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DISCUSSION PAPER SERIES

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ABSTRACT

The Impact of Right-to-Work Laws on Long Hours and Work Schedules

Unions play a crucial role in determining wages and employment outcomes. However, union bargaining power may also have important effects on non-pecuniary working conditions. We study the effects of right-to-work laws, which removed agency shop protection and weakened union powers on long hours and non-standard work schedules that may adversely affect workers' health and safety. We exploit variation in the timing of enactment across US states and compare workers in bordering counties across adopting states and states that did not adopt the laws yet. Using the stacked approach to difference-in-differences estimates proposed by Cengiz et al. (2019), we find evidence that right-to-work laws increased the share of workers working long hours by 6%, while there is little evidence of an impact on hourly wages. The effects on long hours are larger in more unionized sectors (i.e. construction, manufacturing, and transportation). While the likelihood of working non-standard hours increases for particular sectors (education and public administration), there is no evidence of a significant increase in the overall sample.

JEL Classification: J50, I10

Keywords: unions, working conditions, workers' health

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1 Introduction

The role of unions in labor markets has been widely studied with a renewed interest in the last few years (Farber et al., 2021; Farber, 1986; Derenoncourt et al., 2021; Parolin, 2021; Artz et al., 2021; Barth et al., 2020; Callaway and Collins, 2018; Card et al., 2020; Frandsen, 2021). Most work focuses on the role unions have in determining wages, employment outcomes, and income inequality. However, union bargaining power may also play an important role in shaping non-pecuniary working conditions such as work schedules, which have been shown to importantly affect workers' wellbeing (Cassar and Meier, 2018; Mas and Pallais, 2017; Earle and Pencavel, 1990). Workers may have limited control over the number of hours they work (Lewis, 1969), and because of frictions in the labor market and limited competition, firms may set worse working conditions without adequate monetary compensation (Ashenfelter et al., 2021; Manning, 2021; Lang and Majumdar, 2004; Altonji and Paxson, 1986).

Labor union contracts help create working hour limits (Hagedorn et al., 2016). These protections are important in preventing overwork and long working hours (Pega et al., 2021; Dembe, 2009; Finnigan and Hale, 2018). The average number of hours worked declined throughout the 20th century, but stabilized over the last few decades and never approached the fifteen hour work week famously predicted by Keynes (Keynes, 2010). Though in the United States working hours exhibited a gradual decline since the 1950s, they fell by a smaller degree compared to other developed countries (Dolton, 2018). In fact, individuals in the US and the UK tend to work longer hours compared to countries where union policies and institutions limit the number of hours worked, such as France and Germany (Freeman, 2008). Over the same period, union membership in the US decreased substantially: in 1953, 35.7% of private sectors belonged to unions; by 2015, this number was 6.7%. In the public sector, overall union membership is still at 33.1%.[1] A natural question to ask is how this decline in the number and strength of unions affected non-pecuniary working conditions.

To estimate the causal effects of union strength on these working conditions, we explore

[1] https://www.bls.gov/news.release/pdf/union2.pdf.

differences in the enactment of state-level right-to-work (RTW) laws, which significantly weaken unions by removing agency shop protections[2]. The main contribution of this study is to analyze the effects of right-to-work laws which removed agency shop protection and weakened union powers on long hours and non-standard work schedules.

Previous scholars called for more research analyzing the mechanisms and links between unions and working conditions that may affect workers' health and wellbeing (Malinowski et al., 2015). However, and somewhat surprisingly, there is only limited causal evidence on the effects of unions on non-pecuniary working conditions (Zoorob, 2018; Sojourner and Yang, 2022). To the best of our knowledge, this is the first study analyzing the impact of RTW laws on long hours and work schedules. Furthermore, we provide new evidence on the effects of RTW laws on hourly wages and union coverage using the stacked approach to difference-in-differences estimates proposed by Cengiz et al. (2019).

We relate to a voluminous literature investigating the effects of labor unions on wages (Card, 1996; Borjas, 1979; Fortin et al., 2023), employment (Boal and Pencavel, 1994), inequality (Card et al., 2004) and wellbeing (Blanchflower and Bryson, 2020). Moreover, previous work indicated that unionization increases the prevalence of premium pay for overtime and reduces the incidence and extent of overtime hours (Trejo, 1993). Furthermore, our work is closely related to set of studies analyzing the effects of RTW laws on unions' bargaining power, union organizing, and union revenues (Ellwood and Fine, 1987; Eren and Ozbeklik, 2016; Matsa, 2010; Quinby, 2017), labor market outcomes (Holmes, 1998; Reed, 2003; Kalenkoski and Lacombe, 2006; Fortin et al., 2023; Chava et al., 2020a), politics and policy (Feigenbaum et al., 2019), and workers' wellbeing (Makridis, 2019).

Previous studies focused on the effects of RTW laws on the union wage premium, employment and wages, union density, and voting outcomes. Following Holmes (1998) and Feigenbaum et al. (2019), we study pairs of bordering counties where one county is in a RTW state and the other is not. We show that these counties exhibit similar trends and

[2]Under an "agency shop" arrangement, employees must pay union dues before being allowed to work.

levels in our main outcomes of interests and our main covariates before the laws were passed. Our estimates identify the reduced form effect of RTW laws. As proposed by Cengiz et al. (2019), we construct a stacked dataset to conduct our difference-in-difference and event study estimates. The event study analysis documents a clear increase in the share of individuals working long hours in the years following the adoption of RTW laws. We find that RTW increased the share of full-time employees working more than 45 hours by approximately 6% with respect to the mean. The effects are similar when considering other definitions of long hours. Generally, the effects are larger in more unionized sectors (10.9%). Turning to examine a continuous measure of working hours, we show that the average number of hours, if anything, increased by 0.5%. While this estimate is not statistically significant in the overall sample, there is a statistically significant increase when focusing on manufacturing (+1%) and transportation (+1.4%). There is also evidence of a 1% increase in average hours worked among blue-collar workers.

We also estimate the effects of RTW laws on hourly wages and on union coverage which were previously explored in the literature. While point-estimates are not precisely estimated, hourly wages declined after the adoption of RTW laws. Furthermore, there is evidence that RTW laws led to a decline in the hourly wage in the manufacturing sector (-3.2%). This decline in hourly wages may partially contribute to the increase in hours worked as the result of an income effect (Giupponi, 2019; Golosov et al., 2021; Boppart and Krusell, 2020). Consistent with Fortin et al. (2023), we find that RTW laws reduced union coverage (-7.7%).

We assess the sensitivity of our results on long hours by conducting several robustness checks. First, we report the average treatment effects obtained using the alternative estimands proposed by Callaway and Sant'Anna (2021) and Borusyak et al. (2023). Second, we report results obtained using the standard two-way fixed effects model and analyzing the entire sample of counties, without restricting to bordering counties. Third, we show the results obtained using a permutation exercise on the standard difference-in-difference approach. Overall, while there are some differences in the magnitude of the estimates, confidence intervals largely overlap, and these sensitivity analyses confirm our main finding that RTW laws led to an increase in long hours.

We then turn to analyze the effects of RTW laws on work schedules. Non-standard schedules have also been linked to increased workers' health risks and reduced workers' wellbeing (Strazdins et al., 2004; Presser, 2005; Costa, 2003). Unfortunately, information on schedules is limited and noisy in our main data, and our results are somewhat mixed. If anything, RTW laws led to an increase in the share of individuals working non-standard schedules. However, this result is not precisely estimated in the main sample and only significant in two of the most highly unionized sectors: the education (+29.5%) and public administration sector (+15.7%). We also find evidence of significant effects among Blacks (+12.8%), Hispanics (+22%) and younger workers (+7%). The event study analysis on non-standard schedules does not reveal any clear change in the overall trend after the adoption of RTW laws. Taken together, while we find that RTW laws led to a significant effect on long hours, the evidence on work schedules is more mixed and sensitive to the method of analysis.

This paper is organized as follows. Section 2 discusses RTW laws and previous literature on their effects. Section 3 describes the data and empirical specification. Section 4 presents and discusses our results. And, Section 5 concludes.

2 Background

In 1947, the Taft-Hartley Act amended the National Labor Relations Act, thereby allowing states to supersede union security agreements with the adoption of RTW laws. In states that pass RTW laws, agency shop arrangements become illegal. While unions have strongly pushed back against these laws, 27 states have enacted RTW laws since the Taft-Hartley Act. After Congress approved the Act, several states in the South quickly introduced RTW laws and other states soon followed (Table A.1 and Figure A.1). RTW laws are comparable across states from a legal perspective, and courts have usually interpreted them as applying with equal force to public and private sector unions alike.

Previous studies mostly focus on the impact of these laws on union density and activity, as well as on their effects on employment and wages. There is overall agreement that RTW laws weakened unions, which reduced union organizing capacity and density, and therefore labor's overall leverage (Ellwood and Fine, 1987; Eren and Ozbeklik, 2016; Matsa, 2010; Fortin et al., 2023). Using state-level longitudinal data, Ellwood and Fine (1987) finds that in the first five years following the passage of an RTW law in a state, union organizing is reduced by 46% and this reduction persists for later years albeit to a lesser extent. Using an event-study analysis, Fortin et al. (2023) find that the negative effect of RTW on unionization rate gradually increases from -0.017 in the initial year of adoption to -0.040 after five years. There is also some evidence that RTW laws reduced union revenues. Using teacher-level personnel record data and exploiting a ban on collective bargaining in Tennessee public schools, Quinby (2017) finds that teachers' unions suffered a rapid loss of revenue, inhibiting union lobbying activity.

Results are instead more mixed when it comes to the effects on employment and wages. Holmes (1998) exploits county variation on different sides of state borders to show that RTW laws led to a higher growth in manufacturing activity. Consistent with this evidence, Reed (2003) and Kalenkoski and Lacombe (2006) find evidence of positive effects of RTW laws on wages and employment. At the same time, Eren and Ozbeklik (2016)–who focus on the more recent adoption of RTW laws in Oklahoma–find evidence of a decline in union density but no significant effect on employment and wages. Quinby (2017) finds that the ban on collective bargaining enhanced teacher employment but reduced compensation growth. More recently, using RTW as an instrument to estimate the effect of unions on wages, Fortin et al. (2023) show that unions increase wages. Finally, trying to estimate the effect on wellbeing, Makridis (2019) finds evidence that RTW laws, if anything, increased individuals' life satisfaction and economic sentiment.

Previous studies suggest that unionized workplaces are more likely to receive health and safety inspections and that the presence of unions may lead employers to improve workplace safety (Weil, 1991; Li et al., 2017). A few other scholars have highlighted how unions play a crucial role in contributing to workers' health and workplace safety (Wright, 2016). Hagedorn et al. (2016) discuss how labor union contracts create working hours limits and workplace hazard protections that can ultimately improve the health and wellbeing of workers. There is instead surprisingly little evidence on the effects of RTW laws on working conditions. One exception is a recent study by Zoorob (2018), who finds evidence that RTW laws increased the rate of occupational fatalities by 14% through decreased unionization.

Our paper contributes to the literature by analyzing the effects of RTW laws on long hours and non-standard schedules. Earle and Pencavel (1990) underline the importance of trade unions in the setting of working hours limits and restrictions. There are several mechanisms through which RTW laws may affect long hours. First of all, if RTW laws reduce union bargaining power (Chava et al., 2020b), then unions may be less successful in retaining hours protections in subsequent contracts. If so, these effects may take some time to materialize. Second, RTW laws may affect the probability of new union organizing (Ellwood and Fine, 1987), expanding the size of the non-union sector. Third, RTW laws may reduce the threat effect of unions, impacting the contractual arrangements in the non-unionized sector (Fortin et al., 2021, 2023). While our reduced-form approach does not allows us to isolate a specific mechanism through which RTW laws may have affected our outcomes of interest, our results provide some suggestive insights. Finally, we adopt a research design that addresses the concerns that recent work has highlighted with respect to variation in treatment timing (Roth et al., 2022).

3 Data and Empirical Specification

3.1 Data

Our main data are drawn from the American Community Survey (ACS) for the years 2005-2019 (Ruggles et al., 2022). Our sample period begins in 2005 as it is the first year in which the ACS collected data on a full one-percent sample of the US population, and the first year in which information is available on time of arrival at work used to construct one of our outcomes of interest. The sample period ends in 2019, which is the last year for which the survey data are available. Furthermore, as explained below, in our main identification strategy we restrict the sample to a relatively short window around the adoption of RTW laws. Designed as a replacement for the long form of the decennial census, ACS contains a detailed set of standard socio-demographic characteristics; labor market outcomes (e.g., employment, labor force participation, and hourly wage); and relevant information on respondents' home ownership, rental prices, and home characteristics.

The ACS includes information on working hours and the time at which an individual arrives at work which, we use to construct an indicator for individuals working non-standard schedules. Furthermore, the rich set of socio-demographic characteristics includes information on marital status and fertility, which we use to examine the potential effects of RTW on these outcomes.

Our main measure of working long hours is a dummy variable for working longer than 45 hours per week (>45 Hours). However, we show the robustness of our results to the use of alternative metrics (e.g., working more than 40 or 50 hours). While we do not have explicit information on work schedules from the ACS, we use information on the time of arrival at work to define an indicator for arriving at work between 5pm and 8am, which serves as a proxy for working a non-standard shift. We also construct similar indicators for arriving at work between 6pm and 8pm and arriving at work between 10pm and 5am.

3.2 Empirical Specification

The primary challenge to the identification of the causal effects of RTW laws is the endogeneity of RTW laws adoption: states that introduced RTW laws are likely different from non-RTW states along many dimensions that could be importantly correlated with our outcomes of interest and thus bias our estimates. To address concerns of unobservables that may be correlated with the adoption of RTW laws and our outcomes of interest, we restrict the analysis to pairs of bordering counties where treated counties are in a RTW state and control counties are in bordering states that did not adopt the RTW laws yet. In doing so, we follow the approached proposed by Holmes (1998) and Feigenbaum et al. (2019). The key idea underlying this strategy is that bordering counties should be more similar in both trends and levels before the adoption of RTW laws.

In our analysis using ACS data, the sample will be therefore comprised of counties on either side of a RTW border. We therefore have three different groups of counties: a group of counties that were never treated, a group of counties for which RTW was introduced during the period of analysis; and a group of counties that always had RTW in place. Our sample contains 295 counties throughout the period studied, with 26 counties within the sample located along state borders (see Table A.2 and Figures A.2-A.4). Feigenbaum et al. (2019) note how using border pair design limits researcher-degrees-of-freedom in constructing a counterfactual. At the same time focusing on bordering counties results may be attenuated by spillover effects. Furthermore, one may be concerned of the external validity of our results. For this reason we also report results conducted on the entire sample in the Appendix.

Figure A.5 shows that bordering counties of states that adopted RTW laws after 2011 were well-balanced before the adoption of the laws with respect to baseline covariates of bordering counties from states that would not adopt the RTW laws throughout the period studied. We also do not observe significant changes on these covariates in the period following the adoption (Figure A.6). This evidence supports our argument that any difference in working conditions arising after the adoption of RTW laws in RTW counties is more likely to be explained by the RTW laws and less likely to be affected by spurious factors. Thus, we interpret our estimates as the reduced form effects of RTW laws on long hours and work schedules.

Figures A.7-A.8 illustrate the differences between bordering counties and the rest of the country before and after 2011. Overall, bordering counties are fairly similar to the rest of the ACS sample. However, there is a marginally higher share of individuals aged 35-64 in bordering counties (Figure A.7) and a lower share of individuals under the age of 18 (Figure A.8).

To address the concerns raised by recent advances in difference-in-differences methods with multiple periods and variation in treatment timing, we followed Cengiz et al. (2019) and conducted difference-in-differences estimates and event studies while comparing only counties that switched status within the period of our analysis ("adopters") to counties that were not treated or not yet treated at the time of the switch ("non-adopters").

To do so, we constructed a stacked difference-in-differences dataset. An "event" is defined to be the adoption of RTW laws in a particular state (e.g., RTW was passed in Wisconsin in 2015). For each event, we include observations from the state of the event and excluded individuals from any state that would get treated within 7 years of the event. The control group for each event thus consists of individuals from "clean" states that will not be treated for more than 7 years Cengiz et al. (2019). We then combined datasets across events to build our stacked difference-in-differences dataset. For each event, we also restrict the analysis to the set of observations within the event study window (up to 7 years before and up to 7 years after the event)[3].

Using this dataset, we implement an event study approach to examine the effects of RTW on work hours and work schedules.

[3]In our event study estimation, our reference category consists of observations from the period immediately before treatment. We report coefficients from 6 years before to 6 years after treatment.

For the event study analysis, we estimate the following model:

$$
\begin{aligned}
y_{ist} =& \alpha + \sum_{k \in \{-6,\ldots,-2,0,1,\ldots,6\}} \beta_k \cdot 1\{t - \text{RTW year}_s = k\} \\
&+ \beta_{-7} \cdot 1\{t - \text{RTW year}_s \leq 7\} + \beta_7 \cdot 1\{t - \text{RTW year}_s \geq 7\} \\
&+ \Gamma X_{ist} + \delta_t + \delta_s + \varepsilon_{ist},
\end{aligned}
$$

where y_{ist} is an outcome reflecting long hours or non-standard schedules. Our main outcomes of interest are a set of dummy variables for whether an individual worked long hours for the week (i.e. >45 Hours), worked any non-standard schedule (i.e. Arriving between 5pm and 8am). But, we also re-examine effects on working hours and wages. RTW year_s is the year when RTW was passed in state s. X is a vector of covariates, which includes age, female dummy, marital status dummy, dummies for four education categories (no high-school, high-school, some college, and college and beyond), and dummies for three race categories (White, Black, and non-White Hispanic). All estimates include state-by-event (δ_s) and year-by-event fixed effects (δ_t). All regressions are weighted by household-level weights supplied by IPUMS and standard errors are clustered at the state level. When we utilize bootstrapped standard errors, the significance of our results remain largely unchanged.

We complement the event study graphical analysis with two-way fixed effect estimates using the stacked dataset. Formally, we estimate the following model:

$$
y_{ist} = \alpha + \beta(\text{RTW}_{st}) + \Gamma X_{ist} + \delta_s + \delta_t + \varepsilon_{ist},
$$

where y_{ist} is the outcome of interest. RTW_{st} is an indicator that is equal to 1 if the individual's state of residence has passed right-to-work laws in year t or before. This specification's covariates, fixed effects, weighting, and error clustering are as defined in our event study. As a further robustness check, we assess the sensitivity of our main results to the use of the alternative estimands proposed by Callaway and Sant'Anna (2021) and Borusyak et al. (2023).

Furthermore, we report results obtained using a permutation exercise, which compares our estimate to a distribution of counterfactual estimates obtained by randomly assigning RTW law adoption dates.

Further sample restrictions are as follows. For our analysis on long hours and non-standard schedules using the ACS data, we include non-immigrant individuals aged between 25 to 64 who are employed in a full-time job and report a non-missing usual work arrival time.[4] In the Appendix, we show the results are robust when extending the analysis to include individuals aged between 20 and 64 years old.

4 Results

4.1 The Effects of RTW Laws on Long Hours

Figure 1 illustrates the impact of RTW laws in our event study framework using the approach proposed by Cengiz et al. (2019) and restricting our sample to bordering counties. The graph focuses on the likelihood of working more than 45 hours per week. There is no evidence of a pre-trend and we observe a clear positive effect of the adoption of RTW laws on the likelihood of working more than 45 hours. In particular, one year after the adoption of RTW laws, we find an average increase by 0.5 percentage points in the share of individuals working more than 45 hours (not precisely estimated), which further increases with time to almost a 2 percentage point statistically significant difference with respect to the year before the adoption. Figure A.9 shows how these results are driven by a decline in the share of workers reporting to work 31-40 hours mirrored by the parallel increase in the share reporting to work 41-50 hours.[5]

Table 1 reports the estimates obtained using our two-way fixed effects approach using the

[4] Arrival time is used to construct our indicator for working non-standard schedules. Including observations with missing arrival time yields similar results.

[5] Results are substantially identical if including observations with missing information on arrival time (Figure A.10).

stacked dataset as in Cengiz et al. (2019). The adoption of RTW laws led to a 1.5 percentage point increase in the share of workers working more than 45 hours per week, an approximately 6.1% increase with respect to the mean of the dependent variable (Column 1, Panel A). This effect is larger in the sectors of construction (Column 2, Panel A), manufacturing (Column 3, Panel A), and transportation (Column 5, Panel A), ranging between approximately 11% and 14% of the mean of the dependent variable in each respective sector. The effects are milder but sizeable and significant in health and personal care (Column 2, Panel B). Point-estimates are still positive but smaller and not precisely estimated in the business services sector (Column 1, Panel B), education (Column 3, Panel B), and public administration (Column 4, Panel B). The coefficient is negative but small in absolute value and not significant in the retail industry (Column 4, panel A) and in the financial, insurance, and real estate sector (Column 6, Panel A). These findings suggests the effects on long hours were larger in highly unionized sectors, consistent with Fortin et al. (2023).[6] In Table 2, we show a similar impact, ranging between approximately 4% and 8% of the mean of the dependent variable in the overall sample, when focusing on alternative metrics of long hours, such as working more than 40, 50, and 60 hours per week.

Our results on the effects of RTW laws on long hours are robust to the use of the alternative estimands proposed by Callaway and Sant'Anna (2021) and Borusyak et al. (2023). Figure 2 shows our main estimate alongside those obtained using a traditional two-way fixed effect estimation and the alternative approaches proposed by Callaway and Sant'Anna (2021) and Borusyak et al. (2023). As shown in the figure, the estimate ranges between 1.1 percentage points (+4.5%) as calculated using Callaway and Sant'Anna (2021), 1.9 percentage points (+7.7%) when using the traditional two-way fixed effects approach, and 2.4 percentage points (+9.9%) as estimated using Borusyak et al. (2023), (see Table

[6] We obtained qualitatively similar results using data from the American Time Use Survey (ATUS) and looking at the overall sample. In particular, the coefficient estimate from regressions using the ATUS data suggests that the introduction of RTW laws increased the probability of working long hours by approximately 8 percentage points (as opposed to 6 percentage points in our main DID specification). Results are available upon request.

3). We report the estimates obtained using the standard two-way fixed effects approach in Table 4. Overall, these estimates are consistent with our baseline findings. In Table 5, we replicated our main results extending the analysis to the full ACS sample, without restricting to border counties. Overall, we substantially confirm our main findings. The point-estimate is slightly smaller than in our baseline estimates (0.009 vs 0.015). Table A.3 and Figure A.11 replicates our main estimates extending the analysis to workers aged 20 to 64 years old.

As a further check, we replicated our analysis using a permutation exercise on the standard two-way fixed effect model. Each iteration in the simulation involves randomly assigning a RTW treatment year for each state and running our two-way fixed effects specification. In practice, we randomly assign alternative right-to-work adoption dates across the states. Collecting coefficients from all iterations, we obtain an empirical distribution of counterfactual coefficients for each regression equation. Coefficient values corresponding to critical p-values (i.e., 0.1, 0.05, and 0.01) of each of these empirical distributions can then be compared to our actual difference-in-differences estimates. We show that an identical or stronger effect size compared to that of our main coefficient of interest occurs under random assignment in fewer than 5% of all iterations. We report the histograms of these estimates for our main results (Figure 3) and by sector (Figures A.12–A.13). The vertical solid line in red represents our DID estimate. In each histogram, the three p-values obtained with the permutation tests are represented in distinct vertical lines: dashed-dotted (p-value<0.01), dashed (p-value<0.05), and dotted (p-value < 0.01). The permutation test on our main sample yields a p-value lower than 0.05. The analysis by sector confirms our main result is driven by the effects observed in the manufacturing and transportation sectors (Figure A.12, b and d), and concentrated among blue-collar workers (Figure A.13, d). Instead, this exercise suggests that the effect within the construction sector is not significantly different from the placebo tests, although very close to being statistically significant at the 10 percent level.

We then examine the heterogeneity of results by occupational categories and demographics. Considering occupational categories in Table 1, we find that the effect of RTW laws on the likelihood of working more than 45 hours is concentrated among blue-collar workers (+12%, Column 5, Panel B), while it is significantly smaller among white-collar workers (+2.5% , Column 6, Panel B).

While the point-estimate of the effect is larger among men, the impact with respect to the mean is similar among men and women (Columns 1 and 2, Table A.4, approximately 6%). The effect is instead substantially larger among Blacks (+27 %, Column 4) and Hispanics (+18%, Column 5) and is driven by 25-44 years old workers (Column 6, +9%), while there is no evidence of any effect among older workers (see column 8). These results suggest that unions may play a crucial role in the setting of working hours among disadvantaged groups and less-experienced workers who may have lower bargaining power and worse outside options.

Table A.5 shows that RTW laws reduced union coverage by 7.7%. These results are consistent with Fortin et al. (2023). We also find that our results are larger (+10.9%) in more unionized sectors (Columns 1-3, Table A.6). These findings suggest that the decline in union coverage may have contributed to the observed changes in work hours. Following the adoption of RTW laws, unions may also be less successful in retaining hours protections in subsequent contracts because of reduced bargaining power (Chava et al., 2020b). Overall, given the reduced-form nature of our analysis and the previous evidence on the effects of RTW laws on unionization, union threat effects, and union reduced bargaining power, we cannot identify a unique mechanism and rule out other channels through which RTW laws may affect our outcomes of interest.

4.2 The Effects of RTW Laws on Hours Worked and Hourly Wages

In Table 6, we find no evidence of a statistically significant effect when examining a continuous measure of working hours. Nevertheless, the point-estimate suggests an increase of 0.5% (p-value=0.107). RTW laws led to a statistically significant increase in hours worked in the manufacturing (+1%) and transportation(+1.4%) sectors. The effect appears to be sizeable in the construction and health& personal care sectors, too, although less precisely estimated. If anything, RTW led to a decline in hours worked in the retail and wholesale sector (-1.6%). Among blue-collar workers, the effect was close to a 1% increase, while there is no evidence of any effect among white-collar workers. The increase in working hours is larger among Blacks (+2.7%, Column 4, Table A.7) and 25-44 years old age-workers (+1%, Column 6).

Although the evidence for the effects of RTW laws on wages is somewhat mixed and largely depends on the time-frame studied, some studies show RTW laws may have negative effects on hourly wages. Thus, the increase in hours worked and long hours may be a response to reduced wage. There is no evidence of a significant reduction in wages in the main sample (Table 7).[7] However, we do observe a significant reduction in the manufacturing sector (-3.2%, Column 3, Panel A). Point-estimates are negative but not precisely estimated in finance, insurance, and real estate (-0.2%, Column 6, Panel A)); business services (-2.8%, Column 1, Panel B); health & personal care (-1.9%, Column 2, Panel B); and education (-1.6%, Column 3, Panel B). The negative effect of RTW on hourly wage is larger among Blacks (-2.7%, Column 4, Table A.8). Interestingly, while no effect is found among 25-44 year old workers, RTW led to a 2.5% decline of the hourly wage among more experienced workers (45-54 years old, see Column 7).

4.3 The Effects of RTW Laws on Non-Standard Schedules

Previous studies in the public health literature suggest that union bargaining power may also affect the nature of shifts, the likelihood of working non-standard hours, as well as workers' safety and health. However, there is a paucity of causal studies exploring these relationships (see Section 2). Using ACS data, we can explore the effects of RTW laws on non-standard schedules which have been previously linked to detrimental effects on health and wellbeing.

Overall, we find no evidence of significant effects. The introduction of right-to-work laws,

[7] We instead find a significant effect when we do not restrict the analysis to bordering counties (see Table 5).

if anything, increased the likelihood of arriving at work between 5pm and 8am (+6.7%, Column 1, Panel A, Table 8), but the effect is not precisely estimated. Similarly there is no evidence of significant effects when examining the likelihood of arriving at work between 6pm and 8pm. Furthermore, we examine the laws' impact on the likelihood of arriving at work between 10pm and 5am (Night Schedule). The event study analyses does not show any clear pattern (see Figure 4 and Figure A.14 in the Appendix). Furthermore, when checking the sensitivity of the results to alternative estimands, we only find a significant effect when using the approach proposed by Borusyak et al. (2023) (see Figure 5).

Interestingly, when examining the heterogeneity of the effects across sectors, we find evidence of a relative large increase in the education sector (+29.5%) and in the public administration sector (+15.7%, see Panel B of Table 8). These effects are driven by an increase in the share of workers starting to work before 8am (see Figure A.15).[8] Furthermore, Table A.9 shows effects were larger and precisely estimated among Blacks (+12.8%, Column 4), Hispanics (+22.7%, Column 5), and among 25-44 year old workers (+7%, Column 6).

Overall, we conclude that while RTW laws did not have a clear impact on the overall population, there is some evidence of an increase in the share of workers working non-standard hours in the education and public administration sector, among minorities, and among younger workers. As for the results on long hours, these findings suggests that the decline in strength and bargaining power of unions may be particularly detrimental for disadvantaged workers.

5 Conclusions

In this paper, we explored variation in the timing of adoption of right-to-work (RTW) laws across the United States to study the effects of weakened union power on working conditions. Research on the effects of RTW laws has mostly focused on wages and employment. We

[8] We do not find statistically significant effects of RTW laws on the likelihood of arriving at work between 10pm and 5am at the sector level.

document a sizable and significant increase in the share of workers working long hours after the adoption of RTW laws. Our result on long hours is supported by a large set of robustness checks and placebo estimates. RTW laws led to a decline in unionization which together with the reduced bargaining power of unions may contribute to explain the effect on long hours (Wright, 2016; Finnigan and Hale, 2018).

Our difference-in-differences estimates show an increase in the likelihood of working non-standard schedules in highly unionized sectors such as education and public administration, as well among minorities and younger workers. However, these results are less precisely estimated and more sensitive to the alternative methods used in the study (i.e. event study analysis, recent alternatives to the traditional difference-in-differences approach, (Roth et al., 2022)).

Taken together, RTW laws led to a sizeable increase in the share of individuals working long hours. Our results are consistent with previous studies discussing the link between unions and the health of workers and highlighting the importance of non-pecuniary working conditions (Finnigan and Hale, 2018). Given the evidence on the detrimental effects of long hours on physical and mental health, these effects should not be neglected when assessing the overall impact of these policies on workers' health and wellbeing.

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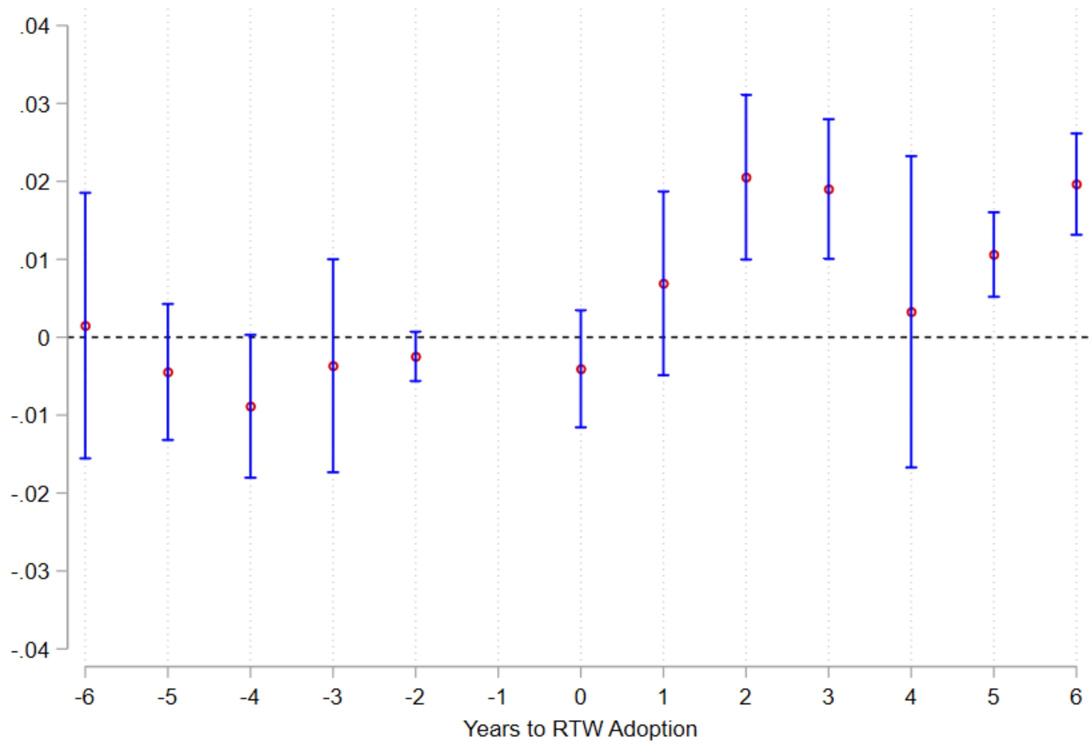
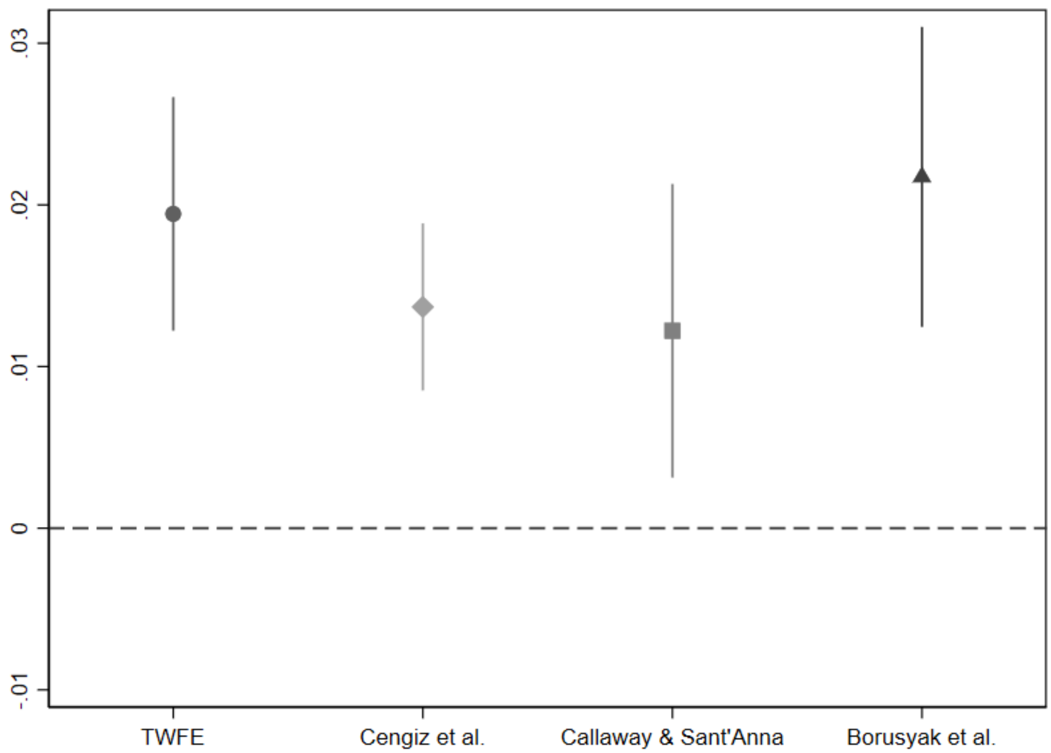
Figure 1: Effect of RTW Laws on > 45Hours



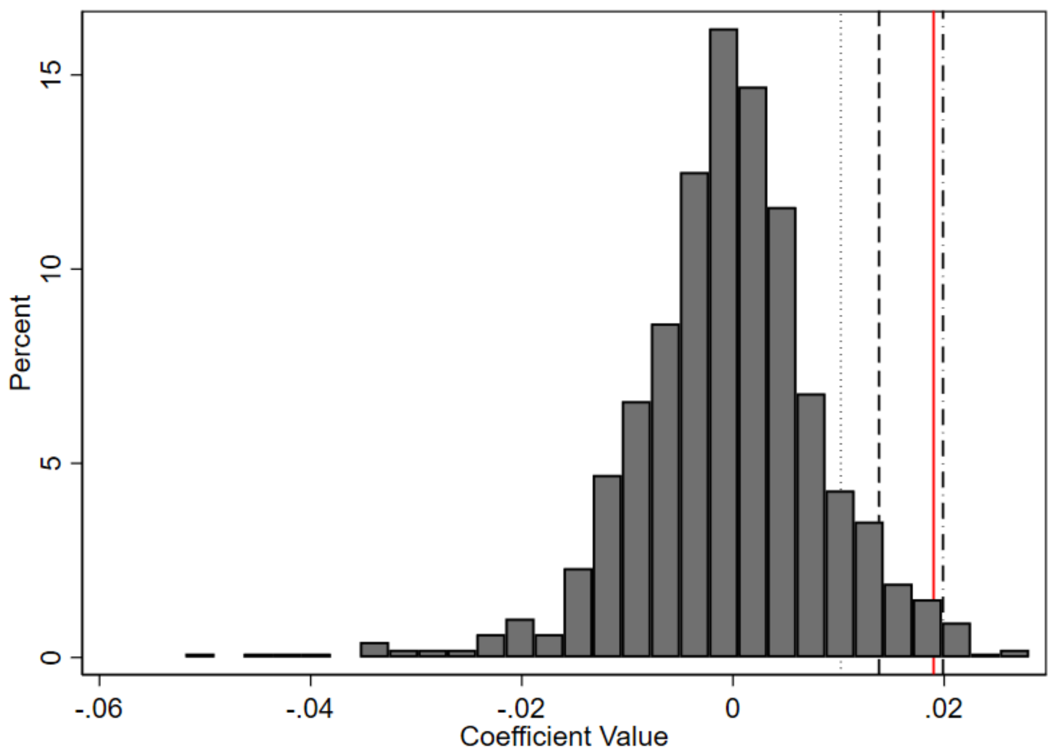
Notes - Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 25-64 who report non-missing work arrival time. The event study presents the leads and lags of the differences between individuals from bordering counties in adopter and non-adopter states after controlling for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects.

Figure 2: Alternative Estimands: Treatment Effect on >45 Hours



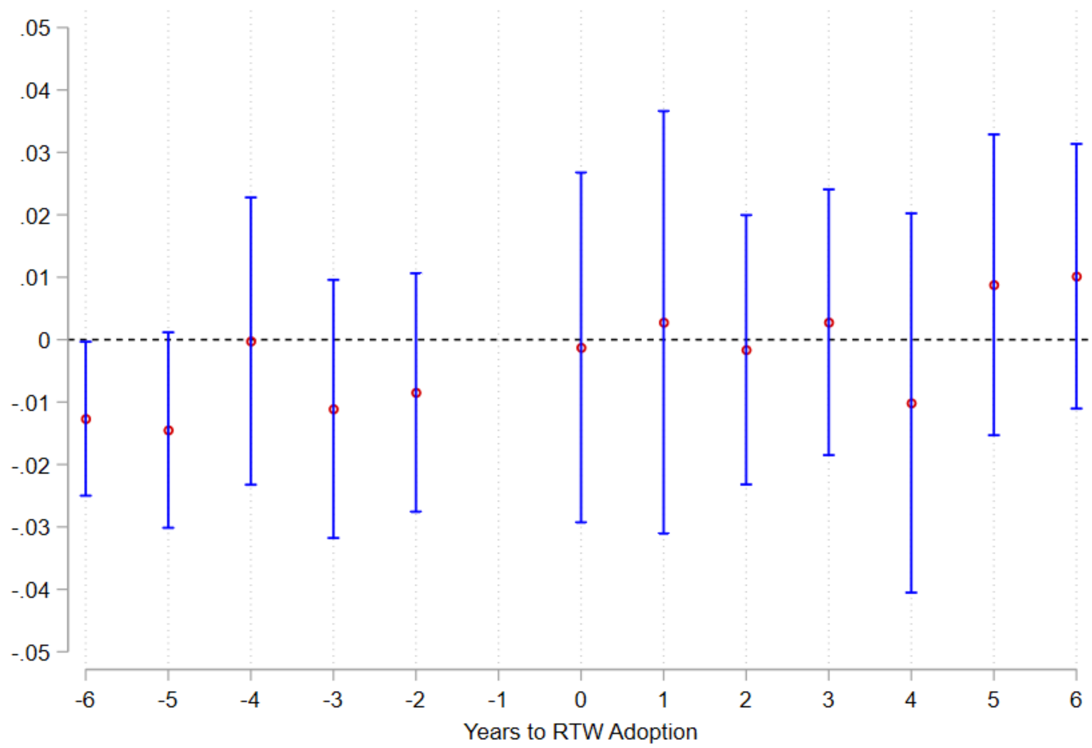
Notes - Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 25-64 who report non-missing arrival time. Only individuals from counties on either side of a RTW border are included. The point estimate in the TWFE column is the simple two-way fixed effect estimator. The point estimate in the Cengiz et al. (2019) column is our baseline specification. We also report robust difference-in-differences estimators from the procedures proposed by Callaway & Sant'Anna 2021 and Borusyak et al. 2023.

Figure 3: Permutation Test: >45 Hours



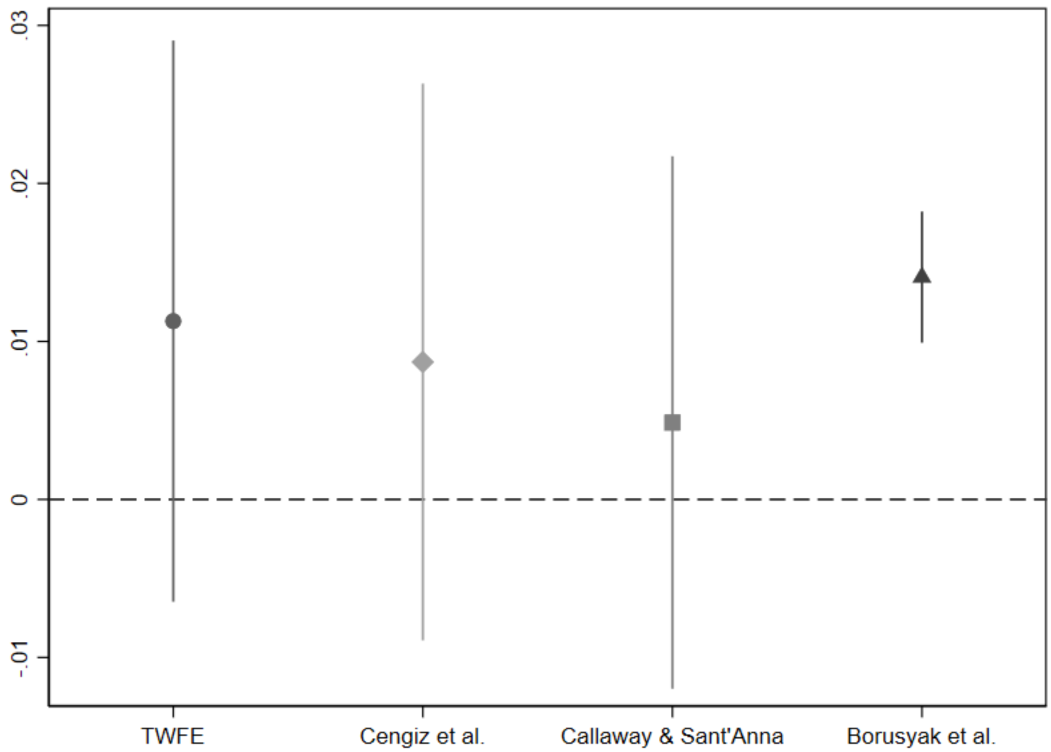
Notes - Permutation test were run on our baseline sample. Each iteration in the simulation involves randomly assigning an RTW treatment year for each state. The histogram represents the empirical distribution of the counterfactual coefficient values using 1,000 simulations. The segmented lines represent critical p-values in the distribution (dash-dotted [p-value<0.01], dashed [p-value<0.05], dash-dotted [p-value<0.01]). The vertical solid line in red represents our DID estimate.

Figure 4: Effect of RTW Laws on Non-Standard Schedules



Notes - The dependent variable is a dummy for arriving at work between 5pm and 8am. Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 25-64 who report non-missing work arrival time. The event study presents the leads and lags of the differences between individuals from bordering counties in adopter and non-adopter states after controlling for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects.

Figure 5: Alternative Estimands: Treatment Effect on Non-Standard Schedules



Notes - The dependent variable is a dummy for arriving at work between 5pm and 8am. Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 25-64 who report non-missing arrival time. Only individuals from counties on either side of a RTW border are included. The point estimate in the TWFE column is the simple two-way fixed effect estimator. The point estimate in the Cengiz et al. (2019) column is our baseline specification. We also report robust difference-in-differences estimators from the procedures proposed by Callaway & Sant'Anna 2021 and Borusyak et al. 2023.

Table 1: The Effect of RTW on >45 Hours by Sector and Occupation, Bordering Counties

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
Dep. Var.: >45 Hours	All	Construction	Manufacturing	Retail & Wholesale	Transportation	Finance & Real Estate
RTW	0.015***	0.036**	0.033**	-0.014	0.032***	-0.005
	(0.003)	(0.015)	(0.012)	(0.009)	(0.007)	(0.018)
Observations	1,078,278	49,536	145,126	168,894	89,057	105,107
Mean of Dep. Var.	0.242	0.264	0.304	0.236	0.275	0.261
Std.Dev. of Dep. Var.	0.428	0.441	0.460	0.425	0.446	0.439
			Panel B			
Dep. Var.: >45 Hours	Business Services	Health & Personal Care	Education	Public Administration	Blue Collar	White Collar
RTW	0.006	0.011**	0.016	0.016	0.029**	0.006**
	(0.006)	(0.003)	(0.010)	(0.009)	(0.009)	(0.002)
Observations	135,479	218,458	100,208	56,222	185,961	890,652
Mean of Dep. Var.	0.296	0.161	0.224	0.196	0.241	0.242
Std.Dev. of Dep. Var.	0.457	0.368	0.417	0.397	0.428	0.428

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included (see Empirical Specification). All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level. Industry and occupation categories were constructed using 1990 codes.

Table 2: The Effect of RTW on Long Hours, Bordering Counties

	(1)	(2)	(3)	(4)
Working Hours:	>45hrs	>40hrs	>50hrs	>60hrs
RTW	0.015***	0.026***	0.007*	0.001*
	(0.004)	(0.004)	(0.003)	(0.001)
Observations	1,078,278	1,078,278	1,078,278	1,078,278
Mean of Dep. Var.	0.242	0.344	0.108	0.026
Std.Dev. Of Dep. Var.	0.428	0.475	0.310	0.160

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included (see Empirical Specification). All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level.

Table 3: Alternative Estimands: Treatment Effect on >45 Hours

	(1)	(2)	(3)	(3)
Dep. Var.: >45 Hours	TWFE	Cengiz et al.	Callaway & Sant'Anna	Borusyak et al.
RTW	0.019***	0.015***	0.011**	0.024***
	(0.004)	(0.004)	(0.005)	(0.005)
Observations	768,616	1,078,278	644,322	644,322
Mean of Dep. Var.	0.248	0.252	0.243	0.243
Std.Dev. of Dep. Var.	0.432	0.434	0.429	0.429

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). The sample is restricted to non-immigrant full-time employed worker aged 25-64 who do not report non-missing arrival time. Only individuals from counties on either side of a RTW border are included. The point estimate in the TWFE column is the simple two-way fixed effect estimator. The point estimate in the Cengiz et al. (2019) column is our baseline specification. We also report robust difference-in-differences estimators from the procedures proposed by Callaway & Sant'Anna 2021 and Borusyak et al. 2022.

Table 4: TWFE Estimates on >45 Hours by Sector and Occupation, Bordering Counties

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Dep. Var.: >45 hours	All	Construction	Manufacturing	Retail & Wholesale	Transportation	Finance & Real Estate
RTW	0.019***	0.034**	0.037**	-0.008	0.041***	-0.007
	(0.004)	(0.014)	(0.013)	(0.009)	(0.009)	(0.016)
Observations	768,616	40,941	84,300	116,145	61,615	61,952
Mean of Dep. Var.	0.241	0.274	0.301	0.240	0.284	0.247
Std.Dev. of Dep. Var.	0.428	0.446	0.459	0.427	0.451	0.431
Panel B						
Dep. Var.: >45 hours	Business Services	Health & Personal Care	Education	Public Administration	Blue Collar	White Collar
RTW	0.015***	0.006	0.023*	0.022**	0.035***	0.009***
	(0.005)	(0.005)	(0.011)	(0.010)	(0.009)	(0.003)
Observations	100,603	155,396	70,129	64,601	132,645	630,159
Mean of Dep. Var.	0.280	0.164	0.221	0.206	0.250	0.237
Std.Dev. of Dep. Var.	0.449	0.371	0.415	0.404	0.433	0.425

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and both year and state fixed effects. Standard errors (in parentheses) are clustered at the state level. Industry and occupation categories were constructed using 1990 codes.

Table 5: The Effect of RTW on Long Hours, Full ACS Sample

	(1)	(2)	(3)	(4)
Dep. Var.:	>45 hours	>40 hours	>50 hours	Hourly wage (log)
RTW	0.009***	0.012***	0.004***	-0.024*
	(0.001)	(0.002)	(0.001)	(0.012)
Observations	43,227,757	43,227,757	43,227,757	32,542,349
Mean of Dep. Var.	0.177	0.247	0.082	3.023
Std.Dev. of Dep. Var.	0.382	0.431	0.275	0.764

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). The sample is restricted to non-immigrant full-time employed worker aged 25-64 who do not report non-missing arrival time.

Table 6: The Effect of RTW on Working Hours by Sector and Occupation, Bordering Counties

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A					
Dep. Var.: Working Hours (log)	All	Construction	Manufacturing	Retail & Wholesale	Transportation	Finance & Real Estate
RTW	0.005	0.010	0.010***	-0.016**	0.014**	0.001
	(0.003)	(0.011)	(0.001)	(0.006)	(0.006)	(0.006)
Observations	1,078,278	49,536	145,126	168,894	89,057	105,107
Mean of Dep. Var.	3.705	3.742	3.771	3.667	3.742	3.747
Std.Dev. of Dep. Var.	0.295	0.246	0.218	0.326	0.258	0.229
	Panel B					
Dep. Var.: Working Hours (log)	Business Services	Health & Personal Care	Education	Public Administration	Blue Collar	White Collar
RTW	-0.008	0.014	0.002	0.002	0.010*	0.001
	(0.008)	(0.008)	(0.007)	(0.007)	(0.005)	(0.002)
Observations	135,479	218,458	100,208	56,222	185,961	890,652
Mean of Dep. Var.	3.743	3.624	3.693	3.722	3.725	3.700
Std.Dev. of Dep. Var.	0.275	0.355	0.296	0.252	0.274	0.299

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level. Industry and occupation categories were constructed using 1990 codes.

Table 7: The Effect of RTW on Hourly Wages by Sector and Occupation, Bordering Counties

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A					
Dep. Var.: Hourly Wage (log)	All	Construction	Manufacturing	Retail & Wholesale	Transportation	Finance & Real Estate
RTW	-0.013	0.039	-0.032**	0.001	0.006	-0.022
	(0.012)	(0.030)	(0.013)	(0.021)	(0.018)	(0.015)
Observations	1,050,246	45,342	144,214	166,054	87,344	101,855
Mean of Dep. Var.	3.136	3.224	3.238	2.874	3.133	3.391
Std.Dev. of Dep. Var.	0.693	0.624	0.641	0.699	0.610	0.739
	Panel B					
Dep. Var.: Hourly Wage (log)	Business Services	Health & Personal Care	Education	Public Administration	Blue Collar	White Collar
RTW	-0.028	-0.019	-0.016	-0.001	-0.020	-0.010
	(0.017)	(0.012)	(0.010)	(0.009)	(0.013)	(0.013)
Observations	128,780	211,749	99,633	56,222	178,407	870,174
Mean of Dep. Var.	3.310	3.012	3.103	3.327	2.955	3.175
Std.Dev. of Dep. Var.	0.756	0.695	0.576	0.517	0.584	0.708

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level. Industry and occupation categories were constructed using 1990 codes.

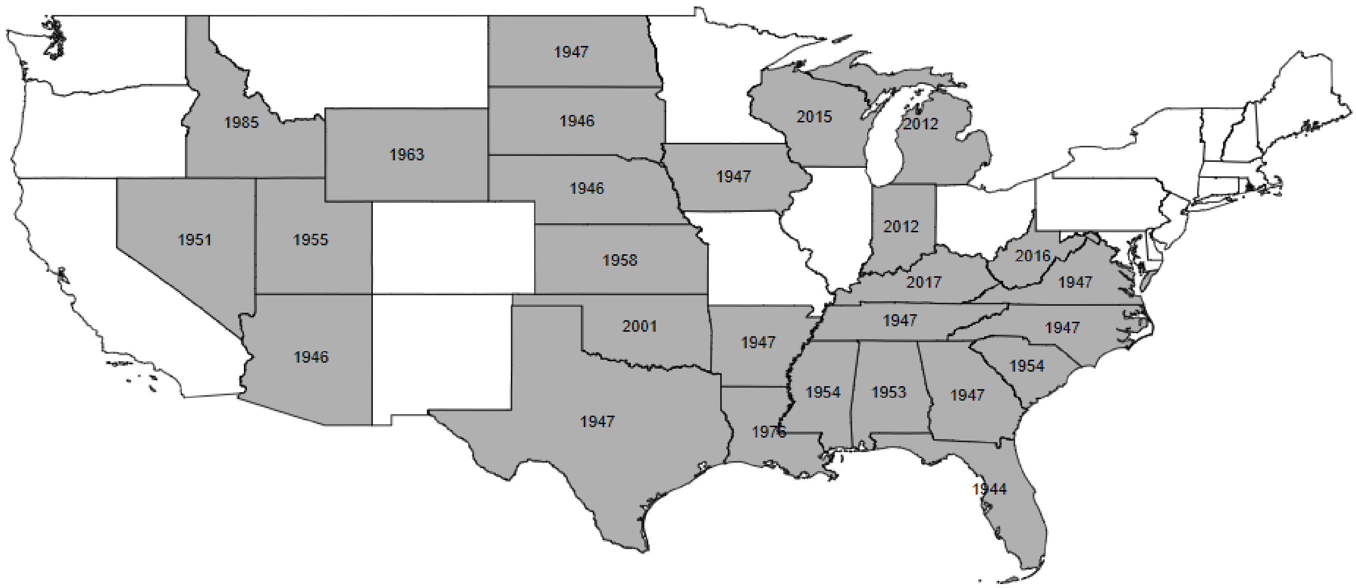
Table 8: The Effect of RTW on Non-Standard Schedules (Arriving between 5pm and 8am), by Sector and Occupation, Bordering Counties

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A					
Dep. Var.: Arriving between 5pm and 8am	All	Construction	Manufacturing	Retail & Wholesale	Transportation	Finance & Real Estate
RTW	0.009	0.001	0.018	-0.003	0.019	0.004
	(0.009)	(0.014)	(0.014)	(0.011)	(0.014)	(0.009)
Observations	1,078,278	49,536	145,126	168,894	89,057	105,107
Mean of Dep. Var.	0.135	0.167	0.207	0.152	0.243	0.051
Std.Dev. of Dep. Var.	0.342	0.373	0.406	0.359	0.429	0.220
	Panel B					
Dep. Var.: Arriving between 5pm and 8am	Business Services	Health & Personal Care	Education	Public Administration	Blue Collar	White Collar
RTW	0.015	-0.000	0.018**	0.025***	0.015	0.006
	(0.013)	(0.007)	(0.006)	(0.005)	(0.018)	(0.005)
Observations	135,479	218,458	100,208	56,222	185,961	890,652
Mean of Dep. Var.	0.079	0.123	0.061	0.159	0.280	0.104
Std.Dev. of Dep. Var.	0.269	0.328	0.239	0.366	0.449	0.306

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level.

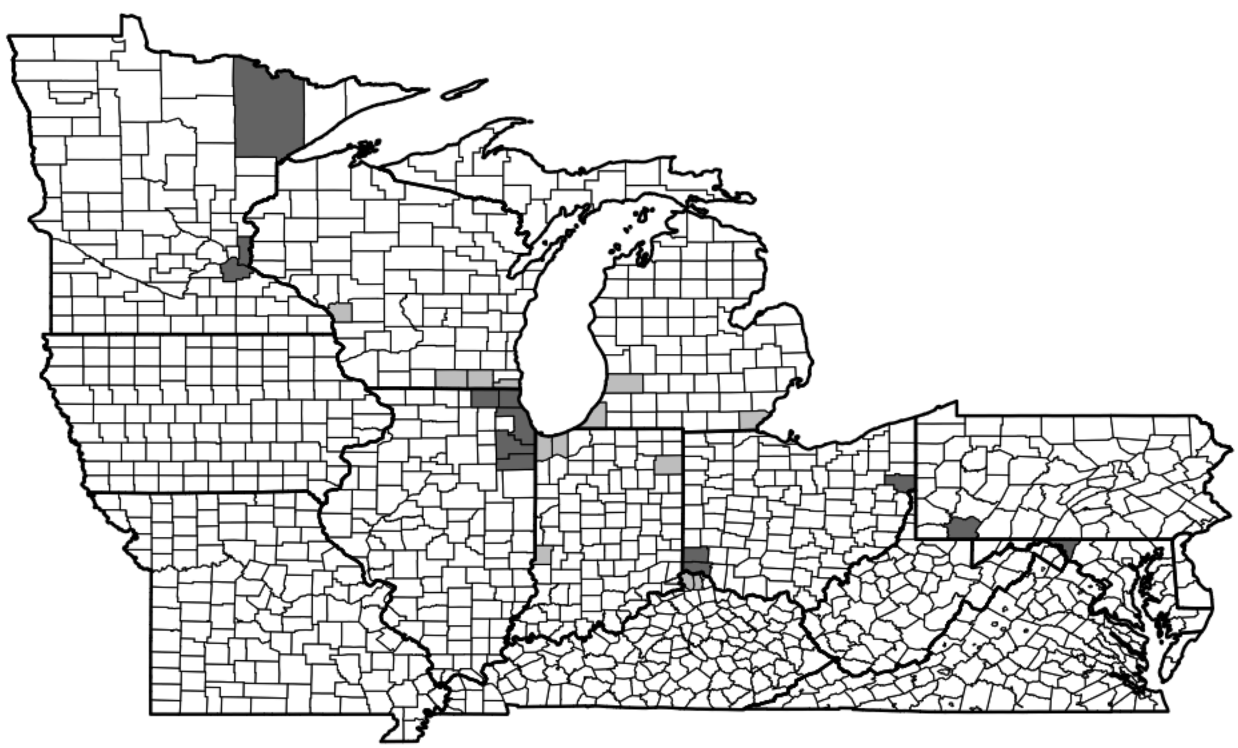
Appendix

Figure A.1: Year of RTW Introduction Across US States



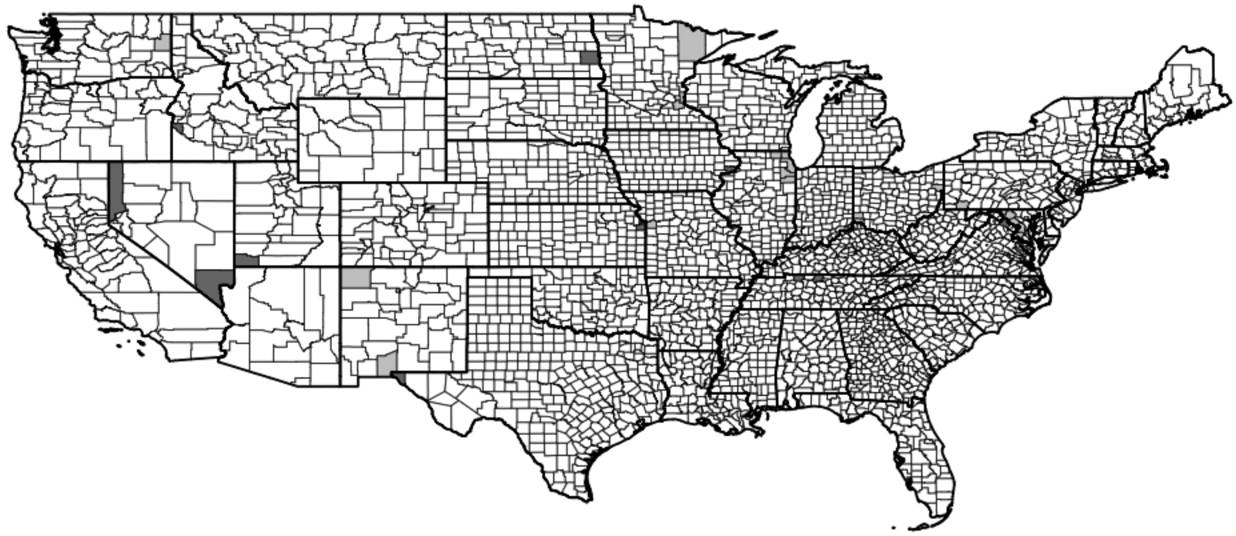
Notes - The figure displays the year of adoption of RTW laws across US states.

Figure A.2: Counties Represented in Main Analysis Sample



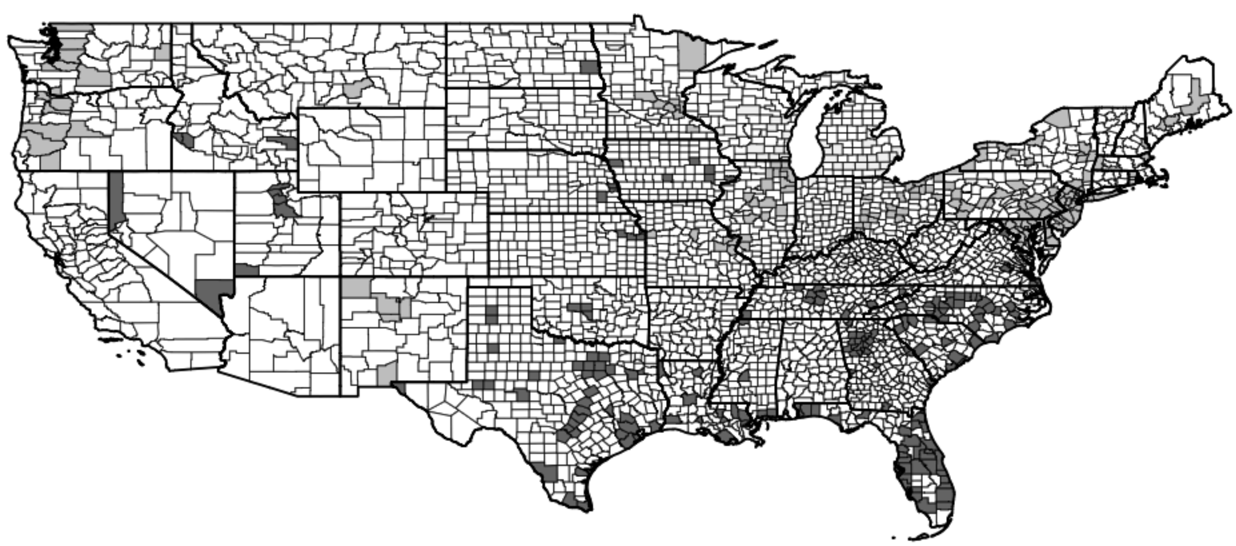
Notes - Never-treated counties are in are in dark-gray; Would-be Treated counties are in light-gray.

Figure A.3: Counties Represented in TWFE Sample



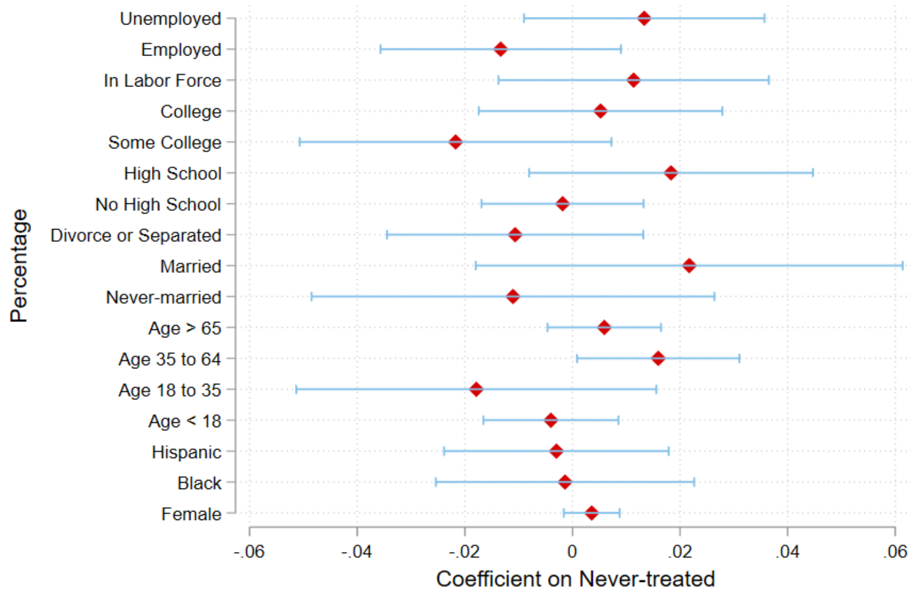
Notes - Never-treated counties are in are in dark-gray; Would-be Treated counties are in light-gray.

Figure A.4: Counties Represented in Full ACS Sample



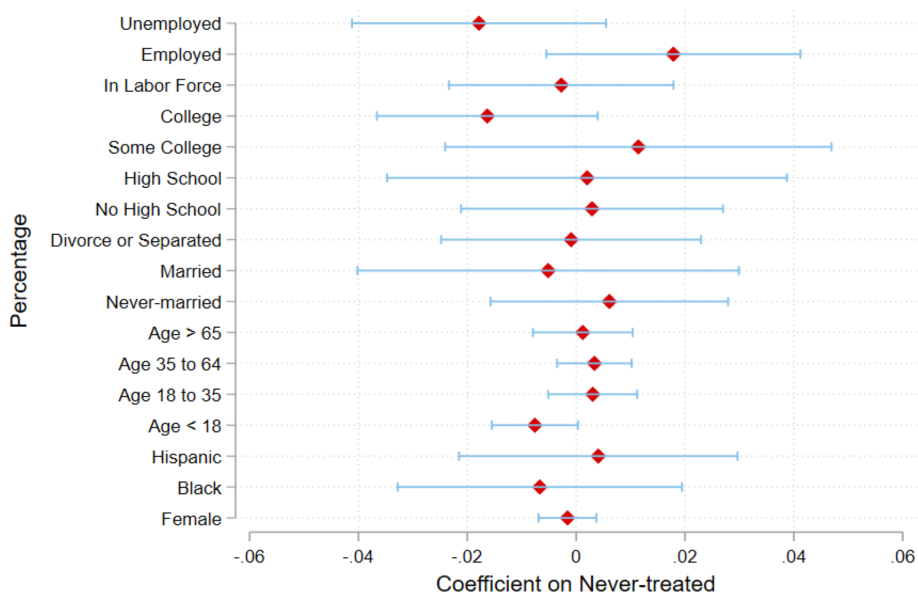
Notes - Never-treated counties are in are in dark-gray; Would-be Treated counties are in light-gray.

Figure A.5: Balance Test: Comparing Never-Treated (Adopter) and Would-be Treated (Non-adopter) Counties along State Borders (2006-2011)



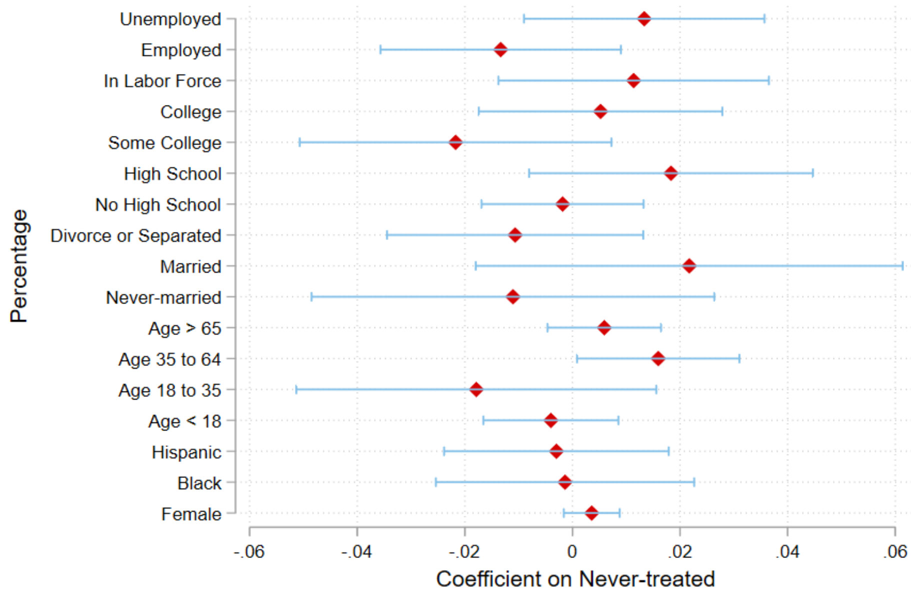
Notes - County-level data are drawn from the American Community Survey. Coefficients and confidence intervals are drawn from county-level regressions of the first-difference in outcomes between 2006 and 2011 on treatment-status, controlling for state fixed effects.

Figure A.6: Balance Test: Comparing Never-Treated (Adopter) and Would-be Treated (Non-adopter) Counties along State Borders (2012-2019)



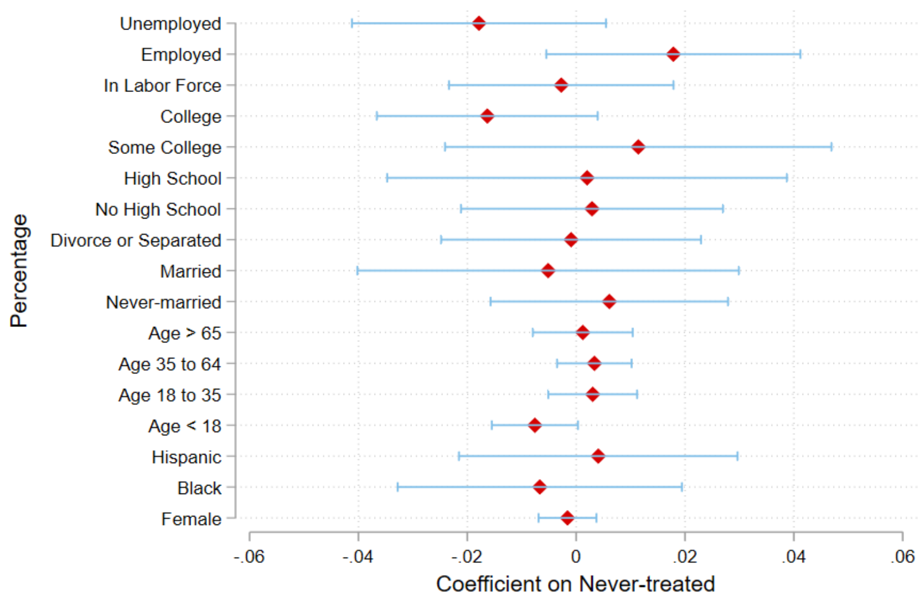
Notes - County-level data are drawn from the American Community Survey. Coefficients and confidence intervals are drawn from county-level regressions of the first-difference in outcomes between 2019 and 2012 on treatment-status, controlling for state fixed effects.

Figure A.7: Balance Test: Comparing Border and Non-Border Counties (2006-2011)



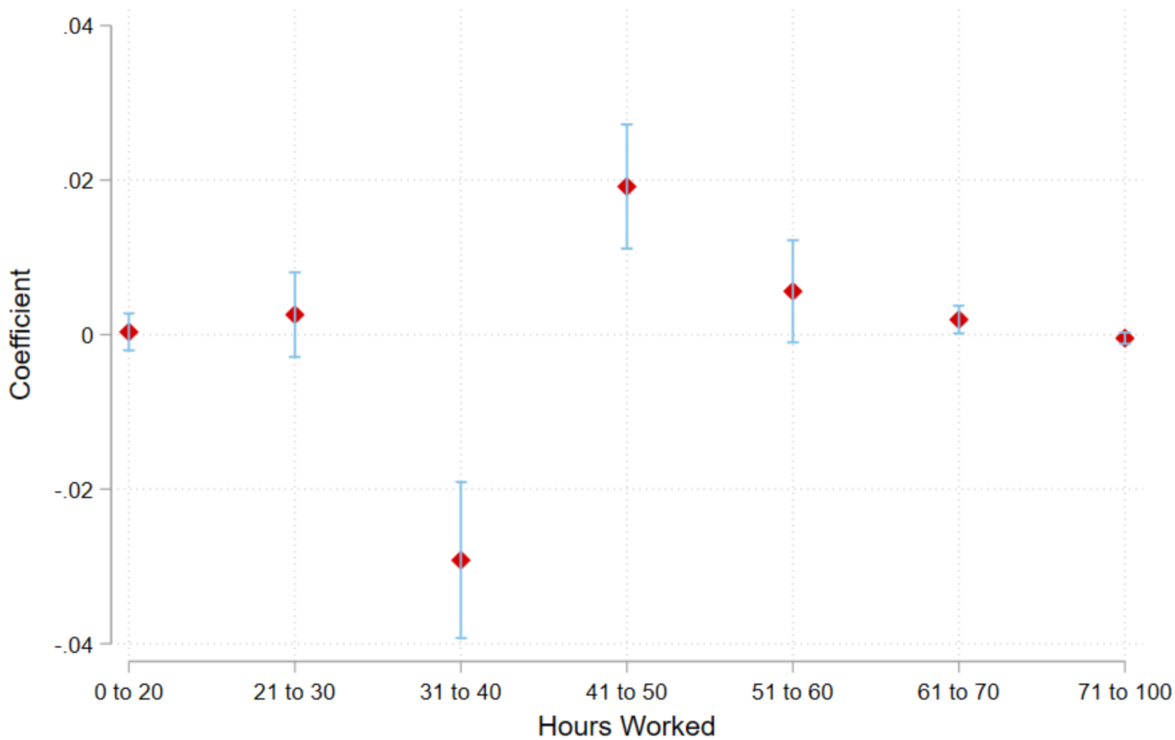
Notes - County-level data are drawn from the American Community Survey. Coefficients and confidence intervals are drawn from county-level regressions of the first-difference in outcomes between 2006 and 2011 on treatment-status, controlling for state fixed effects.

Figure A.8: Balance Test: Comparing Border and Non-Border Counties (2012-2019)



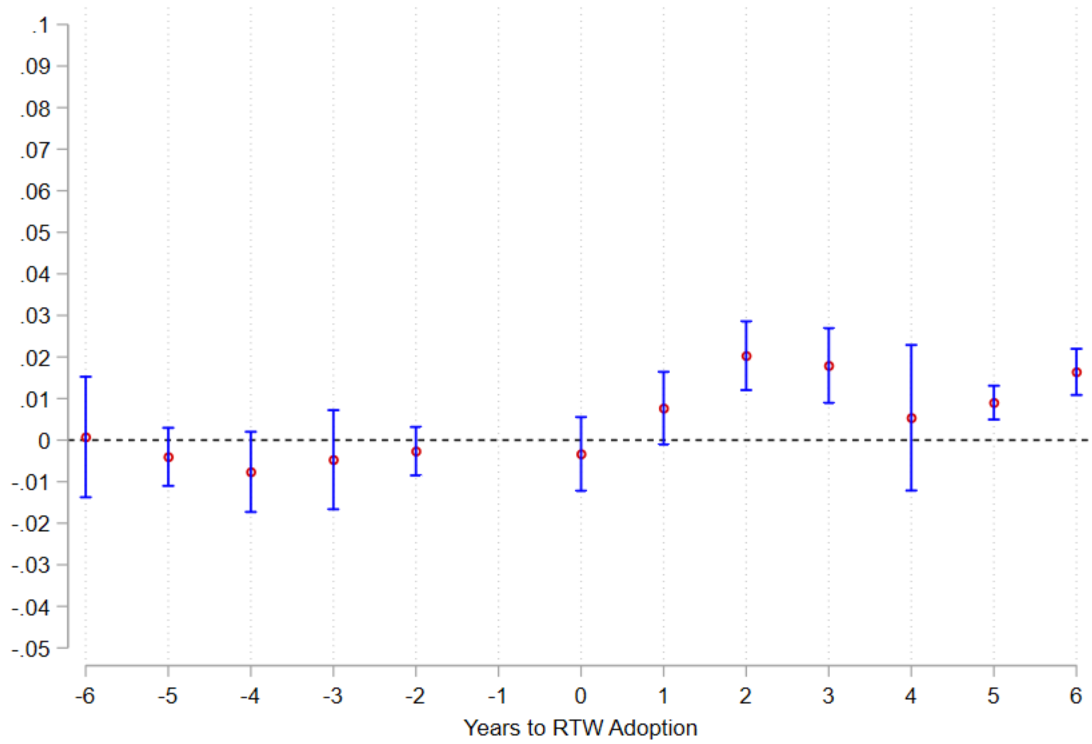
Notes - County-level data are drawn from the American Community Survey. Coefficients and confidence intervals are drawn from county-level regressions of the first-difference in outcomes between 2019 and 2012 on treatment-status, controlling for state fixed effects.

Figure A.9: Estimates for Intervals of Hours Worked



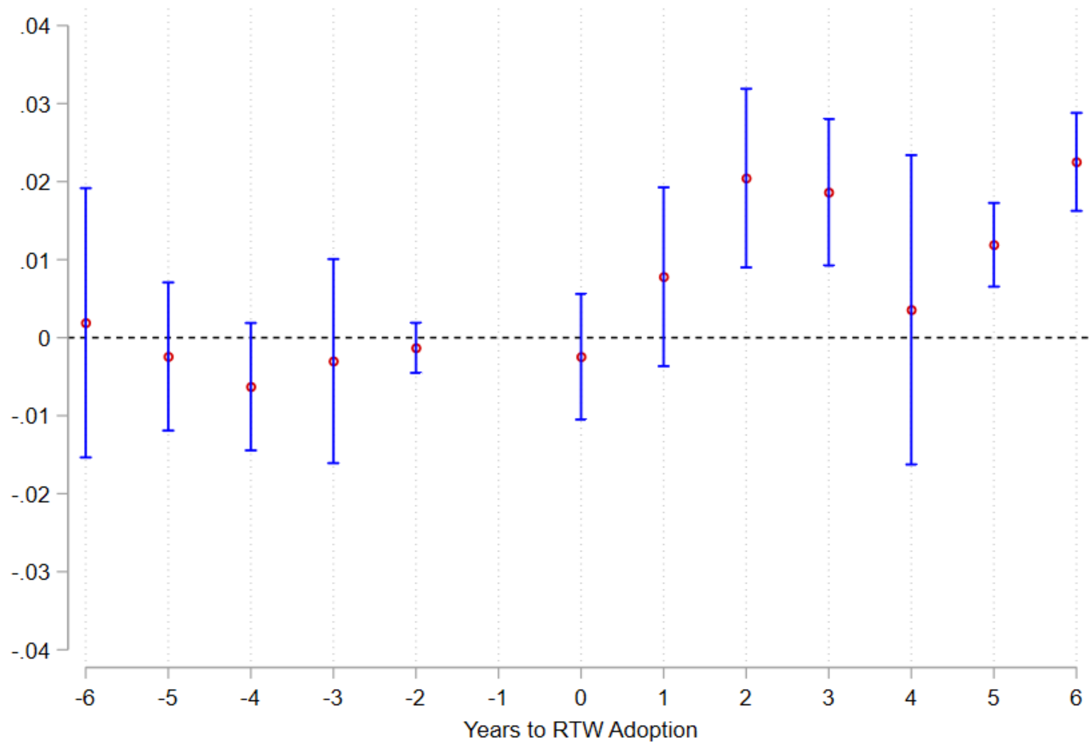
Notes - Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 25-64 who report non-missing work arrival time. As in Cengiz et al. (2019), we construct a control group for each treatment event from states that have not been treated within a fixed time-window in the past (see Empirical Specification). Each coefficient depicted is the estimated β from our stacked difference-in-differences specification with its dependent variable being a dummy that is equal to one if an individual has hours worked within corresponding interval on the x-axis.

Figure A.10: Effect of RTW Laws on >45 Hours, including observations with no information on arrival time



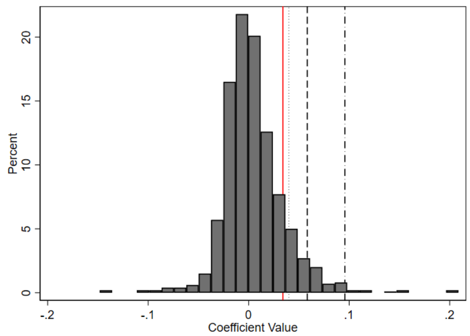
Notes - Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 25-64. The event study presents the leads and lags of the differences between individuals from bordering counties in adopter and non-adopter states after controlling for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects.

Figure A.11: Effect of RTW Laws on >45 Hours, 20-64 year old sample

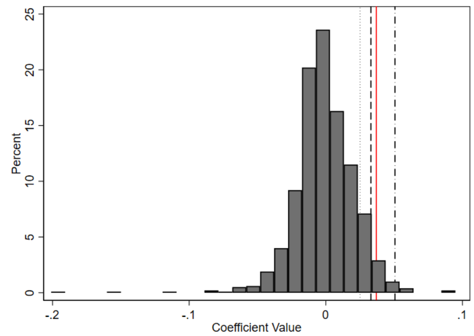


Notes - Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 20-64 who report non-missing work arrival time. As in Cengiz et al. (2019), we construct a control group for each treatment event from states that have not been treated within a fixed time-window in the past (see Empirical Specification). The event study presents the leads and lags of the differences between individuals from bordering counties in adopter and non-adopter states after controlling for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects.

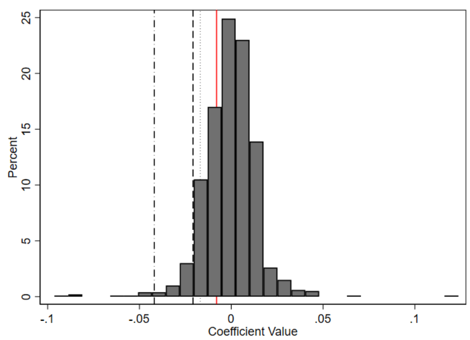
Figure A.12: Permutation Test: >45 Hours, by Industry/Sector



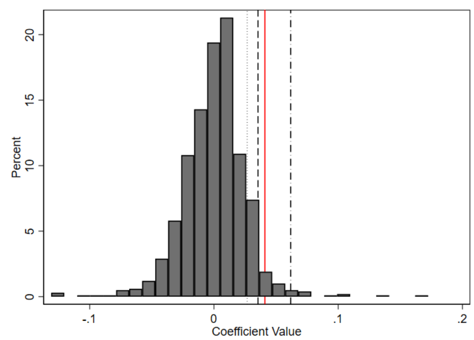
(a) Construction



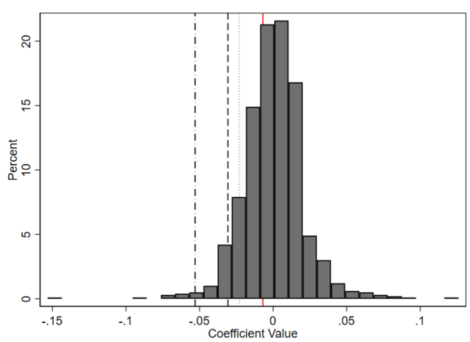
(b) Manufacturing



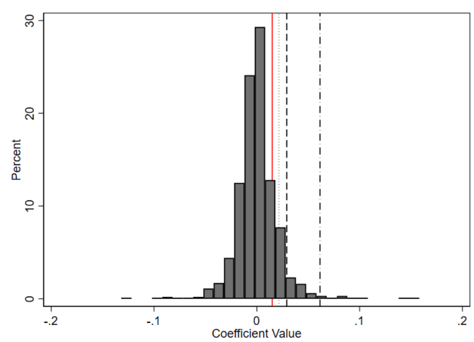
(c) Retail & Wholesale



(d) Transportation



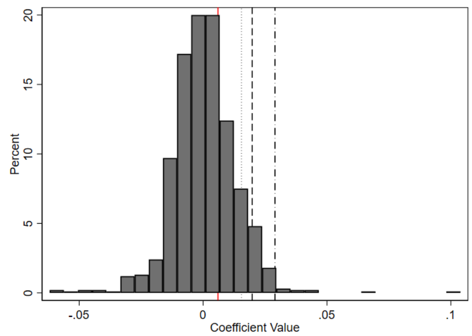
(e) Finance, Insurance & Real Estate



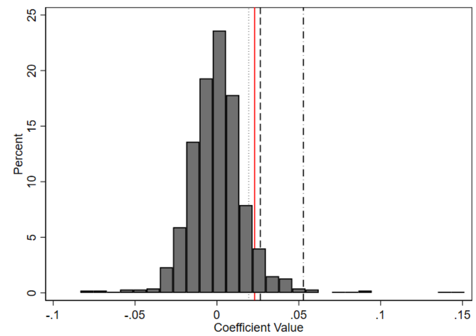
(f) Business Services

Notes - Permutation test done on the baseline sample, which are observations in bordering counties. Each iteration in the simulation involves randomly assigning an RTW treatment year for each state. 1000 simulations were done and this histogram represents the empirical distribution of the counterfactual coefficient values. The segmented lines represent critical p-values in the distribution (dash-dotted [p-value<0.01], dashed [p-value<0.05], dash-dotted [p-value<0.01]). The vertical solid line in red represents our DID estimate. Industry and occupation categories are constructed using 1990 codes.

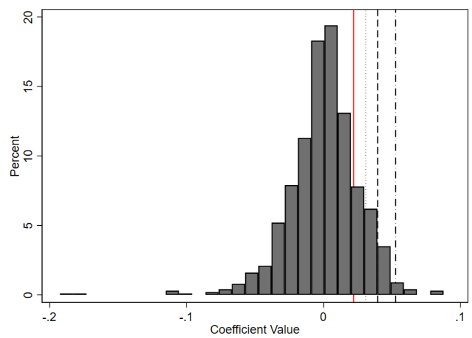
Figure A.13: Permutation Test: >45 Hours, by Industry/Sector (cont'd)



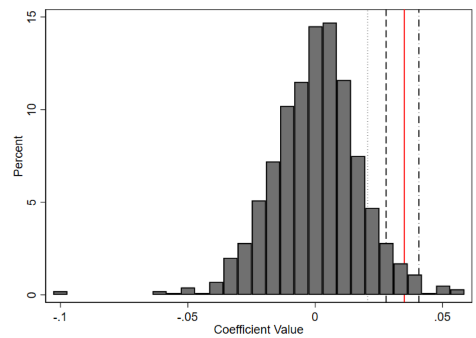
(a) Health & Personal Care



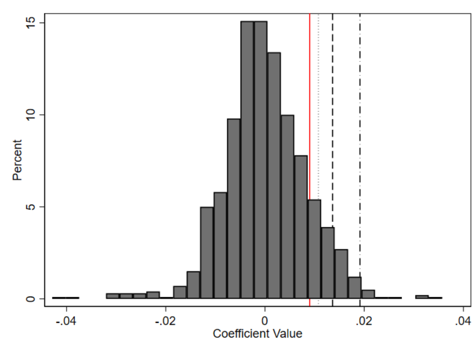
(b) Education



(c) Public Administration



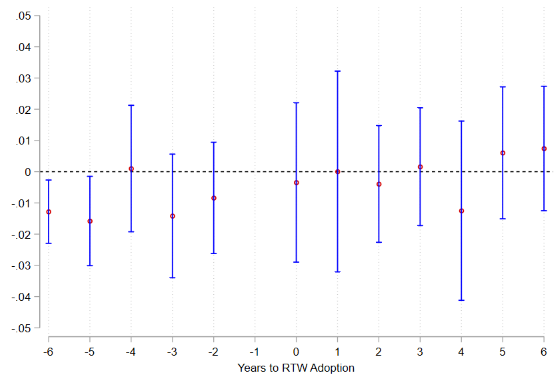
(d) Blue Collar



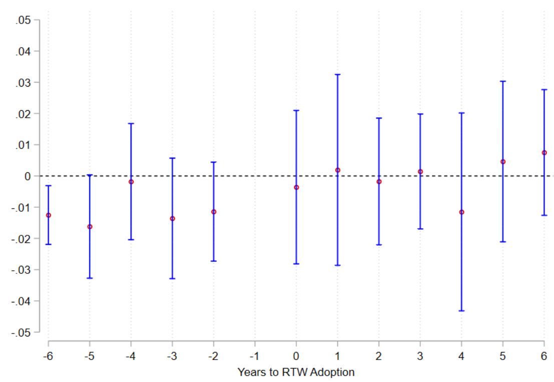
(e) White Collar

Notes - Permutation test done on the baseline sample, which are observations in bordering counties. Each iteration in the simulation involves randomly assigning an RTW treatment year for each state. 1000 simulations were done and this histogram represents the empirical distribution of the counterfactual coefficient values. The segmented lines represent critical p-values in the distribution (dash-dotted [p-value<0.01], dashed [p-value<0.05], dash-dotted [p-value<0.01]). The vertical solid line in red represents our DID estimate. Industry and occupation categories are constructed using 1990 codes.

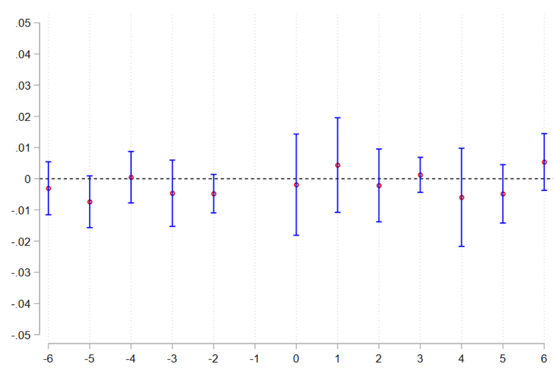
Figure A.14: Effect of RTW on Non-Standard Schedules; ACS



(a) Arriving between 6pm and 8am



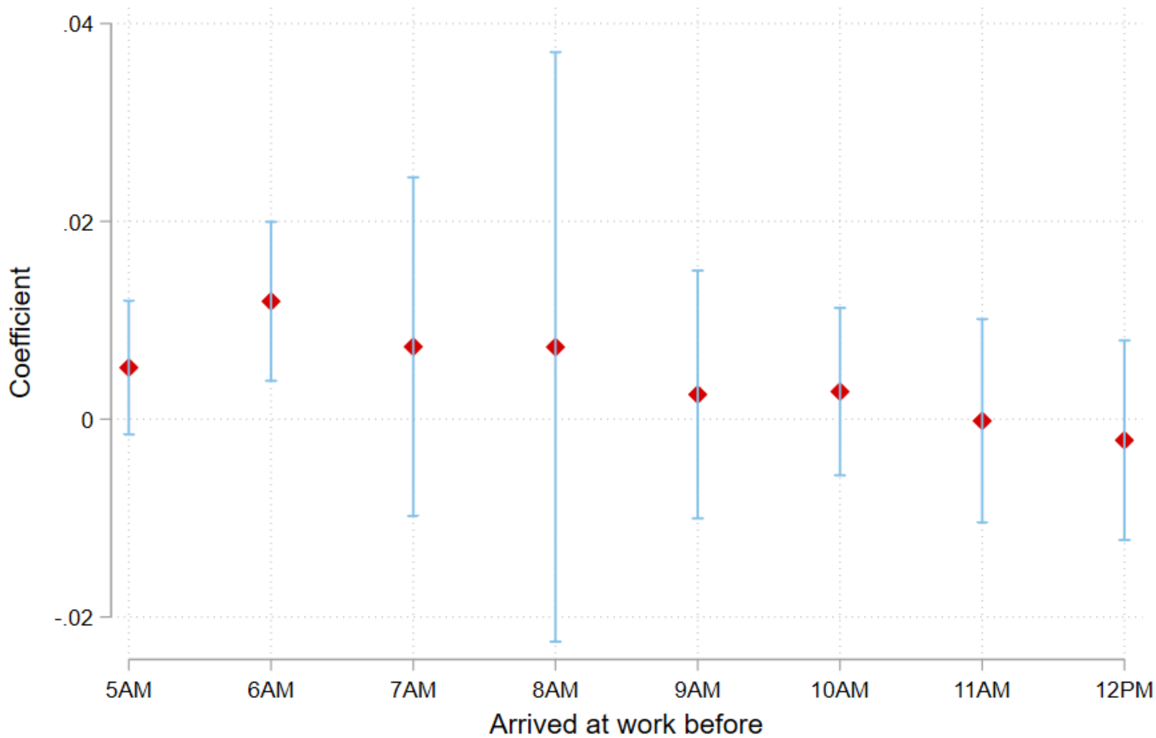
(b) Arriving between 8pm and 8am



(c) Arriving between 10pm and 5am (Night Schedule)

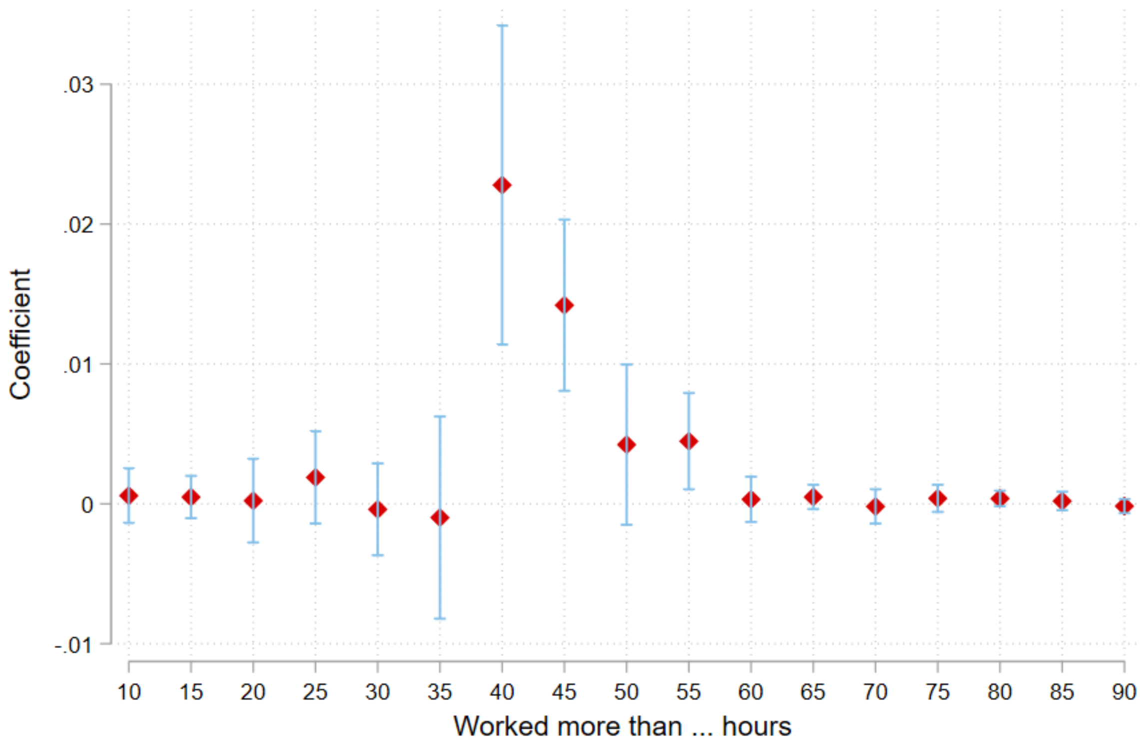
Notes - Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 25-64 who report non-missing work arrival time. As in Cengiz et al. (2019), we construct a control group for each treatment event from states that have not been treated within a fixed time-window in the past (see Empirical Specification). The event study presents the leads and lags of the differences between individuals from bordering counties in adopter and non-adopter states after controlling for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects.

Figure A.15: Estimates for Arrival Time, for Education and Pub. Admin.



Notes - Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 25-64 who report non-missing work arrival time. As in Cengiz et al. (2019), we construct a control group for each treatment event from states that have not been treated within a fixed time-window in the past (see Empirical Specification). Each coefficient depicted is the estimated β from our stacked difference-in-differences specification with its dependent variable being a dummy that is equal to one if an individual arrived at work before the corresponding time of the day on the x-axis. The sample is restricted to individuals in Education and Public Administration.

Figure A.16: Estimates Using Different Cutoffs for Long Hours



Notes - Data are drawn from the ACS (2005-2019). The sample is restricted to non-immigrant full-time employed workers aged 25-64 who report non-missing work arrival time. As in Cengiz et al. (2019), we construct a control group for each treatment event from states that have not been treated within a fixed time-window in the past (see Empirical Specification). Each coefficient depicted is the estimated β from our stacked difference-in-differences specification with its dependent variable being a dummy that is equal to one if an individual has worked more than the corresponding number of hours on the x-axis.

Table A.1: Adoption of Right-to-work Laws across States

Year of RTW Introduction	
Florida	1944
Arizona	1946
Nebraska	1946
South Dakota	1946
Arkansas	1947
Georgia	1947
Iowa	1947
North Carolina	1947
North Dakota	1947
Tennessee	1947
Texas	1947
Virginia	1947
Nevada	1951
Alabama	1953
Mississippi	1954
South Carolina	1954
Utah	1955
Kansas	1958
Wyoming	1963
Louisiana	1976
Idaho	1985
Oklahoma	2001
Indiana	2012
Michigan	2012
Wisconsin	2015
West Virginia	2016
Kentucky	2017

Table A.2: No. of Counties and States by Treatment Type

Treatment Type:	Would-be (Adopters)		Never (Non-adopters)	
Year	Counties	States	Counties	States
2005	9	4	11	5
2006	9	4	11	5
2007	9	4	11	5
2008	9	4	11	5
2009	9	4	11	5
2010	9	4	11	5
2011	9	4	11	5
2012	13	4	12	5
2013	13	4	12	5
2014	13	4	12	5
2015	13	4	12	5
2016	13	4	12	5
2017	13	4	12	5
2018	13	4	12	5
2019	13	4	12	5
2005 to 2019	13	4	13	5

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Each row shows the unique number of counties and states represented in our sample data in either the Would-be treated group or the Never-treated group. The last row shows the total number of unique states and counties by treatment group within our entire sample period.

Table A.3: The Effect of RTW on >45 Hours by Sector and Occupation, Bordering Counties, 20-64 year old sample

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
Dep. Var.: >45 Hours	All	Construction	Manufacturing	Retail & Wholesale	Transportation	Finance & Real Estate
RTW	0.014***	0.038*	0.032**	-0.009	0.027***	-0.012
	(0.003)	(0.019)	(0.012)	(0.008)	(0.007)	(0.017)
Observations	1,159,281	52,220	151,152	198,141	93,655	109,751
Mean of Dep. Var.	0.231	0.259	0.298	0.209	0.268	0.256
Std.Dev. of Dep. Var.	0.422	0.438	0.457	0.406	0.443	0.437
			Panel B			
Dep. Var.: >45 Hours	Business Services	Health & Personal Care	Education	Public Administration	Blue Collar	White Collar
RTW	0.004	0.012***	0.016*	0.018*	0.027***	0.006**
	(0.006)	(0.003)	(0.008)	(0.008)	(0.007)	(0.002)
Observations	144,426	234,524	105,964	57,621	201,208	955,775
Mean of Dep. Var.	0.286	0.154	0.217	0.194	0.234	0.230
Std.Dev. of Dep. Var.	0.452	0.361	0.412	0.396	0.423	0.421

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level. Industry and occupation categories were constructed using 1990 codes.

Table A.4: Demographic Heterogeneity: ACS Border, Working >45 Hours

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.: > 45 hours	Male	Female	White	Black	Hispanic	25-44	45-54	55-64
RTW	0.020***	0.010***	0.014***	0.038***	0.030***	0.022***	0.013*	0.005
	(0.005)	(0.002)	(0.003)	(0.011)	(0.008)	(0.005)	(0.006)	(0.010)
Observations	559,482	518,796	877,883	152,166	48,229	524,760	311,173	242,345
Mean of Dep. Var.	0.322	0.156	0.270	0.141	0.167	0.235	0.262	0.233
Std.Dev. of Dep. Var.	0.467	0.363	0.444	0.348	0.373	0.424	0.440	0.423

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level.

Table A.5: RTW and Rate of Union Coverage

	(1)	(2)	(3)	(4)	(5)
Dep. Var.: Union Coverage	All	Men	Women	High/Med Union Ind	Other Ind
RTW	-0.014***	-0.016***	-0.012***	-0.023***	-0.004
	(0.004)	(0.004)	(0.005)	(0.006)	(0.003)
Observations	820058	421084	398974	435764	372059
Mean of Dep. Var.	0.182	0.198	0.164	0.279	0.071
Std.Dev. of Dep. Var.	0.385	0.398	0.370	0.449	0.258

Notes - Data are drawn from the Current Population Survey Merged Outgoing Rotation Group (CPS-MORG) conducted by the US Bureau of Labor Statistics and provided by the National Bureau of Economic Research.Data from 2009-2019 are used. Observations are restricted to those from Would-be Treated and Never Treated states. All regressions control for age, gender, race, marital status, education, full/part time status, and MSA status. Fixed effects for year, state, occupation, and industry-year cells were included. Standard errors (in parentheses) are clustered at the state-year level. High/Medium unionization industries are: Construction, education, public administration, manufacturing, health, transportation, and utilities. Low unionization industries are: Personal & business services, retail & wholesale trade, and FIRE.

Table A.6: The Effect of RTW by Industry Unionization Rates, Bordering Counties

	(1)	(2)	(3)	(4)	(5)	(6)
	>45 Hours			Arriving between 5pm and 8pm		
Industry:	All	High/Med Union	Low Union	All	High/Med Union	Low Union
RTW	0.015***	0.025***	-0.007	0.009	0.012	0.002
	(0.004)	(0.003)	(0.008)	(0.009)	(0.010)	(0.007)
Observations	1078278	622853	445234	1078278	622853	445234
Mean of Dep. Var.	0.242	0.230	0.257	0.135	0.157	0.105
Std.Dev. of Dep. Var.	0.428	0.421	0.437	0.342	0.364	0.307

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level. Industry and occupation categories were constructed using 1990 codes. High/Medium unionization industries are: Construction, education, public administration, manufacturing, health, transportation, and utilities. Low unionization industries are: Personal & business services, retail & wholesale trade, and FIRE.

Table A.7: Demographic Heterogeneity: Working Hours, Bordering Counties

Dep. Var.: Working Hours (log)	(1) Male	(2) Female	(3) White	(4) Black	(5) Hispanic	(6) 25-44	(7) 45-54	(8) 55-64
RTW	0.004*	0.005	0.002	0.027**	0.015	0.010***	0.004	-0.000
	(0.002)	(0.005)	(0.003)	(0.009)	(0.009)	(0.002)	(0.007)	(0.009)
Observations	559,482	518,796	877,883	152,166	48,229	524,760	311,173	242,345
Mean of Dep. Var.	3.766	3.640	3.717	3.658	3.680	3.707	3.715	3.685
Std.Dev. of Dep. Var.	0.256	0.319	0.292	0.305	0.284	0.284	0.296	0.319

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level.

Table A.8: Demographic Heterogeneity: Hourly Wage, Bordering Counties

Dep. Var.: Hourly wage (log)	(1) Male	(2) Female	(3) White	(4) Black	(5) Hispanic	(6) 25-44	(7) 45-54	(8) 55-64
RTW	-0.017	-0.010	-0.015	-0.027***	0.054	-0.009	-0.025**	-0.003
	(0.015)	(0.013)	(0.012)	(0.007)	(0.042)	(0.015)	(0.009)	(0.015)
Observations	541,823	508,423	853,340	149,506	47,400	514,858	301,993	233,395
Mean of Dep. Var.	3.256	3.010	3.206	2.881	2.963	3.066	3.220	3.199
Std.Dev. of Dep. Var.	0.712	0.648	0.691	0.647	0.632	0.661	0.717	0.719

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level.

Table A.9: Demographic Heterogeneity: Non-Standard Schedules, Bordering Counties

Dep. Var.: Arriving between 5pm and 8am	(1) Male	(2) Female	(3) White	(4) Black	(5) Hispanic	(6) 25-44	(7) 45-54	(8) 55-64
RTW	0.008	0.010	0.008	0.023**	0.035**	0.009**	0.009	0.016
	(0.012)	(0.007)	(0.007)	(0.010)	(0.013)	(0.004)	(0.017)	(0.017)
Observations	559,482	518,796	877,883	152,166	48,229	524,760	311,173	242,345
Mean of Dep. Var.	0.168	0.100	0.124	0.179	0.154	0.128	0.143	0.143
Std.Dev. of Dep. Var.	0.374	0.300	0.330	0.383	0.361	0.334	0.350	0.350

Notes - Data are drawn from the American Community Survey (ACS) (2005-2019). Only observations from either side of an RTW border are included. All regressions control for age, gender, race, marital status, education, and state-by-event and year-by-event fixed effects. Standard errors (in parentheses) are clustered at the state level.