

## Price Floors and Employer Preferences: Evidence from a Minimum Wage Experiment<sup>†</sup>

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*Firms posting job openings in an online labor market were randomly assigned minimum hourly wages. When facing a minimum wage, fewer firms hired, but those they did hire paid higher wages. Hours-worked fell substantially. Treated firms shifted to hiring more productive workers. Using the platform's imposition of a market-wide minimum wage after the experiment, I find that many of the experimental results also hold in equilibrium, including the substitution towards more productive workers. However, there was also a large reduction in the number of jobs posted for which the minimum wage would likely bind. (JEL J22, J23, J31, J38)*

If disemployment effects from minimum wages are modest, a potential reason is that firms can adjust in ways that do not necessarily reduce employee head counts: firms can cut back on hours, reduce nonmonetary compensation, increase prices, have lower profits, and so on. Other nonwage labor costs might fall, say through reduced turnover or increased productivity from enhanced worker morale. Given enough time, firms might also change what “kinds” of workers they hire through labor-labor substitution, perhaps leaving head counts unchanged.

The adjustments firms *might* make are clear enough,<sup>1</sup> but credibly measuring some of these adjustments is challenging. For one, although US state-level variation in minimum wages provides a plausible identification strategy, it is not a perfect strategy. Changes to state minimum wage levels are not made at random, and so much of the back-and-forth in the literature is about how to address the resulting selection issues. An additional empirical problem is measurement—some plausible firm adjustments would simply not show up in conventional administrative or survey

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<sup>†</sup>Go to <https://doi.org/10.1257/aer.20170637> to visit the article page for additional materials and author disclosure statement(s).

<sup>1</sup>See Schmitt (2013) on potential margins of adjustment; Draca, Machin, and Van Reenen (2011) on price pass-through; and Hirsch, Kaufman, and Zelenska (2011) on morale. See Fairris and Bujanda (2008) and Giuliano (2013) on labor-labor substitution.

datasets, either because the needed data are not recorded or they are recorded with too much error to be useful.<sup>2</sup>

Many of the limitations in conventional minimum wage study settings do not apply to this paper, in which I report the results of a minimum wage experiment conducted in an online labor market. During the experiment, treated firms were prohibited from hiring a worker at a wage below that firm's randomly assigned minimum. Firms differed in the degree of experience with the platform, which is observed; some are new, and some have hired in the past.<sup>3</sup> The existence of this minimum wage was not announced to firms or to workers. Instead, job applicants were automatically instructed to raise their wage bids (if needed) when submitting applications, until the floor was exceeded. At the end of the experiment, the platform announced its intention to impose a platform-wide minimum wage and then imposed that minimum wage several months later.

Because of the empirical context of the experiment and the exogenous source of variation, many of the challenges of conventional minimum wage research are not challenges in this study. With individual employers as the unit of randomization, the experimental sample is enormous, consisting of nearly 160,000 job openings. For each job opening, I observe whether anyone was hired, at what wage, and for how many hours; I also have detailed measures on the pre-experiment attributes of all workers. These measurements are made essentially without error because of the computer-mediated nature of the empirical context.

Despite these advantages, there are also limitations and new challenges because of the setting. For one, a minimum wage that only applies to some firms is quite different from a minimum wage that binds market-wide, as the latter scenario would have clear equilibrium effects not relevant in the former scenario. For these equilibrium questions, the platform's announcement and imposition of the minimum wage serve as a useful natural experiment. Although the minimum wage was applied all at once, platform-wide, very different wages prevailed in submarkets for different kinds of work. As such, there are unaffected submarkets that can serve as controls, permitting a difference-in-differences analysis. Another advantage of the platform-wide rollout is that knowledge of the minimum wage was well publicized, highlighting which experimental results (if any) depended on employers not knowing about the minimum wage policy.

The main results of the experiment are as follows. Imposing a minimum wage raised the wages of hired workers, but this imposition also reduced hiring, albeit not by very much. Effects on hiring are only clearly detectable at the highest minimum wages. However, hours worked fell sharply, with reductions as large as 27 percent in subpopulations of job openings expected to pay low wages. Large reductions in hours worked occurred even in subpopulations that saw little or no reduction in hiring. The net effect on hired worker total earnings was a wash; the increase in the wage was offset by the decline in hours worked. Presumably, some of the reduction

<sup>2</sup>See Card (1992); Card and Krueger (1995); Katz and Krueger (1992); Allegretto, Dube, and Reich (2011); Dube, Lester, and Reich (2010); Neumark, Salas, and Wascher (2013); Clemens and Wither (2019); Powell (2022); Meer and West (2016).

<sup>3</sup>I use the terms "worker," "firm," "employer," "hired," and "wage," for consistency with the literature and not as an indication of my views on the legal status of the relationships created in the marketplace.

in hours worked was caused by employers economizing on labor, but it was likely not the only cause.

Hours worked could have fallen, in part, because employers facing minimum wages hired substantially more productive workers, as proxied by greater past earnings and higher past average wages. This labor-labor substitution toward more productive workers occurred even at minimum wages for which there was no detectable decline in hiring, ruling out a pure selection explanation for the shift in hired worker composition. The extent of labor-labor substitution is large enough to explain about half of the reduction in hours worked, under the assumption that a worker's past average wage is a good proxy for their productivity.

The labor-labor substitution I find is only detectable because of the proxies for individual productivity. When I instead look for measures of labor-labor substitution with respect to demographics—namely the country the worker is from—the substitution is far harder to detect and only visible at the highest minimum wage. This is in sharp contrast with the productivity-based measures of substitution, which showed clear effects at all minimum wage levels. Prior empirical work that has examined employment probabilities by affected workers' past wages has often found clearer evidence of negative employment effects (Abowd, Kramarz, and Margolis 1999).

Empirical work that has shown disemployment effects for groups like teenagers or workers near a new floor could be interpreted as the upshot to a better market definition; that is, the researcher is finding the workers employed in the jobs that the minimum wage prices out. In this paper, while I also show that relatively low-wage workers are adversely affected, it is not just the case that jobs that paid low wages all disappeared, affecting the workers that took those kinds of jobs. Rather, *within* the applicant pools of those jobs, firms shifted toward hiring more productive workers, who had become relatively less expensive.

Whether this labor-labor substitution is an effective margin of adjustment depends strongly on how firm attempts at substitution are borne out in equilibrium. If all firms faced a minimum wage and all tried to adjust by hiring workers with higher productivity, sought-after workers would see their wages bid up. The magnitude of that equilibrium bidding-up effect—and thus the desirability and hence extent of substitution—would depend on the labor supply elasticity of the more productive workers. For example, if hiring more productive workers becomes too expensive, firms might forgo hiring altogether. And knowing about this higher minimum wage, firms might be less willing to post jobs. To explore these equilibrium issues, I use the platform-wide announcement and imposition of a minimum wage.

To analyze the platform-wide imposition of a minimum wage, I use the relatively low-wage “Administrative Support” category as the treated submarket and all higher wage categories of work as control submarkets. I find that simply announcing the upcoming minimum wage policy did little. There is no evidence that employers tried to hire workers quickly for relatively low-wage jobs or post more such jobs. In contrast, the imposition of the minimum wage strongly affected several market outcomes. As in the experiment, the wage of hired workers increased, employers shifted toward hiring more productive workers, and hours worked fell substantially. The matchup of experimental results and observational results on substitution suggests the employer's lack of knowledge during the experiment was not driving the

experimental results, as the results are the same when employers do have knowledge of *why* wage bids are higher.

Following market-wide imposition, employers posted fewer jobs likely to pay low wages in the months following the market-wide imposition. The number of hourly jobs posted in administrative support falls by around 25 percent. There is no discernible reduction or increase in the posting of non-wage-based jobs (i.e., fixed-price projects that would not be subject to the minimum wage), suggesting this avoidance strategy was not feasible for all kinds of jobs. Of those hourly jobs still posted, there is no evidence of a reduction in hiring, conditional upon a job post. However, this could simply reflect a compositional change in the kinds of jobs being posted.

In the experiment, only a small fraction of jobs were in the active treatment, and job seekers were free to apply across jobs. This makes identifying individual “winners and losers” at the worker level impossible. However, the experimental results do suggest different groups of workers could be impacted differently. Although wages increased with a minimum wage, labor-labor substitution could have a downside for the kind of labor being substituted away from. To explore this possibility, category-level data are insufficient, so I constructed an individual-worker longitudinal dataset around the time of the minimum wage imposition. I find that workers who had been working below the new platform minimum wage raised their wage bids after the platform-wide minimum wage was imposed, as expected. These same workers experienced a substantial decrease in their probability of being hired, while workers previously working above the minimum wage were unaffected.

While generalization to conventional settings should be done with caution, the labor-labor substitution finding offers a parsimonious partial explanation for why conventional minimum wage studies find such modest or nonexistent short-run disemployment effects. Of course, this result might not generalize to other markets, given the many differences between the empirical context and the conventional low-wage labor market. For example, conventional market jobs might not allow for as much variation in individual productivity, though evidence suggests such variation does exist and can be sizable (Lazear, Shaw, and Stanton 2015; Sandvik et al. 2020). Furthermore, there is intriguing evidence that even these seemingly highly flexible, “spot” online labor markets have imperfect competition (Dube et al. 2020). An intriguing possibility is that productivity-based substitution occurs in conventional markets in response to minimum wage changes but has been largely overlooked simply because it is difficult to detect without very rich individual productivity data. The effects from the market-wide imposition do suggest that when employers have more time to adjust, they respond on margins other than just shifts in hiring.

The plan of the paper is as follows. Section I describes the empirical context of the experiment. Section II introduces the experimental design and explores threats to internal validity. It also presents the methodology for identifying job openings likely to pay low wages and thus be affected by the treatments. Section III presents a simple conceptual framework for understanding the experimental results. Section IV presents the main experimental results of the paper. Section V presents results from the announcement and imposition of a market-wide minimum wage. Section VI concludes.

## I. Empirical Context

A number of online labor markets have emerged in recent years (Horton 2010; Agrawal et al. 2015). In these markets, firms contract with workers to perform tasks that can be done remotely, such as computer programming, graphic design, data entry, and writing. Markets differ in their scope and focus, but common services provided by the platforms include publishing job listings, hosting user profile pages, arbitrating disputes, certifying worker skills, and maintaining feedback systems.

The experiment reported in this paper was conducted in a large online labor market. In this market, a would-be employer writes job descriptions, labels the job opening with a category (e.g., “Administrative Support”), lists required skills, and then posts the job opening to the platform website. Workers generally learn about job openings via electronic searches. Workers submit applications, which generally include a wage bid (for hourly jobs) or a total project bid (for fixed-price jobs) and a cover letter. Only hourly jobs were eligible for the experiment. In addition to worker-initiated applications, employers can also search worker profiles and invite workers to apply. After a worker submits an application, the employer evaluates applicants and can decide to make an offer or offers.<sup>4</sup>

### A. Wage Bidding, Profile Rates, and the Measurement of Hours Worked

Workers have an hourly “profile rate” that is listed on their platform profiles. This profile rate is their default bid for hourly job openings, though they are free to override it, tailoring it for each application. Workers can set their profile rate and change it whenever they like, but they have the incentive to keep it close to what they think their market rate is, as firms searching in the market for workers use the profile rate in their decision-making about whom to recruit (Horton 2019). If a worker is hired, it is at an agreed-upon hourly wage. To work on hourly contracts, workers must install software that precisely records hours worked.

Most relationships formed on the platform are quite short, as the median contract lasts about one week. However, relationships have no set end date, and some relationships can continue for years, with hours worked continuing to grow. To stabilize the data for analysis purposes, I stop experimental measurements 180 days after the formation of the contract. However, no results are sensitive to this duration restriction given how few observations it affects.

### B. Multihoming and Market Definition

The marketplace used for this study was not the only contemporaneous marketplace for online work. As such, a worry is that job openings were simultaneously posted on several other platforms and perhaps in conventional markets as well. However, surveys conducted by the platform suggest that online and offline hiring

<sup>4</sup> Although they can bargain over the wage, there is relatively little wage bargaining, with most employers and workers treating wage bids as take-it-or-leave-it offers (Barach and Horton 2021). Interestingly, Fradkin (2016) finds surprisingly little bargaining on Airbnb. There is perhaps some reluctance to begin a relationship with haggling over price.

are only very weak substitutes and that “multihoming” of job openings is relatively rare. Supporting this view, a main finding of the experiment is that hiring reductions were small or nonexistent, implying that displacement of hiring to other platforms was not an important margin of adjustment, at least in the short run. Furthermore, there is no evidence that treated job openings were subsequently posted on another online labor market that, at the time, had a lower minimum wage (I will discuss this later in the paper).

### *C. Other Research Using Online Labor Markets as a Testing Domain*

There has been some research that uses online labor markets as a domain for research. Pallais (2013) shows via a field experiment that past on-platform worker experience is an excellent predictor of being hired for future job openings. Stanton and Thomas (2016) shows that agencies (which act as quasi-firms) help workers find jobs and break into the marketplace. Agrawal, Lacetera, and Lyons (2013) investigate what factors matter to firms in making selections from an applicant pool and present some evidence of statistical discrimination, which can be ameliorated by better information. Horton (2017) explores the effects of making algorithmic recommendations to would-be employers. Barach and Horton (2021) reports the results of an experiment in which employers no longer had access to wage history when making hiring decisions.

## **II. Experimental Design and Internal Validity**

During the experimental period, firms posting an hourly job opening were immediately assigned to an experimental cell.<sup>5</sup> There were four experimental cells: a control group with the platform status quo of no minimum wage, which received 75 percent of the sample ( $n = 121,704$ ), and 3 active treatment cells, which split the remaining 25 percent of the sample. A total of 159,656 job openings were assigned. The active treatments had minimum wages of \$2/hour in *MW2* ( $n = 12,442$ ), \$3/hour in *MW3* ( $n = 12,705$ ), and \$4/hour in *MW4* ( $n = 12,805$ ). Having relatively small numbers of jobs in the active treatment cells was a deliberate experimental design choice, with the goal being to reduce the potential for market-moving violations of the SUTVA condition, a concern in experiments conducted in a true marketplace (Blake and Coey 2014). I will discuss this issue—and related threats to internal validity—in more depth below.

If the firm posted additional job openings, these openings also received the same experimental assignment as the original opening. I do not include these follow-on openings in the analysis, as whether a job was posted or the attributes of such a job could be affected by the treatment.

The minimum wage was implemented by not allowing workers to submit wage bids below the assigned opening-specific minimum wage. Prior to the experiment,

<sup>5</sup>Firms posting fixed-price job openings were not eligible for the experiment. A small number of very large platform clients were exempted prior to randomization. Firms could have posted a subsequent fixed-price job to avoid the minimum wage, which is part of the reason I only use the first job opening in the analysis, as they could be affected by the treatment. Despite this possibility, there is no evidence that firms switched to using fixed-price contracts as an adjustment strategy.



wage bids were restricted to positive numbers via an automated check of the job application form. For job openings in the active treatment cells, this \$0 floor was simply raised to the appropriate minimum. If an applying worker tried entering a wage below the minimum wage, he or she was instructed (via a dialog box) that the proposed wage was too low and needed to be raised. The worker was not told the precise amount the wage bid had to increase by, in order to reduce bunching at the exact minimum wage. The worker's application was not sent to the employer until the minimum wage condition was met.

### *A. Threats to Internal Validity*

The internal validity of the experiment could have been jeopardized if any of the following occurred: (i) a failed randomization, (ii) applicant attrition at the wage bidding stage, (iii) workers sorting across job openings based on the experimental cell of that opening, (iv) firms sorting across time (i.e., posting the same opening again sometime later to get a better “draw” of applicants), and (v) firms sorting across platforms including the “platform” of the conventional labor market. For both issues (iv) and (v), the concern is that any observed reduction in hiring could actually be displacement to other platforms. However, as will be discussed, there was very little reduction in hiring, so both of these concerns are somewhat moot. For issues (ii) and (iii), the concern is that different cells would have selected applicant pools.

For issue (i)—failed randomization—there is no evidence this occurred. Job openings are well balanced on prerandomization attributes, and the counts of job openings per cell are consistent with a random process. See the online Appendix for this analysis.

For issue (ii)—applicants abandoning the application process before submitting an application—this was regarded by the experimental designers as unlikely. Submitting a wage bid was the last part of the application process, making application costs sunk. As such, workers had little incentive not to comply with the instructions to raise their wage bid.<sup>6</sup> Consistent with this sunk cost argument, there is no evidence that the count of applicants differed across experimental cells. See the online Appendix for this analysis.

For issue (iii)—worker sorting across openings—the potential problem is that workers were free to apply to any job opening in the marketplace. If workers knew the assignment of a job opening, they might seek their preferred opening. While a concern in principle, this kind of sorting would be exceedingly difficult in practice, and the lack of differences in applicant counts by the experimental group is consistent with a lack of sorting.

The reason this sorting is unlikely in practice is those firm treatment assignments were not publicly known to workers, nor was the existence of the experiment. As such, few workers learned there was some opening-specific difference to seek out, much less what preferences they should have over these differences. The only way to learn about any particular job opening assignment was to apply. Compounding

<sup>6</sup> Although workers have a time-based quota of job applications they can send, it is set so high that it is almost never binding, and so withdrawing an application because of a too-high minimum wage would be unlikely.

the difficulty for would-be sorting workers, recall that only 25 percent of job openings had any minimum wage, making finding a preferred opening challenging.

For issue (iv)—firms “sorting” across time by reposting their job opening to get another draw of applicants—the problem is that such sorting would look like a reduction in hiring, as the first job goes unfilled. Firms might post another job opening if they thought they received an idiosyncratically bad “draw” of applicants.<sup>7</sup> Despite the possibility, there is actually a slight decrease in the probability of posting a subsequent job opening for treated firms. See the online Appendix for this analysis.

For issue (v)—firms sorting over platforms—the concern is that this kind of sorting could bias the results toward finding a larger reduction in hiring. To assess this concern, I checked whether firms in the highest minimum wage cell posted their job openings on another online labor market with a lower minimum wage. I find no evidence of increased cross-posting of job openings assigned to the highest minimum wage. See the online Appendix for this analysis.

I have no evidence of whether any work was displaced to offline hiring, but as discussed in Section I, survey evidence suggests that few firms see offline hiring as a substitute for online hiring. Given the nature of work and the typical wages on online labor platforms, it is unlikely that local hiring was a feasible alternative for most firms, particularly given that most employers were from the United States.

Given the lack of internal validity issues, there is a simple way to conceptualize the experiment. Firms got the same applicants they would have gotten, regardless of the experimental cell, but with the distribution of wage bids differing based on their treatment assignment. Those applicants who would have bid below the assigned minimum wage floor simply bid up.

### *B. What Did Employers Know or Infer?*

Some treated employers received atypically high wage bids from workers. Instead of simply taking these wage bids as given, some employers might have been (a) “alerted” they were in an experiment or (b) “persuaded” that some applying workers were more productive because they were proposing higher wages. However, the behaviors of employers, at least collectively, are not consistent with either “persuaded” or “alerted” belief changes. Instead, employers acted *as if* they were well-informed price-takers in a spot market; they simply compared the price offered to the productivity they can infer from worker attributes and made a hiring decision. In the online Appendix, I discuss the platform design and economic reasons why “alerted” or “persuaded” beliefs would be unlikely and present evidence from both the experiment and the platform-wide rollout to support this view.

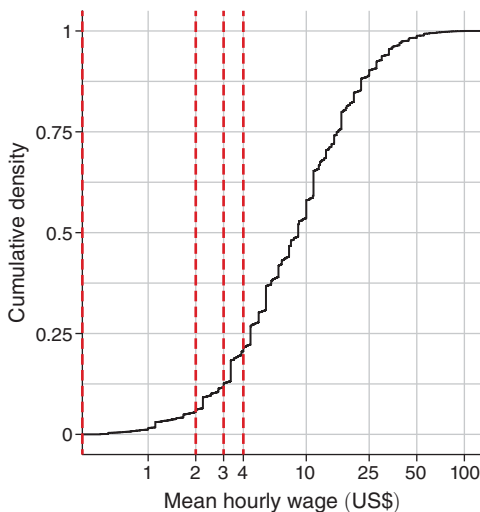
### *C. Identifying Job Openings Likely to Be Affected by a Minimum Wage*

A challenge in all minimum wage research is identifying the segment of the labor market where a minimum wage will matter. For this experiment, I use two approaches

<sup>7</sup>Or they could repost to avoid their treatment cell if they (a) believed they were in an experiment and (b) mistakenly thought the level of randomization was the job post rather than firm or (c) thought the experiment would conclude shortly. These possibilities will be discussed in more depth in Section IIB.



Panel A. Control cell of all



Panel B. Control cell of admin jobs (top) and non-Admin openings (bottom)

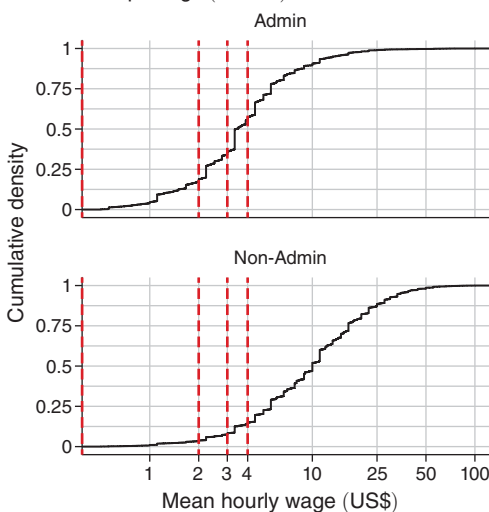


FIGURE 1. EMPIRICAL CDFS OF HIRED WORKERS IN THE CONTROL GROUP

*Notes:* This figure shows the empirical CDF of the hourly wages of hired workers in the control group, on a log scale. The CDF in panel A is for all workers. In panel B, the top CDF is hired workers for control group ADMIN job openings, while the bottom CDF is for all other job openings in the control group.

to find subpopulations of job openings that were likely to pay low wages: I used the posting firm's self-categorization of their job opening by the type of work, selecting those assigned to the category with the lowest average wages on the platform, and I created my own classification, based on a predictive model trained with data from pre-experiment jobs.

The lowest wage category on the market is "Administrative Support," or ADMIN. Panel A of Figure 1 shows the empirical CDF of the log wages of hired workers in the control. This distribution is decomposed into ADMIN and non-ADMIN job openings in panel B of Figure 1. The three levels of the minimum wage are overlaid as dashed vertical lines. We can see that ADMIN jobs pay considerably less than non-ADMIN jobs. In ADMIN, the first quartile of the wage distribution is below \$3/hour, the median is near \$4/hour, and the third quartile is only slightly above \$5/hour. Note that the highest minimum wage of \$4/hour is above the median wage.

Although administrative jobs pay the lowest wages on average, not all low-paying jobs are in the administrative category, and not all administrative category jobs are low paying. To make use of the low-paying but non-ADMIN openings in the analysis, I use historical pre-experiment data from the platform to fit a predictive model and then use it out of sample to label all experimental job openings with low predicted wages as the low-predicted wage sample, or LOWPREDWAGE.<sup>8</sup>

<sup>8</sup>Online Appendix ?? shows the distribution of wages by category and details on the model training to construct the LOWPREDWAGE sample.

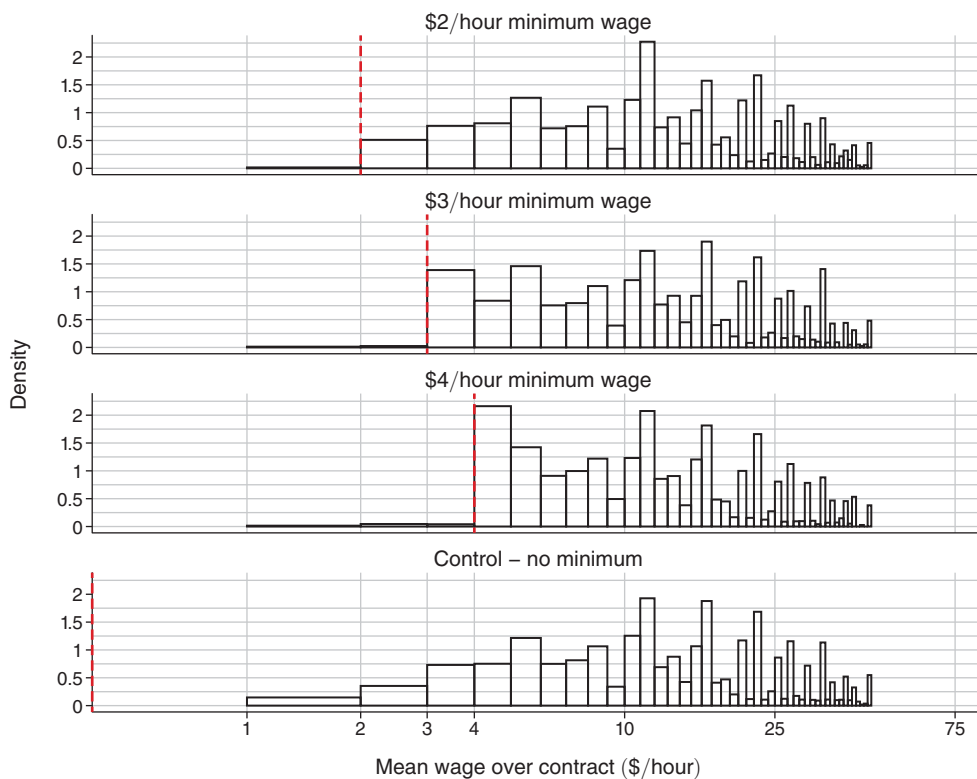


FIGURE 2. THE REALIZED WAGE DISTRIBUTIONS FOR HIRED WORKERS IN ALL, BY EXPERIMENTAL GROUP

*Notes:* This figure shows a density histogram of log observed hourly wages in each of the experimental cells. The x-axis is on a log scale. The bars in the histogram are each \$1 wide, with intervals of  $[a, a + 1)$ , where  $a$  is an integer.

#### D. Effectiveness of the Experimental Intervention at Preventing Wages Below the Assigned Minimum

To test whether the experimental intervention was effective in preventing contracts from being formed below the cell-specific minimum, I plot the distribution of hourly wage by experimental cell, on a log scale in Figure 2. The hourly wage is calculated by taking the total wage bill for each contract and dividing by the total number of hours worked. The bars in the histogram are each \$1 wide, with intervals of the form  $[a, a + 1)$ , where  $a$  is an integer.

The top panel of the figure shows the distribution for the control group. There are substantial numbers of hired workers who received less than the lowest minimum wage. In the active treatment cells, the mass of observed hourly wages is nearly all to the right of the imposed minimum wage for that cell. The small number of noncomplying observations is due to workers offering refunds to their firms, lowering the wage bill but keeping the hours worked the same and hence lowering the effective wage. This small amount of noncompliance notwithstanding, the treatment was clearly delivered.

### III. Conceptual Framework

The focus of the empirical analysis is on how the minimum wage affected the decision-making of employers. Changes in decision-making might be manifested by changes in whether the firm hires at all, the “kind” of worker it hires, the wage of the hired worker, and the hours worked, conditional upon a match being formed. Below, I describe a simple version of the firm’s hiring problem that offers a framework for interpreting changes in these outcomes when the hiring firm faces a minimum wage.

The firm’s hiring problem is to select the applicant offering the greatest return, as measured by the value of the work delivered minus the cost. Suppose the firm has a project of a “size” such that the project can be completed with  $Q$  efficiency units of labor. A worker with technical productivity  $y$  will complete the project in  $h = Q/y$  hours. The firm gets a value of  $V$  from having the project completed. The firm receives wage bids from a pool of applicants with heterogeneous technical productivity. Each worker submits a take-it-or-leave-it hourly wage bid of  $w$ . If worker  $i$  is hired, the project is completed in  $Q/y_i$  hours, and the wage bill is  $w_i Q/y_i$ . What the firm cares about for an applicant  $i$  is  $V - Q(w_i/y_i)$ . As  $V$  and  $Q$  are common across applicants, what matters is the ratio of a worker’s wage bid to their technical productivity,  $w_i/y_i$ . Let  $r$  be the ratio of the wage bid to technical productivity.

One could enrich the model by endogenizing  $Q$ , with  $Q'(r) < 0$ ; that is, the employer scales down the scope of the project when then  $w/y$  is higher. This would change observed hours worked and the firm’s “no hire” condition, but it would not change the selection problem, as the firm would still care about selecting the lowest  $w/y$  applicant.

A seemingly important difference between the context of the experiment and conventional contexts is that on the platform, work is project-based work, whereas, in conventional employment, there is typically no set end date to a relationship. However, this is a difference with little direct bearing on the firm’s selection problem, as the total size,  $Q$ , “drops out,” and so it is only the flow of value rather than the total stock that matters.<sup>9</sup>

The firm’s decision problem is shown graphically in Figure 3, with the firm choosing among three applicants,  $A$ ,  $B$ , and  $C$ . Each applicant differs in their wage bid,  $w$ , (on the x-axis) and technical productivity,  $y$ , (on the y-axis). Although the firm cares about  $w_i/y_i$  in selecting among applicants, whether it hires at all would depend on the surplus the best option affords, and so we can think of the firm as having some reservation ratio,  $\underline{r}$ , beneath which they will not hire at all. The firm’s indifference curves are straight lines, coming down from the upper-left corner and parallel to the  $\underline{r}$  line.

With this diagram, it is easy to see how labor-labor substitution can be an important margin of adjustment, but that at a sufficiently high wage, hiring will not occur. Before the minimum wage is imposed, the firm’s preferences are  $A \succ B \succ C \succ$  Not hiring. Consider a minimum wage,  $\underline{w}$ , indicated by a dashed

<sup>9</sup>One potential difference is that in hiring for roles with no defined end date, it might be impossible to reduce scope below the output of one worker, given the preference workers or firms might have for full-time work. For example, a firm can go from having 10 to 8 data analysts. But given that for many roles, part-time work is uncommon, the firm probably cannot go from one analyst to four-fifths of an analyst.



#### IV. Experimental Results

To analyze the experiment, my primary approach is to regress the job-level outcome of interest,  $y_j$ , on the experimental group indicators,

$$(1) \quad y_j = \beta_0 + \beta_2 MW2_j + \beta_3 MW3_j + \beta_4 MW4_j + \epsilon_j,$$

where  $MW2$ ,  $MW3$ , and  $MW4$  are indicators for whether job opening  $j$  has a minimum wage of \$2, \$3, or \$4, respectively.

For each outcome, I present results for three samples: all job openings (labeled ALL), administrative openings, (labeled ADMIN), and jobs predicted to pay low wages (labeled LOWPREDWAGE). When there are multiple hired workers per job opening, what I do depends on the nature of the outcome. If the outcome is a rate, I take the average for all hired workers and use that, such as the wage of hired workers. If the outcome is a quantity, such as the number of hours worked, I take the sum. However, multiple hires are fairly rare in the experimental data: of employers making a hire, 85 percent only hire 1 worker, while 9 percent hire 2 workers. In any event, there is no evidence that the minimum wage altered the number of hires per opening, conditional upon the employer making at least one hire.

All results from the experiment are reported in Figure 4. Each “row” of the figure reports the effects on a different outcome; outcomes that are conceptually related to each other are grouped together in panels. For each outcome, point estimates are shown for  $\hat{\beta}_2$ ,  $\hat{\beta}_3$ , and  $\hat{\beta}_4$  from equation (1). Around each point estimate, a 95 percent confidence interval is shown, calculated with robust standard errors. The mean for the control group,  $\hat{\beta}_0$ , is reported under the point estimates, labeled “Ctl mean.” I also label each  $\hat{\beta}$  for the minimum wage cells with the implied percentage change in that experimental group relative to the control. The sample size for all cells pooled together is reported next to this mean ( $N = \dots$ ).

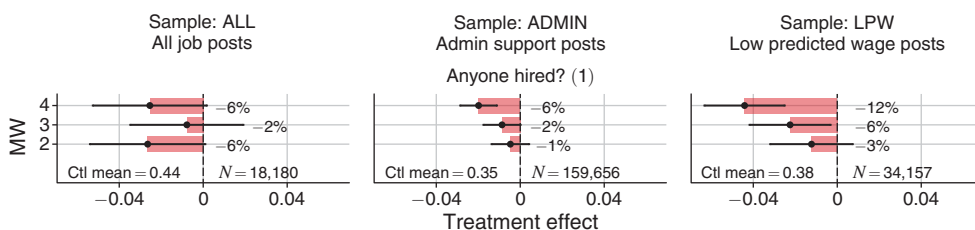
##### A. Match Formation

I begin by examining whether the minimum wage affected whether the employers hired anyone at all. I define “hired” as some number of hours worked by a worker against that particular job opening. These results are reported in panel A of Figure 4, “Match formation,” with the label “Anyone hired?”

Starting with the full sample, ALL, treated cells had a lower probability that anyone was hired compared to the control cell. There is a 6 percent reduction relative to the control in  $MW4$  and a 2 percent reduction in  $MW3$ . The  $MW2$  reduction is 6 percent, but the confidence interval includes 0. In the  $MW4$  cell, which had the largest reduction in hiring, the level decrease is about  $-0.03$ , from a baseline hiring rate of 0.44.

Declines in hiring from the minimum wage were generally larger in the subpopulations of jobs expected to pay lower wages. Still in the top row of Figure 4, but in the middle column, the sample is ADMIN jobs. There are generally larger point estimates for a reduction in hiring, though even in  $MW3$  the percentage change is approximately the same, at 2 percent, matching the  $MW3$  effect in ALL. The 95 percent confidence interval includes 0. In the LOWPREDWAGE subpopulation, in the far

## Panel A. Match formation



## Panel B. Wages, hours worked, and earnings of hired workers

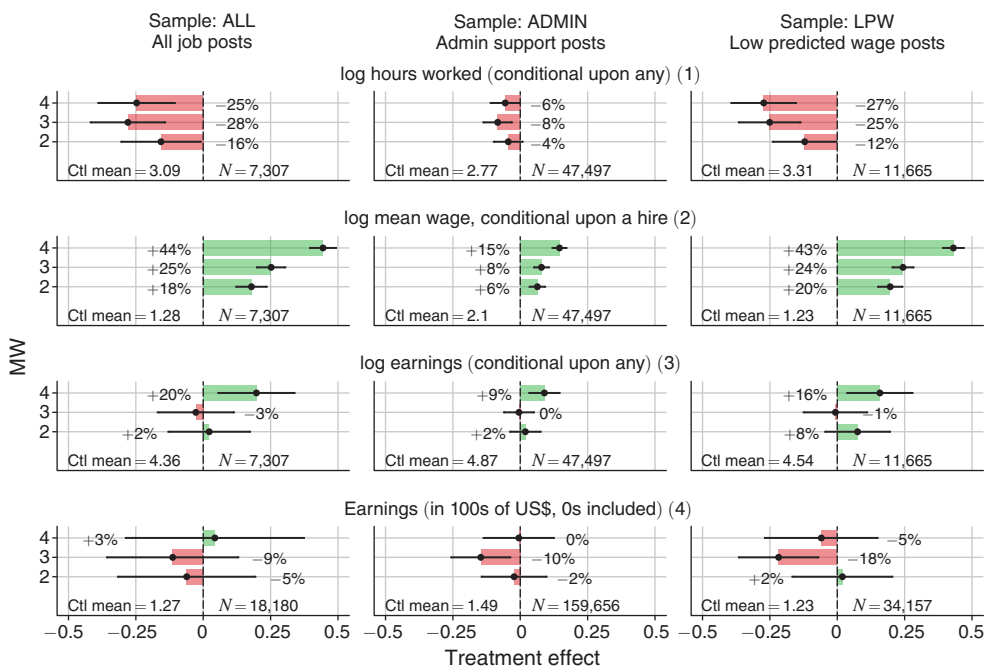


FIGURE 4. EFFECTS OF IMPOSING JOB-SPECIFIC MINIMUM WAGES ON JOB OUTCOMES RELATIVE TO THE CONTROL GROUP ON HIRING AND HOURS WORKED

Notes: This figure reports estimates of equation (1) for a collection of job-specific outcomes. See Section II for details on the experimental design and sample definitions.

right column, we can see that there are larger reductions in the probability of hiring. The *MW4* percentage reduction in hiring is larger, at about 12 percent. In *MW3*, the reduction is close to 6 percent, and in *MW2* it is close to 3 percent, with the *MW2* confidence interval including 0.

The highest minimum wage, *MW4*, clearly decreased hiring, particularly in job openings likely to otherwise pay low wages. However, the largest reduction, in *LOWPREDWAGE* for *MW4*, is still only about  $-0.04$  percentage points, or 12 percent, despite \$4/hour being substantially above the median wage for filled control cell job openings in *LOWPREDWAGE*. If we had naïvely assumed that jobs that would have been filled at a wage below the minimum would simply be screened out, effects are only about a *tenth* of what they should be in *ADMIN MW4*. Although effects are always negative, for the lower values of the minimum wage, the confidence



intervals for the point estimate typically include zero. For example, nowhere is the *MW2* reduction in hiring conventionally significant.

Note that although it is tempting to calculate an elasticity with respect to the minimum wage from these data, it would not have a useful interpretation. The none-to-one jump gives an undefined denominator, and all the subsequent jumps have imprecise differences in hiring rates. Similarly, with experimental variation in the wage, one might think we can cleanly estimate a labor demand schedule, but recall the potential for the imposed minimum wage to alter the composition of the hired workers, never mind inducing labor supply effects. As such, I report the effects of the minimum wage on wages and hours separately without trying to estimate a demand elasticity.

### B. Wages, Hours Worked, and Earnings of Hired Workers

If a worker is hired, I can observe the hourly wage, hours worked, and total earnings. The effects of the minimum wage on these outcomes are presented in panel B of Figure 4, labeled “Wages, hours worked, and earnings of hired workers.” It is critical to note that the sample in these regressions is conditioned on hiring, except that I also include a measure of earnings with zeros, that is, unfilled jobs included.

Starting with the first row, the outcome is labeled “log hours worked (conditional upon any), (1).” The sample is restricted to openings where a worker was hired and he or she billed at least one-quarter of an hour, the minimum chunk of billable time on the platform.<sup>10</sup> In the full sample *ALL*, hours worked fell in all treatment cells. Hours worked fell 25 percent in *MW4*, 28 percent in *MW3*, and 16 percent in *MW2*. Only in *MW2* does the confidence interval contain zero. In the subpopulations, we see much larger reductions in hours worked. In *LOWPREDWAGE*, hours worked fell sharply: 27 percent in *MW4*, 25 percent in *MW3*, and 12 percent in *MW2*. Effect sizes are similar in *ADMIN*. We can see that despite relatively small reductions on the intensive margin, there were large reductions on the intensive margin.

In the second row of panel B, labeled “log mean wage, conditional upon a hire (2),” we can see the imposed minimum wage clearly increased the wage of the hired worker, conditional upon a hire being made. Even in *ALL*, there is about an 18 percent increase in *MW2*, a 25 percent in *MW3*, and a 44 percent increase in *MW4*. Note that the control group average wages in *ALL* are substantially higher than either *ADMIN* or *LOWPREDWAGE*, as expected. In the subpopulations of jobs likely to pay lower wages, the minimum wage had stronger positive effects on wages. In *ADMIN*, hired worker wages rose by 15 percent in *MW4*, 8 percent in *MW3*, and 6 percent in *MW2*. For the *LOWPREDWAGE* subpopulation, the effects are even stronger still; in *LOWPREDWAGE* hired wages rose about 43 percent in *MW4*, 24 percent in *MW3*, and 20 percent in *MW2*. I will return later to the mechanism by which wages increased.

In terms of why hours worked fell, there is no evidence it was caused by selection; e.g., the extensive margin effect screened out the jobs most likely to have many hours. See the online Appendix for this analysis.

<sup>10</sup>I also use the count of hours, with zeros included, as the outcome in the online Appendix. As expected—given the decline in the fraction of jobs where a hire is made—the reduction in hours with zeros included is also negative, but the estimates are less precise given the high variance in hours worked.

Given the increases in average wages, but the decrease in average hours worked, the total effect on hired worker earnings depends on the relative magnitudes. It is also relevant what fraction of jobs are filled at all, as an unfilled job mechanically leads to zero earnings. In the two bottom rows of panel B of Figure 4, two measures of earnings are reported, one conditional upon a job being filled, and the other not, with zeros included.

The first earnings measure is labeled “log earnings (conditional upon any)” with the sample restricted to jobs where a hire was made. In ALL, the effects are close to zero in MW2 and MW3. However, in MW4 there is evidence of a substantial increase in log earnings in ADMIN, but recall this is also the cell with nontrivial reductions in hiring. For log earnings in LOWPREDWAGE and ADMIN, for MW2 and even MW3, the confidence intervals include zero, consistent with the hours worked and wage effects being mostly offsetting. The effects in MW4 are positive, but again, this is also where we had the largest reductions on the extensive margin.

The second earnings measure is “Earnings (in 100s of US\$, 0s included)” with the sample unrestricted. Note that I scaled earnings by 1/100 so as to be able to show this outcome on the same scale as the log measure of earnings. When we look at levels of earnings, there is little consistency. There is perhaps some evidence of a net decline in MW2, but this effect is insignificant in every subpopulation. The formerly strong evidence in MW4 looks decidedly mixed.

Taken together, the results suggest the minimum wage likely had negligible effects on total earnings, with the caveat that statistical power is not high enough to say much confidently. At the highest levels of the minimum wage, we see nontrivial reductions on the extensive margin or job openings with no associated earnings. Further complicating any strong claims about worker welfare, hours worked is a cost to workers, so even a net decline in earnings is not necessarily unwelcome.

*Reasons for the Increase in Wages.*—Although it is clear that imposing a minimum wage caused higher wages paid for jobs still filled, the precise mechanism remains to be shown. There are several potential reasons for the increase. (i) “Selection”—the job openings that do not fill would have paid low wages, and what is left are the relatively higher-paying jobs (recall in Section III, if each applicant were shifted left and down, it would have been more likely for the employer not to hire at all, screening out this relatively low-wage work). (ii) “Substitution”—the firms select higher-productivity workers, who are paid higher wages (recall in Section III the shift from hiring *A* to *C*). (iii) “Markup”—firms hire the same workers they would have hired anyway but at a higher wage (in Section III, had *C* and *A* been unavailable, the firm would have hired *B* with and without the minimum wage, but with the minimum wage, *B* would be hired at a higher hourly rate).

Although these three explanations are not mutually exclusive, I can rule out (i), selection, as the sole explanation. Recall that in the MW3 cell in ADMIN, there was more or less no reduction in hiring, and yet the average wage increased by nearly 8 percent (this mirrors the argument that selection alone cannot explain the decline in hours worked). More generally, if we assume that the minimum wage perfectly screened out the jobs that counterfactually would have paid the lowest wages, using the control group in ALL, the empirical elasticity of the average wage to the fraction of the lowest wage contracts removed is about 1; that is, removing 10 percent of the

observed lowest-wage jobs increases the average for the remaining jobs by about 10 percent. This stands in contrast with the actual finding that the wage effects are, in percentage terms, much larger than the extensive margin reductions. This suggests a substantial role for explanations (ii) substitution and (iii) markup.

### C. *Hired Worker Composition, by Productivity and Markup*

To detect and quantify substitution effects, we do not need to model the microdetails of selection, but rather use as experimental outcomes the pretreatment characteristics of workers. This analysis is motivated by the shift depicted in Section III, where the firm's first choice under a minimum wage switched from the lowest-technical productivity applicant *A* to the highest applicant, *C*. To detect this labor-labor substitution, I use three proxies for hired-worker productivity: (1) average past wage, (2) cumulative past earnings, and (3) profile rate of hired workers. Critically, all of these measures are calculated using data from jobs posted before the start of the experiment. The effect of the treatment on these outcomes is explored in panel A of Figure 5, labeled "Hired worker composition, by productivity."

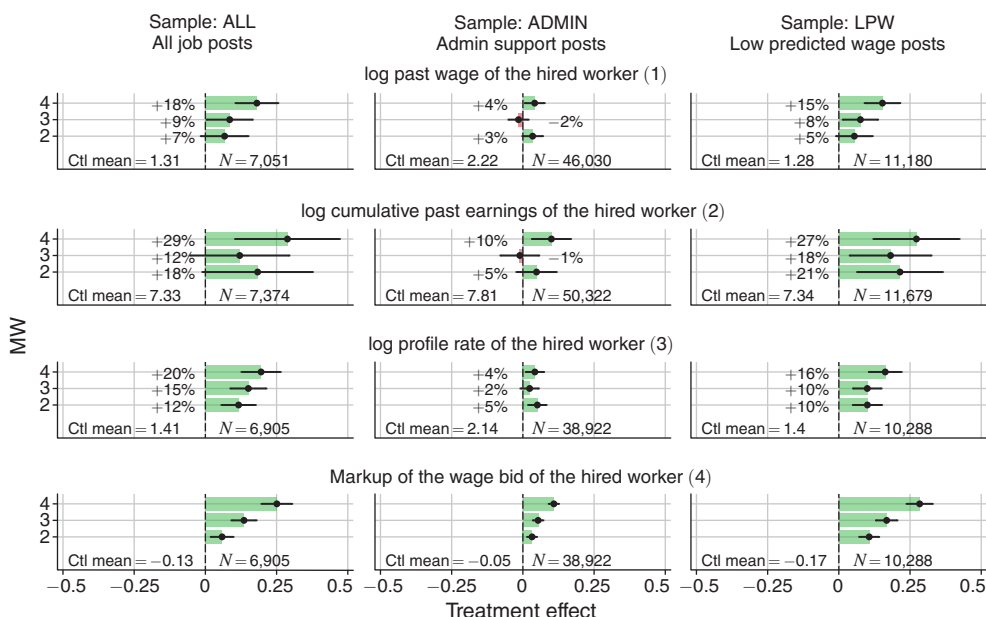
The first outcome is labeled "log past wage of the hired worker (1)." The average wage is calculated by dividing total hourly earnings by the total hours worked, prior to the experiment. In ALL, hired workers in *MW2* and *MW4* had higher past wages, with the effect significant or nearly significant in both cells. The effect is slightly negative in *MW3*. Turning to the subpopulations, we see much stronger effects. In the LOWPREDWAGE subpopulation, hired workers had substantially higher past average wages in the active treatment cells. For example, on the *MW4* cell, in LOWPREDWAGE, hired workers had 15 percent higher past wages than those hired in the control. The *MW2* effects were positive and close to 5 percent and nearly conventionally significant. Effects are somewhat smaller in ADMIN.

The second productivity proxy outcome is earnings, and the row is labeled "log cumulative past earnings of the hired worker (2)." The sample is restricted to a hired worker having at least \$1 in past earnings.<sup>11</sup> The sample is slightly larger than the average past wage sample, as workers can have earnings from fixed-price contracts as well. In ALL, there is some evidence of a shift toward more experienced workers in *MW4*, but the point estimate for *MW2* is actually negative, albeit with a CI that comfortably includes zero. In contrast to ALL, in the subpopulations, the shift toward relatively more experienced workers is obvious. For example, in LOWPREDWAGE, hired workers in *MW4* had 27 percent higher cumulative past earnings compared to the control. The effect was 18 percent higher in *MW3* and 21 percent higher in *MW2*.

Another proxy for a worker's productivity is their profile rate. One advantage of using the profile rate as a proxy for worker productivity is that it is available for all workers, even if they have never worked on the platform. Furthermore, it can potentially give a more accurate measure of the worker's current market wage compared to the average past wage, which can include wages from many long-completed jobs.

<sup>11</sup> This is not an important restriction, as there is no discernible difference among the cells on whether the hired worker had any past experience. This is perhaps unsurprising, as there is little "room" for treatment effects on whether or not the worker has past earnings, as over 90 percent of hired workers in the control group had past platform experience. See the online Appendix for an analysis of the effects of the minimum wage on the probability that the hired worker had prior on-platform experience.

Panel A. Hired worker composition, by productivity



Panel B. Hired worker composition, by country

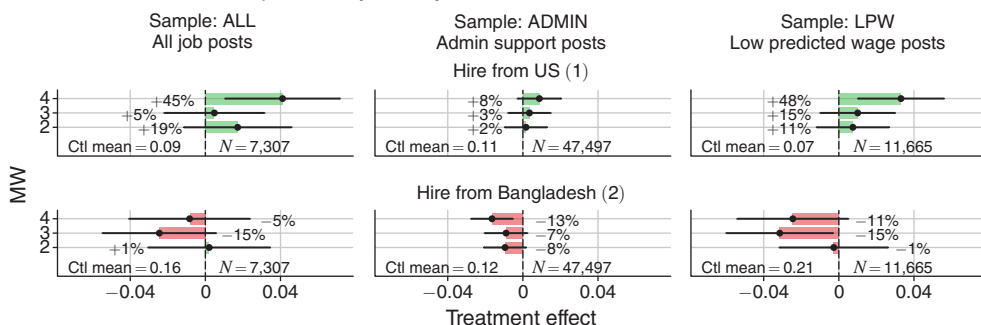


FIGURE 5. EFFECTS OF IMPOSING JOB-SPECIFIC MINIMUM WAGES ON JOB OUTCOMES RELATIVE TO THE CONTROL GROUP ON WORKER COMPOSITION

Notes: This figure reports estimates of equation (1) for a collection of job-specific outcomes. See Section II for details on the experimental design and sample definitions.

In analyzing the effect of minimum wages on the profile rate of the hired worker, I use the pre-experiment profile rate.

In the row of Figure 5 labeled “log profile rate of the hired worker (3),” we can see that there is some evidence of an increase in ALL, but as with the other productivity proxies, in the subpopulations, the treatment effects are larger and the shift obvious; in ADMIN, profile rates are about 5 percent higher in MW2 and 2 percent in MW3, and 4 percent higher in MW4. Effect sizes in LOWPREDWAGE are quite similar to those in ADMIN.<sup>12</sup>

<sup>12</sup>The profile rate is discussed in Section IA.

These shifts in the attributes of hired workers give strong evidence of labor-labor substitution as an employer margin of adjustment. Despite the importance of labor-labor substitution, it does not fully explain the wage increase. To see this, we can use as an outcome the hired worker's "markup" or the difference between their worker's profile rate and their actual wage bid. These results are reported in the row labeled "Markup of the wage bid of the hired worker (4)" in Figure 5. The sample is restricted to hired workers with a listed profile rate. As this outcome is already a worker-specific percentage, I do not report the percentage change. Note that the mean markup in the control is negative; workers often bid below their profile rate.

Across all treatment cells and samples, the minimum wage increased wage bid markups. The increase is large, even in cells with almost no reduction in hiring, such as MW3 in ADMIN. To give a numerical example, the treatment effect on the hired worker markup in MW4 is about 0.25 in ADMIN. If a hired worker in the control group in ADMIN had a wage bid of \$3.60/hour but a profile rate of \$4/hour (so a 10 percent discount), the 0.2 point estimate implies that the bidding worker instead applied a  $-0.10 + 0.25 = 15\%$  increase off their profile rate; that is, they bid \$4.60/hour and offered no discount whatsoever.

#### D. Hired Worker Composition, by Country

Labor-labor substitution in response to a minimum wage has been observed in conventional markets.<sup>13</sup> However, changes in composition are typically detected with respect to demographic characteristics. For example, using personnel data from a single large firm, Giuliano (2013) find that teenagers from higher-socioeconomic status zip codes displaced older workers following a minimum wage increase. Fairris and Bujanda (2008) finds that a Los Angeles living wage that applied to city contractors caused those vendors to substitute in favor of workers with characteristics associated with a wage premium in that local labor market.

If employers were engaging in productivity-based labor-labor substitution, detecting it through changes in demographics would be challenging if most of the variation in individual productivity is within—rather than between—demographic groups. If this "more-within-than-between" characterization holds, the kind of productivity-focused substitution found in the experiment might not result in much evidence of demographic substitution. To make this point more clear, I look for labor-labor substitution in the experiment using a demographic measure, namely, the hired worker's country. On the platform, the best chance for detecting substitution via demographics would be through shifts in the country of the hired worker, as there are substantial differences in the average hourly wages paid to workers from different countries (Agrawal et al. 2015).

To look for labor-labor substitution by demographics, I use an indicator for the country of the hired worker as the outcome. I use the United States and

<sup>13</sup> Although modern empirical work has given relatively little attention to labor-labor substitution, some of the earliest empirical work on the minimum wage considered the possibility. The remarkable study of the introduction of a minimum wage in Oregon by Obenauer and von der Nienburg (1915) looked at changes in employment by workers of different experience levels, which had different associated minimum wages.

Bangladesh, the highest- and lowest-average wage countries, respectively, when the experiment was run. There are stark differences between the two countries; in the control group, hired US workers have a median wage of \$17/hour, whereas Bangladeshi workers have a mean wage of \$5/hour. These differences largely reflect differences in the area of focus, with US workers specializing in work that tends to pay higher wages. Regarding baseline composition, in the control group, 20.9 percent of hires are from Bangladesh, whereas only 7.3 percent are from the United States.

In panel B of Figure 5, labeled “Hired worker composition, by country,” in the row labeled “Hire from US (1),” we can see that in *MW4*, US workers were substantially more likely to be hired in both *ADMIN* and *LOWPREDWAGE*, and the confidence interval does not include zero. However, in *MW2* and *MW3*, while the increase in US hires is positive, the confidence intervals include zero. Without the *MW4* cell, we would likely conclude there is no strong evidence of labor-labor substitution. This is despite the fact that with the more direct productivity measures, such as past wages and earnings, the evidence for substitution was unambiguous even in *MW2*.

The difficulty of seeing the labor-labor substitution based on demographics is also illustrated at the low end by examining “Hire from Bangladesh.” While the fraction of hired workers from Bangladesh decline everywhere, the confidence interval contains zero for all cells and subgroups. Again, an analysis that only had access to demographics could easily miss the labor-labor substitution that is occurring.

### E. *Strengths and Limitations of the Experiment*

The experimental results leave open at least four questions: (a) whether labor-labor substitution is a useful margin of adjustment for employers in equilibrium, (b) whether employer knowledge of the minimum wage policy mattered, (c) whether the quantity and composition of jobs being posted would change with a platform-wide minimum wage, and (d) the equilibrium effects on workers when all jobs were subject to the minimum wage (as opposed to just some fraction).

For (a), in the experiment firms could shift toward more productive workers without much worry that these workers would demand higher wages, as only a relatively small fraction of employers were in the active treatment cells. However, in an equilibrium where all employers were subject to a minimum wage and all tried to hire more productive workers, this margin of adjustment might prove difficult. For (b), employers did not know they were in an experiment, and as such, might have reacted differently compared to employers who understood a minimum wage was in effect. Recall the discussion in Section IIB. For (c), in the experiment, the composition of jobs was fixed, as jobs were only allocated to a cell *after* they were posted. In contrast, firms with knowledge of the minimum wage might decide not to post jobs at all. Recall from Section III that even if a firm still hires with a minimum wage, they receive less surplus than in the counterfactual. For (d), workers were free to apply across jobs, and only a relatively small fraction of job posts were treated; in a setting where a minimum wage applied to all jobs, the effects on workers could be quite different.



In the next section, I will use the platform-wide announcement and imposition of a minimum wage to answer these remaining questions.

## V. Platform-Wide Imposition of the Minimum Wage

After the experiment, the platform implemented a universal \$3/hour minimum wage, making *MW3* the default experience. Unlike during the experiment, this minimum wage policy was publicly announced. The announcement was made about two and a half months before the minimum wage was actually imposed. As the minimum wage was universally applied, it is impossible to report experimental estimates of its effects. However, I can compare various market outcomes before and after the announcement and imposition. I can also compare categories of work that differed substantially in their wages before the policy change, allowing for a difference-in-differences analysis. To do this, I use the low-wage *ADMIN* category as the treatment group and use the remaining relatively high-wage categories as the controls, subject to some restrictions I will discuss. In addition to the category panel, I also construct a dataset of the applications of individual workers to see how different kinds of workers were affected, focusing on their bidding behavior and their per application probability of being hired.

### A. *Effects of the Announcement and Imposition of the Platform Minimum Wage on Hired Worker Wages*

I first inspect whether the platform policy change was actually implemented. Figure 6 plots the weekly time series of quantiles of hired worker wages that week, by category of work. The quantiles are the tenth, twenty-fifth, fiftieth, and ninetieth. The y-axis is the hourly wage, on a log scale. Job categories are ordered left to right and top to bottom by the pre-announcement mean wage in that category. The date of the minimum wage policy announcement and imposition are indicated by vertical dashed lines. The time period for the figure is the year the minimum wage policy change occurred, extended for several months into the postperiod. The panel ends when the category definitions changed.

In all categories, there is no evidence of a systematic change during the period when the minimum wage was announced but not yet implemented; the announcement alone seemingly did nothing. However, once the minimum wage was implemented, the effect on hourly wages is clear and obvious: all wage quantiles that were previously below \$3/hour immediately snap up to \$3/hour. There are three categories where the tenth percentile of the wage distribution was below the platform-imposed minimum: “Administrative Support,” “Customer Service,” and “Sales and Marketing.” However, for the two non-*ADMIN* categories, it is only certain low-wage subcategories that make up most jobs at the tenth percentile. For categories where even the tenth percentile was above the minimum wage, such as in “Web Development” and “Software Development,” we see no change. It is clear the policy change was implemented. Furthermore, simply plotting market aggregates over time shows many of the experimental effects also appear during platform-wide imposition. For example, the online Appendix shows that post-imposition, there was a clear shift toward hiring workers in *ADMIN* with greater past earnings, just as in the experiment. However, to actually quantify these effects, I shift to a difference-in-differences analysis.

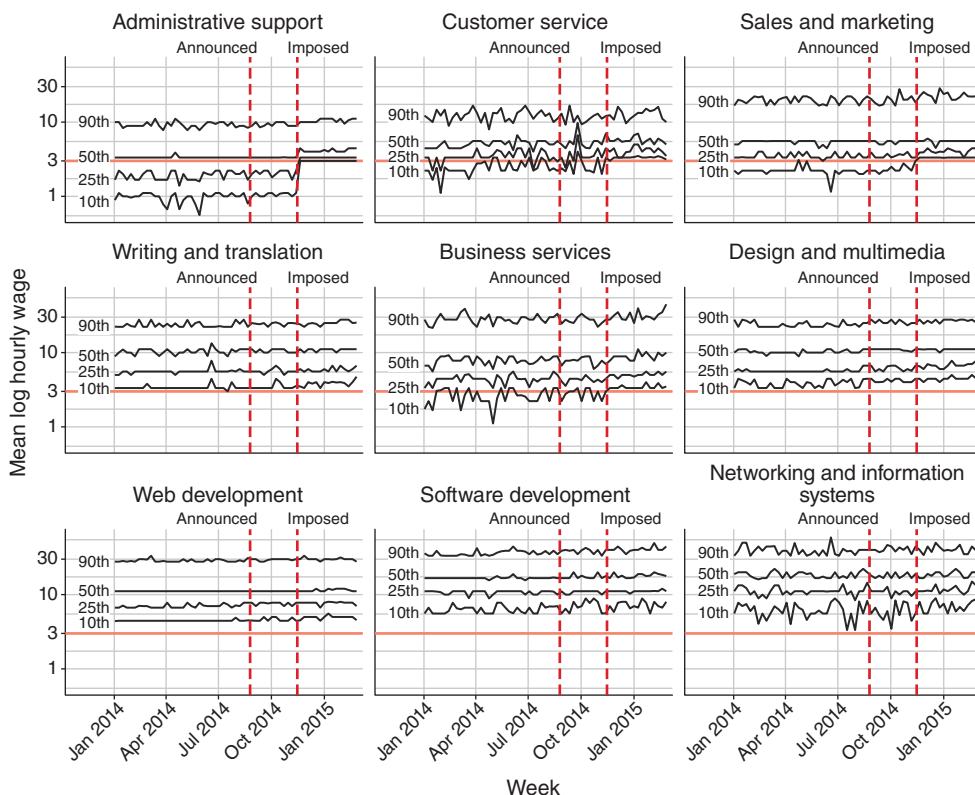


FIGURE 6. HIRED WORKER WAGES BY WEEK, BY CATEGORY OF WORK FOLLOWING THE \$3/HOUR MINIMUM WAGE ANNOUNCEMENT AND IMPOSITION

Notes: This figure plots by-category weekly wage quantiles for all work categories. The minimum wage policy announcement and imposition are indicated with vertical dashed lines.

### B. Panel Construction

To perform a difference-in-differences analysis, I construct a job category-month panel, with ADMIN as the treated unit and all other eight categories as control units. In the panel, the time periods are 30 days long, with 0 day defined as the imposition date.<sup>14</sup>

The categories used as control units are not selected at random, but on the basis of an observable difference, namely, wages. However, these control units are useful for understanding what happened in ADMIN, by providing a contemporaneous measure of seasonality, platform-wide growth trends, fluctuations in demand for online work generally, and so on. One complication with this category panel approach is that there are “subcategories” that have wages below the minimum but are not in ADMIN. Some categories have relatively low-wage subcategories of work, such as “Customer Service” and “Sales and Marketing” in Figure 6. I remove data from these subcategories rather

<sup>14</sup> This time period is longer than the seven-day period used in Figure 6. I used seven days as the period duration because I wanted to show how the change immediately followed the imposition, whereas, for other outcomes, my focus is on how the market equilibrium changed.

than “transfer” them to ADMIN, as I want to compare the experimental ADMIN estimates to the observational estimates using just the ADMIN category.

### C. Inference about the Effects of the Platform-Wide Imposition

My preferred specification is

$$(2) \quad y_i = \sum_t \beta_t \text{ADMIN} + \alpha_i + \delta_t + \gamma_i t + \epsilon_i,$$

where  $\alpha_i$  is a category-specific effect,  $\delta_t$  is a period-specific effect,  $\beta_t$  are a collection of ADMIN-specific effects, and  $\gamma_i$  is a category-specific linear time trend.

As we have a collection of never-treated controls and a single treated unit, there are no negative weighting concerns (Goodman-Bacon 2021). Standard errors are clustered at the level of the category.

### D. Difference-in-Differences Estimates

Figure 7 reports estimates of  $\hat{\beta}_t$  from equation (2) for a collection of outcomes. When there is a corresponding experimental estimate for MW3 in ADMIN, I plot that as a horizontal line in the postperiod. The imposition is indicated with a vertical dashed line.

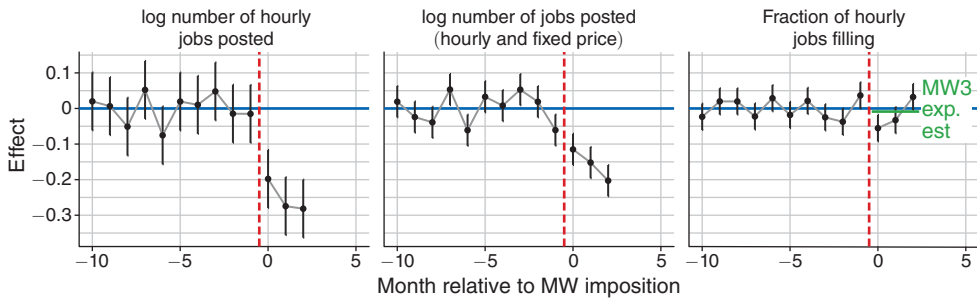
*Effects on Job Posting, Hiring, and Formed Matches.*—The effects on job posting and hiring are shown in panel A of Figure 7. The first outcome of interest is the log number of *hourly* jobs posted in the subcategory, labeled “log number of hourly jobs posted.” In the postperiod, we can see that there is about a 20–30 percent decline in the number of posted jobs in ADMIN. The mean postperiod effect (averaging over the  $\hat{\beta}_t$ ) is  $-0.25$  in log points. If we instead look at all jobs—fixed price and hourly—the reduction is much smaller, on the order of 15 percent. It is not shown, but when the outcome is just fixed-price jobs, the point estimates in the postperiod are all close to zero. While presumably some projects that would have been hourly contracts were reconceived as fixed-price projects, this was not fully offsetting, likely reflecting that switching contractual structures is not costless (Bajari and Tadelis 2001).

The substantial reduction in the number of hourly jobs posted in ADMIN suggests some employers decided the expected surplus from hiring was below the cost of posting a job (in terms of Section III, recall that even firms that still hired with a minimum wage can receive less surplus).

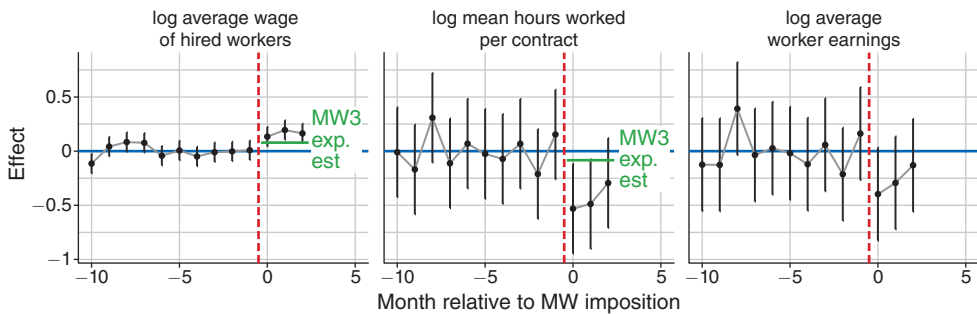
One implication of the reduction in ADMIN job posting is that all effects measured with the sample of filled or posted jobs could potentially just be a composition effect. This caveat aside, it is still useful to estimate these observational estimates and compare them to the experimental estimates.

The next outcome is the fraction of posted jobs that lead to a hire. For this outcome, we can compare the observational estimate to the MW3 ADMIN experimental estimate, which was a  $-0.01$  decline (but that the 95 percent CI includes 0). The difference-in-difference mean effect in the postperiod is  $-0.02$ . However, the point estimate is imprecise and not conventionally significant. Period by period, there is perhaps some evidence of a higher fill rate near the end of the period.

## Panel A. Effects on job posting and hiring



## Panel B. Effects on wages, hours worked, and the wage bill



## Panel C. Effects on labor-labor substitution proxies

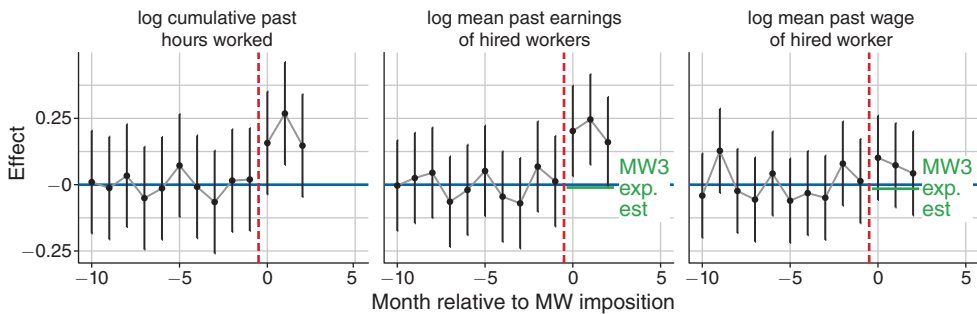


FIGURE 7. DIFFERENCE-IN-DIFFERENCES ESTIMATES OF EFFECTS OF PLATFORM-WIDE IMPOSITION

*Note:* This figure plots the by-week estimates of the treatment effects of the platform-wide imposition of the minimum wage.

However, the point estimate is imprecise and not conventionally significant. Period by period, there is perhaps some evidence of a higher fill rate near the end of the period. But because of the large decline in jobs posted, the net effect is fewer filled jobs.

*Effects on Wages, Hours Worked, and the Wage Bill.*—As in the experiment, if a job is filled, we observed hours worked, the average wage of the hired worker, and total earnings. The estimates for these results are plotted in panel B of Figure 7.

Starting with the “log average wage of hired workers,” the increase in ADMIN is substantial and quite close to what was observed in the experiment. Among those jobs where a hire was still made, hours worked per job declined by  $-0.44$  log points. This outcome is labeled “log mean hours worked per contract.” By comparison, the

effect from the experiment was a  $-0.08$  log points. This larger effect (if not due to sampling variation) is suggestive that the imposition screened out those jobs that would have had a high number of hours.

As in the experiment, the wage and hours worked effects are offsetting in sign. The panel “log average worker earnings” shows the effect on worker earnings, conditional upon being hired. However, the effects are imprecisely estimated, with confidence intervals easily incorporating huge positive and negative effects. As in the experiment, we lack the precision to conclude much.

It would be useful if there were some clean measure of output under different cells or perhaps some measure of net employer surplus. If, for example, workers are happier with clients because of the “gift” of high wages, we might observe some gratitude toward employers. The feedback both parties give at the conclusion of a match might proxy for this, but as I show in the online Appendix, there is no evidence of any difference in the rating workers give employers or employers give workers. One possible explanation is that this feedback channel is simply too inflated to offer a precise measurement of surplus, or efficiency wage effects are not real (Filippas, Horton, and Golden 2022).

*Effects on Labor-Labor Substitution Proxies.*—During the experiment, employers substituted toward more productive workers, as proxied by past wages and on-platform experience. I do the same analysis for hired workers by category post-imposition, reporting results in panel C of Figure 7.

In the observational analysis, we see the same shift in hiring toward more productive workers post-imposition. In the panel labeled “log mean past wage of hired worker,” we can see that hired workers had  $0.07$  log points higher past wages, compared to  $-0.02$  log points in the experiment. Although this measure is imprecise, it has a magnitude increase similar to what was found in the experiment.

Not only did hired workers have higher past wages, but they also had greater cumulative earnings and greater cumulative hours worked. This can be seen in the panels labeled “log mean past earnings of hired workers” ( $0.2$  log points higher in ADMIN post-imposition; the experimental estimate of this shift was  $0.05$  log points). I also include as an outcome “log cumulative past hours worked,” even though this was not an experimental outcome in Figure 4 in the interests of space, but it shows the same marked shift.

The observed shifts toward more productive workers are not just the same sign as in the experiment but also similar in magnitude. This suggests the “persuaded” critique of the experimental results was relatively unimportant and that substitution is a viable margin of adjustment even outside the experimental context.

#### E. Worker-Application Difference-in-Differences Estimates of the Effects of the Platform-Wide Minimum Wage

The experimental and observational evidence shows that firms adjusted to the platform-wide minimum wage by hiring more productive workers. A natural question is how this substitution affected workers in different parts of the pre-imposition wage distribution. The category-month panel is not suitable for exploring this question, as the shift occurs *within* a category of work. As such, I constructed a dataset

of all applications sent to job openings 14 days before and 14 days after the imposition date, as well as the same data from one year prior to the experiment, which I call the “placebo year.” I then compare the wage bid workers proposed and whether the application leads to a hire, conditioned on preperiod wage bidding for both time periods.

The reason for including the “placebo year” data is that new job seekers enjoy strong wage growth early in their tenure on the platform as they gain experience. As such, we expect workers originally bidding low amounts to bid higher in the near future. Without this knowledge of typical new worker wage dynamics, we might overattribute bidding up to the minimum wage in a pure event study.

As workers can send multiple applications, applications are nested within the worker. This structure is useful, as it allows for a within-worker estimate of the effects of the minimum wage on application behavior. To account for the nested structure of the data, I include worker-specific fixed effects and cluster standard errors at the level of the individual worker. I segment workers into “bands” based on their average wage bid in the preperiod. I then estimate a regression,

$$(3) \quad y_{ij} = \sum_{k \in K} \beta_k (POST_{ij} \times PREWAGEBAND_i^k) + c_i + \epsilon_{ij},$$

where  $i$  indexes workers,  $j$  indexes job openings applied to, and  $POST_{ij}$  is an indicator that the application was sent to a job opening posted after the imposition date of the platform-wide minimum wage. The  $PREWAGEBAND_i^k$  is an indicator for whether worker  $i$  had an average wage bid in the preperiod that was in the band  $k$ . The  $c_i$  is an individual worker fixed effect. The coefficients of interest are the collection of  $\beta_k$  coefficients. I also create a worker panel version of this dataset, aggregating within each worker period (pre and post) the total number of applications sent and the total number of hires.

Figure 8 plots the  $\hat{\beta}_k$  coefficients. The estimates are plotted using a solid line for the actual imposition year and a dashed line for the placebo year.

The top panel outcome is the worker’s individual wage bid in logs. In both the actual and placebo years, workers bidding a low wage in the preperiod bid higher in the postperiod. However, in the actual imposition year, workers with below-minimum wage bids in the preperiod bid substantially higher in the postperiod. Workers who were well above the \$3/hour minimum had essentially no change in their wage bids relative to the placebo (or the preperiod for that matter; all points estimates are close to 0). In short, there seems to be little evidence of any spillover effects of the minimum wage higher up the wage distribution.

The outcome in the second panel from the top is an indicator for whether the applying worker was hired for the associated job opening. We can see that in the placebo year there is essentially no change from the post- to preperiods, with all point estimates close to zero. In contrast, for the actual imposition year, we can see that those workers who had to bid up to meet the new minimum wage suffered a decrease in their success probability. To get a sense of the magnitude, consider the (2, 3] band workers, who bid about 10 percent higher relative to what they “should” have bid, given the increase in the placebo. This led to about a 1 percentage point decrease in the per application win probability. While this may not seem large, the average per application hire rate for workers in this band is just 0.018, implying that the per application success probability is less than half of what it was before the change. Interestingly, the decline in win



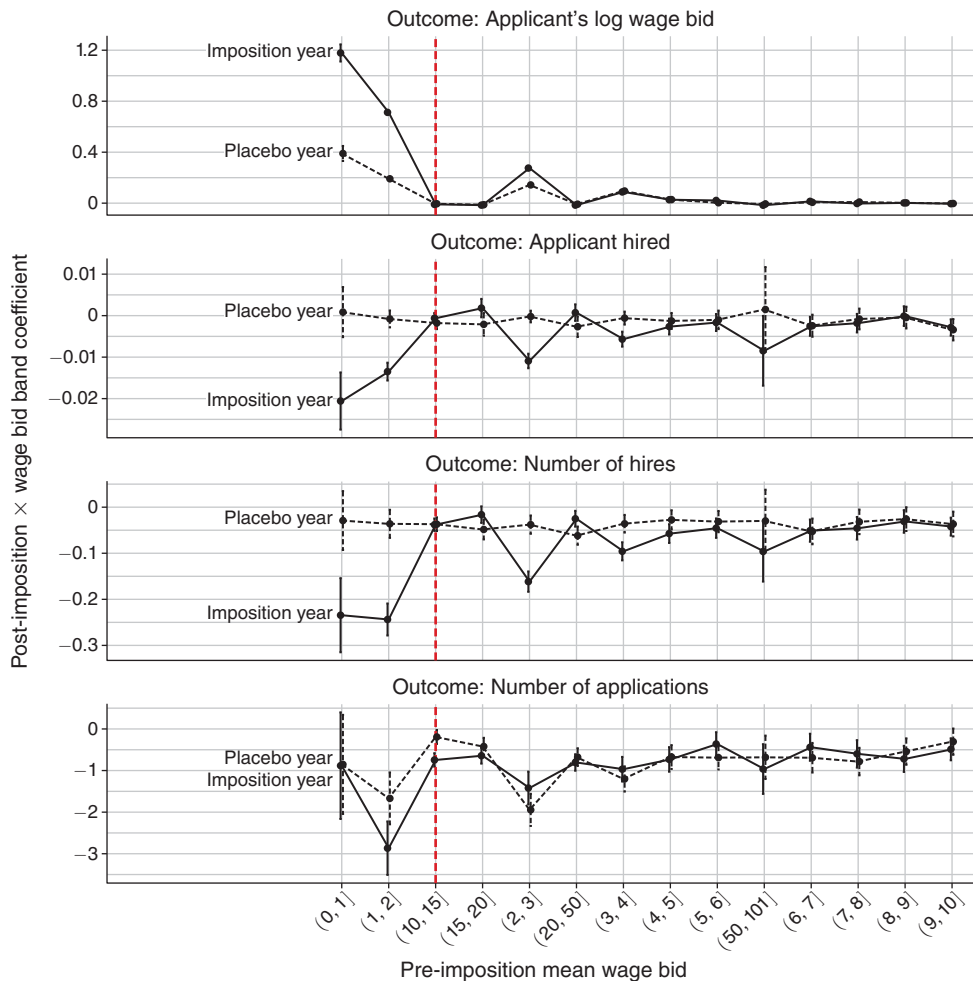


FIGURE 8. CHANGES IN WAGE BIDS AND HIRE PROBABILITY POST-IMPLEMENTATION OF PLATFORM-WIDE MINIMUM WAGE

*Notes:* This figure shows the  $\beta^k$  coefficients from equation (3). The sample consists of all job applications to hourly job openings 14 days before and 14 days after the minimum wage imposition. Standard errors clustered at the level of the individual worker. The top panel shows the change in wage bids in the postperiod relative to the preperiod, by preperiod average wage bid. The bottom panel shows the change in the application success rate relative to the preperiod, by preperiod average wage bid.

probability seems to extend beyond just those clearly bidding up, perhaps reflecting the overall decrease in the posting of ADMIN jobs.

Despite the decline in per application win rates, workers might potentially offset this reduction with a more intensive search. However, if other workers do the same, the equilibrium reduction in success probabilities might be even greater (or this already-large reduction in hire probability already reflects this equilibrium adjustment).

To explore the net effect, I switch to the short panel version. The outcome in the second panel from the bottom is the count of hires. If workers could compensate for decreased win probabilities with a more intensive search effort, the actual and

placebo years could be similar. Instead, for low-wage workers, we see a clear net decline in hires in the postperiod. In the bottom panel, we can see why: low-wage workers did not increase their application intensity and even perhaps decreased it.

## VI. Discussion and Conclusion

The experiment showed that for a firm facing a minimum wage: (i) the wages of hired workers increases; (ii) at a sufficiently high minimum wage, the probability of hiring goes down; (iii) hours worked decreases; and (iv) the size of the reductions in hours worked can be parsimoniously explained in part by the substantial substitution of higher-productivity workers for lower-productivity workers.

The observational findings from the market-wide imposition are that employers posted fewer jobs that would have likely paid low wages. The wage of hired workers increased substantially after the imposition of the minimum wage, in line with the experimental estimate. As in the experiment, firms substitute toward more productive workers. After the imposition, workers that historically bid below the minimum wage raised their wage bids substantially and experienced a reduced probability of being hired, on a per application basis.

A key finding of the paper is that labor-labor substitution is an important margin of adjustment for firms in this market facing a minimum wage. This kind of substitution is conceptually distinct from the typical framing of labor-labor substitution, in which workers have “types” but are imperfectly substitutable in the productive process, as in Katz and Murphy (1992). The substitution I find occurred within a pool of applicants who had all self-selected as being suitable for that particular job. The tasks are well defined, and so applicants are unlikely to offer radically different ways of performing the same task. Instead, the substitution is happening with respect to technical productivity, and yet in a competitive market, these workers should already be getting their marginal product. A natural question is whether the kind of labor-labor substitution observed in this setting can be reconciled with a competitive labor market model. The answer seems to depend on assumptions about the productive process.

One assumption is that a job has a fixed marginal technical productivity. With this assumption, labor-labor substitution cannot be an adjustment strategy; either the minimum wage is above or below the marginal product of the job. An alternative assumption is that workers can have heterogeneous technical productivity, with wages reflecting these differences. If applicants to the same job opening have heterogeneous technical productivity, labor-labor substitution as a response to a minimum wage is not only possible, it explains too much, in the sense that the minimum wage could be completely undone by substitution, so long as workers exist with productivity above the minimum wage. Firms buying labor in a competitive market face a horizontal supply curve for every possible level of technical productivity. If each worker is paid their marginal product, then firms could always substitute toward higher-productivity workers. But there are practical limits to this margin. In the experiment, some jobs facing higher minimum wages clearly go filled; after the imposition, employers clearly post fewer jobs where this kind of adjustment is necessary. At a high enough minimum wage, the hirable worker applicant pool is sufficiently thinned out that jobs go unfilled or firms do not find it worthwhile to post them.

In the experiment, the evidence for labor-labor substitution was clear with proxies for productivity but fairly obscure with respect to demographics. This is suggestive that prior conventional minimum wage research might tend to underestimate the importance of labor-labor substitution. Even then, Clemens, Kahn, and Meer (2021) finds evidence of workers still in jobs following statutory minimum wage increases to be better educated and older, and that firms respond to minimum wage increases by upgrading the skill requirements of jobs (Clemens, Kahn, and Meer 2021). If labor-labor substitution is important in conventional settings, it would have important implications for designing minimum wage policies. It suggests that higher minimum wages should be paired with more targeted exemptions for lower-productivity workers likely to be displaced.

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