Expert Report of
David Neumark
in the matter of
Rasmussen et al. v. The Walt Disney Co. et al.
June 2023

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 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

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[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." *Journal of Labor Economics*, Vol. 21, pp. 887-922.

(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 and labor economics expert to evaluate claims of gender discrimination in pay at the Walt Disney Company.[3] Specifically, I have been asked to examine whether the data are consistent with gender discrimination in pay at Disney during the Class Period, and to investigate the policies that lead to gender differences in pay at Disney 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 $575 per hour.

5. This analysis is based on my current understanding of the data provided by Disney. The data files are listed and described in Appendix A.

6. It is possible that I will learn more about the Disney data, company procedures, and other matters in the course of this case, which could lead to changes in my analysis and findings.

7. Materials that I considered are listed in Appendix B.

8. Appendix C of my report provides an abridged CV listing my publications from the last 10 years. Appendix D of my report details my expert witness work in the last 4 years.

Questions I was asked to consider and summary answers

9. I was asked to consider the following questions:

10. How many women are in the class? In particular, how many women did Disney[4] employ in California in Covered Positions from April 1, 2015 through December 31, 2022?[5]

   a. There are [redacted] female employees in full-time, non-union positions ("Covered

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[3] In particular, my analysis concerns workers in jobs held in the class period by full-time, nonunion employees working in California, in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, and E0, E1, and E1X (excluding Vice Presidents).

[4] The data I was given and asked to analyze cover all full-time, nonunion Disney employees in California, with the exclusion of ESPN, Hulu, Pixar, National Geographic, and 21st Century Fox. There are some other exclusions noted below. I will use the shorthand "Disney" to refer to the portions of the Walt Disney Company and associated entities for which I analyzed data.

[5] While the class definition extends past December 31, 2022, that is the last date for which data has been produced. Covered Positions include full-time, nonunion employees working in California, in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, and E0, E1, and E1X (excluding Vice Presidents).

positions"), appearing on at least one snapshot date (January 15) from 2015 to 2022.[6]

b. There are [redacted] female employees in Covered positions who are assigned a job family and appear on at least one snapshot date (January 15) from 2015 to 2022.

11. Did women at Disney receive lower annual salaries than similarly-situated men? If there was a difference, was it statistically significant? Do other potentially non-discriminatory factors fully account for the female pay penalty?

a. In the Class Period, women at Disney were paid less than similarly-situated men. In my preferred model, I estimate a female pay penalty of 2.01%.

b. This is a difference of 9.2 standard deviations, implying that the estimated difference is statistically significant at the 1% percent level (and indeed at a much lower level). Equivalently, the odds that we would find an estimated gender gap this large in the data, if the true effect of gender on pay was zero (i.e., there was no pay discrimination), is less than 1 in 1 billion.

c. The estimated female pay penalty is not explained by potentially non-discriminatory factors, including potential experience, prior experience, tenure at Disney, education, or performance. Indeed, if anything, accounting for some of these factors would increase the estimated female pay penalty.

12. If there are salary differences in the Class Period between similarly-situated women and men, are they attributable to gender differences in starting pay for similarly-situated women and men?

a. When they start, women at Disney were paid less than similarly-situated men. In my preferred model, I estimate a female pay penalty of 2.81%. This estimate is computed for the Class Period, when I have starting pay information for a sizable share of Disney employees. It is likely that this starting female pay penalty helps account for the female pay penalty in the Class Period, given that pay increases stemming from annual reviews are based on a percentage of salary.

b. This is a difference of 2.9 standard deviations, implying that the estimated difference is statistically significant at the 1% percent level. Equivalently, the odds that we

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[6] The number of class members could ultimately end up slightly larger, if women who started working after one snapshot date and left before the next one are included, and if data are produced beyond 2022.

would find an estimated gender gap this large in the data, if the true effect of gender on pay was zero (i.e., there was no pay discrimination), is less than 1 in 100.

c. The estimated female pay penalty is not explained by potentially non-discriminatory factors, including potential experience, prior experience, education, or the relevance of prior job experience. Based on additional data on prior experience and education available for a subset of the sample, I find evidence that accounting for these factors would increase the estimated female pay penalty, and I find that differences in the relevance of prior experience also cannot account for the female penalty in starting pay.

13. If there is a gender difference in starting pay for similarly-situated women and men, is that evidence consistent with Disney basing starting pay in part on prior pay?

a. After October 2017, Disney stopped asking job candidates about prior pay. The estimated female starting pay penalty for 2015 through October 2017 is 4.36% (2.7 standard deviations). The estimated female starting pay penalty for November 2017 and after is 1.3% (1.2 standard deviations). The substantial decline in the estimated penalty is consistent with prior pay having affected starting pay through October 2017. (For the subset of data with information on prior experience and education, and using that information, the female pay penalty after October 2017 – when Disney still asked about salary expectations – is 1.8% and significant at the 10% level (1.65 standard deviations).)

14. Did women at Disney receive lower annual salaries than comparable men doing substantially similar work? If there was a difference, was it statistically significant? Do other potentially non-discriminatory factors fully account for the female pay penalty?

a. In the Class Period, women at Disney were paid less than comparable men doing substantially similar work. In my preferred model, I estimate a female pay penalty of 0.81%.

b. This is a difference of 3.8 standard deviations, implying that the estimated difference is statistically significant at the 1% percent level (and indeed a much lower level). Equivalently, the odds that we would find an estimated gender gap this large in the data, if the true effect of gender on pay was zero (i.e., there was no pay discrimination), is less than 1 in 1,000.

c. The estimated female pay penalty is not explained by potentially non-discriminatory factors, including potential experience, prior experience, tenure at Disney, education, or performance. Indeed, if anything, accounting for some of these factors would increase the estimated female pay penalty.

Company structure and decision making

Organization

15. Disney is organized into segments, some of which have changed over time.[7] In 2015, at the start of the relevant time period for this case, there were seven segments: [redacted].[8] Although listed separately in many documents, [redacted] and [redacted] fell under the segment called [redacted] and that is how their segment is listed in the data produced by Defendant (see Table 1 below). Since ESPN employees are not part of the proposed class or dataset, for our purposes the [redacted] segment is effectively [redacted].[9] Corporate and Studio segments have persisted throughout the Class Period. The others have consolidated as follows:

16. In 2017, Disney combined DI with DCP to form DCPI, Disney Consumer Products & Interactive. In 2018, Disney further combined WDPR with DCPI to form Disney Parks, Experiences & Consumer Products, "DPECP" which is typically referred to as "DPEP." In 2021, Disney split its Media segment, and moved DATG to the newly created Disney General Entertainment ("DGE"), and made ESPN a stand-alone segment.[10]

17. In September 2017, Disney acquired a company called BAMTECH, and combined it with other portions of Disney's business that deliver content to consumers (streaming services and

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[7] See *Defendant The Walt Disney Company's Supplemental and Amended Response to Plaintiffs' Special Interrogatories, Set Two, No. 19*, explaining Disney is organized into segments, and identifying those active in 2020; *Defendant The Walt Disney Company's Supplemental Objections and Responses to Plaintiffs' Special Interrogatories, Set Six, No. 53*, identifying the segments in place in 2022, and briefly describing the portion of Disney's business encompassed by each segment.

[8] See *Ex. 697, DISNEY-000005790* at 5794; see also Burnley Dep. at 99-103.

[9] Plaintiffs excluded ESPN from their proposed class (and because the class is limited to those working in California, the International segment is also almost entirely irrelevant).

[10] This is referenced in the Burnley Dep. at 99-103, although the testimony about timing was vague. We confirmed this (and the date) in the data provided.

operation of TV stations, separated out from creating content for those services and stations) to form the Direct to Consumer & International Segment ("DTCI") in March 2018. Beginning in October 2020, Disney moved parts around again, and most of DTCI became Disney Media & Entertainment Distribution ("DMED").[11]

18. In December 2022, Disney re-organized again, and aside from the Corporate entity, claims only three segments: Disney Entertainment (encompassing what had been DGE and Studio segments); DPEP; and ESPN. Pate Dep. at 19:23 – 21:1, 22:21-24.

19. Table 1 indicates by year which segments were in operation (except for ESPN as it is not part of this case), and further indicates in a transition year where the operations previously encompassed by a given segment moved to.[12]

20. Within each segment there are divisions that may be referred to as a "business" or "line of business," but all report up to the Chair for the Segment. [redacted][13] [redacted].[14] [redacted][15]

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[11] See *Ex. 590*, *DISNEY-000031562*; *Ex. 622*, *DISNEY-000027349* at 27349-50; Anderson Dep. at 25:1-16; Pate Dep. at 18:15-19, 23:18-21; Olsgaard Dep. at 41:8-23.

[12] Table 1 is based on the appearance of observations by segment and year in the Class Period analysis data set I construct from the SAP data.

[13] *Defendant The Walt Disney Company's Supplemental and Amended Response to Plaintiffs' Special Interrogatories, Set Two, No. 19* at pp. 6-7.

[14] *Defendant's Supplemental and Amended Response to Plaintiffs' Special Interrogatories, Set Two, No. 20* at p. 8-9.

[15] *Defendant's Supplemental and Amended Response to Plaintiffs' Special Interrogatories, Set Two, No. 20* at p. 9.

Table 1: Disney Segments by Year[16]

[redacted]

Source: SAP analysis data set.
Note: Segment classifications reflect segments and years in the snapshots used for the pay analysis.

21. Disney refers to its [redacted][17] The data I analyzed showed only [redacted] companies listed. More significantly, Disney's designated witnesses testified that the company listed as employing a given individual in a segment did not impact the policies they were subjected to, the identity of relevant decision-makers, or anything else about how human resources administered their responsibilities. Indeed, the witnesses rarely knew which company might be listed as the employing entity for any given person in their segment and were not always certain about which company had been listed as their own employer during points of their career with Disney.[18]

22. Finally, the data also includes references to "organizational units," "cost centers," and "personnel areas." However, neither organizational unit nor cost center was used by human

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[16] Note that the data also include two small segments each year: [redacted] Neither was mentioned in Disney's interrogatory responses which identify segments. (See, e.g., *Defendant The Walt Disney Company's Supplemental Objections and Responses to Plaintiff Rasmussen's Special Interrogatories*, p. 5.) Neither has more than [redacted] employees in any given year; and organizational charts list [redacted] as falling under the [redacted] segment. (See *Ex. 583 (DISNEY-000031308)* at 5.) There is also 1 employee per year from 2016 to 2018 in [redacted] which is not included in Table 1. There are also few cases where 1 employee remains in a segment after the years indicated in this table. (See Table E.2 in Appendix E.)

[17] *See, e.g., Defendant The Walt Disney Company's Supplemental and Amended Response to Plaintiffs' Special Interrogatories, Set Two, No. 19* at p. 6.

[18] Anderson Dep. at 24:6-25, 259:12-18, 316:16 – 317:1; Lal Dep. at 16:14-24, 17:15 – 18:2, 26:25 – 27:12, 28:15 – 29:25; Burnley Dep. at 15:20-25, 28:10 – 29:2; Fox Dep. at 23:6-12, 63:10-17, 64:2-4; Bacon Dep. at 12:10-18, 29:8 – 30:21.

resources for any purpose relevant to this case. Cost center was used for accounting purposes.[19] As to "personnel area," Defendant, in response to a question about "personnel area" stated that [redacted].[20] Thus, I did not use such data in my analyses.

Company-Wide Pay Strategy

23. Despite Disney's decision to periodically re-organize its businesses into different segments, and to track data on certain sub-units, my understanding, based on deposition testimony and Disney documents, is that compensation decisions reflect a Disney strategy that is set for the entire enterprise, and that decisions undergo extensive review and coordination at the segment level. For example, a Disney compensation training document says that [redacted][21] Another document states that [redacted] (*Ex. 696* at 31571).[22] This same document lays out the company-wide policies used, including [redacted] (*Ex. 696* at 31601). Other documents make clear the "business unit" referred to as approving pay awards is the segment.[23] Moreover, every year Enterprise Compensation prepares a "Leader's Guide to Annual Compensation

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[19] See Fox Dep. at 98:10-15.

[20] *Defendant's Supplemental and Amended Response to Plaintiffs' Special Interrogatories, Set Two, No. 11* at p. 8.

[21] *4 Ex. 591_DISNEY-000031614.pdf* [redacted] at 31619. In her deposition, Kara Anderson confirms that her directors of compensation understood this to be accurate when they included it in the training document. Anderson Dep. at 53:24 – 54:6. *See also*, *Ex. 696* at 31574 [redacted]

[22] The same document also notes that there are some specific pay programs that can differ across businesses and roles (*Ex. 696* at 31571), such as bonuses (*Ex. 696* at 31574).

[23] *See, e.g., Ex. 604, DISNEY-000026700* at 26708 [redacted]); *Ex. 606, DISNEY-000024569* at 24577 (same); *Ex. 605, DISNEY-000005360* at 5368 ("Segment Senior Management will review all recommendations."); *Ex. 706, DISNEY-000021935* at 21943 (same).

Planning,"[24] guidance that is used in every segment although there can be some variations, mainly associated with bonus or long-term incentive pay.[25]

24. The senior Compensation leaders for each segment meet together weekly to discuss compensation and compensation planning.[26] Through these meetings, segment compensation leaders have input on overall compensation strategy.[27]

25. Enterprise Compensation participates in the Compensation leaders' meetings, and also leads enterprise-wide compensation related projects.[28] For example, Enterprise Compensation develops the global leveling project discussed below,[29] prepared *Ex. 696*, cited above, and presents on it to all segments,[30] prepares annual "leaders guides" for the annual compensation planning process,[31] and interacts frequently with compensation employees within each segment, providing guidance.[32]

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[24] See, e.g., *Ex. 531, 535, 537, 604, 605*, and *706*.

[25] Bacon Dep. at 32:21 – 33:22; Pate Dep. at 120:10 – 122:1 (identifying the changes made for DTCI leader guides as limited to bonus plans, LTI eligibility, and certain changes for international employees); Burnley Dep. at 214:4 – 217:11 (confirming major substantive portions of Leaders Guide came from Enterprise Compensation), 235:7 – 236:7 (identifying changes to DPEP leader guide regarding incentive plans and guidelines for international employees); Burnley Dep. at 236:19 – 238:3 (identifying changes to 2019 DPEP leader guide as including guidelines for bonus planning, short-term incentive plans, non-U.S. budgets, and timeline); Bacon Dep. at 52:6-15 (testifying that Studios has not planned promotions during ACP for approximately last three years); Bacon Dep. at 183:19 – 184:13 (noting page in 2015 leader guide titled "TWDS Salary Ranges/Compa-Ratios" is specific to Studios); Bacon Dep. at 185:3-17 (identifying Studio-specific schedule in leader guide); Burnley Dep. at 226:6 – 230:25.

[26] Anderson Dep. at 48: 3 – 49:23; Bacon Dep. at 37:2 – 44:1 (describing Compensation Leaders weekly meetings – senior compensation leader from each segment, plus multiple people from Enterprise Compensation).

[27] Anderson Dep. at 55:14 – 56:1.

[28] *DISNEY-000031566* at 31574 [redacted]; Burnley Dep. at 51:5-21 (Enterprise Compensation designed and organized annual compensation planning), 95:7-17 (Enterprise Compensation coordinated the global job leveling project); Fox Dep. at 109:8-15 (the global job leveling framework was developed by Enterprise Compensation); Bacon Dep. at 41:7-20 (Enterprise Compensation attends Compensation leaders' meetings).

[29] Fox Dep 108:8-15; Burnley Dep. at 95:7-16.

[30] Burnley Dep. at 38:23 – 40:15.

[31] Burnley Dep. at 212:6 – 217:8, 226:3 – 230:25; Bacon Dep 185:3-20, 242:5-16; Temple Dep. at 15:19 – 16:12, 25:22 – 26:8.

[32] Burnley Dep. at 37:10 – 38:7.

26. Segment compensation leadership is "responsible for implementing The Walt Disney Company compensation strategies at the segment level…"[33] Segment senior management also closely review annual pay decisions, and have final approval over salary.[34]

27. The centralization of compensation strategy is reflected in other witness testimony. Witnesses acknowledged Disney had a common compensation philosophy ("TWDC Total Rewards") that was important to understand, as it applies at least in part to different segments.[35] Witnesses also noted Disney had a common approach to setting pay across segments, as reflected in training.[36] Enterprise Compensation provides services across the company,[37] which also shares a common personnel data system.[38] Compensation employees within each segment have regular interaction with Enterprise Compensation on annual compensation planning, guidance on data systems, and innumerable other topics.[39] Multiple witnesses testified about centralized meetings among compensation leaders, covering compensation policies and strategies, including pay equity.[40]

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[33] Anderson Dep. at 67:16-20.

[34] Anderson Dep. at 230:24 – 232:13; 253:14 – 254:8; Burnley Dep. at 221:11-23; Olsgaard Dep. at 128:6-10; Fox Dep. at 238:19 – 239:9; *Ex. 604, DISNEY-000026700* at 26708; *Ex. 606, DISNEY-000024569* at 24577.

[35] Temple Dep. at 53:9 – 54:14, 112:19 – 113:16.

[36] Bacon Dep. at 147:4-14; Temple Dep. at 106:2-7 (compensation training was common across segments); Burnley Dep. at 44:1-9 (there were training modules on the "hub" that people in different segments could access); Burnley Dep. at 46:4 – 47:24 (there was overall guidance on a consistent pay policy across the company); Temple Dep. at 6:21 – 18:4 (there is a common "Walt Disney hub" that all Disney employees can access for information about training, pay, and more).

[37] Larson Dep. at 44:2-8.

[38] Bacon Dep. at 17:14-25.

[39] Burnley Dep. at 37:13 – 38:6.

[40] Bacon Dep. at 45:4-17 (Compensation leaders discussed pay equity); Burnley Dep. at 19:2 – 20:24 (weekly meetings of compensation leaders include the VP of compensation from each segment, as well as the SVP of Compensation); Burnley Dep. at 112:6-13 (Compensation leaders across segments reached a consensus on compensation policies for technology jobs); Burnley Dep. at 29:19 – 30:5 (Compensation leaders discuss variety of topics at weekly meetings, including annual compensation planning and Disney compensation systems); Burnley Dep. at 104:6-16 (Compensation leaders reviewed technology compensation benchmarking proposal); Anderson Dep. at 46:13 – 49:23 (Compensation leaders meet weekly and discuss topics including Disney pay philosophy, compensation planning tools, and information sharing practices); Pate Dep. at 74:25 – 75:7 (Compensation leaders discussed annual compensation planning tools); Larson Dep. at 144:16-23 (Compensation leaders worked on compensation

28. Enterprise Compensation designed a system for annual compensation planning, including preparing annual Leaders Guides for the process that were used across segments.[41] Common tools and templates were also used for completing annual compensation planning.[42] Enterprise Compensation recommends the merit pay increase budget, which is approved by the Compensation Committee of the Board of Directors.[43] Enterprise Compensation also provides guidance on how to pay for performance without performance ratings,[44] a common guide for communicating pay decisions,[45] and templates and other documents related to salary awards.[46]

29. Witnesses also testified to coordination and calibration across segments with respect to job leveling, assigning job families, establishing salary ranges,[47] and to handling pay equity issues across segments.[48]

Disney's System for Setting Starting Pay

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guidelines and accountabilities presentation during monthly work session); Larson Dep. at 105:21-25 (policy and guidelines team would bring proposed policy or guideline to comp leader meeting for approval); Larson Dep. at 441:16-19, 443:22-25 (Compensation leaders had meetings to discuss salary history legislation).

[41] Burnley Dep. at 51:5-15; Bacon Dep. at 185:3-21 (segments used Leaders Guide based on template from Enterprise Compensation); Temple Dep. at 16:6-12 (confirming a common document from Enterprise Compensation was used in the different segments for annual compensation planning); Temple Dep. at 25:10 – 26:8, 226:6-25, 229:3-16 (the common documents used for annual compensation planning were created by Enterprise Compensation, there were only minor differences specific to her segment (DGE)); Bacon Dep. at 32:21 – 33:22, 145:6-13 (there were few differences in annual compensation planning across segments or business areas, except regarding long-term incentive plans or bonuses); Larson Dep. at 58:10-23, 111:23 – 112:5 (Enterprise Compensation provided Leader Guides to segments, he was unaware of any segments not using them, and suggesting the Leaders Guide could be modified, but not identifying substantive changes by segment).

[42] Bacon Dep. at 196:24 – 197:5 (the SuccessFactors tool for compensation planning was used across segments); Bacon Dep. at 240 (under Workday, there was a common planning template for pay used across segments).

[43] Burnley Dep. at 215:21 – 216:10.

[44] Burnley Dep. at 214:8-21.

[45] Bacon Dep. at 242:1-16.

[46] Burnley Dep. at 216:17-23.

[47] Temple Dep. at 72:19-24 (discussion of marketing roles and job families with team members from multiple segments); Temple Dep. at 160:11 – 162:12 (calibration of global job leveling across segments); Larson Dep. at 125:25 – 126:13, 155:2 – 156:8 (Enterprise Compensation sought to create consistency across segments in leveling, establishing the same salary ranges for similar jobs across segments).

[48] Bacon Dep. at 101:13 – 104:9.

30. Segment Compensation teams are responsible for applying Disney's compensation philosophy and global job framework, described in further detail below, to set starting pay for employees. [redacted][49] [redacted].[50] The Compensation consultant refers to the applicable pay range to develop a starting salary offer, guided by a consistent set of factors, including the candidate's relevant experience, and internal equity.[51] Prior to October 2017, a candidate's current or prior salary could also be considered in setting starting pay.[52] After October 2017, Disney still permitted recruiters to record information about a candidate's salary that has been disclosed voluntarily.[53] And until 2022, recruiters could ask about and record a candidate's salary expectations.[54]

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[49] *Ex. 696* at 31582.

[50] *Id.*

[51] See *Ex. 696* at 31571; Larson Dep. at 422:11-17; Pate Dep. at 134:3 – 135:10; Schultz Dep. at 99:25 – 100:10.

[52] See *Ex. 600* ([redacted]); *Ex. 704* at 24356 ([redacted]); *Ex. 702* at 5535-36 ([redacted]). Indeed, [redacted] See, e.g., *Ex. 816* at 862-63 and *Ex. 768* at 31206. Numerous recruiter witnesses testified to using prior pay to seek higher starting salary for candidates they were seeking to recruit. See Wahab 27:14 – 29:22; 66:12 – 67:3 (information shared with Compensation could include current salary information); Watkins Dep. at 51:4-10 ("Prior to 2018, we could send a candidate's prior pay information [to Compensation]."); Hirst 27:18 – 28:7 ("Back to 2015 through 2018, there could have been information shared [with Compensation/HRBP] in terms of current pay."); Schultz Dep. at 96:2-19 (recruiters "had the ability [to ask for a candidate's prior pay information] if [they] chose to do so"); Weirick Dep. at 45:3-10, 62:14-20 (before 2017, recruiters were never instructed to not ask about current or prior salary); see also Larson Dep. at 392:8-15 ("I was aware that on occasion….[recruiters] would" ask candidates about prior pay).

[53] *Ex. 704* at 24361; *Ex. 599* at 24580; *Ex. 600* at 24350; *Ex. 601* at 5494 ([redacted]); Anderson Dep. at 173:2-21; Brahm Dep. at 68:18-24; Weirick Dep. at 49:17 – 50:21, 53:22 – 54:1.

[54] *Ex. 704* at 24357-58; *Ex. 600* at 24349-50; *Ex. 746* at 32292; Anderson Dep. at 172:18 – 173:1; Wahab Dep. at 57:23 – 58:3, 61:14-16; Weirick Dep. at 49:13-16; Pate Dep. at 188:15-22.

31. Compensation provides a salary recommendation to the hiring leader, HR Business Partner ("HRBP"), and/or recruiter in the form of either a narrow range or a single figure.[55] [redacted][56] If the hiring leader, HRBP, and/or recruiter disagree with the recommendation from Compensation, further discussion is required and ultimately Compensation must approve any revised offer.[57]

32. Thus, the record indicates that policies and practices governing starting pay and annual salary apply across the enterprise. However, the record also shows that the decision-making applying these policies is carried out by (a) the senior management in each segment responsible for review and approval of annual compensation, and (b) a small group of compensation employees in each segment who set starting pay. Because of the potential impact by decision-makers, segment may be relevant to the analysis of compensation decisions, but so-called companies, and business areas, are irrelevant to these decisions. See discussion at ¶¶ 23 above. Thus, in my analysis I aggregate the data for the company, and I control for segment (as explained below), but I ignore company and business area. (I show some results without controlling for segment, but my main analysis relies on models with these controls.) I similarly ignore cost centers, since these are used for accounting but not pay and related decisions.

Organization and Classification of Jobs

33. How Disney organizes jobs is directly germane to my analysis. Job functions are [redacted][58] Job families [redacted][59] Job functions are broader categories. For example, [redacted].[60]

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[55] See Wahab Dep. at 28:1 – 37:8; Weirick Dep. at 38:9-12; Schultz Dep. at 97:10-15, 100:19 – 101:4; Larson Dep. at 406:7-10; Pate Dep. at 133:22 – 134:2, 135:14-19; Brahm Dep. at 55:7-20.

[56] *Ex. 591* at 31631; *Ex. 776* at 35586.

[57] See Bacon Dep. at 231:5-25; Wahab Dep. at 28:1 – 37:8; Hirst Dep. at 32:15-23; Temple Dep. at 184:13 – 185:9.

[58] *Ex. 696* at 31583.

[59] *Ex. 696* at 31583.

[60] See *DISNEY-000005671.pdf* at 5683. [redacted].

Both job family and job function provide horizontal classifications of jobs. [redacted] in the data I analyzed; [redacted][61]

34. [redacted][62] Disney documents explain that job levels are assigned [redacted].[63] Disney concluded that the job level framework [redacted][64] I view job level as a vertical classification of jobs.

35. For purposes of Plaintiffs' FEHA claims, I compare men and women who are similarly situated with respect to factors which I believe may explain differences in pay and are appropriate to include. I interpret "similarly-situated" to mean people who have the same productivity-related characteristics and are in the same jobs as Disney defines them.

36. For purposes of Plaintiffs' EPA claims, I compare men and women who are doing substantially similar work, as I understand that statute requires. Based on analyses by Dr. Leaetta Hough, people in the same job family and job level pair are doing substantially similar jobs.[65]

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[61] Disney identifies, in its data (in the file *DISNEY-000031345*) [redacted] These are listed in *7_Ex. 594_DISNEY-000031306.pdf*. [redacted].

Also, the nomenclature has changed as Disney has used different data systems. [redacted] See *8_Ex. 595_DISNEY-000031352.pdf*. I use the phrases job function and job family throughout this report.

[62] *DISNEY-000005671.pdf* at 5714 [redacted]); *DISNEY-000031403* (definitions of job levels are based on the "specific responsibilities and skills required to perform various jobs.") Deposition testimony confirms this. Anderson Dep. at 116:3-13.

[63] *Ex. 764* at 21922.

[64] *Ex. 764* at 21922.

[65] Disney also classifies jobs by [redacted] but I do not control for this separately. There are [redacted], but their addition has a negligible effect on the estimates. In my preferred specification (Table 3, Model II), the gender disparity is −0.0199 (9.1 standard deviations) with the inclusion of management level fixed effects, vs. −0.0201 with them excluded. [redacted] (e.g., *Ex.*

Report of Dr. Leaetta Hough at pp. 1-4, 41-42. This is consistent with Disney documents, which note that job function, and job family within them, "Defines the nature of the work being performed for purposes of benchmarking to the external market," and that "Job Family is a key element in determining the appropriate salary grade and hiring range for open positions…" (The last quote also references job level, consistent with job family and job level combinations being the appropriate unit of analysis).[66]

37. In addition to Dr. Hough's report concluding that those in the same job family and same job level are doing substantially similar work, [redacted][67] [redacted] *DISNEY-000005671* at 5701). Several Disney witnesses testified to jobs in the same region, family, and level being in the same grade or pay range.[68]

38. Several Disney witnesses confirmed the commonality of job organization and classification across the enterprise. For instance, multiple Compensation VPs testified to the universal application of Disney's global job leveling framework that serves as the foundation for compensation decisions throughout the company. See Bacon Dep. at 255:5-25; Burnley Dep. at 61:1-21, 64:10 – 65:6, 65:22 – 66:3, 97:15 – 98:1; Temple Dep. at 61:6-10; Anderson Dep. at 73:13 – 74:11; 114:3 – 115:23; Larson Dep. at 72:17 – 73:2, 74:18 – 75:10. These witnesses also testified that changes to job families or job levels are done according to the same process across

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592 at 5692 [redacted] See *Ex. 592* at 5714 [redacted]

[66] See *6_Ex. 593_DISNEY-000031317.pdf* at 31318.

[67] *Ex. 591*, *DISNEY-000031614* at 31630 [redacted] *Ex. 592, DISNEY-000005671*, at 5702 [redacted]

[68] Bacon Dep. at 228:9-19; *Ex. 592* at 5701-02; *Ex. 763* at 32967; Fernandez Dep. at 153:9 – 154:1; Anderson Dep. at 186:15 – 187:6; 152:5 – 153:2 (noting possible exceptions for "some data science jobs"); Burnley Dep. at 82:18 – 83:5; Mark Larson said this is not *always* the case, but could only name two exceptions, and also acknowledged that he gave a presentation where he said that jobs in the same family and level will have the same grade (Larson Dep. at 351:22 – 357:13).

segments. Temple Dep. at 85:4 – 86:1, 93:2-22; Bacon Dep. at 282:3-13, 259:10-15.

39. I restrict attention to job levels Plaintiffs use to define their class: P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (which is above E1). I exclude a handful of Vice Presidents in E1X. I also exclude A6 and A9, which include only Vice Presidents and Senior Vice Presidents. I exclude the Human Resources Compensation Job Family as those workers who may be making the decisions in question in this case.[69]

Data

40. To study gender disparities in Class Period pay, I use the following data files: EmpPayHistory[70] and EmpAddPayments[71] for annual salary and lump sum payments respectively; EmpPersonalInfo[72] for employee information such as gender and date of birth (used for the computation of potential experience); EmpActionHistory[73] and EmpDateSpecs[74] for original hire date and tenure at Disney; JobInfo[75] for job level and family; Position[76] for positions and workforce classifications; OrgUnitLocation[77] and CAGeoDiffsHistory[78] for segment and regional information of each employee-year; EmpPerfRating[79] for employee performance ratings; Kenexa Education Data[80] for educational background; and Kenexa Prior Employer Data[81] for actual prior experience. All these data files have an identifier for each personnel (*personnel_number*) or some other variable on which I can match records, which allows me to create one master dataset. As each employee's compensation information as well as

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[69] I understand it may also be appropriate to exclude attorneys who were involved in this litigation. However, I do not yet know how to identify them, as I have only a personnel number by which to identify individuals. My understanding is that there may be information provided at a later date by which to exclude them. Given that my analysis encompasses [redacted], removing even 50 or somewhat more of them at a later date would not be likely to impact the analysis materially.

[70] *DISNEY-000031373*.

[71] *DISNEY-000031355*.

[72] *DISNEY-000031375*.

[73] *DISNEY-000031354*.

[74] *DISNEY-000031356*.

[75] *DISNEY-000031345*.

[76] *DISNEY-000031348-50.*

[77] *DISNEY-00031384*.

[78] *DISNEY-000031343*.

[79] *DISNEY-000031374*.

[80] *DISNEY-000032258, 61, 64, 67*.

[81] *DISNEY-000032260, 63, 66, 69*.

job and level changes over time, I construct an employee-year level data set. I take snapshots of the relevant information on January 15th of the following calendar year to account for instances where compensation changes are realized a few days after the end of the calendar year**.** The data sets I use are detailed in Appendix A.

41. The data I was given cover all full-time, nonunion Disney employees in California (with the exclusion of ESPN, Hulu, Pixar, National Geographic, and 21st Century Fox), below the level of Vice President, during the Covered Period.[82] From the data I was given, I also exclude employees in the business area ILM (Industrial Light and Magic), and 38 employees of BAMTECH who came to Disney via the acquisition of the company.[83]

42. To study starting salary, I use the same files that I use for gender disparity analysis. I restructure the data so that each observation represents an employee's personal information, position held, and starting salary at the time of their original hire at Disney instead of on the first January 15th snapshot after their original hire. I also present an analysis using Kenexa Education Data and Kenexa Prior Employer Data, to consider the influence of education, prior experience, and the relevance of prior experience to jobs at Disney.

Analysis of Class Period salary differences

Summary

43. My analysis of Class Period pay focuses on the sum of base salary plus lump sum pay. It excludes bonuses and long-term incentive (LTI) pay. I include lump sum pay because, as I understand it, it can best be thought of as part of salary (although it may not necessarily persist

[82] See *Plaintiffs' Fourteenth Set of Requests for Production of Documents* (defining "relevant positions" and "relevant employees" for which data was requested, at pp. 2-3), and *Defendant The Walt Disney Company's Objections and Responses to Plaintiff Laronda Rasmussen's Request for Production of Documents, Set Fourteen* (objecting to the time period used to define relevant positions and employees, but agreeing to produce data for the defined relevant positions and relevant employees limited to the class period, see (e.g., pp. 6-7, and repeated with response to each specific data request).

[83] Counsel instructed me that they excluded from their data request recent Disney acquisitions like Hulu and 21st Century Fox, as well as some other portions of Disney not subject to the same compensation policies and practices as the proposed class, or not transitioned to those practices until late in the class period. They further instructed me that based on similar information learned in discovery, they defined their class to exclude the noted BAMTECH and ILM employees.

across years like base salary does).[84]

44. Lump-sum pay is used when a pay increase would take someone over the maximum for their pay range, in which case the increase would be split between a merit increase and a lump sum payment (Burnley Dep. at 266:1-5; *Ex. 611* at 31495). Furthermore, if someone was already above the maximum in the pay range, then the entire increase would be as a lump sum payment (Anderson Dep. at 285:24 – 286:3; see also *Ex. 613* at 5575). Similarly, Kara Anderson, in her deposition, notes that lump sums could be used when someone is moved into a different role and their pay is already above the hiring range (Anderson. Dep at 319:22 – 320:13).

45. I do not analyze bonuses and LTI pay for evidence of discrimination, because my understanding is that Disney's practices with regard to these components of pay are not being challenged in this case. However, since they are set as a percentage of annual salary, to the extent there are unlawful differences in annual compensation, damages for such disparities should also include the value of lost bonus/LTI.

46. My analysis of Class Period pay differences focuses on differences in annual salary (which I will use as a short hand for base salary plus lump sum payments). I show that there is a

[84] I exclude sign-on bonuses for new hires from first-year compensation in my Class Period analysis, and later in my analysis of starting pay. Disney documents and testimony for all segments, Corporate/Enterprise jobs, DPEP, and DATG/DGE [redacted] (for all segments: *Ex. 776* [redacted] at 35597; *Ex. 880, DISNEY-000035575* at 35597; for Corporate/Enterprise jobs: *Ex. 602*, *DISNEY-000031704* [redacted] at 31095 and *Ex. 822*, *DISNEY-000039394* [redacted] at 39398; at DPEP: *Ex. 703*, *DISNEY-000005293* [redacted] at 5300, *DISNEY-000021929* at 21930, 21934, [redacted] and at DATC/DGE: *Ex. 653*, *DISNEY-000023394* [redacted] at 23397, and *Ex. 654*, *DISNEY-000023408* [redacted] at 23410. [redacted]

large and statistically significant female penalty in annual salary at Disney for Covered Workers in the Class Period, even when I account for potentially non-discriminatory factors that could affect pay.

47. For purposes of Plaintiffs' FEHA claims, I compare women and men who are similarly situated with respect to factors that I believe may explain differences in pay and are appropriate to include. This includes controls for the combination of job family and job level. As noted above, [redacted]. I use this field in my analyses when I say I control for job family. However, [redacted]. I use whatever detail Disney used in classifying its employees, as reflected in this field in the data.

48. For purposes of Plaintiffs' EPA claims, I compare women and men who are doing substantially similar work, as I understand that statute requires. Consistent with Dr. Hough's conclusion that the combination of job family and job level defines "substantially similar work," my EPA analyses are limited to include only individuals who have been assigned a full job family, and not merely a job function. [redacted]. The EPA analyses thus cover a subset of the full class.

49. I find that women at Disney earn less than similarly-situated men. In particular, in my preferred model, I estimate a female pay penalty of 2.01%. This is a difference of 9.2 standard deviations, implying that the estimated difference is statistically significant at a level well below 1%. To be more precise, the odds that we would find an estimated gender gap this large in the data, if the true effect of gender on pay was zero (i.e., there was no pay discrimination), is less than 1 in 1 billion. These results are summarized in Table 2 below.

Table 2: Summary of Pay Results, Class Period

Period	Pay measure	Controls	**Estimated female pay gap %**	**Standard deviations**	**Probability of result under null of no discrimination**	Number of observations (% women)	Estimated average female pay gap, April 2023$
Class period pay							
Class period, 2015 - 2022	Base salary + lump sum pay	Exempt, hourly, year, technology, northern and southern regions, technology x region, potential experience at hire and square, Disney tenure and square, job family x job level, segment	**−2.01%**	**9.16**	< 1 in 1 billion	[redacted] (51.35%)	−$2,766 (per year)
Starting pay							
Hired in class period, 2015-2022	Base salary	Exempt, hourly, contractor, union, year, technology, northern and southern regions, technology x region, potential experience at hire and square, job family x job level, segment	**−2.81%**	**2.92**	< 1 in 1 thousand	[redacted] (54.80%)	−$3,303

Source: SAP analysis data set; Kenexa Applicant Data.

Notes: Log differences are reported, which closely approximate percentage differences. The estimated average pay gaps are computed simply in this table, as the estimated percentage difference applied to average male pay (for class period pay, $137,617, and for starting pay, $117,550 in April 2023 dollars). For details, see Tables 3 and 7.

50. After establishing this core result, I use some more limited data sources available for subsets of Covered Workers in the Class Period for which I have richer control variables (like detailed education information). My findings imply that if I had these richer controls for the full sample of Covered Workers in the Class Period, the estimated female pay penalty would be larger than what I report in the previous paragraph.

51. Evidence that women are paid less than men with comparable productivity-related characteristics points to pay discrimination against women. This conceptualization of pay discrimination is standard in the labor economics literature, beginning with the seminal work of Becker (1957),[85] who defined discrimination in pay as unequal pay for equally productive workers. The use of regression models like those I use to estimate gender disparities in pay, in order to assess whether there is evidence consistent with pay discrimination – or whether, instead, the female pay gap is attributable to other productivity-related and non-discriminatory factors – is pervasive in economics, with scores if not hundreds of papers written in recent decades.[86]

52. I then show evidence consistent with the female pay penalty at Disney being driven in part by women being paid less than similarly-situated men when they started at Disney. Given that Disney had a policy of basing starting pay in part on prior pay, see n. 52 *supra*, and that the labor market generally reflects pay discrimination against women, the reliance on prior pay had an adverse effect on starting pay of women hired at Disney, which helps account for women's lower pay during the Class Period.

53. While Disney may argue that it did not require consideration of prior pay in every case, but left it to the discretion of those establishing starting pay whether to consider it, Disney entrusted starting pay decisions largely to its Compensation employees within each segment, who were provided the same set of criteria.[87] I understand that Plaintiffs allege this delegation of decision-making to a small group of compensation workers within each segment was a practice

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

[86] *See*, *e.g.*: 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.

[87] [redacted] These are shown, by segment, in Table E.1 in Appendix E.

that reinforced the reliance on prior pay, and thus contributed to unwarranted disparities in compensation, as these decision-makers could, whether consciously or unconsciously, be biased against women, making them more likely to rely upon prior pay to diminish women's pay or increase men's pay. Their discretion might also be applied in weighing other factors that Disney directed be considered in setting starting pay in a manner adverse to women.

54. These results showing a female penalty in starting pay are also summarized in Table 2.

55. I then turn to EPA analyses, which simply restrict the analyses described above to the subset of individuals in each yearly snapshot for whom we have job family. Here too I find that women at Disney earn less than comparable men performing substantially similar work. In the Class Period, women at Disney were paid less than comparable men doing substantially similar work. In my preferred model, I estimate a female pay penalty of 0.81%. This is a difference of 3.8 standard deviations, implying that the estimated difference is statistically significant at the 1% percent level (and indeed a much lower level). After establishing this core result, I use some more limited data sources available for subsets of workers for which I have richer control variables (like detailed education information). My findings imply that the estimated female pay penalty relative to men doing substantially similar work would be larger adjusting for these variables.

Empirical Approach

56. My analysis compares salaries at Disney for female and male employees, I first study similarly-situated employees, and then, for Plaintiffs' EPA claim, further restrict the comparison to be "within" jobs that are substantially equal or similar work with similar skills, effort, and responsibility (i.e., within combinations of job families and job levels). The data used in these models are records for individuals in specific years. As noted above, I analyze the sum of base salary plus lump sum pay. In addition to pay, the data I use include an indicator for the gender of an employee, and characteristics of the individuals and their jobs.

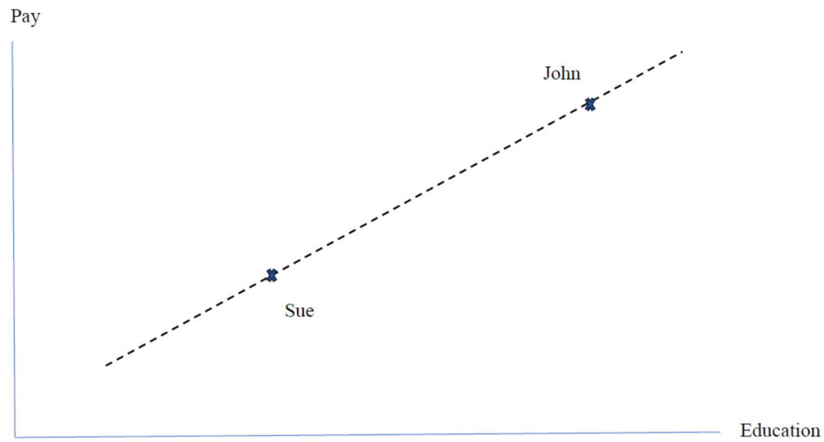
57. The regression models I use estimate the female pay penalty (if there is one) once we adjust for possible differences between female and male employees that could account for this pay gap. For example, suppose that we simply compare average pay of all female and male employees at Disney, and find that average pay of female employees is 10% lower. It is possible that women do different jobs, and those jobs could pay less. It is also possible that women and men are in broadly similar jobs, but the women have lower performance. 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 female pay penalty. Of course, the opposite is also possible, so adding controls for the individual or job could increase the estimated female pay penalty. Indeed, this happens in my analysis.

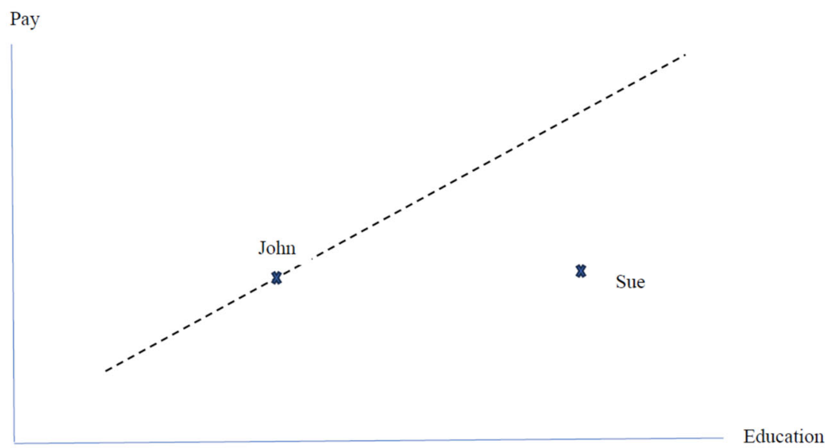
58. This is precisely what a regression model does. A regression model "holds constant" or "controls for" these other factors. These phrases mean that, in estimating a regression model, we adjust the pay gap for differences in the jobs employees hold, and their characteristics and performance, so that we are comparing pay between women and men with similar performance, education, and experience. 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.[88]

59. The figure in Example A below illustrates via an example. Sue earns less than John, as indicated by Sue being lower on the vertical axis for pay. But Sue has less education than John. The dashed line shows the relationship between education and pay. The fact that Sue and John are on the line indicates that education completely explains the pay difference between them. Thus, there would be no reason to conclude that the pay gap between Sue and John is attributable to Sue's gender (i.e., discrimination). However, the figure in Example B demonstrates a different case – and one that turns out to be more consistent with the facts in this case. In this example, Sue is paid equally to John. But Sue has higher education, and once account is taken of education, we see that Sue is underpaid because of her gender.

[88] 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 female pay penalty.



Ex. A: Sue appears to be underpaid, but after accounting for education, she is not.



Ex. B: Sue appears to be paid equally to John, but after accounting for education, she is underpaid.

60. To be clear, this example would correspond to a case where the regression analysis considered only one other factor besides gender – specifically, education. More generally, my analysis asks – in a detailed manner making extensive use of data provided by Disney – whether other factors can explain any gender pay gaps that I find.

61. 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.[89] This convention, and the reasons for it, goes back to the original development of the

[89] For example, if a woman earns $9,000 and a man earns $10,000, the absolute difference in

earnings regression in labor economics (Mincer, 1974).[90] This is usually done by measuring pay in terms of the "natural logarithm," in which case the coefficient estimates approximate percentage differentials.

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

63. An estimated gap in pay might reflect statistical error rather than a true difference between the pay of women and men. In classical statistical theory, the difference between what we estimate and the underlying true behavior comes about because we typically only have a sample from the data. In the employment litigation context, we might have all of the data for a particular period, but there are still sources of randomness, including simple errors of measurement in the data, uncertainty about the precise model to estimate, etc. To assess this formally, statisticians compute the "standard deviations" of an estimate – in this case, the estimated female pay penalty – and summarize the estimated female pay penalties in terms of "standard deviations." This standard deviations metric is used to test whether the measured difference in pay between women and men is statistically significant and differs from a hypothetical null hypothesis of gender-neutral pay setting – i.e., no difference in pay between women and men – which is what we would expect in the absence of discrimination. The more standard deviations from the null hypothesis of zero that the estimated pay gap is, the less likely it is that the estimated female pay penalty is due to chance, as opposed to a systematic difference in pay between women and men.

64. 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

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pay is a $1,000 pay disparity, and the percentage difference for women relative to men is 10% ($1000/$10,000).

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

compensation was neutral with respect to gender is 1 in 20. A difference of 2.58 standard deviations would be statistically significant at the 1% level, meaning that the likelihood of observing this value if compensation was neutral with respect to gender is 1 in 100 (1%). Similarly, the likelihood of observing a difference of more than 3.29 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.

65. Labor economists and econometricians more broadly generally regard any disparity of two or more standard deviations to be "statistically significant."[91] Court rulings also indicate that roughly two or more standard deviations (a 5% level of statistical significance) are considered statistically and legally significant evidence of discrimination.[92]

66. 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 female pay penalty 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:[93]

Standard deviations	Probability	Significance level
1.96	1 in 20	5%
2.58	1 in 100	1%
3.29	1 in 1,000	0.1%
3.89	1 in 10,000	0.01%
4.42	1 in 100,000	0.001%
4.89	1 in 1 million	0.0001%
5.33	1 in 10 million	0.00001%
5.73	1 in 100 million	0.000001%
6.12	1 in 1 billion	0.0000001%

[91] E.g.: Goldberger, Arthur S. 1991. A Course in Econometrics. Cambridge, MA: Harvard University Press, p. 215.

[92] *See, e.g.*, *Hazelwood School Dist. v. United States*, 433 U.S. 299, 309-11 & nn. 14, 17 (1977); *Paige v. California*, 233 F. App'x 646, 648 (9th Cir. 2007) (finding it was error to require more than 1.96 standard deviations to establish disparate impact, consistent with conventions in social science, the federal government's internal standards, and past Ninth Circuit cases) (citing *Segar v. Smith*, 738 F.2d 1249, 1283 (D.C. Cir. 1984)); *Bouman v. Block*, 940 F.2d 1211, 1225 (9th Cir. 1991) (statistical significance of disparate impact shown where disparity was significant at the .05 level); *Stender v. Lucky Stores, Inc.*, No. C 88-1467, 1991 WL 127073, at *3 (N.D. Cal. Apr. 4, 1991) (courts have held that a level of .05 is sufficient to support an inference of discrimination in Title VII cases).

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

67. Finally, it is important to understand what it means to estimate a female pay gap, as my regression model does, and to measure the standard deviations of the estimate (i.e., how precise it is). When I estimate a particular value of the female pay gap – say, for example, a 5% penalty for women – this does not mean that that 5% figure applies to every woman in the sample. Rather, just like if I compare the average height of women and men, I will find that women are on average shorter, but there is variation; in particular, there are some women taller than the average man, and some men shorter than the average woman. This variation does not in any way "invalidate" the average estimate; the average estimate, after all, is just what it says – an average. It is informative about the relative heights of the populations of women and men, and it predicts on average what we would find if we drew a random woman and a random man from the population. But it of course does not predict their heights exactly.

68. And the standard deviations of the estimate reflect this variation, but also tell us how precise the estimate that is – in other words, how likely it would be that the heights of any particular woman and man deviate from the average difference, and how likely large deviations are. When an estimate – like my hypothetical 5% female pay penalty – is statistically significant, that tells us that, despite individual-level variation, we are highly confident that women are in fact paid less than similarly-situated men, even taking into account job-related factors such as job family, job level, education, experience, performance, and tenure. To be precise, when the estimate is statistically significant at the 5% level, the estimate is sufficiently precise that we are more than 95% certain the true gender pay gap is negative, so we can conclude that women are paid less than men.

Level of Analysis

69. Because Disney has a centralized compensation strategy and common compensation policies and practices, I analyze the entire company (to extent included in the class) together. Because segment leadership and segment compensation can play a role in deciding on pay for individuals, I control for segment in the analysis. Similarly, because Disney used job function, job family, and job level to identify work requiring different levels or types of skills and responsibility, which may be paid differently, I control for each job family-job level combination.

Analysis of Annual Pay in the Class Period

70. I first describe my main analysis of pay in the Class Period, and then some additional

analyses I can do on a subset of the data with more information available, and finally provide EPA analyses for the subclass.

71. I estimate models for annual pay (defined as salary plus lump sum) using the natural log of annual salary as the dependent variable, or outcome. When using an earnings measure in logs, the estimated coefficients on the right-hand side or independent variables measure the relative or percentage effects on earnings. The use of log pay measures follows the standard approach in labor economics, based on evidence that these models fit the data better because equal changes in independent variables (discussed below), like years of schooling, have equal relative or percentage effects on earnings. For example, there are scores of papers indicating that each year of schooling raises earnings by about 9 percent.[94]

72. My regression model always includes a "dummy variable" or "indicator variable" for women. This variable is equal to one for women, and zero for men, and the implication is that its coefficient estimates the percentage pay gap between women and men. A negative value (as with my results) indicates that women are paid less. The coefficients from the log specification approximate the percentage differences very closely.[95] I report the more exact estimate of the percentage pay gap by gender in the results reported below.

73. My regression model includes an extensive set of regression control variables, to account

[94] See: Psacharopoulos, George, and Harry Anthony Patrinos. 2018. "Returns to Investment in Education: A Decennial Review of the Global Literature." *Education Economics*, Vol. 26, pp. 445-58.

[95] To be precise, the percentage difference would be calculated as $\exp(b) - 1$, where "exp" is the exponential function, and b is the estimated coefficient of the corresponding independent variable, such as the female dummy variable. For example, for $b = -.05$, the percentage difference would be 4.88%. (See: van Garderen, Kees Jan, and Chandra Shah. 2002. "Exact Interpretation of Dummy Variables in Semilogarithmic Equations." *Econometrics Journal*, Vol. 5, pp. 149-59. They also point out that this is actually slightly more complicated if we try to account for the fact that b is an estimate of the female pay penalty, rather than a known quantity.) In this report, I simply report log differences. For the magnitudes of female pay penalties that I estimate, these are very close to percentage pay differentials.

for potential sources of differences in pay aside from gender.[96]

74. These variables were chosen on two bases: standard control variables dictated by labor economics research; and specific variables indicated by Disney policies and testimony. There is also one additional limitation required by the EPA, which restricts my analyses for the EPA subclass to those for whom there is data available to identify who is performing "substantially similar" work. As noted above, for the analysis related to the EPA claim I restrict the data to observations when job family was available (and include controls for all combinations of job families and job levels).

75. Among the labor economics variables dictated by labor economics research, I include an estimate of potential labor market experience prior to beginning work at Disney, and tenure at Disney, as well as the squares of these variables. These variables are predicted to affect earnings by the human capital model, by capturing variation in investments in workers that occur post-schooling that increase workers' productivity and hence pay.[97] It is common to also include a measure of education – usually years of schooling – based on the human capital model. However, I do not have data on education for a large share of Disney's workers, so my main analysis uses a larger set of data without this information. (When I do this, my potential experience measure assumes people left school at age 22, so potential experience equals age at hire at Disney minus this age minus tenure at Disney).[98]

76. I also include an indicator for whether a worker is or is not exempt from the FLSA, since

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[96] 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 tenure. 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.)

In addition, my models often include dummy variables to indicate when there are missing data on a particular control or set of controls for some observations including in the estimation.

[97] 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.

[98] In cases where employees started work at Disney before the age of 22, the potential experience measure is set equal to zero.

the different regulations governing their pay may impact their salary.[99] For this reason I also include an indicator for hourly employees as the analyzed Disney employees include both salaried and (a small number of) hourly employees.

77. Among the Disney-specific variables, I sometimes include controls for the segment (and I do in my preferred models). [redacted] (*Ex. 592*).[100] Region designations are provided by Disney in *DISNEY-000031343 -* [redacted][101] I use the information in *DISNEY-000031384* to determine each employee's city and state, and then match them to the regions defined in *DISNEY-000031343 -* [redacted]

[99] The field [redacted] in *DISNEY-000031345* takes on the value 1 for exempt employees and 2 for non-exempt employees (Email from Sarah Besnoff, Feb. 2, 2023).

[100] [redacted] (See *5_Ex. 592_DISNEY-000005671.pdf* [redacted] at 5696.) [redacted] Between the inclusion of these two variables, and a broader set of job family controls, the regression model builds in these regional pay differences by job.

[101] [redacted]

[redacted][102] [redacted].[103]

78. In the data sets used in labor economics research, it is not common to have a detailed classification of the jobs workers do. However, with data on specific companies, as is used in litigation of discrimination claims, these classifications typically are available. I control for the different jobs using Disney's classifications of jobs. For the equal pay claim, the classification must be narrow enough to identify women and men in substantially similar jobs. Here, I create controls for the "job families" and "job level" in which people work. Job family is defined as [redacted][104] I use unique pairs of job families and job levels to define job because this is how Disney defines jobs in benchmarking pay to the external job market and in establishing pay ranges for new hires.[105] In the data I analyze, there are [redacted] job families and [redacted] job levels, for a maximum of [redacted] possible combinations. However, the actual

[102] *5 Ex. 592*, *DISNEY-000005671-5715* at 5696.

[103] [redacted]

[104] See *DISNEY-000005671.pdf* at 5714. [redacted] However, as a short-hand (because I always use family when I can), I typically refer to "job family." Because of the greater similarity required for EPA claims than for other claims, not all employee-years can be included in my EPA analysis.

[105] Anderson Dep. at 88:14-17 and 55:24 – 56:1, and, e.g., at 150:2-17 (explaining that job family and level is a starting point for assigning the pay grade, which is associated with a pay range); Anderson Dep. at 144:4-17; Bacon Dep. at 228:9-19 (confirming that jobs in the same family and level will generally be in the same grade); Burnley Dep. at 77:24-78:1, 82:18 – 83:5 (suggesting that roles within the same job family and job level may be assigned different grades if they are located in different regions, before admitting that region actually does not affect grade); Larson Dep. at 351:22 – 357:13; *Ex. 696* at 31586; *Ex. 592* at 5701-02; *Ex. 763* at 32967. See also Fernandez Dep. at 153:9 – 154:1, acknowledging that comparable pay for those in the same job family and job level (and region) is considered internal equity at Disney, and similarly Weirick Dep. at 27:13-20. See also, Hough Report at pp.1-4, 35-39, 41-42.

data has fewer, because not every job level is represented in each job family.[106]

79. When the regression model includes controls for combinations of job families[107] and levels, the only pay differences between women and men that contribute to my estimated gender pay gaps are differences between women and men who are in the same job family and job level. Put differently, when these job controls are included, the gender pay gap I estimate is the gender pay gap *within* the same jobs. This approach of pooling the data, controlling for job classifications, and interpreting the resulting gender gap (if there is one) as evidence of pay discrimination is consistent with the practice of studying labor market discrimination in labor economics when there is job classification data available, and is interpreted as speaking to evidence of equal pay violations.[108]

80. The results of my analysis of gender differences in Covered Positions in the Class Period are reported in Table 3. Table 3 begins by reporting estimates of the model with controls only for worker characteristics, broad job characteristics, and job families and levels, and not Disney

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[redacted] This clearly indicates that job codes are far more detailed than the differentiation needed to identify jobs with substantially similar work.

107 As noted above, [redacted]

108 For two examples of studies that interpret gender differences in pay within job cells (in this case, occupation-by-employer cells), see: Groshen, Erica L.. 1991. "The Structure of the Female/Male Wage Differential: Is It Who You Are, What You Do, or Where You Work?" *Journal of Human Resources*, Vol. 26, pp. 457-72; and Bayard, Kimberly, Judith Hellerstein, David Neumark, and Kenneth Troske. 2003. "New Evidence on Sex Segregation and Sex Differences in Wages from Matched Employer-Employee Data," *Journal of Labor Economics*, Vol. 21, pp. 887-922. Disney's job functions and job levels are much narrower than the occupation and employer combinations studied in the literature. Job families and job level are narrower still.

units. The estimates, reported as model I, indicate that women are paid approximately 2.06% less than comparable men.[109] Measured in terms of standard deviations, this is a difference of 9.3 standard deviations, implying that the estimated pay gap is highly statistically significant (at less than the 1% level); the standard deviations indicate that the odds are less than 1 in 1 billion that I would have estimated a gender gap this large or larger if the true gender gap were zero).

81. Model II adds business segment. Based on the deposition testimony and other evidence discussed above, I view this as the most reliable regression model. (And I correspondingly use it in other analyses that follow.) The estimate is barely changed, indicating that women are paid 2.01% less than comparable men doing substantially similar work. The estimated standard deviations difference is 9.2, meaning that the estimate is strongly statistically significant (significant at less than the 1% level, with odds are less than 1 in 1 billion that I would have estimated a gender gap this large or larger if the true gender gap were zero).

82. Thus, this evidence shows that women with similar productivity-related characteristics as men, and doing similar work to men, were paid less than men, with an estimated pay penalty of 2.01% which is strongly statistically significant (9 standard deviations). This evidence is consistent with pay discrimination against women at Disney.[110]

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[109] I estimate log wage equations. As noted earlier, log differentials approximate percentage differentials. For the magnitudes of gender pay differentials I find, the coefficient on the gender dummy variable very closely approximates the percentage pay gap. I thus use the work "approximately" here, but drop it going forward.

[110] Note that I have also estimated the models in Table 3 controlling separately for job family and job level (i.e., separate sets of dummy variables for each, rather than dummy variables for each unique job family-job level pair). This analysis may be more relevant to the FEHA claim. The results, reported in Appendix E, Table E.3, indicate a slightly larger gender pay gap than in Table 3. The estimate corresponding to Table 3, Model II, becomes II −2.47% (10.4 standard deviations).

Table 3: Regression Model of Gender Disparity in Compensation (Log (Base Salary + Lump Sum)) at Disney, Restricted to Employees in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents), 2015-2022

	(I) Baseline			(II) Baseline + Segment		
	Coeff.	t-stat	p-value	Coeff.	t-stat	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Female	**-0.0206**	**-9.2915**	**0.0000**	**-0.0201**	**-9.1600**	**0.0000**
Potential non-Disney Experience (Sq.)	Yes			Yes		
Tenure at Disney (Sq.)	Yes			Yes		
Exempt Status	Yes			Yes		
Hourly	Yes			Yes		
Technology Job Indicator	Yes			Yes		
Southern California Indicator	Yes			Yes		
Southern California Indicator X Technology Job Indicator	Yes			Yes		
Northern California Indicator	Yes			Yes		
Northern California Indicator X Technology Job Indicator	Yes			Yes		
Year dummy variables	Yes (8)			Yes (8)		
Job Family X Job Level dummy variables	Yes (3458)			Yes (3458)		
Segment dummy variables	No			Yes (14)		
Observations	[redacted]			[redacted]		
R-squared	0.8727			0.8752		

Source: SAP analysis data set.

Notes: Observations defined at the employee-year level. Standard errors are clustered at the employee level. Analysis is restricted to full-time, nonunion employees in specified job levels, in California. If job family is missing I include an interaction between job level dummy variables and a dummy variable for missing job family. I include a dummy variable for observations missing exempt status [redacted]

[redacted] When the t-statistic exceeds 1.96, the estimate is significantly different from zero at the 5% level or less, so the p-value would be 0.05. See the table on p. 26 for other examples.

Analyses with Richer Controls but only for a Subset of the Data

83. When I estimate a gender difference in pay controlling for the variables on which I do have data, the absence of data on a variable that affects pay can generate what is called "omitted variable bias." The "bias" in this term refers to the possibility that the gender gap in pay is incorrectly estimated. For example, I indicated that I do not have data on education in the preceding analysis. If women on average have more education (which increases pay), then the absence of data implies we would underestimate the female penalty in pay, because women's higher education boosts their pay relative to men's; thus, the female pay penalty would be larger if I could fully account for the education difference. Conversely, if women on average have lower education, then we would overestimate the female penalty in pay, since part of the apparently lower pay of women would be due (hypothetically) to their lower education. In recent decades, women have overtaken men in college education and degrees.[111] If the same applies to workers at Disney, then the absence of data on education implies that the estimates in Table 3 could understate the female pay penalty.

84. I report two different analyses for which I have richer control variables available, but for only a subset of the data. First, I am able to obtain more data on Disney workers in the Class Period from data on applications to Disney (from Kenexa) matched to the data on Disney employees (SAP data). I have these for a limited subset of the data, because the applicant data produced was limited to those hired during the Class Period.[112] In particular, in the pay analysis discussed above and reported in Table 3, I have data on [redacted] employee-year combinations. When I match to the Kenexa data, I have only [redacted] employee-year observations, or about 33% of the observations. As a result, it is important to clarify what I can and cannot do with these matched data. What I can do is ask what are the consequences for the estimated gender pay gap of adding data on additional control variable – like education, and others I describe below. What I cannot do, however, is obtain an overall "representative" estimate of the gender pay gap at

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[111] See: England, Paula, Andrew Levine, and Emma Mishel. 2020. "Progress Toward Gender Equality in the United States has Slowed or Stalled." *Proceedings of the National Academy of Sciences*, Vol. 117, pp. 6990-7.

[112] I understand that Plaintiffs sought this data, to the extent available, for all employees in the SAP data that was produced, but that Disney declined to collect or produce such data for individuals hired prior to the Class Period. Should such more complete data be made available, I would incorporate it into my analysis.

Disney – owing to the large share of observations without these richer data.

85. Put differently, this analysis is most useful for assessing whether the estimated gender pay gaps reported in Table 3 and described above are biased from the exclusion of variables measuring worker differences that might explain pay. My overall conclusion is that the estimates reported in Table 3 are biased towards zero. That is, incorporation of richer control variables in this section, for the subset of data for which I have them, leads to a *larger* estimated female pay penalty, which would imply larger damages.

86. I first explain the additional data I have and constructed using the Kenexa data, and then I report the results. I then turn to a second analysis using a limited subset of the data with performance ratings.

87. From the Kenexa applications data, I use information on education of workers, highest degrees, fields of study, and school attended. I use different elements of the education data in the Kenexa Education data for employees who appear in the SAP data. First, I use school names in the Kenexa Education data. There are many colleges and universities in the data. Rather than trying to control for all of them, which would include many schools with so few attendees as to be uninformative, I instead do two things. First, I match, when possible, these colleges and universities to three university rankings datasets and use their rankings information. The Times Higher Education World University Rankings 2023 from The Wall Street Journal[113] (WSJ Rankings) includes rankings for more than 1,500 universities; QS World University Rankings 2023: Top Global Universities[114] (QS Rankings) includes rankings for more than 1,400 universities; and Global 2000 for World University Rankings, 2022-23 Edition from CWUR[115] (CWUR Rankings) includes rankings for 2,000 universities. Second, I create dummy variables for the most common schools in the data. The rankings measures provide a measure of school quality for a very large number of schools in the data. However, there are some schools from which a large number of Disney hires come, but which do not show up in the rankings. For instance, [redacted]

[113] World University Rankings 2023, The Higher Education, https://www.timeshighereducation.com/world-university-rankings/2023/world-ranking.

[114] QS World University Rankings 2023: Top Global Universities, QS Top Universities, https://www.topuniversities.com/university-rankings/world-university-rankings/2023.

[115] Global 2000 List by the Center for World University Rankings (2022-23 Edison), CWUR, https://cwur.org/2022-23.php.

[redacted][116] The dummy variables control for differences across these schools.

88. Before matching schools in the Kenexa data with university rankings, I standardize the university names in the rankings datasets. I first standardize names between CWUR and QS, and then I standardize names in the WSJ data to the resultant list. In the first step, I compute text similarity scores using the "matchit" command in Stata for all the possible pairs of universities (i.e., one from CWUR ranking and another from QS ranking). The outputs with text similarity scores above 0.605 yield good quality matches while outputs with scores below 0.605 might yield incorrect matches in the first stage.[117] Therefore, for each university in CWUR, I match it with the university from the QS ranking with the highest text similarity score only if the score is above 0.605. In the second step, I repeat the process using the standardized names from the CWUR/QS match, and the university names from the WSJ ranking.[118] This results in a standardized list of university name with rankings values from each of the three rankings datasets. For those universities that cannot be matched to any schools in a ranking dataset, I set the respective dataset rankings as missing. In addition to creating university rankings, [redacted][119] [redacted] The end result is then a standardized set of university names, which is associated with rankings in each of the three possible rankings used, [redacted].

89. I then match these to the schools listed in the Kenexa data. I first compute the text similarity scores for all the possible pairs of universities (i.e., one from the Kenexa education

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[116] There are employees whose educational institution could be associated with multiple distinct universities. One prominent example is "University of California," which accounts for over 4% of employees. I assign a fixed effect for each such ambiguous institution, but no ranking. (A far larger number of records indicates a specific University of California campus.)

[117] I reviewed 20 match outputs above similarity text score of 0.605. They generally yield good quality matches. I also reviewed 20 match outputs below 0.605, in which the incidence of incorrect matches increases substantially. I use a similar approach for the thresholds used for the additional matches described below.

[118] The difference in universities included in the WSJ rankings and the CWUR and QS rankings appears to be greater than the difference between the CWUR and QS rankings. Thus, a more stringent threshold of 0.8 is used in this second step.

[119] *DISNEY-000033441* [redacted] at 449.

data and another from the standardized university rankings). For each university in the Kenexa education data, I match it with the university from the ranking output with the highest text similarity score only if the score is above 0.85. Because the university names in the Kenexa education data tend to be more variable than the listings in the rankings, it is more conservative to set the similarity score higher to make sure the university name matched to is more likely to be correct. The next step is to look at the distribution of clean university names that result, to review the top 50 universities in terms of the number of times they appear, and then to manually clean the university names in the Kenexa Education data that should belong to these top universities, but are missed or incorrectly labeled in the prior steps. No further name standardization is performed for the remaining institutions, as they have low representation among employees and hence are less likely to have a material effect on employee compensation.

90. Finally, using university names, the rankings are merged to the schools in the Kenexa data. Given the steps outlined above, there are some schools in the Kenexa data that cannot be reliably merged to the rankings. For these cases, the rankings are coded as missing.

91. As noted above, I also include dummy variables for many separate schools. Only schools with at least 10 employees are included in the starting salary analysis, and only schools with at least 10 employee-years are included in the class compensation analysis. Any schools not included in the analysis become a part of the omitted school category.[120]

92. Between the two methods of capturing schools, my coverage is quite thorough, either through capturing school-specific differences with the dummy variables, or measures of school quality with the rankings. These can provide information on the quality of students.[121] For 60% of employee degrees the degree-granting institution has at least one ranking and a dummy variable; for 5% it has at least one ranking but no frequency dummy; for 19% it does not have a ranking but it has a dummy variable; and for only 17% is there neither.

[120] However, those who went to college vs. not will still be distinguished by the degree dummy variables.

[121] Some labor economics research uses information on the quality/ranking of the college or university to capture additional information on the abilities of the student. See, e.g.: Brewer, Dominic J., Eric R. Eide, and Ronald G. Ehrenberg. 1999. "Does It Pay to Attend an Elite Private College? Cross-Cohort Evidence on the Effects of College Type on Earnings." *Journal of Human Resources*, Vol. 34, pp. 104-123.

93. I also use the degree information in the Kenexa Education data. The entries have to be cleaned to make uniform degrees that are the same but are represented in different ways in the data. (There are initially about [redacted].) After doing so, I classify them as follows: High School Degree; Certificate; Associate degree; Bachelor's Degree; Master's Degree; Doctorate Degree (includes law). I include dummy variables for each of these degrees.

94. In addition to schools and degrees, I identified over [redacted] of applicants' field of study, which I sorted into categories with a sufficient number of observations (as described below) indicated by separate identifier variables. Since an individual may have a degree associated with multiple fields of study, one education background could be associated with multiple field of study identifiers. If an employee studied Business Administration and Marketing, her indicator variables for both Business and Marketing are activated. If a field of study cannot be classified into at least one of these categories, it is assigned to the category "Other." Only fields of study categories with at least 10 employees are separately included in the starting salary analysis ([redacted]), and only fields of study categories with at least 10 employee-years are separately included in the class compensation analysis ([redacted]). Any categories not included in the analysis become a part of the omitted field of study category.

95. I characterize each individual's education in terms of their highest degree. For the Class Period pay analysis I track degrees throughout an employee's tenure at Disney and update them by year if appropriate. For the starting pay analysis described below I use the degrees as of the start date at Disney.

96. I also use the Kenexa data to construct an actual prior experience variable, rather than just using an approximation based on assuming people left school at age 22 and then worked continuously. The Kenexa data lists prior job titles and employers, as well as starting and ending years of each job, and an indicator for the most recent job.[122] If end year is missing, I assume the job continued to the hire year.[123] This gives me a list of jobs with start and end years. I accumulate the time periods covered by these jobs but removing double counting. Specifically, I

[122] There is only one record (one job title) where we cannot ascertain the start year. That record is excluded. I also track prior employer so that all spells of employment at Disney go into the Disney tenure measure.

[123] I only have information on years of job spells, not months or weeks.

remove any portion of prior experience that overlaps with stints with Disney, and I avoid double counting of time in other jobs that appears to be in the same period.

97. The results of my analysis are reported in Table 4. The estimates in rows B-D report the results using the Kenexa data. To make the comparison with Table 3 easier, row A reports the estimates from that table. Note that the estimates in rows B-D are based on [redacted] observations, as compared to [redacted] observations for the full Class Period analysis in Table 3. This echoes what I said earlier; the Kenexa data are generally available only for fairly recent hires (and even then, are sometimes missing). Thus, the value of these data is not in getting the most reliable estimate of the gender gap in pay, but rather of assessing the impact of adding more detailed control variables.

98. To that end, in row B, I report estimates of the exact same specification as in Table 3 (i.e., without adding any details from Kenexa data), but for the subsample of the data for which Kenexa data are available. These estimates are smaller than those in Table 3, but this is not consequential, given the small share of the sample used.[124] In row C, I add the data on prior experience and degrees. This specification parallels the kinds of wage equations commonly estimated in the labor economics literature, where one controls for level of school and actual experience. Then in row D, I add the more detailed information on the school rankings and fields of study. The key results are the differences between row C and D, and row B. In every case, the estimated gender gap – the pay penalty for women – is *larger* when the detailed education and experience controls are added. For example, I already noted that Model II is my preferred specification for comparing pay between similar women and men. In row C, relative to row B, the estimated female pay penalty increases sharply, to 1.65% (5.1 standard deviations). And in row D, the estimate compared to row B is also much larger – 1.48% (4.6 standard deviations) compared to 0.93%.

[124] Nonetheless, the smaller estimate for the subsample with Kenexa data is consistent with findings reported later that towards the end of the class period, after Disney stopped relying on prior pay to set starting pay, the estimated female pay penalty in starting pay is no longer significant; the Kenexa data come predominantly from individuals hired in this latter period.

Table 4: Regression Model of Gender Disparity in Compensation (Log (Base Salary + Lump Sum)) at Disney, Restricted to Employees with Applicant Data in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents), 2015-2022 (Rows B-D are Same Analysis as Table 3, but with Kenexa Subsample and Adding Kenexa Controls)

	(I) Baseline			(II) Baseline + Segment		
	Coeff.	t-stat	p-value	Coeff.	t-stat	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
A. Full SAP Sample Controlling for Potential non-Disney Experience (Sq.) (Table 3)						
Female	**-0.0206**	**-9.2915**	**0.0000**	**-0.0201**	**-9.1600**	**0.0000**
B. Kenexa Sample Controlling for Potential non-Disney Experience (Sq.)						
Female	**-0.0098**	**-3.1277**	**0.0018**	**-0.0093**	**-2.9786**	**0.0029**
C. Kenexa Sample Controlling for Prior Experience (Sq.), Degree						
Female	**-0.0172**	**-5.2674**	**0.0000**	**-0.0165**	**-5.1087**	**0.0000**
D. Kenexa Sample Controlling for Prior Experience (Sq.) and All Education Variables						
Female	**-0.0155**	**-4.7942**	**0.0000**	**-0.0148**	**-4.6388**	**0.0000**
Variables						
Tenure at Disney (Sq.)	Yes			Yes		
Exempt Status	Yes			Yes		
Hourly	Yes			Yes		
Technology Job Indicator	Yes			Yes		
Southern California Indicator	Yes			Yes		
Southern California Indicator X Technology Job Indicator	Yes			Yes		
Northern California Indicator	Yes			Yes		
Northern California Indicator X Technology Job Indicator	Yes			Yes		
Year dummy variables	Yes (8)			Yes (8)		
Job Family X Job Level dummy variables	Yes (2476)			Yes (2476)		
Segment dummy variables	No			Yes (13)		
<u>*Missing Education Controls*</u>						
Employee-years with Missing Highest Degree CWUR Ranking	[redacted]			[redacted]		
Employee-years with Missing Highest Degree QS Ranking						

Employee-years with Missing Highest Degree WSJ Ranking	[redacted]	[redacted]
Employee-years Missing Highest Degree All Rankings		
Observations	[redacted]	[redacted]
R-squared		
Controlling for Age Hired by Disney Minus 22 (Sq.)	0.9056	0.9072
Controlling for Prior Experience (Sq.), Degree	0.8985	0.9004
Controlling for Prior Experience (Sq.), Education	0.9063	0.9076

Source: SAP analysis data set; Kenexa Applicant Data.

Notes: Observations defined at the employee-year level. Standard errors are clustered at the employee level. Analysis is restricted to full-time, nonunion employees in specified job levels, in California. If job family is missing I include an interaction between job level dummy variables and a dummy variable for missing job family. I include a dummy variable for observations missing exempt status [redacted] Education controls are included for the highest degree earned by an individual as reported in their CVs and recorded in the Kenexa data. Degree controls consist of [redacted] Education controls include degree controls, as well as: i. University rankings as reported by CWUR, QS, and WSJ, and indicators of missing rank for each source. Controls for the highest degrees are included in the analysis; ii. Indicators for highest degree fields of study, for fields of study with 10 or more employee-years; iii. Dummies for schools with 10 or more employee-years. When the t-statistic exceeds 1.96, the estimate is significantly different from zero at the 5% level or less, so the p-value would be 0.05. See the table on p. 26 for other examples.

99. The increases in the estimated female pay penalties after I incorporate the education and experience data occur because (conditional on the other controls in the model), on standard measures of these new controls, the productivity-related factors overall favor women (driven by women having higher schooling); hence controlling for these factors increases the female pay penalty. I cannot estimate the gender pay gap for the full sample in Table 3 while also controlling fully for education and prior experience. But the implication of the analysis in Table 4 is that if I could, the estimated gender pay gap would be larger than in Table 3. I cannot definitively say by how much, but as a rough estimate, we might add the relative difference seen in Table 4, row D vs. row B. In that case, the estimated female pay penalty corresponding to Model II – i.e., for comparable women and men doing substantially similar work – would be 2.56% (2.01% + (1.48% − 0.93%)). This, in turn, would imply larger damages.

100. Even with these controls, and even within similar jobs, workers may differ in productivity and performance, and that could affect pay. The performance data available to me in this case are very limited. In particular, I only have numerical ratings for 2015-16, because after that Disney stopped using numerical ratings.[125] Subsequent to that, Disney provided evaluations of employees in qualitative form. While Disney's annual compensation planning process still called for managers to identify a small group of "top performers," and give them larger raises than a large group doing average or above average work, and a small group of low performers, there was no data clearly recording which category an employee was assigned to. I was given information with text descriptions intended to capture these performance groupings, but it did not prove possible to reliably identify the top performers from this text.[126] First, the notes were

[125] Burnley Dep. at 132:24 – 133:4 (DPEP stopped using performance ratings around 2015); Fox Dep. at 213:19 – 214:11 (ratings were phased out at Studios around 2016, or early 2017); Olsgaard Dep. at 85:15 – 86:22 (ratings were not used at DMED, which was formed in 2020, or DTCI, which was formed in 2018); Anderson Dep. at 245:14-17 (performance ratings were not used at DMED).

[126] Information on those classified as top performers was extracted and constructed from the files *DISNEY-000038507-519* and *DISNEY-000040911-940*. This information is not systematically recorded in Disney's centralized data, but instead was captured in text descriptions (Bacon Dep. at 186:20 – 187:11). [redacted]

highly variable and not standardized. For instance, I could not reliably differentiate positive but mediocre assessments from the absence of notes. [redacted] which do not necessarily indicate top talent. Second, the notes often represent mixed evaluations. An identical descriptive attribute might be used differently across reviewers in evaluating an employee depending on context. [redacted][127]

101. Defendant also produced a set of data including employees with top talent mostly for succession planning purposes.[128] However, it appears to be far from comprehensive. [redacted]

102. I therefore show results using the 2015-16 data when clear performance data is available.[129] Like with the Kenexa data, because I only have performance ratings for a small part

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[redacted] Janet Burnley, in her deposition, testified to the policy of giving higher raises to top performers (Burnley Dep. at 222:20-25). She also indicated that a comment should be entered in Success Factors (the data provided to me) if someone was getting a zero merit increase (Burnley Dep. at 251:20-23).

[127] For specific examples, see, e.g., *DISNEY-000038513, DISNEY-000038518, DISNEY-000040927,* and *DISNEY-000038510.*

[128] *DISNEY-000044007-021*.

[129] Performance Ratings are stored in *DISNEY-000031374* and provided for every employee and performance review period (indicated by start and end date). [redacted] Thus, I match performance metrics to compensation based on the end year of the performance period. [redacted] These were excluded here, as are a handful of observations in 2017 or later with completed performance reviews. I also performed the analysis described just below including the [redacted] The results are not materially different (see Table E.4 in Appendix E).

of the sample used for the Class Period analysis, the results are more informative about the consequences of controlling for performance ratings than about what the overall female pay penalty is in the Class Period. In particular, of the [redacted] observations in Table 3 – for Covered Workers in the Class Period observations – I have performance ratings in 2015-16 for only [redacted] observations, just over 60% of all Class employee-years in 2015-2016, [redacted].

103. The results are reported in Table 5. In this case, the estimated female pay penalty is larger than for the full sample. For example, in Model II, the estimated penalty rises from 2.01% to 2.82% (7.3 standard deviations).[130] When I add the performance rating controls, the estimate becomes slightly larger (2.93%, 7.7 standard deviations). Similarly in the other column the estimated female pay penalty grows slightly when performance ratings are added. The implication of this is that women get higher performance ratings, conditional on all the other controls in the model.

104. In other words, the evidence in Table 5 suggests that if I could control for performance ratings for the full sample of Covered Workers in the Class Period, I would estimate a slightly larger female pay penalty than what is shown in Table 3. I cannot definitively say by how much, but as a rough estimate, we might add the relative difference seen in Table 5, row C vs. row B. In that case, the estimated female pay penalty corresponding to Model 3 – i.e., for comparable women and men who are similarly situated – would be 2.12% (2.01% + (2.93% − 2.82%)), which would imply larger damages.

[130] The fact that the estimated female pay penalty is larger using the 2015-16 data is consistent with results reported later that the gender gap in starting pay declined after October 2017.

Table 5: Regression Model of Gender Disparity in Compensation (Log (Base Salary + Lump Sum)) at Disney, Restricted to Employees with Applicant Data in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents), 2015-2016

		(I) Baseline			(II) Baseline + Segment		
	VARIABLES	Coeff.	t-stat	p-value	Coeff.	t-stat	p-value
		(1)	(2)	(3)	(4)	(5)	(6)
A.	*Full SAP Sample*						
	Female	**-0.0206**	**-9.2915**	**0.0000**	**-0.0201**	**-9.1600**	**0.0000**
B.	*Performance Rating Sample*						
	Female	**-0.0287**	**-7.3066**	**0.0000**	**-0.0282**	**-7.2992**	**0.0000**
C.	*Performance Rating Sample Controlling for Performance Ratings*						
	Female	**-0.0293**	**-7.5637**	**0.0000**	**-0.0293**	**-7.6649**	**0.0000**
	Variables						
	Potential non-Disney Experience (Sq.)	Yes			Yes		
	Tenure at Disney (Sq.)	Yes			Yes		
	Exempt Status	Yes			Yes		
	Hourly	Yes			Yes		
	Technology Job Indicator	Yes			Yes		
	Southern California Indicator	Yes			Yes		
	Southern California Indicator X Technology Job Indicator	Yes			Yes		
	Northern California Indicator	Yes			Yes		
	Northern California Indicator X Technology Job Indicator	Yes			Yes		
	Year dummy variables	Yes (2)			Yes (2)		
	Job Family X Job Level dummy variables	Yes (443)			Yes (443)		
	Segment dummy variables	No			Yes (9)		
	Observations	[redacted]			[redacted]		
	R-squared						
	Performance Rating Sample	0.8512			0.8553		
	Performance Rating Sample Controlling for Performance Ratings	0.8552			0.8590		

Source: SAP analysis data set.

Notes: See notes to Table 3. The differences are the inclusion of the performance ratings data and restricting the sample to 2015-2016. [redacted] When the t-statistic exceeds 1.96, the estimate is significantly different from zero at the 5% level or less, so the p-value would be 0.05. See the table on p. 26 for other examples.

Analysis of differences in starting salaries

105. The preceding results indicate that women are paid less than similarly-situated men at Disney. In this section, I report analyses of starting pay. If prior pay reflects gender discrimination in pay in the labor market in general, then basing starting pay in part on prior pay would be expected to replicate that general labor market discrimination in starting pay. And there is certainly evidence consistent with the gender pay gap in the U.S. labor market partly reflecting discrimination against women.[131]

106. There is ample evidence that Disney used prior pay in determining starting pay. [redacted] *Ex. 704* at 24356. [redacted] See, e.g., *Ex. 816* at 862-63; *Ex. 768* at 31206.

107. Several witnesses also testified that, prior to the 2017/2018 policy change, recruiters were allowed to ask candidates for prior/current salary information, which could be shared with Compensation for purposes of creating an offer. See Watkins Dep. at 51:4-10 ("Prior to 2018, we could send a candidate's prior pay information [to Compensation]."); Wahab 27:14 – 29:22; 66:12 – 67:3 (information shared with Compensation could include current salary information); Hirst 27:18 – 28:7 ("Back to 2015 through 2018, there could have been information shared in terms of current pay."); Schultz Dep. at 96:2-19 (recruiters "had the ability [to ask for a candidate's prior pay information] if [they] chose to do so"); Weirick Dep. at 45:3-10, 62:14-20 (before 2017, recruiters were never instructed to not ask about current or prior salary); Larson Dep. at 392:8-15 ("I was aware that on occasion….[recruiters] would" ask candidates about prior pay).

108. Additionally, [redacted]

[131] See, e.g.: Bayard et al. 2003. "New Evidence on Sex Segregation and Sex Differences in Wages from Matched Employer-Employee Data." *Journal of Labor Economics*, Vol. 21, pp. 887-922; 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; Blau, Francine D., and Lawrence M. Kahn. 2007. "The Gender Pay Gap: Have Women Gone as Far as They Can?" *Academy of Management Perspectives*, Vol. 21, pp. 7-23.

[redacted] *Ex. 689* at 5535-36 [redacted] Ryan Schultz testified that the TACOE tool asked for a candidate's current salary and that he was not aware of any direction that filling in that information was optional. Schultz Dep. at 53:2-17. Schultz could also not recall a single instance where a recruiter obtained information about a candidate's current salary or their salary expectation but chose not to use it or enter it in the TACOE tool. Schultz Dep. at 82:8-12.

109. [redacted] See *Ex. 704* at 24361 [redacted]; *Ex. 599* (Kenexa form for recording salary information); Anderson Dep. at 173:2-21; Brahm Dep. at 68:14-24; Weirick Dep. at 49:17-50:21, 53:22-54:1. [redacted] *Ex. 704* at 24361. [redacted] See *Ex. 600* at 24349-50; *Ex. 771* at 31085 [redacted] Anderson Dep. at 163:5 – 164:4, 172:18 – 173:1; Wahab Dep. at 57:23 – 58:3, 61:14-16; Weirick Dep. at 49:13-16; Pate Dep. at 188:15-22.

110. Disney documents explain that their change in policy in October 2017 was due to the change in California law, [redacted], banning asking about an external candidate's current/prior compensation. It is hard to imagine why Disney would have had to note this policy change and issue the directives about not inquiring about prior pay if Disney was not in fact inquiring about prior pay before the new policy went into effect. [redacted]

111. Moreover, this policy change did not necessarily mean prior pay could no longer play a role in setting starting pay. [redacted]

[redacted][132]

112. [redacted][133] [redacted]"[134] [redacted][135] [redacted]

113. I have a very hard time distinguishing between the latter question and asking about current/prior compensation. One option (illegal in California beginning in 2018) is to ask, "What is your current salary?" Suppose the answer is $70,000. [redacted].[136] Ryan Schultz also indicated that offers could respond to money being left on the table (at 102:21 – 103:2).

114. Moreover, from a labor economics perspective, asking about "expectations" may not be very different from asking about prior pay. Labor economics research suggests that there is a close relationship between pay expectations and prior pay; it is natural that a candidate's salary

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[132] See *13_Ex. 600_DISNEY-000024348.pdf* at 24348. See also *12_Ex. 599_DISNEY-000024580.pdf*, a form eliciting salary expectations for job candidates, and *DISNEY-000024352.pdf* ([redacted]) at 24355. See also *14_Ex. 601_DISNEY-000005482.pdf* ([redacted]) at 5492 and *15_Ex. 602_DISNEY-000031074.pdf* ([redacted]) at 31083.

[133] See *14_Ex. 601_DISNEY-000005482.pdf* ([redacted]) at 54922, and *15_Ex. 602_DISNEY-000031074-1.pdf* ([redacted]) at 31083; *Ex. 599*.

[134] See *15_Ex. 602_DISNEY-000031074-1.pdf* ([redacted]) at 31083.

[135] See *13_Ex. 600_DISNEY-000024348.pdf* at 24348, and *DISNEY-000024352.pdf* at 24357.

[136] Kara Anderson describes the information on "leaving something on the table" in terms of bonuses or equity (Anderson Dep. at 162:19-22). But *DISNEY-000024352.pdf* at 24358 [redacted]

expectations would be based in part on prior pay, as a candidate changing jobs typically does this to receive a raise.[137]

115. Similarly, Kelly Weirick testified that prior to 2017, Disney did not have a policy prohibiting recruiters from asking about prior pay, Disney adopted a policy in October 2017 that recruiters could no longer ask about prior pay, but at the same time the policy said that candidates could be asked about salary expectations.[138] Moreover, she testified, referencing *Ex. 600*, that after that policy change, if a candidate volunteered their current salary, this information was recorded by Talent Acquisition (at 49:17 – 50:18, 63:4-11).[139] She also testified that in making salary offer recommendations, Compensation relies on, among other things, salary expectations (at 51:1-7), and that discussing salary expectations is how they determine if a job candidate in the pay range for a job (at 56:3-7, and 57:17-20). Mezhgan Wahab also confirmed this (at 61:11-20). And referencing prior pay in the earlier period, Mezhgan Wahab testified that recruiters could ask about prior pay (at 66:12 – 67:1).

116. [redacted][140] While this may comply with the law regarding asking about prior pay, it does not mean prior pay is no longer being used in setting starting pay.

117. Overall, there are clearly statements that Disney did use prior pay in setting starting, although there is also testimony to the contrary. Thus, the best I can conclude from the testimony (and documents) is that it is certainly plausible that Disney relied on prior pay. However, especially given the contradictory testimony, I rely more heavily on empirical evidence on starting pay, and in particular how the gender gap in starting pay changed after the Disney policy change in October 2017 to stop asking about prior pay, and to instead ask about salary expectations. While I would expect salary expectations to reflect prior pay in part, it can also be a noisier measure of prior pay, because it is less explicitly about prior pay. Moreover, one might

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[137] *See* the evidence of wage growth with job changes in the seminal paper: Topel, Robert H., and Michael P. Ward. 1992. "Job Mobility and the Careers of Young Men." *Quarterly Journal of Economics*, Vol. 107, pp. 439-79.

[138] Weirick Dep. at 49:4-15.

[139] See similar evidence in Wahab Dep. at 59:10 – 60:5.

[140] See *13 Ex. 600 DISNEY-000024348.pdf* at 24350. And *DISNEY-000024352.pdf* at 24361 [redacted]

reasonably have expected Disney to be more cautious in tying starting pay to prior pay (even if measured by salary expectations) after the statewide ban on asking about salary history took effect. I thus next turn to this empirical evidence.

118. Based on the preceding evidence, I analyze the impact of gender on starting pay in two broad steps. I first document that there is a large and statistically significant female penalty in starting pay, for similarly-situated women and men.[141] In this analysis, I use regression models very similar to those discussed in my Class Period pay analysis. Second, I contrast results for the period through October 2017, and after. When I do this, I find that the female penalty in starting pay was much higher in the prior period, which is consistent with a greater reliance on prior pay in the period before the policy change that barred explicitly asking about prior pay.

119. The baseline starting pay analysis is presented in Table 6. For this analysis, I consider those who ever worked in the Covered Positions during the Class Period, and I also restrict attention to those starting at the parts of Disney I include (see the earlier discussion about excluded units). I also focus on base salary only, which is most relevant for starting pay. The regression I report controls for potential experience; there is no control for tenure at Disney since here I am studying starting pay. Additionally, as some class members started their careers as union employees or contractors, I also include controls for those employee types. The other control variables are the same as in Table 3. All models control for the job family and job level in which people begin working at Disney, although note that Disney developed more detailed classifications of jobs towards the later years of the data. As a result, when I use starting pay all the way back to 2002, for many of the earlier observations jobs are not distinguished by job levels.

[141] I am unable to estimate prior pay regressions to compare to the starting pay regressions because prior pay information is only provided by Disney for the period October 2017 through July 2022. This coincides with the change in California state policy dictating that employers are not allowed to ask applicants for prior compensation. As such only prior pay from applicants who volunteer such information is available. This limits the prior pay data available for analysis to a level that makes any meaningful analysis impossible.

Table 6: Regression Model of Gender Disparity in Starting Salary (Log Base Salary) at Disney, Employees Ever Employed in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents) During the Class Period, Hired in 2002-2022

	(I) Baseline Model			(II) Baseline Model + Segment		
	Coeff.	t-stat	p-value	Coeff.	t-stat	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Female	**-0.0515**	**-6.2480**	**0.0000**	**-0.0474**	**-5.7977**	**0.0000**
Potential non-Disney Experience (Sq.)	Yes			Yes		
Exempt Status	Yes			Yes		
Hourly, Contractor, and Union	Yes			Yes		
Technology Job Indicator	Yes			Yes		
Southern California Indicator	Yes			Yes		
Southern California Indicator X Technology Job Indicator	Yes			Yes		
Northern California Indicator	Yes			Yes		
Northern California Indicator X Technology Job Indicator	Yes			Yes		
Year dummy variables	Yes (21)			Yes (21)		
Job Family X Job Level dummy variables	Yes (1273)			Yes (1273)		
Segment dummy variables	No			Yes (11)		
Employee-Years with Missing Job Family X Job Level	353			353		
Observations	[redacted]			[redacted]		
R-squared	0.8189			0.8202		

Source: SAP analysis data set.
Notes: Observations defined at the employee-year level. Standard errors are clustered at the employee level. Analysis is restricted to full-time, nonunion employees. If job family is missing I include an interaction between job level dummy variables and a dummy variable for missing job family. And if job level is missing I include an interaction between job family dummy variables and a dummy variable for missing job level. (Note that job level can only be missing in my starting pay regressions, because in the Class Period pay regressions I condition on employment in a specific set of job levels, whereas in my starting pay regression I condition on ever working in one of these job levels.) I include a dummy variable for observations missing exempt status. [redacted]

[redacted] When the t-statistic exceeds 1.96, the estimate is significantly different from zero at the 5% level or less, so the p-value would be 0.05. See the table on p. 26 for other examples.

120. The estimates indicate large and statistically significant female penalties in starting pay, for comparable women and men. For example, in Model II, which also controls for segment, the estimated penalty is 4.74% (5.8 standard deviations).

121. Table 7 restricts the sample period in two ways (all models in this table correspond to Model II from Table 6). First, I begin the data only in 2015, to correspond to the Class Period, as well as the completion of the global job leveling project.[142] One implication of shortening the sample period is I have a far greater share of observations with job family and job level defined. Between 2002 and 2022 [redacted] Further, the Kenexa applicant data provided by Disney was only produced for applications submitted starting April 2015.[143] As such, the vast majority of available data is only relevant to employees first hired at Disney in 2015 or later.[144] When I limit to the Class Period, the estimated female penalty in starting pay remains large and strongly statistically significant. The estimated penalty is 2.81% (2.9 standard deviations).[145]

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[142] *Ex. 690* at 5798 ([redacted]

[143] Email from Besnoff, Sarah G., Re: Disney data questions, April 6, 2023.

[144] The Kenexa applicant data covers rehires, so it also contains data for employees originally hired before 2015. However, [redacted].

[145] Note that the 2.81% female starting pay penalty for the 2015-2022 period is closer to the estimated class period pay penalty from Table 3 (2.01%). Given the results documented below – that the gender gap in starting pay after Disney no longer asks about prior pay is much smaller, this change relative to the larger estimate for 2002-2022 makes sense, because [redacted]. In addition, however, Disney did not have job levels in the earlier period, and typically had job function without the more specific family, so the starting pay estimates extending back to 2002 effectively include a longer period with controls for job function, not job family-job level pairs.

Table 7: Regression Model of Gender Disparity in Starting Salary (Log Base Salary) at Disney, Employees Ever Employed in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents) During the Class Period

	(I) Hired in 2002 - 2022			(II) Hired in 2015 - 2022			(III) Hired in 2015 - Oct 2017			(IV) Hired in Nov 2017 - 2022		
	Coeff.	t-stat	p-value	Coeff.	t-stat	p-value	Coeff.	t-stat	p-value	Coeff.	t-stat	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variables												
Female	**-0.0474**	**-5.7977**	**0.0000**	**-0.0281**	**-2.9196**	**0.0035**	**-0.0436**	**-2.7413**	**0.0062**	**-0.0134**	**-1.1820**	**0.2373**
Potential non-Disney Experience (Sq.)	Yes			Yes			Yes			Yes		
Exempt Status	Yes			Yes			Yes			Yes		
Hourly, Contractor, and Union	Yes			Yes			Yes			Yes		
Technology Job Indicator	Yes			Yes			No			Yes		
Southern California Indicator	Yes			Yes			Yes			Yes		
Southern California Indicator X Technology Job Indicator	Yes			Yes			No			Yes		
Northern California Indicator	Yes			Yes			Yes			Yes		
Northern California Indicator X Technology Job Indicator	Yes			Yes			No			Yes		
Year dummy variables	Yes (21)			Yes (8)			Yes (3)			Yes (6)		
Job Family X Job Level dummy variables	Yes (1273)			Yes (1257)			Yes (392)			Yes (1158)		
Segment dummy variables	Yes (11)			Yes (11)			Yes (8)			Yes (10)		
Employee-Years with Missing Job Family X Job Level	353			108			91			17		
Observations	[redacted]			[redacted]			[redacted]			[redacted]		
R-squared	0.8202			0.7543			0.7772			0.7576		

Source: SAP analysis data set.

Notes: Observations defined at the employee-year level. Standard errors are clustered at the employee level. Analysis is restricted to full-time, nonunion employees. If job family is missing I include an interaction between job level dummy variables and a dummy variable for missing job family. And if job level is missing I include an interaction between job family dummy variables and a dummy variable for missing job level. (Note that job level can only be missing in my starting pay regressions, because in the Class Period pay regressions I condition on employment in a specific set of job levels, whereas in my starting pay regression I condition on ever working in one of these job levels.) I include a dummy variable for observations missing exempt status. [redacted]

[redacted]
When the t-statistic exceeds 1.96, the estimate is significantly different from zero at the 5% level or less, so the p-value would be 0.05. See the table on p. 26 for other examples.

122. The second step is to split the Class Period sample to go through October 2017, and then after. The first period corresponds to the period when Disney could (and much evidence suggests did) ask about prior pay and it could influence starting pay. The second period follows the policy change discussed above, when it is possible that the link between starting pay and prior pay would have weakened. The evidence is fully consistent with this. For the early period (2015 through October 2017), the estimated female penalty in starting pay is 4.36% (2.7 standard deviations). But for the latter period, the estimated female penalty in starting pay, while still adverse to women (1.34%), is not statistically significant at the 5% level (1.2 standard deviations).

123. Recall that for my Class Period pay analysis, I also reported results for a subset of the observations for which I had application data from Kenexa. In the discussion of that analysis, I explained the data I could extract and construct from Kenexa, including degrees, field of study, schools, and prior experience. Table 8 reports estimates of the same starting pay regressions, for the same periods, as Table 7, but now exploring the effects of adding these richer control variables. Note that in this case, because I am restricting attention to those who started in 2015 or after, I have Kenexa data for a somewhat larger share of observations ([redacted], or 58%). Still, I view this evidence largely as informing us about the effect of adding the richer control variables, not as providing the best, most representative estimate of the gender gap in starting pay.[146]

[146] I have salary expectations data for an even smaller subset of the observations, and only for November 2017 and after. Per my opinion that these subsamples are useful only for assessing the bias from adding richer control variables, but not for obtaining representative estimates of gender pay gaps, I do not report estimates of gender differences in salary expectations. In this case, I would have only slightly over [redacted] observations. And I have the richer control variables both before and after November 2017, while any expectations data is only from November 2017.

Table 8: Regression Model of Gender Disparity in Starting Salary (Log Base Salary) at Disney, Employees Ever Employed in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents) During the Class Period (Rows B-D are Same Analysis as Table 7, but with Kenexa Subsample and Adding Kenexa Controls)

		(I) Hired in 2015 - 2022			(II) Hired in 2015 - Oct 2017			(III) Hired in Nov 2017 - 2022		
		Coeff.	t-stat	p-value	Coeff.	t-stat	p-value	Coeff.	t-stat	p-value
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A.	*Full SAP Sample Controlling for Potential non-Disney Experience (Sq.) (Table 7)*									
	Female	**-0.0281**	**-2.9196**	**0.0035**	**-0.0436**	**-2.7413**	**0.0062**	**-0.0134**	**-1.1820**	**0.2373**
B.	*Kenexa Sample Controlling for Potential non-Disney Experience (Sq.)*									
	Female	**-0.0275**	**-2.3324**	**0.0197**	**-0.0398**	**-1.5384**	**0.1241**	**-0.0156**	**-1.3373**	**0.1812**
C.	*Kenexa Sample Controlling for Prior Experience (Sq.) and Degree*									
	Female	**-0.0401**	**-3.3051**	**0.0010**	**-0.0600**	**-2.2156**	**0.0268**	**-0.0238**	**-1.9839**	**0.0473**
D.	*Kenexa Sample Controlling for Prior Experience (Sq.) and All Education Variables*									
	Female	**-0.0311**	**-3.0871**	**0.0020**	**-0.0548**	**-2.5844**	**0.0098**	**-0.0183**	**-1.6481**	**0.0994**
	Variables									
	Exempt Status	Yes			Yes			Yes		
	Hourly, Contractor, and Union	Yes			Yes			Yes		
	Technology Job Indicator	Yes			No			Yes		
	Southern California Indicator	Yes			Yes			Yes		
	Southern California Indicator X Technology Job Indicator	Yes			No			Yes		
	Northern California Indicator	Yes			Yes			Yes		
	Northern California Indicator X Technology Job Indicator	Yes			No			Yes		
	Year dummy variables	Yes (8)			Yes (3)			Yes (6)		
	Job Family X Job Level dummy variables	Yes (999)			Yes (311)			Yes (909)		
	Segment dummy variables	Yes (11)			Yes (8)			Yes (10)		
	Employee-Years with Missing Job Family X Job Level	52			44			8		
Missing Education Controls										
	Employees with Missing Highest Degree CWUR Ranking	1,909			646			1,263		
	Employees with Missing Highest Degree QS Ranking	2,513			846			1,667		
	Employees with Missing Highest Degree WSJ Ranking	2,760			933			1,827		
	Employees with Missing Highest Degree All Rankings	1,844			630			1,214		
	Observations	[redacted]			[redacted]			[redacted]		

R-squared			
Controlling for Potential Experience (Sq.)	0.8134	0.7829	0.8424
Controlling for Prior Experience (Sq.) and Degree	0.8079	0.7712	0.8398
Controlling for Prior Experience (Sq.) and Education	0.8213	0.8135	0.8504

Source: SAP analysis data set; Kenexa Applicant Data.
Notes: Observations defined at the employee-year level. Standard errors are clustered at the employee level. Analysis is restricted to full-time, nonunion employees. If job family is missing I include an interaction between job level dummy variables and a dummy variable for missing job family. And if job level is missing I include an interaction between job family dummy variables and a dummy variable for missing job level. (Note that job level can only be missing in my starting pay regressions, because in the Class Period pay regressions I condition on employment in a specific set of job levels, whereas in my starting pay regression I condition on ever working in one of these job levels.) I include a dummy variable for observations missing exempt status. [redacted] Education controls are included for the highest degree earned by an individual as reported in their CVs and recorded in the Kenexa data. Degree controls consist of [redacted] Education controls include degree controls, as well as: i. University rankings as reported by CWUR, QS, and WSJ, and indicators of missing rank for each source. Controls for the highest degrees are included in the analysis; ii. Indicators for highest degree fields of study, for fields of study with 10 or more employees; iii. Dummies for schools with 10 or more employees. When the t-statistic exceeds 1.96, the estimate is significantly different from zero at the 5% level or less, so the p-value would be 0.05. See the table on p. 26 for other examples.

124. In row B of Table 8, I report the estimates using the same models and periods as in Table 7, but simply for the subsample of observations for which I have Kenexa data. This provides a baseline for seeing the effects of adding the richer control variables. These results are not qualitatively different from the full sample. For the full period, the estimated female penalty in starting pay is 2.75% (2.3 standard deviations). For the early period (2015 through October 2017), the estimated female penalty in starting pay is larger, at 3.98% though no longer statistically significant at the 5% level (1.5 standard deviations). For the later period, the estimated female penalty in starting pay, while still adverse to women (1.56%), it is also not statistically significant at the 5% level (1.3 standard deviations). In row C, I swap prior experience for potential experience and add degree information, and in row D I add the remainder of the education variables. In every case, the estimated female pay penalty *increases*. For example, using row D, which has the most controls, the estimated female pay penalty for the full period is 3.11% (3.1 standard deviations). For the early period (2015 through October 2017), the estimated female penalty in starting pay is larger, at 5.48% (2.6 standard deviations). For the latter period, the estimated female penalty in starting pay is smaller, as in the earlier analysis. However, it is statistically significant at the 10% level – a female penalty in starting pay of

1.83% (1.6 standard deviations).[147]

125. Note, however, that the key issue for which I use the Kenexa data is not to test the statistical significance of the starting pay gap in this subsample of data, but rather to gauge the potential impact of differences between women and men that are not captured in the SAP data. There are two implications of the analysis in Table 8. First – in relation to this key question – on

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[147] While sometimes the dichotomy "statistically significant or not" is used to summarize whether an estimate is significantly different from zero at the 5% level or not, this is not ideal statistical practice. The significance level *does* tell us how much confidence we should have that the true value we are estimating is different from zero. But economists often report results significant at the 10% level (or more generally simply report estimates and what the p-value is, so that the "reader" can assess how strongly to be convinced by the evidence). For examples of reporting results at the 10% significance level from my own work, see: Neumark, Burn, and Button. 2019. "Is It Harder for Older Workers to Find Jobs? New and Improved Evidence from a Field Experiment." *Journal of Political Economy*, Vol. 2, pp. 922-70 (Tables 5, 6, 7, and 8); Neumark et al. 2019. "Do State Laws Protecting Older Workers from Discrimination Reduce Age Discrimination in Hiring? Evidence from a Field Experiment." *Journal of Law & Economics*, Vol. 62, 373-402 (Tables 5 and 8); Neumark and Rich. 2019. "Do Field Experiments on Labor and Housing Markets Overstate Discrimination? A Re-Examination of the Evidence." *ILR Review*, Vol. 72, pp. 223-52 (Table 2B (column 7). Here are some other examples: "For the two exceptions, percentage nonwhite has a positive and significant effect at a 10 percent level." Significance levels of 10 percent are also noted in Tables 2 and 3 of that paper. (Chiswick. 1973. "Racial Discrimination in the Labor Market: A Test of Alternative Hypotheses." *Journal of Political Economy*, Vol. 81, pp. 1330-352. See p. 1342.); "When the natural logarithm of WGLOS, the ratio of weekly wage in January 1984 to weekly wage in January 1983, is regressed on gender and all the other variables described above except for WKSNOJOB and SEXCOMP as indicated by equation (1), the coefficient for FEMALE is −0.106 with a standard error of .066, indicating that women displaced workers of equivalent age, education, industry, occupation, location and wage in 1983 to displaced men workers experience a 10.6 percent greater loss in wage growth between 1983 and 1984." (Madden. 1987. "Gender Differences in the Cost of Displacement: An Empirical Test of Discrimination in the Labor Market." *American Economic Review*, Vol. 77, pp. 246-51. See pp. 249-250. Note that in this case the significance level is actually slightly higher than 10%.); "A negative estimate for β_1 is found in all specifications, and statistical significance at the ten-percent level (on a one-tailed test) is achieved in each specification." (DeVaro et al. 2018. "Job Characteristics and Labor Market Discrimination in Promotions. *Industrial Relations*, Vol. 57, pp. 389-434. See pp. 411-12.); Nunley et al. (2015) include indicators for significance at the 10% level in Tables 5, 7, and 8, and discuss statistical significance of the results at the 10% level. (Nunley et al. 2015. "Racial Discrimination in the Labor Market for Recent College Graduates: Evidence from a Field Experiment." *BE Journal of Economic Analysis & Policy*, Vol. 15, pp. 1093-125.); "The equations for handicapped and disabled men, as presented in this article, are significantly different at the 10 percent level." (Baldwin and Johnson. 1994. "Labor Market Discrimination against Men with Disabilities." *Journal of Human Resources*, Vol. 29, pp. 1-19. See p. 6.).

what are likely key missing variables in the full-sample analysis in Table 7, women overall have higher values of the characteristics (like education) that positively affect pay – as we saw before in the Class Period annual pay analysis. Second, although the evidence comes from only a subsample, the results for November 2017 through 2022 indicate that women still experienced a penalty in starting pay that was significant at the 10% level, perhaps because of Disney's continued reliance on salary expectations, or because the small groups of compensation employees in each segment who were setting starting pay were influenced by bias in their decision-making.

126. While these analyses have evaluated the impact of education and prior experience on my analyses, it is possible that some of a new hire's experience in prior jobs could be viewed as more relevant than other experience, and that "relevance" could have impacted starting pay. Janet Burnley refers to trying to capture "applicable experience" for external candidates (at 148:6-22), and indicates that this is not in the human resources data, but comes from the recruitment process (at 148:23 – 149:1). Similarly, she says (at 181:25 – 182:11) "… we might not have initially counted something as relevant experience because of how it was titled or framed up on a resume. But the hiring leader could potentially come with, well, I – you know, in the interview process, I talked to them about this, and here's why it's relevant, and that might be conveyed." Ryan Schultz testified to the TACOE tool (*Ex. 689*) being introduced in 2016-17 (at 29:5-8), and subsequently being rolled out for most jobs (at 38:9-20). He refers to this tool eliciting input on "relevant experience" (at 45:8 – 46:10). He refers to compensation requests prior to use of this tool also referring to experience candidates had (at 91:13 – 92:6). And Kaitlyn Watkins testified to relevant experience being a critical component for starting pay, particularly for Functional Technology roles (at 41:4-18).

127. I have no direct way to measure the relevance of a new hire's prior experience. However, I have used machine learning/computational linguistics tools to compare the similarity of prior job titles to the job family names that people were hired into, and this analysis reveals no difference in the extent to which women's or men's prior job experience was relevant to the job family at hire.[148]

[148] I have used these methods to characterize similarity between other bodies of text in labor market data, in particular between age stereotypes and the text of job adds. See: Burn, Ian,

128. To evaluate the relevance of applicants' prior experiences to the jobs they were hired into, I build three similarity metrics by comparing an employee's prior experiences and their job family at hire at Disney. One input is the job families into which employees were hired.[149] Job family variable sometimes contains abbreviations in addition to specific job titles; to improve the accuracy of similarity scores, abbreviations are expanded. As a few examples, I made the following changes: [redacted] There were some cases where I could not remove abbreviations either because their meaning was unclear, or they were unnecessary since the remaining parts of the job family contained sufficient information.

129. The other input is the prior job titles from the Kenexa data. For each prior job title, on the one hand, and each job family, on the other, I compute the semantic similarity score (which ranges from −1 to 1) using the *word2vec-google-news-300* API,[150] which uses "[p]re-trained vectors trained on a part of the Google News dataset" with about "100 billion words." The pre-trained model allows us to calculate the "semantic similarity" between two set of words.[151]

130. As examples, at the "high" end, the prior job title "software engineer" has a similarity score of 0.94 with the Disney job family "technology software engineer." At the other extreme, the prior job title "project manager" has a similarity score of only 0.11 with the job family "game games-production." To see more cases, I randomly selected 100 pairs of job families and prior job titles.[152] The table below shows, of these 100, the 10 pairs with the highest and the lowest

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Patrick Button, Luis Munguia Corella, and David Neumark. 2022. "Does Ageist Language in Job Ads Predict Discrimination in Hiring." *Journal of Labor Economics*, Vol. 40, pp. 613-67. See also other papers cited therein that use text as data to study labor market discrimination.

[149] [redacted] was used for job family.

[150] https://huggingface.co/fse/word2vec-google-news-300. "API" stands for Application Programming Interface.

[151] The formal name for the semantic similarity measure we use is the "cosine similarity score," which ranges from −1 to 1, where 1 is a perfect/near-perfect match. The methods are explained in an appendix to: Burn, Ian, Patrick Button, Luis Munguia Corella, and David Neumark. 2022. "Does Ageist Language in Job Ads Predict Discrimination in Hiring." *Journal of Labor Economics*, Vol. 40, pp. 613-67. In that paper, we use Wikipedia rather than Google News to train the model, but show that the results are robust to using different corpora. Both Google News and Wikipedia have been used in recent research using computational linguistics in applications related discrimination. See: Durrheim, et al. 2023. "Using Word Embeddings to Investigate Cultural Biases." *British Journal of Social Psychology*, Vol. 62, pp. 617-29.

[152] See Appendix E, Table E.5.

semantic similarity scores. Table 9 demonstrates how the algorithm generally assigns a higher semantic similarity score to job families and job titles that appear to have similar content. For the lower scores, note that many job titles in the Kenexa data are [redacted] But including these, and those that differ, one can see how jobs that seem, on common sense, to be less related, have much lower semantic similarity scores (and see also Table E.5).

131. I do not claim that this method is perfect. First, reading through the many thousands of possible pairs, one can find isolated cases of pairs that might be expected to be similar that do not get high semantic similarity scores. Second, this method does not literally measure the similarity of job content. Rather, semantic similarity is related to the extent to which words are used in the same body of text in the corpus used to train the model. On the other hand, Disney recruiters may not have this information either, and may be relying largely on language-related associations between job titles held by candidates and the job into which they are being hired.

132. After doing these computations, I assign to each hire three measures of the relevance of their prior experience:[153] the similarity score of the latest job; the simple average of the similarity scores with each prior job title; and the weighted average of all available similarity scores, weighted by length of each prior job spell.

[153] Note that approximately 5% of prior job titles were removed due to either missing job family or missing job title.

Table 9: Similarity Scores between Job Titles and Job Families, Top and Bottom 10 from 100 Randomly Selected Workers

	Prior Experience Job Title	Job Family	Similarity Score
	(1)	(2)	(3)
	Highest 10 Similarity Score Examples		
1.	[redacted]	[redacted]	0.812
2.	[redacted]	[redacted]	0.808
3.	[redacted]	[redacted]	0.788
4.	[redacted]	[redacted]	0.762
5.	[redacted]	[redacted]	0.681
6.	[redacted]	[redacted]	0.669
7.	[redacted]	[redacted]	0.665
8.	[redacted]	[redacted]	0.656
9.	[redacted]	[redacted]	0.646
10.	[redacted]	[redacted]	0.625
	Lowest 10 Similarity Score Examples		
1.	[redacted]	[redacted]	0.186
2.	[redacted]	[redacted]	0.170
3.	[redacted]	[redacted]	0.157
4.	[redacted]	[redacted]	0.148
5.	[redacted]	[redacted]	0.057
6.	[redacted]	[redacted]	0.054
7.	[redacted]	[redacted]	0.040
8.	[redacted]	[redacted]	0.006
9.	[redacted]	[redacted]	0.000
10.	[redacted]	[redacted]	-0.054

Source: SAP analysis data set; Kenexa Applicant Data.

133. The key question is whether relevant experience is an important omitted variable in the starting pay regressions that could explain the female penalty in starting pay. To assess whether omitting relevant experience biases the estimated gender gaps in Table 8, I re-estimate those models controlling (in three separate models) for the three measures of the relevance of prior experience. The results are reported in Table 10. The results indicate that adding these controls has no impact on the estimated female penalty in starting pay. In fact, if anything the point estimates always point to a slightly larger penalty (Panels C-E vs. Panel B).[154]

Analysis of Differences in Compensation for EPA Subclass

133. For the EPA subclass, I understand that the statute is only violated when differences in pay occur between men and women who are in jobs with substantially similar work. As noted above, Dr. Hough has concluded that when employees are in the same job family (not just function) and job level at Disney, they have substantially similar work. As noted, I do not have full job family specifications for all people for all years. Thus, to assess the evidence on the EPA claim, I repeat the analyses from Table 3, but restricting the data to those persons for whom a full job family is specified. This provides [redacted] employee-year observations as compared to [redacted] included in Table 3. Other than this limitation, the model specifications are the same as for Table 3.

[154] This is equivalent to saying that, conditional on the same controls, men do not have more relevant prior experience, as I measure it. (This is because the omitted variable bias is related to the relationship between the omitted variable and gender after controlling for all of the other variables in the regression. See, e.g.: Maddala, G.S. 1992. Introduction to Econometrics, Second Edition (New York: Macmillan Publishing Company), pp. 161-163.)) This is documented in Appendix Table E.6, which shows that there is generally no statistically significant relationship between gender and any of the three measures of the relevance of prior experience. The estimated coefficients are very small – always less than 0.007 for an outcome that has a range of more than 1. And the estimates are positive, indicating that if anything women have more relevant experience (consistent with the female pay penalties increasing in Table 10 when the experience relevance controls are added). I thus conclude that differences in relevant experience do not explain the female penalty in starting pay.

Table 10: Regression Model of Gender Disparity in Starting Salary (Log (Base Salary)) at Disney Controlling for Similarity Score between Prior Job Titles and Starting Job Family, Employees Ever Employed in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents) During the Class Period (Rows B-D are Same Analysis as Table 7, but with Kenexa Subsample and Adding Kenexa Controls)

		(I) Hired in 2015 - 2022		
	VARIABLES	Coeff.	t-stat	p-value
		(1)	(2)	(3)
A.	*Full SAP Sample (Table 7)*			
	Female	**-0.0281**	**-2.9196**	**0.0035**
B.	*Sample with Available Similarity Scores*			
	Female	**-0.0355**	**-2.9642**	**0.0031**
C.	*Sample with Available Similarity Scores Controlling for Similarity Score for Latest Prior Job Title*			
	Female	**-0.0356**	**-2.9716**	**0.0030**
D.	*Sample with Available Similarity Scores Controlling for Average Similarity Score for All Prior Job Title*			
	Female	**-0.0362**	**-3.0288**	**0.0025**
E.	*Sample with Available Similarity Scores Controlling for Average Similarity Score for All Prior Job Title Weighted by Prior Job Title Spells*			
	Female	**-0.0363**	**-3.0336**	**0.0024**
	Variables			
	Prior Experience (Sq.)	Yes		
	Exempt Status	Yes		
	Hourly, Contractor, and Union	Yes		
	Technology Job Indicator	Yes		
	Southern California Indicator	Yes		
	Southern California Indicator X Technology Job Indicator	Yes		
	Northern California Indicator	Yes		
	Northern California Indicator X Technology Job Indicator	Yes		
	Job Family X Job Level dummy variables	Yes (980)		
	Year dummy variables	Yes (8)		
	Segment dummy variables	Yes (11)		
	Employee-Years with Missing Job Family X Job Level	7		
Missing Education Controls				
	Employees with Missing Highest Degree CWUR Ranking	1,602		
	Employees with Missing Highest Degree QS Ranking	2,109		
	Employees with Missing Highest Degree WSJ Ranking	2,322		
	Employees with Missing Highest Degree All Rankings	1,546		
	Observations	[redacted]		
	R-squared			
	Sample with Available Similarity Scores	0.8126		
	Sample with Available Similarity Scores Controlling for Similarity Score for Latest Prior Job Title	0.8129		
	Sample with Available Similarity Scores Controlling for Average Similarity Score for All Prior Job Title	0.8130		
	Sample with Available Similarity Scores Controlling for Average Similarity Score for All Prior Job Title Weighted by Prior Job Title Spells	0.8130		

Source: SAP analysis data set; Kenexa applicant data.
See notes to Table 8.

134. The results are reported in Table 11. In the Class Period, women at Disney were paid less than comparable men doing substantially similar work. In my preferred model, I estimate a female pay penalty of 0.81%. This is a difference of 3.8 standard deviations, implying that the estimated difference is statistically significant at the 1% percent level (and indeed a much lower level). Equivalently, the odds that we would find an estimated gender gap this large in the data, if the true effect of gender on pay was zero (i.e., there was no pay discrimination), is less than 1 in 1,000.

135. The estimated female pay penalty is not explained by potentially non-discriminatory factors, including potential experience, prior experience, tenure at Disney, education, or performance. Indeed, if anything, accounting for some of these factors would increase the estimated female pay penalty. This is shown in Table 12, which parallels the analysis in Table 4, but, again, for the limited data with job families. The estimates in rows B-D report the results using the Kenexa data. To make the comparison with Table 11 easier, row A reports the estimates from that table. Again, I have many fewer observations. Thus, the value of these data is not in getting the most reliable estimate of the gender gap in pay, but rather of assessing the impact of adding more detailed control variables. To that end, in row B, I report estimates of the exact same specification as in Table 11 (i.e., without adding any details from the Kenexa data), but for the subsample of the data for which Kenexa data are available. These estimates are smaller than those in Table 11, but this is not consequential, given the small share of the sample used. In row C, I add the data on prior experience and degrees. Then in row D, I add the more detailed information on the school rankings and fields of study. The key results are the differences between row C and D, and row B. In every case, the estimated gender gap –the pay penalty for women – is *larger* when the detailed education and experience controls are added. For example, I already noted that Model II is my preferred specification for comparing pay between similar women and men. In row D, relative to row B, the estimated female pay penalty triples, to 0.89% (3.0 standard deviations). The implication is that the estimated female pay penalty in Table 11 is likely understated – possibly by a large margin.

Table 11: Regression Model of Gender Disparity in Compensation (Log (Base Salary + Lump Sum)) at Disney, Restricted to Employees in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents) with Job Families, 2015-2022

	(I) Baseline			(II) Baseline + Segment		
	Coeff.	t-stat	p value	Coeff.	t-stat	p value
	(1)	(2)	(3)	(4)	(5)	(6)
Female	**-0.0081**	**-3.7174**	**0.0002**	**-0.0081**	**-3.7560**	**0.0002**
Variables						
Potential non-Disney Experience (Sq.)	Yes			Yes		
Tenure at Disney (Sq.)	Yes			Yes		
Exempt Status	Yes			Yes		
Hourly	Yes			Yes		
Technology Job Indicator	Yes			Yes		
Southern California Indicator	Yes			Yes		
Southern California Indicator X Technology Job Indicator	Yes			Yes		
Northern California Indicator	Yes			Yes		
Northern California Indicator X Technology Job Indicator	Yes			Yes		
Year dummy variables	Yes (8)			Yes (8)		
Job Family X Job Level dummy variables	Yes (3123)			Yes (3123)		
Segment dummy variables	No			Yes (13)		
Observations	[redacted]			[redacted]		
R-squared	0.9067			0.9080		

Source: Disney SAP Analysis Data.

Notes: Observations defined at the employee-year level. Standard errors are clustered at the employee level. Analysis is restricted to full-time, nonunion employees in specified job levels, in California. If job family is missing I include an interaction between job level dummy variables and a dummy variable for missing job family. I include a dummy variable for observations missing exempt status. [redacted]

[redacted] When the t-statistic exceeds 1.96, the estimate is significantly different from zero at the 5% level or less, so the p-value would be 0.05. See the table on p. 26 for other examples.

Table 12: Regression Model of Gender Disparity in Compensation (Log (Base Salary + Lump Sum)) at Disney, Restricted to Employees in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents) with Job Families, 2015-2022 (Rows B-D are Same Analysis as Table 11, but with Kenexa Subsample and Adding Kenexa Controls)

		(I) Baseline			(II) Baseline + Segment		
		Coeff.	t-stat	p value	Coeff.	t-stat	p value
		(1)	(2)	(3)	(4)	(5)	(6)
A.	*Full SAP Sample Controlling for Potential non-Disney Experience (Sq.) (Table 11)*						
	Female	**-0.0081**	**-3.7174**	**0.0002**	**-0.0081**	**-3.7560**	**0.0002**
B.	*Kenexa Sample Controlling for Potential non-Disney Experience (Sq.)*						
	Female	**-0.0028**	**-0.9525**	**0.3409**	**-0.0027**	**-0.9171**	**0.3591**
C.	*Kenexa Sample Controlling for Prior Experience (Sq.), Degree*						
	Female	**-0.0085**	**-2.8148**	**0.0049**	**-0.0084**	**-2.7992**	**0.0051**
D.	*Kenexa Sample Controlling for Prior Experience (Sq.) and All Education Variables*						
	Female	**-0.0090**	**-3.0340**	**0.0024**	**-0.0089**	**-3.0115**	**0.0026**
	Variables						
	Tenure at Disney (Sq.)	Yes			Yes		
	Exempt Status	Yes			Yes		
	Hourly	Yes			Yes		
	Technology Job Indicator	Yes			Yes		
	Southern California Indicator	Yes			Yes		
	Southern California Indicator X Technology Job Indicator	Yes			Yes		
	Northern California Indicator	Yes			Yes		
	Northern California Indicator X Technology Job Indicator	Yes			Yes		
	Year dummy variables	Yes (2189)			Yes (2189)		
	Job Family X Job Level dummy variables	Yes (8)			Yes (8)		
	Segment dummy variables	No			Yes (13)		
<u>*Missing Education Controls*</u>							
	Employee-years with Missing Highest Degree CWUR Ranking	8,096			8,096		
	Employee-years with Missing Highest Degree QS Ranking	11,004			11,004		

Employee-years with Missing Highest Degree WSJ Ranking	12,214	12,214
Employee-years Missing Highest Degree All Rankings	7,857	7,857
Observations	[redacted]	[redacted]
R-squared		
Controlling for Age Hired by Disney Minus 22 (Sq.)	0.9316	0.9324
Controlling for Prior Experience (Sq.), Degree	0.9268	0.9277
Controlling for Prior Experience (Sq.), Education	0.9326	0.9334

Source: Disney SAP Analysis Data; Kenexa Applicant Data.

Notes: Observations defined at the employee-year level. Standard errors are clustered at the employee level. Analysis is restricted to full-time, nonunion employees in specified job levels, in California. If job family is missing I include an interaction between job level dummy variables and a dummy variable for missing job family. I include a dummy variable for observations missing exempt status. [redacted] Education controls are included for the highest degree earned by an individual as reported in their CVs and recorded in the Kenexa data. Degree controls consist of 7 degree classifications (listed below in perceived ascending order) as well as an indicator for a missing degree: Others; High School Degree; Certificate; Associate degree; Bachelor's Degree; Master's Degree; Doctorate Degree. Education controls include degree controls, as well as: i. University rankings as reported by CWUR, QS, and WSJ, and indicators of missing rank for each source. Controls for the highest degrees are included in the analysis; ii. Indicators for highest degree fields of study, for fields of study with 10 or more employee-years; iii. Dummies for schools with 10 or more employee-years. When the t-statistic exceeds 1.96, the estimate is significantly different from zero at the 5% level or less, so the p-value would be 0.05. See the table on p. 26 for other examples.

Damages

136. I have not yet done a formal damages analysis, which would take account of interest, liquidated damages, waiting time penalties, partial years or work, and other details to arrive at the most precise estimate. However, damages can be readily addressed for the class as a whole based upon the data analyses. As an illustration of this point, I have done an approximate calculation of the cumulative lost back pay for women in Covered Positions in the Class Period, for the FEHA claim. To do this, I convert pay to April 2023 dollars. During the Class Period, in Covered Positions average male pay at Disney was $137,617. I apply the 2.01% female pay penalty from Table 3, Model II, to this figure, which implies an average underpayment of women in each year of $2,766. There are 12,511 women employed at Disney in Covered Positions in the Class Period, for an average of 4.38 years. Thus, the estimated pay penalty implies cumulative underpayment of covered women in the Class Period of approximately $151.6 million. I will refine this estimate substantially if this case goes to trial.

137. This is a very low estimate compared to the damages to which plaintiffs would be entitled, given that interest charges are substantial, and then liquidated damages for the EPA damages double the figure after applying interest.

138. There are alternative ways one could do this damages calculation, but all involve straightforward modifications to the calculation just described.

139. One modification, already mentioned earlier, is to use the estimated biases in the female pay penalty from omitted variables to arrive at a different estimate of the female pay penalty with which to do the calculation. For example, as noted earlier, I estimate that the female pay penalty would be larger – 2.56% – accounting for the role of education and prior experience. In this case, the damages estimate above would be increased by about 27%.

140. A second modification would be to assume that the damages experienced by each woman were equal to the percentage female pay penalty applied to her pay (rather than as a percentage of average male pay). This, in isolation, would lead to slightly lower damages – precisely because average male pay exceeds average female pay. It would also attribute larger damages to higher paid women than to lower paid women, which would make the damages allocation more individually tailored to the impacts experienced by each woman. A woman with a $75,000 salary and one with a $150,000 salary may both be impacted by the same pay penalty, and in the same 2% or 2.5% range. But losing out on 2% of $75,000 yields a different dollar loss than losing 2%

of $150,000. Such nuance can be readily accomplished in using the results of my analyses to determine lost wages for class members.

141. Moreover, I have not analyzed bonuses and long-term incentive pay. My understanding is that Disney's practices regarding decisions about these two components of pay are not at issue with respect to class certification. My pay disparity analysis has focused on annual base salary (and lump sum payments) salary. However, Disney employees are also eligible for other forms of compensation, including bonuses and long-term incentive compensation (LTIC). Company documents indicate these other forms of pay are set as a fixed percentage of base annual salary.[155] The implication is that female penalties in annual salary will be reflected in similar percentage penalties in these other components of pay, for otherwise comparable men. Hence, in computing damages, I would also apply the estimated percentage penalties in annual salary to these other components of pay, which will increase the damages.

142. Damages for the EPA claim would be calculated similarly.

143. Finally, there may be additional employees who worked in California, but not at the time of the annual snapshots I use in my analysis of Class Period pay. My final damages analysis would include partial years worked at Disney, including time in California even for years when employees were not working in California as of the snapshot date.

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[155] See, e.g., *DISNEY-000026712.pdf* ([redacted]) at 26717 for bonuses [redacted] and at 26719 for LTI [redacted] For bonuses, see also Anderson Dep. at 234:5-8. Janet Temple's deposition (February 7, 2023), confirms the same – that bonuses are based on a percent of salary (Temple Dep. at 197:6-22).

Appendix A: Data Sets Used in Analysis and Descriptions

Dataset Name	Source	Main Years Covered	Number of Observations	Number of Unique Observations	Unit of Observation	Description	Analysis Used for: Class Period Pay	Analysis Used for: Starting Pay
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[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 B: Materials Considered

Documents/productions	Bates No. (if applicable)
DISNEY-000000711.pdf	DISNEY-000000711
DISNEY-000000714.pdf	DISNEY-000000714
DISNEY-000001222.pdf	DISNEY-000001222
DISNEY-000001500.pdf	DISNEY-000001500
DISNEY-000001504.pdf	DISNEY-000001504
DISNEY-000001509.pdf	DISNEY-000001509
DISNEY-000003328.pdf	DISNEY-000003328
DISNEY-000005275.pdf	DISNEY-000005275
DISNEY-000005278.pdf	DISNEY-000005278
DISNEY-000005293.pdf	DISNEY-000005293
DISNEY-000005303.pdf	DISNEY-000005303
DISNEY-000005331.pdf	DISNEY-000005331
DISNEY-000005349.pdf	DISNEY-000005349
DISNEY-000005360.pdf	DISNEY-000005360
DISNEY-000005371.pdf	DISNEY-000005371
DISNEY-000005399.pdf	DISNEY-000005399
DISNEY-000005402.pdf	DISNEY-000005402
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DISNEY-000021650.pdf	DISNEY-000021650

DISNEY-000021658.pdf	DISNEY-000021658
DISNEY-000021666.pdf	DISNEY-000021666
DISNEY-000021916.pdf	DISNEY-000021916
DISNEY-000021929.pdf	DISNEY-000021929
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Defendant's Production 60: DISNEY-000032502-DISNEY000032505	DISNEY-000032502-DISNEY-000032505
Defendant's Production 61: DISNEY-000032506-DISNEY000032513	DISNEY-000032506-DISNEY-000032513
DISNEY-000032927	DISNEY-000032927
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DISNEY-000041387	DISNEY-000041387
Defendant's Production 83: DISNEY-000044007-DISNEY000044021	DISNEY-000044007-DISNEY000044021
PLFS000062.pdf	PLFS000062
PLFS000067.pdf	PLFS000067

PLFS000205.pdf	PLFS000205
PLFS001464.pdf	PLFS001464
Spreadsheet of files produced Bates match to RFP.xlsx	
DMED Leader Training_Platform Distribution 07.30.2021.pdf	
Employee Policy Manual May 2021 (5.05.2021).pdf	
Global Job Leveling_Reference Guide_Leader Module.pdf	
Leveling and Harmonization Leader Brief (Final_7.25.21).pdf	
Leveling and Harmonization Leader Talking Points (FINAL 7.27.21) vPD.pdf	
ROSTR Profile for Becky Train.pdf	
Discovery requests and responses	
14th Request for Production of Documents	
Defendant TWDC Responses to RFPD Set 14 20221129.pdf	
Defendant TWDC Supplemental & Amended Responses to Rasmussen ROGs 18-23 of SROGs Set 2 20200831 .pdf	
Defendant TWDC Supplemental Objections & Responses to SPROGs Set 2 30300814	
Defendant Disney Suppl. Obj. & Resp. to SROGS(6).pdf	
Defendant TWDC Supplemental Objections to SROG 19 20201016.pdf	
Depositions and exhibits	
Anderson, Kara deposition Transcript	
Bacon, NaShawn deposition transcript	
Brahm, Jill deposition transcript	
Burnley, Janet deposition transcript	
Fernandez, Ibelka deposition transcript	
Fox, Karmen deposition transcript	
Hirst, Brett deposition transcript	
Larson, Mark Vols. 1 & 2 deposition transcripts	
Mrudula, Lal deposition transcript	
Olsgaard, Alison deposition transcript	
Pate, Janet deposition transcript	
Schultz, Ryan deposition transcript	
Temple, Janet deposition transcript	
Train, Rebecca deposition exhibits 1-14	
Train, Rebecca deposition transcript	
Wahab, Mezhgan deposition transcript	
Watkins, Kaitlyn deposition transcript	
Weirick, Kelly deposition transcript	
Plaintiffs' Deposition exhibits 500-882	

Emails	Subject
4/26/23 email from Besnoff to Webber, forwarded to Neumark on 4/26/23	FW: Disney data questions
4/20/23 email from Besnoff to Webber, forwarded to Neumark on 4/21/23	Fwd: [EXTERNAL] RE: Disney data questions
4/18/23 email from Besnoff to Webber, forwarded to Neumark on 4/18/23	FW: Disney data questions
4/6/23 email from Besnoff to Webber, forwarded to Neumark 4/7/23	FW: Disney data questions
3/24/23 email from Besnoff to Webber, forwarded to Neumark on 3/24/23	Fwd: [EXTERNAL] RE: Disney data questions
2/14/23 email from Besnoff to Webber, forwarded to Neumark on 2/15/23	Fwd: [EXTERNAL] RE: Rasmussen v Disney data follow up
2/2/23 email from Besnoff to Webber, forwarded to Neumark on 2/2/23	FW: Disney -- two data follow ups
1/19/23 email from Besnoff to Webber, forwarded to Neumark 1/19	Fwd: [EXTERNAL] RE: Rasmussen v Disney data follow up
1/12/23 email from Besnoff to Webber, forwarded to Neumark 1/13/23	FW: Rasmussen v Disney data follow up

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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.

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Topel, Robert H., and Michael P. Ward. 1992. "Job Mobility and the Careers of Young Men." *Quarterly Journal of Economics* 107(2): 439-79.

Appendix C: Abridged CV with Publications from Last 10 Years

David Neumark
Department of Economics

611 Gennessee St.
San Francisco, CA 94127
Home phone: 415-264-8946

3151 Social Science Plaza
University of California, Irvine
Irvine, CA 92697
Work phone: 949-824-8496
E-mail: dneumark@uci.edu

PERSONAL: Born July 7, 1959
United States Citizenship

EDUCATION:

Fields: Labor Economics, Econometrics

Thesis Topic: Male-Female Differentials in the Labor Force: Measurement, Causes and Probes.

Fellowships: National Science Foundation Graduate Fellowship, Fulbright Scholarship

Graduate: Harvard University, Awarded Master of Arts Degree in Economics in 1985, Ph.D. in Economics in 1987.

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

AWARDS/HONORS:

Distinguished Professor, University of California, Irvine, 2018-present

National Longitudinal Surveys, Michael E. Borus Memorial Dissertation Award

National Institute on Aging, Special Emphasis Research Career Award

2000 Minnesota Award for "Age Discrimination Laws and Labor Market Efficiency"

Bren Fellow, Public Policy Institute of California, 2009-2010

Choice Outstanding Academic Title, 2009, for Minimum Wages (Cambridge: MIT Press, 2008)

Chancellor's Professorship, University of California, Irvine, 2012-2018

UCI Associated Graduate Students, 2015, Faculty Mentoring Award

2016 Harris Distinguished Visiting Professor, Clemson University

Selected to teach at IZA European Summer School in Labor Economics, 2016

Lady Davis Fellowship, Hebrew University of Jerusalem, 2018

Elected Fellow of the American Association for the Advancement of Science, 2019

RESEARCH AND PROFESSIONAL EXPERIENCE:

2005-present: University of California, Irvine, Department of Economics—Professor of Economics (now Distinguished Professor of Economics)

1995-present: National Bureau of Economic Research—Research Associate

2019-present: University of California, Irvine, Center for Population, Inequality, and Policy—Founding Co-Director

2004-present: IZA, Institute for the Study of Labor—Research Fellow

2011-present: Federal Reserve Bank of San Francisco—Visiting Scholar

2012-present: Workers Compensation Research Institute—Senior Research Fellow

2016-present: Beijing Normal University—Visiting Professor

2018-present: CESifo—Research Fellow

2018: Tel Aviv University—Visiting Professor

2016-2019: University of California, Irvine, Economic Self-Sufficiency Policy Research Institute (ESSPRI)—Founding Director

2012: Renmin University, Hanqing Institute, Beijing, China—Visiting Lecturer

2011-2015: University of California, Irvine, Center for Economics & Public Policy—Founding Director

2009-2011: University of California, Irvine—Director of Graduate Studies

2002-2011: Public Policy Institute of California—Bren Fellow/Senior Fellow, Economics

1994-2002: Michigan State University, Department of Economics—Professor of Economics

1989-1994: University of Pennsylvania, Department of Economics—Assistant Professor of Economics

1987-1989: Board of Governors of the Federal Reserve System—Economist, Division of Research and Statistics

1984-1985: Abt Associates, Inc.—Economic consultant

2009-2016: Charles River Associates—Senior consultant

2000-2001: Public Policy Institute of California—Visiting Fellow

1999-2002: Michigan State University, Department of Economics—Director of Graduate Studies

1989-1994: National Bureau of Economic Research—Faculty Research Fellow

EDITORIAL RESPONSIBILITIES:

2022-present: Journal of Urban Economics, Editorial Board

2009-present: Journal of Labor Research, Editorial Board

2004-present: Industrial Relations, Editorial Board

2002-present: Contemporary Economic Policy, Editorial Board

2012-2022: Journal of Urban Economics, Co-Editor

2012-2016: IZA Journal of Labor Policy, Editor

2009-2012: Journal of Urban Economics, Editorial Board

2006-2012: Review of Economics of the Household, Associate Editor

2003-2010: Economics of Education Review, Editorial Board

2004-2006: California Economic Policy, Editor

PEER-REVIEWED PUBLICATIONS:

Neumark, David, "Age Discrimination in Hiring: Evidence from Age-Blind vs. Non-Age-Blind Hiring Procedures," forthcoming in Journal of Human Resources.

Neumark, David, and Bogdan Savych, "Effects of Opioid-Related Policies on Opioid Utilization, Nature of Medical Care, and Duration of Disability," forthcoming in American Journal of Health Economics.

Neumark, David, and Maysen Yen, "The Employment and Redistributive Effects of Reducing or Eliminating Minimum Wage Tip Credits," forthcoming in Journal of Policy Analysis and Management.

Burn, Ian, Daniel Firoozi, Daniel Ladd, and David Neumark, "Stereotypes of Older Workers and Perceived Ageism in Job Ads: Evidence from an Experiment," forthcoming in Journal of Pension Economics and Finance.

Freedman, Matthew, Shantanu Khanna, and David Neumark, 2023, "Combining Rules and Discretion in Economic Development Policy: Evidence on the Impacts of the California Competes Tax Credit," Journal of Public Economics, 104777.

Ladd, Daniel, and David Neumark, 2023, "Workplace Injuries and Receipt of Benefits from Workers' Compensation and SSDI," Journal of Occupational and Environmental Medicine, pp. 261-70.

He, Haoran, David Neumark, and Qian Weng, 2023, "'I Still Haven't Found What I'm Looking For": Evidence of Directed Search from a Field Experiment," Economic Journal, 258-80.

Neumark, David, and Giannina Vaccaro, 2023, "The Career Evolution of the Sex Gap in Wages: Discrimination vs. Human Capital Investment," Research in Labor Economics, pp. 117-50.

Freedman, Matthew, Shantanu Khanna, and David Neumark, 2023, "The Impacts of Opportunity Zones on Zone Residents," Journal of Urban Economics: Insights, 103407.

Neumark, David, and Peter Shirley, 2022, "Myth of Measurement: What Does the New Minimum Wage Research Say about Minimum Wages and Job Loss in the United States?," Industrial Relations, pp. 384-417.

Burn, Ian, Patrick Button, David Neumark, and Luis Felipe Munguia Corella, "Does Ageist Language in Job Ads Predict Age Discrimination in Hiring?" 2022, Journal of Labor Economics, pp. 613-667.

Neumark, David, 2022, "Strengthen Age Discrimination Protections to Help Confront the Challenge of Population Aging," Journal of Aging & Social Policy, pp. 455-470.

Neumark, David, and Maysen Yen, 2022, "Effects of Recent Minimum Wage Policies in California and Nationwide: Results from a Pre-specific Analysis Plan," Industrial Relations, pp. 228-55.

Drucker, Lev, Katya Mazirov, and David Neumark, 2021, "Who Pays for and Who Benefits from Minimum Wage Increases? Evidence from Israeli Tax Data on Business Owners and Workers," Journal of Public Economics, 104423.

He, Haoran, David Neumark, and Qian Weng, 2021, "Do Workers Value Flexible Jobs: A Field Experiment," Journal of Labor Economics, pp. 709-38.

Neumark, David, and Timothy Young, 2021, "Heterogeneous Effects of State Enterprise Zone Programs in the Shorter Run and Longer Run," Economic Development Quarterly, pp. 91-107.

Neumark, David, and Luis Felipe Munguia Corella, 2021, "Do Minimum Wages Reduce Employment in Developing Countries? A Survey and Exploration of Conflicting Evidence," World Development, 105165.

Asquith, Brian, Judith K. Hellerstein, Mark J. Kutzbach, and David Neumark, 2021, "Social Capital and Labor Market Networks," Journal of Regional Science, pp. 212-60.

Neumark, David, 2020, "Point/Counterpoint: Can We Do Better than Enterprise Zones?" Journal of Policy Analysis and Management, pp. 836-44, 851-54.

Neumark, David, and Katherine Williams, 2020, "Do State Earned Income Tax Credits Increase Program Participation at the Federal Level?" Public Finance Review, pp. 579-626.

Neumark, David, and Peter Shirley, 2020, "The Long-Run Effects of the Earned Income Tax Credit on Women's Earnings," Labour Economics, Vol. 66.

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.

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.

Neumark, David, and Maysen Yen, 2019, "Relative Sizes of Age Cohorts and Labor Force Participation of Older Workers," Demography, pp. 1-31.

Savych, Bogdan, David Neumark, and Randy Lea, 2019, "Do Opioids Help Injured Workers Recover and Get Back to Work? The Impact of Opioid Prescriptions on Duration of Temporary Disability Benefits," Industrial Relations, pp. 549-90.

Neumark, David, Ian Burn, Patrick Button, and Nanneh Chehras, 2019, "Do State Laws Protecting Older Workers from Discrimination Reduce Age Discrimination in Hiring? Evidence from a Field Experiment," Journal of Law and Economics, pp. 373-402.

Neumark, David, and Cortnie Shupe, 2019, "Declining Teen Employment: Minimum Wages, Other Explanations, and Implications for Human Capital Investment," Labour Economics, pp. 49-68.

Neumark, David, 2019, "The Econometrics and Economics of the Employment Effects of Minimum Wages: Getting from Known Unknowns to Known Knowns," German Economic Review, 293-329.

Neumark, David, Ian Burn, and Patrick Button, 2019, "Is It Harder for Older Workers to Find Jobs? New and Improved Evidence from a Field Experiment," Journal of Political Economy, 922-70.

Asquith, Brian, Sanjana Goswami, David Neumark, and Antonio Rodriquez-Lopez, 2019, "U.S. Job Flows and the 'China Shock'," Journal of International Economics, pp. 123-37.

Neumark, David, and Judith Rich, 2019, "Do Field Experiments on Labor and Housing Markets Overstate Discrimination? A Re-examination of the Evidence," Industrial and Labor Relations Review, pp. 223-52.

Neumark, David, and Bogdan Savych, 2018, "The Effects of Provider Choice Policies on Workers' Compensation Costs," Health Services Research, pp. 5057-77.

Neumark, David, 2018, "Experimental Research on Labor Market Discrimination," Journal of Economic Literature, pp. 799-866.

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Figinski, Theodore, and David Neumark, 2018, "Does Eliminating the Earnings Test Increase Old-Age Poverty of Women?" Research on Aging, pp. 27-53.

Neumark, David, and Diego Grijalva, 2017, "The Employment Effects of State Hiring Credits," ILR Review, pp. 1111-45.

Neumark, David, and William Wascher, 2017, "Reply *to Credible Research Designs for Minimum Wage Studies*," ILR Review, pp. 593-609.

Bradley, Cathy J., and David Neumark, 2017, "Small Cash Incentives Can Encourage Primary Care Visits by Low-Income People with New Health Care Coverage," Health Affairs, pp. 1376-84.

Neumark, David, Joanne Song, and Patrick Button, 2017, "Does Protecting Older Workers from Discrimination Make It Harder to Get Hired? Evidence from Disability Discrimination Laws," Research on Aging, pp. 29-63.

Neumark, David, Ian Burn, and Patrick Button, 2016, "Experimental Age Discrimination Evidence and the Heckman Critique," American Economic Review Papers and Proceedings, pp. 303-8.

Neumark, David, 2016, "Policy Levers to Increase Jobs and Increase Income from Work after the Great Recession," IZA Journal of Labor Policy, 5:6 (on-line).

Neumark, David, and Jennifer Muz, 2016, "The 'Business Climate' and Economic Inequality," Review of Income and Wealth, pp. 161-80.

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

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Neumark, David, J.M. Ian Salas, and William Wascher, 2014, "Revisiting the Minimum Wage-Employment Debate: Throwing Out the Baby with the Bathwater?" Industrial and Labor Relations Review, 608-648.

Burnes, Daria, David Neumark, and Michelle White, 2014, "Fiscal Zoning and Sales Taxes: Do Higher Sales Taxes Lead to More Retailing and Less Manufacturing," National Tax Journal, 7-50.

Brueckner, Jan, and David Neumark, 2014, "Beaches, Sunshine, and Public-Sector Pay: Theory and Evidence on Amenities and Rent Extraction by Government Workers," American Economic Journal: Economic Policy, pp. 198-230.

Hellerstein, Judith K., Mark Kutzbach, and David Neumark, 2014, "Do Labor Market Networks Have an Important Spatial Dimension?" Journal of Urban Economics, pp. 39-58.

Neumark, David, and Joanne Song, 2013, "Do Stronger Age Discrimination Laws Make Social Security Reforms More Effective?" Journal of Public Economics, pp. 1-16.

Neumark, David, Matthew Thompson, Francesco Brindisi, Leslie Koyle, and Clayton Reck, 2013, "Simulating the Economic Impacts of Living Wage Mandates Using New Public and Administrative Data: Evidence for New York City," Economic Development Quarterly, pp. 271-83.

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

Bradley, Cathy J., David Neumark, and Scott Barkowski, 2013, "Does Employer-Provided Health Insurance Constrain Labor Supply Adjustments to Health Shocks? New Evidence on Women Diagnosed with Breast Cancer," Journal of Health Economics, pp. 833-49.

Kolko, Jed, David Neumark, and Marisol Cuellar Mejia, 2013, "What Do Business Climate Indexes Teach Us About State Policy and Growth?" Journal of Regional Science, pp. 220-55.

Neumark, David, 2013, "Spurring Job Creation in Response to Severe Recessions: Reconsidering Hiring Credits," Journal of Policy Analysis and Management, pp. 142-71.

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Neumark, David, and Kenneth Troske, 2012, "Point/Counterpoint: 'Addressing the Employment Situation in the Aftermath of the Great Recession,' and 'Lessons from Other Countries, and Rethinking (Slightly) Unemployment Insurance as Social Insurance Against the Great Recession," Journal of Policy Analysis and Management, pp. 160-68, 188-91.

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KEYNOTE/INVITED LECTURES:

“What Can We Conclude from the Evidence on Minimum Wages and Employment? Recent Progress,” Seoul National University, Seoul, South Korea, Nov. 2022.

“Older Workers Need Not Apply? Ageist Language in Job Ads and Age Discrimination in Hiring.” Maastricht University Research Centre for Education and the Labour Market Workshop on Older Workers’ Skills and Labour Market Behavior, Maastricht, Netherlands, Nov. 2019.

“Fighting Inequality and Poverty with Minimum Wages.” Keynote lecture sponsored by National Diet Library, Tokyo, Japan, Oct. 2019.

“Fighting Inequality and Poverty with Minimum Wages.” Reading group sponsored by O’Neil Center for Global Markets and Freedom at SMU, Baugh Center for Entrepreneurship and Free Enterprise at Baylor University, the Free Market Institute at Texas Tech University, and the Arkansas Center for Research in Economics at the University of Central Arkansas, Dallas, TX, Sept. 2019.

“The Long-Run Effect of the Earned Income Tax Credit on Women’s Earnings.” 3rd Forum on Advances in Labor Economics in China,” Beijing Normal University, Beijing, China, May, 2019.

“The Econometrics and Economics of the Employment Effects of Minimum Wages: Getting from Known Unknowns to Known Knowns.” Conference on the “Evaluation of Minimum Wages,” DIW, Berlin, Germany, July, 2018.

"Using Minimum Wages to Fight Inequality and Poverty." Adam Smith Lecture, Adam Smith Institute, London, England, June, 2018.

"The Long-Run Effects of Anti-Poverty Policies on Disadvantaged Neighborhoods," Keynote lecture, Workshop on Minimum Wages and Experimental Economics, Beijing Normal University, Beijing, China, November, 2017.

"New and Improved Evidence from Field Experiments on Discrimination," Keynote lecture, Labor Econometrics Workshop, University of Auckland, Auckland, New Zealand, August, 2017.

"Using Minimum Wages to Fight Inequality and Poverty," Invited lecture, Economic Policy Research Network Conference, University of Copenhagen, Copenhagen, Denmark, June, 2017.

"Preliminary Studies of the Long-Run Effects of Anti-Poverty Policies," keynote lecture, Belgian Day for Labour Economists, Université Catholique de Louvain, Louvain-la-Neuve, Belgium, May, 2017.

"Using Minimum Wages to Fight Inequality and Poverty," 2017 Mattersdorff-Steinhardt Lecture, Lewis and Clark University, Portland, Oregon, March, 2017.

"Why I Think the U.S. Evidence Says that Minimum Wages Reduce Employment, and Implications for Policy," keynote lecture, Beijing Normal University Minimum Wage Workshop, Beijing, China, October, 2016.

"Labor Market Discrimination," lectures, IZA Summer School, Munich, Germany, May, 2016.

"Is It Harder for Older Workers to Find Jobs? New and Improved Evidence from a Field Experiment," keynote lecture, Aix-Marseilles School of Economics – Banque de France Labor Market Conference, Paris, France, December, 2015.

"What Does Research Tell Us About the Effects of the Minimum Wage?" keynote lecture, Central Bank of Colombia conference, Bogota, Colombia, November, 2015.

"Is It Harder for Older Workers to Find Jobs? New and Improved Evidence from a Field Experiment," keynote address, Conference on Discrimination and Labor Market Research, Linnaeus University, Kalmar, Sweden, August, 2015.

"Age Discrimination Laws and Age Discrimination in the United States," keynote address, RIETI/IZA World of Labor Symposium, Tokyo, Japan, May, 2015.

"Employment Effects of Minimum Wages," opening address, 1st OECD/IZA World of Labor Seminar, OECD, Paris, France, December, 2014.

"Policy Levers to Increase Jobs and Increase Income from Work after the Great Recession," keynote address, Conference to Celebrate the Centennial of the Federal Reserve Bank of Dallas, Dallas, TX, November, 2014

"Policy Levers to Increase Jobs and Increase Income from Work after the Great Recession," keynote address, Workshop on Minimum Wages and Employment in Hong Kong, China, and the World, Hong Kong University of Science and Technology, Hong Kong, September, 2014.

"Revisiting the Minimum Wage-Employment Debate: Throwing Out the Baby with the Bathwater?" distinguished lecture, Middle Tennessee State University, Murfreesboro, TN, April, 2013.

"Revisiting the Minimum Wage-Employment Debate: Throwing Out the Baby with the Bathwater?" keynote address, Bank of Portugal Conference on Labor Market Policy, Lisbon, Portugal, May 2013.

"Race and Ethnicity in Hiring," keynote address, Workshop on Discrimination and Segregation, Sciences-Po, Paris, December 2011.

"Detecting Discrimination in Audit and Correspondence Studies," keynote address, IZA Workshop on Discrimination and Ethnicity: A Cross-Disciplinary Look at New Methodological Advances, Bonn, Germany, October 2011.

"Future Skill Shortages in the U.S. Economy?" keynote address, European Commission conference: Catch the Train: Skills, Education, and Jobs, Brussels, Belgium, June 2011.

"Neighbors and Co-Residents: The Importance of Residential Labor Market Networks," keynote address, International Conference on Labor Economics, Xiamen University, Xiamen, China, December 2009.

"How Do Economists Think About and Study Discrimination?" keynote address, 46th Annual Economic Education Winter Institute, St. Cloud State University, St. Cloud, Minnesota, February 2008.

"Population Aging and Age Discrimination," keynote address, Conference on Measuring Discrimination, Universite d'Evry Val D'Essonne, France, December 2007.

POLICY-RELATED TESTIMONY:

Testified on AB 225 (California EITC expansion) at California State Assembly Committee on Appropriations, 2017.

California State Senate testimony on job creation policy, 2011.

California State Senate testimony on enterprise zones, 2010.

California State Senate testimony on business relocation, 2006.

Congressional testimony on minimum wages and poverty, U.S. House Committee on Education and the Workforce, 2000.

Congressional testimony on minimum wages and employment, U.S. House Committee on Government Reform and Oversight, 1996.

Appendix D: Expert Witness Work in Last 4 Years

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

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

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.

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 sex discrimination in pay. Deposed. Qualified as expert witness.

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 sex discrimination in hiring. Deposed and testified.

Ellis et al. v. Google LLC, No. CGC-17-561299, Superior Court of the State of California

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

Cahill et al. v. Nike, Inc., No. 3:18-cv-1477-JR, United States District Court District of Oregon

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

Bragg et al. v. Pacific Maritime Association, International Longshore and Warehouse Union, and International Longshore and Warehouse Union Local 13, No: 19STCV35714, Superior Court of the State of California County of Los Angeles—Central District

Served as plaintiffs' expert witness to address statistical evidence on discrimination against pregnant workers.

Boynes et al. v. Limetree Bay Ventures, LLC et al.; Shirley et al. v. Limetree Bay Ventures, LLC et al.; Charles and Charles et al. v. Limetree Bay Ventures, LLC et al.; Beecher Cotton et al. v. Limetree Bay Ventures, LLC et al.

Served as plaintiffs' expert witness to address criteria for eligibility for programmatic relief owing to contamination of water supplies.

Appendix E: Additional Tables

Table E.1: Distribution of Full-Time Non-Union HR Compensation Employees by Segment and Year, 2015-2022

	Segment	2015	2016	2017	2018	2019	2020	2021	2022
	(1)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
2.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
3.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
4.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
5.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
6.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
7.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
8.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
9.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
10.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
11.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
12.	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]

Source: SAP analysis data set.
Note: Segment classifications reflect segments and years in the snapshots used for the pay analysis.

Table E.2: Disney Employees by Segment and Year, 2015-2022

Year	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
(1)	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
2015	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]			[redacted]			
2016	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]	[redacted]		[redacted]			
2017	[redacted]	[redacted]	[redacted]		[redacted]	[redacted]		[redacted]			
2018	[redacted]	[redacted]	[redacted]			[redacted]	[redacted]	[redacted]		[redacted]	
2019	[redacted]	[redacted]				[redacted]	[redacted]	[redacted]		[redacted]	
2020	[redacted]	[redacted]					[redacted]	[redacted]		[redacted]	[redacted]
2021	[redacted]	[redacted]					[redacted]	[redacted]	[redacted]	[redacted]	[redacted]
2022	[redacted]	[redacted]					[redacted]		[redacted]		[redacted]

Source: SAP analysis data set.

Note: Segment classifications reflect segments and years in the snapshots used for the pay analysis.

Table E.3: Regression Model of Gender Disparity in Compensation (Log Base Salary + Lump Sum) at Disney, Restricted to Employees in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents), 2015-2022, Separate Job Family and Job Level Dummy Variables

	(I) Baseline		
	Coeff.	t-stat	p-value
	(1)	(2)	(3)
Variables			
Female	**-0.0247**	**-10.3907**	**0.0000**
Potential non-Disney Experience (Sq.)	Yes		
Tenure at Disney (Sq.)	Yes		
Exempt Status	Yes		
Technology Job Indicator	Yes		
Southern California Indicator	Yes		
Southern California Indicator X Technology Job Indicator	Yes		
Northern California Indicator	Yes		
Northern California Indicator X Technology Job Indicator	Yes		
Year dummy variables	Yes (8)		
Job Family dummy variables	Yes (670)		
Job Level dummy variables	Yes (29)		
Segment dummy variables	Yes (14)		
Employee-Years with Missing Job Family	4,685		
Observations	[redacted]		
R-squared	0.8424		

Source: SAP analysis data set. See notes to Table 3.

Table E.4: Regression Model of Gender Disparity in Compensation (Log Base Salary + Lump Sum) at Disney, Restricted to Employees with Applicant Data in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents), 2015-2017, Including "Current" Reviews

		(I) Baseline			(II) Baseline + Segment		
		Coeff.	t-stat	p-value	Coeff.	t-stat	p-value
		(1)	(2)	(3)	(4)	(5)	(6)
A.	*Full SAP Sample*						
	Female	**-0.0206**	**-9.2915**	**0.0000**	**-0.0201**	**-9.1600**	**0.0000**
B.	*Performance Rating Sample*						
	Female	**-0.0291**	**-8.7810**	**0.0000**	**-0.0282**	**-8.6805**	**0.0000**
C.	*Performance Rating Sample Controlling for Performance Ratings*						
	Female	**-0.0294**	**-8.9701**	**0.0000**	**-0.0288**	**-8.9299**	**0.0000**
	Variables						
	Potential non-Disney Experience (Sq.)	Yes			Yes		
	Tenure at Disney (Sq.)	Yes			Yes		
	Exempt Status	Yes			Yes		
	Technology Job Indicator	Yes			Yes		
	Southern California Indicator	Yes			Yes		
	Southern California Indicator X Technology Job Indicator	Yes			Yes		
	Northern California Indicator	Yes			Yes		
	Northern California Indicator X Technology Job Indicator	Yes			Yes		
	Year dummy variables	Yes (3)			Yes (3)		
	Job Family X Job Level dummy variables	Yes (691)			Yes (691)		
	Segment dummy variables	No			Yes (10)		
	Observations	[redacted]			[redacted]		
	R-squared						
	Performance Rating Sample	0.8504			0.8548		
	Performance Rating Sample Controlling for Performance Ratings	0.8523			0.8566		

Source: SAP analysis data set. See notes to Table 5, except that here the data extend through 2017.

Table E.5: Similarity Scores between Job Titles and Job Families, 100 Randomly Selected Workers

	Prior Experience Job Title	Job Family	Similarity Score
	(1)	(2)	(3)
1.	[redacted]	[redacted]	0.812
2.	[redacted]	[redacted]	0.808
3.	[redacted]	[redacted]	0.788
4.	[redacted]	[redacted]	0.762
5.	[redacted]	[redacted]	0.681
6.	[redacted]	[redacted]	0.669
7.	[redacted]	[redacted]	0.665
8.	[redacted]	[redacted]	0.656
9.	[redacted]	[redacted]	0.646
10.	[redacted]	[redacted]	0.625
11.	[redacted]	[redacted]	0.625
12.	[redacted]	[redacted]	0.623
13.	[redacted]	[redacted]	0.620
14.	[redacted]	[redacted]	0.616
15.	[redacted]	[redacted]	0.612
16.	[redacted]	[redacted]	0.604
17.	[redacted]	[redacted]	0.601
18.	[redacted]	[redacted]	0.598
19.	[redacted]	[redacted]	0.583
20.	[redacted]	[redacted]	0.554
21.	[redacted]	[redacted]	0.541
22.	[redacted]	[redacted]	0.538
23.	[redacted]	[redacted]	0.533
24.	[redacted]	[redacted]	0.527
25.	[redacted]	[redacted]	0.526
26.	[redacted]	[redacted]	0.524

	Prior Experience Job Title	Job Family	Similarity Score
27.	[redacted]	[redacted]	0.523
28.	[redacted]	[redacted]	0.512
29.	[redacted]	[redacted]	0.503
30.	[redacted]	[redacted]	0.493
31.	[redacted]	[redacted]	0.489
32.	[redacted]	[redacted]	0.488
33.	[redacted]	[redacted]	0.472
34.	[redacted]	[redacted]	0.469
35.	[redacted]	[redacted]	0.468
36.	[redacted]	[redacted]	0.457
37.	[redacted]	[redacted]	0.455
38.	[redacted]	[redacted]	0.454
39.	[redacted]	[redacted]	0.447
40.	[redacted]	[redacted]	0.446
41.	[redacted]	[redacted]	0.438
42.	[redacted]	[redacted]	0.437
43.	[redacted]	[redacted]	0.435
44.	[redacted]	[redacted]	0.434
45.	[redacted]	[redacted]	0.430
46.	[redacted]	[redacted]	0.426
47.	[redacted]	[redacted]	0.420
48.	[redacted]	[redacted]	0.415
49.	[redacted]	[redacted]	0.406
50.	[redacted]	[redacted]	0.394
51.	[redacted]	[redacted]	0.392
52.	[redacted]	[redacted]	0.388
53.	[redacted]	[redacted]	0.379
54.	[redacted]	[redacted]	0.377
55.	[redacted]	[redacted]	0.374
56.	[redacted]	[redacted]	0.369
57.	[redacted]	[redacted]	0.369
58.	[redacted]	[redacted]	0.364

	Prior Experience Job Title	Job Family	Similarity Score
59.	[redacted]	[redacted]	0.359
60.	[redacted]	[redacted]	0.357
61.	[redacted]	[redacted]	0.356
62.	[redacted]	[redacted]	0.354
63.	[redacted]	[redacted]	0.351
64.	[redacted]	[redacted]	0.348
65.	[redacted]	[redacted]	0.345
66.	[redacted]	[redacted]	0.342
67.	[redacted]	[redacted]	0.341
68.	[redacted]	[redacted]	0.341
69.	[redacted]	[redacted]	0.336
70.	[redacted]	[redacted]	0.332
71.	[redacted]	[redacted]	0.325
72.	[redacted]	[redacted]	0.325
73.	[redacted]	[redacted]	0.317
74.	[redacted]	[redacted]	0.316
75.	[redacted]	[redacted]	0.294
76.	[redacted]	[redacted]	0.292
77.	[redacted]	[redacted]	0.278
78.	[redacted]	[redacted]	0.261
79.	[redacted]	[redacted]	0.261
80.	[redacted]	[redacted]	0.248
81.	[redacted]	[redacted]	0.241
82.	[redacted]	[redacted]	0.233
83.	[redacted]	[redacted]	0.222
84.	[redacted]	[redacted]	0.215
85.	[redacted]	[redacted]	0.214
86.	[redacted]	[redacted]	0.207
87.	[redacted]	[redacted]	0.204
88.	[redacted]	[redacted]	0.199
89.	[redacted]	[redacted]	0.192
90.	[redacted]	[redacted]	0.187
91.	[redacted]	[redacted]	0.186
92.	[redacted]	[redacted]	0.170
93.	[redacted]	[redacted]	0.157
94.	[redacted]	[redacted]	0.148
95.	[redacted]	[redacted]	0.057
96.	[redacted]	[redacted]	0.054
97.	[redacted]	[redacted]	0.040

	Prior Experience Job Title	Job Family	Similarity Score
98.	[redacted]	[redacted]	0.037
99.	[redacted]	[redacted]	0.030
100.	[redacted]	[redacted]	0.010

Source: SAP analysis data set; Kenexa Applicant Data.

Table E.6: Regression Model of Gender Disparity in Similarity Score between Prior Job Titles and Starting Job Family, Employees Ever Employed in Job Levels P1-P6, P2L-P5L, M1-M3, B1-B4, T1-T4, TL, A1-A5, E0, E1, and E1X (Excluding Vice Presidents) During the Class Period, 2015-2022

		(I) Hired in 2015 - 2022		
		Coeff.	t-stat	p-value
		(1)	(2)	(3)
A.	*Similarity Score for Latest Prior Job Title*			
	Female	**0.0011**	**0.1706**	**0.8646**
B.	*Average Similarity Score for All Prior Job Title*			
	Female	**0.0062**	**1.2170**	**0.2237**
C.	*Average Similarity Score for All Prior Job Title Weighted by Prior Job Title Spells*			
	Female	**0.0067**	**1.3055**	**0.1918**
	Variables			
	Prior Experience (Sq.)	Yes		
	Exempt Status	Yes		
	Hourly, Contractor, and Union	Yes		
	Technology Job Indicator	Yes		
	Southern California Indicator	Yes		
	Southern California Indicator X Technology Job Indicator	Yes		
	Northern California Indicator	Yes		
	Northern California Indicator X Technology Job Indicator	Yes		
	All Education Variables	Yes		
	Year dummy variables	Yes (8)		
	Job Family X Job Level dummy variables	Yes (980)		
	Segment dummy variables	Yes (11)		
	Employee-Years with Missing Job Family X Job Level	7		
<u>*Missing Education Controls*</u>				
	Employees with Missing Highest Degree CWUR Ranking	1,602		
	Employees with Missing Highest Degree QS Ranking	2,109		
	Employees with Missing Highest Degree WSJ Ranking	2,322		
	Employees with Missing Highest Degree All Rankings	1,546		
	Observations	[redacted]		
	R-squared			
	Similarity Score for Latest Prior Job Title	0.4249		
	Average Similarity Score for All Prior Job Title	0.4857		
	Average Similarity Score for All Prior Job Title Weighted by Prior Job Title Spells	0.4780		

Source: SAP analysis data set; Kenexa applicant data.
See notes to Tables 8 and 10.

Table E.7: Equal Pay Analysis for Named Plaintiffs Using Segment, Job Family and Job Level

Employee Name	Personnel Number	Year	Segment	Job Family
Becky Train	[redacted]	[redacted]	[redacted]	[redacted]
Becky Train	[redacted]	[redacted]	[redacted]	[redacted]
Enny Joo	[redacted]	[redacted]	[redacted]	[redacted]
Enny Joo	[redacted]	[redacted]	[redacted]	[redacted]
Enny Joo	[redacted]	[redacted]	[redacted]	[redacted]
Enny Joo	[redacted]	[redacted]	[redacted]	[redacted]
LaRonda Rasmussen	[redacted]	[redacted]	[redacted]	[redacted]
LaRonda Rasmussen	[redacted]	[redacted]	[redacted]	[redacted]
LaRonda Rasmussen	[redacted]	[redacted]	[redacted]	[redacted]
LaRonda Rasmussen	[redacted]	[redacted]	[redacted]	[redacted]
LaRonda Rasmussen	[redacted]	[redacted]	[redacted]	[redacted]
LaRonda Rasmussen	[redacted]	[redacted]	[redacted]	[redacted]

Source: SAP analysis data set.