Salary Disclosure and Hiring: Field Experimental Evidence from a Two-Sided Audit Study

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Abstract

How does banning employers from seeking applicants' wage histories affect employment and salary offers? We implement a field experimental design we call a two-sided audit study, in which recruiters evaluate job applications with randomized characteristics under randomly assigned salary disclosure conditions. We begin by estimating the effects of candidates' salary disclosure under a variety of settings. Then, we combine our estimates to examine the likely effect of recent salary history ban laws like those passed in Massachusetts, California, New York City, and Chicago banning these questions. We find that disclosing generally improves outcomes for workers, however we find mixed results about the effect of disclosures on the gender wage gap. Disclosure increases male salaries more than female salaries, although this may only affect workers who are hired. Disclosure also improves female callback rates, without affecting men's. The overall effect of bans on the gender wage gap hinges on how we assess greater female representation.

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1 Introduction

In the last few years, 11 US states and 10 jurisdictions adopted laws banning employers from seeking salary history information. These laws aim to increase wages and income equality for historically disadvantaged workers. Little empirical or theoretical research has assessed these laws. Banning salary history could affect the labor market in a variety of plausible ways. The presence of salary history information could entrench historical inequality, either for standard price discrimination reasons or behavioral-economics style anchoring. Alternatively, salary histories could contain information employers use to equalize employment opportunities; if so, banning salary history information could *increase inequality* and lower wages. Bans could also fail to change outcomes at all. They may be difficult to enforce and do not forbid voluntary disclosure. Employers also do not need exact salary amounts to realize that certain groups face lower compensation, and may rely on statistical discrimination after ban implementation.

In this paper, we study how salary history information affects employers' hiring decisions and use this to assess salary history bans empirically. We use a novel field experimental design we call a *two-sided audit*. In our experiment, we hire a professional recruiting workforce to evaluate job applications on behalf of a client. Across recruiters, we randomize the availability and presentation of salary history information. Across applicants, we randomize gender and the level of the previous salary.

Like traditional audit studies, the applications reviewed by the recruiters were secretly fictitious (Bertrand and Mullainathan, 2004; Baert, 2018). Unlike traditional audit studies, the job requisition was varied for the purpose of research as well.[1] Both sides of the labor market in our experiment were created for research. By contrast, our recruiting workforce – including their incentives, their wages and the expertise they brought to this job – were real and we believe are typical for many employers.

Our workforce was comprised of 112 recruiters, 70% of whom had over three years of prior recruiting experience. We paid normal market rates for recruiting services (including contracted performance incentives)[2] and sought standard assessments about each candidate, i.e. callback and salary recommendations. These tasks are common for recruiters; 77.7% of our recruiters report that salary input is required in the majority of prior recruiting jobs.[3]

We find that under salary history bans, recruiters believe workers will require 11.5% *lower* salaries for an equivalent probability of accepting an offer (in comparison to applications that banned salary history disclosure). Reservation wages for this job, in other words, are believed by recruiters to be higher when salaries are disclosed. Our data shows that disclosure not only changes recruiters' beliefs, but also these changes in beliefs translate into action. Our recruiters suggest lower wage offers to candidates in the banned condition.

[1] See Benson et al. (forthcoming); Gee (2018); Leibbrandt and List (2018); Mas and Pallais (2017); Samek (forthcoming); Flory et al. (2014); Abraham and Burbano (2019) for examples of papers utilizing experimentally varied job posts without fictitious candidates, i.e., one-sided audits randomizing job posts.

[2] We paid on average $42/hour, more than one-third higher than thethe BLS average wage for Human Resources (HR) professionals.https://www.bls.gov/oes/2017/may/oes131071.htm

[3] 62.5% said they are asked for salary input "most of the time" or "always", 15.2% reported "about half the time," 20.5% reported "sometimes." Only 1.8% said they were never asked for salary input during a recruiting process.

Importantly, these results also apply to the female subset of candidates. Women's average salary offers are lower under bans. Men's salaries, however, are hurt even more by non-disclosure, which suggests that bans improve the gender wage gap, one of their purported goals. However, they do so at the expense of overall wage levels.

In our salary ban conditions, women are not only paid less than under disclosure, but are also called back less. Disclosure hurts women partly because of the disproportionately negative effect of non-disclosure on callbacks for female applicants – a mechanism mostly unexplored in the policy debate about bans. By contrast, non-disclosure decreases male salaries but does not affect representation or selection. Therefore, although banning decreases the overall gender wage gap, it also decreases callback rates for women. To assess the net effect of bans, we examine the interaction of salary offers and callbacks. We find a net negative effect on gender equality – contrary to the intentions of the policy.

Do salary bans reduce or increase gender inequality? The answer hinges on how we assess the change in representation of women overall. If we ignore the change in representation, bans appear to decrease the wage gap. However after we account for the reduction in female representation, bans actually *increase*gender inequality – the opposite effect of the policy's aim. These results point to the importance of *jointly* evaluating wages and workforce composition. The merits of these bans depend on how policymakers weigh tradeoffs between greater income, representation and equality for workers and job applicants.

2 Background

As of February 2019, 11 states and 10 jurisdictions have adopted some form of salary history bans.[4] These salary history banscan pertain either to state/jurisdictions agencies (public employers) or to all employers in the jurisdiction (both public and private). Seven states and two jurisdictions have bans that pertain to *all* employers. We list these states Appendix Table A4, along with their effective dates.

These laws prohibit oral or written questions about salary history, even if the questions are posed as voluntary or optional. They also prohibit employers from relying on salary histories in compensation and hiring decisions. Importantly, the laws do not ban questions about salary *expectations*. In addition, applicants under the bans are still permitted to *voluntarily* and *without prompting* disclose salary history information. In these instances jurisdictions vary on how employers can react to this information. In California, employers are expressly prohibited from relying on even voluntarily disclosed information. In other jurisdictions they may use or confirm this information when voluntarily disclosed. However, the 9th Circuit Court recently ruled that employers cannot use previous salary as a justification for paying a woman less than a man for the same work (*Rizo v. Yovino*).

The movement has faced some resistance and backlash. Two states (Michigan and Wisconsin) have banned local jurisdictions from implementing their own salary history bans. These "bans on the bans" guarantee employers' right to request this information if they wish. In Philadelphia, the

[4]See https://www.hrdive.com/news/salary-history-ban-states-list/516662/ for most up-to-date list. This list includes the 2 states that have banned salary history bans.

ruling in *The Chamber of Commerce of Greater Philadelphia v. City of Philadelphia et al.* concluded that the prohibition on salary history inquiries violates the First Amendment.[5]

The expressed goal of the bans is to decrease pay inequality, as is apparent in the titles of the laws: e.g. "The Act to Establish Pay Equity" (MA) and "An Act Concerning Pay Equity" (CT). On the day the Illinois law was announced, one of the sponsors said "I am proud to stand with our new governor today as he takes strong, immediate action to close the gender pay gap and move towards pay equity[.] [6]

For these laws to act as a solution to pay inequality, we must first establish that a pay inequality problem exists. There is a large and well-developed literature documenting and exploring the causes of the gender wage gap specifically.[7] Second, we need to establish that firms actually engage in the act of asking for previous salaries. Here survey evidence finds that between 30 to 43% of firms do make such inquiries (Hall and Krueger, 2012; PayScale, 2017). Third, we need to show that previous salary might have some effect on current salary. This last item is less clearly established and as such a contribution of our paper is to test how previous salaries affect future salaries. However, other work finds that entering the job market during a downturn, an arguably exogenous shock to starting income, has not only an initial negative effect on wages, but also has effects that persist over a decade later (Kahn, 2010; Oyer, 2008).

Even with these three items established, it is not clear that banning disclosures will decrease wage inequality without other consequences. Recent work has found that laws that change the recruiting process like banning employers from asking if a candidate has a criminal record, allowing drug testing, gender blinding resumes, or adding messages encouraging diversity often have unintended consequences (Clifford and Shoag, 2016; Agan and Starr, 2018; Leibbrandt and List, 2018; Doleac and Hansen, forthcoming; Behaghel et al., 2015; Wozniak, 2015). However, some find that laws meant to decrease gender inequality by removing gender cues in the application process do have their intended effects (Goldin and Rouse, 2000).

One reason a salary history ban may have unintended consequences is there may be noncompliance with the intention of the law either through firms continuing to obtain previous salary information or through job candidates voluntarily disclosing their previous salaries (Sherman et al., 2019). There is reason to believe that women would be less likely to voluntarily disclose than men, because women are less like to disclose income than men on surveys (Goldfarb and Tucker, 2012), and female software engineers are less likely to disclose their skills than men (Murciano-Goroff, 2017). Also those with higher previous salaries may be more likely to disclose that number than those with lower previous wages. Other work finds that restaurants with better health grades are more likely to disclose than those with lower grades, but that those with the highest grades don't

[5]Philadelphia enacted a salary history ban that pertained to all employers in January 2017. The Philadelphia Chamber of Commerce sued the city over the law, and in 2018 a federal judge ruled that the part of the law that prohibited employers from inquiring about salary history was an unconstitutional violation of free speech. However, the judge made no such claim about the part of the law banning employers from relying on this information. So as it stands Philadelphia employers may ask, but cannot rely on this information in making salary offers. Both parties have appealed this ruling and arguments are scheduled for March 2019. Any ruling made by the Third Circuit Court will only apply in the states within the circuit, namely Pennsylvania, New Jersey, Delaware and Virgin Islands.

[6]https://www2.illinois.gov/Pages/news-item.aspx?ReleaseID=19609

[7]See Goldin (1990); Solnick (2001); Eckel and Grossman (2001); Blau and Kahn (2006); Thilmany (2000); Blackaby et al. (2005); Takahashi and Takahashi (2011); Moss-Racusin et al. (2012); Mazei et al. (2015); Ponthieux and Meurs (2015); Rozada and Yeyati (2018).

post these (Bederson et al., 2018). Similarly, Luca and Smith (2015) find that the most highly ranked MBA programs do not post this information on their websites.

What perhaps matters most is how a firm or it's agents interpret the voluntary disclosure of previous salary. It seems likely that lack of disclosure may be viewed as a negative signal of quality (Jin et al., 2015; Milgrom, 1981). Theoretically this may lead to unravelling where all job candidates would begin to disclose, but empirically in markets with quality disclosures such unravelling isn't always observed (Dranove and Jin, 2010; Mathios, 2000).

There are few papers looking only at the effect of making previous salary information available. Barach and Horton (2017) use data from hiring hourly workers in an online labor market. In this labor market they randomly assign some employers to be unable to see a job candidate's previous salary information, however job candidates self-select an initial wage bid which is always seen by the potential employer. They find that when previous salary is not disclosed, then employers evaluate more workers and are more willing to hire lower wage workers. Their paper does not include general results on wage inequality or results broken out by gender.

A related law, and one that is sometimes passed in conjunction with a salary history ban, requires employers to increase pay transparency by making their wages or wage ranges available. McNichols (2018) uses data from the United States to explore the combined effect of requiring employers to provide salary ranges upon request with a salary history ban in the state of California. He finds increased transparency coupled with a salary history ban has no effect on the the employment probabilities of men or women. However these policies result in a decline in the gender wage gap that comes from men's wages falling and women's wages rising (only the change in gender wage gap is significant, while the changes on male and female wages are not statistically significant).

Looking solely at the effect of increased transparency without a salary ban, Cullen and Pakzad-Hurson (2016) present a theoretical model which predicts that full wage transparency will result in a decline in wage inequality coupled with a fall in wages. They find patterns consistent with this model using data from TaskRabbit, and go on to present the results from a field experiment which finds exogenously assigned wage transparency does indeed lower inequality while also lowering wages, however they do not break these results out by gender.

They also find that hiring increases when wages are transparent. Bennedsen et al. (2019) find that when pay transparency laws go into effect in Denmark this increase the female hiring rate and lowers the gender wage gap. However, the change in the gender wage gap comes from the slower wage growth of men's salaries rather than faster wage growth for women's.

The previous work has shown that policies that alter the hiring process can affect many different outcomes of interest including call-backs, wages, and wage inequality. In the next section we describe a conceptual framework for how banning disclosures might affect job applicants and employers.

3 Theoretical/Conceptual Framework

We begin with a simple theoretical description as a guide for interpreting our empirical results. The two players in our model are an employer and a job applicant. The employer's utility comes

from money and productivity from hiring a job candidate. If hired, a job applicant yields a stream of utility payments to the employer ($v_1 + v_2 + v_3 + v_n$+...) and salary payments to the worker (s_1 + s_2+...) from the employer (these payments may be zero after a worker quits or is fired). The net present value of these streams are v and s.

The applicants' utility comes from salary payments and the disutility of work. Each applicant has a set of k outside offers. For simplicity of exposition, we assume that the disutility of work is constant across all employers.[8] The outside offers are the net present value of the salaries offered by other employers.[9] The k offers are drawn from a distribution F with mean μ, and includes the candidate's current position if he/she is currently employed. Additional draws come from a combination of the active search for others employers, or from employers' active recruitment of the candidate.[10] A candidate's best outside option, η, is the 1st order statistic from the k draws from F.

The firm will prefer to hire a candidate if $v - s$ exceeds the up-front cost of hiring (training and onboarding the new candidate, etc). This formulation implies that the employer can hire multiple candidates as long as they are profitable.[11]

Salaries s are set according to the bargaining protocol. Our experiment examined two bargaining protocols, both versions of take-it-or-leave-it ("TIOLI"). The first is an employer-initiated TIOLI protocol where the employer makes a one-time, non-negotiable offer that a candidate rejects or accepts. The second is a candidate-initiated TIOLI protocol in which the candidate generates a similar one-time, take-it-or-leave-it offer to the employer (who can accept or reject).

In our experiment, we asked our recruiters to prepare an employer-initiated TIOLI offer. However, we also told recruiters that candidates sometimes make a candidate-initiated TIOLI offer, and we asked recruiters to tell us the maximum candidate-initiated TIOLI offer the firm should accept.

We describe this setup in greater detail in our experimental design section, including the incentives we created to to set these values optimally. We now analyze the employers' equilibrium strategies in these protocols and their implications for disclosure policies.

Employer-Initiated TIOLI: In this protocol, the employer's challenge is to generate an optimal TIOLI offer for the candidate. The optimal offer balances two competing factors. The employer enjoys a higher "margin" ($v - s$) from hiring the candidate if the salary s is low. However, a low salary offer could increase the probability of rejection ($\Pr(s > \eta)$).

If disclosure policies increase firms' TIOLI offers, this could be caused either by a) higher beliefs about the candidate's value v, or b) higher beliefs about the candidate's best outside offer η. η itself could be higher either because μ increases (the mean of the outside offer distribution F) or because k increases (the number of outside offers drawn from F). k could increase because of greater search from the candidate, or from greater search by competing employers.

[8] This does not substantially affect our conclusions, and we have empirical tests to address the possibility that some employers have non-pecuniary benefits.

[9] They may be expressed to the candidate in expected annual amounts.

[10] We do not model the worker's endogenous effort choices once on the job, as it does not relate to our experiment. Insofar as paying workers more would increase effort, this would be represented in our framework as an increase in v.

[11] This matches empirical setting in the software industry and other markets featuring labor shortages. As we describe in our empirical setting, firms in these markets are unable to find enough qualified workers and will hire multiple workers if several are above the threshold.

Our experiment is designed to disentangle how disclosure effects outcomes through these various mechanisms.

Candidate-Initiated TIOLI: From the employer's perspective, optimal strategy in the candidate-initiated TOILI protocol is simpler. The strategy is to set a threshold above which the offer is rejected, and below which it is accepted. This is effectively a BDM mechanism as the outside offer η is a random variable. As such, the optimal threshold is v, the employer's valuation of the candidate (Becker et al., 1964).

Should non-disclosure/banning policies increase employers' thresholds in this protocol, the BDM setup suggests non-disclosure increased employers impression of candidates' values. In principle, the hiring employer's valuation could be completely uncorrelated with other employers' valuations for the same candidate. However, employers could also make inferences about a candidate's valuation from other employers' salary offers (including a candidate's current or historical offer).

If this were the case, then the *dollar amount disclosed* by the candidate would teach the new employer something about the level of the candidate's value to the new employer (assuming the employer found it sufficiently believable).[12] A unique benefit to our two-sided audit methodology is that we can vary salaries randomly for the same candidate biography, and thus test how valuations are sensitive to the disclosed amount in dollars.[13]

Callback Decisions: Recruiters in our experiment were not only asked for TIOLI offers and salary thresholds, but also to make decisions about callbacks. Given the setup above, a candidate should get a callback if $(v - s)\Pr(s > \eta)$ – the the expected margin $\times$ the probability of acceptance – exceeds the employer's cost of a callback.

Increases in callback rates could arrive through two mechanisms. The employer may believe the margin $(v - s)$ is higher. Or the employer may believe that the probability of acceptance is better, thus increasing the benefit of a callback. Note that better outside offers increase the optimal employer-initiated offer, but *decrease* the returns to calling back a candidate (because the expected acceptance rate is lower).

Certain conditions may induce both higher equilibrium TIOLI offers – an outcome that may seem to be good for candidates – and lower callback rates. This would happen if conditions make candidates appear "too expensive" for a callback or "too unlikely to accept our offer."

Even at the optimal TIOLI salary, certain workers may appear to be a bad deal, possibly because of low expected margin or low expected acceptance rate, and thus *not* be worth pursuing. This is possible even if valuations and salaries are both high because the employer cares about the difference. Evaluations of disclosure must consider both the effects on salary *as well* as the effects on selection decisions.

With our conceptual framework in hand we next discuss how we gathered data to test the effect of banning salary disclosures.

[12] Only 36% of our recruiters stated they thought a candidate would lie about their current salary.

[13] This would be impossible with completely "real" candidates, who would only disclose one salary per biography.

4 Setting and Experimental Design

To examine the impact of these salary disclosure and laws that ban salary disclosure, we implemented a two-sided audit study. To our knowledge, this design has been utilized in only one other paper (Cowgill and Wang, 2019). We hired a real recruiting workforce to screen (fictitious) candidates on behalf of a (fictitious) firm. This design allows us to vary both characteristics of the candidates and characteristics of the firm. In our case we varied the gender and salary of the candidates, and varied whether the firm had a mandatory, optional, or no salary history question on the job application.

4.1 Our Recruiting Workforce

To hire our recruiting workforce, we utilized the Recruitment Process Outsourcing (RPO) industry. Many firms either hire recruiting workers on a temporary, contract basis or entirely outsource recruiting work. As a result, a large industry exists to support temporary contract recruiting services. In 2017, RPO was a five billion dollar a year industry[14], serving a variety of industries including the software industry. Eight of the larger RPO companies reported filling just under one million positions in 2018, or about 36% of the total number of jobs created in the US that same year.[15] According to surveys of this industry, most recruiters in the RPO industry work remotely, rather than onsite at a particular firm, are paid per hire plus a flat rate, and over 90% are tasked with screening candidates' applications.[16]

The RPO industry creates an opportunity to implement our two-sided audit design. We hired recruiters on a contract basis as the subjects of our experiment. The recruiters were solicited to work on selecting software engineering candidates for a client. We discuss our selection procedures in Appendix B.

We gave our subjects directions comparable to typical recruiting tasks. We used a platform to identify, correspond and pay the recruiters. Such platforms facilitate the RPO industry by providing search, payment, dispute resolution and reputation monitoring to recruiters, RPO staffing agencies and corporate clients.

In total 112 recruiters completed the experiment. Based on their self-disclosed experience, about 70% of our recruiters had at least 3 years of recruiting experience prior to starting our job. Our recruiters were 25% male and 31% black. 98% stated they were asked for salary input in prior jobs. Their mean hourly wage was $42.63 per hour. When compared with the BLS statistics about human resource workers in the US, our recruiters were slightly less likely to be male, more likely to be black, and had a higher hourly wage.[17] As we describe in Section 4.5, they were randomly

[14] https://www.workforce.com/2018/01/25/sector-report-tech-gaining-foothold-rpo-space/

[15] https://www.workforce.com/2019/01/24/recruitment-process-outsourcing-providers-think/

[16] Staffing Industry Analysts, RPO Market Developments, December 2017.

[17] According to the BLS in 2018 Human Resource workers across all industries were 30.3% male, 10.5% black, and the median hourly wage was $29.01. (See https://www.bls.gov/cps/cpsaat11.htm and https://www.bls.gov/ooh/business-and-financial/human-resources-specialists.htm). Note that the BLS publishes average wages for industry×occupation cells. For the software industry, the median wage for human resource workers is $41.93 (mean $38.39), which is very close to our average hourly wage https://www.bls.gov/oes/current/naics4_511200.htm. The BLS does not report demographic characteristics of industry×occupation cells. However, these figures can be calculated

assigned to treatment groups.

4.2 Creating Candidates

Each recruiter's job was to review eight candidates and provide feedback about each one confidentially to the firm through observations, commentary and structured assessments. We call each set of eight candidates a "packet". Each packet of eight candidates contained four male and four female applicants.

Our goal in assembling these biographies was to create a realistic variety of candidates that could pass as applicants. To achieve a statistically-high powered experiment, we sought to make candidates broadly similar to each other. The exceptions were for several key dimensions in which we had specific hypotheses to test. Those dimensions were: candidates' genders, whether the candidate currently works at relatively high- or low- wage firm, and whether the candidate was high or low in his/her current employer's salary distribution for his/her job. We describe the details of how the attributes of a candidate were chosen (e.g. name, college, previous jobs) in Appendix C.[18]

4.3 Creating a Job Posting

We tasked our recruiting workforce with screening applicants for a software engineer position. The software sector is an ideal labor market for studying the effects of salary ban policies. The market for software jobs features several particularly attractive features for this study.[19]

First, technical roles exhibit persistent gender wage and employment differences that span multiple decades (Blau et al., 2013; Goldin et al., 2017). Given the high wage and employment growth in this sector, technology may be a growing source of income inequality overall (Krueger, 1993; Acemoglu and Autor, 2011).

Second, by choosing this industry, we bias our study towards finding smaller differences between experimental conditions. The technology sector features well-documented labor shortages and high-levels of competition between employers for qualified workers. Technology executives regularly lobby Congress for expansions to the H1-B visa program to address the undersupply of software developers. Furthermore, there is a particular shortage of female software engineers, as such one might expect in all our experimental conditions that female applicants might be offered

using the 5-Year (2012-2017) American Community Survey Public Use Microdata. There are approximately 115 human resource specialists in the software industry in this sample. They are approximately 20% male, 75% white.

[18]The details of how we created candidates is contained in Appendix C, however one of the most important attributes of our candidates are their current salaries (which they either disclose or don't disclose). For this we used data from Payscale.com because it offers the ability to see the distribution of salaries by company, job roles, city and level of experience (a level of granularity that is not publicly available from sources like the CPS, BLS, Census or Glassdoor.com). Data from websites like Payscale.com and Glassdoor.com are self-reported by workers who visit these websites, which could mean these data are inaccurate. However, Glassdoor has periodically compared their data to that provided by the Census and they've found the distribution of base salaries reported are quite similar (Glassdoor, 2019).

[19]We chose to examine the market for engineers with moderate experience so that our candidates had a previous wage history that could (or could not) be disclosed.

more call-backs and higher salary offers.[20]

These factors should erode the effectiveness of interventions. This is particularly true for salary bans. Behavioral economics phenomena such as "framing" and "anchoring" are often used to justify salary bans. Other papers have found such behavioral biases are disciplined by competition and incentives like those in the technology market. Effects in other, less competitive sectors may be stronger.

Lastly, the labor shortage creates an additional research feature: Firms routinely fail to meet hiring objectives. Firms in this sector are generally interested in hiring multiple qualified candidates wherever possible. As the H1-B lobbying shows, hiring is limited not by demand, but by supply of qualified workers. As a result, we can measure how bans affect the total number of selected candidates, in addition to how the composition of selections and salaries change

4.4 Experimental Treatments

The primary goal of our experiment was to test the effect of salary history bans on hiring outcomes. To do this we use our two-sided audit design which varies both attributes of the candidates that are sent to our recruiters and attributes of the firm the recruiters are reviewing the candidates for. The main unit of randomization in our study is the recruiter. Each of our 112 recruiters was assigned to a single treatment.

Main Treatments We have three main treatments, and a number of sub-treatments within each main treatment. Treatments were randomized across recruiters. They are: (1) Banned/8 Non-Disclosers, (2) Mandatory/0 Non-Disclosers, (3) Optional/4 Non-Disclosers.

Banned/8 Non-Disclosers: In our "8 Non-Disclosers" treatments, recruiters were shown eight completed job applications in which no salary history information appears in any way. The question did not appear on the job application.

Mandatory/0 Non-Disclosers: In our "0 Non-Disclosers" treatments, recruiters were shown eight completed job applications in which salary history information was solicited and provided for all eight candidates. The job application explicitly solicits salary history and states that candidates are required to answer.

Our 0 Non-Disclosers treatment serves multiple purposes. First: Some employers may truly require all applicants to disclose. In addition, full revelation through unravelling may be an equilibrium outcome, even if disclosure is purportedly optional (Milgrom, 1981; Cullen and Pakzad-Hurson, 2016).[21]

Optional/4 0 Non-Disclosers: In the "4 0 Non-Disclosers" treatments, four of the eight candidates disclosed their previous wages. We varied exactly which candidates disclosed according to several

[20] Also our recruiters are sent eight applicants which are evenly split between male and female names, whereas the software industry as a whole is male-dominated. This may make our recruiters, in all experimental conditions, more likely to think the employer is actively hoping to hire more female workers.

[21] The authors are unaware of any data about how many employers or jobs require salary history disclosure. We know from survey evidence that 30-43% of job seekers said that an employer asked for their previous salary information, but we do not know if that question was mandatory (Barach and Horton, 2017; PayScale, 2017).

subtreatments (reviewed in Table 2). For half of the packets sent out in this treatment it was clear that the disclosures were in response to an optional question about salary history, while for the other half the salary disclosure was unprompted. The unprompted disclosures were made in the "Additional Skills and Information section of the application form. It is our understanding that most of the salary ban laws would not allow a firm to ask for previous wages even if the question was optional.

Figure 1 shows how a single candidate (Jessica) would appear to different recruiters who were assigned to each of our treatment arms. While Table 1 shows how that single candidate fits into the larger set of eight candidates each recruiter is sent across the main treatment arms.

Sub-Treatments Nested within our main treatments are sub-treatments. Our sub-treatment introduce variations on two additional dimensions. First: We introduce variation in which of the eight candidates disclose (in packets with a mixture of disclosure decisions). This allows us to estimate how one candidate's disclosure decision affects others candidates.

Second: Our sub-treatments vary the amounts of disclosed salaries, both for individual candidates and for the group as a whole. We implement sub-treatments in the about by alter whether candidates' disclose at the 25th or 75th percentile of their current job (using the Payscale data described above). This allows us to measure how the effects of disclosure depend on the amount disclosed by the candidate. This also allows us to estimate how the *level* of one candidate's disclosure affects others candidates.[22]

We set up sub-treatments that were meant to explore how the effect of disclosures vary by, for example, if mostly high salaries are disclosed versus mostly low salaries are disclosed.[23]

4.5 Randomization and Covariate Balance

Recruiter Characteristics Balance Our study randomized applicants' salary information at the recruiter level. Prior research suggests that hiring decisions differ according to managers' characteristics.[24]

As such, we implemented a randomization procedure to guarantee covariate balance on recruiter characteristics such as race and gender across treatments.[25] This effectively implemented stratified

[22]For example, one might suspect that a recruiter would react differently to seeing these four previous salaries [72,000 at Dell; 84,000 at SAP; 93,000 at IBM; 97,000 at Cognizant] instead of these four [94,000 at Facebook; 115,000 at Oracle; 116,000 at Apple, 125,000 at Amazon]. In the second set of salaries it appears that most people who disclose have a fairly high salary, which could imply that those who don't disclose are choosing to keep a previously low salary private. In the first set, that doesn't seem to be the case.

[23]To understand if a salary is high or low it is important to think about which previous firm the salary comes from. Cognizant has a median salary of around 73,000 per year, while Facebook has a median salary of around 119,000 per year according to Payscale.com. So in the two sets of salaries mentioned above a candidate who makes 97,000 at Cognizant is actually high within the firm, while a candidate that makes 94,000 at Facebook is low within the firm. Over 75 percent of recruiters in our experiment stated they used websites like Payscale.com or Glassdoor.com while evaluating the candidates, so we believe they were familiar with the general range of salaries differ by firm.

[24]For example, Giuliano et al. (2009) report that non-black managers hire more white workers and fewer black workers. (Dee, 2005) find that educators evaluate students of the opposite gender more harshly

[25]We describe the randomization technique in detail in the Appendix A.

randomization, guaranteeing that (for example) male recruiters were not over-assigned to one particular experimental arm by accident. Table 3 shows our stratification procedure succeeded; the demographics are balanced across our main treatments. No mean differences between our treatments approach traditional levels of statistical significance for pairwise t-tests.

Candidate Balance We balanced candidate characteristics by sending the same application materials to all treatment arms.

We created a set of four recruiting packets, each of which contained eight candidate biographies on a standard job application.[26] Each packet was sent to all treatment arms twice (once to two separate recruiters). This is a form of a blocked or paired design (Athey and Imbens, 2017) that guarantees balance. The design has attractive efficiency properties that allow controls for "Biography Fixed Effects."

By using the blocked design, the candidate attributes are balanced across our main treatments and sub-treatments by construction. The only candidate attributes that differ by treatment are how many salaries are disclosed and the amounts of the salaries disclosed. Summary statistics for candidate characteristics and balance can be seen in Table 3.

4.6 Outcome Variables

Each of our 112 recruiters was tasked with evaluating a set of eight candidates. The recruiters' evaluations were done via an online form where we asked them a number of question. Recruiters were not told what specific questions they would be asked ahead of time, and each group of questions was on a separate page that did not allow subjects to revise previous answers. Below we describe the main outcome variables.

Callback Similar to a traditional audit study, our recruiters told us whether we should call back each of the eight candidates. Recruiters were informed that we were not hiring for a single position, so they should feel free to call back as many of the eight candidates who they deemed a good fit for the position.

Offer We asked recruiters to make a take-it-or-leave-it base salary for each job candidate (even if they did not believe the candidate should be called back).[27] Hall and Krueger (2012) find that two-thirds of workers report believing that the offer they were made by an employer was a take-it-or-leave-it-offer. This was an open response question.

Valuation According to a survey run by Barach and Horton (2017) the firm makes the first offer about 60% of the time, but about 40% of the time a job candidate makes a first offer, so we also asked our recruiters for their valuation of each candidate by asking what was the maximum offer

[26] A biography is a name, previous job, 2nd job, internship, school, graduation year etc.

[27] We asked specifically about the annual base salary of compensation only, though we said We also offer benefits including health insurance, stock, and performance-based annual bonuses without specifying their amounts.

from the candidate that the firm should accept. We asked recruiters for the maximum take-it-or-leave-it-offer a firm should accept from these candidates.[28] One can think of this as the firm's valuation of the candidate since a firm should be willing to pay a worker up to the value that worker brings to the firm.

The three variables above (callback, offer and valuations) were the first questions asked to recruiters. They are natural questions to discuss when hiring anyone. These questions were also were explicitly used in each recruiter's bonus formula (and many counterpart bonus formulae in the real world). The recruiters therefore faced monetary incentives to set these numbers correctly.

After these questions, we asked for additional assessments about each candidate that were not used in the bonus formula. Although recruiters did not face formal contractual incentives to answer these numbers correctly, they still face reputational incentives for accurate answers.

Reservation Wage For each candidate, recruiters were asked for job offers that the candidate would be highly likely to accept, highly *unlikely* to accept and indifferent about accepting. We specifically defined these terms to mean 95% probability, 50% probability and 5% probability. We average these and refer to the averages as the candidate's *reservation wage* for this job in the emprics below.

Number and Source of Outside Offers Lastly, we asked recruiters to estimate how many competing offers each candidate would receive during his or her search from other employers. To simplify this task, recruiters could chose either "zero or one," or "two or more." Prior academic research suggests that outside offers increase the bargaining power of the candidate (Blackaby et al., 2005), and that most employed workers rarely receive more than one job offer at a time during an on-the-job search. We also asked whether competing job offers from each candidate would come from the candidate's own search efforts or from other employers' search effort to identify and recruit such a candidate.

5 Summary Statistics and Empirical Strategy

Table 4 displays average outcomes by treatment and gender for our 7 main outcomes of interest described in the previous section: probability of callback, wage offer suggested by the recruiter, valuation, the interaction of callback with wage offer or valuation, reservation wage, and whether the recruiter expected the number of outside offers to be greater than 2. In Panel B we also break down by whether the individual's salary was disclosed.

As outlined in the previous section, our experiment featured 3 main treatment arms (8 Disclosures, 4 Disclosures, and 0 Disclosures) x 2 genders (male and female) x prompted or not prompted (within 4 Disclosures). In addition there is variation in the amount of the salary disclosed both for the candidate and those around him/her. This allows us to test several different hypotheses of in-

[28] Recruiters could choose a maximum between \$20,000 to \$200,000 in \$10,000 increments. One can think of this as asking should the firm be willing to pay \$20,000, \$30,000 etc. up to \$200,000. Recruiters did not appear to feel constrained by this range since the minimum valuation was \$60,000 and the maximum valuation was \$180,000.

terest, but also presents a challenge presenting the results in parsimonious manner. We will handle this in a few different manners.

We consider both individual-level outcomes (each observation is a candidate) and recruiter-level outcomes (each observation is a recruiter/packet).

For our main recruiter-level results, we present regressions with "proportion not disclosing" as the main independent variable of interest. Proportion not disclosing = 1 for 0 Disclosures/Banned, = 0.5 for 4 Disclosures/Optional, and = 0 for 8 Disclosures/Mandatory.

$$Y_i = \alpha + \beta_1 PropNotDisclosing + \epsilon \tag{1}$$

Where Y_i is one of the main outcomes of interest, i.e. TIOLI wage offer. This allows us to quickly summarize the impact of our main treatment without running several different tests of significance. However, it does imply a strong assumption of linearity in the number of individuals disclosing and combines the two forms of 4 Disclosures into one. Looking at Table 4, the assumption of linearity does not seem that far off. Similarly, in general the two forms of 4 Disclosures do not have statistically significantly different results on outcomes. However, given the suggestive differences on interview results for women in Table 4 we will, in a future draft, explore the potential differential impacts of prompted and unprompted within the 4 Disclosures treatments.

We also consider individual level regressions, that explore the impact of an individuals own disclosure on their outcomes. However, just including disclosure ignores the fact that the recruiter was looking at 8 applications at once, and that the characteristics of other applicants may impact individual outcomes. So we additionally test for spillover effects by adding in the number of other non-disclosers in the packet.

$$Y_i = \alpha + \beta_1 SalaryNotDisclosed + \beta_2 NumOtherNonDisclosers + \epsilon \tag{2}$$

We will also look at the amount of salary the individual discloses and how that impacts our various outcomes, by replacing $SalaryNotDisclosed$ with $AmountofSalaryDisclosed$ and including an additional spillover term for the average amount that others in the packet disclose and including biography level fixed effects.

One major goal of these bans is reduce inequality in wages. To test this, at the recruiter level, we construct a recruiter-level Gini-coefficient for our various outcomes and additionally use this as a dependent variable in our regressions. We standardize our Gini-coefficients to have a mean 0 and standard deviation of 1 to ease interpretation.

Standard errors in all regressions are clustered at the recruiter level.

6 Results

We begin by examining average recruiter-level and individual-level outcomes in our experimental conditions, and then turn to gendered effects and implications of disclosure for inequality.

Table 5 shows recruiter-level results from Equation 2 in Panel A and individual-level results for our various outcomes of interest. In Column 1 of Panel A, we see that when salaries are banned (i.e. 100% not disclosing), recruiters report believing they need to pay $11,935 less per candidate on average for the same probability of acceptance, in reservation wages per candidate, an 11.5% decrease over when 8 salaries are disclosed (i.e. 0% not disclosing). When no one is allowed to disclose their salary, they seem to believe that workers will require lower salaries for an equivalent probability of accepting an offer.

Why do employers feel they need to pay less for the same probability of accepting an offer when salaries are banned? Theory offers two main reasons. The first is that disclosure affects beliefs about the number and aggressiveness of competing offers for workers from rival employers. Alternatively, disclosure may affect a firm's beliefs about a worker's value. In Column 2 of Panel A, we directly measure recruiter beliefs about competing offers, and find no statistically significant impact on beliefs about outside offers.[29] In Column 3, we show that banning disclosures appears to decrease firms' average valuation (maximum salary to accept from a candidate). Average valuations are approximately $6,500 lower per worker in banned than in 8 Disclosures (a 6% decrease). When all salary histories are withheld, average salary offers decrease by about $7,200, a 7% decrease over when all salaries are disclosed.

Despite higher valuations, we detect no effects on the overall amount of callbacks (Table 5). Our experiment gave recruiters flexibility to choose the quantity of candidates hired without a cap. However, the recruiters declined to callback more candidates in response to a greater number of salary disclosures. We revisit this result next.

The results in Panel A above are mostly mimicked in the individual level regression in Panel B - the costs and benefits of disclosure primarily accrue to the applicants doing the disclosing (Table 5, Panel B). The main qualitative difference is in number of outside offers, where in the individual level regression *not* disclosing one's salary decreases the probability the recruiter thinks you will have several outside options. We also run several tests for whether one applicant's disclosure affects others, and we find little evidence for spillovers between candidates' choices to not disclose.

As mentioned above, we detect no statistically significant effects on the total quantity of callbacks. In Table 6, we examine the components of the callback decision discussed theoretically in Section 3 above.

Here we find several results suggesting that non-disclosure makes callbacks more attractive for the employer. Previously in we saw that non-disclosure lowered employers' valuation and their expected salary offers for candidates. In Column 1 of Table 6 we see that difference between these is (on average) unchanged by disclosure. However, Column 2 and 3 suggest that recruiters believe non-disclosing candidates are more likely to accept the employer's offer. The implication is that the employer expects greater yield from callbacks to non-disclosing candidates, even though these candidates will be receiving lower offers.

The higher expected yield should also make callbacks to non-disclosing candidates more attractive (because they yield a higher probability of accepting offers while delivering the same value above salary). Our average estimates about the overall level of callbacks do not detect effects.

[29] If anything, bans appear to slightly increase the recruiters beliefs that the average worker has multiple competing offers.

For candidates who disclosed, our experiment also included random variation in the dollar amounts of the disclosures. In Table 8 we look at how the *amount* of the disclosed salary affects outcomes for the candidates. On average, every $1 higher salary disclosed by an applicant increases his/her salary offer by $0.69, and increased valuations by $0.66 (Table 8). Similar to the act of disclosure itself, the disclosure amount does not appear to impact the probability of getting an callback. The increased offer seems to come from an increase in the amount the recruiter believes the candidate needs to receive for an equivalent probability of saying yes, and the not the number of outside of offers.

These regressions also include the average amount disclosed by others in the packet (as well as the number of other non-disclosers in the packet) to try to capture spillover effects at this level. Like the choices to disclose at all, the dollar amount listed mainly affects the candidate in question and does not spillover onto others (results omitted in table for space).

Importantly, the results we find about offers and valuations are robust to interactions with candidates' callback outcomes. In theory, the benefits of higher valuations could be inaccessible to some candidates if they could not obtain a callback. However, we find that the product of callbacks and valuations/offers also decreases with less disclosure (Table A1). Although the magnitudes of decreases differ slightly between candidates who received a callback (and not),[30] the sign and overall level are the same. Our findings about higher dollar amounts are robust to interactions with callback outcomes (see Table A2).[31]

6.1 Inequality Results

As we discussed in Section 2, the salary ban laws directly targeted inequality, and in particular salary inequality on the basis of gender. Our previous results showed that on average valuations and salary offers were higher when disclosure was permitted, but has not yet addressed how disclosure, or lack thereof, impacts inequality of outcomes between candidates. In Table 9, we show results using standardized Gini coefficients for our various outcomes as dependent variables. Banning salary histories decreases inequality in reservation wages, valuations, and offers, by .8 to 1.3 standard deviations depending on the outcome. There is no change in inequality about employer beliefs about the number of competing offers a candidate has on average.

This reduction in inequality via banning salaries does not tell the whole story, however. For that, we want to look at how disclosure and the disclosure regime impacts men and women differently. In Table 10 Panel A, we differentiate the impact of individual salary disclosure between men and women, and in Panel B we consider the total impacts net of any potential spillovers from other disclosures, similar to Table 5 Panel B. In Panel C we consider the impact of our outcomes interacted with whether the candidate actually got a callback from the recruiter.

We find divergent effects depending on our outcome measure. On the one hand, banning salary disclosure harms female representation in callback selections. Not disclosing a salary reduces the probability a female candidate is interviewed versus a male candidate by 0.12 percentage points (a 19% decrease over male non-disclosers). Salary offers, however, are not as harmed for women as

[30] Non-disclosure actually decrease valuations more for non-callback candidates.

[31] The effects are stronger for candidates selected for a callback. $1 increases in dollar amounts increase the product of Callback $\times$ valuation by $0.41).

they are for men when they do not disclose (Column 5).

These results suggest that bans fail to advance a major policy goal: Improving average outcomes for women. To the contrary, our results suggest that enabling disclosure, not banning, improves average outcomes for women. Disclosure benefits women partly because of the disproportionately positive effect of disclosure on callbacks for female applicants – a mechanism mostly unexplored in the policy debate about bans. Disclosure gives women a credible tool for signaling their market value.

Yet, the ban does achieve one of it's goals: reducing the average ratio of male and female salaries (i.e., the gender wage gap) and the equivalent ratio for worker valuations. As we see in Table 5 Panel B, across most outcome measures, bans harm average female outcomes. However they harm male outcomes even more, leading to a reduction in the gender gaps.

Banning salary disclosures makes salary offers more equal between men and women. However, this appears to come at the expense of the overall *level* of wages, including median female wages (both for women receiving a callback and not).

Our results about the gender wage gap come with a significant caveat. As previously mentioned, the benefits of higher valuations and salary offers could be inaccessible to candidates who fail to obtain a callback. For this reason, in Table 10 Panel C we combine our measures together by multiplying callback outcomes with salary offers. This gives credit to pro-disclosure policies for selecting a greater number of women, which can be balanced against anti-disclosure policies' greater decrease of male salaries (compared to the decrease in female salaries). After this adjustment, the results about the gender wage gap go the *opposite* direction. Banning increases gender inequality for those who got a callback.

These results point to the importance of *jointly* evaluating wages and workforce composition. Do salary bans reduce or increase gender inequality? The answer hinges on how we assess the increased representation of women overall (Table 10). Researchers who examine only wages – and not representation or workforce composition effects – may find differing and possibly misleading results.

Our theoretical framework above implied that employers would be more likely to interview a candidate if the "margin" (i.e. valuation - salary) was higher, or if their probability of acceptance was higher (which will happen the as the their reservation wage is further from their offer/valuation). In Table 11 we assess these various components to try to understand why bans are reducing interview rates for women but not for men. In these tables we can see that recruiters believe female margins are much lower than they do for a man if a woman does not disclose her salary.

As we show shortly, beliefs about greater reservation wages lead to higher salary offers under disclosure. It is not necessary for workers to *accept* these higher salary offers in order for them to benefit job applicants; higher salary offers place workers in a stronger bargaining position with whichever firm they prefer to work.

Disclosure particularly increases beliefs about the origin of these outside offers. Non-disclosure makes recruiters believe outside offers are more likely to originate from a candidate's search efforts, rather than rival employers' poaching or recruiting (Table 7).

6.2 Potential Mechanism: Average Wage Reminders

One explanation for our result is that our recruiters in the 0 Disclosures treatment systematically underestimate the overall wage level for these jobs. The primary mechanism for the results therefore come from a simple reminder of overall wage averages. Several aspects of our experiment allow us to directly measure this hypothesis. As we describe below, our findings are generally robust to our tests for this factor.

First, our packets include experimental variation not only in the number of disclosures, but also the average dollar amounts disclosed (for those doing the disclosing).[32]

These introduced significant variation in the overall salary levels, ranging from $84,000 to $110,000. If our effects were driven by reminders of the overall average wage levels, we should expect this variation to significantly impact valuations and wage offers.

In Table A2, we assess this hypothesis and find little evidence that this matters. Our findings about the effect of bans are broadly robust to controls for the average wage levels of disclosures in the 8 Disclosures/4 Disclosures counterfactuals.

In addition, recruiters do not idly conjecture overall wage levels when disclosures are banned. Instead, they actively seek out information, compensating for the blindness induced through the ban. This possibility was first raised and documented by Barach and Horton (2017) who note that compensating behavior could undo the entire purpose of the ban.

We find similar results in our setting. We asked recruiters whether they sought such outside information, for example, by browsing websites such as PayScale.com or Glassdoor.com that contain wage data about the candidates' employers. These websites were actually used in the design of this experiment – in a sense, browsing them literally gives the recruiter the same information about "average levels" (both in the market overall and at each candidate's incumbent employer).

In Table 12, we find that recruiters randomly assigned to the 0 Disclosures condition were 19 percentage points more likely to report visiting such a website, compared to workers randomly assigned to our 8 Disclosures condition. We also find they are 25 percentage points more likely to report consulting a colleague or associate for help (Table 12). Taken together, these results suggest that the higher wages in our 8 Disclosure treatments are not caused by reminders of the overall wage level.

If reminders of wage levels are *not* responsible for these results, what are? Theoretical work by Cullen and Pakzad-Hurson (2016) suggest that workers will require higher salaries when disclosure is mandatory (or is practically mandatory as a result of unraveling). Accepting lower wages in their model not only reduces today's income, but also allows future employers to price discriminate more effectively. As mentioned above, our data confirm that employers indeed expect workers' reservation wages increase with disclosure.[33] However, we cannot measure whether this

[32] Table 2 contains a list of our subtreatments. Of these, the following subconditions featured particularly high salary levels for all disclosing candidates: 8 Disclosures-AllHigh and 4 Disclosures-MoreHigh. The following sub-treatments featured particularly low salary levels for all disclosing candidates: 8 Disclosures-AllLow, and 4 Disclosures-MoreLow. The following were in the middle: 8 Disclosures-Mixed, 8 Disclosures-FlipMixed, and 4 Disclosures-Mixed.

[33] Because our candidates are fictional, we don't know if they actually do.

finding is driven by beliefs about other employers' price discrimination.[34]

Our results suggest an additional possible mechanism: Disclosure makes employer competition more salient to the employer. Non-disclosure negatively affects beliefs about how aggressively rival firms will pursue a candidate (Table 7). By contrast, non-disclosure *increases* beliefs about how hard the candidate is searching. Non-disclosure creates the impression that candidates – rather than rival firms – are leading the courtship necessary for an outside offer.

Disclosure also increases beliefs about the total number of competing offers. This appears to be the one outcome variable in our study exhibiting spillovers between candidates. Table 5 shows that applicants' disclosure decisions affect beliefs about how non-disclosing candidates' number of competing offers.

These results suggest that disclosure increases wages by reminding employers they are not evaluating candidates in a monopsonistic setting, but alongside competitive rivals. Given recent evidence about employer market power (Boal et al., 1997; Manning, 2003; Ashenfelter et al., 2010; Naidu et al., 2016; Azar et al., 2017; Dube et al., 2018; Berger et al., 2019), perhaps it is unsurprising that employers need a reminder that other employers could theoretically compete for labor, and that individual firms are not gatekeepers to employment in an entire occupational$\times$geographic unit. Nonetheless, it is particularly striking that we find these employer competition results in technology, a sector well-known for intense employer competition and a (purported) skilled labor shortage requiring H1-B visas.

7 Effects of the Ban

Our paper so far has examined the effects of disclosure. The goal of the ban is to suppress such disclosure. However, any analysis of a ban must account for the possibility of non-compliance. Salary history bans forbid employer prompting, but do not forbid voluntary disclosure. Some workers may volunteer current salaries under a ban, even without prompting. Similarly, some workers may refuse to provide current salaries, even when asking is legally permitted.

Any analysis of the effects of the ban must make assumptions about whose disclosure choices would actually change under the ban. In this section, we combine evidence from prior theory, empirical studies, surveys of the public and original data collection to to estimate the effects of bans. For exposition, we introduce the terminology below to refer segments of the workers in our experiment.

- "**Always-disclosers**." Disclose salary history voluntarily, even if not prompted.
- "**Never-disclosers**." Refuses salary history discloses, even when prompted.
- "**Ban-compliers**."Disclose when prompted, don't disclose when not prompted.
- "**Ban-defiers**." Disclose when not prompted, don't disclose when prompted (probably doesn't exist).

[34]These would be third order beliefs: Recruiters beliefs about candidates' beliefs about future employers' beliefs.

Public discourse around bans emphasizes compliers. However, the ban may also have significant consequences for other groups. The presence of the prompt changes the signaling content of disclosure choices, even if the choices themselves aren't changed. Choices to disclose could signal distinct latent characteristics that depend on the circumstances of the disclosure.

More specifically: Under the ban, always-disclosers reveal themselves as forward and aggressive by disclosing without prompting. However, the pre-ban hides this signal about always-disclosers by providing the prompt. An always-discloser could be mistaken as compliant, rather than forward.

Similar issues affect never-disclosers. Before the ban, never-disclosers reveal themselves as refusing direct requests (possibly for strategic reasons). However after the ban, never-discloser could be mistaken as conforming or overlooking the disclosure possibility (since the application did not specifically ask).

For each candidate in our data, the experiment contains the variation necessary estimate the effects of prompting, no matter which category of compliance the candidate would fall into. Below, we begin by estimating the effects of prompting for all four compliance types using all candidates. Then, we use additional data to these

results not finished...

8 Conclusion

This paper assesses salary history bans empirically. We introduce a novel experimental design – a *two-sided audit* – which allows us to vary characteristics of job applicants and of firms, while soliciting evaluations of these candidates from a set of real-world recruiters. This methodology could permit researchers to examine the impact of a wide variety of interventions that alter firm hiring practices.

We then apply this field experiment to examine the impact of a new series of labor market laws, adopted by 11 states and 10 jurisdictions, that ban employers from asking about salary histories. We find evidence that salary history bans tend to suppress wages, both overall and for women specifically. We find mixed results about what this does to the gender wage gap. Banning disclosure decreases male salaries more than female salaries, although this may only affect workers who are hired. Banning disclosure also lowers female callback rates, without affecting men's.

The overall effect of bans on the gender wage gap hinges on how we assess the change in female representation. These results point to the importance of *jointly* evaluating wages and workforce composition. The merits of bans depend on how policymakers weigh tradeoffs between greater income, representation and equality for workers and job applicants. In future versions of this paper, we hope to offer additional results and details about the mechanisms behind our findings.

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Tables and Figures

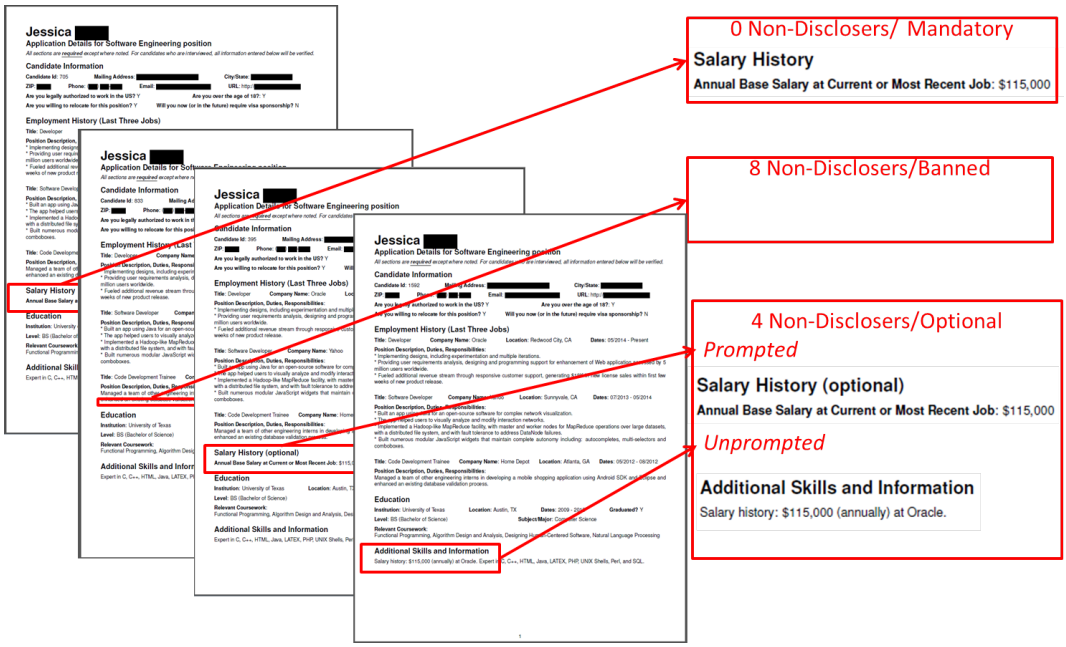


Figure 1: Variations on a single candidate across the main treatment arms

Table 1: **Variations on a packet of eight candidates across main treatment arms**

Name	**Previous Employer**	Prompted		Unprompted	
		All	**Half**	**None**	**Half**
Andrew	Cognizant Technology	97,000			
Christopher	Apple	116,000			
Emily	Dell	72,000	72,000		Current base salary: 72,000 yearly.
Jessica	Oracle	115,000	115,000		Salary history: 115,000 (annually) at Oracle.
Joshua	SAP	84,000	84,000		My current annual base salary is 84,000.
Samantha	IBM	93,000			
Sarah	Facebook	94,000			
Tyler	Amazon	125,000	125,000		I make about 125,000 per year right now (pre-bonus).

Notes: There were a total of 112 screeners and each screener was assigned to see a single packet of eight candidates. This table shows how those eight candidates would vary across four screeners.

Table 2: Description of Sub-Treatments

Sub-treatment	**Description**
Mandatory-AllHigh	All eight candidates disclose a salary at the 75th percentile of his/her current employer.[1]
Mandatory-AllLow	All eight candidates disclose a salary at the 25th percentile.
Mandatory-Mixed	A mixture of four 75th (two male, two female, randomly selected) and four 25th percentile disclosures (two male, two female, randomly selected).[2]
Mandatory-FlipMixed	Each candidate who disclosed at the 75th above (Mandatory-Mixed) now discloses the 25th percentile (and *vice-versa*). This was evaluated $2\times$.[3]
PromptOpt-Mixed	Takes the Mandatory-Mixed packet and discloses only four salaries: the maximum male/female salaries, and the minimum male/female salaries.
PromptOpt-MoreHigh	Takes the Mandatory-Mixed packet and discloses only four salaries: Three from above the median salary and one from below the median.
PromptOpt-MoreLow	Takes the PromptOpt-MoreHigh packet. Each candidate who used to disclose now doesn't, and each candidate who didn't disclose now does.
UnpromptOpt-Mixed	Same as PromptOpt-Mixed but without a prompt for salary on job application.
UnpromptOpt-MoreHigh	Same as PromptOpt-MoreHigh but without a prompt for salary on job application.
UnpromptOpt-MoreLow	Same as PromptOpt-MoreLow but without a prompt for salary on job application.
Banned	None of the eight candidates disclose a salary.

Notes: 1. See Section 4.2 for additional details about how we created candidate biographies.

2. For "0 Non-Disclosures-Mixed," the specific mixture of salaries was as follows: two people (one male, one female) disclose at the 75th percentile from a higher wage firm, two people (one male, one female) disclose at the 75th percentile from a lower wage firm. The remaining four people disclosed at the 25th percentile at their firm.

3. 0 Non-Disclosures-FlipMixed was evaluated $2\times$ in order to maintain balance in the average disclosed salaries across main treatment arms.

Table 3: Characteristics Balance Across Treatment Arms

Panel A: Recruiter Characteristics and Covariate Balance

	Prompted All Disclose	Prompted Half Disclose	Unprompted Half Disclose	Unprompted None Disclose	Overall
Female	0.75	0.71	0.75	0.79	0.75
White	0.42	0.54	0.46	0.62	0.50
Black	0.23	0.33	0.38	0.21	0.28
3+ Years of Exp	0.70	0.71	0.62	0.62	0.67
Exp Salary Input	0.97	0.96	1	1	0.98
Hourly Rate	41.7	43.4	43.1	43.0	42.6

Panel B: Candidate Characteristics

	Mandatory	Prompted	Unprompted	Banned	Total
female_candidate	0.50	0.50	0.50	0.50	0.50
School.Grad.Year	2013.7	2013.7	2013.7	2013.7	2013.7
Second.Job.Length	329.1	329.1	329.1	329.1	329.1
Salary Disclosed = 1	1	0.50	0.50	0	0.57

Notes: In Panel B, the mean salary disclosed was 0.6% higher in 4 Disclosures ($97,718.75) than in 8 Disclosures ($97,050). This is due to the randomization of which salaries were disclosed in the 4 Disclosures treatments. This difference is not statistically significant using one observation for each candidate.

Table 4: Descriptive Statistics: Candidate Outcomes

Panel A: Candidate Outcomes by Treatment Arm & Gender

	Overall	Banned	Unprompted Optional	Prompted Optional	Mandatory
Female Candidates					
Res. Wage	94,832.14	90,126.39	94,490.62	95,923.61	97,205.62
Offers$\geq$2	0.50	0.52	0.49	0.45	0.54
Valuation	102,812.50	100,520.83	104,479.17	101,145.83	104,187.50
Callback	0.64	0.60	0.71	0.54	0.68
Offer	96,599.21	93,900.50	97,802.08	96,322.92	97,662.50
CB $\times$ Offer	65,049.71	61,356.46	72,854.17	55,843.75	68,106.56
CB $\times$ Valuation	69,508.93	66,041.67	78,229.17	58,645.83	72,875.00
Male Candidates					
Res. Wage	102,354.90	93,158.81	98,708.85	102,364.58	110,054.38
Offers$\geq$2	0.54	0.60	0.55	0.53	0.51
Valuation	108,750.00	104,479.17	106,875.00	106,875.00	113,562.50
Callback	0.62	0.61	0.64	0.65	0.59
Offer	102,423.00	96,549.53	101,541.67	100,916.67	107,379.69
CB $\times$ Offer	66,283.11	62,721.72	68,541.67	67,953.12	66,062.81
CB $\times$ Valuation	70,647.32	68,020.83	72,395.83	71,770.83	70,500.00
Total					
Res. Wage	98,593.52	91,642.60	96,599.74	99,144.10	103,630.00
Offers$\geq$2	0.52	0.56	0.52	0.49	0.52
Valuation	105,781.25	102,500.00	105,677.08	104,010.42	108,875.00
Callback	0.63	0.61	0.67	0.59	0.63
Offer	99,511.11	95,225.02	99,671.88	98,619.79	102,521.09
CB $\times$ Offer	65,666.41	62,039.09	70,697.92	61,898.44	67,084.69
CB $\times$ Valuation	70,078.12	67,031.25	75,312.50	65,208.33	71,687.50

Panel B: Candidate Outcomes by Disclosure & Gender

	Overall	Banned	Unprompted Optional	Prompted Optional	Mandatory
Female Discloser					
Res. Wage	97,171.61		97,125.69	97,104.17	97,205.62
Offers$\geq$2	0.55		0.62	0.50	0.54
Valuation	104,726.56		108,125.00	103,125.00	104,187.50
Callback	0.68		0.79	0.58	0.68
Offer	98,480.47		101,041.67	98,645.83	97,662.50
CB $\times$ Offer	69,591.99		83,125.00	61,010.42	68,106.56
CB $\times$ Valuation	74,296.88		89,375.00	63,958.33	72,875.00
Male Discloser					
Res. Wage	108,989.97		105,730.56	108,701.39	110,054.38
Offers$\geq$2	0.54		0.62	0.56	0.51
Valuation	112,851.56		113,333.33	110,000.00	113,562.50
Callback	0.61		0.67	0.60	0.59
Offer	106,940.43		107,458.33	104,958.33	107,379.69
CB $\times$ Offer	68,050.98		76,375.00	66,354.17	66,062.81
CB $\times$ Valuation	72,382.81		81,250.00	69,791.67	70,500.00
Female Non-discloser					
Res. Wage	91,712.85	90,126.39	91,855.56	94,743.06	
Offers$\geq$2	0.45	0.52	0.35	0.40	
Valuation	100,260.42	100,520.83	100,833.33	99,166.67	
Callback	0.58	0.60	0.62	0.50	
Offer	94,090.88	93,900.50	94,562.50	94,000.00	
CB $\times$ Offer	58,993.33	61,356.46	62,583.33	50,677.08	
CB $\times$ Valuation	63,125.00	66,041.67	67,083.33	53,333.33	
Male Non-discloser					
Res. Wage	93,508.14	93,158.81	91,687.15	96,027.78	
Offers$\geq$2	0.55	0.60	0.48	0.50	
Valuation	103,281.25	104,479.17	100,416.67	103,750.00	
Callback	0.63	0.61	0.60	0.69	
Offer	96,399.77	96,549.53	95,625.00	96,875.00	
CB $\times$ Offer	63,925.96	62,721.72	60,708.33	69,552.08	
CB $\times$ Valuation	68,333.33	68,020.83	63,541.67	73,750.00	

Notes:

Table 5: Recruiter-Level Outcomes: Average Outcomes

Panel A: Recruiter-Level Averages

	Res. Wage	Offers≥2	Valuation	Callback	Offer
Proportion of Applicants Not Disclosing $\in [0,1]$	-12,413***	.036	-6,634*	-.017	-7,375**
	(2,837)	(.066)	(3,731)	(.05)	(3,498)
Mean Dep. Var	98,594	.52	105,781	.63	99,511
Recruiters	112	112	112	112	112
Observations	896	896	896	896	896
R^2	.077	.011	.028	.023	.043

Panel B: Effects on Individuals (Net Spillovers)

	Res. Wage	Offers≥2	Valuation	Callback	Offer
Salary Not Disclosed	-9,065***	-.12**	-7,483***	-.052	-7,712***
	(1,413)	(.049)	(1,575)	(.053)	(1,473)
Num. Other Non-Disclosers in Packet	-478	.023**	121	.0051	48
	(404)	(.011)	(512)	(.0097)	(478)
Net Effect	-9543.57***	-.1**	-7361.7***	-.05	-7664.17***
Mean Dep. Var	98,594	.52	105,781	.63	99,511
Recruiters	112	112	112	112	112
Observations	896	896	896	896	896
R^2	.097	.02	.043	.024	.059

Notes: *Panel A*: Each observation is a candidate. The explanatory variable is the percentage of candidates who disclosed inside the recruiter's packet (a recruiter-level variable). Standard errors are clustered by recruiter.

Panel B: Each observation is a candidate. "Salary Not Disclosed == 1" refers to whether each candidate's salary was not disclosed. "Num. Other Non-Disclosers in Packet" refers to how many other candidates in the same packet disclosed (excluding each candidate's own disclosure). The "Net Effect" refers to the sum of the above two coefficients; that is, the effect of one person's disclosing on his own outcomes, minus average spillover from another candidate disclosing. Standard errors are clustered by recruiter. $p-$ values for the net effect were from an $F-$ test of the sum of two coefficients.

Table A1 examines the same regressions, with dependent variables interacted with each candidate's callback outcome.

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

Table 6: Recruiter-Level Outcomes: Average Outcomes Leading to Callbacks

Panel A: Recruiter-Level Averages

	Margin	Offer-Res. Wage	Value-Res. Wage
Proportion of Applicants Not Disclosing $\in [0, 1]$	741	5,038***	5,779***
	(781)	(1,917)	(2,065)
Mean Dep. Var	6,270	918	7,188
Recruiters	112	112	112
Observations	896	896	896
R^2	.041	.051	.054

Panel B: Effects on Individuals (Net Spillovers)

	Margin	Offer-Res. Wage	Value-Res. Wage
Salary Not Disclosed	229	1,353	1,582
	(399)	(1,044)	(1,116)
Num. Other Non-Disclosers in Packet	73	526*	599**
	(112)	(279)	(300)
Net Effect	302.47	1879.4*	2181.87**
Mean Dep. Var	6,270	918	7,188
Recruiters	112	112	112
Observations	896	896	896
R^2	.041	.051	.055

Notes: *Panel A*: Each observation is a candidate. The explanatory variable is the percentage of candidates who disclosed inside the recruiter's packet (a recruiter-level variable). Standard errors are clustered by recruiter.

Panel B: Each observation is a candidate. "Salary Disclosed == 1" refers to whether each candidate's salary was disclosed. "Num. Other Disclosers in Packet" refers to how many other candidates in the same packet disclosed (excluding each candidate's own disclosure). The "Net Effect" refers to the sum of the above two coefficients; that is, the effect of one person's disclosing on his own outcomes, minus average spillover from another candidate disclosing. Standard errors are clustered by recruiter. $p-$ values for the net effect were from an $F-$ test of the sum of two coefficients.

Table A1 examines the same regressions, with dependent variables interacted with each candidate's callbacks outcome.

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

Table 7: Expectations about Search Intensity of Candidate and Rival Employers

Panel A: Recruiter-Level Averages

	Candidate Pursues Rivals	Rivals Pursue Candidate
Proportion of Applicants Not Disclosing $\in [0, 1]$	.1**	-.076
	(.045)	(.053)
Mean Dep. Var	.83	.58
Recruiters	112	112
Observations	896	896
R^2	.043	.029

Panel B: Effects on Individuals (Net Spillovers)

	Candidate Pursues Rivals	Rivals Pursue Candidate
Salary Not Disclosed	.058*	-.1**
	(.034)	(.051)
Num. Other Non-Disclosers in Packet	.0064	.0035
	(.0075)	(.0096)
Net Effect	.06**	-.1**
Mean Dep. Var	.83	.58
Recruiters	112	112
Observations	896	896
R^2	.045	.034

Notes: For each candidate, recruiters were asked if the candidate would pursue offers from rival firms aggressively, or if rival firms would pursue the candidate aggressively, or both.

Panel A: Each observation is a candidate. The explanatory variable is the percentage of candidates who disclosed inside the recruiter's packet (a recruiter-level variable). Standard errors are clustered by recruiter.

Panel B: Each observation is a candidate. "Salary Not Disclosed == 1" refers to whether each candidate's salary was not disclosed. "Num. Other Non-Disclosers in Packet" refers to how many other candidates in the same packet disclosed (excluding each candidate's own disclosure). The "Net Effect" refers to the sum of the above two coefficients; that is, the effect of one person's disclosing on his own outcomes, minus average spillover from another candidate disclosing. Standard errors are clustered by recruiter. $p-$ values for the net effect were from an $F-$ test of the sum of two coefficients.

Table A1 examines the same regressions, with dependent variables interacted with each candidate's callback outcome.

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

Table 8: Dollar Amounts of Salary Disclosures

	Res. Wage	Offers$\geq$2	Valuation	Callback	Offer
Amount of Disclosed Salary	.77***	1.4e-06	.66***	8.3e-07	.69***
	(.047)	(1.5e-06)	(.055)	(1.5e-06)	(.05)
Biography Fixed Effects	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var	98,594	.52	105,781	.63	99,511
Recruiters	112	112	112	112	112
Observations	896	896	896	896	896
R^2	.55	.08	.36	.11	.41

Notes: Each observation is a candidate. All regressions include several control variables described below. Since these are used as controls, we have excluded them. However, the coefficients values can be reviewed in Table A2 display (Panel B). Table A2 also includes the same regression variables with all outcomes interacted with the callback outcomes.

All regressions include a dummy variable for whether the salary was disclosed and biography fixed-effects. In addition, all regressions include two variables capturing spillovers between candidates in the same packet. The first, "Num. Other Disclosers in Packet" refers to how many other candidates in the same packet disclosed (excluding each candidate's own disclosure). The second, "Salary Amount Spillover," refers to the average disclosed salary within the packet (excluding focal candidate's own salary). Standard errors are clustered by recruiter.

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

Table 9: Recruiter-Level Outcomes: Inequality between Candidates

	Gini(Res. Wage)$_\sigma$	Gini(Valuations)$_\sigma$	Gini(Offers)$_\sigma$
Proportion of Applicants Not Disclosing $\in [0, 1]$	-1.3***	-.84***	-1***
	(.2)	(.26)	(.24)
Mean Dep. Var (Undstandardized)	.077	.079	.08
Recruiters	112	112	112
Observations	896	896	896
R^2	.28	.14	.22

Notes: This table contains recruiter-level measures of inequality. To ease interpretation, the dependent variables are presented in normalized terms. We also report the average dependent variable in non-standardized terms for reference. Note: The variances in the "mean dependent variable" row are reported in units of 100,000.

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

Table 10: Gender Effects of Salary Disclosure Policies

Panel A: Effects of Disclosure by Gender

	Res. Wage	Offers≥2	Valuation	Callback	Offer
Salary Not Disclosed	-15,392***	.0037	-9,150***	.018	-10,105***
	(2,007)	(.055)	(2,601)	(.05)	(2,431)
Salary Not Disclosed × Female Candidate	9,554***	-.11*	4,499**	-.11*	5,510***
	(1,492)	(.065)	(1,853)	(.057)	(1,662)
Biography Fixed Effects	Yes	Yes	Yes	Yes	Yes
Recruiters	112	112	112	112	112
Observations	896	896	896	896	896
R^2	.32	.069	.2	.11	.23

Panel B: Effects by Gender, Net Spillovers

	Res. Wage	Offers≥2	Valuation	Callback	Offer
Effect of Non-Disclosure for Men (Net All Spillovers)	-15559.02***	-.03	-11049.14***	0	-12439.06***
Effect of Non-Disclosure for Women (Net All Spillovers)	-4479.24*	-.1	-3629.66	-.11**	-3178.3
Biography Fixed Effects	Yes	Yes	Yes	Yes	Yes
Clusters	112	112	112	112	112
Observations	896	896	896	896	896
R^2	.32	.08	.2	.11	.23

Panel C: Effects by Gender, Net Spillovers w/ Callback Interactions

	CB × Res. Wage	CB × Offers≥ 2	CB × Valuation	CB × Offer
Effect of Non-Disclosure for Men (Net All Spillovers)	-8315.73	-.01	-7391.79	-7602.89
Effect of Non-Disclosure for Women (Net All Spillovers)	-12655.18**	-.12**	-11877.65**	-10957.05**
Biography Fixed Effects	Yes	Yes	Yes	Yes
Clusters	112	112	112	112
Observations	896	896	896	896
R^2	.12	.094	.11	.11

Notes: For all regressions in this table, the unit of observation is a candidate. Standard errors are clustered by recruiter. All regressions include biography fixed effects.

The regressions in Panels B and C include additional variables. In the tables above, we include only the net effects of disclosure for men and women, which is a sum of several coefficients. We explain these individual coefficients below. To examine the individual coefficients of the net effects, see Appendix Table A3.

The additional controls in Panel B and C are: The number of women disclosures and the number of male disclosures in each candidate's packet (excluding his/her own disclosure, if he/she made one). These two variables were each interacted with a dummy for whether the candidate was himself/herself female. This allowed us to assess the effect of male disclosures on other men, male disclosures on women, female disclosures on other women and female disclosures on men. As Appendix Table A3 shows the size and statistical significance of these latter effects. They are generally negative, indicating that one candidate's disclosure does slightly "crowd out" others' outcomes. does slightly crowd out the others.

The net effect for men was equal to the effect of disclosing, minus the spillover effect of an additional male disclosure on men and the spillover effect an additional female disclosure on men. The net effect for women was equal to the effect of disclosing, plus the effect of disclosing for women (disclosed × female) minus the spillover effect of an additional male disclosure on women and the spillover effect an additional female disclosure on women. $p-$ values for the net effects were from an $F-$ test of the sum of two coefficients.

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

Table 11: Gender Effects of Salary Disclosure Policies: Factors Leading to Callbacks

Panel A: Effects of Disclosure by Gender

	Margin	Offer-Res. Wage	Value-Res. Wage
Salary Not Disclosed	954	5,287***	6,241***
	(577)	(1,398)	(1,530)
Salary Not Disclosed × Female Candidate	-1,011*	-4,044***	-5,055***
	(586)	(1,022)	(1,285)
Biography Fixed Effects	Yes	Yes	Yes
Recruiters	112	112	112
Observations	896	896	896
R^2	.066	.11	.11

Panel B: Effects by Gender, Net Spillovers

	Margin	Offer-Res. Wage	Value-Res. Wage
Effect of Non-Disclosure for Men (Net All Spillovers)	1389.92**	3119.95	4509.87**
Effect of Non-Disclosure for Women (Net All Spillovers)	-451.36	1300.93	849.570
Biography Fixed Effects	Yes	Yes	Yes
Clusters	112	112	112
Observations	896	896	896
R^2	.071	.12	.12

TODO: update notes

Notes: For all regressions in this table, the unit of observation is a candidate. Standard errors are clustered by recruiter. All regressions include biography fixed effects.

The regressions in Panels B and C include additional variables. In the tables above, we include only the net effects of disclosure for men and women, which is a sum of several coefficients. We explain these individual coefficients below. To examine the individual coefficients of the net effects, see Appendix Table A3.

The additional controls in Panel B and C are: The number of women disclosures and the number of male disclosures in each candidate's packet (excluding his/her own disclosure, if he/she made one). These two variables were each interacted with a dummy for whether the candidate was himself/herself female. This allowed us to assess the effect of male disclosures on other men, male disclosures on women, female disclosures on other women and female disclosures on men. As Appendix Table A3 shows the size and statistical significance of these latter effects. They are generally negative, indicating that one candidate's disclosure does slightly "crowd out" others' outcomes. does slightly crowd out the others.

The net effect for men was equal to the effect of disclosing, minus the spillover effect of an additional male disclosure on men and the spillover effect an additional female disclosure on men. The net effect for women was equal to the effect of disclosing, plus the effect of disclosing for women (disclosed × female) minus the spillover effect of an additional male disclosure on women and the spillover effect an additional female disclosure on women. $p-$ values for the net effects were from an $F-$ test of the sum of two coefficients.

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

Table 12: How Did Recruiters Arrive at Decisions?

	Used Website	Other	Spoke w/ Others	Prev. Experience
Proportion of Applicants Not Disclosing $\in [0, 1]$	.16	.059	.24**	-.063
	(.11)	(.11)	(.12)	(.11)
Mean Dep. Var	.73	.21	.29	.72
Recruiters	112	112	112	112
Observations	896	896	896	896
R^2	.18	.14	.18	.16

Notes:

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

A Appendix: Details of Randomization Procedure

Our randomization procedure was sequential, proceeded in batches and was designed to address covariate balance through re-randomization. For recruiters who were invited, accepted and met our pre-screening qualifications (signed NDA and relevant experience), the recruiters' demographics were manually coded.[35] We merged the coded demographics data with data about the recruiters' prior work experiences and posted wage rate.

Before sending out the experimental materials for recruiters' feedback, we performed a covariate balance check (described below). If our covariate balance checked passed, we would send the experimental materials to the recruiters. If the balance checks failed, we re-randomized the current batch (previous batches had already been sent to recruiters who'd already begun work and could not be re-randomized).

Our balance test checked for equality of the average of five covariates across treatment arms. The covariates were: Race (dummy variables for white and black), gender, the recruiters' advertised hourly rate, and a dummy variable for whether the recruiter had previously logged hours on the website we used to hire them.

We tested for equality of these means across all treatment groups (a single test per variable for equality across all treatment arms). In addition, we tested for pairwise equality across all treatment arms. For assignments where these tests' p-values were less than 0.2, we re-randomized. We also randomized if the pairwise comparison for any two subtreatments was less than 0.05.

The sequential balance checks were "cumulative." The tests above included observations for all prior assignments including the current batch. However, the current batch was the only batch that could be potentially adjusted if re-randomization were necessary. Batches were processed approximately once per week, so that recruiters would not have to wait long after accepting our offer to begin work.

B Appendix: Details of Recruiter Selection

The platform we used allowed workers to either be paid an hourly rate or to negotiate a pay-by-task contract. Each recruiter's profile includes an hourly rate suggested by the recruiter. We offered to pay our subjects the hourly rate posted on their profile. We also offered a bonus contract designed to align their interests with the firm's as they made decisions. The incentive scheme assessed the recruiters' decision based on candidates' outcomes, and rewarded better decision-making according to a formula.[36] Such bonus systems were common in real-world HR work.

[35] The recruiter's self-reported gender matched our manually coded gender 99% of the time. The recruiter's self-report of identifying as black matched our manually coding of this variable 99% of the time, while self-report of identifying as white matched our manually coding 92% of the time.

[36] The bonus formula for a hired candidate was:
$5x(technicalscore + innovationscore + leadershipscore) - Salary/100000$.
The technical/innovation/leadership score each took integer values between one and three, and recruiters were told these scores would be assigned after a worker was on the job for four weeks (details available in the Experimental Materials Appendix). Recruiters also lost $5 for each candidate they suggested calling back who was not interested in hiring. This could result in a negative bonus, so recruiters were told that at worst they would earn a $0 bonus. These

All recruiters worked remotely and corresponded with us directly over Internet. Each qualified recruiter was sent the materials containing a set of applications to review and an online application for qualitative and quantitative assessments of each candidate, along with commentary. Recruiters were also sent a description of the firm and the hiring needs for the opening.

Each recruiter was required to sign a non-disclosure agreement, a common practice in real-world recruitment outsourcing in order to protect firm and candidate confidentiality. All these materials are available in the Experimental Materials Appendix. We did not directly tell any recruiters that they were part of a larger recruiting workforce containing peers, but our instructions did reference the firm's other HR staff. The NDA also helped to address the possibility that recruiters would discover each other through circumstance and discuss the assignment. All recruiters signed the NDA, although some felt it was unnecessary because it was covered by the platform's terms-of-service.

To be eligible for an invitation into our workforce, recruiters on the platform had to be listed as independent (rather than affiliated with an agency),[37] based in the United States according to their profile[38] and worked previously in real-world recruiting roles for office jobs.

We searched on keywords such as: recruiter, sourcing, talent acquisition, staffing, and human resources. We did this in the summer of 2018 and ended up with a list of approximately 10,000 possible recruiters, and examined a random sample of approximately 2,700. A research assistant marked about 650 recruiters as qualified by checking the recruiter's profile for prior real-world experience in hiring or recruiting for non-manual work. We then invited each qualified recruiter charging less than or equal to \$100/hour.[39] Approximately 175 wrote back to our inquiry to accept the job offer within the time-frame of our experiment. Most of the remainder did not write back at all, or wrote back after the experiment was completed.

These job requirements are typical for recruiting. The BLS' occupational data suggests that human resource work is mid-skill work requiring a bachelor's degree, but no related work experience or prior on-the-job training.[40] According to the BLS, our requirement of prior experience for recruiters is actually more stringent than a typical requisition for a recruiter. Over 70

bonuses were eventually paid. However, since there were no actual candidates nor firms, we simulated the outcomes of callbacks, salary offers/acceptances, and performance from data from similar real-world firms. Recruiters were lead to believe that the bonus was paid based on real-world outcomes including an assessment of the candidate four weeks after starting the job. All bonuses were paid between 30 to 45 days after we received feedback from a recruiter, which we believed was a reasonable time period for a candidate to be hired and given an initial assessment of value by a firm.

[37] We did not hire agencies in order to avoid the possibility of recruiters in different treatment arms discussing among each other.

[38] We focused on US-based recruiters who would be familiar with the qualifications of US-based candidates.

[39] The recruiters all indicated interest in HR or hiring through the keywords they put in their profile. We also asked each invited recruiter for a resume or LinkedIn profile. Before officially having them start the project, we checked these resumes/profiles for hiring experience. If the experience wasn't clear we offered them the chance to clarify by asking them to tell us about their hiring experience. If this answer implied that a firm would be interested in hiring this person for this role based on their response, then we proceeded. Approximately 40 individuals who responded to our initial inquiry were ultimately not sent experimental materials, mostly due to insufficient experience with hiring/recruiting/screening.

[40] https://www.bls.gov/ooh/business-and-financial/human-resources-specialists.htm#tab-1

C Appendix: Details of Creating Candidates

For first names, we used the top five given names for Americans according to the Social Security Administration (making job candidates between 24-27 years old at the time we began our experiment).[41] We blacked out last name so that recruiters could not try to contact our candidates or look them up online (Acquisti and Fong, 2015); we also encouraged recruiters to make decisions based on the application materials than investigating them online.

Each candidate was assigned a bachelor's degree in computer science from universities ranked 3-9 in computer engineering in by US News and World report.[42] We excluded the top two universities (MIT and Berkeley) to avoid the possibility that the top institutions might have some special cache.

Previous firms were chosen from the top firms that hire software engineers.[43] To ascertain previous salaries, we matched these firms with salaries reported on payscale.com.[44] Payscale provides very granular data indexed by company, job roles, city and level of experimence. We obtained the 25th, 50th and 75th percentile of salaries for software engineers with 1-3 years of experience salaries in the city of each firm's headquarter citiess for software engineers.[45]

Each candidate biography required a realistic salary that could be disclosed when assigned to disclosure treatments. To approximate realistic gender gaps in salaries, we analyzed data from the 2015 American Community Survey (ACS).[46]

Our goal is to adjust the firm-city specific salaries from Payscale to create plausible male/female salaries for all candidate biographies. We adjust the payscale salaries for men at each firm by multiplying the appropriate salary by 1.05, multiplying the result by 0.80 to get the estimate female salaries at the same firm, location and job. We derived these estimates from our analysis of the ACS data, described in a footnote.[47]

[41] Male names were Andrew, Tyler, Joshua and Christopher. Female names were Jessica, Emily, Samantha, and Sarah. See https://www.ssa.gov/oact/babynames/top5names.html. We excluded the name "Ashley" as it could be interpreted as male or female.

[42] There are in fact nine schools ranked between 3-9 as a result of ties. They are: Carnegie Mellon, University of Illinois Urbana-Champaign (UIUC), Georgia Tech, University of Michigan, University of Texas - Austin, Cornell University, Cal Tech, University of Washington, and Purdue. We randomly selected from the three schools tied for 9th place, so that our final applicants did not attend Purdue. See: https://www.usnews.com/best-graduate-schools/top-engineering-schools/computer-engineering-rankings.

[43] See https://www.techrepublic.com/article/the-10-companies-hiring-more-software-engineers-than-anyone-else-in-silicon-valley/ and https://www.monster.com/career-advice/article/top-tech-employers-job-listings.

[44] We also verified that PayScale.com's estimates were comparable to those on Glassdoor, a similar website collecting salary data. For example https://www.payscale.com/research/US/Job=Software_Engineer/Salary/3f79787f/Amazon.com-Inc-Seattle-WA and https://www.glassdoor.com/Salary/Amazon-Software-Engineer-Salaries-E6036_D_KO7,24.htm. The distribution of base salaries reported to these types of websites are quite similar to those reported to the U.S. census. For example Glassdoor has benchmarked its salary data against Census data and published the results several times, and they are remarkably similar for base pay (Glassdoor, 2019).

[45] For IBM which had no software engineer salary data in its headquarters of Armonk NY we instead used salaries from its other major campus in San Jose, California.

[46] The actual wage gap is difficult to compute, and is beyond the scope of this paper. Publicly available salary data about specific firms – including the sources we used above (Glassdoor and PayScale) and all others we consulted do not contain gender-specific wage values.

[47] We restrict the ACS data to individuals with a bachelor's degree (only), who are employed either in computer occu-

The salaries reported on our job applications use these numbers with a few additional adjustments. We added a small amount of noise[48] and rounded to the nearest $1,000. The noise and rounding produced only trivial changes to the distribution of salaries. However, it guaranteed that the "roundness" of disclosed salary numbers was randomly assigned and uncorrelated with a candidate's gender, current employer and other characteristics. Prior research suggests that round numbers are received differently in negotiation (Mason et al., 2013).

Each applicant had one job after graduation before his/her current job, as well as a college internship. Two jobs since graduation is typical considering our candidates were in the full time workforce four to five years by the time of their applications.[49] We injected small amounts of random variation in the start date and duration of the first job. This was in order to create realistic variation across candidates so they did not all contain identical dates. The post-college job started shortly after college graduation and had a total tenure between six and 17 months (randomly selected). The duration of the current job will varied by when the recruiter views the applicant's materials, but all the current jobs started between February 2014 and November 2015. The applications also listed additional skills, achievements and coursework modeled after resumes of real software engineers.

D Appendix Tables and Figures

Table A1: Disclosure Policies and the Applicant Pool: Interactions w/ Callbacks

	CB × Res. Wage	CB × Offers≥ 2	CB × Valuation	CB × Offer
Salary Not Disclosed	-10,279*	-.11**	-10,702*	-10,001*
	(5,341)	(.049)	(5,959)	(5,624)
Num. Other Non-Disclosers in Packet	398	.022**	965	835
	(986)	(.01)	(1,136)	(1,070)
Net Effect	-9880.559**	-.09*	-9736.76*	-9165.639*
Mean Dep. Var	63,602	.44	70,078	65,666
Recruiters	112	112	112	112
Observations	896	896	896	896
R^2	.026	.02	.023	.022

Notes:

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

pations (ACS Occupation Codes 10XX and 11XX) or Computer Software Engineers specifically (ACS Occupation code 1020). Note that our payscale data combines data for men and women. On average, in the ACS, men in both computer and specifically software engineer occupations make 1.05 times the overall average. For computer occupations, women make on average 0.81 times what men make; for developer occupations women make on average 0.78 times what men make.

[48] Draws from a uniform random distribution from -$2,000 to +$2,000 in $1K increments.

[49] According to the BLS, median job tenure for those 20-24 is 1.3 years and for those 25-34 is 2.8 years (https://www.bls.gov/news.release/tenure.t01.htm).

Table A2: Spillovers in Dollar Amounts Disclosed

Panel A: Effects of Dollar Amounts Disclosed, Outcomes (with spillover terms)

	Res. Wage	Offers≥2	Valuation	Callback	Offer
Salary Not Disclosed	65,494***	-.012	56,815***	.017	59,306***
	(4,412)	(.15)	(5,442)	(.15)	(4,919)
Amount of Disclosed Salary	.77***	1.4e-06	.66***	8.3e-07	.69***
	(.047)	(1.5e-06)	(.055)	(1.5e-06)	(.05)
Num. Other Non-Disclosers in Packet	-450	.013	-127	.0061	-13
	(406)	(.014)	(512)	(.012)	(450)
Salary Amount Spillover	17	-.011	-250	-.0012	-57
	(366)	(.0086)	(476)	(.0074)	(457)
Biography Fixed Effects	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var	98,594	.52	105,781	.63	99,511
Recruiters	112	112	112	112	112
Observations	896	896	896	896	896
R^2	.55	.08	.36	.11	.41

Panel B: Effects of Dollar Amounts Disclosed, Outcomes interacted with Callbacks (with spillover terms)

	CB × Res. Wage	CB × Offers≥ 2	CB × Valuation	CB × Offer
Salary Not Disclosed	40,027**	.07	35,506**	36,813**
	(16,039)	(.14)	(17,486)	(16,451)
Amount of Disclosed Salary	.52***	2.0e-06	.48***	.49***
	(.16)	(1.4e-06)	(.17)	(.16)
Num. Other Non-Disclosers in Packet	630	.017	1,026	1,038
	(1,295)	(.014)	(1,404)	(1,327)
Salary Amount Spillover	48	-.0066	-144	14
	(753)	(.0081)	(844)	(807)
Biography Fixed Effects	Yes	Yes	Yes	Yes
Mean Dep. Var	63,602	.44	70,078	65,666
Recruiters	112	112	112	112
Observations	896	896	896	896
R^2	.13	.092	.12	.12

Notes:

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

Table A3: Gender Effects of Salary Disclosure Policies, Net Spillovers (Including All Spillover Coefficients)

Panel A: Effects by Gender, Net Spillovers

	Res. Wage	Offers≥2	Valuation	Callback	Offer
Salary Not Disclosed	-14,291***	-.085	-10,716***	-.0078	-11,709***
	(2,627)	(.084)	(3,186)	(.078)	(2,949)
Salary Not Disclosed × Female Candidate	10,409***	-.063	6,215	-.12	7,660**
	(3,537)	(.12)	(3,993)	(.095)	(3,413)
Fem. Non-Disclosures Spillover onto Males	638	.022	2,373	.013	3,029
	(1,718)	(.049)	(2,175)	(.043)	(2,013)
Fem. Non-Disclosures Spillover onto Other Fems	791	.079*	2,330	.000068	2,787*
	(2,102)	(.041)	(1,656)	(.043)	(1,604)
Male Non-Disclosures Spillover onto Other Males	-1,906	.032	-2,706	-.00061	-3,760*
	(1,842)	(.05)	(2,333)	(.048)	(2,169)
Male Non-Disclosures Spillover onto Fems	-1,387	-.029	-1,459	.016	-1,917
	(1,996)	(.037)	(1,631)	(.036)	(1,577)
Effect of Non-Disclosure for Men (Net All Spillovers)	-15559.02***	-.03	-11049.14***	0	-12439.06***
Effect of Non-Disclosure for Women (Net All Spillovers)	-4479.24*	-.1	-3629.66	-.11**	-3178.3
Biography Fixed Effects	Yes	Yes	Yes	Yes	Yes
Clusters	112	112	112	112	112
Observations	896	896	896	896	896
R^2	.32	.08	.2	.11	.23

Panel B: Effects by Gender, Net Spillovers w/ Callback Interactions

	CB × Res. Wage	CB × Offers≥ 2	CB × Valuation	CB × Offer
Salary Not Disclosed	-8,972	-.065	-9,127	-8,894
	(8,132)	(.079)	(8,992)	(8,427)
Salary Not Disclosed × Female Candidate	-5,188	-.1	-5,630	-4,731
	(10,036)	(.11)	(10,793)	(9,963)
Fem. Non-Disclosures Spillover onto Males	2,330	.045	3,377	3,776
	(4,197)	(.037)	(4,736)	(4,387)
Fem. Non-Disclosures Spillover onto Other Fems	853	.047	2,512	2,642
	(4,819)	(.042)	(4,728)	(4,454)
Male Non-Disclosures Spillover onto Other Males	-1,674	.0067	-1,643	-2,484
	(4,815)	(.04)	(5,312)	(4,919)
Male Non-Disclosures Spillover onto Fems	651	-.00066	367	26
	(4,141)	(.036)	(3,907)	(3,688)
Effect of Non-Disclosure for Men (Net All Spillovers)	-8315.73	-.01	-7391.79	-7602.89
Effect of Non-Disclosure for Women (Net All Spillovers)	-12655.18**	-.12**	-11877.65**	-10957.05**
Biography Fixed Effects	Yes	Yes	Yes	Yes
Clusters	112	112	112	112
Observations	896	896	896	896
R^2	.12	.094	.11	.11

Notes:

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are robust.

Table A4: **Salary Ban Laws - Private Employers**

State or City	Eff. Date	Comments
California	1/1/2018	Even if the information is disclosed (applicant volunteers), cannot use it. Must also give payscale information if request
Connecticut	1/1/2019	May not ask, if voluntarily disclosed law no longer applies. May ask about other elements of compensation structure but not the value
Delaware	12/14/2017	Not prohibited from discussing salary expectations. Can confirm salary history after offer. Does not prohibit use of information if voluntarily disclosed.
Hawaii	1/1/2019	Can't ask, and cannot rely on the information unless voluntarily disclosed.
Massachusetts	7/1/2018	Can't ask, can confirm prior history if disclosed. However, employees salary history cannot be used to justify paying the employee less than an employee of a different gender performing similar work. Unclear whether can be used.
Oregon	10/6/2017	
Vermont	7/1/2018	May ask about expectations but not salary. If voluntarily disclosed, may confirm. Unclear whether it can be used
New York City	10/31/2017	Cant ask, if voluntarily disclosed may verify and rely on it for setting salary, benefits.
San Francisco	7/1/2018	
States Banning Salary History Bans		
Michigan	6/24/2018	Bans salary history bans
Wisconsin	4/28/2018	Bans salary history bans

Notes: The following states have laws that only pertain to public employers: Illinois, New Jersey, New York, Pennsylvania. The following jurisdictions have laws that only pertain to public employers:Chicago IL, Louisville KY, New Orleans LA, Kansas City MO, Albany County NY, Suffolk County NY, Westchester NY, Pittsburgh PA. Philadelphia PA enacted a salary history ban pertaining to all employers in January 2017, but as of 2018 it is on hold due to a lawsuit.

Sources: https://www.hrdive.com/news/salary-history-ban-states-list/516662/ (Accessed February 9, 2019). https://www.connecticutlaboremploymentlawjournal.com/connecticut-legislation/connecticut-poised-to-ban-salary-history-inquiries/, https://www.littler.com/publication-press/publication/delaware-enacts-law-address-gender-pay-gap-prohibiting-employers, https://www.shrm.org/resourcesandtools/legal-and-compliance/state-and-local-updates/pages/hawaii-joins-salary-history-ban-trend.aspx, https://www.mass.gov/files/documents/2018/05/02/AGO%20Equal%20Pay%20Act%20Guidance%20%285-2-18%29.pdf, https://www1.nyc.gov/site/cchr/media/salary-history-frequently-asked-questions.page