Spillover effects from voluntary employer minimum wages *

Ellora Derenoncourt, Clemens Noelke, and David Weil

February 28, 2021

Click here for most recent version.

Abstract

Low unionization rates, a falling real federal minimum wage, and prevalent non-competes characterize low-wage jobs in the United States and contribute to growing inequality. In recent years, a number of private employers have opted to institute or raise company-wide minimum wages for their employees, sometimes in response to public pressure. To what extent do wage-setting changes at major employers spill over to other employers, and what are the labor market effects of these policies? In this paper, we study recent minimum wages by Amazon, Walmart, Target, and Costco using data from millions of online job ads and employee surveys. We document that these policies induced wage increases at low-wage jobs at other employers. In the case of Amazon, which instituted a $15 minimum wage in October 2018, our estimates imply that a 10% increase in Amazon's advertised hourly wages led to an average increase of 2.6% among other employers in the same commuting zone. Using the CPS, we estimate wage increases in exposed jobs in line with our magnitudes from employee surveys and find that major employer minimum wage policies led to small but precisely estimated declines in employment, with employment elasticities ranging from -.04 to -.13.

*Ellora Derenoncourt: UC Berkeley. Email: ellora.derenoncourt@berkeley.edu; Clemens Noelke: Brandeis University. Email: cnoelke@brandeis.edu; David Weil: Brandeis University. Email: davweil@brandeis.edu. We thank Bledi Taska (Burning Glass Technologies), Andrew Chamberlain (Glassdoor), and Ray Sandza (Homebase) for generously sharing data. We thank Daron Acemoglu, Josh Angrist, David Autor, Sydnee Caldwell, David Card, Raj Chetty, Arindrajit Dube, Amy Finkelstein, Carol Heim, Lawrence Katz, Pat Kline, Alan Manning, Alex Mas, Claire Montialoux, Suresh Naidu, Jim Poterba, Jesse Rothstein, Jeff Smith, and numerous seminar and conference participants for many helpful comments. We thank Alaa Abdelfattah, Teresa Kroeger, Meghna Manohar, and Kartik Trivedi for outstanding research assistance. This work is generously supported by the Washington Center for Equitable Growth and Russell Sage Foundation award #R-1902-11776. Any opinions expressed are those of the author alone and should not be construed as representing the opinions of the Foundation.

1 Introduction

Declining labor market institutions characterize the low wage sector in the United States, where real wages have fallen or stagnated for the last 40 years. The federal minimum wage has been $7.25 for over 10 years, unions represent just 7% of private sector workers, and the rise in alternative work arrangements, from outsourcing to the gig economy, means fewer workers are covered by labor and employment laws.[1] With limited policy levers for boosting wages, worker advocates have called on high-profile companies like Amazon and Walmart to boost pay for their workers and act as standard bearers in the low-wage labor market (Thomas, 2017a; Hamilton, 2018).

This paper examines whether the wage setting behavior of major employers influences labor markets more broadly and, if so, by what mechanisms. We do so by exploiting sudden changes in the wage policies of three large low-wage employers to estimate the impact on jobs at other employers. Amazon, Walmart, and Target all instituted substantial company-wide minimum wages between 2015 and 2020. These three companies alone employ over 2 million workers in the US, or approximately 1.6% of the total workforce (Amazon.com, 2020; Walmart, 2020; U.S. Bureau of Labor Statistics, 2019). A major contribution of this study, therefore, is to provide some of the first empirical evidence of the impacts their policies have had on the broader labor market in which they operate. A second contribution of the study will be an extensive exploration of the mechanisms behind these spillover effects, providing insight into why wage setting shocks do or do not ripple outward given different underlying labor market characteristics.

Cleanly identified estimates of cross-employer wage spillovers in the US are limited, largely due to lack of data on specific employers' wage policies. To conduct our analysis, we use millions of online vacancy postings from Burning Glass Technologies and worker salary reports from Glassdoor, a job search and review platform. Data from online platforms are increasingly being used to study local labor market concentration, trends in the wages for new hires, and changing demand for skills (Azar et al., 2018; Deming and Kahn, 2018a; Hazell and Taska, 2019). We use these data to show that first, when these large employers announce a wage policy change, they do in fact update their advertised wages. Second, we are able to use information from online job ads to identify low-wage jobs at other employers based on the distribution of their advertised wages.

We use an event-study approach to estimate spillovers from major employers' wage policies to others operating in the same labor market. We identify the effect of the policies on jobs at other firms using variation in bite or exposure, defined as the fraction

[1] See recent work on rising wage inequality and the erosion of labor market institutions by Piketty and Saez (2003); Song et al. (2019); Kalleberg (2013); Osterman and Shulman (2011); Western and Rosenfeld (2011); David et al. (2016); Weil (2014); and Katz and Krueger (2019).

of job ads with pre-period wages below the new large employer minimum wage within detailed occupation, employer, and commuting zone categories. This approach mirrors that of papers estimating the causal effect of the federal minimum wage using state-level variation in the portion of the state's wage distribution under the new higher minimum wage (Card, 1992; Bailey et al., 2020). Here, however, we are able to exploit variation in bite at a much finer level, across tens of thousands of employers and hundreds of occupations and commuting zones. This level of variation allows us to precisely estimate effects and conduct several robustness checks to rule out alternative explanations for wage increases.

Our identification strategy relies on the assumption that within CZ, six-digit occupational categories, and employer cells (what we refer to as "jobs"), exposure to these large employers' minimum wages is uncorrelated with other factors affecting wages over time. Stable pre-trends, sharp effects around the exact time of the wage policy announcement, and placebo treatment date analyses provide strong corroborating evidence of this assumption.

We estimate substantial spillovers from Amazon, Walmart, and Target's wage policies. Prior to the policy change, the wages of more exposed versus less exposed jobs at other firms evolved in parallel. Exactly in the month after the announced wage increases, wages at exposed jobs jumped significantly. These effects persisted or rose steadily over the post-treatment period. We then employ a bunching estimator and show that wages of other employers shift out of wage bins below and spike at the wage announced by the large retailer in the months after the latter announces its policy. These results suggest other employers target the wage announced by the large employer and provide strong evidence that employers are responding to these wage policies rather than contemporaneous but unrelated shocks to labor demand.

In the case of Amazon, we estimate an increase in average hourly wages as a result of the policy of 4.7%, controlling for unrelated trends in wages at the occupation and commuting zone level. Given the size of the increase for Amazon's wages, roughly 20%, our results imply a cross-employer wage elasticity of 0.26. Our estimates fall in a similar range as previous estimates for cross-firm spillovers in the US: Staiger et al. (2010) finds a wage-setting elasticity in the market for registered nurses of about 0.19.

We are able to rule out several alternative explanations for the wage responses we estimate. Our baseline specification, which includes occupation-by-month and commuting-zone-by-month fixed effects, controls for simultaneous CZ-specific and occupation-specific demand shocks that might instead explain wage increases in highly exposed jobs. We also show that our results are robust to controlling for even finer grained shocks, such as those to specific occupation-by-CZ groups or specific employers. These latter results suggest our findings are not driven by shifts in employer wage posting behavior, such as

the decision to increasingly withhold or reveal the wage on highly exposed job categories. We further confirm that changes in advertised wages reflect true changes in wage policies by using data on worker-reported wages from the job review platform Glassdoor. Across all major employer policy changes, we show that workers at other employers experience spillover wage increases at magnitudes highly comparable to our results using Burning Glass Technologies job ads data.

To examine the broader labor market effects of these policies, we replicate our wage effects and estimate employment effects of large employer minimum wages using the Current Population Survey. We identify treated workers as those in occupation-by-CZ cells with wages below Amazon, Walmart, or Target's minimum wage in the year prior to treatment. Wage effects are strongly comparable to our results from the job ads and employee survey data, suggesting our results are unlikely to be driven by sample selection in the latter two datasets. We then turn to estimating the effects of the policies on employment. We find that employment slightly declines in highly exposed jobs in response to major employers' minimum wage increases. Excluding the specific industries of the employers implementing the wage policy change, we find own-wage employment elasticities ranging from -.04 to -.13. Despite stemming from very different mechanisms, our estimated own-wage employment elasticities are similar to those from the recent minimum wage literature. For example, in a meta-analysis, Dube (2019) finds an overall median elasticity of -0.17 and a low-wage worker median of -0.04 across a large number of studies of local, state, and national minimum wage hikes.

The wage spillover results we document provide direct evidence of the presence of labor market power by the companies that introduced voluntary increases. In a competitive labor market, deviations from a "market" wage by some employers should have no effect on the wages of other employers. Yet we show that other employers not only adjust their wages, but try to match the wage announced by large retailers, suggesting the presence of wage setting power and strategic interactions between firms (Berger et al., 2019). We expect that employment changes at individual employers will differ based on their own wage setting power. Firms with the most labor market power may increase employment after wage hikes while other firms also adjust wages but ultimately lose workers to leading firms. Heterogeneous responses to large employer minimum wages may average out to near zero effects in the aggregate. Such reallocation to larger firms would also echo recent findings in the minimum wage literature (Dustmann et al., 2019). In future work, we investigate heterogeneous employment responses by firm type to more fully understand the distribution of labor market power in the low wage labor market.

Our paper relates to several literatures on wage determination, employer wage setting, and monopsony power in labor markets. An older literature focused on a period when unions played a larger role in the US economy and sought to estimate the spillover effect

of unions on non-union wages in the same industry (Slichter et al., 1960; Budd, 1992; Kessler and Katz, 2001; Farber, 2005; Freeman and Medoff, 1985). More recently, a large literature has explored the role of firms in wage setting using matched employer-employee administrative data, concluding that firms explain a large share of wage variation across similar workers (Barth et al., 2016; Card et al., 2018; Song et al., 2019). Some have used these types of data to estimate the impact of shocks, such as patents granted to the firm on the wages of workers in those firms. Others have explored cross-employer wage spillovers in other countries, including through former coworker networks in Denmark or between temp agencies and client firms in Argentina (Caldwell, 2018; Drenik et al., 2020). Finally, related work examines the role of a workers' plausible outside options for employment in determining their wage at their current firm as well as defining the boundaries of the labor market (Caldwell and Danieli (2018); Schubert et al. (2019)). These types of spillovers and determinants of workers' wages are not well explained by perfect competition models of the labor market (Caldwell, 2018; Kline et al., 2019).

Perhaps most directly related to our study, Staiger et al. (2010) study the effects of a wage policy change at the Department of Veterans Affairs Hospitals ("VA Hospitals") on the wages of nurses at neighboring hospitals. They provide evidence of monopsony power in this market, estimating substantial cross-hospital wage spillovers and small labor supply elasticities, both of which indicate monopsonistic power in this labor market. Other studies of employer market power in this setting include Sullivan (1989); Matsudaira (2014)[2] A related paper by Dube et al. (2017) study bunching in firms' wages at round numbers in both online and traditional labor markets, indicative of optimization frictions as well as employer wage-setting power. A handful of recent papers have explored cross-employer wage spillovers in other countries, including through former coworker networks in Denmark; across temp agencies and clients in Argentina (Caldwell, 2018; Drenik et al., 2020); across substitute occupations for teachers in Sweden Willén (2019); and cross-country establishments within multinationals Hjort et al. (2019). To our knowledge, ours is the first paper to provide estimates of wage spillovers across a broad class of jobs in the low wage sector in the US, one that has been traditionally viewed as highly competitive.

In doing so, we contribute to a burgeoning literature measuring local monopsony power in the US (Azar et al., 2018, 2019; Beaudry et al., 2018). One difficulty in this literature is isolating exogenous variation in wages. Our approach, which exploits sudden shocks to wages stemming from voluntary minimum wages by large firms, may contribute new estimates that can be used to measure employer wage setting power across different labor markets. Our paper also provides empirical findings consistent with the predictions

[2]See Naidu et al. (2018) for an overview.

of models such as Berger et al. (2019), who model oligonopsonistic competition in labor markets and provide predictions of the labor market effects of minimum wages in this context.

Methodologically, we draw from the minimum wage literature, including analyzing shifts in the wage distribution in response to Amazon, Walmart, or Target's minimum wages using a bunching approach (Cengiz et al., 2019; **?**; Harasztosi and Lindner, 2019). We also draw on methods for evaluating the effects of national minimum wage changes, reflecting the national nature of the large retailers we study. Card (1992) and Bailey et al. (2020) leverage state-level variation in the fraction of workers affected by federal minimum wage increases. We construct the fraction of workers affected at the job level (defined as employer-by-occupation-by-commuting-zone cells), leveraging variation within locations, within job categories, and within employers in the sensitivity of wages to the policies of the large retailers. This empirical strategy allows us to estimate the wage and employment effects of large retailer minimum wages on other employers as well as the aggregate wage and employment effects of these recent increases. Further, we are able to document the extent of spillovers to higher wage bins, contributing to the evidence on minimum wage spillover effects up the wage distribution (David et al., 2016).

In addition to providing novel empirical estimates of employer wage-setting spillovers, our study contributes to the search for policy levers to improve wages in the low wage sector. Policy makers' targeted attempts to influence large employers may be an effective form of policy due to employer wage-setting power and declining worker bargaining power.[3] Our setting relates closely to prevailing wage policy for federal and state contractors (e.g. the federal Service Contract Act), with our results suggesting that minimum wages for federal contractor may have significant spillover effects on non-contractor firms. In the aggregate, the wage employment spillover effects of large major employer minimum wage policies mirror the effects of federal, state, and local minimum wages, despite very different mechanisms (transmission through competitive mechanisms as opposed to a binding minimum wage law). Similar to the evidence on government minimum wage effects, our results on smaller employers suggest that significant reallocation effects may be at play, with potentially substantial reductions in small firm employment (Dustmann et al., 2019; Berger et al., 2019). To the extent that these reallocation lead to increased concentration in the labor market, policy makers may wish to explore alternative or complimentary measures such as anti-trust legislation (Naidu et al., 2018).

The paper is structured as follows. Section 2 provides an overview of the recent voluntary employer minimum wage policies we study. Section 3 introduces a brief conceptual

[3]In luncheon remarks at the 2018 Kansas City Federal Reserve's conference on changing market structure, Alan Krueger discussed the need for even monetary policy makers to take into account monopsony power and concentration in labor markets. See Krueger (2018) for the full address.

framework for our analysis. Section 4 describes our data sources for employer-specific wages, and section 5 details our empirical approach leveraging job-level exposure to large employer policies using Amazon as an illustrative case study. We report our main spillover estimates and robustness checks in the case of Amazon in section 6, and extend this analysis to other employer policies in section 7. Section 8 investigates the broader wage and employment effects of these policies using the CPS. Section 10 concludes.

2 Voluntary minimum wage announcements, 2014-2019

In recent decades, US federal labor and employment regulation have lagged behind a restructuring low-wage sector. In many industries employing large numbers of low wage workers, unions lost density or were never significantly present. Corporate outsourcing and franchising have presented further challenges to worker collective bargaining. Workers in the gig economy or other alternative work arrangements fall outside traditional employment classifications and thus outside the scope of employment law (Weil, 2014). In this context, wages at the bottom of the wage distribution have been stagnant or declining in real terms.

Beginning in 2012, worker organizations and advocacy groups, led by the Service Employees International Union ("SEIU") launched the "Fight for $15" campaign to advocate for higher wages and union representation. The coalition drew on the union's earlier efforts to institute "living wages" through local ordinances and government contracting and sought to bring attention to persistently low earnings among workers in fast food, retail, and other service occupations despite a growing economy and low unemployment. Indeed, recent local governments' adoption of $15 minimum wages have been attributed to the efforts of the "Fight for $15" campaign (Rolf, 2015).

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 introduced increases in their minimum wage laws. Around the same time, a number of large, low-wage, and predominantly retail and service sector employers voluntarily instituted minimum wage increases for their employees (see Figure 1). Descriptive evidence on the implementation of these policy changes within the companies, let alone on their broader impacts in the labor market, is largely lacking. In this section, we provide descriptive evidence and background information on the wage policy changes adopted by Amazon, Walmart, and Target, three of the largest private sector employers in the US. Between 2014 and 2019, these employers implemented a total of 9 company-wide minimum wage increases, which we describe below. We provide a full description of these policies, including details on

coverage and applicability to new versus incumbent workers, in Appendix A.

Amazon/Whole Foods In October of 2018, Amazon announced a minimum wage of $15 per hour for all employees effective November 1, 2018. The increase affected an estimated 350,000 workers (including those at Whole Foods) (Amazon.com, 2019).[4] At $15 an hour, Amazon's minimum wage is more than double the federal minimum wage and far exceeds the majority of state and local minimum wages in the US.

We provide initial "first stage" evidence of Amazon's 2018 company-wide minimum wage increase in Figure 2 using Burning Glass Technologies ("BGT") data. The figure illustrates that company-wide minimum wage policies are identifiable in online job ads. Prior to October 2018, 80% of wages for hourly jobs advertised by Amazon and Whole Foods were below $15 an hour. Starting in October 2018 and over the next eight months, the percentage of jobs below $15 falls to zero. The percentage of jobs advertised exactly at $15 increases immediately starting in October of 2018, as do the percentage of jobs at $16-19 an hour. One potential reason for the increases at other wage levels was to maintain rankings in pay for workers who were formerly additionally compensated through bonuses and stock options, which were phased out with the minimum wage increase announcement (Abbruzzese and Cappetta, 2018).

Walmart and Target As Figure 1 revealed, several other employers implemented voluntary minimum wages, both before and after Amazon's policy. We analyze the policies of two other salient and large employers who have implemented increases: Walmart and Target.

Walmart, the largest employer in the US with a workforce of 1.5 million, has implemented 3 company-wide minimum wage policies since 2015, from $9 to $11 in 2018. At nearly twice the size of Amazon's workforce, Walmart's wage policies are likely to have had ripple effects on other low wage employers. The first minimum wage was an increase to $9 per hour announced in February 2015. Subsequent increases to $10 and $11 were announced in 2016 and 2018. A big box store competitor, Target, followed close on the heels of Walmart, with a $9 minimum wage announced just one month after Walmart's February 2015 announcement of its $9 minimum wage. Target then increased to $10 in April of 2016, to $11 in September of 2017, to $12 in March 2018, and to $13 in April 2019.[5] We analyze each of these increases in turn, exploiting differences in the timing and levels of these voluntary minimum wages. In cases where announcements were made

[4] Amazon's acquisition of Whole Foods was approved by Whole Foods' shareholders in August 2017 (Amazon.com, 2017).

[5] Target followed through on their 2015 commitment to increase their minimum wage to $15 by 2020 with an increase in June of this year. However, due to the irregularities of the labor market during the Coronavirus recession, we do not include this most recent increase in our analysis.

in close succession, such as the Walmart and Target $9 minimum wages, we pool these two natural experiments and examine their joint effect on employers operating in the same local labor market.

3 Wage determination in low-wage labor markets

The notion that some employers exercise wage setting power is not a new one. Indeed, it was the prevailing conceptualization of labor markets in the mid-20th century. Robinson (1969) laid out a theory of imperfect competition in labor markets giving rise to monopsony power of employers, and scholars such as John Dunlop and other "institutionalists" focused on the role of institutions in shaping the structure of wages.[6] In recent years, there has been a resurgence of empirical scholarship on monopsony and growing consensus that frictions in the labor market drive a wedge between firm wages and a worker's marginal product (Barth et al., 2016; Song et al., 2019; Card et al., 2018; Dube et al., 2017; Caldwell, 2018; Dube et al., 2020).

Despite this recent resurgence, there is little evidence documenting wage setting spillovers in the US and none to our knowledge studying the low-wage sector. The closest paper to our study is Staiger et al. (2010), who examine spillovers stemming from a wage policy change at the Veteran's Affairs (VA) hospital system affecting registered nurses. The authors estimate the spillover effects of the policy and wages and employment of registered nurses by hospitals in close physical proximity to a VA hospital. They find both substantial spillovers—cross employer wage elasticities of around 0.19—and a small, positive employment elasticity, though they cannot rule out negative employment effects.

Our study, by contrast, estimates spillovers from wage shocks to the low wage sector, broadly defined. Workers in service and retail occupations have some of the highest occupational mobility rates compared to other workers (Schubert et al., 2020). Thus wage shocks to stock clerks and packers at Amazon warehouses plausibly affects food service workers, cashiers, or customer service representatives. We allow for spillovers to these other occupations by measuring exposure to these policies solely through the pre-existing wage rate of jobs at other employers.

High occupational mobility may indicate ease of switching and widespread availability of substitute jobs for low-wage workers, consistent with a highly competitive labor market. In such a setting, wages would be determined by supply and demand and equivalent to workers' marginal productivity. No employer would deviate from the market wage as they would incur costs in excess of revenue in doing so. For the same reason, should a

[6] See Weil (2017a) for an overview of this literature and the history of economic thought as it pertains to wage determination.

single employer raise the wage above the market rate, other firms would have no incentive to follow.

The very public announcements of voluntary minimum wages by firms like Amazon, Walmart, and Target indicate a departure from this perfectly competitive benchmark. Further, the emulation of their policies by other employers suggests wage setting power is widespread, even in the low wage sector. Though we do not explicitly test different models of the labor market, we believe our findings are more consistent with theories of oligopsonistic competition as recently modeled by (Berger et al., 2019). In this context, a finite number of employers exercise varying degrees of wage setting power. A wage increase by a major employer can ripple across to other firms as they seek to stem the flow of their workers to the larger firm. Our findings to date provide evidence on this first front, of strategic wage responses among low-wage employers. Our evidence on small changes in employment in the aggregate is also consistent with a model where both the leading firm's wage increase as well as those of their competitors influence a new allocation of workers across firms. In ongoing work, we study these within-market employment responses in order to better understand the nature of competition in low-wage labor markets.

4 Data on employer wages

A key difficulty in measuring and identifying cross-employer wage spillovers in the US is the lack of available datasets that provide time-stamped, employer-specific information about hourly wages offered by establishments.[7] One of the contributions of this project will be integrating data from major online job platforms in order to better identify cross-employer wage spillover effects in the US. Data from online job platforms are increasingly being used in studies of labor markets in economics (Deming and Noray, 2018; Deming and Kahn, 2018b; Azar et al., 2017; Hazell and Taska, 2019). Websites like CareerBuilder, Indeed, and Burning Glass Technologies provide wages posted by employers, often with rich information on job title, desired skill or experience level, and the geographic location of the establishment posting the vacancy. Glassdoor, a platform with worker participation, collects worker reports on their pay and satisfaction at specific employers and can be further used to understand the effects of employer wage policies on the received pay and reported satisfaction of workers.

[7] Establishments are the physical location of a specific branch of a firm.

4.1 Burning Glass Technologies

The key data for our cross-employer wage regressions come from Burning Glass Technologies ("BGT"). BGT collects data on the near-universe of online job postings from roughly 40,000 websites, including job boards and company pages (Hazell and Taska, 2019; Carnevale et al., 2014).[8] The data cover job postings from 2010 onwards, 20% of which include information on the posted wage for that job. Here we briefly describe features of the data and the available variables that make the data appropriate for the analysis we will be conducting.

Frequency The dataset on posted wages is high frequency, including information on the day, month, and year of the posting. These high frequency wage posting data are essential for testing the parallel trends assumption for pre-period wages of highly exposed versus less exposed jobs and to isolate effects occurring precisely around the announcement of the increases.

Direct measures of outcome of interest The dataset on vacancies with posted wages includes a variable indicating the posted minimum salary for specific time units of pay. For example, for hourly wage jobs, the posted minimum hourly wage is available. This is the outcome of interest in this study as we are interested in how local wage shocks influence the wage setting behavior of employers.

Employer and other information Approximately 127 million job postings in the BGT database since 2010 contain information on the employer posting the vacancy. Nearly all postings (98%) contain detailed information on the location of the job; 96% contain occupation information; and 79% contain industry information.

Representativeness of BGT data A number of papers using BGT data have analyzed its representativeness.[9] We conduct our own comparison of the occupation, industry, and geographic distribution of hourly workers in the CPS to those of hourly job vacancies in BGT. The comparison is summarized in Table 1 which provides estimated hourly job characteristics for the BGT and CPS data sets. We find that relative to existing stocks of hourly workers in the CPS, a higher share of hourly job vacancies are

[8] Job postings are at the establishment level, or the specific physical branch of a firm.

[9] Hazell and Taska (2019) provide extensive evidence on the validity of these data and their consistency with overall US new hire wage trends from sources such as the Current Population Survey ("CPS") and the Quarterly Census of Employment and Wages ("QCEW"). Hazell and Taska (2019) confirm that industries that are less likely to post vacancies online are underrepresented in BGT relative to CPS. Studies by Azar et al. (2018); Deming and Noray (2018); Deming and Kahn (2018b) provide further evidence on the value of and validity of BGT data.

present in the West and a lower share in the South. Job vacancies with wage information are skewed towards health care and services and away from retail; however, focusing on hourly job vacancies partially corrects for this. These discrepancies may represent differences between sectoral growth versus current sectoral composition; Hershbein and Kahn (2018) find that the degree to which BGT under-represents some industries and over-represents others is stable over time.

Sample Our sample consists of online job ads from January 2012 through February 2020 that contain the following information: the posted minimum hourly wage; employer name; the county in which the job is located; and the occupation of the position being advertised (using the SOC code). We further restrict our sample to those jobs for which the pay frequency is hourly. In some of our analyses, we further restrict the industry code (up to 6-digits NAICS code) to be non-missing. We restrict the data further to focus on specific observation periods of 24 months around the wage policy changes analyzed below.[10] Because we use employer-by-occupation-by-CZ fixed effects models, we restrict to employer-by-occupation-by-CZ cells that appear at least once before and once after treatment within an observation period. Finally, we restrict each analysis to only those commuting zones for which we observe policy firm job ads in the BGT data in the pre-treatment period. The reason for this is that there are very few CZs with job postings in which there are no policy firm advertisements. For example, 90% of all BGT postings fall in CZs in which Amazon advertised in the year prior to Amazon's minimum wage.[11] We found that the small share of postings in CZs without policy firm ads differ significantly from the labor markets of relevance to our study.

4.2 Glassdoor

Glassdoor is a two-sided online job search and review platform where employers post vacancies, but importantly, job-seeking users of the platform also upload information about salaries for specific job titles at specific firms. Salary information for hourly workers contains exactly the hourly wage. The Glassdoor data are complementary with the BGT data as they allow us to see whether changes in advertised wages translate into changes in wages workers report they receive. Wage changes estimated in Glassdoor also confirm that any effects found in BGT are not driven by systematic changes in which jobs are advertised online as opposed to a shift in the wage distribution at the treated firm.

In addition to variables also contained in BGT data, including employer identity, the location of the establishment, and wage information, Glassdoor data provide additional

[10] In our event-study analysis, we stack the data from these observation periods together and include month-by-event and employer-by-event fixed effects.

[11] Walmart and Target advertise in a larger set of CZs than Amazon.

worker-level characteristics. For example, a large fraction of workers using Glassdoor report their gender and their age when workers create a Glassdoor account. These worker-level characteristics will allow us to test for further heterogeneity in any estimated wage spillover effects.

5 Empirical strategy: job-level exposure

The use of company-wide wage floors by large low wage employers over the last five years represent a break from localized wage setting and a potential response to the Fight for $15 movement's call to boost service and retail workers' wages. Our analysis is the first to estimate the spillover impacts of those wage policies on the wage and employment policies of other firms in their labor markets. These shocks differ from shocks to narrowly defined sectors, such as the market for nurses, in that they potentially apply more broadly across occupations and industries in the low wage sector. We explicate our empirical strategy using Amazon as a case study. In Section 7, we report the results for the remaining 8 employer minimum wage changes from Walmart and Target.[12]

We use variation in bite or exposure to identify the effects of Amazon's voluntary minimum wage policy on non-Amazon employers. This methodology echoes the literature studying the effects of US federal minimum wage policies using geographic variation in bite (Card, 1992; Bailey et al., 2020). The difference in our case is that we are able to measure exposure at a much finer level. We define exposure at the job level, where jobs are defined as employer-by-occupation-by-CZ cells. Our key treatment variable is the fraction of postings at the job level below $15 in the year before Amazon's policy takes effect in October 2018.

Formally, we define exposure or fraction of postings i affected at the job level j as follows:

$$D_{j(i)} = \frac{\sum_{i \in j(i)} \sum_{t \in [-12,-1]} \mathbb{1}(w_{it} < w^*)}{N_{j(i),t \in [-12,-1]}} \tag{1}$$

or the fraction of job postings at non-Amazon firms that are below the minimum wage set by Amazon in the 12 months prior to the policy change. Therefore, in the case of Amazon, we calculate the fraction of postings appearing between October 2017 until September 2018 with wages below $15. We restrict our analysis to the 188 commuting zones where Amazon advertised in the year before treatment.[13] In practice, this restriction does not

[12] We also occasionally report results for Costco's $14 and $15 minimum wages announced in June, 2018 and March, 2019, respectively. The results are extremely similar to those for other large retailer minimum wage policies.

[13] To obtain the best possible measure of the location of Amazon warehouses and Whole Foods grocery stores, we include locations with Amazon postings with and without wage information.

greatly affect the sample size as 90% of non-Amazon postings with valid wage information in our sample appear in the same CZ as an Amazon CZ.

There are over 90,000 employers with pre- and post-treatment postings, over 800 six-digit occupational categories, and 188 commuting zones in which Amazon or Whole Foods advertise. On average, about 58% of postings fall below $15 at the job level. Figure 3 shows the geographic distribution of job level exposure at the commuting zone level across the US. Exposure varies within every region of the US and is not concentrated in lower income regions of the country. Areas designated "Not present" in the legend of Figure 3 are those where no job ads were placed by Amazon in the year before the policy announcement.

The size of the BGT dataset and the many degrees of variation we are able to exploit allows us to rule out several alternative stories for wage increases at non-Amazon employers after Amazon's policy takes effect. We discuss these robustness checks in great detail in Section 6.1.

The empirical strategy we employ is a series of event-study and difference-in-difference analyses around the time of Amazon and other employers' minimum wage policies that exploit both variation in exposure to and the precise timing of the policies. Specifically, we estimate the following event study models:

$$\log w_{it} = \alpha + \sum_{k=-12}^{12} \beta_k D_{j(i)} \times \mathbb{1}_{[t=k]} + \eta_{j(i)} + \delta_{c(i)t} + \chi_{o(i)t} + \varepsilon_{f(i)} \tag{2}$$

The outcome variable is the log hourly wage advertised on a posting i at time t. The key coefficient is β_k, the coefficient on the interaction between fraction affected at the job level ($D_{j(i)}$) and month t. In addition to fixed effects for the job ($\eta_{j(i)}$), our baseline specifications includes fixed effects for changes in the composition of postings. Over our two-year observation window around the Amazon policy announcement, the average advertised hourly wage in our BGT sample declined from $18 to $15, suggesting an increasing share of lower paid jobs being advertised online. We include occupation-o-by-month-t and CZ-c-by-month-t fixed effects that help account for these changes as well as potential confounding shocks such as state or city minimum wages. The treatment month is denoted k=0 and is omitted for the model to be identified. We cluster standard errors at the employer level ($f(i)$).

Identifying assumption and proposed validity tests Our identifying assumption is that the fraction of a job's pre-period wages that are below Amazon's new minimum wage is uncorrelated with changes in wages prior to the policy change. Parallel pre-trends as well as a sharp increase in wages immediately after the policy provide corroborating evidence that this assumption holds. We conduct a number of checks in Section 6.1 that

further bolster this assumption.

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

$$Y_{it} = \alpha + \tilde{\beta} D_{j(i)} \times \text{Post} + \eta_{j(i)} + \delta_{c(i)t} + \chi_{o(i)t} + \varepsilon_{f(i)} \tag{3}$$

where Y_{it} is the outcome of interest, including log hourly wages as well as indicators for a posting's advertised wage falling within specific wage bins. Analyzing the wage bin of a posting as an outcome allows us to document whether non-policy firms match the voluntary minimum announced by the large employer as well as the extent of spillovers up the wage distribution in response to the announcement.

6 Spillovers from Amazon's wage increase

We observe substantial spillovers to other employers resulting from Amazon's $15 minimum wage. Figure 4 plots β_k from estimating equation 2 and shows that starting exactly in October 2018, the month of Amazon's announcement, employers with greater exposure to Amazon's policy boosted their own advertised hourly wages. Corroborating our assumption that exposure is uncorrelated with wage dynamics prior to the policy, our results indicate stable pre-trends centered around zero in the 12 months leading up to the policy. Moving from zero percent exposure to 100% exposure is associated with an 5 log-point increase in advertised hourly wages immediately post treatment in October 2018. This effect strengthens over the 12-month post treatment period, rising to about 10 log points.[14]

In the remainder of this section, we present a series of analyses and robustness checks focusing on Amazon's policy that validate our empirical strategy and provide further evidence that the wage increases we observe stem from the large retailer's policy. Section 7 extends these findings to Walmart and Target's recent minimum wage increases and relates the extent of spillovers to their bite in the labor market as well as the level of the large employer's minimum wage.

To bolster our evidence that this sharp increase in the wages of non-Amazon employers

[14]We show in a robustness check that pre-trends are also throughout a 24-month pre-period in Appendix Figure C1. Wages are gradually trending up in highly exposed jobs, likely due to wage growth at the lower part of the wage distribution—about 5 log points over the two year period. By contrast, moving from zero to 100% exposure causes wages to jump 5 log points in the exact month of treatment after Amazon's policy comes into place.

is a response to Amazon's $15 minimum wage policy, we perform an analysis of changes in the bunching of the wage distribution in response to the shock. If employers in the labor market were responding to an unrelated but simultaneous demand shock leading to higher wages, we would expect to find a more continuous set of adjustments by employers.

Figure 5 plots coefficients $\tilde{\beta}$ from regression equation 3 where the outcome variables are indicators for hourly wages falling within a specific wage bin. The figure shows that exposure to Amazon's policy is associated with a large increase in the probability of wages at exactly $15 an hour after the policy is announced. The probability of wages being exactly $15 has the highest estimated increase, at 17 percentage points, with smaller but statistically significant effects up to $18. For wages below $15, the largest drop comes wages that were at $11 prior to treatment—of 5 percentage points—with significant drops from $9 to $14 dollars. This evidence suggests employers were responding specifically to the Amazon announcement by targeting their announced wage, resulting in post-treatment postings concentrated at $15. Despite stemming from a different mechanism—strategic wage responses by employers to large employer wage changes—our finding of modest spillovers to wage bins higher than $15 is consistent with recent minimum wage papers that have found spillovers to wage levels above the statutory minimum wage being introduced. See, for example, Dube (2019), Engbom and Moser (2018), and Haanwinckel (2018).

6.1 Ruling out alternative explanations

Our empirical strategy leverages two sources of variation in a event-study or differences-in-differences approach to estimating wage spillovers in response to Amazon's minimum wage policy: variation in bite or fraction affected at the job level and variation from the exact timing of Amazon's policy. This evidence described above of wages bunching at exactly $15 undermines the notion that correlated but unrelated demand shocks drive the increase in non-Amazon wages immediately at the time of Amazon's policy. Still, we demonstrate robustness to a number of alternative hypotheses, which we discuss below.

Occupation-CZ-specific demand shocks Our baseline specification includes occupation-by-month and CZ-by-month fixed effects which rule out common demand shocks to specific occupations as well as sharp changes in wage policies or labor market conditions in specific commuting zones. For example, if a city or state minimum wage increase is implemented around the same time, our CZ-by-date fixed effects will absorb the effect of these policy changes. We can further show our results are robust to the inclusion of occupation-by-CZ-by-month fixed effects. In other words, we are able to exploit variation in pre-existing wage rates among employers advertising in the same occupation-CZ cell.

The results when including these controls are shown in column 2 of Table 2. Comparing column 1 to column 2 in Table 2 indicates that the key parameter is unchanged with the inclusion of occupation-by-CZ-by-month specific fixed effects.[15]

Employer decision to post wage As discussed in Section 4.1, about 20% of job postings contain information on the wage of the job. Amazon's announcement of their new minimum wage may have affected the posting behavior of firms. For example, firms may have had higher paying hourly jobs but were not including the wages for these jobs on their ads. Alternatively they may stop advertising the wage on jobs paid less than $15 in order to obscure the fact that they pay lower wages than Amazon. We conduct two additional analyses to test whether changes in the wage posting behavior of firms were driving the results reported in Figure 4. First in column 3 of Table 2, we directly include the share of ads that include wages for each employer in the regression to see how this affects our estimated coefficient $\tilde{\beta}$. Directly including the wage posting probability in the regression has no effect on the magnitude or precision of the estimate of Amazon's policy's impact.[16]

Employer-specific shocks In our strictest specification, column 4 of Table 2, we show that our results are robust to the inclusion of both occupation-by-date-by-month fixed effects and employer-by-month fixed effects. These latter controls insure we rely solely on variation within employers across differentially exposed occupation-by-CZ cells and within occupation-by-CZ cells across differentially exposed employers. The estimated coefficient on fraction affected times post is larger with the inclusion of these controls, but not statistically different from the coefficients in specifications without them. The results suggest the spillovers we estimate are not driven by unrelated shocks to employer's wage setting practices or employer-specific demand shocks.

Placebo treatment dates The validity of our research designs rests on the argument that less exposed and more exposed jobs experience a differential shock from Amazon's announcement of their new minimum wage, a form of non-random exposure to an exogenous shock (Borusyak and Hull, 2020). If the sharp increase in wages is driven by

[15] Appendix Figure C2 reports robustness of our event history design to the inclusion of these shocks.

[16] To the extent that changes in wage posting may be a secondary outcome of Amazon's policy, we look directly at the impact of the policy on wage posting behavior (results available upon request). It appears more exposed employers do gradually increase their tendency to post wages on advertised jobs, perhaps wishing to signal the presence of higher wage jobs. However, this change in posting behavior is delayed compared to the change in the wage distribution of ads with posted wages. While wages increase immediately, the likelihood of posting a wage increases more slowly, beginning a couple months after the policy change. Increases in the probability of posting the wage are inconsistent with the hypothesis that wage increases are due to exposed firms omitting the wage on lower wage jobs as these firms are increasing their wage posting over time.

Amazon's policy, then the degree of exposure to the policy should not predict an increase in wages at placebo treatment dates. Otherwise our effects may be driven by mean reversion or growth in wages at the lower end of the wage distribution. We confirm that this is not the case by splitting our observation period into rolling 4-month rolling windows covering months 12 to 9 months prior to treatment, 11 to 8, 10 to 7, and so on.

Figure 6 shows the results of this analysis. Each plotted coefficient represents the effect of exposure interacted with an indicator for postings in the last two months of the observation period. Coefficients are indexed by the last month of the observation period. Therefore, the coefficient indexed -9 represents the coefficient on exposure times an indicator for months -10 and -9 and expresses the increase in log hourly wages relative to a pre-period of months -11 and -12. The first observation window to include the actual treatment month in the post period is indexed by month 0. As shown in the figure, wage effects first become detectable only when the actual treatment month enters the post period of the difference-in-differences observation window. The largest effect appears in the month indexed 1 which is the first window with all post-treatment months in the actual post-treatment period. The effect drops off sharply once the entire 4-month window falls in the actual post-treatment period. That it does not fall to zero indicates reflects the steady increase in the treatment effect. As can be seen in Figure 4 the treatment effect begins at 5 log points and ends at 10 log points.

Functional form We check that our results are not sensitive to functional form by binning our treatment variable and using a non-parametric approach to estimating the treatment effect. We divide jobs into three groups: those that were fully exposed pre-treatment (100% of pre-treatment postings below $15), those that were partially exposed, and those that were not at all exposed (0% of postings below $15). Appendix Figure C4 plots the effect of being in the fully exposed group relative to the zero exposure group in blue and the effect of being in the partially exposed group relative to the zero exposure group in red. We then show robustness to dropping the zero exposure group in the event that they are a poor comparison group for the fully exposed group. Appendix Figure C5 shows these results: the effect of being in the fully exposed group relative to the partially exposed group, over time. The results from these additional analyses suggest that functional form does not drive our results and that our findings can be replicated using a more non-parametric approach.

6.2 Increases in worker-reported wages on Glassdoor

The results thus far strongly support non-Amazon employers responding to Amazon's minimum wage policy by adjusting their own wage-setting behavior. But Amazon's

policy as well as Walmart and Target's apply to incumbent workers, not just new hires.[17] To test whether spillovers from Amazon's minimum wage policy extend to incumbent employees at non-Amazon firms, we turn to an alternative data source and set of results: the effect of Amazon's policy on worker-reported wages at non-Amazon employers using data from Glassdoor.

As described in Section 4.2, Glassdoor is a two-sided online jobs platform used by workers to search and evaluate jobs and by employers to recruit. Glassdoor contains workers' reports on their salary and time rate of pay at a given employer. We re-estimate equation 2 using log worker-reported hourly wages as the outcome, including the same set of baseline controls.[18] Appendix Figure C6 depicts the results from this analysis. The results show a sharp increase in the wages workers at more exposed jobs report receiving beginning in the month of the policy change. Prior to the policy announcement, exposure is uncorrelated with wages. During the month of implementation of Amazon's pay increase, workers' reported wages at the average non-Amazon hourly job increase by around 5 log points. The effect persists and increases slightly to about 6 log points by the end of the post period. These results are remarkably consistent with the increase in advertised wages found using BGT data and confirm that changes in advertised wages resulted in changes in wages workers reported receiving.

7 Impacts of other retailer voluntary minimum wage and spillover moderators

Other retailers announced voluntary minimum wage increases in the period 2014-19 as shown in Figure 1. We use these wage shocks to further explore the nature of spillover effects. Our empirical strategy for Walmart and Target is identical to the one outlined above and in equation 2.[19] Our baseline again includes employer-by-occupation-by-CZ fixed effects as well as occupation-by-month and CZ-by-month fixed effects. Because Walmart and Target's $9 minimum wages were announced within one month of each other, we pool these announcements and analyze them jointly, excluding both Walmart and Target from the sample of employers analyzed.

Figure 7 shows the estimated spillover effects for the different voluntary minimum

[17]For more details on Amazon, Walmart, and Target's minimum wage policies, including which employees were affected by the increases, see Appendix A.

[18]Glassdoor provides the city of the posting, as opposed to county provided in the BGT data. We crosswalk cities to commuting zones. The analysis is restricted to commuting zones where Amazon (or Whole Foods) has advertised in the year prior to the policy change.

[19]We also tested the two successive announcements of wage increases by Costco during the study period. The results are similar in magnitude as those of the other retailers and hold up in the same robustness checks described for the other retailers.

wages announced by Walmart, Target and an additional retailer, Costco, over the study period. In all cases, the results indicate sharp increases in wages at more exposed jobs immediately in the month of the wage announcement. We perform similar robustness checks on these results as those reported for Amazon in Section 6. These are reported in Appendix C. For example, Figure C7 shows that the results are robust for the Walmart, Target, and Costco effects when occupation x CZ x FEs controls are included. Effects are moderated somewhat when additional controls are added for month and employer (Figure C8). We verify these spillover impacts by using data from Glassdoor that provides worker-reported wages in Figure C9. As in the case of Amazon, Walmart and Target wage announcements impact worker reported wages among other employers in their relevant labor markets. Figure C10 shows that for each of Walmart and Target's minimum wage, which range from $9 to $13, spillover effects in wages lead to large spikes right at the value of the announced minimum wage similar to the matching behavior we observed in the Amazon case. Finally, we test to see that the results for the other retailers withstand a placebo treatment test by splitting our observation period around each of the different wage announcements into 4-month rolling windows, similar to our robustness test for Amazon in Figure 6. Figure C11 confirms that our spillovers do not reflect mean reversion for low wage jobs but that wage effects appear in the exact month of treatment as opposed to at placebo treatment dates.

The Amazon, Walmart and Target announcements varied considerably over the time period, from a low of $9 voluntary wage announcement by Walmart in February 2015 to a high of the $15 announcement by Amazon in October 2018. To compare estimated spillovers across the different policy announcements, we normalize effect sizes by the average exposure of non-policy jobs at the time of the announcement. For example, 58% of non-Amazon hourly jobs had wage rates below $15 prior to Amazon's announcement while only 3% of jobs had wages below Walmart's $9 minimum.

We rescale spillovers to represent the effect of going from 0% to average pre-period exposure and plot our normalized effects against the level of announced voluntary minimum wage level. Results are reported in Figure 8. Spillovers increase monotonically in the level of the announced voluntary minimum wages announced by these three major employers. In Appendix Figure C14 we document the same monotonically increasing relationship between spillovers and the degree of exposure of non-policy firms to the large employer's minimum wage (ranging, again, from 3% for Walmart's 2015 announcement to 58% for Amazon's 2018 increase).

Cross-employer wage elasticities To interpret the magnitudes of our estimated wage spillover effects we compute cross-employer wage elasticities for each voluntary wage announcement. For a given percent increase in Amazon's hourly wages, what is the percent

increase for non-Amazon employers? For the more recent large employer minimum wage increases, we are able to measure the average increase across the pre- and post-period in the large employer's minimum wage. For these we compute the following wage elasticity with respect to the policy firm's average increase, where policy firms refer to Amazon, Walmart and Target while non-policy firms refer to all other firms:

$$\frac{\%\Delta \text{w}^{\text{non-policy firm}}}{\%\Delta \text{w}^{\text{policy firm}}} \tag{4}$$

For earlier policy changes, e.g., Walmart and Target's minimum wages prior to 2019, there are insufficient observations in Burning Glass Data to precisely measure the increase in their wages. Therefore we also compute the wage elasticity with respect to Amazon, Walmart, or Target's *statutory* wage increase, or the percent change in the large employer's stated minimum wage. The wage elasticity with respect to the policy firm's statutory MW increase is as follows:

$$\frac{\%\Delta \text{w}^{\text{non-policy firm}}}{\%\Delta \text{MW}^{\text{policy firm}}} \tag{5}$$

For example, in 2016, Walmart increased their \$9 minimum wage to \$10–a statutory increase of \$1. The vast majority of the statutory increases across voluntary wage announcements is \$1. For the first company-wide minimum wage, we take the midpoint of any previous minimum wage policies. For example, prior to their February, 2015 announcement of their \$9 minimum wage, Walmart set different minimum wage policies for stores depending on the state they were located in, ranging from \$8.05 to \$8.50. We take the midpoint of these minimum wages as the previous statutory minimum wage, or \$8.27. For Amazon, company minimum wages also varied by region prior to the announcement of their \$15 minimum wage, ranging from \$10 in Texas to \$13.50 in New Jersey.[20]

Figure 9 plots the cross-wage elasticities for each announcement. Elasticities with respect to the large employer's announced minimum wage increase range from about 0.02 (Walmart and Target's \$9 in 2015) to 0.43 (Target \$13). We also provide elasticities with respect to Amazon's average hourly wage increase and Target's average hourly wage increase after moving to a \$13 minimum wage in March 2019; they are 0.23 and 0.22, respectively. The interpretation is that for a 10% increase in Amazon or Target's wage, non-Amazon and non-Target employers increased their wages by just over 2%.

[20]Information we collected on regional wage policies is summarized in Appendix A.

Comparison to wage spillovers literature As a comparison, Staiger et al. (2010) estimate a cross-employer spillovers in the context of the Department of Veterans Affairs hospitals changing their wage policy and find elasticities ranging from 0.19 to 0.28.[21] An alternative benchmark is Hjort et al. (2019)'s estimate of cross establishment spillovers in multinationals after an increase in the headquarter country's minimum wage: an elasticity with respect to the headquarter's wage increase of approximately 0.43.[22] Thus, our estimated elasticity is very similar to this previous estimate despite differences in institutional context and industry. Our estimates suggest that voluntary wage increases by major employers elicited significant responses by other employers in their labor markets, growing with the size of those increases relative to prevailing labor market conditions.

8 Local labor market moderators of wage spillovers

Local labor market characteristics may play a role in how spillover effects propagate in CZs. The tightness of local labor markets, for example, are likely to amplify the spillover impacts of voluntary minimum wages. Alternatively, the level of statutory minimum wages in place at the locality may also moderate spillover impacts. Given the consistent pattern of wage spillovers documented for the retailers in Sections 6 and 7, we can aggregate the different announcements in order to measure factors that moderate the degree to which the wage shocks ripple across labor markets. To do so, we "stack" the separate wage announcements into one event study, with fixed effects for the individual announcements. We use two different measures of local labor market conditions at the CZ level at the time of the announcement to capture effects.

In Table 3, we investigate moderation of spillovers from employer minimum wage policies via local labor market characteristics. Column 1 shows moderation with the average CZ unemployment rate in the year prior to the policy. We find no relationship between the local unemployment rate and wage spillovers. This may be due to relatively low levels and minimal variation in unemployment rates during the time frame of our analysis.

The degree to which non-policy firms respond to wage shocks is also likely affected by the level of statutory minimum wages in the labor market where the voluntary announcement is made: a voluntary minimum wage that is much higher than that required by state minimum wage requirements will likely induce greater responses by other employers than in a labor market where the statutory minimum is already high. We test

[21] See Naidu et al. (2018) for a discussion of the elasticities in Staiger et al. (2010) and what they imply regarding monopsonistic competition in the labor market under different assumptions of labor supply elasticities and market share.

[22] Given that we are estimating propagation across employers rather than across establishments, making the Staiger et al. (2010) estimate a closer reference point.

this by interacting our key exposure variable with a measure of the local minimum wage, measured as the maximum of applicable federal, state, county, or city minimum wage. Column 2 of Table 3 presents the estimated interaction term. Wage spillovers are larger in areas where the local minimum wage is below that of the large employer voluntary minimum wage. We observe spillovers in areas with local minimum wages at least as high as the large employer minimum wage. Spillovers to higher wage bins as shown in our bunching results (see Figure 5 and Appendix Figure C10) may explain wage effects in areas with higher local minimum wages.

9 Wage spillovers and employment effects in the CPS

The results above indicate that voluntary minimum wages adopted by major retailers significantly impacted the wages posting and reported worker earnings of other employers in their labor markets. If so, we would expect that those employers needed to adjust other policies in response to matching the new voluntary minimums. The data used to estimate the above relationships provide information on job postings (BGT) or reported worker earnings (Glassdoor) but do not provide information on employment levels. In order to explore the impact of spillovers, we use the CPS ORG to examine employment effects

To estimate the effects of employer minimum wages on both wages and employment, we turn to the CPS ORG. The CPS ORG does not solicit the identity of an individual's employer. We therefore define exposure at the job level by calculating the fraction of workers earning below $15 an hour at 4-digit-occupation-by-CZ level. Although this limitation means that we cannot exploit variation in exposure across employers within-occupation-CZ, we show that we are still able to detect sizable spillovers from large employers' wage setting policies in the CPS using variation in bite as defined above and exploiting the precise timing of employer minimum wage announcements. We first present the results for Amazon's minimum wage.

We examine both wage and employment spillover effects as well as aggregate wage and employment effects of Amazon's policy. To examine spillovers, we exclude the 3-digit NAICS code that Amazon and Whole Foods fall under: electronic shopping and mail-order houses and grocery and convenience stores. Throughout these analyses, we restrict our sample to individuals in identifiable counties in the commuting zones in which Amazon advertises, based on the geographic information from postings in the BGT data. We further restrict our sample to individuals between the ages of 25 and 65.

9.1 Wage effects

Do we find comparable evidence of the spillover impacts of voluntary wage increases found using BGT and Glassdoor in the CPS data? For our wage analysis, we focus on those who are employed and exclude those who report usually working less than three hours a week. We estimate wage effects using a similar estimating equation as equation 2. In addition to occupation-by-CZ, occupation-by-month, and CZ-by-month fixed effects, we include controls for education, a quadratic in experience, part-time vs. full-time status, marital status, gender, and race and ethnicity. Our key dependent variable is the worker's log hourly wage, where hourly wage is defined as the usual weekly earnings divided by usual hours worked per week in the individual's primary job. The results are reported in Figure 10. Consistent with our prior two sets of analyses in BGT and Glassdoor, we observe a large increase in wages right at the time of Amazon's minimum wage announcement. The magnitudes are comparable to our estimates in Glassdoor and BGT. The average job experiences an 6 log point increase in wages over the post period, relative to the pre-period.

9.2 Employment effects

We begin by reporting the employment effects of Amazon's minimum wage announcement. Our empirical strategy leverages variation in bite by occupation or last occupation of the unemployed to estimate wage effects. In the CPS ORG, this variable is not well defined for those not in the labor force (only 6.9% report an occupation). We therefore follow Derenoncourt and Montialoux (2021) in measuring the employment effects by looking at the effect of the policy announcement on the probability of being employed vs. unemployed.[23] Figure 11 reports the results. All of the point estimates in the post-period on exposure interacted with month are negative and four out of the 12 post-treatment estimates are significantly different from zero. Our difference-in-differences estimates pooling the pre- and post-treatment periods suggest a reduction in the probability of employment by 0.8 percentage points relative to the pre-treatment period.

We summarize the employment effects across all employer policies in Figure 12, which shows that like the wage effects, employment effects are more pronounced the larger the level of the major employer's minimum wage (Appendix Figure C15 shows the analogous figure for the bite).

Figure 13 summarizes the wage and employment effects across all voluntary wage shock events. We provide the implied employment elasticities with respect to own-wage

[23] If Amazon's announcement causes individuals not in the labor force to start searching for work and therefore be categorized as "unemployed," this could explain increases in the probability of unemployment.

(our estimated employment effect divided by our estimated wage effect) in Table 4. Table 5 reports the wage and employment effects keeping in the industry of the policy firms. The results are virtually unchanged, indicating that slightly negative employment results hold in the aggregate.

We situate our estimated employment elasticities for two of the voluntary wage shocks amongst other estimates from the minimum wage and monopsony literature in Figure 14. Our estimates are well within the estimates of the larger literature, implying relatively small negative employment effects on net arising from the spillover effects. The small net effects we estimate may result from heterogeneous effects across leading firms who expand employment after emulating large retailers' minimum wage and a set of follower firms that lose workers to leading firms (as in Berger et al. (2019)).

10 Conclusion

This study examines wage spillover effects from recent wage policy changes by large low-wage employers, Amazon and Walmart. We use data on online vacancy postings as well as data from a large job review platform to document evidence of wage policy changes at these employers and estimate broader spillover effects in the labor market. Using a measure of the exposure of other employers operating in the same labor market, we estimate substantial spillover effects of both Amazon, Walmart, and Target's policies. In the case of Amazon, which raised its minimum wage to $15 in 2018, the cross-employer wage elasticity is approximately 0.26, in line with a limited set of other recent estimates of wage spillovers (Staiger et al., 2010; Hjort et al., 2019; Willén, 2019).

We turn to the CPS to investigate the employment effects of large employer minimum wages. We find slight declines in employment in response to voluntary employer minimum wage policies, with own-wage employment elasticities ranging from -.04 to -.13. These estimates are highly similar to those from the recent minimum wage literature. We hypothesize that small aggregate employment responses may mask larger employment responses at individual firms as workers reallocate across firms within the sector. Our ongoing work explores this heterogeneity as well as additional local labor market mediators of cross-employer wage policy transmission.

References

Abadie, A., Athey, S., Imbens, G. W., and Wooldridge, J. (2017). When should you adjust standard errors for clustering? NBER Working Paper.

Abbruzzese, Jason, D. I. and Cappetta, M. (2018). "Amazon cuts to bonuses lead to questions about wage hike.".

Abowd, J. M., Kramarz, F., and Margolis, D. N. (1999). High wage workers and high wage firms. *Econometrica*, 67(2):251–333.

Akerlof, G. A. and Yellen, J. L. (1990). The fair wage-effort hypothesis and unemployment. *The Quarterly Journal of Economics*, 105(2):255–283.

Amazon.com (2017). *Amazon and Whole Foods Market Announce Acquisition to Close This Monday, Will Work Together to Make High-Quality, Natural and Organic Food Affordable for Everyone.* Amazon.com. https://press.aboutamazon.com/news-releases/news-release-details/amazon-and-whole-foods-market-announce-acquisition-close-monday.

Amazon.com (2019). *2018 Annual Report.* Amazon.com. https://ir.aboutamazon.com/static-files/0f9e36b1-7e1e-4b52-be17-145dc9d8b5ec.

Amazon.com (2020). *Amazon.com Announces Fourth Quarter Sales up 21Billion.* Amazon.com. https://ir.aboutamazon.com/news-releases/news-release-details/amazoncom-announces-fourth-quarter-sales-21-874-billion.

Angrist, J. D. and Pischke, J.-S. (2008). *Mostly harmless econometrics: An empiricist's companion.* Princeton University Press: Princeton, NJ.

Azar, J., Berry, S., and Marinescu, I. E. (2019). Estimating labor market power. Working Paper.

Azar, J., Marinescu, I., and Steinbaum, M. I. (2017). Labor market concentration. NBER Working Paper.

Azar, J. A., Marinescu, I., Steinbaum, M. I., and Taska, B. (2018). Concentration in US labor markets: Evidence from online vacancy data. NBER Working Paper.

Bailey, M. J., DiNardo, J., and Stuart, B. A. (2020). The economic impact of a high national minimum wage: Evidence from the 1966 fair labor standards act. Technical report, National Bureau of Economic Research.

Barth, E., Bryson, A., Davis, J. C., and Freeman, R. (2016). It's where you work: Increases in the dispersion of earnings across establishments and individuals in the united states. *Journal of Labor Economics*, 34(S2):S67–S97.

Bartik, A. W., Bertrand, M., Lin, F., Rothstein, J., and Unrath, M. (2020). Measuring the labor market at the onset of the covid-19 crisis. Technical report, National Bureau of Economic Research.

Bayer, P., Ross, S. L., and Topa, G. (2008). Place of work and place of residence: Informal hiring networks and labor market outcomes. *Journal of Political Economy*, 116(6):1150–1196.

Beaudry, P., Green, D. A., and Sand, B. M. (2018). In search of labor demand. *American Economic Review*, 108(9):2714–57.

Becker, G. (1970). *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education*. National bureau of economic research publications: General series. Columbia University Press.

Bergen, M. and Eidelson, J. (2018). "Inside Google's Shadow Workforce." *Bloomberg*, 7/25/18, (https://www.bloomberg.com/news/articles/2018-07-25/inside-google-s-shadow-workforce).

Berger, D. W., Herkenhoff, K. F., and Mongey, S. (2019). Labor market power. Technical report, National Bureau of Economic Research.

Bewley, T. F. and Bewley, T. F. (2009). *Why wages don't fall during a recession*. Harvard University Press: Cambridge, MA.

Borusyak, K. and Hull, P. (2020). Non-random exposure to exogenous shocks: Theory and applications. Technical report, National Bureau of Economic Research.

Breza, E., Kaur, S., and Shamdasani, Y. (2017). The morale effects of pay inequality. *The Quarterly Journal of Economics*, 133(2):611–663.

Budd, J. W. (1992). The determinants and extent of uaw pattern bargaining. *ILR Review*, 45(3):523–539.

Caldwell, S. (2018). Outside Options, Bargaining, and Wages. Working Paper.

Caldwell, S. and Danieli, O. (2018). Outside Options in the Labor Market. Working Paper.

Canal, E. (2018). Why Amazon's Minimum Wage Hike Could Shock the Already Tight U.S. Labor Market. Library Catalog: www.inc.com Section: Hiring.

Card, D. (1992). Using regional variation in wages to measure the effects of the federal minimum wage. *ILR Review*, 46(1):22–37.

Card, D., Cardoso, A. R., Heining, J., and Kline, P. (2018). Firms and labor market inequality: Evidence and some theory. *Journal of Labor Economics*, 36(S1):S13–S70.

Card, D. and Krueger, A. B. (1993). Minimum wages and employment: A case study of the fast food industry in new jersey and pennsylvania. Technical report, National Bureau of Economic Research.

Card, D., Mas, A., Moretti, E., and Saez, E. (2012). Inequality at work: The effect of peer salaries on job satisfaction. *American Economic Review*, 102(6):2981–3003.

Carnevale, A. P., Jayasundera, T., and Repnikov, D. (2014). Understanding online job ads data. *Georgetown University, Center on Education and the Workforce, Technical Report (April)*.

Cengiz, D., Dube, A., Lindner, A., and Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics*, 134(3):1405–1454.

Chetty, R., Friedman, J. N., Hendren, N., and Stepner, M. (2020). Real-time economics: A new platform to track the impacts of covid-19 on people, businesses, and communities using private sector data. *NBER Working Paper*, 27431.

Collins, W. J. and Niemesh, G. T. (2017). Unions and the Great Compression of Wage Inequality in the United States at Mid-Century. Working Paper.

Corporation, T. (2020a). From 10-K: Target Corporation.

Corporation, T. (2020b). Target Increases Starting Wage to $15; Thanks Frontline Team Members with Recognition Bonus. Library Catalog: corporate.target.com.

Costco Wholesale Corporation (2020). CORPORATE PROFILE | Costco Wholesale Corporation.

David, H., Manning, A., and Smith, C. L. (2016). The contribution of the minimum wage to us wage inequality over three decades: a reassessment. *American Economic Journal: Applied Economics*, 8(1):58–99.

Deming, D. and Kahn, L. B. (2018a). Skill requirements across firms and labor markets: Evidence from job postings for professionals. *Journal of Labor Economics*, 36(S1):S337–S369.

Deming, D. and Kahn, L. B. (2018b). Skill requirements across firms and labor markets: Evidence from job postings for professionals. *Journal of Labor Economics*, 36(S1):S337–S369.

Deming, D. J. and Noray, K. L. (2018). STEM Careers and Technological Change. NBER Working Paper.

Derenoncourt, E. (2018). Job satisfaction, pay, and inequality in US firms: Evidence from Glassdoor. Working Paper.

Derenoncourt, E. and Montialoux, C. (2021). Minimum Wages and Racial Inequality*. *The Quarterly Journal of Economics*. qjaa031.

D'Innocenzio, A. (2017). Target is raising minimum hourly wage to $15 by end of 2020. Library Catalog: apnews.com Section: Target Corp.

Drenik, A., Jäger, S., Plotkin, P., and Schoefer, B. (2020). Paying Outsourced Labor: Direct Evidence from Linked Temp Agency-Worker-Client Data. Working Paper.

Dube, A. (2019). ?impacts of minimum wages: Review of the international evidence. *Independent report, https://www. gov. uk/government/publications/impacts-of-minimum-wages-review-of-the-international-evidence*.

Dube, A., Giuliano, L., and Leonard, J. (2019). Fairness and frictions: The impact of unequal raises on quit behavior. *American Economic Review*, 109(2):620–63.

Dube, A., Jacobs, J., Naidu, S., and Suri, S. (2020). Monopsony in online labor markets. *American Economic Review: Insights*, 2(1):33–46.

Dube, A. and Lindner, A. S. (2020). City limits: What do local-area minimum wages do? Technical report, National Bureau of Economic Research.

Dube, A., Manning, A., and Naidu, S. (2017). Monopsony and employer mis-optimization account for round number bunching in the wage distribution. *Unpublished manuscript*.

Dunlop, J. T. (1957). The task of contemporary wage theory. In *The theory of wage determination*, pages 3–27. Springer.

Dunlop, J. T. (1993). *Industrial relations systems*. Harvard Business Sch.

Dustmann, C., Lindner, A., Schönberg, U., Umkehrer, M., and Vom Berge, P. (2019). Reallocation effects of the minimum wage: Evidence from germany. Technical report, mimeo.

Dustmann, C., Ludsteck, J., and Schönberg, U. (2009). Revisiting the german wage structure. *The Quarterly Journal of Economics*, 124(2):843–881.

Engbom, N. and Moser, C. (2018). Earnings inequality and the minimum wage: Evidence from brazil. *Federal Reserve Bank of Minneapolis-Opportunity and Inclusive Growth Institute Working Paper*, 7:18–50.

Farber, H. (2005). Nonunion wage rates and the threat of unionization. *ILR Review*, 58(3):335–352.

Fargo, W. (2020). Wells Fargo Today. page 2.

Fehr, E., Goette, L., and Zehnder, C. (2009). A behavioral account of the labor market: The role of fairness concerns. *Annu. Rev. Econ.*, 1(1):355–384.

Fehr, E. and Schmidt, K. M. (1999). A theory of fairness, competition, and cooperation. *The Quarterly Journal of Economics*, 114(3):817–868.

Ferguson, J.-P. (2016). Racial diversity and union organizing in the united states, 1999–2008. *ILR Review*, 69(1):53–83.

Folk, M. and Gilchrist, P. (2017). Wells Fargo to Raise Minimum Hourly Pay Rate to $15, Target $400 Million in 2018 Philanthropic Contributions, Including Expanded Support for Small Businesses and Homeownership. Library Catalog: www.businesswire.com.

Freeman, R. and Medoff, J. (1985). *What do unions do?* Basic Books.

Freyaldenhoven, S., Hansen, C., and Shapiro, J. M. (2018). Pre-event trends in the panel event-study design. NBER Working Paper.

Haanwinckel, D. (2018). Supply, demand, institutions, and firms: A theory of labor market sorting and the wage distribution. *Unpublished manuscript*.

Hamilton, I. A. (2018). "Amazon is raising its minimum wage to $15 following pressure from Bernie Sanders." Business Insider, 10/2/18 (https://www.businessinsider.com/amazon-raises-minimum-wage-to-15-dollars-2018-10).

Hanbury, M. (2018). Costco just became an even more appealing place to work after raising its minimum wage to $14 an hour.

Hanbury, M. (2019). Costco is raising its minimum wage to $15 as the war for talent rages on.

Harasztosi, P. and Lindner, A. (2019). Who pays for the minimum wage? *American Economic Review*, 109(8):2693–2727.

Hazell, J. and Taska, B. (2019). Downward Rigidity in the Wage for New Hires. Working Paper.

Hellerstein, J. K., McInerney, M., and Neumark, D. (2011). Neighbors and coworkers: The importance of residential labor market networks. *Journal of Labor Economics*, 29(4):659–695.

Hershbein, B. and Kahn, L. B. (2018). Do recessions accelerate routine-biased technological change? evidence from vacancy postings. *American Economic Review*, 108(7):1737–72.

Hicks, J. R. et al. (1974). *Crisis in Keynesian economics*. Basic Books.

Hjort, J., Li, X., and Sarsons, H. (2019). Uniform Across-Country Wage-Setting in Multinationals. Working Paper.

Jäger, S., Schoefer, B., Young, S. G., and Zweimüller, J. (2018). Wages and the Value of Nonemployment. NBER Working Paper.

Jardim, E., Long, M. C., Plotnick, R., Van Inwegen, E., Vigdor, J., and Wething, H. (2017). Minimum wage increases, wages, and low-wage employment: Evidence from seattle. Technical report, National Bureau of Economic Research.

Kahn-Lang, A. and Lang, K. (2018). The promise and pitfalls of differences-in-differences: Reflections on '16 and pregnant'and other applications. NBER Working Paper.

Kahneman, D., Knetsch, J. L., and Thaler, R. (1986). Fairness as a constraint on profit seeking: Entitlements in the market. *American Economic Review*, pages 728–741.

Kalleberg, A. L. (2013). *Good Jobs, Bad Jobs*. New York: Russell Sage Foundation.

Katz, L. F. and Krueger, A. B. (1992). The effect of the minimum wage on the fast-food industry. *ILR Review*, 46(1):6–21.

Katz, L. F. and Krueger, A. B. (2019). The rise and nature of alternative work arrangements in the united states, 1995-2015.

Kavilanz, P. and Business, C. (2020). Target is raising its minimum wage to $15 an hour in July. Library Catalog: www.cnn.com.

Kessler, D. P. and Katz, L. F. (2001). Prevailing wage laws and construction laborc markets. *ILR Review*, 54(2):259–274.

Keynes, J. M. (1930). "The Question of High Wages." The Political Quarterly (reprinted in Moggridge, Donald (ed.) The Collective Writings of John Maynard Keynes, Volume XX, London: MacMillan Inc., 1981).

Kline, P., Petkova, N., Williams, H., and Zidar, O. (2019). Who profits from patents? rent-sharing at innovative firms. *The Quarterly Journal of Economics*, 134(3):1343–1404.

Krueger, A. (2018). Luncheon Address at Kansas City Federal Reserve's *Changing Market Structures and Implications for Monetary Policy Conference*.

Lane, B. (2018). U.S. Bank becomes latest bank to increase wages, hand out bonuses after tax reform. Library Catalog: www.housingwire.com Section: Mortgage.

Lathrop, Y. (2018). Impact of the Fight for $15: $68 Billion in Raises, 22 Million Workers. *New York: National Employment Law Project*.

Layne, N. (2014). Exclusive: U.S. minimum wage hikes to affect 1,400-plus Walmart stores. *Reuters*.

Manning, A. (2003). *Monopsony in motion: Imperfect competition in labor markets*. Princeton University Press.

Marshall, A. (1887). "A Fair Rate of Wages." In Pigou, Arthur (ed.), Memorials of Alfred Marshall. (New York: Kelley and Millman, Reprint 1957), pp. 212-226.

Matsudaira, J. D. (2014). Monopsony in the low-wage labor market? evidence from minimum nurse staffing regulations. *Review of Economics and Statistics*, 96(1):92–102.

Mergent (2020). Company overview: Target. Company Reports, FTSE Russell.

Minaya, E. and Trentmann, N. (2018). Amazon Unleashes More Wage Pressure With Hourly Wage Boost.

Moretti, E. (2011). Local labor markets. In *Handbook of labor economics*, volume 4, pages 1237–1313. Elsevier.

Murphy, B. (2018). "Amazon Says It's Fixing the Big Problem With Its 15 Pay Promise (And Just in Time)." Inc., 10/11/18. (https://www.inc.com/bill-murphy-jr/amazon-just-caught-a-key-problem-in-its-plan-to-pay-workers-15-an-hour-heres-what-they-say-theyre-doing-instead).

Nagele-Piazza, L. (2018). Amazon Workers Will Soon Earn $15 an Hour. Library Catalog: www.shrm.org.

Naidu, S., Posner, E. A., and Weyl, G. (2018). Antitrust remedies for labor market power. *Harv. L. Rev.*, 132:536.

NRF (2019). Top 100 Retailers 2019. Library Catalog: nrf.com.

Oi, W. (1983). The fixed employment costs of specialized labor. In *The measurement of labor cost*, pages 63–122. University of Chicago Press.

Osterman, P. and Shulman, B. (2011). *Good Jobs America: Making Work Better for Everyone*. New York: Russell Sage Foundation.

Piketty, T. and Saez, E. (2003). Income inequality in the united states, 1913–1998. *The Quarterly Journal of Economics*, 118(1):1–41.

Putnam, R. D. (2000). Bowling alone: America's declining social capital. In *Culture and politics*, pages 223–234. Springer.

Putnam, R. D. (2007). E pluribus unum: Diversity and community in the twenty-first century the 2006 Johan Skytte Prize Lecture. *Scandinavian political studies*, 30(2):137–174.

Putnam, R. D., Leonardi, R., and Nanetti, R. Y. (1993). *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton University Press: Princeton, NJ.

Robinson, J. (1969). *The economics of imperfect competition*. Springer.

Rolf, D. (2015). *The Fight for $15: The Right Wage for a Working America*. New Press, The.

Romano, B. (2018). Costco boosting hourly wages for 130,000 U.S. employees.

Rosenfeld, J. and Kleykamp, M. (2009). Hispanics and organized labor in the united states, 1973 to 2007. *American Sociological Review*, 74(6):916–937.

Roth, J. (2018). Pre-test with Caution: Event-study Estimates After Testing for Parallel Trends. Working Paper.

Rothacker, R. and Roberts, D. (2018). Wells Fargo workers to get raise, CEO says | Charlotte Observer.

Rudden, J. (2020). Number of employees at Wells Fargo from 2009 to 2019. Library Catalog: www.statista.com.

Ruggles, Steven, Sarah Flood, Ronald Goeken, Josiah Grover, Erin Meyer, Jose Pacas, and Matthew Sobek (1990). IPUMS USA: Version 8.0 [dataset]. Minneapolis, MN: IPUMS, 2018. https://doi.org/10.18128/D010.V8.0.

Rupasingha, A., Goetz, S. J., and Freshwater, D. (2006). The production of social capital in us counties. *The Journal of Socio-economics*, 35(1):83–101.

Samuelson, P. and Collection, K. M. R. (1947). *Foundations of Economic Analysis*. Harvard economic studies. Harvard University Press.

Sarsons, H. (2017). Interpreting Signals in the Labor Market: Evidence from Medical Referrals. Working Paper.

Scheiber, N. (2017). "How Uber Uses Psychological Tricks to Push its Drivers' Buttons." *New York Times*, 4/2/17, (https://www.nytimes.com/interactive/2017/04/02/technology/uber-drivers-psychological-tricks).

Schubert, G., Stansbury, A., and Taska, B. (2019). Getting labor markets right: Outside options and occupational mobility. Technical report, working paper.

Schubert, G., Stansbury, A., and Taska, B. (2020). Employer concentration and outside options. Technical report, mimeo, Harvard University.

Settembre, J. (2018). Amazon raises minimum wage to $15 – plus 8 other companies that have made similar moves. Library Catalog: www.marketwatch.com Section: Personal Finance.

Shambaugh, J., Nunn, R., Liu, P., and Nantz, G. (2017). Thirteen facts about wage growth. *The Hamilton Project*.

Slichter, S., Healy, J., and Livernash, E. (1960). *The Impact of Collective Bargaining on Management. By S.H. Slichter, James J. Healy, E. Robert Livernash*. Brookings Institution.

Slichter, S. H. (1950). Notes on the structure of wages. *The Review of Economics and Statistics*, pages 80–91.

Solow, R. M. (1990). *The labor market as a social institution*. Blackwell: Cambridge, MA.

Song, J., Price, D. J., Guvenen, F., Bloom, N., and Von Wachter, T. (2019). Firming up inequality. *The Quarterly journal of economics*, 134(1):1–50.

Staff, D. O. (2018). Amazon Raises Minimum Wage to $15 for All U.S. Employees. Library Catalog: blog.aboutamazon.com.

Staiger, D. O., Spetz, J., and Phibbs, C. S. (2010). Is there monopsony in the labor market? evidence from a natural experiment. *Journal of Labor Economics*, 28(2):211–236.

Sullivan, D. (1989). Monopsony power in the market for nurses. *The Journal of Law and Economics*, 32(2, Part 2):S135–S178.

Thomas, L. (2017a). "As Wal-Mart blitzes Internet retail, debate rages over company's impact on US wages." CNBC, 4/22/2017 (https://www.cnbc.com/2017/04/20/wal-mart-still-front-and-center-of-debate-over-minimum-wages.html).

Thomas, L. (2017b). "Whole Foods shareholders vote in approval of Amazon deal." CNBC, 8/2417. (https://www.cnbc.com/2017/08/23/whole-foods-shareholders-vote-in-approval-of-amazon-deal).

U.S. Bureau of Labor Statistics (2019). "Civilian labor force, by age, sex, race, and ethnicity.".

Walker, D. (2020). Wells Fargo raises minimum wage | Fox Business.

Walmart (2018). "Walmart to Raise U.S. Wages, Provide One-Time Bonus and Expand Hourly Maternity and Parental Leave." https://corporate.walmart.com/newsroom/2018/01/11/walmart-to-raise-u-s-wages-provide-one-time-bonus-and-expand-hourly-maternity-and-parental-leave.

Walmart (2020). "Our Business." https://corporate.walmart.com/our-story/our-business/.

Weber, L. and Silverman, R. E. (2015). "On-Demand Workers: 'We are not Robots'. Wall Street Journal, 1/27/15. (https://www.wsj.com/articles/on-demand-workers-we-are-not-robots-1422406524).

Weil, D. (2006). Crafting a progressive workplace regulatory policy: Why enforcement matters. *Comp. Lab. L. & Pol'y J.*, 28:125.

Weil, D. (2014). *The fissured workplace*. Harvard University Press: Cambridge, MA.

Weil, D. (2017a). Income inequality, wage determination, and the fissured workplace. *After Piketty: The Agenda for Economics and Inequality, Harvard University Press, Cambridge*, pages 209–231.

Weil, D. (2017b). ."Income Inequality, Wage Determination, and the Fissured Workplace." in Bradford DeLong, Heather Boushey, and Marshall Steinbaum, (eds) *After Piketty: The agenda for economics and inequality*. (Cambridge, MA: Harvard University Press, pp. 209-231.

Western, B. and Rosenfeld, J. (2011). Unions, norms, and the rise in us wage inequality. *American Sociological Review*, 76(4):513–537.

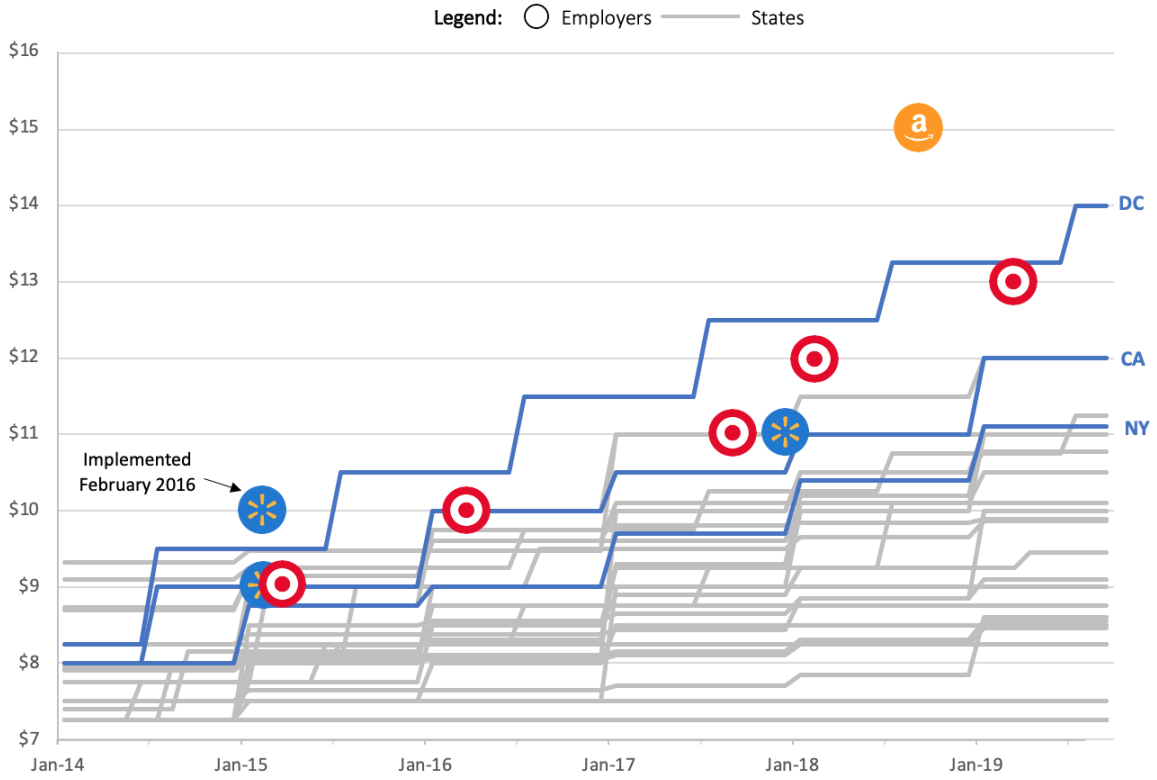
Wiener-Bronner, D., Isidore, C., and Business, C. (2018). Amazon announces $15 minimum wage for all US employees. Library Catalog: www.cnn.com.

Wiese, K. (2018). "Amazon to Raise Minimum Wage to 15 for All U.S. Workers." New York Times, 11/2/18 (https://www.nytimes.com/2018/10/02/business/amazon-minimum-wage).

Willén, A. (2019). Decentralization of wage determination. evidence from a national teacher reform. Technical report.

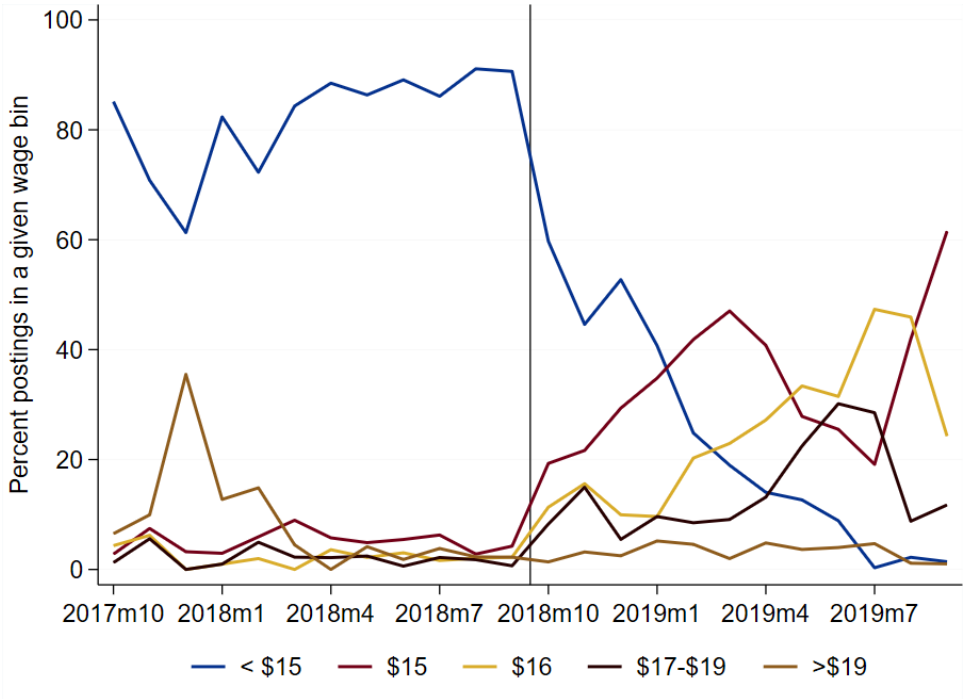
Wozniak, A. (2010). Are college graduates more responsive to distant labor market opportunities? *Journal of Human Resources*, 45(4):944–970.

Figure 1: Voluntary and statutory minimum wages, 2014-2019



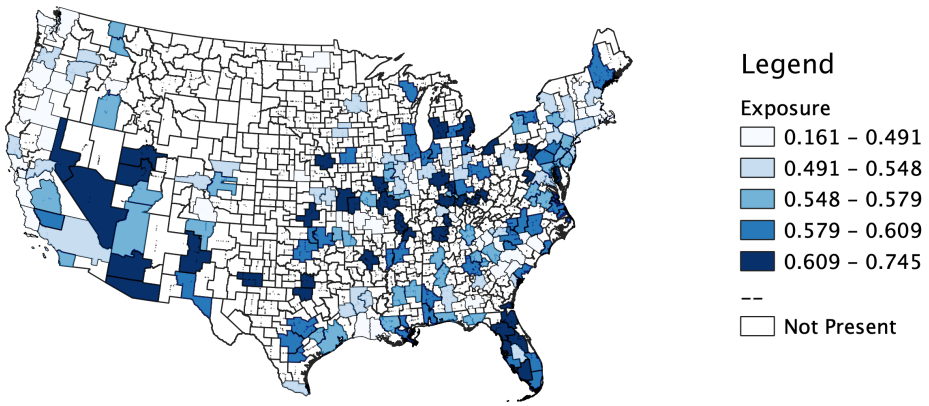
Notes: This figure plots voluntary employer minimum wage increases that have been announced in the US between 2015 and 2019. Gray lines indicate state minimum wages above the federal minimum wage of $7.25. Select states are shown in blue. Employer logos show treatment firms (Walmart, Target, and Amazon/Whole Foods from left to right) in the months they announced minimum wage increases. Target's 2017 announcement included increases to $15 over multiple years. Walmart's 2015 announcement of a $9 minimum wage was also accompanied by a statement they would increase to $10 by the following year. *Source*: National Employment Law Project and authors' research.

Figure 2: Percentage of Amazon job ads below or above $15, 2017-2019



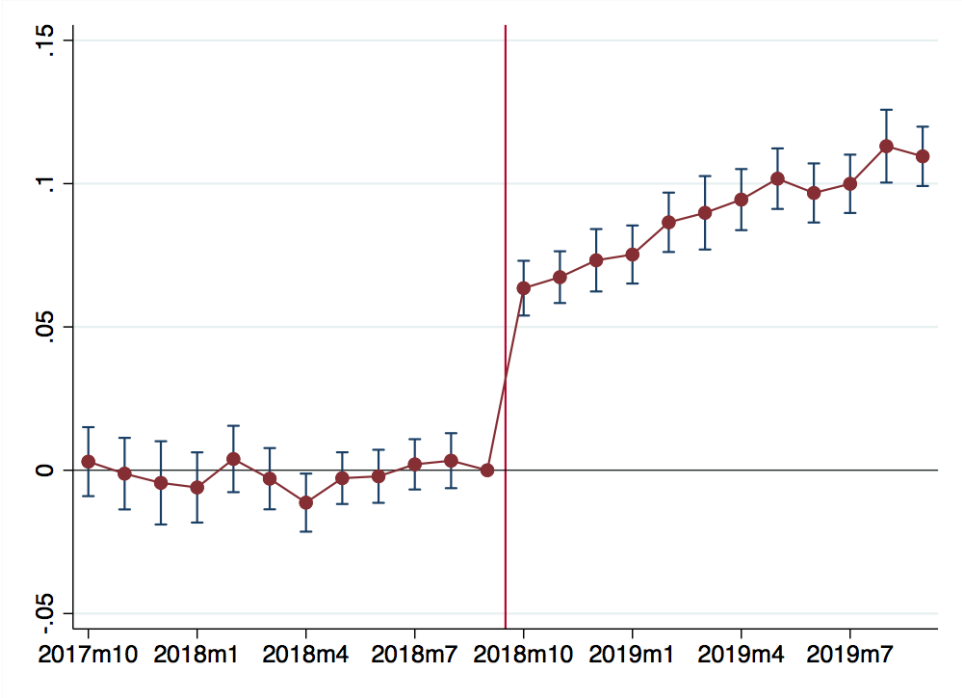
Notes: Percentage of Amazon job ads at wage bins below, at, or above $15. Sample restricted to postings with valid wage data and hourly rate of pay, employer name, county, and occupation. Whole Foods was acquired by Amazon in August 2017 and is included in the sample. *Source*: Burning Glass Technologies online vacancy data.

Figure 3: Average exposure to Amazon's MW by CZ



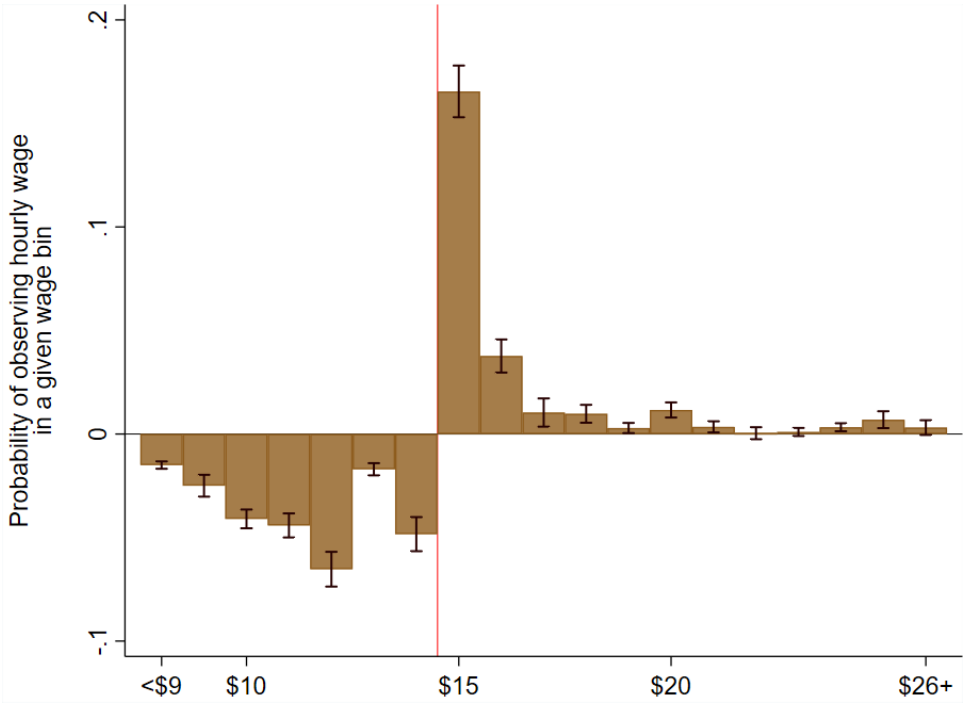
Notes: This figure shows the fraction of postings by employer-by-occupation cells that were below $15 at the commuting zone level in the year prior to Amazon's October 2018 minimum wage announcement. Sample restricted to non-Amazon postings with valid wage data and hourly rate of pay, employer name, county, and occupation. *Source*: Burning Glass Technologies online vacancy data.

Figure 4: Spillovers in advertised wages from Amazon's $15 MW, 2018



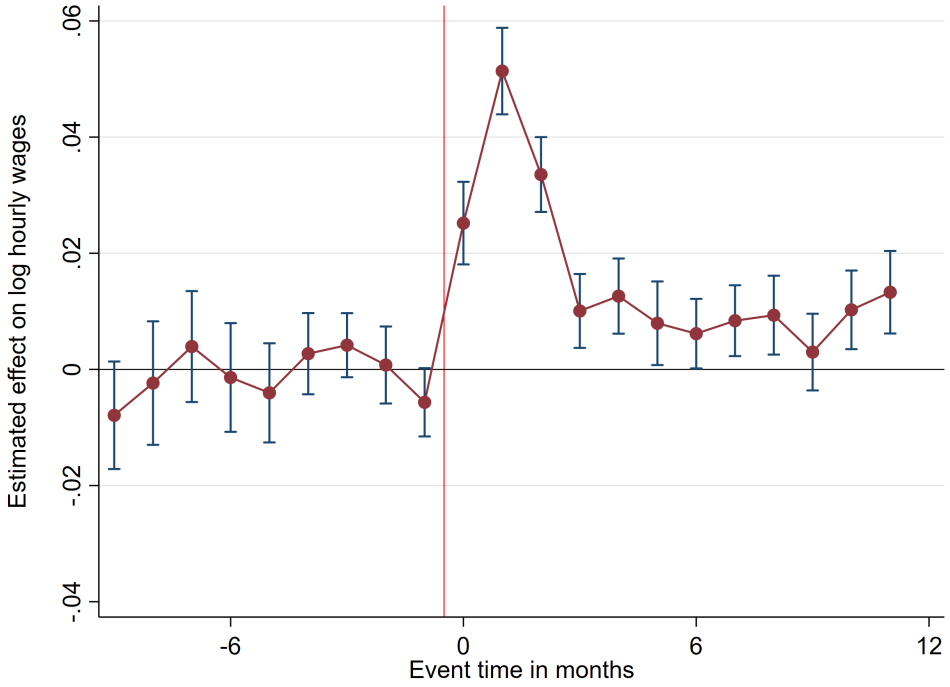
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below $15 in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure 5: Amazon spillovers concentrated at $15



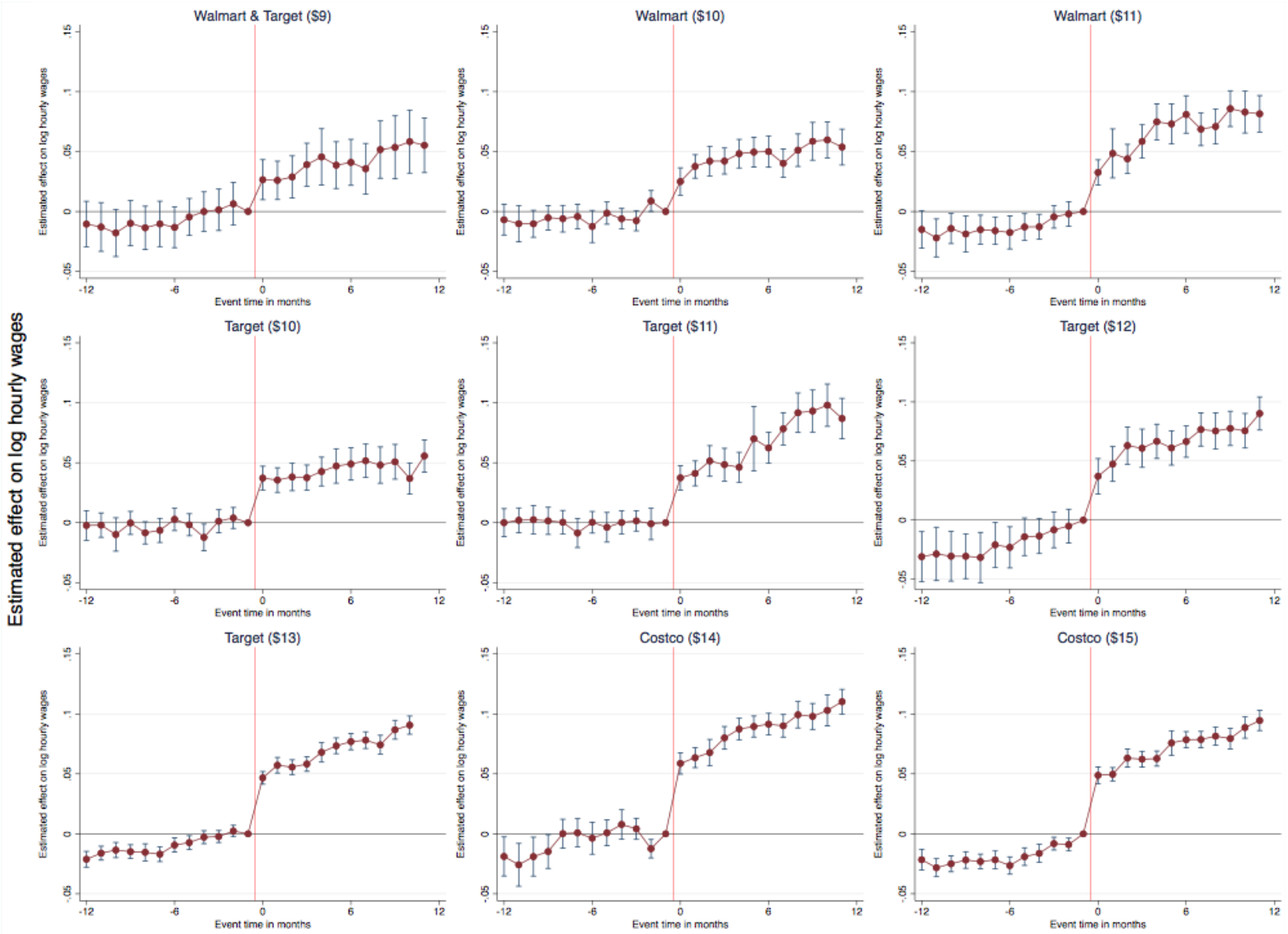
Notes: This figure plots the coefficients from linear probability regressions of hourly wages being in a given wage bin on the interaction between job-level exposure to Amazon's policy for non-Amazon employers and an indicator for post-October-2018. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below $15 in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure 6: Null effects of Amazon's $15 at placebo treatment dates



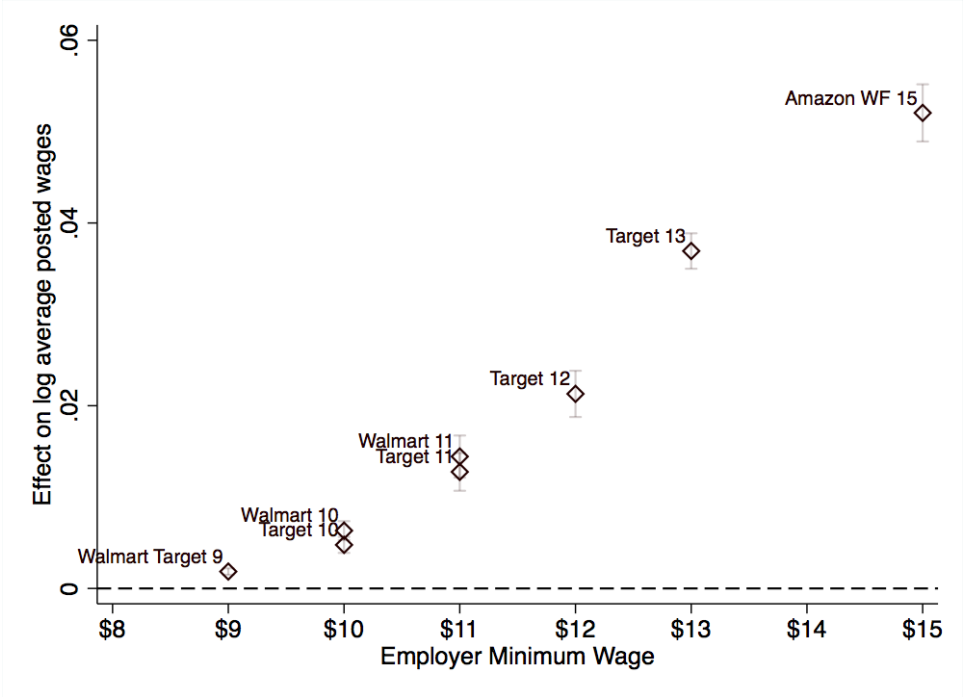
Notes: This figure plots the regression coefficients on the interaction between job-level exposure to Amazon's policy for non-Amazon employers and an indicator for post-treatment for placebo treatment dates, using a 4-month observation window. Coefficients are indexed by the last month of the observation period. For example, the coefficient at date equal to 0 is the coefficient on exposure interacted with an indicator for one month before zero and zero (the first month of treatment). Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below $15 in the year before October 2018. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure 7: Spillovers in advertised wages from Walmart, Target, and Costco MWs



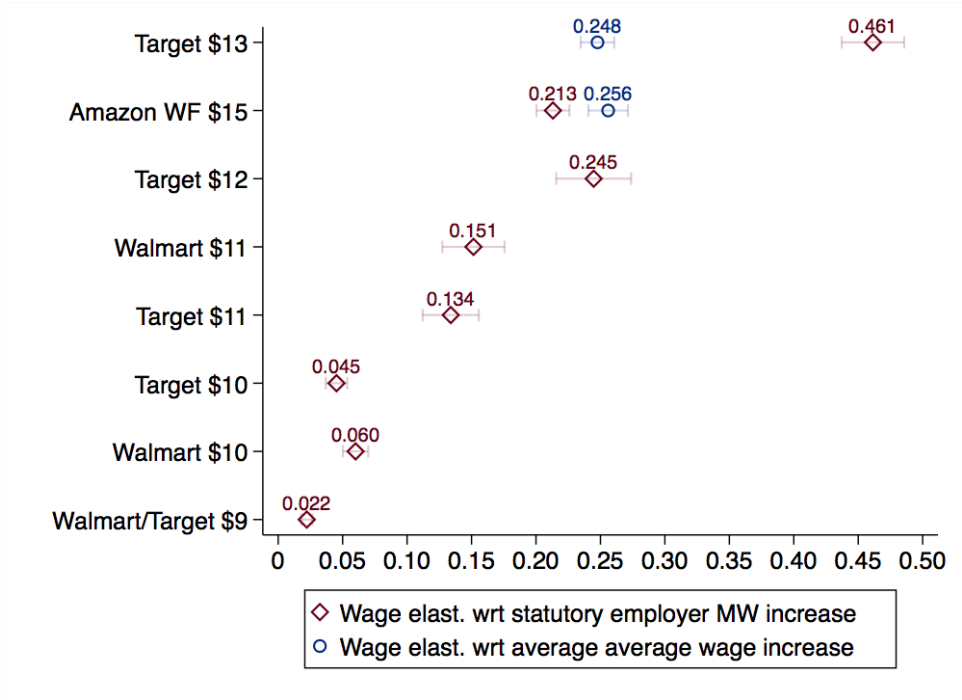
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy firms employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ cell with wages below the policy firm minimum wage in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure 8: Spillover effects increase with level of employer MW



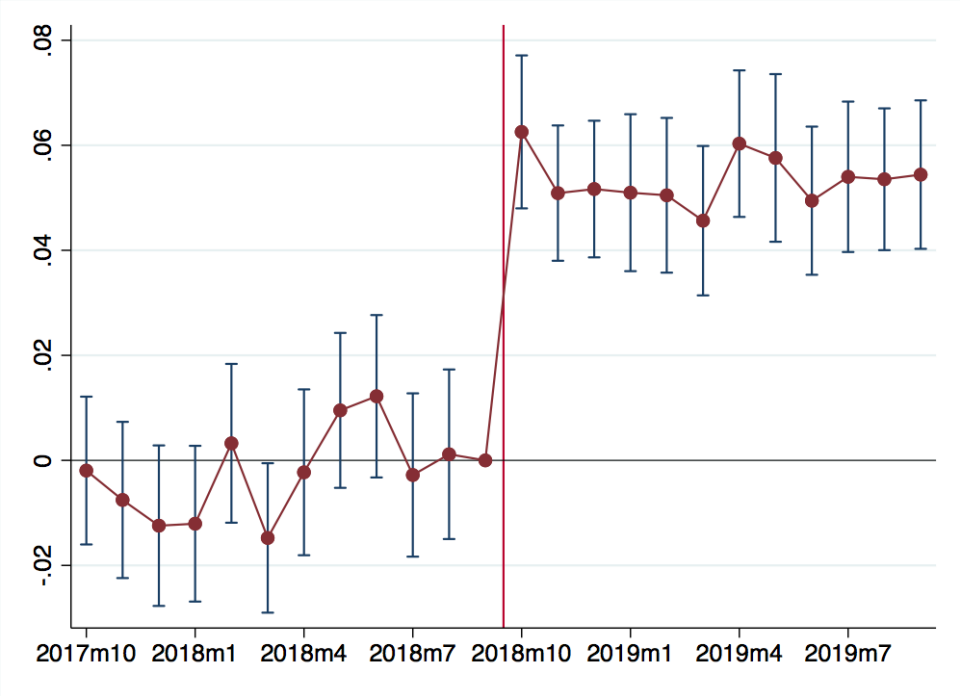
Notes: This figure plots the coefficients on the interaction between job-level exposure to Amazon, Walmart, or Target's minimum wage policies for non-Amazon, non-Walmart, or non-Target industries and an indicator for post-treatment. The dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-Amazon, non-Walmart, or non-Target postings in each occupation-employer-CZ cell with wages below the Amazon, Walmart, or Target minimum wage in the year prior to treatment. Exposure is normalized by the average job's exposure. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample is restricted to postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure 9: Cross-employer wage elasticities from employer MWs, 2015-2019



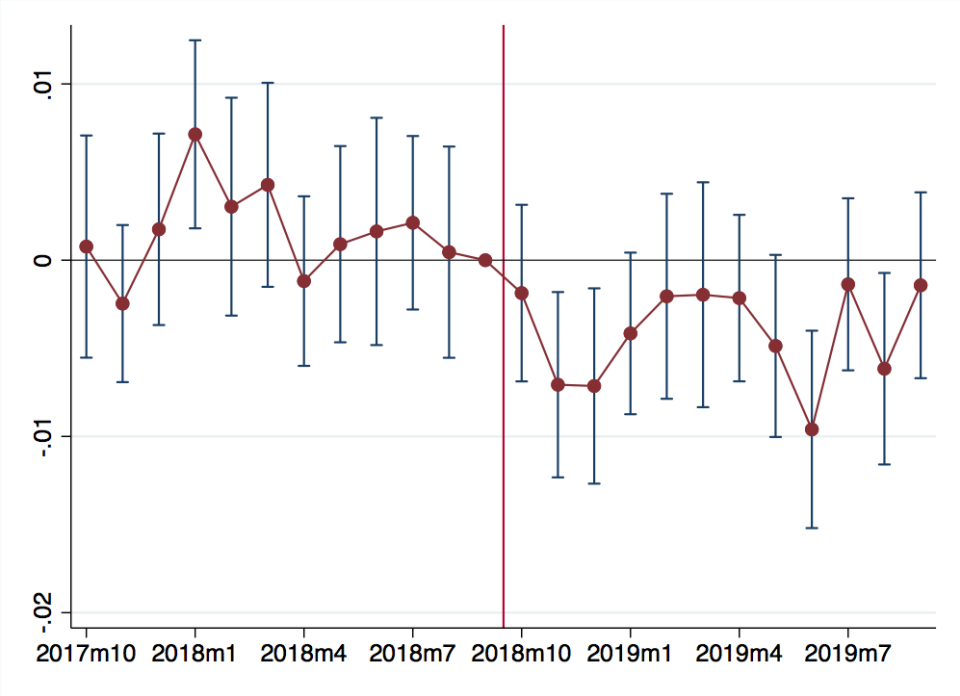
Notes: This figure plots the cross-employer wage elasticities in response to Amazon, Walmart, or Target's minimum wages. The average wage elasticity with respect to the announced minimum increase in Amazon, Walmart, or Target's minimum wage. See Appendix A for more information. Also reported is the wage elasticity with respect to Amazon's $15 and Target's $13 average wage increase. Measures of Target's earlier average wage increases, as well as Walmart's, are unavailable due to insufficient postings for those firms in the BGT data. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure 10: Cross-industry spillovers from Amazon's $15 MW in the CPS



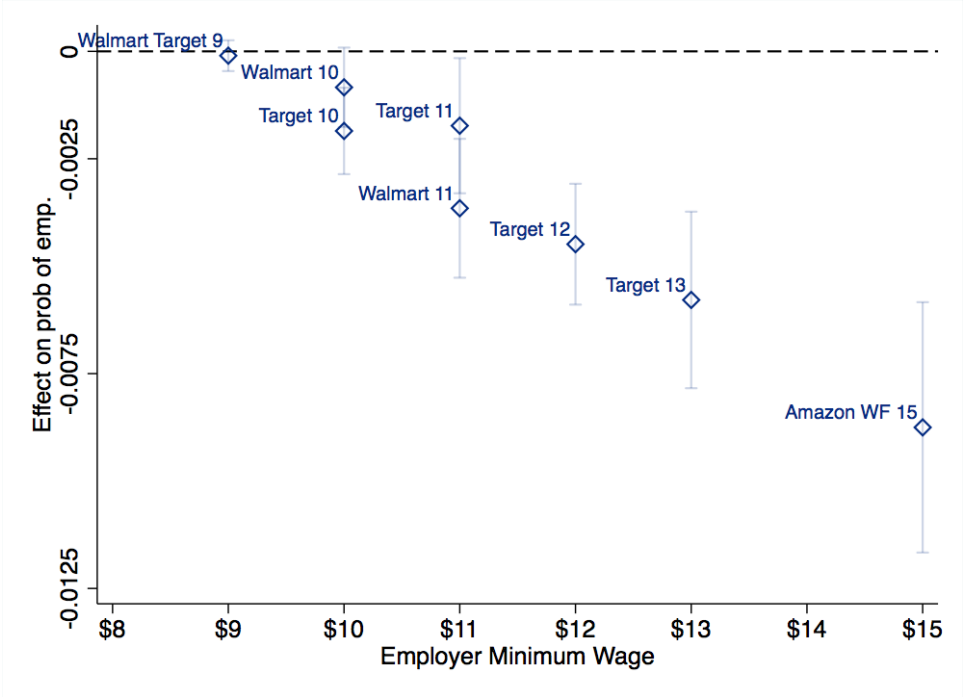
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon industries interacted with month fixed effects, where the dependent variable is log hourly wage. Exposure is defined as the fraction of non-Amazon industry workers in each occupation-CZ cell with wages below $15 in the year before treatment. Exposure is normalized by the average job's exposure. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-Amazon industry workers aged 25-65, excluding those missing occupation or hours information, the self-employed, and those usually working less than 3 hours per week. 95% confidence intervals shown. *Source*: CPS ORG.

Figure 11: Cross-industry employment effects of Amazon's $15 MW



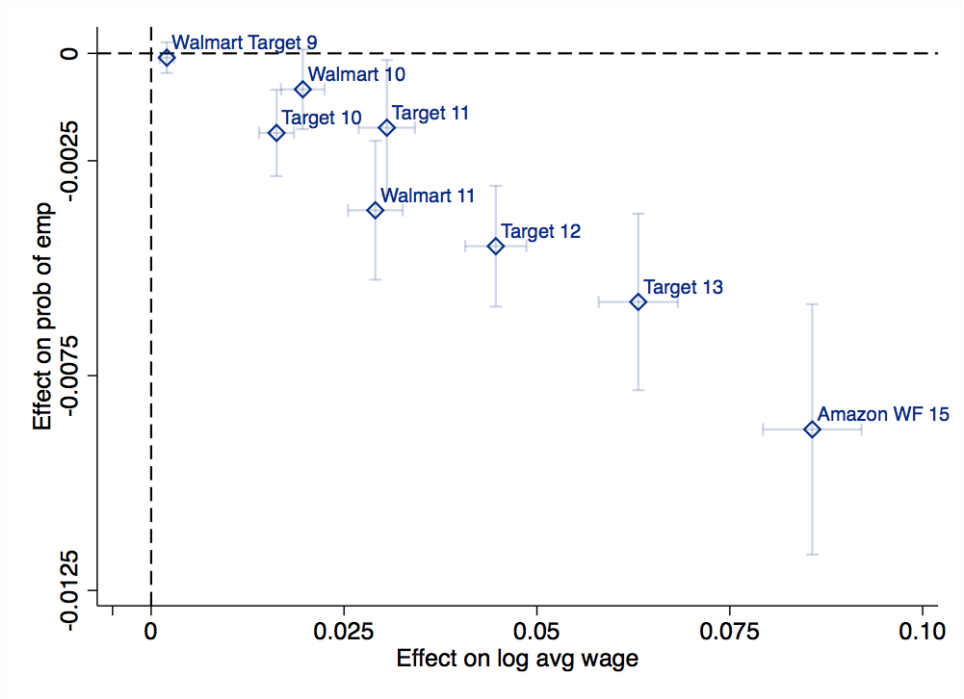
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon industries interacted with month fixed effects, where the dependent variable is probability of being employed vs. unemployed. Exposure is defined as the fraction of non-Amazon industry workers in each occupation-CZ cell with wages below $15 in the year before treatment. Exposure is normalized by the average job's exposure. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Treatment is assigned to the unemployed based on their last occupation while employed. Sample is restricted to individuals aged 25 to 65 and excludes those not in the labor force. 95% confidence intervals shown. *Source*: CPS ORG.

Figure 12: Disemployment effects increase with level of employer MW



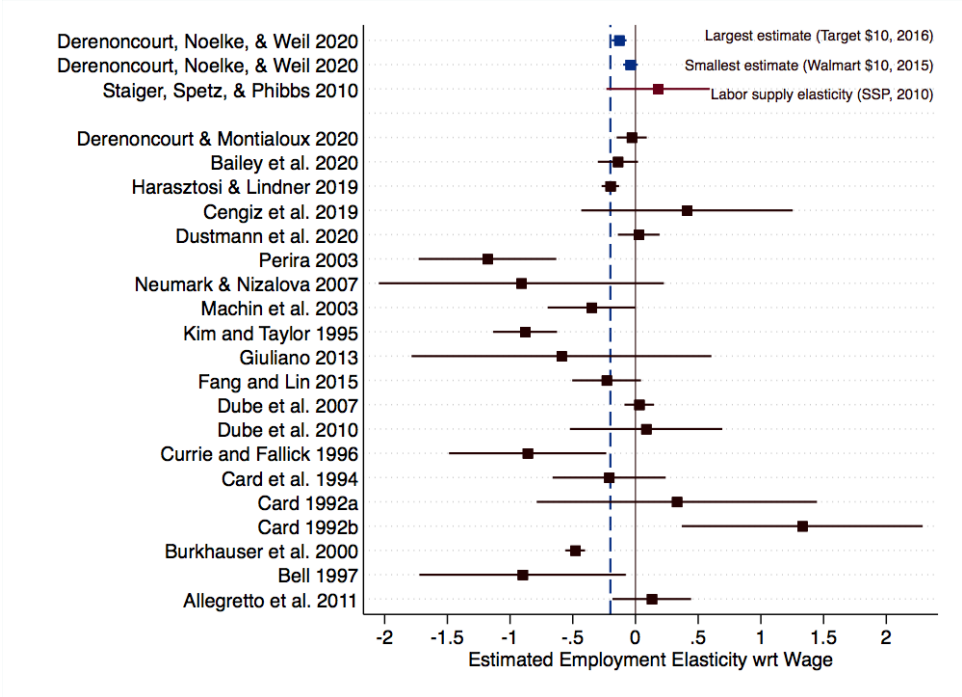
Notes: This figure plots the regression coefficients on job-level exposure to Amazon, Walmart, or Target's minimum wage policies for non-Amazon, non-Walmart, or non-Target industries interacted with an indicator for post-treatment, where the dependent variable is probability of being employed vs. unemployed. Exposure is defined as the fraction of non-Amazon, non-Walmart, or non-Target industry workers in each occupation-CZ cell with wages below the Amazon, Walmart, or Target minimum wage in the year prior to treatment. Exposure is normalized by the average job's exposure. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Treatment is assigned to the unemployed based on their last occupation while employed. Sample is restricted to individuals aged 25 to 65 and excludes those not in the labor force. 95% confidence intervals shown.a *Source*: CPS ORG.

Figure 13: Employment and wage effects of employer MWs in the CPS



Notes: This figure plots the treatment effects on wages against treatment effects on employment. The plotted coefficients are those on the interaction between job-level exposure to Amazon, Walmart, or Target's minimum wage policies for non-Amazon, non-Walmart, or non-Target industries and an indicator for post-treatment. Exposure is defined as the fraction of non-Amazon, non-Walmart, or non-Target industry workers in each occupation-CZ cell with wages below the Amazon, Walmart, or Target minimum wage in the year prior to treatment. Exposure is normalized by the average job's exposure. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. For the wage regressions, the sample restricted to non-Amazon industry workers aged 25-65, excluding those missing occupation or hours information, the self-employed, and those usually working less than 3 hours per week. For the employment regressions, the sample is restricted to individuals aged 25 to 65 and excludes those not in the labor force. *Source*: CPS ORG.

Figure 14: Employment elasticities and comparison with the literature



Notes: This figure summarizes our largest and smallest estimated employment elasticities with respect to average wage and situates these in the previous literature. The estimates in the literature were collected by Harasztosi and Lindner (2019) and Derenoncourt and Montialoux (2020). The dashed vertical line gives the lower bound of our largest estimate. A zero employment effect is indicated by the plain dark line.

Table 1: BGT hourly job ads versus hourly workers in the CPS

	BGT	CPS
Hourly wage	16.1047	23.7221
Full-time/part-time status		
Full-time	0.5	0.9
Part-time	0.24	0.11
Occupation		
Management, business, and financial	0.07	0.17
Professional and related	0.24	0.26
Service	0.22	0.16
Sales and related	0.07	0.08
Office and administrative support	0.17	0.12
Farming, fishing, and forestry occupations	0.00	0.01
Construction and extraction	0.02	0.05
Installation, maintenance, and repair	0.06	0.04
Production	0.04	0.06
Transportation and material moving	0.11	0.06
Region		
North Central	0.24	0.21
North East	0.12	0.18
South	0.28	0.37
West	0.36	0.24
N	5445351	1683271

Sample: Workers in CPS are between 25 and 65 and restricted to those reporting usually working more than three hours a week.

Notes: Sample means for hourly jobs in BGT job ads data and hourly workers in the CPS from 2014 to 2019. *Source*: BGT. CPS-ORG.

Table 2: Wage spillovers: robustness checks

Frac. Affected x Post	0.087***	0.091***	0.087***	0.141***
	(0.004)	(0.005)	(0.004)	(0.007)
Postings with valid wage data / month			0.016***	
			(0.002)	
Obs	1,227,612	1,094,018	1,227,612	919,455
Employer X Occ X CZ FE	Y	Y	Y	Y
CZ X Time FE	Y	Y	Y	Y
Occupation X Time FE	Y	Y	Y	Y
CZ X Occ X Time FE	N	Y	N	Y
Employer X Time FE	N	N	N	Y

Sample: Job vacancies with valid wage data for hourly jobs. Restricted to commuting zones where Amazon advertised in the year before the policy change. Winsorized at the 5% level.
Notes: The outcome variable is log posted hourly wage. Standard errors are clustered at the occupation level. *Source*: Burning Glass Technologies online vacancy data.

Table 3: Wage spillovers: interaction with local labor market characteristics

Dependent variable: Log hourly wage		
	Unemployment Rate	Local MW Below
Frac. Affected × Post	0.082***	0.057***
	(0.003)	(0.005)
Frac. Affected × Moderator × Post	-0.001	0.017***
	(0.001)	(0.004)
Obs	15093690	15099629
Employer X Occ FE	Y	Y
Occupation X Time FE	Y	Y
Lower order interactions & main effects	Y	Y

Sample: Job vacancies with valid wage data for hourly jobs. Restricted to counties where Amazon, Walmart, or Target advertised in the year before the policy change. Winsorized at the 5% level.
Notes: The outcome variable is log posted hourly wage. The local minimum wage is the maximum of the applicable federal, state, county, or city minimum wage. Standard errors are clustered at the employer level. *Source*: Burning Glass Technologies online vacancy data and sub-state minimum wage data from Zipperer (2019).

Table 4: Employment elasticity estimates

	Walmart/ Target $9	Walmart $10	Target $10	Target $11	Walmart $11	Target $12	Target $13	Amazon $15 Whole Foods $15
Exposure × Post								
Employment	-0.000	-0.001*	-0.002***	-0.002**	-0.003***	-0.004***	-0.005***	-0.008***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	142,362	142,540	142,706	140,820	139,717	138,808	117,735	125,810
Wages	0.002***	0.017***	0.014***	0.027***	0.025***	0.039***	0.055***	0.080***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
	81,589	82,642	82,848	82,363	81,882	81,365	68,661	74,345
Emp. elasticity	-0.06	-0.04	-0.13***	-0.06**	-0.13***	-0.11***	-0.10***	-0.11***
se	0.10	0.03	0.04	0.03	0.03	0.02	0.02	0.02
Lower bound	-0.26	-0.10	-0.19	-0.12	-0.20	-0.14	-0.14	-0.15
Upper bound	0.14	0.01	-0.06	-0.01	-0.07	-0.08	-0.07	-0.07

Notes: This table reports employment and wage effects and the estimated employment elasticities among non-policy industry workers in response to each policy firm's minimum wage policy. Each column reports the coefficient on job-level exposure interacted with post in separate difference-in-difference regressions. *Data sources*: CPS-ORG.

Table 5: Aggregate employment elasticity estimates

	Walmart/ Target $9	Walmart $10	Target $10	Target $11	Walmart $11	Target $12	Target $13	Amazon $15 Whole Foods $15
Exposure × Post								
Employment	-0.000	-0.001*	-0.002***	-0.002**	-0.003***	-0.004***	-0.005***	-0.008***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	144,366	144,352	144,514	142,419	141,257	140,305	118,983	128,411
Wages	0.002***	0.017***	0.014***	0.027***	0.025***	0.040***	0.055***	0.080***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
	82,682	83,617	83,860	83,297	82,781	82,211	69,388	75,770
Emp. elasticity	-0.06	-0.04	-0.12***	-0.06**	-0.13***	-0.11***	-0.10***	-0.11***
se	0.10	0.03	0.03	0.03	0.03	0.02	0.02	0.02
Lower bound	-0.26	-0.10	-0.19	-0.12	-0.19	-0.14	-0.13	-0.15
Upper bound	0.14	0.01	-0.05	-0.01	-0.07	-0.08	-0.06	-0.08

Notes: This table reports aggregate employment and wage effects and the estimated employment elasticities, including both non-policy industry and policy industry workers in response to each policy firm's minimum wage policy. Each column reports the coefficient on job-level exposure interacted with post in separate difference-in-difference regressions. *Data sources*: CPS-ORG.

Appendices

Appendix A Background on voluntary employer minimum wage policies **53**

Appendix B CPS data appendix **57**

Appendix C Additional evidence from employer minimum wage increases **58**

Appendix D Additional evidence on local labor market moderators **72**

A Background on voluntary employer minimum wage policies

In recent years, several low-wage, predominantly retail and service sector firms have voluntarily instituted minimum wages for their employees. In this appendix, we provide background information on the policies adopted by the firms analyzed in this study. We include the full list of firms with recent minimum wage increases, courtesy of the National Employment Law Project.

Amazon/Whole Foods Amazon employs over 840,000 workers in the US (Amazon.com, 2020). In 2018, Amazon advertised hourly job positions in 188 commuting zones throughout the country. In October of 2018, Amazon announced a minimum wage of $15 per hour for all employees effective November 1, 2018. The Amazon decision provoked almost immediate controversy among its employees because it was accompanied by the elimination of a $2000 bonus for high productivity workers. This meant that the minimum wage increase as originally structured would have actually reduced earnings for some of the company's most productive employees. The proposal was quickly modified to correct for this problem by providing additional increases for those workers otherwise adversely affected by it. Furthermore, the wages of contractors were not included in the new policy (see Abbruzzese and Cappetta (2018), Murphy (2018), and Wiese (2018)). The increase affected regular and seasonal employees, full-time, and part-time workers. Raises were also extended to those currently making $15 of between 25-55 cents. The increase applied to both incumbent and new hires.

Prior to their announcement on October 1, 2018, Amazon's minimum wage started at $11 (Settembre, 2018). On the company blog announcing the wage increase, Amazon posited their wage increase as a response to critics of their then prevailing wage policies (Staff, 2018). Tight labor markets were also cited as a reason behind Amazon's wage increase. And with its timing around holiday season the wage increase was more in line to attract holiday season workers. Arguably its wage increase put more pressure on smaller employers to increase their wages (see (Canal, 2018), and (Minaya and Trentmann, 2018)). Amazon's wage increase followed several city and county level wage increase regulations.

Walmart Walmart remains the largest employer of workers in the US, with a workforce of nearly 1.5 million (Walmart, 2020). Its 4,177 stores in the US are dispersed throughout the country. According to BGT data, Walmart advertised in 592 counties over the 2010-2019 period. In February of 2015, Walmart announced that it was increasing entry-level wages for its part-time and full-time sales associates across the country to $9 per hour effective in April 2015, and to $10 an hour one year later. Walmart reported that 40% of its workforce was affected by the change. In January of 2018 they announced a further increase to $11 an hour, effective February, 2018 (Walmart, 2018).

Prior to their February 2015 announcement, the majority of Walmart's locations followed the federal minimum wage of $7.25. However, when 21 states raised their minimum wage in 2015, Walmart adjusted base salaries for 1,434 stores (Layne, 2014). The average wage posted on Walmart's online job ads prior to February 2015 was $12.53.

Target Target is the 8th largest retailer in the US (NRF, 2019). Target employs 360,000 people and has annual sales of approximately $78 billion, making it the second biggest discount chain behind Walmart (Mergent, 2020). It has a total of 1,868 stores and 42 distribution centers located across the country. Around 40% of its stores are in the five states - California, Texas, Florida, Illinois, and New York Corporation (2020a). Up until June 2016, Target's starting minimum wage was $9, which was then

increased to $10. In September 2017, Target announced a minimum wage increase to $11 from $10 and aimed to increase minimum wage to $15 by end of 2020 (D'Innocenzio, 2017).

Target announced on June 17th, 2020 that effective July 5, their minimum wage will increase from $13 to $15 Corporation (2020b). Its most recent increase applies to approximately 275,000 part-time and full-time workers (Kavilanz and Business, 2020). In 2017, Target cited tight labor market as its reason behind increasing minimum wages to $15 by 2020. The average wage posted for Target's online jobs ads prior to September 2017 was $13.14.

Costco Costco Wholesale Corporation is an international chain of membership warehouses. In the United States, Costco has 803 warehouses, 558 locations, and employees 185,000 full and part-time workers (Costco Wholesale Corporation, 2020). Costco's most recent annual revenue was $163.2 billion (Costco Wholesale Corporation, 2020). On May 1, 2018, Costco announced that it was raising its minimum wage for its hourly workers from $13 to $14. Costco cited the 2017 Corporate Tax Cut as its motivation (Hanbury, 2018). This wage increase impacted approximately 130,000 of its employees (Romano, 2018). Less than a year later in March 2019, Costco increased its minimum wage another dollar to $15 for its store employees and supervisors. There was not comment on the percentage of workforce impacted by this increase (Hanbury, 2019).

Company	No. of US Employees	Previous Min Wage	New Min Wage	Annoucement Date	Start Date	Which Occupation	Entry-Level?	For existing employees?	For new employees?
Walmart	1,500,000	$7.25	$8.05 - $8.50 (depends on state)	December 24, 2014	January 1, 2015	Hourly employees below new state min wage		Existing employees at 1,423 stores (1/3 of Walmart locations)[1]	
		$7.25	$9	February 18, 2015	April 1, 2015	FT & PT associates	Yes, applicable to entry level	Yes[2]	
		$9	$10	February 18, 2015 (Reannounced: January 20, 2016)	February 20, 2016	All hourly associates hired before Jan 2016		Yes	Not applicable to new hires. They start at $9 and must complete the 6 month Pathways Training Program[3]
		$10	$11	January 1, 2018	February 17, 2018	All hourly associates	Yes, applicable to entry level	Yes, and eligible employees get one-time cash bonus of $1000	Yes[4]
		$11	$15* (for certain roles)	September 17, 2020	October 1, 2020	Deli and bakery associates		Yes, ≈ 165K hourly associates impacted	No[5]
		$11	$18 - $21 (up to $30)	September 17, 2020	October 1, 2020	Team leaders in supercenters		Yes	No[6]
Amazon	840,400	$13.68 (median). Min wage varies by state, $10 (TX) vs $13.50 (NJ)	$15	October 1, 2018	November 1, 2018	All employees	Reg & Seasonal (FT & PT). ≈250K reg employees and ≈100K seasonal impacted	Yes, even those making $15/hr will receive a raise. Already started increasing wages by 25 - 55 cents for fulfillment centers	Yes[7, 8, 9]
Whole Foods	*included in Amazon	$13.68 (median)	$15	October 1, 2018	November 1, 2018	All employees	Yes, for FT & PT workers	Yes	Yes[10]
Target	386,000	$7.25	$9	March 1, 2015	April 1, 2015 [11,12]				
		$9	$10	April 1, 2016	May 1, 2016	Hourly workers[13]			
		$10	$11	September 25, 2017	October 1, 2017	Entry level hourly workers, including temp holiday hires		Yes, no comment on % of workforce impacted	Will apply to the 100K temp workers hired for holiday season[14,15,16]
		$11	$12	March 1, 2018	March 1, 2018	Starting with existing employees		Yes[17,18,19]	
		$12	$13	April 4, 2019	June 1, 2019	Entry level hourly workers, including new seasonal hires[20]			
		$13	$15	September 25, 2017 (Reannounced: June 17, 2020)	July 5, 2020	Hourly FT & PT team members		Yes[21]	
Costco	185,000	$14	$15 ($15.50 if previous wage was $14.50)	March 1, 2019	March 4, 2019	Store employees and supervisors		Yes, no comment on % of workforce impacted	Yes[22,23]
		$13	$14 ($14.50 if previous wage was $13.50)	May 1, 2018	June 11, 2018	Hourly employees		Yes, ≈130K employees impacted[24,25]	

Sources for policy firm table

1	https://www.reuters.com/article/us-walmart-wages/exclusive-u-s-minimum-wage-hikes-to-affect-1400-plus-walmart-stores-idUSKBN0K20AE20141224
2	https://money.cnn.com/2015/02/19/news/companies/walmart-wages/index.html
3	https://corporate.walmart.com/newsroom/2016/01/20/more-than-one-million-walmart-associates-to-receive-pay-increase-in-2016
4	https://corporate.walmart.com/newsroom/2018/01/11/walmart-to-raise-u-s-wages-provide-one-time-bonus-and-expand-hourly-maternity-and-parental-leave
5	https://ktvo.com/news/local/walmart-to-raise-wages-some-staff-up-to-30-an-hour
6	https://ktvo.com/news/local/walmart-to-raise-wages-some-staff-up-to-30-an-hour https://corporate.walmart.com/newsroom/2020/09/17/investing-in-our-associates-and-roles-of-the-future
7	https://www.washingtonpost.com/business/2018/10/02/amazon-announces-it-will-boost-minimum-wage-all-workers-after-facing-criticism/
8	https://www.cnbc.com/2018/10/02/amazon-raises-minimum-wage-to-15-for-all-us-employees.html
9	https://www.marketwatch.com/story/amazon-reaches-1-million-workers-as-pandemic-pushes-total-up-11596136565
10	https://www.washingtonpost.com/business/2018/10/02/amazon-announces-it-will-boost-minimum-wage-all-workers-after-facing-criticism/
11	https://www.cnbc.com/2017/09/25/target-to-raise-its-hourly-minimum-wage.html
12	https://www.wsj.com/articles/target-to-increase-wages-to-minimum-9-hour-for-all-workers-in-april-1426709296?mod=mktw
13	https://www.reuters.com/article/us-target-wages-exclusive-idUSKCN0XF2L4
14	https://corporate.target.com/press/releases/2017/09/Target-Raises-Minimum-Hourly-Wage-to-11-Commits-to
15	https://apnews.com/d3c07cc6d9e44ac0a3ed9ddd8ee91e26/Target-is-raising-minimum-hourly-wage-to-$15-by-end-of-2020
16	https://www.cnbc.com/2017/09/25/target-to-raise-its-hourly-minimum-wage.html
17	https://in.reuters.com/article/target-wages/target-raises-hourly-minimum-wage-to-13-further-topping-walmarts-11-idINKCN1RG1TS
18	https://corporate.target.com/article/2018/03/wage-update
19	https://www.dropbox.com/s/7s81cezmnj2sm0x/Target_Company%20Details.pdf?dl=0
20	https://in.reuters.com/article/target-wages/target-raises-hourly-minimum-wage-to-13-further-topping-walmarts-11-idINKCN1RG1TS
21	https://corporate.target.com/press/releases/2020/06/target-increases-starting-wage-to-15-thanks-frontl
22	https://www.washingtonpost.com/business/2018/10/02/amazon-announces-it-will-boost-minimum-wage-all-workers-after-facing-criticism/
23	https://www.businessinsider.com/costco-raises-minimum-wage-war-for-talent-2019-3
24	https://www.seattletimes.com/business/retail/costco-employees-anticipate-benefits-news-with-quarterly-earnings-thursday/
25	https://www.businessinsider.com/costco-raises-its-minimum-wage-2018-6

B CPS data appendix

The Bureau of Labor Statistics provides workforce data in the Current Population Survey Outgoing Rotation Group ("CPS ORG"). The CPS ORG is a survey in which households are in the survey for four months, not included for eight months, and included again for four final months. This creates a semi-panel structure of the data that links individuals across two calendar years.

We use nationally-representative, person-level CPS ORG data from January 2014 to December 2019.[24] The data include the employed and unemployed, allowing for analyses of disemployment effects. The following briefly describes key variables and features of the data that are central to the analyses.

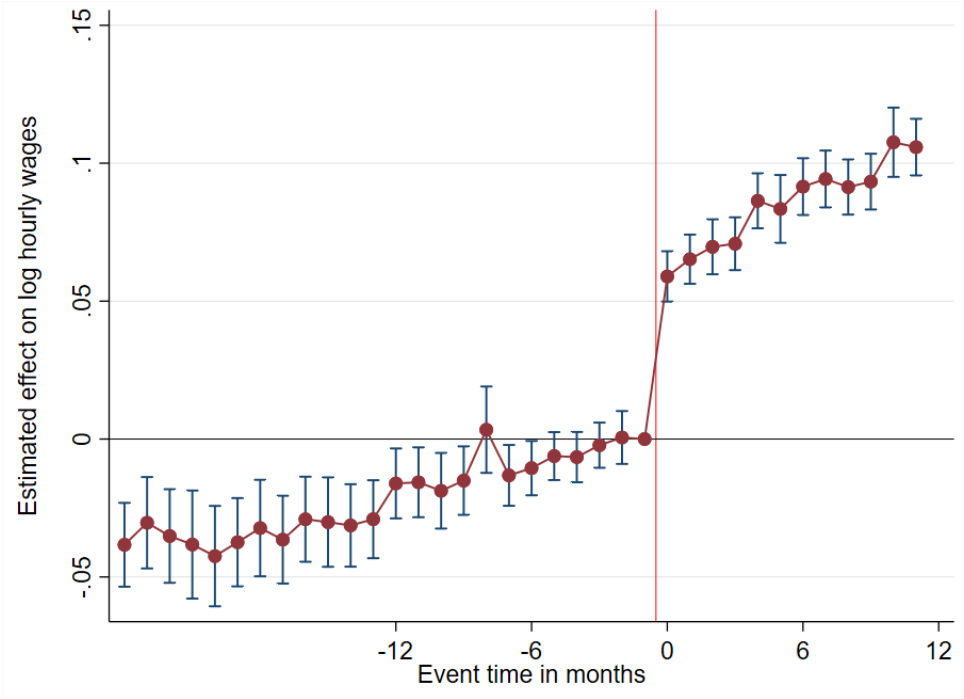
Sample Our sample includes individuals between the ages of 25 and 65 who are not self-employed. Wage analyses are further restricted to those who are employed and usually work more than three hours per week. Employment analyses include the unemployed.

Outcomes of interest The dependent variable for the wage analyses is a worker's hourly wage. We calculate this rate by dividing a worker's usual weekly earnings by the usual hours worked per week at their main job. This variable is then winsorized and converted to a natural logarithm. For employment analyses, the outcome of interest is whether a worker is employed or unemployed, and excludes those not in the labor force. Occupation information is available for 97.1% of workers and 87.7% of the unemployed, for whom last occupation is given. Last occupation is provided for only 6.9% of those not in the labor force, therefore this group is excluded from the analyses.

[24] CPS ORG files were downloaded from the Economic Policy Institute, 2020, Current Population Survey Extracts, Version 1.0.10, https://microdata.epi.org.

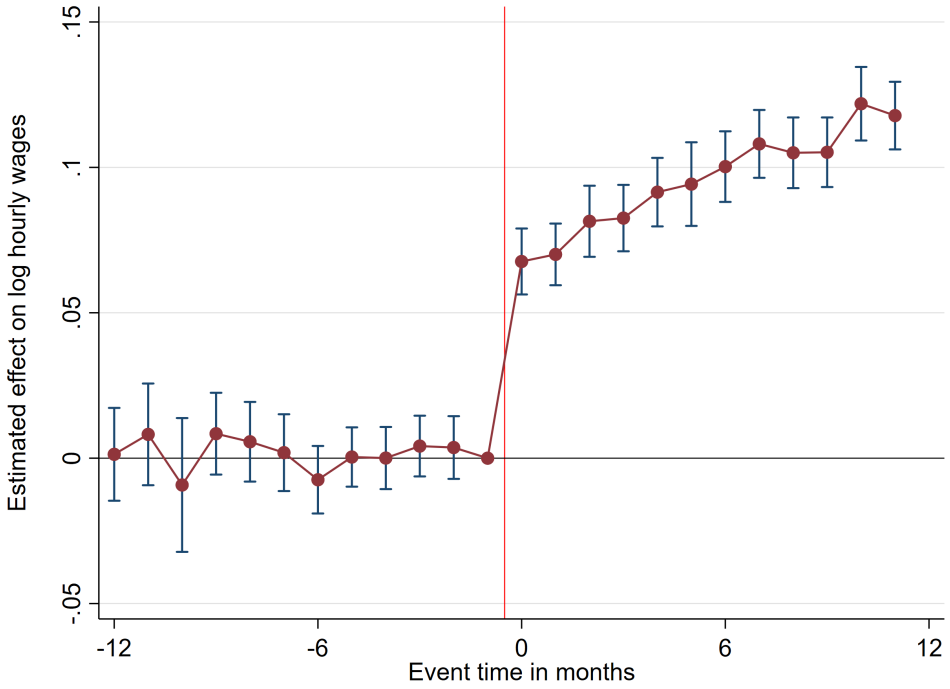
C Additional evidence from employer minimum wage increases

Figure C1: Amazon spillovers, 24-month pre-period



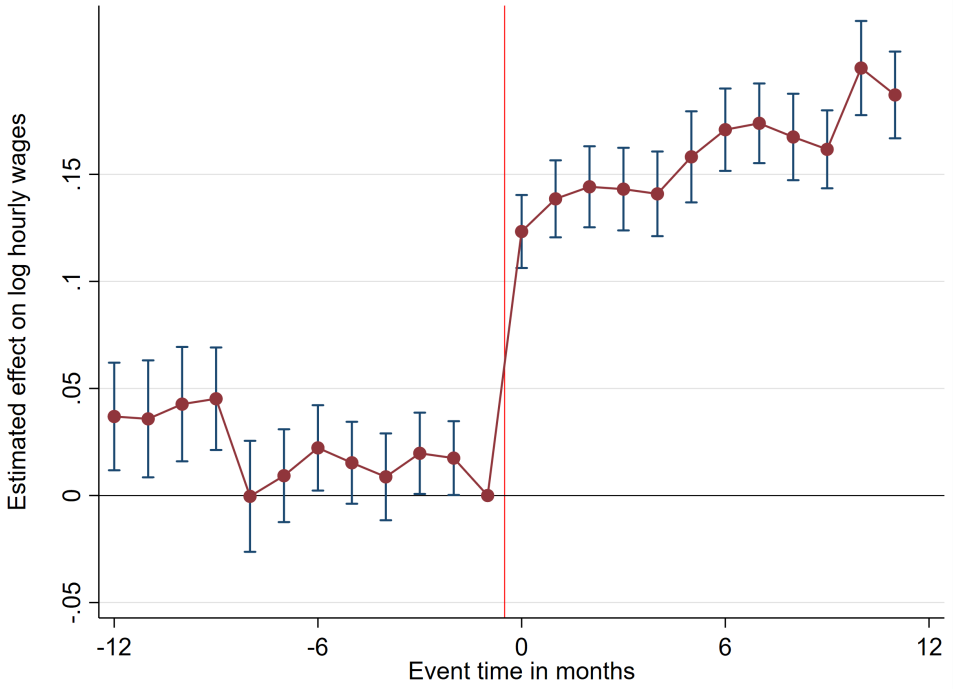
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. A two-year pre-period is shown. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below $15 in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C2: Amazon spillovers, with occupation-by-CZ-by-month fixed effects



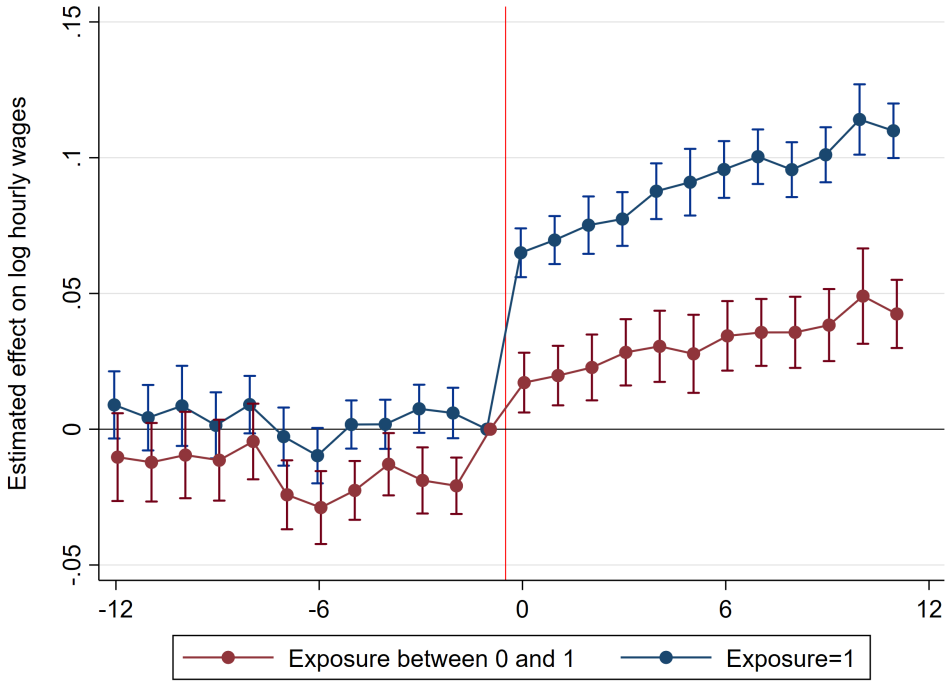
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below $15 in the year before treatment. Employer-by-occupation-by-CZ, and occupation-by-CZ-by-month fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C3: Amazon spillovers, with occ-by-CZ-by-month, employer-by-month fixed effects



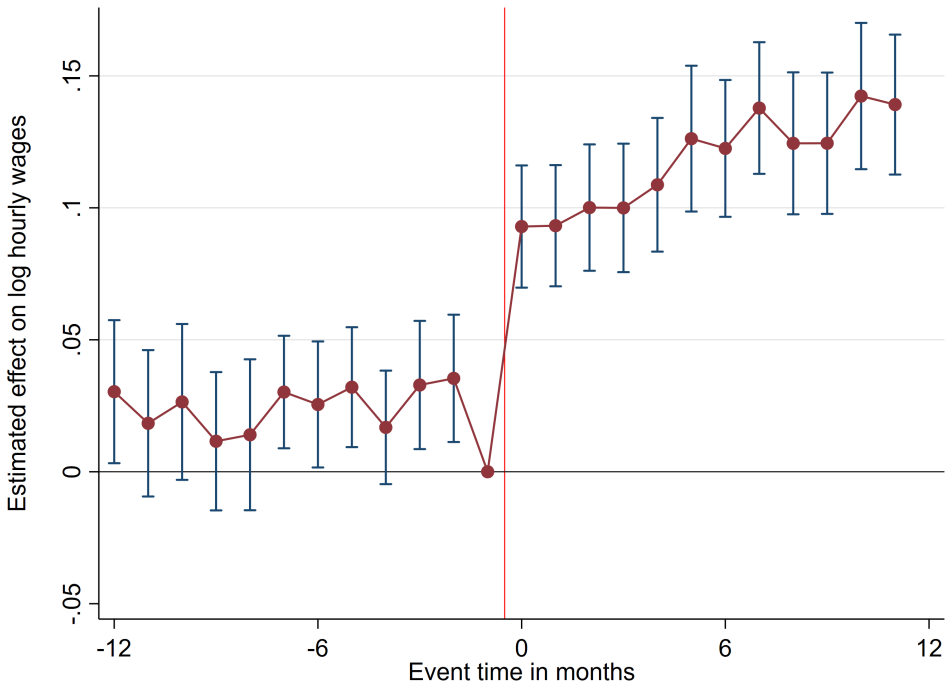
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below $15 in the year before treatment. Employer-by-occupation-by-CZ, and occupation-by-CZ-by-month and employer-by-month fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C4: Amazon spillovers, binned exposure



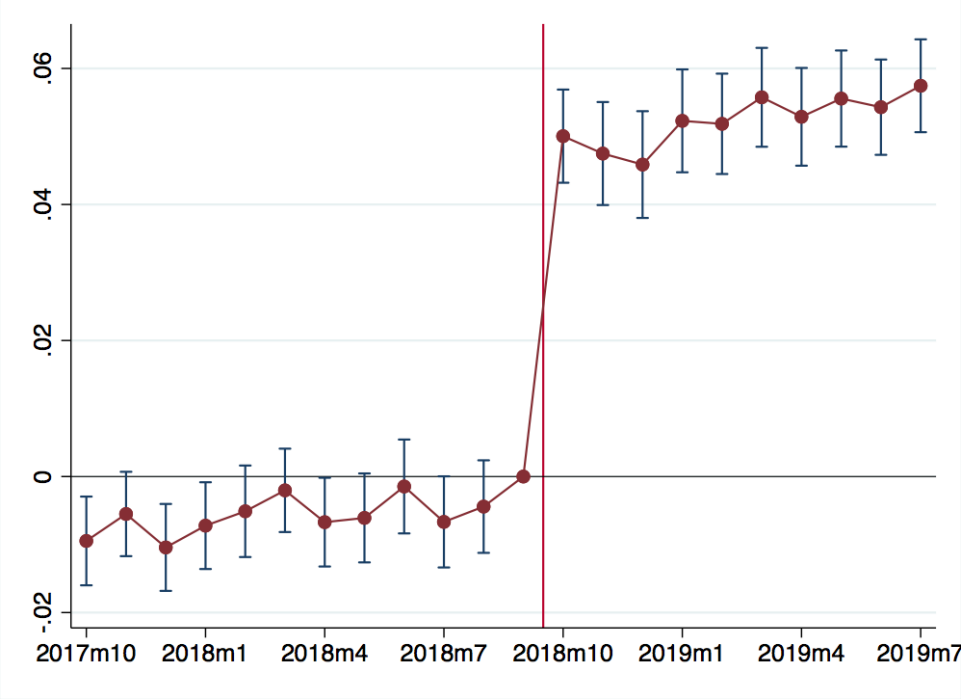
Notes: This figure plots the regression coefficients on job-level exposure group to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. The three exposure groups are jobs with 100% of postings offering below $15 in the year prior to treatment, jobs which are partially paid below $15, and those where 0% of postings are paid below $15. The final group is the omitted category. Jobs are defined as occupation-employer-CZ cells. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C5: Amazon spillovers, binned exposure: partially vs. fully exposed



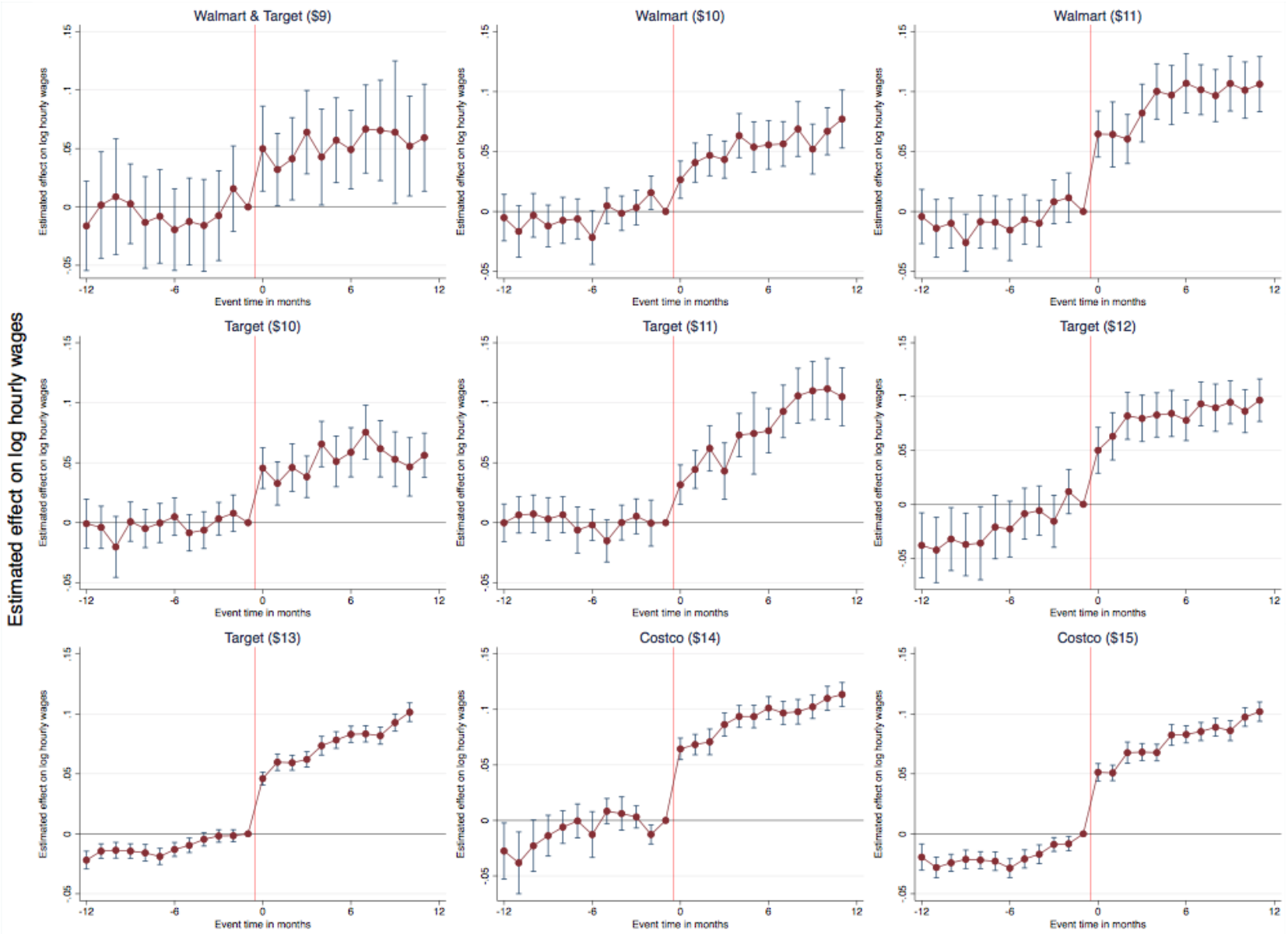
Notes: This figure plots the regression coefficients on job-level exposure group to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. The two exposure groups are jobs with 100% of postings offering below $15 in the year prior to treatment and jobs with some positive fraction of postings offering below $15. The final group is the omitted category. Jobs with zero percent exposure are excluded from the sample. Jobs are defined as occupation-employer-CZ cells. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C6: Spillovers from Amazon's MW in worker reported wages, 2018



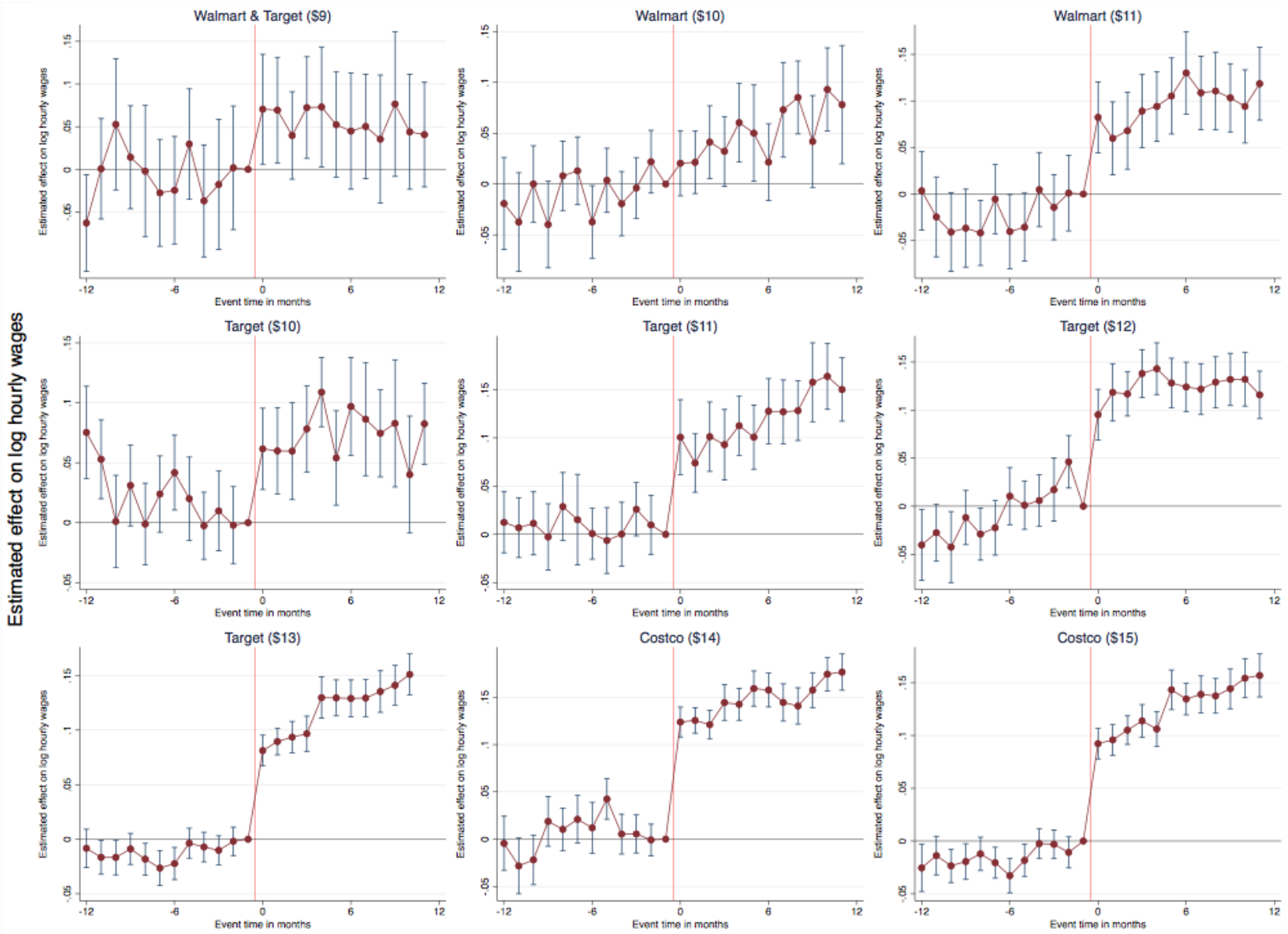
Notes: This figure plots the coefficients on the interaction between exposure to Amazon's minimum wage policy and month fixed effects, where the dependent variable is log reported hourly wage by workers at non-Amazon employers. Exposure is defined as the fraction of each non-Amazon employer's job postings with wages below $15 in the year before treatment. Exposure is normalized by the average job's exposure. Employer, county, and month-by-occupation fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Glassdoor salary reports.

Figure C7: Walmart, Target, and Costco MW spillovers: robust to occupation × CZ × month FEs



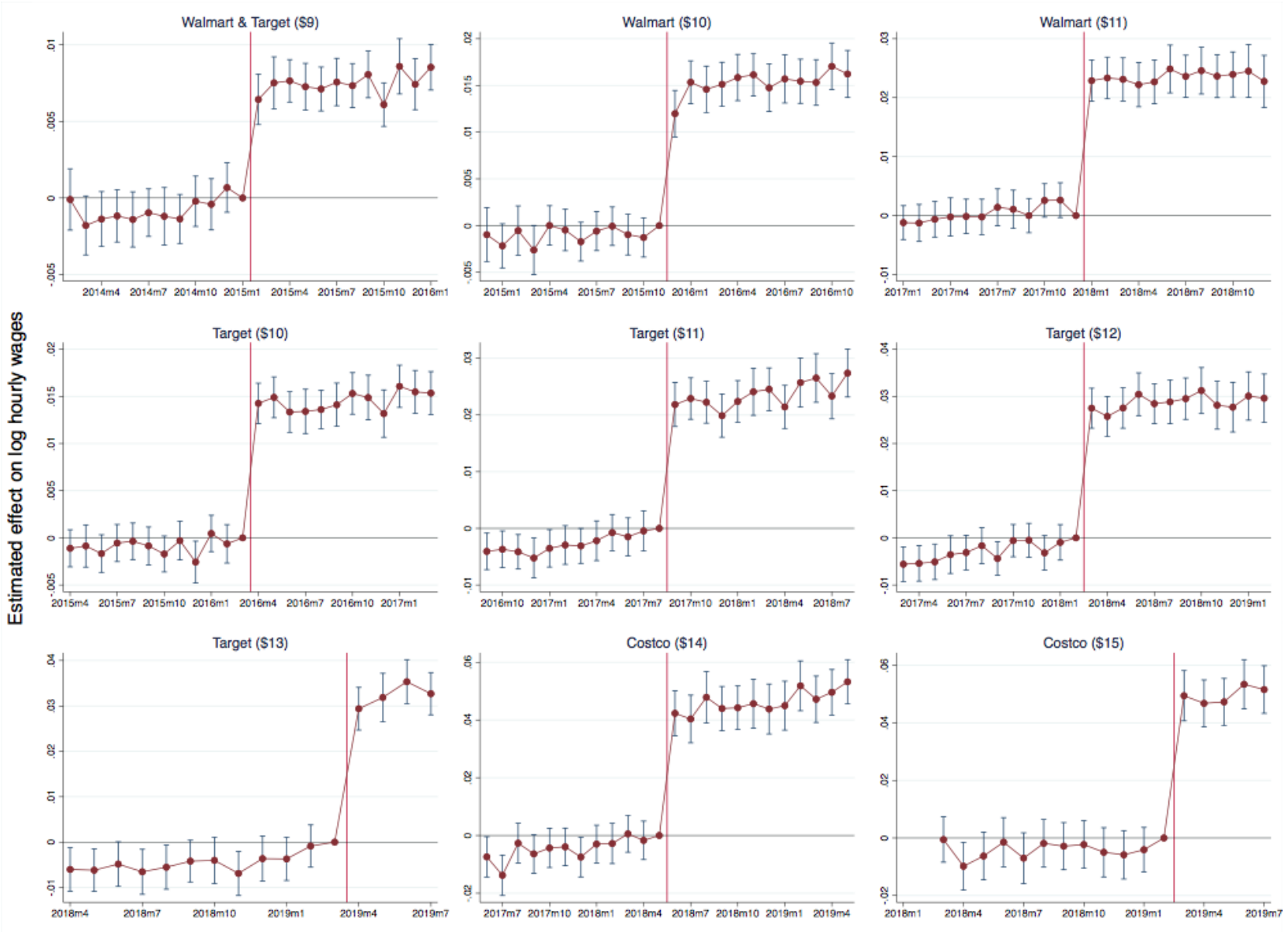
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy firms employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ cell with wages below $15 in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation-by-CZ fixed effects are included. Sample restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C8: Walmart, Target, and Costco MW spillovers: robust to occ. × CZ × month & employer × month FEs



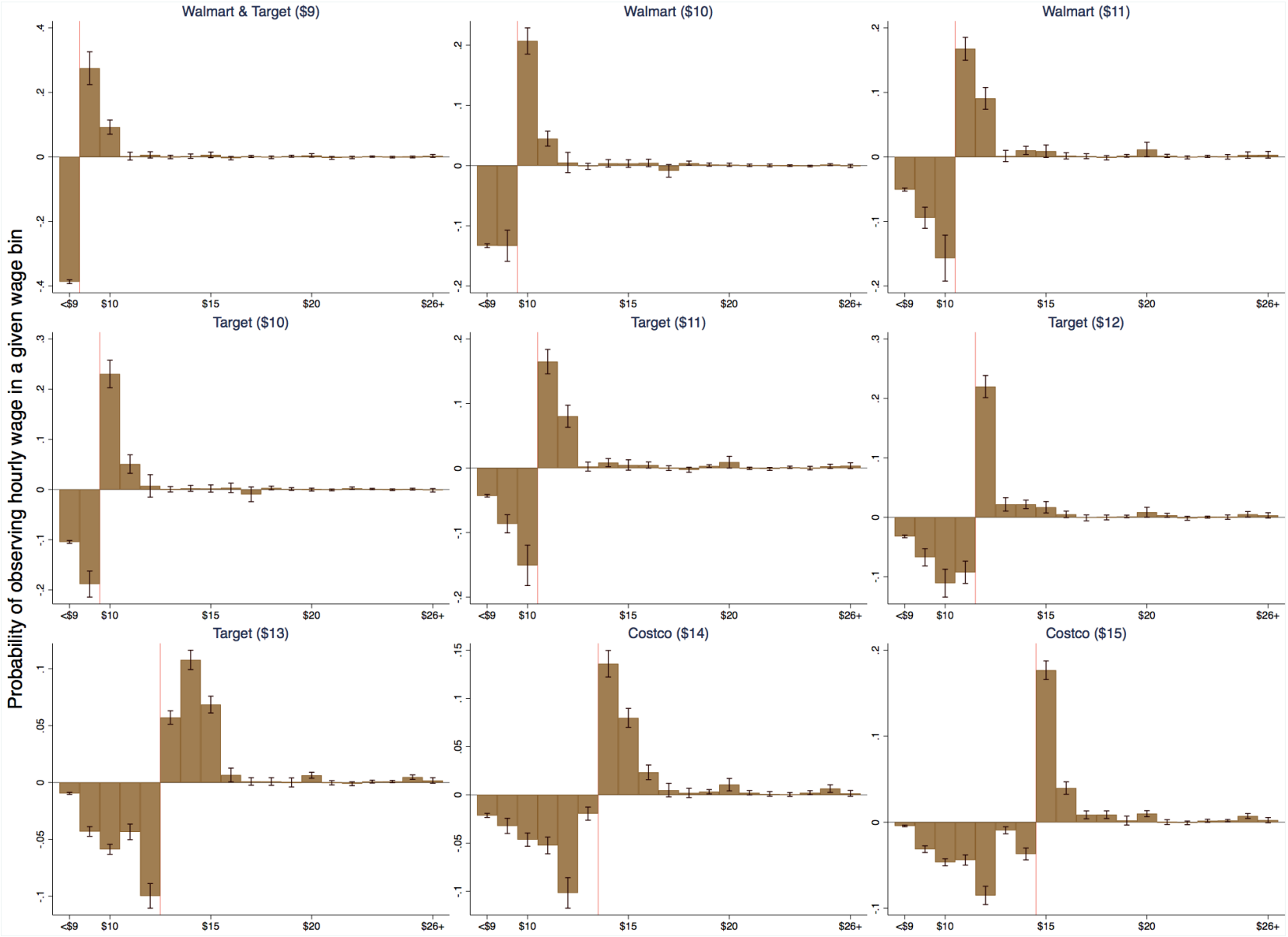
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy firms employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ cell with wages below $15 in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation-by-CZ, and month-by-employer fixed effects are included. Sample restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C9: Spillovers in worker-reported wages from Walmart, Target, and Costco MWs (Glassdoor)



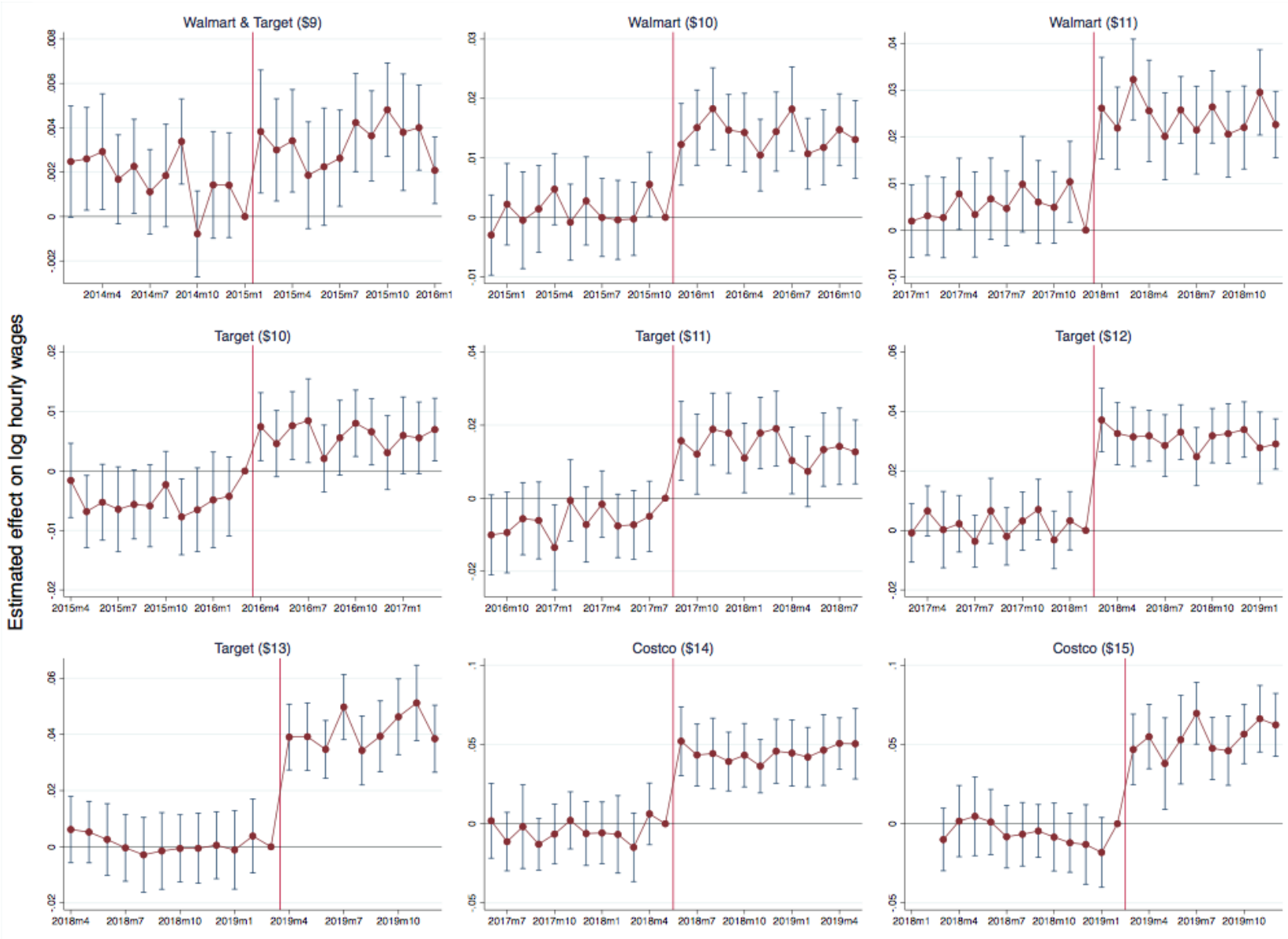
Notes: This figure plots the coefficients on the interaction between exposure to policy firm minimum wages and month fixed effects, where the dependent variable is log reported hourly wage by workers at non-policy employers. Exposure is defined as the fraction of each non-policy employer's job postings with wages below the policy firm minimum wage in the year before treatment. Employer, county, and month-by-occupation fixed effects are included. Sample restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Glassdoor salary reports.

Figure C10: Bunching in response to Walmart, Target, and Costco MWs



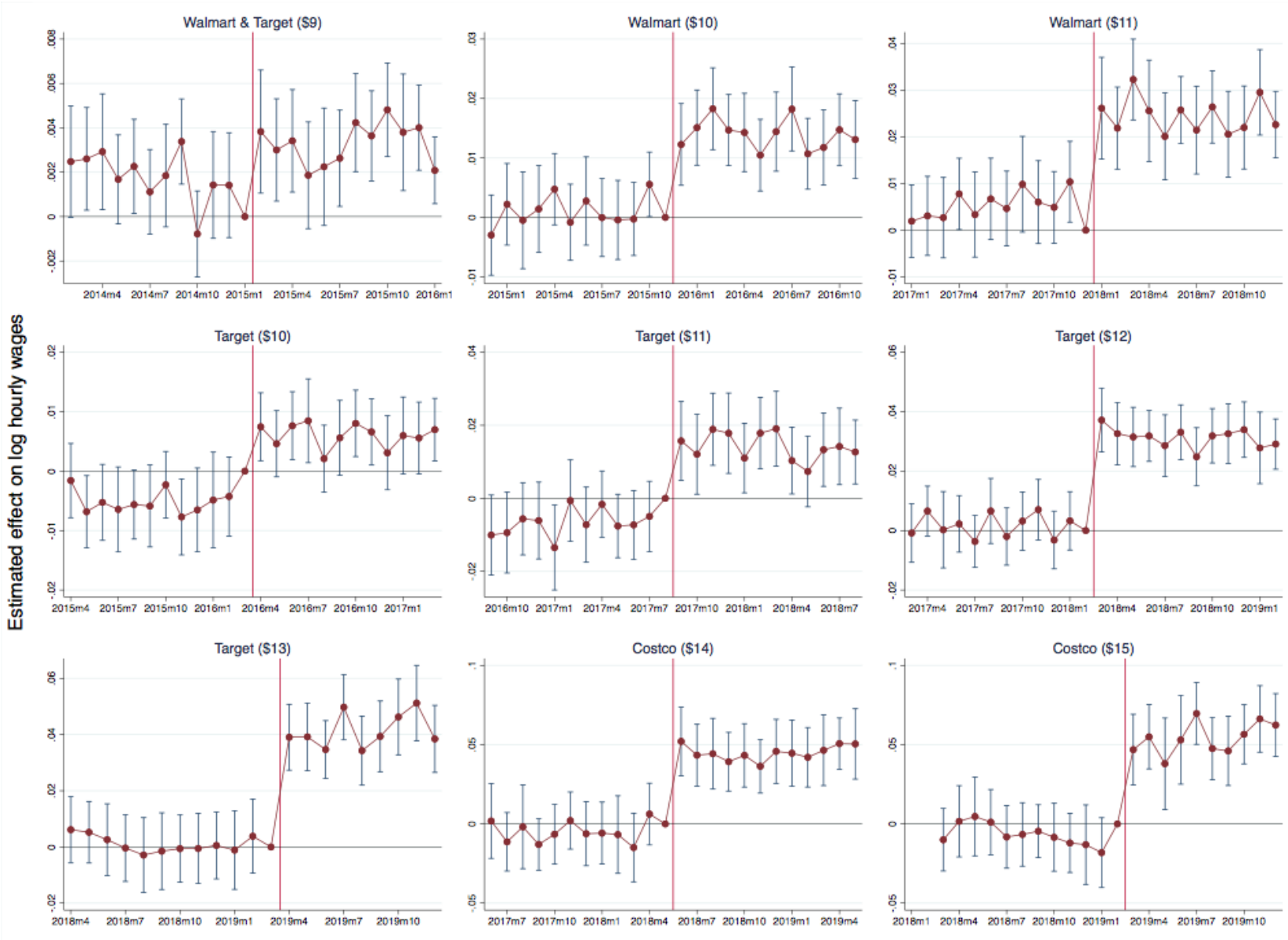
Notes: This figure plots the coefficients from linear probability regressions of hourly wages being in a given wage bin on the interaction between job-level exposure to policy firm minimum wages for non-policy employers and an indicator for post-October-2018. Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ cell with wages below the policy firm minimum wage in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C11: Null effects at placebo treatment dates for Walmart, Target, and Costco MWs



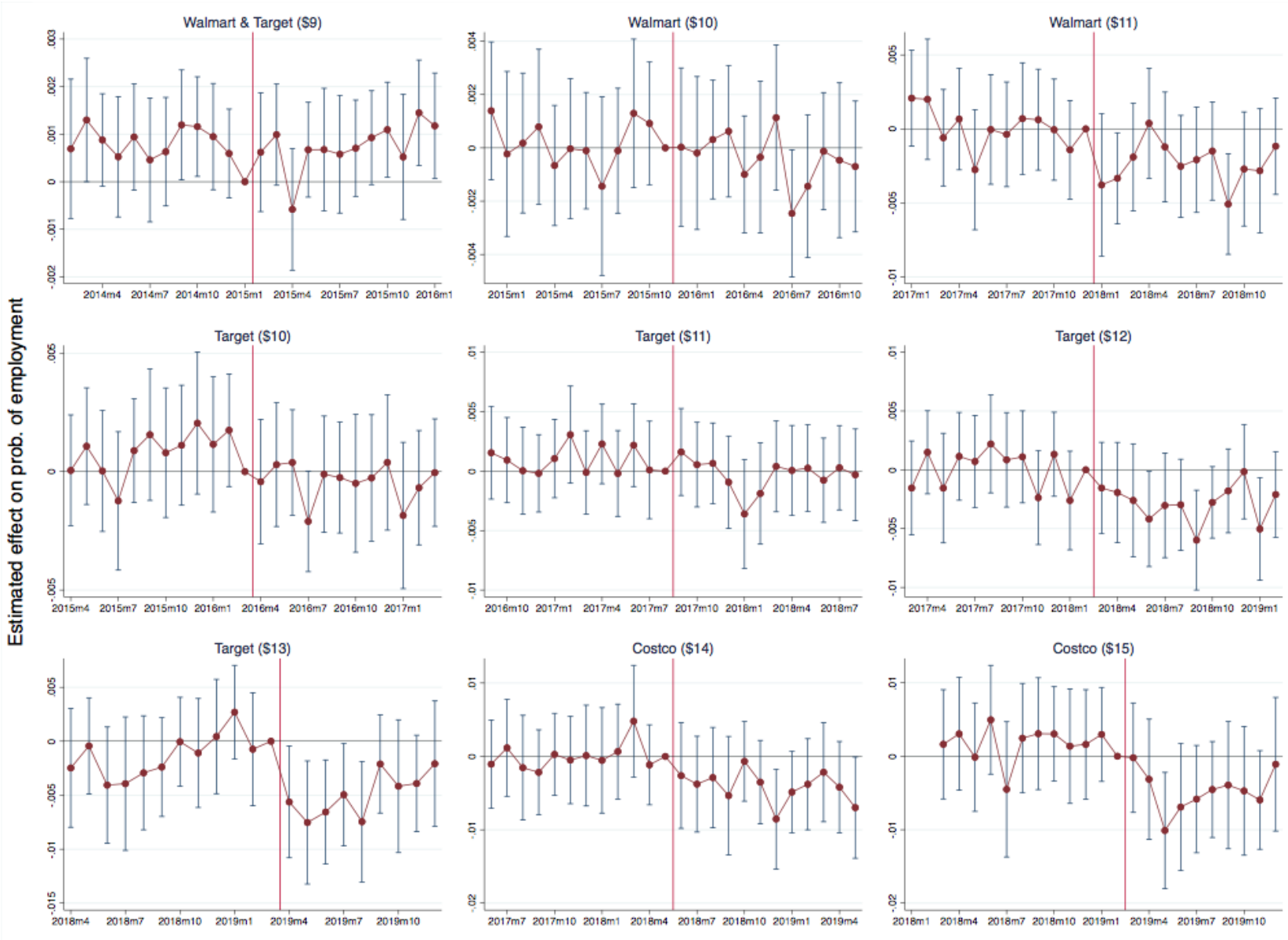
Notes: This figure plots the regression coefficients on the interaction between job-level exposure to policy firm minimum wages for non-policy employers and an indicator for post-treatment for placebo treatment dates, using a 4-month observation window. Coefficients are indexed by the last month of the observation period. For example, the coefficient at date equal to 0 is the coefficient on exposure interacted with an indicator for one month before zero and zero (the first month of treatment). Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ cell with wages below the policy firm minimum wage in the year before October 2018. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C12: Wage spillovers of Walmart, Target, and Costco MWs in the CPS



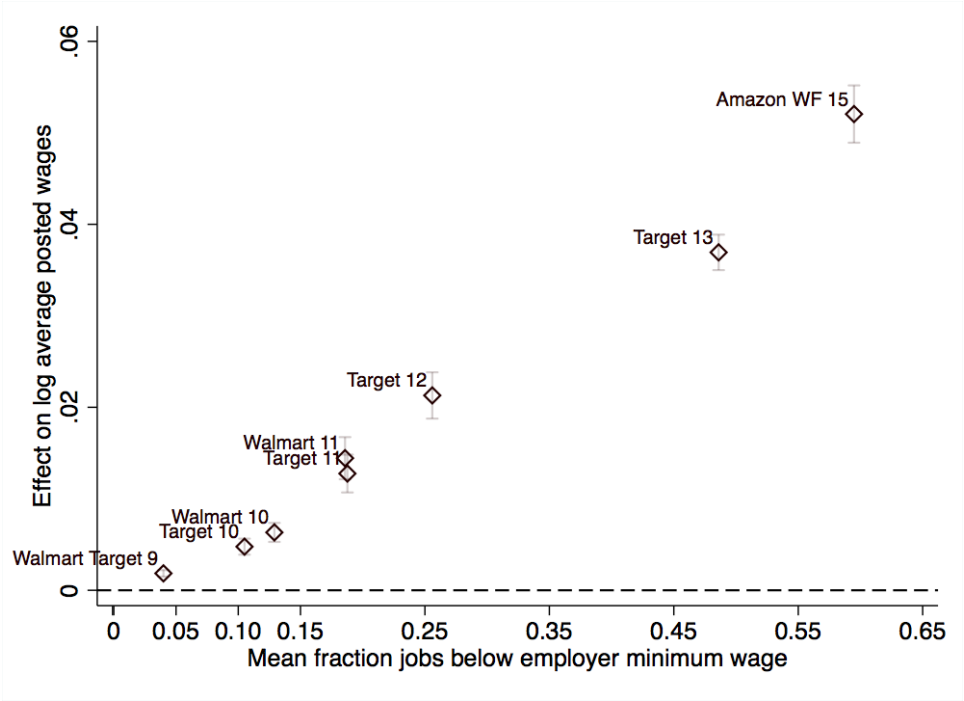
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy industries interacted with month fixed effects, where the dependent variable is log hourly wage. Exposure is defined as the fraction of non-policy industry workers in each occupation-CZ cell with wages below the policy firm minimum wage in the year before treatment. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Sample restricted to non-policy industry workers aged 25-65, excluding those missing occupation or hours information, the self-employed, and those usually working less than 3 hours per week. 95% confidence intervals shown. *Source*: CPS ORG.

Figure C13: Employment effects of Walmart, Target, and Costco minimum wages



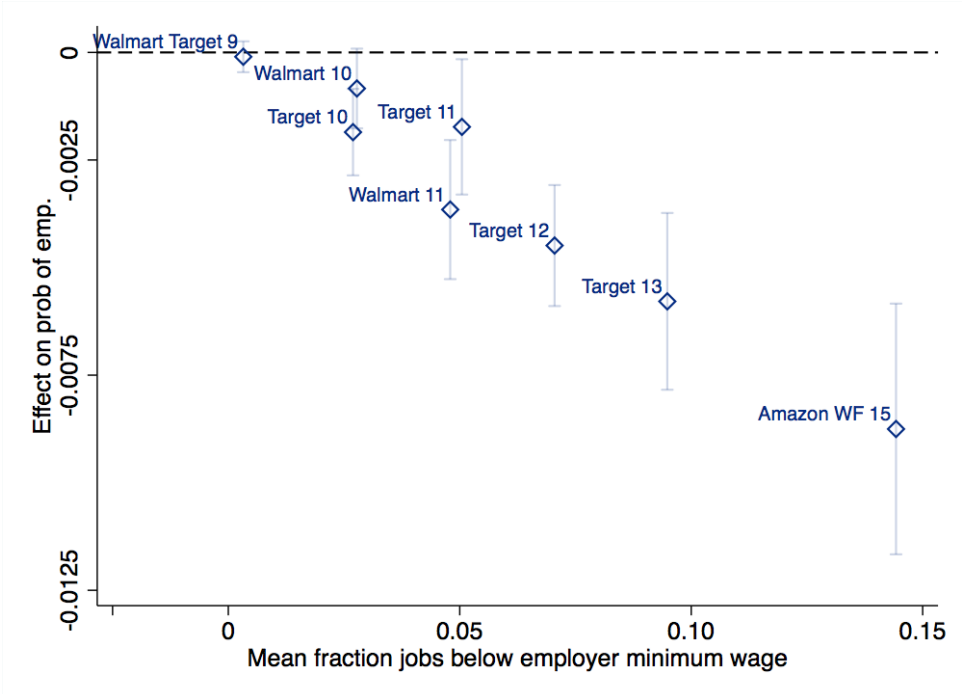
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy industries interacted with month fixed effects, where the dependent variable is probability of being employed vs. unemployed. Exposure is defined as the fraction of non-policy industry workers in each occupation-CZ cell with wages below the policy firm minimum wage in the year before treatment. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. Treatment is assigned to the unemployed based on their last occupation while employed. Sample is restricted to individuals aged 25 to 65 and excludes those not in the labor force. 95% confidence intervals shown. *Source*: CPS ORG.

Figure C14: Spillover effects increase with bite of employer minimum wage



Notes: This figure plots the coefficients on the interaction between exposure to Walmart's 2018 $11 minimum wage policy and month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of each non-Walmart employer's job postings with wages below $11 in the year before treatment. Employer, county, and month-by-occupation fixed effects are included. Sample restricted to non-Walmart employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure C15: Disemployment effects increase with bite of employer minimum wage

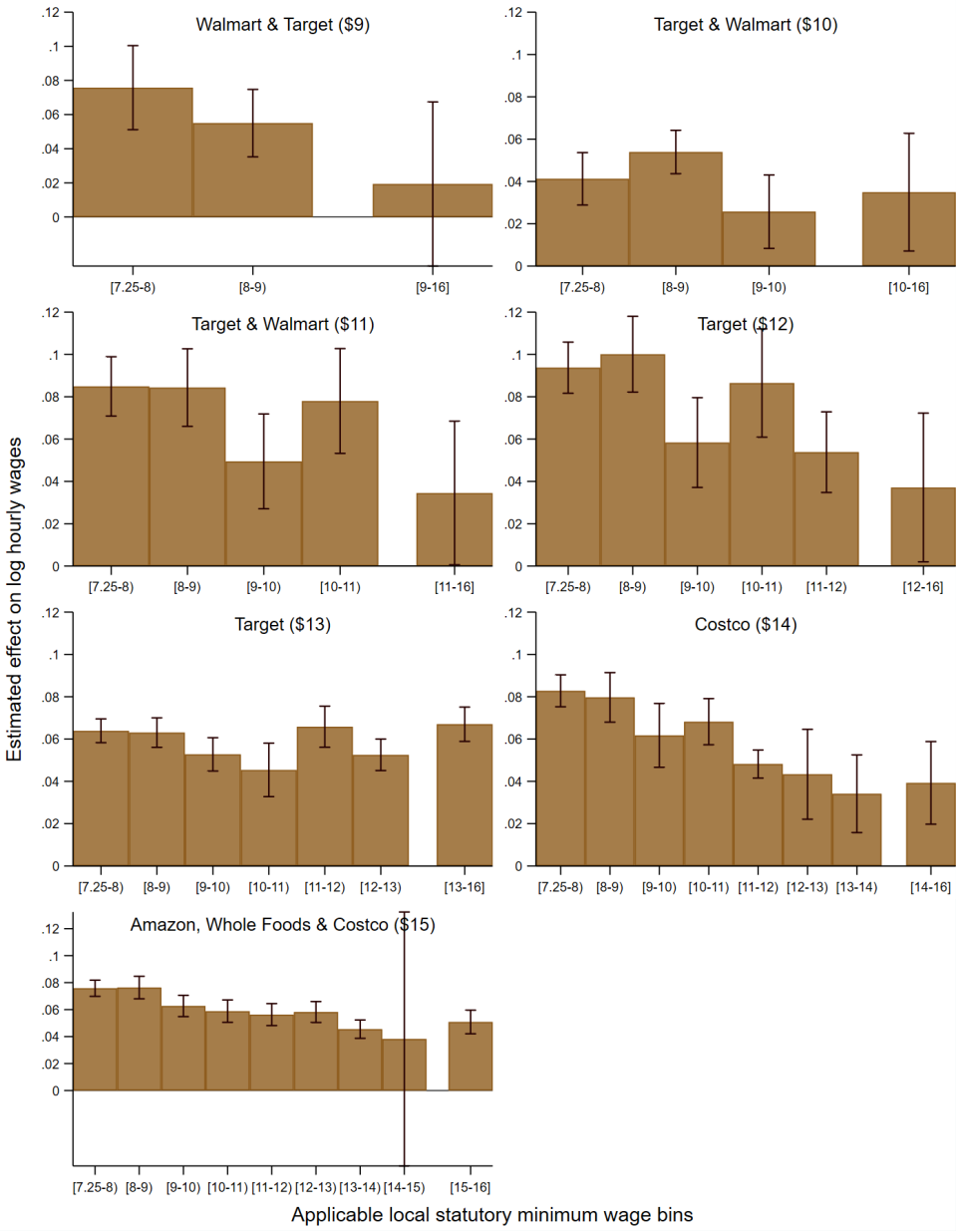


Notes: This figure plots the coefficients on the interaction between exposure to Walmart's 2018 $11 minimum wage policy and month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of each non-Walmart employer's job postings with wages below $11 in the year before treatment. Employer, county, and month-by-occupation fixed effects are included. Sample restricted to non-Walmart employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

D Additional evidence on local labor market moderators

Perfectly competitive models of the labor market posit that wages are the equilibrium outcome of labor supply and demand conditions. On the supply side, workers' preferences over leisure and their reservation wage due to the presence of outside options affect their probability of entering the labor force, the hours they choose to work, and their likelihood of moving across jobs. On the demand side, employers set wages based on the value they receive from the additional production by workers. What drives or mediates the transmission of wage policies across employers? We test the role of potential mechanisms by examining interactions between local moderating factors and our treatment variables, $D_{f,t-1} \times \mathrm{X}_{c,t}\mathrm{Post}$. Table 3 provides initial evidence. Labor market tightness as measured by the unemployment rate moderates transmission of wage policies. However, the interaction effect is small, leaving room for other local factors to determine the extent of wage spillovers.

Figure D1: Moderation of spillover effect with local minimum wage



Notes: This figure plots the coefficients on the interaction between exposure to the policy firm's minimum wage and an indicator for post, where the dependent variable is log advertised hourly wage. Each bar indicates a separate regression where only postings in the indicated minimum wage areas are included. Exposure is defined as the fraction of each non-policy job postings in specific employer-by-occupation-by-CZ cells with wages below the policy firm minimum wage in the year before treatment. Employer-by-occupation-by-CZ fixed effects and occupation-by-month are included. Sample restricted to non-policy employer postings with valid wage data and hourly rate of pay indicator, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.