Spillover effects from voluntary employer minimum wages *

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Version 0.1 – Very Preliminary Draft.
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March 31, 2020

Abstract

Low unionization rates, a falling real federal minimum wage, and prevalent non-competes characterize low-wage jobs in the United States and contribute to growing inequality. In recent years, a number of private employers have opted to institute or raise company-wide minimum wages for their employees, sometimes in response to public pressure. To what extent do wage-setting changes at major employers spill over to other employers in a local labor market? This paper examines spillover effects from recent company minimum wage increases, including Amazon's increase to $15 an hour in 2019 and Walmart to $9, $10, and $11 an hour from 2015-2018. We estimate the impact of these policies on other low-wage employers in the same county using data on the hourly wage posted on online job ads. We find large spillover effects from Amazon's minimum wage, with a cross-employer wage elasticity of 0.25. We discuss plans to extend the analysis to over 100 recent voluntary employer minimum wage increases across the US.

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1 Introduction

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

This paper examines whether the wage setting behavior of major employers influences wages more broadly and, if so, by what mechanisms. We do so by exploiting sudden changes in the wage policies of Amazon, Walmart, and other large employers to estimate the impact on wages advertised by others in the same labor market. Amazon and Walmart alone employ over 2 million workers in the US, or approximately 1.3% of the total workforce (Amazon.com, 2020; Walmart, 2020; U.S. Bureau of Labor Statistics, 2019). A major contribution of this study, therefore, is to provide some of the first empirical evidence of the impacts their policies have had on the broader labor market. A second contribution of the study will be an extensive exploration of the mechanisms behind any estimated spillover effects, providing insight into why wage setting shocks do or do not ripple outward given different underlying labor market characteristics.

Cleanly identified estimates of cross-employer wage spillovers in the US are limited, largely due to lack of data on specific employers' wage policies. To conduct our analysis, we employ a relatively novel data source on employer wages—online vacancy postings and salary reports from job search platforms. Data from online platforms are increasingly being used to study local labor market concentration, trends in the wages for new hires, and changing demand for skills (Azar et al., 2018; Deming and Kahn, 2018a; Hazell and Taska, 2019). We use these data to show that first, when employers like Amazon or Walmart announce a wage policy change, they do in fact update their advertised wages. Second, we are able to use information from online job ads to identify employers also operating in the low wage sector (based on the distribution of their advertised wages) and the county or commuting zone in which they are advertising.

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

[1]See recent work on rising wage inequality and the erosion of labor market institutions by Piketty and Saez (2003); Song et al. (2015); Kalleberg (2013); Osterman and Shulman (2011); Western and Rosenfeld (2011); David et al. (2016); Weil (2014); and Katz and Krueger (2019).

an employer's pre-policy wages that were below the minimum wage being introduced by Amazon or Walmart, for example. This approach mirrors the approach in papers estimating the causal effect of the federal minimum wage using state-level variation in bite (Card, 1992; Bailey et al., 2018). Here we are able to exploit indicators of bite at the employer level, allowing for a finer-grained approach to analyzing the impact of the wage policies of large, national employers. Our identification strategy relies on the assumption that within counties, specific job categories, and employers, employer exposure is uncorrelated with other factors affecting wages over time. Stable pre-trends provide and sharp effects around the exact time of the policy provide corroborating evidence of this assumption.

We estimate substantial spillovers from the policy changes examined thus far—those of Amazon and Walmart. Prior to the policy change, wages of more exposed versus less exposed employers evolve in parallel. Exactly in the month after Amazon and Walmart announced their wage increases, the wages at exposed firms jump significantly. These effects persist or rise steadily over the post-treatment period. In the case of Amazon, the average non-Amazon employer increased their posted wage by about 3.6% in the 12 months post treatment. Given the size of the increase for Amazon's wages, roughly 13.4%, our results imply a cross-employer wage elasticity of 0.27. Our estimates fall in a similar range as previous estimates for firms in the US: Staiger et al. (2010) finds a wage-setting elasticity in the market for registered nurses of about 0.20. Hjort et al. (2019) examine wage propagation across multinationals' establishments in different countries and find an elasticity of 0.30.

We are able to rule out several alternative explanations for the wage responses we estimate. Our results are robust to controlling for county-specific demand shocks, the decision to post the wage on a job ad,[2], or changes in the composition of occupations advertised on online job ads. Labor market conditions like the unemployment rate do moderate the response of non-policy firms—the wage increase is larger in tighter labor markets—but leave the vast majority of the wage response unexplained.

Our paper relates to several literatures on wage determination, employer wage setting, and monopsony power in labor markets. An older literature focused on a period when unions played a larger role in the US economy and sought to estimate the spillover effect of unions on non-union wages in the same industry.[3] Budd (1992), for example, provides empirical evidence of "pattern bargaining" behavior by unions, leading to wage uniformity

[2]Only a fraction of each employers' postings contain wage information. Although this fraction rises gradually in affected counties after Amazon's minimum wage increase, this shift in behavior does not explain the increase in wages posted by non-Amazon employers.

[3]Perhaps the most comprehensive in this literature, Slichter et al. (1960)'s nearly 1,000-page tome examined the industry-wide impacts of unions on wages, benefits, seniority systems, and other human resource practices.

across firms covered by the same union, even across arguably unrelated industries. Kessler and Katz (2001) study prevailing wage laws that required private construction companies with public contracts to pay wages matching local prevailing rates, the repeal of which led to relative wage losses for construction workers. Finally, Farber (2005) uses CPS data to explore the "union threat hypothesis"—the notion that non-unionized firms experience positive wage spillovers from the presence of unionized firms and the credible threat of unionization. The clearest evidence for spillovers was in the context of de-regulation of the trucking industry, which reduced union and non-union rents through the entry of non-unionized firms.[4]

Several papers have studied employer wage setting and monopsony power in the context of the nursing industry. Perhaps most directly related to our study, Staiger et al. (2010) study the effects of a wage policy change at the Department of Veterans Affairs Hospitals ("VA Hospitals") on the wages of nurses at neighboring hospitals. They provide evidence of monopsony power in this market, estimating substantial cross-hospital wage spillovers and small labor supply elasticities, both of which indicate monopsonistic power in this labor market.[5] More recently, Dube et al. (2017) study bunching of firm wages at round numbers in both online and traditional labor markets, indicative of optimization frictions as well as monopsony power.

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

In addition to providing novel empirical estimates of employer wage-setting spillovers, our study contributes to the search for policy levers to improve wages in the low wage sector. Policy makers' targeted attempts to influence large employers may be an effective form of policy due to employer wage-setting power and declining worker bargaining power.[6] The specific mechanisms through which wage policies propagate, whether through labor market competition or by influencing local wage norms, is also relevant

[4]Freeman and Medoff (1985) provide a broader study of the impact of unions on wages and inequality, finding spillover effects most pronounced in larger non-union firms as well as those facing an imminent union organizing drive.

[5]Other studies of employer market power in this setting include (Sullivan, 1989; Matsudaira, 2014). See Naidu et al. (2018) for an overview. A handful of recent papers have explored cross-employer wage spillovers in other countries, including through former coworker networks in Denmark and across temp agencies and clients in Argentina (Caldwell, 2018; Drenik et al., 2020).

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

for policy makers. Our ongoing analysis of the moderators of wage spillovers, including labor market tightness and occupational structure, will shed light on light on potential sites of policy intervention.

The paper is structured as follows. Section 2 provides an overview of the specific employer wage policy changes we study. The next section discusses online vacancy and other data sources used to analyze spillovers to other employers. Section 4 outlines our empirical approach leveraging employer exposure to wage policy changes. Section 5 discusses spillover results and estimates of cross-employer wage elasticities. In section 6, we provide initial evidence on the moderators of wage spillovers. Section 7 concludes.

2 Overview of recent employer wage policy changes

In recent decades, US federal labor and employment regulation have lagged behind the restructuring of the low-wage sector. Workers in the gig economy and other alternative work arrangements fall outside traditional employment classification, and corporate outsourcing and franchising present challenges to worker collective bargaining. Beginning in 2012, worker organizations and advocacy groups, led by the Service Employees International Union ("SEIU") launched the "Fight for $15" campaign to advocate for higher wages and union representation. The coalition drew on the union's earlier efforts to institute "living wages" through local ordinances and government contracting and sought to bring attention to persistently low earnings among workers in fast food, retail, and other service occupations despite a growing economy and low unemployment.[7]

Notably, within a couple of years, a number of low-wage, predominantly retail and service sector employers voluntarily instituted minimum wage increases for their employees. Descriptive evidence on these policy changes, let alone on their broader impacts in the labor market, is largely lacking. In this section, we provide background information on the wage policy changes of Walmart and Amazon, the major focus in our study. The bulk of our analysis focuses on Amazon's minimum wage due to its magnitude and potential impact on wages more broadly. At $15 an hour, Amazon's minimum wage is nearly double the federal minimum wage and far exceeds the majority of state minimum wages in the US.

Amazon/Whole Foods In October of 2018, Amazon announced a minimum wage of $15 per hour for all employees effective November 1, 2018.[8] The increase impacts an

[7]Indeed, recent local governments' adoption of $15 minimum wages have been attributed to the efforts of the "Fight for $15" campaign. See Rolf (2015) for a history of the campaign and its influence on local politics and worker-organizing efforts.

[8]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

estimated 350,000 workers (including those at Whole Foods) (Amazon.com, 2019).[9]

We provide initial "first stage" evidence of Amazon's 2018 company-wide minimum wage increase in Figure 1 using BGT data. The figure illustrates that company-wide minimum wage policies are identifiable in online job ads. Prior to October 2018, average wages at Amazon and Whole Foods were $12.67 an hour. After October 2018, they were $14.68 an hour.[10] The shift represents a 13.4% increase in the average Amazon and Whole Foods' posted wage.

Walmart Walmart remains the largest employer of workers in the US. Its 4,177 stores in the US are dispersed throughout the country. Changes in its wage-setting policies therefore have the potential to influence wage levels in areas where the chain operates. Walmart instituted a major change in its hourly wage policies in recent years. In February of 2015, Walmart announced that it was increasing entry-level wages for its part-time and full-time sales associates across the country to $9 per hour effective in April 2015, and to $10 an hour one year later. Walmart reported that 40% of its workforce was affected by the change. In January of 2018 they announced a further increase to $11 an hour, effective February, 2018 (Walmart, 2018). To date, however, there has been little systematic evidence or evaluation of the policy change, as the wage distribution of Walmart workers is not publicly available. We provide initial "first stage" evidence of Walmart's 2015 company-wide minimum wage increase in Appendix Figures A1, A2, and A3.[11]

3 Data

A key difficulty in measuring and identifying cross-employer hourly wage spillover effects in US local labor markets is the lack of readily available datasets with employer information as well as the hourly wages offered by establishments. [12] One of the contributions

minimum wage increase as originally structured would have actually reduced earnings for some of the company's highest productivity employees. The proposal was quickly modified to correct for this problem by providing additional increases for those workers otherwise adversely affected by it. Furthermore, the wages of contractors were not included in the new policy. See Abbruzzese and Cappetta (2018), Murphy (2018), and Wiese (2018).

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

[10]We define October 2018 as the start of treatment in case ads for positions beginning on or after November 1 are advertised in October. This may be one reason there persists a small mass of post-treatment wages below $15.

[11]Our ability to detect wage policy changes at Walmart hinges on their use of online job posting as well as their tendency to post wage information on job ads, which increases over time according to our analysis of Walmart's wage postings in Burning Glass Technologies ("BGT") online vacancy data covering 2015-2019.

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

of this project will be integrating data from major online job platforms in order to better identify cross-employer wage spillover effects in the US. Data from online job platforms are increasingly being used in studies of labor markets in economics (Deming and Noray, 2018; Deming and Kahn, 2018b; Azar et al., 2017; Hazell and Taska, 2019). Websites like CareerBuilder, Glassdoor, Indeed, and BurningGlass provide wages posted by employers, often with rich information on job title, desired skill or experience level, and the geographic location of the establishment posting the vacancy. Platforms with worker participation, such as Glassdoor, often collect rich worker-level information, including age and gender. This information will be used in supplementary analyses to understand any heterogeneity by worker characteristics.

3.1 Burning Glass Technologies

The key data source for our main cross-employer wage regressions will come from Burning Glass Technologies ("BGT"). BGT collects data on the near-universe of online job postings from roughly 40,000 websites, including job boards and company pages.[13] The data cover job postings from 2010 onwards, an estimated 10% of which include information on the posted wage for that job (Hazell and Taska, 2019; Carnevale et al., 2014). Hazell and Taska (2019) provide extensive evidence on the validity of these data and their consistency with overall US new hire wage trends from sources such as the Current Population Survey ("CPS") and the Quarterly Census of Employment and Wages ("QCEW").

Studies by Azar et al. (2018); Deming and Noray (2018); Deming and Kahn (2018b) provide further evidence on the value of and validity of BGT data. With the appropriate econometric framework and assumptions, which are detailed in Section 4,[14] we believe analysis of these data will yield precise and unbiased estimates of wage spillover effects arising from the local wage setting shocks detailed in the Section 2 above.

Here we briefly describe features of the data and the available variables that make the data appropriate for the analysis we will be conducting:

1. **Frequency:** The dataset on posted wages is high frequency, including information on the day, month, and year of the posting. These high frequency wage posting data will allow us to test for parallel trends in the wages of treated versus untreated establishments and to isolate effects occurring precisely around the announced increases.

2. **Direct measures of outcome of interest:** The dataset on vacancies with posted wages includes a variable indicating the posted minimum salary for specific time

[13] Job postings are at the establishment level, or the specific physical branch of a firm.
[14] Tests of which are also discussed in this section.

units of pay. For example, for hourly wage jobs, the posted minimum hourly wage is available. This is directly the outcome of interest in this study as we are interested in how local wage shocks influence the wage setting behavior of employers.

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

A number of papers using BGT data have analyzed its representativeness. Hazell and Taska (2019) confirm that industries that are less likely to post vacancies online are underrepresented in BGT relative to CPS. We conduct our own comparison of the occupation, industry, and geographic distribution of hourly workers in the CPS to those of hourly job vacancies in BGT.[15] We find that relative to existing stocks of hourly workers in the CPS, a higher share of hourly job vacancies are present in the West and a lower share in the South. Hourly job vacancies are skewed towards health care and services and away from retail. These discrepancies may represent differences between sectoral growth versus current sectoral composition; Hershbein and Kahn (2018) find that the degree to which BGT under-represents some industries and over-represents others is stable over time.

3.2 Glassdoor

Glassdoor is a two-sided online job search website where employers post vacancies but importantly, job-seeking users of the platform also upload information about salaries for specific job titles at specific firms. Salary information for hourly workers contains exactly the hourly wage. The Glassdoor data are complementary with the BGT data and allow us to compare wage trends and conduct validity checks across two separate online data sources. Importantly, because Glassdoor contains worker-reported wages, the dataset allows us to check whether changes in advertised wages translate into changes in wages received by workers. Wage changes in Glassdoor confirm that any effects found in BGT are not simply driven by systematic changes in which jobs are advertised online as opposed to a true shift in the wage distribution at the treated firm.

In addition to variables also contained in BGT data, including employer identity, the location of the establishment, and wage information, Glassdoor data provide additional worker-level characteristics. For example, a large fraction of workers using Glassdoor report their gender when workers create a Glassdoor account. These worker-level characteristics will allow us to test for further heterogeneity in any estimated wage spillover

[15] Available from the authors upon request.

effects.

4 Estimation strategy

There is little empirical evidence documenting whether the actions of major employers have spillover effects in their local labor market. An older literature on the union threat hypothesis, union wage spillovers, and the Davis Bacon Act suggests that organizational wage policies played an important role in shaping inequality and the broader wage structure (Budd, 1992; Farber, 2005; Western and Rosenfeld, 2011; Kessler and Katz, 2001).[16]

The empirical strategy we propose is a series of differences-in-differences and event-study analyses for the wage policies and reform efforts described in Section 2: Amazon's and Walmart's company-wide minimum wage increases.[17] We refer to these firms at which there has been a policy change as a "policy firm" and a firm that does not experience a policy change as a "non-policy firm." In this section, we describe the sample of jobs to be analyzed, exposure to treatment, the estimating equation and its underlying assumptions, and robustness tests.

Sample Our sample consist of online job ads from January 2014 (CHECK) through May 2019 that contain the following information: the posted minimum hourly wage; employer name; the county in which the job is located; and the occupation of the position being advertised (using the SOC code). We further restrict our sample to those jobs for which the pay frequency is hourly. In some of our analyses, we further restrict the industry code (up to 6-digits NAICS code) to be non-missing. We restrict the data further to focus on specific observation periods of 24 months around the wage policy changes analyzed below.[18] Because we use employer fixed effects models, we restrict to employers who appear at least once before and once after treatment within an observation period. Finally, we restrict each analysis to only those counties for which we observe policy firm job ads in the BGT data in the pre-treatment period.

Treatment and Labor Market Definition We examine the impact of wage policy changes by major employers on the wage-setting policies of other employers in a local labor market. We define these local labor markets as counties and commuting zones.

[16] See Kessler and Katz (2001) in particular for a relevant study of spillover impacts of the Davis Bacon Act.

[17] We are also currently investigating the wage spillover effects of over 100 additional employers who increased their company minimum wages to at least $15 an hour in the last 6 years, based on data kindly shared with us by the National Employment Law Project.

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

For a given policy change, we define a treatment variable for a job posting at a non-policy employer f as follows:

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

or the pre-period fraction of job postings at non-policy firms that are below the minimum wage (w^*) set by the policy firm. We consider postings occurring between 1 to 12 months prior to the policy change for calculating the fraction of jobs affected. For illustration purposes, in the case of the Amazon experiment this equals the fraction of postings from October 2017 until September 2018 with wages below $15. We restrict our analysis to counties where the policy firm advertised in the year before treatment. In practice, this restriction does not greatly affect the sample size. In the case of Amazon and Whole Foods, which advertised in 366 counties in the pre-period, 85% of all postings with wage data in the BGT database occur in an Amazon or Whole Foods County. Walmart advertised in over 1,400 counties, covering XX% of postings with valid wage data in BGT.

We estimate the following event history model using wage data on job postings by non-policy firms in policy-firm counties:

$$\log w_{jofct} = \alpha + \sum_{k=1}^{T} \beta_k D_{f,t-1} \times \lambda_{t+k} + \delta_f + \delta_c + \delta_o \times \delta_t + \varepsilon_{jofct} \tag{1}$$

The outcome variable is the posted log hourly wage for a specific job j at non-policy firm f in county c at time t. We include indicator variables for employer, county, and year, as well as a vector of observable characteristics associated with that job that may be correlated with the posted wage (e.g., skill level, and indicators for occupation and industry). λ_t is a set of calendar month dummy variable indicating calendar months from k=-12 (the beginning of our observation period for a given reform) to k=+11 (the end of our observation period). The dummy variable for k=0 is omitted for the model to be identified. δ_f are employer fixed effects. We include occupation-by-calendar-month fixed effects, $\delta_o \times \delta_t$, control for changes in the composition of jobs posted over the time frame.[19]

The coefficients of interest are the β_k which explains how exposure to the policy at time $t = k$ affects posted wages for non-policy job j.

[19] Results are nearly identical excluding these occupation-by-month shocks. In the lead-up to the Amazon policy change, treated counties experienced an increase in the number of ads lower wage occupations. Including occupation-by-month fixed effects controls for this shift in the occupation composition of jobs in the pre-period. See Figure **??** for the results without the occupation-by-month fixed effects.

Identifying assumption and proposed validity tests The main identifying assumption is that the treatment variable—the fraction of an employer's pre-period posted wages that are below the policy firm's new minimum wage—is uncorrelated other factors affecting wages over time. Parallel trends provide corroborating evidence of this assumption, as does the evidence that wages in the post-treatment period bunch at the new minimum wage set at the policy firm.

Difference-in-difference analysis In further analysis, including in a series of robustness checks, we pool the post-treatment (pre-treatment) months into one post-treatment (pre-treatment) period to increase our power. In these analyses, we run the following specification:

$$Y_{jofct} = \alpha + \tilde{\beta} D_{f,t-1} \times \text{Post} + \delta_f + \delta_c + \delta_o \times \delta_t + \varepsilon_{jofct} \tag{2}$$

where Y_{jofct} is the outcome of interest, including log hourly wages as well as indicators for wages falling within specific wage bins.

5 Results

We first present results separately for each of the Amazon/Whole Foods and Walmart experiments. We then stack the four experiments together to gain power and test robustness to a series of alternative explanations, including contemporaneous shocks to wages in policy firm counties, as well as to particular industries. Our baseline specification already accounts for any shocks to occupations advertised over the period analyzed.

5.1 Amazon

We observe substantial spillovers to other employers resulting from Amazon's $15 minimum wage policy. Figure 2 plots β_k from estimating equation 1 and shows that starting exactly in October 2018, the month of Amazon's announcement, employers with greater exposure to Amazon's policy boosted their own posted hourly wages. An increase in exposure from 0 to 100% ("more exposed") is associated with just over a 4 log point increase in posted wages immediately post treatment. The effect grows stronger over the following months, rising to just under 8 log points by April 2019. In the pre-period, our exposure variable shows little significant co-movement with wages in the year prior to treatment.[20]

[20] If anything, exposure to Amazon's policy change is correlated with a slight downward trend in wages at the end of 2017, a year prior to treatment. This slight downward trend is consistent with either an omitted correlate driving down wages in the early pre-period or reversion to the mean in the later pre-

Figure 3 plots coefficients $\tilde{\beta}$ from regression equation 2 where the outcome variables are indicators for hourly wages falling within a specific wage bin. The figure shows bunching in the post-treatment wage distribution of non-policy employers at $15 an hour. The probability of wages being exactly $15 has the highest estimated increase, at 10 percentage points, with smaller but statistically significant effects up to $18 and at $20. For wages below $15, the largest drop comes wages that were at $10 prior to treatment—of 5 percentage points—with significant drops from $9 to $14 dollars.[21]

This additional evidence of wages bunching at exactly $15 suggests that it is Amazon's policy driving non-Amazon employers' posted wages up rather than an unrelated demand shock. Still, in Table 1, we explore robustness to a number of alternative hypotheses. The table reports $\tilde{\beta}$, the coefficient on employer exposure to Amazon's policy change interacted with an indicator for the post-period. Column 1 is our baseline specification, which includes occupation-by-calendar-month fixed effects that control for changes in the composition of jobs advertised for by the exposed firms. Column 2 tests sensitivity to not including these time-varying occupation controls; the coefficient on exposure interacted with Post is nearly identical, in both magnitude and precision. This suggests that employers are not systematically shifting the composition of jobs in response to the policy change, which could bias estimates of $\tilde{\beta}$. In column 3, we include county-by-calendar-month fixed effects to absorb any county-level labor demand shocks or policy changes that may be driving the increase in non-Amazon employer wages after October 2018. Once again, our estimated impact of Amazon's policy is nearly identical.

Amazon's policy to raise their minimum wage may also affect the posting behavior of firms. For example, firms may have had higher paying hourly jobs but were not including the wages for these jobs on their ads (only about 17% of online vacancies post wages, according to BGT's data). We conduct two additional analyses to determine that changes in the wage posting behavior of firms are not driving the results reported in Figure 2. First in Column 4 of Table 1, we directly include the share of ads that include wages for each employer in the regression to see how this affects our estimated coefficient $\tilde{\beta}$. Directly including the wage posting probability in the regression has no effect on the magnitude or precision of the estimate of Amazon's policy's impact.[22]

period and early post-period. However, the sharpness of the jump in wages at the time of treatment as well as the magnitude of the effect, which increases over time, suggests the treatment effect is not simply driven by mean reversion.

[21] Drops at $10 and increases at $20 are consistent with evidence from Dube et al. (2017) that employers tend to set wages at round numbers, suggestive of both employer mis-optimization and wage setting power in labor markets.

[22] Note that to the extent that changes in wage posting may be a secondary outcome of Amazon's policy, including this measure as a control may be a mis-specification. We directly look at the impact of the policy on wage posting behavior in Appendix Figure A4. It appears more exposed employer's do gradually increase their tendency to post wages on advertised jobs, perhaps wishing to signal the presence of higher wage jobs. However, this change in posting behavior is delayed compared to the change in the

We conduct a second analysis to verify impacts on hourly wages stem from real changes in the hourly wage and not just employer posting behavior. We estimate employer-level wage spillover effects using an entirely different data source: worker salary reports from Glassdoor. As described in Section 3.2, Glassdoor is a two-sided online jobs platform used by workers to search and evaluate jobs and by employers to recruit. Glassdoor contains workers' reports on their salary and time rate of pay at a given employer. We re-estimate equation 1 using log worker-reported hourly wages as the outcome, including the same set of baseline controls.[23] Figure 4 depicts the results from this analysis. Beginning exactly in November 2018, the month of implementation of Amazon's pay increase, workers' reported wages increase by around 6 log points. The effect persists and increases slightly to about 8 log points by the end of the post period. These results are remarkably consistent with the increase in advertised wages found using BGT data and confirm that changes in advertised wages resulted in changes in wages workers reported receiving starting in November 2018.

We can use our estimates of Amazon's effects on other employers' posted wages to calculate a cross-employer wage setting elasticity. We define this elasticity as follows:

$$\text{Cross-employer wage elasticity} = \frac{\%\Delta \text{w}^{\text{non-policy firm}}}{\%\Delta \text{w}^{\text{policy firm}}}$$

We first calculate the average increase in Amazon's posted wages from before to after October 2018. Second, to calculate the average increase in posted wages for non-policy firm employers, we multiply the average employer's exposure to each policy change by $\tilde{\beta}$, the coefficient on $D_{f,t-1} \times \text{Post}$, as this represents the average employer's wage increase. For Amazon's policy change, the cross-employer wage elasticity is 0.25: in other words, for a 1% increase in Amazon's wages, non-Amazon employers increase their wage by 0.25%. As a comparison, Staiger et al. (2010) estimate a cross-employer spillovers in the context of the Department of Veterans Affairs hospitals changing their wage policy and find elasticities ranging from .19 to .28.[24] An alternative benchmark is Hjort et al. (2019)'s estimate of cross establishment spillovers in multinationals after an increase in the headquarter country's minimum wage: an elasticity of approximately 0.30.[25] Thus, our estimated elasticity is very similar to this previous estimate despite differences in

wage distribution of ads with posted wages. While wages increase immediately, the likelihood of posting a wage increases more slowly, beginning a couple months after the policy change.

[23]Glassdoor provides the city of the posting, as opposed to county provided in the BGT data. The analysis is restricted to cities with Amazon distribution centers present.

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

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

institutional context and industry.

Moderators of spillover effects What drives or mediates the transmission of wage policies across employers? We test the role of potential mechanisms by examining interactions between local moderating factors and the our treatment variables, $D_{f,t-1} \times \mathrm{X}_{c,t}\mathrm{Post}$. Table 2 provides initial evidence. Labor market tightness as measured by the unemployment rate moderates transmission of wage policies. However, the interaction effect is small, leaving room for other local factors to determine the extent of wage spillovers.

5.2 Walmart

Several other employers have implemented voluntary minimum wages, both before and after Amazon's policy. Walmart, the largest employer in the US with a workforce of 1.5 million, has implemented 3 company-wide minimum wage policies since 2015, which are detailed in Section 2. At nearly twice the size of Amazon's workforce, Walmart's wage policies are likely to have had ripple effects on other low wage employers.

We estimate the spillover effects; Figures 5, 6, and 7 show the impacts on other employers of the company's \$9, \$10, and \$11 minimum wage experiments, respectively. Our empirical strategy is identical to the one outlined above and in equation 1. Our baseline again includes employer and county fixed effects as well as occupation-by-month fixed effects. In response to Walmart's \$9 minimum wage policy, non-Walmart firms that were more exposed to the policy increased posted hourly wages by 5 log points immediately up to 10 log points one year after the announcement. The estimates are noisier compared to the spillovers from Amazon's minimum wage, partly due to the fact that non-Walmart employer exposure is low: for the average employer, only 3.2% of their advertised wages were below \$9.

Exposure and precision both increase in the next experiment. 12% of the average employer's postings were below \$10 in 2015, and Walmart's \$10 minimum wage resulted in a 5 log point increase for more exposed employers, with effects remaining relatively stable over the post-treatment period. It should be noted that the 2016 increase was a scheduled increase announced in 2015 at the time of the \$9 increase. Thus, the 2016 increase was likely anticipated by more employers, potentially explaining the upward trend in the treatment effect after the \$9 increase.

In January 2018, Walmart announced a third increase to \$11. The impact of Walmart's \$11 minimum wage on other employers in the same county was large. First, for the average employer, a greater fraction of pre-period jobs paid below the new minimum wage compared to prior experiments (15%). second, the spillovers to more exposed employers were substantial: posted hourly wages for non-Walmart employers increased by

5 log points immediately in January 2018, with this effect increasing to approximately 12.5 log points by the end of the post-treatment period.

5.3 Stacked event study

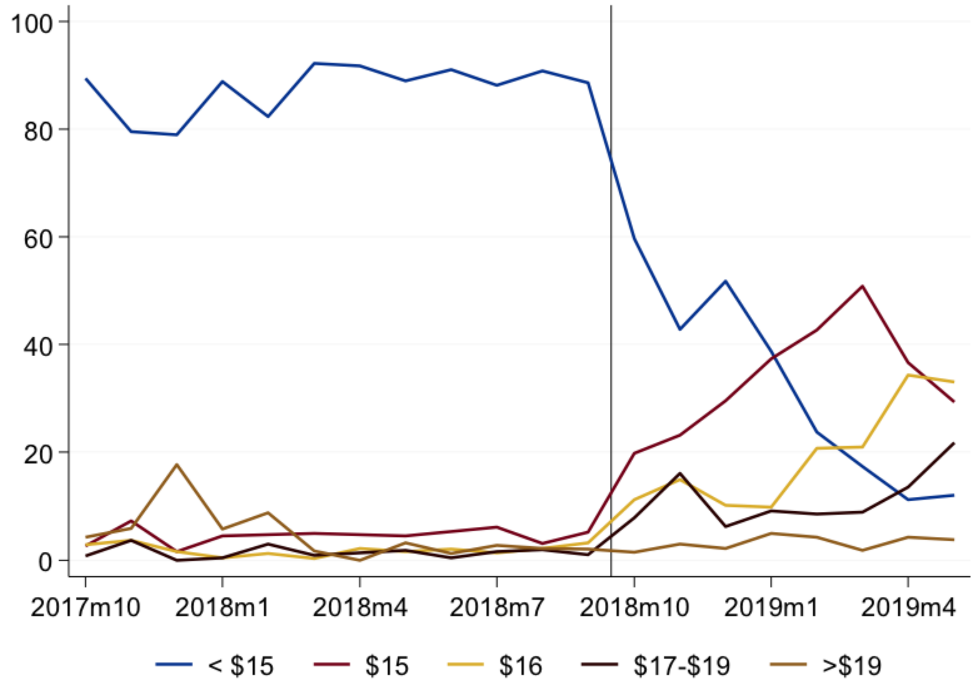
We pool together all four experiments in a stacked event study plot to increase our power and estimate an average spillover effect. In this stacked analysis, time is measured in months from the policy firm's announcement; pre-trend (post-treatment) estimates up to a year prior to (after) treatment are shown. Figure 8 depicts the results from this analysis. Pooling the experiments reduces noise in the estimates, resulting in a flatter pre-trend and a more precisely estimated treatment effect. We find that posted wages at non-policy firms increase 5 log points in the immediate month post-treatment and over the following 12 months increases to 10 log points.

We have obtained data from the National Employment Law Project ("NELP") on over 100 voluntary employer minimum wage increases between 2014 and 2020. In future work, we intend to pool the full set of these policy changes together to describe average and aggregate effects of these recent employer wage setting shocks. Appendix Figure A5 describes these recent increases.

6 Conclusion

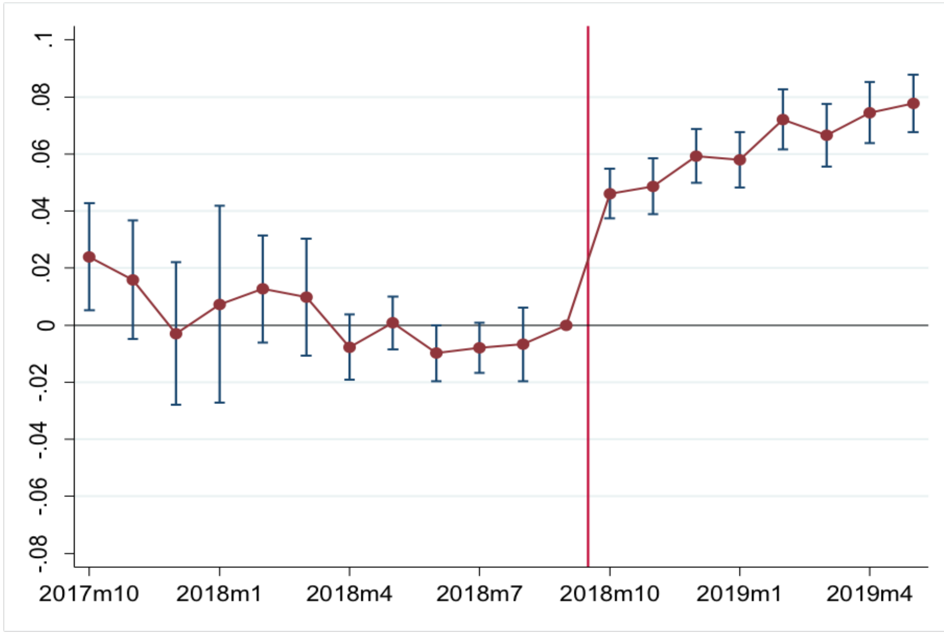
This study examined wage spillover effects from recent wage policy changes by large low-wage employers, Amazon and Walmart. We use data on online vacancy postings as well as data from job search platforms to document evidence of wage policy changes at these employers and estimate broader spillover effects in the labor market. Using a measure of the exposure of other employers operating in the same labor market, we estimate substantial spillover effects of both Amazon and Walmart's policies. In the case of Amazon, which raised its minimum wage to $15 in 2018, the cross-employer wage elasticity is approximately 0.25. Our estimates provide insight into the wage setting power of large employers, and future work investigates the local labor market moderators of this substantial cross-employer wage policy transmission.

Figure 1: Percentage of Amazon job ads below or above $15, 2017-2019



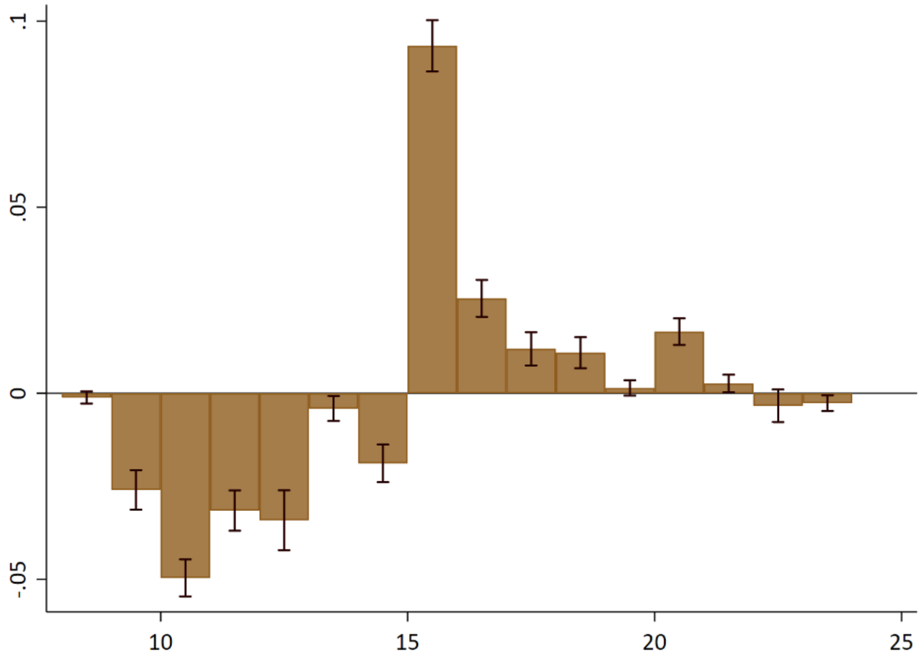
Notes: Percentage of Amazon online job ads with hourly wages below $15, at $15, at $16, between $17 and $19, and above $19. Sample restricted to postings with valid wage data and hourly rate of pay, employer name, county, and occupation. Whole Foods was acquired by Amazon in August 2017 and is included in the sample. *Source*: Burning Glass Technologies online vacancy data.

Figure 2: Spillovers from Amazon's $15 minimum wage, 2018



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

Figure 3: Amazon spillovers concentrated at $15

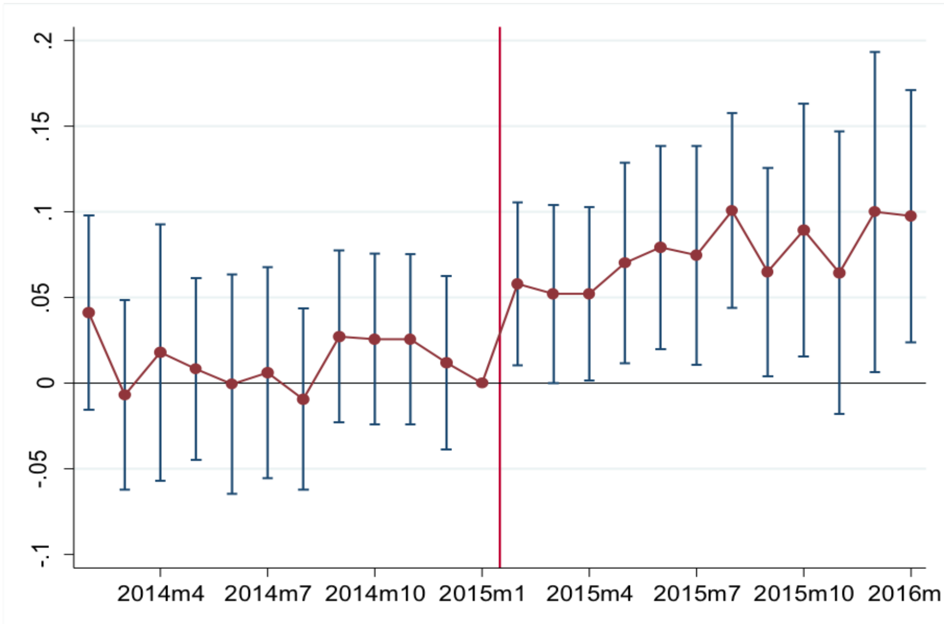


Notes: This figure plots the coefficients from linear probability regressions of hourly wages being in a given wage bin on the interaction between exposure to Amazon's policy and an indicator for post-October-2018. Exposure is defined as the fraction of each non-Amazon employer's job postings with wages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

Figure 4: Spillovers from Amazon's MW in worker reported wages, 2018

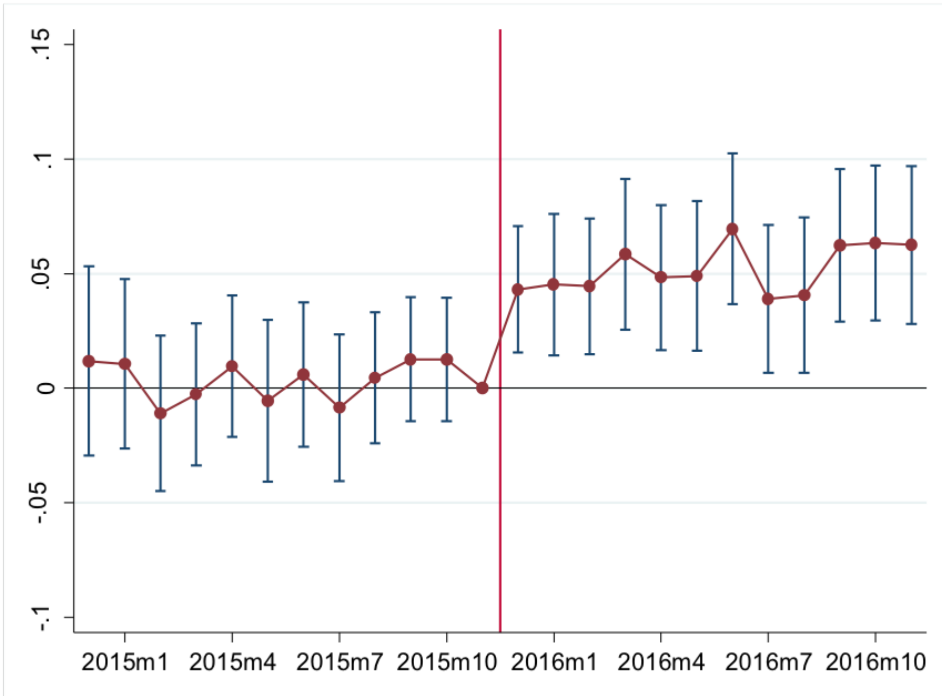
Notes: This figure plots the coefficients on the interaction between exposure to Amazon's minimum wage policy and month fixed effects, where the dependent variable is log reported hourly wage by workers at non-Amazon employers. Exposure is defined as the fraction of each non-Amazon employer's job postings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

Figure 5: Spillovers from Walmart's $9 minimum wage, 2015



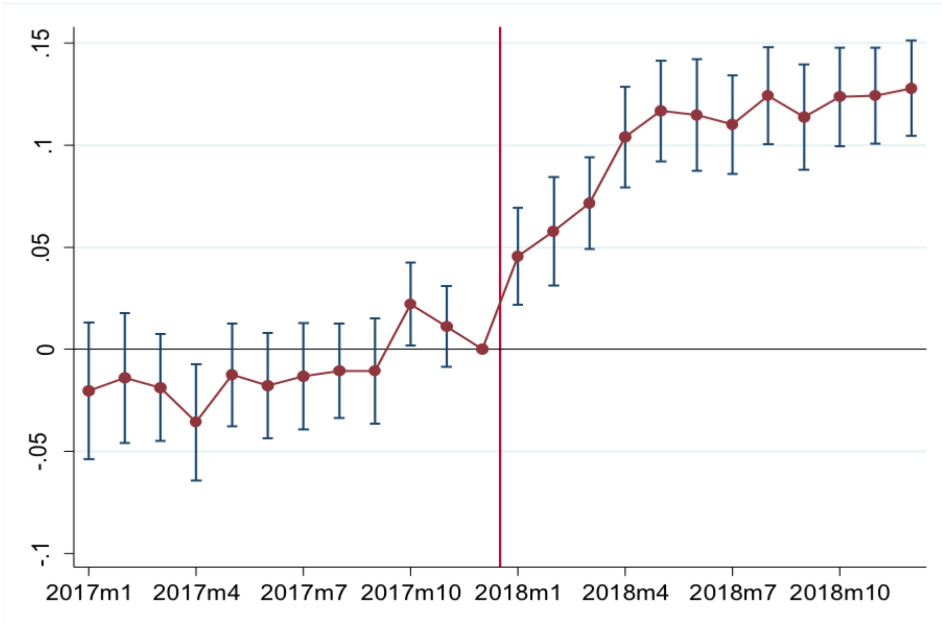
Notes: This figure plots the coefficients on the interaction between exposure to Walmart's 2015 $9 minimum wage policy and month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of each non-Walmart employer's job postings with wages below $9 in the year before treatment. Employer, county, and month-by-occupation fixed effects are included. Sample restricted to non-Walmart employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure 6: Spillovers from Walmart's $10 minimum wage, 2016



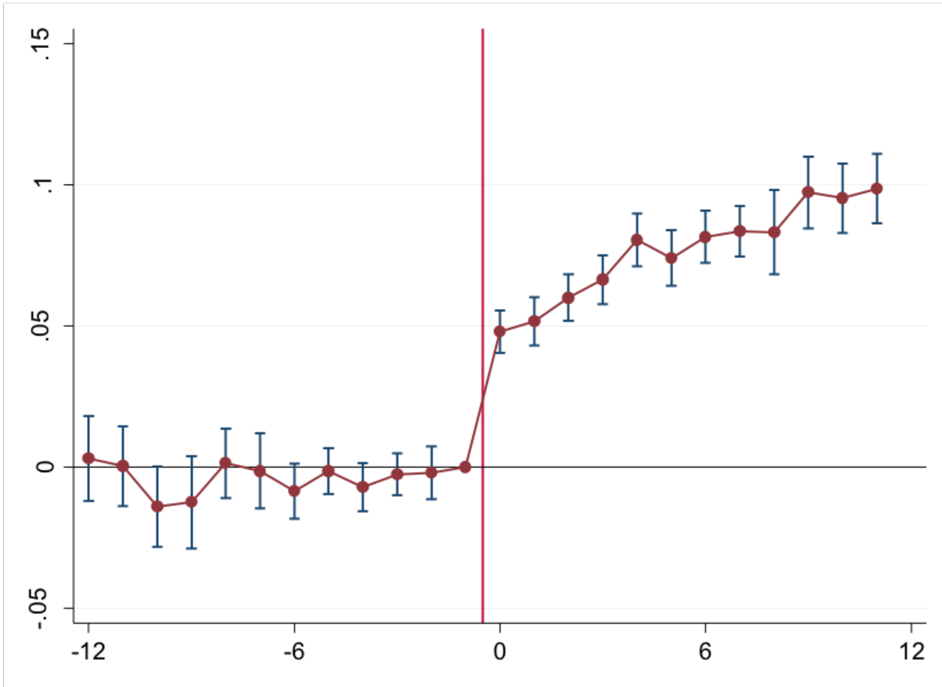
Notes: This figure plots the coefficients on the interaction between exposure to Walmart's 2016 $10 minimum wage policy and month fixed effects, where the dependent variable is log posted hourly wage. Exposure is defined as the fraction of each non-Walmart employer's job postings with wages below $10 in the year before treatment. Employer, county, and month-by-occupation fixed effects are included. Sample restricted to non-Walmart employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure 7: Spillovers from Walmart's \$11 minimum wage, 2018



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

Figure 8: Stacked event study: spillovers from employer MWs, 2015-2018



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

Table 1: Wage spillovers: robustness checks

Dependent variable: Log hourly wage				
Frac. Affected x Post	0.065***	0.073***	0.066***	0.065***
	(0.003)	(0.004)	(0.003)	(0.003)
Frac. postings with wage				0.002
				(0.003)
Obs	1,964,468	1,965,817	1,964,308	1,964,468
Employer FE	Y	Y	Y	Y
Occ. X Month FE	Y	N	Y	Y
County X Month FE	N	N	Y	N

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

Table 2: Wage spillovers: interaction with unemployment rate

Dependent variable: Log hourly wage			
Frac. Affected × Post	0.065***	0.064***	0.070***
	(0.003)	(0.003)	(0.004)
Unemp. Rate		-0.011***	-0.010***
		(0.001)	(0.001)
Frac. Affected × Unemp. Rate × Post			-0.002*
			(0.001)
Obs	1,964,468	1,964,468	1,964,468
Employer FE	Y	Y	Y
Occ. X Month FE	Y	Y	Y

Sample: Job vacancies with valid wage data for hourly jobs. Restricted to counties where Amazon advertised in the year before the policy change. Winsorized at the 5% level.
Notes: The outcome variable is log posted hourly wage. The unemployment rate is measured at the county level. Standard errors are clustered at the employer level. *Source*: Burning Glass Technologies online vacancy data and BLS Local Area Unemployment Statistics.

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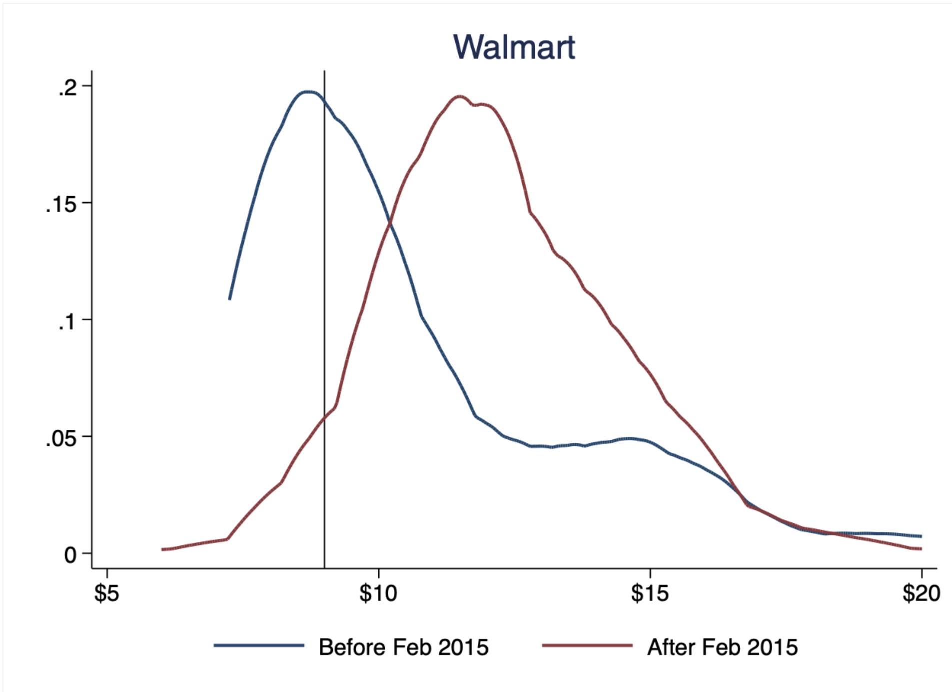
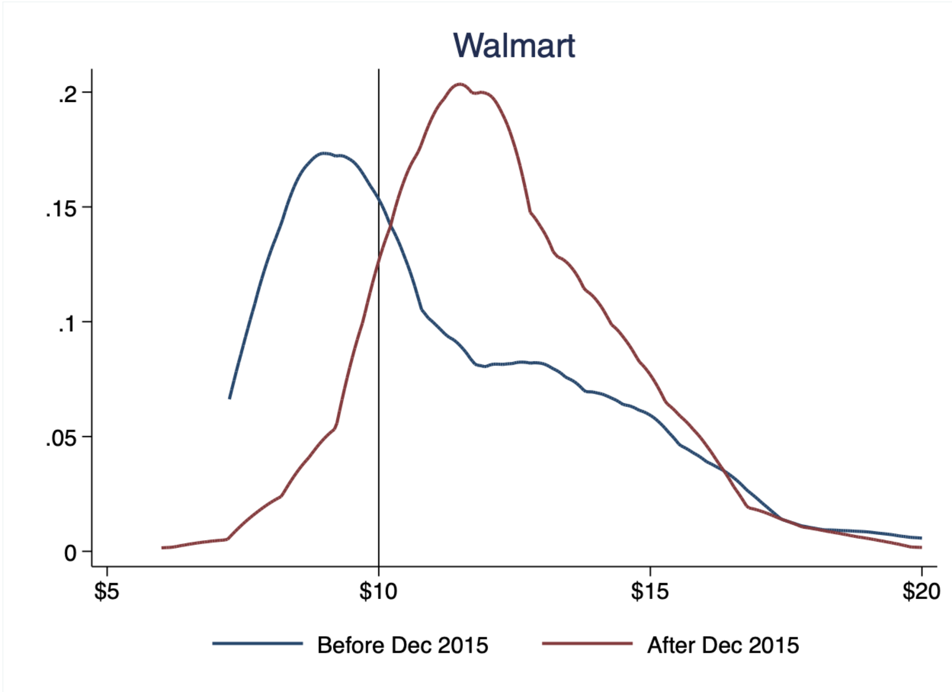
Appendix A Additional evidence on voluntary employer minimum wage increases

Figure A1: Shift in Walmart's wage distribution after 2015 $9 MW



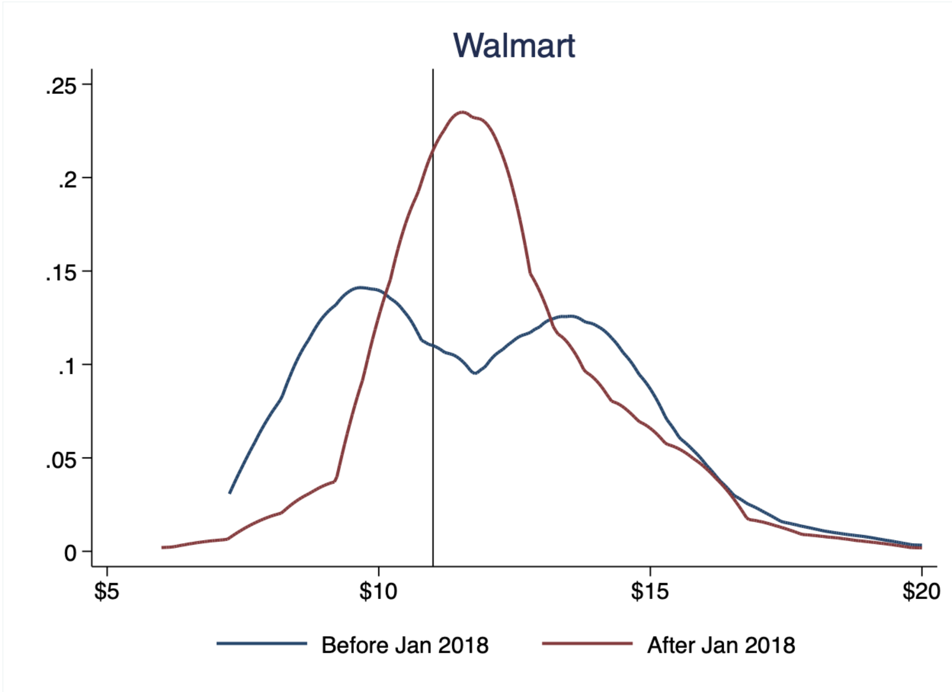
Notes: Kernel density plots of Walmart's advertised hourly wages before and after January 2015. Sample restricted to postings with valid wage data and hourly rate of pay, employer name, county, and occupation. *Source*: Burning Glass Technologies online vacancy data.

Figure A2: Shift in Walmart's wage distribution after 2015 $10 MW



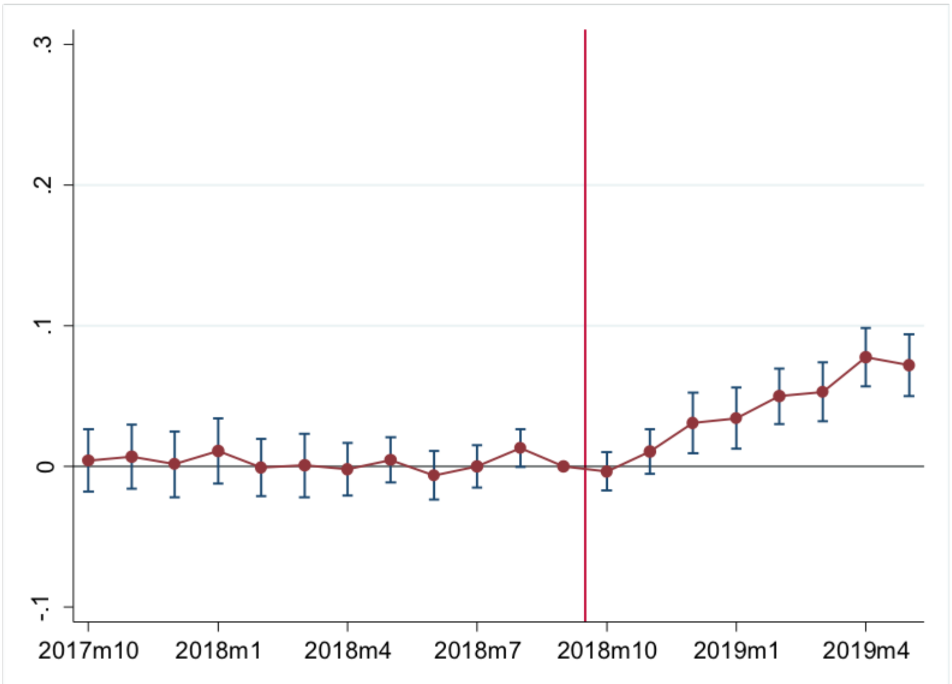
Notes: Kernel density plots of Walmart's advertised hourly wages before and after December 2015. Sample restricted to postings with valid wage data and hourly rate of pay, employer name, county, and occupation. *Source*: Burning Glass Technologies online vacancy data.

Figure A3: Shift in Walmart's wage distribution after 2015 $11 MW



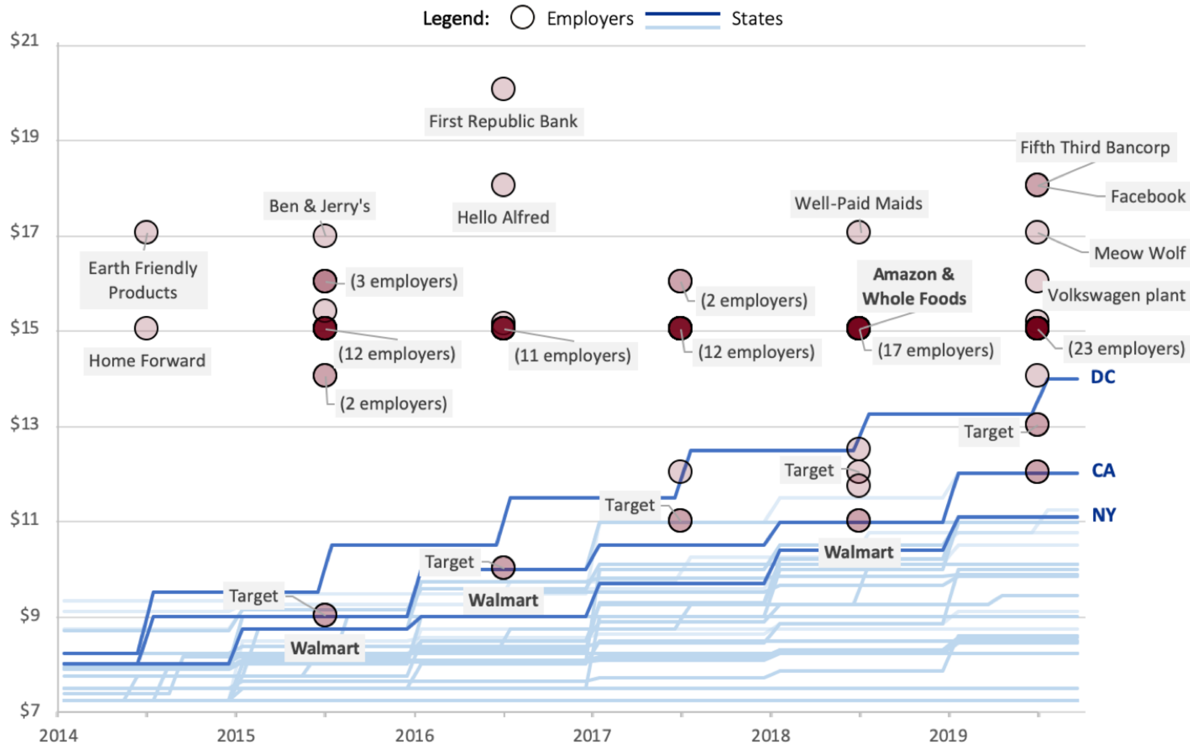
Notes: Kernel density plots of Walmart's advertised hourly wages before and after January 2018. Sample restricted to postings with valid wage data and hourly rate of pay, employer name, county, and occupation. *Source*: Burning Glass Technologies online vacancy data.

Figure A4: Changes in wage posting behavior after Amazon's MW



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 an indicator for posting the wage on a given job ad. Exposure is defined as the fraction of each non-Amazon employer's job postings with wages below $15 in the year before treatment. Employer, county, and month fixed effects are included. Sample restricted to non-Amazon employers' postings with valid wage data and hourly rate of pay, employer name, county, and occupation. 95% confidence intervals shown. *Source*: Burning Glass Technologies online vacancy data.

Figure A5: Employer minimum wage increases in the US, 2014-2019



Notes: This figure plots voluntary employer minimum wage increases that have taken place in the US between 2014 and 2019. The lines in blue indicate state minimum wages above the federal minimum wage of $7.25. *Source*: National Employment Law project and authors' research.