

# What's My Employee Worth?

## The Effects of Salary Benchmarking

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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 utilize 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.

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*Keywords:* compensation, salary benchmarking, pay inequality, information frictions.

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# 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 utilize 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](#)). For instance, in a recent 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](#)). 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 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 three 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. Third, firms can behave strategically, adjusting their offers when other firms gain access to the same benchmarks.

In our model, each firm  $j$  observes the 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.

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 583 “treatment” firms that gained access to the tool and 1,431 “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. However, rather than taking these sources of exogenous variation as given, our research design provides sharp tests by way of an event-study framework and differences-in-differences approaches.

We assign each new hire into one of three categories. *Searched positions* correspond to the 4,686 unique hires in positions that are (eventually) searched in treatment firms. *Non-Searched positions* correspond to the 36,049 hires in positions that are not searched by treatment firms. *Non-Searchable positions* correspond to the 162,450 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.<sup>1</sup> Among Searched positions, the dispersion to the benchmark was on average 19.3 pp before the firms gained access to the tool. After gaining access to the tool, the dispersion dropped from 19.3 pp to 14.8 pp. This drop is not only highly statistically significant ( $p\text{-value}<0.001$ ), but also large in magnitude, corresponding to a 23% 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. 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 to-

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<sup>1</sup>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).

wards 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 36% of the sample is classified as low-skill, and the remaining 64% 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 14.8 pp to 8.7 pp (p-value<0.001), equivalent to a 41.2% decline. By comparison, for high-skill positions the change in dispersion is smaller, dropping from 21.9 pp to 18.9 pp (p-value=0.028), a 13.7% decline. This finding is in sharp contrast to the expert forecasts, who 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 average 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 4.9% (p-value=0.008) and 5.7% (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 12.4 pp (p-value=0.003) and 9.2% (p-value=0.008), 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 causal 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.<sup>2</sup>

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

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<sup>2</sup>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).

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 several key features of our setting. First, modeling the process as an auction implies that the individual worker's labor supply curve is not infinitely elastic—raising one firm's offer smoothly increases the probability of hiring that worker. Second, 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.<sup>3</sup> Third, the affiliated values assumption implies that wage benchmarks matter, in the sense that they convey information about the distribution of competing offers that a firm faces.<sup>4</sup>

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  $[\underline{v}, \bar{v}]$  with  $0 \leq \underline{v} < \bar{v} < \infty$ , and that  $S$  has support on some arbitrary interval  $\mathcal{S}$ . Let  $f(s, v_1, \dots, v_n)$  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  $(S, V_1, \dots, V_n)$  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

$$f(z \vee z')f(z \wedge z') \geq f(z)f(z'). \quad (1)$$

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

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

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<sup>3</sup>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.

<sup>4</sup>If firms' values are drawn independently across workers according to a known distribution, then past salaries convey no information about current offers.

where  $f : \mathbb{R} \rightarrow \mathbb{R}_{\geq 0}$  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  $f_{Y_1}(x | v, s)$  denote the density of  $Y_1$  conditional on  $V_1 = v$  and  $S = s$ , with cumulative distribution  $F_{Y_1}(x | v, s)$ .

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

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.<sup>6</sup> 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  $b^* : [\underline{v}, \bar{v}] \rightarrow \mathbb{R}$  is strictly increasing and satisfies the first-order linear differential equation defined by*

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

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

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

$$\frac{f_{Y_1}(v | v, s)}{F_{Y_1}(v | v, s)} < \frac{E[f_{Y_1}(v | v, S) | V_1 = v]}{E[F_{Y_1}(v | v, S) | V_1 = v]}. \quad (5)$$

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  $f_{Y_1}(v | v, s)$ . The benefit is that firm 1 pays less if it wins, which is proportional to  $F_{Y_1}(v | v, s)$ . 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 to  $b^*$ . Consider the informed firm's best-response correspondence, which depends on its value

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<sup>5</sup>Lemma 1 of Milgrom and Weber (1982b).

<sup>6</sup>Ties are zero-probability events, so the analysis does not depend on the tie-breaking condition.

$V_1$  and the benchmark  $S$ ,

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

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  $\tilde{v} < \bar{v}$  such that for all  $v > \tilde{v}$ , we have that*

$$E [\tilde{b}(v, S) \mid V_1 = v] < b^*(v), \quad (7)$$

and also that

$$E [\tilde{b}(v, S) \mathbb{1}_{\tilde{b}(v, S) \geq b^*(Y_1)} \mid V_1 = v] < b^*(v). \quad (8)$$

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

$$\lim_{v \downarrow \underline{v}} (E [\tilde{b}(v, S) \mid V_1 = v] - b^*(v)) \geq 0. \quad (9)$$

Moreover, for various natural distributions  $f$  the direct effect is single-crossing and strictly positive on an interior interval.

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  $[v, \bar{v}]^N \times \mathcal{S}$ . There exists  $\tilde{v} < \bar{v}$  such that for all  $v > \tilde{v}$ , we have that*

$$P (\tilde{b}(v, S) \geq b^*(Y_1) \mid V_1 = v) < P (b^*(v) \geq b^*(Y_1) \mid V_1 = v). \quad (10)$$

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  $b^{**} : [\underline{v}, \bar{v}] \times \mathcal{S} \rightarrow \mathbb{R}$  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

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

$$b^{**'}(v, s) = (v - b^{**}(v)) \frac{f_{Y_1}(v | v, s)}{F_{Y_1}(v | v, s)}. \quad (12)$$

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

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

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  $(S, V_1, \dots, V_n)$  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.

Last, 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 an stylized model of retention concerns.

## 2.2 A model of benchmark manipulation

So far, we have studied equilibrium effects assuming that each firm treats the benchmark as exogenous (Theorem 2.6). This is a reasonable approximation when each firm is small relative to the market, so that its own bidding behavior has little effect the benchmark.

What if some firms are large enough that their bidding behavior substantially affects the benchmark? To speak to this question, we now study an alternative model that allows one firm to manipulate the benchmark.

There are  $n$  workers. One large firm has value  $V_0$  for each worker. Each worker  $i$  also has a disjoint set of  $m$  small firms seeking to hire them, each with value  $V_j^i$  for that worker. For each worker  $i$ , there is a first-price auction involving the large firm and the corresponding small firms. Let  $b_0^i$  denote the bid that the large firm makes for worker  $i$ , and let  $b_j^i$  denote small firm  $j$ 's bid.  $W_j^i$  is an indicator function equal to 1 if firm  $j$  wins the auction for worker  $i$ . The large firm's payoff is  $\sum_i (V_0 - b_0^i) W_0^i$ . The small firm's payoff is  $(V_j^i - b_j^i) W_j^i$ .

We assume that the values of the large firm and the small firms are independently and identically distributed with continuous density  $f$ , with support on non-negative interval  $[\underline{v}, \bar{v}]$ . The independence assumption implies that the large firm faces no aggregate uncertainty about the small firms' bidding behavior. Thus, benchmarks serve solely to reveal the large firm's offers to the small firms, isolating the effects of benchmark manipulation.

Suppose first that there is no benchmark. In the unique symmetric Bayes-Nash equilib-

rium, all firms, including the large firm, bid according to some continuous increasing function  $b^* : [\underline{v}, \bar{v}] \rightarrow \mathbb{R}$  that satisfies  $b^*(v) \leq v$  for all  $v$ . In particular, the large firm makes the same bid for all workers.

Now we add a salary benchmark—to highlight the effects of benchmark manipulation, suppose that the salary benchmark fully reveals the large firm's bid. That is, the large firm chooses a bid  $b_0$  that will be made for all workers, and then the small firms observe  $b_0$  and simultaneously choose their bids. In the equilibrium that we derive, aggregate statistics such as the median accepted offer or the mean accepted offer are enough to consistently estimate the large firm's bid (as the number of workers  $n$  grows). Assuming that  $b_0$  is directly observed is a stylized way to study large  $n$ .

Suppose that the small firms play a Bayes-Nash equilibrium of the continuation game following a bid of  $b_0$ . To ensure that the small firm's best-response correspondence is non-empty when  $m = 1$ , we break ties in favor of the small firms.<sup>7</sup> Then the large firm will lose if  $b_0 < \max_{j \neq 0} V_j^i$  and will win if  $b_0 > \max_{j \neq 0} V_j^i$ . Thus, the large firm's equilibrium bid  $\tilde{b}(v)$  maximizes  $(v - b_0)F(b_0)^m$ .

The large firm must account for how its bids affect the benchmark, and thus the small firms' bids. On the one hand, when the large firm raises its bid, the small firms raise their bids in response, and this ‘pass-through’ effect is a reason for the large firm to reduce its bid. On the other hand, the benchmark makes it less safe for the large firm to shade its bid. Without the benchmark, the large firm bids  $b^*(V_0) \leq V_0$ , and by symmetry will win the auction for worker  $i$  if and only if  $\max_{j \neq 0} V_j^i < V_0$ . But if we add the benchmark and the large firm still bids  $b^*(V_0)$ , then the large firm will win if and only if  $\max_{j \neq 0} V_j^i < b^*(V_0)$ , which is less often than before.

The overall effect of the benchmark depends on the distribution  $F$ . Some distributions increase the large firm's bids; that is,  $\tilde{b}(v) \geq b^*(v)$ , with strict inequality for  $v > \underline{v}$ . Some distributions decrease the large firm's bids; that is,  $\tilde{b}(v) \leq b^*(v)$ , with strict inequality for  $v > \underline{v}$ .<sup>8</sup>

In a Stackelberg oligopoly, moving first can be advantageous compared to moving simultaneously. Hence one might guess that the benchmark, by providing commitment power, is potentially advantageous to the large firm. However, the model predicts that adding the benchmark always reduces the large firm's profits, as we now state.

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<sup>7</sup>This continuation game is equivalent to a first-price auction with reserve equal to  $b_0$ . One equilibrium of the continuation game is for small firm  $j$  with value  $v$  to bid  $E[\max\{b_0, \max_{j' \notin \{j, 0\}} V_{j'}^i\} \mid \max_{j' \notin \{j, 0\}} V_{j'}^i < v]$  if  $v > b_0$  and to bid  $v$  otherwise, as in Myerson (1981).

<sup>8</sup>For instance, consider the class of linear densities,  $f(v) = (1 - \beta/2) + \beta v$  defined on  $[0, 1]$ . Solving numerically, one finds  $\tilde{b}(v) \geq b^*(v)$  for  $\beta = -1$ ,  $\tilde{b}(v) = b^*(v)$  for  $\beta = 0$ , and  $\tilde{b}(v) \leq b^*(v)$  for  $\beta = 1$ . The inequalities are strict for  $v > \underline{v}$ .

**Proposition 2.7.** *Adding the benchmark weakly reduces the expected profit of every type of the large firm.*

*Proof.* Suppose the large firm's value is  $V_0 = v$ . The expected profit of the large firm without the benchmark is

$$E \left[ \sum_i (v - b^*(v)) \mathbb{1}_{b^*(v) > \max_{j \neq 0} b^*(V_j^i)} \right] \geq E \left[ \sum_i (v - \check{b}(v)) \mathbb{1}_{\check{b}(v) > \max_{j \neq 0} b^*(V_j^i)} \right] \quad (14)$$

$$\geq E \left[ \sum_i (v - \check{b}(v)) \mathbb{1}_{\check{b}(v) > \max_{j \neq 0} V_j^i} \right] \quad (15)$$

where the first inequality follows since  $b^*$  is an equilibrium strategy and the second inequality follows by  $v - \check{b}(v) \geq 0$  and  $b^*(V_j^i) \leq V_j^i$ . The right-hand side of (15) is the large firm's expected profit with the benchmark.  $\square$

## 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).<sup>9</sup> In a recent survey of 5,003 firms from the United States, 96.3% of them reported using market data to inform their compensation strategy and structure (PayScale, 2021). Moreover, interviews of Human Resources and other 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 seems to be quite representative of 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*

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<sup>9</sup>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.

*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 quite 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. This is probably 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.

The PayScale (2021) survey indicates that the majority (86.4%) of firms use not only one, but multiple sources for market data. This survey data also provides hints as to how popular the different sources of data are. The most traditional source of benchmark data are surveys from providers that aggregate actual salaries from employers. This source is utilized by 37.5% of respondents. The most popular source, used by 59.7% of respondents, is free online data (e.g., Glassdoor). Paid online data sources are the second most popular option, with 39.1% of surveyed organizations using them. Other popular options are industry surveys and government data (29.2% and 27.4% respectively).

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). Indeed, 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 DO: INTRODUCE SURVEY DATA

## 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.<sup>10</sup> 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.

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<sup>10</sup>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.

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 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.<sup>11</sup> 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.<sup>12</sup> 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 lenses that distinguish between preschool, primary, secondary, middle school, substitute, and special education teachers.<sup>13</sup>

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).<sup>14</sup>

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

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

<sup>12</sup>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).

<sup>13</sup>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.

<sup>14</sup>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.

equivalent experience. For some jobs this may also require a graduate degree or additional 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.<sup>15</sup> The vast majority of the total cash compensation comes from base salary.<sup>16</sup>

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 try to zoom in by industry and state, and that leaves 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

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<sup>15</sup>81.9% of our sample is hourly, and the rest are salaried.

<sup>16</sup>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.

indicates the specific date to which the statistics refer, and it even shows some information 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.<sup>17</sup>

**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.<sup>18</sup> 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.<sup>19</sup> 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

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<sup>17</sup>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.

<sup>18</sup>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.

<sup>19</sup>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.

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%.<sup>20</sup> 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.<sup>21</sup> 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.<sup>22</sup>

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 three steps. First, we map ONET codes to identify positions in job zones 1 and 2 (typically requiring no more than a high school diploma).<sup>23</sup> Second, we exclude positions in which the average worker is above 35 years of age. Third, we exclude positions with average annual salaries above \$34,000, which is the median salary in our sample and roughly twice the minimum wage level. Roughly 36% of the sample is classified as low-skill, and the remaining 64% 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 – indeed, this view is supported by the analysis of utilization data.<sup>24</sup> Focusing on new hires

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<sup>20</sup>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.

<sup>21</sup>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.

<sup>22</sup>For more details about the data, see Appendix D.

<sup>23</sup>Education status is imputed for 24.5% of observations using the Job Zones for related ADP lenses. The results are consistent without this imputation.

<sup>24</sup>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.

has other important advantages. Most importantly, firms often set a salary at the time of hiring a new employee – in contrast, firms set the salaries of their existing employees infrequently and, even when doing so, they may be subject to constraints such as downward wage rigidities. For these reasons, our main analysis focuses on new hires.<sup>25</sup>

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.<sup>26</sup> 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 317 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.<sup>27</sup> 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

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<sup>25</sup>We plan on analyzing the existing employees in a future version of this study.

<sup>26</sup>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.

<sup>27</sup>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.

analytic services as part of a broader conversation.

Our main sample comprises 583 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.<sup>28</sup> 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.<sup>29</sup>

We obtained data on an additional 1,431 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.<sup>30</sup> For example, if Ford gains access but Fiat does not, we assume Ford would have gained access when Fiat did.<sup>31</sup>

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

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<sup>28</sup>More precisely, among the 583 firms with access to the tool, 558 (96%) conducted at least one search during the period for which we have usage data.

<sup>29</sup>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.

<sup>30</sup>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.

<sup>31</sup>We use Ford and Fiat purely for illustration purposes, as we work with de-anonymized data and thus do not know the names of any of the companies in our sample.

representative of industries with larger firms. While not perfectly representative of the U.S. 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 512 employees, 46.0% of which are female, and the average employee is 34 years old and earns a salary of \$46,501. 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,<sup>32</sup> 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

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<sup>32</sup>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.

as non-searched. To minimize the scope for information spillovers, we exclude from the non-searched positions all the positions “adjacent” (i.e., in the same SOC group) to those that *were* searched.

- 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 532 firms who had onboarded prior to the last quarter of 2019. During that quarter, 196 (36.9%) of these firms hired in at least one position. These firms searched the benchmark for 21.5% of the positions in which they hired.<sup>33</sup> 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 4,686 new hires in the Searched category, 36,049 new hires in the Non-Searched category and 162,450 new hires in the Non-Searchable category.

Table 4 lists the 35 most common positions in the Searched category, out of the total of 317 unique Searched positions in the sample. The 3,129 hires in these 35 positions account for a majority (66.8%) 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 428 hires for Customer Service Representative in the Searched category are distributed across 41 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 429 new hires Customer Service Representative in the searched category, there are 3,806 hires for that same position in the Non-Searched category and 4,907 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, 50.9% of them are female, 81.4% work for

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<sup>33</sup>More precisely, around 62.8% of these firms did not search for any of the positions in which they hired; among the remaining firms, they looked up on average 57.7% of the positions in which they hired.

an hourly wage, they have an annual base salary of \$41,689, an external median benchmark that is slightly higher (\$42,161) and the starting salaries differ from the median benchmark (in absolute value) by an average of 19.7%. The last rows shows the main occupation groups in the sample. 16.4% of the positions are in Office and Administrative Support, 7.2% in Management, 9.0% in Production, 8.9% in Transportation and Material Moving, 7.4% in Building and Grounds Cleaning, and the rest (51.2%) 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 \$44,718, \$44,309 and \$41,135 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 highly statistically significant ( $p\text{-values}<0.001$ ), despite modest differences in economic terms. The difference between the Searched and Non-Searched group is not significant ( $p\text{-value} = 0.583$ ). 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 ( $\pm 2.5\%$ ) 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 ( $\pm 2.5\%$ ) to the median benchmark increases from 11.4% before onboarding to 21.9% 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 19.4 pp to 15.2 pp, which is highly statistically significant (p-value<0.001) and also large in magnitude (equivalent to a 21.6% 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 (20.9 pp) to the post-onboarding period (22.3 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 (19.0 pp) as in the post-onboarding period (19.2 pp). This difference is statistically significant ( $p\text{-value}=0.061$ ) 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 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.<sup>34</sup>

## 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  $\Theta_1$  to refer to observations categorized as either Searched or Non-Searched, and  $\Theta_2$  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  $t$  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

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<sup>34</sup>There is also some evidence that employees, not just employers, may pay particular attention to median salaries (Roussille, 2021).

correspond to the “hypothetical” onboarding date). Finally, let  $\delta_t$  denote year dummies,  $\psi^k$  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  $\epsilon_{i,j,t}^k$  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:

$$Y_{i,j,t} = \alpha_1^k \cdot A_{j,t} \cdot T_{i,j} + \alpha_2^k \cdot A_{j,t} + \alpha_3^k \cdot T_{i,j} + X_{i,j,t} \alpha_4^k + \delta_t^k + \psi^k + \epsilon_{i,j,t}^k, \quad \forall \{i, j, t\} \in \Theta_k \quad (16)$$

When  $k = 1$ , equation (16) boils down to the first identification strategy (Searched vs. Non-Searched). When  $k = 2$ , equation (16) boils down to the second identification strategy (Searched vs. Non-Searched). The differences-in-differences coefficient of interest is  $\alpha_1^k$ , which measures the effect of the benchmark tool. When  $k = 1$ ,  $\alpha_1^1$ , measures the difference in outcomes between Searched (treatment) and Non-Searched (control) groups changed post-onboarding relative to pre-onboarding. When  $k = 2$ ,  $\alpha_1^2$ , 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 (16) 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 ( $A_{j,t}^{\text{fake}}$ ) which is identical to the true post-treatment dummy ( $A_{j,t}^{\text{true}}$ ) except that it takes value 1 in the two quarters before the onboarding date. We can expand equation (16) as follows:

$$Y_{i,j,t} = \alpha_1^k \cdot A_{j,t} \cdot T_{i,j} + \alpha_2^k \cdot A_{j,t} + \alpha_3^k \cdot A_{j,t}^{\text{fake}} \cdot T_{i,j} + \alpha_2^k \cdot A_{j,t}^{\text{fake}} + \alpha_4^k \cdot T_{i,j} + X_{i,j,t} \alpha_5^k + \delta_t^k + \psi^k + \epsilon_{i,j,t}^k, \quad \forall \{i, j, t\} \in \Theta_k \quad (17)$$

The coefficient of interest is  $\alpha_3^k$ , 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).<sup>35</sup> We expand equation (16) as follows:

$$Y_{i,j,t} = \sum_{s \in S} \alpha_{1,s}^k \cdot A_{j,t}^s \cdot T_{i,j} + \sum_{s \in S} \alpha_{2,s}^k \cdot A_{j,t}^s + \alpha_3^k \cdot T_{i,j} + X_{i,j,t} \alpha_4^k + \delta_t^k + \psi^k + \epsilon_{i,j,t}^k, \quad \forall \{i, j, t\} \in \Theta_k \quad (18)$$

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: 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

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<sup>35</sup>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.

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 19.4 pp prior to the onboarding, but then dropped sharply to around 15.2 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 20.9 pp prior to onboarding, and remained stable at a similar level (22.3 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 19.3 pp to 14.5 pp ( $p\text{-value}<.001$ ), equivalent to a 24.8% 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 19.3 pp to 14.2 pp ( $p\text{-value}<0.001$ ). The drop in dispersion from panel D (5.1 pp) is close in magnitude to the corresponding drop from panel C (4.8 pp) – indeed, 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.,  $\alpha_1^k$  from equation (16)). Column (1) of Table 6 corresponds to the baseline specification. The post-treatment coefficients are negative and statistically significant: -4.241 ( $p\text{-value}<0.001$ ) when using non-searched positions as control group, and -4.770 ( $p\text{-value}<0.001$ ) 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  $\alpha_3^k$  from equation (16). 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 (-0.135 and -0.602, respectively), statistically

insignificant (p-values of 0.906 and 0.558) 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 21.8% ( $= \frac{4.241}{19.442}$ ), while the corresponding coefficient from column (3) suggests a decline of 24.7% ( $= \frac{15.765}{63.686}$ ).

The specification from column (4) is identical to the baseline specification from column (1), except that the dependent variable is winsorized at  $\pm 100\%$  instead of  $\pm 75\%$ . 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 317 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 14.8 pp to 8.7 pp ( $p\text{-value}<0.001$ ). On the contrary, there is a more modest compression for high-skill positions (Panel B): dispersion goes from 21.9 pp to 18.9 pp ( $p\text{-value}=0.028$ ).

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 0.007 and 0.021 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 productivity 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.<sup>36</sup>

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 (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

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<sup>36</sup>According to the benchmark data, among low-skill positions, 95.1% of the total cash compensation comes in the form of base salary. For high-skill positions, the corresponding figure is 91.7%. 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).

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. Indeed, 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.<sup>37</sup>

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. 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 increased the average salary by 0.018 log points, which is statistically insignificant

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<sup>37</sup>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.

(p-value=0.759) and also economically modest: equivalent to an effect of just 1.8%.<sup>38</sup>

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 0.036 log points (p-value=0.148), equivalent to a salary raise of 3.6%. 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 4.9% (p-value=0.008) and 5.7% (p-value=0.001), respectively.

Appendix I provides additional details about the heterogeneity analysis. First, 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 0.005 and 0.051 for comparisons of Searched vs. Non-Searched and Searched vs. Non-Searchable, respectively. In the main specification, the definition of skill combines information on the position averages by education, age, and salary. We show that, while statistically less significant, the results are roughly consistent if we look at the heterogeneity by each of these

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<sup>38</sup>To be more precise, the effect is 1.816% ( $= 100 \cdot (\exp(0.010) - 1)$ ). Since the approximation error is so small, in the remainder of the paper we treat log-point effects and percent-effects as interchangeable.

position characteristics separately. We also show that, in contrast, there is no robust evidence of heterogeneity by other position characteristics.

## 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 12.4 pp (p-value=0.003) and 9.2% (p-value=0.008), 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.<sup>39</sup> Indeed, 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.<sup>40</sup> 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

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<sup>39</sup>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 under-powered.

<sup>40</sup>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).

predictions of the model. For instance, we find that access to the tool compresses salaries towards the market benchmark quite significantly, and specially 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, additionally, the causal 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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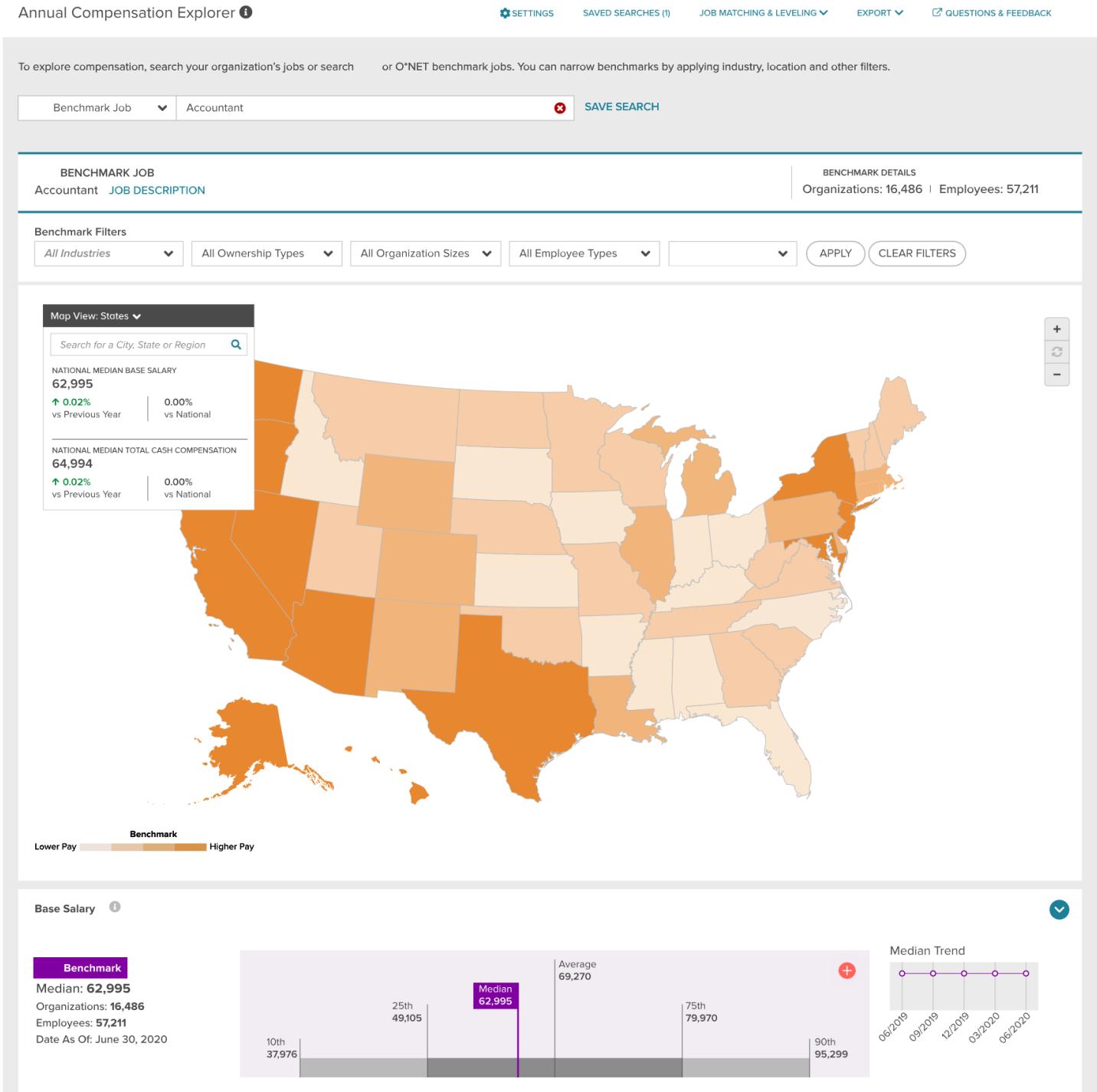
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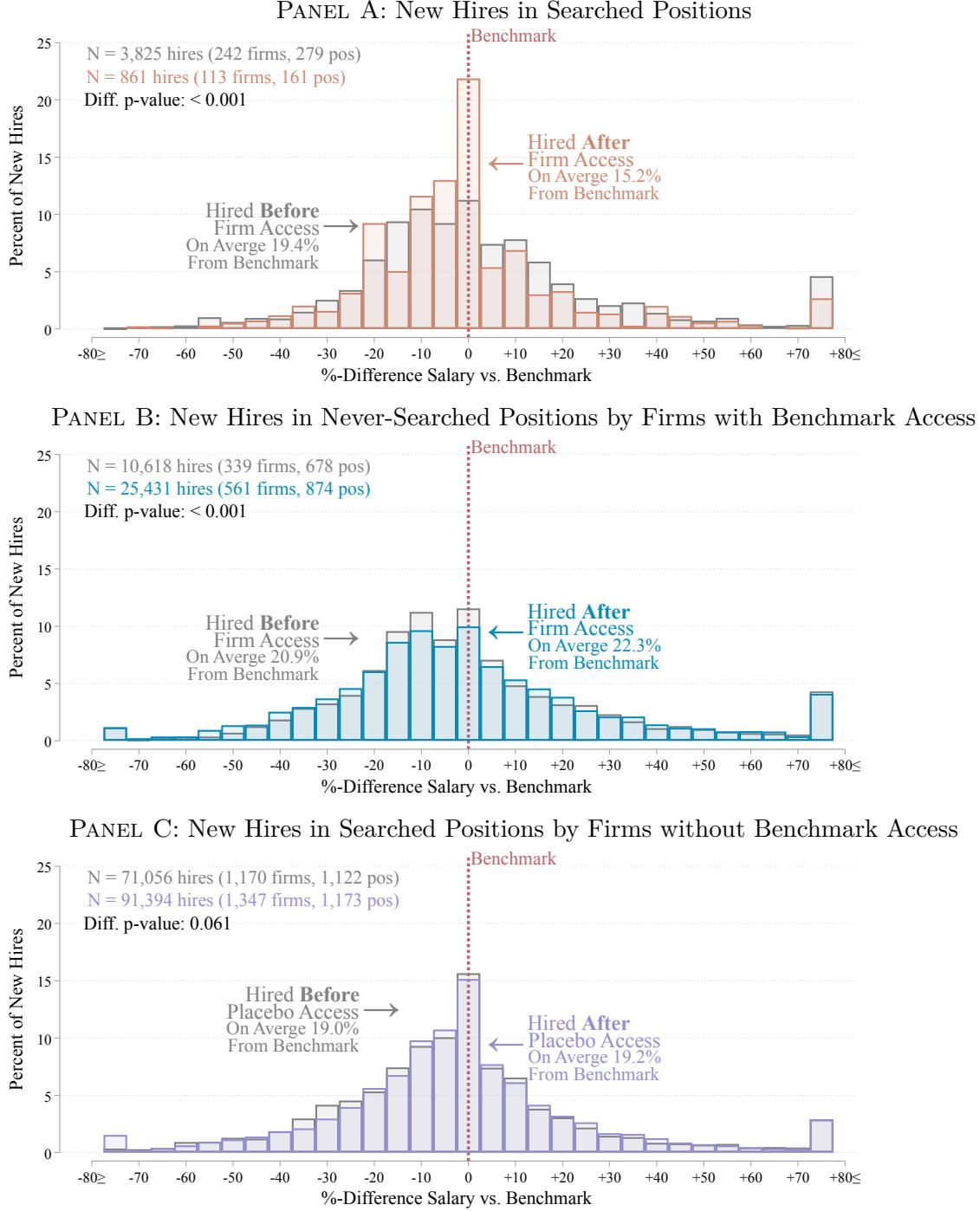
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Figure 1: Screenshot of the Salary Benchmarking Tool



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*.

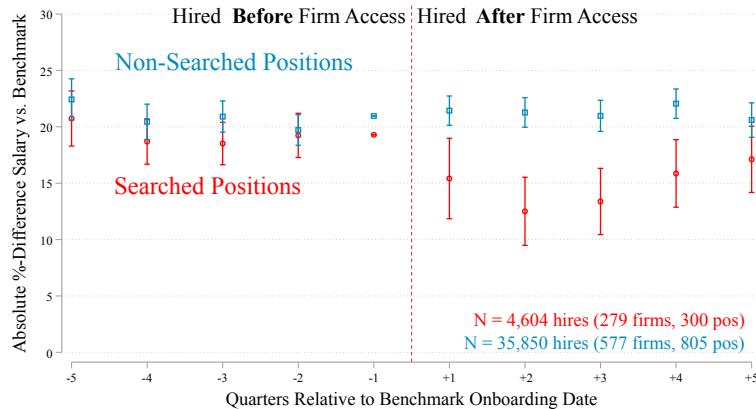
Figure 2: The Effects of the Compensation Benchmark: Non-Parametric Analysis



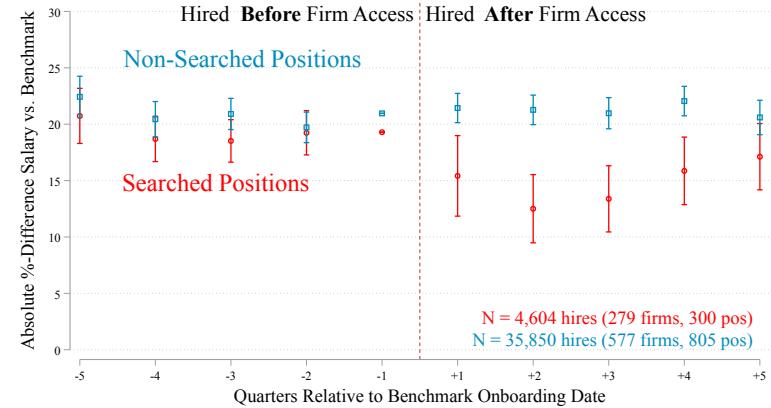
Notes: Histograms of the starting base salary relative to the corresponding external benchmark (winsorized at  $\pm 75\%$ ). Each panel corresponds to a different set of positions: panel A for *searched* positions (i.e., positions in firms with access to the benchmark tool that are eventually searched 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

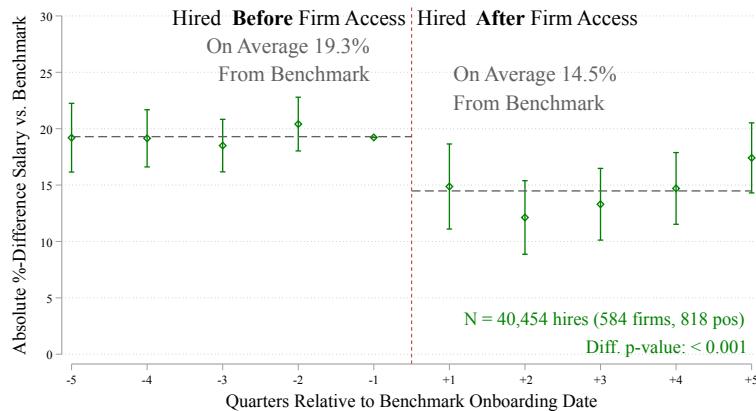
PANEL A: Searched vs. Non-Searched



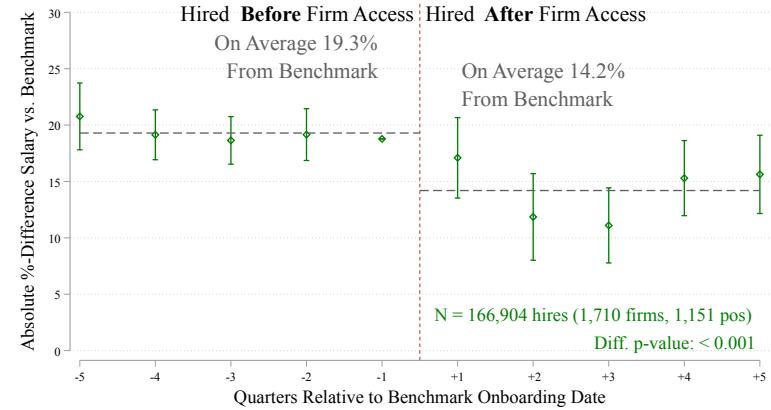
PANEL B: Searched vs. Non-Searchable



PANEL C: Difference Searched *minus* Non-Searched



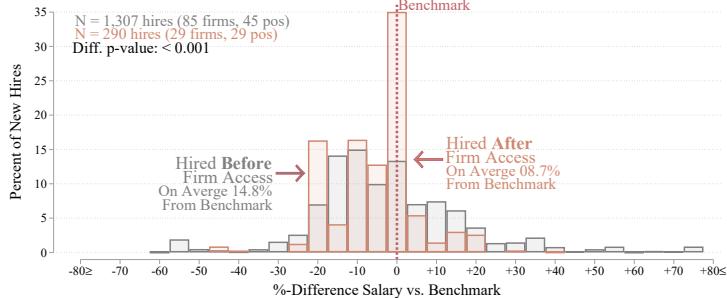
PANEL D: Difference Searched *minus* Non-Searchable



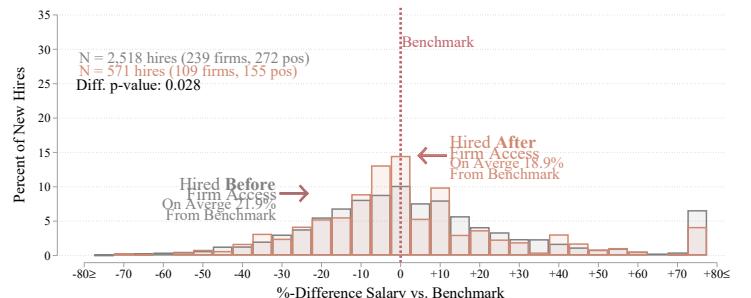
Notes: Point estimates with 90% confidence intervals in brackets, using standard errors clustered at the firm-position-month level. Panels A and C are based off one regression for searched and non-searched positions, while panel A presents the estimates for each position type and panel C presents the 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  $\alpha_{1,s}^k \forall s \in S$  from equation (18) (see Section 4.2 for details).

Figure 4: Heterogeneity: Non-Parametric Analysis

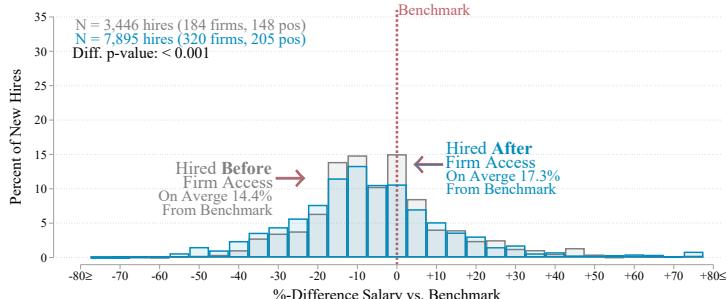
PANEL A: Low Skill: Searched Positions



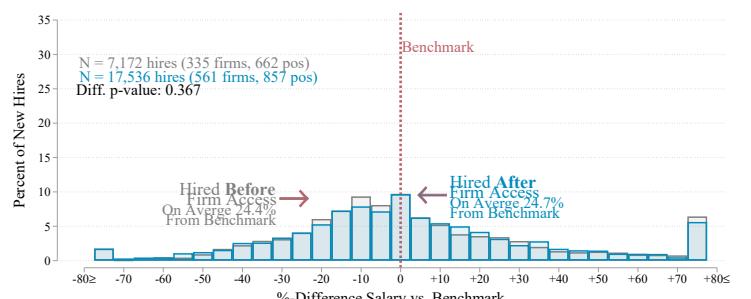
PANEL B: High Skill: Searched Positions



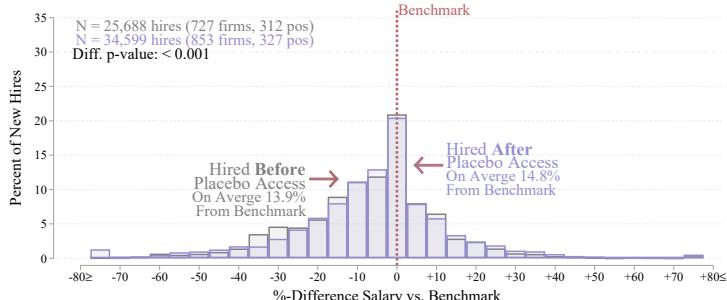
PANEL C: Low Skill: Non-Searched Positions



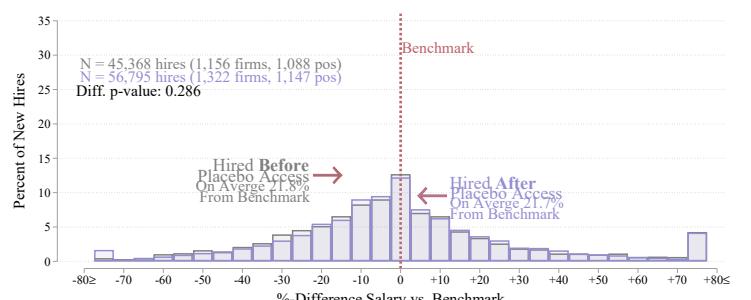
PANEL D: High Skill: Non-Searched Positions



PANEL E: Low Skill: Non-Searchable Positions

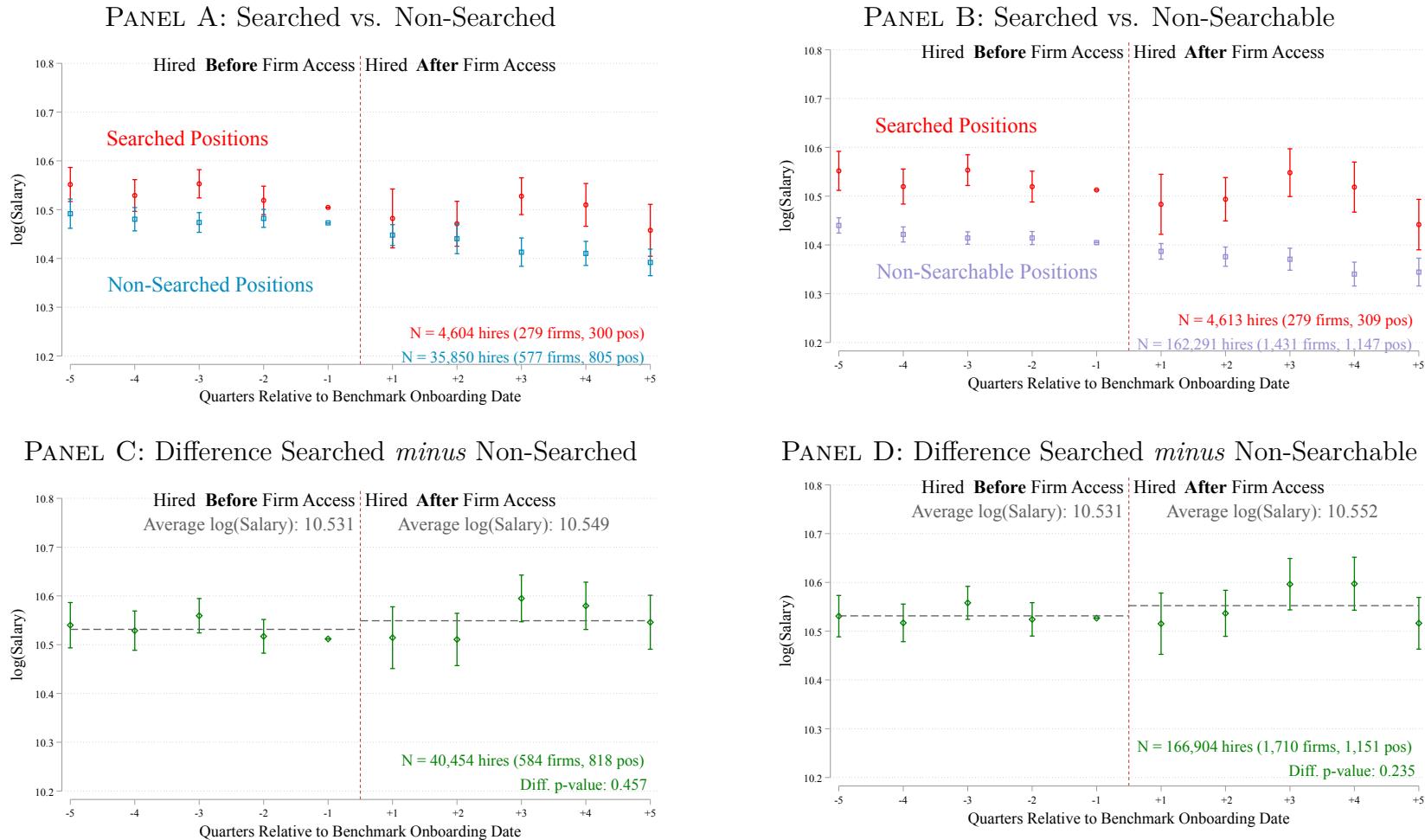


PANEL F: High Skill: Non-Searchable Positions



Notes: All figures are a reproduction of the corresponding panel of Figure 2 for low skill positions (left) and high education positions (right). Education is classified by mapping ONET codes to job zones.

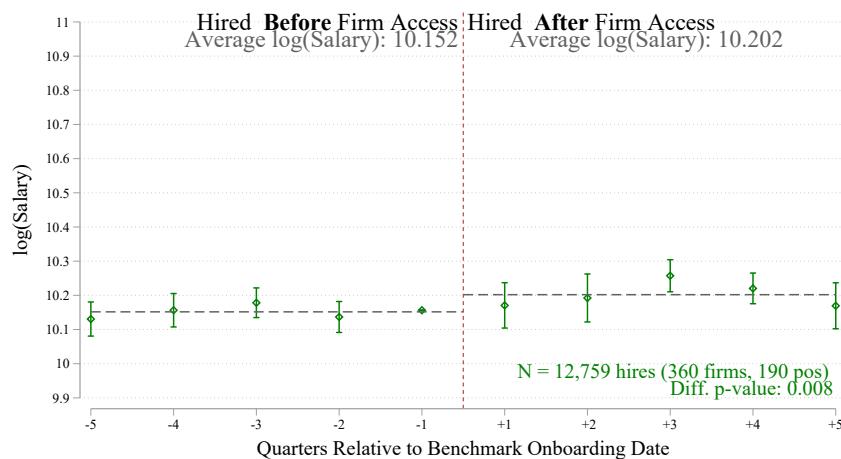
Figure 5: Event-Study Analysis: The Effects on Salary Levels



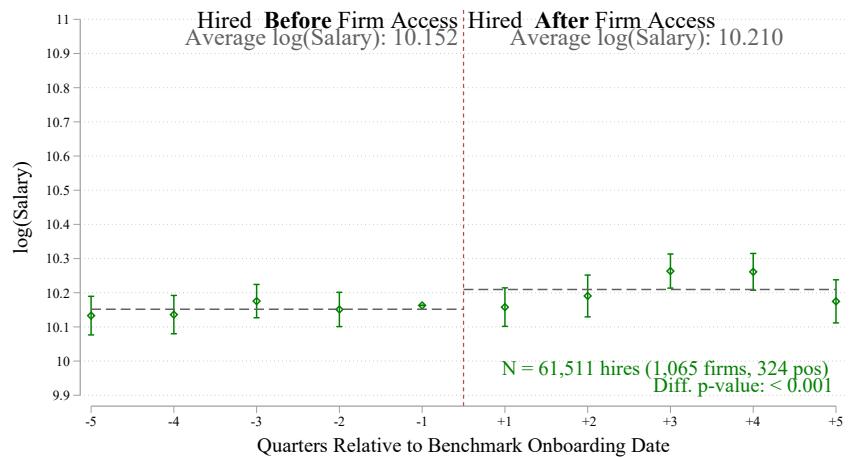
Notes: Point estimates with 90% confidence intervals in brackets, using robust standard errors. Panels A and C are based off one regression for searched and non-searched positions, while panel A presents the estimates for each position type and panel C presents the difference. Panels B and D are analogous 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  $\alpha_{1,s}^k \forall s \in S$  from equation (18) (see Section 4.2 for details).

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

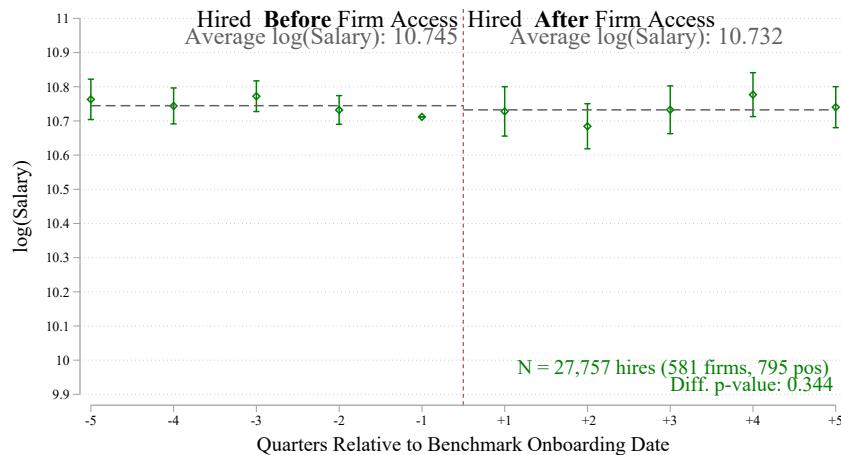
PANEL A: Low Skill: Searched vs. Non-Searched



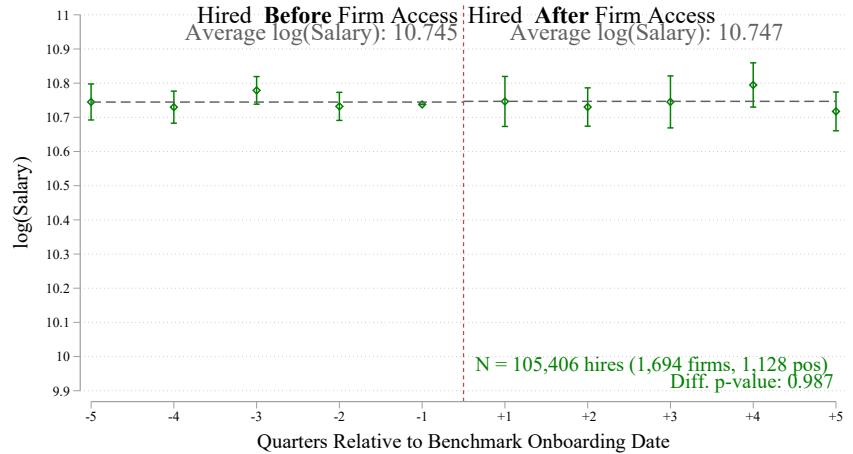
PANEL B: Low Skill: Searched vs. Non-Searchable



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



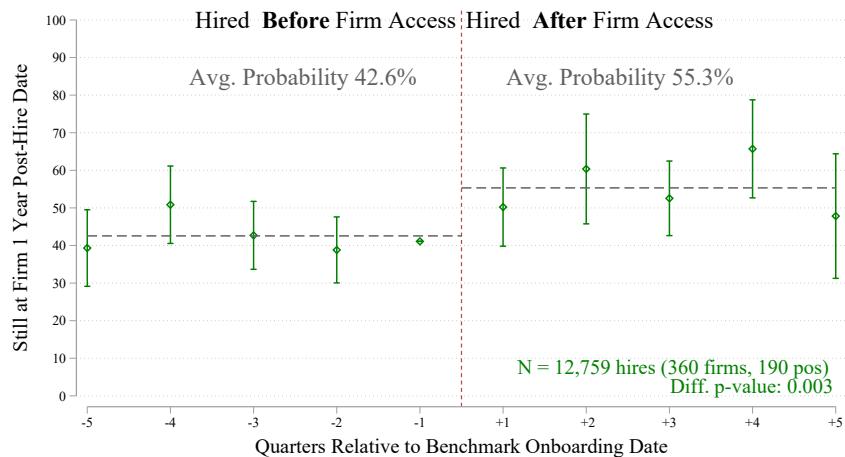
PANEL D: High Skill: Searched vs. Non-Searchable



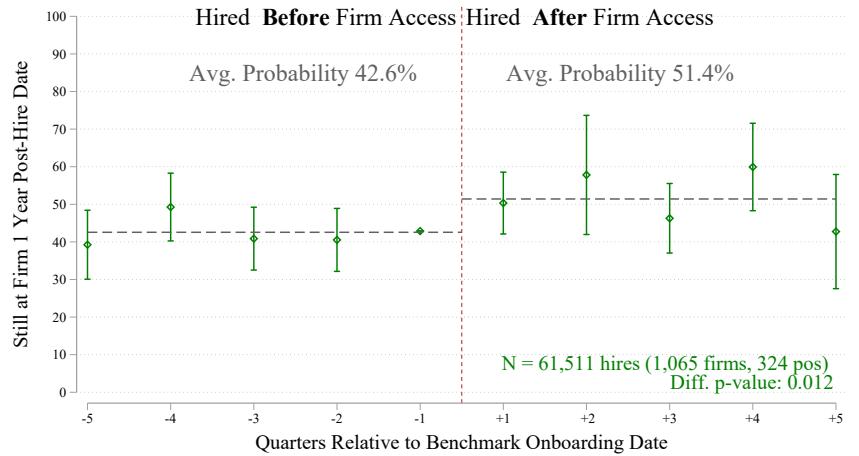
Notes: Panels A and C are a reproduction of panel C from Figure 5, and panels B and D are a reproduction of panel D, but for the specified sub-samples. *Skill* is defined in Section 3.4. See the notes of Figure 5 for more details.

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

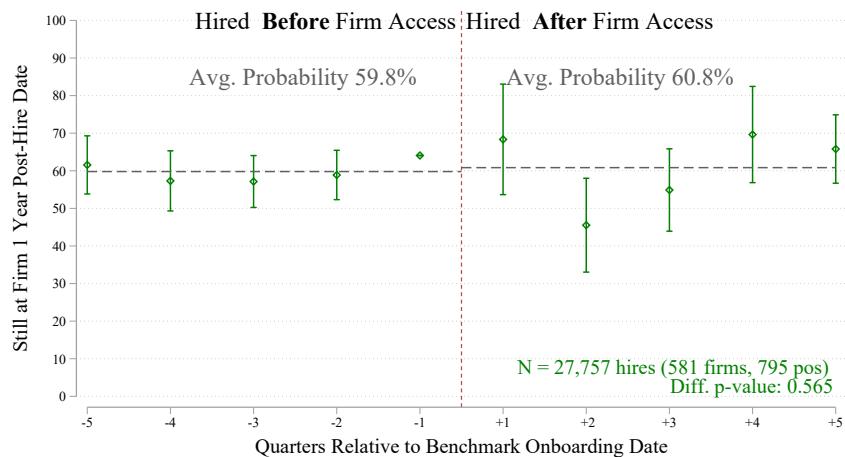
PANEL A: Low Skill: Searched vs. Non-Searched



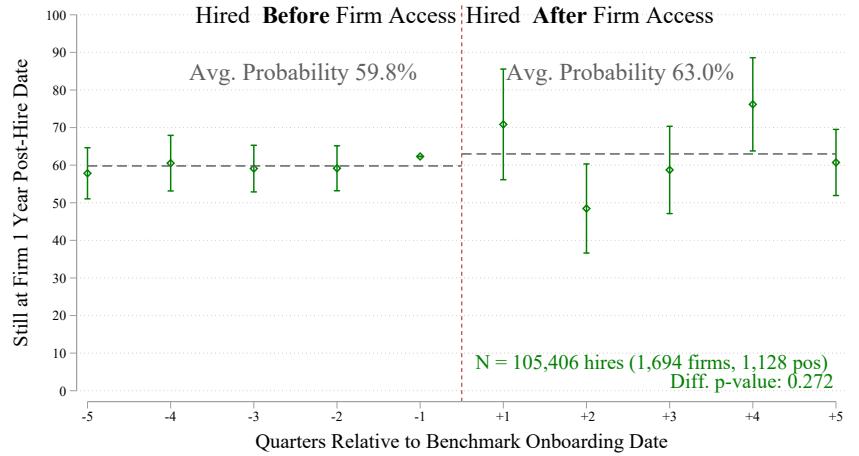
PANEL B: Low Skill: Searched vs. Non-Searchable



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



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



Notes: This is a reproduction of Figure 5, 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 5. *Skill* is defined in Section 3.4.

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 &amp; Firms

Sector	Firms (%)		Employees (%)	
	(1) Our Sample	(2) U.S.	(3) Our Sample	(4) U.S.
Agriculture, Forestry, Fishing and Hunting	0.42	0.37	0.18	0.13
Mining, Quarrying, and Oil and Gas Extraction	0.58	0.32	0.24	0.45
Utilities	0.37	0.10	0.36	0.50
Construction	2.62	11.58	1.16	5.08
Manufacturing	21.19	4.10	21.46	9.12
Wholesale Trade	8.76	4.92	10.73	4.76
Retail Trade	3.99	10.70	5.95	12.21
Transportation and Warehousing	2.15	3.05	2.25	3.78
Information	2.94	1.32	3.71	2.73
Finance and Insurance	13.58	3.94	12.19	4.98
Real Estate and Rental and Leasing	2.88	5.11	1.72	1.67
Professional, Scientific, and Technical Services	11.80	13.39	7.96	6.93
Management of Companies and Enterprises	1.10	0.45	0.94	2.69
Administrative and Support and Waste Management	4.77	5.74	7.58	9.25
Educational Services	2.15	1.54	1.37	2.87
Health Care and Social Assistance	10.96	10.81	14.40	15.74
Arts, Entertainment, and Recreation	0.79	2.15	0.32	1.84
Accommodation and Food Services	3.30	8.91	4.49	10.96
Other Services (except Public Administration)	5.66	11.50	2.98	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) All	(2) No	(3) Yes	(4) Higher	(5) Lower
Average Firm Characteristics					
Average Employment	512.1 (27.6)	520.6 (32.3)	484.4 (52.3)	530.1 (51.2)	444.0 (87.5)
Turnover Rate (%) <sup>†</sup>	2.378 (0.052)	2.391 (0.059)	2.335 (0.107)	2.389 (0.147)	2.287 (0.154)
Business Services Sector (%)	17.09 (0.96)	16.51 (1.09)	18.99 (2.08)	13.69 (2.66)	23.68 (3.09)
Hospitality Sector (%)	2.62 (0.41)	2.82 (0.48)	1.96 (0.73)	2.38 (1.18)	1.58 (0.91)
Retail & Wholesale Trade Sector (%)	13.43 (0.87)	13.86 (1.01)	12.01 (1.72)	16.07 (2.84)	8.42 (2.02)
Health Care Sector (%)	8.84 (0.73)	8.47 (0.81)	10.06 (1.59)	11.90 (2.51)	8.42 (2.02)
Banking Sector (%)	6.88 (0.65)	6.76 (0.73)	7.26 (1.37)	7.14 (1.99)	7.37 (1.90)
Other Sector (%)	51.15 (1.28)	51.58 (1.46)	49.72 (2.65)	48.81 (3.87)	50.53 (3.64)
Average Employee Characteristics					
Salary (annual \$) <sup>†</sup>	46,503 (753)	45,891 (892)	48,500 (1,351)	45,304 (1,650)	51,325 (2,069)
External Benchmark (annual \$) <sup>†</sup>	47,654 (631)	47,047 (722)	49,636 (1,301)	46,658 (1,653)	52,270 (1,952)
Abs. %-Diff. Salary vs. Benchmark <sup>†</sup>	22.11 (0.37)	22.30 (0.43)	21.49 (0.68)	19.82 (0.85)	22.96 (1.03)
Age	34.42 (0.18)	34.30 (0.21)	34.80 (0.33)	34.57 (0.44)	35.01 (0.49)
Share Female (%)	45.87 (1.25)	47.09 (1.43)	41.90 (2.57)	45.24 (3.83)	38.95 (3.47)
Share High Education (%)	41.45 (1.24)	40.33 (1.41)	45.11 (2.58)	41.96 (3.76)	47.89 (3.54)
Share Hourly (%)	71.61 (1.14)	72.58 (1.29)	68.44 (2.44)	72.02 (3.47)	65.26 (3.42)
Number of Firms	2,014	1,431	583	180	403

Notes: Average characteristics in the main sample of new hires, with robust standard errors in parentheses. Variables marked with <sup>†</sup> 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

ADP Lens	(1) Searched	(2) Non-Searched	(3) Non-Searchable
Bank Teller	511 [12]	271 [22]	1,965 [87]
Customer Service Representative	433 [42]	3,801 [169]	4,907 [401]
Security Guard	256 [5]	129 [42]	4,311 [100]
Hotel Cleaner	179 [2]	315 [5]	1,017 [17]
Legal Associate Specialist	156 [1]	6 [3]	13 [8]
Hand Packer	133 [4]	209 [15]	1,736 [56]
Patient Care Coordinator	83 [3]	93 [14]	167 [32]
Receptionist	83 [14]	289 [85]	2,692 [227]
Waiter/Waitress	74 [7]	1,041 [18]	3,014 [85]
Delivery Driver	72 [5]	28 [9]	834 [26]
Medical Assistant	66 [9]	349 [17]	959 [51]
Welder	62 [8]	104 [27]	612 [57]
Registered Nurse	61 [11]	225 [22]	2,410 [110]
Software Developer/Programmer	58 [22]	395 [75]	1,185 [167]
Assembler	57 [9]	552 [25]	3,246 [89]
Warehouse Laborer	56 [9]	593 [42]	4,872 [124]
Other Housekeeper and Related Worker	54 [5]	155 [16]	931 [65]
Dish Washer/Plate Collector/Table Top Cleaner	53 [5]	163 [17]	1,286 [69]
Mammographer	53 [1]	9 [1]	3 [2]
Nursing Assistant	51 [4]	652 [13]	6,412 [64]
Licensed Practical Nurse	48 [9]	176 [21]	1,499 [68]
Sales Manager	46 [18]	162 [65]	633 [184]
Bartender/Mixologist	45 [2]	211 [12]	625 [47]
General Practitioner/Physician	45 [2]	142 [16]	293 [27]
Lawyer	43 [5]	17 [10]	248 [47]
Ophthalmic Technician	40 [2]	4 [1]	30 [3]
Business Development Specialist	39 [2]	123 [27]	458 [44]
Warehouse Manager	39 [7]	129 [23]	411 [75]
Cook	37 [6]	318 [20]	1,327 [86]
Operations Officer	37 [2]	71 [17]	107 [37]
Other Social Work and Counseling Professional	37 [1]	1 [1]	35 [9]
Building Caretaker/Watchman	31 [1]	228 [55]	8,604 [132]
Fork Lift Truck Operator	31 [3]	237 [22]	416 [47]
Shipping Clerk	31 [4]	38 [19]	229 [67]
Administrative Assistant	29 [16]	338 [119]	2,139 [354]

Notes: New hires in each position [firms hiring in each position]. Tabulations across all new hires for the 35 searched *ADP Lenses* 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 \$) <sup>†</sup>	41,689 (127)	44,707 (641)	44,308 (371)	41,135 (139)
External Benchmark (annual \$) <sup>†</sup>	42,164 (101)	43,805 (507)	43,458 (287)	41,883 (111)
Abs. %-Diff. Salary vs. Benchmark <sup>†</sup>	19.73 (0.07)	19.94 (0.31)	20.81 (0.19)	19.56 (0.07)
Age	34.83 (0.04)	34.83 (0.20)	34.77 (0.12)	34.84 (0.05)
Share Female (%)	50.86 (0.17)	57.41 (0.80)	48.82 (0.49)	50.81 (0.19)
Share High Education (%)	30.27 (0.16)	31.24 (0.75)	34.48 (0.46)	29.59 (0.17)
Share Hourly (%)	81.38 (0.13)	77.41 (0.68)	77.20 (0.41)	82.22 (0.14)
Occupation Groups				
Office and Administrative Support (%)	16.40 (0.13)	30.41 (0.74)	27.77 (0.43)	13.95 (0.13)
Building and Grounds Cleaning (%)	7.40 (0.09)	4.13 (0.32)	1.82 (0.13)	8.41 (0.10)
Management (%)	7.15 (0.09)	8.94 (0.46)	9.02 (0.28)	6.77 (0.09)
Production (%)	9.00 (0.10)	7.69 (0.43)	7.16 (0.25)	9.34 (0.11)
Transportation and Material Moving (%)	8.85 (0.10)	5.88 (0.38)	9.13 (0.28)	8.97 (0.11)
Other (%)	51.20 (0.17)	42.95 (0.80)	45.10 (0.48)	52.55 (0.19)
Number of Firms	2,014	280	576	1,431
Number of Positions	1,406	317	960	1,306
Observations	203,185	4,686	36,049	162,450

Notes: Average characteristics in the main sample of new hires, with robust standard errors in parentheses. Variables marked with <sup>†</sup> 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)  % $\Delta$	(2) $log\Delta$	(3)  % $\Delta$   > 10	(4)  % $\Delta$	(5)  % $\Delta$	(6)  % $\Delta$	(7)  % $\Delta$	(8)  % $\Delta$	(9)  % $\Delta$	(10)  % $\Delta$	(11)  % $\Delta$	(12)  % $\Delta$
Panel (a): Post-treatment												
Searched vs. Non-Searched	-4.241*** (1.135)	-4.742*** (1.235)	-15.765*** (3.795)	-4.538*** (1.298)	-4.241*** (0.914)	-4.284*** (1.172)	-4.865*** (1.323)	-4.033*** (1.297)	-4.003*** (1.145)	-4.232*** (1.154)	-5.184*** (1.228)	-4.184*** (1.167)
Searched vs. Non-Searchable	-4.770*** (1.149)	-5.401*** (1.260)	-12.430*** (3.832)	-5.314*** (1.279)	-4.770*** (0.891)	-4.813*** (1.151)	-6.161*** (1.240)	-5.920*** (1.552)	-4.335*** (1.158)	-4.811*** (1.156)	-4.456*** (1.083)	-4.368*** (1.197)
Panel (b): Pre-treatment												
Searched vs. Non-Searched	-0.135 (1.143)	0.066 (1.314)	-6.262* (3.593)	0.036 (1.261)	-0.135 (0.790)	-0.320 (1.153)	-1.533 (1.466)	-2.312** (1.145)	-0.639 (1.107)	0.092 (1.172)	-2.351 (1.496)	-0.104 (1.156)
Searched vs. Non-Searchable	-0.602 (1.028)	-0.437 (1.187)	-4.660 (3.199)	-0.818 (1.132)	-0.602 (0.667)	-0.647 (1.036)	0.113 (1.361)	-1.145 (0.995)	-0.045 (1.026)	-0.724 (1.035)	-0.824 (1.283)	-0.598 (1.070)
Winsorizing at +/- 100%				✓								
No Clustering					✓							
No Additional Controls						✓						
No Position FE							✓					
Firm FE								✓				
Exclude High-Tip Jobs									✓			
Searched Lenses Only										✓		
No Re-weighting											✓	
Ages 21-60												✓
Mean Dep. Var. (Baseline)	19.442	20.275	63.686	20.430	19.442	19.442	19.442	19.442	19.136	19.442	19.694	19.493
Observations												
Searched	4,671	4,671	4,671	4,671	4,671	4,671	4,686	4,683	4,539	4,671	4,854	4,125
Non-Searched	35,894	35,894	35,894	35,894	35,894	35,894	36,049	36,036	34,328	31,892	36,156	31,168
Non-Searchable	162,291	162,291	162,291	162,291	162,291	162,291	162,450	162,398	155,222	135,861	162,770	138,633

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  $\alpha_1^k$  from equation (16), while pre-treatment coefficients in panel (b) refer to parameters  $\alpha_3^k$  from equation (17) (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 ( $\Delta$ ). The dependent variable in col (2) is the log of  $\Delta$  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.  $\Delta$  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 lenses of non-searched or non-searchable new hires in positions that are searched and hired by firms in the data.

# Online Appendix (For Online Publication Only)

**What's My Employee Worth? The Effects of Salary Benchmarking**  
**Cullen, Li and Perez-Truglia (August 12, 2022)**

## A Proofs of Theorems

### A.1 Proof of Theorem 2.3

Consider the informed firm's payoff when  $V_1 = v$  and  $S = s$  and it places the bid  $b(\hat{v})$ :

$$E[(V_1 - b^*(\hat{v})) \mathbb{1}_{\{Y_1 < \hat{v}\}} \mid V_1 = v, S = s] \quad (\text{A.1})$$

$$= (v - b^*(\hat{v})) F_{Y_1}(\hat{v} \mid v, s). \quad (\text{A.2})$$

Taking the derivative with respect to  $\hat{v}$  yields

$$(v - b^*(\hat{v})) f_{Y_1}(\hat{v} \mid v, s) - b^{*\prime}(\hat{v}) F_{Y_1}(\hat{v} \mid v, s). \quad (\text{A.3})$$

Let us define

$$\check{s} \equiv \sup \left\{ s : \frac{f_{Y_1}(\bar{v} \mid \bar{v}, s)}{F_{Y_1}(\bar{v} \mid \bar{v}, s)} < \frac{E[f_{Y_1}(\bar{v} \mid \bar{v}, S) \mid V_1 = \bar{v}]}{E[F_{Y_1}(\bar{v} \mid \bar{v}, S) \mid V_1 = \bar{v}]} \right\}, \quad (\text{A.4})$$

which by local relevance is not equal to  $-\infty$ . By affiliation,  $\frac{f_{Y_1}(\bar{v} \mid \bar{v}, s)}{F_{Y_1}(\bar{v} \mid \bar{v}, s)}$  is non-decreasing in  $s$ .

It follows that for all  $s < \check{s}$  we have

$$\frac{f_{Y_1}(\bar{v} \mid \bar{v}, s)}{F_{Y_1}(\bar{v} \mid \bar{v}, s)} < \frac{E[f_{Y_1}(\bar{v} \mid \bar{v}, S)]}{E[F_{Y_1}(\bar{v} \mid \bar{v}, S)]} = \frac{b^{*\prime}(\bar{v})}{\bar{v} - b^*(\bar{v})} \quad (\text{A.5})$$

where the equality follows by Theorem 2.2. Theorem 2.2 implies that  $\bar{v} - b^*(\bar{v}) > 0$ . Rearranging (A.5) yields

$$(\bar{v} - b^*(\bar{v})) f_{Y_1}(\bar{v} \mid \bar{v}, s) - b^{*\prime}(\bar{v}) F_{Y_1}(\bar{v} \mid \bar{v}, s) < 0 \quad (\text{A.6})$$

Observe that (A.6) implies that (A.3) is negative for  $\hat{v} = v = \bar{v}$  and  $s < \check{s}$ . Thus,  $\tilde{b}(\bar{v}, s) < b^*(\bar{v})$  for all  $s < \check{s}$ . Moreover,  $F_S(\check{s} \mid \bar{v}) > 0$  by local relevance. It is strictly suboptimal to place bids that exceed  $b^*(\bar{v})$ , so we have  $\tilde{b}(\bar{v}, s) \leq b^*(\bar{v})$  for all  $s$ . Thus, for any best response  $\tilde{b}$ , we have

$$E[\tilde{b}(\bar{v}, S) \mid V_1 = \bar{v}] < b^*(\bar{v}). \quad (\text{A.7})$$

Suppose that the informed firm must commit to its average bid after seeing  $V_1$  but before seeing  $S$ . That is, let  $B(x, v)$  denote the set of functions  $\beta : \mathcal{S} \rightarrow \mathbb{R}$  such that

$$E[\beta(S) \mid V_1 = v] = x. \quad (\text{A.8})$$

Let us define the payoff from choosing an average bid of  $x$  when the informed firm's value is  $v$ , that is

$$\Pi(x, v) \equiv \sup_{\beta \in B(x, v)} E \left[ (V_1 - \beta(S)) \mathbb{1}_{\{b^*(Y_1) < \beta(S)\}} \middle| V_1 = v \right] \quad (\text{A.9})$$

$$= \sup_{\beta \in B(x, v)} E \left[ E \left[ (V_1 - \beta(S)) \mathbb{1}_{\{b^*(Y_1) < \beta(S)\}} \middle| V_1 = v, S \right] \middle| V_1 = v \right] \quad (\text{A.10})$$

$$= \sup_{\beta \in B(x, v)} E \left[ \int_v^{b^{*-1}(\beta(S))} (v - \beta(S)) f_{Y_1}(\alpha \mid v, S) d\alpha \middle| V_1 = v \right]. \quad (\text{A.11})$$

Theorem 2.2 implies that  $b^*$  is continuous and hence that  $\Pi(x, v)$  is continuous in  $x$ .  $\Pi(x, v)$  is continuous in  $v$  by uniform continuity of  $f$  in  $v$ . The optimal average bid is in the interval  $[b^*(\underline{v}), b^*(\bar{v})]$ , so we can consider the correspondence

$$X^*(v) \equiv \operatorname{argmax}_{x \in [b^*(\underline{v}), b^*(\bar{v})]} \Pi(x, v). \quad (\text{A.12})$$

By Berge's theorem,  $X^*$  is upper hemicontinuous, and for all  $v$  the set  $X^*(v)$  is nonempty and compact. By construction, we have that for all  $v$  and any best response  $\tilde{b}$ ,

$$E \left[ \tilde{b}(v, S) \middle| V_1 = v \right] \in X^*(v). \quad (\text{A.13})$$

By (A.13), compactness of  $X^*(\bar{v})$ , and (A.7) we have

$$\sup X^*(\bar{v}) < b^*(\bar{v}). \quad (\text{A.14})$$

Thus, by (A.13), (A.14), upper hemicontinuity of  $X^*$ , and continuity of  $b^*$ , we have (B.4).

Suppose that instead of committing to an average bid, the firm must commit to an average payment. Paralleling the construction of (A.8), let  $\hat{B}(x, v)$  denote the set of functions  $\hat{\beta} : \mathcal{S} \rightarrow \mathbb{R}$  such that

$$E \left[ \hat{\beta}(S) \mathbb{1}_{\hat{\beta}(S) \geq b^*(Y_1)} \middle| V_1 = v \right] = x. \quad (\text{A.15})$$

*Mutatis mutandis*, the same argument establishes that for  $v$  close enough to  $\bar{v}$ , we have (8).

## A.2 Proof of Theorem 2.5

This follows as in the proof of Theorem 2.3, with the following modification. Suppose that the informed firm must commit to its average probability of hiring after seeing  $V_1$  but before seeing  $S$ . Let  $\hat{B}(x, v)$  denote the set of functions  $\hat{\beta} : \mathcal{S} \rightarrow \mathbb{R}$  such that

$$P \left( \hat{\beta}(S) \geq b^*(Y_1) \middle| V_1 = v \right) = x. \quad (\text{A.16})$$

Observe that  $\tilde{b}(\bar{v}, s) < b^*(\bar{v})$  for all  $s < \check{s}$ . Thus, by  $f$  strictly positive everywhere on  $[\underline{v}, \bar{v}]^N \times \mathcal{S}$ , we have

$$P \left( \tilde{b}(\bar{v}, S) \geq b^*(Y_1) \middle| V_1 = \bar{v} \right) < 1 = P \left( b^*(\bar{v}) \geq b^*(Y_1) \middle| V_1 = \bar{v} \right). \quad (\text{A.17})$$

*Mutatis mutandis*, the same argument as in the proof of Theorem 2.3 then establishes (10).

## B Model Extensions

### B.1 An extension to multiple benchmarks

Firms observe salary benchmarks even before they gain access to the benchmark tool that we study. Our results extend to this case. Suppose an extended model with multiple signals,  $S_1, S_2$ , with  $S_1$  a real-valued random *vector* that captures all public information that was previously available and  $S_2$  a real-valued benchmark. We assume that the random variables  $(S_1, S_2, V_1, \dots, V_n)$  affiliated. It follows from the definition (1) that conditional on any realization  $S_1 = s_1$ , the random variables  $S_2, V_1, \dots, V_n$  are affiliated. Thus, there exists a symmetric increasing equilibrium of the first-price auction in which the firms publicly observe  $S_1$ , characterized by the equations

$$b^*(v, s_1) = \underline{v}, \quad (\text{B.1})$$

$$b^{*\prime}(v, s_1) = (v - b^*(v, s_1)) \frac{E[f_{Y_1}(v | v, s_1, S_2) | V_1 = v, S_1 = s_1]}{E[F_{Y_1}(v | v, s_1, S_2) | V_1 = v, S_1 = s_1]}. \quad (\text{B.2})$$

The key additional assumption is that the benchmark is **locally conditionally relevant**, meaning that for all  $v$  and all  $s_1$ , there exists  $s_2$  such that  $0 < F_{S_2}(s_2 | v, s_1)$  and

$$\frac{f_{Y_1}(v | v, s_1, s_2)}{F_{Y_1}(v | v, s_1, s_2)} < \frac{E[f_{Y_1}(v | v, s_1, S_2) | V_1 = v, S_1 = s_1]}{E[F_{Y_1}(v | v, s_1, S_2) | V_1 = v, S_1 = s_1]}. \quad (\text{B.3})$$

This condition requires that with positive probability the firm with value  $v$  that observes  $S_1 = s_1$  would wish to revise their bid after seeing the benchmark  $S_2$ . It implies that  $S_1$ , considered as a signal of  $Y_1$ , does not Blackwell-dominate  $S_2$ .

With these assumptions, Theorem 2.3 and Theorem 2.6 extend naturally, for  $\tilde{b}$  and  $b^{**}$  defined analogously.

**Theorem B.1.** *There exists  $\tilde{v} < \bar{v}$  such that for all  $v > \tilde{v}$  and all  $s_1$ , we have that*

$$E[\tilde{b}(v, S_1, S_2) | V_1 = v, S_1 = s_1] < b^*(v, s_1). \quad (\text{B.4})$$

**Theorem B.2.** *The equilibrium with the benchmark yields higher expected salaries than the no-benchmark equilibrium, that is*

$$E\left[\max_i b^{**}(V_i, S_1, S_2)\right] \geq E\left[\max_i b^*(V_i, S_1)\right]. \quad (\text{B.5})$$

We omit the proofs because they are trivial adaptations of the earlier arguments.

### B.2 A foundation for affiliated benchmarks

We offer a stylized foundation for affiliation. Let us suppose that there are  $m > 1$  workers, each of whom receives offers from a disjoint set of  $n \geq 2$  firms. To simplify the analysis, we assume that each firm is attempting to hire for this position-title for the first time,

We are going to permit the firms bidding for worker  $m$  to observe the median of the accepted offers from the ‘earlier’  $m - 1$  workers. To simplify the calculation we assume that  $m - 1$  is odd.

Let  $V_j^i$  be the value of the firm  $i$  for worker  $j$ . We assume that the set of random variables  $V_j^i$  for  $i = 1, \dots, m$  and  $j = 1, \dots, m$  are affiliated with joint density  $f$ . For vector  $\vec{v} \in \mathbb{R}^{m \times n}$ , we use  $\vec{v}_j$  to denote the elements relating to worker  $j$ , and  $\vec{v}_{-j}$  to denote the other elements.

We assume that  $f$  is symmetric within workers; that is, for any worker  $j$  and any permutation  $\pi : \{1, \dots, n\} \rightarrow \{1, \dots, n\}$ , we have that  $f(\vec{v}) = f(\pi(\vec{v}_j), v_{-j})$ . We assume that  $f$  is symmetric across the earlier workers; that is, for any permutation  $\pi : \{1, \dots, m - 1\} \rightarrow \{1, \dots, m - 1\}$ , we require that  $f(\vec{v}) = f(\vec{v}_m, \vec{q}_{-m})$ , where  $q_j^i = v_{\pi(j)}^i$ .

By symmetry within workers, for each  $j$  there exists a symmetric no-benchmark equilibrium of the first-price auction, with associated non-decreasing bidding strategy  $\bar{b}_j$ . By symmetry across earlier workers, there exists  $\bar{b}$  such that for all  $j \leq m - 1$ ,  $\bar{b}_j = \bar{b}$ . Let  $Y_j^k$  be the  $k$ th highest value in the set  $\{V_j^1, \dots, V_j^n\}$ .

We now state a theorem that offers a foundation for identifying the signal  $S$  with the median of previously accepted offers. We start with firm values for workers of a given position-title that are affiliated across workers, and derive the conclusion that the median of previously accepted offers is affiliated with the values of firms bidding for the current worker.

**Theorem B.3.** *Let the benchmark be  $S \equiv \text{median}_{j \leq m-1} (\bar{b}(Y_j^1))$ . The random variables  $S, V_m^1, \dots, V_m^n$  are affiliated.*

*Proof.* We start by stating a mild generalization of Theorem 2 of Milgrom and Weber (1982b); we omit the proof because it is essentially identical.

**Lemma B.4.** *For any random variables  $X_1, \dots, X_r$  and any  $q < r$  let  $Y^l$  denote the  $l$ th highest random variable in  $X_1, \dots, X_q$ . If  $X_1, \dots, X_r$  are symmetric in  $X_1, \dots, X_q$  and are affiliated, then  $Y^1, \dots, Y^q, X_{q+1}, \dots, X_r$  are affiliated.*

By symmetry within workers and Lemma B.4, we have that  $(Y_k^l)_{j \leq m-1}^{l=1, \dots, n}, V_m^1, \dots, V_m^n$  are affiliated. By Theorem 4 of Milgrom and Weber (1982b), we have that  $(Y_j^1)_{k \leq m-1}, V_m^1, \dots, V_m^n$  are affiliated. By  $\bar{b}$  non-decreasing and Theorem 3 of Milgrom and Weber (1982b), we have that  $(\bar{b}(Y_k^1))_{k \leq m-1}, V_m^1, \dots, V_m^n$  are affiliated. Let us define  $S \equiv \text{median}_{k \leq m-1} (\bar{b}(Y_k^1))$ . By symmetry across earlier workers, Lemma B.4, and Theorem 4 of Milgrom and Weber (1982b), we have that  $S, V_m^1, \dots, V_m^n$  are affiliated, which completes the proof.  $\square$

Theorem B.3 explains how, under competitive bidding, salary benchmarks based on accepted offers for previous workers can be informative about bids for the current worker.

**Example B.5.** *There is a latent productivity component  $Q$  that characterizes the productivity of all workers in a given position-title. Every  $V_j^i$  is independent conditional on  $Q$  with a density  $g(v_j^i | q)$  symmetric across all  $i$  and  $j$  and satisfies the monotone likelihood ratio property. Then the variables  $Q, (V_j^i)_{j \in \{1, \dots, m\}}$  are affiliated, and the assumptions of Theorem B.3 are satisfied. It follows that in the resulting equilibrium, the benchmark  $S \equiv \text{median}_{j \leq m-1}(\bar{b}(Y_j^1))$  and the firm values  $V_m^1, \dots, V_m^n$  are affiliated.*

Theorem B.3 can be extended to allow the benchmark to be updated periodically, so that we have a sequence of workers separated into tranches, with the firms bidding in the tranche  $k$  observing the median accepted offer for each tranche in  $\{1, \dots, k-1\}$ .

### B.3 Heterogeneity by Skill level

In particular, consider the following two types of positions. Low-skill positions are standardized and easy to monitor, and as a result any two workers in that position can provide the same productivity. On the contrary, high-skill workers can vary a lot in productivity. This individual variation can be captured by modifying Example B.5, with the result that observing the accepted salaries of previous workers is uninformative about other firms' offers for the current worker. If we model high-skill positions as having *more individual productivity variation*, then the model predicts that the benchmark will have a stronger effect on low-skill offers.

**Example B.6.** *There is a latent productivity component  $Q_i$  that affects each worker, with  $Q_i$  independently and identically distributed across workers. Every  $V_j^i$  is independent conditional on  $Q_i$  with a density  $g(v_j^i | q_i)$  symmetric across all  $i$  and  $j$  and satisfies the monotone likelihood ratio property. By construction, the conditional distribution of values for any individual worker is the same as in Example B.5, but bids on earlier workers are not informative about bids on the current worker, so the benchmark has no effect on offers, regardless of whether it is covertly or publicly observed.*

This is of course just one approach to modelling high-skill versus low-skill positions. One could take alternative approaches, which could yield different, and even the opposite prediction. For instance, assume that firms making offers for low-skill positions already face little uncertainty about competing offers, so that the benchmark does not much move their beliefs. In the extreme case, we have  $F_{Y_1}(x | v, s) = F_{Y_1}(x | v)$  for low-skill positions, but  $F_{Y_1}(x | v, s) \neq F_{Y_1}(x | v)$  for high-skill positions. This yields the prediction that the benchmark will have a larger effect on offers for high-skill positions than for low-skill positions.

## B.4 Interpreting first-price auctions as capturing retention concerns

We have modeled firms making simultaneous offers to each worker, but plausibly firms use benchmarks because they are motivated by retention concerns. However, we can show that the theoretical predictions hold in an stylized model of retention concerns.

Retention concerns rely on information frictions; if a firm can easily verify competing offers to its employees, then it can wait to match those offers instead of paying more from the start. One natural model with such frictions is as follows:

1. Firm 1 makes an offer  $w_1$  to the worker.
2. The worker observes  $w_1$ , and either accepts or rejects.
3. Firm 2 makes an offer  $w_2$  to the worker, without observing  $w_1$ .
4. The worker observes  $w_2$ , and either accepts or rejects.

In this simple model, firm 1 has to trade off between paying too high (relative to firm 2) and risking unwanted turnover. Each firm must make offers without a chance to learn or respond to competing offers, and because of these information frictions the game between firms is strategically equivalent to a first-price auction. Hence, the earlier theoretical predictions hold also for this stylized model of retention concerns.

## C Expert Prediction Survey

### C.1 Survey Design

To assess whether the experimental results are surprising, we conduct a forecast survey with a sample of experts. A sample of the full survey instrument is attached as Appendix M. In this survey, which follows best practices (DellaVigna and Gentzkow, 2019), we start by describing the benchmarking tool and outlining a hypothetical experiment where some firms are randomly given access to salary benchmarks and other firms are not.<sup>41</sup> We then elicit beliefs about the effects of access to the benchmarking tool on average salary levels, compression, heterogeneity by education and heterogeneity by gender. For each forecast, we ask about confidence and include an open-ended question for respondents to explain their choices. To elicit beliefs about effects on compression, we show the six histograms in Figure C.1.

### C.2 Implementation

We collected responses from experts in two ways. First, we posted the survey on the Social Science Prediction Platform from May 6, 2022, to June 24, 2022. Second, on May 9, 2022, we emailed an invitation to the prediction survey directly to a list of 500 professors with publications related to our experiment, and gave them 7 days to complete the survey.

We exclude respondents who are not academics (8 respondents), with less than a Ph.D. (unless they are currently Ph.D. students), who had already seen our study (4 subjects). The final sample includes 68 experts. Of these, 11.8% responded to the survey through the Social Science Prediction Platform, and the remaining 88.2% responded through our email invitation. This final is comprised of 90.7% professors, 2.9% PhD students, and 7.4% researchers. Most (91.2%) are from the field of economics; 85.3% report having done research on labor economics.

### C.3 Survey Results

Figure C.2 shows the distribution of certainty for each question where we asked about confidence. The majority (64.3%) of responses to all four confidence questions were *Not Confident at All* or *Slightly Confident*. Few respondents (4.0%) were ever *Very Confident* and no one ever responded *Extremely Confident*. There was more confidence in the responses to some questions than other; for example, 45.6% of respondents were *Very* or *Somewhat* confident in their response to the question about education heterogeneity.

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<sup>41</sup>We opt for this simpler version because the full quasi-experimental design would have added too much complexity to the survey.

Figure C.3 displays which histogram, among the options in Figure C.1, respondents selected as their prediction of the effects of benchmarking. Only 30.9% of respondents selected the histogram corresponding to our experimental results, which showed compression from above and below. However, the vast majority of respondents predicted there would be *some* effect—only 5.9% of respondents selected the histogram that showed no effects. The most common recurring theme in the open-ended responses, regardless of answer selected (except for those who selected no effect), is an expectation to see a reduction in variance in the percent difference between salary and benchmark.

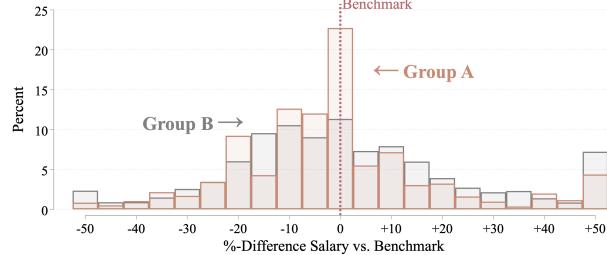
Figure C.4 shows the predictions about salary levels. Here, respondents were presented with a text box to enter their predicted percent change in salary levels as a result of benchmarking. Responses varied across the options of increasing, decreasing, and no effect on salaries, but 67.7% of numeric responses were within the 90% confidence intervals of our estimates from Table G.1, overlaid on Figure C.4. Many of the open-ended responses to this question echo the sentiment of one respondent who reasoned that it would be “equally likely that [employers] would revise their salary up or down given the information from the benchmarking.”

Panel A of Figure C.5 displays the predictions about whether high or low education positions will be more strongly affected by benchmarking. 61.8% percent of experts predicted that high-education positions would be most strongly affected, in contrast to our results which show it is actually low-education positions which are more strongly impacted. This was also the question where experts indicated the highest confidence. In the open-ended responses, those who selected high and low education positions both frequently noted there should be less compression at baseline among high-education positions. Those who believed high education positions would be more strongly affected tended to interpret this to mean “information about the true distribution should be more valuable” for high education positions. On the other hand, those who select low education positions took that to mean the benchmark would be less useful for high-education positions (e.g. “Higher end jobs are more heterogeneous and therefore firms have more reasons to differentiate from the market median”).

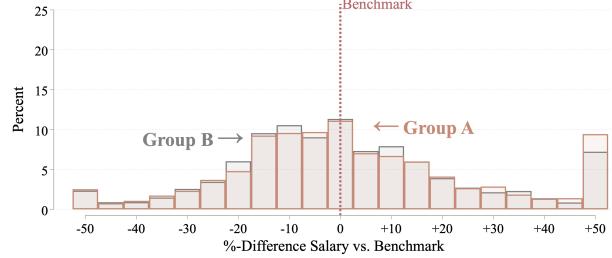
Panel B of Figure C.5 shows predictions on the effect of benchmarking on the gender pay gap. This is perhaps the most accurate prediction, as 66.2% of experts responded that the wage gap would be reduced and Table I.1 shows salaries increase for women and stay the same for men. Open-ended responses among those who predicted a reduction in the gender pay gap often mentioned that “bargaining becomes less important if an external source anchors the salary” or that “the employer may rely less on individual negotiations and biases, which often work against women.”

Figure C.1: Histogram Choices in Survey Question About Compression

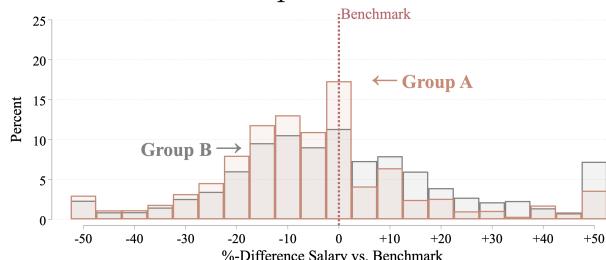
PANEL A: Compression from Above and Below



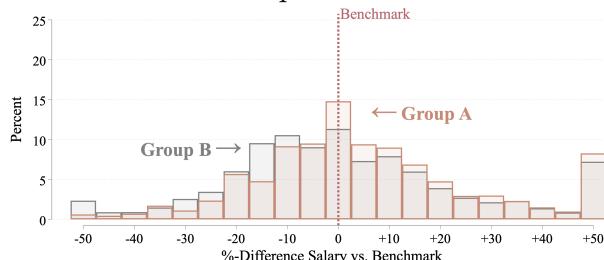
PANEL B: No Effect



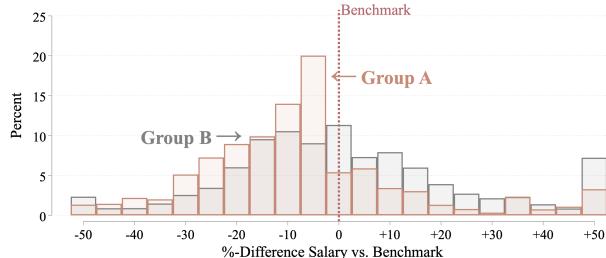
PANEL C: Compression from Above



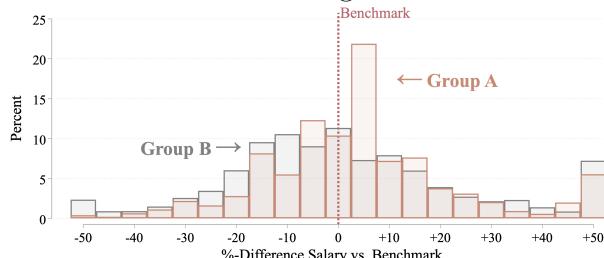
PANEL D: Compression from Below



PANEL E: Left Shift



PANEL F: Right Shift



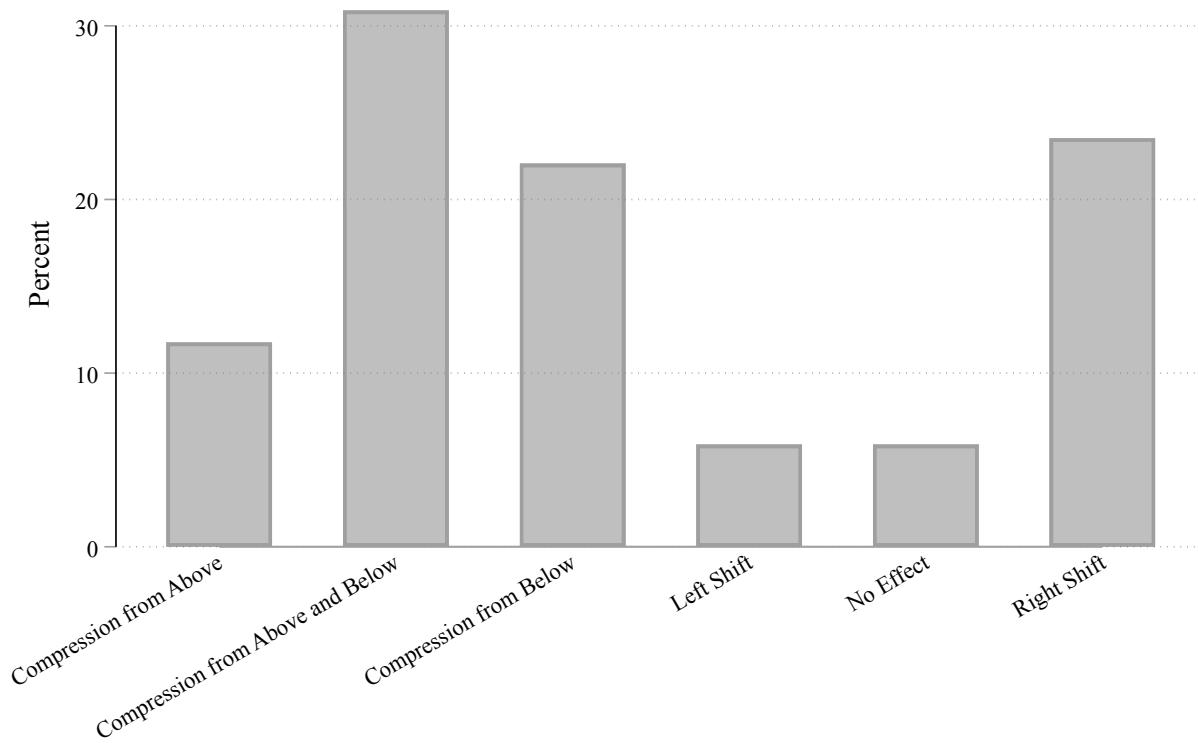
Notes: These are the images respondents could choose from when considering the compression effects of the benchmarking tool. Each figure is intended to show the effect described in the panel title. Panel A is an altered reproduction of Panel A of Figure 2.

Figure C.2: Certainty in Survey Responses



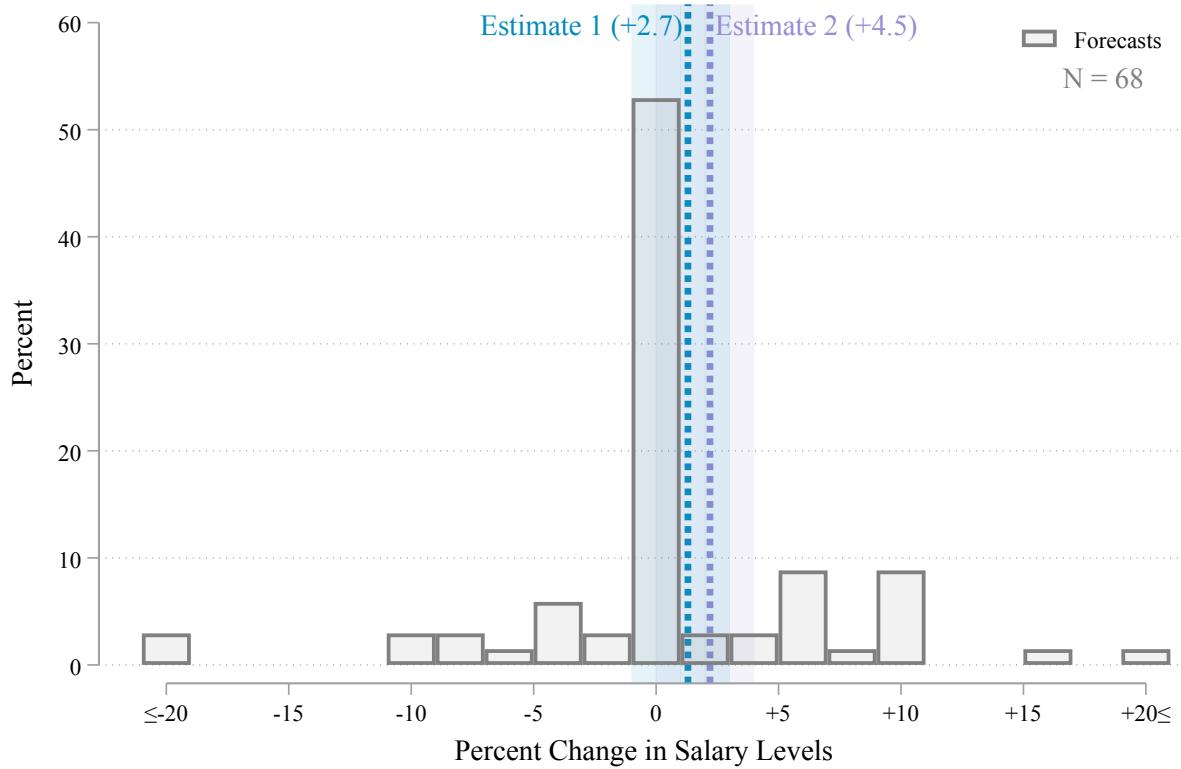
Notes: Histogram of the response certainty for each question in the survey. Possible answers are *Not Confident at All*, *Slightly Confident*, *Somewhat Confident*, *Very Confident* and *Extremely Confident*.

Figure C.3: Histogram Choice in Survey Responses



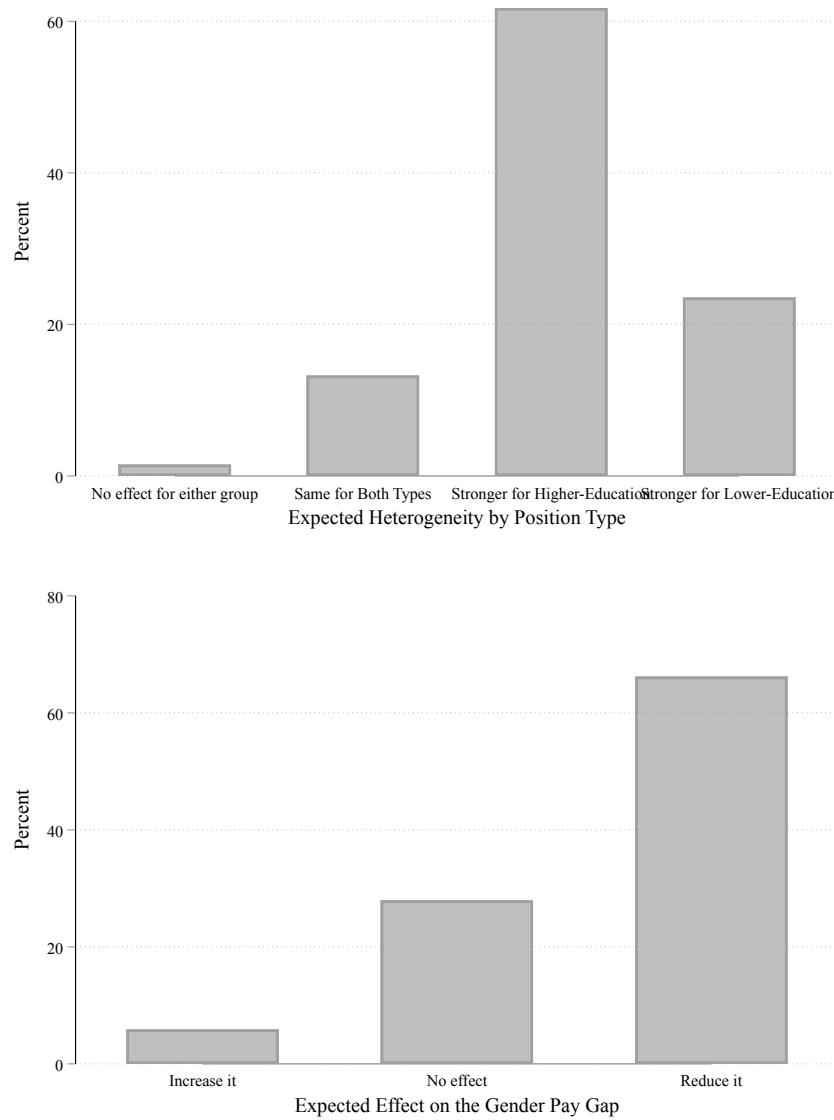
Notes: Histogram of the responses to survey question about the effects of benchmarking on compression. The six possible choices are displayed in Figure C.1.

Figure C.4: Salary Level Predictions



Notes: Histogram of the responses to survey questions about percent change in salary levels. Respondents were presented with a text box if they predicted salaries would go up or down. If they responded salary levels would stay the same, we include that here as 0% change. *Estimate 1* is the searched vs. non-searched estimate of salary level effects from Table G.1 and *Estimate 2* is the searched vs. non-searchable estimate. Displayed are the 90% confidence intervals.

Figure C.5: Heterogeneity Predictions



Notes: Histogram of the responses to survey questions about heterogeneity by education and gender in the effects of benchmarking.

## D Additional Details about the Data

### D.1 Proprietary Job Taxonomy

Before September 2020, the company used a proprietary job taxonomy that spanned 2,236 distinct position titles. To understand the granularity of this taxonomy, the first column of Table D.1 shows a list of 31 position titles related to teaching. The titles are quite specific in that, for example, they distinguish between grade (e.g., primary vs. secondary school) as well as by different topics (e.g., Math vs. Humanities).

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. However, the results presented in Appendix F, in which we expand the sample to include hires after March 2020, do include the old as well as the new taxonomies. The new taxonomy spans 9,002 distinct position titles. For example, as shown in the second column of Table D.1, the new taxonomy expanded the list of teachers from 31 to 87 job titles.

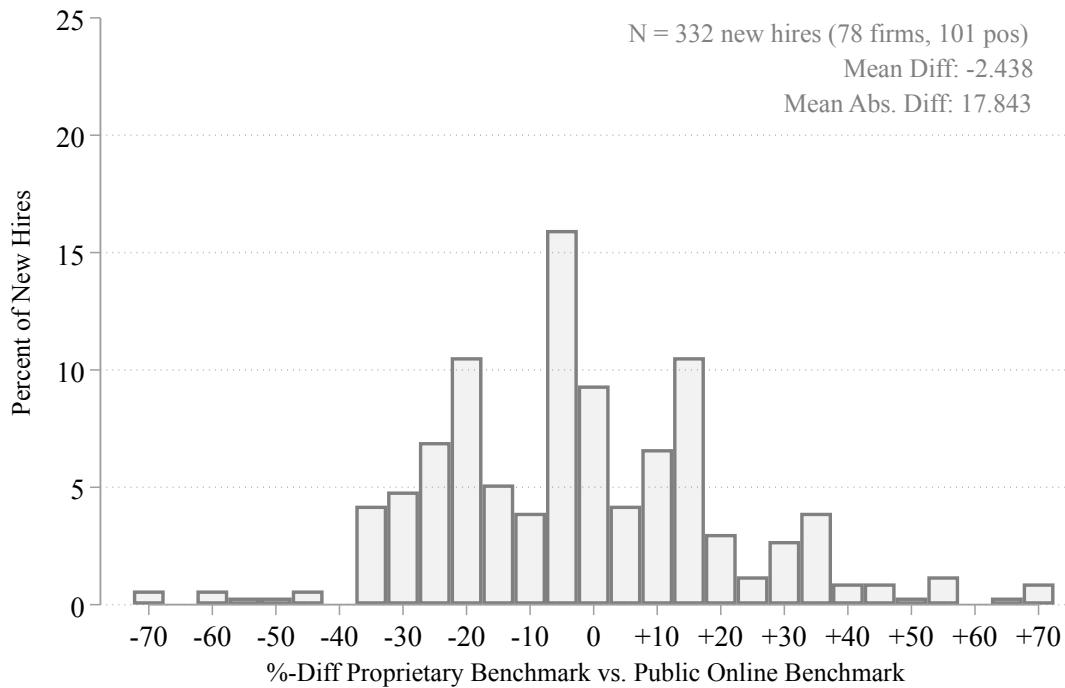
### D.2 Benchmark Data

We have quarterly benchmark data for each ADP Lens, as well as for each ADP Lens, work state, and sector combination. However, we do not have the O\*NET benchmark data (what a firm would have seen if they searched by O\*NET) and thus we exclude positions searched only by O\*NET from the searched positions. These positions are also excluded from the non-searched positions because firms obtained information from the O\*NET search.

In Figure D.1, we compare our salary benchmark, constructed from proprietary payroll data, with a popular free online salary benchmark (Glassdoor) constructed from anonymous users who supply their salary information in order to see the salaries of others (all anonymously). According to Adler (2020a), approximately a fifth of HR professionals report using Glassdoor exclusively and many more used it at one point or another. Indeed, Glassdoor is the most popular alternative to proprietary sources according to her ethnographic study. To compare these two benchmarks, we use the salient number provided by Glassdoor (outwardly this number is referred to as the “average salary” on the website, however in the technical notes Glassdoor report that this number is calculated using the median salary reported), with the similarly salient median salary reported in our proprietary salary benchmark. We apply the same filters to both benchmarks— the position title, industry and state— and discard data for which there are fewer than 30 data points available to construct the benchmark. Figure D.1 is a histogram of the percent different between the proprietary salary benchmark and

the crowdsourced public benchmark. We observe a great deal of variation. Approximately 47 percent of positions in the proprietary source are more than 15% higher or lower than the public benchmark, and distributed roughly symmetrically around the public benchmark.

Figure D.1: Comparison between Proprietary vs. Public Online Benchmarks



Notes: Histogram of the percent difference between the proprietary median base salary benchmark and a public online benchmark of average salary. The public online source provides a benchmark for average salary, but does not provide a median benchmark. The graph includes searched new hires in Q4 of 2019 in the main analysis for which the free online benchmark is available. We use the proprietary benchmark with state, sector, and lens filters when available for more than 30 employees, otherwise we used the unfiltered benchmark; we use the corresponding filters for the public benchmark comparison. The percent difference is winsorized at  $\pm 70\%$ .

Table D.1: Sample Job Taxonomy for Positions Related to Teaching

Taxonomy Before Sept-1-2020	Taxonomy After Sept-1-2020
Art and Craft Instructor	Arts Teacher, Calligraphy Teacher, Dress Making Teacher, Flower Arrangement Teacher
Conductor/Director of Orchestra/Band/Choir	Choir Teacher
Distance Learning Specialist	Live E-Learning Teacher
High School Math Teacher	High School Math Teacher
Humanities School Teacher	Humanities School Teacher
Junior College and Pre-University Teacher	College And Pre-University Teacher, Sciences Teacher
Language Instructor	Chinese Teacher, English As A Second Language Teacher, English Teacher, German Teacher, Hebrew Teacher, Spanish Teacher, Subject Teacher Languages And Literature
Middle School Mathematics Teacher	Middle School Algebra Teacher
Music Instructor	Flute Teacher, Guitar Teacher, Music Teacher, Music Teacher Viola, Piano Teacher, Violin Teacher
Primary School Music Teacher	Band Teacher, School Music Teacher
Other Special Education Teacher	Adaptive Physical Education Teacher, Special Education Teacher
Other Teaching Professional n.e.c.	Lower School Dean; Teacher 5th Grade, Teacher Of Post-Secondary Non-Tertiary Education
Physical Fitness Instructor	Physical Education Teacher
Pre-School Teacher Assistant	Pre-School Teacher Assistant
Preschool and Kindergarten Teacher	Lead Teacher (Preschool), Preschool And Kindergarten Teacher, Preschool Education Teacher
Primary School Teacher	Primary School Art Education Teacher, Primary School Classroom Teacher, Elementary School Teacher, Primary School Foreign Language Teacher, Montessori Teacher
Secondary School Special Education Teacher	Secondary School Special Education Teacher
Secondary School Teacher	Middle School Art Teacher, French Language Teacher, High School Teacher, History Teacher, Language Teacher, Middle School, Latin Teacher, Mathematics Teacher, Middle School Science Teacher, Physics Teacher, Secondary Education Science Teacher
Social Studies Teacher	Social Studies Teacher
Special Education Teacher	Intellectual Disability Teacher, Special Educational Needs Teacher Primary School
Special Education Teacher for Infants and Children	Early Childhood Special Education Teacher, Early Childhood Teacher
Speech and Drama Instructor	Speech Teacher
Substitute Teacher	Substitute Teacher
Middle School Teacher	Middle School Reading Teacher, Middle School Teacher, Middle School Visual Arts Teacher
Teacher of the Blind	Teacher For The Visually Impaired, Teacher Of The Sight Impaired
Teachers Aide	Teacher Assistant, Teacher's Aide
Technical/Vocational/Commercial Education Institute Teacher and Trainer	Commerce Vocational Teacher, Commercial School Teacher, Secretarial School Teacher, Technical School Teacher, Vocational Education Teachers And Trainers, Vocational School Teacher, Vocational Teacher Welding
Theater Education Teacher	Theater Education Teacher
Title I Teacher (Primary School)	Title I Teacher (Primary School)
University Lecturer	Adjunct Professor Of Teacher Education, Anthropology Teacher, Postsecondary, Computer Science Teacher, Postsecondary, Handwork Teacher, Teacher In Engineering And Architecture (University), Teacher, Arts Subjects (University), University And Higher Education Teacher
Vocational Education Teacher (Hospitality)	Cooking Teacher

Notes: *Taxonomy Before Sept-1-2020* shows the 31 position titles related to teaching from the taxonomy that was used prior to Sept-1-2020. *Taxonomy After Sept-1-2020* shows the corresponding 82 position titles from the taxonomy that was used starting on Sept-1-2020. We exclude 5 lenses that were introduced in the new taxonomy but do not match to a specific job from the old taxonomy: Business Teacher, Chemistry Teacher, Middle School English Teacher, Primary School Teacher Spanish and Writing Teacher.

## E Utilization Analysis

Among firms with access to the tool in a post-onboarding month, a minority of firms (4.2%) do not use the tool. Among the remaining firms, the average search probability is 19% per month, and the firm with the highest utilization has probability of searching of 92%. Among positions in which the firm will hire in the near future (3 months), the probability that a position is looked up in the tool is 1.48%. Among the positions for which the firm will not hire in the near future, the probability of searching goes down to 1.02%.<sup>42</sup>

In Table E.2, we display the position titles of the most commonly searched positions, normalized by the overall number of new hires in that position. To be explicit, we consider firms that hire at least 10 people in a particular position after gaining access to the salary benchmarking tool. Then we ask, what share of those new hires were classified as Searched, in the sense that the firm looked up their position prior to their hire date. In Table E.2, we list those positions, the number of new hires in that position after access, and the share who were Searched. We order them so the top of the list is the position which had the highest Search share. While the very top positions searched are highly specialized, the upper quartile includes general management positions as well as standardized lower skill positions.

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<sup>42</sup>Some of the searches in this latter group could also be motivated by new hires, as it is possible that the firm was planning to hire someone new for that position but the hire did not materialize.

Table E.1: Predictors of Utilization

	(1) Visit (=1)	(2) No. of Visits	(3) Visit (=1)	(4) No. of Visits
Hire This or Next Quarter (=1)	1.371*** (0.069)	1.412*** (0.087)	1.360*** (0.068)	1.376*** (0.046)
Log(No. of Existing Employees) <sup>†</sup>	1.163*** (0.024)	1.175*** (0.031)	1.276*** (0.027)	1.271*** (0.018)
Log(Median Salary) <sup>†</sup>	1.023 (0.051)	1.068 (0.062)	1.204*** (0.063)	1.269*** (0.046)
Share $ \% \Delta  > 10$	0.838*** (0.042)	0.909 (0.059)	0.894** (0.043)	0.950 (0.031)
Share Employees Paid Above Benchmark	0.888** (0.045)	0.897* (0.052)	1.036 (0.052)	1.071** (0.036)
Share Female (0-1)	1.385*** (0.062)	1.408*** (0.079)	1.444*** (0.065)	1.515*** (0.047)
Share High Education (0-1)	1.264*** (0.058)	1.219*** (0.069)	1.235*** (0.054)	1.156*** (0.035)
Share Hourly (0-1)	0.730*** (0.039)	0.700*** (0.045)	0.738*** (0.042)	0.713*** (0.028)
Log(Median Age)	0.561*** (0.043)	0.521*** (0.050)	0.624*** (0.049)	0.602*** (0.033)
Log(Median Tenure)	0.914 (0.052)	0.952 (0.072)	0.952 (0.053)	0.963 (0.037)
Firm FE			✓	✓
Number of Firms	570	570	414	414
Number of Positions	1,481	1,481	1,401	1,401
Observations	83,871	83,871	66,430	66,430

Notes: Significant at \*10%, \*\*5%, \*\*\*1%. Standard errors in parentheses. Each column corresponds to one regression: columns (1) and (3) use logistic regression and report odds ratios, columns (2) and (4) use Poisson regression and report incidence-rate ratios. Observations are at the firm-position-quarter level for quarters 2019Q4 and 2020Q1. The dependent variable *Visit* (=1) in columns (1) and (3) is a dummy indicating if a firm searched that position in the current quarter, and the dependent variable *No. of Visits* in columns (2) and (4) is zero when *Visit* is zero and otherwise the number of times a firm searched that position in the current quarter. Variables marked with <sup>†</sup> are computed using data from the previous quarter. *Share  $|\% \Delta| > 10$*  is the share of employees in a firm-position whose base salary is more than 10% away from the median benchmark. *Share Employees Paid Above Benchmark* is the share of existing employees with salaries above the median benchmark.

Table E.2: Most Common Searched Position Titles as a Share of New Hires

ADP Lens	(1) Share Searched (%)	(2) No. Hires
Ophthalmic Technician	89.5	19
Legal Associate Specialist	82.4	34
Physiotherapist	57.9	19
Shipping Clerk	51.9	54
Mammographer	43.8	16
Dental Assistant	37.5	32
Human Resources Business Partner	36.4	11
Medical Clerk	35.0	20
Food and Beverage Manager	33.3	18
Facilities Maintenance Manager	33.3	27
Accounting Assistant	27.8	36
Hotel Night Auditor	25.0	32
Manufacturing Engineering Technician	24.3	37
Dish Washer/Plate Collector/Table Top Cleaner	22.5	160
Production Operations Engineer	19.2	26
Hotel Cleaner	18.8	378
Other Housekeeper and Related Worker	18.5	168
Delivery Driver	17.6	17
Network, Servers and Computer Systems Administrator	17.4	23
Quality Control/Assurance Engineer	17.1	35
Building Caretaker/Watchman	17.1	152
Medical Laboratory Technician	16.7	12
Packing/Bottling/Labeling Machine Operator	15.6	32
Lawyer	15.4	13
Housekeeping and Cleaning Services Manager	14.3	14
Bank Teller	13.4	202
Registered Nurse	13.1	214
Loan Specialist	13.0	23
Data Entry Clerk	12.5	16
Heavy Truck Driver	11.8	17
Welder	10.8	83
Production Manager	10.5	19
Licensed Practical Nurse (LPN)	10.2	137
Purchasing Clerk	10.0	20
Benefits Specialist	9.5	21

Notes: Tabulations across all new hires at treatment firms after onboarding. *Share Searched* is the share of new hires in a given ADP lens searched by their firm before their hire date. Restricted only to ADP lenses with at least 10 new hires in the sample.

## F Additional Robustness Checks: Effects on Salary Compression

### F.1 Compression around other Benchmarks

In panel A of Figure 2, we present the compression of salaries around the median benchmark in searched positions both before and after onboarding. In Figure F.1, we replicate this figure using the percent difference from five other points of the benchmark distribution.

Panel A shows the change in percent difference from the 10th percentile benchmark before (gray bars) and after (red bars) onboarding among searched positions. Before onboarding, salaries were on average 33.8 pp from the 10th percentile benchmark. After gaining access, the average distance to the benchmark fell slightly to 29.8 pp ( $p\text{-value}=0.003$ ). Panel A shows this decrease is driven by a higher share of salaries 10-20% above the 10th percentile benchmark, rather than salaries at the 10th percentile benchmark itself. Panel B is analogous for the 25th percentile benchmark. It shows that among searched positions, the average distance to the benchmark dropped following onboarding from 24.8 pp to 19.8 pp ( $p\text{-value}<0.001$ ). Panel C is exactly panel A of Figure 2, showing percent difference from the median. Panel D looks at compression around the mean benchmark. The average distance to the mean benchmark drops from 21.5 to 17.2 percent after onboarding ( $p\text{-value}<0.001$ ), or a 20.0% reduction. While this drop is similar to the magnitude of the reduction in average distance to the median (21.6%), the histograms show that the percent of salaries within 2.5 pp of the median benchmark more than doubles while the percent of salaries within 2.5 pp of the mean benchmark stays constant. Panels E and F show the same results for the 75th and 90th percentiles, respectively. Average distance from the 75th percentile benchmark falls from 22.6 pp to 20.3 pp ( $p\text{-value}=0.010$ ), while average distance from the 90th percentile falls from 30.8 pp to 29.1 pp ( $p\text{-value}=0.033$ ).

### F.2 Effects on Composition of New Hires

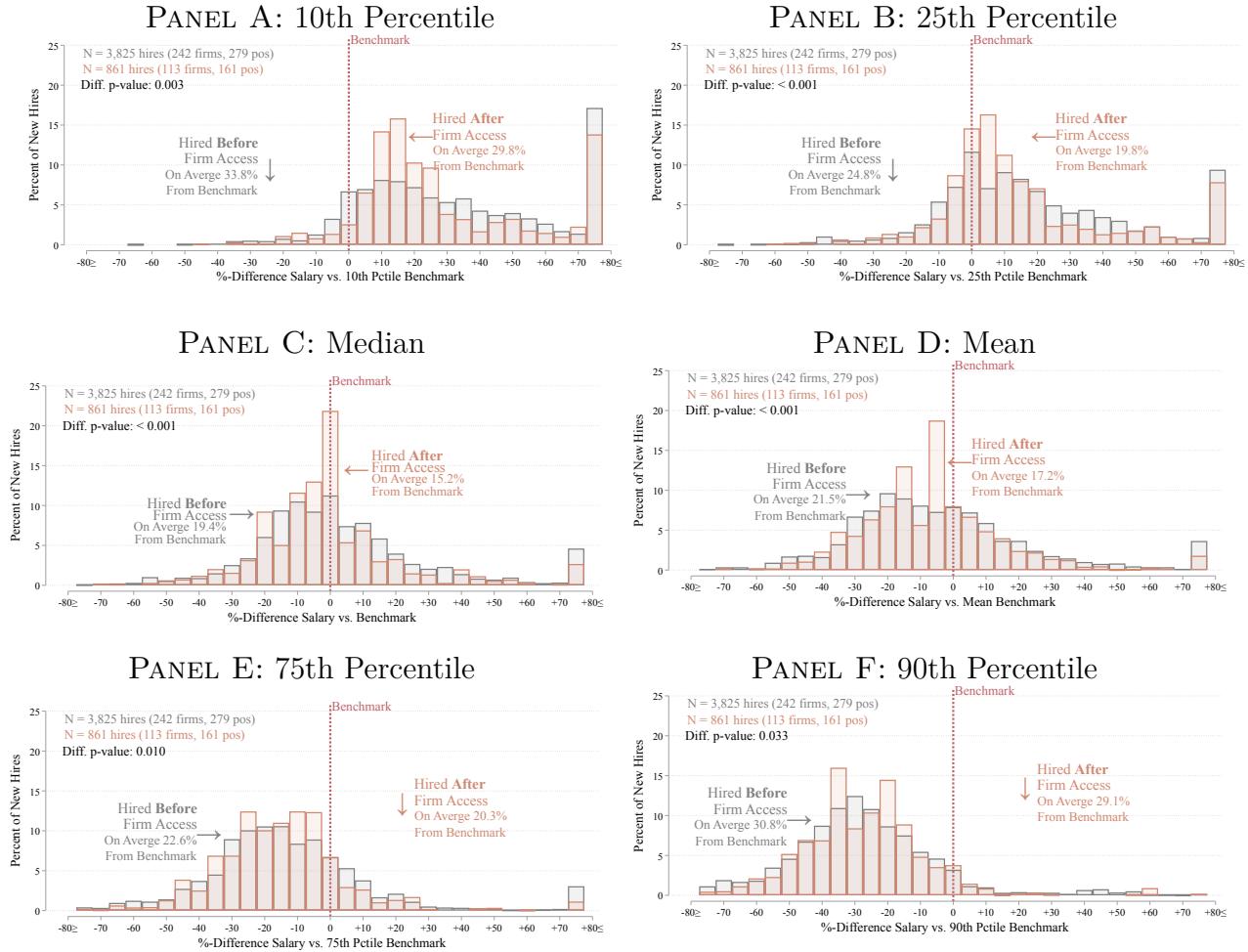
Table F.1 presents differences-in-differences estimates for effects on new hire composition. Column (1) uses gender an outcome, column (2) uses whether a new hire hourly vs. salaried as an outcome, and column (3) uses age an outcome.

### F.3 Other Robustness Checks

Table F.2 presents additional robustness checks for our differences-in-differences estimates for the effect on absolute dispersion from the benchmark, building on table 6. Column (1)

replicates column (1) of table 6 using the baseline specification. Columns (2) through (7) each change a different aspect of the baseline specification. In columns (2) and (3), we use alternative benchmark filters to compute dispersion from the benchmark. In our main specification we use benchmarks for a given ADP lens filtered by state and sector when that benchmark is based on more than 30 employees, and no filters otherwise. In column (2), we use only the unfiltered benchmark and in column (3) we use the filtered benchmark only when it based on more than 100 employees and no filters otherwise. In column (4), we include job titles with low “match scores” that are filtered out of the baseline specification. The “match score” reflects the quality of the mapping between a firm-specific job title and an ADP lens. Column (5) uses only non-searched and non-searchable new hires after September 2019, the start of our search data. Column (6) includes data from August 2020 through July 2021 (which uses the new ADP lens taxonomy starting in September 2020 discussed in Appendix D.1). Finally, column (7) excludes all human resources positions. Intuitively, HR employees may be looking their own salaries up due to curiosity, or for their own salary planning, rather than to negotiate with the new HR hires. These positions are only 2.28% of the sample.

Figure F.1: The Effects of the Compensation Benchmark Across the Distribution: Non-Parametric Analysis



Notes: Panel C is a reproduction of Panel A of Figure 2. All other panels are identical, but using absolute dispersion from the specified percentile of the benchmark distribution rather than the median. For more details, see notes to Figure 2.

Table F.1: The Effects of Benchmarking on Employee Composition

	(1) Female	(2) Hourly	(3) Age
Panel (a): Post-treatment			
Searched vs. Non-Searched	-2.264 (2.530)	0.494 (1.663)	-0.991 (0.760)
Searched vs. Non-Searchable	-2.689 (2.362)	3.239* (1.677)	-0.966 (0.685)
Panel (b): Pre-treatment			
Searched vs. Non-Searched	-0.411 (2.244)	1.640 (1.683)	-0.775 (0.618)
Searched vs. Non-Searchable	0.314 (1.941)	3.186** (1.558)	0.148 (0.544)
Mean Dep. Var. (Baseline)	54.089	76.127	34.637
Observations			
Searched	4,671	4,671	4,671
Non-Searched	35,894	35,894	35,894
Non-Searchable	162,291	162,291	162,291

Notes: Significant at \*10%, \*\*5%, \*\*\*1%. Robust standard errors in parentheses. All columns follow the specification of column (1) from Table 6, with the exception that here we exclude the additional controls. The dependent variables are a dummy equal to 100 if a new hire is *Female* and zero otherwise in column (1), a dummy equal to 100 if a new hire is an *Hourly* worker and zero otherwise in column (2), and a new hire's *Age* in column (3).

Table F.2: Additional Robustness Checks: The Effects of Benchmarking on Absolute %-Distance from the Benchmark

	(1)  %Δ	(2)  %Δ	(3)  %Δ	(4)  %Δ	(5)  %Δ	(6)  %Δ	(7)  %Δ
Panel (a): Post-treatment							
Searched vs. Non-Searched	-4.250*** (1.135)	-6.127*** (1.824)	-4.647*** (1.103)	-3.901*** (1.124)	-3.282*** (1.204)	-4.345*** (1.079)	-4.517*** (1.143)
Searched vs. Non-Searchable	-4.774*** (1.147)	-5.954*** (1.435)	-5.137*** (1.132)	-4.439*** (1.127)	-4.606*** (1.132)	-4.646*** (1.055)	-5.160*** (1.172)
Panel (b): Pre-treatment							
Searched vs. Non-Searched	-0.134 (1.142)	-0.398 (1.626)	-0.445 (1.149)	0.305 (1.192)	0.0872 (1.114)	-0.161 (1.144)	-0.379 (1.169)
Searched vs. Non-Searchable	-0.602 (1.028)	-0.821 (1.342)	-0.941 (1.018)	0.189 (1.152)	-0.293 (0.999)	-0.600 (1.029)	-0.853 (1.054)
No Filters	✓						
Filtered Benchmark $\geq 100$		✓					
Include Match Outliers			✓				
Restricted Sample				✓			
After Aug-2020					✓		
Exclude HR Positions						✓	
Mean Dep. Var. (Baseline)	19.442	23.554	19.771	20.542	19.442	19.442	19.395
Observations							
Searched	4,671	4,671	4,671	5,578	4,664	4,804	4,505
Non-Searched	35,894	35,894	35,894	46,605	14,449	42,148	34,632
Non-Searchable	162,291	162,291	162,291	199,441	87,029	165,679	157,758

Notes: Column (1) follows the specification of column (1) from Table 6. Column (2) uses the same specification as column (1), except using absolute dispersion from the unfiltered median benchmark, as opposed to using the state and sector filtered benchmark when available, as the outcome. Column (3) uses only filtered benchmarks computed using 100 or more employees, as opposed to the baseline threshold of 30 employees, and unfiltered benchmarks otherwise. Column (4) drops new hires who's organization specific job title has a low match score to the designated ADP lens (scores less than the 20th percentile of scores in that quarter). Column (5) using the *Restricted Sample* uses only control observations after September 2019, the start of our search data. Column (6) adds data from Aug-2020 to July-2021 to the sample. Column (7) excludes new hires in human resources positions. See Table 6 for more details.

## G Additional Robustness Checks: Effects on Salary Levels

### G.1 Main Robustness Checks

The differences-in-differences estimates are presented in Table G.1. The post-treatment coefficients ( $\alpha_1^k$ , from equation (16)) are presented in Panel A. Column (1) of Table G.1 corresponds to the baseline specification. The post-treatment coefficients are positive: 0.013 log points (p-value=0.446) when using Non-Searched as a control group and 0.022 log points (p-value=0.166) when using Non-Searchable as control group.

Columns (2) through (11) of Table G.1 are identical to column (1), except that they change a different feature of the baseline specification. In column (2) we use salary as dependent variable: i.e., in \$s, without the log transformation. The results from column (2) are qualitatively consistent with the results from column (1): the post-treatment coefficients are modest (-\$372.24 and \$596.97 for the comparison to Non-Searched and Non-Searchable, respectively) and statistically insignificant (p-values of 0.677 and 0.546). The results are consistent in magnitude too. For example, the first post-treatment coefficient from column (1) suggests a -0.8% ( $= \frac{-\$372.24}{\$44,432.59}$ ) increase in average salary relative to the baseline, while the corresponding coefficient from column (2) suggests an increase of 1.3% ( $= \frac{\$596.97}{\$44,432.59}$ ).

The specification from column (3) is identical to the baseline specification from column (1), except that the dependent variable is winsorized at 10% and 90% of the benchmark instead of the 2.5 and 97.5 percentile by ADP lens. Column (4) is identical to column (1), except that the standard errors are not clustered. Column (5) is identical to column (1), except that it does not include any of the additional control variables. Column (6) is identical to column (1), except that it adds position fixed effects. Column (7) is identical to column (1), except that it adds firm fixed effects. Column (8) 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 (9) is identical to column (1), except that it restricts to the 317 positions that are searched at least once in the sample. Column (10) is identical to column (1), except that it does not re-weight by SOC groups. Last, column (11) 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). Panel B of Table G.1 presents the corresponding “pre-treatment” coefficients. As expected, with few exceptions, these coefficients are close to zero, statistically insignificant and precisely estimated.

## G.2 Additional Robustness Checks

In Table 6, we show that the effects on salary compression are robust to a wide range of alternative specifications. In this Appendix, we show that the effects on salary levels are also robust to this same range of alternative specifications. The results are presented in Table G.2. Columns (1)–(5) measure the effect on the log of average base salary, with column (1) identical to column (1) of table G.1. Columns (2)–(5) each change one feature of the baseline specification. In column (2), we include *Match Outliers* (positions with low match scores, indicating low match quality between the firm-specific job title and the ADP Lens) that are excluded from the main analysis. In column (3), we restrict the same to only include non-searched and non-searchable positions that are after September 2019, the start of our search data. In column (4), we include new hires from August 2020 through July 2021. In column (5), we exclude human resources positions, as HR professionals are the most common users of the benchmarking tool.

One potential concern is that firms may be reacting by changing beyond base salary, through bonuses, commissions or even hours worked. To address this concern, the last columns of Table G.2 measures the effects on average gross pay (instead of average base salary). In the first specification of column (6), we define the annual gross wage as the average monthly gross pay during the first three months working at the firm, then multiplied by 12 to transform it to an annual basis (i.e., so that it is comparable to the base salary outcome).<sup>43</sup> The base salary and the gross compensation are highly correlated, but not perfectly so (correlation coefficient of 0.852, p-value<0.001). One minor shortcoming with the gross pay data is that it is missing for 5.3% of the observations, for a variety of reasons.<sup>44</sup> In any case, as shown in column (9) of table G.2, the results are also similar if we impute these missing values.

The results using the (log) gross pay outcome are presented in columns (6)–(9) of table G.2. First of all, notice that the coefficients for gross pay are much less precisely estimated than the corresponding coefficients for base salary. For example, the standard errors of the post-treatment coefficients for gross pay (0.049 and 0.050, from column (6)) are 2 times as large as the corresponding coefficients for base salary (0.031 and 0.023, from column (1)). This should be expected: relative to the base salary outcome, the gross pay outcome is more volatile because it includes a myriad of factors such as differences in tax withholdings, commissions, bonuses, withholdings and hours worked. The point coefficients for the gross

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<sup>43</sup>We compute the average starting on the 1st day of the month following the hire date, to make it more comparable across different employees. For employees who work fewer than three months at the firm, the average will be based on the one or two months they worked at the firm.

<sup>44</sup>For example, the payroll data is not available for 7 firms, and for other firms it is missing for some employees for a variety of reasons such as failure of data entry from the manager.

pay outcome (0.022 and 0.055, from column (6)) are very similar in magnitude to the corresponding coefficients for base salary (0.027 and 0.045, from column (1)), and statistically indistinguishable from each other. However, due to the larger standard errors, the results are less statistically significant for the gross pay outcome (p-values of 0.782 and 0.208, from column (6)) than for the base salary (p-values of 0.446 and 0.166, from column (1)).

### G.3 Effects on Vacancy Fill Rate

In Figure G.1, we apply our event study framework to examine how the number of new hires by a firm, in a given position, evolves after the firm gains access to the salary benchmark. Panel C of figure G.1 presents the difference between the Searched and Non-Searched estimates from Panel A. This analysis suggests a small positive but insignificant effect of tool access on the number of new hires. Specifically, the average  $\log(\text{Hires})$  rose 4.2% (p-value = 0.528). Panel D presents the same difference for searched and non-searchable positions. After onboarding, this probability rose 6.3% (p-value=0.215).

In Figure G.2 we split our sample by Low and High Skill groups to examine evidence of heterogeneous responses to salary benchmarking. While we are underpowered to detect differences, we note that the point estimates rise from 0.234 monthly new hires on average to 0.275 across both Searched vs. Non-Searched and Searched vs. Non-Searchable among the Low Skill positions (p-values 0.277 and 0.484 respectively), while the new hire count remains virtually unchanged (staying with 0.004) among High Skilled positions. However we cannot rule out swings in either direction of 0.10 new hires, hence we remain largely agnostic about the heterogeneous effects on new hires.

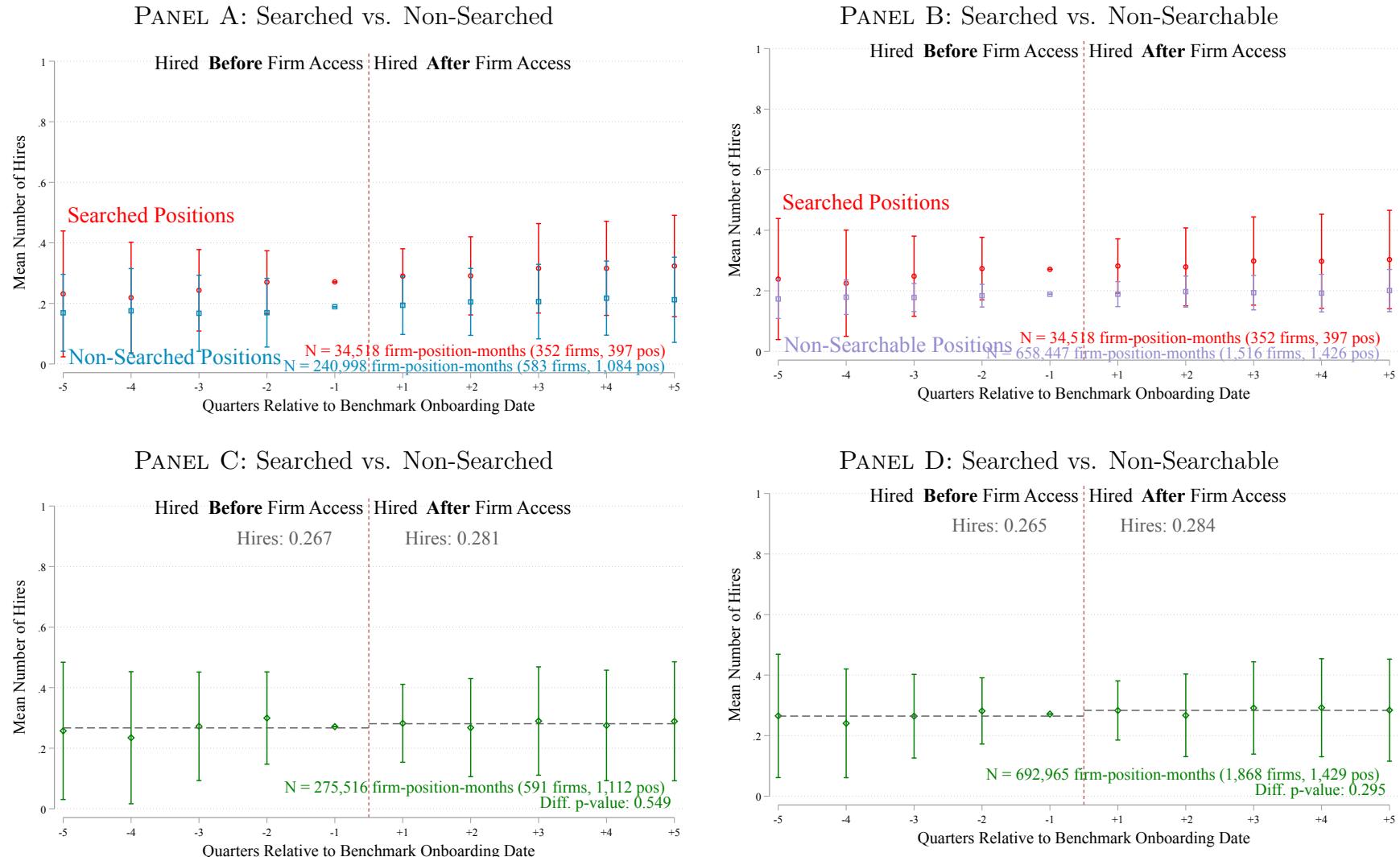
### G.4 Effects on Retention Levels

In Figure G.3, we apply our event study framework to a retention outcome. Specifically, we look at the whether an employee hired in a given month is still employed at the same firm one year later. Panel C of figure G.3 presents the difference between the searched and non-searched estimates in Panel A. This analysis suggests an insignificant effect of tool access on the probability an employee is still at the same firm 1 year after their hire date. Specifically, this probability rose from 53.7% to 57.1% after access (p-value=0.216). Panel D presents the same difference for searched and non-searchable positions. After onboarding, this probability rose from 53.7% to 57.8% (p-value=0.115).

Some observations may be somewhat affected by the COVID pandemic. Since our sample includes employees hired up to March 2020, for the employees hired most recently (between March 2019 and March 2020) their 12-month horizon of retention will partially overlap with

the pandemic period (after April 2020). It is not obvious, however, whether including the pandemic should make the effects stronger or weaker.

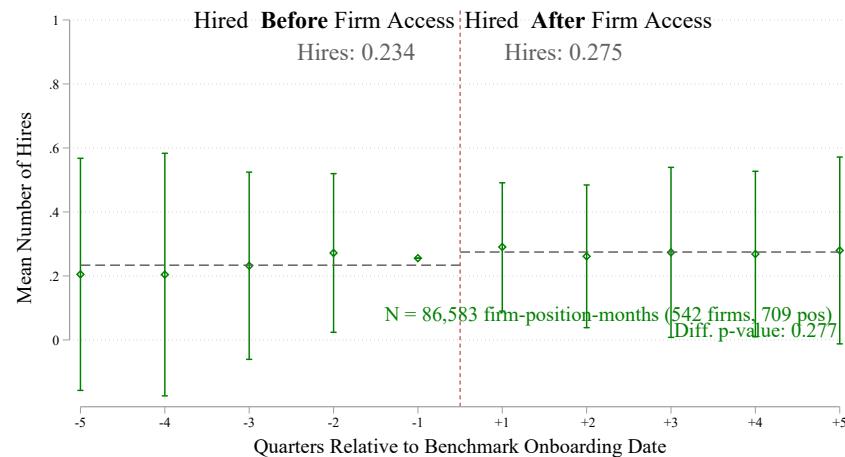
Figure G.1: Vacancy Fill Rate: Event-Study Analysis



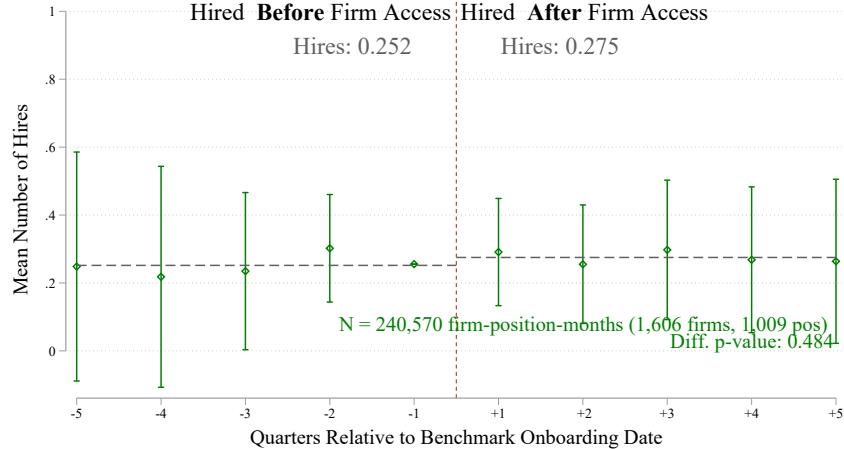
Notes: Event study coefficients from a Poisson regression at the firm-position-month level. Data includes an observation for every month from Jan-2017 to Mar-2020 for each firm-position that has at least one new hire in our sample. The outcome is the number of new hires in a month in a given firm-position (winsorized at the non-zero 99th percentile). Positions are classified as “Searched” if they are searched anytime in the post-onboarding period. The specification includes firm-position clustering, SOC group pweights, standard controls aggregated at the firm-position-month level, and 10 dummies for the quintiles of the mean number of new hires in the firm-position in the past 12 months.

Figure G.2: Heterogeneity: Event-Study Analysis

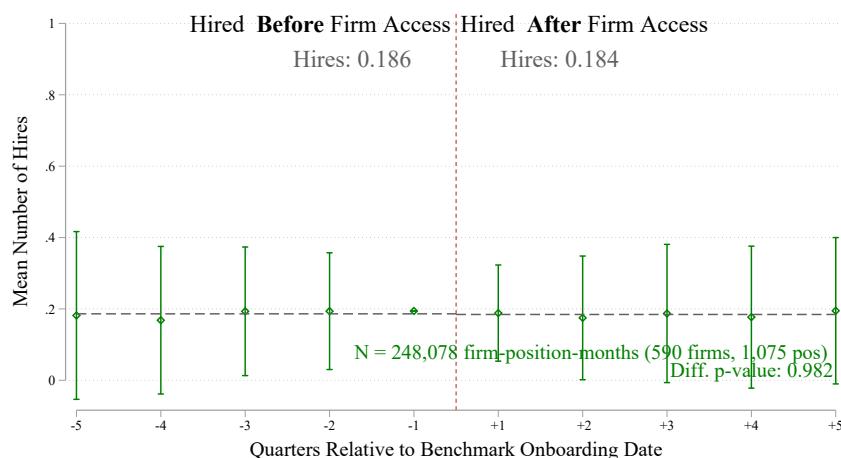
PANEL A: Low Skill: Searched vs. Non-Searched



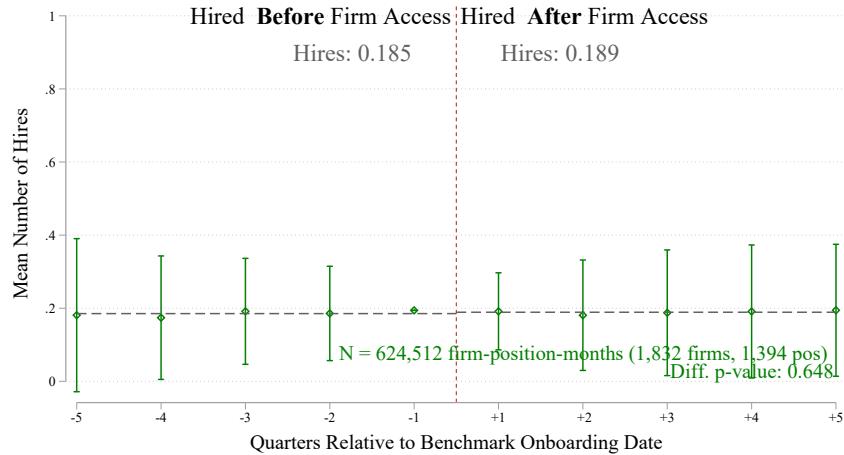
PANEL B: Low Skill: Searched vs. Non-Searchable



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

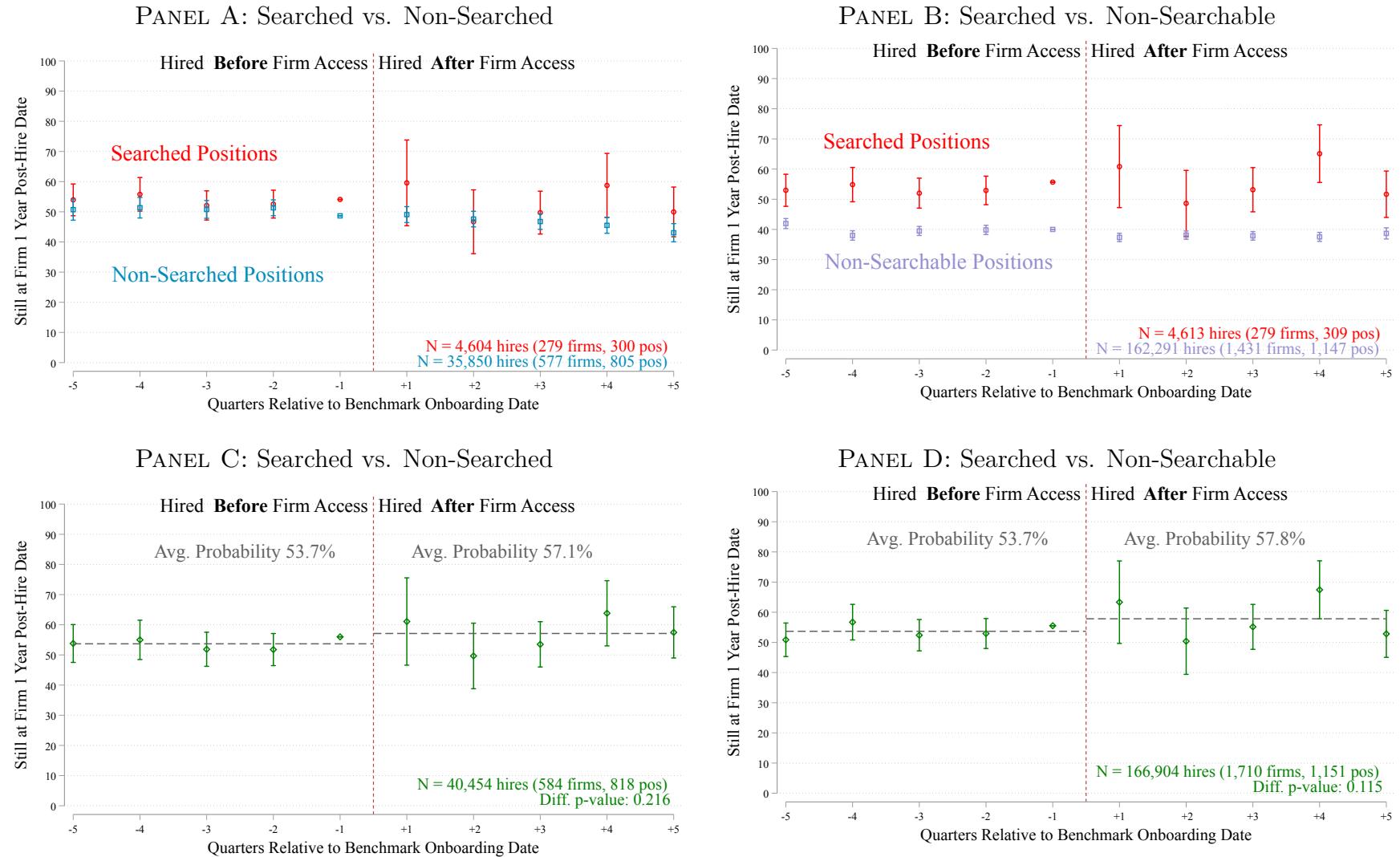


PANEL D: High Skill: Searched vs. Non-Searchable



Notes: This is a reproduction of Figure G.1 Panel C and D, split by *Skill*. See the notes of Figure G.1 for more details. *Skill* is defined in Section 3.1

Figure G.3: Retention: Event-Study Analysis



Notes: This is a reproduction of Figure 5, 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 5.

Table G.1: The Effects of Benchmarking on Salary Levels

	(1) $\log(\text{Salary})$	(2) Salary	(3) $\log(\text{Salary})$	(4) $\log(\text{Salary})$	(5) $\log(\text{Salary})$	(6) $\log(\text{Salary})$	(7) $\log(\text{Salary})$	(8) $\log(\text{Salary})$	(9) $\log(\text{Salary})$	(10) $\log(\text{Salary})$	(11) $\log(\text{Salary})$
<b>Panel (a): Post-treatment</b>											
Searched vs. Non-Searched	0.013 (0.017)	-372.235 (893.417)	0.014 (0.017)	0.013 (0.012)	0.011 (0.019)	0.005 (0.032)	0.021 (0.019)	0.009 (0.017)	0.014 (0.017)	0.017 (0.018)	0.005 (0.017)
Searched vs. Non-Searchable	0.022 (0.016)	596.967 (987.713)	0.024 (0.016)	0.022* (0.012)	0.011 (0.018)	0.003 (0.026)	0.040* (0.021)	0.019 (0.016)	0.023 (0.016)	0.026 (0.018)	0.014 (0.016)
<b>Panel (b): Pre-treatment</b>											
Searched vs. Non-Searched	-0.023 (0.018)	-1236.888 (911.533)	-0.022 (0.018)	-0.023* (0.012)	-0.031 (0.019)	-0.031 (0.038)	0.001 (0.015)	-0.021 (0.018)	-0.021 (0.018)	-0.020 (0.019)	-0.024 (0.017)
Searched vs. Non-Searchable	-0.003 (0.017)	-435.633 (848.090)	-0.001 (0.017)	-0.003 (0.011)	-0.013 (0.019)	0.005 (0.029)	0.016 (0.013)	-0.008 (0.016)	-0.001 (0.017)	-0.001 (0.018)	-0.001 (0.017)
Alternate Winsorization			✓								
No Clustering				✓							
No Additional Controls					✓						
No Position FE						✓					
Firm FE							✓				
Exclude High-Tip Jobs								✓			
Searched Lenses Only									✓		
No Re-weighting										✓	
Ages 21-60											✓
Mean Dep. Var. (Baseline)	10.538	44432.586	10.531	10.538	10.538	10.538	10.550	10.538	10.515	10.570	
Observations											
Searched	4,671	4,671	4,671	4,671	4,671	4,686	4,683	4,539	4,671	4,837	4,125
Non-Searched	35,894	35,894	35,894	35,894	35,894	36,049	36,036	34,328	31,892	35,994	31,168
Non-Searchable	162,291	162,291	162,291	162,291	162,291	162,450	162,398	155,222	135,861	162,608	138,633

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  $\alpha_1^k$  from equation (16), while pre-treatment coefficients in panel (b) refer to parameters  $\alpha_3^k$  from equation (17) (see Section 4.2 for details). All columns include year fixed effects. In columns (1) and (3)–(11) the dependent variable is the log of annual base salary. The dependent variable in col (2) is the annual base salary (in \$s). Log salary and salary are winsorized to the 2.5 and 97.5 percentiles of all salaries for their position. The exception is column (3) where wages are winsorized to  $\pm 90\%$  of the median benchmark. All columns except (5) include additional controls (female dummy, high education dummy, hourly dummy, age, position tenure). Column (8) excludes the three positions where gross pay most exceeds base pay: Waiter/Waitress, Chauffeur, and Bartender/Mixologist. Column (9) restricts the sample to only lenses of non-searched or non-searchable new hires in positions that are searched and hired by firms in the data.

Table G.2: Additional Robustness Checks: The Effects of Benchmarking on Salary Levels

	(1) $\log(\text{Salary})$	(2) $\log(\text{Salary})$	(3) $\log(\text{Salary})$	(4) $\log(\text{Salary})$	(5) $\log(\text{Salary})$	(6) $\log(\text{Gross})$	(7) $\log(\text{Gross})$	(8) $\log(\text{Gross})$	(9) $\log(\text{Gross})$
<b>Panel (a): Post-treatment</b>									
Searched vs. Non-Searched	0.0130 (0.017)	0.0117 (0.016)	0.00648 (0.019)	0.0124 (0.016)	0.0116 (0.018)	0.0114 (0.042)	0.00816 (0.043)	0.00236 (0.040)	0.00587 (0.041)
Searched vs. Non-Searchable	0.0220 (0.016)	0.0153 (0.016)	0.00754 (0.017)	0.0158 (0.015)	0.0218 (0.017)	0.0486 (0.039)	0.0440 (0.040)	0.0356 (0.038)	0.0399 (0.038)
<b>Panel (b): Pre-treatment</b>									
Searched vs. Non-Searched	-0.0228 (0.018)	-0.0379** (0.018)	-0.0156 (0.017)	-0.0229 (0.018)	-0.0182 (0.018)	0.0251 (0.035)	0.0330 (0.035)	0.0320 (0.035)	0.0231 (0.034)
Searched vs. Non-Searchable	-0.00252 (0.017)	-0.00784 (0.018)	-0.0153 (0.016)	-0.00290 (0.017)	0.00237 (0.017)	0.0273 (0.033)	0.0209 (0.033)	0.0324 (0.033)	0.0204 (0.032)
Include Match Outliers		✓							
Restricted Sample			✓						
After Aug-2020				✓					
Exclude HR Positions					✓				
3 Month Window						✓			
2 Month Window							✓		
6 Month Window								✓	
Imputed									✓
Mean Dep. Var. (Baseline)	10.538	10.541	10.538	10.538	10.511	10.385	10.385	10.397	10.385
Observations									
Searched	4,671	5,578	4,664	4,804	4,505	4,599	4,594	4,604	4,671
Non-Searched	35,894	46,605	14,449	42,148	34,632	34,962	34,923	34,993	35,894
Non-Searchable	162,291	199,441	87,029	165,679	157,758	152,487	152,288	152,715	162,291

Notes: Columns (1)–(5) look at effects on the log of annual base salary. Column (1) is exactly column (1) from Table G.1. Column (2), *Include Match Outliers*, reproduces column (1), but including new hires whose organization specific job title has a low match score to the designated ADP lens (scores less than the 20th percentile of the scores in that quarter). Column (3) using the *Restricted Sample* uses only control observations after September 2019, the start of our search data. Column (4) includes data from August 2020 through July 2021. Column (5) excludes new hires in human resources positions. Columns (6)–(9) look at effects of the log of annual gross wages (as described in Section G.2). Column (6) uses a 3 month window to compute gross wages, while columns (7) and (8) use a 2 and 6 month window, respectively. Column (9) is equivalent to column (6) with missing values imputed. See Table G.1 for more details.

## H Additional Heterogeneity Analysis: Effects on Salary Compression

### H.1 Further Results about Heterogeneity by Skill Level

Figure H.1 presents the event study results for low and high skill positions. These results indicate that there is more salary compression among low skill positions than among high skill positions. As described in section 4.5, we use the O\*NET Job Zones to classify positions that require low-skill versus those that require more education.

The panels in the top row of Figure H.1 (i.e., panels A and B) correspond to the low-skill positions, while panels on the left hand side (i.e., panels C and D) correspond to high-skill positions. Before firms had access to the tool (i.e., the left side of each panel), there was more compression among searched low-skill positions (14.8%) than among the searched high-education positions (21.9%).

The differences-in-differences comparison for searched vs. non-searched low skill positions (Panel A) suggest the benchmark tool reduced the salary dispersion from 14.8 pp to 7.9 pp (p-value<.001), equivalent to a 46.6% reduction. For searched vs. non-searchable low education positions (Panel B), compression drops from 14.8 pp to 8.5 pp (p-value<.001), or a 42.5% reduction.

For high skill positions, the differences-in-differences comparison for searched vs. non-searched (Panel C) suggests that the benchmark tool reduced the salary dispersion from 21.9 pp to 19.3 pp (p-value = 0.068), equivalent to a 11.8% reduction. Finally comparing searched vs. non-searchable high skill positions (Panel D) suggests a drop in compression from 21.9 pp to 18.6 pp (p-value = 0.012), equivalent to a 15.0% reduction.

### H.2 Comparison to Other Forms of Heterogeneity

We test 10 different forms of heterogeneity. First, we compare low skill and high skill positions described in Section 3.4. Second, we compare low and high education, corresponding to Job Zones 1 and 2 (low) and 3-5 (high) mapped by ONET and linked through the ONET codes in our administrative data. Third, we compare new hires less than or equal to 34 years old and those above 34 years old at the time of hire. Fourth, we compare positions with a mean salary of above or below \$34,000. Fifth, we compare positions in markets with low and high market concentration. We use data from (Azar et al., 2020), which is at the 6-digit SOC group and commuting zone level. We collapse this to 3-digit SOC groups and the most common state in each commuting zone to merge onto our data at the SOC group-state level.<sup>45</sup> We split at the

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<sup>45</sup>We impute HHI for the 1.7% of observations where it is missing using the 3-digit SOC group median.

median HHI in our data, 4500. Sixth, we compare high-turnover and low-turnover positions. We compute the average turnover for each ADP lens at control firms in our data, and split at the new-hire weighted median (26 months). Seventh, we compare blue and white collar jobs, which like skill, are also classified using the first digit of ISCO codes. Eighth, we compare female and male new hires.<sup>46</sup> Ninth, we compare hourly and salaried new hires. Tenth, we compare positions with less stable benchmarks with those with more stable benchmarks. We split based on the median coefficient of variation for each position's benchmarks over time (0.029).

Figure H.2 depicts the correlation between all 10 of heterogeneity splits. Our main heterogeneity variable, low-skill, shares a particularly high correlation with low education and low average annual base salary (\$34,000) (correlation 0.74 and 0.82), and only a modest correlation with the age of the employee, 0.018, categorized as less than or greater than 35. This is largely by construction, as low skill is defined as having low education, below-34k income and below-35 age. Low Skill has a correlation of 0.49 with having hourly pay, which is itself highly correlated with annual base salary (correlation 0.57). All other variables have correlations less than 0.50 and the majority have correlations less than 0.20.

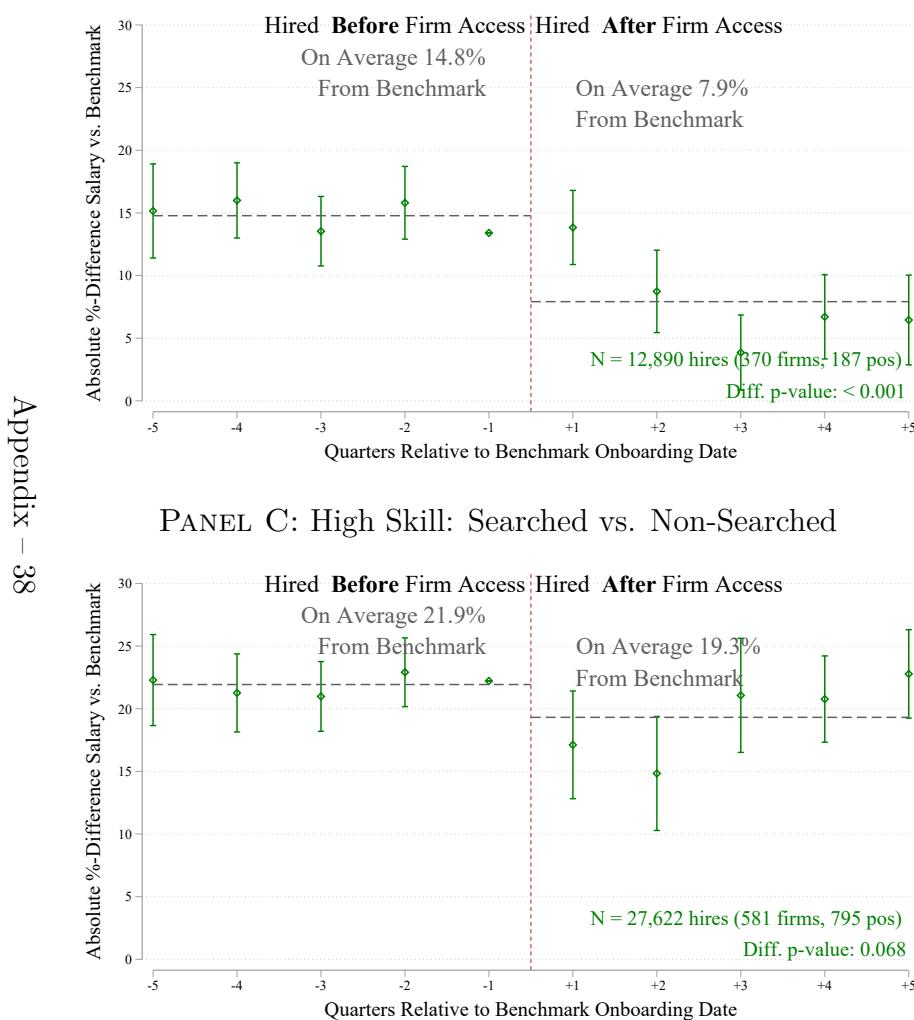
Table H.1 presents the results corresponding to panel (a) in Table 6 for each group. The table also displays the baseline compression for each group, the number of observations classified as a given type in each split, and the p-values of the difference between the coefficients for two compared groups. This table shows that the differences in the effects of salary benchmarking are large and statistically significant for each of the comparisons between Searched vs. Non-Searched and Searched vs. Non-Searchable when it comes to Low Skill and Low Education (Col. 1 and 2). The differences are consistent in magnitude and marginally statistically significant for Low Salary positions (Col. 3), and similarly consistent in magnitude though less precise when it comes to Low Age positions (Col. 4). By contrast, there is minimal heterogeneity across other position characteristics.

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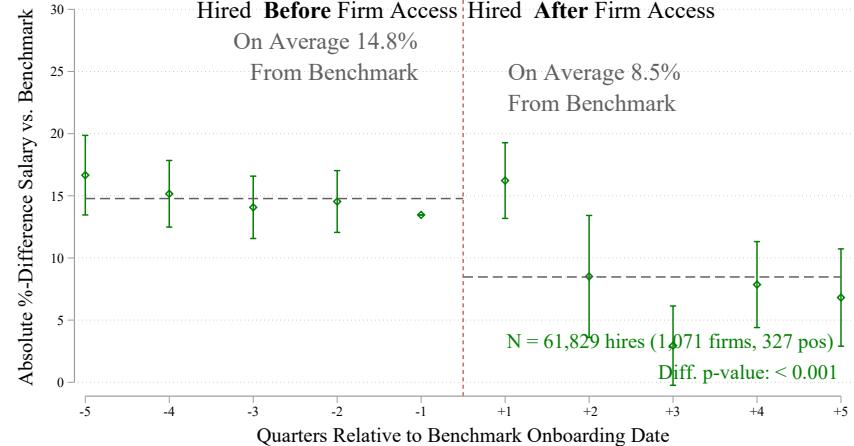
<sup>46</sup>We impute age for .1% of observations based on the median age in a new hire's ADP lens.

Figure H.1: Heterogeneity: Event-Study Analysis

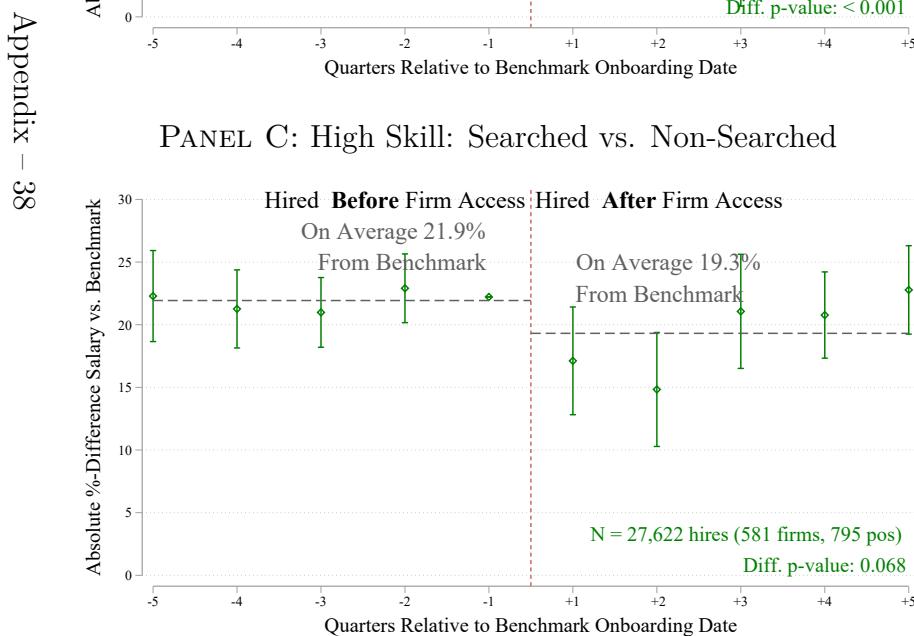
PANEL A: Low Skill: Searched vs. Non-Searched



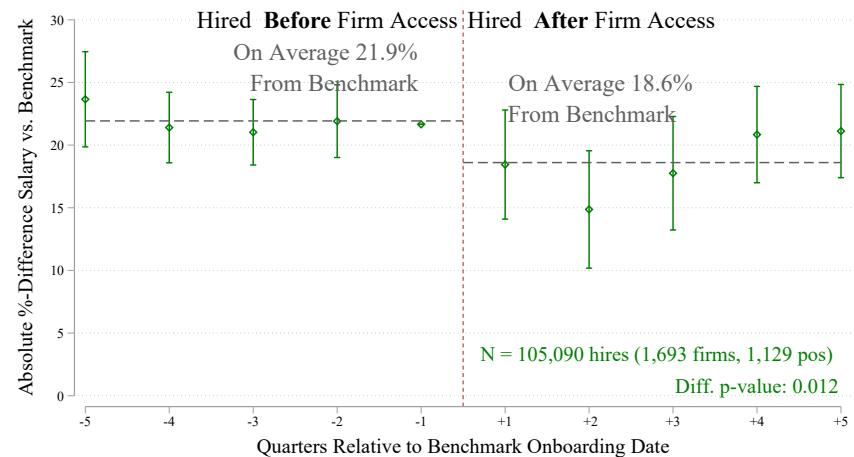
PANEL B: Low Skill: Searched vs. Non-Searchable



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



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



Notes: Panels A and C are a reproduction of panel C from Figure 3, and panels B and D are a reproduction of panel D, but for the specified sub-samples. *Skill* is defined in Section 3.1. See the notes of Figure 3 for more details.

Figure H.2: Correlations Among Heterogeneity Variables

Low Skill	0.74	0.18	0.82	0.12	0.45	0.27	0.01	0.49	0.12
Low Educ.		0.12	0.48	0.10	0.34	0.26	-0.06	0.36	0.15
Age <= 35			0.22	0.01	0.13	0.01	-0.00	0.15	0.01
<= \$34K				0.12	0.45	0.24	0.04	0.57	0.11
HHI > 4500					0.05	0.30	-0.07	0.12	0.02
High-Turn						0.15	-0.05	0.34	0.10
Blue-Collar							-0.25	0.25	-0.07
Female								0.07	-0.08
Hourly									0.06
	Low Educ.	Age <= 35	<= \$34K	HHI > 4500	High-Turn	Blue-Collar	Female	Hourly	Less Stable

Notes: Correlations between binary variables used in the heterogeneity analysis.  
Variables are as defined in Appendix H.2.

Table H.1: Heterogeneity Analysis: Effects of Benchmarking on Salary Compression

	Dep. Var.: $ \% \Delta $										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Searched vs. Non-Searched	Low-Skill -7.161*** (1.236)	Low Educ. -6.141*** (1.360)	$\leq \$34K$ -4.138*** (0.998)	Age $\leq 35$ -5.663*** (1.132)	Age $\leq 27$ -5.231*** (1.294)	HHI $\leq 4500$ -5.545*** (1.812)	High-Turn. -4.358** (1.763)	Blue-Collar -3.103** (1.441)	Female -4.126** (1.638)	Hourly -3.918*** (1.048)	Less Stable -4.664*** (1.597)
Searched vs. Non-Searchable	-7.325*** (1.396)	-7.475*** (1.377)	-5.507*** (1.153)	-5.986*** (1.149)	-5.102*** (1.340)	-5.590*** (1.528)	-4.363** (1.785)	-3.083** (1.449)	-5.394*** (1.564)	-4.425*** (1.123)	-7.442*** (1.932)
Mean Dep. Var. (Baseline)	10.152	10.373	10.170	10.395	10.271	10.618	10.240	10.333	10.483	10.332	10.481
Observations	72,673	140,102	129,888	117,073	69,722	101,526	92,795	50,272	103,004	166,108	80,844
	High-Skill	High Educ.	$> \$34K$	Age $> 35$	Age $> 27$	HHI $> 4500$	Low-Turn.	White-Collar	Male	Salaried	More Stable
Searched vs. Non-Searched	-2.175 (1.551)	1.519 (1.788)	-2.011 (2.174)	-1.458 (1.508)	-3.137** (1.286)	-1.968 (1.340)	-4.790*** (1.367)	-4.536*** (1.363)	-4.402*** (1.393)	0.017 (2.247)	-3.884** (1.526)
Searched vs. Non-Searchable	-2.872** (1.456)	1.391 (1.691)	-1.548 (1.812)	-2.237 (1.719)	-4.075*** (1.334)	-3.513** (1.420)	-5.130*** (1.359)	-4.997*** (1.326)	-3.988*** (1.504)	-0.402 (2.442)	-3.396** (1.374)
Mean Dep. Var. (Baseline)	21.944	22.499	24.913	22.236	21.020	17.278	20.907	20.579	19.899	28.026	18.956
Observations	130,090	62,699	72,696	85,407	132,795	101,039	110,067	152,593	99,496	36,471	122,021
Diff. p-value											
Searched vs. Non-Searched	0.007	0.002	0.753	0.019	0.236	0.084	0.847	0.470	0.615	0.319	0.724
Searched vs. Non-Searchable	0.021	< 0.001	0.722	0.161	0.552	0.195	0.732	0.330	0.621	0.788	0.088

Notes: Searched positions: N = 4,686, Non-searched positions: N = 36,049, Non-searchable positions: N = 162,450. Significant at \*10%, \*\*5%, \*\*\*1%. Robust standard errors in parentheses. Each column reports the the searched vs. non-searched and searched vs. non-searchable estimates for two groups. Estimates correspond to those in panel (a) in Table 6, but for the specified group. The dependent variable is the absolute percent difference between the annual base salary and median benchmark in all columns. Column (1) compares low skill and high skill positions. Column (2) compares low education and high education positions. Column (3) compares positions with mean salaries below \$34,000 and above \$34,000. Column (4) compares new hires with less than 35 years of age. (5) compares new hires with less than 27 years of age. (6) compares positions in markets with low and high market concentration, measured using HHI. Column (7) compares high-turnover and low-turnover positions. Column (8) compares blue and white collar positions. Column (9) compares female and male new hires. Column (10) compares hourly and salaried new hires. Column (11) compares positions with less stable benchmarks with those with more stable benchmarks. See Appendix H.2 for details on split classifications.

# I Additional Heterogeneity Analysis: Effects on Salary Levels

## I.1 Comparison to Other Forms of Heterogeneity

Table I.1 presents the results corresponding to panel (a) in Table G.1 for each group. The table also displays the log salary for each group, the number of observations classified as a given type in each split, and the p-values of the difference between the coefficients for two compared groups. The heterogeneity variables are defined in Appendix H.2.

First, 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 0.005 and 0.051 for comparisons of Searched vs. Non-Searched and Searched vs. Non-Searchable, respectively. The definition of skill combines information on the position averages by education (ONET Job Zones 1 and 2), age (less than 35), and salary (less than \$34,000). When comparing Searched vs. Non-Searched, point estimates for salary levels of low education positions are higher than high education positions, though the difference is statistically insignificant (p-value=0.737). Similarly point estimates for salary levels of younger workers is higher than older workers in Searched vs. Non-Searched positions (p-value = 0.219), and salaries for low paid positions are higher than highly paid positions (p-value = 0.062). Point estimates for Low Skill, Low Education, Low Age, and Low Salary are similar in magnitude, 0.053 (0.022), 0.020 (0.020), 0.020 (0.020) and 0.025 (0.019). Similar patterns can be found when comparing Searched vs. Non-Searchable. Point estimates are 0.058 (0.020), 0.030 (0.022), 0.023 (0.017), 0.058 (0.017) for Low Skill, Low Education, Low Age, and Low Salary respectively, while the High categories for each of these exhibit point estimates closer to zero or negative. We only have statistical power to distinguish Low from High when we look at the restricted Low-Skill category, and marginal statistical significance when we look at positions with annual base salaries below \$34,000.

Table I.1: Heterogeneity Analysis: Effects of Benchmarking on Average Salary

	Dep. Var.: $\log(\text{Salary})$										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Searched vs. Non-Searched	Low-Skill	Low Educ.	$\leq \$34K$	Age $\leq 35$	Age $\leq 27$	HHI $\leq 4500$	High-Turn.	Blue-Collar	Female	Hourly	Less Stable
	0.053** (0.022)	0.020 (0.020)	0.025 (0.019)	0.020 (0.020)	0.059*** (0.022)	0.047** (0.021)	-0.003 (0.034)	-0.029 (0.023)	0.014 (0.024)	0.031* (0.017)	-0.044 (0.034)
Searched vs. Non-Searchable	0.058*** (0.020)	0.030 (0.022)	0.058*** (0.017)	0.023 (0.017)	0.065*** (0.020)	0.001 (0.020)	-0.006 (0.028)	0.008 (0.031)	0.027 (0.020)	0.029* (0.016)	0.024 (0.033)
	Mean Dep. Var. (Baseline)	10.152	10.373	10.170	10.395	10.271	10.618	10.240	10.333	10.483	10.332
Observations		72,673	140,102	129,888	117,073	69,722	101,526	92,795	50,272	103,004	166,108
	High-Skill	High Educ.	$> \$34K$	Age $> 35$	Age $> 27$	HHI $> 4500$	Low-Turn.	White-Collar	Male	Salaried	More Stable
Searched vs. Non-Searched	-0.011 (0.021)	-0.011 (0.031)	-0.023 (0.024)	0.009 (0.021)	-0.009 (0.019)	0.000 (0.023)	0.019 (0.018)	0.024 (0.020)	0.004 (0.022)	-0.032 (0.032)	0.045** (0.017)
	Searched vs. Non-Searchable	0.005 (0.020)	0.011 (0.026)	-0.013 (0.021)	0.017 (0.025)	-0.003 (0.019)	0.053** (0.023)	0.055*** (0.019)	0.021 (0.018)	0.019 (0.023)	-0.012 (0.033)
Mean Dep. Var. (Baseline)		10.745	10.929	11.027	10.744	10.670	10.456	10.625	10.580	10.604	11.198
Observations		130,090	62,699	72,696	85,407	132,795	101,039	110,067	152,593	99,496	36,471
Diff. p-value											
Searched vs. Non-Searched	0.005	0.737	0.062	0.219	0.007	0.137	0.577	0.086	0.764	0.143	0.020
Searched vs. Non-Searchable	0.051	0.609	0.018	0.685	0.006	0.059	0.071	0.729	0.755	0.241	0.830

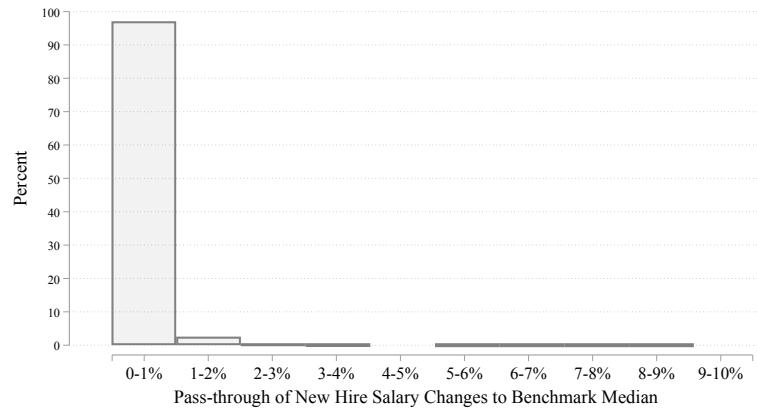
Notes: Searched positions: N = 4,686, Non-searched positions: N = 36,049, Non-searchable positions: N = 162,450. Significant at \*10%, \*\*5%, \*\*\*1%. Robust standard errors in parentheses. Each column reports the the searched vs. non-searched and searched vs. non-searchable estimates for two groups. Estimates correspond to those in panel (a) in Figure G.1, but for the specified group. The dependent variable is the log of annual base salary in all columns. Column (1) compares low skill and high skill positions. Column (2) compares low education and high education positions. Column (3) compares positions with mean salaries below \$34,000 and above \$34,000. Column (4) compares new hires with less than 35 years of age. (5) compares new hires with less than 27 years of age. (6) compares positions in markets with low and high market concentration, measured using HHI. Column (7) compares high-turnover and low-turnover positions. Column (8) compares blue and white collar positions. Column (9) compares female and male new hires. Column (10) compares hourly and salaried new hires. Column (11) compares positions with less stable benchmarks with those with more stable benchmarks. See Appendix H.2 for details on split classifications.

## J Pass-through from Salaries to Benchmark

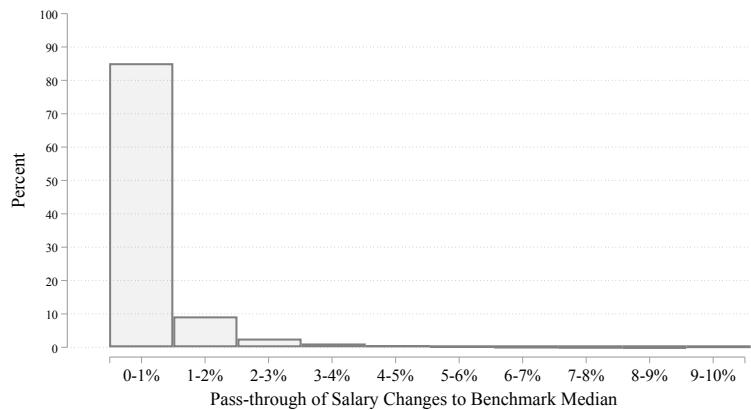
One potential reason that firms might hesitate to raise the salaries of employees after learning about the existence of a high quality benchmark, comprised of their own employee data, could be a concern that their own data would influence the benchmark itself and sharpen competition. To investigate whether some firms hire a sufficiently large share of the employees in a labor market to merit this consideration, we carry out an exercise to assess the extent of “pass-through” from the salaries of new hires (and existing hires) to the median of the benchmark. In Figure J.1, we simulate how much the median of the salary benchmark for a position-industry-state would shift in the following quarter, when the benchmark is recalculated, if a firm decided to raise the salaries of all new hires (panel A) and all existing employees (panel B). The result is very stark, shifting the salaries of all new hires by 10% would shift the benchmark median by 0.23% on average, and only 3% of firm-positions could shift the median by more than 1%. Even if a firm were to raise the salaries of all its employees by 10%, the median of the benchmark would only shift on average 0.59%.

Figure J.1: Salary Pass-through to Benchmark

PANEL A: New Hires



PANEL B: All Employees



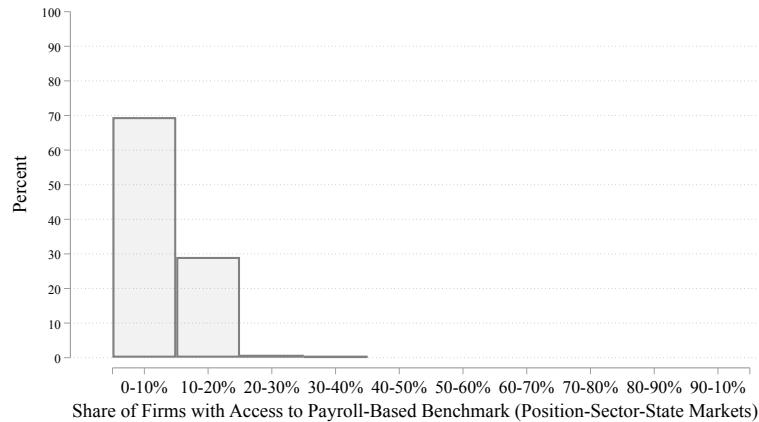
Notes: Distribution of the rate of pass-through for position-state-sector-firms with access to the benchmark. Panel A reflects the pass-through of raising all new hire salaries 10%, while Panel B shows the same but for all employees, not just new hires. Mean (median) pass-through for new hires is 0.23 (0.12) and for all employees is 0.59 (0.25).

## K Market Access to the Salary Benchmark over Time

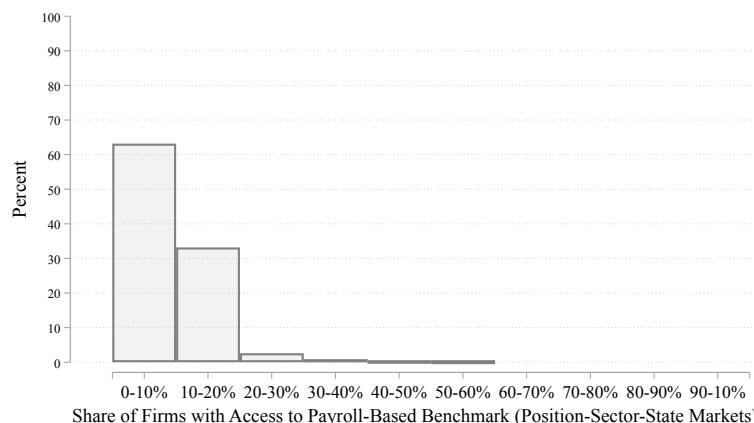
While we cannot ask firms directly their perception about how many other firms in a particular labor market can access the proprietary benchmark, we can look in the data to see how the number of firms with access compares to the number of firms used to create the benchmark (the full set of payroll clients). For each position title X industry X state labor market, we calculate the share of firms in the labor market that have access, among all the payroll clients in that same labor market. In Figure K.1 we display this distribution across all labor markets at two points in time. At the start of our panel, 2017 Q1, on average 5.9% of firms had access, and this grew to 7.5% of firms by 2020 Q1, the last quarter for which we collect firm access data.

Figure K.1: Market Access to the Salary Benchmark

PANEL A: 2017Q1



PANEL B: 2020Q1



Notes: Distribution of market access to the proprietary salary benchmark of position-state-sector benchmarks, defined as the share of firms with access by a given date over the share of firms whose data contribute to the benchmark. For 2017Q1 (panel A) mean (median) market access is 5.9 (7.4). For 2020Q1 (panel B), mean (median) market access is 7.5 (9.0)

## L SHRM Survey

A sample of the full survey instrument is attached as Appendix N.

Table L.1: Characteristics of the sample.

Questions	Share of responses (%)
How many years of experience do you have setting salaries?	
Less than 1 year	2.44
1-5 years	34.30
6-10 years	25.70
11+ years	37.56
How many employees does your company have?	
1-49	22.15
50-99	23.48
100-999	36.67
1000-4999	9.70
5000 or more	8.00
Are you working in the private sector or the public sector?	
Private sector	80.81
Public sector	19.19
Do you participate in salary settings for:	
New hires	6.44
Current employees	2.52
Both	91.04
Do you typically set salaries for:	
Higher-education positions	13.93
Lower-education positions	6.07
Both	80.00
Do you have access to the median salary that your company pays employees in a specific position?	
No, I could not access that data even if I wanted to	0.96
Yes, I can access it easily	79.78
Yes, but it would take some work	19.26
If your employees get an offer from another company, do they share the terms of the offer with you?	
Never	3.56
Rarely	15.56
Sometimes	61.85
Often	17.33
Always	1.70

Notes: Average characteristics of the sample of 1,350 professionals who set employee pay that participated in the survey. The survey was conducted in collaboration with the Society for Human Resource Management.

Table L.2: Use of salary benchmark tools.

Questions	Share of responses (%)
In your organization, do you use salary benchmarks?	
Yes	83.95
No	16.05
Which sources do you use to obtain salary benchmarks? (Select all that apply)	
Free online data sources	58.07
Paid online data sources	34.37
Industry surveys	68.00
Government data	37.11
Compensation consultants	26.30
Payroll data services	23.19
What do you use the salary benchmark for? (Select all that apply)	
To set precise salaries for new hires	54.07
To change salaries for current employees	76.81
In salary negotiations	53.11
To set salary ranges for specific job titles	89.78
To determine salary in job advertisement	40.89
To plan ahead for headcount	25.33
How frequently do you use salary benchmarks to set salaries for new hires?	
Never	2.58
A minority of hires	6.99
Some of the hires	26.06
A majority of hires	27.81
For every hire	36.55
When do you use salary benchmarks in relation to new hires? (Select all that apply.)	
Before I publicize the position to include the expected salary in a job advertisement	67.19
Right before I make an offer to the candidate	34.96
After the candidate receives the offer, if the candidate wants to negotiate	22.30
When the candidate presents an outside offer	12.44
How frequently do you use salary benchmarking to change salaries for current employees?	
Never	1.82
For a minority of employees	10.61
For some of my employees	25.89
For a majority of my employees	21.77
For all my employees	39.90
When do you use salary benchmarks with current employees? (Select all the apply)	
When the employee goes through an annual review	48.96
When the employee is up for promotion	47.48
When the employee presents an outside offer	33.11
When adjusting the salary ranges for positions	74.30
Why do you use salary benchmarks for some but not all employees? (Select all that apply)	
I search for some specific employees (within a given position)	17.85
I search only in some specific positions (and apply it to employees in those positions)	68.91
Other	18.10
For which positions are salary benchmarks most useful, higher-education or lower-education?	
Not useful for either group	0.30
Most useful for higher-education positions	21.78
Most useful for lower-education positions	3.93
Equally useful for both groups	74.00

Notes: General use of salary benchmark tools. Total responses are 1,350. For questions where more than one option could be selected (“Select all that apply”), the percentage for each answer represents the share of total responses that selected that option.

Table L.3: Strategic incentives.

Questions	Share of responses (%)
Suppose you raised the salaries of your new hires by 10% and your competitors learned this information, what do you expect them to do with their new hires?	
Nothing, salaries of competitors would stay the same	31.41
Salaries of competitors would rise between 1-5%	17.85
Salaries of competitors would rise between 5-10%	27.11
Salaries of competitors would rise 10%	13.26
Salaries of competitors would rise by more than 10%	10.37
Suppose you lowered the salaries of your new hires by 10% and your competitors learned this information, what do you expect them to do with their new hires?	
Nothing, salaries of competitors would stay the same	83.78
Salaries of competitors would fall between 1-5%	9.70
Salaries of competitors would fall between 5-10%	4.67
Salaries of competitors would fall 10%	1.56
Salaries of competitors would fall by more than 10%	0.30
When choosing filters there is a trade-off: applying filters can allow you to focus on a more relevant subgroup, but at the cost of smaller sample sizes and thus statistically imprecise benchmarks. Taking this into account, please select any filters you would typically apply.	
Industry	87.33
State	84.15
Firm size	48.00
Revenue size	38.96
Hourly vs. salaried	37.11
None of the above, the position-level filter is sufficient	0.00
Have you ever used Glassdoor as your salary benchmark source?	
Yes	48.59
No	51.41
Have you ever used ADP's Data Cloud Compensation Explorer as your salary benchmarking source?	
Yes	9.48
No	90.52

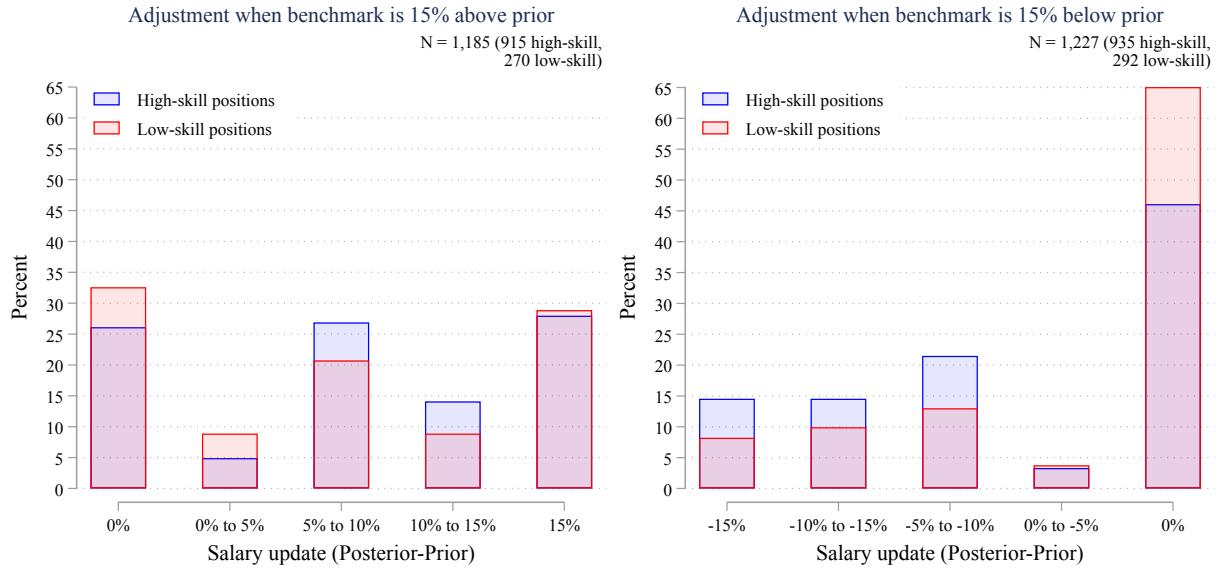
Notes: Strategic incentives on the use of salary benchmarks. Total responses are 1,350. For questions where more than one option could be selected, the percentage for each answer represents the share of total responses that selected that option.

Table L.4: Most relevant piece of information in salary benchmark tools.

	Share of responses (%)				
	All	Higher education	Lower education	Higher education	Lower education
	(1)	(2)	(3)	(4)	(5)
Median salary	56.73	58.47	48.75	57.03	53.70
Average salary	32.59	26.23	45.00	32.38	34.57
10th percentile	1.46	2.73	0.00	1.19	1.85
25th percentile	3.84	4.92	2.50	3.56	5.56
75th percentile	3.77	5.46	1.25	4.06	3.09
90th percentile	1.61	2.19	2.50	1.78	1.23
N	1301	183	80	1010	162

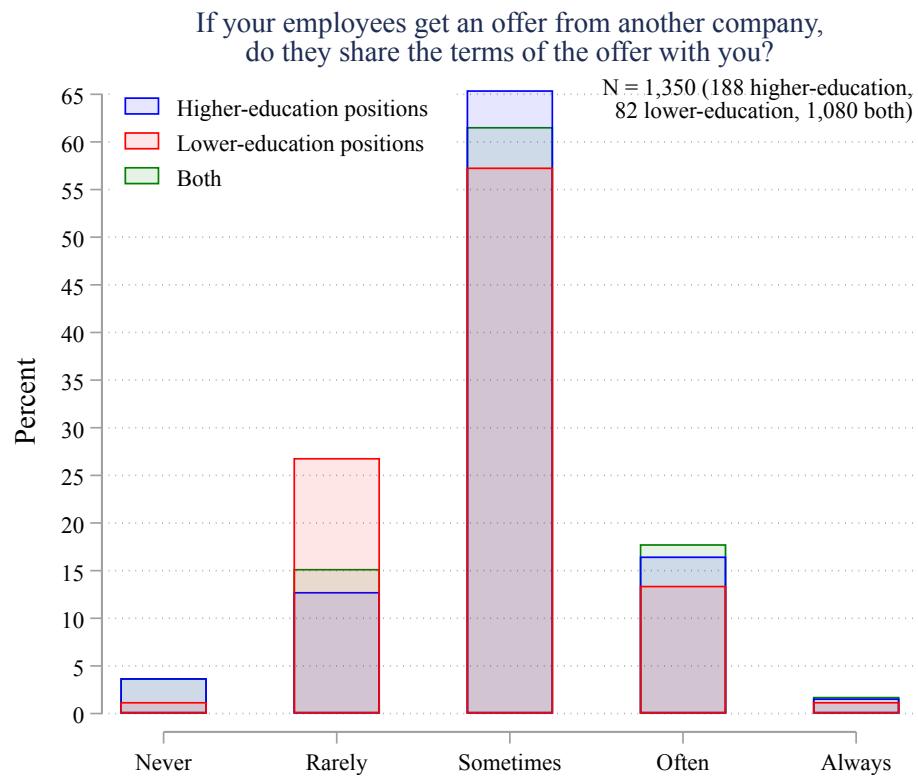
Notes: Percentage of responses that selected each piece of information as the one they care about the most when consulting salary benchmarks. Column (1) includes all responses to the survey. Columns (2) and (3) present responses of individuals who answered “Higher-education” and “Lower-education”, respectively, to the question “Do you typically set salaries for higher-education positions, lower-education positions or both?”. Columns (4) and (5) split responses by whether the position selected in the question “For which position is the salary benchmark most useful for you?” is classified as a higher or lower-education position according to the O\*NET classification of occupations into Job Zones.

Figure L.1: Salary updating with benchmark information.



Notes: Salary updating with benchmark information in experimental setting. Participants are asked to choose two positions they expect to hire in the future and to indicate the base salary for the new hires, before and after receiving information on an hypothetical benchmark for each position. The left panel shows the base salary update for positions where the salary benchmark was 15% above the prior salary, and the right panel for positions where the benchmark was 15% below. Positions are classified as “low-skill” or “high-skill” according to the O\*NET classification of occupations into Job Zones.

Figure L.2: Outside offers.



Notes: Histograms on information about outside offers for individuals who typically set pay for higher-education positions, lower-education positions, and for both.

## M Expert Prediction Survey

We are conducting an empirical study on labor markets. Due to your research record, you have been identified as an expert on the topic. We would love to elicit your expectations about the results of our analysis.

Please read the consent form below and click "I Agree" when you are ready to start the survey.

This survey involves no more than minimal risk to participants (i.e., the level of risk encountered in daily life). Participation typically takes between 5 and 10 minutes and is strictly confidential. Many individuals find participation in this survey enjoyable, and no adverse reactions have been reported thus far. Participation is voluntary, and participants may withdraw from the survey at any time.

Yes, I would like to take the survey



We would like to begin by providing some background information about this study.

When setting salaries, U.S. legislation prohibits employers from directly sharing compensation information with each other. However, employers are still allowed to use aggregated compensation data (e.g., median salary by position) provided by third parties. The practice of using aggregated market data is known as salary benchmarking.

We study the effects of salary benchmarking on the pay-setting of new hires. More precisely, we collaborated with a company that offers an advanced salary benchmarking tool that allows employers to look up market salaries in specific positions.

### **Employers in the Sample**

Our sample covers a total of 1,982 firms from the United States, 583 of which gain access to the salary benchmarking tool. The average firm in our sample has 517 employees, ranging from 3 to 19,370 employees. Our firms cover all the main sectors in the U.S. economy, with the most common sectors being Manufacturing (21% of firms) and Finance and Insurance (14% of firms).

### **Employees in the Sample**

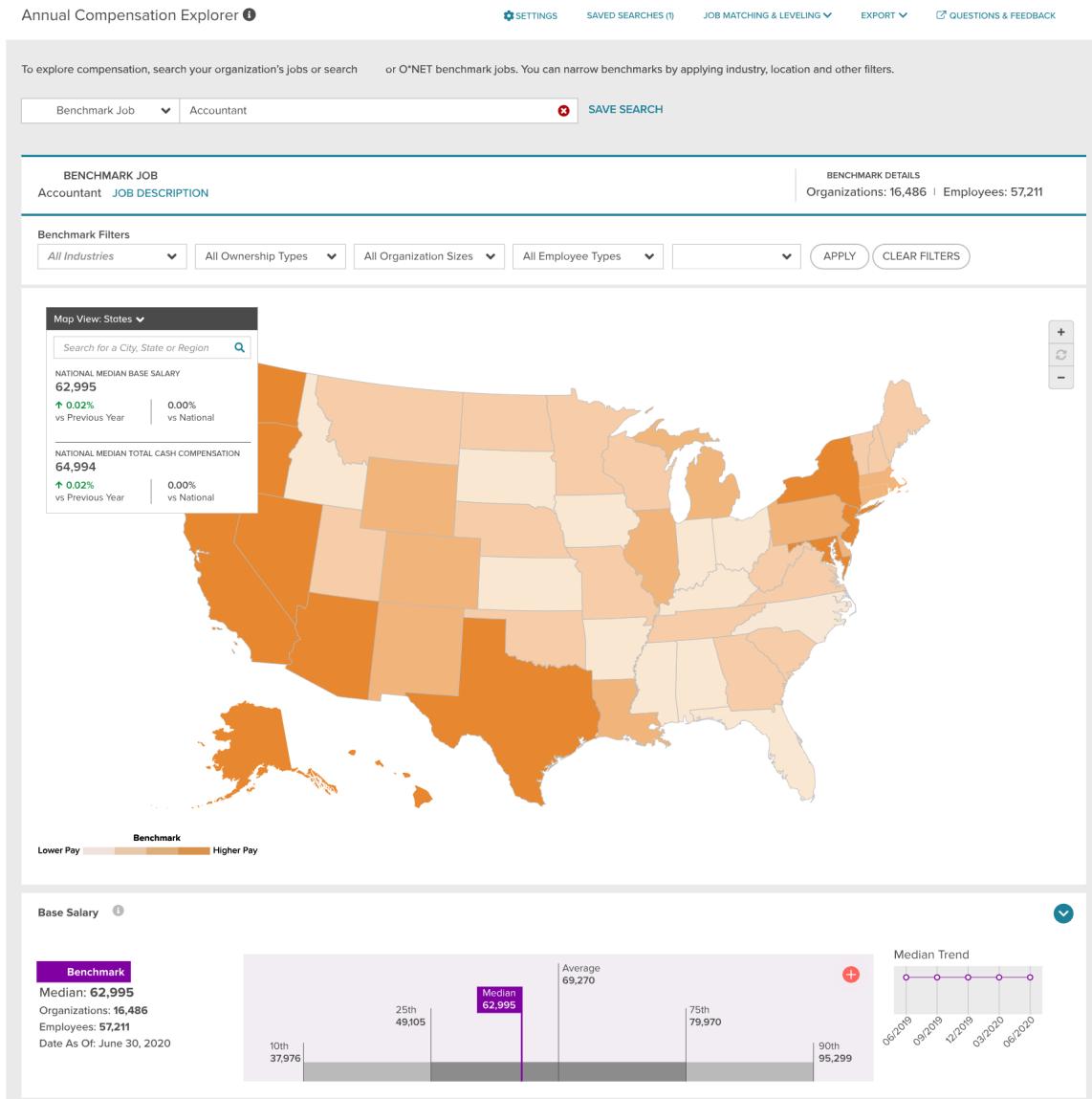
The average employee in our sample earns \$41,441 per year in base salary. On average, base salary accounts for the vast majority (92.7%) of the total compensation.

There are over three hundred unique positions that are looked up in the salary benchmarking tool. Some of the most commonly searched positions are Bank Teller, Customer Service Representative, Patient Care Coordinator and Software Developer.



## Benchmarking Tool

To give you a bit more context, find below a screenshot of the benchmarking tool:



Employers can look up any position (e.g., in the above screenshot, "Accountant"). Employers can apply filters to see the aggregate statistics within a specific state or industry, among other user-friendly features. The search results display the median annual base salary for the position, along with other key statistics (e.g., the 25th and 75th percentiles).

The benchmarks shown to the employers are of the highest quality. They are calculated using accurate payroll records from hundreds of thousands of firms and tens of millions of employees. As a result, the benchmarks are precisely estimated: e.g., in the above screenshot, the distribution of Accountants' salaries is based on 57,211 unique employees working at 16,486 unique firms. Moreover, the monthly frequency of the payroll records provides the most up-to-date benchmarks.



Are you familiar with the results from our research (e.g. have you seen it presented in a seminar)?

Yes

No



First, we want to elicit your forecasts about the effects of salary benchmarking on the average salary of new hires.

Consider the following thought experiment. Two employers (A and B) who just hired a new employee (e.g., a bank teller). The two employers are otherwise identical, except that Employer A was randomly chosen to gain access to a salary benchmarking tool, while Employer B was not chosen to receive access to the tool. As a result, employer A looked up the market pay before setting the salary of the new employee, while Employer B did not have access to that information at the time of setting the salary of the new employee.

Relative to Employer B (without salary benchmarking), do you think the average salary set by Employer A (with salary benchmarking) will be higher, lower, or about the same?

About the same

Lower

Higher



How confident are you regarding the previous forecast?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident

---

Can you please explain briefly why you expect salaries to be about the same, on average, for Employer A?



How much lower do you expect the average salary of Employer A to be (in percent terms)? Please enter a number between 0% and 100%.

%



How confident are you regarding the previous forecast?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident

---

Can you please explain briefly why you expect salaries to be lower, on average, for Employer A?



How much higher do you expect the average salary of Employer A to be (in percent terms)? Please enter a number between 0% and 100%.

0 %



How confident are you regarding the previous forecast?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident

---

Can you please explain briefly why you expect salaries to be higher, on average, for Employer A?



In the previous question, we asked you about the effects of salary benchmarking on the **average** salary.

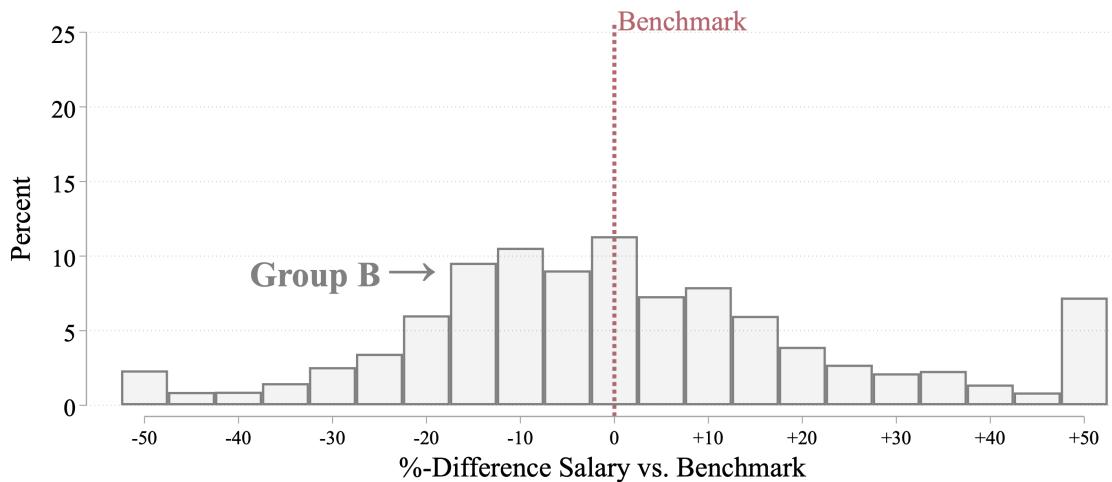
Next, we will ask you to forecast the effects of salary benchmarking on the **distribution** of salaries.

Consider two groups of employers:

**Group A** corresponds to employers **with** salary benchmarking: i.e., those who have access to the benchmark tool and look up the benchmarks before hiring a new employee.

**Group B** corresponds to employers **without** salary benchmarking: i.e., those who do not have access to the benchmark tool and thus cannot look up the benchmarks before hiring a new employee.

Consider the salaries of new hires relative to their corresponding benchmark (the median market salary for the position). For example, this is what the distribution of salaries looks like in Group B (without salary benchmarking):

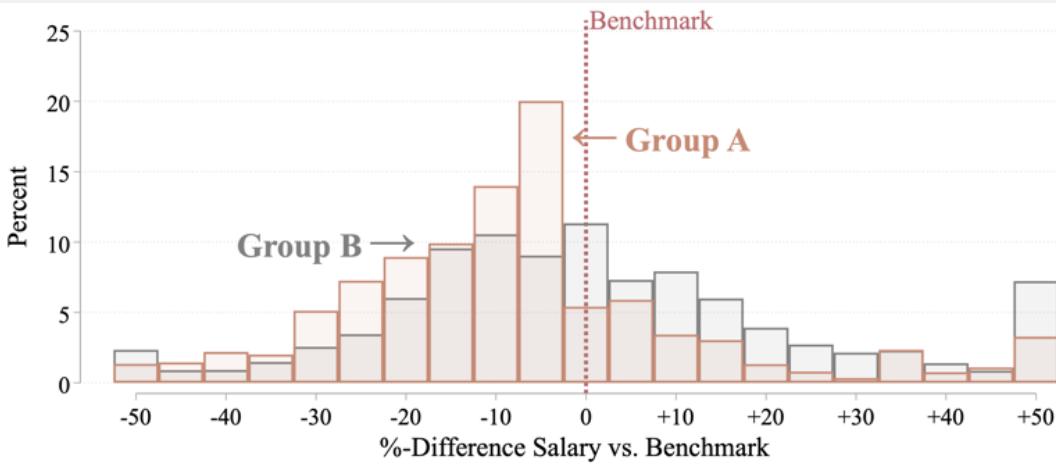
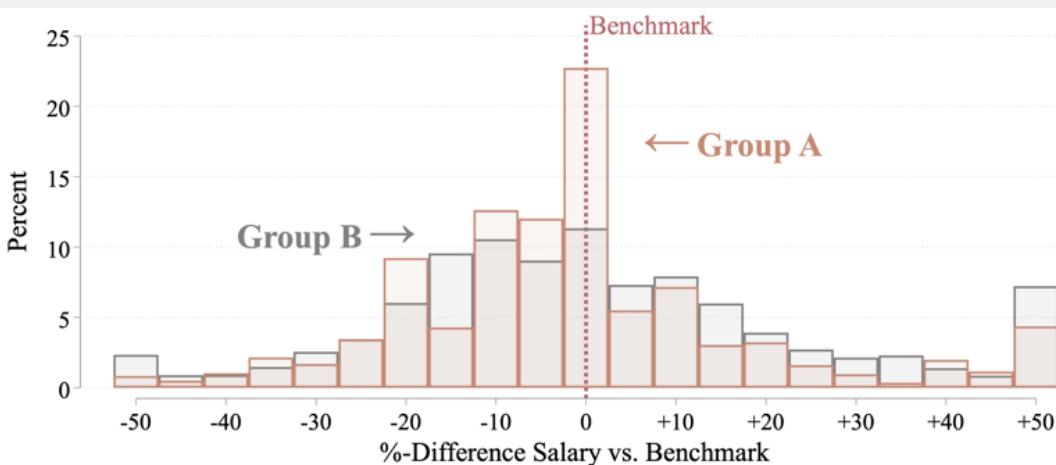


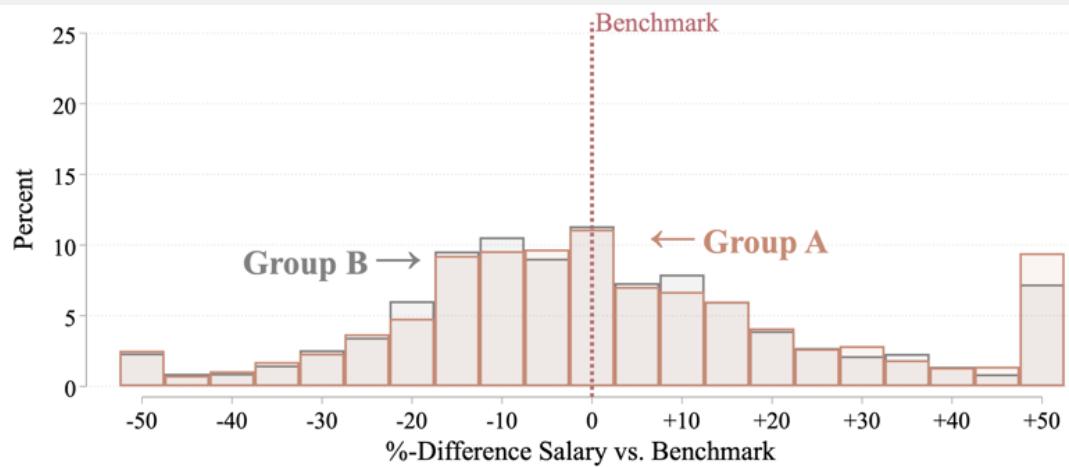
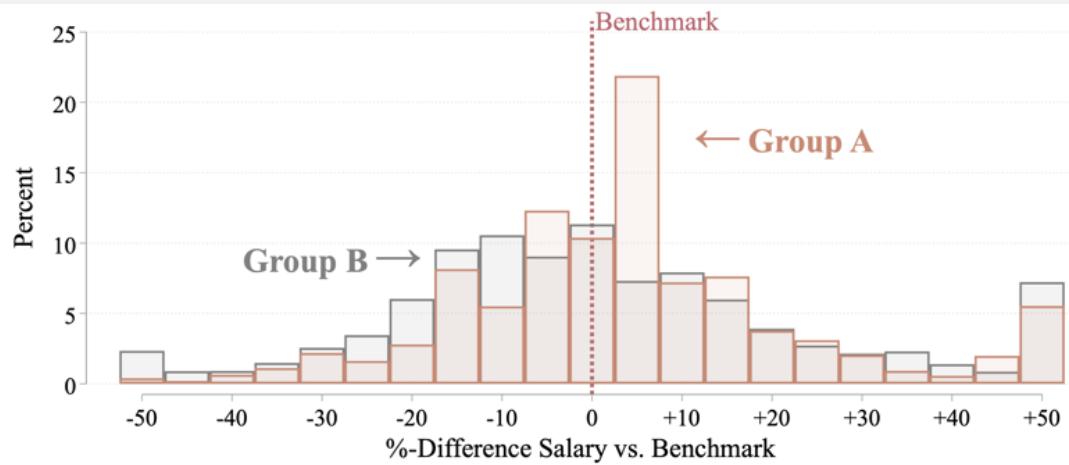
The middle bar corresponds to salaries that are close (i.e., within 2.5%) of the benchmark. The bars to the left of the middle bar correspond to new hires who are paid below the market benchmark, while the bars to the right of the middle bar correspond to new hires who are paid above the market benchmark.

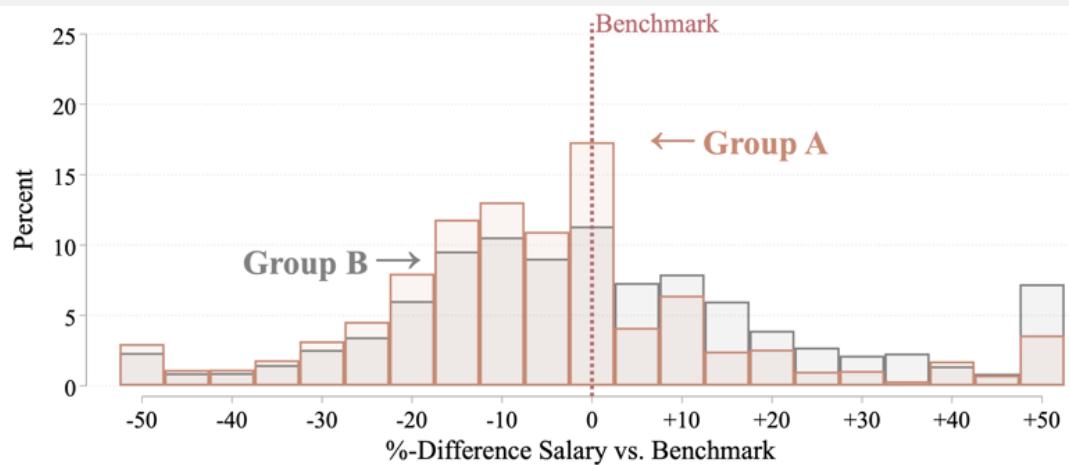
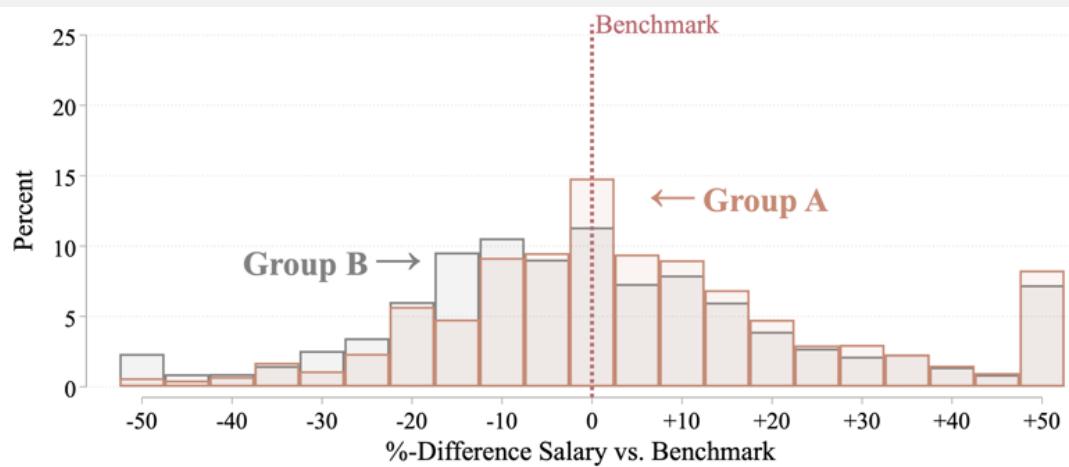
Now that you have seen what the distribution of salaries looks like in Group B (without salary benchmarking), we want you to predict what the distribution would look like for Group A (with salary benchmarking).

Find below six histograms. In each of them, the gray bars denote the salaries in Group B (without salary benchmarking), while the red bars correspond to salaries of Group A (with salary benchmarking).

In your opinion, which of the following histograms best describes the effects of salary benchmarking?







How confident are you regarding the previous forecast?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident

---

Can you please explain briefly why you think the histogram you selected best represents the effects of salary benchmarking?



Now, we want you to forecast which type of positions (if any) will be most strongly affected by salary benchmarking.

Consider lower-education vs. higher-education positions. The lower-education positions require little training and no more than a high school degree. The higher-education positions require more training and a College degree or more. Some common examples of lower-education positions are Bank Teller, Receptionist and Delivery Driver. Some common examples of higher-education positions are Legal Associate Specialist, Registered Nurse and Software Developer.

Do you expect the effects of salary benchmarking to differ between lower-education and higher-education positions?

No effect for either group

Stronger for lower-education positions

Stronger for higher-education positions

Equally strong for both groups



How confident are you regarding the previous forecast?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident



How confident are you regarding the previous forecast?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident

---

Can you please explain briefly why you think the effects will be stronger for lower-education positions?



How confident are you regarding the previous forecast?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident

---

Can you please explain briefly why you think the effects will be stronger for higher-education positions?



How confident are you regarding the previous forecast?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident

---

Can you please explain briefly why you think the effects will be about the same for lower and higher education positions?



Do you expect salary benchmarking to affect the gender pay gap?

No, it will not affect the gender pay gap

Yes, it will reduce the gender pay gap

Yes, it will increase the gender pay gap

How confident are you regarding the previous forecast?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident



Can you please explain briefly why you think salary benchmarking will not affect the gender pay gap?



This is the last section of the survey. We would appreciate if you could share some information about yourself.

---

Are you currently one of the following: graduate student (either Master level or PhD level), faculty, post-doc or non-academic researcher?

Yes

No



Which of the following describes your current position?

Professor (Associate or Full)

Assistant Professor

Post-doc

Researcher

PhD Student

Master Student

---

Please select your discipline

Economics

Management

Political Science

Psychology

Sociology

Other

Do you have research experience in the following fields? Please select all that apply:

Labor Economics

Personnel Economics

Public Economics

Behavioral Economics

Organizational Economics

None of the above



This is the end of the survey. We thank you for taking the time to provide your forecasts!

If you have any comments for us, please leave them below:



## N SHRM Survey

Harvard Business School professor Zoe Cullen is conducting a study about salary setting.

*The following is a short summary of this study to help you decide whether or not to be a part of this research.*

**Here is some Key Information about the study:**

- We are asking you to take part in a research study because you have hiring expertise.
- If you agree to be in this study you will be asked to complete a 10-minute online survey. In the survey, you will answer questions about compensation. At the start of the survey, you will be asked a screening question to determine your eligibility for this study.
- Your participation is completely voluntary. You can choose not to participate, or you can agree to participate and change your mind later and your decision will not be held against you. Your refusal to participate will not result in any consequences or any loss of benefits that you are otherwise entitled to receive. You can ask all the questions you want before you decide.
- If you have questions, concerns, or complaints, or think the research has hurt you, talk to Professor Cullen. She can be reached at 617-495-1876, or [zcullen@hbs.edu](mailto:zcullen@hbs.edu).

Yes, I agree to take the survey



Do you participate in setting the salaries for employees?

- Yes
- No

→

How many years of experience do you have setting salaries?



How many employees does your company have (please consider all locations)?

- 1-49
- 50-99
- 100-999
- 1,000-4,999
- 5,000 or more

→

What main industry do you operate in? (start typing, then select a category that best describes your business.)

*Please select a category from the list below to continue.*



Are you working in the private sector or the public sector?

- Private sector
- Public sector



How would you describe your current role?

- Human Resources Professional
- Chief Human Resources Officer
- Executive (outside HR division)
- Manager (outside HR division)
- Recruiter (outside HR division)
- Other



Do you participate in salary settings for:

- New hires
- Current employees
- Both



Suppose you wanted to know the median salary that your company pays employees in a specific position. Would you be able to access that data?

- Yes, I can access it easily
- Yes, but it would take quite a bit of work
- No, I could not access that data even if I wanted to

→

When setting the compensation of their employees, some organizations use aggregate data on the market salaries for specific positions. This type of data is typically referred to as ***salary benchmarks***.

In your organization, do you use ***salary benchmarks***?

- Yes
- No



Which sources do you use to obtain ***salary benchmarks***? (Select all that apply.)

- Payroll data services
- Industry surveys
- Free online data sources
- Compensation consultants
- Paid online data sources
- Government data

→

What do you use the **salary benchmark** for? (Select all that apply.)

- To set salary ranges for specific job titles
- To plan ahead for headcount
- To determine salary in job advertisement
- To change salaries for current employees
- To set precise salaries for new hires
- In salary negotiations
- Other



How frequently do you use ***salary benchmarks*** to set salaries for new hires?

- For every hire
- A majority of hires
- Some of the hires
- A minority of hires
- Never

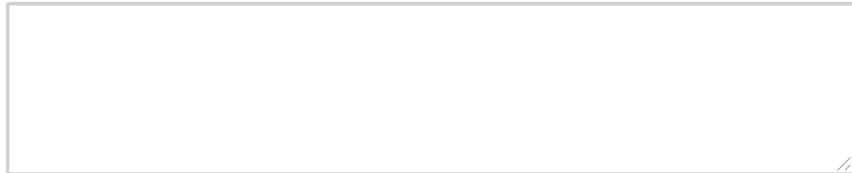
→

When do you use **salary benchmarks** *in relation* to new hires? (Select all that apply.)

- Before I publicize the position to include the expected salary in a job advertisement
- Right before I make an offer to the candidate
- After the candidate receives the offer, if the candidate wants to negotiate
- When the candidate presents an outside offer
- Other



Please explain briefly how you typically use ***salary benchmarks*** to set the salaries of new hires?



How frequently do you use ***salary benchmarking*** to change salaries for current employees?

- For all my employees
- For a majority of my employees
- For some of my employees
- For a minority of employees
- Never

→

When do you use **salary benchmarks** with current employees? (Select all that apply)

- When the employee goes through an annual review
- When the employee is up for promotion
- When the employee presents an outside offer
- When adjusting the salary ranges for positions
- Other



Can you please explain briefly how you typically use ***salary benchmarks*** to set the salaries of current employees?



Next, we'd like you to pick two different positions for which you are expecting to be hiring soon and tell us how you would set the salary for the new hire in each position.

Pick a position for which you are expecting to hire soon (start typing, then select a category - kindly allow a few seconds for the bold arrow to re-appear to continue.)

*Please select a category from the list below to continue.*

Pick a **second different** position for which you are expecting to hire soon (start typing, then select a category - kindly allow a few seconds for the bold arrow to re-appear to continue.)

*Please select a category from the list below to continue.*



Think about a future new hire in the role of Sales Engineers. What would be the **annual base salary** that you set for this person? (Please provide your best guess, and do not use commas.)

\$

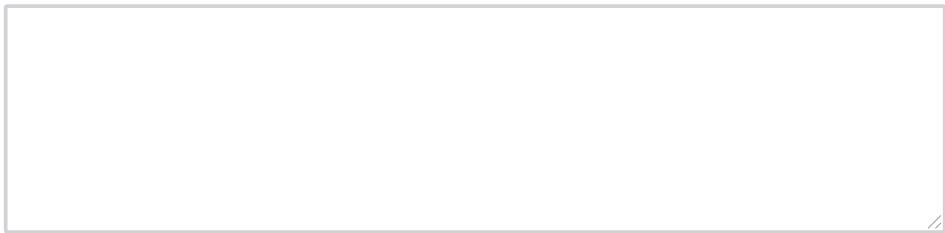


Suppose you look up the salary benchmark for this position using a highly accurate, up-to-date, compensation database and discover the median annual base salary is \$93500. Upon reviewing that information, what salary would you pick?

\$



Why did you use (or not use) the salary benchmark information in this compensation decision?



Think about a future new hire in the role of Sales Managers. What would be the **annual base salary** that you set for this person for a full-time position? (Please provide your best guess, and do not use commas.)

\$



Suppose you look up the salary benchmark for this position using a highly accurate, up-to-date, compensation database and discover the median annual base salary is \$92000. Upon reviewing that information, what salary would you pick?

\$



Why did you use (or not use) the salary benchmark information in this compensation decision?

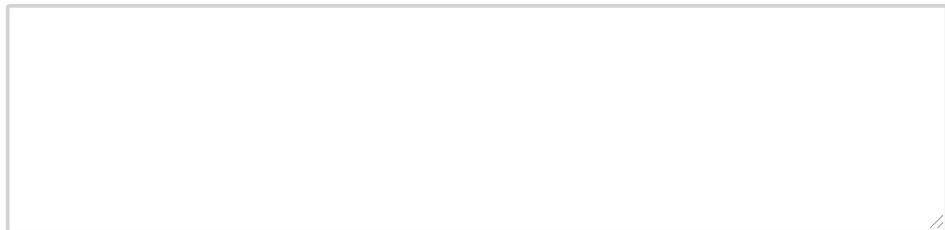


For which positions are ***salary benchmarks*** most useful?

- Most useful for lower-education positions
- Most useful for higher-education positions
- Equally useful for both groups
- Not useful for either group



Can you please explain briefly why salary benchmarks are most useful for higher-education positions?



Can you please explain briefly why salary benchmarks are most useful for lower-education positions?



Assume you are using a salary benchmark tool. The tool allows you to look at benchmark salaries, and to apply filters. When choosing filters there is a trade-off: applying filters can allow you to focus on a more relevant subgroup, but at the cost of smaller sample sizes and thus statistically imprecise benchmarks. Taking this into account, please select any filters from the set below that you would typically apply *after filtering for a particular position* (you can select more than one if you wish).

- Hourly vs Salaried
- State
- Industry
- Firm Size
- Revenue Size
- None of the above, the position-level filter is sufficient



Assume you are using a salary benchmark tool. It tells you the certain pieces of information about the salaries for that position. Please rank which information you typically care about the most (**drag and drop the options you care about, and then order them from most important to least important**). (1) Median salary (2) 10th Percentile (3) 25th Percentile (4) 50th Percentile (5) 75th Percentile (6) 90th Percentile (7) Average salary

Items	Order from most to least important (1=most important)
Median salary	
10th percentile	
25th percentile	
50th percentile	
75th percentile	
90th percentile	
Average salary	



For which position is the salary benchmark most useful for you? (start typing, then select a category - kindly allow a few seconds for the bold arrow to re-appear to continue.)  
Please select a category from the list below to continue.



Think of your two closest competitors who also hire Retail Salespersons. For anonymity reasons, we'll refer to your competitors as firm A and firm B.

What is the maximum annual salary you think firm A would be willing to pay to hire in the full-time role of Retail Salespersons? (Please do not use commas)

\$



Let's say you find out that firm A would be willing to pay a maximum salary of \$57500? After reviewing that information, what is the maximum annual salary you think firm B would be willing to pay to hire in the role of Retail Salespersons?

\$



Suppose you lowered the salaries of your new hires by 10% and your competitors learned this information, what do you expect them to do with their new hires?

- Nothing, salaries of competitors would stay the same
- Salaries of competitors would fall between 1-5%
- Salaries of competitors would fall between 5-10%
- Salaries of competitors would fall 10%
- Salaries of competitors would fall by more than 10%



Suppose you raised the salaries of your new hires by 10% and your competitors learned this information, what do you expect them to do with their new hires?

- Nothing, salaries of competitors would stay the same
- Salaries of competitors would rise between 1-5%
- Salaries of competitors would rise between 5-10%
- Salaries of competitors would rise 10%
- Salaries of competitors would rise by more than 10%



If you adjusted the salaries of your new hires by 10%, do you think it would affect the salient numbers of the most commonly used salary benchmarks?

- No, the popular salary benchmarks would stay the same or shift by a negligible amount
- Yes, somewhat (eg. the median would shift by 1-2%)
- Yes, a significant amount (eg. the median would shift by >2%)

→

Have you ever used Glassdoor as your salary benchmark source?

- Yes
- No

Have you ever used ADP's Data Cloud Compensation Explorer as your salary benchmarking source?

- Yes
- No



What share of your competitors do you think use ADP's Data Cloud Compensation Explorer as a salary benchmarking source?

- The vast majority of my competitors
- Some of my competitors
- Very few of my competitors

→

Please share any feedback you have for us on the survey!

