Online Appendix

to

"Spillover effects from voluntary employer minimum wages"

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A Correction appendix

This appendix describes a problem we identified with the empirical approach in our working paper "Spillover Effects from Voluntary Employer Minimum Wages" (October 21, 2021). We discovered the issue after conducting a check requested by a referee in the review process at a journal. In this suggestion, the referee recommended we redo our placebo treatment date analysis in Figure 6 of the working paper by calculating exposure separately before each placebo treatment date instead of our baseline exposure measure that uses the date of the announced voluntary minimum wage.

When we ran this check, we found large effects at all of the placebo treatment dates. Furthermore, we saw no greater effect at the actual announcement date of October 2018. We then began conducting extensive additional analyses to check for mean reversion as a potential confounder of our wage effect estimates.

In the rest of this memo, we describe each of the checks and their results in greater detail, but the conclusion we have drawn is that after properly accounting for mean reversion we do not see any evidence of spillover effects using our exposure-based estimation approach for Amazon or, we believe, other retailers examined in the paper.

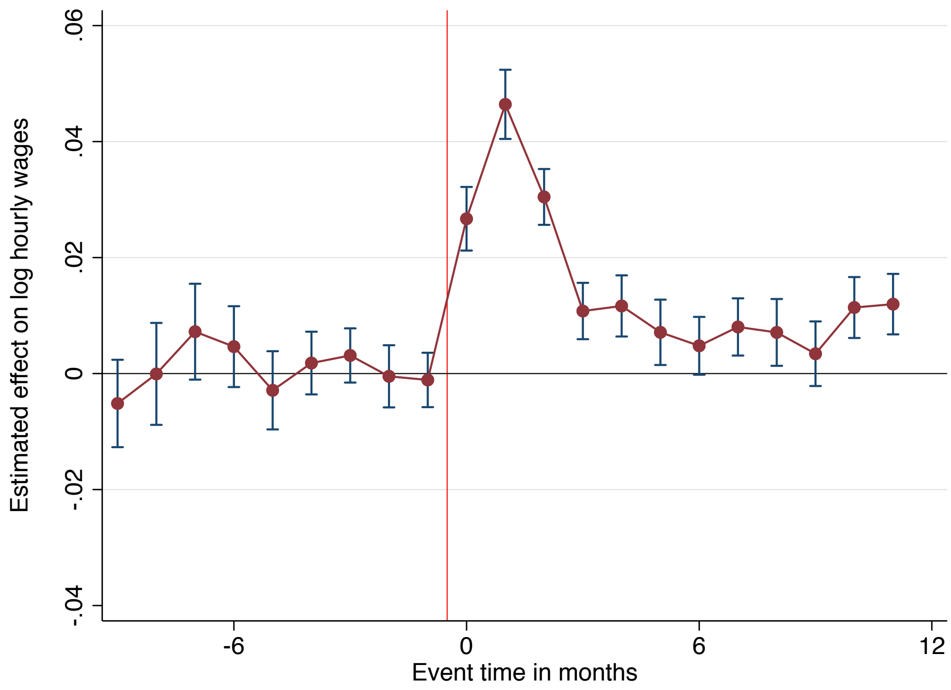
A.1 Problem with exposure-based identification of voluntary firm minimum wage effects

In our baseline analysis, we calculate exposure to large retailer voluntary minimum wage announcements as the fraction of ads in job cells (6-digit occupation by employer by commuting zone in BGT; 4-digit occupation by commuting zone cells in the CPS) that are below the large retailer's announced minimum wage in the year before the announcement. In doing so, we follow a standard approach in the minimum wage literature leveraging variation in exposure to minimum wage increases using lagged measures of the wage distribution across individual workers/jobs or groups of workers/jobs, dating back to at least Card (1992) and used in numerous papers since.

A key concern with this approach is that workers or jobs with low wages in the pre-period may experience faster growth than higher wage workers/jobs (Ashenfelter and Card, 1982).

To check that our estimated treatment effects did not reflect mean reversion we reran our analysis over placebo treatment periods and plotted the coefficient on our baseline exposure measure interacted with placebo post-treatment months. We reproduce this figure, Figure 6 from the paper, as Figure A1 below.

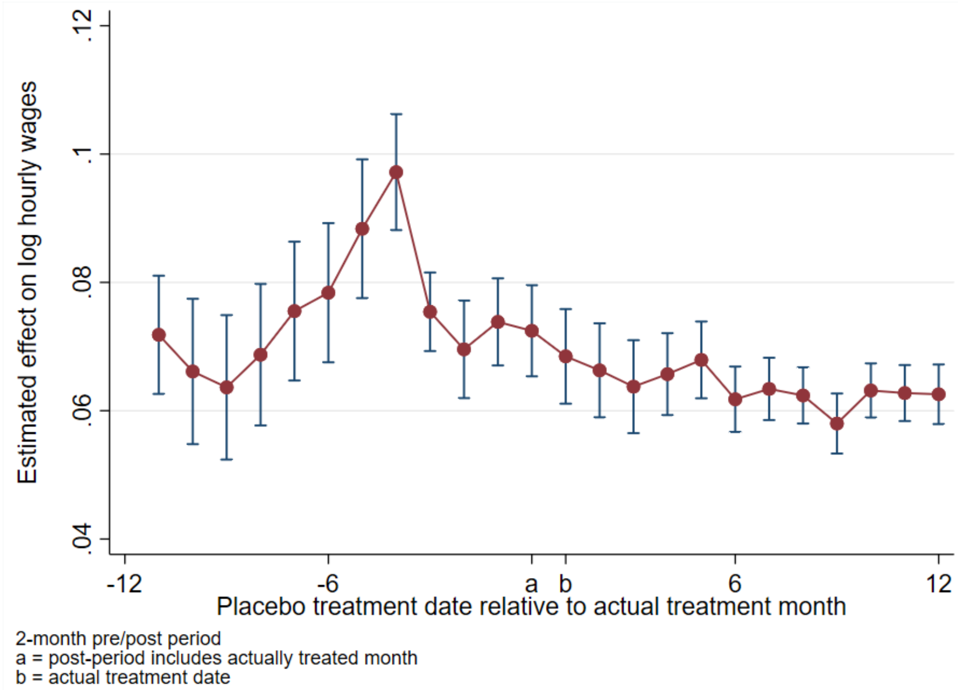
Figure A1: Null effects of Amazon's $15 at placebo treatment dates



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.

Following a referee comment, we recalculated exposure using the placebo pre-treatment period as opposed to using our baseline exposure measure, which is calculated over the 12 months prior to treatment. Below, we show that this approach yields statistically significant and large treatment effects even at the placebo dates (Figure A2). Furthermore, the estimated effect at the actual announcement date is no larger than the effect at the placebo dates. There is some variation in the magnitude of the placebo effect months -10 and -5 prior to treatment, but no spike in the treatment effect at the actual date of treatment (labeled "b" in Figure A2).

Figure A2: Effects of Amazon's $15 at placebo treatment dates, exposure measured in placebo pre-period



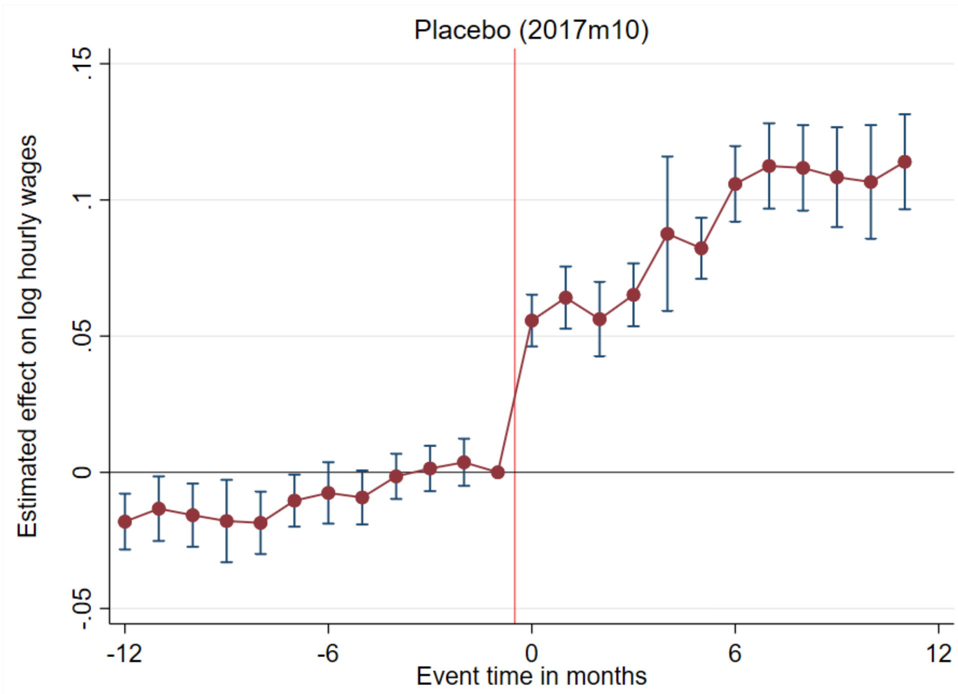
Notes: This figure plots the regression coefficients on the interaction between job-level exposure to Amazon's policy for non-Amazon employers and an indicator for post-treatment for placebo treatment dates, using a 4-month observation window. Coefficients are indexed by the last month of the observation 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 over the two months of the placebo pre-period. 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.

One potential issue with this approach is that exposure calculated over such a short window (two months) is much more likely to pick up transitorily low wage jobs as opposed to permanently low wage jobs. Nevertheless, the failure of this particular check led us to conduct additional checks. We conducted three additional checks.

First, we estimated full event studies shifting announcement dates by one month and recalculating exposure over the new 12-month period each time. We also examine the impact relative to a placebo treatment date one year before the actual announcement. Second, we estimate the expected mean reversion effect using simulated data and the approximate observed standard deviation in wages within and across job cells in the BGT data. Note that in these simulated data, there is no real wage growth. Third, we calculate exposure over a two-year and even three-year period to see if this mitigates the issue of picking up transitory rather than permanent variation in wages across job cells.

Placebo treatments with exposure calculated over 12 months before placebo date Figure A3 below shows that shifting the announcement date to a placebo date one year prior and recalculating exposure over the 12-month window prior to this placebo date results in very similar estimated spillover effects as in our baseline design.

Figure A3: Amazon placebo announcement date: October 2017

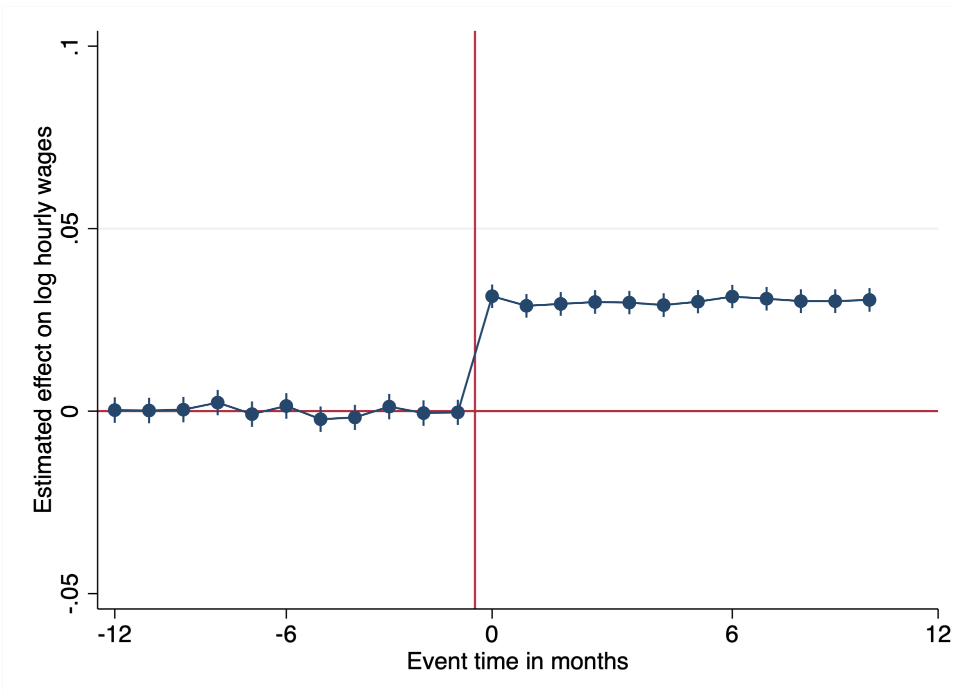


Notes: This figure plots results from a placebo regression that set Amazon's minimum wage announcement one year prior, in October 2017, instead of October 2018. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below $15 over the 12 months of the placebo pre-period. 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.

Mean reversion effect in simulated data with no wage growth In this section we show that we can generate a mean reversion effect similar to our estimated spillovers effect using simulated data with random, rather than systematic, variation in wages within and across groups over time. Note the purpose of this exercise is to demonstrate that data with no systematic wage growth can generate large jumps in the wage. We are not matching the complete data generating process, therefore we do not expect these simulated effects to match our estimated effects exactly.

We simulate data that matches certain key characteristics of our BGT data: the number of employer $\times$ occupation $\times$ CZ groups ($\sim$270,000), the across group mean in log wages (2.7) and standard deviation (0.32) and the average standard deviation within group (0.10).[34] The average group in our BGT is observed 5 times in the 24 month period—in other words, we have an unbalanced panel of job cells. For this simulation we also draw 2 pre-period and 3 post-period observations for each group to match the fact that the density of postings increases over the observation period, thus jobs are likely to have more observations in the post-period than pre-period. The result is shown in Figure A4. Even without full matching the data generating process, we simulate a post-period wage increase of 3 log points or 60% of the initial effect in the BGT sample – reproduced below as Figure A5 – and we match the sharp jump at exactly the post-treatment period.

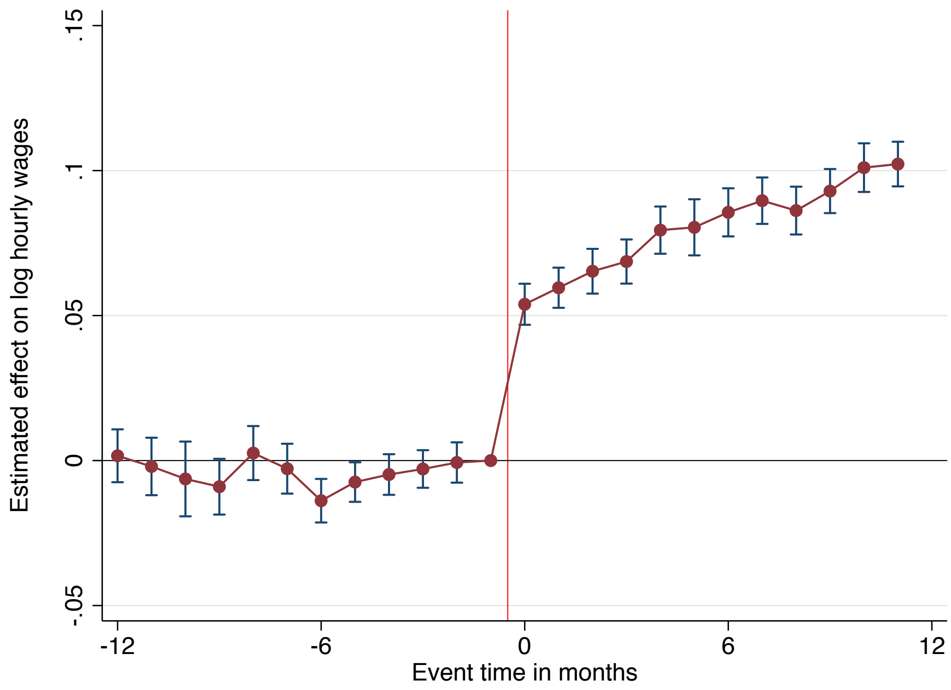
Figure A4: Expected mean reversion effect using simulated data



Notes: This figure plots results from an event-study using simulated data calibrated to have the within-group and across group deviation similar to what we observe in our BGT sample for the Amazon/Whole Foods experiment.

[34]Note that the within-group standard deviation in the data is not equal across groups. For the purposes of this exercise, we use the average within-group standard deviation to calibrate the simulation.

Figure A5: Spillovers in advertised wages from Amazon's $15 MW, 2018



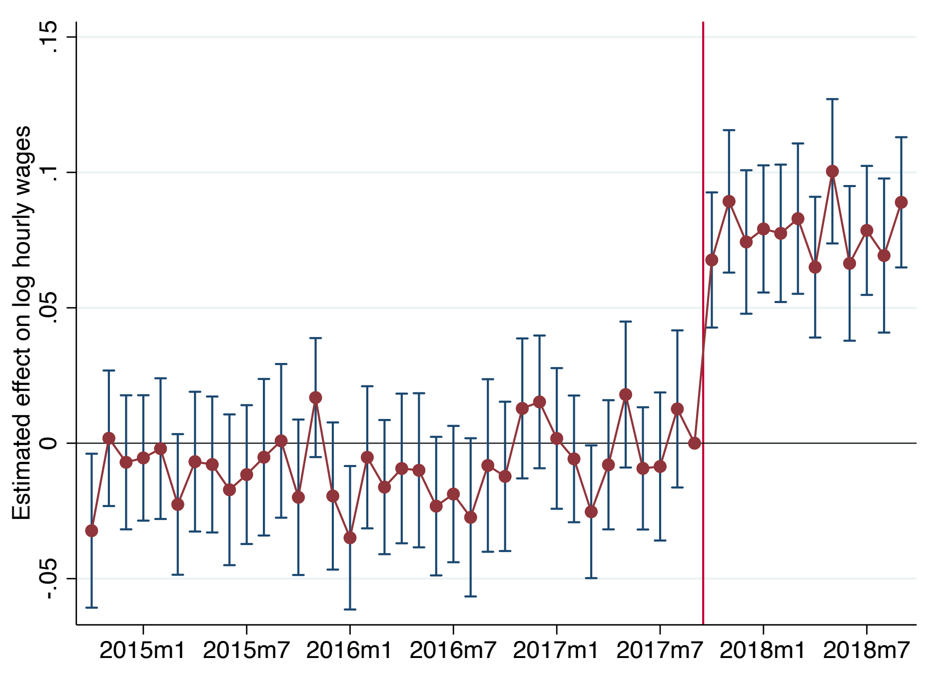
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ 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.

Longer pre-period We examine whether a three-year pre-period allows us to overcome transitory variation in the data by conducting a placebo analysis where we measure exposure over a three-year placebo pre-period for the Amazon/Whole Foods announcement (from October 2014 to September 2017) with a placebo announcement date in October 2017. The results are shown in Figure A6 below. Here, too, we estimate large effects at the placebo treatment date, even with a three-year pre-period exposure measure.

It's notable that mean reversion doesn't seem to manifest itself as a linear or even non-linear trend, but as a distinct spike/jump.

Thus, we are concerned that our wage results are picking up reversion to the mean of transitorily low wage jobs, as opposed to permanently low wage jobs experiencing wage growth post-treatment. Additionally, this analysis suggests that measuring exposure over a 12-, 24- or even 36-month period is insufficient for picking up permanently lower as opposed to transitorily lower wage jobs.

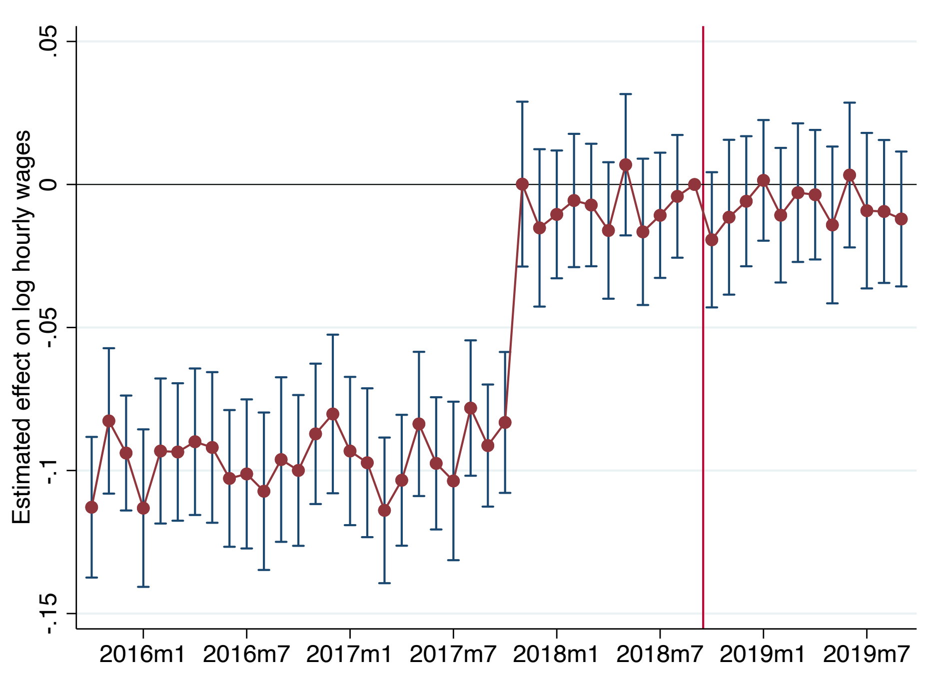
Figure A6: Amazon spillovers, 36-month pre-period, placebo announcement date one year prior



Notes: This figure plots the placebo regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon-industry workers interacted with month fixed effects, where the dependent variable is log hourly wage. A three-year pre-period is shown. Exposure is defined as the fraction of non-Amazon postings in each occupation-CZ cell with wages below $15 in the three years prior to the placebo treatment date. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The sample is restricted to non-Amazon-industry workers in our CPS sample of individuals aged 16-65. 95% confidence intervals shown. *Data sources*: CPS.

Longer pre-period with gap between exposure measure and treatment date The proximity of the period over which exposure is calculated to the placebo or actual treatment period is likely the explanation behind the sharp jump at exactly the (placebo or actual) treatment date. In this section, we calculate exposure over the first two years of a three-year pre-period and run the event history around the actual treatment date. The results are reported in Figure A7. Instead of a jump at the actual treatment date, the jump occurs one year earlier, exactly once the period over which exposure is calculated has ended. This is further evidence that this design will generate a mean reversion effect related to the period over which exposure is calculated as opposed to picking up effects of Amazon's voluntary minimum wage policy.

Figure A7: Amazon spillovers, 36-month pre-period, exposure calculated over first two years of pre-period



Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon-industry workers interacted with month fixed effects, where the dependent variable is log hourly wage. A three-year pre-period is shown. Exposure is defined over the first two years of the pre-period. Postings appearing in the year prior to treatment are not used to calculate exposure. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The sample is restricted to non-Amazon-industry workers in our CPS sample of individuals aged 16-65. 95% confidence intervals shown. *Data sources*: CPS.

A.2 Replication of regional design in Dustmann et al. (2022)

In this section, we replicate the regional design of Dustmann et al. (2022) and show that our method withstands the detrending adjustment implemented in this section of Dustmann et al. (2022).

In Dustmann et al.'s (2022) analysis of the reallocation effects of Germany's 2015 national minimum wage, the authors examine the labor market effects of the policy using the standard gap design. Exposure to the minimum wage ("GAP") for each district r and pre-policy year t is calculated as follows:

$$\text{GAP}_{rt} = \frac{\sum_{i \in r} h_{it} \max\{0, MW - w_{it}\}}{\sum_{i \in r} h_{it} w_{it}}$$

where h_{it} is the individual i's weekly hours in year t, w_{it} is their hourly wage, and

MW is the incoming minimum wage.

The authors then average the gap measure over three years in the pre-period:

$$\overline{\text{GAP}}_r = \frac{\sum_{i \in r} h_{it} \max\{0, MW - w_{it}\}}{\sum_{i \in r} h_{it} w_{it}}$$

The event-study regression is:

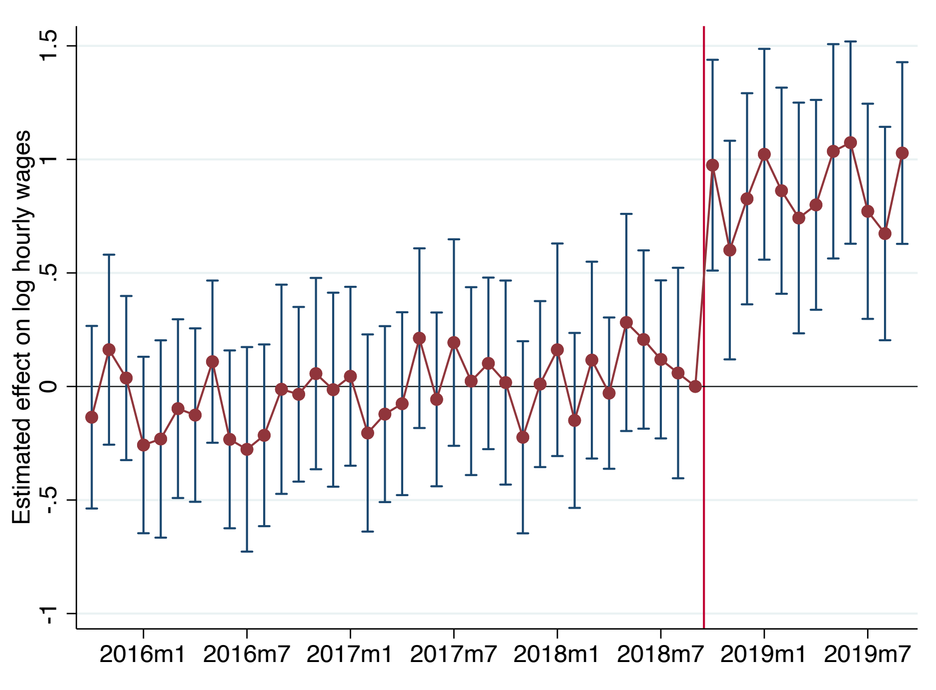
$$Y_{rt} = \alpha_r + \zeta_t + \sum_{\tau=2011, \tau \neq 2014}^{2016} \gamma_t \overline{\text{GAP}}_r + \epsilon_{rt} \tag{6}$$

The authors perform the following checks on the analysis in the main text:

1. Use the estimates of γ_t from the pre-policy years to fit a linear time trend.
2. Plot deviations between estimates of γ_t and the linear time trend for the postpolicy years.

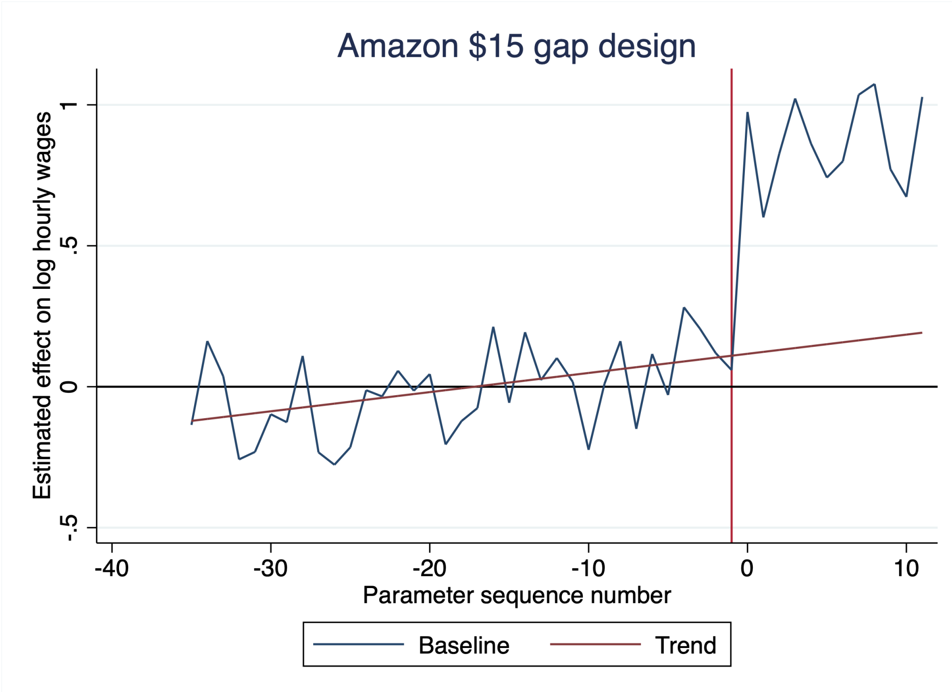
In what follows, we implement the gap design to analyze Amazon's minimum wage and show the result is robust to detrending from a trend line estimated over the three years prior to the policy. These results are reported in Figures A8, A9, and A10 below. Thus, detrending appears insufficient for controlling for mean reversion when there is sufficient transitory variation in wages within groups.

Figure A8: Amazon spillovers, using gap design from Dustmann et al. (QJE 2022)



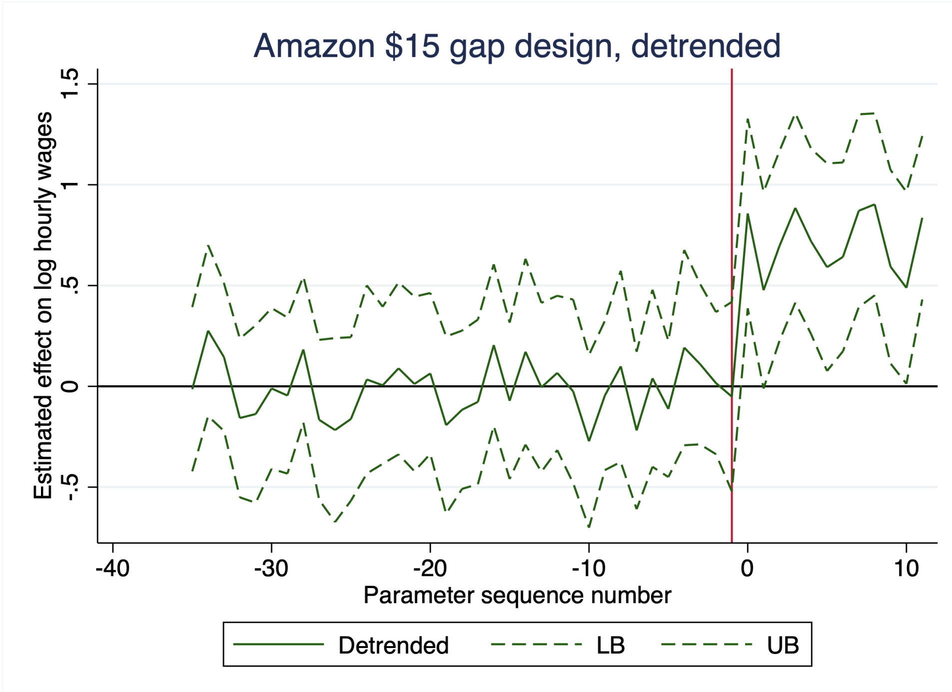
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon-industry workers interacted with month fixed effects, where the dependent variable is log hourly wage. A three-year pre-period is shown. Exposure is defined as the weighted average gap between workers' pay and Amazon's minimum wage of $15 in each occupation-CZ cell. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The sample is restricted to non-Amazon-industry workers in our CPS sample of individuals aged 16-65. 95% confidence intervals shown. *Data sources*: CPS.

Figure A9: Amazon spillovers, gap design and linear trend



Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon-industry workers interacted with month fixed effects, where the dependent variable is log hourly wage. A three-year pre-period is shown as well as a linear trend fitted on the pre-period coefficients only. Exposure is defined as the weighted average gap between workers' pay and Amazon's minimum wage of $15 in each occupation-CZ cell. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The sample is restricted to non-Amazon-industry workers in our CPS sample of individuals aged 16-65. 95% confidence intervals shown. *Data sources*: CPS.

Figure A10: Amazon spillovers, gap design detrended



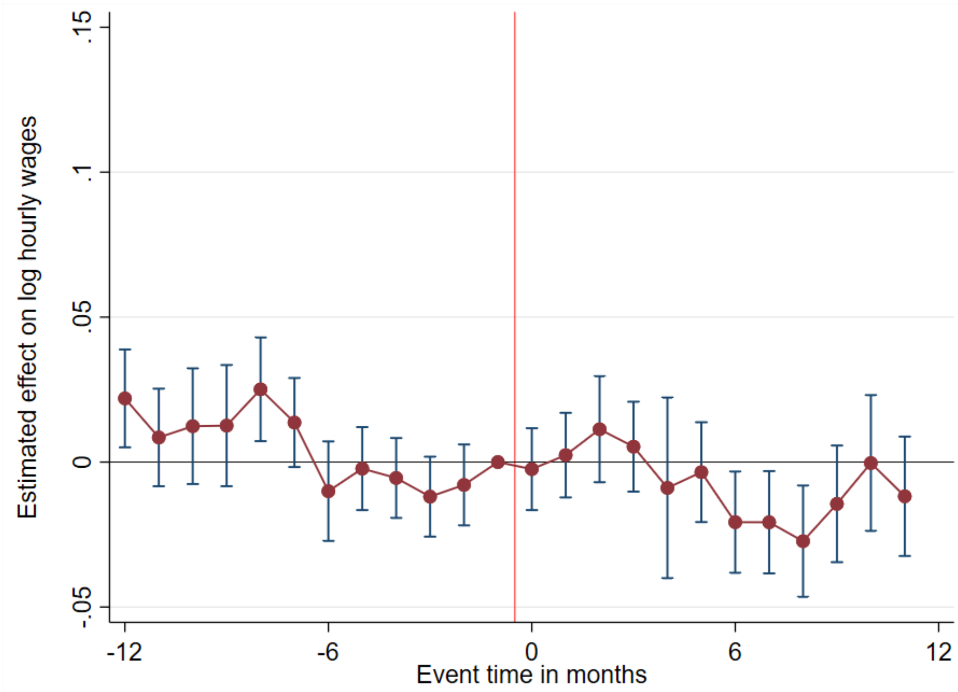
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon-industry workers interacted with month fixed effects, where the dependent variable is log hourly wage. A three-year pre-period is shown. Estimates are detrended using the linear trend fitted through pre-period estimates. Exposure is defined as the weighted average gap between workers' pay and Amazon's minimum wage of $15 in each occupation-CZ cell. Occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The sample is restricted to non-Amazon-industry workers in our CPS sample of individuals aged 16-65. 95% confidence intervals shown. *Data sources*: CPS.

A.3 Adjusted estimates using a triple differences-in-differences approach

To account for the mean reversion effect that we believe our baseline approach is picking up, we calculated a triple difference taking the difference between the estimates from the actual treatment year and those from a placebo event study one year earlier. This approach is similar to the first analysis of Germany's national minimum wage in Dustmann et al. (2022) in which the authors compare wage growth over pre-policy years to wage growth around the policy year for workers designated lower vs. higher wage in the pre-period.

Our triple differences approach provides an estimate of the true treatment effect of Amazon's policy on non-Amazon firms under the assumption that mean reversion is constant across years and can therefore be differenced out using data from a prior year. Below we show the event study version of this triple differences approach. Using this approach, trends remain relatively flat in the pre-period while the post-period estimates suggest no significant spillover effect from Amazon's announcement.

Figure A11: Amazon placebo announcement date: October 2017



Notes: This figure plots the coefficients on a triple interaction between time, exposure over the 12 months prior to placebo or treatment month, and an indicator for falling in the actual treatment period. Exposure is defined as the fraction of non-Amazon industry workers in each occupation-CZ cell with wages below \$15 in the year before the placebo or actual treatment date. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ cell with wages below \$15 over the 12 months of the placebo or actual treatment pre-period. Employer-by-occupation-by-CZ, month-by-occupation, and month-by-CZ fixed effects are included. The model is fully interacted with a dummy for being in the actual treatment observation window as opposed to the placebo observation window. 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.

We compare the pooled difference-in-differences results for the Amazon/Whole Foods experiment across these different strategies in Table A1 below. Column 1 presents the placebo difference-in-differences regression (treatment assigned 12 months earlier); column 2 presents results for the actual treatment period. Column 3 stacks the placebo and treatment period datasets and interacts all covariates with a dummy for the treated period. Column 4 presents the coefficient on the triple interaction of exposure $\times$ post $\times$ an indicator for being in the actual treatment period.

The result is small and statistically significantly negative, meaning we can reject a positive effect of Amazon's announcement on non-Amazon firms' wages under the assumption that the placebo effect captures the time-consistent mean reversion effect.

Table A1: Amazon/Whole Foods triple difference results

	Placebo	Treatment	Stacked	Triple-Diff
Diff-in-diff	0.100***	0.086***	0.091***	0.100***
	(0.001)	(0.001)	(0.001)	(0.001)
Triple-diff				-0.014***
				(0.002)
N	584,474	1,010,831	1,595,305	1,595,305

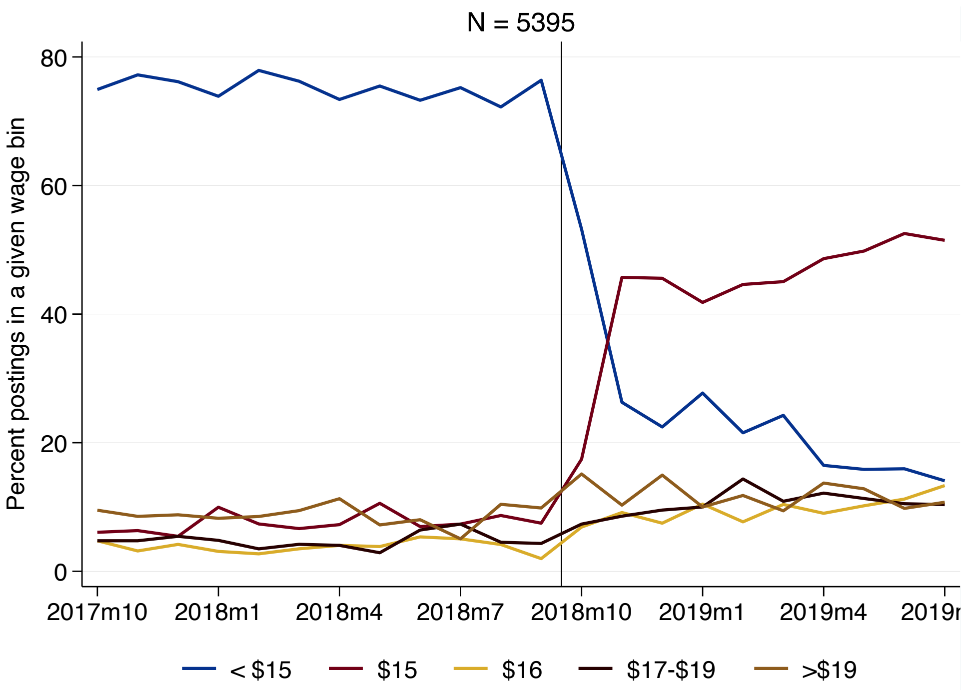
Notes: This table reports the coefficients from a placebo analysis of the treatment effect one year prior to Amazon's announcement, a stacked analysis of the treatment effect from the placebo and actual treatment period, and a triple difference that differences out the placebo effect from the baseline treatment effect. 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.

A.4 Additional evidence on voluntary firm minimum wage increases, from Glassdoor

Using evidence from Glassdoor salary reports, we confirm that Amazon workers report substantially higher after the company's voluntary minimum wage announcement. Figure E9 below shows changes in the distribution of hourly wages reported received by Amazon workers on Glassdoor. Nearly 80% of workers filling out salary reports for Amazon reported being paid less than \$15 an hour prior to Amazon's \$15 minimum wage announcement. Immediately after the announcement, the percentage of workers reporting a sub-\$15 minimum wage plummets while the fraction reporting exactly \$15 rises dramatically from around 10% to over 50% by mid-2019. Consistent with our evidence on changes in Amazon's advertised wages (see Figure 2), this evidence affirms that Amazon instituted a policy with significant consequences for their own hourly wage distribution.

We are continuing to examine the impacts of these policies as well as the apparent absence of spillovers. The ability of major firms like Amazon to set and maintain sudden, large increases in their minimum wages implies the presence of labor market power. But the seeming absence of response by other firms in their local labor markets raises important questions regarding local and national wage determination dynamics. We will be examining these results and their implications in the next version of this paper.

Figure A12: Percentage of Amazon Glassdoor salary reports below or above $15



Notes: Percentage of Amazon salary reports at wage bins below, at, or above $15. Whole Foods was acquired by Amazon in August 2017 and is included in the sample. *Data sources*: Glassdoor salary reports.

B 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.[35] 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

[35] 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).[36]

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).

[36] 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 B2: 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

C 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.

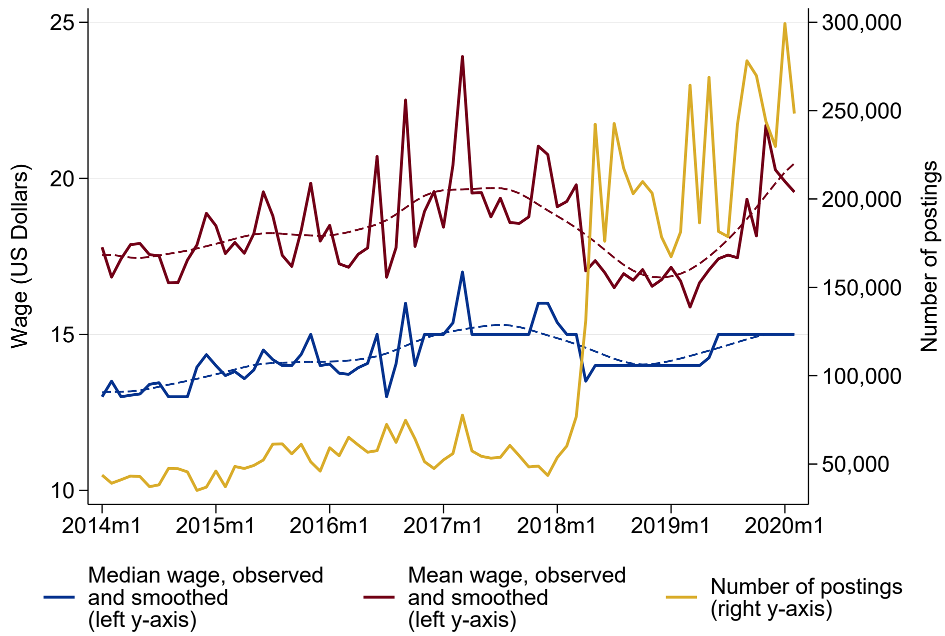
The BGT analyses use postings from February 2014 through February 2020. The BGT data include 154,176,423 postings over this period. Of these, 20.4% have non-missing wage data, 73.8% have non-missing employer names. 20,240,413 postings (13.1%) have both non-missing wages and employer names. We further 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 C1 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 C1: 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.

D 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.[37] 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.

[37]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 D1 provides the number of policy firm CZs identifiable in the CPS ORG data for each policy.

Table D1: 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

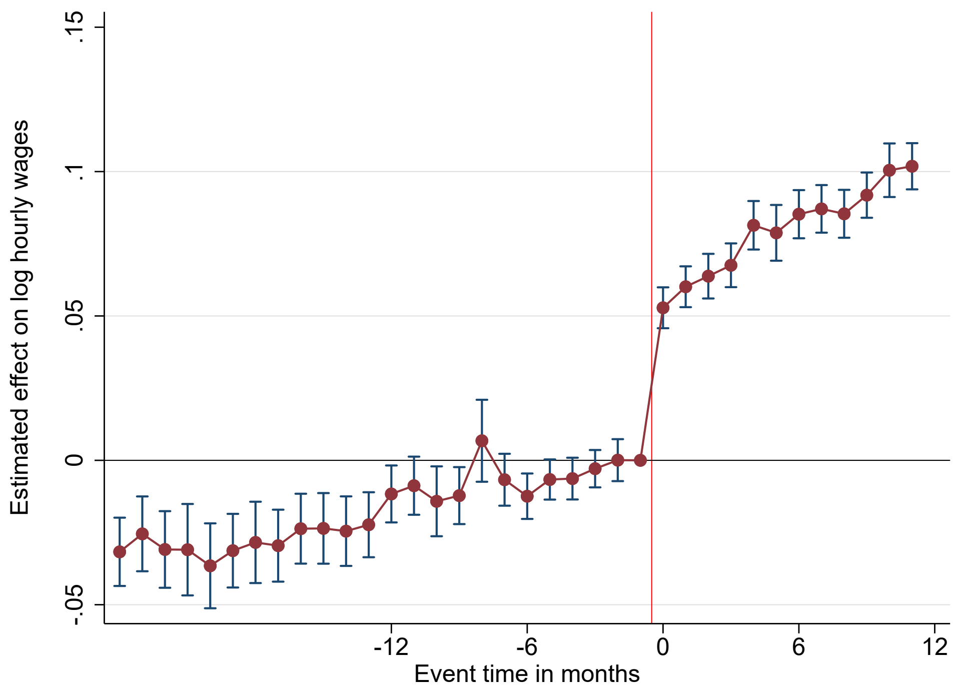
E Additional robustness checks

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

E.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 E1 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 E1: Amazon spillovers, 24-month pre-period



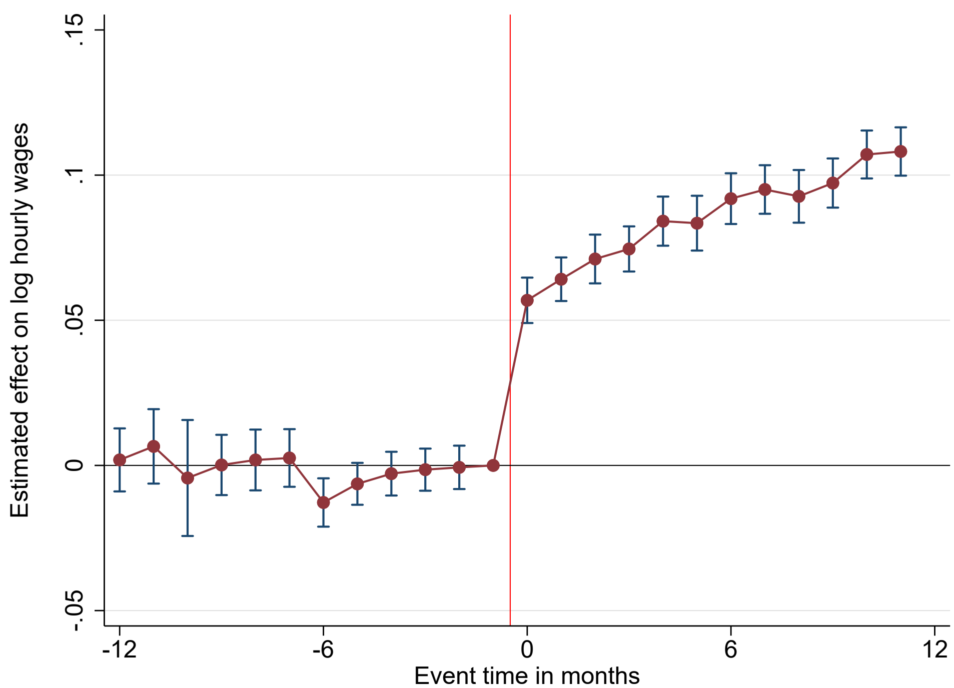
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. A two-year pre-period is shown. Exposure is defined as the fraction of non-Amazon 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.

E.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 E2 depicts the results. The inclusion of these controls does not affect our results.

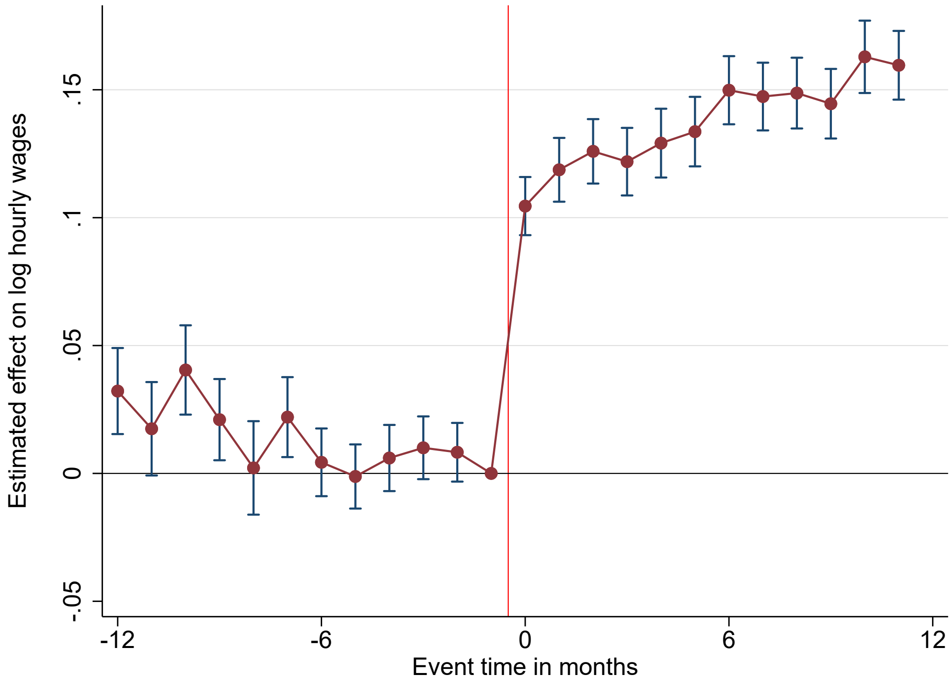
In Figure E3, we add employer-by-month fixed effects to the specification. The inclusion of these controls absorbs any employer-level changes in policy, such as the decision to post wages on job ads, or shocks to specific kinds of employers. In this specification, we leverage within-employer variation 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 E2: 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 E3: Amazon spillovers, with occupation-by-CZ-by-month, employer-by-month fixed effects



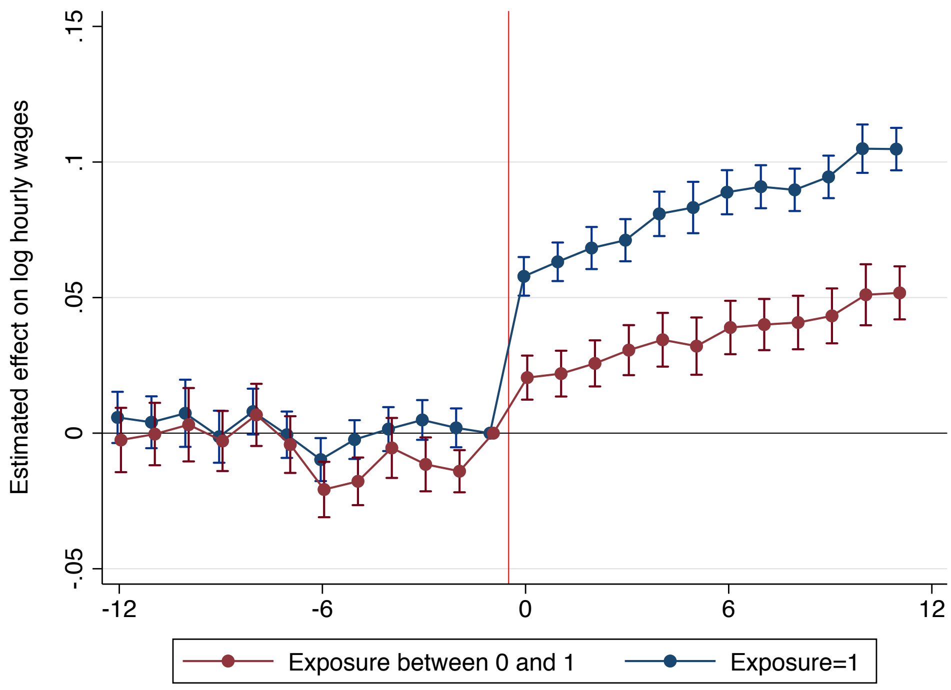
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-Amazon postings in each occupation-employer-CZ 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.

E.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 E4 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.

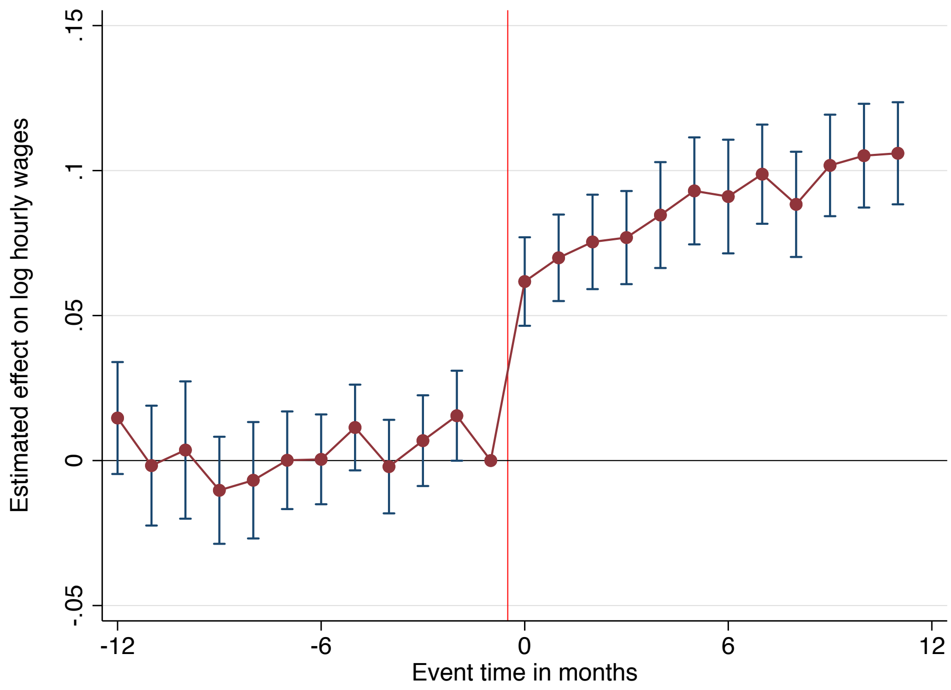
Finally, Figure E5 restricts the sample to jobs with at least some fraction of postings below \$15 and plots the effect of an indicator for fully exposed interacted with month, where the omitted category is the partially exposed group. Fully exposed jobs experience a large and immediate increase in wages 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 E4: 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 E5: Amazon spillovers, binned exposure: partially vs. fully exposed



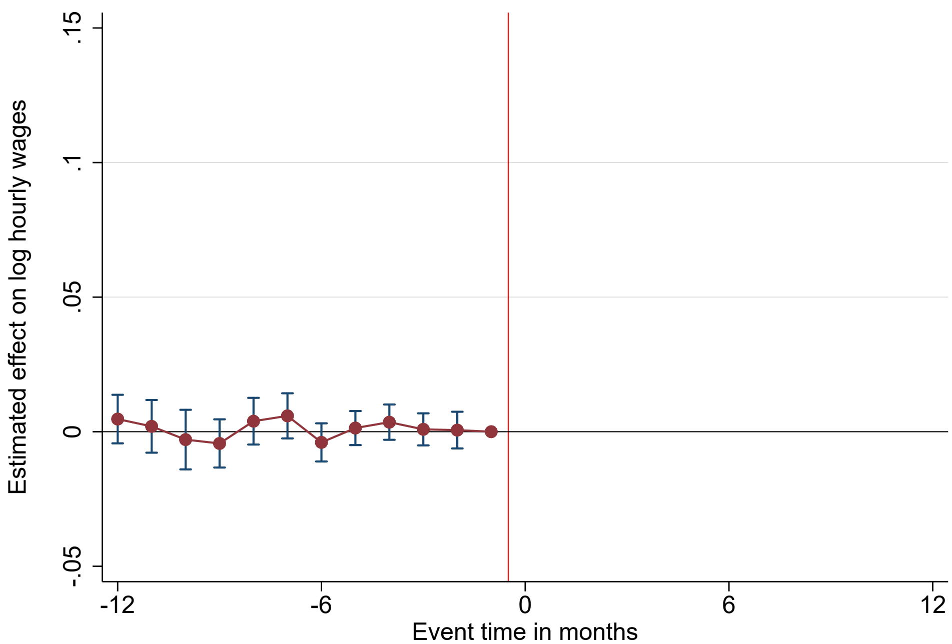
Notes: This figure plots the regression coefficients on job-level exposure group to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. The two exposure groups are jobs with 100% of postings offering below $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.

E.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 E6, 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.

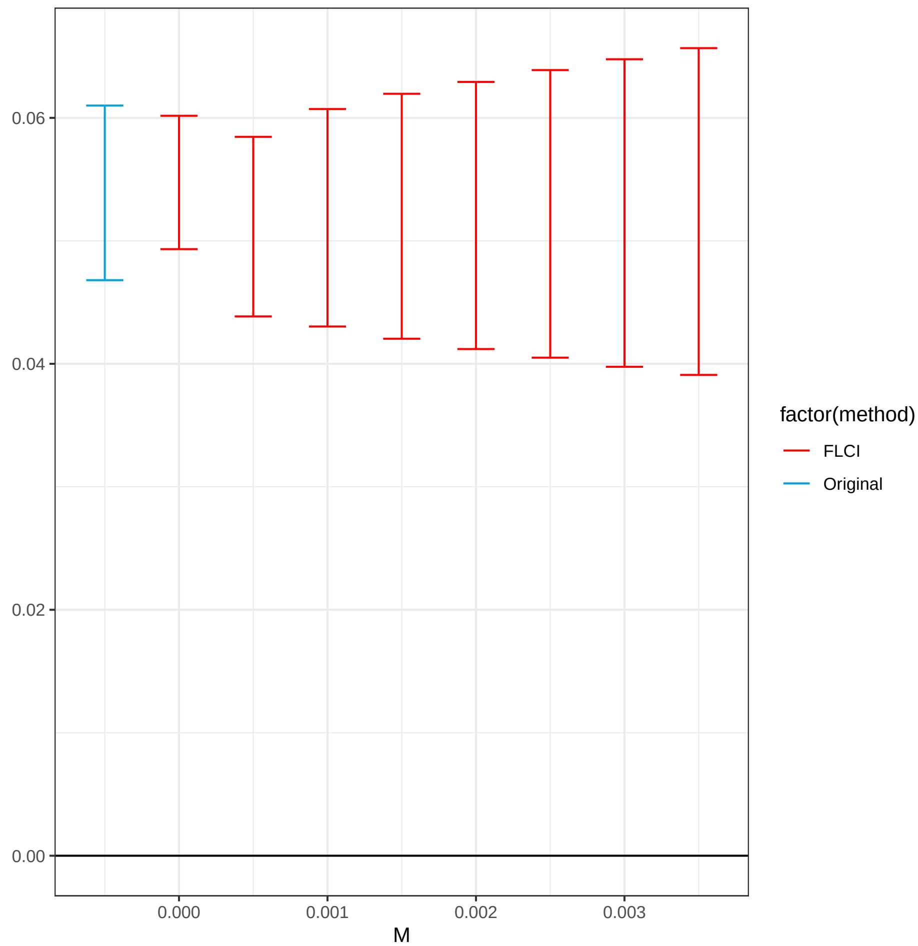
To further explore potential bias from a pre-trend in wages, we follow Rambachan and Roth (2021) and construct alternative dynamic confidence intervals with varying assumptions on the trends prior to the announcement. Allowing for linear deviations from parallel trends up to a standard deviation of the coefficient β_0, the coefficient on fraction affected interacted with the first month of the announcement, results in the confidence sets depicted in Figure E7. Allowing for a positive bias in the pre-trend results in the confidence sets depicted in Figure E8. 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 E6: Separately estimated pre-trends in spillovers for Amazon minimum wage



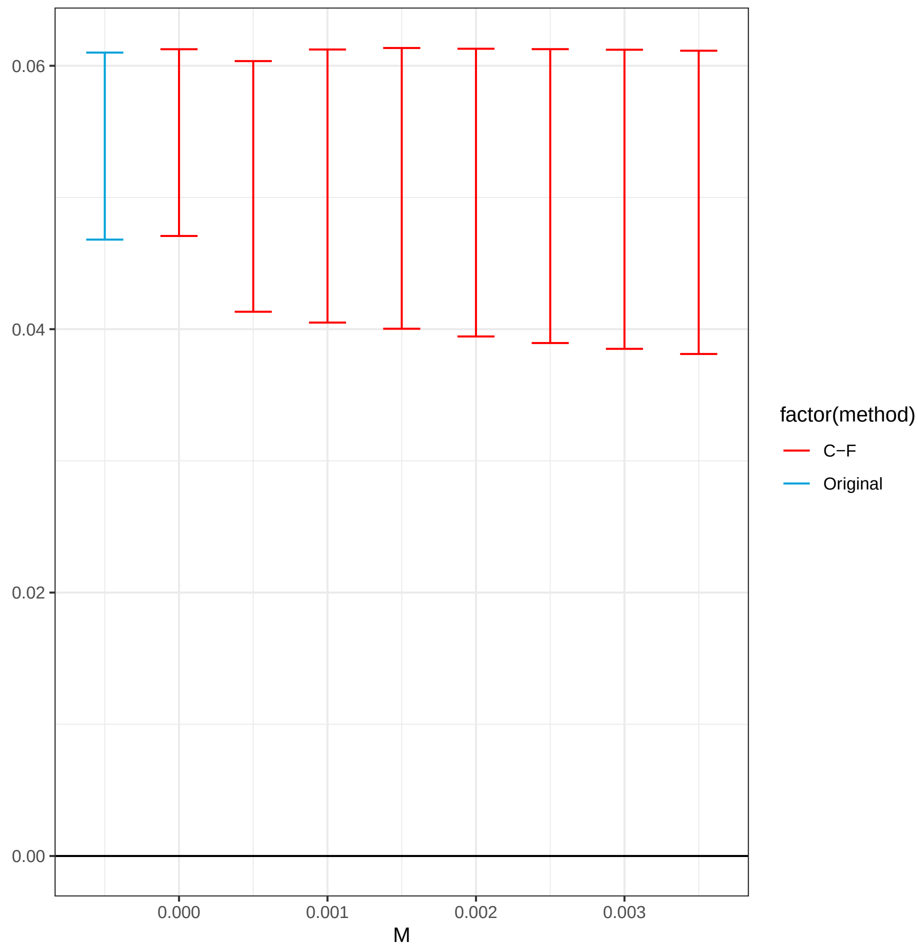
Notes: This figure plots the coefficients β_k from equation 2 for pre-announcement months only, following the proposed tests for absence of pre-trends in (Borusyak et al., 2021; Borusyak and Schönberg, 2021). The p-value of the F-test of joint significance is 0.04 , however the pre-announcement estimates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

Figure E7: 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 E8: Honest pre-trends in spillovers from Amazon's minimum wage (positive bias)

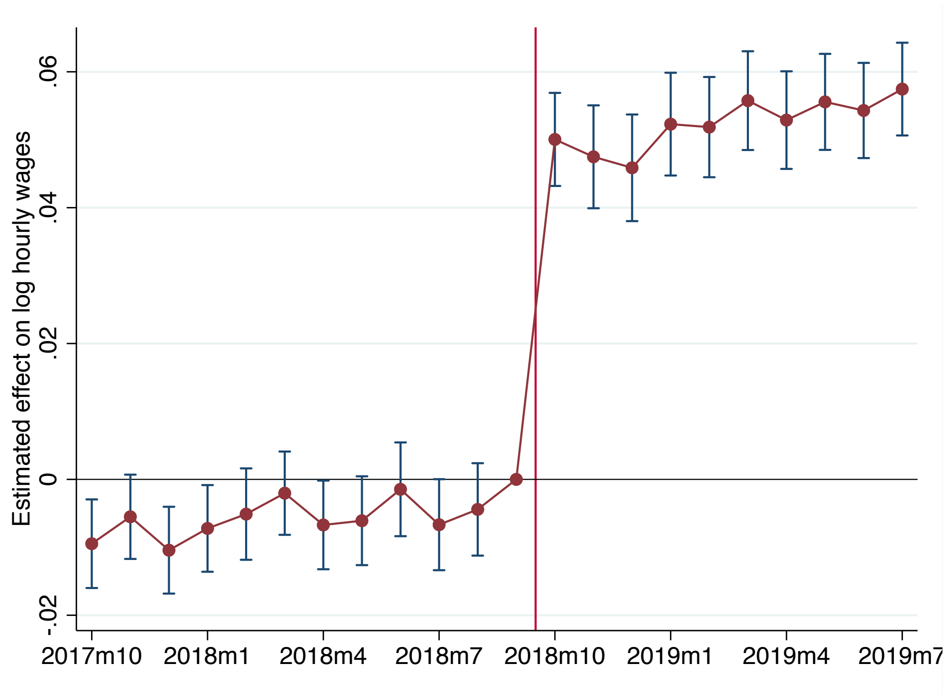


Notes: This figure plots the coefficients β_k from equation 2 for the first post-treatment month only and plots alternative confidence intervals that allow for deviations from parallel trends in the pre-period, following the honest parallel trends estimation procedure of Rambachan and Roth (2021), and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

E.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 E9 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 E9: 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.

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

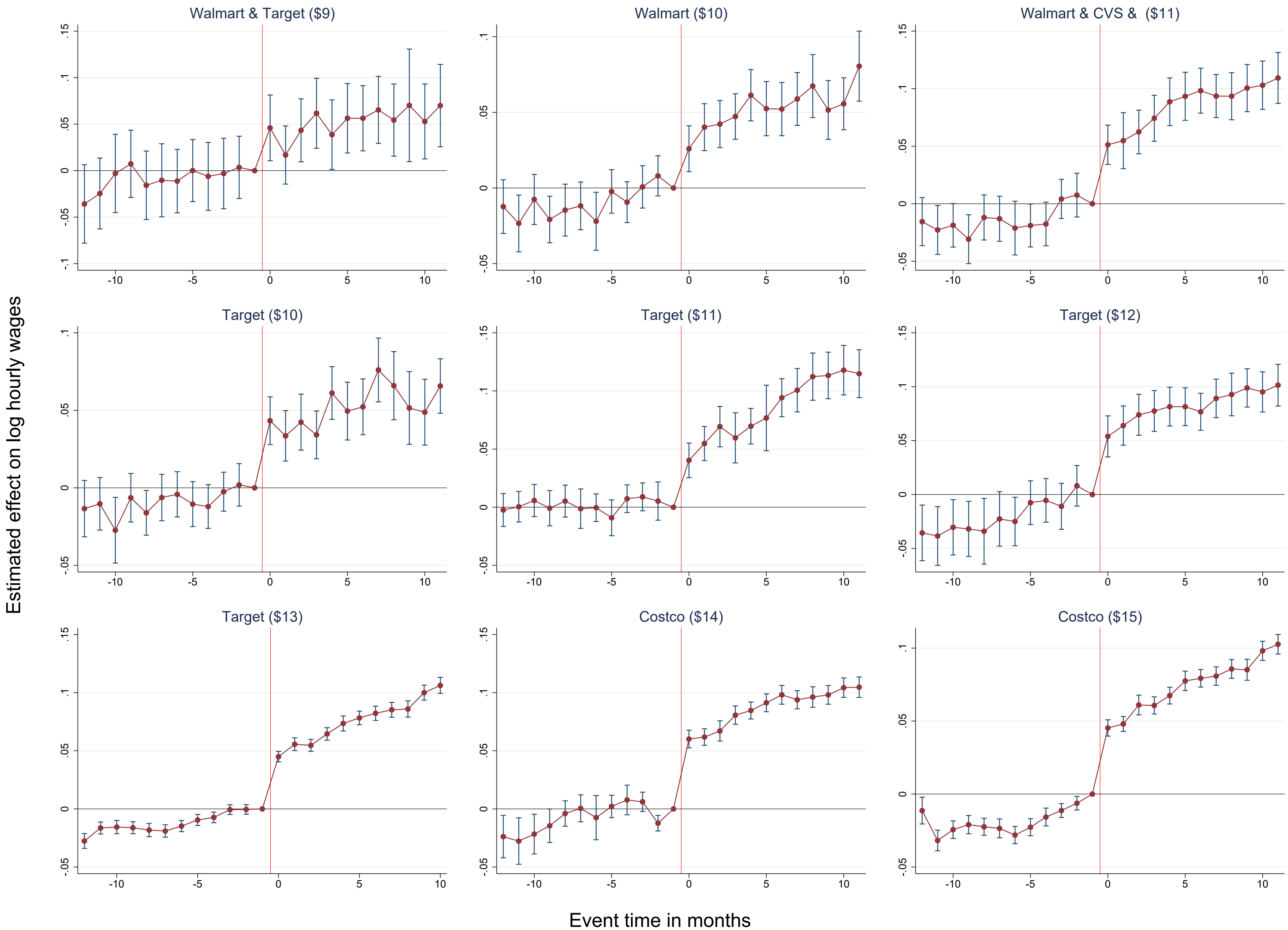
The following section replicates our key robustness checks for the nine other minimum wage announcements we study by Walmart, Target, and Costco. Figures E10 and E11 report robustness to the inclusion of occupation-by-CZ-by-month fixed effects and employer-by-month fixed effects. Figure E12 documents similar increases in worker reported wages at non-policy firms in the wake of policy firm minimum wage announcements. Figure E13 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 E14, 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 E15 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.

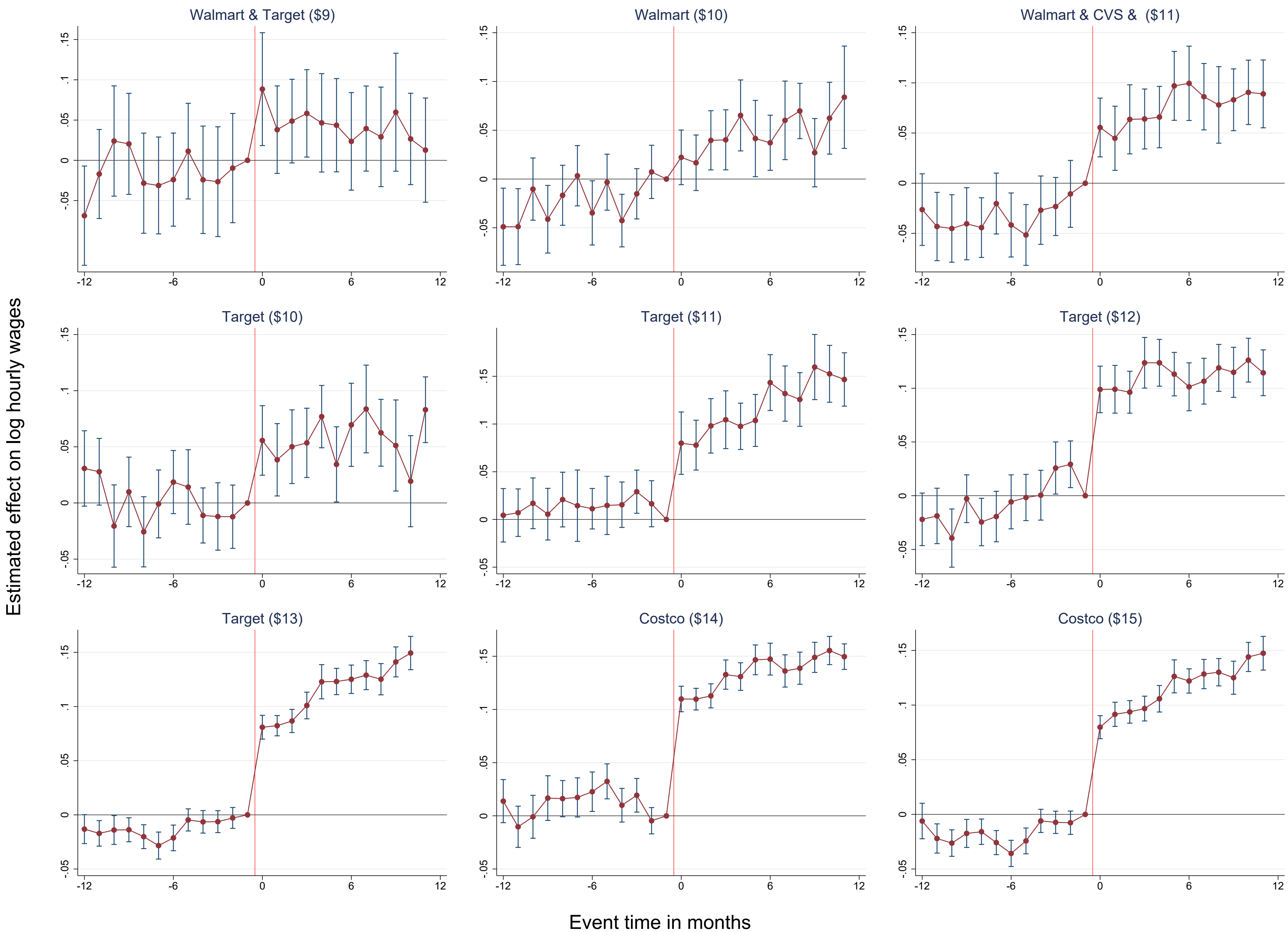
To assess the bias from pre-trend violations, we once again calculate alternative confidence sets following Rambachan and Roth (2021) for the remaining 9 employer minimum wage announcements in Figure E16. In no case do we observe confidence intervals crossing zero for deviations in parallel trends up 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 E17).

Figure E10: Walmart, Target, and Costco MW spillovers: robust to occupation-by-CZ-by-month fixed effects



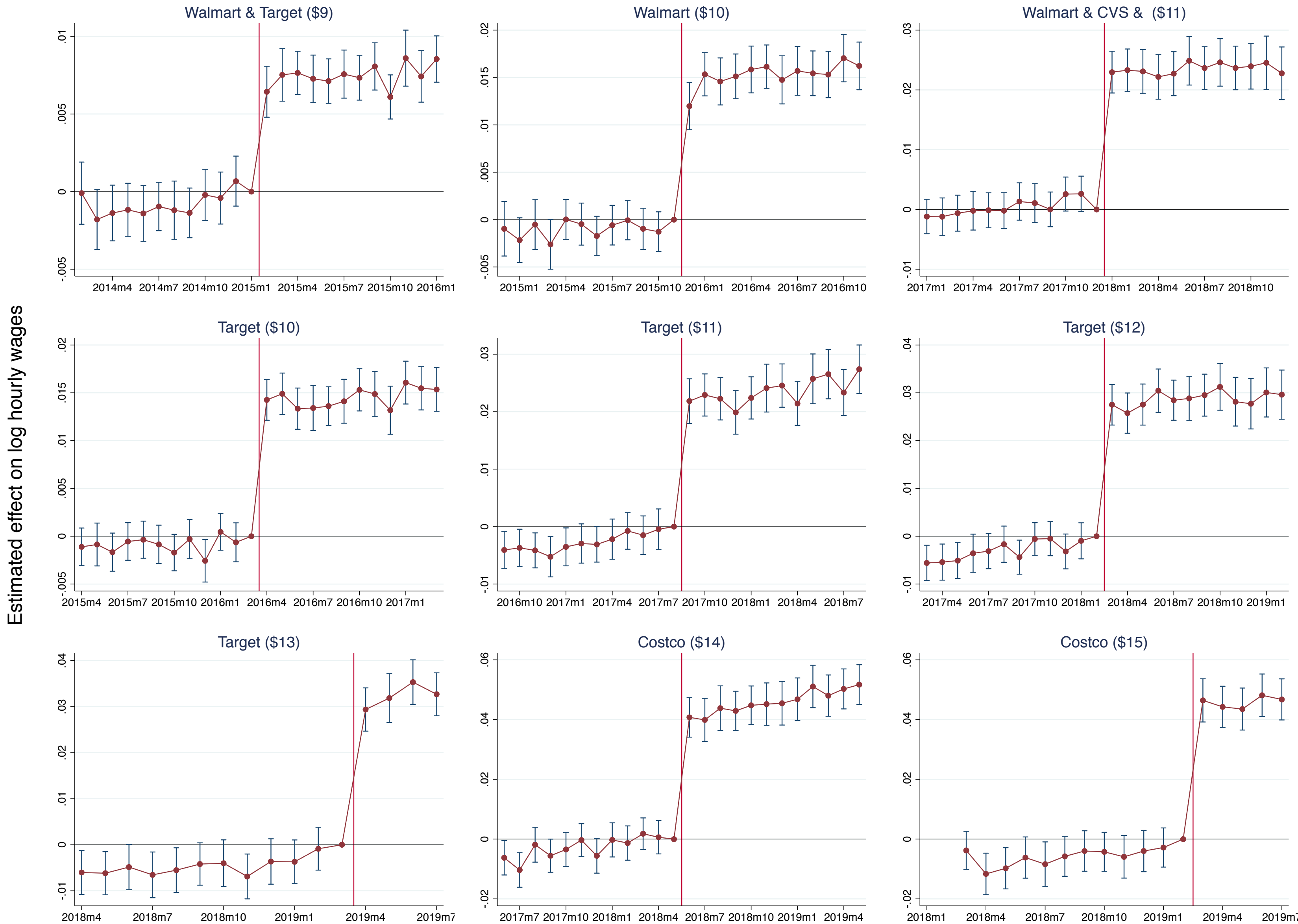
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ 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 E11: Walmart, Target, and Costco MW spillovers: robust to occupation-by-CZ-by-month, employer-by-month fixed effects



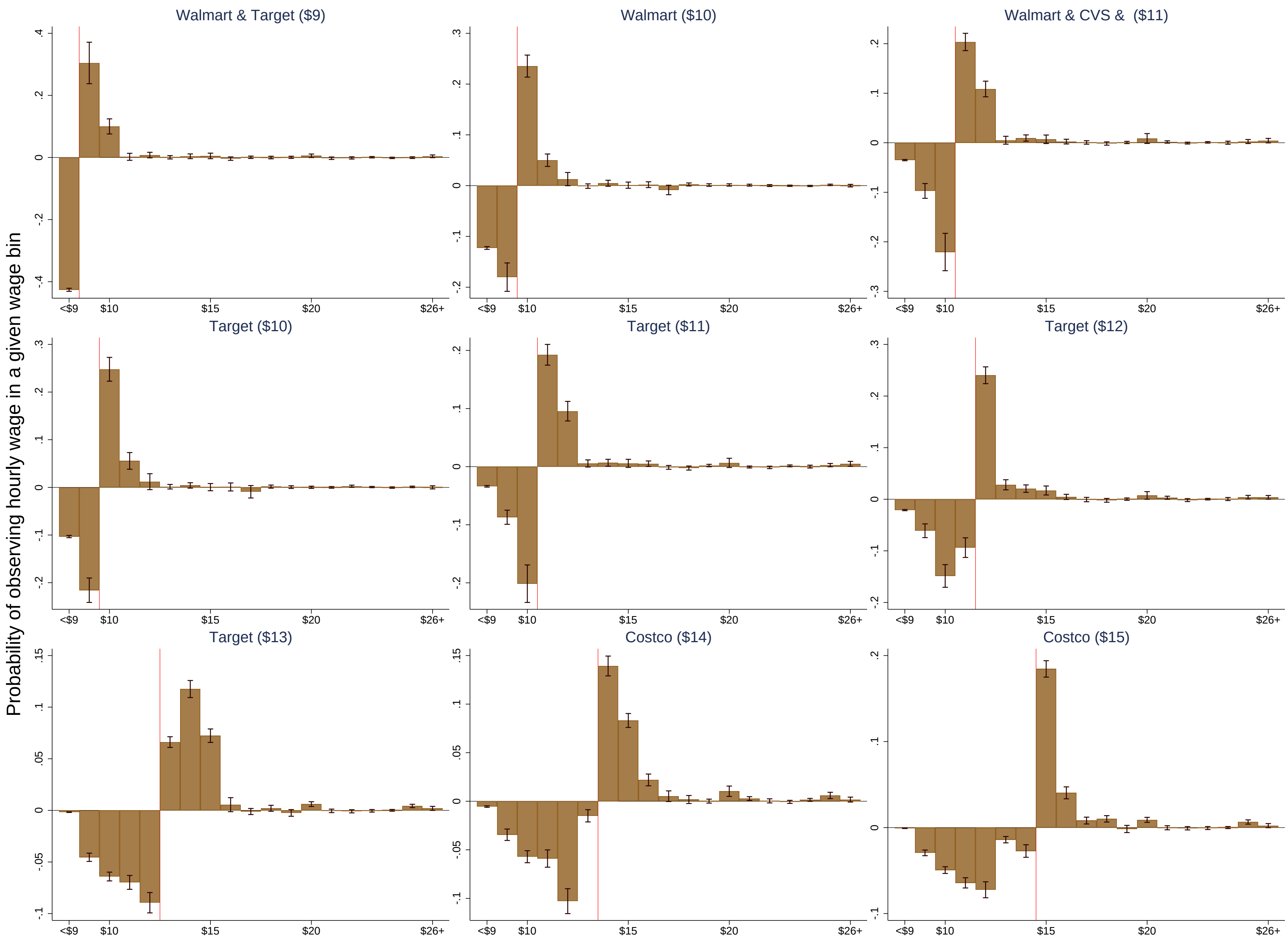
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of non-policy postings in each occupation-employer-CZ 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 E12: Spillovers in worker-reported wages from Walmart, Target, and Costco minimum wages (Glassdoor)



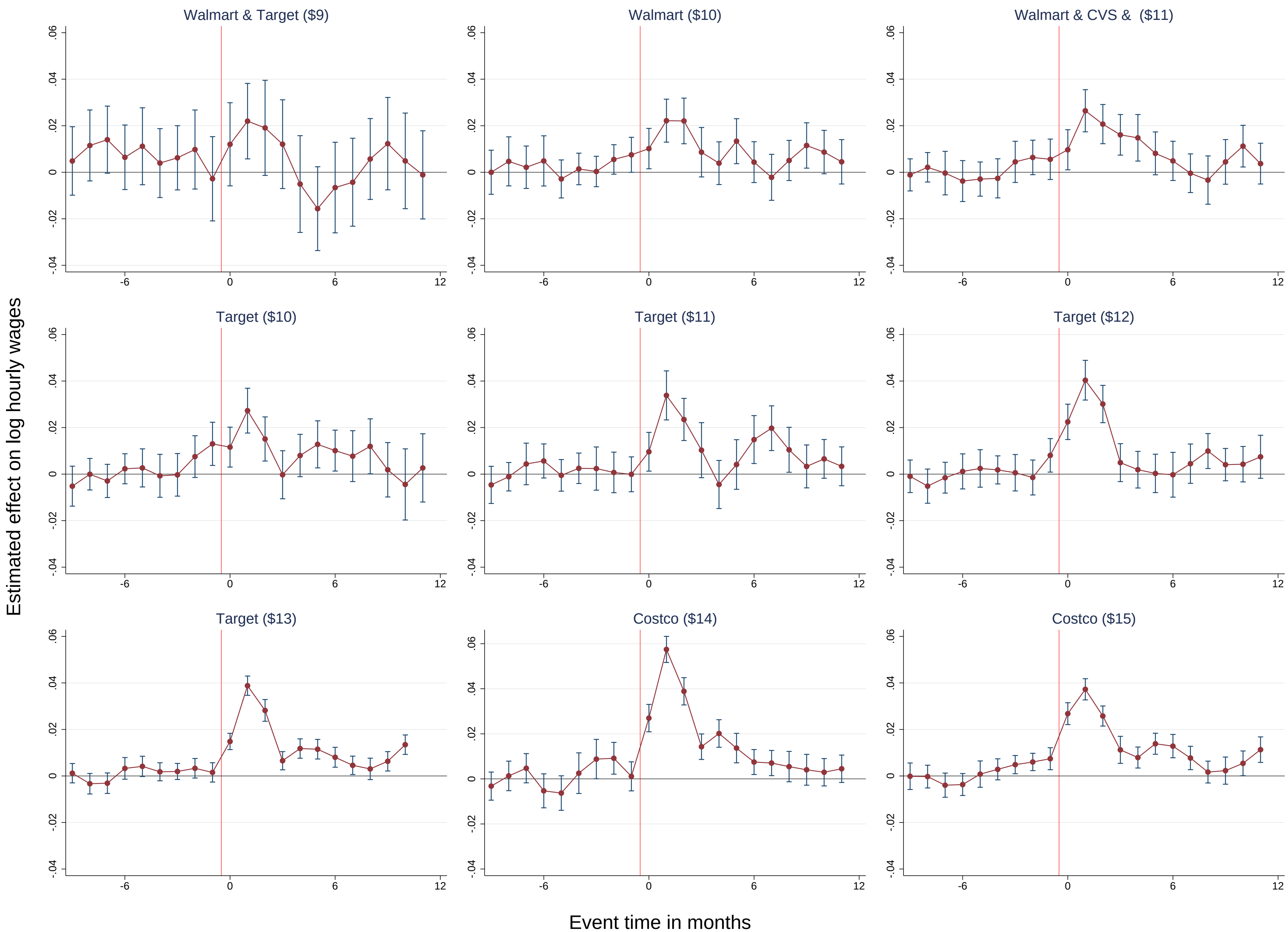
Notes: This figure plots the coefficients on the interaction between exposure to policy firm minimum wages and month fixed effects, where the dependent variable is log reported hourly wage by workers at non-policy employers. Exposure is defined as the fraction of each non-policy employer's job postings 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 E13: Bunching in response to Walmart, Target, and Costco minimum wages



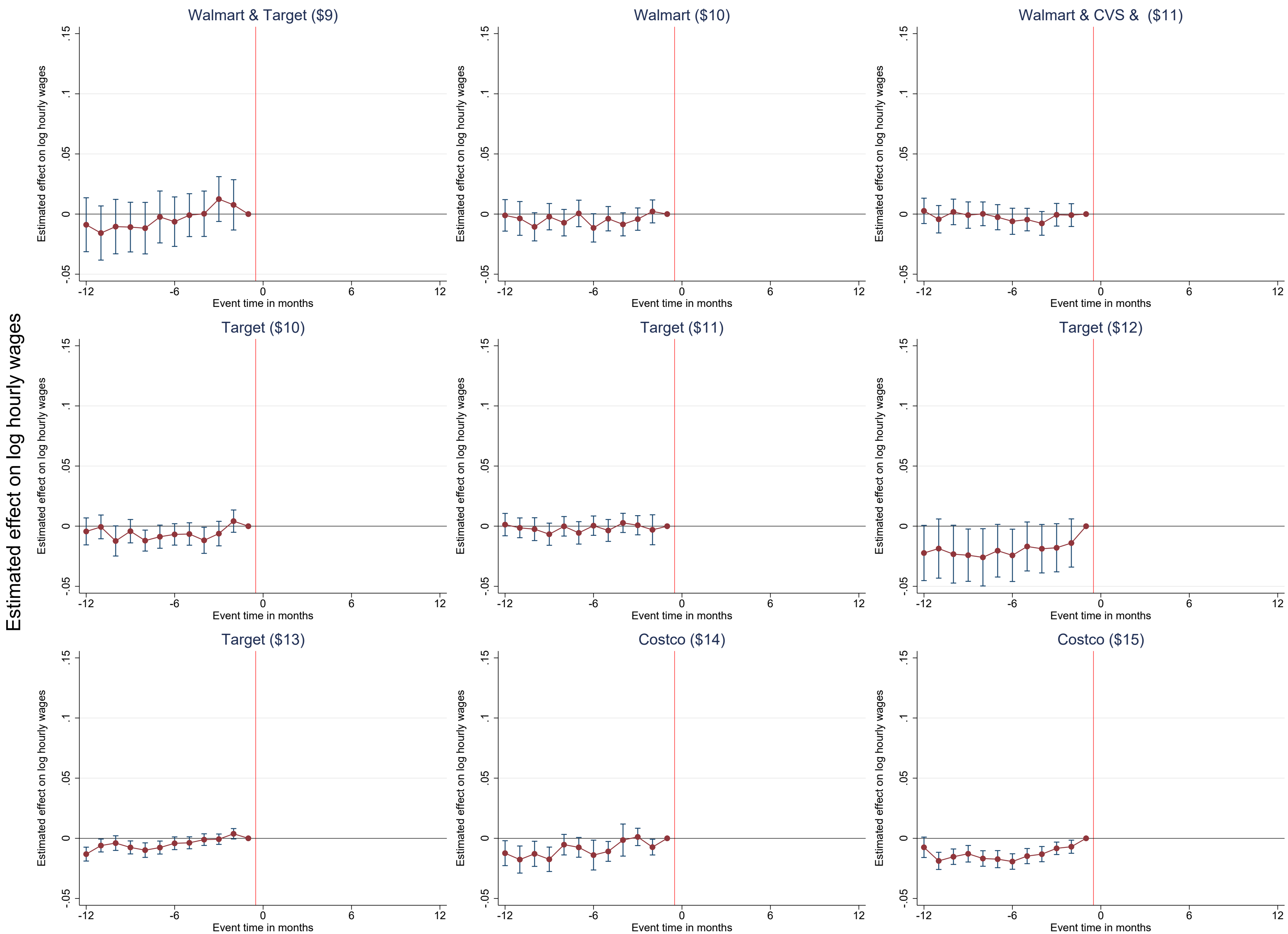
Notes: This figure plots the coefficients from linear probability regressions of hourly wages being in a given wage bin on the interaction between job-level exposure to policy firm minimum wages for non-policy employers and an indicator for post-October-2018. Exposure is defined as the fraction of non-policy 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 E14: Null effects at placebo treatment dates for Walmart, Target, and Costco minimum wages



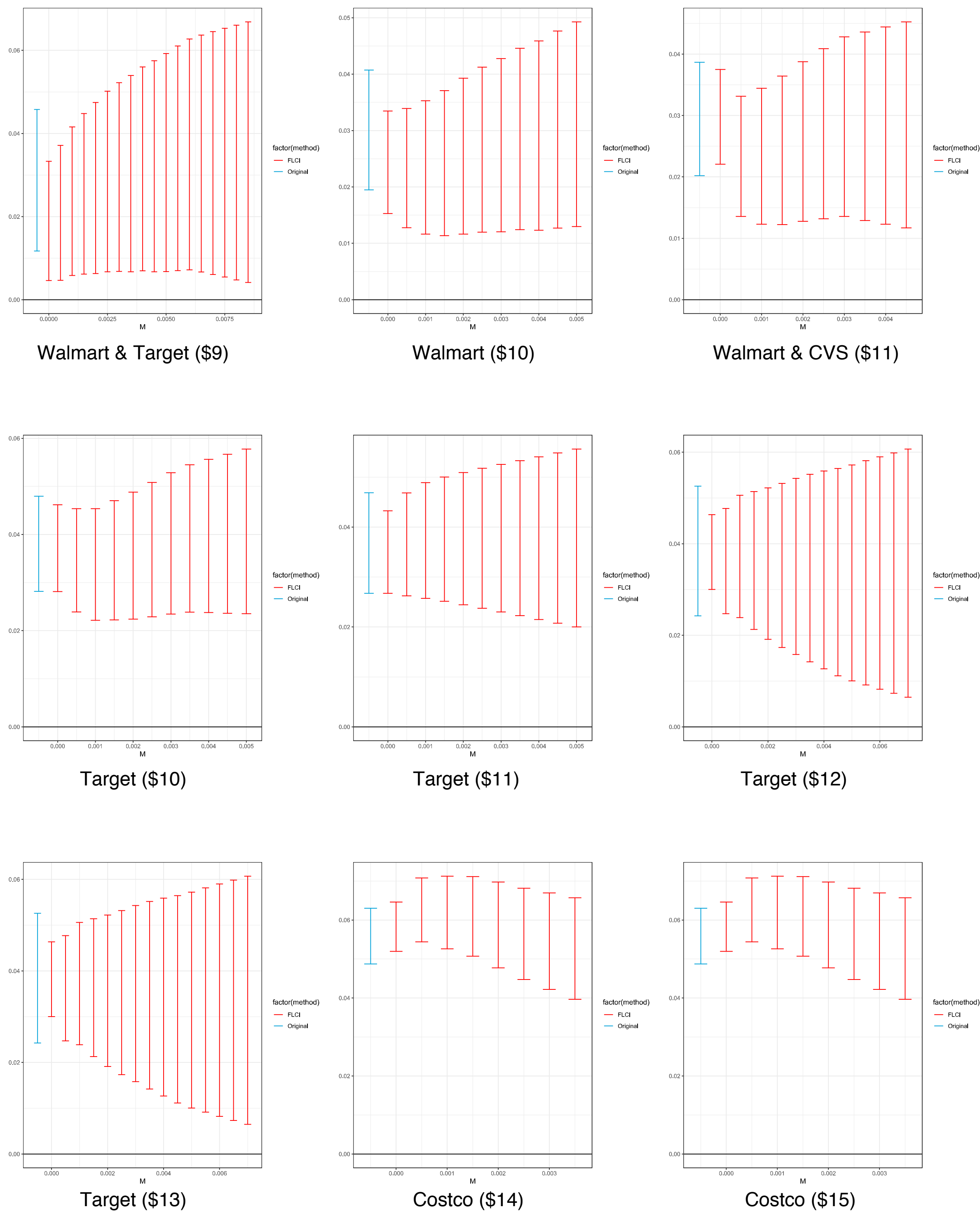
Notes: This figure plots the regression coefficients on the interaction between job-level exposure to policy firm minimum wages for non-policy employers and an indicator for post-treatment for placebo treatment dates, using a 4-month observation window. Coefficients are indexed by the last month of the 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

Figure E15: Separately estimated pre-trends in spillovers for Walmart, Target, and Costco minimum wages



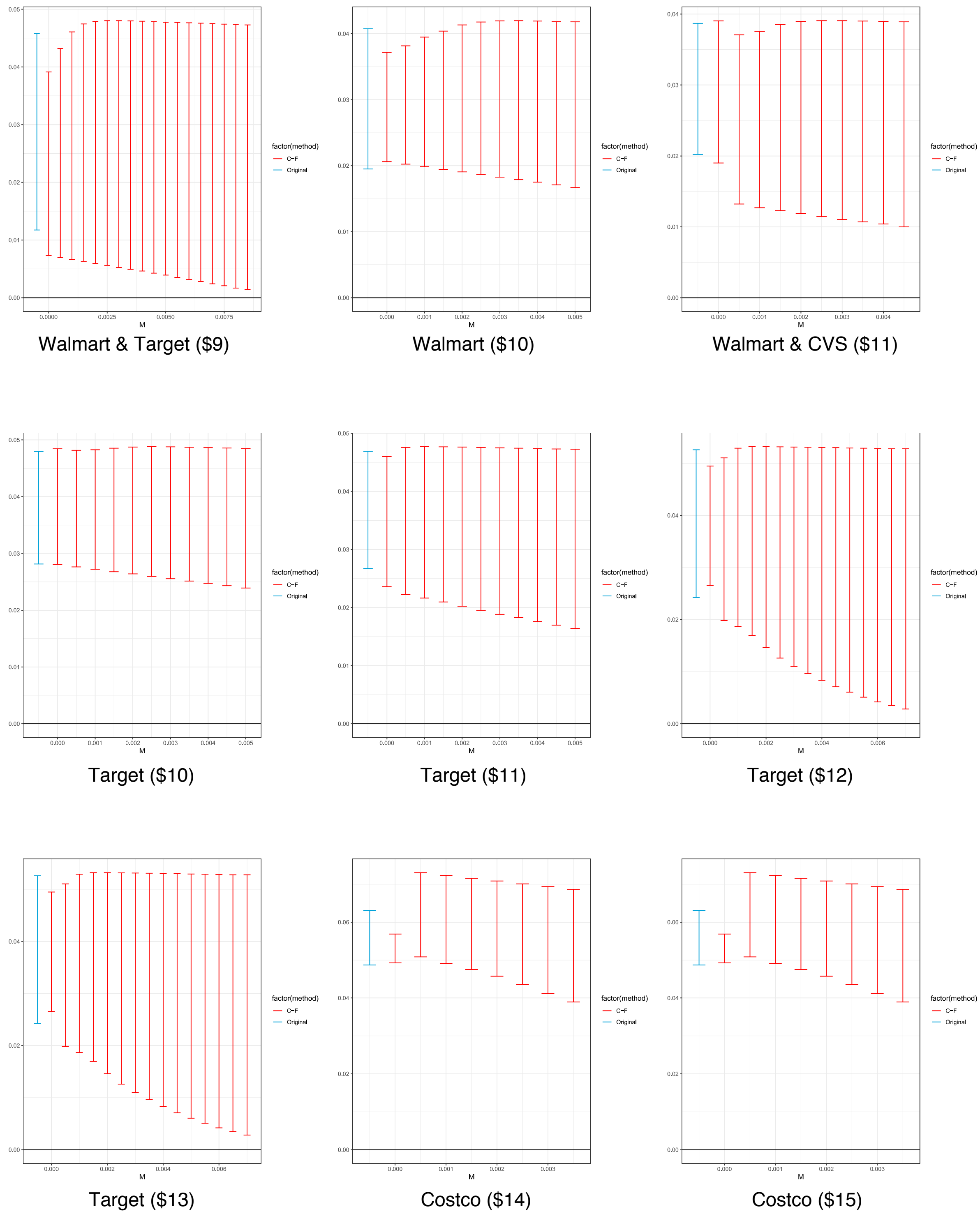
Notes: This figure plots the coefficients β_k from equation 2 for pre-announcement months only, following the proposed tests for absence of pre-trends in (Borusyak et al., 2021; Borusyak and Schönberg, 2021). The joint significance F-test p-values are 0.70 (Walmart/Target $9), 0.70 (Walmart/Target $9), 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 E16: 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 E17: 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.

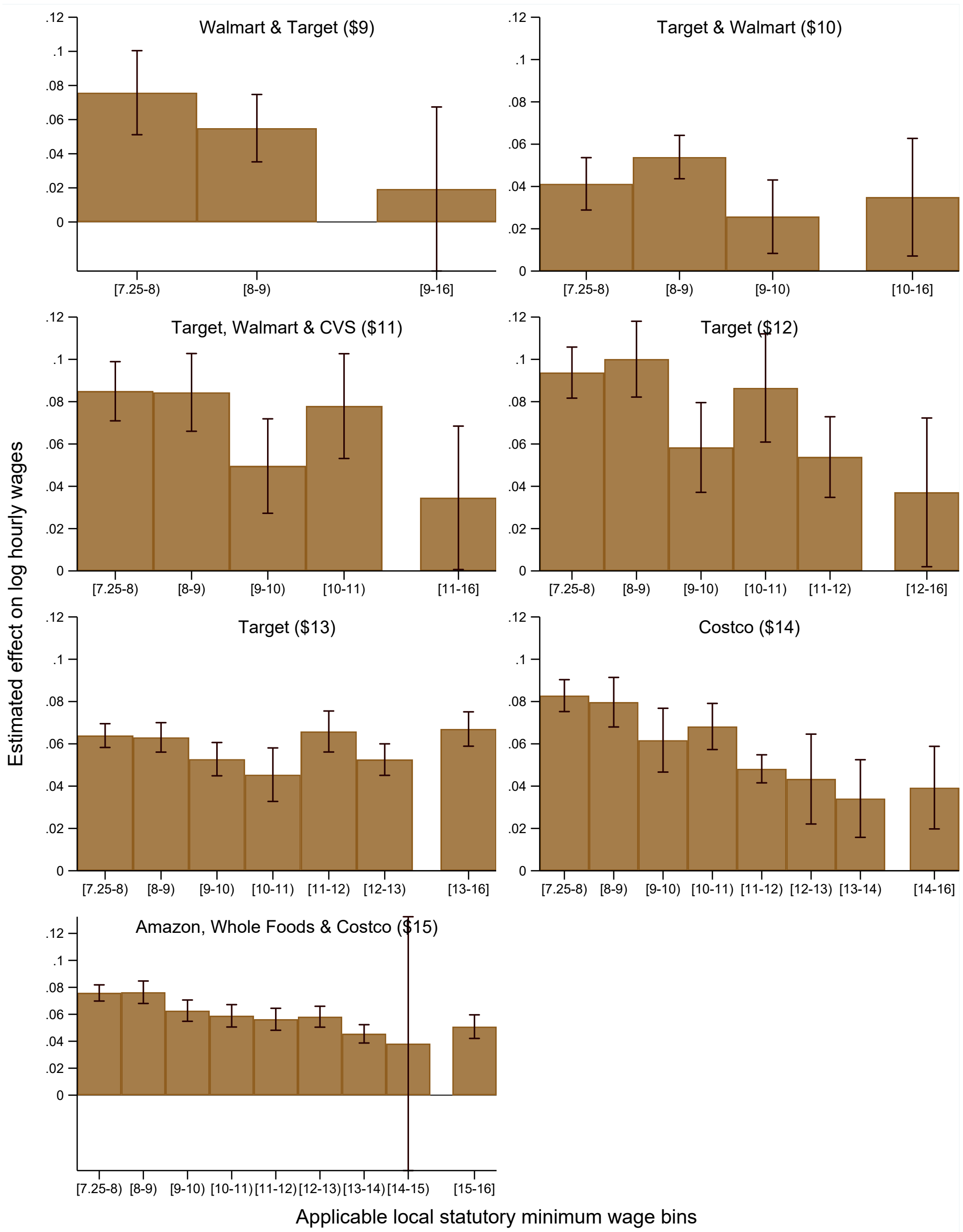
F Additional evidence on employer minimum wage spillovers

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.

F.1 Local moderators of spillovers

Figure F1 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 E13.

Figure F1: Moderation of spillover effect with local minimum wage

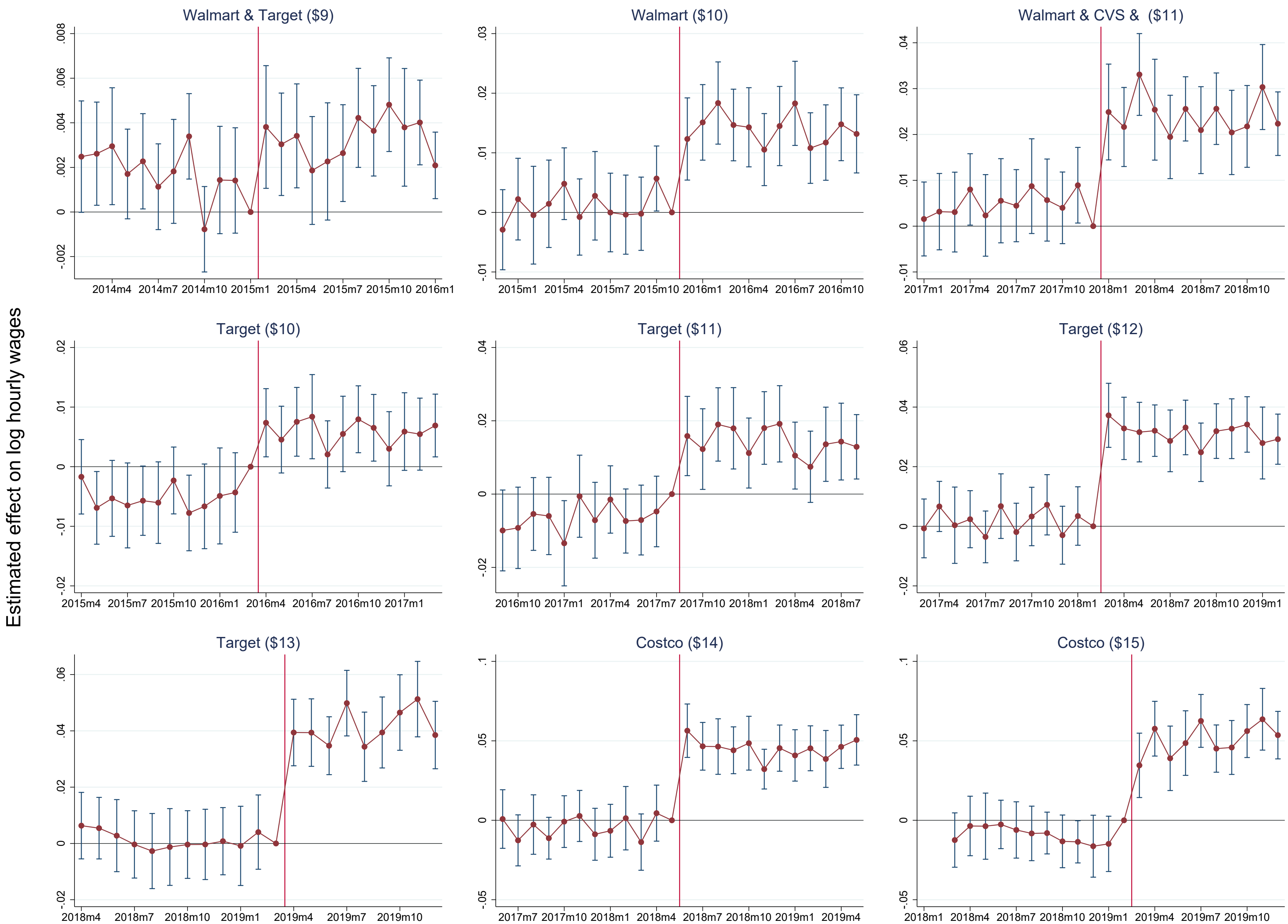


Notes: This figure plots the coefficients on the interaction between exposure to the policy firm's minimum wage and an indicator for the post period, where the dependent variable is log advertised hourly wage. Each bar indicates a separate regression where only postings in the indicated minimum wage 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.

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

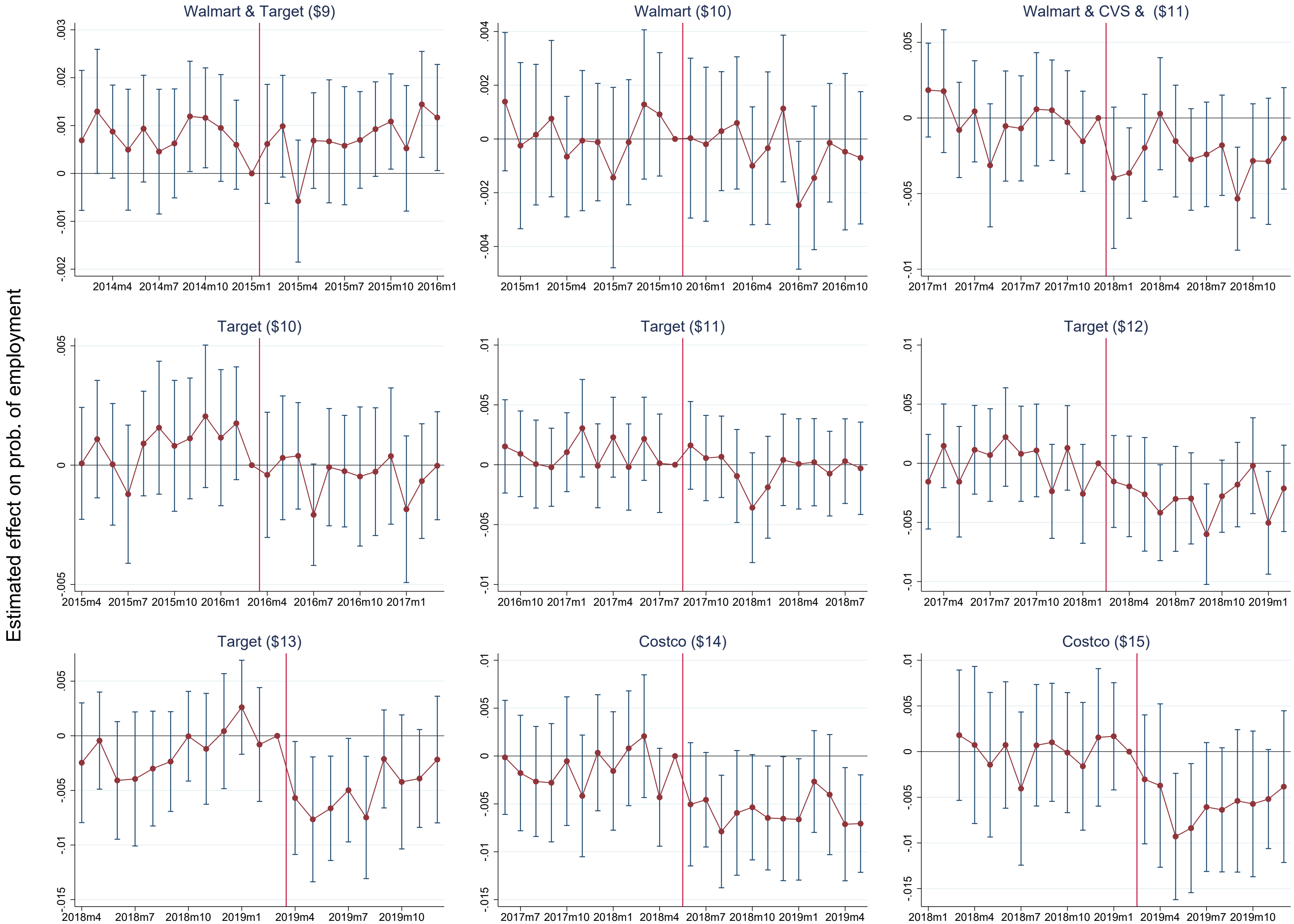
Below in Figures F2 and G4, we report the wage and employment effects of Walmart, CVS, Target, and Costco minimum wages described in Section 5.

Figure F2: 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 F3: Employment effects of Walmart, Target, and Costco minimum wages, using CPS data

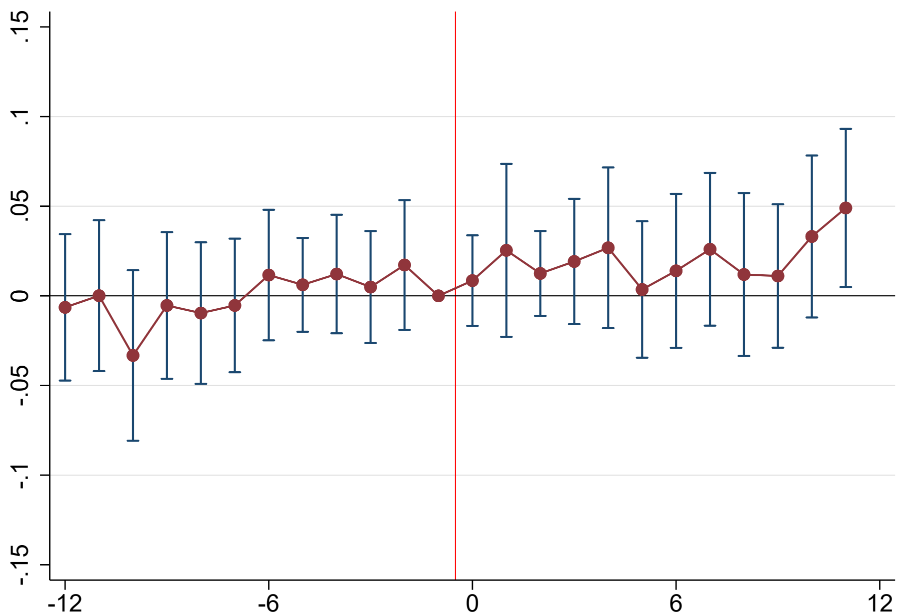


Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy industries interacted with month fixed effects, where the dependent variable is probability of being employed vs. unemployed. Exposure is defined as the fraction of non-policy industry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

F.3 Examining other margins of employer adjustment

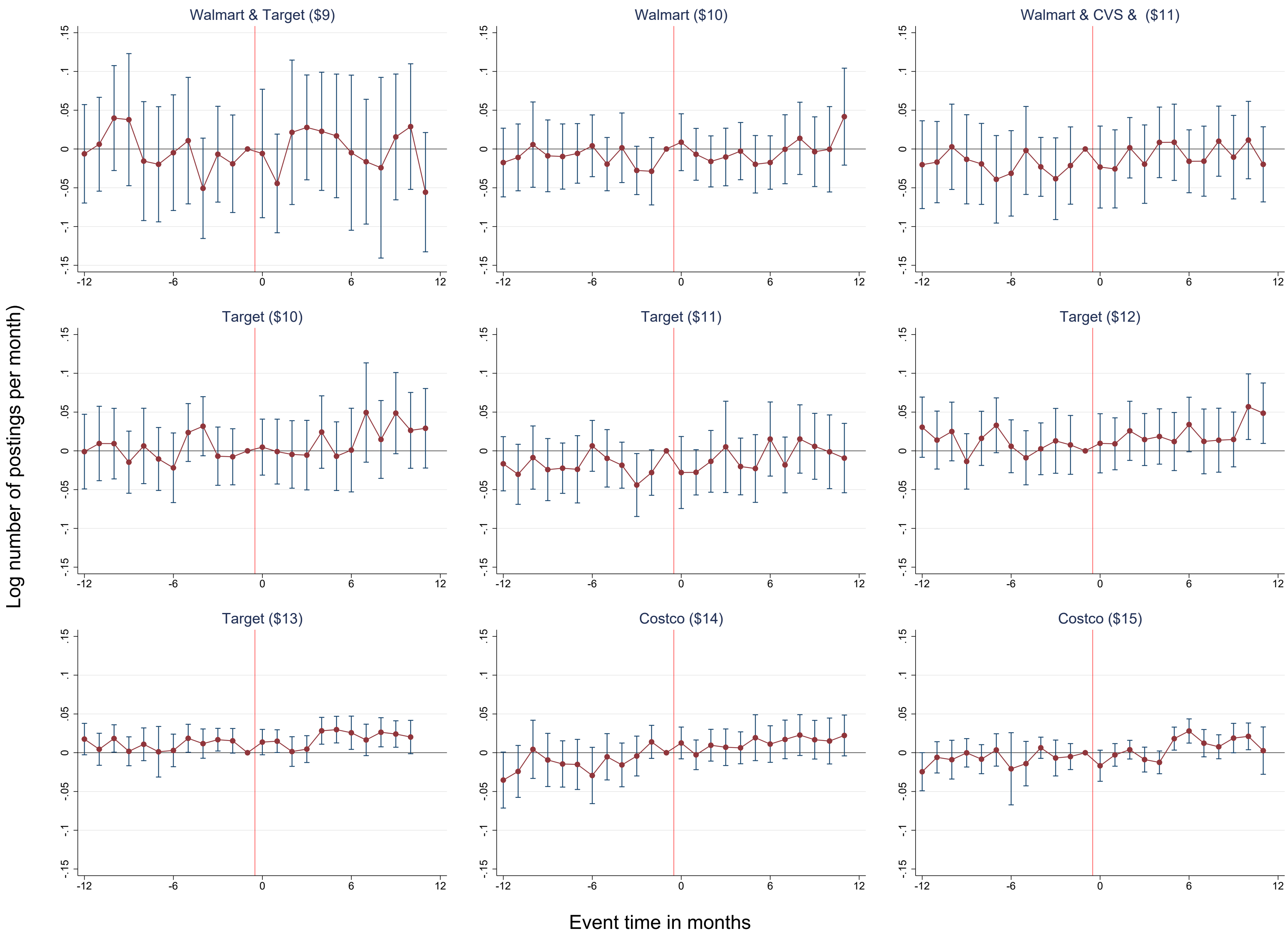
Below in Figures F4, F5, F6, F7, F8, and F9, we report the effect of policy firm minimum wages on the number of postings and the presence of experience and degree requirements on job ads by non-policy employers, as described in Section 5.2.

Figure F4: Effects of Amazon minimum wage on log number of job postings



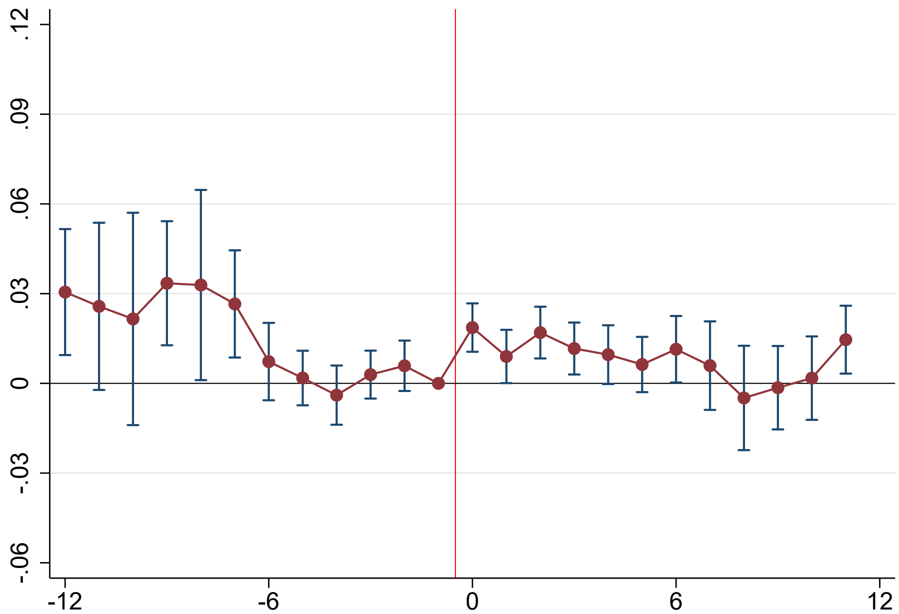
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage policy for non-Amazon employers interacted with month fixed effects, where the dependent variable is log number of postings per employer in a given month, occupation and commuting zone. Exposure is defined 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 F5: Effects of Walmart, Target, and Costco minimum wages on log number of job postings



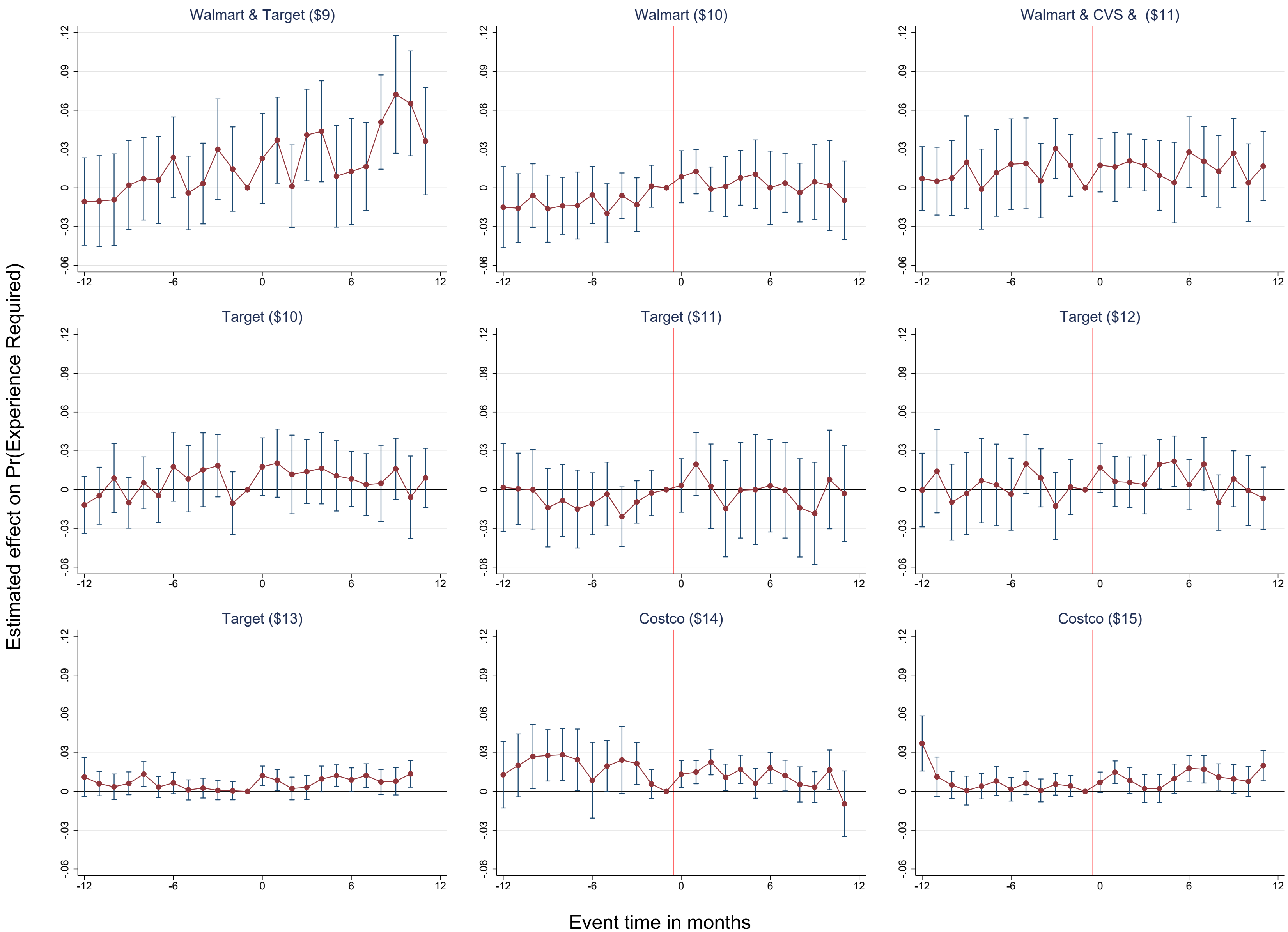
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is the log number of postings per employer in a given month, occupation and commuting zone. Exposure is defined 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 F6: Effects of Amazon minimum wage on experience requirements in job ads



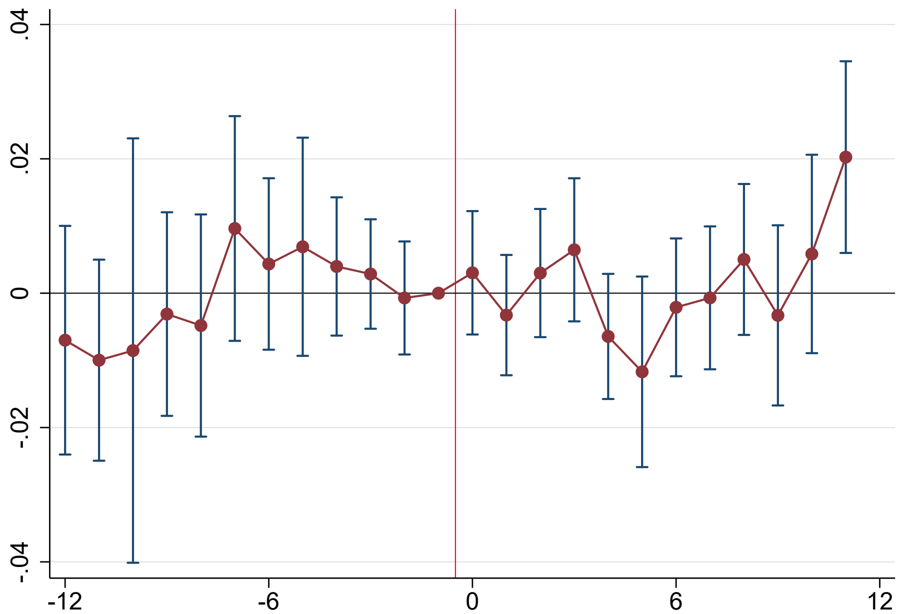
Notes: This figure plots the regression coefficients on job-level exposure to Amazon's minimum wage for non-Amazon employers interacted with month fixed effects, where the dependent variable is a binary variable equal to one if the posting includes a minimum number of years of required experience. Exposure 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 F7: Effects of Walmart, Target, and Costco minimum wages on experience requirements in job ads



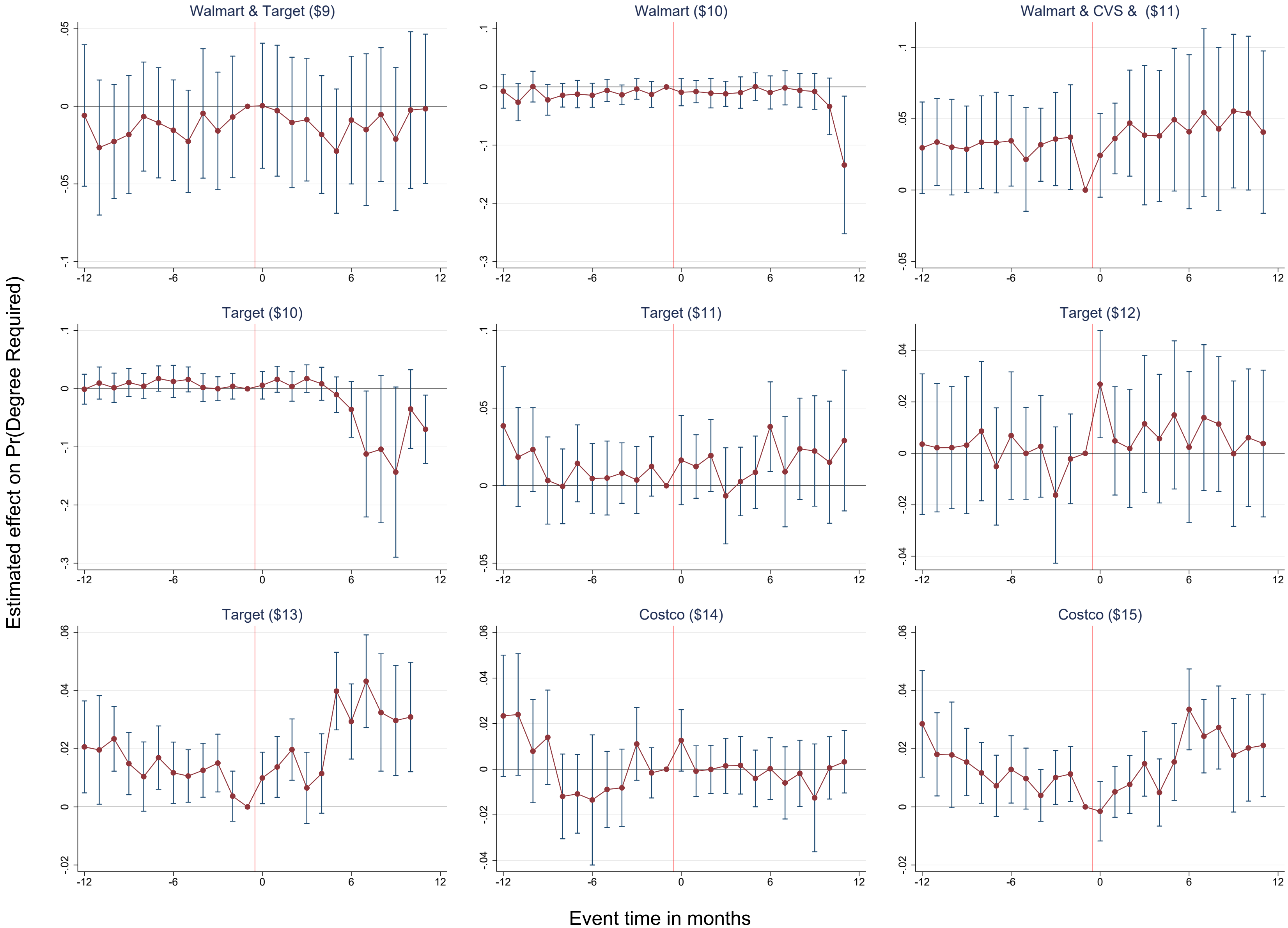
Notes: This figure plots the regression coefficients on job-level exposure to policy firm minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is a binary variable equal to one if the posting includes a minimum number of years of required experience. 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 F8: 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 F9: Effects of Walmart, Target, and Costco minimum wages on degree requirements in job ads

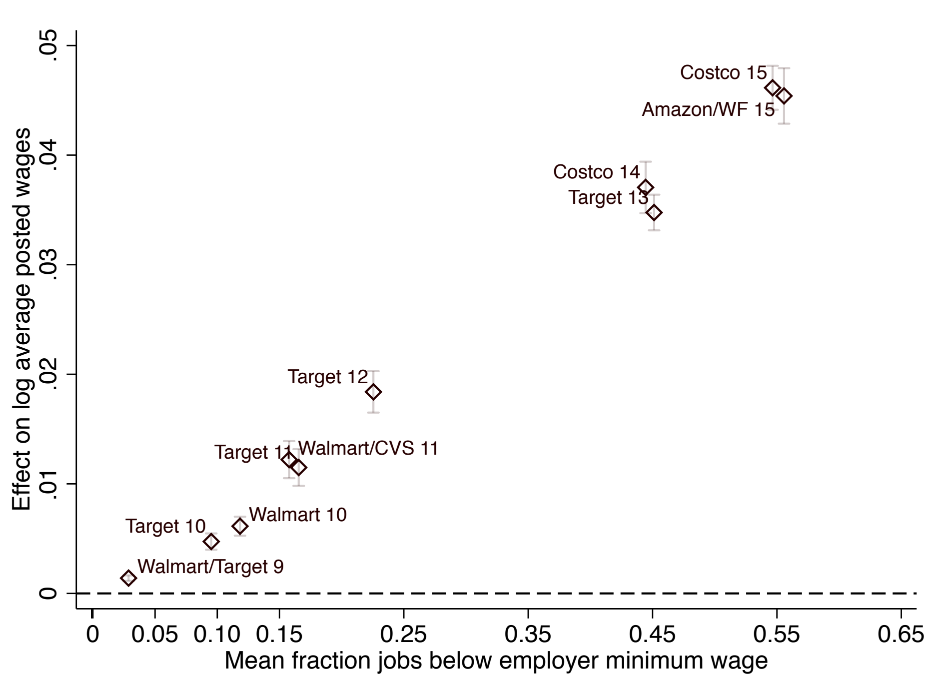


Notes: This figure plots the regression coefficients on job-level exposure to policy firms' minimum wages for non-policy employers interacted with month fixed effects, where the dependent variable is a binary variable equal to one if the posting includes a minimum required educational degree. Exposure 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.

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

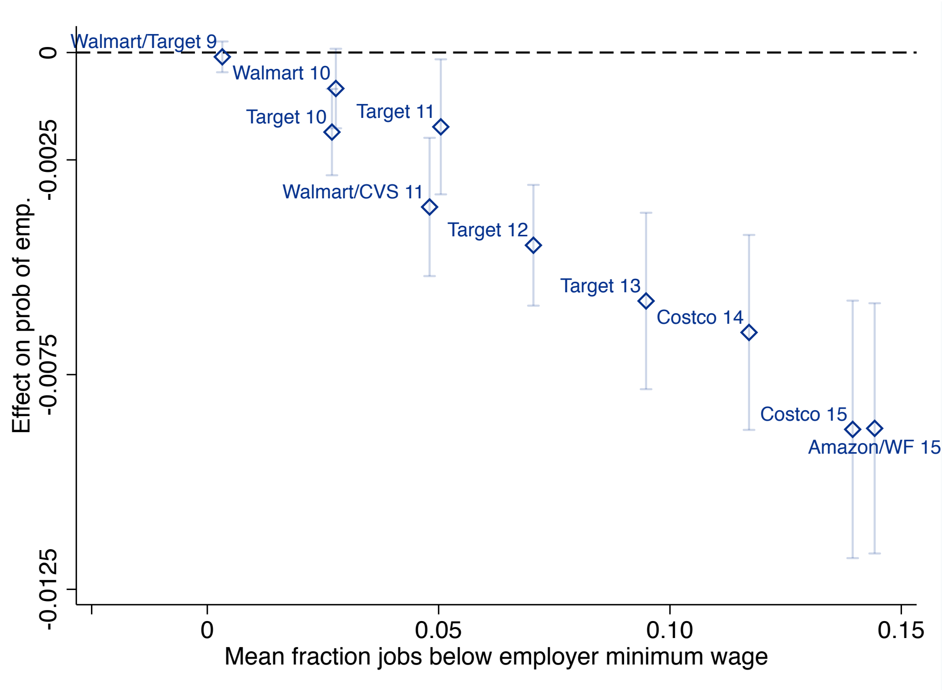
The following figures plot the relationship between spillovers in employer minimum wage policies and the bite of the announced minimum wage among non-policy firm job postings. As shown in Figures F10 and F11, wage and employment spillovers respectively increase and decrease monotonically with the bite of the policy firm's announced minimum wage.

Figure F10: 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 F11: 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.

G 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 G.1); how tight local labor markets are as measured by the unemployment rate in the commuting zone (Section G.2); the percentage of occupational postings accounted for by policy firms in commuting zones (Section G.3); and the likelihood that workers in a subset of occupations move to Amazon in the local market (Section G.4).

G.1 Amazon hiring in local labor markets

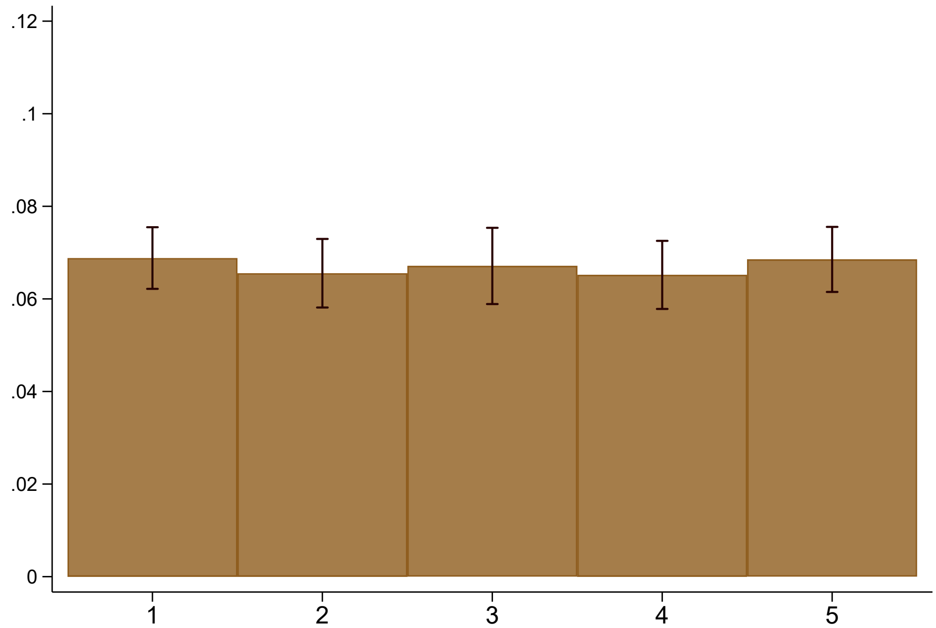
We assess whether Amazon's local hiring is large enough to generate wage responses consistent with demand channels in a simple, perfectly competitive labor market. Abstracting from other inputs and modeling production as a concave function of labor with a demand elasticity of -1, Amazon would have to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

G.2 Moderation by the local unemployment rate

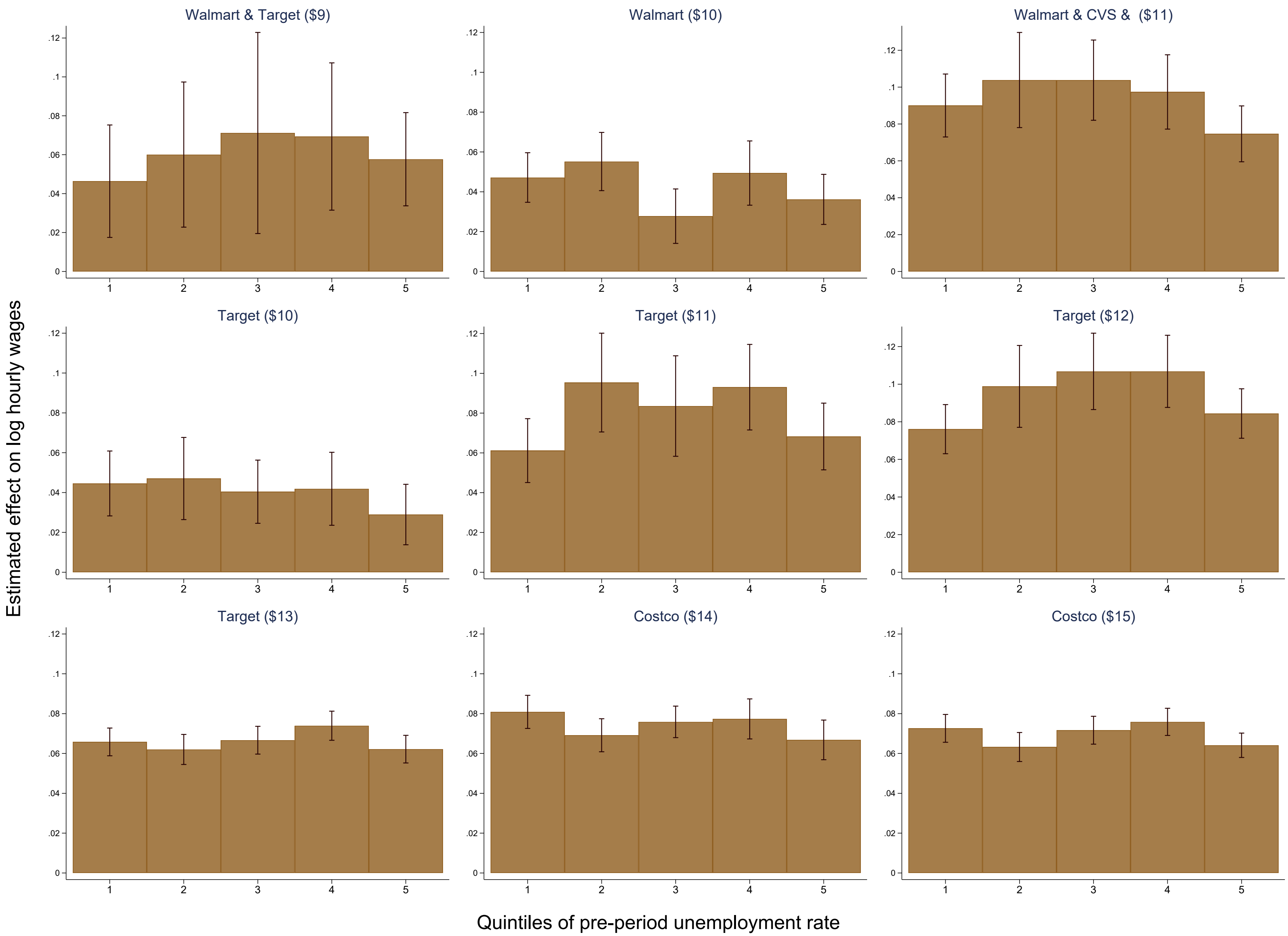
We examine whether wage spillovers are smaller in slack labor markets, as measured by the pre-period unemployment rate. We divide non-policy firm postings into quintiles of their CZ's average monthly unemployment rate in the 12 months before each policy firm's announcement. We then estimate the spillover effect separately for each quintile. Figure G1 reports the results for Amazon's minimum wage while Figure G2 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 G1: 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 G2: Heterogeneity in Walmart, Target, and Costco wage spillover effects by pre-period unemployment rate



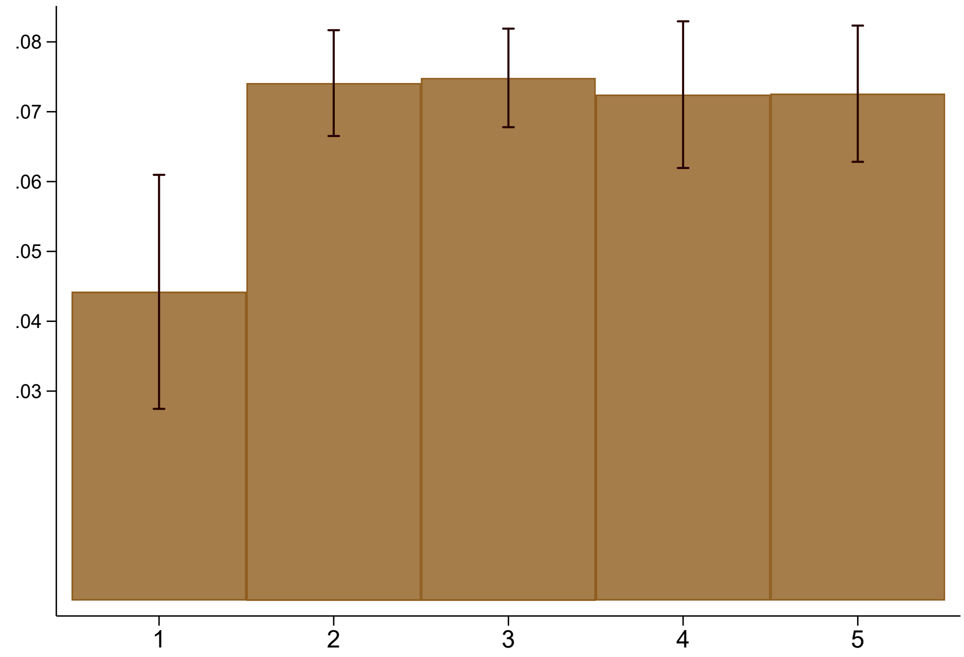
Notes: This figure plots the coefficients on the interaction between exposure to the policy firm's minimum wage and an indicator for the post-announcement period, for non-policy employers, where the dependent variable is log advertised hourly wage. Each bar indicates a separate regression where only 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

G.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.

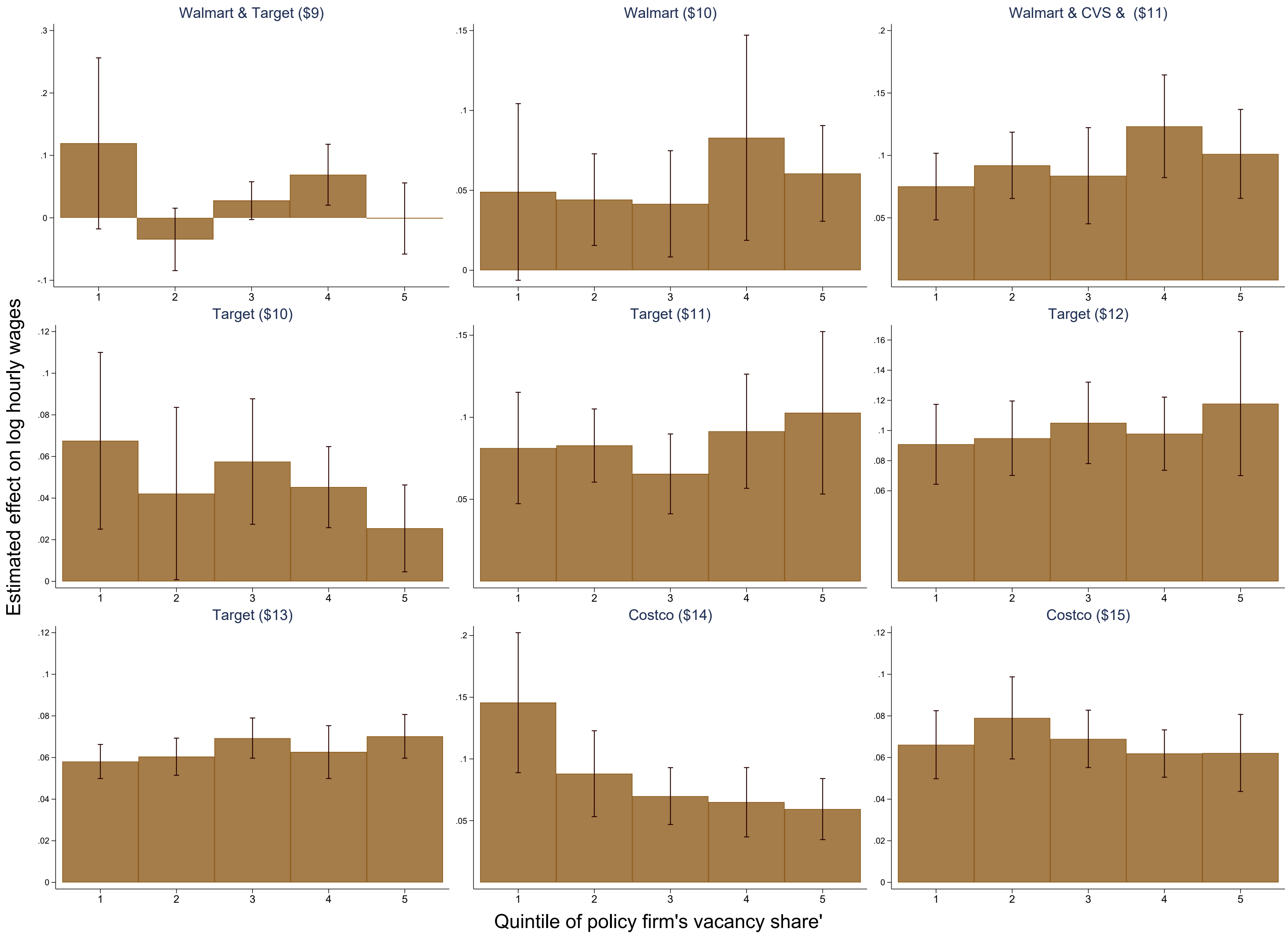
To test this mechanism, for each announcement, we calculate the share of all vacancies in an occupation-by-CZ cell that belong to the policy firm. We then divide our sample of postings into quintiles of the policy firm's vacancy share and estimate the wage spillover effect separately for each quintile. Figure G3 reports the results for Amazon's minimum wage and Figure G4 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 G3: 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 G4: 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.

G.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 G1.[38] 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.[39] 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 G2). 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 G5 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.

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

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

Table G1: 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

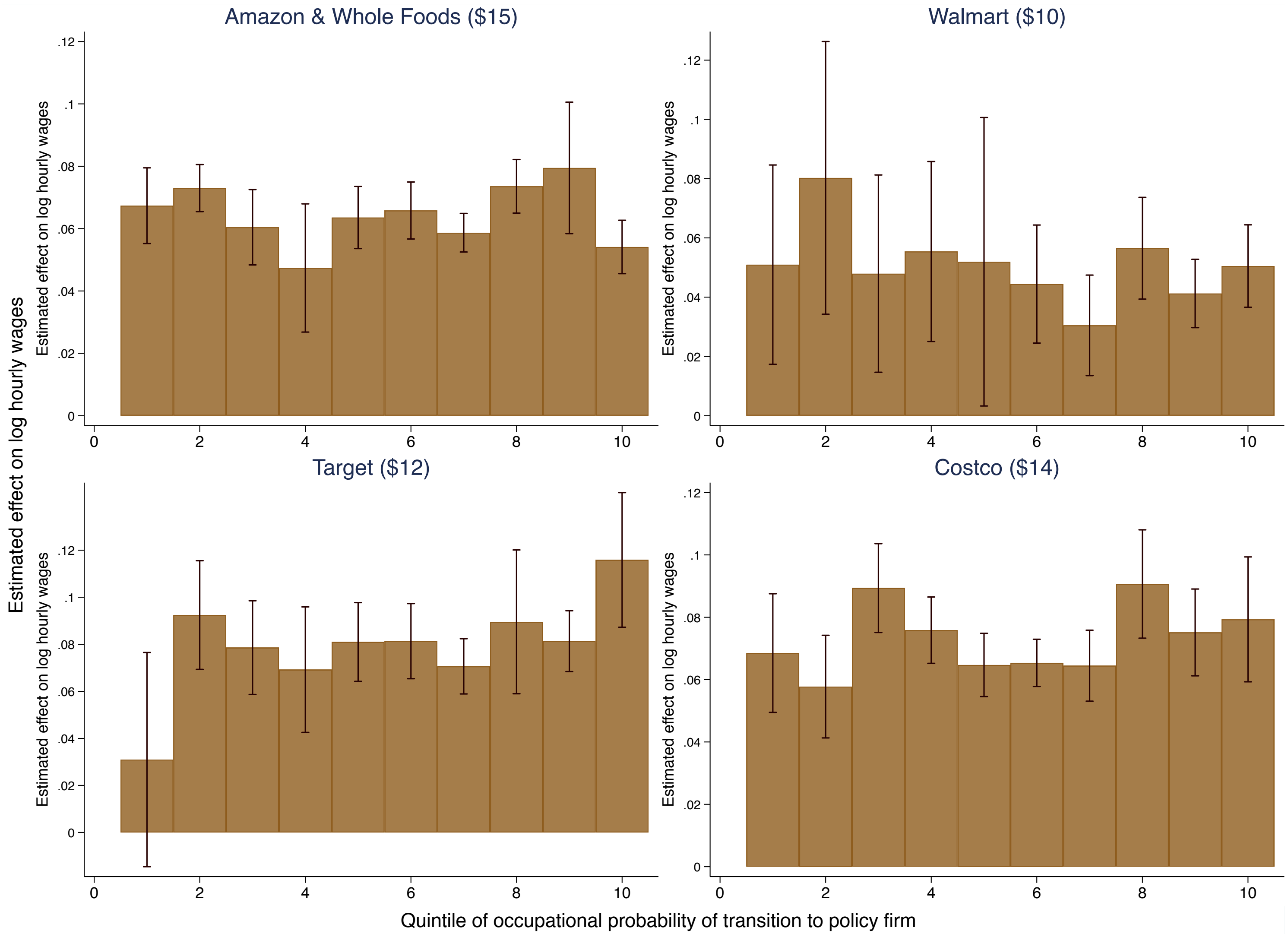
Employer	Occupation (O*NET)	Location	Start date	End date
Amazon Retail LLC	Hand Packer & Packager	Lenexa, KS	Nov. 1, 2017	March 1, 2018
Kids at Heart Child Care Center	Preschool Teacher	Kansas City, KS	Oct. 1, 2015	March 1, 2018
Janitorial	Customer Service Representative	Kansas City, MO	Oct. 1, 2015	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 G2: 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 G5: 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.