Online Appendix

A Theoretical Appendix

A.1 Extensions

As discussed during the presentation of our model, our framework extends to more realistic settings. In this section, we present two remarks describing these extensions.

Remark 2. *We have described a game in which all negotiations occur simultaneously, and an employer's acceptance of an employee's counteroffer does not oblige the employee to matriculate at that employer. More realistically, job offers are negotiated sequentially, and negotiating plausibly commits the employee to accepting that job, i.e., the employee may need to declare, "if you accept my counteroffer, I'll sign on the dotted line right now," in order for the employer to take the counteroffer seriously.*

The solution to our simultaneous-negotiation game is equivalent to that of a sequential-negotiation game in which the employee selects an order in which to negotiate with the employers, and she is matched to the first employer that either accepts her counteroffer, or to whom she makes no counteroffer (i.e., she accepts the employer's initial offer).

Remark 3. *We have assumed that a failed negotiation results in offer rescission. An alternative assumption is that that making a counteroffer exposes an employee to a distribution of negative consequences, reflecting frictions in the negotiation process. Specifically, a failed negotiation may result in the initial offer still being available to the employee, but some of the match surplus may be destroyed due to a deterioration of the relationship. Additionally, a negotiation (whether successful or not) may result in costly delays in onboarding. Our results are qualitatively similar if we embed these possibilities as follows: rejected counteroffers result in the offer either being withdrawn or having the offer remain constant and the match value lowered to w_i, each with positive probability; "accepted" offers are only available to the employee probabilistically, reflecting the time cost of negotiating.*

A.2 Proofs and an Additional Theoretical Result

Proof of Proposition 1:

Proof. It suffices to consider only employers with match values greater than the employee's outside option, so without loss of generality, let $v_1 > v_2 > ... > v_N > 0$. Because the employee's match value is lower at employer N than at all other employers, the employee accepts a job at employer N only if, for each employer $i < N$, she was either rejected from i, or she did not make a counteroffer to employer i.

Consider the following algorithm.

Stage [1] Let n be the smallest-index employer such that either $\hat{\rho}_n = 1$ or $w_n = v_n$. If no such n exists, then define $n = N+1$. If $n < N+1$, then $o_n^* = v_n - w_n$, and $o_i^* = 0$ for all $i > n$. If $n = 1$, then terminate the algorithm. Otherwise, go to Stage [2].

The remainder of this algorithm considers $i < n$. We inductively construct the optimal counteroffers via a double induction argument.

Stage [2] Let $\theta_{n-1} := v_n \mathbb{1}_{n<N+1}$.

Stage [2.1] If $\hat{\rho}_{n-1} v_{n-1} + (1-\hat{\rho}_{n-1})\theta_{n-1} \geqslant w_{n-1}$, then set $o_{n-1}^{[2.1]} = v_{n-1} - w_{n-1}$ and set $\theta_{n-2}^{[2.1]} = \hat{\rho}_{n-2} v_{n-2} + (1-\hat{\rho}_{n-2})\theta_{n-1}$. Otherwise, set $o_{n-1}^{[2.1]} = 0$ and set $\theta_{n-2}^{[2.1]} = w_{n-1}$. If $n-1 = 1$ then terminate the algorithm and let $o_i^{[2.1]} = o_i^*$ for all $i \leqslant n$. Otherwise, go to Stage [2.2].

Stage [2.k] Let $o_i^{[2.k]} = o_i^{[2.k-1]}$ for all $i \in \{n-(k-1), ..., n\}$. If $\hat{\rho}_{n-k} v_{n-k} + (1-\hat{\rho}_{n-k})\theta_{n-k}^{[2.k-1]} \geqslant w_{n-k}$, then set $o_{n-k}^{[2.k]} = v_{n-k} - w_{n-k}$ and set $\theta_{n-k-1}^{[2.k]} = \hat{\rho}_{n-k-1} v_{n-k-1} + (1-\hat{\rho}_{n-k-1})\theta_{n-k}^{[2.k-1]}$, moreover, if $n-k = 1$ then terminate the algorithm and let $o_i^{[2.k]} = o_i^*$ for all $i \leqslant n$ and if $n-k > 1$ then go to Stage [2.k+1]. If $\hat{\rho}_{n-k} v_{n-k} + (1-\hat{\rho}_{n-k})\theta_{n-k}^{[2.k-1]} < w_{n-k}$, go to Stage [3].

Stage [m] Let $K^{[m]}$ be the final employer considered in Stage $[m-1]$. Let $o_{K^{[m]}}^{[m.1]} = 0$, let $o_i^{[m.1]} = 0$ for all $i > K^{[m]}$ such that $v_i \leqslant w_{K^{[m]}}$ or $\hat{\rho}_i = 0$, and let $o_j^{[m.1]} = v_j - w_j$ for all other employers $j > K^{[m]}$. Let $L^{[m]}$ be the set of employers such that for each $\ell \in L^{[m]}$, $w_{K^{[m]}} < v_\ell < v_{K^{[m]}}$ and $\hat{\rho}_\ell > 0$. Set $\theta_{K^{[m]}-1}^{[m.1]} = \sum_{j=1}^{|L^{[m]}|} \left(\prod_{\ell=1}^{j-1} (1-\hat{\rho}_{k+\ell}) \right) \hat{\rho}_{k+j} v_{k+j} + \left(\prod_{\ell=1}^{|L^{[m]}|} (1-\hat{\rho}_{k+\ell}) \right) w_{K^{[m]}}$.

Stage $[m.1\ ]$ If $K^{[m]}-1=0$ then terminate the algorithm and let $o_i^{[m.1]}=o_i^*$ for all $i\leqslant n$. Otherwise, if $\hat{\rho}_{K^{[m]}-1}v_{K^{[m]}-1}+(1-\hat{\rho}_{K^{[m]}-1})\theta^{[m.1]}_{K^{[m]}-1}\geqslant w_{K^{[m]}-1}$, then set $o^{m.1}_{K^{[m]}-1}=v_{K^{[m]}-1}-w_{K^{[m]}-1}$ and set $\theta^{m.1}_{K^{[m]}-2}=\hat{\rho}_{K^{[m]}-2}v_{K^{[m]}-2}+(1-\hat{\rho}_{K^{[m]}-2})\theta^{m.1}_{K^{[m]}-1}$, then go to Stage $[m.2]$. If $\hat{\rho}_{K^{[m]}-1}v_{K^{[m]}-1}+(1-\hat{\rho}_{K^{[m]}-1})\theta^{[m.1-1]}_{K^{[m]}-1}<w_{K^{[m]}-1}$, go to Stage $[m+1]$.

Stage $[m.k\ ]$ Let $o_i^{[m.k]}=o_i^{[m.k-1]}$ for all $i\in\{K^{[m]}-(k-1),...,n\}$. If $\hat{\rho}_{K^{[m]}-k}v_{K^{[m]}-k}+(1-\hat{\rho}_{K^{[m]}-k})\theta^{[m.k-1]}_{K^{[m]}-k}\geqslant w_{K^{[m]}-k}$, then set $o^{[m.k]}_{K^{[m]}-k}=v_{K^{[m]}-k}-w_{K^{[m]}-k}$ and set $\theta^{[m.k]}_{K^{[m]}-k-1}=\hat{\rho}_{K^{[m]}-k-1}v_{K^{[m]}-k-1}+(1-\hat{\rho}_{K^{[m]}-k-1})\theta^{[m.k-1]}_{K^{[m]}-k}$, moreover, if $K^{[m]}-k=0$ then terminate the algorithm and let $o_i^{[m.k]}=o_i^*$ for all $i\leqslant n$ and if $K^{[m]}-k>0$ then go to Stage $[m.k+1]$. If $\hat{\rho}_{K^{[m]}-k}v_{K^{[m]}-k}+(1-\hat{\rho}_{K^{[m]}-k})\theta^{[m.k-1]}_{K^{[m]}-k}<w_{K^{[m]}-k}$, go to Stage $[m+1]$.

We claim that the algorithm terminates, does so at an optimal portfolio of counteroffers, and that this optimal portfolio is generically unique.

To see that the algorithm terminates, suppose for contradiction that it does not. This means that at no stage $[m]$, $m>2$ does the algorithm reach a substage $[m.k]$ such that $K^{[m]}-k=0$ and $\hat{\rho}_{K^{[m]}-k}v_{K^{[m]}-k}+(1-\hat{\rho}_{K^{[m]}-k})\theta^{[m.k-1]}_{K^{[m]}-k}\geqslant w_{K^{[m]}-k}$. However, by construction, $(K^{[m]})_{m>2}$ is a strictly decreasing sequence of integers, i.e., a lower index employer is the first employer referenced at the beginning of each Stage. Therefore, there is some stage $[m^*]$, $m\leqslant n+1$ such that $K^{[m^*]}=1$. Then the algorithm immediately terminates, i.e., by Stage $[m^*.1]$. This contradicts that the algorithm never terminates, which completes the desired argument.

We now claim that the outcome of the algorithm o^* is optimal, and uniquely so generically.

To see this, let o' be an optimal counteroffer portfolio. First note that, defining n as in Stage $[1]$, it must be the case that $o'_n=v_n-w_n$ and $o'_i=0$ for all $i>n$ in any optimal counteroffer portfolio. Therefore, $o'_j=o^*_j$ for all $j\geqslant n$.

Consider any Stage $[m-1]$, $m-1\geqslant 2$. We claim that if the algorithm terminates at the conclusion of Stage $[m-1]$ then o^* is generically uniquely optimal, and if it does not terminate, then $(o^{[m.1]}_{K^{[m]}},o^{[m.1]}_{K^{[m]+1}},...,o^{[m.1]}_N)$ is generically the uniquely optimal portfolio of counteroffers supposing the employee's initial portfolio comprised of only offers from employers $(K^{[m]},K^{[m]+1},...,N)$. Our argument is by induction on the index of Stages.

Base case, $m-1=2$**:** If the algorithm terminates in Stage $[2]$ then, by construction, the employee optimally negotiates with all employers $j<n-1$, and the employee has strict in-

centives to negotiate with all such employers if $\hat{\rho}_{n-k}v_{n-k}+(1-\hat{\rho}_{n-k})\theta_{n-k}^{[2.k-1]}>w_{n-k}$ in all Stages $[2.k]$, $k>1$ which is generically satisfied given that $\hat{\rho}_{n-k}v_{n-k}+(1-\hat{\rho}_{n-k})\theta_{n-k}^{[2.k-1]}\geqslant w_{n-k}$ is satisfied (note that by a similar argument, the employee's generically optimal choice is defined for employer $n-1$). If the algorithm does not terminate in Stage $[2]$ then there is some final substage $[2.\hat{k}]$, in which the employee considers negotiation with employer $K^{[3]}$. At the conclusion of stage $[2.\hat{k}]$, the (generically unique) optimal negotiation decision has been determined, by the most recent argument, for all employers $i>K^{[3]}$ under the inductive hypothesis that the employee negotiates with all employers $i\geqslant K^{[3]}$, however, this assumption has been violated by the fact that the transition to Stage $[3]$ implies that the employer does not optimally negotiate with employer $K^{[3]}$ given an expected value $\theta_{K^{[3]}}^{[2.\hat{k}]}$ conditional on being rejected from employer $j\leqslant K^{[3]}$ because

$$\hat{\rho}_{K^{[3]}}v_{K^{[3]}}+(1-\hat{\rho}_{K^{[3]}})\theta_{K^{[3]}}^{[2.\hat{k}]}<w_{K^{[3]}}. \tag{A.1}$$

Assume, subject to later verification, that in any optimal vector of counteroffers given a portfolio comprised only of offers from employers $(K^{[3]},K^{[3]+1},...,N)$, that the employer elects not to negotiate with employer $K^{[3]}$. Then, given this portfolio, the employee achieves a payoff of at least $w_{K^{[3]}}$ and must negotiate with employer $i>K^{[3]}$ if and only if $v_i\geqslant w_{K^{[3]}}$, $\hat{\rho}_i>0$, and $w_i<v_i$, where by genericity we assume that there are no employers i such that $v_i=w_{K^{[3]}}$. Doing so implies that, the employee matches with employer $K^{[3]}$ if and only if she is rejected from all such employers i. This mechanically lowers the employee's continuation value, conditional on being rejected from all such employers i and $K^{[3]}$ to a value lower than $\theta_{K^{[3]}}^{[2.\hat{k}]}$. Therefore, our assumption subject to verification, and the proof of the Base Case, is shown to be correct if and only if the employee optimally elects not to negotiate with employer $K^{[3]}$ given this lower continuation value. But this is satisfied, because if Equation (A.1) is satisfied, it is also satisfied if instead the employee's continuation value $\theta_{K^{[3]}}^{[2.\hat{k}]}$ is reduced.

Induction case, $m-1>2$**:** Our argument thus far implies that $(o_{K^{[m]}}^{[m.1]},o_{K^{[m]+1}}^{[m.1]},...,o_N^{[m.1]})$ is generically the uniquely optimal portfolio of counteroffers supposing the employee's initial portfolio comprised of only offers from employers $(K^{[m]},K^{[m]+1},...,N)$. This is clearly true if $K^{[m]}-1=0$, i.e $K^{[m]}=1$. Otherwise, the construction of $\theta_{K^{[m]}-1}^{[m.1]}$ gives

the employee's optimal continuation value given that she is rejected from all employers j such that $j < K^{[m]}$, or $v_j \geqslant w_{K^{[3]}}$, $\hat{\rho}_j > 0$, and $w_j < v_i$. The remainder of the argument follows similar logic as that of the Base Case.

We have already shown that the algorithm terminates at some Stage $[m]$, $m \leqslant n+1$. When it does so, by construction, it specifies a value o_i^* for each employer i. By the inductive argument presented above, it must be the case that o_i^* is generically the unique negotiation vector.

□

Proof of Proposition 2

Proof of Part 1: Consider an employer ℓ such that $v_\ell > v_j$. We argue that the employee makes a counteroffer to employer ℓ. To this end, it is either the case that $w_\ell > w_j$ or $w_\ell \leqslant w_j$:

- Suppose $w_\ell > w_j$. Then it must be the case that $o_\ell^* > 0$, as desired, or else j would not be the highest-initial-offer employer that the employee does not make a counteroffer to.
- Suppose $w_\ell \leqslant w_j$, and further suppose for contradiction that $o_\ell^* = 0$. First, note that there is a strictly positive probability that the employee is matched with employer j; if not, then because we assume $\hat{\rho}_i \in (0,1)$ for all i, there exists some employer i' with $v_{i'} = w_{i'}$ such that $v_{i'} > v_j$. But combining the inequality and equality in the previous sentence implies that $w_{i'} > w_j$, which contradicts the ongoing assumption that j is the highest-initial-offer employer for which $o_j^* = 0$. Given the strictly positive probability of matching with employer j, we claim that $w_\ell \leqslant w_j$ implies that the employee would be better off making a counteroffer to employer ℓ. This claim is clear if $w_\ell < w_j$, and if $w_\ell = w_j$, then the maintained assumption that $v_\ell > v_j$ implies that $v_\ell - w_\ell > 0$ meaning that there are strictly positive gains from negotiating with employer ℓ. Therefore, $o_\ell^* = 0$ contradicts the assumed optimality of the counteroffers.

In the two exhaustive cases above, we arrive at a contradiction if $o_j^* = 0$, thus completing the argument. □

Proof of Part 2: This follows from inspection of the algorithm provided in the proof of Proposition 1—at each stage, the employee negotiates with the indicated employer i if and only if the stated condition is satisfied. □

Proof of Proposition 3

Proof of Parts 1 and 2. The first two parts of this proposition follow straightforwardly. To see that a partial equilibrium exists, note that we have already established in Proposition 1 that for any portfolio of offers and beliefs $\{w_i, v_i, \hat{\rho}_i\}_{i \in N}$ there exists an optimal portfolio of counteroffers for the selected employee.[62] Therefore, any given strategy profile of employers generates a distribution over payoffs for each employer; it follows from established equilibrium-existence results (see, e.g., Reny, 2020, Theorem 3) that the truncated game which "automates" the portfolio bargaining problem and directly yields each employer the expected payoff given the selected strategies by all employers contains a Nash equilibrium.[63] Our game, which is a concatenation of the employer-side decisions followed by the employee-side decisions, therefore always has a partial equilibrium, as rational expectations are not required. To see that a general equilibrium exists if the support of F contains only the "correct" beliefs $\hat{\rho}$, we claim that each employer i's payoff is unaffected by the choice of ρ_i. This follows for two reasons: first, as can be seen in Part 2 of Proposition 2, each employee type's decision to make a counteroffer is affected by the belief $\hat{\rho}_i$ but not by the choice ρ_i. Second, the payoff of any employer is zero if the employee negotiates with it, as either the employee receives the entire match value, or the employer fails to hire the employee. Therefore, the same argument ensuring the existence of a partial equilibrium in any labor market implies that, for a distribution F with a single belief vector, there is a general equilibrium of the employer-side truncated game in which each employer selects $\rho_i = \hat{\rho}_i$. □

Proof of Part 3.

"if" direction Fix $(\mathcal{N}, \Theta)$ and let $\hat{\rho}$ be a belief vector such that $\hat{\rho}_i \in \{0,1\}$ for all i.

First consider an employer $j \in \mathcal{N}$ such that $\hat{\rho}_j = 0$, which implies that no employee type makes a counteroffer to j in general equilibrium. Because $w_j^g < v_j$ means by definition that j negotiates with no group-g employees, the claim is demonstrated if we can show that for any $g \in \{A, B\}$, $w_j^g < v_j$ is supported in general equilibrium in any labor market $(\mathcal{N}, \Theta, F, \{v_i\}_{i \in \mathcal{N}})$ with only belief vector $\hat{\rho}$ in the support of F. Holding fixed the strategies of other employers, consider two exhaustive cases. First, suppose that almost no employee type is matched to j for any $w_j^g < v_j$. By the full support assumption on F, and by Part 2 of Proposition 2, this implies

[62] Proposition 1 assumed that the outside option $\theta = 0$ but this was merely a normalization that does not change the applicability to the current equilibrium setting.

[63] A similar "automation" argument used to show existence of equilibrium is presented in Gentile Passaro et al. (2024).

that $v_j \leqslant \underline{\theta}$, so that any $w_j^g < v_j$ can be supported in general equilibrium. Second, suppose that at least one employee type is matched to j for some $w_j^g < v_j$. Then $v_j > \underline{\theta}$. Consider $\tilde{w}_j^g = \underline{\theta} + \epsilon$, where $\epsilon > 0$. For ϵ sufficiently small, the full support assumption on F and Part 2 of Proposition 2 imply that with positive probability, an employee type r will be selected such that: 1) $\theta^r < \tilde{w}_j^g$, 2) $g^r = g$, and 3) $N^r = \{j\}$. Because such an employee type r optimally does not make a counteroffer to j, r will be employed at employer j at wage $\tilde{w}_j^g < v_j$, and because such a type is selected with strictly positive probability, j earns positive expected profit by selecting initial wage offer $\tilde{w}_j^g$. Thus, j cannot set $w_j^g = v_j$ in general equilibrium, as this yields zero profit (from employees of group g). This completes the argument in the case that $\hat{\rho}_j = 0$.

Next, consider an employer $\ell \in \mathcal{N}$ such that $\hat{\rho}_\ell = 1$. For each employee type r of group g who interacts with subset of employers N including ℓ, the employee will match to employer ℓ if and only if $v_\ell > w_i^g$ for all employers $i \in N$ such that $\hat{\rho}_i = 0$ and $v_\ell > v_i$ for all employers $i \in N \setminus \{\ell\}$ such that $\hat{\rho}_i = 1$, regardless of w_ℓ^g. Therefore, for any labor market $(\mathcal{N}, \Theta, F, \{v_i\}_{i \in \mathcal{N}})$ with only belief vector $\hat{\rho}$ in the support of F, there is a general equilibrium in which employer ℓ sets $w_\ell^g = v_\ell$ for all $g \in \{A, B\}$. In such a general equilibrium, the negotiation probability at employer j is 1 by fiat, as desired.

"only if" direction The following argument demonstrates that $\hat{\rho}$ is not robustly self-enforcing if $\hat{\rho}_j \in (0,1)$ for some $j \in \mathcal{N}$.[64] Suppose for contradiction that given $(\mathcal{N}, \Theta)$, there exists a robustly self-enforcing belief vector $\hat{\rho}$ such that $\hat{\rho}_j \in (0,1)$ for some $j \in \mathcal{N}$. Consider a labor market $(\mathcal{N}, \Theta, F, \{v_i\}_{i \in \mathcal{N}})$ in which $v_j < \underline{\theta}$. Then no employee type make counteroffers, i.e., each employee optimally sets $o_i^* = 0$. Therefore, in order for the employer to negotiate with the proper probability $\hat{\rho}_i$, it must be that either $w_i^A = v_j$ or $w_i^B = v_j$, but not both: if $w_i^A < v_j$ and $w_i^B < v_j$, then $o_i^* = 0$ for all employee types implies that the negotiation probability at employer i is equal to $0 < \hat{\rho}_i$, and if $w_i^A = v_j = w_i^B$, then the negotiation probability at employer i is, by definition, equal to $1 > \hat{\rho}_i$. Similarly, the negotiation probability at employer i will equal $\hat{\rho}_i$ only if the probability of an employee being a member of some group $g \in \{A, B\}$ is equal to $\hat{\rho}_i$, and $w_i^g = v_i$ and $w_i^{-g} < v_i$ where $-g \neq g$. As the probability of an employee being a member of some group $g \in \{A, B\}$ being equal to $\hat{\rho}_i$ does not hold across all possible distributions F such that $\hat{\rho}$ is the unique belief vector in its support (indeed it fails to hold generically), $\hat{\rho}$ is not robustly self enforcing, leading to a contradiction. □

[64] We note the existence of, but do not present, more complicated arguments that yield the same conclusion.

Proof of Proposition 4

The argument behind this result follows from a similar one to that presented in the Proof of Proposition 5 below, and is therefore omitted.

Proposition 5. *Consider an infinite sequence of labor markets* $\{(\mathcal{N},\Theta,F^h,\{v_i\}_{i\in\mathcal{N}})\}_{h=1,2,\ldots}$ *where for each* h*, there is a single belief vector* $\hat{\rho}^h$ *in the support of* F^h *and* $\hat{\rho}^h \to \vec{1}$ *in* h*. For any* $\epsilon > 0$*, there exists some* h^* *such that for all* $h > h^*$ *and in any general equilibrium* $e^h \in E^h$*, the efficiency and employee surplus is no less than* $1-\epsilon$*, while the wage gap between groups is no more than* ϵ*.*

Proof. Let $\hat{\rho}$ be the only belief vector in the support of the labor market. We show that for any $\epsilon > 0$ there exists $\delta > 0$ such that if $\hat{\rho}_i > 1-\delta$ for all $i \in \mathcal{N}$ for an employee type r with $V^{N^r} > \theta$ (i.e., $V^{N^r} = v_{i^*}$ for some $i^* \in N^r$) either $w_{i^*}^{g^r} > V^{N^r} - \epsilon$ or the employee optimally negotiates with employer i^* in any general equilibrium. In words the previous sentence states that an employee with beliefs that each employer entertains counteroffers with probability of at least $1-\delta$ and has a match value with at least one employer that exceeds her outside option will either negotiate with the employer with the highest match value, or must receive an initial offer that yields almost all of the match value from this employer. Recalling that general equilibrium imposes $\hat{\rho}_i^r = \rho_i$ for all $i \in \mathcal{N}$ and all employee types r, showing this statement suffices to prove the desired result, because for small enough ϵ either the employee negotiates with the maximum-match-value employer—in which case she is matched there with probability $\hat{\rho}_i > 1-\delta$—or she does not negotiate with this employer but accepts its offer anyway because its offer is higher than the match value at any other employer: $w_{i^*}^{g^r} \geqslant v_{i^*} - \epsilon > v_j$ for all $j \neq i^*$, where the second inequality holds for small enough ϵ because $v_i \neq v_j$ for all $i \in \mathcal{N}$.

Fix $\epsilon > 0$ and let $v_{i^*} - \epsilon > w_{i^*}^{g^r}$ for all $j \neq i^*$. In the event that $\hat{\rho}_{i^*}^r = 1$, the desired claim follows straightforwardly. In the case that $\hat{\rho}_{i^*}^r \neq 1$, the desired claim follows from Part 2 of Proposition 2, which finds that an employee r optimally negotiates with employer i^* if and only if $\theta_{i^*} \geqslant \frac{w_{i^*}^{g^r} - \hat{\rho}_{i^*}^r v_{i^*}}{1-\hat{\rho}_{i^*}^r}$. By construction, the left-hand side of this inequality is bounded away from $-\infty$ because $\theta_{i^*} \geqslant \underline{\theta} > -\infty$, while the right-hand side of this inequality approaches $-\infty < \underline{\theta}$ as $\hat{\rho}_{i^*}^r \to 1$ because $v_{i^*} > w_{i^*}^{g^r}$. Therefore, for sufficiently small δ, the employee either negotiates with i^* or is initially offered approximately the entire match value at i^* as desired. □

A.3 Employee May Optimally Negotiate any Subset of Offers

As discussed in Section 5.2, negotiation with any subset of employers in an employee's portfolio can be rationalized. The following example demonstrates this, and in doing so shows that finding an optimal portfolio of counteroffers may involve non-trivial considerations, as it is insufficient to "locally" optimize—negotiating with an employer has consequences on the marginal risk courted by the rest of the portfolio.

Example 1. *Let* $N=\{1,2\}$ *and let* $v_1>v_2$*. To rule out uninteresting complexities, let* $w_1<v_1$ *and* $w_2<v_2$*, and let* $\hat{\rho}_1,\hat{\rho}_2\in(0,1)$*. Given Remark 1, there are four potentially optimal portfolios of counteroffers to consider:* $o_1^*=v_1-w_1$ *and* $o_2^*=v_2-w_2$ *(the employee negotiates with both employers),* $o_1^*=v_1-w_1$ *and* $o_2^*=0$ *(the employee negotiates only with employer 1),* $o_1^*=0$ *and* $o_2^*=v_2-w_2$ *(the employee negotiates only with employer 2) and* $o_1^*=o_2^*=0$ *(the employee negotiates either neither employer).*

Option 1: $o_1^*=v_1-w_1$, $o_2^*=v_2-w_2$**.** *Because* $v_1>v_2$*, the employee will match with employer 1 if her offer is not rejected. Therefore, her expected payoff is* $\hat{\rho}_1v_1+(1-\hat{\rho}_1)\hat{\rho}_2v_2$.

Option 2: $o_1^*=v_1-w_1$, $o_2^*=0$**.** *The employee's expected payoff is* $\hat{\rho}_1v_1+(1-\hat{\rho}_1)w_2$.

Option 3: $o_1^*=0$, $o_2^*=v_2-w_2$**.** *By the assumption that the employee will never negotiate with an employer if she has zero probability of matching with that employer, it must be the case that* $v_2>w_1$*. Therefore, the employee's expected payoff is* $\hat{\rho}_2v_2+(1-\hat{\rho}_2)w_1$.

Option 4: $o_1^*=o_2^*=0$**.** *By the assumption that the employee will never negotiate with an employer if she has zero probability of matching with that employer, it must be the case that* $w_1>w_2$*. Otherwise, if* $w_2\geqslant w_1$*, because* $\hat{\rho}_1>0$*, the employee's expected payoff from setting* $o_1^*=o_2^*=0$ *is* w_2 *while the employee's expected payoff from setting* $o_1^*=v_1-w_1$, $o_2^*=0$ *is* $\hat{\rho}_1v_1+(1-\hat{\rho}_1)w_2>w_2$*, where the inequality follows because* $v_1>v_2$ *by the assumption of this example, and* $w_2\leqslant v_2$ *by our ongoing assumption that initial offers are no greater than the match value.*

Because $w_1>w_2$ *and* $o_1^*=0$*, then the employee will optimally negotiate with employer 2 if* $v_2>w_1$*. Therefore, this option is optimal only if* $w_1\geqslant v_2>w_2$*, and the employee's expected payoff is* w_1.

Holding fixed v_1, v_2, w_1, w_2, it is clear that Option 1 is optimal if the risk of negotiating with both employers is sufficiently low. Specifically, there exists $\rho^* < 1$ such that if $\hat{\rho}_1, \hat{\rho}_2 > \rho^*$, the employee will optimally negotiate with both employers.

Similarly, holding fixed v_1, v_2, w_1, w_2 such that $w_1 \geqslant v_2 > w_2$ (recall that these inequalities are necessary for the optimality of option 4), it is clear that Option 4 is optimal if the risk of negotiating with both employers is sufficiently high.

Option 2 is optimal if the risk of negotiating with employer 1 is low and the risk of negotiating with employer 2 is high. Specifically, suppose the employee negotiates with employer 1. Because $v_1 > v_2 \geqslant w_2$, the employee will be matched to employer 1 with probability $\hat{\rho}_1$, i.e., if her counteroffer to employer 1 is accepted. With the complementary probability, her offer is rejected. In the event she is rejected by employer 1, she earns $\hat{\rho}_2 v_2$ in expectation by negotiating with employer 2, and w_2 for sure if she does not negotiate. Therefore, for $\hat{\rho}_1$ sufficiently small, and $w_2 > \hat{\rho}_2 v_2$, option 2 is optimal, i.e., the employee will "risk it" with employer 1 and "play it safe" with employer 2.

Option 3 is similar to option 2, except that employee uses employer 1's offer as a safety, and negotiates instead with employer 2. Recall our previous argument that this option is optimal only if $v_2 > w_1$. Therefore, the employee will be matched to employer 2 with probability $\hat{\rho}_2$, and with the complementary probability, her offer is rejected. In the event she is rejected by employer 2, she earns $\hat{\rho}_1 v_1$ in expectation by negotiating with employer 1, and w_1 for sure if she does not negotiate. It is tempting, mirroring the argument surrounding the optimality of option 2, to reason that option 3 is optimal if $\hat{\rho}_2$ is sufficiently small, $v_2 > w_1$, and $w_1 > \hat{\rho}_1 v_1$.

However, an additional consideration is necessary here because employer 1 is the highest-value employer that the employee will be matched to following a successful negotiation. In other words, the decision of whether or not to negotiate with employer 1 cannot be determined only by conditioning on rejection from employer 2 and "locally" optimizing. To "globally" optimize her portfolio, the employee must consider negotiating with employer 1.[65] *If she does so, she would not negotiate with employer 2 only if $w_2 \geqslant \hat{\rho}_2 v_2$. Therefore, if $w_2 \geqslant \hat{\rho}_2 v_2$, she optimally selects Option 3 if and only if her expected utility from Option 3 exceeds that from Option 2: $\hat{\rho}_2 v_2 + (1 - \hat{\rho}_2) w_1 \geqslant \hat{\rho}_1 v_1 + (1 - \hat{\rho}_1) w_2$. Similarly, conditional on negotiating with*

[65] Note that the maintained assumptions that $\hat{\rho}_1, \hat{\rho}_2 > 0$, $v_1 - w_1, v_2 - w_2 > 0$ and $v_1 > v_2$, mean that the employee cannot optimally negotiate with neither employer if $v_2 > w_1$; she would be better off negotiating with only employer 1. Because we have previously argued that $v_2 > w_1$ is a necessary condition for the optimality of Option 3, it must be that if Option 3 is potentially optimal, then Option 4 is not.

employer 1, she would negotiate with employer 2 if $w_2 < \hat{\rho}_2 v_2$*. Therefore, if* $w_2 < \hat{\rho}_2 v_2$*, she optimally selects Option 3 if and only if her expected utility from Option 3 exceeds that from Option 1:* $\hat{\rho}_2 v_2 + (1-\hat{\rho}_2) w_1 \geqslant \hat{\rho}_1 v_1 + (1-\hat{\rho}_1)\hat{\rho}_2 v_2$.

A.4 The Proposed Bargaining Model Rationalizes Experimental Finding on Initial Misperceptions

Evidence from our extensive-margin experimental treatment arm shows that employees who are initially pessimistic about the viability of bargaining experience the largest gains from receiving accurate information (see Figure 2). This short section rationalizes this finding in the context of our model.

Consider a labor market $(\mathcal{N},\Theta,F,\{v_i\}_{i\in\mathcal{N}})$ in partial equilibrium,[66] and consider two employee types r and r'. We assume that the two employee types are identical in every way, except that they have different beliefs, r has accurate beliefs while r' is pessimistic about the efficacy of bargaining at all employers: $N^r = N^{r'}$, $\theta^r = \theta^{r'}$, $g^r = g^{r'}$, but $\hat{\rho}^r = \rho$ while $\hat{\rho}^{r'} < \rho$.

It follows straightforwardly that employee r's expected payoff (weakly) exceeds that of employee r', as r optimizes her portfolio using accurate information, and r' optimizes using inaccurate information. This observation leads to the following conclusion about how eliminating misperceptions about extensive-margin uncertainty reduces inequality.

Claim 3. *Suppose an information treatment (for example, one similar to that in our experiment) induces correct beliefs about the viability of negotiation in all employees. Then an employee with misperception in her prior belief will benefit relative to an otherwise-identical employee with accurate prior beliefs.*

[66] Recalling Proposition 3, the labor market cannot be in general equilibrium if different employee types have different belief vectors, as this would violate rational expectations.

B Additional Implementation Details

In this Appendix, we provide additional details about the institutional context, research design, and implementation of the experiment.

B.1 Survey Recruitment

For the first wave, we recruited the survey sample from individuals who had registered an account with levels.fyi.[67] We sent email invitations to 131,830 individuals. Of these, 44,478 had registered with levels.fyi within one week prior to the invitations, while the remaining 87,352 had created an account within the past year. A total of 3,346 individuals began the baseline survey, but many were filtered out—that is, their surveys were terminated automatically for not meeting the qualifying conditions. After applying these filters, 2,355 respondents completed the baseline survey. We consider a baseline survey complete if the respondent reached the stage, near the end of the survey, at which they were randomly assigned to all treatment arms.

Figures B.1 and B.2 present flow charts of the baseline and follow-up surveys. At the start of the baseline survey, we applied screening criteria to recruit participants actively searching for a job in the United States. More precisely, to qualify, respondents had to either already have a job offer or, if not, satisfy additional conditions: they must have had at least one job interview in the past 30 days and report a positive probability of receiving an offer in the near future.

In response to participant feedback, we adjusted the timing and structure of incentive payments. For participants recruited between May 24 and July 17, 2023, we offered $25 for completing the baseline survey and an additional $25 for completing the follow-up survey. For those recruited between July 18 and July 25, 2023, the follow-up survey payment increased to $50. For participants recruited after July 25, 2023, the $50 payment was made only upon completion of the follow-up survey.

B.2 Further Details on the Coaching Treatment Arm

We begin by presenting a set of screenshots that provide useful context about the coaching service. Figure B.3 displays levels.fyi's frequently asked questions for the negotiation coaching service. Figure B.4 shows publicly available customer reviews from the website, and

[67] When registering an account, individuals are required to fill out a registration form, shown in Figure K.

Figure B.5 presents the company's landing page at the time of the experiment, which includes salary information across levels and companies. In Wave 2, we recruited participants through a button placed directly below the "Book Now" button on levels.fyi's coaching service page. A screenshot of this entry point is shown in Figure B.6.

As part of the coaching treatment arm, we offered a substantial subsidy for the coaching services. The exact terms of the offer were informed by feedback from the partner company and participants, resulting in adjustments during the early stages of data collection. Initially, we piloted a design with a discounted price of $600, payable only after participants completed the service. Based on feedback, we quickly abandoned this model in favor of a simpler structure with a discounted price of $450. Beginning on August 18, 2023, we further reduced the price to $250. We also took steps to ensure that participants who received the discounted offer and wanted to use it could do so easily and seamlessly. If a survey participant was randomly assigned to receive the discount and expressed interest in the service, they were redirected to a Calendly page to schedule their first session with a coach (Figure B.7). In addition, the partner company followed up by email a few days after survey completion, providing instructions on how to claim the offer.

B.3 Survey Timeline

We emailed invitations to participants to complete a follow-up survey six weeks after they finished the baseline survey. If they did not respond, we sent email reminders one and two weeks after the initial invitation. For participants who completed the follow-up survey but indicated they had not yet received a job offer, we informed them that it was too early to complete the survey and that we would follow up again soon. For this group, we sent a new invitation two weeks after their previous response, followed by a reminder one week later. If they still reported no job offer, we repeated this cycle once more—sending a new invitation two weeks later and a reminder one week after that. After three consecutive responses without a job offer, we ceased further follow-up. In Wave 2, we added a reminder method suggested by the partner organization: if a participant did not respond after the initial two email reminders, we sent an additional reminder via LinkedIn.

B.4 LinkedIn Data Collection

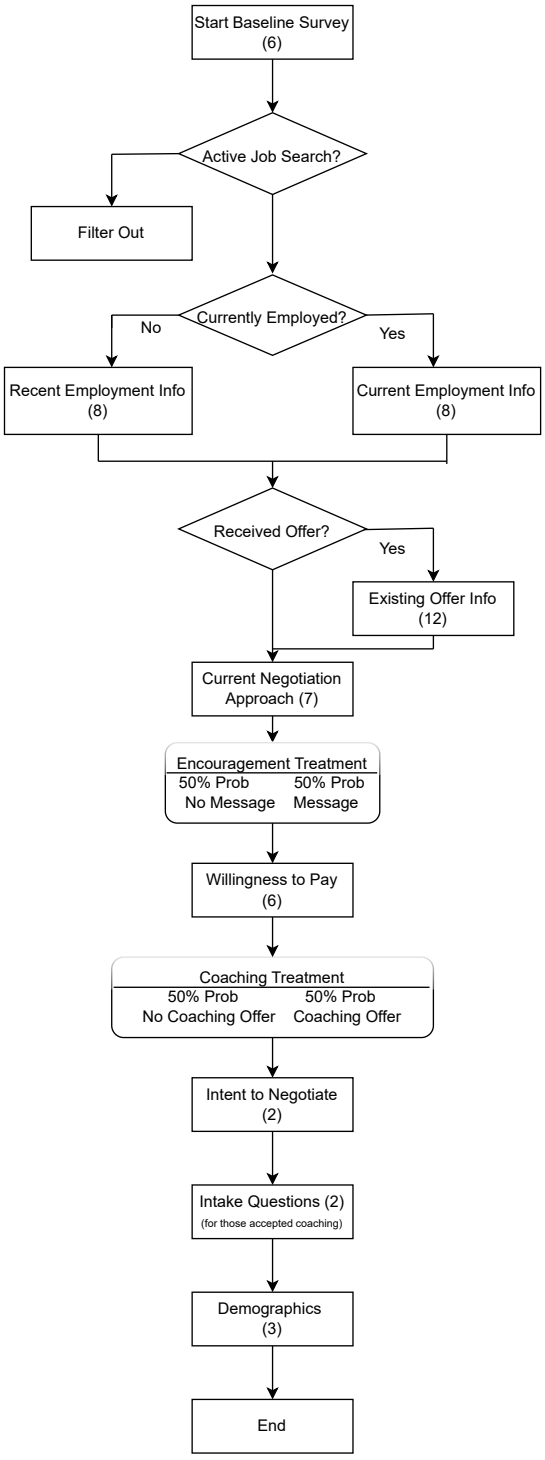
In Wave 1, we located participants' LinkedIn profiles using information they provided in the survey. By combining their first and last names, recent job titles, and educational backgrounds, we successfully matched 88% of participants to a valid LinkedIn profile. This high match rate aligns with anecdotal evidence that LinkedIn is widely used in the tech sector—employees may have strong incentives to maintain an up-to-date LinkedIn profile, as recruiters frequently use the platform to source candidates for new roles. We believe that most unmatched participants did not have a LinkedIn account; in a few cases, we were unable to disambiguate among multiple profiles due to common names at large employers. In Wave 2, participants were asked directly to provide their LinkedIn profile as a verification method, and 98% supplied a valid profile.

For each participant with a valid LinkedIn account, we collected the following data from their profiles. Shortly after they completed the baseline survey, we recorded information on their employment history (including companies, roles, and start dates), education, location, skills, and number of connections. This data serves two main purposes. First, it allows us to validate the consistency of the information provided in the survey and assess the truthfulness of participants' responses. Second, LinkedIn profiles often include details not captured in the survey—such as the name of the college attended—which can serve as a useful supplement to the survey data.

The LinkedIn data serves a third purpose. We collected a second round of LinkedIn data at least six months after each participant completed the baseline survey.[68] The primary goal was to measure whether participants changed jobs within the first six months following the baseline survey, as indicated by updates to their LinkedIn profiles. A key advantage of using LinkedIn to measure this outcome is that it allows us to observe job changes even for participants who did not complete the follow-up survey. However, a limitation of this approach is that the observed effects may reflect either real changes in job-switching behavior or reporting effects—that is, among those who do switch jobs, there may be treatment-induced differences in the likelihood of updating their LinkedIn profiles.

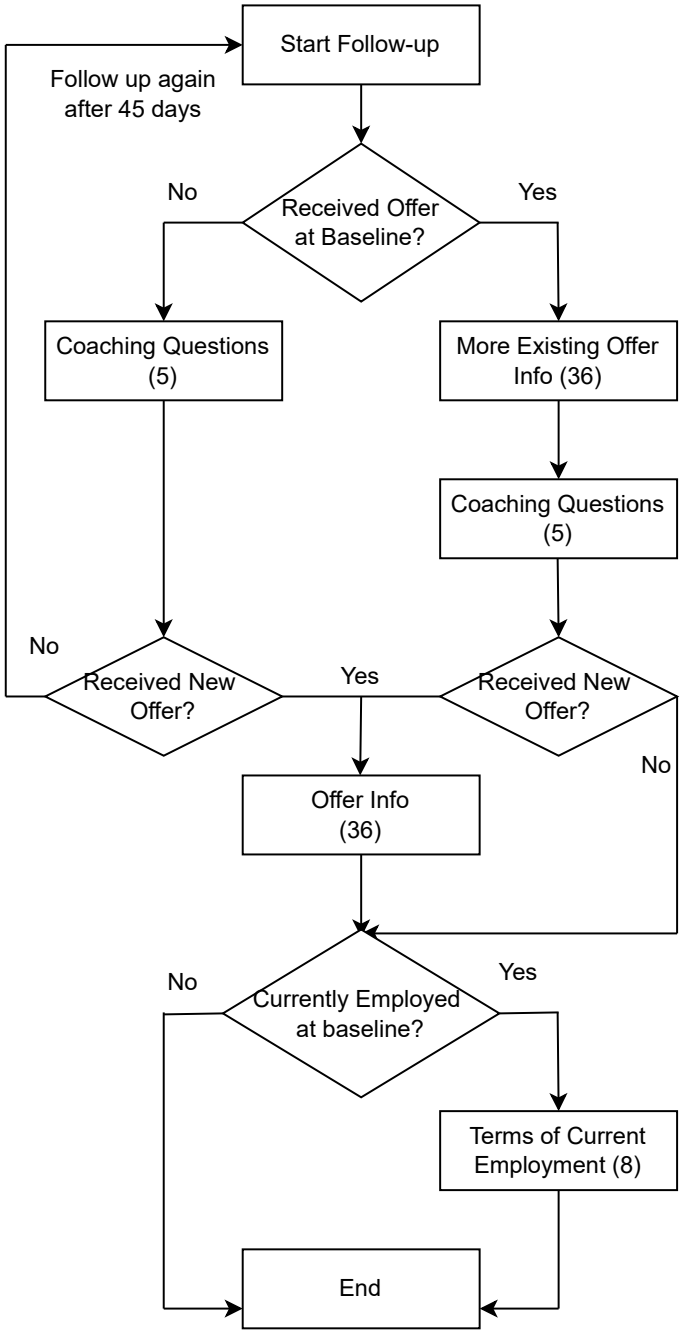
[68] Since the last data collection was done in May 2025, for a few participants who completed the baseline survey on December 2024 and January 2025, the second round of data collection occurred before the six-month mark.

Figure B.1: Survey Flowchart: Baseline Survey



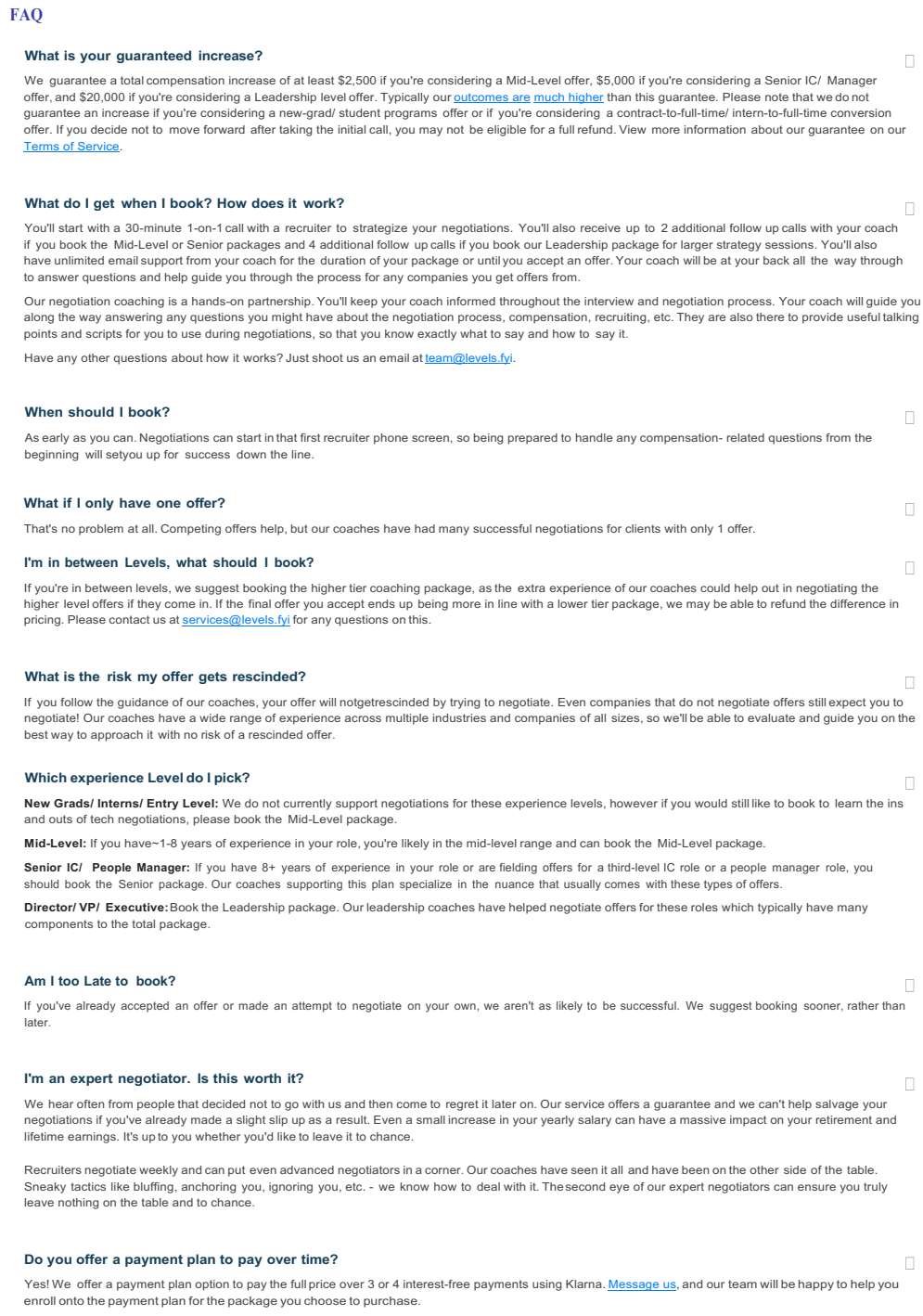
Notes: The numbers in parentheses correspond to the number of unique questions in the module.

Figure B.2: Survey Flowchart: Follow-up Survey



Notes: The numbers in parentheses correspond to the number of unique questions in the module.

Figure B.3: Levels.fyi's Frequently Asked Questions



FAQ

What is your guaranteed increase?

We guarantee a total compensation increase of at least $2,500 if you're considering a Mid-Level offer, $5,000 if you're considering a Senior IC/ Manager offer, and $20,000 if you're considering a Leadership level offer. Typically our outcomes are much higher than this guarantee. Please note that we do not guarantee an increase if you're considering a new-grad/ student programs offer or if you're considering a contract-to-full-time/ intern-to-full-time conversion offer. If you decide not to move forward after taking the initial call, you may not be eligible for a full refund. View more information about our guarantee on our Terms of Service.

What do I get when I book? How does it work?

You'll start with a 30-minute 1-on-1 call with a recruiter to strategize your negotiations. You'll also receive up to 2 additional follow up calls with your coach if you book the Mid-Level or Senior packages and 4 additional follow up calls if you book our Leadership package for larger strategy sessions. You'll also have unlimited email support from your coach for the duration of your package or until you accept an offer. Your coach will be at your back all the way through to answer questions and help guide you through the process for any companies you get offers from.

Our negotiation coaching is a hands-on partnership. You'll keep your coach informed throughout the interview and negotiation process. Your coach will guide you along the way answering any questions you might have about the negotiation process, compensation, recruiting, etc. They are also there to provide useful talking points and scripts for you to use during negotiations, so that you know exactly what to say and how to say it.

Have any other questions about how it works? Just shoot us an email at team@levels.fyi.

When should I book?

As early as you can. Negotiations can start in that first recruiter phone screen, so being prepared to handle any compensation- related questions from the beginning will setyou up for success down the line.

What if I only have one offer?

That's no problem at all. Competing offers help, but our coaches have had many successful negotiations for clients with only 1 offer.

I'm in between Levels, what should I book?

If you're in between levels, we suggest booking the higher tier coaching package, as the extra experience of our coaches could help out in negotiating the higher level offers if they come in. If the final offer you accept ends up being more in line with a lower tier package, we may be able to refund the difference in pricing. Please contact us at services@levels.fyi for any questions on this.

What is the risk my offer gets rescinded?

If you follow the guidance of our coaches, your offer will notgetrescinded by trying to negotiate. Even companies that do not negotiate offers still expect you to negotiate! Our coaches have a wide range of experience across multiple industries and companies of all sizes, so we'll be able to evaluate and guide you on the best way to approach it with no risk of a rescinded offer.

Which experience Level do I pick?

New Grads/ Interns/ Entry Level: We do not currently support negotiations for these experience levels, however if you would still like to book to learn the ins and outs of tech negotiations, please book the Mid-Level package.

Mid-Level: If you have~1-8 years of experience in your role, you're likely in the mid-level range and can book the Mid-Level package.

Senior IC/ People Manager: If you have 8+ years of experience in your role or are fielding offers for a third-level IC role or a people manager role, you should book the Senior package. Our coaches supporting this plan specialize in the nuance that usually comes with these types of offers.

Director/ VP/ Executive: Book the Leadership package. Our leadership coaches have helped negotiate offers for these roles which typically have many components to the total package.

Am I too Late to book?

If you've already accepted an offer or made an attempt to negotiate on your own, we aren't as likely to be successful. We suggest booking sooner, rather than later.

I'm an expert negotiator. Is this worth it?

We hear often from people that decided not to go with us and then come to regret it later on. Our service offers a guarantee and we can't help salvage your negotiations if you've already made a slight slip up as a result. Even a small increase in your yearly salary can have a massive impact on your retirement and lifetime earnings. It's up to you whether you'd like to leave it to chance.

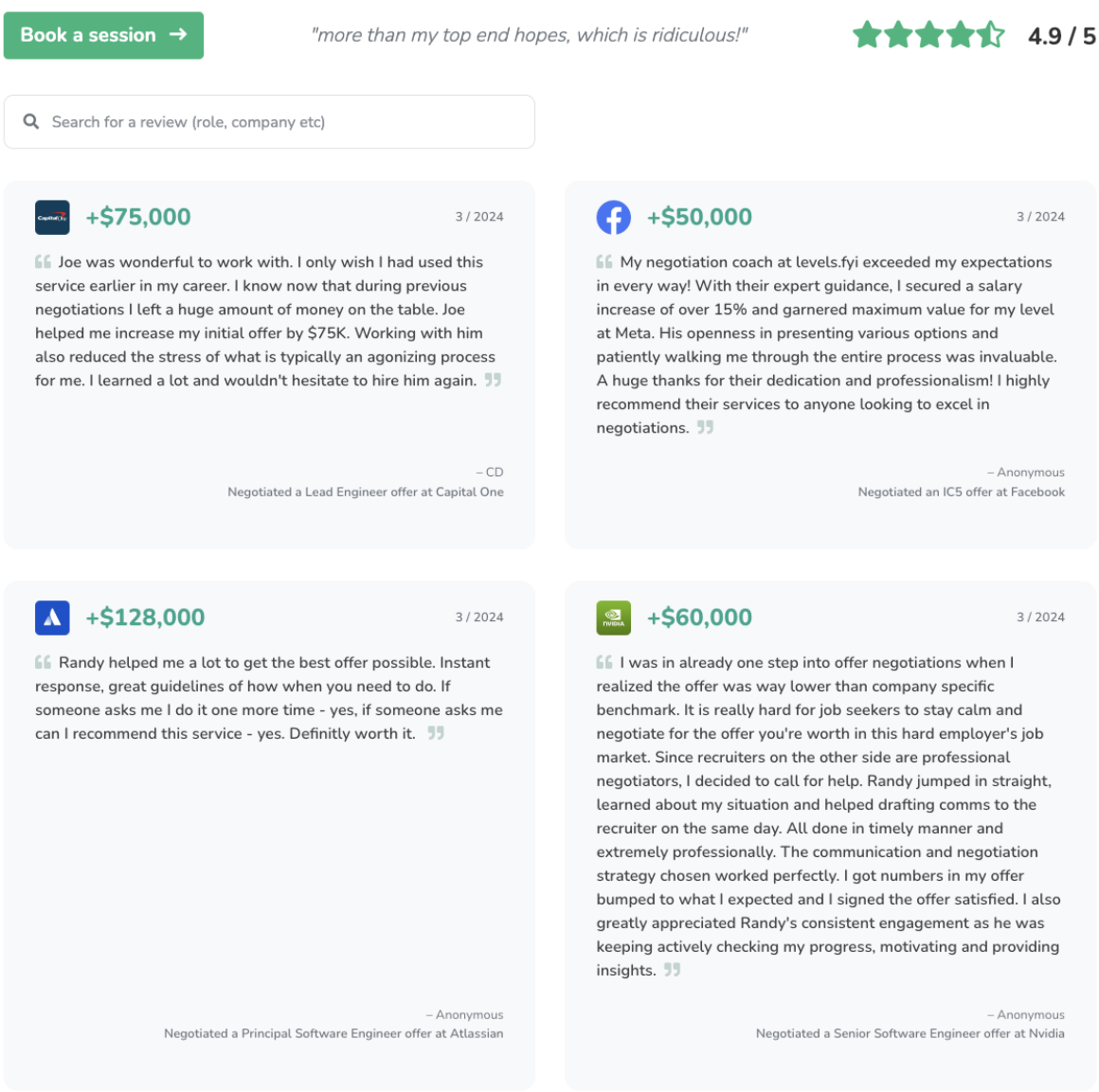
Recruiters negotiate weekly and can put even advanced negotiators in a corner. Our coaches have seen it all and have been on the other side of the table. Sneaky tactics like bluffing, anchoring you, ignoring you, etc. - we know how to deal with it. Thesecond eye of our expert negotiators can ensure you truly leave nothing on the table and to chance.

Do you offer a payment plan to pay over time?

Yes! We offer a payment plan option to pay the full price over 3 or 4 interest-free payments using Klarna. Message us, and our team will be happy to help you enroll onto the payment plan for the package you choose to purchase.

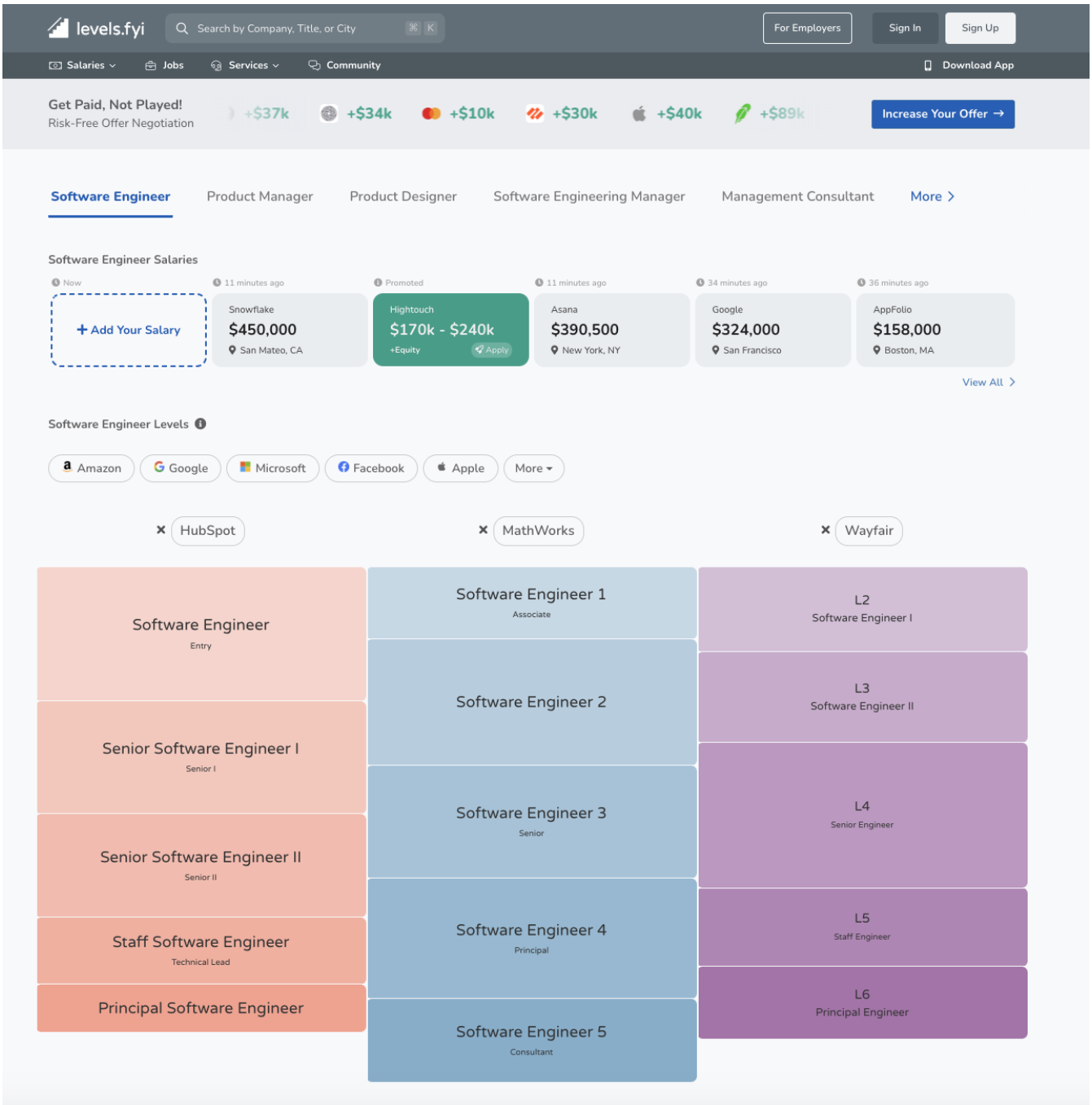
Notes: This is a screenshot of levels.fyi's Frequently Asked Questions (FAQs) for the negotiation coaching service.

Figure B.4: Levels.fyi Negotiation Reviews



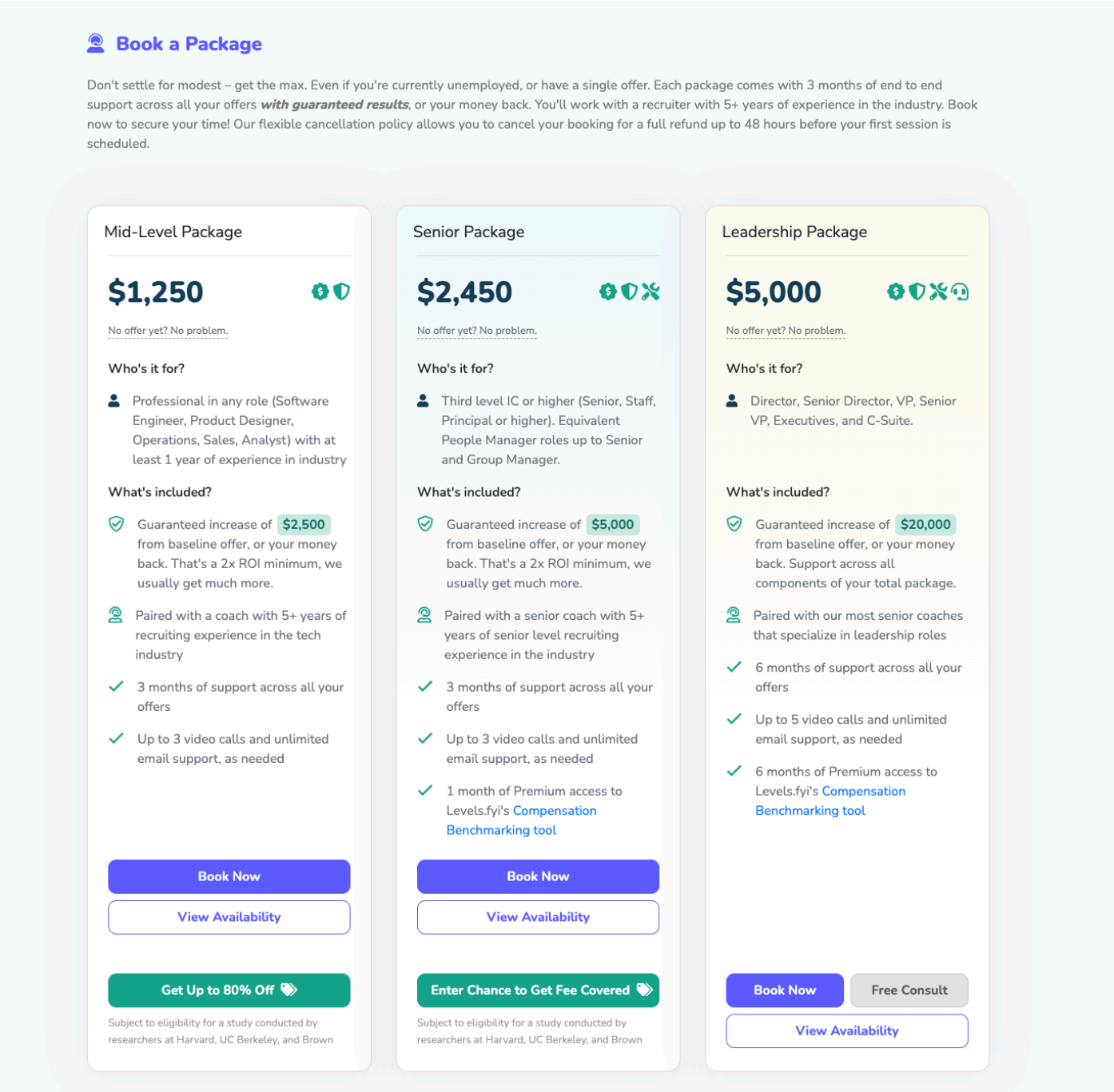
Notes: This is a screenshot of the levels.fyi salary negotiation service reviews. These are the top four reviews among the 300+ reviews.

Figure B.5: Screenshot of levels.fyi's Landing Page



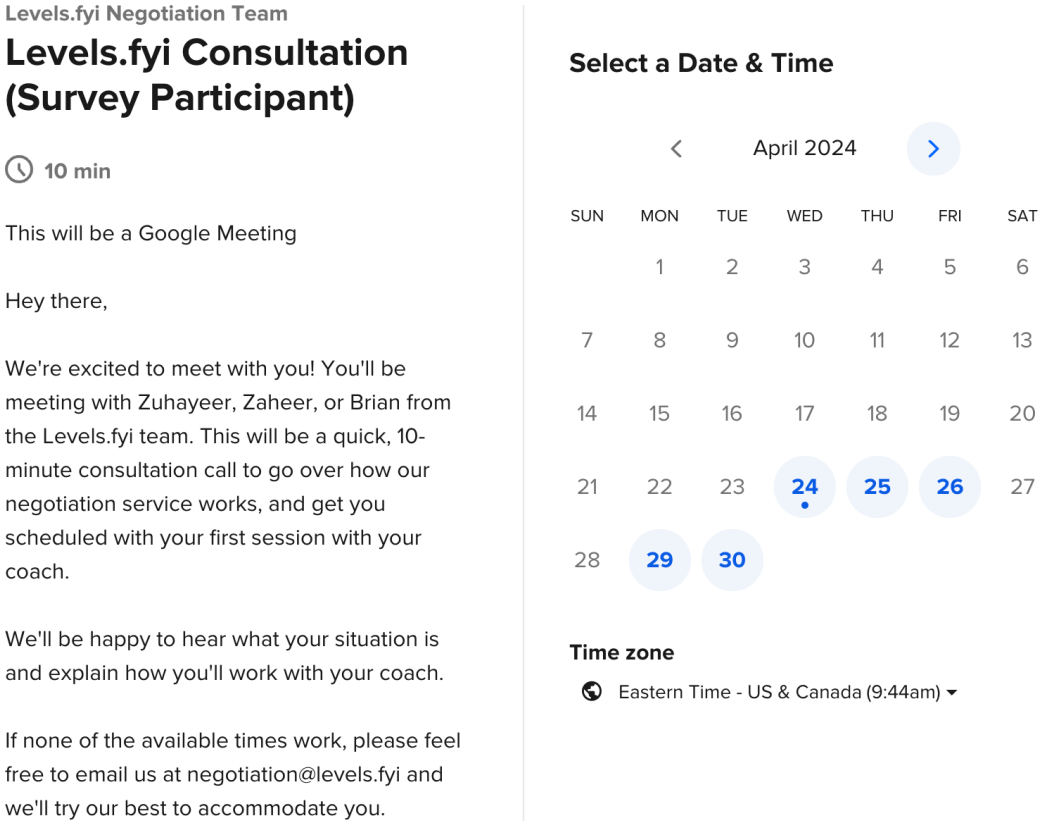
Notes: This is a screenshot of `https://www.levels.fyi/`.

Figure B.6: Screenshot of the Website with Survey Links



Notes: This is a screenshot of the website with the links to the baseline survey.

Figure B.7: Coaching Services Booking Page



Notes: This is a screenshot of the calendly page to book levels.fyi coaching sessions if survey participants accept the coaching services from the survey.

Supplemental Material

C Further Details about the Sample

In this Supplement, we present basic descriptive analyses of the sample such as the timing of survey responses, subject characteristics, and balance tests.

C.1 Timing of Survey Responses

Figure C.1 shows the timing of subject recruitment. Panel A displays the number of baseline survey responses by calendar month from May 2023 to February 2025, with green bars indicating the Wave 1 sample and gray bars representing Wave 2. Recruitment for the first (and main) wave began on May 25, 2023, and ended on October 10, 2024. Due to logistical challenges related to subject payments and quality control, we had to smooth out invitations over time. Nevertheless, we were able to successfully recruit the target number of subjects within five months.

Recruitment for Wave 2 began as Wave 1 was wrapping up. While we would have preferred to spread recruitment over a few months—as we did in Wave 1—this was not feasible. Wave 2 participants were drawn from individuals who came organically to the levels.fyi website, which meant we had to wait a considerable amount of time to reach our target. As a result, Wave 2 responses span nearly 17 months, from September 21, 2023, to February 10, 2025. Participation fluctuates month-to-month, largely reflecting variations in traffic to the levels.fyi's website—for example, traffic tends to slow down around major holidays.

Panel B of Figure C.1 presents the distribution of the time gap between the baseline and follow-up surveys. The earliest a subject could complete the follow-up survey was 45 days after the baseline, when the first invitation was sent. A substantial share of participants responded at that point. On average, the follow-up survey occurred 56 days after the baseline.

C.2 Subject Characteristics and Randomization Balance

Table C.1 reports average characteristics by survey wave: column (1) corresponds to the main wave, and column (2) to Wave 2. The statistics reflect individuals who completed both the baseline and follow-up surveys. For example, column (1) shows that in the main sample, the average respondent had 6.89 years of work experience, and 20.0% were female.

On average, the positions offered $217,039 per year in total compensation, and 57.76% were remote. The most common job titles were Software Engineer (51.31% of the sample), Data Scientist (6.91%), and Product Manager (8.64%). Prominent employers included Amazon (5.69% of the sample), Google (3.52%), and Microsoft (1.84%). The most common locations were California (33.20%), New York (12.72%), and Washington (10.88%).

Comparing columns (1) and (2) of Table C.1 highlights differences between the two survey waves. Wave 2 recruitment specifically targeted individuals with the highest WTP, so it would not be surprising if they differed along other dimensions as well. The most pronounced difference is in total compensation: $237,000 in Wave 1 versus $322,000 in Wave 2. This gap is most likely a byproduct of the recruitment strategy—individuals with a higher WTP for the service are likely to be wealthier. The average differences between the two waves are statistically significant, due to large sample sizes. However, aside from total compensation, the differences are relatively modest in magnitude. For example, the average years of experience was 6.89 in Wave 1 compared to 7.63 in Wave 2; the share of remote positions was 57.76% versus 51.65%; and the share female was 18.52% versus 20.65%. The distributions of job titles, employers, and locations are also broadly similar across the two waves.

C.3 Comparison to Benchmark Sample

The last column of Table C.1 provides a benchmark for assessing the representativeness of our sample. The levels.fyi website operates on a reciprocity model: to access detailed salary information, users must first contribute their own.[69] Users can upload a PDF document, such as a W-2 form or offer letter, or manually enter their information using the form shown in Figure L. We use the sample of individuals who submitted salary information to levels.fyi during a similar period as our data collection as a benchmark. Specifically, the salary submission database contains over 105,155 entries collected between June 2023 and June 2024. Column (3) of Table C.1 reports the average characteristics in the benchmark sample. Comparing columns (1) and (3), we find that while our main sample is not identical to the benchmark, it is relatively similar along basic dimensions. Given the large sample sizes, even small differences are statistically significant. However, the magnitudes tend to be economically small. For instance, the Wave 1 sample reported an average offer of $236,000 per year, compared to $222,000 in the benchmark.

[69] For reference, Figure B.5 shows a screenshot of the company's landing page during the experiment, displaying salary data across levels and companies.

Similarly, the average years of experience was 6.89 in Wave 1 versus 7.10 in the benchmark.

C.4 Randomization Balance and Attrition

The upper panel of Table C.2 reports randomization balance checks for the main survey wave. Columns (1) through (5) correspond to the full sample of individuals who responded to the baseline survey. Columns (1) through (4) show average characteristics for each of the four treatment groups, while column (5) presents the p-value from a joint test of the null hypothesis that the means are equal across groups. All characteristics included in Table C.2 were measured prior to treatment assignment and thus should not be affected by it. The results confirm that observable characteristics are well balanced across treatment groups: differences are consistently small and statistically insignificant. Columns (6) through (10) repeat the same balance checks for the subsample of respondents who completed the follow-up survey and had at least one job offer—this is the key sample for much of the experimental analysis. Again, the observable characteristics are balanced across the four treatment groups.[70]

The randomization balance results for Wave 2 are similar. Table C.3 replicates the structure of Table C.2, but for the second wave. The upper panel of Table C.3 presents the balance checks. As in Wave 1, the differences in pre-treatment characteristics across treatment groups are small and mostly statistically insignificant, consistent with successful random assignment.[71]

The bottom panel of Table C.2 reports survey attrition for the main sample. On average, approximately 66.5% of individuals who completed the baseline survey also completed the follow-up survey. Another important metric is the share of subjects who not only completed the follow-up but also reported at least one job offer, as this is the key analysis sample for much of the experimental work. On average, 57.1% of baseline respondents completed the follow-up survey and received at least one offer. Most importantly, for both attrition measures, we observe some differences across treatment groups that are statistically significant. These differences raise concerns about potential selection bias in the experimental estimates. For this reason, we present a range of robustness checks throughout the paper, including placebo outcomes and Lee bounds.

[70] One characteristic—whether the subject had an offer at baseline—shows a statistically significant difference across groups. However, the magnitude is small, and the result is likely spurious: with 16 characteristics tested, we would expect approximately 1.6 to be significant at the 10% level purely by chance.

[71] Two of the 16 characteristics show statistically significant differences across groups, which is close to the 1.6 expected to reach significance at the 10% level purely by chance.

Table C.3 presents survey attrition for Wave 2. Average attrition rates were similar to those observed in Wave 1. However, unlike in Wave 1, we find no evidence of statistically significant differences in attrition across treatment groups in Wave 2. As a result, we do not believe there is meaningful scope for selection bias in the experimental estimates. Nonetheless, to err on the side of caution, we still include robustness checks—such as placebo outcomes and Lee bounds—throughout the paper.

C.5 Compensation Breakdown and Preferences over Compensation

Figure C.2 presents descriptive statistics on compensation. Panel A displays the distribution of total cash compensation across all offers in the data. There is substantial variation, largely driven by factors such as position title, seniority, and employer. Total cash compensation is defined to include base salary, equity, and bonus. Panel B decomposes compensation into these three components. On average, base salary accounts for 77.9% of total compensation, followed by equity (15.9%) and bonus (6.2%). Panel B also highlights variation across offers in the relative shares of base salary, equity, and bonus—likely reflecting differences in compensation practices across employers.

Our analysis focuses on total cash compensation, implicitly assuming that employees value its three components—base salary, equity, and bonus—equally. This assumption is supported by anecdotal accounts; for example, offer letters typically present total cash compensation as the headline figure. Next, we test this assumption directly by estimating individuals' preferences using revealed preference data—that is, how individuals choose between offers.

Following Cullen et al. (2025), we estimate a conditional logit model. Let I denote the set of individuals, where each individual $i \in I$ chooses one alternative j from a set J_i of available options. These include staying at the current job (for those currently employed), each of the offers received from different employers, or the outside option (not being employed). Each individual i selects the alternative that maximizes utility: $U_{ij} = V_{ij} + \epsilon_{ij}$, where ϵ_{ij} is an unobserved component of utility, assumed to follow a Gumbel (Type I extreme value) distribution. The observed component is $V_{ij} = \mathbf{x}_{ij}\beta$, where $\mathbf{x}_{ij}$ is a vector of alternative characteristics and β is the vector of coefficients estimated via maximum likelihood. In addition to compensation, following Cullen et al. (2025), we include two basic control variables: (i) Glassdoor's employer rating, which aggregates reviews from current and former employees on a 1–5 star scale, and (ii) an indicator

for whether the offer allows remote work.[72] Our outcome variable is whether an individual has chosen, or is most likely to choose, a given offer, based on responses to the follow-up survey.

Before discussing the results, it is important to acknowledge one key limitation of this analysis: the explanatory variables may be correlated with unobserved attributes of the offers, which could introduce omitted variable bias. Also, note that the conditional logit model requires individuals to have multiple alternatives in order to identify preference parameters. For instance, individuals with no offers or only a single offer do not contribute meaningful information to the estimation. To ensure sufficient statistical power, we therefore pool respondents from both survey waves. This yields a sample of 1,568 individuals choosing among 3,775 offers—an average of 2.4 offers per person.

Table C.4 presents the results from the conditional logit model. In column (1), the only compensation-related variable on the right-hand side is the logarithm of total cash compensation. The coefficient is positive and highly statistically significant, indicating that, holding other factors constant, individuals prefer offers with higher total compensation. Column (2) adds two additional variables related to compensation: the share of total cash compensation coming from equity and the share coming from bonuses. Under the null hypothesis that individuals care only about total compensation, the coefficients on these two variables should be zero. Consistent with this hypothesis, both coefficients are close to zero and statistically insignificant. As a robustness check, column (3) replicates the specification in column (2) but includes a set of additional control variables used in Cullen et al. (2025): indicators for publicly listed and start-up companies, the dollar value of employer-provided benefits, indicators for current employer and state, as well as the state's cost of living and quality of life. In column (3), the coefficients on the compensation shares remain statistically insignificant and, if anything, are even smaller.

The key takeaway from this analysis is that treating total cash compensation as the primary object of interest remains a very reasonable first-order approximation. That said, we do not interpret these findings to mean that no individuals care about the composition of their compensation packages. While the coefficients on the share of equity and the share of bonus are statistically insignificant, we cannot rule out small positive coefficients. In other words, on average, individuals may exhibit a slight preference for equity and bonus compensation over base salary. A related limitation of this analysis is the assumption of homogeneous preferences across individ-

[72] Glassdoor ratings are missing for about 18% of observations, mainly due to companies that are too small to appear on Glassdoor. Thus, we always control for a separate dummy indicating whether the Glassdoor data is missing.

uals. This implies that the average estimates may mask underlying heterogeneity—some individuals may prefer certain components of compensation, while others may favor different ones.

C.6 Distribution of Prior Beliefs

Figure C.3 shows the distribution of prior beliefs, with Panel A corresponding to the perceived share of individuals who negotiate and Panel B corresponding to the share of those negotiations that are successful. For the interpretation of the effects of the encouragement treatment, what matters is the comparison between the prior beliefs and the feedback provided. In each panel of Figure C.3, the feedback provided in the encouragement message is denoted with a vertical line. Panel A shows that a majority of subjects (62%) underestimated (with respect to the feedback) the share of negotiators, while Panel B shows that a vast majority (89%) underestimated (with respect to the feedback) the success rate.

The previous analysis compares individuals' prior beliefs to the feedback they received, which is the relevant comparison for assessing belief updating and interpreting heterogeneity in treatment effects. A different question, however, is whether those prior beliefs were accurate. To evaluate this, we must determine whether the feedback provided represents the ground truth.

One alternative benchmark for the ground truth is the average responses to our own survey. For the first prior belief—regarding the share of individuals who negotiate job offers—47.7% of respondents in the control group reported having attempted to negotiate at least one of the offers they received.[73] This is close to the 42% figure used in the feedback, suggesting that comparing prior beliefs to the feedback provides a reasonable approximation of whether respondents underestimated or overestimated relative to the truth.

For the second prior belief—regarding the success rate of negotiations—we find a more substantial discrepancy between our survey data and the feedback figure. Among control group respondents who negotiated, 48.5% reported an improvement in compensation terms. This is significantly lower than the 85% success rate cited in the encouragement treatment. Several factors may explain this difference: the samples differ (tech employees in our study versus a broader group of professionals in the Fidelity survey), the survey instruments were not identical, and the tech labor market was relatively weak during our study period (as discussed

[73] Note that this figure may vary slightly depending on the precise definition. For instance, for offers already held at baseline, this figure is based on whether the individual reported attempting to negotiate the offer at follow-up. Including earlier negotiation attempts would raise the overall rate.

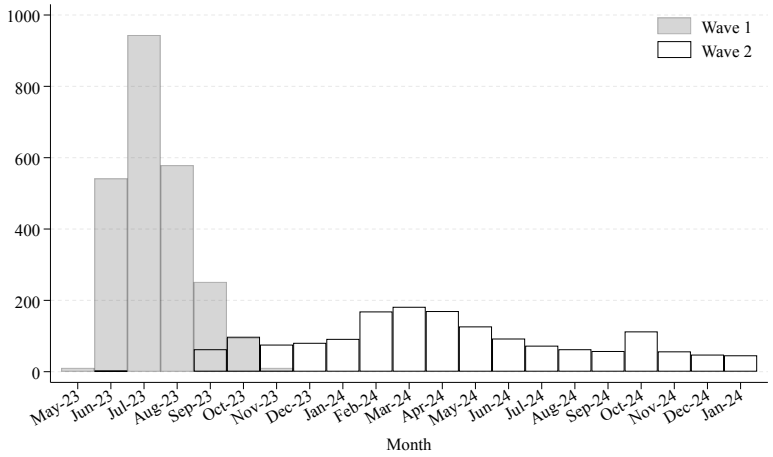
in Section 2.6). Relative to this alternative ground truth, fewer individuals would have underestimated the success rate. However, there is yet another potential benchmark to consider: the success rate among marginal individuals who were persuaded to negotiate by the treatment—78.1%, as reported in Section 3.2. This estimate is quite close to the 85% success rate cited in the encouragement treatment, suggesting that the feedback may reasonably approximate the experience of those most influenced by the intervention.

C.7 Stated Willingness to Pay

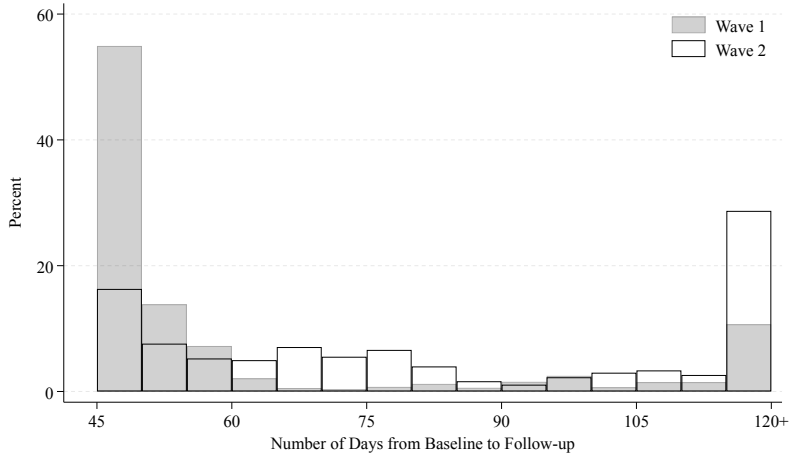
Figure C.4 presents the distribution of WTP, broken down by wave. Participants were asked to make choices across five scenarios that varied the monetary trade-off against the coaching service. Following best practices (e.g., Cullen and Perez-Truglia, 2023), we use the variation in responses within individuals across scenarios to assess response consistency—for example, whether those who chose the coaching service over $300 also chose it over $200. We find that the vast majority of subjects—95.3% in Wave 1 and 98.0% in Wave 2—made internally consistent choices.

Figure C.1: Timing of Baseline and Follow-up Survey Completion

PANEL A: Baseline Completion Dates



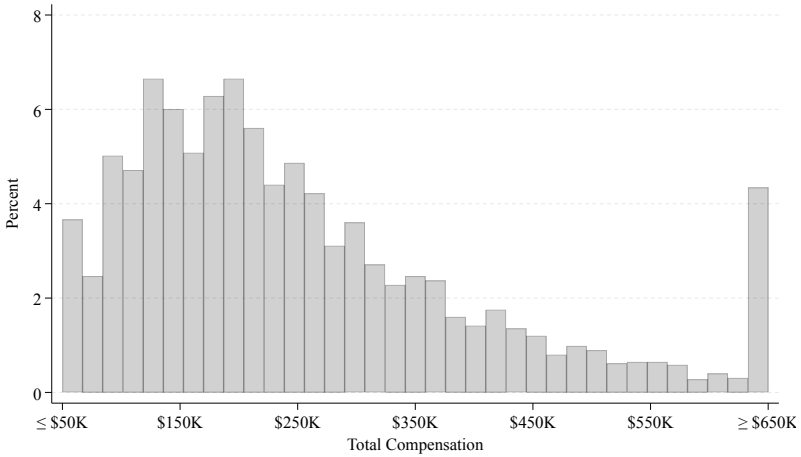
PANEL B: Number of Days from Baseline to Follow-up



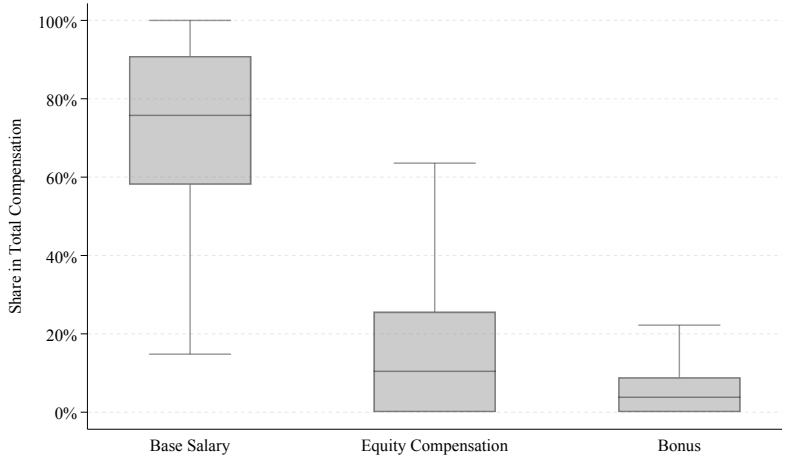
Notes: Panel A presents the baseline survey completion dates for the survey sample recruited in two waves. Panel B presents the number of days elasped from the baseline survey to the follow-up survey.

Figure C.2: Distribution and Composition of Total Compensation

PANEL A: Distribution of Total Compensation

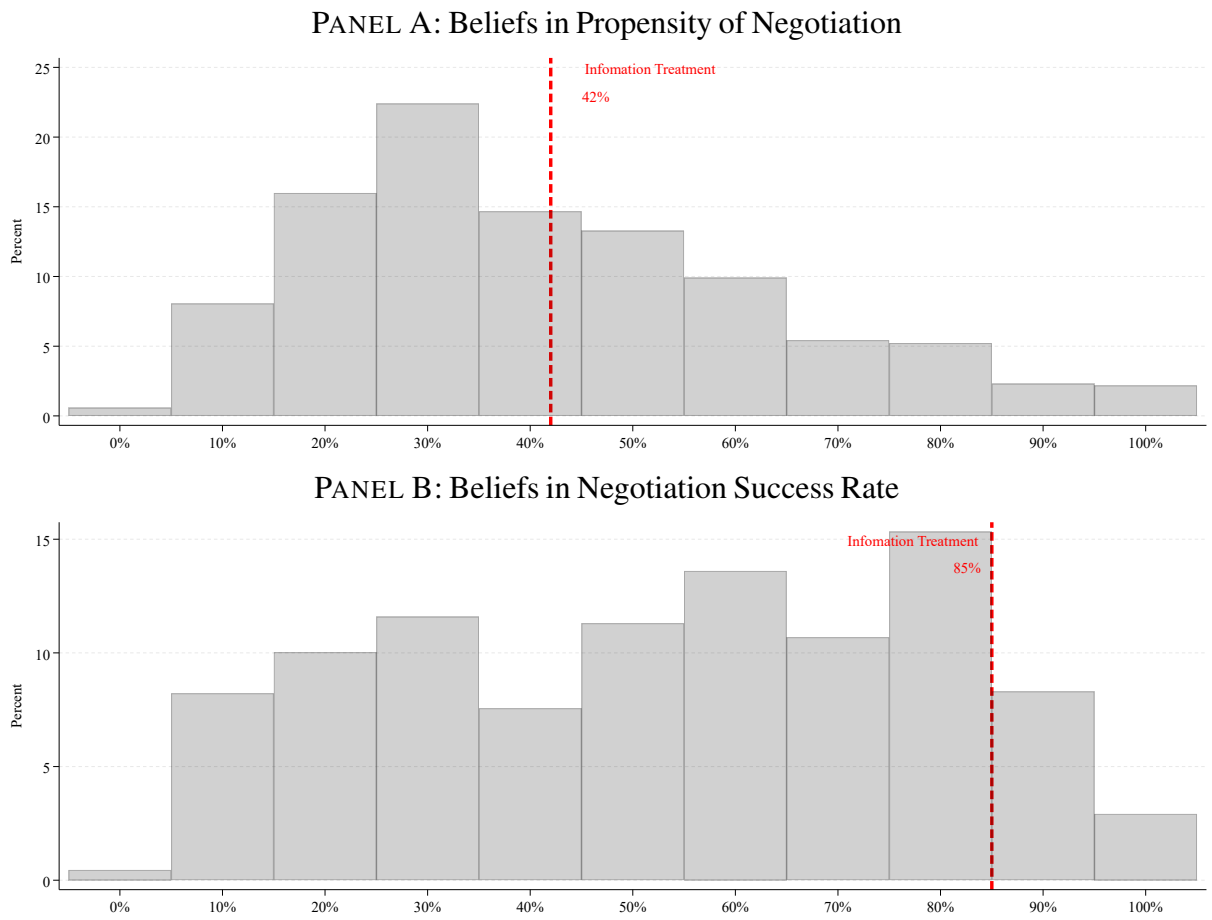


PANEL B: Breakdown of Total Compensation



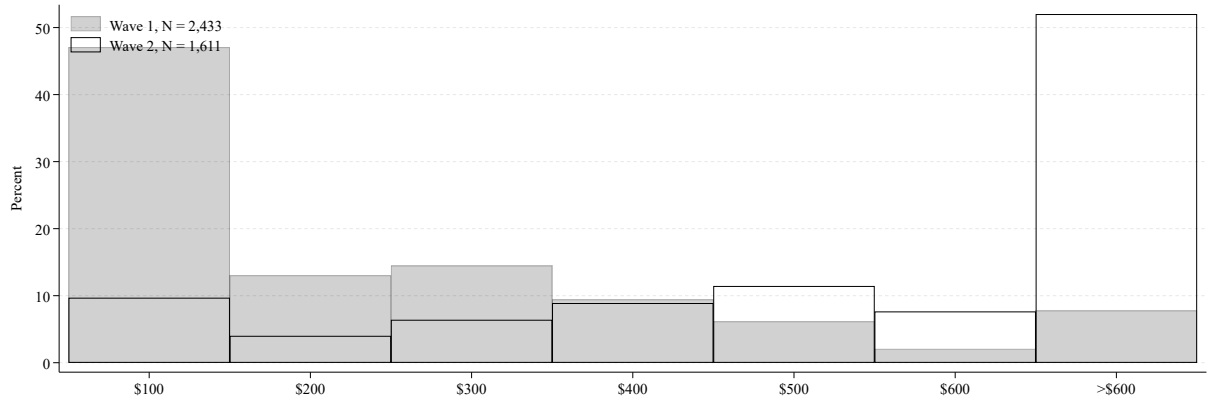
Notes: Panel A shows the distribution of total compensation among offers collected in both survey waves. Panel B reports the breakdown of total compensation in base salary, equity, and bonus.

Figure C.3: Distribution of Prior Beliefs



Notes: Panel A is based on the question "What percent of people do you think try to negotiate the compensation terms after receiving a job offer?" Panel B is based on the question "Among individuals who attempt to negotiate, what percentage of them do you think were successful? In other words, what percentage got at least some of what they asked for?" The dashed lines show the numbers we presented on the encouragement lesson screen.

Figure C.4: Distribution of Willingness to Pay for Coaching



Notes: The sample includes all valid responses to the WTP questions, representing 97% of the full sample. We exclude inconsistent responses in which participants chose the coaching service over a higher gift card amount but not over a lower amount.

Table C.1: Average Characteristics of Survey Respondents and a Benchmark Sample

	(1) Wave 1	(2) Wave 2	(3) Benchmark
Total Compensation ($1,000s)	236.82	321.68	221.50
	(4.505)	(22.585)	(0.460)
Years of Experience	6.89	7.63	7.10
	(0.100)	(0.112)	(0.014)
Share with Remote Work	57.78	51.61	51.69
	(1.002)	(1.246)	(0.154)
Share of Female	18.51	20.63	24.27
	(0.816)	(1.009)	(0.236)
Share in Major Positions			
Software Engineer	51.31	54.71	55.33
	(1.180)	(1.559)	(0.153)
Data Scientist	6.91	5.88	5.74
	(0.599)	(0.737)	(0.072)
Product Manager	8.64	11.18	10.20
	(0.663)	(0.987)	(0.093)
Share in Major Companies			
Amazon	5.69	8.73	9.53
	(0.547)	(0.884)	(0.091)
Google	3.51	3.53	5.23
	(0.435)	(0.578)	(0.069)
Microsoft	1.84	3.92	3.38
	(0.318)	(0.608)	(0.056)
Facebook	0.28	0.10	2.85
	(0.125)	(0.098)	(0.051)
Apple	1.73	1.47	2.12
	(0.308)	(0.377)	(0.044)
Share in Major States			
California	33.18	39.22	31.24
	(1.112)	(1.529)	(0.143)
New York	12.72	14.90	14.26
	(0.787)	(1.116)	(0.108)
Washington	10.88	17.25	12.88
	(0.735)	(1.184)	(0.103)
Texas	7.36	4.80	9.00
	(0.617)	(0.670)	(0.088)
Observations	2,430	1,610	105,155

Notes: Average characteristics in the survey sample and benchmark data, with standard errors in parentheses. Benchmark data are from the levels.fyi salary submission database, covering submissions from June 2023 to June 2024. Total compensation represents the annual sum of base salary, equity, and bonuses, reported in thousands of dollars. The category "Remote work" includes both fully remote and hybrid work arrangements. The gender variable is missing for 67% of benchmark data, so the reported value is the share of female among non-missing observations.

Table C.2: Randomization Balance: Wave 1

	Full Sample Treatment Assignment					Follow Up Sample Treatment Assignment				
	Encouragement (1)	Coaching (2)	Both (3)	Neither (4)	P-Val (5)	Encouragement (6)	Coaching (7)	Both (8)	Neither (9)	P-val (10)
Pre-Treatment Characteristics:										
Share Currently Employed	70.6	69.5	72.6	69.7	0.626	70.9	68.9	72.7	72.9	0.617
	(1.816)	(1.870)	(1.813)	(1.892)		(2.310)	(2.601)	(2.377)	(2.443)	
Current/Recent Base Compensation ($1000s)	142.9	148.8	150.5	151.7	0.498	143.5	152.8	152.0	153.3	0.371
	(3.356)	(4.268)	(4.257)	(4.735)		(4.875)	(7.306)	(6.385)	(6.963)	
Years of Experience	6.8	6.8	6.9	6.9	0.951	6.4	6.1	6.5	6.4	0.179
	(0.196)	(0.201)	(0.201)	(0.199)		(0.245)	(0.269)	(0.258)	(0.262)	
Share of Graduate or Professional Degree	51.4	48.4	49.0	54.1	0.181	51.8	47.8	47.4	51.8	0.479
	(1.993)	(2.030)	(2.032)	(2.051)		(2.540)	(2.806)	(2.665)	(2.746)	
Share Female	17.0	18.4	20.0	18.6	0.611	16.3	15.1	17.6	17.0	0.855
	(1.519)	(1.658)	(1.653)	(1.696)		(1.879)	(2.016)	(2.033)	(2.069)	
Share White	33.2	33.1	32.1	29.3	0.477	34.1	30.8	31.1	27.9	0.423
	(1.905)	(2.019)	(1.932)	(1.986)		(2.413)	(2.605)	(2.473)	(2.472)	
Share Asian	48.7	43.2	46.4	45.2	0.258	49.7	50.6	53.1	54.5	0.311
	(1.993)	(2.012)	(2.027)	(2.049)		(2.542)	(2.808)	(2.664)	(2.737)	
Share with Baseline Offer	49.8	52.7	52.0	52.1	0.753	61.3	59.1	62.2	61.4	<0.001
	(1.994)	(2.028)	(2.031)	(2.057)		(2.475)	(2.761)	(2.588)	(2.675)	
Attition:										
Responded to Follow-up	72.2	62.3	67.5	64.0	<0.001					
	(1.786)	(1.969)	(1.904)	(1.977)						
Responded to Follow-up and had 1+ Offers	61.6	52.4	58.1	56.2	<0.001					
	(1.939)	(2.029)	(2.006)	(2.043)						
Observations	630	607	606	591		388	318	352	332	

Notes: Average characteristics and attrition statistics of sub-samples by treatment assignments. The left panel includes the full sample, and the right panel only includes those who responded to follow-up and had at least one offer. P-values test the null hypothesis that all four treatment groups are equal.

Table C.3: Randomization Balance: Wave 2

	Full Sample Treatment Assignment					Follow Up Sample Treatment Assignment				
	Encouragement (1)	Coaching (2)	Both (3)	Neither (4)	P-Val (5)	Encouragement (6)	Coaching (7)	Both (8)	Neither (9)	P-val (10)
Pre-Treatment Characteristics:										
Share Currently Employed	67.2	71.9	72.0	69.2	0.399	66.3	68.7	66.8	68.6	0.961
	(2.457)	(2.285)	(2.135)	(2.270)		(3.466)	(3.329)	(3.204)	(3.095)	
Current/Recent Base Compensation ($1000s)	163.6	159.5	167.6	160.6	0.606	159.2	160.7	171.6	155.2	0.330
	(5.605)	(2.832)	(5.744)	(3.133)		(4.828)	(4.310)	(11.187)	(4.325)	
Years of Experience	7.6	7.5	7.6	7.7	0.977	7.5	7.0	7.1	7.1	0.304
	(0.242)	(0.228)	(0.210)	(0.222)		(0.342)	(0.317)	(0.301)	(0.297)	
Share of Graduate or Professional Degree	49.2	48.7	54.0	56.9	0.060	46.0	45.1	55.8	54.4	0.101
	(2.617)	(2.541)	(2.371)	(2.434)		(3.654)	(3.573)	(3.379)	(3.320)	
Share Female	20.8	23.7	20.8	17.3	0.164	19.9	21.0	19.4	15.0	0.327
	(2.128)	(2.162)	(1.929)	(1.861)		(2.935)	(2.926)	(2.688)	(2.383)	
Share White	39.0	38.5	38.9	39.7	0.989	41.1	39.4	38.4	37.5	0.980
	(2.567)	(2.487)	(2.322)	(2.411)		(3.627)	(3.526)	(3.317)	(3.242)	
Share Asian	44.5	49.0	45.6	46.0	0.645	46.0	48.2	48.4	48.7	0.700
	(2.601)	(2.541)	(2.369)	(2.450)		(3.654)	(3.587)	(3.400)	(3.332)	
Share with Baseline Offer	32.5	34.5	31.2	34.9	0.629	37.4	42.6	39.6	44.2	0.015
	(2.452)	(2.417)	(2.203)	(2.343)		(3.548)	(3.550)	(3.328)	(3.311)	
Attition:										
Responded to Follow-up	67.8	65.2	68.8	71.8	0.098					
	(2.446)	(2.421)	(2.203)	(2.211)						
Responded to Follow-up and had 1+ Offers	51.1	50.3	49.0	54.5	0.242					
	(2.616)	(2.542)	(2.378)	(2.448)						
Observations	366	388	443	415		187	195	217	226	

Notes: Average characteristics and attrition statistics of sub-samples by treatment assignments. The left panel includes the full sample, and the right panel only includes those who responded to follow-up and had at least one offer. P-values test the null hypothesis that all four treatment groups are equal.

Table C.4: Conditional Logit Model of Offer Acceptance

	(1)	(2)	(3)
log(Total Compensation)	1.339***	1.237***	1.240***
	(0.125)	(0.156)	(0.158)
Share Equity Compensation		0.004	0.003
		(0.004)	(0.004)
Share Bonus		0.004	0.004
		(0.005)	(0.005)
Additional Controls			✓
Individuals	1,568	1,568	1,568
Observations	3,775	3,775	3,775

Notes: Significant at *10%, **5%, ***1%. Conditional logit model results analyzing job offer choices from both waves. Each column represents a different specification. All regressions include a basic set of control variables: the Glassdoor's employer rating, a dummy for whether the Glassdoor's rating is not available, and a dummy for whether the offer is for partly- or fully-remote work. Additional controls include dummies for publicly listed and start-up companies, dollar value of employer-provided benefits, dummies for current employer and state, and the state's cost of living and quality of life. Standard errors are clustered at the individual level.

D Additional Experimental Results and Robustness Checks

D.1 Placebo Outcomes

We present a series of falsification tests in the style of an event-study analysis. In the baseline survey, we measured to pre-treatment outcomes related to negotiations. First, we use a general question about whether the subject had attempted to negotiate offers in the past. Second, we use a more specific question about whether the subject had attempted to negotiate the offer from their current or most recent job. Because these questions were asked before subjects were exposed to any treatments, we should expect no treatment effects on these two outcomes.

The results for the main sample are presented in Table D.1. Columns (1) and (3) include all individuals who responded to the baseline survey. Columns (2) and (4) mirror columns (1) and (3), but restrict the sample to individuals who completed the follow-up survey and received at least one offer—since this is the relevant sample for much of the analysis. The results in columns (2) and (4) are particularly informative: if post-treatment effects were driven by selection bias, we would expect to observe similar effects in pre-treatment outcomes as well. As expected, all coefficients are close to zero. While a few coefficients are (weakly) statistically significant, this is unsurprising—given the large number of tests conducted simultaneously in this table, some are expected to be spuriously significant just by chance. The conclusions are similar for Table D.2, which reproduces the falsification test for Wave 2.

D.2 Robustness to Control Variables

In the baseline specification, we estimate the raw differences between treatment groups. Because this is a randomized controlled trial, including control variables is not necessary. In this section, however, we show that the main results remain robust when control variables are included. Table D.3 presents the results. Columns (1) and (2) correspond to the baseline specification without control variables—corresponding to Panels B and C of Figure 2 and Figure 3—while columns (3) and (4) correspond to the same specifications but with additional control variables: current employment status, current or most recent compensation, years of experience, and indicator variables for having a graduate degree, being female, identifying as White, identifying as Asian, and having received a job offer at baseline. For brevity, this table focuses on the two main outcomes: the probability of a negotiation attempt (columns (1) and (3))

and the probability of experiencing a compensation gain (columns (2) and (4)). Panels A and B of Table D.3 report the results for the encouragement and coaching treatments, respectively, including the corresponding sources of heterogeneity. Comparing column (1) vs. (3), and column (2) vs. (4), the results are robust. For example, for the encouragement treatment, the effect on negotiation attempts changes slightly from 0.073 (p = 0.028) to 0.060 (p = 0.027). For the coaching treatment, the effect remains stable at 0.011, with p=0.028 and p=0.027, respectively.

D.3 Additional Outcomes

The follow-up survey measures a host of additional outcomes intended to measure the broader impact of the interventions. For example, in addition to affecting compensation terms, the treatment may also influence non-compensation terms such as remote work, job title, and paid time off. For each offer, we asked whether there had been any improvement in non-compensation terms. The outcome variable equals 100 if the individual reported an improvement in non-compensation terms for at least one offer, and 0 otherwise. Improvements in non-compensation terms do occur but are less frequent than compensation gains. For example, 32.7% of individuals with at least one offer experience compensation improvements in at least one offer, whereas the corresponding share for non-compensation terms is just 8.3%.

We measure a series of outcomes that can capture whether had broader negative or positive effects besides compensation and non-compensation terms. In addition to changes in terms, employees may also learn about their fit with the job during the negotiation process. To capture this, we asked respondents to rate how well the job aligns with their skills, interests, and career goals on a 1–5 scale.[74] The decision to negotiate may end up influencing whether the subject accepts one of the offers and thus the job mobility. To capture this, we asked subjects whether they have accepted each offer or are likely to accept it in the future. We construct an outcome that takes the value 100 if the subject changed employers or is likely to do so. We have an alternative version of this outcome which we observe for subjects regardless of whether they responded to the follow-up survey or not. More precisely, we measure whether they changed employers according to their LinkedIn profile. More precisely, the outcome takes the value 100 for individuals who switched employers within 12 months of the baseline survey according to their LinkedIn profiles. We also asked individuals to rate their satisfaction with the negotiation

[74] For individuals with multiple offers, we take the average across all offers.

on a 1–5 scale.[75] Finally, we use the open-ended data to measure whether the subject mentions explicitly experiencing backslash or fearing backslash.[76]

We also measured an outcome that may shed light on the causal mechanisms underlying the treatment effects on compensation terms: the number of new offers received. The intervention could plausibly influence this margin—if individuals anticipate negotiating more as a result of the treatment, they may place greater value on securing outside offers, either to gain leverage or to ensure a fallback option. The outcome of interest is the number of new offers received between the baseline and follow-up surveys. During negotiations, it is common for employers to ask about salary history or, where such inquiries are prohibited, about salary expectations (Agan et al., 2020). To assess whether the treatment influenced disclosure behavior, we asked respondents whether the employer requested salary information and, if so, whether they chose to provide it.

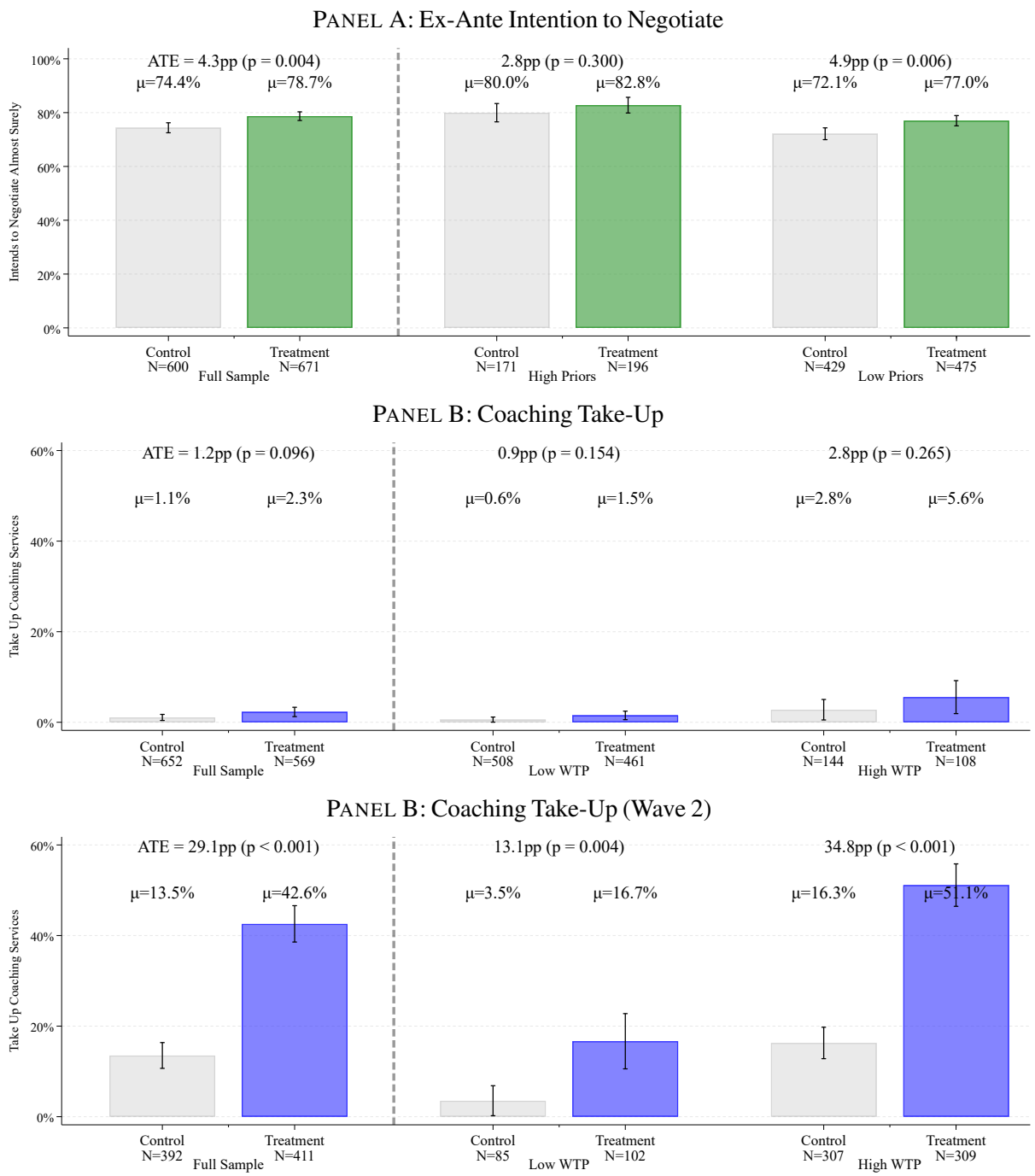
Table D.4 presents the results for these additional outcomes. Columns (1) and (2) replicate the baseline specifications from Table D.3, while columns (3) through (11) report treatment effects on the additional outcomes. Column (4) shows that, consistent with the increase in the share of subjects who experienced compensation gains, the encouragement treatment also raised the average percentage gain in compensation. In other words, we observe effects on both the extensive and intensive margins. Column (4) also shows no significant effect of the encouragement treatment on non-compensation terms—which is unsurprising, given that such improvements are rarer and the encouragement message focused solely on compensation. Columns (5)–(9) explore whether the additional negotiations prompted by the encouragement treatment led to any negative side effects. For example, more negotiations might make candidates feel worse about the position, reduce the likelihood of accepting an offer, decrease satisfaction with negotiations, or increase the likelihood of reporting backlash. We find no evidence of negative repercussions. If anything, the results point to some positive effects: column (5) shows a positive and highly statistically significant impact of the encouragement treatment on perceived alignment with the job. Column (10) provides weakly significant evidence that the encouragement treatment may have motivated some subjects to seek additional offers. Lastly, column (11) shows no significant effect on individuals' decisions to disclose

[75]This outcome is, by construction, defined only for individuals who attempted to negotiate at least one offer. If individuals attempted multiple negotiations, we define the outcome as the highest satisfaction across all negotiation attempts.

[76]This outcome is, by construction, defined only for individuals who attempted to negotiate at least one offer.

salary history or expectations—which is unsurprising, given that the encouragement message did not mention anything about salary disclosure.

Figure D.1: Robustness Check: Results with Sample Adjustments



Notes: Panel A replicates Panel A from Figure 2, except that the sample is restricted to those who responded to the follow-up and had at least 1 offer, matching the sample used in Panels B and C of Figure 2. Panel B replicates Panel A from Figure 3, except that the sample is similarly adjusted to match the one used in Panels B and C of Figure 3. Panel C replicates Panel A from Figure 4, except that the sample is similarly adjusted to match the one used in Panels B and C of Figure 4.

Table D.1: Placebo Outcomes: "Effects" on Pre-Treatment Negotiations (Wave 1)

	Past		Current/Recent	
	(1) Full Sample	(2) Follow-up	(3) Full Sample	(4) Follow-up
Encouragement Treatment				
All Respondents	0.008	0.027	0.025	0.045
	(0.021)	(0.028)	(0.020)	(0.027)
Low Prior	-0.001	0.025	0.045*	0.056*
	(0.025)	(0.033)	(0.024)	(0.033)
High Prior	0.030	0.029	-0.024	0.014
	(0.038)	(0.052)	(0.036)	(0.048)
Coaching Treatment				
All Respondents	0.005	-0.002	0.012	0.008
	(0.021)	(0.028)	(0.020)	(0.027)
High WTP	0.014	0.009	0.074*	0.088
	(0.043)	(0.064)	(0.041)	(0.061)
Low WTP	-0.012	-0.007	-0.007	-0.012
	(0.025)	(0.032)	(0.024)	(0.031)
Observations	2,292	1,273	2,292	1,273
Baseline Mean	0.524	0.521	0.597	0.588

Notes: Significant at *10%, **5%, ***1%. The main outcome in Column (1) and (3) is the attempt to negotiate the current offer when they first received the offer if employed or the attempt to negotiate the most recent employment if unemployed. The outcome in Column (2) and (4) is from the question "Do you have past experience negotiating your compensation?" The upper panel corresponds to the encouragement treatment and the lower panel corresponds to the coaching treatment. Row 1 and 4 include the full Wave 1 sample. Row 2 and 3 split the sample by prior beliefs in negotiations. Row 5 and 6 split the sample by the stated WTP for coaching services (above/below WTP = $300).

Table D.2: Placebo Outcomes: "Effects" on Pre-Treatment Negotiations (Wave 2)

	Past		Current/Recent	
	(1) Full Sample	(2) Follow-up	(3) Full Sample	(4) Follow-up
Encouragement Treatment				
All Respondents	-0.009	-0.017	0.004	-0.022
	(0.025)	(0.035)	(0.024)	(0.033)
Low Prior	-0.032	-0.048	-0.015	-0.044
	(0.030)	(0.043)	(0.030)	(0.042)
High Prior	0.042	0.063	0.043	0.036
	(0.042)	(0.057)	(0.040)	(0.055)
Coaching Treatment				
All Respondents	-0.011	-0.053	0.051**	0.017
	(0.025)	(0.035)	(0.024)	(0.033)
High WTP	0.010	-0.020	0.055**	0.012
	(0.028)	(0.040)	(0.027)	(0.039)
Low WTP	-0.126**	-0.186***	0.035	0.037
	(0.055)	(0.070)	(0.053)	(0.069)
Observations	1,612	818	1,612	818
Baseline Mean	0.546	0.591	0.612	0.642

Notes: Significant at *10%, **5%, ***1%. The main outcome in Column (1) and (3) is the attempt to negotiate the current offer when they first received the offer if employed or the attempt to negotiate the most recent employment if unemployed. The outcome in Column (2) and (4) is from the question "Do you have past experience negotiating your compensation?" The upper panel corresponds to the encouragement treatment and the lower panel corresponds to the coaching treatment. Row 1 and 4 include the full Wave 1 sample. Row 2 and 3 split the sample by prior beliefs in negotiations. Row 5 and 6 split the sample by the stated WTP for coaching services (above/below WTP = $300).

Table D.3: Main Outcomes with Controls

	OLS		With Controls	
	(1) Negotiated	(2) Improved	(3) Negotiated	(4) Improved
Encouragement Treatment				
All Respondents	0.073***	0.057**	0.067**	0.052**
	(0.028)	(0.026)	(0.027)	(0.026)
Low Prior	0.090***	0.073**	0.083***	0.068**
	(0.033)	(0.030)	(0.032)	(0.030)
High Prior	0.030	0.016	0.018	0.013
	(0.051)	(0.052)	(0.048)	(0.051)
Coaching Treatment				
All Respondents	0.011	-0.045*	0.012	-0.046*
	(0.028)	(0.026)	(0.027)	(0.026)
High WTP	0.051	0.014	0.041	-0.001
	(0.061)	(0.062)	(0.060)	(0.062)
Low WTP	0.002	-0.056*	0.004	-0.056*
	(0.032)	(0.030)	(0.031)	(0.030)
Observations	1,272	1,272	1,272	1,272
Baseline Mean	0.510	0.327	0.510	0.327

Notes: Significant at *10%, **5%, ***1%. The main outcomes: negotiating an offer and having offer compensation increased. The upper panel corresponds to the encouragement treatment and the lower panel corresponds to the coaching treatment. Row 1 and 4 include the full sample. Row 2 and 3 split the sample by prior beliefs in negotiations. Row 5 and 6 split the sample by the stated WTP for coaching services (above/below WTP = $300). Controls include current employment status, current or most recent compensation (in thousands of dollars), years of experience, and dummy variables for holding a graduate degree, being female, identifying as White, identifying as Asian, and having received a job offer at baseline.

Table D.4: Additional Outcomes

	Main Outcomes		Additional Outcomes								
	Offer Negotiated	Offer Improved	Comp. Improvement (%)	Non-Comp. Improved	Alignment	Switch Jobs (Survey)	Switch Jobs (LinkedIn)	Negotiation Satisfaction	Backlash	No. New Offers	Expectations Provided
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Encouragement Treatment											
All Respondents	0.073***	0.057**	0.909**	0.003	0.143***	-0.013	0.015	0.052	0.007	0.089*	0.005
	(0.028)	(0.026)	(0.449)	(0.015)	(0.052)	(0.072)	(0.022)	(0.088)	(0.024)	(0.052)	(0.028)
Low Prior	0.090***	0.073**	1.045**	0.002	0.175***	0.064	0.030	0.102	0.026	0.100*	0.020
	(0.033)	(0.030)	(0.480)	(0.018)	(0.061)	(0.086)	(0.027)	(0.111)	(0.028)	(0.059)	(0.033)
High Prior	0.030	0.016	0.524	0.006	0.061	-0.204	-0.018	-0.033	-0.034	0.062	-0.034
	(0.051)	(0.052)	(0.995)	(0.028)	(0.097)	(0.132)	(0.041)	(0.136)	(0.044)	(0.107)	(0.052)
Observations	1,272	1,272	1,272	1,272	1,272	1,272	1,909	584	584	1,477	1,274
Baseline Mean	0.510	0.327	4.570	0.083	3.878	2.173	0.398	3.738	0.077	0.727	0.417

Notes: Significant at *10%, **5%, ***1%. The upper panel corresponds to the encouragement treatment and the lower panel corresponds to the coaching treatment. Row 1 and 4 include the full Wave 1 sample. Row 2 and 3 split the sample by prior beliefs in negotiations. Row 5 and 6 split the sample by the stated WTP for coaching services (above/below WTP = $300). Each column corresponds to a different dependent variable: negotiating an offer, having offer compensation increased, percent increase in total compensation, having non-compensation terms of an offer improved, alignment rating with the job offer, offer acceptance, updating LinkedIn profiles with a new job, satisfaction with the negotiation conditional on negotiating, experiencing backlash conditional on negotiating, number of new offers at follow-up and disclosing salary expectations.

E Differences by Gender

E.1 Differences at Baseline

In this section, we examine heterogeneity in treatment effects by gender. This pre-registered analysis is motivated by the fact that negotiations are often cited as a contributing factor to the gender pay gap (e.g., Babcock and Laschever, 2003; Sandberg and Scovell, 2013). Moreover, because the tech sector is a male-dominated industry, women may be particularly susceptible to gender biases (Kline et al., 2022). Indeed, we observe several notable baseline differences that may be suggestive of such biases. Compared to men, women are on average 5.1 pp less likely to attempt negotiation and, consequently, 4.0 pp less likely to experience a compensation improvement. If anything, women have even stronger incentives to negotiate: as shown in Figure E.1, after controlling for company and position, women receive initial offers that are 5.9% lower than those received by men. Women also differ in other respects, such as believing that negotiations are less common and reporting lower confidence than men.

Any gender differences in the treatment effects should be interpreted with caution. First, men and women may differ in observable and unobservable characteristics besides gender. As a result, the gender differences may be partly or entirely attributed to these other factors. Second, there are power limitations. Since women make up a relatively small share of the sample (17.3% in Wave 1 and 20.6% in Wave 2), the treatment effects for women are imprecisely estimated.

E.2 Differences in Treatment Effects by Gender

We begin by examining heterogeneity in the effects of the encouragement treatment. Figure E.2 mirrors Figure 2, except that the sample is split by the gender of the subjects rather than by their prior beliefs. We find suggestive evidence that while the encouragement treatment increased negotiation attempts—and successful ones—for both men and women, the effects are relatively stronger for women. Panel A of Figure E.2 shows that, for the ex-ante intention to negotiate at baseline, there are no significant gender differences in treatment effects. However, Panels B and C reveal that some significant differences emerge later. Panel B indicates that the effect on actual negotiation attempts is positive and statistically significant for men (5.7 pp, $p = 0.062$), and also positive and statistically significant for women, but three times as large (16.8 pp, $p = 0.014$). Panel C shows a similar pattern for compensation gains: a treatment

effect of 3.7 pp (p = 0.201) for men versus 15.2 pp (p = 0.024) for women. While these gender differences in treatment effects are only borderline significant (p=0.141 in Panel B and p=0.110 in Panel C), the consistency in direction and magnitude across both outcomes suggests a meaningful underlying effect.

Next, we examine the heterogeneity in the effects of the coaching treatment. Figure E.3 mirrors Figure 4, but splits the sample by the gender of the subject rather than by their stated WTP. Panel A of Figure E.3 shows that when it comes to the effects of the coaching treatment on take-up, there are no statistically significant differences by gender.[77] In contrast, Panels B and C reveal more pronounced gender differences in the treatment effects on negotiation attempts and compensation gains. Among men, these effects are close to zero and statistically insignificant: -0.2 pp (p = 0.960, Panel B) for negotiation attempts and -1.2 pp (p = 0.754, Panel C) for compensation gains. In other words, the men who took up coaching as a result of the treatment were going to negotiate—and experience compensation gains—regardless.

By contrast, most of the women who enrolled in coaching due to the discount would not have attempted to negotiate otherwise—and, as a result, would have been less likely to experience compensation gains. Indeed, this pattern aligns with the perceptions of the coaches themselves, most of whom believe their service is particularly beneficial for women.[78] Panel B of Figure E.3 shows that, for women, the coaching treatment had a large positive effect on the probability of negotiating (9.9 pp), though the estimate is borderline insignificant (p = 0.174). Consistently, Panel C shows a positive effect on the likelihood of experiencing compensation gains (12.5 pp), although this effect is again borderline insignificant (p = 0.125). While each estimate is individually statistically insignificant, the fact that they are both consistent in direction and magnitude is reassuring. Moreover, the effects of the coaching treatment on women (Panels B and C of Figure E.3) closely mirror those of the encouragement treatment (Panels B and C of Figure E.2). In both cases, the interventions increased the likelihood that women would negotiate, and these additional negotiations were largely successful. Thus, even if each individual estimate is not statistically significant, the consistency of the patterns across treatments provides stronger evidence of meaningful gender differences.

We can also discuss how our findings relate to literature on the gender pay gap. Salary negotiations are frequently cited as a contributing factor to gender disparities in pay (e.g., Card

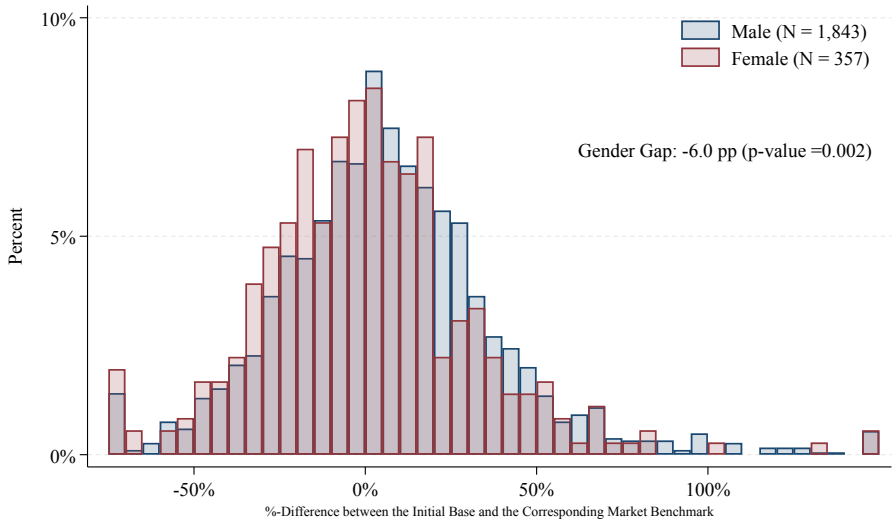
[77] The treatment effect on take-up is somewhat larger for men, but the difference is not statistically significant.

[78] Among the 12 coaches who responded to our survey, 8 believed that the coaching service would be more beneficial for women, while only 1 believed it would benefit men more.

et al., 2016; Recalde and Vesterlund, 2023). Reflecting this concern, public discourse has increasingly encouraged women to "lean in" by negotiating more assertively (Babcock and Laschever, 2003; Sandberg and Scovell, 2013). The scientific evidence, however, is mixed. While women tend to negotiate significantly less in laboratory settings (e.g., Babcock and Laschever, 2003), doing so can lead to costly impasses (Exley et al., 2020; Dannals et al., 2021). Outside the lab, some survey studies find that the gender gap in negotiation may be less pronounced (Lachowska et al., 2022; Caldwell et al., 2024) and even reversed (Kray et al., 2024). Experimental studies with MBA students and online survey participants find that information provision can shift perceptions about the gender differences in negotiation, though the behavioral impacts are less clear (Capozza, 2024; Cortes et al., 2024). In our context—a male-dominated industry—we find that both the encouragement and coaching interventions are relatively more beneficial for women, suggesting that such interventions could be leveraged to narrow the gender pay gap. Our findings are consistent with Roussille (2024), who studies the tech sector too, and shows that setting default asking salaries can reduce gender disparities.[79] Moreover, our theoretical model formally demonstrates that policies promoting a shift toward a lean-in equilibrium could enhance equity.

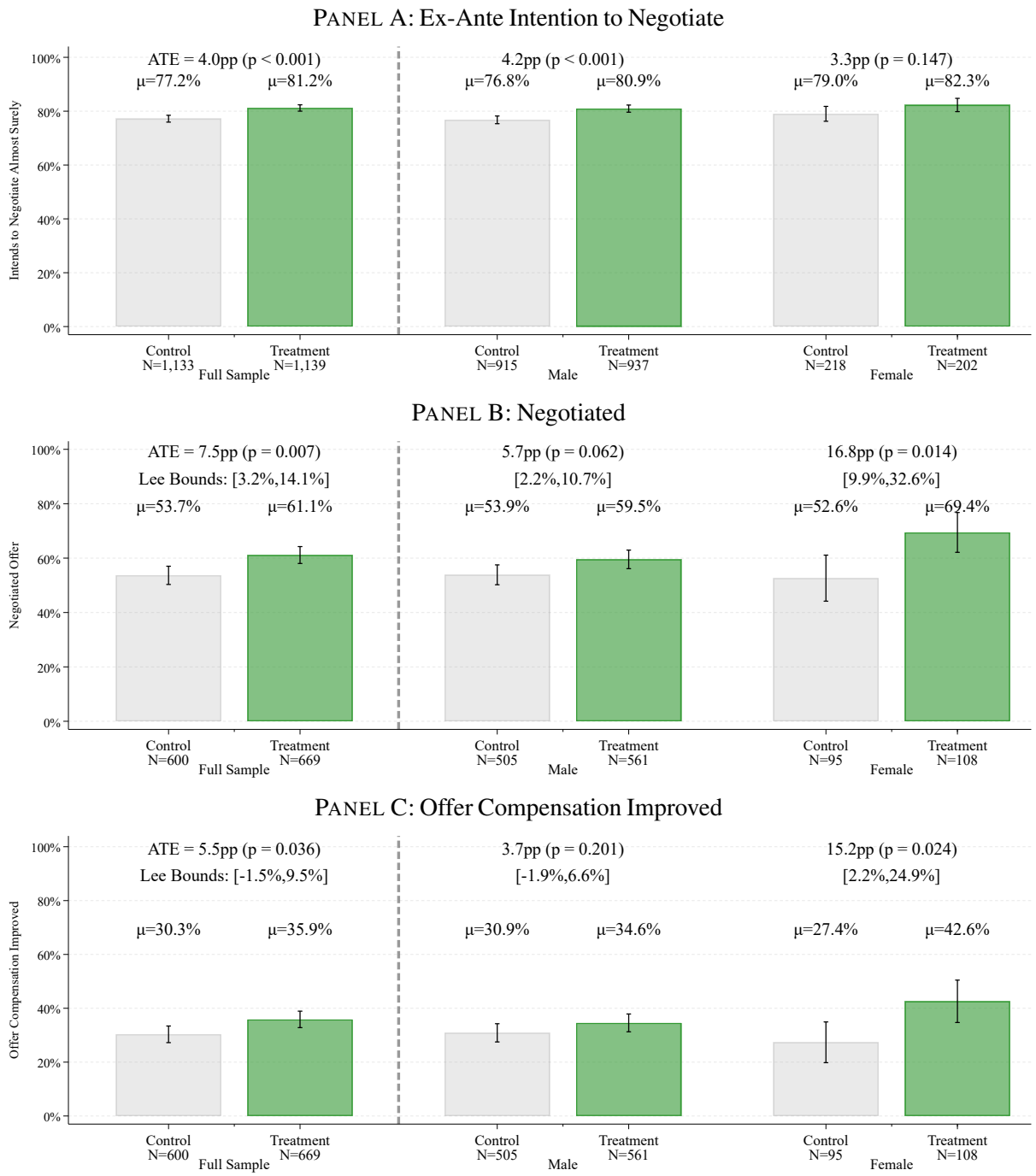
[79] Also consistent with gender disparities in negotiations, evidence on public school teachers shows that the introduction of individual bargaining can widen the gender pay gap (Biasi and Sarsons, 2021).

Figure E.1: Gender Differences in Initial Offers



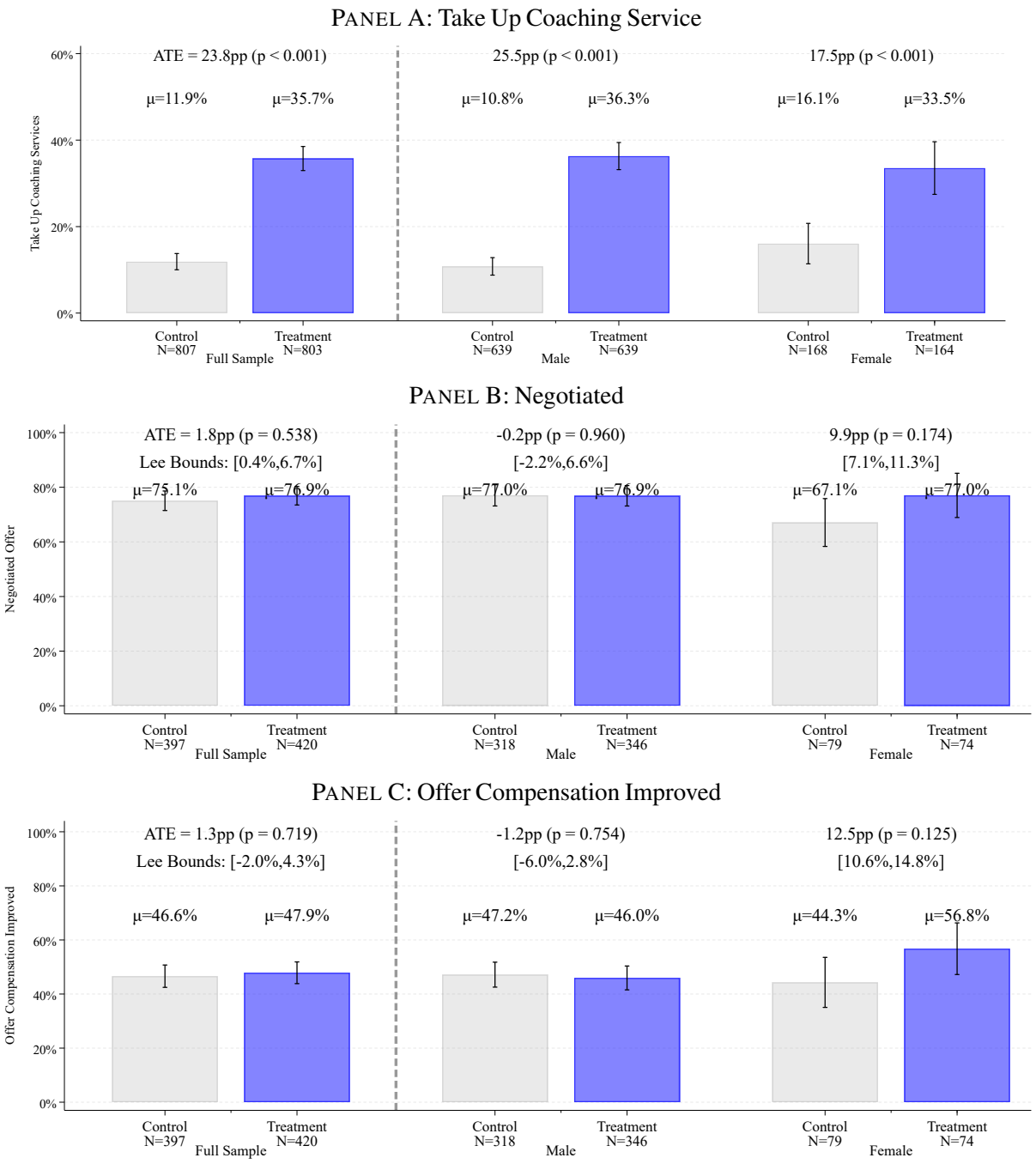
Notes: The figure reports the offer compensation relative to benchmark by gender. The sample includes offers reported in the survey. Benchmarks are constructed using levels.fyi salary submission data, predicting market compensation based on location, company, job description, and years of experience.

Figure E.2: The Effects of Encouragement Treatment and Heterogeneity by Gender (Wave 1)



Notes: Main outcomes by encouragement treatment status (with the treated group in green bars and 90% CI in brackets). Each panel corresponds to a different dependent variable: Panel A displays ex-ante intention to negotiate, Panel B negotiating an offer, and Panel C having offer compensation increased. In each panel, the first two bars represent the full Wave 1 sample, and the rightmost four bars split the sample based on gender.

Figure E.3: The Effects of Coaching Treatment and Heterogeneity by Gender (Wave 2)



Notes: Main outcomes by coaching treatment status (with the treated group in blue bars and 90% CI in brackets). Each panel corresponds to a different dependent variable: Panel A displays taking up coaching services, Panel B negotiating an offer, and Panel C having offer compensation increased. In each panel, the first two bars represent the full Wave 2 sample, and the rightmost four bars split the sample based on gender.

Table E.1: Summary Statistics by Gender

	(1) Male	(2) Female	(3) p-value
Age	31.74	31.64	0.728
	(0.131)	(0.254)	
Share of Graduate or Professional Degree	50.77	53.99	0.113
	(0.895)	(1.819)	
Currently Employed	71.67	63.70	0.000
	(0.806)	(1.755)	
Current/Recent Base ($1,000s)	155.86	149.21	0.100
	(1.686)	(4.361)	
Have offer at Baseline	44.56	41.62	0.145
	(0.889)	(1.799)	
Number of Offers at Baseline	1.65	1.67	0.767
	(0.025)	(0.064)	
Total Compensation of Offer at Baseline ($1,000s)	278.01	223.53	0.021
	(10.683)	(7.244)	
Negotiate current/recent Employment	61.88	64.63	0.162
	(0.869)	(1.745)	
Share of Having Past Negotiation Experience	53.27	53.06	0.919
	(0.893)	(1.821)	
Belief in Percent of People Who Negotiate	42.02	44.18	0.015
	(0.388)	(0.802)	
Belief in Percent of People Who Successfully Negotiate	55.04	55.13	0.926
	(0.453)	(1.004)	
Comfort about Negotiating (1-4)	2.91	2.71	0.000
	(0.015)	(0.030)	
Share of Software Engineer	2.62	2.71	0.908
	(0.339)	(0.715)	
Share in FAANG	19.06	11.61	0.000
	(0.835)	(1.410)	
Observations	3,124	752	

Notes: Average characteristics by gender, with standard errors in parentheses.

F Comparison between Subjects with and without Coaching

Although not the primary focus of our study, a related question is whether individuals who used the coaching service experienced different outcomes—such as final compensation terms—than they would have without using the service. This section explores these additional hypotheses.

The results are presented in Figure F.1, based on Wave 2 data, as few subjects in the main wave used the coaching service. To maximize statistical power, we compare average outcomes between individuals who used the coaching service and those who did not. In Panel A, for example, the left bar shows an average compensation gain of 5.6% for those without coaching, compared to 8.4% for those with coaching—implying a Mean Difference (MD) of 2.9 pp. This implied effect is positive and statistically significant, with a 90% confidence interval ranging from 1.5 to 4.3 pp.

While this simple comparison offers the most statistical power, it is potentially subject to omitted variable bias. To assess the severity of this concern, we begin by examining whether the two groups are comparable in observable characteristics. If they are not comparable on observables, it would suggest potential imbalance in unobservables as well. However, as shown in Table F.1, the observable characteristics of individuals who used coaching are broadly similar to those who did not, even if not perfectly identical.

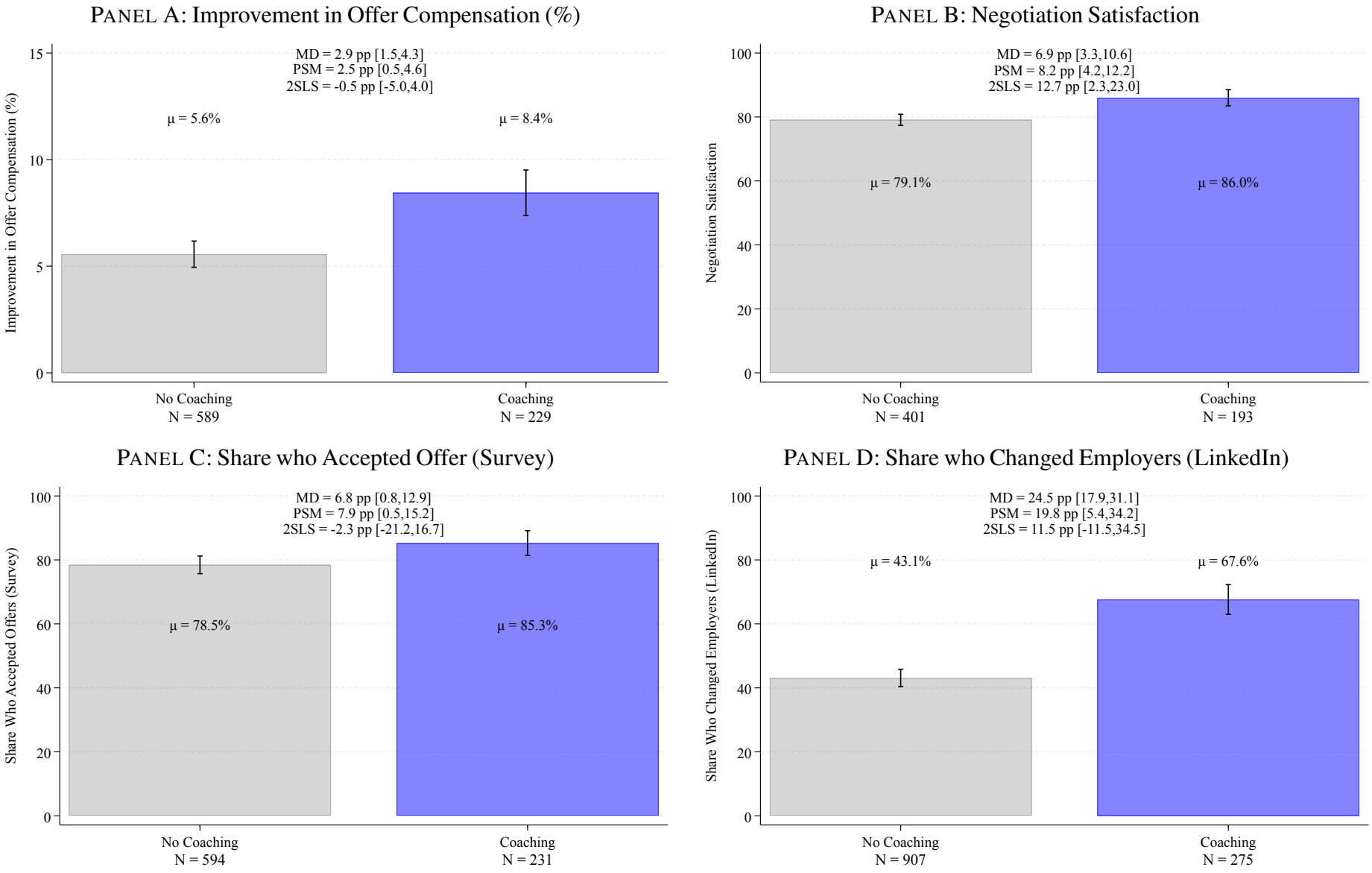
While the groups are comparable in observable characteristics, omitted variable bias due to unobservables remains a concern. To address this, in addition to the simple comparison of means, each panel of Figure F.1 includes two additional estimates. The first adjusts for observable differences using a Propensity Score Matching (PSM) approach. The second is based on a Two-Stage Least Squares (2SLS) regression, where coaching uptake is instrumented by assignment to the coaching treatment. By isolating variation in coaching that is purely due to random assignment, the 2SLS approach fully addresses concerns about omitted variable bias. However, this method relies on a smaller source of variation and, as a result, yields much less precise estimates.

Again, in Panel A of Figure F.1, the PSM estimate (2.5 pp) is close to—and statistically indistinguishable from—the MD estimate (2.9 pp). The 2SLS estimate is lower, at -0.5 pp; however, the 90% confidence interval extends up to 4.0 pp. As a result, the 2SLS estimate cannot rule out modest positive effects of the magnitude suggested by the MD estimate.

Panel B of Figure F.1 presents the estimated effects of coaching on negotiation satisfaction. To facilitate direct comparison with other panels, we rescaled the satisfaction measure by a factor of 20, so that it ranges from 20 to 100. All three models (MD, PSM, and 2SLS) indicate that the coaching service had a positive and statistically significant effect on satisfaction with the negotiation. One natural interpretation is that individuals who used coaching were more satisfied because they secured better terms. However, the effects may go beyond improvements in compensation. For example, coaches may have helped set more realistic expectations, provided valuable information, or assisted clients in managing the anxiety associated with the negotiation process.

Panels C and D provide alternative measures of whether subjects accepted an offer. Panel C reports the share of respondents who accepted an offer, based on follow-up survey responses. The MD and PSM estimates suggest significant positive effects. However, these results must be taken with a grain of salt, as the 2SLS estimate is slightly negative, statistically insignificant, and imprecisely estimated. In turn, Panel D reports the share of individuals who switched employers, based on LinkedIn profile data. Here, all three estimates (MD, PSM, and 2SLS) show positive effects, although only the first two are statistically significant. The fact that the magnitudes of these effects are quite large suggests that, while they may partially reflect actual job changes, they probably also partly reflect nominal changes—for example, coaches may have encouraged clients to update their LinkedIn profiles, leading to more reported employer changes even if the effect on actual changes was smaller.

Figure F.1: Comparison of Outcomes for Subjects With vs. Without Coaching



Notes: The sample includes all Wave 2 respondents with at least one job offer. Main outcomes by coaching treatment status, with 90% CI in brackets. Estimation methods include OLS, Propensity Score Matching (PSM), and Two-Stages Least Squares (2SLS). In Panel A, the outcome is the %-increase in total compensation. Panel B corresponds to the question "How satisfied are you with your decision to negotiate this offer?" Panel C reports the share who accepted and offer, based on responses to the follow-up survey. Panel D reports the share who switched employers within 12 months of the baseline survey, based on data from LinkedIn profiles.

Table F.1: Baseline Characteristics of Subjects by Coaching Service Uptake

	Full Sample Used Coaching Service?			Follow Up Sample Used Coaching Service?		
	(1) No	(2) Yes	(3) P-Val	(4) No	(5) Yes	(6) P-Val
Share Currently Employed	70.85	67.97	0.282	68.18	66.23	0.592
	(1.297)	(2.384)		(1.913)	(3.118)	
Current/Recent Base Compensation ($1000s)	161.44	167.67	0.246	159.53	167.31	0.320
	(2.863)	(2.860)		(4.659)	(3.648)	
Years of Experience	7.59	7.74	0.558	6.99	7.60	0.079
	(0.131)	(0.213)		(0.189)	(0.271)	
Share of Graduate or Professional Degree	50.98	56.77	0.047	48.99	54.98	0.123
	(1.427)	(2.531)		(2.053)	(3.281)	
Share Female	20.37	21.35	0.679	17.88	20.78	0.337
	(1.150)	(2.094)		(1.575)	(2.675)	
Share White	38.11	41.99	0.176	35.76	47.37	0.002
	(1.391)	(2.532)		(1.975)	(3.314)	
Share Asian	47.07	43.75	0.255	51.18	39.39	0.002
	(1.425)	(2.535)		(2.053)	(3.222)	
Share with Baseline Offer	31.27	39.58	0.003	41.58	39.83	0.646
	(1.323)	(2.499)		(2.024)	(3.228)	
Observations	1,228	384		594	231	

Notes: Average characteristics by coaching service take-up. Columns (1) and (2) include the full Wave 2 sample, while columns (3) and (4) includes those who responded to follow-up and had at least one offer.

G Additional Facts about Negotiations

In Section 5.3, we present a series of empirical facts that validate the theoretical model. Here, we provide additional details on these facts.

The results are presented in Figure G.1. Panel A shows that individuals negotiate different subsets of the offers they receive, and that they are more likely to negotiate the offer with the most favorable initial terms rather than the one with the least favorable terms. For simplicity, this panel focuses on a subset of individuals who were not employed at baseline. On the left side of the panel, we see that among individuals with exactly one offer, approximately 53% attempted to negotiate that offer. In contrast, the right side of the panel displays data for individuals who received exactly two offers. As expected, these individuals are much more likely to negotiate at least one of the offers. In the context of our model, this may be because having a fallback option makes individuals feel safer about initiating negotiations. Indeed, 54% of individuals in this group chose to negotiate exactly one of the two offers. Importantly, models of Bertrand-like competition predict that individuals should always negotiate the offer with the lowest initial terms, asking the employer to match the higher offer. This prediction is not supported by the data: individuals are more likely to negotiate the higher offer than the lower one. This behavior is consistent with our model, in which individuals may choose to negotiate the more attractive offer while using the other as a fallback.

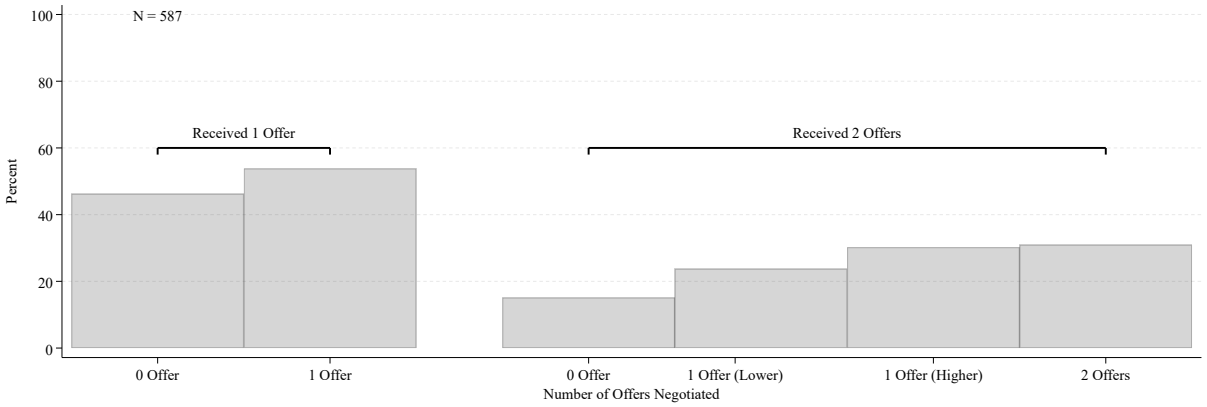
Panel B of Figure G.1 shows that, as a result of negotiations, the gap between offer terms becomes more dispersed. This panel focuses on individuals who were not employed at baseline and received exactly two offers. The solid bars depict the distribution of the difference in initial compensation between the two offers received. The hollow bars represent the difference between the initial terms of the higher offer and the final terms of what was initially the lower offer. According to models of Bertrand-like competition, the hollow bars should shift toward zero relative to the solid bars—that is, individuals should be able to negotiate the lower offer upward to match, at least partially, the terms of the higher offer, thereby narrowing the compensation gap between the two offers. However, we find no evidence of this pattern. On the contrary, the gap between the higher and lower offers tends to widen. This is partly explained by the finding in Panel A: individuals are more likely to negotiate the offer that already has the highest initial terms. To the extent that these negotiations are successful, the most generous offer becomes even more favorable, further increasing the dispersion.

Lastly, Panel C of Figure G.1 shows that, consistent with the hypothesis that employees have bargaining power, negotiation attempts are often effective in improving compensation terms. Specifically, among those who attempted to negotiate, 44% secured improved terms, with an average increase of 12% over the initial offer. In contrast, compensation gains were considerably more modest among individuals who did not attempt to negotiate.

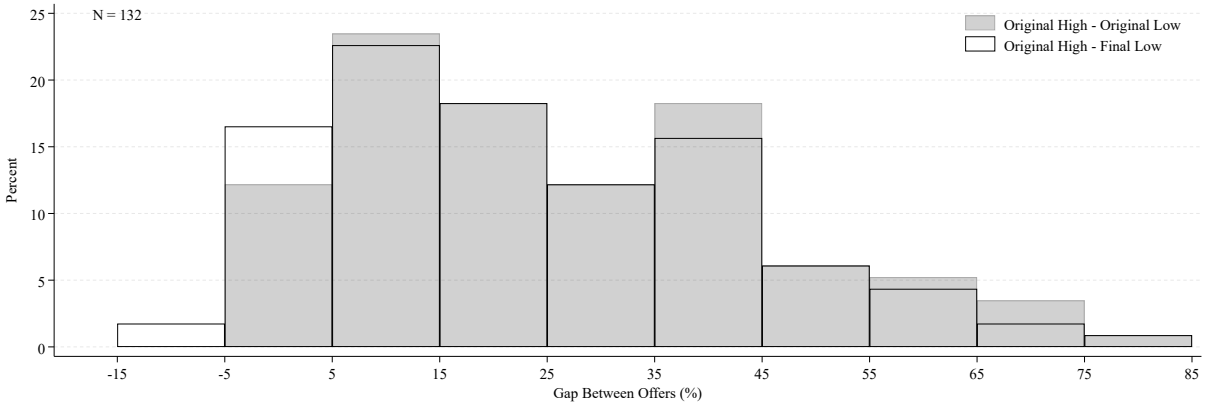
In our forecast survey with academic experts, we also elicited predictions about some of the non-experimental findings discussed above. As detailed in Supplement H, the bargaining facts we document above run counter to the predictions of the majority of experts, most of whom appear to have aligned their expectations with those of Bertrand-like bargaining models.

Figure G.1: Some Basic Facts about Negotiations

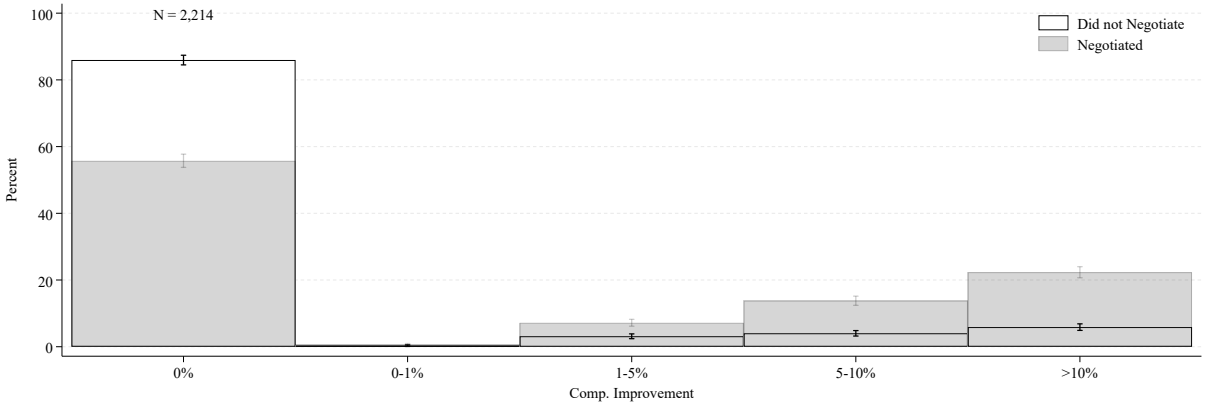
PANEL A: Prob. of Negotiating



PANEL B: Gaps between Offers



PANEL C: Offer Improvement by Negotiation



Notes: Panel A restricts the sample to survey participants who were not currently employed and had one or two offers. Panel B further restricts the sample to participants with exactly two offers. Panel C includes all observations on job offers.

H Expert Forecast Survey

H.1 Survey Design

To assess whether the results of the experiment were surprising or predictable, we conducted a forecast survey with a sample of academic experts. The full survey instrument is provided in Supplement N. The survey begins by summarizing key details of the field experiment that experts would likely find relevant for forming their forecasts. This includes descriptions and screenshots of the treatment messages, participant characteristics, and precise definitions and baseline levels of the main variables of interest.

We randomized the order in which experts were exposed to the treatment arms: half of the experts saw the encouragement treatment first, and half saw the coaching treatment first. Given that academic respondents have limited time, the survey was kept short. For that reason, we elicited beliefs about the treatment effects on two primary outcomes: negotiation attempts and compensation gains. Additionally, we included a third outcome—the likelihood that a subject changes employers. For each outcome, we presented the average level observed in the control group and asked experts to forecast the corresponding average in the treatment group. Beyond these main elicitations, we included a few additional questions. For example, we asked experts to rate their confidence in their forecasts, to indicate whether they expected heterogeneous effects by gender, and we included an optional open-ended question where they could explain their reasoning.

There are a couple of details about the design of the forecast survey that deserve further discussion. First, we chose to elicit expert predictions about the effects of the coaching services in Wave 2. In making these forecasts, respondents are implicitly being asked to predict the price elasticity of demand for the service. However, assessing experts' knowledge of this elasticity was not our primary objective. To assist them, we provided the following message: "Due to this significant discount, the treatment has a notable effect on the take-up of the service. In the control group, 10% of the subjects signed up for the coaching service, while in the treatment group, around 30% signed up for the service."[80] Accordingly, in Figure H.2, we scale both the experimental estimates and the expert predictions from Intention-to-Treat effects of the coaching treatment to Treatment-on-the-Treated effects of the coaching service. That

[80] Because data collection was still ongoing at the time, this approximate 20 pp effect on take-up was based on preliminary data and does not exactly match the final estimated effect of 24.1 pp, as reported in Panel A of Figure 4.

said, the conclusions are similar if we instead focus on Intention-to-Treat effects.

At the end of the survey, we included a few questions about non-experimental and theoretical scenarios. First, we presented experts with a hypothetical case in which an unemployed worker receives two offers from different employers, each with a different level of initial total compensation. We asked whether the worker would negotiate with the employer offering the higher initial offer, and whether the gap between the offers would increase or decrease following negotiations. Finally, we introduced a hypothetical policy that shifts the equilibrium such that all workers always attempt to negotiate. We then asked experts whether they believed workers would be better or worse off under such a policy—and posed the same question regarding employers.

H.2 Results

We recruited experts for the prediction survey via email invitations sent to approximately 500 researchers with publications related to the topic, consisting almost entirely of labor economists. The invitations were sent on November 11, 2024, and most responses were collected within a couple of weeks. To ensure unbiased forecasts, we included a question asking whether the respondent had seen a presentation of the results or discussed them with any of the coauthors. We excluded one respondent who reported having already seen the study. The final sample consists of 68 experts. All respondents identified as economists, and 98.5% identified as Professors.

The first key finding is that most expert respondents did not feel confident in their predictions for either the encouragement or coaching treatments. Panel A of Figure H.1 shows the distribution of confidence levels. For the encouragement treatment, 70.0% reported being not at all confident or only slightly confident, and none reported being very or extremely confident. Similarly, for the coaching treatment, 64.7% expressed low confidence, and again, no one reported feeling extremely confident. Our preferred interpretation of this lack of confidence is that there were no prior studies featuring interventions closely resembling those in our experiment, leaving experts with little empirical basis for their predictions. Consistent with this view, in the optional open-ended responses, experts did not reference specific studies as benchmarks. Instead, they referred to broader concepts—such as the general effects of nudges—to justify their predictions.

The second finding is that there is no consensus among experts regarding the predicted

effects of either the encouragement or coaching treatments. Figure H.2 displays the distribution of predicted treatment effects. The top three panels correspond to the encouragement treatment: Panel A shows forecasts for negotiation rates, Panel B for the percent improvement in compensation terms, and Panel C for the probability of switching employers. Panels D–F mirror Panels A–C but report forecasts for the coaching treatment instead of the encouragement treatment. The main takeaway is that expert predictions are highly dispersed across experts. For the same treatment and outcome, some experts expected no effect at all, while others predicted very large effects. Moreover, each panel of Figure H.2 includes a vertical line representing the actual treatment effect observed in the experiment, with shaded bands indicating the 90% confidence interval. In every panel, a substantial share of experts missed the mark. Most notably, experts predicted that the coaching treatment would have a much stronger impact on negotiation rates and compensation gains than the encouragement treatment. In reality, we observe the opposite pattern.

According to the wisdom of the crowd, the average forecast across all experts may be more informative than individual predictions. To examine this possibility, each panel of Figure H.2 includes a vertical dashed red line representing the average expert forecast. However, even after averaging across all experts, notable discrepancies remain between the forecasts and the actual experimental findings. For example, the average predicted effect is substantially larger for the coaching treatment than for the encouragement treatment—whereas, in reality, we find the opposite.

Panels B and C of Figure H.1 show that not all of the findings were surprising to experts. As shown in Panel B, although there is no clear consensus, the most common response was that the treatments would have stronger effects for women than for men—consistent with our results. Similarly, Panel C shows that a strong majority of experts predicted that the treatments would not lead to backlash, which also aligns with our findings.

Panels D and E of Figure H.1 present forecasts related to the non-experimental findings. Panel D shows that, in the first hypothetical scenario, experts predicted that, on average, 45% of employees would negotiate with the employer offering higher initial total compensation. This is substantially lower than the observed share of 76%. Panel E shows that, in the second hypothetical scenario, 56% of experts expected the gap in total compensation to decrease after negotiations, whereas in our data, 73.5% of participants who had two job offers experienced an increase in the compensation gap following negotiation. Notably, in both scenarios, expert

predictions align with the logic of models of Bertrand-like competition, suggesting that many experts may have explicitly drawn on these models when forming their forecasts.

Lastly, Panel F of Figure H.1 shows the forecasts related to the theoretical question. As some experts noted in the open-ended question, one could construct models to justify predictions in either direction. However, the goal of this question was to elicit experts' gut intuition about whether inducing more negotiations would be beneficial for employees or employers. While there was no clear consensus, the most common responses were that increased negotiation would make employees better off and employers worse off—aligning with the predictions of our theoretical model.

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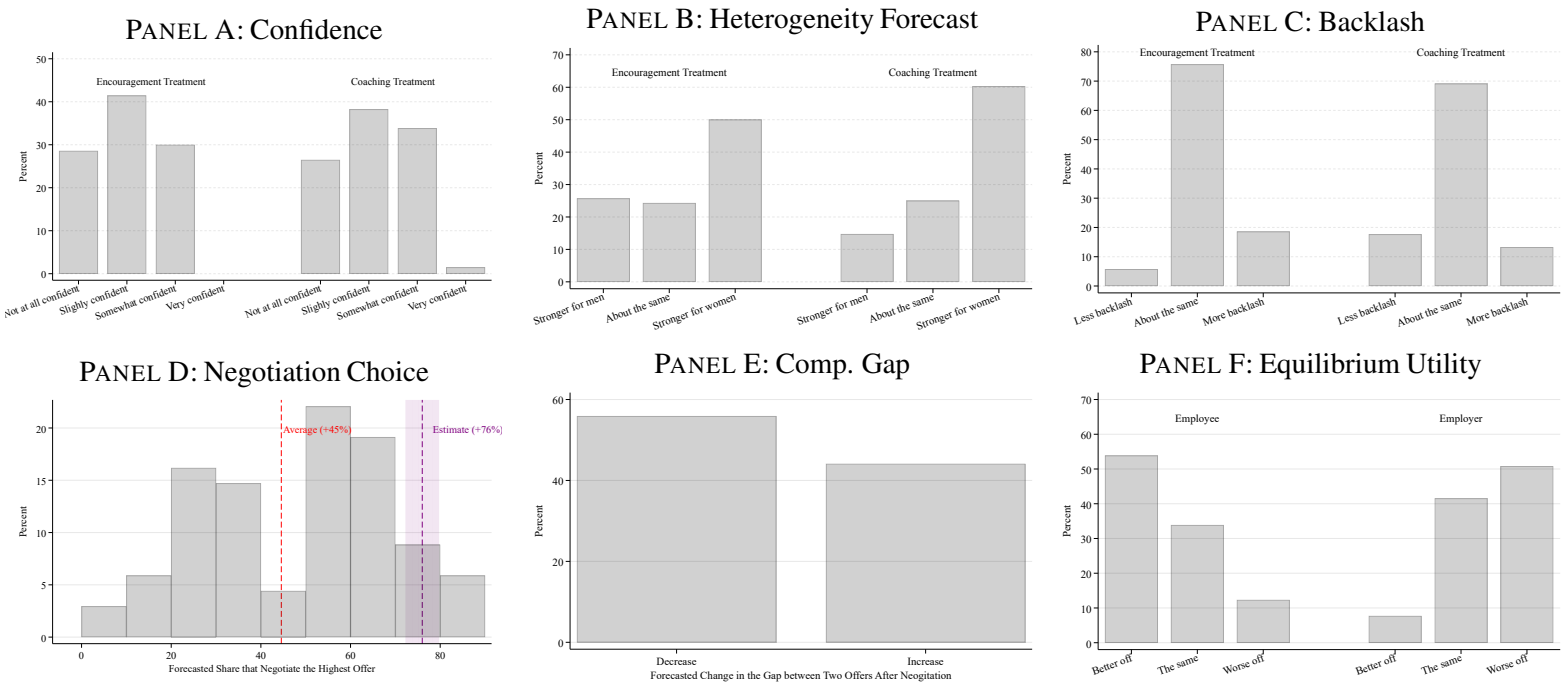
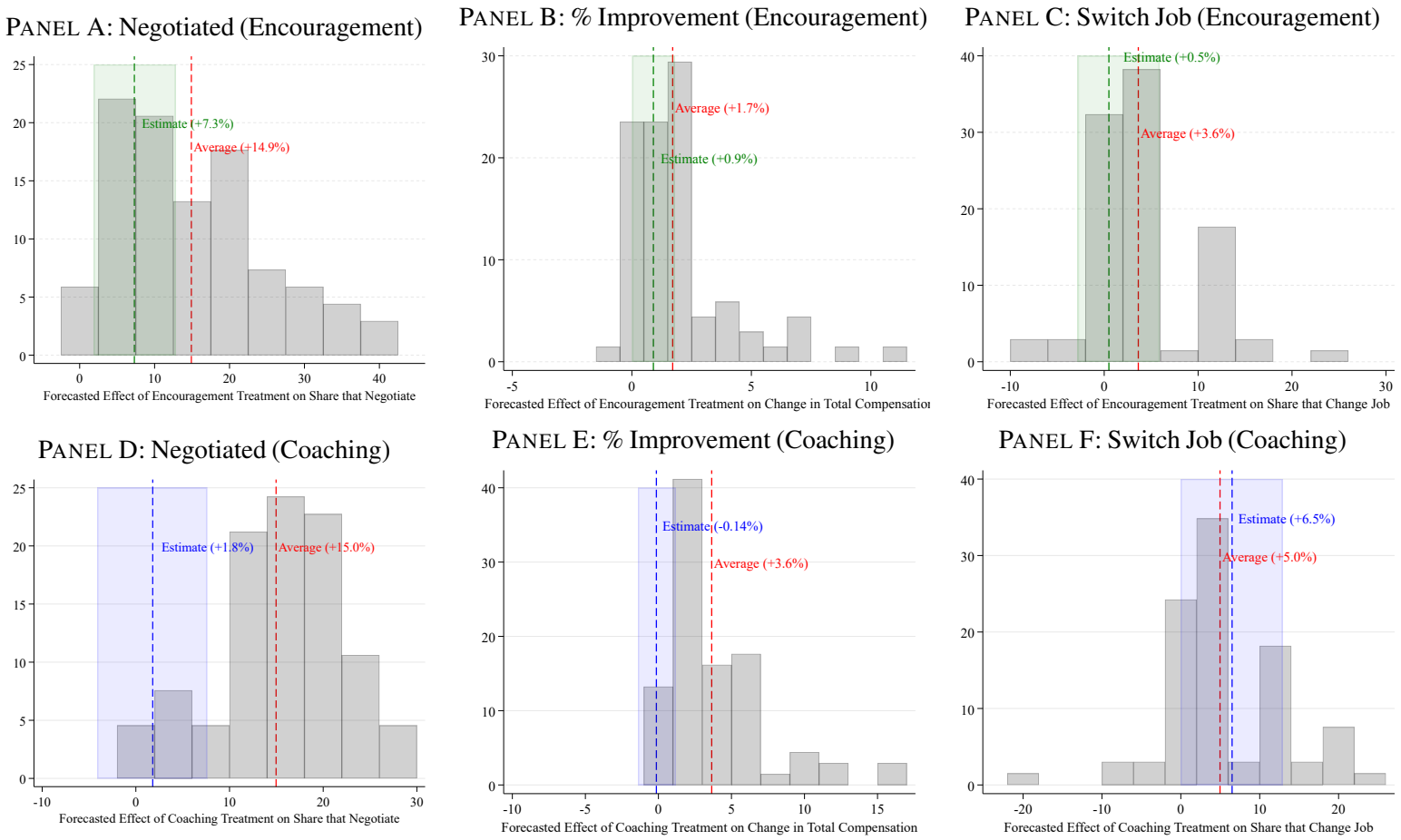
Figure H.1: Forecast Results



Notes: N = 68. Panel A presents the forecast certainty for each treatment arm. Possible answers are *Not confident at all, Slightly confident, Somewhat confident, Very confident* and *Extremely confident*. Panel B shows the predicted effectiveness of treatments by gender. Panel C presents the forecast results on the level of backlash from employers when attempting to negotiate. In Panel A–C, the leftmost set of bars are for the encouragement treatment and the rightmost set of bars are for the coaching treatment. Panel D is a histogram of the forecasted share of employees whonegotiated their highest offer. Our corresponding study findings are denoted by the purple line. Panel E shows the forecasted gap in final compensation after negotiation, relative to initial offers (increased vs. decreased). Panel F displays the expected welfare change for employees and employers under a policy that eliminates risk in negotiating. The leftmost three bars are for employee welfare and the rightmost bars are for employer welfare.

Figure H.2: Treatment Effect Forecast



Notes: N = 68. Histograms of the responses to survey questions about the treatment effects on outcomes. Panels A–C correspond to the encouragement treatment, and Panels D–F correspond to the coaching treatment. Panel A and D presents the forecasted share of subjects who attempted to negotiate. Panel B and E shows the forecasted percent change in total compensation. Panel C and F shows the forecasted share of subjects who switched jobs. The summary statistics of the forecast survey are presented with the red dotted line. We display our experimental estimates and 90% confidence interval using dotted lines and shaded bands (green for encouragement treatment and blue for coaching treatment).

I Baseline Survey

Levels.fyi is partnering with academic researchers from Harvard University, UC-Berkeley, and Brown University to understand how to serve you in the job market. This study consists of two surveys – one 10-minute survey today, and a 5-minute follow-up survey that you will receive via email in one month.

Participants who meet three eligibility criteria will receive a $25 Amazon gift card within 10 business days after completing this survey, and another $25 gift card after completing the follow-up survey. Participants may also receive subsidies for coaching services and reports with insights from the study. If you would like to participate, please continue with this confidential 10-minute survey.

Key Information

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

Why am I being invited to take part in a research study? We invite you to take part in a research study because you registered on Levels.fyi website and provided your contact information. Your responses will be shared with Levels.fyi, Harvard University, UC-Berkeley University and Brown University.

What should I know about a research study?

- Someone will explain this research study to you.
- Whether or not you take part is up to you.
- Your participation is completely voluntary.

[81] For completeness, the following instruments include both the encouragement and coaching treatments. In the actual surveys, participants will be randomly assigned to either see or not see these screens.

Are you currently employed?

- ○ Yes
- ○ No

Where are you located?

Location

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What is your age?

- ◯ 20 years old or younger
- ◯ 21-25 years old
- ◯ 26-30 years old
- ◯ 31-35 years old
- ◯ 36-40 years old
- ◯ 41-45 years old
- ◯ 46-50 years old
- ◯ 51-55 years old
- ◯ 56-60 years old
- ◯ More than 60 years old

What is the highest level of education you have completed?

- ◯ Some high school or less
- ◯ High school diploma or GED
- ◯ College degree
- ◯ Graduate or professional degree

Do you already have an offer from another employer?

- ◯ Yes
- ◯ No

Are you actively searching for a new job? (e.g. applying to jobs and doing interviews)

- ○ Yes
- ○ No

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What do you think is the probability that you receive a job offer in the next 30 days?

- ○ 0%-10%
- ○ 10%-20%
- ○ 20%-30%
- ○ 30%-40%
- ○ 40%-50%
- ○ 50%-60%
- ○ 60%-70%
- ○ 70%-80%
- ○ 80%-90%
- ○ 90%-100%

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What are the details of your most recent employment?

Company

Job title

Which state was the job in?

State

Was the work in person or remote?

- ○ Fully in-person
- ○ Partly remote (hybrid or other)
- ○ Fully remote

How many years of work experience do you have?

Years of experience

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What was your **annual** base salary?

$

What was the value of your average annual stock-based compensation in the offer?

(i.e., your expected annual earnings in Restricted Stock Units or Stock Options. If the stock-based compensation change from year to year, please report the per-year average.)

$

What was your average annual bonus compensation?

$

At the time you were first hired by [Recent Employment Company], did you negotiate, or attempt to negotiate, compensation terms?

- ◯ Yes
- ◯ No

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Since you have received multiple offers, we would like to ask you a few questions specifically about the offer you consider your favorite.

What are the details of the offer?

Company

Job title

Is the offer written or verbal?

- ◯ Written
- ◯ Verbal

Which state is the offer in?

State

Is the work in person or remote?

- ◯ Fully in-person
- ◯ Partly remote (hybrid)
- ◯ Fully remote

Do you have a deadline to make a decision?

- ◯ Yes
- ◯ No

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What is the **annual** base salary in the offer?

$

What is the value of your average annual stock-based compensation in the offer?

(i.e., your expected annual earnings in Restricted Stock Units or Stock Options. If the stock-based compensation change from year to year, please report the per-year average.)

$

What is the average annual bonus compensation in the offer? (excluding sign-on or relocation bonuses)

Enter "0" if there is no annual bonus

$

What is the relocation bonus in the offer?

Enter "0" if there is no relocation bonus

$

What is the year 1 sign-on bonus in the offer?

Enter "0" if there is no year 1 sign-on bonus

$

What is the year 2 sign-on bonus in the offer?

Enter "0" if there is no year 2 sign-on bonus

$

Did [Company Name] inquire about your previous salary or salary expectations before presenting the original terms of the offer?

- ◯ Yes
- ◯ No

Did you provide [Company Name] with any information regarding your previous salary or salary expectations?

- ○ Yes
- ○ No

Have you already negotiated, or attempted to negotiate, these compensation terms with [Company Name]?

- ◯ Yes
- ◯ No

How did the compensation terms of the offer change as a result of the negotiation?

- ◯ The compensation was reduced
- ◯ The compensation did not change
- ◯ The compensation was increased by 1%-5%
- ◯ The compensation was increased by 5%-10%
- ◯ The compensation was increased by 10%-15%
- ◯ The compensation was increased by 15%-20%
- ◯ The compensation was increased by more than 20%

Did the *non-compensation terms* (e.g. benefits, remote work options) of the offer improve as a result of the negotiation?

- ◯ Yes
- ◯ No

What were the original compensation terms of the offer (i.e., prior to your negotiation attempt)?

Base (annual)	$
Stock (annual)	$
Bonus (annual)	$

Have you ever received any training or information on how to negotiate? Please select all that apply.

- [ ] I hired a negotiations coach
- [ ] I took a course on negotiations
- [ ] I read a book on negotiations
- [ ] I received mentorship on negotiations from a colleague
- [ ] I read an article on negotiations

Do you have past experience negotiating your compensation?

- ◯ Yes
- ◯ No

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What percent of people do you think try to negotiate the compensation terms after receiving a job offer?

- ○ 0%
- ○ 1%-10%
- ○ 11%-20%
- ○ 21%-30%
- ○ 31%-40%
- ○ 41%-50%
- ○ 51%-60%
- ○ 61%-70%
- ○ 71%-80%
- ○ 81%-90%
- ○ 90%-100%

Among individuals who attempt to negotiate, what percentage of them do you think were successful? In other words, what percentage got at least some of what they asked for?

- ◯ 0%
- ◯ 1%-10%
- ◯ 11%-20%
- ◯ 21%-30%
- ◯ 31%-40%
- ◯ 41%-50%
- ◯ 51%-60%
- ◯ 61%-70%
- ◯ 71%-80%
- ◯ 81%-90%
- ◯ 90%-100%

How comfortable would you feel negotiating compensation terms of an offer?

- ○ Very uncomfortable
- ○ Uncomfortable
- ○ A little uncomfortable
- ○ Not at all uncomfortable

On a scale of 1 to 10, how confident do you normally feel when you have to negotiate?

1 - Not at all confident 2 3 4 5 6 7 8 9 10 - Extremely confident

○ ○ ○ ○ ○ ○ ○ ○ ○ ○

On a scale of 1 to 10, where 1 represents being extremely socially introverted and 10 represents being extremely socially extroverted, how would you rate yourself?

1 - Extremely introverted 2 3 4 5 6 7 8 9 10 - Extremely extroverted

Next, some participants of this survey will be chosen to receive information related to the previous questions. Please continue to the next screen to find out if you will be randomly selected to receive this information.

According to a survey conducted by Fidelity Investments, around 42% of Americans attempted to negotiate the initial offer that they received. There is evidence suggesting that people should attempt to negotiate even more often.

Don't feel guilty about negotiating. While it's natural to feel guilty or afraid about it, companies expect you to negotiate. In some cases, the person you are negotiating with has been hired precisely for that job. They'd be happy if you attempt to negotiate – that's what they were hired for! And negotiating the compensation can make the employment relationship stronger.

The success rate is quite high. According to the Fidelity survey, 85% of Americans who attempted to negotiate the compensation terms of their offer were successful – that is, they got at least some of what they asked for.

The gains from negotiating add up quickly. According

to the Harvard Business School Negotiations Course, by age 65, the salary gap between those who did and did not negotiate when they were 30 rises to more than $30,000, totaling $1.6 million gains over the working years.

Levels.fyi offers a negotiation coaching service that includes an initial 30 minute 1-on-1 call with your coach, up to 2 follow up calls, and unlimited email support for a duration of up to 90 days. Your coach will be a tech recruiter from the industry who has experience approaching these conversations from the other side. The team of recruiters has negotiated thousands of offers and regularly increase offers by $30k+. They will help you strategize the best negotiation process for your situation and provide guidance through your interactions. The service costs $650.

For more information about the service, visit our FAQ page

Were you aware of this coaching service?

- ◯ Yes
- ◯ No

Below you are presented with 6 hypothetical scenarios. In each scenario, you will be presented a choice between receiving free coaching services OR extra money as part of your reward for responding to the survey.

We will randomly choose 10 survey respondents. If you are one of these 10 lucky respondents, one of the 6 scenarios will be randomly chosen to be implemented. As a result, it is in your best interest to respond honestly to these scenarios.

Please make your choices below, and in the next screen you will find out if your responses will be implemented or will remain hypothetical.

Between the following two options, which one would you prefer?

- ◯ Coaching service
- ◯ $100 Amazon gift card

Between the following two options, which one would you prefer?

- ◯ Coaching service
- ◯ $200 Amazon gift card

Between the following two options, which one would you prefer?

- ◯ Coaching service
- ◯ $300 Amazon gift card

Between the following two options, which one would you prefer?

- ◯ Coaching service
- ◯ $400 Amazon gift card

Between the following two options, which one would you prefer?

- ◯ Coaching service
- ◯ $500 Amazon gift card

Between the following two options, which one would you prefer?

- ◯ Coaching service
- ◯ $600 Amazon gift card

You have NOT been randomly selected among the 10 participants who will have one of their 6 scenarios implemented. As a result, your choices in the 6 scenarios remain hypothetical. Please continue with the survey.

You have been randomly selected to receive a special offer for negotiation coaching services.

Typically coaching services require you to pay $650 up front, but we are giving you the opportunity to sign up at no cost. You will be paired with a coach that helps you through your negotiation process. If the total compensation increase for the first year is greater than $1,200 after negotiations, you will pay a flat fee of $550 to Levels.fyi. If there is no successful outcome after you have received an offer, no payment will be required. You can see the full terms here.

Do you want to claim your special offer?

- ◯ Yes
- ◯ No

If you select Yes, we will provide instructions to sign up later in the survey. If you select No, you'll continue to the end of the survey. If you select No here, you will *not* be able to claim the offer later.

You have claimed your special offer for negotiations coaching. At the end of the survey, you'll be directed to a scheduling tool to schedule your initial 1-on-1 call with a coach.

Do you plan to negotiate the terms of any current offers or offers that you may receive soon?

- ◯ Yes, almost surely
- ◯ Yes, probably
- ◯ Yes, but unlikely
- ◯ No

What's your expectation about what you'd get out of the negotiation?

- ○ My compensation will decrease
- ○ My compensation will not change
- ○ My compensation will increase by 1%-5%
- ○ My compensation will increase by 6%-10%
- ○ My compensation will increase by 11%-15%
- ○ My compensation will increase by 16%-20%
- ○ My compensation will increase by more than 20%

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Please answer the following questions to help inform your negotiations coach.

List any companies you are in interviews with where you do not yet have an offer. For each, include Company Name, Status, Role & Location.

	Interview Details			
	Company	Role	Location	Offer Status
Company 1				
Company 2				
Company 3				
Company 4				
Company 5				
Company 6				
Company 7				

What are your career goals? Include any other information that may be relevant to negotiations.

LinkedIn Profile or Resume URL?

What is your gender?

- ◯ Male
- ◯ Female
- ◯ Other

What is your ethnicity?

- ◯ American Indian or Alaska Native
- ◯ Asian
- ◯ Black or African American
- ◯ Native Hawaiian or Other Pacific Islander
- ◯ White
- ◯ Hispanic/Latino
- ◯ Other

How would you rate your current level of financial stress or need?

- ◯ Very low
- ◯ Low
- ◯ Moderate
- ◯ High
- ◯ Very high

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Recent research on decision making shows that choices are affected by the context in which they are made. Differences in how people feel, in their previous knowledge, experience, and in their environment can influence the choices they make. To help us understand how people make decisions, we are interested in information about you. Specifically, whether you actually take the time to read instructions. If you don't, some results may fail to tell us very much about decision making in the real world. To help us confirm that you have read these instructions, please do not select an answer corresponding to how you are currently feeling, and instead only check the "none of the above" option. Thank you very much.

☐ Interested	☐ Hostile	☐ Nervous
☐ Distressed	☐ Enthusiastic	☐ Determined
☐ Excited	☐ Proud	☐ Attentive

☐ Upset	☐ Irritable	☐ Jittery
☐ Strong	☐ Alert	☐ Active
☐ Scared	☐ Inspired	☐ None of the above

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In your opinion, were the questions in this survey easy or difficult to understand?

- ○ Easy to understand
- ○ Neither easy nor difficult
- ○ Difficult to understand

Did you have any issues with this survey?

Yes	No
○	○

To complete the survey, you must schedule your first session with your coach below, and click the right arrow at the bottom of the page to advance to the final screen. If you have any questions, reach out to us at services@levels.fyi.

You'll receive a $25 amazon gift card within 10 business days. In one month, we will email you a 5-minute follow up survey. Once you complete the follow-up survey, you'll receive a second $25 gift card.

We thank you for your time spent taking this survey. Your response has been recorded.

You'll receive a $25 amazon gift card within 10 business days. In one month, we will email you a 5-minute follow up survey. Once you complete the follow-up survey, you'll receive a second $25 gift card.

J Follow-Up Survey

Levels.FYI is partnering with academic researchers from Harvard University, UC-Berkeley, and Brown University to understand how to serve you in the job market.

We greatly appreciate your involvement and the completion of our initial survey. We hope that you can complete this short (5-minute) follow-up survey. As a token of our gratitude, participants who complete this survey will receive a $25 Amazon gift card. Additionally, participants may also receive reports containing valuable insights derived from the study.

Rest assured that all your responses will be treated with the utmost confidentiality and used solely for research purposes.

Did you use the negotiation coaching service from Levels.fyi?

- ◯ Yes
- ◯ No

On a scale of 1 to 10, where 1 is not helpful at all and 10 is extremely helpful, how would you rate your experience with levels.FYI's negotiation coaching?

1 - Not helpful at all 2 3 4 5 6 7 8 9 10 - Extremely helpful

How many times did you meet with your coach?

- Never
- 1 time
- 2 times
- 3 times
- 4 times or more

How many emails or messages did you exchange with your coach?

- Never

- ○ 1-5 times
- ○ 6-10 times
- ○ 11-15 times
- ○ 16 times or more

On a scale of 1-10, where 1 is very poor and 10 is excellent, how would you rate the overall compatibility and rapport you had with your coach?

1 - Very poor	2	3	4	5	6	7	8	9	10 - Excellent
○	○	○	○	○	○	○	○	○	○

How many positions/companies have you interviewed for?

- ○ None
- ○ 1
- ○ 2
- ○ 3
- ○ 4
- ○ 5 or more

Have you received any offers since then?

- ○ Yes
- ○ No

How many offers have you received since the initial survey?

If you have received 3 or more offers, select 3.

- ◯ 1
- ◯ 2
- ◯ 3

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What are the details of your offer?

Company

Job title

Company location

Is the work in person or remote?

- ◯ Fully in-person
- ◯ Partly remote (hybrid)
- ◯ Fully remote

When did you first receive this offer?

Date:

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We now want to ask you about the **latest terms** of your [Offer Job Title] offer from [Offer Company Name].

What is the **annual** base salary in the offer?

What is the value of your average annual stock-based compensation in the offer?

(i.e., your expected annual earnings in Restricted Stock Units or Stock Options. If the stock-based compensation change from year to year, please report the per-year average.)

$

What is the average annual bonus compensation in the offer? (excluding sign-on or relocation bonuses)

Enter "0" if there is no annual bonus

$

What is the relocation bonus in the offer?

Enter "0" if there is no relocation bonus

$

What is the year 1 sign-on bonus in the offer?

Enter "0" if there is no year 1 sign-on bonus

$

What is the year 2 sign-on bonus in the offer?

Enter "0" if there is no year 2 sign-on bonus

$

What is the average annual bonus compensation in the offer? (excluding sign-on or relocation bonuses)

Enter "0" if there is no annual bonus

$

What is the relocation bonus in the offer?

Enter "0" if there is no relocation bonus

$

What is the year 1 sign-on bonus in the offer?

Enter "0" if there is no year 1 sign-on bonus

$

What is the year 2 sign-on bonus in the offer?

Enter "0" if there is no year 2 sign-on bonus

$

Did you accept this offer?

- ◯ Yes, I accepted the offer
- ◯ No, I declined the offer
- ◯ I have not yet responded to the offer

Do you expect to accept this offer in the future?

- ◯ Yes, almost surely
- ◯ Yes, probably
- ◯ Possibly, but unlikely
- ◯ No

You just told us about the latest terms of the offer. Now, we want to ask you about the original offer – that is, the terms when you first received the offer.

Have the compensation terms changed since you received the original offer?

- ◯ Yes
- ◯ No

Would you say the compensation terms improved since you received the original offer?

- ◯ Yes
- ◯ No

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For latest offer, you reported an annual base salary of $70000. What was is the annual base salary in the **original** offer?

$

For latest offer, you reported an average annual stock-based compensation of $5000. What was the value of your average annual stock-based compensation in the **original** offer?

$

For latest offer, you reported an annual bonus of $20000. What is the annual annual bonus compensation in the **original** offer?

$

For latest offer, you reported a relocation bonus of $0. What is the relocation bonus in the **original** offer?

$

For latest offer, you reported a year 1 sign-on bonus of 0. What is the year 1 sign-on bonus in the **original** offer?

$

For latest offer, you reported a year 2 sign-on bonus of $0. What is the year 2 sign-on bonus in the **original** offer?

$

Did [Offer Company Name] inquire about your previous salary or salary expectations before presenting the original terms of the offer?

- ◯ Yes
- ◯ No

When [Offer Company Name] asked you for your prior salary or salary expectations, did you provide any information?

- ◯ Yes
- ◯ No

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Who did you speak to at [Offer Company Name] regarding the terms of your offer? Select all that apply.

- [ ] Human Resources (HR) Representative
- [ ] Recruiter
- [ ] Hiring Manager
- [ ] Direct Supervisor
- [ ] Other

Have you already negotiated, or attempted to negotiate, these compensation terms with [Offer Company Name]?

- ◯ Yes
- ◯ No

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How satisfied are you with your decision to negotiate this offer?

1 - Not at all Satisfied	2	3	4	5 - Extremely Satisfied
○	○	○	○	○

(Optional) Please explain why:

(Optional) Did you experience, or do you expect to experience, any negative consequences from your negotiation?

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We asked you about the compensation terms. Now we want to ask you about the **non-compensation** terms (e.g. benefits, remote work options). Have the **non-compensation** terms changed since you received the original offer?

- ◯ Yes
- ◯ No

Would you say the non-compensation terms improved since you received the original offer?

- ◯ Yes
- ◯ No

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Which non-compensation terms have improved? Select all that apply.

- [ ] Remote work options
- [ ] Number of days of PTO
- [ ] Role/job/title
- [ ] Other

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What is the number of additional days of PTO per year that are now part of your offer?

If you had to put a price on these additional days of PTO, what annual price would you set?

$

What is the number of additional days of remote work per week that are now part of your offer?

If you had to put a price on these additional days of remote work, what annual price would you set?

$

If you had to put a price on the improvements to your role/job/title, what annual price would you set?

$

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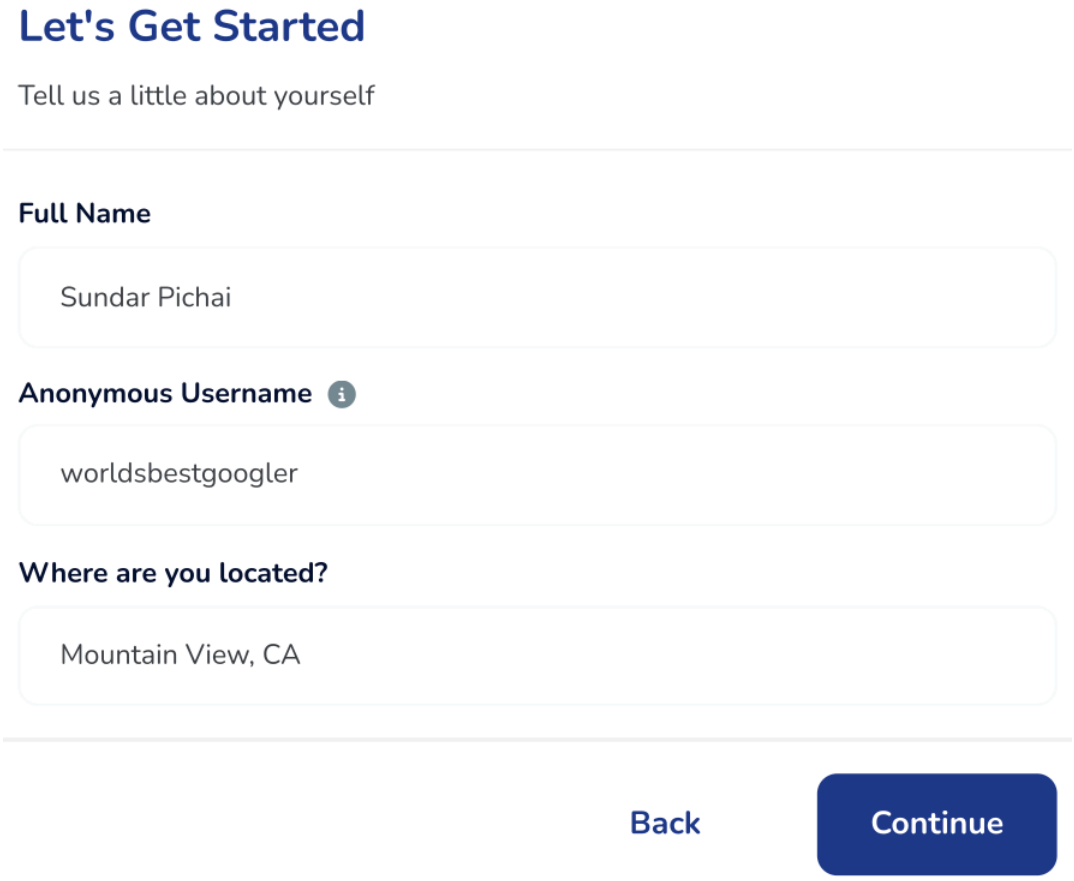
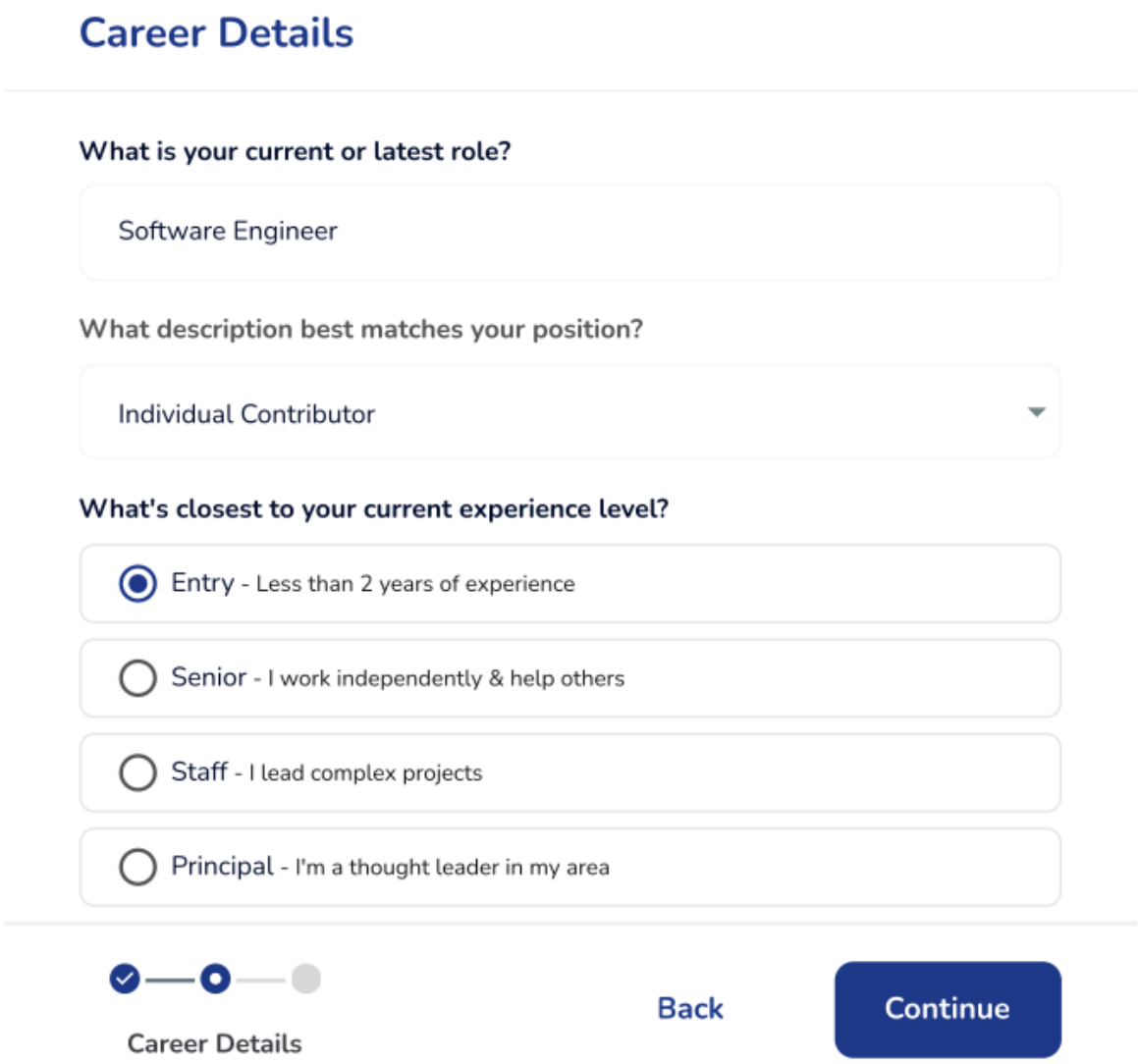
On a scale of 1 to 5, with 1 being the lowest and 5 being the highest, how well do you think the job [Offer Job Title] at [Offer Company Name] aligns with your skills, interests, and career goals?

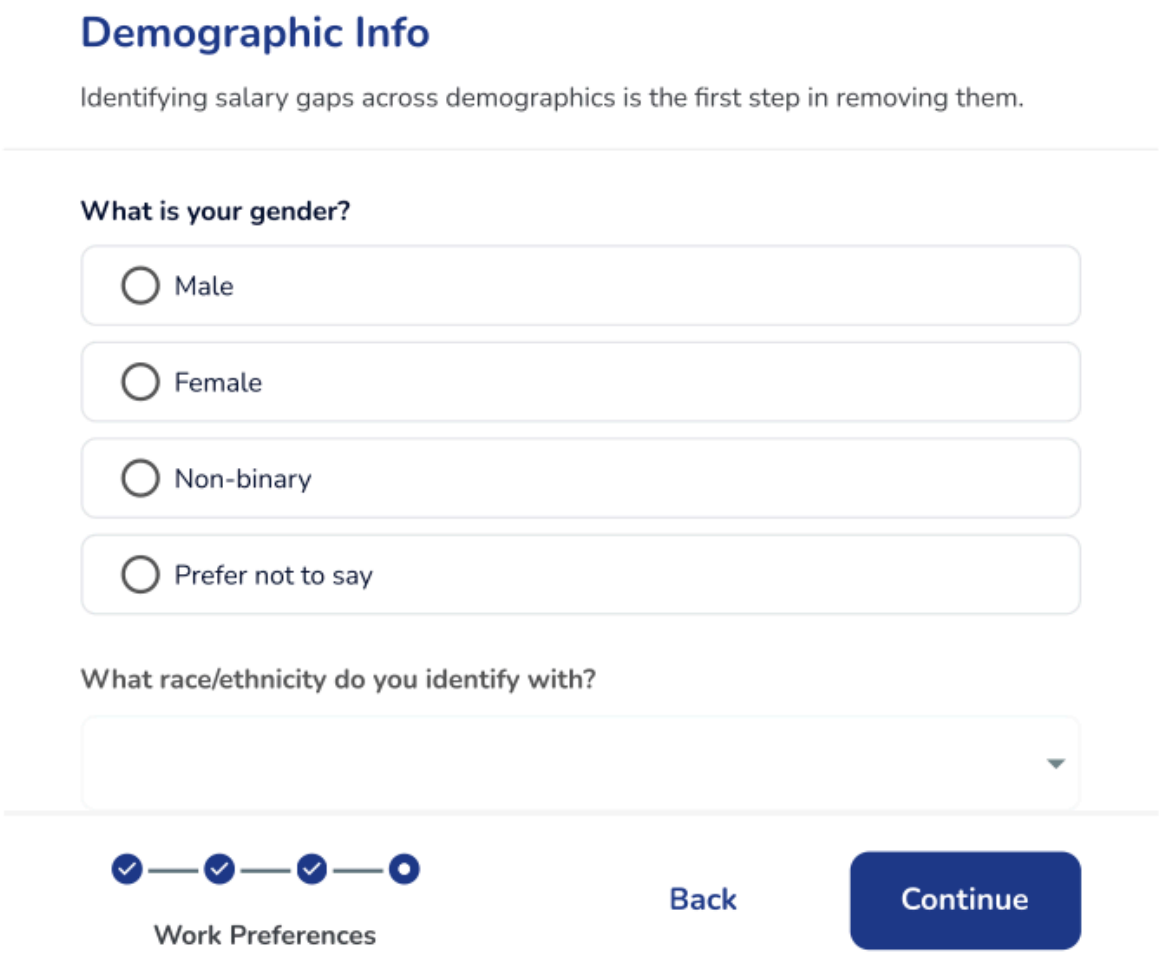
1 - Lowest	2	3	4	5 - Highest
○	○	○	○	○

We thank you for your time spent taking this survey.
Your response has been recorded.

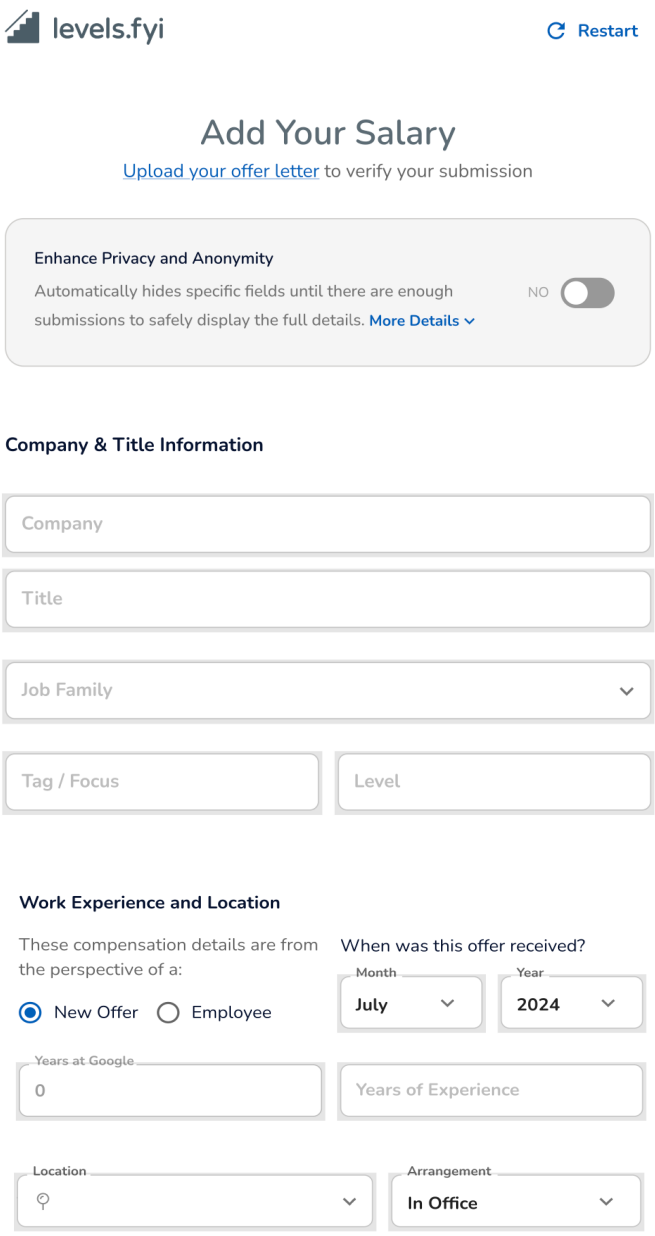
K Levels.fyi Registration Form

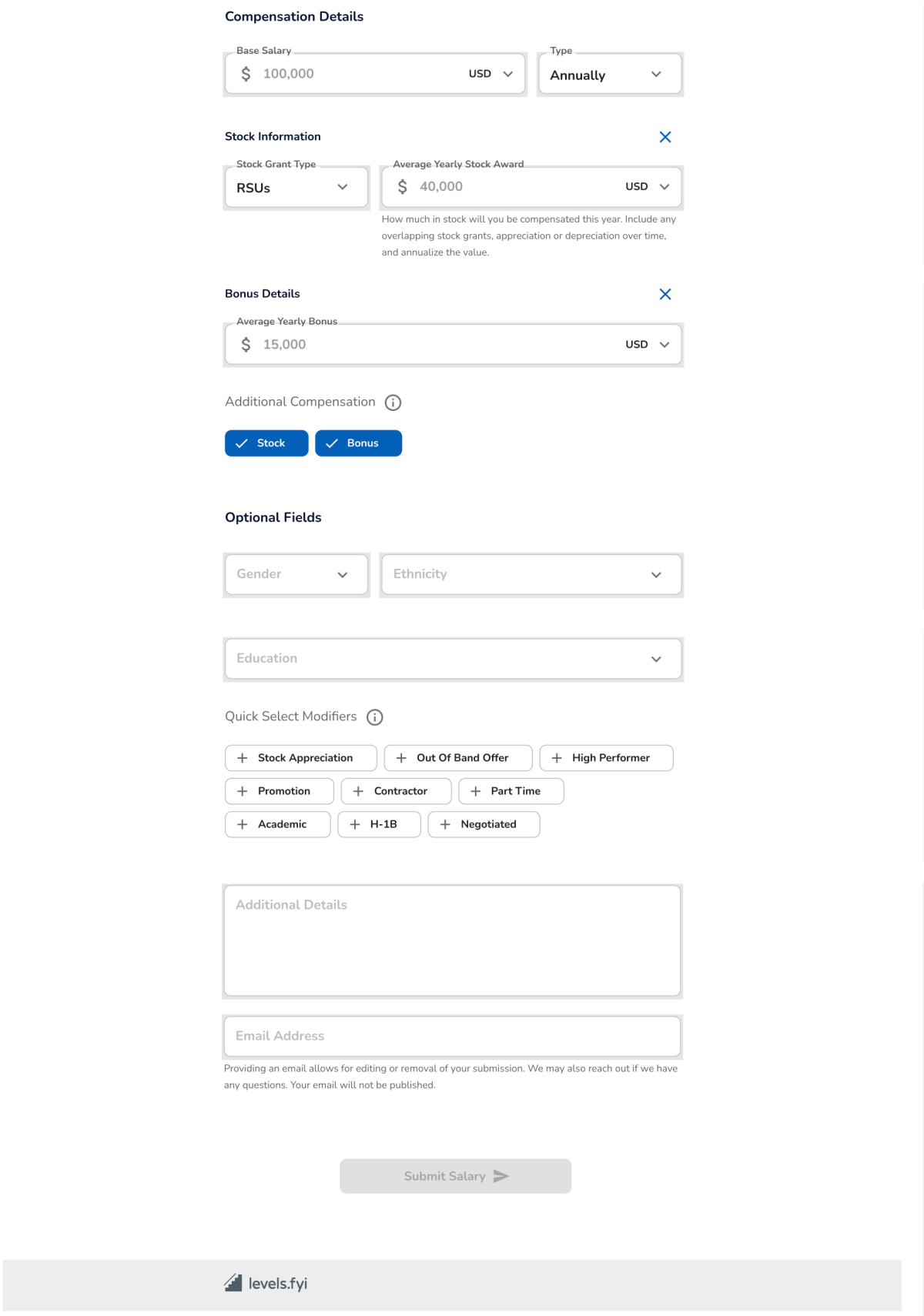



Career Details
What is your current or latest role?
Software Engineer
What description best matches your position?
Individual Contributor
What's closest to your current experience level?
Entry - Less than 2 years of experience
Senior - I work independently & help others
Staff - I lead complex projects
Principal - I'm a thought leader in my area
Career Details
Back
Continue


Demographic Info
Identifying salary gaps across demographics is the first step in removing them.
What is your gender?
Male
Female
Non-binary
Prefer not to say
What race/ethnicity do you identify with?
Work Preferences
Back
Continue

L Levels.fyi's Manual Salary Submission Form




Compensation Details
Base Salary
$ 100,000 USD
Type
Annually
Stock Information
Stock Grant Type
RSUs
Average Yearly Stock Award
$ 40,000 USD
How much in stock will you be compensated this year. Include any overlapping stock grants, appreciation or depreciation over time, and annualize the value.
Bonus Details
Average Yearly Bonus
$ 15,000 USD
Additional Compensation
Stock
Bonus
Optional Fields
Gender
Ethnicity
Education
Quick Select Modifiers
Stock Appreciation
Out Of Band Offer
High Performer
Promotion
Contractor
Part Time
Academic
H-1B
Negotiated
Additional Details
Email Address
Providing an email allows for editing or removal of your submission. We may also reach out if we have any questions. Your email will not be published.
Submit Salary
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M Survey Invitation Messages

Dear Jane Doe,

Levels.fyi is partnering with academic researchers from Harvard University, UC-Berkeley, and Brown University to understand how to serve you in the job market. We would greatly appreciate your participation.

This is intended for people actively exploring job opportunities.

Participation entails a 15-minute survey split into two parts – one 10-minute survey today, and a 5-minute follow-up survey that you will receive via email in one month. *If you qualify,* you will receive a $50 gift card after completing the follow-up survey. You may also receive subsidies for coaching services and reports with insights from the study. Screening questions to determine if you qualify will take no more than 2 minutes.

If you would like to participate, please continue with this confidential 10-minute survey here.

You can alternatively participate by copy-pasting the URL: https://hbs.qualtrics.com/jfe/form/SV_6JMPlYM5Lj8PEsS.

Sincerely,

Zuhayeer Musa and Zaheer Mohiuddin, Levels.fyi Co-founders
Zoe Cullen, Harvard Business School Assistant Professor
Ricardo Perez-Truglia, UC-Berkeley Associate Professor
Bobak Pakzad-Hurson, Brown University Assistant Professor

Follow the link to opt out of future emails: link.

Hi there,

It's Zoe from Harvard Business School. Thanks for filling out our initial survey with Levels.fyi a month or so back. Now that some time has passed, we wanted to check back in on some of your job prospects through a final follow up survey. This is the second part to what you had initially filled out and will help us understand how the job market is trending.

Please use the link here.

Rest assured that all your responses will be treated with the utmost confidentiality and used solely for research purposes. Your individual responses will not be shared nor used for any commercial purpose.

As a token of our gratitude, we'll also send over a $50 Amazon gift card after you complete the survey. Thanks for your support and feel free to email me with any questions! Thank you,

Zoe

Follow the link to opt out of future emails: link.

N Expert Forecast: Survey Instrument

The survey will take approximately 5 minutes to complete. Your responses will remain completely anonymous. Participation is voluntary, and you may withdraw from the survey at any time.

Please click “I Agree” when you are ready to begin the survey.

Yes, I would like to take the survey

→

Background

To study salary negotiations, we conducted a panel survey of individuals in the U.S. technology sector who are active in the job market. To recruit subjects, we partnered with a company that offers a range of services to tech workers through its website and app, most prominently data on market salaries.

There are two treatment arms:

- Information Treatment: a light-touch intervention that encourages subjects to consider negotiating the offer terms.
- Coaching Service Treatment: a deep discount for a negotiation coaching service.

The subjects were recruited in two waves during 2023 and 2024, with each wave using a different recruitment strategy tailored to one of the treatment arms.

We will ask you to forecast the results for each of the two treatment arms. The order in which the arms appear in your survey has been chosen at random.

→

Main Outcomes of Interest

The follow-up survey asks a comprehensive set of questions about the offers individuals received and the outcomes of each. Participants must also provide their LinkedIn profile to join the study, which we use to measure job mobility, even for those who do not respond to the follow-up survey.

We ask you to please forecast the effects on a selected set of outcomes:

- Negotiation Attempt: conditional on receiving an offer, the probability that the individual attempts to negotiate the compensation terms.
- Compensation Improvement: conditional on receiving an offer, the %-change in total cash compensation from the initial offer terms to the final terms.
- Change of Employer: the probability of changing employers, measured via LinkedIn.

For individuals with multiple offers, the negotiation attempt is the probability of negotiating with at least one employer, and the compensation improvement corresponds to the maximum improvement across all offers.

→

Information Treatment

The information treatment is a light-touch intervention that encourages subjects to consider negotiating the offer terms.

In the 2023 wave, we recruited subjects who had recently registered an account with the partner company. The average subject in our sample earns around $160,000 per year in total compensation and has 7 years of work experience. Most subjects already have a job but are open to switching employers. The offers are for a variety of positions such as Software Engineer, Product Manager, and Data Scientist, and come from a range of companies including Amazon, Google, and Tesla.

→

The only difference between the control and treatment groups is that the treatment group sees an additional screen, shown below:

According to a survey conducted by Fidelity Investments, around 42% of Americans attempted to negotiate the initial offer that they received. There is evidence suggesting that people should attempt to negotiate even more often.

Don't feel guilty about negotiating. While it's natural to feel guilty or afraid about it, companies expect you to negotiate. In some cases, the person you are negotiating with has been hired precisely for that job. They'd be happy if you attempt to negotiate – that's what they were hired for! And negotiating the compensation can make the employment relationship stronger.

The success rate is quite high. According to the Fidelity survey, 85% of Americans who attempted to negotiate the compensation terms of their offer were successful – that is, they got at least some of what they asked for.

The gains from negotiating add up quickly. According to the Harvard Business School Negotiations Course, by age 65, the salary gap between those who did and did not negotiate when they were 30 rises to more than $30,000, totaling $1.6 million gains over the working years.

→

Next, we ask you to forecast the effects of this treatment arm for each of the three main outcomes described earlier.

Consider individuals who, by the follow-up survey, have received at least one offer. In the control group (who did not see the information treatment), 50% of subjects attempted to negotiate at least one of their offers. In the treatment group (who saw the information treatment), what percent of subjects do you think attempted to negotiate at least one of their offers?

[ ] %

Consider again individuals who, by the follow-up survey, have received at least one offer. In the control group (who did not see the information treatment), the average total compensation changed by +3.5%. In the treatment group (who saw the information treatment), what do you guess was the average change in total compensation?

[ ] %

In the control group (who did not see the information treatment), 40% of subjects changed employers. In the treatment group (who saw the information treatment), what percent of subjects do you think changed employers?

[ ] %

→

On a scale from 1 (Not confident at all) to 5 (Extremely confident), how confident are you in the above predictions?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident

(Optional) We'd love to hear your reasoning behind the predicted effects you shared with us. Could you please explain your thought process for the predictions you made?

→

How do you believe the treatment's effectiveness will vary between men and women?

Stronger effect for men than women

Stronger effect for women than men

About the same

Compared to the control group, do you expect the treatment group to experience more, less, or the same level of backlash from employers when attempting to negotiate?

More backlash

Less backlash

About the same backlash

→

Coaching Treatment

The coaching service treatment consists of a deep discount for the following service. The partner company offers a negotiation coaching service priced at $1,250 for the mid-level package. This service is designed to equip candidates with the knowledge and confidence to secure the best possible compensation package during the hiring process. Clients are paired with an experienced negotiation coach who provides personalized advice and strategies for negotiating salary and other benefits. Coaching is typically delivered via video calls and email, with support available for up to three months. The service includes a customer guarantee: clients can request a full refund if their total compensation does not increase by at least $2,500.

In the 2024 wave, we recruited subjects directly from the negotiation coaching website. Specifically, the partner company added a button below the “Book Now” option, offering the opportunity to receive a deep discount on the service if they participate in a research study. The average subject in this sample earns around $160,000 per year in total compensation and has eight years of work experience. Most subjects are currently employed but are open to switching jobs. The offers span various positions, such as Software Engineer, Product Manager, and Data Scientist, and come from companies including Amazon, Google, and Tesla.

→

At the end of the survey, the coaching service is described to both the treatment and control groups. However, only the treatment group is shown an additional screen with a special discount: they can book the service for the special price of $250 (an $1,000 discount, or 80% off).*

Due to this significant discount, the treatment has a notable effect on the take-up of the service. In the control group, 10% of the subjects signed up for the coaching service, while in the treatment group, around 30% signed up for the service.

* Among the subjects who signed up for the service in our sample, around 60% qualify for the mid-level package, while the remaining qualify for the senior-level package. The senior-level package costs $2,200. For these individuals, the treatment consisted of a $1,950 discount (or 89% off).

→

Next, we ask you to forecast the effects of the negotiation coaching treatment arm.

Consider individuals who, by the follow-up survey, have received at least one offer. In the control group (who did not receive the discount), 70% of subjects attempted to negotiate at least one of their offers. In the treatment group (who received the discount), what percent of subjects do you think attempted to negotiate at least one of their offers?

[ ] %

Consider again individuals who, by the follow-up survey, have received at least one offer. In the control group (who did not receive the discount), the average total compensation changed by +4%. In the treatment group (who received the discount), what do you guess was the average change in total compensation?

[ ] %

In the control group (who did not receive the discount), 40% of subjects changed employers. In the treatment group (who received the discount), what percent of subjects do you think changed employers?

[ ] %

→

On a scale from 1 (Not confident at all) to 5 (Extremely confident), how confident are you in the above predictions?

Not confident at all

Slightly confident

Somewhat confident

Very confident

Extremely confident

(Optional) We'd love to hear your reasoning behind the predicted effects you shared with us. Could you please explain your thought process for the predictions you made?

→

How do you believe the treatment's effectiveness will vary between men and women?

Stronger effect for men than women

Stronger effect for women than men

About the same

Compared to the control group, do you expect the treatment group to experience more, less, or the same level of backlash from employers when attempting to negotiate?

More backlash

Less backlash

About the same backlash

→

Before taking this survey, have you seen preliminary results for this project?

No

Yes

→

Lastly, we would like to ask you to forecast some non-experimental results.

Consider an unemployed worker with exactly two job offers from different employers, each offering a different total compensation. This worker will attempt to negotiate the compensation with only one of the two offers. What is the probability that this worker will negotiate with the employer who offered the highest initial compensation?

[ ] %

Consider an unemployed worker who receives two job offers from different employers. The two initial offers differ in terms of total compensation. After the worker negotiates, the difference in compensation between the two offers changed. Which of the following is more likely?

- The difference in compensation (in absolute value) between the two offers decreased
- The difference in compensation (in absolute value) between the two offers increased

→

Imagine there is a policy (e.g., banning exploding offers) that eliminates the risk of a job offer being rescinded if a worker attempts to negotiate. In equilibrium under this policy, employees always attempt to negotiate offers. In this new equilibrium, would employees be better off, the same, or worse off?

- Better off
- The same
- Worse off

In this new equilibrium, would employers be better off, the same, or worse off?

- Better off
- The same
- Worse off

→

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

Which of the following describes your current position?

- Professor
- Associate Professor
- Assistant Professor
- Post-Doc
- Researcher
- PhD Student
- Master's Student

Please select your discipline

- Economics
- Management
- Psychology

Sociology

Other

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

Labor Economics

Personnel Economics

Public Economics

Behavioral Economics

Organizational Economics

None of the above

→

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

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

→