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WHAT'S MY EMPLOYEE WORTH? THE EFFECTS OF SALARY BENCHMARKING

Zoe B. Cullen
Shengwu Li
Ricardo Perez-Truglia

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ABSTRACT

While U.S. legislation prohibits employers from sharing information about their employees' compensation with each other, companies are still allowed to acquire and use more aggregated data provided by third parties. Most medium and large firms report using this type of data to set salaries, a practice that is known as salary benchmarking. Despite their widespread use across occupations, there is no evidence on the effects of salary benchmarking. We provide a model that explains why firms are interested in salary benchmarking and makes predictions regarding the effects of the tool. Next, we measure the actual effects of these tools using administrative data from one of the leading providers of payroll services and salary benchmarks. The evidence suggests that salary benchmarking has a significant effect on pay setting and in a manner that is consistent with the predictions of the model. Our findings have implications for the study of labor markets and for ongoing policy debates.

Zoe B. Cullen
Rock Center 210
Harvard Business School
60 N. Harvard
Boston, MA 02163
and NBER
zcullen@hbs.edu

Ricardo Perez-Truglia
Haas School of Business
University of California, Berkeley
545 Student Services Building #1900
Berkeley, CA 94720-1900
and NBER
ricardotruglia@berkeley.edu

Shengwu Li
Harvard University
Department of Economics
Littauer Center
1805 Cambridge Street
Cambridge, MA 02138
shengwu_li@fas.harvard.edu

A data appendix is available at http://www.nber.org/data-appendix/w30570

1 Introduction

Employee compensation is the largest source of expenditures for firms. Setting the right salaries is of first order importance. How do firms find out what their employees are worth?

While U.S. legislation, in an effort to hinder collusive practices, prohibits employers from sharing compensation information with each other, employers are still allowed to acquire and use more aggregated data provided by third parties. This practice of using market pay data to identify the typical market salaries for an internal position is known as *salary benchmarking*. According to historical accounts, salary benchmarking has long been central to pay setting strategies (Adler, 2020a). In a survey of members of the U.S. Society for Human Resource Managers, 87.6% report using salary benchmarks to set pay. Interviews with HR executives also indicate that salary benchmarking plays a crucial role in their pay-setting practices (Adler, 2020b). Even the Human Resources textbooks dedicate entire chapters on how to use salary benchmarking tools (e.g., Berger and Berger, 2008; Zeuch, 2016).

Despite their ubiquity, salary benchmarking tools rarely make their way into public view, and their broad application has not been studied by economists. Understanding how these tools affect pay-setting can shed light on how labor markets operate in practice. Furthermore, the effects of these tools are of direct interest to policy-makers, who have recently expressed their intention to investigate whether they suppress wages (White House, 2021).

Our analysis focuses on the compensation of new hires. We provide a simple theoretical framework based on a standard model of competitive bidding (Milgrom and Weber, 1982b). Our model provides an economic rationale for why firms care about salary benchmarks, and it generates testable predictions. More precisely, we model the market for new hires as a first-price private values auction in which firms bid for employees. This captures two key aspects of our setting. First, making a higher offer raises the probability of hiring the worker, but at the cost of a higher salary. Second, firms make offers without knowing what the competing offers are, creating a role for salary benchmarks.

In our model, each firm j observes its own marginal revenue of hiring worker i to fill position X, a random variable that we denote V_{ji}^{X} (the "value"). Each firm chooses a bid b_{ji} for worker i, the worker accepts the highest bid, and the highest bidder receives profit $V_{ji}^{X} - b_{ji}$. We assume that the marginal revenues within each position are **affiliated**. In essence, this means that if worker i is valuable to firm j, then it is more likely that workers eligible for the same position are valuable to other firms. Affiliation is a standard technical condition in auction theory, that ensures that equilibria are tractable and well-behaved even when values are correlated. This formulation allows that the joint distribution of values might be different across positions—for instance, that the marginal revenue generated by distinct bank tellers is highly correlated, but the marginal revenue generated by distinct software developers is not.

Suppose that earlier auctions have been conducted for other workers eligible for the same position, with disjoint sets of firms bidding for each worker. Let S denote the salary benchmark, which we model as the median accepted offer in the earlier auctions. To generate testable predictions, we study the *direct* effect of the benchmark. That is, one firm covertly learns S while the other firms' bidding strategies are held constant. We prove that, in expectation, the access to the benchmark information must reduce the offers at the top end of the distribution. Intuitively, if the firm was going to make a high offer even without the benchmark, then raising their offer cannot increase the probability of hiring much. Hence, their use for the benchmark is to safely lower their offer when they were already likely to win. On the other hand, the effects at the lower end of the distribution can be positive, negative, or zero, depending on the distribution of firm values.

Additionally, we use the model to study the *equilibrium* effect of the benchmark. More precisely, we consider the thought experiment in which we move from no firms with access to the benchmark to all firms having access. The access to the benchmark is common knowledge, meaning that firms will be reacting not only to the benchmark information, but also to the knowledge that other firms can use the benchmark. While we cannot test the equilibrium effects with our data, this thought experiment can be quite informative for the policy discussion. Contrary to the expectation of policy-makers that benchmark tools would suppress wages (White House, 2021), we find that the equilibrium effect of salary benchmarking is to raise salaries, building on a canonical result of Milgrom and Weber (1982b). Intuitively, in a first-price auction, firms exploit their private information by shading their bids below their value. The salary benchmark helps to inform firm j that firm j' has a high value, so that firm j makes higher offers, and it is less safe for firm j' to shade its bid. Thus, in equilibrium the benchmark leads to less bid-shading and hence higher salaries.

In a first-price auction, firms each firm must choose its offer without knowing its competitors' offers. If instead each firm could observe and respond to its competitors offers, then a natural alternative model would be an English auction. Because English auctions have dominant strategies, that model would predict that salary benchmarks are strategically irrelevant, with no effect on firms' wage offers. As we will see, that prediction is at odds with the data.

Next, we provide empirical evidence on the effects of the benchmark tool on pay-setting. We collaborated with the largest U.S. payroll processing company serving 20 million Americans and approximately 650,000 firms. In addition to the payroll services, the company aggregates the salary data from their payroll records in the form of salary benchmarks.

Clients can access these tools online, through a website. This online search tool allows firms to search for any job title they want in a user-friendly way. Currently, this benchmark tool is among the most advanced tools of their kind and is being used by many prominent firms.

Our analysis is made possible thanks to the combination of three sources of administrative data. The first dataset corresponds to the payroll records, which include detailed information such as the hire date, position and compensation. The second dataset contains information about the usage of the benchmark tool, allowing us to reconstruct which firms looked up which positions and when. Third, we have the historical data on the salary benchmarks, allowing us to observe the salary benchmarks that a firm saw (or would have seen) in the compensation explorer when searching for a specific position at a particular point in time.

Our data covers the roll-out of the benchmark tool when it was first introduced to the market. Our sample includes 586 "treatment" firms that gained access to the tool and 1,419 "control" firms that did not gain access to the tool but were selected to match treatment firms along observable characteristics. We focus on new hires that took place between January 2017 and March 2020, and during a narrow window of 10 quarters around the firm's onboarding date.

Our identification strategy is based on a differences-in-differences design. We leverage three sources of plausible exogenous variation. First, while some firms gain access to the tool, some other firms do not. Among the firms who gain access to the tool, some gain access earlier than others. And even within firms with access to the tool, some positions are searched and others are not. According to the provider of the benchmark tool, which firms end up gaining access to the tools, and when they gain access, is largely arbitrary. For example, when the benchmark tool was introduced to the market, its adoption relied heavily on direct contact from the sales representative of the payroll firm to its clients. As a result, some firms adopted earlier than others, to a great extent, due to the arbitrary order in which they were approached by the sales team. Rather than taking them as granted, we conduct a series of empirical tests (e.g., event-study) to confirm testimonies that the timing of tool adoption is as good as random.

We assign each new hire into one of three categories. *Searched positions* correspond to the 5,266 unique hires in positions that are (eventually) searched in treatment firms. *Non-Searched positions* correspond to the 39,686 hires in positions that are not searched by treatment firms. *Non-Searchable positions* correspond to the 156,865 hires in control firms, who by construction could not be searched in the tool. For treatment firms, we analyze how the salaries in Searched and Non-Searched positions evolved around the date when the firm gained access to the benchmark tool. For control firms, we analyze how the salaries in Non-Searchable positions evolved around the date when the firm could have gained access to the benchmark tool: for each "control" firm we assign a "hypothetical" onboarding date, equal to the actual onboarding date of the treatment firm that is most similar in observables.

To assess whether the results were surprising or predictable, we conduct a forecast survey using a sample of 68 experts, most of whom are professors doing research on these topics. After receiving a brief explanation of the context, the experts are asked to make forecast about some of the potential effects of salary benchmarking (or lack thereof).

We start by measuring the effects of salary benchmarking on the distribution of salaries. According to the theoretical framework, there should be compression from above: firms who would have otherwise paid above the market benchmark should reduce salaries, thus moving towards the benchmark. On the other hand, the model predicts that there may be compression from below too: in some cases, but not always, firms who would have otherwise paid below the market benchmark will increase salaries, thereby moving towards the benchmark. Notably, this ambiguous prediction is present in the expert forecasts too. Some respondents predict compression from above, others from below, others from both above and below – and many others predict something entirely different from compression. Moreover, experts show low confidence in their own predictions.

Our evidence suggests that salaries get compressed towards the benchmark, both from above and below. Among Searched positions, and after gaining access to the tool, the distribution of salaries gets more compressed towards the median market benchmark. To quantify the compression effect more parametrically, we construct a dependent variable equal to the absolute %-difference between the employee's starting salary and the corresponding market benchmark. This formula is closely related to a common measure of dispersion in statistics and economics: the Mean Absolute Percentage Error.[1] Among Searched positions, the dispersion to the benchmark was on average 19.8 pp before the firms gained access to the tool. After gaining access to the tool, the dispersion dropped from 19.8 pp to 14.9 pp. This drop is not only highly statistically significant (p-value<0.001), but also large in magnitude, corresponding to a 25% decline. Moreover, our event-study analysis indicates that these effects on salary compression coincide precisely with the timing of access to the benchmark: the compression was stable in the quarters before the firm gained access to the tool, dropped sharply in the quarter after the firm gained access, and remained stable at the lower level afterwards.

Next, we use the Non-Searched and Non-Searchable positions as two alternative control groups, in a differences-in-differences fashion. Because the firms never see the relevant benchmark, we should not expect compression towards the benchmark for Non-Searched positions.

[1]More precisely, the relevant "error" in our context is be the difference between the employee's starting salary and the corresponding benchmark (i.e., the median salary for that position).

We show that, indeed, the compression around the benchmark is stable before the firm gains access to the tool, and remains stable at the same level after the firm gains access to the tool. Next, we use Non-Searchable positions as an alternative control group. Because firms cannot see the benchmarks for the Non-Searchable positions, we should not expect compression towards the benchmark either. We show that, indeed, the compression around the benchmark is stable before the (hypothetical) onboarding date, and remains stable at the same level after the (hypothetical) onboarding date. Comparing the evolution of Searched positions to each of these control groups yields estimates of the impact of gaining access that are similar in magnitude and statistically indistinguishable from each other. The fact that the results are consistent across these two identification strategies is reassuring. Moreover, these results are robust to a host of additional validation checks.

While our estimated effects on compression are economically significant, they are probably a lower bound on the *true* effects due to various sources of attenuation bias. We also note that this average effect masks substantial heterogeneity. We categorize positions by skill levels. We define low-skill positions as those that typically require no more than a High-School diploma, that typically employ younger employees and with modest pay. Around 42% of the sample is classified as low-skill, and the remaining 58% as high-skill. Some examples of low-skill positions are Bank Teller and Receptionist, and some examples of high-skill positions are Ophthalmic Technician and Software Developer.

When we break down the effects on salary compression by skill levels, we observe large and statistically significant differences, with stronger effects in the low-skill positions. In low-skill positions, dispersion around the benchmark drops from 14.5 pp to 8.7 pp (p-value<0.001), equivalent to a 40.0% decline. By comparison, for high-skill positions the change in dispersion is smaller, dropping from 21.9 pp to 18.9 pp (p-value=0.021), a 14.6% decline. This finding is in sharp contrast to the expert forecasts, which predicted that the effects would be concentrated on high-education positions. However, this finding is largely consistent with the anecdotal accounts in interviews with compensation managers, according to which low-skill positions are treated as commodities and thus should be paid the market rate (Adler, 2020b). This finding is also consistent with the model. Salary benchmarks may be more informative for low-skill positions because there is less heterogeneity across workers in the marginal revenue they generate for competing firms. If marginal revenue is less heterogeneous across workers, then the salaries of past workers are more informative about the offers for present workers, resulting in larger reactions to the benchmark.

The above evidence suggests that the use of salary benchmarks has a significant effect on the wage determination process. The natural next question is what the average effects of this practice may be. Is salary benchmarking having a negative effect on the average salary? Is salary benchmarking helping companies to retain their newly hired employees? These average effects can be quite relevant for employers, as it may indicate whether it is in their best interest to use salary benchmarks. These average effects can be particularly relevant to policy-makers too, as it may provide hints on who are the winners and losers from salary benchmarking. The empirical evidence is particularly valuable, as the model provides ambiguous predictions. This ambiguity is present in the expert forecasts too: only a minority of experts feel confident about the effects on average salary. And the expert forecasts vary widely, with some predicting negative effects and others positive effects.

To estimate the average effects of salary benchmarking, we use the same identification strategy from the analysis of compression described above. The key difference is that, instead of using salary compression as dependent variable, we use other outcomes, such as the salary level or retention. Our evidence suggests that, for the average employee, and regardless of the specification, salary benchmarking does not have a negative effect on the average salary. For the whole sample, the effect on the average salary is positive, but small in magnitude and statistically insignificant. When considering the low-skill employees, the evidence points to a modest increase in their average salary. Depending on whether the Non-Searched or Non-Searchable positions are used as control, the gains in average salary are estimated at 5.0% (p-value=0.014) and 6.7% (p-value=0.001), respectively. We also find evidence suggesting that, among low-skill employees, the gains in average salary were followed by an increase in retention rates, measured as the probability that the employee is still working at the firm 12 months after the hiring date. Depending on whether the Non-Searched or Non-Searchable positions are used as control, the gains in retention probability are estimated at 6.6 pp (p-value=0.101) and 6.8 pp (p-value=0.029), respectively. The relative magnitude between the effects on average salary and retention are consistent with the best estimates of retention elasticities (Dal Bo et al., 2013). This evidence suggests that firms may be using salary benchmarking to raise some salaries in an effort to improve, among other things, the retention of their employees.

This study contributes to various strands of literature. First and foremost, we contribute to the fields of labor economics, personnel economics and management by measuring the effects of salary benchmarking tools. In spite of their widespread use, there is no evidence on their effects. We fill that gap by providing the first casual estimates. Moreover, to the best of our knowledge, ours is the first study to analyze the effects of business analytic tools more generally. The existing literature is either theoretical (Duffie et al., 2017; Blankmeyer et al., 2011) or descriptive (Schiemann et al., 2018) in nature.[2]

[2]One notable exemption is Grennan and Swanson (2020), which is discussed below. More broadly, our findings are related to the effects of information technology (e.g., Jensen, 2007).

This study is related to a recent but growing body of literature on pay transparency. Evidence from field experiments and natural experiments indicate that making salaries more transparent to *employees* affects a variety of employee outcomes such as satisfaction, effort, turnover and pay (Card et al., 2012; Mas, 2017; Perez-Truglia, 2020; Dube et al., 2019; Breza et al., 2018; Cullen and Pakzad-Hurson, 2016; Cullen and Perez-Truglia, 2022; Baker et al., 2019; Bennedsen et al., 2019; Duchini et al., 2022). Relatedly, there is work documenting significant misperceptions of employees about salaries, even the salaries of coworkers at the same firm (Cullen and Perez-Truglia, 2022, 2018; Caldwell and Harmon, 2018; Caldwell and Danieli, 2021; Jäger et al., 2021; Roussille, 2021). This literature is, however, entirely focused on the information frictions on the employee's side. The whole literature implicitly assumes that the transparency policies operate by affecting the information that employees can see. We contribute to this literature by showing that firms too, even the large ones, face significant information frictions. Our evidence suggests that some of the documented effects of transparency policies may be driven by the beliefs and decisions of firms, not just employees.

This project is also related to a small but growing literature on "behavioral firms" (DellaVigna and Gentzkow, 2019), more specifically on a series of biases in setting wages such as rounding (Dube et al., 2018), wage anchoring (Hjort et al., 2020; Hazell et al., 2021) and downward wage rigidities (Grigsby et al., 2021; Kaur, 2019). While the existing evidence is focused on *optimization* frictions, we contribute to this literature by showing direct evidence that firms face *information* frictions.

Our study is also related to a literature on dispersion in wages for similar workers, and more specifically studies attributing variation in wages to firm wage setting policies (Mortensen, 2005; Abowd et al., 1999). Canonical models in this literature start from the premise that workers have limited information about the wages that firms are offering, and as a consequence employers engage in a wage setting game that results in differentiated offers. Recent empirical advances in this literature focus on measuring firm-specific premiums and rent sharing elasticities (Card et al., 2018). Our evidence suggests that firm-level pay setting decisions do in fact impact the extent of wage dispersion among observably similar workers; however, we highlight information frictions on the firm side as a novel factor contributing to this dispersion.

Finally, our study is related to a literature on auction theory and industrial organization. On the theoretical side, Milgrom and Weber (1982a) and Milgrom and Weber (1982b) study what happens when bidders in an auction can observe private and public signals. On the empirical side, Tadelis and Zettelmeyer (2015) conducted a field experiment in wholesale automobile auctions and show that disclosing quality information about the goods being auctioned leads to higher revenues. Luco (2019) provides evidence that, in the context of retail gasoline industry, a policy of online price disclosure increased the average margins. And Grennan and Swanson (2020) provides evidence that, in the context of U.S. hospitals, access to a web-based benchmarking database has a significant effect on price negotiations for health services.

The rest of the paper proceeds as follows. Section 2 presents the theoretical predictions. Section 3 describes the institutional context, data and research design. Sections 4 and 5 present the empirical results. The last section concludes with implications for researchers and policy-makers.

2 The Model

We study the wage offer process as a first-price auction with affiliated private values, a canonical model due to Milgrom and Weber (1982b). This modeling approach captures two key features of our setting. First, modeling the process as a first-price auction implies that higher offers increase the probability of hiring the worker, but at the cost of raising their salary; a key trade-off according to Human Resources handbooks.[3] Second, the affiliated values assumption allows that wage benchmarks matter, in the sense that they can convey information about the distribution of competing offers that a firm faces.[4]

To start with, consider one worker and $n \geq 2$ firms. Each firm j has a value for the worker that is a real-valued random variable V_j, known to that firm. This captures the marginal revenue that the worker would generate at firm j. The salary benchmark S is a real-valued random variable. We interpret this as capturing past offers made by other firms for similar workers.

We assume that V_j has support on interval $[\underline{v}, \overline{v}]$ with $0 \leq \underline{v} < \overline{v} < \infty$, and that S has support on some arbitrary interval $\mathcal{S}$. Let $f(s, v_1, \ldots, v_n)$ denote the joint density of the benchmark and the firm values. We assume that the density f is symmetric in its last n arguments and uniformly continuous with respect to each v_j.

We assume that the random variables $(S, V_1, \ldots, V_n)$ are **affiliated**, as we now define. Let $z, z' \in \mathbb{R}^k$ for some integer k. $z \vee z'$ denotes the component-wise maximum, and $z \wedge z'$ the component-wise minimum. Random variables are *affiliated* if for all z and z', their joint density f satisfies

$$f(z \vee z')f(z \wedge z') \geq f(z)f(z'). \tag{1}$$

[3]Alternative auction formats such as second-price auctions and English auctions do not exhibit this trade-off, since raising the winning firm's offer, holding all other offers fixed, has no effect on the worker's salary. In such formats, information about other firms' offers is strategically irrelevant.

[4]If firms' values are drawn independently across workers according to a known distribution, then past salaries convey no further information about current offers.

Example 2.1. *There are two firms, and the marginal revenue of worker i to firm j is*

$$V_j = f(Q + Q_i + Q_{ij}) \tag{2}$$

where $f : \mathbb{R} \to \mathbb{R}_{\geq 0}$ is a continuous increasing function, and Q, Q_i, and Q_{ij} are random variables. Q is a position-specific component, Q_i is a worker-specific component, and Q_{ij} is a match-specific component, all independent and normally distributed. Each firm observes its marginal revenue V_j, but not the individual components. Then the variables (Q, V_1, V_2) are affiliated.

We define another random variable $Y_1 \equiv \max_{j \neq 1} V_j$. Let $f_{Y_1}(x \mid v, s)$ denote the density of Y_1 conditional on $V_1 = v$ and $S = s$, with cumulative distribution $F_{Y_1}(x \mid v, s)$.

By a standard argument[5], affiliation implies that $\frac{f_{Y_1}(x|v,s)}{F_{Y_1}(x|v,s)}$ is non-decreasing in s. We use $f_S(s \mid v)$ to denote the density of S conditional on $V_1 = v$, with cumulative distribution $F_S(s \mid v)$.

We start by studying the **no-benchmark equilibrium** of the first-price auction. Each firm j observes V_j and then chooses a bid b_j. Firm j's payoff is equal to $(V_j - b_j)$ if $b_j > \max_{k \neq j} b_k$ and 0 otherwise.[6] A standard argument, adapting the proof of Theorem 14 of Milgrom and Weber (1982b), yields this characterization of the equilibrium:

Theorem 2.2. *There exists a symmetric no-benchmark equilibrium of the first-price auction. The equilibrium strategy $b^* : [\underline{v}, \overline{v}] \to \mathbb{R}$ is strictly increasing and satisfies the first-order linear differential equation defined by*

$$b^*(\underline{v}) = \underline{v}, \tag{3}$$

$$b^{*\prime}(v) = (v - b^*(v)) \frac{E\left[f_{Y_1}(v \mid v, S) \mid V_1 = v\right]}{E\left[F_{Y_1}(v \mid v, S) \mid V_1 = v\right]}. \tag{4}$$

We assume that the benchmark is **locally relevant**, meaning that for all v, there exists s such that $0 < F_S(s \mid v)$ and

$$\frac{f_{Y_1}(v \mid v, s)}{F_{Y_1}(v \mid v, s)} < \frac{E\left[f_{Y_1}(v \mid v, S) \mid V_1 = v\right]}{E\left[F_{Y_1}(v \mid v, S) \mid V_1 = v\right]}. \tag{5}$$

This condition essentially requires that the benchmark is informative about the ratio $\frac{f_{Y_1}(v|v,s)}{F_{Y_1}(v|v,s)}$. If firm 1 were to slightly reduce its bid from $b^*(v)$, the cost is a reduced probability of winning, proportional to $f_{Y_1}(v \mid v, s)$. The benefit is that firm 1 pays less if it wins, which is proportional to $F_{Y_1}(v \mid v, s)$. So local relevance implies that the benchmark is informative about the expected profits from slightly changing 1's bid.

We now study the direct effect of the benchmark. That is, suppose that firm 1 covertly observes S before placing its bid, while believing that the other firms continue to bid according

[5]Lemma 1 of Milgrom and Weber (1982b).

[6]Ties are zero-probability events, so the analysis does not depend on the tie-breaking condition.

to b^*. Consider the informed firm's best-response correspondence, which depends on its value V_1 and the benchmark S,

$$\underset{b \geq 0}{\operatorname{argmax}}\, E[(V_1 - b)\mathbb{1}_{\{b^*(Y_1) < b\}} \mid V_1 = v, S = s]. \tag{6}$$

The correspondence (6) is monotone non-decreasing in v, by Topkis's theorem. Let $\tilde{b}(v, s)$ be an arbitrary selection from (6).

The next theorem states that if the firm's value is high enough, then covertly observing the benchmark strictly reduces its expected bid as well as its expected payment.

Theorem 2.3. *There exists $\tilde{v} < \overline{v}$ such that for all $v > \tilde{v}$, we have that*

$$E\left[\tilde{b}(v, S)\middle| V_1 = v\right] < b^*(v), \tag{7}$$

and also that

$$E\left[\tilde{b}(v, S)\mathbb{1}_{\tilde{b}(v,S) \geq b^*(Y_1)}\middle| V_1 = v\right] < b^*(v). \tag{8}$$

The proof is in Appendix A.1.

For lower quantiles of the distribution, one can show using the boundary condition (3) that the direct effect is non-negative as V_1 approaches $\underline{v}$. Formally, there exists a selection from the informed firm's best-response correspondence that satisfies

$$\lim_{v \downarrow \underline{v}} \left(E\left[\tilde{b}(v, S)\middle| V_1 = v\right] - b^*(v)\right) \geq 0. \tag{9}$$

Moreover, for various natural distributions f the direct effect is single-crossing and strictly positive on an interior interval. On the other hand, for lower quantiles of the distribution, the sign of the direct effect depends on the joint distribution of $(S, V_1, \ldots, V_n)$. One can construct distributions such that the effect is positive, negative, or zero.

These results motivate the following prediction:

Prediction 2.4 (Direct effect)**.** *Gaining access to the benchmark will reduce salaries at high quantiles of the distribution, but not necessarily at low quantiles of the distribution.*

The next theorem states that if the firm's value is high enough, then covertly observing the benchmark strictly reduces its expected probability of hiring the worker.

Theorem 2.5. *Suppose that the joint density f is strictly positive everywhere on $[\underline{v}, \overline{v}]^N \times \mathcal{S}$. There exists $\tilde{v} < \overline{v}$ such that for all $v > \tilde{v}$, we have that*

$$P\left(\tilde{b}(v, S) \geq b^*(Y_1)\middle| V_1 = v\right) < P\left(b^*(v) \geq b^*(Y_1) | V_1 = v\right). \tag{10}$$

The proof is in Appendix A.2. Suppose that a firm was making high wage offers before observing the benchmark. Theorem 2.5 predicts that such a firm will become less likely to hire the worker after observing the benchmark. However, it is possible that firms that were initially making low wage offers will become more likely to hire, and the sign of the overall effect on hiring probability is an open question.

In our data, individual firms gain access to the salary benchmark, which does not tell us what would occur if many firms gained access to the benchmark, and if it was common knowledge that they used the benchmark to set salaries. To speak to this question, we now examine the model's predictions for the new equilibrium that arises when all firms observe the benchmark and best-respond to each others' bidding strategies.

Let $b^{**} : [\underline{v}, \overline{v}] \times \mathcal{S} \to \mathbb{R}$ be the symmetric equilibrium strategy in a first-price auction after all bidders observe the benchmark. This is characterized by the first-order linear differential equation

$$b^{**}(\underline{v}, s) = \underline{v}, \tag{11}$$

$$b^{**\prime}(v, s) = (v - b^{**}(v)) \frac{f_{Y_1}(v \mid v, s)}{F_{Y_1}(v \mid v, s)}. \tag{12}$$

Theorem 2.6. *The equilibrium with the benchmark yields higher expected salaries than the no-benchmark equilibrium, that is*

$$E\left[\max_i b^{**}(V_i, S)\right] \geq E\left[\max_i b^{*}(V_i)\right]. \tag{13}$$

Theorem 2.6 is a special case of Theorem 16 of Milgrom and Weber (1982b). Both the benchmark equilibrium and the no-benchmark equilibrium lead to the same winner, namely the firm with the highest value V_i. But in the benchmark equilibrium, each firm's bid b^{**} is increasing in the benchmark S, which is affiliated with that firm's private information V_i. In this way, the benchmark strengthens the statistical linkage between the bid $b^{**}(V_i, S)$ and the firm's private information V_i, reducing the firm's information rents and raising salaries.

2.1 Extensions to the Model

In Appendix B, we provide a number of extensions, which we summarize below. So far we have assumed that there is only one signal S, and examined comparative statics from allowing one firm to observe S, and allowing all firms to observe S. But the firms in our data already had access to other, arguably less accurate, salary benchmarks before gaining access to the one that we study. In Appendix B.1, we extend the model to allow for multiple signals, some of which the firms already observe, and find that the same comparative statics hold for the effects of observing an *additional* signal.

The baseline model treats the benchmark as an arbitrary signal S such that $(S, V_1, \ldots, V_n)$ are affiliated; the results do not require further structure on S. But the benchmarks in our data are not arbitrary—in particular, we focus on the median of past salaries for each position-title. Why should this be affiliated with the marginal revenue each firm has for the current worker? In Appendix B.2, we provide a simple foundation for affiliated signals. We consider a sequence of auctions, imposing that firms' values for the current worker are affiliated with other firms' values for past workers. In equilibrium, the median of the winning bids in past auctions is affiliated with firms' values in the present auction. Hence, rather than an exogenous signal S, we can regard S as being determined by the equilibrium offers made by other firms to similar workers.

Our model can also make predictions about the heterogeneity across different types of position, such as between low-skill and high-skill positions. Intuitively, low-skill positions are easier to standardize and monitor, so any two workers in that position can provide similar productivity. Appendix B.3 shows that, if we model low-skill positions as having less individual productivity variation, then the prediction is that the benchmark will have a stronger effect on this group.

We have modeled firms making simultaneous offers to each worker. This was an intentional design choice, as a dynamic model would have significantly complicated the setting. In reality, firms may be motivated to use benchmarks, among other things, because of retention concerns. In Appendix B.4 we show that the theoretical predictions hold in a stylized model of retention concerns.

In our baseline model, each firm treats the benchmark as exogenous. In reality, a large firm's salary offers may substantially affect the benchmark, and hence affect its competitors' offers. While these cases are rare, we observe them in our setting.[7] In Appendix B.5 we study a model of benchmark pass-through, finding that the benchmark has ambiguous effects on the large firm's equilibrium offers, but consistently reduces the large firm's equilibrium profits.

3 Institutional Context and Data Sources

3.1 Background on Salary Benchmarking

Salary benchmarking refers to use of surveys or other sources of market pay data to identify the typical market salaries for an internal position. This practice dates back to 1980s (Adler, 2020a), and it can be found in the private as well as public sectors (Faulkender and Yang, 2010; Thom and Reilly, 2015).[8] In our survey of 2,085 professionals who set pay in their

[7] Appendix Section J describes the extent of pass-through and Appendix M documents the dynamic response of competitors' wages. A 10% raise in the salaries of all new and existing hires in a position would shift the salary benchmark median by 0.59% on average.

[8] Adler (2020a) puts forward the hypothesis that the use of external benchmarks was, at least in part, motivated by a need to reduce the firms' liability for discrimination lawsuits.

organizations, and are members of the Society for Human Resource Managers in the U.S., 87.6% report using salary benchmarks to set pay.[9] Moreover, interviews of executives from the United States indicate salary benchmarking plays a major role in their pay-setting practices (Adler, 2020a,b). Many Human Resources handbooks dedicate entire chapters to the practice of salary benchmarking. For example, Chapter 48 from Zeuch (2016) is dedicated to the "Essentials of Benchmarking." And Chapters 9 and 10 of Berger and Berger (2008) are dedicated to "Salary Surveys" and "Benchmarking". The latter has an excerpt that could reflect how HR managers view benchmarking:

> *"Using surveys to benchmark compensation levels ensures that the pay levels determined by the organization are not extraordinarily misaligned with market practice – i.e., pay is not too low or too high. Determining the appropriate amount of compensation is a balancing act. No organization wants to waste their financial resources by paying too high relative to the market; and those who pay too low risk unwanted turnover from employees looking for a better deal elsewhere."* – Berger and Berger (2008), p. 125.

Salary benchmarking is used across the entire organization, even for the highest echelons (i.e., executive pay). In 2006, the Securities and Exchange Commission issued a new disclosure requirement, requiring companies to state whether they engaged in "any benchmarking of total compensation, or any material element of compensation, identifying the benchmark and, if applicable, its components (including component companies)" (Securities and Exchange Commission, 2006). In fiscal year 2015, over 95% of the S&P 500 companies disclosed a peer group of firms that they used to benchmark executive salaries against (Larcker et al., 2019).

The earliest forms of salary benchmarks were compensation surveys administered by consulting firms. To meet these demands, some personnel management consultants grew specialized in providing market data through compensation surveys, with some notable examples being Abbott, Langer and Associates, Korn Ferry, Hayes Group, Mercer, Radford, and Willis Towers Watson. In the last decade, some tech companies started to offer online tools that allow employees, but also employers, to find information about the market salaries in specific positions. Some of these websites, such as Glassdoor, Comparably, and LinkedIn, have become popular because they allow anyone to conduct searches for free. These websites rely primarily on crowdsourcing: i.e., employees who visit the website can fill out a quick survey reporting their pay at their current or past companies. These data are probably

[9]The magnitude of this estimate is consistent with the results from an industry survey of 5,003 U.S. firms: 96.3% of them reported that they use some form of salary benchmarking to inform their compensation strategy and structure (PayScale, 2021).

not the highest quality, among other things, because of biases in who decides to self-report their salary, whether they self-report it truthfully, and also the limited number of observations. There are other online tools that require a paid subscription, such as Salary.com and Payscale.com. These other tools are based mainly on data from traditional salary surveys.

More recently, the largest U.S. provider of payroll services started to offer data analytics tool to their clients, including but not limited to salary benchmarking tools. Payroll data is arguably the highest-quality data one could think of to construct salary benchmarks. Any error in payroll is immediately corrected as it impacts someone's day to day life. The most comparable data is probably tax records, but tax records fall short of payroll records in terms frequency, accuracy and detail. For example, payroll records include information about the position title of the employee, which is missing from tax records. And while tax records include the gross taxable income of the employee, it does not show the critical break down by base salary, commissions, bonuses, etc. The payroll data has even bigger advantages over salary surveys and crowd-sourced data, which raise flags about the smaller sample sizes, measurement error and biases due to selection into the survey. Moreover, due to the massive sample sizes of payroll, covering several millions of employees at any point, salary benchmarks are much more precisely estimated. And due to the high-frequency nature of the payroll data, the benchmarks can be updated more frequently.

Salary benchmarking is part of the broader phenomenon of people analytics, brought about by growth in business data capacity. HR functions at leading companies leverage data to attract and retain talent, predict employee turnover, identify talent shortages, and other aspects of workforce planning (Davenport and Shapiro, 2010). In a survey of more than 10,000 HR and business leaders across 140 countries implemented by Deloitte in 2017, 71% of companies saw people analytics as a high priority in their organizations, and recruiting came up as the highest-priority area of focus within that (Collins et al., 2017). HR has come to be one of the most data-driven functions at major companies (Davenport, 2019).

3.2 Survey on Uses of Salary Benchmarking

To provide evidence on how firms use salary benchmark tools and assess the validity of assumptions of the model we conducted a survey with Human Resources (HR) managers in collaboration with the Society for Human Resource Management (SHRM). We collected responses from July 15, 2022 to July 20, 2022, using SHRM's Voice of Work Research Panel. From a sample of 9,537 panelist, we had 2,696 responses, for a response rate of 28.3%. As a filter to access the main module of the survey, we asked them if they participate in setting salaries for employees (2,085 respond affirmatively) and, then, if they use salary benchmarks (1,827, an 87.6%, respond affirmatively). From these, 1,350 complete the entire survey and constitute our sample. The sample broadly covers firms across industries and size in both the public and private sector in the US. More details on the implementation of the survey, sample characteristics and results are provided in Appendix L.

Our survey indicates that among firms that use salary benchmarks the majority (72.3%) use multiple sources to obtain market data on salaries. Compensation surveys that consultants administer to their business clients are a traditional source of salary data. This source is used by 26.3% of respondents. The most popular sources are industry surveys and free online data sources (68.0% and 58.1% of participants, respectively, indicate they use these). Other popular options are government data (37.1%), paid online data sources (34.4%) and payroll data services (23.2%). Among HR managers in our sample, 48.6% have used Glassdoor as their salary benchmark source and 9.5% ADP's Data Cloud Compensation Explorer.

The survey also provides evidence on how frequently firms use benchmark tools and for which purposes. The most common uses of benchmark tools are to set the salary ranges for specific job titles and to change salaries for current employees (89.8% and 76.8% of participants, respectively, indicate they use them with these purposes). Other popular uses are to set precise salaries for new employees (54.1%), in salary negotiations (53.1%), to determine salary in job advertisement (40.9%), and to plan ahead for headcount (25.3%). Most, but not all, HR professionals in our sample report using benchmarks to set salaries for a majority or all their new hires, and a majority or all of their current employees (64.4% and 61.7%, respectively).

3.3 The Compensation Explorer Tool

The study builds on an ongoing collaboration with the largest payroll processing firm in America, a publicly-traded firm with a current market cap of $72.5 billion. This company provides payroll services for 650,000 firms, including many of the most prominent ones, for a total of 20 million employees. In addition to providing payroll services, this firm uses the massive payroll data from its clients to provide business analytic tools as a subscription service. In this study, we are interested in the *Compensation Benchmark Tool*, consisting of a search engine to view detailed compensation statistics.

To better illustrate how the compensation explorer works, Figure 1 provides a screenshot of this online tool.[10] The online tool allows the user to browse the benchmarks in different ways. Most prominently, there is a search bar at the top of the screen.

One challenge for the creators of this tool was to aggregate data across different job titles. For example, one company might call a job "warehouse handler," another might

[10]This is a screenshot of how the tool looked like in 2020. There have been some changes to the tool during the period of study, but the overall look and functionality remained similar.

call the same job "inventory handler" or "material handler." The firm is able to convert the raw position titles from each company into a homogeneous taxonomy, with the use of standard machine learning tools for probabilistic matching.[11] Each observation in our data includes a match score that reflects the quality of the match between the firm-specific job title and the title in the taxonomy.[12] Until August 2020, which covers the vast majority of our sample (95.7%), the company used a taxonomy that spanned 2,236 distinct position titles. To understand the granularity of this taxonomy, take the example of teachers. The taxonomy includes 31 position titles that distinguish between preschool, primary, secondary, middle school, substitute, and special education teachers.[13] In our main sample, there are on average 3.84 unique position titles for each 6-digit O*NET code.

Users can search by the position names in the company's proprietary taxonomy. The search tool has an auto-complete functionality, making it easier to find the positions the user is looking for. Because this is the default option, the vast majority of the search results originate through the company's proprietary taxonomy. Additionally, a drop-down menu allows users to search using alternative taxonomies. For instance, users can search for the position titles of their existing employees (i.e., as they appear in the client's own payroll records).[14]

Once the user selects a position title, the tool provides a job description. For illustrative purposes, we will use the position of "Accountant," which is the same example featured in Figure 1. The tool describes the "Accountant" position with the following tasks: "(i) Maintains the accounting operations for a department within the organization; (ii) Checks and verifies records, prepares invoices, vouchers, and filings; (iii) Posts ledgers and general journal entries and balances all records related to accounts receivables and payable; (iv) Assists the financial services manager with accounting and administrative duties; (v) Undertakes responsibility for financial analysis and administration or overseeing the projects occasionally." The job description also includes information about the typical qualifications of the candidate, which in the case of an accountant are: "Requires an undergraduate degree or equivalent experience. For some jobs this may also require a graduate degree or additional

[11]To improve the quality of the match, users are allowed to approve each position match, or to suggest a different one if they disapprove.

[12]We restrict our main sample to observations with match scores above the 20th percentile match score in each quarter. The results are similar without this restriction (see Table F.2 and Table G.2).

[13]Starting September 2020, the company switched to a new taxonomy that expanded the number of position titles. Since our main sample stops at March 2020, our baseline results are not affected by this change. For more details and examples, see Appendix D.1.

[14]In the usage data, around 70.9% of the searches are through the proprietary taxonomy and 22.6% are through the raw position titles. The remaining 6.5% of searches are through the Occupational Information Network (O*NET), which is a standard occupational classification system used widely by researchers and in the private sector. However, this type of searches must be excluded from our analysis as we do not have data on the O*NET benchmarks prior to 2019.

certification. This is typically a knowledge worker who applies information and judgment in a specific area to achieve results and solve problems."

Once a position has been selected, the compensation benchmark tool provides rich data on compensation statistics for that position. The most salient figure is the median base salary, in that it is the first figure shown in the screen, and is also highlighted in other parts (e.g., highlighted in purple in the bottom panel of Figure 1). This is no coincidence, as conversations with the product team indicate that the median base salary is what their clients are most interested in learning about, and also the type of information highlighted in handbooks on Human Resources (e.g., Berger and Berger, 2008; Zeuch, 2016). For that reason, the base salary constitutes our main focus. The definition of base salary in the compensation tool is straightforward and consistent with the definition used in other studies about compensation (Grigsby et al., 2021). For salaried employees, the base pay is just the yearly base salary (i.e., before commissions or bonuses). For the hourly employees, the annual base salary is defined as the annual equivalent of hourly pay: e.g., for a full time employee, it is the hourly wage times 40 hours times 52 weeks.[15] The vast majority of the total cash compensation comes from base salary.[16]

While the median base salary is the most salient piece of information, the tool offers more comprehensive information about pay. In addition to the median, the tool shows a chart with additional information about the distribution of base salary (see the bottom of Figure 1): the 10th, 25th, 75th and 90th percentiles, as well as the average. Likewise, in addition to base salary, the tool allows the user to learn about bonuses, overtime and total cash compensation.

The tool also allows the user to apply some filters to the set of employers and employees included in the benchmark. For instance, users can click on drag-and-drop menus to zoom into a specific industry, or they can use a map to filter by geography, for example by clicking on their own state. However, these filters are only available to the extent that there is enough data, more precisely at least 5 other firms collectively hiring at least 10 employees in the position of interest – for instance, if you tried to zoom in by industry and state, and that left you with an insufficient sample size, you would not be able to see the statistics. The screen also shows the sample size upon which the statistics displayed on the screen are based upon, measured by the number of organizations and the number of employees. The tool also indicates the specific date to which the statistics refer, and it even shows some information

[15] 81.2% of our sample is hourly, and the rest are salaried.

[16] In addition to base salary, employees may receive other forms of compensation such as bonuses and commissions. According to the benchmark data, on average 93.2% of the total cash compensation comes in the form of base salary. A negligible fraction of positions (<1%) receive less than 60% of total cash compensation as base salary. However, our data does not include stock options which may be a significant part of compensation for some employees, especially at the executive level.

about the change of the median salary during the past 12 months. The benchmark is generally stable on quarter-over-quarter basis. For example, the median absolute quarter-over-quarter change in the benchmark is 1.12%.

3.4 Data Sources

We have access to the following datasets:

Payroll Database: this is the key dataset covering all employees in a firm, including the new hires, and with a monthly frequency, from January 2017 through July 2021. It includes detailed information about the position of the employee, exact hire date and compensation details. Our main focus of interest is the base salary, but we also have additional information such as on bonuses. The data on employee characteristics such as gender and age.

Tool Usage Database: this is the key dataset that indicates which positions were searched for and which were not. These data track the web navigation of clients using the benchmark tool. The data include a timestamp for each search, and the position searched. Due to the firm's data storage policy, the data was made available to us from September 2019 through August 2021.[17]

Benchmark Database: this is the database that allows us to reconstruct the search result for each search that we observe in the tool usage dataset, and is available from the first quarter of 2017 through the second quarter of 2021.[18] This database contains the compensation benchmarks, at each point in time and for all positions. As explained in Section 3.3 above, users can apply filters for their search results. The usage data does not indicate which filters the user applied, or whether they applied any filters at all. In our baseline specification, we assume that subjects applied filters for State and Industry whenever there is sufficient data, and then show that the results are robust under alternative specifications.[19,20] In our sample we restrict to employees for which the benchmark information was available in the compensation explorer, regardless of whether the information was looked up by the firm or not.

There are some additional details about the data that deserve mention. To prevent the

[17] Due to the default setting in the tool, the company would automatically delete the usage data older than 6 months. For this reason, we do not have access to this data prior to the date when we pull data for the first time.

[18] Unfortunately, due to reasons outside of our control, we do not have the benchmark data for the second quarter of 2020, and thus we will always have to exclude this period from the analysis. In any case, since that quarter was the worst-hit from the COVID pandemic, we would have excluded that period from the baseline analysis anyways.

[19] More precisely, in the baseline specification we assume the firm used the State and Industry if, after applying those filters, there are at least 30 observations.

[20] Industry and State are the most popular filters used by HR managers according to our SHRM Survey (87.33% and 84.15% of participants indicate they use these filters, respectively).

influence of outliers, we winsorize all dependent variables in the analysis. For example, in the baseline specification, we winsorize the outcome of absolute dispersion; when a new hire earns more (less) than 75% above (below) the median salary, we set their value equal to 75%.[21] To minimize concerns about seasonality in hiring of some positions, in all of the analysis we re-weight observations to maintain the same composition across Standard Occupational Classification (SOC) groups over time.[22] In addition to the base salary, our employee data includes the monthly gross wage: this is how much money the firm effectively pays to the employee each month, which reflects not only the base salary but also a myriad of other factors such as tax withholdings, commission, bonuses and reimbursements. Last, we complement the administrative data from our partner firm with data from other sources. For example, we can categorize positions by mapping the O*NET codes to some well-known crosswalks.[23]

For the heterogeneity analysis, we categorize positions by skill levels. We define low-skill positions as those that typically require no more than a High-School diploma, that typically employ younger employees and with modest pay. More precisely, we construct the low-skill group in two steps. First, we map O*NET codes to identify positions in job zones 1 and 2 (typically requiring no more than a high school diploma).[24] Second, we exclude positions in which the average worker is above 31 years of age and has an average annual salary above $30,000. Roughly 42% of the sample is classified as low-skill, and the remaining 58% as high-skill. Some examples of low-skill positions in the sample are Bank Teller, Customer Service Representative and Receptionist, while some examples of high-skill positions are Ophthalmic Technician, Production Operations Engineer and Software Developer.

3.5 Sample of New Hires

Firms may use the salary benchmarking tools with different goals in mind. Anecdotal accounts indicate that one of the primary uses of the tool is setting salaries of new hires – this view is supported by the analysis of utilization data.[25] Focusing on new hires has other important advantages. Most importantly, firms often set a salary at the time of hiring a new

[21]Moreover, we drop outlier observations: employees with annual base salaries over $2,000,000 or below $1,000. We also winsorize the salary levels: the base salary and gross wages are winsorized at the 2.5 and 97.5 percentiles within its corresponding position.

[22]More precisely, for each position type, we compute the distribution of SOC groups in the month before onboarding and re-weight all the other periods to match that distribution.

[23]For more details about the data, see Appendix D.

[24]Education status alternatively classified for 27% of observations where there is no job zone classification. We classify positions as low-education if more than 10 percent of employees have at most a high school degree, using data from Zippia.com

[25]Results presented in Appendix E. Firms can use the benchmark data for other goals too. For example, they may use this information to set salaries of their existing employees after they are promoted, or to decide how to respond to an existing employee who received an outside offer.

employee – in contrast, firms set the salaries of their existing employees infrequently and, even when doing so, they may be subject to constrains such as downward wage rigidities. For these reasons, our main analysis focuses on new hires.[26]

The theoretical framework from Section 2 provides a stylized version of hiring new employees, where employers make a take-it-or-leave-it offer. In that model, the salary benchmarks come in handy to set that first offer. In practice, however, the hiring process is more nuanced and, as such, the information on salary benchmarks may be used at different steps of the process. For example, the firm may find that information useful later in the hiring process, when deciding whether to respond to a counter-offer.[27] Or the information may also come in handy earlier in the hiring process, to post wages in job advertisements. For example, using data from Burning Glass, Hazell et al. (2021) reports that only 17% of the job ads includes a posted wage or wage range.

Our main sample of interest consists of new hires from January 2017 through March 2020. We stop at March 2020 for several reasons, most importantly because we want to avoid our baseline results from being affected by the COVID pandemic. In any case, we show that the results hold when we expand the sample to include new hires after March 2020 – for more details, see Appendix F. Since we are interested in what happens around the date when the firm gains access to the tool, we restrict our sample to a window of 10 quarters around the date of onboarding: i.e., up to 5 quarters before the onboarding date, and up to 5 quarters after the onboarding date. In this sample of new hires, we observe 329 unique positions that are ever searched in the compensation explorer.

3.6 Firms in the Sample

The salary benchmarking tool is only available to the payroll clients that subscribe to the cloud services, which launched in late 2015.[28] Most important for our analysis, we observe the exact date firms were granted access to the tool since its inception. Anecdotally, which firms are granted access to the business analytic tools, and when they do so, depends on many arbitrary factors. During the roll-out, account managers were instructed to introduce the tool to business clients at any opportunity, such as calls pertaining to payroll and other services. Nearly all firms that gain access to the business analytics service did not search for the service or request it, but rather, their account manager introduced them to business analytic services as part of a broader conversation.

[26]Results for existing employees are presented in Appendix I.

[27]As suggestive evidence that this channel may play a role, 16.4% of the companies surveyed by PayScale (2021) report that they shared their own benchmarking data with their employees.

[28]However, the benchmarks themselves are based on payroll records for all clients of the payroll company, not just the ones subscribing to the cloud services.

Our main sample comprises 586 firms that gained access to the tool, with onboarding dates between December of 2015 and January of 2020. The vast majority of these firms used the tool at least once.[29] Among the firms with access, we have suggestive evidence that the tool was being used by a small set of employees – most likely members of the Human Resources unit or the compensation team.[30]

We obtained data on an additional 1,419 firms that never gained access to the tool but were selected to match observable characteristics of firms that did get access to the tool: number of employees, state and 6-digit industry codes. We assigned a "hypothetical" onboarding date to the firms that never gain access to the tool. For each control firm, we find the firm with access that is most similar in observable characteristics, and assign the date when that firm obtained access as the hypothetical access date for the control firm.[31] For example, if Ford gains access but Fiat does not, we assume Ford would have gained access when Fiat did.[32]

Table 1 provides a comparison between the firms in our sample and a representative sample of U.S. firms. In terms of size, measured in number of employees, our sample is most representative of the top quartile of firms in the United States. This may reflect the fact that businesses with fewer than 100 employees do not have enough scale to justify the use of data analytics services. In terms of salaries, the employees in our sample are representative of the population of U.S. employees, with the exception that our sample has limited coverage of the bottom quartile of the distribution (earning less than $20,000 per year).

Table 2 provides some statistics about the distribution of industries, given by the first 2 digits of the firm's main 6-digit NAICS code. Columns (1) and (2) compares the distribution of sectors in our sample (column (1)) to the U.S. distribution according to Census data (column (2)). Columns (3) and (4) are the same as columns (1) and (2), except that they are based on the number of employees instead of the number of firms. We should not expect our sample to be perfectly representative of the U.S. industries. For example, as discussed above, the firms in our sample are larger than the U.S. average and as a result they will be more representative of industries with larger firms. While not perfectly representative of the U.S.

[29]More precisely, among the 586 firms with access to the tool, 561 (96%) conducted at least one search during the period for which we have usage data.

[30]For a subset of the utilization data, we observe an identifier for the person conducting the search. For 50% of the firms with access to the tool, there is a single user conducting the searches. Even in firms with multiple users, the searches are concentrated: if you take a random pair of searches, there is a 58.2% probability that they were conducted by the same user. These results have to be taken with a grain of salt, however, as it is possible that the account is being shared by multiple employees, or that one employee is using the tool per request of other employees.

[31]More precisely, for each control firm, we restrict to all treatment firms in the same sector, and then select the firm which is closest according to the Mahalanobis distance for firm size and state.

[32]We use Ford and Fiat purely for illustration purposes, as we work with de-identified data and thus do not know the names of any of the companies in our sample.

average, our sample provides broad coverage of the U.S. industries. Some industries, such as Manufacturing and Finance are somewhat over-represented, while some other industries, such as Construction and Accommodation and Food Services, are somewhat under-represented.

Table 3 presents more descriptive statistics for the firms in our sample. Column (1) shows that the average firm employs 503 employees, 45.3% of which are female, and the average employee is 34 years old and earns a salary of $46,945. Columns (2) and (3) breaks down these average characteristics by whether firms that gain access to the tool (i.e., treatment firms) and firms that do not gain access (i.e., the control firms). Due to the large sample sizes, the pairwise differences are often statistically significant. However, these differences tend to be modest or negligible in magnitude. This finding should not be surprising, given that we asked the partner institution to select control firms that are similar to the treatment firms. Columns (4) and (5) break down the treatment firms in the top half and bottom half based on a measure of higher versus lower utilization of the benchmark tool. Again, firms with high vs. low utilization look very similar to each other in almost all observable dimensions.

3.7 Classification of New Hires

Based on the utilization data, we assign each new hire to one of the following three groups:

- Searched Positions: positions in treatment firms that were either searched in the compensation explorer prior to the hire date or that they will be eventually searched in the tool.

- Non-Searched Positions: positions in treatment firms that were not searched. One potential concern with the classification is that some searched positions may be incorrectly attributed as non-searched. This may be due to the limited window of the searched data,[33] or due to information spillovers. For example, assume a company hires an "accountant" and an "accounting analyst", and searched for the benchmark of "accountant" (and thus this is a searched position) but not for the "accounting analyst" (the non-searched position). Perhaps the two positions are close enough that the company is using the benchmark for "accountant" to set pay for the "accounting analyst" too. In this case, the comparison between searched and non-searched would yield a null effect of the benchmark only because "accounting analyst" is incorrectly being classified as non-searched. To minimize the scope for information spillovers, we exclude from the

[33] For example, it is possible that some positions are being attributed to non-searched because they were not searched after the start of the usage data (September 2019), yet perhaps they were searched prior to September 2019.

non-searched positions all the new hires in positions "adjacent" (i.e., in the same SOC group) to those new hires that *were* searched in the same month.

- Non-Searchable Positions: all positions in the control firms (i.e., those that never gain access to the tool).

The utilization data shows that while firms have access to the benchmark tool, that does not mean that all firms use it, or that they use it all the time. Consider the 534 firms who had onboarded prior to the last quarter of 2019. During that quarter, 199 (37.3%) of these firms hired in at least one position. These firms searched the benchmark for 20.8% of the positions in which they hired.[34] For this reason, there are substantially more new hires categorized as Non-Searched than as Searched. Also, since our sample includes more control firms than treatment firms, we have an even larger number of new hires in the Non-Searchable category. Our final sample includes 5,266 new hires in the Searched category, 39,686 new hires in the Non-Searched category and 156,865 new hires in the Non-Searchable category.

Table 4 lists the 35 most common positions in the Searched category, out of the total of 329 unique Searched positions in the sample. The 3,129 hires in these 35 positions account for a majority (66.7%) of the hires in the Searched category. These common Searched positions include all sorts of occupations such as Bank Teller, Customer Service Representative and Software Developer. Table 4 also reports the number of employees being hired in each position, and number of hiring firms, broken down by whether the hire falls into the categories Searched (column (1)), Non-Searched (column (2)) and Non-Searchable (column (3)). This figure shows that there is quite a bit of overlap in the positions that different firms are searching for. For example, the 468 hires for Customer Service Representative in the Searched category are distributed across 44 different firms. This table also shows that there is no such thing as positions that are always searched: for each firm that searches for a given position (column (1)), there are many other firms hiring in that position that did not conduct a search because they didn't choose to (column (2)) or because they didn't have access (column (3)). For example, while there are 468 new hires Customer Service Representative in the searched category, there are 4,401 hires for that same position in the Non-Searched category and 4,012 in the Non-Searchable category. In other words, these positions are *searched* the most often, largely because those are the positions in which firms *hire* the most often.

Column (1) of Table 5 shows the average characteristics of the employees in the sample of new hires. The average employee is 35 years old, 50.6% of them are female, 81.2% work for an hourly wage, they have an annual base salary of $41,359, an external median benchmark

[34] More precisely, around 62.3% of these firms did not search for any of the positions in which they hired; among the remaining firms, they looked up on average 55.2% of the positions in which they hired.

that is slightly higher ($41,412) and the starting salaries differ from the median benchmark (in absolute value) by an average of 20.4%. The last rows shows the main occupation groups in the sample. 19.8% of the positions are in Office and Administrative Support, 8.0% in Management, 6.6% in Production, 9.3% in Transportation and Material Moving, 4.8% in Building and Grounds Cleaning, and the rest (51.5%) belong to other groups.

Next, we can compare the characteristics across treatment and control groups. As usual in differences-in-differences designs, the key (testable) assumption is that, prior to the onboarding date, the outcome of interest *evolved* similarly between treatment and control groups. As a result, it should not matter whether the treatment and control groups start at difference baselines, or whether they are different in observable characteristics. However, it is always re-assuring to check that there are no extreme differences between the treatment and control groups. Columns (2) through (4) of Table 5 break down the average characteristics for each of the three categories: Searched, Non-Searched and Non-Searchable. Perhaps the two most important characteristics are the (pre-treatment) salary and its absolute %-difference with respect to the benchmark, because they constitute the outcome variables in the analysis that follows. The differences are economically small. For example, the average salaries are $39,064, $42,013 and $41,405 in the Searched, Non-Searched and Non-Searchable categories, respectively. Due to the large sample sizes, the difference between the Searched and Non-Searchable groups is statistically significant (p-value = 0.013), despite modest differences in economic terms. The difference between the Searched and Non-Searched group is not significant (p-value = 0.617). For the other characteristics, the pairwise differences are again almost always statistically significant, but they tend to be economically small. Some exceptions are that, relative to Non-Searched and Non-Searchable positions, Searched positions have a higher share of female employees and a higher share of office and administrative support positions.

4 Effects on Salary Compression

We start by measuring the effects of salary benchmarking on the distribution of salaries. According to the theoretical framework, there should be compression from above: firms who would have otherwise paid above the market benchmark should reduce salaries, thus moving towards the benchmark. On the other hand, the model predicts that there may, or may not be, compression from below too.

4.1 Salary Compression: Histograms

To measure the effects on salary compression, we compare the distribution of the gap between the salaries chosen by the employers and the benchmarks they saw (or could have seen) in the benchmark tool. The results are presented in Figure 2, where each panel shows a pair of histograms for different position types. The x-axis denotes the difference between the starting salary and the corresponding benchmark (i.e., the median market pay). For example, the middle bin corresponds to salaries that are close (±2.5%) to the median benchmark, the bins on the left half of the figure correspond to salaries below the benchmark and the bins on the right half correspond to salaries above the benchmark.

Panel A of Figure 2 corresponds to the Searched positions, with gray bars corresponding to employees who were hired before the firm gained access to the benchmark tool (i.e., when the benchmark information *was not* visible to the firm) and the red bars correspond to employees hired after the onboarding date (i.e., when the benchmark information *was* visible to the firm). The comparison between the two histograms from panel A suggest that, after onboarding, salaries are more compressed around the benchmark. The compression towards the benchmark comes from both sides of the histogram. The model from Section 2 provides a natural interpretation for this finding: as a response to the benchmark information, firms that were going to pay below the benchmark end up offering higher salaries while firms that were going to pay above the benchmark end up offering lower salaries. Indeed, firms are more likely to "hit" the benchmark: the probability that the firm chooses a salary close (±2.5%) to the median benchmark increases from 11.6% before onboarding to 22.1% after onboarding.

One way of measuring dispersion to the benchmark is by means of the absolute mean difference between the salaries and the corresponding benchmarks. This metric suggests that, among Searched positions, salaries were on average 19.4 pp from the benchmark before the firms gained access to the tool. After gaining access to the tool, the average distance to the benchmark dropped from 19.8 pp to 14.9 pp, which is highly statistically significant (p-value<0.001) and also large in magnitude (equivalent to a 24.7% drop).

Next, we use the Non-Searched and Non-Searchable positions as two different control groups. Because the firms never see the benchmark, we should not expect compression towards the benchmark for Non-searched positions. The results for Non-Searched positions are presented in Panel C of Figure 2. The dispersion around the benchmark is similar in magnitude in the pre-onboarding period (20.8 pp) to the post-onboarding period (22.0 pp). Due to the large sample sizes this difference is statistically significant (p-value<.001) and, most importantly, precisely estimated and economically small. Next, Panel C of Figure 2 uses Non-Searchable positions as alternative control group. Because firms cannot see the benchmarks for the Non-Searchable positions, we should not expect compression towards the benchmark either. We find that, again, dispersion around the benchmark is similar in magnitude in the pre-onboarding period (21.1 pp) as in the post-onboarding period (21.9 pp). This difference is statistically significant (p-value<.001) but, it is negligible in magnitude. In our expert prediction survey, only a minority of experts were able to predict this compression finding (Appendix C).

We find that firms want to "aim" for the median market pay. Ex-ante, one could have expected that, instead, firms would have preferred to be stingy, for example, by "aiming" for the 25th percentile of market pay instead of the median. For a more direct comparison, Appendix F reproduces the analysis but, instead of using the median benchmark, it uses each of the alternative benchmarks: 10th, 25th, 75th and 90th percentile of market pay, and the average too. The results confirm that firms are, for the most part, aiming for the median market pay. This evidence is consistent with the SHRM Survey where 56.73% of the HR managers ranked the median salary as the piece of information they care about the most when searching for a position benchmark (see Table L.2). [35]

4.2 Econometric Model

Next, we extend the above analysis to a more traditional differences-in-differences design. Let subscript t denote time, subscript i index employees, and subscript j index firms. Let $\omega_{i,j,t}$ be the starting base salary of employee i when hired by firm j at time t. And let $\bar{\omega}_{i,j,t}$ denote the corresponding benchmark: i.e., the median base salary according to the search tool. Let $Y_{i,j,t}$ denote the outcome variable. For example, in this section the outcome of interest is the absolute difference between the salary of the employee and the benchmark: $100 \cdot |\frac{\omega_{i,j,t}-\bar{\omega}_{i,j,t}}{\bar{\omega}_{i,j,t}}|$. This outcome is normalized so that the effects can be interpreted readily as percentage points.

We have two distinct differences-in-differences designs: one based on the comparison between Searched vs. Non-Searched positions, and the second one based on the comparison between Searched vs. Non-Searchable positions. For the sake of brevity, we'll use Θ_1 to refer to observations categorized as either Searched or Non-Searched, and Θ_2 to the set of observations categorized as either Searched or Non-Searchable. Let $T_{i,j}$ be a dummy variable that takes the value 1 if the employee i's position at firm j was categorized as a Searched position, and 0 if it was categorized as Non-Searched or Non-Searchable. Let $A_{j,t}$ be a dummy variable that takes the value 1 if firm j has access to the benchmark tool in period

[35] It is also consistent with the anecdotal accounts from HR managers, as well as the advice from handbooks on Human Resources (e.g., Berger and Berger, 2008; Zeuch, 2016), which highlight that firms should aim for the median market pay.There is also some evidence that employees, not just employers, may pay particular attention to median salaries (Roussille, 2021).

t and 0 otherwise. This variable will take the value 0 in every month until the month of onboarding, after which it will take the value 1 always (and in the case of control firms, it would correspond to the "hypothetical" onboarding date). Finally, let δ_t denote year dummies, ψ^k denote position dummies and $X_{i,j,t}$ denote a vector of additional controls consisting of the employee's age, a dummy for gender, and a dummy for hourly pay type. And let $\epsilon^k_{i,j,t}$ be the standard error term – unless stated otherwise, all of the analysis in this paper uses standard errors that are clustered at the firm-position-month level. Consider the following regression specification:

$$Y_{i,j,t} = \alpha^k_1 \cdot A_{j,t} \cdot T_{i,j} + \alpha^k_2 \cdot A_{j,t} + \alpha^k_3 \cdot T_{i,j} + X_{i,j,t}\alpha^k_4 + \delta^k_t + \psi^k + \epsilon^k_{i,j,t}, \ \forall \{i,j,t\} \in \Theta_k \quad (14)$$

When $k = 1$, equation (14) boils down to the first identification strategy (Searched vs. Non-Searched). When $k = 2$, equation (14) boils down to the second identification strategy (Searched vs. Non-Searched). The differences-in-differences coefficient of interest is α^k_1, which measures the effect of the benchmark tool. When $k = 1$, α^1_1, measures the difference in outcomes between Searched (treatment) and Non-Searched (control) groups changed post-onboarding relative to pre-onboarding. When $k = 2$, α^2_1, measures the difference in outcomes between Searched (treatment) and Non-Searchable (control) groups changed post-onboarding relative to pre-onboarding.

These two alternative differences-in-differences designs (given by equation (14) for $k \in \{1, 2\}$) are based on different control groups (Non-Searched and Non-Searchable, respectively), and as such they have different pros and cons. The key potential advantage of the comparison between Searched and Non-Searchable positions is that it is not subject to the potential concern about misattributing Searched positions as Non-Searched positions described in Section 3.7. On the other hand, the comparison between Searched and Non-Searched positions has the advantage that it reduces concerns about picking up effects from other tools besides the compensation explorer. While we do not have a strong preference for one strategy versus the other, we want to emphasize that being able to compare the results across the two strategies provides a validation check for the research design.

To test the hypothesis of pre-trends, we follow the standard practice in differences-in-differences studies by introducing a "fake" post-treatment dummy ($A^{\text{fake}}_{j,t}$) which is identical to the true post-treatment dummy ($A^{\text{fake}}_{j,t}$) except that it takes value 1 in the two quarters before the onboarding date. We can expand equation (14) as follows:

$$Y_{i,j,t} = \alpha^k_1 \cdot A_{j,t} \cdot T_{i,j} + \alpha^k_2 \cdot A_{j,t} + \alpha^k_3 \cdot A^{\text{fake}}_{j,t} \cdot T_{i,j} + \alpha^k_2 \cdot A^{\text{fake}}_{j,t} + \alpha^k_4 \cdot T_{i,j} + X_{i,j,t}\alpha^k_5 + \delta^k_t + \psi^k + \epsilon^k_{i,j,t}, \ \forall \{i,j,t\} \in \Theta_k \quad (15)$$

The coefficient of interest is α_3^k, which measures if the difference in outcomes between Searched (treatment) and Non-Searched (control) groups was already changing even before the onboarding date. Under the null hypothesis of no differences in pre-trends between treatment and control groups, we expect $\alpha_3^k = 0$. We can expand the differences-in-differences specification even further to an event-study analysis, by expanding $A_{j,t}$ into a set of dummies. Let $A_{j,t}^s$ be a dummy variable that takes the value 1 if the firm onboarded on period $t-s$. For example, $A_{j,t}^{+1}$ would take the value 1 one quarter post-onboarding, while $A_{j,t}^{-4}$ would take the value 1 four quarters prior to onboarding. And let S be the set of non-zero integers between -5 and +5, but excluding -1 (the reference category).[36] We expand equation (14) as follows:

$$Y_{i,j,t} = \sum_{s \in S} \alpha_{1,s}^k \cdot A_{j,t}^s \cdot T_{i,j} + \sum_{s \in S} \alpha_{2,s}^k \cdot A_{j,t}^s + \alpha_3^k \cdot T_{i,j} + X_{i,j,t}\alpha_4^k + \delta_t^k + \psi^k + \epsilon_{i,j,t}^k, \; \forall \{i,j,t\} \in \Theta_k \quad (16)$$

The set of coefficients $\alpha_{1,s}^k \; \forall s \in S$ correspond to the event-study coefficients. For example, $\alpha_{1,+1}^k$ would correspond to the effect one quarter post-onboarding (relative to the base category, one quarter pre-onboarding), while $\alpha_{1,-4}^k$ would correspond to the "effect" four quarters pre-onboarding.

4.3 Differences-in-Differences Estimates

The event-study results are presented in Figure 3. In each of the panels, the y-axis corresponds to the salary dispersion around the benchmark. The y-axis starts at 0, which is the minimum value that the outcome can take, corresponding to the extreme case in which salaries are exactly equal to the corresponding benchmarks. The higher the value of the y-axis, the more different salaries are from the benchmark. For example, a value of 20 would mean that salaries differ from the benchmark, on average, by 20%. The x-axis corresponds to the time since the date of onboarding, from -5 (i.e., 5 quarters prior to the month of onboarding) to +5 (i.e., 5 quarters after to the month of onboarding). To make the interpretation of the effect sizes more straightforward and intuitive, we follow Hastings and Shapiro (2018) by normalizing the y-axis. In this and all other event-study graphs, all coefficients are shifted by the same constant, as to match the average of the baseline outcome in the pre-treatment period. That's the reason why the coefficient for quarter -1 is the omitted category yet its value is different from 0.

The event-study findings are presented in Figure 3. These findings indicate that the effects on salary compression coincide precisely with the timing of access to the benchmark:

[36] In all the analysis, we drop observations for employees who were hired in the exact month of onboarding. Due to the coarseness of the timestamps, it would be impossible for us to distinguish between the hires that were post- vs. pre-onboarding.

the dispersion with respect to the benchmark was stable in the quarters before the firm gained access to the tool, dropped sharply in the quarter after the firm gained access, and remained stable at the lower level afterwards.

Panel A of Figure 3 corresponds to the comparison between Searched (denoted in red dots) and Non-Searched (blue squares) positions. For the Searched positions, the dispersion with respect to the benchmark was stable at around 19.8 pp prior to the onboarding, but then dropped sharply to around 14.9 pp in the quarter after onboarding and remained stable at that lower level afterwards. In contrast, the compression in non-searched positions was stable around 20.8 pp prior to onboarding, and remained stable at a similar level (22.1 pp) after the onboarding date. Panel C of Figure 3 corresponds to the difference between the two series from Panel A. This differences-in-differences comparison suggests that the benchmark tool reduced the salary dispersion from 19.8 pp to 14.8 pp (p-value<.001), equivalent to a 25.3% reduction.

Panel B of Figure 3 corresponds to the comparison between Searched (denoted in red dots) and Non-Searchable (purple squares) positions. While the compression for Searched positions dropped sharply after onboarding, the compression in Non-Searchable positions remained stable around the date of onboarding. Panel D of Figure 3 correspond to the difference between the two series in panel B. This differences-in-differences approach suggests that the benchmark tool reduced the salary dispersion from 19.8 pp to 13.6 pp (p-value<0.001). The drop in dispersion from panel D (6.2 pp) is close in magnitude to the corresponding drop from panel C (5 pp) – furthermore, these two effects are statistically indistinguishable from each other. The fact that the results are qualitatively and quantitatively consistent across the two identification strategies is re-assuring about their validity.

4.4 Robustness Checks

Table 6 presents the differences-in-differences estimates in table form. The main advantage of this table is that it summarizes the differences-in-differences results in fewer coefficients, which maximizes the statistical power and also is more practical for the purpose of comparing the results across alternative specifications. Panel A of Table 6 presents the post-treatment coefficients (i.e., α_1^k from equation (14)). Column (1) of Table 6 corresponds to the baseline specification. The post-treatment coefficients are negative and statistically significant: -4.775 (p-value<0.001) when using non-searched positions as control group, and -6.149 (p-value<0.001) when using non-searchable positions as control.

In turn, Panel B presents the corresponding "pre-treatment" coefficients. In the comparison to non-searched positions, this parameter corresponds to parameters α_3^k from equation (14). Under the assumption of no differences in pre-trends between treatment and control groups, we expect these coefficients to be close to zero. As expected, the pre-treatment coefficients in column (1) are close to zero (-0.346 and -0.310, respectively), statistically insignificant (p-values of 0.749 and 0.604) and precisely estimated.

Columns (2) through (12) are identical to column (1), except that they change a different feature of the baseline specification. In columns (2) and (3), we use alternative versions of the dependent variable. In column (2), we measure dispersion using the log difference: $100 \cdot |log(\omega_{i,j,t}) - log(\bar{\omega}_{i,j,t})|$. This outcome is multiplied by 100, just like the outcome from column (1), so that they can be interpreted as percentage points and also readily comparable to each other. The results from column (2) are qualitatively and quantitatively consistent with the results from column (1). In column (3), we measure dispersion with a dummy variable that takes the value 100 if the salary is over 10% away from the benchmark, and 0 otherwise. Again, the results are both qualitatively and quantitatively similar between columns (1) and (3). For example, the first post-treatment coefficient from column (1) suggests that, relative to the baseline, dispersion dropped by 24.1% ($= \frac{4.775}{19.812}$), while the corresponding coefficient from column (3) suggests a decline of 25.5% ($= \frac{16.270}{63.732}$).

The specification from column (4) is identical to the baseline specification from column (1), except that the dependent variable is winsorized at ± 100% instead of ± 75%. Column (5) is identical to column (1), except that it uses heteroskedasticity-robust standard errors instead of clustered standard errors. Column (6) is identical to column (1), except that it does not include any of the additional control variables. Column (7) is identical to column (1), except that it adds position fixed effects. Column (8) is identical to column (1), except that it adds firm fixed effects. Column (9) is identical to column (1), except that it excludes positions for which the base salary is not a major component of compensation: Waiter/Waitress, Chauffeur, and Bartender/Mixologist. Column (10) is identical to column (1), except that it restricts the sample to include only the 329 positions that appear at least once as Searched positions. Column (11) is identical to column (1), except that it does not re-weight observations by SOC groups. Column (12) is identical to column (1), except that it only includes new hires aged 21 through 60. In all these alternative specifications, the results are both qualitatively and quantitatively similar to those from column (1).

In Appendix F, we present some additional robustness checks. In Appendix F.2 we show that there was no significant effects on the composition of new hires. In Appendix F.3 we show that the results are consistent under a range of alternative specifications such as using no filters, excluding outliers and including new hires after March-2020.

4.5 Heterogeneity Analysis

The estimates presented above mask some meaningful heterogeneity. To illustrate this, Figure 4 breaks down the baseline results from Figure 2 by low-skill and high-skill positions. The panels on the left hand side of Figure 4 (i.e., panels A, C and E) correspond to the low-skill positions, while panels on the left hand side (i.e., panels B, D and F) correspond to high-skill positions. Let's start with panels A and B of Figure 4, corresponding to the Searched positions. The comparison between these two panels shows two stark differences. First, before the firms had access to the tool (i.e., the gray bins), there was a lot more compression among the low-skill positions (Panel A) than among the high-skill positions (Panel B). The second finding is that, among low-skill positions (Panel A), salaries get significantly more compressed around the benchmark: dispersion drops from 14.5 pp to 8.7 pp (p-value<0.001). On the contrary, there is a more modest compression for high-skill positions (Panel B): dispersion goes from 24.0 pp to 20.5 pp (p-value=0.021).

Panel C through F of Figure 4 reproduce the analysis for Non-Searched and Non-Searchable positions, for falsification purposes. As expected, the differences in compression between post-onboarding and pre-onboarding salaries are sometimes statistically significant, due to the large sample sizes, but mostly economically small. In Appendix H, we present additional results on the heterogeneity analysis. Using the differences-in-differences framework, we show that the difference in effects between low-skill versus high-skill groups is not only large, but also statistically significant: p-values of 0.070 and 0.403 for the comparisons of Searched vs. Non-Searched and Searched vs. Non-Searchable, respectively. As additional robustness check, this appendix provides the detailed event-study analysis broken down by skill levels. Last, in the main specification, the definition of skill combines information on the position averages by education, age, and salary. We show that the results are roughly consistent if we look at the heterogeneity by each of these position characteristics separately. We also show that, in contrast, there is no heterogeneity by other position characteristics.

Why do benchmarks have a stronger effect for low-skill jobs than for high-skill jobs? Low-skill positions often involve standardized task, minimal training, and can be easily monitored. For that reason, one worker is as good as another for the purposes of the job. As one HR practitioner put it, workers in those jobs are "viewed as interchangeable" (Adler, 2020b). According to interviews with compensation experts, low-skill jobs can lead to what Adler (2020b) calls *standardization*: once a candidate is deemed qualified for the job, their pay is a function of the job, not their individual characteristics. All this suggests that low-skill jobs can be modeled as auctions for workers whose productively is, to a large extent, common across firms hiring in these positions. Specifically, worker productivity has a common component Q that leads worker values to be affiliated across firms. In such markets, our model predicts compression of pay and, in equilibrium, higher pay.

For high-skill workers, on the contrary, the value that they can create may be very different for different employees and for different companies. For example, a Software Engineer may be an excellent fit for some firms, and thus create a lot of value, but a poor fit for others, and thus create less value. According to interviews with HR practitioners, candidates for these positions are treated as unique and the offers are tailored to the specific candidate (Adler, 2020b). When tailoring the offer, the HR manager may use the market pay data as starting point, but there are a myriad of other factors that can come into play, such as the line manager's opinion of the candidate, and the match-specific set of skills the line manager needs. For these reasons, the salaries offered to these workers across firms, revealed through the salary benchmark, may be less informative about the marginal revenue the worker could create at any one particular firm. Additionally, when tailoring the offer, the HR manager may also have more personal information above and beyond the salary benchmark, such as the candidate's own salary history, outside offers and salary expectations. These interpretations could explain the main findings from Panels A and B of Figure 4. Consider the salary dispersion with respect to the benchmark before onboarding. Relative to high-skill positions, the salaries of low-skill positions are more compressed around the benchmark even before the firms gain access to the tool. This is consistent with the idea of standardization, according to which firms are trying to pay all candidates as closely as possible to the market pay. After onboarding, the salaries in low-skill position get even more compressed around the benchmark, again suggesting that employers are trying to hit that mark.

In addition to the qualitative interviews with HR managers, there is also survey evidence consistent with the interpretation provided above. Relative to low-skill employees, high-skill employees are substantially more likely to engage in salary negotiations (Hall and Krueger, 2012).

An alternative explanation is that when firms look up low-skill positions, they are interested in learning about base salary, but when they look up high-skill positions firms may be more interested in other forms of compensation (such as bonuses and commissions). However, this is unlikely to explain our results, considering that base salary comprises the vast majority of compensation in both lower and higher skill positions.[37]

In our expert prediction survey, the experts predicted the opposite of what we find: a majority predicted benchmarking would more strongly influence high-education positions

[37] According to the benchmark data, among low-skill positions, 95.2% of the total cash compensation comes in the form of base salary. For high-skill positions, the corresponding figure is 92.9%. One caveat, however, is that our measure of total compensation does not include stocks, which may be important at the highest levels of the organizations (e.g., executives) and also in some particular contexts (e.g., software developers working at startups).

(Appendix C). The open-ended responses reveal experts, regardless of their response, often note high-education positions should have less compression at baseline. The responses diverge because those who believed high education positions would be more strongly affected tended to interpret this to mean that for high education positions "information about the true distribution should be more valuable". Those who select low education positions interpreted that for high-education positions the benchmark would be less relevant (e.g. "Higher end jobs are more heterogeneous and therefore firms have more reasons to differentiate from the market median").

4.6 Magnitude and Interpretation of the Effects

The drop in compression documented above is not only highly statistically significant, but also large in magnitude. This estimate is probably a lower bound on the *true* effect of benchmarks, due to multiple potential sources of attenuation bias. The first source is that the tool offers many figures (e.g., median salary, different combination of filters) but we do not know exactly which number each person searching was interested in and paid closest attention to. Another source of attenuation bias is that in some cases, even though the firm hired in position X, they may have looked up the benchmark for position X to negotiate the salary of an existing employee in that position, but not to set the salary of a new hire. Likewise, when multiple people get hired in a particular firm-position, our specification is implicitly assuming that the firm will use that information for everyone who gets hired in that position going forward. However, perhaps the manager was looking that information up for one specific new hire and will "forget" the information for future hires. A last source of attenuation is that the tool we study is not the only source of data on market values, so firms in the treatment and control groups may be using other sources of data on market salaries. Therefore, our estimates should be interpreted as intention-to-treat effects from adding one source of benchmark information.

To the extent that the effects can be heterogeneous across positions, we are estimating a treatment effect on the treated. In other words, we estimate the effects of salary benchmarking for positions that end up being searched – had they been searched, the effects could have been different for positions that were not searched. For example, following the logic of rational inattention, it could be argued that firms are looking up the positions for which they need the information the most. If they need the information the most, they are arguably planning to use it the most too. In that case, our estimates for the positions that are looked up may overestimate the strength of information frictions for the average position. Nevertheless, the fact that we estimate treatment effects on the treated is not necessarily a limitation. On the contrary, for the purpose of policy implications, the treatment effects on the treated may be the most relevant object of interest. For example, from the perspective of policy implications, the counterfactual of interest is not what would happen if all firms were "forced" to look up every position, but what would happen if all firms had the "option" to look up the positions they want. In that sense, the treatment effects on the treated are the right object of interest.

Last, it is worth noting that our model makes a prediction about the distribution of salaries among those bids that get accepted, and this is precisely what we test with our data. Additionally, it would be interesting to estimate the effects on the distribution of all bids. For instance, it is possible that some firms who were planning to make an offer below the benchmark, after looking up the benchmark information, end up deciding not to hire at all. Unfortunately, we do not have sufficient data to test these additional hypotheses.

5 Average Effects of Salary Benchmarking

The above evidence suggests that the use of salary benchmarks has a significant effect on the wage determination process. We next explore how this practice effects average salary levels and its employment implications, such as the retention of new hires.

5.1 Effects on Salary Levels

To estimate the average effects of salary benchmarking, we use the same identification strategy from the analysis of compression described in Section 4 above. The key difference is that, instead of using salary compression as dependent variable, we use other outcomes, such as the salary level.[38]

The event-study results for the salary levels are presented in Figure 5. This figure is identical to Figure 3, except that the y-axis is the level of salary (in logs). This evidence suggests that, for the average employee, and regardless of the specification, salary benchmarking does not have a negative effect on the average salary. If anything, the effect on the average salary is positive, but small in magnitude and statistically insignificant.

Panel A of Figure 5 corresponds to the comparison between Searched (denoted in red dots) and Non-Searched (blue squares) positions. During the pre-onboarding period, the Searched and Non-Searched positions were stable and at similar levels. In the post-onboarding period, both the Searched and Non-Searched positions continued at their pre-onboarding levels.

[38]The estimates on average salary are not subject to one of the sources of attenuation bias described in Section 4.6 above: this analysis does not require data on the benchmarks that the firm saw in the platform, so it is not subject to that source of measurement error. These estimates, however, are still subject to some of the other sources of attenuation bias.

Panel C of Figure 5 corresponds to the difference between the two series in panel A. This differences-in-differences comparison suggests that there is no significant effect of access to the benchmark. More precisely, the differences-in-differences estimate suggest that access to the tool decreased the average salary by 0.002 log points, which is statistically insignificant (p-value=0.756) and also economically modest: equivalent to an effect of just 0.2%.[39]

Panel B of Figure 5 corresponds to the comparison between Searched (denoted in red diamonds) and Non-Searchable (purple circles) positions. Again, the average salary evolved similarly between Searched and Non-Searchable positions during the pre-onboarding period, and these pre-onboarding levels remained similar in the post-onboarding period too. Panel D of Figure 5 corresponds to the difference between the two series in panel B. This differences-in-differences comparison indicates that access to the tool had a slight positive effect on the average salary. More precisely, access to the tool increased the average salary by 0.017 log points (p-value=0.308), equivalent to a salary raise of 1.7%. Moreover, the results from panel D are close in magnitude to the results from panel C, and statistically indistinguishable from each other. The fact that the results are qualitatively so consistent across the two identification strategies is re-assuring about the validity of the findings. In our expert prediction survey, the experts' most accurate predictions were for this outcome (Appendix C). In the Appendix, we present some additional robustness checks, which are briefly summarized below. In Table 6, we show that the effects on salary compression are robust to a wide range of alternative specifications. In Appendix G.1, we show that the effects on salary levels are also robust to this same range of alternative specifications. Appendix G.2 also shows that the results are consistent under a more extensive set of specifications.

Given the strong heterogeneity in salary compression between low-skill and high-skill positions reported in Section 4 above, it is natural to explore this same heterogeneity for salary levels. The results are presented in Figure 6. Panels A and B correspond to the results for low-skill positions, while Panels C and D correspond to the high-skill positions. When considering high-skill positions, there is no evidence of significant effects on the salary level. When considering the low-skill employees, the evidence points to a modest increase in their average salary. Depending on whether the Non-Searched or Non-Searchable positions are used as control, the gains in average salary are estimated at 5.0% (p-value=0.014) and 6.7% (p-value=0.001), respectively.

[39]To be more precise, the effect is 0.2002% ($= 100 \cdot (exp(0.002) - 1)$). Since the approximation error is so small, in the remainder of the paper we treat log-point effects and percent-effects as interchangeable.

5.2 Effects on Retention Levels

Next, we estimate the effects of salary benchmarking on retention of new hires. The results are presented in Figure 7. For the sake of brevity, this figure presents the results broken down by low-skill and high-skill positions – the results for the full sample are presented in Appendix G. We find evidence suggesting that, among low-skill employees, the gains in average salary were followed by an increase in retention rates, measured as the probability that the employee is still working at the firm 12 months after the hiring date. Depending on whether the Non-Searched or Non-Searchable positions are used as control, the gains in retention probability are estimated at 6.6 pp (p-value=0.101) and 6.8 pp (p-value=0.029), respectively. The relative magnitude between the effects on average salary and retention are consistent with the best estimates of retention elasticities (Dal Bo et al., 2013).

This evidence suggests that firms may be using salary benchmarking to raise some salaries in an effort to improve, among other things, the retention of their employees.[40] This interpretation coincides with the typical motivation for salary benchmarking given in textbooks on Human Resources. For example, Berger and Berger (2008) states that: "No organization wants to waste their financial resources by paying too high relative to the market; and those who pay too low risk unwanted turnover from employees looking for a better deal elsewhere."

6 Conclusions

Most medium and large firms use salary benchmarking in their compensation strategies. Despite their pervasiveness, there is no evidence on the effects of these tools.[41] To fill this gap, we provide theoretical and empirical evidence. Our model makes predictions about the effects of salary benchmarking. We then test those predictions using administrative data from the largest payroll company in the United States. The evidence suggests that salary benchmarking has a significant effect on pay setting, and in a manner consistent with the predictions of the model. For instance, we find that access to the tool compresses salaries towards the market benchmark quite significantly, and especially in low-education positions.

Our findings have implications for the understanding of how labor markets work in practice. We are the first to document how firms use their salary benchmarking tools and,

[40] In addition to the retention rate, we can also use our data to estimate the effects of salary benchmarking on the average hiring rate. The estimates, which are reported in Appendix G.1, are unfortunately underpowered.

[41] A literature on the disclosure of CEO pay has been framed in terms of salary benchmarking, likely because of the practice of choosing peer CEOs against which to compare compensation. These CEO compensation data are generally disclosed to the employer, employee, and the broader public, making the practice more similar to other forms of full pay transparency such as the posting of salaries for public employees (Mas, 2016).

additionally, the effects of these tools on pay-setting. This evidence has two important implications for the understanding of labor markets. First, it shows that salary benchmarking plays an significant role in pay-setting and as such it deserves further study. Second, this constitutes direct evidence that information frictions around salaries are significant, even among medium and large firms with hundreds or thousands of employees. Furthermore, our evidence shows that firms can use big data to ameliorate their information frictions.

Our findings have implications for a current policy debate. While U.S. legislation currently allows employers to use aggregated data on market wages, that practice has been challenged by an Executive Order in July 2021 (White House, 2021) stating that "Workers may also be harmed by existing guidance (...) that allows third parties to make wage data available to employers and not to workers (...)." This gut feeling is arguably rooted on a simple intuition about bargaining: if employers get access to information that employees do not have, it could give them more leverage in salary negotiations. Despite this renewed interest in the policy, there is not evidence as to whether the use of market-level data indeed leads employers to suppress wages. Our study takes the first step by providing theoretical and empirical evidence.

While we cannot rule out that salary benchmarking could have some undesirable effects, our evidence runs counter to views of policy-makers. Our theoretical model indicates that, far from suppressing pay, in equilibrium benchmarking tools would lead to gains in average salary. And while our empirical evidence cannot speak to the equilibrium effects, it shows that when one firm has access to the benchmark, it leads, if anything, to modest salary gains, concentrated among low-skill employees. On the other hand, employers benefit too, as the evidence suggests that those salary gains are accompanied by gains in retention.

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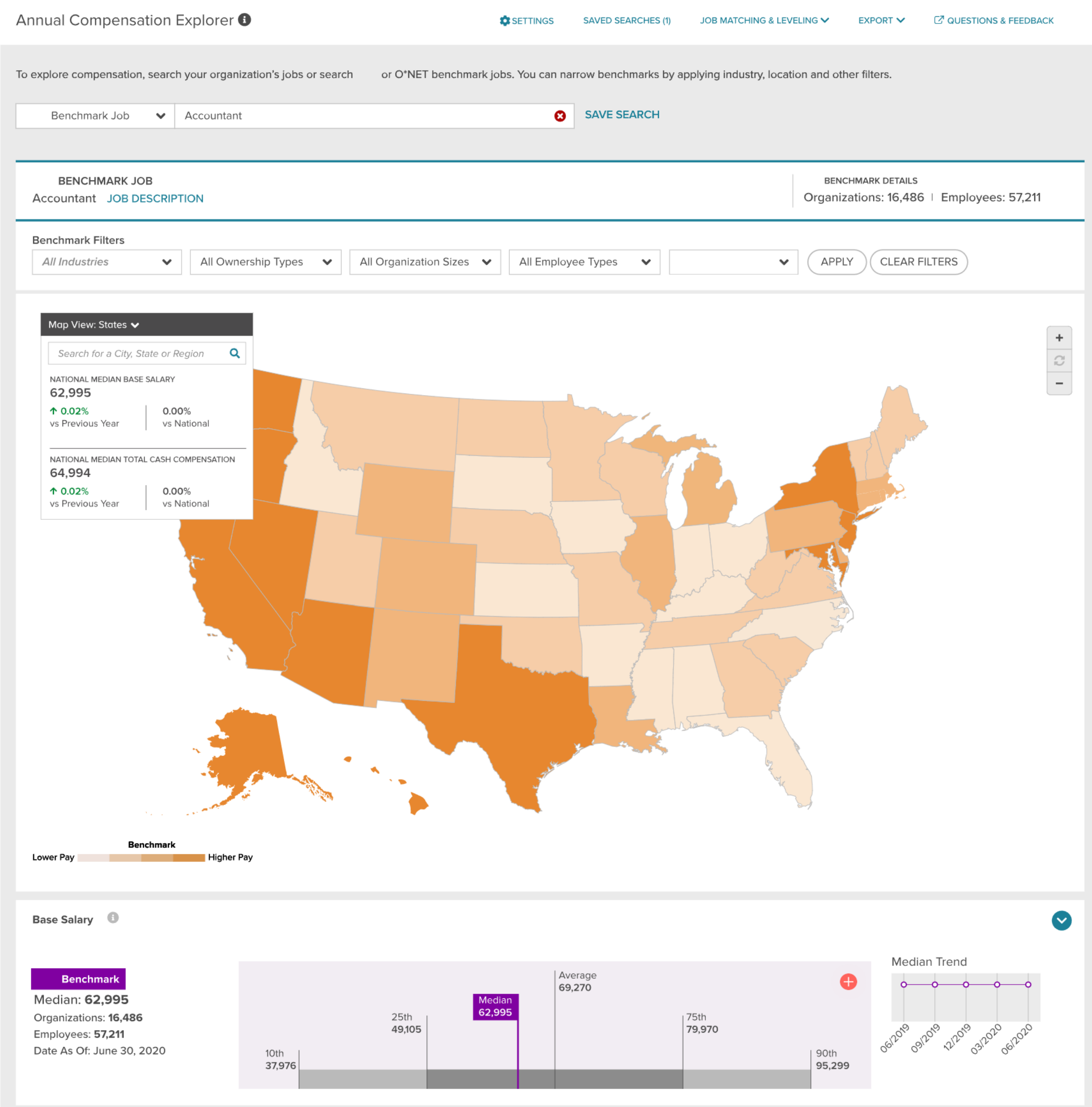
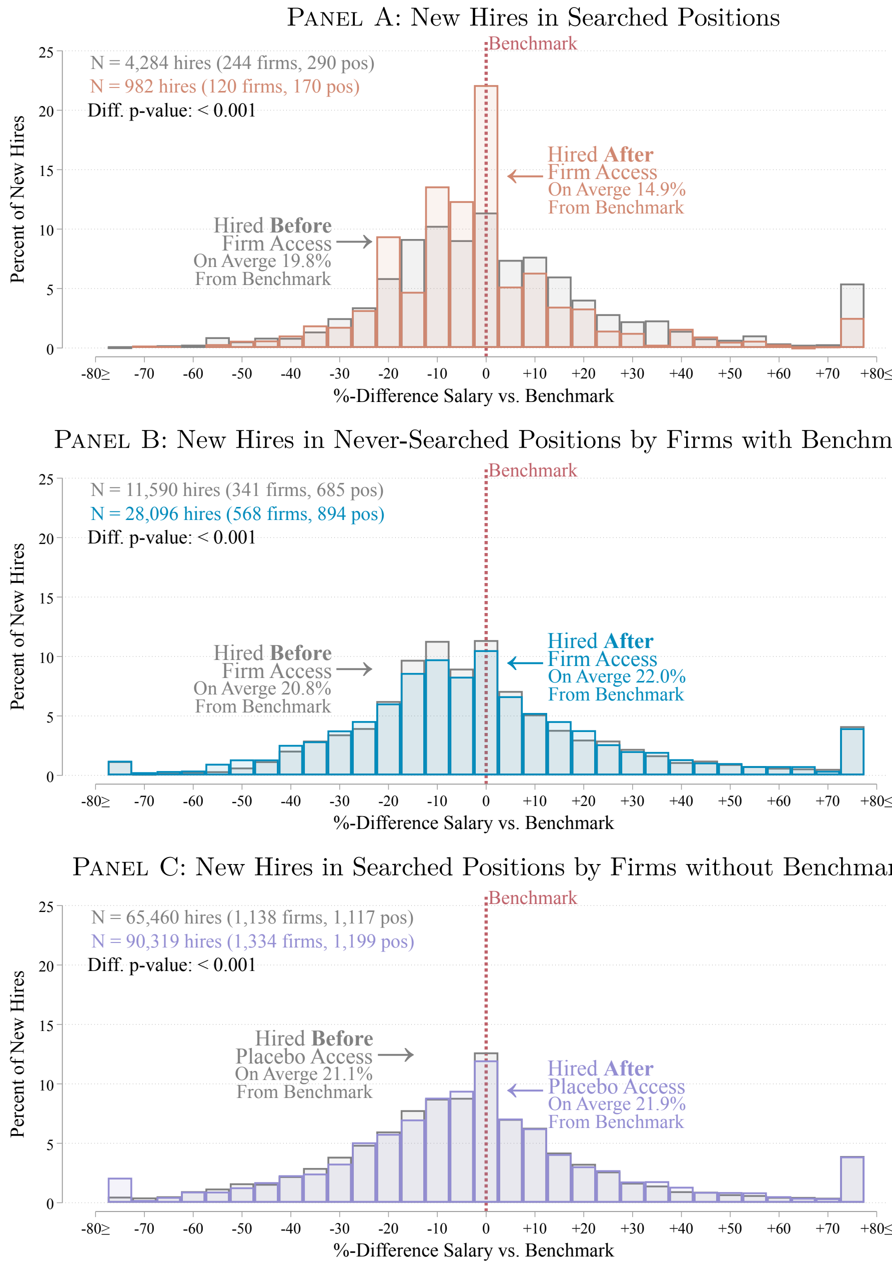
Figure 1: Screenshot of the Salary Benchmarking Tool



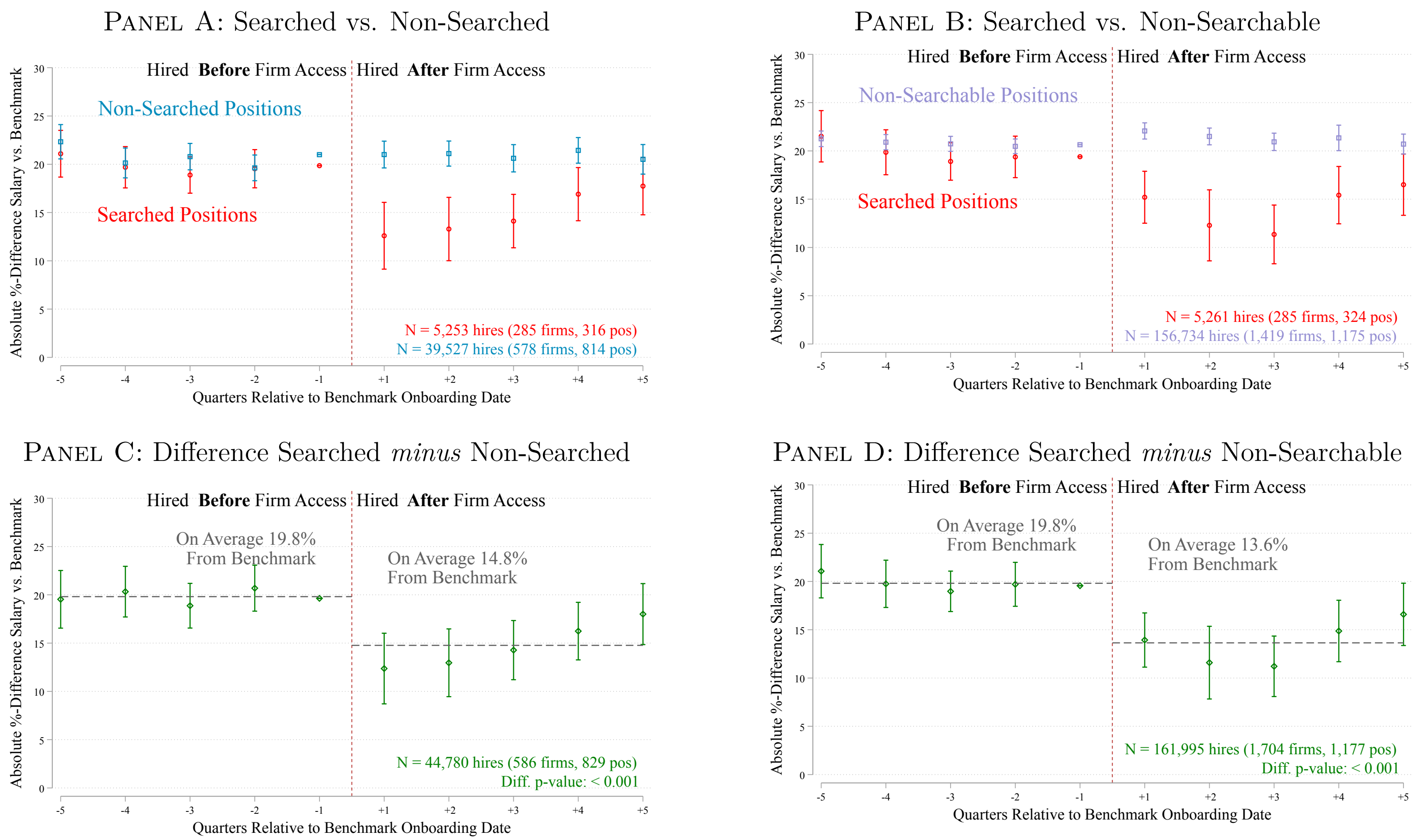
Notes: This is a screenshot of the pay benchmarking tool. It has been slightly altered to conceal the identity of the firm. This is the top of the screen. If you scroll down, you can see panels similar to the bottom panel titled *Base Salary* but for *Bonus*, *Overtime*, and *Total Compensation*.

Figure 2: The Effects of the Compensation Benchmark: Non-Parametric Analysis



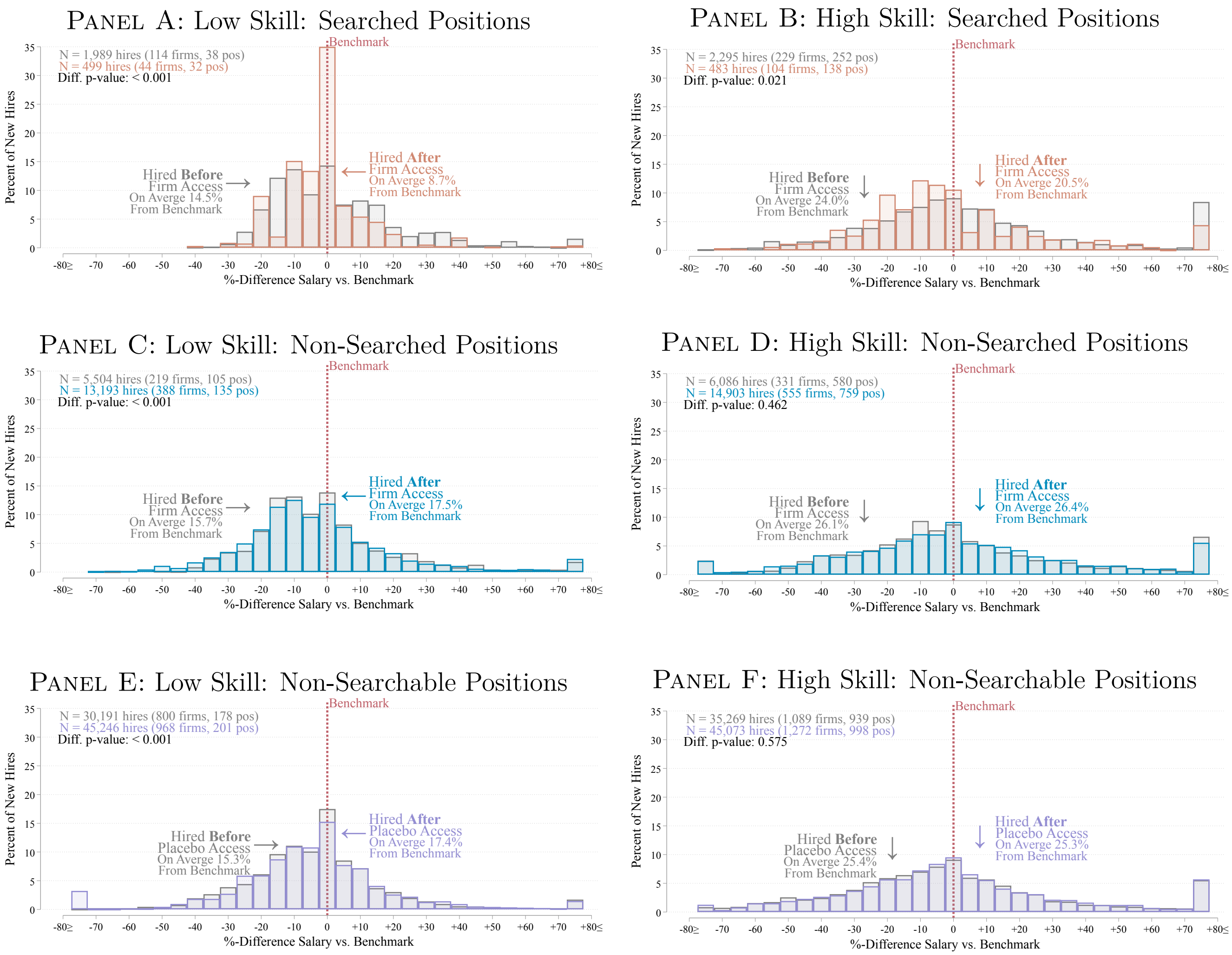
Notes: Histograms of the starting base salary relative to the corresponding external benchmark (winsorized at ± 75%). Each panel corresponds to a different set of positions: panel A for *searched* positions (i.e., positions in firms with access to the benchmark tool that are eventually searched for by the firm), panel B for *non-searched* positions (i.e., positions in firms with access to the benchmark tool that are not eventually searched for by the firm), and panel C for *non-searchable* positions (i.e., positions in firms without access to the benchmark tool). In each panel, the solid and hollow bars correspond to the observations before and after the firm gains access to the benchmark tool, respectively (and in panel C, that date corresponds to the "placebo" onboarding date assigned to the firm that never gains access to the tool).

Figure 3: Event-Study Analysis: Effects on Pay Compression



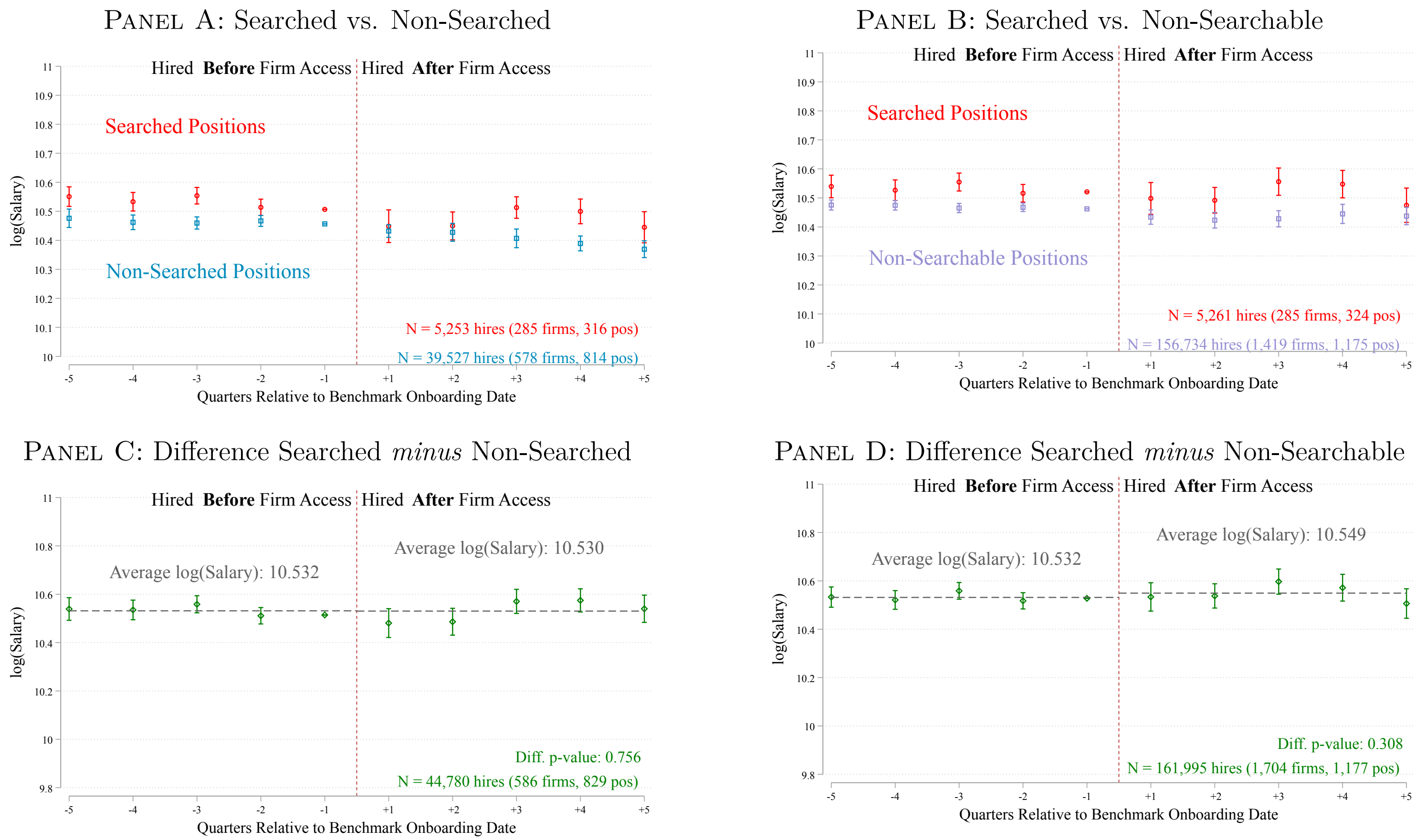
Notes: Point estimates with 90% confidence intervals in brackets, using standard errors clustered at the firm-position-month level. Panels A and C are based off one regression for searched and non-searched positions, while panel A presents the estimates for each position type and panel C presents the difference. Panels B and D are analogous for searched vs. non-searchable positions. All coefficients are shifted such that the pre-treatment coefficients average to the pre-treatment mean of the absolute dispersion outcome. Coefficients in panels C and D refer to parameters $\alpha^k_{1,s}$ $\forall s \in S$ from equation (16) (see Section 4.2 for details).

Figure 4: Heterogeneity: Non-Parametric Analysis



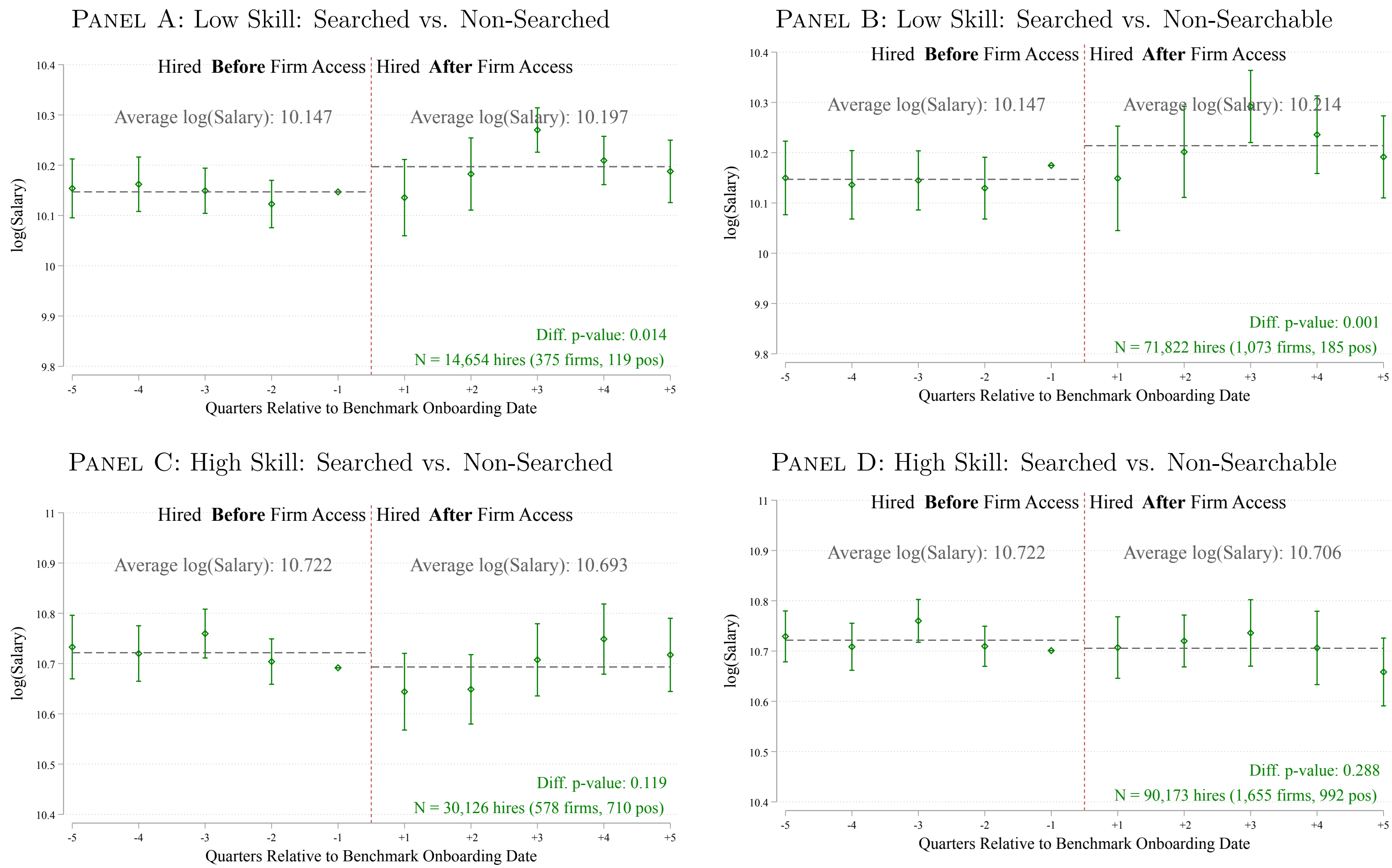
Notes: All figures are a reproduction of the corresponding panel of Figure 2 for low skill positions (left) and high skill positions (right).

Figure 5: Event-Study Analysis: The Effects on Salary Levels



Notes: Point estimates with 90% confidence intervals in brackets, using robust standard errors. Panels A and C are based off one regression for searched and non-searched positions, while panel A presents the estimates for each position type and panel C presents the difference. Panels B and D are analogous for searched vs. non-searchable positions. All coefficients are shifted such that the pre-treatment coefficients average to the pre-treatment mean of log salary. Coefficients in panels C and D refer to parameters $\alpha^k_{1,s}\ \forall s \in S$ from equation (16) (see Section 4.2 for details).

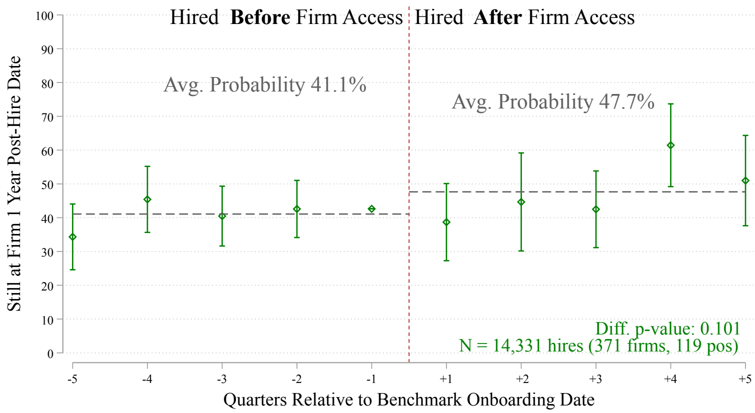
Figure 6: Heterogeneity by Skill: The Effects on Salary Levels



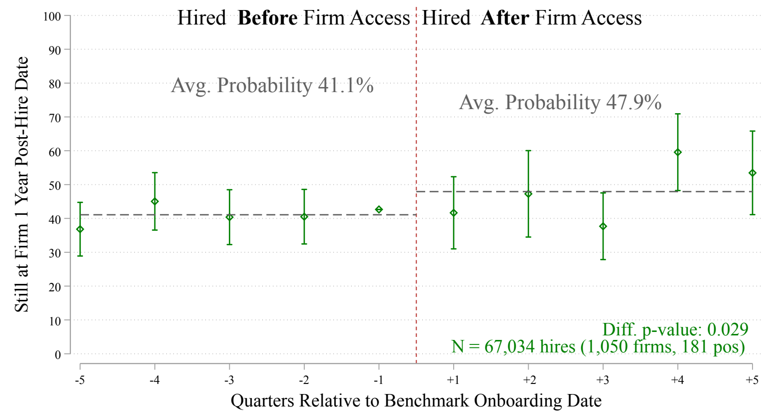
Notes: Panels A and C are a reproduction of panel C from Figure 5, and panels B and D are a reproduction of panel D, but for the specified sub-samples. *Skill* is defined in Section 3.4. See the notes of Figure 5 for more details.

Figure 7: Heterogeneity by Skill: The Effects on Retention Rates

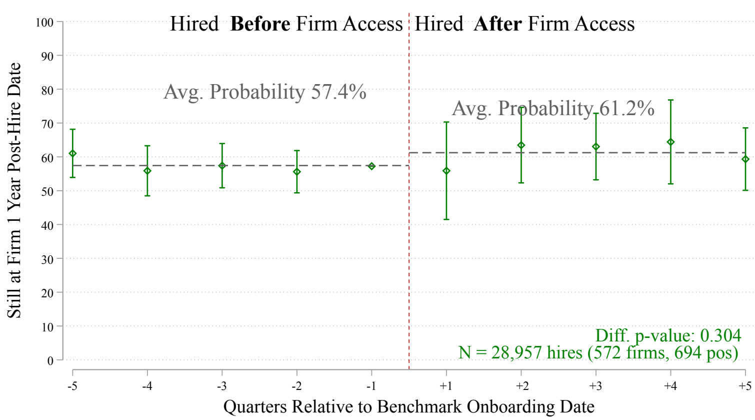
Panel A: Low Skill: Searched vs. Non-Searched



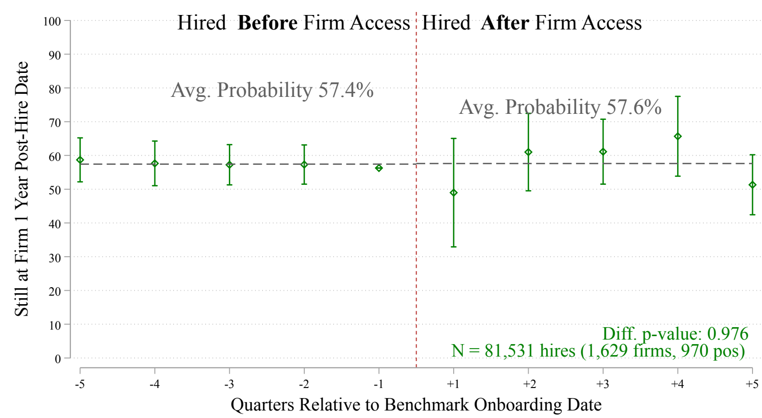
Panel B: Low Skill: Searched vs. Non-Searchable



Panel C: High Skill: Searched vs. Non-Searched



Panel D: High Skill: Searched vs. Non-Searchable



Notes: This is a reproduction of Figure 6, but with the outcome being a dummy equal to 100 if a new hire in a given month is still at the same firm 1 year later. Because our main sample ends in March 2020 and our data ends in July 2021, we observe this outcome for all new hires in our main sample. For more details, see notes to Figure 6.

Table 1: Comparison of Firms in Our Sample vs. Representative Sample of U.S. Firms

	Percentile				
	10th	25th	50th	75th	90th
Number of Employees					
Our Sample	68	109	225	529	1,159
U.S. Representative Sample	22	26	39	79	189
Salary (Annual $)					
Our Sample	20,071	25,468	38,177	64,604	105,689
U.S. Representative Sample	9,820	19,200	36,000	63,200	104,000

Notes: *U.S. Representative Sample* corresponds to the statistics of firms taken from the most recent year (2013) of Song et al. (2019). *Our Sample of Firms* corresponds to the sample of 2,051 firms in our dataset for the earliest period for which data is available (January 2016). To make the statistics more comparable across the two samples, we match the sample restrictions from Song et al. (2019) by excluding firms with less than 20 employees and employees younger than 20 years old or older than 60 years old. Our *Salary* statistics are based off the distribution of individual annual base salaries across employees in all firms. Song et al. use earnings. To make the two samples more comparable, we converted the salary statistics in our sample to 2013 dollars using the PCE deflator published by the Bureau of Economic Analysis.

Table 2: Comparison of Sector Representation in Our Sample vs. U.S. Employees & Firms

	Firms (%)		Employees (%)	
	(1)	(2)	(3)	(4)
Sector	Our Sample	U.S.	Our Sample	U.S.
Agriculture, Forestry, Fishing and Hunting	0.25	0.37	0.37	0.13
Mining, Quarrying, and Oil and Gas Extraction	0.44	0.32	0.11	0.45
Utilities	0.44	0.10	0.34	0.50
Construction	2.33	11.58	0.51	5.08
Manufacturing	22.22	4.10	21.94	9.12
Wholesale Trade	8.87	4.92	14.24	4.76
Retail Trade	3.90	10.70	7.82	12.21
Transportation and Warehousing	2.20	3.05	1.25	3.78
Information	2.77	1.32	3.71	2.73
Finance and Insurance	13.91	3.94	11.10	4.98
Real Estate and Rental and Leasing	3.02	5.11	1.58	1.67
Professional, Scientific, and Technical Services	11.83	13.39	8.56	6.93
Management of Companies and Enterprises	1.01	0.45	1.29	2.69
Administrative and Support and Waste Management	4.59	5.74	6.58	9.25
Educational Services	2.64	1.54	2.51	2.87
Health Care and Social Assistance	11.33	10.81	13.42	15.74
Arts, Entertainment, and Recreation	0.57	2.15	0.40	1.84
Accommodation and Food Services	1.95	8.91	1.59	10.96
Other Services (except Public Administration)	5.73	11.50	2.70	4.30

Notes: Percent of firms and employees in each sector in our sample vs. in the U.S. The NAICS code *Public Administration* excluded from statistics of our sample because the Census does not report data for that code.

Table 3: Summary Statistics for Firms with vs. without Access

		Has Access?		By Usage	
	(1)	(2)	(3)	(4)	(5)
	All	No	Yes	Higher	Lower
Average Firm Characteristics					
Average Employment	503.3	509.8	483.2	525.7	444.5
	(28.1)	(33.2)	(52.1)	(50.3)	(88.4)
Turnover Rate (%)†	2.424	2.438	2.382	2.392	2.374
	(0.061)	(0.070)	(0.126)	(0.159)	(0.192)
Business Services Sector (%)	17.27	16.73	18.94	14.62	22.87
	(0.99)	(1.13)	(2.07)	(2.71)	(3.07)
Hospitality Sector (%)	2.62	2.83	1.95	2.34	1.60
	(0.42)	(0.50)	(0.73)	(1.16)	(0.92)
Retail & Wholesale Trade Sector (%)	12.04	11.97	12.26	16.37	8.51
	(0.85)	(0.98)	(1.73)	(2.84)	(2.04)
Health Care Sector (%)	8.47	7.95	10.03	11.70	8.51
	(0.73)	(0.82)	(1.59)	(2.46)	(2.04)
Banking Sector (%)	7.16	7.13	7.24	7.02	7.45
	(0.68)	(0.78)	(1.37)	(1.96)	(1.92)
Other Sector (%)	52.44	53.38	49.58	47.95	51.06
	(1.31)	(1.51)	(2.64)	(3.83)	(3.66)
Average Employee Characteristics					
Salary (annual $)†	46,945	46,439	48,488	45,232	51,449
	(794)	(956)	(1,356)	(1,632)	(2,103)
External Benchmark (annual $)†	47,643	47,008	49,579	46,491	52,389
	(652)	(752)	(1,307)	(1,650)	(1,977)
Abs. %-Diff. Salary vs. Benchmark†	22.16	22.46	21.26	19.41	22.95
	(0.38)	(0.45)	(0.68)	(0.84)	(1.04)
Age	34.40	34.30	34.72	34.36	35.04
	(0.18)	(0.22)	(0.32)	(0.42)	(0.48)
Share Female (%)	45.29	46.39	41.92	44.74	39.36
	(1.29)	(1.48)	(2.57)	(3.78)	(3.51)
Share High Education (%)	56.92	55.30	61.84	57.89	65.43
	(1.28)	(1.49)	(2.53)	(3.74)	(3.42)
Share Hourly (%)	71.89	73.08	68.25	71.35	65.43
	(1.17)	(1.33)	(2.44)	(3.47)	(3.44)
Number of Firms	2,005	1,419	586	183	403

Notes: Average characteristics in the main sample of new hires, with robust standard errors in parentheses. Variables marked with † are computed using only pre-onboarding data. *Higher Usage* are firms that search at least once and *Lower Usage* are firms with access that never search. *Turnover Rate* is defined as number of employee departures in a month over the number of employees employed at the firm during that month. *Business Services Sector* through *Other Sector* correspond to the distribution of industry sectors. *Salary* is the annual base salary at the time of hire. *External Benchmark* is the median annual base salary benchmark in the position of the new hire during the quarter of the hire date.

Table 4: Most Common Searched Position Titles

Position Title	(1) Searched	(2) Non-Searched	(3) Non-Searchable
Bank Teller	539 [12]	287 [24]	1,976 [87]
Customer Service Representative	468 [44]	4,401 [170]	4,012 [385]
Security Guard	286 [6]	139 [44]	6,263 [95]
Hotel Cleaner	208 [2]	379 [5]	1,058 [17]
Legal Associate Specialist	163 [1]	7 [4]	14 [9]
Hand Packer	155 [4]	234 [17]	1,957 [55]
Patient Care Coordinator	117 [3]	103 [14]	133 [29]
Receptionist	93 [15]	310 [86]	2,911 [238]
Cook	86 [6]	334 [21]	1,606 [85]
Waiter/Waitress	84 [7]	1,113 [18]	2,986 [87]
Delivery Driver	79 [5]	34 [9]	744 [26]
Dish Washer/Plate Collector/Table Top Cleaner	69 [5]	187 [18]	1,350 [67]
Medical Assistant	69 [10]	370 [17]	889 [55]
Welder	66 [8]	112 [27]	652 [59]
Cashier	65 [2]	175 [11]	2,706 [48]
Registered Nurse	64 [11]	244 [22]	2,699 [110]
Assembler	60 [9]	606 [26]	3,823 [90]
Other Housekeeper and Related Worker	59 [5]	173 [17]	948 [63]
Software Developer/Programmer	59 [23]	403 [78]	1,285 [173]
Warehouse Laborer	59 [10]	761 [43]	3,025 [116]
Mammographer	55 [1]	9 [1]	3 [2]
Nursing Assistant	51 [4]	662 [13]	7,346 [65]
Bartender/Mixologist	49 [2]	228 [12]	611 [46]
Production Operations Engineer	49 [1]	41 [16]	68 [29]
Licensed Practical Nurse	48 [9]	189 [23]	1,605 [69]
Sales Manager	48 [18]	166 [67]	693 [181]
General Practitioner/Physician	46 [2]	143 [17]	340 [28]
Lawyer	43 [5]	17 [10]	268 [52]
Ophthalmic Technician	42 [2]	4 [1]	34 [4]
Business Development Specialist	41 [2]	124 [27]	447 [41]
Warehouse Manager	40 [7]	133 [23]	430 [72]
Other Social Work and Counseling Professional	39 [1]	1 [1]	32 [9]
Building Caretaker/Watchman	38 [2]	288 [59]	917 [139]
Operations Officer	37 [2]	73 [18]	108 [36]
Shipping Clerk	37 [4]	39 [19]	218 [63]

Notes: New hires in each position [firms hiring in each position]. Tabulations across all new hires for the 35 searched *Position Titles* with the most new hires.

Table 5: Summary Statistics by Position Type

		by Position Type		
	(1) All	(2) Searched	(3) Non-Searched	(4) Non-Searchable
Salary (annual \$)†	41,359	39,064	42,013	41,405
	(146)	(462)	(390)	(166)
External Benchmark (annual \$)†	41,412	38,649	41,092	41,672
	(113)	(409)	(295)	(128)
Abs. %-Diff. Salary vs. Benchmark†	20.36	17.36	21.03	20.45
	(0.08)	(0.28)	(0.21)	(0.09)
Age	34.77	34.53	34.54	34.83
	(0.05)	(0.22)	(0.13)	(0.06)
Share Female (%)	50.63	60.14	51.01	49.87
	(0.20)	(0.83)	(0.53)	(0.23)
Share High Education (%)	42.21	34.49	42.28	42.76
	(0.20)	(0.80)	(0.52)	(0.23)
Share Hourly (%)	81.11	82.94	80.13	81.16
	(0.16)	(0.64)	(0.42)	(0.18)
Occupation Groups				
Office and Administrative Support (%)	19.84	32.44	28.97	17.23
	(0.16)	(0.79)	(0.48)	(0.17)
Building and Grounds Cleaning (%)	4.77	5.22	2.58	5.14
	(0.09)	(0.38)	(0.17)	(0.10)
Management (%)	8.04	8.10	9.21	7.81
	(0.11)	(0.46)	(0.31)	(0.12)
Production (%)	6.59	6.48	6.35	6.64
	(0.10)	(0.42)	(0.26)	(0.11)
Transportation and Material Moving (%)	9.30	6.62	9.72	9.42
	(0.12)	(0.42)	(0.31)	(0.13)
Other (%)	51.47	41.14	43.16	53.75
	(0.20)	(0.83)	(0.52)	(0.23)
Number of Firms	2,005	285	578	1,419
Number of Positions	1,406	329	973	1,306
Observations	201,817	5,266	39,686	156,865

Notes: Average characteristics in the main sample of new hires, with robust standard errors in parentheses. Variables marked with † are computed using only pre-onboarding data. *Salary* is the annual base salary at the time of hire. *External Benchmark* is the median annual base salary benchmark in the position of the new hire during the quarter of the hire date. Variables under *Occupation Groups* correspond to a new hire's SOC group.

Table 6: The Effects of Benchmarking on Absolute %-Distance from the Benchmark

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$\|\%\Delta\|$	$\|log\Delta\|$	$\|\%\Delta\| > 10$	$\|\%\Delta\|$	$\|\%\Delta\|$	$\|\%\Delta\|$	$\|\%\Delta\|$	$\|\%\Delta\|$	$\|\%\Delta\|$	$\|\%\Delta\|$	$\|\%\Delta\|$	$\|\%\Delta\|$
Panel (a): Post-treatment												
Searched vs. Non-Searched	-4.775***	-5.155***	-16.270***	-5.148***	-4.775***	-4.786***	-5.324***	-4.950***	-4.421***	-4.887***	-4.880***	-4.564***
	(1.143)	(1.266)	(3.626)	(1.338)	(0.906)	(1.198)	(1.282)	(1.286)	(1.153)	(1.165)	(1.276)	(1.178)
Searched vs. Non-Searchable	-6.149***	-7.118***	-13.861***	-6.836***	-6.149***	-6.128***	-7.494***	-7.450***	-5.714***	-6.163***	-5.044***	-5.934***
	(1.070)	(1.211)	(3.681)	(1.220)	(0.824)	(1.076)	(1.233)	(1.576)	(1.078)	(1.087)	(1.231)	(1.127)
Panel (b): Pre-treatment												
Searched vs. Non-Searched	-0.346	-0.129	-5.872	-0.233	-0.346	-0.488	-1.646	-2.062*	-0.714	-0.144	-2.205	-0.199
	(1.167)	(1.313)	(3.690)	(1.289)	(0.751)	(1.185)	(1.514)	(1.200)	(1.133)	(1.199)	(1.528)	(1.174)
Searched vs. Non-Searchable	-0.310	0.156	-4.221	-0.513	-0.310	-0.318	0.021	-1.029	0.241	-0.247	-0.754	-0.500
	(1.055)	(1.175)	(3.246)	(1.184)	(0.643)	(1.057)	(1.375)	(1.116)	(1.046)	(1.069)	(1.342)	(1.105)
Winsorizing at +/- 100%				✓								
No Clustering					✓							
No Additional Controls						✓						
No Position FE							✓					
Firm FE								✓				
Exclude High-Tip Jobs									✓			
Searched Positions Only										✓		
No Re-weighting											✓	
Ages 21-60												✓
Mean Dep. Var. (Baseline)	19.812	20.590	63.732	21.004	19.812	19.812	19.812	19.812	19.430	19.812	19.802	19.903
Observations												
Searched	5,253	5,253	5,253	5,253	5,253	5,253	5,266	5,262	5,105	5,253	5,331	4,611
Non-Searched	39,527	39,527	39,527	39,527	39,527	39,527	39,686	39,673	37,841	34,954	39,810	34,338
Non-Searchable	156,734	156,734	156,734	156,734	156,734	156,734	156,865	156,817	148,521	127,145	157,018	135,051

Notes: Significant at *10%, **5%, ***1%. Standard errors clustered at the firm-position-month level in parentheses. Each column corresponds to two regressions: one for searched vs. non-searched new hires and one for searched vs. non-searchable new hires. Post-treatment coefficients in panel (a) refer to parameters α_1^k from equation (14), while pre-treatment coefficients in panel (b) refer to parameters α_3^k from equation (15) (see Section 4.2 for details). All columns include year fixed effects. In columns (1) and (4)–(12) the dependent variable is the absolute percent difference between the annual base salary and median benchmark (Δ). The dependent variable in col (2) is the log of Δ and in col (3) is a dummy that equals 100 if $|\%\Delta|$ is greater than 10% and zero otherwise. We multiply $\%\Delta$ and $log(\Delta)$ by 100 so that the effects can be interpreted as percentage points. Δ is winsorized to $\pm$ 75 except in column (4) where it is winsorized to $\pm$ 100. All columns except (6) include additional controls (female dummy, high education dummy, hourly dummy, age, position tenure). Column (9) excludes the three positions where gross pay most exceeds base pay: Waiter/Waitress, Chauffeur, and Bartender/Mixologist. Column (10) restricts the sample to only positions of non-searched or non-searchable new hires in positions that are searched and hired by firms in the data.