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ABSTRACT

Low unionization rates, a falling real federal minimum wage, and outsourcing have hampered wage growth in the low-wage sector in the US. 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 broader labor market effects of these policies? In this paper, we study recent minimum wages by Amazon, Walmart, Target, CVS, 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, where the modal response was to match the wage announced by the large retailer. In the case of Amazon's $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 2.3% 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 large employer minimum wage policies led to small but precisely estimated declines in employment, with employment elasticities ranging from -0.04 to -0.13. Large employer minimum wage announcements influenced wages more broadly. The magnitude of these wage spillovers cannot easily be explained by standard competitive pressures, suggesting a role for both market power and norms in wage determination.

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1 Introduction

Declining labor market institutions like the falling real federal minimum wage and low union density characterize the low-wage sector in the United States, where wage growth has stagnated for the last 40 years.[1] With limited policy levers for boosting wages, worker advocates have called on large, retail and service sector employers to raise pay and act as standard bearers in the low-wage labor market (Thomas, 2017; Hamilton, 2018). In recent years, a number of high-profile companies—Amazon, Walmart, Target, CVS, and Costco, together employing nearly 2% of the total US workforce[2]—have indeed announced wage increases, instituting company-wide minimum wages for their workers.

We exploit these sudden, public changes in wage policy to estimate the impact on other employers. After minimum wage announcements by large retailers (whom we term "policy firms"), wages advertised by other employers ("non-policy firms") increase sharply and persistently. Further, wages at other firms bunch at the level announced by the large retailer. Spillovers tend to be larger in areas with low state and local minimums and in jobs at closer proximity to the large firm. In addition to the impact on wages, large employer minimum wages led to small, but precisely estimated, declines in employment, both in the aggregate and excluding the industry of the policy firm. The implied employment elasticities are small, ranging from -0.04 to -0.13, and similar to those from the recent minimum wage literature.[3] We find no evidence that wage increases are smaller in slack labor markets or larger in jobs where employers plausibly face greater competition from the announcing firm. Together, our findings point to factors beyond competitive pressures through which large companies influence wages throughout the low-wage sector.

Estimating spillover effects of employer minimum wages requires data with information on employer name and high frequency measures of hourly wages. To conduct our analysis, we use two such data sources, each covering millions of jobs: online vacancy postings from Burning Glass Technologies and worker salary reports from Glassdoor, a job search and review platform. Using these sources, we first show that when 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 calculate the bite of the large retailer's minimum wage as the fraction of job ads by other employers with pre-period wages below the announced minimum wage,

[1]See recent work on rising wage inequality and the erosion of labor market institutions by Piketty and Saez (2003); Kalleberg (2013); Osterman and Shulman (2011); Western and Rosenfeld (2011); Weil (2014); Autor et al. (2016); Katz and Krueger (2019); Song et al. (2019); and Mishel and Bivens (2021).

[2]Workforce estimates are from Amazon.com (2020); Walmart (2020); U.S. Bureau of Labor Statistics (2019); Target Corporation (2020a); CVS (2021); Costco Wholesale Corporation (2020).

[3]See, for example, Azar et al. (2019); Dube (2019); Cengiz et al. (2019); Derenoncourt and Montialoux (2021); Harasztosi and Lindner (2019).

within detailed occupation, employer, and commuting zone ("CZ") 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., 2021). 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.

We then use an event-study approach to estimate spillovers from major employers' wage policies to others operating in the same labor market, comparing jobs with higher exposure to those with lower exposure in the months before and after the announcement. 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 employer 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.[4]

We estimate substantial spillovers from Amazon, Walmart, Target, and Costco'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 analyze bunching in wages after the announcements and show that wages of other employers shift out of wage bins below and spike at the precise wage announced by the large retailer. These findings suggest that other employers are responding directly to the large firm's announcement rather than contemporaneous, but unrelated, labor demand shocks.

Still, we rule out several alternative explanations through a series of robustness checks. Our baseline specification, which includes occupation-by-month and CZ-by-month fixed effects, controls for simultaneous CZ-specific and occupation-specific demand shocks. 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. Thus, our findings are not driven by employers' selective inclusion or omission of wage information on ads for the highly exposed jobs. Instead, the increase in advertised wages translates into higher take-home wages for workers, as we show using data on worker-reported pay from Glassdoor. Across all major employer policy changes, workers at other employers report wage increases at magnitudes highly comparable to our results using vacancy data.

[4]Alternative pre-trends assessment drawing from the recent difference-in-differences literature further support our empirical strategy (Borusyak et al., 2021; Rambachan and Roth, 2021).

The company-wide minimum wages we study and the spillovers they induce provide direct evidence of employer wage-setting power over low-wage workers. In a perfectly competitive labor market, no single employer would have incentive to deviate from the market wage, as such deviations would incur higher costs and lower profits. Further, deviations from a "market" wage by some employers should have no effect on the wages of other employers for the same reason. Yet, we show that other employers not only adjust their wages, but even match the wage announced by the large retailer.

There is scant existing evidence on cross-employer wage spillovers in the US.[5] Staiger et al. (2010) study the effects of a wage policy at Veterans Affairs hospitals that increased the pay for registered nurses. They show that wages of nurses at neighboring hospitals also rose, with a cross-hospital wage elasticity of 0.19. Expanding on this base of evidence, we estimate wage spillovers from 10 large retailer policies and show their impact on tens of thousands of employers from a broad spectrum of industries. Despite differences between the two settings, our wage elasticity estimates are in line with this prior work. In the case of Amazon, we estimate an increase in average non-Amazon hourly wages of 4.5%. Given the increase in Amazon's own wages (approximately 20%), our results imply a cross-employer wage elasticity of 0.23.

Our paper relates to several literatures on wage determination, imperfect competition, and employer wage-setting power. An older literature focused on the "union threat hypothesis," or 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). A more recent literature documents the role of firms in wage setting using linked employer-employee administrative data, showing that firms explain a large share of wage variation across similar workers (Barth et al., 2016; Card et al., 2018; Song et al., 2019). Finally, a growing body of work provides direct empirical evidence of monopsony power and the impact of workers' outside options on wages (Caldwell, 2019; Caldwell and Danieli, 2018; Schubert et al., 2021; Azar et al., 2019).

Compared to the variation in wages analyzed by this prior literature, the voluntary minimum wage announcements we study represent a unique type of shock to labor markets. Their wide-ranging effects on the wages of other employers may also stem from the salience and visibility of the firms announcing the policies.[6] Though we cannot test the role of these norms-based mechanisms directly, we show through a series of analyses that standard competitive or demand-based mechanisms are insufficient for explaining our findings. Neither labor market tightness, nor the degree of large firm advertising

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

[6] For example, the tendency to follow national chains appears to have influenced closure decisions by local businesses during the COVID-19 pandemic (de Vaan et al., 2021).

in specific jobs, nor the likelihood that workers leave a given occupation to work at the large employer meaningfully moderate spillovers. We hypothesize that large retailers also influence wages through a norms or lighthouse effect, but we leave the exploration of this channel to future work.

Methodologically, we draw from the minimum wage literature, including analyzing shifts in the wage distribution in response to Amazon, Walmart, Target, or Costco's minimum wages using a bunching approach (Cengiz et al., 2019; Harasztosi and Lindner, 2019; Derenoncourt and Montialoux, 2021). 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. (2021) 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), thus leveraging variation within locations, within job categories, and within employers in the sensitivity of wages to large employers' policies. 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, providing clear evidence of minimum wage spillovers up the wage distribution (Autor et al., 2016; Haanwinckel, 2018; Fortin et al., 2021).

In addition to providing novel empirical estimates of employer wage-setting spillovers, our study speaks to the search for policy levers to improve wages in the context of low worker bargaining power. Targeted attempts to sway large employers with monopsony power may be effective at influencing wages more broadly.[7] Our setting also closely relates to prevailing wage policies for federal and state contractors (e.g. the federal Service Contract Act), which also seeks to set standards that can ripple throughout the labor market.

The paper is structured as follows. Section 2 provides an overview of the recent voluntary employer minimum wage policies we study. Section 3 describes our data sources on employer wages. In Section 4, we detail our empirical approach leveraging job-level exposure and report our main spillover estimates and robustness checks. Section 5 documents wage and employment effects in the CPS; Section 6 explores competitive pressures as a mechanism. Section 7 concludes.

[7] 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.

2 Voluntary minimum wages, 2014-2019

In recent decades, a number of institutional factors have placed downward pressure on wages in low-wage sectors. Unions have lost density or were never significantly present. Corporate outsourcing, subcontracting, and franchising have further depressed wages. Additionally, workers in the gig economy fall outside traditional federal and state legal protections and thus outside the scope of employment and labor law. In this context, wages at the bottom of the wage distribution have been stagnant or declining in real terms (Weil, 2014, 2017).

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

Following the Fight for $15 movement's launch and the pressure applied by the campaign on both government and private actors, a number of states 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, Target, and Costco, the four largest retailers announcing company-wide minimum wages in recent years. Between 2014 and 2019, these employers implemented a total of 10 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).[8] At $15 an hour, Amazon's minimum wage was more than double the federal minimum wage and far exceeds the majority of state and local minimum wages in the US.

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

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

Walmart, the largest employer in the US with a workforce of over 1.5 million, has implemented 3 company-wide minimum wage policies since 2015, and its minimum wage went from $9 to $11 by 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 increase was 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 steadily increased its minimum wage over the following three years, increasing it to $10 in April, 2016; $11 in September, 2017; $12 in March, 2018; and finally to $13 in April, 2019.[9] 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 in close succession, such as Walmart and Target's $9 minimum wages and Walmart and CVS's $11 minimum wages, we pool these natural experiments and examine their joint effect on employers operating in the same local labor market.[10]

Finally, big-box retailer Costco, a company employing 189,000 workers in the US, also announced increases to its company-wide minimum wage during this period. The firm

[9]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 COVID-19 pandemic recession, we do not include this most recent increase in our analysis.

[10]CVS announced its sole company-wide minimum wage of $11 just one month after Walmart announced an $11 minimum wage in January 2018. In the case of these announcements and Walmart and Target's $9 announcement, we exclude ads by both policy firms when studying wages spillovers and use the month of the earlier announcement as the treatment date.

announced an increase to \$14 from \$13 in May 2018 and from \$14 to \$15 in March 2019.

3 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.[11] 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 US labor markets (Deming and Noray, 2018; Deming and Kahn, 2018; Azar et al., 2017; Hazell and Taska, 2020). 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 actual pay workers report receiving.

3.1 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, 2020; Carnevale et al., 2014).[12] 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 vacancy 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 timing of the announcements.

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

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

[12] Job postings are at the establishment level.

pay. For example, for hourly wage jobs, the posted minimum hourly wage is available. This is the direct outcome of interest in this study as we are evaluating whether large employer wage policies influence the wage-setting behavior of other employers.

Employer and other information Approximately 154 million job postings in the BGT database contain information on the employer posting the vacancy (February 2012 to February 2020). Almost all postings (98.8%) contain detailed information on the location of the job (valid state and county FIPS codes); 96.2% contain occupation information (6-digit SOC codes); and 63.8% contain industry information (3-digit NAICS codes).

Representativeness of BGT data A number of papers using BGT data have analyzed its representativeness.[13] We conduct our own comparison of the occupation, industry, and geographic distribution of 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 workers in the CPS, a higher share of hourly job vacancies are 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 February 2014 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 limit the sample to those jobs for which the pay frequency is hourly. We further restrict the data to focus on specific observation periods of 24 months around the wage policy changes analyzed below. 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

[13] Hazell and Taska (2020) 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 (2020) confirm that industries that are less likely to post vacancies online are underrepresented in BGT relative to CPS. Studies by Azar et al. (2020); Deming and Noray (2018); Deming and Kahn (2018) provide further evidence on the value of and validity of BGT data.

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, 92% of all BGT postings fall in CZs in which Amazon advertised in the year prior to Amazon's minimum wage.[14] We provide additional details on the BGT data in Appendix B.

3.2 Glassdoor

Glassdoor is a two-sided online job search and review platform where employers post vacancies, but importantly, users of the platform can also upload information about salaries for specific job titles at specific firms. For hourly workers, pay information contains the exact 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 the wages workers report receiving. Wage changes estimated using these data confirm that spillovers are not driven by systematic changes in which jobs are advertised online, as opposed to genuine shifts in wages at the non-policy firms.

4 Wage spillovers from employer minimum wages

The use of company-wide wage floors by large employers represents a break from localized wage setting, potentially in response to the Fight for $15 movement's call for higher wages in the retail and service sectors. We estimate the spillover impacts of these wage policies on the wages of other firms in the same labor markets. These shocks differ from shocks to narrowly defined sectors, such as the market for nurses, in that they potentially apply more broadly to multiple occupations and industries in the low-wage sector. We explicate our empirical strategy below in Section 4.1 using Amazon as a case study. Section 4.2 presents the spillover effects from Amazon's $15 minimum wage. In Section 4.3, we report the results for the remaining 9 employer minimum wage changes we study, by Walmart, Target, and Costco.

4.1 Empirical strategy: job-level exposure

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., 2021). 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

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

jobs are defined as employer-by-occupation-by-CZ cells. Our key treatment variable is the fraction of postings in each job cell that are below \$15 in the year before Amazon's policy was announced in October 2018.

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

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

Therefore, in the case of Amazon, we calculate the fraction of postings appearing between October 2017 and September 2018 with wages below \$15. We restrict our analysis to the commuting zones where Amazon advertised in the year before its announcement.[15] In practice, this restriction does not greatly affect the sample size as 92% 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-announcement postings, over 700 six-digit occupational categories, and 188 commuting zones in which Amazon or Whole Foods advertise. On average, about 56% 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 conduct robustness checks to rule out alternative stories for wage increases at non-Amazon employers. Section 4.2.1 presents these robustness checks in great detail.

Event-study and difference-in-differences design We conduct event-study and difference-in-difference analyses around the time of Amazon's and other employers' minimum wage policies to estimate spillovers. Our empirical strategy exploits both variation in exposure to employer policies as well as the precise timing of the announcements. Specifically, we estimate the following model:

$$\log w_{it} = \alpha + \sum_{k=-12}^{11} \beta_k D_{j(i)} \times \mathbb{1}_{[t=k]} + \eta_{j(i)} + \delta_{c(i)t} + \chi_{o(i)t} + \varepsilon_{f(i)} \tag{2}$$

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

[15] 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.

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 the first 18 months of observation window around the Amazon policy announcement, the average advertised hourly wage in our BGT sample declined from \$19 to \$16, suggesting an increasing share of lower paid jobs being advertised online (see Appendix B for descriptive statistics on wage trends in the BGT data). 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 wage increases. The treatment month is denoted k=0 and is omitted for the model to be identified. We cluster standard errors at the employer level ($f(i)$).

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

$$Y_{it} = \tilde{\alpha} + \tilde{\beta} D_{j(i)} \times \text{Post} + \tilde{\eta}_{j(i)} + \tilde{\delta}_{c(i)t} + \tilde{\chi}_{o(i)t} + \tilde{\varepsilon}_{f(i)} \tag{3}$$

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 two phenomena. First, we can assess whether non-policy employers tend to match the wage level announced by the large employer, suggestive of a lighthouse effect of large employer policies. Second, we can examine the extent of spillovers up the wage distribution in response to the announcement.

Assumptions and validity of empirical strategy 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. We provide evidence of parallel pre-trends as well as a sharp increase in wages immediately after the announcement as corroborating evidence that this assumption holds. We conduct a number of robustness checks in Section 4.2.1 that the spillover wage effects we estimate stem from large employer announcements as opposed to contemporaneous shocks to low-wage jobs.

A recent literature on difference-in-differences has highlighted concerns regarding the aggregation of heterogeneous treatment effects using OLS as well as the validity of visual parallel pre-trend testing (Borusyak et al., 2021; Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2020; Rambachan and Roth, 2021). Rather than estimate a single average spillover effect by aggregating estimates across voluntary minimum wage announcements, we study each announcement separately (there are 10 total). Because these large retailers operate nationally and announce company-wide minimum wages, our empirical strategy leverages continuous variation in the "bite" of the announced minimum wage rather than a binary comparison of treated and never-treated units. Supporting our estimation strategy, however, we show that wages at other employers bunch at the announced minimum wage of the large retailer and that spillovers vary monotonically with the bite and level of the announced minimum wage. Finally, we show robustness of our estimated spillover effects to alternative assumptions of wage trends in the pre-announcement period in Appendix D.4.

4.2 Spillovers from Amazon's $15 minimum wage

We observe substantial spillovers in wages resulting from Amazon's $15 minimum wage. Figure 4 plots β_k from 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 after treatment in October 2018. This effect strengthens over the 12-month post-treatment period, rising to about 10 log points.[16]

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 4.3 extends these findings to Walmart, Target, and Costco's recent minimum wage increases and relates the extent of spillovers to the average bite and level of the large employer's minimum wage.

To bolster our evidence that this sharp increase in the wages of non-Amazon employers 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 $\tilde{\beta}$ coefficients from regression equation 3, where the outcome variable is an indicator for the hourly wage falling within a specific wage bin, with separate regressions for each bin. The figure shows that exposure to Amazon's policy is associated

[16]We show in a robustness check that pre-trends are relatively modest throughout a 24-month pre-period in Appendix Figure D1. Wages are gradually trending up in highly exposed jobs, likely due to wage growth at the lower part of the wage distribution—approximately 3 log points over the two-year period. By contrast, moving from zero to 100% exposure is associated with a 5 log-point jump in wages in the exact month Amazon's announcement.

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 the announcement—of 5 percentage points—with significant drops from $9 to $14 dollars. This evidence suggests employers were responding specifically to Amazon's minimum wage by targeting the announced wage, resulting in post-period wages concentrated at $15. Despite stemming from a different mechanism—responses to voluntary minimum wage announcements by large retailers—our finding of modest spillovers to higher wage bins is consistent with recent minimum wage papers finding spillovers to wage levels above the statutory minimum wage.[17]

4.2.1 Ruling out alternative explanations

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

Occupation-by-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 fixed effects.[18]

[17]See, for example, Dube (2019), Engbom and Moser (2021), and Haanwinckel (2018). Drops at $10 and increases at $20 are also consistent with evidence from Dube et al. (2017) that employers tend to set wages at round numbers, suggestive of both employer mis-optimization and wage-setting power in labor markets.

[18]Appendix Figure D2 reports robustness of our event-study estimates to the inclusion of these shocks.

Employer decision to post wage As discussed in Section 3.1, 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. In column 3 of Table 2, we directly control for the share of an employer's ads with wage information in the regression to see how this affects our estimated coefficient $\tilde{\beta}$. Directly including the wage posting probability in this specification has no effect on the magnitude or precision of the estimated impact of Amazon's policy.[19]

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 ensure 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, 2021). If the sharp increase in wages is driven by 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 the announcement, 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

[19] In Sections 4.2.2 and 5.1, we document increases in worker-reported wage of comparable magnitude to the increase in advertised wages. This provides further evidence that employers' decisions to post wages on job ads are unlikely to drive our findings.

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. The fact that it does not fall to zero is consistent with the steady increase in the treatment effect, as can be seen in Figure 4.

Functional form We explore sensitivity 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 D4 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 D5 plots the effect of being in the fully exposed group relative to the partially exposed group, over time. These results replicate our baseline findings that use a linear specification with continuous treatment with a more non-parametric approach.

4.2.2 Increases in worker-reported wages on Glassdoor

The results thus far strongly support the hypothesis that non-Amazon employers responded to Amazon's minimum wage policy by adjusting their own advertised wages. But these results do not speak to whether workers at non-Amazon employers genuinely earned higher wages after these changes. To test whether spillovers in advertised wages translated into true wage gains for workers, 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.[20]

As described in Section 3.2, 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 logged worker-reported hourly wages as the outcome, including the same

[20]It's worth noting that Amazon's policy, as well as Walmart, Target, and Costco's, applied to incumbent workers, not just new hires (see Appendix A). Another advantage of using Glassdoor data, which contains survey responses from current employees, is the ability to estimate spillovers to incumbent workers whereas advertised wages may only apply to new hires.

set of baseline controls.[21] Appendix Figure D9 depicts the results from this analysis. The results show a sharp increase in wages at more exposed jobs beginning in the month of the policy change. Prior to the 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 nearly 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 translated into increases in received wages.

4.3 Effects of other large retailer minimum wages

A number of other retailers announced voluntary minimum wages in the period 2014-2019 (see Figure 1). We use these wage shocks to further explore the nature of spillover effects. Our empirical strategy for Walmart, Target, and Costco is identical to the one outlined above and in equation 2. Our baseline specification 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. We do the same for Walmart and CVS's \$11 announcements.

Figure 7 shows the estimated spillover effects for the minimum wages announced by these three companies, over the study period. In all cases, the results indicate sharp increases in wages at more exposed jobs immediately in the month of the announcement. We perform similar robustness checks on these results as those for Amazon in Section 4.2; these are reported in Appendix D. Results for Walmart, Target, and Costco are robust to including occupation-by-CZ fixed effects as well as employer-by-month fixed effects (see Figures D10 and D11).

We verify these spillover effects on wages using data from Glassdoor that provides worker-reported wages in Figure D12. As in the case of Amazon, voluntary minimum wage announcements by Walmart, Target, and Costco increase worker-reported wages among other employers in their relevant labor markets.

Figure D13 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.[22] Finally, we test to see that the results for the other retailers withstand a placebo

[21]Glassdoor provides the city of the worker, as opposed to county of posting provided in 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.

[22]In the case of Target's \$13 minimum wage, bunching also occurs at \$14 and \$15, potentially due to

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 D14 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.

4.4 Local moderators of wage spillovers

To better understand mechanisms behind wage spillovers, we examine two potential sources of local moderation.[23] First, we examine whether spillovers decay with distance from the large retailer announcing the increase, by examining jobs advertised in a different city (within the same CZ) from the large retailer as opposed to jobs in the same city. Table 3 shows the coefficient on the triple interaction of exposure to the large firm's minimum wage, an indicator for the post-announcement period, and an indicator for being in a different city as the large retailer. Each column represents a different minimum wage announcement. The coefficient on the triple interaction is negative in all but one case, Walmart's $10 announcement, and statistically significantly negative in a majority of the cases, indicating smaller spillovers further from the large retailer.

Reactions to large retailers' minimum wages are also likely mediated by the level of state and local minimum wages in the labor market where the national retailer operates. If city or state minimum wages are above the firm's announced minimum wage, we would not expect large spillover effects in these areas. We examine this in Table 4 and Appendix Figure E1. We do so 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 wages. Table 4 reports the coefficient on the triple interaction of exposure to the large firm's minimum wage, an indicator for the post-announcement period, and an indicator for the announced minimum wage exceeding the highest locally applicable government minimum wage.

Up to voluntary minimum wages of $12 per hour, spillovers are almost entirely driven by locations with a smaller statutory minimum wage. Above $12 an hour, the results are more nuanced. In the case of Target's $13 and Amazon and Costco's $15 minimum wages, spillovers are if anything larger in areas with local minimum wages that are at least at the level of the announced minimum wage. This may be due to wages increasing beyond the announced minimum wage level to higher wage bins as indicated Figures 5 and Appendix Figure D13. In the case of Costco's $14 minimum wage, results are larger

the close timing with Costco's $15 minimum wage announcement one month after.

[23] We adapt our main exposure variable to be the fraction of jobs below the policy firm's minimum wage in employer-by-occupation cells rather than employer-by-occupation-by-CZ cells, as the latter is correlated the moderator of interest.

in areas with a statutory minimum wage below \$14, but they are also present in areas with higher minimum wages.

4.5 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 a policy firm's wages, what is the percent increase in average wages among non-Amazon employers?

We compute two kinds of cross-employer wage elasticities: one with respect to the observed increase in policy firm average wages ("average employer wage increase"), and the second with respect to the increase in the firm's announced minimum wage ("statutory employer minimum wage increase").

To obtain the percent increase in average hourly wages at non-policy employers, we rescale spillovers to represent the effect of going from 0% to average pre-period exposure across jobs. For example, in the case of Amazon, we present results of the impact of $\frac{\beta_k}{0.56}$, as the average job had 56% of pre-period postings below \$15. As can be seen in Figure 8, these normalized spillovers increase monotonically in the level of the announced voluntary minimum wages announced by these three major employers. In Appendix Figure E10, we document the same monotonically increasing relationship between spillovers and the degree of exposure among non-policy jobs to the large employer's minimum wage (ranging, again, from 3% for Walmart's 2015 announcement to 56% for Amazon's 2018 increase).

For Amazon, Target, and Costco's most recent announcements, we observe a sufficient number of job ads to measure the increase in their average hourly wages across the pre- and post-period. For these three announcements, we compute the following wage elasticity with respect to the policy firm's average increase:

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

For earlier policy changes, e.g., those prior to late 2018, there are insufficient observations in BGT data to reliably measure the increase in policy firm average wages. For these, we compute the wage elasticity with respect to the policy firm's statutory minimum wage increase:

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

where $\%\Delta MW^{\text{policy firm}}$ is the percent increase in announced minimum wages. The vast majority of the statutory increases are $1, as in the case of Walmart, Target, and Costco's increases.

For the first company-wide minimum wage, we take the midpoint of any previous minimum wages that may have varied regionally.[24] 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, thus we use the midpoint of $11.75.

Figure 9 plots the cross-wage elasticities for each announcement. Elasticities with respect to the policy firm's statutory increase range from 0.02 (Walmart and Target's $9 in 2015) to 0.67 (Costco $15).[25] Elasticities with respect to policy firm's average wage increase are 0.22 (Target $13), 0.23 (Amazon $15), and 0.33 (Costco $15). Thus, in the case of Amazon, for example, the interpretation of this cross-wage elasticity is that for a 10% increase in Amazon's average wage, wages at non-Amazon firms rise by 2.3%.

Comparison to wage spillovers literature As a comparison, Staiger et al. (2010) estimate cross-employer spillovers in the context of a wage policy change at Veterans Affairs hospitals applying to registered nurses. The authors find elasticities ranging from 0.19 to 0.28.[26] Willén (2021) studies a law decentralizing teacher wages in Sweden and estimates a cross wage elasticity to substitute occupations for teachers of 0.36.[27] An alternative benchmark is Hjort et al. (2020)'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.[28] Thus, our estimated average wage elasticities are very similar to these previous estimates despite the differences in institutional context, industry, and potential mechanisms. We conclude that voluntary wage increases by major employers elicited significant responses by other

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

[25] Target's $13 minimum wage announcement occurred just one month before Costco's $15 announcement in April of 2019. Thus, we suspect this second announcement may also be influencing other employers in CZs with both Costco and Target.

[26] 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.

[27] We calculate this cross-wage elasticity by dividing the wage effect of the reform for substitute occupations by the wage effect of the reform for teachers (see Panel A of Table 5 vs. Panel A of Table 3 in Willén (2021).)

[28] Given that we are estimating propagation across employers rather than across establishments within an employer, the Staiger et al. (2010) and Willén (2021) estimates represent a closer reference point.

employers in their labor markets, with spillovers up to a third of the increase in policy firm average wages.

5 Wage spillovers and employment effects in the CPS

The results above indicate that voluntary minimum wages by large retailers significantly increased the wages of other employers in their labor markets. What other adjustments did employers make in the wake of these wage policy changes? In particular, did employment at other firms change in response to large retailer minimum wages? The data used in the prior section of the analysis do not contain measures of employment at non-policy firms. Thus, to explore the impact of large retailer minimum wages on employment, we turn to the Current Population Survey ("CPS").

The CPS does not ask individuals for the name of their employer. Thus, to estimate spillovers from employer minimum wages in the CPS, we exclude the policy firm's industry from the sample and define exposure as the fraction of workers earning below $15 an hour at the 4-digit-occupation-by-CZ level.[29] Although this limitation means we cannot exploit variation in exposure across employers within occupation-CZ cells, we show we are still able to detect precise and sizable spillovers with this design.

We first present the results for Amazon's minimum wage. Throughout these analyses, we restrict our sample to individuals in the CZs in which Amazon advertises, obtained from BGT postings data. For our wage analysis, we focus on employed individuals working at least three hours a week and aged 25-65. For additional details on the CPS data, including our sample restrictions and the level of geographic detail available in the survey, see Appendix C.

To ease eventual comparisons across different employer wage policies, which have varying degrees of bite across jobs in the sample, we again normalize the treatment variable by the average fraction of postings below the policy firm's minimum wage. The coefficient on exposure interacted with month can be interpreted as the wage increase (or change in employment) for the average job after the policy firm's announcement.

5.1 Wage effects

We estimate wage effects in the CPS 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,

[29]We exclude electronic shopping and grocery stores (Whole Foods) for analyzing Amazon's policy; department stores and discount stores for Walmart, Target, and Costco; and drug stores and pharmacies for CVS.

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 using BGT and Glassdoor data, 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 a 6 log-point increase in wages over the post-period, relative to the pre-period.

We extend our analysis of wage spillovers in the CPS to the other 9 employer minimum wage policies we study. Figures E2 reports these results and shows wage increases in line with our findings from BGT and Glassdoor data for these company policies.

5.2 Employment effects

To estimate the effects of Amazon's minimum wage on non-Amazon employment, we use variation in bite by occupation-CZ cells, where the occupation is the last occupation of the unemployed. In the CPS, 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. If Amazon's announcement causes individuals not in the labor force to start searching for work and therefore be categorized as "unemployed," this could also lead to increases in the probability of unemployment and should be taken into account when interpreting the results.

Figure 11 reports the estimated effect on the probability of being employed, plotting β_k from equation 2. All of the point estimates in the post-period on are negative, and 4 out of the 12 post-treatment estimates are significantly different from zero. Figure F4 extends the employment analysis to each of the other 9 employer policy changes. For the smaller employer minimum wages, we find no statistically significant effects on employment. However, for the larger minimum wages, we find effects comparable to Amazon's.

Figure 12 summarizes the employment effects across all employer policies and shows that like the wage effects, employment effects are more pronounced the larger the level of the major employer's minimum wage (Appendix Figure E11 shows the analogous figure for the bite). Figure 13 plots the wage effect of each policy in relation to the employment effect for all 10 policies. The strong linear relationship between the wage and employment effects suggests a relatively uniform employment elasticity across the different policies, which we directly examine next.

Table 5 reports for each policy the difference-in-differences estimates of wage and employment effects and provides estimated employment elasticities. The first panel reports the employment effects, which range from 0 percentage points after Walmart and Target's $9 minimum wage to -0.8 percentage points after Amazon and Costco's $15 minimum wages. The second panel reports the wage effects, ranging from 0.2 log points after Walmart and Target's $9 minimum wage to 8 log points after Amazon and Costco's $15 minimum wages. The implied employment elasticities with respect to own-wage are reported in the third panel and range from -0.04 to -0.13.[30] Table 6 reports the aggregate wage and employment effects, including the industry of the policy firms. The results are virtually unchanged, indicating that any increases in policy firm employment after wage increases is not enough to offset declines among other employers.

How do our estimated employment elasticities compare to the minimum wage and monopsony literature? Figure 14 presents are largest and smallest employment elasticities in context. Our estimates are well within the estimates of the larger literature, implying relatively small negative employment effects on net arising from large employer minimum wages.[31] We note that these average elasticities may mask heterogeneity across different labor markets. Azar et al. (2019) find, for example, that the employment effects of government minimum wages differ based on the degree of local labor market concentration.

Exploring other margins of adjustment We explore other margins of employer adjustment using the BGT job ads data. Increased labor costs may lead non-policy employers to cut back on hiring or target more experienced or more educated workers for the now higher wage positions. Though we lack data on the number of positions non-policy firms are advertising for, we explore whether the number of postings by these firms changes. We also examine whether postings at non-policy employers are more likely to have experience or degree requirements after policy firm minimum wage announcements. Specifically, we explore whether employers decrease the number of postings, add experience requirements, or add degree requirements to their job ads in the post-announcement period.

Number of postings We find no evidence in a decrease in the number of postings, as shown in Figures E4 and E5. The outcome variable is the log number of postings. It should be noted, however, that the number of postings is an imprecise measure of firm

[30]We calculate the employment elasticity by dividing our estimated employment effect, normalized by mean employment, by our estimated wage effect.

[31]Staiger et al. (2010) provide a labor supply elasticity. The point estimate is positive, consistent with oligopsonistic competition under certain conditions, but the estimate is not statistically different from zero.

labor demand, as a single job ad can advertise multiple positions. Thus, there may be a decrease in the number of de facto positions available with each posting, which we are not able to measure.

Experience requirements on job ads We also see little evidence of changes in experience requirements in job ads after policy firm minimum wage announcements. Here, the outcome is an indicator for whether any experience requirement appears on the job ad. Results are reported in Figures E6 and E7.

Degree requirements on job ads We find no systematic evidence of increases in degree requirements. The experience requirement outcome is measured as the presence of any experience requirement (e.g., positive years of experience) in the job posting. Figures E8 and E9 report the results.

In the case of Target and Costco's \$13 and \$15 respective minimum wage announcements, we estimate a positive effect on the presence of degree requirements among non-policy firm postings starting about six months after the announcement. In other cases we estimate declines, also several months after the announcement. Given volatility in the number of postings with degree requirements, we interpret these results with some caution.

6 Evaluating competitive pressures as a mechanism

Why do the wage policies of large employers propagate to others? One possibility is that Amazon hires a large share of the local labor market after their voluntary minimum wage announcement. This loss of workers could lead to increased labor demand at other firms, driving up wages. In a simple competitive model with a large labor demand elasticity of -1, the minimum hiring Amazon would have to do to induce the wage increases we observe is around 4.5% of the local labor force. By contrast, hiring announcements by Amazon rarely exceed 1% of a local labor market.[32] Additionally, we estimate employment elasticities closer to -.1, on average, an order of magnitude smaller than the elasticity needed to generate large wage effects even through implausibly high local hiring rates by Amazon. Additionally, if competitive pressures were a key mechanism, then tightness in the labor market would also moderate wage effects, presumably for low-wage jobs especially. However, we find no interaction between wage spillovers and the pre-announcement unemployment rate (see Appendix Figures F1 and F2.)

Another potential explanation is strategic interactions between firms with wage-setting power (Berger et al., 2019). Wage increases by Amazon may induce competitor

[32]See Appendix F.1 for details on the data we collected on Amazon's local hiring announcements.

firms to increase their wage and limit the flow of their workers to Amazon. In this case, wage effects would be more pronounced among those firms that more closely share a labor market with Amazon. We test this hypothesis in two different ways. First, we examine whether wage increases are more pronounced in occupations where the policy firm makes up a large share of the vacancies for that occupation. Second, using resume data from Burning Glass Technologies, we test whether wage increases are more pronounced among occupations at non-policy employers with a high fraction of workers moving to the most common occupations at the policy firm (for example, a food service worker who moves to a hand packer job at an Amazon warehouse).[33] We see no clear moderation of wage spillovers by either measure of labor market proximity between non-policy firms and policy firms (see Appendix Sections F.3 and F.4).

Taken together, this evidence suggests that there are factors beyond labor demand and competitive pressures that influence the wage-setting behavior of low-wage employers in the US. In the wake of minimum wage announcements by major companies, norms around wages may have shifted. Investigating these other potential mechanisms for wage spillovers is an important direction for future research.

7 Conclusion

Highly publicized voluntary minimum wages by large retailers have had ripple effects across the low-wage sector in the US. These policies have emerged in the context of a declining real federal minimum wage, low union representation, and factors such as outsourcing and non-compete agreements that have been shown to drive down wages. Worker advocates calling on large companies to increase wages argued that well known firms could act as standard-bearers in the low-wage sector, inducing other employers to increase wages as well. We find evidence consistent with this claim.

In this paper, we assessed the spillover effects of 10 voluntary minimum wage announcements by Amazon, Walmart, Target, Costco, and CVS–companies who collectively employ over 3.5 million workers in the US. Using job ads data, we find that these announcements prompted wage increases at other employers in the same labor market. The magnitude of these spillovers was substantial. For example, a 10% increase in hourly wages at Amazon led to a 2.3% increase in hourly wages at non-Amazon jobs in the same CZ. More broadly, after each large firm's announcement, wages at other employers bunched at the exact new minimum announced by the large employer.

Turning to the effect of these announcements on employment using the CPS, we find small declines in employment, with employment elasticities ranging from -.04 to -.13,

[33]For sample job histories of Amazon workers, see Appendix Table F1.

quite similar to those estimated in the recent minimum wage literature.

These spillover effects provide direct evidence of labor market power in wage setting and strategic interactions between firms in the low-wage sector. Yet we also show that neither labor market tightness nor plausible measures of inter-firm competition for workers can explain the size of the spillovers we observe. Instead, it seems likely that other factors, such as norms or the lighthouse effects of large employers, may explain the propagation of these policies to other firms.

Our evidence contributes to current debates on the presence and impact of employer market power and the search for public policies to address it. Our consistent finding that employers rapidly match salient wage levels in local markets also has implications for government policies that seek to influence wages through standard setting, for example, via federal contractor minimum wages. Better understanding the mechanisms through which large actors shift wages is an important topic for further study and can help inform policies aimed at reducing wage inequality.

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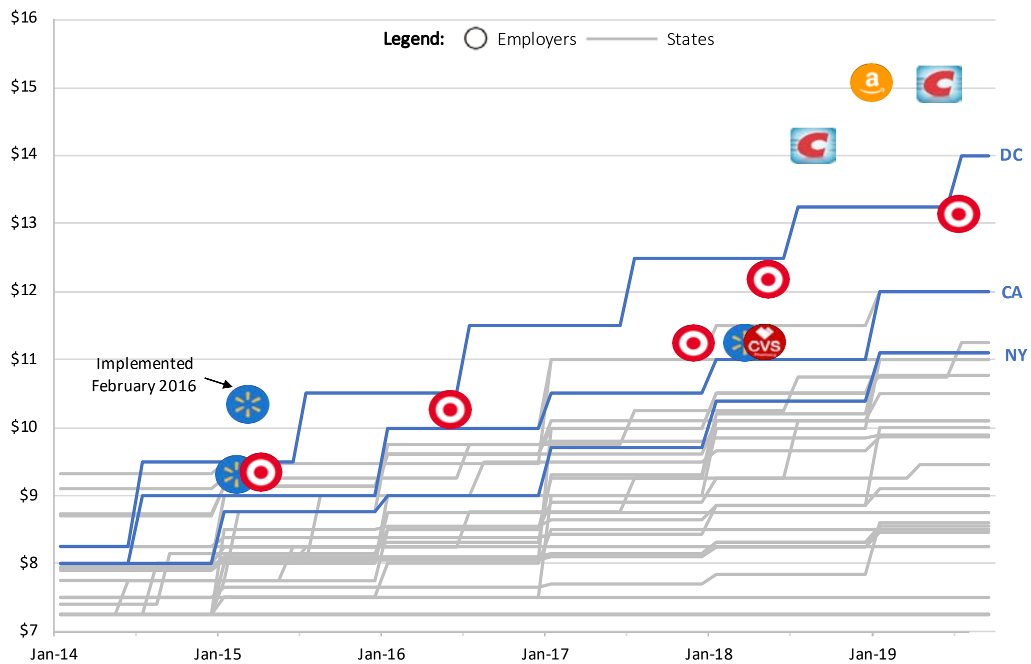
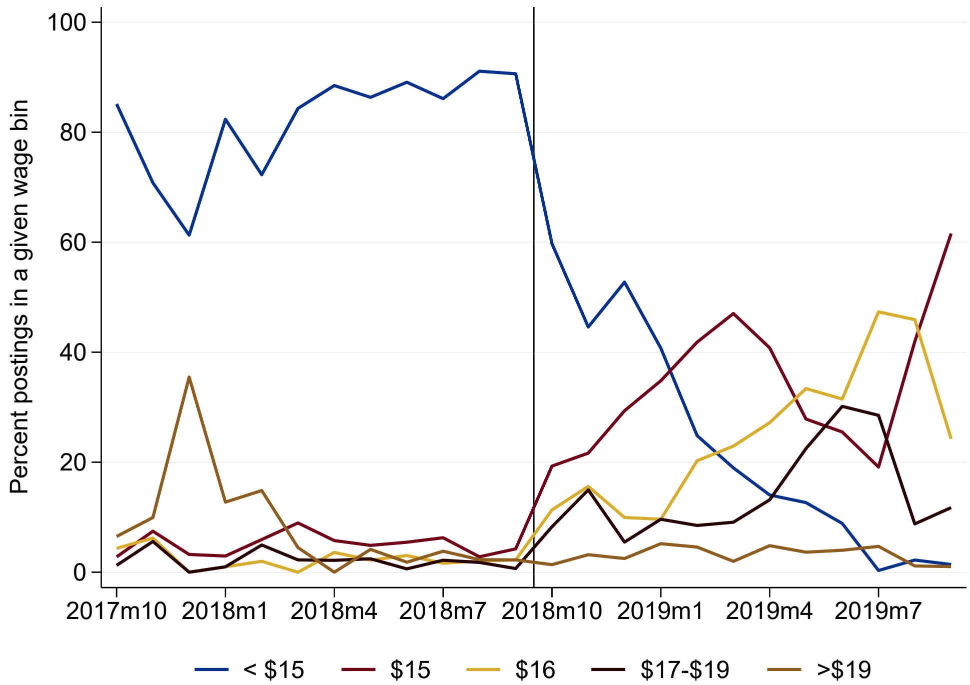
Figure 1: Voluntary employer and state minimum wages, 2014-2019



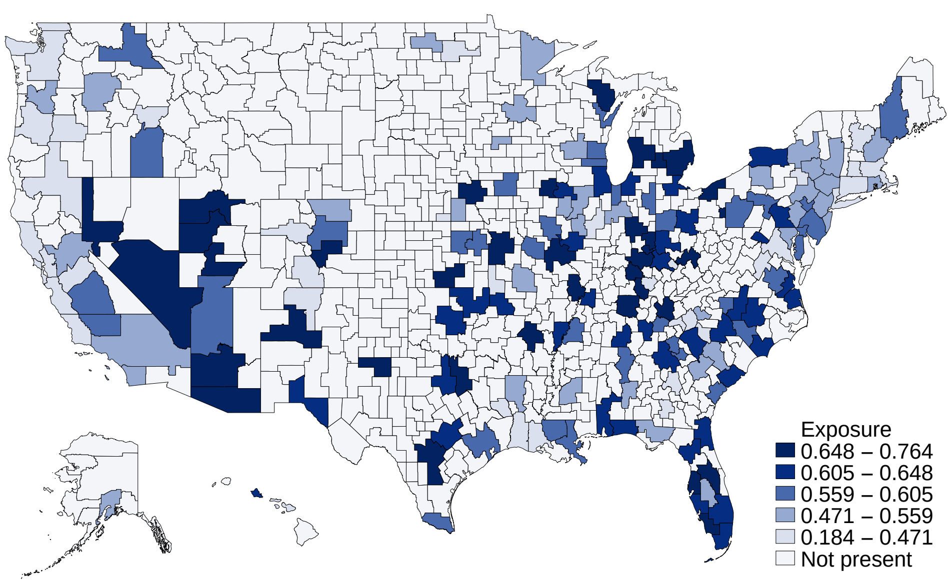
Notes: This figure plots voluntary employer minimum wage increases that have been announced in the US between 2015 and 2019. Gray lines indicate state minimum wages above the federal minimum wage of \$7.25. Select states are shown in blue. Employer logos show treatment firms (Walmart, Target, and Amazon/Whole 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. *Data sources*: National Employment Law Project and authors' research.

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



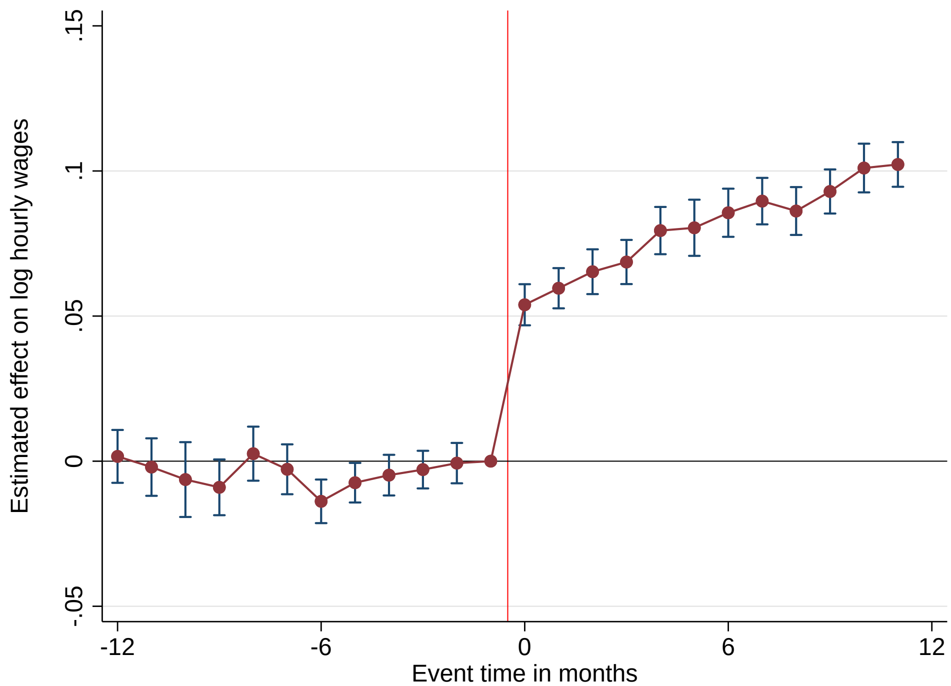
Notes: Percentage of Amazon job ads at wage bins below, at, or above $15. The sample is restricted to postings with valid wage data and hourly rate of pay, employer name, location, and occupation. Whole Foods was acquired by Amazon in August 2017 and is included in the sample. *Data sources*: Burning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

Figure 3: Average exposure to Amazon's min. wage by CZ



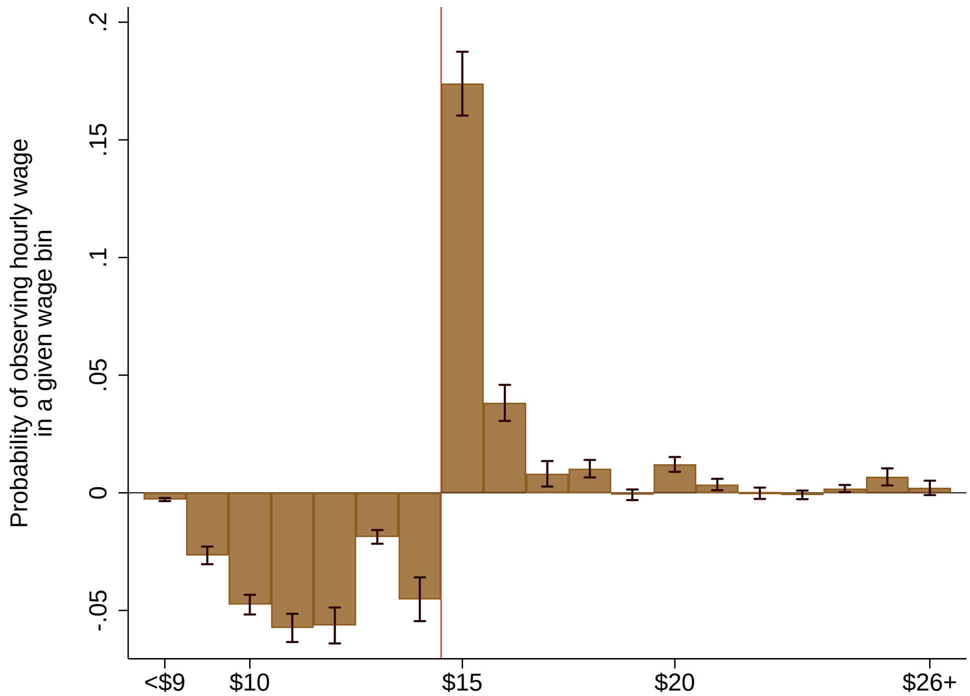
Notes: This figure shows the fraction of postings by employer-by-occupation cells that were below $15 at the commuting zone level in the year prior to Amazon's October 2018 minimum wage announcement. The sample is restricted to non-Amazon postings with valid wage data and hourly rate of pay, employer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

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



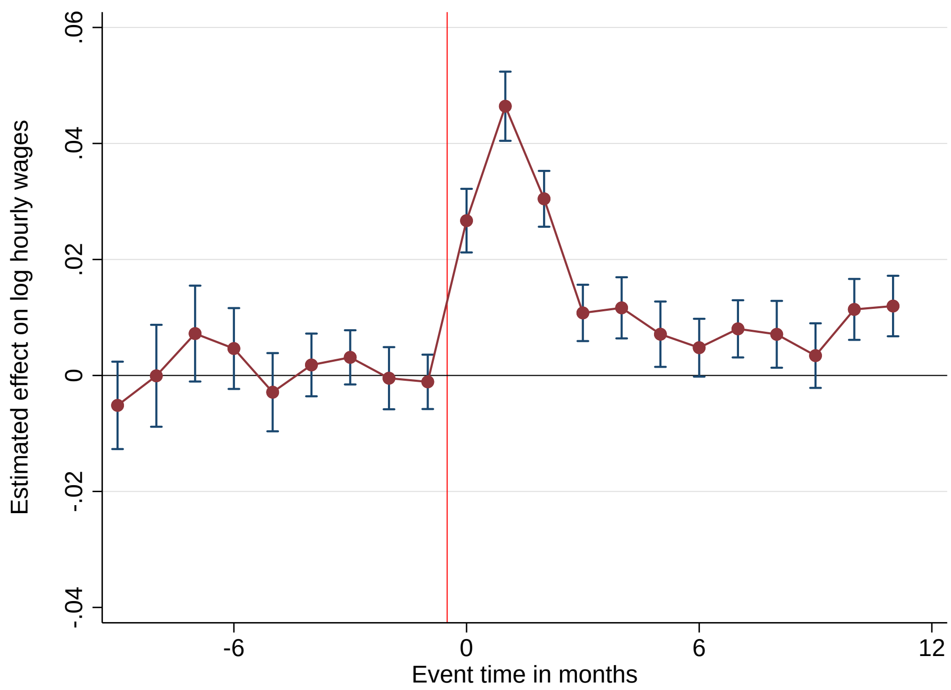
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure 5: Amazon spillovers concentrated at $15



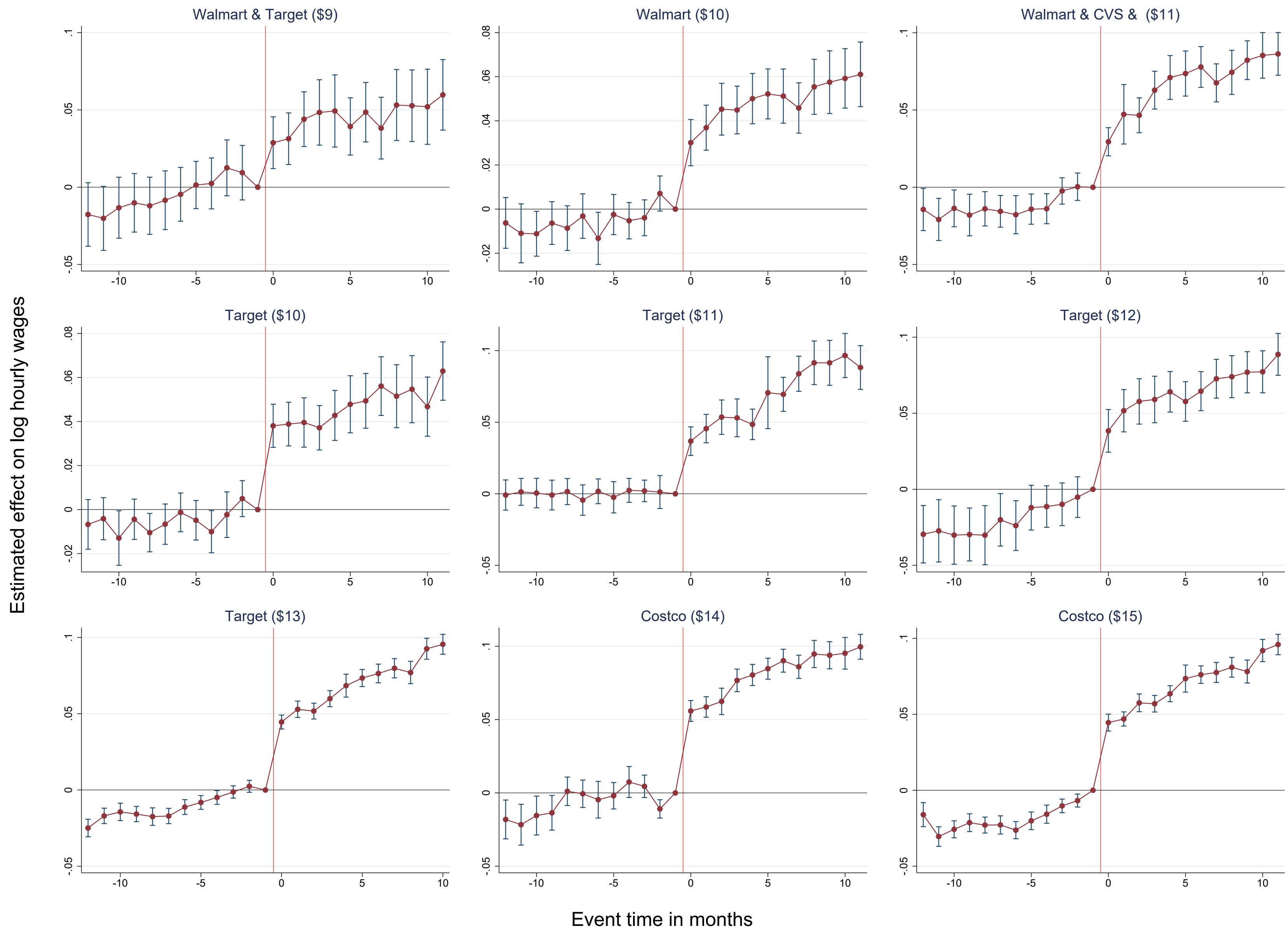
Notes: This figure plots the coefficients from linear probability regressions of hourly wages being in a given wage bin on the interaction between job-level exposure to Amazon's policy for non-Amazon employers and an indicator for post-October-2018. Exposure is defined as the fraction of non-Amazon postings 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

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



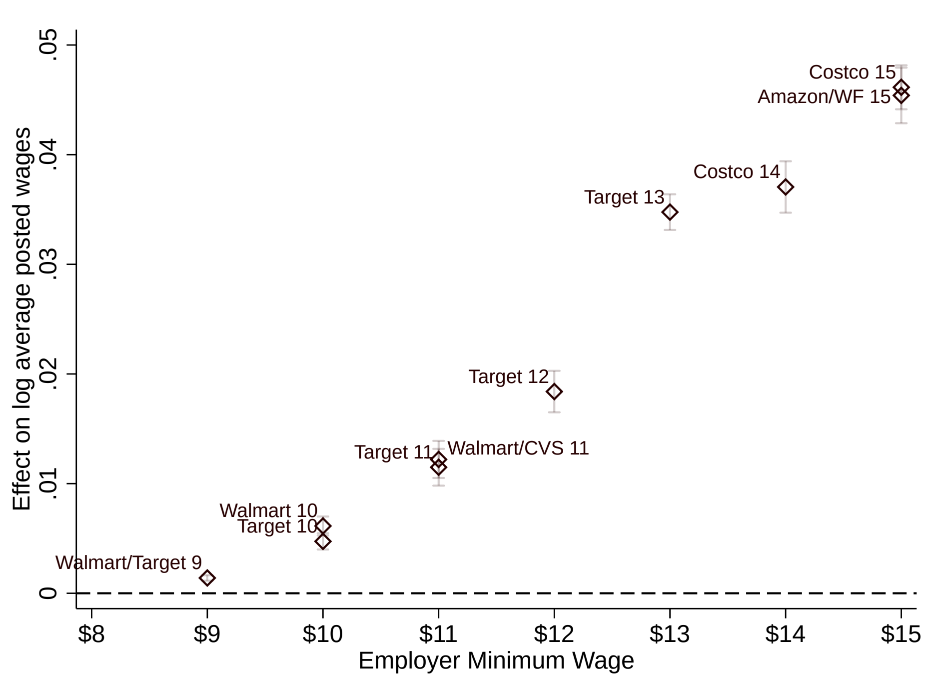
Notes: This figure plots the regression coefficients on the interaction between job-level exposure to Amazon's policy for non-Amazon employers and an indicator for post-treatment for placebo treatment dates, using a 4-month observation window. Coefficients are indexed by the last month of the observation 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

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



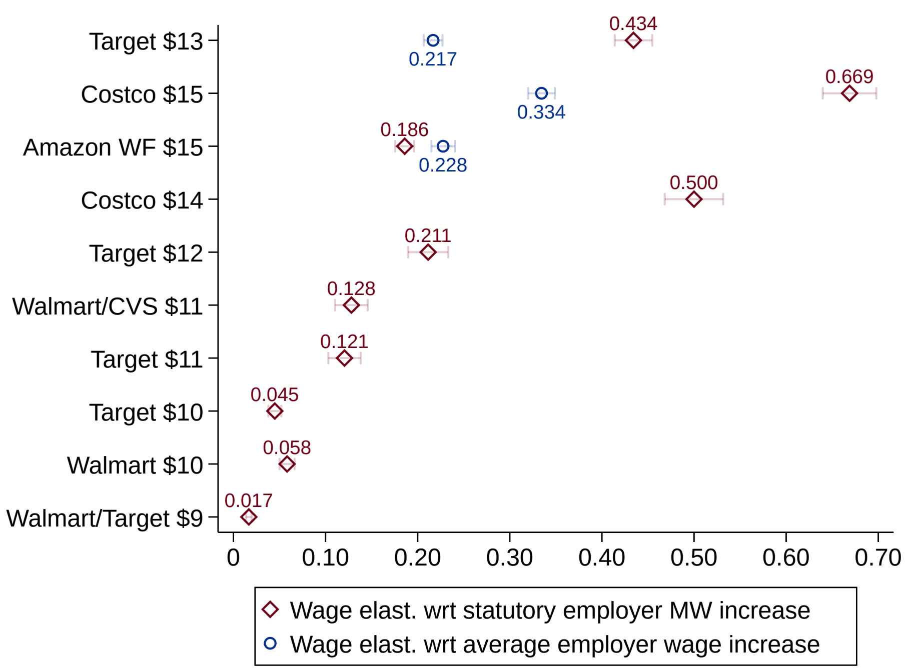
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ 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. The sample is restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure 8: Wage spillover effects increase with level of employer MW



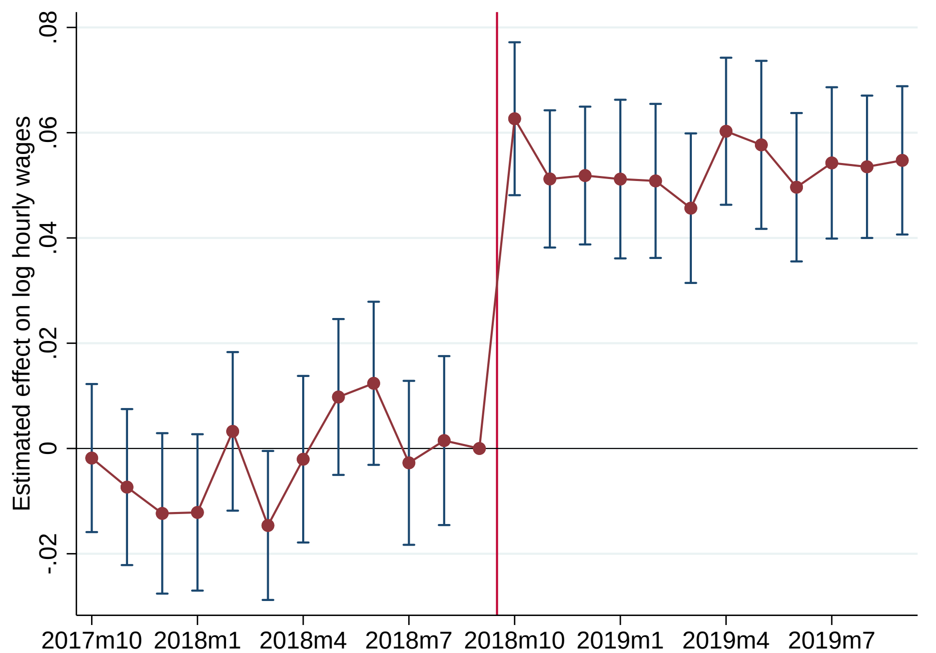
Notes: This figure plots the coefficients on the interaction between job-level exposure to policy firm minimum wages and an indicator for post-treatment period. The dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy firm postings in each occupation-employer-CZ cell with wages below the policy firm minimum wage in the year prior to the announcement. Exposure is normalized by the average job's exposure. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The x-axis measures the minimum wage level of the policy firm. Sample is restricted to postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

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



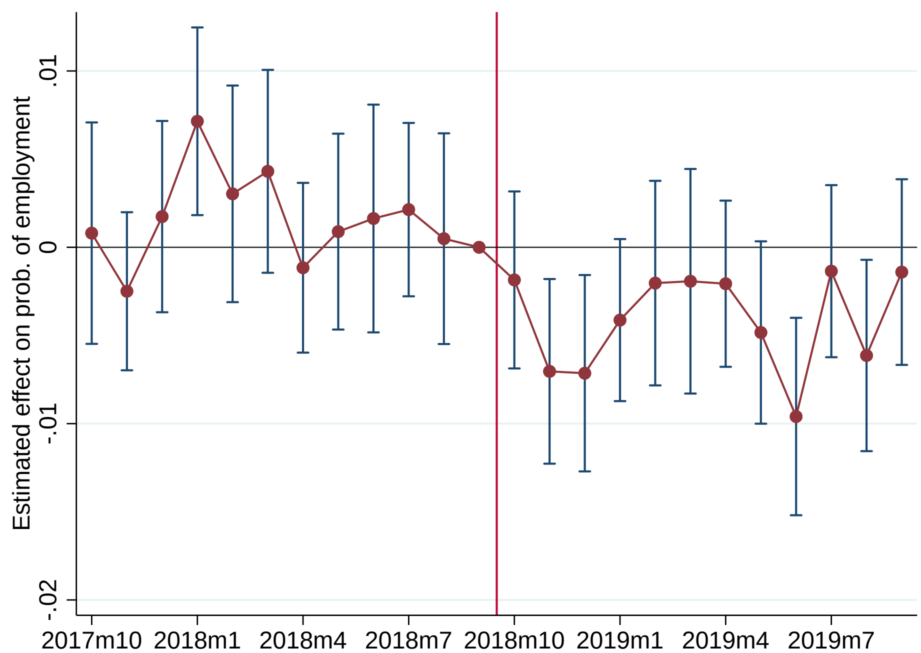
Notes: This figure plots the cross-employer wage elasticities in response to policy firm minimum wages. In red is the average wage elasticity with respect to the increase in the policy firm's minimum wage. In blue is the wage elasticity with respect to Amazon's $15, Costco's $15, and Target's $13 average wage increase. Measures of Target and Costco's earlier average wage increases, as well as Walmart's and CVS's, are unavailable due to insufficient postings for those firms in the BGT data. 95% confidence intervals shown. *Data sources*: 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. The sample is 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. *Data sources*: CPS ORG.

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

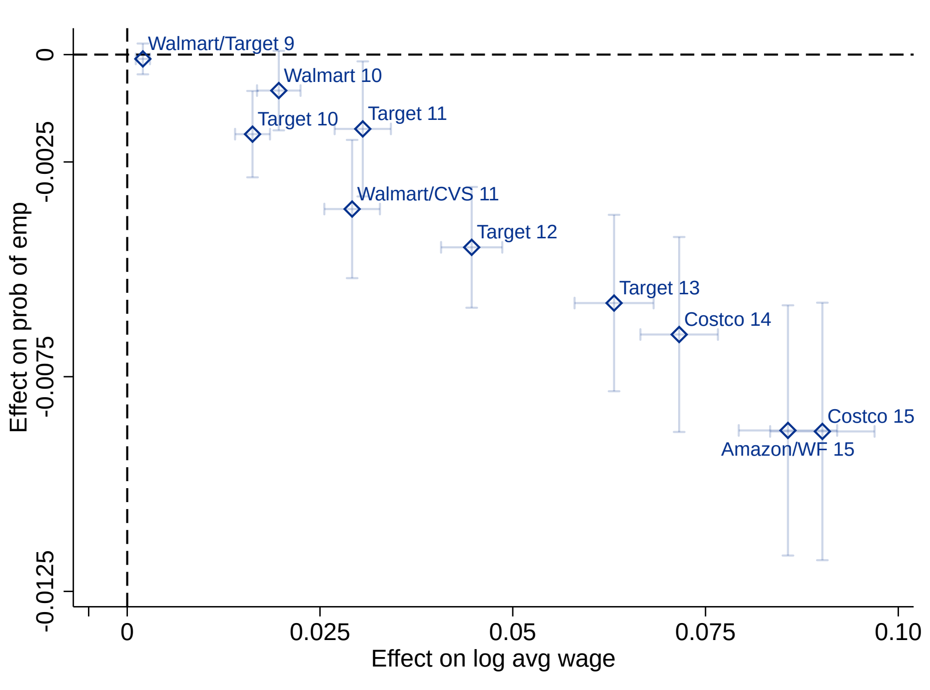


Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon industries interacted with month fixed effects, where the dependent variable is probability of being employed vs. unemployed. Exposure is defined as the fraction of non-Amazon industry 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. *Data sources*: CPS ORG.

Figure 12: Disemployment effects increase with level of employer MW

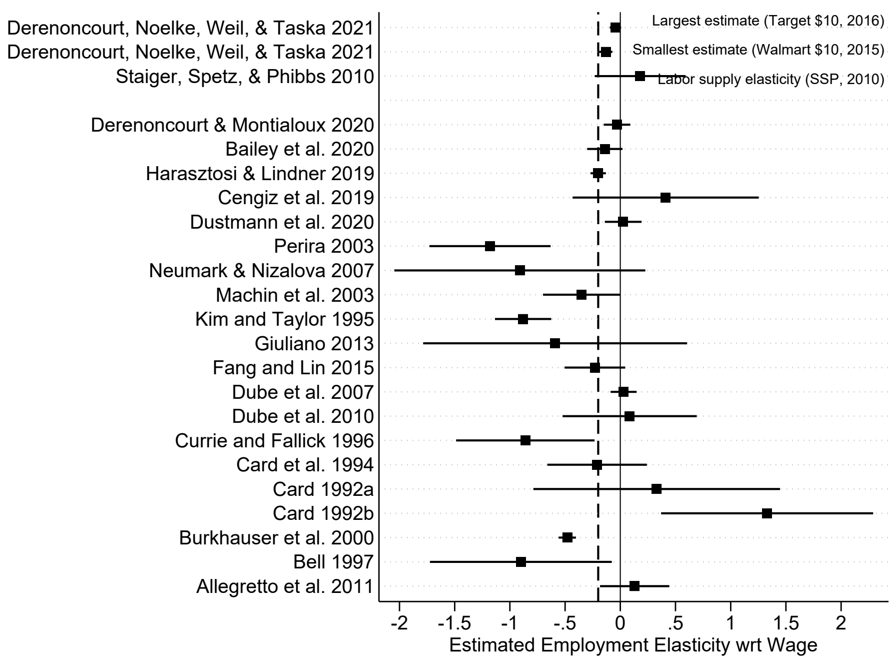
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy industries interacted with an indicator for post-treatment, where the dependent variable is probability of being employed vs. unemployed. Exposure is defined as the fraction of non-policy industry workers in each occupation-CZ cell with wages below the policy firm minimum wage in the year prior to the announcement. 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. The x-axis measures the minimum wage level of the policy firm. Sample is restricted to individuals aged 25 to 65 and excludes those not in the labor force. 95% confidence intervals shown. *Data sources*: 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 policy firm minimum wages for non-policy industries and an indicator for post-treatment. 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 prior to the announcement. 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-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. For the employment regressions, the sample is restricted to individuals aged 25 to 65 and excludes those not in the labor force. *Data sources*: 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 (2021). 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: Characteristics of BGT hourly vacancy and CPS worker samples

	BGT	CPS
Hourly Wage	16.10	23.78
Full-time/part-time status		
Full-time	0.50	0.87
Part-time	0.24	0.13
Occupation		
Management, business, and financial	0.07	0.18
Professional and related	0.24	0.25
Service	0.22	0.16
Sales and related	0.07	0.09
Office and administrative support	0.17	0.11
Farming, fishing, and forestry occupations	0.00	0.01
Construction and extraction	0.02	0.05
Installation, maintenance, and repair	0.06	0.03
Production	0.04	0.06
Transportation and material moving	0.11	0.06
Region		
North Central	0.24	0.22
North East	0.12	0.18
South	0.28	0.37
West	0.36	0.24
N	5450258	871223

Notes: Sample means for hourly jobs in BGT job ads data and hourly workers in the CPS from 2014 to 2019. In column 1, the sample is restricted to job vacancies for hourly jobs with valid wage, employer, occupation, and location data, and to commuting zones where policy firms advertised in the year before the policy change. In column 2, the sample is restricted to workers between the ages of 25 and 65 who report usually working more than three hours a week. For both samples, wages are winsorized at the 5% level. *Data sources*: BGT. CPS ORG.

Table 2: Wage spillovers: robustness checks

Frac. Affected x Post	0.082***	0.085***	0.081***	0.127***
	(0.004)	(0.004)	(0.004)	(0.006)
Postings with valid wage data / month			0.009***	
			(0.002)	
Obs	1,710,709	1,546,121	1,710,709	1,292,664
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

Notes: This table reports the coefficients from estimating equation 3 in column 1. In column 2, we add occupation-by-CZ-by-month fixed effects. In column 3, we control for the share of an employer's postings that contain a wage. In column 4, we include both occupation-by-CZ-by-month fixed effects and employer-by-month fixed effects. The sample is job vacancies with valid wage data for hourly jobs, restricted to commuting zones where Amazon advertised in the year before the policy change. Wages are winsorized at the 5% level. Significance levels are as follows: * = $p < 0.1$, ** = $p < 0.05$, and *** = $p < 0.01$. Unless otherwise indicated, standard errors are in parentheses. *Data sources*: Burning Glass Technologies online vacancy data.

Table 3: Wage spillovers weaker with distance

	Walmart/ Target \$9	Walmart \$10	Target \$10	Walmart/ CVS \$11	Target \$11	Target \$12	Costco \$14	Amazon/ Whole Foods \$15	Costco \$15	Target \$13
Exp. X Post	0.0394***	0.0453***	0.0471***	0.0662***	0.0626***	0.0740***	0.0808***	0.0778***	0.0810***	0.0700***
	(0.00442)	(0.00347)	(0.00400)	(0.00514)	(0.00526)	(0.00428)	(0.00271)	(0.00210)	(0.00185)	(0.00176)
Exp. X Post X Same City	0.00967	-0.00564	-0.00963	-0.0189*	-0.0190***	-0.0212***	-0.0172***	-0.0157***	-0.0144***	-0.0122***
	(0.0110)	(0.00451)	(0.00508)	(0.00807)	(0.00498)	(0.00483)	(0.00271)	(0.00261)	(0.00218)	(0.00240)
R-Squared	0.911	0.917	0.915	0.914	0.919	0.906	0.901	0.901	0.895	0.897
N	486588	641348	654850	739877	708883	717483	885137	1464337	1874001	2186438
Mean job exposure	0.0334	0.130	0.105	0.165	0.174	0.240	0.467	0.577	0.571	0.468

Notes: This table reports the coefficients on the interaction between job-level exposure to policy firm minimum wages and an indicator for post-treatment period (row 1) as well as the triple interaction between exposure, a post-treatment period indicator, and an indicator for the posting being in a city other than the policy firm's city (within the same commuting zone). The dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy firm postings in each occupation-employer-CZ cell with wages below the policy firm minimum wage in the year prior to the announcement. 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, location, and occupation. Significance levels are as follows: * = p < 0.1, ** = p < 0.05, and *** = p < 0.01. Unless otherwise indicated, standard errors are in parentheses. *Data sources*: Burning Glass Technologies online vacancy data.

Table 4: Wage spillovers moderation by local minimum wage

	\$9	\$10	\$11	\$12	\$13	\$14	\$15
Exp. X Post	-0.00352	0.0118	-0.00157	0.0143	0.0935***	0.0389	0.0902***
	(0.0299)	(0.0178)	(0.0198)	(0.0208)	(0.00384)	(0.0210)	(0.00582)
Exp. $\times$ Post $\times$ Firm MW > Local MW	0.0733*	0.0389*	0.101***	0.0823***	-0.0264***	0.0423*	-0.0129*
	(0.0297)	(0.0195)	(0.0194)	(0.0200)	(0.00420)	(0.0209)	(0.00615)
R-Squared	0.855	0.873	0.855	0.833	0.829	0.827	0.827
N	679232	1749403	2195345	1171348	2799315	1437401	4625241

Notes: This table reports the coefficients on the interaction between job-level exposure to policy firm minimum wages and an indicator for post-treatment period (row 1) as well as the triple interaction between exposure, a post-treatment period indicator, and an indicator for the local minimum wage being below the policy firm's announced minimum wage. The dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy firm postings in each occupation-employer cell with wages below the policy firm minimum wage in the year prior to the announcement. 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, location, and occupation. Significance levels are as follows: * = p < 0.1, ** = p < 0.05, and *** = p < 0.01. Unless otherwise indicated, standard errors are in parentheses. *Data sources*: Burning Glass Technologies online vacancy data.

Table 5: Employment elasticity estimates

	Walmart/ Target $9	Walmart $10	Target $10	Walmart/ CVS $11	Target $11	Target $12	Costco $14	Amazon/ Whole Foods $15	Costco $15	Target $13
Exposure var. × Post										
Employment	-0.000	-0.001*	-0.002***	-0.002**	-0.003***	-0.004***	-0.006***	-0.008***	-0.008***	-0.005***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	142,362	142,540	142,706	140,820	140,350	138,808	120,125	125,810	106,127	117,735
Wages	0.002***	0.017***	0.014***	0.027***	0.026***	0.039***	0.070***	0.080***	0.084***	0.055***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
	81,589	82,642	82,848	82,363	82,192	81,365	70,858	74,345	62,239	68,661
Emp. elasticity	-0.06	-0.04	-0.13***	-0.06**	-0.13***	-0.11***	-0.10***	-0.11***	-0.11***	-0.10***
Std. Error	0.10	0.03	0.04	0.03	0.03	0.02	0.02	0.02	0.02	0.02
Lower bound	-0.26	-0.10	-0.19	-0.12	-0.19	-0.14	-0.13	-0.15	-0.14	-0.14
Upper bound	0.14	0.01	-0.06	-0.01	-0.07	-0.08	-0.07	-0.07	-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. Significance levels are as follows: * = p < 0.1, ** = p < 0.05, and *** = p < 0.01. Unless otherwise indicated, standard errors are in parentheses. *Data sources*: CPS ORG.

Table 6: Aggregate employment elasticity estimates

	Walmart/ Target $9	Walmart $10	Target $10	Walmart/ CVS $11	Target $11	Target $12	Costco $14	Amazon/ Whole Foods $15	Costco $15	Target $13
Exposure var. × Post										
Employment	-0.000	-0.001*	-0.002***	-0.002**	-0.003***	-0.004***	-0.006***	-0.008***	-0.008***	-0.005***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	144,366	144,352	144,514	142,419	141,257	140,305	121,305	128,411	107,157	118,983
Wages	0.002***	0.017***	0.014***	0.027***	0.025***	0.040***	0.070***	0.080***	0.084***	0.055***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
	82,682	83,617	83,860	83,297	82,781	82,211	71,531	75,770	62,846	69,388
Emp. elasticity	-0.06	-0.04	-0.12***	-0.06**	-0.13***	-0.11***	-0.10***	-0.11***	-0.10***	-0.10***
Std. Error	0.10	0.03	0.03	0.03	0.03	0.02	0.02	0.02	0.02	0.02
Lower bound	-0.26	-0.10	-0.19	-0.12	-0.19	-0.14	-0.13	-0.15	-0.14	-0.13
Upper bound	0.14	0.01	-0.05	-0.01	-0.07	-0.08	-0.07	-0.08	-0.07	-0.06

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. Significance levels are as follows: * = p < 0.1, ** = p < 0.05, and *** = p < 0.01. Unless otherwise indicated, standard errors are in parentheses. *Data sources*: CPS ORG.

Online Appendix

to

“Spillover effects from voluntary employer minimum wages”

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A 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; Sumagaysay, 2020). In 2018, Amazon advertised hourly job positions in 188 commuting zones throughout the country.[34] 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 would have actually reduced earnings for employees surpassing productivity targets. However, the proposal was quickly modified to correct for this problem by providing additional increases for those workers otherwise adversely affected by it. The wage increase affected non-contractor employees, including regular, seasonal, full-time, and part-time workers (see Abbruzzese and Cappetta (2018), Murphy (2018), and Wiese (2018)). 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 the company's announcement on October 1, 2018, Amazon's minimum wage started at $11 (Settembre, 2018). On the company blog announcing the wage increase, Amazon framed the wage increase as a response to critics of the company's then prevailing wage policies (Staff, 2018). Tight labor markets were also cited as a reason behind Amazon's wage increase. With its timing just before holiday-shopping season, the wage increase may have also served to attract additional workers during peak business months.

Walmart Walmart remains the largest employer of workers in the US, with a workforce of nearly 1.6 million (Walmart, 2020; Fordham, 2020). The company has 4,177 stores in the US and has 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. According to the company, 40% of its workforce was affected by the change. The company announced a further increase to $11 an hour in January 2018, to be effective starting February, 2018 (Walmart, 2018).

Prior to its February 2015 announcement, the majority of Walmart's locations followed

[34] Authors' calculation using BGT job ads data.

the federal minimum wage of \$7.25. However, when 21 states raised their minimum wages in 2015, Walmart adjusted base salaries for 1,434 stores (Layne, 2014). The average hourly 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 and the second biggest discount chain behind Walmart, employing approximately 360,000 people and with annual sales of about \$78 billion (NRF, 2019; 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 five states: California, Texas, Florida, Illinois, and New York (Target Corporation, 2020a).

In March 2015, Target announced its first company-wide minimum wage of \$9 an hour. In June 2016, it increased the minimum to \$10 an hour. One year later, the company raised it again to \$11 while expressing an intent to increase it to \$15 by the end of 2020, citing tight labor markets as its reason for doing so (D'Innocenzio, 2017). The average wage posted for Target's online jobs ads prior to September 2017 was \$13.14.

Target announced on June 17th, 2020 that its \$15 minimum wage would apply to approximately 275,000 part-time and full-time workers (Target Corporation, 2020b; Kavilanz and Business, 2020).[35]

Costco Costco Wholesale Corporation is an international chain of membership warehouses. In the US, Costco has 817 warehouses and 565 locations and employs 189,000 full and part-time workers (Costco Wholesale Corporation, 2020). Costco's annual revenue for the fiscal year ending August 2021 was \$192 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 no information on the percentage of the workforce impacted by this increase (Hanbury, 2019).

[35] We do not study this increase or any others implemented after the start of the Coronavirus pandemic.

Table A1: Voluntary employer minimum wage polices

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	$13	$14 ($14.50 if previous wage was $13.50)	May 1, 2018	June 11, 2018	Hourly employees		Yes, ≈130K employees impacted[24,25]	
		$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]

Table A2: 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 Burning Glass Technologies job ads data

The following Appendix examines trends in the number of postings and wage trends in our BGT analysis data file.

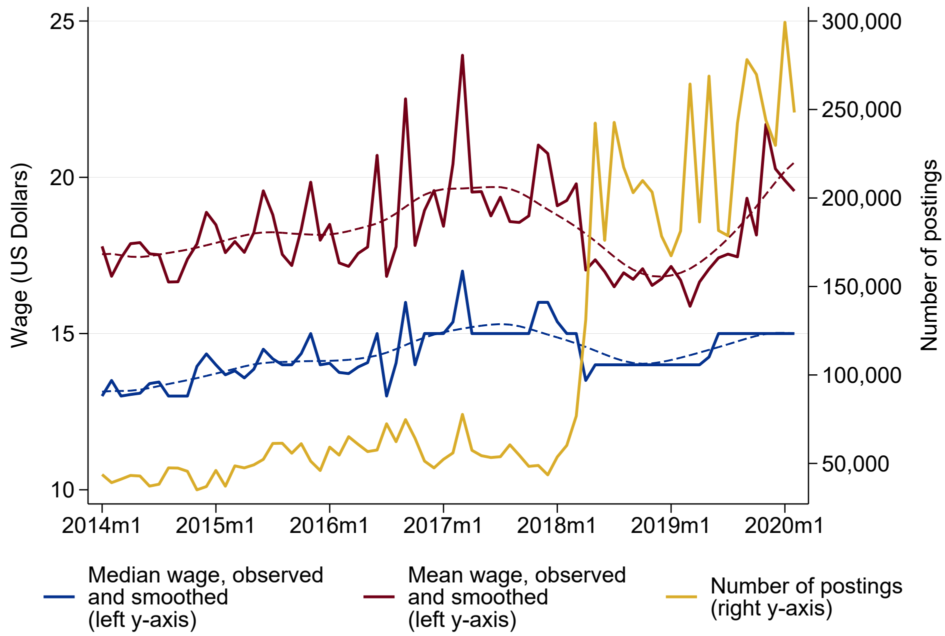
The BGT analyses use postings from February 2014 through February 2020. The BGT data include 154,176,423 postings over this period. Of these, 20.4% have non-missing wage data, 73.8% have non-missing employer names. 20,240,413 postings (13.1%) have both non-missing wages and employer names. We further subset to postings that contain hourly wages, which leaves us with 8,252,926. Dropping postings with missing geographic identifiers (county and state FIPS codes), postings outside the 50 states or Washington DC, and postings with missing occupations data, leaves us with 7,790,373 postings. Finally, for the regression analyses specific to each policy announcement, we add additional restrictions. We drop postings with wages below the 5th and above the 95th percentile of the hourly wage distribution during the observation period for that announcement. Finally, we drop postings made by the announcing employer. This leaves us with 6,976,351 unique postings.

To examine trends in wages and postings in the BGT data, we created a dataset that is not winsorized and excludes all postings by policy employers. It contains 7,738,740 postings. Figure B1 shows the trend in the number of postings, the median hourly wage and the average hourly wage.

In February 2014, there were about 40,000 postings. This number increases slightly between 2014 and 2018, then rises abruptly from about 50 thousand to 200 thousand in early 2018, and continues to grows thereafter. The increase in the number of postings in early 2018 is driven by both abrupt increases in the overall number of postings and the number of postings with non-missing wage data and non-missing employer names. Median and mean hourly wages rose gradually from 2014 to 2018, dropped in the spring of 2018, and then began to rise again. The wage trend is consistent with an increase in the number of online job postings for low-wage positions recorded in the BGT data in early 2018.

Changes in the availability of employer and wage information on job postings may reflect a change in how BGT collects wage data, i.e., a change in algorithms and/or data sources. However, these changes may also reflect an increase in online advertising of low-wage jobs. Shifts in the composition of job ads advertised online can influence wage trends through a selection effect. This selection effect could in turn confound our attempts to estimate wage spillover effects using the BGT data. To address this, our analyses flexibly control for changes in the occupational composition by including interactions between 6-digit occupational SOC codes and calendar month in our baseline specification. Because six digit occupational codes are highly predictive of wage levels, they capture exogenous changes in sample composition that are due to changes in data collection by BGT or (national) changes in online advertising of low-wage occupations.

Figure B1: Trends in wages in analysis sample of BGT job ads data



Notes: This figure depicts trends in mean and median wages as well as the number of postings in our analysis sample of BGT job ads data. The sample includes postings by non-policy employers with non-missing employer name and non-missing hourly wage data. *Data sources*: Burning Glass Technologies online vacancy data.

C Current Population Survey data

The Bureau of Labor Statistics provides workforce data in the Current Population Survey Outgoing Rotation Group ("CPS ORG"). The CPS ORG is a sixteen-month, household survey. CPS ORG surveys households for the first four months, excludes households for the middle eight months, and surveys households again for the final four months

We use nationally-representative, individual-level CPS ORG data from January 2014 to December 2019.[36] The data include employed and unemployed individuals, 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.

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

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.

Identification of commuting zones in the CPS Our main treatment variable for examining wage spillovers in the CPS is the fraction of affected workers at the occupation-by-CZ level. We include fixed effects for a worker's CZ and occupation and CZ-by-month and occupation-by-month fixed effects.

According to IPUMS-CPS documentation, approximately "45 percent of households in recent years are located in a county that is identified" (Flood et al., 2021). Although no explicit threshold is provided in the documentation, our calculations suggest that identified counties have at least 55,000 labor force participants. We map each identified county to its 1990 commuting zone. Table C1 provides the number of policy firm CZs identifiable in the CPS ORG data for each policy.

Table C1: Number of policy firm commuting zones identifiable in the CPS ORG

Policy experiment	Number of identifiable CZs
Amazon & Whole Foods $15, 2018	93
Walmart & Target $9, 2015	171
Walmart $10, 2015	161
Walmart & CVS $11, 2018	136
Target $10, 2016	161
Target $11, 2017	134
Target $12, 2018	135
Target $13, 2019	136
Costco $14, 2018	56
Costco $15, 2019	55

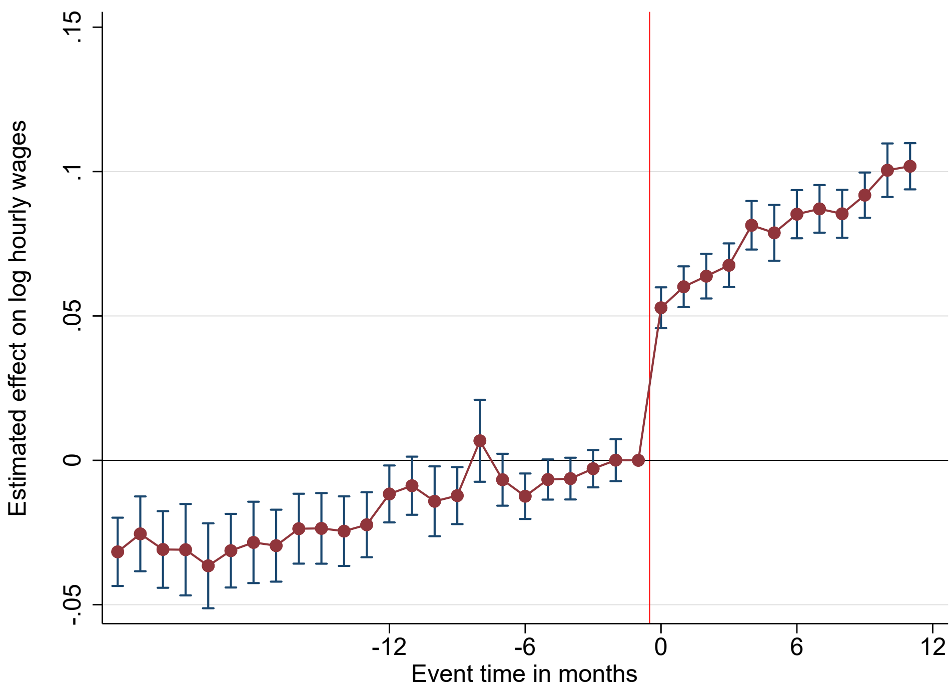
D Additional robustness checks

This section explores robustness of the core results to alternative specifications, functional forms, and testing of the parallel pre-trends assumption.

D.1 Longer pre-period for Amazon spillover effects

We examine a longer pre-period in the case of Amazon's minimum wage announcement to see how exposure to Amazon's policy is correlated with wages two years prior to the announcement. Figure D1 shows the results. Wages at highly exposed jobs gradually trend upwards over the two years prior to the announcement, consistent with wage growth in lower wage jobs. However, there is a sharp jump in wages at highly exposed jobs immediately after Amazon's announcement.

Figure D1: Amazon spillovers, 24-month pre-period



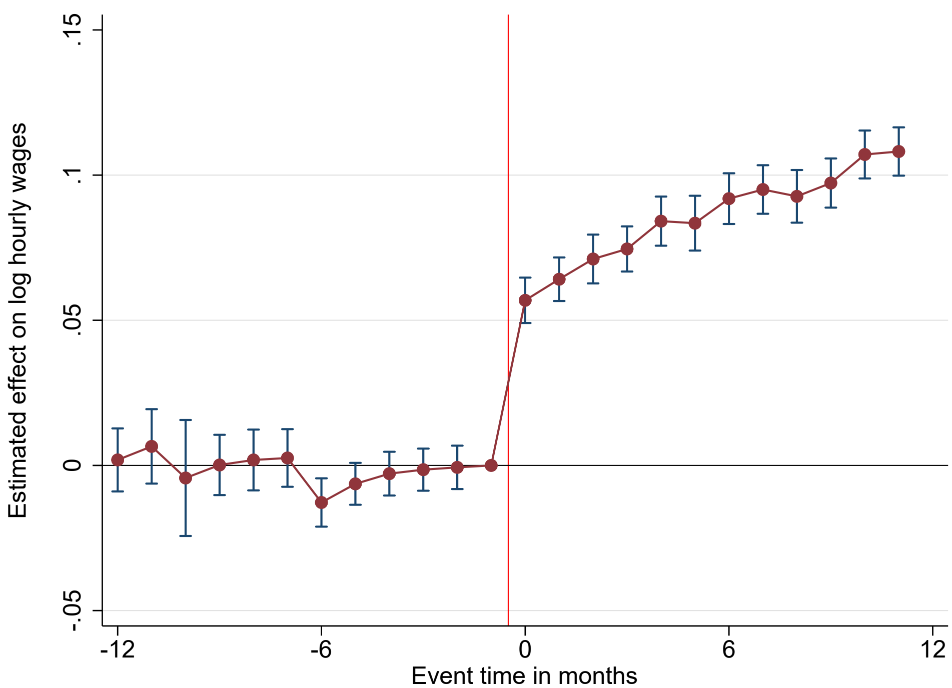
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. A two-year pre-period is shown. Exposure is defined as the fraction of non-Amazon 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

D.2 Additional controls for Amazon spillover effects

We explore robustness of our results to the inclusion of occupation-by-CZ-by-month fixed effects, which control for demand shocks or policy changes affecting particular occupations in particular areas. These could include, for example, increased demand for retail or warehousing occupations during the holidays or increases in state or local minimum wages. Figure D2 depicts the results. The inclusion of these controls does not affect our results.

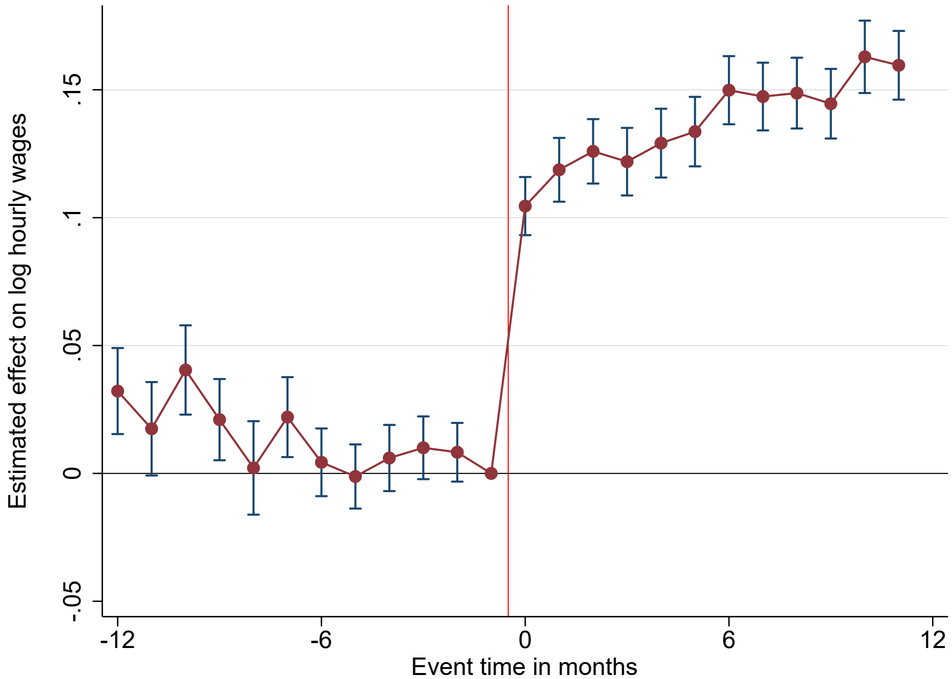
In Figure D3, we add employer-by-month fixed effects to the specification. The inclusion of these controls absorbs any employer-level changes in policy, such as the decision to post wages on job ads, or shocks to specific kinds of employers. In this specification, we leverage within-employer variation in exposure to Amazon's policy across occupation-by-CZ cells. After including these controls, the post-treatment effect on average wages is slightly larger than in our baseline specification. Thus, we find no evidence that our results can be explained by demand shocks or policy changes affecting specific occupations or employers. Within employers and within occupations, jobs highly exposed to Amazon's minimum wage (as measured by the fraction below $15) experience a larger increase in wages.

Figure D2: 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure D3: Amazon spillovers, with occupation-by-CZ-by-month, employer-by-month fixed effects



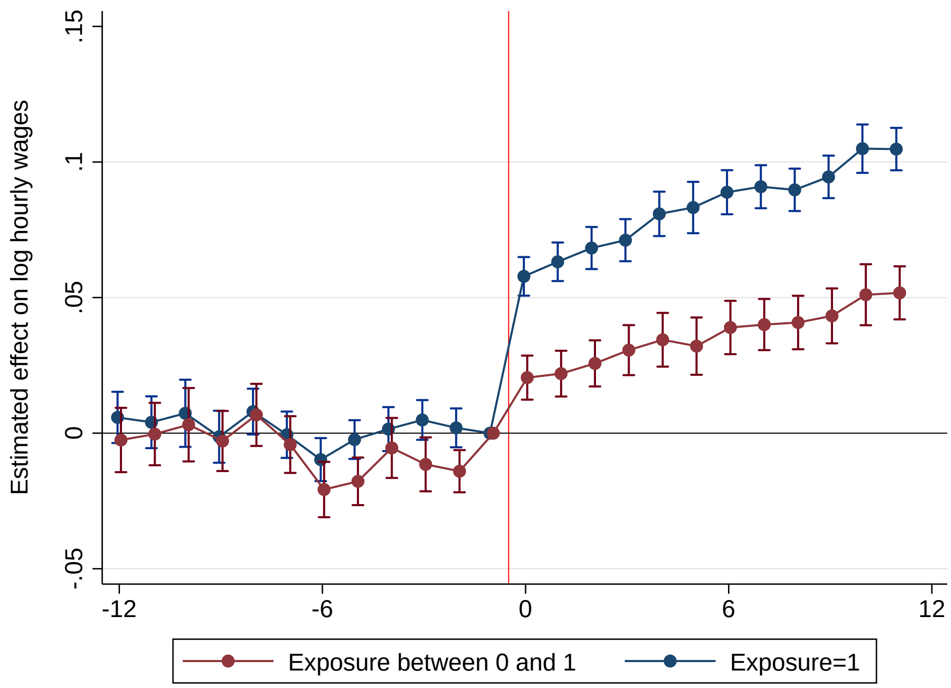
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

D.3 Alternative functional form for Amazon spillover effects

We examine the sensitivity of our results to the functional form chosen in the baseline analysis. Instead of the continuous measure of bite or exposure used in the baseline analysis, we bin exposure into three groups: jobs with 0% of postings below \$15, jobs with between 0 and 100% percent of postings below \$15 (partially exposed), and jobs with 100% of postings below \$15 (fully exposed). Figure D4 plots the coefficients on indicators of partially exposed interacted with month and fully exposed interacted with month. Jobs with 0% exposure are the omitted category. The figure shows that both partially and fully exposed jobs experience an increase in wages after Amazon's minimum wage announcement in October 2018.

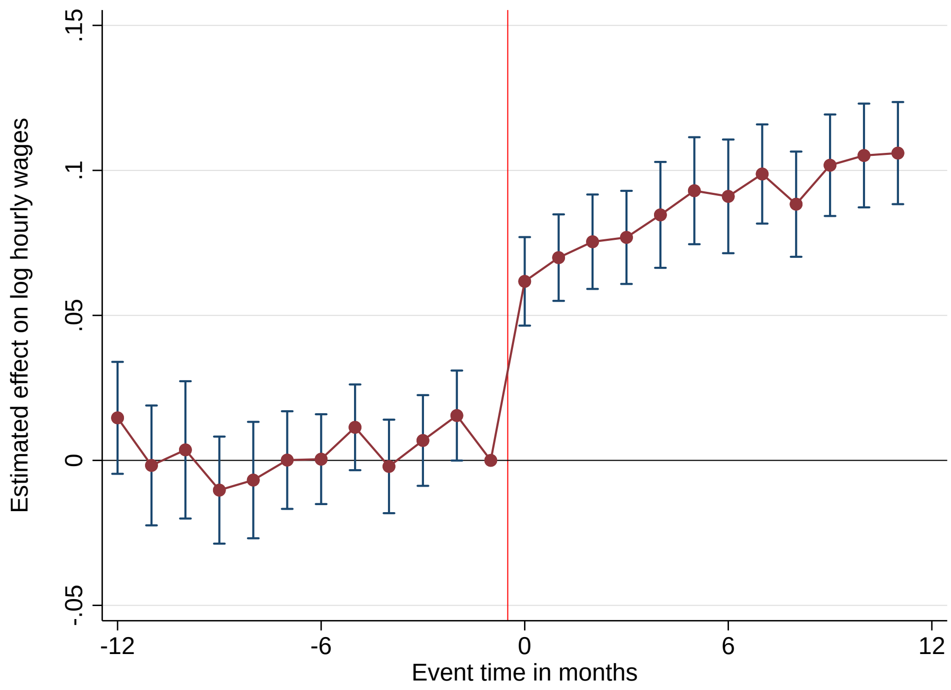
Finally, Figure D5 restricts the sample to jobs with at least some fraction of postings below \$15 and plots the effect of an indicator for fully exposed interacted with month, where the omitted category is the partially exposed group. Fully exposed jobs experience a large and immediate increase in wages after Amazon's announcement relative to the partially exposed group. Both results are similar to our baseline analysis using a continuous measure of exposure.

Figure D4: 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 the announcement, 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure D5: Amazon spillovers, binned exposure: partially vs. fully exposed



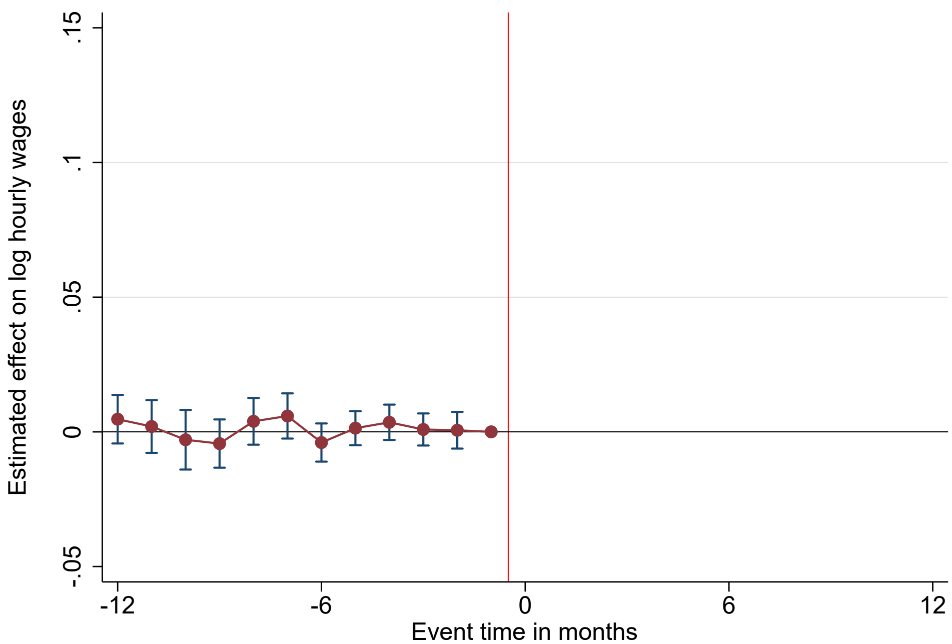
Notes: This figure plots the regression coefficients on job-level exposure group to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. The two exposure groups are jobs with 100% of postings offering below \$15 in the year prior to the announcement 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

D.4 Alternative parallel pre-trends assessment for Amazon spillover effects

A recent literature on difference-in-differences methods proposes alternative ways of evaluating the parallel trends assumption (Borusyak et al., 2021; Rambachan and Roth, 2021). Borusyak et al. (2021) propose separately estimating pre-trends and testing their joint significance as a more formal test of the parallel pre-trends assumption. In Figure D6, we show the coefficient on fraction affected interacted with month using only the data from the 12 months prior to Amazon's minimum wage announcement (the last month right before the announcement is omitted). Although the p-value of the F-test of joint significance is 0.04 , it is clear from the figure that there is no systematic pre-trend, with some estimates positive and others negative.

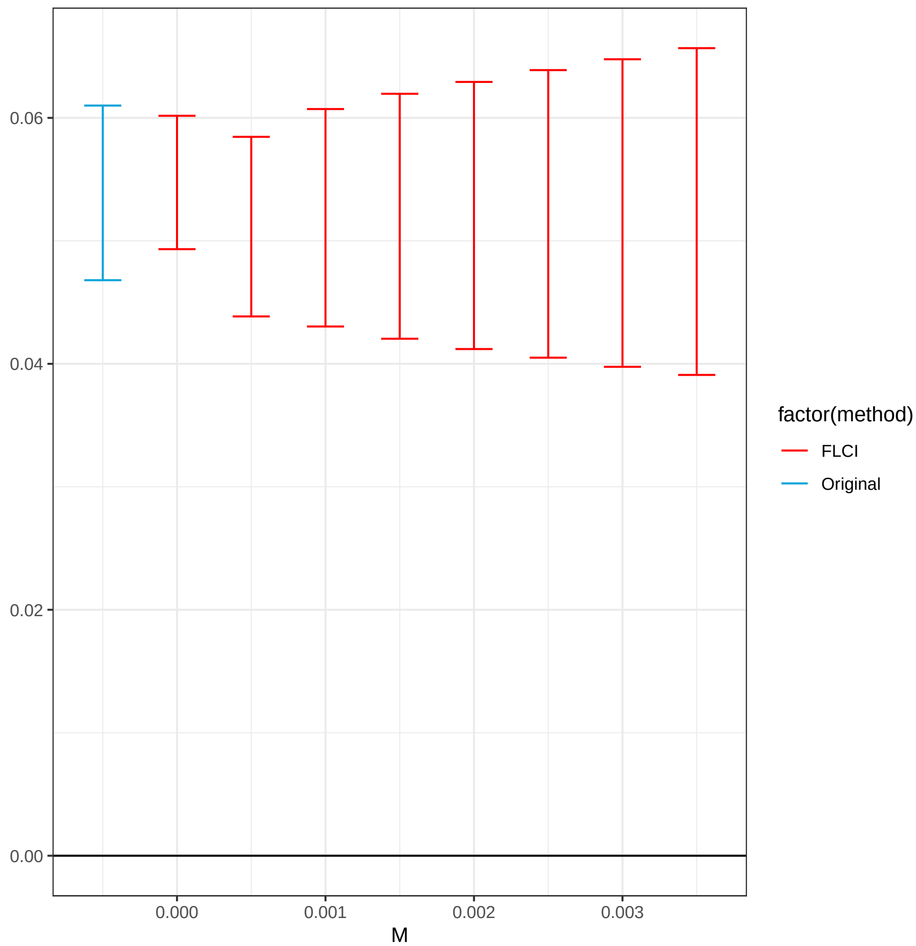
To further explore potential bias from a pre-trend in wages, we follow Rambachan and Roth (2021) and construct alternative dynamic confidence intervals with varying assumptions on the trends prior to the announcement. Allowing for linear deviations from parallel trends up to a standard deviation of the coefficient β_0, the coefficient on fraction affected interacted with the first month of the announcement, results in the confidence sets depicted in Figure D7. Allowing for a positive bias in the pre-trend results in the confidence sets depicted in Figure D8. Both figures indicate that our estimates are robust for M up to 100% of the standard deviation. Thus, even allowing for a sizable upward trend in wages (that is still consistent with the uncertainty in our pre-trend estimates), we find a large increase in wages at non-Amazon employers in the month of Amazon's announcement.

Figure D6: Separately estimated pre-trends in spillovers for Amazon minimum wage



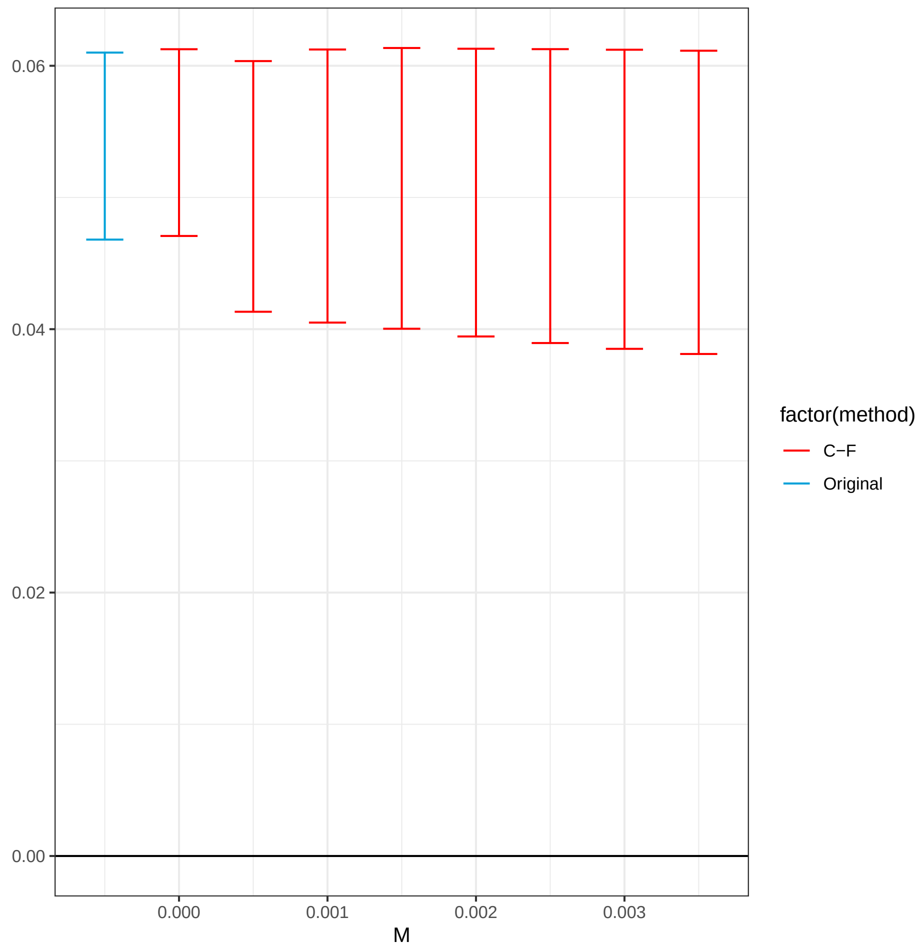
Notes: This figure plots the coefficients β_k from equation 2 for pre-announcement months only, following the proposed tests for absence of pre-trends in (Borusyak et al., 2021; Borusyak and Schönberg, 2021). The p-value of the F-test of joint significance is 0.04 , however the pre-announcement estimates exhibit no clear pre-trend, with some positive and some negative. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure D7: Honest pre-trends for Amazon minimum wage spillover effect



Notes: This figure plots the coefficients β_k from equation 2 for the first post-treatment month only and plots alternative confidence intervals that allow for deviations from parallel trends in the pre-period, following the honest parallel trends estimation procedure of Rambachan and Roth (2021). 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure D8: Honest pre-trends in spillovers from Amazon's minimum wage (positive bias)

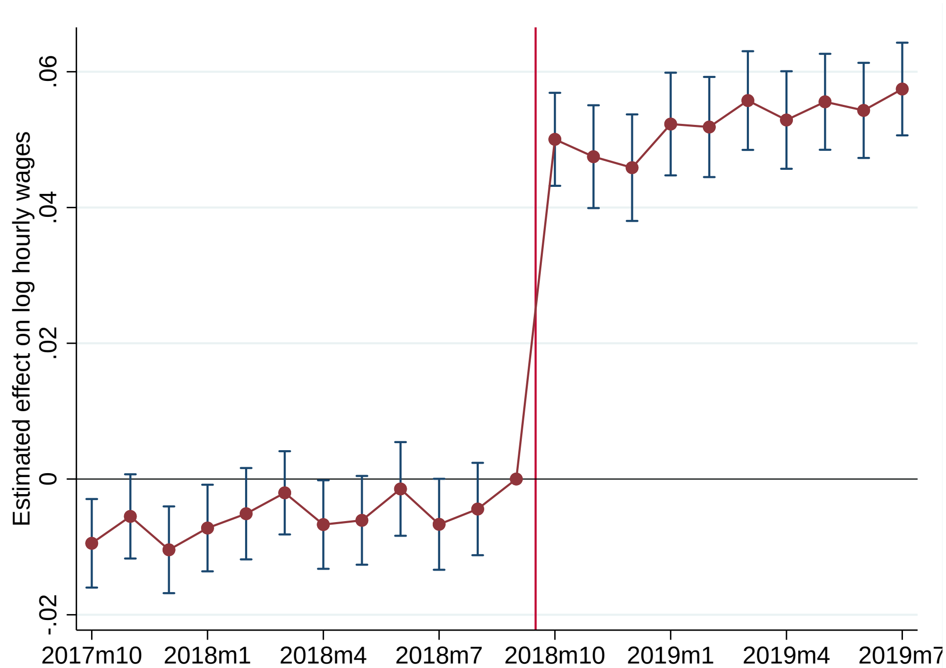


Notes: This figure plots the coefficients β_k from equation 2 for the first post-treatment month only and plots alternative confidence intervals that allow for deviations from parallel trends in the pre-period, following the honest parallel trends estimation procedure of Rambachan and Roth (2021), and allowing for positive bias in the pre-trend. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

D.4.1 Amazon spillover to worker-reported wages from Glassdoor

In this section, we examine whether spillover effects in advertised wages after Amazon's minimum wage translate into increases in wages actually received by workers at other firms. Our data come from Glassdoor, an online job search and review platform. Using the model in equation 2, we estimate the effect of the announcement on reported wages. Figure D9 reports the results, plotting the coefficient β_k on fraction affected interacted with month. Consistent with our evidence on advertised wages using BGT data, the reported wages of workers in highly exposed jobs at non-Amazon employers increased sharply in the month after Amazon's announcement and this increase persisted over the following year. These results indicate that increases in advertised wages translated into increases in the take-home wages of workers at non-Amazon employers.

Figure D9: Spillovers from Amazon's minimum wage in worker-reported wages (Glassdoor)



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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Glassdoor salary reports.

D.5 Robustness checks for Walmart, Target, and Costco minimum wage spillovers

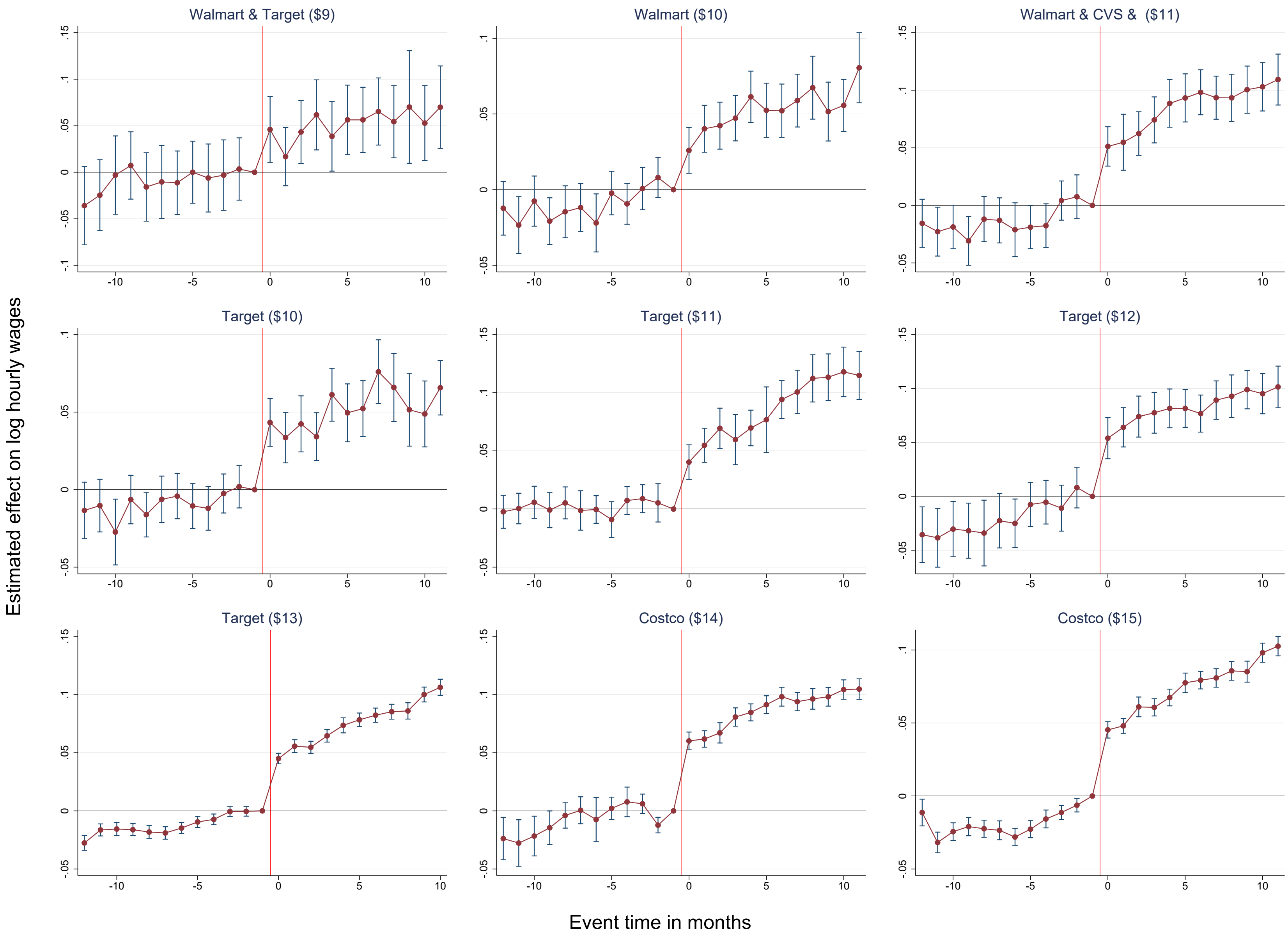
The following section replicates our key robustness checks for the nine other minimum wage announcements we study by Walmart, Target, and Costco. Figures D10 and D11 report robustness to the inclusion of occupation-by-CZ-by-month fixed effects and employer-by-month fixed effects. Figure D12 documents similar increases in worker reported wages at non-policy firms in the wake of policy firm minimum wage announcements. Figure D13 shows that, with the exception of Target's \$13 minimum wage announcement, advertised wages at non-policy firms bunch at the policy firm's announced minimum wage. Bunching also occurs at \$14 and \$15 an hour in the case of Target's \$13 minimum wage, potentially due to the close timing with Costco's \$15 minimum wage announcement (one month after).

In Figure D14, we replicate our analysis examining alternative announcement dates for Amazon's \$15 minimum wage across the other 9 employer minimum wage announcements. Wage effects appear only in the true month of the announcement and are highest in those months consistent with a sharp increase in wages only in the month of the announcement and a stable persistent increase relative to the pre-period in the months that follow each announcement.

Finally, we extend our alternative tests of parallel pre-trends to the other 9 announcements. Figure D15 shows that only in the case of the last three announcements are the p-values for the F-tests of joint significance smaller than 0.05. During this period several announcements followed closely on the heels of others. For example, Target's \$13 announcement occurred one month after Costco's \$15 announcement, and Costco's \$14 announcement occurred three months after Target's \$12 announcement.

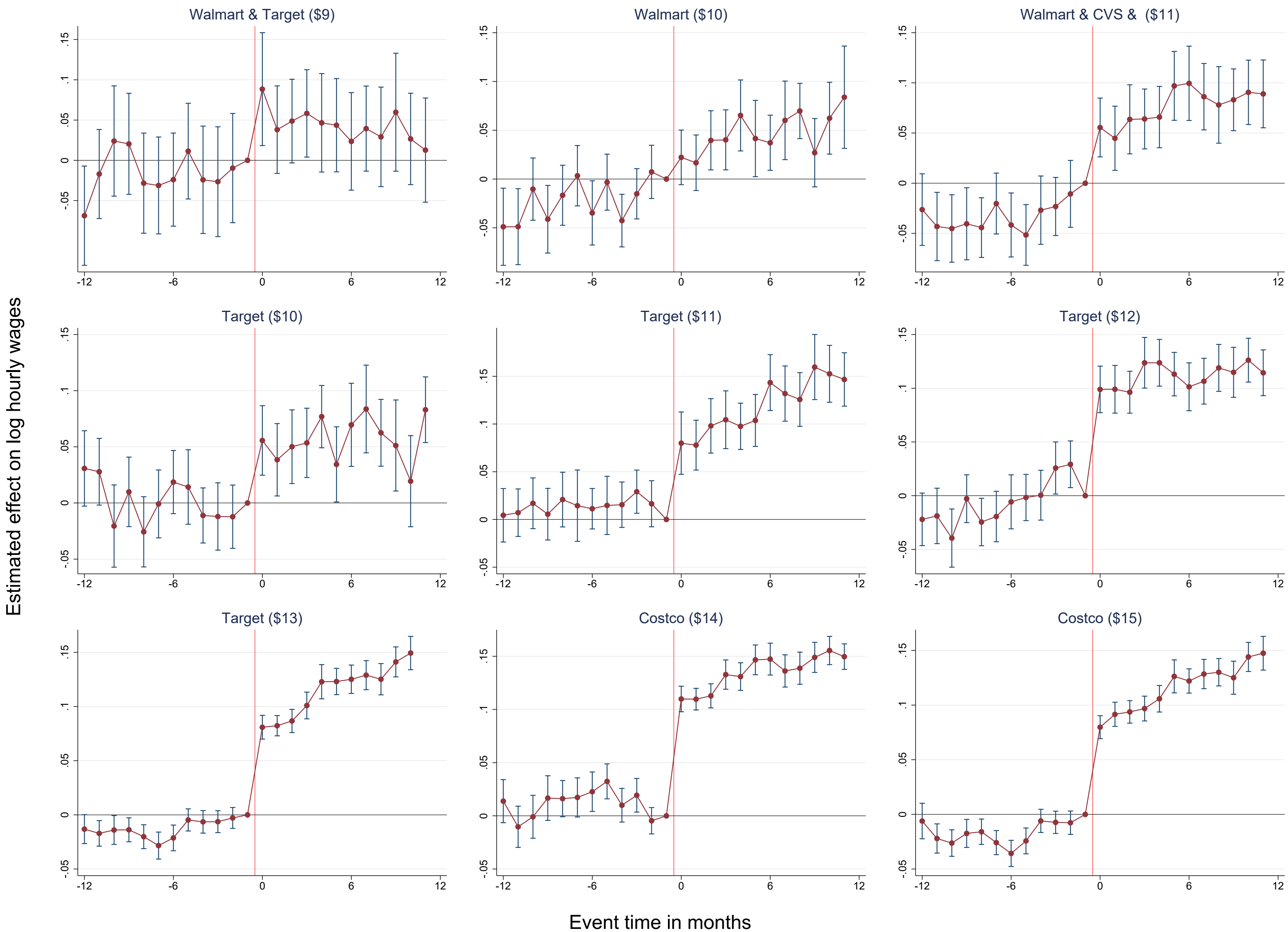
To assess the bias from pre-trend violations, we once again calculate alternative confidence sets following Rambachan and Roth (2021) for the remaining 9 employer minimum wage announcements in Figure D16. In no case do we observe confidence intervals crossing zero for deviations in parallel trends up to 1 times the standard deviation of the coefficient β_0, including when we consider positive bias (wages trending upward) in the pre-period (see Figure D17).

Figure D10: Walmart, Target, and Costco MW spillovers: robust to occupation-by-CZ-by-month fixed effects



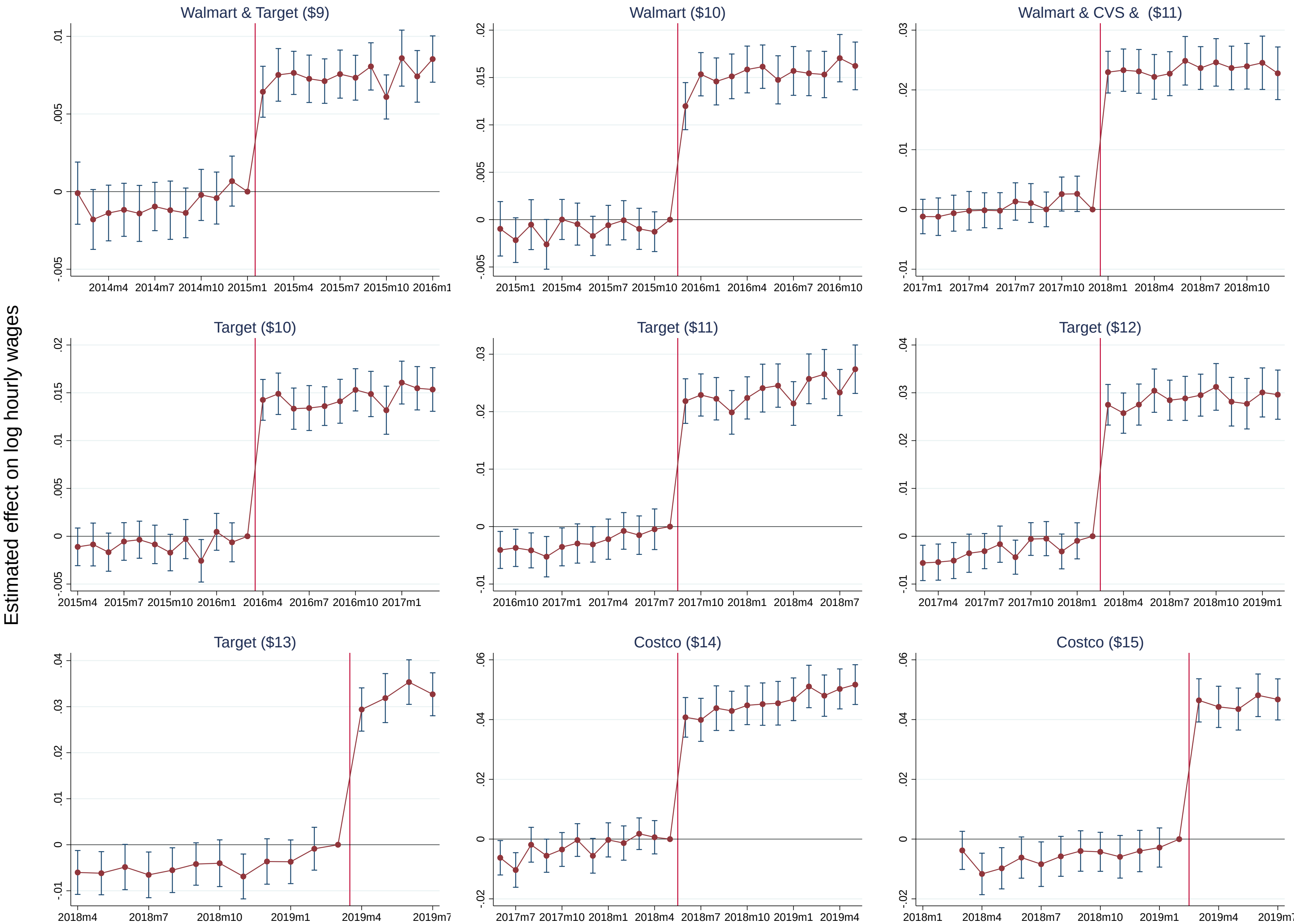
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ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

Figure D11: Walmart, Target, and Costco MW spillovers: robust to occupation-by-CZ-by-month, employer-by-month fixed effects



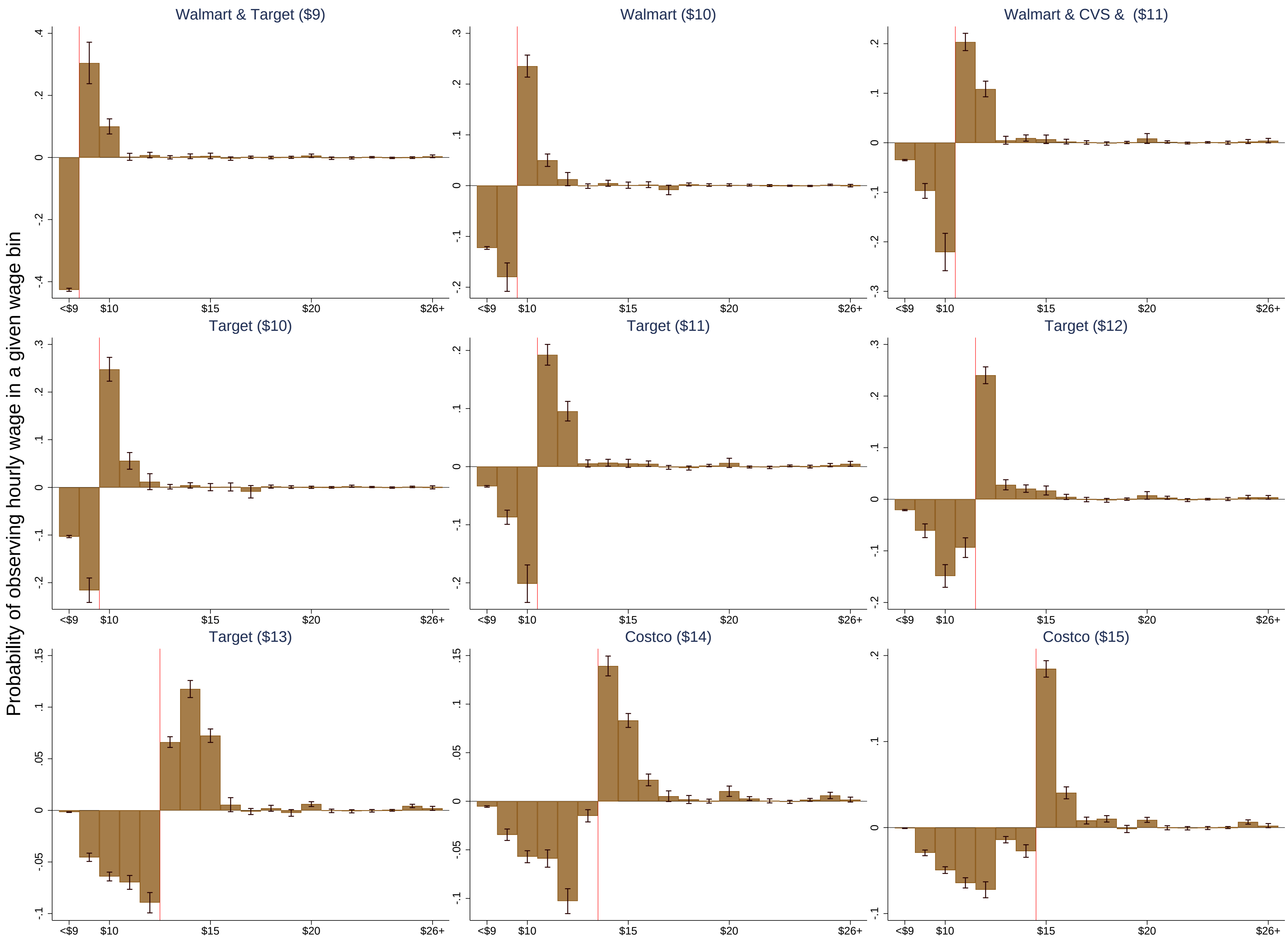
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ 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. The sample is restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure D12: Spillovers in worker-reported wages from Walmart, Target, and Costco minimum wages (Glassdoor)



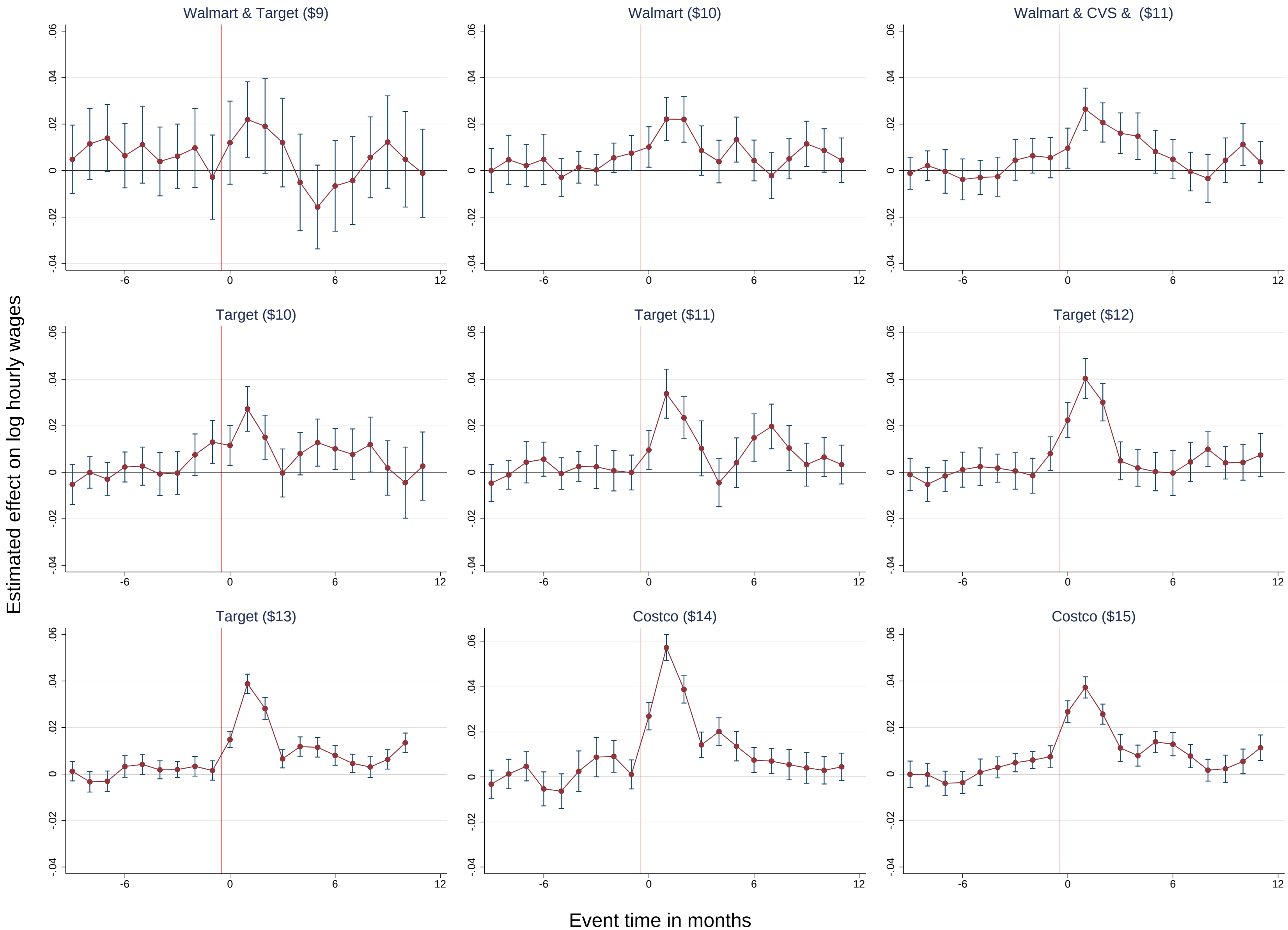
Notes: This figure plots the coefficients on the interaction between exposure to policy firm minimum wages and month fixed effects, where the dependent variable is log reported hourly wage by workers at non-policy employers. Exposure is defined as the fraction of each non-policy employer's job postings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

Figure D13: Bunching in response to Walmart, Target, and Costco minimum wages



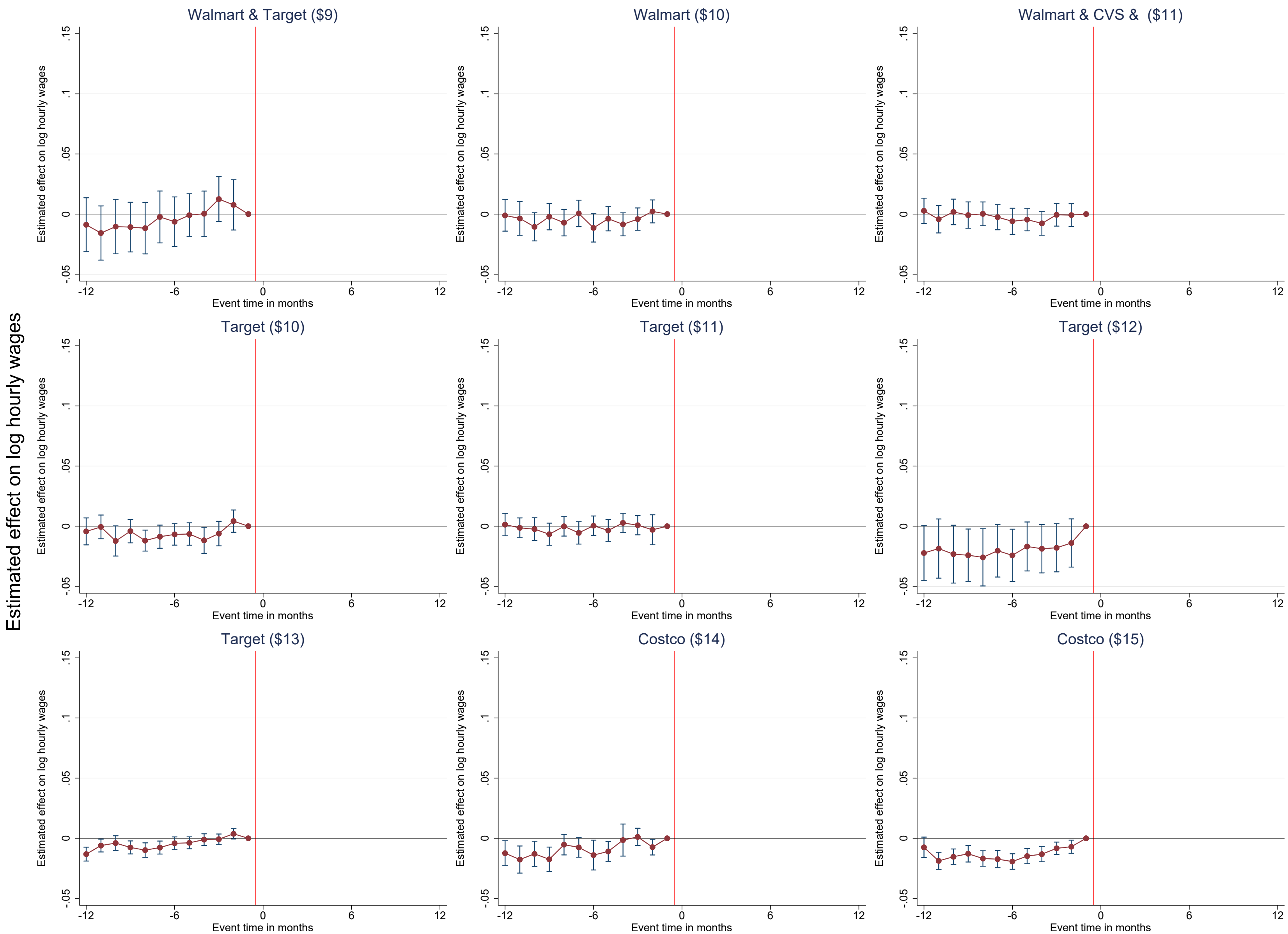
Notes: This figure plots the coefficients from linear probability regressions of hourly wages being in a given wage bin on the interaction between job-level exposure to policy firm minimum wages for non-policy employers and an indicator for post-October-2018. Exposure is defined as the fraction of non-policy 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure D14: Null effects at placebo treatment dates for Walmart, Target, and Costco minimum wages



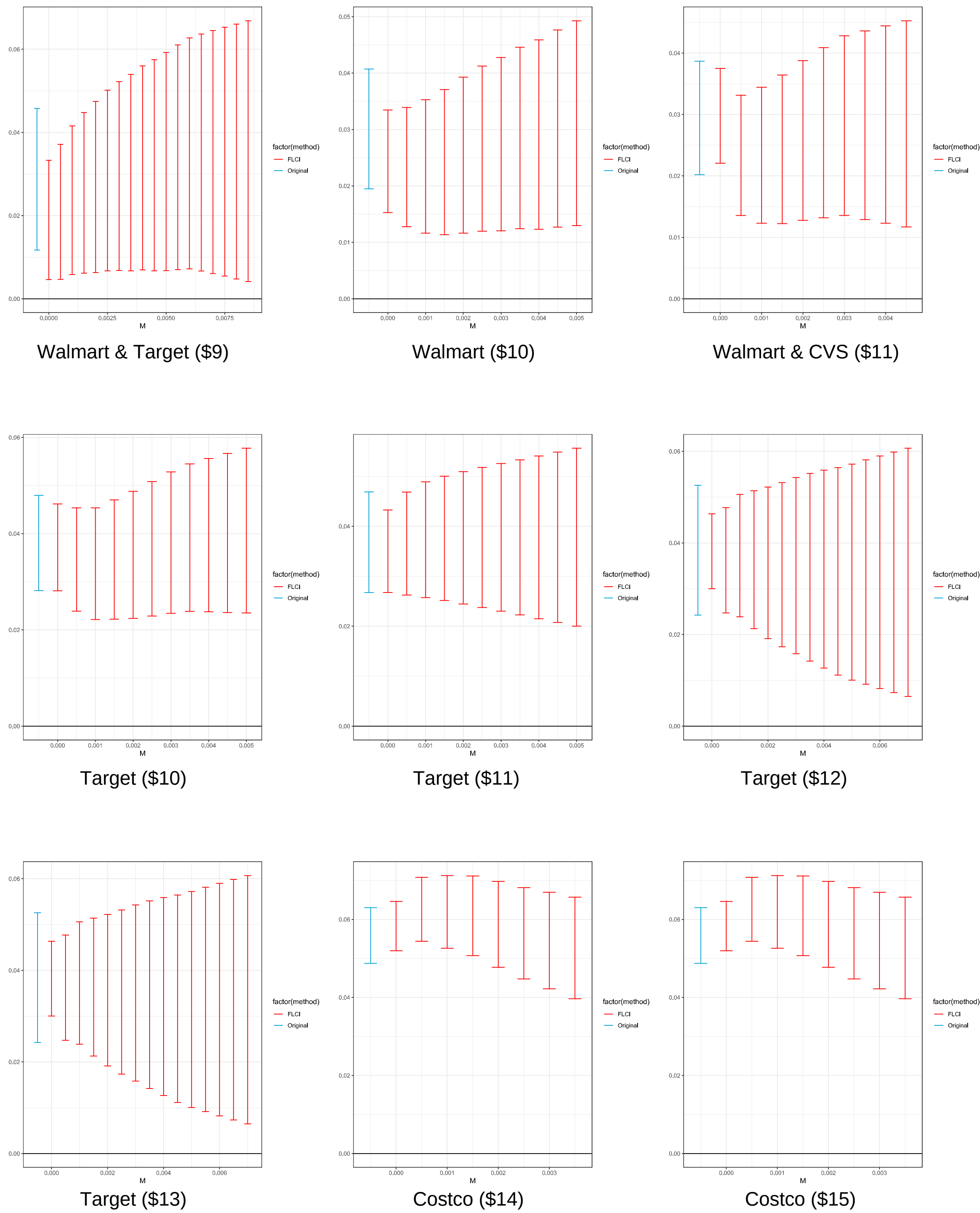
Notes: This figure plots the regression coefficients on the interaction between job-level exposure to policy firm minimum wages for non-policy employers and an indicator for post-treatment for placebo treatment dates, using a 4-month observation window. Coefficients are indexed by the last month of the 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. The sample is restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure D15: Separately estimated pre-trends in spillovers for Walmart, Target, and Costco minimum wages



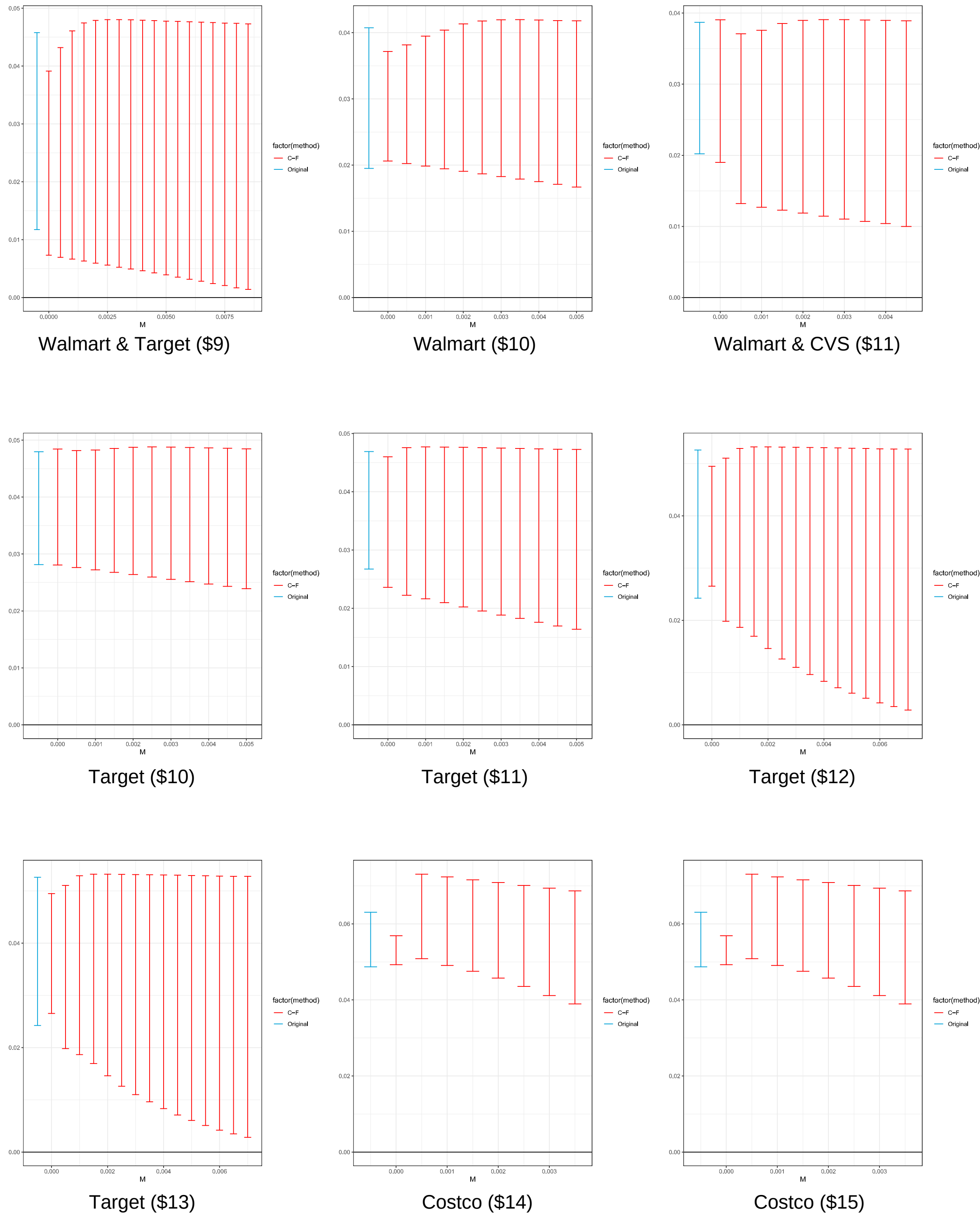
Notes: This figure plots the coefficients β_k from equation 2 for pre-announcement months only, following the proposed tests for absence of pre-trends in (Borusyak et al., 2021; Borusyak and Schönberg, 2021). The joint significance F-test p-values are 0.70 (Walmart/Target \$9), 0.70 (Walmart/Target \$9), 0.70 (Walmart/Target \$9), 0.70 (Walmart/Target \$9), 0.21 (Walmart \$10), 0.42 (Walmart/CVS \$11), 0.08 (Target \$10), 0.80 (Target \$11), 0.29 (Target \$12), 0.00 (Target \$13), 0.00 (Costco \$14), and 0.00 (Costco \$15). Pre-trends are more pronounced in later policy firm announcements, particularly those that follow close after a previous announcement, including those by other firms. 95% confidence intervals shown.
Data sources: Burning Glass Technologies online vacancy data.

Figure D16: Honest pre-trends in spillovers from Walmart, Target, and Costco minimum wages



Notes: This figure plots the coefficients β_k from equation 2 for the first post-treatment month only and plots alternative confidence intervals that allow for deviations from parallel trends in the pre-period, following the honest parallel trends estimation procedure of Rambachan and Roth (2021). 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure D17: Honest pre-trends in spillovers for Walmart, Target, and Costco minimum wages (positive bias)



Notes: This figure plots the coefficients β_k from equation 2 for the first post-treatment month only and plots alternative confidence intervals that allow for deviations from parallel trends in the pre-period, following the honest parallel trends estimation procedure of Rambachan and Roth (2021), and allowing for a positive bias in the pre-trend. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

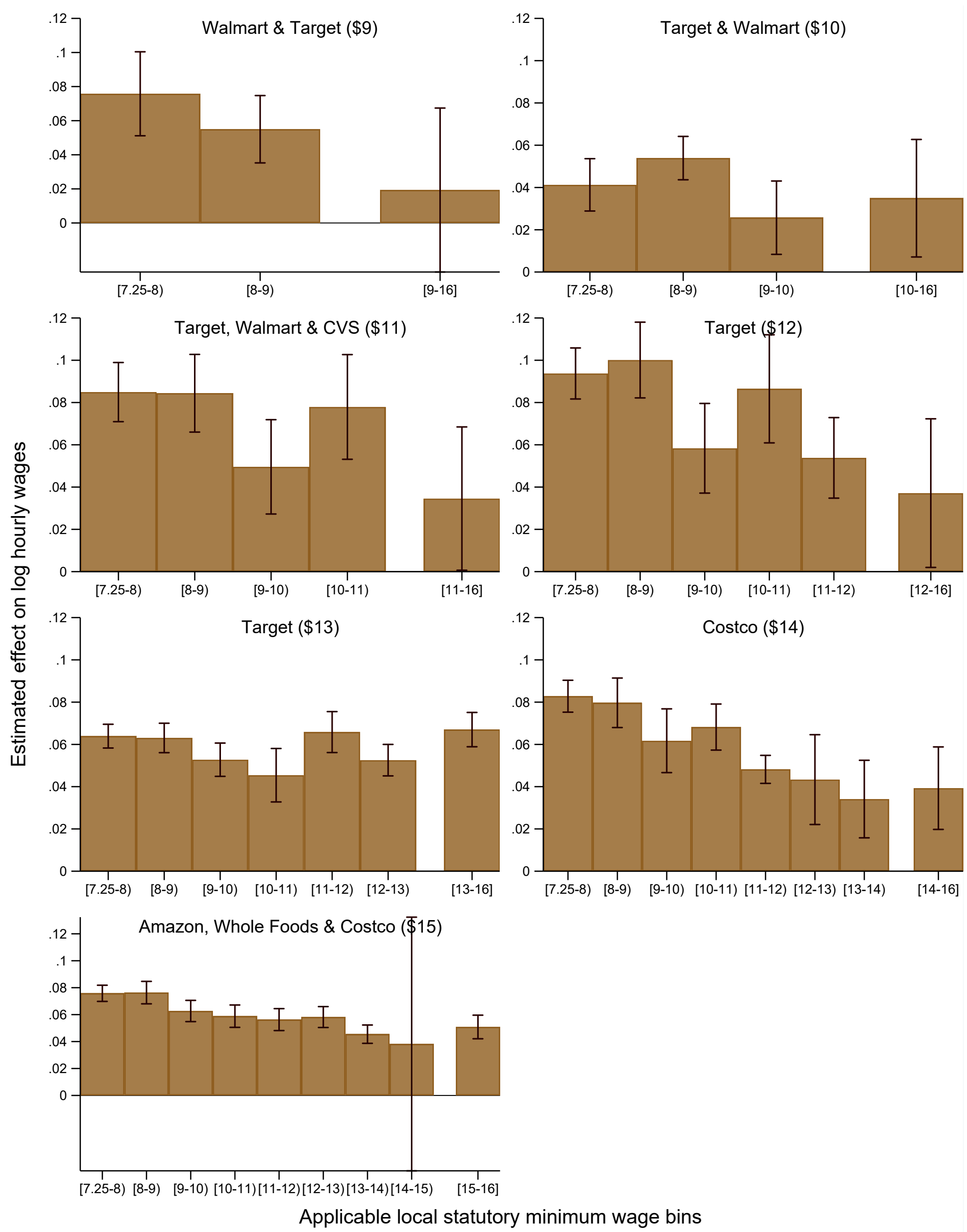
E Additional evidence on employer minimum wage spillovers

In this section, we provide additional evidence on spillovers from employer minimum wages. First, we assess the degree to which local labor market characteristics moderate wage spillovers. Next, we report results on the wage and employment effects of Walmart, Target, and Costco minimum wages estimated using data from the CPS. Finally, we explore other margins of employer adjustment to policy firm minimum wages, such as changes in the number of postings and the inclusion of skill requirements on job ads.

E.1 Local moderators of spillovers

Figure E1 illustrates moderation of spillovers via local statutory minimum wages. We stack all policy firm announcements for the same dollar level and plot the coefficient on exposure interacted with post separately for different bins of the local statutory minimum wage. In 4 of the 6 announced employer minimum wages, effects are larger in areas with statutory minimum wages below the announced wage level. This is not the case of Target's \$13 minimum wage; however, it should be noted that Costco's \$15 announcement occurred one month before Target's announcement, which may explain the larger than expected wage spillover effects in the $13-16$ bin. Furthermore, effects in areas with higher statutory minimum wages are consistent with spillovers up the wage distribution which we document in our bunching analysis in Figure D13.

Figure E1: Moderation of spillover effect with local minimum wage

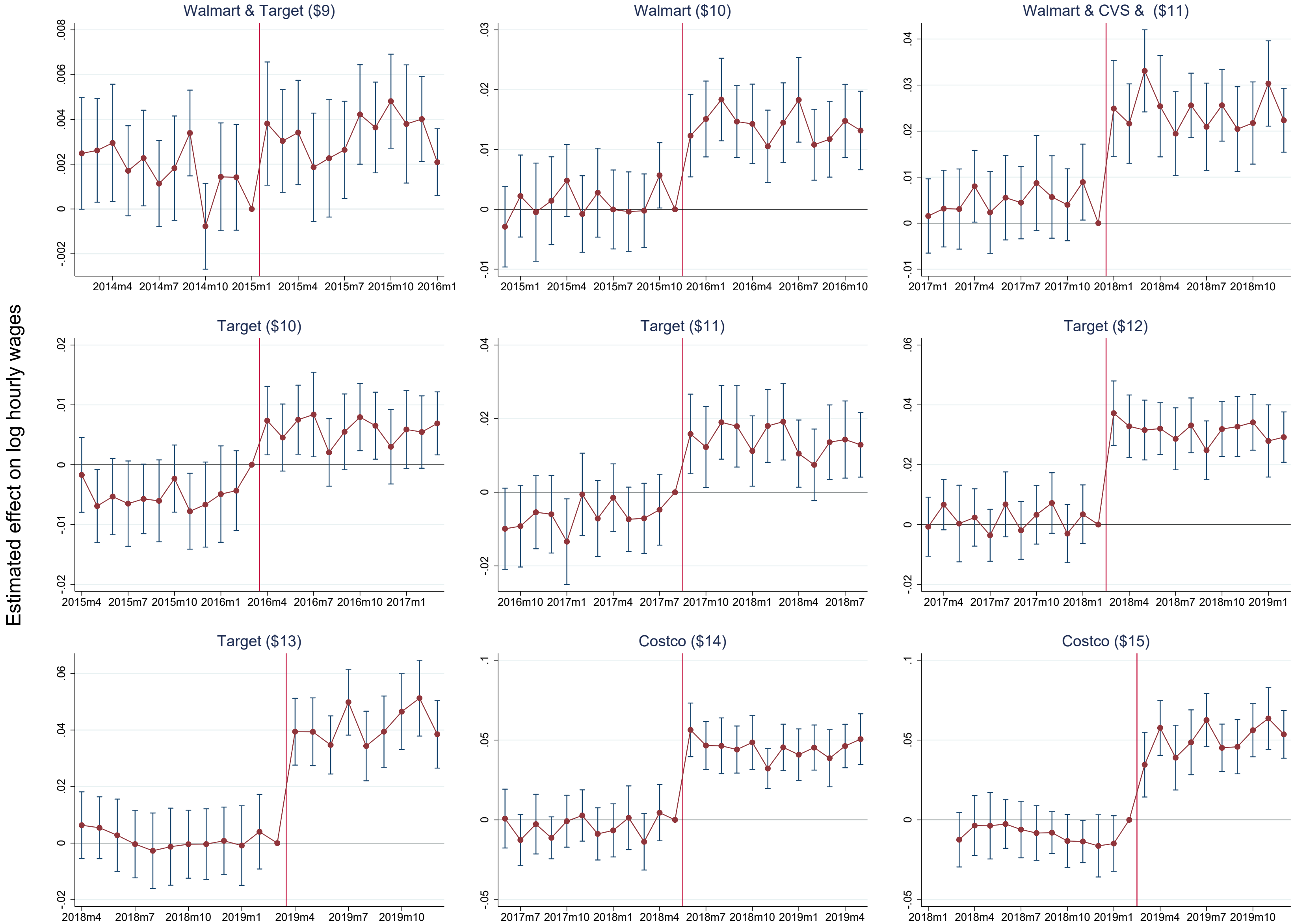


Notes: This figure plots the coefficients on the interaction between exposure to the policy firm's minimum wage and an indicator for the post period, where the dependent variable is log advertised hourly wage. Each bar indicates a separate regression where only postings in the indicated minimum wage 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. The sample is restricted to non-policy employer postings with valid hourly wage data, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

E.2 Wage and employment spillovers from Walmart, Target, and Costco minimum wages, estimated in the CPS

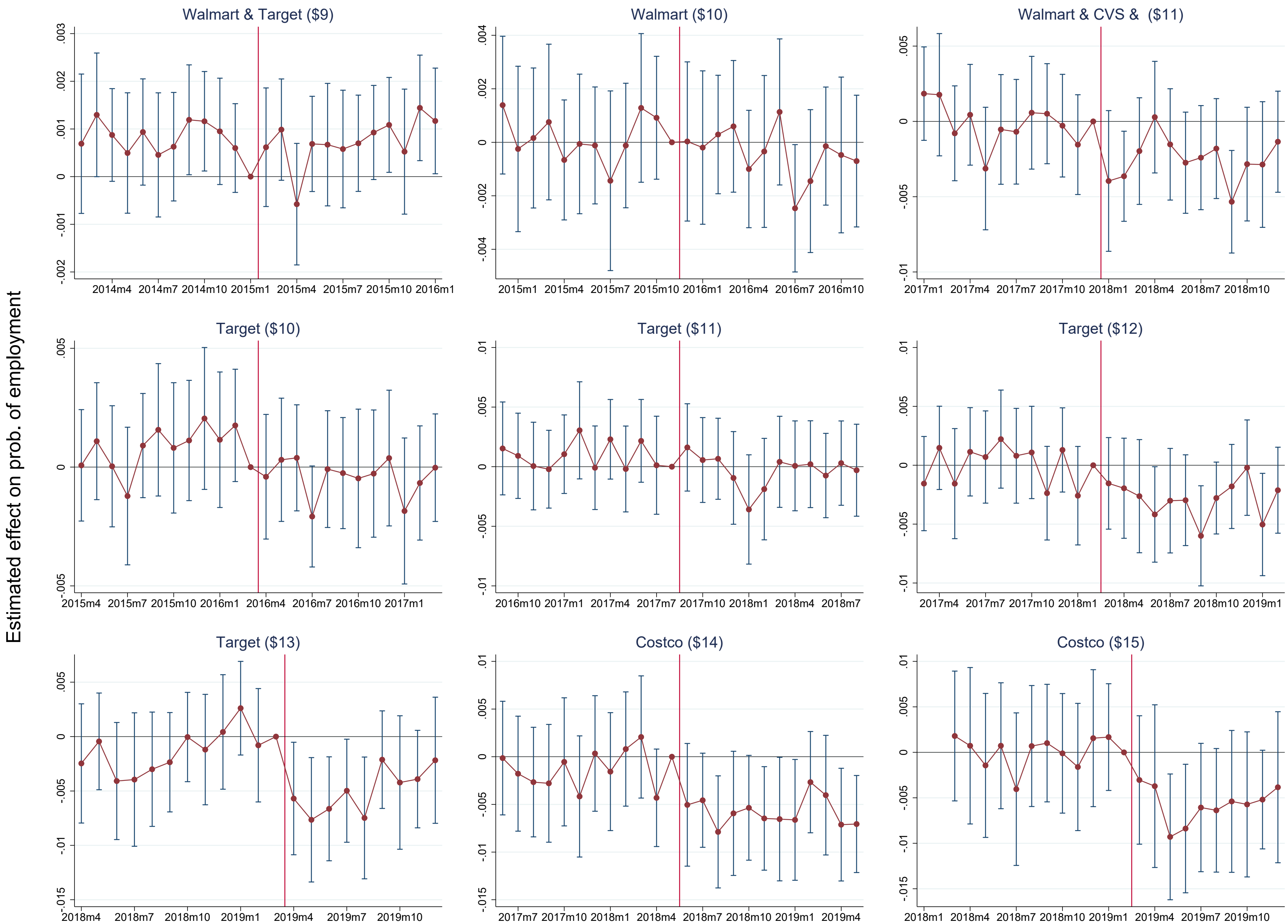
Below in Figures E2 and F4, we report the wage and employment effects of Walmart, CVS, Target, and Costco minimum wages described in Section 5.

Figure E2: Wage spillovers from Walmart, Target, and Costco minimum wages, using CPS data



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. The sample is 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. *Data sources*: CPS ORG.

Figure E3: Employment effects of Walmart, Target, and Costco minimum wages, using CPS data

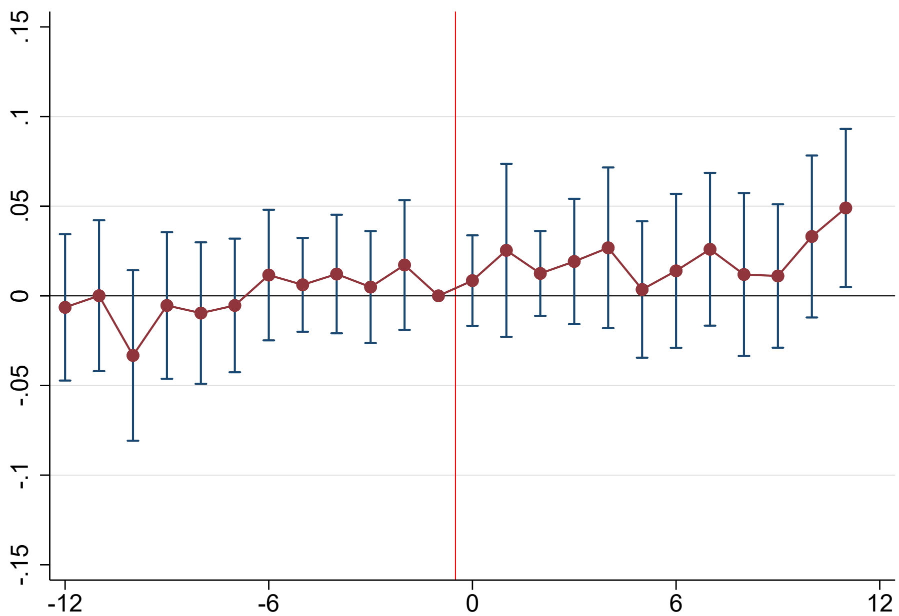


Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy industries interacted with month fixed effects, where the dependent variable is probability of being employed vs. unemployed. Exposure is defined as the fraction of non-policy industry 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. *Data sources*: CPS ORG.

E.3 Examining other margins of employer adjustment

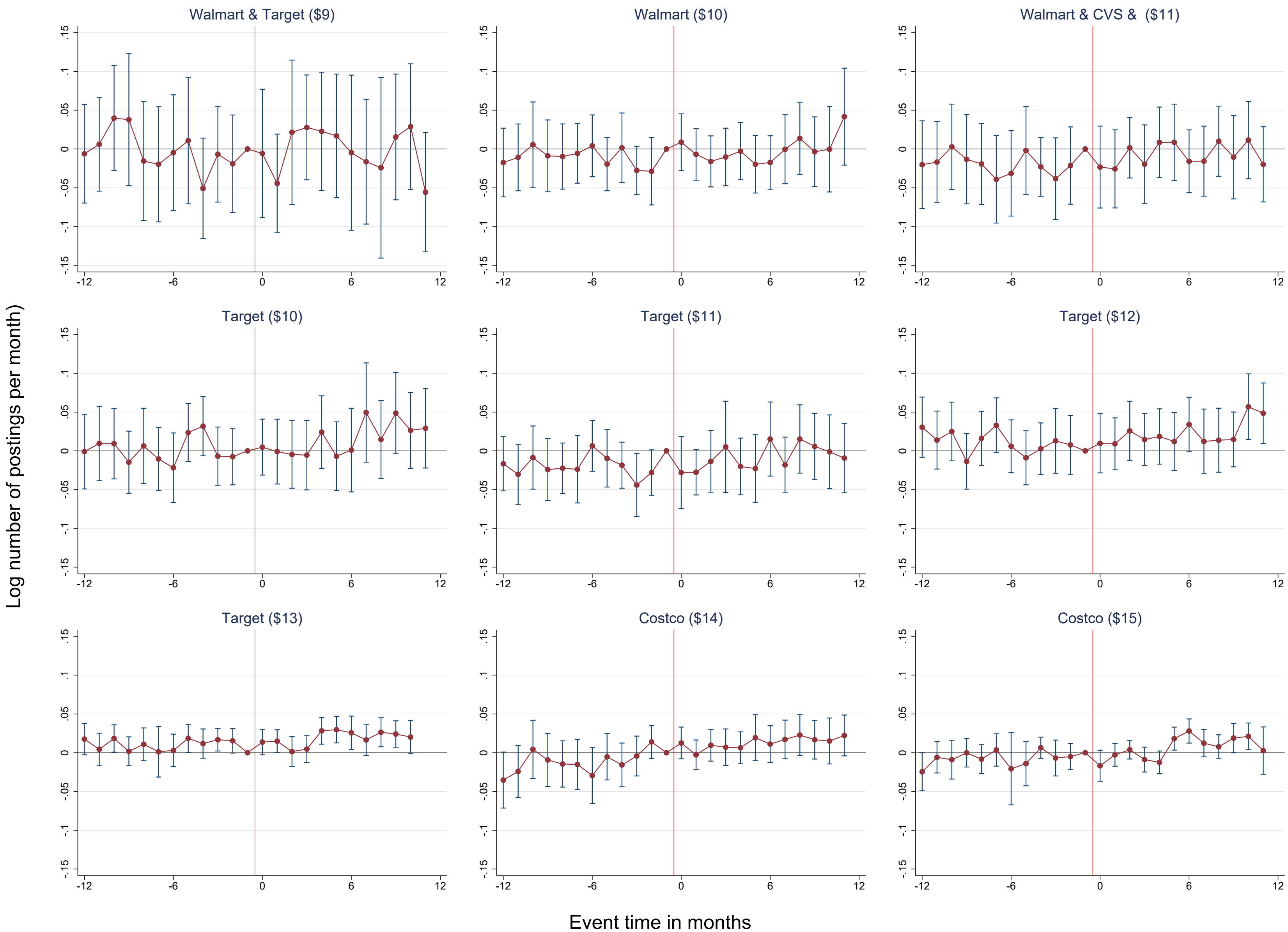
Below in Figures E4, E5, E6, E7, E8, and E9, we report the effect of policy firm minimum wages on the number of postings and the presence of experience and degree requirements on job ads by non-policy employers, as described in Section 5.2.

Figure E4: Effects of Amazon minimum wage on log number of job postings



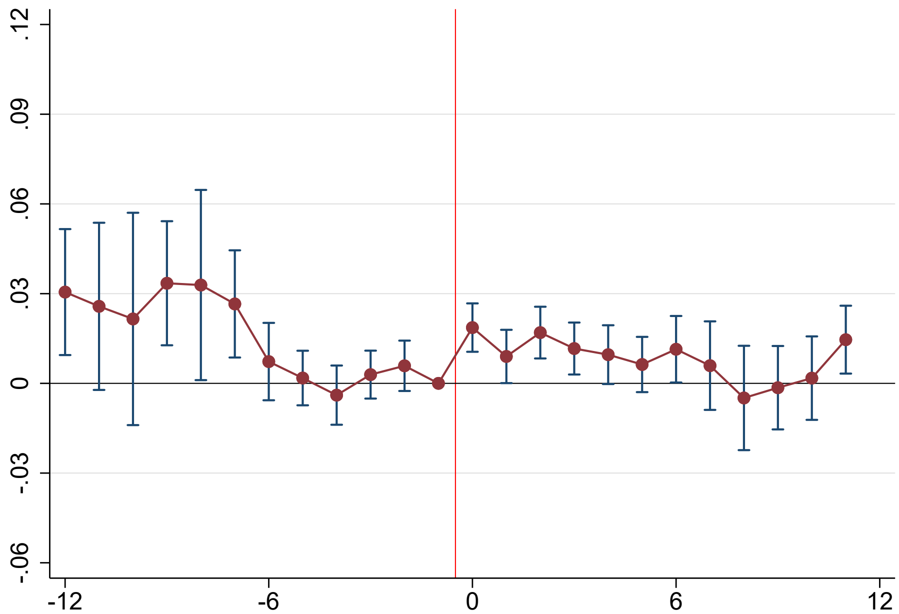
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log number of postings per employer in a given month, occupation and commuting zone. Exposure is defined 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. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure E5: Effects of Walmart, Target, and Costco minimum wages on log number of job postings



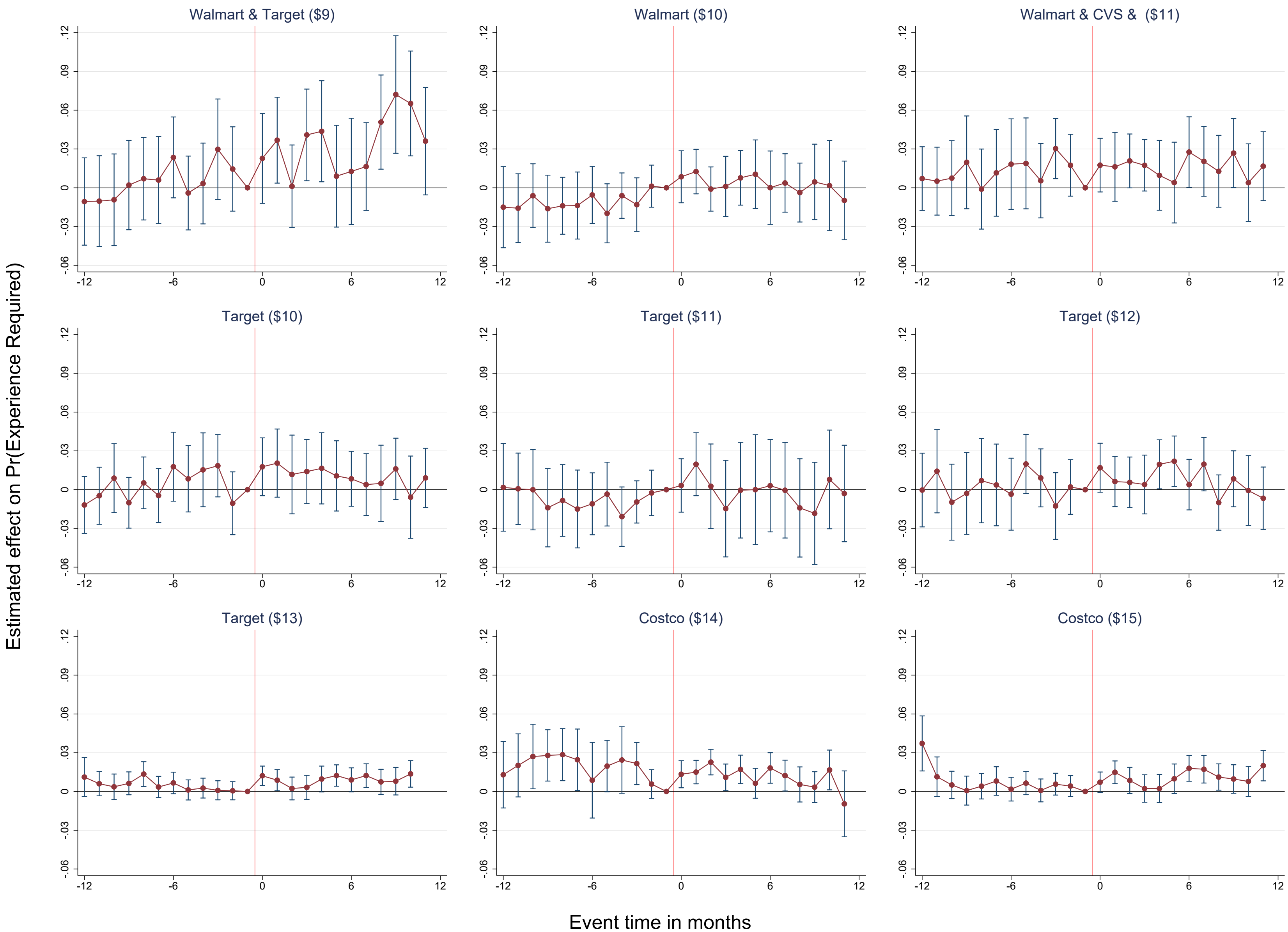
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is the log number of postings per employer in a given month, occupation and commuting zone. Exposure is defined as the fraction of non-policy firm 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. The sample is restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure E6: Effects of Amazon minimum wage on experience requirements in job ads



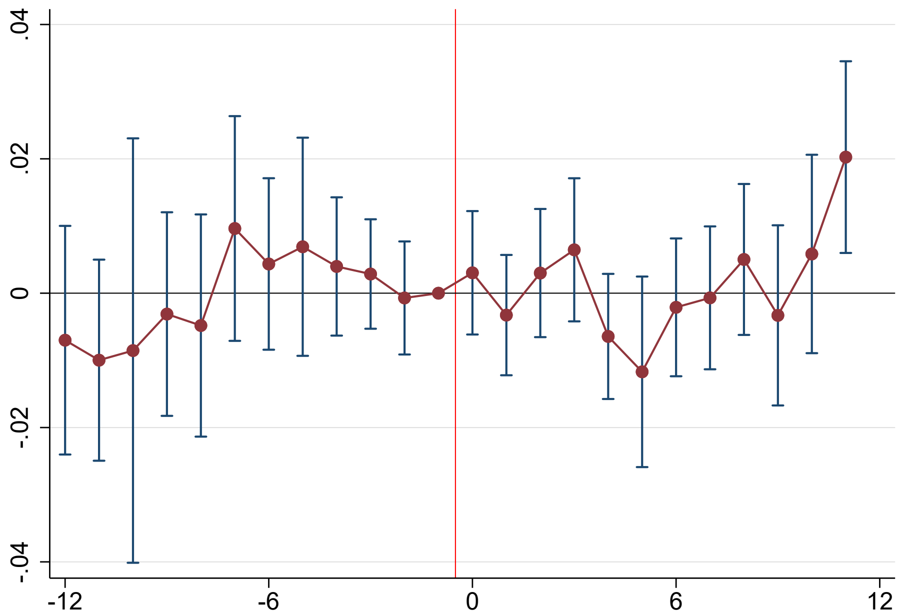
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage for non-Amazon employers interacted with month fixed effects, where the dependent variable is a binary variable equal to one if the posting includes a minimum number of years of required experience. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below Amazon's minimum wage in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure E7: Effects of Walmart, Target, and Costco minimum wages on experience requirements in job ads



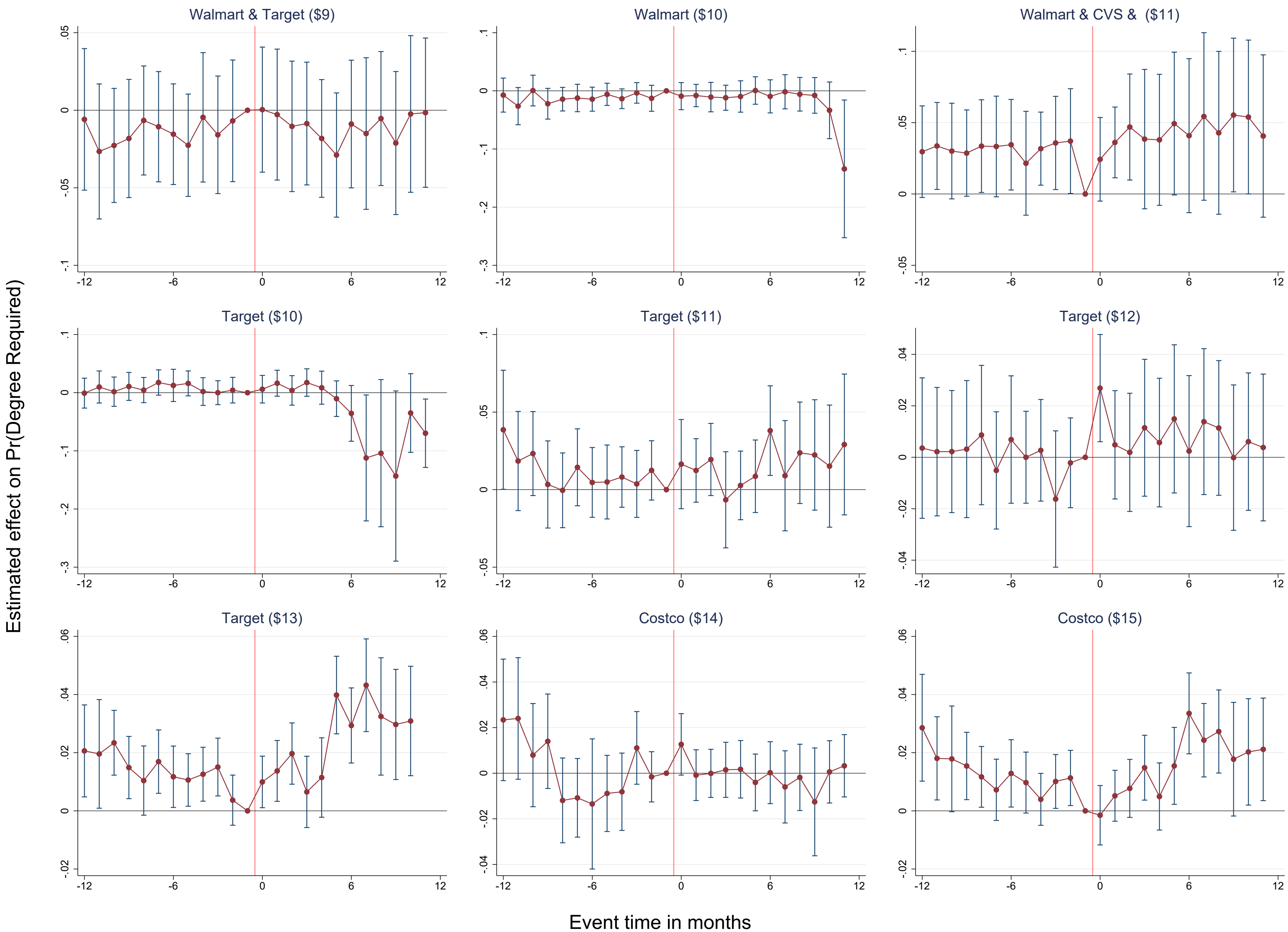
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is a binary variable equal to one if the posting includes a minimum number of years of required experience. Exposure is defined as the fraction of non-policy firm postings in each occupation-employer-CZ cell with wages below the policy firm's minimum wage in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The sample is restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure E8: Effects of Amazon minimum wage on degree requirements in job ads



Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage for non-Amazon employers interacted with month fixed effects, where the dependent variable is a binary variable equal to one if the posting includes a minimum required educational degree. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below Amazon's minimum wage in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The sample is restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure E9: Effects of Walmart, Target, and Costco minimum wages on degree requirements in job ads

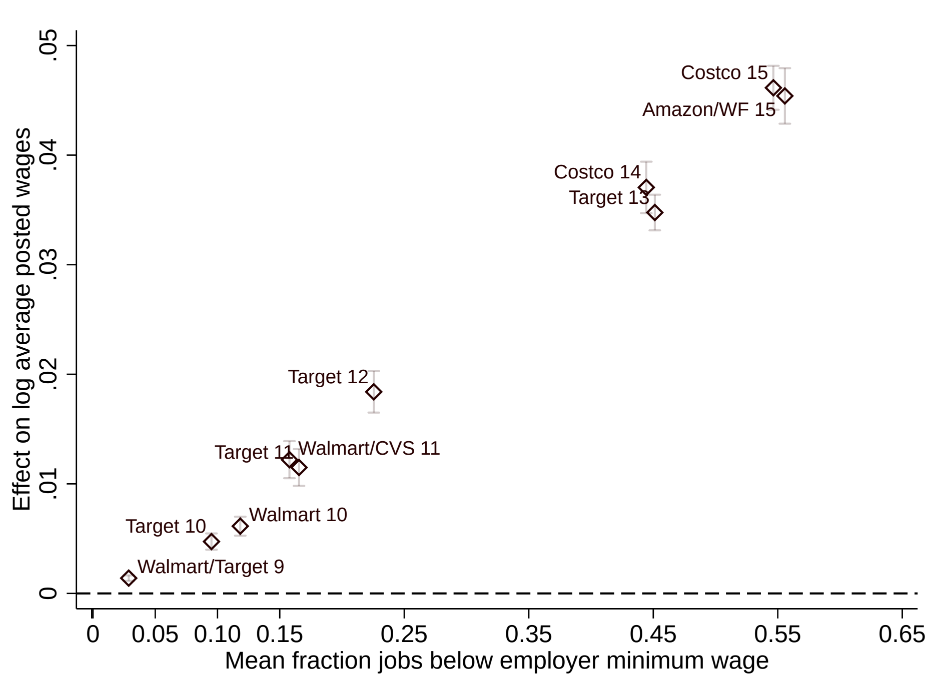


Notes: This figure plots the regression coefficients on job-level exposure to policy firms' minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is a binary variable equal to one if the posting includes a minimum required educational degree. Exposure is defined as the fraction of non-policy firm postings in each occupation-employer-CZ cell with wages below the policy firm's minimum wage in the year before treatment. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The sample is restricted to non-policy employers' postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

E.4 Comparison of spillover effects across employer minimum wage policies

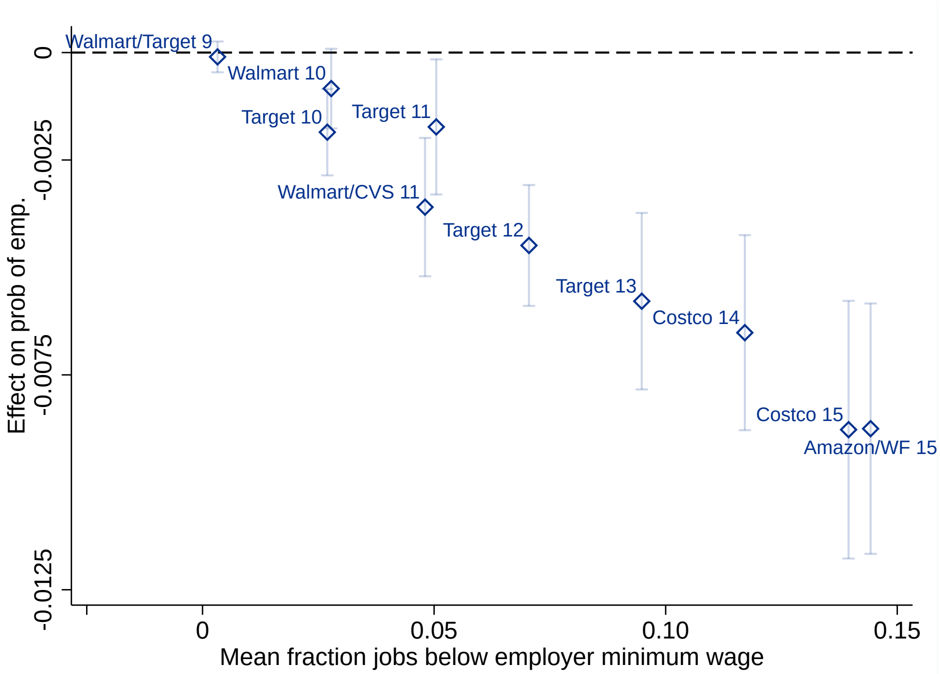
The following figures plot the relationship between spillovers in employer minimum wage policies and the bite of the announced minimum wage among non-policy firm job postings. As shown in Figures E10 and E11, wage and employment spillovers respectively increase and decrease monotonically with the bite of the policy firm's announced minimum wage.

Figure E10: Wage spillover effects increase with bite of employer minimum wage



Notes: This figure plots the coefficients on the interaction between job-level exposure to policy firm minimum wages and an indicator for post-treatment period. The dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy firm postings in each occupation-employer-CZ cell with wages below the policy firm minimum wage in the year prior to the announcement. Exposure is normalized by the average job's exposure. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The x-axis measures average exposure to the policy firm's minimum wage. Sample is restricted to postings with valid wage data and hourly rate of pay, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure E11: Disemployment effects increase with bite of employer minimum wage



Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy 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-policy industry workers in each occupation-CZ cell with wages below the policy firm minimum wage in the year prior to the announcement. 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. The x-axis measures average exposure to the policy firm's minimum wage. Sample is restricted to individuals aged 25 to 65 and excludes those not in the labor force. 95% confidence intervals shown. *Data sources*: CPS ORG.

F Evaluating competition as a mechanism

This section provides additional details on our analysis of competition between employers as a mechanism for wage spillovers in the context of voluntary employer minimum wage announcements. We test to see if wage spillover effects are moderated by inter-firm competition at the local level along the following dimensions: in the case of Amazon, its overall level of hiring at the commuting zone level (Section F.1); how tight local labor markets are as measured by the unemployment rate in the commuting zone (Section F.2); the percentage of occupational postings accounted for by policy firms in commuting zones (Section F.3); and the likelihood that workers in a subset of occupations move to Amazon in the local market (Section F.4).

F.1 Amazon hiring in local labor markets

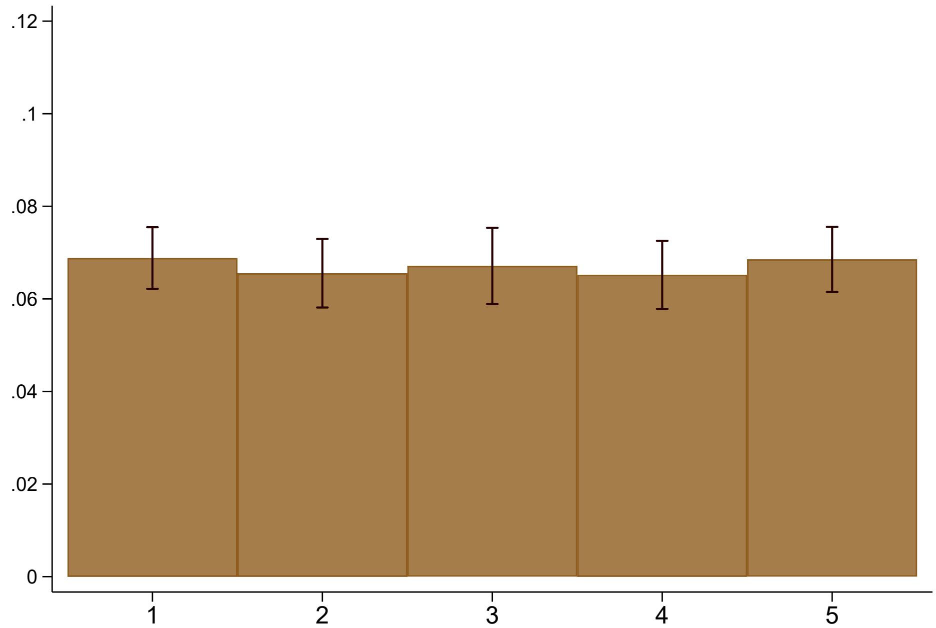
We assess whether Amazon's local hiring is large enough to generate wage responses consistent with demand channels in a simple, perfectly competitive labor market. Abstracting from other inputs and modeling production as a concave function of labor with a demand elasticity of -1, Amazon would have to hire 5% of the local labor force to generate a wage increase of 5% in the rest of the labor market.

We collected data on all Amazon hiring announcements we could obtain from local news sources and the company's website between 2017 and 2019 (86 announcements total), which we make available in our replication files (see `www.elloraderenoncourt.com/us-inequality-data`). We combine this information with data on the size of the civilian labor force in each commuting zone that contains an Amazon facility (56 CZs total). We find that, on average, Amazon's hiring between 2017 and 2019 amounted to 0.71% of the 2017 local labor force size. If we take into account estimates of turnover in counties with Amazon warehouses in 2017 from Tung and Berkowitz (2020) and assume turnover at these rates without replacement, we generate a maximum estimate of 2.14%. Even this maximum average labor force share for Amazon is insufficient to explain increases in average wages of 5%.

F.2 Moderation by the local unemployment rate

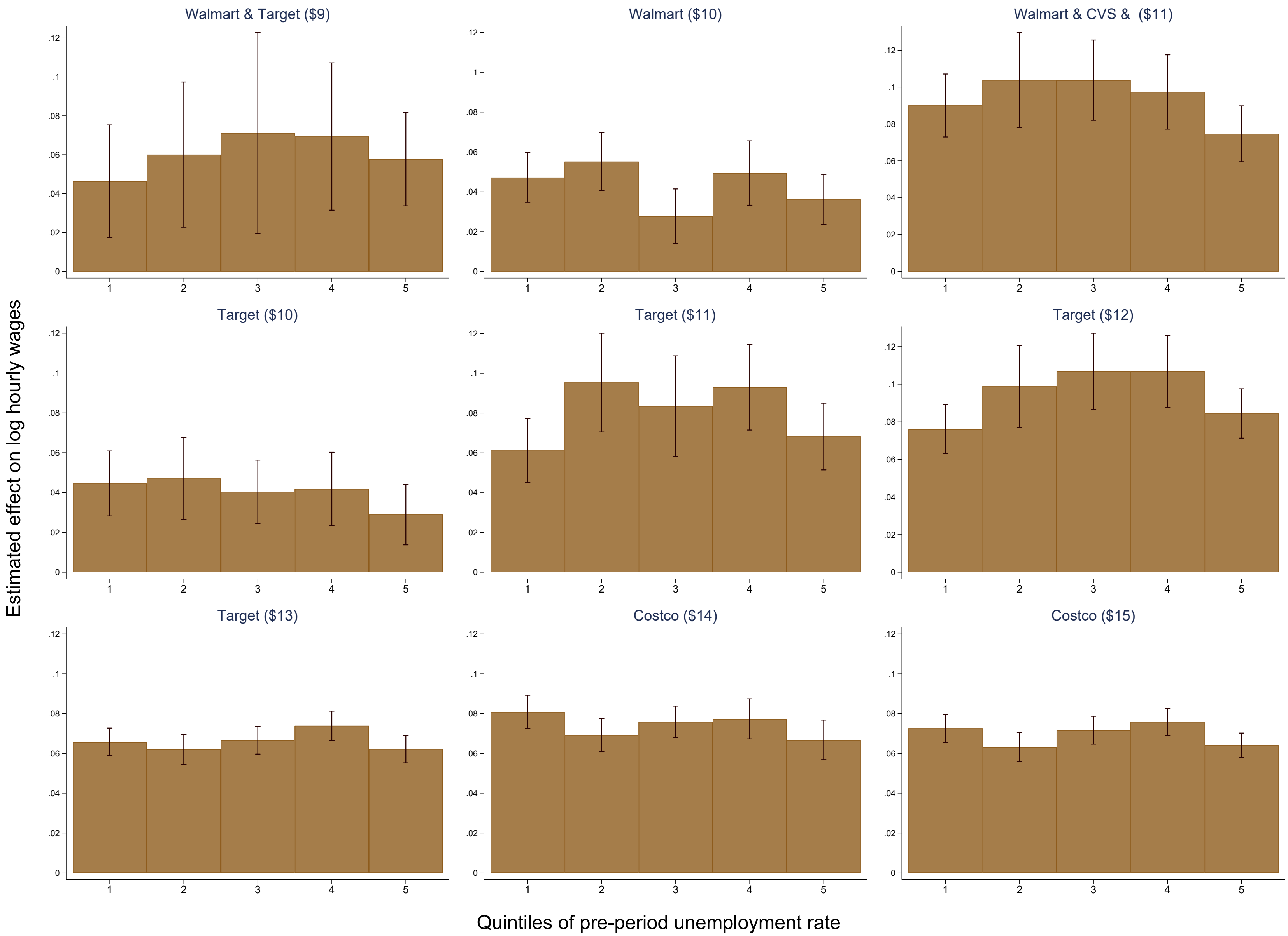
We examine whether wage spillovers are smaller in slack labor markets, as measured by the pre-period unemployment rate. We divide non-policy firm postings into quintiles of their CZ's average monthly unemployment rate in the 12 months before each policy firm's announcement. We then estimate the spillover effect separately for each quintile. Figure F1 reports the results for Amazon's minimum wage while Figure F2 reports the results for the rest of the employer minimum wage changes. There is no clear moderation by the local unemployment rate. Labor markets were quite tight during the period of these announcements, which may explain the lack of clear moderation by this factor.

Figure F1: Heterogeneity in Amazon wage spillover effect by pre-period unemployment rate



Notes: This figure plots the coefficients on the interaction between exposure to the Amazon's minimum wage and an indicator for the post-announcement period, for non-Amazon employers, where the dependent variable is log advertised hourly wage. Each bar indicates a separate regression where only postings in a given quintile of the pre-period average CZ unemployment rate are included. Exposure is defined as the fraction of each non-Amazon employer postings in specific employer-by-occupation-by-CZ cells with wages below Amazon's minimum wage in the year before the announcement. Employer-by-occupation-by-CZ fixed effects and occupation-by-month are included. The sample is restricted to non-Amazon employer postings with non-missing hourly wage data, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure F2: Heterogeneity in Walmart, Target, and Costco wage spillover effects by pre-period unemployment rate



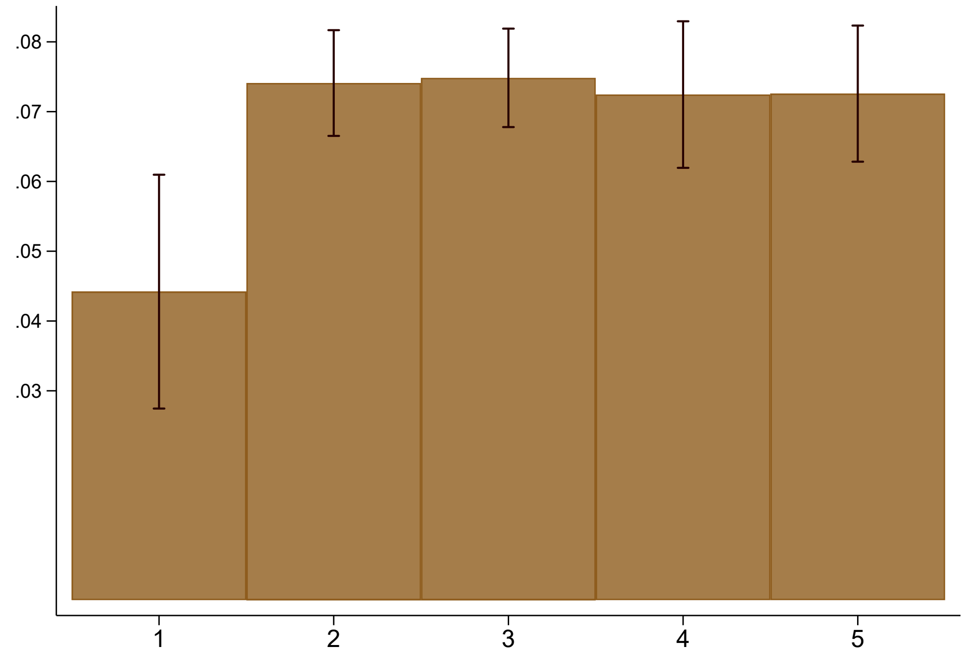
Notes: This figure plots the coefficients on the interaction between exposure to the policy firm's minimum wage and an indicator for the post-announcement period, for non-policy employers, where the dependent variable is log advertised hourly wage. Each bar indicates a separate regression where only postings in a given quintile of the pre-period average CZ unemployment rate are included. Exposure is defined as the fraction of each non-Amazon employer postings in specific employer-by-occupation-by-CZ cells with wages below Amazon's minimum wage in the year before the announcement. Employer-by-occupation-by-CZ fixed effects and occupation-by-month are included. The sample is restricted to non-Amazon employer postings with non-missing hourly wage data, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

F.3 Moderation by policy firm vacancy share

We examine whether wage spillovers are larger in occupations where the policy firm makes up a high share of vacancies within 6-digit occupation by commuting zone cells. Non-policy employers advertising in these occupation-by-CZ cells may face greater competition from the policy firms in the wake of a wage increase at the latter. They may be particularly likely to increase wages in response in order to stem the flow of workers to the policy firm.

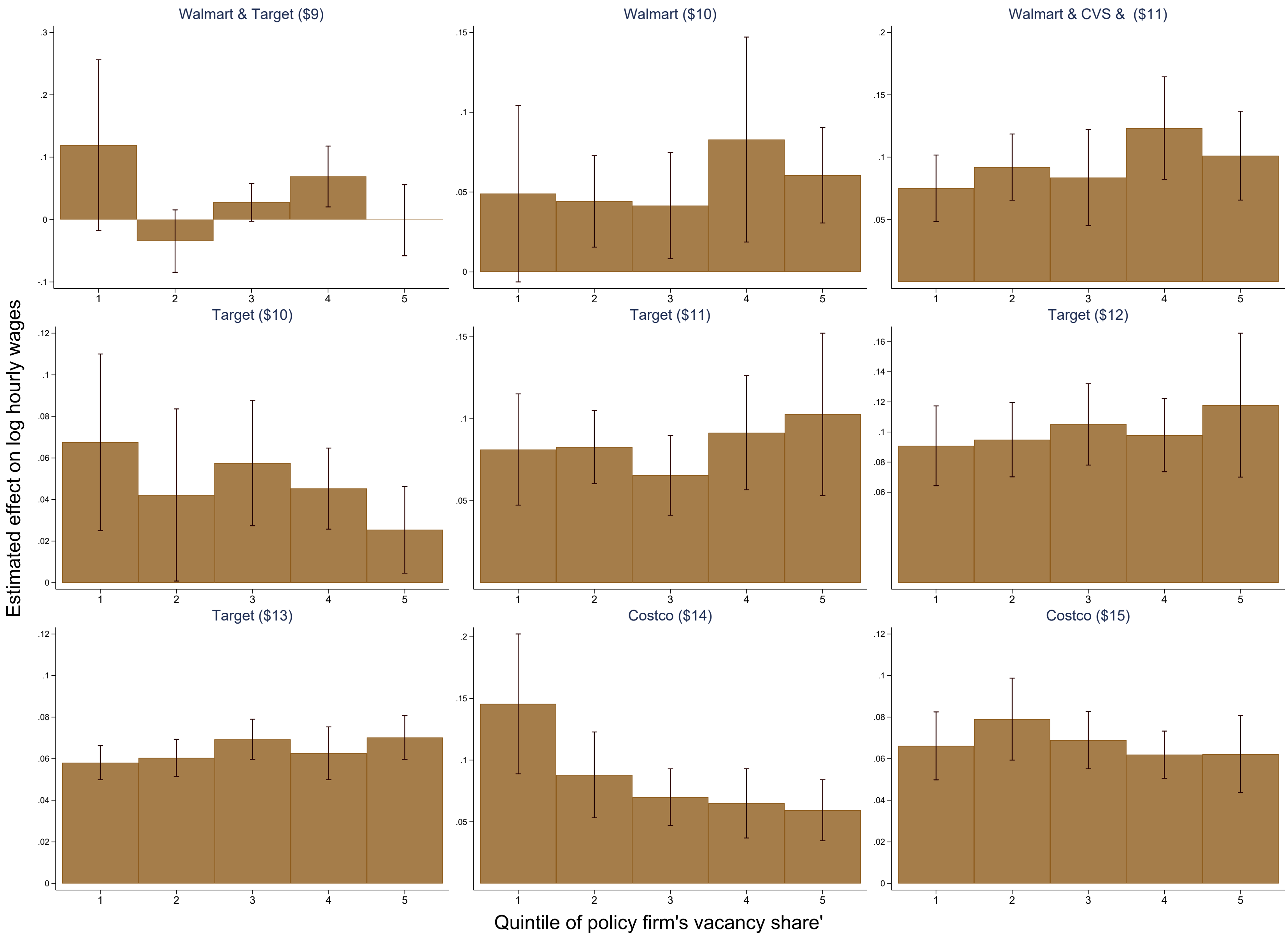
To test this mechanism, for each announcement, we calculate the share of all vacancies in an occupation-by-CZ cell that belong to the policy firm. We then divide our sample of postings into quintiles of the policy firm's vacancy share and estimate the wage spillover effect separately for each quintile. Figure F3 reports the results for Amazon's minimum wage and Figure F4 reports the results for the rest of the employer minimum wage policies. In the case of Amazon, spillovers are smaller in the lowest quintile of Amazon's vacancy share, but do not vary across higher quintiles. There is no systematic relationship between policy firm quintile and the size of the spillover in the case of the other employer minimum wage announcements.

Figure F3: Amazon spillover effect by Amazon's vacancy share quintile



Notes: This figure plots the coefficients on the interaction between exposure to the Amazon's minimum wage and an indicator for the post-announcement period, for non-Amazon employers, where the dependent variable is log advertised hourly wage. Each bar indicates a separate regression where only postings in a given quintile of the pre-period Amazon vacancy share are included. Exposure is defined as the fraction of each non-Amazon employer's postings with wages below Amazon's minimum wage in the year before treatment. Employer fixed effects and occupation-by-month fixed effects are included. The sample is restricted to non-Amazon employer postings with non-missing hourly wage data, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

Figure F4: Walmart, Target, and Costco spillover effects by policy firm vacancy share quintile



Notes: This figure plots the coefficients on the interaction between exposure to the policy firm's minimum wage and an indicator for the post-announcement period, for non-policy employers, where the dependent variable is log advertised hourly wage. Each bar indicates a separate regression where only postings in a given quintile of the pre-period policy firm vacancy share are included. Exposure is defined as the fraction of each non-policy employer's postings with wages below the policy firm's minimum wage in the year before treatment. Employer fixed effects and occupation-by-month fixed effects are included. The sample is restricted to non-policy employer postings with non-missing hourly wage data, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.

F.4 Moderation by occupational transition probabilities

It's possible that despite making up a large share of vacancies for particular occupations, policy firms may nevertheless draw workers from different occupations. Thus, vacancy share variation may not fully capture which non-policy employers face the most competition from policy firms. Concretely, Amazon may make up a large share of warehousing vacancies in a particular location while largely filling those vacancies with former retail or food service employees. Thus, retail and food service employers may be the more relevant labor market competitors for Amazon than other employers of warehouse workers. This is illustrated with the job histories of a sample Amazon workers shown in Table F1.[37] Two of the three resumes show workers transitioning from retail and food service into warehousing occupations. The third shows the resume of a worker who transitioned from early childhood education into a warehousing position with Amazon.

We use the full set of occupational transition probabilities derived from Burning Glass Technologies resume data to test this hypothesis.[38] First we identify the top occupations advertised by policy firms or listed by resumes of workers at those firms. For Amazon, for example, the plurality of ads are for two occupation categories: 1) Laborers and Hand Freight, Stock, and Material Movers and 2) Stock Clerks and Order Fillers (see Table F2). We also consider the two occupation categories that make up a substantial share of the positions listed on Amazon workers' resumes: Order Clerks and Hand Packers and Packagers. We then calculate the share of non-Amazon workers in a given occupation who transition to Amazon for their next job and work in any of these four common Amazon occupations. We repeat this exercise for Walmart, Target, and Costco.

We then examine whether or not wage spillovers to non-policy postings are moderated by these occupational transition probabilities, focusing on four experiments that occur later in our sample period and which are not close to announcements by other policy firms. We split our sample of postings into deciles of occupational transition probabilities and estimate spillovers separately within each decile. Figure F5 reports these results. In general, we see no systematic moderation of the spillover effects by quintile of transition probability. In the case of Target's $12 announcement, the lowest decile has smaller wage spillovers than the top decile but the effects are quite uniform across deciles 2 through 10. Thus, we do not see strong evidence that high transition rates to policy firm positions moderates the wage spillover effect.

[37] Data are from Burning Glass Technologies resume data.

[38] A description of the original resume data is available in Schubert et al. (2021).

Table F1: Sample job histories of Amazon workers

Worker 1

Employer	Occupation (O*NET)	Location	Start date	End date
Amazon.com	Shipping, Receiving, & Inventory Clerk	Dupont, WA	Jan. 1, 2017	March 1, 2018
Amazon Fulfillment Center		Tacoma, WA	Nov. 1, 2016	March 1, 2018
AT&T Authorized Retailer	Retail Salesperson	Lakewood, WA	May 1, 2016	Oct. 1, 2016
Costco Wholesale	Cashier	Tacoma, WA	Nov. 1, 2015	Jan. 1, 2016
Finishline	Retail Salesperson	Tacoma, WA	May 1, 2015	July 1, 2015
AllStarz Staffing		Auburn, WA	Oct. 1, 2014	Feb. 1, 2015
Provident Electric	Electrician	Covington, WA	Jan. 1, 2013	Jan. 1, 2015
	Cook		June 1, 2011	Jan. 1, 2013

Worker 2

Employer	Occupation (O*NET)	Location	Start date	End date
Amazon	Hand Packer & Packager	Lithia Springs, GA	Jan. 1, 2017	March 1, 2017
Norred & Associates, Inc.	First-Line Supervisor of Food Prep	Atlanta, GA	June 1, 2015	May 1, 2016
Staples	Cashier	Atlanta, GA	March 1, 2015	May 1, 2016
Au Bon Pain Cafe	Cashier	Atlanta, GA	Jan. 1, 2014	March 1, 2015

Worker 3

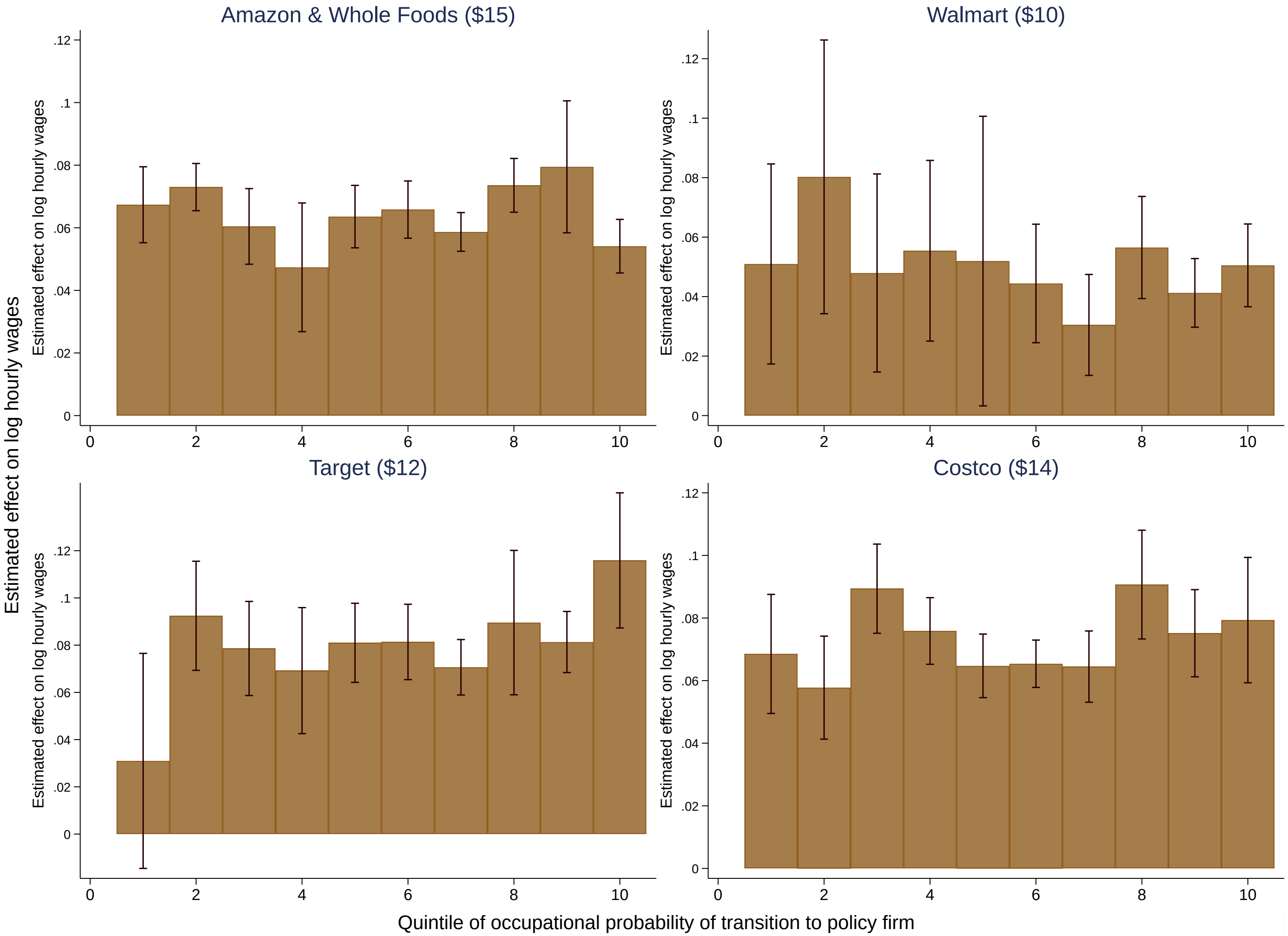
Employer	Occupation (O*NET)	Location	Start date	End date
Amazon Retail LLC	Hand Packer & Packager	Lenexa, KS	Nov. 1, 2017	March 1, 2018
Kids at Heart Child Care Center	Preschool Teacher	Kansas City, KS	Oct. 1, 2015	March 1, 2018
Janitorial	Customer Service Representative	Kansas City, MO	Oct. 1, 2015	Feb. 1, 2016
Walmart	Customer Service Representative	Kansas City, MO	April 1, 2012	Oct. 1, 2015
Neovia Logistics Services, LLC	Customer Service Representative	Kansas City, MO	Sept. 1, 2014	July 1, 2015
Dunkin' Brands	Customer Service Representative	Kansas City, MO	April 1, 2011	March 1, 2012

Table F2: Top 25 occupations advertised by Amazon

Laborers and Freight, Stock, and Material Movers, Hand	0.302
Stock Clerks and Order Fillers	0.227
Driver/Sales Workers	0.053
Order Clerks	0.048
Retail Salespersons	0.030
Butchers and Meat Cutters	0.022
Shipping, Receiving, and Traffic Clerks	0.013
Marketing Managers	0.010
First-Line Supervisors of Retail Sales Workers	0.009
Waiters and Waitresses	0.008
Combined Food Preparation and Serving Workers, Including Fast Food	0.008
Business Operations Specialists, All Other	0.008
Light Truck or Delivery Services Drivers	0.007
Database Administrators	0.007
Transportation, Storage, and Distribution Managers	0.007
Cashiers	0.007
Customer Service Representatives	0.006
Cooks, Restaurant	0.006
Bakers	0.005
Inspectors, Testers, Sorters, Samplers, and Weighers	0.005
Food Preparation Workers	0.005
Sales and Related Workers, All Other	0.005
Dishwashers	0.004
First-Line Supervisors of Food Preparation and Serving Workers	0.004
Heavy and Tractor-Trailer Truck Drivers	0.004

Notes: Amazon's share of vacancies in the top 25 occupations in which they advertised between 2014 and 2019. *Data sources*: Burning Glass Technologies online vacancy data.

Figure F5: No clear moderation by occupational likelihood of moving to policy firm



Notes: This figure plots the coefficients on the interaction between exposure to the policy firm's minimum wage and an indicator for the post-announcement period, for non-policy employers, where the dependent variable is log advertised hourly wage. Each bar indicates a separate regression where only postings in a given decile of each occupation's probability of transitioning to the policy firm are included. Exposure is defined as the fraction of postings in employer-by-CZ cells with wages below the policy firm's minimum wage in the year before treatment. Employer-by-CZ fixed effects and occupation-by-month fixed effects are included. The sample is restricted to non-policy employer postings with non-missing hourly wage data, employer name, location, and occupation. 95% confidence intervals shown. *Data sources*: Burning Glass Technologies online vacancy data.