Voluntary Minimum Wages

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Abstract

Recent wage growth at the bottom of the earnings distribution in the U.S. has reversed a decades-long trend of widening wage inequality. Numerous state and local minimum wage increases have overtaken an effectively non-binding federal minimum, and robust labor demand in the post-pandemic recovery drove wage growth in the low-wage sector. An increasingly pervasive phenomenon over this same period (2014-2023) is the use of company-wide, voluntary minimum wages (VMWs) by private employers, including some of the largest U.S. retailers. We use anonymized payroll data for thousands of firms collected by a major credit bureau to study the effects of these policies on large retailers' own wages and employment, as well as spillover effects onto other employers in shared labor markets, variously defined. Using stacked event studies centered around multiple VMW events and a continuous treatment variable defined as the gap between local area wages and the company minimum, we find that VMWs result in sizable wage increases and reductions in turnover at the companies that implemented them. Turning to wages at other companies, including those connected to the large retailer by worker flows, we estimate precise, economically negligible spillover effects. Despite the decline in separations from companies with voluntary minimums, overall hiring rates at connected employers do not decline, consistent with substitutability across new hires. Although voluntary minimum wage policies have affected over 3 million jobs among the largest retailers, their impact on the broader labor market is limited.

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1 Introduction

In the mid 2010s, a growing number of states and localities increased their minimum wages above the federal level, which has remained $7.25 since 2009. Over this same period, wage growth at the bottom of the wage distribution began to overtake wage growth at the top, for the first time in 40 years (Gould and deCourcy, 2023; Autor et al., 2023). The impact of state and local minimum wages on inequality, wages, and employment has been well studied in recent literature (Dube, 2019a,b; Cengiz et al., 2019; Dube and Lindner, 2021). However, another widespread policy–voluntary minimum wages set by large national retailers–has not been the object of systematic study, despite garnering substantial media attention.[1]

In this paper, we study the impacts of large retailer voluntary minimum wages on wages and employment in low wage U.S. labor markets. To do so, we use data on the wages, employment, and employment flows of thousands of firms, including several large companies that have adopted or raised voluntary minimums in the last 10 years. The anonymized data, which we obtained from a large credit bureau, cover over 18 million hourly workers (or 24% of the U.S. total). They allow us to answer both how these policies have affected wages and employment at the companies that have adopted them, and how the policies have affected the wage setting and employment decisions of local competitors.

Voluntary minimum wages at large national retailers will have varying effects in different areas depending on the local size and wage distribution of the adopting employer. We combine this geographic variation in the policy's bite with information on worker flows between firms to estimate the effects of large retailer voluntary minimums on their own outcomes as well as their spillover effects to the broader local labor market. Specifically, we calculate a gap measure–borrowed from the literature on national minimum wage changes–for each commuting zone ("CZ"), which represents the percent increase in the company's average hourly wage rate in that CZ implied by moving all workers in the area up to the company minimum wage. We first use this variation in the gap across CZs

[1] A previous version of this paper, titled "Spillover effects from voluntary employer minimum wages," studied the spillover effects of voluntary minimum wages using data from online job ads and online job reviews and found sizable cross-employer wage elasticities from 10 voluntary minimum wage events at five large retailers. However, our previous estimates of spillovers were biased by mean reversion due to the sparsity of wage information at the employer-by-occupation-by-commuting-zone level in online job ads and job reviews. The 2022 update to the National Bureau of Economics Research Working Paper version of the paper contains a correction Appendix that documents zero spillovers when mean reversion is accounted for using placebo-in-time analyses (see Derenoncourt et al. (2022)). The present paper uses administrative payroll data on wages and employment for roughly 18 million hourly workers, including at several large retailers with voluntary minimum wages, to reassess the effects of such policies in a dataset with little-to-no measurement error in wages at the firm-by-fine-geography level. The new data allow us to examine the direct wage and employment effects of such policies on adopters themselves, as well as spillover effects—including on close competitors identified via worker flows.

to estimate the impact of the policy on the large retailers' own wages and employment. We then look at how wages at other companies respond to changes in the large retailers' wages and employment, starting with a broad definition of the low wage labor market based on worker flows across industries and then narrowing our focus on spillovers to a set of highly exposed firms, defined by the flow of workers between the large retailers and those firms.

Using external information on voluntary minimum wages drawn from an inventory maintained by the National Employment Law Project and supplemented by our own comprehensive review of media reports, we assigned voluntary minimum wage announcements to the anonymized companies in the database using industry, company size, and the timing of shifts in the companies' national wage distributions. Our final sample of events consists of the 20 voluntary minimum wage policies across 5 large retailers–defined as those with employment greater than 150,000 workers nationally,[2] where there was no other major wage policy change at the retailer in the six months prior to or following the event. We focus on three levels of voluntary minimum wages, varying by their position relative to statutory minimum wages and their bite in the adopting firm's wage distribution: all $15 voluntary minimums, which exceeded most state and local minimum wages at the time of their adoption, all voluntary minimums affecting more than 30% of a large retailer's workforce, and all policies affecting 10% of the workforce.[3]

First, we study the effects of VMWs on own wages and employment. Drawing on gap designs used in the evaluation of national minimum wages (Card et al., 1994; Draca et al., 2011; Dustmann et al., 2022), we measure the percent increase in the company's average hourly wage required to bring all employees in a given commuting zone ("CZ") up to the new voluntary minimum. Based on our results from a stacked event study of all $15 VMW events, we estimate that moving from CZs with a gap of 0 to those with a gap of 1 is associated with an 88 log point increase in the average hourly wages in the six months after adoption of the policy. To put this magnitude in perspective, given that the average establishment-level gap among $15 VMWs is 0.11, the average establishment would need to raise wages by 11% to comply with the company $15 minimum. Our estimates imply nearly exactly this magnitude: a 10.45% average increase in hourly wage rates at the large retailers with the policy. Placebo-in-time analyses confirm our results are not driven by mean reversion and instead reflect changes in wages driven by the policy.

Turning to employment, VMWs lead to increases in employment at adopting large retailers: employment of workers earning under $30 an hour rose by 4.62% after the adop-

[2]We define retailers as large if they reached a monthly employment of at least 150,000 workers between 2013 and 2023.

[3]All policies we identified at large retailers affected at least 10% of their workforce.

tion of $15 VMWs, and by 1.25% after all VMW events.[4] Importantly, we find that these increases are entirely driven by increased retention as opposed to new hiring, for which we estimate marginally significant declines. Rates of separations to other companies in the database fell by 0.07-0.19 percentage points on base separation rates ranging from 1.23-1.44% (thus representing a 13.48% decline in separations at the upper range of our estimates). Labor supply elasticities implied by the effects on log total employment of below $30-an-hour workers are approximately 0.35-0.45 while those implied by the quit elasticity are between 2.20 and 2.38.[5]

Next, we turn to the effects of these VMWs on other employers in the same labor market. To estimate the overall spillover effect of the policy, we start with a broad definition of the labor market, which we then progressively narrow. First, we consider all hourly workers at other companies in the same commuting zone as the large retailer. Then, we focus on hourly workers in industries connected to the large retailer by worker flows. Finally, we restrict our attention to the specific establishments at other employers with a history of worker flows between them and the large retailer.

Across all three of these definitions of the relevant labor market, we estimate precise zero cross-employer wage rate elasticities in the six months following a large retailer VMW event.[6] Focusing on markets where the large retailer's employment share is relatively large, we continue to find zero spillover effects of the policy. The same is true when considering a measure of exposure based on the wage distribution of other employers: the gap between each of their establishments' average wage and the large retailer's new voluntary minimum.

To help us understand the lack of spillover effects on wage rates at other employers, we examine changes in hiring at other establishments after large retailer voluntary minimum wage events. We find that in the aggregate, at the commuting zone level, hiring away from the large retailer falls after the latter's adoption of a voluntary minimum, consistent with the reduction in separations at those companies. At the establishment level, we find that the probability that non-policy companies hire from VMW employers in any given month falls after adoption of the policy. However, we find no impact on overall hiring by other employers. Combined with neutral to potentially negative effects on hiring by the large retailer, likely as a consequence of a reduced need to replace separating workers, we conclude that the main impact of large retailer voluntary minimums is a decline in

[4]Results are similar when examining effects on all hourly employment, not just employment of those with wage rates below $30, consistent with minimal impacts on workers earning $30 or more.

[5]To arrive at this estimate of the labor supply elasticity, we apply the insight from standard dynamic monopsony models that the quit elasticity and the retention elasticity are equal to each other in steady state, and hence the labor supply elasticity is two times the quit elasticity (Manning, 2003).

[6]We also explore spillovers over a longer time horizon for those large retailer VMW events with no other event in the 12 months preceding or following the policy. We continue to see no wage spillover effects on other companies over the 12 months following the large retailer event.

the overall churn in the labor market, rather than a change in the quantity of labor supplied to other companies. This null effect on labor supplied to other companies helps explain the absence of wage spillover effects despite the introduction of VMWs in the labor market. Our results on the composition of hiring at other firms suggests they were able to adjust their hiring practices in the wake of large retailer policies, thereby muting any potential spillover effects to the broader labor market. These adjustments occurred without meaningful changes in the wages of new hires, suggesting substitutability across hiring sources.

Voluntary minimum wages at the nation's largest retailers have raised wages in over three million jobs over the last decade, increased total compensation for these workers, and reduced turnover. Taken together, these results suggest improved job quality for workers at adopting employers. Because the main effect of these policies on large retailer employment has been via decreased separations, we find that these shocks were "self-contained"–with little detectable impact on the wage rates of other employers. This is true across a range of definitions of the labor market and a range of measures of the intensity of the shock to other employers.

Estimates of cross-employer wage spillovers from company wage policy changes in the U.S. are limited.[7] Staiger et al. (2010) study the effects of a wage policy at Veterans Affairs hospitals that increased pay for registered nurses. They show that wages of nurses at neighboring hospitals also rose, with a cross-hospital wage elasticity of around 0.19. We look at a very different sector in the U.S. labor market–low-wage retail and service jobs. Two factors that ostensibly explain differences in our estimates relative to prior literature are that first, voluntary minimums did not result in major expansions in existing establishments, rather they reduced separations, and second, other employers' overall hiring did not change in the wake of large retailer policies, suggesting a thick market of potential new hires, possibly in contrast to the more concentrated market for RNs.

Our results on the own-effects of VMWs are highly consistent with both older and more recent studies of the effect of adopting a company-wide minimum on wages and turnover. Raff and Summers (1987) study the Ford Motor Company's introduction of the $5-dollar workday and finds a large reduction in quit rates in response to the policy, increased queuing for jobs at the company, and higher productivity. Emanuel and Harrington (2022) study the modern equivalent–a Fortune 500 warehouse, which implemented a voluntary $15 minimum wage–and find the policy reduced quits substantially and increased productivity to levels that offset the increased labor costs. In keeping

[7]Relevant papers in other contexts include Willén (2021), who examines spillovers from teacher wage decentralization in Sweden, and Hjort et al. (2020b), who examine the cross-establishment diffusion of headquarter minimum wages in multinationals.

with these findings, we show that voluntary minimum wages reduced separations at large retailers, with similar labor supply elasticities as those implied in the Ford case by Raff and Summers (1987). Our setting allows us to study 20 of these policies by five of the largest retailers in the U.S., thus confirming effects shown in the earlier case studies in the current labor market.

Our results also connect to a growing body of work providing direct empirical evidence of monopsony power and the impact it has on wages and inequality (Barth et al., 2016; Card et al., 2018; Song et al., 2019; Caldwell, 2019; Caldwell and Danieli, 2018; Schubert et al., 2021; Azar et al., 2019; Datta, 2023).[8] Evidence also points to behavioral and informational forces as well as fairness concerns playing a role in how employers choose wages (Cullen et al., 2022; Hjort et al., 2020a; Hazell et al., 2022; Dube et al., 2018). We contribute to this literature by studying a prevalent form of employer wage policy, particularly among retail and service employers in the U.S.–voluntary, binding company-wide wage floors–and their impact on the broader labor market. Because of our focus on large retailers, our paper also speaks to the literature on the effect of large employers in the low-wage labor market (see, for example, Wiltshire (2021)).

Additionally, our detailed payroll data spanning thousands of firms allow us to explore spillover effects on other companies, including those connected to VMW employers by labor flows, something previously not examined in the U.S. context. We find little support in this setting for the key mechanisms that would give rise to strategic wage setting as predicted in oligopsonistic models of the labor market (see Berger et al. (2022))—i.e., changes in the overall labor supplied to competitor firms in large retailers' labor markets. Instead, our results show that spillovers from these policies via local labor market competition are limited, primarily because the policies do not change the overall level of labor available to other employers.

In addition to providing novel empirical estimates of employer wage-setting spillovers, our study speaks to the search for policy levers to improve wages in the context of low worker bargaining power. Targeted attempts to sway large employers with wage setting power can improve wages for workers at those companies, but our evidence suggests these shocks are relatively "self-contained," with little detectable impact on other employers. These results contrast with the evidence on spillovers within firm boundaries (Hjort et al., 2020a; Giupponi and Machin, 2022). They also highlight the difference between government-mandated minimum wage floors that significantly alter wage setting practices throughout the market when introduced versus voluntary minimums at specific companies where broader effects are limited. Our results also differ with the limited empirical evidence on spillover effects from collective bargaining agreements (see Bassier (2022)),

[8] Card (2022) for a comprehensive review.

but a key contrast is that the voluntary minimum wages occurred in a largely non-union sector and were not collectively bargained.

Given the sharp rise in the adoption of voluntary minimum wages, particularly during the tight labor markets of 2021-2022, we caution against the interpretation that voluntary minimums have not "spilled over" on a national scale. The proliferation of these types of announcements by firms mimics an arms race in an effort to attract workers in a period of high quit rates and job-to-job mobility. Rather, we find that there is little support that local competitive pressures explain the adoption of these policies.

The paper is structured as follows. Section 2 describes the rise of voluntary minimum wages among U.S. retailers. Section 3 describes our data sources on employer wage distributions and worker flows. In Section 4, we describe our use of a gap design to study the effects of large retailer voluntary minimum wages, and we report our findings on large retailers' own wages and employment. In Section 5, we report our main spillover estimates, first for the broad shared labor market and second on highly exposed employers. Section 6 discusses mechanisms and interpretations for limited spillovers while Section 7 concludes and describes directions for future work on wage spillovers in low wage markets in the U.S.

2 The rise of voluntary minimum wages in the U.S.

From the 1980s to the present, a number of institutional factors placed downward pressure on wages in low-wage sectors. Unions continued to lose density or were never significantly present in many sectors of the economy (Western and Rosenfeld, 2011; Farber et al., 2021). The federal minimum wage was increased to $7.25 in 2009, but the failure of subsequent increases to pass Congress means it has been declining in real terms ever since, diminishing its impact on wage inequality (Autor et al., 2016). Corporate outsourcing, subcontracting, and franchising further depressed wages. Additionally, workers in the gig economy fall outside traditional federal and state legal protections and thus outside the scope of employment and labor law (Krueger and Ashenfelter, 2022; Weil, 2014, 2017). In this context, wages at the bottom of the nominal wage distribution stagnated and declined in real terms until very recently (Autor et al., 2023).

In response to stalled federal action on the minimum wage and the challenges of organizing workers under current labor law, in 2012 worker organizations and advocacy groups, led by the Service Employees International Union (SEIU) launched the "Fight for $15" campaign to advocate for higher wages and union representation. The coalition drew on the union's earlier efforts to institute "living wages" through local ordinances and government contracting. Worker advocates sought to bring attention to persistently

low earnings among workers in fast food, retail, and other service occupations, despite a growing economy and low unemployment. Indeed, recent local governments' adoption of $15 minimum wages have been attributed to the efforts of the Fight for $15 campaign (Rolf, 2015; Lathrop, 2018).

Following the Fight for $15 movement's launch and the pressure applied by the campaign on both government and private actors, a number of states and localities introduced increases in their minimum wage laws. The pandemic brought further attention to conditions facing frontline workers in many of the industries focused on by worker advocates (retail, food, logistics, and hospitality, for example). In response, more states and localities responded to political pressure to raise minimums in the absence of federal legislation. In addition, the Fight for $15 campaign and union-organizing efforts have put pressure on major companies that had either been long-term targets of organizing (Walmart) or more recent ones (Amazon).

One response by major companies in these industries was the adoption of voluntary minimum wages—that is, public statements that entry-level positions for employees would be raised to a new minimum wage, usually set well above the applicable state- or local minimum and adopted by the companies across all geographic operations in the US. Walmart was an early adopter of this policy, announcing a voluntary minimum wage of $9 per hour in 2014 and subsequently raising it again to $10 in 2018 and $12 in 2021; Amazon made headlines when it announced the adoption of a voluntary minimum of $15 per hour, effective October 2018. Figure 1 depicts the adoption of both statutory and voluntary minimum wages from 2014 to 2023.[9]

As was the case with state-level statutory minimum wage increases, voluntary minimum wage activity increased during the pandemic and post-pandemic period, spiking to 23 announcements of new company-wide minimum wages or increases in voluntary minimums in 2021 alone (see Appendix Figure A1).[10] Although the major firms adopting voluntary minimum wages like Amazon, Walmart, and Target have gained the most attention, a cross-section of differently sized companies, with both regional and national span and operating in a number of different industries, make up the companies in Figure 1. The majority of voluntary minimum wages that have been adopted since 2014 have been in retail, accommodation, and food services (accounting for over 75% of voluntary minimums). An additional 18% are in finance and insurance, primarily due to banks instituting voluntary minimum wages.

[9] A full description of the company-wide voluntary minimum wage events depicted in this figure as well as the news sources underlying the database is available in Appendix Table A1. The database consists of a historic inventory of VMWs drawn from media announcements that is maintained by the National Employment Law Project. We conducted our own comprehensive review and verification of each VMW announcement via an independent examination of business news articles covering VMW events.

[10] Appendix Figure A2 shows the number of new employers adopting VMWs each year.

Public statements about the reasons underlying these policies varied, but often focused on the company's concern for its workforce as well as its impacts to households and communities. In 2016, following Walmart's first minimum wage increase to $9, Fortune magazine added the company to its annual "Change the World" list (Wartzman, 2022). Amazon claimed broader motivations for its voluntary minimum wage announcement, noting "In 2018, Amazon raised its starting wage for all US employees to at last $15 an hour. We've seen the positive impact this has had on our employees, their families and their communities" (Amazon, 2018).

The underlying motivations may be multi-faceted. The ability of companies to announce and implement significant changes in their wage structures suggests frictions in the market that give employers wage-setting power in the labor markets in which they operate. Adopting higher entry-level wages may represent a response to unionization attempts, as shown in studies in earlier time periods, or adjustments induced from anticipated increases in statutory minimums. Alternatively, large employers holding a significant presence in local labor markets may have sought to use publicly announced increases in their entry wages to attract and retain higher productivity workers. The pressure to do so by publicly adopting voluntary minimum wages might have been amplified by pandemic-induced labor shortages in these sectors.

Alternatively, recent accounts describe shifts in corporate strategy at Walmart and Amazon that sought to improve employee retention as a means of improving operations and increasing productivity through reductions in turnover (Wartzman, 2022; Stone, 2022). Finally, the increases might have simply reflected public announcement of already existing wage policies if their wage structures already exceeded their announced voluntary minimums in most of the labor markets in which they operated.

A contribution of our study is our ability to examine the evolution of wages, employment, and other company outcomes at major VMW employers before and after their adoption of these policies. We are able to do so because the data we use is derived from the administrative payroll data of firms. The next section describes these data in detail.

3 Data on company wage distributions and identification of voluntary minimum wage events

We obtained data from a large credit bureau on the number of hourly workers per wage rate bin per firm covering the period from January 1, 2013 until August 31, 2023. Hourly employment of the 4,000-plus anonymized firms in the database totaled around 18 million

as of August 2023, or approximately 22% of the U.S. total.[11]

The credit bureau database is based on the payroll records provided to the bureau by companies and other payroll providers. The employment records from participating firms are provided to the credit bureau and periodically updated for purposes of employment and income verification of current and former workers. We estimate that the data from the credit bureau, including both hourly and salaried employees, covers roughly 30% of the U.S. workforce at any point in time.[12]

We also obtained information on worker flows between companies in the database, which we use to delineate the labor market of large retailers with voluntary minimums. Below we summarize the key features of the dataset. Appendix B describes the construction of our dataset on large retailers in detail, including procedures used to address periods of non-reporting in the database. Appendix Tables B1-B6 report missing observation and imputation rates across key variables.

3.1 Company-level data on wages and employment

The core dataset provides the number and average hourly wage of hourly workers in mutually exclusive nominal wage bins ranging from less than $8, at least $8 and up to $9, at least $9 and up to $10, and so on and so forth, up to $30 an hour. A final bin contains the number of workers earning $30 or more so that combining all wage bins together yields the total number of hourly workers for each firm. The data are disaggregated by month, employer, and worker commuting zone of residence.

We construct our key outcome variable, the log average hourly wage at the company-by-CZ level for workers earning less than $30 per hour by taking the employment weighted average hourly wage among workers in each bin up to the $29-bin and taking the log of this number. We also calculate the total number of workers earning less than $30 and use this as our measure of employment, taking its log. In robustness checks, we also examine effects on total employment, including workers earning $30 or more.

Table 1 compares the wages, sectoral composition, and size of firms in the database to nationally representative data. The average hourly wage among sub-$30-per-hour workers in the credit bureau database was $14.74 while the average wage among U.S. hourly workers in the Current Population Survey ("CPS") was $16.82. The share of workers earning less than $30 per hour was also higher in the credit bureau database compared to the U.S. overall: 86% vs. 63%. Coverage in the credit bureau database skews towards retail and services. Over a third of the workers covered in the database

[11] Data on the total number of hourly workers in 2023 (80,538,000) was obtained from the Bureau of Labor Statistics (BLS) report "Characteristics of minimum wage workers, 2023" (available at `https://www.bls.gov/opub/reports/minimum-wage/2023/home.htm#technical-notes`).

[12] Information on the size of the U.S. civilian labor force is from the BLS.

work in retail and over half work in retail and services combined, compared to less than a quarter in the U.S. overall. The share of employment in health and manufacturing in the database was similar to the U.S. average. Professional services, education, and public administration are underrepresented in the database relative to the U.S. overall. Firms in the dataset are larger than the typical firm in the U.S., with the median firm in the database having nearly 2,000 hourly workers while the median U.S. firm has fewer than 5 workers (including both hourly and non-hourly). Below we describe key definitions and outcome variables used in the analysis.

Establishments The underlying credit bureau data provides anonymized company identifiers for each worker. However, geographic identifiers correspond to the workers' place of residence, not place of employment. Accordingly, we use data aggregated to the commuting-zone level in order to approximate location of employment. We refer to company-CZ cells as "establishments."

As a check, we used data on establishment locations for three large retailers in our sample of VMW adopters and found that 99% of workers at these three companies lived in a commuting zone with at least one of their company's establishments. Workers are classified as working for a "policy" company if their company was a large retailer (employment greater than 150,000) that adopted a voluntary minimum wage during our period of study. We describe the procedure for identifying voluntary minimum wage events among large retailers in Section 3.2. All other workers are identified as working for "non-policy" companies.

Wages Our core wage outcome variable is the log average establishment hourly wage for workers earning less than $30 an hour. We construct this variable by calculating the employment-weighted average hourly wage within wage-bin, at the establishment-level, for workers earning less than $30. In other words, we exclude the highest bin, which contains workers earning $30 or more, as we do not expect these workers to be particularly affected by the policies we study.

For policy companies, we also construct measures of total compensation, which includes all other forms of pay, such as bonuses and overtime pay. We use this as an outcome to examine changes in total take-home pay, not just the hourly wage rate, for workers at companies with voluntary minimum wages. Measures of total monthly pay are derived from aggregate year-to-date total gross compensation across all workers in a wage bin.[13]

[13]We use two derived measures, one is the difference in year-to-date total gross pay from the previous month's total gross pay for all workers who can be matched to the previous month. To obtain the average total gross pay per worker, we divide the aggregate monthly pay measure by the number of workers with valid monthly pay measures. Because workers need to be matched to the previous month

Employment We compute the log total number of workers earning less than $30 for each establishment per month by summing the total number of workers in the lowest bin up to the second highest bin and taking the log of this number. We also compute the log total number of all hourly workers, including those paid $30 or more.

Separations Our data include the number of workers each month who match to a record at the company (using the company identifier) in the previous month based on a unique individual identifier. We take this as a measure of monthly retention. We define monthly retention as the number of workers who can be matched to the previous month divided by the previous month's employment. We calculate the separation rate as one minus the monthly retention rate. We also obtain information on the count of new hires at non-policy companies each month who worked at a policy company at some point in the previous 12 months. We normalize this number of separated employees by the current month's employment. We construct this measure for both workers paid under $30 an hour and all hourly workers, including those paid $30 or more.

Hires We obtained information on the number of all new policy (non-policy) company hires as well as all new hires who separated from a non-policy (policy) employer in the previous 12 months.

Worker flows We define the labor market for large retailers with voluntary minimum wages using information on flows between workers across firms in the database. For example, for all new hires at a policy employer who held a job at a non-policy company in the 12 months before their current job, we note the industry of their previous job. For all workers hired at a non-policy company who held a job at a policy company in the previous 12 months, we note the industry of their new job. Our industry-based definition of large retailers' shared labor markets is the set of industries (3-digit NAICS code level) that make up either 80% of new hires' previous industries or 80% of separating employees' future industries. We also use the information on worker flows to identify establishments with a history of hiring workers from policy companies ("poaching establishments") or with a history of workers being hired by policy companies ("feeder establishments").

in order to calculate current monthly earnings, we only have information on monthly pay for workers who work at least two consecutive months. Furthermore, not all workers matched across consecutive months have valid information on monthly earnings constructed in this way (the average missing rate for this variable across all ten years of the data was 14%). A second derived measure we construct uses aggregate year-to-date total gross compensation across all workers with valid total gross compensation divided by aggregate months worked year-to-date across all workers. The upside of this derived measure is that it is available for a higher fraction of observations (the average missing rate across all ten years of the data was less than 3%). A downside to this measure is that it is serially auto-correlated, as it consists of average monthly pay over all prior months in the calendar year.

3.2 Identifying large retailer voluntary minimum wage events

We built a list of large retailers' (those with more than 150,000 workers) voluntary minimum wage events from media announcements and then assigned those publicly-reported events to anonymized companies in the wage distribution data using employer size, industry, and timing of wage distribution shifts observed in the data. Using this approach, we identified 20 events across the anonymized large retailers, whom we refer to as policy companies. An additional three events were identified using a data-based procedure. For each VMW wage level, we flagged all months and wage bins where the share earning below that dollar was at least 10% in a month t and less than 5% in month $t+1$, and the share earning exactly that dollar bin increases to more than 10%.[14]

We focus on the 20 events with no other major company wage policy in the six months preceding or following it.[15] Appendix Table B7 presents information on the events, averaged at the voluntary minimum wage level, to preserve company anonymity. Column 1 shows the nominal wage level of the policy; column 2 shows the number of distinct events per wage level; and column 3 shows the company-employment-weighted average share of workers affected by the policy across all companies with a VMW policy at that wage level. All shares are out of total hourly employment, including workers earning $30 or more. All 20 events affected at least 10% of employment. VMWs at $9 affected 25% of workers on average. VMWs at $10 or more typically affected more than a third of the hourly workforce. $15 voluntary minimums affected more than 50% of the workforce on average. Few states or localities had adopted $15 minimum wages at the time these last four policies were adopted, thereby making the bite of the policy both in the company's wage distribution, and in the local labor market, of particular interest.

Figure 2 shows the change in the share of wages below, at, and above the VMW wage level for large retailers with a $15 voluntary minimum. The x-axis has been re-centered around the month that each retailer adopted the policy. Between 60-80% of workers were paid below the $15 wage bin across the adopting retailers before the policy. The share paid exactly in the $15 wage bin rises from less than 20% to around 70% exactly after the policy is adopted, and the share below $15 falls to zero within months of the policy being adopted. There is only a very small increase in the share paid above the $15 wage bin, as indicated by the change in the share in the $16-17 bins.[16]

Appendix Figure B1 depicts similar stark shifts in the wage distribution for voluntary

[14] We adjusted this procedure slightly for $15 VMW events, which had higher initial bite than other VMW wage levels. For $15 policies, we use 20%, 10%, and 20% for the relevant cutoffs.

[15] In all but one case, the other major policy is another voluntary minimum wage event. The one exception is an event where the company implemented substantial raised wages across several wage bins four months prior to a voluntary minimum wage event, including in bins well above the incoming voluntary minimum.

[16] All shares are calculated out of total employment, including workers earning $30 or more.

minimum wages at other wage levels, and Appendix Figure B2 documents changes in VMW company employment by wage bin before and after VMW adoption across three groups of events around which we organize our analyses: all events, major events–defined as those affecting at least 30% of the company workforce, and $15 VMW events. The figure shows that shifts in the employment distribution are concentrated almost entirely around the minimum with no change in employment in the highest bin, which includes workers earning $30 or more per hour.

4 The effects of company voluntary minimum wages on own wages and employment

In this section, we quantify the effects of voluntary minimum wages on a company's own wage distribution and employment. We then use these results to inform our examination of spillovers from policy company VMWs to the broader labor market.

4.1 Quantifying VMW own-wage and employment effects with a gap design

We use the gap design from studies of national minimum wage policies to examine the impacts of large retailer voluntary minimum wages on their own wages and employment (Card, 1992; Draca et al., 2011; Dustmann et al., 2022). The gap measure captures the percent increase in the company's average hourly wage required to bring all employees in an area up to the new voluntary minimum wage.

The gap measure leverages more variation in bite than using the fraction of workers below the minimum wage. For example, for a $15 voluntary minimum wage, an area where all workers are paid $10 has a larger gap than an area where all the workers are paid $12 an hour (50% vs. 25%). In an area where all workers are paid $7.50, the gap takes on a value of 1 (or 100%).

More formally, we define the gap for a given company by month t and commuting zone c as:

$$\text{GAP}_{c,t} = \frac{\sum_{w=7}^{29} \max\{0, N_c^{wt}(MW - w)\}}{\sum_{w=7}^{29} N_c^{wt} w}, \tag{1}$$

where w is the wage bin $w \in [7, 29]$, and N_c^{wt} is the number of workers in that wage bin by CZ-month. Wage bin 7 corresponds to workers earning less than $8 per hour, and wage bin 29 corresponds to workers earning at least $29 and up to, but not including,

$30 per hour.[17]

We average the gap measure over four pre-policy months:

$$\overline{\text{GAP}}_c = \sum_{t=-6}^{-3} \text{GAP}_{c,t}.$$

We use the first three months of the event window, months -6 to -3, in order to build in a placebo-in-time robustness check into our analysis. If wage measures are noisy, then our approach may spuriously identify cells as low wage and estimated wage effects may represent reversion to the cell's mean as opposed to true increases in the average hourly wage in that bin. A jump in wages that reflects mean reversion would manifest in the period corresponding to the first period after the period used to measure the gap.

We estimate a stacked event study around 20 firm VMW events. We construct a dataset for each event that consists of the CZ-by-month wages and other outcomes for the policy company only, for a 12-month window centered around each event (6 months pre- and 6 months post). We stack these datasets and estimate the effect of our continuous treatment variable, the gap measure in equation 1, on log average hourly wage using the following equation[18]:

$$\log w_{c,t} = \alpha + \sum_{k=-6}^{5} \beta_k \overline{GAP}_c \times \mathbb{1}_{[t=k]} + \eta_c + \delta_t + \varepsilon_{c,t} \tag{2}$$

In the above equation, $\log w_{c,t}$ represents log average hourly wages in CZ c in month t; $\overline{GAP}_c$ is the average pre-period gap in CZ c, which we interact with an event-time indicator, $\mathbb{1}_{[t=k]}$; and η_c and δ_t represent CZ and month fixed effects, respectively. In addition, every regressor is interacted with an event-specific indicator, not shown above.

In addition to our event-study analysis, we perform difference-in-differences analyses where we pool the pre- and post-policy periods and estimate the average change in wages and other outcomes relative to the pre-period. Specifically, we estimate the following model:

$$Y_{c,t} = \tilde{\alpha} + \tilde{\beta} \overline{GAP}_c \times \text{Post}_t + \tilde{\eta}_c + \tilde{\delta}_t + \tilde{\varepsilon}_{c,t} \tag{3}$$

where Y_{ct} is the outcome of interest, including log average hourly wage, log employment, the separation rate, and the year-on-year change in log new hires. Once again, all regressors are interacted with event-specific dummies.

[17] We do not know the exact wage bin of workers earning $30 or more, so we exclude this bin from our gap measure and from our wage analysis. We analyze the effects of VMWs on total employment, including the $30-plus bin, in a robustness check discussed in Section 4.2.

[18] We provide detailed quality checks on our dataset of policy company stacked events in Appendix B (see Appendix Tables B1-B6).

Thus, our analysis compares wages within a company across locations with smaller versus larger gaps from the company's VMW. The coefficient on the gap measure can be interpreted as the impact on log average hourly wages or other outcomes of moving from a gap of 0 to a gap of 1. Standard errors in all specifications are clustered at the commuting zone level, the level at which the gap varies for each event.

4.2 Results on own wages and employment

Below we report the effects of policy company voluntary minimums on their own wages and employment–examining several facets of the latter, from overall employment to separations to new hiring behavior. We begin with an examination of how average wages at the CZ-level for each policy company evolve in areas facing higher vs. lower gaps vis-à-vis the company voluntary minimum.

Wages Figure 3 shows the impact of company $15 voluntary minimum wage events on the log average establishment wage among workers earning less than $30 an hour. The figure shows that as a CZ's gap moves from 0 to 1, average hourly wages increase by around 80 log points, starting exactly the month the policy is adopted. To understand the magnitude of this change, the average gap among CZs for these policies was 0.11. Multiplying the coefficient by this number, going from 0 to the average gap of 0.11 is associated with an increase of about 9%.

The average gap measure conveniently provides the expected increase if areas with a gap of 0 experienced no wage increase, and in areas with a positive gap, there was compliance with the policy and no spillovers to higher wage bins. If areas with a gap of 0 experienced increases, then the estimated coefficient from the gap design would be smaller than the average gap. If wage bins higher than the minimum experienced substantial increases in areas with positive gaps, the estimated coefficient would be larger than the average gap. We find that the estimated coefficient is close to but slightly smaller than the average gap, more consistent with comparison areas receiving some boost or incomplete compliance with the policy in positive gap areas. The descriptive evidence from Figure 2 suggests that there is strict compliance with the policy.[19]

As discussed in Section 4.1, our analysis includes a built-in placebo-in-time robustness

[19]To investigate whether areas with zero gap also experience hourly wage increases, in Figure C1, we plot log average hourly wages normalized to the month before the policy in two types of CZs, those with a gap of zero and those with positive gaps. CZs with zero gap show only a very slight increase in average hourly wages in the months after the policy is adopted. Given that the gap design estimates the relative effects of the policy moving from areas of lower to higher gaps, however, this small increase in zero-gap CZs could provide one explanation why the average wage increase is 9% rather than 11% (the average gap across all CZs). Nevertheless, the magnitude is highly consistent with what we expect given the average gap across CZs for the $15 VMW events.

check. We measure the gap over months −6 to −3. The black dotted line in Figure 3 reflects the end of the period over which the gap is measured. However, the increase in wages occurs only after the voluntary minimum is adopted. We also conduct a robustness test where we measure the gap a full year before the policy, in months −12 to −9 for company events with no other major company policy within 12 months of the event in question (see Appendix Figure C2).[20] Appendix Figures C3 and C4 show stacked event studies around all large retailer VMWs and around all major VMW events, defined as the subset of VMW events affecting at least 30% of a company's workforce. Wage increases across all events and major events show extremely similar patterns as the $15 VMW events.

Employment How do voluntary minimums affect employment? Figure 4 shows changes in log total establishment employment in panel (a), separation rates in panel (b), the rate of separations to non-policy companies in the database in panel (c), and the year-on-year change in log new hires in panel (d). With the exception of separations to non-policy companies, all results pertain to workers earning less than $30 an hour. The results indicate that employment increases after the policy and that these increases are driven by a reduction in separations as opposed to an increase in new hiring. Panel (a) shows that log total employment under $30 rises after $15 VMWs while panels (b) and (c) show that separations and separations to non-policy companies in the database both fall sharply. Declines in separations occur immediately following the adoption of the new voluntary minimum, and the decline is sustained throughout the six months of the post-policy event window.

Finally, we examine the effect of the policy on new hiring. Because of the seasonality in hiring, we examine the effect of the policy on the change in hiring relative to the same month the previous year, calculated as the year-on-year difference in log new hires that month. Constructed in this way, the effect on new hiring can be interpreted as an impact on the log change relative to the prior year. Thus, our results suggest large reduction in new hiring after policy implementation. We place the magnitudes of these estimates in context in our discussion of Table 2, which reports the impacts on wage and employment outcomes from estimating Equation 3, the difference-in-differences specification.

Total monthly base pay and gross compensation When voluntary minimums are adopted, are there other adjustments that companies make? It might be the case, for example, that hours are reduced or other forms of compensation, such as overtime or bonus compensation, are reduced. To assess whether firms adjust hours or overall

[20] Appendix Figure C2 also shows that wage increases are persistent throughout the 12 months following the VMW event.

compensation, we look at the impact of VMW events on log average monthly pay and log average gross compensation. Appendix Figures C5 and C6 show that both monthly base pay and gross monthly compensation increase after company $15 voluntary minimums, suggesting increases in take home pay.[21]

Magnitudes of employment and turnover effects Table 2 reports difference-in-difference estimates of the effect of VMW events on the various outcomes described above. The table reports results for all 20 VMW events in the first column, major events in the second column–defined as those affecting more than 30% of the workforce, and $15 VMW events. In each column, the sample is restricted to CZ-months with no missing observations for any of the dependent variables. Each row represents a separate regression where the variable in the leftmost column corresponds to the dependent variable. The first row shows the effect of voluntary minimums on log average hourly wages. As we restrict the sample to events with higher and higher average bite in company wage distributions (moving from columns 1 to 3), the impact on hourly wages increases, and we find consistent patterns for the other outcomes.

The remaining rows of Table 2 recap our results on employment, reporting the difference-in-differences estimated effects on log total employment of workers earning under $30 per hour, the separation rate of these workers, the rate of separations of all workers to non-policy companies in the database, and the year-on-year change in log new hires under $30.[22] Scaling the coefficient on the gap measure by the average gap across CZs, total employment of workers earning less than $30 rises by 4.62% after $15 VMW events, by 2.01% after major events, and by 1.25% across all events. The subsequent rows confirm that increases in employment stem from reductions in separations rather than new hires which are, if anything, lower relative to last year's hires during the same calendar month.

Once again scaling the separation rate coefficients on the gap measure as above, separation rates fall by 0.42p.p., 0.57p.p., and 1.09p.p. after all, major, and $15 VMW events,

[21] Both figures show the effect of VMW events on average year-to-date pay divided by months worked year-to-date (see Section 3 for a description of the construction of monthly pay and total compensation variables). Note that this measure only reflects actual monthly pay for the first month of the year. After this, the variable reflects average monthly pay over all months since January of that year. Thus, if a policy is adopted in June, the monthly pay variable reflects the average pay over the past six months. For this reason, we do not expect monthly and gross pay to jump in the month of the policy adoption given that these measures reflect moving averages of pay. Nevertheless, both figures indicate that monthly base pay and gross monthly compensation increase after company voluntary minimums, consistent with increases in overall take home pay. In Appendix Table C1, we show consistent results using the month-to-month difference in year-to-date base pay for those workers who match across two consecutive month and have valid year-to-date base pay.

[22] Appendix Table C2 reports employment, separation, and hiring results for all hourly workers, including those earning $30 or more. Results are similar, consistent with the results being driven by workers earning less than $30 an hour.

respectively. These effects translate to 6.57%, 8.73%, and 15.33% reductions relative to the pre-period mean. Focusing on separations to non-policy companies, separation rates fall by 0.07p.p., 0.09p.p., and 0.19p.p. after all, major, and $15 VMW events respectively. Relative to the average rate of separations to non-policy companies in the pre-period, these effects are sizable, amounting to 5.63%, 6.81%, and 13.48% relative to the mean for all, major, and $15 VMW events, respectively.

The final row of the table presents the total employment elasticity, which ranges from 0.35 to 0.45 across the three groupings of events. The impact on separations (relative to their pre-period mean) is larger, and interpreted as a quit elasticity, suggests a labor supply elasticity of 2.20 to 2.38 under standard dynamic monopsony models.[23]

Overall the effect of company voluntary minimums on employment appears driven by reductions in turnover. Furthermore, if anything, new hires appear to fall after voluntary company minimums, potentially reflecting a reduced need to replace separating employees. These results guide our approach to examining spillover effects of large retailer voluntary minimums on other employers in their shared labor market. In particular, as we narrow in on segments of the labor market that are most likely affected by the VMW events we study, we pay close attention to non-policy establishments with a history of hiring workers from or having workers separate to the VMW employer.

5 Spillover effects of large retailer VMWs

We test for spillover effects of large retailer voluntary minimums on the other companies included in the credit bureau database. We term these companies "non-policy" companies, and we refer to company-by-CZ cells as non-policy establishments. Excluded from this sample are the five anonymous large retailers who form our sample of policy companies studied in the previous section. For each voluntary minimum wage event, we construct a balanced panel of establishments with at least 10 workers per month per establishment for the duration of each event window, which amounts to a 12-month period (6 months pre- and post-event).[24] Across all 20 events, there are 106,597 unique

[23]We estimate the effect of the gap measure interacted with an indicator for the post-period on the separation rate. Thus the units of the dependent variable are percentage points. Comparing the effect on separation rates to the effect on log average wages generates a semi-elasticity of separation rates with respect to wages. To convert this semi-elasticity to an elasticity, we compare the effect on separation rates to the pre-period mean separation rate, thus calculating the change in separation rates in percent terms relative to the base period mean. We divide this normalized effect on separations by the effect on log wages to obtain the quit elasticity. We multiple the quit elasticity by two to obtain the labor supply elasticity under the steady state assumption in standard dynamic monopsony models that the recruit elasticity equals the quit elasticity equals one half the labor supply elasticity (see, e.g., Manning (2003) and Bassier et al. (2022)).

[24]We also examine 24-month windows as an additional robustness check.

establishments belonging to 2,873 companies in the analysis sample.[25] Characteristics of the companies in the credit bureau database are described in Table 1.[26]

5.1 Defining the labor market of large retailers with VMWs

We start by testing for spillovers in a very broad definition of the labor market for large retailers with VMWs—all non-policy establishments in the same commuting zones as the large retailer. We then narrow our definition down to commuting zone plus industries connected to the large retailer by worker flows (see description of these data in Section 3). Finally we narrow in on the set of establishments connected to the large retailer through worker flows, as measured in the year prior to the VMW event. Below we describe these latter two definitions of the labor market and how we construct them.

Connected industries Our data on worker flows allows us to identify the previous industry of every new hire and the subsequent industry of every separated employee at the policy companies so long as their previous or subsequent employer is another company in the database. Appendix Figure D1 panels (a) and (b) documents the fraction of large retailers' new hires (separations) whose former (future) employer is another company in the database. Over the 10-year period we study, 1-6% of new hires at policy companies came from non-policy companies in the database and 15-30% of separations flowed to non-policy companies in the database. Focusing on new hires and separations that work at another company in the database, we identify the industries that account for at least 80% of new policy company hires' previous industry as well as those that make up at least 80% of separated employees' future industry. Fifteen 3-digit NAICS industry categories account for either 80% of new hires previous industry or separated employees' future industry (see Appendix Table D1). Just five industries account for over half of new hires and separated employees' past or future industry: administrative and support services, which includes staffing agencies, food service and drinking places (restaurants), food and beverage stores, general merchandise stores, and clothing and clothing accessories stores.[27] Thus, our second most restrictive definition of the labor market consists of

[25] We do not have information on whether a company is in the public versus private sector; however, we anticipate that the vast majority of the companies are in the private sector. Federal government employees are excluded from the database and just 3 of the 2,873 companies are in the public administration sector. However, state and local educational and health services employers may be included.

[26] The table describes both policy and non-policy companies included in the database.

[27] The full set of industries includes: administrative and support services; food services and drinking places; food and beverage stores; general merchandise stores; clothing and clothing accessories stores; couriers and messengers; building material and garden equipment and supplies dealers; hospitals; professional, scientific, and technical services; sporting goods, hobby, book, and music stores; motor vehicle and parts dealers; educational services; miscellaneous store retailers; food manufacturing; and nonstore retailers.

establishments in the 15 strongly connected industries as well as in the same CZ as the large retailers' employees.

Connected establishments Our final and most restrictive definition of the market identifies specific establishments with a history of hiring from or having workers separate to the large retailer. For each event, we identify what we call "poaching establishments"–firm-by-CZ cells that in the 12 months before the VMW event, hired at least one worker who worked at a policy company within 12 months of their hire date. Similarly, we identify "feeder establishments"–those with at least one employee who was hired by the policy company in the 12 months before the VMW event. On average across all VMW events, the percent of establishments that poached at least once from the policy company in the pre-period was 23% while the percent with workers that separated to policy companies in the pre-period was 3%. In Appendix Table D2, we show that being a poaching or feeder establishment in the pre-period is predictive of poaching from or feeding to the policy company in the post-period.

5.2 Estimating spillovers effects on other employers

We estimate a stacked event study around large retailer voluntary minimum wage events. For each event, we estimate the effect of the large retailer's policy on log average hourly wages using the following equation:

$$\log w_{f,c,t} = a + \sum_{k=-6}^{5} \gamma_k \overline{GAP}_c \times \mathbb{1}_{[t=k]} + \rho_{f,c}, + \mu_t + u_{c,t} \tag{4}$$

where the outcome is the log average hourly wage ($\log w_{f,c,t}$) of non-policy establishments from company f and in CZ c in month t. $\overline{GAP}_c$ is the gap measure for the large retailer described in Section 4.2. We include firm-by-CZ fixed effects as well as firm and CZ main effects (not shown above). μ_t are month fixed effects. Errors ($u_{c,t}$) are clustered at the CZ level.

In addition to our event-study analysis, we perform difference-in-differences analyses, where we pool the pre- and post-policy periods and estimate the average change in wages and other outcomes relative to the pre-period. Specifically, we estimate the following model:

$$\log w_{f,c,t} = \tilde{a} + \tilde{\gamma} \overline{GAP}_c \times \text{Post}_t + \tilde{\rho}_{f,c} + \tilde{\mu}_t + \tilde{u}_{c,t} \tag{5}$$

The coefficient of interest is $\tilde{\gamma}$ on the interaction between the gap measure and an indicator for the post period.

Figure 5 plots the estimated γ_k coefficients from equation 4 for a stacked event study of large retailer \$15 VMWs across three samples of non-policy establishments: all establishments in the same CZ as the large retailer in panel (a); all establishments in the same CZ and in industries connected by worker flows in panel (b); and all establishments connected to the large retailer by worker flows in the period before the policy in panel (c). Importantly, we use the same scaling of the y-axis as in Figure 3 where the effect on wages exceeded 80 log points in some months of the post period.[28] The results show no detectable association between the large retailer's gap measure in a commuting zone and the wages of other employers in the six months following a large retailer voluntary minimum. Estimated coefficients are precisely estimated and either centered around zero or extremely close to zero. Appendix Figure D2 presents a zoomed-in y-axis to illustrate the small magnitudes of the monthly coefficients on the gap measure. Appendix Figure D3 documents this lack of relationship over a longer time horizon.

Table 3 reports estimated cross-employer wage elasticities using the regression in Equation 5. The first row reports our estimated $\tilde{\gamma}$ for all establishments in connected industries across all large retailer VMWs. Column 2 reports these effects for all major large retailer VMWs where at least 30% of the workforce was affected. Finally, columns 3-4 reports these effects for \$15 VMW events. Column 4 reports the estimated coefficient when the sample is restricted to connected establishments. The second row reports the effect on the large retailer wage. Finally, the third row of the table reports the estimated cross-employer wage elasticity, where the large retailer wage change is instrumented for using the gap measure interacted with post as in Equation 5.

Across the board we estimate precise, economically negligible effects on the wages of non-policy employers (see row 1 of Table 3). As a consequence we estimate cross-employer wage elasticities that are also precise zeros (row 3). Among establishments with a history of poaching from or feeding to the large retailer, the cross-employer wage elasticity is positive and slightly larger, but still economically negligible, and we can rule out spillovers greater than 0.2% using the upper bound of the confidence interval for this estimate. We also find no evidence of positive spillover effects when focusing specifically on the wages of new hires at non-policy establishments, which we document in Appendix Table D3. The second row of Table 3 mirrors Table 2 from Section 4.2 and shows that policies with more bite have larger effects on policy employers' wages than those with smaller bite.

Table 4 estimates the reduced form effect of large retailer VMWs on connected establishments relative to unconnected establishments. We continue to find evidence of zero spillovers. Finally, Table 5 estimates the reduced form effect of large retailer VMWs on

[28] Appendix Figure shows these same panels with a zoomed in y-axis.

connected establishments relative to unconnected establishments and separately for CZs with above and below median share of large retailer employment. We continue to find no evidence of spillovers.

We hypothesize that because large retailers reduce churn though company VMWs, their impact on labor supplied to other firms may be quite limited. In the next section, we directly examine the extent to which large retailer VMWs affect hiring at non-policy companies. We focus on hiring as the main impact on employment at large retailers appears to be through reductions in separations. First, we discuss results using an alternative treatment variable for testing for spillovers–that of the gap between non-policy establishments and large retailer VMWs.

Non-policy gap with respect to large retailer VMW Our baseline approach to test for spillovers is to examine the relationship between the establishment-level gap between the large retailer's wages and their voluntary minimum with wages at other employers' establishments. We then examine this separately for different samples of establishments that hone in on their most likely competitors. We also examine heterogeneity by the large retailers' employment share. This approach allows us to compare our estimates of spillovers to estimates in prior literature. We also examine spillovers using an alternative methodology that leverages the fact that establishments at other employers vary in the expected change in their relative wage, which we proxy for using the gap in wages at non-policy establishments and the large retailer VMW. As in the case with the analysis of large retailer VMWs on their own wages, we measure the non-policy gap in months -6 to -3, thus building in a placebo-in-time check for mean reversion.

Appendix Figure D4 shows that wages begin rising in months prior to the policy and that the timing of the increase in wages matches the last month used to measure the gap relative to the large retailer's VMW. This is indicative of mean reversion bias in the estimated spillover. To correct for this, we fit a linear trend through pre-period estimates for months -6 to -1, predict the evolution of wages in the post-period consistent with this trend, and detrend post-period estimates using these predicted effects on wages. The detrended effects on wages are presented in Appendix Figures D5 and D6, where the latter figure is scaled to the same y-axis as the effects on policy employers' wages. The figures indicate no detectable spillovers using this alternative methodology.

6 Mechanisms behind and interpretation of limited spillovers

In the previous section, we established that large retailer voluntary minimum wage events have little detectable impact on the wages of other employers in their labor market, including those with a history of hiring from and having workers separate to the large retailer. One interpretation for this lack of an effect is that we have not identified firms affected by the large retailer's policy. In this section, we present evidence on the composition of workers hiring in these labor markets that suggests why VMWs have had limited impacts on the wage setting behavior of companies closely connected to the major retailers. We also discuss the interpretation of our results in light of the limitations of the credit bureau database, which does not include all non-policy employers in a given area. Finally, we discuss the juxtaposition of small spillovers from employer VMWs against the growth of VMWs depicted in Figure 1.

Effects on the composition of hiring at non-policy establishments Table 6 shows the effect of large retailer VMWs on hiring behavior of non-policy establishments. The sample is restricted to connected establishments–those with a history of either hiring from or having workers separate to the large retailer in the 12 months prior to the VMW event. The outcome variable is an indicator for making at least one hire from another company in the database that month. The independent variable is the interaction between the large retailer's gap and an indicator for the post-period. The first three columns report the effect on the probability of hiring a worker from any other company in the database.

Column 1 shows the effect for all VMW events, column 2 for major VMW events, and column 3 for $15 VMW events. Columns 4-6 report the effect of large retailer VMWs on the probability of hiring from the large retailer in a given month. The final 3 columns report the effect of large retailer VMWs on hiring from firms other than the large retailer.

The coefficients in Columns (1) to (3) indicate relatively small increases in the probability of new hires among non-policy companies following the adoption of VMWs. Columns (4) to (6) document effects on the probability of hiring from policy companies. We find large reductions when examining all experiments pooled together (column (4))–roughly a reduction by one-third of the pre-treatment mean–across experiments. Effects are smaller and statistically insignificant in columns (5) and (6), which restrict to major policies and finally to $15 VMW policies specifically.

Finally, columns (7) to (9) provide estimates of the impact on hiring from other non-policy employers in the database. The estimated coefficients are all positive though statistically insignificant and relatively small compared to the mean probability of hiring

from other non-policy employers.

Although the Table 6 results are sensitive to the VMW experiments included in the estimates, they suggest that non-policy firms found other sources to recruit after VMWs reduced turnover among the large retailers.

We examine the effects at the market level in Tables 7 and 8. Specifically, we calculate the log total non-policy new hires in a CZ that come from other non-policy employers vs. policy companies vs. all other employers, excluding the policy companies.

Table 7 estimates indicate that non-policy companies increased hiring following the introduction of VMWs, but those hires came from non-policy firms rather than the large retailers (i.e., the coefficients for hiring from large retailers are negative while the coefficients measuring hires from other non-policy companies are positive).

Table 8 breaks the sample further into CZs grouped in terms of the relative size of large retailers (measured as their employment share being above or below median values). The coefficients once again indicate modest increases in hiring following the introduction of VMWs in non-policy firms where the gap for those non-policy firms are larger, but that the source of that hiring is from other non-policy firms rather than the large retailers.

Generalizability of zero spillovers result Although the results above suggest we have identified non-policy employers whose hiring behavior is affected by large retailer VMWs, the employers covered by the database do not represent all of employment in the commuting zones where larger retailers operate. Comparing total hourly employment across firms in the database to total U.S. hourly employment in the BLS, the credit bureau database covers less than a quarter of hourly workers in the U.S., leaving over 75% of workers outside the database. Appendix Table E1 shows the share of all employment as well as the share of employment in connected industries covered by the non-policy employers in each database. We have already shown in Table 1 that non-policy firms are larger than the typical firm, on the basis of their hourly workers alone. Thus, smaller firms are far less likely to be included in our analysis. It may be the case that reductions in separations affects these smaller firms more than it does the employers in our database. For example, it is possible that smaller firms are less able to adjust their hiring practices in the wake of large retailer VMWs and thus face a decline in the labor available to them after a large retailer VMW. Assessing this possibility using payroll data for smaller firms operating in these areas may be illuminating.

Interpretation of limited VMW spillovers We have documented that large retailer VMWs reduce the probability that workers at those companies will be hired by employers with a history of hiring from the large retailer. These findings are consistent with recent accounts of large retailers like Amazon and Walmart that have adjusted wage

and related policies as a result of concerns over turnover (Stone, 2022; Wartzman, 2022). Nevertheless, the overall level of hiring by these employers remains unaffected, suggesting they are able to substitute towards alternative sources of new hires in the wake of large retailer wage changes. This suggests that the market for new hires is relatively thick.[29] Together, we believe the evidence points to little role for local labor market frictions such as concentration playing a role in the transmission of large retailers' wage policies to other firms.

This being said, it is clear that large retailer VMWs have proliferated in recent years, indicating they have sufficient market power to unilaterally raise their wage structures, sometimes quite dramatically given the labor markets in which they operate. The marked increase in large retailer introduction of VMWs, as well as their repeated increases of them, could be indicative of national level dynamic wage setting activities. Thus we hesitate to conclude that such policies do not have spillover effects; rather, that imperfect competition at the local labor market level does not appear to be a mechanism for transmission of these policies. Our results therefore point to the need for further examination of the strategic interactions of large employer wage determination at the national level.

7 Conclusion

In February 2014, Gap Inc made headlines by announcing it would institute a company-wide minimum pay rate of $9 an hour (CBS News, 2014). Since then, hundreds of employers have followed suit, with another surge in voluntary minimum creations and increases during the rapid economic recovery from the COVID-19 pandemic in 2021 and 2022. Despite their pervasiveness, these voluntary minimum wage policies, which have been adopted by some of the largest employers in the U.S., have not been systematically studied.

In this paper, we examine 20 different voluntary minimum wage events that we identified as occurring at 5 anonymous large retailers since 2014. Over the last 9 years, the events we study have affected over 3 million jobs at these employers, with a number of these voluntary minimums resulting in wage increases for a majority of workers at the relevant company.

With detailed data on company wage distributions and employment for thousands of companies, including several large retailers with voluntary minimums, we study both the direct and spillover effects of these policies.

In some locations, workers for the company were paid significantly below the voluntary minimum before the policy came into place. We use the local gap between workers' wages

[29] One additional potential reason for the minimal impact on labor supply to other employers is that hiring by the large retailer also appears to decline, though our evidence is less clear on this front.

and the company's voluntary minimum to study the effects of the policy on company wages and employment, including retention and hiring. After a voluntary minimum, wages in a local area increase to a degree commensurate with the local gap between workers' wages and the company's new minimum. Voluntary minimums result in sharp and sizable reductions in separations at large retailers that implement them while we find little evidence that hiring increases after voluntary minimums, suggesting that reducing turnover was the major result of these policies.

Turning to other employers in the same labor market, we examine the spillover effects that have occurred as a result of large retailer voluntary minimums. We focus on narrowing definitions of the labor market, from other employers' establishments in the same commuting zone, to those in industries with a history of worker flows to and from the large retailer, to individual establishments at other employers with a history of hiring from or having workers separate to the large retailer. Across all three definitions of the labor market, we find no detectable impact on other employers' wages. These findings are inconsistent with a local labor market characterized by strategic responses in wages across employers.

When we examine the reasons behind minimal spillover effects of large retailer voluntary minimums, we find that though the degree of hiring workers away from the large retailer falls after a voluntary minimum, other companies' overall rate of hiring does not fall, suggesting substitutability across new hires in these labor markets.

We conclude from this evidence that the voluntary minimum wages that have been adopted by some of the country's largest employers have constituted rather "self-contained" shocks. The sharp increases in retention that result from voluntary minimums is consistent with job quality improving for workers at large companies with the policy. However, the fact that that the key response has been a reduction in labor market churn helps explain why these policies have had so little effect on the wage-setting behavior of competitors, at least at a local level.

To the extent that these types of policies ripple across the low wage sector in the U.S., they do so at a national level—as can be seen by their rapid, successive adoption across major retailers throughout the U.S. Further work on this potentially national level of strategic interaction is therefore warranted.

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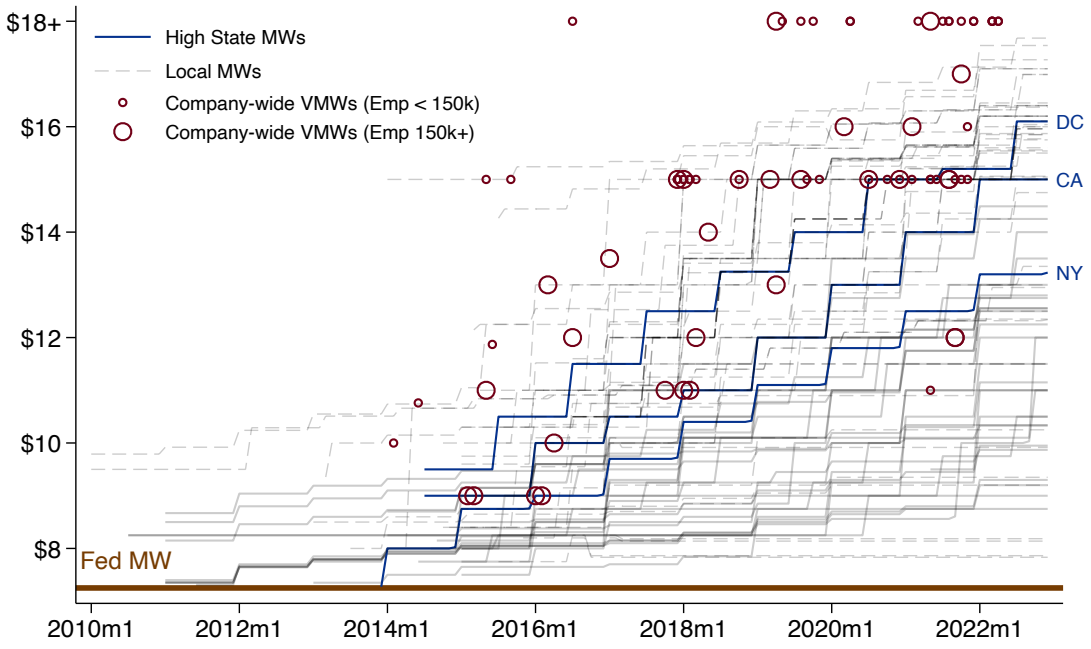
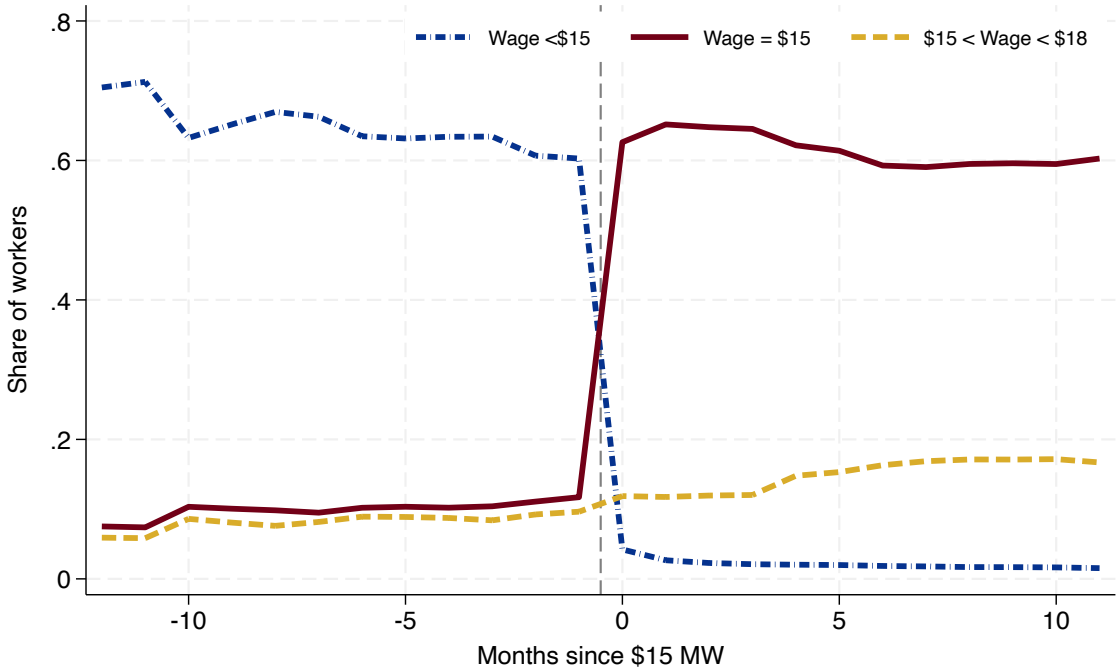
Figure 1: Landscape of minimum wages in the U.S. since 2010



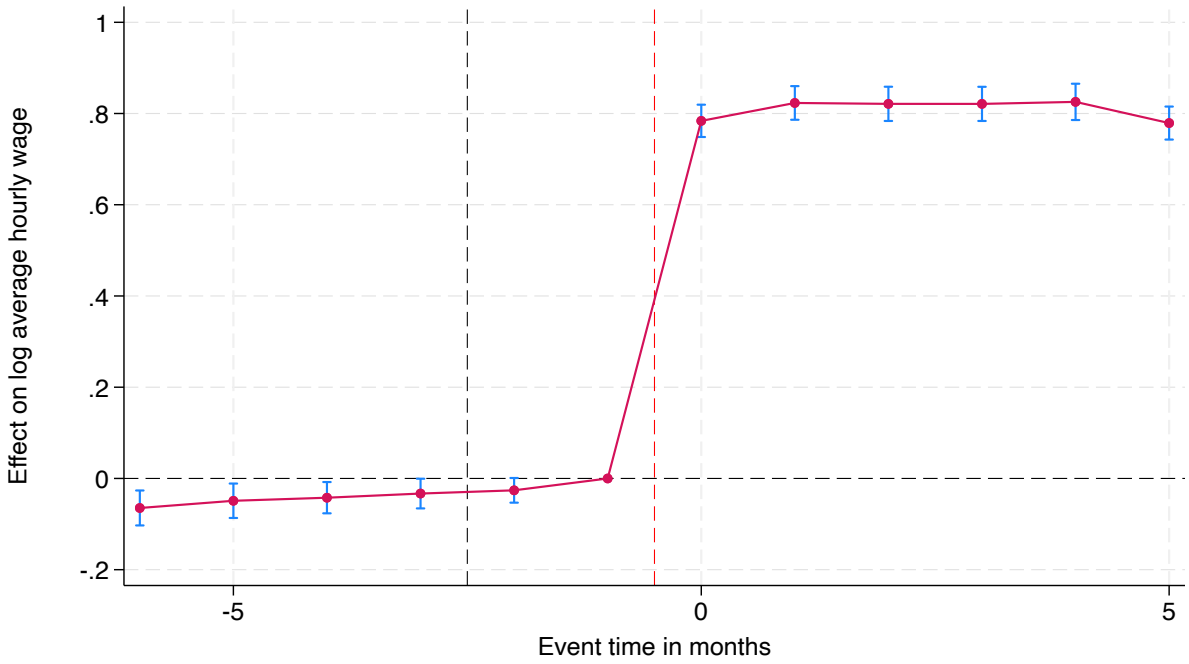
Notes: This figure plots various types of minimum wage policies in the U.S. since 2010, with the nominal minimum wage value on the y-axis. The federal minimum wage of $7.25 is shown in the thick solid brown line at the bottom of the figure. Solid grey lines indicate state minimum wages above the federal minimum wage. Select states are shown in blue. Local minimum wages are indicated by the gray dashed lines. Company-wide voluntary minimum wages by employers are shown in crimson circles. Small circles represent companies with fewer than 150,000 employees while larger circles represent companies with 150,000 employees or more. *Data sources*: State and local minimum wages from Vaghul and Zipperer (2022); voluntary employer minimum wage announcements collected by the National Employment Law Project, edited and supplemented by authors' list. See Appendix A for more detail on the sources underlying the voluntary minimum wage announcements depicted in the above figure.

Figure 2: $15 minimum wage among large retailers (empl. > 150k)



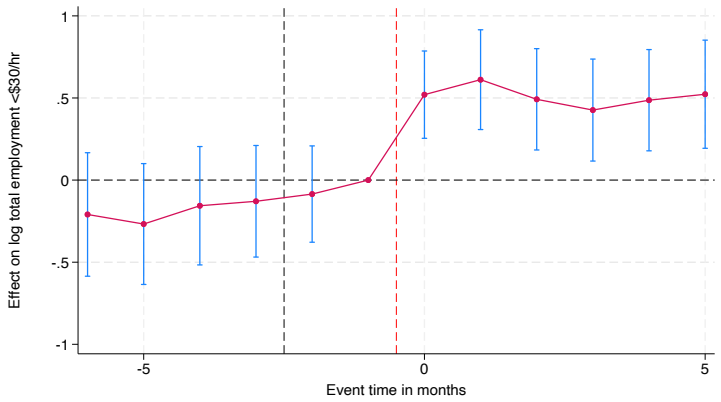
Notes: Share of all hourly workers (including those earning $30 or more) below $15, at $15 up to, but not including, $16, and at $16 up to, but not including, $18 at three large retailers with a voluntary $15 minimum wage, before and after the policy's implementation. *Data sources*: Large credit bureau.

Figure 3: Effect of large retailer $15 MW on own wages

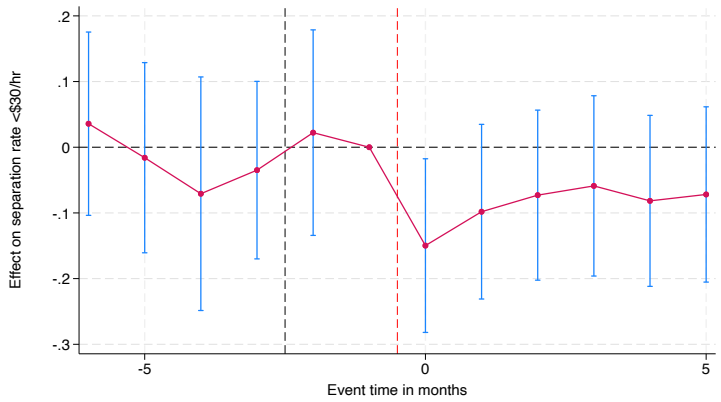


Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between employees' wage bins and the company minimum wage among large retailers with a voluntary $15 minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average hourly wage of employees in that CZ. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

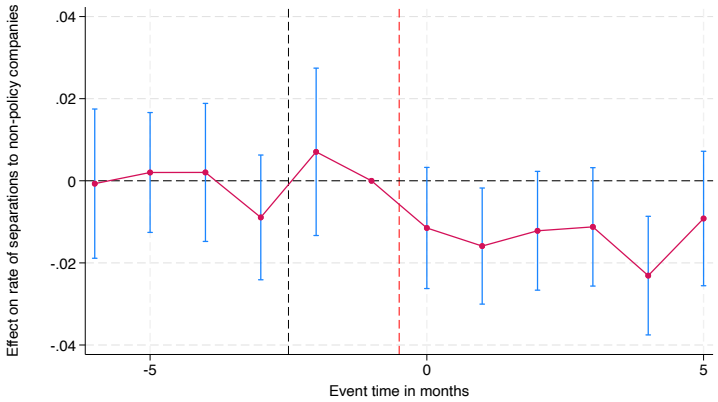
Figure 4: Effect of large retailer $15 MW on own employment, separation rates, and new hiring



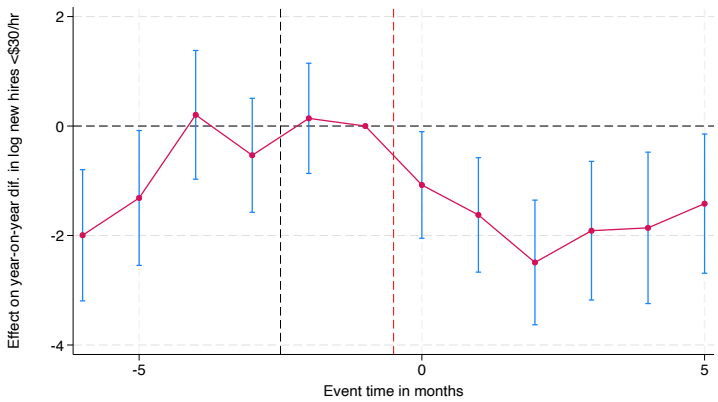
(a) Log employment of workers earning less than $30



(b) Separation rate of workers earning less than $30



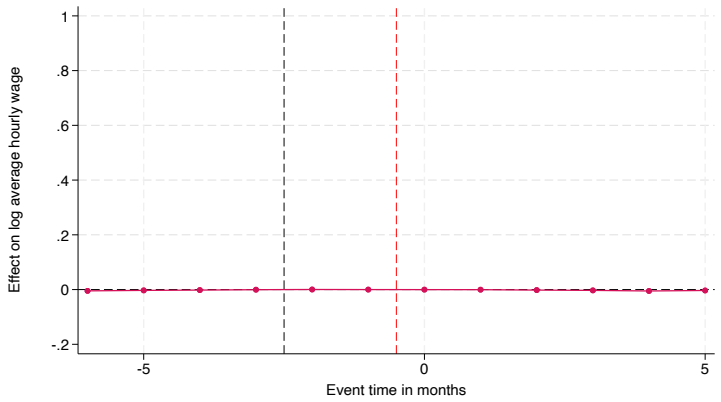
(c) Rate of separations to non-policy companies



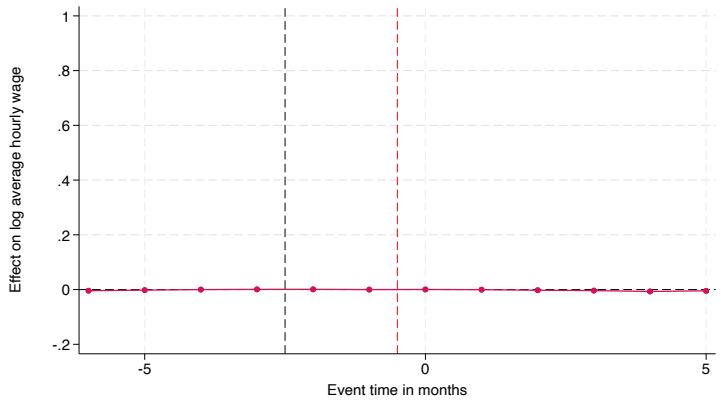
(d) Year-on-year change in log new hires under $30

Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between employees' wage bins and the company minimum wage among large retailers with a voluntary $15 minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects. The dependent variable in Panel (a) is log employment of workers earning under $30; in Panel (b), the dependent variable is the separation rate among employees earning less than $30; in Panel (c), the dependent variable is the rate of separations to non-policy companies in the database; in Panel (d), the dependent variable is the year-on-year change in log new hires earning less than $30 an hour. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

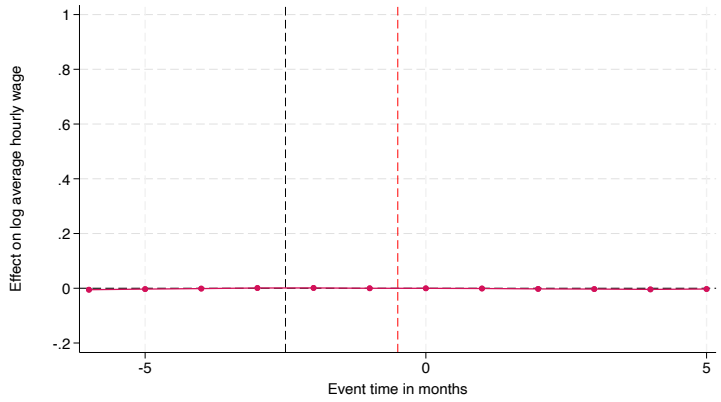
Figure 5: Effect of large retailer $15 MW on other firms' wages



(a) All establishments



(b) Establishments in industries connected by worker flows



(c) All establishments connected by worker flows

Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between binned wages of employees at large retailers with a voluntary $15 minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average hourly wage of non-policy employees in that CZ. Panel (a) includes all non-policy establishments in the CZ; Panel (b) restricts the sample to non-policy establishments in industries connected to the large retailer by worker flows; and Panel (c) restricts the sample non-policy establishments with a history of hiring workers from or having workers separate to the large retailer in the year before the policy is adopted. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 policy employees and to non-policy establishments with at least 10 employees during every month of the event window. 95% confidence intervals included. Appendix Figure D2 provides a zoomed in y-axis. *Data sources*: Large credit bureau.

Table 1: Comparison of credit bureau sample with nationally representative data

	Credit Bureau	CPS/BLS
Wages		
Avg. hourly wage < $30	14.74	16.82
Share with wage < $30	0.86	0.63
Industry		
Retail	0.35	0.11
Health	0.14	0.14
Administrative/Support Services	0.12	0.04
Accommodation/Food Service	0.11	0.07
Manufacturing	0.07	0.08
Education	0.04	0.09
Transportation/Warehousing	0.03	0.04
Finance/Insurance	0.03	0.05
Arts	0.02	0.02
Prof/Sci/Tech Services	0.02	0.08
Unknown	0.02	0.02
Information	0.01	0.02
Public Admin	0.01	0.05
Real Estate	0.01	0.02
Construction	0.01	0.07
Utilities	<0.01	0.01
Wholesale Trade	<0.01	0.02
Other Services	<0.01	0.05
Agriculture	<0.01	0.02
Mining	<0.01	0.01
Management	<0.01	<0.01
Size		
Median firm size	1,902	1 to 4

Notes: This table compares characteristics of hourly workers in the credit bureau database (column 1) compared to hourly workers in the U.S. overall (column 2), from the Current Population Survey (CPS), from 2013 to 2023. The first panel shows the average hourly wage of workers earning less than $30 per hour as well as the share of all hourly workers earning less than $30 per hour in the credit bureau database vs. the U.S. overall. The second panel shows the sectoral composition of sub-$30 hourly workers in the two data sources. The third panel compares median firm size in the credit bureau database (column 1) to median firm size in private sector employment in the U.S. overall (column 2), from the Bureau of Labor Statistics (CPS). In column 1, the sample is restricted to establishments with valid wage data and with any employment within a year of at least one voluntary minimum wage event. The sample for wages in column 2 is hourly workers in the CPS with valid wage data. *Data sources*: Large credit bureau; CPS Outgoing Rotation Group; BLS.

Table 2: Large retailer VMW effects on own wages and employment

	All events	Major events	$15 events
Log avg. wage	0.730***	0.834***	0.879***
	(0.009)	(0.015)	(0.018)
Log employment	0.256***	0.321**	0.399**
	(0.088)	(0.143)	(0.168)
Separation rate	-0.086***	-0.092***	-0.097***
	(0.009)	(0.014)	(0.016)
Separation rate to other firms	-0.014***	-0.015***	-0.017***
	(0.003)	(0.004)	(0.004)
Year-on-year Δ log new hires	-0.860***	-0.798***	-1.123***
	(0.203)	(0.286)	(0.338)
Total employment elasticity	0.351***	0.385**	0.454**
	(0.123)	(0.176)	(0.197)
	[0.109,0.593]	[0.039,0.731]	[0.067,0.841]
Obs	62,742	30,867	9,388
CZs	632	631	380
Events	20	8	4
Average gap	.05	.06	.11
Pre-period mean:			
Sep rate	.064	.065	.071
Sep rate to other firms	.012	.014	.014
YOY Δ log new hires	-.01	-.08	-.18
Month from event FEs	Y	Y	Y
CZ FEs	Y	Y	Y

Notes: This table reports the estimated coefficients on the gap measure interacted with a post-period indicator from regression equation 3 on a stacked policy company dataset of voluntary minimum wage events. Each row of the table represents a separate regression with the following sequence of dependent variables: log average hourly wage, log employment of workers under $30, the separation rate of workers under $30, the rate of separations to other firms in the database (all workers, including those paid $30 or more), the year-on-year change in log new hires under $30, and log employment on log wages, where log wages are instrumented for using the gap measure interacted with a post-period indicator. Column 1 reports the effects for all events, column 2 for major events (those affecting at least 30% of the workforce), and column 3 for $15 dollar events. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. Significance levels are as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources*: Large credit bureau.

Table 3: Cross-employer wage elasticity estimates

	All events	Major events	$15 events	$15: pos. flows
Log avg. wage, non-policy	-0.005***	-0.005***	-0.002**	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)
Log avg. wage, policy	0.701***	0.781***	0.802***	0.756***
	(0.013)	(0.021)	(0.024)	(0.023)
Cross-employer wage elasticity	-0.008***	-0.007***	-0.003**	-0.002
	(0.002)	(0.001)	(0.001)	(0.002)
	[-0.011,-0.005]	[-0.010,-0.004]	[-0.005,-0.000]	[-0.006,0.002]
Obs	5,646,091	2,464,118	1,015,749	397,930
CZs	633	630	380	376
Company X CZ FEs	Y	Y	Y	Y
Month from event FEs	Y	Y	Y	Y

Notes: This table reports the estimated coefficients on the gap measure interacted with a post-period indicator from regression equation 5 on a stacked dataset of voluntary minimum wage events. Each row of the table represents a separate regression with the following sequence of dependent variables: log average hourly wage at non-policy establishments, the log average hourly wage at the CZ level for the policy company, and log non-policy wage on log policy wage, where log policy wage is instrumented for using the gap measure interacted with a post-period indicator. Column 1 reports the effects for all events, column 2 for major events (those affecting at least 30% of the workforce), column 3 for $15 VMW events, and column 4 for $15 events, restricting to non-policy establishments with a history of hiring from the policy company. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). Company-by-CZ, company, CZ, and month fixed effects are included. The sample is restricted to a balanced panel of commuting zones where the policy company has at least 30 employees and a balanced panel of non-policy establishments with a minimum of 10 employees each month. Columns 1-3 restrict to non-policy establishments in connected industries while column 4 restricts to connected non-policy establishments in any industry. Significance levels are as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources*: Large credit bureau.

Table 4: Spillover effects, heterogeneity by flows

Independent variables	All events	Major events	$15 events
Large retailer gap X 1(Post) X 1(Flows)	-0.004***	-0.003**	0.002
	(0.001)	(0.001)	(0.001)
Large retailer gap X 1(Post)	-0.004***	-0.004***	-0.002**
	(0.001)	(0.001)	(0.001)
1(Post) X 1(Flows)	0.002***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)
Obs	5,666,694	2,468,229	1,015,749
CZs	633	630	380
Company X CZ FEs	Y	Y	Y
Month from event FEs	Y	Y	Y

Notes: This table reports the estimated coefficients on the triple interaction of the gap measure, a post-period indicator, and an indicator for a history of employment flows with the policy company (see equation 1 for the definition of the gap measure). The outcome is the log average hourly wage for non-policy establishments. Column 1 reports the effects for all events, column 2 for major events (those affecting at least 30% of the workforce), and column 3 for $15 dollar events. The gap measure is averaged over months -6 to -3 relative to the policy implementation date (t = 0). Company-by-CZ, company, CZ, and month fixed effects are included. The sample is restricted to a balanced panel of commuting zones where the policy company has at least 30 employees and a balanced panel of non-policy establishments in connected industries with a minimum of 10 employees each month. Significance levels are as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources:* Large credit bureau.

Table 5: Spillover effects, heterogeneity by flows and employment share of the large retailer

	≤ Median emp. share			> Median emp. share		
Independent variables	All events	Major events	$15 events	All events	Major events	$15 events
Large retailer gap X 1(Post) X 1(Flows)	-0.003**	-0.001	0.004**	-0.005***	-0.005***	-0.004*
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Large retailer gap X 1(Post)	-0.007***	-0.008***	-0.006***	0.001	0.002	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
1(Post) X 1(Flows)	0.002***	0.001***	0.001*	0.002***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Obs	3,043,378	1,330,663	422,889	2,828,636	1,005,474	449,555
CZs	360	341	263	602	581	275
Mean policy emp. share	0.026	0.031	0.007	0.051	0.058	0.023
Company X CZ FEs	Y	Y	Y	Y	Y	Y
Month from event FEs	Y	Y	Y	Y	Y	Y

Notes: This table reports the estimated coefficients on the triple interaction of the gap measure, a post-period indicator, and an indicator for a history of employment flows with the policy company (see equation 1 for the definition of the gap measure). Columns 1-3 limit the sample to firms in CZs where the large retailer's share of employment is less than or equal to the median. Columns 4-6 limit the sample to firms in CZs where the large retailer's share of employment is greater than the median. The outcome is the log average hourly wage for non-policy establishments. Columns 1 and 4 report the effects for all events, columns 2 and 5 for major events (those affecting at least 30% of the workforce), and columns 3 and 6 for $15 VMW events. The gap measure is averaged over months -6 to -3 relative to the policy implementation date (t = 0). Establishment and month fixed effects are included. The sample is restricted to industries connected to the large retailer by worker flows and to a balanced panel of non-policy establishments with a minimum of 10 employees each month of the event window. Significance levels are as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources:* Large credit bureau.

Table 6: Large retailer VMW effects on the composition of hiring at connected non-policy establishments

	Probability of new hires			Probability of new hires from large retailer			Probability of new hires from other firm(s)		
Independent variable	All	Major	$15	All	Major	$15	All	Major	$15
Large retailer gap X 1(Post)	0.001	0.036**	0.024	-0.072**	-0.025	0.001	0.004	0.035**	0.023
	(0.015)	(0.016)	(0.016)	(0.028)	(0.030)	(0.032)	(0.015)	(0.017)	(0.016)
Obs	2,582,556	1,374,612	402,720	2,582,556	1,374,612	402,720	2,582,556	1,374,612	402,720
CZs	630	627	376	630	627	376	630	627	376
Dep var pre-treat mean	.79	.78	.81	.22	.22	.19	.78	.77	.8
Company X CZ FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month from event FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table reports the estimated coefficients on the gap interacted with a post-period indicator from regression equation 5, where the dependent variable is the probability of hiring workers from different sources in a given month. (See equation 1 for the definition of the gap measure.) In columns 1-3, the outcome is the probability of any new hiring. In columns 4-6, the outcome is the probability of new hiring from the large retailer. In columns 7-9, the outcome is the probability of new hiring from other firms, i.e., not the large retailer. Columns 1, 4, and 7 report the effects for all events, columns 2, 5, and 8 for major events (those affecting at least 30% of the workforce), and columns 3, 6, and 9 for $15 VMW events. The gap measure is averaged over months -6 to -3 relative to the policy implementation date (t = 0). Company-by-CZ, company, CZ, and month fixed effects are included. The sample is restricted to connected non-policy establishments with a minimum of 10 employees each month. Significance levels are as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources:* Large credit bureau.

Table 7: CZ-level effects of VMWs on hiring from large retailers

	All events	Major events	$15 events
Log new hires	0.141**	0.108*	-0.008
	(0.055)	(0.062)	(0.059)
Log large retailer new hires	-0.367*	-0.255	-0.306
	(0.203)	(0.295)	(0.344)
Log other new hires	0.130**	0.102	-0.028
	(0.057)	(0.066)	(0.060)
Obs	156,714	71,384	23,208
CZs	623	614	379
Pre-treatment mean			
Log new hires	5.51	5.43	6.26
Log large retailer new hires	1.83	1.99	1.7
Log other new hires	5.37	5.28	6.12
CZ FEs	Y	Y	Y
Month from event FEs	Y	Y	Y

Notes: This table reports the estimated coefficients on the gap interacted with a post-period indicator from regression equation 3 on a stacked dataset of VMW events, where the dependent variables capture the composition of total non-policy new hires at CZ-level. (See equation 1 for the definition of the gap measure.) Each row of the table represents a separate regression with the following sequence of dependent variables: log total non-policy hires from any other employer in the database, log total non-policy new hires from the policy company, and log total non-policy new hires from any other employer in the database, excluding the policy company. Column 1 reports the effects for all events, column 2 for major events (those affecting at least 30% of the workforce), and column 3 for $15 VMW events. The gap measure is averaged over months -6 to -3 relative to the policy implementation date (t = 0). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 policy employees every month during the event window. Significance levels are as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources:* Large credit bureau.

Table 8: CZ-level effects of VMWs on hiring from large retailers, heterogeneity by employment share

	≤ Median Employment Share			> Median Employment Share		
	All events	Major events	$15 events	All events	Major events	$15 events
Log new hires	0.045	0.015	-0.067	0.218***	0.180*	0.008
	(0.071)	(0.087)	(0.081)	(0.081)	(0.094)	(0.092)
Log large retailer new hires	-0.672*	-0.884	-0.927	-0.471**	-0.425	-0.582*
	(0.375)	(0.556)	(0.638)	(0.213)	(0.287)	(0.341)
Log other new hires	0.077	0.052	-0.042	0.185**	0.161	-0.020
	(0.074)	(0.091)	(0.083)	(0.085)	(0.101)	(0.092)
Obs	79,558	36,335	11,640	77,156	35,049	11,568
CZs	422	400	266	546	518	274
Mean policy emp. share	0.043	0.046	0.008	0.099	0.106	0.027
Pre-treatment mean						
Log new hires	6	5.88	6.43	5	4.95	6.09
Log large retailer new hires	1.93	2.06	1.27	1.72	1.91	2.1
Log other new hires	5.87	5.76	6.3	4.84	4.79	5.94
CZ FEs	Y	Y	Y	Y	Y	Y
Month from event FEs	Y	Y	Y	Y	Y	Y

Notes: This table reports the estimated coefficients on the gap interacted with a post-period indicator from regression equation 3 on a stacked dataset of VMW events, where the dependent variables capture the composition of total non-policy new hires at CZ-level. (See equation 1 for the definition of the gap measure.) Each row of the table represents a separate regression with the following sequence of dependent variables: log total non-policy hires from any other employer in the database, log total non-policy new hires from the policy company, and log total non-policy new hires from any other employer in the database, excluding the policy company. Columns 1-3 limit the sample to firms in CZs where the large retailer's share of employment is less than or equal to the median. Columns 4-6 limit the sample to firms in CZs where the large retailer's share of employment is greater than the median. Columns 1 and 4 report the effects for all events, columns 2 and 5 for major events (those affecting at least 30% of the workforce), and columns 3 and 6 for $15 VMW events. The gap measure is averaged over months -6 to -3 relative to the policy implementation date (t = 0). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 policy employees every month during the event window. Significance levels are as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources:* Large credit bureau.

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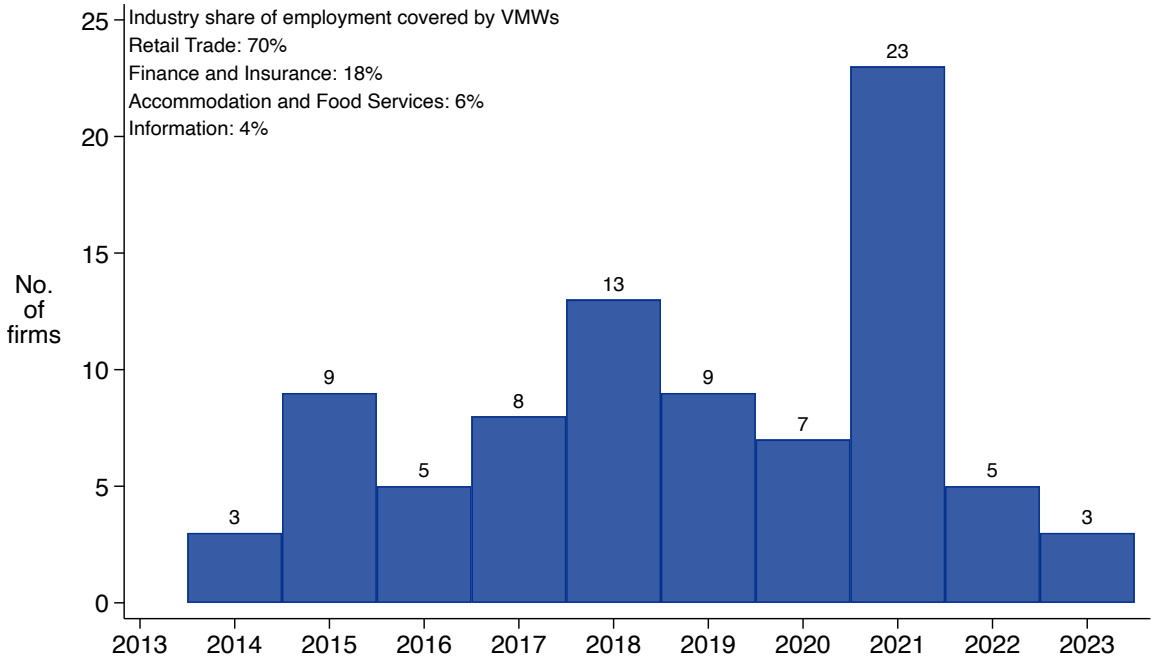
A Voluntary employer minimum wages: additional descriptives

This appendix provides descriptives on company-wide voluntary minimum wage policies adopted by U.S. employers between 2014 and 2023. Figure A1 shows the number of voluntary minimum wage announcements per year as well as the industry breakdown of employers adopting VMWs. Figure A2 shows the number of new employers adopting VMWs each year over the same time period. Policies and new adoptions spike in 2021 during the sharp surge in demand following the recovery from the COVID-19 pandemic. Details of all VMW policies are listed in Table A1, followed by the news sources underlying the database.

Analysis sample of voluntary minimum wages Using an external dataset of voluntary employer minimum wages assembled by the National Employment Law Project and supplemented through our team's own checks and research, we were able to identify 20 voluntary minimum wage events by five employers. Focusing on the largest employers in the database–those with over 150,000 workers in August of 2023–we were able to identify 6 employers whose wage distributions underwent sharp changes in keeping with a new hourly minimum wage at the firm. We were able to match 20 of these policies to a database of voluntary employer minimum wage events based on the company's industry, employment size, and the timing and level of the company's minimum wage. The three additional events were confirmed via media searches.

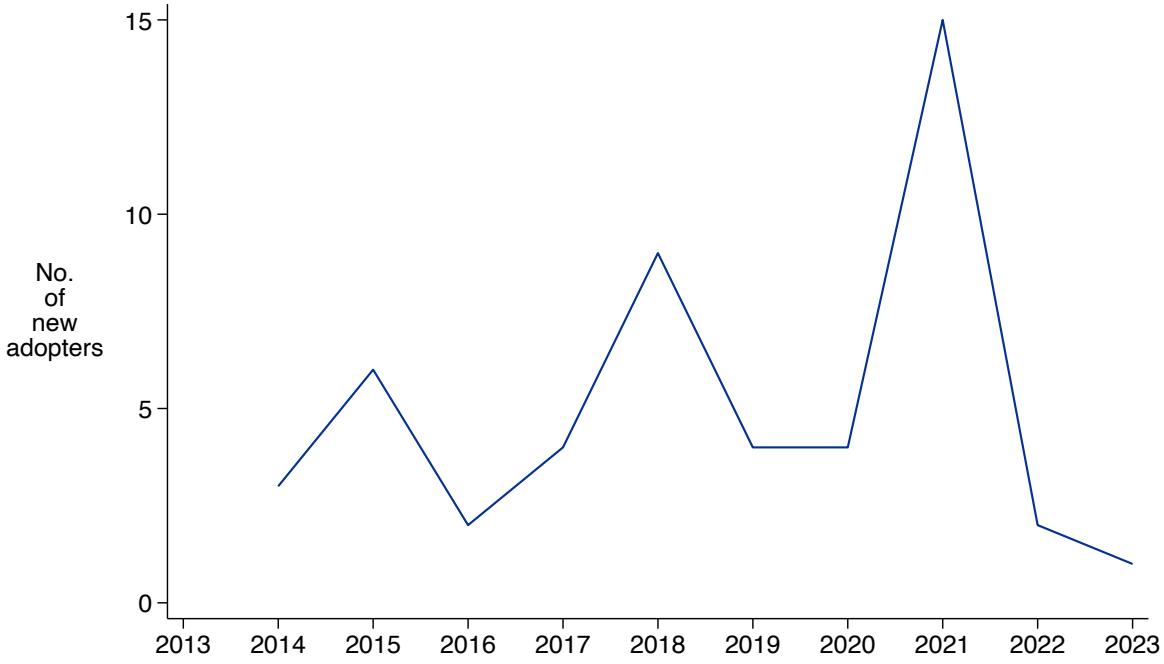
Voluntary minimum wages not studied. According to the database, 16 companies with employment greater than 150,000 adopted a voluntary company minimum wage. We were unable to find a match for 11 of these companies in the database. However, we do find a match for 4 out of the 5 largest employers with voluntary minimum wages.

Figure A1: Employer voluntary minimum wage adoptions, 2014-2023



Notes: The figure depicts the number of nation-wide voluntary minimum wage announcements from 2014 to 2023 across employers, as well as their industry composition (weighted by employment). *Data sources*: NELP and authors' additions. See Appendix Table A1 for descriptions of each policy, followed by a list of all news sources used to assemble the database.

Figure A2: New employer voluntary minimum wage adoptions, 2014-2023



Notes: The figure depicts the number of new employers adopting nation-wide voluntary minimum wages for the first time in the period from 2014 to 2023. *Data sources*: NELP and authors' additions. See Appendix Table A1 for descriptions of each policy, followed by a list of all news sources used to assemble the database.

Table A1: Company-wide voluntary minimum wage announcements and sources

Company	No. Global Employees	Previous MW	New MW	Announce Date	Start Date	Coverage Details
Air Culinaire[1]	352		\$15-\$.	September 18, 2019	October 1, 2019	
Amazon[2,3,4,5]	1,100,000		\$15-\$15	October 1, 2018	November 1, 2018	Reg & Seasonal (FT & PT). 250K reg employees and 100K seasonal impacted. Even those making \$15/hr will receive a raise. Already started increasing wages by 25 - 55 cents for fulfillment centers
Aquesta Financial Holdings[6]	70		\$15-\$.	December 21, 2017	January 1, 2018	
Ascension Healthcare[7]	150,000		\$11-\$.	May 22, 2015	July 5, 2015	
Assurant[8]	13,700		\$15-\$.	May 19, 2021	July 1, 2021	
BB&T[9]	55,126	\$12	\$15-\$.	December 22, 2017	January 1, 2018	
BMO Harris Bank[10]	35		\$15-\$.	January 30, 2018	January 30, 2018	
Bank of America[11,12,13]	217,000	\$15	\$20-\$.	April 9, 2019	March 1, 2020	United States
Bank of America[14,15]	217,000	\$20	\$25-\$.	May 18, 2021	January 1, 2025	United States
Barclays[16]	1,624	\$17	\$20.5-\$.	March 31, 2022	March 31, 2022	United States
Best Buy[17]	90,000		\$15-\$.	July 22, 2020	August 2, 2020	
CVS Health[18]	300,000	\$9	\$11-\$11	February 8, 2018	.	
CVS Health[19]	300,000		\$15-\$15	August 4, 2021	July 1, 2022	Incremental increases beginning immediately, to meet 15 by July 2022, increases in the rest of the wage structure too, will affect 35% of workers
Charter Communications[20,21]	101,700		\$15-\$.	February 2, 2018	August 31, 2018	
Charter Communications[22,23]	101,700	\$15	\$20-\$.	April 2, 2020	March 16, 2022	
Chipotle[24]	104,958		\$20-\$.	May 10, 2021	June 30, 2023	
Chobani[25,26]	2,400	\$13	\$20-\$.	October 26, 2020	January 1, 2021	
Chobani[27]	2,400	\$15	\$20-\$.	June 21, 2023	June 21, 2023	
Citigroup[28]	240,000		\$15-\$.	August 28, 2019	June 1, 2019	United States
Costco[29]	304,000	\$11.5-\$12	\$15-\$.	March 29, 2016	March 29, 2016	United States
Costco[30]	304,000	\$13	\$15-\$.	May 1, 2018	June 11, 2018	Hourly employees
Costco[31]	304,000	\$14-\$14.5	\$15-\$.	March 1, 2019	March 4, 2019	Store employees and supervisors

Company	No. Global Employees	Previous MW	New MW	Announce Date	Start Date	Coverage Details
Costco[32]	304,000	$15-$15.5	$16-$16	February 25, 2021	March 1, 2021	U.S. store employees
Costco[33]	304,000	$16	$16-$16	October 19, 2021	October 25, 2021	United States
Earth Friendly Products[34]	257	$15	$17-$.	August 22, 2014	August 22, 2014	
Facebook[35]	4,000		$15-$.	May 12, 2015	May 1, 2015	Employees of contractors and vendors
Facebook[36,37]	4,000	$15	$15-$.	May 13, 2019	.	All US contract workers
Fifth Third Bancorp[38,39,40,41]	17,437	$12	$15-$.	December 20, 2017	January 3, 2018	
Fifth Third Bancorp[42]	17,437	$15	$18-$.	August 6, 2019	October 28, 2019	
Fifth Third Bancorp[43]	17,437	$18	$20-$.	April 19, 2022	July 4, 2022	
Fifth Third Bank[44]	17,437	$12	$15-$.	January 1, 2018	January 1, 2018	
Fifth Third Bank[45]	17,437	$15	$18-$.	October 28, 2019	October 28, 2019	
Fifth Third Bank[46]	17,437	$18	$20-$.	April 18, 2020	July 4, 2020	
First Horizon National Corp.[47]	7,542		$15-$.	February 15, 2018	March 1, 2018	
First Horizon National Corp.[48]	7,542	$15	$18-$.	March 1, 2022	April 10, 2022	
First Republic Bank[49,50,51]			$20-$.	July 22, 2016	January 1, 2016	
First Republic Bank[52]		$25	$30-$.	March 25, 2021	March 28, 2021	
First Tennessee Bank[53]	31		$15-$.	February 15, 2018	February 15, 2018	
Hobby Lobby[54]	23,000	$17	$18.5-$.	December 15, 2021	January 1, 2022	
Home Depot[55,56]	471,600		$15-$.	February 21, 2023	February 6, 2023	United States
Ikea[57]	31,240	$9.17	$10.76-$.	June 25, 2014	January 1, 2015	United States
Ikea[58]	31,240	$10.76	$11.87-$.	June 23, 2015	January 1, 2016	United States
Ikea[59]	31,240		$11.87-$.	November 10, 2021	January 1, 2022	United States
JP Morgan Chase[60,61]	293,723	$10.15	$11.87-$.	July 12, 2016	January 1, 2019	United States

Company	No. Global Employees	Previous MW	New MW	Announce Date	Start Date	Coverage Details
JP Morgan Chase[62]	293,723	$12-$16.5	$11.87-$.	January 23, 2018	February 25, 2018	United States
Macy's[63]	94,570		$15-$.	November 8, 2021	May 1, 2022	
MetLife[64]	45,000	$15	$20-$.	August 19, 2021	.	
Nationwide Mutual Insurance Co.[65]	34,417		$15-$.	September 9, 2015	June 9, 2016	
PNC[66]	61,545		$15-$.	December 22, 2017	December 31, 2017	
PNC[67,68]	61,545	$11-$15	$18-$.	August 30, 2021	November 22, 2021	
Sam's Club[69]		$11	$15-$15	September 2, 2021	September 25, 2021	
Signet Jewelers[70,71,72]	29,660		$15-$.	February 25, 2021	October 1, 2021	United States
Starbucks[73,74]	402,000		$15-$.	December 12, 2020	August 1, 2022	United States
Synchrony Financial[75]	18,500		$20-$.	July 31, 2021	August 1, 2021	United States and Puerto Rico
T-Mobile[76,77]	71,000	$15	$20-$.	December 10, 2021	December 1, 2021	
TJX Companies[78,79,80]	329,000		$20-$.	February 26, 2015	January 1, 2016	United States
Target[81,82]	440,000		$9-$9	March 1, 2015	April 1, 2015	
Target[83]	440,000	$9	$10-$10	April 1, 2016	May 1, 2016	Hourly workers
Target[84,85,86]	440,000	$10	$11-$11	September 25, 2017	October 1, 2017	Entry level hourly workers, including temp holiday hires
Target[87]	440,000	$13	$15-$15	September 25, 2017	July 5, 2020	Hourly FT & PT team members, also one-time $200 bonus
Target[88]	440,000	$11	$12-$12	March 1, 2018	March 1, 2018	Starting with existing employees
Target[89]	440,000	$12	$13-$13	April 4, 2019	June 1, 2019	Entry level hourly workers, including new seasonal hires
Terrapin Care Station[90,91]	10		$15-$.	November 12, 2019	.	
The Container Store[92]	5,100		$15-$.	October 18, 2021	October 1, 2021	
The Gap[93]	95,000		$10-$.	February 20, 2014	June 1, 2015	United States
Truliant Federal Credit Union[94]	500		$15-$.	March 21, 2018	April 2, 2018	

Company	No. Global Employees	Previous MW	New MW	Announce Date	Start Date	Coverage Details
Under Armour[95]	15,000	$10	$15-$.	May 19, 2021	June 6, 2021	United States and Canada (90% of global workforce)
UnityPoint Health[96]	18,923		$15-$.	December 9, 2020	January 1, 2021	
Vail Resorts[97]	6,900	$12.5	$15-$.	June 7, 2021	August 1, 2021	
Vail Resorts[98,99]	6,900	$15	$15-$.	March 15, 2022	October 1, 2022	North America
Verizon[100]	117,100		$20-$.	April 18, 2022	May 1, 2022	United States
Walgreens[101]	210,500		$15-$.	August 31, 2021	November 1, 2022	
Walmart, Inc.[102]	1,600,000		$9-$9	February 18, 2015	April 1, 2015	Full-time and part-time associates
Walmart, Inc.[103,104]	1,600,000	$9	$9-$9	February 18, 2015	February 20, 2016	All hourly associates hired before January 2016. not applicable to new hires, who start at $9 and mustcomplete 6-month Pathways Training Program
Walmart, Inc.[105]	1,600,000		$9-$.	February 19, 2015	April 1, 2015	
Walmart, Inc.[106]	1,600,000	$9	$9-$.	January 20, 2016	February 20, 2016	United States
Walmart, Inc.[107]	1,600,000	$9-$10	$11-$11	January 1, 2018	February 17, 2018	All hourly associates, applicable to entry level. Eligible employees get one-time cash bonus of $1000.
Walmart, Inc.[108,109]	1,600,000	$11	$11-$11	September 2, 2021	September 25, 2021	United States
Walmart, Inc.[110,111]	1,600,000	$11	$12-$.	September 3, 2021	September 25, 2021	Targeting those who work at registers, in the food and household goods areas and who restock shelves. Ending quartlerly bonuses
Walmart, Inc.[112]	1,600,000	$12	$12-$.	January 24, 2023	March 1, 2023	Store employees
Wayfair[113]	15,745		$12-$.	January 7, 2021	January 3, 2021	United States
Wells Fargo[114]	238,000	$12-$16	$12-$.	January 5, 2017	January 8, 2017	
Wells Fargo[115]	238,000	$13.5-$17	$15-$.	December 20, 2017	March 1, 2018	
Wells Fargo[116,117]	238,000	$15	$15-$.	March 4, 2020	December 1, 2020	United States
Whole Foods[118]	95,000		$15-$15	October 1, 2018	November 1, 2018	

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B Credit bureau database: descriptives and analysis dataset construction

In this appendix we describe the construction of our policy company analysis dataset, including detailed description of data quality checks for our key outcome variables as well as the procedures we implement to adjust for months with missing data at the company level.

B.1 Policy company analysis dataset

We read in raw anonymized datasets of worker counts by company code, worker commuting zone of residence, month-year, and wage bin of gross hourly pay rate for the policy companies whose VMW events we study. The sample is restricted to workers with hourly pay frequency. We drop all company by CZ by month rows with missing pay rate information. The number of records with invalid or missing pay rates is very small, 228 missing observations out of 240 million records. Bins are mutually exclusive categories that range from less than $8, between $8 and less than $9, and so on and so forth until $29 to less than $30, and $30 or more.

We generate a month-year variable based on the archive, as well as month and year variables. We calculate total employment for each establishment, which we define as a CZ-company pair. We also calculate total monthly retained employment for each establishment. We drop the highest bin, which represents workers earning $30 or more.

We calculate the exact average hourly wage per bin as well as average total base pay. We construct two measures of the latter: average total base pay based on year-to-date total base pay divided by year-to-date months worked for workers who can be linked to the previous month and for whom valid total base pay information exists. The second measure is the average of current month's year-to-date total base pay minus last month's year-to-date total base pay across all workers whose pay record from the previous month is available.

For total compensation measures, we do not have current month minus last month's year-to-date total compensation and instead only know year-to-date compensation and months worked year-to-date for all workers for whom this information is available (over 98% of records).

We calculate total employment and monthly retention for workers earning less than $30 per hour. We also count the number of records with monthly pay available (based on the difference between current month and last month's year-to-date pay and total year-to-date base pay divided by months worked year-to-date). We create average pay and log pay variables at the CZ-month level for each company.

We reshape the data into a wide format, such that each observation is a company-CZ-month. The dataset includes variables such as total employment, total retention, average monthly base pay, average gross total compensation, and a separate variable containing the number of workers and retained workers for each wage bin.

We then adjust the data for outliers in reporting based on months with extremely low values for total employment and number of valid records with year-to-date pay. We smooth outlier months for each of our key variables: total employment, average hourly wage, average number of records with valid year-to-date pay, total monthly pay and hours variables, number per wage bin and number retained per wage bin. We identify months each policy company has unusually low reporting of wages. See Section B.1.2 for the rate of low reporting months. For these months, we impute the value as the average of the value for the month prior to the low reporting period and that of the month just following the low reporting period. We do not adjust total monthly pay and hours variables.

B.1.1 Stacked event dataset construction

For each policy experiment, we select observations of the key variables listed above for the anonymized policy company within a year of the policy change, that is, 12 months before to 11 months after. We keep only CZs with at least 30 reported employees at the policy company in each reported month within a year of the policy change. We create indicators for balance within the full time frame (-12 to +11 months from the event) and a short time frame (-6 to +5 months from the event) by identifying CZs with valid employment and wage data for the policy company in each month of the time frame.

B.1.2 Analysis of missings and rate of imputation

Our key outcome variables from the core dataset are average hourly wage, employment, running monthly earnings and hours, and the separation rate (which is a function of retention and last month's employment). Below we show rates of imputed data in the full stacked event study dataset (Table B1) and the stacked event study dataset balanced and limited to the short time frame (-6 to +5 months from the event) (Table B2). Variables are imputed for all commuting zone observations when a variable has a low reported rate in a given month for a given company. As described earlier, we impute the value as the average of the value for the month prior to the low reporting period and that of the month just following the low reporting period. In Table B3 and Table B4, we show the share of observations that are imputed for the full stacked dataset and the balanced sample, respectively, by time from event.

Note that we do not impute the value for current monthly earnings, which is defined as the average of current month's year-to-date total base pay minus last month's year-to-date total base pay across all workers whose pay record from the previous month is available. We are unable to reliably impute the data due to the seasonality of the variable and the lack of coverage. In the tables below, the share imputed for current monthly earnings refers to the share of observations where reporting is unusually low or average earnings are unusually high. However, we do not actually impute these. In Table B5 and Table B6, we show that coverage of the current earnings variable is lower than the coverage of the running earnings variable for the full and balanced stacked datasets respectively.

Table B1: Share of full stacked dataset with imputed variables

Variables	Share imputed	Share imputed (weighted)
Wage bins and employment	0.02	0.03
Running monthly earnings	0.02	0.03
Current monthly earnings*	0.05	0.06
Retention	0.07	0.08

Notes: Share of commuting zone-company-month observations with imputed variables listed by variable category. Running monthly earnings are year-to-date total earnings. Column 2 weighted by employment. Sample is stacked event study dataset of policy employers' wages, employment, earnings, and retention within a year of the policy experiment. Current monthly earnings not imputed - instead, share of observations with unusually low reporting or unusually high average earnings summarized. *Data sources:* Large credit bureau.

Table B2: Share of balanced stacked dataset with imputed variables

Variables	Share imputed	Share imputed (weighted)
Wage bins and employment	0.03	0.04
Running monthly earnings	0.03	0.03
Current monthly earnings*	0.03	0.03
Retention	0.05	0.06

Notes: Share of commuting zone-company-month observations with imputed variables listed by variable category. Running monthly earnings are year-to-date total earnings. Column 2 weighted by employment. Sample is stacked event study dataset of policy employers' wages, employment, earnings, and retention within a year of the policy experiment. Sample is restricted to -6 to +5 months from the policy experiment and balanced on commuting zone-company pairs with data available for all months. Current monthly earnings not imputed - instead, share of observations with unusually low reporting or unusually high average earnings summarized. *Data sources:* Large credit bureau.

Table B3: Share of full stacked dataset with imputed variables, by time from event

Time from event	Share imputed: Employment and wage bins	Running monthly earnings	Current monthly earnings*	Retention
-12	0	0	0	0
-11	0	0.04	0.09	0
-10	0.03	0.06	0.07	0.03
-9	0	0	0	0.04
-8	0	0	0.09	0.09
-7	0	0	0.09	0.09
-6	0.09	0.01	0.01	0.01
-5	0.04	0.04	0.04	0.06
-4	0	0	0	0.04
-3	0.07	0.07	0.07	0.07
-2	0.05	0.10	0.09	0.18
-1	0	0	0.04	0.13
0	0	0	0	0
1	0	0	0	0
2	0.08	0.08	0.08	0.08
3	0	0	0	0.08
4	0	0	0	0
5	0	0	0	0
6	0	0	0	0
7	0	0	0.03	0.03
8	0.13	0.04	0.30	0.30
9	0.12	0.12	0.26	0.31
10	0	0.02	0	0.12
11	0	0	0	0

Notes: Share of commuting zone-company-month observations with imputed variables listed by variable category. Running monthly earnings are year-to-date total earnings. Sample is stacked event study dataset of policy employers' wages, employment, earnings, and retention within a year of the policy experiment. Current monthly earnings not imputed - instead, share of observations with unusually low reporting or unusually high average earnings summarized. *Data sources:* Large credit bureau.

Table B4: Share of balanced stacked dataset with imputed variables, by time from event

Time from event	Share imputed: Employment and wage bins	Running monthly earnings	Current monthly earnings*	Retention
-6	0.08	0.01	0.01	0.01
-5	0.04	0.04	0.04	0.05
-4	0	0	0	0.04
-3	0.08	0.08	0.08	0.08
-2	0.05	0.10	0.10	0.18
-1	0	0	0.05	0.10
0	0	0	0	0
1	0	0	0	0
2	0.08	0.08	0.08	0.08
3	0	0	0	0.08
4	0	0	0	0
5	0	0	0	0

Notes: Share of commuting zone-company-month observations with imputed variables listed by variable category. Running monthly earnings are year-to-date total earnings. Sample is stacked event study dataset of policy employers' wages, employment, earnings, and retention within a year of the policy experiment. Sample is restricted to -6 to +5 months from the policy experiment and balanced on commuting zone-company pairs with data available for all months. Current monthly earnings not imputed - instead, share of observations with unusually low reporting or unusually high average earnings summarized. *Data sources:* Large credit bureau.

Table B5: Share of employment with valid monthly wage data in full stacked dataset

	Share w/ valid earnings	
Time from event	Running	Current
-12	0.98	0.90
-11	0.98	0.89
-10	0.95	0.85
-9	0.98	0.93
-8	0.98	0.81
-7	0.98	0.82
-6	0.99	0.90
-5	0.95	0.87
-4	0.99	0.92
-3	0.92	0.86
-2	0.94	0.84
-1	0.99	0.88
0	0.98	0.90
1	0.99	0.90
2	0.92	0.85
3	0.99	0.93
4	0.99	0.91
5	0.99	0.91
6	0.98	0.91
7	0.99	0.89
8	0.93	0.71
9	0.93	0.74
10	0.98	0.92
11	0.98	0.91

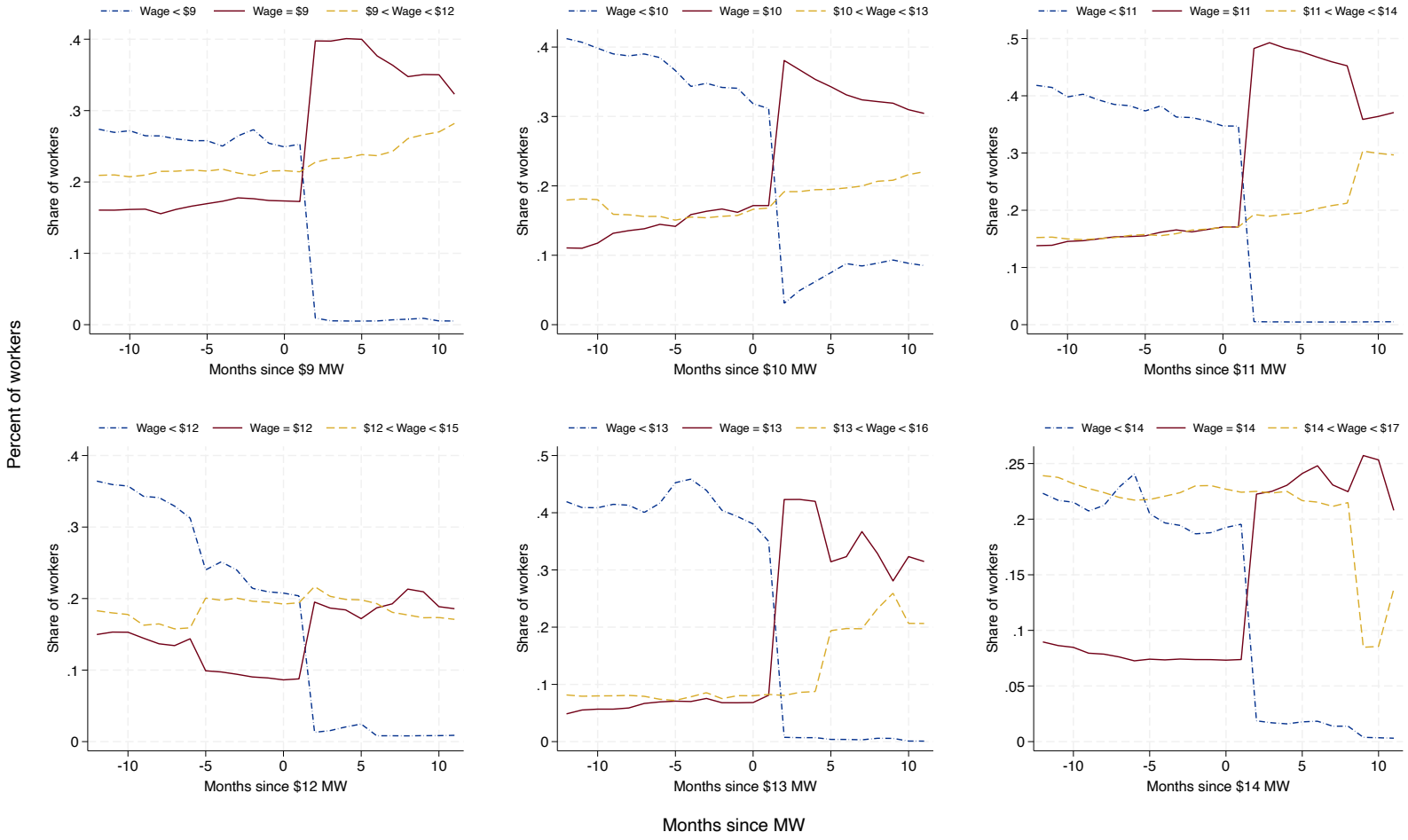
Notes: Share of employees with valid wage data who have valid earnings data with imputed variables listed by variable category. Running monthly earnings are year-to-date total earnings. Current monthly earnings is year-to-date total earnings minus last month's year-to-date total earnings for employees who have valid earnings data in both months. Sample is stacked event study dataset of policy employers' wages, employment, and earnings within a year of the policy experiment. *Data sources:* Large credit bureau.

Table B6: Share of employment with valid monthly wage data in balanced stacked dataset

Time from event	Share w/ valid earnings	
	Running	Current
-6	0.98	0.90
-5	0.94	0.86
-4	0.99	0.91
-3	0.91	0.85
-2	0.94	0.84
-1	0.99	0.87
0	0.98	0.90
1	0.99	0.90
2	0.92	0.85
3	0.99	0.93
4	0.99	0.91
5	0.99	0.91

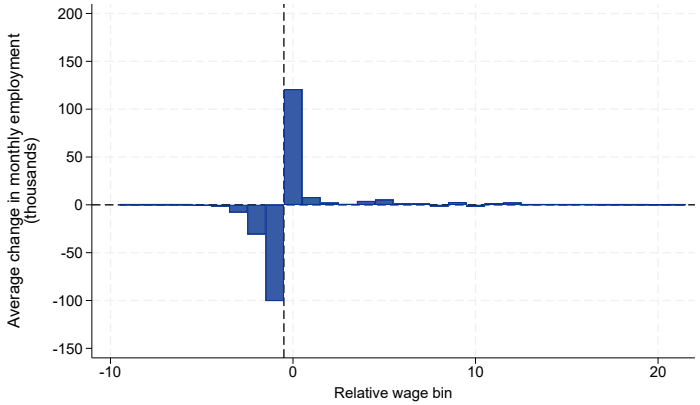
Notes: Share of employees with valid wage data who have valid earnings data with imputed variables listed by variable category. Running monthly earnings are year-to-date total earnings. Current monthly earnings is year-to-date total earnings minus last month's year-to-date total earnings for employees who have valid earnings data in both months. Sample is restricted to -6 to +5 months from the policy experiment and balanced on commuting zone-company pairs with data available for all months. *Data sources:* Large credit bureau.

Figure B1: Share around company voluntary minimum, by wage level

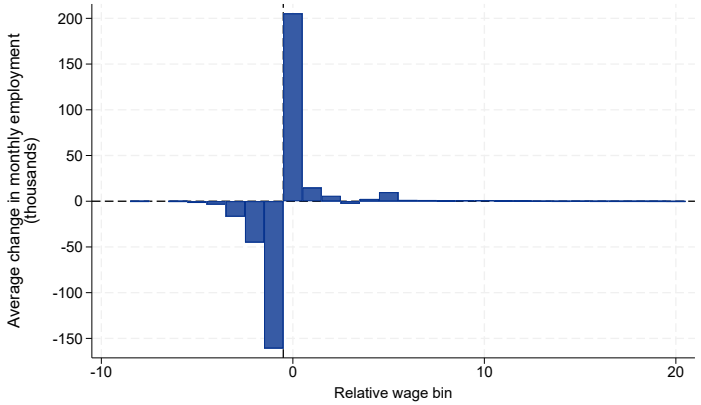


Notes: For each voluntary minimum wage level, the figure shows the share of hourly workers below the VMW, in the wage bin of the VMW, and in the two wage bins above the VMW at large retailers with voluntary minimum wage events, before and after the policy's implementation. *Data sources*: Large credit bureau.

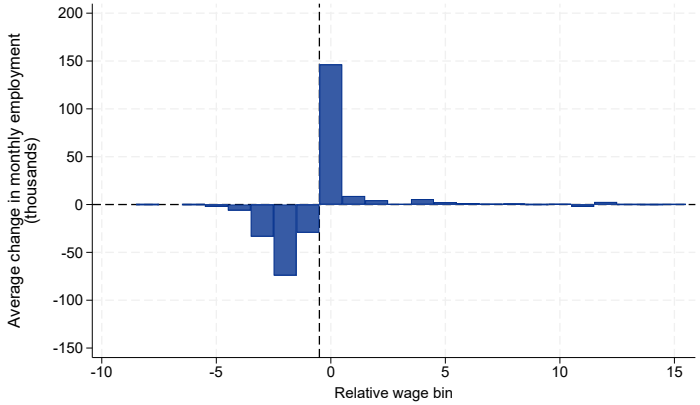
Figure B2: Difference in employment by wage bin at policy companies



(a) All events



(b) Major events



(c) $15 events

Notes: The figure shows the difference in pre versus post average monthly employment for policy companies by wage bin relative to the company minimum wage. The pre-period is the 6 months prior to VMW adoption, and the post-period is the 6 months following adoption. Employment is reported in the thousands. Panel (a) includes all events; Panel (b) includes major events defined as those affecting at least 30% of the workforce; and Panel (c) includes $15 VMW events. *Data sources*: Large credit bureau.

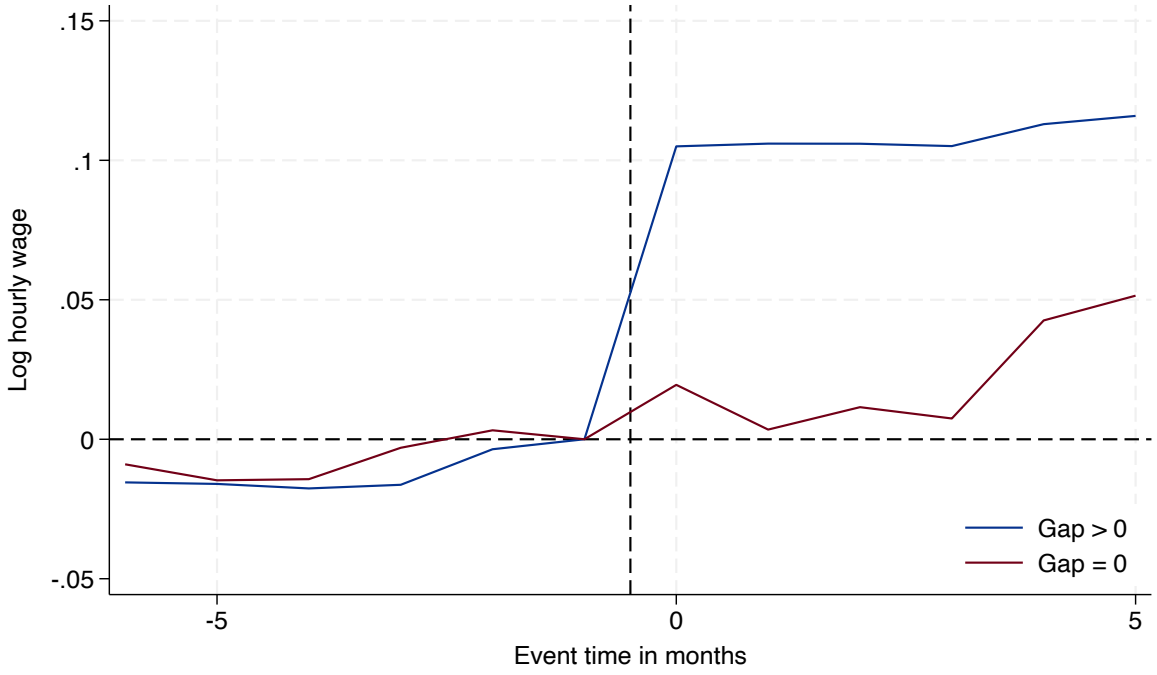
Table B7: Voluntary minimum wage events, by wage level

Minimum wage ($)	Number policies	Avg. share affected
9	3	.25
10	3	.32
11	2	.35
12	2	.42
13	2	.53
14	2	.18
15	4	.61
16	1	.22
17	1	.17

Notes: Number of VMW events and average share affected by voluntary minimum wage level. The share affected is the company-employment-weighted average share affected across all companies with a voluntary minimum policy corresponding to that wage level. Note that the share affected is out of all hourly workers, including those earning $30 or more. *Data sources*: Large credit bureau.

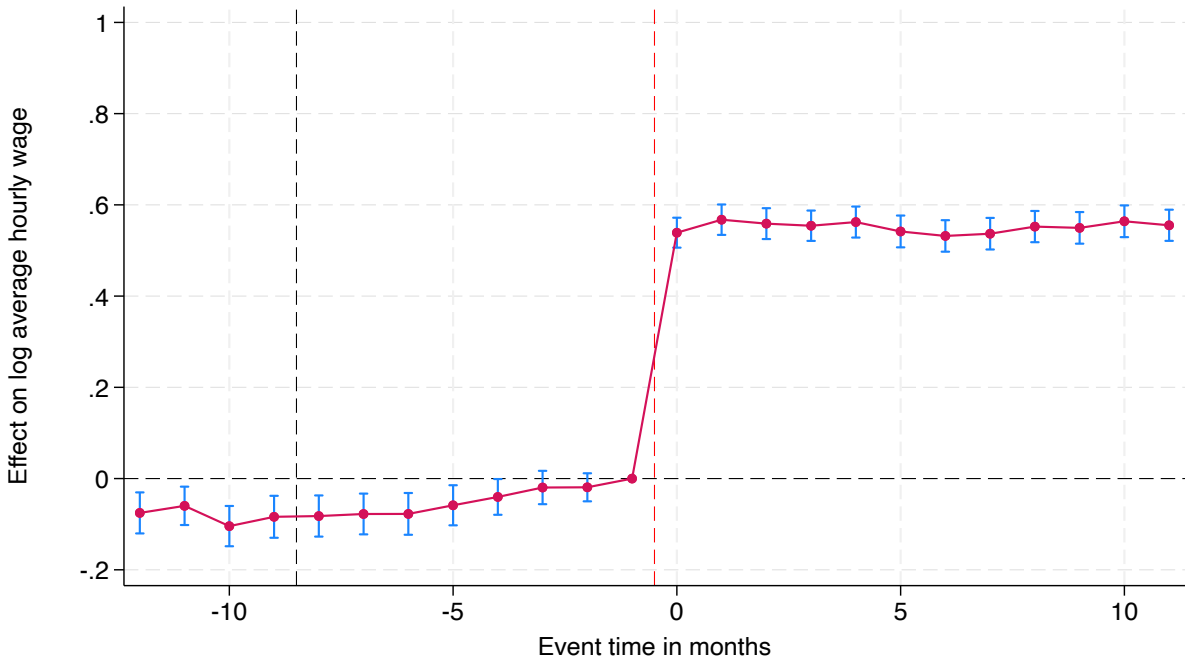
C Additional results and robustness checks for policy company analysis

Figure C1: Raw wage changes in zero gap vs. positive gap CZs



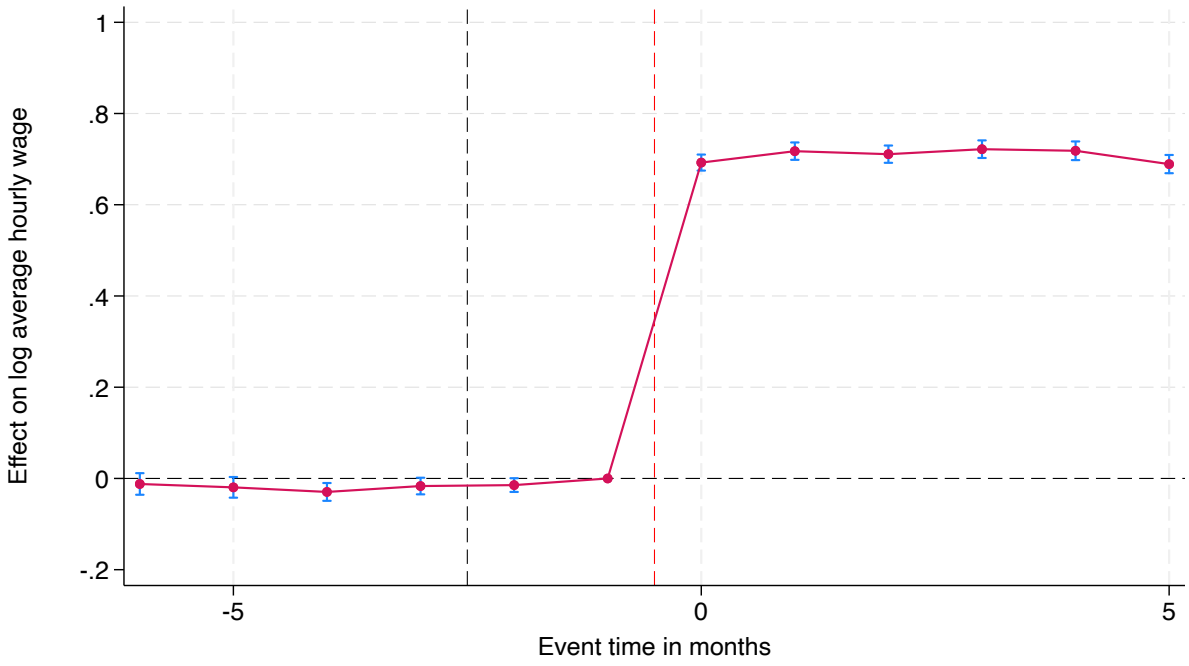
Notes: This figure plots log average wages at the CZ-level for policy companies with a $15 VMW for two groups of CZs: those with a positive gap and those with a gap of zero (see equation 1 for the definition of the gap measure). Log average wages are normalized to their $t = -1$ values. The sample is restricted to commuting zones with at least 30 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

Figure C2: Own wage effects, 24-month event window



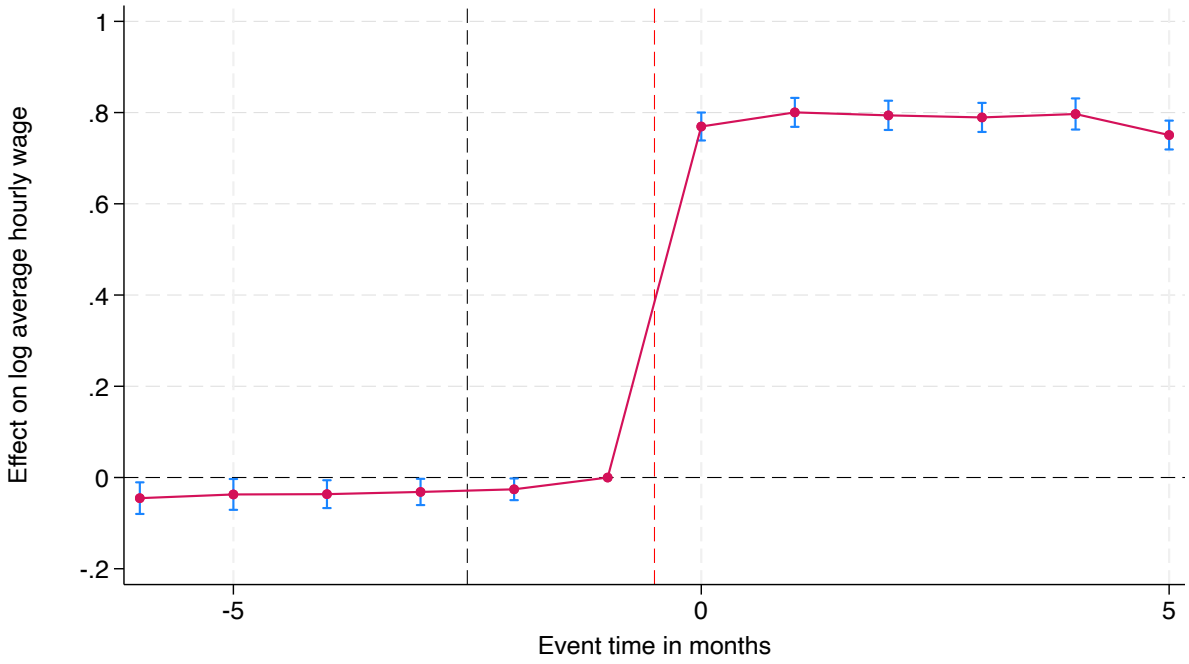
Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between binned wages of employees at large retailers with a voluntary $15 minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects over a 24-month event window. The coefficients are estimated using a stacked regression model, stacking the 8 events with no other policy within 12 months prior and after the event of interest. The dependent variable is log average hourly wage of employees in that CZ. The gap measure is averaged over months -12 to -9 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

Figure C3: Effect of large retailer VMWs on own wages



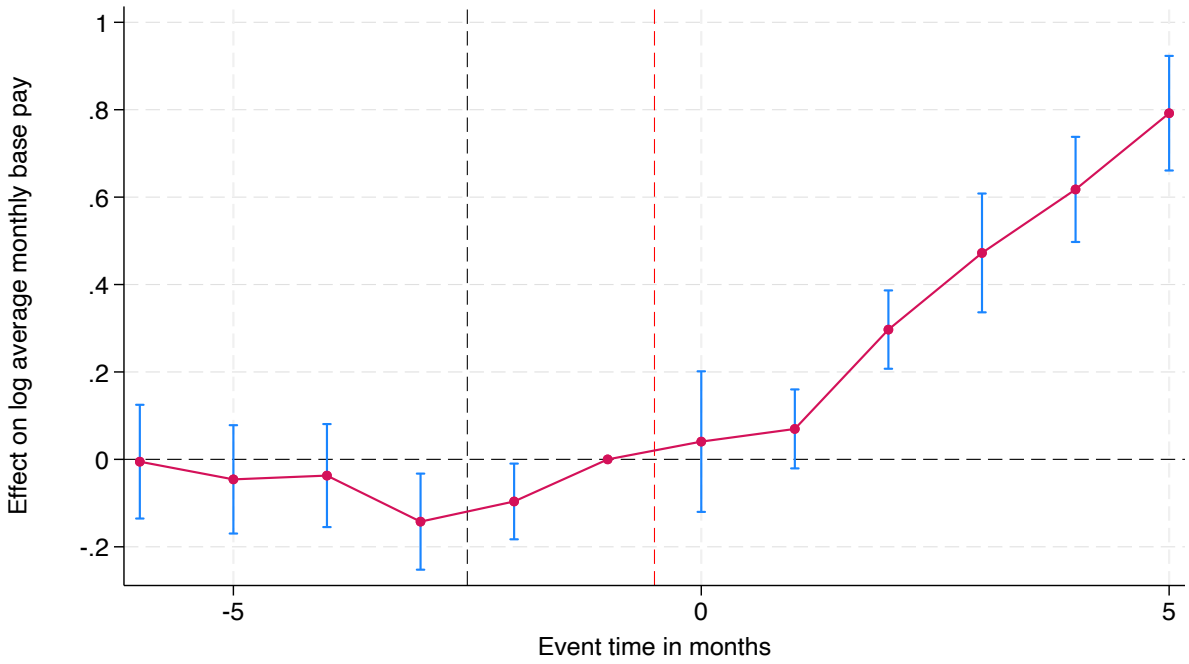
Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between employees' wage bins and the company minimum wage among large retailers with a voluntary minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average hourly wage of employees in that CZ. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

Figure C4: Effect of large retailer major VMWs on own wages



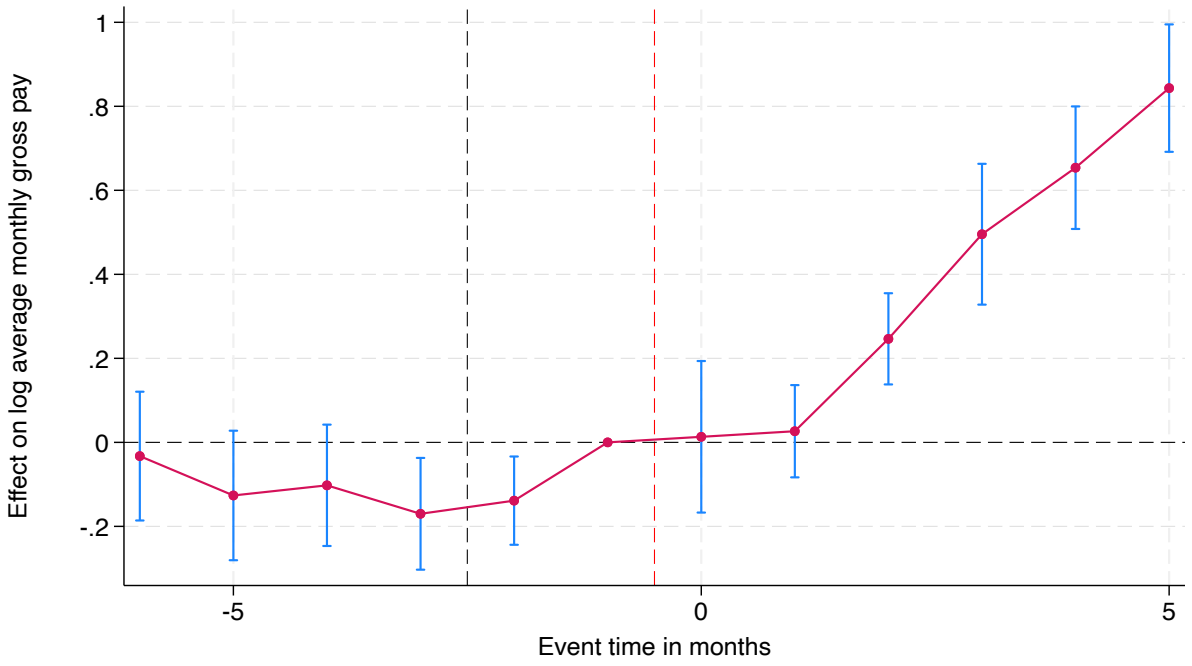
Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between employees' wage bins and the company minimum wage among large retailers with a voluntary major minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects. We define major VMW policies as those affecting more than 30% of the company's workforce. The dependent variable is log average hourly wage of employees in that CZ. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

Figure C5: Effect of large retailer $15 MW on own monthly pay



Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between binned wages of employees at large retailers with a voluntary $15 minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average monthly base pay of employees in that CZ. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

Figure C6: Effect of large retailer $15 MW on own monthly gross pay



Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between binned wages of employees at large retailers with a voluntary $15 minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average total monthly compensation of employees in that CZ. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

Table C1: Large retailer VMW effects on own wages, base pay, and gross pay

	All events	Major events	$15 events
Log avg. wage	0.707***	0.791***	0.823***
	(0.010)	(0.016)	(0.019)
Log avg. monthly base pay	0.596***	0.770***	0.784***
	(0.030)	(0.052)	(0.060)
Log avg. base pay (YTD)	0.354***	0.441***	0.435***
	(0.029)	(0.049)	(0.054)
Log avg. gross pay (YTD)	0.397***	0.486***	0.475***
	(0.034)	(0.056)	(0.065)
Obs	71,846	33,566	10,970
CZs	632	631	380
Events	20	8	4
Average gap	.05	.06	.11
Pre-period mean:			
Log wage	2.6	2.6	2.7
Log base pay	7.4	7.3	7.5
Log base pay (YTD)	7.4	7.3	7.4
Log gross pay (YTD)	7.5	7.4	7.5
Month from event FEs	Y	Y	Y
CZ FEs	Y	Y	Y

Notes: This table reports the estimated coefficients on the gap measure interacted with a post-period indicator from regression equation 3 on a stacked policy company dataset of voluntary minimum wage events. Each row of the table represents a separate regression with the following sequence of dependent variables: log average hourly wage, log average monthly base pay (for workers with base pay reported in the previous month), log average monthlyy year-to-date base pay, and log average monthly year-to-date gross pay. Column 1 reports the effects for all events, column 2 for major events (those affecting at least 30% of the workforce), and column 3 for $15 dollar events. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. Significance levels are as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources*: Large credit bureau.

Table C2: Large retailer VMW effects on own wages and employment, including workers paid $30 or more

	All events	Major events	$15 events
Log avg. wage	0.730***	0.834***	0.878***
	(0.009)	(0.015)	(0.018)
Log employment	0.255***	0.324**	0.403**
	(0.086)	(0.140)	(0.164)
Separation rate	-0.083***	-0.086***	-0.090***
	(0.009)	(0.013)	(0.015)
Separation rate to other firms	-0.014***	-0.015***	-0.017***
	(0.003)	(0.004)	(0.004)
Year-on-year Δ log new hires	-0.861***	-0.769***	-1.091***
	(0.203)	(0.286)	(0.339)
Total employment elasticity	0.350***	0.388**	0.459**
	(0.120)	(0.172)	(0.192)
	[0.113,0.586]	[0.051,0.726]	[0.081,0.837]
Obs	62,843	30,904	9,423
CZs	632	631	380
Events	20	8	4
Average gap	.05	.06	.11
Pre-period mean:			
Sep rate	.063	.065	.071
Sep rate to other firms	.012	.014	.014
YOY Δ log new hires	-.01	-.08	-.18
Month from event FEs	Y	Y	Y
CZ FEs	Y	Y	Y

Notes: This table reports the estimated coefficients on the gap measure interacted with a post-period indicator from regression equation 3 on a stacked policy company dataset of voluntary minimum wage events. Each row of the table represents a separate regression with the following sequence of dependent variables: log average hourly wage, log employment of all hourly workers, the separation rate of all hourly workers, the rate of separations to other firms in the database, the year-on-year change in log new hires of all hourly workers, and log employment on log wages, where log wages are instrumented for using the gap measure interacted with a post-period indicator. Column 1 reports the effects for all events, column 2 for major events (those affecting at least 30% of the workforce), and column 3 for $15 dollar events. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 employees every month during the event window. Significance levels are as follows: * p < 0.1, ** p < 0.05, and *** p < 0.01. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources*: Large credit bureau.

D Descriptives on large retailer labor markets and robustness checks for spillover results

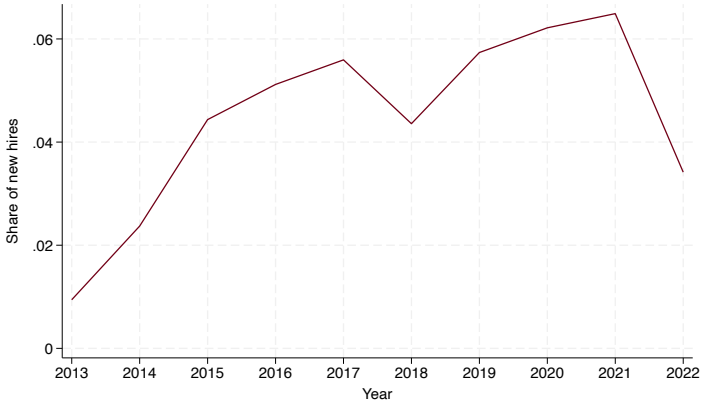
D.1 Industries connected to policy employers by worker flows

Table D1: Industries connected to policy employers by worker flows

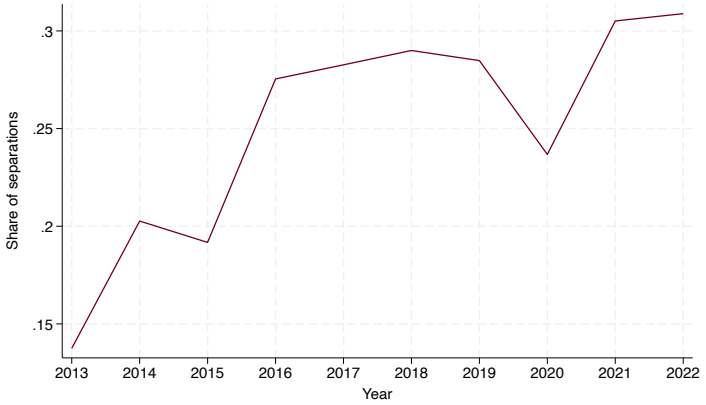
Industry (3-digit NAICS)	% of separations	% of new hires
Administrative and Support Services	18	18
Food Services and Drinking Places	12	14
Food and Beverage Stores	10	8
General Merchandise Stores	8	16
Clothing and Clothing Accessories Stores	8	8
Couriers and Messengers	6	6
Building Material and Garden Equipment and Supplies Dealers	6	4
Hospitals	2	<2
Professional, Scientific, and Technical Services	2	<2
Sporting Goods, Hobby, Book, and Music Stores	2	2
Motor Vehicle and Parts Dealers	2	<2
Educational Services	2	2
Miscellaneous Store Retailers	2	<2
Food Manufacturing	2	<2
Nonstore Retailers	<2	4

Notes: Industry unknown for 6% of separations and 2% of new hires. Data sources: Large credit bureau.

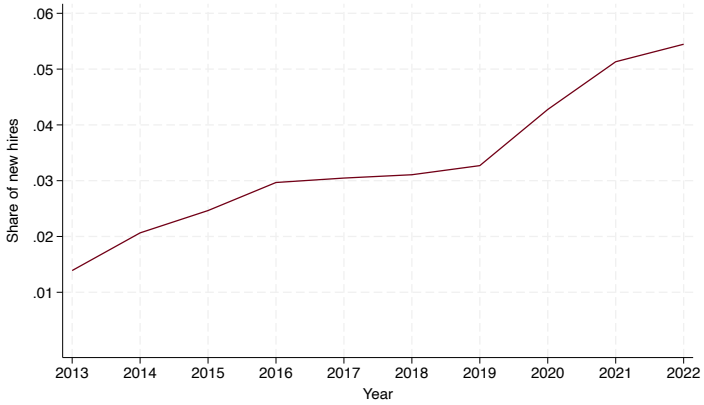
Figure D1: Share of hiring and separations from and to other employers in the database



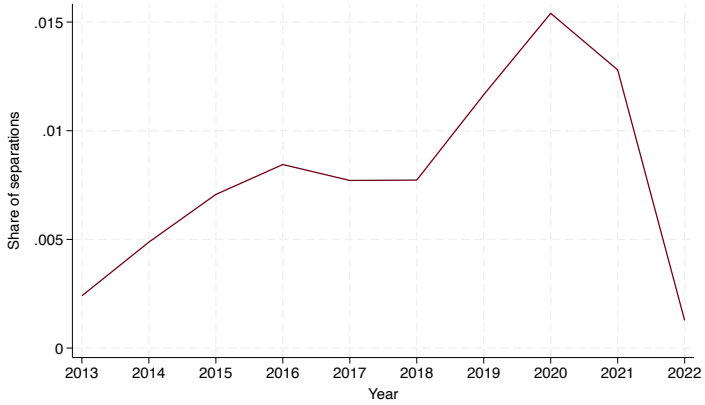
(a) Fraction of policy new hires from non-policy



(b) Fraction of policy separations that flow to non-policy



(c) Fraction of non-policy new hires from policy



(d) Fraction of non-policy separations that flow to policy

Notes: This figure plots the fraction of all new hires at policy companies that come from non-policy companies in the database (panel (a)); the fraction of all employees that separate from a policy company that flow to another company in the database (panel (b)); the fraction of all non-policy new hires that come from policy companies (panel (c)); and the fraction of all employees that separate from a non-policy company that flow to a policy company in the database (panel (d)). *Data sources*: Large credit bureau.

Table D2: Past flows between non-policy and policy establishments predict future flows

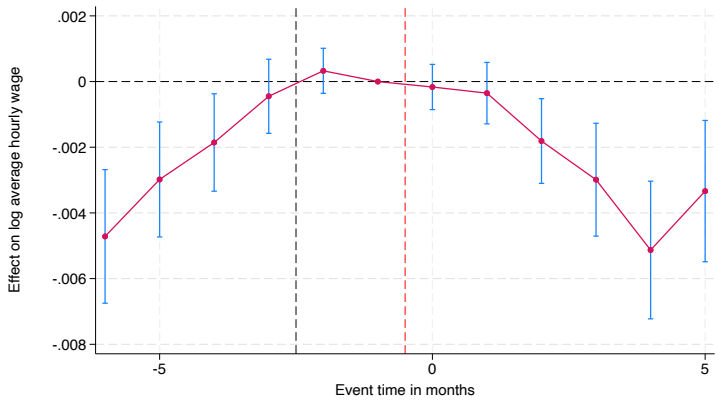
Experiment	Increased probability of post-treatment poach		Increased probability of post-treatment feed	
	When poach in prior year	When poach in all prev. years	When feed in prior year	When feed in all prev. years
1	.65 (0.002)	.59 (0.002)	.78 (0.008)	.76 (0.007)
2	.64 (0.002)	.55 (0.002)	.71 (0.007)	.63 (0.007)
3	.66 (0.002)	.48 (0.002)	.76 (0.007)	.52 (0.005)
5	.46 (0.002)	.19 (0.001)	.15 (0.006)	4.0e-02 (0.002)
6	.69 (0.008)	.58 (0.006)	.57 (0.023)	.52 (0.019)
7	.63 (0.008)	.46 (0.005)	.58 (0.022)	.41 (0.014)
8	.59 (0.008)	.41 (0.004)	.57 (0.024)	.37 (0.013)
9	.57 (0.009)	.3 (0.004)	.35 (0.017)	.21 (0.009)
10	.51 (0.008)	.26 (0.003)	.36 (0.020)	.16 (0.008)
11	.7 (0.006)	.6 (0.004)	.63 (0.018)	.53 (0.014)
12	.6 (0.005)	.37 (0.003)	.51 (0.015)	.29 (0.008)
13	.56 (0.005)	.32 (0.003)	.39 (0.013)	.21 (0.006)
14	.43 (0.005)	.22 (0.002)	.16 (0.010)	6.7e-02 (0.004)
17	.72 (0.003)	.67 (0.003)	.7 (0.014)	.7 (0.011)
18	.72 (0.003)	.62 (0.003)	.69 (0.011)	.6 (0.010)
19	.72 (0.003)	.55 (0.002)	.76 (0.010)	.55 (0.008)
20	.71 (0.003)	.53 (0.002)	.75 (0.009)	.52 (0.008)
21	.69 (0.003)	.47 (0.002)	.7 (0.010)	.46 (0.007)
22	.65 (0.003)	.4 (0.002)	.63 (0.009)	.38 (0.006)
23	.72 (0.003)	.6 (0.003)	.81 (0.008)	.64 (0.007)

Notes: Using establishment-level data, Columns 1 and 2 report the coefficient estimate when regressing an indicator for employee flows from the policy firm to the establishment prior to the minimum wage event on an indicator for employee flows from the policy firm to the establishment following the minimum wage event. Columns 3 and 4 report the coefficient estimate when regressing an indicator for employee flows from the establishment to the policy firm prior to the minimum wage event on an indicator for employee flows from the establishment to the policy firm following the minimum wage event. Columns 1 and 3 limit the pre-period to one year prior to the minimum wage event. Columns 2 and 4 expand the pre-period to include all available data prior to the event. Robust standard errors are in parentheses. *Data source:* Large credit bureau.

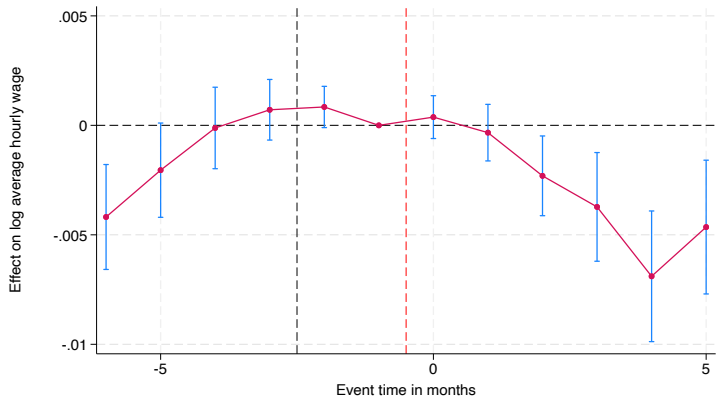
D.2 Spillover effects, zoomed in y-axis

This appendix presents spillover results with a zoomed-in y-axis.

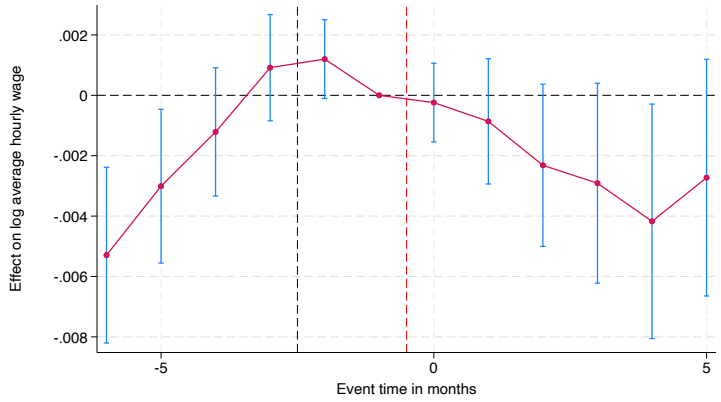
Figure D2: Effect of large retailer $15 MW on other firms' wages, zoomed in y-axis



(a) All establishments



(b) Establishments in industries connected by worker flows



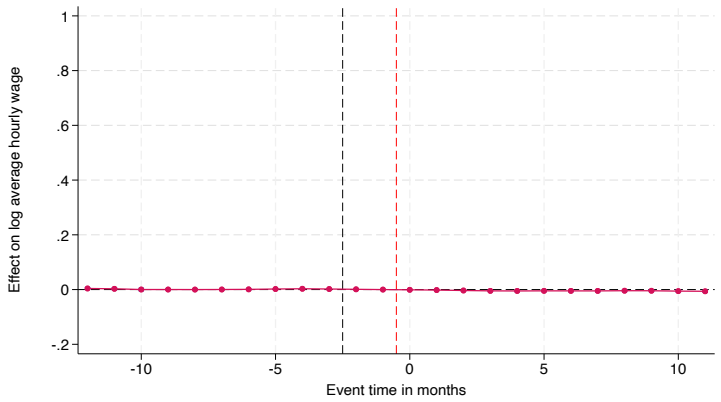
(c) All establishments connected by worker flows

Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between binned wages of employees at large retailers with a voluntary $15 minimum wage against a zoomed-in y-axis (see equation 1 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average hourly wage of non-policy employees in that CZ. Panel (a) includes all non-policy establishments in the CZ; Panel (b) restricts the sample to non-policy establishments in industries connected to the large retailer by worker flows; and Panel (c) restricts the sample non-policy establishments with a history of hiring workers from or having workers separate to the large retailer in the year before the policy is adopted. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 policy employees and to non-policy establishments with at least 10 employees during every month of the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

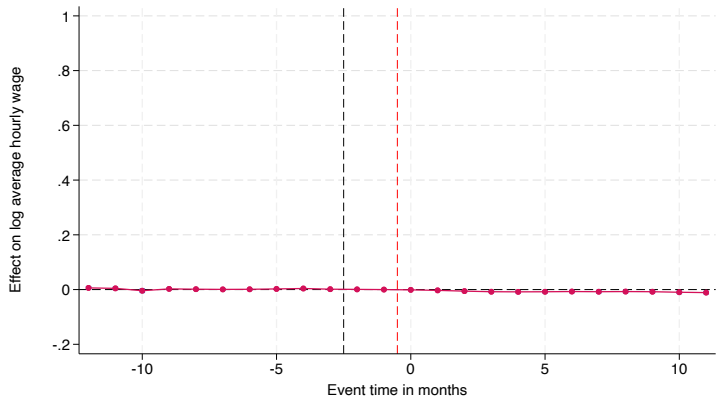
D.3 Spillover effects over longer time horizon

This appendix presents spillover results over a longer time horizon (12-month post period).

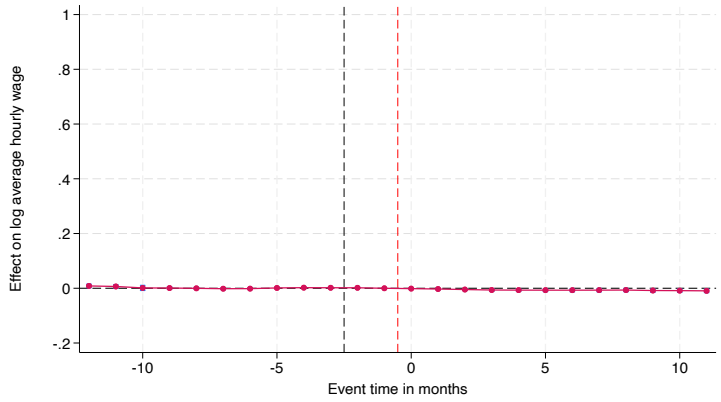
Figure D3: Effect of large retailer $15 MW on other firms' wages, 12-month post-period



(a) All establishments



(b) Establishments in industries connected by worker flows



(c) All establishments connected by worker flows

Notes: This figure plots the regression coefficients on the gap in each commuting zone (CZ) between binned wages of employees at large retailers with a voluntary $15 minimum wage over a 12-month pre- and post-period (see equation 1 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average hourly wage of non-policy employees in that CZ. Panel (a) includes all non-policy establishments in the CZ; Panel (b) restricts the sample to non-policy establishments in industries connected to the large retailer by worker flows; and Panel (c) restricts the sample non-policy establishments with a history of hiring workers from or having workers separate to the large retailer in the year before the policy is adopted. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 policy employees and to non-policy establishments with at least 10 employees during every month of the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

D.4 Spillover effects on new hire wages

This appendix presents spillover results on wages of new hires at non-policy firms.

Table D3: Spillover effects on new hire wages

	All events	Major events	$15 events	$15: pos. flows
Log avg. wage, non-policy	-0.017***	-0.027***	-0.019***	-0.019***
	(0.005)	(0.006)	(0.006)	(0.006)
Obs	4,747,427	2,059,227	825,565	379,680
CZs	633	630	380	377
Company X CZ FEs	Y	Y	Y	Y
Month from event FEs	Y	Y	Y	Y

Notes: This table reports the estimated coefficients on the gap measure interacted with a post-period indicator from regression equation 5 on a stacked dataset of voluntary minimum wage events. The dependent variable is the log average hourly wage of new hires at non-policy establishments. Column 1 reports the effects for all events, column 2 for major events (those affecting at least 30% of the workforce), column 3 for $15 VMW events, and column 4 for $15 events, restricting to non-policy establishments with a history of hiring from the policy company. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). Company-by-CZ, company, CZ, and month fixed effects are included. The sample is restricted to a balanced panel of commuting zones where the policy company has at least 30 employees and a balanced panel of non-policy establishments with a minimum of 10 employees each month. Columns 1-3 restrict to non-policy establishments in connected industries while column 4 restricts to connected non-policy establishments in any industry. Significance levels are as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors in parentheses. Errors are clustered at the CZ level. *Data sources*: Large credit bureau.

D.5 Spillover effects using gap between non-policy wage and large retailer VMW

In this Appendix, we use an alternative approach to estimating spillover effects from large retailer voluntary minimum wage events. Our baseline analysis examines the association between the large retailer's gap measure and non-policy establishments' wages before and after the policy's implementation. Below we explore whether the relative position of non-policy establishments vis-à-vis the large retailer's voluntary minimum predicts wage increases for non-policy establishments.

Specifically, we measure the gap between the non-policy establishment and the large retailer VMW in the pre-period exactly as we did to measure the gap between each policy company's establishments and their incoming voluntary minimum (see Equation 1). We then estimate a stacked event study around large retailer voluntary minimum wage events using the following equation:

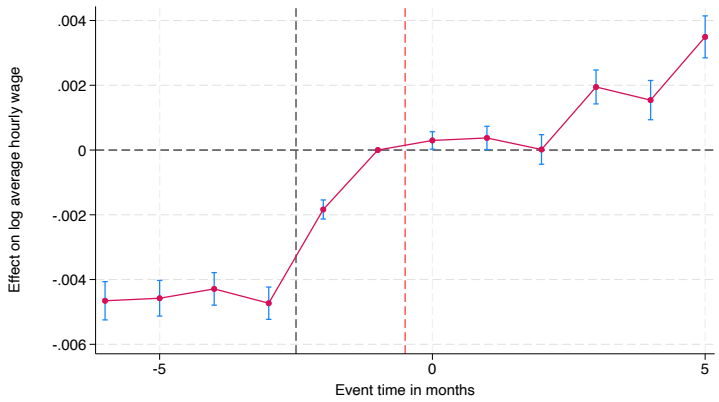
$$\log w_{f,c,t} = \iota + \sum_{k=-6}^{5} \phi_k \overline{GAP}_{f,CZ} \times \mathbb{1}_{[t=k]} + \xi_c + \zeta_t + e_{c,t}, \tag{6}$$

where $\log w_{f,c,t}$ is the log average hourly wage at the establishment level, $\overline{GAP}_{f,c}$ is the increase in the employment-weighted hourly wage for non-policy establishments required to bring workers up to the policy company's voluntary minimum, $\xi_{f,CZ}$ are establishment fixed effects, ζ_t are month fixed effects, and standard errors $e_{c,t}$ are clustered at the CZ level.

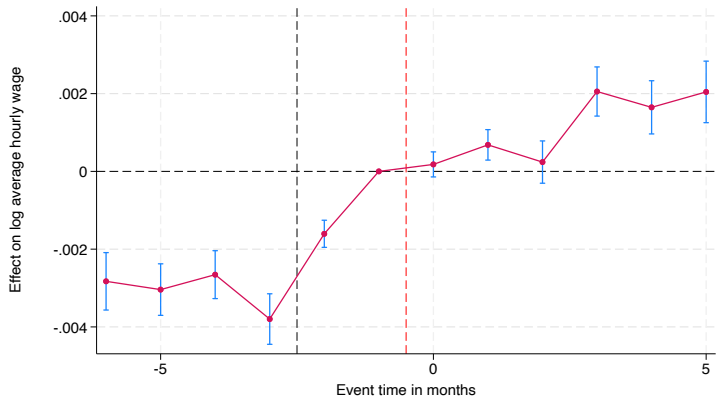
The coefficient of interest is ϕ_k which plots the relationship between the non-policy establishment's gap and log average establishment wages k months from the policy company's voluntary minimum wage event. Once again, we measure the gap over months -6 to -3 in order to build in a placebo-in-time test for mean reversion.

Appendix Figure D4 shows a sharp increase in the wage after the gap measurement period ends as opposed to the beginning of the treatment period, consistent with mean reversion. To account for mean reversion, we detrend the event studies by fitting a linear trend through the pre-policy months -4 to -1, generate predicted values for the post-period, and subtract these predicted values from the estimates of ϕ_k from 6. Appendix Figures D5 and D6 shows these results, where the latter figure's y-axis is adjusted such that the scale matches that of Figure 3. Once again, we find negligible spillovers using this approach.

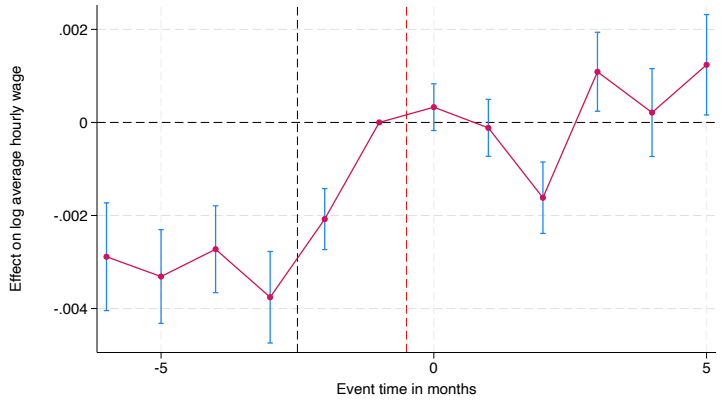
Figure D4: Effect of large retailer $15 VMW on other firms' wages, own gap



(a) All establishments



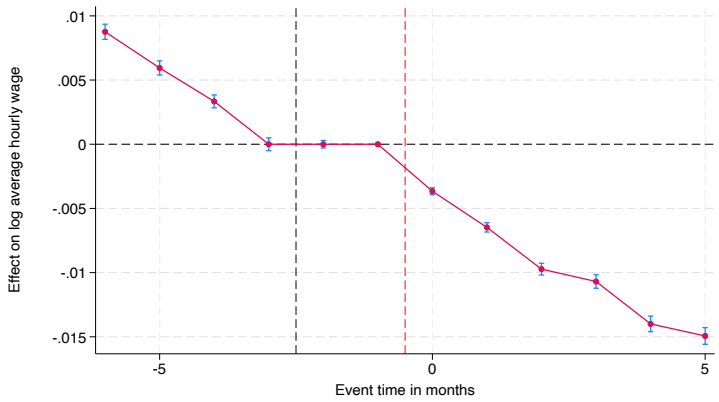
(b) Establishments in industries connected by worker flows



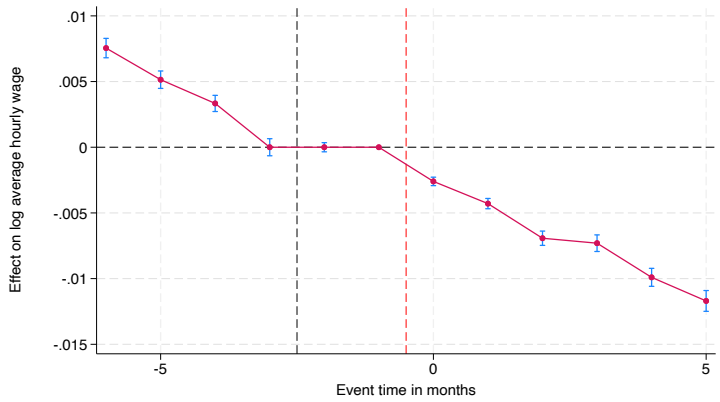
(c) All establishments connected by worker flows

Notes: This figure plots the regression coefficients on the gap interacted with month fixed effects, but where the gap represents the gap for non-policy establishments as opposed to the policy establishments. Thus, the gap represents the percent difference between the large retailer's voluntary minimum (in this case $15 events) and the average hourly wage at the CZ-level for non-policy companies. The outcome is the log average hourly wage at the company-by-CZ level for non-policy firms. Panel (a) includes all non-policy establishments in the CZ; Panel (b) restricts the sample to non-policy establishments in industries connected to the large retailer by worker flows; and Panel (c) restricts the sample non-policy establishments with a history of hiring workers from or having workers separate to the large retailer in the year before the policy is adopted. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). Company-by-CZ, company, CZ, and month fixed effects are included. The sample is restricted to non-policy establishments with at least 10 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

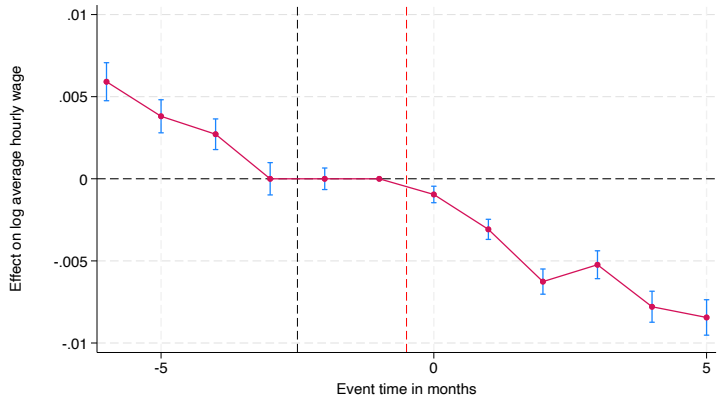
Figure D5: Effect of large retailer $15 VMW on other firms' wages, own gap, detrended (zoomed in y-axis)



(a) All establishments



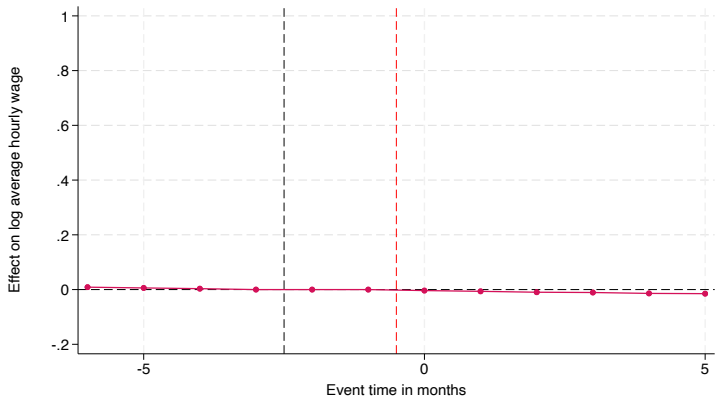
(b) Establishments in industries connected by worker flows



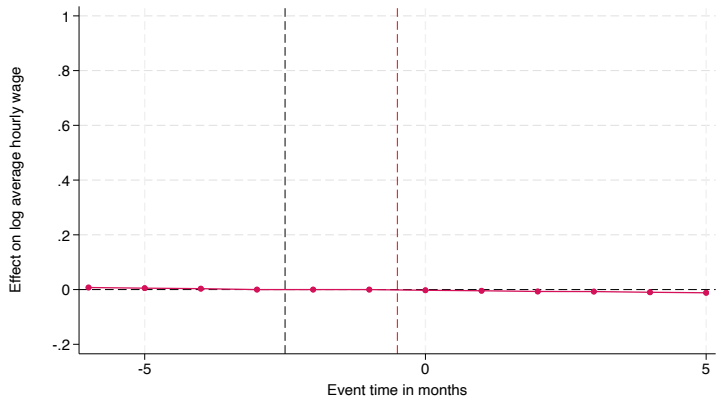
(c) All establishments connected by worker flows

Notes: This figure plots detrended regression coefficients on the gap interacted with month fixed effects, but where the gap represents the gap for non-policy establishments as opposed to the policy establishments. Thus, the gap represents the percent difference between the large retailer's voluntary minimum (in this case $15 events) and the average hourly wage at the CZ-level for non-policy companies. Coefficients are detrended by estimating the relationship between the coefficients on gap interacted with month from the pre-period and a linear event time variable, fitting values for these coefficients for the post-period, and subtracting these fitted values from the original coefficients estimated each month of the event window. The outcome is the log average hourly wage at the company-by-CZ level for non-policy firms. Panel (a) includes all non-policy establishments in the CZ; Panel (b) restricts the sample to non-policy establishments in industries connected to the large retailer by worker flows; and Panel (c) restricts the sample non-policy establishments with a history of hiring workers from or having workers separate to the large retailer in the year before the policy is adopted. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). Company-by-CZ, company, CZ, and month fixed effects are included. The sample is restricted to non-policy establishments with at least 10 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

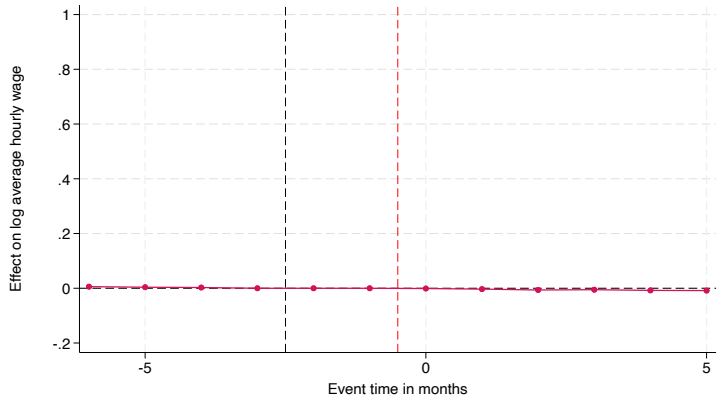
Figure D6: Effect of large retailer $15 VMW on other firms' wages, own gap, detrended



(a) All establishments



(b) Establishments in industries connected by worker flows



(c) All establishments connected by worker flows

Notes: This figure plots detrended regression coefficients on the gap interacted with month fixed effects, but where the gap represents the gap for non-policy establishments as opposed to the policy establishments. Thus, the gap represents the percent difference between the large retailer's voluntary minimum (in this case $15 events) and the average hourly wage at the CZ-level for non-policy companies. Coefficients are detrended by estimating the relationship between the coefficients on gap interacted with month from the pre-period and a linear event time variable, fitting values for these coefficients for the post-period, and subtracting these fitted values from the original coefficients estimated each month of the event window. The outcome is the log average hourly wage at the company-by-CZ level for non-policy firms. Panel (a) includes all non-policy establishments in the CZ; Panel (b) restricts the sample to non-policy establishments in industries connected to the large retailer by worker flows; and Panel (c) restricts the sample non-policy establishments with a history of hiring workers from or having workers separate to the large retailer in the year before the policy is adopted. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). Company-by-CZ, company, CZ, and month fixed effects are included. The y-axis is scaled to the same range as the figure showing own-wage effects (Figure 3). The sample is restricted to non-policy establishments with at least 10 employees every month during the event window. 95% confidence intervals shown. *Data sources*: Large credit bureau.

E Coverage of overall employment by credit bureau database

Table E1: Coverage of total employment by non-policy firms in credit bureau database

Experiment	Tot Emp Coverage	Retail Emp Coverage
1	0.12	0.25
2	0.13	0.26
3	0.14	0.27
4	0.14	0.27
5	0.12	0.25
6	0.13	0.27
7	0.14	0.28
8	0.14	0.28
9	0.15	0.29
10	0.14	0.28
11	0.14	0.26
12	0.15	0.29
13	0.14	0.28
14	0.13	0.26
17	0.12	0.26
18	0.13	0.26
19	0.14	0.27
20	0.14	0.27
21	0.15	0.28
22	0.15	0.29
23	0.15	0.29

Notes: Share of private employment as reported in the Quarterly Census of Employment and Wages (QCEW) in sample CZs that is covered by large credit bureau by experiment. Sample includes only CZ-month observations with valid QCEW data for both total employment and retail employment. Numerator limited to hourly wage workers at in-sample establishments in large credit bureau data. Column two is limited to employment in the retail sector (NAICS 44-45). *Data source:* Quarterly Census of Employment and Wages; Large credit bureau.