

SLOW DOWN OR SLOW DAWN?
ORGANIZATIONAL PRACTICES, CULTURE, AND EMERGING
OPPORTUNITIES FOR WOMEN AND MINORITIES IN A SHRINKING JAPAN

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Hilary Jane Holbrow

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Hilary Jane Holbrow, Ph. D. Cornell University

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Japan's population is rapidly contracting. If current trends continue, its population will fall to just one third of its 2010 peak by as early as 2095, with even faster declines in the working age population. Scholars are unsure how population contractions in Japan and the rest of the developed world will alter or uphold existing status hierarchies. On one hand, those at the top of the economic hierarchy may strengthen their grip on a shrinking number of good jobs. Alternatively, labor shortages may create new opportunities for formerly disadvantaged people. In three papers this dissertation examines the forms and causes of economic inequality in the context of Japan's demographic decline. It uses original data collected from 539 white collar workers at twelve large Japanese firms.

The first paper on gender inequality finds little evidence that firms exclude women from good jobs. However, within jobs, firms continue to pay women less than men, even after adjustments for performance. The results indicate that labor shortages do induce firms to admit more women to good jobs, but may even increase their incentives to discriminate against them within jobs.

The second paper compares economic outcomes for skilled foreign workers and their Japanese counterparts. After adjustments for acculturation and human capital quality, the data show that Western immigrants to Japan earn more even than Japanese doing similar jobs, while East Asians earn less. This pattern of stratification suggests that context of reception—particularly the attitudes of Japanese people towards members of different groups—is more influential in generating stratification within firms than the acculturation of foreign workers.

The third paper tests directly whether ethnic and racial attitudes matter for inequality between Japanese and foreign workers. Using the results of a survey experiment on attitudes, I show that in firms where coworkers are more biased against non-Japanese East Asians, inequality between Japanese and other Asians is greater. Similarly, in firms where coworkers demonstrate more pro-Western bias, Western employees are at a greater wage advantage.

Together, the three papers show that, in a context of demographic decline, outsiders do move into good jobs, but do not overturn existing status hierarchies.

BIOGRAPHICAL SKETCH

Hilary Jane Holbrow received a B.A. in East Asian studies from Boston University in 2005. Prior to enrolling in Cornell's sociology Ph.D. program in 2011, she worked in the Program on U.S.-Japan Relations at Harvard University, in local government in Okinawa, Japan, and for the Japanese embassy in Washington, D.C. She also studied Japanese as a Blakemore Freeman Fellow at the Inter-University Center for Japanese Language Studies. She earned an M.A. in sociology from Cornell in 2014. Her graduate research has been funded by the Robert J. Smith Fellowship and the Morse Woodbury Fellowship at Cornell University, and by a Fulbright Graduate Research Fellowship and a Kakenhi award from the Japan Society for the Promotion of Science. She has research interests in the sociology of organizations, and in inequality, immigration, and East Asian societies.

Dedicated to my grandparents, Mary R. and Charles H. Holbrow.

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Shuo Zhang, and Yuqi Lu assisted with the iterative process of refining the baseline translation and together produced a faithful and natural final version in Mandarin. Dozens of other anonymous people pre-tested the survey in all three language and sent comments, and Yasuko Nagatsuka and Taro Okabe helped create a short questionnaire for HR managers.

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CHAPTER 1

WHEN DO FIRMS DISCRIMINATE? COMPARING EVIDENCE FOR DISCRIMINATION IN JOB ASSIGNMENT AND IN PAY DETERMINATION AT ELITE JAPANESE FIRMS

Introduction

The gender wage gap is a fixture of every industrial economy in the world (Polachek and Xiang 2014), ranging from over 25% in Japan, to under 10% in Denmark and New Zealand (OECD 2014). But despite decades of research, the evidence on the gender gap in pay remains inconclusive on several key points. Most strikingly, we still know relatively little about how important employer discrimination is to wage gaps overall, compared to supply-side factors like workers' performance or decisions about where to work (see Blau and Kahn 2007). Secondly, if employers discriminate, we do not know if discrimination occurs predominantly in assortative employment processes (e.g. hiring, promotion, and termination), predominantly in wage determination processes (e.g., in how employers set wages for classes of jobs or individual employees within those classes), or relatively evenly across both sets of processes. Answers to these questions are of both theoretical and practical importance. The theoretical aspects can help us understand why the gender wage gap persists, and the practical aspects are crucial for designing policies that can most effectively combat discrimination.

This paper addresses the key question: Where is discrimination most likely to occur? Specifically, I examine whether discrimination is more likely to occur in hiring, as Petersen and Saporta (2004) argue, or in wage determinations within jobs, as Castilla (2008; 2012; 2015; Castilla and Benard 2010) suggests. I conduct these analyses using recent matched employer-employee data from 420 workers nested in 77 teams at twelve elite Japanese firms. These firms present an intriguing case study in their own right—Japan's level of gender pay inequality is unusually high compared to peer nations (Estevez-Abe 2013). Researchers agree

that many institutional constraints, such as a tax code that favors families with one breadwinner and an oversubscribed public childcare system, contribute to this pay gap (Brinton 1993; Boling 1995), but it is unclear to what extent direct discrimination plays a role after one takes into account women's and men's responses to other institutional constraints.

Evidence on Gender Wage Gaps and Discrimination

A massive literature documents the wage gap between men and women across the developed world, but determining whether discrimination is to blame is a thorny problem in any context (see Blau and Kahn 2007 for a review).

We know that, in the United States, and in most other industrial societies, men's and women's segregation into different types of jobs is responsible for a sizeable portion of the wage gap (Reskin 1993; Petersen and Morgan 1995; Charles and Grusky 2004). However, it is less clear to what extent segregation occurs because men and women make different choices in the labor market, and to what extent employers exclude women from high paying positions, either by not hiring them for such positions in the first place or by failing to promote them from lower-paid jobs.

It is possible that self-selection is primarily or even solely responsible for job segregation. Beginning at early ages, boys and girls report different career aspirations and choose different classes and majors in high school and college (Jacobs 1995; Correll 2001). Among adults, attitudinal studies show that men are more likely than women to say that high income (Gorman 2000) and promotional opportunities (Tolbert and Moen 1998) are the most important characteristics of a good job. Women, on the other hand, are more likely to say job

content is the most important (Tolbert and Moen 1998).¹ These same trends have been observed in decades of U.S.-based research stretching back to the 1930s (see Konrad et al. 2005 for a review). These differences in aspirations, interests, and priorities may be the main reason job segregation is so pervasive.

To test this possibility, in recent decades researchers have begun to examine more explicitly gender differences in job application patterns, and whether and how employers respond differently to male and female candidates in their applicant pools. Like the attitudinal data, these recent studies support the intuition that the bulk of segregation stems from the supply side, not the demand side. For example, in Fernandez and Sosa's (2005) study of applicants to customer service positions at a call center, the pre-screening applicant pool is 67% female. This job could thus become female-dominated even in the absence of demand-size processes disproportionately steering women into this position. Studies that consider applications into multiple types of jobs also demonstrate that women self-select into lower-paid work. For example, in a study of applicants to a high-tech company, Fernandez and Campero (2017) find that, even after adjusting for education and experience, women apply to jobs at a lower hierarchical level than do men. Similarly, among male and female MBA students at the same university, Barbulescu and Bidwell (2013) show that women are less likely to apply for jobs in finance and consulting, where pay is particularly high, in part because the women MBAs value work-life balance more than their male classmates do.

The evidence that discrimination may also play a role in job segregation is more limited. Audit studies in which large numbers of employers receive resumes for candidates whose backgrounds are identical, but whose gender the researchers have varied randomly, do

¹ Differences in attitudes tend, however, to be small in magnitude (Tolbert and Moen 1998; Konrad et al 2000) and do not appear in all populations. A study of MBA holders found no differences in stated preferences for high pay between men and women (Barbulescu and Bidwell 2013).

not find systematic evidence that employers call female applicants back at lower rates. On the contrary, some studies even show a female advantage (see Neumark 2016 for a review; but also see Correll, Benard, and Paik 2007).

However, these null findings may be because audit studies focus only on callbacks, not on job offers. Employers may be just as likely to interview women, but less likely to hire them. Fernandez and Sosa's call center study offers a test of this possibility. They show that, to the contrary, hiring managers interview and hire women at a *higher* rate than men with similar qualifications, further increasing female overrepresentation in the customer service position at the target firm.

Null findings in audit studies may also be a result of the type of jobs it is possible to test with an audit approach, which are mostly entry-level or early career jobs. At the upper end of the labor market the story may be different. Indeed, at the tech firm in Fernandez and Campero's (2017) study, women are more likely to receive offers for entry-level jobs but less likely to receive job offers at the mid-career and experienced stages, even after the data are adjusted for education, work history, and managerial experience. Similarly, in a study of executive search, Fernandez and Fernandez-Mateo (2016), after adjusting for candidates' experience, find that search firm screeners are more likely to deem women candidates for executive jobs as "unsuitable." Barbulescu and Bidwell (2013), however, find that men and women MBAs are equally likely to get job offers in male-dominated fields once they have applied. Thus there is some inconsistent evidence that firms may discriminate against women in upper-level jobs. But even when studies detect possible discrimination, the magnitude of these effects is small. For example, in Fernandez and Fernandez-Mateo's study of executive search, women's probability of being deemed unsuitable is just 3.6% higher than men's.

These findings may be reason for optimism if discrimination occurs more in hiring than in other stages of the employment process, as Petersen and Saporta (2004) argue. Petersen and Saporta discuss how transparency matters for eliminating discrimination, and note that hiring processes are notoriously opaque. If a firm chooses not to offer a woman a job, it is almost impossible to document that she was turned down *because she is a woman*. Not only is information about other candidates for a position difficult to obtain, it is also likely to be ambiguous; firms assess job candidates on various dimensions and do not necessarily quantify them or weight them consistently between candidates. These opacities in the hiring process, Petersen and Saporta maintain, will make it easier for firms to discriminate when hiring than when determining pay within jobs, where plaintiffs have access to others in the candidate pool and decision making standards are more formalized and explicit. If their hypothesis is correct, employers' gender discrimination may be a relatively small contributor to remaining gender pay gaps in OECD countries.

But Petersen's and Saporta's claim has never been empirically tested. Their own study does not ask whether firms' hiring processes are biased. Rather, the authors use data from a large U.S. service firm to test whether women and men earn the same salaries within job grades, and whether the likelihood of promotion differs by gender. They find that within hierarchical levels of jobs men earn 3.6% more than women at time of hire, but that the within-grade gender difference vanishes as employees acquire seniority. This small pay difference at point of hire may be due to discrimination, or it may be due to gender differences in experience prior to the workers' employment at the study firm. Petersen and Saporta find no evidence of discrimination against women in promotion.

The lack of data to compare potential discrimination at hiring has led others to dispute the claim that discrimination is more likely in this stage of the employment process. In counterpoint, Castilla (2008: 1502), argues that discrimination is actually even more likely to

occur in pay determinations. Consistent with this, in a single-firm study he finds after adjusting for performance that women's annual wage growth is 0.4% lower than men's, a substantively small but statistically significant difference. The reason for this, Castilla suggests, is that pay determinations are as opaque to employees as their hiring decisions. Hiring decisions are, however, visible to other employees and to regulators at least in aggregate—it may become obvious over time if a company hires or promotes unusually few women and minorities. Castilla argues that individuals are more likely to interpret aggregate patterns as evidence of discrimination than individual experiences, which are too rich to allow for clean comparisons (1515-1516). Because employees almost never see aggregate wage data for employees by group, in Castilla's framework discrimination is more likely to go undetected and unquestioned in pay determinations than in hiring.

Like Petersen and Saporta, Castilla emphasizes the importance of transparency for constraining employer discrimination. However, Castilla differs in his assessments of both the information that must be transparent, and the audience for that information. While Petersen and Saporta suggest the relevant information is the treatment of oneself and that of peer applicants and employees, Castilla suggests it is the general pattern of how the firm treats different groups on average. While Petersen and Saporta argue that the audience who must perceive this information is the employee who experiences discrimination, Castilla implies that more general audiences are of greater importance, including perhaps company managers, other employees, members of the community, and government regulators.

However, also like Petersen and Saporta, Castilla does not compare evidence for discrimination in hiring with evidence for discrimination in pay. Although he finds a small amount of “performance reward bias”—firms' tendency to offer lower rewards to women than men with equivalent performance—the administrative records he uses do not permit an

investigation of potential gender bias in the way in which the firm allocates jobs to women and men in the first place.²

The current study is, to my knowledge, the first to look for evidence of discrimination in both initial job allocation and in pay determination within jobs. This analysis can help adjudicate between the two accounts of “the opportunity structure for discrimination” described above. It also provides new insight into the empirically interesting Japanese case.

The Japanese Case

Since becoming the world’s second largest economy in the 1960s, Japan’s economic development has paralleled that of the major Western economies. However, gender inequality has declined much more slowly in Japan than in peer nations, leaving it an outlier (Brinton 1993; Gender Equality Bureau Cabinet Office 2007, 2013; Estevez-Abe 2013; Nemoto 2016). Today, Japan’s gender pay gap is the third highest in the OECD, following South Korea and Estonia. What processes maintain Japan’s unusually high gender wage gap? In addressing the theoretical questions outlined in the previous section, this paper also addresses this empirical puzzle.

On some indicators of women’s wellbeing, such as education levels and health rates, Japan compares favorably to countries with similar levels of development. Japanese women’s labor force participation rate of 66% even exceeds the OECD average of 58% (OECD 2015). However, other labor market indicators reveal high levels of gender inequality. For example, the unadjusted gender pay gap is 27%, nearly twice the OECD average of 15% (OECD 2014) and in 2012, women made up about 42% of employees, but only 11% of managers, in

² Like Petersen and Saporta, Castilla (2008) finds no evidence of discrimination in promotions. Gender-neutral promotions are consistent with both Petersen and Saporta’s and Castilla’s frameworks.

comparison to 30% in Germany and 43% in the United States (Gender Equality Bureau Cabinet Office 2013).

Scholarship on Japan has identified how various structural features of the economy contribute to the gap. As in other countries, men and women are unevenly distributed across industries and occupations. Japanese women are, for example, more likely to work in the service industry and less likely to work in construction or transport; they are also more likely to do clerical work (Gender Equality Bureau Cabinet Office 2013). However, the degree of industry, occupational, and workplace segregation, like the labor force participation rate, is comparable to that of other developed countries (Mun 2010), and explains less of the gender pay gap than in other contexts (e.g. Kumlin 2007 for a comparison with Sweden; Avent-Holt and Tomaskovic-Devey 2012 for a comparison with the United States; Tachibanaki 1998). Rather, women's segregation into certain labor arrangements within occupations, industries, and firms explains more of the gap (Brinton 1993).

Women's shorter tenures and concentration in contingent employment are perhaps the largest structural contributors (Brinton 1993; Yu 2013; Kim and Shirahase 2014; Boling 2015). In Japan, regular fulltime employees (*seishain*) with long tenures at one employer have the highest incomes (Holbrow 2015). But many women quit their jobs and leave the labor force temporarily when they have children (Brinton 1993; Yu 2009), reducing their tenures relative to men who do not interrupt their careers. Further, at all stages of the life course, women cluster in irregular jobs, which tend to have few fringe benefits and low wage growth (Song 2014: 97, 173). In 2015, about a quarter—22%—of men worked in irregular positions such as contract, temporary, and part-time work, but a majority—56%—of women did (Statistics Japan 2016).

In addition, even among regular workers, women's employment categories differ from men's (Kanai 2013). Although current law prohibits Japanese firms from explicitly differentiating between men's and women's jobs (Mun 2010), some companies maintain a two-track system for full-time employees that creates de facto gender segregation (Kumamoto-Healy 2005; Mun 2016). The two-track system consists of a management track (*sougou shoku*) and a general track (*ippan shoku*). Management track employees are expected to work long hours, rotate jobs to get a well-rounded picture of their firm's business activities, and accept transfers to distant locations. General track employees work in more limited business areas and are not assigned long-distance job postings. Although relatively rare in small and medium firms, about 45% of firms with over 1000 employees used the tracking system in 2012. In 59% of firms with a general track, over 80% of the general track hires were women and in 72% of firms with management tracks with nation-wide job transfers, over 80% of the hires were men (Ministry of Health, Labor, and Welfare 2013).

Like women's segregation into contingent employment, the tracking system contributes to the gender pay gap (Kanai 2013). A survey of 394 firms found, for example, that at workers' peak earning year (age 55), the average monthly salary of a general track employee with a college degree was only 55% of that of a management track employee with identical education (Keidanren 2014).

There is thus extensive evidence that job sorting contributes to Japan's gender wage gap. There is, however, no data on the extent to which sorting reflects workers' choices. Indeed, there are many reasons to believe that Japanese women prefer to opt out of the most demanding jobs. Work conditions in Japanese firms are notoriously punishing, and many employers expect regular, management track workers to put in long hours, either by pressuring them to remain in the office until their supervisor leaves for the night, or by encouraging extensive, unpaid after-hours socialization with coworkers and clients (Rebick

2005). In addition, Japan has one of the lowest levels of job satisfaction among OECD countries (OECD 2009), perhaps because job mobility is so low (Ono 2010; Holbrow 2015), and workers in regular employment find it difficult to switch jobs to improve job fit. Unlike men, women have a culturally acceptable alternative to working long hours year after year in a job they do not particularly like by choosing contingent employment or general track jobs.

Women with children may also have little option but to select contingent or general track jobs, because Japanese women bear a larger portion of the childcare and housework burden than women in many other OECD countries. Among married couples with children under the age of six, employed men spend an average of 67 minutes per day on housework and childcare, compared to 356 minutes for employed women (Gender Equality Bureau Cabinet Office 2013). Japan is also the only OECD country where women are more likely than men to say that their jobs interfere with their non-work responsibilities (Ruppanner and Huffman 2014). Because of a comparatively heavy burden of household labor, Japanese women may work less productively and voluntarily choose less demanding roles more often than women in other developed countries.

It is also possible, of course, that discrimination pushes women into lower-paid job classes. For example, Mun (2010) finds that in 1995 (before the government outlawed gender-specific jobs), advertisements for entry-level jobs for high school graduates that targeted women offered lower pay than advertisements targeting only men. Whether in the absence of this practice many women would have applied for higher-paying positions is unknown, however.

Data that would allow researchers to estimate the magnitude and causes of within-job pay gaps are even more difficult to obtain. National surveys do not collect information on track status, making it impossible to use their results to compare wages of similar women and

men within tracks, much less within jobs. Private data from single or multi-firm studies also usually lack tracking variables (e.g. Aiba and Wharton 2001; Avent-Holt and Tomaskovic-Devey 2012; Hashimoto and Sato 2014). The one exception, a study of a single firm used HR data from over 10,000 employees, and after controlling for skill, job grades, and work hours the researchers (Kato, Kawaguchi, and Owan 2013) found no significant gender differences in pay.

Setting aside questions about the magnitude of within-job pay gaps, researchers also speculate on their causes but lack the data to test their hypotheses. Historically, age and tenure have been important variables in Japanese firms' wage determinations (Kalleberg and Lincoln 1988; Tsuru, Abe, and Kubo 2005). Because women tend to interrupt their careers to have children, these human capital differences may be solely responsible for within-job pay disparities between women and men.

However, as in the United States, Japanese firms have increasingly linked pay with short-term performance (Mitani 2010; Conrad 2009; Kato and Kodama 2015; Heneman and Werner 2005; Burke 2005). If women perform similarly to men, this change may have reduced or eliminated within-job pay inequality, because it flattens the relationship between age or tenure and wages that tends to advantage men (MHLW 2002; Kataoka 2005; Mitani 2010). On the other hand, performance pay may perpetuate wage inequality because women's performance is unlikely to match that of men in a corporate environment that requires management-track employees transfer to distant posts and dedicate extensive after-hours time to their work (Nakashima 2013). In this case, gender wage inequality would persist in analytic models with adjustments for human capital, but disappear after adjustments for performance, as in Kato, Kawaguchi, and Owan's (2013) study.³ Finally, firms may simply

³ This study cannot detect whether gender bias distorts managers' assessments of female employees, as some suggest it might (Aiba and Wharton 2001; Kato and Kodama 2015).

discriminate against women, and pay them less than male counterparts with the same human capital and performance.

In sum, there is extensive job segregation in Japan, but little evidence on the relative impact of selection and discrimination on shaping this pattern. With regards to within-job pay gaps, few data sources allow researchers to estimate their magnitude, and consequently, whether and how discrimination may produce them is still largely a matter of speculation.

Data

To investigate to what extent discrimination impacts the wage gap, and at what stages discrimination occurs, I use a novel employer-employee matched dataset I collected between February and April 2015, called the Survey on Workplace Environment and Diversity Management. The data are a cross-sectional sample drawn from employees at twelve elite firms contacted through the Japan Association of Corporate Executives' (JACE) sub-committee on diversity issues. JACE sent a research request to the CEOs of all 205 sub-committee member firms. Of these, twelve firms (5.8%) agreed to participate. The participating firms represent a range of industries including manufacturing (three firms), business services (five firms), and consumer services (four firms). The average size of these organizations is very large—ten of the twelve firms have more than 1000 employees and three have more than 10,000. Using the same selection strategy as Lincoln and Kalleberg (1990), participating firms chose several white-collar work teams and distributed an online survey to every employee on the selected teams, for a total of at least 25 workers per firm.

The twelve participating firms distributed the survey to 909 employees, for a return of 539 valid responses, and a response rate of 59% overall.⁴ This response rate exceeds the mean response rate of 52.7% in organizational surveys (Baruch and Holtom 2008).

Response rates varied by firm, ranging from a low of 34% at one firm to a high of 100% at five firms.⁵ Assuming that demographic characteristics of the workers at each firm are similar, I can compare the results for those with 100% response rates to those with lower response rates to assess patterns of non-response. There is no evidence of non-response bias by gender: 26% of the workers in the firms with high response rates were female, compared to 31% at the other firms, a statistically insignificant ($p = 0.28$, two-tailed test) difference. However, workers at the high-responding firms were significantly older ($p > .001$, two-tailed test, 95% confidence interval 1.3 to 5.1 years). Again, assuming similar demographics across firms, this implies that senior employees were less likely to respond to the survey in low-response firms. Because gender inequality is generally higher among older workers in Japan (Kumlin 2007), this implies that residual wage gaps will be downwardly biased in low-response firms and overall.

The survey took 30-60 minutes to complete and covered a rich variety of topics, including job satisfaction, information about the respondents' job history, job content and salary, respondents' attitudes towards work, and demographic backgrounds. Descriptions of the coding process for the variables used in these analyses appear in Table 1.1.

⁴ One firm declined to specify how many workers received the survey. To calculate the total response rate, I therefore assume that the response rate at the firm with missing data is equal to the mean response rate of all firms (78.6%). Because I received 50 responses from this firm, the estimated number of survey recipients is 64. This estimate is included in the response rate denominator of 909 employees.

⁵ I did not disclose response status of individual respondents to their employers, and survey materials made clear that participation was voluntary. To increase response rates, I sent periodic updates to my contact at each firm with the percentage of workers who had responded at their firm. At the firms with high final response rates, the contact person sent out periodic reminders to all targeted workers to complete the survey.

The mix of subjective measures such as job quality and objective measures such as earnings, job class, and job content is a strength of this dataset, as is the coverage of multiple firms. To my knowledge it is the most comprehensive employer-employee matched dataset collected in Japanese firms by any researchers since Kalleberg and Lincoln's seminal data collection effort in the mid-1980s (1988; also see Lincoln and Kalleberg 1990), and the only one to include contract workers employed alongside regular employees. As such, it presents a unique opportunity to update our understanding of inequality in Japanese firms after three decades of economic, social, and demographic change.

Of course the data have limitations as well. Because the dataset includes only large firms and white collar workers, the results are not generalizable to other labor market segments. In addition, because the data are self-reported and respondents are sampled, this dataset is more subject to error than administrative HR data used in several recent studies (e.g., Hashimoto and Sato 2014; Kato, Kawaguchi, and Owan 2013). Unlike the single-firm studies using administrative HR data, it covers twelve firms in three major industries, creating a broader basis for generalization among white collar employees of large firms.

Table 1.1: Description of Variable Coding

<i>Dependent variables</i>	<i>Description</i>
Job quality	Mean value of respondent's answers to questions about the interest (1 = Very boring, 5 = Very interesting), value (1 = Complete waste of time, 5 = Very worthwhile), and relative quality (1 = Much worse than most; 5 = Much better than most) of his or her job.
Monthly base pay	Respondent's pretax income (including base salary, overtime pay, and allowances) from the previous month in 1000s of yen.
Total annual earnings	Respondent's pretax monthly income (including base salary, overtime pay, and allowances) from the previous month, multiplied by 12, plus total value of annual or semi-annual bonuses received during the previous year, in 1000s of yen.
<i>Independent variables</i>	<i>Description</i>
Performance (bonus rate)	Total value of respondent's annual or semiannual bonus from the previous year as a proportion of previous month's earnings multiplied by 12. Top-coded at 95th percentile.
Job class	Respondents were asked separately about their contract type and job track. If workers said they are on a fixed term contract, they were coded as contract workers. Workers who selected no contract, indefinite contract, or don't know are coded as regular workers. Among regular workers, respondents who selected general track are coded as general track. Workers who selected management track, not applicable, or don't know were coded as regular workers on the management track. Workers who said they were contract workers and on the general track were coded as contract workers. 1 = Regular, management track employee, 2 = Contract employee, 3 = Regular, general track employee.
<i>Control variables</i>	<i>Description</i>
Sex	0 = Female, 1 = Male.
Education	1 = Less than BA, 2 = BA or equivalent, 3 = MA or higher.
Age	Respondent's current age in years. Missing values set to gender mean.
Tenure	Respondent's year of entry into the current firm, subtracted from survey year (2015). Missing values estimated from years of education and number of jobs.
Number of previous employers	Respondent's total number of past employers, excluding current employer.
Work hours	Respondent's typical work hours, selected from a range. Values are coded at range mid-points. Top category (60 or more) hours coded as 66. Missing values set to mean (43).
Job content	Respondents selected the type of work they do from a list of 10 options, including accounting, human resources, legal or intellectual property, product design or engineering, sales or business development, clerical, information technology, management, public relations or advertising, and research. Respondents whose work did not fit the assigned categories completed a text response box. Two additional categories, Business consulting and consumer service, emerged from the free entry responses, for a total of 12 job types and 1 one group of missing or uncategorized. 0 = Non-clerical, 1 = Clerical.
Authority	Number of subordinates the employee supervises, selected from 5 ranges. Values are coded at range mid-points. Top category (20 or more) coded as 23. Missing values set to 0.

Finally, because the data are cross-sectional, I cannot observe exits from the firm. This may lead to an underestimate of discrimination if workers who experience discrimination are more likely to quit.

There are also several other reasons why these data might be less likely to reveal discrimination than a randomly generated sample or a census. Companies that are aware of gender disparities in wages would be unlikely to participate in a survey on diversity management, and firms with a commitment to gender and cultural diversity would be unlikely to send the survey to work teams where gender discrimination is more severe than elsewhere. As such, the analyses below can be interpreted as a conservative test for discrimination. If I find evidence of discrimination in this context, it is likely its effects on white collar workers in other large Japanese firms are as large or larger.

In the analyses below I exclude 100 non-Japanese employees, because the intersections between nationality and gender and their relationship to wages are outside the scope of this paper. I also exclude 19 Japanese respondents with missing data on gender, income, or job quality, which leaves an analytical sample of 420 workers.

Research Design, Measures, and Hypotheses

I use these data to investigate whether there is any evidence of discrimination at two stages of the hiring process—employees' assignment to job class (management track, general track, and contract jobs) and their pay within jobs.

As a background for my investigation of whether and where discrimination occurs, I begin by examining descriptive statistics on women's and men's representation across the three job classes. I then describe the observed wage gaps between men and women, beginning with unconditional differences, followed by estimates of within- and between-job

gender wage gaps that account for gender differences in representation across firms and sections and for gender differences in human capital. I estimate these wage gaps by modeling total annual compensation, including both base pay (measured in the most recent calendar month and multiplied by twelve) and bonus pay (measured as total bonus payments received during the previous fiscal year) in a mixed, or hierarchical, linear model. Human capital, job characteristics, and job content are modeled as fixed effects. Team- and firm-level effects are modeled as nested random effects (see Gelman and Hill 2007). I use this same hierarchical modeling approach, which adjusts for the dual nested structure in the data, in all analytic models below. After giving a descriptive picture of gender job segregation and the size of within-job wage gaps, I turn to the main theoretical questions about the role of discrimination in job allocation and in pay determination.

My first analytic concern is whether firms discriminate against women by placing them on the lower-paying general track or in insecure contract jobs, not only at a disproportionate rate, but also contrary to the women's own preferences. The classic approach to this question is to compare the qualifications of women and men in the applicant pool, and the firm's decision about how to hire and place them. Unfortunately, as in studies by Petersen and Saporta (2004) and by Castilla (2008) data on the applicant pool are not available. Nonetheless, subjective measures taken in the survey allow me to examine whether or not placements produce an effect that is consistent with employer discrimination in job placement.

To look for evidence of discrimination in job placement, I consider respondents' self-assessed job quality. I measure job quality using a composite variable, constructed from the Job Descriptive Index, the most widely used measure of job satisfaction (van Saane et al. 2003). Respondents were asked to rate how interesting their jobs are (ranging from "very boring" to "very interesting"; whether their job was worthwhile (ranging from "a complete

waste of time” to “very worthwhile;” and how their jobs compared to other possible jobs (ranging from “current job is much worse than most” to “current job is much better than most”). All these variables are measured on five point scales; I take the mean to generate the composite.⁶ Cronbach’s alpha for the three components of the job quality measure is 0.84, indicating a high degree of inter-item correlation, and validating the combination of these three measures as a reliable index of one underlying construct (Carmines and Zeller 1979).

Even without direct information on the applicant pools for different job classes, we can infer that, in the presence of discrimination, women in female-typed job classes would report lower job quality relative to women in management track jobs. The logic of this is as follows: even among graduates of top universities, some women aspire to the management track and some women aspire to the general track or to contract work (Unozawa and Kimura 2015). However, if firms reserve management track jobs for less qualified male applicants, some of the women seeking management track jobs will enter the pool of workers applying for general track jobs. The timing of the job market for new graduates has historically encouraged this: until 2015, most firms would accept applications for general track positions only after they had made hiring decisions for management track jobs (Unozawa and Kimura 2015). This allowed applicants at a particular company who were not selected for management track jobs to reapply for general track positions. Thus, if firms do discriminate in hiring, post-hire pools of female workers in the management track are likely to rate their jobs particularly highly, knowing perhaps that they have beat the odds in landing such a job, while post-hire pools of female workers in the general track or in contract positions are likely to rate their jobs more poorly on average, because at least some of the workers in these

⁶ For respondents with missing data on one or two of the questions, I take the mean of non-missing questions. I exclude from the sample respondents who answered none of the job quality questions.

positions did not get their first choice of a place on the more remunerative management track, and instead turned to their second-choice jobs.

As this discussion shows, if there is discrimination in job placement, we can predict that:

Women in contract jobs (H1a) and in general track positions (H2b) will report lower job quality than women in management track jobs, ceteris paribus.

Of course, this same pattern could occur simply because of competition. If there are not enough management track jobs for every worker who aspires to such a position, women aspiring to management track jobs could still end up in general track or contract jobs, even if firms do not disproportionately exclude them from management track jobs. However, the same is true of men in the absence of discrimination. On the other hand, if firms do discriminate, the post-hire pool of women in jobs off the management track will contain a larger percentage of dissatisfied workers, on average, than the post-hire pool of men off the management track. We can thus predict that, if firms do discriminate:

The gap in self-assessed job quality between workers on the management track and workers in other job classes will be larger for women than for men, ceteris paribus (H1c).

To test these hypotheses, I use job quality as the outcome variable in a hierarchical linear model with random effects at the team and firm levels. I include fixed effects for age, tenure, education level, and work hours, as these affect, independently of gender, the quality of jobs to which firms assign employees. I interact a male dummy variable with the variable for job class (management track, general track, or contract job) to generate job quality estimates for men and women in different tracks, net of human capital differences.

Next, I look for evidence that discrimination occurs in wage determination within jobs. I define a job as the same work content, in the same job class, with the same level of

supervisory authority, on the same team, at the same firm.⁷ I model job content, job class, and supervisory authority as fixed effects and random effects for teams and firms. Because all firms in the sample use performance pay, it is of particular interest whether differences in men's and women's assessed performance explain within-job gender wage gaps.

Figure 1.1: Relationship Between Performance and Compensation at Japanese Firms

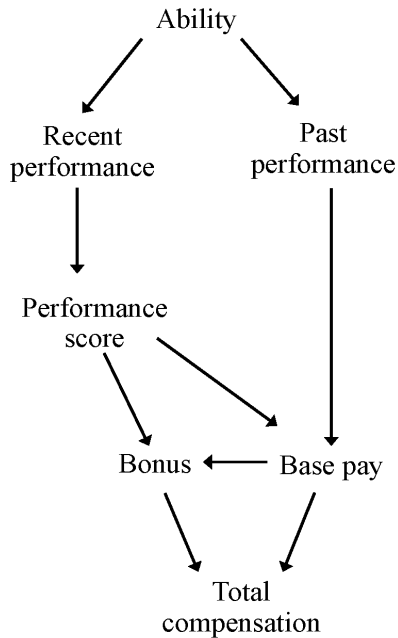


Figure 1.1 describes how most large Japanese firms calculate compensation, and helps to illustrate the logic behind the analysis of within-job pay gaps. As this graph shows, workers' compensation consists of two main parts: Base pay and bonus (see Rebeck 2005; Tsuru, Abe, and Kubo 2005 for detailed descriptions of pay setting policies in large Japanese firms). Base pay is also a function of a number of variables, such as age and tenure. I adjust for these in my analytic models, but for simplicity's sake I omit them from Figure 1.1.

Employers generally conduct annual or biannual performance evaluations, which generate a performance score for each employee. Based on this score, employees receive a

⁷ In the interests of parsimony, I model job content as a binary variable for clerical and non-clerical work; using the full thirteen categories of job content available in the dataset does not change the main results.

raise, indicated in Figure 1.1 via the arrow connecting performance score with base pay. Because in previous periods, performance scores will already have created differentials in raise amounts, base pay for employees doing the same jobs may already be different in any given period, as indicated by the arrow connecting past performance to base pay.

Recent performance scores also determine the size of the bonus, which firms calculate as a percentage of (pre-raise) base pay (Rebick 2005: 44, 47).⁸ As in most datasets, I do not observe workers' true ability or their true past performance and true recent performance. Further, because the data are self-reported, I also do not observe the employers' assessments of recent performance directly. I can, however, estimate these assessments by reversing the calculation that firms make to calculate bonus amounts. I divide bonus amount by annual base pay to obtain a proportion. The resulting proportion is an estimate of unobserved recent performance scores, and removes the direct mathematical relationship between bonus and base pay that we see in the raw data. I can then use this proportion to investigate the relationship between recent performance and compensation for men and women within jobs.⁹

If recent performance is not representative of career-long performance, we cannot draw conclusions about the relationship between career-long performance and pay from this measure. However, as long as the relationship between long-term performance and recent

⁸ In this sample, the mean response to the question "How much do the results of your individual performance review influence your bonus?" was 3.7, in between "A moderate influence" (3) and "A large influence" (4) on the 1 to 5 scale. For effects of individual performance on raise, the mean value was 3.5.

⁹ Because performance is estimated from the previous month's base pay and from the previous year's bonus, the calculations used here will bias estimates of the relationship between performance and compensation towards zero. The magnitude of the downward bias will be greater for higher performers. Imagine two employees, both of whom were earning \$100 in Period 0, which was unobserved. Employee A receives a raise of \$1 and a bonus of \$1 (representing the lowest possible performance, a score of 1). Employee B receives a bonus of \$20 and a raise of \$20 (representing the highest possible performance, a score of 20). For Employee A, we thus observe a Period 2 base pay of \$101 and a Period 1 bonus of \$1. For Employee B, we observe a Period 2 salary of \$120 and a Period 1 bonus \$20. Our estimate of assessed performance will fall only slightly below the true value of 1 for Employee A (1/101 or 0.99%). However, for employee B, our estimate will fall further below the true value of 20 (20/120 or 16.67%). The magnitude of this bias depends not on the bonus amounts, but on the size of raises, and will lead to underestimates of the strength of the true relationship between performance and compensation. However, this downward bias should not hamper our ability to look for gender differences in returns to performance, because in the absence of discrimination it would affect men and women equally.

performance is similar for women and for men, we can still test if gender differences in within-job base pay disappear after I account for estimated recent performance, or if gender gaps in pay deepen or remain unchanged after performance adjustments. If there is discrimination, we can predict that:

An unexplained within-job wage gap will remain between women and men after adjustments for performance, ceteris paribus (H2).

For this analysis, I model both monthly base pay and total annual compensation as a function of human capital and job characteristics, with performance (measured by bonus transformed into a proportion, and capped at the 95th percentile to minimize the effects of influential outliers) as a predictor variable. I use both quadratic and linear terms for estimated performance, because visual examination of the data show a curvilinear relationship between estimated performance and total compensation. Because all respondents are from the same teams on the same firms, the data do not have a long right tail, and a log transformation of base pay or total compensation is unnecessary.

The focus of these two models of performance effects on compensation is the difference in the intercept of pay for men and women. However, it is also possible that the slope of performance on compensation varies by gender. Psychological experiments suggest that people respond negatively to those who violate gender stereotypes and that these reactions penalize women who engage in stereotypically masculine behaviors in the workplace (e.g. Eagly and Karau 2002; Rudman and Fairchild 2004). Because stereotypes paint women as cooperative and caring, rather than competitive and agentic, employer bias would be particularly salient for high-performing women, who likely violate these expectations. This is especially true in Japan, where women's traditional role is servile and obedient (Pharr 1984). I therefore expect that the slope of performance on base pay will be

steeper for men than for women and that the gender gap in compensation will widen with level of performance. Specifically, I interact performance with gender to test whether:

The gender gap in compensation will be largest among high performers, ceteris paribus (H3).

I generate one interaction model with base pay as the outcome variable, and one with total annual earnings as the outcome.

Results

Analysis of Job Segregation and Pay Inequality

I begin by comparing descriptive statistics for male and female workers, shown in Table 1.2.

Table 1.2: Descriptive Statistics

	Women (N=124)		Men (N=296)		Comparison
	Mean or %	SD	Mean or %	SD	
<i>Personal</i>					
Education					**
Below BA	16.13		6.42		
BA	71.77		70.95		
MA or higher	12.10		22.64		
Age	37.94	8.54	40.73	9.03	**
<i>Job characteristics</i>					
Weekly work hours	40.67	7.52	43.15	8.44	***
Clerical work	20.97		7.43		***
Job class					***
Management track	57.26		81.08		
Contract job	25.00		12.50		
General track	17.74		6.42		
Authority (# of subordinates)	1.48	3.74	4.41	7.03	***
<i>Job history</i>					
Tenure	9.69	8.61	12.31	9.84	*
Number of previous employers	1.09	1.20	0.72	1.00	**
Performance (bonus rate)	0.24	0.25	0.31	0.24	*
<i>Family characteristics</i>					
Married	35.48		73.99		***
Parent	21.77		59.80		***
<i>Outcome variables</i>					
Self-assessed job quality (1-5)	3.74	0.69	3.92	0.82	*
Monthly base pay (1000s of yen)	389.34	196.87	552.54	239	***
Total annual earnings (1000s of yen)	5820.69	3160.52	8791.20	4307.75	***

*** p<0.001, ** p<0.01, * p<0.05 (t-test and chi2 tests)

Female employees are about three years younger on average, and have three years less tenure than men. They are less likely than men to have an advanced degree, and more likely to have completed their educations without graduating from a four-year college or university. Although the gap between men's and women's ages is not large, men are about twice as likely to be married as women. Men are also about three times more likely to have children. These suggest that while women (or their parents: see Brinton 1993) underinvest in education

compared to men, household duties are an unlikely explanation for any gender gaps here: most of the women in the sample have focused on their careers, perhaps to the exclusion of family life.

Turning to the statistics about job sorting, we see that women are overrepresented in jobs that at a national level are female-typed. Although a majority of women (57%) hold regular jobs on the management track, women are still twice as likely as men to hold short-term contracts and are about three times as likely to work on the non-promotional general track. In terms of job characteristics, women are more likely than men to work in clerical roles, and have less supervisory authority.

Turning next to the gender pay gap, the unadjusted gap is 34%, with women earning approximately 5.8 million yen (\$58,000) on average, compared to 8.8 million yen (\$88,000) for men. I also estimate the size of the gender gap after making adjustments for other variables that may affect it. I do not display the coefficients of these models, but they are available from the author on request. A summary of the findings appears in Table 1.3.

As this table shows, adding random effects for teams and firms does not change the estimated gap very much—with these effects, the estimated gender gap actually rises slightly to 36% (Model B), indicating that sorting into teams and firms with lower pay does not drive the female wage disadvantage in this sample.

Adding adjustment variables for education, age, age squared, tenure, tenure squared, number of previous jobs, and work hours does narrow the gap, but does not eliminate it (Model C). With these adjustments, predicted annual earnings for women rise to 7.1 million yen (\$71,000), compared to 8.8 million yen (\$88,000) for men. This still represents a sizeable gap of 19%.

Table 1.3: Summary of Analysis of Gender Pay Gap

<i>Controls</i>	Model A	Model B	Model C	Model D
Random effects for teams and firms				
Education				
Age and age ²				
Tenure and tenure ²				
Number of previous employers	34%***	36%***	19%***	15%***
Work hours				
Job Content				
Authority				
Job Class				

Shaded portion represents which adjustment variables the model includes.
 Percentages represent the unexplained wage gap between men and women.
 *** p<0.001, ** p<0.01, * p<0.05 (significance of gender difference)

Surprisingly, adding job-level variables, including controls for job content (clerical versus non-clerical), supervisory authority, and job class (management track, general track, and contract), has only a relatively minor impact on the residual gap between men and women (Model D). After these adjustments, women’s predicted earnings are 7.3 million yen (\$73,000), compared to 8.6 million yen (\$86,000) for men, a gender gap of 15%.

These analyses of income indicate that within-job pay gaps contribute more to overall gender inequality than between-job pay gaps in this context. In all three models, the substantively large pay gaps between men and women are also statistically significant, even after job-level adjustments in Model D. This final model suggests that between-job pay gaps also exist, but only between regular and management track jobs, not between contract and management track jobs. Predicted earnings for an employee on the management track are \$7.7 million yen (\$77,000). For an employee in a contract position they are \$7.3 million yen (\$73,000, a statistically insignificant difference), but in general track jobs, predicted wages are much lower, at 5.8 million yen (\$58,000). The gap between general and management track workers (25% ceteris paribus) is thus larger than the within-job pay gap between men and women (15% ceteris paribus), but within-job wages differences contribute more to the gender pay gap overall because the majority of women work in the management track.

Analysis of Discrimination and Preferences in Job Placement

Next, I look for evidence that discrimination drives women’s concentration in contract and general track jobs, using job quality as an outcome variable. The results from this regression appear in Table 1.4.

Table 1.4: Regression of Job Class on Job Quality

	Model 1	
	Beta	SE
<i>Individual-level variables</i>		
Male	0.17	0.10
Age	0.02***	0.01
Education ^a		
BA	0.05	0.15
Above BA	0.00	0.17
Tenure	-0.01+	0.01
Work Hours	0.01	0.01
Job class ^b		
Contract	0.01	0.18
General track	-0.12	0.20
Contract * male	0.08	0.22
General track * male	-0.34	0.26
Constant	2.95***	0.33
<i>Model information</i>		
Observations	420	
Number of firms	12	
Individual-level variance component	0.54	
Team-level variance component	0.00	
Firm-level variance component	0.04	

Results are from ANCOVA with random effects models (HLM). All slopes are fixed.

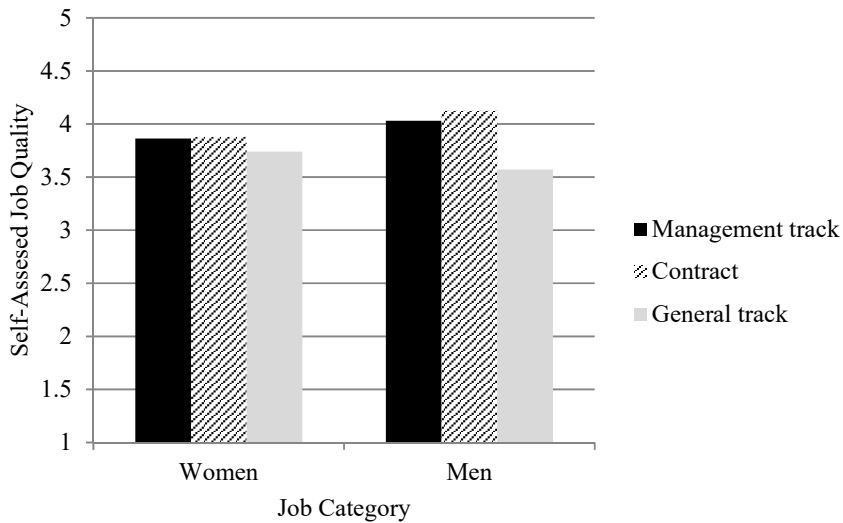
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

^a Reference category is less than BA

^b Reference category is Management track

None of the main effects for job class are significant, indicating that differences in self-assessed job quality between women in the two female-typed job classes and women in management track positions are statistically indistinguishable from zero. The main effect for men and the interaction effects between job class and the male dummy are also insignificant, showing that there are no statistical differences between job quality for men in any job class and women on the management track. Predicted job quality for men and women by job class appears in Figure 1.2.

Figure 1.2: Predicted Self-Assessed Job Quality by Gender and Job Class



As Figure 1.2 shows, substantively as well as statistically, job quality does not differ very much for women in the three tracks. Job quality for women in contract and management track positions is equivalent, and job quality for women on the general track is only 3% lower than that of women on the management track. There is thus no support for Hypotheses 1a and 1b. Only men on the general track rate their jobs notably lower than other groups; because the gap in subjective job quality for men on the management track and the general track is larger than for women, there is also no support for Hypothesis 1c.

Analyses of Discrimination and Performance in Wage Determination

Next I turn to the analysis of within-job wage determination. As described above, large unexplained wage gaps between women and men remain, even after standard controls. Could this gap be attributable to performance, or employers' assessments of performance? As we can see from the descriptive statistics, as a proportion of base pay, women's bonus payments are about 21% lower than men's on average (0.24 versus 0.31). Depending on the relationship between performance and base pay, this could entirely explain within-job wage

gaps. I generate four additional models to examine how men's and women's performance is rewarded. Results appear in Table 1.5.

Models 2 and 3 use monthly base pay as the outcome variable, while Models 4 and 5 use total annual income, including both base pay and bonus amounts. Models 2 and 4 constrain the slope of performance to be identical for men and women, while Models 3 and 5 permit the slope of performance to vary by gender.

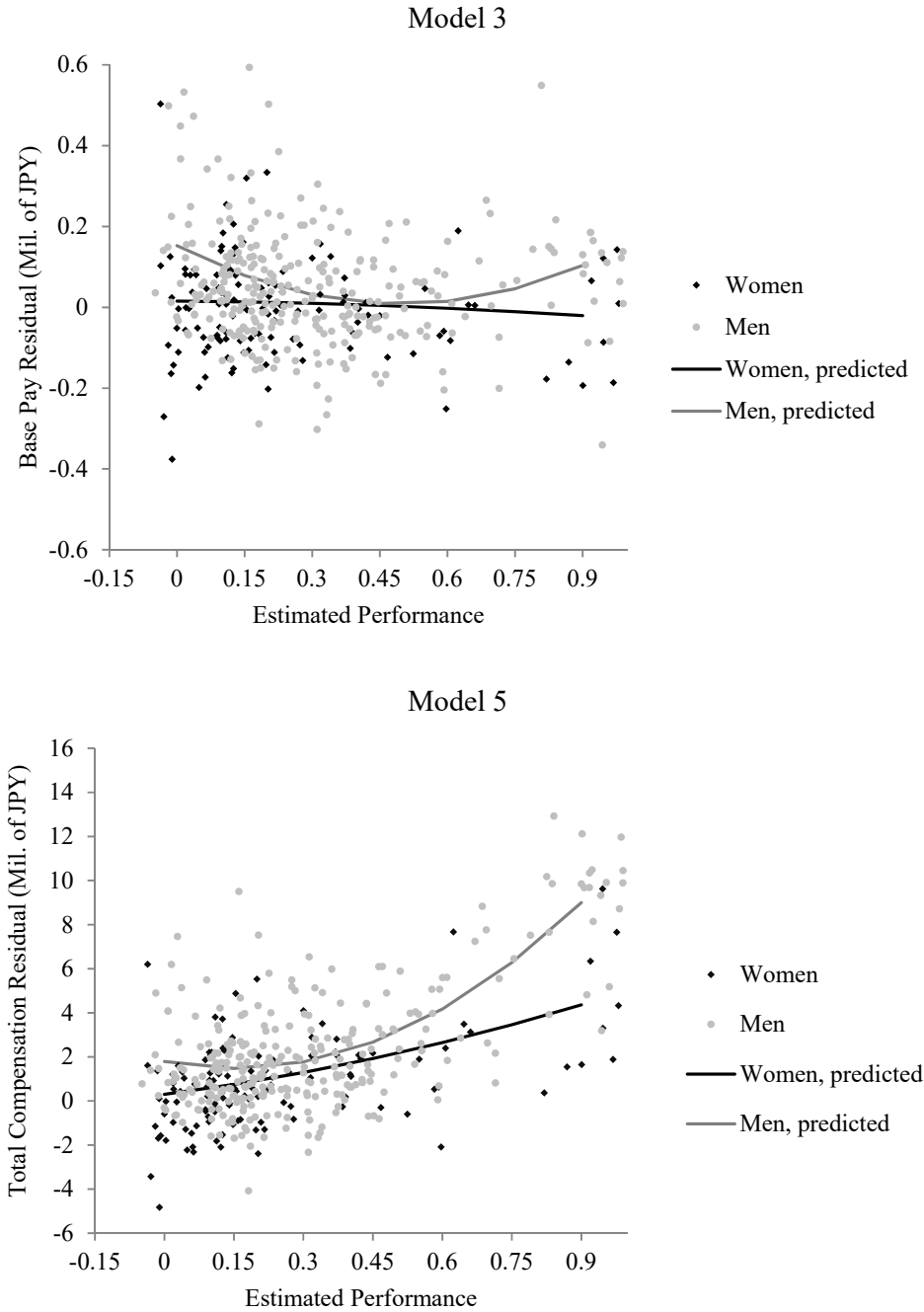
I turn first to Model 4, which is identical to Model D, but with the addition of a curvilinear term for performance. The main effect for men is substantively large, and statistically significant. This indicates that the intercept of performance on pay is higher for men, and that recent performance does not explain away the gender pay gap in total pay. Indeed, adding the performance term does almost nothing to reduce the unexplained gender pay gap. Predicted pay for women and men in Model 4 is 6.8 million yen (\$68,000) and 7.9 million yen (\$79,000), respectively, a pay gap of 14% compared to 15% in Model D. Similarly, Model 2 produces a large male effect on wages, and estimates a 13% gender gap in base pay, net of performance and other adjustment variables. These results provide support for Hypothesis 2.

Table 1.5: Regression of Performance on Monthly Base Pay and Total Annual Earnings

	Model 2 (Base Pay)		Model 3 (Base Pay)		Model 4 (Annual Earn.)		Model 5 (Annual Earn.)	
	Beta	SE	Beta	SE	Beta	SE	Beta	SE
<i>Individual level variables</i>								
Performance	-406.98***	100.51	-7.84	172.81	-2,242.99	1,546.02	2,687.50	2,614.53
Performance squared	399.51***	107.81	-35.67	187.65	10,261.80***	1,658.19	2,026.15	2,839.05
Male	66.51***	17.81	137.24***	33.04	1,129.23***	273.97	1,490.87**	499.85
Male * performance			-573.32**	204.49			-6,807.59*	3,094.01
Male * performance sq.			621.11**	219.99			11,449.58***	3,328.57
Age	25.34**	8.12	26.88***	8.10	324.35**	124.91	395.94**	122.59
Age squared	-0.11	0.10	-0.13	0.10	-1.26	1.47	-2.10	1.44
Education ^a								
BA	64.76*	30.15	65.14*	29.89	1,003.10*	463.60	1,092.63*	452.10
Above BA	65.87+	34.56	65.86+	34.25	947.14+	531.46	1,021.53*	518.06
Tenure	2.43	3.61	1.75	3.60	89.01	55.58	60.25	54.46
Tenure squared	-0.23*	0.10	-0.21*	0.10	-4.70**	1.50	-3.81**	1.47
Number of prev. employers	-18.51+	10.73	-17.87+	10.65	-245.48	165.04	-264.26	160.98
Work Hours	1.60	0.98	1.63+	0.97	20.08	15.04	19.67	14.65
Clerical work	-80.11**	28.23	-79.48**	28.49	-1,462.19***	433.33	-1,135.38**	430.57
Authority	8.29***	1.32	8.23***	1.30	124.35***	20.23	122.23***	19.71
Job class ^b								
Contract	-45.68*	22.08	-42.43+	21.92	-673.67*	339.58	-575.69+	331.39
General track	-92.43***	27.43	-96.00***	27.54	-1,571.33***	422.11	-1,384.07***	416.56
Constant	-416.65*	166.05	-489.15**	166.80	-6,290.16*	2,550.49	-7,970.49**	2,518.37
<i>Model information</i>								
Observations	420		420		420		420	
Number of firms	12		12		12		12	
Individual-level var. comp.	9634		19143		4622294		4380537	
Team-level variance comp.	2486		2304		551498		537006	
Firm-level variance comp.	19479		9844		2033000		1951602	

Notes to Table 1.5: Results are from ANCOVA with random effects models (HLM). All slopes are fixed.
 *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.10$. ^a Reference category is less than BA; ^b Reference category is Management track

Figure 1.3: Performance and Compensation Residuals for Men and Women



Y axes represent actual income minus expected income, where expected values are calculated from education level, age, age squared, tenure, tenure squared, work hours, number of previous employers, job content, job class, level of supervisory authority, team, and firm. Lines represent expected values accounting for these variables plus estimated performance and gender. Points have some random jitter for visual clarity. Estimated performance cannot fall below 0. 1 million JPY \approx 10,000 USD.

Turning to Models 3 and 5, curvilinear interaction terms between the male dummy and performance are also significant, indicating the slope (as well as the intercept) for performance on pay differs for women and men. I show a visualization of these effects in Figure 1.3.

The points on the graphs in Figure 1.3 represent the compensation residual—respondents' actual income minus their expected income. To visually preserve the relationships between gender, performance, and compensation illustrated by the coefficients in Models 3 and 5, the residuals represented by points are calculated using all model components except for gender and performance. The predictions plotted by lines show expected values of compensation given gender and performance. Y values above zero represent respondents who earn more than expected, given their human capital and job characteristics (but not their gender or performance), while Y values below zero represent respondents who earn less than expected, given these background characteristics. If recent performance has a positive impact on wages, we would expect more Y values above zero on the right side of the graph (among high performers) and more Y values below zero on the left side of the graph (among low performers).

Turning first at the results for base pay, we see that indeed, most men (69%) with high performance (above 0.45) have Y values above 0. In other words, men with high performance tend to have higher compensation than we would expect given their background characteristics. However, we do not observe the same result for women. On the contrary, only about half of women with high performance (44%) have higher than expected wages; the slope of female performance on base pay is flat and there is no evidence that female high performers earn more in base pay than comparable women with average or below average performance. Of course, as we can see in the graph for Model 5, when bonus amounts are factored in, women with high performance do earn higher total incomes than other women.

But because we do not see this relationship in base pay, we can infer that high performing women's earning advantage comes solely from the bonus itself, not from higher base pay. In comparison, for men in Model 5, we see a much steeper slope of performance on total compensation than we do for women. This is because high performing men are advantaged both in terms of base pay, as we see in Model 3, and in terms of bonus.

Overall, the results plotted in Figure 1.3 support both Hypothesis 2 and Hypothesis 3. Unexplained pay gaps remain between men and women after adjusting for recent performance, and these gaps appear larger among the highest performers. However, there are also some unexpected results. Turning again to the visualization of Model 3, we see that the Y values for men with performance below 0.45 are not concentrated below 0. Rather, 58% of men with estimated performance below 0.45 also have Y values above 0. So although a higher percentage of high performing men have higher than expected salaries, a majority of low performing men do as well. Indeed, a small number of men (20) have very high base pay (more than 0.2 million yen higher than expected) and well below average performance (below 0.2). These men's presence generates the left-hand side of the U-shaped predicted relationship between recent performance and base pay for men. This relationship is less apparent in the model for total compensation, because bonus is such a major component of total pay, so by definition here low performers cannot earn the highest salaries. However, a distinct group of highly paid people (predominantly, but not exclusively men) with performance below 0.2 is still apparent in the Model 5 graph.

On the other hand, women with estimated performance below 0.45 are about as likely to be undercompensated as overcompensated based on their human capital and job characteristics. 49% of women with performance of 0.45 or below have base pay that is higher than expected, similar to the proportion among female high achievers who do. There are a handful of women (4) with very high pay and below average performance, but their

performance is not as low and their pay is not as high as the men in this group. Moreover, their presence is counterbalanced by a much larger number of women with low performance and low pay, such that we do not observe the same U-curve for women that we do for men. I discuss potential reasons for these unexpected results further in the following section.

Discussion

The analysis above examines whether there is any evidence of gender discrimination in Japanese firms' job placement and pay determination processes. I find no evidence of gender discrimination in job placement. If the firms in this study exclude women from the management track, we would expect women on other tracks to rate their job quality lower than women on the management track, because discrimination would push some women who prefer the management track into what they see as less desirable jobs on other tracks. In fact, women's job quality is substantively and statistically equivalent across tracks. If anything, it appears that men are more likely to working in general track jobs when they would prefer management track jobs.

Unlike studies (e.g. Fernandez and Sosa 2005) that examine how companies treat their applicant pools, and how that treatment differs by applicant gender, this approach cannot specify the magnitude of discrimination, because we cannot know how women who may be excluded from the management track responded. Certainly not all of them would apply for general track jobs at the same firms, nor would it be likely that everyone who is working on the general track unwillingly had previously applied unsuccessfully for a management track job at that same firm. However, given that general track jobs at prestigious firms still offer relatively generous salaries, it seems likely that if women are excluded from the management track, some of them would apply for and win jobs on the general track.

Moreover, the fact that women excluded from management track jobs at other firms might apply to general track jobs at the firms in this sample makes it more likely that we would detect discrimination in this instance, not less so. The null finding here thus suggests that, despite the general assumption that the tracking system is a way to hoard the highest-paid opportunities for men (e.g. Kumamoto-Healy 2005; Mun 2016; Nemoto 2016), women are in some cases content to forgo those opportunities.

At the same time, the results do not suggest that Japanese firms treat women equally within jobs. We observe sizable wage gaps of around 15% between women and men doing the same work, in the same job class, on the same teams, in the same firms. These results are not attributable to gender differences in recent performance.

But is the within-job gender wage gap attributable to discrimination? I considered several other possibilities in supplementary analyses. Firstly, it is possible that firms compensate workers directly for earning professional certificates and participating in training (e.g. Tam 1997). While I do not know in detail how much training workers have undergone, I asked survey respondents to select up to seven types of training they participated in at their current employer, including training in business manners, computer skills, international management, supervisory skills, negotiations, sales, and legal compliance. On average, women participate in approximately one fewer training types than men, even after adjustments for tenure, age, and track status; but participating in more trainings (or in particular types of trainings) is not associated with higher wages. It is therefore unlikely that training differences account for observed within-job pay gaps.

Alternatively, it is possible that assumptions I rely on to estimate performance are violated. Although the analysis does not require performance to be constant over workers' careers, in order to detect discrimination, I assume that the relationship between recent

performance (which is captured in the measure of bonus), and past performance (which is not captured by bonus) does not vary by gender. However, this assumption may be wrong if, for example, women's performance varies more over the life course, depending perhaps on the ages of children. If women's life course variance in performance is large, this could explain why current performance has a null relationship with base pay for women, since women who performed highly in the most recent period may have performed poorly in the past, and vice versa.

If variation in household responsibilities over the female life course makes women's performance more unstable than men's over time, we might reasonably expect this effect to be concentrated among mothers. I therefore reran Model 3 without mothers to see if, among childless women, the relationship between recent performance and base pay would more closely approximate that for men. Specifically, we might expect to observe more mothers in the lower right quadrant of the charts in Figure 1.3 (e.g. current high performers with surprisingly low pay), and perhaps the upper left quadrant (e.g. current low performers with surprisingly high pay). This was not the case. There were only four mothers who were high performers, but they were equally distributed in the upper and lower quadrants on the right side. There were 23 mothers with low or average performance, and they were also evenly split between the upper (11 women) and lower (12 women) quadrants on the left. In sum, there is no evidence that life course variation in performance drives the different relationships between men's and women's performance and their compensation.

Thirdly, estimates of performance I derived from bonus amounts may also be biased by gender. This is unlikely to generate the gender differences in returns to performance I observe here, however. Castilla (2008, 2015) finds that in a large U.S. service firm, women received lower bonuses than men with identical performance ratings. If this occurs in this study context, it would bias our estimates of female performance downwards, relative to men's, and

reduce rather than exaggerate estimates of the difference in male and female returns to performance.

Because of the weaknesses of these alternative explanations for within-job pay differences net of performance, it seems most likely that discrimination causes the residual gender pay gaps. Moreover, the evidence suggests that discrimination is the single most important contributor to the overall gender gap after adjustments for human capital in this sample. Men's and women's preferences for different jobs also produce wage differences, but because most women in this context do not work in female-typed jobs, job segregation contributes relatively little to the overall gap. Performance differences, on the other hand, contribute almost nothing.

Lastly, I discuss the reasons for the unexpectedly high number of men with low performance and high base pay. I suspect that these are men who accumulated large raises during previous periods when their performance was high. In the most recent period or periods their performance was low, but because base pay reductions for poor performance are smaller and less likely than salary increases for high performance (Tsuru, Abe, and Kubo 2005), the base compensation of these men remains high even though they have performed poorly. There are few women in this group, because as suggested by the flat relationship between performance and base pay for women, women do not receive raises commensurate with performance, and thus do not have the same opportunities as men to earn high salaries while resting on their laurels.

Conclusion

Theory on “the opportunity structure for discrimination” (Petersen and Saporta 2004) generates contradictory predictions about whether discrimination is more likely in job

allocation or in pay determination. While theorists agree that transparency is likely to reduce employer discrimination, they differ in their emphasis on what information needs to be transparent and who must be able to see it. Petersen and Saporta (2004) suggest that information about how the firm treats a worker's peers is required, and the plaintiff herself must have access to that information. Castilla (2008) suggests that more general aggregate information about average treatment of different groups is necessary, and visibility to more general audiences, such as other employees, the public, or government regulators may be more important than transparency to the immediate persons concerned. This analysis examines, for the first time, evidence of discrimination in job allocation and in pay determination in the same context. I find no evidence of gender discrimination in job allocation, but I do find evidence that strongly suggests discrimination in pay determination.

These findings support the perspective that general information about group treatment needs to be available to general audiences to constrain discrimination. Consider first the availability of information about pay for men and women within jobs at Japanese firms. Even anonymized data about pay at Japanese companies are extremely rare. My data and those of Hashimoto and Sato (2014) and Kato, Kawaguchi, and Owan (2013) are unusual exceptions. However, individual women and men do have some sense of their pay relative to their peers. Receiving higher total compensation or base pay than expected based on human capital and job characteristics (the Y values plotted in Figure 1.3) is strongly associated with respondents' likelihood of saying that they earn more than similar peers at their firm. But regulators, researchers, and the public have very little to go on to consider whether or not firms discriminate against women in pay within jobs.

Conversely, there is extensive evidence in the public sphere about job segregation. Private companies have collected and sold company-level data on women's representation among new hires and in management since the 1980s (Mun 2016). More recently, this

information has also become publicly available online through a government clearinghouse website (Ministry of Health, Labor, and Welfare 2016). The data provided on the website are not necessarily consistent across companies, and can be ambiguous—there is little to indicate whether a firm that hires few women does so because it makes many offers to women, and women reject the offers, because the firm excludes qualified women, or because the female applicant pool is of lower quality than the male. Nonetheless, even if the data available do not permit straightforward accusations of discrimination, companies with “bad numbers” may face uncomfortable questions from activists, the media, and the courts. This added scrutiny may prompt companies to reconsider problematic hiring processes preemptively.

The findings also raise new questions about other mechanisms that cause discrimination to occur in one stage of employment but not in another. The results of this study are consistent with the transparency mechanism, but I do not test it directly here (for one such test, see Castilla 2015). Thus other mechanisms may also be at work, in addition to the transparency mechanism. For example, the apparent lack of discrimination in job placement accompanied by a comparatively high level of pay discrimination suggests that there may be tradeoffs between different metrics of equality. In particular, as women’s representation in male jobs grows, so do the costs of raising women’s pay to match men’s. I do not suggest that employers deliberately collude to keep women’s wages low—but in order to control labor costs senior managers in firms with many women in traditionally male jobs may quietly choose not to investigate wage inequality closely enough to find evidence of a problem.

A body of recent research (e.g. Kalev, Dobbin, and Kelly 2006; Dobbin, Schrage, and Kalev 2015) tests the effects of changes in human resource practices on gender and racial integration of managerial ranks, and has found that some policies and practices have positive effects on integration. However, the findings of the current paper suggest that the same

policies that promote the gender integration of jobs may have null or even negative effects on within-job pay gaps, depending perhaps on levels of transparency in pay. Investigating whether and under what conditions particular policies have consistent effects across different measures of gender inequality is a fruitful area for future research.

What do these results tell us more broadly about gender inequality and human resource practices in Japan? Scholars agree that interlocking institutional processes all contribute to Japan's high level of gender inequality, including but not limited to Japan's tax policy favoring a breadwinner model, the oversubscribed public childcare system, men's low contributions to household tasks and childrearing, and the gendered tracking system in employment (Boling 2015; Estevez-Abe 2013; Nemoto 2016). However, we have seen here that even women who work at some of the most progressive firms in the country, many of whom have never married or had children, still earn less than their male counterparts doing the same jobs. In other words, even if all other barriers to gender inequality disappeared overnight, the unequal way in which firms compensate men and women's performance within jobs would perpetuate a high level of gender inequality.

The results also demonstrate that even in firms that emphasize performance in determining compensation, women's pay disadvantage does not disappear. Age-wage profiles have become less steep as firms have put a greater emphasis on performance (Mitani 2010; Tsuru, Abe, and Kubo 2005) Theoretically this could benefit women, whose careers at a single firm may be shorter than those of their male counterparts, and who thus accrue fewer of the benefits to age and seniority (Nemoto 2016). But even if performance pay has shrunk the pay gap between men and women, we see here that it has not closed it, and that high-performing women are even less able to close the pay gap with men than low-performing women.

Moreover, the findings cast serious doubt that performance pay effectively motivates either male or female employees at large Japanese firms. The results suggest that men can accumulate large raises through high performance early in their careers. However, after reaching a certain income level, some men may decide that the marginal returns to high performance have diminished and subsequently reduce the effort they put into their jobs. Theoretically, these men could coast along with poor performance and high base pay for years to come. Women, on the other hand, have less incentive to perform well in the first place because they do not appear to have the same opportunities as men to increase their base pay through high performance. It would not be surprising if part of women's relative underperformance compared to men in this sample is simply a rational response to their negligible returns to performance.

The Abe government has made building "a society where women can shine" (Ministry of Foreign Affairs 2015) a centerpiece of its economic policy, and has argued that Japan needs women's economic contributions to break out of its three decade long economic slump. To promote gender integration, the government encourages companies to promote women to management roles and has set (non-binding) targets of filling 15% of managerial roles in private companies with women by 2030. The current study suggests that these already unambitious efforts are likely to fall flat. Because women see lower returns to performance and thus have less incentive to perform well, firms may struggle to identify women appropriate for promotion. Further, unless firms also face greater pressure to equalize wages, within-job wage gaps will persist in spite of (or perhaps because of) women's greater representation in management.

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CHAPTER 2

CAPITALS OR CONTEXTS? FOREIGN WORKERS' ECONOMIC ASSIMILATION IN JAPAN'S HIGHLY SELECTIVE IMMIGRATION REGIME

Introduction

Why do some immigrants succeed economically while others struggle? Since the early days of scholarly interest in this question (Glazer and Moyinhan 1963; Gordon 1964), scholars have debated the relative contributions of two families of predictors: capitals and contexts (e.g. Alba and Nee 2003; Kasinitz et al. 2008; Portes and Rumbaut 2001). Scholars agree that immigrants' economic resources, their human capital and training, and their social capital facilitate economic assimilation. But, at the same time, immigrants' ability to assimilate is not solely a function of their individual and family characteristics. Laws, institutions, norms, and attitudes in the host society—the contexts into which immigrants integrate—also help or hinder their economic attainment.

Recent scholarship in the capitals tradition has focused on refining measures of economic and human capital and estimating how fine-grained differences in the quality of capital are associated with immigrants' earnings (Chiswick, Lee, and Miller 2005). For example, rather than simply quantifying immigrants' years of education, researchers have distinguished between human capital acquired in the host country and human capital acquired in the origin country (e.g. Friedberg 2000; Zeng and Xie 2004). Human capital acquired in the origin country can be further differentiated by its "quality," where education obtained in wealthy countries of origin facilitates speedier economic assimilation than education obtained in poorer origin countries (e.g. Kaushal 2011; Bratsberg and Ragan 2002; Li and Sweetman 2014).

These refinements in the measurement of human capital have reliable associations with labor market outcomes but nonetheless suffer from two methodological and conceptual

problems. First, in Western countries, host country human capital and the quality of immigrants' foreign human capital—predictors in the capitals tradition—are strongly associated with natives' attitudes towards members of different immigrant groups, a predictor in the contexts tradition (Li 2001; Reitz 1998). The most successful immigrants in Western countries—those from Western Europe, North America, and Oceania—are also those who quickly develop (or already hold) host country human capital; these same immigrants also have “high quality” home country human capital, and face little or no prejudice from natives. Because immigrants to Western destinations tend to be simultaneously advantaged or disadvantaged in terms of their capital and the receiving context, measures intended to capture individual-level capitals, such as host country language ability or sending country GDP, do not cleanly do so, and instead may pick up on the effects of context of integration.

Second, these recent studies of human capital ignore differences in immigrant selectivity by national origin (Feliciano 2005). This is a challenge for studies of human capital quality that use sending country level variables such as GDP per capita or student-teacher ratios as a proxy for the quality of the education that immigrants from that country acquired prior to migration (e.g. Li and Sweetman 2014; Kaushal 2011). When the motivations and selection mechanisms for emigration vary by sending country, it is problematic to assume that sending country level measures describe immigrants from all sending countries equally well.

This paper addresses these twin shortcomings by assessing the relationship between fine-grained measures of human capital and economic outcomes for immigrants in the context of Japan. Unlike in Western countries, in Japan, the immigrants who face the most hostile context of reception—other East Asians—are relatively advantaged in terms of their human capital. Immigrants from the West, on the other hand, face a positive context of reception, but do not acculturate as quickly. In other words, the collinearities between capitals

and contexts that plague analyses of immigrants' economic assimilation in the West are largely absent in Japan. This lack of collinearity means that measures of human capital are not "polluted" by context of reception, and make possible a cleaner measurement of capitals' true effects on immigrants' economic outcomes.

This study uses an original sample of 524 workers from Japan, other East Asian countries, Southeast Asia, and the West. All workers are white-collar professionals employed in a homogeneous sample of twelve large Japanese firms. This design mitigates concerns about the effects of differential selectivity by country of origin by comparing immigrants who, regardless of origin country, are presumably similar to each other in unobserved ways (such as level of motivation or ability), because they were hired to work on the same teams at the same firms.

In this context, I find that acquisition of host country human capital and estimated quality of education in the sending country do not matter for immigrants' economic assimilation, defined as the achievement of economic parity with natives. Observed inequalities, with lower wages for East Asians relative to Japanese and higher wages for Westerners, are best explained by natives' positive attitudes towards Westerners and negative attitudes towards other East Asians. Although these results are, of course, specific to the Japanese context, they imply that previous research may overestimate the benefits of acculturation and "high quality" human capital for immigrants, and underestimate the importance of the context of reception and, more specifically, natives' attitudes towards members of different groups.

Why Are There Differences in Economic Attainment between Immigrant Groups?

Human capital theory explains a great deal of the differences in earnings between natives and the foreign-born and among immigrants of different national origins. For

example, in the United States, members of economically prosperous immigrant groups such as Asian Indians are more likely to hold undergraduate or advanced degrees than native-born whites (Pew Research Center 2012), and members of impoverished groups, such as Guatemalans, are more likely to have very low levels of education (Brown and Patten 2013). The positive association between human capital and earnings among immigrants is well established and robust across all contexts in which it has been estimated (Chiswick, Lee, and Miller 2005). But while these obvious variations in human capital account for some of the differences in immigrants' economic attainment, in many contexts immigrants still earn less than natives with similar qualifications (e.g. Chiswick and Miller 2009), and within levels of education, members of some immigrant groups still earn more than others (e.g. Portes and Rumbaut 2001, 2006). These remaining differences present an enduring empirical puzzle (Phythian, Walters, and Anisef 2011), and although scholars have made some progress towards understanding the sources of these residual gaps, their results are often causally ambiguous.

One strategy to account for residual earnings gaps is to refine the measurement of human capital (e.g. Zeng and Xie 2004). For example, immigrants with more education and work experience in the host country experience better economic outcomes than those with equivalent years of education and experience obtained in the country of origin. In particular, immigrants with more host-country education hold more prestigious occupations (e.g. Akresh 2006; Kanas and van Tubergen 2009), earn higher wages (e.g. Bratsberg and Ragan 2002; Friedberg 2000; Kaushal 2011; Zeng and Xie 2004), and have higher net worth (Painter 2013) than their immigrant counterparts without only (or primarily) home country education. Researchers have replicated these findings for a number of countries, including the United States (e.g. Bratsberg and Ragan 2002; Akresh 2006), Canada (e.g. Li 2001; Aydemir and Skuterud 2008; Skuterud and Su 2011), the Netherlands (Kanas and van Tubergen 2009),

Belgium (Kanas and van Tubergen 2014), Sweden (Duvander 2001; Nordin 2009), Norway (Støren and Wiers-Jenssen 2010), and Israel (Friedberg 2000).¹⁰

Why, though, is the payoff to host country education and experience so consistently higher? One potential explanation is acculturation. Immigrants who receive their education and work experience in their host country are not only more likely to be fluent in the language of the host country, but they are more likely to acquire diffuse, unmeasured assets, such as knowledge of “business practices, and of norms and institutions of the host country” (Kaida 2013). Consistent with this argument, in some studies, wage differences between native-born Whites and foreign-born Asian immigrations to the United States disappear after adjusting for the place of education (Zeng and Xie 2004).

At the same time, other studies find that, even after accounting for different distributions of home and host country education and experience by ethnic group, group differences in the size of the benefit of host country education and experience (or disadvantage of a home country education and experience) persist (see Painter 2013 for a review). Origin country education is more valuable for some groups than for others (see for example, Bratsberg and Terrell 2002). Further, in a few exceptional cases, origin country education is *more* valuable than host country education: for example, among graduates of European universities living in Israel (Friedberg 2000), graduates of both Chinese and Western universities employed in Japan (Takenaka, Ishida, and Nakamuro 2015), and graduates of Japanese universities in the United States (Kim and Sakamoto 2010).

This unexplained group variation has prompted a further extension of human capital theory that focuses on variations across countries in the quality of the education system and

¹⁰ Where studies have compared migrants with high levels of educational attainment to those with lower levels, research has found greater returns to host country human capital among the more educated group (Bratsberg and Ragan 2002; Painter 2013). In other words, for more educated workers, host country human capital provides an even larger advantage than for less-educated workers.

in prior work experience (e.g. Bratsberg and Ragan 2002; Bratsberg and Terrell 2002; Li and Sweetman 2014). When sending countries have better educational systems, home country education will be more valuable in the host country labor market, and the relative benefit of host country education will be lower. Conversely, when host countries have better educational systems, the value of host country education will exceed that of home country education. Consistent with these claims, immigrants with schooling from high GDP and high test score countries tend to earn more than immigrants from low GDP and low test score countries, but these patterns are weaker or non-existent for 1.5 generation immigrants who migrated at an early age and thus received most of their education in their new homes (Li and Sweetman 2014; Bratsberg and Ragan 2002; Kaushal 2011; Bratsberg and Terrell 2002).

As promising as these extensions to the basic human capital narrative may be, they are susceptible to two potential challenges. First, in the Western contexts where this research has been conducted, it is very difficult to distinguish between acculturation and quality narratives, and no studies actually attempt to disentangle these two factors in a rigorous way.¹¹ Immigrants who are advantaged in terms of (unmeasured) acculturation are also those most likely to be advantaged in terms of human capital quality. Consider, as an example, the case of European immigrants to the United States. Studies of educational quality rank education from Canada, Australia, and Western Europe as the best (Kaushal 2011; Bratsberg and Ragan 2002; Li and Sweetman 2014). But because of a shared cultural history between these regions and the United States, immigrants from these countries are also most likely to have an advantage in terms of unmeasured acculturation. In other words, there is a high degree of collinearity between quality and acculturation.

¹¹ To some extent, it is possible to measure acculturation with language ability, but, as discussed above, there are aspects of acculturation such as familiarity with norms, which standard datasets do not measure.

Omitted measures of discrimination may also bias estimates of the economic advantages of acculturation and educational quality. That is, not only are European immigrants the most acculturated and the most likely beneficiaries of a high quality education system in their home countries, but they are also the least likely to experience racial or ethnic prejudice (Kim 2015; Oreopoulos 2011). Conversely, the same immigrants who would be the most disadvantaged in terms of acculturation and human capital quality—those from poor Asian, Latin American, and African countries—are also the most likely to experience prejudice and discrimination (Bonilla-Silva and Dietrich 2008).

Studies of human capital quality and acculturation attempt to address the issue of race by making within-race or national origin comparisons for the first and 1.5 generations, but this circumvention ignores issues of selection bias. When immigrants are educated primarily in their home countries and migrate as adults, it is the immigrants themselves who select into immigration. Conversely, among immigrants who migrated as children and received their education in the host country, it is immigrant parents who have selected into immigration, not their children. Some scholars (e.g. Kasinitz et al. 2008; Lee and Zhou 2015) argue that the children of immigrants have a special advantage because their parents' sacrifices motivate them to excel. Thus, when researchers use large national surveys to compare outcomes for immigrants educated abroad and at home, they may confound the attitudinal advantage of the children of immigrants with the effects of host country schooling. Further, because of correspondence between race and schooling quality, the advantage to host country schooling we observe for the 1.5 generation in some groups but not others could simply reflect that those groups are subject to greater discrimination and must signal belonging more strongly than European immigrants and their children.

An alternative solution, which I adopt here, is to estimate the net associations between place of schooling, racial and ethnic background, and economic outcomes in a labor market

where migrants are not simultaneously advantaged on all three axes—acculturation, quality of education in the country of origin, and experience of prejudice or discrimination in the host country. Specifically, I estimate these associations in the contemporary Japanese context. Of course, the mechanisms identified in the Japanese context do not necessarily generalize to other contexts, where institutional factors that influence immigrant integration trajectories may differ (Reitz 1998; Kogan 2006). As I shall show, the Japanese case illustrates the importance of disentangling these related mechanisms, and of course it is interesting in its own right because Japan is a major economic powerhouse in East Asia and the world.

The Japanese Case

By the year 2060, Japan's population is projected to decline 32% from 2010 levels; by 2100, the population may fall by as much as 61% (Toyo Keizai 2016). Despite these demographic circumstances, Japanese politicians and bureaucrats have eschewed the expansion of low-skilled migration in order to avoid the challenges of integrating those immigrants (Törngren and Holbrow 2017). Instead, immigration policy focuses on attracting immigrants whom the government believes will be easiest to integrate: ethnic Japanese (Shipper 2008), pre-college and college students whose education in Japan helps them acculturate (Liu-Farrer 2009; 2011), and skilled workers (Tsukasaki 2008; Oishi 2012; Holbrow and Nagayoshi 2016).

Japan's efforts to join the "global race for talent" (Shachar 2013) began in the early 1990s, when the government established one of the most liberal skilled immigration programs in the OECD (Fuess 2003; Oishi 2012; Holbrow and Nagayoshi 2016). Unlike the United States, Japan has no quota system for skilled work visas, so in principle any foreigner who lands a skilled job can obtain a visa. Moreover, there is no mandate for employers to identify and employ qualified natives before offering a position to a foreigner, and foreigners

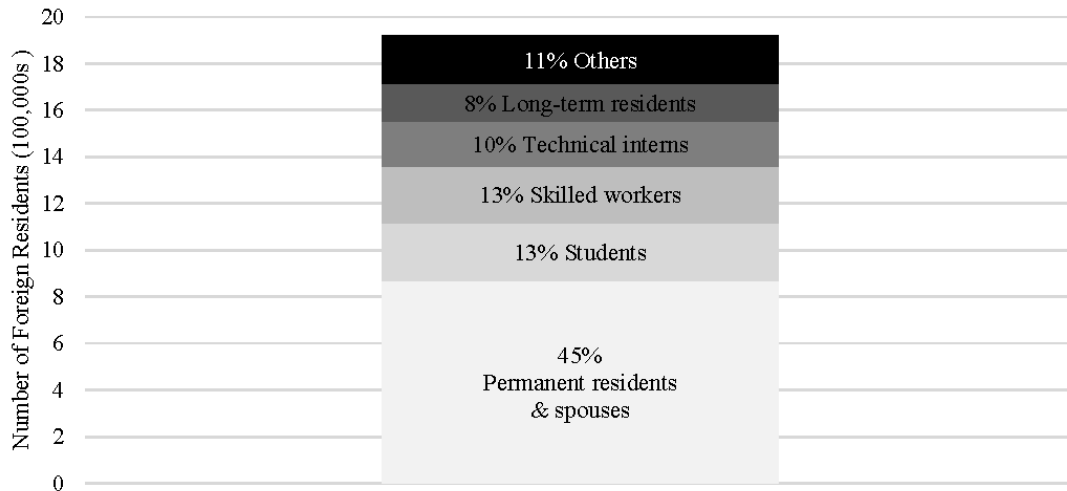
can renew their work permits indefinitely. In 2012, the government also introduced a new visa for highly skilled professionals (HSP visa hereafter) that gives workers with the requisite number of points for experience, income, and educational background a fast track to permanent residency and other preferential treatment (Green 2015; Törngren and Holbrow 2017).

Figure 2.1 shows the percentage of foreigners in various skilled and unskilled visa categories.¹² The largest category is permanent residents, their spouses, and the spouses of Japanese citizens. Permanent residents make up 81% of this group, and 19% are spouses of permanent residents or Japanese citizens. To achieve permanent residency, foreigners must live in Japan continuously for ten years and meet other criteria established by the government. Long-term visa holders, who are ethnic Japanese who may do either skilled or unskilled jobs, have little incentive to switch to permanent residency because they have the same rights and privileges as permanent visa holders without the arduous application burden. Students and trainees would be unable to accumulate the continuous ten years of residency necessary to apply for permanent residency. It thus seems reasonable to assume that most permanent residents are skilled foreigners.¹³ After permanent residents, students and workers with skilled work visas take up the largest shares, at 13% each. The only category unambiguously dominated by unskilled workers, the technical trainee visa, is 10% of the total.

¹² Special Permanent Residents are excluded from Figure 2.1 and Figure 2.2 because they are not immigrants, but third and fourth generation descendants of colonial era migrants to Japan (Chung 2010).

¹³ Spouses of Japanese citizens may be more likely to be low skilled because this is one of the only pathways to long-term settlement for unskilled workers (Ivory forthcoming).

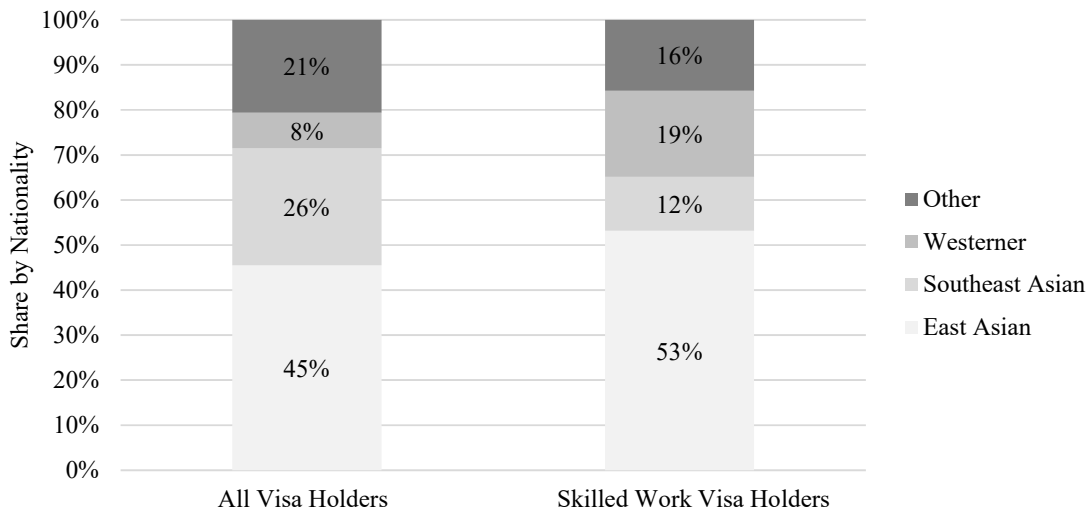
Figure 2.1: Foreign Residents by Visa Type (Excluding Special Permanent Residents)



In terms of composition and size of the immigrant population, the results of Japan's immigration control have been mixed. On one hand, the government has successfully contained low-skilled migration. On the other hand, skilled immigration trails other developed countries (Oishi 2012), as do overall levels of immigration. In 2015, Japan had an estimated 1.9 million immigrant residents, just 1.5% of the population.¹⁴

¹⁴ As above, this does not include Special Permanent Residents. Further, because Japan does not have birthright citizenship and counts foreign residents on the basis of nationality, naturalized immigrants are not included in this total. Japanese-born, second generation immigrants are.

Figure 2.2: Regional Origin of Foreign Residents (Excluding Special Permanent Residents)



This paper will focus on skilled immigrants from three world regions: from the West, from East Asia, and from Southeast Asia. As Figure 2.2 shows, immigrants from these three regions represent more than 75% of all foreigners in Japan, and over 80% of those on skilled work visas.

The unusually high level of selectivity of immigration to Japan makes it an interesting site in which to study economic assimilation. Although human capital theory would predict (and government bureaucrats expect) largely positive integration outcomes from such a highly selected immigrant population, data to assess workers' economic assimilation are scarce and difficult to access. The government does not release census microdata, and nationwide social science surveys do not ask about nationality or ethnicity (Lie 2001). Only a few surveys have collected any data about economic outcomes for immigrants to Japan, and none have collected data from comparable Japanese citizens, making it difficult to assess whether migrants economically assimilate and achieve parity or near parity with natives (see Takenoshita 2006; Takenaka, Ishida, and Nakamuro 2015; Holbrow and Nagayoshi 2016).

Is the highly selective policy successful in generating positive integration outcomes? If skilled immigrants achieve economic outcomes similar to those of Japanese natives, we

could term the policy a success. On the other hand, if even these highly selected skilled workers form a professional underclass who earn considerably less than their Japanese counterparts, the policy could be called a failure.

Japan is also a very useful case in which to study acculturation and human capital, because, unlike in the United States, skilled immigrants are unlikely to be simultaneously advantaged or disadvantaged in terms of acculturation, human capital quality, and context of reception. Consider the three immigrant groups that are the subject of this study. In the United States, as Table 2.1 shows, the same groups are advantaged or disadvantaged regardless of which predictors of economic assimilation are used. Using measures of human capital quality such as GDP, class size, and standardized test scores (see Bratsberg and Ragan 2002; Li and Sweetman 2014), Westerners come from the regions with the best educational institutions, followed by (non-Japanese) East Asians, followed by Southeast Asians. As such, home country human capital quality is the highest for Westerners, lower quality for East Asians, and lowest quality of all for Southeast Asians.

Table 2.1: Predicted Rankings of Labor Market Outcomes for Immigrants by Theoretical Perspective (Japan and the U.S.)

<i>Predicted Ranking</i>	Japan			USA		
	Educational Quality	Acculturation	Ethnoracial Hierarchy	Educational Quality	Acculturation	Ethnoracial Hierarchy
1	W	EA	W	W	W	W
2	EA	SEA	SEA	EA	EA	EA
3	SEA	W	EA	SEA	SEA	SEA

EA = East Asian foreign-born, SEA = Southeast Asian foreign-born, W = White Western foreign-born.

Turning to acculturation and host-country specific capital, Westerners are the most likely to speak (or find it easier to learn) the English language. They are more likely to be familiar with host country norms and values. In other words, Western immigrants to Western countries are also the most likely to quickly acculturate and easily obtain host country specific human capital. Because countries in East Asia are more developed and secular than Southeast Asian countries, and because the average level of English ability is higher in East than in Southeast Asia (Education First 2016), it seems reasonable to assume that acculturation would follow the same pattern. Finally, Western immigrants to Western countries also face quite positive contexts of reception. Because they are most often white, they are least likely of any racial group to face ethnic or racial prejudice and discrimination. East Asians, by contrast, are more likely to face racial or ethnic prejudice than whites, but less so than Southeast Asians (Jones 2013; Bonilla-Silva and Dietrich 2008). Regardless of which predictors one considers, the same ethnic ordering of immigrant groups to the United States emerges.

In Japan, however, no group is advantaged on all three axes. If human capital quality is truly a measure of something universal, Westerners will be advantaged along that axis, followed by East Asians and then Southeast Asians. However, in terms of acculturation, other East Asians—whose languages share an alphabet and many cognates with the Japanese language (Reischauer 1974), and who are exposed to Japanese culture through the ubiquity of Japanese media in their home countries (Otmazgin 2016), have the greatest advantage. Southeast Asians, insofar as they are part of Japan's sphere of cultural influence (Otmazgin 2016), will be less acculturated than East Asians, but more so than Westerners.

In terms of prejudice, Westerners are also highly advantaged. Japan has a long history of elevating whiteness, both in attitudes towards racial groups (Oguma 2002; Befu 2001; Ivory, forthcoming) and in evaluations of appearance and skin tone (Ashikari 2005; Li et al.

2008). East Asians are most disadvantaged in terms of prejudice. Today, around 40% of respondents to national surveys say they would not accept an Asian foreigner as a relative by marriage (author's analysis of JGSS), and extremist groups and individuals harass and belittle resident Chinese and Koreans, particularly on the internet, but also in the public sphere (McLelland 2013; Ryang 2013; Osaki 2016), with little fear of government censure or sanction (Murai 2016). Southeast Asians also face prejudice, but perhaps because there are fewer of them, they are less likely to be targets of vitriolic attacks and less likely to be accused of coming to Japan with criminal intent (NPA 2016). On national surveys, Japanese people are equally or more likely to say they would accept a Southeast Asian as a relative, coworker, or neighbor, compared to their willingness to accept other East Asians, especially Chinese, in these roles (author's analysis of JGSS).

Because advantage (or disadvantage) in terms of acculturation, home country human capital quality, and native attitudes does not align for immigrants to Japan, measures of acculturation and human capital quality are cleaner. In a regression analysis, for example, a variable for Japanese language skill is less likely to pick up the effects of home country educational quality or of context of reception than a variable for native language skill in a Western study context. Further, residual income differences between ethnic groups after controls for human capital are more easily interpreted. In the West, collinearity between acculturation, education quality, and context of reception, means that patterns of residual income differences are ambiguous. If Western immigrants in Western destinations earn more than other immigrants after controls, we could plausibly attribute this advantage to *unmeasured* differences in capitals, or to the context of reception. In contrast, because the two capital mechanisms and the context mechanism suggest different stratification orders in Japan, residual income differences may themselves suggest which mechanism is at work.

Data

To examine stratification between Japanese and foreign employees, and between foreign employees of different national backgrounds, I use a novel employer-employee matched dataset that I collected between February and April 2015, called the Survey on Workplace Environment and Diversity Management. One reason little is known about foreign workers in Japan is that they represent such a small share of the total workforce (Oishi 2012). Only a minority of firms hire non-Japanese citizens (Holbrow and Nagayoshi 2016), and as a result, surveys that randomly sample either individuals or firms fail to capture many foreign workers. To ensure the inclusion of an adequate sample of foreign workers, I sampled firms from the Diversity Subcommittee of the Japan Association of Corporate Executives (JACE), a major business group of Japanese firms. This is an effective sampling frame for two main reasons. First, JACE firms tend to be large and large firms are more likely to hire foreign workers (JILPT 2009). However, even among large firms that hire foreign workers, the modal number of foreign employees is less than four (Holbrow and Nagayoshi 2016). Firms that belong to the Diversity Subcommittee, are particularly likely, even among large firms, to hire non-Japanese, and to hire them in greater numbers. JACE sent a research request to the CEOs of all 205 sub-committee member firms. Twelve firms (5.8%) allowed me to sample their workers.

Ten of the twelve firms in the sample have more than 1,000 employees, and three have more than 10,000. The industries of the twelve firms are diverse. Three are high-tech manufacturing firms, five are primarily business service organizations (e.g. finance, trade), and four focus on consumer service (e.g. retail). Because firms that deliberately treat foreign workers more poorly than native workers would be unlikely to respond to such a survey, the integration of foreigners in this sample can be interpreted as a best-case scenario for foreign workers in large Japanese firms.

I asked each participating firm to select two or more teams with at least one foreign worker and to send an electronic survey to all members of these teams. The twelve participating firms distributed the survey to 909 employees, for a return of 539 valid responses, and a response rate of 59%.^{15 16} This response rate is appreciably larger than the mean response rate of 52.7% in organizational surveys (Baruch and Holtom 2008). The electronic survey was available in Japanese, Mandarin Chinese, and English.

For the analyses, I excluded thirteen workers with missing data on income and/or sex and two workers from Latin America, yielding a final analytical sample of 524 workers: 427 Japanese workers and 97 foreign workers. 55 of the foreign workers are from other East Asian countries, eleven are from Southeast Asia, and 29 are from Europe, North America, and Oceania. Two workers are from India and Uzbekistan, and I include them in the Southeast Asia group.

The survey instrument collected detailed information about respondents' language ability in Japanese, English, Mandarin Chinese, Korean, and up to two other languages specified by the respondent. Respondents were asked to list where they were born and, if they were born abroad, how old they were when they first lived in Japan and how many years they lived in Japan in total. I calculated total years abroad by subtracting years in Japan from respondents' current ages. Respondents born in Japan were asked if they had ever lived outside Japan, and if so, in what countries, and for how many years. These respondents' years in Japan are calculated by subtracting years abroad from age.

Respondents indicated what levels of education they had completed (high school, junior college, university, master's, MBA, and other advanced degree), and, for each level,

¹⁵ Response rates varied considerable between firms, with a response rate of 100% at 5 firms, and to a low of 34% at one firm.

¹⁶ One firm declined to specify how many workers received the survey. To calculate the total response rate, I therefore assumed that the response rate at the firm with missing data was equal to the mean response rate of all firms (78.6%). Because I received 50 responses from this firm, the estimated number of survey recipients is 64. This estimate is included in the response rate denominator of 909 employees.

whether they were educated in Japan, another country, or both. I assigned years of education for each level and summed them to estimate total years of education, ranging from four (for high school grads) to fourteen (for Ph.D.s). I calculate total work experience by adding fourteen (the age at which students begin post-primary education) to years of post-primary education, and subtracting this from respondents' current ages.

I estimate years of post-primary education in Japan/abroad as follows: If respondents completed a level of education in Japan alone, I assign all years for that level to Japanese education. If a respondent completed a level of education in a foreign country alone, I code all those years as foreign education. If the respondent selected that they completed a level of education in both Japan and a foreign country, I split the years evenly between foreign and Japanese education. When estimated years of foreign education exceed the total number of years the respondent reports spending abroad, I replace years of foreign education with the total number of years abroad, and assign the remaining years of education as Japanese education.

The estimate of years of work experience follows a similar logic. For native-born respondents whose entire time abroad is accounted for by years of education, I assume that all work experience is in Japan, and code foreign work experience as 0. For native-born respondents whose time abroad is not accounted for by years of education, I base the assignment of additional years abroad on where they completed high school. If native-born respondents completed high school in Japan or in Japan and abroad, I assume that all remaining years abroad are working years (up to respondents' total working years). For respondents who completed high school abroad, I assign half of remaining years abroad to work experience years (up to respondents' total working years).

For foreign-born respondents, I assign all working years as Japan years if respondents report that they came to Japan to live before completing their educations. If years in Japan are

lower than total years of work experience, I replace work years with the number of years in Japan not already accounted for by education and assign other work years as foreign work. If foreign-born respondents report coming to Japan after completing their educations, I assign all years after age of arrival as years of work in Japan up to total number of years in Japan. Descriptive data on the sample by national origin appears in Table 2.2.

As a tool for understanding stratification between and among migrants and natives in Japan, this dataset has both advantages and disadvantages. One advantage is the relatively precise measurement of host and home country education and work experience described above, which is an improvement over existing studies (Skuterud and Su 2011; Kanas and van Tubergen 2009). Another is that all employees work in the same sections of the same firms. In the Japanese context, workplace cohesion and community are highly valued (Rohlen 1974), job mobility is low (Ono 2010; Holbrow 2015), and scrutiny of job applicants is intense: even entry-level job applicants to large firms typically write essays, take written aptitude tests, and participate in two to four in-person interviews (DODA 2016; Mai Nabi 2016). This intense scrutiny minimizes unobserved differences between native workers and migrants (for example in ability or motivation), and between migrants of different backgrounds. Unobserved differences by national background in selection into immigration are therefore unlikely to have a large impact on the results.

A disadvantage is that the estimated group differences are by virtue of the study design only those that emerge within firms and positions. Social processes that contribute to sorting of different groups into different firms or labor market sectors are by definition unobserved. Because the current study covers only a relatively small number of elite firms, it cannot identify how acculturation, human capital quality, or context of reception affect how immigrant and Japanese workers' sort themselves into different types of firms.

Table 2.2: Descriptive Statistics

	Japanese (N=427)		Other East Asians (N=55)		Southeast Asians (N=13)		Westerners (N=29)	
	% or Mean	SD	% or Mean	SD	% or Mean	SD	% or Mean	SD
<i>General</i>								
Male	70.3		36.4		61.5		79.3	
Years education (post-primary)	8.1	1.4	9.2	1.5	8.5	0.9	9.4	2.3
Age	39.9	9.0	31.8	6.3	30.0	5.3	37.4	8.8
Tenure	11.5	9.5	3.9	4.5	4.3	4.8	5.7	7.8
Weekly work hours	42.4	8.3	47.1	8.1	45.8	8.8	47.3	9.6
Clerical job	11.2		12.7		7.7		3.5	
Job class								
Management track	74.5		69.1		100.0		79.3	
Contract job	15.7		12.7		0.0		17.2	
General track	9.8		18.2		0.0		3.5	
Number of subordinates	3.6	6.4	0.7	2.3	1.2	1.7	4.8	8.7
<i>Host Country Human Capital</i>								
Advanced Japanese language	99.3		81.8		76.9		72.4	
Years in Japan	38.6	9.4	8.1	7.2	6.9	4.0	16.2	12.3
Years in Japanese education (post-primary)	8.0	1.5	3.9	2.3	3.8	1.0	3.9	3.0
Years work in Japan	16.8	9.4	7.5	6.3	5.2	3.3	10.1	8.5
<i>Home Country/Global Human Capital</i>								
Advanced English language	27.6		52.7		100.0		96.6	
Advanced Chinese/Korean language	3.3		94.6		23.1		10.3	
Years abroad	1.3	3.0	23.7	5.4	23.1	7.0	21.2	12.5
Years in foreign education (post-primary)	0.2	0.7	5.3	2.7	4.6	1.5	5.5	4.5
Years work abroad	0.9	2.1	1.1	2.1	2.4	5.8	3.9	5.3
<i>Outcome variables</i>								
Annual income (1000s of yen)	7,939.6	4,241.2	4,829.9	2,385.6	5,393.5	3,714.5	9,707.2	5,500.5

Empirical Strategy

To examine the differences in economic outcomes for members of these immigrant groups, I use a series of hierarchical linear models with random effects for teams and firms. The outcome variable of these models is annual income, including base salary, bonus, and allowances. The focus of the models is whether and when, net of controls, we observe wage inequality between Japanese employees and the three immigrant groups under study. Annual income is measured in thousands of Japanese yen. The dollar conversion rate for yen is approximately 100 yen per dollar, so coefficients are thus easily interpretable in dollar terms by multiplying them by ten.¹⁷

I apply a series of hierarchical linear models (HLM) to these data, where individuals are nested within firms. These models produce unbiased beta estimates when standard errors are non-randomly clustered (Heck and Thomas 2000) and allow me to compare foreign and Japanese employees from the same teams in the same firms. The first HLM models estimate earnings for Japanese employees and members of the three immigrant groups as a function of standard factors in wage-determination models: sex, years of education, age, age squared, tenure, tenure squared and work hours (Model 1). The next set of models add job-level covariates (job class, job content, and authority), given migrant disadvantage could occur within jobs, between jobs, or both (Model 2). These baseline models show the overall pattern of stratification within the firms. Subsequent models use the same baseline controls as Model 1.

Model 3 adds covariates relevant to the human capital quality hypothesis. If educational quality is a significant predictor of income, we would expect, first, that holding total years of education constant, years of foreign education would have a null effect on

¹⁷ I do not log transform income because the income distribution does not have a long right tail. This is a consequence of Japanese firms' relatively flat compensation structures, and of the fact that the sample of firms includes only large, elite employers.

earnings. This is because foreign education is of varying quality, some better or equal to Japanese education, and some worse. Model 4 adds an interaction between years of foreign education and national background.¹⁸ If Western education and Japanese education are of equal quality (and equally valued by employers), we would expect insignificant interaction coefficients for Westerners, whose foreign education is also from the West, and negative interaction coefficients for the East and Southeast Asians, whose education is assumed to be of lower quality. On the other hand, if Western education is better than Japanese education, we would expect positive coefficients for the effect of foreign education for Japanese (i.e. the main effect of foreign education), and a positive interaction term for Westerners with foreign education.

The next set of models tests the role of acculturation or host country specific human capital. Variables that measure acculturation directly include total years in Japan, and Japanese language skills. If acculturation improves outcomes for immigrants, we expect a positive relationship between these variables and immigrants' relative earnings. Model 5 includes the main effects for these variables. The main effect for years in Japan is not necessarily meaningful on its own, because most of the sample is Japanese. 69% of the Japanese sample have never spent any time abroad, and 92% have spent five years or less abroad. Thus there is a strong correlation between age and years in Japan for Japanese respondents, and the variable cannot be said to represent acculturation among Japanese respondents. However, the main effect of Japanese language skills (which is measured as binary variable: advanced or not advanced), should be meaningful even if all the people who do not speak advanced Japanese are foreigners. Model 6 retains the language variable and

¹⁸ Among the 27 Japanese respondents in the sample who report some foreign education, two did not report the names of the countries they resided in. 21 reported foreign sojourns only in Western countries (the U.S., the UK, Australia, and Germany). Two reported stays in both a Western and a non-Western country, and two reported stays only in Asian countries (Malaysia and Singapore).

adds an interaction term between national background and years in Japan. Generally speaking, we would expect positive interactions between foreign background and years in Japan, especially for Westerners and to a lesser extent for Southeast Asians, who may initially differ culturally from Japanese people in unobserved ways more than East Asian immigrants.

The third set of models examines the importance of other human capital that immigrants have. Previous research suggests that foreign language skills, most notably English, are valuable in the Japanese labor market (Ono 2007; Takenaka, Ishida, and Nakamuro 2015). Indeed, it is possible that “global capital”—hard and soft skills, including language ability, that are useful in international business—could allow immigrants to earn even more than natives (Chiswick and Miller 2011). In Model 7, I test how these processes contribute to ethnic stratification by adding a variable for years of foreign work experience (foreign educational experience is already tested in Models 3 and 4). In Model 8, I add variables for English and East Asian language skills (Mandarin Chinese and other Chinese dialects, Korean), because these are the languages we would expect to be most valuable for Japan’s global trade. Because of collinearity between ethnicity and language skills (i.e. there are no East Asian immigrants who do not speak an East Asian language, nearly all fluent speakers are East Asian immigrants), I create binary categories for advanced skills in each of these language categories. There are Japanese people in the sample who speak advanced Chinese, Korean, and English, so this ensures that the language skills variables are not simply a proxy for ethnicity. If global capital matters for immigrants’ economic assimilation, we should expect a positive relationship between foreign work experience and earnings, and between foreign language skills and earnings.

Finally, in Models 9, 10, and 11, I combine the significant effects from earlier models to determine if the overall trends in stratification hold when we adjust for all the known

processes affecting immigrants' economic assimilation. Model 9 uses all significant measures from earlier models, and Models 10 and 11 include these variables plus a decomposition of time in Japan into years in education (Model 10) and years of work experience (Model 11). A summary of the predicted relationships between the variables of interest and immigrants' earnings appears in Table 2.3.

Table 2.3: Expected Signs of Human Capital Coefficients in Earnings Regression by Theoretical Perspective

	Educational Quality	Acculturation	Global Human Capital
<i>Linear Predictions</i>			
Japanese language ability		+	
Years in Japanese education		+	
Years work in Japan		+	
English language ability			+
East Asian language ability			+
Years in foreign education	0		+
Years work abroad			+
<i>Interaction Predictions</i>			
Years in Japan * EA		+	
Years in Japan * SEA		++	
Years in Japan * W		+++	
Years in foreign education * N	+ / 0		
Years in foreign education * EA	-		
Years in foreign education * SEA	-		
Years in foreign education * W	+ / 0		

N = Natives from the ethnic majority, EA = East Asian foreign-born, SEA = Southeast Asian foreign-born, W = White Western foreign-born.

None of the models directly test for the effects of prejudice on immigrant assimilation. However, after we adjust for known processes that influence immigrant integration, we can observe what stratification pattern, if any, emerges. Although in Western contexts, the stratification pattern would be difficult to interpret because even after adjustments, the same groups would tend to be advantaged or disadvantaged in unmeasured ways in terms of acculturation, human capital quality, and context of reception, the same is not true in this context, where we can examine which pattern the stratification order best matches.

Results

Table 2.4: Baseline Models: Regression of Human Capital and Job Characteristics on Annual Earnings

	Model 1		Model 2	
	Beta	SE	Beta	SE
<i>Individual level variables</i>				
Male	1,761.05***	281.71	1,267.68***	272.14
Years of education (HS and beyond)	293.11**	92.35	133.14	89.05
Age	165.06	130.47	252.43*	122.90
Age squared	1.05	1.61	-.28	1.52
Tenure	274.90***	55.94	185.31***	53.53
Tenure squared	-9.06***	1.68	-7.01***	1.59
Weekly work hours	42.13**	16.18	21.34	15.38
<i>Job class^a</i>				
Contract job			-599.09+	350.27
General track			-1,695.26***	425.66
Clerical job ^b			-1,089.51*	450.63
Number of subordinates			137.67***	21.81
<i>National background^c</i>				
East Asian	-202.16	456.19	-23.85	429.95
Southeast Asian	560.06	823.68	241.54	770.73
Westerner	1,502.31**	566.39	1,246.91*	532.79
Constant	-7,422.47**	2,652.75	-5,667.77*	2,510.38
<i>Model information</i>				
Observations	524		524	
Number of firms	12		12	
Individual-level variance component	7,435,593		6,428,418	
Team-level variance component	397,756		498,639	
Firm-level variance component	2,826,723		2,835,005	

Results are from ANCOVA with random effects models (HLM). All slopes are fixed.

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1. ^a Reference category is management track ^b Reference category is all other types of jobs ^c Reference category is Japanese

Models 1 and 2 in Table 2.4 estimate the differences in earnings between ethnic national origin groups, both between jobs (Model 1), and within them (Model 2).

There is no evidence that East or Southeast Asian foreigners have different earnings than Japanese workers net of baseline adjustment variables. Westerners, however, earn significantly more than Japanese employees: an estimated \$15,000 more annually between jobs, and \$12,000 within jobs. Because job class, job content, and level of authority explain relatively little of these wage differences, this suggests that ethnic wage gaps are primarily

driven by factors other than job sorting. Based on this, I do not use job-level variables in subsequent models. However, results are not substantively different in subsequent models if I include these variables.

Table 2.5: Human Capital Quality Models: Regression of Place of Education on Annual Earnings

	Model 3		Model 4	
	Beta	SE	Beta	SE
<i>Individual level variables</i>				
National background ^a				
East Asian	-514.44	617.41	-537.06	865.59
Southeast Asian	264.07	912.56	-66.10	2,610.31
Westerner	1,184.75+	705.82	494.86	857.68
Years in foreign education	65.29	87.65	-283.33	206.70
Years in foreign education * East Asian			343.49	248.46
Years in foreign education * S. East Asian			406.58	578.58
Years in foreign education * Westerner			467.03*	235.15
Constant	-7,190.25**	2,669.66	-7,105.49**	2,661.56
<i>Model information</i>				
Observations	524		524	
Number of firms	12		12	
Basic human capital controls	Yes		Yes	
Job characteristic controls	No		No	
Individual-level variance component	7,421,109		7,389,636	
Team-level variance component	411,836		351,720	
Firm-level variance component	2,815,388		2,878,079	

Results are from ANCOVA with random effects models (HLM). All slopes are fixed.
 *** p<0.001, ** p<0.01, * p<0.05, + p<0.1. ^a Reference category is Japanese

Model 3 in Table 2.5 shows that there is no significant relationship between years of foreign education after junior high/middle school (adjusting for total years of education) and earnings. Model 4 tests whether education acquired abroad is more or less valuable depending on its origin. As indicated by the negative and non-significant coefficient for Japanese, there is no evidence that Western education produces higher returns than Japanese education for Japanese natives. Interaction coefficients are positive and significant for Westerners, and positive and of similar magnitude (but insignificant) for other groups. This pattern does not support the human capital quality hypothesis, which predicts lower incomes

for East and Southeast Asians educated in their countries of origin, and equal or higher incomes for both Japanese and Westerners educated outside Japan.

Models 5 and 6 in Table 2.6 examine whether differences in acculturation, as measured by Japanese language skills and years in Japan, can account for ethnic differences in wages. Surprisingly, the data show no indication that acculturation helps foreigners' income. Advanced speakers of Japanese earn no more than those with poorer language ability. Moreover, we see from the interaction terms between national background and years in Japan in Model 6 that foreigners do not earn more, relative to Japanese, as they accumulate experience in Japan. The interaction effects for Westerners and Southeast Asians are small and insignificant, suggesting no earnings premium for acculturation. Further, the interaction coefficient for East Asians is negative and significant. The negative main effects for years in Japan in Table 2.6 indicate that foreign experience is valuable to Japanese respondents. Either foreign experience is even more valuable to East Asians than to Japanese, or there is a cumulative disadvantage for East Asians who spend more years in Japan.

Table 2.6: Acculturation Models: Regression of Japan-Related Experience on Annual Earnings

<i>Individual level variables</i>	Model 5		Model 6	
	Beta	SE	Beta	SE
National background ^a				
East Asian	-2,126.06**	779.60	-1,117.01	1,140.21
Southeast Asian	-1,340.23	1,025.70	-2,430.38	1,952.12
Westerner	-119.57	770.17	-1,392.07	1,429.42
Years in Japan	-85.79**	29.92	-93.89**	35.24
Years in Japan * East Asian			-141.39*	58.28
Years in Japan * S. East Asian			138.91	204.64
Years in Japan * Westerner			69.19	50.20
Advanced Japanese language	37.14	687.26	58.48	696.27
Constant	-6,400.45*	2,723.00	-6,996.54*	2,762.12
<i>Model information</i>				
Observations	524		524	
Number of firms	12		12	
Basic human capital controls	Yes		Yes	
Job characteristic controls	No		No	
Individual-level variance component	7,270,557		7,098,328	
Team-level variance component	479,558		534,441	
Firm-level variance component	2,688,455		2,776,557	

Results are from ANCOVA with random effects models (HLM). All slopes are fixed.

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1. ^a Reference category is Japanese

Table 2.7: Global Human Capital Models: Regression of Global Work Experience and Foreign Language Skills on Annual Earnings

<i>Individual level variables</i>	Model 7		Model 8	
	Beta	SE	Beta	SE
National background ^a				
East Asian	-303.51	454.72	-1,599.60*	711.41
Southeast Asian	287.43	824.48	-563.92	845.10
Westerner	1,127.40 ⁺	579.80	608.16	590.31
Years work abroad	131.98**	49.88	75.95	52.11
Advanced English language			930.63**	307.12
Advanced Asian language			1,210.67 ⁺	618.41
Constant	-6,692.84*	2,648.88	-7,000.70**	2,615.10
<i>Model information</i>				
Observations	524		524	
Number of firms	12		12	
Basic human capital controls	Yes		Yes	
Job characteristic controls	No		No	
Individual-level variance component	7,330,559		7,110,266	
Team-level variance component	427,411		563,005	
Firm-level variance component	2,622,756		2,185,313	

Results are from ANCOVA with random effects models (HLM). All slopes are fixed.

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1. ^a Reference category is Japanese

The next models investigate the associations between global human capital and earnings: specifically, the ability to speak languages that are useful to Japanese businesses, and international work experience. Model 7 of Table 2.7 estimates a significant, positive relationship between years of foreign work experience and wages. (This is unsurprising given the negative main effects for years in Japan in Models 5 and 6.) Model 8 adds variables for skills in East Asian languages other than Japanese, and English. With the inclusion of these variables, foreign work experience is no longer significant. I cannot ascertain whether this null relationship is because the advantage of foreign experience comes primarily from the language skills acquired during foreign experience, or simply because foreign language skills and foreign experience are both indicators of the same underlying ability to do business in a global environment. In either case, however, this finding is consistent with the hypothesis that global capital helps foreign workers achieve economic success in Japan (Takenaka, Ishida, and Nakamuro 2015). Adding the covariate for English language ability causes the magnitude of the positive effect of Western background to be reduced by about half and to lose statistical significance. At least some of the earnings advantage of Westerners is hence a result of their superior English language abilities.

Models 9, 10, and 11 appear in Table 2.8 and combine the effects we have found significant in previous models—foreign (but not Japanese) language skills, years in Japan, foreign work experience, and the interaction between national background and years in Japan. Because I decompose years in Japan into years of work experience and years of Japanese education, and include age and total years of education as controls, I model foreign work experience as categorical rather than continuous to reduce collinearity. In Model 9, adding the categorical variable for foreign experience reduces the size and significance level of the main effect of years in Japan. This is consistent with the

interpretation that Japanese employees benefit economically from working in foreign countries. However, foreign experience, by contrast, does not appreciably change the coefficient of the interaction of years in Japan and East Asian background.

Models 10 and 11 break down the effect of years in Japan into years in education (Model 10), and years of work experience (Model 11), holding foreign language ability and foreign work experience constant. Years spent in Japanese education have no negative effect on East Asians' earnings (Model 10), but years of work experience do (Model 11). For Westerners, years in Japanese education have a negative effect (Model 10). This is parallel to the positive effect for foreign education for Westerners apparent in Model 4. Although total years in Japan and years of education in Japan do not appear to benefit Westerners, in Model 11, years of work in Japan have a positive and significant effect on Westerners' earnings. In other words, East Asians who accumulate more work experience in Japan earn less than similar Japanese, while Westerners earn more, net of both groups' advantages in foreign work experience and foreign language skills.

To illustrate the implications of these findings for mid-career workers, Figure 2.3 displays predicted annual incomes for mid-career workers, based on Model 11.¹⁹ The solid bars represent predicted values for workers with only domestic, Japanese experience, and the dotted bars represent workers with four to six years international experience. All predictions are for male workers, aged 45, with eight years of post-primary education, working 43 hours per week. All workers are assumed to speak a non-Japanese language. For Japanese and Westerners, this language is set to English, and for East Asian foreigners it is set to Chinese/Korean.

¹⁹ Similar point estimates for younger workers do not show any significant group differences.

Table 2.8: Full Models: Regression of Foreign and Japanese Experience on Annual Earnings

<i>Individual level variables</i>	Model 9		Model 10		Model 11	
	Beta	SE	Beta	SE	Beta	SE
National background ^a						
East Asian	-854.79	1,313.03	-924.99	1,592.15	91.22	880.24
Southeast Asian	-1,964.09	1,964.91	206.06	3,534.39	-833.79	1,535.37
Westerner	-74.44	1,510.98	2,977.72 ⁺	1,742.09	-858.25	885.50
Advanced East Asian language	993.28	612.96	903.85	620.88	835.29	615.74
Advanced English language	792.46 [*]	311.98	939.37 ^{**}	308.95	829.39 ^{**}	306.27
Years in Japan	-35.79	41.55				
Years in Japan * East Asian	-144.80 [*]	57.36				
Years in Japan * S. East Asian	116.17	199.42				
Years in Japan * Westerner	21.60	51.68				
Foreign work experience ^b						
1-3 years	-646.53 ⁺	364.42	-469.58	361.90	-301.27	413.13
4-6 years	725.91	539.85	1,016.75 [*]	510.02	1,338.99 ⁺	751.63
7 or more years	557.76	679.98	1,219.57 [*]	599.48	1,833.29	1,202.45
Years Japanese education			66.62	152.66		
Years Japanese education * East Asian			-9.46	224.37		
Years Japanese education * S. East Asian			-137.86	831.34		
Years Japanese education * Westerner			-483.38 ⁺	257.39		
Years work in Japan					67.87	113.97
Years work in Japan * East Asian					-169.79 ^{**}	63.52
Years work in Japan * S. East Asian					82.45	238.41
Years work in Japan * Westerner					169.87 ^{**}	63.08
Constant	-6,630.02 [*]	2,639.07	-6,037.10 [*]	2,643.52	-5,684.16 ⁺	3,063.21

Table continues on next page.

Table 2.8, Cont.

	Model 9	Model 10	Model 11
<i>Model information</i>			
Observations	524	524	524
Number of firms	12	12	12
Basic human capital controls	Yes	Yes	Yes
Job characteristic controls	No	No	No
Individual-level variance component	6,865,549	6,940,715	6,741,773
Team-level variance component	559,137	533,847	625,919
Firm-level variance component	2,453,802	2,222,958	2,451,689

Results are from ANCOVA with random effects models (HLM). All slopes are fixed.

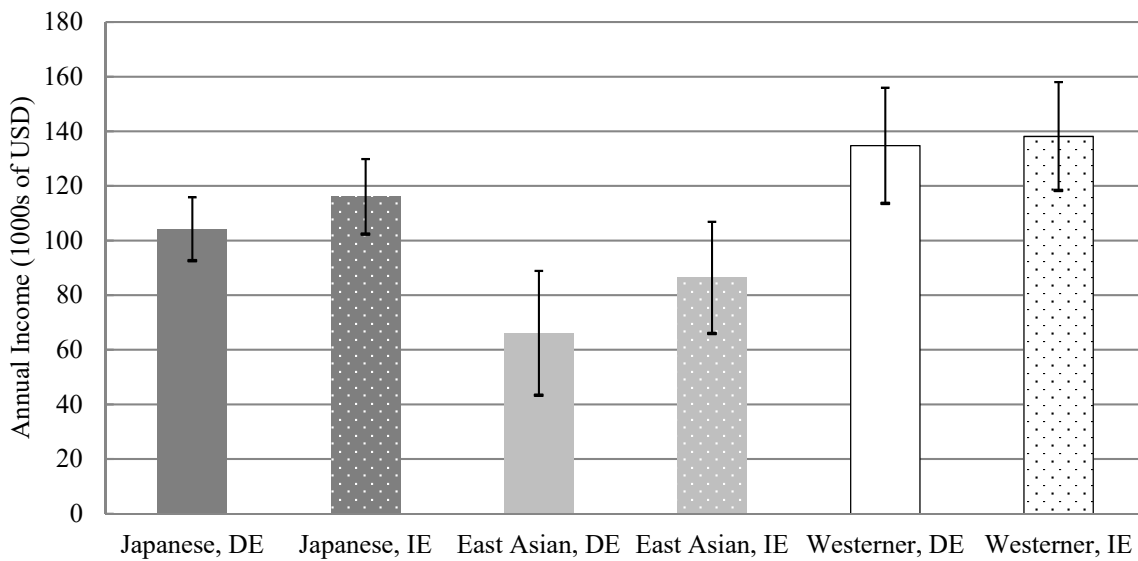
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$

^a Reference category is Japanese

^b Reference category is none

For employees with only domestic, Japanese experience (labeled DE), their tenure is set to 20 years and their years of Japanese work experience is set to 23, their whole working lives. For employees with mixed experience (labeled IE), Japanese experience is set to 18 and foreign experience is set to four to six years. Their tenures are set to 15. The error bars show the 95% confidence intervals for the estimates.

Figure 2.3: Predicted Annual Earnings by Region of Origin and International Experience



DE = domestic experience only, IE = 4-6 years experience outside Japan

Because the point estimates are based on all model parameters, each with its own uncertainty, confidence intervals for each group are wide. Nonetheless, the results show that East Asian immigrants without international experience have lower predicted incomes than Japanese and Western workers with or without international experience. In contrast, Western workers earn more than Japanese workers with only domestic experience, regardless of Westerners' level of international experience. The estimated differences between a Westerner with only domestic

experience, and a Japanese with only domestic experience is about \$30,000, a substantively very large gap. In contrast, for an East Asian with only domestic job experience, the predicted wage disadvantage compared to a similar Japanese worker is \$38,000.

Discussion

The above analyses have two main goals. The first goal is to understand the stratification order in Japanese firms; the second is to illuminate potential mechanisms that undergird this order. The stratification order is complicated: East Asian workers earn about the same as Japanese, but as workers accumulate experience in Japan, wages for East Asian immigrants diverge from those of their Japanese counterparts and fall below the compensation for similar Japanese. Westerners, on the other hand, earn more than East Asians, and more even than Japanese. Among younger workers, better English ability appears to account for Westerners' advantage, but for workers with more work experience in Japan, wages exceed even those of Japanese employees with advanced English ability.²⁰

The models are inconclusive as to the place of Southeast Asian immigrants in the stratification order. Southeast Asians make up the smallest respondent group, and as such their

²⁰ Another possible explanation for the positive interaction between Western ethnicity and years work in Japan and the negative interaction between East Asian ethnicity and years work in Japan is that, even though we are comparing very similar workers who have all been hired by the same firms, selection mechanisms are different, with low-earning Westerners and high-earning East Asians more likely to leave Japan. However, I found no evidence of this. I asked respondents what their intentions for the future are. They could select "settle in Japan," "maintain residences in Japan and another country" "return to a country where you lived previously" "move to a country where you have never lived before" and "don't know." Earning more than expected (i.e. having a positive residual in Model 2) is positively and significantly associated with "settle in Japan" for East Asian foreigners but not for Westerners, indicating positive selection for East Asians. Similarly, East Asian foreigners but not Western foreigners, are less likely to select an option indicating they will leave Japan if their income residuals are high. If anything, therefore, the analyses underestimate the ethnic penalty East Asians face, because high earning East Asians are the most likely to stay and thus be captured as older, more experienced workers in this cross-sectional sample.

earnings are estimated imprecisely. These data offer no indication that their earnings differ from those of Japanese employees.

Turning to the posited mechanisms themselves, I find no support for the acculturation hypothesis. Neither Japanese language ability nor years in Japan are positively associated with earnings among foreign workers. Similarly, education in Japan is not more valuable than education earned abroad. The caveat, however, is that Westerners (and Westerners alone) see an income premium for work experience in Japan. As shown in Table 2.3, it is possible that years in Japan would be more valuable to Western respondents than to other immigrants, because Westerners are initially disadvantaged along unmeasured aspects of acculturation. However, if Westerners' cumulative advantage to work experience represents gains made through acculturation, we would not expect the wages of midcareer Westerners to surpass those of Japanese workers, as we see in Figure 2.3. Further, if acculturation really helped Westerners do better, we would expect that years of education in Japan would also contribute to higher earnings. They do not. Because only longer Japanese work experience, rather than Japanese schooling and language skills, contributes to higher earnings, this suggests that it is positive and subjective value placed on Western culture and people (Owens forthcoming; Befu 2001) that generates this pattern. Positive biases towards Westerners may produce small positive differences in rewards, which accumulate over the course of workers' careers until their wages are much higher than those of their Japanese counterparts.

There is also no evidence that "educational quality" improves economic outcomes for workers in Japanese firms. As we see in Model 4, the positive effect of foreign education is only significant for Westerners, as is its complement in Model 10, a negative effect for Japanese education. If Western education were truly superior to Japanese education, we would expect

Japanese people who study in Western countries to benefit as well. That we do not suggest that the advantages Westerners accrue for earning their educations in the West may also be an effect of the positive value the Japanese place on Western culture and people.

Like previous research (Ono 2007; Takenaka, Ishida, and Nakamuro 2015), I find that English ability is a strong predictor of earnings. This paper also shows that ability in East Asian language is valuable as well, although the findings are less robust than for English because they are not significant in all models.

Conclusion

In Western contexts, it can be difficult to interpret gaps in the earnings of different immigrant groups, because advantaged immigrants tend to be favored in multiple ways. For example, in the United States, the most successful immigrants generally enjoy a racial advantage as whites, come from countries with high quality education systems, and arrive more familiar with U.S. norms, customs, and language. This makes it easy to misestimate every type of integration effect: measures of race may actually capture the effects of education systems; measures of educational quality may capture the effects of acculturation, and so on. Further, even after adjustments for obvious measures like English language ability, these multilayered advantages make it difficult to interpret residual gaps for particular groups of immigrants, since unmeasured differences in any of the integration processes could explain the gaps for each group.

This paper has examined income inequality in the context of large Japanese firms. In this context, immigrant groups are not, for the most part, advantaged on more than one axis of integration. It has also controlled for differences in immigrant selection from different countries

by comparing immigrants and natives who have gone through the intensive screening process used by elite Japanese employers.

The empirical findings do not support hypotheses that acculturation and human capital quality improve immigrants' economic outcomes. Variables that capture acculturation and human capital quality do not consistently predict higher earnings for immigrants. Moreover, the residual stratification order after fine-grained adjustments for human capital, with Westerners on top, followed by Japanese, followed by other East Asians suggests context of reception as the mechanism most likely to generate this outcome.

What aspects of these findings can be attributed to the research setting in elite firms, or to the Japanese context more generally, and how might they give insight into immigrant integration elsewhere?

It would be hasty to conclude from the results that acculturation never matters for immigrants' success in Japan, much less in other contexts. Other studies in Japan using more heterogeneous samples of immigrants (Takenoshita 2006; Holbrow and Nagayoshi 2016) do suggest that acculturation, as measured by language skills and time in Japan, is associated with higher earnings for East Asian immigrants. The null finding in this study thus may be partially attributable to the selectivity of the sample. For respondents who are able to get jobs at elite firms, acculturation does not seem to matter, but more acculturated immigrants may be better able to obtain such jobs in the first place. Research from Canada and the United States suggests that much of immigrants' positive earnings growth over time is due to mobility between establishments and jobs (Ayedemir and Skuterud 2008). If the same is true in Japan, a sample drawn from elite firms will miss some of the potential positive effects of acculturation.

At the same time, the results clearly indicate that lack of acculturation is not all that is holding East Asian immigrants to Japan back, and cast doubt on the master narrative in both Japan and the West that unexplained differences in immigrant and native earnings are predominately a problem of acculturation. Indeed, the results of the current study are similar to findings in the U.S. that foreign-born employees have lower starting salaries, are less likely to receive salary increases, and when they do receive increases, receive smaller increases than native-born workers (Castilla 2008). As a result, within firm inequality between native-born whites and the foreign-born grows over time. Unlike this current study, Castilla has relatively little demographic information, and cannot identify foreign-born workers' countries of origin, places of education, or length of U.S. experience. However, that he found results similar to those in the current study after adjusting for performance scores lends weight to the hypothesis that much U.S. and European research may indeed overestimate the benefits of acculturation for immigrants.

The findings of this study cast even more doubt on research on human capital quality. To my knowledge, no existing studies of human capital quality rigorously account for the effects of race, and although a few include standard measures of acculturation such as language skills, they ignore correspondence between country of origin and unmeasured aspects of acculturation. They also ignore differences in immigrant selectivity by sending country and by immigrant generation by uniformly assigning average sending country characteristics to individual immigrants. In the current study, I reduce concerns about differences in selectivity by comparing the most similar immigrants—those who work at the same firms. In this context, there is no discernable effect of human capital quality. Of course, this null result could be because the benefits of high quality human capital emerge primarily when immigrants sort into places of employment, and the

employees at these elite firms all have high quality human capital, whether they were educated in their home countries or in Japan. However, there is evidence to suggest this is not the case.

Oreopoulos (2011) finds that attending a higher ranked foreign school has no effect on callback rates for foreign-named applicants to Canadian white-collar jobs. This suggests that employers are not necessarily capable of evaluating the quality of human capital obtained in foreign countries. Future investigations of human capital quality need to improve measurement of schooling quality at the individual level and control more effectively for confounding factors to make a convincing case that effects from previous studies are not spurious.

The effect of “global capital” has been found in other studies of immigrants to Japan (Ono 2007; Takenaka 2014). This makes it unlikely that the value of English and other East Asian languages is specific to the context of these elite firms. It also suggests that this may be neglected factor in other national environments. Some of the effects of English language proficiency in English-speaking countries no doubt reflect its status as the language of international business. Similarly, in non-English speaking countries, English proficiency may be as valuable to immigrants as host country language skills. Other global languages such as French, Spanish, and Mandarin Chinese may also have value, although the most disadvantaged immigrants with low levels of education are less likely to benefit from these skills than immigrants with college degrees. Although earlier research on bilingualism in the U.S. was inconclusive or suggested a labor market penalty for bilingualism (see Gándara 2015 for a review), more recent research using richer covariates suggests that there is an economic benefit (Agirdag 2014; Rumbaut 2014). If the Japanese case is a guide, this type of global capital can override some of the economic disadvantages immigrants face. Conversely, omitting measures of

bi- or even trilingualism from multivariate models can disguise true economic disadvantage among immigrants.

This finding echoes the “overachievement” hypothesis of Hirschman and Wong (1984), who argue that Asian immigrants to the United States earn more than native whites because of greater investments in human capital. While Hirschman and Wong suggest that this is because of immigrants’ dedication to self-improvement, and attempts to mitigate the effects of discrimination, the Japanese case suggests that the “overachievement” of immigrants is at least in part due to retention of their native language, an inadvertent byproduct of the immigrant life course rather than a deliberate investment.

Finally, the current study underscores the need for further research into how the context of reception (Portes and Rumbaut 2001, 2006) shapes immigrant earnings in both Japan and the West. I argue that East Asian immigrants are at a greater disadvantage (and Westerners at a greater advantage) the longer they work in Japan because of the cumulative effects of attitudes towards these two groups. Context of reception is by definition a feature of the local and national environment of the immigrant receiving country, and consequently, these specific results are not generalizable to other countries. They do, however, bolster theoretical claims that context of reception is key to understanding immigrant outcomes (Portes and Rumbaut 2001; 2006 Alba and Nee 2003), and point to a need for further studies that empirically measure these theoretically important processes (see Lewin-Epstein et al. 2003; Kogan 2006; Holbrow and Nagayoshi 2016 for existing examples), particularly the effects of prejudice and discrimination.

There has been some debate about whether immigrants to Japan experience positive assimilation (Holbrow and Nagayoshi 2016), meaning that although they may earn less than their Japanese counterparts upon arrival, the earnings gap shrinks with time in Japan, or negative

assimilation (Takenaka, Ishida, and Nakamuro 2015), meaning that immigrants initially earn more than native workers, but that foreign workers' earnings decline to match those of native workers (e.g. Chiswick and Miller 2011). This study, which is the first to investigate the relative wages of comparable Japanese and foreign workers, indicates that economic "assimilation," whether positive or negative, does not adequately capture immigrants' earnings trajectories in Japan. In elite firms, neither East Asian nor Western immigrants' earnings converge with comparable Japanese, and ethnic wage gaps appear to be the smallest at the beginning of workers' careers, when foreigners' time in Japan is by definition the shortest. Owing to lack of data, it is not possible to say with certainty whether these results hold for self-employed immigrants or immigrants in other types of firms. However, because the firms in this sample are all committed to diversity and were willing to give me access to their employees, have dedicated staff human resource staff who manage diversity issues (see Kalev, Kelly, and Dobbin 2006), and hire and promote relatively large numbers of foreign workers, it seems unlikely that conditions for foreign workers are worse in this study setting than in other Japanese employment contexts.

What are the implications of these results for evaluating Japanese immigration policy? First, they indicate that assumptions about the relationship between human capital and immigrant economic assimilation are broadly correct. Even within the highly selected sample of immigrants I study here, human capital matters to outcomes. But it is not necessarily the human capital that immigration policy privileges that best facilitates immigrants' economic assimilation. The new point system and HSP visa prioritize host country specific human capital, by awarding points for Japanese language skills and Japanese university degrees (Ministry of Justice 2013). However, at least within elite firms, these types of human capital appear to be irrelevant for immigrants'

economic outcomes. Rather, it is *foreign* language skills which, for younger East Asian workers, shrink or close the pay gap with natives, and for Westerners, strengthen their earnings advantage.

Although the results do not suggest that foreign workers as a group form a kind of underpaid professional underclass, it is clear that immigrants' high levels of human capital do not obviate all concerns about inequity. Indeed, that inequity persists even among the most successful skilled immigrants should be an issue of concern for the government. When East Asians earn less than similar coworkers, they are more likely to say they wish to leave Japan. Further, income levels are one of the most important contributors to immigrants' eligibility for the fast-track to permanent residency available through the HSP visa. East Asian immigrants may have less access to this desirable visa than Westerners not because they are less productive or integrated, but simply because their employers discriminate against them. By allowing wage inequity to persist, the government may therefore stymie its own efforts to attract and retain more skilled immigrants.

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CHAPTER 3

THE ROLE OF ETHNIC BIAS IN WAGE INEQUALITY

Introduction

The public is divided on whether bias and discrimination continue to limit economic opportunities for minorities (Pew Research Center 2016), and social science does not provide straightforward answers. In developed countries, significant racial and ethnic wage gaps remain after adjustments for human capital (for the United States, see Wilson and Rodgers 2016; Neal and Rick 2014; Kim and Sakamoto 2010; for Europe see Adsera and Chiswick 2007). But do employers' racial and ethnic biases cause these gaps, or are they attributable to pre-labor market processes and structural features of the labor market?

Data from the United States indicate that racial bias has declined, albeit at a slowing pace since the 1990s (Bobo et al. 2012). On one hand, this may indicate that bias and discrimination in the labor market exert a shrinking influence on racial and ethnic inequality. On the other hand, true levels of bias may be higher than standard surveys suggest, especially because majority group members are unwilling to admit to racist views (Feagin 1999; Kuklinski et al. 1997). Indeed, field experiments show that some employers do discriminate: North American employers are less likely to call back or offer jobs to blacks (Pager, Bonikowski, and Western 2009; Bertrand and Mullainathan 2004), Hispanics (Pager, Bonikowski, and Western 2009), and Asians (Oreopoulos 2011; Gaddis 2015), even when their qualifications are identical to those of whites; in Europe, Muslims are similarly disadvantaged (Rooth 2010; Andriessen et al. 2012).

However, skeptics are quick to point out that even if bias is stronger than standard surveys imply, institutions may effectively restrain employers from acting on it (Tetlock and

Mitchell 2009). Further, point-of-hire discrimination observed in audit studies could have a relatively minor impact on overall wage inequality, if members of disadvantaged groups avoid biased employers (Heckman 1998), or bias's effects diminish after the hiring stage (Pager and Shepherd 2008). Given these ambiguities, current research cannot answer whether labor market bias and discrimination have a large or a small effect on observed racial and ethnic inequalities in earnings (Lucas 2009).

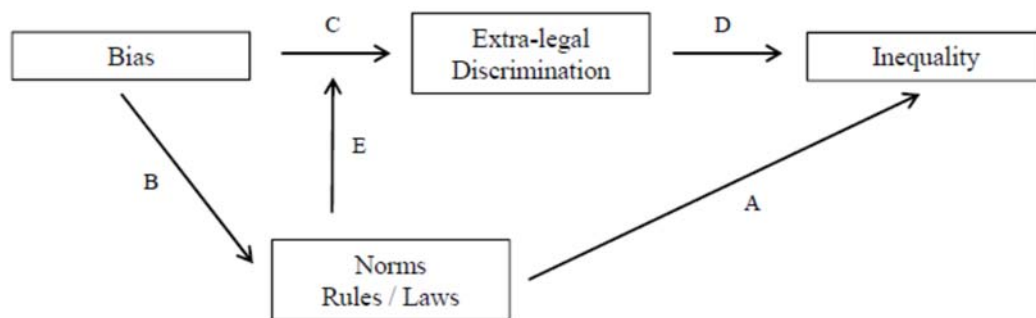
This paper takes a novel approach to investigating the economic impact of bias on inequality beyond the callback and hiring stage, in a novel setting. I use a sample of white-collar workers at twelve large, bureaucratic firms in Japan, and I employ a vignette experiment to examine whether there is bias against Asian and Western foreign workers at these firms. I then aggregate responses to the vignette experiment to identify firms with higher and lower levels of bias and examine whether wage inequality between foreign and Japanese workers is higher when levels of bias are higher. While previous studies have shown modest associations between bias and discriminatory behavior in laboratory settings (Greenwald et al. 2009; Greenwald, Banaji, and Nosek 2015) and at the callback stage in real employment settings (Rooth 2010), we know little about whether and how the effects of bias persist past the point of hire. If a more biased work environment results in more discrimination, inequality will be greater at firms with higher levels of bias. Conversely, if other situational factors intervene, including, for example, positive interpersonal relationships that develop through equal-status contact among coworkers (Pettigrew and Tropp 2006; Merton 1949), or accountability structures (Kalev, Kelly, and Dobbin 2006; Castilla 2008, 2015), levels of bias should not be associated with pay inequality. My paper is, to my knowledge, the first to examine the impact of bias on post-hire outcomes, and

the first to test whether biased labor market actors exacerbate inequality in real-life organizational settings.

Linking Bias to Discrimination and Inequality

According to Gordon Allport’s classic definition, bias is an “antipathy based on faulty or inflexible generalization” towards groups or members of a group (Allport 1954: 9). As schematized in Figure 3.1, the relationship between bias and labor market inequality is complex (Merton 1949; Bonilla-Silva 1997; Quillian 2006).

Figure 3.1: Relationship Between Bias and Inequality



As this figure shows, norms, rules, and laws have a direct effect on inequality (line A). For example, laws define who has the right to work, under what conditions, and with what access to legal protection; the different rights and privileges granted to classes of workers by law can create more economic opportunities for members of some groups than for others (e.g. citizens versus work visa holders versus undocumented immigrants), even in the absence of bias. However, norms, rules, and laws do not emerge in a vacuum (line B), and may reflect bias of

lawmakers or their constituents. Historical examples of laws that enshrine racial or ethnic bias include the Chinese Exclusion Act, which prohibited naturalization for persons of Chinese descent, and Jim Crow laws enforcing racial segregation (Alba and Nee 2003). However, even laws and rules that are not explicitly racial or ethnic in content may be motivated by bias and can have a “disparate impact” on outcomes for members of different groups (Pager and Shepherd 2008; Soss, Fording, and Schram 2011).

Bias also affects inequality more directly, mediated by extra-legal or unsanctioned discrimination (lines C and D). Extra-legal discrimination is differential treatment towards members of different groups that is not mandated by laws or rules. As the diagram shows, unless actors engage in extra-legal discrimination, bias will not affect inequality, except through the creation of biased laws and rules. At the same time, whether or not laws and rules are biased, they moderate actors’ ability and willingness to engage in extra-legal discrimination (line E). For example, fines for employers who treat employees differently by race can potentially prevent bias from leading to extra-legal discrimination and inequality (Tetlock and Mitchell 2009). Similarly, norms of equal treatment or fear of confrontation may also prevent people from discriminating even when they are unconstrained by formal laws and rules (Merton 1949; Kutner, Wilkins, and Yarrow 1952; Pager and Quillian 2005).

Scholars in sociology, political science, and psychology have all shown an abiding interest in these relationships. Recent research in sociology, for example, examines the effects of corporate policies and practices on minorities’ representation in management (Kalev, Kelly, and Dobbin 2006) and on wage inequality between minorities and whites (e.g. Bielby 2011; Castilla 2008), corresponding to line A in Figure 3.1. Scholarship on the effects of law, particularly

affirmative action policies, on minority representation in the workforce goes back much further (see Kurtulus 2014 for a review).

Work in political science and political psychology has also explored the relationships represented by line B, linking prejudice to support or opposition for progressive policies such as affirmative action (e.g. Kuklinski et al. 1997; Sears and Henry 2005; Huddy and Feldman 2009; Soss, Fording, and Schram 2011), and to support or opposition for immigration policies (Knoll 2013).

Social scientists, particularly in psychology, have also examined the link between prejudice and discrimination (line C). Over time, the techniques for measuring prejudice have evolved, and so has this strand of research. Early studies (LaPiere 1934; Kutner, Wilkins, and Yarrow 1952) sent minority auditors to service establishments to see if they would be served; they later followed up by asking whether such establishments would be willing to serve minorities. These studies found little relationship between proprietors' actual willingness to serve minorities and their stated willingness to do so. In fact, nearly all service personnel served minority customers in person, whereas majorities in both studies refused or expressed reluctance to do so in theory.

In a more recent study inspired by these classic papers (Pager 2003; Pager and Quillian 2005), a researcher sent black and white auditors to low-wage employers to apply for work, and measured how auditor race affected the likelihood of a callback. The researcher then surveyed employers to ask if they would hire someone fitting the auditor's description. As in earlier studies, the researcher found little correspondence between stated willingness to hire a minority and actual callbacks. In a reversal of these earlier studies, however, employers were much more

likely to say that they would hire a black applicant than they were to actually call black applicants back.

Other recent studies rely on contemporary psychological measurement of prejudice, particularly the IAT, to assess bias and its relationship to discrimination (line C). The IAT can be used to compare attitudes towards two classes of people, objects, or concepts (Greenwald et al. 1998). In the black-white race IAT, respondents classify positive words (e.g. “delightful,” “cherish”) and negative words (e.g. “poison,” “despise”) together with photographs of black and white faces. Bias is measured by differences in classification speed when respondents pair black faces together with positive words (and white faces together with negative words) compared to white faces together with positive words (and black faces together with negative words). These studies have been conducted almost exclusively in laboratory settings, and have used many measures of discrimination, including subtle non-verbal behaviors such as smiling or body positioning, or more overt measures such as a stated preference for a partner of a particular race in a group activity (see Oswald et al. 2013 for descriptions of typical outcomes). In contrast to field studies that compare stated behavioral intentions with actions, which have found no relationship between intention to discriminate and actual discrimination, lab studies have found small to moderate relationships between bias and discriminatory behavior (see Greenwald et al. 2009; Oswald et al. 2013 for reviews and meta-analyses). In a rare exception to this lab-based work, Rooth (2010) conducted a resume audit study in Sweden using paired resumes with randomly varied Swedish and Arabic names, and measured the callback rates by ethnicity. In a follow-up, he administered an IAT comparing attitudes towards ethnic Swedes and ethnic Arabs to the hiring managers for the positions in the audit study. Higher bias scores on the IAT were associated with a decreased likelihood of a callback for the Arabic-named applicant.

In sum, there is considerable research addressing line A, line B, and line C in Figure 3.1. However, studies that holistically address the role of bias on inequality are much rarer (for exceptions, see Charles and Guryan (2008) and Carlsson and Rooth (2016), discussed in greater detail below). Hence both sides of this debate speak in the language of hypotheticals and probability: levels of bias *could* have “societally significant” labor market effects (Greenwald et al. 2009; Greenwald, Banaji, and Nosek 2015), but the actual existence of such effects is “unknown” (Oswald et al. 2013, 2015) and “unproven” (Tetlock and Mitchell 2009).

Context of the Current Study

The current study measures bias and wage inequality within the same organizations, using samples of workers from twelve large, bureaucratic Japanese firms. Specifically, I measure bias against non-Japanese Asians and Westerners among employees in two or more teams at these firms, and examine whether higher levels of bias correlate with greater wage inequality for the immigrant workers in each firm, all else equal. The study design addresses many of the core concerns of bias skeptics.

The first advantage of this study is that it can examine the relationship between bias and inequality in post-hire wages. Although studies have documented ethnic biases in HR personnel (Rooth 2010) and discrimination in pre-hire processes, particularly callbacks (e.g. Pager 2003; Bertrand and Mullainathan 2004; Oreopoulos 2011; Kang et al. 2016; Gaddis 2015), bias skeptics suggest the impact of bias and discrimination on wage inequality may be minor. The first reason for this is Becker’s theory (1957) of “the marginal discriminator” (Charles and Guryan 2008), which suggests that wage inequality is determined not by the mean level of bias

in society, but by the most prejudiced employer who nonetheless hires members of disadvantaged groups and is also an employer acceptable to members of this group.

Discrimination observed in pre-hire audit experiments identifies the mean but not the marginal level of discrimination and thus does not permit us to speculate about relationships between bias (or discrimination) and wage inequality determined at the margin (Heckman 1998). The current study includes only firms that hire foreign workers, and thus measures bias only in relevant places of employment.

Theories of statistical discrimination also prompt bias skeptics to question whether discrimination documented in audit studies is relevant for understanding the role of bias in post-hire outcomes. According to models of statistical discrimination, employers with preconceptions about average group-level productivity avoid hiring members of certain groups and prefer to hire members of high-productivity groups. These preconceptions could fully explain discrimination at the pre-hire stages. However, once an employee enters an organization, employers observe his or her productivity directly and need not make assumptions based on group membership. Hence, biases that lead to racial differences in hiring may theoretically have no effects on post-hire outcomes, including wages. By examining the relationship between firm-level bias and wages within firms, this study can address the claims springing from models of statistical discrimination that bias is largely irrelevant to post-hire outcomes.

A second advantage of this study is the realistic organizational settings in which it was conducted. As discussed above, dozens of lab studies have shown that bias is associated with differential treatment for preferred and non-preferred groups. However, these studies are vulnerable to criticisms about their external validity (Bagenstos 2007; Tetlock and Mitchell 2009). For example, many studies use outcome variables such as smiling or body positioning, the

relevance of which to employment decisions is unclear. Moreover, even when studies simulate more realistic employment scenarios (e.g. Ziegert and Hanges 2005), bias skeptics suggest that the “cacophony of competing cues of real life” could drown out similar effects in actual workplace interactions (Tetlock and Mitchell 2009, 16). On real teams, bias skeptics argue, “positive team spirit, norms of reciprocity, [and] team-based in-group definition” (Tetlock and Mitchell 2009, 16; Pettigrew and Tropp 2006) may override conscious and unconscious biases towards minority group members. The current study examines the effects of bias in real-life relationships where all these competing cues are also present.

Besides these interpersonal processes, critics also believe that bureaucratic, deliberative aspects of organizational life can constrain the effects of bias (e.g. Elvira and Graham 2002). Like the interpersonal dynamics described above, organizational bureaucracy is thus an additional moderating factor that can alter and perhaps block the link between bias and discrimination. The current study, conducted inside twelve large, bureaucratic businesses is thus a useful tool to observe whether higher levels of bias result in higher levels of inequality even within bureaucratic organizations.

In addition to addressing these concerns of the bias skeptics, this study also extends research by economists that has more directly addressed relationships between bias and inequality. As discussed above, with the exceptions of Charles and Guryan (2008) for blacks in the United States and Carlsson and Rooth (2016) for immigrants in Sweden, few studies examine the relationship between bias and inequality directly. These two studies estimate distributions of prejudice within regions, and set region-specific marginal levels of prejudice at the percentile of the prejudice distribution equal to share of minority workers in that region (i.e. if minorities compose 20% of the labor force, the 20th percentile of the prejudice distribution represents the

marginal level of bias). Both studies find that regional wage inequality is strongly associated with regional variation in the marginal level of bias. While these studies generally support the hypothesis that bias continues to cause inequality, the high level of aggregation in both studies (U.S. states in Charles and Guryan; municipalities in Carlsson and Rooth) has drawbacks. These aggregations rely on the assumptions that within regions attitudes towards both minorities, and minority workers themselves are evenly distributed. However, other research shows that level of aggregation may significantly alter regional estimates of bias (DellaPosta 2013). Potential clustering of both biased workers and minorities in particular firms, labor markets, or cities makes it difficult to assess how meaningful the estimates of marginal bias at the state or municipal level are. Also, neither study can show that it is bias *among employers* that matters. Bias could affect wages through many mediating processes, including in schools, in the criminal justice system, and in customer attitudes. The current study measures bias among immediate coworkers of minority workers and thus does not have to make assumptions about distributions of bias or sorting patterns of minority workers.

Data and Methods

Data Source

The data from this study come from the Survey on Workplace Environment and Diversity Management (SWEDM). I conducted this survey through the Diversity Subcommittee of the Japan Association of Corporate Executives (JACE), a major business group of Japanese firms. Twelve members of the subcommittee agreed to participate. All twelve firms have highly bureaucratized human resource systems and personnel responsible for diversity and inclusion.

Ten of these twelve firms have more than 1,000 employees, and three have more than 10,000. The sample of firms includes three high-tech manufacturing firms, five business service firms (e.g. finance, trade), and four consumer service organizations (e.g. retail).

I asked each participating firm to select two or more white-collar work teams with at least one non-Japanese member. HR personnel at these firms emailed a web link to an online survey to all members of the selected teams, requesting that they complete the survey. Respondents could take the survey in Japanese, Mandarin Chinese, or English. They were assured of anonymity; employers could not see individual response status or the content of individual responses.

Employees took the survey between February and April 2015. At each firm, the survey was open for a two to three week period, depending on preferences of HR staff. During the survey period, HR employees sent periodic messages to all members of the target teams, reminding them to complete the survey if they had not already done so. In total, the firms distributed the survey to 909 employees for a return of 539 valid responses and a response rate of 59%.^{21 22}

Control and Outcome Variables

The main outcome variable of interest in this study is annual income. The survey asked respondents to report their pay divided into two components: their past month's income, including base pay, overtime, and allowances (e.g. commuting and housing allowances); and the

²¹ One firm declined to specify how many workers they received the survey. To calculate the total response rate, I therefore assumed that the response rate at the firm with missing data was equally to the mean response rate of all firms (78.6%). Because I received 50 responses from this firm, the estimated number of survey recipients is 64. This estimate is included in the response rate denominator of 909 employees.

²² Response rates varied considerable between firms, with a response rate of 100% at 5 firms, and to a low of 34% at one firm.

total amount of bonus payments received over the previous calendar year. Respondents selected income ranges and their responses were coded at range mid-points. To determine annual income, I multiplied past month's income by twelve and added annual bonus amounts. The survey also collected standard information used in earnings analyses, including sex, age, tenure, education level, work hours, job content, contract type, and level of supervisory authority.

National Background

The independent variables of interest are respondents' national backgrounds and the level of bias among their coworkers. The standard way to measure national background in Japan is to ask about citizenship (Lie 2001). This was, however, undesirable in this case, as foreigners working in large Japanese firms tend to be highly assimilated (Liu-Farrer 2011), and may even take Japanese citizenship (even though this practice remains rare in the foreign population (Chung 2010)). I therefore impute national background from a series of questions about place of birth and language skills. Respondents were classified as Japanese if they were born in Japan and report Japanese as their native language. Similarly, respondents born in China or Korea are classified as Chinese or Korean if they selected Chinese or Korean as their native language. Approximately 88% of the sample could be classified in this straightforward manner.

The remaining 12% consisted predominately of people born in Japan who did not report native fluency in Japanese or reported native fluency in more than one language. These people were classified by their strongest language, or as Japanese if they reported equal fluency in Japanese and another language. After this classification, the sample includes 437 Japanese, 55 Chinese, Taiwanese, and Koreans, 13 people from other Asian countries, 32 people from West, including Europe, North America, and Oceania, and two people from Latin America. I drop the

Latin Americans from the sample, and aggregate the other non-Japanese into two categories: Asians and Westerners. All company samples include at least one respondent with a non-Japanese Asian background; Nine companies have at least one respondent with a Western background (see Table 3.2 in the Results section for details).

Measuring Bias

The most challenging task of the survey is the measurement of bias. Bias or prejudice are fundamentally unobservable mental states that researchers inevitably measure with error (Quillian 2006; Charles and Guryan 2008). There are three main measurement techniques in widespread use today: explicit attitudinal survey methods (as in the GSS: Bobo et al. 2012 for a description), randomized survey experimental methods (Emerson, Chai, and Yancey 2001; Kuklinski et al. 1997), and latency methods (the IAT) (Greenwald, McGhee, and Schwartz 1998). Below I discuss the strengths and weaknesses of these approaches, and describe the approach of the current study.

Explicit attitudinal measures ask respondents to report racial and ethnic attitudes directly. Common types of questions include feeling thermometers, where respondents report feelings of warmth or coolness towards a particular group (Iyengar et al. 2011), questions about endorsement of particular stereotypes, such as about blacks' work ethic (Bobo et al. 2012; Sears and Henry 2005) or Asians' trustworthiness (A. Kim and Yeh 2002), and questions about willingness to live beside, work with, or marry a person of a person of a particular race or ethnicity (Bobo et al. 2012). The main advantages of these questions are that they are easy to administer and that it is possible to identify the content of negative stereotypes about different groups. The main disadvantage is that they are obtrusive. Because respondents may wish to hide

bias, both from researchers and even from themselves, they often either refuse to answer questions about racial attitudes (Barreto et al. 2015) or deliberately give false answers (Krysan 1998; Kuklinski et al. 1997; Janus 2010).

In response to concerns about social desirability bias in responses to explicit survey questions, researchers have developed more unobtrusive measurement techniques. One such method is the vignette experiment (Alexander and Becker 1978; Wallander 2009). Vignettes describe realistic, detailed scenarios and ask respondents to evaluate and respond to them. One or more attribute of the vignette, such as the race or ethnicity of the persons described, is randomly varied. Researchers then compare how recommendations or assessments differ depending on these subtle changes to the vignette. Because all other aspects of the vignette are held constant or statistically controlled, researchers can estimate a population-level race effect, for example in managers' judgements about the type of job that would be most appropriate for a black/white job candidate with matching qualifications (Braddock et al. 1986) or in social workers' recommendations of sanctions for black/white/Hispanic welfare recipients who have violated welfare rules (Schram et al. 2009). By including a wealth of details, vignettes grant individual respondents plausible deniability that their judgements reflect racial sentiment, reducing social desirability bias, compared to traditional survey questions (Schachter 2016). Moreover, attitudes that respondents express in surveys are notoriously unstable and subject to framing effects (Gaines, Kuklinski, and Quirk 2007). If we can assume that attitudes and bias are complex and multivalent, vignette experiments have the advantage that they can tap into the attitudes and beliefs that are most relevant in real decision-making situations where discrimination may occur. Further, unlike list experiments or direct questions about racial stereotypes, they may reveal the presence of preferences and antipathies that do not reach the level of conscious thought. A

disadvantage is that reasons for racial and ethnic effects are not always clear, and may be influenced by other conscious and unconscious attitudes such as sympathy for members of disadvantaged groups. At a population level, respondents to a criminal justice vignette (as in Applegate et al. 1994) may include a mix of individuals who reduce the sentences for black offenders compared to white offenders in response to blacks' perceived societal disadvantages, and prejudiced individuals who exaggerate sentences for black offenders because they judge black offenders more harshly for the same crime.

A final method for measuring bias, employed mainly by psychologists, is the IAT (Greenwald, McGhee, and Schwartz 1998), described earlier. Compared to explicit attitude measures, the IAT is less affected by social desirability bias because its outcome measure—response time in the classification exercise—is measured in intervals too small for the respondent to consciously control it. However, critics of the IAT have brought up some of same criticisms that apply to vignette studies. Like vignettes, the IAT may measure a number of underlying attitudes other than bias. Plausible underlying attitudes and associations include familiarity with out-group members, awareness of general societal inequities and stereotypes, or sympathy (Tetlock and Mitchell 2009). Another disadvantage of the IAT is that, unlike vignettes, it is obvious to respondents that attitudes towards members of different racial or ethnic groups are the subject of its study.

Social desirability pressures to conceal prejudice are presumably very strong in this research context. First, the survey respondents are all educated professionals, the group where social desirability bias in survey responses is the highest (Krysan 1998; Janus 2010). Second, the Japanese respondents in this study engage with foreign coworkers on a daily basis in workplaces that emphasize the value of diversity. Third, although the respondents did not interact with the

researcher before or during the study, they would have been able to see from the consent form at the beginning of the survey that the lead researcher has a non-Japanese name. Concerns about appearing biased would almost certainly nearly eliminate the expression of prejudice in explicit questions and lead to non-response and survey break-offs if either the IAT or explicit measures had been used. As the most unobtrusive measure of bias, vignettes are the most appropriate measure of bias for this survey.

Vignette Methodology of SWEDM

The current survey used four vignettes to measure bias. Two vignettes describe an employee who had done something praiseworthy—helped a coworker swamped with work, and negotiated a cost-saving contract with vendors—and two vignettes describe an employee who had done something blameworthy—been absent and tardy without explanation, and falsified records. The negative vignettes appeared in succession, as did the positive vignettes, but the order within and between the negative pair and the positive pair varied randomly. Following each positive/negative vignette, the survey asked respondents to recommend rewards or punishments for the employee.

In response to employee malfeasance in the negative vignettes, respondents selected appropriate penalties from a list, which included penalties without any immediate financial consequences (informal discussion, a warning from HR), penalties with short-term financial consequences (unpaid leave; one-time reduction in salary), and penalties with long-term financial consequences (demotion, dismissal). Respondents could combine formal and informal penalties (e.g. informal discussion with demotion), but they could not select more than one formal penalty

(e.g. employees who selected demotion could not also select dismissal or one-time salary reductions). If respondents selected a one-time pay reduction, they were also asked to specify the amount in percentage terms. If respondents selected an unpaid leave, they were asked to specify the length of the leave in weeks.

In response to laudable behavior in the positive vignettes, respondents selected appropriate rewards, including rewards without immediate financial consequences (public or private praise), rewards with short-term financial consequences (one-time bonus increase), and rewards with long-term financial consequences (promotion). In the case of the positive vignettes, respondents could select any combination of rewards, because managers could realistically use all such rewards together. If respondents selected a bonus increase, they were asked to specify the size of the increase in percentage terms.

The name of the employee in each vignette varied randomly. In keeping with standard practices in Japanese workplaces, the vignettes refer to the employee by his last name alone, which appeared as either a typical Japanese, Chinese, Korean, or English last name. This design permits measurement of whether rewards or penalties recommended for Japanese employees differ from those recommended for employees with other national or ethnic backgrounds. The full text of the vignettes, the names used, and the follow-up questions appear in Appendix 1.

There are several possible ways to analyze the results and compare them by national background. One is to convert all responses to an ordinal scale. However, this is not ideal, because response categories do not represent equidistant points on some hypothetical axis of punishment or reward (i.e. demotion and firing are significantly worse outcomes than unpaid leave, while unpaid leave is only somewhat worse than a formal warning). Another technique is to convert the recommendations to a continuous scale, for example as a percentage of monthly

salary lost or gained. This is preferable to the ordinal method, because it can capture the size of the gap between short-term economic penalties or benefits and the punishments or rewards that involve no economic gain or loss at all. Nonetheless, even with this method, it is still difficult to quantify the relative gap between the effects of short-term economic actions like a one-time bonus payment or pay cut, with the effects of actions like promotion, demotion, and firing that have career-long effects.

Because these long-term economic rewards and punishments are both quantitatively and qualitatively different from the other rewards and punishments, in this paper I focus on the relative likelihood of assigning rewards or punishments with long-term economic effects to employees of different national or ethnic backgrounds.

Although vignette experiments reduce social desirability bias, they do not eliminate it entirely. To further mitigate its risks, I took several other steps. First, the survey section containing the vignettes was introduced using the following language:

Vignettes are short paragraphs that describe a situation or occurrence. In this section, you will be asked to read several vignettes about things that might happen at your workplace. You will be asked to think about the vignettes, and choose how you think other people would or should respond to the situation. We ask for your judgments on the vignettes because they will help us to understand the environment and culture of your firm, department, and section.

This language stresses that the researcher's interest is in firms' culture, rather than in the attitudes of individuals, and does not trigger respondents to think consciously about ethnicity or bias.

Second, this section also included six "non-sensitive" vignettes meant solely to elicit employees' attitudes towards personnel practices and working styles. For example, one non-

sensitive vignette describes an employee whose career aspirations clash with his managers' views of how he can provide the best value to the firm, and asks how the employee should respond to the conflict. Language in non-sensitive vignettes described employees with Japanese names, and all respondents viewed the same version. The "sensitive" vignettes used to measure bias appeared in a block after an initial non-sensitive vignette. I embedded the bias vignettes with other questions about appropriate personnel management and employee behavior in order to minimize respondents' self-consciousness censoring around racial and ethnic attitudes.

Finally, to further minimize self-censoring, it seemed prudent to avoid long sequences of vignettes with foreign names. Within pairs of bad and good vignettes, I programmed the randomizer to eliminate the nine combinations that include only non-Japanese names, out of the total 16 combinations of ethnicity possible across two vignettes. Thus, respondents viewed one of seven possible combinations of ethnicities within the bad and good vignette pairs. Across all four vignettes, respondents viewed a maximum of two vignettes (one bad and one good) with non-Japanese names, and for each individual vignette the likelihood of viewing a Japanese name was four in seven, and the likelihood of viewing a foreign name was three in seven, or one in seven respectively for Chinese, Korean, and English names.

Analyses

The analysis proceeds in three stages. First, I look at simple differences in the proportion of respondents who recommended the harshest punishments for Japanese, Chinese, Korean, and English-named people in the vignettes. I run these results excluding all foreigners, excluding all Asian foreigners, and excluding all Western foreigners. I look for evidence that respondents are

more likely to recommend the harshest punishments and less likely to recommend the highest rewards for foreign-named respondents, particularly for non-coethnics. A pattern of harsher penalties and/or lower rewards would be indicative of bias.

The second stage of the analysis compares levels of bias between companies. To create company-level measures of bias, I take the proportion of respondents within each company who recommend the harshest punishment for a Japanese-named employee and subtract this from the proportion of respondents at that company who recommend the harshest punishment for foreign-named employees. For rewards vignettes, I reverse this calculation, such that positive numbers always indicate a preference for Japanese over foreigners.

Of course, attitudes towards foreigners of different backgrounds are not necessarily similar (e.g. Kobayashi et al. 2015). However, national surveys such as the JGSS suggests that attitudes towards Chinese, Taiwanese, and Koreans are highly correlated. In the company-level bias calculations, I therefore combine vignette responses for Chinese and Korean-named employees to more precisely estimate within-company bias against other Asians. When I calculate the level of anti-Asian bias at each firm, I exclude responses from non-Japanese East Asians, and from Southeast Asians, many of whom are ethnically Chinese. I calculate anti-Western bias at the company level separately, using the responses to the vignettes for English-named persons. In the calculations of anti-Western bias, I remove responses from employees with Western backgrounds.

The third stage of the analysis is a series of hierarchical linear models in which company-level bias measures of bias calculated at the second stage are interacted with respondents' national backgrounds to predict wages, net of adjustment variables typically used in wage regressions. Because of small sample size within firms, respondents are classified into three

groups: Japanese, other Asians, and Westerners. These models use random effects for firms and teams, and fixed effects for sex, years of education, age, age squared, tenure, tenure squared, work hours, Japanese language ability, and English language ability. A negative and significant interaction between the bias measure and national background indicates that wage inequality is greater where bias is greater. Because bias could influence numerous employment decisions, including performance evaluations, job assignments, and promotions, I do not use job categories or level of supervisory authority as controls.

Results

Punishments and Rewards

Table 3.1 shows the percentage of respondents with different backgrounds who recommended the harshest punishments (demotion or firing) in the negative vignettes, and the highest rewards (promotion, either alone or in combination with other rewards) in the positive vignettes, as well as the number of respondents who viewed each vignette-ethnicity pair. The first pair of columns shows the percentage and denominator for Japanese respondents alone, the second two columns show these quantities for Japanese and Western respondents together, and the third pair of columns displays results for Japanese and other Asian respondents together.

Table 3.1: Punishments and Rewards by Vignette Name and Respondent Background

	Japanese Respondents		Japanese and Western Respondents		Japanese and Other Asian Respondents	
	% highest pun. or rew.	Denominator N	% highest pun. or rew.	Denominator N	% highest pun. or rew.	Denominator N
Vignette name - records falsification						
J	29.0	263	29.2	276	28.8	304
K	31.7	64	35.2	72	28.6	71
C	34.5	56	35.7	57	32.8	68
E	27.5	54	31.1	64	23.7	63
Vignette name - tardiness						
J	1.7	241	1.6	260	1.5	277
K	1.5	68	1.4	71	2.7	77
C	0.0	63	0.0	69	0.0	74
E	3.2	65	3.1	69	2.7	78
Vignette name - saving money						
J	16.2	254	17.4	273	17.4	288
K	17.9	58	17.5	66	21.2	68
C	16.4	63	15.9	65	20.0	77
E	27.6	62	26.2	65	31.9	73
Vignette name - helping coworkers						
J	16.5	245	16.9	263	16.7	289
K	11.6	70	13.7	74	11.7	78
C	11.5	61	13.4	67	11.8	68
E	15.3	61	16.1	65	15.9	71

J=Japanese, K=Korean, C=Chinese, and E=English

Denominator N refers to the number of respondents of each background who viewed vignettes with particular names.

The first obvious pattern is that respondents viewed records falsification as a much more serious offense than tardiness. Almost no respondents (2% overall) recommended the harshest punishments for the tardy employee, whereas for the employee who falsified records, approximately one third did so.

The responses to the two positive vignettes are much more similar to each other than the responses to the two negative vignettes, as is apparent from the relatively equal rates at which respondents recommended promotion in both the saving and the helping vignettes. This similarity emerges at the individual level as well. There is a moderate correlation (Pearson's $r=0.53$) between respondents' recommendations in the saving vignette and their recommendations for the helping vignette. In contrast, the correlation for responses in the negative vignettes is negligible (Pearson's $r=0.11$). Relationships between responses to the tardiness vignette and the two positive vignettes, and between responses to the records falsification vignette and the two positive vignettes are also very small, all between -0.1 and 0.1 .

Next, I consider the response patterns by the ethnicity of the name that respondents viewed in the vignette. In the records falsification vignette, both Japanese respondents and Western respondents were marginally more likely to recommend the harsher punishments when they viewed a Korean or Chinese name. For example, among Japanese respondents, the rate of harsh punishment is 3% higher for Korean names relative to Japanese names, and about 5% higher for Chinese names relative to Japanese names. When Western employees' responses are included, both Korean and Chinese names have a 6% higher likelihood of being assigned the harshest punishment relative to Japanese. These differences are not statistically significant at the 0.1 level. However, they are comparable in magnitude to the 6% gap in positive responses for Latino and white applicants to low-skilled entry level jobs in a New York City audit study

(Pager, Bonikowski, and Western 2009), and to the 3-5% difference in callbacks for Asian ethnics with Canadian education and experience and similar Anglo applicants to entry-level white collar jobs in Canada (Oreopoulos 2011).

There are no interpretable patterns for the Chinese and Korean names in the tardiness vignette. Only eight respondents in the entire sample recommended the harshest punishments to names of any ethnicity. Because of this lack of variation, I do not analyze the results of this vignette further. If there is bias against ethnic minorities, it is apparently not activated in the context of this relatively mild infraction.

In the saving vignette, differences in recommended rewards for Chinese and Koreans on one hand and Japanese on the other are negligible (less than 2%), whether we consider attitudes of Japanese alone or attitudes of Japanese and Westerners together.

In the helping vignette, Japanese respondents are less likely to recommend the highest rewards for Koreans or Chinese than they are for their own co-ethnics, a difference of about 5%. The direction of this effect is the same, but it is attenuated to about 3% when Westerners' responses are included. As with the records falsification vignette, these differences do not reach statistical significance at the 0.1 level.

In sum, there is some evidence of bias against Korean and Chinese ethnics in this study as demonstrated by a greater likelihood of harsh punishment in the falsification vignette and a smaller likelihood of promotion in the helping vignette. However, it is possible that these differences do not represent bias. The sizes of the effects are small, and may represent random noise. Further, even if differences are real and not the result of random variation, they suggest that bias is not very severe in this population of firms, is not consistently activated in personnel

management decisions, and/or is counterbalanced by competing motivations and concerns depending on the decision-making context.

The results for English-named employees present a different picture. In all vignettes, the likelihood of an English-named employee receiving the harshest punishment or the most generous reward is within two percentage points of the recommendations for Japanese-named employees (as in the tardiness and helping vignettes), or is actually more lenient or generous (as in the saving and falsification vignettes). The results are particularly striking for the saving vignette, where the magnitude of the English name advantage over the Japanese name is quite large (11% for Japanese respondents and 14% when Japanese are combined with other Asian respondents). In both cases, this difference is statistically significant at the 0.05 level. In other words, the results provide no evidence of negative bias against Western (English-named) employees.

Firm-Level Bias Calculations

Table 3.2 presents the estimates for bias within each firm based on the records falsification, saving, and helping vignettes. Positive numbers in this table indicate favorable treatment for Japanese relative to the target non-Japanese ethnic group.

Because sample size within each firm is relatively small, these estimates are imprecise. As a gauge of this imprecision in the company-level estimates, I include the number vignettes referring to foreign-named persons that respondents viewed at each firm in Table 3.2. The estimates of bias against English-named persons are more imprecise than the estimates of bias against Chinese/Korean-named persons, because the likelihood of seeing either a Korean or

Chinese-named vignette was two in seven compared to one in seven for the English-named vignette.

The records falsification vignette identifies harsher treatment for Chinese and Koreans relative to Japanese in eight out of twelve firms. In four out of twelve firms, the differences in punishment recommendations are particularly large, with differences of greater than 10% in respondents' recommendations for other East Asians' punishments compared to Japanese. We would naturally expect more extreme bias estimates in firms with smaller sample sizes, but these four high estimates do not come from the firms with the most imprecise estimates (Firms J and L).

In the saving vignette, firms are almost evenly split between those where generous treatment is recommended for other East Asians more often than for Japanese (values below 0), and those where it is recommended more often for Japanese than for other East Asians (values above 0). Moreover, most of the values are clustered close to 0. This is the pattern we might expect if responses towards Japanese and other East Asians do not vary.

Patterns for the vignette on helping show a disadvantage for other East Asians in nine out of twelve firms. There are, however, fewer firms with differences greater than 0.1, compared to the records falsification vignette. The range of bias values is larger than in the records falsification vignette, but this is due to the very low outlying estimate for Firm L, the most imprecise estimate. Without this outlier, the range of firm-level bias is smaller in the helping vignette than in the records falsification vignette.

Table 3.2: Estimates of Firm-Level Bias

	Company Samples			Falsified Records				Saved Money			Helped Coworkers				
	N _{Total}	N _{Asian}	N _{Western}	CK-J	N _{CK}	E-J	N _E	J-CK	N _{CK}	J-E	N _E	J-CK	N _{CK}	J-E	N _E
<i>Company name</i>															
A	32	5	2	0.20	7	0.61	3	0.03	7	-0.20	4	0.23	9	-0.31	4
B	43	12	1	-0.32	6	-0.27	5	-0.03	7	-0.15	7	0.03	11	-0.04	5
C	35	2	6	0.18	10	-0.37	2	0.05	9	-0.02	5	-0.16	9	0.20	6
D	50	8	0	0.20	9	-0.06	6	0.15	11	-0.06	6	0.02	14	-0.07	7
E	30	4	0	-0.09	8	-0.05	5	0.00	7	-0.08	5	0.07	7	-0.13	4
F	81	9	2	0.09	21	-0.15	11	0.08	21	-0.13	11	-0.06	18	-0.02	11
G	27	2	0	0.05	8	0.40	3	0.00	8	-0.18	4	0.06	7	0.06	4
H	39	11	5	-0.07	7	-0.09	4	-0.08	7	-0.07	4	0.23	10	0.06	5
I	142	7	8	0.05	38	-0.05	18	0.02	39	-0.13	20	0.09	40	0.08	18
J	23	7	1	-0.17	3	-0.20	3	-0.18	5	0.15	3	0.02	5	-0.18	3
K	29	1	7	0.23	9	0.36	3	-0.31	8	-1.00	2	0.01	10	0.20	3
L	10	2	2	0.08	3	NA	0	0.00	3	-0.50	2	-0.50	2	0.20	1
<i>Counts of Firms by Bias Level</i>															
N < -0.1				2		4		2		7		2		3	
N < - 0.1 ^a				1		2		2		5		1		2	
N ≤ 0.0				4		8		7		11		3		6	
N > 0.0				8		3		5		1		9		6	
N > 0.1				4		3		1		1		2		3	
N > 0.1 ^a				4		0		1		0		2		1	

Information in “Company Samples” columns refers to number of respondents. Asian refers to non-Japanese Asians. Subcolumns under “Falsified Records,” “Saved Money,” and “Helped Coworkers” columns refer to 1) Difference in the rate that the harshest punishment/most generous reward was recommended for Chinese or Korean-named persons, compared to a Japanese; 2) the number of Japanese and Western respondents at each company who viewed the vignette with a Chinese or Korean name; 3) difference in the rate that the harshest punishment/most generous reward was recommended for an English-named person compared to a Japanese; 4) the number of Japanese and East Asian respondents at each firm who viewed the vignette with the an English name. In subcolumns 1) and 3) positive numbers indicate recommendations that favor Japanese-named persons.

^a Counts in these rows exclude firms with the most imprecise estimates: those where estimates are based on fewer than four minority-named vignettes.

As in the across-firm results, both the records falsification and the helping vignette suggest some negative bias against Chinese and Koreans, at least in some firms. An important question for the next stage of the analysis is whether measurements in these two vignettes capture the same underlying construct. To investigate this possibility, I look at the correlation between the two firm-level measures that appear to detect some negative bias. The Pearson's R for the correlation is -0.15. This is a very low correlation, and it is in the opposite direction of what we would expect. There are two ways of interpreting this finding. It may indicate that the firm-level estimates of bias are too imprecise to produce a reasonable ordering of more and less biased firms. Alternatively, it may indicate that the two vignettes tap into two entirely different types of stereotypes and beliefs about members of different ethnic groups. In either case, the low correlation suggests that it is better to consider separately the effects on ethnic wage inequality of estimated firm-level bias derived from each of these vignettes.

With regards to anti-Western bias, the firm-level results support the conclusions from the overall analysis that anti-Western bias is not widespread, or at least is not activated by the type of personnel decisions described in these vignettes. The records falsification vignette reveals more negative judgements towards English-named persons in just three firms. Moreover, the three firms with values above zero all have very small sample sizes, casting doubt on whether these positive numbers truly indicate anti-Western (English-named) bias. A similar but even stronger pattern is apparent in the firm-level analysis of the saving vignette for Westerners. There is only one firm with an estimate above zero, and this firm is once again one of those with more imprecise estimates. In seven firms (five, excluding those with the smallest sample sizes) there is

a 10% or greater likelihood that English-named employees will be recommended for promotion, compared to Japanese-named employees.

The helping vignette does not indicate any preference between Japanese-named and English-named employees. Firm-level values are distributed evenly above and below zero, and most are within 0.1 of zero.

Two out of the three vignettes, the records falsification vignette and the money saving vignette, thus suggest some preference for or favorable treatment of English-named persons. Once again, these positive attitudes at the firm level are not correlated in the way we would expect. Pearson's R for firm-level bias towards Westerners measured by these two vignettes is -0.56 . Once again, the order of firms may be unreliable because estimates are imprecise, or these two vignettes may capture completely different attitudes. Because estimates of bias are more imprecise for Westerners than for East Asians, the former possibility is more likely in this case compared to the case of attitudes towards East Asians.

Inequality Analyses

The next series of analyses investigates whether firm-level bias is associated with wage inequality. Models 1 and 2 in Table 3.3 examine the interaction between firm-level bias against non-Japanese East Asians and annual income. Model 1 uses the measure of bias derived from the records falsification vignette; Model 2 uses the measure from the vignette on helping coworkers. In Model 1, there is a negative interaction between being from a non-Japanese Asian country and

the level of bias. This negative interaction is significant at the 0.1 level. In other words, there is some evidence that East Asians experience a larger wage gap in firms with more bias.

As discussed above, it is possible that this is a spurious result. Because the estimates of bias at each firm are imprecise, the ordering of firms from least biased to most biased is likely to be inaccurate. To test whether this result is spurious, I reran the model (results available on request) without the two most imprecisely measured firms (Firms J and L). The magnitude of the interaction effect increases slightly and reaches significance at the 0.05 level after removing the two most imprecisely measured firms. To further reduce spurious order effects, I grouped together firms with bias measured at or below 0.05 (Firms B, E, G, H, I, and J) and bias measured above 0.05 and interacted this binary bias measure with national background. I chose this cutpoint as a conservative test, because firms with *observed* bias above 0.05 are more likely than other firms to have *true* levels of bias greater than zero. The resulting binary measure of bias also has a significant (at the 0.1 level) and negative interaction with non-Japanese Asian background.

Table 3.3: Regression of Firm-Level Anti-Asian Bias and National Background on Annual Earnings

	Model 1		Model 2	
	Beta	SE	Beta	SE
<i>Individual level variables</i>				
Male	1,772.89***	275.50	1,832.58***	275.29
Years of education	302.53***	90.81	279.48**	91.25
Age	184.74	128.22	181.93	128.43
Age squared	0.77	1.58	0.81	1.58
Tenure	270.43***	55.04	260.35***	55.09
Tenure squared	-8.77***	1.65	-8.49***	1.66
Weekly work hours	35.22*	16.07	34.87	16.05
Advanced English	1,140.27***	287.88	1,042.78***	289.47
Advanced Japanese	-331.43	644.94	-649.57	653.40
National background ^a				
Non-Japanese Asian	-587.60	437.46	-741.20+	449.34
Westerner	295.94	695.22	666.44	617.37
<i>Firm level variables</i>				
Anti-Asian bias (records falsification)	4,360.13	2,706.47		
Anti-Asian bias (rf) * Asian	-3,957.00+	2,138.31		
Anti-Asian bias (rf) * Westerner	5,663.37	4,143.39		
Anti-Asian bias (helping coworkers)			-5,252.83*	2,552.43
Anti-Asian bias (hc) * Asian			3,756.23	2,682.84
Anti-Asian bias (hc) * Westerner			4,235.74	3,615.46
Constant	-7,659.45**	2,700.87	-6,793.01*	2,688.83
<i>Model information</i>				
Observations	525		525	
Number of firms	12		12	
Individual-level variance component	7124386		7128433	
Team-level variance component	475935		556818	
Firm-level variance component	1862972		1755657	

Results are from ANCOVA with random effects models (HLM). All slopes are fixed.

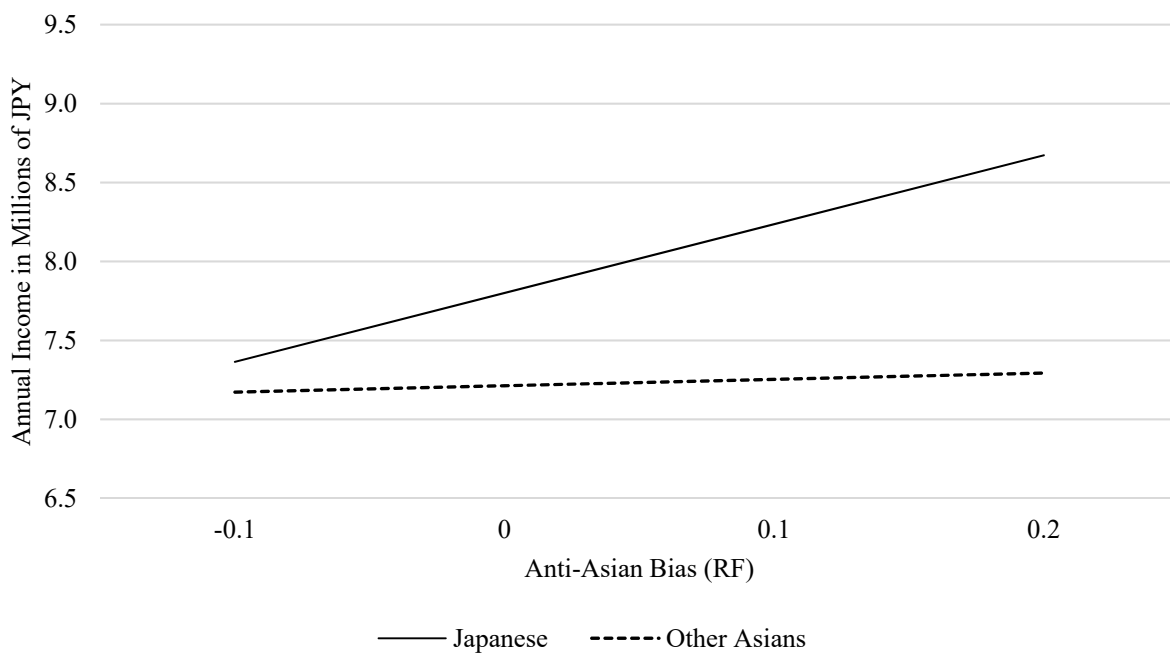
*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

^a Reference category is Japanese

In other words, there is evidence that in firms with higher levels of bias, non-Japanese Asians are at a relative disadvantage, and this finding does not appear to be a spurious result of imprecision in the measurement of bias. To contextualize the magnitude of this effect, consider Figure 3.2, which plots predicted incomes for Japanese and other Asian employees by firms'

level of bias based on Model 1. Predicted income is measured in millions of yen, equivalent to tens of thousands of dollars. In firms where respondents recommend the harshest punishments for Chinese or Korean-names persons 10% more often than for Japanese-named persons, the predicted annual income gap is about 1 million yen or \$10,000.

Figure 3.2: Predicted Annual Income for Japanese and Other Asians by Level of Anti-Asian Bias



Turning to Model 2, there is no evidence of a wage penalty for East Asians in firms judged to be more biased using the helping vignette. The interaction coefficient is non-significant and its sign is positive, the opposite of what we would expect if East Asians earned less than Japanese in more biased firms. The main effect for bias is positive and significant, suggesting wages for everyone are higher when anti-East Asian bias is higher, but this result disappears if the interaction term is removed from the model.

Models 3 and 4 in Table 3.4 test the effects of attitudes towards English-named persons on wage outcomes for those with European, North American, and Oceanian backgrounds. Model

3 uses the measure of bias from the records falsification vignette and Model 4 uses the measure from the saving vignette. The sample sizes are lower in these two models because they do not include three firms that have no Western employees in the sample (Firms D, E, and G). In addition, Model 3 excludes Firm L, because no non-Western respondents viewed the records falsification vignette with an English name, making it impossible to use this vignette to estimate bias there.

Table 3.4: Regression of Firm-Level Anti-Western Bias and National Background on Annual Earnings

	Model 3		Model 4	
	Beta	SE	Beta	SE
<i>Individual level variables</i>				
Male	2,080.42***	324.21	2,039.00***	315.84
Years of education	295.86**	112.52	275.65*	110.14
Age	261.30+	156.01	260.13+	154.25
Age squared	0.15	1.92	0.08	1.89
Tenure	279.35***	63.55	288.96***	63.05
Tenure squared	-9.45***	1.97	-9.64	1.95
Weekly work hours	34.13+	18.28	34.06+	18.16
Advanced English	1,280.47***	321.87	1,250.60***	315.55
Advanced Japanese	-792.74	760.20	-713.88	731.33
National background ^a				
Non-Japanese Asian	-395.55	526.73	-795.64	588.11
Westerner	830.29	663.21	-472.16	772.42
<i>Firm level variables</i>				
Anti-Western bias (records falsification)	-2,421.64	1,732.49		
Anti-Western bias (rf) * Asian	603.63	1,774.36		
Anti-Western bias (rf) * Westerner	4,760.68*	2,214.41		
Anti-Western bias (saving money)			1,182.86	1,682.76
Anti-Western bias (sm) * Asian			-2,916.94	2,384.71
Anti-Western bias (sm) * Westerner			-4,441.20**	1,662.12
Constant	-8,748.71**	3,293.65	-8,265.13*	
<i>Model information</i>				
Observations	414		423	
Number of firms	8		9	
Individual-level variance component	7,685,458		7,600,426	
Team-level variance component	791,736		751,233	
Firm-level variance component	1,752,346		1894,567	

Results are from ANCOVA with random effects models (HLM). All slopes are fixed.

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1

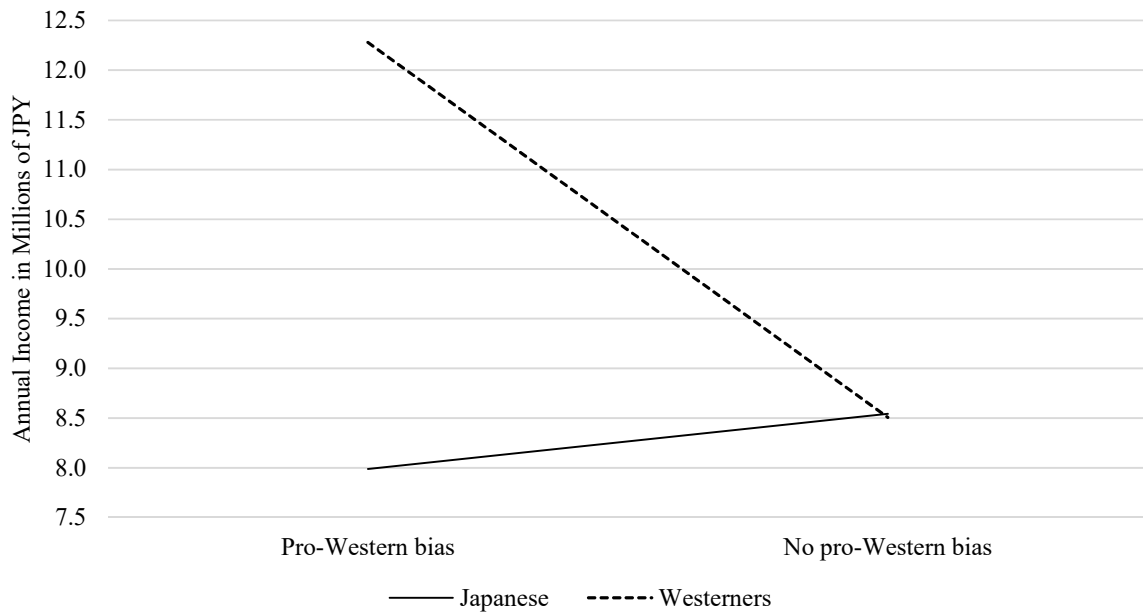
^a Reference category is Japanese

Surprisingly, there is a positive interaction between bias and wages for Westerners in Model 3. The model suggests that Westerners are at a wage advantage in firms where more people recommend harsh punishment for Westerners. However, concerns about measurement error and firm ordering are even more pertinent here than in the analysis of Asians, because bias against English-named persons is estimated more imprecisely than bias against East Asians. Moreover, removing the most imprecise estimates—those based on just two or three vignettes—is not a feasible robustness check. Removing firms with only two or three vignettes on English-named persons leaves just four firms for analysis, and excludes the only two firms (Firms A and K) where any anti-Western bias is detected and where any Western employees appear in the sample. However, as above for anti-East Asian bias, it is possible to create a binary variable representing more or less anti-Western-biased firms, and to interact this binary variable with employee ethnicity. To create this binary variable, I use a cutpoint of -0.05 , the highest cutpoint that allows me to include firms other than Firm A and Firm K in the biased group. The interaction between the binary measure of bias and Western ethnicity is positive, but it does not approach statistical significance ($p=0.20$). In other words, the evidence that anti-Western bias is associated with higher wages for Westerners is not supported by the robustness checks. Indeed, further examination of the data shows that this effect is entirely fueled by firms A and K, where estimates of bias are particularly imprecise.

Model 4 examines anti-Western bias as measured by the saving money vignette. The interaction coefficient is significant and negative, indicating that Westerners do earn less when anti-English-named bias is stronger. To test the robustness of this result, I once again create a binary variable. Here I used a cut point of -0.06 . I use this cut point (as opposed to $+0.05$ or -0.05 used above) in order to include three rather than two firms in the bias group. However, because I detect anti-Western bias in only one firm (Firm J), it is more appropriate to interpret a value of 1 for this binary variable as a sign that *pro*-Western bias was not

detected, or is measured only at low levels, rather than to interpret it as a measure of anti-Western bias. A negative interaction between national background and the binary measure of bias therefore means that Westerners earn less when pro-Western bias is low, not when anti-Western bias is high. When this binary variable for bias is used, the interaction coefficient between bias and Western background remains negative and significant at the 0.05 level. In other words, unlike the counter-intuitive results from the records falsification vignette, the evidence that Westerners earn relatively less when pro-Western bias is weaker is also supported in the robustness checks.

Figure 3.3: Predicted Annual Income for Japanese and Westerners by Level of Pro-Western Bias



To understand the magnitude of this difference, I depict the predicted incomes for Japanese and Westerners in pro-Western biased firms and other firms in Figure 3.3. I use the binary interaction to show the magnitude because outlying bias values in imprecisely estimated firms K and L could have an outsized impact on predictions in the linear model. Westerners have a sizable advantage of over 3 million yen, or \$30,000, in firms with pro-

Western bias and no detected advantage in firms where this pro-Western bias was not detected or was detected only at low levels.

Discussion

In the context of large, bureaucratic Japanese firms, I find that non-Japanese Asians earn less than Japanese when coworkers are more biased, as measured by a vignette about records falsification. I also show that in firms with pro-Western bias, measured through a vignette about saving money, Westerners earn more than Japanese coworkers. A vignette about helping coworkers suggests some bias against East Asians, but firm-level bias measured by this vignette does not predict wage inequality. Similarly, the records falsification vignette suggests some pro-Western bias, but firm-level results do not reliably predict wage inequality. In this section, I will consider first, why some vignettes detected bias, but not others; and second, why some vignette results predicted inequality, but not others.

A rich history of vignette studies shows that the effect of racial or ethnic cues on responses depends heavily on other contextual signals in the vignette (Applegate et al. 1994; Soss, Fording, and Schram 2011; Schachter 2016). In particular, “stereotype consistent” cues have been shown to exaggerate the effects of racial minority status (Schram et al. 2009). A large body of research in Japan suggests that Japanese people tend to hold positive stereotypes of people from the West and negative stereotypes about other Asians (see Long 2010 for a review), and that these stereotypes emerge as early as the elementary school years (Toriyama and Shiota 2015). Widespread positive associations with Westerners may explain why English-name effects appear much more clearly in the money saving vignette than in the records falsification vignette. It does not explain why we do not observe these same effects in

the helping vignette, although we can speculate that perhaps the concept of “negotiating skill” triggers these positive associations more than the concept of “helpfulness.” It is also clear why we might detect anti-Asian bias in the records falsification vignette. Asians from other countries living in Japan are often stereotyped as criminals (Rankin 2012). Even though Asian foreigners are less likely to commit crimes than Japanese, crimes by Asians receive wide press coverage and outsized attention from the National Police Agency (Arudou 2013; National Police Agency 2016). Of the four vignettes, the records falsification story is most likely to trigger these associations. However, there is no equally obvious stereotype to explain why the helping vignette generated less generous rewards for East Asians.

It is significant for understanding the link between bias and inequality that not all employment vignettes consistently captured pro-Western or anti-Asian biases. A strength of the vignette methodology is that it allows researchers to capture unobtrusively what cues produce racial or ethnic effects. Identifying these cues in Japanese organizational settings was not the focus of this research. Nonetheless, one implication of the findings is that negative stereotypes about East Asians and positive stereotypes about Westerners do not uniformly penalize or benefit employees with those backgrounds. Depending on the information employers are considering, bias may or may not be activated.

Researchers have questioned why laboratory studies of the bias-discrimination link do not produce consistent results (Blanton et al. 2009; Oswald et al. 2015). Although null results may occur because errors in the measurement of bias are too great, the results of the current study, like other vignette studies (e.g. Schram et al. 2009), indicate that null results may also occur because the cues that activate bias are not present in the experimental setting (Pager and Quillian 2005).

The results also raise the questions, why did bias measured by the saving vignette (but not the falsification vignette) predict higher wages for Westerners; and why did bias measured by the falsification vignette (but not the helping vignette) predict lower wages for non-Japanese Asians? I believe the answer lies in who holds bias against members of different groups (see Table 3.1) and how bias is aggregated at the firm level in the inequality analyses. Recall that I combined answers to vignette questions from Japanese and Western respondents to create the firm-level measures of anti-Asian bias and that I combined answers from Japanese and other Asian respondents to create the firm-level measures of anti-Western bias.

Consider responses to the records falsification vignette. For an English-named records falsifier, Japanese respondents alone recommend the harshest punishment at rates slightly below those for Japanese-named records falsifiers. A more sizable gap in recommendations only emerges when we consider the responses of all Asian respondents, including non-Japanese. But given the marginalization of non-Japanese Asian employees, who face negative bias from Japanese, it is not clear that their more pro-Western attitudes would actually have an impact on wages. Asian employees are also less likely to be managers and have shorter tenures than either Japanese or Western employees (see Chapter 2 for details). Hence, when their attitudes differ from those of Japanese employees, they may have little or no effect on employment outcomes. In comparison, in the saving vignette, there is a sizable pro-Western bias even among Japanese employees alone. In this vignette, the responses of non-Japanese Asians contribute less to the overall measure of pro-Western bias. Because attitudes of Japanese are more likely to be influential in employment outcomes, this could explain why the savings vignette predicts inequality while the falsification vignette does not.

With regards to the anti-Chinese or Korean bias in the records falsification vignette, we find it slightly more extreme among Western respondents compared to Japanese

respondents alone. In contrast, in the helping vignette, including Western responses attenuates the anti-Chinese and Korean effect. Because of the pro-Western bias among Japanese respondents, it is possible that Westerners' attitudes do have a significant effect on decision-making in the workplace. Western employees also have a structural advantage in that they are much more likely than non-Japanese Asian employees to have supervisory authority (38% of Westerners in the sample supervise at least one employee, compared to 21% of non-Japanese Asians). It is perhaps because *Westerners'* anti-Asian bias is triggered only by the falsification vignette that only the falsification vignette predicts inequality.

Conclusion

Does bias continue to affect personnel outcomes past the hiring stage? This question is of interest to scholars of inequality and to policymakers wishing to design effective workplace affirmative action and inclusion policies. However, because it is difficult to measure bias and to gain access to organizational settings, scholars' answers have relied heavily on theory without complementary empirical testing in real-life employment situations. For example, researchers predict that bias will have little or no effect on post-hire wages because market-based sorting mechanisms will allocate minority workers to the most unbiased employers (Becker 1957; Heckman 1998; Tetlock and Mitchell 2009). Others argue that biases are most salient in low-information environments (Petersen and Saporta 2004; Pager, Bonikowski, and Western 2009). This implies that even if bias matters in hiring when employers have little information about applicants, it will matter less post-hire when employers have rich information about employees. Finally, scholars suggest that even though bias is present, countervailing interpersonal mechanisms such as person-positivity bias and

equal status contact in the workplace, or organizational and legal mechanisms like punishment for discriminatory behavior will limit its influence (Tetlock and Mitchell 2009).

This paper is, to my knowledge, the first to empirically examine these predictions about the role of workplace bias in post-hire outcomes. Contrary to the hypothesis that the job allocation process sorts minorities out of firms with levels of bias that are consequential for career outcomes, either because biased employers refuse to hire minorities, or because minorities avoid positions with biased employers (Pager and Pedulla 2015), these findings suggest minorities do work in firms where bias is strong enough (or unconstrained enough) to influence post-hire outcomes. This is a particularly significant finding given Japan's workplace demography. Skilled foreign workers make up a slim percentage of workforce, and their share is smaller in Japan than in any other developed country (Oishi 2012; Holbrow and Nagayoshi 2016). However, attitudes towards immigrants are not less welcoming in Japan than in major Western countries; if anything, they are slightly more positive (Kage, Rosenbluth, and Tanaka 2016). If minorities anywhere are able to sort away from firms in which bias is strong or unconstrained enough to affect employment outcomes, we would expect this to occur in Japan. These results indicate it does not. Even in this sample of firms, all of which have dedicated staff focusing on diversity, and all of which have CEOs who have made diversity a cornerstone of their employment strategy, we observe biases among employees that predict outcomes for minorities employed in those firms.

In addition, contrary to the hypothesis that the information-rich relationship between employees and employers eliminates the impact of bias on decision-making post-hire, or that interpersonal mechanisms such as team loyalty and positive intergroup contact override it in real-life employment scenarios, it appears that bias has effects with long-term impacts post-hire. Naturally, other factors thought to moderate the relationship between bias and inequality, such as the content and enforcement of anti-discrimination law, are quite different

in Japan, the United States, and other developed countries (e.g. Dobbin 2009; Hasegawa 2013). This research cannot speak to the effects of particular moderating factors in Japan or elsewhere; but it does demonstrate that bureaucratic organization and intergroup contact that could theoretically short-circuit the bias-inequality link in any large organization are not enough to prevent its negative effects. The results of this research thus cast doubt on several of the major objections that critics have expressed towards claims that bias has post-hire effects on wage inequality.

At the same time, this research also has limitations. At the callback stage of the employment process, researchers may use quasi-experimental methods to test for discrimination (Pager and Quillian 2005; Pager, Bonikowski, and Western 2009) and the link between bias and discrimination (Rooth 2010). However, real-life post-hire employment decisions are not amenable to experimental manipulation. Thus, as with other observational research designs, an unobserved factor may hypothetically drive the relationship between attitudes and wage inequality we have observed here. Perhaps the most likely suspect is performance: true, unobserved performance gaps by ethnic group that vary by firm could cause both differences in attitudes towards these groups (as predicted by status construction theory: see Ridgeway and Correll 2006) and differences in wages.

It is very unlikely, however, that variation in performance explains the full relationship between bias and inequality that we observe. First, bias is measured based on recommendations for punishments and rewards for hypothetical, differently named employees who have done the exact same thing. For differences in punishment or reward recommendations in these vignettes to emerge based on performance of actual employees with corresponding ethnicities, respondents must be generalizing stereotypes to a case where they do not necessarily apply. As such, even if performance differences are affecting both the dependent and independent variables, it is likely that at least some (although arguably not all)

of the observed wage inequality is caused by similar generalizations in actual employment decisions made by these same actors.

Second, for performance variation to explain the observed bias-inequality relationships, we would have to assume that Western employees outperform both Japanese and other Asian employees in nearly all firms. Although performance is unobserved, this pattern is unlikely. If anything, Asian immigrant employees should have better performance than Western employees because Western employees are less likely to speak good Japanese and are more culturally distant from Japanese than Asian immigrants (see Chapter 2) and because Asian immigrants have fewer opportunities in other developed countries and are thus more motivated or “hungry” to make it in Japan (Career Connection 2014). Not only would we expect Asian employees to enjoy a performance advantage over Westerners, but we would also expect *both* groups to have a performance disadvantage compared to Japanese because immigrants may lack soft and hard skills that are valued in the Japanese labor market. Thus unobserved performance differences are also unlikely to fully explain the results.

To further disentangle these processes, future research might adopt several strategies. For example, in a longitudinal design, researchers might measure managers’ bias at Time 1. At Time 2, researchers could then examine how managers rate the performance of employees of different races and ethnicities who were not their subordinates at Time 1, and determine if managers’ T1 bias has a relationship to ratings for new employees. Researchers might also reverse this and measure employee evaluations at Time 1. After employees transfer or change jobs, researchers could then measure bias of employees’ new managers and coworkers at Time 2, to determine if T1 performance of subordinates predicts coworkers’ attitudes, indicating whether or not people update their attitudes based on recent workplace experiences.

A second limitation of the current study is that, assuming the relationship between bias and inequality is causal, I cannot identify the mechanisms through which bias among immediate coworkers generates inequality. As shown in Figure 3.1, bias may affect wage inequality both through the creation of biased rules and by increasing the likelihood that individuals will engage in discrimination above and beyond that codified by rules. This research design does not permit me to assess whether or in what combination rulemaking and unsanctioned discrimination produce the observed inequalities. However, features of the design suggest that the bias detected in this study primarily affects minority workers through unsanctioned discrimination. Bias is measured at the local level among respondents' immediate coworkers; these coworkers and supervisors have the opportunity for unsanctioned discrimination, particularly in performance reviews and allocation of rewards. However, immediate coworkers and supervisors do not usually determine policies that are consequential for workers' career progression, such as requirements for promotion or pay scales for different types of jobs. If the views of the employees surveyed here perfectly represent those of the workforce at their companies as a whole, or, if not representative, at least deviate only in some predictable way, then it would be plausible that the bias I detect among immediate coworkers is correlated with discriminatory rule-making at the firm level, which in turn affects inequality. However, these assumptions about representativeness are unnecessary to link local-level attitudes towards local, unsanctioned discriminatory behavior. This mechanism would be consistent with status construction theory, which suggests that group stereotypes can lead members of the dominant group to devalue the contributions and accomplishments of disadvantaged group members (Ridgeway and Correll 2006). Measuring global biases at the firm level, the biases of immediate coworkers, and the biases of strategic actors like CEOs or HR managers and the relationships between inequality and bias at these

different levels could help answer questions about the mechanisms through which bias affects wages.

Unobtrusive measures of bias, such as vignette studies or the IAT, find persistent differences in how Americans interpret and respond to people's actions based on race. Audit studies in employment contexts also demonstrate racial and ethnic discrimination at the callback stage. Critics question, however, whether employer bias or discrimination really matter for post-hire wage inequality. This study suggests that low levels of bias are a poor justification for optimism that bias has a minimal impact on post-hire outcomes. Levels of bias detected here are equivalent or smaller to those detected in audit and vignette studies in the United States, and the minority population is much smaller. And yet, minorities still work with biased coworkers, with serious implications for their post-hire earnings, to the detriment of East Asian immigrants and to the benefit of Western ones.

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CHAPTER 4

CONCLUSION: SLOW DOWN OR SLOW DAWN?

Japan's population is rapidly contracting. If current trends continue, its population will fall to just one third of its 2010 peak by as early as 2095, with even faster declines in the working age population. These numbers are shocking, but they are not unique. Other countries also face below replacement birthrates and are poised to experience similar population collapses. Japan is noteworthy, however, in that it has reached the point of demographic decline much sooner than other major economies. Its place at the forefront of this demographic shift therefore makes Japan the ideal laboratory in which to investigate the social and economic implications this unprecedented transition.

Scholars are unsure how population contractions will maintain or disrupt patterns of inequality. On one hand, inequality may rise if these changes sap demand and place an unsustainable tax burden on the shrinking pool of working adults (e.g. Guest and Swift 2008; Reher 2011). On the other, growth in the global economy may offset local economic stagnation, and companies' labor demand may outpace labor supply, creating new opportunities for disadvantaged people (Alba 2009). In Japan, higher tax burdens, reduced opportunities, and growing economic uncertainty could prompt Japanese men, who have historically monopolized the best jobs, to tighten their grip on high-status, stable employment. Alternatively, labor shortages may disrupt men's historical monopoly and open new doors for female and foreign workers' advancement.

To illuminate patterns of inequality in the context of negative population growth, this dissertation analyzes original survey data gathered from 539 white-collar foreign and Japanese workers employed in twelve large Japanese firms. The findings present both cause for optimism and cause for concern.

As a cause for optimism, I find little evidence that firms restrict women's access to

jobs on the prestigious management track or that they shunt ambitious women into less remunerative contract or general track jobs. Although there are many women who work in contract jobs and in general track jobs, there is no indication that these women do so unwillingly; female employees in contract or general track jobs rate their job quality as highly as do women who have “made it” in management track positions.

Increasing government pressure on firms to include women in the best jobs in the economic core undoubtedly contributes to women’s new access to management track jobs. For example, in 2014, the Cabinet Office introduced a website publicizing statistics about women’s representation in management and ratios of applicants to hires for men and women at thousands of firms (Ministry of Health, Labor, and Welfare 2016). This greater transparency likely discourages firms from discriminating in hiring and job placement (Castilla 2008; 2015). At the same time, however, firms that wish to skirt regulation and scrutiny often develop sophisticated strategies for doing so (Mun 2016; Abe 2014). If firms wished to maintain or strengthen men’s preferential access to the management track, they would undoubtedly be able to do so, despite government counterpressure. Women’s apparently unimpeded access to the management track thus implies that negative population growth does not necessarily lead advantaged persons to cling more tightly to their prerogatives.

A second cause for optimism is the high level of attainment of Westerners at Japanese firms. In these sample firms, Western employees earn more than Japanese people with similar jobs and skills, an advantage that widens over the course of their careers. Indeed, Japanese employees believe that Westerners *deserve* higher rewards for the same behaviors than do Japanese or other Asian employees. These findings stand in contrast to the popular wisdom of the 1980s and 1990s about prospects for Westerners in Japanese firms. During this period, Japanese firms operating in the United States settled lawsuits with Americans who

claimed that their employers had discriminated against them on the basis of nationality (Kilborn 1991). Contemporary observers suggested that, because Japanese management culture was insular and suspicious of outsiders, Japanese firms had constructed a “rice paper ceiling” limiting advancement opportunities for Americans and other non-Japanese (Kopp 2000). Either concerns about discrimination against Western employees in the 1980s and 1990s were overblown (e.g. Rapp 2002), or conditions for Western employees in Japanese firms have markedly improved since that time.

Both status construction theory (Ridgeway and Correll 2006) and intergroup contact theory (Allport 1954) predict that if women or ethnic minorities move into more powerful positions, attitudes towards members of these groups will become more positive. Consequently, in a best case scenario of declining population, formerly excluded persons will move into more powerful roles, perhaps simply due to labor shortages. Subsequently, however, their increased representation in elite jobs can also overturn mental models and hierarchies that lead people to categorize members of some groups as lower status, perhaps reaching a tipping point where discrimination no longer so disproportionately affects members of these groups, even if labor demand slackens. In this case, demographic decline may not just provide new opportunities to women and minorities, but may also fundamentally reshape the status hierarchy.

These analyses, however, give little reason to believe that Japan or other shrinking societies will follow this path. For example, within the management track, women continue to earn significantly less than men. Their performance is assessed more poorly, but even taking their lower (assessed) performance into account, they still earn considerably less than men with similar human capital characteristics. In fact, women’s lower wages *within* tracks account for a greater share of the gender pay gap in this context than does women’s overrepresentation in female-typed jobs. Discrimination against women thus persists in other

parts of the employment process, including in how performance is rewarded, and perhaps also in how it is assessed, even though firms do not exclude women from the management track, and the majority of women in the sample (57%) work in management track jobs. This implies that traditional gender status beliefs continue to have a major impact on employment outcomes.

The experience of East Asian foreign workers at Japanese firms also undermines optimism for the best case scenario. Although there is equal status contact between Japanese and other East Asian workers in these firms, Japanese and Westerners continue to hold anti-East Asian biases. Firm level aggregates of these biases show that coworker bias is also associated with relatively lower wages for East Asians, whose wage disadvantage compared to Japanese with similar human capital compounds with the length of time they work in Japanese firms. In Japan, bias against other East Asians has a history stretching back at least to the nineteenth century (Befu 2001; Oguma 2002). Current geopolitical tensions may exacerbate it as well (Kobayashi et al. 2014). Equal status contact in the white collar workplace has not undermined the influences of these more global processes.

It is possible, of course, that women's and East Asians' attainment of more prestigious positions in Japanese firms simply has not progressed far enough to overturn long-held attitudes towards members of these groups. The share of female managers in the study firms ranges from 3% to 34%. Clearly, even though women are not excluded from the management track, they still lag behind men in occupational attainment and in many cases have only token representation in the managerial ranks. In this case, as their structural attainment continues to improve, they may still be able to disrupt mental models of group status in the future. Alternatively, women's and East Asians' current levels of structural attainment may be high enough to change attitudes, but either the pace of attitudinal change is slow relative to disadvantaged groups' structural attainment, or not enough time has passed

for this attitudinal change to erase the impact of past discrimination.

Although these outcomes are possible, however, there are two reasons why this best case scenario still appears unlikely. The first is Western workers' surprisingly rapid incursions into the ranks of top management. On the face of it, their relatively poor Japanese skills, lack of familiarity with the Japanese context, and small share of the foreign population in Japan should make Westerners unlikely candidates for structural advancement in Japanese firms. That they have advanced so quickly and that their wages exceed even those of their Japanese counterparts suggests they have benefitted from *existing* attitudes among Japanese that privilege Western peoples and culture (see Oguma 2002), or perhaps have been able to use as leverage the promising employment opportunities that they enjoy in their home countries. Since Japanese women and East Asians are much less likely to have better employment options elsewhere, and since neither enjoys the same racial or ethnic privilege as Westerners, their opportunities have been relatively less. This pattern suggests that, even when negative population growth creates labor shortages, the most advantaged or high status (but not necessarily the most qualified) outsiders stand to benefit the most. This trend would tend to limit how quickly members of more disadvantaged groups can advance, and the consequent likelihood of fundamental status reordering.

A second reason to question the best case scenario applies mainly to women. Even if most women work in historically male positions, and women's managerial authority continues to grow, their overrepresentation is likely to persist in low status clerical jobs, simply because more women than men are willing to trade lower opportunities for greater flexibility (Zou 2015; Nemoto 2016). Although research on status construction focuses more on women's representation at the top of the occupational hierarchy, women's representation at the bottom may be equally or perhaps even more important to how people perceive women's status. Persistent female overrepresentation in low status jobs will thus lead to

continued devaluation of women's accomplishments even in more prestigious jobs and work against the countervailing pressures that could destabilize the gender hierarchy.

Together, the three papers of this dissertation address questions about patterns of inequality in the context of demographic decline. Will population decline slow opportunities for advancement for women and minorities, exacerbating inequality? Or will a shrinking population bring a new dawn of greater mobility for disadvantaged groups, perhaps even disrupting existing hierarchies? The analyses suggest that even a demographic cliff such as a Japan's does not lead insiders to hoard opportunities for themselves. Indeed, "good jobs" appear increasingly accessible to women and minorities. At the same time, however, the best of these dawning opportunities do not go to the groups who have historically faced greater disadvantage—in this context, women and East Asians—even when they are highly qualified. Further, existing status hierarchies continue to impact earnings even for those who work in high level positions. The most likely scenario is therefore that demographic decline will open new opportunities to women and minorities, but rejigger rather than upend the status ordering of pre-decline times.

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APPENDIX 1

DESCRIPTION OF VIGNETTES

Negative Vignettes

Tardiness

Names: Suzuki (Japanese), Pak (Korean), Li (Chinese), Brown (English)²³

_____ -san is an employee with 5 years of seniority. Recently, he did not come to work for two days, without requesting permission or informing anyone in advance. Now he is back at work. He apologized, but has not explained why he was absent. Since returning to work, he has been late to several departmental meetings, and to one meeting with clients.

Records Falsification

Names: Takahashi (Japanese), Pak (Korean), Li (Chinese), Brown (English)

Sato-san and _____ -san are responsible for entering the sales records of employees in their department into a computer database. Supervisors use the information in the database when they evaluate employees. One day, Sato-san needs to look up information that _____ -san entered the week before. He finds that _____ -san's entries do not match records kept elsewhere. Sato-san decides to check some of _____ -san's other work. He finds that, in fact, all _____ -san's entries for the past 8 weeks, and possibly even longer, are false. It appears that _____ -san exaggerated his own sales records and those of his friend. Sato-san tells his supervisor what he has discovered.

²³ Because respondent viewed, at most, one negative and one positive vignette with a non-Japanese name, I used the same non-Japanese names across the tardiness and records falsification vignettes, and the same non-Japanese names across the helping and saving vignettes. To avoid the same name appearing twice for one respondent, I used unique Japanese names in each of the four vignettes.

Follow-up Questions to Negative Vignettes

Q1 How should the supervisor respond?

Check what the supervisor should do. You may check more than 1 item.

The supervisor should not do anything.

The supervisor should have a discussion with _____-san about his behavior.

The supervisor should have a discussion with other employees in his section about _____-san's behavior.

The supervisor should have a discussion with other managers or HR about _____-san's behavior.

Q2 Should the supervisor or HR issue a formal warning or punishment for _____-san?

No

Yes

[Viewed by respondents who chose "Yes" for Q2]

Q3 What type of formal warning or punishment would be the most appropriate for _____-san?

Warning

One-time salary reduction

Unpaid suspension from work

Demotion

Firing

Other, please specify:

[Viewed by respondents who chose "One-time salary reduction" for Q3]

Q4 By what percent should _____-san's base salary be reduced, when he receives the one-time salary reduction?

Write the percentage below.

[Viewed by respondents who chose "Unpaid suspension from work" for Q3]

Q5 How many weeks unpaid suspension from work should Smith-san receive as punishment?

Write the number of weeks below.

Positive Vignettes

Saving Money

Names: Tanaka (Japanese), Kim (Korean), Wang (Chinese), Smith (English)

_____ -san has been assigned to negotiations with vendors that his company uses for business services. Recently, business costs have been rising, and his supervisor tells _____ -san that he should do his utmost to control the costs, even if it means breaking off relationships with long-term vendors and finding new ones. However, _____ -san successfully negotiates with his company's two largest existing vendors to lower their prices by 5%, while keeping the level of services the same. This keeps overall costs in control and means that employees at _____ -san's firm can continue working with the familiar vendors.

Helping Coworkers

Names: Ikeda (Japanese), Kim (Korean), Wang (Chinese), Smith (English)

It is the busiest season in the human resources department. Everyone is desperately trying to complete their work. However, _____ -san notices that Fujiwara-san, the newest member of their group, is really struggling. _____ -san offers to help Fujiwara-san, even though he is very busy himself. At first, Fujiwara-san tries to decline _____ -san's help, because he doesn't want to be a burden. Nonetheless _____ -san insists, and eventually, Fujiwara-san gratefully accepts his help.

Once the busy season is over, his supervisor congratulates Fujiwara-san on how well he did. Fujiwara-san explains that although he worked hard, it is really thanks to _____ -san that he was able to complete his job.

Follow-up Questions to Positive Vignettes

Q1 How should the supervisor respond?

Check what the supervisor should do. You may check more than 1 item.

- The supervisor should not do anything.
- The supervisor should privately tell _____ -san he did a good job.
- The supervisor should praise _____ -san to other members of the section.
- The supervisor should praise _____ -san to managers in other departments or to HR.
- The supervisor should recommend _____ -san for a higher than usual bonus.
- The supervisor should recommend _____ -san for a promotion.

[Viewed by respondents who chose “The supervisor should recommend _____-san for a higher than usual bonus.” for Q1]

Q2 By what percentage should _____-san's bonus be increased?

Write the percentage below.