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The Hard-To-Measure Aspects Of Firms

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Abstract

This dissertation explores the aspects of firms that are hard-to-measure, hard-to-observe, and hard-to-quantify.

Chapter 3 investigates amenities of work that go beyond a wage. Using matched employee-employer data for the United States, this chapter estimates the joint distribution of wages, amenities, and job satisfaction across firms. There are three main findings. First, high-paying firms are high-satisfaction firms because they offer better amenities. Second, workers, especially high-earners, are willing to pay for job satisfaction, gaining in amenity value at least 50 percent of the average wage when moving from the worst- to the best-amenity firms. Third, since the elasticity of total compensation inclusive of amenity value to wages across firms exceeds one, incorporating non-wage amenities raises total compensation variance across firms by at least 52 percent.

Chapter 4 investigates firm reputation. Using workers' volunteered reviews on the platform Glassdoor, we find that the content most valuable to jobseekers (negative information) is the kind most risky to supply, pointing to a Catch-22. Higher ratings increase labor supply to less well-known firms, creating an incentive for smaller firms to discourage negative reviews. Concerns about employer retaliation discourage negative reviews and motivate employees who do disclose to conceal aspects of their identity, degrading the information's value. Reputation institutions provide valuable but partial solutions to workers' information problems.

Chapter 5 investigates firm culture. Using a sample of corporate scandals and data from the website Glassdoor, we study how negative reputation shocks affect the relationship between firms and their employees. Worker sentiment declines sharply and persistently following scandals, driven by diminished perceptions of management and culture. While base earnings and fringe benefits remain unchanged, variable compensation falls 10 percent. Our results demonstrate that rank-and-file employees are adversely impacted by corporate misconduct.

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ABSTRACT

THE HARD-TO-MEASURE ASPECTS OF FIRMS

Jason Sockin

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Chapter 1 investigates amenities of work that go beyond a wage. Using matched employee-employer data for the United States, this chapter estimates the joint distribution of wages, amenities, and job satisfaction across firms. There are three main findings. First, high-paying firms are high-satisfaction firms because they offer better amenities. Second, workers, especially high-earners, are willing to pay for job satisfaction, gaining in amenity value at least 50 percent of the average wage when moving from the worst- to the best-amenity firms. Third, since the elasticity of total compensation inclusive of amenity value to wages across firms exceeds one, incorporating non-wage amenities raises total compensation variance across firms by at least 52 percent.

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1.1 Introduction

Jobs are inherently complex, reflecting "many margins" beyond just a wage (Clemens, 2021). Whether these many margins, i.e. job amenities, complement or substitute for wages remains empirically unanswered, despite the growing consensus that workers value non-pecuniary aspects of work (Akerlof et al., 1988; Mas and Pallais, 2017; Maestas et al., 2018). Do higher-paying firms offer more favorable work, or compensate for offering less favorable work? Since either is theoretically possible (Sorkin, 2018), data on firm-level amenities should offer the best opportunity to settle this debate.[1] Obtaining such data, however, has proven difficult. Current wage levels are easily observed and abundantly available, non-wage attributes are not. For one, it is not obvious what non-wage aspects would be included or even how to go about measuring them. In addition, workers may have heterogeneous experiences

[1] On one hand, Rosen (1986a) motives push wages and job attributes to move inversely, depending on firms' marginal costs of providing amenities: Among firms of similar productivity levels, those with a comparative advantage in amenity provision provide better amenities but lower wages. On the other hand, Mortensen (2003) motives push wages and job attributes to comove, as amenities are an alternative medium by which to compensate workers: More-productive firms can afford greater wages and more amenity value than less-productive ones. While some have presented evidence that the labor market operates within a compensating differential framework (Eriksson and Kristensen, 2014; Sorkin, 2018; Jäger et al., 2021), others have documented the absence of such differentials (Bonhomme and Jolivet, 2009; Maestas et al., 2018).

with amenities, meaning objective measures alone may not fully capture amenity quality.[2] Determining how firms' amenities correlate with wages would require new data on amenities for workers and firms over time. With the advent of online labor market platforms, in this case Glassdoor, such an exercise is now feasible.

This paper estimates which job amenities workers value, which firms offer more favorable amenities, and the aggregate importance of amenities as compared with wages. The building block for this analysis is workers' free response descriptions of the positive and negative aspects of their jobs on the website Glassdoor. To extract nuanced amenities from these unstructured but meaningful texts, I apply the semi-supervised topic modeling approach of Gallagher et al. (2017). Topic modeling allows researchers to capture hard-to-define themes in text that humans may be unable to detect themselves. A semi-supervised approach allows researchers to further guide the model to help ensure the outputted themes are interpretable. Applying this model to workers' reviews on Glassdoor, I summarize the quality of fifty amenities workers detail about their employers. The fifty amenities capture the multidimensional nature of different characteristics of work, including pecuniary traits related to wages and fringe benefits, as well as non-pecuniary traits related to working conditions, human capital investments, and interpersonal relationships. Since each employee review constitutes an employee-employer match where I can

[2]Suppose, for instance, coworker quality has value to workers (Jäger et al., 2021) and is a function of both ability and friendliness. While the former can be proxied for using coworkers' wages, the latter reflects a degree of sentiment that requires individuals' own perceptions of their workplaces.

identify the worker and the firm, by using data on job switchers who leave multiple reviews, I can capture firm-level differences in job satisfaction and the quality of each amenity vis-à-vis the firm fixed effects from the canonical two-way fixed effects model of Abowd et al. (1999), hereafter AKM.

This first main contribution of this paper fills a gap in our understanding of firms by first developing measures of job satisfaction and non-wage amenities that are firm-specific. Previous empirical work has established differences in amenity evaluations (Maestas et al., 2018) and job satisfaction (Jäger et al., 2021) between differently-paid workers, but has not been able to speak to the effects attributable directly to firms. Estimating each firm's relative premium for wages and for job satisfaction, I find that higher-paying firms provide a higher degree of job satisfaction: a one-standard-deviation increase in the wage premium a firm offers its workers (compared with other firms) is associated with 0.10 standard deviations improved job satisfaction. As such, workers who transition to a higher-paying firm tend to enjoy not only greater wage growth on average, but also an associated increase in job satisfaction. This increase in satisfaction is broad-based, with workers reporting improved sentiment along dimensions of work at best indirectly related to wages, including career opportunities, culture and values, senior management, and work-life balance. This pattern is evident within and across industries, suggesting that inter-industry wage differentials do not fundamentally reflect equalizing compensation for unfavorable work (Krueger and Summers, 1988). Even when looking within the same firm, workers' job satisfaction levels improve when the firm raises pay though the relation attenuates by three-quarters.

In capturing job amenities directly, my analysis does not rely upon the commonly used assumption that moves to lower-wage firms can be rationalized by unobserved, positive changes in amenities (Bonhomme and Jolivet, 2009; Sullivan and To, 2014; Sorkin, 2018; Lamadon et al., 2019; Taber and Vejlin, 2020). In fact, I find little empirical evidence validating this assumption: pay cuts in my data are more frequent when moving to lower-satisfaction firms, not higher-satisfaction ones. Rather, most job amenities improve with pay. While high-paying firms offer worse job security, they provide better interpersonal relationships, invest more in human capital, supply more favorable fringe benefits, and offer superior working conditions. This finding runs counter to the notion that firms' wage premia primarily compensate for unfavorable job characteristics (Rosen, 1986a; Sorkin, 2018), and instead supports more-productive firms offering improved amenities (Mortensen, 2003; Lamadon et al., 2019). Decomposing job satisfaction into these fifty amenities reveals that only 8–12 percent of workers' job satisfaction ratings relate to pay satisfaction — in part reflecting how one-third of non-wage amenities have a more pronounced effect on job satisfaction than does satisfaction with pay.

The second main contribution estimates the value workers place on job satisfaction. Non-wage amenities, e.g. one's coworkers, managers, autonomy, and respect, are non-pecuniary in nature and as such, not explicitly priced. In the spirit of

Gronberg and Reed (1994), I estimate how long workers remain with an employer (job tenure) as a function of both wages and job satisfaction. If the utility a worker derives from her employment rises with her wage and satisfaction with the job, then both should positively influence whether she remains longer in the match. The resulting estimates can be used to provide a dollar-equivalent of job satisfaction in terms of its ability to attract and retain workers. Looking at workers' completed job spells, since greater wages and improved job satisfaction each elongate firm tenure, workers exhibit a positive willingness to pay for job satisfaction. Even after removing the portion of job satisfaction attributable to pay satisfaction, there are stark disparities in non-wage amenity value between firms: workers gain in amenity value at least 50 percent of the average wage when moving from the first to the ninety-ninth percentile of firms.

The third main contribution of this paper examines whether amenities amplify or attenuate firm-level inequality in total compensation (wages plus amenity value). Settling this debate remains an ongoing issue: While Lamadon et al. (2019) find that more-productive firms offer better amenities, Sorkin (2018) concludes that three-quarters of the wage premia firms offer reflect compensating differentials. If high-wage firms offer less favorable non-wage amenities, then the disparity in total compensation will be thinner than the disparity in wages. The reverse is true if high-wage firms offer more favorable amenities. Since relating firms' total compensation premia to their wage premia reveals an elasticity above one, amenities amplify inequality across firms. Incorporating non-wage amenities raises compensation variance across firms by 50–65 percent and widens the compensation gap between the tenth and ninetieth percentiles of firms by 10–15 log points. Job satisfaction data thus reveal that wages understate inequality between firms in the U.S. labor market. Improved amenities may therefore also help explain the high degree to which high-wage workers sort into high-paying firms (Card et al., 2013; Song et al., 2019; Bonhomme et al., 2020)

The rest of this paper is organized as follows: Section 3.1 discusses the relevant literature, Section 1.3 describes and validates the Glassdoor data, Section 1.4 investigates the relation between wages and job satisfaction across firms, Section 1.5 introduces fifty job amenities extracted from workers' descriptions of their employers, Section 1.6 quantifies how workers are willing to pay for job satisfaction, Section 1.7 estimates firm-level dispersion when amenity value is considered alongside wages, Section 1.8 investigates the robustness of the results to alternative modeling decisions, Section 1.9 highlights implications of the results while mentioning limitations, and Section 1.10 concludes.

1.2 Relevant Literature

In uncovering disparities in amenity quality across firms, this work relates to a number of important strands of the literature. First is a budding literature on the importance of the non-wage aspect(s) of jobs for understanding labor market dynamics. Non-wage characteristics of jobs have been found to be valued enough to affect workers' preferences for jobs and labor market sorting (Sullivan and To, 2014; Hall and Mueller, 2018).[3] Examples include occupational fatality risk (DeLeire and Levy, 2004), the degree of social interaction (Krueger and Schkade, 2008), and flexibility with respect to time and location (He et al., 2021). Non-wage attributes have also been found to be especially important in understanding differences in jobseeker behavior by gender.[4]

Since non-wage amenities vary between jobs, there is dispersion that wages alone may fail to capture. Taber and Vejlin (2020) estimate that the variance of wages plus non-pecuniary aspects is more than twice as large as the variance of wages alone — through the lens of a Roy model with compensating differentials (i.e., omitting the Mortensen (2003) channel), search frictions, and human capital. From omitting job characteristics, such as workplace safety (Park et al., 2021), working at convenient times of the day (Hamermesh, 1999), fringe benefits (Piketty et al., 2017), sexual harassment (Folke and Rickne, 2020), or labor rights violations (Marinescu et al., 2020), we may understate total inequality between workers of different education levels (Duncan, 1976) or wages (Maestas et al., 2018).

Second is a literature characterizing workers' willingness to pay for non-wage

[3]Improved signals of employer quality that reflect the non-wage aspects work have been shown to increase labor supply: Turban and Cable (2003) using "the best companies to work for" lists published by various media outlets and Sockin and Sojourner (2020) using Glassdoor employer ratings.

[4]Examples include the provision of parental leave benefits (Liu et al., 2019; Fluchtmann et al., 2020), commuting length (Herzog and Schlottmann, 1990; Le Barbanchon et al., 2020), competition (Sockin and Sockin, 2019b), and workplace flexibility (Bender et al., 2005; Goldin and Katz, 2011).

attributes. Workers will accept lower wages to avoid bad working conditions and frequent physical activity (Gronberg and Reed, 1994), enjoy reduced workplace hazards and a flexible work schedule (Felfe, 2012), have job security (Bonhomme and Jolivet, 2009), avoid unanticipated work schedules (Mas and Pallais, 2017), receive faster earnings growth (Wiswall and Zafar, 2017), have a flexible work arrangement (Chen et al., 2019), conduct more meaningful work, experience less work-related stress and have job autonomy, teamwork, job training, and paid time off (Maestas et al., 2018). Willingness-to-pay estimates can also be quite large: Maestas et al. (2018) estimate that transitioning from the worst-amenity job to the best (as characterized by their set of nine amenities) would be valued at a 56 percent wage increase.

Third is a literature related to the determinants and implications of job satisfaction. Locke (1969) theorizes that job satisfaction captures every element of which a job is comprised, and reflects not only the objective quality of each aspect, but individuals' subjective perceptions and value rankings as well. Although greater pay is associated with more pay satisfaction (Bryson et al., 2012), overall job satisfaction has been found to predominantly reflect non-pecuniary rather than pecuniary aspects of work (Akerlof et al., 1988; Clark, 1998) — though disparities in pay among peers, which could arguably reflect non-wage characteristics such as fairness and respect, can stunt job satisfaction (Card et al., 2012). Consistent with other work that has found job satisfaction to be an important predictor for why workers voluntarily quit (Freeman, 1978; Bartel, 1982; Akerlof et al., 1988; Clark, 2001; Card et al., 2012), I find that more-satisfied workers exhibit longer firm tenure. And since job satisfaction primarily reflects non-wage characteristics (88–92 percent), job amenities constitute meaningful drivers for worker turnover.[5] To borrow a quote from Akerlof et al. (1988), "As man does not live by bread alone, people do not quit only for wages."

Last is a literature on the role of firms in explaining worker compensation. The AKM model quantifies the role of firms by regressing workers' wages on fixed effects for the worker and the firm, a linearly additive view validated by Bonhomme et al. (2019). Estimates for the share of the variance in wages attributable to firms typically ranges from 15–25 percent (see Bonhomme et al. (2020) for a summary of the literature). That range falls to 5–13 percent and the contribution from the sorting of workers into firms rises after accounting for limited mobility, i.e. firms on average having few job switchers in the data (Andrews et al., 2008; Kline et al., 2020; Bonhomme et al., 2020). To account for limited mobility, I consider, in addition to the full sample of firms, a more-connected set of firms with many job switchers in the data. While there is some nascent work examining non-wage attributes across firms, empirical measures for amenities are almost entirely absent, likely reflecting the unavailability of such data.[6] As a result, amenity value has to be inferred from

[5] Jäger et al. (2021) find that in a survey where workers were asked their reasons for not switching to new employers, the primary reasons given pertained to non-wage components such as job security, work atmosphere, work schedule, and colleagues rather than difficulty in finding a better-paying job.

[6] One notable exception is the work of Lagos (2019) who captures amenities across Brazilian

wages and job transitions. The positive correlation I document between wages and non-wage attributes (without having to impute amenity value from wage data) lends empirical support to the findings of Lamadon et al. (2019), while at the same time, is in line with workers at higher-paying firms participating more in social insurance programs (Bana et al., 2018; Lachowska et al., 2021) and job satisfaction improving with coworkers' wages (Clark et al., 2009).

1.3 Data Description

1.3.1 Sources

The data for this analysis come primarily from the online platform Glassdoor, where jobseekers can go to obtain labor market information about prospective employers provided by current and former employees of each firm. Workers are incentivized to volunteer their own experiences through a "give-to-get" policy whereby contributors gain access to the information others have provided. Workers can submit one or more of the following: an employer review, a pay report, an interview review, or a benefits review. This work makes use of the first two: the former captures job satisfaction and non-wage amenities, the latter wages.

firms using a textual-based analysis of collective bargaining agreements between unions and employers. Whereas I use within-worker differences in job satisfaction to identify firm-level amenities and allow for vertical differentiation in amenities across firms, Lagos (2019) estimates firm-level amenities based on how collective bargaining agreements with the firm change over time, where amenity value is estimated conditional on wage growth vis-à-vis compensating differentials, capturing instead horizontal differentiation in amenities, i.e. analyzing how the wage-amenity bundle evolves holding productivity constant.

When submitting an employer review, each worker is first asked which firm they would like to review and whether they are a current or former employee of the firm. If she is a former employee, she is then prompted for the last year she was employed at the firm. The worker can then rate the employer overall on a one-to-five-stars integral scale, with more stars indicating greater satisfaction, and provide free-text responses describing the 'Pros' (i.e., positive characteristics) and the 'Cons' (i.e., negative characteristics) of working for the firm.[7] In addition, the worker can rate the employer along five sub-dimensions (career opportunities, compensation and benefits, culture and values, senior management, and work-life balance) on the same one-to-five-stars scale. Each worker can also provide the location of their employment, their job title, and their years of tenure with the firm.[8] For an in-depth description of the Glassdoor reviews data, which span 2008–2021, see Green et al. (2019).

When submitting a wage report, each worker is first asked for their job title and whether they are a current or former employee of the firm. Again, if she is a former employee, she is prompted for the last year employed with the firm. The worker then provides her base wage, pay frequency (annually, hourly, or monthly), variable pay (e.g., bonuses and commissions), years of experience, employment status (e.g., full-time or part-time), employer name, and location. Given that hours are not

[7]Respondents are not prompted to report their wage when submitting an employer review. As such, the concern that workers will not discuss pay when completing the free-response text because they provide wage information elsewhere in the submission form is not present.

[8]Disclosing one's job title and one's location is not required to submit a review.

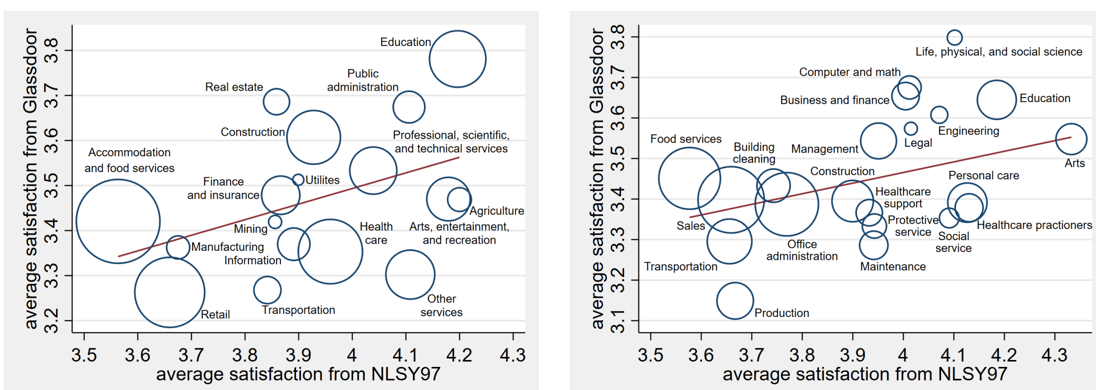
observed, I restrict the sample to only full-time workers. From here on, workers' wages refers to their base earnings, meaning variable pay — which itself could be considered an employer's fringe benefit — is omitted. For consistency across workers, I annualize wages assuming hourly employees work 2,000 hours per year and monthly employees work for twelve months. For a thorough discussion of the Glassdoor wage data, which span 2008–2021, see Sockin and Sockin (2019a).

1.3.2 External Validity

In order to make broad statements about the U.S. labor market, it is important to first show that Glassdoor ratings accurately capture labor market patterns observed in other datasets. Given the subjective nature of our main measure of interest, job satisfaction, possible datasets that can be used for comparison are necessarily restricted to worker surveys. Though measures of job satisfaction in publicly-available surveys are scant, the National Longitudinal Survey of Youth 1997 (NLSY97) asks respondents whether they are satisfied with their jobs on a 1–5 integral ranking, the same system used for Glassdoor ratings. Workers in the NLSY97 sample are more positive in their job assessments than workers in the Glassdoor sample. In the NLSY97, the average satisfaction level is 3.85, and only 10.7 percent of workers report having either of the two lowest satisfaction levels. For comparison, the average overall rating in Glassdoor is 3.47 and 25.4 percent of workers submit ratings of either one or two stars.

Though the average and shape of the ratings distributions may be dissimilar, for our purposes, the validity of using these ratings data rests in whether the Glassdoor sample accurately reflects disparities observed between different employment opportunities. To that end, I compare the average job satisfaction level between the two datasets both by two-digit NAICS industry and by two-digit SOC occupation, the scatterplots for which are displayed in panels (a) and (b) of Figure 1.1, respectively.[9] Across seventeen industries, we observe a robust positive correlation (0.51, p-value = 0.037), meaning that industries that tend to have high levels of satisfaction in the NLSY97 also have relatively high ratings in Glassdoor. Similarly, across twenty-one occupations, there is a strong positive correlation (0.47, p-value = 0.031), highlighting that Glassdoor ratings also capture differences between jobs.

Figure 1.1: Comparison of Glassdoor and NLSY97 Satisfaction Levels



(a) By industry

(b) By occupation

Notes: This figure plots the relation between the average job satisfaction in the National Longitudinal Survey of Youth 1997 and average overall rating in Glassdoor by industry or occupation. Solid line indicates linear line of best fit and shaded region indicates 95% prediction interval. Industries and occupations are weighted by the total representative weight for each grouping from the NLSY97.

[9] Across 309 NAICS industry x two-digit SOC occupation pairs, the correlation is 0.35 (p-value < 0.000).

Next, I show that Glassdoor wage data capture broad trends in the U.S. labor market. One main advantage of using this dataset is that each observation constitutes an employer-employee match, allowing for the study of firm-specific wage-amenity bundles. Glassdoor wages have been to validate findings from other datasets, e.g., Derenoncourt et al. (2021). Karabarbounis and Pinto (2019) find that, conditional on industry or region, the wage distribution in Glassdoor broadly captures the respective distributions obtained from the Quarterly Census for Employment and Wages (QCEW) and the Panel Study of Income Dynamics (PSID); though Karabarbounis and Pinto (2019) note that the distribution of employment by industry in Glassdoor is not representative — an issue less relevant for this work given the use of industry- and firm-level controls in predicting worker outcomes. Martellini et al. (2021) compare the average earnings of college graduates by university within Glassdoor with the averages produced by the U.S. Department of Education's College Scorecard from tax data, and find that Glassdoor provides an unbiased sample of U.S. earnings by college. I add to this by showing that Glassdoor wages reflect the differences observed in the Annual Social and Economic Supplement of the Current Population Survey (ASEC) between industries and occupations.

For both Glassdoor and ASEC, I take the (logarithm of) annual labor earnings for full-time workers and calculate the mean, median, standard deviation, and interquartile range within industry-occupation pairs. The relations between the two samples for these four summary statistics are presented in Table 1.1. Glassdoor earnings data capture the first moment of wages well, exhibiting a correlation above 0.9 with ASEC and an elasticity of 1.2–1.3, highlighting that Glassdoor somewhat overestimates earnings for high-wage jobs. With regards to the dispersion of earnings, the semblance between the two datasets is noticeably weaker albeit still appreciably positive. For the standard deviation and interquartile range, we observe correlations of 0.44–0.48 between the two surveys. Therefore, conditional on industry and occupation, Glassdoor data capture meaningful differences in U.S. labor earnings observed in other datasets.

Table 1.1: Earnings in Glassdoor and the Annual Social and Economic Supplement

	ASEC earnings statistic			
	Mean log earnings	Median log earnings	Standard deviation log earnings	Interquartile range log earnings
Glassdoor wage statistic	1.348***	1.217***	0.709***	0.677***
	(0.028)	(0.027)	(0.072)	(0.061)
Industry-occupations	409	409	409	409
R^2	0.85	0.83	0.19	0.23
Mean ASEC weight	18839	18839	18839	18839
Correlation	0.923	0.911	0.439	0.483

Notes: This table reflects coefficients from regressions of moments in ASEC-level earnings data on the same moments for Glassdoor wage data at the Glassdoor industry x two-digit SOC occupation. Earnings in ASEC reflect inflation-adjusted total pre-tax wage and salary income for full-time workers. Glassdoor wages excluding variable-based earnings such as bonuses, commissions, tips or overtime pay. Regressions are weighted according to representative ASEC weights. Industry-occupations restricted to those with at least fifty observations in Glassdoor. Significance levels: * 10%, ** 5%, *** 1%.

1.4 Relation Between Wages and Job Satisfaction

Despite the overwhelming growth in new data available to researchers, the question of whether wages and job amenities move inversely or in tandem remains an open debate. For attributes that are unambiguously undesirable ex ante, such as increased risk of fatality, the answer is fairly definitive.[10] However, to what extent job characteristics that are harder to observe and harder to measure vary with wages remains unclear. While some empirical work has touched upon this question, more work is needed as jobs are complex and amenities numerous.[11] Starting with job satisfaction, I show in this section that high wages command greater levels of satisfaction, first presenting suggestive cross-sectional evidence and then estimating two-way fixed effects models to gauge whether workers trade-off wages and job satisfaction across firms.

1.4.1 Job Satisfaction as a Reflection of the Amenity Bundle

Determining whether variance in wages alone overstates or understates differences in total compensation requires also estimating the degree to which job amenities differ.

[10] As Smith (1979) framed it then, the evidence of compensating differentials — in which wages trade-off with dis-amenities — has been ambiguous with regards to job attributes except for fatality risk.

[11] For instance, Pierce (2001) shows that high-income earners receive more voluntary non-wage compensation vis-à-vis fringe benefits, Dey and Flinn (2005) that jobs offering health insurance pay higher wages, and Maestas et al. (2018) that working conditions appear to broadly improve with wages.

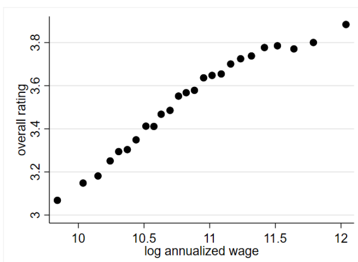
The sheer breadth of possible job amenities makes this task especially daunting. To quote Clemens (2021), there is a "'many margins' problem, in which the relevance of various attributes may vary substantially across settings." Some firms may offer better fringe benefits but offset those benefits with worse working conditions such as requiring more tasks and imposing more rigid work schedules. Some firms may invest more in on-the-job training while promoting a competitive environment with worse job security. Comparing any singular amenity though will inherently overlook the correlation with other amenities provided by the firm. To quantify the full scope of job amenities, especially non-pecuniary ones, ideally one would use an aggregation mechanism that incorporates how workers value the collective bundle. To this end, I use workers' levels of job satisfaction.

The extent to which workers are satisfied with their jobs will depend not just on the pecuniary rewards they receive, but the non-pecuniary aspects as well. Akerlof et al. (1988) find that more than 80 percent of workers cite a non-pecuniary attribute as the primary reason for their satisfaction if they like their job. And just because a worker is highly-paid does not necessarily mean they will be more satisfied with their job: While high-wage workers in our sample on average report greater levels of job satisfaction (Figure 1.2: panel a), 18 percent of workers in our sample who earn above $50,000 (in 2018 dollars) report either of the two lowest satisfaction levels.[12] My interpretation of job satisfaction is one akin to Hamermesh (2001),

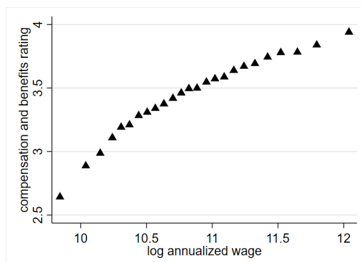
[12] This positive correlation between wages and job satisfaction is evident in other survey data, such as the NLSY97 (Figure A.3), and is well-documented in the literature (Judge et al., 2010).

who argues that “the satisfaction that workers derive from their jobs, might be viewed as reflecting how they react to the entire panoply of job characteristics.” In other words, job satisfaction reflects a mapping from total compensation inclusive of amenities, $(w, \vec{a})$, to a measure of satisfaction — where the resultant level of satisfaction also depends upon how the worker subjectively weights w and each amenity a.

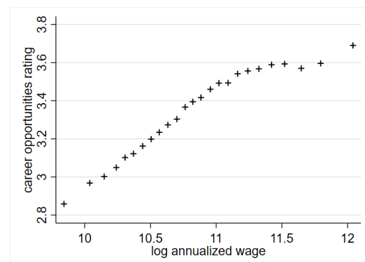
Figure 1.2: Workers’ Overall and Subcategory Ratings by Wage Level



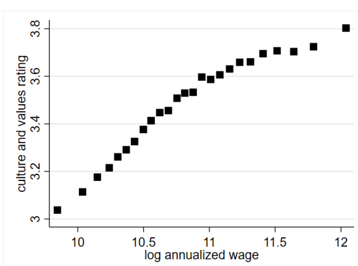
(a) Overall



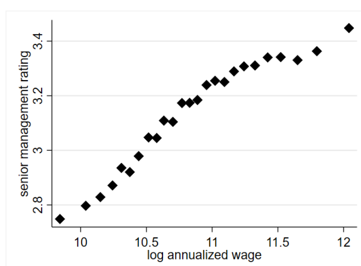
(b) Compensation and benefits



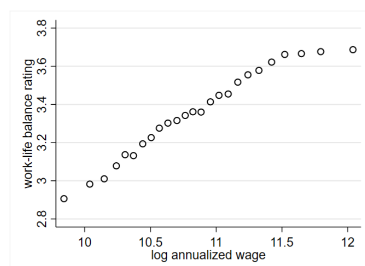
(c) Career opportunities



(d) Culture and values



(e) Senior leadership



(f) Work-life balance

Notes: This figure plots the average rating among Glassdoor reviews for overall ratings and the five sub-categories against workers’ log wages.

Naturally then, if job satisfaction rises, this increase could reflect either an improvement in wages or amenities, or both. And conversely, if wages rise, we would expect job satisfaction to improve as workers become more satisfied with their pay. The fact that high-wage workers report greater levels of both job satisfaction and pay satisfaction (panel b) is consistent with this narrative. However, that high-wage workers also report greater levels of satisfaction with aspects of work at best tangentially related to pay, including career opportunities, culture and values, senior leadership, and work-life balance (panels c–f), would suggest that high-wage workers also enjoy higher quality amenities.

1.4.2 Estimating Firm-Specific Wage and Job Satisfaction Premia

The theory of compensating differentials reflects the wage premia required to equalize the advantages and disadvantages that arise between different work opportunities (Rosen, 1986a). Given the utility-based nature of compensating differentials — necessitated by the fact that some aspects of work are non-pecuniary in nature — empirical researchers often estimate workers' willingness to pay for job atributes to understand the trade-offs workers face. Typically, this has taken the form of studying job transitions (Bonhomme and Jolivet, 2009; Sorkin, 2018) or work decisions (Chen et al., 2019) through a revealed preference framework, or interpreting hypothetical employment choices workers make in surveys (Maestas et al., 2018; Wiswall and Zafar, 2017). These approaches exist through the perspective of workers choosing between a menu of options; however, the composition of said menu matters for understanding amenity valuations. If some amenities, for instance career opportunities, monotonically improve with wages, then job transitions alone will fail to dis-entangle the value workers place on career opportunities as no trade-off is present. To the extent that firms have control over both the wages they offer and the bundle of amenities (and their quality) that they produce, then the trade-offs workers face should arise from looking at supply-side differences across firms.

Given that Glassdoor wages and ratings data are comprised of employee-employer matches, I follow the two-way fixed effects literature to estimate firm-specific premia separately for wages and job satisfaction. Employee-employer matched data have been seldom used in the compensating-differentials literature, and when they have, the research question typically centers on understanding workers' willingness to pay for safety and reduced fatality risk (Lalive, 2003; Dale-Olsen, 2006; Lavetti and Schmutte, 2018). Whereas the fixed effects in these analyses act as nuisance parameters to abstract from unobserved differences across workers and firms, this work instead uses the firm fixed effects as objects of study for understanding the causal effect from moving between firms, as in Bana et al. (2018) and Lachowska et al. (2021). Though each worker alone only provides a vignette of the firm by detailing the quality of a few attributes in a few reviews, the collection of experiences provided by the set of workers who have transitioned into or out of each firm can provide a complete picture of firms' amenity bundles.

For log wages and overall job satisfaction ratings — the distributions for which are presented in Figure A.4 — I estimate AKM models of the form,

$$Y_{ikt} = \lambda_i + \lambda_k + \lambda_t + \gamma X_{it} + \varepsilon_{ikt} \tag{1.4.1}$$

where Y_{ikt} is log annual wage or overall star rating for worker i employed at firm k in year t, λ_i, λ_k, and λ_t are worker, firm, and year fixed effects, respectively, and X_{it} is a vector of workers observables (a fourth-order polynomial in work experience for wages; indicators for current or former employee and employment status, e.g., full-time or part-time, for overall ratings).[13] In this model, the firm fixed effects λ_k are identified from job switchers who report their wage or satisfaction for different firms, thereby capturing the extent to which the same worker receives more or less pay and higher or lower satisfaction at firm k compared with the other firms at which the individual has worker.[14] From workers' wages, I obtain firm-specific pay premia $\hat{\lambda}_k^w$ — the traditional AKM application, and from workers' overall ratings, I obtain firm-specific satisfaction premia $\hat{\lambda}_k^R$ — the novel AKM application intended to holistically capture dispersion in non-wage amenities.

To assess the importance of firms, I first decompose the variance of workers'

[13]A firm represents the collection of establishments across the United States rather than each establishment separately, though treating establishments separately does not alter the findings (Table 1.8: Row 22).

[14]The firm fixed effects for wages are estimated using a mostly different sample of workers than that used to estimate the firm fixed effects for job satisfaction. Roughly three-quarters of the workers in each sample are not represented in the other. In turn, because most workers contribute to either the wage premia or the ratings premia but not both, this sidesteps the concern that the wage-satisfaction relation I observe across firms is driven by selection related to workers' own wage-satisfaction preferences. The takeaway is unchanged though when only workers who are included in both panels are considered (Table 1.8: Row 23).

wages and overall ratings in Table 1.2 into components attributable to the worker, the firm, the covariance between the two, and the left unexplained error term. I consider two samples. The "Full" sample reflects any firm for which a fixed effect is obtainable, i.e. there exists at least two movers in our sample who transition into or out of the firm either from or to a firm which also has at least two such movers. The "Connected" sample restricts the set of firms to those with at least fifteen such movers, in the spirit of Bonhomme et al. (2020) to address the issue of limited mobility bias in two-way fixed effects models (Abowd et al., 2003).[15]

Firms account for 9.3 percent of the variance in log wages in the Full sample and 6.5 percent in the Connected sample. While below the roughly 20 percent consensus found in the literature (Card et al., 2018), these estimates are consistent with those from a more-connected set where limited mobility bias is of less consequence and positive sorting between workers and firms plays an increasingly important role (Bonhomme et al., 2020). For job satisfaction, firms play a more substantive role. Firms account for 21.9 percent and 11.0 percent of the variance in ratings for the Full and Connected samples, respectively — roughly twice the contribution of firms to wages. That firms are relatively more predictive of job satisfaction would suggest that firms play a more important role in setting amenities

[15] Limited mobility bias refers to how the precision with which the firm fixed effects in an AKM framework are estimated relies upon how many movers there are to represent each firm. The fewer movers there are, the more important firms are in explaining the variance across workers (Andrews et al., 2008). Since the job transitions of movers identify the firm-specific constants, using a more-connected set of firms with many movers can correct this bias. Bonhomme et al. (2020) argue this point by re-estimating their AKM model using iteratively smaller fractions of the total movers present for each firm. I implement a similar exercise for the Connected set of firms using the wages and ratings data in Figure A.5 and find similar patterns.

than wages, and that there is more dispersion across firms in amenities than in wages. While about 8 percent of the variance in wages is left unexplained by workers, firms, and observable characteristics, 23–26 percent of the variance in ratings is left unexplained. The unexplained variance in satisfaction could reflect myriad factors, such as match-specific effects, occupation, location, time-varying preferences, or measurement error induced by a discrete metric.

Table 1.2: Decomposition of Variance for Wages and Ratings

	Log wages		Overall ratings	
Variance	Full	Connected	Full	Connected
Total	0.279	0.276	2.284	2.124
Worker	0.189	0.190	1.376	1.199
Firm	0.026	0.018	0.499	0.234
Cov(Worker, Firm)	0.014	0.016	-0.191	-0.034
Residual	0.021	0.021	0.527	0.562
Number of Firms	117,749	14,230	99,167	9,841
Number of Workers	1,025,916	691,546	565,704	312,149
Number of Observations	2,207,583	1,497,251	1,263,222	703,147

Notes: This table displays the variance decomposition for log wages and overall ratings for the Full sample of firms (all firms) and the Connected sample of firms (firms represented by at least fifteen movers).

Further details regarding the panel of movers' wages and ratings used in estimating these two-way fixed effects models are provided in Table A.11. Although I observe more than one wage or overall rating for many workers — allowing for the identification of firm-specific premia — each worker records on average only 2.2 observations in the wages panel and 2.4 observations in the ratings panel. As such, one limitation of this analysis is the inability to conduct robustness exercises for these AKM models, such as controlling for match-specific differences that may endogenously determine mobility decisions (Lavetti and Schmutte, 2018), e.g., through learning about ability or match quality over time (Gibbons et al., 2005; Menzio and Shi, 2011b), or estimating event studies of dynamic wage or ratings changes around job transitions between firms of varying premia to confirm exogenous growth at transitions (Card et al., 2013). However, estimating the gains and losses from transitioning to a firm of a higher or lower decile of firm premium for wages (Table A.12) and satisfaction ratings (Table A.13) reveals that the changes are roughly symmetric, supporting the linearly additive AKM framework. The average duration between a workers' pair of observations is 2.5 years for wages and 2.0 years for ratings, 78–80 percent of pairs in each panel represent the worker switching firms, and transitions for both current and former employees are observed frequently in each sample, highlighting that the samples do not appear negatively selected on representing low productivity workers who have left the firm and searching off the job.

1.4.3 When Workers Transition Between Firms

In this subsection, I examine how workers' individual outcomes change when transitioning to firms of different wage premia $\hat{\lambda}_k^w$ or job satisfaction premia $\hat{\lambda}_k^R$. Under the AKM framework, how workers' earnings or satisfaction levels co-move with these firm-level measures can be interpreted as the causal effect of the firm.

Consider a worker i who was employed with firm k in year t and decides to transition to a new firm k' where they are observed in year t'. If the worker leaves an employer review for both firms, then I observe the pair of ratings $(R_{ikt}, R_{ik't'})$. Having experienced the wage-amenity bundles offered by each firm, the worker reports her overall satisfaction with each. How does the difference in wages offered by each translate into differences in satisfaction? On one hand, if higher-paying firms cut amenities to offset labor costs, then moving to a higher-paying firm may result in a non-positive change in job satisfaction, depending upon how workers subjectively weight wages and amenities. Inversely, if higher-paying firms supplement high wages with better amenities, then moving to a higher-paying firm should directly boost job satisfaction, as the compensation bundle improves along both dimensions.

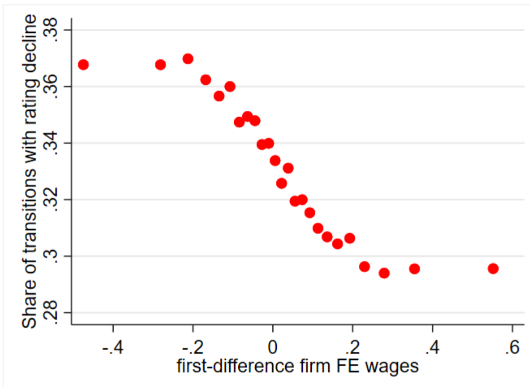
I relate firm-level wage and satisfaction premia to individual outcomes by considering first-difference models of the form

$$\mathbb{1}\{R_{ik't'} < R_{ikt}\} = \beta_R(\hat{\lambda}^w_{k'} - \hat{\lambda}^w_k) + \xi_t + \xi'_t + \varepsilon_{iktk't'} \tag{1.4.2}$$

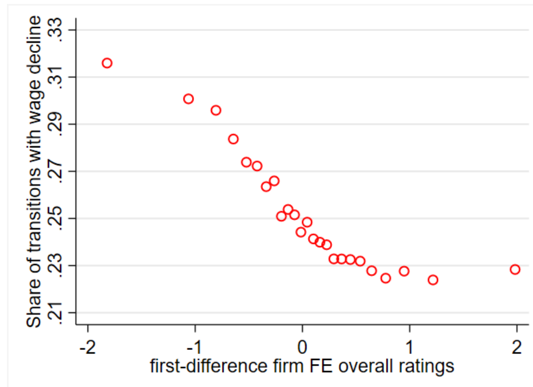
$$\mathbb{1}\{w_{ik't'} < w_{ikt}\} = \beta_w(\hat{\lambda}^R_{k'} - \hat{\lambda}^R_k) + \xi_t + \xi'_t + \varepsilon_{iktk't'}. \tag{1.4.3}$$

The first coefficient of interest, β_R, captures the difference in the probability of a worker experiencing a job satisfaction decline from working for a firm offering one percent greater wages. Panel a of Figure 1.3 depicts this relation within bins according to $\hat{\lambda}^w_{k'} - \hat{\lambda}^w_k$, and reveals a clear *negative* effect. As workers move to lower-paying firms, outside the tails of the distribution, the probability of experiencing a job satisfaction decline rises steadily.[16]

Figure 1.3: Growth in Job Satisfaction and Wages by the Change in Firm Premia



(a) P(rating decline)



(b) P(wage decline)

Notes: This figure depicts the probability of a worker experiencing a decline in overall rating (panel a) and the probability of a worker experiencing a (real) wage decline (panel b) when transitioning between firms that differ in their wage and ratings premia, respectively (x-axis). Observations are partitioned into twenty-five bins according to the measure on the x-axis.

The second coefficient of interest, β_w, captures the difference in the probability of a worker experiencing a real wage decline working for a firm that offers one star greater job satisfaction. Panel b of Figure 1.3 depicts this relationship within bins according to $\hat{\lambda}^R_{k'} - \hat{\lambda}^R_k$, and reveals a clear negative effect, i.e., pay cuts are increasingly *more* likely to occur when workers transition to lower-satisfaction firms. A worker accepting a wage decline is not an infrequent occurrence in the U.S. labor market, with estimates in the range of 23–43 percen of job transitions (Jolivet et al., 2006; Tjaden and Wellschmied, 2014; Sorkin, 2018). In Glassdoor wage data, 29 percent of job transitions are characterized by a real wage cut. Because workers are willing to accept lower wages at their new firms, the literature typically rational-

[16]Using a survey of German workers, Jäger et al. (2021) find a similar pattern: individuals report higher levels of job satisfaction with work when moving to higher-paying firms.

izes these observed flows by arguing that there must be a compensating differential through improved non-wage amenity value that these workers are receiving (Bonhomme and Jolivet, 2009; Sorkin, 2018; Taber and Vejlin, 2020). However, if this were the case, we would expect wage declines to be more frequent when moving to higher-satisfaction firms, not lower-satisfaction ones. In documenting the opposite, I find scant evidence in the data supporting the assumption that pay cuts are offset by improvements in job amenities. This evidence points to either higher wages driving the improvement in satisfaction — a possibility I rule out and bound in Section 1.5.4 — or better quality amenities fueling the increase.

One limitation of studying how the likelihood of a job satisfaction decline relates to firms' wage premia within this sample is that why the workers transition jobs is unobservable. If workers in this panel only transition to lower-paying firms when they are fired or laid off, then the negative slopes observed in Figure 1.3 may not apply more broadly. In other words, voluntary job transitions may experience the inverse pattern if they are under-represented in the data. To address this concern, I confirm that this negative relation, i.e. a greater likelihood of a job satisfaction decline among lower-paying employers, is observed robustly across different types of job transitions that likely span both voluntary and involuntary moves (Table A.14). These transitions include workers who exited short or long spells, workers who kept the same job title or transitioned to a managerial role, and workers who changed their employment status to full-time or part-time when switching firms.

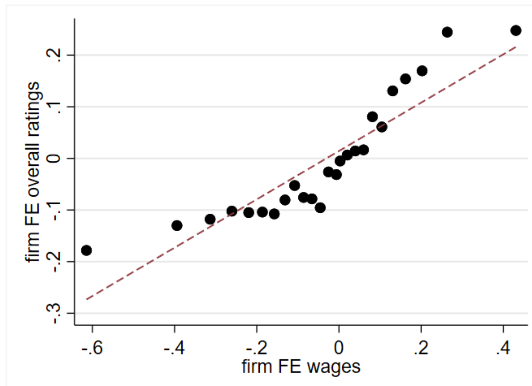
Suggestive evidence that the non-wage amenities workers enjoy improve when moving to higher-paying firms can be seen in studying how workers' satisfaction changes for different sub-categories. Replacing the the left-hand side of equation 1.4.2 with the first difference in ratings for career opportunities, culture and values, senior management, compensation and benefits, and work-life balance reveals that seemingly every aspect of the job improves (Table A.15). Perhaps not surprisingly, satisfaction with compensation and benefits rises the most, 0.15 standard deviations per one-standard-deviation increase in firm wage premia. But satisfaction even with non-pecuniary dimensions, such as career opportunities, culture and values, senior management, and work-life balance, improve as well, each rising on average 0.04–0.07 standard deviations per one-standard-deviation increase in wage premia.

1.4.4 Are High-Paying Firms High-Satisfaction Firms?

Given that workers experience fewer wage declines moving to higher-satisfaction firms and fewer satisfaction declines moving to higher-paying firms, naturally it follow to what extent then is attributable to the firm? Since I observe the wage premium $\hat{\lambda}_k^w$ and satisfaction premium $\hat{\lambda}_k^R$ for the same employer k, I can directly relate the two, $\hat{\lambda}_k^R = \rho\hat{\lambda}_k^w + \upsilon_k$. The coefficient ρ then captures the extent to which greater wages translate into more satisfaction across firms. This relation is summarized in Figure 1.4, where a strikingly positive correlation is observed. Formally estimating this model reveals a coefficient of $\rho = 0.468$ (standard error

= 0.017) for the Full set of firms.[17] Given standard deviations of 0.22 and 1.03 in the wage and rating premia, respectively, a one standard deviation increase in firm-level wages is associated with 0.10 standard deviations greater job satisfaction. For the Connected set of firms, the estimate is even larger at 0.19 standard deviations.[18]

Figure 1.4: Relation between Firms' Pay and Job Satisfaction Premia



Notes: This figure depicts the firm fixed effects for wages $\hat{\lambda}_k^w$ (x-axis) against the firm fixed effect for job satisfaction $\hat{\lambda}_k^R$ (y-axis). The set of firms included reflects the Full sample. Observations are partitioned into twenty-five bins according to the measure on the x-axis.

While greater wages corresponded to more job satisfaction across the 70,000 firms in the Full set, there may be heterogeneity between different types of firms. One particularly salient firm characteristic I observe is the industry in which the firm operates, of which there are seventeen NAICS categories. It is well-established that there are differences in pay between industries (Wachtel and Betsey, 1972; Krueger and Summers, 1988) and one posited theory for rationalizing these differences is

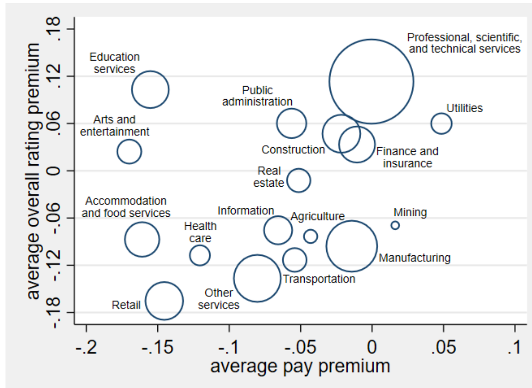
[17]Table A.16 presents the results with and without industry fixed effects for the Full and Connected samples.

[18]For the relation between wages and amenities across firms within each industry, see the discussion in Appendix A.4, which highlights broadly positive intra-industry correlations except within the Educational Services industry where there is a clear negative slope driven entirely by schools and colleges.

that workers in higher-paying industries are compensated for worse working conditions (Holzer et al., 1991; Sorkin, 2018). If it were the case that inter-industry wage differences equalized inter-industry amenity differences, then we would anticipate an inverse relation across industries between the wage and satisfaction premia firms offer. Figure 1.5 plots for each industry ι, the average wage premium in the industry $\hat{\lambda}_\iota^w = \frac{1}{N_{k \in \iota}} \hat{\lambda}_k^w$ against the average satisfaction premium $\hat{\lambda}_\iota^R = \frac{1}{N_{k \in \iota}} \hat{\lambda}_k^R$. If such compensating differentials are evident, then low-paying industries would offer greater levels of job satisfaction, and vice-versa; however, the opposite is apparent. Relatively high-paying industries such as Professional, Scientific, and Technical Services and Finance and Insurance also provide more satisfaction than low-paying industries such as Accommodation and Food Services and Retail. Across industries, the weighted correlation between the wage and overall ratings premia is 0.47 (p-value=0.055). One industry where there may be strong compensating differentials is Educational Services, which offers relatively low wages but comparatively high levels of job satisfaction. Excluding Educational Services, the weighted correlation is 0.66 (p-value=0.005). Therefore, consistent with Krueger and Summers (1988), inter-industry wage differentials do not reflect compensation for disagreeable work characteristics.

Within the AKM framework, the effect from the firm by assumption is constant over time. However, firm fundamentals or outlooks may shift over time and spillover into changes in worker compensation. In the spirit of Lachowska et al. (2020), I

Figure 1.5: Heterogeneity in Wage-Rating Premia Across Industries



Notes: This figure plots the average wage premium against the average firm rating premium for each industry. Industries reflect two-digit NAICS and are weighted by firm count in the Full set.

re-estimate a time-varying version of equation 1.4.1 where the firm fixed effects are allowed to drift over time, λ_{kt}. For the same firm, I relate the growth in relative wage premium offered over time to the growth in relative job satisfaction provided over time by estimating $\hat{\lambda}^R_{kt} = \rho\hat{\lambda}^w_{kt} + \xi_k + \xi_t + \upsilon_{kt}$. Controlling for firm and year accounts for inherent differences across firms (e.g., industry, size, and location) and trends over time that may reflect sample composition or the business cycle. The results including and excluding ξ_k (presented in Table A.17) reveal that ρ remains significantly positive even when looking within firms over time, further suggesting that wage growth corresponds to job satisfaction growth, consistent with widening wage inequality exacerbating satisfaction inequality (Hamermesh, 2001).

One potential explanation for observing improved satisfaction at higher-paying firms that would be orthogonal to the quality of non-wage attributes is a warm glow effect, whereby the stature of being employed with a higher-paying firm elevates one's satisfaction with their job. This could reflect, for instance, a heightened sense of accomplishment from achieving employment with a high-paying firm, especially if such a feat is considered difficult or rare. One way to proxy for this warm glow effect would be to capture disparities in interview practices, such as the level of difficulty or success rate, across firms. On glassdoor, workers can separately detail their experiences interviewing with firms, including how challenging they perceived the interview to be and whether they received an offer.[19] Estimating an AKM model for each of these two interview metrics and relating the resultant firm fixed effects to those for log wages and job satisfaction (Table A.18) reveals that a warm glow is not driving this positive relation. While higher-paying firms carry out more difficult interviews and are more selective in extending offers (Column 1), accounting for differences in the interview process across firms attenuates the slope between firms' wage and ratings premia by about 10 percent — suggesting warm glow effects play a limited role.

1.5 Introducing Job Amenities

How does one capture the 'many margins' of job amenities? Doing so would involve not only gauging the quality and/or availability of each attribute, but determining an exhaustive set to measure. Labor market surveys have attempted to gauge the

[19]Each observation in the Glassdoor interviews data is an employee-employer match that includes an assessment for the interview's difficulty level on a one-to-five ordinal scale (corresponding in increasing order to very easy, easy, average, difficult, and very difficult, respectively) and an indicator for whether the worker received a job offer. There are roughly 180,000 observations covering 14,000 employers for the panel of workers with multiple interviews. For further discussion of the data, see Sockin and Zhao (2020).

non-wage characteristics of work to varying degrees.[20] However, unlike Glassdoor, the nature of each of these surveys precludes any firm-level analysis.

1.5.1 Semi-Supervised Topic Modeling

While workers provide ratings along five broad sub-dimensions, such as career opportunities and senior leadership (see Figure 1.2) when submitting an employer review, these sub-dimensions reflect how workers perceive an amalgamation of different work aspects. To isolate specific job amenities, I make use of the free-text responses that workers submit for the 'Pros' and 'Cons' sections of their reviews. This has the advantage of, unlike other surveys where it is explicit what attributes are being captured, allowing workers to tell us (the researchers) what amenities matter to them. Advantageously, because workers partition their sentiment into the positive ('pros') and negative ('cons') features, I can measure an amenity's quality based on whether it is discussed in the former or the latter. While workers do not mention every amenity, I interpret the worker choosing to mention an amenity as signaling that the quality is especially above or below average or expectation.

There are fifty amenities in total, spanning six main categories. Some are obvious, others motivated by the literature, and the rest identified after implementing

[20] In the ASEC, respondents are asked whether they receive health insurance or a pension from their employers — measures analyzed by Simon and Kaestner (2004) and Clemens et al. (2018) — as well as usual hours of work per week on the job. In the NLSY97, respondents are asked about work schedules, available fringe benefits, and in the most recent wave of the survey, required job tasks. More recently, the American Working Conditions Survey (AWCS), administered by RAND and studied in Maestas et al. (2018), captures differences along a range of workplace conditions (see Appendix A.3).

unsupervised topic modeling to learn what latent attributes naturally arise. The first category is traditional pay or base earnings (pay and pay growth). The second is other forms of pecuniary compensation, including variable earnings[21] (bonuses and commissions) and fringe benefits[22] (paid time off, health insurance, retirement contributions, employee discounts and free food). The third and most extensive is working conditions, which includes work-life balance, hours, work schedule, short breaks, office space, commuting, teleworking, location, autonomy/responsibility, respect/abuse, communication, support, difficulty, requirements, stress, pace, safety, recognition, morale, fun, culture, diversity/inclusion, leadership, office politics, change, and job security.[23] Fourth is human capital, which includes career concerns, promotions, experience, skill development, on-the-job training, mentoring, recruiting, contracting, and industry.[24] The fifth is interpersonal relationships, comprised

[21] Wiswall and Zafar (2017) estimate workers' willingness to pay for bonus compensation; Sockin and Sockin (2019b) relate jobseeker activity to the competitiveness of a role, where competitiveness is proxied for by the share of variable pay attributable to commissions.

[22] Maestas et al. (2018) examine paid time off and Simon and Kaestner (2004) evaluate health insurance and retirement plans. Employee discounts and free food were included in surveys of workers by Glassdoor (2015), and Fractl (2020) shows that 15–30 percent of workers surveyed would consider accepting these fringe benefits over higher pay. I attempted to include an amenity for tuition assistance, which received a similar valuation in these surveys, but could not recover an interpretable topic.

[23] Maestas et al. (2018) consider work schedule, teleworking, stress, pace, and autonomy/responsibility; Le Barbanchon et al. (2020) commuting; Hersch (2011) sexual harassment (respect/abuse); Bradler et al. (2016) recognition; Wiswall and Zafar (2017) hours; Wasmer and Zenou (2002) location; Autor and Handel (2013) job tasks (requirements), Gadgil and Sockin (2020) culture and leadership; Gronberg and Reed (1994) fun; Quinn (1974) the challenge of the job (difficulty), help (support), and physical surroundings (office space); and Park et al. (2021) workplace safety. Pollak (2019) finds workers value workplace diversity/inclusion; Carpenter et al. (2010) find office politics can hamper labor productivity; Breza et al. (2017) relate morale to the opacity of coworker productivity; and Hamermesh (1990) examines the marginal return to short breaks.

[24] Acemoglu and Pischke (1999) examine on-the-job training; Tambe et al. (2020) skill development among information technology workers; Johnston and Lee (2013) promotions; Gibbons and Murphy (1992) career concerns; Starr et al. (2021) non-compete and Sockin et al. (2021)

of managers, coworkers, teams, and customers.[25] And the sixth is a residual category comprised of two un-anchored topics meant to freely capture the rest of the review text.

To extract amenities from review text, I borrow a topic-modeling machine learning algorithm from the computer science literature. I implement the Anchored Correlation Explanation (CorEx) model of Gallagher et al. (2017) — a semi-supervised approach that allows the researcher to specify topic-specific "anchor words" that guide topics to convergence.[26] The model is semi-supervised in that the researcher identifies part of the topic (the 'anchor words') while the machine fills in the rest of the topic according to the objective. A semi-supervised approach is used to ensure the topics can be interpreted as specific amenities.[27] The Anchored CorEx model is particularly well-suited for this task since compared with other topic modeling methods, it has been found to more readily produce coherent topics that are less overtly discussed and may not naturally emerge (Gallagher et al., 2017).

I first calibrate the CorEx model using the full review text (stacked pros and cons) for a sample of three-million reviews. Applying the model to a segment of

non-disclosure agreements (contracts); Dustmann and Meghir (2005) experience; Quinn (1974) job security; Athey et al. (2000) model the interaction of mentoring and diversity; and Faberman and Menzio (2018) relate recruiting intensity to starting wages.

[25] Maestas et al. (2018) consider teamwork; Stinebrickner et al. (2019) the beauty wage premium in jobs that rely upon interpersonal interaction (customers); and Quinn (1974) coworkers and supervisors (managers).

[26] For details on how the CorEx model successfully identifies latent topics, see Steeg and Galstyan (2014) who first introduced the algorithm. When implementing the model in Python, I search for fifty topics with seed set at two and anchor strength set at nine.

[27] An alternative topic-modeling algorithm that is more common — the Latent Dirichlet Allocation (LDA) model — was considered and even implemented using a semi-supervised, anchored approach; however, the topics that were produced with LDA were more amorphous and less interpretable than those produced with CorEx, even under the same assignment of anchor words.

text outputs a vector $\{p_a \in [0,1]\}_{a=1\ldots50}$ of probabilities that amenity a is discussed. The twenty highest-incidence (or most-weighted in the case of anchors) words for each attribute are presented in Tables A.6–A.10. For each of the 8.5 million reviews r, I separately score the pros and cons sections to obtain the vectors $\{p^{pro}_{r,a}\}$ and $\{p^{con}_{r,a}\}$. Taking the weighted difference between the two — where the weight (ω_r) is the share of review text in the pros section — I gauge the quality of amenity a from review r according to $q^a_r = \omega_r p^{pro}_{r,a} - (1-\omega_r)p^{con}_{r,a} \in [-1,1]$. If the amenity is not mentioned in the review, its quality will be neutral $q^a_r = 0$. The frequency with which each amenity is discussed within the panel of reviews is presented in Figure A.6. Amenities that are frequently discussed pertain to characteristics that are important predictors for overall job satisfaction but would be difficult to discern about the firm ex ante such as respect/abuse (26 percent), coworkers (18 percent) and leadership (17 percent). Importantly, it is not uncommon for workers to highlight satisfaction with pay (15 percent) or pay growth (3 percent), implying that one could capture the pass-through of pay satisfaction to overall satisfaction through these text-based amenities, as in Section 1.5.4.

1.5.2 Internal and External Validity of Amenities

Crucially, using the output from this topic modeling approach rests on the interpretability of the topics, i.e., the label assigned to each amenity is accurate. For reassurance, I first show that within Glassdoor reviews, the amenities are consistent with other measures of sentiment that respondents provide. Recall each worker evaluates their employer on a one-to-five stars scale along five sub-categories: career opportunities, compensation and benefits, culture and values, senior leadership, and work-life balance. Amenities that relate more to a given sub-category should play an outsize role in predicting an employee's satisfaction along that dimension. Within the panel of workers' reviews, I can relate the change in satisfaction within each of these sub-categories to the change in quality of each job amenity according to

$$Y_{ikt} = \sum_{a=1}^{50} \beta_a q_{ikt}^a + \lambda_i + \lambda_k + \lambda_t + \varepsilon_{ikt}, \tag{1.5.1}$$

where Y_{ikt} is the star rating and λ_i, λ_k, and λ_t represent worker, firm and year fixed effects, respectively. The coefficients β_a capture the degree to which amenity a predicts satisfaction *conditional* on the quality of the rest of the amenity bundle. While job satisfaction is positively correlated with every amenity individually (Table 1.3: Column 7), the estimates from equation 1.5.1 isolate the relative contribution of each amenity. The coefficients on overall rating and the five sub-categories are presented in the first six columns of Table 1.3. More-positive values of β_a signify more import, while more-negative signify less.

The first column reveals which amenities workers value the most when determining overall satisfaction. A few takeaways are worth highlighting. First, the most desirable amenities are those which are hard to observe from outside the firm. In other words, job satisfaction appears driven by attributes that are learned through experience, such as employee respect/abuse, leadership and management, work-life balance, culture, and morale. Second, aspects related to compensation and benefits are considerably less influential in determining employer quality. While improved pay and pay growth, health insurance, retirement contributions, and bonuses have a significant effect on workers' overall satisfaction, they are second-order compared with the harder-to-observe intangibles such as culture and leadership. Third, workers appreciably value when employers make strides in issues related to social issues, as evidence by the strongly positive coefficient on diversity/inclusion — perhaps reflecting why employers are increasingly making investments on environmental, social, and governance (ESG) and diversity, equity, and inclusion (DEI) issues.[28] Last, workers seem to prefer work arrangements that are increasingly difficult or involve heightened responsibility through more requirements — possibly reflecting the importance workers place on developing their human capital.

Comparing the coefficients for overall rating with those for each of the subcategories provides reassuring evidence that these amenities in fact reflect their labels. We see that the promotions amenity is more important for career opportunities than overall rating (0.23 vs. 0.04), as is the amenity for career concerns (0.25 vs. 0.14) and pay growth (0.17 vs. 0.09) — though not the amenity for pay, lending

[28]The three largest institutional investors in 2017 successfully campaigned to increase female representation on corporate boards (Gormley et al., 2021) and 53 percent of S&P 500 companies now employ a chief diversity officer (Green, 2021). Moreover, in a 2019 survey of institutional investors and asset managers querying why they incorporate ESG in investment decisions, 47 percent cited brand image and reputation while 27 percent cited attracting new talent (Boffo and Patalano, 2020).

Table 1.3: Relative Importance and Validation of Amenities

Attribute	Bundled: Overall rating	Bundled: Career opp.	Bundled: Comp. and benefits	Bundled: Culture and values	Bundled: Senior mgmt.	Bundled: Work life balance	Separate: Overall rating
Respect/abuse	0.61†	0.43†	0.25†	0.68†	0.51†	0.47†	1.53†
Residual I	0.44†	0.44†	0.25†	0.42†	0.50†	0.26†	1.32†
Leadership	0.42†	0.34†	0.19†	0.41†	0.54†	0.29†	0.96†
Residual II	0.38†	0.29†	0.24†	0.31†	0.31†	0.28†	1.30†
Work-life balance	0.38†	0.22†	0.16†	0.36†	0.36†	1.14†	0.83†
Culture	0.33†	0.25†	0.13†	0.48†	0.32†	0.24†	0.92†
Managers	0.29†	0.22†	0.15†	0.27†	0.33†	0.20†	0.97†
Morale	0.27†	0.19†	0.13†	0.34†	0.28†	0.20†	0.84†
Diversity/inclusion	0.21†	0.20†	0.11†	0.32†	0.21†	0.11†	0.87†
Support	0.21†	0.18†	0.12†	0.21†	0.22†	0.17†	0.87†
Mentoring	0.20†	0.20†	0.14†	0.16†	0.19†	0.10†	0.74†
Job security	0.19†	0.21†	0.04†	0.19†	0.23†	0.03	0.79†
Fun	0.17†	0.14†	0.09†	0.18†	0.13†	0.10†	0.67†
Office politics	0.17†	0.12†	-0.02	0.21†	0.26†	0.04	0.62†
Teams	0.16†	0.14†	0.07†	0.16†	0.17†	0.12†	0.87†
On-the-job training	0.16†	0.11†	0.07†	0.14†	0.13†	0.09†	0.73†
Coworkers	0.15†	0.11†	0.07†	0.17†	0.13†	0.12†	0.81†
Career concerns	0.14†	0.25†	0.10†	0.11†	0.15†	0.06†	0.75†
Pay	0.14†	0.12†	0.38†	0.07†	0.09†	0.07†	0.69†
Commissions	0.12†	0.11†	0.17†	0.08†	0.10†	0.06†	0.76†
Industry	0.12†	0.12†	0.07†	0.08†	0.12†	0.03†	0.73†
Safety	0.11†	0.06†	0.05†	0.15†	0.09†	0.11†	0.78†
Health insurance	0.10†	0.08†	0.25†	0.07†	0.08†	0.00	0.62†
Autonomy/responsibility	0.10†	0.08†	0.05†	0.07†	0.10†	0.07†	0.73†
Pay growth	0.09†	0.17†	0.33†	0.04†	0.09†	-0.06†	0.62†
Recognition	0.09†	0.11†	0.09†	0.08†	0.09†	0.04†	0.75†
Bonuses	0.08†	0.09†	0.19†	0.06†	0.06†	0.02	0.73†
Retirement contributions	0.08†	0.05†	0.21†	0.05†	0.03	-0.03	0.62†
Customers	0.07†	0.06†	0.03†	0.08†	0.06†	0.06†	0.71†
Work schedule	0.07†	0.01	-0.02	0.04†	0.05†	0.32†	0.57†
Stress	0.07†	-0.04†	-0.05†	0.07†	0.05†	0.32†	0.68†
Recruiting	0.06†	0.09†	0.04†	0.03†	0.03†	0.00	0.77†
Skill development	0.06†	0.09†	0.00	0.01	0.02	-0.04	0.72†
Pace	0.05†	0.03	0.03	0.01	0.03	-0.01	0.44†
Contracting	0.05†	0.07†	0.11†	0.02	0.01	0.01	0.72†
Promotions	0.04†	0.23†	0.05†	0.02	0.09†	-0.04†	0.72†
Employee discounts	0.03	0.03	0.14†	0.05†	0.02	0.01	0.50†
Teleworking	0.03	-0.01	0.02	0.05†	0.04	0.17†	0.61†
Paid time off	0.03†	0.01	0.12†	0.02	-0.01	0.13†	0.72†
Experience	0.03†	0.04†	0.00	0.01	0.02	0.00	0.74†
Communication	0.02	-0.03†	-0.06†	0.03†	0.04†	0.00	0.75†
Hours	0.00	0.02	0.09†	-0.02	-0.04†	0.02	0.55†
Short breaks	-0.01	-0.04†	-0.01	-0.02	-0.04†	0.05†	0.70†
Office space	-0.02†	-0.01	0.00	0.01	-0.02	0.00	0.69†
Free food	-0.02	-0.02	0.04†	0.01	-0.01	0.01	0.61†
Commuting	-0.06†	-0.08†	-0.06†	-0.05†	-0.07†	-0.04†	0.52†
Change	-0.07†	-0.06†	-0.07†	-0.06†	-0.02	-0.07†	0.55†
Location	-0.09†	-0.07†	-0.04†	-0.10†	-0.09†	-0.06†	0.38†
Requirements	-0.11†	-0.14†	-0.08†	-0.12†	-0.14†	0.02	0.53†
Difficulty	-0.19†	-0.16†	-0.15†	-0.21†	-0.21†	-0.14†	0.28†

Notes: This table displays the coefficients from regressing the amenities on the stars-based rating listed in the header of each column with worker, firm, and year fixed effects. Amenities listed in ascending order according to the coefficient for overall rating bundled. † indicates significance at the one percent level.

credence to the pay growth amenity capturing a separate and unique characteristic. For compensation and benefits, the coefficient on the pay amenity is more salient (0.38 vs. 0.14), as is that of pay growth (0.33 vs. 0.09), health insurance (0.25 vs. 0.10), retirement contributions (0.21 vs. 0.08), bonuses (0.19 vs. 0.08), employee discounts (0.14 vs. 0.03), paid time off (0.12 vs. 0.03), hours (0.09 vs. 0.00), and free food (0.04 vs. –0.02). For culture and values, encouragingly culture plays a more important role (0.48 vs. 0.33), as does diversity/inclusion (0.32 vs. 0.21). For senior leadership, the estimates are highly similar to those obtained from predicting overall rating — signifying the importance of management for overall satisfaction — though the coefficient on office politics is greater (0.26 vs. 0.17). And for work-life balance, we observe that the work-life balance amenity is by far the largest driver (1.14 vs. 0.38), but other amenities play an outsize role as well, including stress (0.32 vs. 0.07), work schedule (0.32 vs. 0.07), teleworking (0.17 vs. 0.03), paid time off (0.13 vs. 0.03), short breaks (0.05 vs. –0.01), and requirements (0.02 vs. –0.11). In all, the amenities appear internally consistent within Glassdoor data.

Importantly, these job amenities capture meaningful variation in other labor market data. Appendix A.2 shows how seven of the amenities — diversity/inclusion, health insurance, hours, job security, paid time off, retirement contributions, and work-life balance — trace relevant patterns observed across industries and occupations in ASEC. Appendix A.3 highlights how ten amenities mostly pertaining to working conditions — autonomy/responsibility, communication, on-the-job training, pay, recognition, safety, short breaks, support, work schedule, and work-life balance — align with the AWCS across industries and occupations. Given that both of these surveys are representative, I take this as evidence that inferences using these Glassdoor amenities are valid for the U.S. labor market more broadly.

1.5.3 Relation between Wages and Amenities

I next turn to how wages relate to individual amenities across firms. As with overall ratings, I estimate firm-specific premia for each amenity a by re-estimating equation 1.4.1 but substituting q^a_{ikt}, the quality of attribute a reported by worker i at firm k in year t, on the left-hand side. Then, relating the firm premia for each amenity $\hat{\lambda}^a_k$ to the corresponding firm's wage premium $\hat{\lambda}^w_k$ — formally estimating $\hat{\lambda}^a_k = \rho^a \hat{\lambda}^w_k + \upsilon_k$ — captures the degree to which the quality of amenity a varies with the firm's offered wage premium. I convert the amenity-related fixed effects to standardized normal for ease of exposition, and record the estimated coefficients for the Full set of firms in Table 1.4.[29]

I emphasize three key takeaways. First, the pay and pay growth amenities elicit particularly positive relations with the wage premia, highlighting that workers not only recognize the receipt of greater wages, but that increased satisfaction with pay contributes to the improved perception of overall satisfaction. Second,

[29] The results change little when amenity quality is calculated without weighting by the share of the review text in each section (Table A.19). Broadly, the coefficients fall slightly, with the negative coefficient for difficulty becoming statistically significant — though it is worth noting that, conditional on the rest of the amenity bundle, the loading for difficulty on overall satisfaction is appreciably negative.

nearly all amenities are improved at higher-paying firms. In particular, the full set of fringe benefits (free food, paid time off, health insurance, retirement contributions, and employee discounts) along with amenities related to flexible labor supply (short breaks and teleworking), working conditions (respect/abuse, leadership, safety, autonomy/responsibility, support, office space, recognition), interpersonal relationships (managers, coworkers, customers, and teams), and human capital development (career concerns, promotions, and experience) exhibit improved quality at higher-paying firms.[30] Third, there are few dis-amenities that come with working for higher-paying firms. The standout trade-off workers face is worsened job security, as a one-standard-deviation greater wage premium is associated with a 0.02-standard-deviations reduction in job security.

1.5.4 Decoupling Pay Satisfaction from Job Satisfaction

One concern with using job satisfaction to argue that non-wage amenities are better at higher-paying firms is that the increase in job satisfaction may simply reflect improved pay satisfaction. If workers are more content with their pay, then naturally they will be more content with their jobs overall. However, with two amenities related to pay satisfaction (pay and pay growth), I can decompose the boon to job satisfaction enjoyed at higher-paying firms into the fraction that is attributable to

[30] One drawback is that amenity quality is assigned based on workers' satisfaction with each amenity, but fringe benefits are inherently pecuniary in nature, e.g., the number of paid leave days or an employer's contribution to a retirement account. Improved satisfaction with fringe benefits may not necessarily translate into increased spending by the firm though in practice, the two are likely to be highly correlated.

Table 1.4: Relation between Wages and Attributes Across Firms

Standardized amenity	Overall rating weight	Slope with standardized wage FE	Standardized amenity	Overall rating weight	Slope with standardized wage FE
Pay	0.14	0.070***	Commuting	-0.06	0.012***
Residual I	0.44	0.050***	Retirement contributions	0.08	0.012***
Residual II	0.38	0.037***	Diversity/inclusion	0.21	0.011**
Pay growth	0.09	0.035***	Promotions	0.04	0.011***
Respect/abuse	0.61	0.034***	Location	-0.09	0.011***
Short breaks	-0.01	0.033***	Recognition	0.09	0.011***
Managers	0.29	0.033***	Requirements	-0.11	0.010**
Culture	0.33	0.029***	Experience	0.03	0.009**
Industry	0.12	0.029***	Work-life balance	0.38	0.006*
Teleworking	0.03	0.028***	Work schedule	0.07	0.005
Free food	-0.02	0.026***	Mentoring	0.20	0.005
Leadership	0.42	0.025***	Contracting	0.05	0.004
Coworkers	0.15	0.025***	Fun	0.17	0.004
Teams	0.16	0.024***	Recruiting	0.06	0.004
Commissions	0.12	0.020***	Hours	0.00	0.002
Safety	0.11	0.020***	Pace	0.05	0.002
Health insurance	0.10	0.020***	Bonuses	0.08	0.002
Office politics	0.17	0.018***	Communication	0.02	0.002
Support	0.21	0.018***	On-the-job training	0.16	0.001
Career concerns	0.14	0.017***	Morale	0.27	0.001
Autonomy/responsibility	0.10	0.017***	Change	-0.07	-0.003
Office space	-0.02	0.017***	Stress	0.07	-0.005
Paid time off	0.03	0.016***	Skill development	0.06	-0.005
Employee discounts	0.03	0.014***	Difficulty	-0.19	-0.006
Customers	0.07	0.013***	Job security	0.19	-0.019***

Notes: This table reflects coefficients from a linear regression of the firm fixed effects for each amenity on the firm fixed effects for wages. Standard errors are bootstrapped. Overall rating weights reflect the first column of Table 1.3. Significance levels: * 10%, ** 5%, *** 1%.

pay satisfaction and the fraction that is not. This decomposition is feasible since each amenity contributes to overall job satisfaction with some weight ψ^a (Table 1.4: overall rating weight) and each amenity relates to firms' wages with some slope ρ^a (Table 1.4: slope with wage FE). For each of the six broad amenity categories (pay, compensation excluding pay, working conditions, human capital, relationships, and residual — the mappings for which are detailed in Tables A.6–A.10 — the contribution from group g to the cumulative effect on overall satisfaction can be approximated according to

$$contribution_g = \frac{\sum_{a \in g} \psi^a \rho^a}{\sum_a \psi^a \rho^a}. \tag{1.5.2}$$

If the positive correlation observed between wages and job satisfaction across firms is driven primarily by workers reporting greater satisfaction with pay, then the contribution from the pay amenity group $g = \{pay, pay\ growth\}$ will near 100 percent; however, if the uptick in job satisfaction observed at higher-paying firms is attributable to non-wage aspects improving, then the contribution from the pay amenity group will be closer to 0 percent. The contributions using the Full and Connected sets are presented in Table 1.5.

Using the Full sample, it is clear that pay satisfaction is not driving the increased job satisfaction. The pay-based amenities are responsible for only 8 percent of the increase in overall satisfaction, meaning that 92 percent is attributable to aspects not related to satisfaction with pay. Both improved working conditions (37 percent) and better interpersonal relationships (13 percent) account for larger shares than pay satisfaction. The two residual amenities — which capture general sentiment towards the firm — also account for 35 percent. That this residual grouping has an outsize contribution invites the possibility of under-counting the true contribution from pay satisfaction if these residuals (partially) reflect pay. However, examining the highest incidence words for each of the two residual amenities (Table A.10) reveals no discernible semblance to pay, mitigating this concern. Evidently, non-wage amenities explain a predominant share (89–92 percent) of the improved satisfaction workers report at higher-paying firms. Further, the non-wage attributes fueling the increase in job satisfaction reflect hard-to-observe non-pecuniary qualities, such as working conditions and interpersonal relationships, rather than pecuniary ones — consistent with the results, presented in Appendix A.5, from analyzing workers' ratings of fringe benefits directly.

Table 1.5: Contribution by Amenity Type to Increased Satisfaction at Higher-Paying Firms

Amenity category	Full	Connected
Pay	8.2%	11.1%
Fringe benefits	2.0%	1.9%
Working conditions	36.7%	28.8%
Human capital	4.9%	4.9%
Relationships	13.1%	9.7%
Residual	35.1%	43.6%

Notes: This table reports the percent of the total slope between job satisfaction and wage premia across firms attributable to each of the six amenity categories for the Full and Connected sets.

Further evidence of the limited role pay satisfaction has in driving overall satisfaction can be observed in relating the change in workers' five subcategory ratings to the change in their overall ratings. As workers become more satisfied with these sub-dimensions, overall satisfaction should rise; and the rates at which these sub-ratings pass through to overall ratings summarizes the relative importance of each dimension. A one-star increase across all five categories raises overall rating by about one star (Table A.20), yet the rate of pass through for compensation and benefits is only .107 — indicating that only about 10 percent of overall satisfaction relates to compensation. This estimate accords with the text-based estimates of

Table 1.5.

1.6 Workers' Willingness to Pay for Job Satisfaction

The extent to which disparities in non-wage amenities contribute to welfare depends on how much value workers place on them. If workers are indifferent to the (dis-)amenities of work, then the dispersion in amenity offerings between firms matters little for welfare — though empirical evidence strongly rejects this notion (Wiswall and Zafar, 2017; Maestas et al., 2018). Estimating a dollar value workers place on improved job satisfaction (or amenities) would allow the construction of a total measure of compensation that includes amenity value.

Researchers have utilized different methodologies for calculating workers' willingness to pay for job attributes. The most common historically has been a hedonic approach, in which characteristics are considered implicitly priced into the accepted wage. A (usually unfavorable) non-wage attribute is added as an explanatory variable for predicting wages, and the coefficient captures the additional wage needed to undertake the burden of the dis-amenity (Thaler and Rosen, 1976; Herzog and Schlottmann, 1990; Hwang et al., 1992; Lavetti and Schmutte, 2018).A more recent contingent valuation approach presents workers with a menu of hypothetical alternatives and from their choices, willingness to pay can be inferred (Mas and

Pallais, 2017; Wiswall and Zafar, 2017; Maestas et al., 2018). Additionally, there is a revealed preference approach that estimates willingness to pay based on workers' employment decisions, e.g., the length of job spells (Gronberg and Reed, 1994), the length of non-employment spells after childbirth (Felfe, 2012), or the timing of labor supply provision in a flexible work arrangement (Chen et al., 2019).

To estimate how much workers would be willing to pay to work with improved job satisfaction, in the spirit of Gronberg and Reed (1994), I examine the length of workers' job spells. If worker utility is increasing in both wages and non-wage attributes, i.e., the two are normal goods, then the decision to exit the match will factor in both wage and non-wage aspects of alternative offers.[31] Workers would then stay at their firms longer if they receive greater wages, improved amenities, or both. To this end, I estimate an ordered probit model using workers' completed job spells predicing a worker's tenure with the firm as a function of both their wage and job satisfaction.[32] Because firm tenure is recorded in discrete intervals (less than 1, 1–2, 3–4, 5–7, 8–10, and more than 10 years), an ordered probit approach preserves the ordinal scale while accounting for the non-linearity in time elapsed across intervals.[33]

Let $tenure_{ijkt}$ be the number of years worker i with job title j spent employed at

[31] Identifying whether a worker exits because of a forced separation or voluntary quit is infeasible. Only whether the worker is a current or former employee when providing her review is known.

[32] For now, I exclude workers who are still employed with the firm since their job spells are ongoing. Gronberg and Reed (1994) incorporate both complete and incomplete job spells using a maximum likelihood estimation (MLE) procedure in which a survival function is applied to ongoing spells. Applying a similar MLE procedure in this context is ongoing.

[33] Excluded are the roughly 30 percent of workers who do not report their tenure at the firm.

firm k as of year t. Since workers do not provide wages when filling out an employer review, to impute a wage for each worker, I make use of each worker's job title.[34] I calculate the median log wage among the set of workers with job title j at firm k in year t from the wages dataset, $\bar{w}_{jkt}$.[35] Using each worker i's individual job satisfaction rating with firm k in year t, R_{ikt}, I estimate

$$tenure_{ijkt} = \beta_w \bar{w}_{jkt} + \beta_R R_{ikt} + \lambda_{\iota(k)} + \lambda_t + \varepsilon_{ijkt}, \tag{1.6.1}$$

where $\iota(k)$ corresponds to firm k's industry.[36] Here, β_R captures the effect that a one-star increase in a worker's overall job satisfaction has on the probability she spends more years employed with the firm. If $\beta_R > 0$, then this would confirm that workers value non-wage aspects of work when making employment and mobility decisions. Otherwise, this would suggest that conditional on their wage, workers do not factor the quality of job amenities into their separation decisions. Because

[34] Job titles have been found to carry meaningful weight in the determination of wages. Marinescu and Wolthoff (2020) find that job titles can explain upwards of 90 percent of the variance in (the midpoints of) posted wages on an online job board, while Sockin and Sockin (2019a) find that 90 percent of base earnings are explained by the average among peers (same job title and firm in the same year).

[35] One alternative would be to use reported earnings from a worker's wage report in lieu of an imputed wage. However, this requires a worker to submit both an employer review and wage report to be included, more than halving the sample. In this case, a similar, albeit even steeper slope in MWP at the upper-tail of the wage distribution is observed (Table A.21). Since an individual's wage may be reported with measurement error, using the imputed median among peers largely sidesteps this concern and is thus the preferred specification.

[36] The industry of employment is included to account for heterogeneity in workers' employment opportunities, as is allowed for in the model of Gronberg and Reed (1994). One could further account for differences in job opportunities by controlling for one's occupation; however, occupation, which is obtained from Glassdoor's mapping of job titles to occupations, is unavailable for two-thirds of full-time workers' completed job spells. Nevertheless, adding occupational controls reveals the same overall pattern (Table A.22) though with a more pronounced MWP for the highest-earners.

workers may value job amenities differently depending upon their wage (Maestas et al., 2018), I implement equation 1.6.1 within wage quintiles. The results are presented in Table 1.6.

Consistent with Akerlof et al. (1988), the first row confirms that greater job satisfaction translates into longer employment spells (since β_R is robustly positive). This is true for workers of all wage levels, though the effect appears to increase monotonically with one's earnings. For workers in the top quintile, the coefficient on job satisfaction is roughly 80 percent larger than that for workers in the bottom quintile. The second row reveals, perhaps unsurprisingly, that greater wages also lead to workers staying longer with their employers. Unlike with job satisfaction, the effect wages have on tenure is largest for workers in the (lower-)middle of the wage distribution. A wage increase elongates firm tenure more than 50 percent more for workers in the third quintile compared with workers in the upper two.

Table 1.6: Willingness-to-Pay for Improved Job Satisfaction by Wage Quintile

	1st Wage Quintile	2nd Wage Quintile	3rd Wage Quintile	4th Wage Quintile	5th Wage Quintile
Overall rating	0.063***	0.071***	0.083***	0.094***	0.114***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Log wage	0.680***	0.865***	0.922***	0.588***	0.607***
	(0.019)	(0.034)	(0.035)	(0.023)	(0.010)
Observations	151944	150988	140225	147535	147481
Ratio of coefficients	.093	.082	.09	.159	.187
Mean wage	23266	32592	43702	61488	109746
MWP one additional star	2160	2665	3923	9785	20560

Notes: This table reflects ordered probit models of wages and overall job satisfaction ratings on firm tenure by the worker's wage quintile, where the wage reflects the median among workers with the same job title and firm that year. Sample is restricted to completed job spells for full-time workers. Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

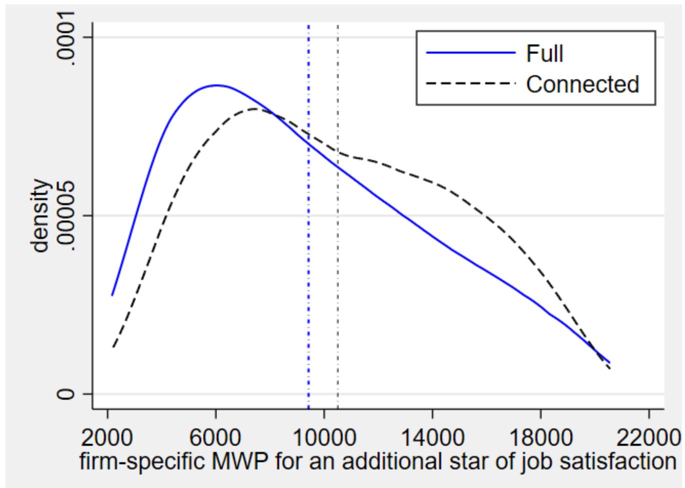
Because wages and job satisfaction affect firm tenure, comparing their contributions provides a means by which to convert stars of job satisfaction into dollars. The marginal willingness to pay (MWP) for a one-star increase in job satisfaction can be approximated by calculating $\frac{\beta_R}{\beta_w} \times \bar{w}$, the estimates of which are presented in the final row of Table 1.6. For the lower three wage quintiles, because the ratio of the coefficients is approximately half that of the upper two quintiles, the MWP in these wage brackets is noticeably less at \$2,200–3,900 per star. For the upper two quintiles, increased coefficient ratios combined with noticeably greater mean wages produces relatively high MWP estimates of \$9,800–\$20,600 per star. Evidently the value of job satisfaction, and thus non-wage amenities, appears to monotonically rise with wages, and increasingly so along the upper-half of the wage distribution.

Since workers of varying wage levels value job amenities differently, then the amenity value each firm offers its workforce will differ as well since the average MWP will differ depending on the composition of the firms' workforce. Low-paying firms employ more low-wage workers with low MWP compared with high-paying firms which employ more high-wage workers with high MWP. To account for this heterogeneity in MWP, let ϕ_k^l represent the share of firm k's workers with wages in the l quintile. Then, the average MWP for firm k can be approximated by $MWP_k = \sum_{l=1}^{5} \phi_k^l MWP^l$. For the Full and Connected sets, the distributions of MWP_k are plotted in Figure 1.6.[37] Because high-wage workers are willing to pay

[37] The two distributions are highly similar in shape though the Connected sample's is shifted slightly to the right, highlighting how the Connected set is comprised of higher-paying firms (which employ more workers in the upper two wage quintiles and so exhibit greater MWP).

more for job satisfaction, then firms that increasingly employ high-wage workers offer greater amenity value. In turn, a firm's MWP can range from \$2,000 to \$20,000 per star.

Figure 1.6: Distribution of Firm-Specific Wage-Based MWP for Job Satisfaction



Notes: This figure plots the distribution for the MWP for an additional star of job satisfaction across firms, where the MWP for each firm is the weighted average according to the distribution of the firms' workers across wage quintiles and the MWP within each quintile from Table 1.6. Solid blue line reflects the Full sample of firms, the dashed black line the Connected sample. The thick (thin) dotted vertical lines reflect the mean across firms.

Section 1.5.3 documented a strong positive relation across firms between the wage premia offered and the job satisfaction experienced by employees. With estimates for the dollar-value workers place on an additional star of job satisfaction and the share of job satisfaction attributable to non-wage amenities in hand, I quantify the firm-specific amenity value in dollars firm k offers its workers, A_k, according to

$$A_k = \underbrace{(1 - contribution_{pay})}_{\substack{\text{non-pay-satisfaction share}\\ \text{of job satisfaction}}} \times \underbrace{MWP_k}_{\substack{\text{dollar-equivalent}\\ \text{of job satisfaction}}} \times \underbrace{(\bar{R} + \hat{\lambda}_k^R)}_{\substack{\text{firm premia}\\ \text{of job satisfaction}}} . \quad (1.6.2)$$

The difference in firms' wage offerings W_k can be captured by converting the log wage premia $\hat{\lambda}_k^w$ into dollars through multiplying by the sample average $\bar{w}$, i.e.

$$W_k = \bar{w} \times e^{\hat{\lambda}_k^w}. \tag{1.6.3}$$

The firm's total compensation (relative to other firms) is then summarized by $W_k + A_k$.

1.7 Firm-Level Dispersion Accounting for Amenities

Just how important are firms for explaining the distribution of worker compensation (wages plus amenities)? Recent work has emphasized a more limited role for the firms themselves, documenting instead an increased role for labor market sorting of high-wage workers into high-paying firms (Card et al., 2013; Song et al., 2019; Bonhomme et al., 2020). Other work has attributed a sizable fraction of what differences there are in pay between firms to compensating differentials for less-favorable workplace attributes (Sorkin, 2018; Morchio and Moser, 2019). However, because I find instead that higher-paying firms offer workers more amenity value, wages alone will in fact *understate* the degree to which firms explain the distribution of worker compensation.

Ignoring job amenities, the dispersion across firms is captured by the distribution of W_k. Incorporating amenities, the dispersion across firms reflects the distribution of wages plus amenity value, $W_k + A_k$. Using both the Full and Connected sets, three measures of dispersion — the variance, the log difference between the 10th and 50th percentiles, and the log difference between the 90th and 50th percentiles — for W_k and $W_k + A_k$ are presented in Table 1.7. Two specifications for $W_k + A_k$ are considered. The "Fixed MWP" approach computes A_k holding MWP constant across firms, i.e. $MWP_k = M\bar{W}P$. The "Wage MWP" approach computes A_k allowing MWP to vary across firms. The former captures differences in amenity value absent heterogeneous preferences, whereas the latter captures the additional dispersion attributable to variation in how workers value job amenities.

For wages alone, the variance across the Full sample is 4.8 log points. When the value of firms' amenities are included, and assuming no heterogeneity in willingness-to-pay, the variance across firms rises to 8.7 log points, a 81 percent increase. This increase attenuates to 65 percent (7.9 log points) after accounting for the fact that low-to-middle wage workers exhibit relatively low MWP for job satisfaction. This would imply that the high wages enjoyed at higher-paying firms do not primarily reflect equalizing differences for worse fringe benefits, unfavorable working conditions, stunted human capital development, or poor interpersonal relationships. Rather, workers at these firms enjoy better wages and better amenities. When looking instead across the Connected set, the jump in compensation variance across firms is shallower but still pronounced at 50 percent, from 2.2 to 3.3 log points.

Table 1.7: Dispersion Across Firms Adjusting for Amenity Quality

Measure of dispersion	Full sample			Connected sample		
		With amenities			With amenities	
	Wages only	Fixed MWP	Wage MWP	Wages only	Fixed MWP	Wage MWP
Variance	0.048	0.087	0.079	0.022	0.034	0.033
p50 - p10	0.287	0.375	0.337	0.200	0.247	0.215
p90 - p50	0.227	0.290	0.325	0.164	0.222	0.250

Notes: This table summarizes how three measures of dispersion differ when the firm amenity value is incorporated into the difference in compensation across firms. Fixed MWP uses a constant MWP (the sample average) across firms, and Wage MWP reflects the distribution of firm-specific MWP displayed in Figure 1.6 that allows MWP to vary with workers' earnings according to Table 1.6.

A similar takeaway of increased firm-level dispersion can be observed using distributional comparisons such as the log difference between the 50th and 10th percentiles (50–10 ratio) or the log difference between the 90th and 50th percentiles (90–50 ratio). Comparing wages alone, 29 log points separate the median and 10th percentile while 23 log points separate the 90th percentile and the median. Incorporating amenity value absent heterogeneity, these two gaps widen by 9 and 6 log points, respectively. When heterogeneity in MWP is incorporated, the 90–50 ratio widens another 4 log points, while the 50–10 ratio attenuates 4 log points — reflecting the steep rise in MWP at the upper-tail of the wage distribution. Taken together, the gap between the 90th and 10th percentiles (90–10 ratio) widens by 15 log points, equivalent to a 29 percent increase. For the Connected set, a similar pattern arises: Accounting for amenities with firm-specific MWP raises the 90–10 ratio by 10 log points, equivalent to a 28 percent increase.

Taken holistically, the bundle of job amenities is inequality exacerbating, ratther than attenuating. Employees at higher-paying firms benefit from higher-quality amenities, and in turn, higher-paying firms are even higher compensating than wages alone report. Although two-way fixed effects models have found a growing role for working sorting and a more limited role for the firm (Song et al., 2019; Kline et al., 2020), differences in amenity offerings between firms and a non-zero willingness to pay for improved amenities translates into missing dispersion between firms, on the order of 50–65 percent.

1.8 Sensitivity Analysis

In this section, I investigate the robustness of these findings under alternative specifications. For the baseline and each alternative, Table 1.8 details the following: the number of firms for which W_k and A_k are estimated, the slope and its standard error between firms' wage and rating premia, the percent of the slope attributable to increased pay satisfaction, the difference in amenity value between the first and ninety-ninth percentile relative to the mean wage, the elasticity of total compensation inclusive of amenity value ($W_k + A_k$) to wages, and the increase in compensation dispersion across firms (variance and ninety-ten ratio) compared with wages alone. Rows 1 and 2 present these measures for the Full and Connected sets, respectively. For these two sets, the difference between the bottom (first percentile) and top (ninety-ninth percentile) firms by offered amenity value can be as large as 50–84 percent of the average wage; reassuringly, the 56 percent estimate of Maestas et al.

(2018) for moving from the worst to the best jobs falls within the lower end of this range.

Under the baseline approach, wages reflect workers' annualized base earnings and as such, omit the receipt of bonuses and commissions. Since more profitable firms increasingly offer variable earnings (Sockin and Sockin, 2021), omitting variable pay and focusing solely on base earnings may understate the degree of dispersion in firms' wage premia. As shown in row 3 however, incorporating variable pay into workers' wages alters the results little.

Table 1.8: Alternative Specifications

Specification	Firms	Wages vs. ratings: Slope	Wages vs. ratings: Standard error	Share pay satisfaction (%)	Amenity gap p1-p99 (% avg wage)	Elasticity	Increase from wages: Variance (%)	Increase from wages: 90-10 ratio (log points)
1. Baseline	70,115	0.47	(0.02)	8	84	1.07	64	15
2. Connected sample	10,737	0.72	(0.04)	11	50	1.09	52	10
3. Incorporate variable pay†	68,566	0.46	(0.02)	8	83	1.07	59	15
4. Include length of spell FE^	70,115	0.46	(0.02)	8	83	1.07	64	15
5. Completed 0-1 years spell^	27,842	0.43	(0.04)	11	91	1.05	75	16
6. Completed 5+ years spell^	27,478	0.39	(0.04)	11	103	1.05	106	19
7. Reviews from full-time employees^	54,499	0.60	(0.03)	8	95	1.09	88	18
8. Reviews mentioning 5+ amenities^	22,488	0.77	(0.04)	7	105	1.10	124	21
9. Exclude possible sock puppetry^	41,163	0.49	(0.03)	9	86	1.07	74	15
10. Relax assumption of linear ratings^	70,095	–	–	8	93	1.05	68	14
11. Only female employees	28,632	0.49	(0.04)	11	101	1.08	98	18
12. Only male employees	30,185	0.45	(0.03)	7	88	1.07	64	16
13. Only current employees	29,061	0.61	(0.03)	5	95	1.08	88	19
14. Only industry switchers	35,729	0.39	(0.03)	11	84	1.06	52	13
15. Only low-paying jobs	16,737	0.55	(0.05)	12	137	1.10	165	26
16. Only high-paying jobs	19,235	0.66	(0.06)	7	90	1.10	131	20
17. Only job title stayers	9,642	0.41	(0.10)	10	127	1.06	319	22
18. Include metro-year and job title FE	38,616	0.63	(0.04)	10	101	1.09	131	21
19. Include order of observation FE	70,115	0.48	(0.02)	8	84	1.07	64	15
20. Workers with 3+ observations	20,951	0.43	(0.04)	8	81	1.07	59	13
21. Restrict sample to 2017–2019	13,772	0.43	(0.05)	5	109	1.08	119	21
22. Establishment (firm-metro) premia	56,052	0.49	(0.02)	7	104	1.08	84	19
23. Only workers in both panels	30,362	0.58	(0.03)	9	89	1.10	70	16

Notes: This table displays the coefficients from regressions of the firm fixed effects for job satisfaction on the firm fixed effects for wages under alternative specifications. Metros in Glassdoor correspond roughly to core-based statistical areas (CBSAs); there are 858 unique metros in Glassdoor and 929 CBSAs. Standard errors are bootstrapped. ^ indicates only the ratings panel is affected and the wages premia are unaltered. † indicates only the wages panel is affected and the ratings premia are unaltered.

In the AKM framework, the identification for each firm-specific premium relies on the experiences of job switchers entering or exiting the firm. But, not all job transitions are alike. Workers who experience a low-quality match, i.e. a job that lasts at most one year, will have different outside options and reasons for exiting compared with workers who experience a high-quality match, i.e. a job that lasts at least five years. To account for these factors related to firm tenure, I incorporate fixed effects for the length of the worker's job spell when estimating the ratings premia; as shown in row 4, the results are unchanged. Additionally, workers who have experienced a low- or high-quality match may be uniquely situated to speak to differences in amenity quality across firms from having salient benchmarks for comparison. In rows 5 and 6, I restrict the sample to only workers who have experienced a low-quality or high-quality match, respectively, when estimating the ratings premia, and find that the takeaway results hold, even magnifying somewhat.

Next, I address concerns that related to sample composition with workers' employer reviews. First, while the wage premia are captured using only full-time workers (because hours are unobserved), the ratings premia are estimated using employees of various work arrangements, including full-time, part-time, contract, and intern workers. Re-calibrating the ratings premia using only reviews from full-time employees in row 7 only strengthens the results. Second, workers differ in their willingness to discuss workplace amenities. Quantifying amenity value — and the relative importance of pay satisfaction for overall satisfaction — could vary based upon whether the ratings premia are gauged from workers who are increasingly willing to volunteer information. Restricting the sample to only employer reviews that detail at least five of the fifty amenities in row 8 reveals even starker results after incorporating amenity value. Third, employees may plant dishonest reviews if they are incentivized or threatened by their firms to do so. Identifying potentially suspect reviews following the methodology of Sockin and Sojourner (2020) and excluding such reviews from the analysis in row 9 does not alter the results.

One key assumption under the baseline model is that workers interpret the five stars scale for ratings linearly. This implies that workers value jumping from one star to two as equally as they would moving from four stars to five. However, if workers are risk averse, then we might anticipate utility to be concave in job satisfaction rather than linear — as avoiding poor outcomes would be increasingly desirable. To relax this assumption, I create binary indicators for each of the five star ratings, $1\{R = r\}$. Then, rather than estimating premia in overall ratings λ_k^R and a willingness-to-pay per additional star MWP_k, I calculate firm-specific premia under a linear probability model for the likelihood of each star rating λ_k^r, a willingness-to-pay for each individual star rating (relative to a one-star rating), MWP_k^r, and aggregate $\sum_{r=1}^{5} \lambda_k^r MWP_k^r$. As shown in row 10, the results are highly similar.

Next, to address concerns related to sample selection into the wages and ratings panels, I investigate whether the results are driven by any particular category of worker or job transition. First, although male employees are over-sampled in the

Glassdoor data (Sockin and Sockin, 2019b), restricting the samples in the two panels to only include female or male employees in rows 11 and 12, respectively, reveals that both on average receive greater amenity value at higher-paying firms. Second, because Glassdoor is a platform through which workers learn about employment opportunities, the concern may arise that the workers supplying their wages and reviews to the website are negatively selected on ability, disgruntled former employees that have been laid off, or both. As shown though in row 13, restricting the sample to only wages and employer reviews provided by still currently employed workers changes the takeaway results little. Third, the salience of non-wage amenities may differ depending on whether the worker chooses to remain in or exit an industry. Workers may decide to switch industries precisely to achieve improved amenities, especially if there are more salient differences across industries than within. Focusing only on industry switchers in row 14 however attenuates the results only slightly.

Additionally, workers' evaluations of job satisfaction and amenity quality could differ depending upon their position with the firm. As workers ascend the job ladder, amenities could differ because they exhibit heterogeneous preferences, have heterogeneous experiences, or both. In fact, looking across workers within the same firm, low- and high-wage workers have markedly different evaluations of amenity quality (Table A.23).[38] Consequently, a firm's overall rating premium could differ

[38]There are two takeaways worth highlighting. First, forty of the fifty amenities exhibit a statistically significant slope with wages. Second, the amenities that are negatively related with individuals' wages include many pecuniary attributes, whereas the amenities that are positively

depending upon whether the Glassdoor sample is comprised of low or high earners from the firm. Restricting the panels however to only low-paying or high-paying job titles in rows 15 and 16, respectively, only strengthens the results. In fact, focusing solely on job transitions in which workers retain their job title — and thereby minimizing differences between firms attributable to disparities in tasks or responsibilities — in row 17 while greatly reducing the sample of firms covered, reveals an even starker increase in dispersion when amenities are incorporated. Lastly, since amenities may vary between across locations as well, I show in row 18 that the results are robust to incorporating metropolitan area by year and job title fixed effects into the AKM models.[39]

Further, the arrival of a new wage report or employer review may be non-random. For one, subsequent wages or employers reviews may be selected on whether match quality improves or worsens. Additionally, workers transitioning into a firm may report systematically different sentiment than workers separating from a firm. Addressing these concerns by including fixed effects for the arrival order of each workers' wage or review into the AKM models in row 19 does not alter the results.[40]

related with individuals' wages pertain to working conditions and interpersonal relationships — though job security and office politics are notable exceptions. One explanation is that low-wage workers care primarily about pecuniary compensation beyond wages, while high-wage workers care more about intangibles. Another is that as workers climb the job ladder, workers benefit from improved attributes that were previously inaccessible at lower rungs. Examining these possibilities further I leave to future researchers.

[39]Since reporting one's location and job title in an employer review is optional and may reflect a strategic concealment decision (Sockin and Sojourner, 2020), the baseline approach conditions on neither.

[40]Subsequent wages are on average more positive, consistent with movement up the job ladder over the life cycle or positive selection on unobservables, and subsequent ratings are on average more negative, possibly reflecting an increased propensity for workers to voice their views when dissatisfied (Table A.24).

Most workers in the two panels though are only observed twice, implying that the worker fixed effects from the AKM models will be imprecisely estimated, which in turn, could spillover into the firm fixed effects. Restricting the sample in row 20 to include only workers in each panel who are observed at least three times though does not change the results.

These findings are also not driven by employers' differential responses to the COVID-19 pandemic, or by structural changes to the website and its users over time, as restricting the sample to only wages and reviews for the three year period 2017–2019 in row 21 only strengthens the results. Finally, in the AKM literature, the employer traditionally corresponds to an individual establishment rather than a firm in aggregate. Because providing the location for an employer review is optional, the baseline approach does not distinguish between firms' various establishments. Recalibrating the model in row 22 to separate firms into establishments according to metropolitan area offers the same findings.

1.9 Discussion and Limitations

If firms do not set amenities according to a compensating differential framework, it raises the question how do firms choose the quality of amenities to supply? Why do some firms offer better quality amenities than others? One obvious possibility is that firms differ in their marginal costs of amenity provision (Rosen, 1986a). However, to be consistent with the positive relation observed across firms between wages and amenities, this would imply that high-paying firms uniformly exhibit economies of scale in providing amenities compared with their lower-paying counterparts. One alternative is that more-productive firms compensate workers through greater wages and amenities because both are normal goods, as in the theoretical exposition of Appendix A.1 (Mortensen, 2003; Lang and Majumdar, 2004). Relating the firm premia for wages and satisfaction with a measure of average labor productivity for public firms available through Compustat (Figure A.7) lends support to this view.

There are alternative theories though that warrant further exploration. For one, amenity provision may reflect a strategic decision for targeting optimal tenure with the firm. Since more satisfied workers stay longer on the job, firms interested in fostering a low-turnover, high-retention workforce may offer improved amenities. While a dollar in wages is identical across firms, observing the quality of an employer's amenities may require direct inspection through on-the-job experience (Menzio and Shi, 2011b), thereby rendering exiting a firm and relinquishing its amenities a risky decision. Second, improved amenities may cause productivity to increase and subsequently drive wage growth, though the evidence on this relation is mixed (Iaffaldano and Muchinsky, 1985; Böckerman and Ilmakunnas, 2012). If employees who are more satisfied — a concept that predominantly reflects non-wage aspects of work — are able to produce more efficiently (Bellet et al., 2019) or have improved complementarities with peers, then firms may become higher-paying through promoting amenity quality. Finally, the provision of high-quality amenities could reflect a means by which to attract top talent. Given that high-wage workers place more value on job satisfaction, higher-paying firms providing more-favorable non-wage amenities may help explain the high degree to which productive workers sort into productive firms (Card et al., 2013; Borovičková and Shimer, 2017; Hagedorn et al., 2017; Lopes de Melo, 2018).[41]

Although this work contributes to a budding literature on job amenities by making use of novel data on job switchers' satisfaction and amenity values at different jobs, there are a number of limitations that future work in this area may help address. For one, the analysis rests on the firm fixed effects being precisely estimated in the two-way fixed effects models for both wages and satisfaction levels. However, workers in each of the two samples only have on average 2.2–2.4 observations (Table A.11). The thinness of the two panels implies that the worker fixed effects will be noisily estimated, which can spillover into the firm fixed effects being imprecisely estimated if there are few identifying movers. This concern is somewhat alleviated by the fact that the main takeaways follow through when the sample is restricted to a more-connected set of firms with many movers. However, the inability to observe the same workers' wages and amenities more frequently for each employer hampers the possibility of controlling for potential violations to the assumption of exogenous mobility, e.g. match-specific quality, on-the-job learning, peer effects, and

[41] Since amenities are unobserved in administrative employee-employer matched datasets of labor earnings, they are necessarily omitted. Analyses using wages alone may then understate the role of firms and overstate the role of sorting. While the variance between firms has risen in the United States since the late 1970s, that increase has been attributed to changes in the composition of workers within firms (Song et al., 2019).

labor demand shocks, as well as accounting for time-varying preferences in workers' valuations of the firms' amenities throughout their tenure with the firm.

Additionally, since I seldom observe each worker and only at instances of employment, the time between between jobs is unobserved. The average time between observations is 1.7–2.3 years (Table A.11), suggesting that workers' preferences for amenities are unlikely to have changed much between observations. However, because workers are not observed in consecutive time periods — as is typical for administrative employee-employer matched data often used in the AKM literature — determining whether the pair of matches observed for job switchers constitutes a job-to-job transition, job-to-nonemployment followed by nonemployment-to-job transitions, or even if there were intermediate unreported jobs in between. The absence of continuous employment histories hampers determining whether the jobs I observe in the data are selected in some manner relative to jobs that are unobserved. Moreover, although the data include whether each worker was no longer employed with the firm at the time of the review, the reason for each separation is unobserved. Although the job duration approach for estimating willingness-to-pay of Gronberg and Reed (1994) incorporates voluntary and involuntary separations, the wage declines and rating declines observed in the data may reflect low bargaining power from an involuntary spell of non-employment between jobs rather than a willingness to separate from a match to accept lower pay or satisfaction at another.

Last, the empirical analysis is limited to coverage in Glassdoor data. Only firms represented by job switchers will be included. Unfortunately, I cannot test for whether the results regarding firm-level amenities extend to all U.S. firms. In particular, if there is heterogeneity in the relation between wages and amenities by firm age or firm size, then the applicability of the results for new or small firms that are more likely to be overlooked in the sample may be limited. Furthermore, because the sample period only covers the years 2008–2021, making conclusions about how non-wage amenities, their relation to firm-level wages, and their dispersion have changed over time is not feasible. While Pierce (2001) speaks to how dispersion in fringe benefits has evolved over time, research that could speak to how dispersion in non-pecuniary amenities has changed over time would uniquely contribute to the inequality literature.

1.10 Conclusion

Using matched employee-employer data on workers' wages and job satisfaction levels, I find that higher-paying firms offer their workers more in amenity value than lower-paying firms. In gauging a comprehensive set of hard-to-observe, hard-to-quantify amenities from workers' descriptions of their employers, I document how nearly the entire bundle of amenities improves with the wage premium a firm offers, including better working conditions, fringe benefits, interpersonal relationships, and human capital development. This evidence runs contrary to the idea that the boon in wages workers enjoy from employment at a higher-paying firm primarily reflects an equalizing difference for lower-quality amenities. That is not to say though that there is not a compensating differential whereby workers are willing to accept somewhat lower wages in exchange for better amenities.[42] However, when looking across vertically-differentiated firms, the higher-paying employer is more likely to offer an improvement in job amenities than a decline.

Since high-paying firms are high-amenity firms, wages alone understate labor market inequality. In turn, if we were to account for job amenities, low-wage workers would have even lower lifetime compensation than high-wage workers, the opportunity cost of work would be even greater for high-wage workers, and the returns to investing in human capital and climbing the firm ladder would be even more pronounced. Now that capturing hard-to-measure non-wage amenities is feasible given the advent of online job boards, linking such data to labor market interventions that may alter compensation packages, such as minimum wage laws (Clemens, 2021) and tax policies (Powell and Shan, 2012), as well as theories of job search, occupational sorting, educational attainment, worker bargaining, and firm dynamism would be promising avenues for future research.

[42]Implementing a hedonic approach for estimating the willingness to pay for an additional star of job satisfaction reveals a positive MWP, but only after conditioning on the productivity of the match through worker fixed effects. More details are provided in Appendix A.6.

Chapter 2 What's the Inside Scoop? Challenges in the Supply and Demand for Information on Employers

2.1 Introduction

Workers face information problems in choosing between employers that appear the same but differ in unobservable ways. Jobseekers value information that helps them better understand how a prospective employer treats workers, especially in hard-to-observe and hard-to-contract dimensions. Nelson (1970) described experience goods as those whose quality cannot be learned before transaction. Menzio and Shi (2011a) develop a directed search model that permits analysis of the extent to which prospective jobs are experience goods versus inspection goods, where match quality and the value of a prospective job is known to the jobseeker before accepting it. They find that the experience-good model better explains cyclical labor-market dynamics and that this difficult-to-observe match quality explains a huge share of variance in job productivity.[43]

[43]Jobs are complex, difficult to fully characterize and subject to change. An incomplete list of job attributes includes wages; aspects of health insurance quality and cost; criteria for and schedule of potential raises; opportunities for career development and advancement; risk of illness, injury, or fatality; degrees of autonomy and micro-management; personal and professional (dis)courtesy paid by one's supervisor and peers; presence of sexual harassers; layoff risk in a downturn; whether one is routinely asked to work overtime and what consequences follow from refusal; and ease in scheduling time off to take a child to the doctor. Attributes' starting levels and possible future

Hungry to understand the job quality they can expect, jobseekers seek the inside scoop from firms' current and former employees who have private knowledge of how the firm treats its workers. Carmichael (1984a) developed a theory of employer reputation in the labor market. He argues, "Since a searching worker does not typically get to observe a firm very closely before he joins it, it does not seem sensible to assume he has intimate knowledge of its technology or the tastes of its owners." An employer's public reputation helps jobseekers deal with this information problem, "if the worker himself is not very different from other workers the firm has hired in the past, then he may do very well just by assuming that the firm will treat him as it has treated everyone else." However, insiders' private information about jobs at a particular firm does not flow easily to interested jobseekers, unless they are lucky enough to share a social network and take the time to discuss it individually. An insider supplying accurate, private information about an employer creates a positive externality for other workers and so, such information will tend to be undersupplied via voluntarism, a theoretical point Avery et al. (1999) developed with respect to general voluntary evaluation systems. Lacking a market-clearing mechanism, there's no reason to expect the supply of information to meet demand.

Better evidence on workers' use of information about job quality and attributes would improve our understanding of labor markets but these processes have been difficult to measure until recently. The introduction of online platforms such as LinkedIn, Glassdoor, and Indeed has enabled workers to search for, learn about,

paths can matter. These are challenging for jobseekers to credibly understand.

and apply to jobs online. Before online search existed, workers had to rely on newspapers word of mouth, personal networking, and best-places-to-work stories in magazines to learn about potential employers. The movement toward online job search has changed how workers gather, process, and act on job-related information and what economists can observe. Now, seventy percent of unemployed Americans use the internet to search for jobs.[44]

Harnessing data from an institution that facilitates the flow of information about job quality and attributes between workers, Glassdoor, we develop evidence to better understand workers' supply of and demand for information about employers. Broadly, Glassdoor is similar to other third-party review sites like Yelp or Tripadvisor, in that each relies on volunteers to supply private information toward the public good of publicly-available reputation for counter-parties. However, in the case of Glassdoor, the reputation system helps workers share information about employers rather than consumers about sellers.

We focus on three research questions:

- How do volunteered ratings affect labor supply to firms, if at all? Are the effects stronger for less well-known firms?

- How do jobseekers value marginal information supplied? Equally across the range of employer ratings or more-strongly for negative information?

- How do risks of employer retaliation affect the supply of inside information

[44] Authors' analysis of November 2019 Current Population Survey.

from potential volunteers?

We begin by developing novel evidence on how reputation affects firm-specific labor supply in the broad labor market. We exploit an ability to see each firm's overall rating at high precision when the firm posts each job opening and measure the impact on application rates, adapting the regression discontinuity logic of Luca (2016) in analyzing restaurant reviews on Yelp. Looking at jobseeker application rates to multiple job postings by each firm, we contrast application rates for postings made when the firm's average overall rating was just above versus just below a threshold for rounding to a tenths-place digit. This generates a +0.1 star (on a one-to-five stars system) shock between postings to the jobseeker-observed rating when the true ratings actually differ by very little. For example, we contrast application rates for postings with an average rating of 3.651, which rounds to 3.7, versus 3.649, which rounds to 3.6. We pool across multiple such thresholds. This design reveals that higher ratings tend to increase application rates among firms with relatively-few prior reviews but not among those with many prior reviews. Because of labor supply effects, firms, especially smaller ones, have incentives to discourage insiders from volunteering negative reviews as well as to encourage supply of positive ones.[45]

[45]Benson et al. (2020a) found evidence of positive labor supply effects of better firm reputation and that this effect is strongest among less well-known firms in a market for online micro-tasks: Amazon Mechanical Turk. We extend this to the broad labor market and find similar results. While literatures in economics, management, and personnel psychology have all tried to estimate effects of firm reputation on labor supply, other prior evidence comes almost exclusively from the lab or from observational studies that lack credibly-exogenous variation in firm reputation (Turban and Cable, 2003; Collins and Han, 2004; Schmidt et al., 2012; Harvey and Morris, 2012; Lievens and Slaughter, 2016; Makarius and Stevens, 2019). An exception is Brown and Matsa (2016), which exploited high-frequency changes in major financial firms valuations and found

Secondly, we test whether jobseekers value new information across the full spectrum of employer quality equally. They do not. We find that jobseekers find reviews that help guide them away from worse employers more helpful than reviews that help guide them towards better employers. This finding contrasts with those from consumer review sites and, thus, does not appear to be a fundamental property of reputational information flow. Meta-analysis of the consumer review literature does not find that negative reviews are rated more helpful (Hong et al., 2017). We do not know of prior investigations into what constitutes helpful information in employer reviews.

The finding that jobseekers vote reviews delivering more-negative information more helpful is robust to using any available uni-dimensional, vertically-differentiated measure of how positively/negatively volunteers evaluate their firms, such as the one-to-five star overall rating, the share of "Pros" versus "Cons" text written in the review, and whether the volunteer would recommend the firm to a friend. This stronger relative demand for negative information holds across firms of different ages, sizes, review counts, and pay premia. Where we can precisely track a particular volunteer's votes, we find that the increased demand for more negative information holds true regardless of the volunteer's level of satisfaction with their own employer, highlighting that demand for negative information is not restricted to the dissatisfied or the satisfied alone. The only exception is when the jobseeker votes

that jobseeker applications to a firm fall as public information about prospects for firm survival diminish.

for a review of their own employer: those who rate their own employer highly also tend to vote more helpful others' positive reviews of their employer. This could be sincere or part of employers' efforts to promote their own reputations.

We develop and discuss evidence on why workers may demand negative information. Risk-averse workers will value a signal that shifts their posterior beliefs about the expected value of a potential job down by ϵ more than a signal that shifts it up by ϵ. Given employers' incentives to supply positive information and worker risk aversion, it would perhaps be surprising if jobseekers expressed no preference for negative information. In addition, positive information about a potential employer may be available outside a reputational information system. In fact, we establish that job descriptions written within postings more closely resemble the "Pros" text of reviews than the "Cons" text, a new though perhaps not surprising finding. Further, we develop evidence that the preference for negative information is robust to omitting weeks with unusual spikes in the supply of positive reviews—which may be due to employer encouragement of positive reviews (sock puppetry) as well as considering differences in how abstract the language used is between positive and negative text.

Thirdly, we explore a potential challenge for the flow of accurate information into a reputation system: whether risk of employer retaliation off-site against insiders who supply negative information discourages its supply and distorts the ability of jobseekers to distinguish better from worse employers.[46] We document that vol-

[46] A Society for Human Resource Management article on managing employer reputation online

unteers on Glassdoor are more likely to conceal aspects of their identity—their job title or location—when reporting negative rather than positive information about a particular firm. Corroborating evidence comes by testing whether, conditional on supplying negative information, volunteers were more likely to conceal aspects of their identity when they face higher retaliation risks, either reporting on a current (rather than former) employer or working at a smaller firm where they blend into a smaller pool of likely suspects. Even looking across multiple reviews from the same volunteer, a volunteer is more likely to conceal aspects of their identity when leaving a more negative review, when leaving a review about a current rather than former employer, and when reviewing a smaller rather than larger employer. Further, the (inverse) relationship between review rating and rates of identity concealment is stronger in contexts with greater risk of retaliation. We study this phenomenon using multiple identification strategies, including examining differences within person, using variation in the implicit costs of retaliation driven by variation in state unemployment rates over time, and quasi-experimental variation in the availability of identity concealment options to potential volunteers within the Glassdoor platform over time.

Although the option for volunteers to conceal identifying aspects causes more

reports an attorney's advice to have all employees sign confidentiality agreements on hire so that, if they share negative information online, the company can assert that they are in violation and threaten legal action (Grensing-Pophal, 2019). Further, over 100 employers have sued Glassdoor to try to get the company to reveal the identity of reviewers posting negative reviews. Though almost all suits failed, the fact that companies file them is consistent with worker concern about potential retaliation if they were to communicate unflattering information about an employer (Glassdoor, 2018; Grothaus, 2020).

negative information to be supplied on the platform, we also document for the first time that concealment tends to degrade the value of the supplied information for jobseekers. Jobseekers tend to vote reviews less helpful when volunteers conceal aspects of their identity, consistent with concealment eroding jobseekers' ability to judge the supplied information's relevance to their own decision. Taken together, this implies a "Catch-22", leaving some volunteers stuck between honesty and fear, and jobseekers stuck between positive bias and extra noise.

Retaliation risk exists in other markets' reputation systems. Filippas et al. (2019) found that, within a market for online gig work, when the system shifted from reviews being observable only by the site operator to being public so that the rated worker and their future, potential employers could see what was said by whom, ratings of workers submitted by employers inflated. Others have studied the potential for retaliation to bias consumer or similar online gig work settings, including Cabral and Hortaçsu (2010) and Nosko and Tadelis (2015) for eBay consumers, Bolton et al. (2013) for eBay, the gig labor market RentACoder.com, and a few other sites, and Fradkin et al. (2019) for Airbnb. On Glassdoor itself, Marinescu et al. (2018a) found that marginal reviews by workers tend to be more negative than average reviews, suggesting a tendency for negative potential reviews to remain unvolunteered. Glassdoor reviews pertain to the market for in-person, long-term U.S. jobs and focuses on workers rating employers, making the stakes in choice and in retaliation plausibly higher than in previously-studied settings.

Retaliation risks are likely to be particularly severe with respect to reviews of long-term employers. When considering whether to volunteer a potential negative review, workers may worry much more about retaliation risk from their employer than they would worry about potential retaliation as a consumer from a seller. Retaliation risks for a negative review of a restaurant on Yelp or a printer on Amazon are quite limited. Employment is different. For most working-age adults, their single largest market relationship is with an employer. As Hart (1989) puts it, "the reason an employee is likely to be more responsive to what his employer wants than a grocer is to what his customer wants is that the employer has much more leverage over his employee than the customer has over his grocer. In particular, the employer can deprive the employee of the assets he works with and hire another employee to work with these assets, while the customer can only deprive the grocer of his custom and as long as the customer is small, it is presumably not very difficult for the grocer to find another customer." Is it worth the possibility of antagonizing your boss, losing your job, and causing a lifetime of retaliatory references in order to help jobseekers—whom you do not know—make more informed employment decisions?[47]

Taken together, this analysis suggests that workers struggle to get accurate, relevant information about prospective jobs in the U.S. labor market, making vertically-differentiating information about employers, especially less well-known employers,

[47] On Glassdoor, potential employer retaliation would happen off-site. There is no way for employers to retaliate on site. Unlike Mechanical Turk or oDesk, there are no jobs done through the site and no firm rating of workers.

valuable to workers. Institutions and policies that seek to improve the flow of information must confront the challenge that, when a system succeeds in affecting firm labor supply through the provision of such information, employers have stronger incentives to retaliate against volunteers—inducing bias and noise into the system, thereby degrading its value and its potency.

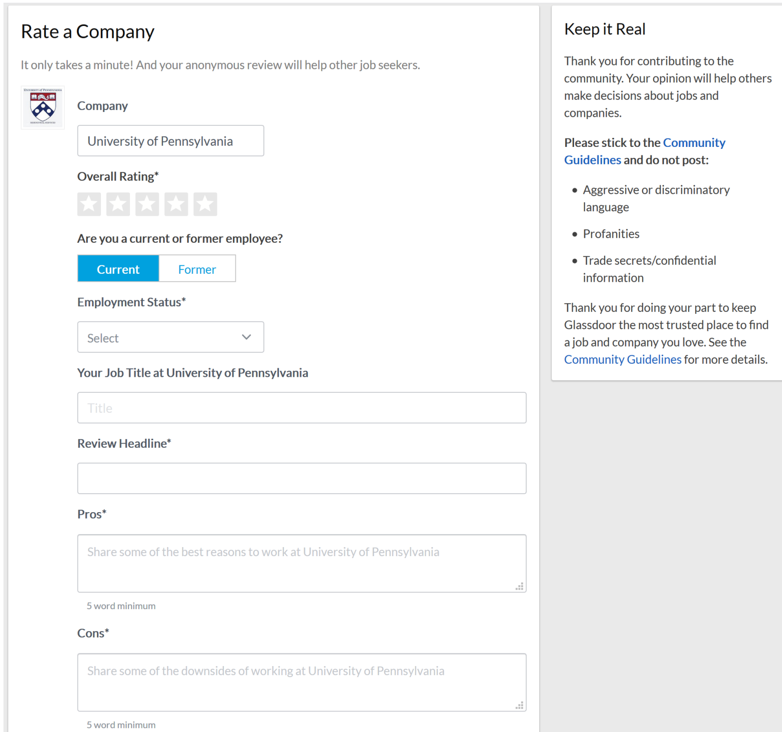
2.2 Setting and data

A volunteer can submit one employer review per employer-year, which she is free to update, but can review multiple employers within the same year.[48] Each review-r pertains to a job at a specific firm-f. Each has an associated volunteer $v(fr)$ and creation time $t(fr)$.

Each review contains many kinds of information. Figure 2.1 displays a blank review form for the University of Pennsylvania. Volunteers are asked to provide feedback on their employer, through both supplying a star rating for the firm and submitting free response text for the "Pros" and the "Cons" of working for that firm. In order to complete the review, volunteers are required to supply the following information: an overall rating, employment status, review headline title, "Pros" text, and "Cons" text. They also may have the option to voluntarily supply information about their job title, tenure at the firm, and location of employment.

[48]To create some accountability on volunteers, Glassdoor requires them to have a verified active email address or a valid social network account, assesses the content of each submitted review, and suppresses those outside their guidelines. Assessment guidelines are here: `https://help.glassdoor.com/article/Community-Guidelines/en_US`.

Figure 2.1: Blank Volunteer Review Form



Notes: Figure is a screenshot of the survey users fill out when submitting an employer review to Glassdoor. An asterisk indicates that the field is required. Overall rating is restricted to integer ratings between one and five stars. Once text is added to the “Review Headline,” “Pros,” or ”Cons” sections, users are asked to provide additional information (not shown), which includes location of employment.

The volunteer’s employment status, job title, and geographic location (if available) are displayed with their review to site visitors but not their identity. If a volunteer leaves multiple reviews under the same account identity, Glassdoor and the researchers can link these reviews as being left by the same person. Site users cannot.

Each volunteer assigns the firm an overall rating (R) on a one-to-five-star Likert scale, a vertically-differentiated summary of the volunteer’s overall evaluation of the employer explicitly meant to inform other workers about the quality of employment provided by the firm. We sometimes refer to reviews that include one- or two-star ratings as negative reviews and those including four- or five-star ratings as positive reviews. Broadly, the site contains about twice as many positive reviews as negative reviews.[49]

Once a volunteer's review is added to the website, people who go to the website to learn about that employer can see the review and vote it as helpful. Until a few years ago, users could also vote it as unhelpful (Appendix B.3 discusses in greater detail). We use these (un)helpful votes as a way to measure jobseeker demand for different kinds of information about firms and jobs.

Given the public nature of each review, worried volunteers may try to conceal aspects of their identity. We measure whether a volunteer attempts to conceal their identity by how they fill in the job title and location fields. Forty-five percent of reviews' volunteers choose to either leave the job title field blank, include the word "anonymous" in the job title, or end their job title with "employee." Volunteers can also hide identifying information by leaving the geographic location field blank. About forty-two percent of volunteers do not report a location. These aspects of the volunteer's identity, or lack thereof, pipe into the display of the review that jobseekers see.

When a jobseeker reads a review, she can click to classify the review as either helpful, unhelpful, or refrain entirely from expressing an opinion. To understand

[49]The distribution of reviews by overall rating in the data is 15.3% one-star, 12.9% two-star, 20.4% three-star, 24.2% four-star, and 27.3% five-star.

the demand for information, we measure the helpfulness of any review by the share of jobseeker votes that are helpful rather than unhelpful. Review-r's helpfulness to jobseekers is then given by $H_r = \#\text{helpful}_r / (\#\text{helpful}_r + \#\text{unhelpful}_r)$, which we refer to as helpful share.[50] When analyzing helpful share, we analyze the 685,505 reviews posted before 2015 that received at least one of the 1.90 million helpful or 0.23 million unhelpful votes. Reviews averaged 2.76 helpful votes, 0.34 unhelpful votes, and a helpful share of 0.86 with a standard deviation of 0.31. Using the helpful share, rather than the helpful count, allows us to control for the number of jobseekers who saw a review but results are similar in any case. Other reviews posted during this time received no votes at all (731,989) and are excluded because we lack a way to control for how (in)frequently jobseekers viewed them or a measure of how meaningful the review was to jobseekers who did in fact view them.

Glassdoor has additional information about reviewed firms through an employer lookup table. For each firm, there is a single entry that contains the following information (when available): the industry of the firm, the most-recent employment total for the firm (meaning that our measure of firm employment has no time variation), and the year in which the firm was founded.[51] Firm age is then calculated

[50] Analyses with this outcome will always control for the year-month the review was posted. In 2015, Glassdoor phased out the option for jobseekers on its website to classify reviews as unhelpful, leaving the helpful and refrain options. For an in-depth description of the time line, see Appendix B.3. For the share of unhelpful classifications submitted for employer reviews over time, see Figure B.3.

[51] There are 25 broad industries in the data: Accounting & Legal, Aerospace & Defense, Agriculture, Arts & Recreation, Biotech & Pharmaceutical, Business Services, Construction, Consumer Services, Education, Energy, Finance, Government, Health Care, Hospitality, Information Technology, Insurance, Manufacturing, Media, Mining, Non-Profit, Real Estate, Restaurants, Retail, Telecommunications, and Transportation.

as the difference between the year in which the review is submitted and the year the firm was founded. We incorporate each of these three firm characteristics in order to study differences in identity concealment, review helpfulness and job satisfaction among workers across firms.

2.3 Results

The next three subsections present evidence on our three research questions. In a fourth, we discuss the findings overall and complementary evidence guiding interpretation.

2.3.1 How do volunteered ratings affect labor supply to firms, if at all? Are effects stronger for less well-known firms?

Our measure of labor supply is the rate at which jobseekers apply to a firm's job posting, which we compute as the number of jobseekers who click on a posting's "Apply Now" button per 100 jobseekers who are presented a summary view of the posting within a list of search results (example in Figure B.1). Section B.1 details the sample for this analysis and how it was constructed. This measure – appliers per 100 viewers – helps focus on jobseekers' choices and adjusts for any differences in how frequent postings were presented to jobseekers.

Our design takes advantage of a rounding discontinuity. The employer star rating observed by jobseekers on each job posting is rounded to the tenths digit but based on a more-precisely calculated rating index that we can measure. To credibly identify the effect of a 0.1 higher observed rating within an otherwise very homogeneous set of postings, we limit the sample to postings by employers with a latent rating within a 0.01 bandwidth on each side of a rounding threshold and define an indicator for whether the posting's observed rating reflects a round up rather than a round down.[52] We estimate effects of the induced 0.1 higher observed star rating on apply rates according to equation (2.3.1), which pools across all thresholds and includes firm f, job title j, metropolitan area m, day-of-posting t, and #.#5-threshold c fixed effects.

$$ApplyRate_{fjmt} = \beta \mathbb{1}\{R_{ft} >= c\}_{fjmt} + \lambda_f + \lambda_j + \lambda_m + \lambda_t + \lambda_c + \varepsilon_{fjmt} \qquad (2.3.1)$$

Theory and evidence in Luca (2016) and Benson et al. (2020a) suggest that effects may differ depending on how well-known the employer is to workers from off-site sources. To proxy for this aspect of firms, we measure the posting firms' number of prior reviews. Across job postings, we create percentiles based on the posting firm's number of prior volunteer ratings submitted up to the time when the posting was first listed on the website. We then implement equation (2.3.1) first

[52] Around each #.# $= 1.0, 1.1, 1.2...4.9$, latent values in the [#.#4,#.#5) interval round down to #.# and those in [#.#5,#.#6] round up to (#.#)+0.1. We refer to a generic such threshold as #.#5.

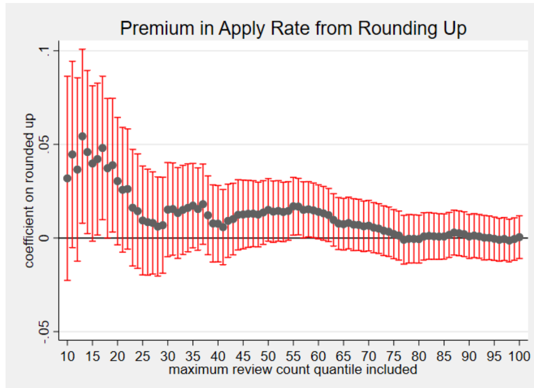
for postings listed by firms with few reviews on the website, and then iteratively incorporate more-reviewed firms.

The left-most estimate in Figure C.2 restricts the sample only to postings in the bottom decile of prior reviews. Here, we see a positive effect of a 0.1 higher star rating on apply rates. Among firms with relatively-few prior reviews, a 0.1 higher star rating—induced by rounding—raises apply rates by about 0.03–0.05 applies per 100 impressions. Relative to the sample average of 0.76 applies per 100 impressions (Table B.1), this constitutes a 4–7% increase. As the sample expands to include postings from firms that had more prior reviews, the estimated effect of a 0.1 higher rating falls towards null. On-site ratings appear to matter most for firms that are least well-known otherwise, extending the Benson et al. (2020a) result from labor supply in an online gig market to the broader U.S. labor market.[53]

We assess the robustness of this result by considering three alternative approaches, the results of which are displayed in Figure B.4. First, we reverse the order in which review count quantiles are iteratively added to the sample, starting instead with the top decile of prior reviews. As shown in panel (a), we observe no effect from rounding discontinuity on apply rates for larger, well-known firms, with the effect turning positive as the smallest quantiles by prior review count are incorporated. Second, we expand the bandwidth around each cutoff from ± 0.01

[53]This analysis focuses only on how the rating affects labor supply via the channel of jobseeker choice conditional on seeing a posting listed in their search results. If a higher employer rating also makes it more likely that a jobseeker will see the posting summary or see it earlier on the page (Nosko and Tadelis, 2015), this would only add force to the result.

Figure 2.2: Effect of 0.1-Star Higher Rating on Apply Rate at Rounding Threshold, Robustness to Including Postings by More Well-Known Firms



Notes: Sample is restricted to job postings for which the employers' weekly rating's hundredths places falls within ±0.01 bandwidth of the #.#5 cutoff. Numbers on the x-axis refer to the maximum prior review count percentile included in the regression sample. Each job posting is weighted by its impression total. Regressions include employer, job title, metro, day-of-posting, and #.#5 threshold fixed effects. Vertical red bars indicate a 95% confidence interval around each point estimate. Standard errors clustered by firm.

to ±0.02, and find similar results in panel (b). Third, following the framework of Schmieder et al. (2012) and Luca (2016), we incorporate a flexible function in our running variable—the difference between employer rating and the nearest cutoff—on each side of the cutoff, specifically a piece-wise linear function. Incorporating linear slope parameters before and after the cutoff and expanding the bandwidth to ±0.04 (to facilitate estimating now three parameters), we find the same pattern in panel (c). Across these alternative specifications, rating increases labor supply for lesser-known firms only.

Last, we consider whether the effect of a 0.1 higher average rating on apply rates is constant across the range of employer ratings, or whether the effect varies across the range of values. While the estimates presented above pool across all cutoff values, here we allow estimates to differ across the spectrum of employer quality.

As we did with the employer review count at the time of posting, we bin job postings into percentiles based on the firm's overall rating displayed to jobseekers at the time of posting. Given the result above that ratings affect labor supply only for less well-known firms, we focus this analysis only on the subsample of job openings whose firms were in the bottom quintile of prior review count. We begin with a subsample including only postings from the lowest-rated employers, and iteratively add job postings from higher-rated ones to the sample. Figure B.6 displays the results.[54] The labor supply boost we find from pooling across thresholds is driven by positive impacts on labor supply for ratings throughout the bottom and middle, but not the top, of the ratings scale. This pattern illustrates that it is lesser-known, low-road and middle-of-the-road employers that benefit vis-a-vis increased labor supply from improved reputation.

2.3.2 How do jobseekers value marginal information supplied? Equally across the range of employer ratings or more-strongly for negative information?

We focus on whether a review's positive or negative content relates to jobseekers' perceptions of the review's helpfulness. Our analysis rests on the idea that, when jobseekers see a volunteer's evaluation of her job, they are more likely to classify

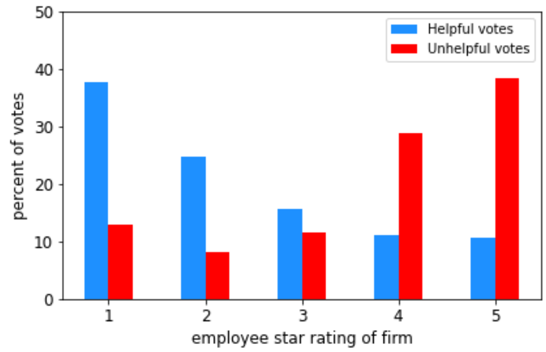
[54]Though outside the purview of this work, we note that this pattern is consistent with jobseekers using a reservation-cutoff strategy, whereby they are indifferent to employer quality insofar as it satisfies a minimal level. Of course, it could also be that jobseekers discount ratings that appear unreasonably high.

the review as helpful if it contains the kinds of information about which they want to learn and are more likely to classify it as unhelpful if it does not. Conceptually, we would expect helpful reviews are those that shift one's posterior belief about the value of a potential job at a firm either by shifting the mean belief up or down or by reducing uncertainty. Unhelpful reviews do not.

Jobseekers find reviews containing negative information about employers unequivocally more helpful than those containing positive information. Straightforward evidence comes from comparing the distributions of helpful and unhelpful votes by the overall rating value attached to each review (Figure 2.3). Not only are helpful votes predominantly concentrated among the lowest two rating options (one or two stars), but unhelpful votes—a clear indication of relative informational value—are predominantly concentrated among the highest two rating options (four or five stars). Reviews where the volunteer gave the firm a one-star rating account for thirty-eight percent of helpful votes but only thirteen percent of unhelpful votes. For the other extreme (five-star ratings), the vote shares flip. And although reviews with less extreme values (two, three, or four stars) may be less biased, they are each deemed less helpful than the most negative (one star) reviews.

The value of information, in particular negative information, might reasonably depend upon the firm in question. For example, information may be less in demand for more-established firms with well-understood, off-line reputations or for firms that already have more-established, on-line reputations with many reviews

Figure 2.3: Marginal Distributions of Jobseeker (Un)Helpful Votes by Review Rating



Notes: The figure above shows the distribution of all helpful votes and unhelpful votes submitted by users of the website for employer reviews submitted before 2015 (see Section B.3), where reviews are partitioned according to the employer rating.

on Glassdoor. With this in mind, we examine whether the value of positive and negative information to jobseekers changes across firm characteristics. Although the magnitudes vary, in every type of firm observed, jobseekers rate more positive reviews as less helpful than other reviews. Negative information about the firm is most sought after by jobseekers regardless of how long the firm has been in operation, how highly rated the firm was, how many prior reviews the firm had, or how much the firm pays its workers compared with other firms (see Figure B.7).[55] The ranking of helpfulness across review ratings is qualitatively highly stable. The average helpful vote share for one- and two-star reviews consistently hovers around ninety-five percent, whereas average helpful vote share for four- and five-star reviews is consistently below eighty percent.

[55]Firm-specific pay premia are estimated using an AKM-style approach (Abowd et al., 1999) with two-way fixed effects for firm and due to the thin panel of repeat observations for workers, job title in lieu of worker fixed effects. The adjusted R^2 from this regression is 0.86. For detailed analysis of Glassdoor pay data and for a comparison to other income surveys, see Liu et al. (2019); Sockin and Sockin (2019a,b).

Additional evidence comes from a regression of a review's helpful share on the review's star rating of the firm, controlling for other factors that might influence helpfulness. In particular, we control for firm fixed effects; year-month fixed effects; and measures of the nature of the review text, including the length of the review, the sophistication of its language, and the subjectivity of its tone.[56] Reviews communicating a one-star-higher rating were classified as helpful by a smaller share of voting jobseekers, resulting in an 8.5 percentage points lower helpful share (Table 2.1: Column 1). This result is robust to predicting a review's helpful share with alternative measures for the volunteer's evaluation of her job in lieu of the employer rating. One such alternative is whether the volunteer would recommend the employer to a friend (59.6 percent would per Table B.2). On average, the helpful share of would-recommend reviews is 22.8 percentage points lower than that of would-not-recommend reviews (Column 2). Reviews where the volunteer approves of the CEO witness a 17.3-percentage-points-lower helpful share (Column 3), while those that report a positive business outlook for the firm see a 17.7-percentage-points-lower helpful share (Column 4). An additional alternative—which is also a more continuous measure of a review's positivity—is the share of text characters spent discussing pros of the job, rather than cons. The pro share of review text (which averages 46.8 percent) gauges the volunteer's overall evaluation of her job, avoiding the coarseness

[56] Although not the main takeaway from Table 2.1, the helpful share regressions indicate that longer, better-written, and more objective reviews are viewed as more helpful by jobseekers. While the quality and subjectivity of the text have relatively muted associations, doubling the length of a review is associated with a helpful share that is about 4 percentage points higher, conditional on the volunteer's evaluation of the firm.

of the discrete five-star rating system. Reviews with a 10-percentage-points-higher pro share of review text experience on average a 3.8-percentage-points-lower helpful share (Column 5). Finally, we apply sentiment analysis to the review text and measure the positive versus negative emotional polarity of the text and similarly find that more-negative sentiment improves helpful share (Column 6). When we include all of these measures together, they all negatively predict helpfulness conditional on one another: Evidently, this finding is extremely robust.[57]

Next, we consider whether there is heterogeneity in demand for negative information depending on the voting jobseeker's satisfaction with her own job. We split the sample between jobseekers who expressed dissatisfaction with their own employer (one or two stars), moderation (three or four stars), or satisfaction (five stars). This requires us to restrict the analysis to a subsample of (un)helpful votes where the jobseeker reviewed her own employer. To try to clarify the influence of potential insincere voting by "jobseekers," we further split each subsample between jobseekers voting on the helpfulness of a review who: (a) do not work at the reviewed firm and are, therefore, presumably more likely to be engaged in genuine job search, or (b) do work at the reviewed firm, and therefore, may be more likely to be engaged in insincere sock puppetry. This partitions observations into six (un)helpful voter subsamples, by the voter's own expressed satisfaction at her own employer

[57] An alternative specification—where the count of helpful votes is predicted rather than the helpful share—reaffirms this robust finding that negative information is most helpful. Results from these count-helpful regressions are displayed in Table B.7. This sample of reviews is not restricted to the pre-2015 period.

Table 2.1: Predicting Helpfulness of Volunteer's Review

	Share of review votes helpful						
Star rating	-0.085***						-0.044***
	(0.001)						(0.001)
Would recommend employer to friend		-0.228***					-0.075***
		(0.004)					(0.003)
Approves of the CEO			-0.173***				-0.028***
			(0.003)				(0.002)
Positive business outlook for the firm				-0.177***			-0.041***
				(0.003)			(0.002)
Pro share of review text					-0.383***		-0.039***
					(0.006)		(0.004)
Polarity of text						-0.165***	-0.017***
						(0.003)	(0.003)
Log character length of review	0.044***	0.043***	0.057***	0.049***	0.040***	0.062***	0.034***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Flesch-Kincaid reading grade	0.002***	0.001***	0.002***	0.001***	0.002***	0.001***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Subjectivity of text	-0.015***	-0.020***	-0.029***	-0.020***	-0.021***	0.007**	-0.008**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
Employer, year-month FE	✓	✓	✓	✓	✓	✓	✓
N	530502	458916	407704	355688	530502	530502	291785
Adjusted R^2	0.26	0.26	0.20	0.21	0.19	0.16	0.28

Notes: The dependent variable, share helpful votes, is defined as the ratio of helpful votes to the sum of helpful and unhelpful votes. Sample is restricted to reviews submitted before 2015 (see Section B.3) as well as reviews for which the Flesch Kincaid reading grade is non-negative and no greater than 20. Polarity and subjectivity of each review are measured through natural language processing using the *TextBlob* library in Python. Standard errors clustered at the employer level. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively. For a summary of the dataset, see Table B.2.

crossed by whether or not her employer is the review's firm on which she is voting.

In each subsample, we predict whether a jobseeker votes the review helpful rather than unhelpful as a function of the review's overall rating, to capture if jobseekers show a stronger relative demand for negative versus positive information (Table 2.2). For jobseekers voting on reviews of firms where they do not work, demand for negative information is very similar regardless of their own job satisfaction. Among those who rated their own firm as one- or two-stars, reviews with one-star higher overall rating by the volunteer are 7.9 percent less likely to be voted helpful by the jobseeker. Among those who rated their own firm as four- or five-stars, the analogous estimate is 7.8 percent less likely.

Table 2.2: Conditional Probability of Helpful Vote by Voter Rating and Firm Coincidence

	Voter's rating of employer from own review					
	Review's firm ≠ Voter's firm			Review's firm = Voter's firm		
	1–2 stars	3–4 stars	5 stars	1–2 stars	3–4 stars	5 stars
Review's rating of employer	-0.079***	-0.063***	-0.078***	-0.181***	-0.057***	0.123***
	(0.005)	(0.008)	(0.011)	(0.005)	(0.009)	(0.038)
N	77990	24537	9509	51888	8155	5515
Adjusted R^2	0.42	0.43	0.37	0.51	0.28	0.42

Notes: Sample consists of a panel of (un)helpful votes for different employer reviews. The dependent variable is a dummy variable that the user up-voted the review helpful. Because the dataset consists only of helpful and unhelpful votes—meaning it excludes decisions where no vote was given—this dummy is conditional on submitting a vote. Sample is restricted to voting users who submitted at least one of their own employer reviews on the website prior to submitting the (un)helpful vote. Each regression includes fixed effects for the reviewed employer. Standard errors are clustered by voter. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively. For a summary of the dataset, see Table B.2.

In contrast, among jobseekers who work at the reviewed firm, there's a sharp difference in voting behavior depending on own job satisfaction. Those who express dissatisfaction with their own job are more likely to vote negative reviews of their firm as helpful. However, those who express satisfaction with their own firm are instead more likely to vote positive reviews as helpful. This is the only exception to the pattern of more negative information being more demanded. This could be due to sock puppetry, a boss rewarding employees for supplying positive reviews of the firm and upvoting other positive reviews of the firm. It could also be sincere; I like my firm and think reviews by others who like it will be most helpful to jobseekers.

2.3.3 How do risks of employer retaliation affect the supply of inside information from potential volunteers?

When workers decide whether to supply negative feedback, they must weigh the benefits and costs of providing such information. An altruistic volunteer will value the prospect of helping other jobseekers make more informed employment decisions, but a negative review also creates some risk that the employer retaliates against the volunteer personally.[58]

Externalities from a volunteer's review on firms are mixed, depending on if the information will tend to increase or decrease labor supply to the firm. This gives firms incentives to intervene in the information supply process. The employer that would be hurt by a negative review has a concentrated interest in the worker not supplying it and an incentive to discourage its supply, whereas the set of workers and competitors with any interest in the information being revealed each have only a minor interest, thereby making it difficult to coordinate (Olson, 2009). Workers with negative, but not positive, information about an employer have little incentive to volunteer it and some incentives to withhold it.

There is anecdotal and survey evidence that workers risk employer retaliation

[58]Appendix Section B.2 offers a model to illustrate how we see the firm's, insider's, and jobseeker's interactions fitting together. We wrote this down after obtaining the estimates but it aims to help readers understand the logic we had in mind when designing the analysis.

when sharing negative information about working conditions.[59] Women defying pressure and retaliation threats from their bosses to share information about sexual harassment at work gave rise to the #MeToo movement. Firms use broad interpretations of nondisclosure agreements (NDAs) and nondisparagement clauses to threaten lawsuits to prevent workers from sharing negative, private information about jobs there, and over one-third of U.S. workers report being bound by NDAs (Starr et al., 2019).[60] According to Lobel (2019), "NDAs regularly include information beyond traditionally defined secrets under trade secrecy laws, including general know-how, skills, client lists, and salary information. They also include provisions prohibiting the employee from disparaging the company." Many companies have sued Glassdoor seeking the identities of workers who anonymously reviewed them, with many companies claiming that the reviews violated former employees' nondisclosure clauses (Kidwai, 2020). Employers' interest in suppressing negative information and willingness to retaliate against employees who share it provide the

[59] Eidelson (2020) writes, "In the past few months, U.S. businesses have been on a silencing spree. Hundreds of U.S. employers across a wide range of industries have told workers not to share information about COVID-19 cases or even raise concerns about the virus, or have retaliated against workers for doing those things, according to workplace complaints filed with the National Labor Relations Board (NLRB) and the Occupational Safety and Health Administration (OSHA)."

[60] Silver-Greenberg and Kitroeff (2020) say, "Employees who are fired or resign in frustration are often pushed to sign contracts that prohibit them from in any way disparaging the company, several of the former employees said in interviews. Those pacts bar the employees from even acknowledging the existence of the agreements.... The Times spoke to 13 former Bloomberg employees... who said they wanted to be released from their exit agreements so that they could speak openly about the culture at the company... If they were free to talk, some of the former employees said, they would describe a company that, while it provides generous pay and benefits, can be an uncomfortable place to work, especially for women." Benner (2017) says, "Nondisparagement clauses are not limited to legal settlements. They are increasingly found in standard employment contracts in many industries, sometimes in a simple offer letter that helps to create a blanket of silence around a company."

rationale for many whistle-blower protections laws and procedures (Weil and Pyles, 2005; U.S. Occupational Safety and Health Administration, 2017). Systematic evidence about the chilling role of retaliation fears comes from a recent study showing that stronger protection of workers who blow the whistle on rights violations by employers increases the willingness of workers to report such violations (Johnson et al., 2020). Cortina and Magley (2003) found in a survey of public-sector employees, where incentives for managers to retaliate may be weaker than in the private sector, that only 27 percent of respondents who experienced some recent interpersonal mistreatment in the workplace voiced concern over their mistreatment. Among those who did, 66 percent reported being the subject of work-related or antisocial retaliatory behavior. Workers who report workplace violations might face retaliation that lowers their income or sours their job satisfaction vis-a-vis, e.g., reduced pay, fewer hours, and task reassignment.[61] The consequences though can be even more dire, resulting in more extreme responses such as deportation.[62]

If the worker can more easily blend into the crowd of co-workers, this reduces expected costs because the probability of identification and subsequent retaliation falls. Qualitative evidence suggests that the fear of being discovered does alter

[61] Covert (2020) discusses retaliation against McDonald's workers reporting sexual harassment, "Instead of the harassers facing discipline, punishment was often meted out to the victims...assigned difficult or uncomfortable tasks—working the grill all day or being stuck at the drive-through window for an entire shift. She was also disciplined for minor infractions, had her hours cut, was demoted, and got suspended for two weeks. She was eventually fired." This is an awful example of workers expressing dissatisfaction with workplace conditions and bearing the negative consequences from their current employer for doing so.

[62] For example, when undocumented workers in Minnesota complained to their employer about working conditions and said they would complain to others, the employer reported them to U.S. Immigration & Customs Enforcement, which deported them (Walsh, 2018; Chen, 2018).

workers' disclosure patterns: A Fractl (2019) survey of 1,096 workers conducted who left online employer reviews revealed that one-third of those sampled who waited some time to leave their review did so because they did not want to be identified by their former employers. On Glassdoor, a volunteer can conceal aspects of their identity—their job title, location, or both—when leaving the review, making it harder for the employer to potentially infer who wrote the review. About fifty-three percent of volunteers conceal at least one of these two aspects.

Volunteers leaving more negative reviews are more likely than other volunteers to conceal aspects of their identity. This holds across volunteers for the full sample of reviews (Table 2.3: Column 1) and the pre-2015 sample of reviews used for the share helpful analysis (Column 2). Reviews by volunteers who conceal some aspect of their identity tend to rate their employers on average 0.075–0.091 stars lower overall. One concern is that volunteers who tend to be more negative in their employer evaluations may also be more anxious about retaliation and more prone to identity concealment. However, this relationship is not driven by individual differences across volunteers in their tendency to review negatively and to withhold identifying information. Among those who volunteer multiple reviews, the reviews for which they choose to conceal an aspect of their identity are associated with 0.087 stars lower average rating (Column 3).[63]

If risk of retaliation contributes to concealment behavior, it would be less com-

[63] The same volunteer leaving multiple reviews when logged into their Glassdoor account each time can be linked across reviews by Glassdoor and by the researchers. However, a jobseeker cannot identify multiple reviews left by the same volunteer.

Table 2.3: Overall Employer Rating, Retaliation Risk Measures, and Review Helpfulness by Identity Concealment

	Star rating			$\mathbb{1}$\{Current employee\}	Log employment	Share of review votes helpful		
Conceals aspect of identity	-0.075***	-0.091***	-0.087***	0.059***	-0.454***	-0.002*	-0.012***	-0.018***
	(0.003)	(0.003)	(0.014)	(0.006)	(0.025)	(0.001)	(0.001)	(0.001)
Star rating							-0.078***	-0.088***
							(0.002)	(0.002)
Sample	Full	pre-2015	pre-2015	pre-2015	pre-2015	pre-2015	pre-2015	pre-2015
Mean dependent variable	3.34	3.17	2.83	0.540	8.615	0.86	0.86	0.86
Share conceal	0.53	0.39	0.35	0.35	0.35	0.40	0.40	0.40
Employer FE	✓	✓	✓	✓				✓
Volunteer FE			✓	✓	✓			
N	6308865	1268132	101361	101361	101361	657691	657691	657691
Adjusted R^2	0.14	0.16	0.45	0.21	0.49	0.04	0.16	0.24

Notes: When submitting a review, users are asked to provide their job title and location, but can leave them blank. Respondents can also report an anonymized job title, i.e., job titles including the word "anonymous" or ending in "employee." Firm employment is based on a fixed employer lookup table and does not vary over time. Each regression includes year-month fixed effects. Standard errors are clustered at the employer level. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively. For a summary of the data, see Table B.2.

mon in contexts where this risk is less of a concern. What kinds of workers should worry less? First, former employees should worry less than current employees. Retaliation threats that apply to former employees (e.g., the prospect of a bad reference in the future) also apply to current ones but current employees face additional risks (e.g., undesirable schedule or task assignments, demotion, cuts to pay and hours, personal harassment, or firing).[64] Second, workers in firms with more employees might worry less about the boss inferring their identity, because they can blend into a larger crowd. Consistent with these predictions, among volunteers who leave multiple reviews, respondents are more likely to conceal an aspect of their identity if they are currently an employee and when they are reporting on a smaller firm

[64] According to Weil and Pyles (2005), "Public law groups and other organizations representing low-wage workers note that many employee complaints... are filed after a worker has been fired by an employer, often for other causes (thereby lowering the cost of complaining at that point)."

(Columns 4 and 5, respectively).[65] Volunteers are 5.9 percentage points more likely to conceal an aspect of their identity when reporting on a firm that currently, rather than formerly, employs them and firms where they conceal aspects of their identity are 36.5 percent (45.4 log points) smaller on average than firms where the job title and location are revealed. For these specifications, we incorporate volunteer fixed effects, meaning that our estimates are identified off of the same worker reviewing as both a current and former employee, and who left multiple reviews for firms of different sizes, respectively.

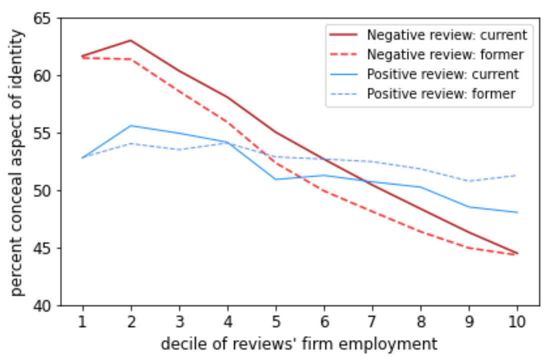
Finally, when volunteers conceal aspects of their identity, it may degrade the value of the information they supply, since doing so limits the ability of jobseekers to judge the relevance of the informational content to their own decisions. For instance, a jobseeker deciding whether to apply to a particular job posting may derive the greatest value from reviews by volunteers with the same job title and in the same location as the posting. To test this, we predict each review's helpful share with whether the volunteer concealed either aspect of their identity. Indeed, jobseekers tend to classify reviews from volunteers who concealed their identity as less helpful conditional on the reviewed firm, the review's year-month, and the volunteer's overall rating of the firm (Column 8). Jobseekers classify reviews for which either aspect of volunteer identity is concealed as 1.8 percentage points less helpful on average than non-concealed reviews with the same star rating for the

[65] In terms of the model, current employees face higher costs of retaliation (c_R). If a firm tries to uncover the reporting insider's identity among all possible insiders to retaliate, smaller firms will have a higher probability of success (ρ).

same firm.

Beyond a general desire for anonymity, if fear of retaliation works to suppress the supply of negative information, then we would anticipate workers facing greater retaliation risks to seek anonymity more when relaying negative information than when relaying positive information. Figure 2.4 presents flexible evidence that this is in fact the case. We stratify the sample by measures of retaliation risk—whether the volunteer was a current or former employee and the size of the firm reviewed. Reviews are grouped into decile based on firm size with reviews of larger firms further right on the horizontal axis. Reviews by current employees are represented by solid lines and former employees by dashed lines. Those with negative ratings (one- and two-stars) are represented by thicker, red lines and those with positive ratings (four- and five-stars) by thinner, blue lines.

Figure 2.4: Share of Volunteers Concealing Aspects of Their Identity by Their Rating of Firm, Employment Status, and Firm Size Decile



Notes: The figures above detail the rate at which volunteers conceal potentially identifying information depending upon the size of the employer and whether the volunteer is still currently employed at the firm. The sample of positive reviews reflects four- and five-star reviews, while the sample of negative reviews reflects one- and two-star reviews. Sample of volunteers is restricted to those who leave multiple reviews on the website. Firm size deciles are defined across reviews.

First, we observe a markedly steeper slope in concealment with respect to the size of the firm for negative reviews than for positive reviews. When the review is negative, employees in the smallest two deciles by firm size conceal at rates that are 17–18 percentage points above that of employees in the highest decile. For positive reviews, the slope in employer size is negative but noticeably flatter. Second, across the spectrum of employer size, when supplying negative information, current employees are consistently more likely to conceal aspects of their identity, consistent with current employees facing a greater risk than former employees. When supplying positive information, the same relation is not observed. For larger firms, former employees are more likely to conceal than current ones. Taken together, we observe that if retaliation risks affect employee disclosure, such risks appear most relevant when supplying negative content, consistent with expectations.

To formally test whether retaliation risks are borne out among workers when supplying negative rather than positive information, we predict concealment with an interaction between the volunteer's rating of the firm and the two measures of retaliation risk—current employee status and firm size. Let $\mathbb{1}(Conceal)_{ir}$ indicate if volunteer-i conceals an identifying aspect of their identity in review-r. We consider the following linear probability model,

$$
\begin{aligned}
\mathbb{1}(Conceal)_{ir} =& \beta_1 Rating_{ir} + \beta_2 log(FirmSize)_{ir} + \beta_3 \mathbb{1}(CurrentEmployee)_{ir} \\
& + \mathbb{1}(Rating_{ir} \leq 2) \times [\beta_4 log(FirmSize)_{ir} + \beta_5^a \mathbb{1}(CurrentEmployee)_{ir}] \\
& + \delta_{ir}^{Industry} + \delta_{ir}^{Year-Month} + \gamma_i + \epsilon_{ir}.
\end{aligned}
\tag{2.3.2}
$$

If fear of retaliation drives identity concealment—and thus likely works to suppress negative feedback more broadly—we should observe that reviews of one or two stars are more likely to have identifying aspects concealed among volunteers facing higher retaliation risk. Our hypothesis then is that a worker's concealment probability is higher when leaving a negative review and being either at a smaller firm ($\beta_4 < 0$) or a current employee ($\beta_5 > 0$). Estimates of this model both across and within volunteers are displayed in Table B.3 and are consistent with the predicted signs. Figure B.8 presents more flexible evidence that allows for but does not find meaningful non-linearities in the key relationships.[66]

The evidence presented so far looks at variation in measures of retaliation risk

[66]Panel (a) considers how the difference in the probability of concealment when leaving a negative rather than a positive review relates to firm size. Panel (b) considers how the difference relates to current employee status. The two provide a visual representation of the semi-parametric relationship expressed in β_4 and β_5, respectively. The relationships appear reasonably linear and robust. In the case of firm size, the negative slope is more than twice as steep for negative reviews than positive reviews, pointing to systematically higher rates of concealment when supplying negative information at smaller firms than at larger firms. A negative slope is observed for positive reviews as well, possibly reflecting a widespread desire for anonymity. In the case of current employee status, concealment is higher in the context of negative reviews and higher retaliation risk, as an appreciably steeper positive slope is observed among negative reviews.

specific to and reported by the worker who is deciding on a rating to leave and whether to conceal identity. Next, we focus on a more-remote source of variation in retaliation risks. We assume that workers in weaker labor markets—those with higher unemployment rates—face a higher expected cost of retaliation and study whether relationships strengthen in this case. Low unemployment rates means more robust employment alternatives. Higher unemployment rates mean workers have worse outside options and, thus, a higher expected cost of retaliation. In sum, we expect worker demand for anonymity to be counter-cyclical.

To test this, for volunteers who report their location of employment, we assign their review the unemployment rate in that state for the year-month in which the review was written. Although these employees have revealed their location—foregoing one layer of anonymity—the workers can still choose whether or not to conceal their job title. Above we highlighted two contexts in which workers may fear retaliation risk more and thus have heightened desire for anonymity: being a current employee and working for a smaller firm. This theory predicts that, as the unemployment rate rises, current employees and workers from smaller firms will increasingly conceal their identity. The model we estimate is,

$$\begin{aligned}\mathbb{1}(ConcealJobTitle)_{fsti} =& \beta_1 Rating_{fsti} + \beta_2 UR_{st} + \gamma_i + \gamma_{\iota(f)} + \gamma_s + \gamma_t \\ & + \beta_3 \mathbb{1}(CurrentEmp)_{fsti} + \beta_4 UR_{st} \times \mathbb{1}(CurrentEmp)_{fsti} \\ & + \beta_5 log(FirmSize)_f + \beta_6 UR_{st} \times log(FirmSize)_f + \epsilon_{fsti}\end{aligned} \tag{2.3.3}$$

where fixed effects are included for volunteer i, industry $\iota(f)$ for firm-f, state s, and year-month t. If retaliation risk contributes to concealment, then we would expect $\beta_4 > 0$ and $\beta_6 < 0$. Columns 4 and 6 of Table 2.4, which correspond respectively to β_4 and β_6, reveal that these relations do arise and are statistically significant. For current employees—who risk being fired and having to search for new employment opportunities—a five-percentage-point rise in the local unemployment rate is associated with a three-percentage-point increase in concealment. Given that the average rate of job title concealment among reviews for which location is available is about twenty percent, an increase of three percentage points would constitute a roughly 15 percent increase in concealment, suggesting that retaliation risk plays a non-trivial role in the supply of reviews.

The evidence presented so far establishes a reliable association between volunteers concealing aspects of their identity and their reviews being more negative in ways consistent with concerns about retaliation risk, which in turn suppresses helpful information. Is this relation causal? Consider an ideal experiment. If one

Table 2.4: Labor Market Tightness and Identity Concealment

	Share conceal	Star rating	Unemployment rate (UR)	$\mathbb{1}$\{Current employee\}		Log employment	
				Alone	× UR	Alone	× UR
Conceals job title	0.199	-0.017***	-0.001	0.032***	0.006***	-0.014***	-0.001***
		(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)

Notes: The table above presents estimates of how identity concealment varies with labor market tightness along two dimensions of retaliation risk—current employee status and working for a smaller firm. Estimates are from a single regression which includes volunteer, industry, state, and year-month fixed effects. Sample size is 508,004 reviews. The unemployment rate (UR) is the monthly-state rate, in percentage points, from the Federal Reserve Economic Database (U.S. Bureau of Labor Statistics, Unemployment Rate in [State], retrieved from FRED, Federal Reserve Bank of St. Louis). Standard errors are clustered by state. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively.

population of potential reviewers were randomly assigned the ability to conceal identifying aspects and another was not, how would the ability to conceal identifying aspects change what information is volunteered? The phenomenon described above implies that, reviews volunteered by those randomly assigned an ability to conceal would differ in the following ways: 1) more negative ratings on average, 2) a higher share of reviews from workers in higher risk contexts — from current employees and firms that are smaller on average, and 3) conditional on the rating, jobseekers rate reviews as less helpful on average due to the greater prevalence of identity concealment and degraded ability to judge relevance.

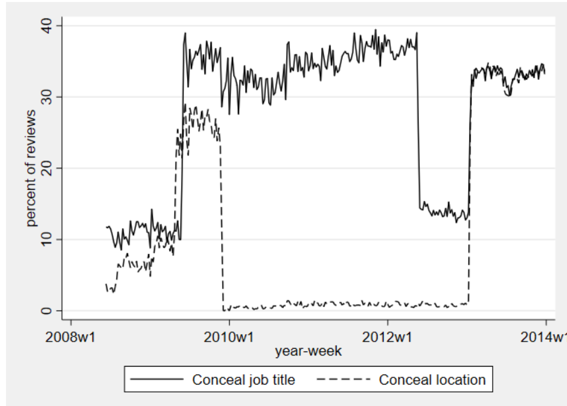
To approach this, we harness large, discontinuous, persistent changes in the proportion of volunteers whom conceal aspects of their identity over time. There have been four such discontinuities in concealment rates since Glassdoor started. We believe these are due to changes in Glassdoor user interfaces, as they changed the logic of required fields on the forms volunteers use to review employers. Figure 2.5

plots the trends in concealment rates for job title and location in each calendar week since Glassdoor started recording employer reviews through the end of 2013. There were no such discontinuities after 2013 (Figure B.9). In the eighteenth and twenty-second weeks of 2009, rates of location and job title concealment, respectively, jumped sharply. In the forty-eighth week of 2009, rates of location concealment collapsed to zero. In the twenty-first week of 2012, rates of job title concealment dropped roughly twenty percentage points between weeks. And in the third week of 2013, rates of job title and location concealment both jumped drastically. Given the sharpness in and persistence following the change in concealment rates around these dates, we interpret these events as structural changes that are exogenous shifters in the ability for volunteers to conceal.[67]

To exploit this quasi-experimental variation, our baseline estimates restrict attention to the sample of reviews volunteered within four weeks of a policy change and assume that, there was not a shift in the flow of potential reviews from another source within this time window. We define two instruments to represent shifts in the ability to conceal each identifying aspect, which take different values across a change date. The first is an indicator for job title concealment becoming more available

[67]We cannot say for certain what the exact causes were for each of these shifts. As location concealment falls to near-zero rates, we suspect that the option to leave location blank was entirely removed during this period, with the modicum of concealment during this period perhaps attributable to user error where entries cannot be matched to a real location or to A/B testing. For job titles, concealment rates collapse but not to zero, per conversations with Glassdoor staff, this likely reflects a change in the availability of concealment for users on some platform (e.g. web via computer) but not another (e.g. iPhone app). Nevertheless, as long as the changes are unanticipated by users to the site and do not influence outcomes via other paths besides concealment ability, the sudden, persistent changes would suffice as valid instruments for concealment.

Figure 2.5: Trends in Rates of Job Title and Location Concealment



Notes: The figure plots the share of reviews that conceal job title or location in each calendar year-week. This range includes all exploitable discontinuous probability changes. Concealment trends after 2013 are available in Figure B.9, which spans from the twenty-fourth week of 2008 through the final week of 2018.

to respondents ($Z^{JobTitle}$), which takes the value one on the side of a policy change with a discontinuously higher job title concealment rate, zero on the lower side of such a change, and zero on both sides of a policy change that affected only location concealment, not job title concealment. The second instrument is for location concealment becoming more available ($Z^{Location}$) and is constructed analogously. Table B.4 provides detail about the definition of these instruments. These policy variables are instruments for a volunteer's decision to conceal on a given review, isolating the variation in concealment behavior driven by the policy changes. The IVs have a strong first stage in predicting actual rates at which volunteers conceal an identifying aspect (Table B.5). In this ±4 week bandwidth sample, we estimate both ordinary least square (OLS) and instrumental variable (IV) regressions of concealing any identifying aspect on average rating, whether the respondent is a current employee, the size of the firm being reviewed, and the helpfulness of the review. Table 2.5 summarizes the results.

These models do not include firm or volunteer fixed effects because the key variation is coming from the policy changes. When the option to conceal is more available to potential volunteers, different sets of volunteers may opt into leaving reviews, changing the set of firms reviewed. We want to capture the total effect, including both changes in information supplied within volunteer and firm who report always as well as changes in the composition of volunteers and firms depending on concealment availability.

Table 2.5: Overall Rating, Retaliation Risk Measures, and Review Helpfulness by Identity Concealment Driven by Exogenous Policy Shifts, OLS and IV Estimates

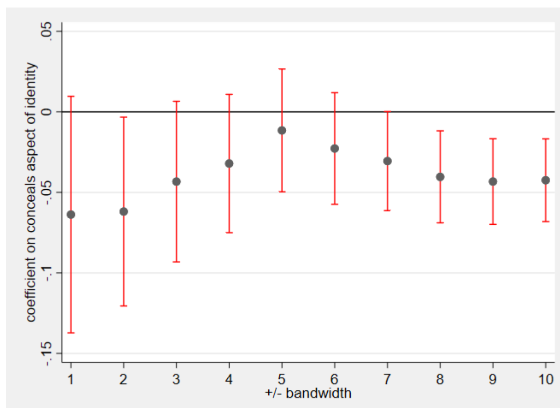
	Star rating		$\mathbb{1}${Current employee}		Log(employment)		Share of review votes helpful			
Conceals aspect of identity	-0.119***	-0.260***	0.023***	0.043**	-0.679***	-0.484***	0.010**	0.010	-0.005	-0.032
	(0.011)	(0.052)	(0.004)	(0.019)	(0.035)	(0.122)	(0.004)	(0.025)	(0.004)	(0.022)
Star rating									-0.104***	-0.105***
									(0.003)	(0.003)
Specification	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Dep. var. mean	3.15	3.15	0.57	0.57	8.56	8.56	0.81	0.81	0.81	0.81
Dep. var. std. dev.	1.31	1.31	0.49	0.49	2.82	2.82	0.35	0.35	0.35	0.35
N	77176	77176	77176	77176	71368	71368	39119	39119	39119	39119
Adjusted R^2	0.00	0.00	0.00	0.00	0.02	0.02	0.00	0.00	0.17	0.17
F statistic	35.86	18.74	35.54	30.08	124.41	58.03	3.25	2.25	227.18	220.12
Anderson-Rubin chi-sq		29.91		28.32		16.66		1.76		2.82

Notes: Sample is restricted to reviews that arrive within ±4 weeks of each large, sudden, persistent shift in the availability of identity concealment options. Each regression includes a linear time trend in year-week and a dummy variable for each of the three latter policy change periods: late-2009, mid-2012, and early-2013. Indicators for concealment of location or concealment of job title becoming more available (detailed in Table B.4) are used as instruments for IV specifications. Standard errors are clustered by firm. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively.

Consistent with the results from Table 2.3, the OLS results in the sample within four weeks of the discontinuities reveal similar associations with identity concealment: more-negative average ratings, increased likelihood of volunteers being a current employee or from a smaller firm, and slightly less helpful (but not statistically significant in this specification) to jobseekers conditional on rating. The IV finds qualitatively similar patterns. The option for volunteers to conceal identifying aspects causes reviews to be on average 0.260 stars lower, more than twice as large of an effect as the OLS estimate and equivalent to about one-fifth of a standard deviation. With regards to the probability volunteers are current employees, the IV estimate shows an even stronger relation, with the option to conceal causing a 4.3 percentage points increase. The IV estimate for firm size attenuates somewhat compared with the OLS estimate but remains robustly negative at –38.4 percent (–48.4 log points), confirming that the option to conceal induces reviews from volunteers at smaller firms.

Lastly, the OLS and IV estimates for review helpfulness unconditional with respect to review's rating suggest reviews with identity concealed are more helpful or about the same on average. However, this combines two countervailing channels – concealment draws in more negative information, which is helpful, but degrades its value by obscuring relevance, which is unhelpful. When conditioning on review rating as in the last two columns, more-negative reviews are estimated to be more helpful, with an average increase in helpfulness share of 10.5 percentage points for reviews with a one-star lower rating in the IV. On the other hand, identity concealment itself conditional on review rating appears to make reviews less helpful. The IV point estimate for review helpfulness, –3.2 percentage points, is similar in sign and order of magnitude to the estimates in Table 2.3, but is not statistically different from zero in this ±4-week bandwidth sample. We consider though alternative bandwidths for the number of weeks around each discontinuity to include in the sample. Reasonable arguments could be made for focusing on both narrower and wider bandwidths, consistent with the logic of a standard bias-variance tradeoff. The results from re-estimating the IV specification for review helpfulness conditional on the review rating with bandwidths ranging from ±1 to ±10 are displayed in Figure 2.6.

Figure 2.6: Effect of Concealment on Helpful Share, IV Estimates with Alternative Bandwidths



Notes: Sample is restricted to reviews that arrive within ± bandwidth weeks of each large, sudden, persistent shift in the availability of identity concealment options. Each regression includes a linear time trend in year-week and a dummy variable for each of the three latter discontinuity periods: late-2009, mid-2012, and early-2013. Each regression additionally controls for the review's star rating. Indicators for concealment of location or concealment of job title becoming more available (see Table B.4) are used as instruments. Vertical red bars indicate a 95% confidence interval around each point estimate. Standard errors are clustered by firm. The result for ±4 bandwidth is displayed in the final column of Table 2.5.

The effect appears negative and relatively stable at around 3–4 percentage points across the choice of bandwidth. While the estimate is not statistically significant for a bandwidth of ±4 weeks, it is stable in magnitude and significant for a smaller bandwidth (±2) and larger bandwidths (above ±7), offering evidence that identity concealment per se lowers the helpfulness of reviews conditional on rating.[68] Concealed reviews might be less helpful to workers because determining the relevance of the content to one's own employment opportunities becomes harder. If workplace attributes are specific to establishments and differ by the job—for instance the management and coworkers with whom one would interact may vary greatly even for the same firm—then inability to tie the review to a specific employment opportunity could render the information less helpful.

We also consider alternative instrument definitions that use the same discontinuities but aggregate them differently.[69] The first stage remains robust (Columns 1 and 3 of Table B.5) and second-stage estimates (Table B.6) change little with respect to the choice of instrument(s).

2.3.4 Why stronger demand for negative information?

Insiders will tend to under-supply negative reviews if they face retaliation risk.[70] The cost of retaliation — which depends on the probability of being identified as well as the magnitude of punishment if identified — creates a wedge whereby employees with bad experiences at the firm might strategically choose not to volunteer

[68]The analogous IV results for star rating, whether the volunteer is a current employee, and firm size change little when shrinking or widening the bandwidth (Figure B.10).

[69]The first alternative is to use only a single indicator for whether concealing either aspect becomes more available ($Z^{either} \equiv \max\{Z^{JobTitle}, Z^{Location}\}$). The second alternative is to use three indicators for concealing job title only, location only, or both becoming more available ($Z^{JobTitle} \setminus Z^{Location}$, $Z^{Location} \setminus Z^{JobTitle}$, and $Z^{JobTitle} \times Z^{Location}$).

[70]Section B.2 uses a game to describe how.

this information. Consequently, signals of employer reputation will be biased upwards, and especially so for employers with strong retaliatory stances. A firm can have a high rating because it is a high-road company where workers have good experiences or because it retaliates against its workforce to suppress negative ratings. To the jobseeker, these two are observationally equivalent, as only publicly disclosed information is available to them. Negative reviews run counter to this bias, and can move employer reputation closer to true quality. Much evidence is consistent with this as part of the explanation for jobseekers' finding negative reviews more helpful.

Other potential channels—jobseeker risk aversion, the tendency of employers to advertise positive information, a large supply of employer-induced positive reviews, and differences in the perceived truthfulness of negative content—would also push this way. This section discusses and offers some evidence on these mechanisms to build an understanding about the process. Given employer preferences for supplying positive information and worker risk aversion, it would be perhaps surprising if jobseekers expressed no preference for negative information from volunteers.

Worker risk aversion would contribute to the demand for negative information. Consider a risk-neutral jobseeker for whom each new review is an independent, mean-zero signal about the true quality of an employer and who so far has seen n ratings of the firm with average $\bar{R}_n$. Consider the $n+1^{th}$ signal: R_{n+1}. The value of this signal to the jobseeker does not depend on the particular value of R_{n+1} or the difference $R_{n+1} - \bar{R}_n$. A signal leads to an updating of the prior and increases belief precision. If it does this by confirming that the prior average was correct, this is as valuable as a signal that leads to an update up or down. On the other hand, if the jobseeker is risk averse, then the value of a signal that shifts her mean posterior belief down ϵ is more valuable than one that shifts it up by the same amount. Avoiding an ϵ-worse outcome is worth more than gaining an ϵ-better one. That workers tend to be risk averse seems uncontroversial and likely part of the explanation.

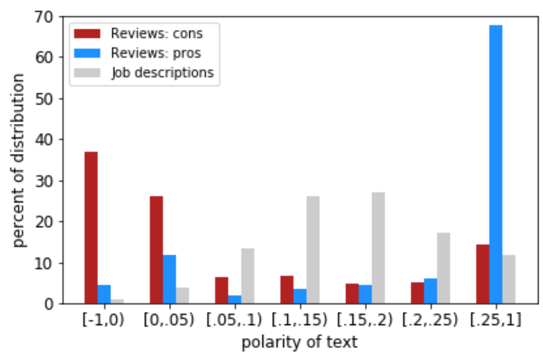
Second, jobseekers acquire information about jobs through communication with the hiring firm and it seems reasonable to suppose that the hiring firm supplies positive, rather than negative, information. To test this, we study a sample of 413,846 job postings on Glassdoor for which we observe the text of the job description associated with the posting.[71] These job descriptions can be interpreted as advertisements for the firm. As one might expect, firms write job postings full of positive language. Sentiment analysis of these descriptions confirms this. Job descriptions are less positive than the pros text of reviews but far more positive than the cons text of reviews (Figure 2.7). Almost no employer advertises with negatively-charged text, as less than one percent of descriptions are interpreted as relaying negative sentiment. It is in the firm's interest to supply positive, not negative, information.[72] Without a reputation system in place where jobseekers

[71] Glassdoor collects postings from three main sources—online job boards, applicant tracking systems, and company websites—and captures about 81 percent of U.S. job openings, as measured in the Job Openings and Labor Turnover Survey from the U.S. Bureau of Labor Statistics (Chamberlain and Zhao, 2019).

[72] Firms have incentives to supply information that horizontally differentiates them from com-

can obtain information about the firm from experienced sources, presumably the only employer-specific information non-referral workers would have access to would be job advertisements and public media coverage. The former, as evidenced by Figure 2.7, contains little if any negative content, while the latter would likely be limited in scope to only large, well-known employers.

Figure 2.7: Emotional Polarity of Job Postings and Reviews' Cons and Pros Sections



Notes: Polarity of the "Pros" section for employer reviews, "Cons" section for employer reviews, and job posting descriptions are measured through natural language processing using the *TextBlob* library in Python. The polarity measure ranges from -1 to 1, with more positive (negative) values reflecting more positively (negatively) charged text, and is partitioned into seven bins. The leftmost, center, and rightmost bars reflect the share of reviews within a polarity bin for the "Cons" field, "Pros" field, and job descriptions, respectively. Bars sum to 1 for each text category.

Finally, we generate evidence on the extent to which jobseeker preference for negative information is driven by employers inducing the supply of more positive information through sock puppetry, i.e., directly producing positive reviews under false identities or creating some kind of incentives for employees to supply them

petitors in workers' eyes. This can improve fit. They do not have incentives to supply information that vertically differentiates them in a negative way. The challenge for high-road firms is to credibly differentiate themselves from others. Institutions, including Glassdoor, may help communicate both kinds of information. Further, note that what distinguishes a dimension of horizontal versus vertical differentiation is primarily the correlation in workers' tastes. More highly correlated tastes imply greater vertical differentiation. Less correlated tastes imply greater horizontal differentiation.

using their own identities. This does not seem to be the main explanation. Prior research in consumer markets shows that firms have incentives to increase the supply of positive reviews about their own firm and the supply of negative reviews about their competitors (Mayzlin et al., 2014), as firms benefit from improved reputation in online [product] markets (Cabral and Hortaçsu, 2010) and if better ratings contributes to an improved search ranking on the platform (Nosko and Tadelis, 2015). In turn, firms may take it upon themselves to farm positive reviews on the website through their employees. Firms may incent their employees with monetary and non-monetary rewards, or engage in campaigns to promote reviews from current employees, especially those with positive sentiment towards the firm. The extent to which sock puppetry exists could impact our findings. On the supply side, if a large share of reviews are planted by employers, then the information may not reflect true qualities of the firm. Conversely, on the demand side, if jobseekers believe that a large share of positive reviews are employer-planted and thus disingenuous, then they may discount positive reviews on the website, shifting helpfulness of supplied information towards more negative reviews.

To explore these concerns, we attempt to identify reviews submitted disingenuously on the website, motivated by the investigational analysis done by The Wall Street Journal.[73] If employers launch a campaign or create some incentive for producing positive reviews, or if a deadline is approaching for an acknowledgement

[73]See Fuller and Winkler (2020) for a case study analysis of how employers farm positive reviews on the website, the incentives such firms have to engage in sock puppetry, and the mechanisms by which employers will get their employees to supply positive reviews.

from Glassdoor, then we would expect resulting reviews to arrive in abnormally large waves during this period relative to trend.[74]

Motivated by this, we measure surge months as those with extreme, positive growth rates in reviews submitted by firm, study how reviews during surge months tend to differ from a firm's reviews in other months, and whether the basic relationship of interest—jobseekers' demand for negative information—holds excluding surge months. For each firm f in each calendar year-month t, we calculate rates of change in review count relative to the three months before and after t:

$$g^B_{ft} = log(r_{ft}) - log(\frac{\sum_{-3\leq\tau\leq-1} r_{f\tau}}{3} + 1) \text{ and } g^A_{ft} = log(r_{ft}) - log(\frac{\sum_{1\leq\tau\leq 3} r_{f\tau}}{3} + 1)$$

where r_{ft} is the number of new reviews submitted by volunteers for firm f in year-month t. Together, the two growth rates detail the extent to which the number of reviews submitted in a given month deviates from trend. If these surge months capture some degree of sock puppetry, then we would anticipate reviews that are submitted during these months to be more positive on average than other months and disproportionately submitted by current employees who are more responsive to employer manipulation. Evidence presented in Columns 1–4 of Table 2.6 confirms

[74]For example, if an internal promotion starts at the beginning of April, we would expect reviews in April to be large relative to preceding months. Additionally, the impact of an incentive may dissipate after introduction as employees motivated to participate do so sooner rather than later. Consequently, we would expect the reviews in April to be large relative to the subsequent months as well. Alternatively, if there is a deadline for when Glassdoor considers firms for awards or acknowledgements, we would anticipate a wave of disingenuous reviews to be submitted leading up to that deadline.

that this is the case, and increasingly so as the degree of abnormality in review count used to define a surge month (increasingly high cutoff) is applied.

Table 2.6: Identifying Sock Puppetry

	Star rating		$\mathbb{1}$\{Current employee\}		Share of review votes helpful	
	100% cutoff	50% cutoff	100% cutoff	50% cutoff	100% cutoff	50% cutoff
$\mathbb{1}$\{Above monthly review growth cutoff\}	0.394***	0.166***	0.128***	0.047***		
	(0.005)	(0.003)	(0.002)	(0.001)		
Star rating					-0.088***	-0.093***
					(0.002)	(0.002)
Year-month FE	✓	✓	✓	✓	✓	✓
Employer FE	✓	✓	✓	✓	✓	✓
Share of reviews above cutoff	0.109	0.278	0.109	0.278	.	.
N	6606743	6606743	6606743	6606743	615358	470626
Adjusted R^2	0.15	0.14	0.07	0.07	0.23	0.24

Notes: Table above attempts to identify sock puppetry in the reviews data and investigates whether reviews identified as possible sock puppetry are driving the helpfulness of negative reviews. For each firm in each year-month, we calculate the log change in reviews relative to the three months prior (g^B_{ft}) and the three months after (g^A_{ft}). The x% percent cutoff refers to firm-year-months in which $g^B_{ft} \geq x\%$ and $g^A_{ft} \geq x\%$. For Columns 5–6, reviews that lie above the cutoff are excluded. Standard errors are clustered by firm. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively.

To illuminate the extent to which sock puppetry drives jobseeker demand for negative information, we re-estimate the specification for helpful share in Column 1 of Table 2.1 but removing potentially disingenuous reviews in an increasingly conservative fashion. The last two columns of Table 2.6 affirm that the helpfulness of negative information is not driven by the lack of helpfulness of employer-planted reviews.

Even outside of an orchestrated campaign to boost positive reviews, workers may be more honest or truthful when relaying negative content. To test whether perceived truthfulness drives the expressed preference for negative information, we draw on a finding from Hansen and Wänke (2010). They found in an experimental setting that individuals attributed statements written in language that was more concrete than abstract as more likely to be true, even when the statements conveyed the same content. If jobseekers value truthfulness in volunteers' reviews of their employers, we would anticipate more-concretely written reviews to receive a greater helpful share. And, if volunteers tend to write using more concrete language when supplying negative feedback—perhaps due to providing more detailed experiences—then the increased perception of truth could drive jobseekers' outsized demand for negative reviews. Using the word-to-concreteness mapping of Brysbaert et al. (2013), we measure each reviews' mean level of word concreteness and standardize the cross-sectional measure to standardized z-scores. We find that negative reviews tends to be written more concretely. Reviews written with one standard deviation more concrete language are on average 0.04–0.07 stars more negative (Table 2.7: Columns 1–3). Such an increase in concreteness is associated with reviews being 0.4–0.9 percentage points more helpful to jobseekers (Columns 4–6). However, controlling for each review's degree of language concreteness, jobseekers still find more-negative reviews more helpful.

2.4 Conclusion

This work contributes to our understanding of workers' information problems, how workers acquire information differentiating employment opportunities, and how rep-

Table 2.7: Relation between Concreteness of Review Language and Helpfulness

	Star rating			Share of review votes helpful		
Standardized language concreteness	-0.065***	-0.058***	-0.038***	0.009***	0.004***	0.005***
	(0.002)	(0.001)	(0.007)	(0.001)	(0.001)	(0.000)
Star rating					-0.073***	-0.085***
					(0.001)	(0.001)
Mean dependent variable	3.22	3.21	2.88	0.86	0.86	0.85
Year-month FE	✓	✓	✓	✓	✓	✓
Employer FE		✓	✓			✓
Volunteer FE			✓			
N	1206567	1124343	70897	554190	554190	527372
Adjusted R^2	0.04	0.19	0.47	0.06	0.16	0.26

Notes: The table above relates overall star rating and share helpful to the relative concreteness of the language used in each review's text. Word concreteness is taken from a list of 37,058 unigrams and 2,896 bigrams compiled by Brysbaert et al. (2013), and reflects the review's average across all unigrams and bigrams successfully matched to this list. Language concreteness is then converted to standardized z-score. Each specification controls for the log character length, Flesch-Kincaid reading grade, and text subjectivity. Sample is restricted to reviews written prior to 2015. Standard errors are clustered by firm. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively.

utation matters for firms.[75] Successful reputation systems shift jobs from experience goods towards inspection goods. The difficulty for workers to obtain information about the real nature of each potential job creates friction. Lack of credible information can prevent workers from pursuing or even accepting job offers that would have higher value to the worker than their better-understood current job. Even if a worker knows her current job is bad, she may not trust another employer's

[75]Personnel economics has focused heavily on the manager's information problem of choosing among workers who appear the same but actually differ in unobservable ways. Workers differ in unobservable type ex ante (adverse selection, unobserved productivity) and in unobservable strategy ex post (moral hazard, unobserved effort). Abundant theory and empirical knowledge have developed how these asymmetries create inefficiencies and how employers attempt to deal with these challenges using screening mechanisms (credentials, interviews, monitoring) and incentive mechanisms (pay-for-performance, promotion tournaments, job security). As Oyer et al. (2011) note, "Because many researchers in this field must take their insights into MBA classrooms and offer advice to future managers, Personnel Economists are typically interested in how firms can solve human resource management problems and how the solutions to HR problems are related to firms' broader strategic contexts." Workers' information problems with respect to employer heterogeneity merits greater attention.

promising offer. In some ways, increased uncertainty acts like a larger mobility cost. Institutions that reduce workers' uncertainty about the nature of jobs should increase efficiency. If workers face adverse selection in choosing employers such that there are low-road employers that break their promises but successfully pool with high-road employers that fulfill their promises, the worker should discount attractive promises. In this case, institutions that help workers distinguish firms that treat workers well versus ill would have value, as modelled in Carmichael (1984a) and Benson et al. (2020a).

Reputation institutions may help promote entrepreneurship and entry from the competitive fringe by reducing the risk that workers face when considering going to work for such firms. Younger, smaller firms succeed and grow by gaining the trust necessary to attract customers and financing. Our finding here, that rating effects on applications are strongest among the least-well-known firms, extending the finding of Benson et al. (2020a) from Amazon Mechanical Turk to the broader U.S. labor market, supports the idea that lesser-known firms face another challenge: gaining potential workers' trust. Well-functioning reputation institutions enable firms with better jobs to credibly communicate that to potential workers and create disincentives against offering worse jobs. The rise of new institutions that make information exchange visible to economists creates new opportunities for improving our understanding of the worker's information problem.

The challenge in establishing a market for firm reputation is not due to the information lacking aggregate value. Rather, at least in part, the sheer number of employers, workers, and the high degrees of differentiation in tastes, productivities, and amenities between them makes the information space required to characterize all jobs for all workers quite complex. Demand for and supply of information is highly differentiated and thinly distributed. There is no mass market for singular pieces of labor-market information. It's all long tails, with only a tiny share of people interested in any one piece of information and only a tiny share of others possessing it. Lots of small transactions would have to be coordinated among strangers. With the rise of online labor market platforms—where employer information can be readily shared, aggregated, and consumed—information flow has improved but there is reason to believe that it remains quite imperfect. A market for information about jobs is missing, making the full quality of jobs difficult to observe. Improving institutions that elicit, aggregate, and distribute workers' private information can create value.

New evidence reported here points to a problem with the flow of private information between workers about employers. Because jobs matter so much to workers' lives, they have incentives to guard against risks of employer retaliation. This may lead them to refrain altogether from supplying negative information or to conceal in ways that degrade the value of the negative information supplied. The evidence developed here—along with that in Marinescu et al. (2018a)—suggests that the negative information supplied may be just the tip of an iceberg of negative information that remains largely out of workers' and researchers' view, owing to the reluctance of informed firms and workers to supply it. This highlights a "Catch-22" with the flow of information about employers. Volunteers are reluctant to supply negative information and aspects of their own identity together. From the perspective of jobseekers, this creates bias, from a lack of negative information supplied, or noise, from coarsening of volunteer identity. Threats to each worker's exercise of voice increases other workers' uncertainty about job quality at prospective employers, potentially reducing their ability to match efficiently and, thus, their expected value of exit.

Chapter 3 Caught in the Act: How Corporate Scandals Hurt Employees

3.1 Introduction

Corporate scandals are costly for firms. Revelations of impropriety hurt firm reputation and lead to poor performance (Karpoff et al., 2008; Armour et al., 2017). Academic studies and the popular press have extensively covered the effects of scandals on financial measures such as earnings, but employee outcomes have received considerably less attention.[76] The disparity is notable, as corporate governance and social responsibility have become increasingly important considerations for members of the workforce (Winograd and Hais, 2014).

In this paper, we use data from the online employer rating platform Glassdoor to help fill the gap. The website crowd-sources employee reviews of firms, pay reports decomposed into base and variable wages, and employee reviews of fringe benefits packages. The data thus allow us to study the effects that scandals have on job satisfaction and multiple types of compensation. We seek to determine if there are decreases along any of these dimension and, if so, whether they are offset by increases in another. For example, if worker sentiment falls after a scandal, firms

[76]Examples from the popular press include "Wells Fargo Posts Weaker Earnings After Sales-Practices Scandal," *Wall Street Journal*, October 13, 2017, and "Sexual-harassment scandals are hurting companies' reputations and balance sheets," *The Economist*, September 27, 2018.

may look to mollify employees by improving wages or benefits. Alternatively, if declines are not offset in any way, we can conclude that employees are left strictly worse off by corporate misdeeds. The breadth and granularity of the data allow us to conduct analysis across a variety of individual and employer characteristics within narrow time intervals. If changes in job satisfaction and compensation arise due to, say, increased news coverage or stock price reactions in the immediate aftermath of a scandal, they may quickly revert to their original levels. If the misdeeds have long-lasting effects on firms, however, negative outcomes for employee may persist.

We begin by investigating how corporate scandals affect workers' perceptions of their employers. Utilizing a difference-in-differences framework, we find that employees report less satisfaction with their firm in the wake of such events. More specifically, the overall rating among reviews drops on average 0.069 stars (on a 1–5 star scale) in the two years following a scandal. Based on the Glassdoor-star-to-dollar estimate of Sockin and Sojourner (2020), this decrease translates to a $725 annual loss for each employee. Subcategory ratings reveal that the decline in job satisfaction is driven by worse evaluations of firm culture and values, senior leadership, and career opportunities. Evidence from the gig economy suggests that lower ratings may make it more difficult for firms to attract new applicants and retain current employees (Benson et al., 2020b). As high employee satisfaction is associated with better stock returns, the reduction in sentiment may also lead to worse performance in the long run (Edmans, 2011). We further find that CEO approval rates drop 7.6 percentage points and that employees are less likely to recommend their firm to friends in the aftermath of a scandal. Referred workers generate higher profits and have longer tenures than their peers (Burks et al., 2015; Brown et al., 2016), so a weakened referral network may hamper a firm's productivity and impair its ability to fill future vacancies.

To validate our identification strategy and determine if the declines are persistent, we repeat the analysis using finer time intervals. We find that ratings drop immediately after misdeeds become public and remain lower throughout the twenty-four month post-event period. Further, we find that ratings of scandal-hit and control firms evolve similarly leading up to event dates. Thus, while underlying characteristics may render certain firms more likely to engage in misconduct (Liu, 2016; Ji et al., 2017), there is no evidence that the parallel trends assumptions underlying our analysis are violated.

We further explore how reviewer heterogeneity affects our results. While we utilize a rich set controls, our findings could stem from changes in reviewer composition. To address this concern, we re-estimate the baseline regressions with additional person fixed effects. This limits the sample to individuals who leave multiple reviews, but the results are similar to those from our benchmark specification. We also test if certain types of employees drive the drop in ratings. We find decreases along every margin, particularly in the firm culture and values subcategory, indicating that our results are not explained by losses in wages, job seniority, or other observable characteristics. The one exception to this universal decline comes when we extend the post-event period and test if employees hired after scandals also exhibit reduced sentiment. We find no effect for these newer hires, suggesting that workers less perturbed by misconduct may sort into scandal-hit firms.

We next study how corporate misdeeds impact employee compensation. We find, after controlling for a rich set of worker and firm characteristics, that base pay is unaffected by such events. Annual variable compensation, however, falls on average 10.2 percent for workers at scandal-hit employers relative to their peers at firms that did not experience a scandal. It also appears that the set of employees who are awarded variable pay may shrink after scandals. Our results are evidence in support of nominal rigidity in base pay and demonstrate that variable compensation acts as a mechanism for passing firm-level shocks onto employees. Separating workers based on experience shows that junior employees bear larger percentage point in reductions in variable wages, but that their senior colleagues face larger decreases in dollars. Decomposing the effects into finer time bins reveals that the declines persist throughout the the three year post-event period. Again, there is no indication that the parallel trends assumption is violated.

Using employees' reviews of their fringe benefit packages, we find no evidence that firms improve benefits following corporate scandals. Together, our results demonstrate that workers at scandal-hit firms are left strictly worse off. They experience a reduction in job satisfaction and variable pay, but are not compensated for these declines.

Our work is most closely related to three studies from the accounting literature. Lee et al. (2021) and Zhou and Makridis (2021) show that Glassdoor ratings decline following news about tax avoidance and Auditing Enforcement Releases (AAERs) by the Securities and Exchange Commission (SEC), respectively. Zhou and Makridis (2021) also find that reviewer more negatively about firm culture and that base wages remain unchanged following misconduct announcements. Choi and Gipper (2019) use confidential Census data to show that wages fall during and after periods of misconduct, and that employee turnover increases following AAERs. We add to this literature by considering a broader set of events and outcome variables. Unlike Choi and Gipper (2019), we find no difference in wages prior to the actual revelation of corporate misdeeds. Further, the granularity of our allows us to separately measure impacts on base and variable pay, a decomposition not included in the other studies. We also utilize non-wage benefits ratings and present evidence that misconduct may hinder a firm's ability to attract job seekers.[77]

Other work on negative reputation shocks has investigated the effects of financial misconduct (Karpoff et al., 2008; Murphy et al., 2009; Armour et al., 2017), environmental violations (Karpoff et al., 2005), product recalls (Jarrell and Peltzman, 1985; Liu and Shankar, 2015), and data breaches (Kamiya et al., 2020). The ma-

[77]In Appendix C.5, we show that Glassdoor users are less likely to click on and apply to job listings in the months following a scandal. Our job search data have several limitations, but the results provide preliminary evidence that firm reputation is an important consideration for job seekers in the traditional labor market.

jority of these studies have shown declines in financial outcomes such as stock price volatility and earnings. A notable exception is Akey et al. (2019), who find that firms increase corporate social responsibility spending to offset losses in reputation associated with data breaches. By contrasting the effects of scandals with those of breaches, we highlight how destructive corporate misconduct can be to employer reputation.

A related strand of literature focuses on collective reputation (Freedman et al., 2012; Bai et al., Forthcoming; Bachmann et al., 2021). These papers find that incidents such as the Volkswagen emissions scandal and Chinese dairy quality scandal have negative spillovers on firms in similar product markets and industries. In contrast, we focus only on effects for employees at firms committing misconduct. If there are indeed negative externalities on ratings and wages at competing firms, our difference-in-differences estimates will understate the true impact of corporate scandals.

We also contribute to a broader literature linking firm reputation and the labor market. Early theoretical contributions include Holmstrom (1981), Carmichael (1984b), and Bull (1987). Turban and Cable (2003) document that business school graduates are more likely to apply to firms that appear on lists of "the best companies to work for" published by various media outlets. More recently, Benson et al. (2020b) find that employers with high ratings are able to attract workers more quickly on Amazon Mechanical Turk and Sockin and Sojourner (2020) show that higher employer ratings on Glassdoor lead to improved job seeker application rates for firms with fewer ratings on the website. We add to this literature by documenting causal links between firm reputation, employee sentiment, and the ability to fill openings in the traditional labor market.

3.2 Data

3.2.1 Corporate Scandals

Our sample of corporate scandals comes from the popular press. Several publications, including Fortune Magazine, produce annual articles detailing the year's most notable "business misdeeds." We aggregate these lists and restrict attention to firms with appreciable coverage on Glassdoor. To ensure the presence of sufficient data both before and after a scandal, we consider only events that take place between 2013 and 2018. Table 3.1 presents information on the 23 scandals in our primary sample.[78] As shown in Column 3, the events are heterogeneous, but each involves misconduct that conveys unfavorable information about firm management and culture. Event dates are those on which a transgression became publicly known, not necessarily when it occurred. Our maintained assumption is that while certain insiders may have been aware of impropriety prior to these dates, the majority of rank-and-file employees were not.

[78] Additional details about sample construction as well as links to background news articles and the underlying scandal lists are provided in Appendix C.1.

Table 3.1: Summary of Scandal Employer Samples

Event date	Employer	Description	CEO exits	Employer reviews	Base pay	Variable pay	Benefits reviews
February 10, 2013	Carnival*	Stranded ship	06/25/2013	138	143	63	–
March 18, 2013	lululemon*	Product recall	06/10/2013	153	225	51	–
July 11, 2013	GlaxoSmithKline	Bribery	N	480	1,011	769	–
October 24, 2013	Macy's*	Racial profiling	N	6,462	5,189	796	–
December 05, 2014	Sony*	Data breach	02/06/2015	1,317	1,248	684	–
July 20, 2015	Toshiba	Accounting fraud	07/21/2015	458	440	232	–
September 20, 2015	Volkswagen	Emissions fraud	09/23/2015	301	328	177	–
October 21, 2015	Valeant	Accounting fraud	03/21/2016	144	129	85	–
July 06, 2016	Fox*	Sexual harassment	07/21/2016	836	1,113	252	135
August 18, 2016	Mylan Inc*	Price gouging	N	285	374	237	108
September 02, 2016	Samsung*	Product recall	N	1,359	1,435	920	354
September 08, 2016	Wells Fargo	Fake account fraud	10/12/2016	13,184	18,051	8,263	3,204
February 19, 2017	Uber*	Sexual harassment	06/21/2017	1,909	3,737	1,304	766
April 10, 2017	United Airlines*	Customer abuse	N	1,796	2,486	779	581
September 07, 2017	Equifax*	Data breach	09/26/2017	464	820	425	166
December 20, 2017	Apple*	Slowing old phones	N	7,201	12,710	3,400	2,575
January 25, 2018	Wynn Resorts*	Sexual harassment	02/06/2018	229	447	78	59
February 01, 2018	Guess?*	Sexual harassment	06/12/2018	462	625	75	142
March 06, 2018	Google*	Secret gov't contract	N	4,258	17,247	8,551	2,236
March 15, 2018	Facebook*	Data misuse	N	1,971	7,765	4,180	1,305
July 27, 2018	CBS*	Sexual harassment	09/09/2018	562	181	7	212
August 07, 2018	Tesla	Potential fraud	N	2,293	5,325	1,441	732
November 19, 2018	Nissan*	Misusing funds	11/19/2018	541	1,340	577	175

Notes: This table briefly describes each of the scandals in our sample. Observation counts are provided in windows around event dates. For employer reviews, windows range from 24 months before through 24 months after each event; for pay reports, four years before through four years after; and for benefits reviews, three years before through three years after. Events with starred names are included in our "non-fraud" subsample.

The accounting literature has shown that wages decrease during periods of fraud (Choi and Gipper, 2019) and that employee sentiment falls after AAERs (Zhou and Makridis, 2021). To demonstrate that our results are not driven or unduly influenced by these types of events, we conduct robustness tests on the set of "non-fraud" scandals. This subsample consists of the firms with starred names in Table 3.1.

3.2.2 Glassdoor Data

Our data come primarily from Glassdoor, an online platform with information about firms, compensation, and the labor market more broadly. Glassdoor data are particularly well-suited for the study of scandals, as employers are identified by name and we observe both compensation and job satisfaction for employees at each firm. Public surveys and other standard data sources typically redact employer identities and are published at low frequencies with a non-trivial lag between data acquisition and publication. We make use of three Glassdoor datasets: i) employee reviews of their employers, ii) employee pay reports, and iii) employee reviews of fringe benefits.

Employer Reviews

Employer reviews are provided voluntarily and anonymously by visitors to Glassdoor. Individuals are incentivized to submit reviews through a "give to get" policy, whereby visitors gain access to more information on the website by contributing to its content. This policy has been shown to reduce the selection bias inherent to online reviews, by motivating individuals with more moderate opinions to share their views (Marinescu et al., 2018b). Ratings are measured on a Likert scale from 1 star to 5 stars, with more stars corresponding to higher degrees of satisfaction. Beyond an overall rating, current and former employees can evaluate their firm on the same 5-star scale along the following dimensions: culture and values, career opportunities, senior management, compensation and benefits, and work-life balance. Respondents are also asked if they approve of CEO performance, whether they would recommend the employer to a friend, and if they have a positive business outlook for the firm over the next six months. For each of these three questions, we generate indicator variables equal to one for positive responses and zero for neutral or negative responses. Respondents may also disclose the following information: whether they are a current or former employee, employment status (i.e., full-time, part-time, contract, intern, or freelance), job title, length of employment, and location. We assign the 41 percent of reviews without a location, the 40 percent of reviews without a job title, and the 22 percent of reviews without an employment status to the "concealed" group for each variable. In total, we observe 6.22 million employer reviews. Observation counts around event dates for firms in the scandal sample are reported in Column 5 of Table 3.1.

Employee Pay Reports

Employee pay reports are also submitted to Glassdoor voluntarily, anonymously, and under the "give to get" policy. Since reports are anonymous, individuals have little, if any motive to distort wages. Karabarbounis and Pinto (2018) show that the distribution of Glassdoor salaries broadly matches income distributions observed in publicly available datasets within but not across industries. As our pay regressions exploit variation within firm and industry-job title pairs, the lack of representative-ness across industries does not compromise the validity of our results.

When submitting a pay report, respondents are asked for the following information: firm name, job title, year of salary, base income, additional income earned through cash and stock bonuses, profit sharing, sales commissions and tips/gratuities, pay frequency (annual, hourly, or monthly), gender, years of work experience, location, whether current or former employee, and employment status.[79] Pay is reported by calendar year, and we inflation-adjust to 2018 dollars using U.S headline CPI. We define variable pay as the sum of an employee's cash and stock bonuses, profit sharing, and sales commissions. To limit the influence of outliers and misreporting, we exclude reports that document less than $200 in any compensation category and those in the top and bottom 0.05 percent of base or variable pay. We observe a total of 5.24 million pay reports. Table 3.1 displays the number of base pay (Column 6) and variable pay (Column 7) observations for each firm in the scandal sample.

Employee Benefits Reviews

Employer benefits reviews are also submitted voluntarily and anonymously under the aforementioned "give to get" policy. Respondents are asked to rate their overall benefits package, which includes items such as health insurance and retirement

[79]For the 24.6 percent of reports providing hourly wages, we annualize pay by multiplying by 2000 (50 weeks with 40 hours per week). We drop the 1.3 percent of reports providing monthly wages. The 31 percent of pay reports without gender information are assigned to an "unavailable" group.

plans, on a Likert scale from 1 star to 5 stars. They are further asked whether they are a current or former employee and for their employment status, job title, and location. These reviews are available starting in 2014, which limits our sample to 14 scandals. We observe 887,000 benefits reviews in total. Counts around event dates are presented in Column 8 of Table 3.1.

3.3 Effects on Employee Sentiment

In this section, we study the effects of corporate scandals on employee sentiment. Because these events convey negative information about firms, we expect them to adversely affect Glassdoor ratings. In particular, we expect worse assessments of senior management, firm culture, and CEO approval. If shocks to reputation are long-lived, as in Liu and Shankar (2015), the decline in sentiment will be persistent. If, however, diminished employee perception is driven by negative news coverage or other short-term phenomena, ratings will quickly revert to their pre-event levels.

In order for a scandal to shift ratings, it must constitute a "surprise." If workers believe ex-ante a firm is prone to misconduct, their sentiment may not change when a scandal occurs. Further, if employees learn that misconduct has taken place before the public, we will not observe changes in ratings around event dates. Actions taken by a firm in the aftermath of a scandal may also lead to improved sentiment. If firm leadership quickly addresses misconduct or attempts to placate employees by bettering working conditions, ratings might actually increase.

3.3.1 Baseline Regressions

Summary statistics for variables in the Glassdoor ratings dataset are presented in Table 3.2. The control group consists of firms that did not experience a corporate scandal or data breach between 2013 and 2018.[80] Since scandal-hit firms tend to be large employers, we also restrict the control sample to only include large employers. Following Haltiwanger et al. (2010), we define large firms as those with at least 500 employees. As shown in Columns 2 and 5, average ratings range from 3.05–3.58 stars across categories for both sets of firms. The means of the binary response and demographic variables are also similar for both groups.

To ensure our estimates are not biased by secular trends in ratings over time, we employ a generalized difference-in-differences framework to assess how scandals affect employee sentiment. The key identifying assumption is that absent an event, ratings of scandal-hit firms would have evolved in the same manner as those of control firms. Our benchmark specification is given by the equation

$$R_{ikst} = \beta \cdot PostScandal_{kt} + \lambda X_i + \gamma_k + \gamma_{\iota(k)t} + \epsilon_{ikst} \tag{3.3.1}$$

where R_{ikst} is the star rating or indicator response for worker i employed at firm k in state s in year-month t, γ_k is a firm fixed effect, $\gamma_{\iota(k)t}$ is an industry-year-

[80]In Appendix C.3, we study the evolution of ratings around data breaches to demonstrate that our findings are not driven by increased news coverage. For parsimony, we therefore exclude the 27 firms that experienced large data breaches from the control sample.

Table 3.2: Summary Statistics for Employee Reviews

	Corporate scandals			Control firms		
Measure	N	mean	sd	N	mean	sd
Overall rating	47	3.58	1.25	4,837	3.40	1.33
Career opportunities	40	3.40	1.33	4,114	3.17	1.38
Compensation and benefits	40	3.58	1.26	4,111	3.28	1.30
Culture and values	40	3.50	1.41	4,083	3.33	1.46
Senior management	40	3.12	1.40	4,048	2.96	1.46
Work-life balance	40	3.32	1.34	4,118	3.26	1.39
Would refer a friend	47	0.52	0.50	4,837	0.49	0.50
Positive business outlook	28	0.59	0.49	3,089	0.54	0.50
Approve of CEO performance	28	0.41	0.49	3,089	0.46	0.50
Overall benefits	1	4.14	1.07	795	3.69	1.22
Is current employee	47	0.53	0.50	4,837	0.53	0.50
Concealing employee	47	0.52	0.50	4,837	0.49	0.50
Low tenure employee	28	0.59	0.49	3,089	0.54	0.50
Female employee	45	0.28	0.45	4,440	0.28	0.45
Full time employee	35	0.69	0.46	3,970	0.74	0.44
HQ employee	26	0.19	0.39	2,832	0.26	0.44
Managerial employee	47	0.08	0.28	4,828	0.11	0.31
Age	15	31.55	9.41	1,296	33.29	10.54
Firm employment	47	133.96	96.24	4,836	63.96	155.23

Notes: This table reports summary statistics for scandal-hit and control firms from our Glassdoor ratings data. The control sample consists of large firms that did not suffer a scandal or data breach between 2013 and 2018. Sample sizes (N) and firm employment are reported in thousands.

month fixed effect, X_i is a vector comprised of current employee and employment status indicators, and $PostScandal_{kt}$ is an indicator equal to one if firm k faces a scandal prior to or during year-month t. Standard errors are clustered at the firm level, as employee ratings for a given firm are likely to be correlated across time. In our primary specification, the pre-period consists of the twenty-four calendar months before an event and the post-period is composed of the event month and the subsequent twenty-three months.

The results are reported in Panel A of Table 3.3. The overall rating among newly-submitted reviews drops by an average of 0.069 stars in the two years following a scandal. Relative to the pre-scandal average of 3.84 out of 5 stars, this represents a 1.8 percent decrease. Sockin and Sojourner (2020) estimate that one star in overall Glassdoor rating is worth about \$10,500 in additional annual pay. The 0.069-star decline therefore translates to an average loss of about \$725 per year for each employee at the firm. Consistent with our hypotheses, coefficients from the sub-category rating regressions indicate that the decline in overall rating is driven primarily by diminished perceptions of firm culture and values (–0.115 stars) and senior management (–0.094 stars). The significant, negative estimate for career opportunities suggests that scandals may also reduce employees' opinions of their firms' future prospects. The insignificance of the compensation and benefits and work-life balance coefficients confirm that employees do not indiscriminately report that all aspects of the firm are worse.

The final three columns in Panel A present results for regressions on the three binary response variables. We estimate that a corporate scandal causes a 2.3 percentage point decline in the fraction of employees who would recommend their employer to a friend. As referral networks are a valuable recruiting channel (Brown et al., 2016), this finding provides suggestive evidence that misconduct hinders a firm's ability to hire new workers. We also find a 6.4 percentage point decline in the share of employees who hold a positive outlook for their employer. This result accords with the reduction in perceptions of career opportunities at the firm and

Table 3.3: Ratings Difference-in-Differences Results

	Overall rating	Culture and values	Senior mgmt.	Career opp.	Comp. and benefits	Work/life balance	Would refer a friend	Positive business outlook	Approve of CEO performance
	Panel A: Corporate scandals								
After scandal	-0.069***	-0.115***	-0.094***	-0.057***	-0.030	-0.000	-0.023***	-0.064***	-0.076***
	(0.026)	(0.029)	(0.029)	(0.021)	(0.023)	(0.040)	(0.009)	(0.017)	(0.022)
Pre-scandal mean	3.64	3.58	3.17	3.45	3.66	3.31	0.69	0.59	0.61
N	4872600	4111917	4076533	4143486	4140066	4147415	3839869	3617913	3241784
Scandal firm N	46803	39902	39544	40277	40247	40233	37283	35187	33112
Adjusted R^2	0.17	0.17	0.15	0.15	0.17	0.15	0.14	0.13	0.14
	Panel B: Corporate scandals excluding fraud								
After scandal	-0.098***	-0.117***	-0.110***	-0.056*	-0.045	-0.062*	-0.031***	-0.051**	-0.042**
	(0.032)	(0.045)	(0.041)	(0.030)	(0.030)	(0.034)	(0.011)	(0.021)	(0.017)
Pre-scandal mean	3.84	3.77	3.35	3.48	3.83	3.43	0.75	0.64	0.66
N	4855740	4097290	4062021	4128738	4125325	4132664	3826443	3605257	3230196
Scandal firm N	29943	25275	25032	25529	25506	25482	23857	22531	21524
Adjusted R^2	0.17	0.17	0.15	0.15	0.17	0.15	0.14	0.13	0.14

Notes: This table reports coefficients when Equation 3.3.1 is estimated for corporate scandals on the dependent variable listed in each column. The pre- and post-periods are each 24 months. Regressions include firm, industry x year-month, current employee, and employment status fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

may predict poor future stock performance (Sheng, 2021). Lastly, we find a 7.6 percentage point drop in CEO approval rate in the wake of a scandal. The magnitude of this effect indicates that a loss of employee confidence in firm leadership may contribute to the high rate of CEO turnover shown in Table 3.1.

In Panel B of Table 3.3, we report results when Equation 3.3.1 is estimated on the set of "non-fraud" events. The coefficient estimates remain significant and similar in magnitude to those for the full sample. The findings indicate that all types of scandals, not just financial misconduct, lead to declines in employee sentiment. Additional robustness checks are presented in the Internet Appendix. They include regressions with a broader set of control firms, stacked regressions to avoid bias arising from the staggered timing of events, and a series of "leave-one-out" regressions to ensure no single event drives our findings. In all cases, the results are similar to those from our benchmark specification.

We next study how the effects of corporate scandals evolve over time. This decomposition allows us to determine if employee sentiment is eroded in the immediate aftermath of an event and quickly recovers, or if the drop persists. Moreover, we are able to test if there are parallel trends in the outcome variables prior to scandals taking place. We re-estimate Equation 3.3.1, but allow the coefficient on the scandal indicator to vary over time by estimating separate coefficients for consecutive two-month bins ranging from twenty-four months before to twenty-three months after an event. The omitted group consists of the two calendar months immediately prior to the event.

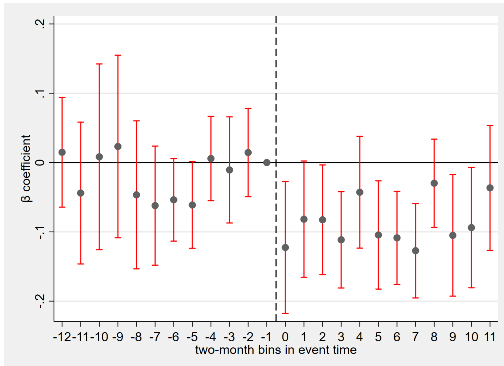
Results for the focal outcome variables are presented in Figure C.5. The estimates from the pre-event periods affirm that pre-trends do not drive our baseline results. Though the time-varying estimates have less statistical power, the negative effects of scandals on overall ratings, culture and values, and senior management develop quickly and are persistent. Ratings in each category drop more than one-tenth of a star in the immediate aftermath of a scandal and remain below their pre-scandal levels more than one year after an event. Referral probabilities also fall sharply and remain depressed over the two years following a scandal. Because executives exit in the post-event period, we take greater care when decomposing the effects of scandals on perceptions of CEOs. The bottom two panels show that approval rates decrease regardless of whether a CEO leaves, but that the short-term decline is sharper for separations. Beyond impairing a firm's ability to attract and retain workers, the longer-term reduction in job satisfaction may also lead to diminished employee productivity (McGregor, 1960).

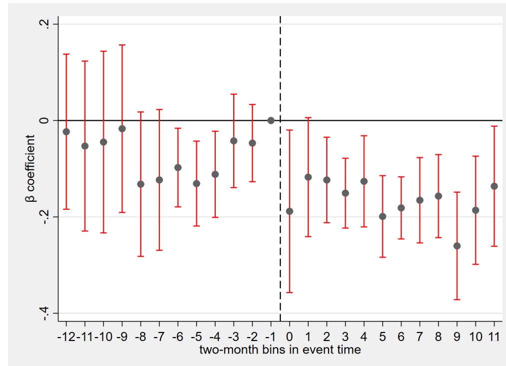
3.3.2 Further Results

Given that our dataset is comprised of cross-sections of individuals, the previous findings may be partially due to shifts in the types of employees who submit ratings after an event. We therefore restrict the sample to individuals who leave multiple reviews and re-estimate Equation 3.3.1 with additional worker fixed effects to account for time-invariant idiosyncratic characteristics. Results for the focal rating categories are presented in Table 3.4. Despite an appreciably smaller number of observations, we find significant declines in overall and culture and values ratings in the specifications with the extra fixed effects. These estimates suggest that changes in reviewer composition do not explain our findings.

In the subsequent section, we show that base pay remains unchanged after a scandal, but that variable pay falls. To determine if decreased pay is associated with stronger reactions to misconduct, we therefore re-estimate Equation 3.3.1 with an interaction term between high-likelihood of receiving variable pay and post-scandal indicators. We classify a reviewer as having a high (low) likelihood if the percentage of workers with their firm-job title pair who earn variable wages is above (below) the median. Panel A of Table 3.5 shows that there is a significant decrease in

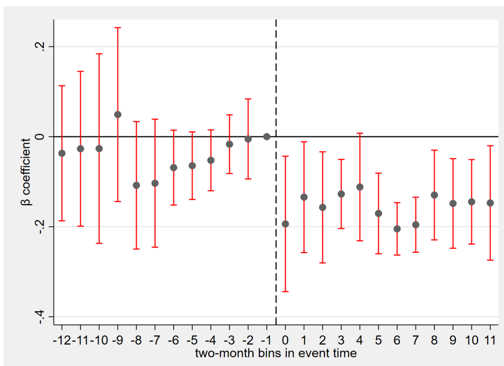
Figure 3.1: Employee Ratings around Scandal Dates, Dynamic Results

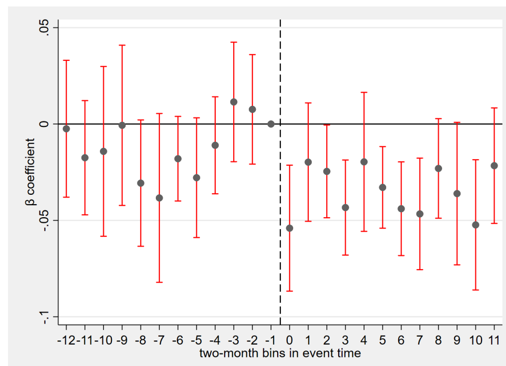




Overall rating

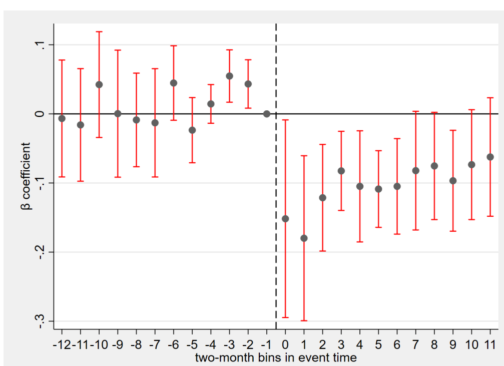
Senior management

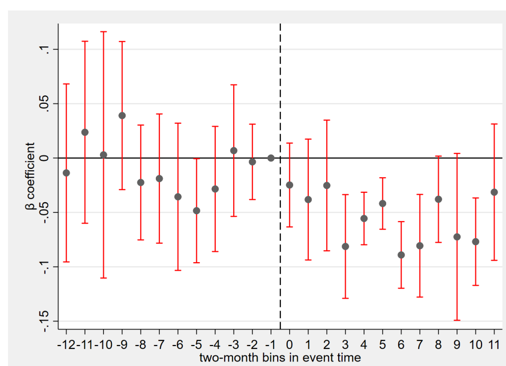




Culture and values

Would refer a friend





CEO approval (when exits)

CEO approval (when stays)

Notes: The left panel displays the cumulative share of CEOs that leave their firm after a scandal. Specific exit dates are available in Column 4 of Table 3.1. The middle panel depicts the mean share of employees who approve of their CEO in a 30-month window around scandal dates when the sample is split based on whether or not the CEO exits within a year of the event. The right panel displays coefficients from the dynamic version of Equation 3.3.1 on CEO approval rates within the same 30-month window. The horizontal dashes represent 95 percent confidence intervals for the point estimates. Regressions include firm, job title, industry x year-month, state, and employment status fixed effects. Each coefficient is relative to the two-month period prior to the event. Standard errors are clustered by firm.

Table 3.4: Outcomes After Scandals Incorporating Worker Fixed Effects

	Overall rating		Culture and values		Senior management	
	Panel A: Corporate scandals					
After scandal	-0.031	-0.071*	-0.122**	-0.119**	-0.047	-0.062
	(0.055)	(0.041)	(0.053)	(0.057)	(0.051)	(0.053)
Worker FE	N	Y	N	Y	N	Y
Pre-scandal mean	3.28	3.28	3.28	3.28	2.88	2.88
N	492896	492896	426978	426978	422869	422869
Scandal firm N	4243	4243	3675	3675	3631	3631
	Panel B: Corporate scandals excluding fraud					
After scandal	-0.077	-0.104**	-0.113	-0.140**	-0.057	-0.058
	(0.080)	(0.053)	(0.078)	(0.062)	(0.077)	(0.066)
Worker FE	N	Y	N	Y	N	Y
Pre-scandal mean	3.46	3.46	3.44	3.44	3.02	3.02
N	490362	490362	424783	424783	420685	420685
Scandal firm N	2747	2747	2373	2373	2336	2336

Notes: This table reports results when Equation 3.3.1 is re-estimated with additional worker fixed effects on the dependent variable listed in each column heading. The pre- and post-periods are each 24 months. Regressions include firm, industry x year-month, current employee, and employment status fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

ratings for both groups. The point estimates are sharper for employees who are less likely to receive variable pay, which suggests the decline in sentiment is not due to a reduction in wages. The same is true in panel B whether we partition the sample between employees with greater span of control, i.e. managers, and more rank-and-file employees, i.e. non-managers. We conduct similar tests for other characteristics such as job seniority, location, gender, age, firm tenure, and whether identifying aspects are concealed in the Internet Appendix. In all cases, we find statistically significant cases across groups.

Given that scandals have long-lasting effects on sentiment, they may lead to worker sorting. Individuals who value culture, for example, may choose to avoid

Table 3.5: Difference-in-Difference Results for Scandals, Worker Heterogeneity

	Overall rating	Culture and values	Senior management
Panel A: Probability of earning variable pay			
Total effect: low probability	-0.075*	-0.088**	-0.072*
	(0.040)	(0.041)	(0.040)
	[6630]	[6041]	[5986]
Total effect: high probability	-0.065***	-0.124***	-0.101***
	(0.025)	(0.028)	(0.026)
	[18671]	[15179]	[15051]
Panel B: Managerial role			
Total effect: manager	-0.139***	-0.207***	-0.175***
	(0.049)	(0.057)	(0.055)
	[2199]	[2048]	[2046]
Total effect: non-manager	-0.062**	-0.106***	-0.085***
	(0.026)	(0.029)	(0.028)
	[23073]	[19145]	[18964]
Panel C: When hired			
Total effect: hired before scandal	-0.078***	-0.105***	-0.099***
	(0.027)	(0.027)	(0.028)
	[8766]	[8205]	[8169]
Total effect: hired after scandal	-0.014	-0.038	0.022
	(0.048)	(0.059)	(0.059)
	[2230]	[1984]	[1942]

Notes: This table reports coefficient estimates for the effects following a scandal on different partitions of reviewers. The pre- and post-periods are each 24 months, except the post-period for panel C is 60 months to sufficiently incorporate new hires. Regressions include firm, industry x year-month, current employee, and employment status fixed effects. To account for the partitioned characteristic, the following observables are included in each panel respectively: an indicator for low probability, an indicator for managerial job title, and firm tenure fixed effects. Standard errors are clustered by firm. Sample counts for scandal-hit in the post-period are given in brackets. Significance levels: * 10%, ** 5%, *** 1%.

firms associated with impropriety. To test for such effects, we add an indicator for workers hired after scandals to our baseline specification. We also extend our post-event window to six years in order to augment the number of new employees. The results, presented in panel C, reveal significant decreases in ratings only for workers hired before scandals. This disparity suggests that individuals who are less disapproving of misconduct may indeed sort into firms that have recently suffered a scandal.

Together, the findings in this section demonstrate that scandals erode employee sentiment. Workers' ratings of employers drop appreciably and persistently in response to such events, signifying a deterioration in job satisfaction. The results do not stem from changes in reviewer composition and are robust to alternate specifications.

3.4 Effects on Compensation

Our findings in the preceding section show that employee sentiment worsens following a corporate scandal. If workers derive utility from workplace attributes unrelated to compensation (referred to herein as job attribute value), these reductions represent a loss in value from their employee-employer match. Firms may, thus, respond to scandals by raising wages in order compensate employees for the loss. They may also improve fringe benefits such as health insurance and retirement plans.

Alternatively, firms may lower wages in response to corporate scandals. If they seek to keep total expenditures unchanged despite the pecuniary costs associated with such events (e.g. fines, loss of revenue) or productivity declines due to diminished job satisfaction, firms may lower their labor bills by reducing pay. In a model where the surplus of the employee-employer match is split at a fixed rate between the worker and firm, as in Mortensen and Pissarides (1994), a negative shock to surplus is passed on to workers through lowered wages. In the presence of downward nominal rigidity in base wages for current employees (Fallick et al.,

2016) and centropy toward base pay equity for new hires (Bewley, 1995), the shock would be most apparent in variable pay, the more flexible component of employee compensation (Grigsby et al., 2021; Sockin and Sockin, 2019b).

3.4.1 Base and Variable Pay

Summary statistics for the sample of 5.15 million Glassdoor pay reports are presented in Table 3.6. We report statistics separately for scandal-hit and control firms (i.e., the 41,321 large firms that faced neither a corporate scandal nor a data breach). While demographic information is similar across samples, employees at scandal-hit firms earn about $20,000 more on average in base wages than those at other firms. A key advantage of the Glassdoor data is that earnings are broken down into base and variable components. Scandal-hit firms are 12 percentage points more likely to compensate their employees with variable pay than control firms, and employees at the former earn significantly more in variable wages. In our regression specifications, we control for a rich set of observables, including firm and job title, to account for these differences.

We further classify workers as low or high experience, with the former (latter) group comprised of employees with at most (more than) three years of work experience. Across samples, low-experience workers are approximately eight years younger, twenty percentage points less likely to receive variable pay, and earn 38 percent less in base wages than their more-experienced peers. Conditional on earn-

Table 3.6: Summary Statistics for Wages Sample

	Corporate scandals			Control firms		
	all	low exp.	high exp.	all	low exp.	high exp.
Sample size (1000s)	82	40	42	5,151	2,365	2,786
Base pay ($1000s)	88.9	68.9	107.9	69.0	52.9	82.7
	(56.4)	(43.8)	(60.3)	(42.5)	(29.5)	(46.9)
Variable pay ($1000s)	21.5	9.9	32.5	5.9	3.0	8.5
	(54.5)	(29.9)	(68.6)	(22.7)	(13.2)	(28.1)
Earns variable pay (%)	40.5	30.5	50.1	28.3	21.6	33.9
Age (years)	31.8	28.2	35.5	33.3	29.0	37.5
Years of experience	5.6	1.6	9.5	6.4	1.5	10.5
Salaried (%)	74.9	64.4	84.8	73.1	63.9	80.9
Junior position (%)	55.5	74.9	36.2	48.0	68.7	30.2

Notes: This table presents summary statistics from the pay reports data for scandal-hit and control firms. Each entry reflects the within-sample mean. Base and variable pay are inflation-adjusted using U.S. headline CPI to 2018 dollars, and their standard errors are reported in parentheses. The sample is restricted to full-time workers and pay for hourly workers is annualized assuming 40 hours of work for 50 weeks. For variable pay, we present the conditional mean among those who earn it. Low (high) experience reflects employees having at most (more than) three years of experience. Junior positions are industry-job titles for which the median years of work experience is at most three years.

ing variable pay, high-experience workers receive on average 2.5x–3x more than their low-experience colleagues. Demographics are similar within groups across samples, but employees at scandal-hit firms are still paid appreciably more on average.

We also partition employees based on their standing in the corporate hierarchy, as this assignment may better capture how firms allocate variable pay (Sockin and Sockin, 2019a). To do so, we bin workers by their industry and job title, and take the median years of work experience among all employees within each pair.[81] We define junior (senior) positions as pairs for which the median is at most (more

[81]Industries are assigned through a mapping from firm to industry. There are 21 industries: Accounting & Legal, Aerospace & Defense, Arts, Entertainment & Recreation, Biotech & Pharmaceuticals, Business Services, Construction, Consumer Services, Education, Finance, Government, Health Care, Information Technology, Insurance, Manufacturing, Media, Non-Profit, Energy, Real Estate, Retail, Telecommunications, Transportation, and Travel. To limit measurement error, industry-job title pairs with fewer than 20 observations are omitted.

than) three years. As evidenced by the final row of Table 3.6, there is significant commonality between low experience and junior standing, but the classifications do not perfectly overlap.

To formally test how firms alter employee base and variable pay in the wake of a scandal, we again implement a generalized difference-in-differences framework. The benchmark regression specification is

$$w_{ijkmt} = \beta \cdot PostScandal_{kt} + \lambda X_i + \gamma_k + \gamma_{\iota(k)jt} + \gamma_{mt} + \epsilon_{ijkmt} \tag{3.4.1}$$

where w_{ijkmt} is log base or variable pay for worker i with job title j employed at firm k in metro m in year t, γ_k is a firm fixed effect, $\gamma_{\iota(k)jt}$ is an industry-job title-year fixed effect, γ_{mt} is a metro-year fixed effect, X_i is a vector of individual controls that includes years of experience squared and gender, and $PostScandal_{kt}$ is an indicator equal to one if firm k faced a scandal prior to year t.[82] Standard errors are clustered by firm. By incorporating firm, industry-job title-year, and metro-year fixed effects, our coefficient of interest, β, captures the change in pay for employees at scandal-hit firms in the years following an event relative to employees in similar roles at peer firms, accounting for trends over time in local labor markets. The rich set of control variables ensures that we recover tightly identified estimates.

Results for the full sample of corporate scandals are presented in the first three

[82] Because employee pay is reported by calendar year, we are unable to determine exactly when a worker's base and variable compensation is set. We therefore exclude the year of the scandal from the analysis. Metro areas in Glassdoor data correspond roughly to core-based statistical areas (CBSAs). There are 858 unique metros in the Glassdoor pay data and 929 CBSAs.

columns of Table 3.7. Column 1 shows that base pay is unchanged following a scandal, which we view as evidence in support of downward nominal wage rigidity. We note that firms' total labor bill for base wages may not remain unchanged, however, as scandal-hit firms could respond by laying off workers. Column 2 reveals that variable pay *is* affected by scandals. Employees at scandal-hit firms see their variable pay fall 10.2 percent (10.8 log points) on average in the post-event period relative to their peers at control firms.[83] This finding affirms that variable compensation acts as a mechanism for passing firm-level shocks on to workers. In Column 3, we estimate Equation 3.4.1 using an indicator equal to one if the worker earns variable pay as the dependent variable. The marginally significant negative coefficient suggests that firms may shrink the set of workers who receive variable pay after a scandal.

Columns 4–6 report findings when we repeat the exercise using only the "non-fraud" events. Again, we find that base pay is unchanged. The estimate for variable pay remains significant and, at –12.4 percent (–13.2 log points), is similar to the full sample counterpart. These results affirm that the reduction in wages is not driven by instances of fraud.

We next return to our full sample of corporate scandals and re-estimate Equation 3.4.1 separately for low- and high-experience employees. Columns 1 and 2 of Table 3.8 indicate that base pay for both groups is unaffected by scandals. Column 3

[83]The decrease is not inconsistent with the null result on employees' ratings of compensation and benefits from table 3.3, as there is no decline in base wages and not all employees earn variable income.

Table 3.7: Difference-in-Difference Results for Employee Pay

	Corporate scandals			Corporate scandals excluding fraud		
	Base pay	Variable pay	Earns VP	Base pay	Variable pay	Earns VP
After scandal	-0.014	-0.108***	-0.054*	-0.021	-0.132***	-0.068*
	(0.013)	(0.035)	(0.031)	(0.016)	(0.038)	(0.037)
Pre-scandal mean	11.09	9.85	0.49	11.24	10.42	0.46
N	4435101	1160649	4683995	4415796	1152794	4663036
Scandal firm N	63615	24228	67866	45034	16770	47653
Adjusted R^2	0.83	0.64	0.32	0.83	0.64	0.32

Notes: This table reports coefficients for corporate scandals and data breaches estimated from Equation 3.4.1 on the dependent variable listed in each column. The pre- and post-periods are four and three years, respectively, and the year of the scandal or breach is excluded. Regressions include years of experience squared along with firm, industry-job title x year, metro x year, gender, and pay frequency fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

reveals that for employees with at most three years of experience, variable pay drops by 16.8 percent (18.3 log points), which amounts to roughly $1,660 per year following an event. The decline is smaller in percentage-point terms for high-experience employees (Column 4), but larger in dollar terms at about $2,530. When we separate employees by job hierarchy, we get a small, marginally significant effect on base pay (Columns 5–6). The decrease in variable pay is more modest for junior employees than those in the low-experience category, but the magnitudes are similar for senior employees and those in the high-experience group.

Results from a series of robustness tests are presented in the Internet Appendix. As with employer ratings, we consider an alternate control sample of only large firms. To ensure our findings for variable pay are not driven by a single event, we again estimate "leave-one-out" regressions by iteratively excluding one firm from the

Table 3.8: Difference-in-Difference Results for Low- and High-Level Employee Compensation

	Work experience				Job hierarchy			
	Base pay		Variable pay		Base pay		Variable pay	
	Low	High	Low	High	Junior	Senior	Junior	Senior
After scandal	-0.010	-0.017	-0.183***	-0.081**	-0.017*	-0.011	-0.140***	-0.090**
	(0.009)	(0.012)	(0.041)	(0.036)	(0.010)	(0.019)	(0.043)	(0.044)
Pre-scandal mean	10.83	11.36	9.29	10.24	10.83	11.42	9.36	10.25
N	1968759	2276654	377385	715761	1952238	2096411	391712	698006
Scandal firm N	30417	31098	8515	14625	33183	26264	10355	12538
Adjusted R^2	0.81	0.82	0.62	0.63	0.81	0.80	0.63	0.63

Notes: This table reports coefficients when Equation 3.4.1 is estimated on the dependent variable listed in each column heading and the sample is split into low and high experience employees, or junior and senior employees. The pre- and post-periods are four and three years, respectively, and the year of the scandal is excluded. Low (high) experience employees have at most (more than) three years of experience. Junior (senior) positions are industry-job title pairs for which the median years of experience is at most (more than) three years. Regressions include years of experience squared along with firm, industry-job title x year, metro x year, gender, and pay frequency fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

scandal sample. We also re-weight observations to account for changes in sample composition across firms over time. In all cases, the results are similar to those obtained under the baseline specifications.

To explore how the effects of scandals on compensation vary over time, we re-estimate the difference-in-differences regressions described by Equation 3.4.1, but allow the coefficient on the scandal indicator to vary by event-year. The top panels of Figure 3.2 reaffirm that, for all employees, base pay remains unchanged while variable pay declines in the aftermath of a scandal. The lower left panel reveals that reductions in pay for less experienced employees is larger throughout the post-event window. The coefficient estimates for high experience workers are negative and stable, but lack statistical significance throughout the post-scandal period.

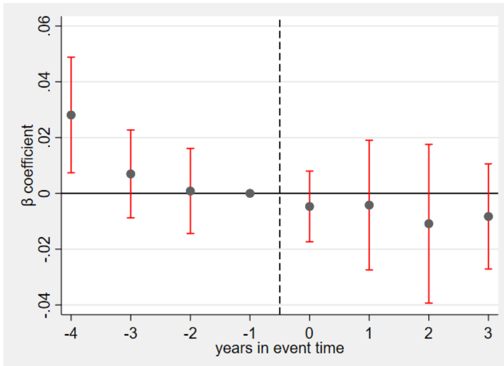
Plots of the estimates for junior and senior employees are included in the Internet Appendix, and closely resemble those for low- and high-experience workers. There is no evidence of pre-trends over the four-year period prior to a scandal occurring, which suggests that the identifying assumption of parallel trends underlying the difference-in-differences framework holds.

3.4.2 Fringe Benefits

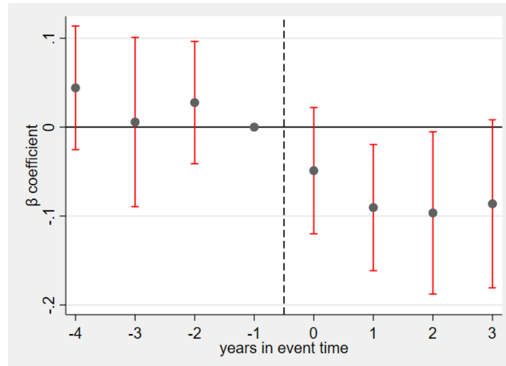
Our findings thus far have shown that employees at firms affected by scandals lose utility from their employee-employer match due to decreases in job-attribute value, and that this decline is not offset by an increase in wages. In fact, for workers who receive variable pay, we observe a sizable reduction in earnings. An alternative channel through which employers may increase compensation is an improvement in fringe benefits such as health insurance and paid time off. Bolstering benefits could be more appealing to employers than raising wages if the marginal cost of doing so is lower (Rosen, 1986b). If firms decide to cut costs in response to a scandal, however, workers' fringe benefits may instead worsen.

Though we cannot directly observe whether firms alter benefits, we can test for changes in employees' Glassdoor ratings of their overall benefits packages. Ratings should increase if firms opt to improve benefits. To formally test for changes following an event, we again employ a difference-in-differences framework. The regression

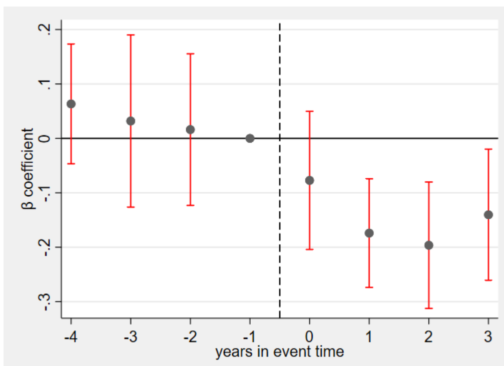
Figure 3.2: Compensation after Corporate Scandals, Dynamic Results



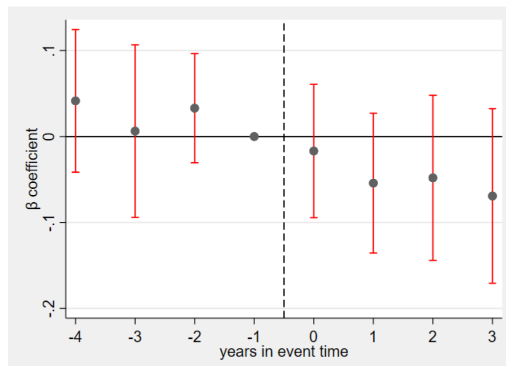
Base pay



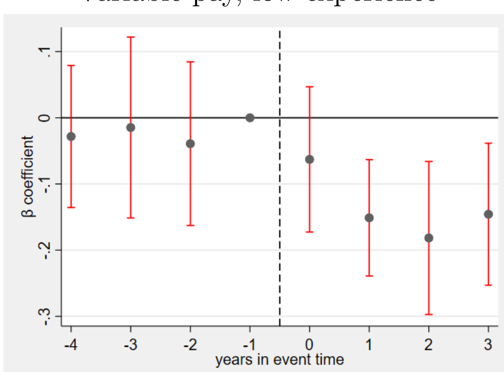
Variable pay



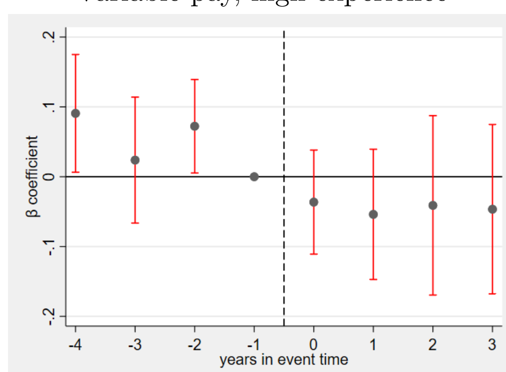
Variable pay, low experience



Variable pay, high experience



Variable pay, junior positions



Variable pay, senior positions

Notes: Each panel displays coefficients estimated from Equation 3.4.1 for log variable or base pay within an eight-year window around event dates. Low (high) experience employees have at most (more than) three years of experience. Junior (senior) positions are industry-job title pairs for which the median years of experience is at most (more than) three years. Regressions include a quadratic in years of experience along with firm, industry-job title x year, metro x year, gender, and pay frequency fixed effects. Horizontal dashes indicate a 95 percent confidence interval around each point estimate. Each coefficient is relative to one year prior to each event. Standard errors are clustered by firm.

equation is

$$r_{ikst} = \beta \cdot PostScandal_{kt} + \lambda X_i + \gamma_k + \gamma_{st} + \gamma_{\iota(k)t} + \epsilon_{ikst} \tag{3.4.2}$$

where r_{ikst} is the benefits rating from worker i employed at firm k in state s in year t, γ_k is a firm fixed effect, γ_{st} is a state-year fixed effect, $\gamma_{\iota(k)t}$ is an industry-year fixed effect, X_i is a vector of individual controls, and $PostScandal_{kt}$ is an indicator equal to one if firm k faced a scandal prior to or during year t. The control group is comprised of 106,112 firms in the sample who experience neither a scandal nor breach from 2013 through 2018.

The results are summarized in Table 3.9. The estimate in Column 1 is small and not statistically different from zero. The null result suggests that firms do not respond to scandals by improving fringe benefits.[84] We also find no effect for the set of non-fraud scandals.

As declines in job-attribute value are not offset by improvements to wages or benefits, we conclude that employees at scandal-hit firms are left strictly worse off. It follows that worker turnover may increase in the wake of such events, especially for workers with strong outside options. The Glassdoor data do not allow us to test this hypothesis, but Choi and Gipper (2019) use confidential data from the U.S. Census to show that employee separation rates increase after periods of fraudulent

[84] In addition to rating an employer's overall benefits package, workers may separately rate fifty-four distinct fringe benefits. Our conclusions are unchanged if, as in Liu et al. (2017), we estimate regressions including the full set of benefit-specific ratings instead of only overall ratings.

financial reporting. In Appendix C.5, we also find that job seekers on Glassdoor express less interest in firms that have recently suffered a scandal. We estimate that users are 10–20 percent less likely to click on and apply to job listings in the months following the revelation of misconduct. As search data are only available from January 2017 onward and Glassdoor is not primarily a job board, we consider these preliminary but suggestive findings.

Table 3.9: Difference-in-Difference Results for Ratings of Overall Benefits

	Corporate scandals	Corporate scandals excluding fraud
After scandal	-0.039	-0.080
	(0.055)	(0.069)
Pre-event mean	4.12	4.23
N	801591	797655
Scandal firm N	12750	8814
Adjusted R^2	0.25	0.25

Notes: This table reports coefficients for corporate scandals and data breaches estimated from Equation 3.4.2 on employee ratings of employers' overall benefits. The pre- and post-periods are each three years, respectively. Regressions include firm, industry x year, current employee, and employment status fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

3.5 Conclusion

In this paper, we explore how corporate scandals affect the relationship between firms and their employees. Using data from the website Glassdoor, we find that scandals have both immediate and longer-term negative effects. Employee sentiment experiences a sharp, lasting decline, driven primarily by diminished perceptions of senior management and firm culture. This drop is evident for scandals that do not reveal compromising information about firm fundamentals and does not materialize following data breaches. Further, workers at scandal-hit firms appear less optimistic about their employer's prospects. They express lower opinions of business outlooks, less willingness to recommend their firm to members of their social networks, and more disapproval of chief executives. While base wages and fringe benefits are unchanged following a scandal, variable pay declines. The decrease is more pronounced for less experienced workers, consistent with the notion these individuals wield less bargaining power.

While the academic literature and popular press have shown that negative reputation shocks lead to poor financial performance, we instead focus on employee outcomes. Our results highlight two channels—reduced worker satisfaction and an impaired ability to hire—through which firm productivity may be harmed. As workers do not receive compensation, pecuniary or otherwise, to offset the decline in sentiment, corporate scandals leave them strictly worse off. We conclude that firms and employees jointly bear the adverse consequences of corporate misconduct.

APPENDIX A

Additional Results for Chapter 1

A.1 Positive and Negative Correlations Possible

In this appendix, I introduce a simple firm maximization problem which reveals through comparative statics that amenities can complement or substitute for wages. Suppose there is a continuum of firms of varying productivity z looking to hire a single worker for production. Each firm posts a compensation package (w, a) where w is the wage workers can then spend on consumption and a is the amenity bundle characterizing all of the job characteristics associated with working at the firm. The firm can produce amenities a at cost $c(a)$, where $c'(a) > 0$. There is a continuum of workers whose utility depends on both the wage and amenities consumed, $U(w, a)$, and is increasing in both arguments, i.e. $U_w(w, a) > 0$ and $U_a(w, a) > 0$. The objective function of the firm is to choose a compensation package that maximizes profits,

$$\max_{w,a \geq 0} z - w - c(a).$$

Under perfect competition with perfect information, firms make zero profits, so firms with productivity z will offer (w, a) such that $z = w + c(a)$.

Workers choose the employment opportunities that offer them the most utility.

As such, they select the firm offering $(w = z - c(a), a)$ that solves

$$\max_{a \geq 0} U(z - c(a), a)$$

. The first order condition for the worker's maximization problem is given by $U_w(z - c(a), a)c'(a) = U_a(z - c(a), a)$. Rearranging slightly, we obtain the equation governing the equilibrium level of amenities provided by the firm:

$$c'(a) = \frac{U_a(z - c(a), a)}{U_w(z - c(a), a)}.$$

The left-hand side represents the marginal cost to the firm of providing more amenity a, while the right-hand side constitutes the marginal rate of substitution between amenities and wages for the worker, i.e. the added benefit the worker would gain from giving up part of their wage for more amenities.

Suppose that $U(w, a) = \log(w) + \beta \log(a)$, where β is a scaling parameter dictating to what extent workers prefer amenities compared with wages. Further, let us assume a linear cost function for amenity production $c(a) = \kappa a$. Under these functional forms, the wage and amenities offered by a firm with productivity z is

$$w = \frac{z}{1 + \beta} \text{ and } a = \frac{\beta z}{(1 + \beta)\kappa}.$$

Comparative statics reveal how the correlation between w and a can be positive or negative. Consider first the degree of firm productivity, z. In this case, $\frac{\partial w}{\partial z} > 0$ and $\frac{\partial a}{\partial z} > 0$, so $corr(w, a) > 0$ since both are positively correlated with firm output per worker. Second, consider the workers' preference for trading off amenities for wages, β. In this case, $\frac{\partial w}{\partial \beta} < 0$ and $\frac{\partial a}{\partial \beta} > 0$, so $corr(w, a) < 0$. Because workers with relatively high β increasingly prefer amenities to wages, firms will shift compensation for these workers away from wages toward amenities. Finally, with regards to the marginal cost of providing amenities κ, while the wage is unaffected, since $\frac{\partial a}{\partial \kappa} < 0$, the amenity value provided by the firm falls (Rosen, 1986a). Therefore, the wage-amenity bundles we observe across firms will reflect differences in firms' productivity levels (z), employees' preferences for amenities (β), and costs of amenity provision (κ), which together, can induce a positive or negative relation between w and a.

A.2 ASEC and Glassdoor Amenities

In this appendix, I detail how the measures used for externally validating the Glassdoor amenities against the Annual Social and Economic Supplement (ASEC) dataset are constructed. I first restrict attention to the thirteen survey waves from 2008–2020, for which the microdata are made available by Flood et al. (2020) through IPUMS-CPS. I then map workers from their industries in ASEC according to 1990 Census Bureau classifications into twenty-two Glassdoor industries.[85] I

[85]The industries and corresponding Census Bureau mappings are: Accounting & Legal (841, 890); Arts, Entertainment & Recreation (800–810, 872); Biotech & Pharmaceuticals (891); Busi-

then map workers from their occupations in ASEC according to 2010 Census Bureau classifications into twenty-one two-digit standard occupational classification (SOC) occupations. Each of the measures used in Table A.1 are then calculated by taking weighted averages (according to representative ASEC weights) by industry-occupation pairing for the following observables, where the relevant variables are included in parentheses.

- Offers pension: The worker responds that there is a pension plan at work, but she is not included or that she is included in a pension plan at work (*pension*).
- Offers insurance: The worker was included in an employer group health plan last year (*inclugh*).
- Using paid time off: The worker was absent from work last week or working part-time last week for a vacation or personal days (*whyabsnt, whyptlwk*) .
- Absent due to layoff: The worker was unemployed because she was on layoff or lost her job for other reasons (*whyunemp*).
- Employment white-male: The worker responds that is male and white (*sex, race*)
- Weekly time at work: The worker's usual hours worked per week at main job,

ness Services (721–731, 740–741, 882, 891–893); Construction, Repair & Maintenance (60, 751–760); Finance (700–710); Health Care (812–840, 861–870); Information Technology (732); Insurance (711); Manufacturing (100–392); Media (440); Non-Profit (880–881); Oil, Gas, Energy & Utilities (450–472); Real Estate (712); Retail (580–640, 642–691); Telecommunications (441–442); Transportation & Logistics (400–432); and Travel & Tourism (762–770).

conditional on the worker being employed at work (*uhrswork1,empstat*).

As shown in Table A.1, Glassdoor amenities capture labor market patterns observed across metrics related to these variables. I first consider fringe benefit information contained in ASEC by calculating the share of workers for whom the firm offers a pension or group health insurance plan, as well as the share of workers who were absent from work or worked part-time last week because they were on vacation or using personal days. These three measures should each increase as the amenities for retirement contributions, health insurance, and paid time off, respectively, improve. Since the availability of (and thus likely satisfaction with) fringe benefits depends on hours worked, employment status controls are not included in the formal regressions. The first three columns confirm these patterns, with robustly positive and significant correlations of 0.39, 0.18, and 0.14, respectively, between the Glassdoor amenities and ASEC measures. Next, I consider whether actual increased risk of forced job separation by industry-occupation according to ASEC translates into an increasingly negative amenity for job security in Glassdoor reviews and find a robustly negative correlation of –0.18. Next, I use the gender and racial composition of employment to examine whether the amenity for diversity/inclusion captures differences in diversity across labor market opportunities. In industry-occupations with a greater share of workers who are white males, the diversity/inclusion amenity is increasingly more-negative, with a correlation of –0.08. In work arrangements where employees spend more hours on the job, we would anticipate workers to have less-favorable work-life balance, as they have less time for leisure. Column 6 confirms that jobs in which workers supply more hours on average have the dis-amenity of worse work-life balance, with a correlation of –0.19. Finally, we would anticipate jobs where workers spend more hours on the job to report greater satisfaction with hours — as they have access to more full-time labor — but conditional on employment status, e.g. full-time or part-time, workers that spend increasingly many hours on the job would be more dis-satisfied with the increased hours they spend on the job. The final two columns confirm this pattern: In Glassdoor reviews, workers in industry-occupations with more work hours are significantly more positive about hours, but upon controlling for the distribution of workers by employment status, workers with longer work hours are significantly more negative about hours.

Table A.1: Relating Glassdoor Amenities and Outcomes from the Annual Social and Economic Supplement (ASEC)

	Measure calculated from ASEC							
Standardized Glassdoor amenity	Share offers pension (%)	Share offers insurance (%)	Share using paid time off (%)	Share absent due to layoff (%)	Share employment white-male (%)	Mean weekly time at work (hours)		
Retirement contributions	0.098***							
	(0.011)							
Health insurance		0.040***						
		(0.010)						
Paid time off			0.003***					
			(0.001)					
Job security				-0.010***				
				(0.002)				
Diversity/inclusion					-0.069***			
					(0.017)			
Work-life balance						-0.821***		
						(0.175)		
Hours							1.348***	-0.916***
							(0.172)	(0.167)
Employment status controls				✓	✓	✓		✓
Industry-occupations	439	439	439	439	439	439	439	439
Pairwise correlation	0.385	0.180	0.142	-0.184	-0.080	-0.185	0.352	0.352
R^2	0.15	0.03	0.02	0.18	0.22	0.59	0.12	0.60
Mean ASEC weight	17699	17699	17699	17699	17699	17699	17699	17699
Mean ASEC measure	0.480	0.430	0.033	0.030	0.452	38.80	38.80	38.80

Notes: This table reflects coefficients from regressions of ASEC-level measures on (standardized normal) Glassdoor amenities at the Glassdoor industry x two-digit SOC occupation. Regressions are weighted according to the representative ASEC weights. Employment status controls refers to the share of workers of each employment status in the Glassdoor reviews sample. Industry-occupation pairings restricted to those with at least fifty Glassdoor employer reviews. Significance levels: * 10%, ** 5%, *** 1%.

A.3 AWCS and Glassdoor Amenities

In this appendix, I detail how amenities from the American Working Conditions Survey (AWCS) are constructed for comparison with Glassdoor amenities. There is only one wave of the AWCS that was fielded in 2015, and had 3,131 respondents. I restrict attention to individuals who are employed, but not self-employed, trimming the sample to 2,117 respondents. We obtain the worker's two-digit North American Industry Classification System (NAICS) industry code and two-digit standard occupational classification (SOC) occupation. I then exclude any workers for whom industry or occupation is unavailable, trimming the sample further to 1,725 respondents. To compare with Glassdoor reviews, I map Glassdoor industries into NAICS codes.[86] Each amenity used in Table A.2 is then measured by taking weighted averages (according to representative AWCS weights) by industry-occupation pairing, where the relevant variables are included in parentheses.

- Autonomy/responsibility: If the respondent is able to choose or change the order of tasks, the methods of work, and speed/rate of work. We sum these three indicators. (*q50a–q50c*)

- On-the-job training: If over the past twelve months, respondents had undergone training to improve their skills that was paid for or provided by their employer or on-the-job training. We sum these two indicators. (*q61a,q61d*)

- Work-life balance: In general, do your working hours fit, (1) very well, (2) well, (3) not very well, or (4) not at all well, in with your family or social commitments outside work? I create an indicator variable for the worker

[86] The seventeen industries and corresponding Glassdoor industries listed in parentheses are: 11 (Agriculture and Forestry); 21 (Mining and Metals); 22 (Oil, Gas, Energy and Utilities); 23 (Construction, Repair and Maintenance); 31 (Manufacturing, Aerospace and Defense); 44 (Retail); 48 (Transportation and Logistics); 51 (Media, Telecommunications); 52 (Finance, Insurance); 53 (Real Estate); 54 (Accounting and Legal, Business Services, Information Technology, Biotech and Pharmaceuticals); 61 (Education); 62 (Health Care); 71 (Arts, Entertainment and Recreation); 72 (Travel and Tourism, Restaurants, Bars and Food Services); 81 (Consumer Services); and 92 (Government).

responds very well or well. (*q41*)

- Short breaks: Whether the respondent can (1) always, (2) most of the time, (3) sometimes, (4) rarely, or (5) never take breaks when wanted, where the integral value assigned to each option is included in parentheses. Inverting the scale, I obtain a metric that is increasing in the degree to which taking breaks when wanted is permissible. (*q51d*)

- Work schedule: In response to whether changes to their work schedule occur often, respondents could say no or yes, with the latter further qualified by either on the same day, the day before, several days in advance, or several weeks in advance. We create an indicator variable for the worker responds no, so that this measure is inversely related to the frequency of scheduling changes. (*q40*)

- Safety: Whether the respondent is exposed to each of the following all of the time, almost all the of the time, around three-fourths of the time, around one-half of the time, around one-fourth of the time, almost never, or never: vibrations - hands tools/machinery, loud noise, high temperatures, low temperatures, breathe smoke/fumes/power/dust, breathe vapors, handling chemical products, breathe tobacco smoke, and handling infectious materials. We create an indicator for each that the worker is exposed at least one-half of the time or more. I then sum the nine indicators and multiply by -1 to obtain a metric that is increasing in the degree of workplace safety. (*q23a-q23i*)

- Support: Whether the worker agrees or disagrees with the statement that their immediate boss provides useful feedback and whether their immediate boss encourages and supports their development. I create an indicator variable for each and sum the two. (*q58f,q58g*)

- Recognition: With regards to their workplace, whether the respondent (1) strongly agrees, (2) agrees, (3) neither agrees nor disagrees, (4) disagrees, or (5) strongly disagrees employees are appreciated when done a good job, where the integral value assigned to each option is included in parentheses. Inverting the scale, I obtain a metric that is increasing in the degree to which the workplace offers employees recognition. (*q51d*)

- Communication: Whether the respondent would describe their work situation as one in which they (1) always , (2) most of the time, (3) sometimes, (4) rarely, or (5) never receive contradictory instructions, where the integral value assigned to each option is included in parentheses. (*q52e*)

- Pay: With regards to their job, whether the respondent (1) strongly agrees, (2) agrees, (3) neither agrees nor disagrees, (4) disagrees, or (5) strongly disagrees that they feel that they get paid appropriately, where the integral value assigned to each option is included in parentheses. Inverting the scale, I obtain a metric that is increasing in the degree to which the worker is satisfied with pay. ($q77b$)

I then test whether the Glassdoor amenities reflect patterns observed for these ten aspects of work in the AWCS. The relations, summarized in Table A.2, confirm that Glassdoor amenities reflect differences in workplace conditions between industries and occupations. There are particularly strong correlations between the two datasets. A one-standard-deviation improvement in the Glassdoor amenity is associated with 0.44 and 0.34 standard deviations improved short breaks and safety in the AWCS, respectively. For work schedule, autonomy/responsibility, and on-the-job-training, we find robustly positive relations as well, with 0.24, 0.22, and 0.21 standard deviations increases in the AWCS per standard deviation in Glassdoor. Last, we find noticeably positive albeit weaker significant relations for support, work-life balance, pay, recognition, and communication between the two surveys with 0.09–0.13 standard deviations increases in the AWCS per standard deviation in Glassdoor. In all, this comparison offers further assurance that findings derived using Glassdoor amenities have real consequence for the U.S. labor market.

A.4 Intra-Industry Wage-Satisfaction Relations

In this appendix, I show that the positive relation observed between a firm's wage and its job satisfaction premia does not reflect across-industry differences but holds within industries as well. First, each firm is mapped a two-digit NAICS industry according to the mapping from Glassdoor industries to NAICS detailed in Footnote

Table A.2: Relating Glassdoor and American Working Conditions Survey (AWCS) Amenities

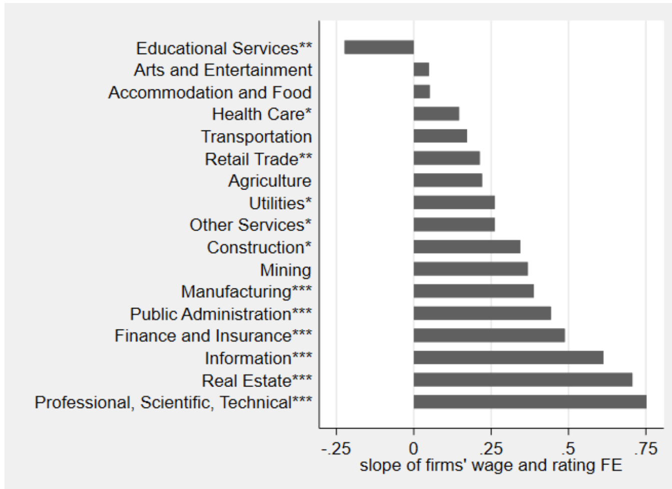
	Amenity in AWCS									
	Short breaks	Safety	Work schedule	Autonomy/ responsibility	On-the-job training	Support	Work-life balance	Pay	Recognition	Communication
Amenity in Glassdoor	0.442***	0.335***	0.241***	0.216***	0.208***	0.133***	0.131***	0.122**	0.111**	0.085**
	(0.047)	(0.057)	(0.040)	(0.053)	(0.053)	(0.041)	(0.044)	(0.047)	(0.046)	(0.043)
Industry-occupations	203	204	204	204	204	203	204	204	203	204
Pairwise correlation	0.552	0.385	0.395	0.275	0.266	0.224	0.203	0.177	0.167	0.139
R^2	0.30	0.15	0.16	0.08	0.07	0.05	0.04	0.03	0.03	0.02
Mean AWCS weight	7.589	7.588	7.588	7.588	7.588	7.620	7.588	7.588	7.620	7.588

Notes: This table reflects coefficients from regressions of (standardized normal) AWCS amenities on (standardized normal) Glassdoor amenities at the two-digit NAICS industry x two-digit SOC occupation. Regressions are weighted according to the representative AWCS weights. Industry-occupation pairings restricted to those with at least fifty Glassdoor employer reviews. Significance levels: * 10%, ** 5%, *** 1%.

86. Then, for the set of firms in each NAICS industry σ, I estimate $\hat{\lambda}_k^R = \rho^\sigma \hat{\lambda}_k^w + \upsilon_k$. The seventeen coefficients are presented in ascending order in Figure A.1. For most industries, ρ^σ is positive, with the most robust relations observed among high-skilled industries. The standalone exception is Educational Services, for which we instead observe a sharply negative relation.

From a fixed employer look-up table Glassdoor maintains, I obtain a rich set of firm characteristics, including firm type, age, and size. With regards to type, firms are partitioned according to private companies, public companies, subsidiaries, non-profits, colleges, governments, hospitals, and schools. Partitioning firms by type allows for a deeper investigation into the negative relation observed within the Educational Services sector. Is this inverse pattern driven by particular firms operating within education, such as primary and secondary school systems? I partition the sample into colleges (46 percent of employers), schools (21 percent) and all other education-based firms, and re-estimate the wage-amenity relation $\hat{\lambda}_k^R = \rho \hat{\lambda}_k^w + \upsilon_k$

Figure A.1: Heterogeneity in Slope Between Wage and Rating Premia Within Industries



Notes: This figure shows the ρ coefficient from estimating $\hat{\lambda}_k^R = \rho\hat{\lambda}_k^w + \upsilon_k$ separately within each industry for the firms in the Full set. Industries reflect two-digit NAICS, and are displayed in ascending order according to ρ. Significance levels: * 10%, ** 5%, *** 1%.

separately for all employers, only colleges, only primary and secondary schools, and all firms that are neither colleges nor schools. The results are recorded in Table A.3. Column 1 confirms the stark inverse relation between wages and job satisfaction, but Columns 2–4 reveal that schools and colleges drive this pattern. For non-college, non-school employers in fact, we observe the positive correlation between wages and job satisfaction observed elsewhere. Learning institutions appear unique in their offering of improved amenities with lower wages.

A.5 Ratings of Firms' Benefits Packages Overall

In this appendix, I show further evidence that higher-paying firms are better-amenity firms by narrowing in explicitly on workers' satisfaction levels with their

Table A.3: Relation between Firms' Pay and Satisfaction Premia within Education Sector

	Overall rating premia			
	All	Colleges	Schools	Other
Wages premia	-0.223***	-0.258*	-0.418**	0.377***
	(0.084)	(0.152)	(0.211)	(0.140)
Std. dev. rating premia	0.979	0.893	1.041	1.027
Std. dev. pay premia	0.219	0.207	0.205	0.216
Observations	3715	1703	768	1244

Notes: This table reflects regressions of firms' overall ratings premia on firms' wage premia within Educational Services by employer type. Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

employers' fringe benefits. When contributing information to the website, a worker can choose to (separately) rate their employers' overall fringe benefits package.[87] Fringe benefits reviews begin in 2014, and are appreciably thinner in size compared with the wage and job satisfaction data. That said, the data constitute employee-employer matches and contain job switchers who rate the benefits overall for multiple firms, and so equation 1.4.1 can be re-estimated with worker i's benefits rating for firm k in year t, B_{ikt}, on the left-hand side to obtain firm-specific premia in fringe benefits satisfaction $\hat{\lambda}_k^B$ for 11,965 firms.

Before relating $\hat{\lambda}_k^B$ to the firms' wage premia $\hat{\lambda}_k^w$, I re-estimate the first differences specification of equation 1.4.2 to see how the change in a workers' fringe benefits satisfaction levels $(B_{ik't'} - B_{ikt})$ relates to the change in the firms' wage premia $\hat{\lambda}_{k'}^w - \hat{\lambda}_k^w$. Panel a of Figure A.2 reveals a clear *positive* effect: Workers who move to lower-paying firms on average report worse satisfaction with fringe benefits, and

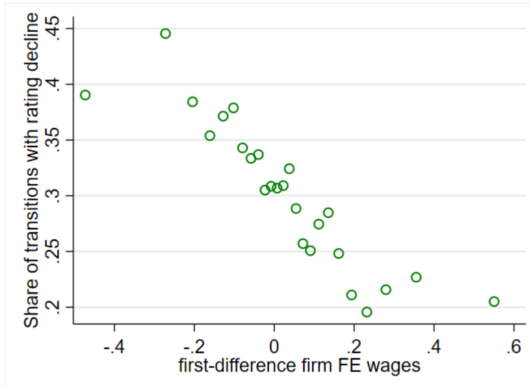
[87] For a further discussion of overall fringe benefits ratings from Glassdoor, see Gadgil and Sockin (2020).

vice-versa. Looking instead at whether the worker experiences a decline in benefits satisfaction, $1(B_{ik't'} < B_{ikt})$, panel b of Figure A.2 reveals that the probability of experiencing a decline in the quality of fringe benefits rises as workers move to lower-paying firms. This is true because, as evidenced in Column 1 of Table A.4, firms that offer relatively greater wages also provide relatively better fringe benefits packages, consistent with Pierce (2001). That said, consistent with Table 1.5, differences in fringe benefits play a limited role in explaining firms' job satisfaction premia: Incorporating firms' benefits ratings premia attenuates the slope between the firms' wage and job satisfaction premia by only 6 percent (Columns 3 and 4). That fringe benefits explain so little of the job satisfaction premia implies that accounting for pecuniary differences in fringe benefits across employers would even further widen firm-level dispersion in total compensation beyond that obtained through gauging job satisfaction levels.

A.6 Hedonic Approach to Estimating MWP

In this appendix, I estimate workers' MWP for improved job satisfaction through a hedonic approach in which job satisfaction is an attribute priced into the wages workers are willing to accept following the two-way fixed effects methodology of Lavetti and Schmutte (2018) — though given the thinness of the wage panel, I do not first residualize workers' wages by a match fixed effect. The attribute of interest that should be priced into workers' accepted wages is $\bar{R}_{\iota\sigma t}$, which reflects

Figure A.2: Growth in Benefits Ratings by the Change in Firm Wage Premia



(a) P(rating decline)

Notes: This figure depicts the average growth rate in workers' benefits ratings (panel a) and the probability of a worker experiencing a decline in fringe benefits rating (panel b) when transitioning between firms that differ in their wage premia (x-axis). Observations are partitioned into twenty-five bins according to the measure on the x-axis.

Table A.4: Firms' Wage and Job Satisfaction Premia Accounting for Fringe Benefits

	Wage premia	Overall rating premia		
Benefits ratings premia	0.015***	0.077***		0.069***
	(0.001)	(0.005)		(0.006)
Wage premia			0.560***	0.527***
			(0.051)	(0.042)
Average movers from wages	102	102	102	102
Average movers from overall ratings	58	58	58	58
Average movers from benefits ratings	4	4	4	4
Std. dev. benefits ratings	1.013	1.013	1.013	1.013
Firms	11965	11965	11965	11965
Adjusted R^2	0.01	0.01	0.02	0.02

Notes: This table displays the coefficients from regressions of the firm fixed effects for job satisfaction on the firm fixed effects for wages incorporating firm-level differences in satisfaction with fringe benefits. Benefits ratings reflect a one-to-five stars rating scale, with more stars indicating a greater level of satisfaction. Benefits ratings premia reflect the firm fixed effects from a two-way fixed effects model (with worker fixed effects) on the rating the worker assigns to the firms' overall fringe benefits package. For further description of Glassdoor benefits data, see Gadgil and Sockin (2020). Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

the three-year rolling average of job satisfaction ratings at year t for each Glassdoor industry ι and two-digit SOC occupation σ. The hedonic specification is given by

$$w_{ikt} = \beta \bar{R}_{\iota\sigma t} + \gamma X_{it} + \lambda_i + \lambda_k + \lambda_\sigma + \lambda_t + \varepsilon_{ikt}. \tag{A.6.1}$$

where X_{it} represents a fourth-order polynomial in years of work experience. Note that since workers are mapped to industries by the firm, then controlling for industry is redundant when firm fixed effects are included. The results are presented in Table A.5.

If job satisfaction is an aspect of work that workers are willing to trade off with wages, then the coefficient β should be negative. Looking across the pooled cross-section of workers with multiple wage observations, absent controlling for the productivity of the worker, the *opposite* relation is observed (Columns 1 and 2). The positive coefficients capture how high-wage workers on average also enjoy greater levels of job satisfaction, not less. When worker fixed effects are included, β now captures the trade-off the same worker would be willing to make between their wage and expected level of job satisfaction as captured through differences across industry-occupations over time. Now, a negative coefficient is observed, consistent with a compensating differential. After further accounting for time-invariant differences across firms (Column 4), a significant compensating differential is observed, with the same worker willing to forego about \$3,300 ($\beta * \bar{w}$) in wages for each additional star of job satisfaction. Since Glassdoor ratings range from one to five stars, a worker would forego roughly $13,200, or 18 percent of the average wage, to transition from a job with the lowest expected level of job satisfaction to one with the highest. Obtaining a hedonic estimate for MWP that is noticeably below estimates obtained from a tenure-based approach is not inconsistent with the literature (Dale-Olsen, 2006; Bonhomme and Jolivet, 2009; Lavetti and Schmutte, 2018). However, this could reflect asymmetry in timing: the measure of job satisfaction used in the hedonic specification is an ex ante expectation for a given labor market whereas the one used for the tenure-based approach is an ex post realization of the match. The hedonic methodology reveals that, although there is a compensating differential for job satisfaction (and thus non-wage amenities), this trade-off is not observed broadly across workers and firms.

Table A.5: Willingness-to-pay for Improved Employer Quality, Hedonic Approach

	Hedonic Specification			
	Pooled	+Industry and Occupation	+Worker	+Firm
	(1)	(2)	(3)	(4)
Overall rating (3-Yr MA)	0.652***	0.131***	-0.060***	-0.046***
	(0.029)	(0.015)	(0.011)	(0.006)
Observations	1239971	1239971	1239971	1239971
R^2	0.27	0.46	0.91	0.93
MWP one additional star	47295	9510	-3964	-3301
95% MWP confidence interval	[43242,51348]	[7379,11642]	[-4972,-2957]	[-4179,-2423]

Notes: This table reflects coefficients from a regression of average overall rating within an industry-occupation pair on wages, where the column headers reflect the level of fixed effects added to the specification. Mean wage for the sample is $72,494. Industry-occupation pairs with fewer than 50 ratings are excluded. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

A.7 Description for Each Amenity

In this appendix, I provide the input and output from the Anchored CorEx model for each of the fifty amenities, along with their respective categories.

Table A.6: Input and Output from Anchored CorEx Model: Amenities 1–10

#	Category	Amenity	Anchor words	Top twenty words
1	Pay	Pay	pay, salary, base, base pay, money	pay, salary, money, base, base pay, pay pay, discrepancy, disparity, way market, low ball, ridiculously low, offer competitive, making much, quite low, make ton, peanuts, differential, incredibly low, one lowest, great place work
2	Pay	Pay growth	raise, annual raise, salary increase, pay raise, raise base	raise, pay raise, salary increase, annual raise, raise base, yearly review, annual pay, infrequent, get cent, years without, room advancement, hard come, keep inflation, room growth, eyebrow, miniscule, minimal pay, chance advancement, little room growth, room promotion
3	Fringe benefits	Bonuses	bonus, performance, cash, stock option	bonus, performance, cash, stock option, bonus base, payouts, cow, make extra, appraisal, rsu, cows, low raise, bonus good, advancement base, eoy, hard achieve, quartly, quaterly, recognition job, sti
4	Fringe benefits	Commissions	sales, commission, quota	sales, commission, quota, selling, sale, sales rep, territory, sales job, sales people, salesperson, base salary, cold calling, commission structure, sales manager, sales goal, make sales, cold call, sales training, sales position, sales person
5	Fringe benefits	Paid time off	vacation, pto, sick days, leave, pay time off	leave, pto, vacation, pay time off, sick days, bereavement, pto sick, benefit unlimited, good amount, must use, paternal, lot desire, generous amount, benefit pay, accumulation, hard take, maternal, benefit generous, vto, pay time off sick
6	Fringe benefits	Health insurance	insurance, health insurance, dental, vision	insurance, health insurance, vision, dental, offer health, health vision, pricey, medical vision, cost health, affordable health, tunnel, pto health, poor health, heath, excellent health, could better, insurance dental, good medical, dental health, unaffordable
7	Fringe benefits	Retirement contributions	retirement, 401k, pension, contribution	401k, contribution, retirement, pension, year, include, plan, benefit, state, increase, le, cover, high, area, policy, 401k match, average, option, match, holiday
8	Fringe benefits	Employee discounts	employee discount, discount, perk	discount, perk, employee discount, merchandise, clothes, clothing, coupon, gratis, credit cards, accessory, concession, merch, apparel, full price, jewelry, sale item, retail job, free movie, phone plan, cute clothes
9	Fringe benefits	Free food	lunch, food, free, cater lunch	free, food, lunch, cater lunch, tip, eating, massage, breakfast, occasional free, cook, delicious, free food, half off, get free, free breakfast, donut, salad, sandwich, menu, free drink
10	Working conditions	Work-life balance	work life balance, work life	work life balance, work life, balance ability, maintain healthy, promote healthy, balance none, balance limited, hard balance, imbalance, difficult maintain, culture good, hard maintain, good balance, culture benefit, balence, long hours little, balace, benefit culture, flexibility good, ballance

Notes: This table details the anchor words and resulting topics from the Anchored CorEx model.

Table A.7: Input and Output from Anchored CorEx Model: Amenities 11–20

#	Category	Amenity	Anchor words	Top twenty words
11	Working conditions	Hours	hours, full time, part time	hours, full time, part time, work full time, part timer, part time employee, college student, get hours, seasonal, get full time, cut hours, position available, full time position, hours cut, require long, normal business, part time job, work part time, hours hours, benefit flexible
12	Working conditions	Work schedule	hours, shift, schedule, flex time	hours, schedule, shift, flex time, scheduling, availability, hour shift, early morning, pick extra, swap, pay flexible, monday friday, late night, inflexible, super flexible, offer flexible, night shift, extremely flexible, schedule change, week advance
13	Working conditions	Short breaks	break, rest, bathroom, lunch	break, lunch, rest, bathroom, take lunch, minute break, two minute, half hour, one hour, min lunch, laurels, hour long, min break, get break, pay lunch, break time, minute lunch, long lunch, 30min, unpaid lunch
14	Working conditions	Office space	office, desk, cubicle, cramp, building	office, building, desk, cubicle, cramp, quiet, windows, amenities, renovate, spacious, elevator, dallas, cube, remodel, natural light, beautiful new, renovation, noisy, layout, open floor plan
15	Working conditions	Commuting	commute, parking, bus, drive	drive, parking, commute, bus, traffic, downtown, shuttle, throw people, garage, public transportation, distance, depend live, long distance, valet, rush hour, parking spot, location free, locate downtown, meter, underground
16	Working conditions	Teleworking	telecommute, telework, work home, home office, remote	work home, remote, home office, telework, telecommute, one day week, flexible work, schedule ability, set hours, flexible work schedule, option available, flex schedule, days per week, hours ability, make schedule, equipment provide, flexibility ability, provide equipment, remote position, benefit ability
17	Working conditions	Location	city, location, metro	location, city, metro, rural, location location, salt lake, twin, suburb, geographic, culver, small town, jersey, inner, midtown, redwood, suburban, geographical, one location, satellite, philadelphia
18	Working conditions	Autonomy/ responsibility	autonomy, independence, responsibility	responsibility, autonomy, independence, given lot, take additional, give lot, shirk, lots flexibility, shirking, variety task, lots freedom, many responsibility, deal flow, minimal supervision, kind coworkers, variety job, work pace, schedule lots, supportive coworkers, atmosphere lots
19	Working conditions	Respect/ abuse	respect, dignity, abuse, harass, hostile	respect, abuse, hostile, harass, reason, lie, joke, upper management, write, literally, dignity, quit, unless, promise, woman, speak, blame, absolutely, claim, ignore
20	Working conditions	Communication	communication, issue, concern, meeting	issue, communication, meeting, concern, resolve, voicing, open line, meeting meeting, management listen, sometimes lack, get resolve, resolving, unresolved, many meeting, poor internal, inter department, inter departmental, need improvement, townhall, need improve

Notes: This table details the anchor words and resulting topics from the Anchored CorEx model.

Table A.8: Input and Output from Anchored CorEx Model: Amenities 21–30

#	Category	Amenity	Anchor words	Top twenty words
21	Working conditions	Support	help, support, supportive, encourage	help, support, encourage, supportive, always available, always ready, always happy, nice willing, student need, worker willing, manager willing, available need, wiling, pay school, further education, class size, support teacher, assist need, lots training, administrative support
22	Working conditions	Difficulty	challenge, growing pains, difficult, easy	challenge, easy, difficult, growing pains, job fairly, application process, peasy, getting time off, work life balance sometimes, communication sometimes, simple job, balancing work, job simple, mindless work, simple work, everyday different, decal, breezy, quick money, working public
23	Working conditions	Requirements	require, requirement, mandatory, optional	require, requirement, mandatory, optional, weekend work, five words, time commitment, weekend hours, pay low amount work, physical labor, heavy lift, extensive travel, low pay amount work, billables, lot travel, high productivity, memorization, lot paperwork, weekend require, exertion
24	Working conditions	Stress	stress, pressure, high stress, high pressure	stress, pressure, high stress, high pressure, undue, cooker, environment high, environment low, unneeded, environment little, reliever, lot unnecessary, stress high, heavy workload, much pressure, lots pressure, schedule low, high pressure environment, lots stress, heavy work
25	Working conditions	Pace	pace, fast pace, speed	fast pace, pace, speed, super fast, busy fast, snail, challenge fast, excite fast, breakneck, really fast, working fast, growing rapid, environment always, glacial, never boring, environment lots, dynamic fast, environment challenge, environment lot, learn fast
26	Working conditions	Safety	injury, dangerous, safety, conditions, workplace	workplace, safety, conditions, dangerous, injury, unsafe working, hazard, precaution, hazardous, chemical, ppe, safety employee, employee safety, safety culture, number one priority, weather conditions, safety first, extremely hot, safe work, fatality
27	Working conditions	Recognition	hard work, effort, reward	reward, hard work, effort, unnoticed, recognize reward, put forth, get reward, go unnoticed, make every, unrecognized, get recognize, recognize appreciate, always recognize, reward recognize, challenge yet, duplication, handsomely, management recognize, little recognition, working child
28	Working conditions	Morale	morale, atmosphere	atmosphere, morale, family type, upbeat, easy going, good working, booster, family friendly, relax work, positive work, family orient, good team, friendly family, friendly work, low staff, turnover low, family style, casual work, friendly fun, friendly relax
29	Working conditions	Fun	fun, boring, mundane, tedious	fun, boring, tedious, mundane, repetitive, lively, monotonous, interactive, chill, make coming work, fun fun, numbingly, lay back, company activity, summer job, get bit, work repetitive, repetitive work, get repetitive, interact customer
30	Working conditions	Culture	culture, values, environment, society, mission	culture, environment, mission, values, society, strong core, unsafe work, noble, cutthroat, fun office, pace work, comfortable working, amaze work, fantastic work, work fast pace, dog eat dog, relax office, highly political, like fast pace, great workplace

Notes: This table details the anchor words and resulting topics from the Anchored CorEx model.

Table A.9: Input and Output from Anchored CorEx Model: Amenities 31–40

#	Category	Amenity	Anchor words	Top twenty words
31	Working conditions	Diversity/ inclusion	diversity, ethnic, multicultural, inclusive, lgbtq, inclusion, equality, diverse	diversity, diverse, inclusive, inclusion, equality, lgbtq, multicultural, ethnic, gender, diversity equity, lack diversity, ethnicity, inclusivity, focus diversity, commitment diversity, race gender, patient population, inclusiveness, nationality, student body
32	Working conditions	Leadership	leadership, management	management, leadership, hands off, ceo upper, change upper, poor senior, overbear, upper middle, transparency upper, access senior, access upper, many level, lack strong, lack direction, exposure senior, support senior, lack true, direction upper, communication senior, poor middle
33	Working conditions	Office politics	politics, bureaucracy, red tape, office politics	politics, bureaucracy, office politics, red tape, get way, big company, politics politics, lots internal, lot internal, slow move, typical corporate, many layer, inter office, mire, typical large, interoffice, difficult navigate, lots red tape, lot bureaucracy, lots bureaucracy
34	Working conditions	Change	change	change, slow make, enact, resist, many change, lots change, averse, adverse, slow adapt, nothing would, slow implement, management change, schedule always, always change, abrupt, much change, scenery, chump, structure change, student life
35	Working conditions	Job security	layoff, lay off, turnover	turnover, layoff, lay off, severance, furlough, severance package, due covid, get lay off, result high, lead high, high rate, people lay off, reorgs, super high, lay off people, instability, downsizing, lay off employee, company lay off, layoff happen
36	Human capital	Career concerns	career, grow, improve, growth	growth, career, grow, improve, always room, tons room, personal career, due rapid, benefit room, room professional, communication could, ton room, real room, opportunity personal, great company, help advance, absolutely room, enough room, good place, place build
37	Human capital	Promotions	promotion, promote, job title	promote, promotion, job title, merit base, base merit, much room, internal candidate, promotion process, base know, hike, promote quickly, limited opportunity, tough get, come promotion, take long time get, promotion system, quick promotion, salary hike, promotion hard, lack promotion
38	Human capital	Experience	experience	experience, opportunity gain, lots hands, looking gain, make break, learn gain, memorable, none great, gain lots, really depend, able gain, place gain, gain much, lot hands, improve customer, help gain, highly dependent, unforgettable, without prior, courtroom
39	Human capital	Skill development	develop, skill	skill, develop, sharpen, help develop, develop new, hone, learn valuable, marketable, gain new, critical thinking, learn many, learn develop, opportunity develop, public speaking, transferrable, communication skill, develop professional, improve communication, transferable, lots opportunity learn new
40	Human capital	On-the-job training	train, training	training, train, trainer, sink swim, pay training, opportunity cross, throw wolf, shadowing, online training, training training, intensive, cpr, expect know everything, informative, management need, lack formal, cdl, provide adequate, provide proper, training do

Notes: This table details the anchor words and resulting topics from the Anchored CorEx model.

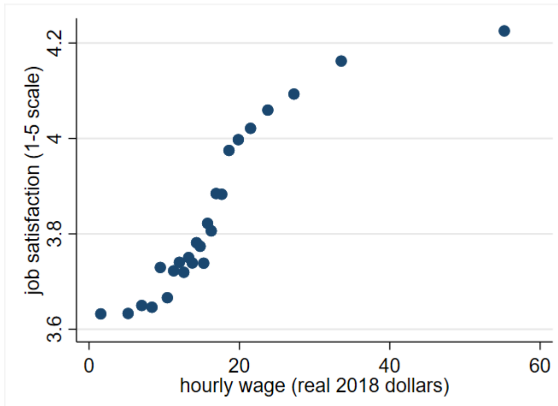
Table A.10: Input and Output from Anchored CorEx Model: Amenities 41–50

#	Category	Amenity	Anchor words	Top twenty words
41	Human capital	Mentoring	intern, internship, mentor	intern, internship, mentor, internship program, learn lot, intern program, intern get, internship experience, intern work, great experience, intern event, hands experience, busy work, intern given, summer internship, even intern, unpaid internship, internship unpaid, end internship, intern project
42	Human capital	Recruiting	hire, recruit, interview, learn	hire, learn, interview, recruit, always something new, interviewer, phone interview, almost anyone, spree, recruit process, without experience, program new, lot information, lots things, multitask, informational, useful skill, service skill, quick hire, req
43	Human capital	Contracting	contract, offer, sign	offer, contract, sign, renew, non compete, contracting, contract end, contract work, clause, rescind, new contract, contract employee, contract company, month contract, contract position, contract hire, year contract, nda, contract sign, perm
44	Human capital	Industry	industry, market, startup, organization, project, product, technology, strategy, design	industry, product, project, organization, technology, market, design, startup, strategy, exposure, beauty, best product, bench, cannabis, manufacture, really interest, volatile, aerospace, bleeding edge, saturate
45	Relationships	Managers	boss, manager, ceo, owner	manager, ceo, owner, boss, micromanager, franchise, great guy, good manager, manager assistant, assistant store, good guy, absentee, difficult work, manager district, asst, manager need, need training, district regional, manager micro, manager good
46	Relationships	Coworkers	coworkers, people, friend, family, colleague	people, family, coworkers, colleague, friend, coworkers become, become close, make feel like part, get meet, meet best, mostly good, working smart, meet wonderful, good hard working, need hire, meet awesome, worker become, meet great, hire enough, generally nice
47	Relationships	Teams	team, teamwork, collaborative	team, teamwork, collaborative, depend team, immigration, feel part, value member, happy part, orientate, supportive leadership, interdisciplinary, great support, amaze leadership, experience depend, multidisciplinary, cooperative, excellent leadership, excellent management, work life balance depend, approachable management
48	Relationships	Customers	customer, client	customer, client, servicing, many client, client want, client client, working client, care client, one client, dealing angry, customer get, building relationship, impatient, client staff, many customer, deal rude, deal angry, customer customer, every client, caregiver
49	Residual	Residual I	–	work, make, like, tell, say, time, know, job, come, working, want, way, day, place, use, start, ask, month, expect, things
50	Residual	Residual II	–	company, employee, business, role, create, new, result, process, focus, provide, level, truly, idea, individual, opportunity, continue, success, bring, allow, means

Notes: This table details the anchor words and resulting topics from the Anchored CorEx model.

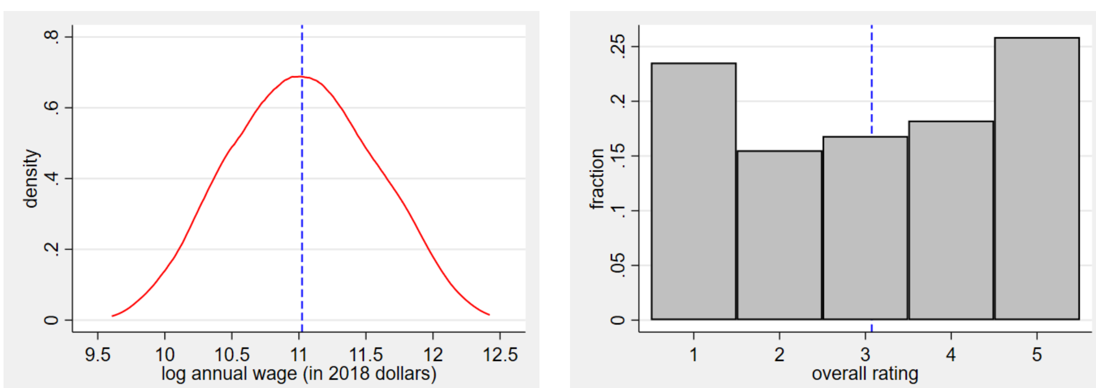
A.8 Additional Figures and Tables for Chapter 1

Figure A.3: Job Satisfaction and Hourly Wage from NLSY97



Notes: This figure depicts the average job satisfaction for each job held by respondents in the National Longitudinal Survey of Youth 1997 against the workers' log hourly wage.

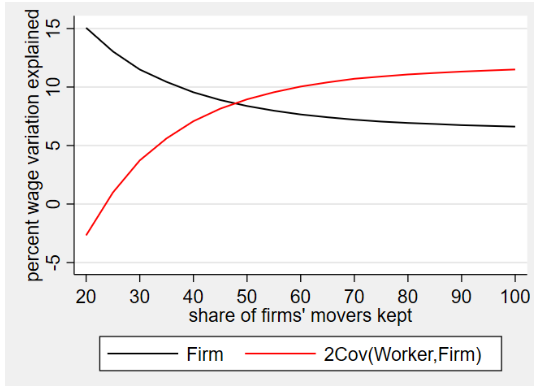
Figure A.4: Log Wages and Overall Ratings Distributions



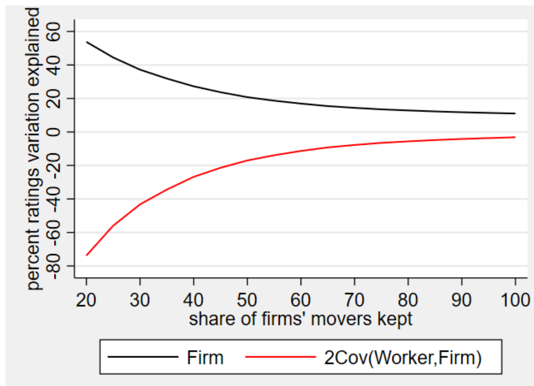
(a) Log wages (b) Overall ratings

Notes: The figure plots the distribution of log wages (panel a) and overall ratings (panel b). Samples are restricted to the panel of workers with multiple wages or multiple reviews, respectively. Dashed blue vertical lines reflect the sample means of 11.042 and 3.053 for log wages and overall ratings, respectively.

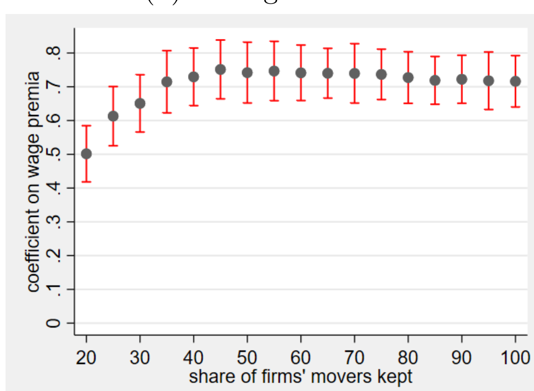
Figure A.5: Firm FE for Overall Rating with Sample Truncation



(a) Wage variance



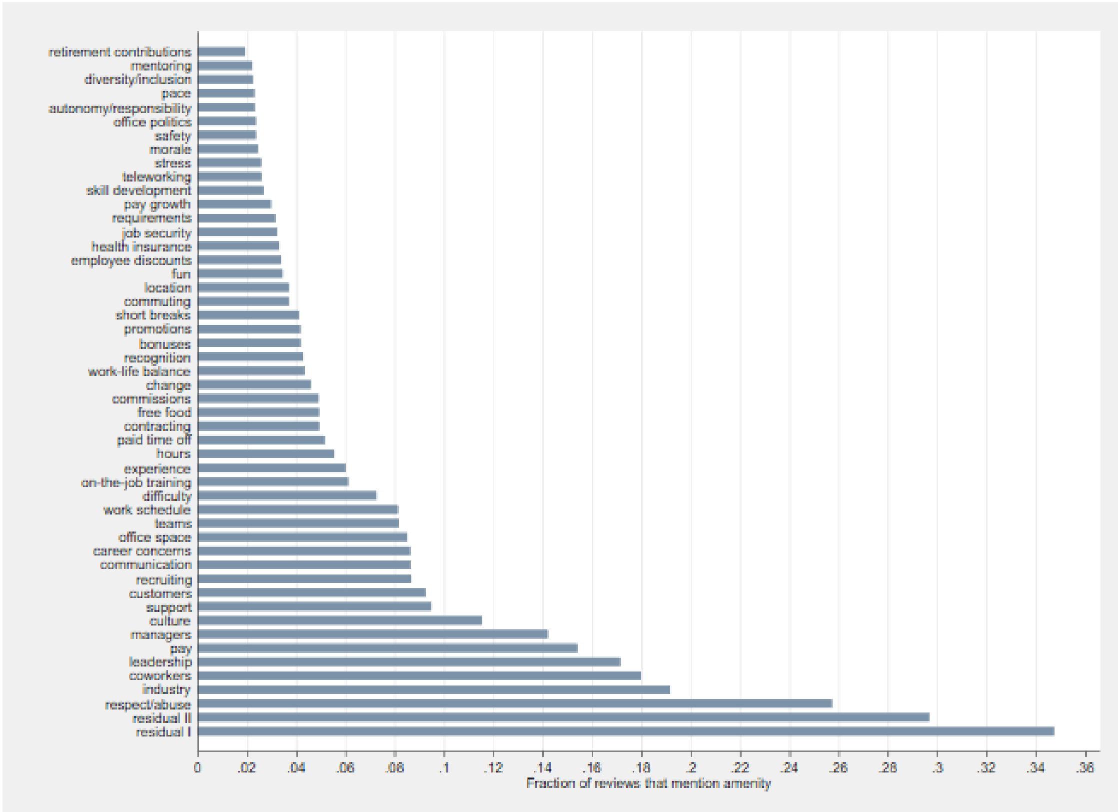
(b) Ratings variance



(c) Slope for rating-wage firm FE

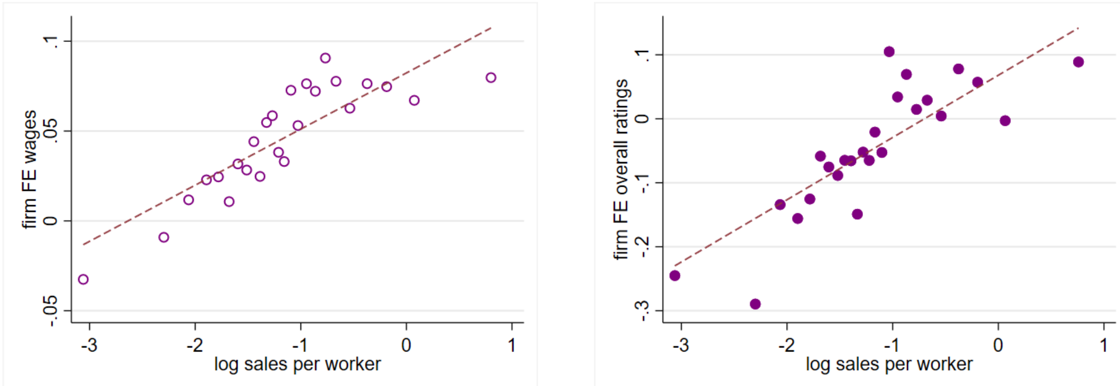
Notes: This figure depicts the share of the variance explained in wages (panel a), the share of the variance explained in overall ratings (panel b), and the coefficient ρ from estimating $\hat{\lambda}_k^R = \rho\hat{\lambda}_k^w + \upsilon_k$ (panel c) when the share of movers for each firm varies from 20 percent of the movers in the full sample to 100 percent. The sample of firms is restricted to those in the Connected sample. For each percentile of firm's movers kept, a random sample of movers is drawn fifty times and for each draw, the two-way fixed effects model of equation 1.4.1 is re-estimated. The firm fixed effects are then averaged across the fifty draws.

Figure A.6: Incidence of Each Amenity in Workers' Reviews



Notes: The figure presents, for each of the fifty amenities, the share of reviews that mention the amenity. Sample is restricted to the panel of workers with multiple reviews. An amenity is considered to be mentioned in a review if $|q_r^a| \geq 0.01$, and not mentioned otherwise. Amenities are listed in ascending order according to the rate of incidence.

Figure A.7: Firm Premia and Average Labor Productivity from Compustat



(a) Wages FE (b) Overall ratings FE

Notes: This figure plots the firm fixed effects for wages (panel a) and job satisfaction (panel b) against the firms' average labor productivity (log sales per worker). Log sales per worker available for public firms in Compustat, and so the sample of firms is restricted to public firms that can be matched from Glassdoor to Compustat. Observations are partitioned into twenty-five bins according to the measure on the x-axis.

Table A.11: Summary Measures for Wages and Ratings Samples in AKM Framework

	Log wages			Overall ratings		
Panel measure	N	mean	std. dev.	N	mean	std. dev.
Worker-year observations	2.51	2.24	0.62	1.45	2.38	0.86
Years between observations	1.34	2.48	1.91	0.80	2.01	1.78
Growth between observations	1.33	0.15	0.34	0.80	-0.03	1.72
Experiences negative growth	1.33	0.29	0.45	0.80	0.32	0.47
Worker switches firm	1.34	0.78	0.42	0.80	0.80	0.40
Current employee at origin firm	1.34	0.65	0.48	0.80	0.48	0.50

Notes: This table displays the variance decomposition for low wages and overall ratings for the Full sample of firms (all firms) and the Connected sample of firms (firms represented by at least fifteen movers). Sample sizes (N) are listen in millions.

Table A.12: Wage Growth Among Job Transitions by Firms' Rankings

Origin Firm Decile	Destination Firm Decile									
	1	2	3	4	5	6	7	8	9	10
1	-0.06	-0.11	-0.20	-0.29	-0.36	-0.40	-0.44	-0.54	-0.68	-0.86
2	0.11	-0.06	-0.09	-0.14	-0.18	-0.21	-0.25	-0.31	-0.41	-0.64
3	0.18	0.04	-0.05	-0.07	-0.11	-0.13	-0.17	-0.20	-0.29	-0.49
4	0.27	0.11	0.03	-0.05	-0.06	-0.08	-0.10	-0.14	-0.20	-0.38
5	0.34	0.18	0.09	0.03	-0.04	-0.03	-0.06	-0.09	-0.14	-0.28
6	0.42	0.22	0.13	0.07	0.02	-0.07	-0.03	-0.06	-0.09	-0.23
7	0.46	0.27	0.17	0.10	0.05	0.03	-0.04	-0.04	-0.07	-0.19
8	0.57	0.35	0.25	0.17	0.11	0.08	0.05	-0.03	-0.02	-0.12
9	0.72	0.48	0.35	0.26	0.19	0.15	0.12	0.06	-0.01	-0.05
10	0.95	0.69	0.53	0.40	0.32	0.28	0.24	0.16	0.11	-0.02

Notes: This table presents the mean wage growth for job transitions based on the rankings of the origin (initial) firm and destination (terminal) firm, where the firm rankings reflect deciles based on the firm fixed effects for wages obtained from equation 1.4.1. Sample wage growth is demeaned and residualized by the first difference in experience and years between observations.

Table A.13: Change in Overall Rating Among Job Transitions by Firms' Rankings

Origin Firm Decile	Destination Firm Decile									
	1	2	3	4	5	6	7	8	9	10
1	-0.2	-0.8	-1.2	-1.5	-1.8	-2.1	-2.2	-2.6	-2.8	-3.4
2	0.8	-0.3	-0.4	-0.7	-0.9	-1.1	-1.3	-1.7	-2.2	-3.1
3	1.3	0.4	-0.2	-0.2	-0.5	-0.7	-0.9	-1.2	-1.7	-2.6
4	1.7	0.7	0.3	-0.2	-0.2	-0.4	-0.6	-0.9	-1.3	-2.3
5	1.9	1.0	0.6	0.2	-0.2	-0.2	-0.3	-0.7	-1.1	-2.1
6	2.0	1.2	0.8	0.4	0.2	-0.2	-0.2	-0.5	-0.9	-1.9
7	2.4	1.4	1.0	0.7	0.4	0.3	-0.2	-0.2	-0.7	-1.6
8	2.7	1.8	1.4	1.0	0.8	0.6	0.4	-0.2	-0.3	-1.3
9	3.1	2.4	1.8	1.5	1.2	1.1	0.8	0.5	-0.2	-0.8
10	3.6	3.1	2.6	2.4	2.1	1.9	1.7	1.3	0.9	-0.2

Notes: This table presents the mean growth in job satisfaction rating for job transitions based on the rankings of the origin (initial) firm and destination (terminal) firm, where the firm rankings reflect deciles based on the firm fixed effects for overall rating obtained from equation 1.4.1. Sample ratings growth is demeaned and residualized by the first difference of years between observations.

Table A.14: Firms' Wage Premia and Probability of Rating Decline

	Full sample	Tenure at former job 0–1 years	Tenure at former job 5+ years	Job title stayer	Becomes a manager	Switched full-time to part-time	Switched part-time to full-time
First-difference firm FE wages	-0.105***	-0.139***	-0.109***	-0.117***	-0.120***	-0.060***	-0.076***
	(0.004)	(0.007)	(0.009)	(0.019)	(0.010)	(0.018)	(0.011)
Mean probability rating decline	0.327	0.337	0.312	0.274	0.293	0.308	0.406
Std. dev. pay premia	0.185	0.198	0.153	0.112	0.170	0.211	0.227
Observations	614244	115946	86055	47847	53681	12800	31022

Notes: This table records the point estimate from equation 1.4.2 for different types of job transitions in the ratings panel. A managerial role refers to job titles that pertain to managers, presidents, directors, chiefs, supervisors, and principals. Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

Table A.15: Change in Firms' Pay Premia and Workers' Sub-Ratings

	Career opportunities	Compensation and benefits	Culture and values	Senior management	Work-life balance
First-difference firm FE wages	0.848***	1.569***	0.555***	0.581***	0.414***
	(0.015)	(0.014)	(0.016)	(0.014)	(0.016)
Std. dev. rating	1.554	1.410	1.638	1.628	1.521
Std. dev. pay premia	0.133	0.133	0.133	0.133	0.133
Observations	479052	479052	479052	479052	479052

Notes: This table displays coefficients from regressions of the first-difference in the ratings workers leave for their employers along five sub-dimensions and the first-difference in the firm fixed effects for wages. The first difference in the number of years between observations included as a control. Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

Table A.16: Firms' Wage and Job Satisfaction Premia

	Overall rating premia			
Wage premia	0.468***	0.419***	0.716***	0.803***
	(0.017)	(0.019)	(0.043)	(0.050)
Sample	Full	Full	Connected	Connected
Industry FE		✓		✓
Average movers from wages	26	28	113	115
Average movers from reviews	17	18	62	63
Std. dev. rating premia	1.026	1.010	0.570	0.570
Std. dev. pay premia	0.220	0.217	0.148	0.149
Firms	70118	62421	10737	10338
Adjusted R^2	0.01	0.02	0.03	0.08

Notes: This table displays the coefficients from regressions of the firm fixed effects for job satisfaction on the firm fixed effects for wages. Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

Table A.17: Firms' Pay and Satisfaction Premia Over Time

	Overall rating premia	
Wage premia	0.353***	0.085***
	(0.024)	(0.026)
Year FE	✓	✓
Firm FE		✓
Firm-years	77556	77556
Adjusted R^2	0.01	0.31

Notes: This table reflects regressions of the firm-year fixed effects for job satisfaction on the firm-year fixed effects for wages. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

Table A.18: Firms' Wage and Satisfaction Premia, Incorporate Interview Process

	Wage premia	Overall rating premia		
Probability of offer premia	-0.053***	-0.128***		-0.088***
	(0.006)	(0.017)		(0.019)
Interview difficulty premia	0.052***	0.122***		0.082***
	(0.003)	(0.011)		(0.009)
Wage premia			0.835***	0.749***
			(0.039)	(0.045)
Average movers from wages	97	97	97	97
Average movers from overall ratings	57	57	57	57
Average movers from interviews	9	9	9	9
Std. dev. probability of offer	0.325	0.325	0.325	0.325
Std. dev. interview difficulty	0.696	0.696	0.696	0.696
Firms	13959	13959	13959	13959
Adjusted R^2	0.05	0.02	0.05	0.05

Notes: This table displays the coefficients from regressions of the firm fixed effects for job satisfaction on the firm fixed effects for wages incorporating firm-level differences in the interview process. Interview difficulty reflects a one-to-five stars rating scale, with more stars indicating a greater level of difficulty. Interview difficulty premia reflect the firm fixed effects from a two-way fixed effects model (with worker fixed effects) on the difficulty rating the jobseeker assigns to interviewing with the firm. Probability of offer premia reflect the firm fixed effects from a two-way fixed effects model (with worker fixed effects) on a dummy variable for the jobseeker received an offer from the firm. For both two-way fixed effects models, the logarithm of months between the date of the interview and the date submitted to Glassdoor is included as a control variable. For further description of Glassdoor interviews data, see Sockin and Zhao (2020). Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

Table A.19: Wages and Amenities Across Firms, Unweighted Amenities

Standardized amenity	Overall rating weight	Slope with wage FE	Standardized amenity	Overall rating weight	Slope with wage FE
Pay	0.14	0.30***	Recognition	0.09	0.04**
Residual I	0.44	0.23***	Retirement contributions	0.08	0.03*
Residual II	0.38	0.16***	Mentoring	0.20	0.03**
Pay growth	0.09	0.14***	Location	-0.09	0.03
Respect/abuse	0.61	0.13***	Commuting	-0.06	0.03
Short breaks	-0.01	0.13***	Employee discounts	0.03	0.03
Managers	0.29	0.12***	Experience	0.03	0.02
Culture	0.33	0.10***	Customers	0.07	0.02
Teleworking	0.03	0.09***	Bonuses	0.08	0.01
Industry	0.12	0.09***	Contracting	0.05	0.01
Leadership	0.42	0.09***	Work-life balance	0.38	0.01
Free food	-0.02	0.09***	Requirements	-0.11	0.01
Coworkers	0.15	0.09***	On-the-job training	0.16	0.01
Safety	0.11	0.08***	Fun	0.17	0.00
Health insurance	0.10	0.08***	Morale	0.27	0.00
Office politics	0.17	0.08***	Work schedule	0.07	-0.01
Teams	0.16	0.08***	Communication	0.02	-0.01
Support	0.21	0.08***	Recruiting	0.06	-0.01
Paid time off	0.03	0.06***	Hours	0.00	-0.01
Diversity/inclusion	0.21	0.05**	Change	-0.07	-0.01
Autonomy/responsibility	0.10	0.05***	Stress	0.07	-0.02
Career concerns	0.14	0.05***	Skill development	0.06	-0.02
Commissions	0.12	0.05***	Pace	0.05	-0.03
Promotions	0.04	0.05***	Job security	0.19	-0.08***
Office space	-0.02	0.04**	Difficulty	-0.19	-0.10***

Notes: This table reflects coefficients from a linear regression of the firm fixed effects for each amenity on the firm fixed effects for wages, where amenities for each review are calculating without the review-based weights ω_r. Standard errors are bootstrapped. Overall rating weights reflect the first column of Table 1.3. Significance levels: * 10%, ** 5%, *** 1%.

Table A.20: Pass-Through of Sub-Category Ratings to Overall Ratings

	Relation to first difference on overall rating				
	Career opp.	Comp. and benefits	Culture and values	Senior mgmt.	Work life balance
First difference sub-category rating	0.233***	0.107***	0.280***	0.275***	0.120***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)

Notes: This table displays the coefficients from regressing the change in star rating for the five subcategories collectively on the change in overall star rating for the panel of workers with multiple employer reviews. Sample consists of 587,626 review pairs, and the R^2 is 0.80. Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

Table A.21: Willingness-to-Pay for Improved Job Satisfaction Using Reported Wages

	1st Wage Quintile	2nd Wage Quintile	3rd Wage Quintile	4th Wage Quintile	5th Wage Quintile
Overall rating	0.054***	0.062***	0.076***	0.106***	0.139***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Log wage	0.642***	1.115***	0.778***	0.668***	0.464***
	(0.027)	(0.047)	(0.035)	(0.033)	(0.017)
Observations	69124	68959	69157	69725	68241
Ratio of coefficients	.084	.055	.098	.159	.3
Mean wage	25166	35806	45342	63213	108619
MWP one additional star	2103	1979	4430	10027	32628

Notes: This table reflects ordered probit models of wages and overall job satisfaction ratings on firm tenure following equation 1.6.1. Workers are partitioned into quintiles by their (imputed) wages. Sample is restricted to completed job spells for full-time workers. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

Table A.22: Willingness-to-Pay for Improved Job Satisfaction, Add Occupation

	1st Wage Quintile	2nd Wage Quintile	3rd Wage Quintile	4th Wage Quintile	5th Wage Quintile
Overall rating	0.065***	0.067***	0.082***	0.105***	0.132***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Log wage	0.558***	0.901***	0.849***	0.609***	0.456***
	(0.032)	(0.066)	(0.047)	(0.046)	(0.021)
Observations	52608	50945	51776	51799	51750
Ratio of coefficients	.116	.074	.097	.172	.288
Mean wage	23992	33541	44688	62082	106569
MWP one additional star	2791	2491	4338	10666	30740

Notes: This table reflects ordered probit models of wages and overall job satisfaction ratings on firm tenure following equation 1.6.1 but adding fixed effects for two-digit SOC occupation. Workers are partitioned into quintiles by their (imputed) wages. Sample is restricted to completed job spells for full-time workers for whom their job title can be matched to an SOC occupation. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

Table A.23: Relation between Wages and Amenities Across Jobs Within Firms

Standardized amenity	Log wage coefficient	Standardized amenity	Log wage coefficient
Customers	0.115***	Managers	0.015***
Pay	0.109***	Career concerns	0.015***
Commissions	0.102***	Communication	0.014***
On-the-job training	0.098***	Residual I	0.009*
Respect/abuse	0.097***	Teams	0.008
Residual II	0.097***	Commuting	0.006
Pay growth	0.083***	Mentoring	0.006
Work schedule	0.078***	Diversity/inclusion	0.006
Short breaks	0.060***	Pace	0.003
Coworkers	0.059***	Experience	0.002
Support	0.045***	Skill development	-0.002
Stress	0.044***	Free food	-0.002
Hours	0.043***	Change	-0.003
Safety	0.042***	Paid time off	-0.004
Autonomy/responsibility	0.040***	Morale	-0.012**
Contracting	0.034***	Retirement contributions	-0.015***
Location	0.032***	Bonuses	-0.017***
Recruiting	0.029***	Culture	-0.017***
Requirements	0.027***	Teleworking	-0.019***
Promotions	0.027***	Difficulty	-0.031***
Office space	0.019***	Industry	-0.034***
Leadership	0.018***	Employee discounts	-0.034***
Fun	0.018***	Health insurance	-0.035***
Work-life balance	0.018***	Job security	-0.054***
Recognition	0.016***	Office politics	-0.086***

Notes: This table reflects coefficients from a regression of log wages on each amenity's quality (standardized normal) separately. Sample is the pooled cross-section of workers with both a review and a wage . Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

Table A.24: Coefficients for the Order of Each Observation, Wages and Ratings

	Arrival order of workers' observations								
	2nd	3rd	4th	5th	6th	7th	8th	9th	10th
Log wages	0.039***	0.080***	0.115***	0.143***	0.147***	0.157***	0.181***	0.162***	0.166***
	(0.001)	(0.001)	(0.002)	(0.004)	(0.008)	(0.012)	(0.020)	(0.038)	(0.061)
	[46.5]	[5.7]	[1.1]	[0.3]	[0.1]	[0.0]	[0.0]	[0.0]	[0.0]
Overall ratings	-0.102***	-0.126***	-0.159***	-0.184***	-0.196***	-0.154***	-0.217***	-0.265***	-0.514***
	(0.004)	(0.007)	(0.012)	(0.019)	(0.030)	(0.046)	(0.063)	(0.095)	(0.153)
	[45.1]	[7.1]	[1.7]	[0.6]	[0.2]	[0.1]	[0.0]	[0.0]	[0.0]

Notes: This table displays the coefficients on indicators for the order in which the observation is observed when added to equation 1.4.1 for log wages and ratings. Point estimates are relative to the first observation. Numbers in brackets refer to the percent of the sample attributable to each order position. Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

APPENDIX B

Additional Results for Chapter 2

B.1 Search activity dataset construction

On Glassdoor, workers can also search for and apply to jobs. Glassdoor collects job postings from three main sources—online job boards, applicant tracking systems, and company websites—and captures about 81 percent of total U.S. job openings, as measured in the Job Openings and Labor Turnover Survey conducted by the U.S. Bureau of Labor Statistics (Chamberlain and Zhao, 2019). When searching, jobseekers are presented with a list of postings that display the following information: name of employer, job title, location of employment opportunity (city and state), a pay estimate range based on Glassdoor salary reviews (if available), and the overall employer rating based on U.S. employee reviews.

A job posting's showing up in a user's display constitutes an "impression"—regardless of whether the user chooses to explore the job posting further. If the jobseeker clicks on the listing and decides she would like to submit an application, then she can click again to start an application, which constitutes an "apply." For each job posting listed on Glassdoor's website from January 2017 through August 2019, we measure the total number of unique, user-specific impressions and applies that are recorded over a 72-hour window beginning from when the job posting first appears on the website.[88]

Looking at unique, user-specific measurements means that each user can record at most one impression and apply for each posting, reducing the influence that any single user can have on our job posting-specific estimates. Here, a user is defined as a jobseeker who has a registered profile on the website and can thus be identified across sessions. We restrict our attention to a 72-hour window after a job posting is listed.[89] In addition, we restrict our attention to job listings that receive at least 5 unique user impressions to ensure our results are not driven by job postings that receive minimal jobseeker attention.

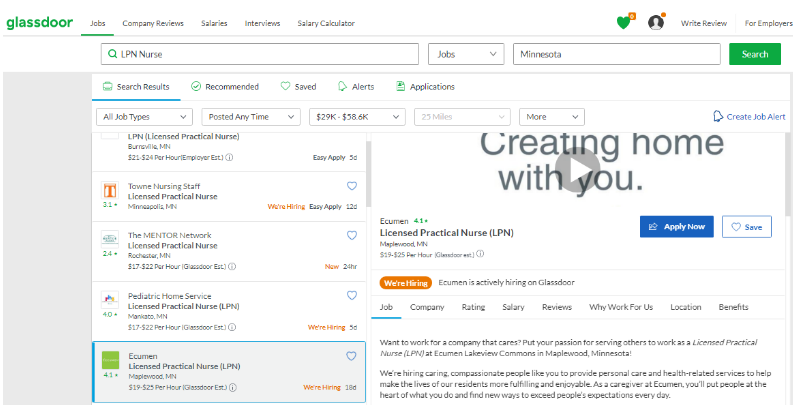
Last, we merge the job posting search totals with weekly recorded data that contain the overall rating and the number of reviews submitted thus far for each employer with at least 10 reviews submitted by that year-week. The weekly rating and review count dataset records a new reading on the Sunday of each week. Given our three-day window for capturing a job posting's total impressions and applies, we assign each job posting to the Sunday for which the window is closest. Consequently, job postings originating on Wednesdays, Thursdays, Fridays, and Saturdays are

[88]Sockin and Sockin (2019b) use a similar dataset to analyze gender differences in search preferences.

[89]We do so for two reasons. One, establishing a fixed window mitigates any trends in job-search intensity that may materialize over the life cycle of a job posting. Two, given the sheer size of the data and memory limits on the SQL query server, there is a trade-off between the length of the horizon window and our computational ability to complete this process for each calendar day. The process we implement first chooses a day of the year; then determines all job postings that originate on the website that day; and finally calculates unique impressions and applies for each posting. Choosing a 72-hour window results excluding only about 17 percent of days over the time horizon, most of which are concentrated in September to December 2018.

assigned to the following Sunday, and those on Mondays are assigned to the Sunday before. Job postings originating on Tuesdays would be equally split in coverage between Sundays and, thus, are entirely excluded from the dataset. We drop job postings for which neither an employer overall rating nor a metropolitan area is available. Employer ratings are displayed to the tenth-digit on job postings (Figure B.1), but calculated with more precision. We restrict our attention to postings for which the employer rating is within a 0.01 bandwidth of a #.#5 rounding cutoff, resulting in a sample of 2,291,920 job listings (Table B.1).

Figure B.1: Example Search Result List and a Posting's Detail with Apply Button



Notes: Figure is a screenshot image of results from a search for postings for licensed practical nurse jobs in Minnesota. Each posting in the left column is an impression, because it is shown to a jobseeker. The jobseeker can click on an impression to see a posting's details, as shown on the right. Then, the jobseeker decides whether to press the Apply Now button to begin an application, usually on the employer's site.

B.2 Report-Retaliate game

This game describes how workers with private information from working at a firm decide whether to contribute to a public reputation system, how firms affect that

Table B.1: Summary Statistics for Search Activity Dataset

Measure of Interest	Summary Statistics							
	N	mean	standard deviation	min	p10	median	p90	max
Impressions	2,291,920	20.4	35.2	5	5	11	41	5962
Applies	2,291,920	0.16	0.65	0	0	0	1	106
Applies per 100 impressions	2,291,920	0.76	1.97	0	0	0	2.63	60
Firm rating	2,291,920	3.41	0.51	1.06	2.76	3.35	4.05	4.96
Review count	2,291,920	1163	2968	10	21	235	2615	38097

Notes: This table reports summary statistics for the data on user search activity for job listings on the website Glassdoor from January 2017 through August 2019. Sample of job postings restricted to those for which the employer rating is within a 0.01 bandwidth of a #.#5 rounding cutoff. Summary statistics for "applies per 100 impressions" are weighted calculations using each job posting's impression total. Sample covers vacancies for 154,659 unique job titles posted by 39,400 employers in 858 metros.

decision through potential retaliation, equilibria that can emerge, and how this interplay determines the information that is available to jobseekers. Suppose there is a continuum of workers $\mathbb{W}$, where each worker is denoted by i, and a continuum of firms $\mathbb{F}$, where each firm is denoted by j. Each firm has quality μ^j, which expresses the probability that a worker will have a good experience working for j.[90] Each worker prefers higher quality firms.

Workers play two separable roles in the model: (1) an employee of a particular firm j deciding whether and how to rate j in a public, employer reputation system and (2) jobseekers trying to discern the quality of potential employers using the reputation system. Workers do not observe each firm's true quality μ^j. Rather, jobseekers observe public signals of quality for each firm, σ^j, based on the average

[90] For the purposes of our model, we assume that μ^j is exogenous. An alternative approach would be to endogenize μ^j and allow firm j to first choose a level of investment that would produce quality μ^j before Nature decides if worker i has a positive or negative experience. Given our focus on the role retaliation risk plays in the disclosure of inside scoops, this extension felt extraneous. We leave this interesting possibility though for future researchers to explore.

rating that firm-j insiders supplied. We assume that the only publicly available information about j is σ^j. Jobseekers do not know how many total workers have employment experience with each firm.[91] We first describe the interaction between a firm and an employee that models the insider's decision to rate the firm. Then, we describe how signals are aggregated, possible equilibria, and how this interaction between employees and their firm affects a jobseeker's ability to overcome their information problem with respect to unobserved firm quality.

Consider a worker (W_i) who starts employment with a firm (F_j). They play the Report-Retaliate game detailed in Figure B.2. Nature first decides whether W_i has a good or bad experience at F_j. With probability μ^j — the firm's quality — the worker has a good experience. And with probability $1 - \mu^j$, the worker has a bad experience. After her private experience with the firm, W_i decides whether to volunteer her inside scoop on F_j. W_i can either leave a positive rating for the firm (assume a value of +1), a negative rating for the firm (assume a value of 0), or choose not to leave a rating at all. The worker's action set is thus $a_W = \{Rate\ Positive, Rate\ Negative, Don't\}$. If W_i leaves a negative rating, then F_j can decide whether to try and retaliate against the worker. The firm's action set is thus $a_F = \{Retaliate, Don't\}$. If the firm is successful in retaliating, then the firm is able to identify the worker and get the negative rating removed. If the retaliation

[91] This assumption sidesteps the concern that workers could further infer a firm's quality by additionally knowing what share of workers have chosen not to leave a rating for the firm. In reality, even knowledge of firm age and size does not reveal a firm's total number of former and current employees because of differences in firms' hard-to-observe turnover-rate histories.

attempt is not successful, the worker is not hurt and the negative rating stands. Footnote 46 highlights how firms threaten lawsuits to this end.

Assume that the probability mass distribution of workers is given by $I(\cdot)$. We assume that the parameters of the model detailed below are primitives known to W_i and F_j.

- g^i: the payoff to the worker from having a good work experience
- a_P^i: altruism benefit from providing honest positive rating for others to consume
- a_N^i: altruism benefit from providing honest negative rating for others to consume
- c_W^i: direct cost of writing any review
- ρ^j: probability that the firm's effort to retaliate succeeds
- c_R^{ij}: cost to the worker if the firm successfully retaliates
- e^j: the firm's expenditures to attempting retaliation
- π_P^j: gain to the firm from marginal positive rating
- π_N^j: loss to the firm from marginal negative rating

For expositional simplicity, we drop i and j superscripts from the game tree.

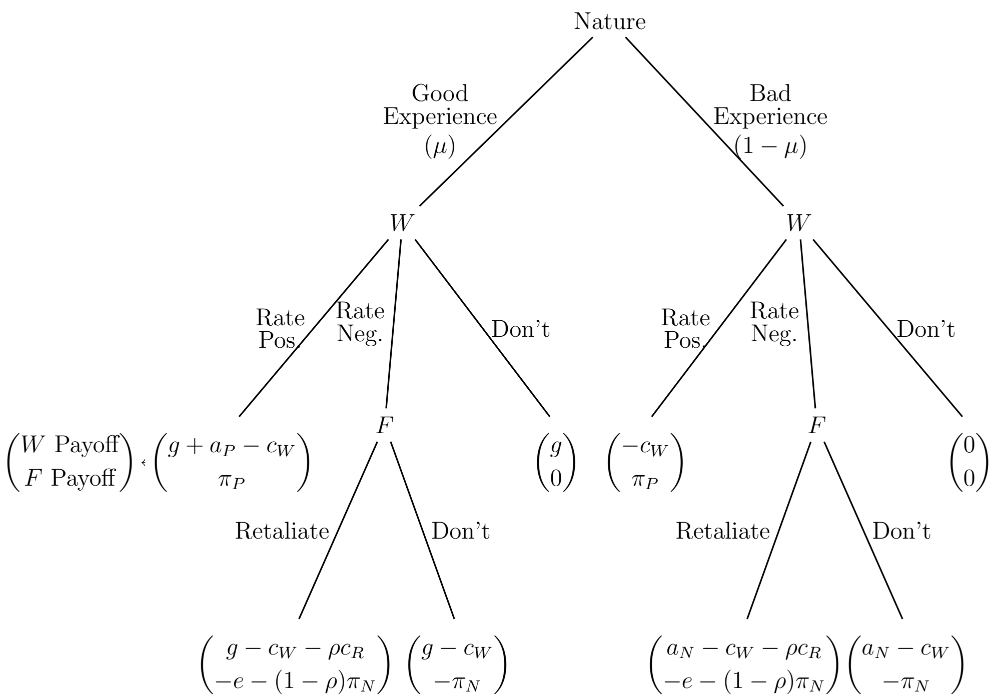


Figure B.2: Report-Retaliate Game

To solve for the subgame perfect equilibrium, consider the following cases. Following a good experience, W will choose *Rate Positive* if her benefit from volunteering exceeds her cost of supplying the review ($a_P \geq c_W$), otherwise W will choose *Don't*. *Rate Negative* is strictly dominated by *Rate Postive* because W does not receive the altruism benefit from supplying a dishonest review. Next, consider W's decision following a bad experience. W will never choose *Rate Positive* since without the altruistim benefit, this strategy is strictly dominated by *Don't*. W though anticipates F's strategy. F will *Retaliate* if the gain from retaliatory behavior exceeds the cost ($e \leq \rho\pi_N$). Otherwise ($e > \rho\pi_N$), F chooses *Don't*. In this no-retaliation case, W discloses the negative rating if and only if her private payoff covers her cost, $a_N \geq c_W$. But, if $e \leq \rho\pi_N$, then W anticipates that F will try to retaliate. W must choose between *Rate Negative* which has a payoff of $a_N - c_W - \rho c_R$ and *Don't* which as a payoff of 0. So, W will choose to disclose despite F's retaliatory stance if and only if $W's$ benefit from providing negative information exceeds the total cost from writing and the risk of retaliation succeeding ($a_N \geq c_W + \rho c_R$).

We highlight two key takeaways. Assuming F would retaliate,

- $\exists \rho^* = \frac{a_N - c_W}{c_R}$ where W keeps private a bad experience if $\rho \geq \rho^*$ and discloses if $\rho < \rho^*$. As the probability that a retaliatory attempt by F is successful rises, e.g., F can infer W's identity more easily, W is less likely to supply negative information.

- $\exists c_R^* = \frac{a_N - c_W}{\rho}$ where W keeps private a bad experience if $c_R \geq c_R^*$ and discloses if $c_R < c_R^*$. As W's cost of successful retaliation by F rises, e.g., current employees potentially facing lower wages or hours, newfound workplace abuse, or forced separation relative to former employees, then W is less likely to supply negative information.

The game above describes the experience between a single firm F and a single worker W. To understand how this aggregates into a signal for each firm j in the public reputation system, consider the set of workers who have been employed with j, $\{W\}^j$, and their collective employment experiences. The signal of firm j's quality is the average score from the set of positive and negative ratings submitted by $\{W\}^j$:

$$
\begin{aligned}
\sigma^j &= \frac{\mu^j Pr(Rate|Good)^j}{\mu^j Pr(Rate|Good)^j + (1-\mu^j) Pr(Rate|Bad)^j} \\
&= \frac{\mu^j \int_{\{W\}^j} 1(a_P^i \geq c_W^i) dI}{\mu^j \int_{\{W\}^j} 1(a_P^i \geq c_W^i) dI + (1-\mu^j)\Big[1(e^j \leq \pi_N^j) \int_{\{W\}^j} 1(a_N^i \geq c_W^i + \rho^j c_R^{ij}) dI + 1(e^j > \pi_N^j) \int_{\{W\}^j} 1(a_N^i \geq c_W^i) dI\Big]}
\end{aligned}
$$

Consider a jobseeking worker trying to use these signals to understand prospective employers. She observes σ^j for each firm j. If the worker takes a job with some firm, then she will play the role of W and the firm will play the role of F in Figure B.2. Because workers prefer good experiences to bad ones ($g > 0$), the jobseeker will look to find the firm with the highest μ, but only has the publicly-available signals $\{\sigma_j\}$ at her disposal.

Collapse of reputation system: Suppose that the cost of supplying ratings was prohibitively high such that $\forall i, c_W^i \geq \max\{a_P^i, a_N^i\}$. That is, the altruistic benefit workers receive from providing their inside scoops for others falls short of the (time and effort) costs that come with doing so. Then, following both good and bad experiences, workers will optimally choose $Don't$, in which case no ratings are supplied and there are no public signals of firm quality for workers to observe.

Completely uninformative signals: Suppose the optimal strategies following a bad experience are that each firm retaliates ($\forall j, e^j \leq \rho^j \pi_N^j$) and no workers review ($\forall i, \rho^j c_R^{ij} + c_W^i \geq a_N^i$). Then, no negative ratings are submitted and so $\forall j, \sigma^j = 1$.

The signals are then uninformative of underlying firm quality μ^j.

Perfectly-informative equilibrium absent retaliation: If F's gain from retaliation was eliminated because negative ratings had little consequence or retaliation was ineffective at mitigating negative reviews ($\rho^j \pi_N^j < e^j$), then assuming $a_P^i \sim a_N^i$, it follows that the probability of disclosing a negative rating following a bad experience would be roughly the same as the probability of disclosing a positive rating following a good experience. Then, $\sigma^j \sim \mu^j$ and the signal is highly informative of the true probability of a good experience with F.

Inability to separate firm quality from retaliation risk: In the presence of firm retaliation, not all workers will supply a negative rating following a bad experience. Workers will choose $Rate\ Negative$ if $\rho^j c_R^{ij} + c_W^i \leq a_N^i$ and $Don't$ if $\rho^j c_R^{ij} + c_W^i > a_N^i$. Assuming that $a_P^i \ggg a_N^i$, it follows that

$$\exists \lambda^{ij} = \frac{Pr(Rate\ Negative|Bad)^{ij}}{Pr(Rate\ Positive|Good)^{ij}} \in [0, 1).$$

As W's retaliation risk ($\rho^j c_R^{ij}$) increases, λ^{ij} falls. If the cost of writing a review is relatively low such that all workers leave a positive rating following a good experience, then the signal of firm quality for j would be given by

$$\sigma^j = \frac{\mu^j Pr(Rate|Good)^j}{\mu^j Pr(Rate|Good)^j + (1-\mu^j)Pr(Rate|Bad)^j} = \frac{\mu^j}{\mu^j + (1-\mu^j)\int_{\{W\}^j} \lambda^{ij} dI} > \mu^j$$

Firm retaliation thus biases the signal of firm quality upward. With jobs as experience goods, jobseekers do not know each firm's true (μ^j, ρ^j) nor do they know $\{W\}^j$ — the set of workers who have worked for firm j —so they cannot separate firm quality from the distorting effect of retaliation risk. The jobseeker's information problem is that a particular firm could have high ratings because of high μ^j or high ρ^j.

This inability to distinguish between better firm quality and stronger retaliatory risk can give rise to a pooling equilibrium unfavorable for jobseekers. Suppose there are two types of firms. The first is "high-road" firms that treat their workers well, characterized by high quality and low propensity for retaliation (μ^H, ρ^L). The second is "low-road" firms that treat their workers poorly, characterized by low quality and high propensity for retaliation (μ^L, ρ^H). Both types will have signals that are not only very positive, but impossible to distinguish. In turn, this creates a perverse incentive whereby a firm can obtain the benefits of having a positive signal by investing in a strong retaliatory stance rather than improved quality. While we do not endogenize the firm's decision to choose a level of quality and a retaliatory stance — and do not allow the firm's choice to retaliate to become public and thereby potentially influence future jobseekers' decisions — we note that these would make for interesting extensions and leave them for future researchers to explore.

Negative reviews are especially helpful to jobseekers: Each worker i has her own threshold for retaliation risk at firm j $\rho^{*ij} = \frac{a_N^i - c_W^i}{c_R^{ij}}$. The degree of censoring of negative ratings will then depend on the share of workers willing to tolerate the risk of retaliation. If $\rho^j < \rho^{*ij}$, the worker will not censor a negative rating. But, if $\rho^j > \rho^{*ij}$, the worker will censor her negative rating. If no workers censor, then σ^j will reflect true quality μ^j. But, if some workers censor, then signal σ^j will be positively biased and jobseekers will struggle to distinguish high μ-low ρ firms from low μ-high ρ ones. In this light, a negative review is more informative to jobseekers about true firm quality μ than a positive review because it runs counter to the upward bias. The unknown degree of missing negative reviews — since jobseekers can only consume volunteered information — should heighten the relative value of arriving negative information. Incorporating jobseeker risk aversion adds an additional channel by which negative reviews could be extra helpful.

Allowing for identity concealment: Finally, suppose now that in addition to observing the public signal σ^j, workers could also observe which workers left positive ratings and which workers left negative ratings. We can then layer in W^i's decision on whether to conceal aspects of their identity. This could be considered two additional strategy option for W at each decision node, {*Rate Positive Openly, Rate*

Positive Concealed, Rate Negative Openly, Rate Negative Concealed, Don't}. Identity concealment has two effects. First, it reduces the probability that the firm could successfully retaliate ($\rho^j_O \geq \rho^j_C$), reducing a cost of supplying negative information. This is only relevant in the bad-experience state. Second, concealment reduces the value of a review to jobseekers by eroding their ability to assess its relevance to their own situation and so, reduces W^i's altruistic benefit from supplying negative information ($a^i_{N,O} \geq a^i_{N,C}$). This latter channel explains why not all volunteers relaying a bad experience choose to conceal.

Relative to an open rating, supplying a negative rating with identifying aspects concealed is a strategy with lower cost, but also lower benefit. W^i will choose to disclose a negative rating and conceal if the cost of writing a review is not too prohibitive, $a^i_{N,C} \geq c^i_W + \rho^j_C c^{ij}_R$, and if the gain from lower retaliation risk outweighs the loss in altruism from partial disclosure, $(\rho^j_O - \rho^j_C)c^{ij}_R > a^i_{N,O} - a^i_{N,C}$. We can see how, even for the same firm, workers may optimally choose to disclose openly, disclose with concealment, or not disclose at all following a bad experience. Conditional on a bad experience, the share of workers that would choose each action would be

$$Pr(\textit{Rate Negative Openly}|Bad)^j = \int_{\{W\}^j} 1\Big(a^i_{N,O} \geq c^i_W + \rho^j_O c^{ij}_R,\ (\rho^j_O - \rho^j_C)c^{ij}_R \leq a^i_{N,O} - a^i_{N,C}\Big)dI$$

$$Pr(\textit{Rate Negative Concealed}|Bad)^j = \int_{\{W\}^j} 1\Big(a^i_{N,C} \geq c^i_W + \rho^j_C c^{ij}_R,\ (\rho^j_O - \rho^j_C)c^{ij}_R > a^i_{N,O} - a^i_{N,C}\Big)dI$$

$$Pr(\textit{Don't}|Bad)^j = \int_{\{W\}^j} 1\Big(c^i_W \geq \max\{a^i_{N,O} - \rho^j_O c^{ij}_R, a^i_{N,C} - \rho^j_C c^{ij}_R\}\Big)dI$$

Workers provide negative ratings openly only when their altruistic benefit of writing an open, negative review outweighs their cost of writing and of facing retaliation with the higher risk of rating openly, and when the marginal altruism payoff from openness $(a^i_{N,o} - a^i_{N,c})$ exceeds the marginal cost increase in retaliation risk $(\rho^j_O - \rho^j_C)c^{ij}_R$. This can be reduced to *Rate Negative Openly* if and only if $a^i_{N,O} - \rho^j_O c^{ij}_R \geq max\{a^i_{N,C} - \rho^j_C c^{ij}_R, c^i_W\}$, where *Rate Negative Openly*'s benefits exceed the benefits of the other two options. The other cases follow related logic.

We use current versus former employees and workers in smaller versus larger firms as proxies for higher retaliation risk $\rho^j c^{ij}_R$. Current employees face greater c^{ij}_R than former employees, and workers in smaller firms face higher ρ^j. Assume that $(a^i_{N,O} - a^i_{N,C})$ is more similar between former and current employees than is $(\rho^j_O - \rho^j_C)c^{ij}_R$, so that concealment reduces altruism benefits similarly between former and current employees but reduces retaliation risk more for current employees. Then current employees should be associated with higher rates of concealment than former employees when disclosing negative ratings.

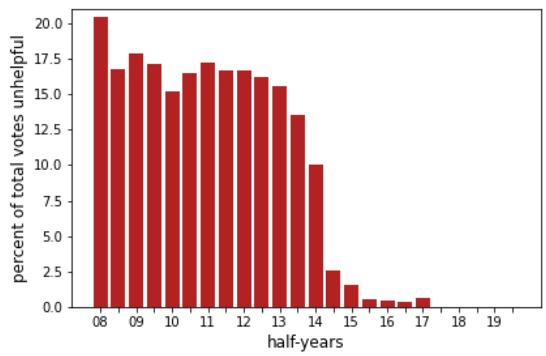
Workers in smaller firms face greater retaliation risks than workers from larger firms as the former have a thinner pool of candidates among whom to blend. How the altruistic benefits compare between workers at smaller and larger firms though is less clear. On one hand, a rating for a larger firm will contribute to informing more jobseekers than a rating from a smaller firm (less benefit). But, smaller firms have smaller workforces — in our model, smaller cardinality of $\{W\}^j$ — meaning each individual rating will be more pivotal in measuring σ^j (more benefit). Nevertheless, if concealment reduces the retaliation risk by more than it affects the altruistic benefits, then workers in smaller as opposed to larger firms would be more likely to choose concealment when relaying negative ratings.

B.3 Review helpfulness

The first employer reviews uploaded on the Glassdoor website are from mid-2008. At that time, users of the website could up-vote an employer review as helpful or down-vote the review as unhelpful, if they wanted to provide feedback for other users on the site about how they valued the information. The combination of these two measures—helpful and unhelpful votes—provides a review-specific metric of how helpful the review's content was to jobseekers. Starting in 2015, however, the option to down-vote an employer review as unhelpful was phased out on the website. Looking at the share of total helpfulness votes submitted each year that were attributable to unhelpful votes shows a clear structural break at the end of 2014 (Figure B.3). This motivates the decision to restrict the sample of reviews for which the share helpful, $\frac{\#helpful}{\#helpful+\#unhelpful}$, is the measure of interest to pre-2015.

The ability to submit unhelpful votes was not completely eliminated from the platform, however. As evidenced in Figure B.3, unhelpful votes still accounted for a sliver of total helpfulness votes submitted (under 2 percent) from 2015 through the first half of 2017. These unhelpful votes were generated through users submitting down-votes through Glassdoor's mobile application, which is accessible via phone or tablet. The dataset that tracks individuals' helpfulness votes stems from this period. By the end of 2017, the option to down-vote an employer review as unhelpful was completely phased out.

Figure B.3: Share of Total Votes Unhelpful over Time



Notes: When reading a user-written review of an employer on the Glassdoor website, the reader can signal to others that the review supplied (un)informative information by submitting a (un)helpful vote for the review. This figure calculates the fraction of submitted votes that were unhelpful votes, partitioning reviews into half-years bins based on the date the review was submitted.

B.4 Additional Figures and Tables for Chapter 2

Table B.2: Summary Statistics for Employer Reviews Dataset

Measure of interest	N	Mean	Standard deviation	p10	p90
A. Dependent variables					
Star rating	6,809,319	3.35	1.40	1	5
Helpful votes	6,809,319	1.24	3.99	0	4
Helpful votes pre-2015	685,505	2.76	6.59	1	6
Unhelpful votes pre-2015	685,505	0.34	1.09	0	1
Share helpful votes pre-2015	685,505	0.86	0.31	0.29	1
B. Firm characteristics					
Firm age (years)	5,334,194	56.3	51.8	10	131
Firm employment (1000s)	6,408,571	53.8	199.6	37.0	140.0
C. Panel of users with multiple reviews					
Next star rating	441,570	2.91	1.60	1	5
Delta star rating	441,570	0.002	1.84	-2	2
Delta helpful votes pre-2015	47,981	0.812	7.41	-3	5
Delta share helpful pre-2015	18,592	2.79	34.5	-20.0	40.0
D. Individual (un)helpful votes					
Voted helpful	182,410	0.852	–	–	–
Review's rating of employer	182,410	2.34	1.39	1	5

Measure of interest	N	Mean	Standard deviation	p10	p90
E. Text-based variables					
Pro share of text	6,809,319	0.468	0.196	0.191	0.723
Character length	6,809,217	336.5	420.8	78	706
Polarity of text	6,809,217	0.22	0.24	-0.04	0.52
Subjectivity of text	6,809,217	0.54	0.17	0.35	0.75
Flesch Kincaid grade	6,158,522	9.2	4.1	4.4	15.1
Language concreteness	1,409,215	2.946	0.132	2.778	3.102
F. Dummy variables					
Would recommend to a friend	5,621,590	0.5961	–	–	–
Approves of the CEO	4,157,888	0.4822	–	–	–
Positive business outlook	5,065,925	0.4815	–	–	–
Current employee	6,809,319	0.5229	–	–	–
Job title blank	6,809,319	0.4401	–	–	–
Job title anonymized	6,809,319	0.0057	–	–	–
Location blank	6,809,319	0.4222	–	–	–
Conceal either	6,809,319	0.5328	–	–	–
G. Labor market conditions					
State unemployment rate	3,929,775	5.29	1.73	3.5	7.8

1. Cross-section of employer reviews

Dimension of interest	Sample coverage
Horizon window	2008-23 to 2019-35
Reviews (pre-2015)	6,809,319 (1,417,494)
Users	6,201,251
Employers	463,968
Job titles	400,279
Helpful votes (pre-2015)	8,459,920 (1,895,376)
Unhelpful votes pre-2015	231,837

2. Individual (un)helpful votes

Dimension of interest	Sample coverage
Horizon window	Jan 2015–Sep 2017
Voted on reviews	92,320
Reviewed employers	10,117
Voting users	6,396
Helpful votes	155,490
Unhelpful votes	26,920

Notes: This table reports summary statistics and group counts for user-submitted employer reviews on the website Glassdoor. Panel of users with multiple reviews excludes job stayers as well as job switchers who submit both reviews in the same year-week. Individual (un)helpful votes comprised of a subsample of voters for whom: (i) data on each up-vote and down-vote are available, and (ii) an employer review was also submitted on the website before submitting the up- or down-vote.

Table B.3: Likelihood of Concealing Identifying Information by Employer Rating

	𝟙{Conceal aspect of identity}			
Star rating	-0.013***	-0.012***	-0.026***	-0.025***
	(0.000)	(0.000)	(0.001)	(0.001)
Current employee	0.006***	0.009***	0.043***	0.044***
	(0.001)	(0.001)	(0.002)	(0.002)
——— x 𝟙{1–2 star rating}		0.039***		0.014***
		(0.001)		(0.003)
Log(employment)	-0.007***	-0.008***	-0.015***	-0.015***
	(0.000)	(0.000)	(0.000)	(0.000)
——— x 𝟙{1–2 star rating}		-0.012***		-0.010***
		(0.000)		(0.001)
Worker FE			✓	✓
Sample mean	0.532	0.532	0.480	0.480
N	6372821	6372821	989996	989996
Adjusted R^2	0.04	0.05	0.26	0.26

Notes: The table above shows that current employees and workers from small firms are increasingly likely to conceal aspects of their identity when they are supplying negative information. Overall star rating, dummy for is current employee, and the logarithm of employment are demeaned by their sample averages. Each specification includes volunteer, industry and year-month fixed effects. Standard errors are clustered at the employer level. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.4: Dates and Directions of Concealment Probability Shifts: Instrument Definitions

			1(Concealment More Available)			
	Probability Shift?		Job Title		Location	
Year-Week	Job Title	Location	Before	After	Before	After
2009w18	Up	No	0	1	0	0
2009w22	No	Up	0	0	0	1
2009w48	No	Down	0	0	1	0
2012w21	Down	No	1	0	0	0
2013w3	Up	Up	0	1	0	1

Notes: The table lists the dates when discontinuous changes in job title or location concealment probabilities occurred, which we exploit for the quasi-experimental analysis in Table 2.5 and Figure 2.6 that focuses on the sample of reviews submitted within a given bandwidth of weeks around these dates. The definitions for two instrumental variables based on reviews arriving on either side of the five dates are shown.

Table B.5: Option to Conceal and Actual Concealment, First Stage of IV

	$\mathbb{1}$\{Conceal aspect of identity\}		
	(1)	(2)	(3)
Conceal either more available	0.230***		
	(0.004)		
Conceal location more available		0.111***	
		(0.007)	
Conceal job title more available		0.176***	
		(0.005)	
Conceal job title more available only			0.133***
			(0.010)
Conceal location more available only			0.183***
			(0.006)
Conceal both more available			0.284***
			(0.005)
N	77176	77176	77176
Adjusted R^2	0.06	0.06	0.06

Notes: The table above shows the effect that having the option to conceal identifying aspects has on the rates at which workers conceal identifying aspects, i.e. the first stage of the IV regression. Sample is restricted to reviews that arrive within four weeks before and after each removal/introduction of a concealment option. Standard errors are clustered by firm. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.6: When Employees Conceal Identity, Robustness in Choice of Instruments

	Star rating			$\mathbb{1}$\{Current employee\}			Log(employment)			Share of review votes helpful		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Conceals aspect of identity	-0.275***	-0.260***	-0.261***	0.058***	0.043**	0.044**	-0.626***	-0.484***	-0.499***	-0.021	-0.032	-0.030
	(0.051)	(0.052)	(0.051)	(0.019)	(0.019)	(0.019)	(0.130)	(0.122)	(0.122)	(0.022)	(0.022)	(0.022)
Star rating										-0.105***	-0.105***	-0.105***
										(0.003)	(0.003)	(0.003)
Specification	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
N	77176	77176	77176	77176	77176	77176	71368	71368	71368	39119	39119	39119
Adjusted R^2	-0.00	0.00	0.00	0.00	0.00	0.00	0.02	0.02	0.02	0.17	0.17	0.17
F statistic	18.10	18.74	18.76	31.18	30.08	30.14	60.59	58.03	58.26	216.73	220.12	219.21
Anderson-Rubin chi-sq	29.87	29.91	31.12	9.19	28.32	28.39	22.77	16.66	26.18	0.88	2.82	4.74

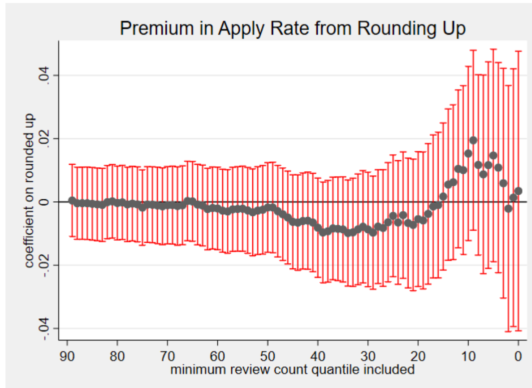
Notes: The table above repeats the IV regressions from Table 2.5 using each of the combination of instruments in Table B.5, where each set of instruments is demarcated by the respective column number. Sample is restricted to reviews that arrive within four weeks before and after each removal/introduction of a concealment option. Standard errors are clustered by firm. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.7: Predicting Helpful Vote Count of Volunteer's Review

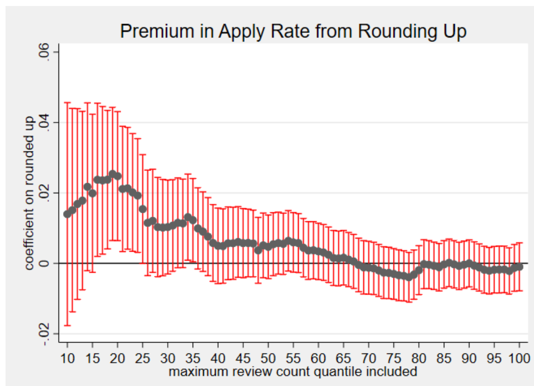
	Logarithm of helpful votes + 1						
Star rating	-0.157***						-0.094***
	(0.002)						(0.001)
Would recommend employer to friend		-0.453***					-0.198***
		(0.007)					(0.005)
Approves of the CEO			-0.312***				-0.024***
			(0.006)				(0.001)
Positive business outlook for the firm				-0.321***			-0.007***
				(0.005)			(0.001)
Pro share of review text					-0.860***		-0.321***
					(0.009)		(0.004)
Polarity of text						-0.270***	0.005***
						(0.005)	(0.002)
Log character length of review	0.190***	0.194***	0.236***	0.224***	0.189***	0.226***	0.187***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Flesch-Kincaid reading grade	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.003***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Subjectivity of text	0.003**	-0.014***	-0.035***	-0.027***	-0.020***	0.048***	0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Employer, year-month FE	✓	✓	✓	✓	✓	✓	✓
N	5958112	4849978	3642220	4373338	5958112	5958112	3191431
Adjusted R^2	0.47	0.47	0.42	0.44	0.44	0.39	0.50

Notes: The table above predicts the helpfulness of an employer review using the logarithm of review votes helpful plus one as the dependent variable. Sample is restricted to reviews for which the Flesch Kincaid reading grade is non-negative and no greater than 20. Polarity and subjectivity of each review are measured through natural language processing using the *TextBlob* library in Python. Standard errors clustered at the employer level. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively. For a summary of the dataset, see Table B.2.

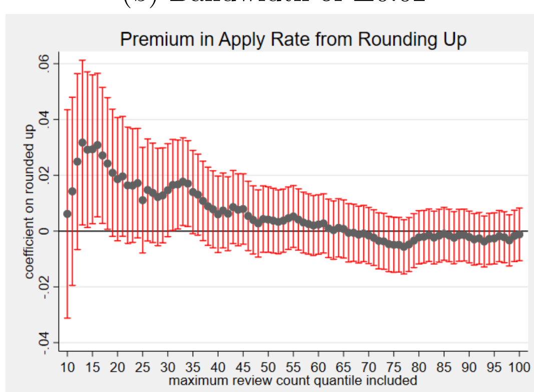
Figure B.4: Alternative Specifications for Effect of 0.1-Star Higher Rating on Apply Rate



(a) Bandwidth of ±0.01 with reverse ordering of review-count quantiles



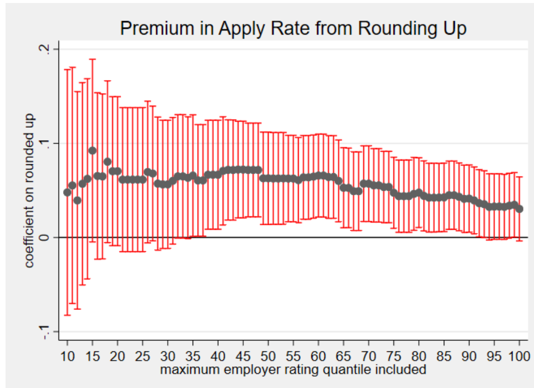
(b) Bandwidth of ±0.02



(c) Bandwidth of ±0.04 with pre- and post-cutoff slope parameters in running variable

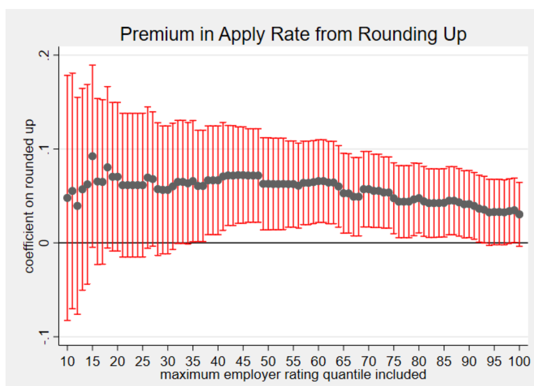
Notes: Sample is restricted to job postings for which the employers' weekly rating's hundredths places falls within bandwidth of a #.#5 cutoff. Running variable refers to the difference between employer rating and the nearest cutoff. Each job posting is weighted by its impression total. Regressions include employer, job title, metro, day-of-posting, and #.#5 threshold fixed effects. Vertical red bars indicate a 95% confidence interval around each point estimate. Standard errors clustered by firm.

Figure B.5: Effect of 0.1-Star Higher Rating on Apply Rate Partitioned by Employer Rating Among Job Postings in Bottom Quintile of Review Count



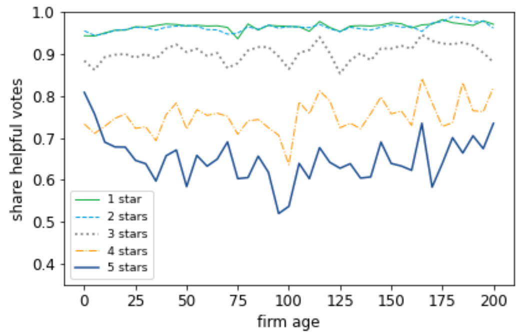
Notes: Sample is restricted to the bottom quintile by review count of job postings for which the employers' weekly rating's hundredths places falls within a ±0.01 bandwidth of a #.#5 cutoff. Numbers on the x-axis refer to the maximum employer rating percentile included in the regression sample. Each job posting is weighted by its impression total. Regressions include employer, job title, metro, day-of-posting, and #.#5 threshold fixed effects. Vertical red bars indicate a 95% confidence interval around each point estimate. Standard errors clustered by firm.

Figure B.6: Effect of 0.1-Star Higher Rating on Apply Rate Partitioned by Employer Rating Among Job Postings in Bottom Quintile of Review Count

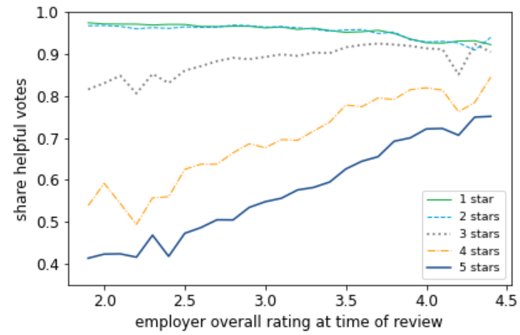


Notes: Sample is restricted to the bottom quintile by review count of job postings for which the employers' weekly rating's hundredths places falls within a ±0.01 bandwidth of a #.#5 cutoff. Numbers on the x-axis refer to the maximum employer rating percentile included in the regression sample. Each job posting is weighted by its impression total. Regressions include employer, job title, metro, day-of-posting, and #.#5 threshold fixed effects. Vertical red bars indicate a 95% confidence interval around each point estimate. Standard errors clustered by firm.

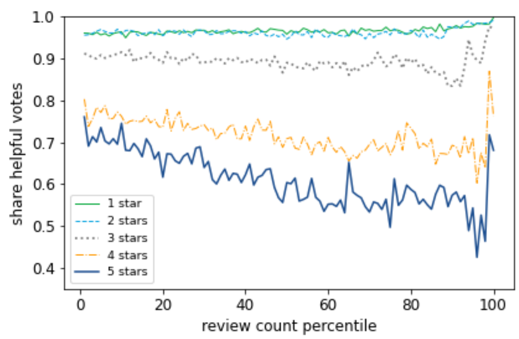
Figure B.7: Share Helpful Votes by Review's Rating and Firm Demographics



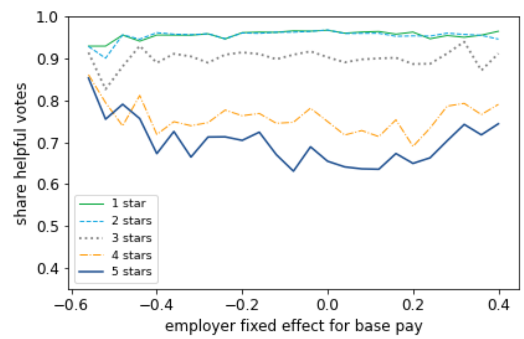
(a) Firm age



(b) Firm overall rating



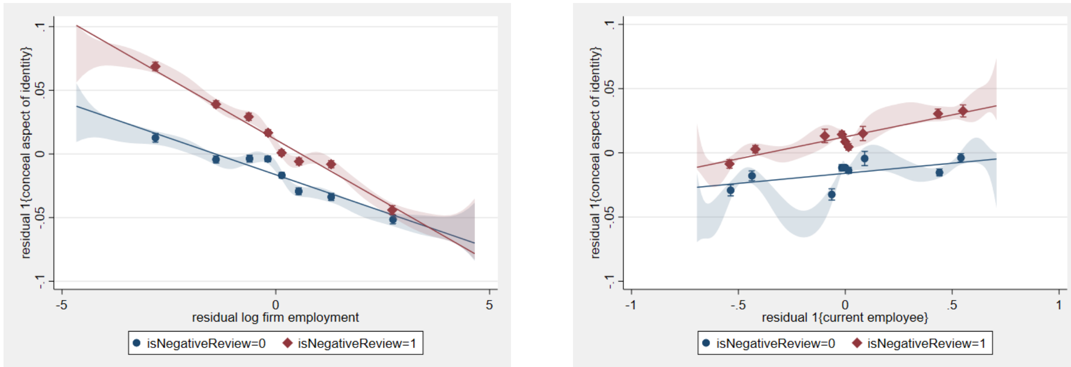
(c) Firm review count



(d) Firm FE for base pay

Notes: Figures display the average share helpful votes across firms partitioned by the star rating of the review. Firm age is calculated as the difference between the year in which the review is submitted and the year in which the firm was founded (which is recorded in an employer lookup table from 2019). Sample is restricted to reviews submitted before 2015 (see Appendix B.3). Sample for panels (b) and (c) restricted to reviews submitted for firms that have had at least 10 reviews submitted prior. Firm FE for base pay, derived using Glassdoor pay data, are from a regression of annualized log base pay on a quadratic in years of specific experience and fixed effects for year, state, gender, educational attainment, pay frequency, industry-job title pairing, and employer. Firm age is grouped into five-year bins. Firm overall rating is grouped into bins rounded to the nearest tenth. Firm FE for base pay is grouped into 0.04 bins. For each panel, bins in which one of the five ratings represents fewer than 150 unique reviews are excluded.

Figure B.8: Relationships Between Probability of Identity-Aspect Concealment and Leaving a More-Negative Review by Measures of Retaliation Risk

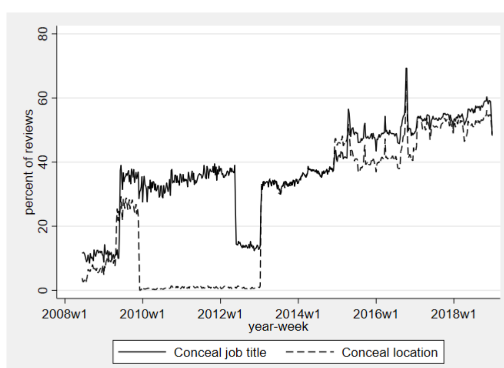


(a) Employer size

(b) Current employee

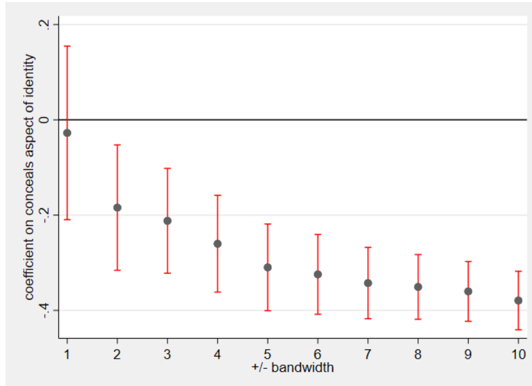
Notes: The figures above detail the rate at which volunteers conceal potentially identifying information depending upon the size of their employers (panel a) or whether they are current employees (panel b). We first residualize an indicator for concealing on the main effects of review rating, log firm size, a current employee indicator, and fixed effects for the volunteer, the year–month, and the firm's industry. We also include the interaction of the other retaliation risk measure—current employee status for panel (a) and firm size for panel (b)—with an indicator of negative review. Only the interaction term between an indicator of a negative review and the focal measure of retaliation risk—log firm size for panel (a) and current employee status for panel (b)—is excluded from the model. Regressions are implemented bin-wise by the residualized interaction between the focal measure of retaliation risk and the indicator for leaving a negative review. Sample of volunteers is restricted to those who leave multiple reviews. Negative reviews (red diamonds) refer to one- and two-star reviews while non-negative reviews (blue circles) refer to four- and five-star reviews. Solid lines reflects linear lines of best fit and shaded regions reflect 95% confidence intervals.

Figure B.9: Rates of Job Title and Location Concealment, Full Date Range

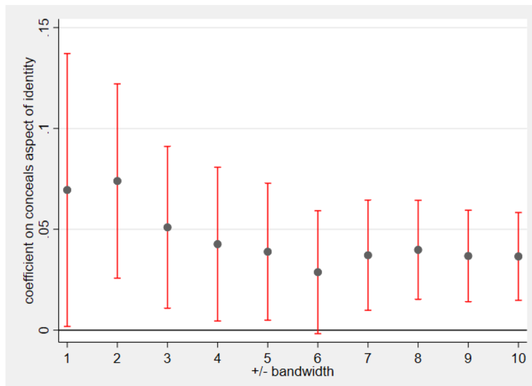


Notes: The figure above plots the shares of reviews in each calendar year-week that conceal each identity aspect, job title or location. The sample window spans from the twenty-fourth week of 2008 through the final week of 2018.

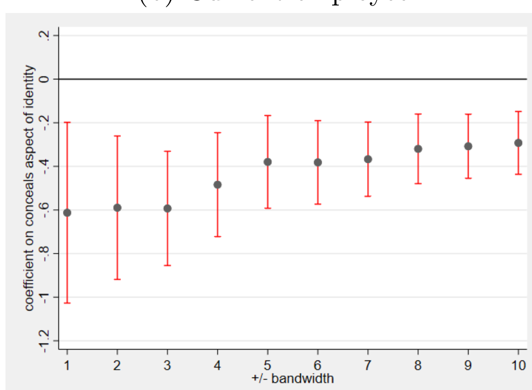
Figure B.10: Robustness to Varying the Bandwidth for IV Specifications



(a) Overall rating



(b) Current employee



(c) Log employment

Notes: Sample is restricted to reviews that arrive within ± bandwidth weeks before/after each increase/decrease in the availability of identity concealment options. Vertical red bars indicate a 95% confidence interval around each point estimate. Standard errors are clustered by firm. Regressions with a bandwidth of ±4 are analogous to the results displayed in Table 2.5.

APPENDIX C

Additional Results for Chapter 3

C.1 Scandal Information

In this appendix, we further describe the construction of our corporate scandals sample. We first aggregate annual lists of "corporate misdeeds" published by Fortune Magazine and Yahoo! Finance. We are unable to locate such a list for the year 2014, so we utilize a list published by Inc. Magazine instead. We supplement these events with allegations of sexual harassment levied against corporate CEOs and chairmen documented by the New York Times in 2018. We then apply a set of filters to ensure that our scandals are legitimate shocks to firm reputation. We exclude events that do not involve any wrongdoing (e.g. the relationship between Sergei Brin and a Google employee), impact several firms, or are continuations of prior events (e.g. J.P. Morgan paying fines for past transgressions). When a firm appears on multiple lists, we use the first event that satisfies the aforementioned criteria. While the persistence of our results may be partially driven by this restriction, the majority of firms in our sample only suffer a single scandal. Furthermore, subsequent events are often directly related to the first or come to light precisely because of the increased scrutiny brought about by the initial scandal. Finally, we exclude events involving firms that do not have an appreciable presence in any segment of Glassdoor data. Table C.1 provides links to news articles summarizing each event as well as the underlying list from which each event was pulled.

Table C.1: Corporate Scandal Background Information

Company	Date Public	Background Article	Source
Carnival Cruise	02-10-2013	Passengers Face Two More Days of Squalor on Carnival Ship Stranded in the Gulf	Fortune
lululemon	03-18-2013	Recall Is Expensive Setback for Maker of Yoga Pants	Fortune
GlaxoSmithKline	07-11-2013	GlaxoSmithKline Accused of Corruption by China	Fortune
Macy's	10-24-2013	Profiling Complaints by Black Shoppers Followed Changes to Stores' Security Policies	Fortune
Sony	12-02-2014	Sony Films Are Pirated, and Hackers Leak Studio Salaries	Inc.
Toshiba	07-20-2015	Scandal Upends Toshiba's Lauded Reputation	Fortune
Volkswagen	09-18-2015	VW Is Said to Cheat on Diesel Emissions; U.S. to Order Big Recall	Fortune
Valeant	10-21-2015	Valeant's Shares Fall on Report's Fraud Claim	Fortune
Fox	07-06-2016	Gretchen Carlson of Fox News Files Harassment Suit Against Roger Ailes	Fortune
Mylan Inc	08-18-2016	Mylan Raised EpiPen's Price Before the Expected Arrival of a Generic	Fortune
Samsung	09-02-2016	Samsung to Recall 2.5 Million Galaxy Note 7s Over Battery Fires	Fortune
Wells Fargo	09-08-2016	Wells Fargo Fined $185 Million for Fraudulently Opening Accounts	Fortune
Uber	02-19-2017	Uber Investigating Sexual Harassment Claims by Ex-Employee	Fortune
United Airlines	04-10-2017	United Airlines Passenger Is Dragged From an Overbooked Flight	Fortune
Equifax	09-07-2017	Equifax Says Cyberattack May Have Affected 143 Million in the U.S.	Fortune
Apple	12-20-2017	Is Apple Slowing Down Old iPhones? Questions and Answers	Fortune
Wynn Resorts	01-25-2018	Stephen Wynn, Casino Mogul, Accused of Decades of Sexual Misconduct	New York Times
Guess?	02-01-2018	Guess Inc. responds to sexual harassment allegations against co-founder Paul Marciano	New York Times
Google	03-06-2018	Google Is Helping the Pentagon Build AI for Drones	Yahoo! Finance
Facebook	03-15-2018	Facebook's Role in Data Misuse Sets Off Storms on Two Continents	Yahoo! Finance
CBS	07-27-2018	Les Moonves and CBS Face Allegations of Sexual Misconduct	New York Times
Tesla	08-07-2018	Elon Musk Says Tesla May Go Private, and Its Stock Soars	Yahoo! Finance
Nissan	11-19-2018	Nissan Chairman, Carlos Ghosn, Is Arrested Over Financial Misconduct Allegations	Yahoo! Finance

Notes: This table provides additional information on the corporate scandals in our sample. It includes links to articles from the popular press detailing each event and the list from which each event was pulled.

C.2 Composition of Reviewers

Further evidence that our results are not driven by a compositional shift can be seen by re-estimating our differences-in-differences specifications with employee characteristics as our outcomes of interest. Using the wage dataset, we first test whether there is a shift in average worker quality following a scandal by testing whether the scandal causes a shift in workers' average human capital — as measured through years of actual experience or potential experience (age). Finding no significantly negative effect in Table C.2 suggests that the sample of firms' workers following the scandal are no less productive than those who were employed beforehand. For employee reviews, we test whether the sample of reviewers changes after a scandal by using the age of the reviewer (when available), along with indicators for whether the worker is a current employee, has been employed with the firm at most two years (low tenure), or conceals aspects of their identity (job title or location) when disclosing.[92] Interestingly, we do find a slight shift toward older and longer tenured workers, suggesting that these employees may feel most compelled to voice themselves after or most affected by corporate misconduct; however, the shift is also accompanied by an increased propensity to conceal self-identifying characteristics, suggesting some concern with being identified for speaking out.

Table C.2: Observable Characteristics of Reviewers After Scandals

	Junior position	Works at HQ	Female	Age	Long tenure	Conceal info
After scandal	-0.013	-0.017	-0.002	0.374***	0.023**	0.017**
	(0.009)	(0.013)	(0.008)	(0.121)	(0.009)	(0.007)
Pre-scandal mean	0.66	0.20	0.26	31.50	0.42	0.51
N	2390417	2848365	4473879	1302412	3106816	4872600
Scandal firm N	21213	25881	44738	14883	27605	46803
Adjusted R^2	0.18	0.45	0.10	0.16	0.09	0.07

Notes: This table reports coefficients when re-estimating difference-in-differences on the worker observable listed in each column. Regressions include firm and industry x year-month fixed effects. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

[92] Respondents who conceal aspects of their identity may by particularly concerned with retaliation risk (Sockin and Sojourner, 2020) and, thus, systematically differ from non-concealers.

Table C.3: Difference-in-Difference Results for Scandals, Additional Heterogeneity

	Overall rating	Culture and values	Senior management
Panel A: Job seniority			
Total effect: junior positions	-0.065*	-0.114***	-0.108***
	(0.039)	(0.034)	(0.033)
	[7858]	[7126]	[7054]
Total effect: senior positions	-0.078	-0.134**	-0.096*
	(0.056)	(0.063)	(0.055)
	[3944]	[3656]	[3642]
Panel B: Location			
Total effect: headquarters	-0.062	-0.195***	-0.088
	(0.068)	(0.066)	(0.059)
	[2429]	[2275]	[2258]
Total effect: non-headquarters	-0.072***	-0.099***	-0.096***
	(0.028)	(0.032)	(0.028)
	[11188]	[10415]	[10337]
Panel C: Employee gender			
Total effect: females	-0.065	-0.102**	-0.089**
	(0.042)	(0.042)	(0.040)
	[7201]	[6156]	[6071]
Total effect: males	-0.069***	-0.124***	-0.096***
	(0.019)	(0.026)	(0.023)
	[16662]	[13957]	[13870]
Panel D: Employee age			
Total effect: younger	-0.047*	-0.092***	-0.052
	(0.024)	(0.033)	(0.036)
	[4312]	[3584]	[3553]
Total effect: older	-0.100***	-0.120**	-0.104**
	(0.035)	(0.054)	(0.051)
	[3163]	[2794]	[2784]
Panel E: Firm tenure			
Total effect: short tenure	-0.059	-0.095**	-0.050
	(0.040)	(0.044)	(0.044)
	[9204]	[8396]	[8299]
Total effect: long tenure	-0.087***	-0.137***	-0.131***
	(0.030)	(0.029)	(0.030)
	[6277]	[5889]	[5871]
Panel F: Conceal identifying characteristics			
Total effect: conceals	-0.035	-0.079**	-0.053*
	(0.027)	(0.031)	(0.030)
	[13302]	[9927]	[9828]
Total effect: does not conceal	-0.108***	-0.147***	-0.129***
	(0.027)	(0.032)	(0.029)
	[11999]	[11293]	[11209]

Notes: This table reports coefficient estimates for the effects following a scandal on different partitions of reviewers. The pre- and post-periods are each 24 months. Regressions include firm, industry x year-month, current employee, and employment status fixed effects. To account for the partitioned characteristic, the following indicators are included in each panel respectively: junior position, at headquarters, female, younger (ages 18–30), short tenure (at most two years), and conceals job title or location. Standard errors are clustered by firm. Sample counts for scandal-hit in the post-period are given in brackets. Significance levels: * 10%, ** 5%, *** 1%.

C.3 Elevated News Coverage

In this appendix, we demonstrate that elevated news coverage does not explain our results. To do so, we contrast the effects of scandals with those of data breaches, another type of negative, firm-specific shock that generates increased media attention. While breaches reflect poorly on firm management, they do not necessarily involve misconduct or reveal information about firm culture. Our sample of such events comes from Privacy Rights Clearinghouse, which defines a data breach as a "security violation in which sensitive, protected or confidential data is copied, transmitted, viewed, stolen or used by an unauthorized individual." We restrict attention to firms that experienced a loss of at least one million records between 2013 and 2018 and are well-covered on Glassdoor. The 27 breaches that fit the criteria are listed in Table C.4. [93]

We use data from Google Trends to validate that scandals and breaches generate heightened news coverage. The web application provides daily scores reflecting the relative search intensity for particular keywords within a specified date window. Scores range from 0 to 100, with 100 representing the peak intensity over the period of interest. For each scandal-hit and breach-hit firm in our sample, we pull the news category trend score for a 60-day window centered around the event date. The

[93] Two of our scandals are, in fact, data breaches. The Equifax hack was particularly damaging to firm reputation due to its magnitude and the fact it was covered up by executives for several months. The Sony breach was harmful despite the loss of comparatively few records, because hackers leaked compromising emails written by executives.

Table C.4: Summary of Breach Employer Samples

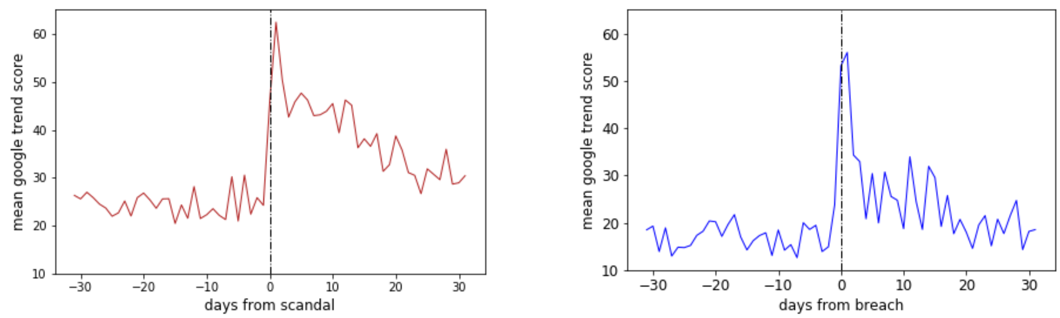
Event date	Employer	Description	CEO exits	Employer reviews	Base pay	Variable pay	Benefits reviews
October 04, 2013	Adobe	2.9m records	N	600	1,231	770	–
December 13, 2013	Target	40.0m records	05/05/2014	11,632	13,532	3,166	–
January 10, 2014	Neiman Marcus	1.1m records	N	472	687	109	–
January 25, 2014	Michaels	2.6m records	N	1,360	810	199	–
May 21, 2014	eBay	145.0m records	N	923	1,532	1,013	–
August 18, 2014	Community Health Systems	4.5m records	N	477	567	59	–
August 28, 2014	J.P. Morgan	76.0m records	N	6,402	11,624	7,251	–
September 02, 2014	The Home Depot	56.0m records	N	9,495	7,847	3,498	–
October 20, 2014	Staples	1.2m records	N	3,555	2,996	1,047	–
February 05, 2015	Anthem	80.0m records	N	316	254	112	–
March 17, 2015	Premera Blue Cross	11.0m records	N	294	338	159	–
May 20, 2015	Carefirst	1.1m records	N	181	258	123	–
July 17, 2015	UCLA Health	4.5m records	N	283	456	55	–
October 01, 2015	Experian	15.0m records	N	598	690	441	–
May 17, 2016	LinkedIn	117.0m records	N	991	1,774	1,260	332
June 13, 2016	Twitter	32.0m records	N	486	790	331	162
August 03, 2016	Banner Health	3.6m records	N	971	1,671	97	286
September 22, 2016	Yahoo!	500.0m records	N	1,415	2,255	1,447	479
September 07, 2017	Equifax	145.5m records	09/26/2017	464	820	425	166
October 12, 2017	T-Mobile	69.6m records	N	4,971	5,488	3,057	1,667
March 30, 2018	Under Armour	150.0m records	N	807	1,022	375	251
April 01, 2018	Lord & Taylor's	5.0m records	N	417	371	47	120
April 01, 2018	Saks	5.0m records	N	877	1,292	228	272
April 20, 2018	SunTrust	1.5m records	N	1,208	2,143	866	417
June 28, 2018	adidas	2.0m records	N	679	1,100	345	225
October 01, 2018	Chegg	40.0m records	N	188	251	55	73
November 30, 2018	Marriott International	327.0m records	N	3,864	9,229	1,240	1,283

Notes: This table describes each of the data breaches in the sample. The list comes from Privacy Rights Clearinghouse and are restricted to hacks of at least one million individual records. Observation counts are provided in windows around event dates. For employer reviews, windows range from 24 months before through 24 months after each event; for pay reports, four years before through four years after; and for benefits reviews, three years before through three years after.

average score across firms for each event type are plotted in Figure C.1. The large jumps when scandals and breaches become public confirm that both are salient, unanticipated shocks that garner media attention.

Table C.5 reports results when we estimate the baseline rating and wage regressions on the sample of data breaches. The coefficient estimates are small and insignificant across the set of outcomes derived from employee reviews, suggesting that negative publicity does not necessarily cause declines in employee sentiment. We also find no decrease in any of the compensation variables. It appears that impropriety, not news coverage drives our results.

Figure C.1: Google Trends Scores around Event Dates



Corporate Scandals   Data Breaches

Notes: These figures display equally-weighted average Google Trends news scores for firm names in the 60-days around scandal and breach dates. Google Trends scores are normalized to range from 0 to 100, with 100 representing the peak intensity over the queried time period.

Table C.5: Difference-in-Difference Results for Data Breaches

	Employee reviews				Employee wages			
	Overall rating	Culture & values	Senior mgmt.	Would refer a friend	Base pay	Variable pay	Earns VP	Benefits rating
After breach	-0.043	-0.035	-0.055	-0.017	-0.001	-0.023	0.006	-0.021
	(0.029)	(0.037)	(0.035)	(0.013)	(0.009)	(0.036)	(0.007)	(0.027)
Pre-breach mean	3.45	3.50	3.01	0.66	10.89	9.28	0.49	3.94
N	4879723	4119107	4083763	3847073	4424839	1155908	4674366	794574
Breach firm N	53926	47092	46774	44487	53348	19584	58240	5733
Adjusted R^2	0.17	0.17	0.15	0.14	0.83	0.64	0.32	0.25

Notes: This table reports coefficients when Equations 3.3.1 and 3.4.1 are estimated on the sample of data breaches. The control group and control variables are the same as in the benchmark regressions for corporate scandals. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

C.4 Labor Productivity

The persistent decline in compensation after scandals suggests that misconduct may negatively impact labor productivity. The loss of reputation after such events could, for example, hurt sales by eroding relationships with clients. Alternatively, lower job satisfaction could prompt employees to exert less effort (McGregor, 1960). While we cannot distinguish between the various channels, we directly test the underlying hypothesis using balance sheet data from Compustat. Formally, we estimate the differences-in-differences regression

$$y_{kt} = \beta \cdot PostScandal_{kt} + \lambda X_{kt} + \gamma_k + \gamma_{\iota(k)t} + \epsilon_{kt} \tag{C.4.1}$$

where y_{kt} is average labor productivity for firm k in quarter t, γ_k is a firm fixed effect, $\gamma_{\iota(k)t}$ is an industry-quarter fixed effect, X_{kt} is log assets, and $PostScandal_{kt}$ is an indicator equal to one if firm k faced a scandal prior to quarter t. The event window extends from four years prior to four years after a scandal. The control group is comprised of all firms in Compustat that experience neither a scandal nor breach. Standard errors are again clustered by firm. We follow Cronqvist et al. (2009) and define average labor productivity as the natural log of sales per employee. We also test for changes in the number of total employees at the firm, though we note this measure is only available at an annual frequency.

Results are presented in Table C.6. The estimate in Column 1 of Panel A indicates that average labor productivity drops by 9 percent in the years following a scandal. The magnitude of the decline is similar when we consider only non-fraud scandals, but the coefficient is significant only at the 10 percent level. The decrease is also consistent with the reduction in variable compensation. As shown in Column, we find no evidence of changes in the number of employees following scandals.

Table C.6: Firm-Level Outcomes Following Scandals

	Corporate scandals		Corporate scandals excluding fraud	
	Log sales per worker	Log employment	Log sales per worker	Log employment
After scandal	-0.086*	0.104	-0.091**	0.113
	(0.047)	(0.076)	(0.045)	(0.087)
Pre-scandal mean	11.55	10.69	11.62	10.55
Dependent var. mean	11.30	6.45	11.30	6.45
Dependent var. std. dev.	1.34	2.72	1.34	2.72
Control firms	10426	10522	10426	10522
N	275765	77862	275609	77823
Scandal firm N	553	138	397	99
Adjusted R^2	0.83	0.98	0.83	0.98

Notes: This table reports coefficients when Equation C.4.1 is estimated on the dependent variable in each column heading. The event window spans from four years prior to four years after a scandal. The log sales-per-worker regressions are at the firm-quarter level, while the log employment regressions are at the firm-year level. The independent variables are log assets and fixed effects for firm and industry–year, where industry is based on six-digit GICS codes. Standard errors are clustered by firm. One, two, and three stars denote significance at the 10, 5, and 1 percent levels, respectively. The scandal-hit firms included in this analysis are: Apple, CBS, Carnival, Equifax, Fox, Facebook, GlaxoSmithKline, Google, Guess?, Macy's, Mylan Inc, Nissan, Sony, Tesla, Toshiba, Valeant, Wells Fargo, Wynn Resorts, and lululemon.

C.5 Scandals and Job Seeker Behavior

In this appendix, we study how facing a corporate scandal affects a firm's ability to attract job seekers. In addition to submitting reviews, users on Glassdoor can search for and apply to jobs.[94] The website's platform presents job seekers with a list of job openings based on their search criteria. A job posting being displayed to a user constitutes an *impression* for that listing, regardless of whether the user chooses to explore it further. If the user ultimately begins an application for the vacancy, this constitutes an *apply.* We do not observe if users complete applications.

For each job listing, we calculate the total impressions and applies over a 72-hour window beginning when the posting is first listed on Glassdoor. Each user contributes at most once to a job listing's totals.[95] We restrict our sample to active job postings by considering only listings with at least ten unique impressions. We also exclude postings for which the metropolitan location of the vacancy is unavailable and those made by firms with fewer than ten employer reviews prior to the listing date. After imposing these filters, our sample consists of 9.60 million listings from large employers. Search data are available from March 2017 through

[94] Glassdoor aggregates job postings from online job boards, applicant tracking systems, and company websites, capturing about 81 percent of total U.S. job openings as measured by the Job Openings and Labor Turnover Survey (Chamberlain and Zhao, 2019).

[95] A user is a jobseeker with a registered profile, and can thus be identified across sessions. We impose the 72-hour window in order to mitigate trends in search intensity over the life cycle of a vacancy. For certain days, we are unable to calculate totals over a 72-hour window and must resort to either a 48-hour or 24-hour window. We incorporate day-of-posting fixed effects in our regressions in part to account for this issue.

August 2019, which limits our sample to eight scandals.[96]

To test if scandals affect job seeker behavior, we estimate difference-in-differences regressions with and apply rates (applies per hundred impressions) as the dependent variables. The formal specification is

$$r_{ijkmt} = \sum_{\tau \neq 2} \beta_\tau \cdot ScandalFirm_{k\{t \in \tau\}} + \gamma_k + \gamma_{\iota(k)jq(t)} + \gamma_{mq(t)} + \gamma_t + \epsilon_{ijkmt} \quad \text{(C.5.1)}$$

where r_{ijkmt} is the apply rate for job listing i advertising job title j at firm k in metro m posted on Glassdoor on calendar date t, γ_k is a firm fixed effect, $\gamma_{\iota(k)jq(t)}$ is an industry-job title-quarter fixed effect, $\gamma_{mq(t)}$ is a metro-quarter fixed effect, γ_t is a date-of-posting fixed effect, $ScandalFirm_{k\{t \in \tau\}}$ is an indicator equal to one if firm k experienced a scandal and calendar date t is in firm k's event-time bin τ. The τ subscripts correspond to two-month bins beginning four months prior and ending twelve months following a scandal. The β_τ coefficient estimates therefore measure differences in apply rates relative to the omitted bin, $\tau = 2$, which consists of the two calendar months immediately preceding an event. In order to avoid undue influence from less visible postings, we weight each job listing by its impression count. The set of control firms consists of the 17,969 large employers with job postings on Glassdoor that are in neither the corporate scandal nor data breach

[96]The eight firms are: Apple (5289 / 259,000), CBS (6324 / 265,000), Equifax (2,342 / 71,000), Facebook (1517 / 99,000), Google (3,617 / 246,000), Guess? (2,843 / 60,000), Tesla (6957 / 203,000), and Wynn Resorts (641 / 14,000). The figures in parentheses are, respectively, the total job listings and unique impressions for each firm in the period four months prior to twelve months after a scandal.

samples. If scandals reduce interest in firms, the β_τ coefficients will be negative in the periods following an event.

Figure C.2 shows that application rates fall in the immediate aftermath of scandals. The point estimate of –0.172 in the event month represents a 20 percent decrease relative to the mean apply rate of 0.87% in the reference period. While the drop is large, the coefficients recover to their initial levels after six months. While scandals may damage employee referral networks for a sustained period of time, it does not appear to have a long-term affect on external job seekers. The estimates in the pre-event bins are stable, suggesting that the assumption of parallel trends underlying our analysis holds.

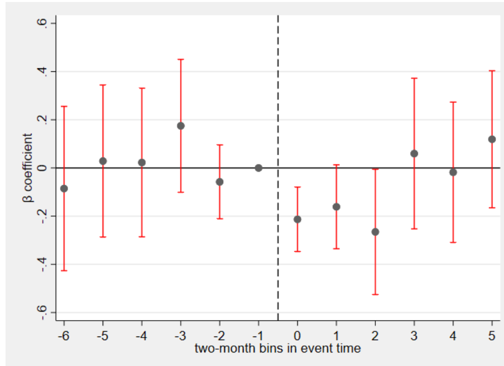
While suggestive, our findings should be taken with caution. Due to data limitations, we are only able to study a limited set of scandals. Further, job search is not typically the primary function associated with Glassdoor. We consider the relationship between misconduct and job seeker behavior to be an interesting topic for further research.

C.6 Robustness Results

C.6.1 Control Sample Includes Small Firms

Given that scandal-hit firms are large and our identification strategy requires a comparable set of control firms, we restrict our control sample to comparably large

Figure C.2: Search Activity for Job Postings after Corporate Scandals



(a) Apply rate

Notes: The panels above display coefficients from Equation C.5.1 for apply rate within a 24-month window around scandals. Horizontal dashes indicate a 95 percent confidence interval around each point estimate. Regressions are weighted by each job posting's impression total and include firm, industry x job title x quarter, metro x quarter, and date-of-posting fixed effects. Each coefficient is relative to the two months prior to the scandal. Standard errors are clustered by firm.

firms. However, we can re-estimate our baseline regressions with control samples comprised of all employers so that the complete Glassdoor dataset is utilized. The difference-in-difference estimates for our main outcomes using this alternate control sample, presented in table C.7, are consistent with those from our primary specifications.

Table C.7: Robustness Results, Control Sample Comprised of All Firms

	Employee reviews				Employee wages			
	Overall rating	Culture & values	Senior mgmt.	Would refer a friend	Base pay	Variable pay	Earns VP	Benefits rating
After scandal	-0.061**	-0.107***	-0.083***	-0.022**	-0.017	-0.108***	-0.059*	-0.039
	(0.025)	(0.028)	(0.027)	(0.009)	(0.013)	(0.035)	(0.033)	(0.054)

Notes: Sample for each specification includes all firms that experienced neither a scandal nor a breach in the control sample. Each specification is identical to the one implemented in the main text. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

C.6.2 Fixed-Sample Composition

We next show that our results are not driven by changes in sample composition over time. We re-estimate each differences-in-differences regression, but set each scandal firms' sample share in the post-event period equal to it's sample share in the pre-event period. The results, presented in table C.8, are broadly unchanged from our benchmark.

Table C.8: Weighted Difference-in-Differences Results

	Employee reviews				Employee wages			
	Overall rating	Culture & values	Senior mgmt.	Would refer a friend	Base pay	Variable pay	Earns VP	Benefits rating
After scandal	-0.066**	-0.113***	-0.091***	-0.020**	-0.012	-0.095***	-0.047*	-0.036
	(0.026)	(0.029)	(0.028)	(0.009)	(0.012)	(0.032)	(0.027)	(0.048)

Notes: This table reports the difference-in-differences coefficients for the dependent variable listed in each column. Regressions are weighted such that each scandal firm's share of the sample in the post-event period is held constant at it's pre-event share. Each regression specification uses the same controls as those used in the benchmark. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

C.6.3 Stacked Events

First, for each scandal, we create a separate dataset that includes the observations for that scandal firm and all of the control firms. Second, we append together all of the separate datasets. We then aggregate across worker-level observations to the level of our fixed effects within each dataset for ease of computation. For employer reviews, we aggregate to the firm–month level. For employee wages, we aggregate to the firm x industry-job-year level. (For computational feasibility, we exclude state-year level controls from the wage specifications.) For fringe benefits reviews, we aggregate to the firm x industry-year level. We then implement our difference-in-differences specification adding a fixed effect for each dataset. Reassuringly, the stacked results, presented in Table C.9, exhibit similar results to our benchmark estimates.

Table C.9: Stacked Difference-in-Difference Results for Corporate Scandals

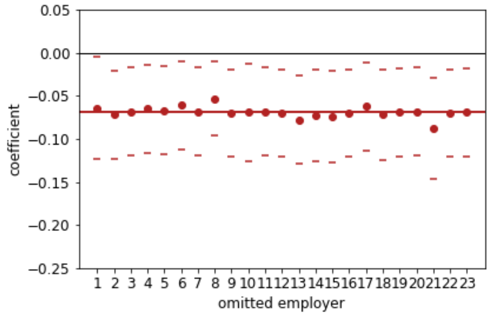
	Employee reviews				Employee wages			
	Overall rating	Culture & values	Senior mgmt.	Would refer a friend	Base pay	Variable pay	Earns VP	Benefits rating
After scandal	-0.068**	-0.116***	-0.096***	-0.021**	-0.013	-0.120***	-0.043	-0.036
	(0.028)	(0.031)	(0.030)	(0.010)	(0.014)	(0.034)	(0.032)	(0.061)
Pre-scandal mean	3.50	3.37	3.06	0.65	11.29	9.91	0.51	3.90
N	24814501	22949130	22864788	22231874	52920825	14583623	55112072	1523396
Scandal firm N	1043	1041	1040	1040	16777	7626	17619	107

Notes: This table reports the difference-in-differences coefficients for the dependent variable listed in each column. Regressions include the fixed effects from the benchmark specifications in addition to a fixed effect for each stacked scandal-hit firm specific dataset. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

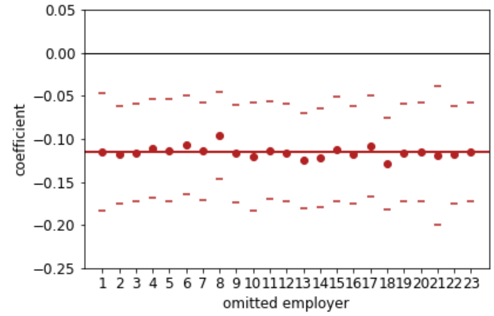
C.6.4 Leave-One-Out

Since our regressions are unweighted and employers are not equally represented in the Glassdoor data, we seek to ensure that no one firm drives our findings. We re-estimate the generalized differences-in-differences regressions for our main outcomes of interest, but iteratively exclude one scandal-hit firm. The coefficients for each remain negative, statistically significant, and deviate relatively little from the full sample estimate regardless of which firm is excluded.

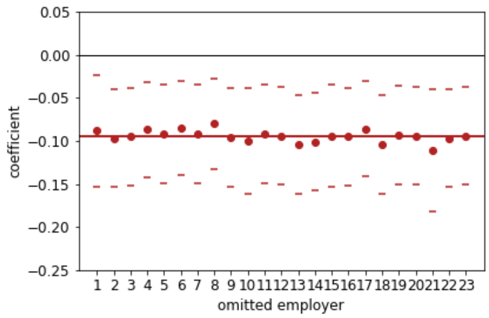
Figure C.3: Leave-One-Out Coefficients for Reviewer Outcomes



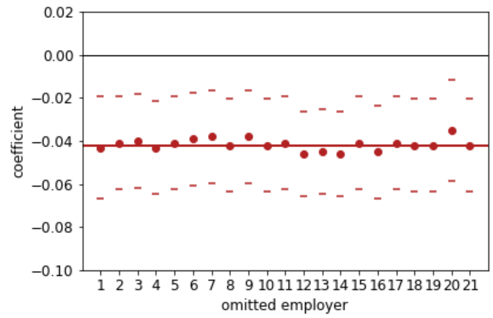
(a) Overall rating



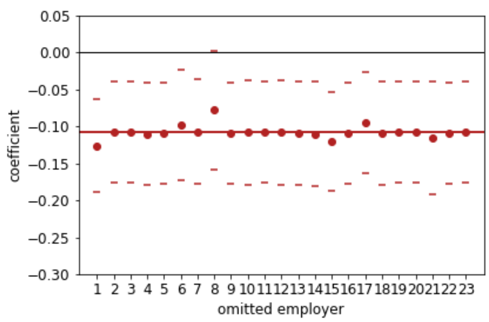
(b) Culture and values



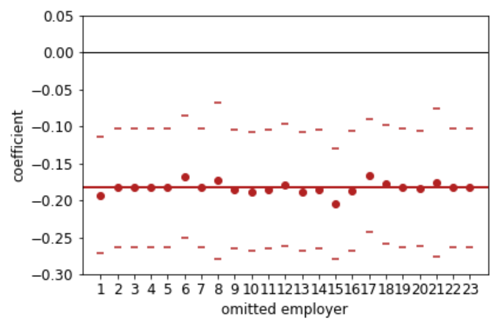
(c) Senior management



(d) Would refer to a friend



(e) Variable pay

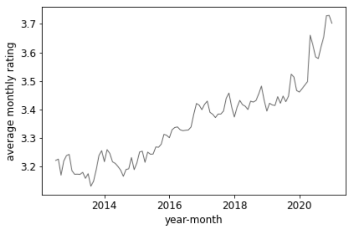


(f) Variable pay, low exp

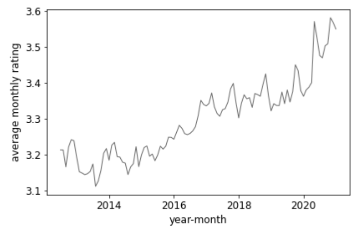
Notes: These figures report the difference-in-difference coefficients when one scandal—as indicated by the position along the x-axis—is omitted in a rotating fashion. The employer numbers correspond as follows: (1) Apple, (2) CBS, (3) Carnival, (4) Equifax, (5) FOX News, (6) Facebook, (7) GlaxoSmithKline, (8) Google, (9) Guess?, (10) Macy's, (11) Mylan Inc, (12) Nissan, (13) Samsung, (14) Sony, (15) Tesla, (16) Toshiba, (17) Uber, (18) United Airlines, (19) Valeant, (20) Volkswagen, (21) Wells Fargo, (22) Wynn Resorts, and (23) lululemon. The solid horizontal line reflects the coefficient from including all scandals. Horizontal dashes indicated a 95 percent confidence interval around each point estimate. Standard errors are clustered by firm.

C.7 Additional Figures for Chapter 3

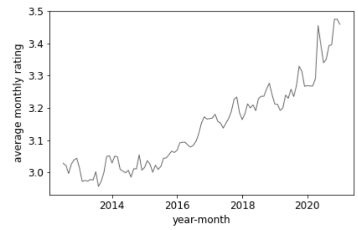
Figure C.4: Average Employee Ratings and Responses for All Other Firms



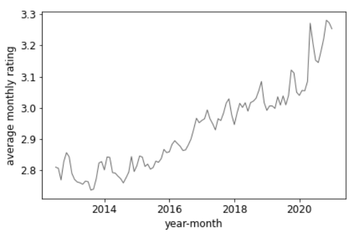
Overall rating



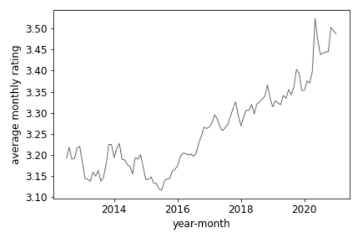
Culture and values



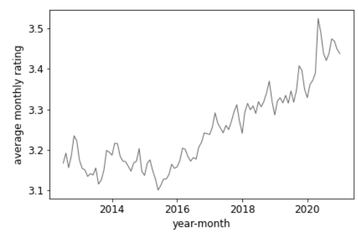
Career opportunities



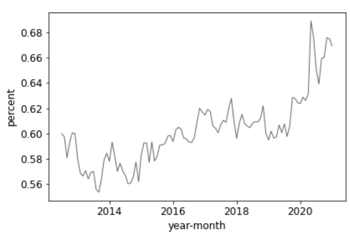
Senior management



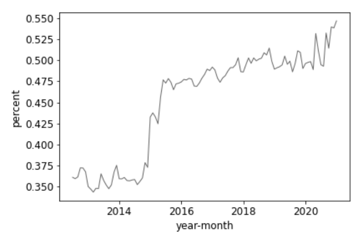
Compensation and benefits



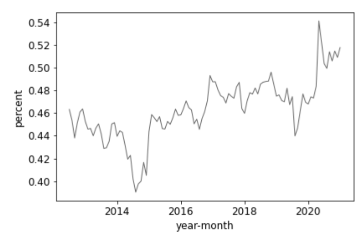
Work-life balance



Would refer a friend



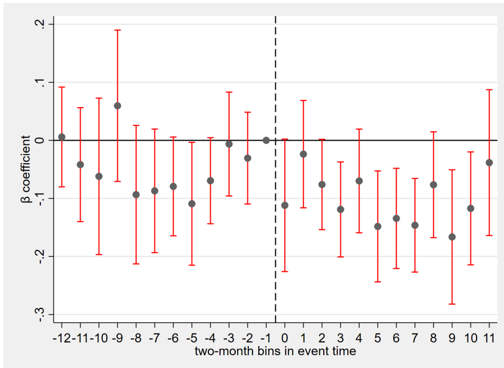
Positive business outlook



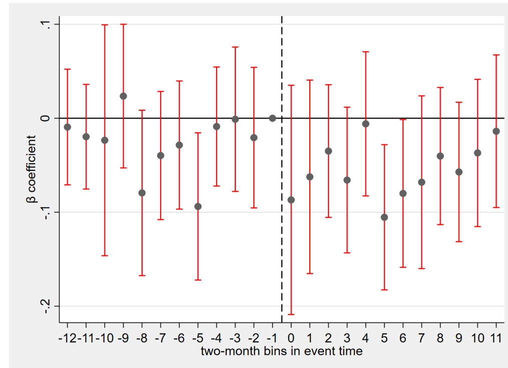
Approve of CEO performance

Notes: These figures display the average rating among newly-submitted reviews each year-month for firms that experienced neither a corporate scandal nor a data breach. The sample period is from June 2012 through December 2020.

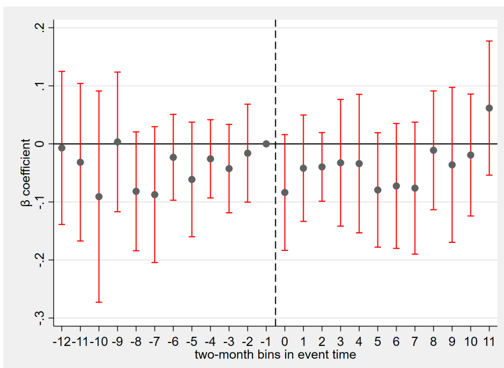
Figure C.5: Employee Ratings around Scandal Dates, Additional Dynamic Results



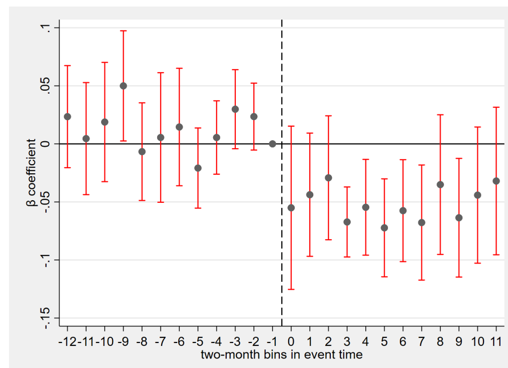
Career opportunities



Compensation and benefits



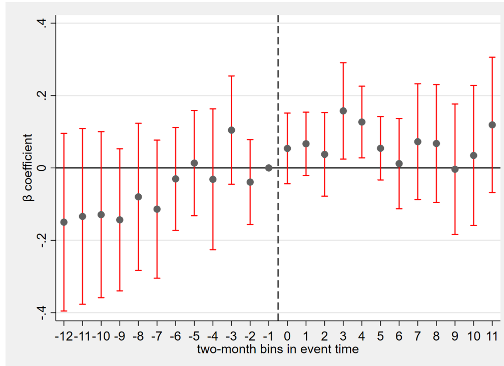
Work-life balance



Positive business outlook

Notes: These figures display dynamic coefficients for the four other main dependent variables. Horizontal dashes indicate a 95 percent confidence interval around each point estimate. Regressions include firm, job title, industry x year-month, state, current employee, and employment status fixed effects. Each coefficient is relative to the two-month period prior to the scandal. Standard errors are clustered by firm.

Figure C.6: Employee Reviews Submitted around Scandal Dates, Dynamic Effects



ln(reviews + 1)

Notes: The left panel displays coefficients from the dynamic version of Equation 3.3.1 using the one plus the log of monthly firm review count as the dependent variable. We use 48-month windows around event dates. Horizontal dashes indicate a 95 percent confidence interval around each point estimate. Regressions include firm and industry x year-month fixed effects. Each coefficient is relative to the two-month period prior to the event. Standard errors are clustered by firm.

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