

Supplemental Appendix for

“Who Benefits from Online Gig Economy Platforms?”

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The appendix contains: **A.** Details about the survey instrument and recruitment; **B.** Model and estimation details; **C.** Counterfactual calculation details and analysis of a counterfactual minimum wage; **D.** Analysis of buyer online-offline substitution; **E.** Appendix figures and tables.

A. SURVEY DETAILS

A1. Survey protocol

The survey was conducted on one of the leading online labor platforms as of 2023. Respondents were recruited by posting a job with the title “[Job Category]: Take a survey about your work”, where [Job Category] was one of Administrative Support, Web Development, Design, or Web Research. The posting contained a description of the study, a consent form, and a link to the Qualtrics site hosting the survey. The consent form made clear that the survey responses would be linked to the worker’s public-facing profile data on the online labor market. Upon providing consent and completing the survey, workers became eligible for a payment of \$6 that was processed through the platform. Payment was offered to all survey respondents who successfully answered two attention check questions that were contained in the survey instrument. The platform automatically converted US dollar payments to local currencies for non-US workers. We estimated that the survey took around eight minutes to complete.³⁴

Workers were eligible to take the survey if they applied for the job and had been hired previously on the platform at least once. There were 40 workers hired in Web Research, 36 in Administrative Support, 21 in Design, and 13 in Web Development. Another six workers were hired under a job titled “Take a compensated research survey” that was used as a test prior to specifying a job category. 113 of these workers passed the attention checks included in the questions and completed the survey.

Because our recruitment was not random, we also collected a random sample of current workers’ profiles from the platform to compare respondents to other workers with prior work history. We logged into the site as a buyer and browsed the workers in Administrative Support, Design and Creative, and Web/Mobile/Software Development, which are three of the largest categories of jobs on the platform. Workers’ profiles are paginated within each category, with ten profiles per page. We collected profile data for three random profiles on pages one and two of the paginated search results. From there, we drew random numbers between one and

³⁴This workflow does not violate the platform’s terms of service.

the maximum number of pages for each job category, and collected three random profiles on each page that we sampled. We continued until we had 500 profiles per job category. We were unable to collect data from 12 sampled profiles using this procedure, leaving us with 1,488 of the intended 1,500 profiles that we sought to sample across the three main job categories. The workers who completed the survey were then compared statistically against the random sample.

Table A.1 summarizes past platform work experience, the hourly profile wage rates, and the probability of being in the U.S. for the random sample of current workers and the surveyed workers. It shows that the surveyed workers have had fewer prior jobs on the platform (22.77 versus 58.59), have lower hourly rates on their profiles (20.87 versus 25.75), and have around the same share in the U.S. Our surveyed workers were actively looking for jobs on the platform at the time they took part in our survey, revealed by the fact that they applied to our job, whereas the randomly sampled workers were drawn from all workers with profiles on the platform. The relative lack of platform experience and lower wages among the surveyed workers suggest they receive less surplus than the typical worker who is hired. The fact that we find that those surveyed include significant markups in their wage bids reassures us that the survey results give a lower bound on the average surplus that hired workers currently earn.

Table 7 summarizes the raw answers given in the sample and also provides statistics that are re-weighted to match the random sample of 1,488 profiles of workers with non-zero prior jobs.³⁵

The survey itself began by collecting background demographics, such as the worker's country, education, off-platform labor market experience, and experience in online labor markets. We then asked the worker's hourly rate on their most recent engagement. We also asked about any off-platform earnings, including the actual (or perceived) hourly rate that workers would anticipate receiving if they worked in their local or non-platform labor market. The survey proceeded to a module on how workers set bids when applying for jobs. All respondents saw the same set of questions—the only variations entailed skipping inapplicable questions or randomizing question order. Respondents were asked what the minimum hourly rate would have been for them to accept their last online job before knowing anything about the difficulty of the work. We then followed up by asking whether, ex-post, the minimum rate they would accept would have been different. After a survey was completed, the respondent got a feedback score of five out of five for the completed job.

For the 113 respondents, the median hourly wage rate posted on their profile was \$10, and ranged from \$1 to \$200. The respondents were spread across 39 countries, with the four most frequent being Pakistan, the US, the Philippines, and India. 40% had been on the platform for six months or less, and 10% had been on the platform for longer than four years.

³⁵The probability of being in the sample was computed using a logistic regression of survey participation on the log of the number of prior jobs and the log of the hourly rate displayed on each profile.

A2. Survey questions related to markups

The survey questions designed to elicit worker markups on the platform were: “From the information that you had in the job posting on your last hourly job, what would have been the lowest hourly rate you would have been willing to accept to do the job? For example, if the job paid you \$10 but you would have been willing to do the work for \$9.00 but not \$8.99, then \$9.00 is the lowest hourly rate you would have been willing to accept”. The second was, “From what you know about the job now that you’ve worked on it, what would have been the lowest hourly rate you would have been willing to accept to take the job? (Note: The lowest hourly rate may be higher than the rate at which you agreed to work).”

To determine how platform wages relate to off-platform options, we asked a series of additional questions. Those who did not have off-platform jobs were asked: “If you were working in your local labor market rather than online, what hourly wage (in US dollars) do you think you would be earning? If you would not be paid hourly, please convert your total pay into an hourly wage (in US dollars) by dividing your US-dollar equivalent compensation by the typical hours worked over a pay period.” Workers with an outside job were asked: “When you work outside of platforms, what hourly wage (in US dollars) do you earn on average? If you are not paid hourly, please convert your total pay into an hourly wage (in US dollars) by dividing your US-dollar equivalent compensation by the typical hours worked over a pay period.”

To ask about markups on fixed price jobs, we used the question: “From the information that you have about the job now that you’ve worked on it, what would have been the lowest contract price you would have been willing to accept to take your last fixed price job? For example, if the contract price was \$22 but you would have been willing to do the work for \$20.00 but not \$19.99, then \$20.00 is the lowest total contract price you would have been willing to accept.”

B. MODEL AND ESTIMATION DETAILS

B1. Variation with the Instruments

This section provides additional detail about how worker behavior changes with the instruments. The main results in Tables 5 and 6 show that surplus estimates decline by 6.6% and 6.1% for buyers and workers, respectively, when we control for workers’ total number of applications in a month. Workers appear to become slightly less likely to apply in later positions when the Dollar to Local exchange rate increases, which can be seen in Figure A.5. A 10% increase in the Dollar to Local rate is associated with a lower average applicant order, by about one position (4% of the average position). Because expected surplus is declining with applicant order, when the Dollar is worth less, workers appear less likely to apply to jobs as later applicants. However, this source of selection is unlikely to have

a meaningful impact on our inference with respect to surplus. The marginal change in applicant position over the full range of the data is small. In addition, we condition on the observed choice set in the estimation problem, and Table 3 shows that the first stage relationship continues to remain strong when we condition on application quantity.

B2. Maximizing the Likelihood

Our estimation algorithm proceeds in two steps. First, we hold fixed a guess of the parameters $\{\rho_k, \lambda_{k\chi}^{\text{CONSIDER}}, \lambda_{k\chi}^{\text{ARRIVAL}}\}$ and estimate $\{\beta, \alpha, \psi\}$ for each buyer type conditional on this guess using the Berndt-Hall-Hausman algorithm. Second, we fix $\{\beta, \alpha, \psi\}$ and update $\{\rho_k, \lambda_{k\chi}^{\text{CONSIDER}}, \lambda_{k\chi}^{\text{ARRIVAL}}\}$ given the results in step 1 using a derivative-free Nelder-Mead algorithm. We iterate until convergence, after which we form the joint likelihood and use Matlab's `fminunc` function until the estimates converge in a local neighborhood of the starting values supplied by the iterative procedure.

B3. Buyer Surplus

BUYER SURPLUS ON A HIRE

To compute buyer surplus conditional on hiring, we draw Type 1 extreme value random variables for each applicant and the outside option. Using the choice parameters, we calculate the buyers' maximum utility including the draw for the unobservables. If the chosen alternative has the maximum utility, we accept the draws. Otherwise, we reject the draws and continue until the chosen alternative has the maximum utility. After we accept a set of draws, we compute surplus upon hiring as the difference in wage bids needed to equalize the utility of the chosen option with the outside option. Conceptually, we capture the amount the buyer would need to be compensated to make her indifferent between hiring her preferred applicant and not hiring on the platform. Denoting the draws for the hired worker and outside option as ε_{oj}^D and ε_{o0}^D , realized simulated surplus for a type k buyer is

$$(B1) \quad \text{RealizedSurplus}_{k\chi} = \exp\left(\frac{X_j\beta_{k\chi} + \varepsilon_{oj}^D - \varepsilon_{o0}^D}{\alpha_k}\right) - w_{oj}.$$

Estimates of type-specific surplus per hour on an opening are weighted by the posterior distribution of buyer types, recovered by Bayes' rule in equation (13), to give a weighted average realized surplus of $\Sigma_k \hat{\rho}_{ik} \text{RealizedSurplus}_{k\chi}$.

DYNAMIC SURPLUS ESTIMATES FOR BUYERS

For experienced buyers, the present discounted value of surplus is

$$(B2) \quad V_{kE} = \lambda_{kE} \times \text{ExpectedSurplus}_{kE} \times \text{Hours}_{kE}/r$$

where λ_{kE} is the job posting arrival rate for an experienced buyer of type k , $ExpectedSurplus_{kE}$ is the average hourly surplus for an experienced buyer of type k multiplied by the average number of hours per opening, and r is the interest rate, which we set to 8.7% annually.³⁶ This says that the present value of the market for experienced buyers is equal to the arrival rate of postings per period multiplied by the expected surplus conditional on a posting, while discounting the future surplus using rate r .

The present value for inexperienced buyers is similar, but must account for the transition to becoming experienced. The value function for an inexperienced buyer contains the term $Pr(Hire|I, k)$, the probability an inexperienced buyer of type k hires and transitions to the experienced buyer value function. Failure to hire leaves the buyer with the inexperienced buyer value function carried forward in time. This can be written as $V_{kI} = Pr(Hire|I, k) \times (ExpectedSurplus_{kI} Hours_{kI} + \frac{1}{1+r} V_{kE}) + (1 - Pr(Hire|I, k)) \frac{1}{1+r} \lambda_{kI} V_{kI}$, which after rearranging gives:

$$(B3) \quad V_{kI} = \frac{Pr(Hire|I, k) \times (ExpectedSurplus_{kI} \times Hours_{kI} + \frac{1}{1+r} V_{kE})}{1 - \frac{1}{1+r} (1 - Pr(Hire|I, k)) \lambda_{kI}}.$$

The numerator contains the probability of hiring while inexperienced. Upon hiring, the buyer receives the surplus on a given job and the discounted continuation value of transitioning to becoming experienced, given in equation (B2). The denominator accounts for the fact that buyers who do not hire return in the future based on the arrival rate for inexperienced buyers, given by λ_{kI} in equation (8).

B4. Worker Bidding With Future Wage Growth in the Relationship

To assess whether anticipated future wage growth during the buyer-worker relationship might alter workers' markups on any one bid and, therefore, affect our surplus estimates, we generalize the model to account for this possibility. Suppose that with probability q the wage is w_{oj} for the entire relationship, and with probability $1 - q$ the average present value of the wage is $w_{oj} \times (1 + g)$, where g is some growth rate that reflects how wages evolve over the relationship. This yields a modified version of the worker's objective function in equation (4)

$$\begin{aligned} E(U_{oj}(w_{oj})) = & \underbrace{E[\tilde{p}(j)]}_{Pr(Hired)} \times \{q \times \exp(\log w_{oj} - \log(1 + \tau)) + \\ & (1 - q) \times \exp(\log w_{oj} + \log(1 + g) - \log(1 + \tau))\} + \\ & (1 - E[\tilde{p}(j)]) \times c_{oj}. \end{aligned}$$

With this change, the optimal wage bid goes from

$$w_{oj}^* = c_{oj} (1 + \tau) \left(1 + E[\tilde{p}(j)] / \frac{\partial E[\tilde{p}(j)]}{\partial \log w_{oj}} \right)^{-1}$$

³⁶The choice of 8.7% is in line with the interest rate on a Small Business Administration loan during the sample, with the SBA being a plausible source of financing for most of the U.S. buyers in the sample.

to

$$(B4) \quad w_{oj}^* = c_{oj} (1 + \tau) \left[\left(1 + E[\tilde{p}(j)] / \frac{\partial E[\tilde{p}(j)]}{\partial \log w_{oj}} \right) \times (1 + g(1 - q)) \right]^{-1},$$

which is smaller than the original bid because $1 + g(1 - q)$ is positive and is in the denominator.

If $g \times (1 - q)$ is large and workers correctly put weight on the prospect of wage growth over the relationship, this would suggest that our markup estimates on the initial wage bid are overstated. However, workers would still gain surplus over the course of the relationship, in expectation, and some of that surplus would come from expected wage growth.

Using billing data, we find positive wage growth since the beginning of the data for 6.6% of buyer-worker pairs, either because of wage changes on the current job or because of wage changes over multiple contracts. Conditional on a wage change, the mean (median) hours-weighted growth rate is 22.8% (13.6%). When we apply the adjustment factor of 1.015 in the denominator of the markup equation (for the probability of wage growth multiplied by the mean wage growth), markups over costs on the initial wage remain positive but get slightly smaller, moving from 27% to 25%.

C. COUNTERFACTUALS

C1. Calculation Details

When we impose the 10% tax, it is included as an additional wedge between the hourly wage workers receive and the price buyers pay. Our baseline results assume that the composition of workers does not change under the counterfactual tax.³⁷

Workers select the optimal bid to maximize their payoffs (equation (4)), given the composition of the applicant pool, the buyer's semi-elasticity of demand to wage bids, and the additional tax. The size and composition of the buyer's consideration set on each posting remains unchanged. A buyer selects the option that maximizes her indirect utility out of her consideration set $\{J_o, 0\}$ given the simulated wage bids, where the probability any worker $j \in J_o$ is chosen is given by equation (2). The wage bids observed on any posted job opening affect the rate at which each buyer posts subsequent jobs, as in equation (8). When buyers post fewer jobs they are assumed to post the existing jobs in the same order but with a delay when compared to the time of posting in the observed data. The number of jobs posted by each buyer is determined by equation (8) up to the cutoff date when the simulated time period ends. Buyers who have prior experience at the start of the period enter the counterfactuals with prior experience. Potential

³⁷In the analysis in Section C.C2, which imposes a minimum wage counterfactual where worker sorting is likely to be more extreme, we conduct robustness checks that relax the assumption that applicant composition is static.

changes in the arrival rate of jobs due to buyers' failure to gain experience will not impact those who start the sample with prior hiring experience.

C2. Imposing a Counterfactual Minimum Wage

This appendix considers the market surplus implications of an hourly wage floor of \$7.00 per hour. Our main counterfactual in Section IV, imposing a 10% tax, reduces surplus mainly through reducing the number of jobs posted. Horton (2025) provides experimental evidence that buyers respond to the imposition of a \$3.00 minimum wage in the platform by posting fewer jobs in the future. We relate our model to this finding by using our structural estimates to examine the impact of a minimum wage on market surplus. We choose a \$7.00 minimum wage to approximate the average hourly minimum wage at the time of the data in the United States, where the majority of the buyers in our data are located.

The wage floor of \$7.00 is included in the workers' problem as a constraint, which directly binds for the lowest-paid workers in the market and has an indirect effect on the optimal wage bid of workers submitting higher wage bids who are no longer exposed to low-wage competition. We run the same base case as in the main counterfactual and for the version that sets δ_3 to zero, breaking the link between past wage bids received and job posting.

For this counterfactual analysis, we also illustrate the sensitivity of our findings to alternative assumptions about the composition of job applicants. This adjustment accounts for the fact that the worker's problem includes the implicit choice of whether or not to apply for a job. It is plausible that workers whose skills and qualifications lead them to submit bids below the relevant wage floor would view the probability of being hired to be so low in the counterfactual that the application costs exceed the expected benefits of applying. The new policy environment might also attract new workers with skills and qualifications that would make them competitive applicants at higher wage levels. We hence want to allow for the possibility of "labor-labor" substitution (Hamermesh, 1986), where buyers would be willing to pay higher wages for more productive workers. This alternative is considered only for the wage floor counterfactual because the 10% tax counterfactual applies across the board, for all workers, and is less likely to have a disproportionate effect on the participation decisions of workers currently bidding low wages.³⁸ This change allows us to analyze static hiring and market

³⁸To approximate the changes to the applicant pool under "labor-labor" substitution, applications with current bids below 90% of the counterfactual wage floor are removed from the applicant set and replaced with an equal number of new applications. The new applicants are assigned observable characteristics, X_j , that are a random draw from the distribution of applicants whose original wage bids are above the wage floor. The random draw is over different candidates, so each application consists of a real resume from a different candidate. We do not assign a random draw of wages, but take the new candidates' estimated opportunity costs of work as a function of observable characteristics and assume that bids are set as markups over costs. We continue to assume that workers' first order conditions hold subject to the wage floor constraint, and all applicants' wage bids are computed to be optimal given buyers' residual elasticity of demand. Buyers proceed by selecting the indirect utility-maximizing option as before. The effect of past wage bids on job posting rates is set to zero, otherwise the effect of past wage bids on future

dynamics under conditions where the market itself becomes more appealing for higher-quality applicants under the \$7.00 wage floor.

Table A.6 presents the results of the minimum wage counterfactual. Column 1 is the baseline specification, where job posting depends on past wage bids received and the applicant pool remains constant. Hourly wage bids increase by 34% and 47% to inexperienced and experienced buyers, respectively. The larger increase for experienced buyers reflects the fact that they receive lower wage bids in the data, so more of their applicants are bound by the counterfactual wage floor. Hiring rates on posted jobs fall in response to higher wage bids, by 30% and 34%, respectively. The reduction in hiring rates may seem large given that fewer than 40% of applicants are bound by the floor, but the reason becomes clear after considering which buyers are most affected. Under the wage floor, some openings, like those in non-technical jobs, see extremely large wage bid increases, while jobs that require greater skills experience little change. A large share of the hiring reduction arises for those posting non-technical jobs.

The static buyer surplus on posted jobs falls by less under the wage floor counterfactual than in the 10% tax scenario in the main text. This is because surplus estimates are averaged over all buyers, and the higher-skill jobs, which generate substantial surplus, are largely unaffected by the wage floor. However, the present value of inexperienced buyer surplus falls by 70%, and for experienced buyers falls by 63%. Both reductions arise because the job posting frequency falls for experienced buyers, reducing the future value of being experienced.

Columns 2 and 3 of Table A.6 show the additional results for the \$7.00 wage floor counterfactual. In column 2, job posting rates do not depend on past wage bids ($\delta_3 = 0$). The static changes are the same as in column 1, but column 2 shows a much smaller reduction in jobs posted by experienced buyers when posting does not depend on past wages. The reduction in buyer surplus in column 2 is also much reduced. Column 3 presents the scenario where the applicant pool is adjusted to mirror the characteristics of workers who typically submit bids above the \$7.00 floor. That is, it presents buyers with “better” applicants. The job posting rate here remains insensitive to past bids received, as in column 2. Because these applicants no longer face competition from lower-wage workers, the optimal bids increase more than in the main scenario. However, the number of jobs posted and hiring rates fall by slightly less than in column 2 because buyers perceive applicants to be better quality.

The final rows of Table A.6 show how worker surplus would be affected by the \$7.00 minimum wage counterfactual. The static percentage change in worker surplus in the baseline specification shown in column 1 is -2.6% , reflecting the balance of higher wages on filled jobs and the reduced share of posted jobs that are filled. However, the difference between columns 1 and 2 shows that the impact of the buyer posting dynamics has a large negative effect. Taking into account that buyers post fewer jobs having received higher previous wage bids reduces

hiring would be confounded by past applicant quality changes under this alternative scenario.

the present value of workers' lifetime surplus by 45% when dynamics are present compared to only 7% when they are shut down. The minimum wage results in an increase in worker surplus only when buyer posting dynamics are shut down and the composition of applicants changes.³⁹

Overall, the main insight from the minimum wage counterfactual is similar to that of the 10% tax: Policies that increase hiring costs serve to shrink the size of the market and reduce total surplus going to the supply side.

D. ONLINE-OFFLINE SUBSTITUTION

The counterfactual 10% tax in Section IV and minimum wage of \$7.00 in Appendix C.C2 both lead to a large reduction in the number of jobs posted. However, the counterfactual findings leave open the possibility that buyers substitute away from online hiring to fill more jobs offline in response to the counterfactual policies that increase the costs of hiring on the platform. Any such substitution would generate surplus offline. While we do not have data on whether buyers would post more jobs in other labor markets under the counterfactuals, we provide context for the degree of substitutability between online and offline labor demand via an analysis of online buyers' response to an increase in the cost of offline hiring. We do this by asking what happens to labor demand on the platform when there are exogenous changes in offline wages in buyers' local labor markets. The exogenous change we explore comes from increases in US state-specific minimum wages.

In July 2009, all but ten US states increased their local hourly minimum wage, either because of the 10.6% increase in the federal minimum wage from \$6.55 to \$7.25 per hour that went into effect that month, or as a state-specific increase at the same time as the federal minimum wage increase.⁴⁰ Online job postings were not subject to the same minimum wage regulations or to any increase in wages around this time.

If buyers view online and offline labor as close substitutes, then we would expect a relative increase in the demand for online labor after July 2009 in affected states. We would also expect the demand increase i) to be concentrated in relatively low wage online job categories and ii) to come from those states with high shares of offline workers in low paid jobs that can be done at a distance via telework. For this reason, we focus on non-technical online jobs in this exercise, as the median hourly wage for online hires by US employers in non-technical categories in 2009 was \$4.44, compared to \$12.00 in technical job categories. Later, in regression analyses, we examine heterogeneous effects in states that had a relatively high share of employment in jobs within \$2.00 of the new minimum wage

³⁹Unlike the main counterfactual of the 10% tax, with results shown in Table 9, there is no lump sum that could potentially be rebated to workers in this minimum wage counterfactual.

⁴⁰The minimum wage increased on July 24, 2009. The state-specific levels at the start of 2009 and start of 2010 are given here: <https://www.dol.gov/agencies/whd/state/minimum-wage/history>. The ten states unaffected by the minimum wage increase were California, Colorado, Hawaii, Iowa, Maryland, Massachusetts, Michigan, New Hampshire, Rhode Island, and West Virginia. The federal minimum wage had last increased to \$6.55 per hour on July 24, 2008.

in teleworkable jobs.⁴¹

A first look at the raw data suggests minimal substitution between online and offline hiring. Figure A.6 plots the log number of hires at the bi-monthly level by buyers located in the group of affected and in the group of unaffected states relative to the group-specific normalized mean number of hires before May 2008. Panel A shows no differential increases in the number of hires in non-technical job categories by buyers located in affected states after the increase in the minimum wage. There is also no apparent increase in hires in technical categories in Panel B.⁴²

We test whether there are significant differences in job postings or hires in US states that increase the minimum wage using a dynamic event study around the July 2009 event. We start by finding the total number of posts and hires in each state in each month over the time period studied. Following Bertrand, Duflo and Mullainathan (2004), we aggregate these observations into a pre- and a post-event period for each state, averaging over the six months prior to July 2009 and from July 2009, respectively. We estimate:

$$(D1) \quad Y_{st} = A_s + B_t + \beta I_s T_t + \epsilon_{st},$$

where A_s and B_t are state and aggregated time period fixed effects, respectively, I_s is an indicator for the state being affected by the minimum wage increase in July 2009, and T_t is an indicator for the post period. The coefficient β provides a test of whether the states with a minimum wage increase had significant difference in the level of the outcome Y_{st} in the six months after the event relative to the six months prior, compared to unaffected states. In a second specification, we also include interactions of the post period and having a high share of low paid telecommutable work, and a triple interaction of this term with the indicator for being in a state affected by the minimum wage increase.

For each of postings and hires, we look at three different outcomes: the total level of each variable, only non-technical postings and hires, and then only postings and hires by buyers who had engaged with the platform prior to April of 2009, and so were aware of it before the minimum wage increase. Each specification has only 102 observations, one for each state and one for DC, in each of the aggregated six-month time periods.

The difference-in-differences regression results from estimating equation (D1) are shown in Table A.7. There are no statistically significant increases in total job postings or hires in states that increase the minimum wage. Column 1 shows that the number of posts and hires is actually lower in affected states in the post period, although the differences are small. If buyers viewed online labor

⁴¹State-level variables are taken from the American Community Survey for 2007. Occupation teleworkability is taken from Dingel and Neiman (2020). The states with a higher-than-median share of teleworkable jobs in low wage administrative work in 2009 are Alabama, Arkansas, Florida, Iowa, Idaho, Kansas, Kentucky, Louisiana, Mississippi, Missouri, Montana, Nebraska, New Mexico, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, West Virginia, and Wyoming.

⁴²The limited online demand response is not due to lack of awareness of the platform, as the patterns are similar for experienced and inexperienced buyers.

as a substitute for local employment, we would instead expect to see positive coefficients on the post-period interaction. The interactions in column 2 show there is no significant differential increase in states where we would expect the greatest substitutability—those with high shares of workers near the minimum wage threshold in jobs that can be done remotely. The specifications in columns 3 and 4 find similar results when looking only at postings and hires in non-technical job categories. Columns 5 to 6 restrict the sample to buyers who were active on the platform at least once prior to April 2009. Although the point estimate turns positive in this subsample, it applies only to those states with a high share of low paid teleworkable jobs. The estimates are again very imprecise and we cannot reject the possibility that they are zero.

The results in Table A.7 show no evidence that the increases in local offline wages in July 2009 induced buyers to look for labor on this platform. If cross-price elasticities are approximately symmetric, this evidence suggests that reducing online hiring would not result in additional offline hiring. Other papers make the case for this symmetry. For example, Horton (2025) discusses surveys of buyers and finds that online and offline hiring are only very weak substitutes. In his experiment, the buyers who were unexpectedly and randomly subject to a minimum wage after posting jobs on the platform did not significantly decrease hiring rates but, instead, switched to higher-quality workers.⁴³ The finding that platform demand responds very little to changes in local offline wages is also consistent with Horton, Kerr and Stanton (2017), who find minimal cross-price elasticities between US and foreign workers. Most of the applicants who would have been impacted by the imposition of a wage floor on the platform are from outside the US. However, the low degree of substitutability between US and foreign workers on the platform, and between platform and offline workers, suggests the counterfactual changes in relative wages studied here would do little to increase offline hiring in the US.

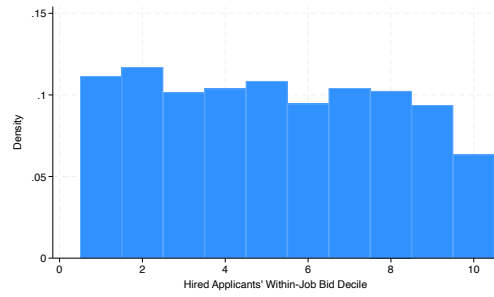
From the data, it is also clear that buyers post task-based jobs rather than hiring for long-term roles, even after they gain hiring experience. That is, the nature of labor demand tends to remain idiosyncratic and the work arrangements look quite different from those seen in traditional offline settings. 27.1% of jobs posted by inexperienced buyers are expected to last for less than one week; that number is nearly identical (28.5%) for those with prior hiring experience. A regression of the total number of hours worked per hire on the number of prior hires and buyer fixed effects shows that there are no within-buyer changes in job length upon gaining experience. This evidence suggests that buyers take advantage of being able to post jobs online on an as-needed basis, something that is likely harder to

⁴³In another paper, Horton, Johari and Kircher (2021), buyers were asked what they would have done with their most recent project if the platform were not available. Only 15% of employers responded that they would have made a local hire. Online employers report that they are generally deciding among (a) getting the work done online, (b) doing the work themselves, and (c) not having the work done at all. The survey also found that 83% of employers said that they listed their last job opening only on the platform in question.

do offline.

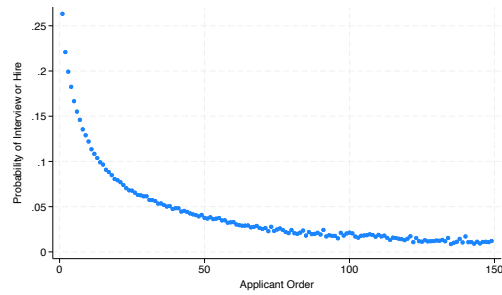
APPENDIX FIGURES AND TABLES

FIGURE A.1. BID DECILE FOR HIRED WORKER



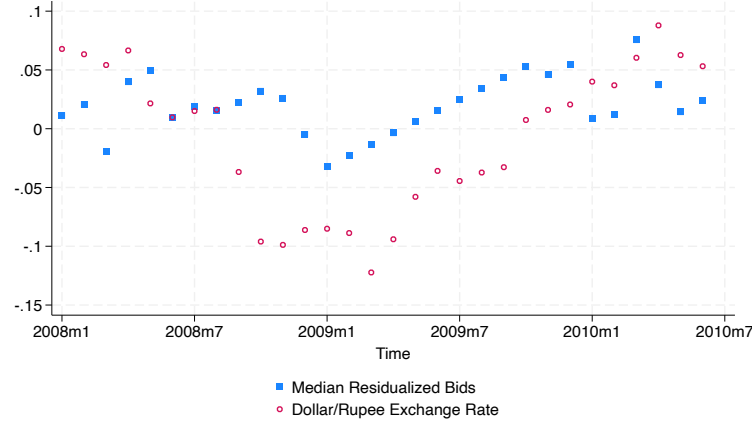
Note: This figure shows the bid decile of the worker hired for a job when a hire is made, on hourly jobs that receive at least 10 applications.

FIGURE A.2. INTERVIEW OR HIRE PROBABILITIES BY APPLICANT ORDER



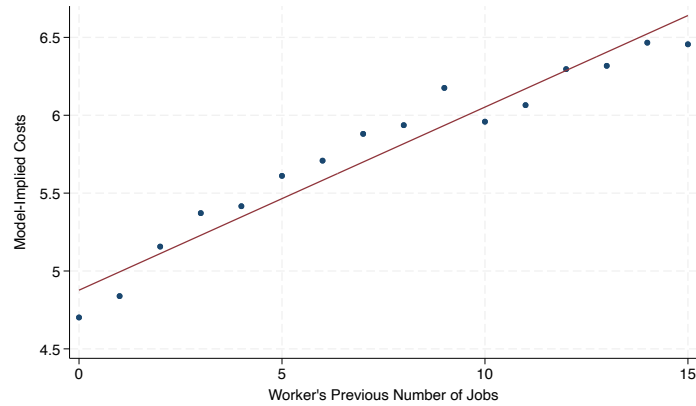
Note: This figure plots the probability that a buyer either interviews or hires a worker as a function of their applicant order on the job posting.

FIGURE A.3. MEDIAN RESIDUAL LOG BIDS AND DETRENDED EXCHANGE RATES FOR INDIA.



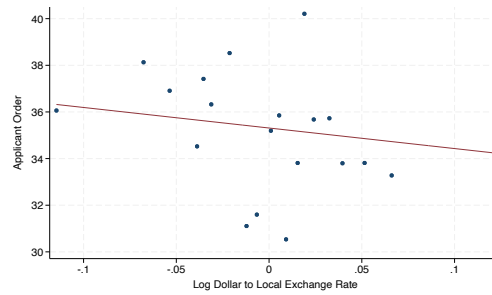
Note: This figure plots median residual log wage bids from Indian applicants in each month against the log US Dollar to Indian Rupee exchange rate, after removing a time trend and setting the series to have mean zero. Log wage bid residuals are net of job category fixed effects and a time trend.

FIGURE A.4. WORKERS' ESTIMATED COSTS INCREASE WITH PRIOR JOBS ON THE PLATFORM



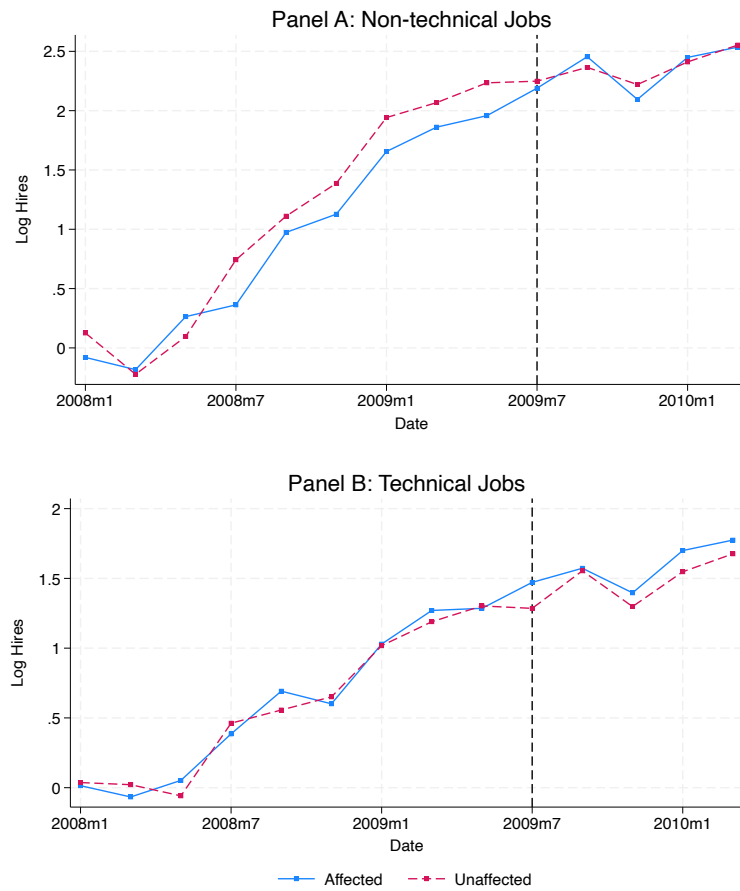
Note: This figure plots the relationship between the number of past online hires and a worker's implied costs from equation (7). The sample contains workers who we can track as having had zero to more than 15 hires in the data. The upward sloping relationship suggests workers' opportunity costs of work on a given job opening are a function of online opportunities on other openings, as the probability of finding online work increases for those who have landed prior jobs.

FIGURE A.5. AVERAGE APPLICANT POSITION (PROXYING FOR EXPECTED SURPLUS) AND THE EXCHANGE RATE INSTRUMENT



Note: This figure plots the relationship between average applicant order and the exchange rate instrument. The binscatter nets out country fixed effects and a time trend.

FIGURE A.6. LOG HIRES IN STATES AFFECTED AND UNAFFECTED BY THE JULY 2009 MINIMUM WAGE INCREASE



Note: These series show the log number of hires from buyers in states affected and unaffected by a local minimum wage increase in July 2009. Each series is normalized relative to the mean level in the data in months prior to May 2008. Panels A and B split the sample for non-technical and technical jobs, respectively. The solid lines represent affected or treated states, while the dotted lines are for control states.

TABLE A.1—CHARACTERISTICS OF PLATFORM WORKERS VERSUS SURVEY PARTICIPANTS

	Platform Participants	Surveyed	Difference
Number of Jobs	58.59 (196.40)	22.77 (46.34)	-35.82 (18.52)
Hourly Rate	25.75 (23.14)	20.87 (29.20)	-4.89 (2.30)
In the US	0.11 (0.31)	0.12 (0.32)	0.00 (0.03)
N	1488	113	

Notes: This table presents characteristics of workers in our survey sample compared to a random sample of platform profiles drawn from Administrative Support, Design, and Web Development job categories. In the random sample, we only collected data on the displayed profile hourly rate, number of prior jobs, and country. The survey was conducted between September - October 2023. There were 113 responses that came from a mix of direct invitations and applicants to jobs posted on the platform. Jobs were posted in the same categories for the random sample plus Web Research. Sample inclusion required workers to have some prior experience, as questions about surplus were framed around their prior jobs on the platform. Respondents were paid \$6 USD upon survey completion. Standard deviations are below means in the first two columns. The final column provides a difference in means test, with the standard error in parentheses.

TABLE A.2—TESTS OF SORT ORDERINGS AND APPLICANT INTERACTION

	(1)	(2)	(3)
Applicant Order / 10	-0.0052 (0.0003)	-0.0055 (0.0003)	-0.0057 (0.0003)
Descending Bid Rank (1 is Highest) / 10	-0.0017 (0.0002)		0.0010 (0.0006)
Ascending Bid Rank (1 is Lowest) / 10		0.0018 (0.0002)	0.0028 (0.0006)
Mean DV	0.175	0.175	0.175
N	4379260	4379260	4379260
R^2	.037	.037	.037

Note: This table considers different rules for how buyers may potentially sort applicants when considering who to hire. We evaluate sorting based on wage bids (where a buyer may consider high or low bidders first) and on the sequential arrival of applicants. The dependent variable is an indicator that the buyer interacted with an application. Interaction is coded based on whether a buyer took an active action to evaluate an applicant that is recorded in the platform's database. In the database, we observe whether buyers message an applicant, hire, or select a reason for not hiring, but not all buyers are proactive about selecting reasons for not hiring. The right hand side variables are ranks based on different sorting rules. Because job openings with few applicants mechanically have ranks clustered around one, all specifications have fixed effects for job category - by - the number of applicants to the job. The sample contains only organic, worker-initiated applications. Standard errors are clustered by job opening.

TABLE A.3—TESTS OF APPLICANT DIFFERENCES FOR HIGH AND LOW VALUES OF THE INSTRUMENTS

	Exchange Rate IV			Competition IV		
	Low	High	P-Value	Low	High	P-Value
Number of Prior Jobs	6.350	6.678	0.110	6.010	6.005	0.936
No Prior Jobs	0.354	0.348	0.377	0.385	0.382	0.430
Log Rate Last Hire	1.147	1.156	0.558	1.082	1.088	0.260
Feedback Score if Non-Zero	4.479	4.463	0.115	4.477	4.477	0.988
Zero Feedback	0.436	0.431	0.593	0.468	0.464	0.332
BA+ Degree	0.369	0.367	0.798	0.351	0.351	0.990
Good English	0.912	0.893	0.000	0.902	0.901	0.743
Agency Affiliate	0.363	0.361	0.736	0.325	0.330	0.010

Notes: This table presents differences in applicant characteristics for low (bottom tercile) and high (top tercile) values of the exchange rate and competition instruments. For the exchange rate instrument, terciles are calculated across country-months. For the competition instruments, terciles are calculated at the job category -by- quarter level. Low values, high values, and the p-value come from the constant, the constant plus high value dummy, and the test statistic on the high-value dummy from a regression. For the exchange rate instrument, the regression includes provider country fixed effects and a country-specific time trend, with standard errors clustered by country. For the competition instrument, the regression contains job category -by- quarter fixed effects, with standard errors clustered by job category-quarter.

TABLE A.4—REGRESSIONS OF THE NUMBER OF NEW UPLOADED FILES OR WORK SAMPLES ON INSTRUMENTS

	(1)	(2)
Exchange Rate	0.017 (0.030)	
Competition		0.001 (0.002)
R-Squared	0.002	0.003
Observations	680314	4428344

Note: Regressions of new items uploaded to workers portfolios or attached in applications on the two instruments. The sample in Column 1 (Column 2) is a worker-month (worker-week) panel for all time periods after a worker first enters the platform through October of 2009 (when this data ends). The exchange rate instrument is the log of the dollar to local currency exchange rate. The competition instrument is the average of the instrument at the job category-week level and is merged into the worker-week panel based on the modal job category for each worker. Column 1 controls for country fixed effects and a time trend. Column 2 controls for modal job category fixed effects and a time trend. Standard errors clustered by worker. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A.5—DEMAND PARAMETER ESTIMATES

	Type 1	Type 2	Type 3	Exper
Log Bid	-6.176 (1.993)	1.716 (1.531)	1.069 (1.963)	
Constant	5.407 (1.967)	-1.375 (1.506)	-0.872 (1.931)	
Constant x Experienced	4.996 (3.689)	-2.528 (3.338)	-1.463 (3.984)	
Log Wage on Last Job	1.142 (0.642)	-0.058 (0.089)	0.053 (0.093)	0.000 (0.000)
No Prior Jobs	2.117 (0.703)	-0.589 (0.540)	-0.376 (0.671)	-0.039 (0.033)
Number of Prior Jobs	3.188 (1.130)	-0.965 (0.864)	-0.665 (1.053)	-0.366 (0.161)
BA+ Degree	0.293 (1.557)	0.014 (1.824)	0.050 (1.581)	-0.345 (0.609)
Agency Affiliate	-0.396 (1.581)	0.043 (1.835)	0.111 (1.613)	-0.174 (0.644)
Agency x No Prior Jobs	0.026 (0.005)	-0.004 (0.003)	-0.003 (0.004)	-0.006 (0.001)
Good English	0.056 (0.054)	0.042 (0.053)	0.002 (0.062)	0.021 (0.033)
Feedback	-0.227 (0.070)	-0.016 (0.077)	-0.074 (0.069)	0.028 (0.019)
Feedback Squared	0.723 (0.274)	-0.134 (0.233)	0.026 (0.261)	-0.028 (0.059)
Feedback Cubed	0.090 (0.171)	0.169 (0.131)	0.045 (0.189)	-0.220 (0.052)

Notes: This table presents selected parameters from the estimates of the buyer choice problem. The first column presents baseline parameters. Columns 2 and 3 report additive interactions for buyer types 2 and 3. Type-shares are reported in the text. The last column reports additive interactions for buyer experience. Each type, however, has a constant that is allowed to shift independently with experience, as displayed in the third row. Unreported parameters are on the control function, an indicator that the worker is experienced but has no feedback, an indicator for buyer initiation, a time trend and a separate trend for technical categories, job category dummies, a spline for applicant order, country dummies for the largest countries, and country-specific time trends. Standard errors are estimated from 20 block-bootstrap iterations of the entire estimation procedure (drawing buyers with replacement).

TABLE A.6—COUNTERFACTUAL CHANGES IN HIRING RATES, POSTINGS, AND SURPLUS WITH A \$7 WAGE FLOOR

	Baseline	No Price Impact on Job Arrival	Labor-Labor Substitution
Panel A: Inexperienced Buyers			
Change in log Bids to Buyers	0.341 (0.004)	0.341 (0.004)	0.494 (0.012)
Static Pct Change in Hiring Rates	-0.303 (0.014)	-0.303 (0.014)	-0.232 (0.069)
Static Pct Change in Buyer Surplus	-0.100 (0.008)	-0.100 (0.008)	0.005 (0.132)
Pct Change in Jobs Posted	0.041 (0.017)	0.126 (0.011)	0.071 (0.040)
Pct Change in P.V. of surplus	-0.698 (0.042)	-0.306 (0.052)	0.046 (0.295)
Panel B: Experienced Buyers			
Change in log Bids to Buyers	0.470 (0.005)	0.470 (0.005)	0.660 (0.015)
Static Pct Change in Hiring Rates	-0.344 (0.016)	-0.344 (0.016)	-0.225 (0.031)
Static Pct Change in Surplus	-0.120 (0.009)	-0.120 (0.009)	0.089 (0.118)
Pct Change in Jobs Posted	-0.646 (0.040)	-0.151 (0.010)	-0.085 (0.047)
Pct Change in P.V. of surplus	-0.633 (0.039)	-0.150 (0.013)	0.170 (0.219)
Panel C: Workers			
Change in log Bids to Workers	0.412 (0.003)	0.412 (0.003)	0.585 (0.013)
Static Pct Change in Surplus	-0.026 (0.015)	-0.026 (0.015)	0.106 (0.068)
Pct Change in Surplus with Tax Rebate	-0.026 (0.015)	-0.026 (0.015)	0.106 (0.068)
Pct Change in P.V. of Surplus	-0.448 (0.037)	-0.065 (0.018)	0.069 (0.075)
Pct Change in P.V. of Surplus with Tax Rebate	-0.448 (0.037)	-0.065 (0.018)	0.069 (0.075)

Notes: Estimates of changes in log bids and percent changes in surplus (by buyer experience) under a \$7 wage floor counterfactual. The static percent changes in hiring rates and surplus are computed holding fixed the number of job openings. Surplus calculations for buyers come from equation (14). Present value calculations are described in the appendix. The percent change in the number of jobs is computed based on opening arrival rates simulating forward wage bids and buyers endogenous experience. Static worker surplus is the to-worker hourly wage less platform fees multiplied by hiring probabilities. The present value of worker surplus is calculated as to-worker hourly wages $\times$ average hours $\times$ hiring probabilities $\times$ the number of jobs posted monthly. We discount future surplus to the start of the sample. Rows that rebate taxes allocate tax revenue back to workers using lump-sum rebates. The second column holds fixed wage bids without accounting under the current regime without accounting for how higher bids change the arrival of future jobs. The third column replaces applicants whose original bids were less than 10% under the wage floor with a draw of a replacement applicant with a higher status quo bid. Standard errors come from 20 block-bootstrap iterations (drawing buyers with replacement).

TABLE A.7—TESTS OF POSTINGS AND HIRES CHANGES IN STATES THAT RAISE THE MINIMUM WAGE

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Job Postings by State-Month						
Min Wage Increaser x Post	-12.765 (23.513)	-17.471 (29.071)	-5.810 (13.311)	-8.978 (16.365)	12.281 (16.878)	15.531 (20.769)
Post x Low Wage Tele		-43.812 (28.093)		-25.875 (15.703)		26.813 (20.649)
Increaser x Post x Low Wage Tele		35.496 (29.944)		21.762 (17.180)		-22.481 (21.449)
Mean DV	59.4	59.4	30.0	30.0	24.1	24.1
N	102	102	102	102	102	102
R ²	.964	.965	.944	.946	.919	.921
Panel B: Hires by State-Month						
Min Wage Increaser x Post	-0.621 (3.753)	-1.045 (4.693)	0.121 (2.087)	-0.035 (2.625)	5.336 (5.712)	7.031 (7.003)
Post x Low Wage Tele		-6.938 (4.452)		-3.813 (2.438)		9.812 (6.990)
Increaser x Post x Low Wage Tele		4.961 (4.925)		2.568 (2.833)		-9.264 (7.183)
Mean DV	14.8	14.8	7.7	7.7	6.5	6.5
N	102	102	102	102	102	102
R ²	.98	.981	.972	.973	.884	.889
Non-tech jobs only:	No	No	Yes	Yes	No	No
Pre-event buyers only:	No	No	No	No	Yes	Yes

Note: The dependent variable in panel A is the average number of job postings by state and month in the six months prior to the minimum wage event. The dependent variable in panel B is the mean number of hires. Regressions in Columns 1, 3, and 5 are two-way fixed effects estimates with state and time fixed effects. Columns 2, 4, and 6 add interaction effects indicating that the state had an above-median share of low wage workers in teleworkable jobs prior to the minimum wage event date. This measure is computed from the 2007 ACS data, where low wage work is defined as earning less than \$9.25 per hour and teleworkable jobs are coded at the occupation level from Dingel and Neiman (2020). Column 3 and 4 restrict to postings in non-technical job categories. Columns 5 and 6 restrict to postings from buyers who were active prior to April of 2009 who were aware of the platform prior to the minimum wage changes. Standard errors are clustered by state.