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SUPERIOR COURT OF THE STATE OF CALIFORNIA

COUNTY OF SAN FRANCISCO

KELLY ELLIS, HOLLY PEASE, KELLI WISURI, and HEIDI LAMAR individually and on behalf of all others similarly situated,

Plaintiffs,

v.

GOOGLE, LLC (formerly GOOGLE, INC.),

Defendant.

Case No. CGC-17-561299

DECLARATION OF DAVID NEUMARK IN SUPPORT OF PLAINTIFFS' MOTION FOR CLASS CERTIFICATION

Judge: Hon. Andrew Y.S. Cheng, Dept. 613
Date: December 2, 2020
Time: 9:00 a.m.
Complaint Filed: September 14, 2017
Trial Date: None Set

I, David Neumark, declare as follows:

1. I am Distinguished 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 30 peer-reviewed journal papers on discrimination based on race, ethnicity, gender, 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*, *Journal of Law and Economics*, *Journal of Political Economy*, *Review of Economics and Statistics*, and *Quarterly Journal of Economics*, as well as other studies in edited books, and a full-length book on gender discrimination and gender 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, gender, 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. Others study the effects of equal pay laws or evidence of violations of them. The graduate courses that I teach in labor economics and my training of Ph.D. students 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) and at CESifo in Germany. I also co-direct the Center for Population, Inequality, and Policy at UC—Irvine. In recognition of my professional accomplishments, in 2019, I was elected a Fellow of the American Association for the Advancement of Science.

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 San Francisco, CA, on July 10, 2020.

By: ______________________
David Neumark

Exhibit A

Expert Report of

David Neumark

in the matter of

Ellis et al. v. Google, LLC

July 2020

I. Introduction

1. I am David Neumark, Distinguished 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 30 peer-reviewed journal papers on discrimination based on race, ethnicity, gender, 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, Journal of Law and Economics, Journal of Political Economy*, *Review of Economics and Statistics,* and *Quarterly Journal of Economics*, as well as other studies in edited books, and a full-length book on gender discrimination and gender 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, gender, or age.

2. 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.[1] Others study the effects of equal pay laws or evidence of violations of them.[2] The graduate courses that I teach in labor economics and my training of Ph.D. students in labor economics focus heavily on

[1] See, e.g.: Neumark, David. 2012. "Detecting Evidence of Discrimination in Audit and Correspondence Studies." *Journal of Human Resources*, Vol. 47, pp. 1128-57; and 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*, Vol. 17, pp. 409-446.
[2] See, e.g.: Neumark, David, and Wendy Stock. 2006. "The Labor Market Effects of Sex and Race Discrimination Laws." *Economic Inquiry*, Vol. 44, pp. 385-419; and Bayard, Kimberly, Judith Hellerstein, David Neumark, and Kenneth Troske. 2003. "New Evidence on Sex Segregation and Sex Differences in Wages from Matched Employee-Employer Data." Vol. 21, pp. 887-922.

econometric methods.

3. 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) and at CESifo in Germany. I also co-direct the Center for Population, Inequality, and Policy at UC—Irvine. In 2019, in recognition for my contributions to labor economics, I was elected a Fellow of the American Association for the Advancement of Science.

4. I have been retained by the Plaintiffs as a statistical expert to evaluate claims of gender discrimination in pay at Google, LLC (henceforth Google). Specifically, I have been asked to examine whether the data are consistent with gender discrimination in pay at Google during the class period, and to investigate the policies that lead to gender differences in pay at Google during the class period and whether those policies act in a manner that is consistent with gender discrimination. I am compensated at the rate of $495 per hour.

5. This analysis is based on my current understanding of the data and supporting materials with which I have been provided by Google. The data files are listed and described in Appendix A of my report, and listed in Appendix C. It is possible that I will learn more about the Google data, company procedures, and other matters in the course of this case, which could lead to changes in my analysis and findings.

6. Appendix B contains supplemental tables with information on job titles at Google, job titles at prior jobs, and education. Materials that I considered are listed in Appendix C. Appendix D of my report provides an abridged CV listing my publications from the last 10 years. Appendix E of my report details my expert witness work in the last 4 years.

II. Questions I Was Asked to Consider

7. I was asked to consider the following questions:

a. How many women did Google employ in California in Covered Positions from September 14, 2013 through December 31, 2018?

b. Compare the compensation (including base pay, bonus, and stock) of men and women in Covered Positions who were performing substantially equal work in jobs the performance of which require substantially equal skill, effort, and responsibility, performed under similar working conditions. If there is a disparity in compensation by gender, is it statistically significant? If there are disparities, are they explained by seniority, performance, or other bona fide factors, such as education or experience?

c. Examine how Google assigns new hires to salary levels and determine whether or not there are disparities by gender in how men and women with comparable education and experience are assigned to levels. If there are disparities, what is the economic impact of this assignment disparity?

d. What relationship, if any, does a new hire's prior pay (at jobs before Google) play with respect to assignment to a particular salary level?

e. With respect to the four named plaintiffs, were they paid less than men performing substantially equal or similar work? What is the relationship between their prior pay and their starting pay at Google?

III. Summary of Findings

8. A summary of my findings is as follows:

a. The class period runs from September 14, 2013, through trial. At this point I have data through December 31, 2018. There were 42,739 people employed in the Covered

Positions in the period covered by the data I currently have (in 231 Covered Positions, from the list in Exhibit 503). Of these, 25.28%, or 10,803, were women.[3]

b. My analysis of men and women in Covered Positions who were performing substantially equal work in jobs the performance of which require substantially equal skill, effort, and responsibility, performed under similar working conditions, indicates that women are paid less than men. Using the most comprehensive measure of compensation that includes base pay, bonuses, and equity, women are paid, on average, $1,894 less than comparable men each year.[4] (See Summary Table 1.) This result is statistically significant: 3.0 standard deviations. The likelihood of this disparity occurring by random chance is less than 1 in 100. These disparities are not explained by seniority, performance, or other bona fide factors such as education or experience.

i. To study gender differences in pay for men and women in Covered Positions who were performing substantially equal work in jobs the performance of which require substantially equal skill, effort, and responsibility, performed under similar working conditions, I compared persons in the same job code.[5] Job families are stratified by levels.[6] [redacted].[7]

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[3] These numbers cover some individuals not included in my analysis of pay below. The pay analysis is based on annual snapshots of the Google workforce as of January 1 of each year and the last one of these I have is for January 1, 2019. Other data I was given include some people hired after this date, or hired for spells too short for the employee to appear in the snapshot data. Also, these numbers include 257 observations with missing data on gender; that is, the 10,803 includes all those identified as women in the data, and the 42,739 includes everyone (including the 257 with missing data on gender; some of them are likely women). In all other analyses reported below, where I focus on estimating gender differences in outcomes, I drop the observations missing data on gender.

[4] Throughout, the dollar figures I report are measured in December 2018 U.S. dollars.

[5] Job codes and job titles are equivalent. One is a numeric code, and one is text.

[6] Wagner OFCCP at 174.

[7] Williams at 99-100.

ii. When doing research on possible discrimination in pay, labor economists often compare persons with similar job skills and responsibilities. I determined that testing for gender differences in pay within job codes is appropriate here because: Google documents and deposition testimony from persons most qualified (PMQs) indicate that persons in the same job codes share skills and responsibilities: see Wagner's OFCCP testimony at 174: 3-6 and 21-24; GOOG-ELLIS-00010907, which indicates that [redacted]; [redacted]).[8] This is buttressed by statements from Google [redacted] (see Wagner's deposition at 94:4-7).[9]

iii. In the statistical model of earnings I use to estimate the gender gap in pay within job codes, I also control (i.e., take into account, or "hold constant") for numerous other possible sources of earnings differences across workers

[8] Many similar documents I reviewed for other job families lead to the same conclusion that people in the same job codes (essentially the same as job levels within families) share skills and responsibilities. These include: Ex. 555 – [redacted]; Google-Ellis-00001691 – [redacted]; Google-Ellis-00004301 – [redacted]; Google-Ellis-00004303 – [redacted] Google-Ellis-00001681 – [redacted]; Google-Ellis-00004286 – [redacted]; Google-Ellis-00004293 – [redacted]; Google-Ellis-00004305 – [redacted]; Google-Ellis-00004311 – [redacted]); Google-Ellis-00004329 – [redacted]); Google-Ellis-00004440 – [redacted]; Google-Ellis-00008310 – [redacted]; Google-Ellis-00008315 – [redacted]; Google-Ellis-00004337 – [redacted] Google-Ellis-00004349 – [redacted] Google-Ellis-00004363 – [redacted]; Google-Ellis-00004379 – [redacted]; Google-Ellis-00004389 – [redacted].

[9] He says: "[redacted]."

highlighted by labor economics research.[10] These include: education (highest degree earned, and detailed controls for the schools and fields of study for the most recent degree); prior work experience; job tenure (years worked) at Google and time in job level at Google; leave of absence; performance ratings; location information (different California offices and other location differences associated with pay variation); whether an employee was a campus hire; job family (e.g., Software Engineers); and the year of the observation. Many of these controls – such as education, prior work experience, and job tenure – are suggested by the standard labor economics literature explaining earnings differences across workers, going back to the seminal work by Mincer (1974) and Becker (1994).[11] Despite including all of these controls, I still find statistically significant gender disparities in pay, adverse to women.

iv. In other words, when I focus on the gender gap in pay *within* job codes, and also adjust for detailed differences across workers in education, experience,

[10] My analysis uses regression models for pay. My regression models go beyond simply looking at average differences in pay. Instead, they adjust for differences across workers in factors *aside from gender* that could explain gender differences in pay, and hence calculates the gender difference in pay – if there is one – for men and women who are have the same values of all of these other differences. This technique is referred to as "multiple regression." The "multiple" label is used because there is more than one variable that can potentially explain differences in pay across workers – in my case, gender, as well as other explanatory variables such as education and experience. The estimated coefficient on which I focus in most of the regression models I estimate is the coefficient of a variable indicating whether an observation is for a woman ("female"). This is a "dummy variable" that takes the value 1 for women, and 0 for men, and hence its coefficient measures the difference in the outcome (pay) between women and men. A negative value (consistent with my results) indicates that women are paid less. When I estimate a multiple regression model for pay (denoted *Y* in the following quote), the estimated coefficient of each variable is called a "multivariate regression coefficient." The estimated coefficient on "female" is hence the gender difference in pay holding constant the other factors included in the model: "… **multivariate regression coefficients** … serve to isolate the impact on *Y* of a change in one variable from the impact on *Y* of changes in other variables." (See Studenmund, A.H.. 2006. Using Econometrics: A Practical Guide, Fifth Edition, Pearson Education Inc., p. 14.)

[11] See: Mincer, Jacob. 1974. Schooling, Experience, and Earnings. Cambridge: National Bureau of Economic Research, Inc.; and Becker, Gary S. 1994. Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education. Chicago: University of Chicago Press.

job tenure, and work performance as measured by Google, there are statistically significant gender disparities in pay that result in women earning less.

v. As noted, this analysis is set forth in Summary Table 1:

Summary Table 1: Equal Pay Analysis, Gender Differences in Pay During Class Period, Models Include Individual Characteristics and Qualification, and Job Codes

A. Estimated Gender Difference in Base Pay During Class Period (Anal. Table 1)

Female shortfall	**-0.35%**
Standard deviations	**5.08**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 million
Implied gender difference in pay	**-$600**

B. Estimated Gender Difference Base Pay + Standard Bonus During Class Period (Anal. Table 1)

Female shortfall	**-0.45%**
Standard deviations	**6.07**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion
Implied gender difference in pay	**-$937**

C. Estimated Gender Difference Base Pay + Standard Bonus + Equity During Class Period (Anal. Table 1)

Female shortfall	**-0.55%**
Standard deviations	**2.97**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 100
Implied gender difference in pay	**-$1,894**

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. The number of observations = [redacted]. See notes to corresponding Analysis Table(s) for additional explanation.

c. The analysis described so far estimates gender pay gaps within job codes. However, a much larger gender gap in pay arises because Google hires women with comparable job experience and education to men into lower job levels (that are associated with lower salary ranges). The impact of this “under-leveling” at hire persists throughout their time at Google. When I estimate models for the gender gap in pay that include the effect of this “under-leveling” of women, using the most comprehensive measure of pay that includes bonuses and equity, the estimated gender gap in pay is 4.9%, implying that, compared to men who have the same characteristics when they start at Google, women, on average, earn $16,794 less per year. This result is statistically significant: 12.0

standard deviations. The probability of obtaining this estimate by chance under the null hypothesis of no discrimination is less than 1 in 1 billion. (See Summary Table 2.)

i. I show this by estimating the pay regression models described above, but controlling only for job families (i.e., groups of persons performing similar types of work, such as Software Engineers), but not job codes, which are the intersection of job family and salary level.[12] Because job codes are strongly related to differences in salary levels within families, these regression models estimate the gender gap in pay removing the influence of Google putting women into lower salary levels within the same job family. In these regression models that control for the same individual characteristics and qualifications described above, but estimate the gender gap in pay within job families but not within level, the gender gaps in pay are much larger.

ii. The estimated approximate percentage gap in <u>base pay</u> is 2.9%, implying an average pay gap, adverse to women, of $5,062 per year. That result is highly significantly significant: 13.0 standard deviations. The probability of obtaining this estimate by chance under the null hypothesis of no discrimination is less than 1 in 1 billion.

iii. When <u>bonuses</u> are also included the approximate percentage pay gap rises to 3.3%, implying an average pay gap, adverse to women, of $6,988 per year. This result is highly statistically significant: 13.0 standard deviations. The probability of obtaining this estimate by chance under the null hypothesis of no discrimination is less than 1 in 1 billion.

[12] Wagner OFCCP at 174:10-15.

iv. When equity is also included, the approximate percentage pay gap rises to 4.9%, implying an average pay gap adverse to women of $16,794 per year. This result is highly statistically significant: 12.0 standard deviations. The probability of obtaining this estimate by chance under the null hypothesis of no discrimination is less than 1 in 1 billion.

Summary Table 2: Equal Pay Analysis, Gender Differences in Pay During Class Period, Models Include Individual Characteristics and Qualification, and Job Families, but Do Not Control for Level

A. Estimated Gender Difference in Base Pay During Class Period (Anal. Table 1)

Female shortfall	**-2.9%**
Standard deviations	**12.97**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion
Implied gender difference in pay	**-$5,062**

B. Estimated Gender Difference Base Pay + Standard Bonus During Class Period (Anal. Table 1)

Female shortfall	**-3.3%**
Standard deviations	**13.00**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion
Implied gender difference in pay	**-$6,988**

C. Estimated Gender Difference Base Pay + Standard Bonus + Equity During Class Period (Anal. Table 1)

Female shortfall adding job family controls	**-4.9%**
Standard deviations	**11.95**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion
Implied gender difference in pay	**-$16,794**

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. The number of observations = [redacted]. See notes to corresponding Analysis Table(s) for additional explanation.

v. As noted, this analysis is set forth in Summary Table 2:

d. Prior pay – that is, pay at the job prior to working at Google – plays an important role in determining an employee's starting pay. Prior pay appears to largely drive the gender gap in starting pay, and it does this through determining the levels at which men and women are hired at Google. In particular, Google hires women with comparable experience and education to men into lower job levels that pay less, and much of this under-leveling of women is driven by prior pay. A new hire's prior pay strongly influences the job level at which he or she gets hired – accounting, to a large extent, for women being hired at lower job levels. The gender gap in starting pay reflects the gender gap in prior pay. This pay gap – starting with prior pay, which in turn influences starting job level and hence starting pay – persists throughout a women's time at Google, helping to account for a large share of the gender gap in pay in the class period.

i. I study this by examining information on how Google sets pay for new employees, which, at least until August 2017, took prior pay into account.[13] The company's pay policies suggest what I term a "target job level based on prior pay" – i.e., a level into which Google tries to slot new hires based on the worker's prior pay. I find that the gender difference in starting pay falls by more than two-thirds once I control for the target job level based on prior pay, which demonstrates that most of the gender difference in starting pay, which happens from the assignment of women to lower job levels than men with comparable experience and education, arises because of how Google uses prior pay to assign new hires to job levels. (See Summary Table 3.)

e. Regarding the four named plaintiffs, my empirical analysis shows that, like other women in the class, the named plaintiffs were compensated less than men in the same job code with similar education, experience, performance scores, etc. (Analysis Table 18). With regard to prior pay and starting pay, Ms. Ellis received a starting salary of $[redacted], equal to her prior pay. Ms. Lamar received starting salary of $[redacted], slightly above her

[13] See Williams at 62-64, 163-164, 190-193; and Ex. 512.

prior pay of $[redacted]. Ms. Pease received a starting salary of $[redacted], while her prior pay was $[redacted]. For Ms. Wisuri, prior pay information is missing in the data. Her starting salary at Google was $[redacted].

IV. Overview and Background Regarding Statistical Methods.

9. My analyses focus on certain Covered Positions (identified in deposition Exhibit 503). I have data through December 31, 2018, with snapshots of the workforce with compensation information as of January 1 of each year from 2014-2018. The goal of my analyses is to estimate whether there is a gender difference in pay once I control for individual differences across workers, and differences in the jobs in which they work.[14]

10. 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 compensation measures. They also include an indicator for the gender of an employee. And, importantly, they include measures of the type of job a person at Google does, the person's job experience and education, the person's performance review scores, job tenure at Google, and tenure in level.

11. The regression models estimate the gender gap in pay once I adjust for possible differences between female and male employees that could account for this pay gap. For example, suppose that I simply compare average pay of all female and male employees at Google, and find that average pay of female employees is 10% lower. It is possible that women do different jobs than men, and those jobs could pay less; for example, perhaps more women work in child care and more men work as software engineers. It is also possible that women and men are in broadly similar jobs, but the women have lower values of measures that could be

[14] Appendix A provides extensive documentation of the data files used for each analysis, and the variable definitions.

related to productivity, such as less tenure, experience, or education, or lower performance ratings. 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 that is unexplained by these differences and hence is consistent with discrimination.

12. 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, I adjust the pay gap for differences in the jobs employees hold, and differences in measures of productivity, such as their tenure, education, experience, and performance ratings, so that I am comparing pay between comparable women and men in similar jobs. 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.[15] Thus, my analysis asks – in a detailed manner making extensive use of data provided by Google, and data I created from other sources of information on Google employees produced by Google in discovery – whether other factors such as job tenure, job experience, education, or performance can explain any pay gaps by gender that I find.

13. If there is evidence that women are compensated 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),[16] who defined discrimination in pay as unequal pay for

[15] 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.
[16] Becker, Gary S. 1957. The Economics of Discrimination. Chicago: University of Chicago Press.

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.[17]

14. 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 difference.[18] This convention, and the reasons for it, goes back to the original development of the earnings regression in labor economics (Mincer, 1974).[19] This is usually done by measuring pay in terms of the "natural logarithm," in which case the coefficient estimates approximate percentage differentials.

15. While my regression models estimate a gender gap in pay, we also have to ask whether the estimated gender gap is "statistically significant." 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. If the estimated gender gap in pay is statistically significantly different from zero, we are more sure that we did not get a non-zero estimate by chance, but rather because there is in fact a gender gap in pay. To assess this, 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

[17] 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.
[18] 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).
[19] Mincer, Jacob. 1974. Schooling, Experience, and Earnings. Cambridge: National Bureau of Economic Research, Inc.

standard deviations metric is used to test whether the measured difference in pay between women and men is statistically significant and differs strongly from the 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. 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."

17. To provide more detail for even higher standard deviations, the following table shows, for different numbers of standard deviations, the probability that the resulting estimate could have occurred under the null hypothesis of no discrimination (i.e., a true gender gap of zero). If the reported standard deviations in my report are higher than the numbers in this table, then the probability is less than the numbers shown here:[20]

[20] For example, for 9 standard deviations, the probability would be less than 1 in 1 billion.

Standard deviations	Probability
1.96	1 in 20
2.58	1 in 100
3.29	1 in 1,000
3.89	1 in 10,000
4.42	1 in 100,000
4.89	1 in 1 million
5.33	1 in 10 million
5.73	1 in 100 million
6.12	1 in 1 billion

V. Detailed Analysis – Gender Differences in Pay from September 14, 2013 – December 31, 2018

18. I begin my analyses by comparing the compensation at Google of men and women with similar experience and education. Looking at men and women employed at Google during the class period, when I account for individual differences across workers (in factors such as education, experience, location, tenure, and performance ratings), women earn approximately 7.5% less than men in base pay. This estimate is highly statistically significant (23.6 standard deviations), and implies a pay gap of $13,021. When bonuses are also included the approximate percentage pay gap rises to 8.5% (23.4 standard deviations), implying a pay gap of $17,783. And when equity is also included, the approximate percentage pay gap rises to 12.2% (24.0 standard deviations), implying a pay gap of $41,820. (See Analysis Table 1, Column (3), Panels A-C).

19. The estimates just discussed can be interpreted as measures of the gender gap in pay that exist accounting for differences across workers, but without taking account of the allocation of workers to specific jobs at Google, because they are the gender differences that remain after controlling for differences across workers in qualifications. Google classifies its workforce by job families, and within job families by job levels; the classification of jobs by families and

levels within families is nearly identical to the classification of jobs by job codes.[21]

20. When I add to the statistical model of earnings controls for the job family in which people work, the estimated approximate percentage gap in base pay is 2.9% (13.0 standard deviations), implying a pay gap of $5,062. When bonuses are also included the approximate percentage pay gap rises to 3.3% (13.0 standard deviations), implying a pay gap of $6,988. And when equity is also included, the approximate percentage pay gap rises to 4.9% (12.0 standard deviations), implying a pay gap of $16,794. (See Analysis Table 1, Column (4), Panels A-C.)

21. When I add to the statistical model of earnings controls for the job code in which a person is employed, the estimated approximate percentage gap in pay is 0.4% (5.1 standard deviations), implying a pay gap of $600. When bonuses are also included the approximate percentage pay gap rises to 0.5% (6.1 standard deviations), implying a pay gap of $937. And when equity is also included, the approximate percentage pay gap rises to 0.6% (3.0 standard deviations), implying a pay gap of $1,894. (See Analysis Table 1, column (5), Panels A-C.)[22]

22. The core results for my analysis of gender differences in pay during the class period are reported in Analysis Table 1. In that table, I estimate models for base pay, for base pay plus bonuses, and for base pay plus bonuses plus equity (in Panels A, B, and C, respectively). I begin, in column (1), with models that control only for "year effects," (i.e., dummy variables for the

[21] Job families and job levels (along with job titles, which differ slightly) are listed in Appendix Table B1. Job families describe what I would term functions (e.g., "Account Executive," "Benefits – Child Care," and "Software Engineer") with sometimes what appear to be distinctions by level (e.g., "Software Engineer" and "Software Engineer Manager"). Job levels are more-detailed descriptions of jobs within job families, which Google interprets as "job ladders" within job families, with the job ladders/levels delineating the attributes, skills, and responsibilities of each job within the job family (e.g., GOOG-ELLIS-00010907).

[22] Because I include detailed controls for job codes, the gender differences in pay that I estimate should be interpreted as pay differences *within* job codes. That is, my estimated gender differences in pay will not reflect the possibility that men and women are in different job codes with different levels of pay. Because the resulting gender differences in pay that I estimate are based only on comparisons of men and women in the same job codes, labor economists describe such estimates as arising from pay differences "within" job codes. See, e.g.: Groshen, Erica. 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*, Vol. 26, pp. 457-472.

year of the observation). The year effects simply control for differences in pay by year, in case, say, different proportions of men and women were hired in each year. In columns (2) and (3) I control for differences in worker characteristics and qualifications, including education, experience, performance rating, and other non-discriminatory factors that might affect pay, such as location.[23] In column (2), I compare men and women while controlling for education (including highest education level, university of the most recent degree,[24] and the field of study for the most recent degree[25]); prior cumulative experience; job tenure; percent of the previous year on leave of absence;[26] location;[27] and whether or not campus hire. Then in column (3) I add a control for Google's performance rating of the employee – in particular, average performance rating for the last year.[28]

23. The evidence from the model including controls for job code (essentially job family and job level within job family) speaks to the question of whether men and women working in the same job at Google are nonetheless paid differently, since this model compares men and women working in the same job as Google itself classifies jobs. Wagner, in his OFCCP testimony, says: "A job level can be thought of as a salary grade. And using common compensation vernacular, it is a level at which the people at that job are performing like level of duties and responsibilities within that job family" (at 174: 21-24). Similarly, he says "A job family is a professional category of job at Google. So those that are doing similar job duties and responsibilities, but

[23] See Appendix A that describes generally how some of these variables are computed.
[24] Appendix Table B2.
[25] Appendix Table B3.
[26] Leave of absence measures percent of year that employee was not eligible for bonus due to unpaid leave or unemployment. See Appendix Table A1.
[27] In his deposition testimony, Mr. Williams indicates that the bonus target for a new hire is based on location, among other things (at 153: 5-6).
[28] My use of the performance ratings data – and how I use them – is supported by deposition testimony. Mr. Wagner indicates that [redacted] (at 81:23 – 83: 7). Ms. Tietbohl provides similar testimony (at 95: 14-23, Feb. 5, 2019).

stratified at different levels of capability or skill sets" (at 174: 3-6) And as noted earlier, many Google documents describing the skills and responsibilities for job levels within job families (job ladders) are consistent with the similarity of required skills and responsibilities within job levels.

24. This evidence is consistent with pay discrimination against women. Whether I look at base pay, base pay plus bonuses, or base pay plus bonuses plus equity, women at Google earn less than men working in the same job codes, when controlling for measures that could be related to productivity and hence could legitimately make a difference in pay, such as education, experience, and performance, and the differences are statistically significant at the 5% level and even at the 1% level (which would be 2.57 standard deviations).

25. However, as I discuss in the following section, there appear to be discriminatory decisions Google makes earlier – at the time of hire – that lead to a larger gender gap in pay than is implied by the models including controls for job codes (families, and levels within families). These decisions include "leveling" – the assignment of women, at the time of hire, to lower job levels (which pay less) within job families, and "channeling" – the assignment of women, at the time of hire, to lower-paying job families among job families that require equal skills and experiences.

26. Before turning to that analysis, though, I establish that the estimated gender gaps in pay are robust to including alternative sets of control variables; these are summarized in Analysis Table 2. I am not sure these controls need to be included. For example, I include controls for cost centers, although I have not seen any direct evidence that pay decisions differ by cost center. Nonetheless, it useful to know that the conclusions of my analysis are robust to considering these different sets of control variables.

a. First, I show that controlling for whether a worker is a manager does little to change the estimated gender gaps in pay, whether I look at base pay, include bonuses, or include equity and bonuses. (See Analysis Table 2, columns (3)-(4), compared to columns (1)-(2).)

b. Second, I show that the estimates are also very robust to including additional controls for dimensions along which Google groups jobs – Department, and Unified Rollup. (See Analysis Table 2, columns (5)-(6).)

c. Third, I show that the results are robust to including controls for cost centers. (See Analysis Table 2, columns (7)-(8).)

d. Fourth, I show that the results are robust to including just standard bonuses (my baseline analysis), or to including all non-sales bonuses or sales bonuses as well (Panels B-D).

e. No matter the specification, women receive significantly less base pay – about 2.6 to 2.9% (12.1 to 13.4 standard deviations), or about $4,600 to $5,100, when controlling for job families, and 0.3 to 0.4% (4.6 to 5.2 standard deviations), or about $540 to $610, when controlling for job codes.

f. The gender gaps in pay are larger when I include bonuses – about 3.0 to 3.3% (12.0 to 13.6 standard deviations), or about $6,200 to $7,000, when controlling for job families, and 0.4 to 0.5% (5.5 to 6.2 standard deviations), or about $840 to $950, when controlling for job codes. (See Panel B, although the results are very similar in Panels C and D.)

g. And the gender gaps in pay are larger still when I also include equity – about 4.4 to 4.9% (10.8 to 12.3 standard deviations), or about $14,800 to $16,800, when controlling for job families, and about 0.5 to 0.6% (2.7 to 3.1 standard deviations), or about $1,700 to

$2,000, when controlling for job codes.

VI. Detailed Analysis – The Role of Prior Pay and Leveling in Explaining the Gender Gap in Pay During the Class Period

27. In this section, I show that the gender gap in starting pay when people are first hired at Google, which is strongly influenced by a gender gap in prior pay, explains most of the gender gap in pay in the class period. In other words, women start out at Google earning less than men, and that disparity in starting salary persists throughout the class period. And the majority of the initial starting salary disparity appears to be driven by prior pay (salary in the job prior to coming to Google). This conclusion comes from a sequence of analyses, which I describe in turn.

28. First, I show that starting pay explains a large share of the gender gap in pay during the class period. The implication is that women are paid less than otherwise similar men when they start at Google, and this pay gap persists into the class period. Note that in this I analysis I refer to base pay only, since that is all I can study for starting pay and prior pay.

29. This evidence is presented in Analysis Table 3. I begin, in Panel A, by repeating the estimates for the full sample in the class period (from Analysis Table 1), with controls for individual characteristics and qualifications, as well as job families, and then controlling for job codes instead of just job families (which is essentially equivalent to controlling for all job families and job levels within families). Because starting pay data are missing for some observations, Panel B reports estimates of the same models, but for the sample for which I have starting pay data. Panel C then reports the estimates controlling for starting pay.

30. For the sample with starting pay data, the model estimated for the class period shows that the estimated gender gap in base pay during the class period, controlling for individual characteristics and job family, is 2.9% (12.3 standard deviations), implying a pay gap of $5,013.

(See Panel B, column (1).) This is the same as the full sample estimate in Panel A. But if I add a control for starting pay to the model, the gender gap in pay falls by more than two-thirds (71%), to 0.8% (4.7 standard deviations), implying a pay gap of $1,436. (See Panel C, column (1).) In other words, starting salary at Google explains more than two-thirds of the pay disparities observed across the class period. Note that the more than two-thirds reduction in the gender gap in pay from controlling for starting pay is similar to the reduction in the gender gap in pay from controlling for job level (by substituting controls for job codes for controls for job families only). This can be seen for this subsample in Analysis Table 3, Panel B, column (2); it can be seen for the full sample in Analysis Table 1, Panel A, column (5) vs. column (4).

31. As we would expect if starting pay explains the gender gap in pay during the class period, there must be a gender gap in starting pay of a similar magnitude to the gender pay gap during the class period. This is demonstrated in Analysis Table 4. Columns (1) and (2) repeat the estimates from Panel B of Analysis Table 3, showing the gender gap in base pay for the class period, for the sample with starting pay data. Columns (3) and (4) instead show the estimates of the gender gap in starting pay. We see that, just like for pay in the class period, when employees start at Google, if we control for individual characteristics and qualifications, as well as job families, women are paid about 3% less than men. Specifically, the starting gender gap in pay is 2.9% (13.5 standard deviations), implying a pay gap of $4,001. Note, though, that when I control for the job code into which Google employees were hired, the estimated gender gap in starting pay evaporates. In column (4), the sex gap in starting pay controlling for the job code into which people were hired becomes statistically insignificant (1.4 standard deviations). This foreshadows the point I develop below – that starting job *level* drives the gender gap in starting pay, and that this starting job level also drives the gender gap in pay during the class period.

32. Moreover, the gender gap in starting pay reflects the gender gap in prior pay – i.e., differences in pay for otherwise similar men and women before they are hired at Google. As a preliminary, Figures 1 and 2 show the relationship between prior pay and starting pay at Google. In Figure 1, the relationship is not so clear, because there are some clear outliers with very high or very low prior pay figures. But in Figure 2, where I drop these outliers, the relationship is much clearer. (Figure 1 shows the cutoffs for these outliers.)[29] In addition to the scatterplot of data points, I show the best fit linear and quadratic regressions. There is a clear upward slope that is close to linear.

33. The evidence that the gender gap in starting pay reflects the gender gap in prior pay – i.e., differences in pay for otherwise similar men and women before they are hired at Google – is shown in Analysis Table 5. Note that I have to work with a smaller sample, because prior pay data are sometimes missing. Thus, Analysis Table 5 first repeats the estimate of the starting gender gap in pay from column (3) of the prior table, and then shows that if I restrict the sample to the observations with prior pay data, the estimate is similar (column (2)). Specifically, the estimated gender gap in starting pay, for this sample, is 2.6% (7.0 standard deviations, implying a pay gap of $3,627). Next, column (3) shows that the estimate for the gender gap in *prior* pay is essentially the same, at 2.4% (3.9 standard deviations). Finally, in column (4), I estimate a model that asks whether there is a significant difference in the gender gaps in starting pay vs. prior pay, and find that there is not; the estimate is effectively zero – equal to 0.2% (0.5 standard deviations).

[29] In all of my analyses using prior pay, I trim the top and bottom 2% of prior pay values, based on evidence that were some extreme values at both ends of the distribution, likely stemming from errors or ambiguities in measuring or reporting of prior pay. The cutoffs are $[redacted] and [redacted] (December, 2018 dollars), while the minimum and maximum are $[redacted] and $[redacted] (so most of the data are in a much narrower range). Note that the figures use the logs of prior and starting pay.

34. The previous two tables show that prior pay drives the gender gap in starting pay (Analysis Table 5), and starting job level drives the gender gap in starting pay (Analysis Table 4). The inference I draw from this is that prior pay drives the starting job level, which in turn drives starting pay, generating a gender gap in starting pay at Google. And the gender gap in starting pay drives the gender gap in pay during the class period, as shown in Analysis Table 3 (and elaborated on more below, Section VIII of this report).

35. I illustrate this connection from prior pay to class period pay even more explicitly in the next section, in analyzing software engineers – a huge job family that constitutes over half (54.2%) of Google's employment. (See Analysis Table 6.)

VII. Detailed Analysis – The Role of Prior Pay and Leveling in Explaining the Gender Gap in Pay During the Class Period, Software Engineers

36. I can do the most thorough examination of how prior pay drives the class period gender gap in base pay – via starting job level – by focusing on a single job family. The reason is that because within a job family, job levels mean the same thing, whereas the difference in, e.g., skills and job requirements between, say, job level 1 and job level 2, may not be the same across different job families. I focus on software engineers, but since they constitute more than half of Google's workforce in the class period, the conclusions I can draw from them are widely applicable. (In contrast, as Analysis Table 6 shows, all other job families constitute a far smaller share of the Google workforce; the next largest job family has fewer than one-tenth the workers as the software engineer job family.)

37. Using these data, I show systematically that prior pay drives the job level at hire and hence starting pay at hire, based on Google's policies that set starting pay based on prior pay. And I show that differences in prior job experience do not explain gender differences in the job

level at hire (and hence starting pay). My analysis proceeds in a number of steps. Most of these parallel the analyses of starting pay and prior pay I just described for the full sample. But when I look just at software engineers, all of whom are classified by the same job levels, I can show even more explicitly the roles of prior pay and starting job level, and examine the effects of differences in prior job experience measured at a very detailed level.

38. First, I repeat the analysis of the gender gap in base pay during the class period, for software engineers.[30] As shown in Analysis Table 7, when I control for individual characteristics and job family, the gender gap in pay is 3.5% (13.6 standard deviations), implying a base pay gap of $6,149 (Panel A, column (1)). Controlling for job level reduces this gap, as in Analysis Table A1, although it remains negative and statistically significant (3.8 standard deviations), implying a pay gap of $548. This result is the same as what I found for the full sample of all men and women in all jobs.

39. Next, I show that the starting gender gap in pay is quite similar to the gender gap in pay during the class period. This is reported in Analysis Table 8, where I first repeat the analysis for the class period for the subsample with non-missing starting pay data. Comparing column (2) to column (1), we see that the gender gap in starting pay is nearly identical to the gender gap in class period pay, in the models controlling for individual characteristics and qualifications, and job families. The estimated gender gap in starting pay is 3.1% (13.8 standard deviations, implying a pay gap of $4,262), virtually the same as the 3.1% estimate for the gender gap in class period base pay in column (1).

40. However, when I control for the job level into which Google employees were hired, the estimated gender gap in starting pay becomes effectively zero; it is very small and statistically

[30] Again, this analysis focuses on base pay, which is the component of compensation that can be meaningfully compared to starting pay.

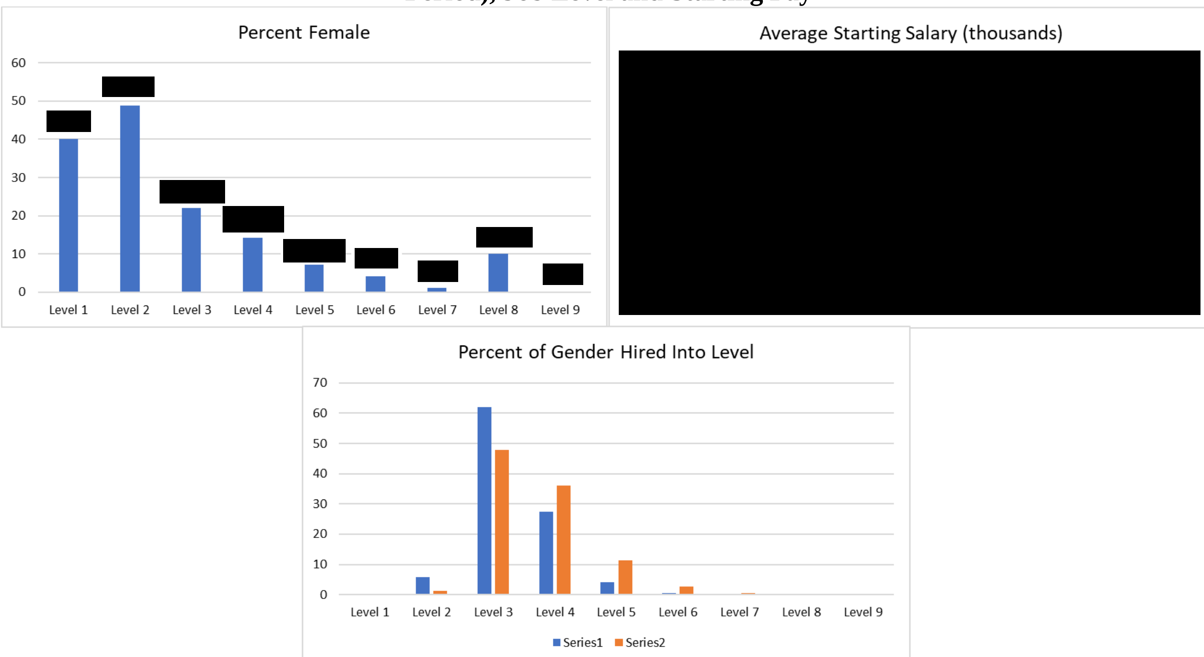
insignificant (0.1 standard deviations; see column (3)). In other words, controlling for level explains the difference in starting pay.

41. The difference in the starting pay gap depending on whether I control for starting job levels implies that, among software engineers, men are hired into higher-paying job levels than are women. This is illustrated in Figure 3. The first panel shows the percent female hired at each job level (and the number). The percentage of women declines dramatically as the salary level increases. The percentage of women among Software Engineer hires is as follows: level 1 (40.0%); level 2 (48.9%); level 3 (22.0%); level 4 (14.2%); level 5 (7.2%); level 6 (4.2%); level 7 (1.1%); level 8 (10.0%); and level 9 (0%).[31]

42. On the other hand, as the second panel of Figure 3 shows, pay rises sharply with job level. Finally, the third panel provides the comparison between the hiring of women by job level and the hiring of men by job level. The panel shows that about 67.8% of women are hired into job levels 2 or 3 (mostly at level 3), while 50.7% of men are hired into job level 4 or higher (most at levels 4 and 5).

[31] The only deviations from this pattern of a declining percent female the higher the job level come from cells with a very small number of observations ([redacted] In contrast, there are [redacted] hires at level 2, [redacted] hires at level 3, [redacted] hires at level 4, [redacted] hires at level 5, [redacted] hires at level 6, and [redacted] hires at level 7 (and [redacted] at level 9).

Figure 3: Hiring of Men and Women Software Engineers (Working as Software Engineers in Class Period), Job Level and Starting Pay



Note: Starting salary is in December 2018 dollars. [redacted]

43. The estimates in column (2) of Analysis Table 8 already established that the hiring of women into lower job levels, in the software engineer family, is not explained by differences in education (including highest degree, field, and school) or in prior years of experience. These education measures are more detailed than is common in labor economics research on pay discrimination, and often labor economists have to use a measure of potential rather than actual labor market experience (that is, number of years since leaving school, rather than an actual measure of time worked). However, I can also show that the hiring of women into lower job levels, in the software engineer family, is not explained by men having more, or more relevant, job experience. I show this in Analysis Table 9. Here, I use detailed information on prior job experience in the Google data to control for the length of time employed at different jobs prior to coming to Google, captured by a very detailed classification of the job titles people held before they came to Google.[32,33] When I control for very detailed measures of the jobs men and women held, and for how long, before they were hired at Google, there is still a statistically significant gap in starting pay; the estimated gender gap in starting pay is 1.8% (7.4 standard deviations),

[32] I construct the job history combining HR data (from HR_Profile_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER) that lists the previous six jobs, and application data (from Applicant_Candidate Employment_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER) that appear to not limit the number of previous jobs (removing duplicates). These data include job titles, start and stop dates, and company names. This is remarkably rich job history data to have available for this kind of analysis. For more explanation on how these data are used, see the notes to Analysis Table 9.

I also considered using job history information from a sample of resumes provided by Google (PROD050). After studying a sample of these resumes and comparing them to the job history data (which are machine-readable), I concluded that the job history data are more useful and reliable. They are available for all hires, and are coded in a consistent manner. Moreover, based on the comparisons I did, there was no clear indication that the machine-readable job history data are less complete than the resumes. There is sometimes a short or other job that appears on one and not the other, but the data did not consistently include fewer jobs than the resumes. Finally, the job history data were more likely to have start and stop dates of jobs. Using the resumes to construct job histories would be much more difficult, as job histories are not directly machine-readable from resumes, and are not coded in a consistent way across resumes. Moreover, many hires had a number of resumes, and they sometimes change, so it is not clear which one should be used. And even then, I would only have a sample of resumes.

The two data sources also provide information on education (in the files HR_Education_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER and Applicant_Candidate Education_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER). Again, I concluded that the machine-readable data are more useful and just as reliable. There were not many discrepancies, and they tended to concern things that do not affect my analysis (like degree dates differing slightly, or the resumes not having degree dates).

[33] I could not do this analysis for the larger sample not restricted to software engineers, because the variety in prior job titles would be too great.

implying a gender gap in pay of $2,457. (See Analysis Table 9, column (2)).

44. Next, I show that the gender gap in starting pay mirrors the gender gap in prior pay. These results are reported in Analysis Table 10, for the subsample of the data that includes both starting pay and prior pay. The estimated gender gaps in starting pay and prior pay are very similar (2.4% for starting pay, statistically significant; and 2.1% for prior pay, also statistically significant). And the estimated difference between the gender gaps in starting pay and prior pay is small statistically insignificant (0.5 standard deviations). (See Analysis Table 10, column (3).)

45. I have already shown in Figure 3 that, among software engineers, women are hired into lower job levels. But that figure did not hold constant differences between men and women hired as software engineers. Analysis Table 11 does hold those differences constant, and the results show that differences between men and women hired as software engineers do not explain the hiring of women at lower levels. Panel A shows results for the level (coded as 1-9) into which people were hired. I find evidence that women are hired at lower levels (which, as Figure 3 shows, pay less), a difference that is statistically significant (14.0 standard deviations). This difference exists despite the fact that my model controls for the detailed measures of education I have used earlier, and also controls for experience – amount of prior experience before they were hired (Panel A, column (1)), and even very detailed measures of the jobs men and women held, and for how long, before they were hired (Panel A, column (2)). As an alternative, I show that, controlling for education and amount of experience prior to Google, the probability that women are hired at level 4 or above vs. level 3 or below – which Figure 3 showed was the major difference between men and women – is lower by 0.056 (or 5.6 percentage points), a strongly significant differences (8.0 standard deviations); see Panel B, column (1). And a statistically significant differential persists (3.9 percentage points; 6.3 standard deviations), even when I add the more detailed experience measures of the jobs men and women held, and for how long, before they were hired (column (2)).

46. What Analysis Table 11 shows, in other words, is that Google hires women with comparable experience and education to men into lower job levels. This conclusion comes directly from the job level results in Analysis Table 11. And it is also reflected in the extent to which controlling for job level at hire explains the gender gap in starting pay (Analysis Table 8, columns (2) and (3)).

47. The findings described thus far in this section suggest that prior pay largely drives the gender gap in starting pay, and it does this through determining the job levels at which men and women are hired at Google. And that same starting pay gap driven by the level at which people are hired in turn drives the gender wage gap in the class period. This explains why, in models not controlling for job level, the starting pay gap and the prior pay gap are of a similar magnitude, and in turn are similar to the class period gender gap in pay. And it explains why job level explains both the starting pay gap and a large share of the class period gender gap in pay.

48. However, I can show more directly how prior pay determines the starting job level (which in turn drives the class period gender gap in pay), based on my understanding of Google's explicit pay policies and an empirical analysis of whether Google in fact made levelling decisions based on prior pay and Market Reference Points (MRPs). Empirical analysis establishes that what the PMQ testimony and Google documents say is the policy is in fact the practice: that Google assigned persons to a job level in which, if paid their prior pay, they would earn [redacted].[34]

49. [redacted]

[34] Williams, in his deposition, states: "…the process we go through to assign a pay point to a job – so we describe that pay point as a 'market reference point,' an MRP. That's the terminology we would use. The market reference point reflects a position in the external market for a role" (at 99: 19-24).

a. Wagner, in his OFCCP testimony, states that in setting base pay for new hires that are not recent college graduates, “We would endeavor to bring them in as – at our baseline rate of 80 percent…If they’re making less than the market median, or 50, we’d give them 80. If they were making 70, we would give them 80. If they were already making 80, we might give a modest or small increase to bring them in. The principle is we try to bring them in as low as possible within our salary below the current employee, so that they can earn future increases based on performance…If they were making 90, we would endeavor to bring them certainly no more than 90, because we don’t want them to – we use the term ‘leap frog.’ We don’t want them to leap past the current employees who are already in that job and performing well” (at 172: 9 – 173: 2).

b. Similarly, in his deposition, Williams states: [redacted] (at 104: 2-7).

c. And Williams further states: “[redacted] (at 134: 3-8).

d. In his deposition, Williams is asked: “[redacted]

[redacted] (at 136: 20 – 137:4).

e. And finally, Williams, in his testimony, indicates that [redacted] (at 191: 1-17; see also Exhibit 510).

50. The adherence of Google to the norm of hiring people so that their starting salary is [redacted] at is reflected in Figure 4.[35] Nearly all observations have a ratio of starting salary/MRP between .8 (80%) and 1 (100%), and there are pronounced spikes at these two values. And there are very few observations above 1 (100%).

51. The PMQ testimony described above, and Google documents,[36] indicate that prior pay has a strong influence on starting level. I therefore define a "target job level based on prior pay" based on the rules described above. "Target job level based on prior pay" is defined as the level for which prior pay is within the range [80% MRP, MRP].[37] Figure 5 shows that this target job level is highly predictive of actual starting job level. Thus, the empirical analysis confirms that Google assigned persons to a job level in which, if paid their prior pay, they would earn between [redacted]

52. Moreover, the relationship between prior pay and starting job level, mediated through the MRP, is also apparent in the relationship between prior pay and starting pay. This is apparent in Figure 6A, which plots starting pay against prior pay, for software engineers, for those with data

[35] Note that I have data on MRP only for the class period.
[36] Exs. 510, 512.
[37] If prior pay is less than the 80% MRP associated with the lowest level for a job family (level 1 for software engineers) then the target level is set to the lowest level (consistent with the leveling guidance in GOOG-ELLIS-00010907). If prior pay is more than the MRP associated with highest level for a job family (level 9 for software engineers) then target level is set to the highest level. If prior pay falls within the ranges of two different levels, target level is set as the lower of the two levels.

on MRP. The relatively flat scatterplot, with a sharp minimum, at low levels of prior pay reflects the role of targeting starting pay at 80% of MRP. This is even more apparent in Figure 6B, which is the same scatterplot, but distinguishing the data points for those whose prior pay is below 80% of the MRP for the target job level (the solid dots in the figure). In particular, the data points for these latter observations are clearly scattered on a much more horizontal line with a sharp minimum for the lower values of prior pay.

53. Turning to the role of prior pay via the target job level, I find, first, that this target job level based on prior pay accounts for one-half or more of the difference in job levels at which women are hired relative to men. This is shown in Analysis Table 12, comparing columns (1) and (2). I show this in two different ways – first just estimating models for the level itself (Panel A), and second estimating a probit model for whether one is hired at level 4 of higher vs. level 3 or lower (Panel B).

54. Even more strikingly, I find that the gender difference in starting pay shrinks by two thirds (from 2.6% to 0.8%) once I control for the target job level based on prior pay. (See Analysis Table 13.) Because I can explain such a large share of the gender difference in starting pay by Google's pay policies that link prior pay to a target starting job level (which in turn closely predicts actual starting job level), I conclude that the job levels into which men and women are hired based on their prior pay explain the overwhelming share of the gender gap in starting pay.

55. Finally, I also show that the starting job level, which is strongly influenced by Google's policies regarding prior pay, persists into the class period. Recall that the first analysis I described (Analysis Table 1, and Analysis Table 7 for software engineers) indicated that job levels in the class period were a key driver of gender differences in pay in the class period. The evidence on how starting job levels persist into the class period is reported in Analysis Table 14. The estimates in columns (1) and (2) show that there is a large and statistically significant

difference in the job level at which women are employed in the software engineer family in the class period. Women are employed at lower job levels than men (an average of 0.15, or about one-sixth of a job level), and the difference is strongly significant (12.3 standard deviations). However, when I control for the job level at which they were hired, this difference shrinks by nearly two-thirds (column (2)). Next, I instead control for the target job level based on prior pay. Restricting the sample to those for whom I have prior pay data and can defined the target job level, women are employed at lower job levels than men (an average of 0.15), and the difference is strongly significant (6.7 standard deviations). Controlling for the target job level (at hire) based on prior pay, this difference shrinks by one-half (column (4)). Analysis Table 14 establishes that job levels during the class period are very strongly influenced by the job levels into which people are hired. And these job levels at which people are hired have little to do with education and prior experience, but instead are driven in large part by prior pay differences.

56. These analyses for software engineers fit together as follows:

a. Because of Google's policies regarding starting pay and prior pay, a new hire's prior pay strongly influences the job level at which he or she gets hired. This accounts, to a large extent, for women being hired at lower job levels, and this accounts for women's lower starting pay.

b. As a consequence, the gender gap in starting pay reflects the gender gap in prior pay.

c. The gender gap in starting pay persists throughout a woman's tenure at Google.

d. Like the gender gap in starting pay, the gender gap in pay during the class period is largely explained by the levels at which women are employed relative to men, which in turn are driven to a large extent by the levels at which women are hired. (However, there is also a gender gap in pay within level; see Analysis Table 7.)

57. In other words, prior pay drives decisions about the levels at which men and women are

hired ("leveling"), and these leveling decisions persist during the class period to explain, to a large extent, the gender gap in pay during the class period. There is a gender gap in pay during the class period for men and women employed in substantially equal or similar jobs (i.e., in the models controlling for both job families and job levels). But there is a much larger gender gap in pay that is generated by these leveling decisions at hire, which are in turn driven by prior pay. This conclusion holds for the software engineer job family, and – as I show below – for Google employees overall.

VIII. Applying the Target Job Level based on Prior Pay to the Analysis of Starting Pay for All Hires

58. I focused on software engineers in this section because I start out by analyzing the determinants of the job level at which a person is hired, and levels are only meaningful within job families. For example, there are [redacted] levels of software engineers and only [redacted] levels of pre-school teachers, so numeric level may have a different meaning for each job family. However, the analysis of Analysis Table 13 can be carried out for the full sample, based on the same definition of the target job level based on prior pay.

59. First, I show, in Figure 7, that for all hires, and not just software engineers, starting pay is almost always between 80% and 100% of MRP (paralleling Figure 4). And I show, in Figure 8, that target job level based on prior pay is highly predictive of actual job level (paralleling Figure 5).

60. Second, Analysis Table 15 repeats the starting pay analysis of Analysis Table 13, but now for all hires. I find that the gender difference in starting pay becomes much smaller (declining by over two-thirds) once I control for the target job level based on prior pay.[38] In other words, the target job levels into which men and women are hired based on their prior pay explain

[38] Because the sample now includes information on job levels in different families, and job levels are not comparable across job families, I include interactions between the targeted job level and each job family.

a very large share of the gender gap in starting pay.

61. Next, paralleling my analysis for software engineers to bring the evidence full circle, I also show that the starting job level, which is strongly influenced by Google's policies regarding prior pay, persists into the class period, now looking at all hires. The new evidence is reported in Analysis Table 16. The estimates in columns (1) and (2) show that there is a large and statistically significant difference in the job level at which women are hired.[39] Women are employed at lower job levels (an average of 0.13, or about one-eighth of a job level), and the difference is strongly significant (11.8 standard deviations). However, as with Software Engineers, when I control for the job level at which they were hired, this difference shrinks by more than two-thirds (column (2)). Next, I instead control for the target job level based on prior pay. Restricting the sample to those for whom I have prior pay data and can defined the target job level, women are employed at lower job levels (an average of 0.11), and the difference is strongly significant (5.7 standard deviations). Controlling for the target job level (at hire) based on prior pay, this difference shrinks by more than one-half (column (4)). Analysis Table 16 establishes that job levels during the class period are largely determined by the job levels into which people are hired. And these job levels at which people are hired have little or nothing to do with education and prior experience, but instead are driven in large part by prior pay differences.

62. Finally, to make the point as unambiguously as possible, Analysis Table 17 presents the same kind of analysis, but for base pay in the class period.[40] The estimates in column (1) replicate the gender difference in base pay in the class period that we have seen before, when I control for individual characteristics such as education, experience, and performance, as well as

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[39] In this table, I restrict attention to women who did not change job family since being hired, paralleling my analysis of software engineers.

[40] This is particularly valuable because the levels I study in Analysis Table 16 are not directly comparable across job families, but pay is directly comparable.

job family; the difference is 2.9% (12.9 standard deviations). If, instead of controlling for job level in the class period, I control for *starting* job level, the gender gap in base pay *in the class period* falls by nearly two-thirds (column (2)). Alternatively, in columns (3) and (4) I instead explore how controlling for the target job level (at hire) based on prior pay accounts for the gender difference in base pay in the class period. In this case, controlling for the target job level *at hire* reduces the gender difference in base pay *in the class period* by more than two-thirds. Analysis Table 17 thus provides a clear illustration of how the leveling decisions Google makes at the time of hire generate lower pay for women in the class period. And the preceding analyses show two key results in interpreting these findings; first, these leveling decisions are not explained by education and experience differences between men and women at hire; and second, these leveling differences do reflect prior pay differences between men and women that Google uses in making its leveling decisions.

IX. Starting Pay and Levels of Named Plaintiffs

63. Like other women in the class, the named plaintiffs were paid less than men in the same job code with similar education, experience, performance, location, tenure, and time in job level. This is shown in Analysis Table 18. Here, I use a regression model very similar to that in Analysis Table 1. The only difference is that instead of including a single dummy variable (or indicator) for women ("Female"), I break this into five separate variables – one for each of the four named plaintiffs, and then one for all other women ("Other Female"). This allows me to estimate the difference in pay, relative to men who were performing substantially equal work in jobs the performance of which require substantially equal skill, effort, and responsibility, performed under similar working conditions, for all women other than the named plaintiffs, and then for the named plaintiffs.

64. The estimates show that the same lower pay for women relative to men who were performing substantially equal work in jobs the performance of which require substantially equal

skill, effort, and responsibility, performed under similar working conditions, holds for each of the named plaintiffs. The estimated pay gap is negative – indicating lower pay than men – for all four named plaintiffs.[41] Not surprisingly, the estimated pay gap for all other women is nearly identical to the estimate for all women in Analysis Table 1 (column (5)).

65. Regarding the four named plaintiffs, Ms. Ellis received a starting salary of [redacted], equal to her prior pay. And Ms. Lamar received starting salary of [redacted], slightly above her prior pay of [redacted]. Ms. Pease received a starting salary of [redacted], while her prior pay was [redacted]. The data do not provide information about Ms. Wisuri's prior pay. Her starting salary at Google was [redacted].

[41] Three of the four estimates specific to the named plaintiffs are statistically significant (standard deviations ranging from 3.9 to 4.7). However, the absence of a statistically significant difference for Ms. Ellis is not noteworthy, since the estimate is for only one person.

Summary Table 1: Equal Pay Analysis, Gender Differences in Pay During Class Period, Models Include Individual Characteristics and Qualification, and Job Codes

A. Estimated Gender Difference in Base Pay During Class Period (Anal. Table 1)

Female shortfall	**-0.35%**
Standard deviations	**5.08**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 million
Implied gender difference in pay	**-$600**

B. Estimated Gender Difference Base Pay + Standard Bonus During Class Period (Anal. Table 1)

Female shortfall	**-0.45%**
Standard deviations	**6.07**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion
Implied gender difference in pay	**-$937**

C. Estimated Gender Difference Base Pay + Standard Bonus + Equity During Class Period (Anal. Table 1)

Female shortfall	**-0.55%**
Standard deviations	**2.97**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 100
Implied gender difference in pay	**-$1,894**

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. The number of observations = [redacted]. See notes to corresponding Analysis Table(s) for additional explanation.

Summary Table 2: Equal Pay Analysis, Gender Differences in Pay During Class Period, Models Include Individual Characteristics and Qualification, and Job Families, but Do Not Control for Level

A. Estimated Gender Difference in Base Pay During Class Period (Anal. Table 1)

Female shortfall	**-2.9%**
Standard deviations	**12.97**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion
Implied gender difference in pay	**-$5,062**

B. Estimated Gender Difference Base Pay + Standard Bonus During Class Period (Anal. Table 1)

Female shortfall	**-3.3%**
Standard deviations	**13.00**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion
Implied gender difference in pay	**-$6,988**

C. Estimated Gender Difference Base Pay + Standard Bonus + Equity During Class Period (Anal. Table 1)

Female shortfall adding job family controls	**-4.9%**
Standard deviations	**11.95**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion
Implied gender difference in pay	**-$16,794**

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. The number of observations = [redacted]. See notes to corresponding Analysis Table(s) for additional explanation.

Summary Table 3: Prior Pay Analysis, Analysis of Starting Pay, Starting Level, and Prior Pay, All Hires

Female shortfall controlling for individual differences	**-3.0%**
Standard deviations	**6.84**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion
Implied gender difference in pay	**-$4,351**
R^2	0.66
Female shortfall controlling for individual differences and <u>dummy variables for "target job level based on prior pay"</u> (highest job level for which prior pay is between [redacted] of MRP)	**-0.9%**
Standard deviations	**2.97**
Implied gender difference in pay	**-$1,317**
R^2	0.84

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. The number of observations = [redacted]. (The number of observations is smaller because we only have information on MRP for the class period.) See notes to corresponding Analysis Tables for additional explanation, including Analysis Table 12 for more details on "target job level based on prior pay."

Analysis Table 1: Estimated Gender Differences in Pay During Class Period

	(1)	(2)	(3)	(4)	(5)
	Year fixed effects only	Add individual controls	Add performance rating	Add job family controls	Add job code and time in job level controls
A. Log of Base Pay					
Female shortfall	**-13.84%**	**-7.59%**	**-7.50%**	**-2.91%**	**-0.35%**
Std. deviations	**33.23**	**23.32**	**23.60**	**12.97**	**5.08**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 million
Pay difference implied by female % shortfall[1]	**-$24,038**	**-$13,177**	**-$13,021**	**-$5,062**	**-$600**
Observations	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
B. Log of Base Pay and Standard Bonus					
Female shortfall	**-15.79%**	**-8.63%**	**-8.50%**	**-3.34%**	**-0.45%**
Std. deviations	**33.08**	**23.03**	**23.44**	**13.00**	**6.07**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 100 million
Pay difference implied by female % shortfall	**-$33,024**	**-$18,057**	**-$17,783**	**-$6,988**	**-$937**
Observations	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
C. Log of Base Pay, Standard Bonus, and Equity					
Female shortfall	**-21.37%**	**-12.39%**	**-12.21%**	**-4.91%**	**-0.55%**
Std. deviations	**33.97**	**23.48**	**23.97**	**11.95**	**2.97**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 100
Pay difference implied by female % shortfall	**-$73,156**	**-$42,433**	**-$41,820**	**-$16,794**	**-$1,894**
Observations	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
Controls (Panels A-C)					
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Tenure	No	Yes	Yes	Yes	Yes
Highest education level	No	Yes	Yes	Yes	Yes
Leave of absence	No	Yes	Yes	Yes	Yes
Actual prior experience	No	Yes	Yes	Yes	Yes
Location	No	Yes	Yes	Yes	Yes
Campus hire	No	Yes	Yes	Yes	Yes
Performance rating	No	No	Yes	Yes	Yes
Job family	No	No	No	Yes	No
Job code	No	No	No	No	Yes
Time in job level	No	No	No	No	Yes
Indicators for each of the 242 most common schools[2]	No	Yes	Yes	Yes	Yes
Indicators for each of the 73 most common fields[3]	No	Yes	Yes	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. The standard deviations for all regression estimates are computed clustering at the individual level. Base pay, Standard bonuses and Equity are all converted to December 2018 dollars. Observations missing data on performance ratings or gender are dropped. As noted in Appendix Table A2 and documented in Appendix Table B1, the Job code controls include a small amount of independent information relative to Job families and Job levels. Highest education level is measured in terms of degree. There are dummy variables included for missing education level (degree) and other education variables (school and field).

[1] This is calculated by multiplying the average male base pay in levels by the estimated female difference in log pay.

[2] This captures 75% of all school names from the most recently obtained degree. Unless otherwise stated these indicators are always calculated on snapshot analysis subsample 1. See Appendix Table B2.

[3] This captures 75% of all fields for the most recently obtained degree. Unless otherwise stated these indicators are always calculated on snapshot analysis subsample 1. See Appendix Table B3.

Analysis Table 2: Estimated Gender Differences in Pay During Class Period, Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline (Analysis Table 1, Columns (4)-(5))		With Manager Controls		With Department and Unified Rollup Controls		With Cost Center Controls	
Job title controls?	No	Yes	No	Yes	No	Yes	No	Yes
A. Log of Base Pay								
Female shortfall	**-2.91%**	**-0.35%**	**-2.62%**	**-0.35%**	**-2.73%**	**-0.32%**	**-2.62%**	**-0.31%**
Std. deviations	**12.97**	**5.08**	**13.37**	**5.17**	**12.32**	**4.80**	**12.09**	**4.61**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 million	< 1 in 1 billion	< 1 in 1 million	< 1 in 1 billion	< 1 in 100,000	< 1 in 1 billion	< 1 in 100,000
Pay difference implied by female % shortfall	**-$5,062**	**-$600**	**-$4,557**	**-$608**	**-$4,734**	**-$563**	**-$4,550**	**-$538**
Observations	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
B. Log of Base Pay and Standard Bonus								
Female shortfall	**-3.34%**	**-0.45%**	**-2.98%**	**-0.46%**	**-3.12%**	**-0.43%**	**-2.99%**	**-0.40%**
Std. deviations	**13.00**	**6.07**	**13.59**	**6.22**	**12.32**	**5.79**	**12.03**	**5.53**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100 million	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 100 million	< 1 in 1 billion	< 1 in 10 million
Pay difference implied by female % shortfall	**-$6,988**	**-$937**	**-$6,238**	**-$953**	**-$6,522**	**-$890**	**-$6,253**	**-$844**
Observations	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
C. Log of Base Pay and All Annual Bonuses								
Female shortfall	**-3.34%**	**-0.45%**	**-2.98%**	**-0.46%**	**-3.12%**	**-0.43%**	**-2.99%**	**-0.40%**
Std. deviations	**13.01**	**6.10**	**13.60**	**6.25**	**12.33**	**5.83**	**12.04**	**5.56**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100 million	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 100 million	< 1 in 1 billion	< 1 in 10 million
Pay difference implied by female % shortfall	**-$6,989**	**-$940**	**-$6,239**	**-$955**	**-$6,524**	**-$894**	**-$6,254**	**-$846**
Observations	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
D. Log of Base Pay and All Annual Bonuses, and Sales Bonuses.								
Female shortfall	**-3.42%**	**-0.56%**	**-3.06%**	**-0.57%**	**-3.21%**	**-0.54%**	**-3.09%**	**-0.52%**
Std. deviations	**13.20**	**7.17**	**13.78**	**7.32**	**12.56**	**6.91**	**12.35**	**6.81**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Pay difference implied by female % shortfall	**-$7,213**	**-$1,181**	**-$6,459**	**-$1,197**	**-$6,759**	**-$1,130**	**-$6,518**	**-$1,099**
Observations	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
E. Log of Base Pay, Standard Bonus, and Equity								
Female shortfall	**-4.91%**	**-0.55%**	**-4.31%**	**-0.57%**	**-4.46%**	**-0.51%**	**-4.35%**	**-0.55%**
Std. deviations	**11.95**	**2.97**	**12.33**	**3.10**	**10.99**	**2.70**	**10.78**	**2.91**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100	< 1 in 1 billion	< 1 in 100	< 1 in 1 billion	< 1 in 100	< 1 in 1 billion	< 1 in 100
Pay difference implied by female % shortfall	**-$16,794**	**-$1,894**	**-$14,758**	**-$1,959**	**-$15,279**	**-$1,733**	**-$14,889**	**-$1,867**
Observations	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Controls (Panels A-C)								
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Tenure	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Highest education level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Leave of absence	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Actual prior experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Campus hire	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Performance rating	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Job family	Yes	No	Yes	No	Yes	No	Yes	No
Job code	No	Yes	No	Yes	No	Yes	No	Yes
Time in job level	No	Yes	No	Yes	No	Yes	No	Yes
Manager	No	No	Yes	Yes	No	No	No	No
Department and Unified rollup	No	No	No	No	Yes	Yes	No	No
Cost center	No	No	No	No	No	No	Yes	Yes
Indicators for each of the 243 most common schools	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indicators for each of the 73 most common fields	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Table 1. Observations missing data on performance ratings or gender are dropped. In columns (5) and (6), observations missing data on Department or Unified Rollup are also dropped.

Analysis Table 3: Estimated Gender Difference in Base Pay During Class Period, Controlling for Starting Salary

	(1)	(2)
Job title controls?	No	Yes
A. On Full Subsample (Anal. Table 1, columns (4) and (5))		
Female shortfall	**-2.91%**	**-0.35%**
Std. deviations	**12.97**	**5.08**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 million
Pay difference implied by female % shortfall	**-$5,062**	**-$600**
Observations	[redacted]	[redacted]
B. On Subsample with Starting Salary Information		
Female shortfall	**-2.89%**	**-0.34%**
Std. deviations	**12.31**	**4.87**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100,000
Pay difference implied by female % shortfall	**-$5,013**	**-$596**
Observations	[redacted]	[redacted]
C. Controlling for Starting Salary		
Female shortfall	**-0.83%**	**-0.17%**
Std. deviations	**4.66**	**2.57**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 100,000	< 1 in 20
Pay difference implied by female % shortfall	**-$1,436**	**-$290**
Log Starting salary	0.5428	0.1445
Std. deviations	106.42	48.64
Observations	[redacted]	[redacted]
Controls (Panels A-C)		
Year fixed effects	Yes	Yes
Tenure	Yes	Yes
Highest education level	Yes	Yes
Leave of absence	Yes	Yes
Actual prior experience	Yes	Yes
Location	Yes	Yes
Campus hire	Yes	Yes
Performance rating	Yes	Yes
Job family	Yes	No
Job code	No	Yes
Time in job level	No	Yes
Indicators for each of the 242 most common schools	Yes	Yes
Indicators for each of the 73 most common fields	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Table 1. Starting pay is converted to December 2018 dollars. Location controls in panel C include controls for starting location (since I include starting pay as a control), as well as location for period covered by the snapshot data. Observations missing data on performance ratings or gender are dropped. Observations missing data on starting pay (or with starting pay reported by not in U.S. dollars), or on starting job level, are also dropped in Panels B and C.

Analysis Table 4: Estimated Gender Difference in Base Pay and Starting Pay, With and Without Controls for Job Level

	(1)	(2)	(3)	(4)
Sample:	Snapshot Data with Starting Salary Information, Baseline (Analysis Table 3, Panel B, Columns (1) and (2))		Hiring Data	
Dependent Variable:	Log of Base Pay in Class Period	Log of Base Pay in Class Period	Log of Starting Salary	Log of Starting Salary
Job code controls?	No	Yes	No	Yes
Female shortfall	**-2.89%**	**-0.34%**	**-2.91%**	**-0.13%**
Std. deviations	**12.31**	**4.87**	**13.50**	**1.41**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100,000	< 1 in 1 billion	> 1 in 20
Pay difference implied by female % shortfall	**-$5,013**	**-$596**	**-$4,001**	**-$185**
Observations	[redacted]	[redacted]	[redacted]	[redacted]
Year fixed effects	Yes	Yes	Yes	Yes
Tenure	Yes	Yes	No	No
Highest education level	Yes	Yes	Yes	Yes
Leave of absence	Yes	Yes	No	No
Actual prior experience	Yes	Yes	Yes	Yes
Location	Yes	Yes	Yes	Yes
Campus hire	Yes	Yes	Yes	Yes
Performance rating	Yes	Yes	No	No
Job family	Yes	No	Yes	No
Job code	No	Yes	No	Yes
Time in job level	No	Yes	No	No
Indicators for each of the 242 most common schools	Yes	Yes	Yes	Yes
Indicators for each of the 73 most common fields	Yes	Yes	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Tables 1 and 3. When I study starting pay or any hiring related outcome, the location controls are for the starting location. Observations missing data on performance ratings (for class period analysis) or gender are dropped. Observations missing data on starting pay (or with starting pay not reported in U.S. dollars), or on starting job level, are also dropped.

Analysis Table 5: Estimated Gender Differences in Starting Pay and Prior Pay

	(1)	(2)	(3)	(4)
Dependent Variable	Ln(Starting Pay) (Analysis Table 4, column (3))	Ln(Starting Pay)	Ln(Prior Pay)	Ln(Starting Pay) – Ln(Prior Pay)
Observations	Full Sample	With prior pay	With prior pay	With prior pay
Female shortfall	**-2.91%**	**-2.55%**	**-2.35%**	**-0.20%**
Std. deviations	**13.50**	**7.02**	**3.87**	**0.45**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1,000	> 1 in 20
Pay difference implied by female % shortfall	**-$4,001**	**-$3,627**	**-$3,138**	
Observations	[redacted]	[redacted]	[redacted]	[redacted]
Year fixed effects	Yes	Yes	Yes	Yes
Job family	Yes	Yes	Yes	Yes
Location	Yes	Yes	Yes	Yes
Highest education level	Yes	Yes	Yes	Yes
Campus hire	Yes	Yes	Yes	Yes
Actual prior experience	Yes	Yes	Yes	Yes
Indicator for each of the 242 most common schools	Yes	Yes	Yes	Yes
Indicator for each of the 73 most common fields	Yes	Yes	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Tables 1, 3, and 4. Observations missing data on gender are excluded. Observations missing data on starting pay (or with starting pay reported by not in U.S. dollars), or on starting job level, are also dropped, as are observations missing data on prior pay in columns (2)-(4). Prior pay is converted to December 2018 dollars. The top and bottom 2% of prior pay observations are trimmed from the sample with non-missing data on prior pay, gender, and starting job level.

Analysis Table 6: Share of Total Hiring Data Set of Observations per Job Family, Job Families with > 1% of Workforce

Job Family Name	Percent of All Observations	Number of Unique Job Levels
Software Engineer	54.23%	9
Program Manager	5.13%	7
ENG_MEMBER	4.25%	8
Product Management	3.93%	5
Product Marketing Manager	3.31%	6
Alphabet, Software Engineer, Tools and Infrastructure	3.29%	6
Technical Program Manager	2.79%	6
Site Reliability Engineer - Software Engineer	1.55%	5
UX Researcher	1.29%	7
Enterprise Program Manager	1.19%	6
Residuals[1]	19.04%	8

[1]Residuals refers to all job families with fewer than 1% of the total observations. We drop individuals with missing starting job level or starting salary.

Analysis Table 7: Estimated Gender Differences in Base Pay During Class Period, Software Engineers

	(1)	(2)
Sample	Software Engineers	
Female shortfall	**-3.49%**	**-0.31%**
Std. deviations	**13.55**	**3.78**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1,000
Pay difference implied by female % shortfall[1]	**-$6,149**	**-$548**
Observations	[redacted]	[redacted]
Controls		
Year fixed effects	Yes	Yes
Tenure	Yes	Yes
Highest education level	Yes	Yes
Leave of absence	Yes	Yes
Actual prior experience	Yes	Yes
Location	Yes	Yes
Campus hire	Yes	Yes
Performance rating	Yes	Yes
Job level	No	Yes
Time in job level	No	Yes
Indicators for each of the 154 most common schools[1]	Yes	Yes
Indicators for each of the 9 most common fields[2]	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Table 1. Observations missing data on performance ratings or gender are dropped.

[1] This captures 75% of all school names from the most recently obtained degree. These indicators are calculated on hiring analysis subsample 1. See Appendix Table B4.

[2] This captures 75% of all fields for the most recently obtained degree. These indicators are calculated on hiring analysis subsample 1. See Appendix Table B5.

Analysis Table 8: Estimated Gender Difference in Starting Pay, Individuals Hired as Software Engineers and Working as Software Engineers in Class Period

	(1)	(2)	(3)
Sample:	Snapshot Data with Starting Salary Information	Hiring Data	
Dependent Variable:	Log of Base Pay in Class Period	Log of Starting Salary	Log of Starting Salary
Job level controls?	No	No	Yes
Female shortfall	**-3.14%**	**-3.09%**	**-0.01%**
Std. deviations	**12.76**	**13.83**	**0.11**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	> 1 in 20
Pay difference implied by female % shortfall	**-$5,239**	**-$4,262**	**-$18**
Observations	[redacted]	[redacted]	[redacted]
Year fixed effects	Yes	Yes	Yes
Tenure	Yes	No	No
Highest education level	Yes	Yes	Yes
Leave of absence	Yes	No	No
Actual prior experience	Yes	Yes	Yes
Location	Yes	Yes	Yes
Campus hire	Yes	Yes	Yes
Performance rating	Yes	No	No
Job level	No	No	Yes
Time in job level	No	No	No
Indicators for each of the 154 most common schools	Yes	Yes	Yes
Indicators for each of the 9 most common fields	Yes	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Tables 1, 3, and 4. Observations missing data on performance ratings (for class period analysis) or gender are dropped. Observations missing data on starting pay (or with starting pay not reported in U.S. dollars), or on starting job level are also dropped.

Analysis Table 9: Estimated Gender Difference in Starting Pay, Individuals Hired as Software Engineers and Working as Software Engineers in Class Period, with Detailed Prior Experience Controls

	(1)	(2)
Prior experience controls:	Single years measure (same as Analysis Table 8, column (2))	By job title for 75% most common titles (recent and all others)
Female shortfall	**-3.09%**	**-1.78%**
Std. deviations	**13.83**	**7.39**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion
Pay difference implied by female % shortfall	**-$4,262**	**-$2,457**
Actual prior experience	0.0209	
Std. error	(0.0003)	
Std. deviations	76.52	
Observations	[redacted]	[redacted]
Year fixed effects	Yes	Yes
Location	Yes	Yes
Highest education level	Yes	Yes
Campus hire	Yes	Yes
Experience not in controlled for job titles	No	Yes
Experience in each of the 484 most common titles (not most recent)[1]	No	Yes
Experience in each of the 109 most common titles (most recent)[1]	No	Yes
Indicators for each of the 154 most common schools	Yes	Yes
Indicators for each of the 9 most common fields	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Tables 1, 3, and 4. Observations missing data on gender, starting pay (or with starting pay not reported in U.S. dollars), or on starting job level are dropped.

[1] This captures 75% of all the job titles in the respective job title subsample (on the sample from Appendix Table A14). Job titles are separated into jobs done at Google or at other companies. See Appendix Tables B6 and B7.

Analysis Table 10: Estimated Gender Differences in Starting Pay and Prior Pay, Individuals Hired as Software Engineers and Working as Software Engineers in Class Period

	(1)	(2)	(3)
Dependent Variable	Ln(Starting Pay)	Ln(Prior Pay)	Ln(Starting Pay) – Ln(Prior Pay)
Observations	With prior pay	With prior pay	With prior pay
Female shortfall	**-2.44%**	**-2.10%**	**-0.34%**
Std. deviations	**5.85**	**2.26**	**0.45**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 100 million	< 1 in 20	> 1 in 20
Pay difference implied by female % shortfall	**-$3,587**	**-$2,817**	
Observations	[redacted]	[redacted]	[redacted]
Year fixed effects	Yes	Yes	Yes
Location	Yes	Yes	Yes
Highest education level	Yes	Yes	Yes
Campus hire	Yes	Yes	Yes
Actual prior experience	Yes	Yes	Yes
Indicators for each of the 154 most common schools	Yes	Yes	Yes
Indicators for each of the 9 most common fields	Yes	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Tables 1, 3, and 4. Observations missing data on gender are excluded. Observations missing data on starting pay (or with starting pay reported by not in U.S. dollars), or on starting job level, are also dropped, as are observations missing data on prior pay. Prior pay is converted to December 2018 dollars. The top and bottom 2% of prior pay observations are trimmed from the sample with non-missing data on prior pay, gender, and starting job level (the sample from Analysis Table 5).

Analysis Table 11: Estimated Gender Differences in Starting Level, Individuals Hired as Software Engineers and Working as Software Engineers in Class Period, with Detailed Prior Experience Controls

	(1)	(2)
Prior experience controls:	Single years measure	By job title for 75% most common titles (recent and all others)
A. Estimated Gender Difference in Job Level (1-9) at Hire		
Female shortfall	**-0.1523**	**-0.0961**
Std. deviations	**14.01**	**8.99**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion
Actual prior experience	0.0928	
Std. error	(0.0009)	
Std. deviations	104.12	
Observations	[redacted]	[redacted]
B. Estimated Gender Difference in Probability of Hire at Level 4 or Higher vs. Level 3 or Lower (Marginal Effect)		
Female shortfall	**-0.0557**	**-0.0387**
Std. deviations	**8.01**	**6.25**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion
Actual prior experience	0.2491	
Std. error	(0.0045)	
Std. deviations	55.44	
Observations	[redacted]	[redacted]
Year fixed effects	Yes	Yes
Location	Yes	Yes
Highest education level	Yes	Yes
Campus hire	Yes	Yes
Experience not in controlled for job titles	No	Yes
Experience in each of the 484 most common titles (not most recent)	No	Yes
Experience in each of the 109 most common titles (most recent)	No	Yes
Indicators for each of the 154 most common schools	Yes	Yes
Indicators for each of the 9 most common fields	Yes	Yes

See notes to Analysis Tables 1, 4, 7, and 9. Observations missing data on gender, starting pay (or with starting pay not reported in U.S. dollars), or on starting job level are dropped. The sample in Panel B differs from the sample in Panel A because probit estimation discards observations with variables that perfectly predict the outcome of interest, and some of the experience indicators perfectly predict placement.

Analysis Table 12: Estimated Gender Differences in Starting Job Levels, Controlling for Prior Pay, Individuals Hired as Software Engineers and Working as Software Engineers in Class Period

	(1)	(2)
Prior Pay Information:	None	Indicators for "Target Job Level based on Prior Pay"
A. Estimated Gender Difference in Job Level (1-9) at Hire		
Female shortfall	**-0.1439**	**-0.0735**
Std. deviations	**5.29**	**3.14**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 million	< 1 in 100
R^2	0.53	0.65
Observations	[redacted]	[redacted]
B. Estimated Gender Difference in Probability of Hire at Level 4 or Higher vs. Level 3 or Lower (Marginal Effect)		
Female shortfall	**-0.054**	**-0.0278**
Std. deviations	**3.48**	**2.01**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1,000	< 1 in 20
Pseudo-R^2	0.45	0.55
Observations	[redacted]	[redacted]
Year fixed effects	Yes	Yes
Location	Yes	Yes
Highest education level	Yes	Yes
Campus hire	Yes	Yes
Actual prior experience	Yes	Yes
Indicator for target job level	No	Yes
Indicators for each of the 154 most common schools	Yes	Yes
Indicators for each of the 9 most common fields	Yes	Yes

See notes to Analysis Tables 1 and 4. The sample in Panel B differs from the sample in Panel A because probit estimation discards observations with variables that perfectly predict the outcome of interest, and some of the school indicators perfectly predict placement. "Target job level based on prior pay" is defined as the level for which prior pay is within the range [80% MRP, MRP]. If prior pay is less than the 80% MRP associated with the lowest level for a job family (level 1 for software engineers) then the target level is set to the lowest level (consistent with the leveling guidance in GOOG-ELLIS-00010907 suggesting that if a candidate falls between two levels, the lower level is recommended, and also consistent with the evidence in Figure 5). If prior pay is more than the MRP associated with highest level for a job family ([redacted]) then target level is set to the highest level. If prior pay falls within the ranges of two different levels, target level is set as the lower of the two levels. If prior pay falls between two ranges (a handful of cases), the target is set as the lower of the two levels. The top and bottom 2% of prior pay (based on the sample from Analysis Table 5, columns (2)-(4)) observations are dropped. Observations missing data on prior pay, gender, or MRP.

Analysis Table 13: Estimated Gender Differences in Starting Pay, Controlling for Prior Pay, Individuals Hired as Software Engineers and Working as Software Engineers in Class Period

	(1)	(2)
Prior Pay Information:	None	Indicators for "Target Job Level based on Prior Pay"
Female shortfall	**-2.63%**	**-0.79%**
Std. deviations	**4.59**	**1.88**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 100,000	> 1 in 20
Pay difference implied by female % shortfall	**-$3,904**	**-$1,177**
R^2	0.51	0.74
Observations	[redacted]	[redacted]
Year fixed effects	Yes	Yes
Location	Yes	Yes
Highest education level	Yes	Yes
Campus hire	Yes	Yes
Actual prior experience	Yes	Yes
Indicator for target job level	No	Yes
Indicators for each of the 154 most common schools	Yes	Yes
Indicators for each of the 9 most common fields	Yes	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Tables 1, 3, and 4. The top and bottom 2% of prior pay (based on the sample from Analysis Table 5, columns (2)-(4)) observations are dropped. Observations missing data on prior pay, gender, or MRP.

Analysis Table 14: Estimated Gender Differences in Job Level in Class Period, Individuals Hired as Software Engineers and Working as Software Engineers in Class Period, with Controls for Starting Job Level and Target job level based on prior pay

	(1)	(2)	(3)	(4)
Starting job level controls:	No	Starting job level	No	Target job level based on prior pay
Female shortfall	**-0.1534**	**-0.0618**	**-0.1508**	**-0.0783**
Std. deviations	**12.32**	**6.10**	**6.76**	**3.87**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100 million	< 1 in 1 billion	< 1 in 1,000
Observations	[redacted]	[redacted]	[redacted]	[redacted]
Year fixed effects	Yes	Yes	Yes	Yes
Tenure	Yes	Yes	Yes	Yes
Highest education level	Yes	Yes	Yes	Yes
Leave of absence	Yes	Yes	Yes	Yes
Actual prior experience	Yes	Yes	Yes	Yes
Location	Yes	Yes	Yes	Yes
Campus hire	Yes	Yes	Yes	Yes
Performance rating	Yes	Yes	Yes	Yes
Indicators for each of the 154 most common schools	Yes	Yes	Yes	Yes
Indicators for each of the 9 most common fields	Yes	Yes	Yes	Yes
Job level at hire	No	Yes	No	No
Target job level based on prior pay	No	No	No	Yes

See notes to Analysis Tables 1, 4, 7, 9, and 12. The starting location controls are included only in columns (2) and (4), where I control for starting job level. In columns (1) and (2), observations missing data on gender, starting job level, or performance rating are dropped. In columns (3) and (4), observations missing data on gender, performance rating, prior pay, or MRP are dropped. The top and bottom 2% of prior pay (based on the sample from Analysis Table 5, columns (2)-(4)) observations are dropped from columns (3) and (4).

Analysis Table 15: Estimated Gender Differences in Starting Pay, Controlling for Prior Pay, All Hires

	(1)	(2)
Prior Pay Information:	None	Indicators for “Target Job Level based on Prior Pay” interacted with Job Family at Hire
Female shortfall	**-2.98%**	**-0.90%**
Std. deviations	**6.84**	**2.97**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100
Pay difference implied by female % shortfall	**-$4,351**	**-$1,317**
R^2	0.66	0.84
Observations	[redacted]	[redacted]
Year fixed effects	Yes	Yes
Location	Yes	Yes
Highest education level	Yes	Yes
Campus hire	Yes	Yes
Actual prior experience	Yes	Yes
Indicator for each of the 242 most common schools	Yes	Yes
Indicator for each of the 73 most common fields	Yes	Yes
Job family	Yes	No
Target job level based on prior pay x Job family at hire	No	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Tables 1, 3, 4, and 12. Observations missing data on gender, prior pay, or MRP are dropped. The top and bottom 2% of prior pay (based on the sample from Analysis Table 5, columns (2)-(4)) observations are dropped.

Analysis Table 16: Estimated Gender Differences in Job Level in Class Period, Individuals Hired, All Hires, with Controls for Starting Job Level and Target job level based on prior pay, Working in Same Job Family in Class Period as Job Family of Hire

	(1)	(2)	(3)	(4)
Starting job level controls:	Starting job level		Indicators for "Target Job Level based on Prior Pay" interacted with Job Family at Hire	
Female shortfall	**-0.1325**	**-0.0460**	**-0.1104**	**-0.0493**
Std. deviations	**11.76**	**5.82**	**5.68**	**2.98**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100 million	< 1 in 10 million	< 1 in 100
Observations	[redacted]	[redacted]	[redacted]	[redacted]
Year fixed effects	Yes	Yes	Yes	Yes
Tenure	Yes	Yes	Yes	Yes
Highest education level	Yes	Yes	Yes	Yes
Leave of absence	Yes	Yes	Yes	Yes
Actual prior experience	Yes	Yes	Yes	Yes
Location	Yes	Yes	Yes	Yes
Campus hire	Yes	Yes	Yes	Yes
Performance rating	Yes	Yes	Yes	Yes
Indicator for each of the 242 most common schools	Yes	Yes	Yes	Yes
Indicator for each of the 73 most common fields	Yes	Yes	Yes	Yes
Job family	Yes	Yes	Yes	Yes
Job level at hire x Job family at hire	No	Yes	No	No
Target job level based on prior pay x Job family at hire	No	No	No	Yes

See notes to Analysis Tables 1, 4, 7, 9, 12, and 14. In columns (1) and (2), observations missing data on gender, starting job level, or performance rating are dropped. In columns (3) and (4), observations missing data on gender, performance rating, prior pay, or MRP are dropped. The top and bottom 2% of prior pay (based on the sample from Analysis Table 5, columns (2)-(4)) observations are dropped from columns (3) and (4).

Analysis Table 17: Estimated Gender Differences in Base Pay in Class Period, All Hires, with Controls for Starting Job Level and Target job level based on prior pay

	(1)	(2)	(3)	(4)
Starting job level controls:	No	Starting job level	No	Indicators for "Target Job Level based on Prior Pay" interacted with Job Family at Hired
Female shortfall	**-2.90%**	**-1.12%**	**-2.34%**	**-0.77%**
Std. deviations	**12.87**	**7.07**	**6.01**	**2.64**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 100 million	< 1 in 100
Pay difference implied by female % shortfall	**-$4,957**	**-$1,914**	**-$3,698**	**-$1,222**
Observations	[redacted]	[redacted]	[redacted]	[redacted]
Year fixed effects	Yes	Yes	Yes	Yes
Tenure	Yes	Yes	Yes	Yes
Highest education level	Yes	Yes	Yes	Yes
Leave of absence	Yes	Yes	Yes	Yes
Actual prior experience	Yes	Yes	Yes	Yes
Location	Yes	Yes	Yes	Yes
Campus hire	Yes	Yes	Yes	Yes
Performance rating	Yes	Yes	Yes	Yes
Indicator for each of the 242 most common schools	Yes	Yes	Yes	Yes
Indicator for each of the 73 most common fields	Yes	Yes	Yes	Yes
Job family	Yes	Yes	Yes	Yes
Job level at hire x Job family at hire	No	Yes	No	No
Target job level based on prior pay x Job family at hire	No	No	No	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Tables 1, 7, 9, 12, and 14. In columns (1) and (2), observations missing data on gender, starting job level, or performance rating are dropped. In columns (3) and (4), observations missing data on gender, performance rating, prior pay, or MRP are dropped. The top and bottom 2% of prior pay (based on the sample from Analysis Table 5, columns (2)-(4)) observations are dropped in columns (3) and (4).

Analysis Table 18: Estimated Gender Difference in Base Pay During Class Period, Named Plaintiffs

	(1)
Other female shortfall	-0.34%
Std. deviations	5.07
Pay difference implied by female % shortfall	-$598
Ms. Pease shortfall	-6.57%
Std. deviations	3.89
Pay difference implied by female % shortfall	**-$11,406**
Ms. Ellis shortfall	-0.75%
Std. deviations	0.91
Pay difference implied by female % shortfall	**-$1,307**
Ms. Lamar shortfall	-2.05%
Std. deviations	4.28
Pay difference implied by female % shortfall	**-$3,552**
Ms. Wisuri shortfall	-5.37%
Std. deviations	4.71
Pay difference implied by female % shortfall	**-$9,333**
Controls	
Year fixed effects	Yes
Tenure	Yes
Highest education level	Yes
Leave of absence	Yes
Actual prior experience	Yes
Location	Yes
Campus hire	Yes
Performance rating	Yes
Job code	Yes
Time in job level	Yes
Indicators for each of the 242 most common schools	Yes
Indicators for each of the 73 most common fields	Yes

The estimated female shortfalls are based on regressions for log of pay, and hence are approximate percentage differences. See notes to Analysis Tables 1 and 3. (The sample size is the same as in Analysis Table 3, Panel A.) Observations with missing data on gender or performance rating are dropped.

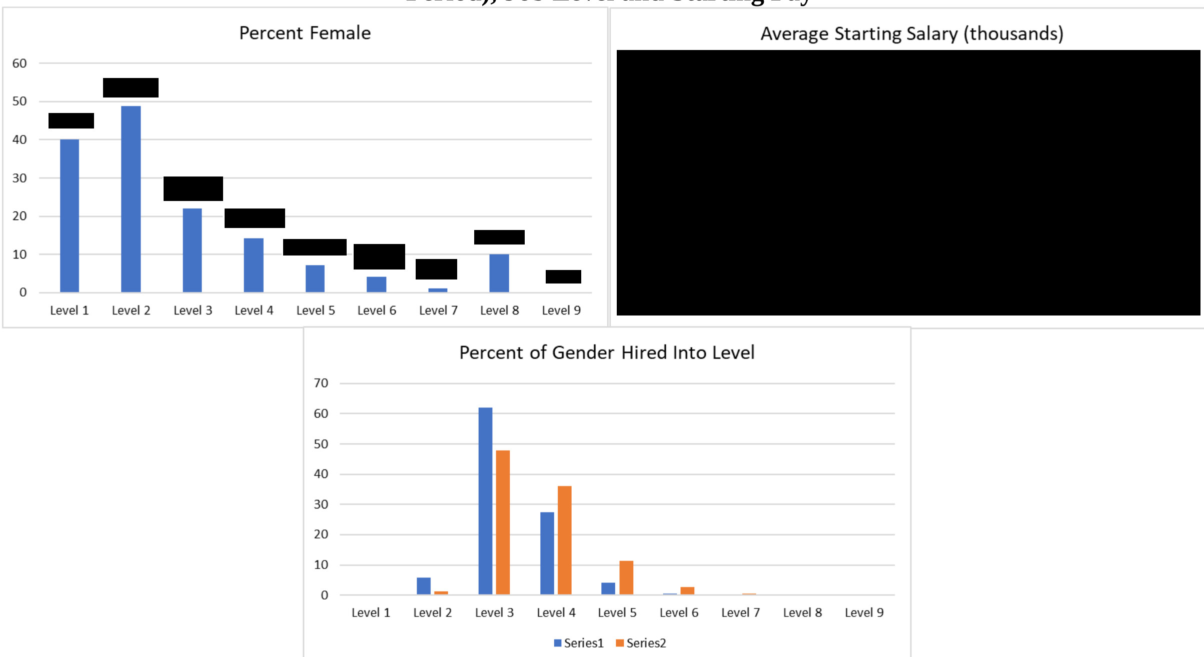
Figure 1: Log Starting Pay and Log Prior Pay Scatter Plot

[redacted]

Figure 2: Log Starting Pay and Log Prior Pay Scatter Plot, Removing

[redacted]

Figure 3: Hiring of Men and Women Software Engineers (Working as Software Engineers in Class Period), Job Level and Starting Pay



Note: Starting salary is in December 2018 dollars. [redacted]

Figure 4: Histogram for Starting Salary/MRP for those Hired as Software Engineers and Working as Software Engineers in Class Period

[redacted]

Figure 5: Difference between Actual Starting Job Level and Target job level based on prior pay, for those Hired as Software Engineers and Working as Software Engineers in Class Period

[redacted]

Figure 6A: Log Starting Pay and Log Prior Pay Scatter Plot, Hired as Software Engineers and Working as Software Engineers in Class

[redacted]

Figure 6B: Log Starting Pay and Log Prior Pay Scatter Plot, Hired as Software Engineers and Working as Software Engineers in Class Period, Highlighting Differences Based on Prior Pay vs. 80% of MRP

[redacted]

Figure 7: Histogram for Starting Salary/MRP for All Hires

[redacted]

Figure 8: Difference between Actual Starting Job Level and Target job level based on prior pay, for All Hires

[redacted]

Figure 9: Hiring and Starting Salary for Program Manager and Technical Program Manager Job Families

Percent of Gender Hired into Position	Starting Salary (thousands)
[redacted]	

Note: Starting salary is in December 2018 dollars. [redacted]

Estimated (Probit) Gender Difference in Hiring into Technical Program Manager (Higher) vs. Program Managers (Lower) Job Family, With and Without Detailed Prior Experience Controls, and Controlling for Application

	(1)	(2)	(3)
Female shortfall (marginal effect of being female on the probability of being hired into the higher position)	**-0.131**	**-0.126**	**-0.091**
Std. deviations	**8.79**	**8.63**	**6.80**
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	[redacted]	[redacted]	[redacted]
Year fixed effects	Yes	Yes	Yes
Location	Yes	Yes	Yes
Highest education level	Yes	Yes	Yes
Campus hire	Yes	Yes	Yes
Has ever applied for a TPM position	No	No	Yes
Has ever applied for a PM position	No	No	Yes
Indicator for each of the 72 most common schools	Yes	Yes	Yes
Indicator for each of the 33 most common fields	Yes	Yes	Yes
Experience not in controlled for job titles	No	Yes	Yes
Experience in each of the 64 most common titles (not most recent)[1]	No	Yes	Yes
Experience in each of the 28 most common titles (most recent)[1]	No	Yes	Yes

See notes to Analysis Tables 1 and 4. For this analysis, we use the most common school names capturing 50% of all school names, and the most common fields capturing 50% of all fields. Observations with missing data on gender are dropped. Because probit estimation discards observations with variables that perfectly predict the outcome of interest, 220 observations (out of [redacted]) for which variables perfectly predict job family are dropped. The [redacted] observations exceeds the number of observations in Figure 9 because Figure 9 drops observations missing data on starting salary.

[1] This captures 25% of all the job titles in the respective job title subsample.

[redacted]

Appendix A: Data Files, Variables, and Descriptive Statistics for Analysis Samples

Appendix Table A1. Descriptions/Definitions of all Variables from Google Data Used in Analysis

Variable	Description	Raw File of Data
Female	Female is a dummy for being female.	HR_Profile_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Campus hire	Campus hire is a dummy equal to 1 if variable *conversiontype* equals to campus, and 0 otherwise. Campus hire intends to be an indicator for new graduates but it is imperfect measure for new graduates.	HR_Profile_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Actual prior experience	Actual experience is calculated as the sum of duration of employment spells prior to joining Google, which comes from the variables "workexp_tenure_1" through "work_exp_tenure_6." We truncate actual experience to be no higher than an individual's age – 18. Measured in years.	HR_Profile_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Highest education level	Highest education level is a set of dummies for the highest education level including a dummy for missing or other education level.	HR_Education_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Job level dummy variables	Job level are dummies for Job levels.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Job family	Job family are dummies for Job families.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Job level	Job level are dummies for Job levels.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Job code	Job code are dummies for Job codes.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Unified rollup	Unified rollup are dummies for Unified rollup level 2.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Department	Department are dummies for Departments.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Cost Center	Cost Center are dummies for Cost Center.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Is manager	Is the average amount of time spent as a manager in the preceding calendar year. Being a manager is defined by the variable "is_emp_manager."	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Performance rating	Performance rating is the average of non-missing performance ratings in a year preceding snapshot date.	HR_Performance_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Tenure	Tenure is calculated as the difference between snapshot_date and hire_date, it is measured in years.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Time in job title	Time in job title is calculated as the difference	HR_PHM_CONFIDENTIAL -

Variable	Description	Raw File of Data
	between snapshot_date and *job_code_start_date*.	SUBJECT TO PROTECTIVE ORDER
Time in job level	Time in job level is calculated as the difference between snapshot_date and *start_date_job_level*.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Leave of absence	Leave of absence measures percent of calendar year preceding snapshot that the employee was ineligible for bonus due to unpaid leave or unemployment.	HR_Leaves_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER HR_Time Off_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Location	Location is a set of dummies for location, such as [redacted], etc.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Base pay	Base pay is the *calculated_annual_salary* variable at the time of the snapshot. It is measured in December 2018 dollars (based on the Bureau of Labor Statistics CPI-U.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Standard bonus	Bonus pay is the sum of all standard bonuses given to an individual the calendar year preceding the snapshot date. Bonuses are measured in December 2018 dollars (based on the Bureau of Labor Statistics CPI-U. Standard bonuses are bonuses titled "OTP_ANNUAL_GROSS."	HR_OTP_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Equity	Equity is the sum of the value of all the shares of equity given to an individual the calendar year preceding the snapshot date. The value of equity is taken by multiplying the number of shares granted by the value of the stock on the day the stock was granted. Equity is measured in December 2018 dollars (based on the Bureau of Labor Statistics CPI-U.	HR_Equity_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Starting salary	Starting salary is the calculated annual salary for the PHM observation with starting date equal to hire date. Starting salary is measured in December 2018 dollars (based on the Bureau of Labor Statistics CPI-U.	HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER
Prior pay	Prior pay is the *prior compensation* variable associated with a certain individual/hire date. Prior is measured in December 2018 dollars (based on the Bureau of Labor Statistics CPI-U).	Applicant_OWF Offers_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

Appendix Table A2. Job Description Variables

Variable	Description[1]	Number of Unique Values	Notes
Job title	"Title associated with a specific job code" where a job code is defined as the "Numeric value associated with a specific job."	[redacted]	
Job family	"Grouping of related job families, which are groupings of job title"	[redacted]	
Job level	"Ranges from 01-09, with 01 being the lowest level and 09 being the highest non-executive level"	9	Job level and Job Family combinations almost always uniquely identify Job Title.
Cost center	"Name used by Finance to describe a cost center."	[redacted]	Unique Job Titles can belong to several different Cost Centers. On average, each Job Title belongs to approximately [redacted] different Cost Centers.
Unified rollup level 2	"Grouping of cost centers at the Product Area Group/Subfunction level as defined by go/unifiedrollup."	[redacted]	
Department	"Grouping of cost centers based on Workday data. People are assigned a Department based on their cost center number. Known as \Cost Center Hierarchy\ in Workday."	[redacted]	

[1] Descriptions come from the file "Merit Snapshots Data Dictionary_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER."

[2] All combinations of Job level, Job Family, and Job Title can be found in Appendix Table A1.

I use two data sets for my analysis. The first, “Master_Snapshot_Dataset.dta,” is a snapshot we created of individuals on our six snapshot dates – January 1st of 2014-2019. All variables are reported as they were on the snapshot date except for Bonus, Equity, Rating and Leave of absence, which are created using the data available the previous calendar year. The next table provides descriptive statistics for this file (excluding the large sets of dummy variables).

Appendix Table A3: Descriptive Statistics for “Master_Snapshot Dataset”

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Note: We keep the snapshots of individuals in the class, retaining individuals with covered positions, working in California, and working for Google LLC. Highest degree obtained is created by taking the education observation with the highest degree type for an individual, among the non-missing data. If no highest degree is available the individual is coded as “Missing degree type.” In this and the following tables I do not show the descriptive statistics for the second location variable (18 office locations). The distribution of observations across these sites, for the main pay analysis I do using the snapshot data (the sample in Appendix Table A5), is as follows:

Location Site	%	Location Site	%
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]

The second data set, “Master_Hiring_Dataset.dta,” compiles the information available on an individual in the observation that begin on their hiring date. The next table provides descriptive statistics for this file.

Appendix Table A4: Descriptive Statistics for “Master_Hiring Dataset”

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

[redacted] not in USD are changed to missing.

The tables that follow provide descriptive statistics for subsamples from the datasets described above that are used in my analyses.

Appendix Table A5: Snapshot Analysis Subsample Dropping Individuals Missing Data on Performance rating or Gender

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A6: Snapshot Analysis Subsample Dropping Individuals Missing Data on Performance rating, Gender, Department, or Unified Rollup

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A7: Snapshot Analysis Subsample, Dropping Individuals Missing Data on Starting Salary in U.S. Dollars, Starting Level, Gender, or Performance Rating

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

[redacted]

Appendix Table A8: Hiring Analysis Subsample, Dropping Individuals Missing Data on Starting Salary in U.S. Dollars, Starting Level, or Gender

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

[redacted]

Appendix Table A9: Hiring Analysis Subsample, Dropping Individuals with Missing Data on Prior Pay, Gender, Starting Salary in U.S. Dollars, or Starting Job Level

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

I trim the top and bottom 2% of prior pay observations from the sample with non-missing data on prior pay, gender, and starting job level.

Appendix Table A10: Snapshot Analysis Subsample for Individuals Working as Software Engineers in Class Period, Dropping Individuals with Missing Data on Performance rating or Gender

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A11: Snapshot Analysis Subsample for Individuals Working as Software Engineers in Class Period and Hired as Software Engineers, Dropping Individuals with Missing Data on Performance rating, Gender, Starting Salary in U.S. Dollars, or Starting Level

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A12: Hiring Analysis Subsample for Individuals Working as Software Engineers in Class Period and Hired as Software Engineers, Dropping Individuals with Missing Data on Gender, Starting Salary in U.S. Dollars, or Starting Level

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A13: Hiring Analysis Subsample, Keeping Individuals Working as Software Engineers in Class Period and Hired as Software Engineers, Dropping Individuals with Missing Data on Prior Pay, Starting Salary in U.S. Dollars, Starting Job Level, or Gender

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

I trim the top and bottom 2% of prior pay observations as calculated for the Appendix Table A9 Sample.

Appendix Table A14: Hiring Analysis Subsample for Individuals Working as Software Engineers in Class Period and Hired as Software Engineers, Dropping Individuals with Missing Data on Gender, Starting Salary in U.S. Dollars, or Starting Job Level

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A15: Hiring Analysis Subsample for Individuals Working as Software Engineers in the Class Period and Hired as Software Engineers, Dropping Individuals with Missing Data on Gender or Starting Job Level, and Dropping Individuals with Perfectly Predictive Prior Experience Variables (See Notes to Analysis Table 11)

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A16: Hiring Analysis Subsample, Working as Software Engineers in Class Period and Hired as Software Engineers, Dropping Individuals with Missing Data on Prior Pay or Gender or MRP

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

I trim the top and bottom 2% of prior pay observations from the sample with non-missing data on prior pay, gender, and starting job level. I trim the top and bottom 2% of prior pay observations as calculated for the Appendix Table A9 sample.

Appendix Table A17: Snapshot Analysis Subsample, for Individuals Working as Software Engineers in the Class Period and Hired as Software Engineers, Dropping Individuals with Missing Data on Gender, Starting Job Level, or Performance rating

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A18: Snapshot Analysis Subsample for Individuals Working as Software Engineers in the Class Period and Hired as Software Engineers, Dropping Individuals with Missing Data on Gender, Performance rating, Prior Pay, MRP

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

I trim the top and bottom 2% of prior pay observations as calculated for the Appendix Table A9 sample.

Appendix Table A19: Hiring Analysis Subsample, Dropping Individuals Missing Data on Prior Pay, Gender, or MRP

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

I trim the top and bottom 2% of prior pay observations as calculated on the Appendix Table A9 sample.

Appendix Table A20: Snapshot Analysis Subsample for Individuals Working in the Same Job Family in the Class Period as the Job Family at Hire, Dropping Individuals with Missing Data on Gender, Starting Job Level, or Performance rating

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A21: Snapshot Analysis Subsample for Individuals Working in the Same Job Family in the Class Period as the Job Family at Hire, Dropping Individuals with Missing Data on Gender, Performance rating, Prior Pay, MRP

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

I trim the top and bottom 2% of prior pay observations as calculated for the Appendix Table A9 sample.

Appendix Table A22: Snapshot Analysis Subsample, Dropping Individuals with Missing Gender, Performance rating, or Starting Job Level

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A23: Snapshot Analysis Subsample, Dropping Individuals with Missing Gender, Performance rating, or Prior Pay, or MRP

Variable	Count	Mean	Std Dev	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

I trim the top and bottom 2% of prior pay observations as calculated for the Appendix Table A9 sample.

Appendix Table A24: Hiring Analysis Subsample: Keeping Individuals Hired as Product Managers or Technical Product Managers, Dropping Individuals with Missing Starting Salary, or Gender

Variable	Count	Average	Std. Dev.	Min	Max
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]					
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Appendix Table A25: Samples for Different Analysis

Tables/Figures in Main Report	Appendix A Tables
Analysis Table 1	Appendix Table A5
Analysis Table 2	Cols 1-4,7-8: Appendix Table A5 Col 5-6: Appendix Table A6
Analysis Table 3	Panel A: Appendix Table A5 Panels B-C: Appendix Table A7
Analysis Table 4	Cols 1-2: Appendix Table A7 Cols 3-4: Appendix Table A8
Analysis Table 5	Col 1: Appendix Table A8 Cols 2-4: Appendix Table A9
Analysis Table 6	Appendix Table A8
Analysis Table 7	Appendix Table A10
Analysis Table 8	Col 1: Appendix Table A11 Cols 2-3: Appendix Table A12
Analysis Table 9	Appendix Table A12
Analysis Table 10	Appendix Table A13
Analysis Table 11	Panel A: Appendix Table A14 Panel B: Appendix Table A15
Analysis Table 12	Appendix Table A16
Analysis Table 13	Appendix Table A16
Analysis Table 14	Cols 1-2: Appendix Table A17 Cols 3-4: Appendix Table A18
Analysis Table 15	Appendix Table A19
Analysis Table 16	Cols 1-2: Appendix Table A20 Cols 3-4: Appendix Table A21
Analysis Table 17	Cols 1-2: Appendix Table A22 Cols 3-4: Appendix Table A23
Analysis Table 18	Appendix Table A5
Figure 1	N.A.
Figure 2	Appendix Table A9
Figure 3	Appendix Table A12
Figure 4	Appendix Table A16
Figure 5	Appendix Table A16
Figure 6A and 6B	Appendix Table A16
Figure 7	Appendix Table A19
Figure 8	Appendix Table A19
Figure 9	Appendix Table A24

Appendix B: Supplemental tables with Information on Job Titles at Google, Job Titles at Prior Jobs, and Education

Appendix Table B1: Job Families, Levels, and Titles

Job Family	Job Level	Job Title
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]
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Job Family	Job Level	Job Title
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Appendix Table B2: Most Common School Names

School Names	Percent of All Females from School	Percent of All Males from School	Percent of All Individuals from School
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School Names	Percent of All Females from School	Percent of All Males from School	Percent of All Individuals from School
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School Names	Percent of All Females from School	Percent of All Males from School	Percent of All Individuals from School
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School Names	Percent of All Females from School	Percent of All Males from School	Percent of All Individuals from School
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School Names	Percent of All Females from School	Percent of All Males from School	Percent of All Individuals from School
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[redacted]	[redacted]	[redacted]	[redacted]
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[redacted]	[redacted]	[redacted]	[redacted]
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[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]

Note: This table contains the most common school names capturing 75% of all school names. We calculate this by ranking the school names of the most recent obtained degree by frequency.

Appendix Table B3: Most Common Fields

Field Name	Percent of All Females in Field	Percent of All Males in Field	Percent of All Individuals in Field
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]

Field Name	Percent of All Females in Field	Percent of All Males in Field	Percent of All Individuals in Field
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]

Note: This table contains the most common fields capturing 75% of all fields. We calculate this by ranking the fields of the most recent obtained degree by frequency.

Appendix Tables B4: Most Common School Names, Individuals Hired as Software Engineers

School Name	Percent of All Females from School	Percent of All Males from School	Percent of All Individuals from School
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]

School Name	Percent of All Females from School	Percent of All Males from School	Percent of All Individuals from School
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
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[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
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[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
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[redacted]	[redacted]	[redacted]	[redacted]
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[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]

School Name	Percent of All Females from School	Percent of All Males from School	Percent of All Individuals from School
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
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[redacted]	[redacted]	[redacted]	[redacted]
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[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
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[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]

Note: This table contains the most common school names capturing 75% of all school names. I calculate this by ranking the school names of the most recent obtained degree by frequency. Note that school entries are sometimes very similar. However, in general I did not try to combine entries that appear very similar, since small difference can be meaningful. For example, one person might list “Harvard” and another “Harvard Business School,” and since the former may or may not refer to business school there is no reason to constrain them to have the same effect. Also, there are other schools that may be the same and have quite different names (e.g., “Cal Poly” vs. “California Polytechnic State University”), and there is no clear way to detect these cases. I only harmonized entries when they differed only by what are called, in computational linguistics, “stopping word”: e.g., “Ohio State University” and “The Ohio State University.” Even if two entries appear to differ but do not in fact differ, no bias in the estimates is introduced by allowing for two separate effects.

Appendix Tables B5: Most Common Fields, Individuals Hires as Software Engineers

Field Name	Percent of All Females in Field	Percent of All Males in Field	Percent of All Individuals in Field
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]

Note: This table contains the most common field names capturing 75% of all field names. We calculate this by ranking the field names of the most recent obtained degree by frequency.

Appendix Tables B6: Most Common Titles for Jobs Not Including the Most Recent, Split by Google and Other Companies, Individuals Hired as Software Engineers

Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]		[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]		[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]		[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]		[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]		[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]		[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]		[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
[redacted]	[redacted]	[redacted]		[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]		[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Note: This table contains the most common titles capturing 75% of all job titles, 25%, and 50% are demarcated by the bold lines. We calculate this by ranking all job titles (dropping the most recent ones) by frequency. Once we have the most frequent job titles we split each job title into experience gotten at Google or at Other companies.

Appendix Tables B7: Most Common Titles for Most Recent Job, Splitting up Companies into Google and Other, Individuals Hired as Software Engineers

Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
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Job Title	Company	Percent of Women	Average Tenure of Women	Percent of Men	Average Tenure of Men	Percent of All Job Titles
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Note: This table contains the most common titles capturing 75% of all job titles. 25%, and 50% are demarcated by the bold lines. We calculate this by ranking the most recent job titles by frequency. Once we have the most frequent job titles we split each job title into experience gotten at Google or at Other companies.

Appendix C: Materials Considered

Data files

HR_Profile_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

HR_Education_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

HR_PHM_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

HR_Performance_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

HR_Leaves_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

HR_Time Off_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

HR_OTP_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

HR_Equity_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

Applicant_OWF Offers_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

Applicant_Candidate Employment_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

Applicant_Candidate Education_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

Other

Merit Snapshots Data Dictionary_CONFIDENTIAL - SUBJECT TO PROTECTIVE ORDER

PROD050

Hearing Transcripts

Transcript of Testimony from Friday, April 7, 2017 hearing in OFCCP v. Google. Inc., No. 2017 OFC 08004 (Before Hon. Steven B. Berlin)

Transcript of Testimony from Friday, May 26, 2017 hearing in OFCCP v. Google. Inc., No. 2017 OFC 08004 (Before Hon. Steven B. Berlin)

PMQ Transcripts and Exhibits

Bucich Deposition Transcript (10/11/2018) with Exhibits 500-504

Williams Deposition Transcript (1/23/2019 with Exhibits 505-514

Wagner Deposition Transcript (1/30/2019) with Exhibits 515-536

Tietbohl Deposition Transcripts (2/5/2019) with Exhibits 537-565

Ong Deposition Transcript (2/7/2019) with Exhibits 566-578

Wolfe Deposition Transcript (2/14/2019) with Exhibit 579

Tietbohl Deposition Transcripts (7/31/2019) with Exhibit 596

Gangadharan Deposition Transcript (7/17/2019) with Exhibits 580-582

Rowe Deposition Transcript (8/7/2019) with Exhibits 597-609

<u>Plaintiff Transcripts and Exhibits</u>

Pease Deposition Transcript (9/27/2018) with Exhibits 1-11

Wisuri Deposition Transcript (10/5/2018) with Exhibits 12-25

Ellis Deposition Transcript (10/8/2018) with Exhibits 26-42

Lamar Deposition Transcript (11/12/2018) with Exhibits 43-55

<u>Google Documents</u>

Google-Ellis-00001681 – [redacted]

Google-Ellis-00001691 – [redacted]

Google-Ellis-00003583 – [redacted]

Google-Ellis-00004275 – [redacted]

Google-Ellis-00004286 – [redacted]

Google-Ellis-00004293 – [redacted]

Google-Ellis-00004301 – [redacted]

Google-Ellis-00004303 – [redacted]

Google-Ellis-00004305 – [redacted]

Google-Ellis-00004311 – [redacted]

Google-Ellis-00004329 – [redacted]

[redacted]

Google-Ellis-00004337 – [redacted]

Google-Ellis-00004349 – [redacted]

Google-Ellis-00004363 – [redacted]

Google-Ellis-00004379 – [redacted]

Google-Ellis-00004389 – [redacted]

Google-Ellis-00004397 – [redacted]

Google-Ellis-00004403 – [redacted]

Google-Ellis-00004440 – [redacted]

Google-Ellis-00004442 – [redacted]

Google-Ellis-00004974 – [redacted]

Google-Ellis-00004977 – [redacted]

Google-Ellis-00004980 – [redacted]

Google-Ellis-00008310 – [redacted]

Google-Ellis-00008315 – [redacted]

Google-Ellis-00010907

Google-Ellis-00017716 – [redacted]

Labor economics literature, articles, and texts, including the below:

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.

Bayard, Kimberly, Judith Hellerstein, David Neumark, and Kenneth Troske. 2003. "New Evidence on Sex Segregation and Sex Differences in Wages from Matched Employee-Employer Data." Vol. 21, pp. 887-922.

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

Becker, Gary S. 1994. Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education. Chicago: University of Chicago Press.

Groshen, Erica. 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,* Vol. 26, pp. 457-472.

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*, Vol. 17, pp. 409-446.

Mincer, Jacob. 1974. <u>Schooling, Experience, and Earnings</u>. Cambridge: National Bureau of Economic Research, Inc.

Neumark, David, and Wendy Stock. 2006. "The Labor Market Effects of Sex and Race Discrimination Laws." *Economic Inquiry,* Vol. 44, pp. 385-419

Neumark, David. 2012. "Detecting Evidence of Discrimination in Audit and Correspondence Studies." *Journal of Human Resources,* Vol. 47, pp. 1128-57

Studenmund, A.H.. 2006. <u>Using Econometrics: A Practical Guide, Fifth Edition</u>, Pearson Education Inc., p. 14.

Appendix D: Publications from Last 10 Years

PEER-REVIEWED PUBLICATIONS:

Neumark, David, and Timothy Young, "Heterogeneous Effects of State Enterprise Zone Programs in the Shorter Run and Longer Run," forthcoming in Economic Development Quarterly.

Neumark, David, and Peter Shirley, "The Long-Run Effects of the Earned Income Tax Credit on Women's Earnings," forthcoming in Labour Economics.

Hellerstein, Judith K., and David Neumark, 2020, "Social Capital, Networks, and Economic Wellbeing," Future of Children, pp. 127-152.

Neumark, David, Brian Asquith, and Brittany Bass, 2020, "Longer-Run Effects of Anti-Poverty Policies on Disadvantaged Neighborhoods," Contemporary Economic Policy, pp. 409-434.

Neumark, David, and Maysen Yen, 2020, "Relative Sizes of Age Cohorts and Labor Force Participation of Older Workers," Demography, pp. 1-31.

Hellerstein, Judith K., Mark Kutzbach, and David Neumark, 2019, "Labor Market Networks and Recovery from Mass Layoffs: Evidence from the Great Recession Period," Journal of Urban Economics, Vol. 113.

Neumark, David, and Timothy Young, 2019, "Enterprise Zones and Poverty: Resolving Conflicting Evidence," Regional Science and Urban Economics, Vol. 78.

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Appendix E: Expert Witness Work in Last Four 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.

Jewett et al. v. Oracle America, Inc., 17-CIV-02669, Superior Court of the State of California

Served as plaintiffs' expert witness to address statistical evidence on gender discrimination in pay. Deposed.

EEOC v. R&L Carriers, Inc. and R&L Carriers Shared Services, LLC, No. 1:17-cv-00515-SJD, U.S. District Court, Southern District of Ohio

Served as plaintiff's expert witness to address statistical evidence on gender discrimination in hiring. Deposed.