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Declaration in Support
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SUPERIOR COURT OF THE STATE OF CALIFORNIA

COUNTY OF SAN MATEO

RONG JEWETT, SOPHY WANG, and XIAN MURRAY, on behalf of themselves, and ELIZABETH SUE PETERSEN, MARILYN CLARK, and MANJARI KANT, on behalf of themselves and all others similarly situated, Plaintiffs, v. ORACLE AMERICA, INC., Defendant.	Case No.: 17CIV02669 **EXHIBIT A: *CONDITIONALLY LODGED UNDER SEAL*** Redacted **DECLARATION OF DAVID NEUMARK IN SUPPORT OF PLAINTIFFS' MOTION FOR CLASS CERTIFICATION** Hearing Date: May 31, 2019 Hearing Time: 9:00 a.m. Location: Dept. 23 Assigned for all purposes to the Honorable V. Raymond Swope Complaint Filed: June 16, 2017 Trial Date: No date set

I, David Neumark, declare as follows:

1. I am Professor of Economics at the University of California—Irvine. I am a labor economist who has done extensive research on labor market discrimination, including methods for measuring and testing for discrimination that have been adopted by many other researchers.

2. I have published approximately 25 peer-reviewed journal papers on discrimination based on race, ethnicity, sex, or age, in journals including *American Economic Review*, *Contemporary Economic Policy*, *Economic Journal*, *Industrial Relations*, *Industrial and Labor Relations Review*, *International Economic Review*, *Journal of Human Resources*, *Journal of Labor Economics*, *Journal of Policy Analysis and Management*, *Review of Economics and Statistics*, *Journal of Political Economy*, and *Quarterly Journal of Economics*, as well as other studies in edited books, and a full-length book on sex discrimination and sex differences in labor markets (based on my papers). The goal of much of this research is to better understand the role of discrimination versus other explanations of differences in labor market outcomes by race, ethnicity, sex, or age.

3. As a labor economist, most of my work involves statistical and econometric analysis of data. As examples, several of my research papers on discrimination focus on the development of new statistical techniques to measure and test for labor market discrimination. And my graduate courses in labor economics focus heavily on econometric methods.

4. I have previously held positions at the Federal Reserve Board, the University of Pennsylvania, Michigan State University, and the Public Policy Institute of California. I am a research associate of the National Bureau of Economic Research, and a research fellow at IZA (the Institute for the Study of Labor) in Germany. I also direct the Economic Self-Sufficiency Policy Research Institute (ESSPRI) at UC—Irvine.

5. I have been asked by counsel for plaintiffs to consider certain issues in conjunction with plaintiffs' motion for class certification.

6. Attached hereto is a true and correct copy of the report I wrote setting forth my opinions and the basis for those opinions.

I swear under penalty of perjury under the laws of the State of California that the

foregoing is true and correct. This declaration is executed in Irvine, CA, on January 16, 2019.

By: _____
David Neumark

EXHIBIT A

REDACTED - PUBLIC VERSION

Expert Report of

David Neumark

in the matter of

Jewett et al. v. Oracle America, Inc.

January 2019

I. Introduction

1. I am David Neumark, Professor of Economics at the University of California—Irvine. I am a labor economist who has done extensive research on labor market discrimination, including methods for measuring and testing for discrimination that have been adopted by many other researchers. I have published approximately 25 peer-reviewed journal papers on discrimination based on race, ethnicity, sex, or age, in journals including *American Economic Review*, *Contemporary Economic Policy*, *Economic Journal*, *Industrial Relations*, *Industrial and Labor Relations Review*, *International Economic Review*, *Journal of Human Resources*, *Journal of Labor Economics*, *Journal of Policy Analysis and Management*, *Review of Economics and Statistics*, *Journal of Political Economy*, and *Quarterly Journal of Economics*, as well as other studies in edited books, and a full-length book on sex discrimination and sex differences in labor markets (based on my papers). The goal of much of this research is to better understand the role of discrimination versus other explanations of differences in labor market outcomes by race, ethnicity, sex, or age.

2. As a labor economist, most of my work involves statistical and econometric analysis of data.[1] As examples, several of my research papers on discrimination focus on the development of new statistical techniques to measure and test for labor market discrimination.[2] And my graduate courses in labor economics focus heavily on econometric methods.

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[1] Econometrics is best thought of as a branch of statistics that focuses on using statistics to understand economic data and phenomena.

[2] Key examples include: Neumark, David, 2012, "Detecting Evidence of Discrimination in Audit and Correspondence Studies," Journal of Human Resources, 53(4), pp. 1128-57; Hellerstein, Judith K., David Neumark, and Kenneth Troske, 1999, "Wages, Productivity, and Worker Characteristics: Evidence from Plant-Level Production Functions and Wage Equations," Journal of Labor Economics, 17(3), pp. 409-446; and Neumark, David, 1988, "Employers' Discriminatory Behavior and the Estimation of Wage Discrimination," Journal of Human Resources, 23(3), pp. 279-295.

3. Appendix F provides an abridged CV listing my publications from the past 10 years. Appendix G details my expert witness work in the last 4 years.

4. I have previously held positions at the Federal Reserve Board, the University of Pennsylvania, Michigan State University, and the Public Policy Institute of California. I am a research associate of the National Bureau of Economic Research, and a research fellow at IZA (the Institute for the Study of Labor) in Germany. I also direct the Economic Self-Sufficiency Policy Research Institute (ESSPRI) at UC—Irvine.

5. I have been retained by the plaintiffs as a statistical expert to evaluate claims of pay discrimination against women at Oracle America, Inc. I am compensated at the rate of $495 per hour.

6. This analysis is based on my current understanding of the data with which I have been provided from Oracle. These data files are listed and described in Appendix E. I have developed my understanding of Oracle's data and their pay policies based on numerous documents also listed in Appendix E. It is possible that new information I obtain may cause me to revise this report.

II. Questions I Consider

7. I have been asked by Counsel for the Plaintiffs to answer the following questions:

   a. How many women were employed by Oracle in California at any time between June 16, 2013, and the present, in the Product Development, Information Technology, or Support Functions, excluding Campus Hires ("Covered Positions")?

   b. Compare the compensation (including base pay, Medicare wages, total compensation, bonuses, and stock grants) of men and women in the Covered Positions who were performing substantially equal work in jobs the performance of which required

substantially equal skill, effort, and responsibility, performed under similar working conditions.

c. If there are statistically significant disparities in compensation in answer to the question above, were the disparities explained by any of the following: (a) a seniority system; (b) a merit system; (c) a measure of the quality or quantity of production; or (d) other job-related factors?

d. For persons who came to Oracle from other jobs, what relationship is there, if any, between their pay at their jobs immediately before joining Oracle and their initial pay at Oracle? If there is a relationship, does it generate a gender disparity in initial pay? If so, is that disparity in base pay corrected over the course of one's employment?

e. Did Oracle compensate the three proposed class representatives less than men performing substantially equal work in jobs the performance of which required substantially equal skill, effort, and responsibility, performed under similar working conditions? If so, were these disparities explained by job-related factors?

III. Summary of Conclusions

8. The summary of my conclusions is as follows:

a. Across three functions in question, in the period 2013-2018, and excluding the Campus Hires, there were 3,461 women employed in Product Development, 422 women employed in Information Technology, and 318 women employed in Support, for a total of 4,201 women in these three functions. (Exhibit 1)[3]

b. There are statistically significant gender disparities in compensation. Looking across base pay, Medicare wages, total compensation, bonuses, and stock grants, women

[3] The exhibits noted in this summary are explained in greater detail below.

received statistically significantly lower compensation than men who were, based on the data available, performing substantially equal work in jobs the performance of which required substantially equal skill, effort, and responsibility, performed under similar working conditions. For the purposes of these analyses, I have treated persons in the same job code and grade as performing substantially equal or similar work, which is how Oracle treats such persons; that treatment is consistent with the practice of studying labor market discrimination in labor economics.[4] (Exhibit 2)

i. The estimated gender gap in base pay using my preferred specification and sample is approximately 3.8%. This represents a difference of 14.7 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

ii. The estimated gender gap in Medicare wages using my preferred specification and sample is approximately 6.3%. This represents a difference of 12.9 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

iii. The estimated gender gap in bonuses using my preferred specification and

[4] For two examples of studies that interpret gender differences in pay within job cells (in this case, occupation-by-employer cells), see: Groshen, Erica L., 1991, "The Structure of the Female/Male Wage Differential: Is It Who You Are, What You Do, or Where You Work?" Journal of Human Resources, 26(3), pp. 457-72; and Bayard, Kimberly, Judith Hellerstein, David Neumark, and Kenneth Troske, 2003, "New Evidence on Sex Segregation and Sex Differences in Wages from Matched Employer-Employee Data," Journal of Labor Economics, 21(4), pp. 887-922.

sample is approximately 13.2%. This represents a difference of 4.2 standard deviations, which is sufficiently large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 10 thousand.

iv. The estimated gender gap in stock grants using my preferred specification and sample is approximately 33.1%. This represents a difference of 5.3 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 million.

v. The estimated gender gap in total compensation using my preferred specification and sample is approximately 5.8%. This represents a difference of 13.7 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

c. My understanding is that Oracle does not use a seniority system or set compensation by a measure of the quality or quantity of production.[5] Based on my analysis of the data, these statistically significant disparities are also not explained by other job-related factors. Job definition, tenure at Oracle, tenure in position, job performance, years of job experience, and location of work site do not explain these statistically significant gender compensation disparities.

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[5] This is based on the absence of references to such factors in *Waggoner Exhibit 24.pdf*, which discusses principles underlying compensation at Oracle, beginning at *ORACLE_JEWETT__00000616*, *Waggoner Exhibit 53.pdf*, beginning at *ORACLE_JEWETT__00006609*, and the transcript of Ms. Waggoner's deposition, *WAGGONER, KATE, Condensed VA (07.26.2018).pdf*, at 207-208.

d. Oracle employees in the class generally came to Oracle from another job (i.e., they came to Oracle as a result of Oracle acquiring the company in which they were working, or they came to Oracle laterally from a job at another company). I have a measure of pay on the prior job, and the gender disparity in starting base pay mirrors the gender disparity in their prior pay. Indeed, prior pay and starting pay at Oracle are very strongly related. A person's prior pay is highly predictive of that person's initial salary at Oracle. On average, prior pay being higher by $1 predicts that starting pay is higher by $.75. Alternatively, prior pay explains 74% of the variation in starting pay. The likelihood that this strong relationship between prior pay and starting pay occurs by chance is less than 1 in 1 billion, as reflected in a t-statistic on the coefficient on prior pay in the starting pay regression of 89.9 (or an effect of prior pay of 89.9 standard deviations). Most importantly, this initial gender gap in starting pay drives the gender gap in base pay that I observed during the Class Period; the magnitude of the gender gap in base pay is similar during the Class Period and in the data on starting pay. (Exhibit 3)

e. The three proposed class representatives in this case (i.e., Marilyn Clark, Manjari Kant, and Sue Peterson) received compensation lower than comparable male employees. The estimated compensation shortfalls range from a 13.2% to 24.9% shortfall in base pay, and a 21.9% to 35.7% shortfall in Medicare wages. The estimated base pay and Medicare wages shortfalls are highly statistically significant for all three proposed class representatives.[6]

IV. Empirical Approach

[6] These regression estimates correspond to regression estimates described later in the report; for the overall sample. See, in particular, Exhibits 14, 19, 24, 29, and 34.

9. My analysis compares compensation at Oracle – measured a number of ways – for female and male Oracle employees who do similar jobs requiring similar skills (i.e., persons in the same job codes and grades). To estimate whether there are gender disparities consistent with gender discrimination in pay, I estimate regression models for pay. The data used in these models are records for individuals in specific years. These records include different pay measures. They also include an indicator for the gender of an employee. And, importantly, they include many measures of the type of job a person at Oracle does, and the skills and experience of that person.

10. A regression model estimates the gender gap in pay once we adjust for possible differences between female and male employees that could account for this pay gap. For example, suppose that we simply compare average pay of all female and male employees at Oracle, and find that average pay of female employees is 10% lower. It is possible that women do different jobs, and those jobs could pay less. It is also possible that women and men are in broadly similar jobs, but the women have lower skills (e.g., less experience in the job). In either case, our intuition would be that the 10% estimate overstates the pay gap for comparable women and men in comparable jobs, and we should hence adjust for these differences between women and men before estimating the gender gap in pay.

11. This is precisely what a regression model does. A regression model "holds constant" or "controls for" these other factors. These phrases mean that, in estimating a regression model, we adjust the pay gap for differences in the jobs employees hold, and the skills they have, so that we are comparing pay between women and men in similar jobs with similar skills. In the example above, it is possible that the 10% gender disparity is fully explained by these other factors, in

which case the estimated gender pay gap from the regression would be zero.[7] Thus, my analysis asks – in a detailed manner making extensive use of data provided by Oracle, and data I created from other sources of information on Oracle employees, produced by Oracle in discovery – whether other factors such as tenure, experience, or performance can explain any pay gaps for gender that I find.

12. If there is evidence that women are paid less than comparable men from the regression estimates, this evidence is consistent with pay discrimination against women. This conceptualization of pay discrimination is standard in the labor economics literature, beginning with the seminal work of Becker (1957),[8] who defined discrimination in pay as unequal pay for equally productive workers. The use of regression models like those I describe above to estimate gender disparities in pay, in order to assess whether there is evidence consistent with pay discrimination, is pervasive in economics, with scores if not hundreds of papers written in recent decades.[9]

13. The regression models I detail in this report provide estimates of the approximate percent difference in pay between women and men. It is common in the labor economics research literature to use regression models for pay that estimate the effects of different variables – most importantly, in this case, gender – on the percentage difference in pay rather than the absolute

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[7] It is important to point out, though, that it is also possible that the estimated gender pay gap would be larger than 10%, if women are on average in higher-paying jobs or have higher skills. We cannot know, before looking at the data and estimating the regression model, whether other factors controlled for in the regression will lead to a lower or a higher estimated gender gap in pay.

[8] Becker, Gary S. 1957. The Economics of Discrimination. Chicago: University of Chicago Press.

[9] See, for example: Altonji, Joseph G., and Rebecca M. Blank. 1999. "Race and Gender in the Labor Market." In Ashenfelter and Card, eds., Handbook of Labor Economics, Vol. 3, Part C, pp. 2943-3630. Amsterdam: Elsevier.

difference.[10] This convention, and the reasons for it, goes back to the original development of the earnings regression in labor economics (Mincer, 1974).[11] This is usually done by measuring pay in terms of the "natural logarithm," in which case the coefficient estimates approximate percentage differentials. However, in this report I use a closely-related measure, called the "inverse hyperbolic sine," or IHS. I do this because part of my analysis looks at data on bonuses or stock grants, which are often equal to zero for some people in some years (whereas base pay is never equal to zero).[12] The IHS has the same property of leading to regression models that estimate the gender gap in pay in terms of approximate percentages, but can accommodate zero values.[13]

14. Finally, while my regression models estimate a gender gap in pay, we also have to ask whether the estimated gender gap is "statistically significant." It is possible that there is no systematic gender difference in setting pay, so that the true gender difference in the process of setting pay is zero, but randomness in the data in estimating the gender gap in pay yields an estimate that is different from zero. The statistical significance of an estimate tells us how likely it is that we would have obtained the estimated gender gap in pay if in fact the true effect of gender on pay was equal to zero.

15. Intuitively, we might think that a very small estimated gap in pay (say, 0.3%, or three-tenths of a percentage point) might reflect statistical error, whereas a larger estimate gap (say,

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[10] For example, if a woman earns $9,000 and a man earns $10,000, the absolute differences in pay is a $1,000 pay disparity, and the percentage difference for women relative to men is 10% ($100/$1,000).

[11] Mincer, Jacob. 1974. Schooling, Experience, and Earnings. Cambridge: National Bureau of Economic Research, Inc.

[12] In contrast, the natural logarithm is not defined at zero.

[13] See: Ravallion, Martin. 2017. "A Concave Log-like Transformation Allowing Non-positive Values." *Economics Letters*, 161(C), pp. 130-32.

10%) seems much less likely to have arisen by chance from randomness. To assess this formally, statisticians compute the "standard deviations" of an estimate – in this case, the estimated gender gap in pay – and summarize the estimated gender gaps in pay in terms of "standard deviations." This standard deviations metric is used to test whether the measured difference in pay between women and men is statistically significant and differs from a hypothetical null hypothesis of gender-neutral pay setting – i.e., no difference in pay between women and men – which is what we would expect in the absence of discrimination. The more standard deviations from the null hypothesis of zero that the estimated pay gap is, the less likely it is that the estimated gender gap in pay is due to chance, as opposed to a systematic difference in pay between women and men.

16. For purposes of comparison, a difference of 1.96 standard deviations would be statistically significant at the 5% level, meaning that the likelihood of observing this value if compensation was neutral with respect to gender is 1 in 20. A difference of 2.58 standard deviations would be statistically significant at the 1% level, meaning that the likelihood of observing this value if compensation was neutral with respect to gender is 1 in 100 (1%). Similarly, the likelihood of observing a difference of more than 3.30 standard deviations would be less than 1 in 1,000. A disparity of two standard deviations is generally sufficient to show that a result is extremely unlikely (less than a 5% probability) to be caused by chance. Labor economists generally regard any disparity of two or more standard deviations to be "statistically significant."

V. Data

17. As the preceding discussion implies, my analysis requires data on Oracle employees covering the following items: gender, compensation, worker characteristics, and job

characteristics. In this section, I briefly note the data I use. Appendix A provides a more detailed discussion of the data, along with tabulations by gender.

18. The measurement of pay is quite complex, as there are alternative measures available in the data. One measure I use is the base rate of pay. The advantage of a base rate of pay measure is that it is easy to interpret, and does not need to be adjusted for other factors that can affect annual take-home pay, such as partial years worked. The disadvantage is that it does not capture total compensation from Oracle, as, for example, compensation can also come in the form of bonuses or stock grants.

19. Exhibit 5 presents simple summary statistics on base pay from this source. The exhibit shows that both median (i.e., the number at which half the population is above and half below), and mean (i.e., the average) base pay are lower for women than for men at Oracle, for every year in the Class Period. Exhibit 6 instead summarizes these data in a histogram. The histogram shows clearly that the distribution of base pay for women is shifted to the left relative to that the distribution of pay for men. That is, the mean or median differences reflected in Exhibit 5 are reflected through the distribution of base pay.

20. An alternative pay measure I use is "Medicare wages." To my understanding, this is taxable earnings reported to the IRS, and hence will capture variation due to factors such as part-year work, bonuses, etc. The potential advantage of Medicare wages relative to base pay is that Medicare wages should capture the full amount of taxable earnings in a year. However, unlike for base pay, Medicare wages may not be comparable across employees unless these wages are adjusted for partial-years worked. I address this issue by adjusting Medicare wages based on days worked in a year, converting the yearly measure of Medicare wages for each employee and year to a "full-year equivalent" measure. I adjust bonuses and stock grants (discussed below) in

the same manner.

21. Exhibit 7 presents simple summary statistics on Medicare wages from this source. The exhibit shows that both median and mean Medicare wages are lower for women than for men at Oracle, for every year in the Class Period. Exhibit 8 instead summarizes these data in a histogram. The histogram shows clearly that the distribution of Medicare wages for women is shifted to the left relative to that of men. Note that I only use Medicare wages through 2017, since I do not have the complete tax year's data for 2018, nor can I adjust for partial years worked. In contrast, I use data on base pay through 2018, because I do not need the full year of data for 2018 to measure base pay. For the other compensation measures discussed below, I also only use data through 2017, for the same reason.[14]

22. I also consider non-salary components of pay, including bonuses and stock grants. I aggregate total bonuses by employee and year, and compute bonuses both unadjusted, and adjusted for partial years worked, as I did for Medicare wages. Exhibit 9 presents simple summary statistics on adjusted yearly bonuses. The exhibit shows that across all years, mean bonuses are lower for women than for men at Oracle.[15]

23. Exhibit 10 reports simple summary statistics on for total stock grants (Restricted stock grants (RSUs) + Options). The exhibit shows that mean total stock grants are lower for women than for men at Oracle in every year.

24. Finally, I also examine results for "total compensation." In principle, total compensation should equal Medicare wages, but it is not quite the same in the data, so I present results for both

[14] I would be able to update the analysis through 2018 for all pay measures if I am provided with 2018 data.
[15] I do not show histograms for bonuses and stock grants because most of the observations are clustered at $0.

measures to ensure that results are not sensitive to which measure I use. This total compensation measure has to be adjusted for partial years worked, as for Medicare wages.

25. Exhibits 11 and 12 report simple comparisons for adjusted constructed total compensation. Exhibit 11 shows that both the median and mean total compensation are lower for women than for men at Oracle in every year, and across all years. Exhibit 12 instead summarizes these data in a histogram. The histogram shows clearly that the distribution of compensation for women at Oracle is shifted to the left relative to men.

26. My regression analysis of gender differences in pay at Oracle makes use of many control variables intended to compare similarly-qualified women and men in similar jobs.

27. I first control for differences in jobs. Job code fully encompasses title, function, specialty area, and global career level.[16] However, job code does not capture job grade, part-time status, hourly status, FLSA status, or work location. I include a full set of job code-job grade interactions (i.e., a separate dummy variable for each job code-job grade pair). In the three functions in question – product development, information technology, and support, there are 218 job codes, 27 grades, and 265 unique job code-job grade interactions. I also include dummy variables for part-time status, hourly status, and work location (zip code).[17,18] Job level, specialty, function, title, and exempt status are not included as separate control variables because they are redundant with the job code-job grade interactions. Including this highly-detailed set of controls in my regression model allows me to compare women's and men's pay within very

[16] Document *ORACLE_JEWETT_00000651.*

[17] I include the location variable to account for both differences in pay and differences in the nature of work across geographic regions.

[18] A "dummy variable" is a variable that takes on values of 0 or 1 depending on whether an observation has a particular non-quantitative characteristic (e.g., 1 for part-time and 0 for full-time, or 1 for women and 0 for men). The estimated coefficient on a dummy variable measures the shift in the dependent variable – the outcome – with that characteristic.

narrowly-defined jobs. Finally, I add controls for the Line of Business.

28. I also construct a number of controls for characteristics of workers. A long literature in labor economics, based on the human capital model, establishes that labor market experience and job tenure are related to workers' productivity, as they reflect workers' accumulation of skills and knowledge acquired in the workforce and on the job.[19] I therefore use the data to construct a measure of tenure at Oracle, including an alternative measure that includes tenure at companies that became part of Oracle through a merger or acquisition (M&A). I also construct a measure of the time a person has been in a specific job role.

29. I do not have data from Oracle with which to construct total labor market experience (which consists of prior labor market experience plus tenure at Oracle). Instead, I use the Oracle data to construct an approximate measure based on what labor economists call "potential experience," which is a measure of how much experience a person would have if they worked continuously since leaving school.

30. It would be better to have a measure of actual work experience. In addition, we know, from an extensive literature in labor economics, that schooling is associated with higher productivity and hence higher wages.[20] While neither measure was available from machine readable data provided by Oracle,[21] I was able to recover information on education, and on work

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[19] The seminal work on this topic is Mincer, 1974, Schooling, Experience, and Earnings Cambridge, MA: National Bureau of Economic Research, Inc.

[20] For work that confirms the causal effect of schooling on earnings, see Card, David, 1999, "The Causal Effect of Education on Earnings," in Ashenfelter and Card, Eds., Handbook of Labor Economics, Vol. 3 (Amsterdam: Elsevier), pp. 1801-63. For direct evidence that the effect of education on earnings reflects an impact on productivity, see Hellerstein, Judith K., David Neumark, and Kenneth Troske, 1999, "Wages, Productivity, and Worker Characteristics: Evidence from Plant-Level Production Functions and Wage Equations," *Journal of Labor Economics*, pp. 409-46.

[21] There is only very limited information on highest educational degree earned in the file

experience prior to coming to Oracle (which, together with time at Oracle, adds up to total labor market experience), from resumes produced by Oracle in discovery.[22] Given that the data I constructed did not come directly from Oracle, I present analyses using these variables as a supplemental analysis to my main analysis. The findings from these analyses are consistent with those from the main analysis.

31. I also consider adding a control for job performance. I have some reservations about doing this. First, as a general matter, performance measures can be tainted, with the same behavior underlying pay discrimination against women also leading to lower – and biased – performance ratings for women. At Oracle, this concern is validated because women receive lower performance review scores than men on the informal scores used to set compensation, even though their formal appraisal scores are almost identical to those of men. See Appendix Exhibit A.13. Second, to the best of my understanding there is not a regular performance appraisal process at Oracle; managers are not required to give formal performance appraisals, and frequently do not do so. It is not clear when or why an employee receives a rating; nor is it clear how to interpret the absence of a rating.[23] Third, there a number of data issues discussed in Appendix A, including missing data. As explained in that appendix, there are also potential differences between performance ratings that result from different systems of performance appraisal used at Oracle – one formal, and one informal. I present analyses that examine whether

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ORACLE_JEWETT_00001180_EmpQual.csv.

[22] Labor economists do not routinely have resume data available to use in studies of pay. However, I have used information scraped from resumes in other work I have done, in particular, for a field experiment on age discrimination in hiring. See Neumark, David, Ian Burn, and Patrick Button, "Is it Harder for Older Workers to Find Jobs? New and Improved Evidence from a Field Experiment." Forthcoming in *Journal of Political Economy.*

[23] *[Oracle-Jewett] 2018.10.25 Ltr to Finberg from Mantoan re 2018.10.11 Data Production Questions* acknowledges that not all employees receive a performance rating.

the different kinds of performance appraisals lead to differences in findings, but I do not regard the analyses including performance ratings as my main analyses. Nonetheless, the findings from incorporating the performance appraisals are consistent with my main analysis.

VI. Analysis of Gender Differences in Pay

32. I now turn to the regression estimates of gender gaps in pay at Oracle. I restrict my regression analysis of gender differences in pay to the Class Period (2013-2018). I also restrict my analysis to the Information Technology, Product Development, and Support Functions. In all regressions, I adjust compensation measures based on the CPI, to adjust for inflation. All dollar figures are measured in 2017 dollars.[24]

A. Base rate of pay

33. I first report regressions for the base rate of pay, in Exhibit 13. In column (1) of the exhibit, I report the estimated difference in base pay, by gender, with no control variables added; this is an estimate of the raw difference. The estimate of −0.1469 implies that base pay of women at Oracle is approximately 15% less than that of men. The difference is strongly statistically significant, so that the measured gender gap in base pay represents a difference of over 27 standard deviations. The probability of observing a difference this large in the data if in fact there was no gender gap in pay is less than 1 in 1 billion. To provide some perspective, as noted earlier a difference of two standard deviations is usually viewed as providing statistically significant evidence of a gap, with the probability of observing a difference this large in the data if in fact there was no gender gap in pay then equal to 5%, or 1 in 20.

[24] The base year used makes no difference in the regressions I estimate, because they are estimated for compensation units (the inverse hyperbolic sine) that measure compensation in relative terms. However, for calculations of damages, which I report below, we want to inflate to the present.

34. The estimates in column (2) add control variables that capture job characteristics. These include controls for the variables I described above, to provide an estimate of the gender gap in pay for similar workers in similar jobs. As the exhibit notes state, these controls are: a full set of interactions between dummy variables for job code and job grade; dummy variables for work location; dummy variables for LOB head; and dummy variables for hourly or part-time status. To repeat what I wrote above, job level, specialty, function, title, and exempt status are not included as separate control variables because they are redundant with the job code-job grade interactions; that is, including the job code-job grade interactions already captures these other differences across jobs. In fact, the controls included – the full set of interactions – are actually more detailed then simply introducing a separate set of dummy variables for each of these job controls separately, because the full set of interactions disaggregates jobs more finely.

35. With the job controls added (column (2)), the estimated gender gap in base rate of pay declines to 5.2%. The smaller estimate relative to column (1) reflects more men working in higher-paying jobs. Nonetheless, the estimated gender gap in pay remains strongly statistically significant. The estimated sex gap in base pay is 18.7 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

36. I next turn, in Exhibit 14, to regression models that add the worker controls discussed above, including job tenure, Oracle tenure, overall tenure, and experience. It is common in labor economics research to include squares of these kinds of variables as well (Mincer, 1974), but including them did not materially change the estimated gender gap in pay, so I report models without these additional variables. When I use these controls, there are some observations with missing data. To isolate the effect of adding the variables from the effect of adding the controls,

in column (1) I repeat the last estimates from the previous exhibit, and then in column (2) I report the estimates for this same specification, but for the subsample for which I have data on these controls. (In this case, I only lose a few hundred observations.) I then add the controls, in columns (3)-(6). The exhibit shows that with the worker controls added, the estimated gender gap in the base rate of pay falls from about 5.2% to 3.8%. The estimated gap in the fullest model – in column (6) – still indicates a strongly statistically significant gender gap in pay. The estimated sex gap in base pay is 14.7 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

37. Next, I consider adding performance ratings. In this case, there is a substantial number of observations with missing data. Again, to isolate the effect of adding the variables from the effect of adding the controls, in column (1) of Exhibit 15, I repeat the last estimates from the previous exhibit, and then in column (2) I report the estimates for this same specification, but for the subsample for which I have data on performance ratings. (In this case, I lose around 21,000 observations.) I then add the controls, in column (3). The exhibit shows that adding performance ratings has no impact on the estimated gender gap in the base rate of pay. It remains 3.8%, and is still strongly statistically significant. The estimated sex gap in base pay is 14.1 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

38. Exhibit 16 compares the results using the formal and the informal performance appraisal ratings. Aside from that, the regressions are the same as in the last column of Exhibit 15. The exhibit shows that the results are not materially affected by using different performance

appraisals. When I use the formal appraisal ratings, the estimated sex gap in base pay is 3.3%, or 9.5 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion. When I use the informal appraisal ratings, the estimated sex gap in base pay is 4.0%, or 14.3 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

39. I next report estimates disaggregated by year, in Exhibit 17. For these disaggregated regressions I use the larger sample, without performance ratings (which did not affect the estimated sex gap in pay anyway). The estimated gender gap in pay is very stable across the years. It ranges from 3.8% to 3.9%, and from 11.4 to 12.9 standard deviations. For each year considered in isolation, these estimated standard deviations are so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

B. Medicare wages

40. I next repeat exactly the same analyses, in Exhibits 18-22, for Medicare wages. Given that the content of the tables is identical to the tables I just discussed for base rate of pay, I simply summarize the results briefly. The conclusions are qualitatively similar as for base pay, although the estimated gender gap in pay using Medicare wages is always larger.

41. Exhibit 18 shows that the estimated gender gap in pay with the job controls added is approximately 6.7%. The estimated sex gap in Medicare wages is 13.8 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

42. Adding worker controls in Exhibit 19 diminishes this estimated gender gap only slightly, with the estimate with all the controls falling to 6.3%. This estimated sex gap in base pay is 12.9 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

43. Adding the performance ratings (Exhibit 20) again has virtually no impact on the estimates. The estimated gender gap is 6.1%. This estimated sex gap in base pay is 12.6 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

44. Exhibit 21 compares the results using the formal and the informal performance appraisal ratings. The exhibit shows that the results are not materially affected by using different performance appraisals, although in this case the difference from using the different performance appraisal ratings is a bit larger than for base pay. When I use the formal appraisal ratings, the estimated sex gap in base pay is 4.8%, or 8.8 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion. When I use the informal appraisal ratings, the estimated sex gap in base pay is 6.4%, or 12.0 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

45. I next report estimates disaggregated by year, in Exhibit 22. For these disaggregated regressions I use the larger sample, without performance ratings (which did not affect the estimated sex gap in pay anyway). The estimated gender gap in pay is very stable across the

years. It ranges from 5.8% to 7.1%, and from 6.8 to 9.4 standard deviations. For each year considered in isolation, these estimated standard deviations are so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

C. Bonuses

46. I next repeat exactly the same analyses, in Exhibits 23-27, for adjusted bonuses. Again, given that the content of the tables is identical to the tables I discussed above for base rate of pay, I simply summarize the results briefly.

47. Exhibit 23 shows that the estimated gender gap in bonuses with the job controls added is about 17.2%. The estimated sex gap in bonuses is 5.4 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in bonuses with these control variables is less than 1 in 10 million.

48. Adding worker controls in Exhibit 24 diminishes this estimated gender gap somewhat, with all the controls falling to 13.2%. This estimated sex gap in bonuses is 4.2 standard deviations, which is sufficiently large that the probability of observing a difference this large in the data if in fact there was no gender gap in bonuses with these control variables is less than 1 in 10 thousand.

49. Adding the performance ratings (Exhibit 25), but also restricting the sample to observations with available data on these ratings, leads to a larger estimated gender gap (but the addition of the performance ratings, in isolation, has virtually no impact, as the comparison between columns (2) and (3) indicates). The estimated gender gap is 15.2%. This estimated sex gap in base pay is 4.3 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in bonuses with these control

variables is less than 1 in 10 thousand.

50. Exhibit 26 compares the results using the formal and the informal performance appraisal ratings. The exhibit shows that the results are not materially affected by using different performance appraisals. When I use the formal appraisal ratings, the estimated sex gap in base pay is 12.5%, or 3.0 standard deviations, which is sufficiently large that the probability of observing a difference this large in the data if in fact there was no gender gap in bonuses with these control variables is less than one in one hundred (0.31%). When I use the informal appraisal ratings, the estimated sex gap in bonuses is 14.9%, or 3.7 standard deviations, which is sufficiently large that the probability of observing a difference this large in the data if in fact there was no gender gap in bonuses with these control variables is less than 1 in 1 thousand (0.02%).

51. I next report estimates disaggregated by year, in Exhibit 27. For these disaggregated regressions I use the larger sample, without performance ratings (which did not affect the estimated sex gap in pay anyway). The estimated gender pay gap in terms of bonuses is large and statistically significant in 2013, 2016, and 2017. For these years, the estimated gender gap in pay ranges from 13.3% to 33.0%, and from 2.5 to 4.8 standard deviations. For the other two years, the estimated gender gap is smaller and not statistically significant, but in 2015 the estimate still indicates that women receive lower bonuses.

D. Stock grants (RSUs + options)

52. I next repeat exactly the same analyses, in Exhibits 28-32, for stock grants (RSUs and options) bonuses. Again, given that the content of the tables (the different controls, the alternative disaggregations, etc.) are identical to the tables I discussed above for base rate of pay and other compensation measures, I simply summarize the results briefly.

53. Exhibit 28 shows that the estimated gender gap in stock with the job controls added is about 8.9%. The estimated sex gap stock is 1.4 standard deviations.

54. Adding worker controls in Exhibit 29 substantially increase the size and the statistical significance of this estimated gender gap. With all the controls added (column (6)), the estimated sex gap in stock grants is about 33.1%. This is an estimate of 5.3 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 million.

55. Adding the performance ratings (Exhibit 30), but also restricting the sample to observations with available data on these ratings, leads to a larger estimated gender gap (but the addition of the performance ratings, in isolation, has virtually no impact, as the comparison between columns (2) and (3) indicates). The estimated gender gap is about 38.6%.[25] This estimated sex gap in base pay is 5.8 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 100 million.

56. Exhibit 31 compares the results using the formal and the informal performance appraisal ratings. The exhibit shows that the results are not materially affected by using different performance appraisals, although in this case the difference from using the informal performance appraisal ratings is larger. When I use the formal appraisal ratings, the estimated sex gap in stock is approximately 22.9%, or 2.6 standard deviations, which is sufficiently large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is about 1 in 1 one hundred (1.03%). When I use the informal

[25] When estimates get above 10% or so, the IHS estimate (or the log estimate) does not provide as good an approximation to the percent change.

appraisal ratings, the estimated sex gap in stock is approximately 45.2%, or 6.4 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

57. I next report estimates disaggregated by year, in Exhibit 32. The qualitative result that there is a significant gender gap disadvantaging women is the same in each year. The estimated gender gaps range from about 25.6% to 50.8%, and the standard deviations range from 1.9 to 5.0.[26]

E. Total compensation

58. Finally, I combine all forms of pay, and repeat exactly the same analyses, in Exhibits 33-37, now for total compensation. Again, given that the content of the tables (the different controls, the alternative disaggregations, etc.) are identical to the tables I discussed above for base rate of pay, I simply summarize the results briefly.

59. Exhibit 33 shows that the estimated gender gap in pay with the job controls added is approximately 6.1%. The estimated sex gap in total compensation is 14.6 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

60. Adding worker controls in Exhibit 34 diminishes this estimated gender gap only slightly, with the estimate with all the controls falling to 5.8%. This estimated sex gap in total compensation is 13.7 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

[26] In Appendix B, I show that the results for bonuses and stock grants are robust to omitting from the analysis job code-job grade cells in which there are neither bonuses nor stock grants.

61. Adding the performance ratings (Exhibit 35) again has virtually no impact on the estimates. The estimated gender gap is 6.0%. This estimated sex gap in total compensation is 14.2 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

62. Exhibit 36 compares the results using the formal and the informal performance appraisal ratings. The exhibit shows that the results are not materially affected by using formal or informal performance appraisals. When I use the formal appraisal ratings, the estimated sex gap in total compensation is 5.2%, or 11.0 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion. When I use the informal appraisal ratings, the estimated sex gap in total compensation is 6.3% or 13.6 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

63. I next report estimates disaggregated by year, in Exhibit 37. For these disaggregated regressions I use the larger sample, without performance ratings (which did not affect the estimated sex gap in pay anyway). The estimated gender gap in pay is very stable across the years. It ranges from 5.1% to 6.4%, and from 7.2 to 11.4 standard deviations. For each year considered in isolation, these estimated standard deviations are so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 billion.

VII. Analysis of the Relationship between Starting Pay and Prior Pay

64. I next turn to the analysis of the relationship between prior pay and starting pay. Workers

who come to Oracle when it acquires a company at which they are working (via M&As), or lateral hires from other companies, are likely to have such a prior salary. The existence of a gender pay gap for these two groups suggests that Oracle may be mimicking the gender pay gap reflected in prior pay of employees who some to Oracle from other employers – especially if the gender gap in prior pay and starting pay is similar. In this section, I examine data on prior pay, and the relationship between prior pay and starting pay, to test this conjecture. The conversion of prior pay information to usable data is described in Appendix C.

65. For the following analysis, I used the cleaned prior pay sample of 6,429 (unique) employees, of whom we were able to match to 5,209 employees with starting pay data at Oracle. However, I further restrict the sample to using prior pay information only when the classification is "New Hire – Regular" as indicated in the prior pay sample. Other types, such as CR (college recruiting) do not have any information about prior pay, nor would we expect them to, and hire types such as Internal Transfer, International Transfer, or Rehires are not relevant to the current question at hand as we are only interested in new hires coming into Oracle.

66. I then cleaned the data further. I ensured that the year in which prior pay was recorded is before the first starting pay year. I dropped cases with missing values, hourly employees, and some cases of ambiguous data (such as older data indicating an employee is from the universe of campus hires or mergers, and some extreme outliers for prior pay). The final sample for this analysis is laid out in Exhibit 38.

67. For the analyses that follow, to create an apples-to-apples comparison with the current measure of starting pay (which uses base pay), I attempt to use prior base pay whenever base pay is explicitly reported (425 employees). However, for most employees (2,358), it is ambiguous whether the salary number given is base pay or total compensation. When I pool the 2,783

employees together, my analysis uses base pay when given, thus treating an ambiguous reported pay amount as base pay. On the other hand, as Exhibit 38 shows, I dropped observations where there is a clear indication that the prior data refers to total compensation.

68. Exhibit 39 plots the number of observations on prior pay by year. The exhibit shows that I tend to have prior pay data for fairly recent hires. Given the small number of observations in 2009, I drop this year from my analysis (as Exhibit 38 shows), since I do not know why observations for 2009 would or would not have been included.

69. Exhibit 40 provides a scatter plot of the data on Oracle starting pay (vertical axis) and prior pay at the last job before Oracle (horizontal axis), for the full sample of new hires based on prior pay explicitly indicated as base pay or likely to be base pay (2,783 employees). Each point in the plot is the pair of observations on prior pay and starting pay for an Oracle employee, when data on both are available. The graph also shows the regression line from a regression of starting pay on prior pay. The line is strongly upward sloping, as is the cluster of plotted points, indicating that starting pay at Oracle is tightly linked to prior pay. On average, prior pay being higher by $1 predicts that starting pay is higher by $.75. Alternatively, prior pay explains 74% of the variation in starting pay. The likelihood that this strong relationship between prior pay and starting pay occurs by chance is less than 1 in 1 billion, as reflected in a t-statistic on the coefficient on prior pay in the starting pay regression of 89.9 (or an effect of prior pay of 89.9 standard deviations).

70. Exhibit 41 reports on the regression analysis of starting pay and prior pay.[27] Column (1)

[27] Since the analysis of prior pay is relevant only to base pay, and in particular is not relevant to bonuses and stock grants, here I use logs instead of the IHS.

looks at a regression for base starting pay from the 2010-2018 sample of non-campus hires.[28] This column reflects a 3.0% starting pay differential (8.6 standard deviations), favoring men over women. In column (2), I report estimates for the same specification, but for the subsample for which I can compare the results using prior data. The estimated sex difference in starting pay declines to approximately 2.4% (4.2 standard deviations).

71. Column (3) reports estimates for the same specification, but for prior pay instead of starting pay. The striking result is that the sex gap in pay in nearly the same – in this case approximately 2.2%. Column (4) shows a closely related result. If I regress the difference in the logs of starting pay and prior pay on the dummy variable for females and the same controls, I obtain an estimate near zero and statistically insignificant.[29] What columns (2), (3), and (4) demonstrate is that the sex gap in starting pay at Oracle reflects the sex gap in prior pay.[30]

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[28] I do the analysis in this section only for base rate of pay, since it is the measure of pay most likely to be tied to prior pay

[29] Note that I do not include log prior pay as an explanatory variable on the right-hand side of the regression. This might seem a natural way to ask whether prior pay differences by sex explain starting pay differences by sex, but this approach is likely to understate substantially the extent to which prior pay explains starting pay. Prior pay surely measures the actual prior pay on which Oracle based its starting pay with error. First, as outlined in Appendix C, there are some difficulties with the prior pay data that create measurement error. Second, there is no reason to believe Oracle always took reported prior pay literally – if, for example, it significantly deviated from the prior pay Oracle perceived as likely. When there is measurement error like this (what econometricians refer to as "classical measurement error") in an explanatory variable, the estimated regression coefficient of the variable is biased towards zero. This leads to understating the effect of prior pay on starting pay, which would in turn lead to overstating the importance of other variables, including gender, in explaining variation in starting pay. In contrast, when we use a variable with measurement error like this as a dependent variable, it does not create any bias in the estimated regression coefficients (because the measurement error simply appears in the error term of the regression and is uncorrelated with the right-hand side variables). Thus, the estimates in columns (2), (3), and (4) provide the most reliable information on the relationship between starting pay, prior pay, and sex, and indicate that starting pay differences by sex reflect prior pay differentials.

[30] In Appendix D, I show that these findings are robust to different ways of treating some of the potentially problematic values in the prior pay data.

72. Finally, column (5) reports the estimated gender gap in base pay, during the Class Period, for the subsample of observations for which there are data on prior pay. This permits me to compare the gender gap in starting pay and the gender gap in the Class Period for the same people (because I often do not have data on prior pay). The estimated gender gap in the base rate of pay is approximately 2.6%. Thus, for this subsample there is a strongly statistically significant gender gap in pay; the estimated gap is 5.2 standard deviations, which is so large that the probability of observing a difference this large in the data if in fact there was no gender gap in pay with these control variables is less than 1 in 1 million. Most importantly, this estimated gender gap in base pay is very similar to the gender gap in starting pay for the same women, shown in column (2), of 2.4%. This evidence is consistent with the gender gap in starting pay driving the gender gap in pay during the Class Period.

VIII. Incorporating Data on Prior Experience and Highest Degree

73. As described in Appendix A, I also use data on prior experience and on highest degree that did not come directory from data provided by Oracle, but instead was scraped from resumes of applicants to Oracle produced in discovery. In Exhibits 42-46 I report estimates of regressions for each of the five compensation measures, using these scraped data.[31]

74. In each table, I begin by reporting estimates corresponding to the full specifications with job and worker controls from column (6) of Exhibits 14, 19, 24, 29, and 34, but for the subsamples of observations with unflagged (and hence not problematic) resume data. These estimates are sometimes a bit different for this subsample, but they are qualitatively similar, with the one exception that the estimated gender gap for bonuses (Exhibit 44) is no longer statistically

[31] Like for the analysis reported above, including squares of the experience and tenure terms in this variable did not materially change the estimated gender gap in pay, so I report models without these additional variables.

significant. However, the important question is how the estimates change when I add or substitute the scraped data on experience and education, because this can be informative about whether there are biases from the omission of measures of experience and education in my main analyses.

75. In column (2) in each of Exhibits 42-46, I substitute the scraped experience measure for the potential experience measure, and add the scraped data on highest degree. In column (3) in each of these exhibits I instead substitute scraped relevant experience measure. The most important result is that the estimated gender gaps in pay are unaffected; the estimates across the columns of each of Exhibits 42-46 barely change. We also see that the scraped data behave as expected, confirming the validity of these data. In every case, there is an estimated positive effect of scraped experience or relevant experience for overall pay measures (base pay, Medicare wages, and total compensation), consistent with evidence on the effects of experience on earnings from a vast labor economics literature. Similarly, there are almost always positive effects of higher educational degrees for these pay measures, also consistent with evidence from the labor economics literature.

IX. Estimated Damages

76. I use the regression estimates to estimate total damages, in Exhibits 47 and 48. For this calculation, I use Medicare wages and total compensation as two alternative measures, since both are intended to capture all compensation. I use the final regression estimates from column (6) of Exhibits 19 and 34, which are the specifications that include the largest set of control variables on which there is relatively complete data.[32]

[32] We know from Exhibits 20 and 35 that adding performance ratings has little to no impact on the estimates.

77. In Exhibit 47, the wage regression estimate indicates a 6.26% shortfall for women with respect to Medicare wages, and the total compensation estimate indicates a 5.76% shortfall. To turn these percentages into dollar amounts, I multiply these percentages by average male compensation for the regression samples from which these percentages are estimated – [redacted] for Medicare wages, and [redacted] for total compensation (all in December 2017 dollars) – yielding a shortfall of [redacted] per woman per year for Medicare wages, and [redacted] for total compensation.[33]

78. To determine how many women to apply these figures to, I take the total number of person-years worked by women, in Product Development, Information Technology, or Support, during the period 2013-2017 – the years for which I estimate these regressions. (Thus, my damages estimates exclude 2018.) These numbers are based on the availability of one earnings measure or the other.[34] Note that this covers more observations than the number of observations in the regression samples, because the latter requires data on all of the control variables. I operate under the assumption that the estimated earnings shortfall would be similar in the regression sample and for the other observations.

79. I then multiply the earnings shortfall by these numbers. Finally, because these numbers are based on person-years, I adjust downward for the number of days worked in the year. This leads to estimated damages of [redacted] for Medicare wages, and [redacted] for total compensation.

80. Exhibit 48 shows the same calculations, but using only the observations in the regression

[33] The numbers are very similar if I also include observations not included in the regression because of missing control variables: [redacted] for Medicare wages, and [redacted] for total compensation.
[34] There are 386 observations for which I have total compensation but not Medicare wages.

samples. The estimated damages are very similar. Assuming the regression estimates apply to the larger sample, the estimates in Exhibit 47 are more appropriate, because they cover all workers in the class.

81. These damages estimates are based on the information and data produced to date. If provided with additional data (such as through 2018), I will update these numbers. This estimation provides an aggregate number for all women in the class. Using these data, I can also compute the amount by which each woman in a covered position was damaged.

Exhibit 1: Number of Unique Female Employees by Job Function

	Product Development	Information Technology	Support
Total	3,461	422	318
2013	2,505	275	275
2014	2,670	302	285
2015	2,709	329	239
2016	2,726	320	215
2017	2,689	330	204
2018	2,067	297	192

Exhibit 2: Summary of Gender Disparities in Compensation

	Base pay	Medicare wages	Bonuses	Stock grants	Total compensation
	(1)	(2)	(3)	(4)	(5)
Years	2013-2018	2013-2017	2013-2017	2013-2017	2013-2017
Corresponding exhibit and column	Exhibit 14, col. (6)	Exhibit 19, col. (6)	Exhibit 24, col. (6)	Exhibit 29, col. (6)	Exhibit 34, col. (6)
Female	-0.0384***	-0.0626***	-0.1317***	-0.3311***	-0.0576***
	(0.0026)	(0.0049)	(0.0314)	(0.0623)	(0.0042)
Standard deviations	14.65	12.85	4.19	5.32	13.65
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 10,000	< 1 in 1 million	< 1 in 1 billion
Observations	66,928	57,066	58,256	58,256	58,256

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of the pay measure indicated in the column heading, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls include controls for job code and job grade interactions, work location (zip code), LOB head, and dummy variables for hourly or part-time status. Job level, specialty, function, title, and exempt status were not included because they are redundant with the job code-job grade interactions. Worker controls include Oracle tenure, overall tenure, job tenure, and experience. Oracle tenure measures the length of time spent in Oracle. Overall tenure measures the length of time spent in Oracle and any company acquired through M&A's. Job tenure measures the length of time spent in the current job code. Experience is estimated as age minus 22. For other details corresponding to each pay measure, see notes to Exhibits 14, 19, 24, 29, and 34. *** denotes statistical significance at the 1% level.

Exhibit 3: Starting Pay and Prior Pay Regressions

Dependent Variable	Ln(Starting Pay)	Ln(Starting Pay)	Ln(Prior Pay)	Ln(Starting Pay) – Ln(Prior Pay)
Controls	Job controls plus experience (full sample after 2010)	Job controls plus experience (prior pay sample)	Job controls plus experience (prior pay sample)	Job controls plus experience (prior pay sample)
	(1)	(2)	(3)	(4)
Female	-0.0301***	-0.0242***	-0.0218**	-0.0025
	(0.0035)	(0.0057)	(0.0108)	(0.0091)
Experience	0.0023***	0.0009***	0.0053***	-0.0045***
	(0.0002)	(0.0003)	(0.0006)	(0.0005)
Standard deviations	8.61	4.24	2.02	0.27
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 10,000	4.39%	78.74%
Observations	9,130	2,783	2,783	2,783

Note: Controls are included for experience, a dummy for whether foreign currencies were converted, and dummies for data ambiguity in the prior pay data (unclear currencies, unclear fractions, unclear hourly, and unclear total compensations). Job controls include controls for job code-job grade interactions, zip code, and part-time status. Prior pay sample size does not match correlation graph (Exhibit 40) sample size due to missing controls data. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level, * denotes statistical significance at the 10% level.

Exhibit 4: Estimated Pay Differences for Named Plaintiffs, Estimated Gender Differences in Base Pay for 2013-2017/8, from Regressions with Controls for Job and Worker Characteristics

	Base Pay	Medicare Wages	Bonuses	Stocks Grants	Total Compensation
	(1)	(2)	(3)	(4)	(5)
Clark, Marilyn J.	-0.1318***	-0.2194***	2.2506***	-3.4047***	-0.1400***
	(0.0131)	(0.0189)	(0.1372)	(0.3292)	(0.0181)
Standard deviations	10.08	11.62	16.41	10.34	7.73
Kant, Manjari	-0.1458***	-0.2853***	-1.6580***	-3.9698***	-0.2441***
	(0.0155)	(0.0330)	(0.1461)	(0.4176)	(0.0311)
Standard deviations	9.42	8.64	11.35	9.51	7.84
Petersen, Elizabeth Sue	-0.2485***	-0.3566***	0.5337***	0.1760	-0.3304***
	(0.0083)	(0.0140)	(0.0785)	(0.1612)	(0.0124)
Standard deviations	30.02	25.56	6.80	1.09	26.54
Observations	66,928	57,066	58,256	58,256	58,256
Corresponding regression	Exhibit 14, col. (6)	Exhibit 19, col. (6)	Exhibit 24, col. (6)	Exhibit 29, col. (6)	Exhibit 34, col. (6)

Note: Each column shows estimates from a separate regression. The dependent variable, by column, is the inverse hyperbolic sine (IHS) of the Adjusted (for partial-years worked) (1) Base Pay, (2) Medicare Wages, (3) Total Compensation, (4) Stock, (5) Bonuses, in 2017 dollars. Coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. *** denotes statistical significance at the 1% level. Regression includes job, LOB head, and tenure controls as described in notes to Exhibits 13 and 14, as well as a dummy variable for female employees other than the named plaintiffs. All dependent variables except for base pay are adjusted for partial years worked, as explained in the report. The coefficient estimates for the named plaintiffs measure the approximate the percent deviation from the regression line for males. Thus, a negative estimate for a named plaintiff means the plaintiff is underpaid relative to similar men. Data cover 2013-2018 for base pay, and 2013-2017 for other measures. Note that in the Oracle data Xian Wang is coded as a non-campus hire. However, other information provided to me by plaintiffs' attorneys indicates that she was a campus hire. Since my analysis excludes campus hires, estimates for Xian Wang are not included in this table. However, in my full analysis of the Oracle data I treat the Oracle data as definitive, and hence Ms. Wang is not dropped from the data.

Exhibit 5: Base Pay Descriptive Statistics (2013-2018)

Year	# of employees	Median base pay	Median female base pay	Median male base pay	Mean base pay	Mean female base pay	Mean male base pay
Total	67,209	[redacted]					
2013	10,894						
2014	11,633						
2015	11,846						
2016	11,932						
2017	11,758						
2018	9,146						

Table based on pay information from *ORACLE_JEWETT_00030955*. Compensation figures for descriptive statistics are always in 2017 dollars.

Exhibit 6: Histogram of Base Salary, by Gender (2013-2018)

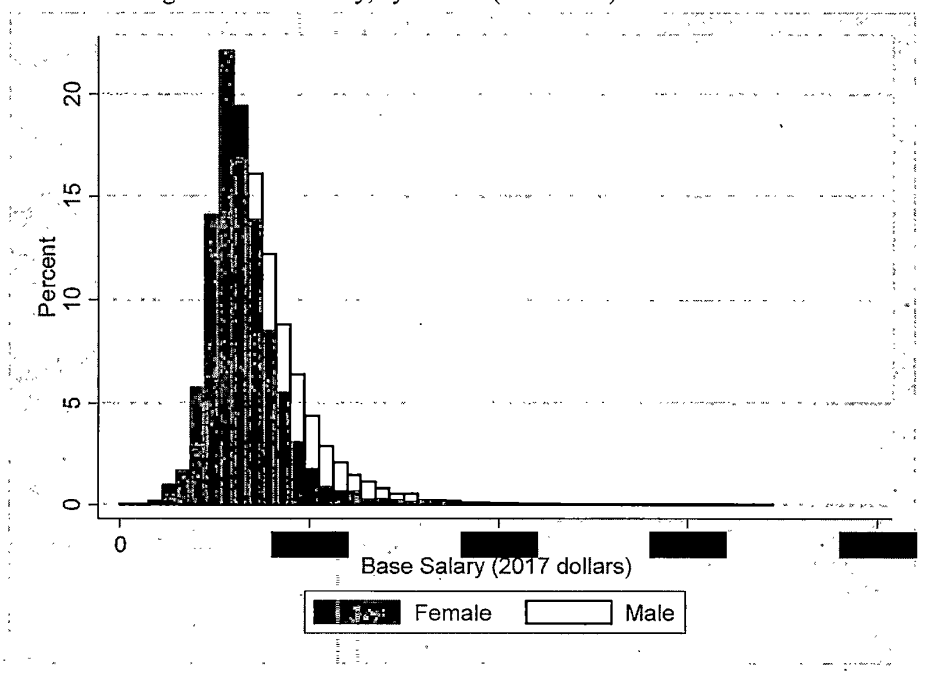
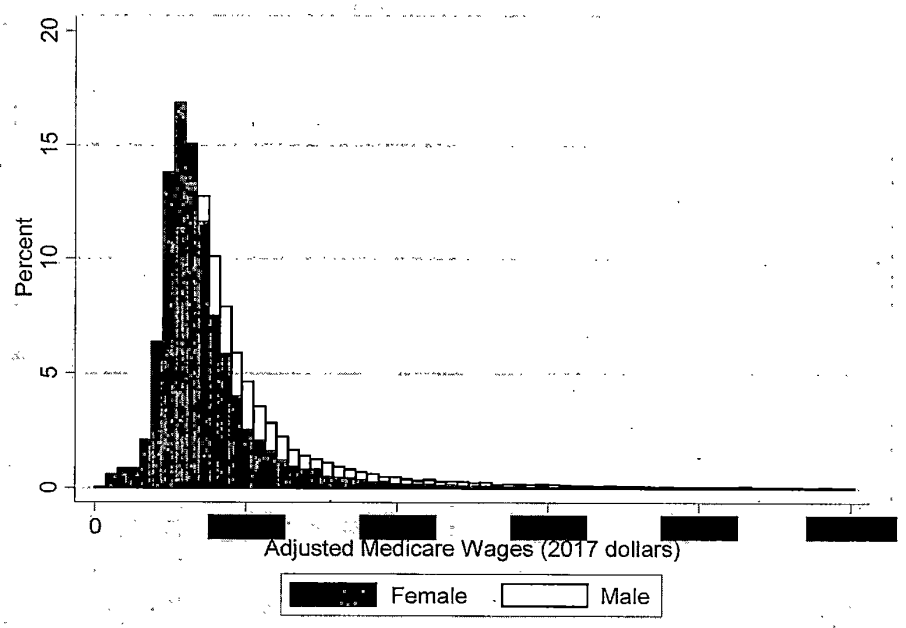


Exhibit 7: Summary of Adjusted Medicare Wages (2013-2017)

Year	# of employees	Median Medicare wages	Median female Medicare wages	Median male Medicare wages	Mean Medicare wages	Mean female Medicare wages	Mean male Medicare wages
Total	57,072	[redacted]					
2013	10,883						
2014	11,412						
2015	11,569						
2016	11,641						
2017	11,567						

Exhibit 8: Histogram of Adjusted Medicare Wages (2013-2017)



Medicare wages exceeding [redacted] are excluded for better visualization of the data.

Exhibit 9: Summary of Adjusted Total Yearly Bonuses (2013-2017)

Year	# of employees	Mean total bonus	Mean female total bonuses	Mean male total bonuses
Total	58,262	[redacted]		
2013	10,940			
2014	11,658			
2015	11,871			
2016	11,972			
2017	11,821			

All median bonuses by year and gender (and pooled by year) are $0.

Exhibit 10: Adjusted Total Stock Grants (RSUs + Options) (2013-2017)

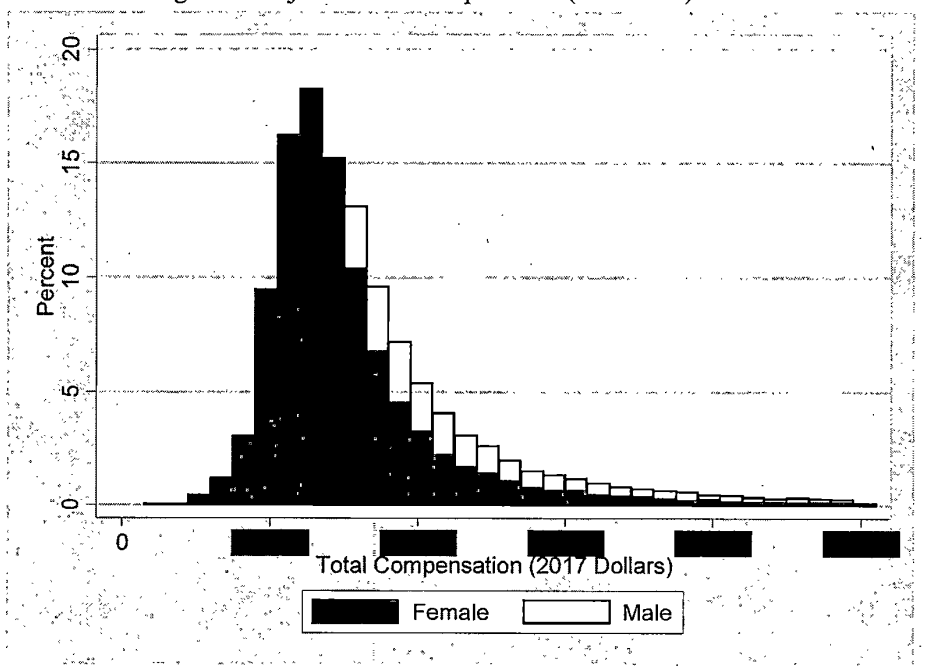
Year	# of employees	Median stock grant	Median female stock grant	Median male stock grant	Mean stock grant	Mean female stock grant	Mean male stock grant
Total	58,262	[redacted]					
2013	10,940						
2014	11,658						
2015	11,871						
2016	11,972						
2017	11,821						

Person_ID 6633, "Tan, Ms Leng Leng," only worked three days in 2013 but exercised her options for [redacted], which creates an outlier for adjusted total stock grants. She is excluded from the data for 2013, in this table and all subsequent ones

Exhibit 11: Summary Statistics for Total Compensation Adjusted for Partial Years Worked (2013-2017)

Year	# of employees	Median total compensation	Median female total compensation	Median male total compensation	Mean total compensation	Mean female total compensation	Mean male total compensation
Total	58,262	[redacted]					
2013	10,940						
2014	11,658						
2015	11,871						
2016	11,972						
2017	11,821						

Exhibit 12: Histogram of Adjusted Total Compensation (2013-2017)



Restricted to [redacted] for better visualization of data.

Exhibit 13: Estimated Gender Difference in Base Rate of Pay, 2013-2018, Controls for Job Characteristics

	No controls	Job controls
	(1)	(2)
Female	-0.1469***	-0.0516***
	(0.0054)	(0.0028)
Standard deviations	27.16	18.68
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion
Observations	67,209	67,209

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of base pay in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls include controls for job code and job grade interactions, work location (zip code), LOB head, and dummy variables for hourly or part-time status. Job level, specialty, function, title, and exempt status were not included because they are redundant with the job code-job grade interactions. *** denotes statistical significance at the 1% level.

Exhibit 14: Estimated Gender Differences in Base Pay for 2013-2018, from Regressions with Controls for Job and Worker Characteristics

	Job controls	Job controls (tenure subsample)	Job controls plus Oracle tenure	Job controls plus Oracle and overall tenure	Job controls plus Oracle tenure, overall tenure, and job tenure	Job controls plus Oracle tenure, overall tenure, job tenure, and experience
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0516***	-0.0517***	-0.0412***	-0.0412***	-0.0387***	-0.0384***
	(0.0028)	(0.0028)	(0.0026)	(0.0026)	(0.0026)	(0.0026)
Standard deviations	18.68	18.81	15.59	15.59	14.74	14.65
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	67,209	66,928	66,928	66,928	66,928	66,928

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of base pay in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls are as described in Exhibit 13. Oracle tenure measures the length of time spent in Oracle. Overall tenure measures the length of time spent in Oracle and any company acquired through M&A's. Job tenure measures the length of time spent in the current job code. Experience is estimated as age minus 22. *** denotes statistical significance at the 1% level.

Exhibit 15: Estimated Gender Differences in Base Pay for 2013-2018, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Performance Ratings

	Job controls plus Oracle tenure, overall tenure, job tenure, and experience	Job controls plus Oracle tenure, overall tenure, job tenure, and experience (performance rating subsample)	Job, tenure, experience, and performance rating controls
	(1)	(2)	(3)
Female	-0.0384***	-0.0382***	-0.0383***
	(0.0026)	(0.0027)	(0.0027)
Average performance rating			0.0247***
			(0.0014)
Standard deviations	14.65	13.92	14.14
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	66,928	46,354	46,354

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of the base pay in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Regressions include job and worker controls as described in Exhibits 13 and 14. The average performance rating is the average of the last rating of the year from each data source. *** denotes statistical significance at the 1% level.

Exhibit 16: Estimated Gender Differences in Base Pay for 2013-2018, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Formal and Informal Performance Ratings

	Job controls and tenure (formal appraisal sample)	Job controls, tenure, and formal appraisal rating	Job controls and tenure (informal appraisal sample)	Job controls, tenure, and informal appraisal rating
	(1)	(2)	(3)	(4)
Female	-0.0327***	-0.0334***	-0.0397***	-0.0396***
	(0.0035)	(0.0035)	(0.0028)	(0.0028)
Performance rating		0.0189***		0.0266***
		(0.0019)		(0.0014)
Standard deviations	9.20	9.46	14.08	14.26
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	22,678	22,678	38,274	38,274

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of base pay in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Regressions include job and worker controls as in Exhibit 15, column (3). The formal appraisal rating is the last rating of the year from *ORACLE_JEWETT_00030954*. The informal appraisal rating is the average of the last rating of the year for all other sources, *ORACLE_JEWETT_00030955*, *ORACLE_JEWETT_00007303*, *ORACLE_JEWETT_00007312*, *ORACLE_JEWETT_00001173*, and *ORACLE_JEWETT_00001170*. *** denotes statistical significance at the 1% level.

Exhibit 17: Estimated Gender Differences in Base Pay by Year, from Regressions with Controls for Job Characteristics and Worker Characteristics

	2013	2014	2015	2016	2017	2018
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0389***	-0.0377***	-0.0385***	-0.0380***	-0.0384***	-0.0392***
	(0.0031)	(0.0031)	(0.0030)	(0.0029)	(0.0030)	(0.0034)
Standard deviations	12.42	12.25	12.73	12.88	12.80	11.41
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	10,852	11,578	11,800	11,894	11,715	9,089

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of base pay in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Regressions include job and tenure controls are as described in Exhibit 14, column (6). *** denotes statistical significance at the 1% level.

Exhibit 18: Estimated Gender Differences in Adjusted Medicare Wages for 2013-2018, from Regressions with Controls for Job Characteristics

	No controls	Job controls
	(1)	(2)
Female	-0.2183***	-0.0671***
	(0.0082)	(0.0049)
Standard deviations	26.47	13.82
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion
Observations	57,072	57,072

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) Medicare Wages in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls include controls for job code and job grade interactions, work location (zip code), and dummy variables for part-time status. (There is no control for hourly workers because this comes from the base pay data, and is meant to adjust for differences in base rate of pay associated with hourly status.) Job level, specialty, function, title, and exempt status were not included because they are redundant from the job code-job grade interactions. *** denotes statistical significance at the 1% level.

Exhibit 19: Estimated Gender Differences in Adjusted Medicare Wages for 2013-2017, from Regressions with Controls for Job and Worker Characteristics

	Job controls	Job controls (tenure subsample)	Job controls plus Oracle tenure	Job controls plus Oracle and overall tenure	Job controls plus Oracle tenure, overall tenure, and job tenure	Job controls plus Oracle tenure, overall tenure, job tenure, and experience
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0671***	-0.0672***	-0.0676***	-0.0679***	-0.0626***	-0.0626***
	(0.0049)	(0.0049)	(0.0049)	(0.0049)	(0.0049)	(0.0049)
Standard deviations	13.82	13.83	13.81	13.87	12.84	12.85
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	57,072	57,066	57,066	57,066	57,066	57,066

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) Medicare wages in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls as described in Exhibit 13, and work controls as described in Exhibit 14. *** denotes statistical significance at the 1% level.

Exhibit 20: Estimated Gender Differences in Adjusted Medicare Wages for 2013-2017, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Performance Ratings

	Job controls plus Oracle tenure, overall tenure, job tenure, and experience	Job controls plus Oracle tenure, overall tenure, job tenure, and experience (performance rating subsample)	Job controls plus Oracle tenure, overall tenure, job tenure, and experience plus performance ratings
	(1)	(2)	(3)
Female	-0.0626***	-0.0612***	-0.0613***
	(0.0049)	(0.0049)	(0.0049)
Average performance rating			0.0401***
			(0.0029)
Standard deviations	12.85	12.42	12.60
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	57,066	45,176	45,176

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) Medicare wages in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 18, 19, and 15 apply. *** denotes statistical significance at the 1% level.

Exhibit 21: Estimated Gender Differences in Adjusted Medicare Wages for 2013-2017, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Formal and Informal Performance Ratings

	Job controls and tenure (formal appraisal sample)	Job controls, tenure, and formal appraisal rating	Job controls and tenure (informal appraisal sample)	Job controls, tenure, and informal appraisal rating
	(1)	(2)	(3)	(4)
Female	-0.0464***	-0.0476***	-0.0637***	-0.0635***
	(0.0055)	(0.0054)	(0.0053)	(0.0053)
Performance rating		0.0326***		0.0420***
		(0.0032)		(0.0031)
Standard deviations	8.51	8.79	11.92	12.04
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	21,837	21,837	37,937	37,937

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial years worked) Medicare wages in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 18, 19, and 16 apply. *** denotes statistical significance at the 1% level.

Exhibit 22: Estimated Gender Differences in Adjusted Medicare Wages by Year, from Regressions with Controls for Job and Worker Characteristics

	2013	2014	2015	2016	2017
	(1)	(2)	(3)	(4)	(5)
Female	-0.0594***	-0.0577***	-0.0618***	-0.0653***	-0.0705***
	(0.0063)	(0.0084)	(0.0075)	(0.0079)	(0.0095)
Standard deviations	9.44	6.83	8.25	8.29	7.45
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	10,881	11,411	11,568	11,640	11,566

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) Medicare wages in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 18 and 19 apply. *** denotes statistical significance at the 1% level.

Exhibit 23: Estimated Gender Differences in Adjusted Bonuses for 2013-2017, from Regressions with Controls for Job Characteristics

	No controls	Job controls
	(1)	(2)
Female	-0.4132***	-0.1724***
	(0.0346)	(0.0318)
Standard deviations	11.93	5.43
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 10 million
Observations	58,262	58,262

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total bonuses in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls include controls for job code and job grade interactions, work location (zip code), and dummy variables for part-time status. Job level, specialty, function, title, and exempt status were not included because they are redundant with the job code-job grade interactions. *** denotes statistical significance at the 1% level.

Exhibit 24: Estimated Gender Differences in Adjusted Total Bonuses for 2013-2017, from Regressions with Controls for Job and Worker Characteristics

	Job controls	Job controls (tenure subsample)	Job controls plus Oracle tenure	Job controls plus Oracle and overall tenure	Job controls plus Oracle tenure, overall tenure, and job tenure	Job controls plus Oracle tenure, overall tenure, job tenure, and experience
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.1724***	-0.1723***	-0.1008***	-0.0978***	-0.1152***	-0.1317***
	(0.0318)	(0.0318)	(0.0317)	(0.0316)	(0.0316)	(0.0314)
Standard deviations	5.43	5.42	3.18	3.09	3.64	4.19
Probability under null hypothesis of no gender pay gap	< 1 in 10 million	< 1 in 10 million	0.15%	0.20%	0.03%	< 1 in 10,000
Observations	58,262	58,256	58,256	58,256	58,256	58,256

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total bonuses in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls are as described in Exhibit 23, and worker controls as described in Exhibit 14. *** denotes statistical significance at the 1% level.

Exhibit 25: Estimated Gender Differences in Adjusted Total Bonuses for 2013-2017, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Performance Ratings

	Job controls plus Oracle tenure, overall tenure, job tenure, and experience	Job controls plus Oracle tenure, overall tenure, job tenure, and experience (performance rating subsample)	Job controls plus Oracle tenure, overall tenure, job tenure, and experience plus performance ratings
		(1)	(2)
Female	-0.1317***	-0.1505***	-0.1516***
	(0.0314)	(0.0359)	(0.0355)
Average performance rating			0.5103***
			(0.0253)
Standard deviations	4.19	4.19	4.27
Probability under null hypothesis of no gender pay gap	< 1 in 10,000	< 1 in 10,000	< 1 in 10,000
Observations	58,256	45,716	45,716

Note: The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total bonuses in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 23, 24, and 15 apply. *** denotes statistical significance at the 1% level.

Exhibit 26: Estimated Gender Differences in Adjusted Total Bonuses for 2013-2017, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Formal and Informal Performance Ratings

	Job controls and tenure (formal appraisal sample)	Job controls, tenure, and formal appraisal rating	Job controls and tenure (informal appraisal sample)	Job controls, tenure, and informal appraisal rating
	(1)	(2)	(3)	(4)
Female	-0.1114***	-0.1249***	-0.1527***	-0.1488***
	(0.0426)	(0.0423)	(0.0411)	(0.0405)
Performance rating		0.4008***		0.5887***
		(0.0311)		(0.0282)
Standard deviations	2.62	2.95	3.71	3.67
Probability under null hypothesis of no gender pay gap	0.89%	0.31%	0.02%	0.02%
Observations	21,975	21,975	38,416	38,416

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total bonuses in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 23, 24, and 16 apply. *** denotes statistical significance at the 1% level.

Exhibit 27: Estimated Gender Differences in Adjusted Total Bonuses by Year, from Regressions with Controls for Job and Worker Characteristics

	2013	2014	2015	2016	2017
	(1)	(2)	(3)	(4)	(5)
Female	-0.2260***	0.0994	-0.0907	-0.1329**	-0.3297***
	(0.0469)	(0.0909)	(0.0554)	(0.0533)	(0.0831)
Standard deviations	4.81	1.09	1.64	2.50	3.97
Probability under null hypothesis of no gender pay gap	< 1 in 100,000	27.42%	10.16%	1.26%	< 1 in 10,000
Observations	10,938	11,657	11,870	11,971	11,820

Note: The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) bonuses in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 23 and 24 apply. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level.

Exhibit 28: Estimated Gender Differences in Adjusted Stock Grants for 2013-2017, from Regressions with Controls for Job Characteristics

	No controls	Job controls
	(1)	(2)
Female	-1.0949***	-0.0894
	(0.0799)	(0.0628)
Standard deviations	13.71	1.42
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	15.43%
Observations	58,262	58,262

Note: The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total stock grants in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls include controls for job code and job grade interactions, work location (zip code), and dummy variables for part-time status. Job level, specialty, function, title, and exempt status were not included because they are redundant with the job code-job grade interactions. *** denotes statistical significance at the 1% level.

Exhibit 29: Estimated Gender Differences in Adjusted Total Stock Grants (RSUs and Options) for 2013-2017, from Regressions with Controls for Job and Worker Characteristics

	Job controls	Job controls (tenure subsample)	Job controls plus Oracle tenure	Job controls plus Oracle and overall tenure	Job controls plus Oracle tenure, overall tenure, and job tenure	Job controls plus Oracle tenure, overall tenure, job tenure, and experience
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0894	-0.0895	-0.2920***	-0.2920***	-0.2887***	-0.3311***
	(0.0628)	(0.0628)	(0.0633)	(0.0633)	(0.0633)	(0.0623)
Standard deviations	1.42	1.43	4.61	4.61	4.56	5.32
Probability under null hypothesis of no gender pay gap	15.43%	15.41%	< 1 in 100,000	< 1 in 100,000	< 1 in 100,000	< 1 in 1 million
Observations	58,262	58,256	58,256	58,256	58,256	58,256

Note: The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total stock grants in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls are as described in Exhibit 23, and worker controls as described in Exhibit 14. *** denotes statistical significance at the 1% level.

Exhibit 30: Estimated Gender Differences in Adjusted Total Stock Grants for 2013-2017, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Performance Ratings

	Job controls plus Oracle tenure, overall tenure, job tenure, and experience	Job controls plus Oracle tenure, overall tenure, job tenure, and experience (performance rating subsample)	Job controls plus Oracle tenure, overall tenure, job tenure, and experience plus performance ratings
	(1)	(2)	(3)
Female	-0.3311***	-0.3839***	-0.3862***
	(0.0623)	(0.0686)	(0.0666)
Average performance rating			1.0445***
			(0.0387)
Standard deviations	5.32	5.60	5.80
Probability under null hypothesis of no gender pay gap	< 1 in 1 million	< 1 in 10 million	< 1 in 100 million
Observations	58,256	45,716	45,716

Note: The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total stock grants in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 28, 29, and 15 apply. *** denotes statistical significance at the 1% level.

Exhibit 31: Estimated Gender Differences in Adjusted Total Stock Grants for 2013-2017, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Formal and Informal Performance Ratings

	Job controls and tenure (formal appraisal sample)	Job controls, tenure, and formal appraisal rating	Job controls and tenure (informal appraisal sample)	Job controls, tenure, and informal appraisal rating
	(1)	(2)	(3)	(4)
Female	-0.2059**	-0.2292**	-0.4592***	-0.4519***
	(0.0907)	(0.0894)	(0.0732)	(0.0709)
Performance rating		0.6895***		1.0940***
		(0.0549)		(0.0409)
Standard deviations	2.27	2.56	6.28	6.37
Probability under null hypothesis of no gender pay gap	2.32%	1.03%	< 1 in 1 billion	< 1 in 1 billion
Observations	21,975	21,975	38,416	38,416

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) stock grants in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 28, 30, and 16 apply. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level.

Exhibit 32: Estimated Gender Differences in Adjusted Total Stock Grants by Year, from Regressions with Controls for Job Characteristics and Worker Characteristics

	2013	2014	2015	2016	2017
	(1)	(2)	(3)	(4)	(5)
Female	-0.5080***	-0.4627***	-0.1785*	-0.2561**	-0.2911***
	(0.1013)	(0.1029)	(0.0934)	(0.1016)	(0.1045)
Standard deviations	5.02	4.50	1.91	2.52	2.79
Probability under null hypothesis of no gender pay gap	< 1 in 1 million	< 1 in 100,000	5.62%	1.17%	0.54%
Observations	10,938	11,657	11,870	11,971	11,820

Note: The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) stock grants in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 28 and 29 apply. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level, * denotes statistical significance at the 10% level.

Exhibit 33: Estimated Gender Differences in Adjusted Total Compensation for 2013-2017, from Regressions with Controls for Job Characteristics

	No controls	Job controls
	(1)	(2)
Female	-0.2015***	-0.0614***
	(0.0075)	(0.0042)
Standard deviations	26.77	14.57
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion
Observations	58,262	58,262

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total compensation in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls include controls for job code and job grade interactions, work location (zip code), and dummy variables for part-time status. Job level, specialty, function, title, and exempt status were not included because they are redundant with the job code-job grade interactions. *** denotes statistical significance at the 1% level.

Exhibit 34: Estimated Gender Differences in Adjusted Total Compensation for 2013-2017, from Regressions with Controls for Job and Worker Characteristics

	Job controls	Job controls (Tenure subsample)	Job controls plus Oracle tenure	Job controls plus Oracle and overall tenure	Job controls plus Oracle tenure, overall tenure, and job tenure	Job controls plus Oracle tenure, overall tenure, job tenure, and experience
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0614***	-0.0614***	-0.0608***	-0.0608***	-0.0568***	-0.0576***
	(0.0042)	(0.0042)	(0.0042)	(0.0042)	(0.0042)	(0.0042)
Standard deviations	14.57	14.58	14.33	14.33	13.45	13.65
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	58,262	58,256	58,256	58,256	58,256	58,256

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total compensation in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Job controls as described in Exhibit 13, and work controls as described in Exhibit 14. There are somewhat fewer observations beginning in column (2) because of missing tenure data. *** denotes statistical significance at the 1% level.

Exhibit 35: Estimated Gender Differences in Adjusted Total Compensation for 2013-2017, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Performance Ratings

	Job controls plus Oracle tenure, overall tenure, job tenure, and experience	Job controls plus Oracle tenure, overall tenure, job tenure, and experience (performance rating subsample)	Job controls plus Oracle tenure, overall tenure, job tenure, and experience plus performance ratings
	(1)	(2)	(3)
Female	-0.0576***	-0.0603***	-0.0604***
	(0.0042)	(0.0043)	(0.0043)
Average performance rating			0.0386***
			(0.0025)
Standard deviations	13.65	13.93	14.16
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	58,256	45,716	45,716

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total compensation in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 33, 34, and 15 apply. *** denotes statistical significance at the 1% level.

Exhibit 36: Estimated Gender Differences in Adjusted Total Compensation for 2013-2017, from Regressions with Controls for Job Characteristics, Worker Characteristics, and Formal and Informal Performance Ratings

	Job controls and tenure (formal appraisal sample)	Job controls, tenure, and formal appraisal rating	Job controls and tenure (informal appraisal sample)	Job controls, tenure, and informal appraisal rating
	(1)	(2)	(3)	(4)
Female	-0.0507***	-0.0517***	-0.0628***	-0.0625***
	(0.0047)	(0.0047)	(0.0047)	(0.0046)
Performance rating		0.0295***		0.0401***
		(0.0028)		(0.0027)
Standard deviations	10.70	11.02	13.43	13.59
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	21,975	21,975	38,416	38,416

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of the adjusted (for partial-years worked) total compensation in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 33, 34, and 16 apply. *** denotes statistical significance at the 1% level.

Exhibit 37: Estimated Gender Differences in Adjusted Total Compensation by Year, from Regressions with Controls for Job and Worker Characteristics

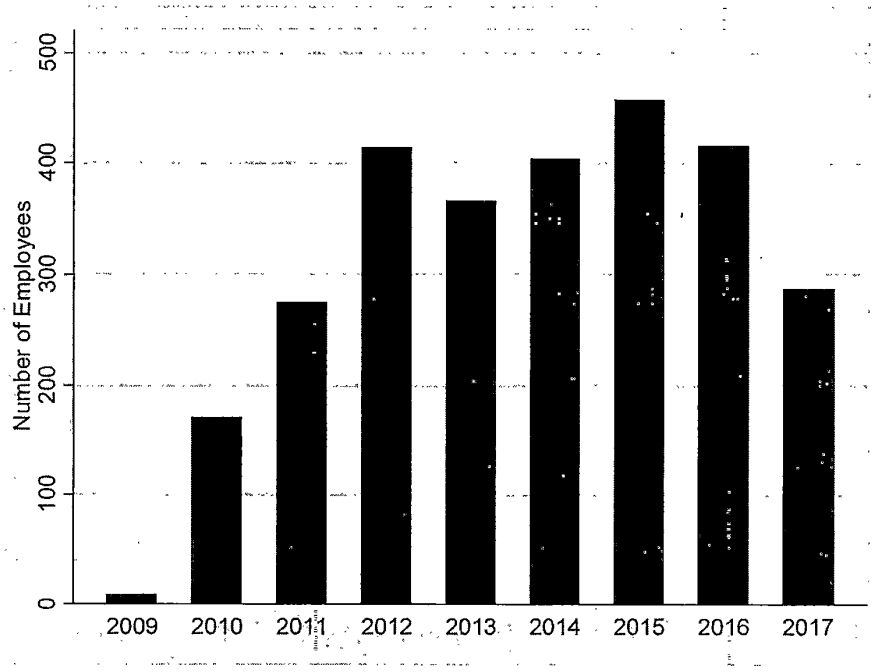
	2013	2014	2015	2016	2017
	(1)	(2)	(3)	(4)	(5)
Female	-0.0637***	-0.0511***	-0.0563***	-0.0608***	-0.0584***
	(0.0056)	(0.0071)	(0.0055)	(0.0058)	(0.0071)
Standard deviations	11.40	7.15	10.22	10.54	8.18
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	10,938	11,657	11,870	11,971	11,820

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted (for partial-years worked) total compensation in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Standard errors are clustered by person IDs. Notes regarding the control variables and performance ratings from Exhibits 33 and 34 apply. *** denotes statistical significance at the 1% level.

Exhibit 38: Sample for Analysis of Prior Pay

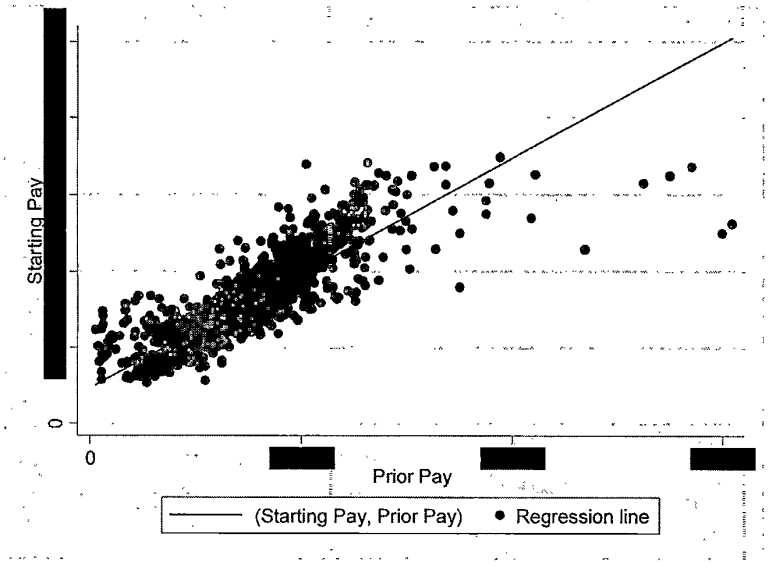
Sample restrictions	Observations
Full sample of prior pay data	6,429
Able to join prior pay data to employee starting pay data (after 2009)	5,123
New hires classification only	3,183
Date of prior pay precedes starting pay	3,180
Drop if prior pay = 0	3,110
Drop if prior pay is missing	2,923
Drop hourly	2,819
Drop likely hourly	2,817
Drop if clear we have total compensation but not base pay	2,806
Drop extreme prior pay values	2,800
Drop 2009 due to small number of observations	2,791
Omitted from regression due to missing job controls or gender	2,783

Exhibit 39: Observations on Prior Pay by Year



The 9 observations in 2009 are omitted from the regressions.

Exhibit 40: Starting Pay and Prior Pay Scatter Plot and Regression Line



Regression of Starting Pay on Prior Pay yields a 0.752 coefficient on Prior Pay, with a t-statistic = 89.94, and $R^2 = 0.74$.

Exhibit 41: Starting Pay and Prior Pay Regressions

Dependent Variable	Ln(Starting Pay)	Ln(Starting Pay)	Ln(Prior Pay)	Ln(Starting Pay) – Ln(Prior Pay)	Ln(Base Pay), Class Period
Controls	Job controls plus experience (full sample after 2010)	Job controls plus experience (prior pay sample)	Job controls plus experience (prior pay sample)	Job controls plus experience (prior pay sample)	Job controls plus experience (prior pay sample)
	(1)	(2)	(3)	(4)	(5)
Female	-0.0301***	-0.0242***	-0.0218**	-0.0025	-0.0264***
	(0.0035)	(0.0057)	(0.0108)	(0.0091)	(0.0051)
Standard deviations	8.61	4.24	2.02	0.27	5.18
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 10,000	4.39%	78.74%	< 1 in 1 million
Observations	9,130	2,783	2,783	2,783	10,732

Note: Controls are included for potential experience, a dummy for whether foreign currencies were converted, and dummies for data ambiguity in the prior pay data (unclear currencies, unclear fractions, unclear hourly, and unclear total compensations). Job controls include controls for job code-job grade interactions, work location (zip code), and part time. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level.

Exhibit 42: Regressions with Scraped Experience and Highest Degree, Base Pay

	Observations with resume information	Observations with scraped data	Observations with scraped data
Female	-0.0326***	-0.0317***	-0.0319***
	(0.0048)	(0.0047)	(0.0047)
Age as experience	0.0012***		
	(0.0003)		
Scraped experience		0.0026***	
		(0.0003)	
Scraped relevant experience			0.0024***
			(0.0003)
Bachelor's		-0.0020	-0.0018
		(0.0076)	(0.0076)
Master's		0.0154***	0.0166***
		(0.0044)	(0.0044)
Ph.D.		0.0533***	0.0565***
		(0.0086)	(0.0087)
Standard deviations	6.85	6.80	6.85
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	16,201	16,201	16,201

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of base pay in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Education is assumed to be additive such that an individual with a Ph.D. as the highest degree is also coded as having a Bachelor's degree and a Master's degree, and an individual coded as having a Master's degree is also coded as having a Bachelor's degree. Other job controls included are as described in notes to Exhibits 13 and 14. Standard errors are clustered by person IDs. *** denotes statistical significance at the 1% level.

Exhibit 43: Regressions with Scraped Experience and Highest Degree, Adjusted Medicare Wages

	Observations with resume information	Observations with scraped data	Observations with scraped data
Female	-0.0476***	-0.0463***	-0.0465***
	(0.0092)	(0.0091)	(0.0091)
Age as experience	0.0002		
	(0.0005)		
Scraped experience		0.0014**	
		(0.0006)	
Scraped relevant experience			0.0010*
			(0.0006)
Bachelor's		0.0006	0.0001
		(0.0142)	(0.0142)
Master's		0.0052	0.0054
		(0.0082)	(0.0083)
Ph.D.		0.0702***	0.0717***
		(0.0202)	(0.0202)
Standard deviations	5.19	5.08	5.10
Probability under null hypothesis of no gender pay gap	< 1 in 1 million	< 1 in 1 million	< 1 in 1 million
Observations	13,379	13,379	13,379

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted Medicare wages in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Education is assumed to be additive such that an individual with a Ph.D. as the highest degree is also coded as having a Bachelor's degree and a Master's degree, and an individual coded as having a Master's degree is also coded as having a Bachelor's degree. Other job controls included are as described in notes to Exhibits 18 and 19. Standard errors are clustered by person IDs. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level, * denotes statistical significance at the 10% level.

Exhibit 44: Regression with Scraped Experience and Highest Degree, Adjusted Total Bonuses

	Observations with resume information	Observations with scraped data	Observations with scraped data
Female	-0.1047	-0.1094	-0.1073
	(0.0721)	(0.0719)	(0.0719)
Age as experience	-0.0450***		
	(0.0043)		
Scraped experience		-0.0360***	
		(0.0056)	
Scraped relevant experience			-0.0395***
			(0.0055)
Bachelor's		-0.2444**	-0.2625**
		(0.1181)	(0.1183)
Master's		0.1039	0.0776
		(0.0700)	(0.0703)
Ph.D.		0.6220***	0.5731***
		(0.1883)	(0.1867)
Standard deviations	1.45	1.52	1.49
Probability under null hypothesis of no gender pay gap	14.62%	12.85%	13.56%
Observations	13,766	12,766	13,766

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted total bonuses in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Education is assumed to be additive such that an individual with a Ph.D. as the highest degree is also coded as having a Bachelor's degree and a Master's degree, and an individual coded as having a Master's degree is also coded as having a Bachelor's degree. Other job controls included are as described in notes to Exhibits 23 and 24. Standard errors are clustered by person IDs. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level.

Exhibit 45: Regression with Scraped Experience and Highest Degree, Adjusted Stock Grants

	Observations with resume information	Observations with scraped data	Observations with scraped data
Female	-0.2307*	-0.2756**	-0.2674**
	(0.1212)	(0.1231)	(0.1229)
Potential experience	-0.1060***		
	(0.0071)		
Scraped experience		-0.1316***	
		(0.0092)	
Scraped relevant experience			-0.1412***
			(0.0090)
Bachelor's		-0.1225	-0.1812
		(0.2069)	(0.2081)
Master's		0.0734	-0.0178
		(0.1141)	(0.1140)
Ph.D.		0.3679	0.1913
		(0.2677)	(0.2649)
Standard deviations	1.90	2.23	2.18
Probability under null hypothesis of no gender pay gap	5.71%	2.51%	2.96%
Observations	13,766	13,766	13,766

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted stock grants in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Education is assumed to be additive such that an individual with a Ph.D. as the highest degree is also coded as having a Bachelor's degree and a Master's degree, and an individual coded as having a Master's degree is also coded as having a Bachelor's degree. Other job controls included are as described in notes to Exhibits 28 and 29. Standard errors are clustered by person IDs. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level, * denotes statistical significance at the 10% level.

Exhibit 46: Regressions with Scraped Experience and Highest Degree, Adjusted Total Compensation

	Observations with resume information	Observations with scraped data	Observations with scraped data
Female	-0.0427***	-0.0423***	-0.0425***
	(0.0075)	(0.0074)	(0.0074)
Age as experience	-0.0011**		
	(0.0004)		
Scraped experience		0.0010*	
		(0.0005)	
Scraped relevant experience			0.0005
			(0.0005)
Bachelor's		-0.0019	-0.0028
		(0.0126)	(0.0126)
Master's		0.0268***	0.0265***
		(0.0071)	(0.0072)
Ph.D.		0.0733***	0.0743***
		(0.0172)	(0.0172)
Standard deviations	5.71	5.74	5.75
Probability under null hypothesis of no gender pay gap	< 1 in 10 million	< 1 in 10 million	< 1 in 100 million
Observations	13,766	13,766	13,766

Note: Each column shows estimates from a separate regression. The dependent variable is the inverse hyperbolic sine (IHS) of adjusted total compensation in 2017 dollars, so coefficient estimates approximate the percentage effect of the variable on pay. Education is assumed to be additive such that an individual with a Ph.D. as the highest degree is also coded as having a Bachelor's degree and a Master's degree, and an individual coded as having a Master's degree is also coded as having a Bachelor's degree. Other job controls included are as described in notes to Exhibits 33 and 34. Standard errors are clustered by person IDs. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level, * denotes statistical significance at the 10% level.

Exhibit 47: Damages Calculations

	Medicare Wages	Total Compensation
Annual pay gap (%), 2013-2017	6.26%	5.76%
Source of estimate	Exhibit 19, col. (6)	Exhibit 34, col. (6)
Mean male compensation (of regression sample), Dec. 2017 dollars	[redacted]	
Annual pay gap per employee ($)	[redacted]	
Number of female-year observations (in three job functions and non-campus hires), 2013-2017, with compensation information and days worked in the year	15,582	15,968
Estimated total damages (unadjusted for days worked), Dec. 2017 dollars	[redacted]	
Estimated total damages (adjusted for actual days worked), Dec. 2017 dollars	[redacted]	

Exhibit 48: Damages Calculation (Using Regression Samples Only)

	Medicare Wages	Total Compensation
Annual pay gap (%), 2013-2017	6.09%	5.43%
Source of estimate	Exhibit 19, col. (6)	Exhibit 34, col. (6)
Mean male compensation (of regression sample), Dec. 2017 dollars	[redacted]	
Annual pay gap per employee ($)	[redacted]	
Number of female-year observations (in regression sample), 2013-2017, with compensation information and days worked in the year	15,548	15,934
Estimated total damages (unadjusted for days worked), Dec. 2017 dollars	[redacted]	
Estimated total damages (adjusted for actual days worked), Dec. 2017 dollars	[redacted]	

Appendix A: Details on Data and Tabulations

A1. Information on gender of Oracle employees comes from *ORACLE_JEWETT_00001180*, in the tab labeled "Emp Hire_Term & Personal Info."

A2. The base rate of pay is found in *ORACLE_JEWETT_00001166*, in the tab labeled "Salary Window," and in *ORACLE_JEWETT_00030955*, in the tab labeled "Base Pay." Both data files are structured to record changes in the base rate of pay for employees. I use the variable "New Salary" in *ORACLE_JEWETT_00030955*, and the variable "annual" in *ORACLE_JEWETT_00001166*, which for both data files provide measures of an annualized salary amount.

A3. I use the data files described above to create data by year that record base pay for each employee in each year. I use the base salary at the end of the year. I also use hire and termination dates, which are in *ORACLE_JEWETT_00001180*, in the tab labeled "Emp Hire_Term & Personal Info," to identify start and end dates for employees. This ensures that I do not carry forward records when an employee has already left the company.

A4. There was a question of which data source to use – *ORACLE_JEWETT_00001166* or *ORACLE_JEWETT_00030955*. By comparing base salaries by year and employee using the two data sources, I verified that there are only minor discrepancies between the two sources. *ORACLE_JEWETT_00001166* appears to cover more employees than are covered in *ORACLE_JEWETT_00030955*, but *ORACLE_JEWETT_00030955* has more recent pay information, with more salary changes recorded for 2018, which is consistent with the fact that I was provided with this data set at a later date. The number of discrepancies between the two data sources is very small, and hence I have not tried to reconcile them. I use the data from *ORACLE_JEWETT_00030955* in my

analysis, given that it has more recent data.

A5. Data on Medicare wages comes from *ORACLE_JEWETT_00001167*, which contains a variable called "balance_name" that takes on 166 unique string (i.e., character) variables ("Medicare EE Taxable," "Regular Earnings," "Nonqual Stock Opt," etc.).[1] Each of these is associated with a value for all employees in each year from 2003-2018, which is the associated amount in each of those years.

A6. I reshape the data in *ORACLE_JEWETT_00001167* to create a record for earnings for each employee in each year. I also drop all zero and negative values for Medicare wages, which constitute 0.6% of the observations; I assume that these entries are errors. Finally, there are some duplicate entries for people in a year, which occurs for two reasons. First, there are two employee identification variables, one labelled "person_id" and one labelled "employee_number." In some instances, a single person id can contain multiple employee number records, creating identical duplicate records with the same Medicare wage number.[2] But it seems clear that in this case the duplicate entry should simply be removed. Second, a single id can contain multiple assignment number records. To my understanding, assignment numbers are generally generated when an employee goes through the normal application cycle in Oracle. Thus, an employee that left Oracle and reapplied can have multiple assignment numbers.[3] In the rare event that an employee left Oracle and returned the same year, their actual Medicare wage is the sum of the records.

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[1] "Medicare ER Taxable" and "Medicare EE Taxable" are identical and either can be used to construct Medicare wages.

[2] *[Oracle-Jewett] 2018.08.03 Mantoan to Finberg Continuing Responses to Data Questions* answer to 22.3 indicates that person ids may receive a new employee number when rehired into a different Oracle entity.

[3] Confirmed by *[Oracle-Jewett] 2018.06.29 Mantoan to Finberg Continuing Responses to Data Qs* answer to 22.6 and 22.7.

A7. As an example, the data for "Reed, Lonnie L" illustrates both issues, resulting in four records being generated, as shown in Exhibit A.1. The assignment records in *ORACLE_JEWETT_00001180* show that Mr. Reed left Oracle on March 21, 2017, and was rehired on September 12, 2017. It is unclear why the employee numbers are duplicated, and I exclude the duplicate value for employee number (which has the identical Medicare wage amount). But I add up the Medicare wages for the different assignment numbers. Thus, Mr. Reed's 2017 Medicare wages would be [redacted].

A8. To adjust Medicare wages for partial years worked, I measure days worked in a year based on information in *ORACLE_JEWETT_00001180* on when employee's "User Status" is "active assignment." My best current understanding is that this will not account for leaves, although I have not received a satisfactory explanation from Oracle as to how to separate paid from unpaid leave. I expect some measurement error that results in Medicare wages being too high when I have treated days on paid leave as days not working.

A9. Information about bonuses is in *ORACLE_JEWETT_00030955*, in the tab labeled "bonus," and in *ORACLE_JEWETT_00001167*, under specific categories in the variable "balance name." For the data in *ORACLE_JEWETT_00030955*, I remove what appear to be mistaken duplicate values (when multiple entries for a bonus have the same Person_ID, Bonus Type, date, and amount). For example, in *ORACLE_JEWETT_00030955*, "Lin, Mr David Cheng Fang" (GSI_PERSON_ID = 139472) [redacted]. To my understanding, [redacted]. I exclude severance-related compensation. Data in *ORACLE_JEWETT_00030955* are used as an annual total; there are sometimes multiple bonuses in a year. The data from

ORACLE_JEWETT_00001167 had to be reshaped to provide data by employee by year.[4] Due to the data structure, when a value is missing, it can be assumed to be zero for that year either due to not being employed that year or an actual zero bonus. If an employee had regular earnings that year but missing bonuses, I assumed the employee did not receive any bonus that year (i.e., I treat the bonus as zero). In addition, I had to eliminate duplicate cases. The issues are the same as for Medicare wages, as bonuses come from the same report, and the procedures I used were hence the same.

A10. Finally, I had to decide which report to use – *ORACLE_JEWETT_00030955* or *ORACLE_JEWETT_00001167*. *ORACLE_JEWETT_00001167* provides more details about specific bonus categories. The categories in *ORACLE_JEWETT_00030955*, which overlap with categories in *ORACLE_JEWETT_00001167*, record annual or quarterly bonuses only. However, *ORACLE_JEWETT_00030955* is more recent, and it even contains what appear to be 2018, third quarter bonuses ("Q3 OCG Bonus for 2018") that are not in *ORACLE_JEWETT_00001167*. Furthermore, I did not identify any discrepancies in the bonus amount of the matched data between the two sources; hence, when data appear in both sources, it does not matter which source I use. Because *ORACLE_JEWETT_00030955* provides more recent information for 2018, I use it for annual and quarterly bonus categories. However, since some bonus categories are reported only in *ORACLE_JEWETT_00001167*, I use the latter source for those bonus categories.

A11. The next non-salary components of pay I consider are stock grants. These are

[4] From 2013-2018, there are 45 observations with Hypo Bonus, 1 observation on Executive bonus, and 2 observations on Sign-On Bonus that take on negative values. There were 22 observations with specific reported zero values. Generally, as discussed in the text, data on bonuses are shown as missing rather than zero. The observations referenced in this footnote are treated as zero if there were regular earnings in that year, or otherwise dropped.

reported in *ORACLE_JEWETT_00001167*, in the form of either "Restricted Stock Units" and "NonQual Stock Opt" (under the category "balance_name").[5] To treat stock grants parallel to other forms of compensation, I do the best, based on my current understanding of the data, to assign the taxable component of compensation associated with stock grants to the appropriate year. My understanding is that the taxable event for RSUs is when they vest 25% automatically for four years and the taxable event for options is when they are exercised.[6] To the best of my understanding, an "Exercised ISO" is not taxable and thus I exclude them from my analysis.[7] I also exclude "RSU DEF INCOME," "RSU DEF VEST EMP," and "RSU Tax Refund," since it is not clear that these are components of RSUs. However, it is clear that "Restricted Stock Units" and "Non Qual Stock Options" are considered taxable. That said, I want to emphasize that I have not been provided with a complete understanding of all of these items; additional information provided later could alter how I treat some of these data and hence could – in principle, at least – change my conclusions.

A12. As before, I use *ORACLE_JEWETT_00001167* to construct measures for each

[5] There is another report, *ORACLE_JEWETT_00030955*, which contains information about shares of stocks granted. Examples of the variables in that report are given below:

	Mean	Min	Max	# of obs. with 0s
TotalShares	[redacted]	0	[redacted]	11
OptionPrice	[redacted]	0	[redacted]	16,635
VestedShares	[redacted]	0	[redacted]	18,065
UnvestedShares	[redacted]	0	[redacted]	16,635
UnvestedGain	[redacted]	0	[redacted]	3,439

UnvestedValue is blank. There are 22,319 obs.

Due to the number of observations with 0s, particularly under options price, I am unable to determine the best way to convert these shares to monetary values, and hence do not use this information.

[6] *Waggoner Transcript Excerpt* re RSUs (line 152:19-25).
[7] Nicholas Sanchez email 9/5.

employee in each year. The approach is similar to bonuses, where I assume a zero in lieu of a missing value if regular earnings are present for that year.[8] I have to drop duplicate entries; the issue is the same as for Medicare wages, described above, as is my procedure for dropping duplicates.

A13. To try to understand whether there valid components omitted from the measure of Medicare wages, I attempted to reconstruct Medicare wages using the various components within "balance_name" in *ORACLE_JEWETT_00001167*. In particular, I added up "Regular Earnings," the listed bonus variable names, "Restricted Stock Units," and "NonQual Stock Opt." Regular Earnings appears to be a combination of various components within "balance name," including "Regular Salary" for salaried employees and "Regular Wages" for hourly employees. There are other components that make up Regular Earnings. However, I have not been provided with information explaining exactly which components will add up to Regular Earnings.[9] Although my best current understanding is that adding up regular earnings, bonuses, and stock grants should provide a total compensation measure equal to Medicare wages, my examination of the data indicates that this is not the case, implying that the discrepancies are not simply attributable to how these variables are defined.

A14. Information on job controls can be found in from *ORACLE_JEWETT_00001180*. The data are structured to record only changes in assignment status. I create an employee-by-year data set based on the job information from the last record for the year (from Effective Start

[8] From 2013-2018, there are 63 observations of RSUs specifically reported as zero and one observation of options reported as zero. Generally, RSU and options are shown as missing in the data rather than reported as zero. There are two negative observations for options, but these are in 2005. I dropped these observations.

[9] *[Oracle-Jewett] 2018.10.05 Mantoan to Finberg Responses to Remaining Dat...* refused to answer questions 29-32 about what components of balance name add up to Medicare Wages and Regular Earnings.

Date). When there is no available salary information for a year, I assume the employee did not work in that year and drop that employee-year record.

A15. Appendix Exhibits A.3-A.7 show information on the distributions of many of these job controls by gender. In every case, the distributions differ by gender (based on Chi-square tests of independence reported in notes to the tables), indicating that adding these controls to the pay regressions could influence the estimated gender differences. Most employees are concentrated in five lines of business headed by Mr. Kurian, Mr. Fowler, Mr. Hurd, Mr. Screven, and Ms. Catz (Exhibit A.7).

A16. I construct tenure using information from *ORACLE_JEWETT_00001180*, in the tab "Emp Hire_Term & Personal Info." "HIRING_DATE" records the date an employee was hired, and "ACTUAL_TERMINATION_DATE" the date they were terminated (if they were). There can be multiple records corresponding to multiple employment spells with Oracle, if an employee left Oracle was re-hired later. Their first hiring date is captured in "DateFirstHired." I use these data to construct a measure of Oracle tenure as of the end of each year, to align with the other data. To do this, I look at the total amount of time an employee has worked for Oracle by the end of the year, defined as December 31st.[10] For 2018, I use May 31st, 2018, owing to dates for which some data were provided. For employees who return to Oracle, previous employment with Oracle is included in current Oracle tenure.

A17. The data include large clusters of hire dates that are associated with becoming part of Oracle via a merger/acquisition (M&A). For example, 2,751 people have a hire date of Feb 15, 2010, from Oracle's acquisition of Sun Microsystems. Because skills and experience from employment at merged/acquired companies might be similar to that stemming from

[10] For terminated employees, I calculate tenure to the last day of their employment of that year.

employment at Oracle, I also construct an additional tenure variable that includes time worked in a merged/acquired company. I measure this from the variable "ContinuousServiceHireDate," which tracks their first date at the acquired company, and incorporate the date provided there.[11] I call the latter measure "overall tenure," and the Oracle-specific measure "Oracle tenure."

A18. Appendix Exhibits A.8 and A.9 provide summary information on these tenure variables. As shown in Appendix Exhibit A.8, Oracle tenure is higher for women than for men, overall and for every year; and the difference is always statistically significant at less than the 0.1% significance level. The differences for overall tenure (Appendix Exhibit A.9) are similar, but the differences are not as large, implying that companies that become part of Oracle via M&A have relatively higher-tenure men.

A19. I also construct a measure of the time a person has been in a specific job role. This is constructed based on the assignment information tab in *ORACLE_JEWETT_00001180.* Job tenure is measured as the total time spent on active assignment in a specific job role, so job tenure will not include any leaves an employee has taken. I define job tenure as of December 31st of each year (with the exception for 2018 noted above), to align with the other data I construct.

A20. Appendix Exhibit A.10 provides descriptive information on job tenure. Across all years, and by year, female job tenure is always slightly higher than male job tenure at Oracle.

A21. Constructing potential experience requires approximation. I begin by

[11] Three employees, Le, Anh Vu T (person id = 891544929), Philip Chacko, Mr Anish (person_id = 891935193), and Shah, Nishant Dharmesh (person_id = 894131042) have a Continuous Service Hire Date that is after their Date First Hired, which appears to be during their employment at Oracle, making it unlikely that the Continuous Service Hire Date represents their first hire date from a merged or acquired company. For these three employees, I use their given Hire Dates to calculate overall tenure and ignore the Continuous Service Hire Dates.

constructing age from birth dates in *ORACLE_JEWETT_00001180*, which are in the tab "Emp Hire_Term & Personal Info." For each year an employee is in the data set and has pay data available (in the form of rate of pay, Medicare wages, or total compensation), I define age as of the end of the calendar year for all years except 2018 (where we use our stopping date of May 31). From this point I have to approximate. I do not have direct date from Oracle on prior work experience. I also do not have information on either level of education attained, or the timing of that education. I make the assumption that employees at Oracle have a college degree, and completed it at age 22, and I assume continuous work after leaving school. This gives me an approximate experience measure which should be viewed as "potential experience," measured as age minus 22.[12] If observed age is less than 22, I use zero for experience rather than a negative value.[13]

A22. I next describe my creation of data on prior labor market experience and on schooling from resumes. The first step to creating the experience measure was to match names to the text documents of Production 24. This was done using a combination of Python programming and hand coding. The end result of this process is that I was able to match 92% of the unique names in the data set to a file in the production. Some names were matched to multiple files (this will be discussed later), and some files were matched to multiple names; these were dropped.

A23. The next step was to scrape the employment spell information from the text documents. The first step of this process is to scrape the experience portion of the resume. This

[12] This construction of potential experience is common in labor economics datasets when a researcher does not have a full work history. See, e.g.: Lemieux, Thomas, 2006, "The "Mincer Equation" Thirty Years After Schooling, Experience, and Earnings, in Grossbard, Ed., Jacob Mincer: A Pioneer of Modern Labor Economics (Boston, MA: Springer), pp. 127-45.
[13] This affects 30 observations in total for 2013-2018.

is done by using a list of resume sections created from the resumes themselves. A good portion of the resumes have their section separating lines in all capitals, such that these can be scraped to create a list of sections headers, e.g., "WORK EXPERIENCE," "EDUCATION," "AWARDS," and "VOLUNTEERING." I keep all items in the list that appear in more than five resumes. The next step is to look through and find the section headers that indicate the start of the experience section. These are used to mark the beginning of the experience section, and then to mark the end of the experience section by the appearance of any of the other section headers.

A24. Once the experience information is scraped from the resume, the information is used to identify employment spells. This is done by looking for two dates on a line, which usually appear as something like "May 2012 - June 2012." A good deal of data cleaning had to be done in order to detect as many employment spells as possible. For example, if a date/spell was listed as "May 2012 to Present," the proposed start date at Oracle was substituted for "Present."

A25. In total, employment spells could be scraped for 75.5% of the observations in the resume data. I construct a prior experience measure by summing over the non-overlapping portions of these spells, based on their start and ending dates.

A26. With these data constructed, I could resolve cases of multiple resume files matched to one name (mentioned above). The scraped employment spells were used to select the resume most likely to correspond to our observation (i.e., the actual beginning of employment for a person in our data). This procedure should retain the resume that is closest to the latest job start date but not after it.

A27. I also used the resumes to fill out the sparse information on highest educational degree earned in the Oracle data. This entailed searching for degree-related keywords such as "master's," "bachelor's," etc.

A28. Finally, I used the highest degree information to create a "relevant" experience measure by only including the post-schooling experience of an individual. I estimate the end of schooling year by adding an individual's years of schooling to their birthday – assuming they attended schooling continuously from age 6. Years of schooling was defined as 18 years if they had no highest degree, 22 years if highest degree was a bachelors, 24 years for a masters, and 27 years for a Ph.D.

A29. After developing these methods, I implemented a number of procedures to check the data quality, since the scraping procedure can generate errors. For highest degree earned, I selected 50 observations randomly, and hand checked the scraped data versus the actual resumes. The highest degree was correct for 84% of cases. Of these 50 cases, there were 8 errors found: first, there was 1 case where a Master's degree was incorrectly assigned to an individual with only a Bachelor's degree; second, in 2 cases no education information was detected when the individual had a Bachelor's degree; third, in 1 case a Bachelor's degree was assigned when the individual had only completed 3.5 years of college; fourth, in 1 case a Bachelor's degree was assigned to an individual that had also completed a Master's degree; fifth, in 1 case no education information was detected for an individual with a Master's degree; and sixth, in 2 cases a Ph.D. was assigned even though the person had not yet completed it (but one would within the year, and the other had advanced to candidacy). This is a fairly small share of errors, and often the errors are not large or might not even represent errors (the last case, in particular).

A30. I also created flags for possibly erroneous measurement of experience from the scraped data. Cases were flagged as "Too high" for individuals whose work experience was greater than their age minus 18, as that would imply continuous work since before graduating high school. Cases were flagged as "Too low" for individuals for individuals for whom experience/(age – estimated post-schooling age) < .4. (Post-schooling age is defined as 18 for those with no highest degree information, 22 for those with a Bachelor's, 24 for those with a Master's, and 27 for those with a Ph.D. Thus, this flag detects those who worked less than 40% of the time since leaving school, based on our data and approximations.) It is not clear there are errors, but in the empirical analysis, I explore the sensitivity of the results to dropping these flagged cases.

A31. Finally, I also hand checked 50 randomly chosen resumes to compare directly the information on the resumes to what was scraped. There were zero errors in name matching in this sample, and very few non-trivial errors. Appendix Exhibit A.11 shows these errors. There are two findings of note. First, the errors tend to be very small. And second, the error flags described in the previous paragraph capture many of these.

A32. Data on job performance can be found in *ORACLE_JEWETT_00030954*, *ORACLE_JEWETT_00030955*, *ORACLE_JEWETT_00007303*, *ORACLE_JEWETT_00007312*, *ORACLE_JEWETT_00001170*, *and ORACLE_JEWETT_00001173*.[14] The reports in *ORACLE_JEWETT_00030954* and *ORACLE_JEWETT_00030955* are clearly structured to show that their purpose is to record performance data, while *ORACLE_JEWETT_00007303*,

[14] Performance ratings in *ORACLE_JEWETT_00001170* are given in the format "4001-4005" rather than the 1-5 scale from the other reports. I assume 4001 is equivalent to 1, 4002 is equivalent to 2, etc. The distributions of performance ratings in the two reports are consistent with this.

ORACLE_JEWETT_00007312, *ORACLE_JEWETT_00001170*, and *ORACLE_JEWETT_00001173* appear to track equity and stock grants, bonuses, or salary increases, often followed by a performance rating, which is perhaps included as justification for the incentive payment. I organize each of these reports on an employee-by-year basis, and in the event of multiple ratings, assign the last reported rating for a year.[15] *ORACLE_JEWETT_00030954* and *ORACLE_JEWETT_00007312* provide a range of dates rather than a specific date; I use the end date.[16]

A33. There are discrepancies between the performance ratings in the different reports using the last record for the year between each report. Among the employees with discrepancies between ratings, 99.97% of them have a different last date assigned to them. Oracle claims that performance rating discrepancies can be due to two separate systems tracking performance, either from the appraisal tool, GSI (corresponding to Compensation Workbench (CWB)), or from Fusion (corresponding to Fusion Workforce Compensation (FWC)). Oracle notes that *ORACLE_JEWETT_00030954* corresponds to the appraisal tool; *ORACLE_JEWETT_00030955*, *ORACLE_JEWETT_00001173*, and *ORACLE_JEWETT_00007303* correspond to CWB; and *ORACLE_JEWETT_00007312* and *ORACLE_JEWETT_00001170* correspond to FWC. CWB and FWC are compensation-related tools and Oracle transitioned from CWB to FWC around

[15] For *ORACLE_JEWETT_00007303*, I use "plan_date" to determine the date of the rating. For *ORACLE_JEWETT_00001170*, I use "AuditDate" to determine the date of the rating. For *ORACLE_JEWETT_00030955*, I use "Date" to determine the date of the rating. For *ORACLE_JEWETT_0001173*, I use "Plan_Date" to determine the date of the rating. In the event of duplicates on the same date, I take the record with the most recent date and time from "Change_Date" and "Change_Time." The remaining duplicates in this report do not conflict.

[16] *ORACLE_JEWETT_00030954* has an appraisal start date, appraisal end date, and appraisal template name that gives year indications, e.g., "FY 16 Appraisal Template." They sometimes conflict and I used appraisal end date regardless. In the event of duplicate records even within the same end date in *ORACLE_JEWETT_00030954*, I use the latest record determined by the "last_update_date."

2014-2015.[17] However, there are discrepancies even within these pairs of reports, so differences in tracking systems cannot be the full explanation.

A34. The discrepancies are documented in Appendix Exhibit A.12. Given these discrepancies, and given Oracle's assertion that there is "no single way to identify the most accurate and complete data regarding performance review,"[18] I use the average of performance rating across these reports (always using last ratings for a year, to correspond to how I treat the compensation data).

A35. I also compare, in Appendix Exhibit A.13, performance ratings across the formal and informal appraisals. (The columns labeled "30954 rating" are from the formal appraisal tool.) This table suggests that performance ratings of women and men are nearly identical under the formal appraisal system, but that ratings for men are somewhat higher under the informal appraisal systems. Given that I would expect the formal system to be more objective, this evidence bolsters my concerns about performance ratings being tainted by bias against women, especially for the informal ratings.

A36. Appendix Exhibit A.14 reports descriptive information on these average performance ratings. The exhibit shows that, on average, men have slightly higher performance ratings, and that this is true of all years except one. As we saw in Appendix Exhibit A.13, this is driven mainly by the informal ratings.

A37. I also estimated gender differences in performance ratings once I condition on the same kinds of job and worker controls I use in my regressions for pay. These estimates are more

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[17] *[Oracle-Jewett] 2018.10.25 Ltr to Finberg from Mantoan re 2018.10.11 Data Production Questions.*

[18] As stated in *[Oracle-Jewett] 2018.10.25 Ltr to Finberg from Mantoan re 2018.10.11 Data Production Questions.*

informative about how performance ratings differ by gender for women and men with similar skills who are doing similar jobs. The estimates, reported in Appendix Exhibit A.15, show that there is no overall performance rating difference between men and women, but that women receive slightly higher ratings on the formal system (the estimate is 0.03, or 2.3 standard deviations).

A38. One other issue is that there are many fewer observations with data on performance ratings than on the other variables discussed thus far (with the exception of the schooling and experience measures constructed from the resumes). Moreover, we know that this happens, at least to some extent, because Oracle does not have a regular performance appraisal for every employee.[19] Appendix Exhibit A.16 compares means of key variables across the observations with non-missing and missing performance data. The observations missing performance data are more likely to be male, and are also higher paid on all five measures I use, and have lower tenure and experience. I have not been provided with any explanation of who gets a performance review that might help me understand this pattern. Thus, while I report results using performance measures, I do not regard the analyses using performance data as my main analyses, because of the potential issue of missing data. That said, I have no evidence to suggest the relationship between pay, gender, and performance ratings would be different for the observations for which performance ratings are missing.

[19] *[Oracle-Jewett] 2018.10.25 Ltr to Finberg from Mantoan re 2018.10.11 Data Production Questions* acknowledges that not all employees receive a performance rating.

Appendix Exhibit A.1: Example of Data Issues for Medicare Wages (Reed, Lonnie L)

PERSON_ID	EMPLOYEE_NUMBER	ASSIGNMENT_NUMBER	YEAR	MEDICARE WAGE
890231761	541473	111855	2017	[redacted]
890231761	111855	111855	2017	[redacted]
890231761	541473	541473	2017	[redacted]
890231761	111855	541473	2017	[redacted]

Appendix Exhibit A.2: Job Codes (Top 30) (2013-2018)

	Female	Male	Total
10540	23.3%	76.7%	[redacted]
10550	13.6%	86.4%	[redacted]
10530	30.9%	69.1%	[redacted]
10030	21.9%	78.1%	[redacted]
10040	20.1%	79.9%	[redacted]
100340	15.6%	84.4%	[redacted]
10520	34.3%	65.7%	[redacted]
10740	33.1%	66.9%	[redacted]
10050	13.1%	86.9%	[redacted]
10750	26.3%	73.7%	[redacted]
17150	34.6%	65.4%	[redacted]
90023	33.5%	66.5%	[redacted]
17140	45.8%	54.2%	[redacted]
10730	30.4%	69.6%	[redacted]
10060	12.0%	88.0%	[redacted]
100330	25.7%	74.3%	[redacted]
76540	30.0%	70.0%	[redacted]
66684	40.8%	59.2%	[redacted]
10020	24.2%	75.8%	[redacted]
100320	24.9%	75.1%	[redacted]
10560	4.4%	95.6%	[redacted]
10840	49.7%	50.3%	[redacted]
17250	19.6%	80.4%	[redacted]
17240	27.2%	72.8%	[redacted]
13540	59.6%	40.4%	[redacted]
90122	34.9%	65.1%	[redacted]
90230	23.0%	77.0%	[redacted]
10830	55.9%	44.1%	[redacted]
100350	10.2%	89.8%	[redacted]
75030	17.4%	82.6%	[redacted]

A Chi-square test of independence, based on all job codes, rejects independence of job level at gender at less than the 0.1% significance level.

Appendix Exhibit A.3: Job Grades (2013-2018)

	Female	Male	Total
e.00X	5.9%	94.1%	[redacted]
e.04	62.7%	37.3%	[redacted]
e.05	29.8%	70.2%	[redacted]
e.06	35.2%	64.8%	[redacted]
e.07	56.0%	44.0%	[redacted]
e.08	31.9%	68.1%	[redacted]
e.09	27.9%	72.1%	[redacted]
e.10	33.0%	67.0%	[redacted]
e.11	20.5%	79.5%	[redacted]
e.12	19.4%	80.6%	[redacted]
e.13	16.2%	83.8%	[redacted]
e.14	15.8%	84.2%	[redacted]
e.15	13.0%	87.0%	[redacted]
N.01	100.0%	0.0%	[redacted]
N.03	0.0%	100.0%	[redacted]
N.04	0.0%	100.0%	[redacted]
N.05	68.8%	31.3%	[redacted]
N.06	42.2%	57.8%	[redacted]
N.07	18.1%	81.9%	[redacted]
N.09	27.8%	72.2%	[redacted]
N.10	32.6%	67.4%	[redacted]
N.11	26.8%	73.2%	[redacted]
N.12	33.9%	66.1%	[redacted]
N.13	41.9%	58.1%	[redacted]
N.14	24.4%	75.6%	[redacted]
N.15	25.0%	75.0%	[redacted]
N.99	5.0%	95.0%	[redacted]

A Chi-square test of independence rejects independence of job level at gender at less than the 0.1% significance level.

Appendix Exhibit A.4: Global Career Levels (2013-2018)

	Female	Male	Total
IC0	4.6%	95.4%	[redacted]
IC1	33.6%	66.4%	[redacted]
IC2	34.7%	65.3%	[redacted]
IC3	34.2%	65.8%	[redacted]
IC4	29.1%	70.9%	[redacted]
IC5	23.9%	76.2%	[redacted]
IC6	6.6%	93.4%	[redacted]

A Chi-square test of independence rejects independence of job level at gender at less than the 0.1% significance level.
"IC" indicates "Individual Contributor."

Appendix Exhibit A.5: Global Career Levels (2013-2018)

	Female	Male	Total
M1	22.7%	77.3%	[redacted]
M2	27.8%	72.2%	[redacted]
M3	24.4%	75.6%	[redacted]
M4	23.7%	76.3%	[redacted]
M5	17.6%	82.5%	[redacted]
M6	14.3%	85.7%	[redacted]
M7	6.1%	93.9%	[redacted]
M8	0.0%	100.0%	[redacted]

A Chi-square test of independence rejects independence of job level at gender at less than the 0.1% significance level.
"M" indicates "Manager."

Appendix Exhibit A.6: Work Location Zip Code (Top 20)
(2013-2018)

	Female	Male	Total
94065	26.9%	73.1%	27,726
95054	24.0%	76.0%	15,537
94588	39.1%	60.9%	6,240
94002	26.4%	73.6%	2,773
95110	32.3%	67.7%	2,465
92121	15.2%	84.8%	1,270
94111	20.5%	79.5%	1,083
92614	21.0%	79.0%	1,070
94066	23.5%	76.5%	784
95765	26.2%	73.8%	618
94403	25.4%	74.6%	425
91006	19.1%	80.9%	408
94107	27.1%	72.9%	329
95014	21.2%	78.8%	274
90245	26.5%	73.5%	264
90230	26.6%	73.4%	233
91436	27.6%	72.4%	203
94612	11.9%	88.1%	143
94025	10.7%	89.3%	140
94568	47.5%	52.5%	118

A Chi-square test of independence rejects independence of job level at gender at less than the 0.1% significance level.

Appendix Exhibit A7: Distribution of Employees of Line of Business Heads

	Female	Male	Total
Catz, Ms Safra A. (Safra)	36.74%	63.26%	[redacted]
Fowler, John F	23.52%	76.48%	[redacted]
Hurd, Mark	29.64%	70.36%	[redacted]
Kabcenell, Mr Dirk A	0.00%	100.00%	[redacted]
Kurian, Mr Thomas	28.21%	71.79%	[redacted]
Lynn, Mr Lawrence S	50.00%	50.00%	[redacted]
Screven, Mr Edward L	19.61%	80.39%	[redacted]

A Chi-square test of independence rejects independence of job level at gender at less than the 0.1% significance level.

Appendix Exhibit A.8: Oracle Tenure (2013-2018)

Year	# of employees	Mean Oracle tenure	Mean female Oracle tenure	Mean male Oracle tenure	Difference in means	t-stat [p-value for test of equality]
Total	67,725	[redacted]			0.919	17.5956 [0.0000]
2013	10,980				0.802	6.9886 [0.0000]
2014	11,713				0.882	7.5877 [0.0000]
2015	11,917				0.935	7.7549 [0.0000]
2016	12,011				0.919	7.3051 [0.0000]
2017	11,864				0.950	7.2383 [0.0000]
2018	9,240				1.131	7.2026 [0.0000]

Appendix Exhibit A.9: Overall Tenure (2013-2018)

Year	# of employees	Mean overall tenure	Mean female overall tenure	Mean male overall tenure	Difference in means	t-stat [p-value for test of equality]
Total	67,725	[redacted]			0.466	7.3935 [0.0000]
2013	10,980				0.305	2.2723 [0.0231]
2014	11,713				0.423	2.9923 [0.0028]
2015	11,917				0.465	3.1079 [0.0019]
2016	12,011				0.457	2.9024 [0.0037]
2017	11,864				0.485	2.9921 [0.0028]
2018	9,240				0.745	4.1153 [0.0000]

Appendix Exhibit A.10: Job Tenure (2013-2018)

Year	# of employees	Mean job tenure	Mean female job tenure	Mean male job tenure	Difference in means	t-stat [p-value for test of equality]
Total	67,448	[redacted]			0.231	7.7504 [0.0000]
2013	10,941				0.226	3.6230 [0.0003]
2014	11,659				0.214	3.2821 [0.0010]
2015	11,872				0.266	3.8534 [0.0001]
2016	11,973				0.209	2.8847 [0.0039]
2017	11,822				0.219	2.8587 [0.0043]
2018	9,181				0.310	3.3611 [0.0008]

Appendix Exhibit A.11: Error Checks for Scraped Experience

Errors (True Experience/Estimated Experience) – 1	Error Level (Years)	# of observations	Caught by flag
3	9	1	yes
3	6	1	yes
1.5	6	1	no
1.44	13	1	yes
1.27	16	1	no
.93	7	1	no
.83	5	1	yes
.66	8	1	no
.57	8	1	no
.18	5	1	no
.06	1	1	no
.04	1	1	no
.009	.1	1	no
No errors	No errors	37	not applicable

Appendix Exhibit A.12: Performance Rating Discrepancies (2013-2017)

Year	Combined # of employees	% with different ratings (all)	Avg. number of reports/person	*30955* # of employees	*7303* # of employees	1173 # of employees	% with different ratings (CWB)	*7312* # of employees	*1170* # of employees	% with different ratings (FWC)	*30954* # of employees (formal)
Total	45,830	7.2%	2.07	17,632	12,250	16,070	6.0%	19,783	7,241	0.09%	21,982
2013	10,137	13.2%	2.55	7,836	4,674	7,636	7.7%	0	0		5,653
2014	10,697	10.4%	2.89	9,796	7,576	8,434	4.5%	0	0		5,068
2015	4,314	0.0%	1	0	0			0	0		4,314
2016	10,867	5.2%	1.89	0	0			10,521	5,692	0.2%	4,327
2017	9,815	2.9%	1.37	0	0			9,262	1,549	0.0%	2,620

Appendix Exhibit A.13: Average Performance Ratings Across Reports

Year	*30954* rating (formal)		*30955* rating (informal)		*7303* rating (informal)		*1173* rating (informal)		*7312* rating (informal)		*1170* rating (informal)	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
2013	[redacted]											
2014												
2015												
2016												
2017												

Appendix Exhibit A.14: Descriptive Statistics on Performance Ratings (2013-2017)

Year	# of employees	Mean performance rating	Mean female performance rating	Mean male performance rating	Difference in means	t-stat [p-value, test of equality]
Total	45,830	[redacted]			-.07	-8.5396 [0.0000]
2013	10,137				-.07	-4.2380 [0.0001]
2014	10,697				-.08	-4.7524 [0.0000]
2015	4,314				.02	1.0333 [0.3015]
2016	10,867				-.08	-5.0568 [0.0000]
2017	9,815				-.07	-4.1553 [0.0000]

Appendix Exhibit A.15: Estimated Performance Rating Differences, 2013-2017, from Regressions with Controls for Job Characteristics and Tenure

	Performance Rating (Mean)	Performance Rating (Formal - 30954)	Performance Rating (Informal – Mean of Non-30954 Reports)
	(1)	(2)	(3)
Female	0.0021	0.0338**	-0.0067
	(0.0107)	(0.0146)	(0.0113)
Oracle Tenure	0.0131***	0.0085***	0.0132***
	(0.0014)	(0.0021)	(0.0015)
Overall Tenure	0.0061***	0.0017	0.0071***
	(0.0011)	(0.0015)	(0.0012)
Job Tenure	-0.0157***	-0.0127***	-0.0166***
	(0.0018)	(0.0027)	(0.0019)
Experience	-0.0215***	-0.0178***	-0.0225***
	(0.0007)	(0.0009)	(0.0007)
Standard deviations	0.20	2.31	0.59
Probability under null hypothesis of no gender pay gap	84.12%	2.11%	55.30%
Observations	45,717	21,975	38,417

Note: Each column shows estimates from a separate regression. The dependent variable is the performance rating score (from 1-5). Standard errors are clustered by person IDs. Regression includes job and tenure controls as described in Exhibits 13 and 14. *** denotes statistical significance at the 1% level, ** denotes statistical significance at the 5% level.

Appendix Exhibit A.16: Comparison of Missing and non-Missing Performance Ratings

	# non-missing	# missing	Mean of non-missing	Mean of missing	Difference in means	t-stat [p-value, test of equality]
% female	45,830	12,918	28.00%	25.07%	2.93%	6.6001 [0.0000]
Base Pay	45,597	12,466	[redacted]			-16.3642 [0.0000]
Medicare Wages	45,181	11,891	[redacted]			-17.1648 [0.0000]
Total Comp	45,721	12,541	[redacted]			-12.4399 [0.0000]
Stocks	45,721	12,541	[redacted]			-10.2294 [0.0000]
Bonuses	45,721	12,541	[redacted]			-16.2684 [0.0000]
Oracle tenure	45,825	12,660	[redacted]			26.4811 [0.0000]
Overall tenure	45,825	12,660	[redacted]			28.8404 [0.0000]
Job tenure	45,722	12,545	[redacted]			22.6097 [0.0000]
Experience	45,825	12,918	[redacted]			20.2672 [0.0000]

Appendix B: Robustness Analysis for Bonus and Stock Grant Regressions

B1. One potential issue with my regressions for bonuses or stock grants is that there may be some jobs (defined by job codes and grades) where bonuses and stocks are not paid, in which case including these observations, with zero values for bonuses or stocks, could end up weakening the relationship between gender and bonuses or stocks for those job codes and grades where bonuses or stock grants are paid.

B2. However, Appendix Exhibit B1 shows that the results are not sensitive to the inclusion of these job code-job grade pairs.

Appendix Exhibit B1: Estimated Gender Differences in Adjusted Total Bonuses and Adjusted Stock Grants for 2013-2017, Omitting Job Code-Job Grade Pairs with No Bonuses or No Stock Grants

	Bonuses (All)	Bonuses, without code-grade pairs with no bonuses	Stocks (All)	Stocks, without code-grade pairs with no stock grants
	(1)	(2)	(3)	(4)
Female	-0.1317***	-0.1368***	-0.3311***	-0.3395***
	(0.0314)	(0.0345)	(0.0623)	(0.0636)
Standard deviations	4.19	3.97	5.32	5.34
Probability under null hypothesis of no gender pay gap	< 1 in 10,000	< 1 in 10,000	< 1 in 1 million	< 1 in 10 million
Observations	58,256	52,843	58,256	56,373

Notes to Exhibits 24 and 29. Regressions correspond to column (6) in those tables.

Appendix C: Conversion of Prior Pay Information to Usable Data

C1. The first step in this work is to turn prior pay information provided by Oracle into usable data. I was given text data that presented prior pay information in many different ways, as indicated by some examples in Appendix Exhibit C.1.

C2. Many steps in converting this information to usable data were carried out using a coding language called Python. The first step in converting the prior pay data was to remove the observations that were zeros, not available, or included no numbers, as these provided no valuable data. Next, a substantial share of the observations contained only a number, so that no extra work had to be done – assuming, as I did, that these observations were in terms of U.S. dollars.

C3. For the rest of the observations I used other information that was either in parentheses, or appeared after a term denoting plus (either the word plus or a plus sign). This appears to have usually been extra information about the main number. Several "string replacements" were also made, such as replacing k's that were preceded by a number with three zeros.

C4. Next, I wanted to identify prior pay that was reported in terms of a foreign currency. The most common pattern for foreign currencies was to have a number followed by the three-letter abbreviation for that currency, or vice versa. For several observations the full currency name had to be converted to its abbreviation. All observations that followed the abbreviation/number pattern were marked as foreign currency.

C5. Another type of prior pay I had to identify and treat separately was prior pay reported in terms of hourly pay. To do this, I looked for words that indicated hourly, such as: "hr," "hourly," or "hour." For these observations, I identified the number given and then created

a flag to indicate that the salary was hourly. I also flagged some cases where prior pay was denoted as part-time/contracting/or from a previous but not current job. These "features" of some of the prior pay data were converted into dummy variables that I could include in the empirical analyses reported below, to control for variation in how the data were recorded or reported.

C6. After all the above steps were implemented, I was left with approximately 400 observations that had to be handled individually, as there were not generally applicable rules could be written into the program.

C7. One issue is that some of the prior pay data was in terms of total compensation, while some was in terms of base pay. For around 500 observations there was an explicit indication of what was base pay and what was total compensation, and for these cases I created two separate pay variables.

Appendix Exhibit C.1: Examples of Prior Pay Information

CANDIDATE_CURRENT_SALARY_ATV
RMB 321,586.84
176620
1511095 INR + Bonus 167900 INR (100%)
175000
1,125,683.83 INR
120000 + 10% average bonus
N/A
$140,000
$89,215 + $12000 House Rent Allowance
135K+25K Bonus
118,112.48 (Inclusive of bonus)
105000 / 20% bonus
65,000 Annual
$70/hr
$1000/week
160000
190,000 with 20% annual bonus
190000
200,000 USD, not including benefits
65,000
$110,000
135000
135,200
135000
0
120,000
128000 ($118K base, + $10K bonus)
110000
$80,000
92,000 plus May focal increase
83000
28
100,000 USD
91,000
$66/hr
120,000
$12,000 (Grad Student stipend)
97000
150,000 + Profit Sharing
$100k
85,000.00
$190k (base + on-target bonus) + $40k RSUs per year = $230k

Appendix D: Robustness Analysis for Relationship between Prior and Starting Pay

D1. In this appendix, I provide alternative estimates of the prior pay and starting pay regressions paralleling Exhibit 41, but using the data somewhat differently to account for possible issues in the prior pay data. First, in Appendix Exhibit D.1, I drop observations where the prior pay data were ambiguous. Second, in Appendix Exhibit D.2, I also drop observations where prior pay was measured in a foreign currency, to avoid complications from translating the value of pay in other countries.[20] The estimates are not materially affected.

D2. Finally, as explained in the main report, there are many fewer observations with data on prior pay than the potential sample for which I would like to analyze the relationship between starting pay and prior pay. Appendix Exhibit D.3 compares means of key variables across the observations with non-missing and missing prior pay data. The observations missing prior pay data are more likely to be female, are lower paid, are more likely to work part-time, and have less experience. Thus, the prior pay data are not missing randomly with respect to these variables, although I have no evidence to suggest the relationship between starting pay and prior pay would be different for the observations for which prior pay data are missing.

[20] Although it is easy to convert using exchange rates, the purchasing power of an equivalent amount of many in terms of exchange rates can be quite different in different countries.

Appendix Exhibit D.1: Starting Pay and Prior Pay Regressions, Removing Data Ambiguities

Dependent Variable	Ln(Starting Pay)	Ln(Starting Pay)	Ln(Prior Pay)	Ln(Starting Pay) – Ln(Prior Pay)
Controls	Job controls plus experience (full sample after 2010)	Job controls plus experience (prior pay sample)	Job controls plus experience (prior pay sample)	Job controls plus experience (prior pay sample)
	(1)	(2)	(3)	(4)
Female	-0.0301***	-0.0238***	-0.0209*	-0.0030
	(0.0035)	(0.0058)	(0.0110)	(0.0092)
Experience	0.0023***	0.0008***	0.0053***	-0.0045***
	(0.0002)	(0.0003)	(0.0006)	(0.0005)
Standard deviations	8.61	4.14	1.91	0.32
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 10,000	5.71%	74.81%
Observations	9,130	2,721	2,721	2,721

See notes to Exhibit 41.

Appendix Exhibit D.2: Starting Pay and Prior Pay Regressions, Removing Data Ambiguities and Foreign Currencies

Dependent Variable	Ln(Starting Pay)	Ln(Starting Pay)	Ln(Prior Pay)	Ln(Starting Pay) – Ln(Prior Pay)
Controls	Job controls plus experience (full sample of non-campus hire and non-MA after 2010)	Job controls plus experience (prior pay sample)	Job controls plus experience (prior pay sample)	Job controls plus experience (prior pay sample)
	(1)	(2)	(3)	(4)
Female	-0.0301***	-0.0235***	-0.0199*	-0.0036
	(0.0035)	(0.0058)	(0.0103)	(0.0084)
Experience	0.0023***	0.0008***	0.0052***	-0.0044***
	(0.0002)	(0.0003)	(0.0006)	(0.0005)
Standard deviations	8.61	4.05	1.93	0.43
Probability under null hypothesis of no gender pay gap	< 1 in 1 billion	< 1 in 10,000	5.42%	66.53%
Observations	9,130	2,693	2,693	2,693

See notes to Exhibit 41.

Appendix Exhibit D.3: Comparison of Observations Missing and Not Missing Prior Pay Data

	# of non-missing	# of missing	Mean of non-missing	Mean of missing	Difference in means	t-stat [p-value, test of equality]
% female	2,786	6,367	23.04%	24.61%	-1.57%	-1.6127 [0.1068]
Starting Pay	2,791	6,381	[redacted]			13.4293 [0.0000]
Part Time	2,791	6,381	0.11%	0.53%	-0.43%	-2.9580 [0.0031]
Experience	2,791	6,381	[redacted]			1.8462 [0.0649]

Appendix E: Materials Used

ORACLE_JEWETT_00030955: Includes information about base pay, bonuses, other compensation, performance, and stocks.

ORACLE_JEWETT_00001166: Contains base pay that is both given as annualized and hourly for all employees. It also contains some more detailed employment characteristics like salary basis, employment category, and normal hours.

ORACLE_JEWETT_00001167: Contains a variable called "balance_name" which takes on 166 unique string variables ("Medicare ER Taxable," "Regular Earnings," "Nonqual Stock Opt," etc.). Each of these is associated with a value for all employees in each year from 2003-2018, which is the associated amount in each of those years.

ORACLE_JEWETT_00001180:

> *PersonalInfo*: The Personal Info sheet contains information on gender, start and end dates of employment durations, and age data.
>
> *Emp Assignment Information*: Contains assignment information for each Oracle employee and is updated when records change. This dataset is used for job descriptive variables that are primarily used as control variables in the regression analysis: i.e., job code, grade, zip code, line of business heads, part time status, and job tenure. I also use this to construct measures for the percentage of the year worked to adjust Medicare wages and total compensation.
>
> *EmpQual*: Contains education on highest educational degree earned for about 1593 unique people.

ORACLE_JEWETT_00030954: Contains performance ratings corresponding to the formal appraisal tool.

ORACLE_JEWETT_00030955: Contains performance ratings corresponding to the informal appraisal tool CWB.

ORACLE_JEWETT_00007303: Contains performance ratings corresponding to the informal appraisal tool CWB.

ORACLE_JEWETT_00001173: Contains performance ratings corresponding to the informal appraisal tool CWB.

ORACLE_JEWETT_00007312: Contains performance ratings corresponding to the informal appraisal tool FWC.

ORACLE_JEWETT_00001170: Contains performance ratings corresponding to the informal appraisal tool FWC.

ORACLE_JEWETT_00007304: Contains prior pay data given by

"CANDIDATE_CURRENT_SALARY_ATV."

Bureau of Labor Statistics Consumer Price Index (CPI) Databases (https://www.bls.gov/cpi/data.htm): Used to adjust pay for inflation.

International Monetary Fund Exchange Rate Query Tool (https://www.imf.org/external/np/fin/ert/GUI/Pages/CountryDataBase.aspx): Retrieved daily exchange rates to convert prior pay data in non-USD denominations.

[Oracle-Jewett] 2018.08.03 Mantoan to Finberg Continuing Responses to Data Questions: Answer to 22.3.

[Oracle-Jewett] 2018.06.29 Mantoan to Finberg Continuing Responses to Data Qs: Answer to 22.6 and 22.7.

[Oracle-Jewett] 2018.10.05 Mantoan to Finberg Responses to Remaining Dat...: Answer to 29-32.

[Oracle-Jewett] 2018.10.25 Ltr to Finberg from Mantoan re 2018.10.11 Data Production Questions: Information about performance rating process and classifications.

Document ORACLE_JEWETT_00000651 – Job code pdf: Confirmed information about job codes.

Waggoner Transcript Excerpt re RSUs (line 19-25): Tax implications of RSUs and options.

Nicholas Sanchez email 9/5: Tax implications of RSUs and options.

Production 24: Variety of documents, mostly resumes, pertaining to the hiring of individuals.

Resumes for named plaintiffs

Waggoner Exhibit 24.pdf

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Ravallion, Martin. 2017. "A Concave Log-like Transformation Allowing Non-positive Values." *Economics Letters*, 161(C), pp. 130-132.

Appendix F: CV Including Publications in Last 10 Years

CURRICULUM VITAE

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PERSONAL: Born July 7, 1959
United States Citizenship

EDUCATION:

Fields:	Labor Economics, Econometrics
Thesis Topic:	Male-Female Differentials in the Labor Force: Measurement, Causes and Probes.
Fellowships:	National Science Foundation Graduate Fellowship Fulbright Scholarship
Awards/Honors	National Longitudinal Surveys, Michael E. Borus Memorial Dissertation Award
	National Institute on Aging, Special Emphasis Research Career Award
	2000 Minnesota Award for "Age Discrimination Laws and Labor Market Efficiency"
	Bren Fellow, Public Policy Institute of California, 2009-2010
	Choice Outstanding Academic Title, 2009, for Minimum Wages (Cambridge: MIT Press, 2008)
	Chancellor's Professorship, University of California, Irvine, 2012-2017
	Distinguished Professorship, University of California, Irvine, 2017
	UCI Associated Graduate Students, 2015, Faculty Mentoring Award
	2016 Harris Distinguished Visiting Professor, Clemson University
	Selected to teach at IZA European Summer School in Labor Economics, 2016
	Lady Davis Fellowship, Hebrew University of

Jerusalem, 2018

Graduate:	Harvard University, Awarded Master of Arts Degree in Economics in 1985, Ph.D. in Economics in 1987.
Undergraduate:	University of Pennsylvania. Awarded Bachelor of Arts Degree in 1982. Graduated Phi Beta Kappa, Summa Cum Laude, with Honors. Shanbaum Award for the Outstanding Student in Economics.

RESEARCH AND PROFESSIONAL EXPERIENCE:

2005-present	**University of California, Irvine, Department of Economics**—Professor of Economics (now Distinguished Professor of Economics)
2016-present	**University of California, Irvine, Economic Self-Sufficiency Policy Research Institute (ESSPRI)**—Founding Director
2018	**Tel Aviv University**—Visiting Professor
2016-present	**Beijing Normal University**—Visiting Professor
2012-present	**Workers Compensation Research Institute**—Senior Research Fellow
2012	**Renmin University, Hanqing Institute, Beijing, China**—Visiting Lecturer
2011-2016	**University of California, Irvine, Center for Economics & Public Policy**—Founding Director
2011-present	**Federal Reserve Bank of San Francisco**—Visiting Scholar
2009-2011	**University of California, Irvine**—Director of Graduate Studies
2002-2011	**Public Policy Institute of California**—Bren Fellow/Senior Fellow, Economics
1994-2004	**Michigan State University, Department of Economics**—Professor of Economics
1989-1994	**University of Pennsylvania, Department of Economics**—Assistant Professor of Economics
1987-1989	**Board of Governors of the Federal Reserve System**—Economist, Division of Research and Statistics
1984-1985	**Abt Associates, Inc.**—Economic consultant
2009-2016	**Charles River Associates**—Senior consultant
2006-present	**Stanford University Center for the Study of Poverty and Inequality**—Fellow
2004-present	**IZA, Institute for the Study of Labor**—Research Fellow

2000-2001	**Public Policy Institute of California**—Visiting Fellow
1999-2002	**Michigan State University, Department of Economics**—Director of Graduate Studies
1995-present	**National Bureau of Economic Research**—Research Associate
1989-1994	**National Bureau of Economic Research**—Faculty Research Fellow

EDITORIAL RESPONSIBILITIES:

2012-present	**Journal of Urban Economics, Co-Editor**
2009-present	**Journal of Labor Research, Editorial Board**
2004-present	**Industrial Relations, Editorial Board**
2002-present	**Contemporary Economic Policy, Editorial Board**
2012-2016	**IZA Journal of Labor Policy, Editor**
2009-2012	**Journal of Urban Economics, Editorial Board**
2006-2012	**Review of Economics of the Household, Associate Editor**
2003-2010	**Economics of Education Review, Editorial Board**
2004-2006	**California Economic Policy, Editor**

PEER-REVIEWED PUBLICATIONS:

Neumark, David, "The Econometrics and Economics of the Employment Effects of Minimum Wages: Getting from Known Unknowns to Known Knowns," forthcoming in German Economic Review.

Neumark, David, and Bogdan Savych, "The Effects of Provider Choice Policies on Workers' Compensation Costs," forthcoming in Health Services Research.

Neumark, David, Ian Burn, and Patrick Button, "Is It Harder for Older Workers to Find Jobs? New and Improved Evidence from a Field Experiment," forthcoming in Journal of Political Economy.

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Neumark, David, Cathy J. Bradley, Miguel Henry, and Bassam Dahman, 2015, "Work Continuation While Treated for Breast Cancer: The Role of Workplace Accommodations," Industrial and Labor Relations Review, pp. 915-954.

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Neumark, David, J.M. Ian Salas, and William Wascher, 2014, "More on Recent Evidence on the Effects of Minimum Wages in the United States," IZA Journal of Labor Policy, 3:24 (on-line).

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Neumark, David, Hans Johnson, and Marisol Cuellar Mejia, 2013, "Future Skill Shortages in the U.S. Economy?" Economics of Education Review, pp. 151-67.

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Appendix G: Expert Witness/Testimony in Last 4 Years

Rabin et al. v. PricewaterhouseCoopers, LLP, No. 3:16-cv-02276-JST, U.S. District Court, Northern District of California.

Serving as plaintiffs' expert witness to address statistical evidence on age discrimination in hiring. Deposed.

EEOC v. Darden Restaurants, Inc., No. 15-20561, U.S. District Court, Southern District of Florida

Served as plaintiffs' expert witness to address statistical evidence on age discrimination in hiring. Deposed and testified. Qualified as expert witness.

Koehler et al. v. Infosys Technologies Limited, Inc., and Infosys Public Services, Inc., No. 2:13-cv.885, U.S. District Court, Eastern District of Wisconsin.

Serving as plaintiffs' expert witness to address statistical evidence on ethnic discrimination in hiring, promotions, and terminations. Deposed.

Heldt et al. v. Tata Consultancy Services, Ltd., No. 4:15-cv-01696, U.S. District Court, Northern District of California.

Served as plaintiffs' expert witness to address statistical evidence on ethnic discrimination in hiring and terminations. Deposed and testified. Qualified as expert witness.

Smiley v. Hologic, Inc., No. 3:2016cv00158, U.S. District Court, Southern District of California.

Served as plaintiffs' expert witness to address reasons for inability of plaintiff to find new employment after termination. Deposed.