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Abstract

What are the returns to firms of paying more? We study a Fortune 500 firm's voluntary firm-wide $15/hour minimum wage, which affected some warehouses more than others. Using a continuous difference-in-differences design, we find that a $1/hour pay increase (5.5 percent) halves worker departures, reduces absenteeism by 18.6 percent, and increases productivity (boxes moved per hour) by 5.7 percent. These productivity gains fully defrayed increased labor costs, offsetting the firm's incentive to mark down wages. We develop a simple model that connects efficiency-wage incentives and monopsony power, showing how these forces can counterbalance each other to keep wages closer to workers' marginal revenues.

JEL classification: M52, J31, J42
Key words: voluntary firm minimum wage, efficiency wages, monopsony, labor market frictions

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Paying good wages is the most profitable way of doing business.
– Henry Ford in My Life and My Work, 1923

Does higher pay increase worker productivity? And if so, how does this affect firms' wage-setting decisions? We study these questions in the context of warehousing, an important industry for Americans without college degrees.[1] Even in thick labor markets, like warehousing, evidence increasingly suggests that firms have some degree of wage-setting power. Firms would seemingly have an incentive to use this power to pay workers less. Yet doing so can undermine workers' incentives and their efforts, as emphasized by the efficiency-wage literature. How much do efficiency-wage effects curb firms' incentives to use wage-setting power to lower pay?

To explore these questions, we study the introduction of a voluntary, firm-wide minimum wage in a Fortune 500 firm.[2] We use personnel data and objective productivity metrics to quantify higher pay's effects on turnover, absenteeism, and productivity among warehouse workers. At the firm's warehouses, workers operate independently but rely on each other's work, which makes it harder to effectively monitor and reward individuals' efforts.[3] We analyze the extent to which raising base pay boosts the throughput of the entire warehouse, as measured by the average number of boxes moved per worker-hour.

Our empirical design leverages the fact that the firm's minimum wage policy had differential impacts across its forty-plus warehouses. When the firm's C-suite executives introduced a firm-wide minimum wage, some warehouses' wages rose by more than others'. Half of the warehouses already had base wages above \$15/hour and were thus unaffected. The other half of warehouses increased base wages for all workers to the new minimum. In these warehouses, larger wage increases were required where the initial base wage was further below \$15/hour, with raises ranging from \$0.50 to \$2/hour. We

[1] Warehouse workers comprise 5 percent of the US labor force (Bureau of Labor Statistics, 2022a).

[2] Many firms have implemented such minimum wages: in 2020 and 2021, Best Buy, Target, Wayfair, CVS, Walgreens, and Amazon all announced voluntary minimum wages for their employees for example.

[3] For example, a worker who retrieves boxes quickly but knocks others out of place hinders their coworkers' ability to find these misplaced boxes. Partially due to concerns about difficult-to-measure work quality, the firm did not incentivize (or systematically track) individual productivity during our study period.

leverage this variation in a continuous difference-in-differences design.[4]

We find that on average, a \$1/hour (or 5.5%) increase in pay halved workers' departure rates, implying the firm faces a short-run labor supply elasticity around nine. This large but finite labor-supply elasticity suggests that warehouse firms face upward sloping labor-supply curves, even in these competitive warehousing labor markets. This evidence supports the theory that search frictions and other barriers to job-to-job mobility can give firms monopsony wage-setting power even in thick labor markets (Burdett and Mortensen, 1998; Manning, 2003).

When pay increased, absenteeism declined. A \$1/hour increase in pay reduces the likelihood that workers do not show up for a day of work by 2.7 percentage points or 18.6%. About half of these declines in absenteeism persist when we include individual fixed effects: existing workers become more reliable once they are paid more.

Warehouses that saw bigger increases in pay also saw bigger increases in productivity. When the firm imposed its \$15/hour minimum wage, the productivity of its lowest paid and least productive warehouses caught up to the productivity in its other warehouses. On average, a \$1/hour (5.5%) increase in pay led to a 5.7% boost in productivity. Thus, our estimates indicate that, at the margin, a one percent increase in pay increases productivity by one percent.

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: in the absence of the firm's minimum wage policy, its warehouses would have followed similar trends. We gauge the plausibility of this assumption using both institutional knowledge and empirical tests. Institutionally, the policy change was driven by C-suite decisions rather than local conditions. The firm-wide minimum wage was not targeted specifically at warehouse workers but instead impacted all low-wage workers, including sales and

[4] Our continuous difference-in-differences approach is similar to designs that use state-level variation in the impact of the federal minimum wage to estimate the economic effects of government-mandated minimum wages (Card, 1992; Bailey et al., 2021). Derenoncourt et al. (2021) also uses a similar design to identify the impact of firm minimum wages on nearby firms' wages.

service workers. Thus, our context offers a rare case where a firm raised pay in some warehouses but not in others without intentionally choosing certain warehouses for the raises. Empirically, we find stable pre-trends, null effects with placebo treatments at alternative times, and robustness to dropping individual warehouses.

One limitation of this analysis is that the voluntary minimum wage was implemented during the pandemic.[5] To assess external validity, we use a complementary pre-COVID design. A few years before the pandemic, one warehouse experienced an abrupt $2/hour pay increase. While this warehouse was not randomly selected, the timing of the increase was plausibly exogenous, as it was determined by corporate decision-makers and significantly delayed from local managers' initial requests. Following the pay increase, turnover and absenteeism declined, while productivity rose. These changes plausibly resulted from the pay increase: we do not see similar changes in the firm's other warehouses, including those nearby and those handling similar boxes. The effects are similar in both direction and magnitude to those from the voluntary minimum wage, indicating that higher pay's strong productivity impact extends beyond the pandemic labor market.

We find evidence that higher pay increases employee effort. When pay increases, absenteeism falls among existing workers in both our voluntary minimum wage and pre-COVID designs. We find complementary evidence using data from a Fortune 100 staffing agency: higher pay is associated with more favorable ratings of warehouse workers' performance, and about half of this relationship persists when we include worker fixed effects. When the same worker does two different jobs, she is more likely to excel in the job that pays her more. Together, these results suggest the continued relevance of efficiency-wage theories that link workers' pay to their productivity through their effort (e.g., Salop, 1979; Shapiro and Stiglitz, 1984).

To bridge monopsony and efficiency-wage theory, we develop a partial-equilibrium model of the firm's wage-setting decisions, which integrates both search frictions and

[5] The voluntary minimum wage was implemented before widespread inflation during the pandemic so the wage increases represented real — rather than only nominal — increases in pay.

efficiency-wage effects.[6] Search frictions create an upward-sloping labor supply curve to the firm, thereby allowing the firm to depress wages (Burdett and Mortensen, 1998; Manning, 2003). Meanwhile, efficiency-wage effects mean that raising pay increases worker effectiveness (e.g., Salop, 1979; Shapiro and Stiglitz, 1984), thereby flattening out the *effective* labor-supply curve and reducing the firm's incentive to mark down wages. This model illustrates how efficiency-wage incentives can offer a counterbalance to firms' monopsony power. Indeed, if efficiency wage effects are sufficiently strong, the firm becomes constrained by workers' no-shirking condition: while the firm could hire workers at lower wages, the firm could not expect such hires to work hard. In this case, our partial-equilibrium framework would extend to Shapiro and Stiglitz (1984)'s canonical model in general equilibrium.

Our empirical results suggest that the reductions in turnover and increases in productivity fully defrayed the increased labor costs of higher pay. Consistent with higher pay being profitable, the firm's stock price increased in the wake of the firm-wide minimum wage relative to comparable firms. Thus, it would seem that the payoffs of higher pay fully offset the firm's incentive to mark down workers' pay — and the equilibrium would be characterized by efficiency-wage theory.

This paper makes three contributions. It offers strong tests of efficiency-wage theories, which have been challenging to test empirically. It tests monopsony theories in thick labor markets. Finally, the paper puts two theories of wage-setting in conversation with one another, documenting that the payoffs of higher pay temper the firm's motive to use its monopsony power to depress wages.

First, we empirically test efficiency-wage theories. While an important literature has developed efficiency-wage theory (e.g., Salop, 1979; Solow, 1979; Shapiro and Stiglitz, 1984; Yellen, 1984; Katz, 1986; Weiss, 2014; Naidu and Carr, 2022), these models have been challenging to test empirically, since most pay changes are endogenous and most

[6] In this partial equilibrium framework, the firm takes as given other firms' wage-setting policies. This assumption is appropriate for two reasons. First, the firm employs just 2% of warehouse workers within its metropolitan areas. Second, the firm was not a wage leader: before its firm-wide minimum wage, its wages were near the middle of the warehouse wage distribution.

workers' productivity is unmeasured. Empirically, Raff and Summers (1987) explore the case study of Henry Ford's voluntary minimum wage policy and argue that higher wages increased productivity by reducing turnover and absenteeism. In a similar vein, Sandvik et al. (2021) investigate a modern case study at a sales firm: when one division reduced commissions, turnover increased, particularly among more productive workers. Cappelli and Chauvin (1991) leverage a uniform pay policy at a multi-plant firm to study whether higher pay relative to local competitors is associated with lower rates of disciplinary dismissals.[7] We use a natural experiment coupled with objective productivity metrics and turnover data to test efficiency-wage theory in a novel and highly relevant setting. Our findings suggest that Henry Ford's positive assessment of his 1914 firm minimum wage policy — as "one of the finest cost cutting moves we ever made" — could be applied to this modern firm's minimum-wage policy (Ford and Crowther, 1923).

A related literature explores the productivity effects of *government-mandated* minimum wages (Jayaraman et al., 2016; Ku, 2022; Coviello et al., 2022).[8] Our approach differs from this literature because, in our setting, pay changes in the firm are independent of those at other local firms, thereby allowing us to estimate the labor-supply elasticity to the firm and quantify its wage-setting power. One of our central contributions is to clarify how efficiency-wage effects change the firm's incentive to use its wage-setting power — an analysis that would not be possible using government-mandated minimum wage changes that simultaneously raise workers' wages and their outside options.

Second, our analysis of thick warehouse labor markets provides a strong test of new monopsony models. Mounting evidence indicates that firms have wage-setting power in a wide array of contexts, such as nursing (e.g., Sullivan, 1989; Staiger et al., 2010), teaching

[7] Papers have also tested the productivity effects of performance pay (e.g., Lazear, 2000; Bandiera et al., 2007; Muralidharan and Sundararaman, 2011) and *relative* pay (e.g., Cohn et al., 2015; Dube et al., 2019).

[8] Ku (2022) finds that increases in the state-level minimum wage increases the productivity of tomato pickers in Florida, while Hill (2018) finds the opposite result among strawberry pickers in California. Jayaraman et al. (2016) find large but ephemeral increases in productivity for tea harvesters in India after a minimum wage increase among workers who are hard to fire. Coviello et al. (2022) find meaningful increases in productivity of sales workers at a large American department store chain: notably, the estimated elasticities of productivity with respect to the wage approach one and offset much but not all of the additional costs of higher compensation. Dustmann et al. (2022) find that a government-mandated minimum wage prompts reallocation of workers to more productive firms, which could also increase productivity.

(Ransom and Sims, 2010), and the civil service (Dal Bó et al., 2013).[9] While others have shown that few firms in a particular region can lead to wage-setting power (e.g., Azar et al., 2022), our paper shows that, even in markets with many firms, employers can still retain some wage-setting power. This finding aligns with Dube et al. (2020), who find evidence of monopsony power even in an online labor market with commoditized tasks.

Finally, our paper puts these two theories of firm wage setting in conversation with one another. The paper documents that the payoffs of higher pay should temper the firm's motive to use its monopsony power to depress wages. It both develops and tests a tractable model that includes (i) search frictions, which can lead to monopsony power, and (ii) worker effort, which generates efficiency-wage incentives. This theory also emphasizes that managers' beliefs about — and attention to — the relationship between workers' pay and productivity can crucially determine wage setting.

The rest of the paper is organized as follows: Section I introduces our model. Section II introduces the data and setting. Section III details the firm's voluntary minimum wage policy and its impacts on productivity, absenteeism, and turnover. Section IV probes the robustness of our findings by examining another sharp jump in pay at a single warehouse before the pandemic. Section V investigates mechanisms and incorporates complementary evidence from a Fortune 100 staffing agency. Section VI estimates the firm's return on their investment in higher pay. Section VII concludes.

I Model

We consider a partial equilibrium model of firms' wage-setting decisions that features both search frictions and efficiency-wage incentives. Search frictions may prevent workers from seamlessly leaving their current firms, as in monopsony models (Burdett and Mortensen, 1998; Manning, 2003). Yet workers' productivity may respond to wages, as in efficiency-wage models (Salop, 1979; Solow, 1979; Shapiro and Stiglitz, 1984; Yellen, 1984). We use this model to organize the implications of our empirical analyses for firms'

[9] Several papers further use linked employer-employee data to draw a connection between firm labor supply and workers' earnings (e.g., Webber, 2015; Bassier et al., 2020).

wage-setting decisions.[10] Finally, we sketch the general-equilibrium implications.

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

$$\max_{w,N} \Pi = Y(Ne(w)) - wN \text{ s.t. } N \leq N(w), \tag{1}$$

where $e(w)$ indicates the average productivity of each worker, which can reflect both the composition and the effort of the firm's workforce. The firm can employ at most the $N(w)$ workers who apply to the job but need not hire all applicants. The firm operates in a market with many other firms and so does not consider how its chosen wage affects rivals' wage-setting decisions. This set-up is appropriate for our empirical setting since warehouses tend to cluster around logistics hubs such as interstate junctures.

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, congestion as the number of workers increases, or imperfect competition in the output market.

We use the first order condition of Equation 1 to express the profit-maximizing wage in terms of two key elasticities. First, we define the productivity elasticity as $\epsilon_{e,w} = \frac{\partial e(w)}{\partial w} \frac{w}{e(w)}$, which captures how workers' productivity responds to wages. Second, we define the labor-supply elasticity to the firm. 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 is recruited by another firm (Manning, 2003).[11] Under this assumption, the labor-supply elasticity is twice the elasticity of

[10] Manning (2003) presents a model with both search frictions and efficiency-wage incentives but assumes firms can observe workers' effort-types. Each firm acts as a monopsonist within the set of workers who won't shirk and refuses to hire those who will. We, instead, assume that the firm can't tell who will shirk.

[11] The standard assumption that recruitment is the mirror image of departures is convenient because we do not have data on recruitment. Recently Datta (2023) tested this assumption and found that the elasticity of recruitment is larger than the elasticity of departures in a large UK services firm. If this also holds in our

departures, $\epsilon_{\text{Departures},w} = \frac{\partial \text{Departures}(w)}{\partial w} \frac{w}{\text{Departures}(w)}$.

Using these two elasticities, we arrive at an equation for optimal wages, whose full derivation is in Appendix A:

$$w = \begin{cases} Y'(Ne)e\left(\frac{2\epsilon_{\text{Departures},w}+\epsilon_{e,w}}{1+2\epsilon_{\text{Departures},w}}\right) & \text{if } \epsilon_{e,w} \leq 1 \text{ and } N = N(w) \\ Y'(Ne)e & \text{if } \epsilon_{e,w} > 1 \text{ and } N < N(w). \end{cases} \tag{2}$$

This optimal wage expression captures several useful predictions. First, if higher pay increases productivity $\left(\frac{\partial e(w)}{\partial w} > 0 \text{ and so } \epsilon_{e,w} > 0\right)$, then it will be profit-maximizing to set a higher wage than if productivity were unrelated to the wage $\left(\frac{\partial e(w)}{\partial w} = 0\right)$. A number of (non-mutually exclusive) mechanisms can link productivity and wages. First, higher wages may increase effort: by making the job more valuable, higher wages may deter fireable offenses like shirking (Shapiro and Stiglitz, 1984) and encourage effort in a sort of reciprocal exchange (Akerlof and Yellen, 1990). Second, higher pay may increase firm-specific human capital, as workers persist longer at the firm (Salop, 1979; Krueger and Summers, 1988).[12] Third, higher pay may improve worker selection if workers' latent productivity and reservation wages are positively correlated (Weiss, 1980).

Expression 2 also captures a fundamental point about the labor-supply curve in the absence of efficiency-wage forces $\left(\frac{\partial e(w)}{\partial w} = 0\right)$. 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 must pay workers their marginal revenues. Yet, as the elasticity of departures shrinks and the labor supply curve tilts upward, wages dip below workers' marginal revenues: $w = Y'(N)\left(\frac{2\epsilon_{\text{Departures},w}}{1+2\epsilon_{\text{Departures},w}}\right) < \text{MRP}$. An upward-sloping labor-supply curve may arise if, for example, there are few local employers or if workers face frictions when considering alternative jobs (Robinson, 1933; Manning, 2003). The resulting wage-setting power leads to a wage markdown for a firm that posts a single wage for all workers: such a firm must

context, then our approach would underestimate the labor-supply elasticity to the firm.

[12] If there are returns to tenure, the firm could make wages tenure dependent. When we extend our model to two periods in Appendix A, we arrive at similar predictions.

weigh the benefits of attracting a marginal worker against the costs of increasing wages for inframarginal workers. In the absence of efficiency-wage forces, this dynamic leads the firm to set an optimal wage rate below the workers' marginal revenue.

The expression also highlights how the productivity elasticity $\epsilon_{e,w}$ moderates the effects of labor-market frictions on wage-setting. When the firm faces an upward-sloping labor-supply curve ($\epsilon_{\text{Departures},w} < \infty$), then $\epsilon_{e,w}$ helps determine whether (and how much) the firm marks down wages from workers' marginal revenue. Visually, accounting for worker effort and selection flattens the *effective* labor-supply curve by causing a bigger change in effective labor supply for a given change in the wage. The resultant question is: by how much? If $\epsilon_{e,w} \in (0, 1)$, efficiency-wage effects will dull the firm's incentives to use its monopsony power, narrowing but not eliminating the gap between the wage and the marginal revenue. If $\epsilon_{e,w} = 1$, efficiency-wage incentives fully offset the firm's incentive to use its monopsony power, thereby eliminating the gap between a worker's wage and marginal revenue. If $\epsilon_{e,w} > 1$, the firm finds it less expensive to increase *effective* labor supply by raising the wage than by hiring more workers. As a result, the firm will set a high wage to induce effort but will not hire all the workers who apply instead choosing $N < N(w)$ such that the marginal revenue equals the wage.

General-Equilibrium Implications. When efficiency-wage effects are strong ($\epsilon_{e,w} \geq 1$), our partial-equilibrium model extends to the general-equilibrium model of Shapiro and Stiglitz (1984). In this case, all firms will have an incentive to raise pay. Yet, when they all do so, this undermines the incentive effects of higher pay. A worker knows if she loses a job at one firm, there are other firms that will pay her just as much. However, as firms raise pay, they move up their labor demand curve and want to hire fewer workers: this will generate involuntary unemployment and restore the incentive effects of higher pay. A worker then knows if she loses a job at this firm, she risks being unable to find another job. The equilibrium in the market can be restored where the labor demand of firms intersects with the incentive constraint of workers.[13]

[13]One concern is that if increasing pay is an optimal strategy, then all firms should raise wages, which could generate inflation. The Shapiro and Stiglitz (1984) model suggests that even if inflation might be a concern in the short run, in the longer run wages would stabilize and inflation would ebb.

In Shapiro and Stiglitz (1984), the firm's binding constraint is its ability to incentivize workers, not its ability to recruit and retain them. Thus, the firm's upward-sloping labor-supply curve provides no strategic advantage — while the firm could theoretically mark-down wages and still recruit and retain workers, these new workers would not have incentives to work hard. As a result, Shapiro and Stiglitz (1984)'s model — with its binding incentive constraint – applies just as well to our frictional efficiency-wage model (when $\epsilon_{e,w} \geq 1$) as it does to the benchmark frictionless one.

Identifying which constraint binds — whether it is incentives or labor supply — is critical to understanding the primary drivers of equilibrium wages and employment levels. In dynamic monopsony models, workers are paid a markdown on their marginal revenue product and unemployment arises due to search frictions. In efficiency-wage theory, workers are paid their marginal revenue products, but unemployment still arises as a disciplinary device to make job loss costly. Yet a high unemployment rate is less necessary when monitoring technology is better and when unemployment benefits are less generous — forces that would not feature in standard monopsony models. By identifying what constraint binds for a specific firm, this paper can speak to the broader determinants of the labor market equilibrium.

II Data & Empirical Setting

We study a Fortune 500 firm. Our data come from several administrative sources, spanning 2018 to 2021. The firm's human resources (HR) data track every hire and termination. From this HR data, we construct a running time-series of the pay and departure rates of the firm's warehouse workers.[14] Additionally, the firm's warehouse management system tracks each warehouse's weekly productivity, based on boxes moved per hour, as well as its workers' absenteeism. In addition to firm data, we use public data to contextualize the firm's labor-market position (detailed in Appendix B).

Table 1 summarizes the data on the firm's warehouses. To preserve the firm's

[14]We focus on warehouse associates, excluding managers, mechanics, and contingent workers. These associates, whom we refer to as workers throughout the rest of the paper, comprise the core team of box movers.

anonymity, we neither specify the precise numbers of workers and warehouses nor reveal the exact timing of the voluntary firm-wide minimum wage. Column 1 includes all US-based warehouse workers at the firm. Between 2018 and 2021, the firm employed over fourteen thousand warehouse associates in more than fifty warehouses. Column 2 summarizes the six months on either side of the firm's introduction of a $15/hour voluntary firm-wide minimum wage, which occurred during the COVID-19 pandemic but not around its major upheavals (the initial lockdown, stimulus checks, or inflation of 2022). Column 3 shows the six months on either side of a discrete pay jump in a single warehouse, which occurred before the pandemic.

II.A Wages

In the firm's warehouses, hourly compensation for warehouse workers follows a rigid pay formula. Each warehouse has a base pay rate, which averages just below $15/hour over our entire period (Table 1A) and always exceeds the state-mandated minimum wage by at least $1/hour. Workers' take-home pay can exceed the base rate due to tenure premiums, machinery operation certifications, or premiums for unpleasant shifts (e.g., night shifts). On average, workers receive an additional $2.30/hour in premium pay. Notably, these premia do not include any individual or group-level performance pay. The firm applied its voluntary $15/hour minimum-wage policy to base wages. As a result, half of the firm's warehouse workers were impacted by the policy, even though many of these workers had take-home pay that already exceeded $15/hour.

Before instituting its voluntary minimum wage, the firm set wages to target the middle of the wage distribution in its labor markets (Figure A.1A). Indeed, the average local pay in warehousing explains 45% of the variation in the initial base pay across the firm's warehouses. In the months leading up to the firm-wide minimum wage, the firm had already started to pay a bit above the local averages (Figure A.2).[15] As a result, after imposing its voluntary minimum wage, the firm offered wages above local averages. Since workers' outside options and cost of living were fairly stable during this period, the pay

[15] A previous draft compared the *base* pay in the firm's warehouse with the *average* pay in other local warehouses, which understated the competitiveness of the firm's pay.

raises constituted stark increases in nominal, real, and relative wages.

II.B Terminations

As is often the case in warehouses, turnover rates are high: each warehouse loses 11 out of every 100 workers monthly (Table 1B), amounting to an annual turnover rate of 132%. Most departures reflect quits, not fires.[16] Given the high rate of turnover at the firm's warehouses, the typical tenure is just over a year. The firm's rate of turnover partially reflects the high separation rate in the entire warehousing sector, which averaged 3.3 workers per hundred each month in the firm's Metropolitan Statistical Areas (MSAs).[17] While turnover across firms in the warehousing sector is common, most workers persist in the industry: over three-quarters of warehouse workers continue to work in the sector from year to year, both before and during the COVID-19 pandemic (Table A.1).

II.C Productivity

The firm defines warehouses' weekly productivity as the ratio of total boxes moved to total hours worked. To calculate the numerator, the firm averages the warehouse's inflow of received boxes and outflow of shipped boxes, thereby ensuring a box counts as one unit moved only once it is both received at the loading docks and shipped out on outgoing trucks. To calculate the denominator, the firm focuses on warehouse workers' paid work hours, excluding absent hours (described below) and hours of other personnel such as supervisors. On average, a worker moves the equivalent of six boxes through the warehouse per hour (Table 1C). Workers do not spend all their time moving boxes: workers must audit inventory, clean up empty pallets, meet with their shift colleagues, go to training sessions, and take breaks. In the two-thirds of their time spent solely moving boxes, the typical worker moves nine boxes per hour or about one box every six minutes.

What happens in a box's journey through the warehouse depends on the type of warehouse. There are two broad types of warehouses in the firm's logistics network:

[16] We categorize those who are fired for poor attendance as quits, since they often resemble quits without notice from both the worker's and firm's perspective.

[17] The industry-level separation rate includes all workers, regardless of occupation, and so includes administrative assistants, managers, and others who tend to have lower turnover than warehouse workers.

the "hubs" that store goods in centralized locations and the "spokes" that connect hubs to consumers. Each hub employs a few hundred workers in a warehouse that could fit tens or even *hundreds* of football fields (as shown in Figure 1). In the six minutes that a box moves through a hub, it must be received from the loading dock, unwrapped, checked by a quality assurance team,[18] put away for storage, retrieved when an order comes in, repackaged and relabeled as needed, and ultimately put on an outgoing truck. By contrast, in the small "spoke" warehouses, there is a quick turnaround time between receipt and shipping, generally without storage or repackaging. However, the packages themselves tend to be quite large, sometimes requiring teams to move.

In the hubs, much of the work requires specialized knowledge. This firm's large and unwieldy inventory limits the use of automation technologies. While the firm uses *directed* picking — where computers identify items' locations — it does not use *automated* picking, where machines actually retrieve and deliver items. Additionally, workers decide where to store items rather than relying on directed put-away system that would specify where to place incoming packages. Consequently, a worker must be familiar with the warehouse's layout, the current status of its shelves, the computer inventory systems, and how their work integrates with that of the rest of their team.

Workers often learn as they go. According to one warehouse manager, workers often learn how to operate new machines on the warehouse floor, akin to "first learning how to drive on the highway." Workers also learn the art of stowing boxes 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, workers must develop the muscle memory to physically move a package off of a (swaying) platform onto a rack while forty feet in the air. Figure 1 depicts workers putting away and retrieving boxes in a hub.

II.D Absenteeism

We collected data on absenteeism. We observe whether a worker missed an assigned shift and whether she had a pre-approved excuse for this absence (e.g., for sick leave or

[18] Quality assurance checks random boxes and then checks the whole load if defective products are found.

jury duty). We focus on unexcused absences as they are the most disruptive for warehouses. On a typical work day, 12.5% of workers who are scheduled to work unexpectedly do not show up (Table 1D).[19] When we also include late starts and early departures, workers unexpectedly miss an average of two hours per day. This high rate of absenteeism is emblematic of the industry: using data from a major staffing agency that contracts with thousands of warehouse firms, we see that 15% of temporary warehouse jobs are terminated early because of poor attendance (Table A.10).

II.E Labor Market Context

In terms of its warehouse workforce, this firm is a reasonably sized fish in a very big pond. On average, the firm employs 1.9% of warehouse workers in the MSAs in which it operates and at most 6.2% (Figure A.1B shows the distribution). The firm also competes with many other firms, since the labor markets in which it operates are generally unconcentrated. As of 2016, these labor markets had an average Herfindahl-Hirschman index (HHI) of 109, suggesting that about a hundred warehouse firms operate in the labor markets of this firm. The average HHI in the US is 4,378, which suggests 2.3 firms operate in the typical market (Azar et al., 2022).[20]

While a portion of our analysis period overlaps with the COVID-19 pandemic, it does not coincide with its big upheavals and adjustments — not the initial lockdowns, stimulus checks, nor the inflation of 2022. Further, when thinking about pandemic risks, it is useful to keep in mind that workers typically work solo in warehouses that span many football fields, and so are usually more than six feet from one another (see Figure 1). In terms of proximity to others, this job is more similar to a solitary Amazon delivery job than to a manufacturing job where operators might sit within a few feet of one another. Perhaps due to this fact, there was not an exodus from warehousing during the pandemic: instead, transition rates out of the sector are comparable to those in previous years (Table A.1).

[19] In total, 21% of scheduled workers do not come in but about forty percent of these absences are excused in advance.

[20] Azar et al. (2022) use job ad data to calculate the HHI, or the sum of the squared market share of each firm in the market. The HHI approaches zero for a market with infinite, similarly-sized firms and is 10,000 for a market with a single firm. An HHI of 109 suggests a relatively competitive market.

III A Voluntary Firm Minimum Wage

We leverage the firm's introduction of a $15/hour minimum wage to identify the determinants of wage-setting in our model. First, the more worker departures respond to pay, the weaker the firm's monopsony power over pay. Second, the more productivity responds to pay, the less motive the firm has to *use* its monopsony power.

III.A Empirical Approach

Our design leverages the fact that the firm's implementation of its $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 so 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 between the initial wage and $15/hour: for example, warehouses with initial pay of $13/hour experienced a $2/hour increase in pay, while those with initial pay of $14.50/hour saw a more modest increase of $0.50/hour. Warehouses that saw larger increases in absolute pay also saw larger increases in relative pay as wages in other local warehousing jobs all smoothly trended upward around the introduction of the voluntary minimum wage (Figure A.2).

Figure 2B shows that the firm-wide minimum wage shifted up the full distribution of take-home pay in affected warehouses. All workers in a warehouse with base pay 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 policy did not change dynamic incentives, in contrast to government-mandated minimum wages that often limit the scope of incentive pay (Hill, 2018; Coviello et al., 2022).[21] Moreover, all workers in each warehouse received the same raise, mitigating feelings of unfairness (Dube et al., 2019). In addition, warehouses that received pay raises were far from those that did not, further limiting workers' exposure to colleagues who saw pay raises that they did not receive and thereby mini-

[21]Managers, being salaried, didn't receive pay increases. However, the resulting compression in pay likely had minimal impact on incentives, as no warehouse associate in our data became a salaried manager.

mizing any spillovers onto the control group.

We use a continuous difference-in-differences design to leverage the heterogeneous 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 of at least \$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' next paychecks). We estimate a two-stage least-squares regression:

$$\begin{aligned} &\textbf{First Stage:} \qquad \$/\text{hour}_{h,t} = \beta_{1\text{st}}(\text{Gap from }\$15_h \cdot \text{Post}_t) + X'_{h,t}\rho + \mu_t + \mu_h + u_{h,t} \\ &\textbf{Second Stage:} \qquad Y_{h,t} = \beta_{2\text{nd}}\,\Delta\widehat{\$/\text{hour}} + X'_{h,t}\phi + \mu_t + \mu_h + v_{h,t}. \end{aligned} \tag{3}$$

where $Y_{h,t}$ represents the outcome of interest (e.g., boxes moved per hour) in warehouse h in week t; μ_t denotes week fixed effects; μ_h, warehouse fixed effects; and $X_{h,t}$, time-varying warehouse controls. We weight the regression by warehouse size (proxied by boxes moved in the preceding year). Given the relatively small number of warehouses, we supplement our robust standard errors with Fisher exact p-values, which report the frequency with which random permutations of "treated" warehouses produce placebo estimates as large as the focal estimate (Fisher, 1925).

Using two-stage least-squares allows us to interpret $\beta_{2\text{nd}}$ as the effect of paying \$1/hour more even though $\hat{\beta}_{1\text{st}}$ is slightly below one (\$0.90/hour, Figure 3A). This approach also allows us to probe robustness to considering discrete, rather than continuous, treatments. We include indicators for whether a warehouse is intensely treated (because its base pay was initially $\leq$\$14/hour), lightly treated (initially \$14.50/hour), or untreated (initially $\geq$\$15/hour), as well as a binary version of this classification.

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:

$$\epsilon_{y,w} = \frac{dy/Y}{dw/w} \quad \rightarrow \quad \hat{\epsilon}_{y,w} = \frac{\hat{\beta}_{2\text{nd}}/\bar{Y}_0}{1/\bar{\$}_0} \tag{4}$$

where $\bar{Y}_0$ and $\bar{\$}_0$ represent averages in the treated warehouses prior to the firm-wide minimum wage.[22] The estimated turnover and productivity elasticities are the empirical analogues of the elasticities in the model's wage-setting equation (Equation 2).

The voluntary firm-wide minimum wage was implemented at one time across all the warehouses, so concerns over staggered two-way fixed effects designs do not apply (e.g., Goodman-Bacon, 2021). Our preferred specification uses a six-month post-period and a three-month pre-period to exclude a pay recalibration that selectively increased pay in some warehouses (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 if not for the firm-wide minimum wage. The top-down nature of the policy suggests the assumption is defensible. Regional managers were not consulted and, instead, only learned about wage changes in their warehouses after the policy was finalized, mere days before the public announcement. As a result, local input did not determine local wage changes. Moreover, the minimum wage policy was not implemented alongside other HR initiatives. Finally, there is no evidence of differential pandemic-related changes, as sick leaves did not change differently across more and less affected warehouses (Figure A.3).

To probe the robustness of this assumption, we include a variety of time-varying warehouse controls, $X_{h,t}$. We include region-by-week fixed effects, where we define regions based on firm-defined logistics regions.[23] 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.[24] Further, Section IV shows similar results using a complementary design based on a pre-pandemic pay jump at a large warehouse.

[22]Elasticities are often computed using log-log regressions. We can't do this for departures, which are zero in some warehouse-weeks. For productivity, log-log regressions yield similar results as Equation 4.

[23]The firm's logistics regions are Northeast, Southeast, Central, Mountain, Southwest, and West. Each region has seven to nine warehouses and features both treated and untreated warehouses.

[24]Our preferred specifications use COVID-19 deaths since they are less susceptible to measurement error from variation in testing. Our results are robust to controlling for recorded cases.

III.B Effects of the Voluntary Firm Minimum Wage

III.B.1 Departures

Increasing pay decreases worker departures from the firm. Before the firm-wide minimum wage, departures in the treated and control warehouses trended similarly. Afterwards, departures declined at the treated warehouses relative to the control warehouses (the right plot of Figure 3B). The raw trends suggest that the increase in pay offset other market forces prompting an increase in worker departures: departures increased in control warehouses (the black circles in the left plot) but not in treated warehouses. The impact of higher pay on departures increases over time, starting small and intensifying throughout our data period, which ends a year after the policy's introduction (see Figure A.4(a) for the full time series). A similar intensifying effect is observed in other studies (e.g., Sandvik et al., 2021), potentially due to the gradual arrival of competing offers.

Increasing pay by \$1/hour or 5.5% (from a base of \$18.02/hour) reduces monthly departures by 4.1 workers per 100 or 48% (from a base of 8.4) (Column 5 of Table 2A). Comparing the 48% decrease in departures to the 5.5% increase in pay implies a departure-elasticity of 8.7 (95% CI = [1.7, 15.7]). This elasticity is larger than ones typically estimated in the literature (e.g., Hirsch et al., 2010; Dal Bó et al., 2013; Bassier et al., 2020), which may reflect the thickness of warehouse labor markets around logistics hubs. Though large, the response is finite, suggesting that the firm still has some wage-setting power from worker frictions. Taken by itself, our model suggests that the firm would use this wage-setting power to mark down the wage by 5 cents on the dollar (95% CI = [1, 10]).[25]

Our results are robust to accounting for changing features of the local labor markets where the firm operates. Table A.4A controls for the pay in workers' outside options. We first control for the average local pay within warehousing. We then control for a more holistic measure of workers' outside options, which incorporates pay not just in warehousing but also in the other occupations that former warehouse workers often enter.[26]

[25] We apply Equation 2, assuming $\epsilon_{e,w} = 0$: $\frac{Y'(Ne)e-w}{Y'(Ne)e} = \frac{1}{1+2\epsilon_{\text{Departures},w}} = \frac{1}{1+2*8.7} = 0.05$.

[26] We characterize workers' outside options based on typical future occupations of warehouse workers, as

Our results are also robust to benchmarking departure rates at the firm to those in the local warehousing and transportation sector (Table A.5), which may help to absorb local pandemic shocks and other factors affecting all firms in the local labor market.

We consider a number of additional robustness checks. First, we find similar results for sparser specifications (Columns $2-3$ of Table 2A) and when using discrete rather than continuous treatment indicators (Table A.3(a)). Second, we consider a placebo check that applies Equation 3 in other years, which reveals insignificant results with the opposite sign (Table A.6(a)). Third, we perform a permutation test that randomly reassigns which warehouses receive placebo pay increases: our one-sided p-value of 0.04 suggests that it is unlikely this change could have resulted by chance (Figure A.5(a)). Finally, we exclude each warehouse in turn and find stable results (Figure A.6(a)).

Unpacking Departures. The decrease in departures is driven by quits (Table 2B). By contrast, we see no change in firings (Table 2C) or departures for other reasons (e.g., layoffs). In addition to this revealed-preference metric of retention, higher pay also improved workers' stated satisfaction or quarterly "net promoter" scores (Figure A.7).[27] A $1/hour increase in pay increases worker satisfaction by 75% (p-value = 0.004), which is even larger than the 48% decline in departures. These pieces of evidence are consistent with efficiency-wage models that predict higher wages mean a job is more valuable to a worker.

Heterogeneity by Labor Market Context. The effects of higher pay on departures may depend on the surrounding market. In places with fewer comparable jobs or fewer competing employers, the scarcity of alternatives likely makes it harder for workers to leave their jobs, regardless of the pay at the firm. As such, we would expect a negative relationship between local job availability and departures in the cross section, and also a weaker

observed in the Current Population Survey (Sarah Flood and Warren, 2020). Table A.1 shows that the vast majority of warehouse workers continue on in warehouse work, though some become drivers, stock clerks, and packers. To construct warehouses workers' pay in their outside options in each MSA, we weight each wage in the Bureau of Labor Statistics (2022a) by these transition probabilities both overall for 2018−2021 and specifically in 2020−2021. This is analogous to the approach in Schubert et al. (2021).

[27] Worker satisfaction surveys ask employees "How likely are you to recommend [this firm] to a friend?" with options from 0 (not at all likely) to 10 (extremely likely). The "net promoter" score is calculated as the percent of promoters (who report 9 or 10) less the percent of "detractors" (who report 0−6).

impact of our pay change on departure rates.

We first use the number of surrounding jobs in warehousing as a proxy for job availability.[28] At any given wage, baseline departure rates are lower in areas with a below-average number of local warehousing jobs (gap = 3.2 pp, p-value < 0.0001). Further, in these areas, departures respond less to the pay increases caused by the firm-wide minimum wage, decreasing by 7.0 pp (p-value = 0.05) less per \$1/hour increase (Figure 4A).

We next use measures of local labor market concentration computed by Azar et al. (2022). The labor markets of this firm's warehouses are quite competitive, with Herfindahl-Hirschman Indices (HHIs) that are all well below levels that antitrust agencies consider to be concentrated.[29] Yet, even within this range, we find that warehouses in markets with higher HHIs — and thus fewer firms hiring — tend to have lower departure rates at a given wage than those with below average HHIs (gap = 4.9 pp, p-value < 0.0001). Furthermore, in more concentrated warehouse labor markets, departures decrease by 12.1 pp (p-value = 0.08) less in response to \$1/hour pay increases caused by the firm-wide minimum wage, as would be predicted by monopsony theory (Figure 4B).

We find the firm has some degree of wage-setting power, especially in certain labor markets. We next investigate its incentives to use this power to markdown wages.

III.B.2 Productivity

Increasing pay substantially increases productivity. When the firm implemented its voluntary minimum wage, the productivity in its lowest paid and least productive warehouses caught up to the productivity in the rest of its warehouses. The left plot of Figure 3C illustrates this pattern: around the implementation of the voluntary minimum wage, productivity sharply increases in the initially low-paid, intensely-treated warehouses (blue diamonds), marginally increases in the slightly treated warehouses (blue triangles), and remains approximately constant in the untreated warehouses (black cir-

[28] We divide the sample so that the treatment group has as many warehouses above as below the threshold. In the top half, the median location has 49,610 warehouse jobs, compared to 16,440 jobs in the bottom.

[29] We again divide the sample so the treatment group has an equal number of warehouses above and below the threshold. In the bottom half, the HHI ranges from 49 to 89 and in the top half from 93 to 402.

cles). The right plot illustrates these differences dynamically. In the three months before the imposition of the firm's minimum wage, productivity trended similarly across the firm's fifty-plus warehouses. When the firm-wide 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.

On average, a $1/hour or 5.5% increase in pay (from a base of $18.02/hour) increases boxes moved per hour by 0.39 or 5.7% (from a base of 6.80 boxes/hour; Column 5 of Table 3A). Comparing the 5.7% increase in productivity to the 5.5% increase in pay reveals a productivity elasticity of 1.02.[30] Since the productivity changes emerge immediately, estimates are similar in narrow bandwidths (Table A.2(b)). The productivity effects are also persistent, with similar effects over the full time series (Figure A.4(b)).

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.[31] Even at the lower end of our confidence intervals, the firm would mark down pay by only 4 cents on the dollar, compared to 10 cents without efficiency wage incentives.

We consider several robustness checks. First, our results are robust to controlling for changes in workers' local outside options and to defining pay in relative rather than absolute terms (Table A.4B). Second, we find similar results when including a sparser set of controls (Columns 2−3 of Table 3A) and when using discrete rather than continuous treatment indicators (Table A.3B). Third, we do not find significant effects in placebo checks around the same months in two other years (Table A.6B). Fourth, we perform a permutation test that randomly permutes the intensity of the treatment across warehouses: this exercise suggests a one-sided p-value of 0.12 (Figure A.5B). Finally, our results are not sensitive to excluding any particular warehouse (Figure A.6B).

[30] Using a log-log regression yields a statistically indistinguishable elasticity of 0.90.

[31] Note, we have: $\frac{w}{Y'(Ne)e} = \frac{2\epsilon_{\text{Departures},w} + \epsilon_{e,w}}{1 + 2\epsilon_{\text{Departures},w}} = \frac{2 \cdot 8.7 + 1.02}{1 + 2 \cdot 8.7} = 1.001$.

We decompose the productivity increase into two components: (i) time spent moving boxes and (ii) the number of boxes handled in an hour dedicated to moving. In the hub warehouses where these data are available, we find that 58.3% of the increase in productivity can be attributed to an increase in the share of hours that workers move boxes (Table 3B). This result is consistent with the warehouse operating more smoothly and thus requiring less time for rearranging inventory, meeting with managers, or training new employees. Despite spending more time moving boxes, injuries if anything decline (2.8 pp, 95% CI = [-8.8, 3.1] in Table A.9).[32]

Absenteeism. Absenteeism substantially decreases in warehouses where pay increases. Figure 3D illustrates this result: treated warehouses see an immediate decline in absenteeism relative to control warehouses, an effect that grows over time. A $1/hour increase in pay reduces the likelihood of absenteeism by 2.7 pp or 18.6% (Column 2 Table 3C). We likewise see a decrease of 1.1 absent hours per day, nearly halving the baseline mean of 2.4 hours (Table A.7). Given the seasonal variation in absenteeism (seen in the left plot of Figure 3D), being able to compare trends in the treated warehouses to those in the control warehouses is essential for isolating the effect of higher pay on absenteeism.

Individual-level data on absenteeism helps us distinguish between possible sources of improvement: are existing workers becoming more reliable, or is the workforce shifting toward more dependable employees? We find that both factors are at play. Specifically, about half of the reduction in unexcused absences (46−58% in Columns 3−4 of Table 3C; Figure 6A) remains when we include worker fixed effects and control for tenure. This result suggests that higher pay incentivizes improved behavior of existing workers, in addition to improving worker selection. Table A.8 shows direct evidence of improved selection. In more treated warehouses, new hires recruited after the policy change are more reliable (41% of the total effect) as are the existing workers who are retained (13%).

Heterogeneity by Labor Market Context. In thick labor markets with many comparable jobs and competing employers, pay may be essential for incentivizing effort, as workers

[32]Each year, the firm reports all injuries to the Occupational Health and Safety Administration (OSHA) in Form 300A, so the outcome is annual not weekly.

know they can quickly find a replacement job if they lose this one — but not necessarily one that pays as much. In contrast, in thinner markets, raising pay may do less to incentivize effort, as workers anticipate difficulty finding a replacement job. We find support for these predictions. Higher pay increases productivity more in markets with more local warehousing jobs (gap of 1.2 boxes per hour, p-value < 0.0001; Figure 4B) and in less concentrated labor markets (gap of 1.2 boxes per hour, p-value $= 0.03$; Figure 4C). This finding suggests that while efficiency-wage effects could dull the effects of monopsony power overall, it could amplify differences across labor markets. More concentrated labor markets may give firms both more monopsony power and also more incentive to use this power to markdown wages.

IV A Pre-Covid Pay Jump

To probe the robustness of our findings, we leverage an abrupt pay change that occurred in one of the firm's warehouses prior to the pandemic. While the warehouse was not randomly selected for a pay raise, the timing was plausibly exogenous: corporate executives approved the raise substantially after local managers initially requested it. We utilize this quasi-random timing in an interrupted time-series design.

IV.A Empirical Approach

In a couple of weeks, the warehouse's pay increased from \$16/hour to \$18/hour (Figure 5A). At the warehouse, the regional manager had long been concerned about high turnover and absenteeism, but it took corporate decision-makers months to get around to the proposal.[33] We study this discrete pay change in an interrupted time-series design:

$$\textbf{First Stage:} \qquad \$_t = \alpha_0 + \delta \text{Post}_t + \nu_t$$

$$\textbf{Second Stage:} \qquad \text{Y}_t = \alpha_1 + \beta_\$ \hat{\$}_t + \epsilon_t \tag{5}$$

[33] In the quarter before the pay raise, turnover was double that of the rest of the firm and absenteeism was 34% higher. This reflected intense local competition and grueling work handling large, unwieldy parcels.

where $\beta_{\$}$ is our parameter of interest. We also estimate a difference-in-differences (DiD) design, comparing changes in the treated warehouse to those in the firm's other warehouses, which did not experience discrete pay changes during this period:

$$\textbf{First Stage:} \qquad \$_{wt} = \delta \text{Post in Treated Warehouse}_t + \mu_w + \mu_t + v_{wt}$$

$$\textbf{Second Stage:} \qquad Y_{wt} = \beta_{\$}^{\text{DiD}} \hat{\$}_{wt} + \mu_w + \mu_t + u_{wt} \tag{6}$$

where μ_w represent warehouse fixed effects and μ_t represent week fixed effects.

For $\beta_{\$}$ to capture the causal effect of higher pay, there must be no other coincident changes in the treated warehouse (and for $\beta_{\$}^{\text{DiD}}$ to be causal, any such changes must parallel those in the rest of the firm). The warehouse's manager confirmed that the pay change did not coincide with changes in the nature of the work nor conditions in the local labor market. We independently verified that there were no contemporaneous changes in hiring by nearby rival firms. Additionally, in placebo checks, we show that there were no similar changes in the firm's other warehouses that were either (i) operating in the same labor market or (ii) handling similar parcels.[34] As with the firm-wide minimum wage, the pay increase resulted in a rightward shift of the whole pay distribution (Figure A.8) and so did not affect dynamic incentives nor feelings of (in)equality.

IV.B Effects of the Pre-Covid Pay Jump

IV.B.1 Departures.

After pay increases, workers depart the warehouse at a lower rate, as illustrated in Figure 5B. Before the pay jump, 19.6 workers per 100 left per month; afterwards, this rate fell to 11.9. Since paying 12.3% ($2/hour) more decreases departures by 39.4% (7.7 from a base of 19.6), this implies a turnover elasticity of 3.22. Using a difference-in-differences design implies a similar turnover elasticity of 4.83 (Column 4 of Table 4A). As with the voluntary firm minimum wage, the overall effect on turnover is driven by voluntary quits

[34]The treated warehouse and two other "twin" warehouses handle large parcels the size of refrigerators or sofas. Other warehouses handle parcels the size of toasters or tea towels. The twin warehouses are not geographically proximate since they each handle different regions.

(Columns 5−6), with no impact on being fired (Column 7−8).[35]

We use two placebo checks to help validate the design. First, as shown in Figure A.9A, departures did not significantly change in two untreated warehouses within a 15-minute drive of the treated warehouse. This null result suggests that the decrease in turnover is not driven by spurious changes in the local labor market, since such changes would also affect these other local warehouses. Second, Figure A.9C shows that if anything departures slightly increased in "twin" warehouses handling similarly large parcels, suggesting that the reduction in turnover is not driven by improvements in the firm's handling of large parcels.

Similar to the results from the firm-wide minimum wage, these departure effects indicate a large but finite labor-supply elasticity, giving the firm some degree of wage-setting power. On its own, this elasticity would suggest that the firm would set a wage-markdown of 13 cents on the dollar (95% CI = [8, 19]).

IV.B.2 Productivity.

After pay discretely increased, productivity also rose. A $2/hour increase in pay increases productivity by 0.78 boxes per hour or 16.1% (Figure 5C). Comparing the 16.1% increase in productivity to the 12.3% increase in pay yields an elasticity of 1.31, which is quite similar to the elasticity of 1.02 measured for the firm-wide minimum wage design.

We find similar estimates using a difference-in-differences design that compares the change in the focal treated warehouse to the change in the firm's other warehouses (Column 2 of Table 4B). In placebo checks, we do not see similar changes in productivity in two, untreated nearby warehouses (Figure A.9B) or in two, untreated "twin" warehouses that handle similarly large parcels and so would be similarly impacted by any demand shocks or technological changes at the firm (Figure A.9D).

The increase in boxes moved primarily stems from workers moving boxes faster rather than spending more time moving boxes (Columns 3−4 of Table 4B). This different source of the productivity change relative to what we found for the firm-wide minimum

[35] There are no other departures such as layoffs or no-shows of new hires.

wage may 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.

IV.B.3 Absenteeism.

After pay increases, the warehouse may operate more efficiently in part because workers are more likely to show up. Figure 5D illustrates the decline in absenteeism around the $2 increase in pay. Once pay increases, absenteeism declines by 3.2 pp or -20.8%. This analysis focuses on non-holidays to reduce seasonal variation in absenteeism (Column 2 of Table 4C). Results are similar, albeit slightly attenuated, if we include holidays (Column 1), since Thanksgiving, Christmas, and New Year's are all in the post-period. Our DiD design produces similar estimates regardless of whether we include holidays (Columns 3−4), since this nets out seasonal variation by comparing changes in the treated warehouse to contemporaneous changes in the control warehouses.

Strikingly, the estimated reduction in absenteeism is almost identical when we include worker fixed effects and further account for worker tenure (Column 5−6). This result indicates that existing workers became more reliable once they are paid more.

IV.B.4 Implications.

The results from the discrete pay jump align with those from the firm-wide minimum wage. Higher pay boosts productivity, partly by motivating existing workers to become more reliable. Incorporating this productivity effect into the optimal wage-setting equation suggests that the firm should not mark down wages at all, in contrast to the optimal 13 cent-per-dollar markdown in a world with solely search frictions.

V Mechanisms

To better understand the payoffs of higher pay, we present additional evidence from our Fortune 500 firm and from an entirely distinct Fortune 100 staffing agency, which places warehouse workers in thousands of companies.

V.A Incentive Effects

We found that higher pay motivated existing workers to become more reliable in both of the designs discussed in Sections III−IV. Figure 6A summarizes these findings, showing that about half of the total reduction in absenteeism persists within workers in the firm-wide minimum wage design, while nearly all of it persists in the pay jump design. Since attendance is a basic measure of effort, these results suggest that workers put in more effort once their job becomes better paid and, thus, more valuable to them.

To move beyond attendance as a measure of effort, we collected data from a Fortune 100 staffing agency that placed 93,176 warehouse workers in a total of 310,080 placements between 2016 and 2018 (Table A.10). Firms rate the performance of workers placed with them after each assignment. Because we often observe the same worker placed in several comparable warehouse jobs with different wages, the staffing agency data offer another valuable opportunity to diagnose the incentive effects of higher pay: do workers become more reliable and more effective when their job pays them more?

In Figure 6B, we first compare outcomes in warehousing jobs in the same labor market that differ in their pay (in light blue) and then see how much of these differences persist within worker (in dark blue, see Appendix C for additional detail on the design). Like in the focal Fortune 500 firm, we find that workers become more reliable when they are paid more: jobs that pay $1/hour more are 1.1 pp (9.5%) less likely to end early because of attendance problems, 35.9% of which is driven by changes within worker. These patterns also extend to more subjective and holistic evaluations of worker effort. Workers are 1.2 pp (10.9%) more likely to get an "excellent" performance evaluation if the job pays $1/hour more. When we include worker fixed effects, 49.0% of this difference persists.[36] Poor evaluations and positive ones also show improved behavior. Taken together, these findings provide evidence that the behavioral responses to higher pay that we observe in the focal firm generalize to other firms and to other metrics of effort.

[36] The results on attendance and excellent evaluations are distinct from one another: the results for excellent evaluations are almost identical when excluding jobs that end because of attendance issues.

V.B Experience Effects

Experienced workers seem to be highly valuable to the firm. Figure 7 shows that higher average tenure in a warehouse is tied to greater productivity, with each additional month of average tenure associated with 1.7% higher productivity. This is particularly true in the more complex hub warehouses, where managers compared learning to operate the machinery to learning to drive on a highway. The high returns to worker tenure may contribute to the stark productivity increases after the opening of new warehouses, particularly larger ones, as workers gain experience (Figure A.11).[37]

We can gauge the productivity increase due to tenure. Taking the cross-sectional correlation between tenure and productivity at face value, higher pay's impact on worker tenure would increase productivity by 1.5%, which could account for 27% of the productivity increase.[38] This pattern suggests that the returns to worker experience may be a key component in reducing firms' incentive to mark down wages.[39]

VI Returns to Raising Pay & The Profit-Maximizing Wage

We now consider how our results would impact optimal wage-setting decisions. To do this, we consider the possibility that the firm rationally incorporates the productivity effects of higher pay or instead partially or fully ignores these returns.

Suppose the firm only acknowledges labor-supply frictions facing workers. The firm then recognizes that it has wage-setting power but does not recognize its own efficiency-wage incentives. In this case, the firm would set a wage-markdown of 5 cents on the dollar, paying \$0.95 per marginal dollar of value based on the estimates from the firm-wide minimum wage (leftmost estimates in Figure A.12A).[40] This narrow view suggests a neg-

[37] The returns to tenure may grow as supervisors can invest more in existing workers when training fewer new hires. An internal memo noted that lower turnover lets supervisors focus on "upskilling" their teams.

[38] A \$1/hour increase leads to a 1.0 month increase in tenure around the firm-wide minimum wage (Figure A.7B). One month of additional average tenure is associated with 0.10 additional boxes moved per hour in the warehouse. Taken together this would increase boxes moved by 0.10 boxes per hour or 1.5%.

[39] A firm could also set wages with an eye toward increasing tenure. We extend our model to two periods in Appendix A and reach similar predictions as our core model.

[40] Estimates from the wage jump suggest a more extreme 13 cent markdown (95% CI = [8, 19]).

ative return on investment from paying workers \$1/hour more in wages plus \$0.30/hour in taxes and benefits (leftmost estimates in Figure A.12B).

In reality, the firm we study does recognize some of its efficiency-wage incentives but takes a fairly narrow view of them. The firm's finance team estimates that the firm spends \$1,950 for each new hire for training, drug testing, badges, and other administrative overhead. When the firm acknowledges this part of efficiency-wage incentives, then the optimal markdown is nearly halved and is only 3 cents (95% CI = [-2, 8]).[41] Since increasing pay reduces turnover costs by only \$0.36 per dollar spent, the investment in higher pay would still have a negative net return (middle estimate in Figure A.12B).[42]

If the firm fully recognizes the relationship between pay and productivity, then the perceived profit-maximizing wage would be much closer to the workers' marginal revenue. In fact, when we incorporate both the increases in productivity (as in Section III) and the savings in turnover costs (as above), our point estimate indicates that workers would be paid their marginal revenue (the rightmost estimate in Figure A.12A). A return-on-investment calculation would likewise recognize that the costs of higher wages would be fully defrayed.[43] Yet internal documents from the firm indicate that the relationship between pay and productivity is not in the finance team's benefit-cost calculations.[44]

Consistent with the pay raise being profitable, the firm's stock price increased by 13% in the month after the firm-wide minimum wage, and 11% relative to other comparable firms' (p-value = 0.028).[45] A permutation test indicates that it is unlikely that this stock-price change occurred by chance: around placebo dates, only 2.7% (1.4%) of the firm's

[41] In this case, estimates from the wage jump suggest a 8 cent markdown (95% CI = [2, 14]).

[42] We find that increasing pay \$1/hour causes 4.1 fewer workers per 100 to leave each month, saving the firm \$7,915 per 100 workers (4.1 fewer departures · \$1,950 cost per new recruit). To realize this change, the firm must pay 100 workers \$1/hour more in wages and \$0.30/hour more in taxes for twenty-one, eight-hour days, at a total cost of \$21,840 (100 workers · 168 hours/month · \$1.30/hour). Thus, the firm's gross return would only be 0.36 (\$7,915/\$21,840) and its net cost would be \$0.64.

[43] Based on the hourly pay in the treated warehouses, in the two quarters before the pay jump, the firm spent \$3.42 per box moved (\$17.87 in hourly wages · 1.30 in taxes / 6.8 boxes moved per person-hour). Since higher pay increased productivity by 0.39 boxes per person-hour, the gross return on a \$1 pay increase, which costs the firm \$1.30/hour in wages and taxes, is \$1.68/hour.

[44] Some said managers believed higher pay could boost productivity of salaried but not frontline workers.

[45] We define comparable firms as other top grossing firms in the industry. The result is similar if we focus on logistics firms and if we use opening, closing, low, or high prices and bandwidths of $1-6$ months.

absolute (relative) stock-price changes are as large (Figure A.10).

Estimate Limitations. Our estimates of the returns to higher pay are incomplete. They do not include "shrink" — an industry term for theft — which likely decreases as higher pay reduces financial need and increases motivation to keep the job. They also omit damages to products , where higher pay's effect is ambiguous — more experienced workers may damage fewer products, but increased effort could lead to congestion and fatigue.

These estimates reflect the consequences of a change in pay from a particular starting level. When starting at higher pay levels, further increasing pay may have less pronounced effects. Nevertheless, our results are striking because they indicate that, at least locally, the firm had no incentive to mark down pay.

VII Conclusion

This paper uses a Fortune 500 firm's voluntary minimum wage to examine how higher pay affects worker departures, absenteeism, and productivity. We find that a one percent increase in pay decreases worker departures from the firm by nearly ten percent. This elasticity is much higher than other estimates in the literature, consistent with the thick warehouse markets around logistics hubs. Nevertheless, this elasticity is finite, indicating that the firm has some wage-setting power. If the firm focused only on its monopsony power, we estimate that the firm would set wages at 5% below workers' marginal revenue.

However, efficiency-wage effects may curb the firm's incentive to use its wage-setting power to mark down wages. We find that a one percent increase in wages leads to a one percent increase in boxes moved per worker-hour. Recognizing the link between pay and productivity suggests that firms like this one have limited incentive to act on their market power by reducing wages. This countervailing force of efficiency wages may be particularly pertinent amidst declining union power,[46] increasing automation, rising trade, and other threats to workers' bargaining power.

[46] While unionization rates increased in 2020, they have continued to fall since then despite some successful, high-profile unionization efforts (U.S. Bureau of Labor Statistics, 2023).

Our emphasis on the interplay between efficiency wages and monopsony power may help explain long-standing puzzles in the literature and important labor-market trends. First, current estimates of monopsony power imply an implausibly low labor share of income. The offsetting force of efficiency-wage incentives is one way to reconcile pervasive monopsony power with reasonable labor shares of income.

Second, changes in efficiency wages may help explain secular declines in real-wages among less-educated workers. For example, improved monitoring technologies may allow firms to shift from the carrot of higher pay toward the stick of termination.[47] In addition, technologies that simplify tasks may substitute for workers' firm-specific human capital — and thereby lessen the returns of retaining workers by raising pay. Indeed, the market may direct technological changes exactly at those that would reduce efficiency wages (Acemoglu, 2023). Relatedly, greater churn in low-wage jobs from non-wage factors — due to, for example, substance-use problems or unstable child-care arrangements — may limit the incentive effects of higher pay, as workers know their time in even a high-paying job may be fleeting. Such changes could mean that efficiency wages have become weaker counterbalances to firms' monopsony power in recent years. Yet despite such changes, our paper suggests that efficiency-wage theory has continued relevance.

Our paper also underscores the value of opening the black box of firm wage-setting decisions in a world in which labor market frictions mean firms are not entirely wage-takers. Focusing on one firm gave us access to objective productivity measures that map directly to the firm's bottom line. Our case-study approach also allowed us to glean insight into which factors do and do not enter into the firm's wage-setting discussions. The firm we studied had overlooked the productivity effects of higher pay for its front-line workers. If many firms do not incorporate efficiency wage incentives when setting wages — especially for lower-wage workers — then they may use their monopsony power more, which could contribute to persistent or widening inequality.

[47] The firm that we study makes relatively limited use of monitoring and automation technologies used by some other warehousing firms because of the nature of the firm's parcels. It is possible that efficiency-wage effects would be less potent in other firms that can use these technologies.

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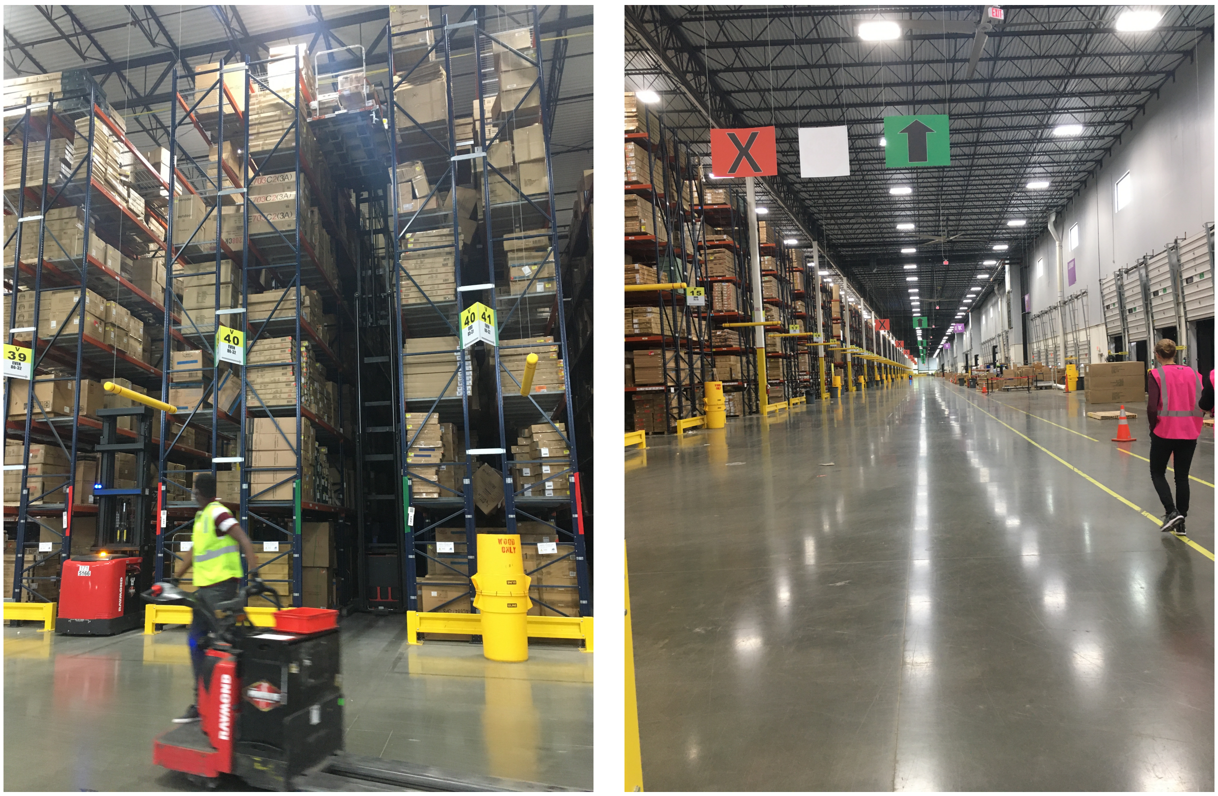
Figures and Tables

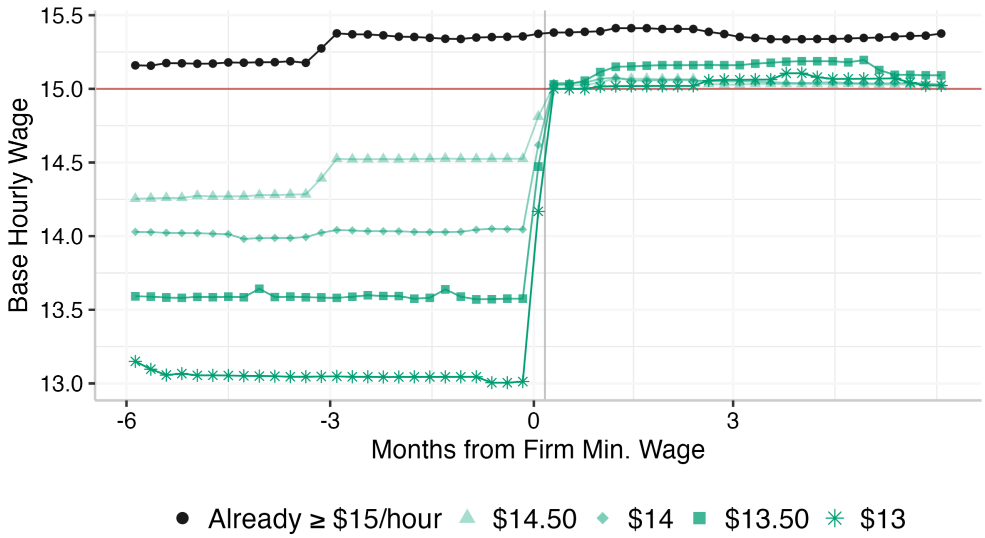
Figure 1: Photos of Warehouses



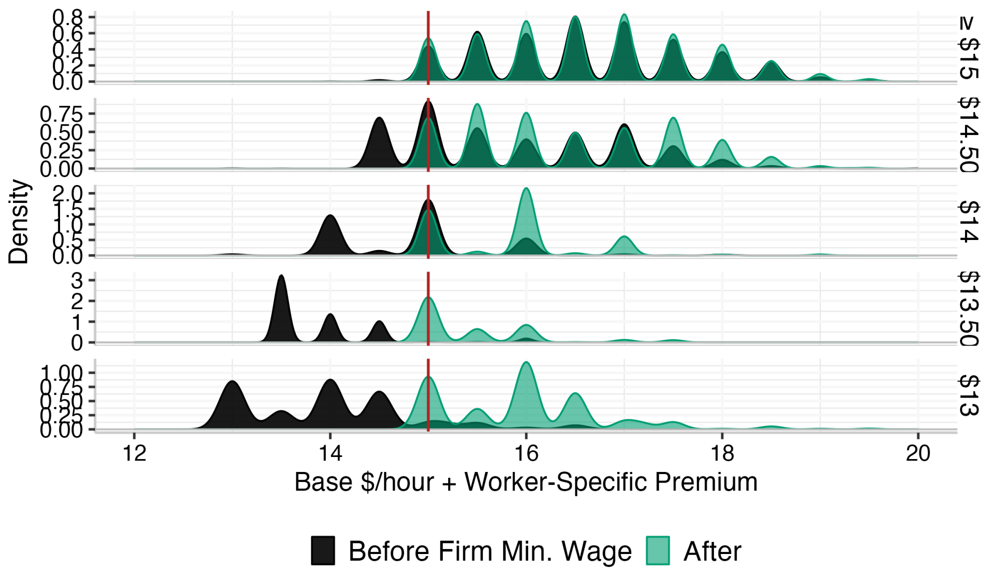
Note: These images show an example of a hub warehouse from the authors' visit. In the left photo, the vehicle in the foreground is returning to the loading dock after having put away boxes; the vehicles in aisles 39 and 40 are putting away and retrieving boxes. The vehicle in aisle 40 is extended to the highest elevation where boxes are loaded onto racks 40 feet off the ground. 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. Coauthor included for scale.

Figure 2: Implementation of a Voluntary Firm-Wide Minimum Wage

Panel A: Average Base Hourly Wages by Initial Pay Over Time

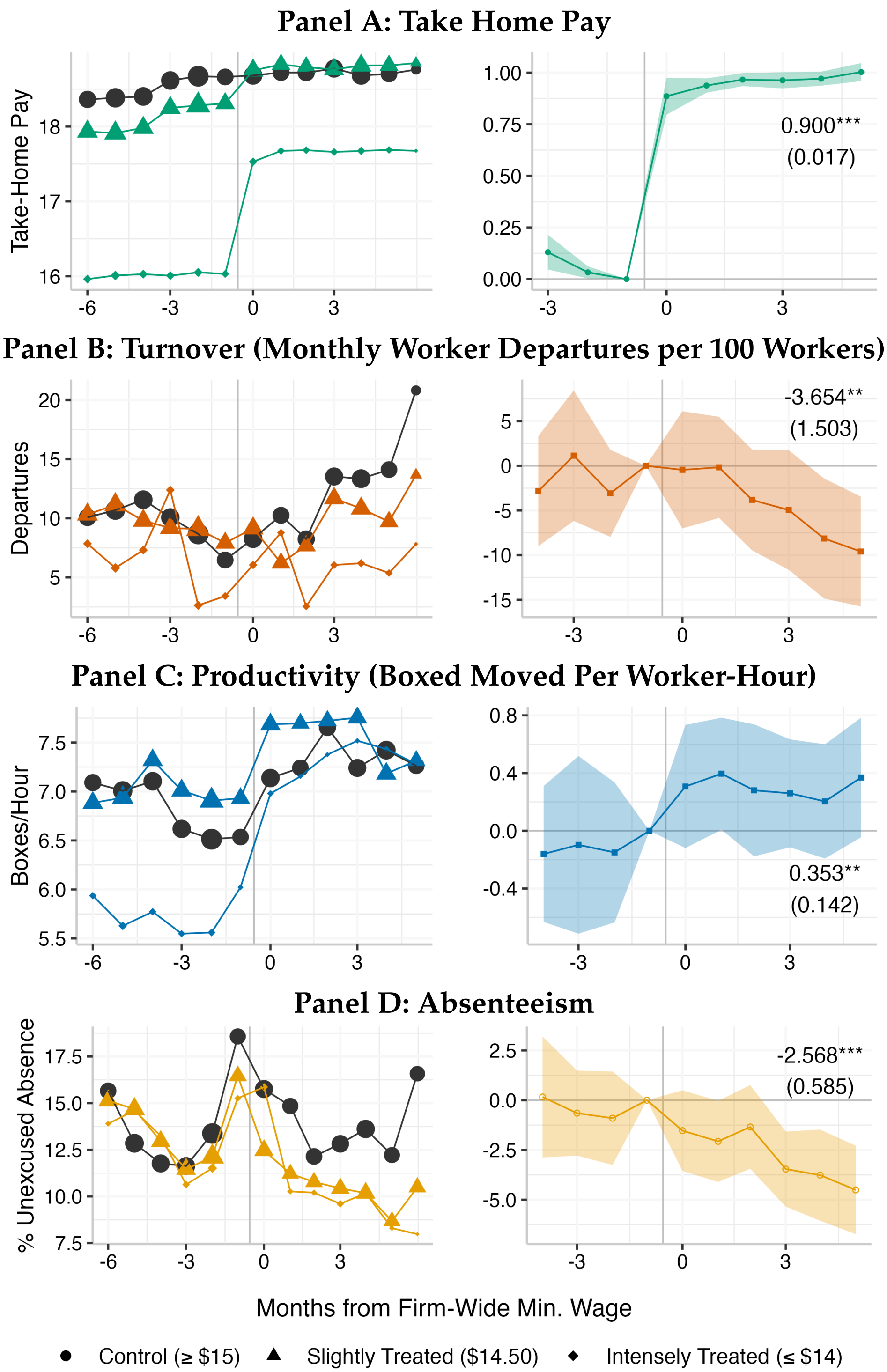


Panel B: Density of Pay Distributions Before and After



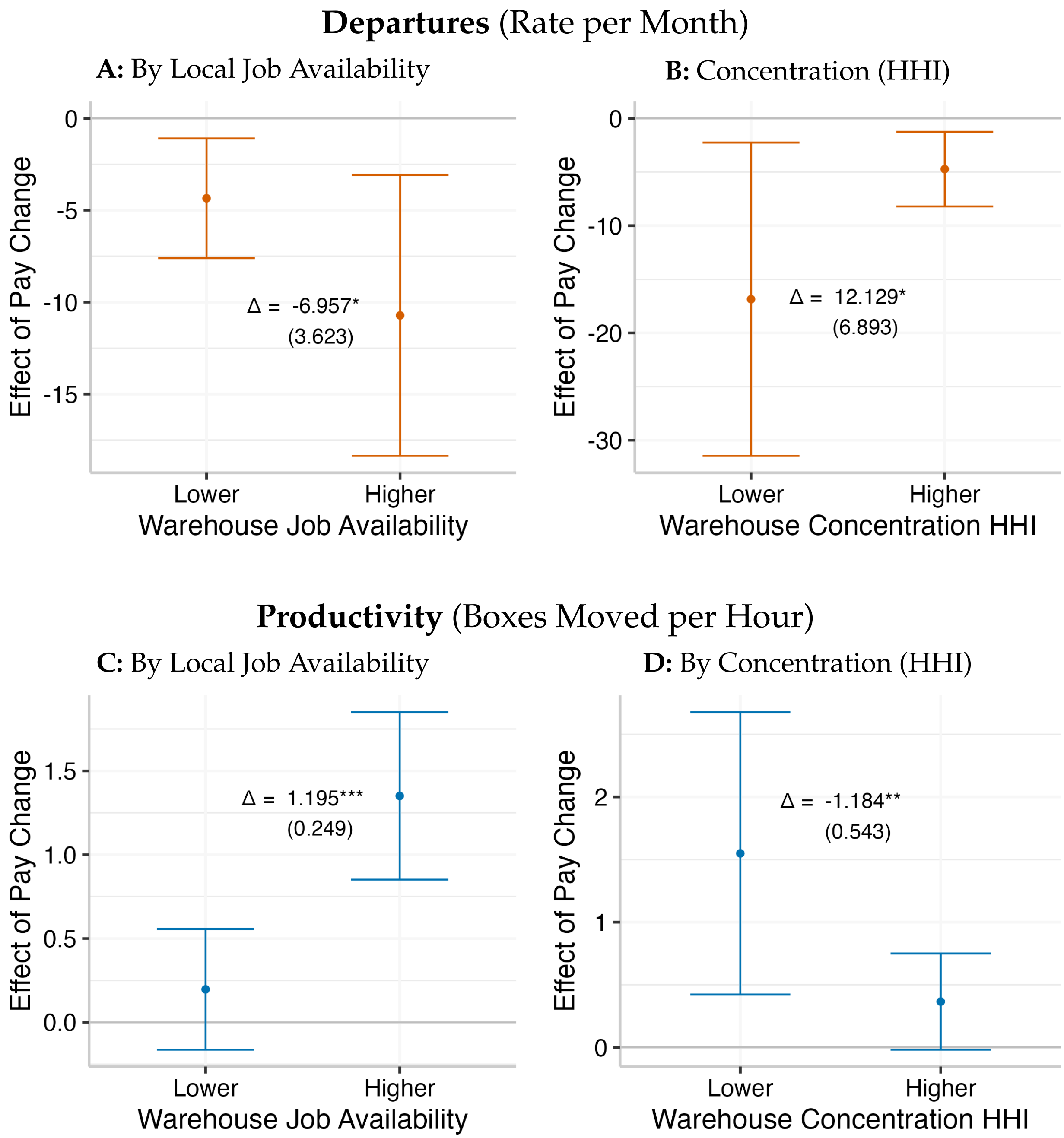
Note: This figure illustrates the differential impacts of the firm's voluntary minimum wage across warehouses. Panel A shows weekly wages around the implementation of the firm-wide minimum wage for warehouses with different base wages prior to its implementation. Warehouses with base pay above $15/hour 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 firm-wide minimum wage across all its warehouses. The exact timing of the firm-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). The firm-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



Note: This figure shows how pay, departures, productivity, and absenteeism evolve around the implementation of a voluntary firm-wide minimum wage. The dates are obfuscated to preserve the firm's anonymity, with 0 denoting implementation. The left plots show raw averages for warehouses that are untreated (black circles), slightly treated (triangles), and intensely treated (diamonds). The right plots present reduced-form estimates from a dynamic version of Equation 3, which control for COVID-19 death rates, state minimum wages, and region-by-week fixed effects (that also interact with warehouse-type for productivity). The annotated coefficients reflect the pooled, reduced-form estimates.

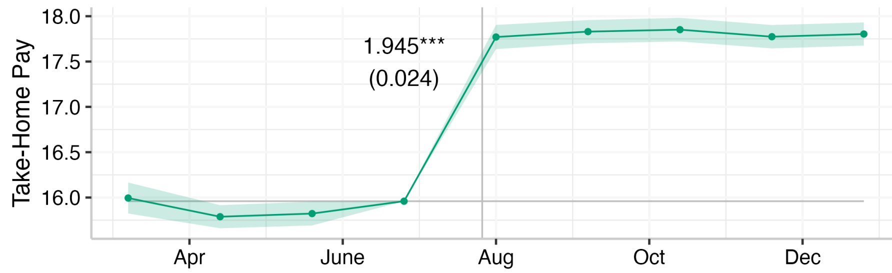
Figure 4: Effects of Higher Pay by Labor Market Context



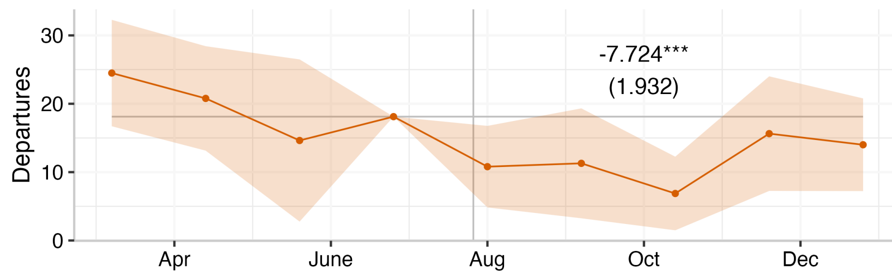
Note: This figure shows heterogeneous effects of pay on (A−B) monthly departure rates and (C−D) productivity (as measured by boxes moved per worker hour). Panels A and C split by the availability of other warehouse jobs in the metropolitan statistical area. Panels B and D split by the local labor-market concentration as measured by Azar et al. (2022)'s Herfindahl-Hirschman Indices (HHIs). For each, we divide the sample so that the treatment group has an equal number of locations above and below the threshold. We estimate an interacted version of the IV regression in Equation 3. For Panels A and C, in the top part of the sample, the median location has about 50,260 warehouse jobs (constituting about 2.2% of the available jobs), compared to 16,900 jobs (constituting about 1.75%) in the bottom. For Panels B and D, in the top segment of our sample, the median location has an HHI of 154, compared to 72 in the bottom.

Figure 5: A Single Warehouse's Pay Jump Before COVID-19

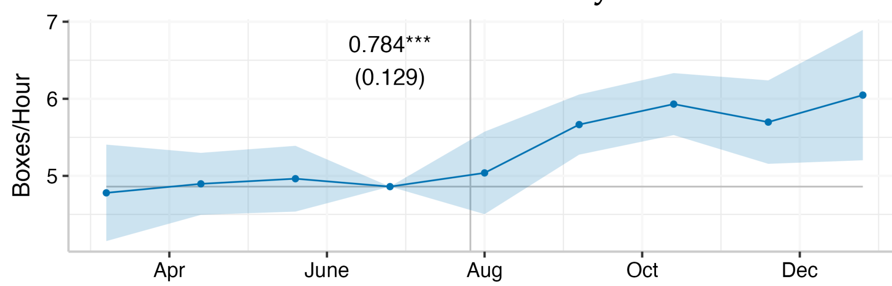
Panel A: Take-Home Pay



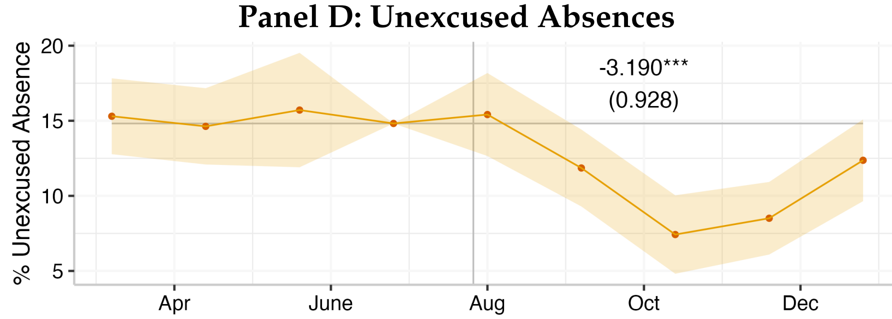
Panel B: Monthly Departures per 100 Workers



Panel C: Productivity

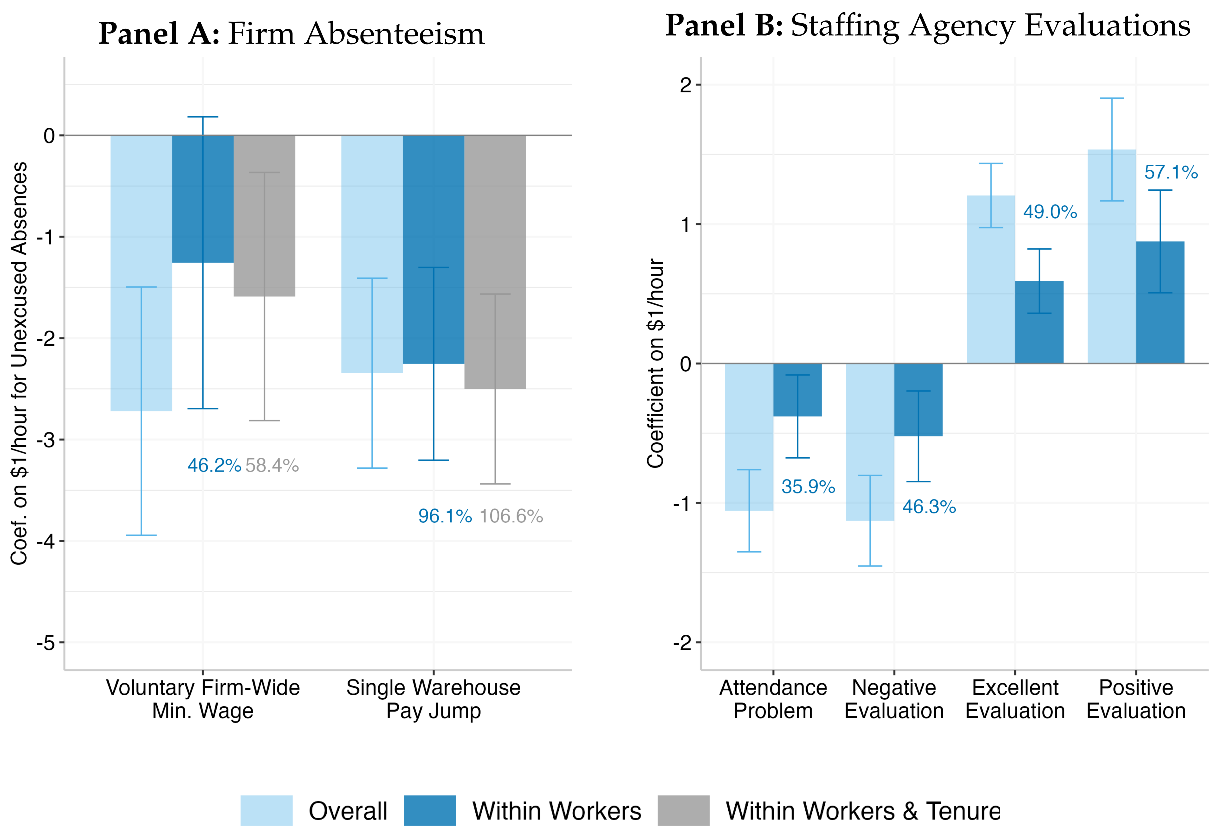


Panel D: Unexcused Absences



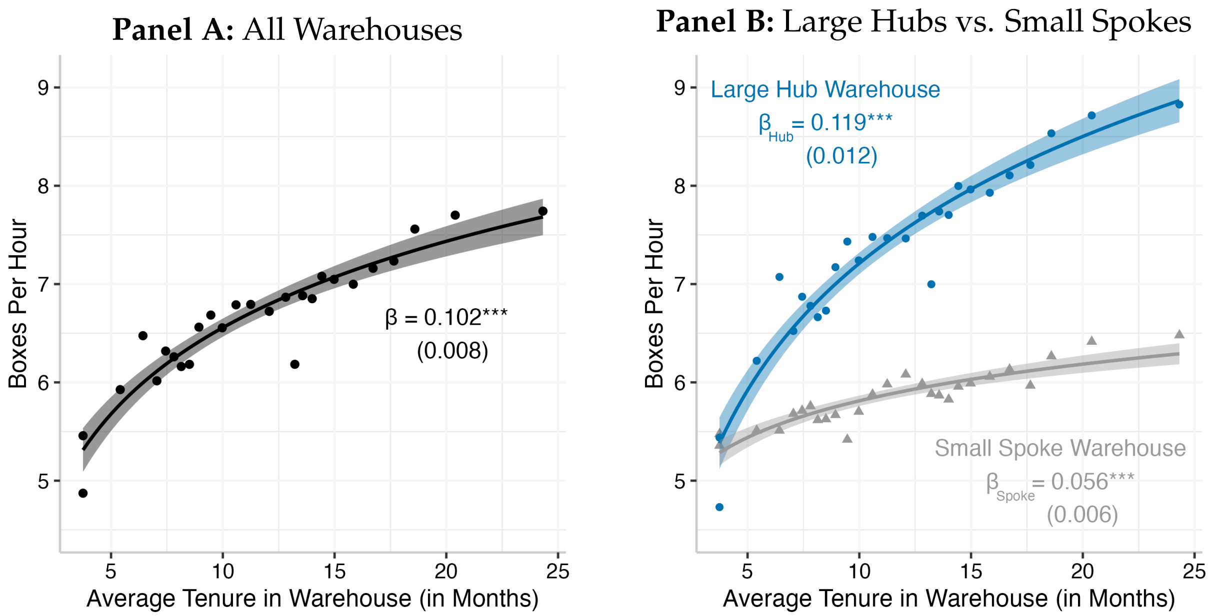
Note: This figure illustrates the changes in (A) pay, (B) departures, (C) productivity, and (D) absenteeism around a discrete pay change at a single warehouse, where the timing was administratively delayed. Each panel plots reduced-form changes within the treated warehouse in the six months before and after the August pay bump. The annotated coefficients reflect these reduced-form estimates. Table 4 shows the corresponding two-stage least squares estimates, which rescale by the first-stage change in pay shown in Panel A. In Panel D, unexcused absences exclude holiday weeks (Christmas, New Years, Thanksgiving, and July 4th). Results are similar when including these weeks and adjusting for seasonality using a difference-in-differences (see Table 4C). Shaded areas display 95% confidence intervals. *p<0.1; **p<0.05; ***p<0.01.

Figure 6: Effort Changes within Worker



Note: This figure investigates whether higher pay incentivizes workers to work harder, using different proxies for worker effort. Panel A focuses on the effect of \$1/hour higher pay on absenteeism at the firm, first overall and then with worker fixed effects and tenure controls. The first set of coefficients focus on the firm-wide minimum wage design (as in Columns 2−4 of Table 3C). The second set focus on the pay jump design (as in Columns 3−6 of Table 4C). Panel B uses a different dataset from a staffing agency where we observe 93,176 warehouse workers who are placed on multiple jobs (see Table A.10 for more descriptive statistics). The outcomes are evaluations of workers' performance on these assignments. The coefficients show the estimated relationship between pay and evaluations within local labor markets, both overall and with worker fixed effects (see Appendix C for more details on the specifications and Table A.11 for the regression table). The annotations report the percent of the overall effect that persists within worker. The error bars represent 95% confidence intervals.

Figure 7: Returns to Tenure



Note: This figure investigates the relationship between the average tenure in a warehouse and its productivity. Panel B differentiates between small spoke warehouses and large hub warehouses, which have more complex inventory systems and more capital equipment. The points reflect 25 quantiles. The log fit line includes an error band for the 95% CI. The annotated coefficients reflect the OLS relationship. *p<0.1; **p<0.05; ***p<0.01.

Table 1: Summarizing a Fortune 500 Firm's Warehouses

	All	Around Voluntary Firm \$15/hour Minimum Wage	Around Discrete Pre-Covid Pay Increase
A. Wages			
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
MSA Avg. Warehouse Wage \$/hour	16.39	17.11	15.22
B. Terminations			
Monthly Turnover/100 Workers	11.24	9.87	14.74
Monthly Quits/100 Workers	8.10	7.84	10.95
Monthly Fires/100 Workers	1.32	0.64	3.07
Tenure at Retailer (in Months)	12.31	15.59	8.75
MSA Separations/100 Workers	3.31	3.50	3.30
C. Productivity			
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
D. Absenteeism			
% Unexcused Absent	12.53%	13.79%	13.97%
Unexcused Absent Hours/Day	1.80	2.10	2.54
E. Labor Market Context			
% MSA Warehouse Employment at Firm	1.87%	2.06%	4.36%
% MSA Warehouse Establishments	1.15%	1.10%	2.85%
Historical HHI	109.05	110.58	82.05
Local Unemployment Rate	6.00	7.68	3.71
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. Column 1 displays information about all US-based warehouses at the firm from 2018-2021. Column 2 limits to the six months on either side of the firm's introduction of a \$15/hour voluntary firm minimum wage. Column 3 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. Background data on the metropolitan statistical areas (MSAs) in which the firm operates come from a variety of publicly available data sources described in Appendix B.

Table 2: Labor Supply Response to a Voluntary Firm Minimum Wage

Panel A: All Departures

	$/Hr	Monthly Departures per 100 Workers			
Gap from $15/hr x Post	0.900***	−2.132**	−4.300***	−3.654**	
	(0.017)	(1.021)	(1.589)	(1.503)	
$\widehat{\Delta\$/\text{hour}}$					−4.059**
					(1.669)
Elasticity		-5.07	-10.22	-8.69	-8.69
		(2.43)	(3.40)	(3.22)	(3.57)
Base Mean	$18.02	8.42	8.42	8.42	8.42
Adjusted R^2	0.990	0.095	0.116	0.121	0.124

Panel B: Quits

	$/Hr	Monthly Quits per 100 Workers			
Gap from $15/hr x Post	0.900***	−1.936**	−3.276**	−2.991**	
	(0.017)	(0.867)	(1.317)	(1.282)	
$\widehat{\Delta\$/\text{hour}}$					−3.322**
					(1.425)
Base Mean	$18.02	6.22	6.22	6.22	6.22
Adjusted R^2	0.990	0.096	0.093	0.095	0.098

Panel C: Fires

	$/Hr	Monthly Fires per 100 Workers			
Gap from $15/hr x Post	0.900***	−0.023	0.122	0.146	
	(0.017)	(0.223)	(0.333)	(0.341)	
$\widehat{\Delta\$/\text{hour}}$					0.162
					(0.378)
Base Mean	$18.02	0.53	0.53	0.53	0.53
Adjusted R^2	0.99	0.024	0.120	0.12	0.12
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 firm-wide minimum wage on worker departures from the firm. Column 1 shows the first stage, which differs from one because more affected warehouses were less likely to further increase pay in the subsequent six months. Columns 2-4 estimate the reduced-form relationship between departures and the exposure to the firm-wide minimum wage using Equation 3, progressively adding controls. Column 5 scales the reduced-form by the first-stage in a two-stage-least-squares regression. The elasticities reported in Panel A use Equation 4. 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 firm-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	0.916*** (0.016)	0.269** (0.105)	0.352** (0.141)	0.353** (0.142)	
$\widehat{\Delta\$/\text{hour}}$					0.385** (0.155)
Elasticity		0.78 (0.30)	1.02 (0.37)	1.02 (0.38)	1.02 (0.41)
Regional FEs	✓		✓	✓	✓
Controls	✓			✓	✓
First Stage F	—	—	—	—	3,314
Base Mean	$18.02	6.8	6.8	6.8	6.8
# Warehouses	4X	4X	4X	4X	4X
# Workers	4,XXX	4,XXX	4,XXX	4,XXX	4,XXX
Adjusted R^2	0.992	0.825	0.859	0.858	0.876

Panel B: Dimensions of Productivity Response in Hubs

	$/Hr	$\frac{\text{Boxes}}{\text{Hour}}$	$\frac{\text{Moving Hrs}}{\text{Total Hrs}}$	$\frac{\text{Boxes}}{\text{Moving Hour}}$
Gap from $15/hour x Post	0.939*** (0.031)			
$\widehat{\Delta\$/\text{hour}}$		0.697*** (0.211)	0.035*** (0.011)	0.419 (0.346)
Contribution			58.3%	41.7%
First Stage F	—	1,801	1,801	1,801
Base Mean	$18.22	7.1	0.64	10.7
# Warehouses	1X	1X	1X	1X
# Workers	1,XXX	1,XXX	1,XXX	1,XXX

Panel C: Absenteeism Response Overall and Within Worker

	$/Hr	% Unexcused Absent Day		
Gap from $15/hr x Post	0.944*** (0.011)			
$\widehat{\Delta\$/\text{hour}}$		−2.720*** (0.625)	−1.256* (0.734)	−1.589** (0.731)
Elasticity		-3.34 (0.77)	-1.54 (0.90)	-1.95 (0.90)
Individual FE			✓	✓
Tenure Quartic				✓
% Effect within Worker			46.2%	58.4%
Base Mean	$18.02	14.66	14.66	14.66
# Warehouses	4X	4X	4X	4X
# Workers	4,XXX	4,XXX	4,XXX	4,XXX

Note: This table evaluates the productivity effects of the voluntary firm-wide minimum wage. Panel A focuses on the average boxes moved per worker hour in each warehouse-week. Panel B decomposes productivity into its component parts, which are tracked weekly in the large hub warehouses. Panel C analyzes absenteeism at the worker-day level. All specifications estimate Equation 3. Regional fixed effects are firm-defined logistics region interacted with warehouse type and week; controls are county COVID-19 death rates and state minimum wages. Every column in Panels B−C includes both. In Panel A, Columns 2−4 probe robustness to varying controls. Every specification uses a three-month pre-period and six-month post-period. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 4: Responses to a Single Warehouse's Pay Jump Before COVID-19

Panel A: Labor Supply Responses

	First Stage		Monthly Turnover		Quits		Fires	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post in Focal Warehouse	1.945***	1.733***						
	(0.024)	(0.036)						
$\widehat{\$/\text{hour}}$			−3.971***	−5.962***	−3.247***	−4.700***	0.045	−0.252
			(1.017)	(1.116)	(0.838)	(0.907)	(0.467)	(0.482)
Elasticity			-3.22	-4.83	-3.42	-4.95	0.25	-1.37
			(0.82)	(0.90)	(0.88)	(0.95)	(2.55)	(2.63)
DiD (Warehouse & Week FE)		✓		✓		✓		✓
First Stage F	—	—	6,537	2,252	6,537	2,252	6,537	2,252
Base Mean	$15.86	$15.86	19.58	19.58	15.07	15.07	2.90	2.90
# Warehouses	1	5X	1	5X	1	5X	1	5X
# Workers	6XX	6,XXX	6XX	6,XXX	6XX	6,XXX	6XX	6,XXX

Panel B: Productivity Responses

	Boxes/Hour		Moving/Total Hrs		Boxes/Moving Hr	
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{\$/\text{hour}}$	0.403***	0.450***	0.002	−0.004	0.479***	0.640***
	(0.067)	(0.103)	(0.008)	(0.008)	(0.113)	(0.179)
Elasticity	1.31	1.47	0.04	-0.09	1.02	1.36
	(0.22)	(0.34)	(0.18)	(0.18)	(0.24)	(0.38)
DiD (Warehouse & Week FE)		✓		✓		✓
First Stage F	6,187	2,111	6,187	2,111	6,187	2,111
Base Mean	4.88	4.88	0.66	0.66	7.46	7.46
# Warehouses	1	5X	1	5X	1	5X
# Workers	6XX	6,XXX	6XX	6,XXX	6XX	6,XXX

Panel C: Absenteeism

	% Unexcused Absence					
	Event Study		Diff-in-Diff		With Worker FE	
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{\$/\text{hour}}$	−1.116*	−1.577***	−2.345***	−2.004***	−2.253***	−2.501***
	(0.628)	(0.459)	(0.478)	(0.457)	(0.485)	(0.486)
Elasticity	-1.14	-1.61	-2.4	-2.05	-2.3	-2.56
	(0.64)	(0.47)	(0.49)	(0.47)	(0.50)	(0.50)
Excluding Holiday Weeks		✓		✓		
DiD (Warehouse & Week FE)			✓	✓	✓	✓
Individual FE					✓	✓
Tenure Quartic						✓
% Effect within Worker					96.1%	106.6%
First Stage F	—	—	9,125	8,577	2,742	7,041
Base Mean	15.51%	15.51%	15.51%	15.51%	15.51%	15.51%
# Warehouses	1	1	5X	5X	5X	5X
# Workers	6XX	6XX	6,XXX	6,XXX	6,XXX	6,XXX

Note: This table shows estimates of higher pay on (A) departures, (B) productivity, and (C) absenteeism, using a discrete, pre-pandemic pay increase in a single warehouse. The IV estimates shown here rescale the reduced-form effects in Figure 5 by the first-stage change in pay. In A−B, the odd columns estimate the change in the focal warehouse (Equation 5). The even columns estimate a difference-in-differences (DiD) design, comparing the focal warehouse to the firm's other warehouses (Equation 6). Observations are at the warehouse-week level. In C, absenteeism is measured at the person-day level. Columns 1−2 estimate the change in the focal warehouse; Columns 3−4, the DiD design. Columns 5−6 add individual worker fixed effects. Columns 2 and 4 exclude holiday weeks as in Figure 5D. *p<0.1; **p<0.05; ***p<0.01.

Online Appendix to

The Payoffs of Higher Pay

Labor Supply & Productivity Responses to a Voluntary Firm Minimum Wage

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Appendix A: Further Model Discussion

We begin by showing the derivation of wages for our model from Section I. We then present a model in which firms set wages of two periods. Finally, we consider how wages would impact tenures.

I.A Deriving the Wage-Setting Equation

Firms set wages to maximize profits:

$$\max_{w,N} \Pi = Y(Ne(w)) - wN \text{ s.t. } N \leq N(w)$$

Assuming the constraint is binding, we can substitute in for N and 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:

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

This expresses w when $\epsilon_{e,w} \leq 1$. When $\epsilon_{e,w} > 1$, this expression would suggest that the firm would set a wage in excess of the last hire's marginal revenue in order to induce greater effort from its workforce. Since hiring this worker would come at a loss, we can conclude that the firm would not make this hire and the constraint of $N \leq N(w)$ must not be binding. In this case, the firm adjusts its hiring such that the marginal revenue of the last hire is equal to the wage so $w = Y'(Ne)e$ but $N < N(w)$.

I.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.[48] In the model, a junior worker will become a senior worker if she persists at the firm. She will persist if the senior wage, $W_s(w_s) = w_s$, 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:

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

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:

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

[48] 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 Solow (1979); Shapiro and Stiglitz (1984); Yellen (1984), that allow for the possibility that productivity depends on the wage.

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

$$
\begin{aligned}
\frac{\partial \Pi}{\partial w_j} &= 0 : Y_j n_j' e_j + Y_j n_j e_j' - n_j - w_j n_j' + \frac{1}{1+r} R(w_{s,+}) n_j' \left[Y_s e_s - w_s \right] = 0 && (7) \\
\frac{\partial \Pi}{\partial w_s} &= 0 : \left[Y_j (n_j' e_j + n_j e_j') - w_j n_j' \right] \frac{\partial W_s}{\partial w_s} \\
&\quad + \frac{1}{1+r} Y_s \left[R(w_s) e_s n_j + R(w_s) e_s' n_j + R(w_s) e_s n_j' \frac{\partial W_s}{\partial w_s} \right] \\
&\quad - \frac{1}{1+r} \left[R(w_s) n_j + w_s R(w_s) n_j + w_s R(w_s) n_j' \frac{\partial W_s}{\partial w_s} \right] = 0 && (8)
\end{aligned}
$$

in the case where $\epsilon_{e,w} < 1$. We can use $\frac{\partial W_s}{\partial w_s} = \frac{1}{1+r} R(w_s)$ and rearrange terms in Equation 8:

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

Simplifying yields an expression for the senior wage:

$$
w_s = Y_s e_s \left[1 + \frac{\epsilon_{e,s}}{\epsilon_{\text{Departures}}} \frac{R(w_s)}{1 - R(w_s)} \right]. \tag{9}
$$

The firm will optimally set a premium on the senior wage over the worker's marginal revenue 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 9 into the expression for the junior wage from Equation 7:

$$
w_j = \left(1 - \frac{1 - \epsilon_{e,j}}{\epsilon_{\text{Recruit}} + 1} \right) Y_j e_j - \frac{1}{1+r} R(w_s) \frac{\epsilon_{\text{Recruit}}}{1 + \epsilon_{\text{Recruit}}} \left[Y_s e_s \frac{\epsilon_{e,s}}{\epsilon_{\text{Departures}}} \frac{R(w_s)}{1 - R(w_s)} \right]. \tag{10}
$$

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 mark down the junior wage if it is paying a senior wage in excess of the senior marginal revenue.

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 revenue:

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

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 I 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 revenue: $\frac{W-MP}{MP} = -0.01$. This is consistent with our findings from Section VI, that incorporating all of the frictions brings the pay much closer to workers' marginal revenue than only incorporating worker-frictions.

I.C Wages and Tenures

If raising wages impacts turnover rates, we analyze how the differential effect on turnover among more junior workers may align well with the effects we see.

Let s_t denote the stay rate in the entire warehouse at time t. The stay rate depends on the composition of the workforce and the wage rate relative to the outside option, w_t.

For simplicity, suppose there are two meaningful categories in tenure: new hires who just joined the company and experienced workers. Let e_t denote the share of experienced workers in the firm at time t. As such, $s_e(w_t)$ denotes the stay rate of experienced workers at relative wage w_t and $s_0(w_t)$ denotes the stay rate of new hires who will become experienced workers if they persist to the second period:

$$s_t = e_t s_e(w_t) + (1 - e_t) s_0(w_t)$$

If workers become experienced after a period of work and all workers who leave are replaced, then:

$$e_t = 1 - \frac{N_{0,t}}{N_t} = 1 - \frac{(1 - s_{t-1}) N_{t-1}}{N_t} = s_{t-1}$$

If the wage changes at time t, there is an immediate effect on attrition:

$$\frac{\partial s_t}{\partial w_t} = e_t s_e'(w_t) + (1 - e_t) s_0'(w_t)$$

Additionally, in the second period there is a further effect due to the change in the composition of the workforce. To see this, first note that we can express the stay rate at time t as

$$s_{t+1} = e_{t+1} s_e(w_t) + (1 - e_t) s_0(w_t) = s_t s_e(w_t) + (1 - s_t) s_0(w_t)$$

$$\begin{aligned}\frac{\partial s_{t+1}}{\partial w_t} + \frac{\partial s_{t+1}}{\partial w_{t+1}} &= s_t s_e'(w_{t+1}) + (1 - s_t) s_0'(w_{t+1}) + \frac{\partial s_t}{\partial w_t}(s_e(w_{t+1}) - s_0(w_{t+1})) \\ &\approx \frac{\partial s_t}{\partial w_t}(1 + s_e(w_{t+1}) - s_0(w_{t+1}))\end{aligned}$$

Thus, the effect will be larger in the second period after the change than it will be immediately after the imposition of the change if $s_e > s_0$. While we find that $s_e > s_0$ in our data (Figure A.13), the reason for this relationship is important. We have assumed that tenure has a causal effect on stay rates: experienced workers are more likely to stay than new hires because, for example, they have made friends with coworkers or organized their lives to accommodate the demands of this job. This inference might not hold if longer tenure is associated with higher stay rates because some workers are idiosyncratically good matches with the firm (e.g., because they already have a friend at the firm or they happen to live close by). If the latter reason causes the correlational relationship, then higher pay might not have these compounding effects over time because the workers who were retained at the firm when pay was higher might not be as good a matches with the firm. The patterns in our analyses and other studies suggests that the effect of higher pay on retention do increase over time (at least in the short term), suggesting that some of the correlational relationship between tenure and stay rates is causal.

We can generalize this idea to the case where workers have many different tenures.

Let $s_\tau(w_t)$ denote the stay rate of workers with tenure τ and relative wage w_t. Let $\theta_{\tau,t}$ denote the share of the workforce who has tenure τ at time t.

$$s_t = \sum_\tau \theta_{\tau,t} s_\tau(w_t)$$

If the warehouse changes the wage at time t, there is an immediate effect on the stay rate at time t:

$$\frac{\partial s_t}{\partial w_t} = \sum_\tau \theta_{\tau,t} s'_\tau(w_t)$$

If the firm's size is not changing, then $\theta_{\tau,t+1} = \frac{s_{\tau-1}(w_t)N_{\tau,t}}{N_{t+1}} = s_{\tau-1}(w_t)\theta_{\tau-1,t}$ for $\tau > 0$. Further, $\theta_{0,t+1} = 1 - s_t$ since everyone who left needs to be replaced. So:

$$s_{t+1} = (1 - s_t)s_0(w_t) + \sum_{\tau=1}^{\infty} s_{\tau-1}(w_t)\theta_{\tau-1,t} s_\tau(w_{t+1})$$

$$\frac{\partial s_{t+1}}{\partial w_t} + \frac{\partial s_{t+1}}{\partial w_{t+1}} = \sum_{\tau=1}^{\infty} s'_{\tau-1}(w_t)\theta_{\tau-1,t} s_\tau(w_{t+1}) - \frac{\partial s_t}{\partial w_t} s_0(w_t) + \sum_\tau \theta_{\tau,t+1} s'_\tau(w_{t+1})$$

As in the two-period model, this creates a larger impact after the first period.

Appendix B: Benchmark Data Sources

This appendix details the sources of contextual data about the metropolitan statistical areas (MSAs) in which our firm operates. We draw different pieces of information from several different data sources:

1. **Hourly Wages and Employment Counts from the Occupational Employment and Wage Statistics (OEWS) (Bureau of Labor Statistics, 2022d).** The OEWS surveys employers about their employees working in different occupations. The data is then aggregated to the occupation-MSA level each year. This yields information on employment counts and hourly wages. We focus on laborers and freight, stock, and material movers, hand (SOC code 53-7062).

2. **Separation Rates from the Quarterly Workforce Indicators (QWI) (Census, 2022).** QWI uses the panel data in the Longitudinal Employment Dynamics (LEHD) to generate statistics on job transitions (Abowd et al., 2009). We focus on separation rates, or the share of employees who begin the quarter at a firm but do not end the quarter at the firm. We divide through by four to compute monthly rather quarterly separation rates. The QWI aggregates firm separation rates to the sector-MSA level: we focus on the warehouse/transportation sector (two-digit NAICS 48-49).

3. **Establishment and Employment Counts from the Quarterly Census of Employment and Wages (QCEW) (Bureau of Labor Statistics, 2022b).** The QCEW aggregates unemployment insurance data by industry-MSA each quarter. We focus on general warehousing (NAICS code 49-3110).

4. **Concentration Measures from Azar et al. (2022).** The authors use 2016 vacancy data from Burning Glass to calculate Herfindahl-Hirschman indices (HHIs) for all U.S. labor markets. The HHI is calculated using the sum of the squared market share of each firm competing in the market. The HHI approaches zero for a market with infinitely similarly-sized firms, and the HHI is at its maximum of 10,000 for a market with a single firm. We focus on laborers and freight, stock, and material movers, hand (SOC code 53-7062).

5. **Unemployment Rate from the Local Unemployment Area Statistics (LAUS) (Bureau of Labor Statistics, 2022c).** This data is generated from large-scale surveys of workers (the Current Population Survey) and establishments (the Current Employment Survey). Unemployment estimates are available at the monthly level for each MSA.

6. **COVID-19 Death Rates come from data compiled in NYT (2022).** The New York Times compiled data from state and local governments and health departments. We focus on COVID-19 death rates since they are less susceptible to differences in testing behavior than COVID-19 death rates.

7. **Government-Mandated State Minimum Wages from U.S. Department of Labor (2022).** We use data collected by the U.S. Department of Labor on state-level minimum wage requirements.

Since these data are not all reported at a monthly frequency, we interpolate a linear trend for each series using a rolling average of the six months before and after each month for the annual data and the two months before and after each month for the quarterly data.

We use several metrics to contextualize how large the firm we study is relative to the markets in which it operates. We first calculate the share of local warehouse employment that the firm captures. The numerator comes from the firm's administrative data. The denominator is based on employment in warehousing occupations from OEWS. If that is missing, we impute it using employment in the specific industry from QCEW. The two series are reassuringly highly correlated. Second, we calculate the share of local warehousing establishments that belong to the firm, with the denominator coming from QCEW data.

We note the hourly wages in local warehouse jobs using data from OEWS. We prefer this data source for two reasons. First, the OEWS is the only source that provides information on hourly wages rather than quarterly earnings (which conflate hourly pay with hours worked). Second, the OEWS provides information at the occupation rather than industry level, which allows us to better map the public data into the relevant outside option for the front-line warehouse workers whom we study. We also benchmark the firm's pay against government-mandated state minimum wages.

We contextualize our firm's separation rate by calculating the local separation rate in the warehouse/transportation industry in the QWI data.

Appendix C: Staffing Agency Analysis

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").[49] On average, temporary warehouse jobs through the staffing agency last 3.4 months, with an hourly pay of \$11.74/hour. Only 44% of these jobs are completed, with 31% of people quitting. Only 13% of workers receive an "Excellent" evaluation.

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.[50] To do this, we begin by estimating the reduced-form relationship between pay and performance:

$$\mathrm{Y}_{ij} = \beta_0 + \beta_w \cdot \mathrm{w}_j + \mu_{o(j),c(j)} + \mu_{d(j),c(j),t(j)} + u_{ij}. \tag{11}$$

where i indexes the worker; j, the job; and t, the time; o, occupation; c, the commuting zone; and d, the season. By including these fixed effects, our estimates are identified off of variation in hourly pay across firms and workers in the same 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:

$$\mathrm{Y}_{ij} = \psi_w \cdot \mathrm{w}_j + \underbrace{\mu_i}_{\text{Worker FE}} + \mu_{o(j),c(j)} + \mu_{d(j),c(j),t(j)} + u_{ij}. \tag{12}$$

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 A.11 presents the results of this analysis. We find that an additional dollar of pay increases job completion by 2.6 pp, off a base of 41% completion—an elasticity of 0.72. Of that effect, 83% arises within the same worker.

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

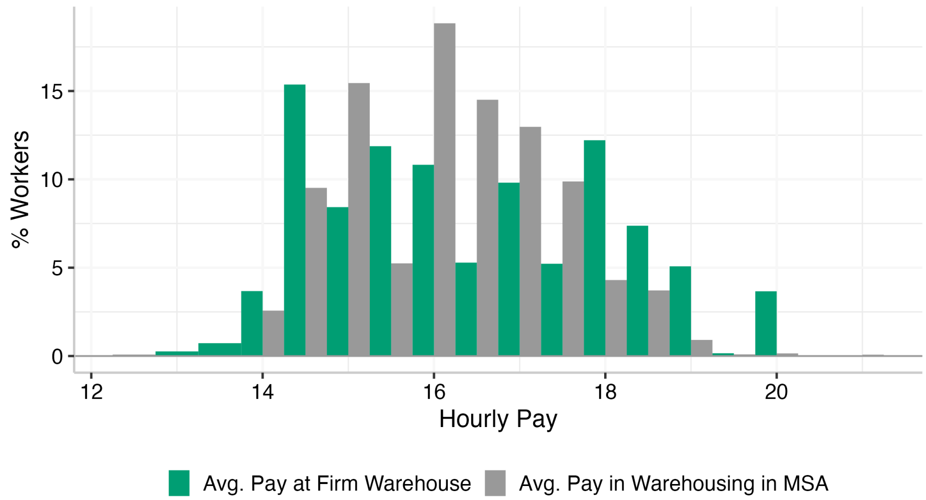
[49]When a firm hires through this staffing agency, it sends the agency a description of the job and the pay rate. In select cases, the firm may ask the staffing agency for a particular worker with whom the firm has had a positive prior experience, but in most cases it is up to the recruiter to locate 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 they do not want to create strife.

[50]Of the workers who took a job through the staffing agency, 64% 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% of workers take at least five jobs with the agency and are employed for a average of 263 days.

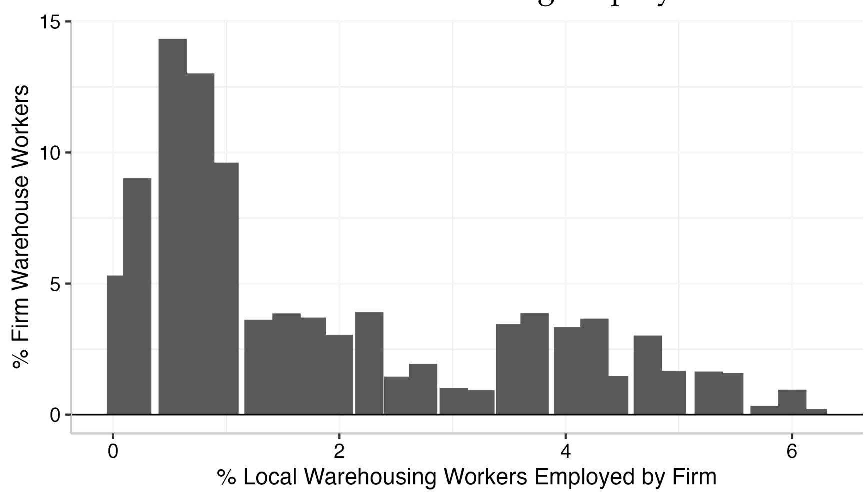
Appendix D: Supplemental Figures & Tables

Figure A.1: Contextualizing the Firm Relative to its Local Labor Markets Before the Firm-Wide Minimum Wage

Panel A: Pay Distribution in the Firm's Warehouses vs. Others Locally

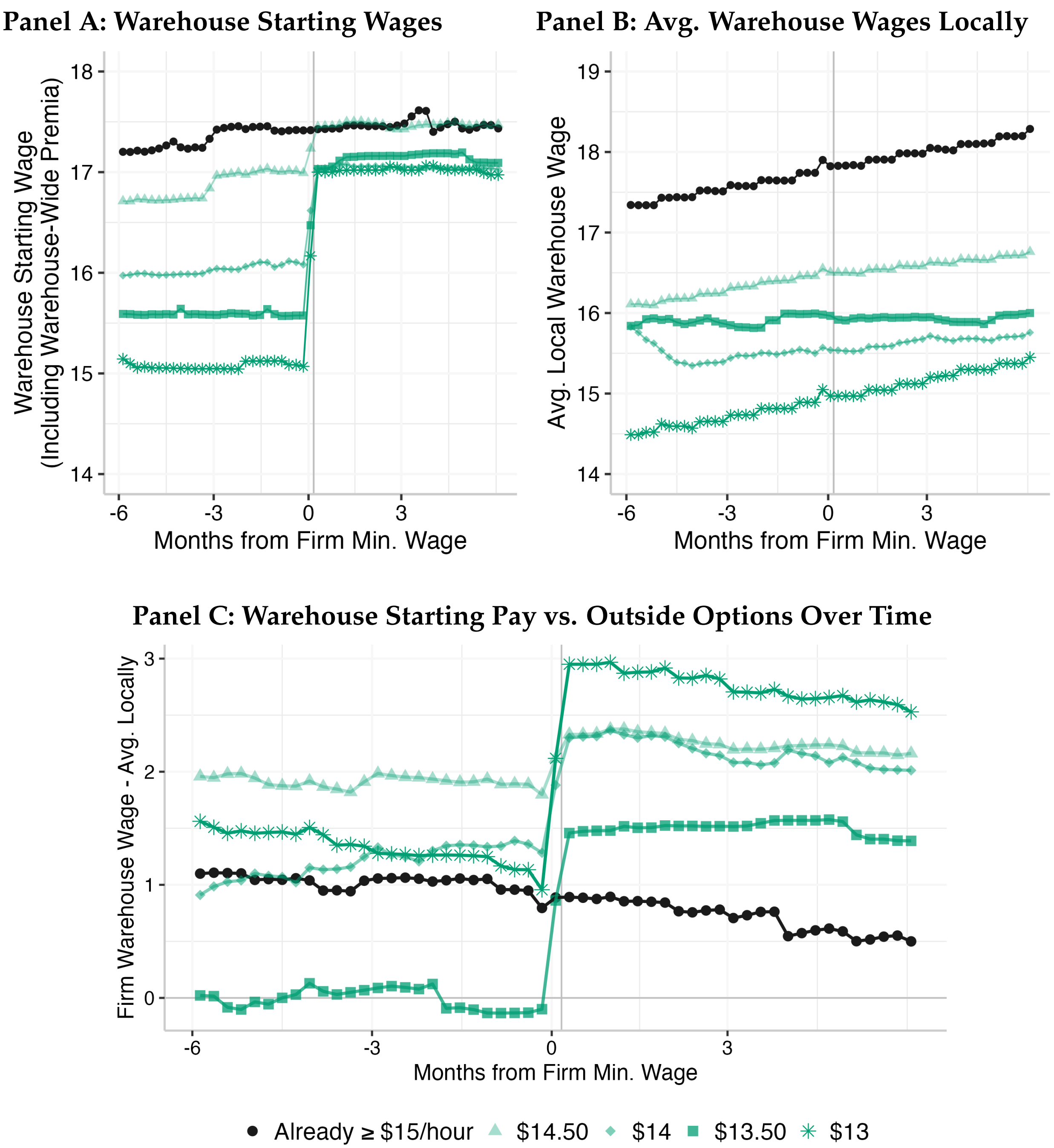


Panel B: Percent of Local Warehousing Employment At Firm



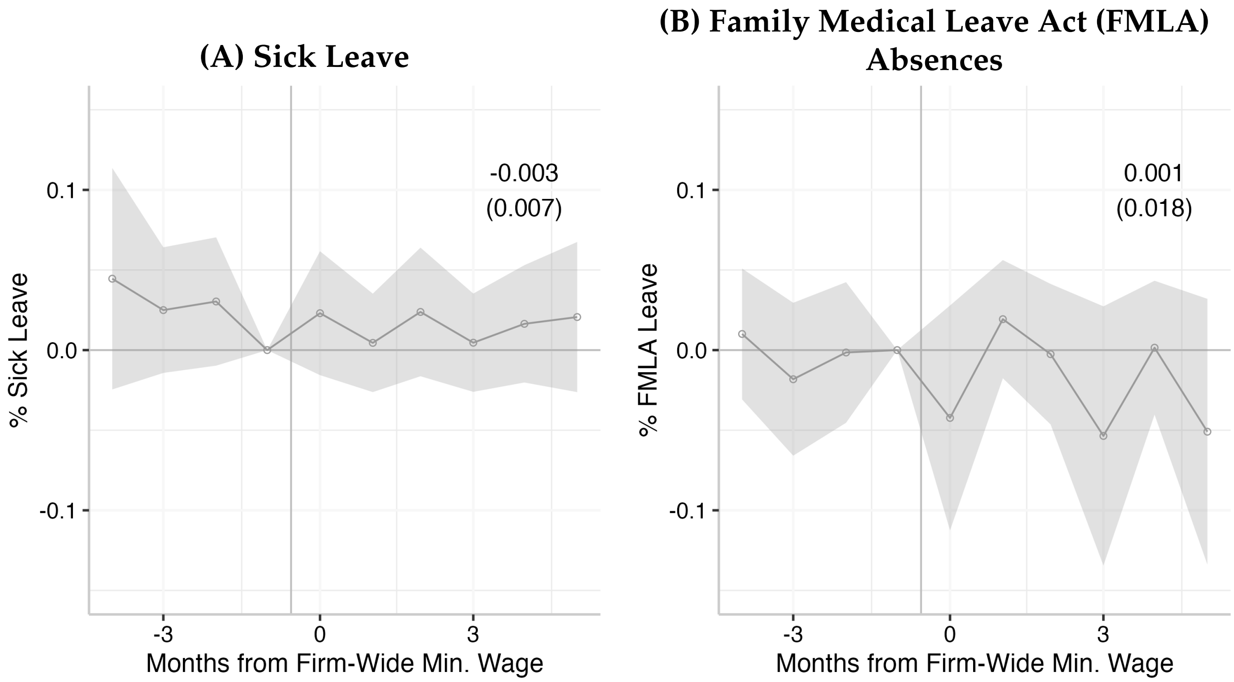
Note: This figure contextualizes the firm's position in its local labor markets. Panel A illustrates where the firm falls in the local pay distribution for warehouse work. The plot the distribution of average take-home pay across the firm's warehouses (in green) and the average warehousing wage in the Metropolitan Statistical Areas (MSAs) where the firm operates (in gray), weighted by the employment share of the firm within those labor markets. The distributions are each shown as histograms with fifty cent bins. The data on average warehouse wages comes from Occupational Employment and Wage Statistics (Bureau of Labor Statistics, 2022d). Panel B shows the percent of the local warehousing employment that the focal firm hires. Data on total local employment in warehousing comes from Occupational Employment Statistics (Bureau of Labor Statistics, 2022d), with missing values imputed using data from the Quarterly Census of Employment and Wages (Bureau of Labor Statistics, 2022b). Data goes from 2018 until the institution of the voluntary firm-wide minimum wage.

Figure A.2: Relative Pay Around the Voluntary Firm-Wide Minimum Wage



Note: This figure illustrates the differential impacts of the firm's voluntary minimum wage across warehouses vis-á-vis the local outside options. Panel A shows weekly starting wages around the implementation of the firm-wide minimum wage for warehouses with different base wages prior to its implementation. Starting wages include warehouse-wide premiums (e.g., for holidays) but not individual premiums (e.g., for night shifts). Panel B plots average warehouse wages in the Metropolitan Statistical Area from Occupational Employment Statistics (Bureau of Labor Statistics, 2022d). Panel C plots the difference in these series. The grey vertical lines show the week when the firm announced and implemented the firm-wide minimum wage across all its warehouses. Regressions that consider the effect of pay relative to the outside option may be found in Tables A.4(a)-(b).

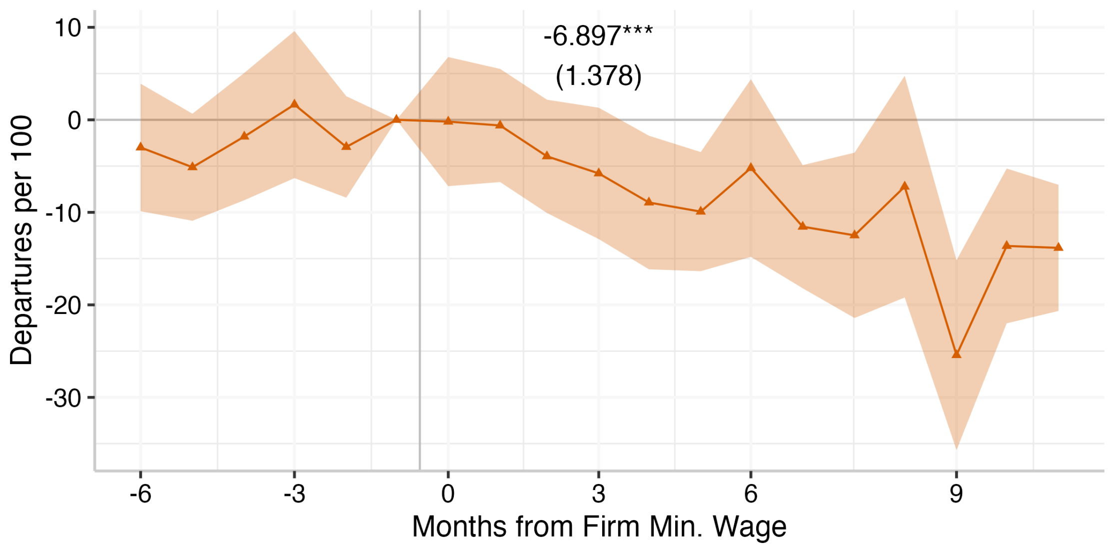
Figure A.3: Balance in Sick Leave and Family-Medical-Leave Absences



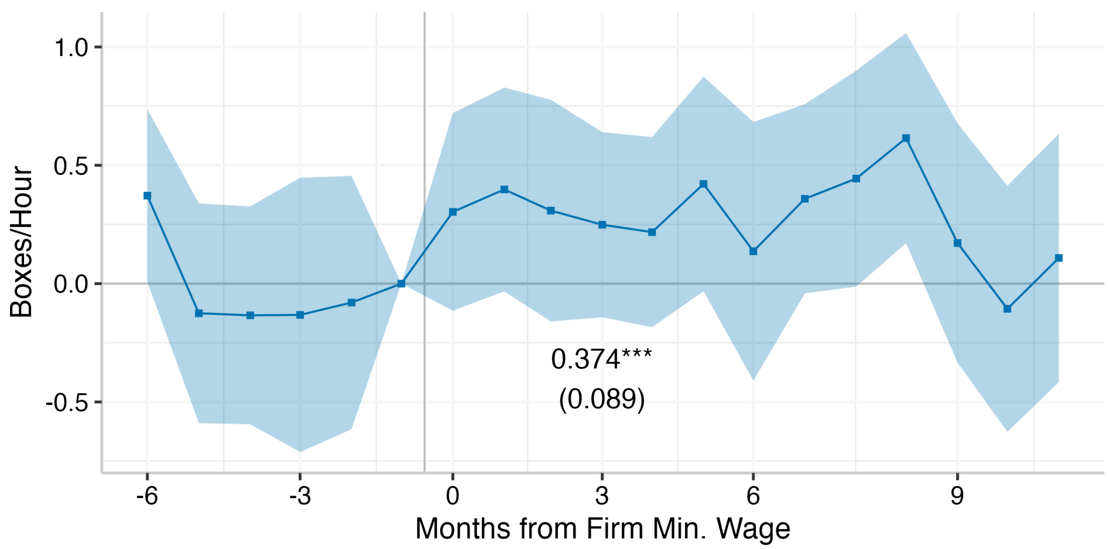
Note: This figure focuses on (A) sick leave and (B) family medical leave act (FMLA) absences around the imposition of the firm's voluntary firm-wide minimum wage. Each plot shows how these outcomes evolve differentially across more and less affected warehouses, using a dynamic version of Equation 3 with controls for COVID-19 death rates, state minimum wages, and region-by-week-by-warehouse type fixed effects. The annotated coefficients reflect the pooled, reduced-form estimates. During this period, 6XX workers had sick leaves and 3X workers went on FMLA leaves. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure A.4: Longer Term Effects

(a) Departures



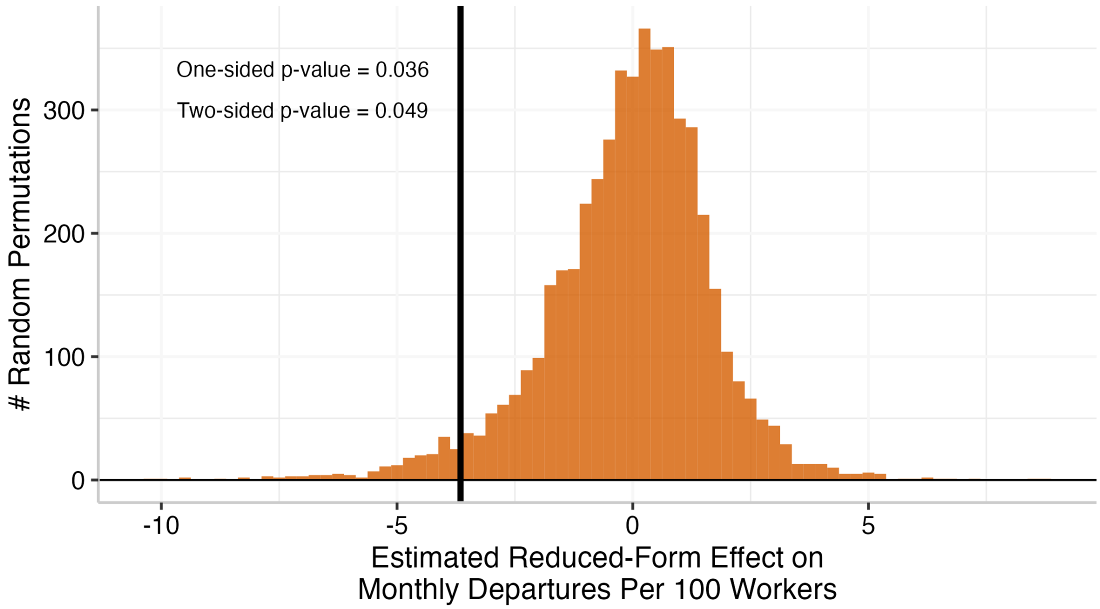
(b) Productivity



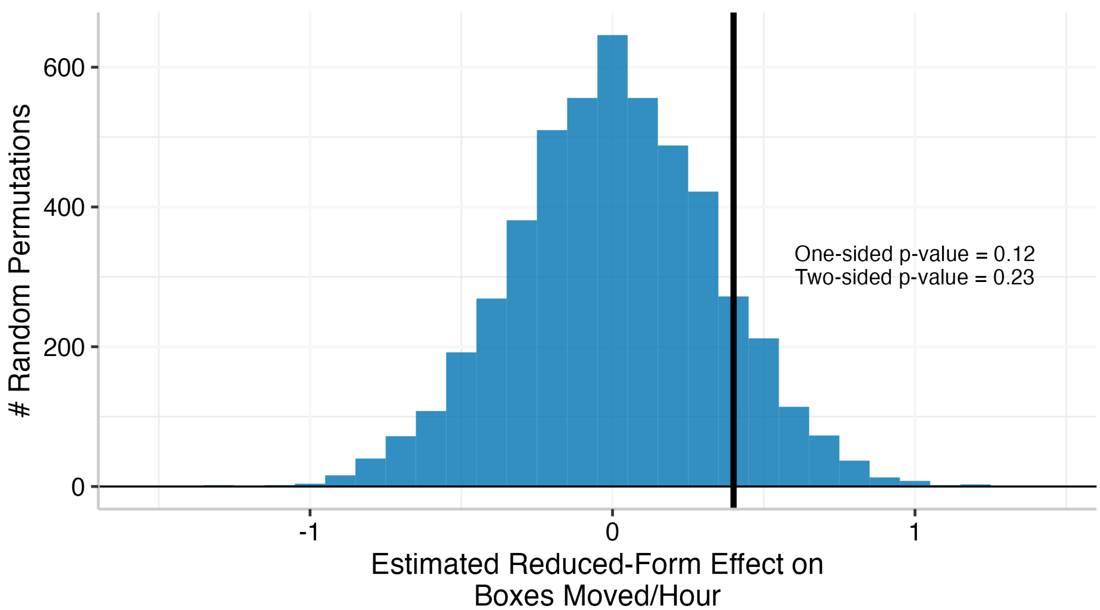
Note: This figure shows how departures and productivity evolve around affected versus unaffected warehouses around the firm's implementation of a voluntary minimum wage. This shows a longer time series than our baseline specification with six months in the pre-period and twelve months in the post-period (including the zeroth month). The dates are obfuscated to preserve the firm's anonymity, and 0 is the point at which the minimum wage was announced and implemented in all warehouses. Each plot presents reduced-form estimates from a dynamic version of Equation 3, which control for COVID-19 death rates, state minimum wages, and region-by-week fixed effects (that also interact with warehouse-type for productivity). The annotated coefficients reflect the pooled, reduced-form estimates. *p<0.1; **p<0.05; ***p<0.01.

Figure A.5: Permutation Tests of Reduced-Form Estimates

(a) Departures

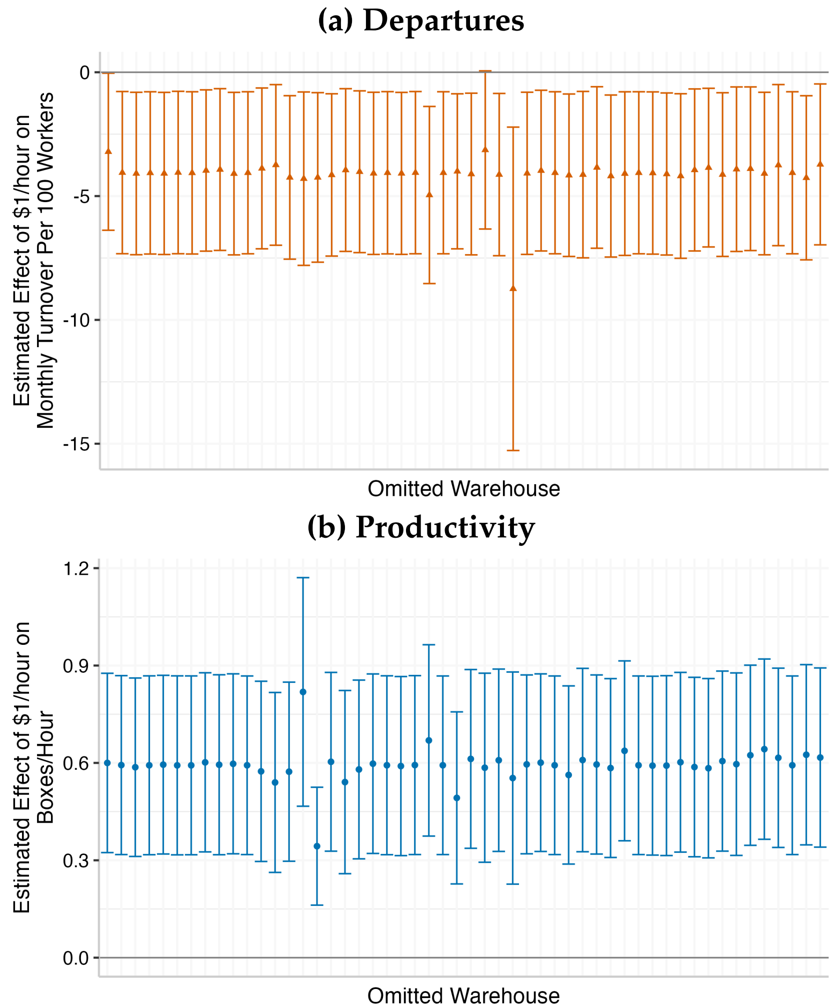


(b) Productivity



Note: This figure shows the result of 5,000 permutations. Each permutation randomly re-assigns 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 randomly chosen warehouses. For each permutation, we calculate the change in (a) departures at these warehouses and (b) productivity (in boxes moved per person-hour in the warehouse). 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: Robustness to Warehouse Exclusions

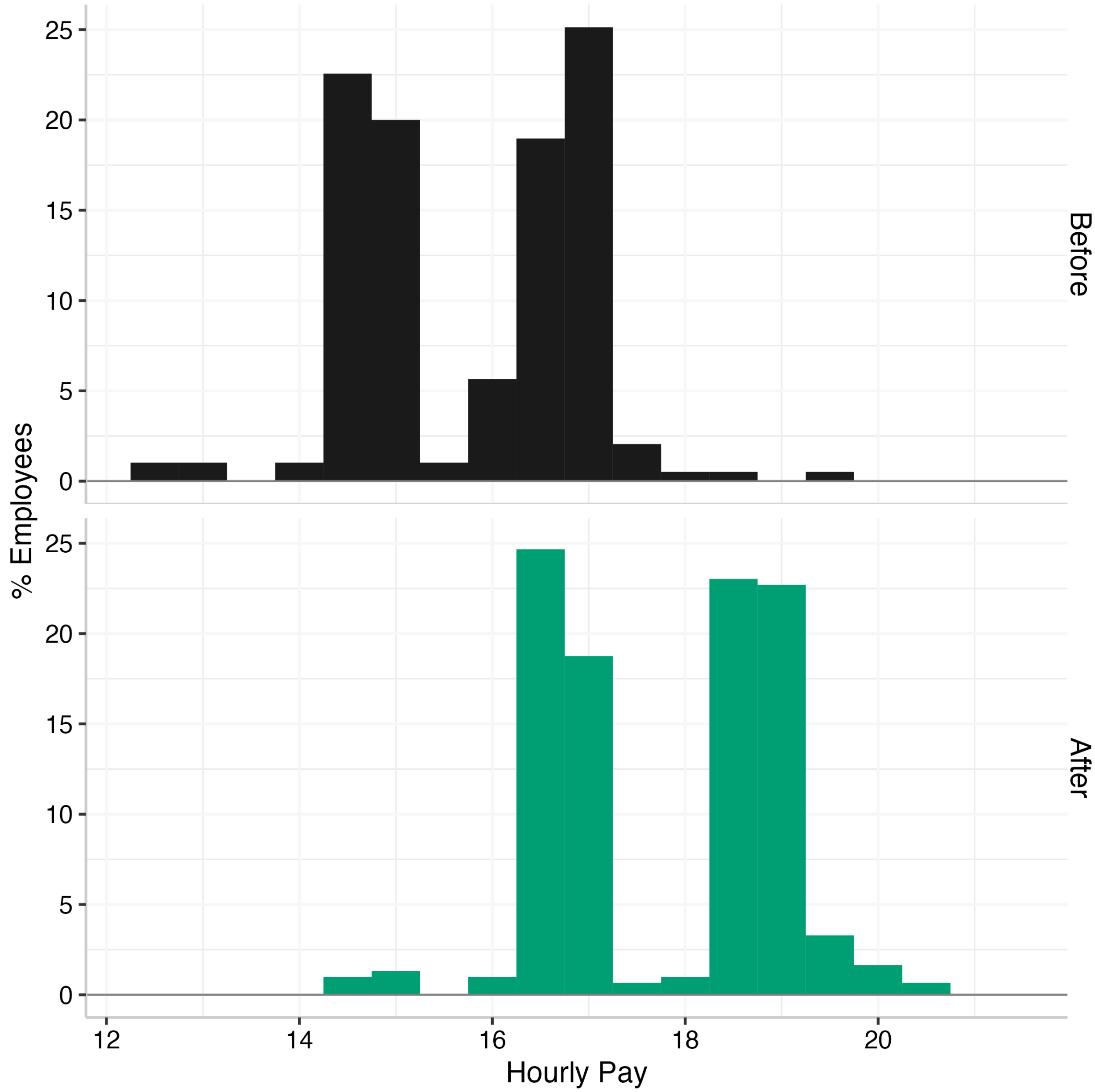


Note: This figure shows robustness of the IV estimates of the response to the voluntary firm minimum wage, omitting each warehouse in turn. Panel (a) focuses on monthly departures per 100 workers, and (b) on productivity (in boxes moved per person-hour in the warehouse). Each point represents an estimate, leaving out one warehouse. The regressions include controls for region by week fixed effects, county COVID-19 death rates, and state minimum wages. Whiskers show 95% confidence intervals. A small number of warehouses have been duplicated to preserve the firm's anonymity.

Figure A.7: Employee Satisfaction and Tenure around a Voluntary Firm Minimum Wage Introduction

Note: This figure shows how employee satisfaction and tenure 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 3. The coefficient and standard error are noted on the graphs. The black circles show control warehouses. The gray 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.8: Distribution of Pay Before and After Warehouse Pay Jump

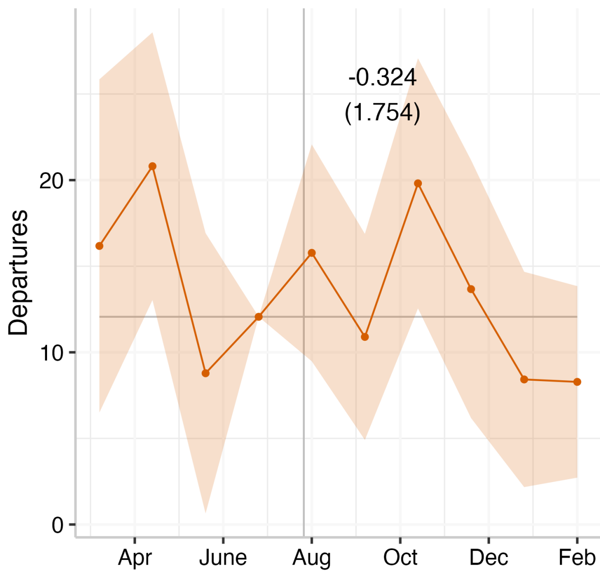


Note: This figure presents the distribution of pay among entry level warehouse workers within the treated warehouse one week before the August of the pay jump (the top panel) and one month afterward (the bottom panel). The change in pay led to a level shift rightward in the whole distribution of pay. Within each period, there are a few sources of individual-level wage variation: (a) a shift premium for unpleasant shifts, (b) a skill premium for being certified to work on specialized machinery, and (c) tenure raises for hitting certain milestones with the firm. The shift and skill premia generate a bimodal distribution, while the tenure raises generate variation around these means. The plots show histograms with fifty cent bins.

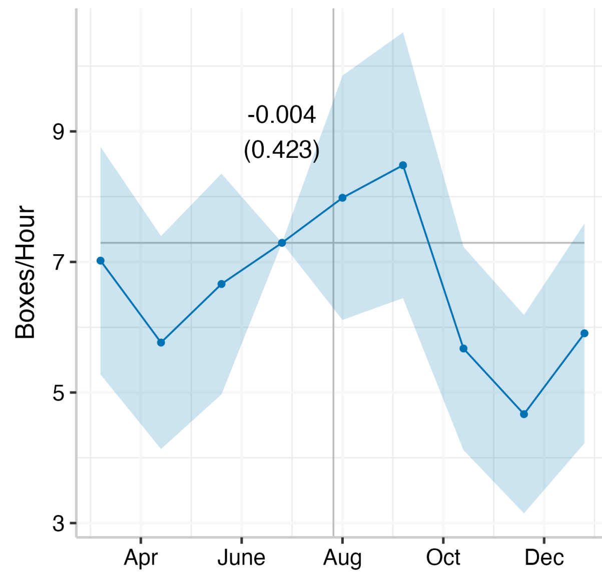
Figure A.9: Placebo Checks for the Pre-COVID Warehouse Pay Jump

Placebo I: Changes in Warehouses in the Same Labor Market as the Focal Warehouse

Panel A: Turnover

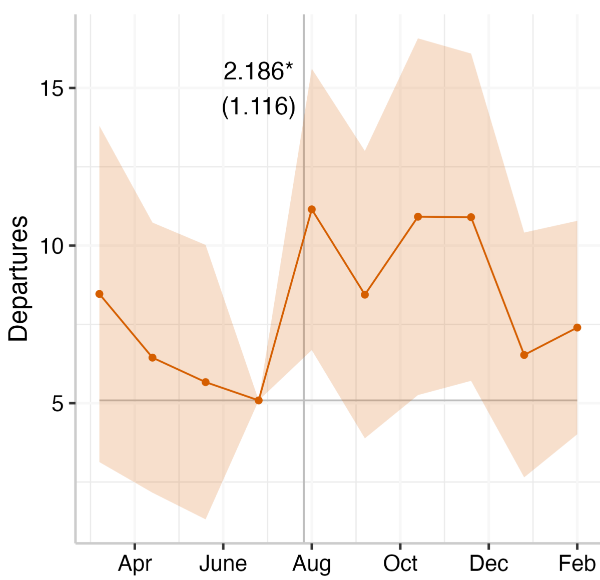


Panel B: Productivity

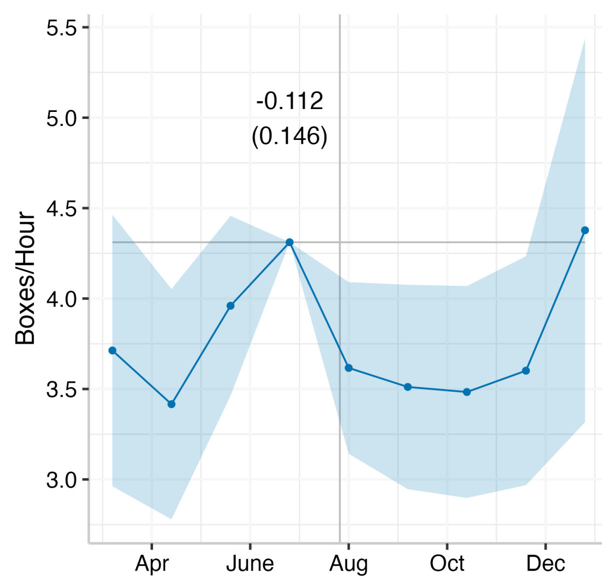


Placebo II: Changes in Twin Warehouses that Handle Similarly Large Parcels

Panel C: Turnover

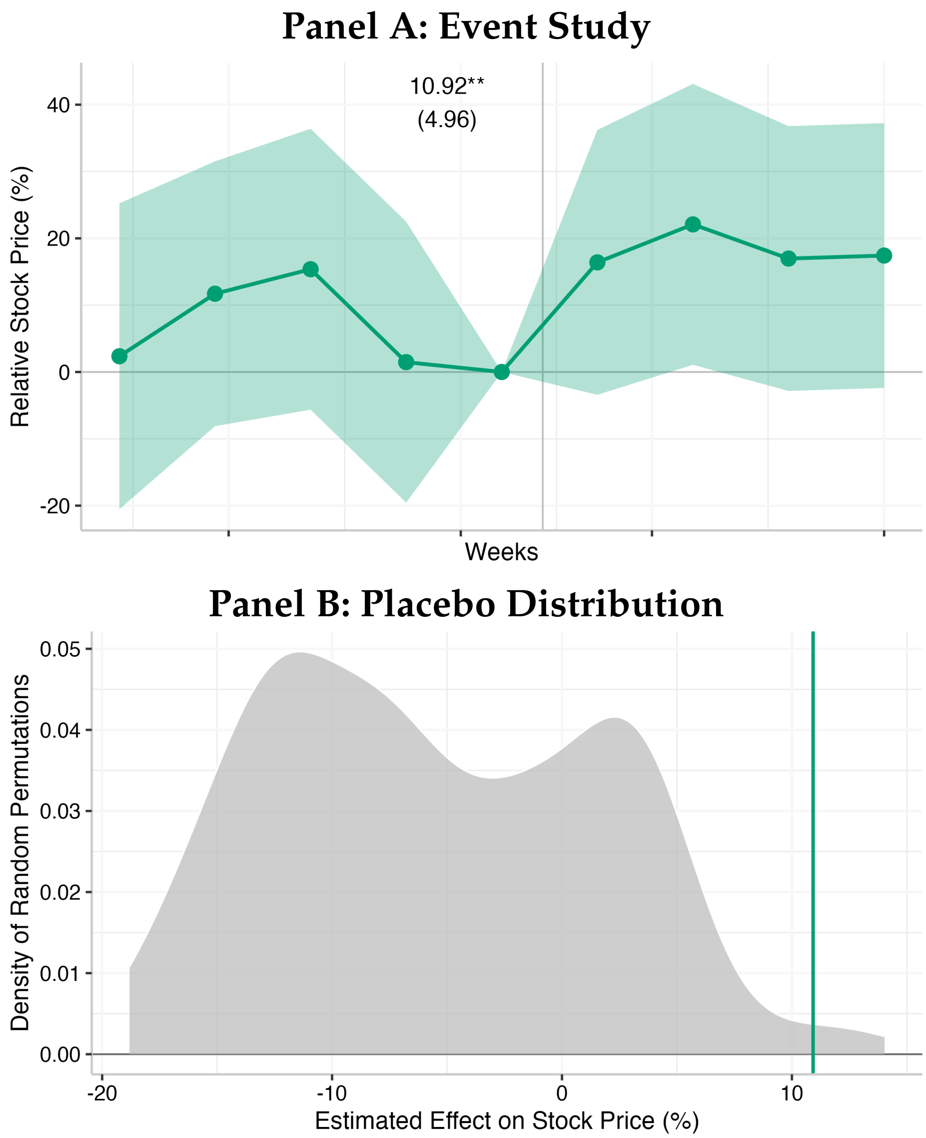


Panel D: Productivity



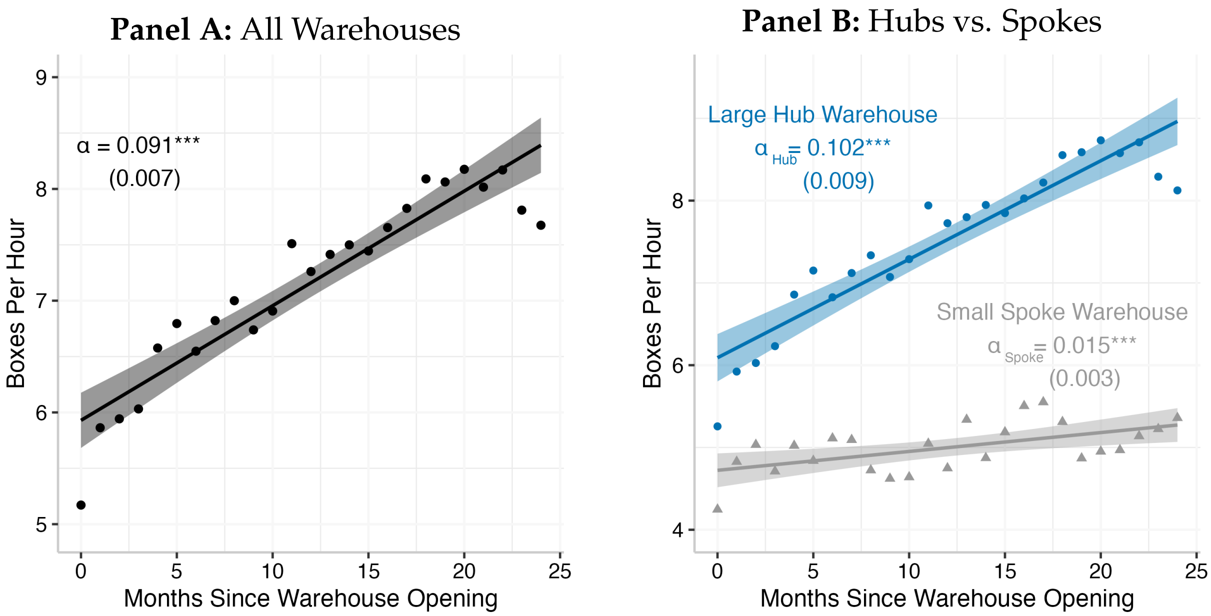
Note: This figure illustrates placebo checks for the design that uses a discrete pay change at a single warehouse prior to the pandemic. The first two panels focus on nearby warehouses that are in the same labor market as the treated warehouse but do not see discrete pay increases in this period. These nearby warehouses are 15-minutes away by car. The bottom two panels focus on "twin" warehouses that handle similarly large parcels as the treated warehouse but do not see pay raises in this period. The annotated coefficients reflect the reduced-form estimated changes for these placebo designs. The shaded areas display 95% confidence intervals. *p<0.1; **p<0.05; ***p<0.01.

Figure A.10: Stock Price Placebo Distribution



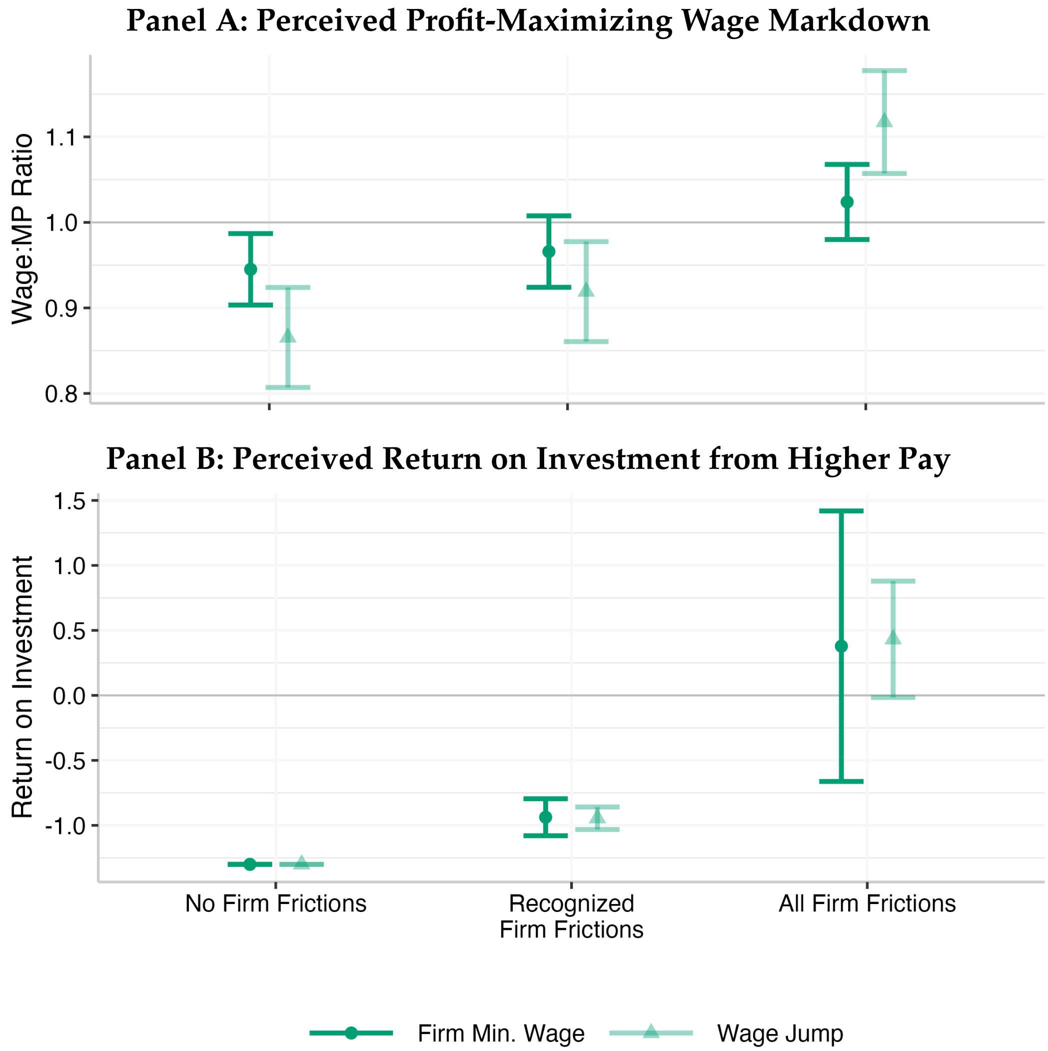
Note: This figure shows the impact of the $15/hour minimum wage policy on the stock price of the firm. Panel A shows the percent change in our firm's opening price relative to that of a random sample of top grossing firms in the industry at the weekly level. The gray vertical line shows the time when the policy change was announced publicly. The error ribbon reflects a 95% confidence interval. The annotated coefficient represents the pooled estimate from this difference-in-differences. Panel B shows the results of imagining that the $15/hour minimum wage was announced and implemented on any other stock-trading day in the surrounding years, excepting the one-month bandwidth around our estimated change and the first six months of the pandemic. Each estimate compares the stock price of our firm to that of same sample of comparison firms as in A. The estimated effect from the true date of announcement is highlighted in the green vertical line. Our estimated effect is larger than all but 1.3% of the placebo-estimated effects. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure A.11: Warehouse Productivity as a Function of Collective Worker Experience



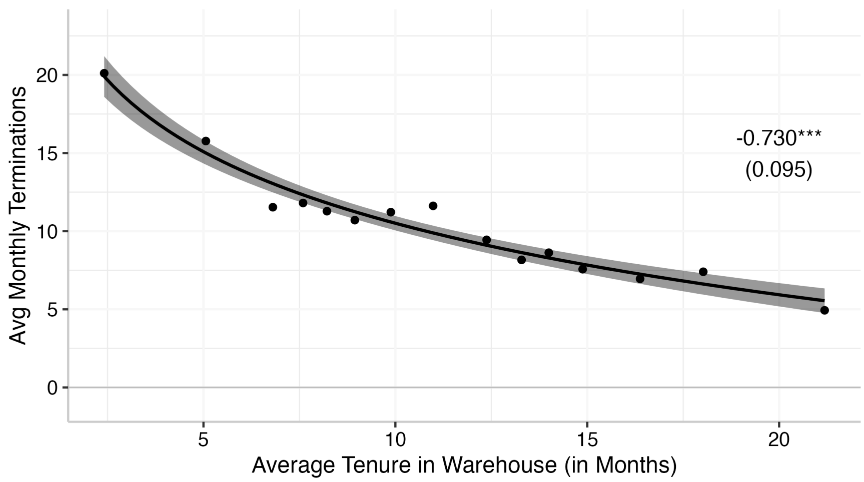
Note: This figure investigates the relationship between warehouse productivity and collective experience. Each plot shows the quantile relationships between the months since the warehouse opened and the productivity in the warehouse. The points reflect 25 quantiles of months. Panel A shows the overall relationship. Panel B distinguishes between football-field-sized, hub warehouses (the blue dots and lines) and small, gym-sized spoke warehouses (the gray triangles and lines). The annotated coefficients report the aggregate linear relationships. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure A.12: Perceived Profit-Maximizing Wage-Setting



Note: This figure shows how a firm might incorporate the estimated elasticities into wage decisions. Panel A shows wages relative to workers' marginal revenue, according to Equation 2, again varying which returns to higher pay the firm recognizes. The horizontal gray line shows the point at which wages are equal to the worker's marginal revenue. Panel B shows how a firm might think about the return on the investment in paying workers \$1/hour more. This increase in pay costs the firm \$1.30/hour including taxes and benefits. These costs can be partially defrayed by the payoffs of higher pay that the firm recognizes and fully defrayed by recognizing the productivity effects of raising pay. The grey horizontal line indicates the point at which the firm has broken even on their investment. Whiskers delineate 95% confidence intervals.

Figure A.13: Monthly Termination Rates by Tenure



Note: The figure shows the cross-sectional relationship between average tenure in the warehouse and average monthly termination rates over the course of the whole period of our data. The fit line represents a logarithmic fit. The error band is a 95% confidence interval for this fit. The annotated coefficient shows the estimated weighted least squares relationship, weighted by warehouse size. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.1: Adjacent Occupations to Warehouse Work

	% Of Warehouse Laborers		
Future Occupation (Code)	Overall	2018-2019	2020-2021
Warehouse Laborers (9620)	77.19	77.40	76.99
Not Employed (9999)	8.24	7.08	9.38
Driver/Sales Workers & Truck Drivers (9130)	2.49	2.92	2.07
Stock Clerks & Order Fillers (5620)	1.09	0.19	1.97
Packers & Packagers, H& (9640)	1.07	1.07	1.08
Industrial Truck & Tractor Operators (9600)	0.83	0.61	1.05
Taxi Drivers & Chauffeurs (9140)	0.76	1.30	0.22
Bus & Ambulance Drivers & Attendants (9100)	0.74	0.70	0.77
Cleaners Of Vehicles & Equipment (9610)	0.67	0.94	0.40
Production Workers, All Other (8965)	0.30	0.19	0.41

Note: This table shows the adjacent occupations to warehouse labor. Data comes from the Current Population Survey for 2018 to 2021 (Sarah Flood and Warren, 2020). The table starts with the sample of people who were in warehouse jobs in the previous year and then presents the percent of those workers who are in various occupations in the current year. The first columns shows all years from 2018 to 2021. The second column focuses on pre-pandemic years of 2018-2019. The third columns focuses on pandemic years of 2020-2021. These percentages are computed using survey weights.

Table A.2: Robustness to Narrowing Bandwidths around Voluntary Firm Minimum Wage

	(a) Monthly Turnover per 100 Workers			
Gap from \$15/hour x Post	-1.290	-0.395	-3.654^{**}	-2.499^{*}
	(2.955)	(1.594)	(1.503)	(1.331)
Base Mean	7.39	8.42	8.42	9.24
	(b) Boxes/Hour			
Gap from \$15/hour x Post	0.380^{*}	0.400^{***}	0.353^{**}	0.308^{***}
	(0.204)	(0.153)	(0.142)	(0.107)
Base Mean	6.8	6.8	6.8	6.8
Bandwidth	1mo	3mo	3,6mo	6mo
# Warehouses	4X	4X	4X	4X
# Workers	3,XXX	4,XXX	4,XXX	5,XXX

Note: We estimate the reduced-form impact of the voluntary firm minimum wage on (a) departures and (b) productivity according to Equation 3, changing the bandwidth around implementation. The baseline mean is calculated in the pre-period. Each specification includes controls for COVID-19 death rates, state minimum wages, and region-by-week fixed effects (that also interact with warehouse-type for productivity)*p<0.1; **p<0.05; ***p<0.01.

Table A.3: Using Discrete Instruments to Estimate Response to a Voluntary Firm Minimum Wage

	(a) Monthly Departures			
	\$/Hr	Monthly Departures Per 100 Workers	\$/Hr	Monthly Departures Per 100 Workers
Start $\leq$\$14/hr x Post	1.369***		1.170***	
	(0.047)		(0.033)	
Start \$14.50/hr x Post	0.228***			
	(0.040)			
$\widehat{\Delta\$/\text{hour}}$		-4.393^{**}		-3.556^{*}
		(1.941)		(1.970)
Elasticity		-9.4		-7.61
		(4.15)		(4.22)
Base Mean	\$18.02	8.42	8.42	8.42
First Stage F		407		621
	(b) Productivity			
	\$/Hr	Boxes/Hour	\$/Hr	Boxes/Hour
Start $\leq$\$14/hr x Post	1.332***		1.265***	
	(0.040)		(0.029)	
Start \$14.50/hr x Post	0.087**			
	(0.041)			
$\widehat{\Delta\$/\text{hour}}$		0.328**		0.344**
		(0.153)		(0.154)
Elasticity		0.87		0.91
		(0.40)		(0.41)
Base Mean	\$18.02	6.8	6.8	6.8
First Stage F		616		972
# Warehouses	4X	4X	4X	4X
# Workers	4,XXX	4,XXX	4,XXX	4,XXX

Note: This table estimates the impact of the voluntary firm minimum wage on (a) monthly departure rates and (b) productivity (in boxes moved per worker-hour) according to a version of Equation 3, with discretized treatment indicators. The baseline mean is calculated in the pre-period. Each specification includes controls for COVID-19 death rates, state minimum wages, and region-by-week fixed effects (that also interact with warehouse-type for productivity) and limits to a bandwidth of 3 months before the firm-wide minimum wage and 6 months afterwards. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.4: Relative Pay and the Response to a Voluntary Firm Minimum Wage

	(a) Monthly Departures per 100 Workers			
$\widehat{\Delta\$/\text{hour}}$	-3.870^{**}	-3.823^{**}	-3.828^{**}	
	(1.645)	(1.641)	(1.641)	
$\Delta\$/\text{hour vs. }\widehat{\text{Avg.}}\text{ in MSA WH}$				-3.897^{**}
				(1.642)
Elasticity	-8.28	-8.18	-8.19	-8.34
	(3.52)	(3.51)	(3.51)	(3.51)
Base Mean	$18.02	8.42	8.42	1.82
First Stage F	3131	3156	3151	1219
	(b) Boxes/Hour			
$\widehat{\Delta\$/\text{hour}}$	0.361^{**}	0.371^{**}	0.370^{**}	
	(0.148)	(0.149)	(0.149)	
$\Delta\$/\text{hour vs. }\widehat{\text{Avg.}}\text{ in MSA WH}$				0.397^{**}
				(0.163)
Elasticity	0.96	0.98	0.98	1.05
	(0.39)	(0.40)	(0.40)	(0.43)
Base Mean	$18.02	6.8	6.8	1.82
First Stage F	3425	3446	3444	1169
Outside Option Control				
Avg. $ in MSA WH	✓			
Avg. $ in MSA WH & OO		✓		
Avg. $ in MSA WH & OO in 2020-2021			✓	
Relative Pay First Stage				✓
# Warehouses	4X	4X	4X	4X
# Workers	4,XXX	4,XXX	4,XXX	4,XXX

Note: This table estimates the impact of the voluntary firm minimum wage on (a) monthly departure rates and (b) productivity (in boxes moved per worker-hour) according to Equation 3. The first three columns control for the average pay in warehouse jobs and other adjacent occupations in the local metropolitan statistical areas (MSAs), using data from the Occupational Employment Statistics (Bureau of Labor Statistics, 2022d). Column 1 uses average pay within the warehouse occupation (laborers and material movers). Column 2−3 broadens the definition by incorporating adjacent occupations. We define adjacent occupations using occupational transition probabilities observed in the Current Population Survey, which asks respondents about their occupation in both the current and previous year (Sarah Flood and Warren, 2020). Table A.1 shows common transitions for warehouse workers. We construct an MSA average outside option for warehouse workers, in which we weight each wage in the Bureau of Labor Statistics (2022a) by the transition probabilities across occupations both overall (2018−2021) in Column 2 and specifically in the pandemic (2020−2021) in Column 3. The last column defines the first stage in terms of relative within warehousing rather than absolute pay. The baseline mean is calculated in the pre-period. Each specification includes controls for COVID-19 death rates, state minimum wages, and region-by-week fixed effects (that also interact with warehouse-type for productivity) and limits to a bandwidth of 3 months before the firm-wide minimum wage and 6 months afterwards. *p<0.1; **p<0.05; ***p<0.01.

Table A.5: Relative Departures Around the Voluntary Firm Minimum Wage

	\$/Hr	Monthly Departures Per 100 Workers		\$/Hr vs. Avg. MSA	
Gap from \$15/hr x Post	0.892^{***}	-3.429^{**}		-3.554^{**}	
	(0.018)	(1.579)		(1.502)	
$\widehat{\Delta\$/\text{hour}}$			-3.845^{**}		-3.948^{**}
			(1.770)		(1.669)
Elasticity		-8.23	-8.23	-8.45	-8.45
		(3.79)	(3.79)	(3.57)	(3.57)
FEs & Controls	✓	✓	✓	✓	✓
Avg. MSA Turnover Controls	✓	✓	✓		
Relative Outcome				✓	✓
Bandwidth	3,6mo	3,6mo	3,6mo	3,6mo	3,6mo
First Stage F			2443		2923
Base Mean	\$18.02	8.42	8.42	5.67	5.67
# Warehouses	4X	4X	4X	4X	4X
# Workers	4,XXX	4,XXX	4,XXX	4,XXX	4,XXX
Adjusted R^2	0.990	0.121	0.124	0.120	0.123

Note: This table estimates the impact of the voluntary firm minimum wage on monthly departure rates according to Equation 3. The first three columns control for the average monthly departure rates in the warehousing and transportation sector in the metropolitan statistical areas (MSAs) where the firm operates, using data from the Quarterly Workforce Indicators (Census, 2022). The last two columns define the outcome in terms of relative rather than absolute departure rates. The baseline mean reflects departure rates in the pre-period. *p<0.1; **p<0.05; ***p<0.01.

Table A.6: Placebo Test of Response to a Voluntary Firm Minimum Wage

	(a) Monthly Departures Per 100 Worker	
	In Previous Year I	In Previous Year II
Gap from \$15/hour x Placebo Post	2.728	2.688
	(2.831)	(1.762)
Base Mean	11.37	13.18
	(b) Boxes Moved Per Hour	
	In Previous Year I	In Previous Year II
Gap from \$15/hour x Placebo Post	0.178	0.177
	(0.197)	(0.147)
Base Mean	6.26	5.27
Bandwidth	3,6mo	3,6mo
# Warehouses	2X	3X
# Workers	2,XXX	4,XXX
Adjusted R^2	0.811	0.880

Note: This table shows the results of running Equation 3 on other calendar years' data where the same calendar week in other years is considered the time of implementation. *p<0.1; **p<0.05; ***p<0.01.

Table A.7: Absent Hours Response to a Voluntary Firm Minimum Wage

	\$/Hr	Unexcused Absent Hours		
Gap from \$15/hr x Post	0.944^{***}			
	(0.011)			
$\widehat{\Delta\$/\text{hour}}$		-1.087^{***}	-0.996^{***}	-0.999^{***}
		(0.106)	(0.113)	(0.111)
Elasticity		-8.18	-7.49	-7.52
		(0.79)	(0.85)	(0.84)
Individual FE			✓	✓
Tenure Quartic				✓
% Effect within Worker			91.6%	91.9%
Base Mean	\$18.02	2.39	2.39	2.39
# Warehouses	4X	4X	4X	4X
# Workers	4,XXX	4,XXX	4,XXX	4,XXX

Note: This table evaluates the absenteeism effects of the voluntary firm-wide minimum wage in terms of total unexcused absent hours, measured at the worker-day level. All specifications estimate Equation 3. Every column controls for regional fixed effects, which are firm-defined logistics region interacted with warehouse type and week, and controls are county COVID-19 death rates and state minimum wages. Every specification uses a three-month pre-period and six-month post-period. *p<0.1; **p<0.05; ***p<0.01.

Table A.8: Mechanisms Driving Absenteeism Response to a Voluntary Firm Minimum Wage

	% Unexcused Absent			Baseline % Absent
Gap from \$15/hour x Post	-2.568^{***}	-1.139^{*}	-2.292^{***}	-0.433^{***}
	(0.585)	(0.667)	(0.595)	(0.151)
Gap from \$15/hour x Hired Post			-5.239^{***}	
			(1.402)	
Hired Post			6.664^{***}	
			(0.631)	
Sample	All	All	All	Existing Workers
Worker FE		✓		
Mechanism		Within Worker	New-Hire Selection	Existing-Worker Selection
% New Hires	20.2%	20.2%	20.2%	20.2%
% Existing Workers	79.8%	79.8%	79.8%	79.8%
Contribution		44.4%	41.2%	13.4%
Base Mean	15.9	15.9	15.9	15.9
# Workers	4,XXX	4,XXX	4,XXX	2,XXX
# Worker Days	391,XXX	391,XXX	391,XXX	284,XXX

Note: This table evaluates the different drivers of the absenteeism effects of the voluntary firm-wide minimum wage. Column 1 estimates the reduced-form effect of higher pay on absenteeism, as in Figure 3C. Column 2 estimates the change within worker, by including worker fixed effects. Column 3 interacts the warehouse's exposure to the voluntary minimum wage with an indicator for whether the worker was hired after the minimum-wage was imposed. The greater reduction in absenteeism among new hires suggests increasing pay improves the selection of new hires. Column 4 estimates the change in selection of existing workers. The dependent variable is the average absenteeism of the worker, as measured in a pre-period before the analysis window. A negative coefficient indicates that more affected warehouses are more likely to retain low absenteeism workers and shed high absenteeism workers. Every column controls for regional fixed effects, which are firm-defined logistics region interacted with warehouse type and week, and controls are county COVID-19 death rates and state minimum wages. Every specification uses a three-month pre-period and six-month post-period. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.9: Accident and Injury Response to a Voluntary Firm Minimum Wage

	\$/Hr	Injuries/Full-time Worker			
Gap from \$15/hr x Post	0.628***	−0.011	−0.018	−0.018	
	(0.074)	(0.009)	(0.019)	(0.019)	
$\widehat{\Delta \$/\text{hour}}$					−0.028
					(0.031)
Elasticity		-6.14	-10.36	-10.15	-10.15
		(5.22)	(6.68)	(6.90)	(10.92)
Base Mean	\$17.87	0.05	0.05	0.05	0.05
Adjusted R^2	0.978	0.003	0.083	0.077	0.039
Region FE	✓		✓	✓	✓
Controls	✓			✓	✓
# Warehouses	4X	4X	4X	4X	4X
Avg. # F-t Equivalent Workers	6,XXX	6,XXX	6,XXX	6,XXX	6,XXX

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.

Table A.10: Temporary Warehouse Positions with a Staffing Agency

	All Assignments	Multi-Assigned
\$/hour	11.45	11.19
Duration (days)	116.85	103.75
% Attendance Problem	15.20	14.87
% Quit	35.75	33.55
% Excellent Evaluation	10.32	11.08
% Positive Evaluation	34.95	36.85
% Negative Evaluation	18.08	18.45
# Workers	421,073	93,176
# Assignments	712,993	310,080

Note: This table summarizes data from a staffing agency's warehouse placements. Statistics are aggregated from job-level data, so each job is weighted equally. The first column displays all warehouse jobs through this staffing agency. The second column shows jobs held by staffers who have held multiple jobs — and are thus included in the analysis in Table A.11.

Table A.11: Absenteeism and Firm Satisfaction Effects of Higher Pay Within and Across Temporary Warehouse Workers

	Attendance Problem		Excellent Eval.		Positive Eval.		Negative Eval.	
	Across	Within	Across	Within	Across	Within	Across	Within
\$1/hr	-1.056^{***}	-0.380^{**}	1.205^{***}	0.591^{***}	1.535^{***}	0.876^{***}	-1.128^{***}	-0.522^{***}
	(0.150)	(0.152)	(0.137)	(0.118)	(0.189)	(0.188)	(0.135)	(0.166)
% of Full Effect		35.9%		49%		57.1%		46.3%
Duration	✓	✓	✓	✓	✓	✓	✓	✓
FEs	✓	✓	✓	✓	✓	✓	✓	✓
F	3.14	4	6.42	5.57	8.59	6.14	4.75	4.34
Mean \$/hr	11.19	11.19	11.19	11.19	11.19	11.19	11.19	11.19
Dependent Mean	14.87	14.87	11.08	11.08	36.85	36.85	18.45	18.45
Workers	93176	93176	93176	93176	93176	93176	93176	93176

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 outcomes. 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. Attendance problem is defined as a job ending early due to an attendance problem; the other outcomes refer to the evaluation from the manager. 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 location-firm level. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.