Dropping Anchor: A Field Experiment Assessing a Salary History Ban with Archival Replication

Eliot L. Sherman
Georgetown University
es1616@georgetown.edu

Raina Brands
UCL School of Management
r.brands@ucl.ac.uk

Gillian Ku
London Business School
gku@london.edu

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Abstract

Could a salary history ban (SHB) reduce the gender wage gap? Proponents of this intervention believe the gap is sustained by the practice of eliciting salary histories from job applicants. Although observational studies suggest that SHB operates as envisioned, two features complicate the interpretation of its effects. These are, respectively, the passage of relevant legislation alongside SHB, and the presence of public campaigns that propel SHB into law. We assessed SHB in the United Kingdom, where neither potential confound was present, and found no evidence that the intervention operated as intended. An intention-to-treat analysis of a 16-month field experiment, conducted with 230 staff hires at a private educational institution, indicates that SHB was about as likely to harm new hires as it was to help them. Additional analyses did not reveal significant differences between women and men. We supplement these results with an interrupted time series analysis of 3,687 placements made by a recruitment firm that voluntarily adopted SHB for its job candidates. Salaries were significantly lower under SHB, but were not significantly different for women versus men. Taken together, our results suggest that SHB was ineffective in isolation from contemporaneous legislative changes, pro-equality messaging, or some combination thereof.

Could a salary history ban (SHB) reduce the gender wage gap? Proponents of the intervention believe the wage gap is sustained by the practice of eliciting salary histories from job applicants. This view is consistent with evidence of unequal financial returns, by gender, to job switching between firms (Brett and Stroh 1997, Dreher and Cox 2000, Fuller 2008, Quintana-Garcia and Elvira 2017). As noted by one practitioner who helped draft SHB legislation: "If there are women statistically making 20% less, and a common practice is to add 10% to your previous pay, then it is impossible to ever get equal pay."

While this logic has intuitive appeal, aspects of its application remain unclear. First, legislation rarely explains how SHB will be enforced. Second, voluntary disclosure of salary history by workers—which cannot be prevented under any legal or normative framework—remains prevalent (Agan et al. 2020a). Third, firms fill about half of their open roles from within (Keller 2018). In such circumstances, hiring managers are often aware of applicants' compensation history.

These potential impediments have not diminished states' enthusiasm for adopting SHB as a policy instrument. To the contrary, by 2022 a total of fifteen states had implemented or announced forthcoming SHB covering all employers.[1] Likewise, several large firms—including Google, Amazon, and Starbucks, among others—claimed to have voluntarily adopted SHB (Miller 2018).

Due to this substantial footprint, as well as its promise as an ameliorative policy, SHB has attracted substantial attention from scholars. Four recent studies leverage Current Population Survey (CPS) data and statewide heterogeneity in the timing of policy adoption to estimate the impact of SHB on the gender wage gap. Using a difference-in-differences estimator, Sinha (2019) reports that SHB reduced the gap in weekly earnings from 13% to 8.5%. Hansen and McNichols (2020) complement this identification strategy with a synthetic control approach and find a one percentage point reduction in the gap. Sran and colleagues (2020) focus on the effect of SHB for job changers specifically. They observe a larger pay increase for women than men, but note that this result is not robust to the inclusion of fully-flexible time trends by hiring industry and geography. Bessen and colleagues (2021) likewise focus on the impact of SHB on job-changers. Their

[1] SHB cover all workers in California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Maine, Maryland, Massachusetts, New Jersey, New York, Oregon, Vermont, and Washington. Employers in Alabama may not refuse to hire, interview, or promote a candidate who does not disclose compensation history. Additional states have enacted partial bans that only apply to government jobs. See www.hrdive.com for more detail and an up to date list.

difference-in-differences estimates suggest that, while SHB increased the earnings of all job-changing workers by four percent, the pay of job-changing women rose by about six percent.

Accordingly, the central tendency of this research is that SHB operates as envisioned. But this consensus warrants further assessment, due to two features of SHB that complicate its interpretation. First, as Sran and colleagues (2020) describe, there is substantial heterogeneity in the enactment of related legal changes alongside SHB. For example, California—which accounts for the preponderance of treated observations in the aforementioned studies—implemented SHB simultaneously with “ban the box” legislation that precludes employers from asking applicants to divulge their criminal histories, and a requirement that employers disclose the salary range for each open position if asked to do so by an applicant.[2] Maryland and Pennsylvania passed SHB at the same time as a salary range transparency requirement—though at present, Pennsylvania’s law only applies to state agencies.[3] Washington bundled SHB with a directive that employers disclose the minimum salary attached to a role, if requested to do so by an applicant (Sran et al. 2020). Further heterogeneity is likely to emerge as more states legislate their own versions of the ban.

Second, in many states the passage of SHB legislation was accompanied by public relations campaigns. The campaign in New York, for example, explicitly linked SHB with pro-equality messaging via opinion articles (e.g., Wong 2019), subway advertisements (see **Appendix A**), and videos produced by the Department of Labor, one of which begins by stating: “In New York State, we are committed to fairness for all. That is why we’re doing everything we can to close the gender pay gap.”[4] Furthermore, once SHB was written into law, Human Resources (HR) consultants and trade publications began disseminating guidance to employers regarding best practices for compliance, which often reflected similar messaging.[5]

These potential confounds contaminate causal inference in observational studies of SHB. Insofar as salaries equalized under SHB, it is difficult to discern whether the policy was the cause or a correlate. This motivates the need for an experimental assessment, conducted with sufficient external validity to draw

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[2] See https://www.hrdive.com/news/california-bans-the-box-outlaws-salary-history-questions/507340/

[3] See https://www.paycor.com/resource-center/states-with-salary-history-bans and https://www.natlawreview.com/article/washington-adds-pay-history-ban-transparency-requirements

[4] See https://www.ny.gov/programs/salary-history-ban

[5] See https://www2.deloitte.com/content/dam/Deloitte/us/Documents/human-capital/us-equal-pay-legislation-banning-salary-history-questions.pdf

credible conclusions. Barach and Horton (2021) provide the only such example to date. They found, in a study of freelancers, that workers whose past earnings were obscured received higher wages—conditional on negotiating, which occurred in 11% of the filled roles in their sample. However, the authors were unable to measure worker gender.

We conducted a 16-month field experiment in the United Kingdom (U.K.), where neither potential confound was present. Our study involved 230 male and female job-changers. The outcome of interest was the starting salary received by the applicant who was hired into each role. To preview our findings, an intention-to-treat analysis indicated that SHB was about as likely to harm new hires financially as it was to help them. Subsequently, we attempted to unbiasedly increase the precision of our estimates by including relevant covariates that were orthogonal to treatment assignment (Athey and Imbens 2017). Our preferred specification from this analysis allows us to reject, at the five percent level, effects larger than a four percent gain in annual salary accepted by women versus men as a result of SHB. At the one percent level, we are able to rule out effects larger than a seven percent gain. This is notable given that we observed at our research site, in the year prior to our experiment, a fourteen percent gender gap in salary offers extended. We can therefore reject, with a high degree of certainty, the hypothesis that SHB could close the gender gap in salary offers within this setting.

We complemented this assessment with an interrupted time series analysis of personnel data from a U.K. recruitment agency that voluntarily adopted SHB on behalf of its job candidates. This analysis complements Study 1 by trading the rigor of randomization for the statistical power of a larger sample size that includes hires across multiple firms, thereby increasing generalizability. However, our assessment of 3,687 placements made by this agency indicates that both women and men earned significantly lower salaries under SHB, without a significant difference between them.

We do not believe it would be appropriate to conclude, from these two studies, that "salary history bans are ineffective" at narrowing the pay gap. What we *can* conclude is that we found no evidence that the intervention was effective at doing so in isolation from contemporaneous legislative changes, pro-equality messaging, or some combination thereof. In concert with these phenomena, SHB may indeed be a valuable policy instrument. It will therefore be incumbent on future scholarship to determine the precise blend of interventions that combine to produce the ameliorative effects reported by prior observational studies. Implemented on its own, however, we are comparatively less optimistic that SHB can achieve this end.

Study 1: Field Experiment at a Private Educational Institution

Our research site was a mid-sized private educational institution in the U.K. The experiment began on August 1, 2018, and concluded on December 3, 2019. Applicants were hired from within and outside the organization. The experiment involved staff and did not extend to faculty hires. Randomization occurred at the level of the role.

Three aspects of our research context were particularly useful. First, because all staff hiring at our research site is centralized through the recruitment team, we were able to obviate concerns about self-selection into the experiment. Second, salary was the only negotiable element of compensation, as opposed to additional benefits such as stock options. This provides us with a direct assessment of SHB. Third, we unobtrusively measured our main outcome of interest—annual salary—as opposed to relying on self-reported measures (e.g., Sherman 2020), making our results less prone to subjective recollection and social desirability bias (Webb et al. 1966).

To establish a need for treatment at baseline, we analyzed all staff hiring over a one-year window prior to our experiment: August 1, 2017 to July 31, 2018. During this time frame, our research site made 218 hires, 158 of whom were female. The average male salary offer was £39,080 (SD = £16,503) compared with £33,742 (SD = £14,100) for the average female salary offer, reflecting a gap of fourteen percent in favor of men. We also estimated a "controlled" gender wage gap by regressing logged salary on the relevant covariates available to us in the HR database for this period. These were: Gender, age at joining the organization, and indicator variables for salary band. This model suggested that women earned 1.2% less than men, but the comparison failed to achieve statistical significance (p = .51), with a 95% confidence interval that ranged from -5% to 2.5%.[6]

Procedure. Each time a hiring manager opened a new role, the recruitment team e-mailed the first author. The first author then randomized the role to either a "don't ask" (SHB) or "ask" (no SHB) condition. There were 114 roles in the "don't ask" condition and 116 roles in the "ask" condition. We employed block-random assignment with respect to staff salary band. Our research site employed six salary bands with increasing salary ranges. Accordingly, we blocked on the median split: Whether a role occupied bands 1 to 3

[6] In addition, we assessed the gender wage gap among the full roster of 713 staff—478 of whom were female—employed as of July 21, 2018, the day before our experiment commenced. Women earned an average of £44,696 whereas men earned an average of £46,937, leading to an "uncontrolled" gender wage gap of five percent in favor of men.

versus 4 to 6. In other words, the lead author used one randomizer for roles that occupied bands 1-3 and a separate randomizer for roles that occupied bands 4-6. The purpose of block-random assignment is to ensure that potential sources of variability are addressed, leading to more precise estimates of treatment effects (Cox 1958). Here, for example, since our outcome of interest was salary, we sought to ensure that a small number of high-paying roles did not disproportionately accrue within one condition.

For each "don't ask" role, the hiring manager received an e-mail from the recruitment team with the following instructions: "*Please do NOT discuss applicants' current or prior compensation during interviews for this position, or at any other point in the process of hiring for this position. It is important that all decisions related to this position be made WITHOUT knowledge of applicants' compensation history. Instead, in interviews please discuss applicants' compensation expectations*." For each "ask" role, the hiring manager received an e-mail from the recruitment team with the following instructions: "*Please make sure to discuss all applicants' current compensation during interviews for this position, as well as at any other relevant point in the hiring process. It is important that all decisions related to this position be made with knowledge of applicants' compensation history. In interviews, please also discuss applicants' compensation expectations*."

In all cases, however, applicants discussed their salary history with a member of the HR team prior to interviewing, to ensure that they were reasonably well-calibrated in terms of compensation expectations. On the basis of weekly meetings conducted between the first author and members of the recruitment team—who were separate from the HR team—we have no reason to believe that confidentiality regarding these discussions was violated, though this possibility is characteristic of SHB in general (Agan et al. 2020b).

For each role that was filled, we unobtrusively observed all cases of salary, gender, salary band, number of applications to a role, maximum offer allowed for each role, whether the hired candidate negotiated their salary, whether the hired candidate was internal, and whether the hiring manager was female.[7] For each closed role, we also surveyed all hiring managers and hired workers, securing response rates of 90% and 82%, respectively (see **Appendix B** for all survey measures).

RESULTS

[7] With the exception of negotiation, which was not recorded in one case.

We provide a balance check and summary statistics in **Table 1**. With two exceptions, subjects in the "ask" and "don't ask" conditions were statistically indistinguishable. Female hiring managers were more prevalent in the "ask" condition (p = .02) and there were marginally more applications to "ask" roles (p = .07). This distribution is, however, in line with prior field experiments (e.g. Dupas and Robinson 2013, Gee 2019, Chatterji et al. 2019).

Regarding our summary statistics, we highlight the following. First, of the 230 hires made during the experiment, 164—or 71%—were *female candidates*. Second, 35% of our observed hires were *internal* to the firm. Third, we did not observe significant gender differences in propensity to *negotiate* (p = .69) or *salary expectations* (p = .62) (cf. Roussille 2022). Finally, hired candidates received, on average, 97% of the maximum allowable salary for a role. Interestingly, this was not an impermeable boundary, as the salary offer for 19 roles exceeded the purported maximum.

TABLES 1 AND 2 ABOUT HERE

Non-compliance. In view of our treatment, we conceptualized non-compliance as hiring managers' awareness of hired candidates' salary history. We assessed two different forms of non-compliance. First, in our survey of hiring managers, we asked the following: "Did one or more of the applicants for this position make you aware of their current salary, either during an interview or at any other stage in the hiring process?" We consider this a conservative assessment, since it is designed to elicit violations on the part of *any* applicant, not just the successful one. At least one applicant disclosed their current salary in 25% of "don't ask" roles, compared with 53% of "ask" roles (p < .001). Thus, random assignment to the "don't ask" condition significantly reduced disclosure propensity, but did not eliminate it.[8]

We also asked hiring managers "Did you know the current salary of one or more applicants through other means, for example if they were an internal applicant from your team?" Here we observed a nearly identical pattern of results: An incidence of 25% in "don't ask" roles versus 46% in "ask" roles. See **Table 2**

[8] We also attempted to measure hiring manager noncompliance in our survey of successful candidates via the following question: "Did your interviewer ask you what your current base salary per annum is?" Unfortunately, an examination of responses, as well as candidates' written comments, indicates that a substantial number of them erroneously conflated this question with the pre-interview conversation conducted with an HR representative regarding their salary history. As a result, we do not consider these data usable.

for a comparison of variable means across these and a third measure, which combines the initial two.[9] We subsequently rely on this combined measure to indicate non-compliance.

Hypothesis-testing. We observed a non-significant (p = .84) difference of £408 between "don't ask" roles ($\bar{x}$ = £38,768, SD = £14,427) and "ask" roles ($\bar{x}$ = £39,176, SD = £15,240). **Figure 1** depicts a visual comparison of unadjusted salary means by experimental condition for men and women.

FIGURE 1 ABOUT HERE

We note, however, that the presence of non-compliance in these data likely dilutes our ability to isolate the treatment effect. Accordingly, **Figure 2** depicts a visual comparison of unadjusted salary means between experimental condition, stratified by non-compliance—that is, hiring manager awareness of candidates' salary history. Contrasting compliant "don't ask" roles and non-compliant "ask" roles is likely the most informative comparison, in terms of assessing the impact of the policy. While this comparison is not significant (p = .20), the direction in which the difference trends is interesting: The 60 employees hired into fully compliant "don't ask" roles earned an average of £2,582 less than the 84 employees hired into "ask" roles in which hiring managers discussed applicants' salary histories. This suggests that, for those roles in which the policy is fully adhered to, SHB may actually suppress the salaries of new hires. We report results that are consistent with this pattern in Study 2 below.

FIGURE 2 ABOUT HERE

We also note that new hires into non-compliant "don't ask" roles—that is, treated roles for which hiring managers were nevertheless aware of at least one applicant's current salary—secured the highest average salaries in our sample. While it would be injudicious to infer too much from this, it is potentially consistent with an adverse selection mechanism (Agan et al. 2020a), whereby applicants are more likely to disclose their salary history if doing so appears advantageous to them.[10]

FIGURE 3 ABOUT HERE

[9] With respect to the consequences of non-compliance, we asked hiring managers "Regarding your successful candidate, to what extent did your knowledge of their current compensation influence the salary amount you offered them? Please provide your answer on a scale from 1 to 5, with 1 being 'not at all' and 5 being 'substantially.'" We observed a significant (p < .001) difference between compliant (1.33) and non-compliant (2.47) roles, similar in magnitude to the difference between "ask" (2.39) and "don't ask" (1.63) roles.

[10] Insofar as this supposition is correct, we may observe that successful applicants to non-compliant "don't ask" roles have comparatively higher current salaries, that is, the amount they earned directly before being hired in our experiment. Conditional upon the excision of one extreme outlier in a compliant "ask" role—a current salary of £156,000, by far the largest in our sample—this is indeed what we observe, as we report in **Figure 3**.

We further assessed the impact of SHB in **Table 3**. In model 1, we regressed *logged salary* on a binary indicator of *SHB* and a binary indicator of *female candidate*. The 95% confidence interval around *SHB* ranged from -9% to 8% ($p = 0.86$). In model 2, we added an interaction term to test for asymmetric treatment effects by gender. The interaction term failed to achieve statistical significance ($p = 0.69$), with a confidence interval that ranged from -14% to 25%. In other words, the range of possible outcomes ran the gamut from exacerbating the existing wage gap to emphatically reversing it in favor of women. Accordingly, we attempted to unbiasedly increase the precision of our estimates. Our first step was to follow the guidance provided by Athey and Imbens (2017, p. 92), who note that "incorporating covariates may make the analysis more informative … [one] can construct test statistics in the Fisher exact p-value approach that may have more power than statistics that do not depend on the covariates."

In order to minimize researcher degrees of freedom in doing so (Simmons et al. 2011), in model 3 we added all the covariates for which we had complete data: An indicator variable for *internal* candidates, an indicator variable for candidates who *negotiated* their salary offer, the number of *applications* to each role, an indicator variable for *female hiring manager*, and fixed effects for salary *band*. All these covariates were orthogonal to treatment assignment with the exception of *female manager*. In their note on the perils of balance testing, Mutz and colleagues (2019) advise against including covariates that are unbalanced across treatment and control groups; however, in our case, the inclusion or excision of this covariate did not alter the pattern of results. Model 3 also excluded the interaction term between *SHB* and *female candidate*. In this specification, the confidence interval around SHB narrowed to a range between 3% and 4% ($p = .79$). This suggests that the presence of *SHB* was about as likely to financially harm new hires as it was to help them.

In model 4, we returned the interaction term between *SHB* and *female candidate*. Although it remained non-significant ($p = 0.46$), the confidence interval narrowed to between -10% and 5%. Next, in model 5, we estimated the local average treatment effect (LATE) by applying the same specification solely to complier roles (Imbens and Angrist 1994). Here we only included "don't ask" roles in which hiring managers were unaware of all applicants' salary history and "ask" roles in which hiring managers *were* aware of applicants' salary history. This specification produces a confidence interval of -12% to 4% regarding the effect of SHB for women versus men ($p = 0.30$).

Lastly, in a separate analysis that we conducted and report in **Appendix C**, we investigated whether SHB affected the average *raise* received by successful candidates. In short, we did not observe a significant main effect of SHB on *raise* nor moderating effects with respect to gender or internal applications.

TABLE 3 ABOUT HERE

Robustness Checks: We conducted two robustness checks to assess the credibility of our inferences. First, we merged in hiring data from the year prior to our experiment and re-ran our models. The goal was to increase estimate precision via a quasi-experimental analysis—effectively a difference-in-differences estimator—that incorporates pre-experiment cases as a baseline. However, this approach limited the number of covariates we could use, due to the need for alignment with the variables that were available to us from the hiring database in the pre-experiment period.[11] We report the results of this analysis in **Appendix D**. In sum, with the inclusion of these covariates and salary *band* fixed effects, the interaction term between *female candidate* and *SHB* remained non-significant (p = .90). It produced a confidence interval that ranged from 6% to 6%, thereby attenuating the relevant point estimate beyond consequence.

An instrumental variables (IV) regression leveraging our compliance data constituted our second robustness check. Specifically, we relied on the question: "Did one or more of the applicants for this position make you aware of their current salary, either during an interview or at any other stage in the hiring process?" We defined a "no" response as treated and a "yes" response as control. We then used random assignment to a "don't ask" role to instrument for this treatment. The intuition is that assignment to a "don't ask" role provides a sizeable exogenous shock to the probability of treatment, that is, making an offer without knowing any applicants' salary history. We report the results of these regressions, which were broadly consistent with our main analysis, in **Appendix E**.

Finally, we considered the possibility that SHB had its intended effect in these data, but our experiment lacked sufficient statistical power to detect it. In order to provide greater insight in this regard, we conducted an interrupted time series analysis of SHB using a larger sample of hires from a different setting.

Study 2: Interrupted Time Series Analysis of Recruitment Firm Placement Data

We conducted our supplemental study using placement data from a recruitment firm in the U.K. This firm—which specialized in media, technology, and design roles—voluntarily adopted SHB in 2018 because the company's founder wanted to address gender-based disparities in compensation among workers that the

[11] We dropped seven cases from the pre-experiment hiring period due to missing salary band observations.

firm placed. We reiterate, however, that the firm's interest in employing SHB was not paired with any legal mandate or coordinated PR campaign, as was the case for SHB implemented within the U.S. In general, there was very little public awareness of the subject: An online petition asking the U.K. government to debate SHB in Parliament, for example, received only 15 signatures.[12]

The first and third authors interviewed 21 recruiters at this firm to glean insight into how they thought SHB operated. Recruiters conceded that there was no clear enforcement mechanism compelling compliance on the part of client firms or job candidates: "Some candidates just automatically blurt it out, but we don't ask the question." Despite this caveat, recruiters at the firm were united in the view that SHB was effective. One noted: "It seems to [help] people that are falling behind, to be honest. Because if you, for two or three years, had not been getting the pay rise you should have had … this person's going to make a big jump." Several recruiters implicated candidate confidence during salary negotiations as a likely mechanism.

The firm provided us with its database of placements. After cleaning these data, our sample comprised 3,687 placements of 3,411 candidates made between November 2009 and June 2019.[13] Unfortunately, candidate gender was not included in the firm's database. Accordingly, we derived gender from candidates' first names via the R package "gender" (Mullen 2018). The placement date was specific to the day, with 687 occurring after the date in 2018 on which the firm adopted SHB. This allowed us to make salary comparisons pre- and post-adoption of the policy (Castilla 2015).

Our first step was to assess the gender wage gap during the pre-period. We observed an eight percent ($p < .001$) uncontrolled gap: The firm's 1,710 female placements earned an average of £38,333 (SD = £20,355) while the 1,290 male placements earned an average of £41,635 (SD = £18,474), for a difference of £3,302. By comparison, in the post-period, the firm's 417 female placements earned an average of £37,072 (SD = £15,178) while the 270 male placements earned an average of £39,833 (SD = £18,266). **Figure 4** depicts this comparison of raw means. A visual inspection suggests that, while the salary of men and women were lower

[12] https://petition.parliament.uk/archived/petitions/205261, accessed July 4, 2021.

[13] The raw data captured 8,205 placements involving 6,227 unique individuals. However, 3,425 observations were missing salary data. These missing observations primarily, but not exclusively, involved placements made prior to 2011. In addition, salary was recorded as "0" for 1,037 placements. We recoded these zeroes as missing to avoid downwardly biasing our salary estimates. This left us with a total of 3,743 placements. However, there remained in these data inconsistencies that required correction. Specifically, we observed 55 instances in which multiple candidates were listed for the same placement. We censored these observations. Finally, there were instances in which the R package "gender" was unable to categorize names, for example due to their relative rarity. Accordingly, the second and third authors went through the data to hand-correct these instances, using online sources www.genderchecker.com and www.namespedia.com. The analysis we report above is derived from this corrected sample of 3,687 placements.

during the post-period, the gender gap may have narrowed slightly. Accordingly, we next assessed whether this apparent difference was significant. The results of these regressions are shown in **Table 4**.

FIGURE 4 ABOUT HERE

Model 1 indicates that placements earned 9.5% less during the SHB time period (p = .029). We add an indicator for *female candidate* in model 2. Model 3, which includes the interaction between *SHB* and *female candidate*, suggests that SHB did not differentially impact women versus men (p = .51). We note, however, that the confidence interval ranges from -6% to 13%. We next narrowed the observation window in models 4-6, in order to compare the treated period to an equivalent number of pre-treatment days. Model 4 indicates that placements also earned 9.5% less under SHB (p = .02) in this specification. We are, however, again unable to detect a differential effect of *SHB* for women versus men in model 6 (p = .49), as the confidence interval ranges between -7% and 17%.

While human capital measures are unfortunately absent from these data, we next leveraged an additional covariate to increase the comparability of male and female placements. Specifically, in **Table 5**, we estimate the same set of models, but include job title indicator variables.[14] We continue to observe lower salaries during the salary history ban period, and continue to not detect asymmetrical effects for women versus men. The inclusion of job title indicator variables does, however, substantially increase the amount of variance explained by our model, while attenuating the statistical significance of *female candidate* beyond conventional levels.

TABLES 4 AND 5 ABOUT HERE

DISCUSSION

Salary history bans are intuitively appealing and increasingly popular. There is strong observational evidence that they benefit women in particular (Sinha 2019, Hansen and McNichols 2020, Sran et al. 2020, Bessen et al. 2021). Yet across two studies—a field experiment involving 230 new hires and an interrupted time series analysis of 3,687 recruitment firm placements—we did not find evidence that SHB achieved its intended aims. It is worth considering why.

[14] Our data include 1,270 uniquely worded job titles. In an additional analysis that we conducted but do not report here, we paid a research associate to sort these job titles into 24 broader job categories. We then re-estimated our **Table 5** models using job category indicator variables. Our results do not substantively change: We continue to observe a negative effect for the salary history ban period, while the significance of the interaction effect in models 3 (p = 0.65) and 6 (p = 0.30) remains attenuated beyond conventional levels.

We believe a compelling rationale can be found in the very circumstances that motivated our experiment. To wit, the interpretation of SHB in observational data is complicated by contemporaneous legislative changes, such as pay scale or minimum salary disclosure requirements, and the public messaging campaigns that often propel SHB into law. That we were unable to recover evidence of SHB operating as designed, in two research sites that notably lacked these features, suggests that a particular blend of such phenomenon, enacted alongside the policy itself, may be required to produce the effects that have been documented by prior observational scholarship.

This is telling with respect to SHB specifically, but also has bearing on the design and implementation of ameliorative policies more broadly. Observational assessments of labor market interventions are generally studied in contexts where they have been voluntarily adopted. But adoption itself can be endogenous to a meaningful desire for change (Ferguson 2015). Insofar as this is the case, we should not be surprised if purported effects disappear—or even reverse—in settings where adherence to ideals of meritocracy and equal opportunity is more symbolic than substantive.

Of course, certain aspects of our analysis may have contributed to the inconsistency between our results and those of the papers with which we are in dialogue. For example, in our experiment we primarily relied on intention-to-treat analyses, which may have impeded our ability to detect an effect given the presence of non-compliance. We do, however, believe that this is the most policy-relevant manner in which to analyze these data, since strict enforcement of SHB with respect to both firms and applicants is infeasible (Agan et al. 2020a).

Likewise, certain features of our research context may limit the generalizability of our findings. First and foremost, we note that the majority of hires in both of our studies, and 71% of hires in our experiment, were women. Without speculating about potential mechanisms, it is reasonable to expect that different results may obtain in a setting that is more demographically balanced, or where men are overrepresented. Second, we reiterate that applicants discussed their salary history with a member of the HR team prior to meeting with the recruitment team and interviewing with a hiring manager. While this was done to ensure that our experiment was not prohibitively disruptive—by verifying that candidates were reasonably well-calibrated in terms of salary expectations—it is possible that these discussions influenced the subsequent interview process. Candidates may have assumed, for example, that hiring managers were already aware of their salary history as a result of these discussions, and behaved differently as a result.

Our inability to rule out this possibility constitutes an important avenue for future research. Specifically, much of the enthusiasm for SHB, and certain ameliorative policies more broadly, hinges on assumptions about bargaining behavior that are inferred but rarely assessed (e.g., Bessen et al. 2021). Chief among these assumptions is that workers actually bargain with relative frequency. Yet the paucity of studies that document the practice in context find extremely low base rates. For example, just 14% of successful candidates reported negotiating their final salary in our experiment, which is remarkably consistent with the 14% rate reported by Leibbrandt and List (2015) and the 11% rate reported by Barach and Horton (2021). Insofar as these rates are generalizable, policymakers implicitly ask a great deal of any intervention that relies on bargaining behavior to narrow the gender pay gap. Acquiring credible insight into candidates' responsiveness to these interventions, with respect to both negotiation propensity and effectiveness, is therefore crucial to understanding the promise of SHB and policies like it.

With that said, our main conclusion remains: Despite its intuitive appeal and increasing proliferation, we are less optimistic that SHB, implemented on its own, can achieve its intended aims.

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Table 1 – Descriptive Statistics and Balance Check for Field Experiment Variables, Study 1

Variable	Observations	Minimum	Maximum	Mean	Mean "Don't Ask"	Mean "Ask"	*t*-test for Differences
Salary (£)	230	22,518	98,000	38,974	38,768	39,176	0.84
Female candidate	230	0	1	0.71	0.70	0.72	0.71
Salary band	230	1	5	3.13	3.10	3.16	0.60
Internal candidate	230	0	1	0.35	0.31	0.40	0.16
Female hiring manager	230	0	1	0.69	0.61	0.76	0.02
Candidate negotiated	229	0	1	0.14	0.16	0.11	0.30
Proportion of max offer	228	0.64	1.14	0.97	0.97	0.97	0.84
Number of applications	227	1	434	59	66	52	0.07
Current salary (£)	212	6,400	156,000	35,983	35,640	36,295	0.80
Candidate number of children	189	0	4	0.43	0.49	0.37	0.34
Candidate age (years)	189	18 years	61 years	33 years	33 years	33 years	0.83
Candidate expected salary (£)	120	20,000	90,000	35,986	36,641	35,238	0.56

We have complete data for the variables gathered unobtrusively from the HR database
Variables with incomplete data derive from self-report surveys of hired applicants.
We report p-values from two-tailed t-tests in the far right column.
The organization did not make any hires into the highest staff salary band (6) during our experiment.

Table 2 – Assessment of Non-Compliance for Field Experiment Variables, Study 1

	Applicant Disclosure of Salary History			Hiring Manager Aware by Other Means			Non-compliance		
Variable Means	Disclosure	No Disclosure	*t*-test	Aware	Unaware	*t*-test	Non-compliant	Compliant	*t*-test
Salary (£)	38,164	37,727	0.83	39,882	36,794	0.12	39,150	35,990	0.11
Female candidate	0.72	0.70	0.79	0.78	0.66	0.07	0.73	0.67	0.38
Salary band	3.01	3.12	0.43	3.24	2.98	0.06	3.14	2.98	0.21
Internal candidate	0.31	0.42	0.11	0.54	0.29	< 0.01	0.42	0.32	0.15
Female hiring manager	0.73	0.63	0.18	0.69	0.66	0.66	0.71	0.61	0.40
Candidate negotiated	0.11	0.14	0.50	0.11	0.14	0.50	0.11	0.16	0.34
Proportion of max offer	0.96	0.98	0.06	0.96	0.98	0.14	0.97	0.98	0.07
Number of applications	62	53	0.31	53	58	0.57	53	61	0.36
Current salary (£)	34,683	35,649	0.72	35,823	34,951	0.75	35,272	35,281	0.99
Candidate number of children	0.45	0.40	0.71	0.44	0.41	0.86	0.44	0.39	0.75
Candidate age (years)	33	32	0.63	33	33	0.95	33	32	0.52
Candidate expected salary (£)	35,322	35,189	0.96	38,556	33,282	0.05	36,033	33,927	0.43
"Don't ask" condition	0.31	0.60	< 0.01	0.34	0.57	< 0.01	0.33	0.73	< 0.01

"Applicant Disclosure of Salary History" derived from hiring manager survey question: "Did one or more of the applicants for this position make you aware of their current salary, either during an interview or at any other stage in the hiring process?"
"Hiring Manager Aware by Other Means" derived from hiring manager survey question: "Did you know the current salary of one or more applicants through other means, for example if they were an internal applicant from your team?"
Non-Compliance is coded as 1 for hiring managers who answered yest to one or both of these questions and as 0 otherwise.
All p-values reflect two-tailed tests.

Table 3 – The Effect of a Salary History Ban on Salary Offers, Study 1

	(1) Salary (ln)	(2) Salary (ln)	(3) Salary (ln)	(4) Salary (ln)	(5) Salary (ln)
Salary history ban	-0.00821	-0.0359	0.00475	0.0255	0.0431
	(0.0431)	(0.0806)	(0.0173)	(0.0328)	(0.0368)
Female candidate	-0.0255	-0.0452	-0.00494	0.00898	0.0406
	(0.0477)	(0.0680)	(0.0194)	(0.0269)	(0.0281)
Salary history ban ×		0.0388		-0.0288	-0.0446
Female candidate		(0.0955)		(0.0386)	(0.0425)
Internal candidate			-0.0120	-0.0127	-0.00631
			(0.0190)	(0.0190)	(0.0205)
Candidate negotiated			0.0323	0.0331	0.00744
			(0.0250)	(0.0251)	(0.0278)
Number of applications			0.000177	0.000184	0.000183
			(0.000144)	(0.000144)	(0.000163)
Female hiring manager			-0.00743	-0.00795	-0.0180
			(0.0188)	(0.0189)	(0.0199)
Compliant roles only	No	No	No	No	Yes
Salary band indicators	No	No	Yes	Yes	Yes
Observations	230	230	226	226	144
R^2	0.001	0.002	0.854	0.855	0.861

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 4 – Correlation Between Salary History Ban and Salary Offers, Study 2

	(1) Salary (ln)	(2) Salary (ln)	(3) Salary (ln)	(4) Salary (ln)	(5) Salary (ln)	(6) Salary (ln)
Salary history ban period	-0.100*	-0.0985*	-0.118*	-0.100*	-0.0987*	-0.123*
	(0.0459)	(0.0459)	(0.0542)	(0.0430)	(0.0430)	(0.0555)
Female candidate		-0.0582**	-0.0639**		-0.0514^{+}	-0.0726^{+}
		(0.0183)	(0.0202)		(0.0292)	(0.0426)
Salary history ban period × Female candidate			0.0314			0.0401
			(0.0473)			(0.0584)
Observations	3687	3687	3687	1306	1306	1306
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.033	0.036	0.036	0.007	0.010	0.010

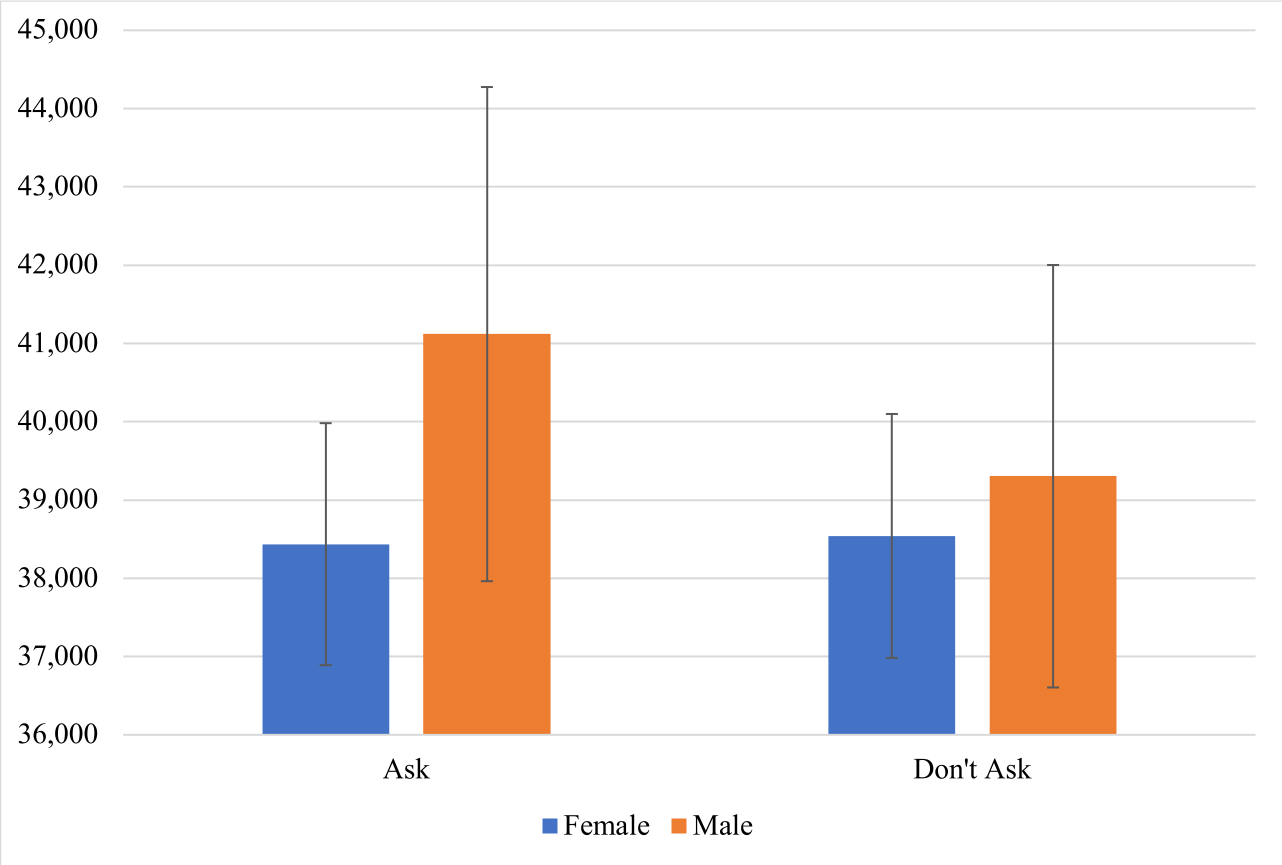
Standard errors in parentheses
We narrow the observation window in models 4-6 to compare the treated period to an equivalent number of pre-treatment days.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 5 – Correlation Between Salary History Ban and Salary Offers With Job Title Indicators, Study 2

	(1) Salary (ln)	(2) Salary (ln)	(3) Salary (ln)	(4) Salary (ln)	(5) Salary (ln)	(6) Salary (ln)
Salary history ban period	-0.100*	-0.101*	-0.0855	-0.139**	-0.137**	-0.149*
	(0.0496)	(0.0496)	(0.0594)	(0.0487)	(0.0487)	(0.0632)
Female candidate		0.00650	0.0105		-0.0476	-0.0580
		(0.0200)	(0.0217)		(0.0344)	(0.0483)
Salary history ban period × Female candidate			-0.0244			0.0202
			(0.0517)			(0.0655)
Observations	3686	3686	3686	1306	1306	1306
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.548	0.548	0.548	0.644	0.645	0.645

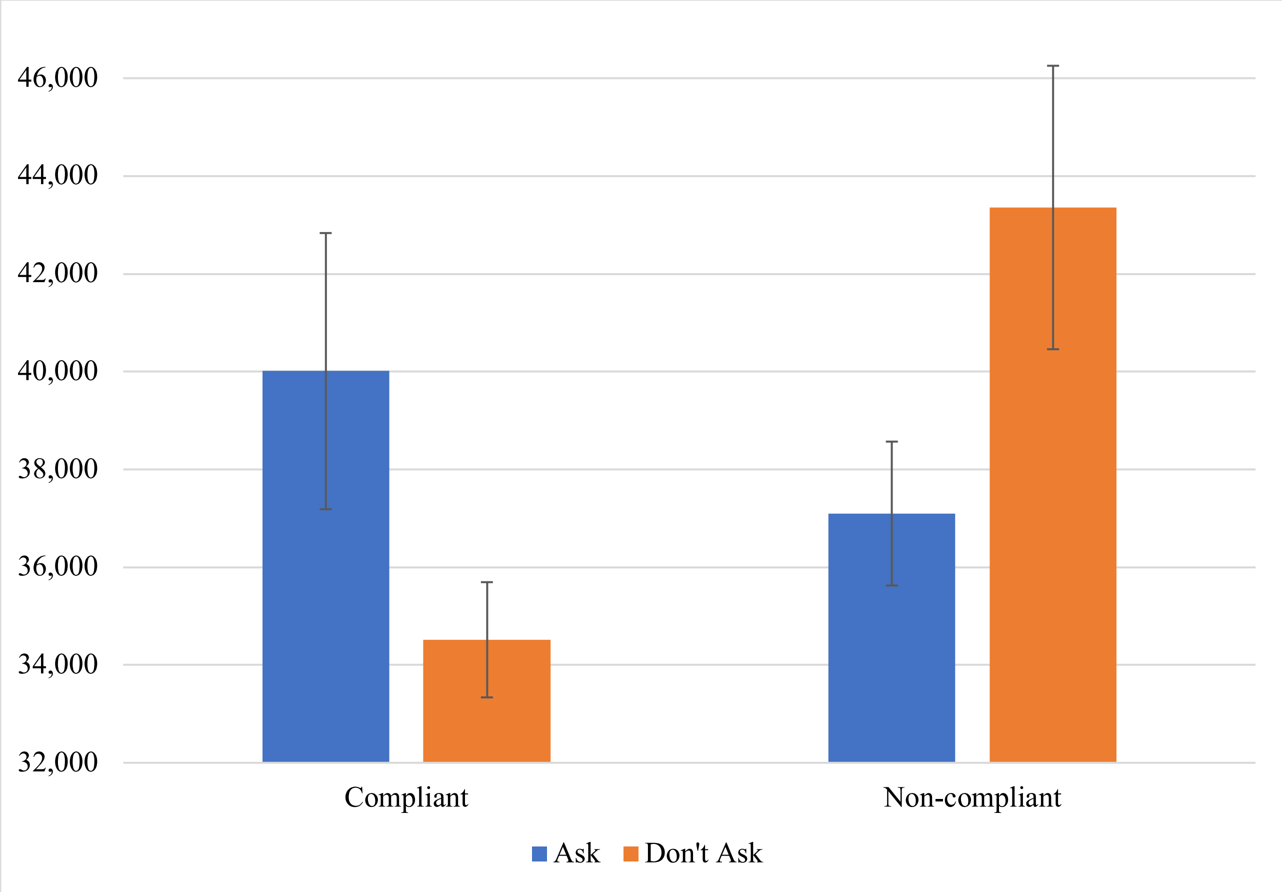
Standard errors in parentheses
We narrow the observation window in models 4-6 to compare the treated period to an equivalent number of pre-treatment days.
All models include job title indicator variables (i.e., fixed effects)
We exclude a single observation for which the job title field was left blank
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Figure 1 “Intent to Treat” Analysis of a Salary History Ban on Salary Offers by Gender, Study 1



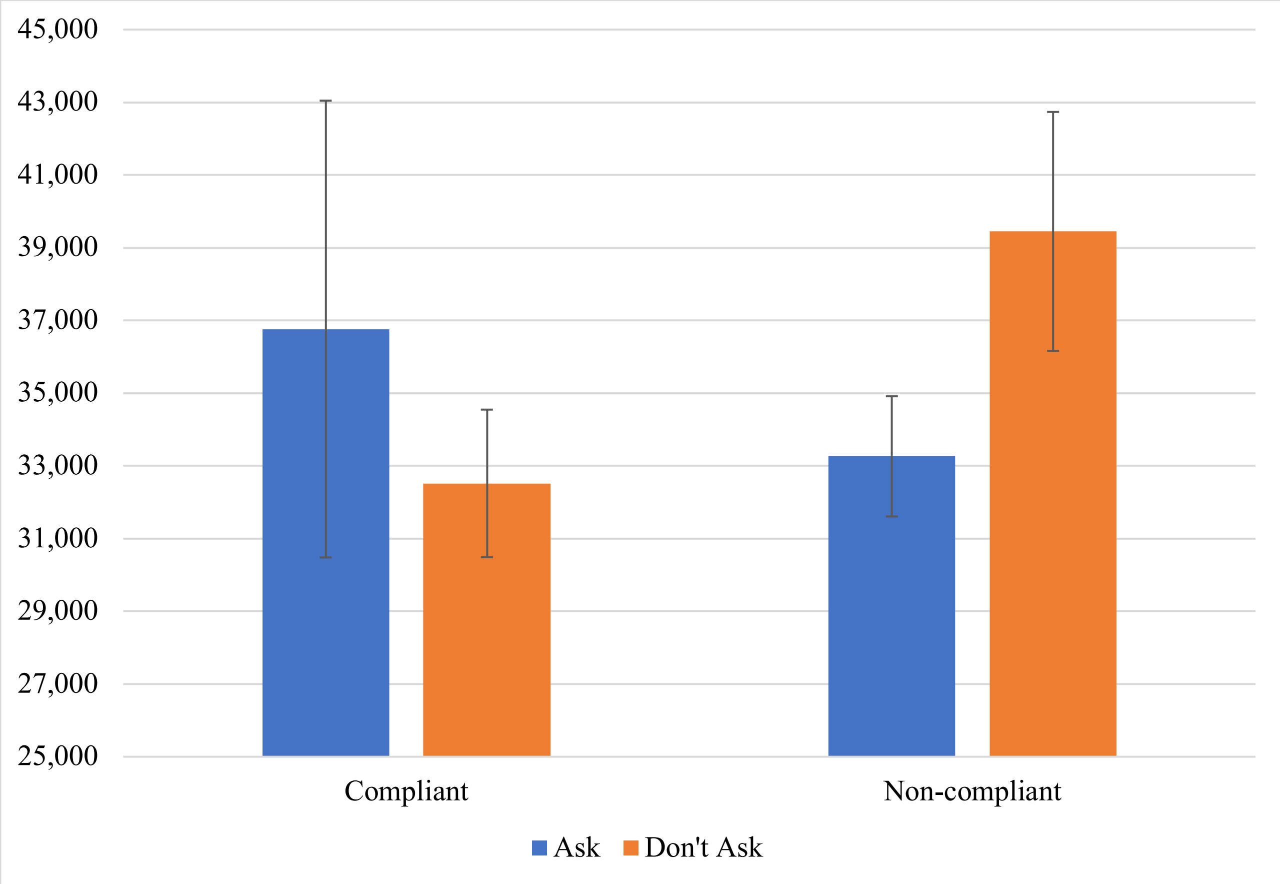
N = 116 “Ask” observations and 114 “Don’t Ask” observations. This figure depicts a raw comparison of means with standard error bars.

Figure 2 “Local Average Treatment Effect” of a Salary History Ban on Salary Offers, Study 1



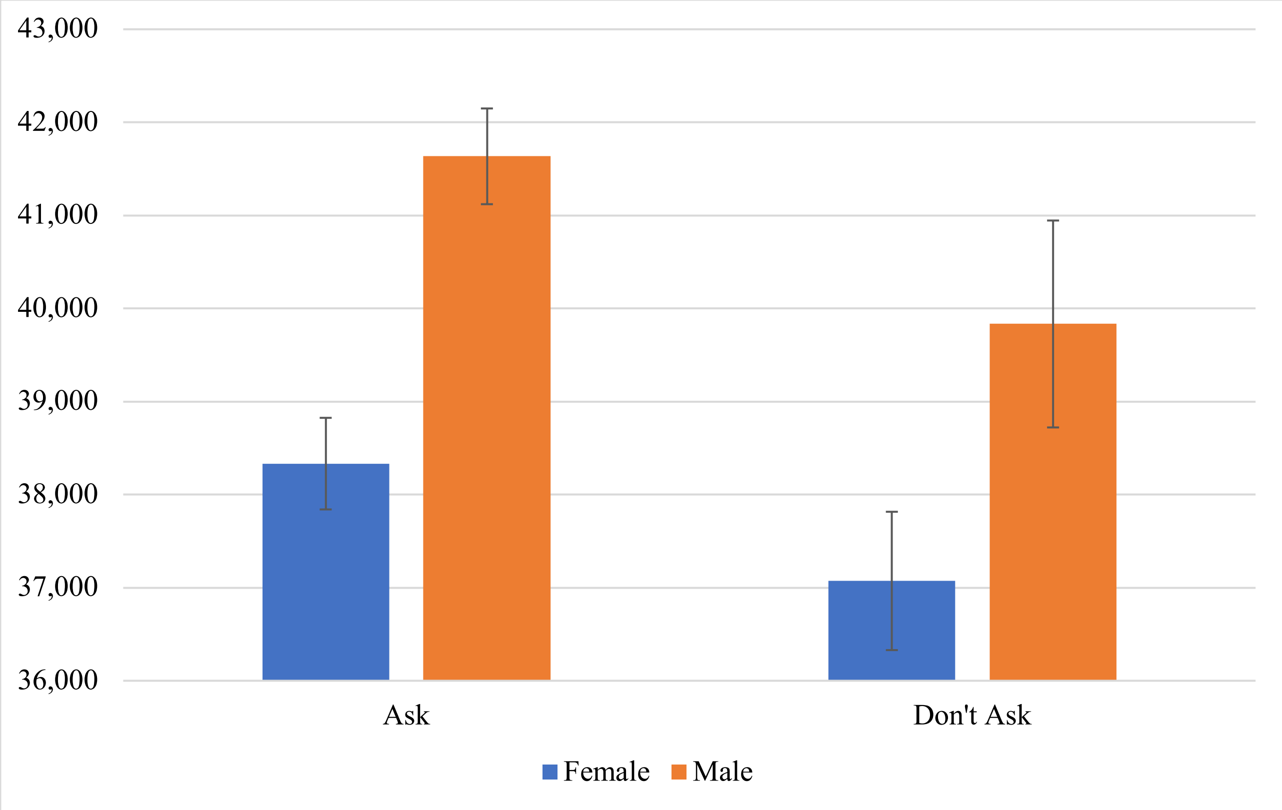
N = 82 fully compliant observations and 125 non-compliant observations. This figure depicts a raw comparison of means with standard error bars. Compliant observations are “don’t ask” roles in which hiring managers were unaware of all applicants’ salary history, and “ask” roles in which hiring managers *were* aware of applicants’ salary history.

Figure 3 Average Current Salary of Successful Candidates Between Condition, by Compliance, Study 1



N = 82 fully compliant observations and 125 non-compliant observations. This figure depicts a raw comparison of means with standard error bars. Compliant observations are "don't ask" roles in which hiring managers were unaware of all applicants' salary history, and "ask" roles in which hiring managers *were* aware of applicants' salary history. "Current salary" refers to salary earned prior to being hired at our research site. Per footnote 10, we have excluded from this graph an extreme outlier in a compliant "ask" role: A current salary of £156,000, by far the largest in our sample.

Figure 4 Correlation Between Salary History Ban and Salary Offers for Men and Women, Study 2



N = 3,000 “Ask” observations and 687 “Don’t Ask” observations. This figure depicts a raw comparison of means with standard error bars.

Appendix A: Salary History Ban Advertisements in New York City Subway

Source: *New York Daily News*

Appendix B: Field Experiment Survey Measures

Hiring Manager Survey:

We successfully surveyed 207 hiring managers out of the 230 who closed roles during our observation window, for a response rate of 90%. Our survey consisted of the following questions:

- Please enter your full e-mail address below.
- Please enter the position reference number for the job you have just filled. If you do not know it, please e-mail the recruitment team.
- Was this position red or green?
- Did one or more of the applicants for this position make you aware of their current salary, either during an interview or at any other stage in the hiring process?
- Did you know the current salary of one or more of the applicants through other means, for example if they were an internal applicant from your team?
- Regarding your successful candidate, to what extent did your knowledge of their current compensation influence the salary amount that you offered them? Please provide your answer on a scale from 1 to 5, with 1 being "not at all" and 5 being "substantially."
- Please describe, for this particular job, how knowing—or NOT knowing—applicants' current compensation influenced your decision-making process. If applicable, please include both positives and negatives.

Successful Candidate Survey

We successfully surveyed 189 new hires out of the 230 who changed roles during our observation window, for a response rate of 82%. With the assistance of the recruitment team, however, we were able to obtain a measure of current salary for 212 hires, or 92%. Our survey consisted of the following questions:

- Please enter the position reference number of the job to which you applied. Your interviewer should have supplied you with this at the end of your interview. If they did not, please feel free to contact them in order to obtain it.
- Did your interviewer ask you what your current base salary per annum is?
- Did you inform your interviewer what your current base salary per annum is?
- What did you state as your **expectation** for base salary per annum? Please enter just the number, for example: 45000. **Please type NA if you did not state your salary expectation during your interview.**
- Please list below your current base salary per annum, as well as your base salaries per annum for the prior three years. In each case enter just the number, for example: 40000. NOTE: "Current salary" refers to the amount you earned prior to your current role with [institution]. Please respond accordingly if you are taking this survey after assuming your new responsibilities. **Please type NA if you did not earn a base salary per annum for any of these prior years.**
- Are you currently employed?
- What is your age? Please enter your response in the form of a number (e.g., 35 instead of thirty-five).
- What is your gender?
- Which of the following options best describes your current living situation?
- How many children currently reside in your household? Please include children who also spend part of their time in another household.
- Please write your name as it is listed on your passport in the box below.
- Please use the space below to add any additional thoughts or comments that you have.

Appendix C: Assessing the Impact of SHB on *Raise*

We observed 230 hires in Study 1. In addition to assessing the impact of SHB on *salary*, we also investigated whether it impacted *raise*, that is, the difference between the offer received by a successful candidate and their current salary. We were able to glean salary history data from the HR database at our research site for all internal candidates and some external candidates; however, we relied on self-report for the remainder of successful external candidates. Although this undoubtedly reduces the precision of our estimates, it should not systematically bias our results given the successful randomization of roles into "ask" and "don't ask" conditions.

Data. Current salary was unobtrusively collected for all *internal* candidates and some external candidates. For the remaining observations we relied on our surveys of successful candidates. In total, we were able to collect current salary for 212 out of 230 hires (92%). The average *raise* was £2,587 with a standard deviation of £10,240.

Analysis. We first tested for a main effect of SHB on *raise* by conducting a t-test with respect to treatment. This comparison did not attain statistical significance ($p = .63$). Next we assessed whether SHB differentially impacted *raise* for women. We did this by regressing *raise* on a binary indicator of *female*, a binary indicator of *SHB*, and an interaction term between the two. This interaction term did not attain statistical significance ($p = .65$). We also regressed raise on a binary indicator of *internal* hire, a binary indicator of *SHB*, and an interaction between the two—this term did not attain statistical significance either ($p = .71$). Restricting the sample to complier roles, including the covariates we specified in our main analysis, and logging *raise* did not materially impact these results.

Robustness check. Exactly 25% of respondents reported a pay cut or no pay change associated with their mobility. Accordingly, we tested for a main effect of *SHB* on *raise*, as well as a moderating effect of gender, after winsorizing the distribution by recoding observations below the 5th percentile (-£10,000) as equal to the 5th percentile and observations above the 95th percentile (£12,500) as equal to the 95th percentile. We did not observe a significant main effect of SHB in these data ($p = .92$ for the t-test), nor an interaction effect with respect to gender ($p = .96$ on the interaction term) or internal candidates ($p = .39$ on the interaction term). In conclusion, we find no evidence that SHB influenced raise in these data.

Appendix D: Quasi-experimental Analysis of Experimental Data

Table A1 – Quasi-experimental Analysis Incorporating Pre-Experiment Hiring Data

	(1)	(2)	(3)	(4)	(5)
	Salary (ln)	Salary (ln)	Salary (ln)	Salary (ln)	Salary (ln)
Salary history ban	0.0588	-0.00764	-0.00936	-0.0774	0.0165
	(0.0362)	(0.0436)	(0.0434)	(0.0705)	(0.0267)
Pre-experiment hire		-0.103^{**}	-0.103^{**}	-0.103^{**}	-0.0143
		(0.0382)	(0.0380)	(0.0380)	(0.0145)
Female candidate			-0.0769^{*}	-0.103^{*}	-0.0126
			(0.0348)	(0.0406)	(0.0155)
Salary history ban × Female candidate				0.0961	-0.00359
				(0.0786)	(0.0297)
Salary band indicators	No	No	No	No	Yes
Observations	441	441	441	441	441
R^2	0.006	0.022	0.033	0.036	0.865

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

For this analysis, we merged in hiring data from the year prior to our experiment and re-ran our models. The goal was to increase estimate precision via a quasi-experimental analysis—effectively a difference-in-differences estimator—that incorporates pre-experiment cases as a baseline. However, this approach limited the number of covariates we could use, due to the need for alignment with the data available to us from the hiring database in the pre-experiment period.[15] In sum, with respect to the interaction of *SHB* and *female candidate*, the inclusion of salary *band* fixed effects produced a confidence interval that ranges from -6% to 6%, attenuating the relevant point estimate beyond consequence.

[15] We dropped seven cases from the pre-experiment hiring period due to missing salary band observations.

Appendix E: Instrumental Variable Analysis of Experimental Data

Table A2 – Instrumental Variable Regression

	(1) Salary (ln)	(2) Salary (ln)	(3) Salary (ln)	(4) Salary (ln)
Salary history withheld (instrumented)	0.0101	0.00910	0.0582	0.280
	(0.153)	(0.153)	(0.0562)	(0.310)
Female candidate		-0.0116	0.0216	0.200
		(0.0472)	(0.0198)	(0.206)
Internal candidate			-0.00846	-0.0134
			(0.0192)	(0.0240)
Candidate negotiated			0.00782	0.0230
			(0.0253)	(0.0324)
Number of applications			0.000254^{+}	0.000304^{+}
			(0.000149)	(0.000183)
Female hiring manager			-0.00673	0.000309
			(0.0196)	(0.0264)
Salary history withheld (instrumented) × Female candidate				-0.273
				(0.306)
Salary band indicators	No	No	Yes	Yes
Observations	207	207	203	203
R^2	0.000	0.001	0.852	0.805

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

For this analysis, we conducted an instrumental variables (IV) regression in order to gain further insight into the potential effect size of SHB. To do so, we leveraged our non-compliance data: Specifically, the question "Did one or more of the applicants for this position make you aware of their current salary, either during an interview or at any other stage in the hiring process?" We defined a "no" response as treated and a "yes" response as control. We then used random assignment to a "don't ask" role to instrument for this treatment. The intuition is that assignment to a "don't ask" role provides a sizeable exogenous shock to the probability of treatment, that is, making an offer without knowing candidates' salary history.

Model 1 shows the effect of the instrument alone. Neither the confidence interval (-25% to 36%, p = 0.95) nor the point estimate is especially compelling. Model 2 adds the covariate *female candidate*, the effect of which is not statistically significant. Model 3 adds our relevant covariates, including indicators for salary band. In this specification, while our instrument remains non-significant (p = .30), its confidence interval shifts upward, with a lower bound of -5% and an upper bound of 18%. Model 4 interacts the instrument with *female* candidate. The interaction term fails to achieve conventional levels of statistical significance (confidence interval range of -58% to 39%, p = .37), but the point estimates suggest that the effect is negative for women in similar proportion to what we find in our main analysis.