Voluntary minimum wages: The local labor market effects of national retailer policies*

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Abstract

Low unionization rates, a falling real federal minimum wage, and outsourcing have hampered wage growth in the low-wage sector in the US for several decades. In recent years (2014-2023), a number of large private retailers – including some of the largest employers in the U.S. – have opted to institute or raise company-wide, voluntary minimum wages (VMWs) for their employees. We use anonymized payroll data from a large credit bureau and a major payroll provider to study the effects of these national retailer policies on adopting employers' own wages and employment as well as their spillovers to other employers in shared local 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, we estimate small, often economically negligible, spillover effects across multiple measures of exposure to VMWs and numerous definitions of relevant competitors, including firms connected by worker flows. Together, the evidence points to little role for strategic interactions in the transmission of large retailers' wage policies to other firms. Voluntary minimum wage policies have affected over 3 million jobs at adopting employers, yet their impact on the broader labor market is limited.

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1 Introduction

From the 1980s to the present, a number of institutional factors placed downward pressure on wages in many sectors of the economy: deunionization, failures to raise the federal minimum wage, and the breakdown of traditional employment relationships through phenomena like outsourcing (Western and Rosenfeld, 2011; Farber et al., 2021; Autor et al., 2016; 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, engendering a variety of policy responses—most notably, many state and local governments increasing their statutory minimum wages above the federal level.[1]

Another response that emerged during this period, however, is the adoption of voluntary minimum wages by some of the largest employers in the United States. Voluntary minimums entail public statements by corporations that entry-level positions for their employees would be raised to a new minimum wage, usually set well above the applicable state or local minimum and introduced by the companies across all their geographic locations in the U.S. Despite their adoption by prominent companies like Walmart, Amazon, Target, and Starbucks, and despite their garnering substantial media attention, voluntary minimum wages (VMWs) and their broader labor market effects have not been the object of systematic study by economists.

In this paper, we study the impacts of voluntary minimum wages adopted by major employers (particularly in the retail industry) on wages and employment in low wage U.S. labor markets. To do so, we use multiple sources of administrative payroll data covering large and small firms. Our primary data source contains information on the wages, employment, and employment flows of thousands of medium-to-large-sized firms, including several national retailers that have adopted or raised voluntary minimums in the last 10 years. The anonymized data, which we obtained from a large credit bureau that offers employment and income verification services to the underlying client firms, cover over 18 million hourly workers (or 24% of the U.S. total). Importantly, the data also cover thousands of potential competitors of these focal retailers, based on location, industry, and a history of hiring from or having workers separate to the VMW firms, something we are able to observe in our data. Further, to assess the effects of VMWs on smaller firms, we use data from a major payroll provider covering millions of employees at small firms (employing 50 or fewer workers). We also estimate overall market effects of these policies using the Quarterly Census of Employment and Wages.

Studying the wage and employment impacts of voluntary minimum wages at large

[1]The impact of these subnational minimums on inequality, wages, and employment has been well studied in recent literature (Dube, 2019a,b; Cengiz et al., 2019; Dube and Lindner, 2021; Autor et al., 2023).

retailers can provide insight into how local labor markets respond to national company policies. Since VMWs are established as uniform increases adopted at a point in time by companies operating across hundreds of labor markets, they will have varying effects in different areas depending on the local salience, size, and wage distribution of the adopting employer. We combine this geographic variation in the policy's bite with various measures of the large retailers' local labor markets to estimate the effects of VMWs on adopting firms' own outcomes as well as their spillover effects to other employers. The national nature of VMWs generates plausibly exogenous variation in wage changes from the perspective of the companies' employees at a local level. For other firms in the market, VMWs represent a sudden shift in the wages of a single competitor, making this an ideal context for isolating cross-firm wage spillover effects.

To estimate the effects of the policy on adopting employers' own wages and employment, we use a gap measure borrowed from the literature on national minimum wage changes.[2] For each commuting zone ("CZ"), the gap measure 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 then look at how wages at other companies respond to changes in the large VMW adopters' wages and employment using two measures of exposure to the policy. The first is the large retailer's "own" gap, as defined above. The second is the market-level gap: the implied increase in the market-level average wage due to the large retailer shifting their local wage structure to comply with the company voluntary minimum. In areas where the large employer makes up a large share of the local wage bill, the market-level gap will be larger than in places where the large retailer makes up a small share of the local wage bill. We then assess the effects of large retailer VMWs on competitor wages, using commuting zone and worker flows across industries and even individual establishments to define relevant local competitors.

The set of voluntary minimum wage policies we study are 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,[3] 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

[2]See, for example, studies of national minimum wages in the U.S., the U.K., and Germany (Card et al., 1994; Draca et al., 2011; Dustmann et al., 2022).

[3]We define retailers as large if they attained a monthly employment of at least 150,000 workers for one or more months between 2013 and 2023.

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 ("major VMWs"); and all policies affecting 10% of the workforce.[4]

First, we study the effects of VMWs on own wages and employment using stacked event studies of the three types of policies: all VMWs, major VMWs, and $15 VMWs. Across all VMW events, we find that a 1 standard deviation increase in a company's own gap measure (equivalent to a 5% increase in the company's local wage rate) is associated with 3.55% increase in average hourly wages in the six months after adoption of the policy. Among major VMWs and $15 VMWs, a 1 standard deviation increase in the gap (6% and 8%, respectively) leads to wage increases of 4.83% and 7.01%, respectively. 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, we find that the main effect of VMWs on adopting employers was through a reduction in separations rather than an increase in new hiring. Initial turnover among these large employers was large, with average pre-period monthly separation rates across events of 6.4, implying annual turnover of greater than 75%. In response to VMWs, monthly rates of separations to other companies in the database fell by 0.10 percentage points on base separation rates ranging from 1.21-1.45% (thus representing a 7.14% decline in separations at the upper range of our estimates). By contrast, we find no evidence of increases in hiring after the policy in areas with a larger vs. smaller gap. Combined with our wage results, our separation results imply quit elasticities of approximately -1.02, consistent with existing literature. These sizable reductions in separations after VMW adoption resonate with the stated motivations for these policies cited in the media and in company press releases—that a major goal of these policies was to improve worker retention.[5]

How did these changes affect hiring and wages at other employers? Given reductions in separations at VMW adopting employers, we first examine the effect of VMWs on hiring at connected establishments, defined as those with a history of worker flows with the large retailer. We find that at connected establishments, the probability of hiring from VMW employers in any given month falls after adoption of the policy. However, we find no impact on overall hiring levels at these employers. We also 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, yet overall hiring remains unaffected, even in markets where VMW employers make up a relatively larger share of local employment.

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

[5] See, e.g., Lalley (2024) and Wartzman (2022).

This suggests that the composition of worker hiring from VMW employers – but not hiring overall – is the margin of adjustment for connected employers.

Across multiple definitions of the relevant labor market and multiple measures of exposure, we estimate precise zero cross-employer wage rate elasticities up to 12 months following large retailer VMW events for other companies in the credit bureau database, again including those connected to the large retailer by worker flows. Focusing on markets where the large retailer's employment share is relatively large, we continue to find zero wage 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.

We explore the generalizability of small wage spillovers by analyzing both effects on smaller firms and on market wages overall. To examine effects on small companies, we use data from a payroll provider that services hundreds of thousands of clients with predominantly 50 or fewer employees, with a total employment coverage in the millions. We estimate extremely small, often statistically insignificant effects on wages in this sample. Across both the large retailer gap and the market gap measures of exposure to large retailer VMWs, a one-standard deviation increase in exposure has almost negligible effects on wages, ranging -.08 to .2 log points. The implied cross-wage elasticities from the (positive) wage effects we estimate are also small, ranging from 0.01 to .043. In other words, we fail to find evidence of major spillovers when examining smaller firms in the labor markets of VMW employers. Finally, to see whether responses to large retailer VMWs are detectable market-wide, we use data from the QCEW and estimate the change in log average earnings at the commuting zone level, including and excluding earnings of the VMW employer. We find precise zero effects on market-level average earnings and on average earnings net of VMW employer earnings.

What are the implications of our results for models of imperfect competition in local labor markets? New benchmark models combining firm monopsony power with local labor market concentration predict wage spillovers through strategic interactions between firms (Berger et al., 2022; Bassier, 2022). The richness of our data allow us to calculate relevant parameters for these models—the employment and payroll shares of large retailers and their large competitors (the relevant set of firms for strategic interactions) across a wide variety of definitions of VMW employers' local labor markets. We use this information combined with our cross-wage elasticity estimates to assess the performance of models of strategic interactions in our setting. Existing quantitative calibrations of leading models of imperfect competition are unable to rationalize our estimates. Model-generated strategic responses yield far larger spillovers than what we observe empirically. Our results are consistent with recent work using more structural tests of imperfect competition models, which have also failed to reject the null of no strategic interactions

(Roussille and Scuderi, 2023). For low-wage retail in the U.S., monopsonistic rather than oligopsonistic competition may be a sufficient characterization of wage setting.

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. Nevertheless, we find that at a local level, 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, samples of firms, and measures of exposure to the shock for other employers. The widespread adoption of VMWs by national and other multi-establishment companies does suggest that perhaps there are propagation mechanisms operating at a national level, which we believe are an important subject for future research. These could include firm reputation considerations or complementary linkages between wage setting and other firm-wide policies, such as pricing or inventory management.

Estimates of cross-employer wage spillovers from company wage policy changes in the U.S. are limited.[6] 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. Compared to the labor market for registered nurses, low-wage retail is significantly less concentrated, with workers flowing across several sectors and occupations. Beyond that, however, we show that the low-wage retail market appears to operate differently, with wage setting among large firms seemingly uninfluenced by levels of local labor market concentration.

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 with these findings, we show that voluntary minimum wages reduced separations at large retailers, with similar quit 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

[6]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.

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, 2023a).[7] 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; Fehr et al., 2009). We contribute to this literature by studying a prevalent form of employer wage policy, particularly among national 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)).

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 voluntary minimums by individual companies and government-mandated minimum wage floors that significantly alter firm hiring and wage setting practices throughout the market when introduced (Rao and Risch, 2024).[8] Finally, our results also differ with the limited empirical evidence on spillover effects from collective bargaining agreements, but a key contrast is that the voluntary minimum wages occurred in a largely non-union sector and were not collectively bargained.[9]

[7] See Card (2022) for a comprehensive review.

[8] Our results also contrast with studies on the impact of local "living wage ordinances" that cities, counties and local public agencies (e.g., San Francisco Airport) introduced beginning in the 1990s and early 2000s. Living wage ordinances seek to ensure that organizations receiving public funds provide wages that provide a sustainable income for working families, usually by setting minimums significantly above federal and state statutes. Numerous studies found those ordinances to increase local wage levels while modestly reducing employment and poverty rates (Adams and Neumark, 2004, 2005; Neumark et al., 2012). In some cases, covered workers non-wage benefits, such as training and health benefits, also improved (Brenner, 2005; REICH et al., 2005).The relatively large wage impacts found in these studies are surprising given the relatively small number of workers actually covered by the ordinances, raising questions about the event study methods used to estimate impacts (Holzer, 2008). The approach used in our study could offer a more precise way to measure the potential effects of programs like living wage ordinances or federal contractor minimum and prevailing wage provisions (Kessler and Katz, 2001) on directly covered firms as well as spillovers to other firms or labor markets.

[9] An older literature found evidence of significant spillovers from union to non-union wages primarily

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 report our findings on large retailers' own wages and employment. In Section 5, we report our main spillover estimates, first on hiring and then on wages across a broad set of firms. Section 6 discusses the implications of 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.

In response to stalled federal action on the minimum wage and the challenges of organizing workers under current labor law, worker organizations and advocacy groups, led by the Service Employees International Union (SEIU) launched the "Fight for $15" campaign in 2012 to advocate for higher wages and union representation. 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. 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 (Rolf, 2015; Lathrop, 2018). The pandemic brought further attention to conditions facing frontline workers in many of these same industries, and states and localities continued to respond to political pressure to raise minimums in the absence of federal legislation.

In addition to advocating for state and local minimum wage adoption, 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) to raise their wages. In 2014, Walmart was one of several large national retailers to announce a company-wide voluntary minimum wage, starting with a minimum of $9 per hour across all operating locations and subsequently raising the floor 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.[10]

in the same industry (Slichter et al., 1960; Budd, 1992; Farber, 2005; Freeman and Medoff, 1985). In contrast, our study provides original evidence about the potential spillovers arising from voluntary minimum wages adopted by very large nonunion employers on wage determination of other firms in their labor markets. In particular, we find very limited impacts of voluntary minimum wages on wage setting at the local labor market level in contrast to the literature on union spillovers that suggest that non-union companies are more sensitive to wages at union workplaces.

[10] 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

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).[11] 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.

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 (Card, 2022). Adopting higher entry-level wages may represent a response to unionization attempts or adjustments induced from anticipated increases in statutory minimums at the state or local level. 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.

Changes in wage policy – particularly those introduced at a company-wide level – may be tied to larger strategic policies at the corporate level, such as strategies rooted in branding, product or service quality competition, or operational performance. Recent detailed accounts of corporate strategy at Walmart, for example, ascribe recent changes in wage policy (including VMW adoption) to efforts seeking to improve store operations

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.

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

and service quality and reviving inventory control practices that had eroded as a result of the high turnover associated with its prior low-wage policies (Wartzman, 2022). Similarly, Stone (2022) describes changes in Amazon's wage policies as arising from a recognition that sustaining productivity gains in distribution centers required upward adjustments to its wage policies through reductions in turnover. These explanations link VMW adoption to objectives going beyond their use as cheap talk (i.e., public announcement of already existing wage policies that already exceeded their announced voluntary minimums in most of the labor markets in which they operated).[12]

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.[13]

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.[14]

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.

[12]In Section 3, we show that these announcements were not "post-hoc," but instead entailed large and dramatic changes to companies' existing wage distributions.

[13]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`).

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

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 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.[15]

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

[15]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 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.

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.[16] 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.[17] 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").

3.2 Identifying large retailer voluntary minimum wage events

We begin with a list of voluntary minimum wage adoptions by large retailers (those with more than 150,000 workers) using data compiled by the National Employment Law Project. We validated and augmented this list of events using media reports and corporate 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%.[18]

[16] New hires are identified based on their original hire date at a given firm.

[17] Separating employees are identified via their termination date at a given firm.

[18] 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.

We focus on the 20 events with no other major company wage policy in the six months preceding or following it.[19] 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.[20]

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

[19] 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.

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

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. While these policies are chosen and adopted by the large retailers in our sample and thus cannot be considered exogenous shocks at a company level, the national nature of these policies means that they nevertheless induce sudden and arbitrary changes in company wages at a local level. It is this variation in the intensity of VMWs across space that we leverage in this section to quantify the effects on wages and to study the impacts on workers' separation rates and on hiring patterns.[21] We then use these results to inform our examination of spillovers from policy company VMWs to firms in 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; Emanuel and Harrington, 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%). By contrast, the fraction of workers below the minimum is 100% in all three cases.

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,

[21] A similar approach is used in prior studies of company-wide wage policy changes, including Staiger et al. (2010) and Emanuel and Harrington (2022).

$30 per hour.[22]

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 four 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.

In all analyses that follow, we standardize the gap measure so that all coefficients represent the effect of a single standard deviation increase in the gap. Figure 3 shows the distribution of the gap measure and reports its mean and standard deviation across the sample of all VMW events, major VMW events, and $15 events. For all events, the mean gap is 4.7% and one standard deviation is 4.9%. For major events, the respective numbers are 6.3% and 5.9%. Finally, for $15 VMWs, the mean gap is 11.3% and a single standard deviation is 8.1%.

We estimate effects on wages and other outcomes at the firm level for the different grouping of VMW events (all, major, and $15 VMWs) in separate, stacked event studies. 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 standardized continuous treatment variable, the gap measure in equation 1, on log average hourly wage using the following equation[23]:

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

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

In addition to our event-study analysis, we perform difference-in-differences analyses

[22] 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.

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

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:

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

where Y_{ct} is the outcome of interest, including log average hourly wage, log employment, the separation rate, and log new hires. Once again, $\tilde{\zeta}_c$ and $\tilde{\delta}_t$ are interacted with event-specific dummies, which are not shown here.

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 a one standard deviation increase in the gap. 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. We then examine 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 4 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 1 standard deviation increase in the CZ-level gap (equivalent to an 8.1% increase in the company's local average wage rate) increases average hourly wages by around 7 log points, or approximately a 7% increase in the average hourly wage.

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 full 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.[24]

As discussed in Section 4.1, our analysis includes a built-in placebo-in-time robustness check. We measure the gap over months −6 to −3. The vertical black dotted line on the left in Figure 4 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).[25] 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 5 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 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, or log total new hires of workers earning up to $30 per hour. 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,

[24]To investigate whether areas with zero gap also experience hourly wage increases, in Appendix 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 wage increase is 7% rather than 8.1%. Nevertheless, the magnitude is highly consistent with what we expect given the average gap across CZs for the $15 VMW events.

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

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 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.[26]

Magnitudes of employment and turnover effects Table 2 reports difference-in-differences 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 log new hires under $30.[27] A 1 standard deviation increase in the CZ-level gap leads to an increase in total employment of workers earning less than $30 rises by 1.58% after all events, by 2.63% after major events, and by 4.46% after $15 VMW 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.

With a 1 standard deviation increase in the gap, separation rates fall by 0.37 p.p.,

[26] 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.

[27] 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.

0.46 p.p., and 0.65 p.p. after all, major, and $15 VMW events, respectively. These effects translate to 5.81%, 7.08%, and 9.08% reductions relative to the pre-period mean. Focusing on separations to non-policy companies, separation rates fall by 0.1 p.p. each 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 4.18%, 4.94%, and 7.14% relative to the mean for all, major, and $15 VMW events, respectively.

The final row of estimates in Table 2 presents the total employment elasticity, which ranges from 0.45 to 0.64 across the three groupings of events. The impact on separations (relative to their pre-period mean) is larger, implying a quit elasticity of -1.02.[28]

Overall the effect of company voluntary minimums on employment appears driven by reductions in turnover. We find no evidence of increases in new hires after voluntary company minimums.[29] The fact that the main effect of voluntary minimum wages is to reduce turnover aligns with both company statements on as well as press coverage of voluntary minimum policies and their expected effects (see Section 2).

These results guide our approach to examining spillover effects of large retailer voluntary minimums on other employers in their shared labor market, which we explore in the next section.

5 Spillover effects of large retailer VMWs

Wage policy changes by individual large employers represent an ideal setting to test for cross-employer wage spillovers as the former represent truly exogenous shocks from the perspective of other firms in the labor market, and particularly so at a local level given differential intensity of national VMWs across locations.

To study these spillover effects, we adopt multiple measures of exposure to large

[28]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 percent effect on wages to obtain the quit elasticity.

[29]Under some models of dynamic monopsony and under certain assumptions, e.g., Manning (2003), recruit and separation elasticities with respect to the wage are equal and opposite, thus generating the insight that the labor supply elasticity to the firm can be obtained by multiplying the absolute value of the quit (or recruit) elasticity by two. However, such predictions are model dependent. Furthermore, in the empirical literature, it is rare to have settings where both quit and recruit elasticities can be cleanly estimated, with most studies only estimating or obtaining reliable estimates of one–usually, the quit elasticity–or the other (Emanuel and Harrington, 2022; Caldwell and Oehlsen, 2022; Dube et al., 2020; Hirsch et al., 2022a; Kambayashi et al., 2023). A couple of exceptions are Hirsch et al. (2022b),Falch (2017), and Datta (2023b). Even in settings where both can be estimated, the two elasticities can vary widely or be heterogeneous across groups.

retailer VMWs, and we analyze several definitions of large retailers' labor markets. In addition to examining broad, market-wide spillovers, we also hone in on segments of the labor market that are most likely affected by the VMW events we study, paying close attention, for example, to non-policy establishments with a history of hiring workers from or having workers separate to the VMW employer.

We begin our examination of spillover effects from large retailer voluntary minimums by studying the hiring and wage responses of other companies included in the credit bureau database. As these tend to be larger companies, we also explore spillovers to smaller firms using a second source of administrative data: a payroll provider whose client firms cover millions of employees at small businesses, typically at firms with under 50 employees. We also examine aggregate market-level responses using the Quarterly Census of Employment and Wages. These results are described in Section 5.6.

We term other companies in the credit bureau database "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).[30] Across all 20 events, there are 106,597 unique establishments belonging to 2,873 companies in the analysis sample.[31] Characteristics of the companies in the credit bureau database are described in Table 1.[32]

5.1 Defining the labor market of large retailers with VMWs

We use multiple definitions of large retailers' labor markets in our examination of spillover effects. Our broadest definition consists of all non-policy establishments in the same commuting zones as the large retailer adopting the VMW. 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 large retailers' presence at the commuting zone level as well as how we construct our increasingly specific definitions of large retailers' labor markets. Note that the relative sizes – as measured by employment and payroll shares – of VMW establishments and those of their competitors varies across

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

[31] 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.

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

these different definitions of large retailer labor markets. In Section 6, we discuss the implications of these shares and the spillovers we estimate in this section for models of imperfect competition in the labor market.

Connected industries We can zoom into a more relevant segment of the labor market by focusing on industries connected to the large retailer by worker flows. For every new hire at policy companies and for every employee separating from a policy company, we are able to identify their previous or subsequent industry, respectively, 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. While the types of non-policy companies in the creditbureau database are not a major source of new hires for large retailers, they do make up a substantial share of separated employees' future companies, and a similar share as the credit bureau companies' share in overall U.S. retail employment (see Appendix Table D1). This is important as separations are the main margin of employment change for large retailers in the wake of VMW adoption.

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 D3). 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.[33] Thus, our second most restrictive definition of the labor market consists of establishments in the 15 strongly connected industries as well as in the same CZ as the large retailers' employees.

It is important to note that non-policy employers in the same industry as the large retailer–while strongly connected to the large retailer by flows–tend to make up just a minority of new hires or separations overall. The large retailers we study include general

[33]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.

merchandise stores, food services and drinking places, and nonstore retailers. These industries make up approximately 20% of their separations and 34% of their new hires, respectively, thus justifying the inclusion of other connected industries in defining their labor markets. Nevertheless, in appendix results discussed in Section 5.5, we also explore whether being in the precise same industry as the larger retailer results in larger spillovers.

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 D4, 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. Figure 6 shows that average wages among firms (aggregating across individual establishments) with a history of poaching from or feeding to large retailers with $15 VMWs are much closer to the average wage of the large retailer around the event window than unconnected firms. Furthermore, as might be expected, firms that poach from the large retailer tend to have higher wages in the preperiod while firms that feed to the large retailer have lower wages. Remarkably, the policy closes the gap between the large retailer wages and poaching firms' wages in the case of a $15 VMW.[34] These national descriptive patterns provide further evidence in support of studying dynamics at connected firms and establishments.

5.2 Estimating spillovers effects on other employers

We explore the spillover effects of large retailer voluntary minimum wages on local competitors (non-policy establishments) using two types of exposure to the large retailer's policy. The first measure of exposure is the large retailer's own gap measure as defined in Section 4.2. If the large retailer is nationally salient as is the case in the set of voluntary minimum wage policies we study, it is plausible that competitors who share local labor markets with these retailers are informed about large retailers' wages locally. If so, the implied increase in large retailers' wages at the local level as a result of voluntary

[34] Appendix Figures D3 and D2 show average wages for policy firms and non-policy firms in the case of all VMW and major VMW events, respectively.

minimum wages set at a national level may be relevant and salient to local competitors as well.

A second type of variation in treatment intensity that we use is the implied change in the market wage generated by the large retailer VMW. We construct this market-level gap by taking the numerator of the gap measure for the large retailer and dividing by the employment-weighted hourly rate among all other establishments from the credit bureau database in the CZ. The rationale for this measure of treatment intensity is that the effects of company voluntary minimum wages on other employers may vary with the local importance of the large retailer. As shown above, while large retailers with policies are some of the largest employers in the US, they still only represent a small share of local employment. We thus use a modified gap design to quantify spillovers that captures not only the regional variation in wage increases at the large retailer, but also the local importance of the large retailer.

This market-level gap measure intuitively answers the question, "What is the implied percent increase in the wage bill in a commuting zone associated with bringing the large retailer's workers up to their company's voluntary minimum wage?" In a CZ where the large retailer is the only employer, the gap and market gap measures are equivalent. In a CZ where the large retailer represents 50% of employment and all workers in the CZ earn \$7.50 per hour, the gap measure is 1, but the market gap measure is 0.5. In a CZ where the large retailer represents 50% of employment and pays its workers \$7.50, but all other workers in the CZ earn \$15 an hour, then the gap measure is still 1, but the market gap measure is 0.33.

More formally, we define the market gap for each large retailer in month t in commuting zone CZ as:

$$\text{MARKET GAP}_{CZ,f,t} = \frac{\sum_{w \in W} \max\{0, N^{wt}_{CZ,f}(MW - w)\}}{\sum_{w \in W}(N^{wt}_{CZ,f} w + N^{wt}_{CZ,-f} w)}, \tag{4}$$

where w is the wage bin $[w, w+1)$, and $N^{wt}_{CZ,f}$ is the number of workers in that wage bin by CZ-month at firm f and $-f$ represents all other firms in the market.

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

$$\overline{\text{MARKET GAP}}_{CZ,f} = \sum_{t=-6}^{-3} \text{MARKET GAP}_{CZ,f,t},$$

which we then standardize to obtain our market gap treatment variable.

Distribution of the market gap measure across CZs Appendix Figure D4 shows the distribution of the market gap measure across CZs for all, major, and \$15 VMWs.

Perhaps unsurprisingly, the implied increase in the market wage across all groups of VMW events is small, particularly given their small share of local employment and the fact that large retailer wages are on par with wages at connected establishments and industries. The market gap is around .002 for both all and $15 VMWs and .003 for major VMWs. The standard deviation in the market gap has a similar range: .002-.003. That means that a 1 standard deviation increase in the market gap implies just a 0.2% or 0.3% increase in the market wage, or less than a 10th the increase in the large retailers' own wage after a VMW event.

Correlation between the market gap and alternative measures of large retailers' local importance The market gap is defined only among firms in the credit bureau database raising the question of how relevant it is for the broader retail and overall local labor market. Because the market gap incorporates both the wages and employment of the local retailer, it does not directly correspond to the large retailers' employment share. Nevertheless, we find that across most events, the market gap and large retailers' employment share are strongly positively correlated (see Appendix Table D2. Furthermore, the correlation between the market gap in the credit bureau database and large retailers' share of overall local employment as measured in the QCEW is 0.71.

Estimating equations For both kinds of exposure measures, 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:

$$\log w_{f,c,t} = a + \sum_{k=-6}^{5} \gamma_k G_c \times \mathbb{1}_{[t=k]} + \rho_{f,c} + \nu_{f,t} + \mu_t + u_c, \tag{5}$$

where $G_c \in \{\overline{\text{GAP}}_c, \overline{\text{MARKET GAP}}_c\}$, the large retailer's own gap measure as described in Section 4.2 or the market gap measure described above. 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. We include event-time fixed effects (μ_t) and firm-by-CZ fixed effects ($\rho_{f,c}$), as well as firm and CZ main effects (not shown above). All fixed effects are interacted with an event indicator, also not shown. Errors (u_c) 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:

$$\log w_{f,c,t} = \tilde{a} + \tilde{\gamma} G_c \times \text{Post}_t + \tilde{\rho}_{f,c} + \tilde{\nu}_{f,t} + \tilde{\mu}_t + \tilde{u}_c \tag{6}$$

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

5.3 Spillover effects of large retailer VMWS on hiring by competitor firms

Given the effect of large retailer VMWs on separation rates of their own workers, including separations to other firms in the credit bureau database, we first examine the effects of large retailer VMWs on hiring of other firms, particularly their hiring of former employees of large retailers with VMW policies.

Table 3 shows the effect of large retailer VMWs (stacking all events) on the composition of hiring at connected non-policy establishments. These are establishments 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 variables are indicators for making at least one hire from another company (overall, from the policy company, or from other non-policy companies) 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 column report the effect on the probability of hiring a worker from any other company in the credit bureau database in a given month. The second column reports the effect of large retailer VMWs on the probability of hiring from the large retailer in a given month. The final column reports the effect of large retailer VMWs on hiring from firms other than the large retailer.

Columns 1 and 3 both show no major change in overall hiring or hiring from firms other than the large retailer though both coefficients are negative. In particular there is no decline in overall hiring after large retailer VMWs. However, there is a clear decline in the probability of hiring from the large retailer, as indicated by the results reported in column 2. A 1 standard deviation increase in the large retailer's gap is associated with a -.48 ppt decline in hiring from the large retailer over a pre-period mean of 21% of new hires coming from the large retailer (or a 2.3% reduction in hiring from the large retailer relative to the mean hiring rate in the pre-period). These are small effects but clearly detectable among the connected set of non-policy establishments. The results suggest a slight shift in the composition of hiring by connected establishments, consistent with large retailers' facing reduced separations to non-policy firms following VMW events documented in Section 4.2.

In Table 4, we examine whether these effects are detectable at a market level (CZ), where the outcomes are log new hires by all non-policy establishments in the CZ, log new hires from large retailers by all non-policy establishments in the CZ, and log new hires from firms other than the large retailers. We estimate small positive effects on overall

new hires and hires from other non-policy firms and a marginally significant but much larger in magnitude reduction in log new hires of workers formerly employed at VMW firms.

Recalling the small local presence of large VMW retailers, we explore how the effect on non-policy hiring from the large retailers varies with their employment share in Figure 7. The figure reports the effect of large retailer VMWs by decile of the large retailer's share of employment in the overall CZ (where overall employment is measured using the QCEW). The effects in deciles 2-10 relative to decile 1 are reported for all VMW events in Panel (a), major events in Panel (b), and \$15 VMW events in Panel (c). The effect of large retailer VMWs is negative in every decile of the large retailer's CZ-level employment share before the policy, but effects are largest and more precisely estimated in deciles 7-10 of large retailer employment share. We summarize this heterogeneity in Table 5, which reports the effects of large retailer VMWs across different subsets of events separately by high and low employment share CZs of the large retailer (top 4 deciles vs. bottom 6 deciles, respectively). Reductions in the log number of new hires from large retailers falls the most in CZs where the large retailers make up a relatively larger share of local employment. In these CZs, non-policy hiring from VMW employers falls by 3-7 log points.

Finally, we examine spillovers on non-policy hiring using our alternative measure of exposure to large retailer VMWs: the implied change in the market wage from large retailer VMWS. Table 6 reports these results. We see patterns consistent with our earlier results. Across all events (column 1), a 0.3% increase in the market wage as a result of VMW policies leads non-policy hiring from VMW employers to fall by 1.8 log points while hiring overall and hiring from other sources increases by 1.4 and 1.8 log points, respectively. Columns 2 and 3 report similar effects for major events and \$15 VMW events. In the latter, reductions in hiring from the large retailer rise to 8.2 log points with no detectable impact on overall hiring or hiring from other firms though these latter point estimates are positive. The picture that emerges is one of a change in the composition of hiring, with some indication that non-policy firms hire from other firms in the database as a substitute. In the end, effects on overall hiring are small and we can reject declines in overall hiring by non-policy employers.

We now proceed to examining VMW spillovers to the wages of non-policy establishments. The above results suggest the effects may be minimal given the small to negligible impacts on overall hiring. Nevertheless, given that the composition of new hires at non-policy establishments changes in response to large retailer VMWs, with specifically fewer employees flowing from VMW firms to non-policy establishments, it is possible this compositional impact may affect the wages paid by non-policy employers. We examine this possibility in the next section on spillover wage effects of large retailer VMWs.

5.4 Spillover effects of large retailer VMWS on wages of competitor firms

Did competitor firms operating in the same labor market as large retailers with VMW policies adjust their wages after these policies were implemented? We begin by examining this with event studies around $15 VMW events before examining effects for all groupings of events using a pooled difference-in-differences specification. For all analyses, we present results using the two measures of exposure discussed in Section 5.2.

Figure 8 plots the estimated γ_k coefficients from equation 5 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). Here, the measure of exposure is the large retailer's gap. The figures shows no clear effect of the VMW event on hourly wages across the three samples of non-policy establishments. If anything point estimates trend negative towards the end of the event window. However, in all cases, even when the estimates that are statistically different from zero, they are economically negligible. It is important to note the scale of the y-axis, which runs up to a 1 log point effect on earnings. Effects on large retailers' own wages after VMW events ranged from 3.5-6.8 log points. Thus, the point estimates of the effects on non-policy establishment wages are orders of magnitude smaller and effectively zero. Appendix Figure D5 documents this lack of relationship over a longer time horizon–up to 12 months after the VMW event.

We also examine potential spillover effects on competitor wages using the market gap as the measure of exposure to large retailer policies. Figure 9 plots these effects for $15 VMWs where the panels represent the same groupings of non-policy establishments as in Figure 8. Here the y-axis is scaled so that the maximum value runs to roughly the value of a one standard deviation increase in the market gap. Effects on non-policy wages are once again small using this alternative measure of exposure. Point estimates are positive in one or two months in panels (a) and (b), but most estimates are centered exactly around zero. For connected non-policy establishments, point estimates are initially centered around zero and then trend negative. In all cases though, even the estimates that are statistically different from zero are economically negligible.

Table 7 reports wage effects on non-policy establishments across all groupings of events using the regression in Equation 6. The sample consists of non-policy establishments in connected industries. 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.

In the first row of Table 7, we report our estimated $\tilde{\gamma}$ for all establishments in connected industries across all large retailer VMWs. Across the board we estimate precise, economically negligible effects on the wages of non-policy employers (see row 1 of Table 7). The second row of Table 7 estimates effects on policy firm wages restricting to each subsequent grouping of events and sample of non-policy establishments.[35] As in Table 2 from Section 4.2, the estimates in this row show that policies with more bite have larger effects on policy employers' wages than those with smaller bite.

The final row of Table 7 (row 3) presents our estimated cross-employer wage elasticities. Across all sets of events, we estimate precise, economically negligible spillovers to non-policy establishments, including those connected to the large retailer via past worker flows. Among this latter group, for $15 VMW events, we can rule out spillovers greater than 2.7% 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 D5.

Table 8 and 9 explore spillovers in connected establishments relative to unconnected establishments within our sample set of industries. The estimates once again show precisely estimated, negligible impacts of large retailer gaps on non-policy firm wages. Table 9 reports these effects split by employment share of the large retailer. Columns 1-3 compare wage effects in connected vs. unconnected establishments in CZs where the large retailer's employment share is relatively low (bottom 6 deciles across CZs). Columns 4-6 report these effects in higher employment share CZs (top 4 deciles). Even in CZs where the large retailer has relatively high employment share (mean employment share of the large retailer in each grouping of CZs is reported at the bottom of the table), we continue to find no evidence of economically meaningful effects on non-policy wages in connected vs. unconnected establishments.

Table 10 reports results on spillovers using the market gap as the measure of exposure to large retailer VMWs. Once again, the sample is restricted to non-policy establishments in connected industries, or to connected non-policy establishments in column 4. The bottom of the table reports the value of a single standard deviation in the market gap measure, which underscores the small magnitude of the shock. A one-standard-deviation increase in the market gap is equivalent to a 0.2% increase in the market-level wage. Movements in wages at non-policy establishments, reported in the first row of the table,

[35]For all regressions in the table, including in row 2 on policy wage effects, the unit of observation is at the non-policy-establishment-by-month level. Thus, policy wage effects are weighted by the number of non-policy establishments across CZs. This, combined with the fact that non-policy establishments may not be present in all CZs where the large retailer operates, particularly for smaller samples of non-policy establishments, explains why policy wage effects in row 2 are not identical to those in Table 2.

are yet another order of magnitude smaller, suggesting little responsiveness to the implied effect of large retailer VMW on the market wage. The association between increases in the market gap and the large retailer's own wages is shown in the second row. A one standard deviation in the market gap is associated with anywhere from a 0.6-1.04% increase in wages at the large retailer or three to five times the implied increase in the local market wage. Taken together, the two pieces of evidence–on non-policy wages and large retailers' wages–point to negligible spillover effects of large retailer VMW events on non-policy firm wages.

5.5 Alternative specifications and robustness checks for spillovers among firms in the credit bureau database

We employ a host of alternative specifications to test for spillovers and implement several robustness checks to confirm the null results reported in Section 5.4 above. We describe these checks in brief below.

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. To reduce measurement error in establishment-level wages, we measure the non-policy gap over months -12 to -3, allowing for a placebo-in-time check for mean reversion after month -3.

Appendix Figure D7 shows that wages in CZs with a higher non-policy gap were lower than and gradually converging towards lower gap areas prior to the event. To account for drift in wages, we fit a linear trend through the pre-period estimates for months -12 to -2, 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 D8. The figures indicate no sizable spillovers using this alternative methodology. We compare these results to the impact of a state minimum wage change on the wages of non-policy companies.

In order to confirm the efficacy of this robustness check, we estimate the impact of the increase of California's statutory minimum wage to $12 on January 1, 2019 on non-policy

companies in our data. We find the expected impact of statutory minimum wages on non-policy firms that is close in magnitude to the expected effect given a mean own-wage gap with respect to the $12 minimum of close to 3%. The effect on wages is an increase of around 2.5% by about 6 months after the policy comes into effect.[36]

Bunching analysis of spillover effects We provide additional analyses on effects of large retailer VMWs along the wage distribution in Appendix Section D.4. Appendix Figure D10 shows effects of large retailer VMWs on their own distributions. The figure has three panels which show the effects for all, major, and $15 VMWs. Plotted is the coefficient from a regression identical to Equation 3, but where the outcome is the share of workers in a given wage bin. The figure then plots the coefficients from separate regressions where the outcome is the share of workers in the wage bin in question. The policies result in sharp bunching at the new minimum with limited spillovers up the wage distribution. For $15 minimum wages, there is some evidence of bunching in the bin just before the minimum, which may indicate delayed or incomplete compliance with the company policy. However, the overwhelming impact on the wage distribution is a spike at the new minimum.

Appendix Figures D11 and D12 shows the effect of large retailer VMWs on non-policy establishments using the large retailers' gap or the market gap as the measure of exposure, respectively. In both cases, we see that the association between exposure to the large retailer's policy in the post period has little effect on wage bins well above the minimum wage. Instead the results indicate shifts out of or into the lowest wage bins and some increases near the large retailer's VMW. However, coefficients for the relevant bins are extremely small in all cases, representing economically negligible shifts in the wage distribution even at these lower wage bins.

Heterogeneity by shared industry with large retailer vs. other industry As a final robustness check, we use whether the policy and non-policy firms are in the same industry as an alternative measure of the proximity (rather than worker flows used above).[37] Appendix Table D8 estimates the reduced form effect of large retailer VMWs on non-policy establishments in the large retailer's industry relative to non-policy establishments in other connected industries. Appendix Table D9 reports these effects split by employment share of the large retailer. Regardless of whether non-policy firms are in CZs where policy firms have a low or high employment share, we continue to find negligible wage spillovers and no differential effect on the wages by large retailer's employment share. Finally, Appendix Table D10 reports the wage spillover effect of large

[36] Appendix Figure D9 reports the results.

[37] These results are reported in Appendix Tables D8, D9, and D10.

retailer VMWs using the market gap as the exposure measure, again using shared industry with the large retailer as the measure of proximity. The results continue to indicate no economically meaningful spillover effect on wages using this measure of proximity and exposure to large retailer policies.

5.6 Evidence on spillovers to smaller companies

The fact that we are able to observe worker flows between large retailers with VMWs and other firms in the credit bureau database make these data a unique and rich setting for exploring potential spillover effects from large retailer VMWs. Firms in the credit bureau database also represent larger firms, making them a relevant set of firms for assessing models of imperfect competition in labor markets, as we discuss in detail in Section 6.

Nevertheless, when assessing the generalizability of negligible wage spillover results presented thus far, it is important to note that the firms in the credit bureau database represent just 12-15% of overall employment and about 25-29% of retail employment in the local labor markets shared with the large retailers (see Appendix Table D1). They also tend to represent larger, multi-establishment firms. Thus, it is possible that there are other, smaller firms more strongly affected by large retailer VMWs and therefore more likely to respond to these policies. We explore this possibility in the next section in two ways.

First, we focus on a large sample composed of smaller companies, for which we obtain detailed wage data from a major payroll provider. These firms represent a key segment of the market not covered by firms in the credit bureau database: smaller companies whose employment does not span multiple local labor markets and whose wage policies may therefore be more responsive to local conditions than larger firms.

Second, we explore effects in the market overall, using data on average quarterly wages from the QCEW. These data allow us to capture the overall market-wide consequences of large retailer VMWS.

Spillover effects using payroll data for small companies We obtain data on the wages of workers at a large sample of small companies from a major payroll provider to smaller firms (hereafter, "Payroll Provider" and "payroll provider firms"). Appendix E describes these data in detail, and descriptive statistics comparing firms in the database to the U.S. overall are provided in Appendix E.1. The Payroll Provider data cover millions of employees at smaller companies, the vast majority of which have employment of 50 workers or less. These smaller companies may be more susceptible to large retailer VMW shocks. Although we do not observe flows between the smaller companies in the Payroll Provider data, there is strong representation of retail and services in the data

and thus the industries most likely affected by large retailer VMWs. We focus on firms in the connected industries (as measured in Section 5) for our analysis. For tractability, we work with a random 10% sample of firms from the overall database of firms with valid wage data. We further restrict to firms we observe at least 3 times in a 12-month pre-period and 3 times in a 12-month post-period. Additional details on our sample, including coverage of our balanced sample, are available in Appendix E.2.

Table 11 reports results using the large retailers' gap as exposure to VMW policies. We also show the effect on large retailers' own wages when restricted to the same set of CZs covered by the Payroll Provider data. The cross-wage elasticity is also reported. We once again find very small effects on wages, even in this sample of small companies. The effect of the policies on large retailer's own wages are consistent with our earlier results though effects are not as large as in the full set of CZs covered by the large retailer. We estimate extremely small and generally statistically insignificant effects on smaller companies' wages. Cross-wage elasticities are also small and statistically insignificant, ranging from -0.02 to .043. Our largest estimated elasticity – again, noisily estimated – implies that just 4.3% of the wage increase at VMW employers after major events passes through to smaller companies.

Table 12 examines spillovers using the market gap as the measure of exposure to large retailer policies. We use the same market gap as defined using wage data from the credit bureau database, but as we discuss in Section 5.2, the market gap is strongly correlated with the large retailer's employment share in the overall CZ-level labor market and with the market-wide wage bill share of large retailers using data from the QCEW (correlation of 0.61). This makes the market gap measure a good proxy for the overall market gap. We continue to find extremely small effects on smaller companies' wages, ranging from .07-.19 log points across the different groupings of VMW events. These magnitudes are in line with the implied effect on the market wage, which is also extremely small in magnitude, representing increases in average wages at the market level of 0.12-.18 log points. Event study versions of Tables 11 and 12 are presented in Appendix Figures E5 and E6.

Spillover effects based on distance to large retailer We also use an alternative approach to estimating spillovers among smaller companies based on the physical distance of small firms from large retailers located in their CZ (mirroring prior approaches in the literature, e.g., Staiger et al. (2010)). To do so, we take VMW employers for whom we know the precise location of their establishments[38] and examine heterogeneity in the spillover effect by distance from the large retailer calculated using the distance between the large retailer and the centroid of the zipcode the smaller company is located in

[38]Data on store locations were obtained from MWPVL: Supply Chain Consultants.

for three major VMW events.[39] Appendix Table E5 shows the results. In column 1, we report the wage effects for the VMW companies. In column 2, row 1, we report effects on small companies within the first quintile of distance from the VMW company's establishments.[40] The second and third row show the effect for small companies in quintile 2 and quintiles 3 and 4, respectively. Spillover effects on wages across all distance categories are small and statistically insignificant. The results show zero or economically negligible spillovers across distance categories.

5.7 Market-wide spillover effects

The firms analyzed in Section 5.6 represent a large cross section of small companies, as the Payroll Provider services hundreds of thousands of small firms covering 9 million workers overall or 11% of the total number of hourly workers in the U.S. We continue to find economically small spillover effects among these firms. What about the U.S. economy overall? Can we detect spillover wage effects from voluntary minimums by a group of some of the largest employers nationally?

We analyze the effect of large retailer VMWs on average quarterly earnings effects across all firms in the local labor market and across all firms net of policy firms using data from the QCEW merged with data on policy firms' quarterly earnings from the credit bureau database. The lack of hourly wage information in the QCEW and the quarterly nature of the data make merging these data with the credit bureau data on large retailers nontrivial. As the credit bureau data does not contain measures of total monthly earnings, but cumulative year-to-date pay each month instead, we measure the aggregate quarterly wage bill precisely only over the first quarter of the year. Because of this, our analysis is a year-on-year difference-in-difference comparing the first-quarter average quarterly earnings of the policy companies, all employers, and all employers net of the policy companies across CZs with different levels of exposure to the policy company's voluntary minimum.

For our exposure measure, we multiply the own-gap measure by the policy company's wage bill share in the local CZ to proxy for the "market-level gap." Since the own-gap measure represents how much the company's employment-weighted hourly wage changes with the voluntary minimum, weighting this by the wage bill share in the local area should yield a measure of the implied change in the market wage bill. We then standardize this

[39] We focus on just three events due to computational constraints involved in calculating bilateral distances between establishments in the Payroll Provider data and the establishments of the three VMW employers. All events occurred at least 12 months prior to the COVID-19 pandemic to avoid contaminating treatment effects with potential effects of the pandemic.

[40] The thresholds for each quintile of distance vary across the different VMW employers due to the number of establishments they have in each CZ. For example, the maximum distance for the first quintile of distance ranges from under 2 miles to 13 miles across the three VMW events considered.

measure of exposure across all CZ-level observations in the stacked dataset. Because of the quarterly nature of the data, some voluntary minimums across different large retailers occur within the same quarter, so we combine these events into one, netting out both companies' employment and wage bill from the total wage bill and employment in that CZ. We use the larger policy company's wage bill share and own-gap measure to construct the treatment variable, but results are robust to using the information corresponding to the smaller of the two policy companies.

Table 13 reports the results of this analysis. We see that the proxy for the market gap is associated with precisely estimated log average quarterly earnings increases for the policy companies only (see column 1), an increase of 0.5 log points. Column 2 reports the effects on overall average quarterly earnings, and column 3 reports the effect on average quarterly earnings after policy firm employment and wage bill has been netted out from the total. The impact on overall average quarterly earnings is extremely small and statistically insignificant: an increase in log average quarterly earnings of .01 log points. The effect on log average earnings net of policy companies is -.06 log points. These results indicate that large retailer VMWs have negligible effects on overall earnings in their local labor markets.

6 Implications of limited spillovers for models of imperfect competition.

What are the implications of limited local spillovers from VMWs for leading models of imperfect competition? Section 5 already documented that the firms we study in the credit bureau data, including those documented to share a labor market with VMW companies are large, multi-establishment firms. What might we expect to be the effect of VMWs on these other large employers in shared labor markets under oligopsonistic wage setting (Staiger et al., 2010; Berger et al., 2022)?

We use the richness of the credit bureau database to calculate pre-period summary statistics on VMW employers and their competitors that are relevant for predicting spillovers from models of oligopsonistic wage setting, including payroll and employment shares at a local level.[41] We benchmark our estimates against the predictions of these models.

[41] We provide a general picture of VMW employers' and non-policy establishments' employment shares in Appendix F.1. Appendix Figures F1, F2, and F3 provide the distribution of VMW employers' employment share out of all CZ-level employment, retail employment, and within the credit bureau sample for all groupings of VMW events. Appendix Figure F4 provides the employment share of non-policy establishments within the credit bureau sample across different event groupings. In our model calibrations, we calculate employment shares of VMW employers and non-policy establishments within each market definition.

Berger et al. (2022) provide a model of oligopsonistic competition that incorporates worker preferences over jobs and labor market concentration. Their model predicts that wage shocks by a "leading" firm k in a concentrated market will "tighten" the labor market, giving rise to strategic wage responses by "follower" firms that depend on the firms' respective payroll shares (s_k and s_j) as well as the degree of within-market (η) and across-market (θ) substitutability for workers. To approximate firm payroll shares, we multiply average hourly wages by employment for firms across each of the markets we define in Section 5: shared commuting zone, connected industry, and connected establishment. We also consider additional versions of the above markets limiting to CZs with high employment share of the VMW employer (in the top 4 deciles of overall employment share across the CZs where they operate). We also consider the CZ X 3-digit NAICS definition used by Berger et al. (2022). Finally, we take our groupings of VMW experiments and examine cross-wage elasticities for each market definition and each grouping of VMW events. Figure 10 plots our reduced-form estimates relative to estimates from the literature.[42]

Berger et al. (2022) show that their quantitative model can reproduce cross-wage elasticities consistent with the effect of VA-hospital wage-spillovers found in Staiger et al. (2010). In their model, the response by a follower firm i to a leader firm k's wage changes in market j can be written as:

$$\Delta \log w_{ij} = (1 - \Gamma^*_{ij}) \frac{s^*_{kj}}{1 - s^*_{ij}} \Delta \log w_{kj}, \tag{7}$$

where Γ^*_{ij} is defined as follows:

$$\Gamma^*_{ij} = \frac{s^*_{ij}(\eta - \theta) + \theta(\eta + 1)}{1 + (\eta + 1)(1 - s^*_{ij})} \tag{8}$$

Thus, the cross-wage elasticity (CWE_{BHM}) can be written as (see Appendix F.2 for a short derivation):

$$CWE_{BHM} = \frac{(1 + \eta) s_{ij} s_{kj} (\eta - \theta)}{[1 + (1 + \eta)(1 - s_{ij})] s_{ij} (\eta - \theta) + \theta(\eta + 1)}. \tag{9}$$

Using the parameters provided by Berger et al. (2022) for θ and η of 10.85 and 0.42, respectively, and calculating the pre-shock payroll shares of VMW companies and the average competitor for each market definition, we obtain a distribution of cross wage elasticities ("BHM-predicted CWEs") which we compare to our estimates in Figure 11.

[42] We compare our estimates to four relevant estimates from the literature: estimates by Staiger et al. (2010) in the context of a wage policy change by the Veterans Affairs Hospital system and estimates by Bassier (2022) of collective bargaining spillovers in the context of South Africa.

There is no relationship between our cross-wage elasticity estimates and those predicted by the model. Uniformly, we estimate economically small or negligible cross-wage elasticities that fall below those predicted by BHM, regardless of the labor market definition used. η/θ would have to be much much smaller to rationalize the pattern of CWE, equal to approximately 1.33 (or close to equality), rather than the 25.8 ratio implied by their parameters, recovered from heterogeneity in employment and wage effects of state-level taxes across firm payroll shares. The strength of strategic interactions is governed by this ratio, with $\eta/\theta = 1$ representing the monopsony limit with no strategic responses.

We also calculate the expected effects of large retailer VMWs using the model by Bassier (2022). While this model shares the logit structure of Berger et al. (2022), it uses flows of workers rather than nested labor markets to deliver strategic interactions. The model helps to explain large spillovers from collective bargaining agreements on uncovered firms connected to the covered sector. Spillovers were estimated to be large in this context, amounting to a cross-wage elasticity of 0.8.

We use the model in Bassier (2022) calibrated with the empirical employment shares observed in our data to predict the expected VMW spillover effects. The model builds off of standard static models of monopsonistic competition based on heterogeneous workers' idiosyncratic preferences across firms. Firms are not atomistic, which enables spillovers to arise. However, the extent of and importance of spillovers depends on the relative sizes of firms and their competitors. The cross-firm wage elasticity in this model is defined as follows:

$$CWE_{\text{Bassier}} = \frac{1+\tilde{\eta}\beta}{1+\tilde{\eta}\beta p_k}\left(\frac{\tilde{\eta}\beta p_k}{1+\tilde{\eta}\beta} + \frac{\beta p_k \beta p_i}{\eta^n_{ii}(1+\eta^n_{ii})(1+\tilde{\eta}\beta)}\right). \tag{10}$$

In the model, $\tilde{\eta}$ is the slope of the marginal revenue product of labor, β captures how worker utility responds to the firm wage, p_i is the employment share of follower firm i, p_k is the employment share of leader firm k, and η^n_{ii} captures the own-labor supply elasticity faced by firm i and is equal to $\beta(1-p_i)$.

We use the same parameters as Bassier (2022): $\beta = 6$ and $\eta = 0.8$. For leader and follower employment shares, we calculate these for each grouping of events and markets defined above. Results are reported in Appendix Figure F5. Estimates of spillovers from Bassier (2022) are uniformly larger than those predicted by Berger et al. (2022). We note the very different context the model is designed to describe: spillovers from collective bargaining agreements. Union threat effects may lead to much larger spillovers than those that might be observed with a voluntary minimum wage by a single large retailer (Fortin et al., 2021). Thus, we use comparisons with Berger et al. (2022) as our benchmark comparison.

Our conclusion is that current oligopsonistic models of wage-setting seem ill-suited

to explain responses to large retailer voluntary minimum wage events across a broad set of events and variously defined markets. While it is clear that large retailers who set voluntary minimums exercise wage-setting power over their employees, models that combine this monopsony power with additional frictions due to labor market concentration predict much higher spillovers than we observe empirically, even accounting for a substantial range of both leader and follower firm payroll shares across markets. Thus, monopsonistic competition may be an appropriate approximation for the low-wage retail sector in the U.S. as measures of employment concentration do not strongly correlate with wage 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.[43] 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 across all markets, 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, 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

[43]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.

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 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 explore spillovers across a variety of definitions of large retailers' labor markets, from firms in the same commuting zone, to those in related industries, to individual establishments connected by worker flows to the large retailer. Across all three definitions of the labor market and multiple measures of exposure to voluntary minimums, we find no detectable impact on other employers' wages. Among a large swath of smaller firms and in the labor market overall, we also find limited evidence for wage spillovers. These findings are inconsistent with existing quantitative calibrations of local labor markets characterized by strategic wage responses across employers.

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 at a local level. 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, these shocks have had 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 may do so at a national level—as suggested by their rapid, successive adoption across major retailers throughout the U.S. Further work on national wage-setting strategies and

their spillover effects in low wage markets is warranted, particularly in the context of eroding labor market institutions.

What are the policy implications of our results for policy surrounding low-wage labor markets? In an era of rapidly eroding labor market institutions and congressional impasse on raising the federal minimum wage, policy makers seeking to shore up wages in the low wage market have entertained a variety of tools for doing so. State minimum wages have proven an effective tool at reducing wage inequality and lowering poverty (Cengiz et al., 2019; Dube, 2019a). The limited recent evidence on spillovers from collectively bargained agreements suggest that when unions are present and the covered sector represents a significant fraction of firms, spillovers may be large (Bassier, 2022). By contrast, policies that rely on the potential influence of large players – national retailers, for example, or federal contractors – are likely to have little broader effect on the low wage market.

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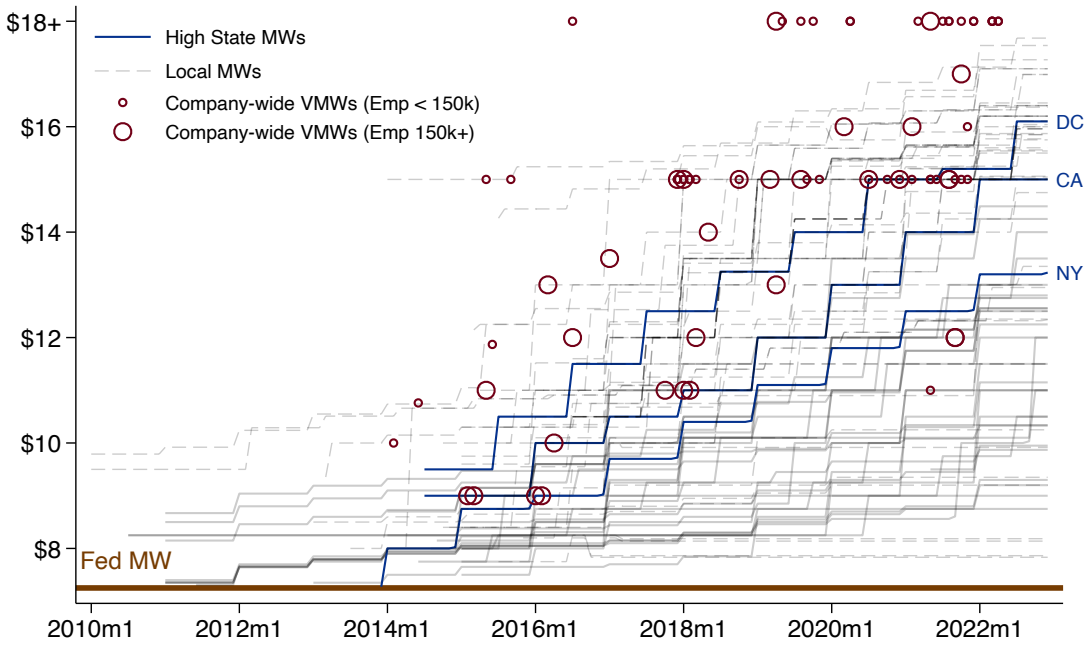
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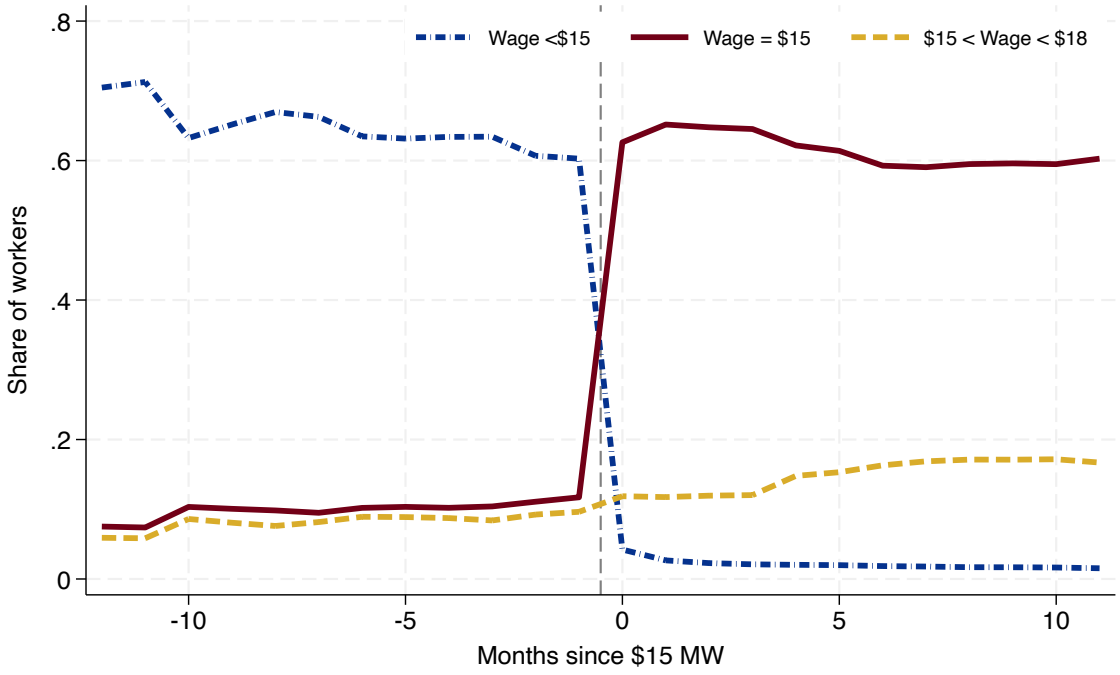
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Figure 1: Landscape of minimum wages in the U.S. since 2010



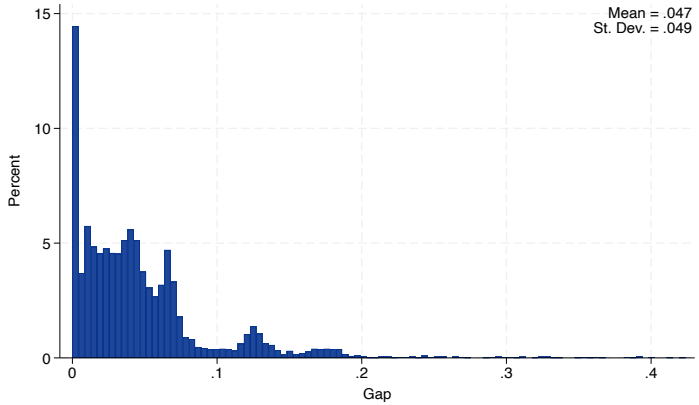
Notes: This figure plots various types of minimum wage policies in the U.S. since 2010, with the nominal minimum wage value on the y-axis. The federal minimum wage of $7.25 is shown in the thick solid brown line at the bottom of the figure. Solid grey lines indicate state minimum wages above the federal 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 voluntary minimum wage among large retailers

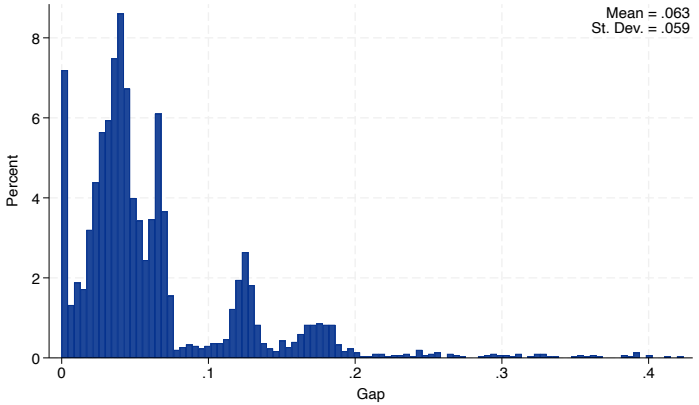


Notes: This figure plots changes in the hourly wage distribution at three large retailers (employment $>$ 150k) around the adoption of a $15 voluntary minimum wage. Depicted is the share of all hourly workers (including those earning $30 or more) below $15, at $15 up to, but not including, $16, and at $16 up to, but not including, $18, before and after the policy's implementation. *Data sources*: Large credit bureau.

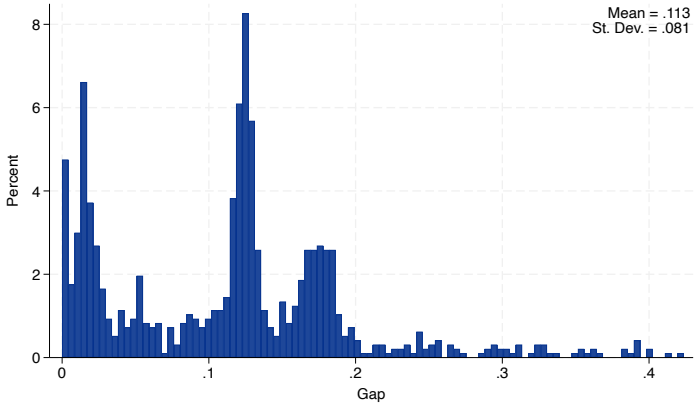
Figure 3: Distribution of own-gap measure across large retailer CZs for all, major, and $15 VMWs



(a) All VMWs



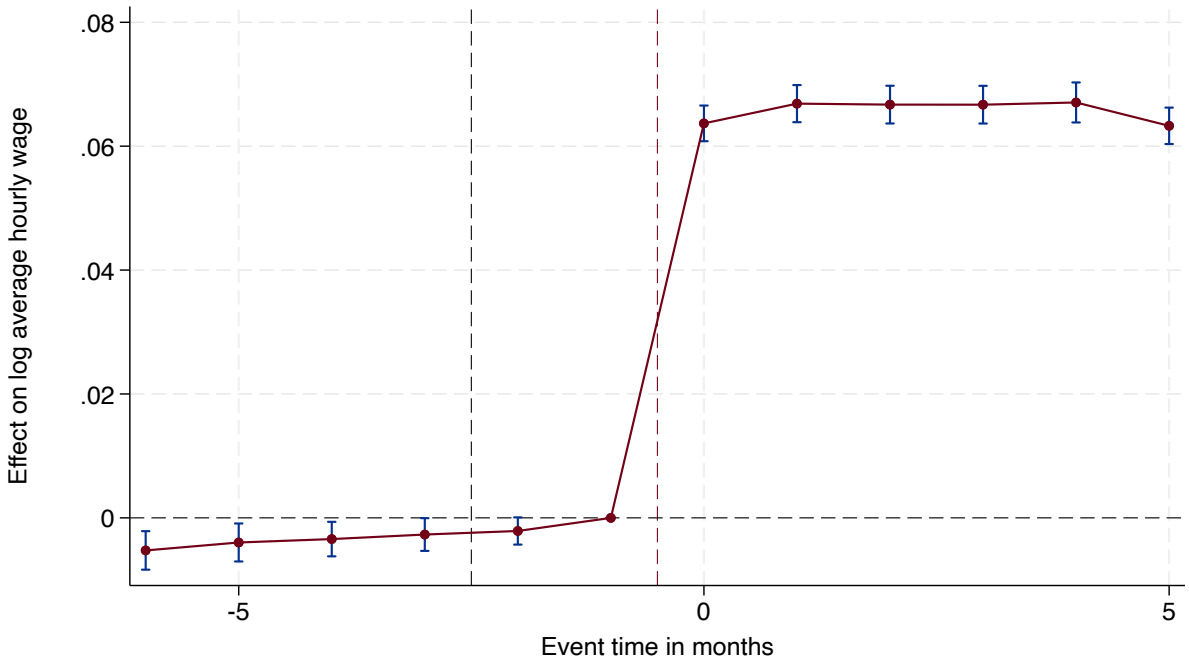
(b) Major VMWs



(c) $15 VMWs

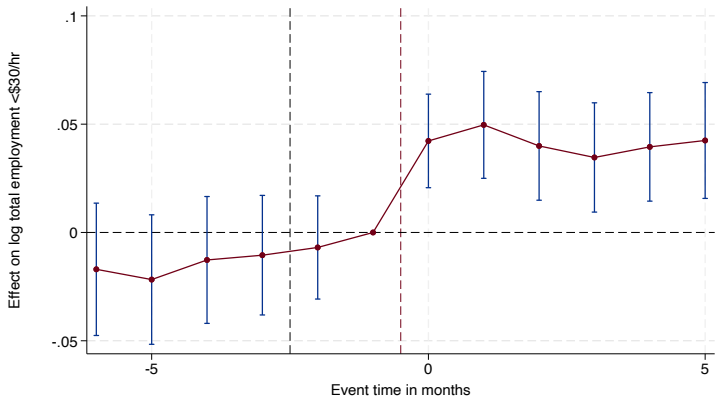
Notes: This figure plots the distribution of the own-gap measure across large retailer CZs for all VMW events, major events, and $15 events (see equation 1 for the definition of the own-gap measure) The mean and standard deviation of the gap measure across all CZs in the included events is reported as well. *Data sources*: Large credit bureau.

Figure 4: Effect of large retailer $15 VMW on own wages

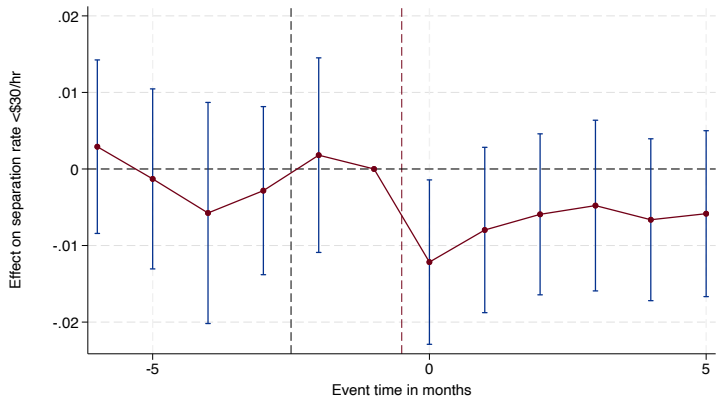


Notes: This figure plots the regression coefficients on the standardized gap in each commuting zone (CZ) between employees' wage bins and the company minimum wage among large retailers with a voluntary $15 minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average hourly wage of employees in that CZ. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($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 are shown. *Data sources*: Large credit bureau.

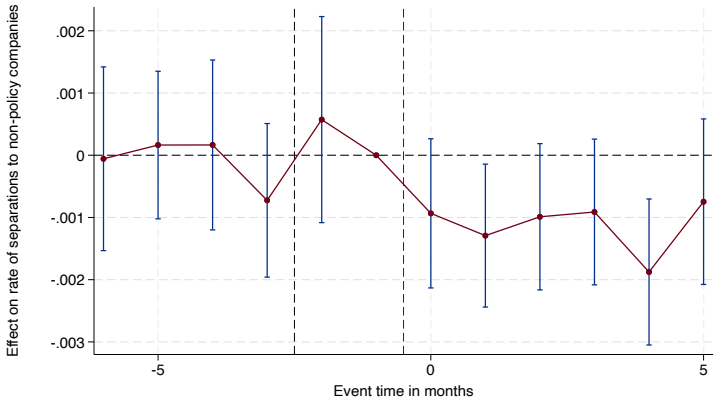
Figure 5: Effect of large retailer $15 VMW on own employment, separation rates, and new hiring



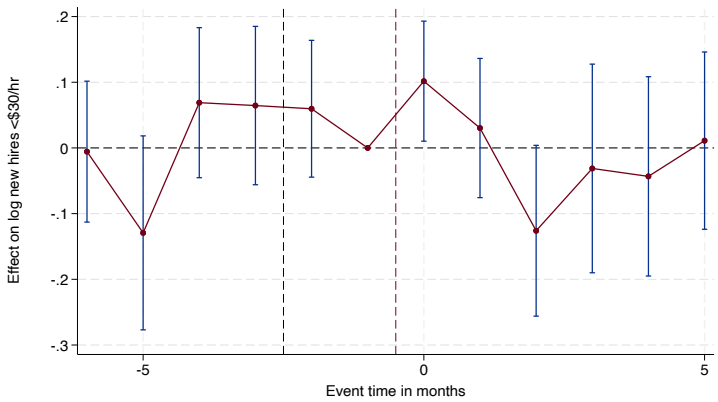
(a) Log employment of workers earning less than $30



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



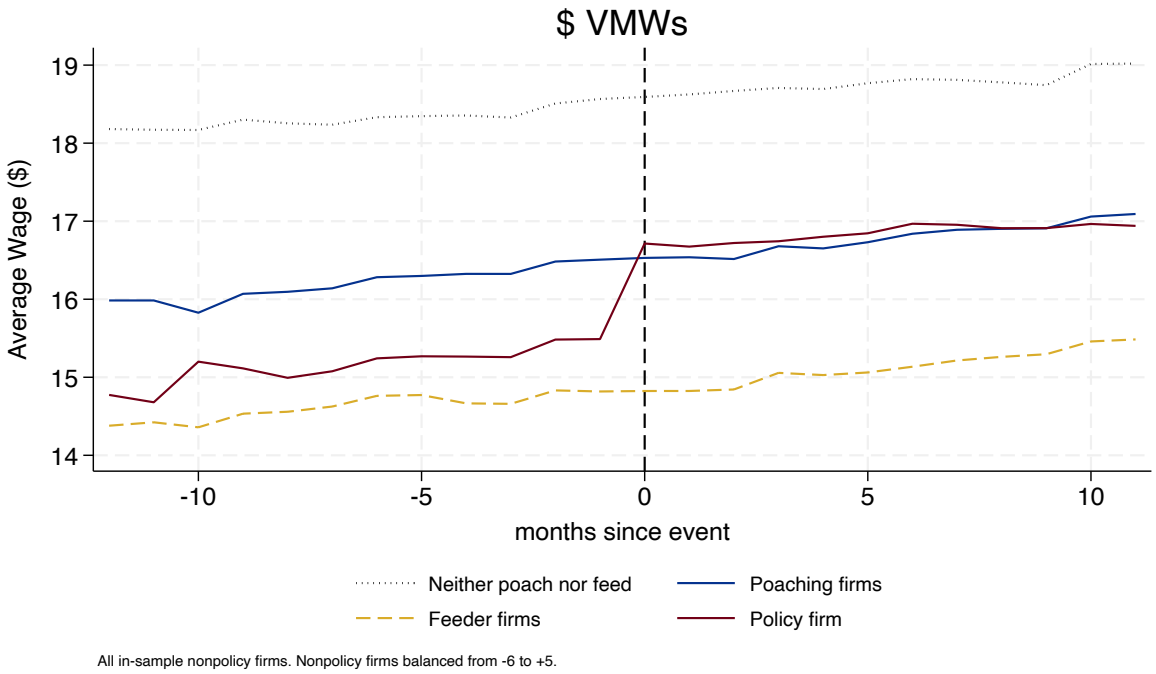
(c) Rate of separations to non-policy companies



(d) Change in log new hires under $30

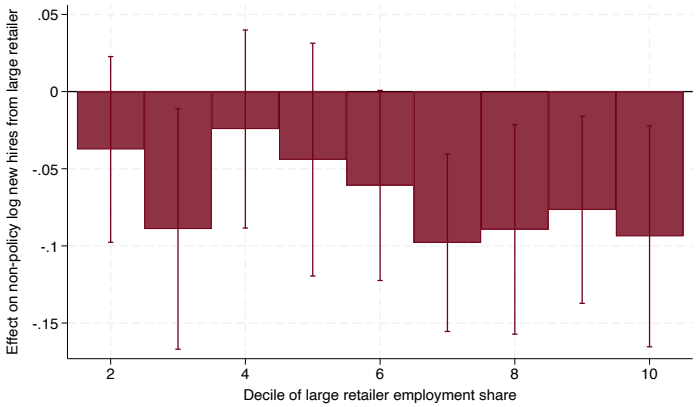
Notes: This figure plots the regression coefficients on the standardized gap in each commuting zone (CZ) between employees' wage bins and the company minimum wage among large retailers with a voluntary $15 minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed 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 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 are shown. *Data sources*: Large credit bureau.

Figure 6: $15 VMW large retailer wages vs. competitor wages

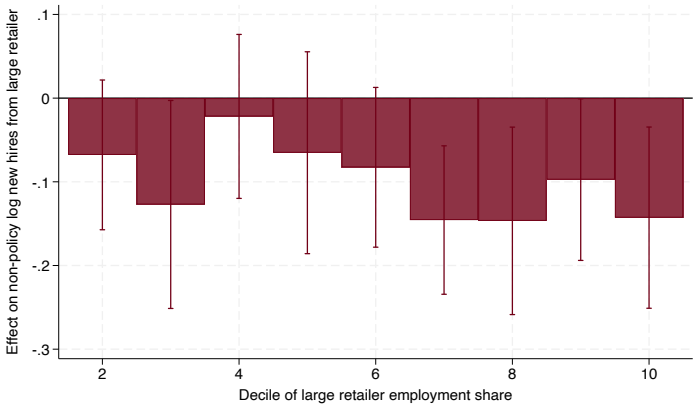


Notes: This figure plots the average hourly wages at large retailers vs. their competitors over a 12-month window around $15 VMW events. *Data sources*: Large credit bureau.

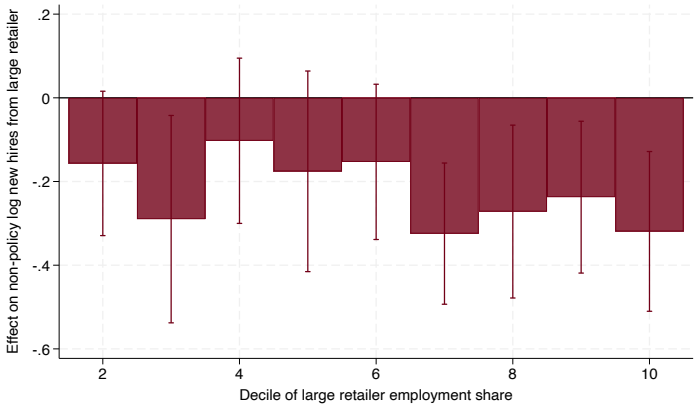
Figure 7: Effect of large retailer VMWs on other firms' hiring from the large retailer by employment share of large retailer



(a) All VMWs



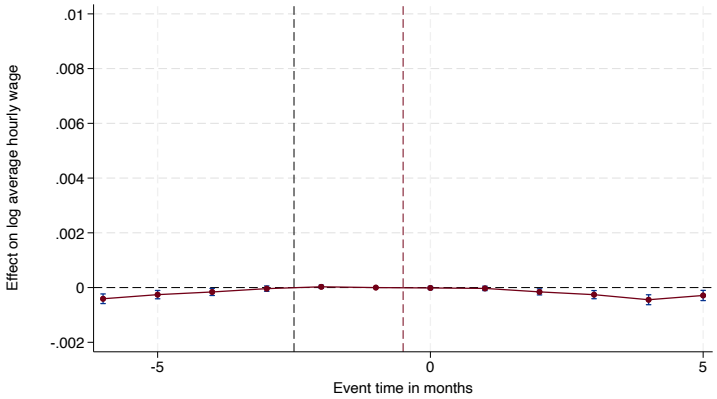
(b) Major VMWs



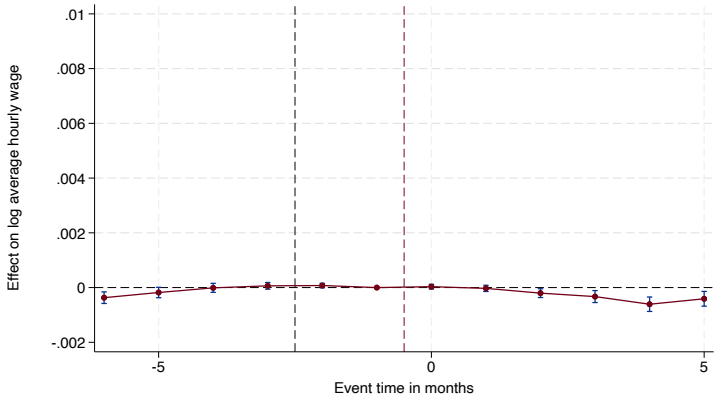
(c) $15 VMWs

Notes: This figure plots the regression coefficients on the standardized large retailer gap in each commuting zone (CZ) (see equation 1 interacted with the decile of the large retailer's employment share in that CZ. Decile 1 is omitted. The dependent variable is the CZ-level log number of new hires by non-policy companies of former policy company employees. Panel (a) shows the results for all VMWs; Panel (b) for major VMWs; and Panel (c) for $15 VMWs. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 policy employees. 95% confidence intervals included. *Data sources*: Large credit bureau.

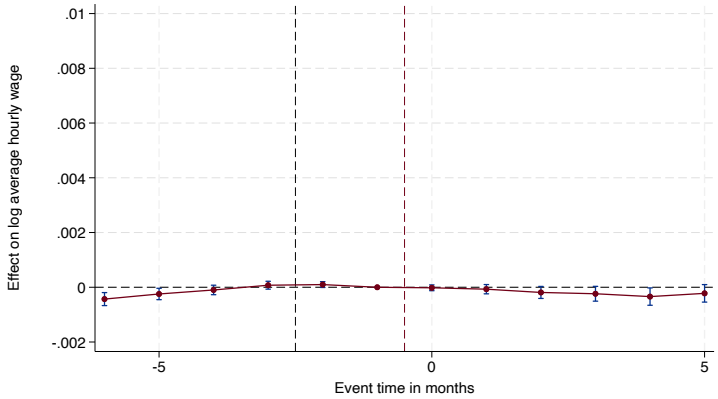
Figure 8: Effect of large retailer $15 VMW on other firms' wages, large retailer gap



(a) All establishments



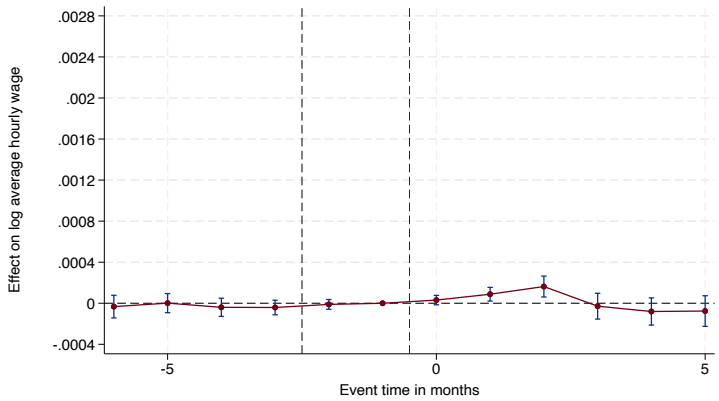
(b) Establishments in industries connected by worker flows



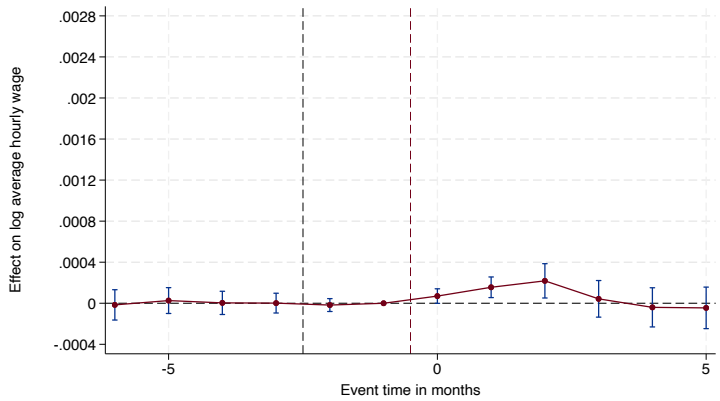
(c) All establishments connected by worker flows

Notes: This figure plots the regression coefficients on the standardized large retailer's 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. *Data sources*: Large credit bureau.

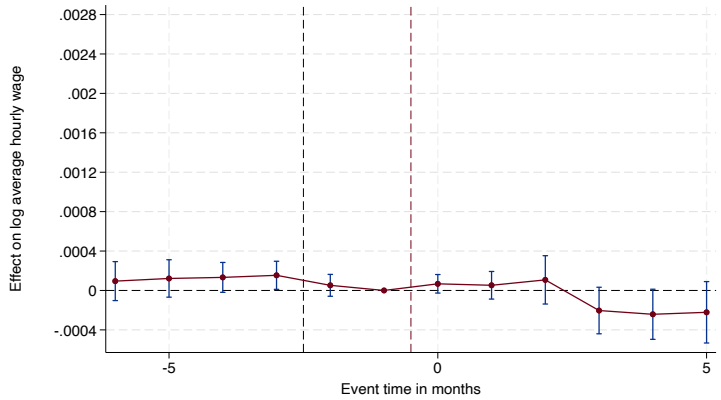
Figure 9: Effect of large retailer $15 VMW on other firms' wages, market gap



(a) All establishments



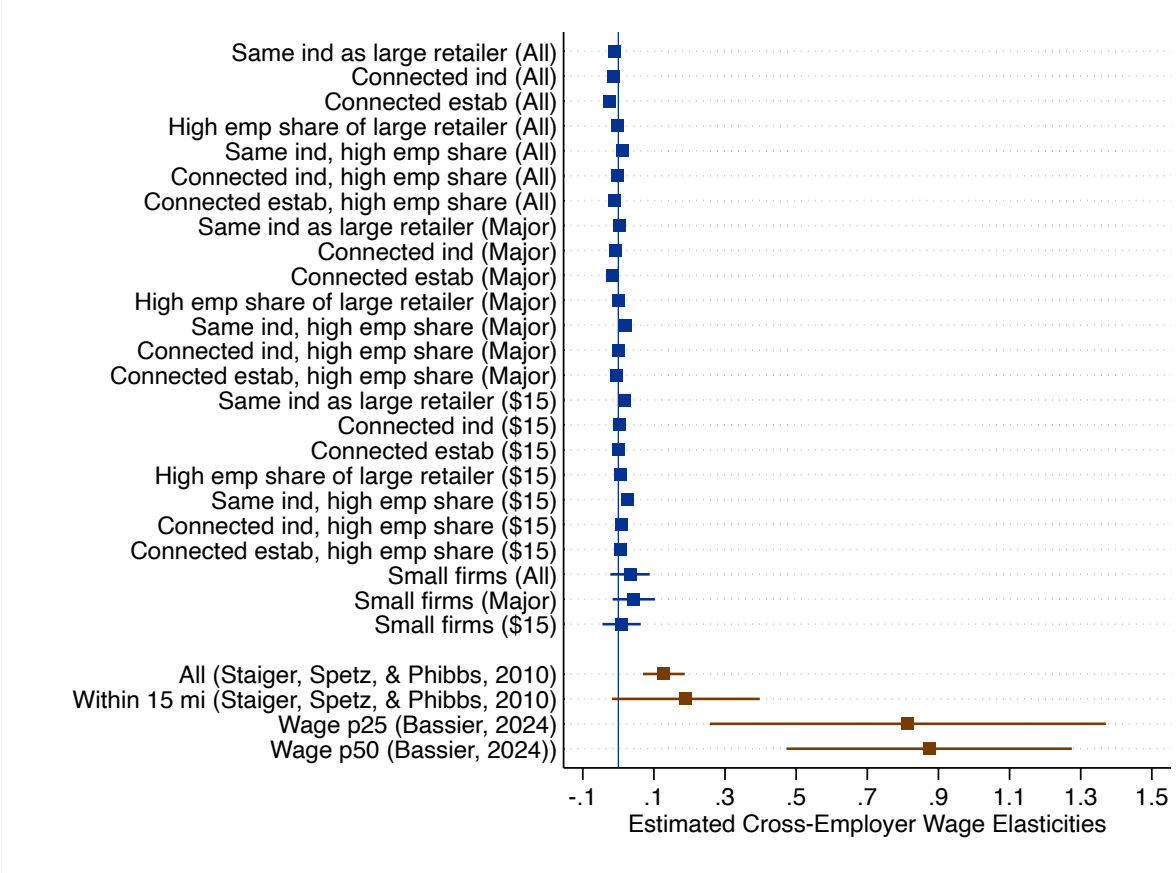
(b) Establishments in industries connected by worker flows



(c) All establishments connected by worker flows

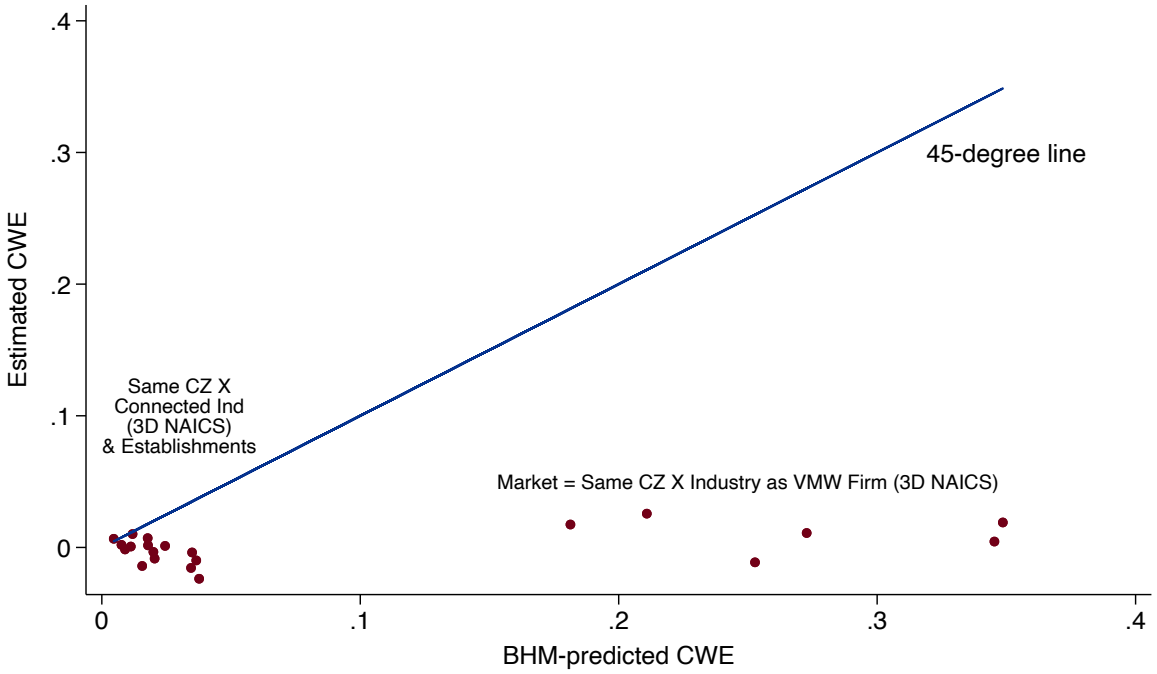
Notes: This figure plots the regression coefficients on the standardized market gap in each commuting zone (CZ) between binned wages of employees at large retailers with a voluntary $15 minimum wage (see equation 4 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average hourly wage of non-policy employees in that CZ. Panel (a) includes all non-policy establishments in the CZ; Panel (b) restricts the sample to non-policy establishments in industries connected to the large retailer by worker flows; and Panel (c) restricts the sample non-policy establishments with a history of hiring workers from or having workers separate to the large retailer in the year before the policy is adopted. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($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. *Data sources*: Large credit bureau.

Figure 10: Cross-employer wage elasticity estimates compared to the literature



Notes: Cross-employer wage elasticities from different samples, datasets, and specifications compared to closely comparable estimates from the literature. *Data sources*: Large credit bureau; Payroll Provider; authors' collection of literature estimates.

Figure 11: Cross-employer wage elasticity estimates relative to BHM model predictions



Notes: Comparison of estimated cross-wage elasticities to those predicted by Berger et al. (2022). *Data sources*: Large credit bureau.

Table 1: Comparison of credit bureau firms 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 (BLS). 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

Indep. Var.: Large Retailer Gap X 1(Post)	All events	Major events	$15 events
Dep. Var.:			
Log avg. wage	0.035***	0.047***	0.068***
	(0.000)	(0.001)	(0.002)
Log employment	0.016***	0.026**	0.044**
	(0.006)	(0.012)	(0.019)
Separation rate	-0.004***	-0.005***	-0.007***
	(0.000)	(0.001)	(0.001)
Separation rate to non-policy firms	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Log new hires	-0.026**	0.010	-0.014
	(0.010)	(0.019)	(0.029)
Total employment elasticity	0.450***	0.551**	0.644**
	(0.169)	(0.257)	(0.288)
	[0.118,0.781]	[0.047,1.055]	[0.078,1.211]
Obs	63,928	30,547	9,238
CZs	632	631	380
Events	20	8	4
St. dev. gap	.05	.06	.08
Pre-period mean:			
Sep rate	.064	.065	.072
Sep rate to non-policy firms	.012	.014	.014
Log new hires	3.1	3.1	2.74
Month from event FEs	Y	Y	Y
CZ FEs	Y	Y	Y

Notes: This table reports the estimated coefficients on the standardized gap measure interacted with a post-period indicator from regression equation 3 on a stacked policy company dataset of voluntary minimum wage events. (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 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), 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: Large retailer VMW effects on the composition of hiring at connected non-policy establishments

	Dep. Var.: Probability of new hires		
Independent variable	Overall	From Large Retailer	From Others
Large retailer gap X 1(Post)	-0.0004	-0.0048***	-0.0003
	(0.0006)	(0.0017)	(0.0006)
Obs	4,032,624	4,032,624	4,032,624
CZs	628	628	628
St. dev. large retailer gap	.054	.054	.054
Dep var pre-treat mean	.81	.21	.79
Company X CZ FEs	Y	Y	Y
Month from event FEs	Y	Y	Y

Notes: This table reports the estimated coefficients on the standardized gap measure interacted with a post-period indicator from regression equation 6 in a stacked dataset of VMW events, 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 column 1, the outcome is the probability of any new hiring. In columns 2, the outcome is the probability of new hiring from the large retailer. In columns 3, the outcome is the probability of new hiring from other firms, i.e., not the large retailer. 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. All VMW events are included. 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: CZ-level effects of VMWs on non-policy firms' hiring

	Dep. Var.: Log new hires		
Independent variable	Overall	From Large Retailer	From Other
Large retailer gap X 1(Post)	0.007**	-0.018*	0.006**
	(0.003)	(0.010)	(0.003)
Obs	156,744	135,873	156,714
CZs	623	623	623
St. dev. large retailer gap	.049	.049	.049
Dep var pre-treat mean	5.51	1.83	5.37
CZ FEs	Y	Y	Y
Month from event FEs	Y	Y	Y

Notes: This table reports the estimated coefficients on the standardized gap measure interacted with a post-period indicator from regression equation 3 in a stacked dataset of VMW events, where the dependent variables are the log number of new hires by nonpolicy establishments from different sources at the CZ-level. (See equation 1 for the definition of the gap measure.) Each column 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. 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 5: CZ-level effects of VMWs on non-policy firms' hiring, heterogeneity by large retailer employment share

	Low Emp. Share			High Emp. Share		
Indep. Var.: Large Retailer Gap X 1(Post)	All events	Major events	$15 events	All events	Major events	$15 events
Dep. Var.:						
Log new hires	0.006*	0.007	-0.003	0.005	0.002	-0.003
	(0.003)	(0.005)	(0.006)	(0.004)	(0.005)	(0.008)
Log large retailer new hires	-0.021	-0.020	-0.035	-0.030***	-0.043***	-0.070***
	(0.016)	(0.029)	(0.045)	(0.010)	(0.016)	(0.026)
Log other new hires	0.008**	0.010*	-0.002	0.003	-0.001	-0.006
	(0.004)	(0.005)	(0.006)	(0.004)	(0.006)	(0.008)
Obs	94,746	43,174	13,968	61,968	28,210	9,240
CZs	485	462	309	480	442	247
St. dev. large retailer gap	.053	.064	.084	.044	.052	.074
Mean policy emp. share	0.053	0.057	0.009	0.097	0.103	0.029
Pre-treatment mean						
Log new hires	5.75	5.63	6.36	5.14	5.11	6.11
Log large retailer new hires	1.8	1.94	1.37	1.88	2.06	2.16
Log other new hires	5.62	5.5	6.23	4.98	4.95	5.95
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 standardized gap measure 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 in the bottom 6 deciles of the large retailer's employment share across CZs. Columns 4-6 limit the sample to firms in CZs where the large retailer's share of employment is in the top 4 deciles of the large retailer's employment share across CZs. 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.

Table 6: CZ-level effects of VMWs on hiring from large retailers, market gap

Indep. Var.: Market Gap X 1(Post)	All events	Major events	$15 events
Dep. Var.:			
Log new hires	0.014***	0.012***	0.002
	(0.003)	(0.004)	(0.006)
Log large retailer new hires	-0.018**	-0.037***	-0.082**
	(0.007)	(0.010)	(0.035)
Log other new hires	0.018***	0.018***	0.008
	(0.004)	(0.005)	(0.005)
Obs	156,714	71,384	23,208
CZs	623	614	379
St. dev. market gap	.003	.003	.002
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 standardized market 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 4 for the definition of the market 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 7: Cross-employer wage elasticity estimates, large retailer gap

Indep. Var.: Large Retailer Gap X 1(Post)	All events	Major events	$15 events	$15: pos. flows
Dep. Var.:				
Log avg. wage, non-policy	-0.0006***	-0.0004***	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0002)
Log avg. wage, policy	0.0394***	0.0524***	0.0713***	0.0623***
	(0.0008)	(0.0014)	(0.0021)	(0.0020)
Cross-employer wage elasticity	-0.0141***	-0.0084***	0.0020	0.0016
	(0.0029)	(0.0026)	(0.0022)	(0.0034)
	[-0.0198,-0.0083]	[-0.0135,-0.0034]	[-0.0023,0.0062]	[-0.0051,0.0083]
Obs	9,021,253	4,163,710	1,744,869	728,323
CZs	631	629	380	376
Firms	1053	1023	930	1579
St. dev. large retailer gap	.055	.067	.088	.082
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 standardized gap measure (see equation 1) interacted with a post-period indicator from regression equation 6 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 8: Spillover effects, heterogeneity by flows

Independent variables	Dep Var: Log average wage, non-policy		
	All events	Major events	$15 events
Large retailer gap X 1(Post) X 1(Flows)	-0.0002*	-0.0002*	-0.0003**
	(0.0001)	(0.0001)	(0.0002)
Large retailer gap X 1(Post)	-0.0004***	-0.0003**	0.0004**
	(0.0001)	(0.0001)	(0.0002)
1(Post) X 1(Flows)	0.0020***	0.0025***	0.0029***
	(0.0001)	(0.0002)	(0.0002)
Obs	9,040,130	4,170,109	1,744,869
CZs	631	629	380
Firms	1053	1023	930
Connected firms	968	929	803
St. dev. large retailer gap	.055	.067	.088
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 standardized 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 9: Spillover effects, heterogeneity by flows and employment share of the large retailer

	Dep Var: Log average wage, non-policy					
	Low Emp. Share			High 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)	-0.00014	-0.00014	-0.00022	-0.00021	-0.00054***	-0.00040
	(0.00012)	(0.00015)	(0.00024)	(0.00016)	(0.00021)	(0.00026)
Large retailer gap X 1(Post)	-0.00088***	-0.00089***	-0.00039	0.00016	0.00041	0.00081**
	(0.00018)	(0.00023)	(0.00027)	(0.00025)	(0.00025)	(0.00031)
1(Post) X 1(Flows)	0.00160***	0.00207***	0.00249***	0.00268***	0.00334***	0.00293***
	(0.00014)	(0.00016)	(0.00025)	(0.00022)	(0.00025)	(0.00026)
Obs	5,234,898	2,380,957	953,154	2,456,084	1,090,308	576,735
CZs	410	400	287	423	388	217
Firms	1025	974	880	988	928	829
St. dev. large retailer gap	.057	.071	.095	.05	.059	.075
Mean policy emp. share	0.028	0.039	0.008	0.041	0.051	0.028
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 standardized 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 10: Wage effects on non-policy companies, market gap

Indep. Var.: Market Gap X 1(Post)	All events	Major events	$15 events	15 : $pos.flows$
Dep. Var.:				
Log avg. wage, non-policy	-0.0007***	-0.0007***	0.0001	-0.0003*
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Log avg. wage, policy	0.0061***	0.0053***	0.0096***	0.0147***
	(0.0007)	(0.0012)	(0.0029)	(0.0032)
Obs	9,021,253	4,163,710	1,744,869	728,323
CZs	631	629	380	376
Firms	1053	1023	930	1579
St. dev. market gap	.002	.002	.002	.003
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 standardized market gap measure (see equation 4) interacted with a post-period indicator from regression equation 6 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 and the log average hourly wage at the CZ level for the policy company. 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 11: Effects of large retailer VMWs on smaller firms' wages, large retailers' gap

	All VMWs	Major VMWs	$15 VMWs
Dependent variable: Log average wage, small companies			
Independent Variable:			
Large retailer gap (Std.) × Post	0.0016*	0.0012	-0.0008
	(0.0009)	(0.0010)	(0.0010)
Dependent variable: Log average wage, policy company			
Independent Variable:			
Large retailer gap (Std.) × Post	0.0376***	0.0407***	0.0434***
	(0.0012)	(0.0016)	(0.0018)
Cross-wage elasticity estimated via 2SLS:			
Dependent variable: Log average wage, small companies			
Independent Variable:			
Log average wage, policy company	0.0425*	0.0302	-0.0187
	(0.0237)	(0.0246)	(0.0223)
Observations	2,053,936	844,942	427,531
CZs	443	402	312
Number of Firms	19,892	17,487	13,716
SD Large Retailer Gap (unstd.)	0.0517	0.0649	0.0816
Firm-FEs	Y	Y	Y
CZ-FEs	Y	Y	Y
Event time-FEs	Y	Y	Y

Notes: This table reports VMW spillover wage effects on smaller firms estimated on a stacked dataset of voluntary minimum wage events. Column 1 reports the effects for all events, column 2 for major events (those affecting at least 30% of the workforce), column 3 for $15 VMW events. Each row of estimates and standard errors comes from a separate regression with the following sequence of dependent variables and independent variables: the log average hourly wage at smaller firms on the large retailer's gap interacted with post; the log average hourly wage at the CZ-level for the policy company on the large retailer's gap interacted with post; and the log average hourly wage at smaller firms on log policy wage, where log policy wage is instrumented for using the gap measure interacted with post. The gap measure (see equation 1 for a definition) is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$) and standardized. Company, CZ, and month fixed effects are included. The sample is restricted to CZs where the large retailer has at least 30 employees per month and to small firms appearing at least three times pre- and three times post- the large retailer VMW event, with at least 5 employees in one of the months in 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*: Payroll Provider.

Table 12: Effects of large retailer VMWs on smaller firms' wages, market gap

	All VMWs	Major VMWs	$15 VMWs
Dependent variable: Log average wage, small companies			
Independent Variable:			
Market gap (Std.) × Post	0.0016**	0.0022**	0.0002
	(0.0007)	(0.0009)	(0.0009)
Dependent variable: Log average wage, policy company			
Independent Variable:			
Market gap (Std.) × Post	0.0087***	0.0078***	0.0120***
	(0.0016)	(0.0020)	(0.0036)
Observations	2,053,936	844,942	427,531
CZs	443	402	312
Number of Firms	19,892	17,487	13,716
SD Market Gap. (unstd.)	0.0012	0.0015	0.0018
Firm-FEs	Y	Y	Y
CZ-FEs	Y	Y	Y
Event time-FEs	Y	Y	Y

Notes: This table reports VMW spillover wage effects on smaller firms using an alternative measure of exposure to VMW events: the market-level gap (see equation 4 for the definition). Column 1 reports the effects for all events, column 2 for major events (those affecting at least 30% of the workforce), column 3 for $15 VMW events. Each row of estimates and standard errors comes from a separate regression with the following sequence of dependent variables and independent variables: the log average hourly wage at smaller firms on the market gap interacted with post and the log average hourly wage at the CZ-level for the policy company on the market gap interacted with post. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$) and standardized. Company, CZ, and month fixed effects are included. The sample is restricted to CZs where the large retailer has at least 30 employees per month and to small firms appearing at least three times pre- and three times post- the large retailer VMW event, with at least 5 employees in one of the months in 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*: Payroll Provider.

Table 13: Effects of VMWs on CZ-level average quarterly earnings

	Dep Var: Log 1st quarter average earnings		
Independent variables:	Policy	All	All minus policy
Market Gap Proxy X 1(Post)	0.0046***	0.0001	-0.0006
	(0.0013)	(0.0008)	(0.0008)
Obs	13,300	13,300	13,300
CZs	631	631	631
Mean Market Gap Proxy	.0001	.0001	.0001
St. Dev. Market Gap Proxy	.0002	.0002	.0002
Dep Var Mean	8.23	9.28	9.28
CZ FEs	Y	Y	Y
Month from event FEs	Y	Y	Y

Notes: This table reports the estimated coefficients on a proxy for market gap interacted with a post-period indicator in a regression of first-quarter log average quarterly wages in the first quarter before and the first quarter after a large retailer VMW event, stacking all events. We proxy for the market gap by multiplying the large retailer's own-gap measure by the company's wage bill share in the local CZ in the first quarter before the event. We standardize this variable across CZs. Each column of the table represents a separate regression with the following sequence of dependent variables: first-quarter log average quarterly wages for the policy company, first-quarter log average quarterly wages in the CZ overall, and first-quarter log average quarterly wages for all establishments net of the policy company 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 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:* First-quarter average quarterly earnings for large retailers from Large credit bureau; CZ-level first-quarter average quarterly earnings overall from Quarterly Census of Employment and Wages.

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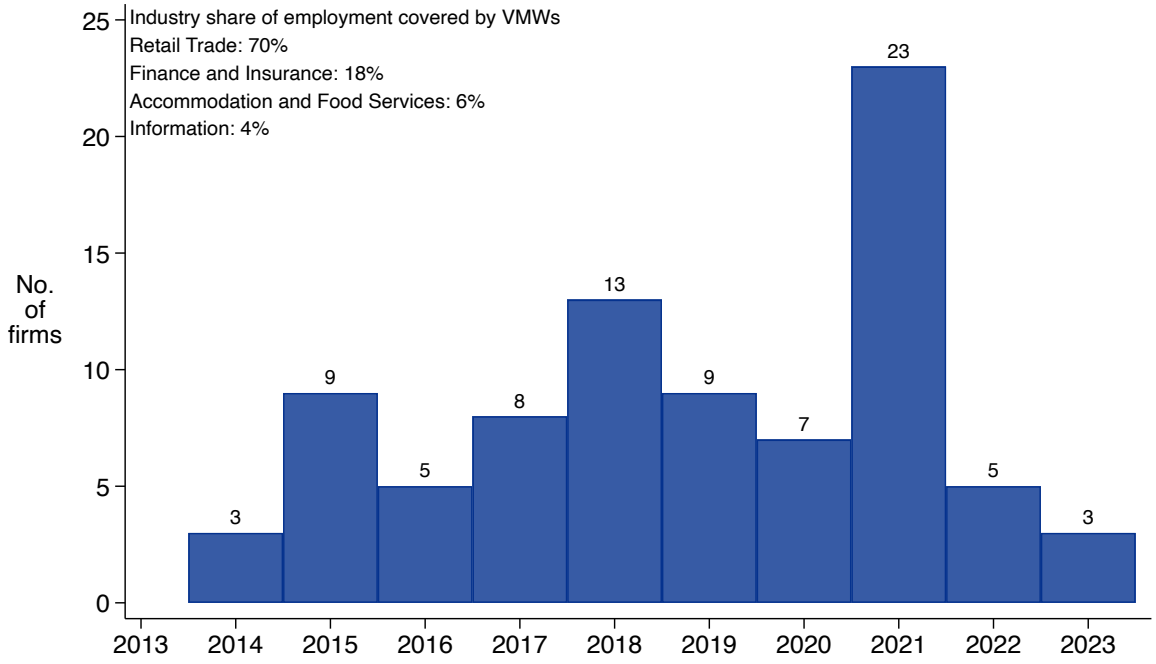
A Voluntary employer minimum wages: additional descriptives

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.

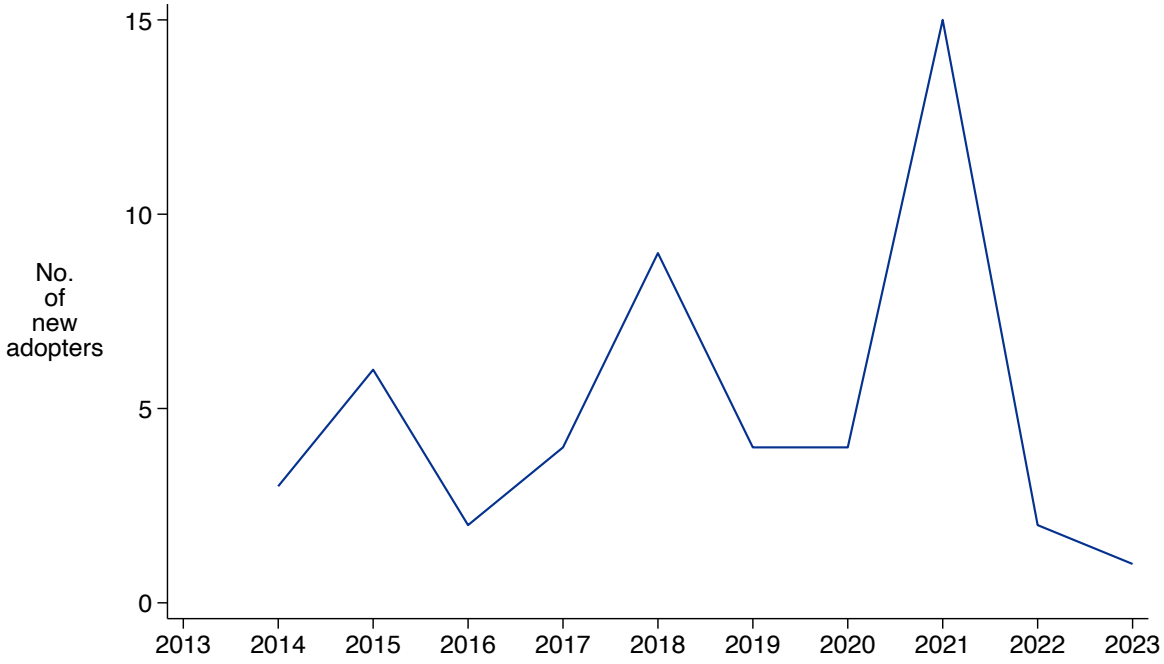
Voluntary minimum wages not studied. According to the database, 16 companies with employment greater than 150,000 adopted a voluntary company minimum wage. We were unable to find a match for 11 of these companies in the database. However, we do find a match for 4 out of the 5 largest employers with voluntary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

Time from event	Share w/ valid earnings	
	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

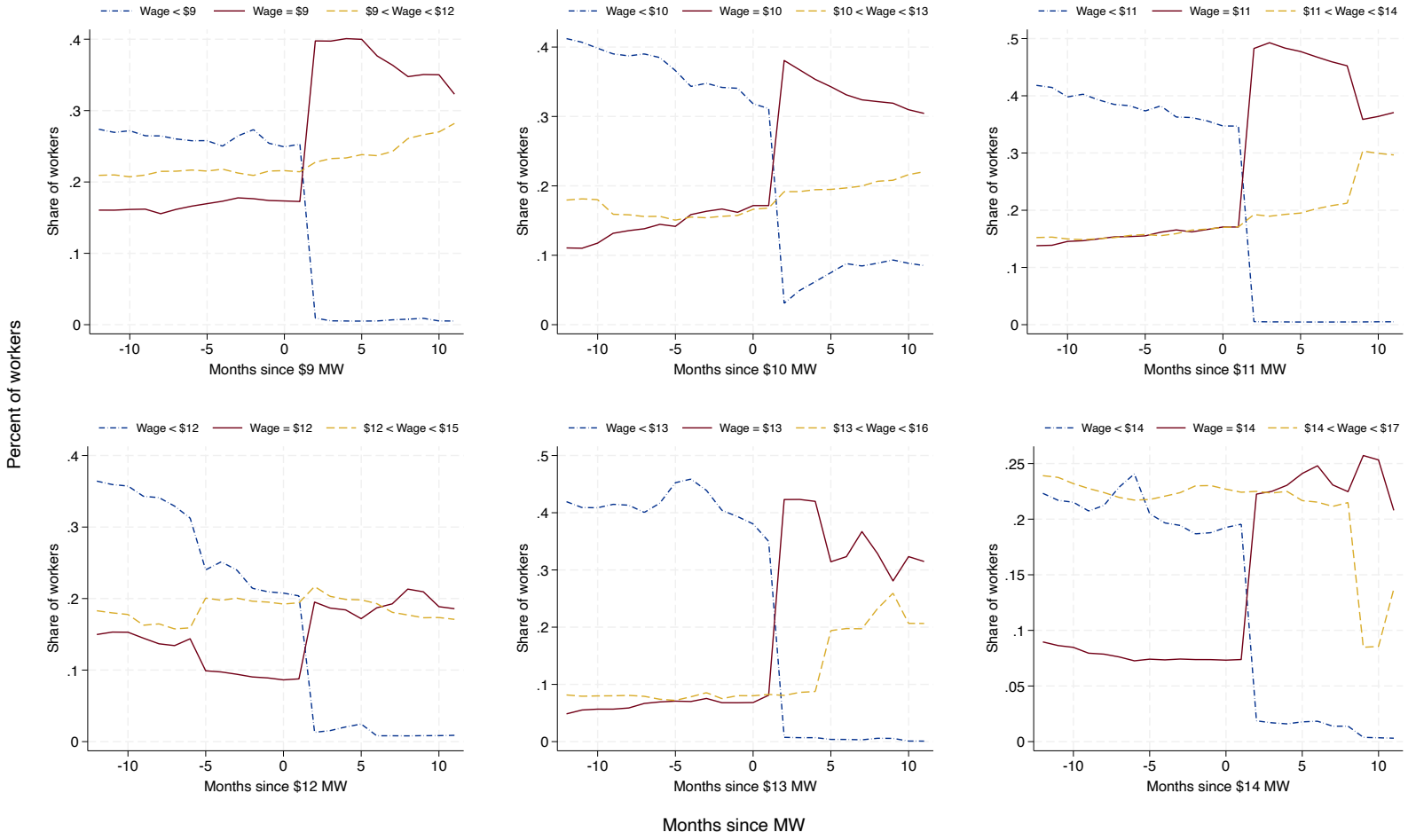
Notes: Share of employees with valid wage data who have valid earnings data with imputed variables listed by variable category. Running monthly earnings are year-to-date total earnings. Current monthly earnings is year-to-date total earnings minus last month's year-to-date total earnings for employees 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

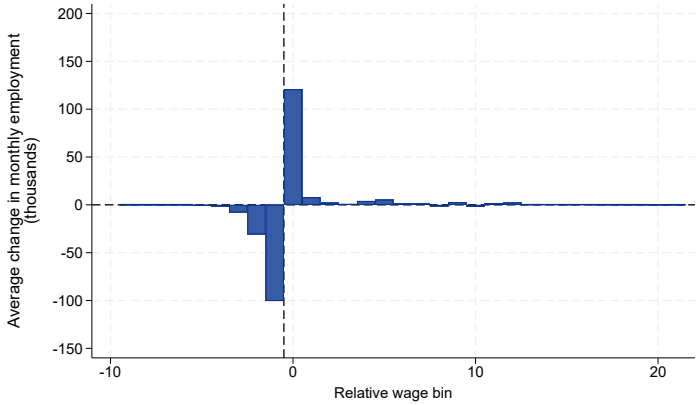
Notes: Share of employees with valid wage data who have valid earnings data with imputed variables listed by variable category. Running monthly earnings are year-to-date total earnings. Current monthly earnings is year-to-date total earnings minus last month's year-to-date total earnings for employees 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

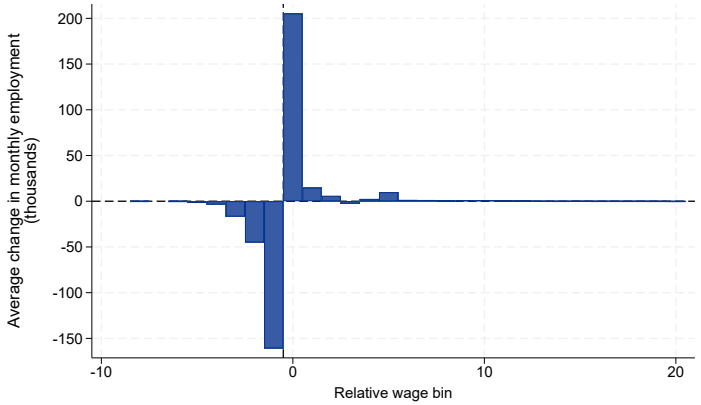


Notes: For each voluntary minimum wage level, the figure shows the share of hourly workers below the VMW, in the wage bin of the VMW, and in the two wage bins above the VMW at large retailers with voluntary minimum wage events, before and after the policy's implementation. *Data sources*: Large credit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

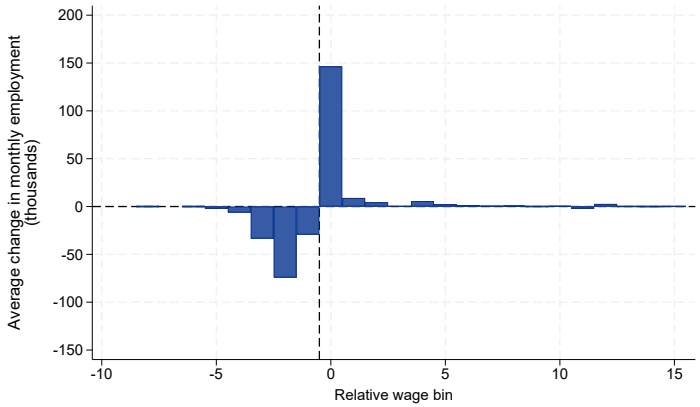
Figure B2: Difference in employment by wage bin at policy companies



(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.

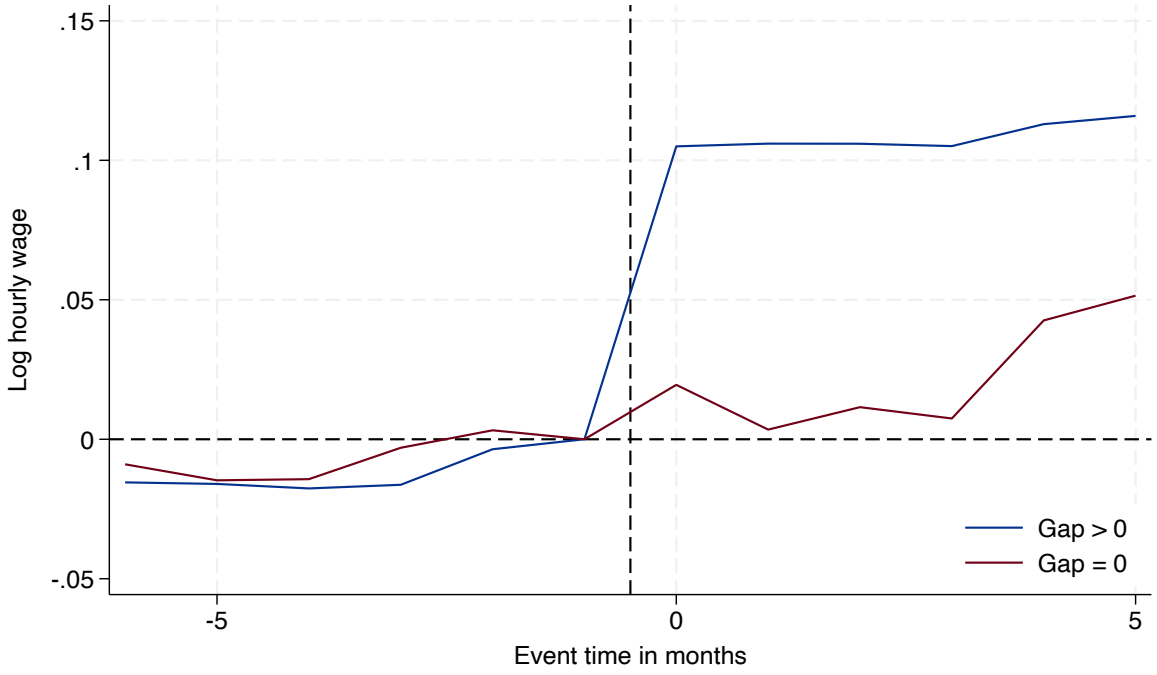
Table B8: Own-gap distribution across events

Experiment	Mean	Std. Dev.	25th	Median	75th	90th
1	0.032	0.020	0.017	0.032	0.048	0.057
2	0.030	0.012	0.024	0.032	0.039	0.043
3	0.031	0.016	0.022	0.036	0.042	0.046
4	0.017	0.017	0.000	0.014	0.032	0.044
5	0.031	0.016	0.021	0.027	0.035	0.055
6	0.013	0.005	0.010	0.012	0.016	0.021
7	0.018	0.007	0.014	0.016	0.021	0.025
8	0.012	0.005	0.010	0.011	0.013	0.016
9	0.009	0.004	0.006	0.008	0.010	0.012
10	0.047	0.033	0.002	0.063	0.075	0.082
11	0.032	0.032	0.001	0.017	0.066	0.072
12	0.075	0.054	0.011	0.072	0.130	0.140
13	0.117	0.069	0.052	0.155	0.174	0.183
14	0.049	0.035	0.023	0.046	0.075	0.098
15	0.042	0.026	0.020	0.046	0.064	0.072
16	0.035	0.017	0.024	0.038	0.048	0.054
17	0.055	0.020	0.049	0.063	0.068	0.072
18	0.057	0.018	0.056	0.064	0.067	0.070
19	0.111	0.031	0.113	0.123	0.127	0.131
20	0.211	0.101	0.137	0.212	0.290	0.353

Notes: Distribution of the own-gap measure across CZs by VMW event. Column 1 shows the mean, column 2 the standard deviation, and columns 3, 4, 5, and 6 show the 25th percentile, the median, the 75th percentile, and the 90th percentile. *Data source:* Large credit bureau.

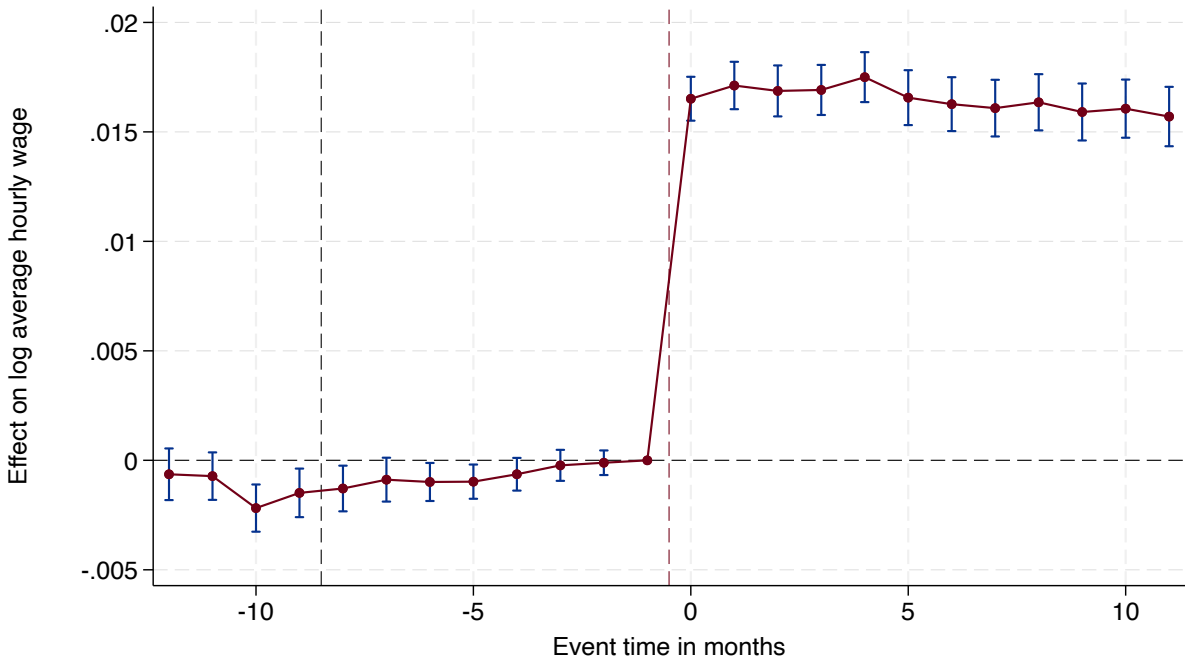
C Additional results and robustness checks for policy company analysis

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



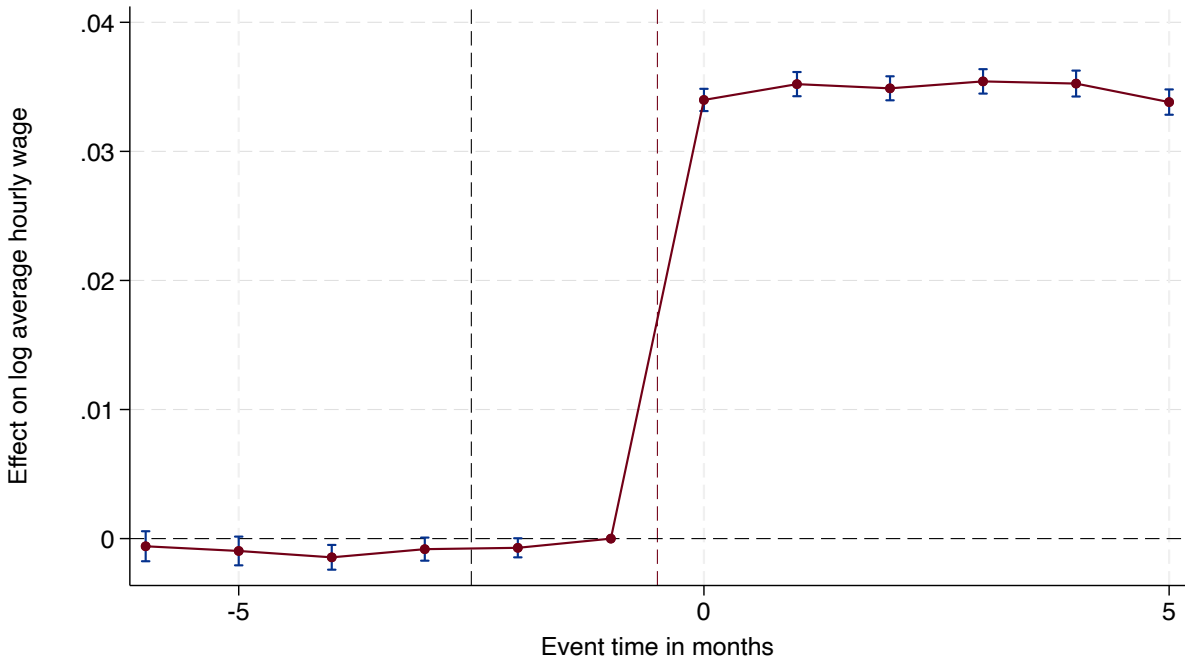
Notes: This figure plots log average wages at the CZ-level for policy companies with a $15 VMW for two groups of CZs: those with a positive gap and those with a gap of zero (see equation 1 for the definition of the gap measure). Log average wages are normalized to their $t = -1$ values. The sample is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

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



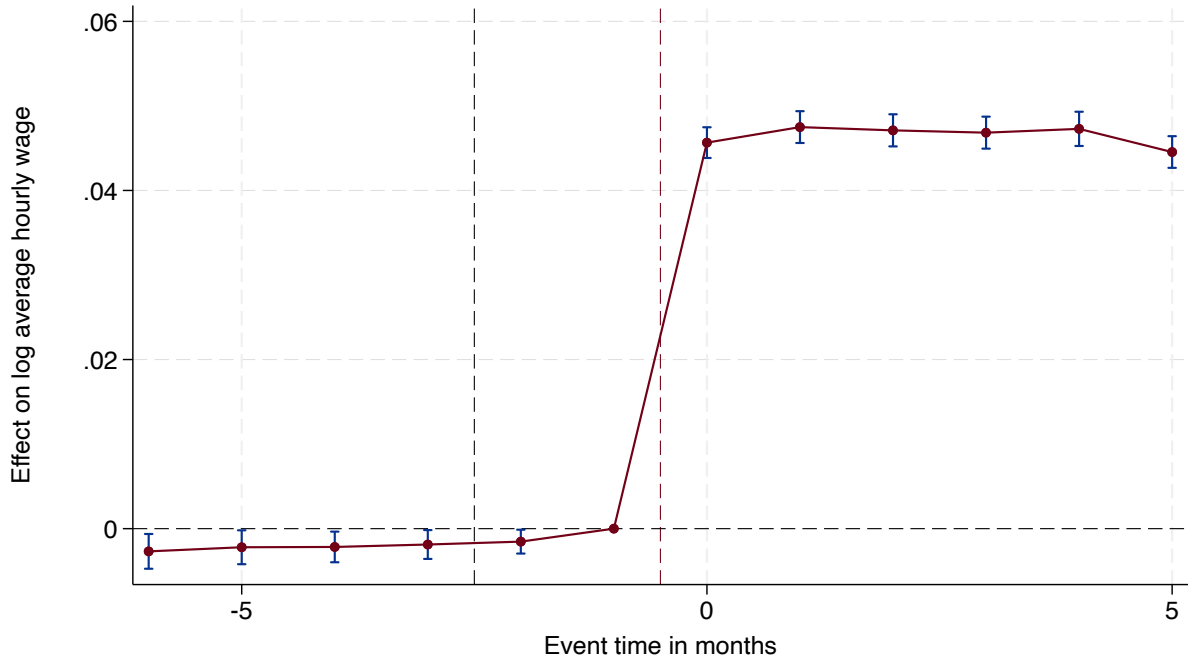
Notes: This figure plots the regression coefficients on the standardized gap in each commuting zone (CZ) between binned wages of employees at large retailers with a voluntary $15 minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects over a 24-month event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

Figure C3: Effect of large retailer VMWs on own wages



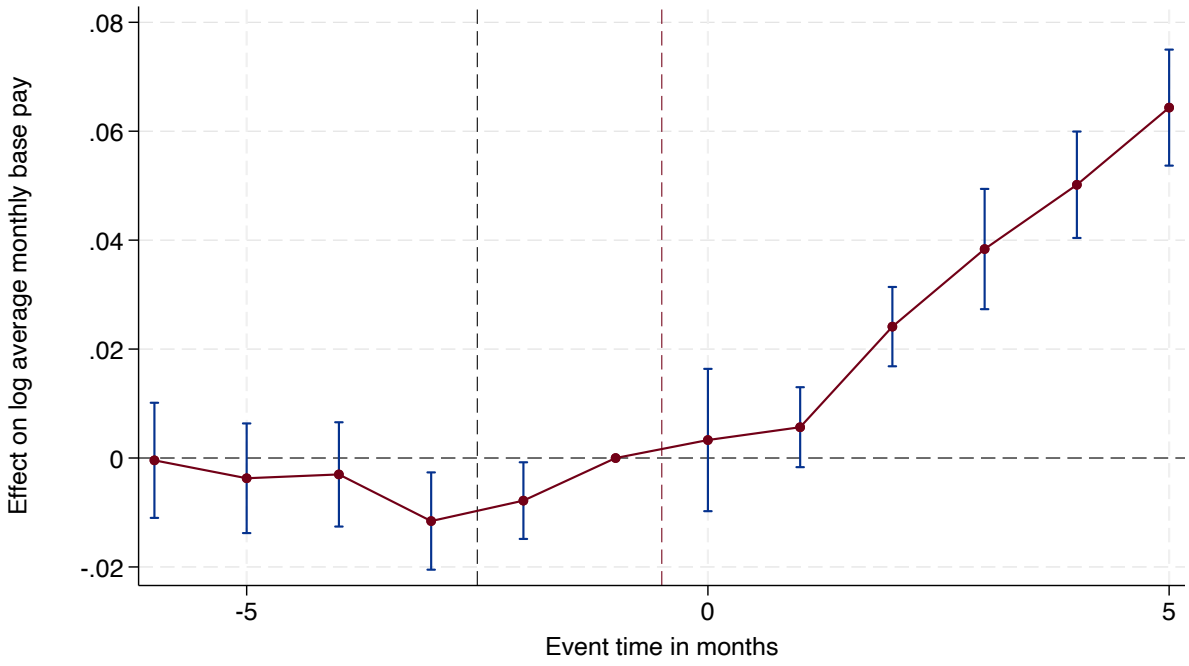
Notes: This figure plots the regression coefficients on the standardized gap in each commuting zone (CZ) between employees' wage bins and the company minimum wage among large retailers with a voluntary minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed effects. 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 are shown. *Data sources*: Large credit bureau.

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



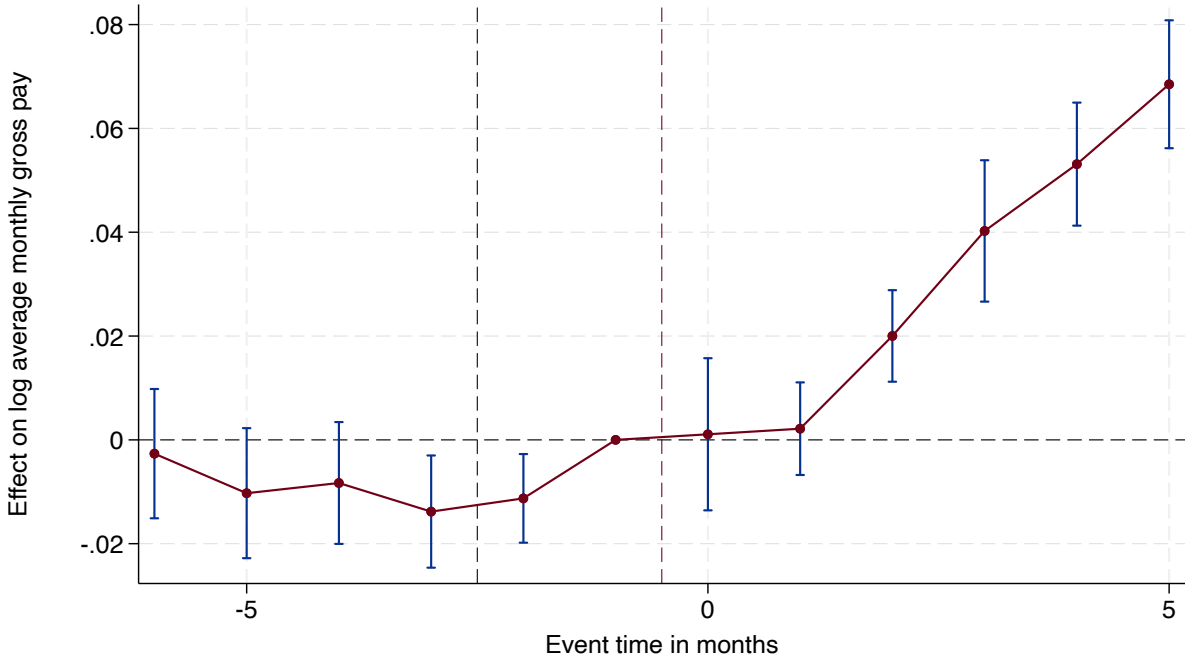
Notes: This figure plots the regression coefficients on the standardized gap in each commuting zone (CZ) between employees' wage bins and the company minimum wage among large retailers with a major voluntary minimum wage (see equation 1 for the definition of the gap measure) interacted with month fixed 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 are shown. *Data sources*: Large credit bureau.

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



Notes: This figure plots the regression coefficients on the standardized 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 are shown. *Data sources*: Large credit bureau.

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



Notes: This figure plots the regression coefficients on the standardized 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 are shown. *Data sources*: Large credit bureau.

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

Indep. Var.: Large Retailer Gap X 1(Post)	All events	Major events	$15 events
Dep. Var.:			
Log avg. wage	0.035***	0.047***	0.067***
	(0.000)	(0.001)	(0.002)
Log avg. monthly base pay	0.030***	0.046***	0.064***
	(0.001)	(0.003)	(0.005)
Log avg. base pay (YTD)	0.018***	0.026***	0.036***
	(0.001)	(0.003)	(0.004)
Log avg. gross pay (YTD)	0.020***	0.029***	0.039***
	(0.002)	(0.003)	(0.005)
Obs	71,846	33,566	10,970
CZs	632	631	380
Events	20	8	4
St. dev. gap	.05	.06	.08
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 standardized 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 monthly 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.

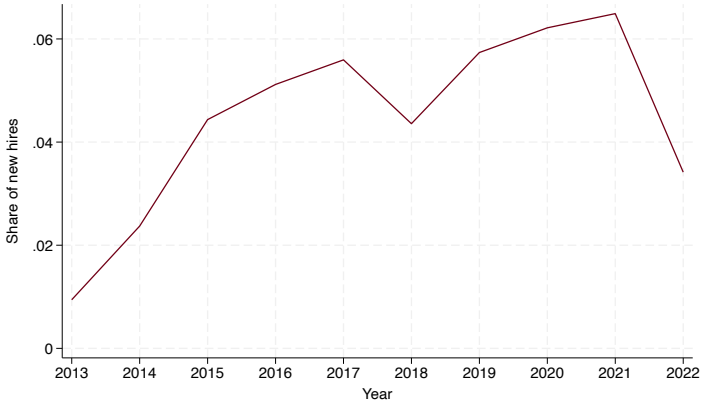
Table C2: Large retailer VMW effects on own wages and employment, including workers paid $30 or more

Indep. Var.: Large Retailer Gap X 1(Post)	All events	Major events	$15 events
Dep. Var.:			
Log avg. wage	0.035***	0.047***	0.068***
	(0.000)	(0.001)	(0.002)
Log employment	0.016***	0.027**	0.045**
	(0.006)	(0.012)	(0.019)
Separation rate	-0.004***	-0.005***	-0.007***
	(0.000)	(0.001)	(0.001)
Separation rate to non-policy firms	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Log new hires	-0.026**	0.009	-0.014
	(0.010)	(0.019)	(0.030)
Total employment elasticity	0.456***	0.565**	0.663**
	(0.167)	(0.254)	(0.285)
	[0.127,0.784]	[0.066,1.063]	[0.102,1.224]
Obs	63,945	30,548	9,240
CZs	632	631	380
Events	20	8	4
St. dev. gap	.05	.06	.08
Pre-period mean:			
Sep rate	.064	.065	.072
Sep rate to non-policy firms	.012	.014	.014
Log new hires	3.11	3.11	2.74
Month from event FEs	Y	Y	Y
CZ FEs	Y	Y	Y

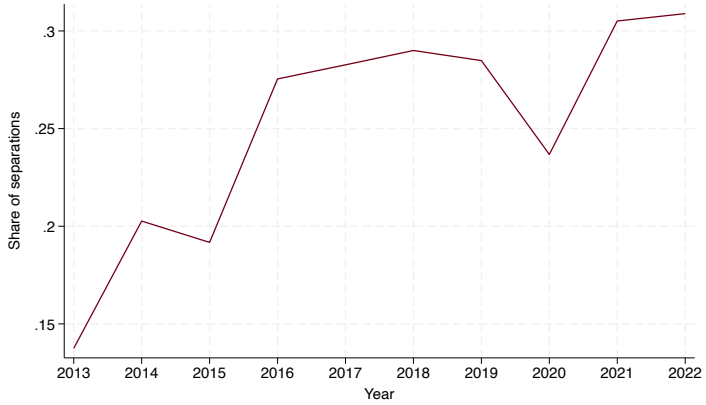
Notes: This table reports the estimated coefficients on the standardized gap measure interacted with a post-period indicator from regression equation 3 on a stacked policy company dataset of voluntary minimum wage events. Each row of the table represents a separate regression with the following sequence 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, 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

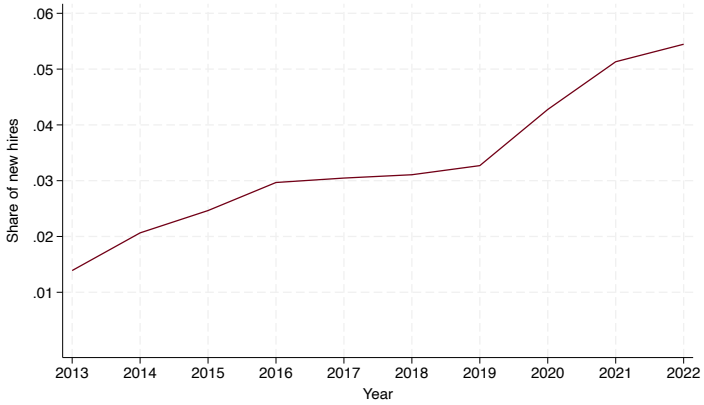
Figure D1: Share of hiring and separations from and to other employers in the database



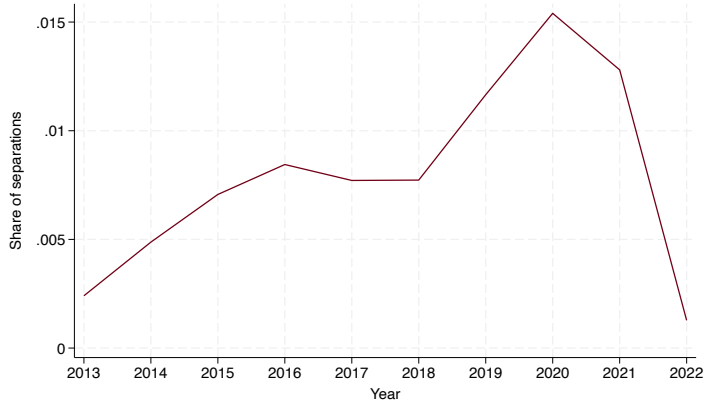
(a) Fraction of policy new hires from non-policy



(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 annual fraction of all new hires at policy companies that come from non-policy companies in the database (panel (a)); the annual fraction of all employees that separate from a policy company that flow to another company in the database (panel (b)); the annual fraction of all non-policy new hires that come from policy companies (panel (c)); and the annual 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 D1: 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	0.12	0.25
5	0.13	0.27
6	0.14	0.28
7	0.14	0.28
8	0.15	0.29
9	0.14	0.28
10	0.14	0.26
11	0.15	0.29
12	0.14	0.28
13	0.13	0.26
14	0.12	0.26
15	0.13	0.26
16	0.14	0.27
17	0.14	0.27
18	0.15	0.28
19	0.15	0.29
20	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.

Table D2: Correlation between different exposure measures

Experiment (% of local emp.)	Own gap – Emp. share	Own gap – Market gap	Market gap – Emp. share
1 (1.66)	0.29	0.74	0.58
2 (1.72)	0.15	0.58	0.58
3 (1.69)	0.15	0.62	0.52
4 (1.70)	0.40	0.82	0.60
5 (0.16)	-0.40	-0.07	0.80
6 (0.15)	-0.25	-0.01	0.83
7 (0.16)	-0.24	0.07	0.82
8 (0.18)	-0.27	0.07	0.76
9 (0.16)	-0.12	0.28	0.78
10 (0.09)	-0.45	0.72	-0.09
11 (0.11)	-0.32	0.80	-0.08
12 (0.13)	-0.37	0.76	-0.04
13 (0.14)	-0.38	0.70	0.05
14 (0.23)	-0.37	0.65	0.15
15 (0.23)	-0.45	0.63	0.07
16 (0.20)	-0.38	0.49	0.24
17 (0.24)	-0.43	0.37	0.30
18 (0.25)	-0.49	0.29	0.29
19 (0.24)	-0.53	0.20	0.33
20 (0.33)	-0.20	0.01	0.91

Notes: Correlation between various exposure measures for spillovers analysis. Column 1 shows the correlation between a policy company's own gap at the CZ-level and the company's share of total employment where total employment is measured using the Quarterly Census of Employment and Wages (QCEW) in sample CZs. The numerator for the employment share variable is limited to hourly wage workers at in-sample establishments in large credit bureau data. Column 2 shows the correlation between a policy company's own gap and the market-level gap, defined as the percent change in the employment-weighted wage implied by policy companies complying with their voluntary minimum. Column 3 shows the correlation between the market-level gap and the policy company's employment share as defined above. *Data source:* Quarterly Census of Employment and Wages; Large credit bureau.

Table D3: 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

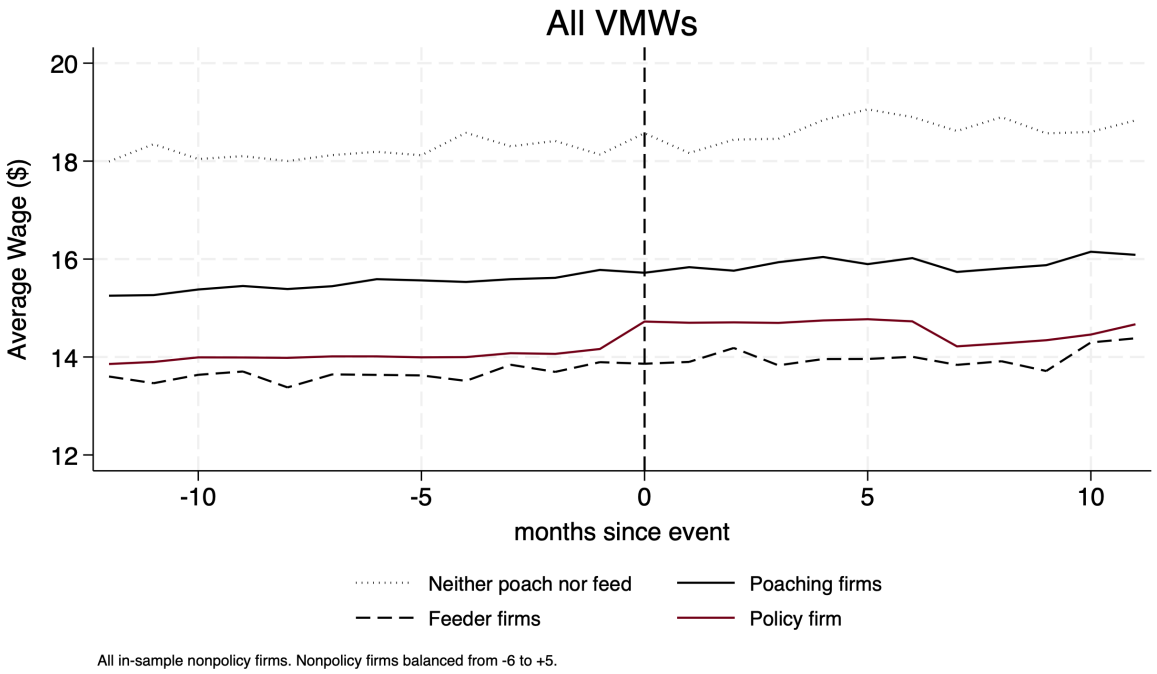
Notes: Industry unknown for 6% of separations and 2% of new hires. Data sources: Large credit bureau.

Table D4: 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	.66 (0.002)	.6 (0.002)	.81 (0.007)	.77 (0.007)
2	.65 (0.002)	.56 (0.002)	.72 (0.007)	.64 (0.007)
3	.67 (0.002)	.49 (0.002)	.77 (0.006)	.52 (0.005)
5	.46 (0.002)	.19 (0.001)	.15 (0.006)	4.0e-02 (0.002)
6	.68 (0.008)	.58 (0.006)	.57 (0.023)	.52 (0.019)
7	.62 (0.008)	.46 (0.005)	.58 (0.022)	.41 (0.014)
8	.59 (0.008)	.41 (0.004)	.57 (0.023)	.37 (0.013)
9	.57 (0.009)	.3 (0.004)	.37 (0.016)	.21 (0.009)
10	.5 (0.008)	.26 (0.003)	.38 (0.019)	.16 (0.008)
11	.7 (0.005)	.6 (0.004)	.64 (0.018)	.54 (0.014)
12	.6 (0.005)	.37 (0.003)	.51 (0.015)	.29 (0.008)
13	.56 (0.005)	.32 (0.003)	.4 (0.012)	.21 (0.006)
14	.43 (0.005)	.22 (0.002)	.16 (0.010)	6.7e-02 (0.004)
17	.73 (0.003)	.67 (0.003)	.73 (0.012)	.7 (0.011)
18	.73 (0.003)	.62 (0.003)	.7 (0.011)	.6 (0.010)
19	.72 (0.003)	.55 (0.002)	.76 (0.009)	.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)	.64 (0.009)	.38 (0.006)
23	.73 (0.003)	.61 (0.003)	.81 (0.008)	.65 (0.007)

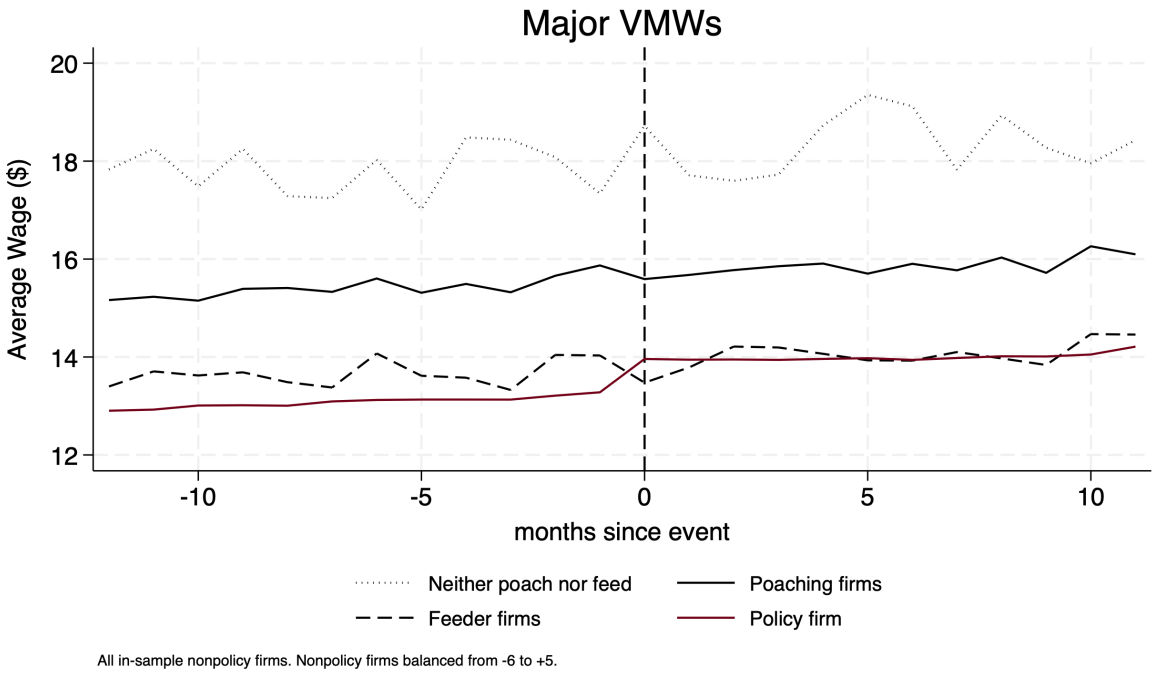
Notes: Using establishment-level data, Columns 1 and 2 report the coefficient estimate when regressing an indicator for employee flows from the policy firm to the establishment prior to the minimum wage event on an indicator for employee flows from the policy firm to the establishment following the minimum 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.

Figure D2: VMW large retailer wages vs. competitor wages



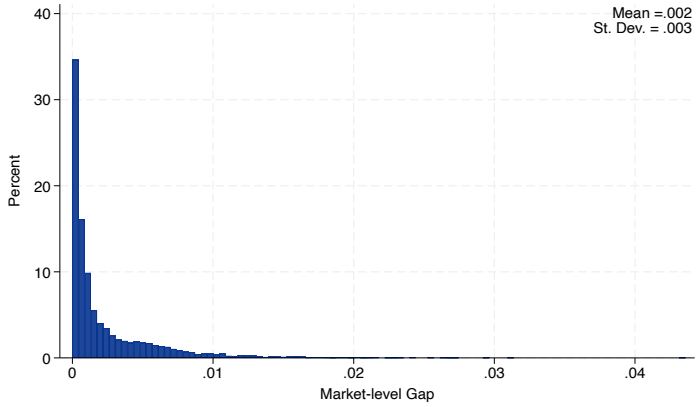
Notes: This figure plots the average hourly wages at large retailers vs. their competitors over a 12-month window around all VMW events. *Data sources*: Large credit bureau.

Figure D3: Major VMW large retailer wages vs. competitor wages

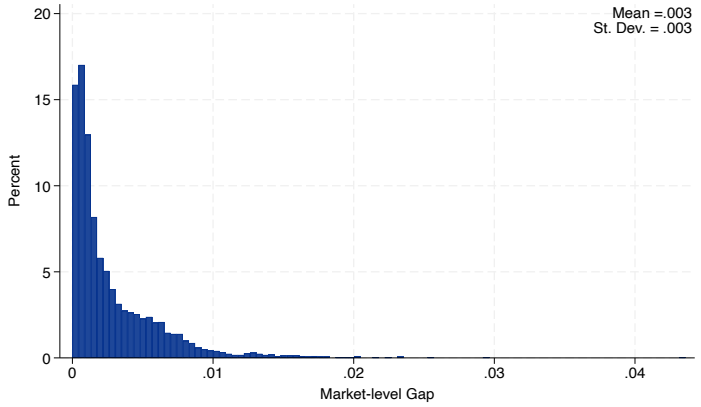


Notes: This figure plots the average hourly wages at large retailers vs. their competitors over a 12-month window around major VMW events. *Data sources*: Large credit bureau.

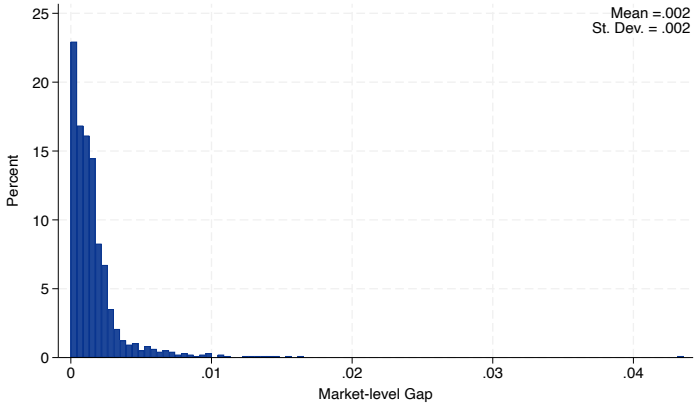
Figure D4: Distribution of market-gap measure across large retailer CZs for all, major, and $15 VMWs



(a) All VMWs



(b) Major VMWs



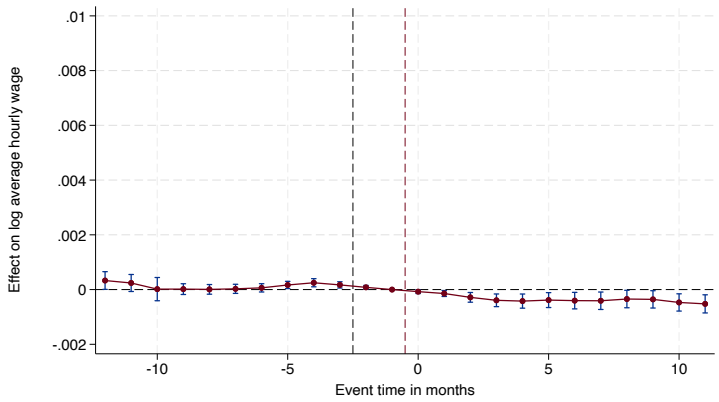
(c) $15 VMWs

Notes: This figure plots the distribution of the market-gap measure across large retailer CZs for all VMW events, major events, and $15 events (see equation 4 for the definition of the own-gap measure). The mean and standard deviation of the gap measure across all CZs in the included events is reported as well. *Data sources*: Large credit bureau.

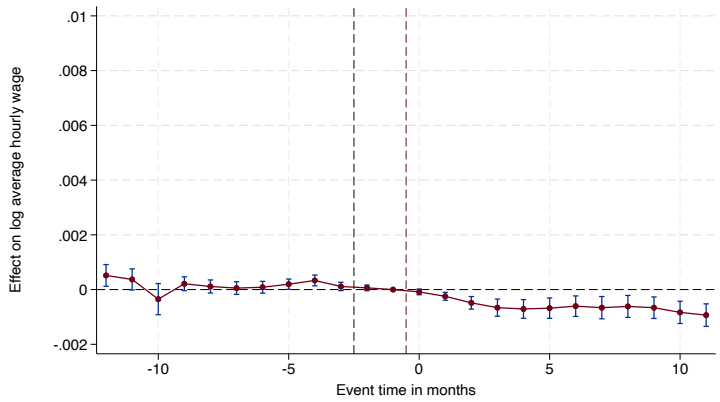
D.1 Spillover effects over longer time horizon

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

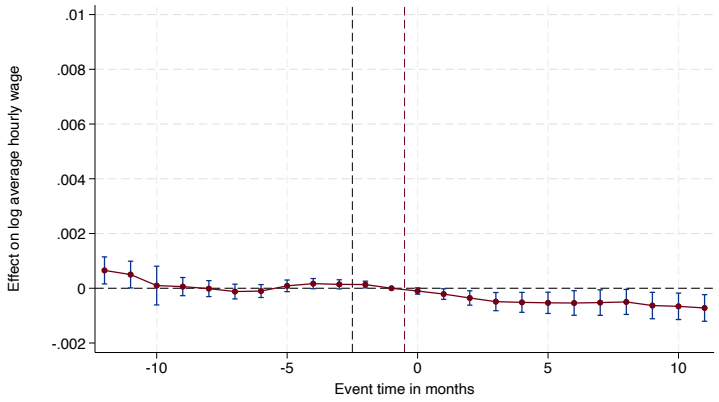
Figure D5: Effect of large retailer $15 VMW on other firms' wages, 12-month post-period, large retailer gap



(a) All establishments



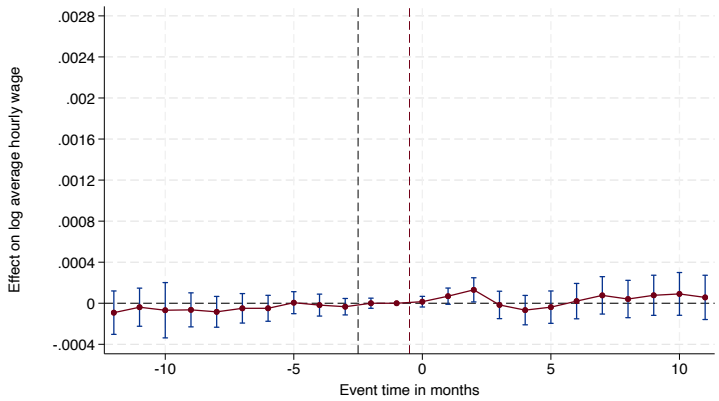
(b) Establishments in industries connected by worker flows



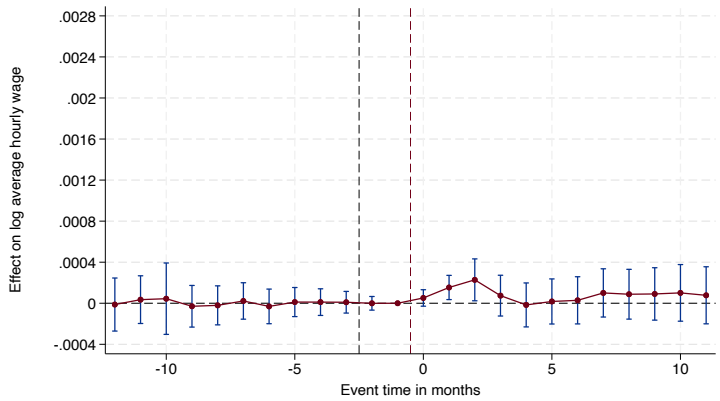
(c) All establishments connected by worker flows

Notes: This figure plots the regression coefficients on the standardized large retailer's 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 are shown. *Data sources*: Large credit bureau.

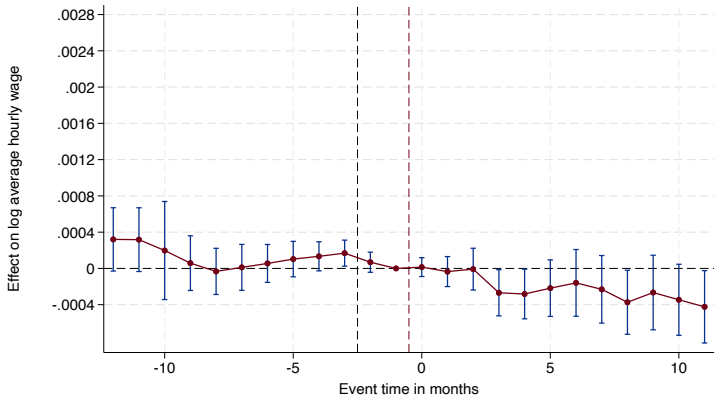
Figure D6: Effect of large retailer $15 VMW on other firms' wages, 12-month post-period, market gap



(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 standardized market gap in each commuting zone (CZ) between binned wages of employees at large retailers with a voluntary $15 minimum wage over a 12-month pre- and post-period (see equation 4 for the definition of the gap measure) interacted with month fixed effects. The dependent variable is log average hourly wage of non-policy employees in that CZ. Panel (a) includes all non-policy establishments in the CZ; Panel (b) restricts the sample to non-policy establishments in industries connected to the large retailer by worker flows; and Panel (c) restricts the sample non-policy establishments with a history of hiring workers from or having workers separate to the large retailer in the year before the policy is adopted. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($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 are shown. *Data sources*: Large credit bureau.

D.2 Spillover effects on new hire wages

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

Table D5: Spillover effects on new hire wages, large retailer gap

Indep. Var.: Large Retailer Gap X 1(Post)	All events	Major events	$15 events	$15: pos. flows
Dep. Var.:				
Log avg. wage, non-policy	-0.002***	-0.003***	-0.002***	-0.001
	(0.000)	(0.000)	(0.001)	(0.001)
Obs	7,494,306	3,445,815	1,425,788	677,266
CZs	631	629	380	376
Firms	1037	1009	920	1580
St. dev. large retailer gap	.055	.067	.088	.082
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 standardized large retailer's gap measure (see equation 1 for a definition) interacted with a post-period indicator from regression equation 6 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.

Table D6: Spillover effects, heterogeneity by flows, market gap

	Dep Var: Log average wage, non-policy		
Independent variables	All events	Major events	$15 events
Large retailer gap X 1(Post) X 1(Flows)	-0.00038***	-0.00053***	-0.00019
	(0.00009)	(0.00012)	(0.00017)
Large retailer gap X 1(Post)	-0.00056***	-0.00049***	-0.00004
	(0.00008)	(0.00010)	(0.00016)
1(Post) X 1(Flows)	0.00217***	0.00263***	0.00285***
	(0.00015)	(0.00016)	(0.00017)
Obs	9,040,130	4,170,109	1,744,869
CZs	631	629	380
Firms	1053	1023	930
St. dev. large retailer gap	.002	.002	.002
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 standardized market gap measure, a post-period indicator, and an indicator for a history of employment flows with the policy company (see equation 4 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 D7: Spillover effects on new hire wages, market gap

Indep. Var.: Market Gap X 1(Post)	All events	Major events	$15 events	$15: pos. flows
Dep. Var.:				
Log avg. wage, non-policy	-0.0019***	-0.0021***	-0.0002	-0.0022***
	(0.000)	(0.000)	(0.001)	(0.001)
Obs	7,494,306	3,445,815	1,425,788	677,266
CZs	631	629	380	376
Firms	1037	1009	920	1580
St. dev. market gap	.002	.002	.002	.003
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 standardized market gap measure interacted with a post-period indicator from regression equation 6 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 $15 events, restricting to non-policy establishments with a history of hiring from the policy company. See equation 4 for a definition of the market gap measure. The measure is averaged over months -6 to -3 relative to the policy implementation date ($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.3 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:

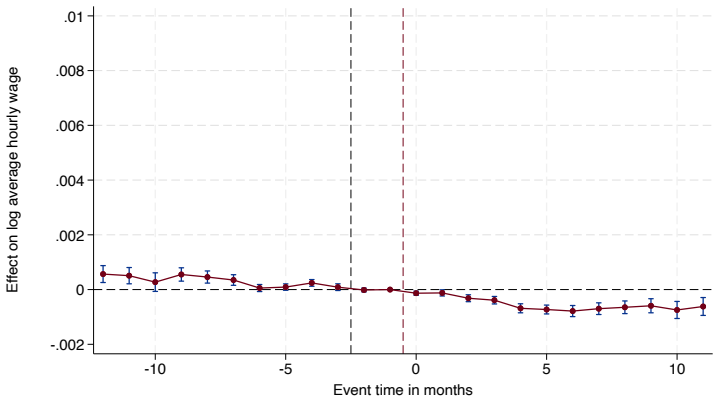
$$\log w_{f,c,t} = \iota + \sum_{k=-6}^{5} \phi_k \overline{GAP}_{f,CZ} \times \mathbb{1}_{[t=k]} + \xi_c + \zeta_t + e_{c,t}, \tag{11}$$

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.

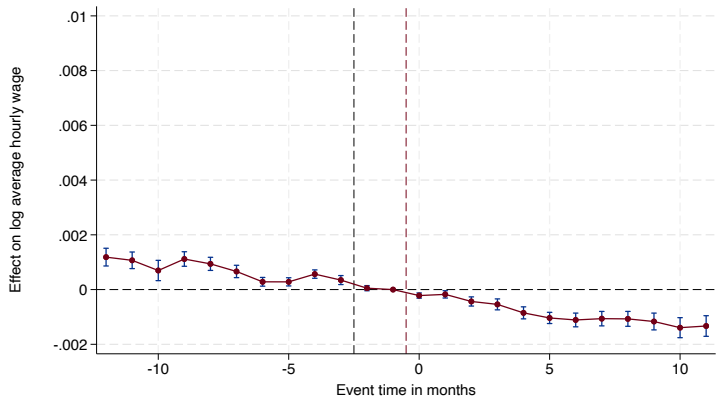
The coefficient of interest is ϕ_k which plots the relationship between the non-policy establishment's gap and log average establishment wages k months from the policy company's voluntary minimum wage event. Once again, we measure the gap over months -6 to -3 in order to build in a placebo-in-time test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

Appendix Figure D7 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 Equation 11. Appendix Figure D8 shows these results. Once again, we find negligible spillovers using this approach.

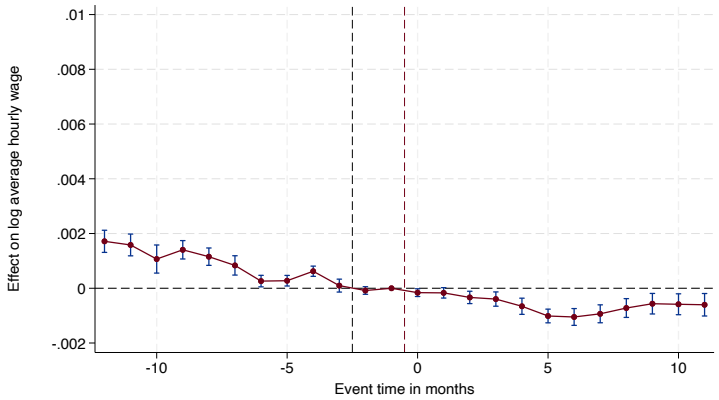
Figure D7: Effect of large retailer $15 VMW on other firms' wages, non-policy gap



(a) All establishments



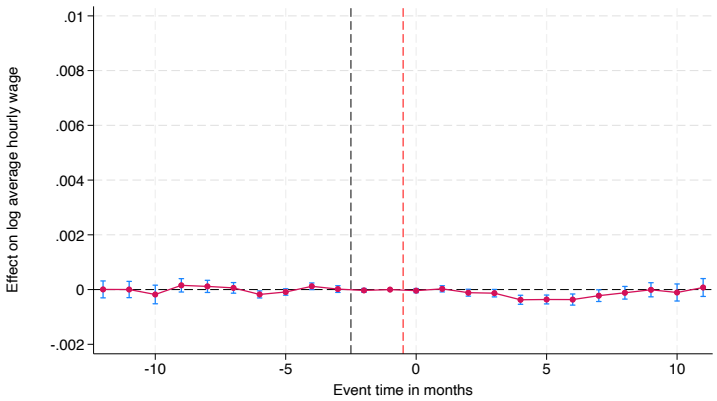
(b) Establishments in industries connected by worker flows



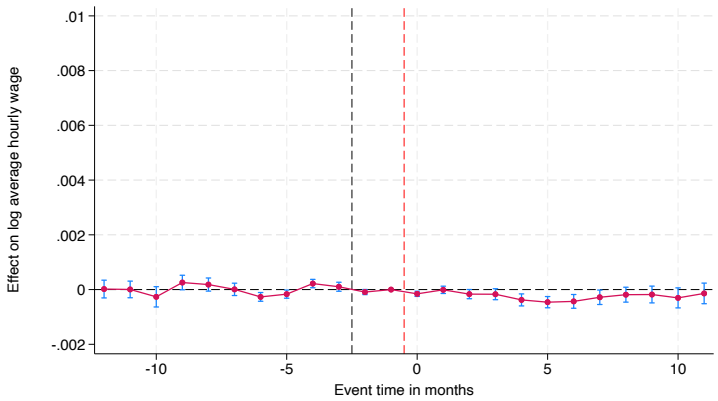
(c) All establishments connected by worker flows

Notes: This figure plots the regression coefficients on the standardized 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 are shown. *Data sources*: Large credit bureau.

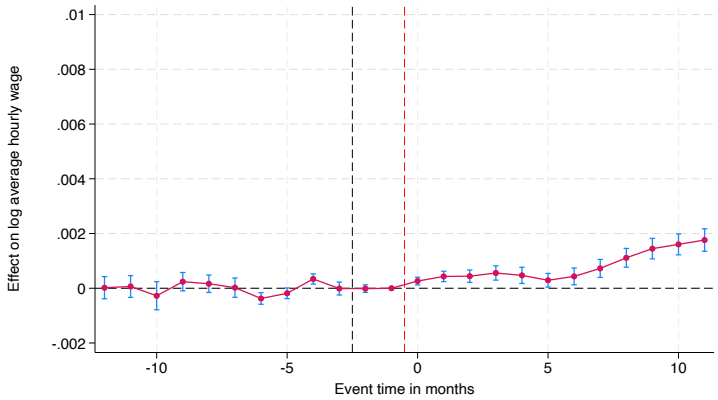
Figure D8: Effect of large retailer $15 VMW on other firms' wages, non-policy gap, detrended



(a) All establishments



(b) Establishments in industries connected by worker flows

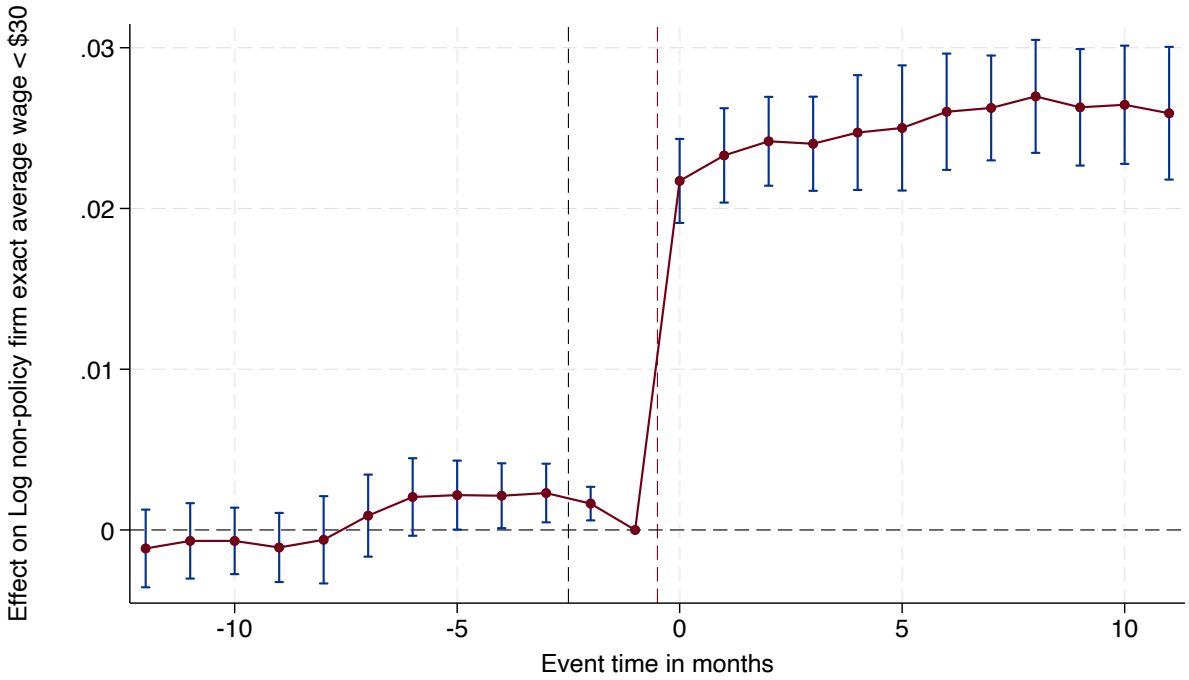


(c) All establishments connected by worker flows

Notes: This figure plots detrended regression coefficients on the standardized 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 are shown. *Data sources*: Large credit bureau.

D.3.1 Effect of California $12 minimum wage on non-policy establishments

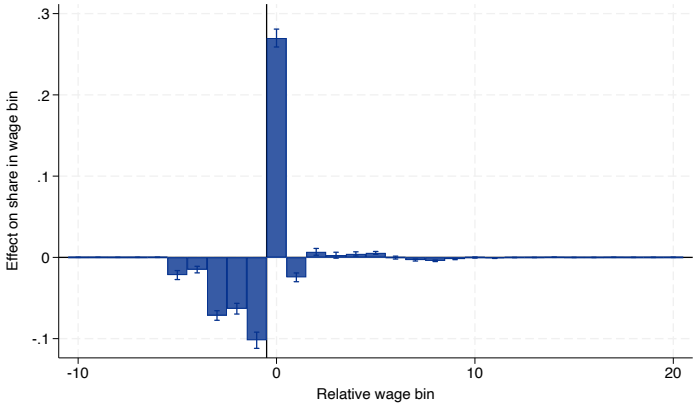
Figure D9: Effect of California's $12 minimum wage on non-policy company wages, non-policy gap



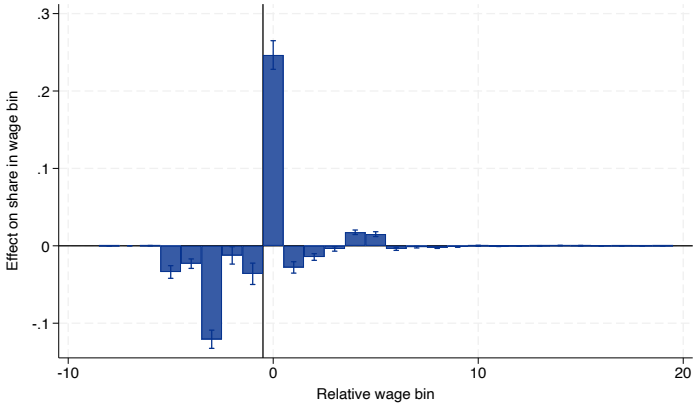
Notes: This figure plots the regression coefficients on the interaction between the standardized gap between each non-policy establishment's hourly wages and $12 and month fixed effects around California's adoption of a $12 minimum wage. The dependent variable is log average hourly wage of non-policy establishments in that CZ. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). Company-by-CZ, company, CZ, and month fixed effects are included. The sample is restricted to commuting zones in California and non-policy establishments with at least 10 employees every month during the event window. 95% confidence intervals are shown. *Data sources*: Large credit bureau.

D.4 Bunching analysis of large retailer VMWs

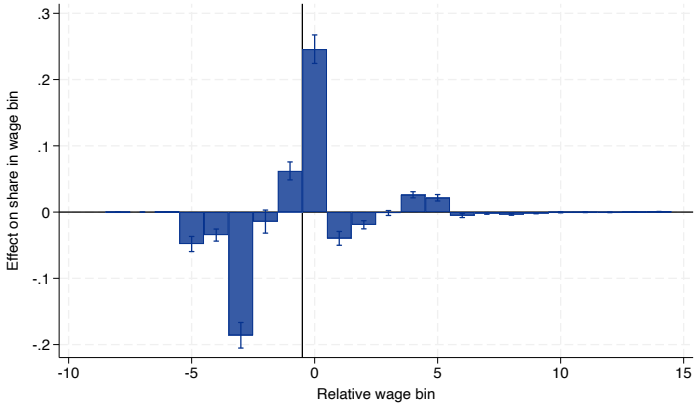
Figure D10: Own wage effect of large retailer VMWs, bunching design



(a) All VMWs



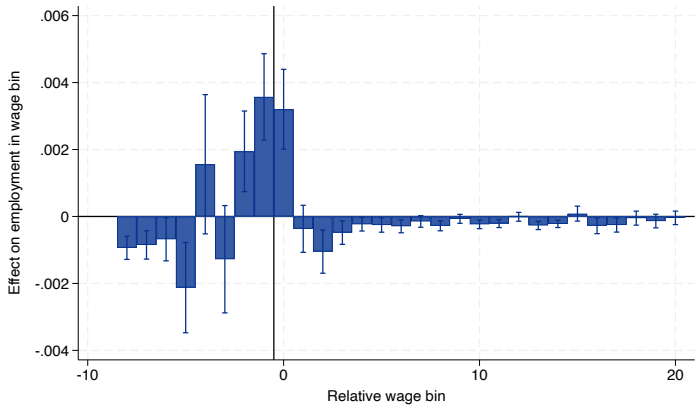
(b) Major VMWs



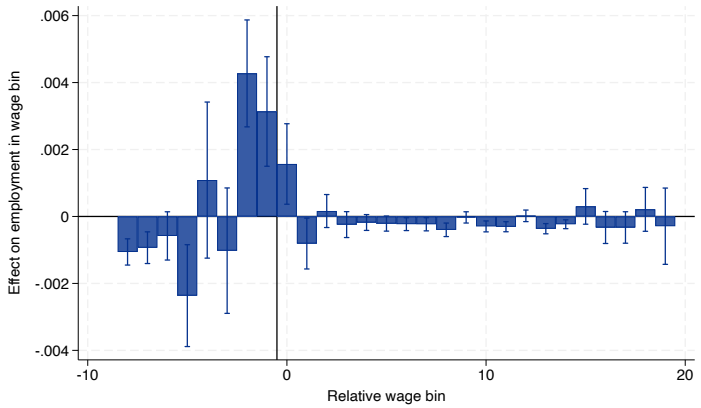
(c) $15 VMWs

Notes: This figure plots the regression coefficients on the standardized gap measure (see equation 1 for the definition of the gap measure) interacted with a post-period indicator, separately for each wage bin. The outcome is the share of the large retailer's employment in that wage bin. Panel (a) shows the results for all VMW events (stacked); Panel (b) for major VMWs (stacked); and Panel (c) for $15 VMWs (stacked). The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). CZ and month fixed effects are included. The sample is restricted to commuting zones with at least 30 policy employees. CZ and month fixed effects are included. 95% confidence intervals are shown. *Data sources*: Large credit bureau.

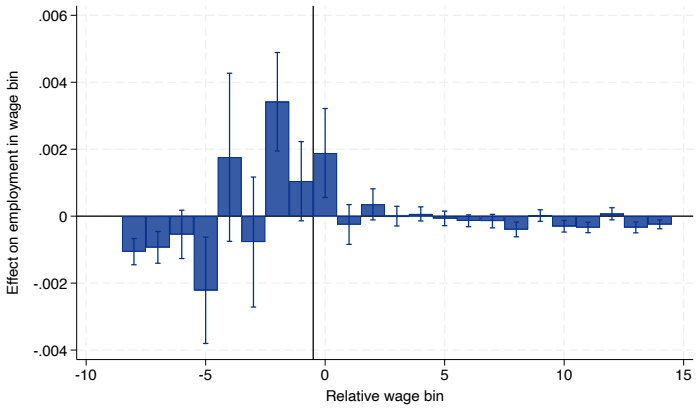
Figure D11: Spillover wage effect of large retailer VMWs, bunching design, large retailer gap



(a) All VMWs



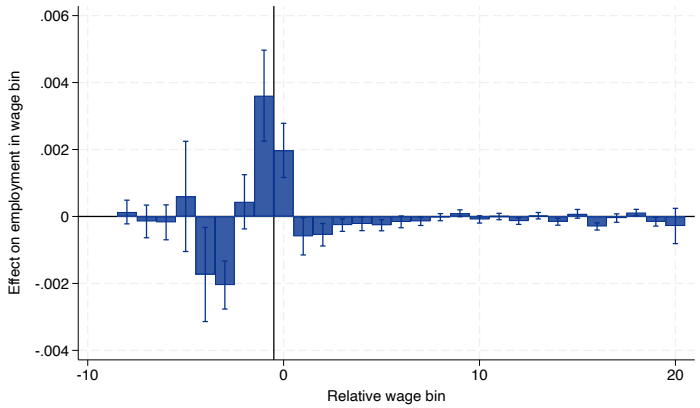
(b) Major VMWs



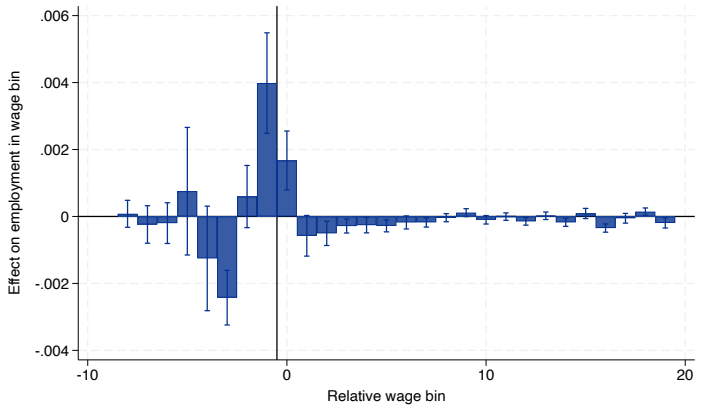
(c) $15 VMWs

Notes: This figure plots the regression coefficients on the standardized large retailer's own-gap measure (see equation 1 for the definition of the gap measure) interacted with a post-period indicator, separately for each wage bin. The outcome is the share of employment at that non-policy establishment in that wage bin. Panel (a) shows the results for all VMWs; Panel (b) for major VMWs; and Panel (c) for $15 VMWs. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). The sample is restricted to commuting zones with at least 30 policy employees and to non-policy establishments with at least 10 employees every month during the event window. Company-by-CZ, company, CZ, and month fixed effects are included. 95% confidence intervals are shown. Note: Panel (a) omits the lowest bins due to outlier employment counts in the less-than-$8 bin. *Data sources*: Large credit bureau.

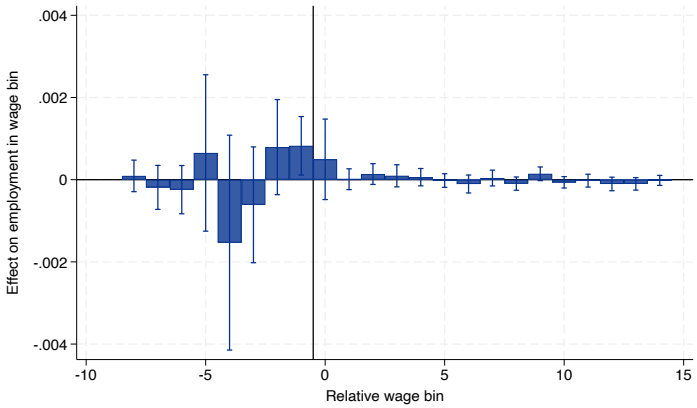
Figure D12: Spillover wage effect of large retailer VMWs, bunching design, market gap



(a) All VMWs



(b) Major VMWs



(c) $15 VMWs

Notes: This figure plots the regression coefficients on the standardized market gap measure (see equation 4 for the definition of the market gap) interacted with a post-period indicator, separately for each wage bin. The outcome is the share of employment at that non-policy establishment in that wage bin. Panel (a) shows the results for all VMWs; Panel (b) for major VMWs; and Panel (c) for $15 VMWs. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($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 every month during the event window. Company-by-CZ, company, CZ, and month fixed effects are included. 95% confidence intervals are shown. Note: Panel (a) omits the lowest bins due to outlier employment counts in the less-than-$8 bin. *Data sources*: Large credit bureau.

D.5 Heterogeneity by shared industry with large retailer

Table D8: Spillover effects, heterogeneity by shared industry with large retailer, large retailer gap

	Dep Var: Log average wage, non-policy		
Independent variables	All events	Major events	$15 events
Large retailer gap X 1(Post) X 1(Policy Ind)	0.00026	0.00082***	0.00056*
	(0.00016)	(0.00020)	(0.00031)
Large retailer gap X 1(Post)	-0.00059***	-0.00050***	0.00010
	(0.00012)	(0.00014)	(0.00016)
1(Post) X 1(Policy Ind)	0.00190***	0.00098***	0.00158***
	(0.00019)	(0.00023)	(0.00033)
Obs	9,040,130	4,170,109	1,744,869
CZs	631	629	380
Firms	1053	1023	930
Firms in large retailer industry	137	86	82
St. dev. large retailer gap	.055	.067	.088
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 standardized gap measure, a post-period indicator, and an indicator for being in the same industry as 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 D9: Spillover effects, heterogeneity by shared industry with large retailer and large retailer employment share, large retailer gap

	Dep Var: Log average wage, non-policy					
	Low Emp. Share			High Emp. Share		
Independent variables	All events	Major events	$15 events	All events	Major events	$15 events
Large retailer gap X 1(Post) X 1(Policy Ind)	0.00015	0.00025	0.00056	0.00052**	0.00156***	0.00106***
	(0.00021)	(0.00027)	(0.00047)	(0.00022)	(0.00029)	(0.00038)
Large retailer gap X 1(Post)	-0.00088***	-0.00087***	-0.00042	0.00002	-0.00003	0.00056**
	(0.00017)	(0.00022)	(0.00027)	(0.00019)	(0.00020)	(0.00027)
1(Post) X 1(Policy Ind)	0.00141***	0.00046*	0.00046	0.00298***	0.00220***	0.00338***
	(0.00020)	(0.00026)	(0.00039)	(0.00032)	(0.00035)	(0.00045)
Obs	6,051,113	2,806,719	1,074,242	2,989,017	1,363,390	670,627
CZs	453	443	296	493	471	236
Firms	1032	989	891	998	951	852
St. dev. large retailer gap	.056	.069	.093	.052	.061	.076
Mean policy emp. share	0.030	0.040	0.008	0.041	0.051	0.027
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 standardized gap measure, a post-period indicator, and an indicator for being in the same industry as 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 in the bottom 6 deciles across CZs. Columns 4-6 limit the sample to firms in CZs where the large retailer's share of employment is in the top 4 deciles across CZs. 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 D10: Spillover effects, heterogeneity by shared industry with large retailer, market gap

	Dep Var: Log average wage, non-policy		
Independent variables	All events	Major events	$15 events
Market gap X 1(Post) X 1(Policy Ind)	-0.00092***	-0.00064***	0.00218***
	(0.00010)	(0.00015)	(0.00052)
Market gap X 1(Post)	-0.00061***	-0.00062***	0.00004
	(0.00009)	(0.00011)	(0.00012)
1(Post) X 1(Policy Ind)	0.00200***	0.00105***	0.00202***
	(0.00018)	(0.00022)	(0.00036)
Obs	9,040,130	4,170,109	1,744,869
CZs	631	629	380
Firms	1053	1023	930
St. dev. large retailer gap	.002	.002	.002
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 standardized market gap measure, a post-period indicator, and an indicator for being in the same industry as the policy company. See equation 4 for the definition of the market 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.

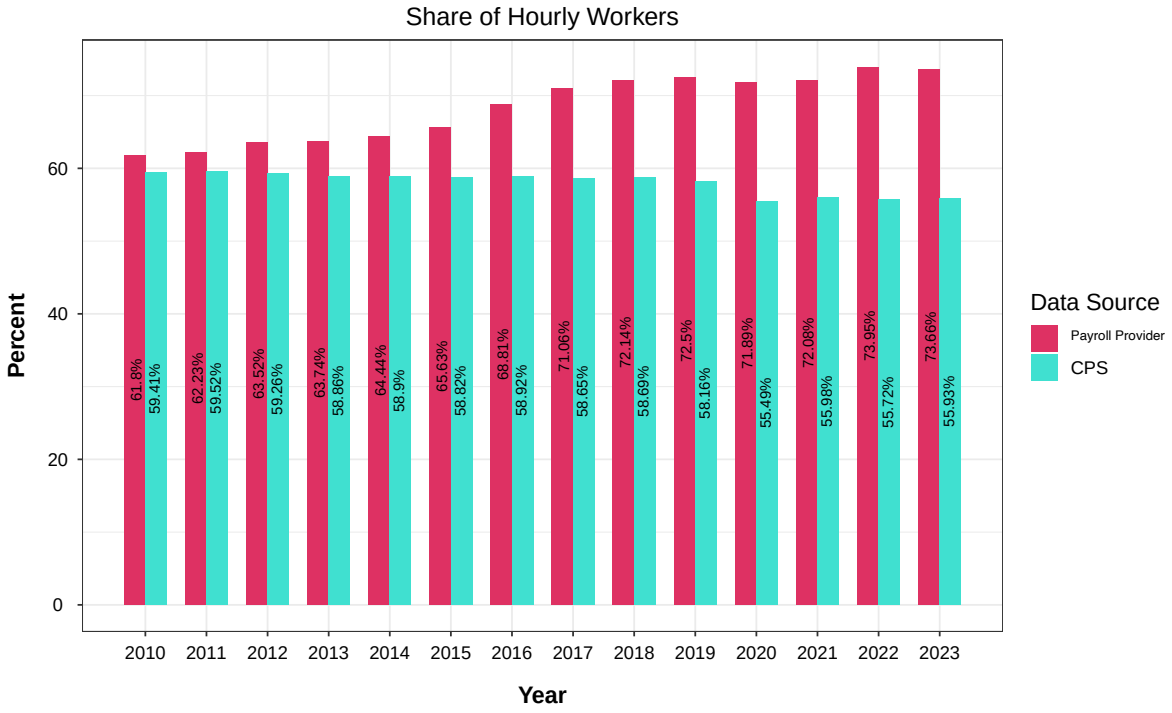
E Payroll Provider data, descriptives, and results

We test for effects of large retailer VMWs on smaller retail and service firms using anonymized data on firm hourly wage distributions from 2013-2023 from a payroll services provider we heretofore refer to as "Payroll Provider." The Payroll Provider data cover hundreds of thousands of client firms per year over this period and an average of over 9 million workers per year. Over 60% of these workers are hourly workers, who are the focus of our analysis.

In the following section, we provide descriptive statistics on the Payroll Provider data and compare these statistics to nationally representative data for the U.S. Next, we provide additional results and robustness checks on wage spillovers to smaller firms in the Payroll Provider data.

E.1 Descriptive characteristics of the Payroll Provider data

Figure E1: Salaried vs. hourly distribution of Payroll Provider vs. CPS



Notes: This figures shows the breakdown of salaried vs. hourly employment in the Payroll Provider data vs. the CPS. *Data sources*: CPS-ORG; Payroll Provider.

E.2 Analysis sample of firms and data on firm wage distribution

For each voluntary minimum wage event, we construct a panel of single-CZ firms (which encompasses the vast majority of the firms in the Payroll Provider data[44]) our analysis, we restrict to firms that reach at least five employees in the 12 month window around the event and that appear at least three times before the event and three times after. These "mild" balance restrictions allow us to retain a majority

[44]The maximum number of CZs per firm in our analysis sample is 3 and this is the 99th percentile of the number of CZs per firm.

Table E1: Distribution of Employment by Firm Size Bin (Percentage)

Source	1-4	5-9	10-19	20-49	50-99	100-249	250-499	500-999	1000+
BLS	4.8%	5.2%	6.9%	10.2%	7.7%	9.6%	7.0%	6.9%	41.8%
Payroll Provider	43.1%	21.3%	16.9%	12.9%	3.9%	1.6%	0.3%	0.07%	0.024%

Notes: Distribution of employment by firm size bin in the U.S. from the BLS and in the Payroll Provider data. *Data source:* BLS; Payroll Provider.

Table E2: Average hourly wage in the Payroll Provider data vs. the CPS

Source	Average Hourly Wage (USD)
CPS	17.72
Payroll Provider	20.299
Payroll Provider (topcoded as CPS)	18.325

Notes: Average hourly wages for hourly workers in the U.S. from the CPS-ORG and in the Payroll Provider data. *Data source:* CPS-ORG; Payroll Provider.

of employment for most events while still insuring we observe the same firm multiple times in both the pre- and post- period. For all analysis, for tractability, we work with a random 10% sample of firms from the overall database of firms with valid wage data.

Table E3 shows the percent of employment covered by mildly balanced firms in the data. The firm size distribution for our analysis sample of firms is shown in Table E4. The industry distribution of analysis firms is shown in Figure E2. Average wages across analysis firms is shown in Figure E3.

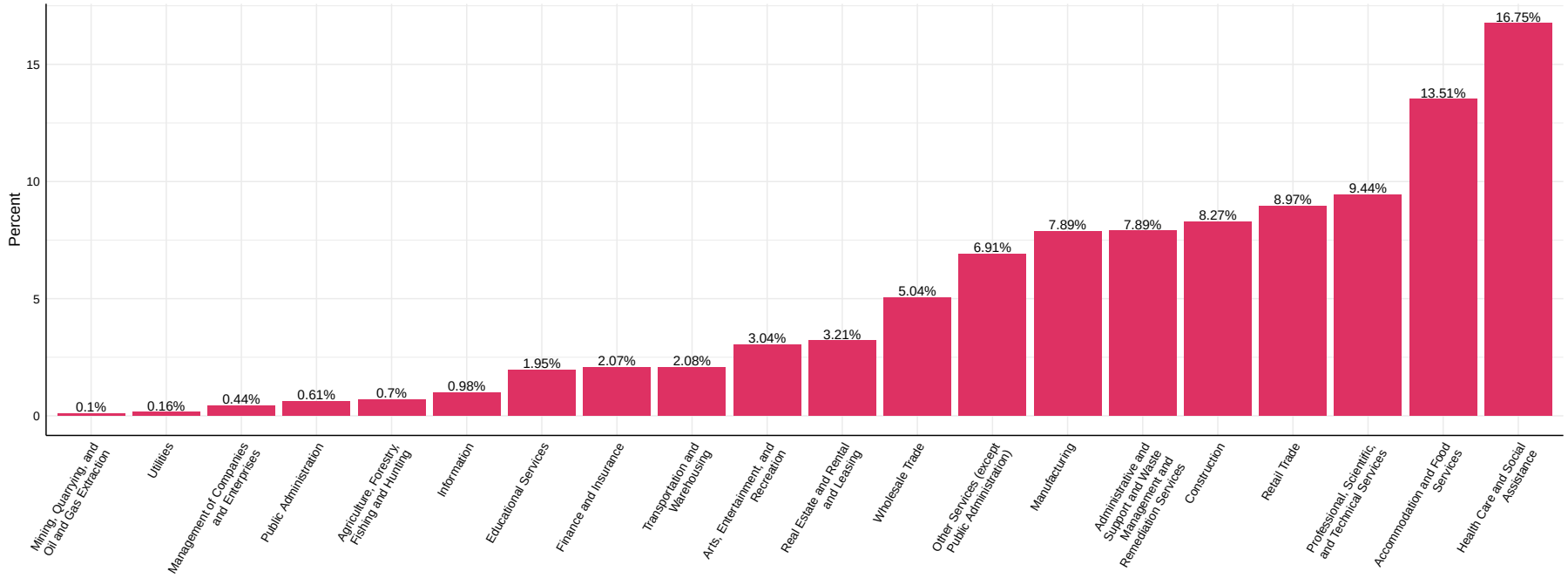
Table E3: Percent of Employment Covered by Mildly Balanced Firms by VMW Event

VMW Event	Employment Covered (%)
1	45.45
2	46.99
3	50.74
4	61.98
5	46.99
6	51.86
7	52.26
8	61.24
9	64.13
10	49.78
11	58.99
12	64.83
13	64.27
14	45.45
15	48.77
16	51.50
17	51.85
18	52.86
19	53.96
20	51.96

Table E4: Firm size distribution in analysis sample of firms

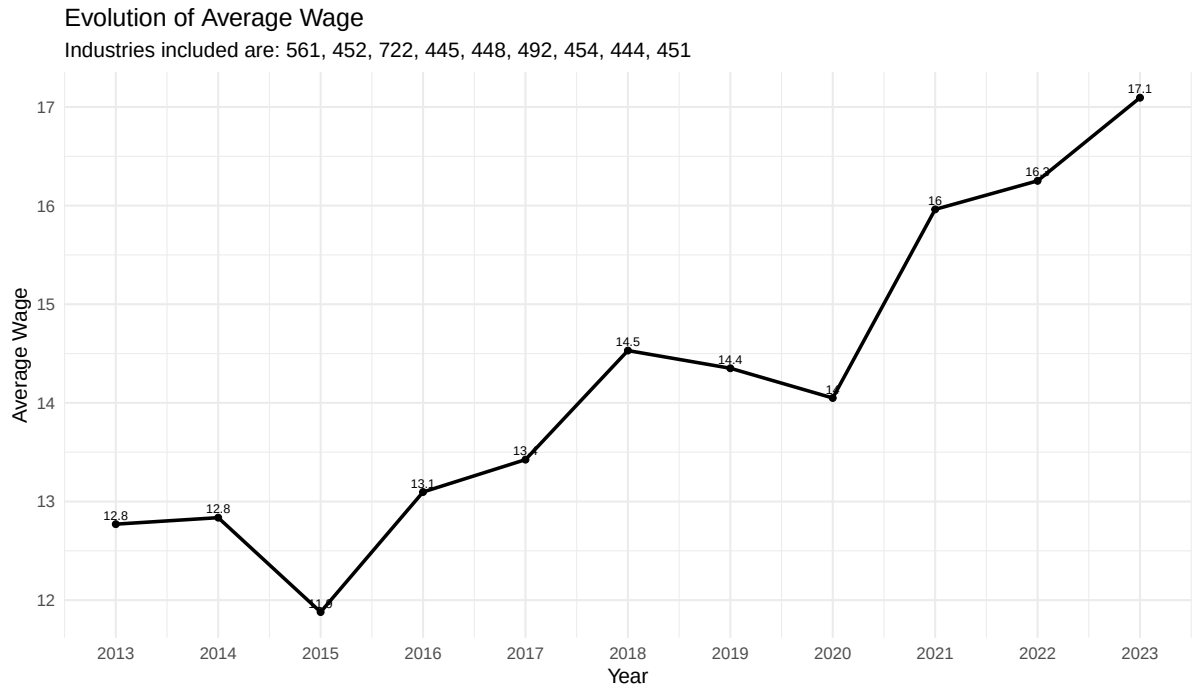
Variable	1-4	5-9	10-19	20-49
Share of employment	52.55%	29.58%	16.96%	0.91%
Share of firms	84.36%	12.01%	3.52%	0.11%

Figure E2: Industry distribution of analysis sample of firms



Notes: This figures shows the industry breakdown of hourly workers in the analysis sample of firms in the Payroll Provider data. *Data sources*: Payroll Provider.

Figure E3: Average wages across analysis sample of firms

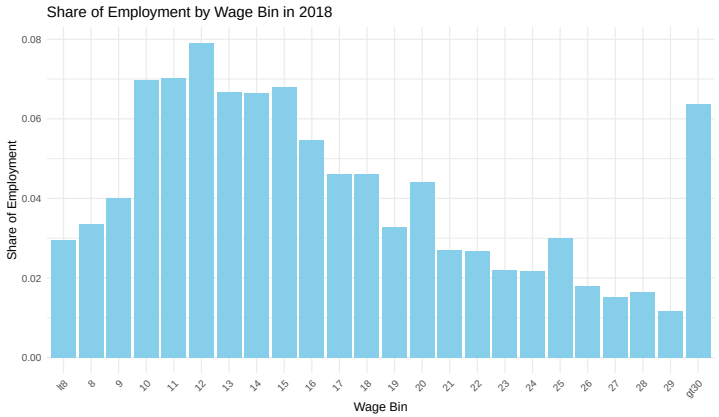


Notes: This figures shows the average wage across the analysis sample of firms. Included 3-digit NAICS industries are administrative and support services; food services and drinking places; food and beverage stores; general merchandise stores; clothing and clothing accessories stores; couriers and messengers; 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. *Data sources*: Payroll Provider.

Figure E4: Wage distribution among analysis firms in 2013, 2018, and 2023



(a) Wage distribution, 2013



(b) Wage distribution, 2018

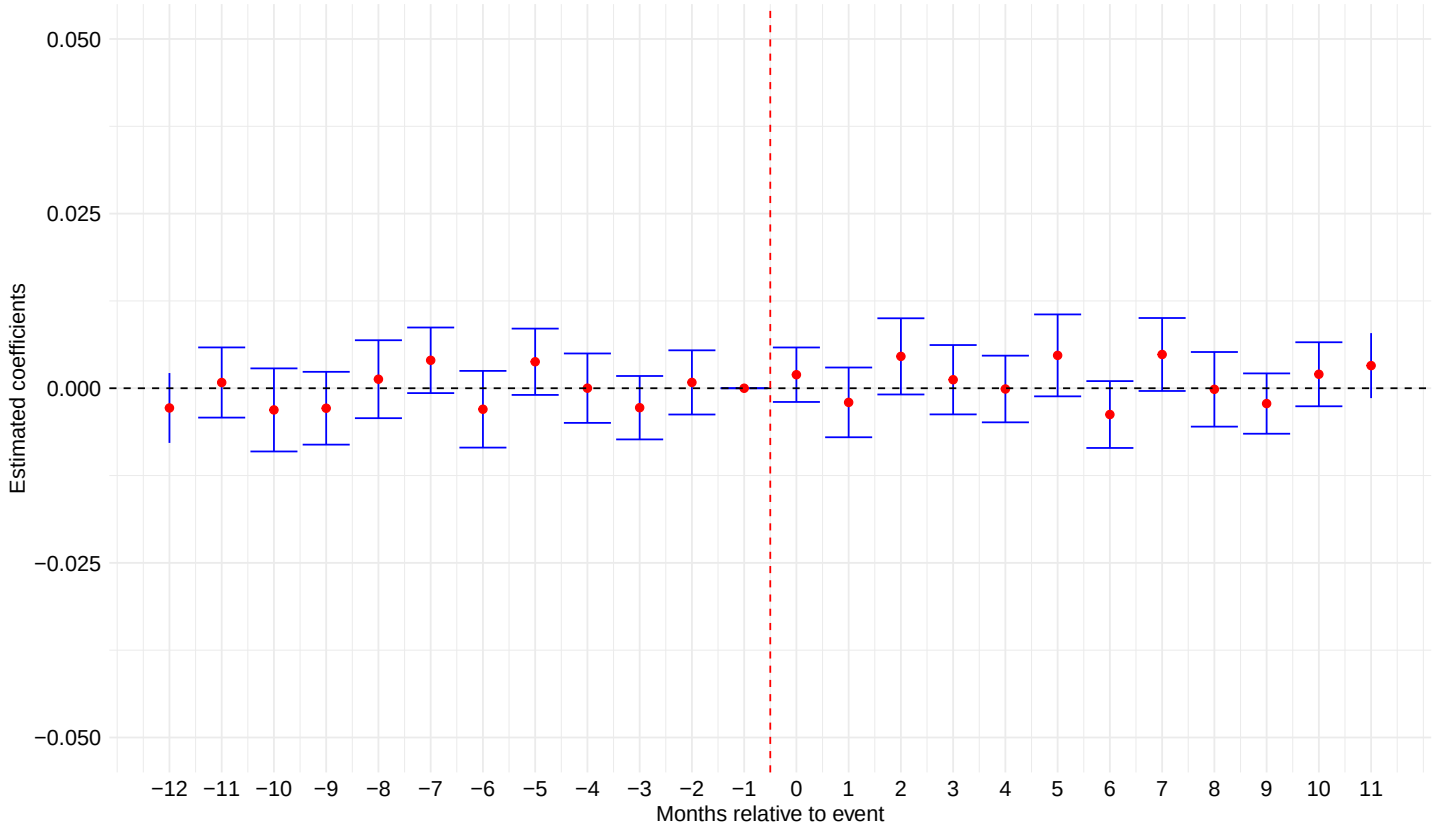


(c) Wage distribution, 2023

Notes: This figure plots the hourly wage distribution among the analysis set of smaller firms in the Payroll Provider data. Panels (a), (b), and (c) show the wage distributions for 2013, 2018, and 2023, respectively. *Data sources*: Payroll Provider.

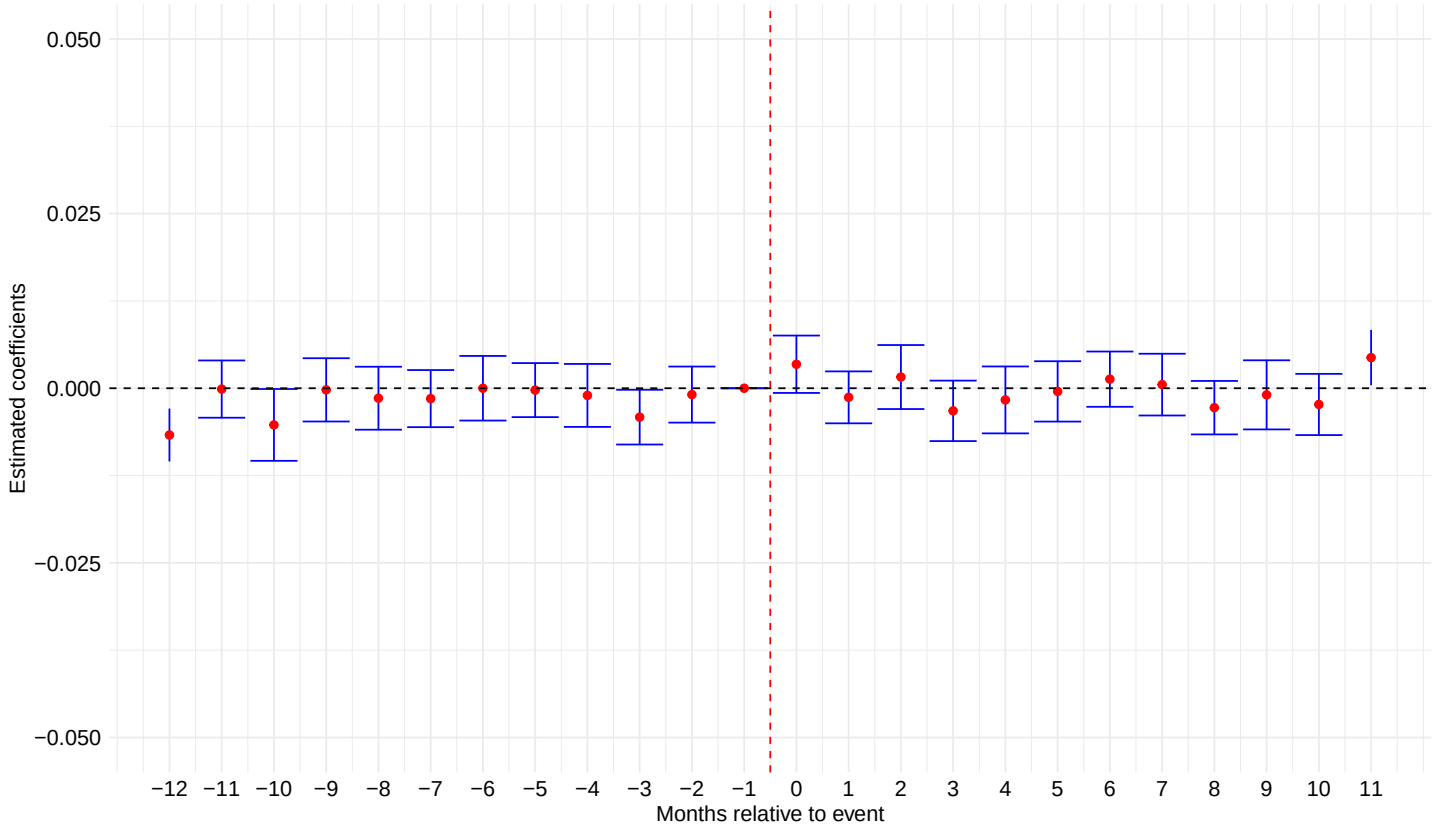
E.3 Additional results on spillover effects on smaller firms

Figure E5: Effect of large retailer VMWs on smaller firms' wages, large retailers' gap



Notes: This figure plots the regression coefficients on the large retailers' own-gap measure (standardized) interacted with month fixed effects. Thus, the gap represents the percent difference between the large retailer's voluntary minimum (in this case $15 VMW events) and the average hourly wage for the large retailer in that CZ. The outcome is the log average hourly wage at the company-level for small firms in the Payroll Provider data. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). Company, CZ, and month fixed effects are included. The sample is restricted to small firms with at least 5 employees in some month during the event window. 95% confidence intervals are shown. *Data sources*: Payroll Provider; Large Credit Bureau.

Figure E6: Effect of large retailer VMWs on smaller firms' wages, market gap



Notes: This figure plots the regression coefficients on the standardized market gap measure interacted with month fixed effects for $15 VMW events. The gap represents the percent difference between the area level wages (as measured using the credit bureau database) and the market wage when the large retailers' wages are adjusted to their voluntary minimum. The outcome is the log average hourly wage at the company-level for small firms in the Payroll Provider data. The outcome in panel (a) is the log average establishment wage among small firms. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). Company, CZ, and month fixed effects are included. The sample is restricted to small firms with at least 5 employees in some month during the event window. 95% confidence intervals are shown. *Data sources*: Payroll Provider; Large Credit Bureau.

Table E5: Effects of large retailer VMWs on smaller firms' wages, distance heterogeneity

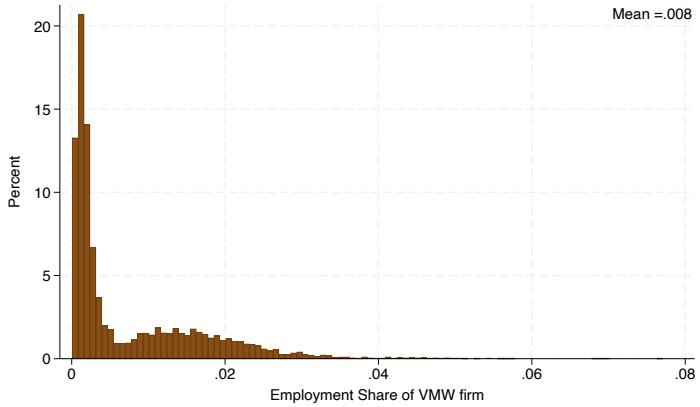
	Log Average Policy Wage (1)	Log Average Wage Exact (2)
Large Retailer Gap (std.) × Post	0.0448***	0.0013
	(0.0032)	(0.0034)
Post × Distance > Q1		-0.0047
		(0.0044)
Post × Distance > Q2		0.0023
		(0.0043)
Large Retailer Gap (std.) × Post × Distance > Q1		0.0057
		(0.0045)
Large Retailer Gap (std.) × Post × Distance > Q2		-0.0004
		(0.0040)
CZs	321	321
Number of Firms	9,272	9,272
SD treat. var. (unstd.)	0.0794	0.0794
Firm-FEs	Y	Y
CZ-FEs	Y	Y
Event time-FEs	Y	Y
Observations	250,741	250,879
R^2	0.97775	0.63287

Notes: This table reports the estimated coefficients on the large retailers' gap measure (standardized) interacted with a post-period indicator and indicators reflecting the distance between smaller firms and large retailer firms for three major VMW events. The dependent variable is the log average hourly wage. Column 1 reports the effect on own-wages for the large retailers. Column 2, row 1, reports the effect for small firms within the first quintile of distance from the nearest large retailer location. The last two rows report the interaction with further distance quintiles–firms in quintiles 2 in row 4 and firms in quintiles 3 and 4 in row 5. We drop the fifth quintile of distance as these encompass firms in smaller areas at much greater distances from large retailer locations. The gap measure is averaged over months -6 to -3 relative to the policy implementation date ($t = 0$). Company, CZ, and month fixed effects are included. The sample is restricted to a panel of firms appearing at least three times pre- and three times post- the large retailer VMW event and who have at least 5 employees in one of the months in 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*: Payroll Provider; Large Credit Bureau.

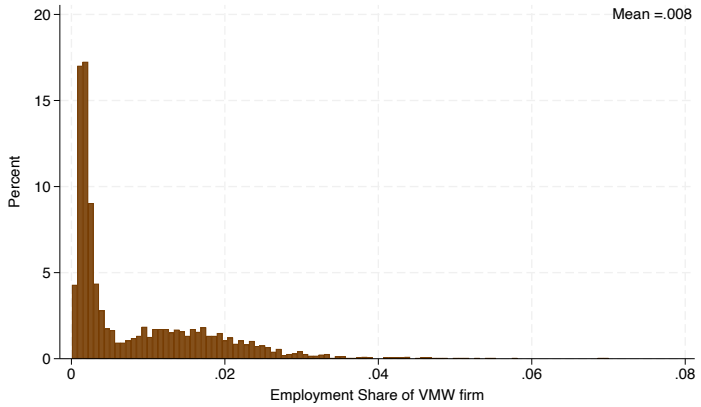
F Further details on implications of spillover results for local labor markets

F.1 Size of credit bureau firms within their labor markets

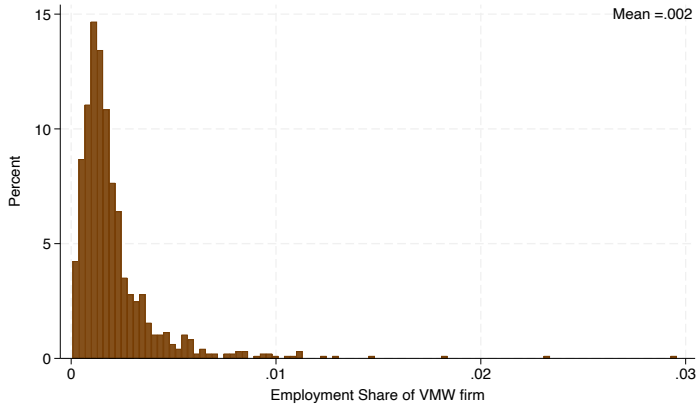
Figure F1: Distribution of VMW large retailer employment share out of total CZ employment for all, major, and $15 VMW events



(a) All VMWs



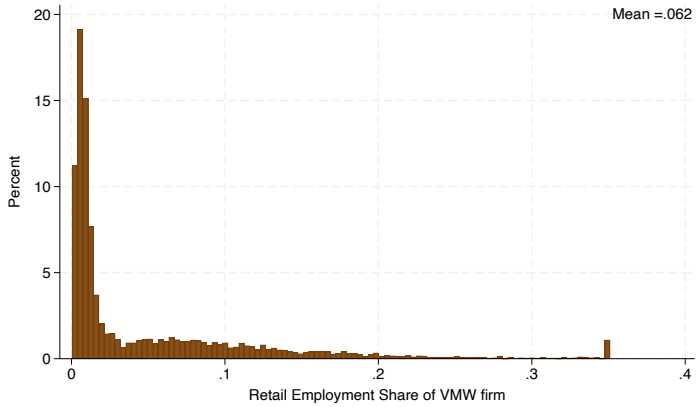
(b) Major VMWs



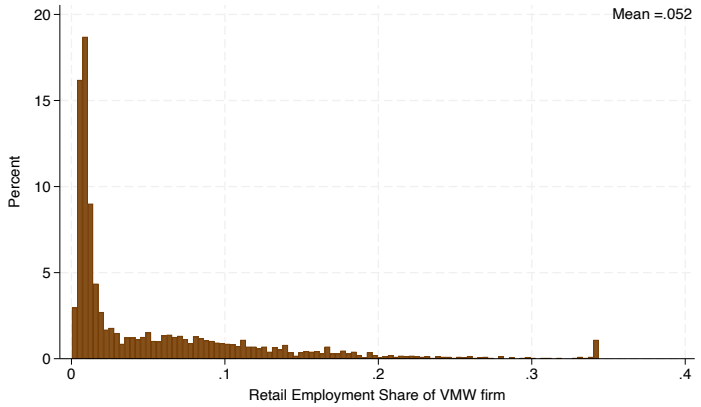
(c) $15 VMWs

Notes: This figure plots the distribution of large retailers' employment share out of total CZ employment across CZs for all VMW events, major events, and $15 events. The mean employment share across all CZs is reported as well. *Data sources*: Large credit bureau for large retailer employment shares; QCEW for total CZ employment.

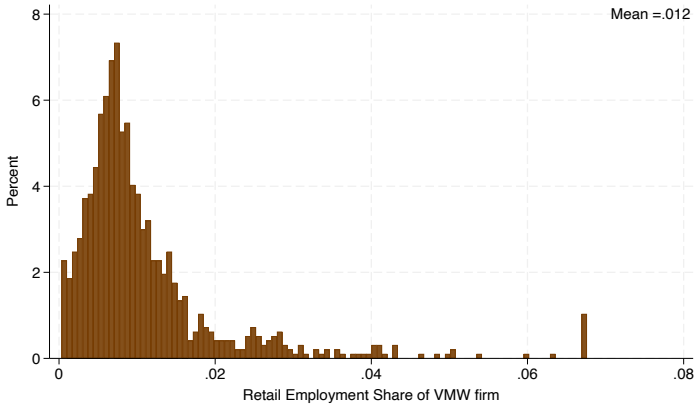
Figure F2: Distribution of VMW large retailer employment share out of total retail employment across CZs for all, major, and $15 VMW events



(a) All VMWs



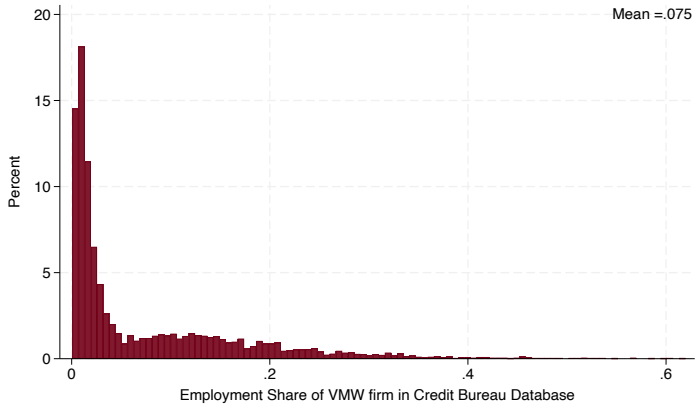
(b) Major VMWs



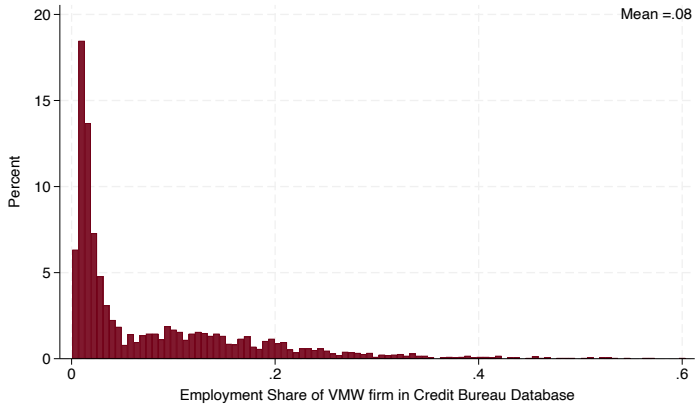
(c) $15 VMWs

Notes: This figure plots the distribution of large retailers' employment share out of total CZ retail employment across CZs for all VMW events, major events, and $15 events. We winsorize the 99th percentile due to a small number of small CZs with suppressed retail employment information in the QCEW. The mean employment share across all CZs is reported as well. *Data sources*: Large credit bureau for large retailer employment shares; QCEW for total CZ retail employment.

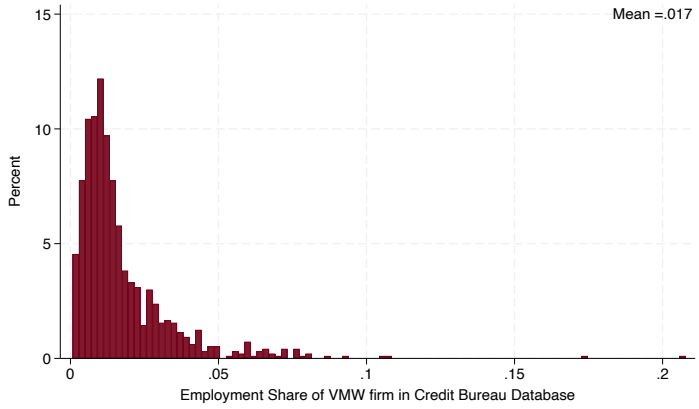
Figure F3: Distribution of VMW large retailer employment share in credit bureau sample across CZs for all, major, and $15 VMW events



(a) All VMWs



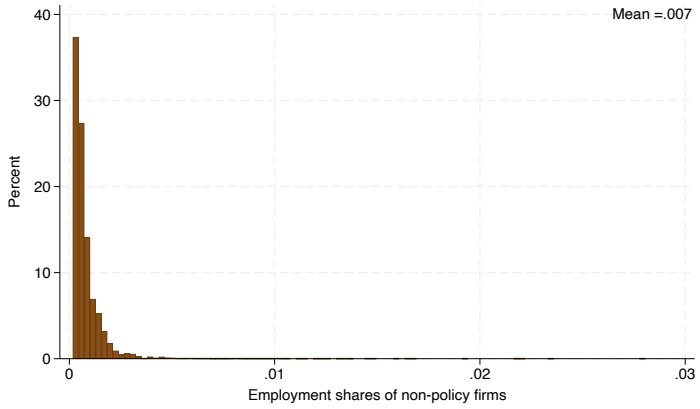
(b) Major VMWs



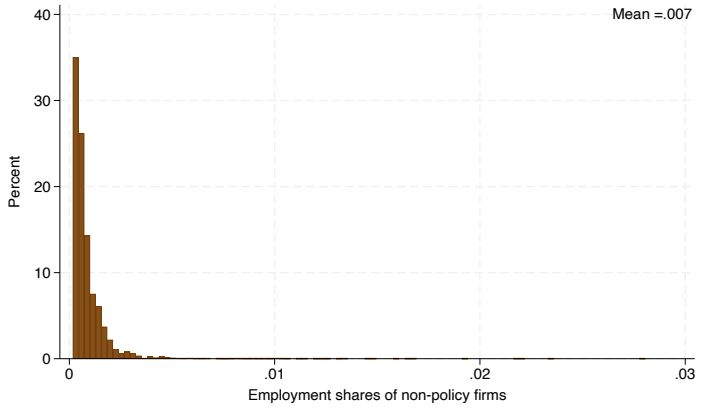
(c) $15 VMWs

Notes: This figure plots the distribution of large retailers' employment share within the credit bureau database across CZs for all VMW events, major events, and $15 events. The mean employment share across all CZs is reported as well. *Data sources*: Large credit bureau.

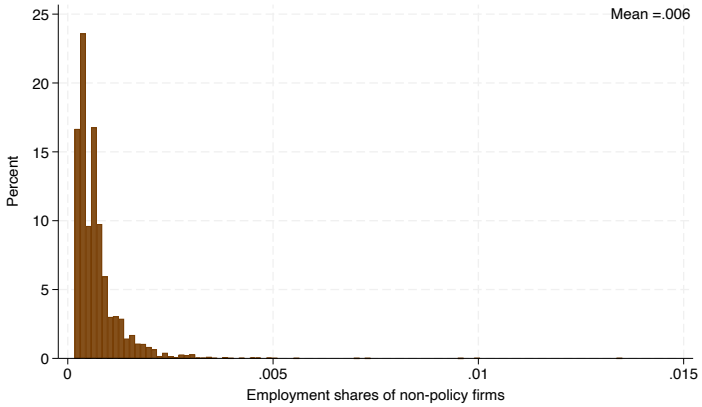
Figure F4: Distribution of non-policy establishment employment share in credit bureau sample across CZs for all, major, and $15 VMW events



(a) All VMWs



(b) Major VMWs



(c) $15 VMWs

Notes: This figure plots the distribution of non-policy establishment employment shares within the credit bureau database across CZs for all VMW events, major events, and $15 events. The mean employment share across all CZs is reported as well. *Data sources*: Large credit bureau.

F.2 Derivation of cross-wage elasticity from Berger et al. (2022)

Berger et al. (2022) illustrate the strategic interactions effect in the following equation describing general equilibrium passthrough effects of firm-level productivity shocks in the context of oligopsonistic competitions:

$$\frac{\Delta \log w_{ij}}{\Delta \log \text{vap}_{w_{ij}}} = \Gamma^*_{ij} + (1 - \Gamma^*_{ij}) \sum_{k \neq i} \frac{s^*_{kj}}{1 - s^*_{ij}} \frac{\Delta \log w_{kj}}{\Delta \log \text{vap}_{w_{ij}}} \tag{12}$$

Rearranging this equation to focus on the responsiveness of firm i's wages to firm k's wages, we obtain:

$$\Delta \log w_{ij} = \Gamma^*_{ij} \Delta \log \text{vap}_{w_{ij}} + (1 - \Gamma^*_{ij}) \sum_{k \neq i} \frac{s^*_{kj}}{1 - s^*_{ij}} \Delta \log w_{kj}. \tag{13}$$

Considering only a shock to firm k's local wage in market j through a voluntary minimum wage increase, we can set the productivity shock to zero and consider only the remaining terms:

$$\Delta \log w_{ij} = (1 - \Gamma^*_{ij}) \sum_{k \neq i} \frac{s^*_{kj}}{1 - s^*_{ij}} \Delta \log w_{kj}. \tag{14}$$

Thus, the cross-wage elasticity is captured by the term:

$$\hat{CWE} = (1 - \Gamma^*_{ij}) \sum_{k \neq i} \frac{s^*_{kj}}{1 - s^*_{ij}}. \tag{15}$$

Γ^*_{ij} is defined as follows:

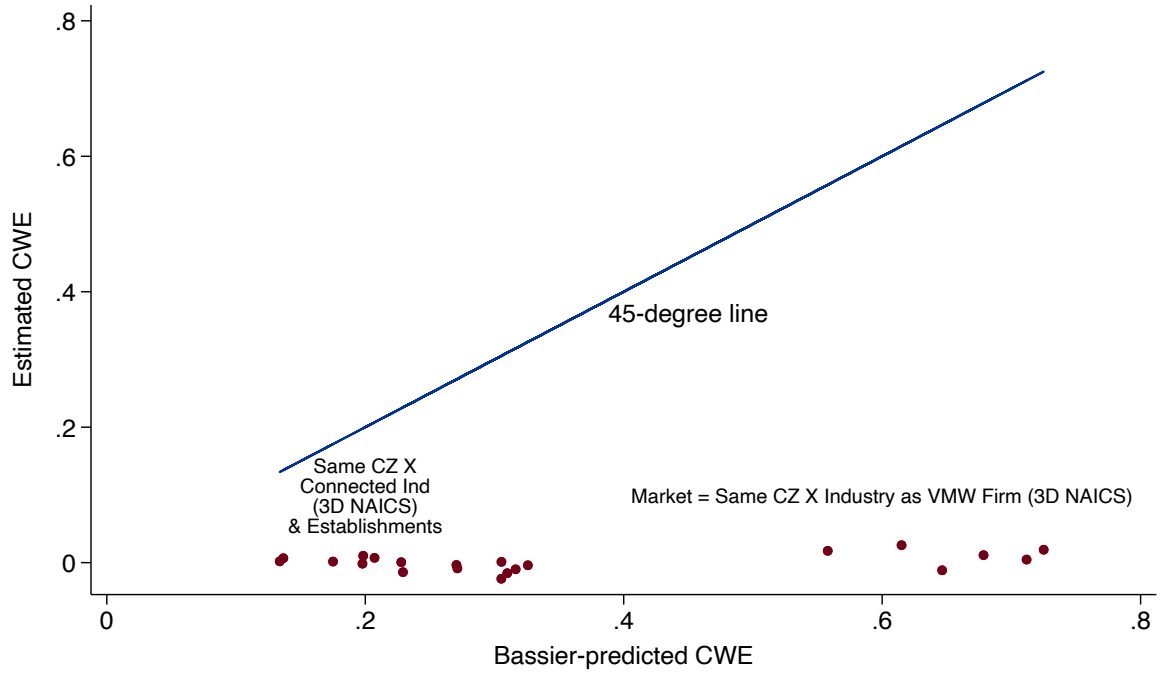
$$\Gamma^*_{ij} = \frac{s_{ij}(\eta - \theta) + \theta(\eta + 1)}{1 + (\eta + 1)(1 - s_{ij})} \tag{16}$$

Substituting this into Equation 15 and simplifying gives the following expression for the cross-wage elasticity:

$$\hat{CWE} = \frac{(1 + \eta) s_{ij} s_{kj} (\eta - \theta)}{[1 + (1 + \eta)(1 - s_{ij})] s_{ij} (\eta - \theta) + \theta(\eta + 1)}. \tag{17}$$

F.3 Comparison of cross-wage elasticities to predictions from Bassier (2022)

Figure F5: Cross-employer wage elasticity estimates relative to Bassier model predictions



Notes: Comparison of estimated cross-wage elasticities to those predicted by Bassier (2022). *Data sources*: Large credit bureau.