Firm Frictions and the Payoffs of Higher Pay:

Labor Supply and Productivity Responses to a Voluntary Firm Minimum Wage*

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Abstract

What are the returns to higher pay for firms — and how much can these returns offset the effects of firms' monopsony power? We study a Fortune 500 firm's voluntary company-wide $15/hour minimum wage, which was had more bite in some of the firm's warehouses than others. In a continuous difference-in-differences design, we find that a $1/hour increase in pay halves worker departures (elasticity=9.6). This finite labor-supply response suggests that the firm had some monopsony power even in the thick labor markets around logistics hubs. Yet a $1/hour pay bump increases objective measures of worker productivity by 5.9% (elasticity=1.06). Our estimates indicate that productivity gains fully defrayed increased labor costs. Though workers' productivity response could offset the firm's monopsony power, internal documents suggest that this major firm did not fully account for the potential payoffs of higher pay when setting wages.

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Our profits, after paying good wages and a bonus. . . show that paying good wages is the most profitable way of doing business.
– Henry Ford in My Life and My Work, 1923

Workers have been leaving firms in record numbers in what has been termed the Great Resignation, with the majority citing low pay as a reason (Parker and Horowitz, 2022). Some large firms have responded to high turnover by increasing wages.[1] These wage increases for low-wage workers contrast with pervasive concerns that firms use monopsony power to depress wages (Azar et al., 2020). We ask whether discrete wage increases might arise from a similar source as firm monopsony power: frictions in the labor market.

Modern monopsony theory emphasizes how firms' wage-setting power can arise from subtle frictions. If the worker finds it costly to leave her current job for a new one — because of search costs, legal restrictions, or idiosyncratic preferences for the firm's amenities for example — then she is likely to stay at her current job, even if the pay is lower than what she could get elsewhere (e.g., Naidu and Yuchtman, 2013; Ransom, 2022).[2] These worker-frictions suggest that a worker need not live in a company town to have her employer set her wage. Instead, even in a thick market, her inertia at her firm can allow her employer to mark down her wage and pay her less than the marginal value that she contributes.

Yet the same logic argues that firms' frictions may blunt their wage-setting power. An employer might find it costly to replace one worker with another due to recruitment and retraining costs for example. Additionally, if firms have imperfect oversight and workers' effort improves productivity, firms may have additional

[1] For example, in 2020 and 2021 Best Buy, Target, Wayfair, CVS, Walgreens, and Amazon all announced voluntary minimum wages for their employees.

[2] Indeed, Robinson (1933) observed that monopsony power can emerge not only from geographic isolation but also from worker preferences and worker ignorance about other work opportunities. However, the latter two explanations remained largely unexplored until Manning (2003) focused on the frictional sources of firms' wage-setting power.

incentives to pay workers more. Collectively, we can consider these employer-frictions. Employer-frictions are consistent with efficiency wage models, in which high turnover costs and high returns to worker effort increase the profit-maximizing wage (Shapiro and Stiglitz, 1984; Yellen, 1984; Katz, 1986).

Given the potential for frictions on both sides of the market, it is an empirical question whether firms will set wages below, at, or above the wages that would prevail in a frictionless, perfectly competitive world. A finite labor-supply elasticity would indicate that worker-frictions give firms some wage-setting power (Manning, 2003). If increasing pay also improves productivity, then employer-frictions give workers back some power. Intuitively, if workers are more productive when paid more, the firm has an incentive to raise pay.

We bring empirical evidence on how employer-frictions and employee-frictions impact pay by analyzing the introduction of a voluntary firm-wide minimum wage in a Fortune 500 firm. We use objective productivity metrics and personnel data from warehouses. Our empirical design leverages the fact that a firm's voluntary minimum wage policy had differential impact across its several-dozen warehouses in a continuous difference-in-differences design. We capture a unique setting in which the size of the pay change is unrelated to the particularities of each warehouse.

Evidence from a large warehouse firm documents several important features of the labor market for warehouse workers. First, we find that a one percent increase in pay reduced turnover by nine percent. This labor-supply response is large but finite, suggesting that frictions in workers' labor supply decisions give firms some wage-setting power even in a labor market that would seem close to the competitive model. Second, we also find significant employer-frictions: productivity increases by one percent for every one percent increase in pay. Employer-frictions temper the firm's motive to depress wages. Third, when both sides of the market face frictions, raising pay can be good for both workers and firms. Yet firms may overlook these

potential Pareto improvements if they take a narrow view on the gains from raising pay.

Our case-study approach has two important advantages. First, we can gather insight about the firm's wage-setting practices. This institutional knowledge helps us craft our empirical strategy and unpack what factors may (or may not) enter into the firm's wage-setting deliberations. Second, within a single firm, we can analyze objective productivity data that we can directly map into the firm's bottom line. Our statements about the labor market and our inferences about how firm's make compensation decisions could be biased if this firm is not representative. Yet managers routinely switch between firms with similar operations and they have assured us that the nature of the warehouse work and the decision-making of corporate executives is emblematic of what they have seen in other large warehouse firms. Further, we provide empirical support of the generalizability of some of our findings by analyzing data on hundreds of warehouse firms that contract with a Fortune 100 staffing agency.

In order to identify the causal effect of higher pay, we must isolate variation in pay that is plausibly unrelated to other determinants of worker turnover and productivity. Our strategy relies on the fact that the company-wide minimum-wage policy had heterogeneous effects on warehouse-specific wages. When the firm's C-suite executives introduced a firm-wide minimum wage, some warehouses' pay rose by more than others'. Half of the warehouses already had base wages above \$15/hour and were thus unaffected. The other half increased wages for all workers so base wages were at the new minimum. Among the affected warehouses, larger wage increases were necessary in warehouses where initial base wages were further from the new \$15/hour minimum.[3] We consequently use a continuous difference-in-

[3]About half of the affected warehouses saw modest increases of \$0.50/hour since initial base wages were already \$14.50/hour; the other half saw wage increases between \$1/hour−\$2/hour since initial base wages were between \$13/hour−\$14/hour.

differences design.[4]

For our continuous difference-in-difference design to capture the causal effect of higher pay on turnover and productivity, a parallel-trends assumption must hold. Warehouses with different initial wages — and thus different exposure to the company-wide minimum wage — would have proceeded along parallel trends in the absence of the minimum wage policy. Institutionally, the company-wide minimum wage was imposed due to centralized C-suite decisions rather than local conditions at the affected warehouses. The minimum wage policy did not have warehouses in mind: indeed, pay had been recently recalibrated in warehouses and warehouse workers made up just over half of the workers affected by the policy.[5] Thus, this would seem to offer a relatively rare setting in which a firm raised pay in some warehouses but not others, without intentionally singling out particular warehouses for the raises. Empirically, we find stable pre-trends, null effects with placebo treatments at alternative times, and robustness to dropping individual warehouses and to limiting comparisons to warehouses in the same region and of similar types. Further, we find similar patterns around an earlier discrete pay jump at the Fortune 500 firm.

Warehouses are an ideal setting to study the impacts of a company-wide minimum wage. Warehouse work constitutes an important job for workers without college degrees, and warehouses have also been the site of many company-wide minimum wage policies (Derenoncourt et al., 2021).[6] Warehouse labor markets also offer a

[4]Our continuous difference-in-differences empirical approach is similar to those that have estimated the causal effect of the federal minimum wage using state-level variation in the share of workers who initially earned less the new federal minimum wage (Card, 1992; Bailey et al., 2021). Instead of leveraging state-level variation in the bite of a statutory, federal minimum wages, we leverage warehouse-level variation in the bite of a voluntary, firm minimum wage. A similar design also examines the impact of firm minimum wages on nearby firms' wages (Derenoncourt et al., 2021).

[5]The policy also impacted other workers at the firm, including administrative assistants and sales workers.

[6]Warehouse workers comprise about five percent of the US labor force. This figure is based off of the Bureau of Labor Statistics' Occupational Outlook Handbook, summing the 2020 number of Hand Laborers, Material Movers, Material Moving Machine Operators, and Material Recording Clerks (Bureau of Labor Statistics, 2022).

useful setting to test different theories of wage-setting because they appear similar to a perfectly competitive market. These labor markets provide a strong test of new monopsony models since many warehouse firms co-locate around ports, airports, railways, and other nodes on the supply chain: thus, only labor-supply frictions and not geographic isolation can generate firm wage-setting power. These labor markets also offer a setting where efficiency-wage theory is both relevant and testable, since the productivity of each warehouse is observable but the productivity of each worker is often not.[7]

Warehouse work is often seen as one in which workers are interchangeable, particularly when we imagine a fully automated warehouse. The warehouses we study are not fully automated, with machines picking and packing the parcels or even individuals doing so under the specific direction of a program. Rather, in the warehouses we study, a software program does not guide where parcels are stored, though they do guide where to retrieve them from. As such, managers report that it takes months for a worker to be fully up to speed. Especially in the firm's large warehouses, it takes time for workers to learn the warehouse's layout, understand where to put away different types of packages, become comfortable on new machines, and integrate onto teams. Indeed, we see a strong cross-sectional correlation between workers' average tenures and the warehouse's productivity, driven by the firm's large warehouses.

Firm managers also stress the potentially high returns to worker effort; to put boxes away quickly and carefully and retrieve them just as efficiently requires concentration. To test the returns to worker effort and generalize beyond the specific Fortune 500 firm, we use data from a Fortune 100 staffing agency. This complementary setting allows us to see individual workers placed at different firms in similar ware-

[7]The firm that we study is just starting to track the number of boxes moved by each individual. Firm managers have stressed the limitations of individual measures, given the potential spillovers across workers. A worker who moves a lot of boxes but knocks other boxes out of place along the way could easily slow down the operation of the warehouse on net.

house jobs with different pay rates. We find a strong cross-sectional relationship between pay and firm satisfaction scores, much of which persists within a given worker. This analysis provides suggestive evidence that some of the returns to higher pay are driven by increases in worker effort.

This paper makes three contributions. First, it underscores that frictions in workers' labor-supply decisions give firms some wage-setting power, even in a labor market that seems like it would adhere to the perfectly competitive ideal. On average, a \$1/hour (or 5.5 percent) increase in pay halved workers' departure rates, implying an elasticity of labor supply around nine. This impact suggests a large but finite elasticity of labor-supply to the firm, giving the firm some wage-setting power even in the thick warehouse labor market. This finding contributes to mounting empirical evidence of wage-setting power of firms in a wide variety of contexts, from nurses (e.g., Sullivan, 1989; Staiger et al., 2010), civil servants (Dal Bó et al., 2013), and school teachers (Ransom and Sims, 2010) to online workers doing narrowly defined tasks (Dube et al., 2020).

Second, the paper documents that employer-frictions give workers some market power and temper the firm's motive to depress wages. On average, a \$1/hour (5.5 percent) increase in pay led to a six percent boost in productivity. This finding suggests the continued relevance of efficiency-wage theory: Henry Ford's positive assessment of his 1914 firm minimum wage policy — as "one of the finest cost cutting moves we ever made" — could seemingly be equally applied to this modern firm's minimum-wage policy (Ford and Crowther, 1923). Our findings contribute to the large and important literature on efficiency wages (e.g., Shapiro and Stiglitz, 1984; Yellen, 1984; Katz, 1986): we use a clean identification strategy to study objective productivity metrics in a novel and highly relevant setting.

Third, the paper shows that when both workers and firms face frictions, higher pay can be good for both groups. Our calculations suggest that the reductions in

turnover and increases in productivity fully defrayed the increased labor costs associated with higher pay. Thus, efficiency-wage incentives offer an important counterbalance to firm monopsony power in wage-setting decisions.

The paper provides insight into firms' wage-setting decisions. Conversations with management and internal reports from the firm suggest that employer-frictions are not fully accounted for in wage-setting decisions. Together, this evidence indicates that firms may overlook the potential Pareto improvements from higher wages.

The rest of the paper is organized as follows: Section 1 introduces a model with two-sided frictions for both workers and employers. Section 2 introduces the data and setting. Section 3 introduces the firm's voluntary minimum wage policy and the impacts that it had on productivity and turnover. Section 4 probes the robustness of our findings by examining an exogenous pay jump that occurred at a single warehouse at the firm. Section 5 estimates the firm's return on their investment in higher pay and the inferences that the firm would draw if it only considered a subset of the payoffs of higher pay. Section 6 investigate the mechanisms by drawing on our case study and pulling in suggestive evidence from a Fortune 100 staffing agency. Section 7 concludes.

1 Model

We consider a partial equilibrium model of firms' wage-setting decisions in a frictional labor market. In the model, worker-frictions may prevent workers from seamlessly leaving their current firms, as in new monopsony models (Burdett and Mortensen, 1998; Manning, 2003). Yet employer-frictions may also prevent firms from lowering wages without reducing worker productivity, as in efficiency wage models (Shapiro and Stiglitz, 1984; Yellen, 1984). We use this model to organize the implications of our empirical analyses for firms' profit-maximizing wage-setting decisions.

We focus on a single firm in a market with many other firms. This set-up is appropriate for our empirical setting since warehouses tend to cluster around logistics hubs such as railway junctures and airports.

In setting wages, the firm maximizes profits. Profits are given by firm output (in dollar-terms), Y, less the wages paid to workers:

$$\max_{w} \Pi = Y(N(w)e(w)) - wN(w), \tag{1}$$

where w indicates wages, N indicates the number of workers employed by the firm, and $e(w)$ indicates the average productivity of each worker's labor, which can reflect both the composition and effort of the firm's workforce.

We focus on the firm's labor inputs and abstract from complementary inputs like capital, which are fixed in the short-term. We assume that the firm has diminishing marginal returns to labor $Y''(N) < 0$, creating a downward sloping labor demand curve. In the warehouse setting, there may be a limited number of boxes to move and congestion as the number of workers increases.[8]

We take the first order condition of Equation 1 and define two elasticities to arrive at an equation for the profit-maximizing wage. First, we define the labor-supply elasticity to the firm, which reflects how departures from the firm respond to wages, $\epsilon_{\text{Departures},w} = \frac{\text{Departures}'(w)w}{\text{Departures}(w)}$. The number of workers at the firm is a function of both the inflow of new recruits and outflow of departing workers. We make the standard assumption that changing the wage has the same impact on recruitment and departures since every worker who leaves one firm must be recruited by another firm. Thus, the labor-supply elasticity is assumed to be twice the elasticity of departures. Second, we define the productivity elasticity as $\epsilon_{e,w} = \frac{e'(w)w}{e(w)}$, which

[8] A downward slowing labor demand curve could also arise for other reasons, such as imperfect competition in the output market.

captures how workers' productivity responds to wages.[9] Using these two elasticities, we arrive at an equation for optimal wages, whose full derivation is noted in Appendix A:

$$w = Y'(Ne)e\left(\frac{2\epsilon_{\text{Departures},w} + \epsilon_{e,w}}{1 + 2\epsilon_{\text{Departures},w}}\right). \tag{2}$$

This optimal wage expression captures several useful predictions.[10] The expression shows that if productivity is increasing in wages ($e' > 0$ and thus $\epsilon_{e,w} > 0$), then it will be profit-maximizing to set a higher wage. Several (non-mutually exclusive) mechanisms can link productivity and wages. First, higher pay may enhance worker selection if workers' latent productivity and reservation wages are positively correlated (Weiss, 1980). Second, higher wages may deter shirking and other behavior that might lead to being fired, by making the job more valuable (Shapiro and Stiglitz, 1984). Third, workers may increase their effort if they feel that they are being paid fairly in a sort of gift exchange (Akerlof and Yellen, 1990).

This expression also captures a fundamental point about the labor supply curve. As the labor supply curve flattens out such that $\epsilon_{\text{Departures},w} \to \infty$, we arrive back in the competitive model where the firm pays wages equivalent to workers' marginal products. Yet as the elasticity of departures shrinks and the labor supply curve tilts upward, if effort is not responsive to wages ($e = 1$ and $e'(w) = 0$) wages may go below workers' marginal products : $w = Y'(N)\left(\frac{2\epsilon_{\text{Departures},w}}{1+2\epsilon_{\text{Departures},w}}\right) < \text{MP}$. An upward sloping labor supply curve may arise if, for example, there are few employers or if workers face frictions when considering alternative jobs (Robinson, 1933; Manning, 2003). The resulting wage-setting power leads to a wage markdown since the firm pays all workers the same and, thus, must weigh the benefits of attracting a marginal worker against the costs of increasing wages for inframarginal workers.

[9] We assume that firms know how workers will respond to higher pay. This assumption might be usefully relaxed to allow firms to err in their assessments of these responses or to be uncertain about these parameters.

[10] We assume that elasticities are locally invariant. Thus when wages are close to workers' marginal products, our optimal wage equation is more likely to hold.

This dynamic leads the firm to set an optimal wage rate below the workers' marginal product.

Finally, the expression captures the importance of noting *both* the labor-supply and productivity elasticities to wages. When there is both an upward sloping labor-supply curve ($\epsilon_{\text{Departures},w} < \infty$) and productivity responds to wages ($e'(w) > 0$), then the relationship between wages and workers' marginal products is ambiguous. In this case, whether wages are on par with marginal product depends on how $\epsilon_{e,w}$ compares to 1. Visually, accounting for worker effort and select may shift the effective labor-supply curve and the resultant question is: by how much? Thus estimating the response of productivity to wages becomes essential in understanding whether wages are below, at, or above workers' marginal product.

2 Data & Empirical Setting

We study a Fortune 500 firm. Our data come from several administrative sources, spanning from the beginning of 2018 to the end of 2021. The firm's human resources (HR) data track every hire, termination, and promotion (for example, from warehouse associate to team-lead) in each warehouse at the firm. From this HR dataset, we construct a running time-series of each warehouse's workforce, with information on pay and departure rates. Additionally, the firm's labor management system tracks each warehouse's weekly productivity, based on the ratio of the total boxes moved over the total hours worked. We supplement these primary datasets with quarterly worker satisfaction surveys.[11]

We focus on warehouse associates, excluding managers, mechanics, and contingent workers. Warehouse associates, whom we refer to as workers throughout the rest of the paper, comprise the core of the team of movers in each warehouse.

[11]The worker satisfaction survey dataset is available for a shorter period. We do not specify the precise period to preserve the firm's anonymity.

Describing the Samples. Table 1 summarizes the data on the firm's warehouses. To preserve the firm's anonymity, we identify neither the precise numbers of workers and warehouses nor the exact timing of the voluntary company-wide minimum wage. The first column includes all US-based warehouse associates at the firm. Between 2018 and 2021, the firm employed over fourteen thousand warehouse associates in more than fifty warehouses. The second column of Table 1 limits to the six months on either side of the firm's introduction of a $15/hour voluntary company-wide minimum wage. The third column limits to the six months on either side of a discrete pay jump in a single warehouse, which is examined in Section 4.

Though we do not reveal the exact timing of the company-wide minimum wage in column two, we do note that its imposition occurred during the pandemic but before the inflation that arose during 2022. Thus, the voluntary firm minimum wage reflects a real wage increase for workers. The discrete pay jump in column three occurred before the pandemic.

The Firm's Pay Formula. In the firm's warehouses, hourly compensation for warehouse associates follows a rigid pay formula. Each warehouse has a base pay rate, which averages just below $15/hour (row 1 of Table 1) and is always at least $1/hour above the state minimum wage (row 5). Workers' take-home pay can exceed the base rate for a couple of reasons. First, workers receive tenure premiums and skill premiums for being certified to operate machinery (e.g., forklifts). Second, workers receive premiums for working in night shifts, holiday seasons, and pandemic periods. On average, workers received an additional $2.30/hour in premium pay (row 2). Notably, these premia do not include any individual or group-level performance pay.

The firm applied its voluntary $15/hour minimum wage to base wages at each warehouse. As a result, half of the firm's warehouse workers were impacted by the firm minimum wage, even though many of them had take-home pay that already

exceeded $15/hour.

Terminations. As is often the case in warehouses, the firm has high turnover rates in its warehouses: each warehouse loses 11 out of every 100 worker monthly amounting to an annual turnover rate of 132% (row 4 of Table 1). The majority of these departures reflect quits (row 5) rather than fires (row 6).[12] Given the high rate of turnover at the firm's warehouses, the typical tenure is just over a year (row 7). We categorize those who are fired due to poor attendance as quits since they impact the warehouse the same way as a quit without warning would.

Productivity Metrics. The firm defines warehouses' output based on throughput — the average boxes received and shipped in the warehouse that week. To count as one unit of output, a box must move all the way from receipt at the loading docks to shipment on outgoing trucks.[13] On average, this process takes ten person-minutes —- or, put differently, each worker moves the equivalent of six boxes through the warehouse in the typical hour (row 4 of Table 1).

To understand what it means to move six boxes an hour, first note that workers spend two-thirds of their time actually moving boxes (row 5 of 1). The rest of workers' time is devoted to organizing and auditing inventory, cleaning up empty pallets and storage containers, meeting with their shift colleagues, and taking breaks. In each hour spent solely moving, the typical worker moves nearly nine boxes or about one box every seven minutes (row 6).

What happens in these seven minutes depends on the type of warehouse. There are two broad types of warehouses in the firm's logistics network: the "hubs" that store

[12]Quits and fires are not collectively exhaustive. There are also departures due to layoffs or simply never showing up.

[13]It is both possible and likely that a given box will spend time being stored in the warehouse before it is actually shipped. This is why warehouses typically count both receipt of goods (inflows to the warehouse) and shipments of goods (outflows from the warehouse) when calculating throughput.

goods in centralized locations and the "spokes" that connect hubs to consumers.[14] Each hub employs a few hundred workers in a warehouse considerably than a football field.[15] In the seven minutes that a box moves through a hub, it must must be received from the loading dock, unwrapped, checked by a quality assurance team,[16] put away for storage, retrieved when an order comes in, repackaged and relabeled as needed, and ultimately put on an outgoing truck either to another hub or a spoke warehouse. By contrast, in the small "spoke" warehouses, there is a quick turnaround time between receipt and shipping, generally without storage or much repackaging, simplifying the shipping process.

In the hubs, much of the work requires specialized knowledge. While warehouse operations at some other firms have automated picking (where a machine pulls the exact item and drops it on a sorting machine to arrive at a packager) or directed putaway (where an inventory system says where to place packages after unloading), this firm's inventory makes using either difficult. They do, however, use directed picking (where an inventory system says where a package is located). Consequently, a worker requires knowledge about the warehouse's layout, its computer inventory systems, and the way their work interfaces with others on their teams. Workers in the hubs often learn how to operate new machines on the warehouse floor, akin to "first learning how to drive on the highway" according to a warehouse manager. Workers also learn the art of finding the right place to stow a box, as they repeatedly solve puzzles of where to fit a potentially large and non-rectangular box into an open slot in the warehouse racks. Finally, many pieces of inventory cannot be put on pallets so workers must develop the muscle memory to physically move a package off of a (swaying) truck onto a rack at 40-feet in the air. Figure 1 depicts workers putting away and retrieving boxes in a hub and the scale of the warehouses.

[14] We group together fulfillment centers and middle mile locations together as "hubs" since these warehouses have similar workforce sizes and throughput.

[15] Indeed, some of the warehouses could fit several *hundred* football fields inside.

[16] A portion of every load received is checked by quality assurance. Depending on the result of the check, more pieces from the load may be checked.

3 A VOLUNTARY FIRM MINIMUM WAGE

We examine a Fortune 500 firm's implementation of a voluntary $15/hour company-wide minimum wage. First, we provide causal evidence on worker-frictions by identifying the effect of higher pay on labor supply. Second, we estimate employer-frictions by identifying the effect of higher pay on worker productivity.

3.A EMPIRICAL APPROACH

Our empirical design leverages the fact that the firm's voluntary implementation of a $15/hour minimum wage had more bite for some warehouses than others. Figure 2A illustrates the first stage of our design. Some of the firm's warehouses initially had base pay above $15/hour and did not see a pay increase after the minimum wage. The remaining warehouses had base pay below $15/hour and were consequently treated by the policy, with the intensity of the treatment determined by the distance from the initial wage to $15/hour. Warehouses with initial pay of $13/hour experienced a $2/hour increase in pay when the minimum wage was imposed, while those with initial pay of $14.50/hour saw a more modest increase in pay of $0.50/hour.

Figure 2B shows that the company-wide minimum wage shifted up the full distribution of take-home pay in affected warehouses. All workers in a warehouse with a base wage below $15/hour saw an increase in their pay — even if their take-home pay already exceeded $15/hour. Given this level-shift in pay, the imposition of the company-wide minimum wage did not change inequality in the warehouses nor dynamic incentives to, for example, acquire certifications or accumulate tenure.[17]

We use a continuous difference-in-differences design to leverage the heterogeneous

[17] Managers are salaried and thus did not receive an increase in pay when the minimum wage was implemented. While the minimum wage did compress the difference between warehouse associate and manager pay, we are not worried about changing incentives because no warehouse associate has made the transition to a salaried manager over the course of our data.

bite of the firm's minimum wage. We define Gap from $\$15_h$ as the gap in the warehouse's initial base pay from \$15/hour, with zeroes for warehouses with initial base pay at or above \$15/hour. Post_t is an indicator for whether week t occurs after the firm announced the minimum wage both internally and externally (and implemented it in workers' very next paycheck). For warehouse h at week t, we estimate a two-stage least-squares regression with the first stage

$$\$/\text{hour}_{h,t} = \beta_{1\text{st}}(\text{Gap from } \$15_h \cdot \text{Post}_t) + X'_{h,t}\rho + \mu_t + \mu_h + u_{h,t} \tag{3}$$

and the second stage

$$Y_{h,t} = \beta_{2\text{nd}}\,\widehat{\Delta\$/\text{hour}} + X'_{h,t}\phi + \mu_t + \mu_h + v_{h,t}. \tag{4}$$

where μ_t represents week fixed-effects, μ_h represents warehouse fixed effects, and $X_{h,t}$ represents time-varying warehouse controls.

Using a two-stage least-squares regression allows us to interpret $\beta_{2\text{nd}}$ as the effect of paying \$1/hour more even though the first stage is not one. In our preferred specification, the first stage is \$0.85/hour because the firm was less likely to subsequently raise pay of more treated warehouses. Including warehouse fixed effects and time-period fixed effects means that we are looking at differences within warehouses and within time periods so any warehouse-level or time-period-level differences will be absorbed.

For ease of interpretation, we also compute elasticities, or the percentage change in the outcome of interest over the percent change in take-home pay:

$$\epsilon_{y,w} = \frac{dy/Y}{dw/w} \quad \rightarrow \quad \hat{\epsilon}_{y,w} = \frac{\hat{\beta}_{2\text{nd}}/\bar{Y}_0}{\hat{\beta}_{1\text{st}}/\bar{\$}_0} \tag{5}$$

where $\bar{Y}_0$ and $\bar{\$}_0$ represent averages in the treated warehouses before the imposition

of the company-wide minimum wage.[18]

In each two-stage-least-squares specification, the unit of observation is the warehouse-week and the regression is weighted by the size of the warehouse, as measured by the number of boxes moved in the preceding year.[19] Given the relatively small number of potential clusters in our study, we supplement our robust standard errors with Fisher exact p-values that report how often random permutations of "treated" warehouses result in as large a placebo estimate as the true estimate (Fisher, 1925).

The voluntary company-wide minimum wage was implemented at one time across all the warehouses, so concerns over two-way fixed effects designs raised by De Chaisemartin and d'Haultfoeuille (2020); Callaway and Sant'Anna (2021); Goodman-Bacon (2021) do not apply. Our preferred specification uses a six-month post-period and a three-month pre-period to exclude the endogenous pay increases in some warehouses during the firm's recalibration of pay, which occurred just over three months before the company-wide minimum wage (see Figure 2). Our results are robust to alternative bandwidths.

Identifying Assumption. Identification relies on a parallel-trends assumption: warehouses with different initial wages would have proceeded along parallel trends had the firm not implemented its minimum wage. The top-down nature of the firm's minimum wage policy suggests the assumption is defensible. Regional managers were not consulted about the firm's minimum wage policy and, instead, only learned about wage changes in their warehouses after the policy was finalized, mere days before workers and the public were notified. As a result, local input did not de-

[18] Elasticities are often computed using log-log regressions. We cannot do so for departures, which are zero in some weeks at some warehouses. However, for productivity metrics, elasticities calculated using log-log regressions and those originating from Equation 5 are statistically indistinguishable.

[19] To measure warehouse size, we use information from the preceding year. We specifically look at same calendar months as the post-period but in the preceding year. This puts more weight on warehouses where we should have more precision in estimating the after-effects of the $15/hour minimum wage, without having the weights themselves be potentially impacted by the treatment.

termine local wage changes. Thus, this setting is a relatively rare instance in which wage changes do not reflect local warehouse idiosyncrasies or local labor-market conditions. Additionally, the firm minimum wage was not implemented in concert with other HR policies that may have targeted specific warehouses.

To probe the robustness of this identifying assumption, we include a variety of time-varying warehouse controls, $X_{h,t}$. We include region-by-week fixed effects, using firm-defined logistics regions. For productivity analyses, we further allow region-by-week effects to vary across hubs and spokes where productivity metrics may differ. We also consider robustness to including state-level minimum wages and weekly county Covid-19 death rates per hundred thousand individuals.[20] In addition to these controls, we consider placebo checks in earlier, untreated years to assess the potential for spurious, seasonal changes to produce the results. Section 4 also compares to a supplemental design using an earlier discrete change in pay at one warehouse.

3.B Effects of the Voluntary Firm Minimum Wage

We use the imposition of the voluntary firm minimum wage to identify the worker- and employer-frictions that determine wage-setting in our model. First, the effect on departures reveals worker-frictions: the less responsive departures are to pay, the more that worker-frictions give the employer power over the wage. Second, the effect on productivity reveals employer-side frictions: the more responsive productivity is to pay, the more that employer-side frictions offset employers' wage-setting power.

Departures. Increased pay decreases worker departures from the firm. Figure 3B illustrates this result. In the three months before the voluntary firm minimum wage,

[20] Our preferred specifications use Covid-19 deaths since they are less susceptible to measurement error from temporal and geographic discrepancies than testing. However, our results are robust to controlling instead for recorded cases.

out of every 100 warehouse workers, an average of 8.45 left per month. Around the imposition of the company-wide minimum wage, departures increased in control warehouses (the black circles in the left plot) due to market conditions. The treated warehouses did not see the same uptick, suggesting that the increase in pay offset forces prompting a worker exodus. This conclusion is summarized in the plot to the right: departures in the treated and control warehouses trended similarly until a few months after the imposition of the firm minimum wage at which point the patterns diverged. The effect on departures is delayed, a pattern which is also observed in other studies (e.g., Sandvik et al., 2021).[21]

Increasing pay by \$1/hour leads 4.5 fewer workers out of a hundred to depart each month, a 53 percent decrease from the initial departure rate (Table 2A). These results imply a departure-elasticity of 9.6 (95% CI = [2.19, 16.9]).[22] This response is larger than elasticities typically estimated in the literature (Dal Bó et al., 2013; Bassier et al., 2020; Hirsch et al., 2010), which may reflect the extremely thick nature of this market. Though large, the response is finite, suggesting that the firm still has some wage-setting power arising from labor-supply frictions, even in a market where firms are geographically clustered rather than isolated. Taken by itself, our model suggests that power over the wage would suggest that the firm would set a wage markdown of 5 cents on the dollar (95% CI = [3, 10]).

We employ three robustness checks on this result. First, we consider a placebo check that applies Equation 4 in other years, which reveals insignificant results with the opposite sign (see Table A.4). When pooled, we find that departures significantly *increase* in the treated warehouses in prior years. Second, we perform a permutation test that randomly reassigns which warehouses receive placebo pay increases:

[21] When we consider the estimates over a series of bandwidths in Table A.1, we see that month two after the company-wide minimum wage narrows the gap between treated and control departures.

[22] We compute the elasticity according to Equation 5, comparing the percentage change in departures to the percentage change in pay. We take the ratio between the 53 percent change in departures, and a \$1/hour change in pay, which represents a 5.5 percent increase in pay (\$1/\$18.03).

we find a one-sided p-value of 0.03 with 5000 repetitions, suggesting that it is unlikely this change could have resulted by chance (see Figure A.2). Third, we exclude each warehouse in turn, which reveals that the result is not driven by any specific warehouse (see Figure A.3).

Unpacking Departures. The decrease in departures is driven by quits (Table 2B). In addition to this revealed-preference metric of worker satisfaction, higher pay also improved workers' stated satisfaction. The bottom panel of Figure A.4 shows a marked improvement in treated warehouses' quarterly satisfaction or "net promoter" scores.[23] These two pieces of evidence are consistent with efficiency wage models that predict higher wages mean a job is more valuable to a worker.

The decrease in quits was sufficiently large that it offset an increase in firings (Table 2C). A $1/hour increase in pay increases firings by 1.2 per 100 workers per month, suggesting that managers were able to become choosier in whom to retain.

We find no effect on departures for other reasons (e.g., layoffs or bails), which serves as an effective placebo, since we would not expect pay to change these types of terminations.

Productivity. Increasing pay substantially increases productivity. In the left plot of Figure 3C, productivity sharply increased in the intensely treated warehouses (in blue diamonds) around the introduction of the company-wide minimum wage; productivity marginally increases in the slightly treated warehouses (in blue triangles); and productivity remains approximately constant in the untreated warehouses (in

[23]Worker satisfaction surveys ask employees "How likely are you to recommend [this employer] to a friend?" with options between 0 (not at all likely) and 10 (extremely likely). Responses are used to calculate a Net Promoter Score (NPS): those who respond with a 9 or a 10 are considered "promoters" and those who give a 0 to 6 are considered "detractors." The NPS is calculated as the percent of promoters less the percent of detractors.

A permutation test of the NPS result gives a one-sided p-value of 0.04 (see Figure A.5). We find similar results when we recalculate the relationship excluding each warehouse in turn (see Figure A.6). Finally, we consider whether the same relationship existed in the other year for which we have data. In the other year, Equation 4 yields a point estimate of -0.727 (se = 5.99).

black circles). Thus, when the firm implemented a voluntary minimum wage, the productivity in its lowest paid and least productive warehouses caught up to the productivity in the rest of the firm.

The plot to the right summarizes this pattern by estimating a dynamic version of Equation 4. In the three months leading up to the imposition of the firm's minimum wage, productivity trended similarly across the firm's fifty-plus warehouses, with each worker moving 6.8 boxes per hour. When the minimum wage was announced and implemented, productivity sharply increased in the warehouses that were more exposed to the minimum wage relative to those that were less exposed. This gap persisted over the next six months.

On average, a \$1/hour increase in pay led to an increase of 0.40 boxes moved per hour, indicating a productivity increase of 5.9 percent (Table 3A). This corresponds to a productivity elasticity of 1.06.[24]

We consider several robustness checks. First, we check placebos around the same months in two other years, which reveals no significant estimates from Equation 4 (see Table A.5). Second, we perform a permutation test that randomly permutes the intensity of the treatment between warehouses: this exercise suggests a one-sided p-value on the estimate of 0.15 (see Figure A.7). This larger p-value stems from the idiosyncratic variation in boxes moved that makes it more difficult to say with certainty that the estimates that we find could not have arisen by chance. For the larger and more centralized hub warehouses — which are less affected by idiosyncratic shocks to local demand — we find a larger point estimate (see Table A.6). Third, we check that our results are not sensitive to excluding any particular warehouse (Figure A.8). Fourth, we show robustness to alternative bandwidths in Table A.2.

[24]We calculate the elasticity by scaling the reduced form increase (0.401 boxes/hour) by the baseline productivity (6.85 boxes/hour) and dividing by the percentage increase in pay \$0.86/\$18.03. Calculating the elasticity by using a log-log regression finds a (statistically indistinguishable) elasticity of 0.951.

The strong link between productivity and pay tempers the firm's incentive to exercise its wage-setting power by marking down pay. Incorporating the productivity response into the wage-setting equation would suggest that the firm would maximize profits by not setting any markdown on the wage, with a ratio of the wage to the marginal product of 1.003 (95% CI = [0.85, 1.10]).

Unpacking Productivity Changes. To understand the source of the increase in productivity, we decompose it into two components in the hub warehouses: (i) how much time are workers spending moving boxes (as opposed to doing indirect work)? and (ii) how many boxes do they move in an hour dedicated to moving? We find that 58.3 percent of the increase in productivity can be attributed to an increase in the share of hours that workers are spending moving boxes (see Table 3B). This is consistent with the warehouse operating more smoothly and thus needing less time to be spent on rearranging inventory, cleaning up the warehouse, or meeting with managers. Section 6 tries to diagnose the underlying sources of the productivity changes.

One might worry that with increased time spent moving, workers end up injuring themselves more frequently. On the other hand, if workers tend to be more experienced when pay is higher, they may be less likely to be injured. We explore this possibility using data on worker accidents and injuries as well as time away from work due to accidents and injuries. These are data that the firm prepares and ultimately reports to Occupational Health and Safety Administration (OSHA).[25] We find suggestive evidence that accidents and injuries are marginally reduced after the pay change. Beforehand, over the course of a year, about one in twenty workers might experience an accident or injury significant enough to report. Our point estimate would suggest that $1/hour more in pay is associated with a 2.8 percentage

[25]Firms in specific industries with more than 250 employees are required to report all accidents, injuries and fatalities to OSHA each year in Form 300A. Reporting does not indicate that the firm was at fault. Injuries include anything from a broken digit to a muscular pain to exposure to chemicals. Happily, fatalities are too rare to observe any change in our period.

point decrease, although the result is not statistically significant (Table A.3).[26] At a minimum, these results suggest that the increase in productivity is not causing a dramatic uptick in injuries.

Taking Stock. In sum, we find significant frictions on both sides of this labor market. Workers face frictions in moving to new jobs, leading to an upward-sloping labor supply curve to the firm. Employers, however, also face frictions that make it costly to reduce pay.

We measure a labor-supply elasticity of ten. We estimate a productivity elasticity of one. Section 5 relates the frictions we measure to the wage markdown and the firm's return on wage investments but first we consider a different source of identifying variation.

4 A Pay Jump

To probe the robustness of the estimated elasticities, we leverage another quasi-experimental pay change that occurred in one of the firm's large hub warehouses prior to the pandemic.

4.A Empirical Approach

In the course of a couple of weeks, the warehouse's pay jumped by fifteen percent from \$15.70/hour in July to \$17.70/hour by September (see the top panel of Figure 4).[27] We leverage the discreteness of this pay increase in an interrupted time-series

[26] This analysis differs from the main text in two ways: First, we do not use the same human resources data. Rather, we use the personnel hours reported to OSHA to calculate full-time equivalent workers, because injuries are reported for all workers, not just associates. The reported hours number includes overtime as well as hours for workers beyond the associates whom we study in our paper (e.g., temporary workers). Second, the firm reports these data at a coarser level than the weekly units of analysis that we have used so far, so the analyses do not include weekly fixed effects, but do include the other controls as in our main specification.

[27] We do not reveal the particular year in which the pay jump occurred to protect the firm's anonymity but note that it was prior to the pandemic.

design. Our first stage is:

$$\$_t = \alpha_0 + \delta \text{Post}_t + \nu_t \tag{6}$$

and our second stage is:

$$Y_t = \alpha_1 + \beta_\$ \hat{\$}_t + \epsilon_t \tag{7}$$

where $\beta_\$$ is our parameter of interest.

For $\beta_\$$ to capture the causal effect of increasing pay, other changes in the treated warehouse must not coincide with the pay change. Gauging the credibility of this assumption requires background on the pay change. The regional manager at the warehouse had long been concerned about high turnover rates, due to intense local competition and grueling work in this warehouse, which handles large, unwieldy parcels.[28] Yet it took C-Suite decision-makers months to get around to the proposal. Thus, while it was not random that this warehouse saw a pay increase, the precise timing was plausibly exogenous.

The warehouse's regional manager further confirmed that the pay change did not coincide with a change in the nature of the work nor the conditions in the local labor market. Empirically, we do not see contemporaneous changes in the observable characteristics of the warehouse workers (Table A.9). We have also independently verified that there was no contemporaneous change in hiring by nearby rival firms.

As with the company-wide minimum wage, the pay increase resulted in a rightward shift of the whole pay distribution (Figure A.12) so did not affect dynamic incentives nor feelings of (in)equality.[29]

[28] Indeed, in the quarter before the pay change, the turnover in this warehouse was double that in the rest of the firm.

[29] Since this pay change occurred throughout the associate workforce, but not the managerial workforce, one might worry that the pay differential between these two rungs was compressed. However, to date, no one has been promoted between those two levels of the warehouse, so we do not believe the constant managerial pay affected worker incentives.

4.B Effects of the Pay Jump

Departures. After pay increases, workers depart the warehouse at a lower rate. As illustrated in Figure 4B, in the six months before the pay jump, 19.6 workers per 100 would leave per month. Paying an additional $2/hour decreases departures by 7.7 per 100 or 39.4 percent. Compared to the 15 percent increase in pay, this represents a turnover elasticity of 3.07.[30] Departures did not change in two other warehouses that were within a 13-minute drive of the treated warehouse but did not see a change in pay (Table A.12); this null result supports the idea that the pay change did not coincide with spurious changes in the local labor market.

Similar to the results from the company-wide minimum wage, these departure effects suggest a large but finite labor-supply elasticity, suggesting the firm has some degree of wage-setting power. Using only the relationship between departures and pay suggests that the firm would set a wage-markdown of 14 cents on the dollar (95% CI = [24, 10]).

Productivity. Productivity also increased after pay discretely increased. Figure 4C illustrates this change. In the six months after the pay increase, workers in the warehouses moved an additional 0.784 boxes per hour over their baseline average of 4.88 boxes per hour. For each dollar increase in pay, this represents an increase of 0.389 boxes per hour or a 7 percent increase in productivity (Table A.10). This corresponds to an elasticity of 1.25, which is remarkably similar to the elasticity of 1.05 measured in the voluntary company-wide minimum wage.[31]

[30] As with the voluntary firm minimum wage, the overall effect on turnover is driven by voluntary quits, with no impact on being fired in this context. There are no other departures such as layoffs or bails so the result is not shown in Table A.10 where the other IV estimates are shown).

[31] The increase in boxes moved primarily stems from workers moving boxes faster rather than spending more time moving boxes (Table A.10). This different source of the productivity change here relative to that found in Section 3 may also reflect the particular demands of this warehouse's work: when moving large boxes, there may be more scope for effort, experience, and selection to increase workers' speed in working in teams and operating machines.

We do not see similar changes in productivity in "twin" warehouses that handle similarly large parcels as the treated warehouse, but did not see a change in pay (see Table A.12).[32] This null suggests that the productivity increase was not driven by a spurious demand shock for large parcels.[33]

When we incorporate this productivity effect into the optimal wage-setting equation, it suggests that the firm would optimally set no markdown on the wage rather than a markdown of 14 cents in a world with solely worker-frictions.

Taking Stock. In the context of the discrete pay jump, we find significant frictions on both sides of this labor market, supporting the conclusions from the analysis of the firm-wide minimum wage.

5 Returns to Raising Pay & the Profit-Maximizing Wage

We now consider how our results impact wages. We take two approaches: one that mimics the thinking of the firm's finance department, one that uses the model from Section 1.

Return on investment. The firm's finance team uses a return-on-investment framework, where they consider: if we pay workers $1/hour more, how will this affect our bottom line? Consequently, we perform the same math, under different assumptions of which frictions the finance team acknowledges.

The overall cost to the firm of increasing wages by $1 per hour is actually $1.30 per hour after associated taxes and overhead are included. So when we calculate gross returns on a $1 per hour increase in workers' take-home pay, we consider the cost

[32]The treated warehouse and its two twin warehouses handle large parcels the size of refrigerators or sofas. Other warehouses handle parcels the size of toasters or tea towels. The twin warehouse are not geographically proximate since they each handle different segments. As such, so there will not be any spillovers between the warehouses.

[33]For the placebo analysis, we do not scale by the size of the pay jump since the other warehouses do not feature a pay jump. We run the reduced-form regression.

to the firm to be $1.30 (left-most estimates in Figure 5A).

When the firm's finance team recognizes employer-frictions related to departures, the costliness of the wage investment shrinks to about $0.78 (middle estimates in Figure 5A). We find that an increase of $1 per hour means that 4.4 fewer workers per hundred employees leave each month, yielding a savings of $8,580 per 100 workers per month (4.4 fewer turnovers · $1,950 cost per new recruit). Since the firm has to pay 100 workers who worked 21 eight-hour days in a month, $1/hour more in order to realize this change, the cost to the firm would be $21,840 (100 workers · 168 hours/month · $1.30/hour). Thus, the firm's gross return on a $1 hourly wage increase would only be $0.39 ($8,580/$21,840), not enough to defray the $1.30/hour increase in labor costs.

Inclusive of higher productivity, however, the costs of higher wages are fully defrayed (right-most estimates in Figure 5A). Indeed, if, the firm recognizes the relationship between pay and productivity, it will see that the net returns are positive. Based on hourly pay in the treated warehouse, in the two quarters before the pay jump, the firm was spending $3.39 per box moved ($17.88 in hourly wages · 1.30 in taxes / 6.85 boxes moved per person-hour). Since the higher pay increased the warehouse level productivity by 0.401 boxes per person-hour, the gross return on a $1 pay increase, which costs the firm $1.30/hour, is $1.36.

When we perform the same exercise using estimates from the pay jump that occurred within a single warehouse, we find notably similar results, with a point estimate of a $1.76 return on a $1.30 investment. Of that, $0.44 arises from a decrease in departures, with the remainder coming from an increase in productivity. In this case, the standard errors are smaller and clearly rule out a loss.

Wage Markdowns. Our results suggest that there are significant frictions on both sides of the market. While worker-frictions alone would imply that the firm could

pay workers a markdown, employer-frictions mitigate the firm's incentive to use its wage-setting power to mark down the wage. Yet firms like this one might respond to — or instead overlook — these frictions when setting wages.

Indeed, conversations with the firm suggest that only some of the market frictions are considered in wage-setting decisions. To understand how an incomplete accounting would impact decision-making in this context, we return to our model. We consider firms' perceived profit-maximizing markdown of wages relative to marginal products when incorporating different frictions in Equation 2.

Suppose the firm only acknowledges worker-frictions (e.g., from search costs). The firm then recognizes that it has wage-setting power but does not recognize its own employer-frictions. In this case, the firm would set a wage-markdown of 5-cents on the dollar, paying \$0.95 per marginal dollar of value (95% CI = [0.81, 0.97]) using the estimates from the company-wide minimum wage. Estimates from the wage jump suggest a more extreme 14-cent markdown (95% CI = [0.76, 0.90]). This is illustrated in the left-most set of estimates in Figure 5B.

The firm we study does recognize some of its own frictions, but takes a fairly narrow view of them. Internal estimates of the cost of training a new worker and admin costs such as drug testing, badges and other associated overhead suggest that the firm pays at least \$1,950 per new recruit. If the firm acknowledges this part of employer-frictions, then the markdown is nearly halved and is only 3 cents, with pay at \$0.97 per dollar of marginal value (95% CI = [0.83, 0.99]). Estimates from the wage jump are also down to only a 9-cent markdown, paying \$0.91 per dollar of marginal value (95% CI = [0.81, 0.96]).

If the firm fully acknowledges the relationship between pay and productivity, then wages are much closer to workers' marginal products, and are conceivably even higher than workers' marginal products. We find a positive point estimate, with

workers being paid a *markup* of 3 cents: $1.03 (95% CI = [0.88, 1.32]). Estimates from the wage jump suggest a strictly positive markup: $1.11 (95% CI = [1.02, 1.35]). Does Of course, these estimates are local, reflecting the consequences of a change in pay from a particular starting pay level. Increasing pay would likely not always have such pronounced impacts on productivity or turnover. For example, if wages had started at 50 percent above their present level, a comparable increase in pay would likely have a much more muted impact. Nevertheless our results are striking since they suggest that the baseline pay is not profit-maximizing.

Estimate Limitations. A drawback of these estimates is that they are incomplete. We particularly do not include two key factors that might be changed by pay. First, the estimates do not include changes in shrink, an industry term for theft from warehouses. We suspect that shrink would decrease with higher pay since workers may be more keen to keep their job or may have less need. Second, we do not include damages. The sign of the change in damages is unclear. Higher pay might mean more experience and competent work occurs in the warehouse and damages decrease. Alternatively, the increase in productivity might cause congestion and increase damages.

That higher pay likely paid for itself has several important implications. First, when there are both worker- and employer-frictions, higher pay can be profit-maximizing. This is particularly possible if pay was set sub-optimally low beforehand. Second, the positive returns to the firm minimum wage (and the conversations with firm managers) suggests seems that even highly competent firms can fail to incorporate employer-frictions when setting wages. This failure to incorporate employer-frictions can indeed lead to pay being set sub-optimally low.

A positive net return is consistent with the market's reaction to the voluntary minimum wage. When the minimum wage was announced and implemented, the firm's stock prices increased over the prior month's average by 12 percent (95% CI = [9,

16]).[34] This result is robust to using 2-week, 1-, 3-, or 6-month bandwidths and to using opening, closing, high or low prices of the day. We further use a permutation test, imagining that the minimum wage had been announced in any other day of the surrounding years, excepting the one-month bandwidth around our estimated change and the first 6 months of the pandemic. Our estimated effect is larger than all but 1.6 percent of the placebo estimated effects.

6 MECHANISMS

To better understand the payoffs of higher pay, we present suggestive evidence on the underlying mechanisms, using both data the focal firm and from other firms who contract with a Fortune 100 staffing agency.

6.A EXPERIENCE EFFECTS: EVIDENCE FROM THE FIRM

A seasoned worker seems to have high value to the firm. Figure 6 provides suggestive evidence that a higher average tenure in the warehouse is associated with greater productivity, particularly in the more complicated hub warehouses. In the cross section, an additional month of experience is associated with 2.8 percent higher productivity. The returns to worker tenure may be one reason why productivity increases so starkly after new warehouses open and workers start to build experience (Figure A.9).[35]

We can do a back-of-the-envelope decomposition of the productivity increase into tenure and other factors using the cross-sectional relationship between tenure and productivity. Taking the cross-sectional correlation between tenure and productivity

[34] To preserve the firm's identity, we do not provide graphical information or pre-minimum wage share prices.

[35] Effects of experience may be compounded if supervisors can spend more time investing in existing workers when they do not need to new workers. Indeed an internal document from the firm noted that lower turnover helps supervisors focus on "training, engagement and upskilling/ career growth for their frontline teams."

at face-value, the increase in tenure that results from the firm-wide minimum wage would increase productivity by 2.2%, recouping almost half of the increase in labor costs.[36] This decomposition suggests that 38% of the productivity increase could be attributable to the increase in worker tenure, with the remaining 62% coming from other factors, such as improved employee selection (if higher pay allows the firm to fire under-performers) and overcoming imperfect monitoring of employee effort.[37] This pattern suggests that the returns to worker experience may be a key driver of worker power when viewing profit-maximizing wage-setting in a frictional world.

6.B Incentive Effects: Evidence from a Staffing Agency

To provide evidence on the incentive effects of higher pay, we turn to a Fortune 100 staffing agency that places many individuals in warehouse positions. This is an entirely different company from the Fortune 500 firm whose data is used throughout the rest of this paper. This staffing agency placed workers in over 222,000 warehouse jobs between 2016 and 2018.

The data includes all of the assignments a worker was placed in through the staffing agency. For each assignment, we observe the pay rate, the reason that the temporary assignment concluded (e.g., the work was over, the worker quit, the worker was fired, etc.), and the rating given by the manager at the firm ("Excellent," "Good," "Fair," "Poor").[38] On average, temporary warehouse jobs through the staffing agency

[36] A $1/hour increase is associated with a 1.1 month increase in tenure, reflecting a 7 percent bump. On average, a one month increase in average tenure is associated with 0.135 additional boxes moved per hour in the warehouse. Taken together this would increase boxes moved by 0.148 boxes per hour or 2.2% of the baseline mean of 6.85 boxes per hour.

[37] If a firm recognizes the returns to tenure, they may set wages with an eye toward increasing tenure. We extend our model from Section 1 to a two period model in Appendix A. The two-period model makes similar predictions as our core model about how frictions would change the mark-down.

[38] When a firm hires through this staffing agency, it sends the agency a description of the job and the pay rate. In select cases, the firm may ask the staffing agency for a particular worker with whom the firm has had a positive prior experience, but in most cases it is up to the recruiter to locate and present potential candidates. Some firms allow room for negotiation on wages, but many refuse to negotiate on wages since they have set their advertised wages to match their full-time workers and

last 3.4 months, with an hourly pay of \$11.74/hour. Only 44 percent of these jobs are completed, with 31 percent of people quitting. Only 13 percent of workers receive an "Excellent" evaluation from the hiring firm's managers.

Because we observe the same worker in several jobs, the staffing agency data offer a valuable opportunity to decompose the effects of pay into selection of better workers versus incentives within the same worker.[39] To do this, we begin by estimating the reduced-form relationship between pay and performance:

$$Y_{ij} = \beta_0 + \beta_w \cdot w_j + \mu_{o(j),c(j)} + \mu_{d(j),c(j),t(j)} + u_{ij}. \tag{8}$$

where i indexes the worker; j, the job; and t, the time. We let o denote occupation; c, the commuting zone, and d, the commuting zone. By including these fixed effects, our estimates are thus identified off of variation in hourly pay across firms and workers in the same local labor market and industry.

To isolate the incentive effects of higher pay, we look at the relationship between pay and assignment outcomes *within* individual workers who work multiple jobs at the staffing agency:

$$Y_{ij} = \psi_w \cdot w_j + \underbrace{\mu_i}_{\text{Worker FE}} + \mu_{o(j),c(j)} + \mu_{d(j),c(j),t(j)} + u_{ij}. \tag{9}$$

We estimate both specifications for the sample of workers with multiple jobs through the agency, since these workers identify the within-worker effect of higher pay.

Table 4 presents the results of this analysis. We find that an additional dollar of pay increases job completion by 2.6 percentage points, off a base of 40 percent

they do not want to create strife.

[39] Of the workers who took a job through the staffing agency, 64 percent did not return in our period for a second job. But for a notable minority of workers, the agency provided continuing stints of work: 5.5 percent of workers take at least five jobs with the agency and are employed for a total of 263 days on average.

completion—an elasticity of 0.72. Of that effect, 83 percent arises within the same worker. A similar share of the quits can be attributed to behavioral responses within workers.

We can also use "Excellent" evaluations as a metric of worker performance. While not the same as on-the-job productivity, it is nevertheless a useful metric of performance insofar as it captures firm satisfaction with the worker. We find that 50% of the increase in excellent evaluations associated with higher pay arises within the same worker.

Together, these results suggest that the positive impact of higher pay on labor supply and firm satisfaction appear to at least partially arise from the incentive effects of higher pay on the performance of individual workers.

7 Conclusion

This paper uses a Fortune 500 firm's voluntary firm minimum wage to examine the warehouse labor market. While frictions that workers face give the firm some wage-setting power such that wages can be set below workers' marginal product, frictions faced by the firm offset this power. In a world with two-sided frictions, higher productivity can fully defray the costs of higher pay. Thus when both firm- and worker-frictions are acknowledged, higher pay can be a Pareto improvement.

We measure workers' labor supply and productivity response to a voluntary firm minimum wages, using objective productivity and human resources data. A 1 percent increase in wages is associated with a 9 percent decrease in worker departures from the firm. This is much higher than other measures, consistent with this market being quite thick with many firms co-located around cargo hubs. Nevertheless, the measure is finite, indicating that the firm has some wage-setting power. If the firm focused only on worker-frictions, we estimate that they would set wages at 95

percent of workers' marginal product.

However, employer-frictions offset some of this wage-setting power. If the firm recognizes recruitment and retraining frictions, wages' markdown relative to workers' marginal product would be halved. Further, higher pay could increase worker effort and improve worker selection. We find a 1 percent increase in wages is associated with a 1 percent increase in boxes moved per person-hour in the warehouses. Accounting for this relationship, firms may actually have an incentive to mark *up* wages relative to workers' marginal product.

Our paper underscores the importance of considering the effects of wages on both productivity and labor-supply when diagnosing labor-market power. In our context, analyzing labor-supply effects alone would suggest that firms have considerable market power to depress wages even in thick labor markets. Yet recognizing the link between pay and productivity suggests that firms have limited incentive to act on their market power by reducing wages. Amidst declining union power, increased automation, heightened trade, and other threats to workers' bargaining power, the continued relevance of efficiency-wage models is one note of optimism for workers' power over wages (albeit indirectly).

Employer-frictions may not aid all low-wage workers. We found evidence of a stronger link between pay and productivity in warehouses where work is more complicated and requires more knowledge. Further, the link between pay and productivity can only inform wage-setting decisions if firms account for it. We found suggestive evidence that the firm that we study may fail to fully account for the returns to higher pay when considering the profit-maximizing wage. Not acknowledging the full impacts of pay would lead a firm to set it's pay sub-optimally low.

Our paper, consequently, underscores the value of opening the black-box of firm wage-setting decisions in a world in which labor-market frictions give firms some

wage-setting power. We studied this question in the context of one important labor market — warehouse work — primarily through the lens of one firm. Looking inside firms in other contexts would give us a deeper understanding of how firms make compensation decisions; what returns firms consider (and ignore) when contemplating increasing pay may differ across types of workers. If firms are particularly likely to overlook the productivity effects of increasing pay for low-wage workers, this oversight could be a key contributor to inequality.

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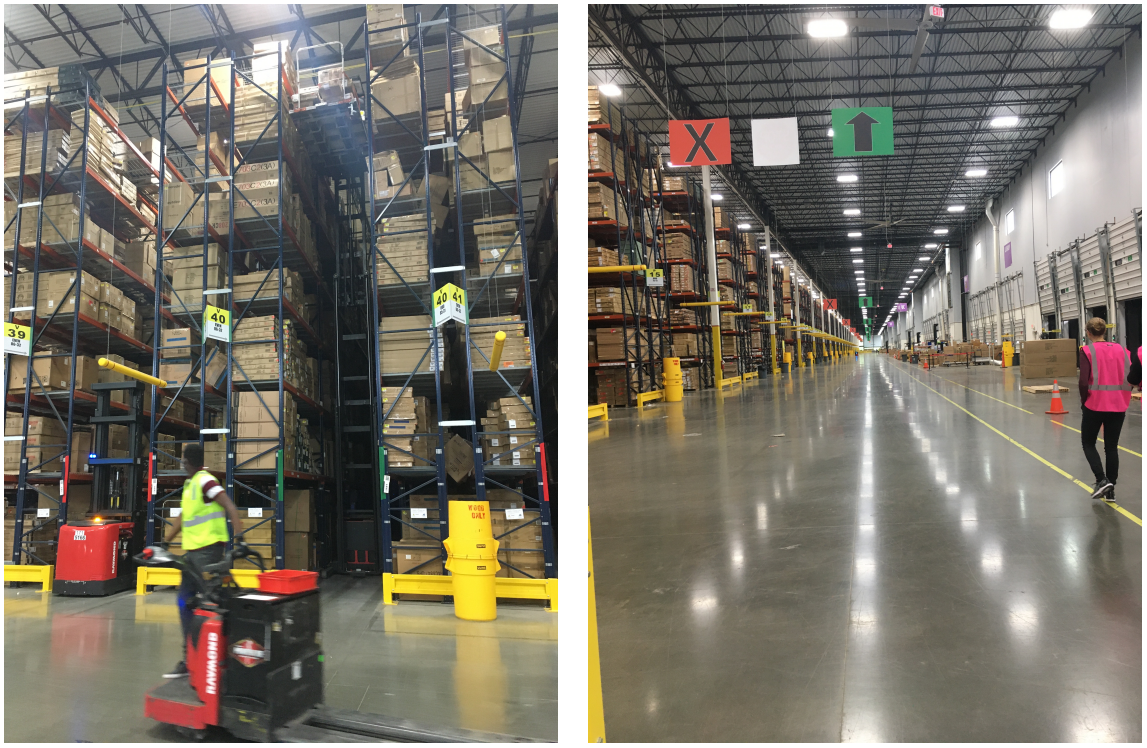
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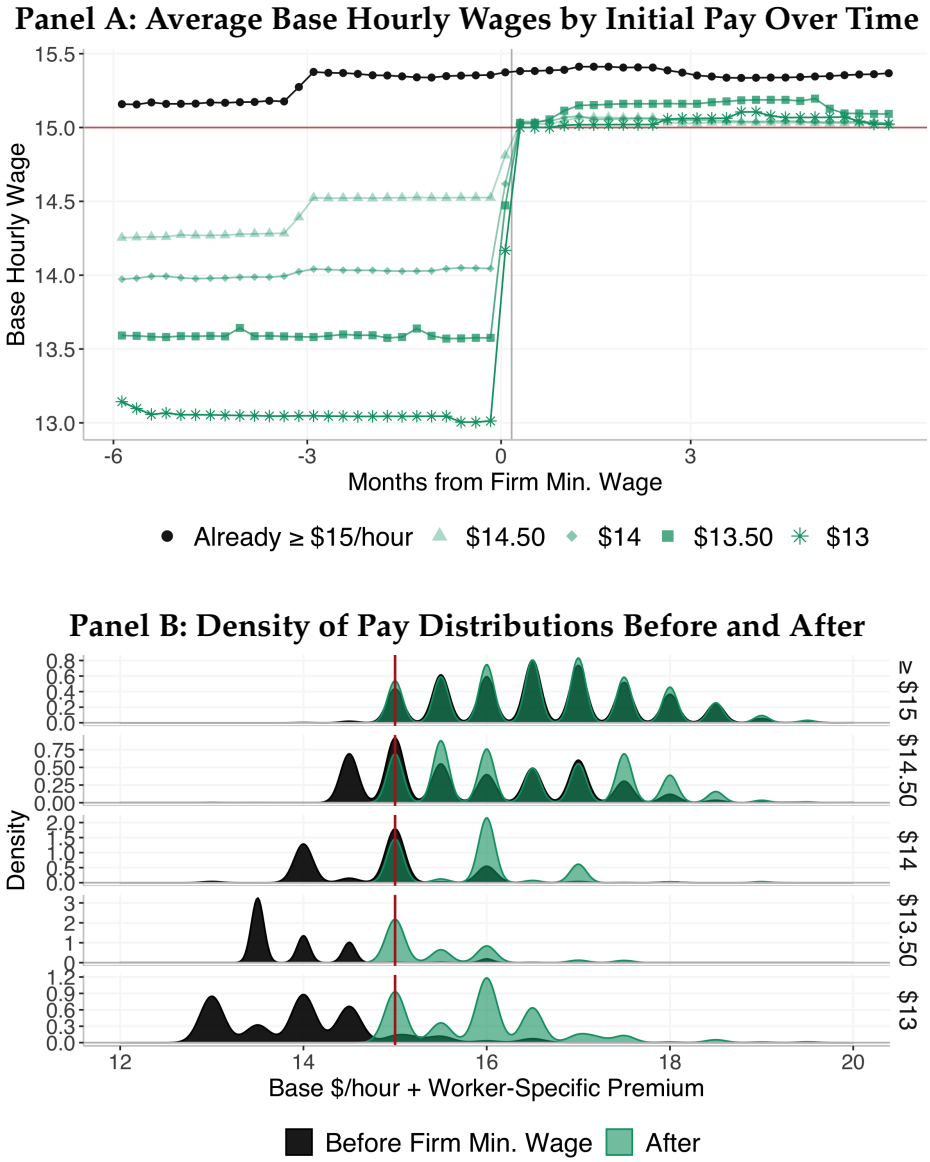
FIGURES AND TABLES

FIGURE 1: PHOTOS OF HUB WAREHOUSES



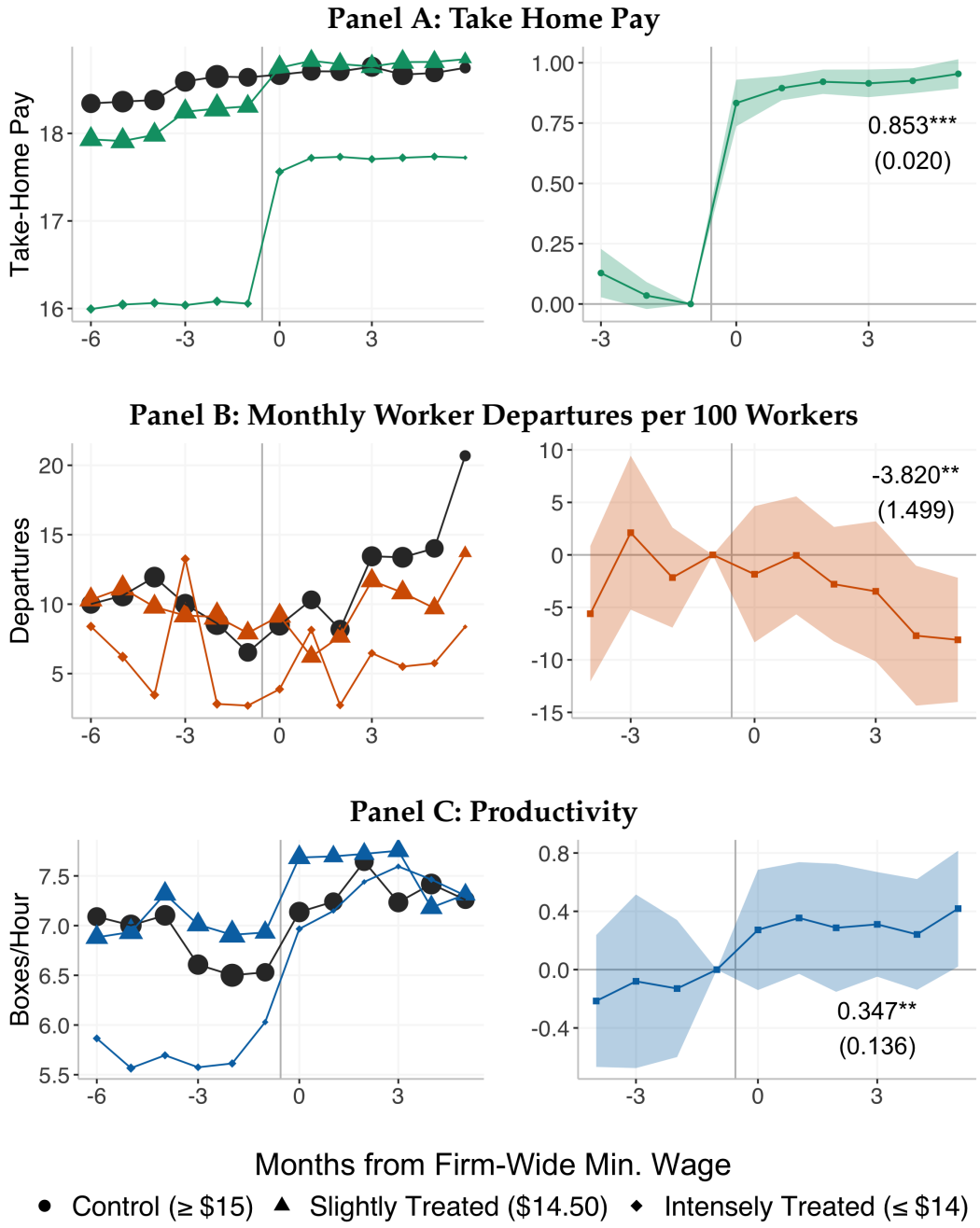
Note: These images show an example of a hub warehouse from the authors' visit. In the left photo, three vehicles converge at the end of an aisle. The vehicle in the foreground is returning to the loading dock after having put away boxes; the vehicles in aisles 39 and 40 are putting away and retrieving boxes. The vehicle in aisle 40 is extended to the highest elevation where boxes are loaded onto racks 40 feet off the ground. Notice that the boxes are not on pallets — in this part of the warehouse, everything must be manually loaded and unloaded. The right photo gives a view down a thoroughfare by a loading dock, showing about half of the length of the warehouse. The exit sign in the top right is a standard-size; coauthor included for scale.

FIGURE 2: WAGES IMPACTS OF A VOLUNTARY FIRM MINIMUM WAGE



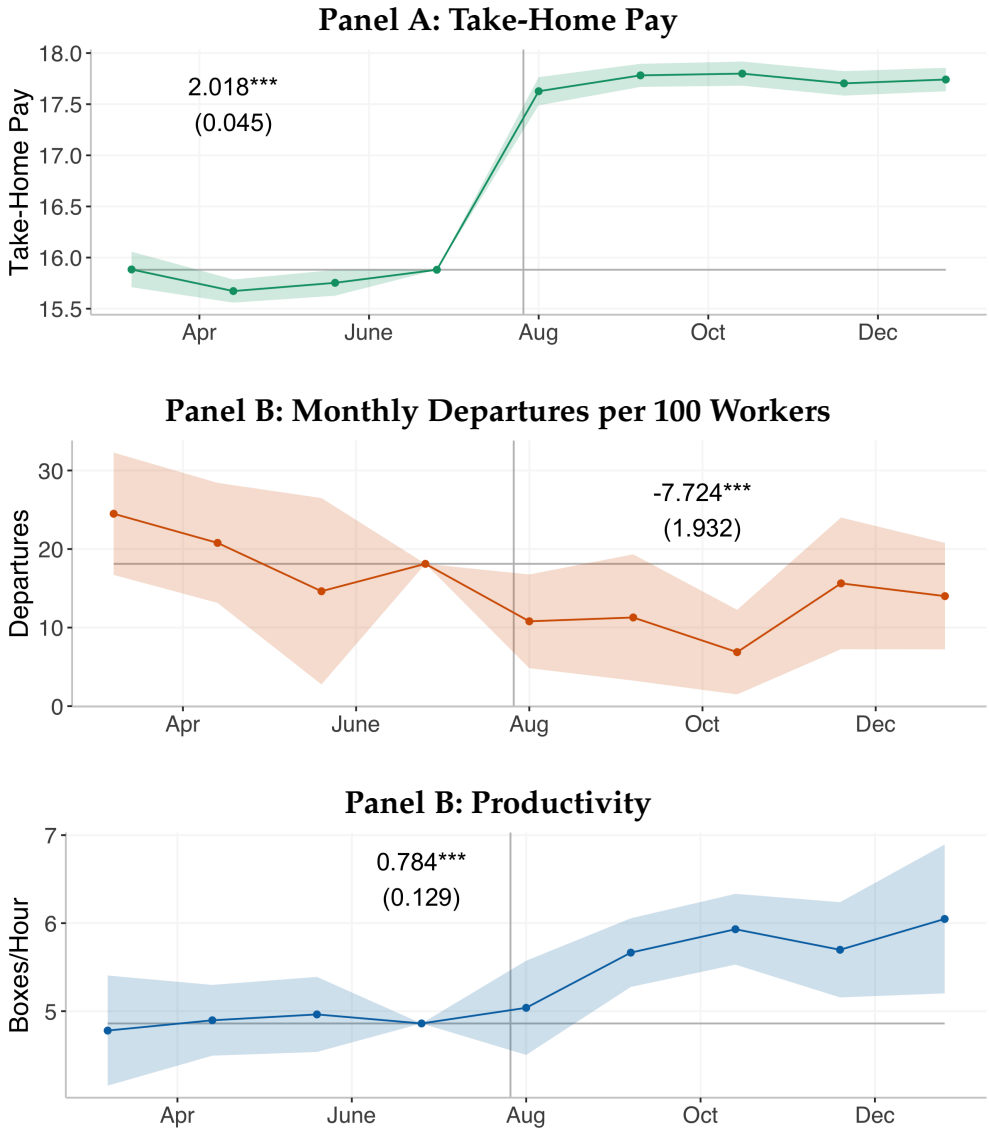
Note: This figure illustrates the differential impacts of the firm's voluntary minimum wage across warehouses. Panel A shows weekly wages around the implementation of the company-wide minimum wage for warehouses with different base wages prior to its implementation. Warehouses with base pay above $15/hour are depicted in black circles and serve as control warehouses. Warehouses with lower base pay are shown in green. The red horizontal line indicates $15/hour. The grey vertical line shows the week when the firm announced and implemented the company-wide minimum wage across all its warehouses. The exact timing of the company-wide minimum wage is obscured to preserve the firm's anonymity. Panel B plots kernel density of wages before the voluntary firm minimum wage (in black) and after the implementation (in green). Each panel shows warehouses with different base pay. The company-wide minimum wage was implemented as level-shift up in pay for warehouses with base wages below $15/hour. For clarity, this figure does not include warehouse-wide short-term incentives like holiday-rush hourly premiums.

FIGURE 3: EFFECTS OF A VOLUNTARY FIRM MINIMUM WAGE ON TAKE-HOME PAY, DEPARTURES, AND PRODUCTIVITY



Note: This figure shows how pay, productivity, and departures evolve around the implementation of a voluntary company-wide minimum wage. The dates have been obfuscated to preserve the firm's anonymity, and 0 is the point at which the voluntary firm minimum wage was announced and implemented in all warehouses. The left panels show raw averages for control warehouses (in black circles), slightly treated warehouses (in triangles), and intensely treated warehouses (in diamonds). The right set of panels show the reduced-form coefficients from estimating a dynamic version of Equation 4 with fixed effects for region-by-week (or region-by-week-by-warehouse-type) and controls for Covid-19 death rates and state minimum wages. The annotated coefficient reflects the pooled, reduced-form estimate. The first stage is $0.85/hour increase in pay. *p<0.1; **p<0.05; ***p<0.01.

FIGURE 4: PRODUCTIVITY AND DEPARTURES RESPONSE TO A PAY JUMP



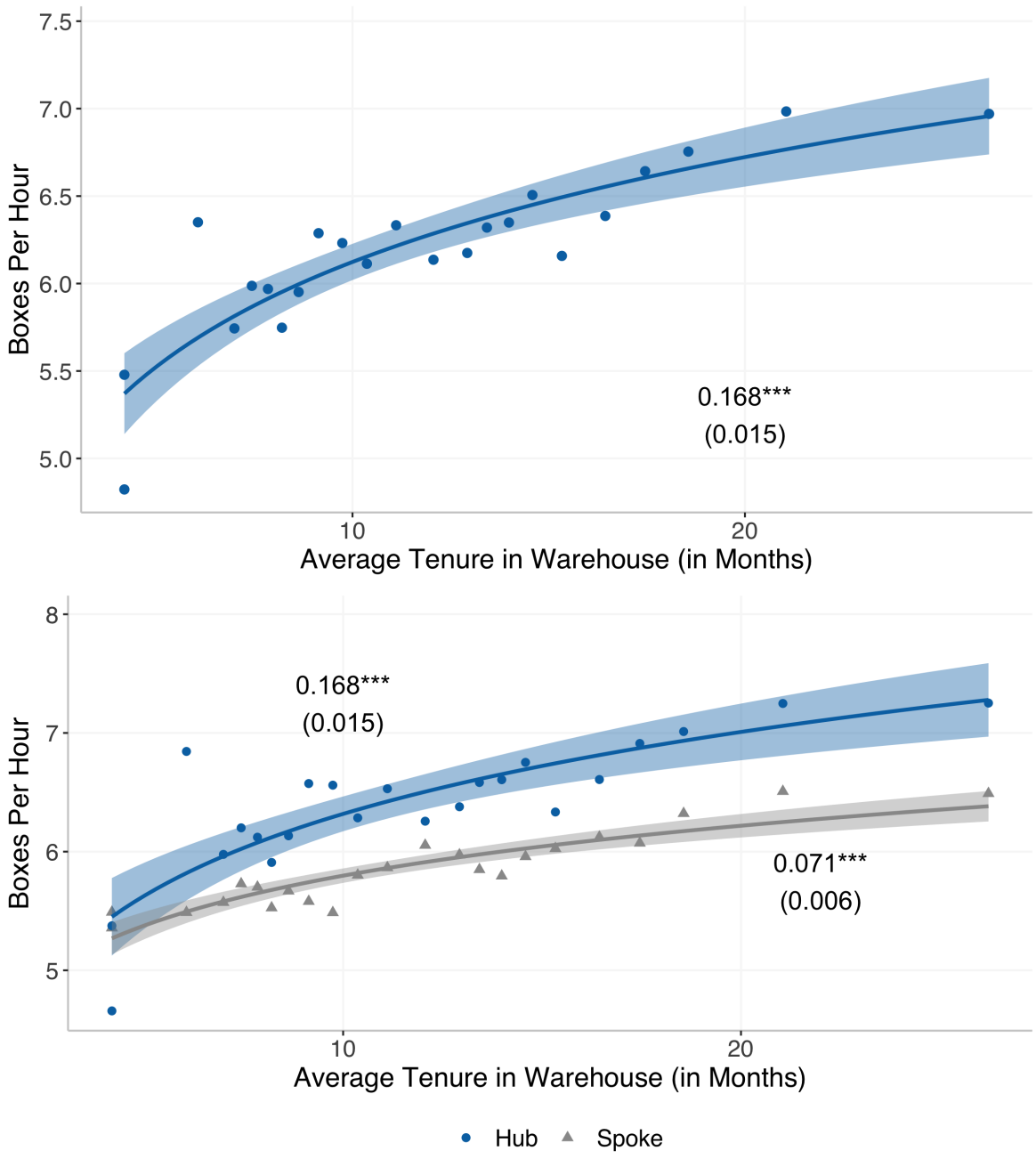
Note: This figure plots reduced form dynamics within the treated warehouse in the six months before and after the August pay bump. Panel A shows the change in average hourly pay among warehouse workers. Panel B reports the departure rate in the warehouse. Panel C displays productivity (total boxes moved/total person-hours). The shaded areas display 95 percent confidence intervals. The annotated coefficients reflect reduced-form estimates, which are not scaled by the size of the pay jump (so differ from the two-stage least-squares estimates in Tables A.10). $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

FIGURE 5: RETURN ON WAGE INVESTMENTS & WAGE MARKDOWNS



Note: This figure shows how a firm might think about the estimated elasticities. Panel A shows how a firm might think about the return on their investment, depending on which frictions they incorporate: none, firm-side labor frictions, or both firm-side labor and productivity frictions as well. The grey horizontal line indicates the point at which the firm has broken even on their investment. Panel B shows wages relative to workers' marginal product, according to Equation 2, again varying which frictions the firm recognizes. The horizontal grey line shows the point at which wages are equal to the worker's marginal product. Whiskers delineate 95 percent confidence intervals.

FIGURE 6: RELATIONSHIP BETWEEN WAREHOUSE PRODUCTIVITY AND EXPERIENCE



Note: This figure plots the quantile relationships between the average tenure in a warehouse and the productivity in the warehouse. The points reflect 25 quantiles of tenure in months.

TABLE 1: STATISTICS SUMMARIZING A FORTUNE 500 FIRM'S WAREHOUSES

	All	Around Voluntary Firm $15/hour Minimum Wage	Around Discrete Pre-Covid Pay Increase
Base $/hour	14.78	15.01	14.75
Premium $/hour	2.30	3.52	2.38
Take-Home $/hour	17.08	18.53	17.13
State Minimum Wage	9.48	10.04	7.25
Monthly Turnover/100 Workers	11.24	9.80	14.74
Monthly Quits/100 Workers	9.25	7.95	11.67
Monthly Fires/100 Workers	1.63	1.30	3.07
Tenure at Retailer (in Months)	12.31	15.57	8.75
Boxes/Hour	5.95	6.39	5.26
% Moving Hours/Total Hours	64.29%	63.54%	66.28%
Boxes/Moving Hours	9.31	10.20	7.95
Covid-19 Deaths/100K	0.19	0.36	0.00
# Workers	14,5XX	5,6XX	6XX
# Warehouses	5X	4X	1
# Warehouse-Weeks	8,6XX	2,5XX	52

Note: This table provides summary statistics about the Fortune 500 firm's warehouses. The first column displays information about all US-based warehouses at the firm from 2018-2021. The second column limits to the six months on either side of the firm's introduction of a $15/hour voluntary firm minimum wage. The third column focuses on the six months on either side of a discrete pay change in a single, large warehouse in 2019. The precise counts of the firm's workers and warehouses are obscured to preserve the firm's anonymity.

TABLE 2: DEPARTURE RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE

Panel A: All Departures

	$/Hr	Monthly Departures per 100 Workers			
Gap from $15/hr x Post	0.853***	−2.391**	−4.568***	−3.820**	
	(0.020)	(1.040)	(1.586)	(1.499)	
$\widehat{\Delta\$/hour}$					−4.479**
					(1.761)
Elasticity		-5.98	-11.43	-9.56	-9.56
		(2.60)	(3.38)	(3.20)	(3.76)
Base Mean	$18.03	8.45	8.45	8.45	8.45
Adjusted R^2	0.988	0.095	0.117	0.121	0.124

Panel B: Quits

	$/Hr	Monthly Quits per 100 Workers			
Gap from $15/hr x Post	0.853***	−2.875***	−5.708***	−4.907***	
	(0.020)	(0.933)	(1.400)	(1.320)	
$\widehat{\Delta\$/hour}$					−5.753***
					(1.558)
Base Mean	$18.03	6.45	6.45	6.45	6.45
Adjusted R^2	0.988	0.121	0.157	0.161	0.164

Panel C: Fires

	$/Hr	Monthly Fires per 100 Workers			
Gap from $15/hr x Post	0.853***	−0.015	1.169**	1.054*	
	(0.020)	(0.363)	(0.524)	(0.546)	
$\widehat{\Delta\$/hour}$					1.236*
					(0.641)
Base Mean	$18.03	1.35	1.35	1.35	1.35
Adjusted R^2	0.988	-0.008	0.000	0.00	-0.001
Regional FEs	✓		✓	✓	✓
Controls	✓			✓	✓
# Warehouses	4X	4X	4X	4X	4X
# Workers	4,XXX	4,XXX	4,XXX	4,XXX	4,XXX

Note: This table presents the estimated impact of the voluntary company-wide minimum wage on worker departures from the firm. Column 1 shows the first stage, which differs from one because some individuals' premium pay changed (e.g., due to tenure and machinery certification). Columns 2-4 estimate the reduced form relationship between departures and the exposure to the company-wide minimum wage using Equation 4, progressively adding fixed effects and controls. Column 5 scales the reduced-form by the first-stage in a two-stage-least-squares regression. The specifications use a three-month pre-period and a six-month post-period to exclude the wage changes that occurred four months before the company-wide minimum wage. *p<0.1; **p<0.05; ***p<0.01.

TABLE 3: PRODUCTIVITY RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE

Panel A: Overall Productivity Response in All Warehouses

	$/Hr	Boxes/Hr			
Gap from $15/hr x Post	0.864***	0.247**	0.346**	0.347**	
	(0.020)	(0.108)	(0.135)	(0.136)	
$\widehat{\Delta\$/hour}$					0.401**
					(0.157)
Elasticity		0.76	1.06	1.06	1.06
		(0.33)	(0.36)	(0.36)	(0.42)
Regional FEs	✓		✓	✓	✓
Controls	✓			✓	✓
Bandwidth	3,6mo	3,6mo	3,6mo	3,6mo	3,6mo
Base Mean	$18.03	6.81	6.81	6.81	6.81
# Warehouses	4X	4X	4X	4X	4X
# Workers	4,XXX	4,XXX	4,XXX	4,XXX	4,XXX
Adjusted R^2	0.990	0.825	0.859	0.858	0.876

Panel B: Dimensions of Productivity Response in Hubs

	$/Hr	Boxes per Hour	Moving:Total Hours Ratio	Boxes per Moving Hour
Gap from $15/hour x Post	0.939***			
	(0.031)			
$\widehat{\Delta\$/hour}$		0.697***	0.035***	0.419
		(0.211)	(0.011)	(0.346)
Contribution			58.3%	41.7%
FEs & Controls	✓	✓	✓	✓
Bandwidth	3,6mo	3,6mo	3,6mo	3,6mo
Base Mean	$18.04	7.1	0.64	10.7
# Warehouses	1X	1X	1X	1X
# Workers	1,XXX	1,XXX	1,XXX	1,XXX
Adjusted R^2	0.994	0.882	0.735	0.847

Note: This table presents the estimated impact of the voluntary company-wide minimum wage on productivity in Panel A. Column 1 shows the first stage, which differs from one because some individuals' premium pay changed (e.g., due to tenure and machinery certification). Columns 2-4 estimate the reduced-form relationship between boxes moved per hour and the warehouse's exposure to the company-wide minimum wage using Equation 4, progressively adding fixed effects and controls. Column 5 scales the reduced-form by the first-stage in a two-stage-least-squares regression. We use a three-month pre-period and a six-month post-period to exclude the wage changes that occurred four months before the company-wide minimum wage. Panel B decomposes the impact of higher pay on productivity into component parts for large hub warehouses. *p<0.1; **p<0.05; ***p<0.01.

TABLE 4: TURNOVER EFFECTS WITHIN AND ACROSS TEMPORARY WAREHOUSE WORKERS

	Job Completion		Quits		Excellent Eval.	
	Across	Within	Across	Within	Across	Within
\$1/hr	2.601***	2.164***	−2.758***	−2.321***	1.192***	0.5971***
	(0.216)	(0.219)	(0.217)	(0.209)	(0.139)	(0.119)
% of Full Effect		83.2%		84.2%		50.1%
Duration	✓	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓	✓
Mean \$/hr	11.19	11.19	11.19	11.19	11.19	11.19
Dependent Mean	40.59	40.59	33.56	33.56	11.07	11.07
Workers	93175	93175	93175	93175	93175	93175

Note: This table presents reduced-form relationships between pay and job outcomes among warehouse workers in temporary assignments. The sample includes all placements in warehouse jobs through the staffing agency for workers who have worked more than one job through the staffing agency. Specifications labeled "Across" measure the relationship between higher pay and job completion (finishing the temporary assignment), quits, and receiving an excellent evaluation from the manager. These specifications include occupation by commuting zone fixed effects and industry by commuting zone by time fixed effects as well as controls for the expected duration of the assignment as a quartic. Specifications labeled "Within" also include individual fixed effects. Expected duration is calculated based on how long similar jobs at that client had lasted in the past. Regressions are weighted by the duration of the job. Standard errors are clustered at the worksite-firm level. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

APPENDIX A: FURTHER MODEL DISCUSSION

We begin by showing the derivation of wages for our model from Section 1. We then present a model in which firms set wages of two periods.

1.A DERIVING THE WAGE-SETTING EQUATION

Firms set wages to maximize profits:

$$\max_w \Pi = Y(N(w)e(w)) - wN(w)$$

We take the first order condition:

$$\begin{aligned} Y'(Ne)[N'(w)e(w) + N(w)e'(w)] - N(w) - wN'(w) &= 0 \\ \frac{Y'(Ne)[N'(w)e(w) + N(w)e'(w)] - N(w)}{N'(w)} &= \frac{wN'(w)}{N'(w)} \\ Y'(Ne)e(w) + Y'(Ne)\frac{N(w)}{N'(w)}e'(w) - \frac{N(w)}{N'(w)} &= w \end{aligned}$$

We use $\epsilon_{N,w} = \frac{N'(w)w}{N(w)}$ to substitute in:

$$\begin{aligned} Y'(Ne)e(w) + \frac{Y'(Ne)e'(w)w}{\epsilon_{N,w}} - \frac{w}{\epsilon_{N,w}} &= w \\ \frac{Y'(Ne)\left[e(w) + e'(w)\frac{w}{\epsilon_{N,w}}\right]}{Y'(Ne)e(w)} &= \frac{w\left(1 + \frac{1}{\epsilon_{N,w}}\right)}{Y'(Ne)e(w)} \\ 1 + \underbrace{\frac{e'(w)w}{e(w)}}_{\epsilon_{e,w}}\frac{1}{\epsilon_{N,w}} &= \frac{w\left(1 + \frac{1}{\epsilon_{N,w}}\right)}{Y'(Ne)e(w)} \\ Y'(Ne)e(w)\left(1 + \frac{\epsilon_{e,w}}{\epsilon_{N,w}}\right) &= w\left(1 + \frac{1}{\epsilon_{N,w}}\right) \\ \frac{Y'(Ne)e(w)\left(\epsilon_{N,w} + \epsilon_{e,w}\right)}{1 + \epsilon_{N,w}} &= w. \end{aligned}$$

1.B A Two-Period Model

We extend the baseline model by allowing the firm to set different wages for junior workers, j, and senior workers, s.[40] In the model, a junior worker will become a senior worker if she persists at the firm because the senior wage and thus her remaining compensation, $W_s(w_s) = w_s$, exceeds her outside option, which is drawn from a distribution $\tilde{w} \sim R(\cdot)$. New recruits choose whether or not to start at the firm based on the expected net present value of the job, which depends on both the junior and senior wage. Letting r denote the interest rate and $R(w)$ indicate the retention rate of workers persisting from junior to senior workers, the net present value of the initial contract is given by:

$$W_j(w_j, w_s) = w_j + \frac{1}{1+r}\left[R(w_s)w_s + \int_{w_s}^{\infty} \tilde{w}R(\tilde{w})d\tilde{w}\right].$$

Junior and senior wages consequently influence the quantity of junior and senior workers (n_j and n_s), as well as the efficiency units of labor each worker provides(e_j and e_s):

$$\begin{aligned} n_{j,t} &\equiv n_j(w_{j,t}, w_{s,t+1}) \text{ and } n_{s,t} \equiv R(w_{s,t})n_{j,t-1}(w_{j,t-1}, w_{s,t}) \\ e_{j,t} &\equiv e(W_j(w_{j,t}, w_{s,t+1})) \text{ and } e_{s,t} \equiv e(W_s(w_{s,t})). \end{aligned}$$

Workers' efficiency units of labor may vary because of unobserved effort or latent differences in worker selection. The firm's output depends on the efficiency units of junior and senior labor according to $Y(e_j n_j, e_s n_s)$, where Y denotes dollar-denominated output. The firm's profits are given by output net of labor costs:

$$\begin{aligned} \Pi &= \sum_{t=1}^{\infty} \frac{\pi_t}{(1+r)^{t-1}} \\ &= \sum_{t=1}^{\infty} \frac{1}{(1+r)^{t-1}}\Big[Y\left(e(W_j(w_{j,t}, w_{s,t+1}))n_j(w_{j,t}, w_{s,t+1}), e(W_s(w_{s,t}))R(w_{s,t})n_j(w_{j,t-1}, w_{s,t})\right) \\ &\qquad - w_{j,t}n_j(w_{j,t-1}, w_{s,t}) - w_{s,t}R(w_{s,t})n_j(w_{j,t-1}, w_{s,t}))\Big]. \end{aligned}$$

[40] This extension has elements that are similar to the model in Ioannides and Pissarides (1985), which includes wage setting over two periods. However, it also has elements more similar to efficiency wage models like Shapiro and Stiglitz (1984); Yellen (1984), that allow for the possibility that productivity depends on the wage.

Expanding terms for any given period clarifies the implications of the firm's choice of the junior wage w_j and senior wage $w_{s,+}$ in the next period:

$$\pi + \frac{\pi_+}{1+r} = Y\left(e(W_j(w_j, w_{s,+}))n_j(w_j, w_{s,+}), R(w_{s,-})e(W_s(w_s))n_{j,-}\right) - w_j n_j(w_j, w_{s,+}) - w_s R(w_s) n_{j,-}$$
$$+ \frac{1}{1+r}\left[Y(n_{j,+}e_{j,+}, e(W_s(w_{s,+}))R\left(w_{s,+}\right)n_j(w_j, w_{s,+})\right) - w_{j,+}n_{j,+} - w_{s,+}R(w_{s,+})n_j(w_j, w_{s,+})\right].$$

If we take the first order conditions and suppress time subscripts, we arrive at:

$$\frac{\partial \Pi}{\partial w_j} = 0 : Y_j n_j' e_j + Y_j n_j e_j' - n_j - w_j n_j' + \frac{1}{1+r} R(w_{s,+}) n_j' \left[Y_s e_s - w_s\right] = 0 \tag{10}$$

$$\frac{\partial \Pi}{\partial w_s} = 0 : \left[Y_j(n_j' e_j + n_j e_j') - w_j n_j'\right] \frac{\partial W_s}{\partial w_s}$$
$$+ \frac{1}{1+r} Y_s \left[R(w_s) e_s n_j + R(w_s) e_s' n_j + R(w_s) e_s n_j' \frac{\partial W_s}{\partial w_s}\right]$$
$$- \frac{1}{1+r}\left[R(w_s) n_j + w_s R(w_s) n_j + w_s R(w_s) n_j' \frac{\partial W_s}{\partial w_s}\right] = 0 \tag{11}$$

We can use $\frac{\partial W_s}{\partial w_s} = \frac{1}{1+r} R(w_s)$ and rearrange terms in Equation 11:

$$\frac{\partial \Pi}{\partial w_s} = 0 : \underbrace{\frac{1}{1+r} R(w_s)\left[Y_j(n_j' e_j + n_j e_j') - w_j n_j' - n_j + \frac{1}{1+r} R(w_s) n_j' [Y_s e_s - w_s]\right]}_{\partial \Pi / \partial w_j = 0}$$
$$+ \frac{1}{1+r} Y_s \left[R(w_s) e_s n_j + R(w_s) e_s' n_j\right] - \frac{1}{1+r}\left[w_s R(w_s) n_j\right] = 0$$

Simplifying yields an expression for the senior wage:

$$w_s = Y_s e_s \left[1 + \frac{\epsilon_{e,s}}{\epsilon_{\text{Departures}}} \frac{R(w_s)}{1 - R(w_s)}\right]. \tag{12}$$

The firm will optimally set a premium on the senior wage over the worker's marginal product if efficiency units of labor are increasing in pay ($\epsilon_{e,s} > 0$) and departures are not perfectly elastic ($\epsilon_{\text{Departures}} < \infty$). If departures are not perfectly elastic, the firm will still not exercise the resulting monopsony power over senior workers because higher senior wages yield a double dividend for the quantity and efficiency of labor. In terms of quantity, a higher senior wage not only increases retention but also increases initial recruitment since new recruits anticipate higher subsequent pay. Similarly, a higher senior wage both increases efficiency units of labor among senior workers and also among junior workers since junior workers incorporate senior wages into their decision-making: higher senior wages can elicit greater effort and better selection among the junior workforce.

To express junior wages, we can substitute the senior wage from Equation 12 into the expression for the junior wage from Equation 10:

$$w_j = \left(1 - \frac{1-\epsilon_{e,j}}{\epsilon_{\text{Recruit}}+1}\right) Y_j e_j - \frac{1}{1+r} R(w_s) \frac{\epsilon_{\text{Recruit}}}{1+\epsilon_{\text{Recruit}}} \left[Y_s e_s \frac{\epsilon_{e,s}}{\epsilon_{\text{Departures}}} \frac{R(w_s)}{1-R(w_s)} \right]. \tag{13}$$

The first term is the same as in our baseline model: if the elasticity of new recruits to the firm is not infinitely elastic ($\epsilon_{\text{Recruit}} < \infty$), the firm will have wage-setting power over the junior wage. The firm will exercise this monopsony power over the wage if the efficiency units of junior labor responds less than one-for-one to an increase in the wage ($\epsilon_{e,j} < 1$). The second term is a new term vis-a-vis the baseline model: this term reflects the fact that the firm can further markdown the junior wage if it is paying a senior wage in excess of the senior marginal product.

Summing across junior and senior wages allows us to compare the net present value of workers' earnings to the net present value of their output. We express this in terms of a markdown on workers' marginal product:

$$\begin{aligned}
\frac{W-MP}{MP} &= \frac{Y_j e_j - w_j + \frac{1}{1+r}[Y_s e_s - w_s]}{Y_j e_j + \frac{1}{1+r}[Y_s e_s]} \\
&= -\frac{1-\epsilon_{e,j}}{\epsilon_{\text{Recruit}}+1} \left[\frac{Y_j e_j}{Y_j e_j + \frac{1}{1+r}[Y_s e_s]} \right] \\
&\quad + \frac{\epsilon_{e,s}}{\epsilon_{\text{Departures}}} \frac{R(w_s)}{1-R(w_s)} \left[\frac{\frac{1}{1+r} Y_s e_s}{Y_j e_j + \frac{1}{1+r}[Y_s e_s]} \right] \left[1 - R(w_s) \frac{\epsilon_{\text{Recruit}}}{\epsilon_{\text{Recruit}}+1} \right].
\end{aligned}$$

When taking this to the data, we make a couple of simplifying assumptions. First, we assume $\epsilon_{e,j} \approx \epsilon_{e,s}$, which is what we can observe in our existing data. Second, we assume $r \approx 0$. Third, we make the standard assumption that $\epsilon_{\text{Recruit}} = \epsilon_{\text{Departures}}$ since workers who leave one firm will be hired by another firm if all workers remain employed. We further define γ_s as the share of senior output in total output.

$$\frac{W-MP}{MP} = -\frac{1-\epsilon_e}{\epsilon_{\text{Departures}}+1}(1-\gamma_s) + \frac{\epsilon_e}{\epsilon_{\text{Departures}}} \frac{R(w_s)}{1-R(w_s)} \gamma_s \left[1 - R(w_s) \frac{\epsilon_{\text{Departures}}}{\epsilon_{\text{Departures}}+1} \right].$$

This expression captures several useful predictions, which are very similar to the model in Section 1 if labor supply ($\epsilon_{\text{Departures}}$) is infinite, there is no markdown. Workers receive a greater share of their output when their productivity is more responsive to pay (higher ϵ_e). The novel contribution of this model is that workers receive a greater share of output when there are greater returns to tenure (higher γ_s).

Estimating this markdown from our data suggests that across the two periods, the markdown is moderately small and close to the marginal product: $\frac{W-MP}{MP} = -0.01$. This is consistent with our findings from Section 5, that incorporating all of the frictions brings the pay much closer to workers' marginal product than only incorporating worker-side frictions.

APPENDIX B: SUPPLEMENTAL TABLES AND FIGURES

TABLE A.1: CHANGING BANDWIDTHS IN ESTIMATING REDUCED FORM DEPARTURES RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE

	Monthly Turnover per 100 Workers			
Gap from \$15/hour x Post	-2.391^{**}	-1.204	-3.820^{**}	-2.329^{*}
	(1.040)	(1.613)	(1.499)	(1.328)
FEs & Controls	✓	✓	✓	✓
Bandwidth	1mo	3mo	3,6mo	6mo
Base Mean	7.34	8.45	8.45	9.22
# Warehouses	4X	4X	4X	4X
# Workers	3,XXX	4,XXX	4,XXX	5,XXX
Adjusted R^2	0.095	0.079	0.121	0.123

Note: We estimate the reduced-form impact of the voluntary firm minimum wage on departures according to Equation 4, changing the bandwidth around implementation. The baseline mean reflects the number of boxes moved per hour in the pre-period. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

TABLE A.2: CHANGING BANDWIDTHS IN ESTIMATING REDUCED FORM PRODUCTIVITY RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE

	Boxes/Hour			
Gap from \$15/hour x Post	0.440	−0.160	0.440***	0.176***
	(0.371)	(0.229)	(0.147)	(0.052)
Gap from \$15/hour x Placebo Post	0.284	0.417***	0.186	0.297***
	(0.199)	(0.145)	(0.131)	(0.099)
Bandwidth	1mo	3mo	3,6mo	6mo
Base Mean	6.81	6.81	6.81	6.81
# Warehouses	4X	4X	4X	4X
# Workers	3,XXX	4,XXX	4,XXX	5,XXX
Adjusted R^2	0.907	0.864	0.859	0.846

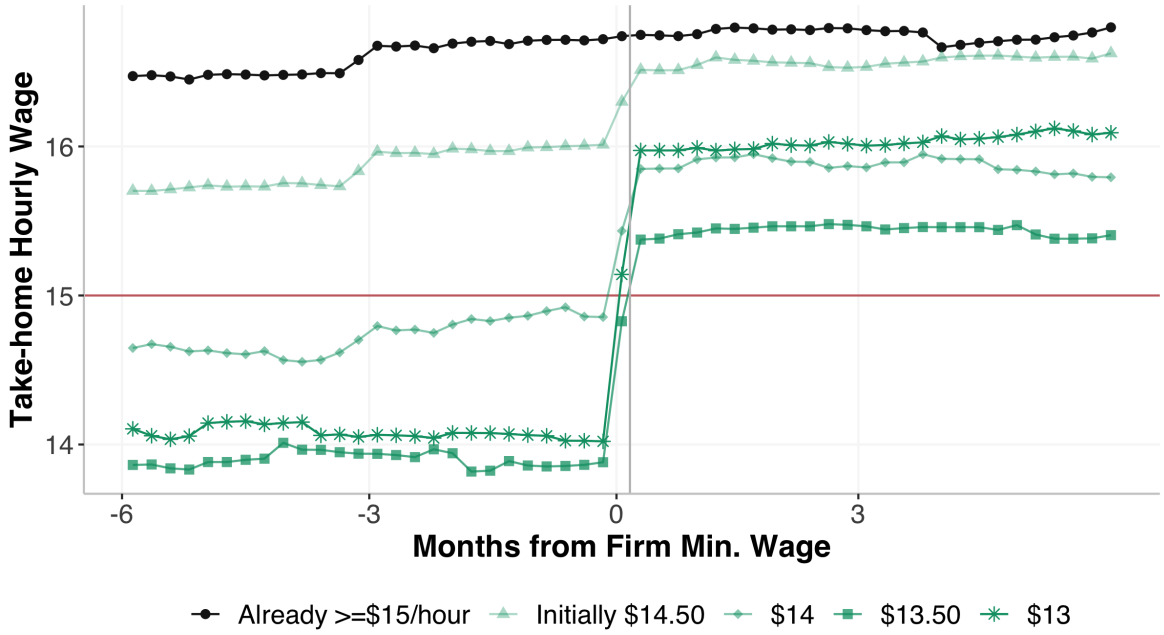
Note: We estimate the reduced-form impact of the voluntary firm minimum wage on boxes moved per hour according to Equation 4, changing the bandwidth around implementation. The baseline mean reflects the number of boxes moved per hour in the pre-period. *p<0.1; **p<0.05; ***p<0.01.

TABLE A.3: ACCIDENT AND INJURY RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE

	$/Hr	Injuries/FTE Workers			
Gap from $15/hr x Post	0.588***	−0.010	−0.016	−0.017	
	(0.107)	(0.010)	(0.021)	(0.022)	
$\widehat{\Delta\$/hour}$					−0.028
					(0.038)
Elasticity		-0.02	-0.03	-0.03	-10.16
		(5.91)	(7.52)	(7.76)	(13.45)
Region FE	✓		✓	✓	✓
Controls	✓			✓	✓
Base Mean	$17.89	0.05	0.05	0.05	0.05
# Warehouses	4X	4X	4X	4X	4X
Avg. # FTE Workers	6,XXX	6,XXX	6,XXX	6,XXX	6,XXX
Adjusted R^2	0.975	0	0.073	0.068	0.032

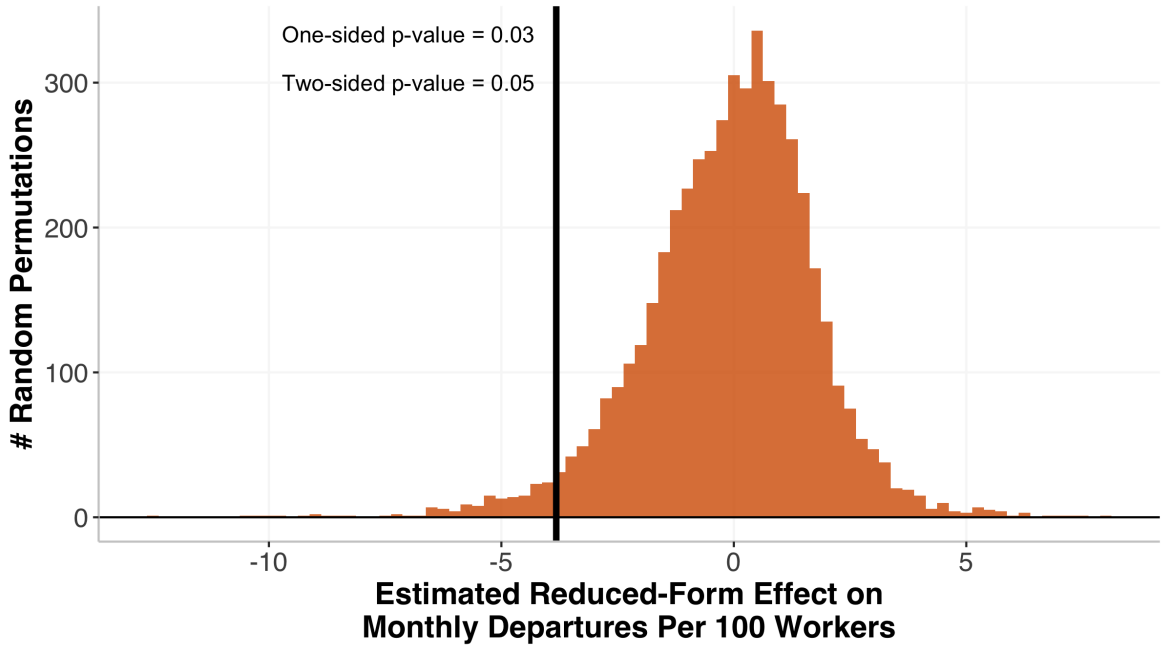
Note: We estimate the impact of the voluntary firm minimum wage on accidents and injuries that ultimately get reported to the Occupational Safety and Health Administration. We use the reported full-time equivalent (FTE) workers based on hours spent by *anyone* in the warehouse, which includes overtime hours by associates as well as hours by managers and temporary staff. Since injury information is collected at coarser time intervals than our human resources data, we use a six-month bandwidth on either side of the minimum wage when estimating wages and omit week-level fixed effects.

FIGURE A.1: TAKE-HOME WAGES AROUND THE IMPLEMENTATION OF A VOLUNTARY FIRM MINIMUM WAGE



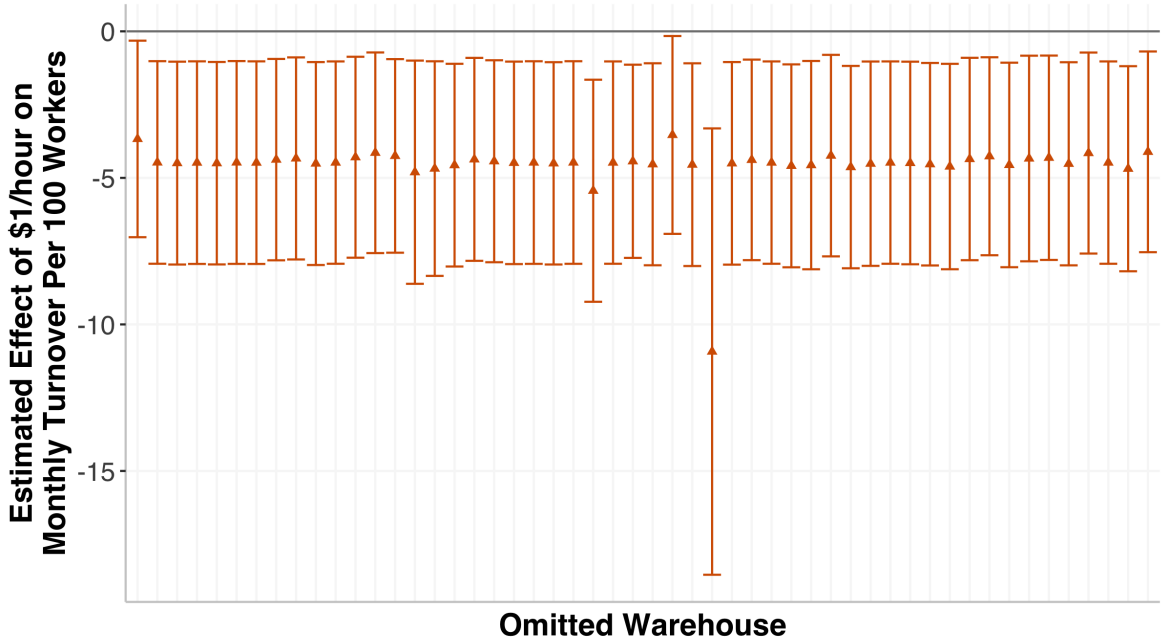
Note: Weekly take-home wages around the implementation of the voluntary firm minimum wage show the differential bite of the voluntary firm minimum wage. We group together warehouses according to their *base* pay rate prior to the implementation of the voluntary firm minimum wage. This figure displays the *take-home* hourly pay of workers, showing that even in warehouses where base pay was $14.50/hour before the voluntary firm minimum wage went into place, take-home pay was about $1.50 higher than that. To preserve the firm's anonymity, we do not reveal the exact timing of the voluntary firm minimum wage; however, it was implemented simultaneously throughout the whole firm. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

FIGURE A.2: WAREHOUSE PERMUTATIONS OF REDUCED FORM ESTIMATES OF THE DEPARTURES RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE



Note: This figure shows the result of 5,000 permutations. Each permutation randomly re-assign warehouses' exposure to the firm's voluntary firm minimum wage, by drawing a random set of wage treatments from the true set with replacement and applying this set to the warehouses. For each permutation, we calculate the change in departures at these warehouses. This exercise produces a distribution of placebo estimates. The solid black line shows the true estimated effect. The annotation reflects the one- and two-sided p-values from assessing where this estimate falls in the distribution.

FIGURE A.3: WAREHOUSE EXCLUSIONS IN ESTIMATING OF REDUCED FORM DEPARTURES RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE



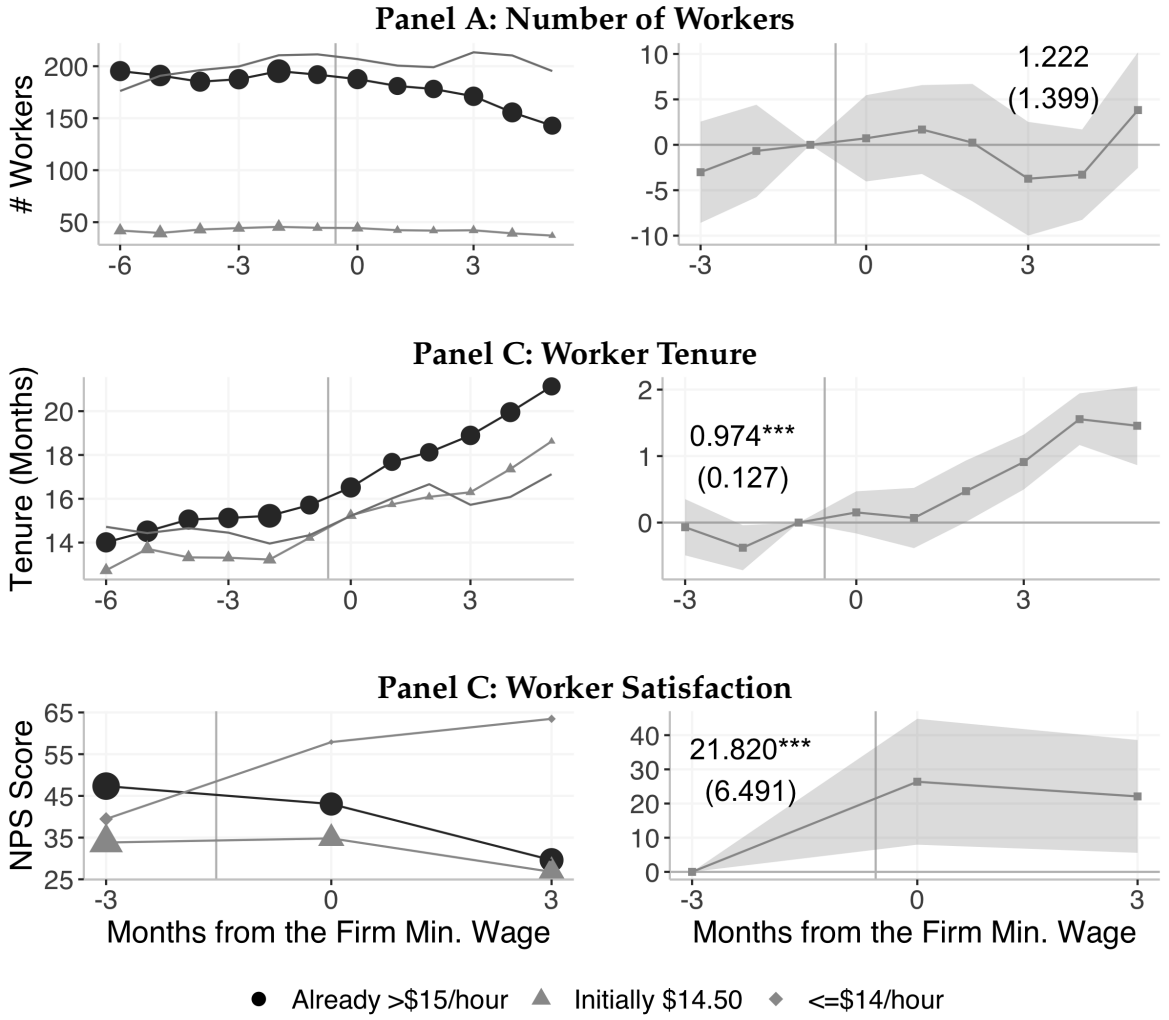
Note: We show the IV estimates of the departures response to the voluntary firm minimum wage, omitting each warehouse in turn. Each point represents an estimate, leaving out one warehouse. Whiskers show 95 percent confidence intervals. A small number of warehouses have been duplicated to preserve the firm's anonymity.

TABLE A.4: PLACEBO TEST OF REDUCED FORM ESTIMATES OF THE DEPARTURES RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE

	Monthly Departures Per 100 Worker		
	Placebo I	Placebo II	Pooled
Gap from $15/hour x Placebo Post	2.521	2.726	2.646*
	(2.807)	(1.749)	(1.496)
Bandwidth	3,6mo	3,6mo	3,6mo
Base Mean	11.37	13.16	11.7
# Warehouses	4X	4X	4X
# Workers	3,XXX	5,XXX	7,XXX
Adjusted R^2	0.084	0.230	0.178

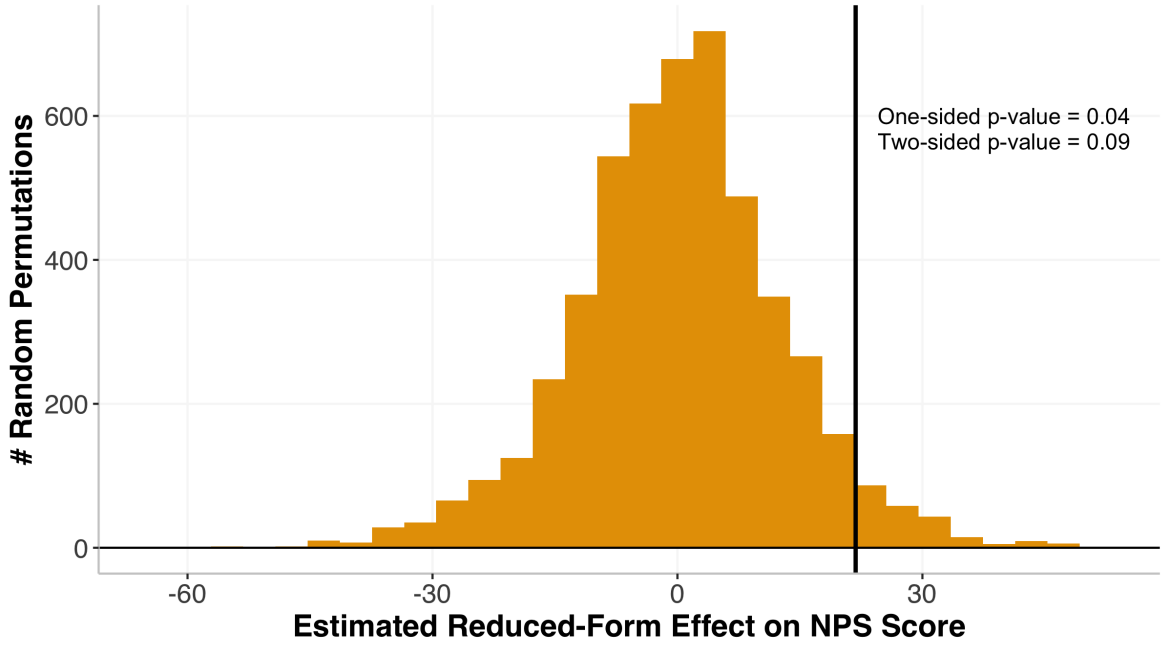
Note: This table shows the results of running Equation 4 on other calendar years' data where the same calendar week in other years is considered the time of implementation. We use a 3-month bandwidth so that we have greater space between the years' comparisons. *p<0.1; **p<0.05; ***p<0.01.

FIGURE A.4: WORKERS, TENURE, AND EMPLOYEE SATISFACTION AROUND A VOLUNTARY FIRM MINIMUM WAGE INTRODUCTION



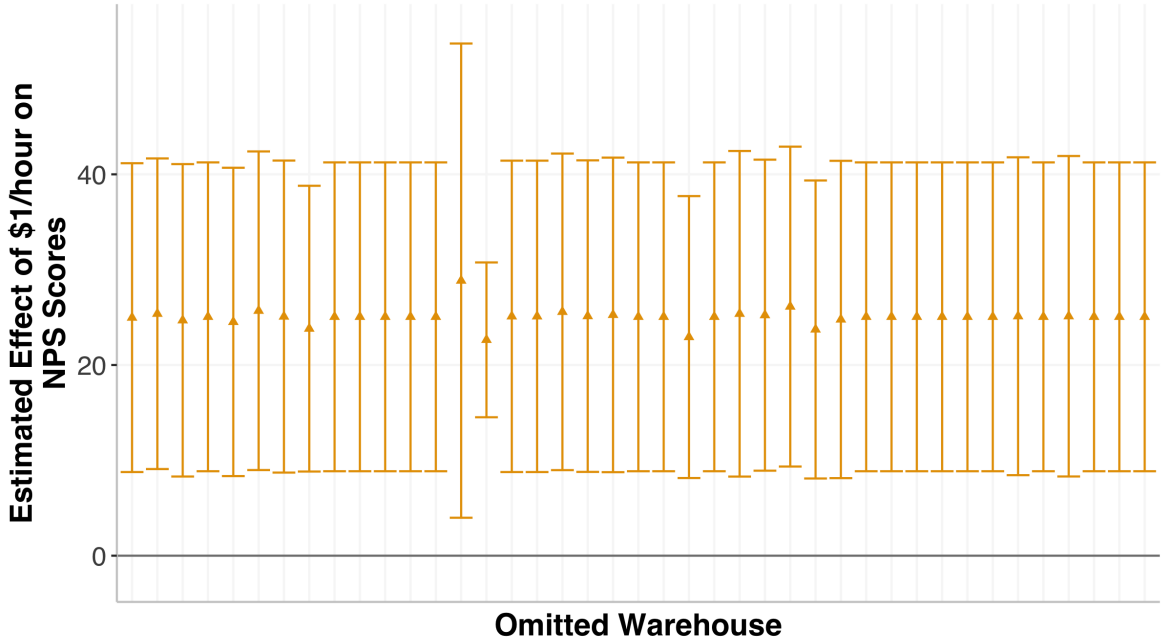
Note: This figure shows how the number of workers, tenure of workers, and employee satisfaction evolve around the implementation of a voluntary firm minimum wage. Dates have been obfuscated to preserve the firm's anonymity; 0 is the point at which the voluntary firm minimum wage was announced and implemented. The left set of panels show raw numbers while the right panels show the reduced form relationship that arises from estimating Equation 4. The coefficient and standard error are noted on the graphs. The black circles show control warehouses. The grey triangles and diamonds show the trends for warehouses that had base pay at $14.50/hour and less than or equal to $14/hour, respectively, and were thus affected by the voluntary firm minimum wage. NPS scores are collected quarterly and are calculated as the percent of people who would strongly recommend the employer to a friend less the percent of people who would certainly not recommend the employer to a friend. *p<0.1; **p<0.05; ***p<0.01.

FIGURE A.5: WAREHOUSE PERMUTATIONS OF REDUCED FORM ESTIMATES OF THE WORKER SATISFACTION RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE



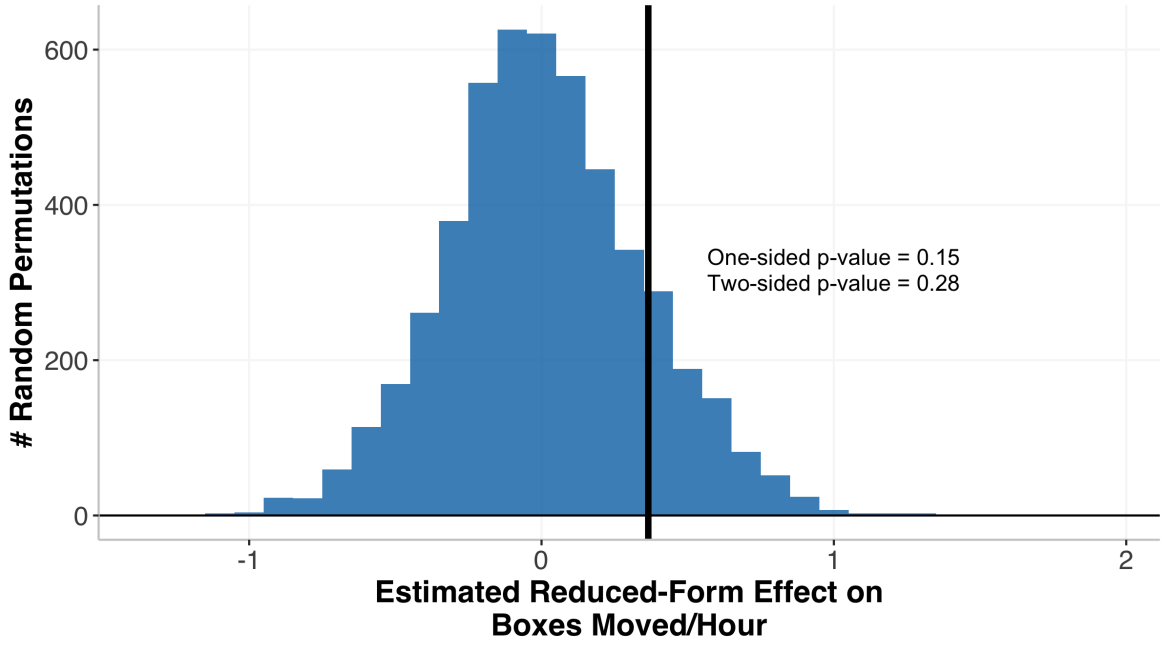
Note: This figure shows the result of 5,000 permutations. Each permutation randomly re-assign warehouses' exposure to the firm's voluntary firm minimum wage, by drawing a random set of wage treatments from the true set with replacement and applying this set to the warehouses. For each permutation, we calculate the reduced-form change in worker satisfaction as measured by NPS scores at these warehouses. This exercise produces a distribution of placebo estimates. The solid black line shows the true estimated effect. The annotation reflects the one- and two-sided p-values from assessing where this estimate falls in the distribution.

FIGURE A.6: WAREHOUSE EXCLUSIONS IN ESTIMATING OF REDUCED FORM WORKER SATISFACTION RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE



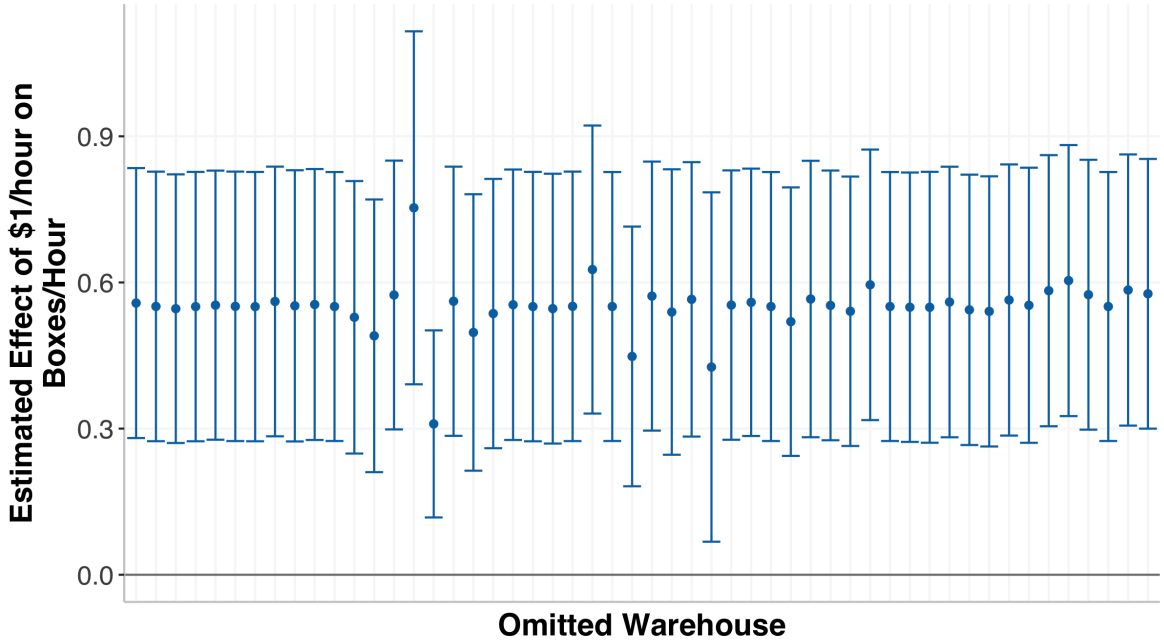
Note: We show the IV estimates of the worker satisfaction response to the voluntary firm minimum wage, omitting each warehouse in turn. Each point represents an estimate, leaving out one warehouse. Whiskers show 95 percent confidence intervals. A small number of warehouses have been duplicated to preserve the firm's anonymity.

FIGURE A.7: WAREHOUSE PERMUTATIONS OF REDUCED FORM ESTIMATES OF THE PRODUCTIVITY RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE



Note: This figure shows the result of 5,000 permutations. Each permutation randomly re-assign warehouses' exposure to the firm's voluntary firm minimum wage, by drawing a random set of wage treatments from the true set with replacement and applying this set to the warehouses. For each permutation, we calculate the change in productivity at these warehouses. This exercise produces a distribution of placebo estimates. The solid black line shows the true estimated effect. The annotation reflects the one- and two-sided p-values from assessing where this estimate falls in the distribution. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

FIGURE A.8: WAREHOUSE EXCLUSIONS IN ESTIMATING OF REDUCED FORM PRODUCTIVITY RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE



Note: We show the IV estimates of the productivity response to the voluntary firm minimum wage, omitting each warehouse in turn. Each point represents an estimate, leaving out one warehouse. Whiskers show 95 percent confidence intervals. A small number of warehouses have been duplicated to preserve the firm's anonymity.

TABLE A.5: PLACEBO TEST OF REDUCED FORM ESTIMATES OF THE PRODUCTIVITY RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE

	Boxes/Hour		
	Placebo I	Placebo II	Pooled
Gap from $15/hour x Placebo Post	0.075	0.165	0.131
	(0.193)	(0.135)	(0.110)
Bandwidth	3mo	3mo	3mo
Base Mean	6.27	5.27	5.61
# Warehouses	2X	3X	4X
# Workers	2,XXX	4,XXX	5,XXX
Adjusted R^2	0.867	0.843	0.853

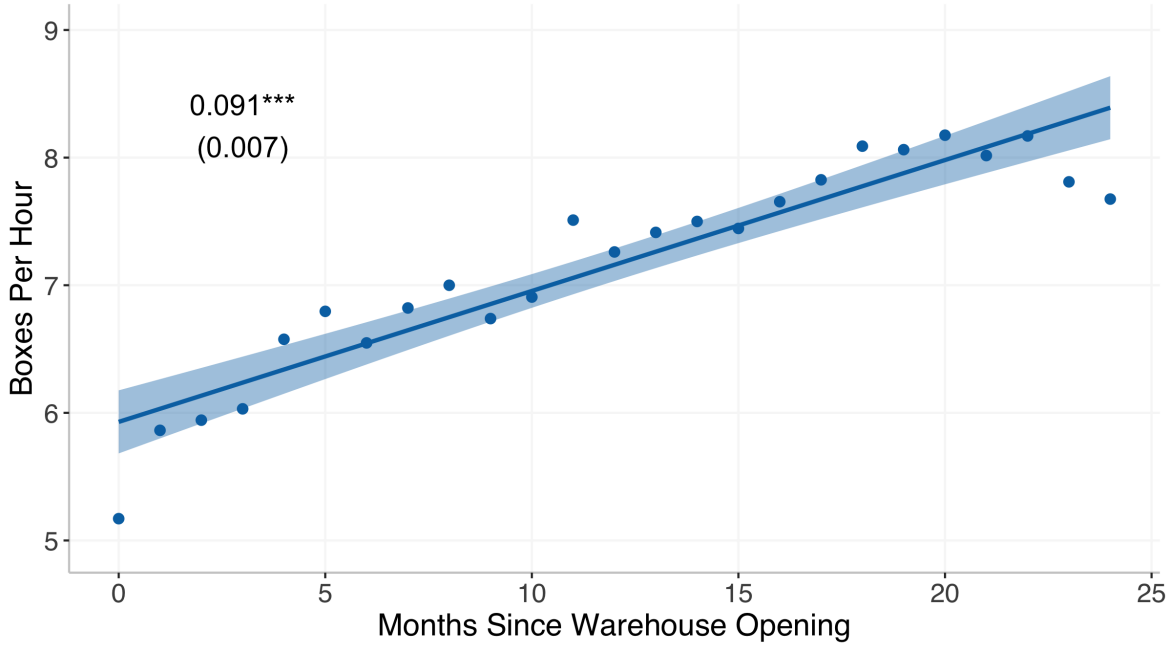
Note: This table shows the results of running Equation 4 on other calendar years' data where the same calendar week in other years is considered the time of implementation. We use a 3-month bandwidth so that we have greater space between the years' comparisons. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

TABLE A.6: PRODUCTIVITY RESPONSE TO A VOLUNTARY FIRM MINIMUM WAGE IN HUB WAREHOUSES ONLY

	$/Hr	Boxes/Hr			
Gap from $15/hr x Post	0.939***	0.228*	0.652***	0.655***	
	(0.022)	(0.135)	(0.194)	(0.195)	
$\widehat{\Delta\$/\text{hour}}$					0.697***
					(0.211)
Elasticity		0.62	1.78	1.79	1.79
		(0.37)	(0.50)	(0.50)	(0.54)
Regional FEs	✓		✓	✓	✓
Controls	✓			✓	✓
Bandwidth	3,6mo	3,6mo	3,6mo	3,6mo	3,6mo
Base Mean	$18.22	7.1	7.1	7.1	7.1
# Warehouses	1X	1X	1X	1X	1X
# Workers	4,XXX	4,XXX	4,XXX	4,XXX	4,XXX
Adjusted R^2	0.993	0.847	0.860	0.860	0.882

Note: This table shows the results of running Equation 4 in the hub warehouses only. We use our preferred bandwidth throughout, progressively adding in equations. Column 1 shows the first stage in hub warehouses. Column 5 scales reduced form estimates by the first stage in a two-stage-least-squares regression.

FIGURE A.9: WAREHOUSE PRODUCTIVITY AS A FUNCTION OF COLLECTIVE WORKER EXPERIENCE



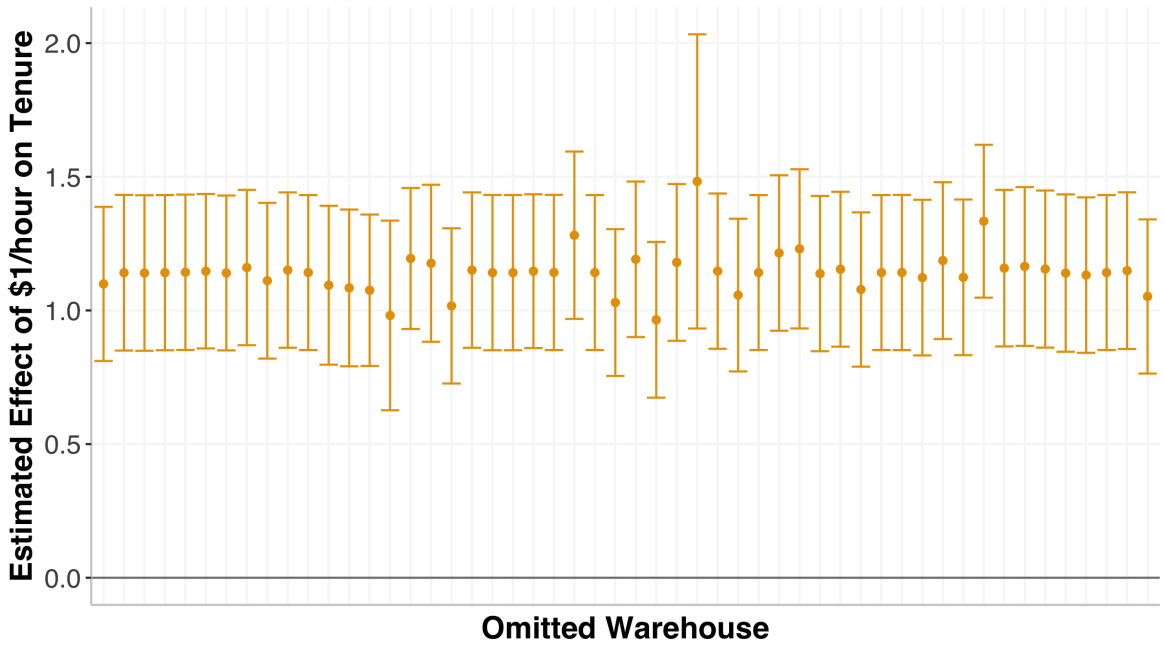
Note: This figure plots the quantile relationships between the months since the warehouse opened and the productivity in the warehouse. The points reflect 25 quantiles of months. The blue dots and lines show the relationship for hubs, the football-field-sized warehouses; the grey triangles and lines show the relationships for spokes.

FIGURE A.10: WAREHOUSE PERMUTATIONS OF REDUCED FORM ESTIMATES OF THE TENURE EFFECTS OF A VOLUNTARY FIRM MINIMUM WAGE



Note: This figure shows the result of 5,000 permutations. Each permutation randomly re-assign warehouses' exposure to the firm's voluntary firm minimum wage, by drawing a random set of wage treatments from the true set with replacement and applying this set to the warehouses. For each permutation, we calculate the change in tenure at these warehouses. This exercise produces a distribution of placebo estimates. The solid black line shows the true estimated effect. The annotation reflects the one- and two-sided p-values from assessing where this estimate falls in the distribution. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

FIGURE A.11: WAREHOUSE EXCLUSIONS IN ESTIMATING OF REDUCED FORM TENURE EFFECTS OF A VOLUNTARY FIRM MINIMUM WAGE



Note: We show the IV estimates of the tenure impacts of the voluntary firm minimum wage, omitting each warehouse in turn. Each point represents an estimate, leaving out one warehouse. Whiskers show 95 percent confidence intervals. A small number of warehouses have been duplicated to preserve the firm's anonymity.

TABLE A.7: PLACEBO TEST OF REDUCED FORM ESTIMATES OF THE TENURE EFFECT OF A VOLUNTARY FIRM MINIMUM WAGE

	Tenure		
	Placebo I	Placebo II	Pooled
Gap from $15/hour x Placebo Post	0.313	−0.104	0.054
	(0.194)	(0.106)	(0.096)
Base Mean	11.48	8.63	9.62
# Warehouses	2X	3X	4X
# Workers	2,XXX	4,XXX	5,XXX
Adjusted R^2	0.968	0.975	0.973

Note: This table shows the results of running Equation 4 on other calendar years' data where the same calendar week in other years is considered the time of implementation. We use a 3-month bandwidth so that we have greater space between the years' comparisons. *p<0.1; **p<0.05; ***p<0.01.

TABLE A.8: DEPARTURES BY TENURE

Panel A: Departures for Workers with Less than 1 Year Tenure

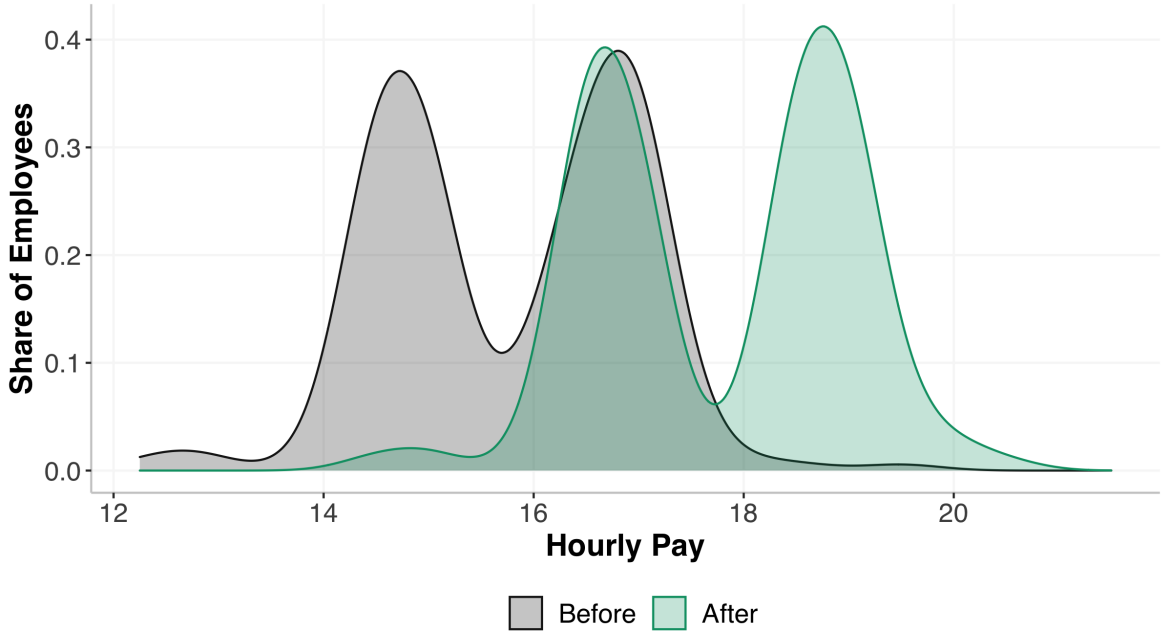
	$/Hr	Departures	Quits	Fires
Gap from $15/hr x Post	0.968***			
	(0.035)			
$\widehat{\Delta\$/\text{hour}}$		−7.556***	−8.357***	1.099
		(2.765)	(2.555)	(0.871)
Base Mean	$17.66	14.59	12.08	1.05
# Warehouses	2X	2X	2X	2X
# Workers	3,XXX	3,XXX	3,XXX	3,XXX
Adjusted R^2	0.972	0.168	0.208	0.016

Panel B: Departures for Workers with 1 Plus Tenure

	$/Hr	Departures	Quits	Fires
Gap from $15/hr x Post	0.798***			
	(0.020)			
$\widehat{\Delta\$/\text{hour}}$		−5.872	−7.701*	1.546
		(4.663)	(4.513)	(1.201)
Base Mean	$18.34	4.86	3.01	1.76
# Warehouses	2X	2X	2X	2X
# Workers	2,XXX	2,XXX	2,XXX	2,XXX
Regional FEs	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Bandwidth	3,6mo	3,6mo	3,6mo	3,6mo

Note: This table shows the IV estimates of workers' departure responses to higher pay by worker tenure. Panel A shows results for workers with less than a year tenure at the firm; Panel B for more senior workers. Standard errors are clustered by location-week. *p<0.1; **p<0.05; ***p<0.01.

FIGURE A.12: DISTRIBUTION OF PAY BEFORE AND AFTER WAREHOUSE PAY JUMP



Note: This figure presents the distribution of pay among entry level warehoue workers within the treated warehouse one week before August 2019 and one month afterward. There are two sources of wage variation: a level shift if the worker works an unpleasant shift or is certified to work on specialized machinery, which generates a bimodal distribution, and wage variation based on when hired, which generates variation around these means.

TABLE A.9: OTHER WAREHOUSE TRAITS AROUND THE PAY JUMP

	Share Hires	Age	Female
Post	−0.030	−0.116	−0.001
	(0.112)	(0.216)	(0.007)
Constant	0.156	35.910***	0.192***
	(0.079)	(0.153)	(0.005)
# Workers	439	439	439
Adjusted R^2	−0.229	−0.166	−0.236

Note: Estimates show changes in warehouse dynamics in the months before and after the pay change. Estimates result from an event-study design comparing traits in the treated warehouse before the pay jump and after. *p<0.1; **p<0.05; ***p<0.01.

TABLE A.10: PRODUCTIVITY AND DEPARTURES RESPONSES TO A PAY JUMP

Panel A: Dimensions of Productivity Increases

	First Stage	Boxes/Hr	Moving/Total Hrs	Boxes/Moving Hr
Post	2.018***			
	(0.045)			
$\widehat{\$/\text{hour}}$		0.389***	0.002	0.462***
		(0.063)	(0.007)	(0.109)
Elasticity		1.25	0.04	0.97
		(0.2)	(0.17)	(0.23)
Contribution			5.4%	94.6%
Base Mean	\$15.71/hr	4.88	0.66	7.46
Workers	6XX	6XX	6XX	6XX
Adjusted R^2	0.975	0.463	−0.016	0.264

Panel B: Dimensions of Departures Decreases

	First Stage	Monthly Turnover	Quits	Fires
Post	2.018***			
	(0.045)			
$\widehat{\$/\text{hour}}$		−3.827***	−3.870***	0.043
		(0.954)	(0.848)	(0.450)
Elasticity		3.07	3.65	0.23
		(0.77)	(0.8)	(2.44)
Base Mean	\$15.71/hr	19.58	16.68	2.9
Workers	6XX	6XX	6XX	6XX
Adjusted R^2	0.975	0.273	0.326	−0.020

Note: IV estimates show the impact of a \$1/hour change in pay on various outcomes in the wake of an August pay jump in a single warehouse. Panel A shows the increase in boxes moved per hour, and whether it arises from an increase the amount of time spent moving or in the number of boxes moved during a moving-hour. Panel B decomposes the observed decrease in departures into its component parts: quits, fires, and other (including layoffs and bails). Observations are at the location-week level. *p<0.1; **p<0.05; ***p<0.01.

TABLE A.11: SENSITIVITY OF PAY JUMP RESPONSES TO TIME SPANS INCLUDED

Panel A: Productivity Response

	Boxes/Hr			Moving/Total Hrs			Boxes/Moving Hr		
	1mo	2mo	3m	1mo	2mo	3m	1mo	2mo	3m
$\widehat{\$/\text{hour}}$	0.082	0.239**	0.279***	0.003	0.018**	0.014**	0.093	0.150*	0.256**
	(0.117)	(0.087)	(0.074)	(0.010)	(0.008)	(0.007)	(0.136)	(0.082)	(0.114)
Workers	3XX	4XX	4XX	3XX	4XX	4XX	3XX	4XX	4XX
Adjusted R^2	−0.033	0.320	0.384	−0.138	0.217	0.132	−0.049	0.173	0.178

Panel B: Departures Response

	Monthly Departures			Quit			Fired		
	1mo	2mo	3m	1mo	2mo	3m	1mo	2mo	3m
$\widehat{\$/\text{hour}}$	−5.954**	−3.592*	−3.865***	−5.472***	−3.853**	−3.930***	−0.482	0.261	0.065
	(2.010)	(1.789)	(1.345)	(1.200)	(1.504)	(1.129)	(1.648)	(0.840)	(0.628)
Workers	3XX	4XX	4XX	3XX	4XX	4XX	3XX	4XX	4XX
Adjusted R^2	0.569	0.219	0.275	0.759	0.296	0.342	−0.137	−0.071	−0.044

Note: IV estimates show the impact of a $1/hour change in pay on various outcomes in the wake of an August pay jump in a single warehouse, as measured at various intervals. Panel A shows the increase in boxes moved per hour, and its component parts: the ratio of moving hours to total hours and the boxes moved per moving hour. Panel B decomposes the observed decrease in departures into its component parts: quits, fires, and other (including layoffs and bails). Observations are at the location-week level. *p<0.1; **p<0.05; ***p<0.01.

TABLE A.12: PLACEBO TESTS OF PAY JUMP RESPONSES IN COMPARABLE WAREHOUSES

Panel A: Productivity in Twin Warehouses

	First Stage	Boxes/Hr	Moving/Total Hrs	Boxes/Moving Hr
Post	0.015	−0.096	−0.007	−0.185
	(0.053)	(0.156)	(0.006)	(0.246)
Base Mean	$15.49/hr	3.89	0.64	6.12
Workers	1,XXX	1,XXX	1,XXX	1,XXX
Adjusted R^2	−0.004	−0.004	−0.004	−0.001

Panel B: Departures in Geographically Proximate Warehouses

	First Stage	Monthly Turnover	Quits	Fires
Post	0.240***	−3.468	−3.269	−0.199
	(0.062)	(3.113)	(3.053)	(0.328)
Base Mean	$14.84/hr	12.11	10.88	1.22
Workers	1,XXX	1,XXX	1,XXX	1,XXX
Adjusted R^2	0.085	0.002	0.001	−0.004

Note: This table shows the reduced form results of placebo tests on the response to the August wage jump. Panel A shows the productivity changes in "twin" warehouses, warehouses that handle the same type of parcel and are in different geographic regions (and thus unlikely to be impacted by a change in productivity in the treated warehouse). Panel B looks at departures in warehouses that are in the same geography as the treated warehouse. The geographically proximate warehouses are less than 30 minutes away by car. Standard errors are clustered by location-week. *p<0.1; **p<0.05; ***p<0.01.