provided by Cornell, and some students prefer attending worship services on campus with other students rather than at a local church. But, such chaplaincies are not financially self-supporting. PCM was formed to combine the financial support of four mainline Protestant denominations, but their support declined throughout the years. Thus, a major contribution of the PCM Board in the 2000s was a campaign to increase the endowment. It was successful, and endowment income now provides an important component of income for this ministry.

A benefit of committee and board service is meeting and working with talented, nice people. As mentioned earlier, I became acquainted with Francille and John Firebaugh as a member of the PCM Board. This led to friendship and invitations to their home. In addition, I came to appreciate the depth of commitment to PCM of some Cornell alumni, like the Rev. Fred Kelsey, as well as Cornell staff and faculty.

Another institution that broadened my circle of friends was the so-called “bachelors’ table” at the Rathskeller in the old Statler Hotel. (It existed on campus until about 1985 when it was replaced by a new larger hotel.) The table was large and circular where unaccompanied people—singles and couples—could join others for dinner and conversation. Through that institution, I met a broad range of faculty. In October 1982, I awoke to hear the news that Ken Wilson had received the Nobel Prize in physics. He was a regular at the bachelors’ table. Then, in October 1983, I learned that Barbara McClintock had received the Nobel in medicine and physiology for her work in genetics. Her lab was on Long Island, but when on campus, she sometimes joined the bachelors’ table. When the Rathskeller closed, a Thursday night dinner group arose that continued to meet at a variety of restaurants.

In addition to Wilson and McClintock, I knew two Nobel recipients in economics. Professor Leo Hurwicz was a member of my PhD guidance committee at Minnesota, and Rob Engle was a PhD student in economics at Cornell. These personal connections strengthened a sense of humility and a recognition that we all have some similar joys and concerns. Ken Wilson, for example, was especially concerned about the public speaking that would be expected of him as a Nobel Prize winner. I could relate to that, as public speaking was not my favorite activity.

In retirement, I agreed to two confidential activities. One was service on the committee that made recommendations for the Stephen Weiss Fellowship for Teaching, the highest recognition for teaching that Cornell gives to tenured professors. The committee’s activities are managed by the Dean of the Faculty’s office, and confidentiality helped avoid outside influences about decisions. The committee consisted of three previous awardees, three upper-division students, and three emeritus faculty members. The dean’s office solicited nominations, and after nominations were closed, committee members read the nomination materials and independently ranked the candidates. The committee then met, tabulated the rankings, and determined those to be kept. The objective was to reduce the number to (usually) five or six nominees to be forwarded to the president, who made the final selection of three.

One important task involved writing a letter to the President for each nominee, summarizing their strengths. This responsibility was shared across the committee. The committee chair also wrote a cover letter to the president. Typically the committee did not rank the nominees, and the president was free to choose the three he preferred. One concern about this process was feedback to nominators. It was possible that a nominee was recommended to the president, not appointed a Weiss Fellow by the president, but then not re-nominated the next year. Hopefully better feedback is now provided so that re-nominations are encouraged when appropriate. There was at least one case of a nominator expressing anger in a letter to the Dean of the Faculty because their nominee did not win, accusing the committee of bias against women. The committee, however, had recommended the candidate, but the president had not placed the person in the top three. With anonymity, we couldn’t respond to explain the situation or implicate the president.

The second activity was a part-time job. Bill Fry, the Senior Associate Dean of CALS, asked me to assist with the college’s promotion process. At that time, Bill was the only senior associate and was overwhelmed with tasks. I agreed to help and did so until he left the dean’s office. My job included making suggestions for members of ad hoc committees who provided the dean’s office with an independent assessment of the candidate’s qualifications. However, my principal task was drafting letters to the provost for the dean’s signature. Susan Henry was the dean, and of course I was told whether the recommendation was to be positive or negative. I certainly did not make decisions. Reviewing the considerable set of materials and writing a letter turned out to be quite time consuming. I should have asked for higher pay (ha), but the job provided insights into the college’s very distinguished faculty.

I never knew whether my letters were revised, edited, or used as is. In most cases, the outcome (provost’s expected decision) seemed clear, but in a few cases, I was left to wonder about the final decision. I remember a case where a department’s faculty was split 50-50 on a promotion. The chair had recommended denial of promotion, but Dean Henry felt strongly that the person should be promoted. (I could understand why; it looked like potential gender bias.) Bill told me that Dean Henry hand-carried the letter to the provost and argued the case in person. She was successful. Hopefully my draft was helpful.

Finally, I have participated in a morning-coffee faculty group from the time that I arrived at Cornell. This group continues today, now consisting mostly of emeritus faculty, but also a few active faculty. The wide-ranging conversations cover Cornell, politics, sports, etc. This group certainly remains interested in the well-being of the Dyson School.

## Final Comments

My thesis adviser at Minnesota, Willard Cochrane (PhD Harvard), did not have a positive view of the Cornell agricultural economics faculty in 1961, and he was surprised that I had accepted their job offer. But he made the perceptive comment, “Cornell is an outstanding university and the department can change.” He was correct. The Dyson faculty evolved to become one of the best of its kind anywhere in the world.

The level of compensation and the superb retirement plan were another blessing. Born in the great depression, my instinct was to live frugally. But, I did indulge one luxury: opera. Over the years, I attended operas in numerous locations, including Seattle, San Francisco, Santa Fe, Sarasota, Florida, summer festivals in Colorado, and of course nearby Glimmerglass Opera in Cooperstown, NY. I purchased out-of-town series at Chicago Lyric Opera and packages for the Met in NY City. It allowed me to attend three or four operas each visit. Vacations and business trips also took me to Sydney, Australia, the former Soviet Union, and some European locations.

On July 1, 2016, the Dyson School became a part of a new Cornell College of Business. The other units are the Hotel School and the Johnson Graduate School of Business. Each school is supposed to retain its unique characteristics, but some decisions seem to suggest otherwise. It will be interesting to see if the Dyson School’s Land Grant mission and other distinctive attributes will survive the reorganization.

I moved to Kendal at Ithaca in 2013, a retirement community not far from the Cornell campus and with friends nearby. I have indeed been blessed.

Bill Tomek

## Appendix: A Review of Research

This appendix surveys much, but not all, of my research. It emphasizes the behavior of the prices of agricultural commodities, including the prices of contracts for the future delivery of commodities. A context for this research can be found in the various editions of the book *Agricultural Product Prices* (e.g., Tomek and Kaiser 2014). A time series of commodity prices has systematic components such as seasonality, cycles, and trends, plus random components (Tomek and Kaiser 2014, Chapter 9). In contrast, a time series of prices of a contract for future delivery can be viewed as a series of predictions of the maturity-date price for the contract’s delivery month; futures prices are sometimes called “anticipatory” prices and are equivalent to the economists’ definition of rationally expected prices (Tomek and Kaiser 2014, Chapter 12). In theory, the price of a futures contract and the price of the underlying commodity should converge as the maturity date approaches.

### *Cash (Spot) and Futures Prices*

Much of my early research was about the behavior of the demand for and supply of agricultural products and, therefore, about the factors influencing the behavior of commodity prices. In other words, one is modeling the reasons for the systematic variability of prices (and quantities) of particular commodities, so-called “structural” models.

The demand for most major agricultural commodities has long been thought to be price inelastic, especially at the farm level. If, for example, supply increases while demand is stable, the price decline will be proportionately larger than the quantity increase; hence, the larger supply reduces total revenue. Proposals for government programs to control supplies were based on the idea that farm-level (derived) demand was price inelastic and hence that increases in supply that exceeded increases in demand would reduce farm revenue. In this debate, opponents of supply control argued that while demand might be price inelastic in the short run, it was likely elastic in the long run. In this context, my PhD research estimated the demand for meats, so-called structural equations, to try to ascertain if demand was price elastic in the long run. The Tomek and Cochrane (1962) paper addressed the question: was the long-run retail demand for beef and pork elastic, even if inelastic in the short run? The answer was no, although the quantity response to price changes was somewhat larger in the long run (Tomek 1965).

Other research estimated farm-level demand relationships for commodities important in New York State, such as apples and cherries. A study of the seasonal demand for apples (Ben-David and Tomek 1965) was motivated by the introduction of controlled atmosphere storage, which extended the storage life of apples. Thus, the question arose, how much storage capacity would be needed to accommodate the potential demand for apples over a longer marketing year? Another study examined the allocation of apple production amongst fresh and processing uses; this is a case where current production is determined by decisions made in the past, but the allocations of this supply to various end-uses are influenced by relative prices. Thus, prices and uses are simultaneously determined, and new software allowed me to estimate the model's parameters with a simultaneous equations estimator (Tomek 1968b).

More recent studies dealt with import or export components of demand (Hanawa, Willett and Tomek 1997; Le, Kaiser and Tomek 1998; Soshnin, Tomek and de Gorter 1999). In contrast to a structural modeling approach, Myers, Piggott and Tomek (1990) reported on a vector auto-regression specification of the Australian wool market. This is an alternative modelling approach to partitioning the variability of prices between systematic and random components.

As time passed, my research shifted toward the behavior of prices for contracts for future delivery, the relationship of futures prices to the cash prices of the underlying commodity (basis relationships), and the implications of these relationships for decision-makers. One of my most cited papers (Tomek and Gray 1970) is about the roles of futures prices in allocating resources, especially inventories, and as predictions of prices at contract maturity. We argued that the resource allocation roles of futures prices are consistent with thinking of these prices as unbiased predictions of maturity date prices. It is also true, however, that the statistical quality of the predictions can differ among commodities, because the quality of the conditioning information varies for different commodities. The paper contrasted the corn with the potato market.

An especially interesting project was a study of the futures market for Maine-grown potatoes (Paul, Kahl and Tomek 1981). This contract required delivery of Maine-grown potatoes, but was being used by producers and dealers from other regions. As time passed, the supply of deliverable potatoes decreased relative to the open interest (quantity that might require delivery), especially in contracts for delivery months late in the storage season. Consequently, the futures market sometimes priced a specialized commodity—potatoes that met delivery requirements—that became independent of the price of potatoes in the broader market; i.e., futures and cash prices (received by most producers) did not converge as the maturity date approached. This is a disaster for hedging positions that require convergence of the two prices to be successful. Our research identified the problem and suggested a solution that required revising the contract. But our suggestions were not adopted, and the market failed. The failure validated the view that, to be successful, futures markets require hedging use; they cannot survive only with speculative use.

As more academics undertook research on futures markets, a huge literature developed. One popular topic was estimating optimal hedge levels, i.e., the size of the position in futures contracts relative to the position in the cash market that maximized or minimized an objective. Research by some of my master's and doctoral students contributed to the literature, though much of our efforts were devoted to trying to understand why estimates of optimal hedge levels were typically much larger than farmers' actual practice.

Often, the implicit or explicit objective function underlying the empirical research, in my view, over-simplified decision-makers' objectives. The assumptions underlying the specification of the objective function were inconsistent with the reality faced by decision-makers. Harwood and Tomek (1987) showed that the inconsistency between the estimated levels and the actual practice could not be explained by technical issues in estimating the parameters (variances and covariances) that are components of the estimated optimal hedges. On the other hand, estimates of optimal hedges for a growing crop typically ignored yield risk, and Greenhall, Tomek and Tauer (1984) show that the optimal size of hedges placed at planting time were perhaps 20 percent of expected production, not the 90 percent found in some research. This analysis benefited from using data from individual farms in Illinois and New York, which provided estimates of yield risk faced by individual producers. As the crop matured and yield risk declined, the optimal hedge level increased, but price (basis) risk remained and can be large for individual farmers.

My last PhD student, Dabin Wang, undertook a project that proved to be undoable in a reasonable length of time, namely estimating optimal hedge levels for New York State dairy farmers. The idea was to use Class III milk futures contracts and corn and soybean meal futures contracts to assure approximately a positive margin between milk prices and feed costs. But, many practical problems make obtaining useful estimates exceedingly difficult. The futures contract for milk specifies cash settlement using the Class III price released monthly by the USDA

(which in turn is based on spot market prices of products like cheese); however, farmers receive a blend price for four classes of milk; this complicates measuring the basis for producers. Since milk is produced continuously, futures contracts are traded for all 12 months of the year, but corn and soybean contracts are not. This raises the question of how many different contracts should a farmer use (how far into the future to lock-in prices?) and what combination of contracts to use, when to initiate positions and roll them over, varying the size of the positions in futures as time passes, etc. Thus, while Dabin made a substantial effort to address these issues, we judged that the empirical estimates were not useful to dairy farmers. Her supplementary research did produce two publications (Wang and Tomek 2005; Wang and Tomek 2007) that are discussed elsewhere.

(*Aside:* Dr. Wang is, as of 2016, the Executive Director, Firmwide Market Risk and Governance, J.P. Morgan Chase. Risk management at this major bank is clearly a much different order of magnitude than on a dairy farm.)

Since hedging can be thought of as assuring a positive margin for the firm, one can argue that optimal operational hedges, where only basis risk exists, are likely near 100% of the anticipated cash position. For example, a grain exporter can bid on an export contract and simultaneously lock in the cost of the grain by buying futures contracts; the bid (sale) price and futures purchase help assure a positive margin (Tomek and Kaiser 2014, Chapter 13). A number of issues related to optimal hedging estimates are summarized in Tomek and Peterson (2001). Tomek (1988) discussed the effects of futures and options markets on farm incomes, both directly through hedging and indirectly through the possible effects on price behavior.

Another project examined the potential for the Australian Wheat Board to use United States-based markets for hedging (Sheales and Tomek 1987). The answer was that little benefit exists. Prices of Australian wheat had low, though not zero, correlation with the prices of contracts that required delivery in the United States. Moreover, an important issue is the effect, if any, of a large increase in the volume of hedging on prices; would the larger hedging volume attract sufficient speculative volume so that placing hedges would not have a large price effect?

Successful hedging requires predictable basis relationships, i.e., what is the relationship of the futures price to the cash price faced by the firm on the date of completion of the hedge? One piece of research illustrated that it is possible to explain the historical behavior of a basis with a statistical model, but that it is difficult to make precise forecasts from the model (Taylor and Tomek 1984). The problem arises because of the difficulty of making ancillary forecasts of the explanatory variables.

Futures markets can provide a relatively low-cost way to manage risk, and hence they are a potential benefit for firms that face price risk. An increase in price risk is an increased cost to the producer and *ceteris paribus* reduces supply. The introduction of a futures market for lumber allowed one of my PhD students to test this hypothesis. She outlined the theory, and specified and estimated a model of lumber supply for the before futures and with futures periods. The results were consistent with the hypothesis; the introduction of a futures market was associated with a larger supply, net of other factors influencing supply (Meyer and Tomek 1987). Her dissertation received an award from the AAEA as an outstanding thesis.

The growth in the literature on futures markets sometimes included papers by authors who did not appreciate that the prices of agricultural commodities and the prices of financial assets (like bonds) behave differently. It is indeed possible that spot prices for a commodity have systematic behavior even though the futures prices do not. A comment clarified this point (Tomek 1994a).

Another paper demonstrated that even if futures prices are random walks, searches for technical trading rules, which can be the basis for profitable speculation, can appear successful based on analyses of past price behavior (Querin and Tomek 1984). Such rules, however, are not predictive of future success, although a small probability exists that they will be. This paper used simple probability concepts and simulations that likely would not be publishable today. Still, it is difficult to appreciate that while there is less than one chance in a thousand of obtaining 10 heads in a row from the toss of a fair coin, the probability approaches one as the number of “samples” of size 10 increases. Moreover, technical analyses often search over a variety of models to find one that is profitable. The samples are not independent, and of course if one searches over many alternatives, it is possible to find rules that would have been profitable in some historical period.

Even if changes in futures price are not correlated, the higher moments of their probability distributions have systematic behavior. Streeter and Tomek (1992) analyzed the monthly variances of soybean futures contracts, using a comprehensive structural model. The model demonstrated that time-to-maturity and seasonal effects exist for the

variance of futures contract prices. That is, the variance grows as contract maturity approaches, and it is greater during the growing season when yields are uncertain. These effects exist because of the trend and seasonality of information flows. Moreover, the depth of speculation relative to hedging use seems to be important for keeping the variance low *ceteris paribus*.

Since the prices of options contracts on futures contracts depend, in part, on the volatility of the futures contract prices over the life of the option, it is important to have models of high frequency (not monthly) volatilities. The simplest model of options prices assumes that they are log-normally distributed with a constant variance. Unfortunately this is not likely to be true for prices of futures contracts of agricultural commodities. Lordkipanidze and Tomek (2014) report on a model and estimates of the variances of daily prices and the implications of these results for pricing options on these contracts.

Another issue is the occasional poor convergence of futures and cash prices for major commodities like corn, soybeans, and wheat. One potential reason is that the implicit options in these contracts can have varying values; these options give the seller of a futures contract some discretion in the timing of delivery within the delivery month, the location of delivery, and the quality (grade) of the delivered lots of the commodity. A research project estimated the value of these options, and used a statistical model to estimate whether or not varying values of the implicit options influenced convergence (Hranaiova and Tomek 2002; Hranaiova, Jarrow and Tomek 2005). This analysis suggests that varying options values have a significant, but small effect on the magnitude of convergence. (Based on research at the University of Illinois, the major factor in poor convergence was a contract provision.)

Another PhD student built a simulation model that generated observations of monthly cash and futures prices and quantities for the U.S. corn market consistent with those observable in the late 1990s and early 2000s. The model was used to generate prices for 10,000 40-year “life times.” This provided empirical probability distributions of prices by month, and it permitted us to evaluate various marketing strategies, such as selling the entire crop at harvest, diversifying sales over the marketing year, hedging, etc. (Peterson and Tomek 2005; Peterson and Tomek 2007; Tomek and Peterson 2005). The results showed, among other things, that an efficient market with rational decision-makers can generate very diverse results; market imperfections and irrational decisions are not required to explain low prices and poor returns to producers over a period of years. The results also remind us that small probabilities are not zero probabilities; the unexpected can occur. Not surprisingly, hedging with futures reduces the variance of returns but at a small cost in mean returns. On the other hand, diversifying cash sales over the marketing year reduces the variance of returns only slightly; since the variance of prices increases as the marketing season progresses, diversification means that some sales are moved from low variance to higher variance months.

In retrospect, I wished that I had placed more emphasis on describing the probability distributions of commodity prices—their means, variances, and higher moments—and on whether or not the predictions or simulations from fitted models were consistent with historical behavior of these distributions. Wang and Tomek (2005) did characterize the monthly price behavior of Class III milk prices, but the only major effort to characterize a distribution and develop a model that could simulate similar prices was the study of corn prices, discussed above.

Also, it appears that we price analysts have not given sufficient attention to the possible jumps in the mean of a price series. The literature has characterized prices as having seasonal, trend, cyclical, and random components, but a description of many commodity prices in the U.S. suggests that their means have shifted on several occasions since World War II. These shifts seem to be the consequence of an occasional large shift in demand that persists, and that is not immediately accommodated by a change in supply (Tomek 1979; Tomek and Kaiser 2014, 176f; Verteramo Chiu and Tomek 2016).

### *Econometrics*

Early in my career, I published some short pieces about interpreting empirical results. These papers included interpreting the coefficients of zero-one variables (Tomek 1963a), models with limited (constrained) dependent variables (Tomek 1968a), and goodness-of-fit measures for simultaneous equation estimators (Tomek 1973). A graduate student and I also published a paper that compared the results from estimating a model by different simultaneous-equations estimators (Piggott and Tomek 1976). These papers were motivated, in part, by my experiences in teaching econometrics.

I also had an “aha moment” in preparing for class when I realized that two papers provided identical estimators, although the authors had not realized it. But soon thereafter someone else published the same discovery, so there

was nothing further to publish. And, as time passed, the literature evolved to show that different estimators for systems of equations could be classified into types, such as the family of Instrumental Variable Estimators. In any case, it was my view that analysts over-emphasized econometric techniques and under-emphasized the importance of model specification and data quality relative to their research objectives. Thus, while not publishing articles on econometric techniques, I did write about the importance of specifying models appropriate for the research objective and understanding the data (i.e., observational, non-experimental data) being used in applied economics research.

The first such paper was merely a note on the specification of a distributed lag model of the supply of cotton. Distributed lag models are intended to capture the possible lagged responses, say, of quantity supplied to price changes that are spread over time. A geometric form of the lagged response was popular, because the model specification simply required adding the dependent variable lagged one time period as an explanatory variable. Marc Nerlove was influential in popularizing this model, especially in supply analysis (e.g., Nerlove 1956). In reviewing the data used in this paper, I noted that a discrete shift in acreage had occurred and that if this shift was modeled, the lagged effect disappeared (Tomek 1972). The significant long-run response of cotton acreage to price changes, found in the 1956 paper, was likely the consequence of a specification error.

A conference paper (Tomek and Mount 1981), a journal comment (Tomek 1994b), and an article (Park and Tomek 1988) provided reviews and critiques of forecasting models. My 1985 presidential address touched on the limits to empirical price analysis, as I viewed them at that time. A 1993 paper discussed the potential value of replication of key research results as a foundation for deeper scholarship; this paper also pointed out that attempts to duplicate past results often find errors that change the empirical results.

An introduction to two presentations on modeling philosophies stressed the value of having a research philosophy (2001). In 1997, I gave the Havlicek Memorial lecture at Ohio State University, which stressed the importance of comprehensive evaluations of empirical results to determine their adequacy. And a 1999 paper (with H. Kaiser) applied these ideas to a model to measure the impacts of advertising generic commodities, like milk, on sales.

These papers seem to have had little effect on research practices, although the article on replication of results is still being read. My most cited paper about specification issues was on a technical topic, unit root tests (Wang and Tomek 2007). This paper starts with the premise that the economics underlying commodity price behavior imply that these prices have important, measurable, systematic components, while in contrast, unit root tests have often concluded that time series of spot prices behave as random walks. How can these conflicting results co-exist? Wang evaluated the idea that the problem lay in the misspecification of the test equations. After all, conclusions from statistical tests are conditional on specification of the test, and our results suggest that test misspecifications were likely the problem. Often tests have not taken account of one-time shifts in the mean of the series (analogous to the cotton supply model mentioned above). Changing the test equation specification typically eliminated the conclusion that nominal spot prices are random walks.

An annoyance for me is the long-standing practice of presenting regression coefficients as significant at the one, five, or ten percent level when in fact the probability of type I error is unknown. This is because researchers typically do much pretesting, i.e., data mining. They estimate a tentative model and, based on these results, modify the specification and estimate the revised model using the same data set. Often this process is repeated numerous times, especially when the initial results are inconsistent with the researcher's hypothesis, and/or the initial results have other problems. Intuitively, the danger is that, if the analyst searches over many alternative specifications, the desired result will be found even though the effect is actually zero. Formally, the final result is based on a "pretest estimator" (Wallace 1977), and thus in my view (most recently, Tomek and Kaiser 2014, 252f), t-ratios should be treated qualitatively. Researchers should be honest about the data mining process.

### *Miscellaneous*

Some research was outside of my focus on commodity markets. One such project analyzed the effect of income instability on farmers' consumption and investment (Girao, Tomek and Mount 1974). Data were obtained from records of both the farm business and household for 30 farmers observed over seven years. The estimates used econometric methods for a sample that combined the time series of the cross section of farms that were novel at that time. The results suggested that income instability had little effect on consumption behavior, but did effect investment behavior. The elasticities of investment with respect to lagged saving was 0.188 for the group with unstable incomes and 0.589 for the group with relatively stable incomes.

By the 1970s, direct marketing from farmers to buyers was growing and the volume traded on central markets was declining. A consequence was a decline in information on transactions prices that was easily available to the public. Thus, while on sabbatical leave in 1978-79, I suggested a more precise definition of “thin markets” and considered the consequences for price behavior. The empirical component used data on volume of trading on terminal markets for beef. I defined a thin market as a problem of sample size relative to obtaining a desired level of precision for estimating the equilibrium price, say, the sample mean of transactions prices. The distribution of transactions prices is unknown, and I based estimates of sample size on Chebyshev’s inequality, which is a conservative approach relative to assuming the transactions prices are normally distributed. One empirical result was that 35 or fewer transactions were required on a Monday (the largest volume day) to discover Denver prices precisely relative to Omaha prices. This paper (Tomek 1980) was judged the best paper in the 1980 volume of the *American Journal of Agricultural Economics*.

In the early 1980s, I agreed to supervise the PhD thesis research of a student who was struggling to find a topic consistent with her interests in food economics. For many years, critics of the Consumer Price Index as a measure of inflation argued that the index over-stated the rate of inflation. The CPI was based on fixed quantity weights, which though revised from time to time, did not take account of consumers’ responses to changes in relative prices by substituting lower priced products for higher priced products. I suggested that the student construct a linear programming model of least cost diets, which met nutritional requirements and also had palatability constraints. That is, the model needed to include constraints that assured diets acceptable to consumers (e.g., not include a steady diet of liver). This topic used the student’s interest in diets and could be used to estimate a price index based on the changes in consumption implied by the model, i.e., varying weights for quantities that account for the least cost alternative that meets dietary needs. This research (O’Brien-Place and Tomek 1983) found that the inflation rate in food prices was over-stated by perhaps one percentage point.

Over my career, I also authored or coauthored literature reviews. The journal articles are about empirical models of commodity prices (Tomek and Myers 1993, and Tomek 2000), risk management for agricultural products (Tomek and Peterson 2001), and the overall literature about the characteristics and behavior of agricultural commodity prices in the 100 years through 2010 (Myers, Sexton and Tomek 2010). Invited chapters are about agricultural product prices and outlook (Tomek and Robinson 1977), analyses of demand for food (Tomek 1977), the state of commodity forecasting methods (Tomek and Mount 1981), price analysis for farm inputs (Tomek 1982), and the literature about futures markets for livestock (Leuthold and Tomek 1980). Some other book chapters have significant coverage of literature, including one on futures markets’ margining systems (Tomek 1985). A short piece about the behavior of agricultural prices appears in *Encyclopedia of Rural America* (Tomek 2008).

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The references are for the citations in this document and are not a complete list of my publications. I published fewer refereed journal articles than is now the case for applied economists in academic positions. Expectations about the number of journal publications required to achieve tenure have increased substantially over the past 50 years.

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