

Gender Differences in Negotiations and Labor Market Outcomes: Evidence from an Information Intervention with College Students*

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Abstract

We assess the role of information gaps in understanding gender differences in negotiation behavior by conducting a randomized information experiment on the 2018 to 2020 graduating cohorts of undergraduate business majors from Boston University. Prior to starting their job search, treated students were provided with objective information about the gender gap in negotiation among their peers along with the earnings changes conditional on negotiating. We find sizable immediate effects on negotiation intentions that persist to actual negotiation behavior, particularly for men. While the treatment affects women’s negotiation behavior through belief-updating, the effects on men’s behavior are primarily through increased salience of the information. Further, we find some evidence that gender-specific treatment spillovers likely contribute to the smaller average treatment effects on behavior for women. Overall, our findings suggest that such information interventions can help to nudge women who have potentially large financial returns to negotiation to realize these gains.

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1 Introduction

The gender pay gap remains a persistent feature of the labor market. Recent research has examined the role of systematic differences in preferences and beliefs in explaining these gaps. One particular explanation that has received considerable attention is differential salary negotiation behavior across genders. A growing body of work, both in the lab and in the field, documents that women are less likely to initiate wage negotiations (see, for example, [Babcock et al., 2006](#); [Babcock and Laschever, 2003](#); [Dittrich et al., 2014](#); [Leibbrandt and List, 2015](#)) and that these differences matter for the gender wage gap ([Card et al., 2016](#); [Biasi and Sarsons, 2022](#)) and for women’s career advancement ([Greig, 2008](#)).

Why these gender gaps in negotiation exist, however, is less well understood. Proposed explanations include the possibility that women may be less likely than men to perceive situations as negotiable and negotiate less effectively in ambiguous situations ([Bowles et al., 2005](#); [Small et al., 2007](#); [Leibbrandt and List, 2015](#)), women face higher real or perceived costs of negotiation due to backlash or reputational effects ([Bowles et al., 2007](#); [Tinsley et al., 2009](#); [Amanatullah and Morris, 2010](#)), and gender differences in preferences such as risk aversion, confidence, or fairness concerns.¹ An alternative explanation has also been offered by [Exley et al. \(2020\)](#) who argue, based on a laboratory experiment, that women may rationally opt-out of negotiation because they are less effective at it. In this paper, we focus on another potential explanation that has received less attention in the literature: the role of information gaps regarding the prevalence and expected gains from negotiation.

That imperfect information can cause ill-informed choices has been explored in a variety of contexts such as education, social benefits, and retirement savings (e.g., [Jensen, 2010](#); [Bleemer and Zafar, 2018](#); [Bhargava and Manoli, 2015](#); [Duflo and Saez, 2003](#); [Beshears et al., 2015](#)). Yet, to our knowledge, no work to date has systematically explored how information gaps affect negotiation behavior in the field and the implications for gender gaps in negotiation and pay. There are reasons to expect that misinformation about the prevalence and gains to negotiation is likely to matter for individuals’ decision to negotiate. In a survey of recent undergraduate alumni from Boston University’s Questrom School of Business (Questrom) that we conducted, we find that men are 1.6 times as likely as women to have negotiated some monetary aspect of their first job after leaving university, and that there are large average returns to negotiating for both sexes, with earnings increasing by about 12%. Moreover, graduates’ beliefs about their peers’ negotiation decisions and the expected wage gain from negotiations are systematically biased, and importantly, these beliefs are correlated with the decision to negotiate. Women are also weakly more likely to cite not knowing that negotiating was an option, suggesting that information gaps could have possible gendered effects.

To formally assess the role of information gaps on negotiation behavior, we conduct a randomized information experiment with the 2018, 2019, and 2020 graduating cohorts from Questrom

¹See survey papers by [Croson and Gneezy \(2009\)](#) and [Bertrand \(2011\)](#) for a comprehensive discussion of gender differences in such preferences.

where, prior to the start of their job search process, we provide a subset of students with information about the relative difference in negotiation rates by gender along with the efficacy of negotiation (i.e., the impact of negotiation on starting salaries, which we found in the initial survey to be similar by gender) and a graphical illustration of the cumulative earnings gains over the lifecycle due to negotiation. The information was derived from the above-mentioned online survey of the 2013 to 2017 Questrom graduating classes that we conducted prior to the start of the information experiment.

The information treatment was designed to provide students with *objective* information on the monetary aspects of negotiation which potentially has differential impact by gender, without explicitly encouraging students to “lean-in.” This is an especially important consideration given the findings of [Exley et al. \(2020\)](#) and the fact that we do not know ex ante whether nudging students to negotiate will make them better off. In terms of the prevalence of negotiation, we chose to provide the relative gender ratio of negotiating and not the levels, since the actual levels of negotiation are low relative to ex ante expectations. Although the information has a gender dimension, neither piece of information is inherently paternalistic nor prescriptive.² Furthermore, the data that underlies the information is not difficult to collect, thus ensuring that the information intervention can be scaled up quite easily.

The intervention took place in two mandatory modules that Questrom students typically take in their junior and senior year. Module instructors set aside about 10 minutes of class time for students to complete the online survey (that embedded the information intervention) in-class. The fact that the survey was conducted in-class ensured a high response rate of about 80% among the 1,200 or so students who were enrolled in the two modules. The survey experiment consisted of three main stages. At baseline, we elicited students’ expectations about the likelihood of negotiating various aspects of their first job after graduation, their perceived increase in total compensation (conditional on reporting a positive likelihood of negotiating some monetary aspect of the offer), as well as their beliefs about the negotiation behavior of recent Questrom undergraduate business majors by gender. In the next stage, we provided a randomly selected subsample of students (two-thirds of the sample) with information about the male-to-female ratio of the share of alumni who negotiated some monetary aspect of their starting job offer and the reported increase in starting salary resulting from negotiation (see Figure 1 for the screenshot of the presented information). The graphical illustration of the earnings gain from negotiation provided as part of the information treatment is meant to highlight the role of simple compounding, where small differences in starting salaries due to negotiations can result in large cumulative gains over one’s lifecycle.³ The remaining one-third of the sample (the control group) was not provided with any additional information. In the final stage, we re-elicited the intended likelihood of negotiation as well as perceived salary changes due to negotiation from all respondents. To investigate the impacts of the information on actual negotiation behavior, we conducted a follow-up survey about seven months post-graduation,

²Consistent with this, we do not find any systematic impact of the information on gender norms.

³The information provided in the figure is meant to be stylized and is based only on differences in starting salary due to negotiation, assuming a constant wage growth of 5%.

where typically, about 90% of students have secured jobs.

We find that the information treatment has a sizable positive immediate effect on students' stated likelihood of negotiating any monetary aspect of their first job. Intended negotiation likelihood increases by about 11 and 9 percentage points for men and women, respectively. Given the base intended negotiation rates of 61 (54) percent for men (women), these are economically large impacts. While the information focused on monetary aspects of negotiation, the intervention appears to have also nudged students' intentions of negotiating non-monetary aspects of the job, with both men and women increasing their stated likelihood of negotiating non-monetary aspects by 7 percentage points (on a base intended likelihood of about 47 percent).

These average effects persist to actual negotiation behavior, particularly for men. In the follow-up survey, male students who were exposed to the information were significantly more likely to negotiate any monetary aspect of their first job relative to control males (an increase of 12 percentage points on a base of about 20 percent). However, treated women were, on average, no more likely to negotiate monetary aspects of their first job than their control female peers. The information intervention also increased the likelihood that students negotiated non-monetary aspects of their first jobs by 7 and 4 percentage points for men and women, respectively.

Next, we investigate the channels through which the provision of information may have affected negotiation behavior. In particular, the information intervention could impact negotiation behavior by (1) modifying students' beliefs about negotiation through an information-based updating channel, and/or (2) increasing the saliency of the information or of negotiation in general (e.g., DellaVigna, 2009; Chetty et al., 2009; Bleemer and Zafar, 2018). We use the term "saliency" rather broadly here: even if respondents were ex ante aware of the information we presented, our information treatment may nudge them to consider the option of negotiating job offers (since we are drawing their attention to one specific aspect of the job search process). Or, even if students were already actively thinking about negotiating and were aware of the presented statistics, the way the information was packaged – especially highlighting the accumulated returns over the lifecycle – may have made some students realize that the gains from negotiating in a dynamic setting may be larger than they had thought. While these two mechanisms are quite distinct, our setup does not allow us to disentangle them. We use our study design to test the relative importance of the information-based vs. saliency channels by relating pre-information beliefs to realized negotiation behavior. Under the information-based updating channel, treatment effects should vary by the size and direction of the difference between a student's pre-information beliefs and the objective information provided (i.e., perception gap). If treated students fully update their beliefs to the provided information, initial perception gaps should not matter for actual negotiation behavior. For salience to be the main channel, treatment effects should not vary by the size of perception gaps.

We find that, for women, the primary channel through which the information treatment affects reported negotiation behavior is information-updating. Female students who significantly overestimated the ratio of male to female negotiation rates (or, conversely, underestimated the relative

negotiation rate of females) were *more* likely to negotiate after the information treatment compared with the control group, whereas those in the treatment group who underestimated the ratio (i.e., thought that women negotiated relatively more than they actually do) were *less* likely to negotiate. The fact that the sizes of the two groups (those who underestimated and those who overestimated the ratio) are fairly similar explains why we find a close to zero *average* treatment effect for women. Men’s negotiation behavior, on the other hand, is generally unaffected by prior beliefs about the gender gap in negotiation.

To further investigate the role of information updating, we consider the two components of the gender ratio in negotiation rates separately. We show that, for women, the magnitude of the treatment effects varies systematically with prior beliefs about the female negotiation rate, but not the male negotiation rate. By contrast, the treatment effects for men do not appear to vary by prior beliefs of either male or female negotiation rates. For both sexes, we find little evidence that prior beliefs about negotiation efficacy matter for the size of the treatment effects. These results suggest that the information intervention affected women’s negotiation behavior largely by inducing those with particularly pessimistic beliefs about female negotiation rates to update their beliefs. On the other hand, for men, the information treatment affected male students’ negotiation behavior not through information-updating channels but through increased salience.

That very different mechanisms are behind the effects of the intervention for men and women suggests that the characteristics of the marginal student moved to negotiate by the experiment might vary by gender. To test this hypothesis, we look at the effect of the treatment on the average efficacy of negotiation by gender. Interestingly, we find that treated men who negotiated have much lower average efficacy than control men who negotiated; indeed, about half of the men in the treatment group who negotiated failed to increase their offer, whereas just one in five men who negotiated in the control group experienced such “failed” negotiations. By contrast, negotiation efficacy for the group of women who were induced to negotiate by the experiment is similar, and if anything, higher than that for the control group.

These results suggest that information frictions might explain why some women do not negotiate even when the returns to doing so are quite high. Our findings suggest that there are settings where correcting misperceptions about the prevalence of negotiation among own-gender peers can induce more women to engage in monetary negotiations that would ultimately benefit them financially.⁴ In our setting, however, treated men do not appear to enjoy the same financial benefits from negotiating more.

It remains somewhat of a puzzle that men, regardless of prior beliefs, uniformly reacted to the new information by negotiating more, whereas women did not. We argue that for men, the presence of a “level” treatment effect is consistent with salience driving men’s decision to negotiate; yet, there is no apparent reason why these channels should only apply to men and not to women. To

⁴While this finding appears to differ from [Exley et al. \(2020\)](#) who find that women’s decisions to negotiate in the lab are consistent with “knowing when to ask,” there are important differences in setting. [Exley et al. \(2020\)](#) considers a stylized laboratory setting where there is no uncertainty regarding the possibility of negotiation, the size of the surplus is ex ante known, participants are anonymous, and there is a large wage penalty for failed negotiations.

provide further insight on this, we examine the behavior of women in previous (untreated) cohorts. Interestingly, we find that women in *both* the treated and control groups negotiate at significantly higher rates than women in previous cohorts. We explore two possible explanations. First, external factors such as concurrent social movements (e.g., #MeToo) and/or changes in the curriculum or career preparation may have had a larger impact on female students' negotiation decisions, thereby muting the (average) effect of our treatment. One specific example is the introduction of a compulsory course in the Questrom undergraduate curriculum on career navigation in the Fall of 2017 that provided students with gender-specific information on negotiation rates and efficacy. The second possibility is that the null average effects for female students could be due to the presence of gendered spillovers from the treatment to the control group.

To test for the presence of gender-specific spillovers, we exploit quasi-random variation in treatment intensity within peer networks generated by the experiment. Defining a peer group based on major specialty, cohort, and gender, we find that control women in peer groups with a larger share of treated individuals are significantly more likely to negotiate some monetary aspect of their first job. Such effects, however, are not observed for control men. We complement these suggestive findings with an additional follow-up survey of the 2018 and 2019 cohorts designed specifically to test for potential spillover mechanisms. Survey evidence indicates that treated individuals shared the information in the treatment message at high rates, and disproportionately with females. Women in the control group were 50 percent more likely than control men to have learned about the treatment information from their peers, although this difference is imprecisely estimated. Having received this information, women in the control group were also more likely to report acting on the information relative to men in the control group. Taken together, these pieces of evidence point toward the presence of spillovers among women in our sample, and further suggest that peer networks play somewhat different roles for men and women in the job search process.⁵

To quantify the potential role of spillovers in explaining the null average treatment effects on monetary negotiation for women, we re-estimate our main model, making some assumptions to limit the extent of spillovers in the control group. First, we exclude control students who report learning about the information. Second, we use an upper bound on the maximum potential role of spillovers to account for the fact that we do not know the spillover status for all responses (much in the spirit of [Manski, 1990](#)). Using either approach, we find large treatment effects for women of between 6 and 7 percentage points (about half the size of the average treatment effect for men); however, the coefficients are not precisely estimated. The fact that the treatment endogenously spread through social networks demonstrates the intrinsic value students placed on the provided information. That this spread was gender-specific highlights the difficulty of predicting the overall impacts of information interventions.

Turning to the overall impacts on the gender earnings gap, comparing across cohorts and across treated and control groups within cohort, perhaps surprisingly, we find little evidence that the

⁵Our results are broadly consistent with existing work on network structure and communication leading to different impacts by gender ([Ibarra, 1993](#); [Yang et al., 2019](#); [Lindenlaub and Prummer, 2021](#)).

treatment affected the overall gender earnings gap. This is likely to be because the treatment-induced gains from negotiation occurred in different parts of the offer distribution by gender. In addition, we find little evidence that the intervention impacted reported job satisfaction, although treated individuals report being more likely to stay with their current employer over the next five years. The information provided appears to be useful to students during the job search process. In the survey, more than two-thirds of treated respondents rated the information as a 4 or 5, on a five-point scale of usefulness. Moreover, the existence of spillovers is further evidence of the usefulness of the information. Taken together, our findings suggest that, given the relatively low cost of information campaigns, such interventions are likely to be welfare improving, without worsening gender gaps.

This paper is related to several strands of research. We contribute to the large and growing literature on gender differences in negotiation, as discussed earlier, by confirming that gender differences in negotiation are also present for early-career salary negotiations and exploring the role of information provision on negotiation behavior using a field experiment implemented at a critical point when college students begin searching for their first job after graduation. While there is a growing literature on initiatives aimed at changing women’s approach to negotiation (see [Recalde and Vesterlund, 2023](#), for a review), there is considerably less work on the role of information gaps on gender differences in negotiation behavior. An important exception is [Rigdon \(2012\)](#) who uses a laboratory setting and shows that the provision of social information on participant negotiation behavior eliminated the gap in negotiations and wages in the demand ultimatum game. Likewise, [Rousille \(2022\)](#) studies gender differences in asking salaries in the technology sector and finds that when candidates are provided with information about the median bid salary for similar candidates, the ask gap is eliminated. [Chotiputsilp and Kim \(2023\)](#) examine the effects of a randomized intervention that provided negotiation training and salary information to Thai job seekers. They find that the provision of salary information increased reported salaries; however, while those who received negotiation training in addition to salary information negotiated more, they reported lower earnings and fewer non-salary benefits compared to the group that only received information. In our setup, we study the impacts of a realistic and scalable intervention that provides timely information on negotiation behavior and efficacy in a general setting of job search after college. Our data allows us to examine effects on actual negotiation behavior, negotiation/earnings outcomes, and directly explore underlying mechanisms.

Our paper also broadly relates to the literature on the role of information in wage setting and the job search process. Recent papers examine the effects of pay transparency on wages and the gender pay gap and generally finds that pay transparency induces wage compression and results in the narrowing of the gender gap in pay ([Mas, 2017](#); [Baker et al., 2019](#); [Bennedsen et al., 2019](#)). Finally, [Cortés et al. \(2023\)](#) focus on gender differences in the job search process among undergraduate business majors, and show that gender differences in overoptimism about future offers contribute to gender gaps in job acceptance timing and pay. Their results suggest that correcting biased beliefs about the offer distribution can improve welfare for both sexes and reduce the gender gap

in starting salaries. Our paper contributes to this literature by exploring how providing objective information about negotiation at a key point when students are starting to think about job search affects their negotiation behavior, earnings, and job search experience.

The remainder of the paper is organized as follows. Section 2 describes the data and main findings from the survey of recent Questrom undergraduate business alumni that motivate the information intervention. Section 3 presents the results of the information intervention. Section 4 investigates underlying mechanisms, focusing on the relative roles of information-based belief updating vs. increased salience/framing, as well as treatment spillovers. Section 5 examines the value of the information and the impacts of the information intervention on other job-related outcomes. Section 6 concludes.

2 Data and Descriptive Statistics

The data are from original surveys administered to undergraduate business majors from Boston University’s Questrom School of Business. Questrom is a selective, private business school that offers both undergraduate and graduate programs. It has a relatively large undergraduate enrollment of about 3,200 students across four years of study. Our analysis is based on three main sets of survey instruments: (1) a retrospective survey of recent Questrom alumni, (2) a prospective set of surveys of current Questrom students, and (3) a follow-up survey conducted after graduation. The online surveys were administered using the SurveyMonkey platform.⁶ We describe each survey in detail in the subsections that follow.

2.1 Alumni Survey

This online survey forms the basis of our information intervention. It was administered to the 2013 to 2017 Questrom graduating classes between April 2017 and February 2018. We obtained a list of student emails from the Questrom Alumni Office and invited eligible alumni to participate in the online survey via email. The survey took approximately 20 minutes to complete and individuals were compensated with a \$20 Amazon gift card for successfully completing the survey. A total of about 1,000 alumni completed the survey, corresponding to a response rate of about 20%.⁷ The survey included questions on demographic and academic background, salary and other job characteristics (for the initial as well as current job), negotiation behavior, perceived ability, risk aversion, and beliefs about salary and negotiation behavior of peers by gender. Central to our analysis, we collected detailed information on the negotiation behavior of alumni for their initial job search, including the aspects of the job negotiated on, negotiation outcomes, and for those who reported not negotiating, the reasons why.

⁶When appropriate, questions have built-in logical checks. Item non-response is rare.

⁷The response rate for our Alumni survey is broadly comparable to that of other surveys conducted on similar populations – for example, the response rate for [Bertrand et al. \(2010\)](#)’s survey of University of Chicago MBA students was 31% while the response rate was around 10% to 12% across the 28 universities that participated in the recent Global COVID-19 Student Survey ([Jaeger et al., 2021](#)).

We discuss three main findings from our alumni survey that motivate our information intervention.

Finding 1: Gender gap in negotiation rates of monetary compensation. Table 1 presents summary statistics characterizing the negotiation behavior of Questrom Alumni from the survey. About 19 percent of men negotiated at least one monetary component of their job offer compared with 12 percent of women (p -value of difference < 0.01).⁸ We observe lower levels of non-monetary vs monetary negotiation for both genders. Interestingly, unlike with monetary negotiation, the non-monetary negotiation rate is very similar between males and females. Conditional on negotiating, efficacy is broadly similar by gender at around 10 percent, with men obtaining a slightly higher increase in their monetary compensation as a result of negotiation. Average earnings for men are around \$65,000, about \$8,000 more than the average for women. When we ask those students who reported not negotiating any aspect of their job offer why they did not negotiate, the most common answers are not wanting to send a negative signal or being perceived as too aggressive, and not being aware that they could negotiate. We find suggestive evidence on differential information by gender – women are five percentage points more likely to mention that they did not know that they could negotiate compared to men, but the difference is not significant at conventional levels (p -value = 0.163).

Finding 2: Both genders have inaccurate beliefs about peer negotiation behavior. To assess how well informed individuals are about the negotiation behavior of peers, we compare alumni beliefs about negotiation rates by gender and negotiation efficacy to actual negotiation rates of alumni.⁹ In particular, in Panel A of Figure 2, we present the distribution of beliefs about the ratio of male to female negotiation rate by gender, and compare it to the actual ratio of 1.6 (shown by the vertical line). A few observations stand out. First, most women (close to 80 percent) and a large share of men (more than 40 percent) are aware that women negotiate at lower rates than men. However, most men and slightly more than half of women underestimate the degree of gender differences in negotiation behavior. Finally, the distribution of beliefs varies by gender (K-S test p -value < 0.01), with female alumni having more accurate views about the male to female negotiation ratio.¹⁰ Focusing on beliefs about negotiation efficacy (i.e., the expected earnings change from negotiating) in Panel B, we see substantial heterogeneity. The median male (female) respondent expects the gains from negotiations to be 10 (7)% compared with the observed gains of 9%. Almost 60% of female students believe that the average efficacy for their Questrom

⁸The monetary components of the offer includes base salary, signing bonus, and bonus pay.

⁹We ask: “According to you, what percent (on a 0 to 100 scale) of those males (females) who started working full-time immediately after graduation negotiated some monetary aspect (base pay, signing bonus, bonus pay) of their offer?” and “Consider the male (female) Questrom graduates who negotiated their starting offer. As a result of negotiating the job offer, by how much on average (in percentage terms) do you think their total compensation (including base pay, signing bonus, and bonus pay) increased?”.

¹⁰Close to 40 percent of men report the same beliefs about the negotiation rates of women and men (i.e., a ratio of one).

peers is lower than what was found in the alumni survey. While women tend to be more pessimistic than their male counterparts, we cannot reject that the distributions are the same.

Finding 3: Beliefs about peer behavior are correlated with own negotiation behavior.

Figure 3 shows binned scatter plots of the relationship between beliefs about peer negotiation behavior and the share of alumni who negotiated some monetary aspect of their salary, where we vary the specific dimension of peer negotiation behavior. As observed, alumni who believe that their peers negotiated at higher rates are more likely to negotiate some monetary aspect of their first job offer. Beliefs about peer negotiation efficacy, on the other hand, appear to have little correlation with own negotiation behavior. These patterns provide suggestive evidence that beliefs about peer negotiation behavior might influence one’s willingness to negotiate.¹¹

Before we move to the details of the intervention, it is worth noting what the determinants of negotiating a job offer are. Using the rich set of observables and negotiating behavior in the alumni survey, we investigate this in Appendix Table A1. The first column shows that, perhaps unsurprisingly, there is negative selection into negotiating an offer: students who receive offers with lower earnings and without retirement benefits are more likely to negotiate. For example, receiving an offer that is \$10,000 lower is associated with a 2 percentage point higher likelihood of negotiating. Column (2) shows that individuals with higher financial risk tolerance are also more likely to negotiate. While most of these patterns are sensible, the R -squared of the regression is fairly low, suggesting that these controls explain a small part of the underlying variation. Moreover, the gender gap in negotiating only declines from 7.4 to 7.1 percentage points when all the controls in the second column are included. This also suggests that factors like risk aversion and offer characteristics explain only a small part of the gender gap in monetary negotiation.

Taken together, these findings suggest that an intervention providing information about negotiation prevalence and outcomes among peers may have the potential to shift one’s negotiation behavior and, perhaps, help close gender gaps in negotiation rates.

2.2 Survey Experiment

Our intervention consists of providing undergraduate business students from the 2018 to 2020 graduating cohorts before they started their job search (in the fall of their senior year or during their junior year) with factual information about the negotiation behavior of recent Questrom alumni. We provided three main pieces of information: (1) the ratio of men to women who negotiated any monetary aspects of their offer,¹² (2) the calculated earnings return to negotiation among those who negotiated, and (3) a graphical illustration of the implied lifetime monetary value of negotiating

¹¹We acknowledge that the observed patterns do not imply causation; reverse causality could also be at work with own behavior driving beliefs about peer behavior.

¹²We decided to present the ratio instead of the prevalence level by gender because a pilot survey showed that students’ beliefs about alumni negotiation rates were about three times as high as the observed rates (see average intentions for our experiment sample in Figure 4). We did not want to risk our intervention lowering negotiation rates.

one’s first salary, based on the calculated return and assuming no further negotiation and a constant wage growth. Figure 1 shows a screenshot of the information that was presented to the students.¹³ Note that we did not make any normative statements about negotiation along with the information that we provided.

2.2.1 In-Class Baseline Survey

We implemented our intervention in the baseline survey conducted in two mandatory courses for business undergraduates. The first survey was conducted in the Fall of 2017 in “Strategy, Innovation, and Global Competition (SI422),” a course mostly taken by seniors, and in “Analytics for Business Decisions (QM323),” a course mostly taken by juniors.¹⁴ We continued surveying students in QM323 in the Spring and Fall of 2018 and 2019. We asked instructors to devote the last 10 minutes of the class to the survey and to show the students a slide with the survey instructions. The slide mentioned that the survey was about students’ expectations about the labor market once they graduate, participation was voluntary, did not count for any course credit, and students would be compensated with a \$10 Amazon gift card for completing the survey. To reach students who were absent on the day the survey was conducted, we sent the survey via email to students who were enrolled but had not participated. Overall, the participation rate for students enrolled in the courses was over 85%, representing close to 70% of the 2018, 2019, and 2020 cohorts.

The survey included a section on demographic and academic background characteristics (gender, race, nationality, parents’ background, concentration, GPA, SAT, etc.), a section on job market expectations (plans after graduation, expected number of offers, accepted job’s industry, hours, monetary compensation and non-monetary benefits), questions specific to expectations about negotiation behavior (percent chance that the student will negotiate starting salary, other monetary compensation, non-monetary aspects of the job, and expected outcome of the negotiations), a section on students’ beliefs about gender-specific negotiation behavior and outcomes of past cohorts of Questrom alumni, and a short section on psychological attributes (risk aversion, self-reported relative ability, locus of control). At the end of the survey, we re-elicited students’ beliefs about their future negotiation behavior.

Importantly, to a randomly selected two-thirds of students, before re-eliciting their beliefs, we provided the information about negotiation behavior and outcomes of past Questrom cohorts collected from the alumni survey. Appendix Table A2 shows covariate balance across treatment and control groups. The reason why two-thirds of the sample was assigned to treatment is that, within the treatment group, half of the respondents in the 2019 and 2020 cohorts were assigned to receive email reminders of the information in the fall of their senior year.¹⁵ We were interested in

¹³The numbers provided in the treatment were calculated based on the 2014-2016 cohorts that we first surveyed, and thus differ somewhat from the numbers based on the full alumni sample that are presented in Table 1.

¹⁴Note that the timing of the intervention varies somewhat across the cohorts. For the first cohort (2018), seniors were treated in October of their senior year. For subsequent cohorts, juniors were treated in either the Fall or the Spring depending on when they took QM323. Students who already had secured jobs were excluded from the analysis. It is worth noting that our analysis includes Cohort fixed effects, and that our results are not driven by any one cohort.

¹⁵Reminders were not provided for the 2018 cohort as the survey experiment was conducted in the senior year.

testing whether such reminders change the saliency of the information. Since we find very noisy impacts of this reminder email, we do not focus on analyzing the treatment effects separately by assignment to the reminder group.¹⁶

Appendix Table A3 presents the descriptive statistics of the participants in the baseline survey, as well as how the sample compares with the overall Questrom population. Around half of our sample are women, and a little less than a third are foreign students. Female students have a slightly higher GPA than male students, and are much more likely to concentrate in marketing, whereas males are overrepresented in finance.¹⁷ Because negotiation rates might vary by industry, skill, and nationality, we will control for all background characteristics in our econometric models. The table also shows that our sample is very similar to the Questrom population with respect to gender and average GPA. Even though we observe small differences in the share of international students, credit hours, and the share of students in each concentration, crucially, gender differences across the samples in the key background characteristics are not statistically different (see Column (7)).

2.2.2 Follow-up Survey

We conducted a follow-up online survey of the participants in our survey intervention around eight months after graduation, when most of the students have found jobs. Our questions focused on students' actual negotiation behavior and job characteristics. We invited students via email, and incentivized their participation with up to a \$50 Amazon gift card. The response rate was 41%. The first column of Appendix Table A4 examines selection into participating in the follow-up survey. While women, native students, and those with higher GPAs are more likely to answer the survey, prior expected earnings are not predictive of answering the follow-up. The estimate on negotiation intentions is statistically precise but economically small (a 10 percentage point increase in the intended likelihood of negotiating increases the probability of taking the follow-up survey by only 1 percentage point).

3 Results from the Intervention

3.1 Immediate Impact on Intentions to Negotiate

Figure 4 displays the stated likelihood of negotiating monetary aspects separately by gender, treatment assignment and time (pre/post treatment).¹⁸ We observe that both women and men in

¹⁶Appendix Figure A10 shows the estimated treatment effects on negotiation behavior separately by reminder and non-reminder treatment arms.

¹⁷Undergraduate business majors in Questrom are required to declare at least one functional concentration. There are 11 functional concentrations that students can choose from. These include Accounting, Finance, General Management, Innovation and Entrepreneurship, International Management, Law, Management Information Systems, Marketing, Operations & Technology Management, Organizational Behavior, and Strategy. It is common to have more than one concentration

¹⁸The question wording was as follows: "We are interested in knowing whether you plan to negotiate the following aspects of the job offers that you may receive: What is the percent chance that you will negotiate (the base pay/signing

the treated group significantly increased their stated likelihood of negotiating a monetary aspect of their first job after viewing the information. By contrast, students in the control group revised their likelihood down a statistically insignificant amount when re-asked about their intentions to negotiate. Table 2 presents the magnitudes of the treatment effects that are visually implied in Figure 4 by providing coefficient estimates from a regression of within-individual changes in stated intentions (post-info intentions minus pre-info intentions) on a female dummy, a treatment dummy, and the interaction of female and treatment, as well as a number of relevant control variables.¹⁹ Column (1) displays results for the intention to negotiate some monetary aspect of the job offer; the estimates indicate that the treatment increased men’s intention to negotiate by an average of 11 percentage points and women’s intention by 9 percentage points, although this difference is not statistically significant. Column (2) shows that the treatment effect is robust to including controls. Treated men also increased their expected negotiation efficacy (measured in percentage points) relative to control men; for women, the treatment effect, although positive, is not distinguishable from zero (Column (3)). Finally Columns (5) and (6) display results for revisions in the likelihood of negotiating a non-monetary aspect of the job. We find a similar effect for both genders of around 7 percentage points. This effect is statistically significant.

Individuals who revise their likelihood of negotiating the monetary aspect of the offer also tend to revise their beliefs about negotiating non-monetary aspects: the correlation in belief revisions across the two domains is 0.294 (0.385) for males (females). Together, the estimates in Table 2 indicate that the information treatment significantly increased both males’ and females’ intention to negotiate for both monetary and non-monetary job benefits. However, Column (3) suggests that the mechanisms underpinning these increased intentions may differ by gender, as only treated men revised their expected negotiation efficacy upwards after receiving the information.

3.2 Impact on Actual Negotiation Behavior

Did changes in intentions translate to a higher probability of negotiating? We answer this question in Table 3, where we present the estimated treatment effects for reported negotiation of their job offer for their first job after graduation using a similar regression strategy as Table 2, except that instead of using within-individual revisions as the dependent variable, we use reported negotiation behavior as the outcome and include the most relevant pre-information intention as a control.²⁰ Column (1) investigates if the information changes respondents’ decision to negotiate any monetary aspect of their job. We find that the information treatment has a large average effect on male’s likelihood of negotiating for more compensation (an increase of nearly 12 percentage points), but no average effect on females (the sum of the coefficient on Treated and Treated \times Female is close to zero). The estimate on the female term – which captures the negotiation behavior of

bonus/bonus pay)?”. We use the highest percent chance reported.

¹⁹These control variables are concentration, GPA, cohort, age, race, a dummy for US born, and dummies for parental education.

²⁰We obtain qualitatively similar results when the sample is restricted to US-born individuals. These results are available on request.

females in the control group (relative to males in the control) – is positive but imprecise. This is a surprising result. Given historical trends, one would have expected this estimate to be significantly negative. We discuss this result in more detail in Section 4.3.

Columns (3) and (4) present models where the outcome is negotiation efficacy (in percent) conditional on negotiating. We find that treated men’s efficacy is lower than control men’s by 6 to 7 percentage points. We interpret this result as suggesting that the most effective negotiators (or those who had most to gain from negotiating) were already doing so; therefore, the men who were nudged to negotiate by the information were those with low returns to negotiation. We observe a different pattern for women: (1) the negotiation efficacy of control women is significantly lower than that of control men, which is not something we saw for earlier cohorts (see Table 1), and (2) the negotiation efficacy of the treated students who negotiated is no different (or even slightly higher) than that for the control students. Our analysis in the next section will provide some clues to explain these divergent results by gender.

Appendix Figure A1 depicts the extensive margin of negotiation (left-most bar) and the distribution of negotiation efficacy conditional on negotiating for the treatment and control group separately by gender (bars on the right). We observe that the distribution of negotiation efficacy for treated men is skewed to the left relative to control men, whereas the distributions for treated and control women are quite similar.²¹ In fact, more than half of the men in the treatment group who negotiate failed to increase their offer compared to just one in five in the control group. By contrast, if anything, women in the treatment group were more likely to be successful in their negotiations relative to their control peers.

Finally, Columns (5) and (6) in Table 3 explore treatment effects on the likelihood of negotiating non-monetary aspects of the job offer. Treatment effects on actual negotiation behavior for men closely track the treatment effects for intentions. Treated women appear to have lost half of the treatment effect on negotiating non-monetary aspects observed in their reported intentions; the 3.7 percentage point estimate, although large relative to the control mean, is statistically insignificant (p -value = 0.272).

In summary, we find that: (i) information on peer negotiation behavior increases average intentions to negotiate for both men and women, (ii) treated men, on average, negotiated monetary aspects of their job offer at much higher rates than their untreated peers while women did not, and (iii) the treatment effect on actual negotiation efficacy goes in opposite directions for men and women.

4 Underlying Mechanisms

The preceding section provides evidence on the average treatment effects on the likelihood of negotiation: large and positive for men, and close to zero for women. In this section, we explore the mechanisms behind these results.

²¹The K-S test p -values for equality of the distributions between treatment and control groups are 0.156 for men and 0.989 for women.

4.1 Belief-updating vs. Other Channels

One potential mechanism is that the information debiased misinformed beliefs about dimensions that factor into a student’s decision to negotiate. In this case, only messaging with high quality and accurate information that shift priors would generate similar results. An alternative mechanism is salience. The informational message may have made negotiation more salient in general, and thus increased negotiation rates. In this case, any message that discusses negotiation could potentially generate similar results. Finally, our treatment might have framed information in a way that was new to the students, such as the calculation of the implied lifetime returns to negotiating one’s first salary, which could have encouraged negotiation behavior.

It is difficult for us to separately identify the pure salience and information-framing channels. However, we can test the importance of these two mechanisms (which we lump under salience) vs. the information-updating channel by examining how the treatment effect varies as a function of the extent of misperceptions of the information provided (i.e., gender ratio in negotiation or negotiation efficacy). Specifically, we estimate the following regression model:

$$Y_i = \beta_0 + \beta_1 Treat_i + \beta_2 PercGap_i^Z + \beta_3 Treat_i \times PercGap_i^Z + \Gamma X_i + \epsilon_i,$$

where $PercGap_i^Z = \hat{Z}_i - Z_{truth}$, \hat{Z}_i is student i ’s pre-treatment belief of the value of the statistic Z (either the gender ratio in negotiation or negotiation efficacy), and Z_{truth} is the actual value of the statistic calculated with the alumni data. If saliency is an important channel through which information impacted behavior, it must be the case that $\beta_1 \neq 0$ (there is a treatment effect even for individuals with unbiased beliefs at baseline). If debiasing of perceptions is an important channel, treatment effects should vary by the size and direction of the perception gap ($\beta_3 \neq 0$), and if there is full updating to the information provided, there should be little correlation between the outcomes and pre-information perceptions for the treated group (because the information would have altered those perceptions), i.e., $\beta_2 + \beta_3 = 0$.

Appendix Figure A2 shows a binned scatterplot to help visualize this model and Table 4 shows the estimated coefficients. The first two columns of Table 4 display estimates for men. Column (1) focuses on pre-treatment beliefs about the gender ratio in negotiation. We find that the treatment coefficient itself is highly significant, but the coefficient on perceptions about the gender negotiation ratio and the coefficient of its interaction with the treatment dummy are both small and not statistically significant. In the figure, these results are represented by the flat relationship between perception gaps and own negotiation behavior, both for the treatment and control groups, and by a parallel shift upwards for the treatment group. These results are consistent with salience being the primary mechanisms for men. That we find little direct effects of perception gaps suggest that these statistics are not relevant for men’s decision-making; that is, changes in men’s negotiation behavior are not due to information-based updating of their prior beliefs about the gender gap in negotiation or negotiation efficacy.

We find opposite results for females. First, as shown in Column (5), the direct effect of the

treatment is small and not statistically significant, implying that there is no treatment effect for those with accurate beliefs about the gender ratio in negotiation.²² This suggests that, unlike for men, the salience or novelty of the information did not impact women’s negotiation behavior, at least on average. Second, perception gaps of the gender ratio strongly correlate with control females’ negotiation decisions. Control women with positive perception gaps (i.e., those who believe women negotiate *less* relative to men that they actually do) tend to be less likely to negotiate themselves. Third, the coefficient on the interaction between the perception gap regarding the gender negotiation ratio and the treatment dummy is positive and highly statistically significant. Women with large and positive perception gaps, who learned from the experiment that women negotiate relatively more than they initially thought, experience a positive treatment effect. On the other hand, women with negative perception gaps, who updated their beliefs in the opposite direction (i.e., they learned that women negotiated relatively less than they thought) experience negative treatment effects. Overall, these coefficients (and the fact that in Appendix Figure A2 there is a flat relationship for treated women, but a negative one for the control group) suggest that the main mechanism through which our information experiment affected women’s negotiation decisions is belief-updating. Note that for women, the mean belief about the gender ratio in negotiation rates is close to the truth (see Figure 2) explaining why the *average* treatment effect for women is close to zero in Table 3.

Somewhat surprisingly, for both genders, beliefs about negotiation efficacy do not seem to matter for one’s likelihood of negotiating. The direct effects of the perception gap regarding negotiation efficacy as well as the interaction with the treatment dummy are small and statistically insignificant (see Columns (2) and (6)).

4.2 Unpacking the Role of Pre-Treatment Beliefs

The previous section shows that the treatment effect for women varies systematically with pre-treatment beliefs over the gender ratio in negotiation rates, consistent with an information-based updating channel. To further understand what is the relevant information that affects women’s negotiation behavior, we examine treatment effects by women’s pre-treatment beliefs about peer negotiation rates of females and males separately. We begin by dividing women into terciles based on their pre-treatment beliefs of the negotiation rates of female and male peers, and estimate the treatment effects separately for each tercile.

Panels (a) and (b) of Figure 5 present the results from this exercise for negotiating some monetary aspect of the job offer and negotiation efficacy (conditional on negotiating), respectively, for female respondents. As observed in Panel (a), we find a large (15 pp.), positive, and statistically significant treatment effect on engaging in monetary negotiation when we compare treated women in the bottom tercile of beliefs about the negotiation rate of same-gender peers to control women

²²Equivalently, we observe in Appendix Figure A2 that the treatment and control lines intersect when the perception gap is zero.

with beliefs in the same tercile.²³ These women were much more likely to underestimate the relative negotiation rates of female peers, and learning about the gender negotiation ratio led them to update their beliefs upwards, and negotiate at higher rates. Consistent with what we might expect if belief-updating is the main mechanism, the treatment effect is close to zero for women with pre-treatment beliefs about female negotiation rates in the middle of the distribution, and negative and sizeable (-10 pp), though not statistically significant, for those who believed that women negotiate at high rates (i.e., the top tercile of the distribution). For the latter group, information about the gender negotiation ratio is likely to have affected the negotiation behavior of treated women by leading them to revise their beliefs about female negotiation rates downwards.

On the other hand, as observed in Panel (c), we find that treatment effects do not vary by women’s beliefs about men’s negotiation rates, suggesting that the main mechanism behind the impact of the provision of information about gender negotiation ratio on some women’s negotiation decisions is belief-updating about female negotiation rates. Similar to what we found previously, pre-treatment beliefs about female (or male) negotiation rates matter little for negotiation efficacy conditional on negotiating (see Panels (b) and (d)). Together, Panels (a) and (b) of Figure 5 indicate that some women with low beliefs about other women’s negotiation rates were nudged to negotiate due to the information and these complier women had comparable negotiation efficacy as their peers with similar beliefs.

Appendix Figure A3 presents the same exercise for men. We do not find similar treatment effect patterns for men, providing additional evidence that belief-updating is not the main driver behind the treatment effects observed for men in Column (1) of Table 4. If anything, we find that the treatment effect is largest and positive for men with very high beliefs about peer negotiation rates. Moreover, conditional on negotiating, treated men typically have lower negotiation efficacy relative to control men regardless of pre-treatment beliefs about male negotiation rates.

Columns (3)-(4) and (7)-(8) of Table 4 present the regression version of the figures by interacting the treatment dummy with pre-treatment beliefs about peer negotiation rates of same- and opposite-gender peers (entered linearly). Consistent with the graphical evidence, women’s likelihood of negotiation (in the treatment group relative to the control group) is affected by pre-treatment beliefs about the negotiation rates of female peers, but not that of male peers, whereas men’s negotiation behavior are largely unaffected by beliefs about peer negotiation behavior.

In sum, the experiment appears to impact men and women’s outcomes through very different mechanisms. The positive treatment effects on negotiation for men are not explained by information updating, and are likely driven by salience or framing of the information contained within the treatment. On the other hand, treatment effects for women depend on their initial beliefs, suggesting an important role for belief-updating. Importantly, we find that female compliers did not have lower negotiation efficacy than comparable controls. These findings are consistent with information frictions acting as a barrier in preventing some women from negotiating even when the returns to doing so are quite high. In such settings, correcting misperceptions about the prevalence

²³See Appendix Table A5 for the table version of these graphs, and for specifications that include controls.

of negotiation among same-gender peers can help to encourage more women to engage in monetary negotiations that yield financial benefits. In our setting, although our treatment induced men to negotiate more, they do not appear to benefit to the same extent as women, possibly consistent with their negotiation behavior being less affected by (information) frictions.

4.3 Treatment Spillovers

Given the importance of salience in explaining the large and positive average treatment effect for men, it is quite surprising that we find little evidence that this mechanism matters, on average, for women. There is no apparent reason why this channel should be gender-specific. To better understand what could be driving the *average* null effects for women, Figure 6 plots the trends in negotiation decisions over time, using data from both the experimental cohorts and the graduated cohorts surveyed earlier (who graduated between 2013 and 2017). The experimental cohorts are pooled together and the average negotiation rates by treatment status are displayed. We see that control men in the experimental cohorts negotiated at a rate consistent with the prevailing trend in negotiation behavior among Questrom males. By contrast, control females in the experimental cohort negotiated at much higher rates than what previous years would have suggested. In fact, if the treatment effect were calculated using the most recent non-experimental cohort as a baseline (instead of control individuals in the experimental cohorts), males and females would have very similar overall treatment effects.

The fact that the null treatment effects for women are driven by a break in trend for both the control and treatment females suggests at least two possible explanations. The first possibility is that external factors may have had a gender-specific impact on students' negotiation decisions and additionally neutralized the effect of the treatment. For example, the #MeToo movement started in 2017, about the same time as our intervention, and may have led to more gender awareness among female students. Another possibility is that negotiation or career workshops tailored to female students could have also affected the negotiation behavior of both control and treated women. Specifically, in the Fall of 2017, Questrom added a required course on career navigation that covered job negotiations. Examining the course material, we found that the course did indeed provide some information on gender differences in negotiation as well as gender-specific information on the earnings impacts of negotiation as shown in Appendix Figure A4. Such information could have induced women in the post-2017 cohorts to negotiate more as observed in Figure 6.²⁴ Note that the fact that we find treatment effects of our information treatment for women as a function of their pre-treatment beliefs as described in Section 4 suggests that the information that we provided was valuable to some female students even after the career navigation course, perhaps because it was more relevant (e.g., actual magnitudes for the gender gap in negotiation behavior calculated using data from Questrom alumni) and/or provided at a more appropriate time than the information

²⁴This is also consistent with the observed decline in negotiation efficacy of control women post-2017 as shown in Appendix Figure A5. The introduction of the negotiation course may have prompted more women to negotiate and these women had low returns to negotiation (either because they were less effective negotiators or had less to gain from negotiating).

conveyed in the course material.

A second possible explanation for the high negotiation rate among control women is treatment spillovers, *i.e.*, the information treatment might have spread amongst students in such a way that women were more likely to receive or act on the information. To evaluate the possibility of gender-specific spillovers, we conduct two exercises. First, we compare the negotiation rates of control students who were quasi-randomly exposed to a large share of peers who were treated to those who were exposed to a smaller share of treated peers. As a proxy for peer group, we use students of the same concentration, cohort, and gender in our survey sample.²⁵ If the information was transferred from treated to control students, we would expect this transfer to be more likely among students where a larger share of their peer group was treated. Table 5 display coefficients from a regression of individual negotiation behavior on the share of one’s peer group that was treated, where the sample is restricted to the control group. For women, we find a strong positive correlation between the likelihood of negotiating and the share of one’s peer group that was treated: the estimate in column (1) indicates that a 10 percentage point increase in the share of one’s peer group that was treated increases the negotiation rate of control females by 5 percentage points. For males, the estimate in column (2) is negative, economically smaller, and not precise.

One potential concern is that these associations could be picking up unobserved differences across gender-concentration groups that are correlated with the share of peers treated and the propensity to negotiate. To address this issue, Appendix Figure A6 plots the distribution of coefficients from placebo regressions where alumni respondents were randomly assigned a share of treated peers (in the corresponding gender-concentration group) observed for one of the major/survey year observations in the survey cohorts. For women, only 0.1% of the estimates from this procedure yielded a coefficient larger than the value reported in Column (1) of Table 5, whereas for men, more than 25% of replications have an estimate with a larger magnitude. These results suggest that unobserved differences across peer groups are unlikely to be driving the spillover effects estimates in column 1 of Table 5. Overall, these findings are consistent with gender-specific spillovers explaining the homogeneous treatment effects by gender on intentions (which were elicited before students had a chance to share the information) and the heterogeneous treatment effects on actions.

Second, we provide direct evidence on the existence of treatment spillovers and provide insights on the mechanisms behind them by conducting an additional retrospective survey of study participants.²⁶ The survey includes specific questions about the sharing of information from the intervention, as well as more general questions about approaches to decision-making and information-sharing about the labor market. Table 6 summarizes the main findings of this survey. The top panel includes both treated and control individuals and shows descriptive statistics for the gen-

²⁵The results are qualitatively similar if peer groups are defined by concentration and cohort instead.

²⁶We conducted the online survey on February of 2021. To incentivize students’ participation, we offered a \$20 Amazon gift card. The second column of Appendix Table A4 explores selection into answering this survey. Reassuringly, individuals do not seem to select into the survey based on treatment status or earnings expectations. The intended likelihood of negotiating is a significant predictor of taking the survey, but the economic magnitude is small. A 10 percentage point increase in the likelihood of negotiating is associated with a 1 percentage point higher likelihood of taking the survey.

eral questions. We find that women are more likely to “make important decisions by asking for advice/observe others’ decisions”. Females were also more likely to respond that peers influenced whether or not they negotiated offers “a lot” and were more likely to have “discussed whether or not to negotiate offers with peers often,” although both these difference are small and not statistically significant. Together, this evidence suggests that females may have been more likely to act on second-hand information that they received through their network and discuss negotiation strategies in general, and thus more likely to have received the treatment information indirectly relative to men. Further, we find strong evidence that both male and female respondents were more likely to “completely agree” that it is important to encourage their female peers to advocate for themselves on the job as compared to encouraging their male peers, with women nearly twice as likely to agree that it is important to encourage their female peers (rather than their male peers) to advocate for themselves on the job ($p < 0.01$). This provides a further reason as to why spillovers may have been gender-specific, as treated individuals may have felt more compelled to share the treatment with their female peers either directly or indirectly through the change in their own intentions to negotiate due to the intervention.

Because we know the treatment status of respondents, we can go one step further and directly ask treated individuals whether they shared the treatment information, and those in the control group whether they received information contained in the treatment. The middle panel of Table 6 shows that the average treated individual shared the information with 1.05 female peers and 0.86 male peers (this difference is significant at 5%, $p = 0.039$). The bottom panel shows that conditional on obtaining information from the treatment, control females are nearly 50% (5pp) more likely to indicate that they had learned about the information (or similar information) from a peer. We see gender gaps of similar magnitudes in the share of individuals who learned about the information (or similar information) from peers, the classroom, or social media. While none of these differences are statistically significant given the small sample sizes, these results are consistent with the mechanisms suggested in the top panel and the responses of treated respondents in the middle panel. Further, we view these responses as a lower bound of the magnitude of actual spillovers, as students may not fully recall casual conversations with treated peers so long after the treatment took place and some students may have shared their increased intention to negotiate without conveying the information directly, thus impacting the controls’ negotiation behavior without directly sharing the treatment information. Finally, the last row of Table 6 indicates that control females who indicate that they were given the information were 5 pp. more likely to have acted on it relative to control men, albeit this difference is not statistically significant at conventional levels.

In sum, the survey offers several pieces of evidence that are consistent with gender-specific spillovers: (i) in general, women are more likely to discuss negotiation with friends and consult friends for guidance when making decisions, (ii) respondents feel more strongly about encouraging female peers to advocate for themselves, (iii) treated individuals report sharing the information with a non-trivial number of peers and with more women than men, and (iv) control women report being more likely to have received the treatment information, and negotiating as a result of the

information than men. That networks seem to play different roles for men and women is broadly in line with evidence that network structure differs for women and may matter for gendered outcomes (Yang et al., 2019; Lindenlaub and Prummer, 2021).

As a final step to quantify the role of spillovers in our experiment, we merge the spillover survey with our main dataset and evaluate the extent to which spillovers may have inflated the negotiation rate of women in the control group. There are two caveats about this exercise. First, as mentioned above, we almost certainly underestimate the share of students who received some type of information spillovers as students may not recall or even realize they were receiving information inspired by the treatment, and so any attempt to remove “contaminated” controls will be partial at best. Additionally, sample attrition in the spillovers survey implies that some control respondents do not have a known “spillover status.”

With these limitations in mind, Figure 7 visually compares the negotiation rates for treated individuals to the negotiation rates of several different control groups. If we use non-experimental cohorts as the control group, we find a statistically significant treatment effect of more than 10 percentage points for both sexes. As documented before, if we compare treatment individuals to control individuals within the same cohorts, the treatment effect for women disappears. However, if we remove those who responded to the follow-up survey and confirmed that they had received the information from the control group, the negotiation rate for control women declines by 5 percentage points.²⁷ We find no change in the negotiation rates for men. Finally, we provide a non-response bound to address the fact that we do not know the spillover status for all respondents by dropping observations with unknown spillover status at the same gender-specific rate observed in the spillovers survey. When deciding which observations to drop, respondents who reported negotiating their job offers were selected before non-negotiators so that the resulting sample represents an optimistic bound on the potential treatment effect on negotiation. Using this approach, we find that the treatment effect for women becomes eight percentage points, a sizeable magnitude, although it is not statistically significant. Appendix Table A6 presents the table version of these estimates and includes additional outcomes. Appendix Tables A5 and A7 also show that the results on the treatment effects for women and men as a function of initial perception gaps are robust to accounting for possible spillovers.

5 Value of the Information and Effects on Other Outcomes

We now consider potential impacts of the treatment on the perceived value of the information, job satisfaction, job characteristics besides pay, and the gender pay gap to verify that the treatment did not have obvious negative effects on other dimensions. One concern, for example, may be that nudging students to negotiate may push them into jobs that otherwise are not a good fit for them, or that labor market outcomes may be worse (Exley et al., 2020).

²⁷The p -value of the difference with the treatment group is 0.255.

Value of Information In the baseline survey, right after receiving the information treatment, we asked treated respondents how useful they found the information on a five-point scale. Appendix Figure A7 displays the distribution of responses separately by gender. We see that most men and women reported that they found the information useful, with more than 70% of respondents rating the information a 4 or higher on a five-point scale of usefulness. We also find very little gender difference in how useful individuals found the information to be.

The follow-up survey also asked respondents: *“How important do you think negotiations are for career advancement?”* Responses were recorded on a 5-point Likert scale. We find that the treatment increased the likelihood of respondents reporting negotiations to be important (i.e., answering 4 or 5 on the Likert scale): 69% of treated men reported that to be the case, versus 58% of control men ($p = 0.076$). The corresponding statistics for women are 65% versus 61% ($p = 0.439$). Thus, it seems that the treatment also increases the importance that respondents attach to negotiations as part of the job search process.

Impact on (Gaps in) Earnings We previously showed that our intervention likely impacted the extensive and intensive margins of negotiation outcomes differentially by gender. Therefore, from studying these outcomes alone, it is difficult to determine the overall impact of our treatment on the gender earnings gap. Additionally, the treatment might have affected other aspects of job search behavior unrelated to negotiation. Columns (1) and (2) of Table 7 presents the same regression specification as Table 3, but where the outcome now is actual earnings. The effect of the treatment on men is positive (an increase of nearly \$3,500 in earnings), but not precisely estimated. The direct effect of the treatment and the interaction term between the treatment and female dummy suggests that, on average, the earnings of female treated students is not affected much by the intervention. Overall, the estimates suggests a small but not precisely estimated widening of the gender pay gap.

For a visual representation, Appendix Figure A8 plots the trend in the earnings gap (the difference between the average female earnings and the average male earnings). The gap is slightly larger for the treated 2018-2020 cohorts compared with the controls and the uncontaminated controls. However, as our evidence suggests that significant sample contamination may have occurred among the 2018-2020 cohorts, a potentially more relevant reference group may be the previous cohorts. Compared to the most recent untreated cohort (2017 graduates), the gender earnings gap declined by approximately \$2,700. Although the difference between these gaps is not precisely estimated, it is unlikely that the treatment widened the gender gap ($p = 0.248$). Given the lower negotiation efficacy for men induced to negotiate by the treatment, the lack of impact of the treatment on the overall gender earnings gap may seem surprising. Nevertheless, as depicted in Appendix Figure A9, this appears to be due to the fact that the earnings gains from negotiation are happening in different parts of the offer distribution by gender.

Overall, we conclude that our information treatment had a modestly positive (though imprecise) impact on average earnings, and little impact on the average gender earnings gap.

Impact on Other Job Characteristics and Outcomes The remaining columns of Table 7 examine the extent to which the treatment affected students’ satisfaction with their current job, the number of benefits offered, and the availability of flexible work.²⁸ Columns (3) and (4) show that the treatment had no economically or statistically significant impact on job satisfaction. It is certainly not the case that treated respondents report lower levels of satisfaction.

Columns (5) and (6) of Table 7 indicate that increases in the likelihood of negotiating a monetary aspect of the job were not accompanied by lower levels of other job benefits; if anything, the average number of job benefits increases by about 0.3 (on a control mean of 3) for females. Likewise, the last two columns show that the treatment did not decrease the likelihood that the job offered a flexible work option, an amenity that has been shown to be valued by workers, especially females (Wiswall and Zafar, 2017; Maestas et al., 2023).

Table 8 presents further evidence that the treatment generally positively impacted both men and women.²⁹ Column (1) shows that the treatment shifted women’s likelihood of remaining at their current employer by 6 pp. ($p = 0.057$). We see a positive impact for men as well, but the estimate is imprecise. Increased expected employer tenure may serve as indirect evidence of improved satisfaction with current employers.

Our survey also elicited longer-term earnings expectations. While we do not find an impact of the treatment on average expected earnings at age 30 for either men or women, columns (3) and (4) of Table 8 show that the reported likelihood of earning more than \$90,000 per year at age 30 increases by about 9pp for men and 5pp for women; the treatment impacts are significant at the 5% level for both men and women. This is an unambiguously positive outcome.

Columns (5) to (8) of the table present different measures of respondents’ subjective likelihood of negotiating with their employer in the next year. We see that treated men are more likely to report intentions to negotiate both monetary and non-monetary aspects of their jobs. However, we see no evidence that the treatment increased women’s intentions to negotiate in the future, and some weak evidence that treated women are less likely to expect to negotiate for money in the next year ($p = 0.097$). Thus, it seems that the treatment has very different impacts on the long-term negotiation intentions of males and females.

Finally, the dependent variable in the last two columns of Table 8 is the change in the reservation wage. Study participants reported their reservation wages both in the post-graduation survey and the baseline survey.³⁰ We see that the treatment has a large (economical and statistical) increase

²⁸The survey question for job satisfaction was: “Taking everything into consideration, how satisfied would you say you are, overall, in your current job?”. Respondents who answered a 4 or 5 on a 5-point Likert scale are coded as being satisfied with the job. The survey question for work flexibility was: “Which of the following benefits does this job have? Flexible work options such as flex time (flexible or alternative work schedule, and flex place (telecommuting - working from home))”.

²⁹Notice that the variables in Table 8 require a respondent to have replied to the post-graduation survey. Further, some of the questions were asked only conditional or only to later cohorts. For these reasons, the sample sizes in Table 8 are smaller than in other analyses. See the table notes for more details.

³⁰The question wording was as follows: “What would have been the lowest annual total compensation you would have accepted for your first job after graduating from Questrom? Include base pay, signing bonus, and bonus pay.”. Since this analysis is restricted to those respondents for whom we have both the baseline and post-graduation data, the sample size is lower here.

in females’ reservation wages, and no impact on men’s reservation wages. In fact, the gender gap in reservation wages is totally closed as a result of the treatment.

Overall, there is no evidence that the treatment negatively impacts other job characteristics. In fact, evidence points towards positive (though imprecise) impacts particularly for women’s job satisfaction as measured by their subjective likelihood of remaining at their current job long-term. In addition, it seems that both men and women’s career expectations were generally positively impacted by the information treatment.

Impact on Gender Norms Finally, one may wonder whether the treatment has an impact on gender norms. For example, informing respondents that males are 1.6 times as likely as females to negotiate may serve as a prescriptive norm that females are not supposed to negotiate. At the end of the follow-up survey, we asked respondents for the number of statements (regarding female stereotypes about negotiating, and labor supply) they agreed with.³¹ The first two columns of Appendix Table A8 show that the treatment has no meaningful impact on the number of statements that male or female respondents agree with.

The survey also asked respondents: *“Out of 100 people, how many do you think would agree with this statement: Women who negotiate for a higher salary or job benefits are too aggressive?”* The last two columns of Appendix Table A8 show that the treatment did not have a meaningful impact on the response to this question for either gender.

Overall, these results suggest that the treatment did not have negative effects on students’ earnings, satisfaction levels, or job benefits. Gender norms also do not seem to have been impacted. Moreover, the students found the information useful for their job search.

6 Conclusion

It is well established that women negotiate their salary at lower rates than men. We implement a field experiment that provides information about the gender ratio in negotiation rates and negotiation efficacy to undergraduate business students to test whether correcting biased beliefs about these variables changes negotiation behavior. The impact of the experiment differed by gender. Whereas the treatment had a large average treatment effect on men’s probability of negotiating their starting salary, the returns to negotiation of those who were nudged by the experiment to negotiate was very small or null. We also find that men primarily respond to the saliency of the information. We interpret these results as suggesting that men who had a lot to gain from negotiating were already doing so, and that the saliency/framing of the information encouraged some men to try to negotiate, even if the ex post returns were low.

³¹The statements were: (1) Women who negotiate for a higher salary or job benefits are too aggressive. (2) A man’s job is to earn money while a woman’s job is to look after the home and family. (3) It is more important for a wife to help her husband’s career than to have one herself. The latter two statements come from the General Social Survey.

Our treatment affected women’s negotiation behavior in a very different way. We do not find an average treatment effect on the likelihood of negotiating for women; however, we find a large, positive, and statistically significant effect for a particular group of women: those who believe that female (relative) negotiation rates are much lower than they actually are. Furthermore, for this group of women, the returns to negotiation were not lower and, if anything, higher than that of comparable control women. This suggests that biased beliefs, in this case about peer negotiation rates, is an important source of low negotiation rates for some group of women, and that there is considerable room for low-cost information interventions to nudge women who have potentially large financial returns from negotiating, to negotiate.

We also found evidence of information spillovers, suggestive of the information being valued by students. The spillovers have a gendered dimension with women appearing both more likely to have received the information indirectly and more likely to have acted upon the information if they received it from their peers. Since we do not have data on students’ networks, the exact reasons why this happens is not clear. More work is needed to understand the role of networks in gender differences in job search behaviors and outcomes.

Finally, our results clearly show the power of low-cost information interventions to shift economic outcomes. While a welfare analysis of the intervention is beyond the scope of this paper, our data does not show respondents being negatively impacted in any obvious way by the intervention.

It is worth noting that, relative to the literature ([Recalde and Vesterlund, 2023](#)), our treatment effects on intended and actual negotiation behavior are quite large. We suspect that this is, in part, due to the fact that the information we use in our intervention is derived from the same population of interest and, hence, highly relevant. In addition, even though it is widely known that men are more likely to negotiate than women, quantitative estimates are not readily available and typically the gains from negotiations are also not something that students have a good prior about (as we see in Panel B of Figure 2). Furthermore, our sense is that the timing of our intervention – just before when most students actively start looking for jobs – also makes it highly salient. However, this is all merely speculative and needs to be systematically investigated in future work.

The dynamic implications of our intervention are, however, not clear. While short-term outcomes are positively impacted and the gender earnings gap does not increase, the longer-term impacts might differ. Could it be that the information intervention differentially changes how males and females view negotiations as a long-term tactic? We have some survey evidence that this may indeed be the case. Treated women, relative to their control counterparts, report a lower likelihood that they would negotiate their salary in the next year. However, the patterns are reversed for men. Thus, it seems that the treatment permanently shifted the outlook of how men view negotiations going forward. Future work investigating the longer-term impacts of one-time information interventions is crucial to fully characterize the welfare implications of such policies.

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Figure 1: Information Treatment

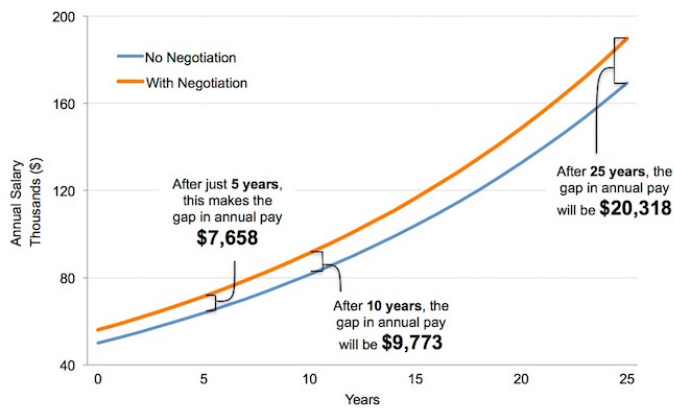
Survey of Questrom Students

Information

In a survey of recent Questrom graduates (who graduated with an undergraduate degree in Business), we found that males were **1.6 times more** likely than females to negotiate their starting job offer. That is, for every 10 females who negotiated their offer, 16 males negotiated their offer.

In addition, as a result of negotiating, on average, the total salary (base pay, signing bonus, and bonus pay) of recent Questrom graduates **increased by about 12%**.

The figure below illustrates the earnings gain from negotiation. Consider two individuals, A and B, who both start jobs paying \$50,000 with a guaranteed 5% raise every year. If B negotiates a 12% increase in starting salary (that is, \$6,000 more), B's annual salary would be \$7,658 more after just 5 years and \$20,318 more after 25 years.



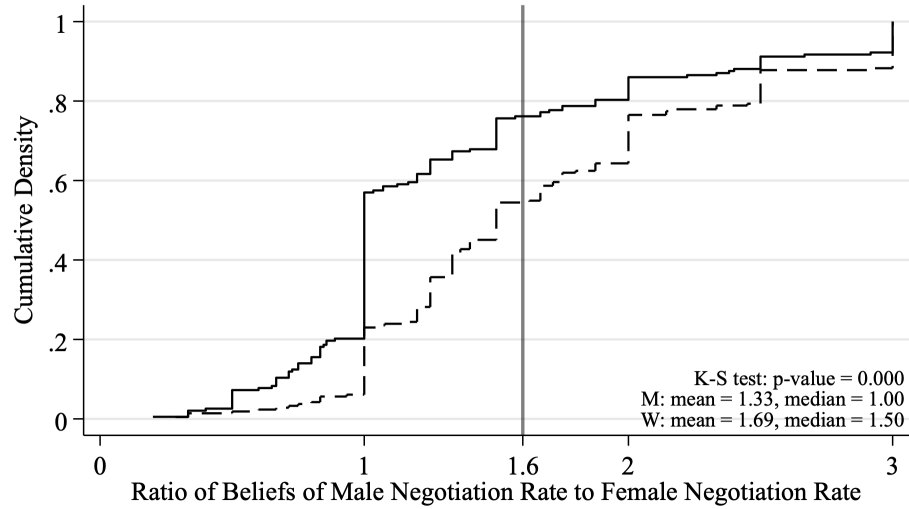
Over one's career, negotiating could result in large cumulative gains.

Relative to Person A:

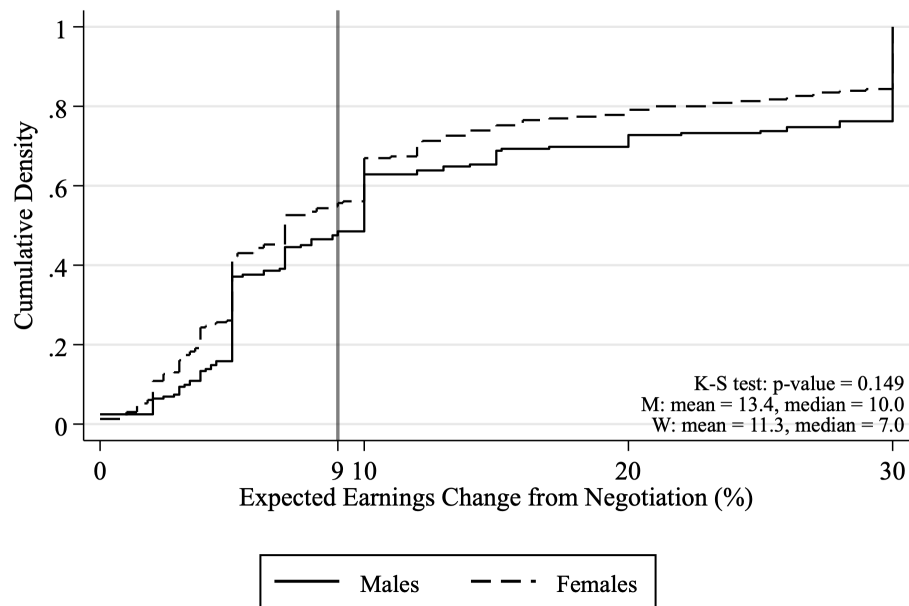
- After 5 years, Person B will have cumulative gains of **\$40,811** in earnings.
- After 10 years, Person B will have cumulative gains of **\$85,241** in earnings.
- After 25 years, Person B will have cumulative gains of **\$306,681** in earnings.

Figure 2: Previous Cohorts' Beliefs About Peer Outcomes

Panel A: Gender Ratio Negotiation Rate

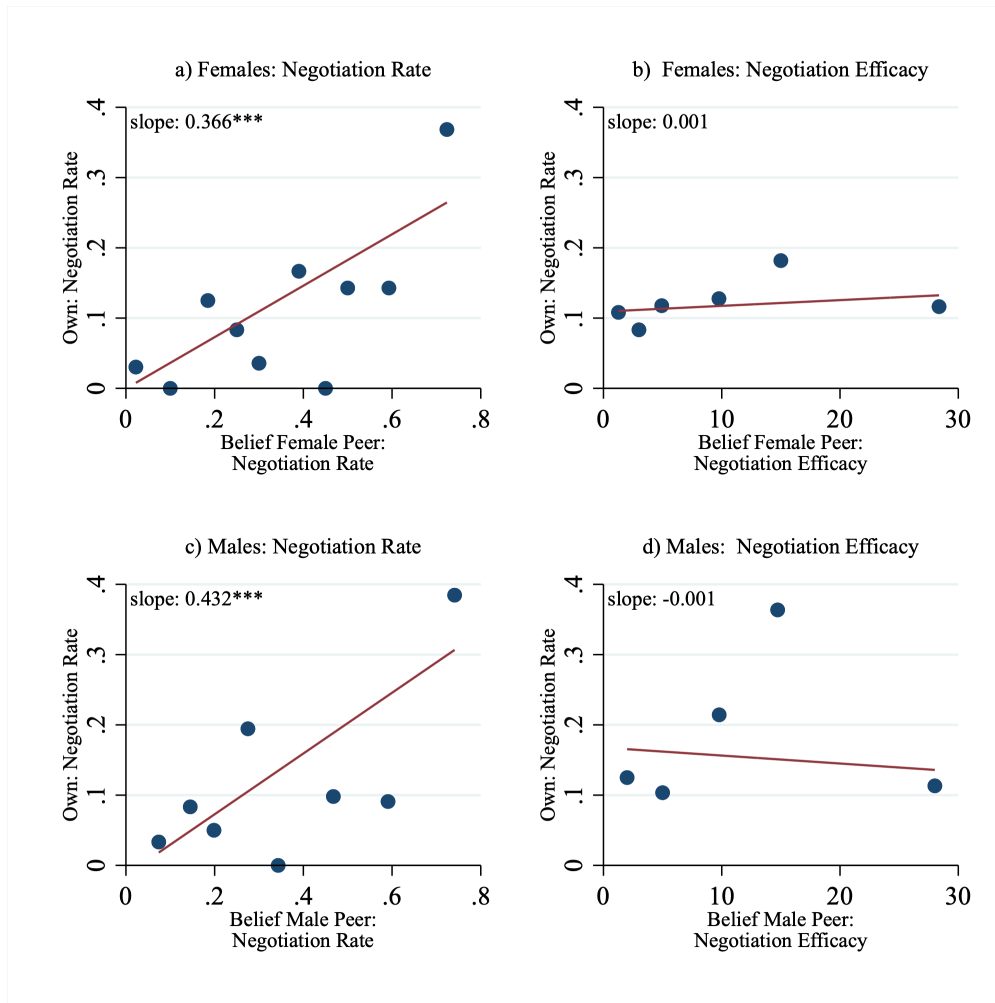


Panel B: Negotiation Efficacy | Negotiating



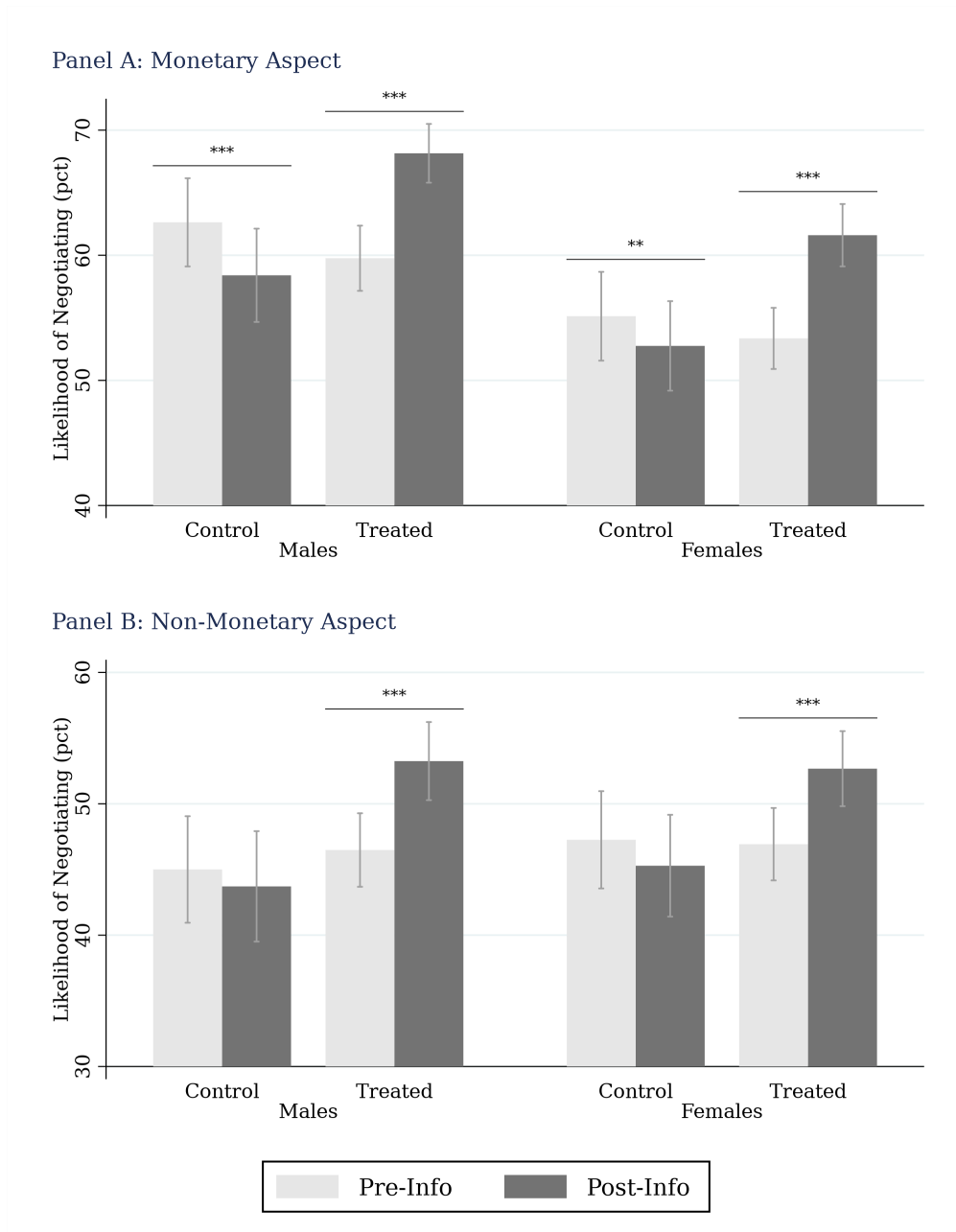
Notes: This figure shows distributions of pre-treatment cohorts' (2013-2017) beliefs about peer negotiation behavior. Panel A shows beliefs about the gender ratio in negotiation. Panel B shows beliefs about negotiation efficacy (conditional on negotiation). Beliefs about efficacy elicited separately by gender. Unconditional efficacy beliefs constructed as the weighted average of gender specific beliefs.

Figure 3: Correlation Between Beliefs about Peers and Own Behaviour



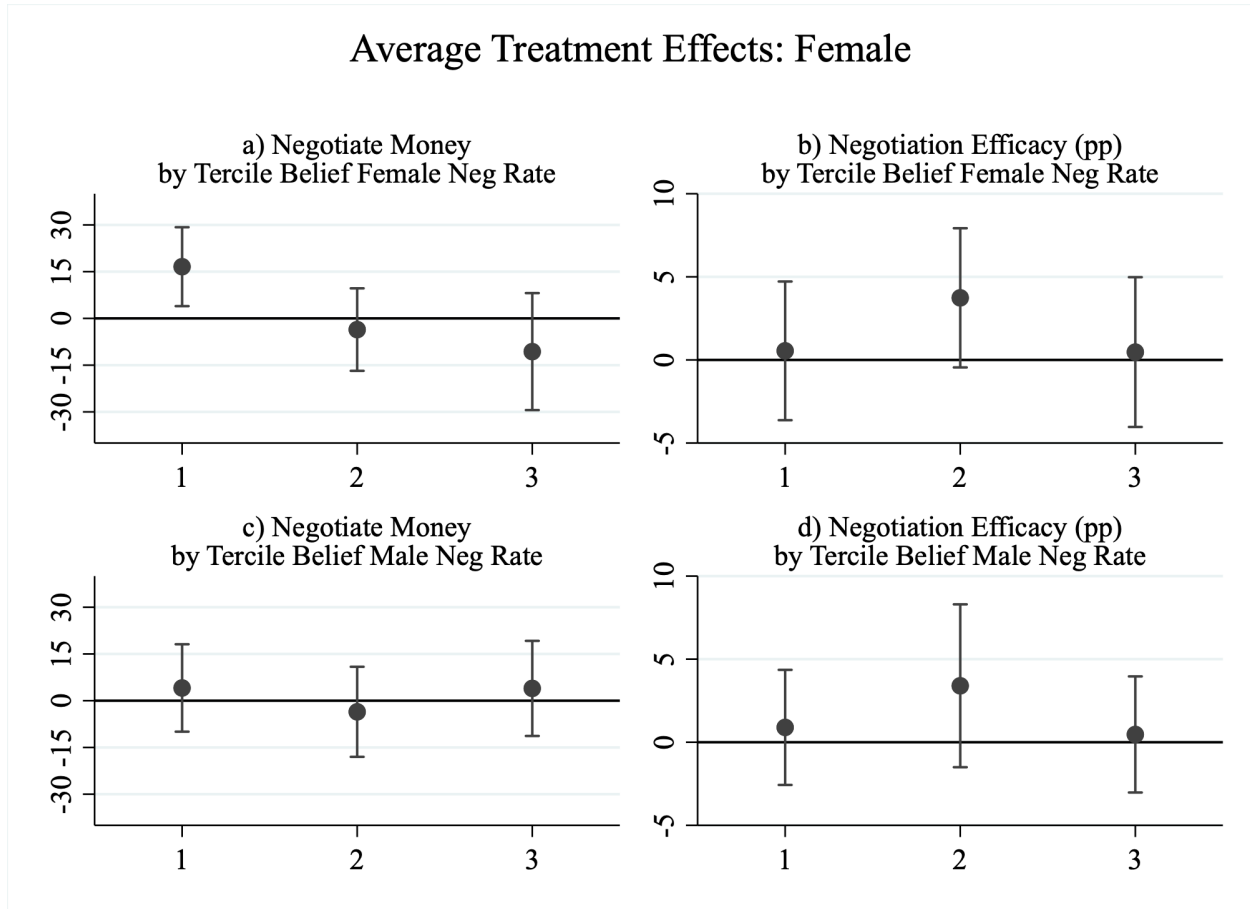
Notes: This figure plots binscatters of monetary negotiation rates by beliefs about peer negotiation behavior for the pre-treatment sample of recent Questrom graduates (2013-2017 cohorts). Efficacy beliefs winsorized above 30pp. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Figure 4: Immediate Impacts on Intentions



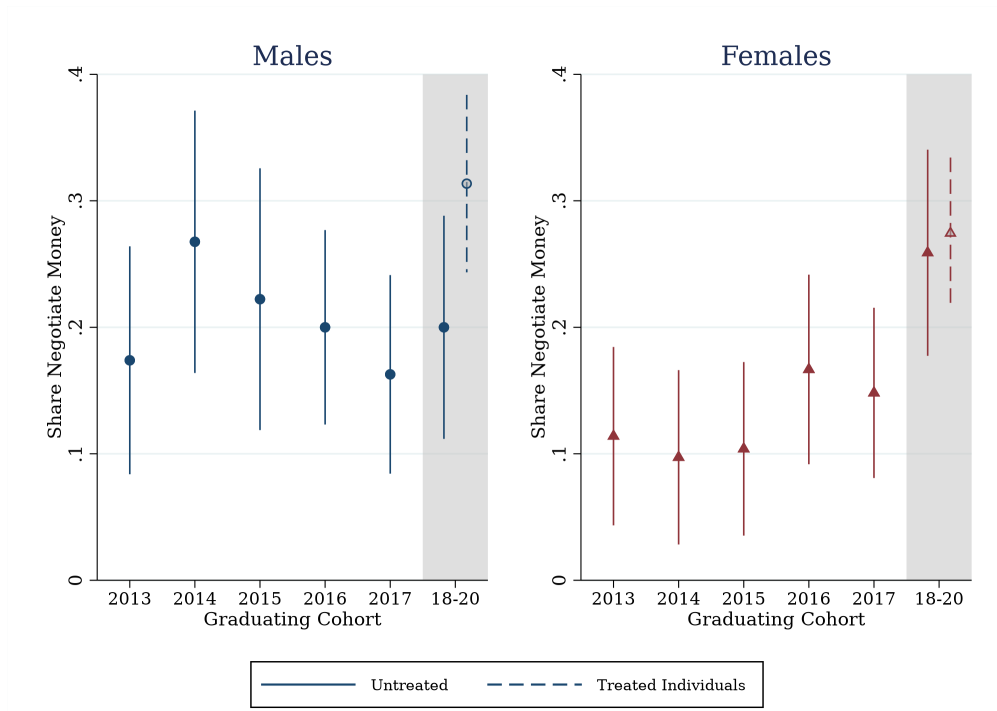
Notes: This figure visualizes the difference in mean intent to negotiate monetary (Panel A) and non-monetary (Panel B) aspects of job offers before (light gray) and after (dark gray) the information treatment. The bars represent 95% confidence intervals. Stars denote statistical significance between pre- and post-treatment assignment mean likelihoods. (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Figure 5: Treatment Heterogeneity by Tercile of Prior Beliefs: Females



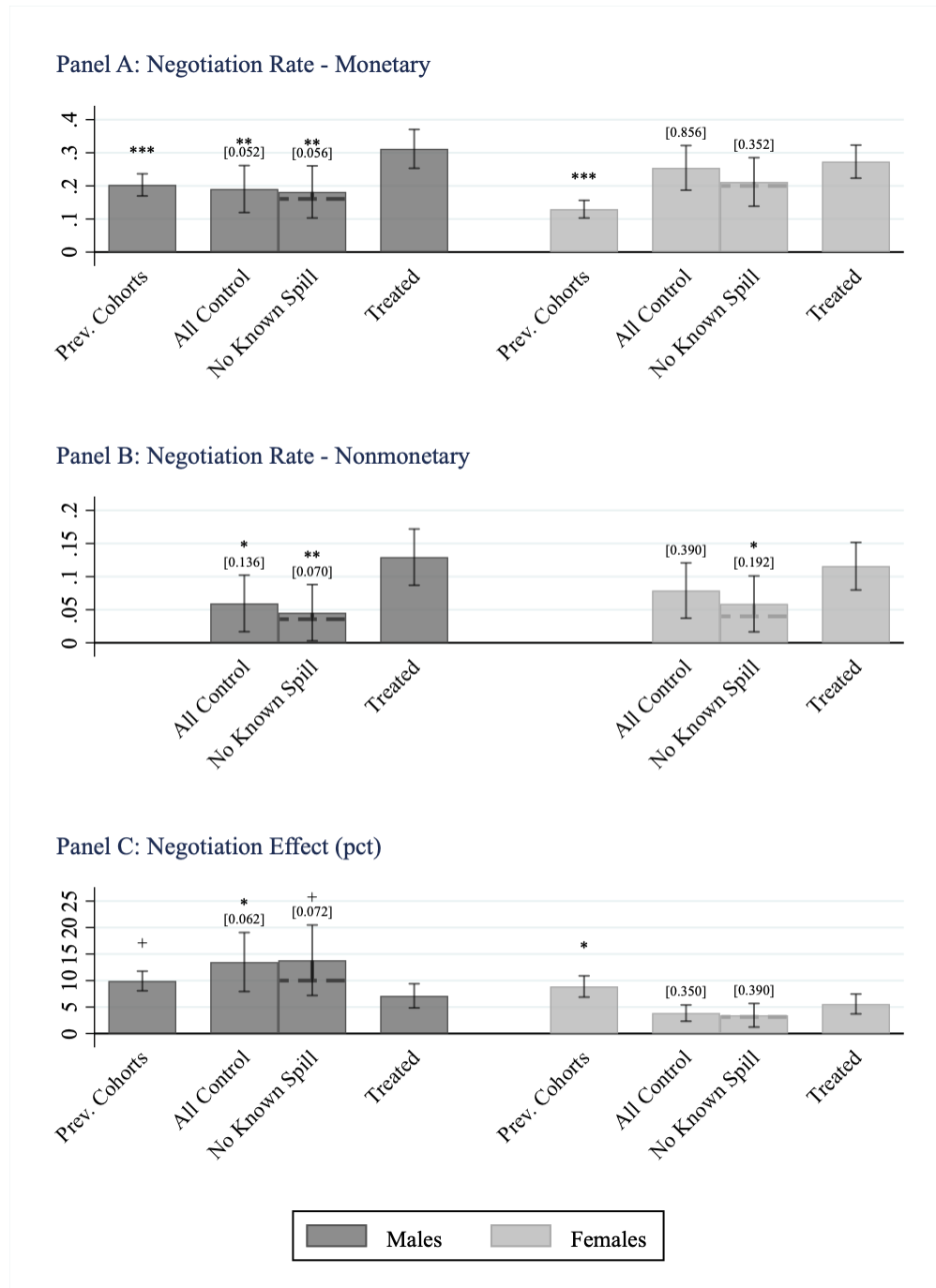
Notes: This figure plots the difference between treated and control respondents' negotiation rates (panels a and c) and negotiation efficacy (panels b and d) by terciles of baseline (pre-information) beliefs about same-gender peer negotiation rates. Y-axis positions correspond to the difference in outcomes between treated and control individuals with baseline beliefs in the respective tercile. For example, panel a) demonstrates the average treated female with beliefs about Questrom females' negotiation rate below the 33rd percentile of all female respondents was 16pp more likely to negotiate than the average control female with similar baseline beliefs. The bars represent 90% confidence intervals.

Figure 6: Pre-treatment Cohorts vs Treated Cohorts



Notes: This figure plots the monetary negotiation rates by cohort for the pre-treatment cohorts (2013-2017) and the treatment cohorts (2018-2020). For the treatment cohorts, rates are shown separately by treatment status.

Figure 7: Treatment Effects With Spillovers



Notes: Panel A shows the monetary negotiation rates for several different samples by gender. Panels B and C show the non-monetary negotiation rates and negotiation efficacy respectively for the same samples. The left most shaded bars correspond to the rate within pre-treatment cohorts (2013-2017). The other shaded bars correspond to different samples of the controls within the treated cohorts (2018-2020), with the relatively more darkly shaded corresponding to the rate for all controls. The “No Known Spill” sample corresponds to the control sample, excluding respondents who indicated that they had learned the treatment information at some point during their search process. The dashed line within the no known spillover sample corresponds to a non-response bound. This bound was constructed by applying the spillover rate observed within gender for respondents to the follow-up survey to non-respondents to this follow-up survey, and excludes the same proportion of non-respondents, selecting individuals with the highest outcome values (those who negotiated for the binary negotiation outcomes, for example) to drop first. The non-response bound presents the largest treatment effect possible if the share of “contaminated” controls was the same for those who did and did not respond to the follow-up survey. Stars represent significance between indicated group and treated group (within gender) in a difference of means test and brackets represent the p-value derived for a permutation test with 1000 replications. (+ $p < 0.15$, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table 1: Survey of Recent Graduates (2013-2017 Cohorts)

	Males	Females	Δ
Obs	444	494	
First Job Outcomes			
Negotiated Money (0/1)	0.19	0.12	0.07***
Negotiated Non-money (0/1)	0.07	0.07	-0.01
Total Pre-Neg. Earnings (1,000s)	63.96	56.36	7.59***
Total Post-Neg. Earnings (1,000s)	64.90	56.83	8.08***
Earn Increase Neg. (pct.)	9.89	7.84	2.04
Beliefs about Peer Negotiation			
Gender Ratio Monetary Neg. Rate M/F	1.33	1.69	-0.36***
Average Earn Increase Neg. (pct.)	13.37	11.33	2.04**
Why Not Negotiate			
I did not know that I could negotiate	0.16	0.21	-0.04
I did not want to send a negative signal	0.30	0.32	-0.02
I did not want to be perceived as too aggressive	0.20	0.26	-0.05
I was advised not to negotiate	0.15	0.14	0.01

Notes: Table displays mean values for indicated outcomes and beliefs by gender from a survey of Questrom graduates in the 2013-2017 graduating cohorts (pre-treatment cohorts). (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table 2: Estimated Treatment Effects: Revisions in Beliefs

	Revision: Likelihood Negotiate (1)	Revision: Negotiation Efficacy (2)	Revision: Negotiation Efficacy (3)	Likeli. Negotiate Non-Monetary (4)	Likeli. Negotiate Non-Monetary (5)	Likeli. Negotiate Non-Monetary (6)
Female	1.5 (1.0) [0.248]	1.3 (1.1) [0.112]	1.6** (0.7) [0.026]	1.5** (0.8) [0.028]	-0.3 (1.5) [0.666]	-0.8 (1.6) [0.582]
Treated	10.7*** (1.0) [0.000]	10.9*** (1.0) [0.000]	3.3*** (0.7) [0.000]	3.3*** (0.7) [0.000]	7.2*** (1.5) [0.000]	7.2*** (1.5) [0.000]
T X F	-1.7 (1.4) [0.294]	-2.1 (1.4) [0.184]	-2.2** (1.0) [0.028]	-2.1** (1.0) [0.032]	-0.8 (2.0) [0.694]	-0.9 (2.0) [0.690]
Constant	-2.8*** (0.7) [0.000]		-1.3** (0.5) [0.000]		-0.8 (1.1) [0.000]	
Prior Mean	57.4	57.4	15.6	15.6	46.5	46.5
N	1,416	1,416	1,367	1,367	1,416	1,416
r ²	0.110	0.128	0.017	0.030	0.030	0.047
Controls:						
Major, GPA, Cohort, Age	No	Yes	No	Yes	No	Yes
Race, US Born, Parent Edu						

Notes: This table displays regression coefficients from regressions of the labeled column variable on the labeled row variables. Robust standard errors in parenthesis. Permutation p-values in brackets (n=1000). Revisions constructed as the posterior likelihood (after treatment) minus the initial likelihood (before treatment assignment). Top and bottom 5% of responses winsorized. Each row presents estimates from separate estimations. “T × F” represents an interaction between female and treated dummies. Controls include race, age, senior in baseline, cohort, father education, mother education, and US born. (* p<0.1, ** p<0.05, *** p<0.01)

Table 3: Estimated Treatment Effects: Actual Job Negotiation

	Negotiate Money (pct)		Negotiation Efficacy Negotiating (pct)		Negotiate Nonmonetary (pct)	
	(1)	(2)	(3)	(4)	(5)	(6)
Female	6.4 (5.9) [0.182]	4.8 (6.1) [0.238]	-9.6*** (3.4) [0.024]	-8.3** (3.4) [0.014]	1.9 (3.6) [0.574]	-0.8 (3.8) [0.582]
Treated	12.1** (5.6) [0.032]	11.6** (5.6) [0.062]	-6.4* (3.6) [0.046]	-7.7** (3.1) [0.010]	7.0* (3.7) [0.072]	6.6* (3.9) [0.096]
T X F	-10.3 (7.6) [0.196]	-9.5 (7.6) [0.244]	8.1** (3.9) [0.028]	9.3** (3.7) [0.012]	-3.3 (5.0) [0.584]	-2.5 (5.2) [0.608]
Constant	19.0*** (4.3) [0.032]		13.5*** (3.3) [0.040]		6.0** (2.6) [0.068]	
Control Mean	22.7	22.7	7.3	7.3	7.1	7.1
N	584	584	157	157	584	584
r ²	0.007	0.089	0.074	0.342	0.007	0.080
Joint Sig. M Treat: 0.000						
Joint Sig. F Treat: 0.000						
Controls:						
Baseline Expectations	No	Yes	No	Yes	No	Yes
Major, GPA, Cohort, Age						
Race, US Born, Parent Edu						

Notes: This table displays regression coefficients from regressions of the labeled column variable on the labeled row variables. Robust standard errors in parenthesis. Each row presents estimates from separate estimations. Permutation p-values in brackets (n=1000). P-values from Wald tests for joint significance of treatment effects for columns 2, 4, and 6 reported on left hand side of table separately by gender. Controls include pre-treatment beliefs about given outcome, race, age, senior in baseline, cohort, father education, mother education, and US born. (* p<0.1, ** p<0.05, *** p<0.01)

Table 4: Baseline Perception Gaps and Treatment Effects

	Negotiate Monetary Aspect of Accepted Job Offer \times 100							
	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated (0/1)	14.30** (5.75)	13.84** (5.68)	9.20 (8.40)	9.56 (9.10)	-2.73 (5.66)	0.74 (5.48)	14.38 (8.77)	1.90 (11.83)
PG: Ratio M/F Rate Neg. Money	2.69 (4.41)				-8.87** (3.67)			
Treated \times PG: Ratio	-3.14 (6.09)				11.53** (4.91)			
PG: Neg. Efficacy (pp)		0.44 (0.33)				0.09 (0.36)		
Treated \times PG: Efficacy		-0.57 (0.46)				-0.10 (0.40)		
Components of M/F Neg. Money:								
PG: F Money Neg. Rate (pp)			0.06 (0.18)				0.46** (0.21)	
Treated \times PG: F Rate			0.15 (0.24)				-0.48* (0.26)	
PG: M Money Neg. Rate (pp)				0.09 (0.16)				0.12 (0.18)
Treated \times PG: M Rate				0.12 (0.24)				-0.03 (0.24)
Constant	17.97*** (4.36)	18.37*** (4.26)	16.91*** (6.01)	15.82** (6.28)	29.52*** (4.63)	26.91*** (4.38)	13.76** (6.72)	21.57** (8.96)
Obs.	239	239	239	239	310	310	310	310
R^2	0.023	0.030	0.030	0.029	0.013	0.000	0.016	0.003

Notes: Each column shows estimates from separate regressions of a dummy variable for negotiating money in accepted job offer (0/100) onto the labeled row variables. Robust standard errors in parenthesis. Perception gap (PG) defined as the difference between individuals' pre-information beliefs and the value provided in the information treatment (negotiation ratio and efficacy) or the observed value in the Questrom Alumni survey (negotiation rates). (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table 5: Spillovers: Effect of Share of Major/Gender Treated on Control

	Neg. Money (0/1)	
	Untreated Females	Untreated Males
	(1)	(2)
Share Major/Gender Treated (0-1)	0.49** (0.20)	-0.17 (0.28)
Constant	-0.08 (0.13)	0.28 (0.19)
Mean Dep.	0.24	0.16
Obs.	159	124
R^2	0.018	0.004

Notes: Each column displays estimates from separate regressions of a dummy variable for negotiating money in accepted job offer onto the row variables. Share major treated denotes the share of other respondents within the same major/survey year/gender who were treated (out of all respondents in to the baseline survey). Students were assigned their most popular major of all reported majors. Regression sample restricted to all untreated respondents. Robust standard errors in parenthesis clustered at the cohort X major level.

Table 6: Gender-Specific Information Spillovers

	All	Female	Male	(2) - (3)	p-value: (2)=(3)
	(1)	(2)	(3)	(4)	(5)
Full Sample					
Obs.	526	280	246		
Make important decision: by asking for advice/observe others' decisions (0/1)	0.22 (0.02)	0.25 (0.03)	0.18 (0.02)	0.07	0.051
Discussed whether or not to negotiate offers with peers: often (0/1)	0.13 (0.02)	0.14 (0.02)	0.12 (0.02)	0.03	0.357
Peers influenced whether or not to negotiate offers: A lot (0/1)	0.12 (0.01)	0.14 (0.02)	0.10 (0.02)	0.04	0.206
It's important for me to encourage my female peers to advocate for themselves on the job: completely agree (0/1)	0.64 (0.02)	0.75 (0.03)	0.51 (0.03)	0.24	0.000
It's important for me to encourage my male peers to advocate for themselves on the job: completely agree (0/1)	0.38 (0.02)	0.35 (0.03)	0.41 (0.03)	-0.07	0.120
Treated					
Obs.	262	135	127		
Number female peers discussed info. with	1.05 (0.09)	1.37 (0.15)	0.72 (0.09)	0.64	0.000
Number male peers discussed info. with	0.86 (0.08)	0.53 (0.09)	1.21 (0.13)	-0.68	0.000
Control					
Obs.	192	109	83		
Learned about info. from peers (0/1)	0.15 (0.03)	0.17 (0.04)	0.12 (0.04)	0.04	0.388
Learned about info. from peers/classroom/social media (0/1)	0.37 (0.03)	0.37 (0.05)	0.37 (0.05)	-0.01	0.927
If learned info: the information made me more likely to negotiate (0/1)	0.47 (0.07)	0.50 (0.10)	0.45 (0.09)	0.05	0.702

Notes: This table displays mean responses to the indicated questions. Column 1 shows means for the full sample, while column 2 restricts the sample to females and column 3 to males. Column 4 displays the difference in means between females and males. Column 5 displays the p-value for a test of significance of the column 4 differences. Standard errors in parenthesis.

Table 7: Estimated Treatment Effects: Job Characteristics

	Job Compensation (\$1,000)		Satisfied with Job (0/1)		Number Benefits		Flexible Work (0/1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	-5.19* (3.13) [0.532]	-4.43 (3.01) [0.774]	0.04 (0.07) [0.712]	0.03 (0.07) [0.528]	0.98*** (0.38) [0.650]	0.67* (0.37) [0.842]	0.07 (0.07) [0.794]	0.03 (0.07) [0.850]
Treated	3.39 (3.18) [0.304]	2.76 (2.99) [0.418]	-0.02 (0.06) [0.746]	-0.04 (0.06) [0.566]	0.08 (0.32) [0.808]	0.12 (0.31) [0.662]	0.05 (0.07) [0.504]	0.04 (0.06) [0.606]
T X F	-2.30 (3.95) [0.544]	-1.33 (3.82) [0.786]	0.03 (0.08) [0.712]	0.05 (0.08) [0.532]	0.22 (0.47) [0.640]	0.10 (0.45) [0.830]	0.02 (0.09) [0.784]	0.02 (0.08) [0.836]
Constant	62.76*** (2.52) [0.302]		0.73*** (0.05) [0.740]		2.49*** (0.26) [0.808]		0.57*** (0.05) [0.504]	
Control Mean	59.77	59.77	0.75	0.75	2.99	2.99	0.61	0.61
N	584	584	522	522	1,434	1,434	584	584
r ²	0.024	0.147	0.004	0.064	0.018	0.131	0.011	0.097

Joint Sig. M Treat: 0.770

Joint Sig. F Treat: 0.773

Controls:

Major, GPA, Cohort, Age
Race, US Born, Parent Edu

No Yes No Yes No Yes No Yes

Notes: Table displays regression coefficients from regressions of the labeled column variable onto the labeled row variables. Robust standard errors in parenthesis. Each row presents estimates from separate estimations. Permutation p-values in brackets (n=1000). P-values from Wald tests for joint significance of treatment effects for columns 2, 4, 6, and 8 reported on left hand side of table separately by gender. Respondents' job satisfaction only collected in the final survey round, and so is unknown for the subset of respondents who answered the mid-search survey after finding a job and did not respond to the final survey. The survey question for job satisfaction was: *"Taking everything into consideration, how satisfied would you say you are, overall, in your current job?"*. Respondents who answered a 4 or 5 on a 5-point Likert scale are coded as being satisfied with the job. The survey question for work flexibility was: *"Which of the following benefits does this job have? Flexible work options such as flextime (flexible or alternative work schedule, and flexplace (telecommuting - working from home))"*. "Number Benefits" indicates the number of benefits at a respondent's first job from a predetermined list including maternity leave, paternity leave, sick leave, childcare, flexible working schedule, pension, 401k/retirement account, health insurance, dental insurance, health savings account, housing benefits, life insurance, and commuting benefits. Controls include race, age, senior in baseline, cohort, father education, mother education, and US born. (* p<0.1, ** p<0.05, *** p<0.01)

Table 8: Treatment Effects on Longer-Term Expected Outcomes

	Prob Same Employer 5 Yrs (pp) ^a		Prob Earn 90k at 30 (pp) ^b		Prob Nego Money in 1 Yr (pp) ^c		Prob Nego Non-money in 1 Yr (pp) ^c		Change in Res. Wage (1,000s) ^d	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Female	-0.3 (3.8) [0.658]	1.7 (4.1) [0.624]	-1.8 (4.1) [0.310]	-0.9 (4.3) [0.446]	15.1** (6.5) [0.004]	15.0** (7.0) [0.004]	7.7 (6.1) [0.070]	6.5 (6.8) [0.082]	-5.2* (3.1) [0.040]	-3.7 (3.3) [0.116]
Treated	3.8 (3.6) [0.338]	3.6 (3.8) [0.366]	9.1** (3.7) [0.006]	9.2** (3.8) [0.004]	16.9*** (6.3) [0.012]	18.5*** (6.6) [0.006]	10.2* (5.8) [0.102]	11.5* (6.1) [0.060]	0.4 (2.7) [0.860]	2.4 (2.7) [0.380]
T X F	2.3 (4.7) [0.640]	2.5 (4.9) [0.600]	-4.3 (4.7) [0.330]	-3.1 (4.9) [0.468]	-25.7*** (8.2) [0.004]	-28.1*** (8.8) [0.004]	-13.0* (7.7) [0.076]	-14.2* (8.4) [0.076]	7.6** (3.8) [0.044]	6.3 (4.2) [0.112]
Constant	31.7*** (2.9) [0.326]		74.9*** (3.3) [0.008]		39.2*** (5.1) [0.012]		42.1*** (4.5) [0.098]		-4.8** (2.2) [0.862]	
mean	34.9	34.9	78.4	78.4	49.5	49.5	48.4	48.4	-4.7	-4.7
obs.	522	522	522	522	332	332	332	332	234	234
R ²	0.01	0.07	0.03	0.14	0.03	0.11	0.01	0.07	0.04	0.18
Joint Sig. M Treat: 0.002 Joint Sig. F Treat: 0.002										
Controls:										
Major, GPA, Cohort, Age	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Race, US Born, Parent Edu										

^a Question asked conditional on accepting an offer and responding to post-graduation survey.

^b Question elicited in post-graduation survey. Sample restricted to those who accepted an offer.

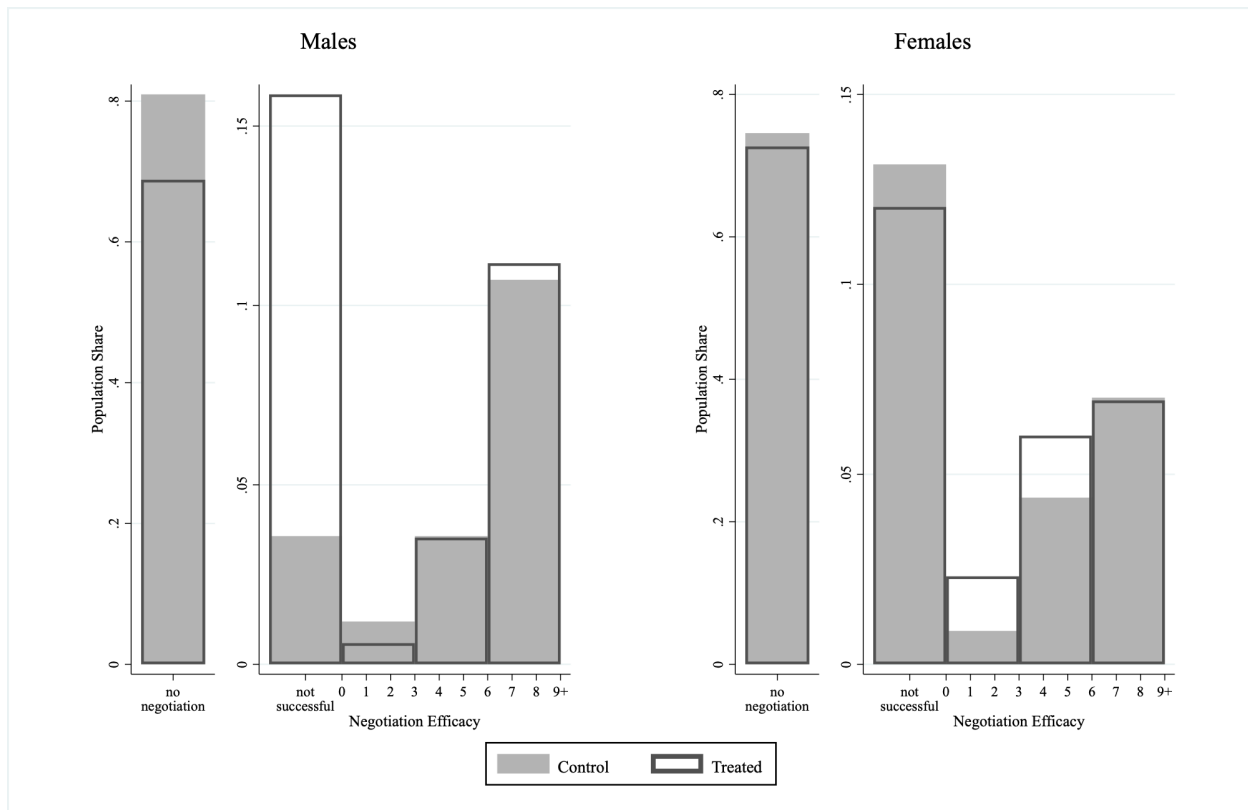
^c Question elicited in post-graduation survey of 2019 and 2020 cohorts.

^d Baseline reservation wages elicited for 2019 and 2020 cohorts. Post-search reservation wages elicited in post-graduation survey only.

Notes: Table displays regression coefficients from regressions of the labeled column variable onto the labeled row variables. Robust standard errors in parenthesis. Each row presents estimates from separate estimations. “T X F” represents an interaction between female and treated dummies. Permutation p-values in brackets (n=1000). P-values from Wald tests for joint significance of treatment effects for columns 2, 4, 6, 8, and 10 reported on left hand side of table separately by gender. (* p<0.1, ** p<0.05, *** p<0.01)

Appendix A: Additional Figures and Tables

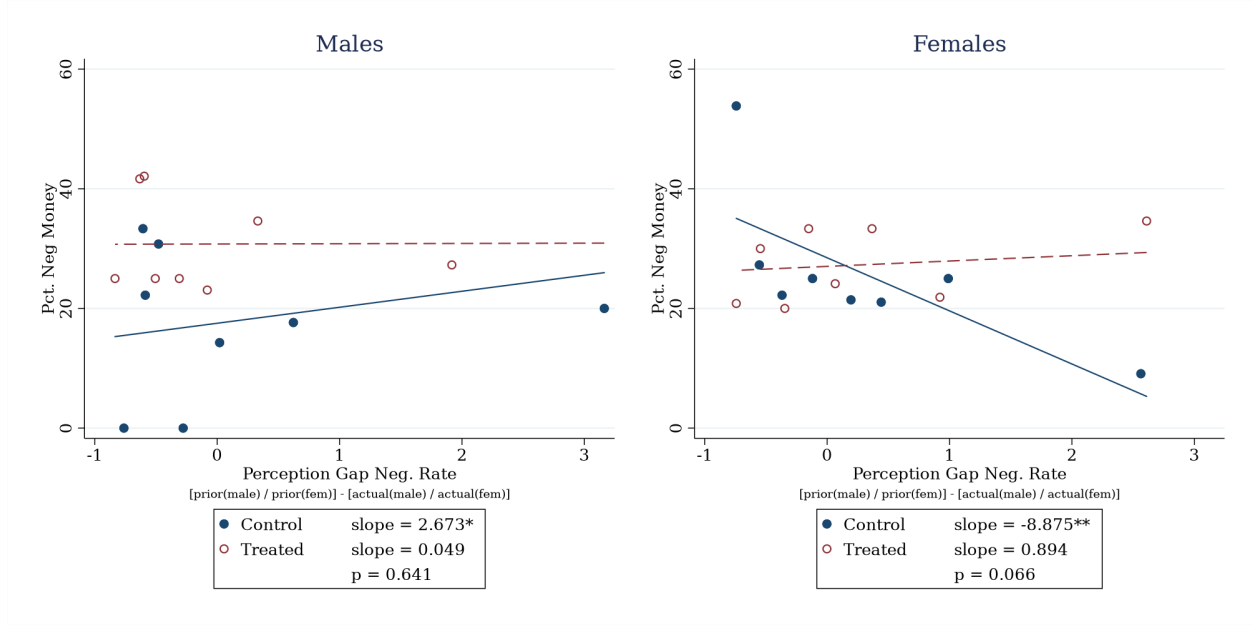
Figure A1: Distribution of Negotiation Efficacy



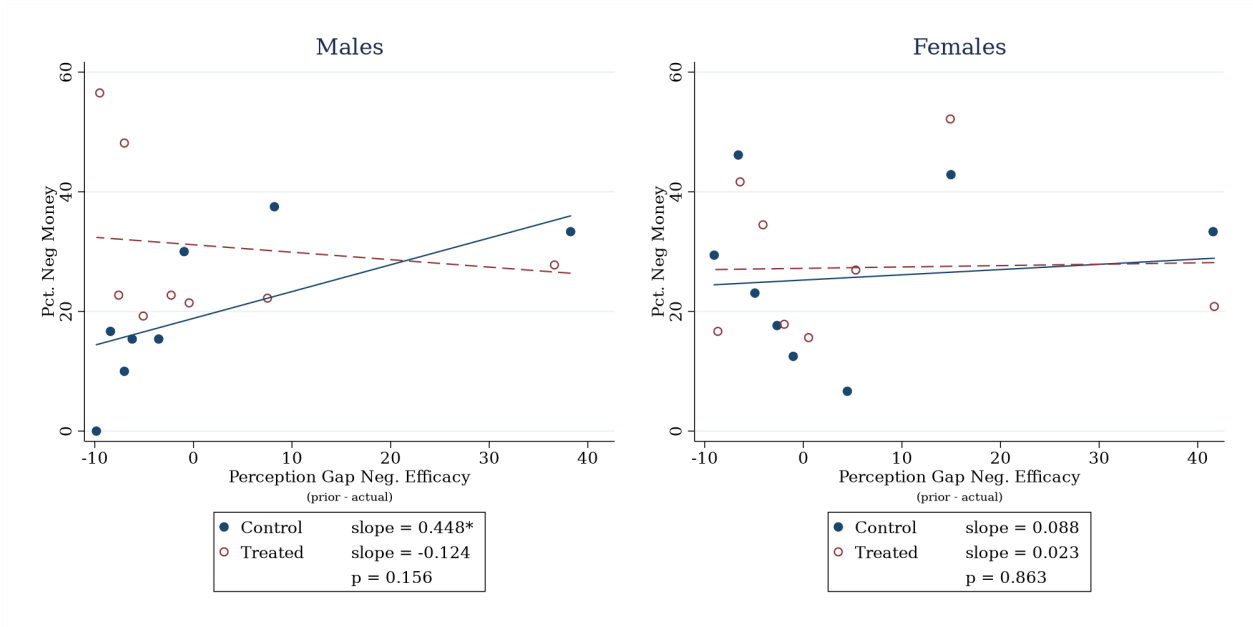
Notes: Figure shows the distribution of negotiation efficacy by gender and treatment status. The left-most bar indicates the extensive margin of negotiation (i.e., the share of each group that chose to negotiate) whereas the bars on the right are conditional on the group of individuals that chose to negotiate. The distribution is measured relative to the number of individuals with the same gender and treatment status.

Figure A2: Negotiation and Error in Priors

(a) Gender Negotiation Gap

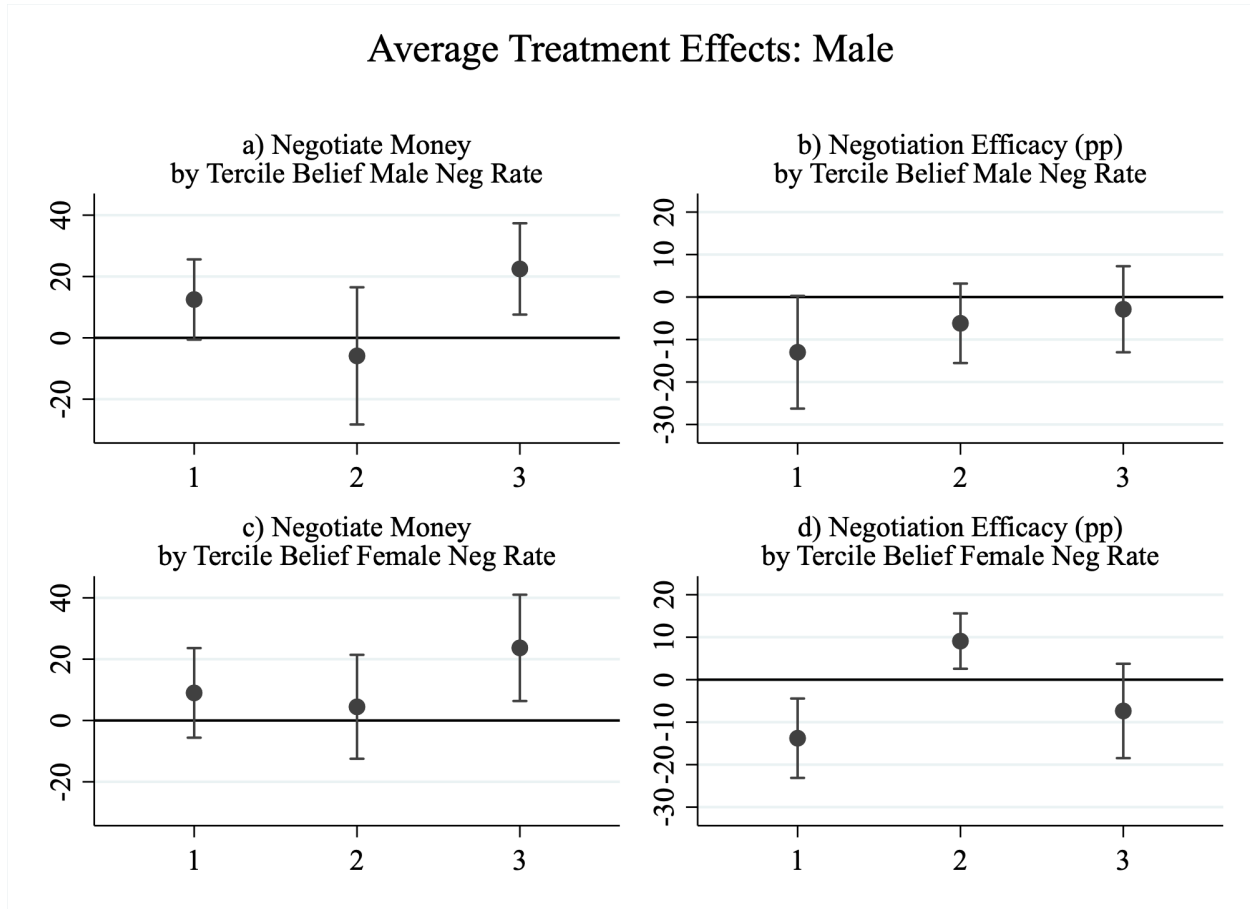


(b) Negotiation Efficacy



Notes: This figure shows binscatter plots of negotiation rates by error in prior beliefs in the gender negotiation gap (panel (a)) and negotiation efficacy (panel (b)) separately by gender (male right hand figures, females left hand figures) and treatment status (controls solid blue, treated open red). The legends for each subplot also show the estimated coefficient from the regression of binary negotiation onto perception gaps, along with the test for equality between the treated and control groups within a sub-figure.

Figure A3: Treatment Heterogeneity by Tercile of Prior Beliefs: Males



Notes: This figure plots the difference between treated and control respondents' negotiation rates (panels a and c) and negotiation efficacy (panels b and d) by terciles of baseline (pre-information) beliefs about same-gender peer negotiation rates. Y-axis positions correspond to the difference in outcomes between treated and control individuals with baseline beliefs in the respective tercile. For example, panel a) demonstrates the average treated male with beliefs about Questrom males' negotiation rate below the 33rd percentile of all male respondents was 15pp more likely to negotiate than the average control male with similar baseline beliefs. The bars represent 90%

Figure A4: Negotiation Course Material Related to Gender Differences in Negotiation

**A WOMAN COLLEGE
GRADUATE WILL EARN
APPROXIMATELY \$1 MILLION
LESS OVER HER WORKING
LIFE THAN A MAN WHO
RECEIVES THE SAME DEGREE
AT THE SAME TIME.**



Boston University Questrom School of Business

Research

Gender differences in the rates of negotiation are significant and account for some of the pay gap:

- Men will negotiate more often and see more opportunities to negotiate

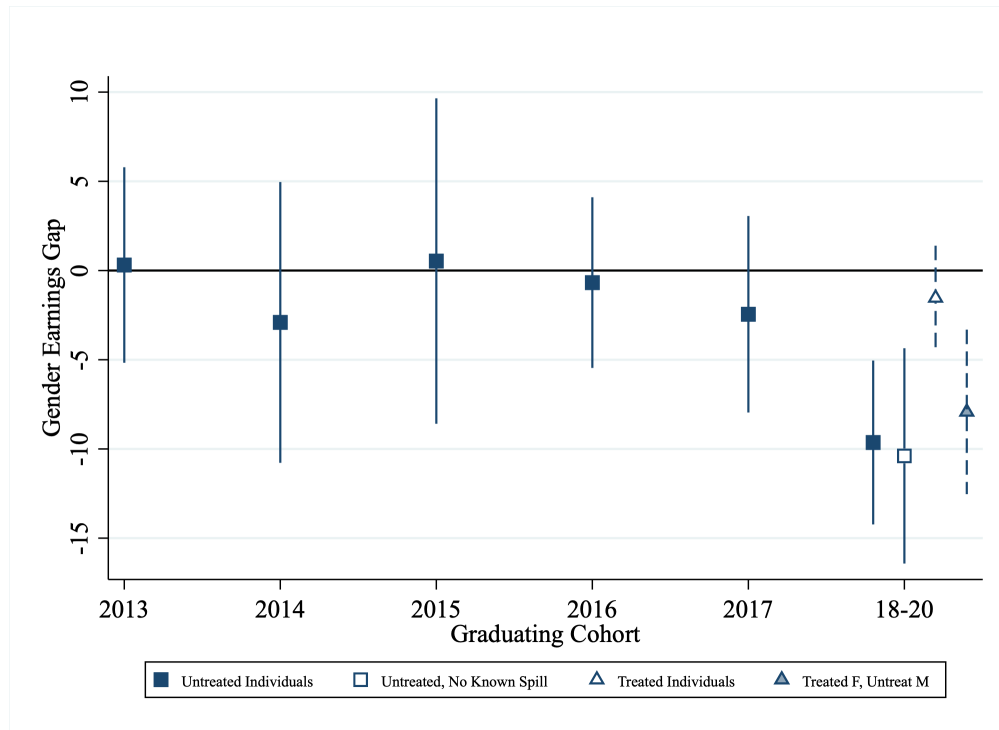
Research indicates that **educating** women about this issue, increases subsequent rates of negotiation

- If negotiation is given as an option in a job posting, women are more likely to negotiate



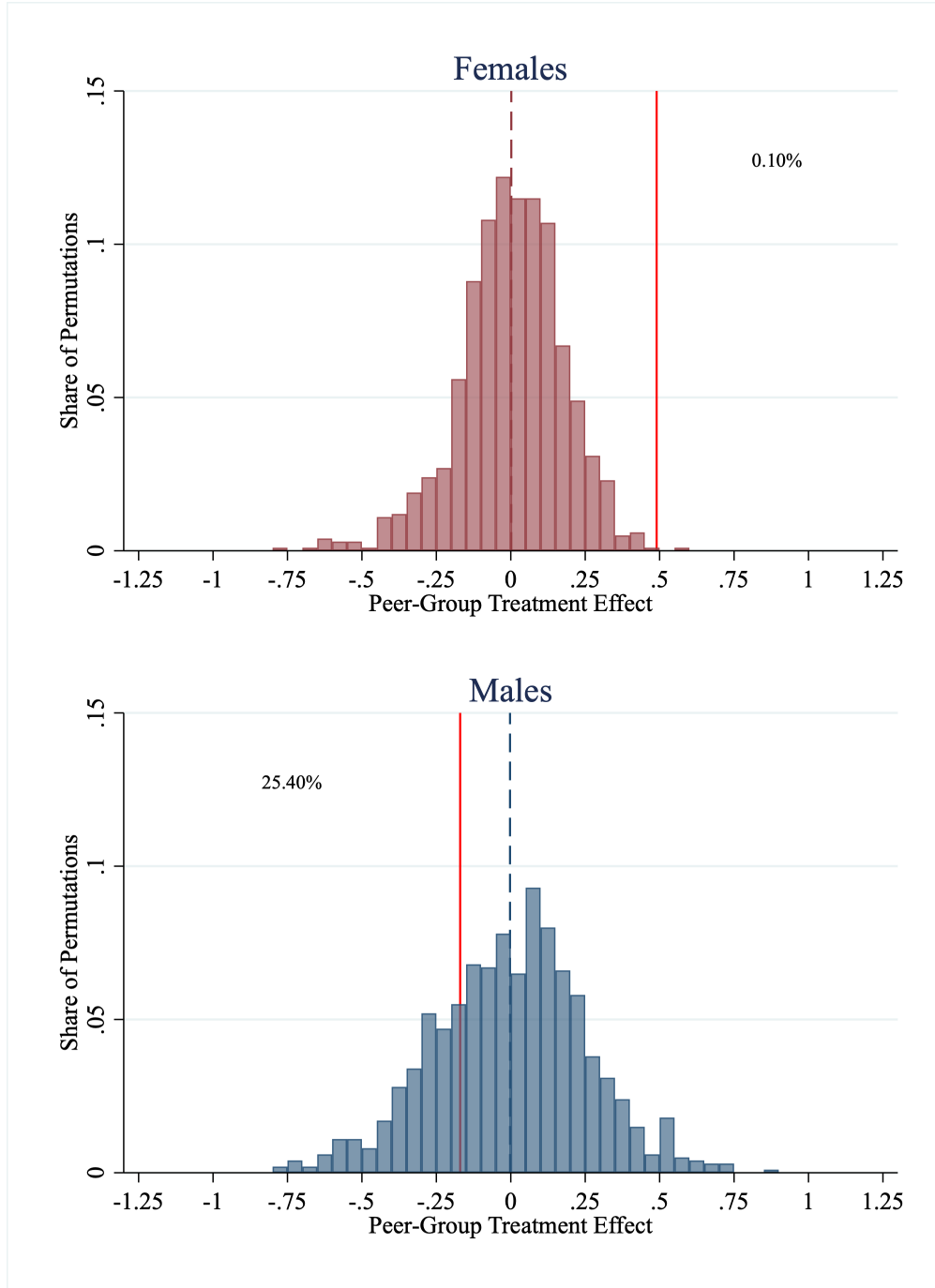
Boston University Questrom School of Business

Figure A5: Gender Gaps in Negotiating Efficacy by Cohort



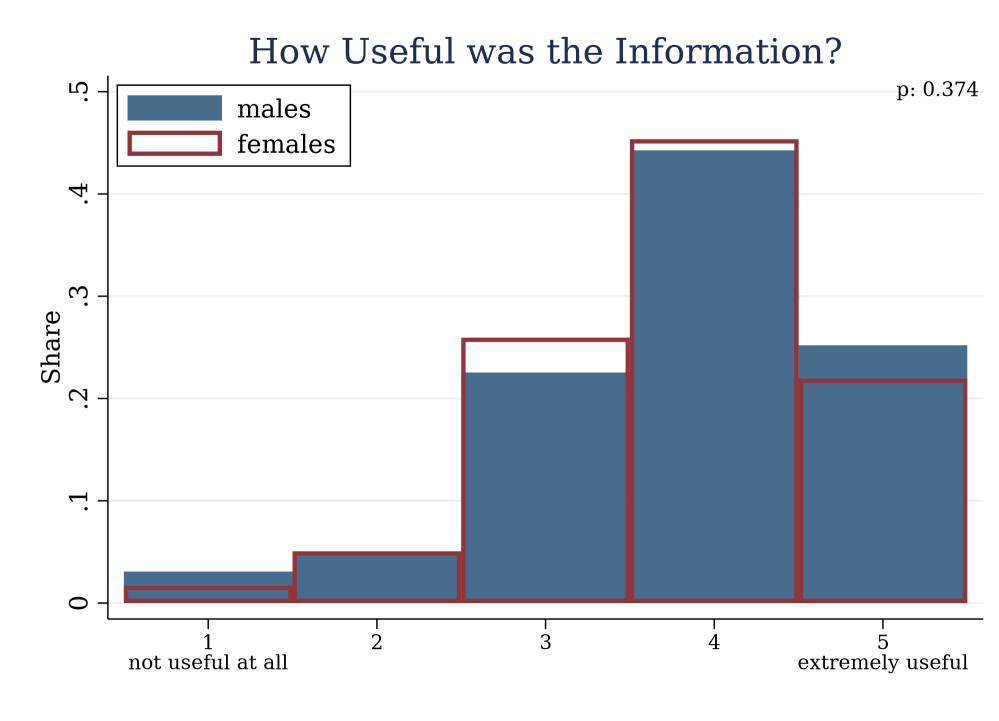
Notes: This figure plots the difference in average negotiation efficacy (conditional on attempting negotiation) between females and males for previous (untreated) cohorts and the treated cohorts separately by treatment status.

Figure A6: Peer-Group Spillover Statistical Test



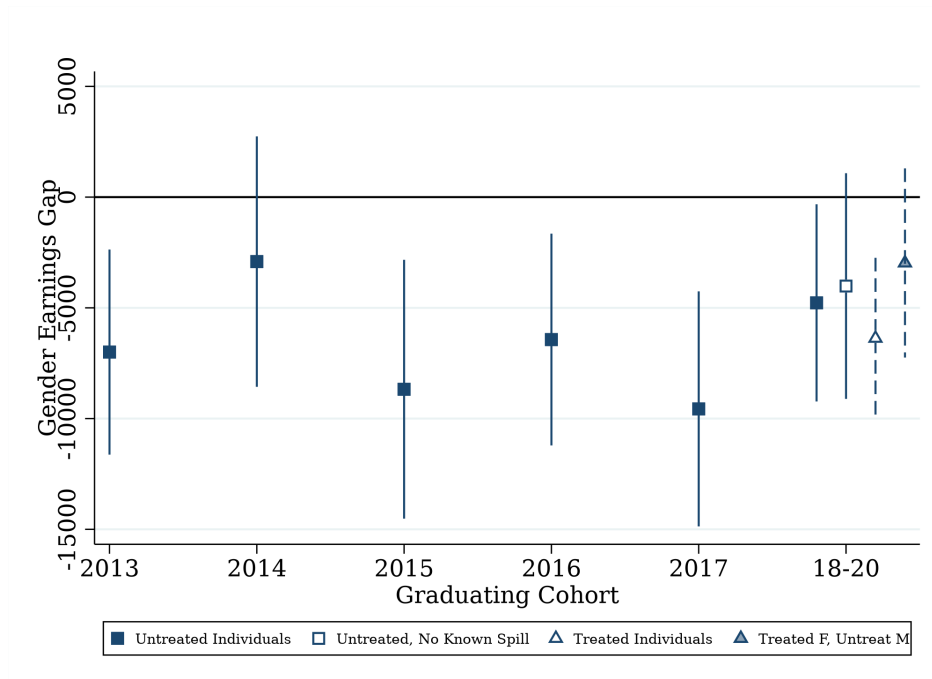
Notes: This figure shows tests of statistical significance of share-peer-treated coefficients in Columns (1) and (2) of Table 5. The histograms display the distribution of estimated share-peer-treated effects from 1000 replications of the same regressions in Table 5 within a sample of Alumni respondents who were randomly assigned a share-peer-treated value observed for one of the major/survey year observations in the survey cohorts. Share-peer-treated block assigned with replacement at the major/cohort level for each replication. Dashed lines represent the average estimate across all replications. Solid red lines represent the estimated value from Table 5. Only 0.1% of estimates from this procedure yielded a coefficient larger than the value in Table 5. For males, more than 25% of replications have an estimate with a larger magnitude.

Figure A7: How Useful was the Information?



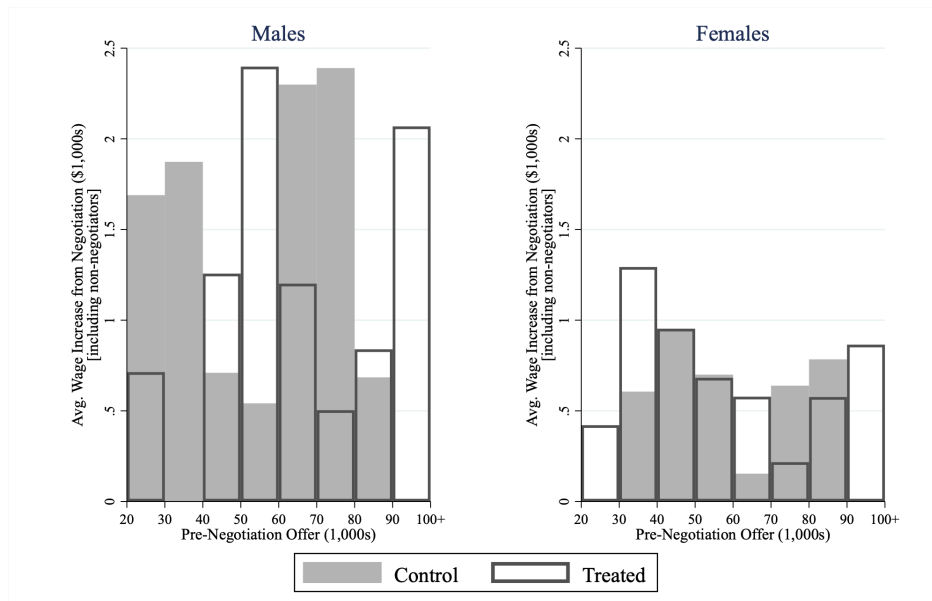
Notes: This figure displays histograms by gender of treated individual's immediate response to the information treatment. P -value in upper right corner for the equality of distribution across gender.

Figure A8: Gender Earnings Gap over Time and by Treatment Status



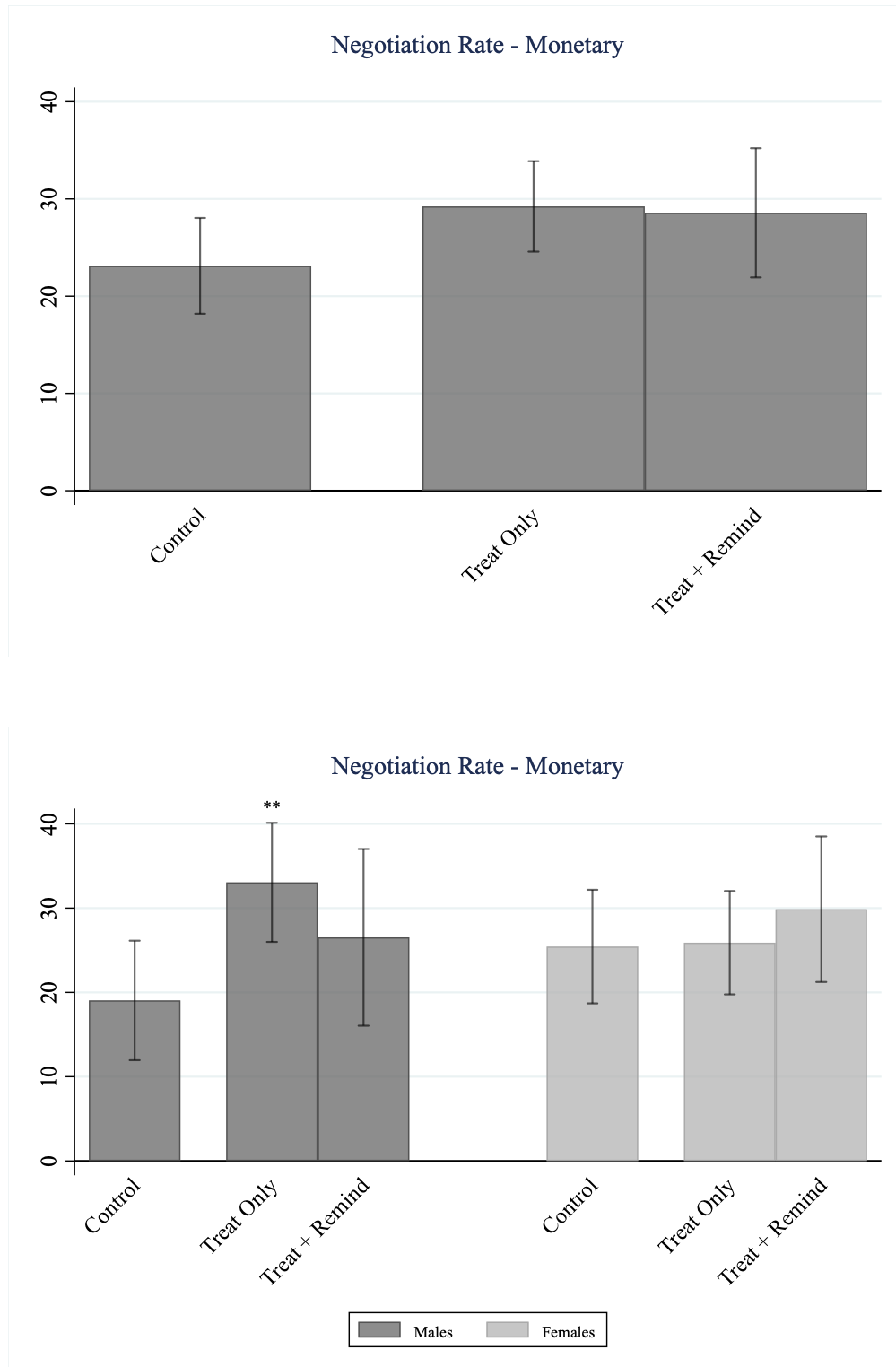
Notes: This figure plots the gender gap in first-year earnings for previous (untreated) cohorts and the treated cohorts separately by treatment status.

Figure A9: Negotiation Gains by Initial Offer



Notes: This figure plots the average wage increase from negotiation (including 0s for non-negotiators) by the pre-negotiation offer amount, separately by gender and treatment assignment.

Figure A10: Treatment Effects by Reminders



Notes: Upper panel shows the monetary negotiation rates separately by treatment arm (control, treated once, and treatment + reminder). Lower panel separates sample by gender. “Treated once” corresponds to individuals who were assigned to the information arm in the initial survey, but were not further randomized into the email reminder arm. “Treatment + reminder” corresponds to individuals who were initially randomized into the information arm and subsequently randomly selected to receive the email reminder with the information treatment. Stars correspond to statistical difference in means relative to the control group. Bars correspond to 90% confidence intervals (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

Table A1: Determinants of Negotiation

	Nego Money (0/1) Sample: 2013 to 2017 cohorts	
	(1)	(2)
Num. offers > 1	0.030 (0.027)	0.022 (0.027)
Offer total (1,000s)	-0.002*** (0.001)	-0.002*** (0.001)
Benefit: health ins. (0/1)	0.038 (0.060)	0.047 (0.060)
Benefit: retirement (0/1)	-0.102** (0.043)	-0.106** (0.042)
Age at grad		0.024 (0.019)
GPA		-0.007 (0.039)
SAT Verbal ^a		-0.001 (0.001)
SAT Math ^a		0.000 (0.001)
ACT ^a		0.017 (0.014)
Fin. risk tol (1-7)		0.021** (0.010)
Daily risk tol (1-7)		0.010 (0.011)
Cohort FEs	Y	Y
Major FEs		Y
Obs.	821	821
R^2	0.030	0.084
Mean Dep.	0.16	0.16

^a Scores were optional responses. Missing dummy included in regression.

Notes: Table shows coefficients from separate regressions of an indicator variable for negotiating money onto the indicated row variables. Sample includes all respondents to the survey of alumni (2013-2017 cohorts). Robust standard errors in parenthesis. (* p<0.1, ** p<0.05, *** p<0.01)

Table A2: Randomization Balance

	Treated	Control	P-val
Observations	937	500	
Female	0.493	0.504	0.679
Age	20.596	20.610	0.767
Senior	0.213	0.242	0.216
White	0.481	0.498	0.547
Asian	0.454	0.430	0.392
US Born	0.603	0.624	0.437
2018 Cohort	0.269	0.278	0.714
2019 Cohort	0.396	0.398	0.940
GPA	3.297	3.269	0.121
Expected Pay in First Year	73,364.5	73,191.7	0.899
Concentration:			
Accounting	0.139	0.162	0.236
Entrepreneurship	0.035	0.040	0.647
Finance	0.523	0.542	0.491
General Management	0.023	0.016	0.345
International Management	0.046	0.038	0.483
Law	0.062	0.048	0.280
Management Information Systems	0.255	0.250	0.833
Marketing	0.240	0.238	0.928
Operations and Technology Management	0.074	0.080	0.665
Organizational Behavior	0.029	0.024	0.593
Strategy and Innovation	0.097	0.090	0.661
Prior Beliefs:			
Likelihood Neg. Money	56.6	59.0	0.127
Likelihood Neg. Nonmonetary	46.7	46.1	0.746
Exp. Earnings Change from Neg.	15.8	15.0	0.440

Notes: This table displays mean observables at the baseline survey separately by treatment arm. Column 3 presents the p-values from a test of mean equality across treatment status. Sample includes all respondents to the baseline survey.

Table A3: Experiment Sample Compared to Questrom Population

	Questrom Population			Experiment Sample			<i>p</i> -value ^a
	Female	Male	Gender Diff.	Female	Male	Gender Diff.	
	(1)	(2)	(3)	(4)	(5)	(6)	
Foreign Student	0.33	0.29	0.04*	0.29	0.24	0.05**	0.695
GPA	3.25	3.17	0.08***	3.28	3.20	0.08***	0.916
Credits	16.40	16.21	0.19	16.60	16.46	0.14	0.786
Accounting	0.15	0.12	0.03*	0.11	0.12	-0.00	0.182
Finance	0.35	0.53	-0.18***	0.32	0.51	-0.19***	0.942
Marketing	0.28	0.10	0.17***	0.29	0.10	0.19***	0.440
Obs.	1,054	1,096		744	726		0.348 ^b

Notes: This table presents summary statistics for the 2018, 2019 and 2020 cohorts. For the 2018 cohort, the information refers to the Spring of 2018; for the 2019 and 2020 cohorts, the data were collected in the Fall of 2018.

^a *p*-value for whether the gender difference between the experiment sample and the Questrom population is different.

^b *p*-value for the difference in female proportion between the experiment sample and Questrom population.

Table A4: Correlates of Attrition

	Responded to Follow-up after Job (0/1)	Responded to Spillover Survey (0/1)
Female (0/1)	0.105*** (0.028)	0.036 (0.028)
Treated (0/1)	0.012 (0.027)	-0.019 (0.027)
Prior Likelihood Neg. Money (0-100)	0.001* (0.000)	0.001** (0.000)
Prior Expected Earn (\$1,000s)	-0.000 (0.001)	0.000 (0.001)
Age (years)	0.022 (0.015)	-0.031** (0.014)
White (0/1)	-0.037 (0.044)	-0.059 (0.042)
Asian (0/1)	-0.066 (0.044)	-0.057 (0.041)
US born (0/1)	0.179*** (0.030)	0.132*** (0.030)
GPA (0-4)	0.245*** (0.043)	0.215*** (0.043)
Accounting major (0/1)	-0.003 (0.038)	0.013 (0.038)
Finance major (0/1)	-0.063** (0.030)	-0.111*** (0.030)
Marketing major (0/1)	0.023 (0.036)	0.006 (0.036)
R^2	0.077	0.062
Obs.	1,343	1,343
Share responded	0.411	0.378

Notes: Table displays regression coefficients from a regression of a dummy for responding to a follow-up survey after finding a job (column 1) and responding to the follow-up survey (column 2) onto the indicated row variables. Robust standard errors in parenthesis. (* p<0.1, ** p<0.05, *** p<0.01)

Table A5: Estimated Treatment Effects by Baseline Beliefs

	Belief: Female Negotiation Rate				Belief: Male Negotiation Rate			
	Negotiate Money (pp)		Neg. Efficacy (pp)		Negotiate Money (pp)		Neg. Efficacy (pp)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
a) Sample: Females								
Treat X Belief Terc = 1	16.6** (7.6)	14.4* (7.8)	0.5 (2.4)	0.7 (3.8)	4.1 (8.4)	3.2 (8.7)	0.9 (1.9)	1.0 (3.0)
Treat X Belief Terc = 2	-3.6 (8.0)	-4.3 (8.3)	3.7 (2.5)	5.3* (3.1)	-3.6 (8.7)	-2.9 (8.9)	3.4 (2.9)	4.1 (3.2)
Treat X Belief Terc = 3	-10.7 (11.3)	-9.7 (11.4)	0.5 (2.7)	0.6 (2.6)	3.9 (9.2)	2.1 (9.3)	0.5 (2.1)	1.4 (2.5)
Belief Terc = 2	14.5* (8.4)	13.3 (8.9)	-1.9 (1.9)	-2.2 (3.7)	13.3 (9.6)	12.0 (9.9)	1.3 (2.4)	1.8 (3.5)
Belief Terc = 3	30.7*** (10.8)	28.0*** (11.2)	0.6 (2.4)	1.2 (4.0)	12.3 (9.7)	13.4 (9.9)	1.3 (2.0)	1.3 (3.2)
Constant	12.2** (5.2)		4.4*** (1.5)		16.7*** (6.3)		2.8* (1.5)	
Controls		Y		Y		Y		Y
Obs.	330	330	88	88	330	330	88	88
R ²	0.03	0.07	0.02	0.08	0.01	0.06	0.04	0.10
b) Sample: Males								
Treat X Belief Terc = 1	9.0 (8.8)	6.3 (9.1)	-13.8** (5.5)	-13.1** (5.9)	12.5 (7.9)	10.8 (8.0)	-13.0* (7.6)	-13.4* (7.8)
Treat X Belief Terc = 2	4.5 (10.2)	6.4 (11.0)	9.1** (3.6)	10.8** (4.3)	-5.9 (13.3)	-9.2 (13.6)	-6.2 (5.4)	-5.3 (6.3)
Treat X Belief Terc = 3	23.7** (10.4)	21.5** (10.1)	-7.4 (6.5)	-8.4 (6.9)	22.5** (9.0)	23.0*** (8.8)	-2.9 (6.0)	-3.3 (6.7)
Belief Terc = 2	-2.3 (10.6)	-4.3 (11.1)	-16.8*** (5.0)	-16.8*** (5.6)	24.7* (12.6)	29.3** (12.7)	-8.4 (8.5)	-10.4 (9.1)
Belief Terc = 3	-3.1 (10.3)	-3.1 (10.4)	-5.3 (8.0)	-3.5 (8.5)	3.5 (8.7)	4.0 (8.6)	-9.0 (8.9)	-8.6 (9.4)
Constant	20.5*** (6.5)		19.0*** (4.9)		12.1** (5.7)		20.0*** (6.9)	
Controls		Y		Y		Y		Y
Obs.	254	254	69	69	254	254	69	69
R ²	0.04	0.07	0.18	0.29	0.04	0.10	0.09	0.22

Notes: Table plots estimates from separate regressions of the dependent variable indicated above each column onto the indicated row coefficients. Panel a restricts the sample to females and panel b restricts the sample to males. Each estimation includes coefficients for terciles of baseline (pre-information) beliefs about peer negotiation rates, with the specific rates indicated above each column. Controls include major fixed effects. (* p<0.1, ** p<0.05, *** p<0.01)

Table A6: Treatment Effects on Outcomes Addressing Spillovers

Controls:	Males				Females			
	Same Cohort	Same Cohort No- Known Spill	Same Cohort Best- Case NRB	Prev. Cohorts	Same Cohort	Same Cohort No- Known Spill	Same Cohort Best- Case NRB	Prev. Cohorts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Obs.	254	236	226	564	330	301	291	648
Nego. Money (0/1)	0.12** (0.06) [0.052]	0.13** (0.06) [0.070]	0.15** (0.06) [0.034]	0.11*** (0.04) [0.002]	0.02 (0.05) [0.856]	0.06 (0.05) [0.330]	0.07 (0.06) [0.316]	0.14*** (0.03) [0.000]
Nego. Non-mon. (0/1)	0.07* (0.04) [0.142]	0.08** (0.04) [0.070]	0.09** (0.04) [0.080]		0.04 (0.03) [0.430]	0.06* (0.03) [0.192]	0.07** (0.03) [0.104]	
Nego. Effect	-0.35 (0.99) [0.682]	-0.30 (1.09) [0.760]	0.61 (0.95) [0.562]	0.20 (0.58) [0.728]	0.54 (0.45) [0.342]	0.79* (0.48) [0.208]	0.90* (0.49) [0.144]	0.39 (0.41) [0.300]
Nego. Effect if Nego.	-6.39* (3.60) [0.058]	-6.72 (4.17) [0.060]	-2.89 (4.26) [0.422]	-2.80 (1.78) [0.128]	1.71 (1.47) [0.344]	2.13 (1.76) [0.354]	2.44 (1.90) [0.298]	-3.30* (1.67) [0.056]
Earnings	3.39 (3.19) [0.336]	5.33 (3.30) [0.120]	9.38*** (3.31) [0.012]	2.05 (2.28) [0.330]	1.08 (2.33) [0.662]	1.73 (2.68) [0.528]	5.31** (2.62) [0.080]	1.60 (1.65) [0.298]
Satis. Current Job (0/1)	-0.02 (0.06) [0.902]	-0.02 (0.07) [0.932]	-0.02 (0.07) [0.932]	-0.04 (0.04) [0.500]	0.01 (0.05) [0.960]	0.03 (0.06) [0.686]	0.03 (0.06) [0.686]	0.04 (0.04) [0.332]

Notes: Table shows the effect of the information treatment on the indicated outcome for each row. Each cell represents estimates from a separate regression, with the control group varying across columns. The first four columns restrict the sample to males while the subsequent 4 columns restrict the sample to females. Columns 1 and 5 use all untreated observations within the treated cohort as a control group. Columns 2 and 6 restrict the control sample to only observations that did not respond in the follow-up survey that they learned about the treatment information. Columns 3 and 7 present a best-case control sample based on follow-up non-response, with non-responder observations preferentially dropped at the same rate as responders within a gender X negotiation strata. Columns 4 and 8 use previous cohorts as the control group. Standard errors in parenthesis. P-values from a permutation test in brackets. Stars indicate significance of the treatment coefficient from a wald test: (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

Table A7: Estimated Treatment Effects by Baseline Beliefs: No Contaminated Controls

	Belief: Female Negotiation Rate				Belief: Male Negotiation Rate			
	Negotiate Money (pp)		Neg. Efficacy (pp)		Negotiate Money (pp)		Neg. Efficacy (pp)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
a) Sample: Females								
Treat X Belief Terc = 1	15.0** (7.6)	11.8 (7.7)	1.8 (2.5)	9.2 (6.8)	8.9 (8.0)	8.3 (8.4)	2.4 (1.6)	5.9* (3.4)
Treat X Belief Terc = 2	0.9 (8.8)	1.5 (9.3)	4.7 (2.9)	4.9 (3.6)	-1.2 (9.1)	-0.2 (9.4)	3.1 (3.1)	3.9 (3.5)
Treat X Belief Terc = 3	1.8 (11.5)	3.1 (11.6)	0.2 (3.4)	-0.1 (3.4)	12.9 (9.2)	11.0 (9.5)	1.7 (2.4)	3.3 (3.2)
Belief Terc = 2	8.7 (9.2)	6.6 (9.5)	-0.7 (2.3)	7.7 (7.5)	17.9* (9.7)	16.2 (10.1)	3.2 (2.4)	7.1* (4.2)
Belief Terc = 3	17.3 (11.2)	14.2 (11.6)	2.5 (3.3)	10.9 (7.9)	9.3 (9.4)	10.8 (9.8)	1.5 (2.1)	4.5 (3.8)
Constant	13.2** (5.5)		2.8 (1.8)		10.7* (5.9)		1.3 (1.1)	
Controls		Y		Y		Y		Y
Obs.	309	309	78	78	309	309	78	78
R ²	0.02	0.06	0.03	0.12	0.02	0.06	0.05	0.12
b) Sample: Males								
Treat X Belief Terc = 1	11.1 (8.9)	8.5 (9.1)	-16.2*** (5.5)	-16.1*** (5.9)	12.5 (8.1)	10.8 (8.1)	-13.0* (7.6)	-13.4* (7.9)
Treat X Belief Terc = 2	6.9 (10.3)	9.2 (11.4)	10.0*** (3.6)	12.4*** (3.9)	-0.3 (13.8)	-3.6 (14.1)	-9.4 (6.7)	-10.3 (9.0)
Treat X Belief Terc = 3	25.3** (10.8)	23.1** (10.2)	-0.9 (3.7)	-1.9 (4.1)	23.3** (9.3)	24.0*** (9.1)	2.6 (3.6)	2.5 (4.3)
Belief Terc = 2	-3.1 (10.7)	-5.7 (11.3)	-20.1*** (5.0)	-21.2*** (5.4)	18.7 (13.1)	23.2* (13.3)	-5.2 (9.4)	-5.2 (11.3)
Belief Terc = 3	-3.1 (10.7)	-3.3 (10.4)	-14.1** (5.8)	-13.0* (6.7)	2.3 (9.1)	2.6 (8.8)	-14.5* (7.5)	-14.0* (8.1)
Constant	18.9*** (6.5)		21.4*** (4.9)		12.5** (5.9)		20.0*** (6.9)	
Controls		Y		Y		Y		Y
Obs.	244	244	66	66	244	244	66	66
R ²	0.04	0.08	0.22	0.33	0.04	0.10	0.12	0.25

Notes: Sample restricted to observations that did not explicitly state they learned about the information treatment in the follow-up survey. Table plots estimates from separate regressions of the dependent variable indicated above each column onto the indicated row coefficients. Panel a restricts the sample to females while panel b restricts the sample to males. Each estimation includes coefficients for terciles of baseline (pre-information) beliefs about peer negotiation rates, with the specific rates indicated above each column. Controls include major fixed effects. (* p<0.1, ** p<0.05, *** p<0.01)

Table A8: Treatment Effects on Perceptions of Gender

	No. of Gendered Statements Agreed With (0-3)		Belief: % Agree Females who Nego. are Aggressive	
	Males	Females	Males	Females
Treated (0/1)	-0.10 (0.09)	0.04 (0.06)	1.68 (2.45)	0.18 (2.56)
Constant	0.41*** (0.08)	0.17*** (0.05)	25.64*** (1.93)	36.73*** (2.06)
Obs.	304	359	304	359
R^2	0.005	0.001	0.001	0.000

Notes: Table displays regression coefficients from separate regressions of the indicated dependent variable onto a treatment dummy and a constant. Columns 1 and 3 restrict the sample to males while columns 2 and 4 is restricted to females. “Num. gendered statements agree (0-3)” is the number of gendered statements the respondent agreed with, ranging from 0 to 3. The statements were: (1) Women who negotiate for a higher salary or job benefits are too aggressive (2) A man’s job is to earn money while a woman’s job is to look after the home and family (3) It is more important for a wife to help her husband’s career than to have one herself. “Belief: prop. agree fem. who nego are aggressive” is the respondent’s belief about the share (from 0-100) of people who would agree with the statement: “Women who negotiate for a higher salary or job benefits are too aggressive.” Robust standard errors in parenthesis. (* p<0.1, ** p<0.05, *** p<0.01)