# THE PAYOFFS OF HIGHER PAY:

## ELASTICITIES OF PRODUCTIVITY AND LABOR SUPPLY WITH RESPECT TO WAGES

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*(Job Market Paper)*

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**Abstract**

What do firms gain from raising pay for low-wage workers? Focusing on a Fortune 500 retailer, we estimate the impact of higher wages on employee productivity, turnover, and recruitment among warehouse and call-center workers, using the quasi-randomness induced by sticky wage-setting policies. We document finite wage elasticities of turnover (between $-3.0$ and $-4.5$) and recruitment (between 3.2 and 4.2), which suggest the firm has some wage-setting power. Yet, on the margin, raising wages by \$1 increases productivity by more than \$1, giving the firm an incentive to pay more, even if they could pay lower wages. These responses to pay emerge both in a setting where the firm discretely raised wages and in a setting where its wages remained constant while other firms raised pay. These effects reflect both changes in worker selection and changes in behavior of existing workers. We estimate that over half of the turnover reductions and productivity increases arise from changes in workers' behavior. Finally, our estimates suggest considerable gender heterogeneity: Men's turnover is more responsive to higher wages than women's. But turnover effects are swamped by women's stronger productivity response to higher pay. Together, the gender-specific elasticities suggest firms have an implicit incentive to set female wages above male wages and thus firm profits cannot explain the gender pay gap.

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Firm wage-setting decisions must balance the benefits to the firm of higher pay — lower turnover, higher worker effort, and enhanced recruitment — against the direct costs of higher compensation. Recent high-profile cases of major employers of low-wage workers voluntarily implementing large wage increases and/or company minimum wages suggest that firms are aware of the potential gains from paying workers above their outside options (e.g., Lardieri, 2018).[2] The trade-offs firms face are formalized in efficiency wage models (Shapiro and Stiglitz, 1984; Yellen, 1984; Katz, 1986) and yet the causal return to paying higher wages than workers' outside options has been difficult to assess given the endogeneity of pay. In this paper, we provide new evidence on the returns of higher pay to the firm. We estimate elasticities of turnover, productivity, and recruitment among warehouse workers and customer service employees at a major online retailer, using sharp, discrete changes in wages or gradual changes in outside options relative to sticky pay at the firm. Our approach permits us to calculate the return to the firm of raising pay, inclusive of productivity effects. Moreover, by comparing different workers' responses to pay to the same workers' responses, we can distinguish whether decreased turnover and increased productivity arise from workers' behavior responses or compositional changes in the workforce due to selection.

We leverage idiosyncracies in the firm's pay-setting policies to estimate these elasticities using three complementary empirical strategies. Nationally, the firm has sticky wages, which leads to exogenous variation in the value of wages relative to workers' local outside options. By comparing changes in relative pay in various cities over the course of the year to changes in the turnover in those cities, we are able to estimate the effect of changes in relative wages on workers' behavior. Second, when the firm gets "unstuck" and adjusts its wages, it changes pay discretely, leading to plausibly exogenous variation in wages. We leverage this large, abrupt jump in pay to look at the difference in turnover and productivity before and after. Finally, to estimate elasticities of recruitment, we use the fact that the firm sets wages nationally for its remote customer service workers, regardless of location, which again creates heterogeneity in wages relative to the outside option.

We estimate a turnover elasticity between $-3.0$ and $-4.5$ and a recruitment elastic between 3.2 and 4.2. While large relative to other estimates of labor supply elasticities, these elasticities are finite, suggesting an upward sloping labor supply curve. Finite elasticities are consistent with firms having some monopsony power even in these labor markets that feature competitive pressure. While firms could use monopsony power to lower wages, the response of productivity to higher pay that we estimate is substantial (elasticity of 1.1-1.2), suggesting a force that would push wages upward. Since these responses to pay could arise from workers' behavioral responses to higher pay or to selection of better workers as noted by Esteves-Sorenson (2018), we use data from a staffing agency to estimate how much of the decrease in turnover arises

[2] Of course, public relations and goodwill gains could also play a role in the decision-making of large and visible employers who are concerned with potential regulatory actions and consumer responses.

within the same worker when facing different wages for comparable work. We find that 80% of the turnover effects and suggestively 50% of firm satisfaction arises from workers' individual responses. Finally, we estimate gender-specific responses to higher pay to understand what our model suggests about the gender pay gap. We find that while women have lower labor supply elasticities than do men, women have much larger productivity responses, pointing toward a force pushing in the opposite direction of the existing gender pay gap.

Our paper makes four contributions. First, we document the effect of higher pay on productivity for warehouse workers and customer service representatives, using objective productivity metrics: boxes moved and calls answered. We estimate that the increase in productivity caused by raising wages fully pays for itself. This contributes to the important literature on efficiency wages, which has hypothesized about the effect of higher pay on productivity, but has struggled to quantify the elasticity of productivity with respect to pay.[3] Our findings echo the analysis of Ford Motor Company where high wages reduced turnover rates and elicited greater effort from workers (Raff and Summers, 1987) and Cappelli and Chauvin (1991), who find that higher relative pay in a multi-plant firm reduced disciplinary infractions as well as Cohn et al. (2014) who find a 25 percent pay cut reduces productivity by 15 percent among sales associates, and Hesford et al. (2020) who find that higher relative wages attract more capable candidates, reduce shirking and ultimately pay for themselves.[4] Our findings are inconsistent with Sandvik et al. (Forthcoming) who find a minimal change in productivity in response to a decrease in commission that lowered take-home-pay.[5] Estimating productivity effects of higher pay is also a contribution to the new monopsony literature (e.g., Manning, 2003) that focuses on the relationship between labor supply elasticities and wages, largely to the exclusion of productivity elasticities.

Second, we estimate labor supply elasticities in two thick labor markets—warehousing and customer service—both of which are characterized by many workers, many firms employing workers to do very similar jobs, and substantial churn of workers across different firms. Warehouses are often located close to cargo hubs, where many logistics firms all draw on the same

[3]Under the efficiency wage hypothesis, employers may pay a premium above the market wage to give the worker an incentive to try to keep their job, to lower recruiting and turnover costs, or to increase morale and effort (Shapiro and Stiglitz, 1984). If all employers raise wages, they employ fewer workers; thus, there is more labor supplied to the market than is demanded by firms so some workers who want jobs are unable to find them (Dickens et al., 1989).

[4]Other methods of studying this question have been used. Krueger and Summers (1988) and Orszag and Zoega (1996), for example, explore whether intra-industry pay differentials can be attributed to efficiency wages. Lang (2020) looks at labor supply responses to tax rates to see whether effort is contractable.

[5]Our findings also stand in contrast to some findings that raising teacher pay has no effect on student outcomes (Fryer, 2013; De Ree et al., 2018), though Biasi (Forthcoming) finds that pay discretion attracts better teachers and improves student outcomes. Parsons et al. (2013) also finds no relationship between wage premia and productivity. Chang and Gross (2014) provide a more nuanced picture of the labor force, finding that higher- and lower-skilled workers have different labor supply elasticities with respect to wages.

pool of workers. The retailer's call-centers are also located in markets with many openings for customer service workers. Nonetheless, we find turnover elasticities between $-3$ and $-4.5$, suggesting that workers' labor supply to the firm is not perfectly elastic even in labor markets that would likely contain substantial competitive pressure. This finding contributes to mounting empirical evidence of wage-setting power of firms in a wide variety of contexts, from nurses (e.g., Sullivan, 1989; Staiger et al., 2010), civil servants (Dal Bó et al., 2013), and school teachers(Ransom and Sims, 2010) to online workers doing narrowly defined tasks (Dube et al., 2020). Several papers further use linked employer-employee data to draw a connection between firm labor supply and workers' earnings(e.g., Bassier et al., 2020; Webber, 2015). While some find relatively little effect of pay relative to the outside option (e.g., Dube et al., 2019), we find that absolute pay and pay relative to the outside option does impact labor supply. Moreover, we separately estimate recruitment elasticities and turnover elasticities. The new monopsony literature often assumes the two elasticities are roughly equivalent (Manning, 2003; Dube et al., 2020),[6] since recruitment elasticities to an individual firm are difficult to estimate.[7] We are able to directly test this assumption and find that the two elasticities are, indeed, similar in magnitude.

Third, we estimate the extent to which the turnover and productivity effects we measure are due to sorting of better workers to higher-paying firms rather than the direct behavioral responses of workers to higher pay. We use data from staffing agency that places many warehouse workers in temporary jobs to assess how much of association between reduced quits and higher pay persists when we focus on the same worker placed in multiple assignments with different pay. We document that over half of the turnover effect arises from workers' behavior responses when we look at the same individual's responses to higher and lower wages, consistent with efficiency wages operating through means beyond just composition of the workforce. The remaining selection effects lead to negative spillovers onto other firms: when a high-paying firm is filling positions, other firms that are hiring for the same type of worker at that time end up with higher turnover rates. This evidence microfounds the literature that documents cross-firm wage elasticities since our estimates explain the need to raise wages in rival firms.[8]

Finally, by estimating gender-specific pay elasticities, we can shed light on how much responses to higher pay may explain gender pay gaps. We contribute to this literature in two ways: (1) we leverage quasi-random changes in relative pay *within* a job rather than variation in pay *across*

[6]This assumption is reasonable if workers joining one firm tend to be leaving another firm. The labor supply elasticity facing a firm is the combination of the job-to-job recruitment elasticity and the turnover elasticity (Manning, 2003).

[7]Recruitment elasticity estimates that do exist typically do not consider elasticities to specific firms and do not hold the attributes of the work fully constant (Katz and Krueger, 1991), with the notable exception of Dal Bó et al. (2013), who successfully randomize wages and measure what happens to recruitment in the Mexican civil service.

[8]For example, Staiger et al. (2010) finds that when Veteran's Affairs hospitals increase their wages for nurses, nearby hospitals do as well. Derenoncourt et al. (2020) examine the effect on local wages at other firm of wage raises such as Walmart's increases in pay from \$9 to \$11 in 2015-2018. They find a cross-employer wage elasticity of 0.25.

jobs to cleanly identify differences in turnover elasticities and (2) we estimate gender-specific productivity responses to higher pay, in addition to gender-specific labor supply responses. Our estimates reveal that women's turnover is less responsive to pay then that of men in customer service, which would be consistent with a 6-cent pay gap. Our findings on turnover are in line with those of Ransom and Sims (2010) in the context of grocery store clerks and Hirsch et al. (2010) in the context of German workers. But point estimate differs from the recent work by Caldwell and Oehlsen (2018) who find a small labor supply gap among Uber drivers, suggesting that gig work or male-dominated occupations may feature different dynamics than other sectors in terms of gendered labor supply elasticities. Importantly, we find that women's productivity response to higher pay is substantially larger than men's, suggesting a force that would push female wages higher than male wages. Together, these estimates underscore the importance of including productivity responses in addition to turnover responses when considering how worker responses to pay may affect firms' pay-setting incentives.

Our paper also illustrates how firms' wage rigidities can be used to estimate the effects of relative pay on turnover and performance. We introduce a novel instrument that leverages the fact that nationally sticky wages lead to greater real wage depreciation in places with faster aggregate wage growth. Given the ubiquity of nominal wage rigidity, these empirical designs could likely be applied in other settings to analyze how relative pay affects outcomes of interest.

In the process of analyzing the effects of higher pay, our work highlights wage-setting strategies among major firms that may deviate from the optimum in the short run. One pay setting strategy we leverage is sticky pay, which may be optimal if there are large adjustment costs. We also highlight uniform wage-setting, a tendency which parallels firms' uniform pricing strategies that leave money on the table given variation in local demand and competition (DellaVigna and Gentzkow, 2019). While not the focus of our paper, our findings suggest that uniform wage-setting policies are unlikely to be optimal long-run strategies, given variation in local labor supply and competitive pressure from other employers. That said, if the costs of tailoring pay to each geography are too large or the firm is concerned about inequities on remote-teams where each worker is located in a different area, uniform wage setting may still be worth the costs we estimate.

The rest of the paper is organized as follows. Section 1 presents a conceptual framework for structuring our empirical investigation and Section 2 introduces our datasets. Sections 3 - 5 document our findings on the elasticities of turnover, productivity, and recruitment with respect to pay. In Section 6 we conduct a cost-benefit calculation to estimate the returns to our firm of marginally higher pay. In Section 7, we explore the degree to which selection versus behavioral responses contribute to our results. Section 8 explores heterogeneity in responsiveness to pay by gender and its implications for the gender pay gap. Section 9 benchmarks our estimates from the Fortune 500 retailer against estimates from another firm, which also allows us to document whether higher pay at one firm has negative spillovers on other local firms. We conclude in Section 10.

# 1 Conceptual Framework

We lay out a simple conceptual framework to illustrate how the the three parameters that we estimate — the turnover elasticity, the recruitment elasticity, and the productivity response to pay — matter for wages in a partial-equilibrium setting where firms are able to optimally set pay. This framework adds productivity to the Burdett-Mortensen-Manning model linking the labor supply elasticity to wages (Burdett and Mortensen, 1998; Manning, 2003), by allowing the output of the worker to depend directly on her wage, as in efficiency wage models (Shapiro and Stiglitz, 1984; Yellen, 1984).

We consider a single firm in a market with several other firms, and a large number of homogeneous workers.

The firm sets wages to maximize profits: the output of the worker $p$, less her wages $w$, times the number of workers $N$:

$$\max_{x}(p(w_t) - w_t)N_t$$

The number of workers at a firm in period $t$ will reflect the number of new recruits, $R$, plus the share of workers in the previous period who do not leave the firm (turn over), $(1 - T)$, multiplied by the number of workers last period:

$$N_t = [1 - T(w_t)]N_{t-1} + R(w_t),$$

Note that we treat $p(w)$ and $R(w)$ as separable, in effect modeling $p(w)$ as a measure of average workforce productivity. As such, $p(w)$ reflects two components: both the composition of the workforce and the effort exerted by the workers employed. We attempt to decompose the two in Section 7.

In steady state, the number of recruits must balance the number of quits at the constant wage, giving us:

$$R(w) = T(w)N \implies N = \frac{R(w)}{T(w)}$$

We can solve for the firm's optimal steady-state wage:

$$\max_{w}(p(w) - w)\frac{R(w)}{T(w)}.$$

We then have:

$$(p'(w) - 1)\frac{R(w)}{T(w)} + (p(w) - w)\left(\frac{R'(w)}{T(w)} - \frac{R(w)}{T(w)^2}T'(w)\right) = 0$$

Dividing through by $\frac{R(w)}{T(w)}$ allows us to isolate the elasticities of recruitment and turnover with respect to wages, where $\epsilon_{R,w} = \frac{R'(w)w}{R(w)}$ and $\epsilon_{T,w} = \frac{T'(w)w}{T(w)}$, noting that $\epsilon_{T,w}$ will be a negative number since we are considering turnover — namely separations from the firm — which we expect to decrease with an increase in wages.

$$p'(w) - 1 + \frac{p(w) - w}{w}\left(\epsilon_{R,w} - \epsilon_{T,W}\right) = 0$$

Further multiplying by $\frac{w}{p(w)}$, we arrive at an expression relating the wage relative to the average product to the underlying elasticities:

$$\frac{w}{p(w)} = \frac{\epsilon_{p,w} + \epsilon_{R,w} - \epsilon_{T,W}}{1 + \epsilon_{R,w} - \epsilon_{T,W}}$$

Rearranging, gives an expression for the optimal wage:

$$w^* = \frac{p(w)(\epsilon_{p,w} + \epsilon_{R,w} - \epsilon_{T,W})}{1 + \epsilon_{R,w} - \epsilon_{T,W}}$$

This expression captures predictions made by both the new monopsony literature and the efficiency wage literature. It shows that as elasticities of turnover and recruitment grow in magnitude, so do wages (Manning, 2003). Intuitively, if workers are unwilling to come to work except at high wages, or are willing to leave at lower wages, wages will be driven upward. The expression also shows that if productivity is increasing in wages, then wages will also be larger. Several explanations are plausible: If ability and reservation wages are positively correlated, then higher pay enhances the selection of workers (Weiss, 1980). Alternatively, if the job is more valuable, higher wages deter shirking (Shapiro and Stiglitz, 1984). Finally, if workers are more likely to feel that they are being paid fairly, they may respond with greater effort in a sort of gift exchange (Akerlof and Yellen, 1990).

The wage is not inclusive of all the costs the firm needs to bear. For each dollar the firm pays to the worker, the firm must also pay about 30% more in taxes. This passes through easily to the model: $\max_w (p(w) - 1.3w)\frac{R(w)}{T(w)}$. This maximization problem yields a very similar optimal wage equation that scales the elasticities by the share of the firm's total cost that workers respond to – namely take-home pay: $w = \frac{0.77p(w)(\epsilon_{p,w}+\epsilon_{R,w}-\epsilon_{T,W})}{1+\epsilon_{R,w}-\epsilon_{T,W}}$.

We estimate p(w), the worker output, based on what the firm had previously been paying for a given level of output, inclusive of taxes. Implicit in this usage is the assumption that any misoptimization in the payment is of second-order. This means that when we arrive at a cost-benefit calculation, the concerns about taxes appear both on the cost-side and on the benefit-side, effectively canceling out. As such, we maintain the original equation, which does not scale by taxes.

Our second order condition is given by:

$$p'(w)\frac{R(w)}{T(w)} + 2(p'(w)-1)\left(\frac{R'(w)}{T(w)} - \frac{R(w)}{T(w)^2}T'(w)\right) < 0$$
$$p'(w) < 1 - \frac{p''(w)w}{2\left(\epsilon_{R,w} + \epsilon_{R,w}\right)}$$

If returns to higher wages are concave — p'(w) <1 and p"(w) < 0 — the second order condition holds. If p'(w) > 1, this may not hold and the first order condition may not yield the global optimum.

Finally, this model excludes two potentially important components of wage-setting: non-labor costs and optimal-wage discovery. We hold constant capital, which is consistent with marginal wage decisions at this firm. However, if the firm could reoptimize capital in light of optimal wages — for instance, by recalibrating warehouse usage — the gains from optimal wages might be higher. Second, we do not include the costs of discovering the optimal wages or recalibrating. We implicitly assume that it is costless to assess and implement optimal wages. While likely not precisely the case, on the margin, it may be a fine assumption.

## 2 Data

We use data from two large firms: a Fortune 500 retailer and a leading staffing agency. Though both organizations function throughout the United States and abroad; we focus on their U.S. operations.

**Online Retailer Data.** The first data source is a major online retailer, which employed 8,597 warehouse workers and 4,551 customer service representatives between 2018 and 2019.

We use the human resources records from the retailer, which detail each active worker's job title, level of employment (e.g., entry-level, associate, senior), pay rate, and location. For warehouse workers, we can observe the shifts they worked.

In addition to HR records, we have two datasets to measure on-the-job productivity of workers at the retailer. First, we observe productivity of each of the retailer's warehouses on each week.[9] The key productivity metrics are boxes moved per hour and boxes moved per moving hour, (total hours worked excluding hours spent eating lunch or attending team meetings).[10]

We look predominantly at a single warehouse that featured a large, plausibly exogenous pay jump, calling this the "treated" warehouse. Summary statistics for this warehouse and other comparison warehouses are shown in the first three columns of Table 1, Panel A. In the three

[9] The retailer does not track productivity of individual warehouse workers.

[10] At the time of data extraction, the retailer did not track data on damages or petty theft. This suggests that while these metrics may be important in principle, they are not first-order concerns for the retailer.

months before the pay jump, fully 13.4 percent of workers in the treated warehouse left in a given month. The treated warehouse tended to move 4.92 boxes per hour before the pay jump.

Second, we are able to directly observe the productivity of each customer service representative on each day they handle calls. These metrics include the total number of calls each representative answered and the average customer satisfaction reviews that day. Customer service representatives handle incoming calls from customers, potentially inquiring about a delivery, a return, or damaged product. Since these representatives do not make outgoing calls or handle incoming sales requests, the metrics we observe represent the key measures of productivity that the firm cares about.

We use two subsets of customer service representatives in our analyses. To investigate recruitment we use the subsample of 593 remote workers, who are drawn from all over the US (column five in Table 1, Panel A). These workers are paid, on average, $14.35/hour, which is 10 cents below the average entry rate for customer service agents in their metropolitan statistical areas (MSAs). On average, about 6.6 percent leave per month. They handled about 25 calls per day, which entails addressing a call in less than 19 minutes.

We also use the sample of 3,061 workers whose wages are sticky, which creates heterogeneity in their relative pay as their outside option changes. These workers include on-site workers, who are paid more, as well as remote workers; in the sample, the average pay is $16.02/hour, well above the $13.52 entry pay in their MSAs. There is lower turnover in this sample: 4.3 percent of workers leave in a given month. Daily call volumes are comparable to the remote sample.

We supplement these administrative records with data from Economics Modeling Specialists, International (Emsi) to find measures of the local pay for customer service representatives. Emsi compiles data from government sources including the Bureau of Labor Statistics and the Census, online profiles and resumes, online job postings and compensation data. Many companies, including the retailer, use Emsi's granular occupation- and labor-market-specific data on wages and labor supply to guide their decisions. While our own checks of Emsi's data against Bureau of Labor Statistics records, as well as the Quarterly Census of Employment and Wages (QCEW) and American Community Survey (ACS) suggest it is highly accurate, the widespread use of Emsi by companies suggests that their metrics reflect the local outside option as understood by firms.

**Staffing Agency Data.** Our second source of data is the segment of a large staffing agency that provides temporary staffing for production and warehouse companies, which placed workers in over 222,000 warehouse jobs between 2016 and 2018. Data from the staffing agency includes all of the assignments a worker was placed in through the staffing agency. For each assignment, we observe the pay rate, the firm that hired them, the reason that the temporary assignment concluded (e.g., the work was over, the worker quit, they were fired for poor performance, etc.), as well as the rating given by the manager at the firm ("Excellent," "Good," "Fair," "Poor").[11]

On average, temporary warehouse jobs through the staffing agency last 3.4 months, with an hourly pay of $11.74/hour (see Panel B of Table 1). Only 44 percent of these jobs are completed, with fully 31 percent of people quitting and 27 percent having a bad ending, which includes being fired for attendance or performance problems or receiving a "Poor" or "Fair" evaluation from the manager at the hiring firm.[12] Only 13 percent of workers receive an "Excellent" evaluation from the hiring firm's managers.

One client of the staffing agency, a shipping company, regularly hires many warehouse workers to load and move boxes. This shipper employs temporary help throughout the entire United States, always employing workers at the same wage — $17/hour — regardless of location. In the three years that our data cover, the shipper hires 5,701 workers for 6,664 positions. On average, these temporary assignments last only 30 days, with 83 percent of the assignments completed. Moreover, 20 percent of workers receive an "Excellent" review from their manager.

Because we observe the same worker in several jobs, the staffing agency data offer a valuable opportunity to decompose the effect of pay on retention into selection of better workers versus incentives within the same worker. Further because we see many firms hiring for the same jobs in this dataset, we can estimate the spillovers of one firm's wage setting on other firms' turnover.

## 3 Productivity Response to Higher Pay

The efficiency wage hypothesis suggests that higher pay may induce greater on-the-job performance due either due to fear of losing a well-paying job or the morale-boosting effects of higher pay. We use the productivity metrics used by the retailer to assess how boxes moved per hour and daily call volumes react to higher absolute and relative pay.

We explore whether higher absolute and relative pay increases productivity. An ideal experiment would randomize wages, allowing one estimate the causal relationship between productivity and wages:

$$\text{Productivity}_{it} = \beta_0 + \beta_{\$}\$_{it} + \epsilon_{it} \tag{1}$$

[11]When a firm hires through this staffing agency, they send to the staffing agency a description of the job their looking to fill and the pay rate. In select cases, the firm may ask the staffing agency for a particular worker with whom they have 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 staffer's wages, however, many refuse to negotiate on wages since they have set their advertised wages in relation to the wages of their full-time workers and they do not want to create strife. Of the workers who took a job through the staffing agency, 64 percent did not return in our period for a second job. But for a notable minority of workers, the Agency provided continuing stints of work: 5.5 percent of workers take at least five jobs with the Agency and are employed for a total of 263 days on average.

[12]Note that quits and bad endings are not mutually exclusive categories. One could quit and also receive a poor evaluation, for example.

where $\beta_{\$}$ is the coefficient of interest, capturing the effect of hourly wages, $\$_{it}$ on productivity. In the absence of this experiment, we rely on two natural experiments that arise from firms' wage setting practices. At the retailer we study, wages are often sticky. This leads to two types of variation that we exploit in our analyses. First, when pay is ultimately changed, it is often done in a large, discontinuous manner. This is the case in one of the retailer's warehouses. The ensuing large, abrupt change allows us to compare worker performance in the warehouse when pay is lower to when pay is higher.

Second, when pay remains constant, changes in prevailing wages in workers' local areas changes their outside options, and thus the relative values of their wage at the retailer. We leverage the fact that pay remains constant from 2018 onward for all customer service agents to estimate how productivity varies with the changes in relative wage in various different metropolitan statistical areas (MSAs).[13]

We find that in both the retailer's warehouse and among their on-site customer service agents, productivity increases when pay, or relative pay, increases. In the warehouse, when pay increases the number of boxes moved per hour by 7 percent (0.325/4.92 boxes per hour), reflecting an elasticity of 1.2. Among customer service representatives, paying $1/hour more than the local outside option increases calls taken per day by 7 percent, reflecting an elasticity of 1.12.

**Warehouse Productivity.** We use a one-time pay-jump in a single warehouse to investigate the effect of higher pay on productivity. In late July 2019, average pay was $16.20/hour among Level 1 workers in this warehouse.[14] One week later, the firm had increased the average pay to $17.39/hour and by the first of September, it was solidly at $18.00/hour, an 11% increase in pay over the course of a month. At the same time, pay remained essentially flat at other warehouses owned by the retailer.[15] Figure 1 depicts the pay bump at the treated warehouse along with relative pay constancy at other warehouses at the retailer.

The pay bump in question arose out of long-standing concerns about high turnover at this warehouse in particular according to the Field Director at the retailer. Indeed, in the quarter before the pay change, turnover at the treated location was nearly twice as high as in other warehouses. As Slichter (1919) observes, high turnover is often cause for raising pay. The Field Director presumed that turnover was higher at this warehouse than at other warehouses because (a) it is in a highly competitive local labor market where other firms' warehouses are located in very close proximity, and (b) the work can be especially grueling given that this

[13] This approach is similar to the approach in Hesford et al. (2020), though they use data from a single year, leveraging local variation in wages rather than over time.

[14] Level 1 workers exclude managers as well as contingent workers. They comprise the core of the team of movers in the warehouse.

[15] We noted that among all warehouse workers hired by the firm, pay was $19/hour. This includes those hired only during surge seasons as well as managers and others who command higher pay; under consideration in this section are Level 1 workers.

warehouse handles larger parcels than other nearby retailer warehouses (e.g., refrigerators or sofas rather than tea towels or books). The Field Director further confirmed that the nature of the work did not change around the pay jump and that it did not coincide with consumer holidays that could have affected work intensity. Correspondingly, there is no significant change in the demographics of those working at the warehouse (see Appendix Table B.1). Thus the treated warehouse differs in important ways from other warehouses but the timing of the pay jump is near random, and we do not see contemporaneous changes in the warehouse.

Table 1 describes the treated warehouse as compared to other warehouses in the quarter before the pay jump. The bulk of warehouse workers are men in their mid-30s working full time. On average, they have been with the firm for 10 months. Of the people working during the quarter before the pay jump, fully 63 percent of those in the treated warehouse and 50% of those in the other warehouses will ever leave the firm.

We compare the productivity in the treated warehouse before and after the pay change in an interrupted time series design. We scale our results so that they reflect the change in productivity that would arise from a single dollar's change in hourly pay. We use a two stage least squares approach. Our second stage is

$$\text{Productivity}_{i,t} = \alpha_1 + \beta_\$ \hat{\$}_{i,t} + \epsilon_{i,t} \tag{2}$$

and our first stage predicts wages based on being before or after the pay jump: $\$_{i,t} = \alpha_0 + \delta \mathbb{1}_{\text{Post}} + \nu_{i,t}$, where $\mathbb{1}_{\text{Post}}$ is an indicator for whether the observed day occurs after the pay change. $\beta_\$$ is our parameter of interest. Because our data includes daily observations of each worker, but the warehouse may be subject to shocks in any given week, we use two-way clustered standard errors, clustering at both the week and employee level.[16]

The bump in pay was just a rightward shift in the whole distribution of pay and thus did not affect relative pay within the warehouse or workers' dynamic incentives to strive for promotions.[17] Figure C.2 shows the distribution of wages in the week before the first pay change and the pay one month later. The standard deviation in pay beforehand is 1.18 and afterward is 1.21. Since this pay change occurred throughout the entry level workforce, but not the managerial workforce, one might worry that the pay differential between these two rungs was compressed. However, to date, no one has been promoted between those two levels of the warehouse, so we don't believe the constant managerial pay affected worker incentives.

Three metrics capture warehouse-level productivity: boxes moved per person-hour; boxes moved per *moving* hour, which removes from the denominator the time spent on non-moving

[16]While we use an interrupted time series design that includes only the treated warehouse, we have also run this as a difference-in-differences approach, and include those results in Appendix Table B.2.

[17]In contrast, increases in the minimum wage often compress the wage distribution of firms with low-wage workers, potentially tempering the workers incentives to climb the ranks of the firm.

activities like morning meetings or lunch; and the ratio of moving hours to total hours. We might expect the ratio of moving hours to total hours to decrease if the team works more seamlessly.

In the three months before the pay change, the treated warehouse moved an average of 4.9 boxes per hour, or 7.7 boxes per moving hour. The time-series of boxes moved per hour is shown in Figure 2, Panel B.

As shown in Table 2, Panel A, in the three months following the pay jump at the warehouse, boxes moved per hour increased by 0.328 off a base of 4.92 boxes moved per hour, an increase in productivity of 7 percent. This corresponds to an elasticity of 1.2. Our metric of boxes moved per *moving* hour is 0.316, an increase of 4 percent. Finally, we find an increase of 0.018 in the ratio of moving to total hours, which corresponds to an increase of 8.6 minutes of moving per person per day. This increase in productivity could come from a number of sources: it could arise from attracting and retaining more productive workers, from workers exerting more effort, or from workers collaborating more seamlessly in light of reduced turnover. We discuss mechanisms more in Section 7.

To contextualize this increase in a more generalizable way we can translate the increase in productivity into dollar terms. In the quarter before the pay jump, the retailer paid $3.29 per box moved. An increase of 0.336 boxes per hour thus represents an hourly savings of $1.10 for the retailer (see Section 6 for greater detail).

We again test our results with a placebo test. The placebo test examines whether the same increase in productivity may be found at other warehouses. In this instance, we compare to the "twin" warehouses that handle the same size parcel as the treated warehouse.[18] Since warehouses handling the same type of product are most likely to be hit by similar demand shocks and have similar interpretation of their units of productivity, considering the twin warehouses is most suitable. As Table B.4 shows, there is no increase in productivity in the twin warehouses.[19]

**Customer Service Productivity.** Our second context looks at customer service representatives at the same retailer. We use the fact that the retailer has maintained sticky wages over time; the retailer has not adjusted its entry-level wages for remote or on-site representatives since at least 2018, when our administrative data begins. The stickiness of the retailer's pay contrasts sharply with the changing nature of representatives' outside options: local pay increases over this period among other firms, and does so more steeply in some MSAs than others. Where pay in local customer service jobs rose faster, the retailer's sticky pay depreciated more compared

[18]The treated warehouse and its two twin warehouses handle large parcels the size of refrigerators or sofas. Other warehouses handle parcels the size of toasters or tea towels.

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

to the outside option. For example, in Tampa, FL, entry-level wages for customer service representatives rose considerably between 2018 and 2019, whereas in Sarasota, FL wages barely budged. We can consequently evaluate how the change in relative pay translates into a change in productivity among the representatives drawn from MSAs with faster and slower wage growth. This strategy allows us to difference away any fixed disparities in productivity across MSAs, while accounting for general trends within the retailer.

Particularly, we consider the first-difference specification, in which we relate the change in wages in an MSA from 2018 to 2019, $\Delta\$_{MSA,'18\rightarrow'19}$ to the change in productivity in the same location during the same time period:

$$\Delta\text{Productivity}_{MSA,'18\rightarrow'19} = \delta_0 + \delta_{\$}\Delta\$_{MSA,'18\rightarrow'19} + \zeta_{MSA}. \tag{3}$$

Since the retailer's pay is sticky between 2018 and 2019, the change in its relative pay is entirely driven by the growth (or stagnation) of the outside options in the MSA. To fully leverage the daily nature of our data and account for fluctuations in consumer demand within a given year, we focus on the analogous individual-level analysis, which allows us to include date-by-time-zone fixed effects.[20] While this approach utilizes individual data, it does not limit the changes to within an individual — thus, these estimates will reflect the changing selection of the retailer's representatives as well as the changing incentives they face. This individual-level approach yields nearly identical point estimates as the collapsed analysis but smaller standard errors, since it absorbs daily fluctuations in call volume.

Our coefficient of interest is $\delta_{\$}$, which reflects the relationship between a \$1/hr change in relative pay and the parallel change in the MSA's productivity between 2018 and 2019. For $\delta_{\$}$ to be an unbiased estimate of the effect of relative pay, other MSA-level changes that would affect productivity must be orthogonal to changes in the relative wage. In particular, changes in the pool of customer service representatives must be orthogonal to changes in the wage. While this is still a strong assumption, the short time-frame of our analysis makes it a credible one: over the span of a single year, it seems more plausible that fluctuations in the demand for customer service representatives would drive changes in wages than would changes in the supply, in terms of either quantity or quality. One advantage of these data is that they are extremely granular: the data track each person's daily calls.

We find that each \$1/hr increase in relative pay is associated with a 7.5% increase in call volume, 1.9 additional calls per day off of a based of 26 (see Table 2, Panel B). Intuitively, in MSAs

[20] We drop the 3.75% of customer service representatives (1.82% of days) with missing wage information. We further drop the 1.47% of representatives (1.55% of days) who are missing information on the local outside option for customer-service representatives — either because their address is missing or because too few customer service representatives work in the MSA for Emsi to construct an outside option. We exclude representatives in the 3 physical call-centers constructed in 2018 and 2019 — 21.6% of representatives (982 of 4551). We also exclude 1424 representatives (37.3%) hired in 2020, since our outside option information from Emsi is only available for 2018 and 2019. Finally, we exclude 170 representatives (7.1%) in MSAs with hires in only one of the two years.

where the retailer's sticky pay depreciated more substantially relative to the representatives' rising outside options, daily call volume fell between 2018 and 2019 compared to what would be expected. To contextualize this figure, the average customer service call costs the firm \$4.60, so an increase of 1.9 calls per day saves the firm \$8.74/day/worker.

Higher relative pay has limited but positive impacts on customer satisfaction, as shown in Table 2, Panel B, Column 2). This is reassuring to the extent that higher call volumes are not coming at the expense of less satisfactory customer experiences. However, the high rate of five-star evaluations and relatively little variation suggest that this metric of performance may not be particularly telling. By contrast, there is no statistically significant change in the share of absences that are unapproved by a manager in advance and thus difficult for the retailer to respond to.

Notably, relative pay seems to have limited impact on hours worked, total absent hours, and overtime hours, as detailed in Table B.5. Thus, such effects do not complicate the interpretation of our key metrics. It is unsurprising that relative pay does not appreciably move the needle on hours worked because relative pay does not necessarily relate to the purchasing power of the earnings of a marginal hour, which is typically the key consideration in extensive-margin labor supply choices. While one could tell stories where relative pay would still affect representatives' scheduling decisions — e.g. because representatives were balancing multiple jobs or balancing job search against hours worked — it is less obvious that relative pay should impact intensive-margin choice of how much labor to supply at one's chosen firm than that it should impact the extensive-margin choice of where to work.

We consider the heterogeneous effects of higher relative pay across workers with different baseline productivity. If less skilled workers at baseline are more at risk of termination or are less likely to be promoted than higher-productivity workers, one might expect the output of these workers to be more sensitive to the relative pay of the retailer.[21] Indeed, in Table B.6, Panel A, we find that call-volume effects are concentrated among representatives who are in the bottom two-thirds of call volumes in their first month after training at the retailer, as consistent with these representatives being more concerned about the possibility of termination or that they won't be promoted. Representatives in the top third of first-month daily call volumes have no appreciable change in their call volumes when their relative pay quasi-randomly changes.[22] This suggests that increased effort may account for some of the boost.

[21] That said, Chang and Gross (2014) find that more skilled workers are the ones to increase productivity in response to higher pay in the context of a pear factory, not lower-skilled workers.

[22] While one might worry about mean-reversion when comparing those who start in the top of the cohort to those who start in the bottom, we would expect this to be symmetrical, and in these analyses we do not find symmetrical mean reversion.

# 4 TURNOVER ELASTICITY

According to both estimates provided by the retailer and analysis of both warehouse and call-center data, turnover is costly, even for workers in jobs that are relatively routine and do not require an advanced degree. Internal estimates from the retailer suggest that training a new warehouse worker costs \$1849 or 5.5 percent of the average worker's annual income.[23] Our estimates of the cost associated with a new customer service worker amount to \$2990.[24] Moreover, objective metrics of productivity decrease when firms face turnover: fewer boxes are moved in warehouses and new customer service representatives answer fewer calls.

In weeks when workers leave a warehouse, the productivity in the warehouse decreases by 8 percent (0.75 fewer boxes per moving-hour off of an average of 9.14). Diminished productivity lasts three weeks. On average, each warehouse loses 2.8 workers per week.

It takes a new customer service representative about 6 months to reach the call volume of the average customer service representative who is answering calls on the same day within the same time-zone. As illustrated in Figure C.1, new representatives — who have just finished their 3 weeks of formal training — answer nearly 3 fewer calls per day, the equivalent of working one fewer hour per day for the firm.[25] This pattern persists when we consider a balanced panel of representatives who stay at the retailer for at least 6 months (in the dotted line), suggesting that selection alone is not driving the observed trajectory. Given the trajectory of learning, a higher rate of churn means that at any given time more workers will be new to the firm and have developed less skill in answering calls. This dynamic also suggests that retention of senior customer service representatives is more valuable than retention of junior ones because they will walk away with more human capital accumulated in the firm.

To estimate the elasticity of turnover with respect to pay, we use the same empirical approaches as we did when estimating the effects of pay on productivity (see Section 3): in the warehouse context, we exploit a large, quasi-random increase in wages; among customer service workers pay is sticky, creating heterogeneity in wages at the retailer compared to the changing outside option.

**Warehouse Turnover.** Using the same pay jump used to estimate the effect of pay on productivity in the warehouse context, we estimate the effects of pay on the warehouse's turnover.

[23] Internal estimates suggest that training costs \$689, reduced productivity and the associated overtime cost \$860, and other costs including advertising, background checks, employee badges cost \$300. This estimate does not include costs of recruiting and interviewing new candidates.

[24] These estimates include the decreased productivity over the course of training and are calculated based on the lower observed productivity times the average price the firm pays per call (\$4.60) as well as the use of trainers' time. It also includes \$300 in costs for advertising, background checks, and employee badges.

[25] Note that in the first six months, all service workers are given easy calls (e.g., change of address). More complicated calls are reserved for senior representatives.

Table 3, Panel A, which displays the results of estimating equation above, shows that increased pay decreases worker turnover. In the three months before the pay increase, out of every 100 workers in the warehouse, on average 13.4 would be leave per month – a monthly retention rate of 86.6 percent. Paying an additional \$1/hour decreases turnover by 2.5 individuals – a decrease in attrition of 18.7 percent, and an increase in retention of 2.8 percent. Since our point estimate captures the effect of a \$1/hour increase off of \$16.20/hour, our point estimate reflects an elasticity of turnover of 3.03. The overall effect on turnover is driven by voluntary quits, which decrease by 21.3 percent — 2.2 fewer quits relative to the base of 10.66 quits out of every 100 workers. There is no effect on being fired for performance.

We present results from a bandwidth of 3 months on either side of the pay jump. Table B.8 show the effects across one-, two- and three-month windows. We do not extend the window beyond 3 months after the pay jump because subsequent months include the holiday shipping season, which has its own impacts on warehouses independent of the late-summer pay bump (e.g., local demand shocks).

We test our results with a placebo test. We perform the same analysis on all other in-state warehouses. Since two of the three of these warehouses are within a 13-minute drive of the treated warehouse, if there were a shock to the local labor market for warehouse workers that were driving the decreased turnover, one would expect to see it decrease turnover in these warehouses as well. However, as Table B.9 shows, there is no decrease in turnover in other in-state warehouses.[26] An analogous experiment is simply to use a difference-in-difference regression, comparing the treated warehouse to the other in-state warehouses. Table B.7 shows there is not a substantive change.

**Customer Service Representatives' Turnover.** We likewise explore whether higher relative pay is associated with reduced turnover among customer service representatives. As in Section 3, we use the retailer's sticky wages alongside changes in the local pay for customer service representatives as in Equation 3 to assess the value of an additional dollar in relative pay to reach these estimates.

As shown in Table 3 Panel B, in places where the retailer's pay lost more ground to the outside option, monthly turnover increased more precipitously. Each \$1/hr loss in relative pay is associated with a 28 percent increase in monthly turnover — 1.2 percentage points off an average of 4.3 percent, reflecting an elasticity of 4.5.

We find that the reduction in turnover stems from both a reduction in quits — which we categorize as all terminations that are not fires — and a reduction in fires for poor performance or misconduct. The final two columns of Table 3 Panel B suggest that quits help fuel the decrease

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

in turnover.[27]

We again consider the heterogeneous effects of higher relative pay on turnover across workers with different baseline productivity. We hypothesize that the pay in local outside options is more important for more productive workers, who are better able to convert lucrative outside options into job offers that draw them out of the retailer. We test this hypothesis by investigating whether turnover rises more sharply for highly productive workers in those MSAs where the retailer's sticky wage loses more ground to the local alternatives. We find that higher pay is particularly effective at retaining representatives who start in the top third of daily call volume in their first month, as shown in Table B.6, Panel B. Each \$1/hr of additional relative pay reduces turnover by 44% for initial top performers, implying a turnover elasticity of 6.6. By contrast, for the rest of the representatives, the same increase in relative pay decreases turnover by 17%, implying a turnover elasticity of 2.76. This suggests that *selective retention* may be an important driver of increased aggregate productivity. Sandvik et al. (Forthcoming) also find that in response to a commission adjustment that effectively lowers wages, high-performing call-center workers are more likely to leave the company.than low-performing workers.

## 5 Recruitment Elasticity

Higher pay may be effective in recruiting more people and more talented people to a firm. An assumption often made in the new monopsony literature is that elasticities of recruitment and turnover are equal in magnitude (Manning, 2003). The notion that recruitment and turnover elasticities might be similarly sized is motivated by the idea that one firm losing a worker is balanced by another firm gaining a worker. In this model of job-to-job moves, recruitment and turnover elasticities are two sides of the same coin. This need not be true if workers also transition in and out of non-employment. For a specific occupation, it may also be violated if higher wages can more effectively retain workers in that occupation than recruit workers into the sector or vis-versa. Our estimates of the effect of higher pay on number of workers recruited allows us to assess this assumption.

To test the effect of higher pay on recruiting more people, we use the fact that both the retailer and employs individuals at the same wage, regardless of their location. This wage-setting strategy creates variation in the advertised wages relative to the local outside option. We test the effect of higher relative wages on the number of people recruited to the retailer, and the quality of the workers through the staffing agency. We find that when the retailer's advertised wages are \$1/hour higher than the local outside option, they recruit 23 to 30 percent more employees in the MSA, reflecting a recruitment elasticity between 3.2 and 4.2. Likewise \$1/hour higher

[27] While we don't find a statistically significant decrease in fires, such an effect would be consistent with managers' expectations for workers not fully adjusting to diminishing relative pay: this may be especially likely in contexts where the nominal pay at the firm does not change and instead the firm's pay only changes in relative terms. This contrasts with the first case study where the retailer actively changed its own pay and we see no changes in fires, which is consistent with no change in the alignment between performance and expectations.

wages are associated with a 5 percent increase in the likelihood of employing a worker rated as excellent by their manager.

The online retailer hires entry-level remote customer service representatives at \$14/hour throughout the United States, despite heterogeneity in the local pay for customer service representatives which is shown in Figure 4, Panel A. If this market were perfectly competitive, the retailer would not hire anyone from MSAs with higher pay. Likewise, if the relative wage were the only determinant of recruitment, the retailer would only attract representatives from the lowest paying MSAs. If instead, limited information or heterogeneous preferences contribute to recruitment, we would expect representatives to be drawn from MSAs with a range of pay. We find that the retailer hires throughout the country and higher relative pay increases recruitment in MSAs throughout the country.

The uniformity of the retailer's wage creates heterogeneity in the retailer's pay, relative to the representatives' local outside options. For example, in Dallas, TX, the retailer's pay is far below the average entry-level rate for customer service; by contrast, in Lufkin, TX, a couple hours from Dallas, the retailer's pay exceeds many of the less lucrative alternatives.

In relative terms, representatives in Lufkin are paid more than representatives in Dallas for the exact same work. We use variation in relative pay to draw inferences about pay's impacts on the number of recruits and the turnover and productivity of those recruits once at the retailer. We define relative pay at the retailer to be the difference between its uniform \$14/hr rate and the entry-level pay for customer service in the MSA according to Emsi.[28]

We consider the the relationship between relative wage in the MSA and the number of people hired in the MSA, in the cross section:

$$\#\ \text{Hired}_{MSA} = \beta_0 + \beta_{\$}(\text{Entry Relative Wage})_{MSA} + \Gamma(\text{MSA Controls})_{MSA} + \epsilon_{MSA}. \tag{4}$$

$\beta_{\$}$ reveals the relationship between relative pay and recruitment, holding fixed the nature of the work. MSA controls include the number of customer service workers in the MSA as well as the local unemployment rate in the MSA, as measured by the BLS's Local Area Unemployment.[29] To test the robustness of these estimates, we also include in the MSA controls other features of the MSA, including whether the retailer has a warehouse in that MSA and the number of people employed by the retailer in that MSA.

For $\beta_{\$}$ to offer an unbiased assessment of the effect of the retailer raising its own wage, determinants of recruitment other than pay and the size of the available pool of customer service

[28]We approximate the changing entry pay in the MSA according to the average of the 25th and 50th percentiles of the local customer-service wage distribution from Emsi.

[29]When MSA-level data on unemployment was not available, we used state unemployment levels.

workers must be orthogonal to pay. This assumption is plausible since we are looking at the number of individuals recruited (not the quality thereof), which may reasonably depend only on the number of available workers and the pay relative to the outside option. Note that this assumption allows relative pay to affect the selection of workers drawn from the pool of available workers — indeed, this is an important component of the return of higher pay from the perspective of the retailer, which we investigate in the context of the shipper.

As shown in Table 4, Panel A, every additional dollar the retailer pays above the average, local entry-level rate is associated with between 0.17 and 0.22 more customer service recruits in the MSA off of an average of 0.73. This translates into an elasticity of recruitment with respect to the wage of between 3.2 and 4.2.[30] When customer service representatives are considering different options at the recruitment stage, their decision-making seems heavily swayed by relative pay. Nevertheless, the fact that this elasticity is finite suggests that informational limitations or working preferences also seem to weigh in decision-making.[31]

We return to the assumption that the elasticity of recruitment equals the elasticity of turnover (Manning, 2003). Our elasticity estimates for turnover ($-4.48$) and recruitment ($3.19 - 4.22$) in the customer service context are similar in magnitude. Thus our estimates provide a measure of confidence in the assumption used in many parts of this literature.

## 6 The Return to Higher Pay

When considering how to procure sufficient effective labor, firms must weigh the benefits of higher pay against the cost of paying more. To better inform this debate, we estimate the returns to paying higher wages, using our estimates of turnover and productivity elasticities.[32] We find that in both the warehouse context, where estimates arise from a deliberate increase in pay, and the customer service setting, where estimates arise from keeping pay constant, productivity shifts are instrumental in offsetting the costs of higher wages.

**Warehouse Workers.** At the retailer's warehouse, a $1/hour increase in pay yields a positive net return from reduced turnover costs and increased warehouse efficiency.

The overall cost to the firm of increasing wages by $1/hour is actually $1.30/hour after associated taxes are included. So when we calculate gross returns on a $1/hour increase, we consider the cost to the firm to be $1.30.

[30] We estimate the recruitment elasticity of a specific firm rather than the job-to-job recruitment elasticity or job-from-non-employment elasticities that might reflect elasticities relevant at a market level. The elasticities captured here reflect those that are relevant to an individual firm.

[31] Our findings are consistent with reports from firms who have voluntarily raised wages. For example, Doug McMillon, the CEO of Wal-Mart, said after a wage hike in 2015 "[o]ur job applications are going up and we are seeing some relief in turnover" (Layne, 2015).

[32] A key drawback of these estimates is that they do not incorporate the recruitment elasticities since the retailer did not have estimates of the cost of recruiting a worker.

The gross returns from decreased turnover in the warehouse are $0.21 to $0.31. Internal estimate of the cost of training ($689), overtime while new workers get up to speed ($860), drug testing, badges and other overhead ($300) suggest that the retailer pays at least $1849 per new recruit. We find that an increase of $1/hour means the warehouse has 2.5 fewer workers per hundred employees leave each month, yielding a savings of (2.5 fewer turnovers x $1849) $4623 per month. If the firm had to pay 100 workers who worked 21 eight-hour days in a month, $1/hour more in order to realize this change, the cost to the firm would be $21,840 (100 workers x 168 hours/month * $1.30/hour). Thus, their gross return on a $1 investment would be $0.21 ($4623/$21,840). However, the data from our firm suggests that each worker was only working 116 hours per month, in which case the gross return would be $0.31.

The gross returns of increased productivity in the warehouse are $1.44. Based on hourly pay in the treated warehouse, in the quarter before the pay jump, the firm was spending $4.27 per box moved ($16.20 in hourly wages * 1.30 in taxes / 4.92 boxes moved per person-hour). Since the higher pay increased the warehouse level productivity by 0.336 boxes per person-hour, the gross return on a $1 pay increase, which costs the firm $1.30/hour, is $1.44.[33]

**Customer Service Representatives.** Among customer service representatives at this retailer, the gross return on a $1/hour increase in the relative wage is also positive.

Among customer service representatives, we find moderately small decreases in turnover from increasing relative pay. Each additional $1/hour is associated with a decrease in monthly turnover of 1.3 representatives out of 100. We estimate the cost of replacing a customer service representative to be $2,100, consisting of $1800 over the course of their 3-week training and $300 in badges and other administrative costs. According to these estimates, increased retention would thus reflect a savings of $2,730. To achieve these savings, 100 customer service representatives working 21 eight-hour days, would have to be paid an additional dollar (totaling $21,840, including benefits), implying a gross return of $0.13 ($2,730/$21,840).

The gross returns from an increase in productivity among customer service representatives is $1.56. Each call costs the firm roughly $6.57 ($15.96/hour · 1.30 in taxes · 8 hours/day / 25.27 calls/day). A higher wage increases call volume by 1.90 calls per day, so the return on an $8/day in wages ($10.40 in total costs to the firm) in higher wages is $12.48 ($6.57 x 1.90) – or $1.56 on the $1/hour investment.

This estimate may yet be an underestimate if firms are able to leverage higher pay's effect on recruitment. The magnitude of the recruitment elasticity suggests that higher pay might appreciably reduce the time it takes recruiters to find acceptable candidates.

[33] These figures do not include any changes in petty theft (which we assume would go down with a better paid workforce), damages (which we could imagine might go up due to increased congestion, or down due to a more practiced team working in the warehouse), or a slower warehouse footprint expansion. Thus, on balance, we suspect this is an underestimate of the returns to boosted productivity.

**Optimal Wages.** One could look at these estimates and conclude that this Fortune 500 retailer did not set wages optimally. Two items are worth noting. First, the estimates in the warehouse reflect the fact that the firm recognized that the wages were suboptimally low and raised them accordingly. From this perspective, it is not surprising that our estimates would suggest that it was profitable for the firm to take the steps that it did.

Second, more curious, perhaps, is that our estimates in the customer service context, which arise from sticky wages, should also suggest that wages could be profitably raised. But this firm uses a sticky wage strategy, in which firms set prices intending to keep them fixed for some time — a strategy that could make sense if there are substantial adjustment costs. The sticky wage strategy acknowledges that before wages are adjusted, they may be suboptimal. So while we do highlight that wages need to be adjusted, this does not mean that the firm is not, therefore, rational.

We can also use our wage equation from Section 1 to estimate what pay should be in the customer service context according to our model.[34] If we assume that marginal productivity, $p(w)$ was captured to a first order by sticky wages, we find that pay would only have been marginally higher at \$15.72 rather than \$15.60. [35] Of course, this is an out-of-sample estimate, and thus makes several assumptions, including constant elasticities and productivity responses.

## 7 Mechanisms: Selection and Behavioral Responses

A key question underlying our results is the mechanisms by which changes in pay affect measured turnover and productivity. Does raising wages for existing workers incentivize higher performance, or is the key advantage of higher pay attracting and retaining more reliable or productive workers?

To understand what share of the effects come from the same worker facing different wages and adjusting their behavior accordingly, we leverage data from a staffing agency. While the dataset is distinct from the retailer data, the staffing agency places many workers in similar warehouse jobs, allowing us to consider the effects of pay on this occupation. Because we observe the same worker in multiple, comparable jobs with different pay, we can see what percent of the reduced-form relationship is present when the same worker faces different pay rates. We find that over half of the turnover reduction and productivity increase arises from behavioral responses of the same worker facing different wages.

For this analysis, we focus on the sample of warehouse workers placed in temporary jobs by the

[34]We refrain from estimating optimal wages in the warehouse context since the recruitment elasticity can only be estimated in the customer service setting.

[35]$w^* = \frac{p(w)}{1+\frac{1-p'(w)}{\epsilon_{R,w}-\epsilon_{T,W}}} = \frac{15.60}{1+\frac{1-1.06}{3.19+4.48}} = \frac{15.60}{1-\frac{0.06}{7.67}} = 15.72$

staffing agency, since this offers a relatively homogeneous set of jobs. The sample is described in the first column of Table 1, Panel B. We begin by estimating the reduced-form relationship between pay and performance:

$$\text{Turnover}_{ij} = \beta_0 + \beta_\$ \cdot \text{Pay}_j + \mu_{oc} + \mu_{dct} + u_{ij}. \quad (5)$$

where $\beta_\$$ captures the relationship of interest. To isolate the pay premium of the firm above the local market, we include occupation-by-commuting-zone fixed effects and industry-by-commuting-zone-by-month fixed effects that together soak up variation in the local labor market. Our estimates are thus identified off of variation in hourly pay across firms and workers in the same local labor market and industry.

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

$$\text{Turnover}_{ij} = \psi_\$ \cdot \text{Pay}_j + \underbrace{\mu_i}_{\text{Worker FE}} + \mu_{oc} + \mu_{dct} + v_{ij}.$$

We estimate both of these specifications for the sample of workers with multiple jobs through the agency, since these workers are used to identify the within-worker effect of higher pay. Table 6 presents the results of this analysis. We find that an additional dollar of pay increases job completion by 2.6 percentage points, off a base of 40 percent completion. This is equivalent to an elasticity of 0.72. We estimate that 83 percent of that effect arises within the same worker. A similar share of the quits may be attributed to behavioral responses.

We can also use "Excellent" evaluations as a metric of whether the firm is satisfied. While not the same as on-the-job productivity, it is nevertheless a useful metric of worker performance. In this case, we find that 50% of the increase associated with higher pay arises within the same worker.

## 8 Gender Differences in Elasticities

Elasticities of retention, turnover, and productivity may differ by gender.[36] Different elasticies would imply that workers of different genders may command different wages, an idea that goes back as far as Robinson (1933). We investigate whether there is indeed heterogeneity in the genders' responsiveness to pay and what this implies about wage gaps.

In the context of customer service agents, we find that labor supply elasticities may explain a $0.08 gap in pay, but that women's productivity is considerably more responsive to pay, suggesting that women should be paid *more* than men.

[36] We refer to differences by gender because the data received from the retailer delineates gender. We guess that this field then documents self-reported gender and that we lack information on workers' sex.

Of course, in the face of anti-discrimination laws, this exercise does not capture what firms can legally carry out in setting wages. However, even in the presence of anti-discrimination laws, gender differences in elasticities can help explain occupation-level gaps in pay for particularly gendered occupations.[37] Moreover, the exercise may be useful even within an occupation. As long as an individual firm is satisfied by certain group of workers, they can adjust wages, knowing that they may end up with a workforce that is mostly composed of one gender. For instance, if men have greater labor supply elasticities, a firm that doesn't mind an all-female workforce can simply keep pay low. Thus at an occupation level, we can still see a wage gap emerge when firms stay within the bounds of the law.

Using the wage expression from Section 1, we can unpack the implications for the gender wage gap by taking the ratio of female to male optimal wages. To put this in conversation with prior work, we assume a constant production function, no productivity gains, and that $\epsilon_{R,w} = \epsilon_{T,w}$— an assumption that we validate in the customer service context— for both male and female workers.

$$\frac{w_f}{w_m} = \frac{\frac{2\cdot\epsilon^f_{T,w}}{1+2\cdot\epsilon^f_{T,w}}}{\frac{2\cdot\epsilon^m_{T,w}}{1+2\cdot\epsilon^m_{T,w}}}$$

We explore the degree to which turnover and productivity elasticities differ by gender among customer service representatives.

**Elasticities of Turnover.** When deciding whether to leave the retailer, men are more elastic than women in customer service.An additional dollar of pay reduces turnover by over 40 percent for male customer service representatives, implying a turnover elasticity of 6.6. Female customer service representatives' response to higher relative pay is economically small and statistically indistinguishable from zero. These findings are consistent with findings from Wiswall and Zafar (2018), among others, that women prefer job stability whereas men prefer jobs with higher earnings growth.

These results are in sharp contrast to the findings in Caldwell and Oehlsen (2018), who suggest there are minimal gender differences in daily labor supply responses among Uber-drivers. Our setting of customer-service representatives contrasts with Caldwell and Oehlsen (2018)'s setting in a few key ways. First, most customer service representatives have full-time jobs at the retailer; thus, our estimates reflect the frictions that keep workers at their primary employers, rather than the rigidities in their decision-making about gig work. Second, in contrast to the male-dominated setting of Uber-driving, our setting is one in which women make up the majority — suggesting that the women in this occupation may be less strongly selected than those

[37]Indeed, in 2019, twice as many women in the Current Population Survey's Annual Social and Economic Supplement were customer service representatives. Yet men had annual incomes above $40,000, whereas women had annual incomes around $33,000, representing a 20-cent gap.

in Uber-driving. Conversely, men in the customer service may be selected.Finally, our setting may have more workers of child-bearing age than Uber, where the gender differences appear most pronounced. Our results are consistent with Hirsch et al. (2010) who find in matched employer-employee data from Germany that women's labor supply elasticities to the firm are smaller than those of male workers.

We are underpowered to estimate elasticities of recruitment by gender since there are only 93 male remote customer service workers. As such, when we trace out implications for the gender pay gap, we use the assumption—common in the new monopsony literature—that $\epsilon_{R,w} = \epsilon_{T,w}$, which we found to be true in the aggregate (see Sections 4 and 5). Nevertheless, results can be found in Appendix Table B.10

The wage equation derived in Section 1 helps us trace out implications for the gender pay gap of our different elasticities. We can take the ratio of female to male wages understand how labor supply elasticities would act on the wage gap.

$$\frac{w_f}{w_m} = \frac{\frac{2\cdot\epsilon^f_{T,w}}{1+2\cdot\epsilon^f_{T,w}}}{\frac{2\cdot\epsilon^m_{T,w}}{1+2\cdot\epsilon^m_{T,w}}}$$

Substituting in our elasticities:

$$= \frac{\frac{2\cdot -3.47}{1+2\cdot -3.47}}{\frac{2\cdot -6.63}{1+2\cdot -6.63}}$$

$$= \frac{0.874}{0.929}$$

$$= 0.94$$

Thus looking simply at the difference in extensive labor supply elasticity, we would end up with a slight wage gap, with women earning 94 cents for each dollar men earn if the law did not prohibit gender-based wage discrimination.

**Elasticities of Productivity.** We explore whether female and male customer service representatives are differentially responsive to pay in their productivity. We find that when relative pay is higher, female workers' productivity increases by 9 percent (2.3 off of a base of 25.58 calls per day) whereas male workers' productivity increases up by just 5 percent (1.24 calls off of a base of 24.41 calls per day), statistically indistinguishably from zero with our sample size. For women, this reflects an elasticity of 1.41, while for male workers it represents an elasticity of 0.8.

To translate this into dollar differences, consider that the retailer pays $6.41 per call. So the male increase of 0.8 calls per day amounts to a savings of $5.13/day or $0.64/hour. In contrast, the female increase of 2.3 calls/day amounts to a savings of \$14.74/call or \$1.84/hour. This would suggest that if productivity responses to pay were incorporated into wages, women should have *higher* wages than men.

# 9 BENCHMARKING

Our primary estimates arise from a Fortune 500 firm. To benchmark our estimates in another context, we turn to the staffing agency. The staffing agency has one client firm—a shipper—that hires thousands of warehouse workers, all at \$17/hour, regardless of their location.[38] Local pay for temporary warehouse workers varies considerably (see Figure C.4, Panel A), allowing us to leverage differences in the shipper's pay relative to the outside option in a particular commuting zone and relate this to the turnover and manager-satisfaction in the commuting zone.

We estimate a regression relating relative pay and the outcomes of interest:

$$\text{Turnover}_{ijt} = \beta(17 - \overline{\$}^{cz}_{ijt}) + \gamma D + \mu_t + \epsilon_{ijt}$$

where $\overline{\$}^{cz}_{ijt}$ reflects the average hourly pay rate for other warehouse jobs in the season and commuting zone in the staffing agency.$D$ is a vector of expected duration variables up to a quartic in case workers are less likely to complete a longer job. We calculate the expected duration based on the duration that other such jobs at that firm tend to last, which reflects the information that recruiters would be able to give to potential hires. We include season fixed effects $\mu_t$ to address the fact that work and work availability may differ season by season in warehouses. We cluster our standard errors at the commuting-zone level in case commuting-zone shocks to the labor market affect workers' on the job performance.[39]

We find that higher relative pay increases job completion rates by 1.2 percentage points of a baseline completion rate of 83 percent. The results are summarized in Table 7 and are shown graphically in Panels B-C of Figure C.4. We estimate that an extra dollar in relative pay is associated with an 8 percent decrease in quits (-0.48 percentage points off of a base quit rate of 5.9 percent). We see no change in the evaluations proffer by on-site managers. If manager are aware of the outside option and how the shipper's pay relates, they may change their baseline

[38]Notably, the shipper is not hiring the warehouse workers to test them out for a permanent position: of the thousands of individuals hired by this firm as warehouse workers, only 16 are offered a permanent position. As such, the possibility of individuals exerting more effort with an eye toward a permanent offer is effectively shut down.

[39]We construct the sample by limiting to season-commuting zone pairs that have more than 10 assignments from the shipper during peak seasons where the shipper hires in more than one commuting zone. We further eliminate the 267 assignments (3.2% of the 8,477 assignments) at the shipper that are hired at a different rate, since these are different jobs. Of the 8,477 temporary assignments that the shipper secures through the staffing agency, 75% are retained in our sample. To construct the outside option, we include all other warehouse jobs begun in the same season and in the same commuting zone filled through the staffing agency. The comparison between the jobs at the shipper and the outside options can be seen in Panel B of Table 1.

expectations, which would show up in a null result.

**Quality of workers recruited**

In addition to benchmarking the job completion results, we can enhance the recruitment results from Section 5. The shipper hires a fixed number of workers in each location, so we cannot look at how the number of people recruited responds to the relative wage. However, we can estimate how the quality of workers secured responds. Through the staffing agency, we can see workers' reviews from their on-site managers, which we use as a proxy for the quality of the worker. Among all warehouse workers, only 13 percent of workers receive an "Excellent" review.

To evaluate the quality of the workers placed in a job, we construct a prediction of workers' evaluations based on their prior assignments, job evaluations, and job endings. Only 13 percent of workers are predicted to earn an excellent evaluation, and another 5 percent are predicted to earn a poor evaluation. An additional 44 percent are predicted to have adequate reviews – neither excellent nor poor. There are an additional 39 percent who are new workers, and thus do not have evaluations from which to predict their quality.

As in Section 5 we estimate the likelihood of securing a worker predicted to be excellent on relative pay, controlling for the number of excellent workers in the staffing agency in that commuting zone as well as for season fixed effects, which might alter the quality of the pool of workers. Standard errors are clustered at the commuting zone level.

As shown in Table 7, Panel B, we estimate that an additional dollar in relative hourly pay means the shipper is 6.7 percent (0.87 percentage points off a base of 13 percent) more likely to have a worker to is predicted to be reviewed excellently and 2 percent (0.75 percentage points off a base of 39 percent) less likely to have a new worker. There is no statistically significant difference in workers who are predicted to be poor.

**Spillovers to Local Firms** A final exercise that we can perform with the data from the staffing agency is unpacking the mechanisms. Since a portion of the boost in relative completion rates may be attributed to sorting more reliable workers to higher paying firms (see Section 7), we can ask whether other local firms suffer the consequences. That is, when the shipper is hiring, we can assess whether other firms that are hiring for warehouse workers at the same time find that they have lower job completion or performance. To do this, we use a difference-in-differences strategy, comparing the change in job outcomes before and during the holiday season when the shipper hires, both in commuting zones where the shipper is present and just outside those commuting zones. To assess the impact of each additional dollar of relative pay, we also estimate a continuous difference-in-differences regression. The details of our approach are in Appendix 12.

As seen in Appendix Table A.1, when the shipper is hiring at all, quits at rival firms increase by 12.4 percentage points off a base of 28 percent. An additional dollar of pay over the outside option is associated with a 1.45 percentage point increase in quits. We also assess bad endings–namely when workers be terminated for performance or attendance, or to receive a "Poor" Evaluation. When the shipper is hiring, bad endings at rival firms increase by 8 percentage points, off a base of 24 percent.

If selection is at work, when the shipper is hiring rival firms may also hire lower quality workers. We do not find a statistically significant effect on worker quality. Such outcomes are consistent either with effects arising through worker behavior or with seeing an uptick in high quality workers during the holiday season anyway such that even lower paying firms can access them.

$$Y_{it} = \alpha_0 \mathbb{1}^{cz}_{ijt} + \alpha_1 + \mathbb{1}^{month}_{ijt} + \beta(T \cdot \mathbb{1}^{cz}_{ijt} \cdot \mathbb{1}^{month}_{ijt} \cdot \overline{\text{Pay}}_{ijt}) + \mu_T + \epsilon_{ijt} \tag{6}$$

We use a simple difference-in-differences approach, where we fully interact the specification with year to ensure that we do not put negative weight on any of our comparisons, in keeping with the recent literature on two-way fixed effects models (e.g., Goodman-Bacon, 2018; Abraham and Sun, 2018; Imai and Kim, 2019; de Chaisemartin and D'Haultfoeuille, 2020).

# 10 CONCLUSION

In this paper we present evidence that warehouse workers and customer service representatives are responsive to wages, not only with regard to recruitment and turnover, but also with regard to their on-the-job productivity. We estimate recruitment elasticities in excess of 3, turnover elasticities between -3 and -4.5, as well as productivity elasticities in excess of one. The productivity response to higher pay yields a net positive return. We estimate that 80 percent of the improvement in turnover arises from workers' behavioral responses to higher pay.

This paper also estimates gender differences in these elasticities. We find that while women's labor supply is slightly less elastic than men's, women increase their productivity in response to higher pay more than do men. The gender difference in labor supply elasticity is important because it suggests that when the concentration of firms is used as a measure of monopsony power, we may underestimate firms' power to set female wages. The productivity response is particularly intriguing because it suggests that if wage discrimination were not illegal, women should be paid more than men in this context. It also suggests that understanding workers' productivity responses will be particularly important in gaining a more comprehensive understanding of gender pay gaps.

Elasticities of labor supply with respect to wages are relevant for public policy choices about minimum wage and Earned Income Tax Credits (EITC). When labor-supply is highly elastic and labor-demand is highly inelastic, the benefits of work subsidies like the EITC tend to accrue to the firm rather than the worker. Yet, in such a world, increases in the minimum wage will increase wages without decreasing employment. To the extent that our results are often measuring the difference between a firm's pay and workers' outside options, minimum wage changes change the outside option. If a minimum wage increase compresses the wage distribution, workers who were paid above the minimum wage will have less difference between their wages and their outside options. Our results suggest that firms can capture lower turnover and higher productivity by raising wages. Thus our paper suggests that in the wake of a minimum wage change, firms may seek to raise wages even for workers who were not paid the minimum wage.

Our results have limitations and leave a number of questions unanswered. While we provide suggestive evidence for the mechanisms underpinning our results, we cannot perfectly estimate the relative contributions of attracting better workers versus eliciting greater effort from the existing workforce. This is a significant shortcoming insofar as it means we are unable to make general equilibrium predictions, such as what might happen if wages were raised universally in a given geography. If the effect we document is coming entirely from greater effort, then all firms might see an increase in productivity when all workers' pay is raised. If higher pay generates greater productivity only because better workers gravitate toward higher-paying firms, then a global increase in pay will not induce greater productivity since no sorting would occur.

Moreover, if on-the-job productivity increases with pay because reduced turnover itself increases output, then the resultant question is whether turnover is a function of relative pay or absolutely higher pay. We find that turnover is responsive to both relative and absolute pay. A more thorough investigation into the mechanisms would be valuable.

Our results that explore the spillovers of high-paying firms on local rival firms can only document the effect in terms of worker turnover and firm satisfaction, not in terms of objective measures of productivity. We would love to know whether work is slower at rival firms when a local firm raises pay, and view this as an important avenue for future work.

While there is ample room for additional research, this paper contributes by (a) estimating turnover and recruitment elasticities with respect to wages among warehouse and customer service workers, (b) bringing objective productivity metrics to bear on the question of how pay affects workers, (c) providing suggestive evidence about the relative contributions of selection and workers' behavioral responses to pay and about the spillovers on other firms that can arise from selection, and (d) estimate gender-specific responses to pay.

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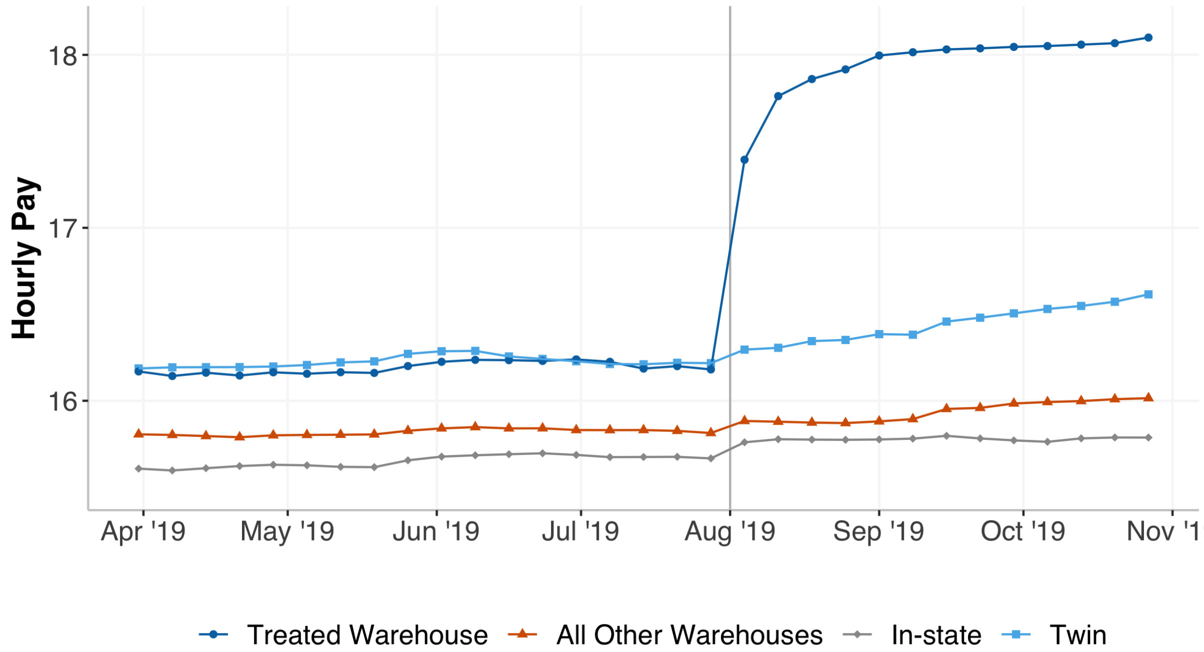
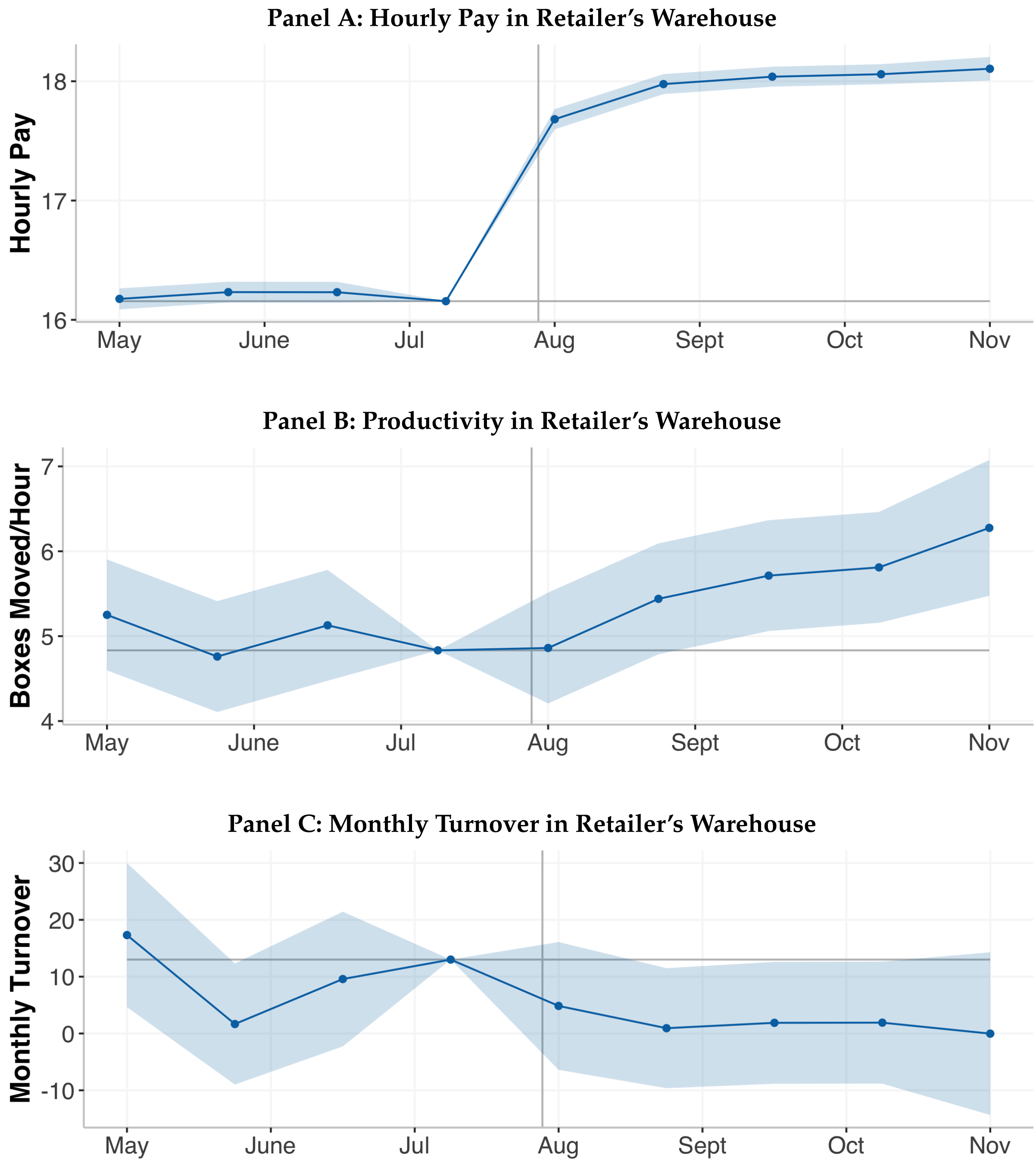
# 11 FIGURES

FIGURE 1: PAY CHANGE IN TREATED AND UNTREATED WAREHOUSES



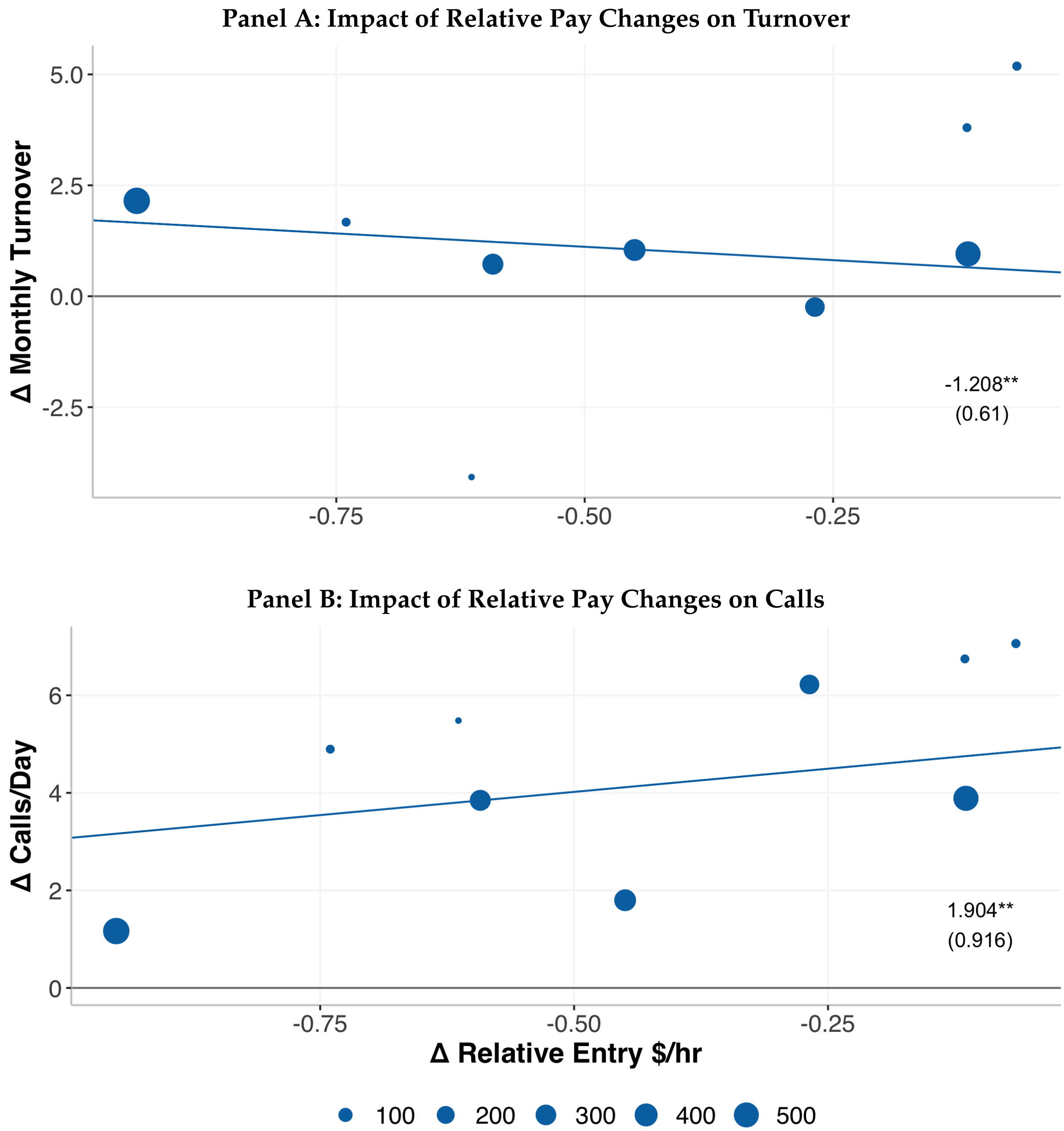

*Note:* This figure shows the average weekly pay within warehouses in 2019. Average pay for all other retailer warehouses are denoted in orange triangles, for warehouses in the same state as the treated warehouse in grey diamonds, and for "twin" warehouses that handle the same type of package in blue squares.

FIGURE 2: IMPACT OF WAREHOUSE WAGE INCREASE ON TURNOVER AND PRODUCTIVITY (NUMBER OF BOXES MOVED/HOUR)




*Note:* This figure plots averages within three week bins around August 2019 in the treated warehouse. Panel A reports the change in average hourly pay among warehouse workers. Panel B reports the turnover rate in the warehouse. Panel C shows the average warehouse-level productivity (total boxes moved in a week/ total hour worked in a week). Standard errors are clustered at the employee- and week- levels. The shaded areas display 95 percent confidence intervals. The coefficients plotted are unscaled, in contrast to the estimates in the regressions displayed in Panel A of Tables 3 and 2, which are scaled by size of the pay jump so that point estimates reflect changes per $1/hour increase.

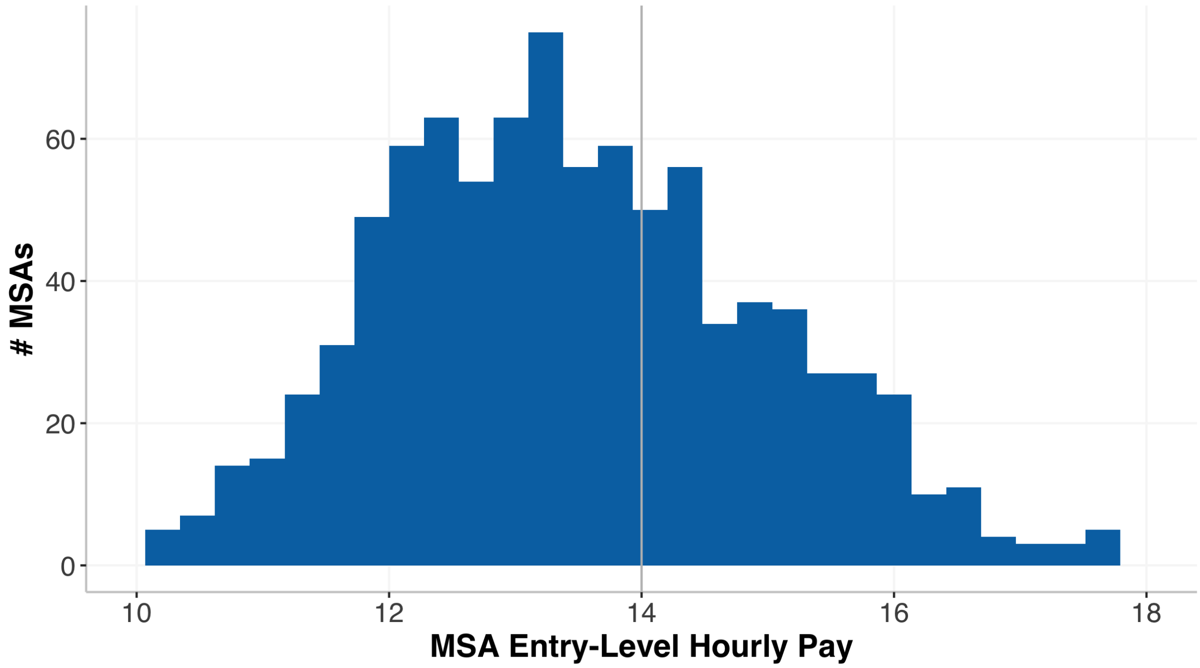
FIGURE 3: CHANGE IN RELATIVE PAY AMONG RETAILERS' CUSTOMER SERVICE AGENTS AND CHANGE IN TURNOVER AND PRODUCTIVITY



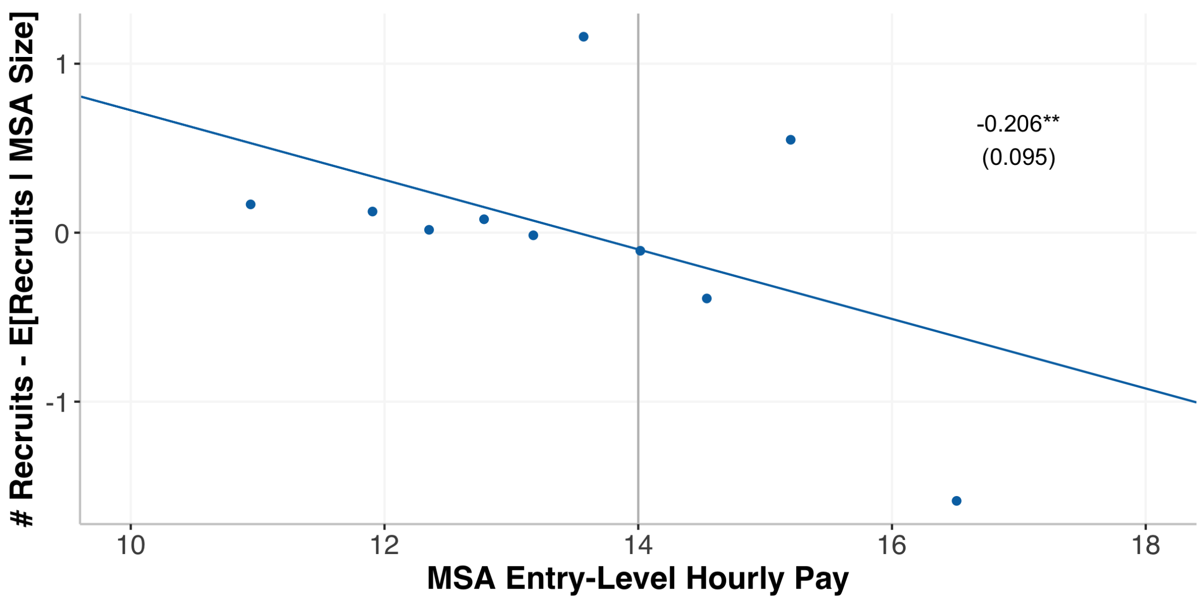

*Note:* These figures show the change in relative pay in each MSA from 2018 to 2019 along the x-axis. Panel B shows the change in monthly turnover for each MSA from 2018 to 2019 along the y-axis. Panel C shows the change in daily call volume for each MSA from 2018 to 2019 along the y-axis. We only plot the MSAs with more than 15 employees in them though the regression lines include all MSAs. The size of the dot shows how many employees are in each MSA. Slopes are reported with standard errors in parentheses. The regression form is displayed in Table 3, Panel B and Table 2, Panel B, respectively. Figure C.3 shows the raw pay changes from 2018 to 2019 for the displayed MSAs.

Figure 4: Impacts of Relative Pay on Recruitment of Remote Customer Service Representatives

**Panel A: Entry-Level Wages for Customer Service Representatives By MSA**




**Panel B: Impact of Relative Pay on Recruits**




*Note:* Panel A plots the entry level hourly wages for customer service representatives by MSA, weighted by the number of customer service representatives in that MSA, across the United States in 2018. Panel B presents a bin-scatter of the number of recruits in a given MSA relative to what would be expected based only on the size of the MSA, as a function of the entry level pay for customer service representatives in the MSA. The grey, vertical line shows the retailer's offered wages; the blue line shows the regression line, controlling for the the local unemployment rate and the number of customer service workers in the MSA as a quartic. The standard error on the slope is shown in parentheses. The regression form is displayed in Table 4.

## 12 TABLES

TABLE 1: SUMMARY STATISTICS

**Panel A: Warehouse and Customer Service Samples from an Online Retailer**

| | Warehouse | | | Customer Service | | |
|---|---|---|---|---|---|---|
| | Treated | In-State | Twin | All CSR | All Remote | Sticky Pay |
| $/hour | 16.20 | 15.66 | 16.24 | 15.83 | 14.35 | 16.02 |
| Entry CSR $/hr in MSA | | | | 13.81 | 14.46 | 13.52 |
| % Turnover/Month | 13.40 | 9.30 | 5.15 | 4.40 | 6.59 | 4.31 |
| % Quit/Month | 10.66 | 7.50 | 4.05 | 3.57 | 5.55 | 3.50 |
| % Fired/Month | 2.02 | 1.24 | 0.70 | 0.62 | 0.83 | 0.58 |
| % Turnover/Month in MSA | | | | 6.25 | 6.96 | 6.27 |
| Days in Co | 276.32 | 314.17 | 235.17 | 325.91 | 172.21 | 333.89 |
| % Female | 21.89 | 52.50 | 20.31 | 70.75 | 88.58 | 69.25 |
| Age | 36.09 | 37.55 | 33.59 | 33.82 | 36.48 | 33.65 |
| Boxes/Hour | 4.92 | 6.51 | 2.76 | | | |
| Boxes/Moving Hour | 7.69 | 10.48 | 5.16 | | | |
| Moving/Total Hours | 0.64 | 0.62 | 0.55 | | | |
| Calls/Day | | | | 25.11 | 25.32 | 25.27 |
| Calls/Hour | | | | 3.27 | 3.28 | 3.30 |
| Absent Unapproved Hrs | | | | 0.43 | 0.43 | 0.43 |
| # Employees | 368 | 690 | 896 | 4,551 | 593 | 3,061 |
| # Days | 20,824 | 48,401 | 59,994 | 1,289,980 | 115,685 | 854,614 |

**Panel B: Temporary Warehouse Positions from a Staffing Agency**

| | All Warehouses | Shipper | Local Warehouses |
|---|---|---|---|
| $/Hour | 11.74 | 17.00 | 12.51 |
| % Job Completed | 44.15 | 83.57 | 41.72 |
| % Quit | 31.84 | 5.85 | 33.98 |
| % Bad Ending | 27.93 | 6.15 | 30.64 |
| % Excellent Eval | 13.15 | 20.87 | 6.45 |
| Expected Duration | 102.87 | 30.70 | 106.02 |
| # Workers | 140,664 | 5,701 | 32,009 |
| # Assignments | 222,904 | 6,664 | 45,454 |
| # Commuting Zones | 374 | 83 | 83 |
| # Firms | 3,950 | 1 | 1,448 |

*Note:* Panel A shows data data from a Fortune 500 online retailer. Statistics are aggregated from daily data, meaning that workers who are present longer have greater weight than workers who are present for a short period. The first three columns display information on the retailer's warehouses from May through July 2019. The Treated column show averages within a single warehouse that our analyses focus upon. The In-State column shows averages for the three warehouses in the same state as the Treated warehouse. The Twin column shows averages for two warehouses that handle the same type of parcel as the Treated warehouse. The next three columns display information on the retailer's customer service agents from 2018-2019. The All CSR column displays information about all customer service representatives. The All Remote column displays information about customer service representatives who were hired to work remotely. The Sticky Pay column displays information about the subsample of customer service representatives who live in metropolitan statistical areas (MSAs) that have retailer employees in them both in 2018 and 2019. Panel B 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 averages for a particular client of the staffing agency, a shipper, that hires many warehouse workers. The third column shows other warehouse hires through the staffing agency in the same commuting zones as the shipper.

TABLE 2: HIGHER PAY'S EFFECTS ON PRODUCTIVITY

**Panel A: Pay's Effects on Productivity in the Warehouse**

| | First Stage | Boxes/Hr | Boxes/Moving Hr | Moving/Total Hrs |
|---|---|---|---|---|
| Post | $1.746^{***}$ | | | |
| | (0.054) | | | |
| \$1/hour | | $0.336^{***}$ | $0.309^{**}$ | $0.017^{**}$ |
| | | (0.088) | (0.138) | (0.008) |
| Productivity $\epsilon$ | | $1.1^{***}$ | $0.65^{**}$ | $0.43^{**}$ |
| | | (0.29) | (0.29) | (0.2) |
| Pre Jump Mean | 16.2 | 4.93 | 7.7 | 0.64 |
| Observations | 26 | 26 | 26 | 26 |

**Panel B: Pay's Effects Among Customer Service Representatives**

| | Daily Call Volume | Satisfaction (out of 5) | % of Absences Unapproved |
|---|---|---|---|
| Entry Relative \$1/hr | $1.904^{**}$ | $0.012^{***}$ | −1.642 |
| | (0.916) | (0.003) | (4.437) |
| Elasticity | $1.2^{**}$ | $0.038^{***}$ | −0.383 |
| | (0.58) | (0.01) | (1.036) |
| Mean \$/hr | 15.96 | 15.96 | 16.11 |
| Dependent Mean | 25.27 | 4.89 | 68.31 |
| MSAs | 41 | 41 | 41 |
| Workers | 2687 | 2687 | 2782 |

*Note:* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Panel A shows the interrupted time series estimates of the effect of pay on productivity in the retailer's treated warehouse. The first column shows the outcome of a regression of pay on an indicator for after August 2019. The next three columns show the outcome of a two-stage-least-squares regression so that coefficients may be interpreted as the effect from a \$1/hour pay increase. The next three columns show the effect of higher pay on three metrics of productivity: boxes moved per hour, boxes moved per moving hour (which eliminates from the denominator lunch time, meeting times, etc), and the ratio of moving hours to total hours. The estimates reflect warehouse level data and a 3-month bandwidth on either side of the pay jump. Appendix Table B.8 shows robustness to different bandwidths. Standard errors are clustered at the week and individual levels.

In Panel B, we show how changes in the relative pay of customer service workers in MSAs from 2018 to 2019 relates to the change in productivity in those MSAs from 2018 to 2019. We show three metrics of productivity: daily call volume, customer satisfaction (on a scale from 1 to 5), and the percent of absences that are not approved in advance by one's manager. Standard errors are clustered at the MSA-level.

TABLE 3: HIGHER PAY'S EFFECTS ON TURNOVER

**Panel A: Turnover Effects In the Retailer's Warehouse**

| | First Stage | Monthly Turnover | Quits | Fires |
|---|---|---|---|---|
| Post | $1.755^{***}$ | | | |
| | (0.079) | | | |
| \$1/hour | | $-2.504^{**}$ | $-2.270^{**}$ | 0.061 |
| | | (1.255) | (0.957) | (0.563) |
| Elasticity | | $-3.03^{**}$ | $-3.45^{**}$ | 0.49 |
| | | (1.52) | (1.45) | (4.53) |
| Base Mean | 16.2 | 13.4 | 10.66 | 2.02 |
| Workers | 514 | 514 | 514 | 514 |
| Observations | 50,478 | 50,478 | 50,478 | 50,478 |

**Panel B: Turnover Effects Among Customer Service Representatives**

| | Monthly Turnover | Quits | Fires |
|---|---|---|---|
| Entry Relative \$1/hr | $-1.208^{**}$ | $-0.821^{**}$ | $-0.386$ |
| | (0.610) | (0.417) | (0.259) |
| Elasticity | $-4.484^{**}$ | $-3.522^{**}$ | $-10.687$ |
| | (2.264) | (1.787) | (7.159) |
| Mean \$/hr | 16.02 | 16.02 | 16.02 |
| Dependent Mean | 4.31 | 3.74 | 0.58 |
| MSAs | 42 | 42 | 42 |
| Workers | 3061 | 3061 | 3061 |

*Note:* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Panel A shows the relationship between a wage increase in a warehouse and monthly turnover in the warehouse. The first column regresses pay on an indicator for after August 2019. The subsequent columns show coefficients scaled by the first stage so that coefficients can be interpreted as the effect of a single dollar of higher pay. The next three columns show different measures of turnover: any termination, voluntary quits due to finding a better job, and fires. The estimates reflect warehouse level data and a 3-month bandwidth on either side of the pay jump. Appendix Table B.8 shows robustness to different bandwidths. We use two-way clustered standard errors at both the week and individual worker level. Panel B shows the relationship between change in relative pay in a metropolitan statistical area (MSA) from 2018 to 2019 and change in turnover in that MSA from 2018 to 2019. Standard errors are clustered at the MSA level.

TABLE 4: EFFECTS OF HIGHER PAY ON RECRUITMENT

| | # Customer Service Representatives Hired | | | | |
|---|---|---|---|---|---|
| Entry Relative $/hr | 0.167** | 0.197** | 0.205** | 0.219** | 0.221** |
| | (0.084) | (0.085) | (0.089) | (0.097) | (0.097) |
| Recruitment Elasticity | 3.18** | 3.76** | 3.9** | 4.17** | 4.21** |
| | (1.61) | (1.61) | (1.7) | (1.84) | (1.85) |
| Pool of CSR Workers | Linear | Log | Quartic | Quartic | Quartic |
| Unemployment | ✓ | ✓ | ✓ | ✓ | ✓ |
| Retailer Non-Csr Presence | | | | ✓ | ✓ |
| Retailer Non-Csr Counts | | | | | ✓ |
| Mean | 0.73 | 0.73 | 0.73 | 0.73 | 0.73 |
| MSAs | 920 | 920 | 920 | 920 | 920 |
| $R^2$ | 0.232 | 0.189 | 0.289 | 0.297 | 0.300 |

*Note:* $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the relationship between the relative wages offered by the retailer for remote workers ($14/hour) compared to entry pay in the MSA for customer service workers. We control for the quantity of remote customer service representatives hired in the MSA between 2018 and 2019 and the local unemployment rate. We exclude MSAs with on-site call-centers, which have different advertising. The first column controls for a linear effect of the number of local customer service representatives in the MSA whom the retailer could potentially draw from. The second column controls for a log in employment. In the remaining columns control for a quartic in local employment in customer service. The fourth column controls for the retailer having a warehouse in the MSA and the retailer having a corporate or sales' office in the MSA. The final column includes controls for counts of the number of warehouse and other non-customer-service workers in the retailer in the MSA.

TABLE 5: HETEROGENEITY IN ELASTICITIES BY GENDER IN CUSTOMER SERVICE

**Panel A: Pay's Effects on Productivity by Gender**

| | Female | Male | Δ |
|---|---|---|---|
| Effect on Calls | 2.32** | 1.24 | 1.08** |
| | (0.95) | (1.03) | (0.54) |
| Elasticity of Calls | 1.41** | 0.8 | 0.6** |
| | (0.58) | (0.66) | |
| Mean Calls | 25.58 | 24.41 | 0.3 |
| | | | (0.34) |
| Mean Pay | 15.47 | 15.75 | -0.07 |
| | | | (0.05) |
| # Workers | 1555 | 618 | |
| # MSAs | 33 | 20 | |

**Panel B: Pay's Effects on Turnover by Gender**

| | Female | Male | Δ |
|---|---|---|---|
| Effect on Turnover | -0.91 | -1.87** | 0.96* |
| | (0.6) | (0.6) | (0.54) |
| Elasticity of Turnover | -3.47 | -6.63** | -3.16* |
| | (2.3) | (2.13) | |
| Mean Turnover | 4.17 | 4.57 | -0.78*** |
| | | | (0.23) |
| Mean Pay | 15.94 | 16.19 | -0.07 |
| | | | (0.07) |
| # Workers | 2097 | 901 | |
| # MSAs | 39 | 23 | |

**Panel C: Pay's Effects on Recruitment by Gender**

| | Female | Male | Δ |
|---|---|---|---|
| Effect on Recruitment | 0.13** | 0.01 | 0.12** |
| | (0.06) | (0.02) | (0.05) |
| Elasticity of Recruitment | 3.27** | 1.86 | 1.41** |
| | (1.58) | (3.43) | |
| Mean Recruited/MSA | 0.55 | 0.1 | |
| Mean Pay | 14 | 14 | 0 |
| # Workers | 508 | 93 | |
| # MSAs | 96 | 40 | |

*Note:* *p<0.1; **p<0.05; ***p<0.01

This table show customer service representatives' responsiveness to the retailer's relative wages, separately by gender. Differences in means among customer service representatives are calculated base on regressions that include date and MSA fixed effects and have standard errors clustered at the MSA-level. Panel A shows how the change in relative wages to the MSA entry pay from 2018 to 2019 relates to the change in call volume in that MSA from 2018 to 2019. The productivity analyses limit to those hired in 2018 or later because representatives hired earlier are able to handle different types of calls, which changes their call volume. Panel B shows how the change in relative wages to the MSA entry pay from 2018 to 2019 relates to the change in monthly turnover in that MSA from 2018 to 2019. Panel C shows how the pay the retailer offers for remote workers, relative to the entry pay in that MSA, relates to the number of workers hired between 2018 and 2019. Standard errors are clustered at the MSA-level.

TABLE 6: TURNOVER EFFECTS WITHIN AND ACROSS WORKERS

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

*Note:* *p<0.1; **p<0.05; ***p<0.01

This table presents reduced form regressions on the relationship effect of pay 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. Regressions labeled “Across” measure the effect of higher pay on job completion (finishing the temporary assignment), quits, and receiving an excellent evaluation from the manager. Regressions include occupation by commuting zone fixed effects, industry by commuting zone by time fixed effects as well as controls for expected duration as a quartic. Regressions labeled “Within” also include individual fixed effects. Expected duration is calculated based on how long similar jobs at that client have lasted in the past. We weight the regression by the duration of the job. Standard errors are clustered at the worksite-firm level.

TABLE 7: RELATIVE PAY'S IMPACT ON TEMPORARY WAREHOUSE WORKERS

**Panel A: Relative Pay's Effects on Job Outcomes**

| | Job Completed | Quits | Excellent Evaluation |
|---|---|---|---|
| Relative Hourly Pay | 1.165* | −0.482*** | −0.765 |
| | (0.678) | (0.168) | (3.497) |
| Elasticity | 0.24* | −1.4*** | −0.62 |
| | (0.14) | (0.49) | (2.83) |
| Season Fixed Effects | ✓ | ✓ | ✓ |
| Controls | Days Quartic | Days Quartic | Days Quartic |
| Base Mean | 83.4 | 5.9 | 21 |
| Workers | 5,763 | 5,763 | 5,763 |
| Observations | 6,398 | 6,398 | 6,398 |
| $R^2$ | 0.127 | 0.116 | 0.027 |

**Panel B: Relative Pay's Effects on Worker Quality**

| | Predicted Excellent | Predicted Poor | New Worker |
|---|---|---|---|
| Relative Hourly Pay | 0.869*** | −0.269 | −0.746* |
| | (0.301) | (0.196) | (0.426) |
| Elasticity | 1.14*** | -0.93 | −0.32* |
| | (0.4) | (0.68) | (0.19) |
| Season Fixed Effects | ✓ | ✓ | ✓ |
| Dependent Mean | 12.91 | 4.92 | 39.14 |
| Workers | 5,763 | 5,763 | 5,763 |
| Observations | 6,398 | 6,398 | 6,398 |
| $R^2$ | 0.065 | 0.001 | 0.036 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

Panel A shows the effect of relative pay on temporary job completion, quits before the end of the job, and excellent evaluations from the site manager. Regressions include expected duration as a quartic as well as season fixed effects. Standard errors are clustered at the commuting zone level. Panel B shows the effect of relative pay on the predicted quality of workers secured by the shipper. Regressions include season fixed effects and control for the predicted quality of workers in the commuting zone overall.

## APPENDIX A: SPILLOVER EFFECTS OF HIGHER PAY

To understand whether the shippers' hiring at higher rates negatively impacts other firms who are hiring warehouse workers at the same time, we use a difference-in-differences strategy, comparing the change in job outcomes around the holiday season when the shipper hires in commuting zones where the shipper is present to the change in job outcomes in control commuting zones.

Our control commuting zones are defined as the other commuting zones in the state where the shipper is not located. We define a treated commuting zone to have at least one month in which the shipper hires more than 45 individuals in that month and to have had at least 20 hires outside the shipper. We require that control months have at least 20 job placements outside the shipper. The entire sample is limited to warehousing jobs. We further restrict the sample to the three months just before the shipper seeks workers to account for potential seasonality.

We use a simple difference-in-differences approach, where we fully interact the specification with year to ensure that we do not put negative weight on any of our comparisons, in keeping with the recent literature on two-way fixed effects models (e.g., Goodman-Bacon, 2018; Abraham and Sun, 2018; Imai and Kim, 2019; de Chaisemartin and D'Haultfoeuille, 2020).

$$\text{Job Completion}_{it} = \alpha_0 \mathbb{1}^{cz}_{ijt} + \alpha_1 \mathbb{1}^{season}_{ijt} + \alpha_2 y_{ijt} + \alpha_3 y_{ijt} \mathbb{1}^{cz}_{ijt} + \alpha_4 y_{ijt} \mathbb{1}^{season}_{ijt} + \beta(y_{ijt} \cdot \mathbb{1}^{cz}_{ijt} \cdot \mathbb{1}^{month}_{ijt}) + \epsilon_{ijt} \tag{7}$$

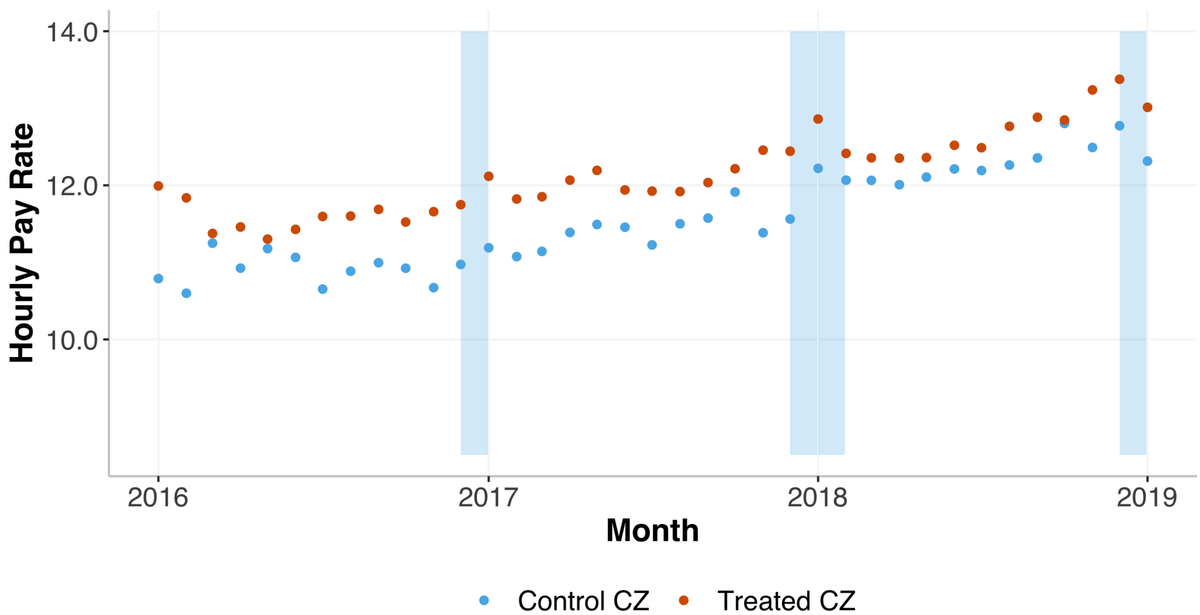
where $\mathbb{1}^{cz}_{ijt}$ is an indicator for each commuting zone, $\mathbb{1}^{season}_{ijt}$ is an indicator for each season, $y_{itj}$ indicates the year, and the $\beta$ coefficients are aggregated into our coefficient of interest using inverse-variance weighting. We cluster standard errors at the MSA level for the regression and calculate bootstrapped standard errors for the weighted coefficient that aggregates the yearly estimates.

We would expect that in areas and times where the central firm's going rate of $17 per hour is greater than the average pay for a warehouse worker, the negative effects on other firms would be larger. As such, in our next specification, we fully interact Equation 7 with $\overline{\text{Pay}}_{ijt}$, the average pay differential in the commuting zone - season pair. In this case, $\beta$ thus captures how much an additional dollar of relative pay offered by the shipper impacts job outcomes at rival firms in each of our treated years.

To assess the parallel trends assumption, we plot in Figure A.1 the average pay rates for warehouse jobs in the treated and untreated commuting zones in orange and blue dots, respectively. The shaded areas show the months where the central firm is hiring more than 50 individuals. While the central firm tends to locate in commuting zones that tend to have slightly higher pay than the areas where their rivals alone locate, the trends in pay are fairly similar throughout the time period. Additionally, we test for pretrends analytically, by adding treatment-season fixed effects, as Pischke (2019) recommends, and we find no significance.

As seen in Table A.1, when the shipper is hiring at all, temporary job completions at rival firms decrease by 20 percent (10.7 percentage points off a base of 53 percent) and quits at rival firms increase by 44 percent (12.4 percentage points off a base of 28 percent). An additional dollar of pay over the outside option is associated with a 1.5 percent (0.87 percentage point) decrease in job completions and 5 percent (1.45 percentage point) increase in quits. Note that we do not see a decrease in the workers who are predicted to be excellent.

FIGURE A.1: AVERAGE PAY FOR WAREHOUSE JOBS IN TREATED AND UNTREATED COMMUTING ZONES




*Note:* We plot average pay for warehouse jobs by month that the job starts in the treated and untreated commuting zones in orange and blue, respectively. The shaded areas reflect the months when the central firm hires more than 50 workers. The central firm tends to locate in commuting zones that pay slightly more as seen by the fact that the orange dots are consistently above the blue ones. However, the trends appear to be fairly parallel throughout the time period. Also note that pay at the central firm is $17 per hour, which is considerably higher than the going for warehouse jobs in either treated or untreated areas.

TABLE A.1: SPILLOVERS FROM THE SHIPPER'S HIRING ON OTHER LOCAL FIRMS

| | Diff-in-diff | | | Continuous | | |
|---|---|---|---|---|---|---|
| | Job Completed | Quits | Pred. Excellent | Job Completed | Quits | Pred. Excellent |
| Effect of Shipper | -10.72*** | 12.4* | -0.43 | -0.87*** | 1.45** | -0.38 |
| | (0.02) | (1.63) | (1.82) | (0.02) | (0.36) | (0.32) |
| Elasticity | -0.34*** | 0.73* | -0.02 | -0.17*** | 0.55** | -0.13 |
| | (0) | (0.1) | (0.1) | (0) | (0.13) | (0.11) |
| Dependent Mean | 53.23 | 28.19 | 31.31 | 53.23 | 28.19 | 31.31 |
| Mean Pay | 10.63 | 10.63 | 10.63 | 10.63 | 10.63 | 10.63 |
| # Workers | 16448 | 16448 | 16448 | 16448 | 16448 | 16448 |
| # CZs | 51 | 51 | 51 | 51 | 51 | 51 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

This table presents both a difference-in-differences and a continuous difference-in-differences regressions, comparing how the shipper's hiring of warehouse workers through the staffing agency affects other client firms hiring in the same local labor market in the same month. The main outcomes are workers completing the job, quitting or being predicted to be excellent. The difference-in-difference specification compares times when the shipper is hiring to three months beforehand in 2016-2018. The continuous difference-in-difference scales by the gap between the shipper and other local warehouse pay. The regressions fully interact the specification with the three years between 2016-2018. Standard errors are clustered at the commuting-zone level. Yearly point estimates are aggregated using inverse-variance weighting and bootstrapped standard errors are reported.

## APPENDIX B: ADDITIONAL TABLES

TABLE B.1: DEMOGRAPHIC CHANGES IN WAREHOUSE AROUND PAY CHANGE

| | Age | Female | Specialization | Share Hires |
|---|---|---|---|---|
| Post | 0.051 | −0.005 | −0.075 | −0.002 |
| | (0.501) | (0.017) | (0.057) | (0.002) |
| Constant | 36.090*** | 0.219*** | 0.792*** | 0.007*** |
| | (0.733) | (0.026) | (0.061) | (0.001) |
| Workers | 514 | 514 | 514 | 514 |
| $R^2$ | 0.00000 | 0.00003 | 0.001 | 0.043 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

We consider the effects on turnover of a discrete pay change in a warehouse. The Field Director of the warehouse confirmed that this pay change was unexpected and did not accord with any change in work or work structure. Accordingly, we test for other changes in the warehouse around the time of the pay change. We compare the before and after period; standard errors are clustered at the week and employee levels.

TABLE B.2: DIFFERENCE-IN-DIFFERENCE OF PRODUCTIVITY EFFECTS IN WAREHOUSE

| | First Stage | Boxes/Hr | Boxes/Moving Hr | Moving/Total Hrs |
|---|---|---|---|---|
| Post x Treated | 1.622*** | | | |
| | (0.026) | | | |
| $1/hour | | 0.375*** | 0.557*** | 0.001 |
| | | (0.021) | (0.031) | (0.002) |
| Productivity $\epsilon$ | | 1.23*** | 1.17*** | 0.03*** |
| | | (0.07) | (0.07) | (0.05) |
| Base Mean | 16.2 | 4.92 | 7.69 | 0.64 |
| Comparison | Twin | Twin | Twin | Twin |
| Workers | 1733 | 1733 | 1733 | 1733 |
| Observations | 187,861 | 187,861 | 187,861 | 187,861 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

This table presents a difference-in-difference regression comparing the change in the treated warehouse's productivity to that seen in "twin" retailer warehouses that handle the same size parcel before and after August 2019. The second stage is scaled by the size of the pay jump so the coefficients may be interpreted as the change in turnover due to $1 pay change. Standard errors are clustered at the week- and individual-levels.

TABLE B.3: SENSITIVITY OF WAREHOUSE PRODUCTIVITY EFFECTS TO TIME SPANS INCLUDED

| | Boxes/Hour | | | Boxes/Moving Hour | | | Moving/Total Hours | | |
|---|---|---|---|---|---|---|---|---|---|
| | 1 Mo | 2 Mo | 3 Mo | 1 Mo | 2 Mo | 3 Mo | 1 Mo | 2 Mo | 3 Mo |
| $1/hour | 0.022 | 0.256*** | 0.348*** | −0.040 | 0.118 | 0.316*** | 0.006 | 0.022*** | 0.018*** |
| | (0.140) | (0.096) | (0.015) | (0.148) | (0.103) | (0.014) | (0.013) | (0.008) | (0.001) |
| Observations | 29,401 | 41,360 | 50,478 | 29,401 | 41,360 | 50,478 | 29,401 | 41,360 | 50,478 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

*Note:* This table shows the sensitivity of the warehouse productivity calculations to different time spans. The main body of the paper presents results for a 3-month bandwidth on either side of the pay jump. This table shows 1-, 3- and 3-month results. We do not extend the window beyond 3 months after August 2019 because we enter the holiday shipping season, which substantively alters warehouse functioning. All regressions are scaled to show the effect of a single dollar; standard errors are clustered at the week and employee levels.

TABLE B.4: PLACEBO TEST OF PRODUCTIVITY IN TWIN WAREHOUSES

| | First Stage | Boxes/Hr | Boxes/Moving Hr | Moving/Total Hrs |
|---|---|---|---|---|
| Post | 0.207*** | 0.003 | −0.333* | 0.027** |
| | (0.031) | (0.142) | (0.169) | (0.013) |
| Pre Jump Mean | 16.24 | 2.79 | 5.19 | 0.55 |
| Observations | 26 | 26 | 26 | 26 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

We perform a placebo test, exploring the change in productivity at other warehouses that handle the same type of parcel. Since warehouses that handle the same type of parcel have similar units of productivity and since demand shocks are likely to hit them all similarly, we suspect we would see an increase in productivity in twin warehouses if it were driven by an uptick in consumer demand for large parcel goods. While the regression in Table 2 scales by the size of the pay change, the unscaled coefficients are presented here since there is no significant first stage in our context. Standard errors are clustered at the employee and week-by-warehouse levels.

TABLE B.5: CUSTOMER SERVICE DAILY LABOR SUPPLY EFFECTS

| | Work Hrs | Absent Hrs | Absent Unapproved Hrs | Overtime Hrs |
|---|---|---|---|---|
| Entry Relative $1/hr | −0.118 | 0.078 | −0.036 | 0.065*** |
| | (0.131) | (0.072) | (0.025) | (0.015) |
| Elasticity | 0.26 | 1.52 | 1.33 | 4.11*** |
| | (0.29) | (1.42) | (0.95) | (0.97) |
| FE: date-timezone | ✓ | ✓ | ✓ | ✓ |
| Mean $/hr | 16.02 | 16.02 | 16.02 | 16.02 |
| Dependent Mean | 7.14 | 0.82 | 0.43 | 0.25 |
| MSAs | 41 | 41 | 41 | 41 |
| Workers | 2871 | 2871 | 2871 | 2871 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

This table leverages stickiness of the retailer's pay to evaluate how the change in relative pay from 2018 to 2018 affect customer service representatives daily labor supply designs.

TABLE B.6: EFFECTS OF PAY BY INITIAL PRODUCTIVITY IN CUSTOMER SERVICE

**Panel A: Productivity Effects by Initial Productivity**

| | Start in Top Third | Rest of Workforce | Δ |
|---|---|---|---|
| Effect on Turnover | 1.1 | 2.7** | -1.59** |
| | (0.71) | (0.87) | (0.59) |
| Elasticity of Turnover | 0.55 | 1.8** | -1.25** |
| | (0.35) | (0.58) | (-0.23) |
| Mean Turnover | 31.43 | 23.66 | 6.26*** |
| | | | (0.37) |
| Mean Pay | 15.62 | 15.78 | -0.27*** |
| | | | (0.04) |
| # Workers | 615 | 1207 | |
| # MSAs | 17 | 25 | |

**Panel B: Turnover Effects by Initial Productivity**

| | Start in Top Third | Rest of Workforce | Δ |
|---|---|---|---|
| Effect on Turnover | -2.13** | -0.77** | -1.36 |
| | (1.06) | (0.33) | (1.05) |
| Elasticity of Turnover | 6.68** | 2.76** | 3.92 |
| | (3.31) | (1.19) | |
| Mean Turnover | 4.98 | 4.39 | 0.32 |
| | | | (0.44) |
| Mean Pay | 15.62 | 15.78 | -0.27*** |
| | | | (0.04) |
| # Workers | 615 | 1207 | |
| # MSAs | 17 | 25 | |

*Note:* *p<0.1; **p<0.05; ***p<0.01

This table shows for workers with different baseline productivities, the effect of relative pay changes in an MSA between 2018 and 2019 on productivity and turnover changes in those MSAs between 2018 and 2019. Baseline productivity is measured according to representatives' daily call volumes in their first month of calls after formal training. Standard errors are clustered the MSA level.

TABLE B.7: DIFFERENCE-IN-DIFFERENCE OF TURNOVER EFFECTS IN WAREHOUSE

| | First Stage | Monthly Turnover | Quits | Fires |
|---|---|---|---|---|
| Post x Treated | 2.152*** | | | |
| | (0.093) | | | |
| $1/hour | | −3.780*** | −3.797*** | 0.245 |
| | | (1.207) | (1.075) | (0.487) |
| Elasticity | | 4.57*** | 5.77*** | 1.97 |
| | | (1.46) | (1.63) | (3.91) |
| Base Mean | 16.2 | 86.6 | 10.66 | 2.02 |
| Comparison | In-State | In-State | In-State | |
| Workers | 1557 | 1557 | 1557 | 1557 |
| Observations | 149,656 | 149,656 | 149,656 | 149,656 |

*Note:* *p<0.1; **p<0.05; ***p<0.01
This table presents a difference-in-difference regression comparing the change in the treated warehouse's turnover to that seen in other retailer warehouses in the same state before and after August 2019. The second stage is scaled by the size of the pay jump so the coefficients may be interpreted as the change in turnover due to $1 pay change. Standard errors are clustered at the week- and individual-levels.

TABLE B.8: SENSITIVITY OF WAREHOUSE TURNOVER EFFECTS TO TIME SPANS INCLUDED

| | Monthly Turnover | | | Quits | | | Fires | | |
|---|---|---|---|---|---|---|---|---|---|
| | 1 Mo | 2 Mo | 3 Mo | 1 Mo | 2 Mo | 3 Mo | 1 Mo | 2 Mo | 3 Mo |
| $1/hour | −3.698** | −2.631** | −2.504** | −3.737*** | −2.482** | −2.270** | 0.280 | 0.188 | 0.061 |
| | (1.571) | (1.307) | (1.255) | (1.132) | (1.003) | (0.957) | (1.318) | (0.687) | (0.563) |
| Observations | 29,401 | 41,360 | 50,478 | 29,401 | 41,360 | 50,478 | 29,401 | 41,360 | 50,478 |

*Note:* *p<0.1; **p<0.05; ***p<0.01
This table shows the sensitivity of the warehouse turnover calculations to different time spans. The main body of the paper presents results for a 3-month bandwidth on either side of the pay jump. This table shows 1-, 3- and 3-month results. We do not extend the window beyond 3 months after August 2019 because we enter the holiday shipping season, which substantively alters warehouse functioning. All regressions are scaled to show the effect of a single dollar; standard errors are clustered at the week and employee levels.

TABLE B.9: PLACEBO TEST OF TURNOVER IN IN-STATE WAREHOUSES

| | First Stage | Monthly Turnover | Quits | Fires |
|---|---|---|---|---|
| Post | 0.114 | 1.810 | 2.249* | −0.294 |
| | (0.115) | (1.503) | (1.338) | (0.501) |
| Base Mean | 15.66 | 9.3 | 7.5 | 1.24 |
| Workers | 1068 | 1068 | 1068 | 1068 |
| Observations | 99,178 | 99,178 | 99,178 | 99,178 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

We perform a placebo test, exploring the change in turnover at other warehouses in the same state. Since 2 of the three other warehouses are within a 13-minute drive of the treated warehouse, if a shock to the local labor market for warehouse workers caused the effects in the treated warehouse, one would expect to see turnover decreases in these warehouses as well. While the regression in Table 3 scales by the size of the pay change, the unscaled coefficients are presented here since there is no significant first stage in our context. Standard errors are clustered at the employee and week-by-warehouse levels.

TABLE B.10: EFFECT OF RELATIVE PAY ON CUSTOMER SERVICE RECRUITMENT BY GENDER

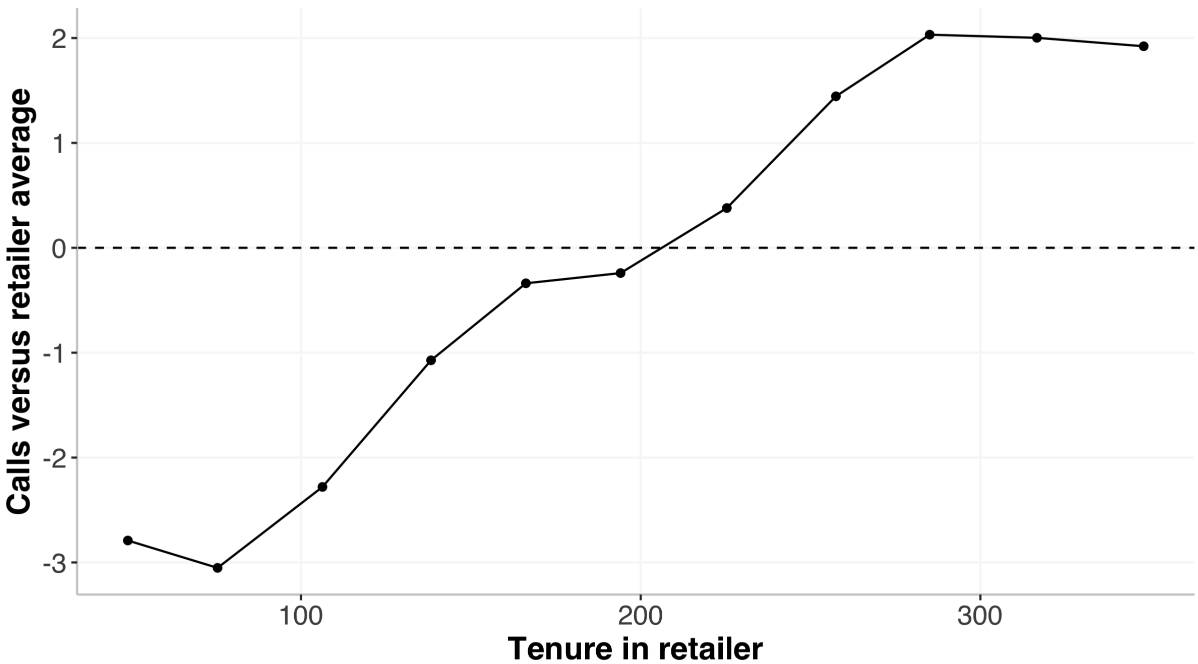
| | # Customer Service Representatives Hired | | | | |
|---|---|---|---|---|---|
| Entry Relative $/hr | 0.013 | 0.010 | 0.008 | 0.008 | 0.015 |
| | (0.025) | (0.009) | (0.011) | (0.011) | (0.010) |
| Female : Entry Relative $/hr | 0.115** | 0.142** | 0.156** | 0.167** | 0.157** |
| | (0.046) | (0.069) | (0.074) | (0.080) | (0.079) |
| Recruitment Elasticity for Men | 1.86 | 1.41 | 1.09 | 1.17 | 2.14 |
| | (3.43) | (1.26) | (1.52) | (1.57) | (1.35) |
| Recruitment Elasticity for Women | 3.27 | 3.87 | 4.14 | 4.45 | 4.38 |
| | (1.58) | (1.92) | (2.06) | (2.21) | (2.21) |
| Employment | Linear | Log | Quartic | Quartic | Quartic |
| Retailer Non-CSR Presence | | | | ✓ | ✓ |
| Retailer n-CSR Counts | | | | | ✓ |
| F | 117.55 | 29.19 | 59.53 | 45.41 | 37.22 |
| Mean Female Recruits/MSA | 0.55 | 0.55 | 0.55 | 0.55 | 0.55 |
| Mean Male Recruits/MSA | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 |
| # MSAs | 920 | 920 | 920 | 920 | 920 |
| $R^2$ | 0.204 | 0.172 | 0.264 | 0.272 | 0.280 |

*Note:* *p<0.1; **p<0.05; ***p<0.01

We consider the relationship between relative pay of the employer and the number of customer service representatives ever recruited and hired in the MSA. Each observation is an MSA, excluding MSAs with on-site call-centers which have different advertising. Relative pay is the gap between the retailer's $14/hr rate and the typical rate for entry-level workers, which we approximate with the average of the 25th and 50th percentiles of the local wage distribution. In the first column, we control only for a linear effect of the number of local customer service representatives in the MSA, whom the retailer could potentially draw from. In the second column, we instead control for a log in employment. In the third column, we control for a quartic in local employment in customer service. In the fourth column, we add indicators for the retailer having a warehouse in the MSA and the retailer having a corporate or sales' office in the MSA. In the final column, we also include controls for counts of the number of warehouse and other non-customer-service workers in the retailer in the MSA.

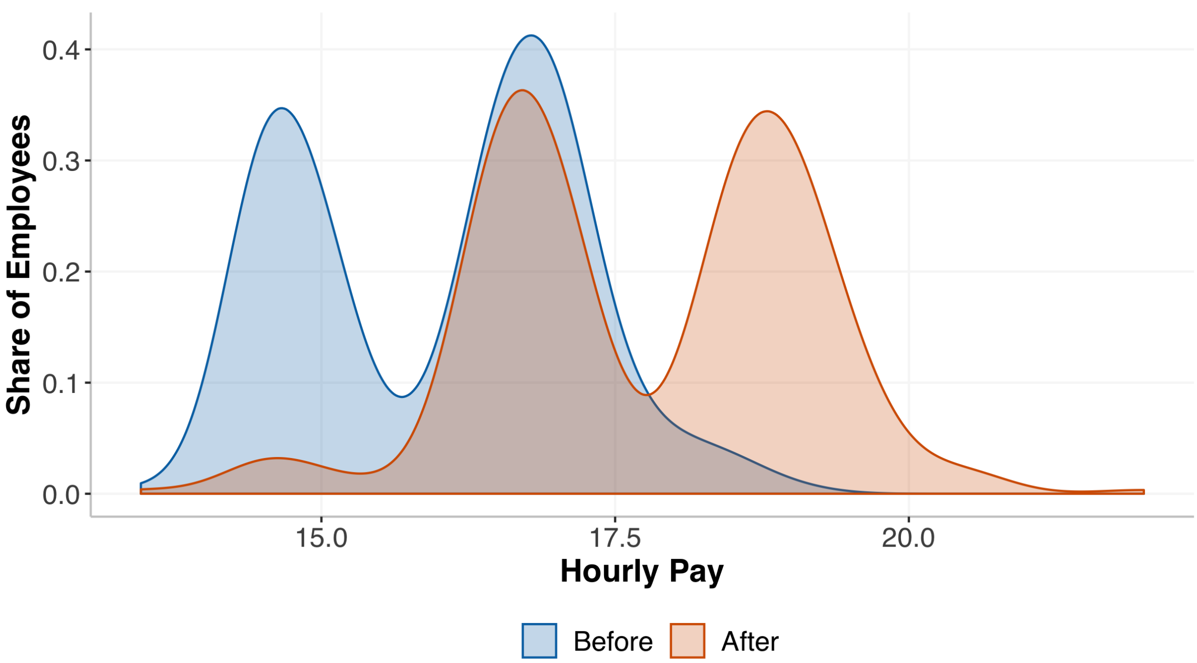
# APPENDIX C: ADDITIONAL FIGURES

FIGURE C.1: PRODUCTIVITY TRAJECTORY FOR NEW CUSTOMER SERVICE REPRESENTATIVES



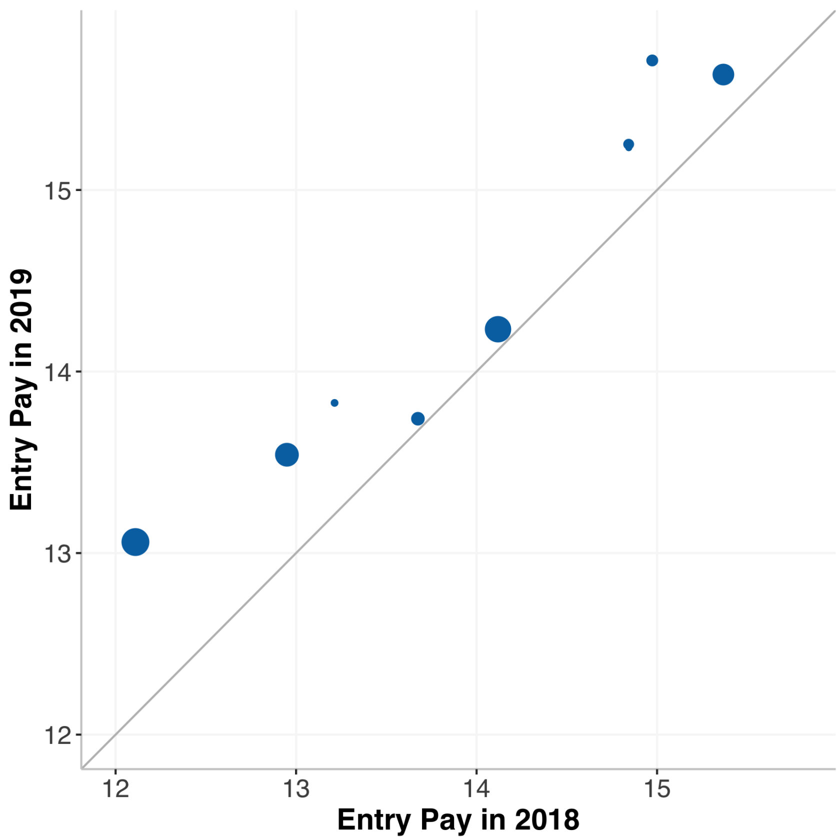

*Note:* This figure presents the daily calls taken by a worker relative to a typical customer service representative in the same time-zone on the same day as a function of their tenure. The x-axis plots representatives' days in the company after their training was completed. We plot the means of each month.

Figure C.2: Distribution of Pay Before and After Warehouse Pay Jump



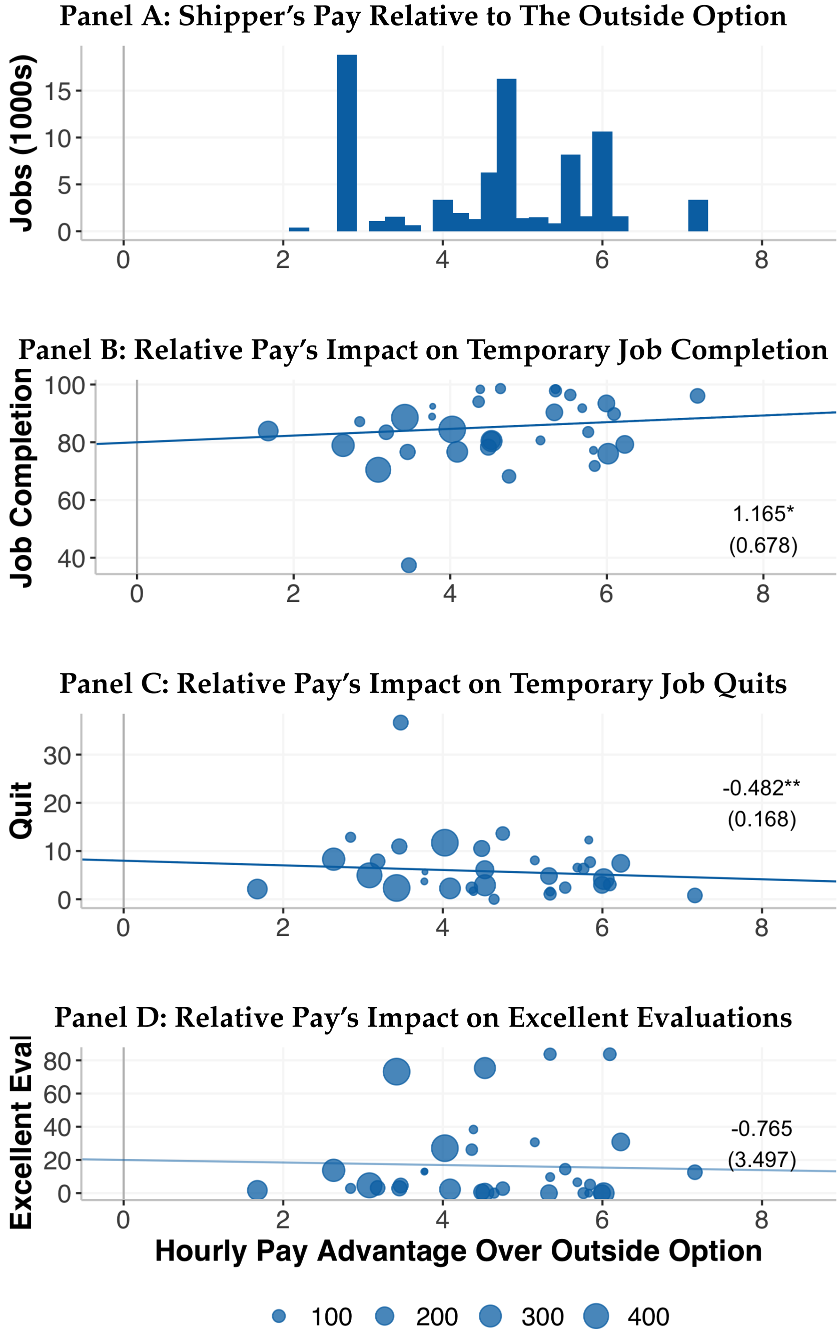

*Note:* This figure presents the distribution of pay among entry level warehoue workers within the treated warehouse one week before August 2019 and one month afterward. There are two sources of wage variation: a level shift if the worker works an unpleasant shift or is certified to work on specialized machinery, which generates a bimodal distribution, and wage variation based on when hired, which generates variation around these means. The distribution of pay before August had a standard deviation of 1.18; afterward it was 1.21.

Figure C.3: Entry-Level Customer Service Pay in 2018 and 2019 in MSAs where Retailer Hires




*Note:* This figure displays the entry-level customer service pay in 2018 and 2019 in MSAs where the retailer has customer service representatives. The grey line indicates the 46-degree line. We approximate the entry-level pay based on the average of the 25th and 50th percentiles of customer service pay in that year and MSA. Throughout this time span, pay remained constant for customer service representatives at the retailer.

Figure C.4: National Pay Setting Among Temporary Warehouse Workers at a Shipper




*Note:* This figure shows how the pay of the shipper ($17/hour) relative to the outside option for warehouse workers in each commuting zone relates to temporary job outcomes. Panel A shows the relative pay of warehouse jobs (outside the shipper) offered through the staffing agency in each commuting zone along the x-axis and the number of warehouse jobs (outside the shipper) offered through the staffing agency in the commuting zones on the y-axis. Panel B shows that average job completion rate at the shipper along the y-axis. Panel C shows the average quit rate at the shipper in each commuting zone along the y-axis. Panel D shows the rate of excellent evaluations at the shipper in each commuting zone. The size of the circles in Panels B-D reflect the number of jobs the shipper secures through the staffing agency in each commuting zone. We only plot commuting zones with 50 or more jobs, but all commuting zones are included in the regressions.