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Declining Unionization and the Despair of the Working Class

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Abstract: While the effects of labor unions on objective conditions have been extensively studied, little is known about its role in individual perceptions of actual economic circumstances. We investigate whether union density affects the subjective well-being of area residents by exploiting the staggered adoption of Right-to-Work laws in the United States through a border-county design. We find that unionization promotes happiness for residents of low socioeconomic status, including the non-college-educated and current/former blue-collar job holders, but has no discernible impact on their high-status counterparts. Of all affected residents, workers stand to reap the most benefit. We also find that the favorable unionization effect is transmitted through the improved assessment of financial situation, personal health, and workplace quality. This finding highlights the role of not only pecuniary, but also non-pecuniary benefits (e.g., on-the-job safety, work-life balance, interpersonal trust, and worker autonomy) that unions afford to protect the society's most marginalized groups.

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1. Introduction

Until the COVID19 pandemic hit the world in early 2020, the United States was experiencing a stretch of robust economic growth and prosperity in the decade following the Great Recession. Unemployment was at its lowest since 1970, as the economy added jobs for nearly eight years in a row between 2010 and 2018 (U.S. Bureau of Labor Statistics, 2020). For working people, therefore, the post-Great Recession recovery period should have been the best of times. Yet, despite a strong labor market, the rising tide did not lift all boats. Peering more carefully behind the strikingly low unemployment rate reveals a steadily decreasing national labor force participation rate among prime-age men, disappearing middle-wage jobs, low-wage newer jobs, rising automation in certain manufacturing industries, and growing despair evidenced by the increasing rate of substance abuse and suicide among the lowest two income quartiles (Case and Deaton, 2017; Song, 2017). While identifying the source of this despair is complex and attributable to what is termed a "cumulative disadvantage" (Case and Deaton, 2017), one significant and incontrovertible domain is the quality of the labor market.[1] To this end, we evaluate the role of labor unions, a potentially important institution tempering individual well-being by advocating for favorable contracts and working conditions for workers (e.g., Freeman and Kleiner, 1999), establishing generalized norms of labor practices through shaping local, state, and national politics, as well as lobbying on labor-friendly policies (e.g., Feigenbaum et al., 2018), and fostering a sense of solidarity among coworkers that helps insulate against work-related stress (e.g., Flavin et al., 2010).

Despite the voluminous research on how unionization influences objective outcomes such as wages and inequality, there are relatively few studies on its role in individual perceptions of, or psychological reactions to actual circumstances (see Appendix A for a more detailed discussion). The vast majority of existing literature focus on the psychological well-being of the unionized (see Blanchflower and Bryson (2020) for a review). The few studies that extend attention to non-union members and/or non-

[1] In a 2017 interview with the Guardian UK (Bible, 2017), economist Anne Case explained the significance of the labor market conditions on individual lives: "[T]he quality of the labor market may have a sweeping impact on a person's life - affecting whether a person marries, the stability of their personal lives, and whether they risk their health at work."

workers have arrived at mixed conclusions (e.g., Redcliff, 2005; Flavin et al., 2010; Keane et al., 2012; Makridis, 2019).[2] Indeed, a strong presence of labor unions can be both a boon and a bane to general well-being. On the one hand, there is evidence for a positive spillover of unionization on likely determinants of happiness, such as non-union wages and income inequality (e.g., Western and Rosenfeld, 2011), healthcare provision (Olson, 2019), occupational safety and workplace hazard protections (Zoorob, 2018), and working-class representation in state legislatures and Congress (Feigenbaum et al., 2018). On the other hand, the economic costs associated with union membership can lead to the counter-prediction of a detrimental effect. Besides the most obvious costs of union dues and potential loss of income due to strikes (Hammer and Avgar, 2005), unionization can adversely affect well-being when it impedes a firm's ability to compete and survive in a globalized world (see Doucouliagos and Laroche (2009) for a review). The inflexibility imposed by union contracts, potentially resulting in firm closures, can increase worker distress (e.g., Brochu and Morin, 2012). The decline in the value of merit may also create frustration among new and/or high-performing employees by discouraging individual creativity and discounting worker education and experience (Freeman, 1978; Borjas, 1979). The above considerations leave open the questions of whether and how unionization may affect subjective well-being on the net.

This study attempts to address both questions using the longest-running public opinion survey in the United States (US), General Social Survey (GSS) through a quasi-experimental design. Our identifying variation comes from the six US states that adopted Right-to-Work (RTW) laws from 1993 through 2018. By allowing an individual to work at any place of employment without being forced to join a union or pay union dues, the passage of RTW laws – in theory – compromises the leverage and negotiating positions of unions by lowering union density, and thereby creates a unique opportunity to bring a better understanding of causality in the extant litera-

[2]Redcliff (2005) presents evidence that cross-national differences in the extent of labor organizations play a significant role in why citizens in some nations express greater satisfaction with life than others. Flavin et al. (2009.) and Keane et al. (2012) report a similarly beneficial effect of unionization on aggregate levels of life satisfaction when considering both union members and non-members. Makridis (2019), on the other hand, finds that the passage of Right-to-Work laws in the United States, which generally weakens unions, positively affect individual current life satisfaction and economic sentiment, although these effects are restricted to union members and lose statistical significance when border-county pairs are employed for identification as a robustness check.

ture about unionization and well-being. Given that states that instituted RTW laws may differ from non-RTW states in unobservable ways that account for the differences in well-being (e.g., labor market opportunities), we focus on a group of individuals in contiguous counties that straddle state borders, balanced through their probabilities of RTW exposure (e.g., Hirano et al., 2003), and compare changes in their happiness within a difference-in-differences (DiD) framework. Meanwhile, there are variations in the RTW statute verbiage across states as well as their scope (e.g., with respect to the coverage of workers), which might cause the treatment strength (e.g., extent of unionization decline) to vary by state. Additionally, the self-reported union status and/or union density estimates are subject to measurement error issues, whose effects on union wage differentials have been extensively explored in the literature (e.g., Card, 1996). As a means to correct for these potential biases, we adopt an instrumental variable approach (IV), exploiting the varied implementation of RTW laws as an instrument for changes in union density, given that that RTW adoption reduces unionization and is plausibly unrelated to happiness except through its effect on union density.

We find evidence that one percentage-point (ppt) increase in union density boosts average levels of happiness among low-socioeconomic-status residents by 0.04 standard deviation (SD) but has a negligible impact on the happiness of their higher-status counterparts.[3] Taken together, this implies an overall 0.14 SD of well-being cost on the more economically vulnerable groups in RTW-adopting relative to non-RTW states. Furthermore, we find the unionization effect is primarily driven by the employed residents, i.e., workers, supporting findings of a positive association between union membership and worker well-being for the United States and many European countries since the 2000s (Davis, 2013; Donegani and McKay, 2012; Blanchflower and Bryson, 2020).[4] This may appear to run counter to the proposition that union members express greater job dissatisfaction than non-union members in the seminal work

[3]For classifying social status, rather than using the more conventional income and wealth-based approach, we adopt an education and occupation-based classification in this study as these are more stable across one's lifetime than income and wealth and, therefore, are less prone to the RTW legislation change. The use of income-related measures, however, results in qualitatively similar conclusions.

[4]Using the same data/GSS, along with the Gallup Daily Tracker Pool for the United States, for example, Blanchflower and Bryson (2020) find that union workers report higher levels life satisfaction, happiness, and job satisfaction than non-union workers and that they are also less likely to be stressed, worried, depressed, sad, or lonely in the post-Great Recession period.

of Freeman (1978) and Borjas (1979), but it is plausible that, post-Great recession, rising uncertainty enhanced the appreciation of a secure work environment facilitated by labor unions, especially among individuals born between the 1960s and 1990s, i.e., cohorts younger than those who would have made up most of the sample in Freeman and Borjas's studies in the 1970s (Blanchflower and Bryson, 2020).[5]

More specifically, this study makes three principal contributions to the literature. First, relative to existing studies on RTW laws, we draw evidence from a longer data set that accommodates RTW adoption prior to the 2000s. This plays a crucial role in quantifying the effect of unionization, partially because the 1980s/1990s represent a time when both the unionized proportion of the workforce and the absolute number of active union members decline considerably (Farber and Krueger, 1992), which provide us with the variations necessary to infer the causal impact of unionization. More importantly, our longer data set allows us to account for potentially delayed responses to RTW among individuals who might be only indirectly affected (i.e., non-union workers and/or non-workers) and speak to a longer-term well-being effect of the RTW laws. It takes a certain period of time for collective agreements to be re-negotiated[6] and for unions to contribute to the improvement in living conditions for the society at large. In this sense, studies that exclusively rely on post-2000s data are only able to focus on short-term outcomes and what remains unknown is whether there might be longer-run repercussions of a declined union density, given that the majority of RTW adoptions under consideration occurred after 2011.[7] Finally, because the underlying mechanisms for short and long-term effects may be entirely different, the additional sub-group analysis undertaken in current study, relative to prior research (e.g., Makridis, 2019),

[5]Besides the exit-voice hypothesis posited in Freeman and Medoff (1984), where union workers might be more likely to complain about working conditions than non-union members to rectify dissatisfying circumstances, many studies who find a negative association of union membership with job satisfaction also attribute the observed differences, at least in part, to differences in the objective characteristic of the job (e.g., nature of job tasks and working conditions), worker preference, and/or the propensity to unionize among the already dissatisfied (Berger et al., 1983; Hersch et al., 1990; Bender and Sloane, 1998; Bryson et al., 2005; Hammer and Avgar, 2005).

[6]While the National Labor Relations Act does not specify any length of time for a labor contract, in practice all collective agreements have a specified length. The normal term of a contract is three years, although in recent years many contracts have moved to longer terms, four or five years, for example (Compa, 2014).

[7]The average time elapsed between the enactment of RTW laws in Makridis (2019) and the measurement of well-being is three years, for example, whereas the average length of post-intervention period in this study is six years.

may uncover variability in the direction and magnitude of the average treatment effects for individuals within a population and unmask heterogeneous effects of RTW laws that predominantly afflict society's most marginalized groups.

Second, besides the intent-to-treat (ITT) analysis conventionally performed in existing RTW studies using border-county design (Holmes, 1986; Feigenbaum et al., 2018; Makridis, 2019), we provide the first local average treatment effect (LATE) estimate of unionization on well-being, acknowledging that neither the legal language nor compliance with RTW laws may be uniform across states, along with the potential measurement errors in union density estimates.

Finally, different from related studies, the main conclusions of this study are replicated through a synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al., 2010). Owing to data limitations, this exercise is performed for one switcher state that offers the most post-RTW information and largest donor pool in the sample (i.e., Oklahoma). While not necessarily generable to other RTW-adopting states at present, this method allows for time-varying unobserved heterogeneity (e.g., anti-union sentiment) and, to some extent, spillover effects of the RTW laws across state borders (e.g., through competition), and thus provides supporting evidence for the identifying assumption of the border-county analysis that, with the exception of the adoption of RTW laws, neighboring border adjacent counties are politically, culturally, and economically similar.

In what follows, Sections 2 and 3 describe our data and estimation strategy, respectively. Sections 4 reports results from both main analysis and robustness checks. Section 5 conducts a preliminary mechanism investigation and Section 6 concludes.

2. Data

2.1 Measures of Subjective Well-Being and Unionization

To study the relation between unionization and happiness, we obtain data from the 1988-2018 GSS, while focusing on the period of 1993-2018 for the border-county analysis when the county location of an individual's residence was reported.[8] The GSS is

[8]The GSS unfortunately does not provide any information on the location of an individual's job(s). This omission could result in an over or under-estimation of the true unionization effect for individuals who live and work in different states where the RTW status is not the same. Additional

a nationally representative survey that tracks both social characteristics and attitudes of American adults over time. It has been conducted every year prior to 1994 (except in 1992) and biennially beginning in 1994, surveying approximately 3,000 individuals in each round. Besides its standard core of demographic, behavioral, and attitudinal questions, the GSS collects information on a broad spectrum of topics such as domain specific psychological well-being (e.g., financial and employment satisfaction) and job experiences (e.g., perceptions of employee-employer interactions) in selected years. The presence of these variables enables us to infer about the well-being implications of unionization in a more comprehensive fashion than is possible with other more traditional labor data sources like the Current Population Survey (CPS).

While the phrasing of the GSS questions was modified to some extent over the years, we are able to identify three essential well-being indicators for all individuals: 1) general happiness (i.e., *"Taken all together, how would you say things are these days–would you say that you are very happy, pretty happy, or not too happy?"*), 2) financial satisfaction (i.e., *"So far as you and your family are concerned, would you say that you are pretty well satisfied with your present financial situation, more or less satisfied, or not satisfied at all?"*), and 3) job satisfaction (i.e., *"On the whole, how satisfied are you with the work you do (including housework) – would you say you are very satisfied, moderately satisfied, a little dissatisfied, or very dissatisfied?"*). The raw response of an individual to each question is first assigned an integer value, ranging from the least desirable response option value equal to one to the most desirable equal to the total number of response options. Each measure is then standardized for all individuals to have a mean of zero and a SD of one, so that all resulting regression coefficients can be interpreted in terms of changes in SD.

Measures related to union density are from the Union Membership and Coverage Database, which provides estimates compiled from the CPS using the same method the Bureau of Labor Statistics uses for publishing estimates at the national level (Hirsch et al., 2001).[9] To best capture state-level variations in union strength, we consider six

investigations in this regard can be important extensions to current study, especially in light of the surge in remote work during and potentially after the COVID-19 pandemic.

[9] Letting w_{ij} represent the annualized CPS sample weight for individual i in group j, where group can be state, metropolitan area, industry, or occupation, employment for group j is: $Employment_j =$

different definitions of union density: the union membership and coverage rates for all sectors, private sectors, and the manufacturing sector. Union membership is the percentage of workers who are members of unions, while the coverage rate represents the percentage of both union members and workers who report no union affiliation but whose jobs are covered by a union or an employee association contract. Since non-union members of a collective bargaining unit in RTW states can benefit from union presence without paying union dues, the distinction between union membership and bargaining coverage may not be trivial, depending on the institutional environment in which a union operates. Therefore, we use both measures to check the sensitivity of our results.

2.2 RTW Legislation

RTW laws are the counter-response to the National Labor Relations Act (NLRA) of 1935, which granted state-level unions the power to get employees fired for refusal to join a union. Immediately, a movement to oppose such statutory sanctions ensued. Eventually, the 1947 Taft-Hartley amendments to the 1935 NLRA granted states the power to permit workers in a unionized workplace to opt out of paying membership fees, even if those workers enjoy benefits from collective bargaining and union representation. Before of the passage of the 1947 Taft-Hartley Act, five states had already passed such laws (i.e., Arkansas and Florida in 1944; and Arizona, Nebraska and South Dakota in 1946). Since then, 21 additional states have passed RTW laws, with the greatest concentration of them in the West and Southeast regions (see Appendix Table 1).[10]

3. Methodology

2.1 Identification Strategy

$\sum w_{ij}$. Letting $M_{ij} = 1$ if individual i is a union member in group j and 0 otherwise, and likewise, $C_{ij} = 1$ if individual i in group j is covered, then union membership and coverage density estimates measure the percentage of employees who are members or covered, respectively, defined as: $\%Mem_j = (w_{ij}M_{ij}/w_{ij}) * 100$, and $\%Cov_j = (w_{ij}C_{ij}/w_{ij}) * 100$. See Hirsch and Macpherson (2003) for a detailed discussion of the methodology.

[10] The RTW law in Texas was originally passed in 1947 and then modified to its current form in 1993. While there is evidence suggesting that the 1947 legislation does not provide any specific means of enforcement (Meyers 1955), we consider it as an always RTW state to obtain the cleanest estimate. In a robustness check, we repeat our analysis treating September 1, 1993 as the enactment date of RTW and find our results remain largely the same.

Our empirical analysis begins with a standard DiD model comparing changes in happiness for all individuals in RTW-adopting ("switcher") states with those in non-RTW states from 1993-2018:

$$Y_{isdt} = \alpha_1 RTW_{sdt} + X'_{isdt}\theta + \pi_{sd} + \sigma_{dt} + \varepsilon_{isdt} \tag{1}$$

Here Y_{isdt} denotes happiness for individual i who live in state s and census division d at survey year t. State fixed effects (π_{sd}) capture state-level determinants of happiness that are stable over time. The census division by year fixed effects (σ_{dt}), consisting of a set of dummies identifying each census division and year pair in the sample and subsuming census division and year fixed effects, effectively controls for the unrestricted time trends in outcome within census divisions. The vector X includes individual-level attributes that are likely correlated with the enactment of RTW laws and also affect well-being: gender, age (in five categories), years of schooling, highest degree obtained (in four categories), race (a dummy for white), household size, marital status, the presence of children under the age of six, and the interaction terms between white and other covariates.[11]

Since switcher states might be systematically different from non-RTW states, we replicate the analysis for individuals in neighboring counties across a RTW border. Because a county may be located on the border of multiple states, we follow the existing literature (Dube et al., 2010; Feigenbaum et al., 2018) and allow counties bordering other counties from multiple states to pair with each other and stack the data accordingly.[12] Due to the stacked nature of the data, a slightly different DiD model is employed:

$$Y_{icst} = \beta_1 RTW_{st} + X'_{icst}\theta + \rho_{cs} + \pi_{bt} + \varepsilon_{icst} \tag{2}$$

[11] Socioeconomic characteristics likely affected by the RTW laws, such as weekly work hours and household income are left out of the equation to avoid potential mechanical endogeneity. From this perspective, analyses in Section 4.3 would under-estimate the true happiness effect of unionization if the adoption of the RTW laws promotes job growth (e.g., Holmes 1998) and employment is associated with a higher level of subjective well-being (e.g., Frey and Stutzer 2002).

[12] For example, individuals living in a county straddling three state borders will appear in our dataset three times each year.

where ρ_{cs} is county fixed effects and π_{bt} is border-pair by year fixed effects accounting for the unrestricted time trends within border pairs to ensure that only the variation from county pairs with different RTW statutes identifies the main RTW effects.

While having the advantage of estimating the net effect of RTW laws, the above specification ignores potential state differences in RTW statues and the degree of compliance. A close comparison of the legal language used in the various RTW statues after 1980 reveals that individual statutes do vary in their coverage of workers and in their penalties and remedies (e.g., invoking civil versus criminal laws or differences in the magnitude of fines or penalties), though the legal "bite" of the laws is not significantly different across states (Feigenbaum et al., 2018). These differences in coverage and deterrents may incentivize unions and employers differently with respect to abiding by the legal constraints. Even in the absence of a violation of the open-shop provisions in switcher states, individual decisions on whether to pay union dues or join the union can vary by states depending on, for example, the prevalence of anti-union sentiments, resulting in a divergence between the LATE and the ITT effect of RTW laws.

In addition, there are some well-documented measurement errors in the estimates of union density, which can bias the estimated unionization effect upward or downward (e.g., Card, 1996 and Olson, 2019). These measurement errors can be caused by household sampling variability in the CPS or erroneous and/or biased responses from individuals when answering the relevant survey questions. Such measurement errors have long been suspected to account for, at least partially, the wide range of values for the union wage gap found in the literature (Bollinger, 2001). To circumvent these concerns, we implement a DiD-IV approach using the varied implementation of RTW laws as the source of identification:

$$Y_{icst} = \gamma_1 \widehat{Density}_{st} + X'_{icst}\theta + \rho_{cs} + \pi_{bt} + \varepsilon_{icst}, \tag{3}$$

where $\widehat{Density}_{st}$ represents variations in union density attributable to RTW legislation and γ_1 provides a consistent estimate of the LATE effect of unionization on happiness.

As the final step to minimize the influence of confounding factors, we apply a propensity-score re-weighting (PSW) technique to balance the distribution of observable characteristics between the switcher and non-RTW states. While dimensionality is not a concern in our context, this procedure eliminates the linearity assumption and, therefore, allows our results to be more robust to misspecification than parametric DiD models (Hirano et al., 2003). Specifically, we estimate the conditional probabilty that a switcher state passes RTW legislation ($\widehat{p(X_i)}$) using the covariates in our parametric model to identify the states who are, on average, the most similar over the duration of the observation period, and then use these estimates to weight outcome values (Y_i):

$$E(Y_1 - Y_0) = \frac{1}{n}\sum_{i=1}^{n}\frac{W_i Y_i}{\widehat{p(X_i)}} - \frac{1}{n}\sum_{i=1}^{n}\frac{(1-W_i)Y_i}{1-\widehat{p(X_i)}} \tag{4}$$

where $W \in (0, 1)$ denotes treatment status and n is the proportion of treated units. In this way, observations with large $\widehat{p(X_i)}$ will be weighted down when treated and weighted up when untreated (and vice versa). Because the inverse of these propensity scores may overinflate the influence of observations at the ends of the distribution, we further calculate the 5th and 95th percentiles of the propensity score and remove the observations that fall outside these limits (Imbens 2015).[13]

The reliability of above estimates rest on the fact that the counterfactual happiness of border-county individuals in switcher states exhibit similar patterns as those in non-RTW states. To assess the validity of this identifying assumption, a series of robustness checks are performed. First, we conduct an event study analysis to evaluate the possibility that the enactment of RTW laws is correlated with any pre-existing differences in happiness in the switcher and non-RTW states. Failure to find any differential trends in happiness immediately prior to the implementation of RTW would suggest that the common trend assumption is likely satisfied. Second, we employ a synthetic control (SCM) approach to create a weighted control group by matching moments of key variables in the pre-RTW period between one switcher state, Oklahoma

[13] We also check the sensitivity of our results by repeating the analysis for the samples trimmed at 90th and 85th percentiles of the propensity score. Our findings remain qualitatively unchanged.

and the non-RTW states. While acknowledging that SCM is restricted in external validity, we use the approach to demonstrate that, at least in the case of Oklahoma, the observed gap in happiness between the switcher and non-RTW states is unlikely a result of unobserved differences across states. Finally, we address the issues related to pre-intervention unionization.

2.2 Descriptive Statistics and Balance Diagnostics

Table 1 provides the first look at the balancing achieved between the switcher and non-RTW states before and after PSW for our border-county sample, consisting of 84,074 individuals residing in 290 border counties, which is over one half of the 452 counties observed in the GSS (also see Figure 1).[14] In the raw sample (columns 1-2 of Table 1), individuals exposed to RTW legislation report a lower level of happiness (2.18 vs 2.20). They are also more likely to be male, older, and have a higher level of education, particularly associate's degrees. Once trimmed and re-weighted by the estimated propensity scores (columns 3-4 of Table 1), however, all of these differences disappear except the observed gap in happiness between individuals from RTW and non-RTW states.

Figure 2 plots corresponding probabilities of RTW adoption after adjusting for covariates. While switcher states tend to have a higher density for the high values of the propensity scores than non-RTW states, the PSW procedure brings both much closer to each other, as is formally confirmed by the insignificant Hosmer-Lemeshow statistic. Similar results are obtained for the low and high-status-individual samples where the propensity scores are estimated separately.[15] Thus, the PSW technique we employ seem to be sufficiently flexible to balance the distribution of the observed characteristics between switcher and non-RTW states.

4. Main Results

4.1 Difference-in-Differences Specification

The DiD and PSW-DiD estimates of the RTW effect on happiness for all individuals and those in border counties are presented in Table 2, where standard errors are

[14] Throughout the analysis, cross-sectional survey weights are employed to reflect representativeness and to adjust for non-response.

[15] For the sake of brevity, these results are not included but they are available upon request.

clustered two-way by state and border-pair to account for potential serial correlation among observations on the same border pair over time. First, across samples and specifications, we observe that the estimated RTW coefficient fluctuates to some extent, suggesting that pre-RTW differences in happiness between switcher and non-RTW states – while bearing no substantive implications for our results – do exist. Conditional on county, year, and border-pair fixed effects, the RTW coefficient is smaller in magnitude when the unrestricted border-specific time trends are considered (i.e., 0.05 vs 0.08 SD decrease in happiness), implying that relative to non-RTW states, switchers tend to experience faster deterioration in happiness in the absence of RTW laws, such that once concurrent local trends are factored out, the RTW effect diminishes. It is consistent with our expectation as well, to see a smaller change in the RTW coefficient in the presence of within-entity time trends for the border-county sample relative to that for the all-county sample. If neighboring counties are indeed culturally and economically similar before the inception of RTW laws, then local trends should play a smaller role compared to those for any arbitrary counties in the US. Second, we find that even in the most comprehensive specification where confounding biases are removed through the PSW procedure (column 6), an adverse well-being impact of RTW laws is still apparent (at 0.08 SD).

4.2 Propensity-Score Weighted Instrumental Variable Approach

The PSW-IV estimates of the unionization effect on happiness using our preferred specification (column 6 of Table 2; also see column 1 of Table 3), through six different definitions of union density, are presented in Table 3. Focusing on results from the first stage (row 3), it is evident that the RTW legislation leads to significant declines in unionization regardless of the measure used for union density, with the partial F statistics ranging from 18 to 61 in the presence of two-way clustering by state and border-county pair. Taking union membership rate as an example (columns 2-4), the passage of RTW laws is associated with a 1.3, 5.1, and 3.2 ppt reduction in the membership rate of all sectors, private sectors, and the manufacturing sector, respectively. Since the RTW-unionization association appears to be strongest for the manufacturing sector (column 4; F=61) among the six measures, we treat it as our preferred specification.

Switching to the second-stage results (row 1), in which the predicted changes in union density from the first stage are included in the original model of happiness, we continue to find a negative happiness-RTW association. Specifically, the coefficient for union density in our preferred specification indicates that one ppt increase in the union workforce leads to a 0.024 SD increase in happiness. Given that the passage of RTW laws is associated with a 3.22 ppt decline in unionization, combining these two effects yields a net negative effect of the RTW laws on happiness of 0.077 SD, nearly identical to the ITT (DiD) estimate seen in column 1 (0.078 SD).

4.3 Heterogeneity

While the previous analyses suggest that unions have an overall positive effect on happiness, one might wonder whether the observed effect differs by an individual's socioeconomic characteristics and/or work arrangement. Compared to high-status individuals, there are several reasons to anticipate county residents of low socioeconomic status, especially labor market participants, to be the main beneficiaries of union efforts. First, hamstringing unions' ability to collect administrative fees from the workers they represent, RTW laws likely result in diminished bargaining power of unions to affect the material conditions in workplace (e.g., wages, benefits, etc.). If the marginal utility of time and money is greater for individuals at the lower end of the income distribution, we expect to see a disproportionate unionization effect on the less advantaged group. Second, individuals of lower socioeconomic status generally have fewer alternatives for employment than their higher-status counterparts. Even if they are unhappy at a job, it may be more difficult for them to "exit" due to limited outside options (Freeman and Medoff, 1984). Thus, the collective voice of a strong union may be more important to the well-being of this segment of the population (Korpi and Shalev, 1979). Third, increased globalization and automation may result in a changing composition of jobs that primarily impact blue-collar and low-education individuals. In the context of the wide-ranging protections against labor market volatility that unions offer, their role can be particularly critical for unskilled labor who are faced with a more elastic demand and for groups that face discrimination, such as women and people of color who are most impacted by unequal treatment, unfair pay gaps, etc.

Defining low-status residents to be the non-college-educated and/or individuals who used to or were working in blue-collar occupations at the time of the survey[16] and separately estimating our preferred PSW-IV model for different status groups confirm this hypothesis. We find that unionization increases the average level of happiness for low-status individuals by 0.04 SD – doubling in size compared to the unionization effect observed for the full sample – but has no discernible impact on their high-status counterparts (Panel A of Table 4).[17] Moreover, the observed effect is concentrated on workers, i.e., labor market participants (Panel B of Table 4). This is plausible if unionization results in improved working conditions, and the increased job satisfaction, in turn, contribute to the greater overall well-being, thus pointing toward workplace quality as a potential mechanism through which unionization may affect happiness.

In unreported results, we also investigate whether the unionization effect is conditioned by union membership. Re-running the original model seperately for union and non-union members reveals a favorable unionization effect for both groups, though the magnitude for union members is 2-5 times greater than that for non-union members, depending on whether the union status of the spouse is taken into consideration. This makes intuitive sense since union members are likely the first to be adversely affected by the loss of union power, while any societal benefits of unionization on non-union members are presumably weakened by the probability of spillover. However, we need to be cautious in interpreting these results. Unlike the CPS, GSS provides informa-

[16]The occupational classification variable was constructed by the GSS based on the 2010 U.S. Bureau of the Census 3 or 4-digit occupation classification and responses to the following three questions: *[1] "What kind of work do you (did you normally) do? That is, what (is/was) your job called?"; [2] "What (do/did) you actually do in that job? Tell me, what (are/were) some of your main duties?"; and [3] "What kind of place (do/did) you work for?*." Following the existing literature (e.g., Brochu and Morin 2012), we make the distinction between professional/service and blue-collar occupations. White-collar occupations include management, business and financial operations, computer and mathematics, architecture and engineering, life, physical and social sciences, community, and social service, legal, education, training, library, arts, design, entertainment, sports, media, and healthcare practitioners and technical occupations. Service occupations include healthcare support, protective services, food preparation and servicing-related, building and grounds cleaning and maintenance, personal care, sales, office, administrative support, and military occupations. Blue-collar occupations include farming, fishing, forestry, construction and extraction, maintenance and repair, production, transportation, and material moving.

[17]We also estimate the unionization effect seperately for the non-college-educated and current/-former blue-collar job holders and obtain qualitatively similar results across these two sub-groups.

tion on individual union membership but not union coverage. Given that a worker can receive benefits from collective bargaining without joining a union under RTW laws, the existence of free riders not captured by our definition of union members can skew our estimates. On the one hand, if free riders report a lower level of happiness due to lower incomes (indicating a higher marginal utility of money) than fee-paying members before the passage of RTW laws, then the omission of this group from post-RTW period will result in an under-statement of the true RTW effect for union members. On the other hand, since free riders receive the benefits but do not pay fees, the boost in income could raise their well-being, implying an over-statement of the true RTW effect on union members. As neither the direction nor magnitude of this bias is obvious in this context, we do not attach much emphasis to this set of results, although they are available upon request.

4.4 Robustness Check

4.4.1 Event Study Analysis

The key assumption of our identification is that absent the introduction of RTW laws, happiness would evolve similarly across the neighboring counties straddling a state border. This assumption can be violated in the presence of any time-varying confounders that jointly affect the adoption of RTW laws and subjective well-being. For example, suppose that the increased competition from RTW states forces wages down in non-RTW states and also forces the non-RTW states to respond to the possible exit of firms to RTW states by adopting RTW laws, then we could observe a similar happiness gap post-RTW. To investigate this possibility, we first conduct an event analysis by focusing on the three switcher states – Oklahoma, Indiana, and Michigan – that provide at least three survey years (i.e., six calendar years) of post and pre-RTW data in the GSS.

One advantage of the event study is that it does not impose any *ex-ante* restrictions on when the structural break will occur and therefore relaxes the standard assumption of DiD that treatment is associated with one-time level shift in the outcome. The observed lead effects also provide us with an important falsification test about any differential, pre-existing trends in the switcher states that may confound our estimates.

Specifically, we estimate a variant of the DiD equation:

$$Y_{icst} = \sum_{k=9}^{-11} \delta_k RTW_{st}^k + X_{icst}^{'}\theta + \rho_{cs} + \pi_{bt} + \varepsilon_{icst} \tag{5}$$

Here, RTW_{st}^k is a series of indicator variables that reflect the time $t = -11, -9, \ldots, 9$ that the RTW laws take effect in county c of state s, k survey years following the passage of the legislation. Since each coefficient in the regression is estimated relative to the year prior to RTW adoption, δ_k represents the change in happiness relative to its pre-RTW level k years after the RTW laws pass.

Owing to the smaller sample sizes and limited identifying variations, the PSW technique is not employed in this exercise, and as demonstrated in Figure 3, the estimated confidence intervals tend to be relatively large for both all (left panel) and low-status individuals (right panel) than those in the main analysis. Despite of these differences, a familar pattern emerges. We observe a clear downward shift in the average level of happiness for low-status individuals, the primary beneficiaries of union efforts, which begins in the adoption year of RTW laws and persists over the post-intervention window. Importantly, the estimated δ_k's in the six pre-adoption years are statistically insignificant, exhibiting no specific upward or downward pre-trends. Thus, the event analysis suggests that even if the RTW adoption decisions were driven by factors such as increased competition caused by some switcher states, they had no material impact on well-being during our observation window, at least as far as Oklahoma, Indiana, and Michigan are concerned.

4.4.2 Synthetic Control Analysis

As a more systematic assessment for the role of hidden factors that vary over time, we implement a synthetic control analysis (Abadie and Gardeazabal, 2003; Abadie et al., 2010) to estimate the ITT effect of the RTW legislation. This method relies on state-level data to construct a counterfactual path of happiness for a switcher state using a weighted average of non-RTW states, where the weights are assigned to best resemble the pre-intervention pattern of happiness in the switcher state. Since it allows the effects of unobserved characteristics to vary over time and for potential inter-state spillover effects of the RTW laws to exist, consistent results obtained in this section with those under the standard DiD specification would serve as an indication that the observed RTW effect is not an artifact.

To smooth out stochastic variation in our estimates and produce results as generalizable as much as possible, ideally, we would adopt a method that aggregates multiple events into a single treatment effect (e.g., Ben-Michael et al., 2021). However, this idea is frustrated by the fact that close to half of the non-RTW states are observed inconsistently (e.g., 10 out of 24 states) – since the primary sampling unit of the GSS is region rather than state – which prevents the achievement of a satisfactory pre-intervention fit, an important practical requirement for the use of synthetic control method (Abadie, 2021). Furthermore, the vast majority of switcher states in our sample did not adopt the RTW laws until after 2012, providing relatively little post-intervention information. For these reasons, we decide to focus on the earliest adopter of the RTW laws, Oklahoma, so that later adopters can be utilized to construct the synthetic controls (e.g., Indiana and Wisconsin). This approach effectively boosts the size of the donor pool by nearly 30%, while permitting a reasonably long post-intervention window in the event where the RTW effect may precipitate after a considerable delay (i.e., 1988-2010).[18]

Following the notation used in Abadie et al. (2010) and indexing units $j = (1, \ldots, J+1)$ such that the first unit is Oklahoma and the others are donor states, the SCM estimate of the RTW effect is computed by subtracting a linear combination of happiness in the donors (Y_{jt}) from actual happiness (Y_{1t}) in Oklahoma $Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$ for the post-RTW period, where w_j^* is a weight for j. While any potential weighted average of donors is a synthetic control, the standard approach is to choose weights based on minimizing the pre-intervention Root Mean Square Prediction Error (RMSPE) through a regression-based method. Since the standard approach maximizes in-sample fit, likely resulting in a prediction that would perform poorly out of sample, a data driven alternative or cross-validation technique has also been proposed in the literature that exploits the first half of the pre-intervention trend to form the synthetic match and reserves the second half for out-of-sample validation and weight

[18]We restrict the start date to 1988 as some questions were not asked consistently due to the rotation design of the GSS in earlier years.

selection (e.g., Cavallo et al., 2012). We utilize results from both methods to check the sensitivity of our results.

For inference, we implement the classical permutation test by examining whether or not the estimated RTW effect falls well inside the distribution of placebo estimates, that is, the effects estimated for donors when a fictitious RTW law is assigned at the same time as the state of interest. More specifically, we compare the post-pre RMSPE ($\frac{PostRMSPE}{PreRMSPE}$) of the actual happiness less the synthetic control predictions for Oklahoma to the distribution of the donors. The ranking of Oklahoma relative to the donors for those ratios determines the significance level for the estimated RTW effect.

The left panel of Figure 4 illustrates the SCM estimates for all individuals using both the standard and cross-validation methods, where we divide the pre-treatment period roughly in the middle into a 1988-1993 training period and a 1994-2000 validation period for the latter.[19] The vertical line indicates the enactment year of RTW legislation. We observe remarkably similar outcome paths from both approaches pre-RTW, with the standard approach leading to a slightly better match. Both synthetic units struggle to track the trends of actual happiness prior to 1994 but are able to closely resemble its trajectories in the eight years immediately before the passage of RTW laws.[20] Due to the volatility of the data, the overall fit measures for the full sample are relatively poor compared to those for low-status individuals (right panel), with 72%-89% of placebos having a pre-intervention RMSPE at least as large as Oklahoma. Perhaps due to this reason, despite a lower average in actual happiness in the post-RTW period (by 0.08 SD), the standardized placebo-based p-value seen in Appendix Table 2 ranges from 0.11-0.55, which indicates that more than 10% of the placebo effects from donors have a post-intervention RMSPE at least as great as Oklahoma,

[19] Since the training/validation split is arbitrary, Appendix Figure 3 displays actual happiness and 5 versions of synthetics that result from different partitions of the pre-treatment period from 1993 through 2000, with darker lines indicating estimates using a longer training period. As shown, in spite of small variations in the ability of the synthetic control to replicate the counterfactual trajectory from the original model, we do not find any systematic upward bias associated with the choice of the threshold.

[20] A set of predictors perceived to contribute to the average level of happiness in Oklahoma are included in the SCM models, such as lagged values of happiness, share of population in blue-collar occupation and in the lowest quartile of income distribution, rates of labor market participation and employment, weekly work hours, occupation composition, proportions of individuals who are white and married, average rating of health status, and state-level union density. The specific combination varies with the data sample.

after taking into consideration the pre-intervention match quality.

Repeating the exercise for individuals of low status produces an improved pre-intervention fit as seen in the left panel of Figure 4 (i.e., Oklahoma having a better pre-intervention match than 93-94% of placebos), allowing the synthetic units to mimic the rises and falls of actual happiness over the entire pre-RTW period. In the post period, a clear decline of actual happiness relative to its synthetic counterparts emerges (i.e., placebo-based p=0.00), averaged at 0.20 SD, 8 ppt greater in size than the RTW effect obtained from the border-county analysis, when we implement the DiD model in column (6) of Table 2 for the same set of states over the same time period (i.e., 0.12 SD).

To evaluate the credibility of these results, several robustness checks suggested in Abadie (2021) are performed in Appendix B. In particular, we examine whether the observed gap in happiness is driven by 1) a lack of predictive power of our SCM models, 2) our choice of donor pool, and 3) inter-state spillover effects. These results, cross-checked with those obtained through the cross-validation method (i.e., Figure 4 and Appendix Figure 3) confirm our original conclusion that RTW laws lower self-reported happiness. It is also worth noting that no significant drop in the happiness gap is observed when the border states of Oklahoma are excluded from the analysis. Assuming that the well-being of a non-RTW state is more likely to adversely react to the RTW adoption of their neighbors than non-neighbors, then it corroborates findings from the event study analysis that even if confounders such as increased competition from an RTW state drive the RTW-adoption decision in a non-RTW state, they are not important correlates of happiness in our context.

4.4.3 Anti-Union Sentiment and Political Orientation of Governments

A distinct, but related concern to increased competition is a state's pre-existing opposition to unions, which potentially affects both the state's probability of RTW adoption (Bryson et al., 2019) and residents' rating of subjective well-being (Okulicz-Kozaryn et al., 2014). A similar argument can also be made for the change in the political orientation of state government that occurs simultaneously or immediately before the passage of RTW laws. For example, one way of interpreting the observed decline in happiness is the political shift toward a Republican governor and legislature majority in historically Democratic states (e.g., Michigan and Wisconsin) rather than the RTW laws per se. While the SCM results obtained for Oklahoma provide indirect proof that an adverse well-being effect of RTW laws exists in the absence of any drastic changes in anti-union sentiment and/or partisan composition of government upon the enactment of the law – given that Oklahoma has long had low union density relative to some of the more recent switchers – we conduct some formal analyses to investigate this hypothesis.

Columns 1-4 of Appendix Table 4 first repeats the border-county analysis seperately for states whose membership/coverage rate was at or above the national average one year prior to the adoption of RTW laws (i.e., Michigan, Wisconsin, West Virginia, and Kentucky) and states whose unionization rates fell below (i.e., Oklahoma and Indiana). If anti-union sentiment is responsible for explaining our results, then we expect to see a greater unionization effect for the former. The evidence we find, however, suggests the opposite. While a positive unionization-happiness association is present for both groups, the size of the unionization effect is greater for the states with weak versus strong union support. Given that the states with weak union support also tend to be early adopters of the RTW laws, allowing for an equal follow-up window by focusing on the states that have at least three survey years of post-intervention data (i.e., Oklahoma, Indiana, and Michigan) does not lead to any substantively different conclusions.

To explictly evaluate the role of electoral outcomes, columns 5 and 6 of Appendix Table 4 add the partisan composition of each state government to the original border-county regressions. It is measured by the percentage of the state legislature controlled by the Republican Party in the senate and house/assembly, respectively, as well as the party affiliation of the state governor in the beginning of a calendar year.[21] There is mixed evidence on whether the Republican control of governments is associated with

[21] These variables are constructed from the various volumes of the Book of the State by the Council of State Governments (*https://www.csg.org/work/publications/*) and the National Conference of State Legislatures website (*https://www.ncsl.org/research/about-state-legislatures/partisan-composition.aspx*). Due to their unique legislative organization, Nebraska and District of Columbia are excluded from this exercise.

a lower level of well-being, although no fundamental meaning is attached to these estimates due to unobserved heterogeneity. Importantly, conditional on these variables, the unionization-happiness relationship remains qulitatively unchanged. To the extent that the sustained decline of organized labor may hurt Democrats in elections (Feigenbaum et al. 2018), this approach could lead to an under-estimation of the true effect of unionization. Thus, even in the face of a likely downward bias, we arrive at a similar conclusion.

In summary, Section 4.4 presents several robustness checks for the plausibility our identifying assumption. The results provide little support that our estimated unionization or RTW effect is biased by unobserved differences between switcher and non-RTW states. It is, however, important to note that even if our estimates survive a battery of indirect tests, unobserved heterogeneity cannot be completely ruled out as a possible explanation due to the limitations of our data and methods employed.

5. Exploring Mechanisms

In Tables 7, 8, and 9, we explore the possible mechanisms through which the RTW laws could adversely affect individual well-being.

5.1. A Domain Satisfaction Approach

The domain satisfaction model of psychology (Campbell 1981; Easterlin and Sawangfa, 2009) views global well-being as a net outcome of reported satisfaction with major domains of life such as financial situation, health, and so on. Tables 5-6 thus decompose happiness into domain satisfaction for low-status individuals, where the unionization effect is concentrated, by utilizing responses to three relevant questions consistently asked in the GSS – financial satisfaction (column 1), self-rated health (column 2), and job satisfaction (column 3) – to understand which specific component(s) of happiness drives the observed well-being differential.[22],[23] The estimated unionization effects using the preferred model specification (i.e., column 4 of Table 3) for all individuals (Panel

[22]The respondents in GSS were also asked of their satisfaction with family, friends, and health prior to 1994. These questions were dropped in later years and therefore cannot be used for our purposes.

[23]Subjective health is modelled as a binary indicator for a report of "excellent" or "fair" health, its top two categories (and zero otherwise) to account for potential non-linearity. Coding it as a continuous scale generates less pronounced but qualitatively similar results.

A) and workers (Panel B) are presented in Table 5. Overall, we find that labor unions contribute to both financial and job satisfaction and that the effect size is roughly the same in magnitude (i.e., 0.05-0.06 SD). Personal health also is favorably affected by unionization, but a significant effect is only present among workers. This is likely if the health-related benefits are primarily conferred through workplace safety rather than other public health initiatives that extend to non-workers (e.g., the Affordable Care Act).

Table 6 further investigates the plausibility of finance, health, and employment as channels of transmission by additionally including these measures in our original models. If the unionization effect operates through these channels, then we would expect a decline in the estimated density coefficient under these specifications. Across samples and model specifications, we find the domain satisfaction variables to be positive and significant on their own, indicating that a more favorable perception of the individual's own financial situation, personal health, and work environment strongly predicts more happiness. Once all three measures are controlled for (columns 3-6 of Table 6), the observed unionization effect completely vanishes, implying that it is fully mediated by improvements in these three domains of happiness. In particular, the inclusion of financial satisfaction is associated with the biggest decline in the estimated density coefficient (34%), followed by job satisfaction (27%), and then personal health (10%). While these domain satisfaction measures are highly correlated with each other and are potentially endogenous in the equation, results from this exercise provides suggestive evidence that both economic and non-economic factors may be at play in mediating the observed unionization-happiness relationship.

5.2. A Closer Examination of Workers

Given the disproportionate effect of unionization observed for workers, Appendix Table 5 provides more evidence on how the quality of life evolved for these individuals after the passage of the RTW laws by focusing on a group of randomly selected workers in the Quality of Work life (QWL) module of the GSS, who provided opinions on related topics including financial reality, workplace safety, work-life balance, discrimination, employee-employer interactions, productivity and promotion. While the statistical power is restricted by the relatively small sample size (N=7,798), this exercise allows us to uncover the varied aspects of each domain satisfaction – finance, health, and employment – that might have contributed to or detracted from workers' sense of well-being as union density changes in the switcher states.

In light of the favorable unionization effect on financial satisfaction observed in Section 5.1, Panel A of Appendix Table 5 provides corroborating evidence that union density positively affects workers' assessments of current finances. More workers report that their financial situation is staying the same (i.e., 8 ppt) or getting better (i.e., 3 ppt) rather than getting worse as union density rises. Switching attention to potential determinants of personal health (Panel B), we find that increased unionization lessens workplace safety concerns by 0.1-0.2 SD, and reduces the incidence of hand, wrist, arm, and/or shoulder pain by 0.03 SD. To the extent that work-life balance and job predictability may affect an employee's perspective on his/her work, Panel C indicates that a strong presence of union leads to fewer work hours beyond the usual schedule by 0.18 SD, and more time for worker to relax or pursue activities that they enjoy outside of work by 0.16 SD, i.e., about the same magnitude. While there is no evidence that unionization alleviates the degree of discrimination based on gender or race at the workplace (Panel D), Panel E finds a positive effect of unionization on employee-employer relationship: workers report a higher level of trust of management (by 0.13 SD), a higher likelihood of taking part with others in making decisions that affect themselves (by 0.09 SD) and an overall happier relationship with the employer (by 0.26 SD). Finally, to the extent that labor unions may restrict individual creativity by generating "group thinking," Panel E reveals a deleterious effect of unionization on perceived fairness in promotion (by 0.15 SD), though the unionization impact on overall productivity is positive (by 0.1 SD).

6.Discussion and Conclusion

The current study investigates the well-being implications of unionization for the general public, an important question that has received little attention in the collective bargaining literature. By carefully purging the effects of unobserved factors from that of union density through a border-county design, we find strong and consistent evidence that unionization makes a positive contribution to the overall happiness of not just the unionized but rather everyone in society. Individuals toward the lower end of the income distribution, including the non-college-educated and current/former blue-collar job holders are the primary beneficiaries of union efforts. Considering that the estimated unionization effect is also concentrated on the employed, the low-education, blue-collar workers thus may be at "double jeopardy" for the adverse effect of a declining unionization.

What is noteworthy in our findings is that the subjective evaluation of financial situation, personal health, and work environment are all tied to the favorable unionization effect. Conditional on these domain satisfaction measures, we no longer observe any significant effect of unionization for low-status workers, the group who drives the well-being differential. This result points toward the value of non-pecuniary aspects of labor market experiences, besides pecuniary factors, such as on-the-job safety, work-life balance, interpersonal trust, and worker autonomy as mediators through which unionization benefits the most economically vulnerable. This finding is further supported by the evidence from a group of randomly selected workers who explicitly expressed opinions about how their quality of life evolved after the passage of RTW laws.

Finally, we find that the health channel is significant for only the low-status workers. This finding has particular significance when we consider it side by side with the evidence that a more pronounced adverse well-being effect of reduced union presence is felt by the same individuals. Typically employed in more physical occupations, these workers may be more vulnerable to injuries or developing chronic health conditions such as back or shoulder pain and thus, to poor health status. Therefore, union efforts to create safe workplaces by advocating regulations enforced by public health entities such as the Occupational Safety and Health Administration (OSHA); by investing in programs to educate workers about on-the-job hazards; and/or by working with employers to reduce worker injuries and the time lost due to injury may be particularly fruitful avenues to protect the well-being of this socio-economically vulnerable group from environmental shocks.

The present study is certainly not the final word on the broader impact of organized labor; nor do we comment on the debate around the merits or demerits of unionization with regard to objective economic measures (e.g., productivity, profitability, growth). There is a rich panoply of existing research from related fields (e.g., economics, political science, psychology) that have helped make substantial progress toward our understanding of the economic value and role of unions. However, our finding that the average happiness declines in response to the decline in unionization raises some interesting questions as to whether anti-unionization policies may be detrimental to individual well-being, which can in turn carry long-term repercussions, not just for the individual themselves but also for firm productivity and the economy overall (Oswald et al., 2015).

Rapid skill-biased technical advancement is changing the economic landscape. Abstract, non-routine jobs are replacing fixed-wage routine jobs, widening income inequality with increased job polarization. Structural changes in the economy have led to a shift from durable goods production to service occupations where cognitive skills enjoy higher returns, and artificial intelligence replaces routine manual jobs in the manufacturing sectors. Such a shift has been a boon for the firms and the economy on the aggregate but perhaps not so much a boon for the well-being of those who make up the economy – the working class. While there is evidence suggesting that policies such as RTW laws that increase competition and may lead to improved firm profitability, we ultimately need to weigh any potential benefits of increased efficiency (e.g., from maximizing worker motivation by protecting their freedom not to join a union) against any potential costs of these provisions imposed on individual well-being, particularly on health, especially in the context of the trends in suicide and substance abuse that has gripped the nation over the last decade, often attributed to the "despair" or emotional pain and suffering of our lower-education, blue-collar working class. Thus, this topics merits further in-depth scrutiny.

7. References

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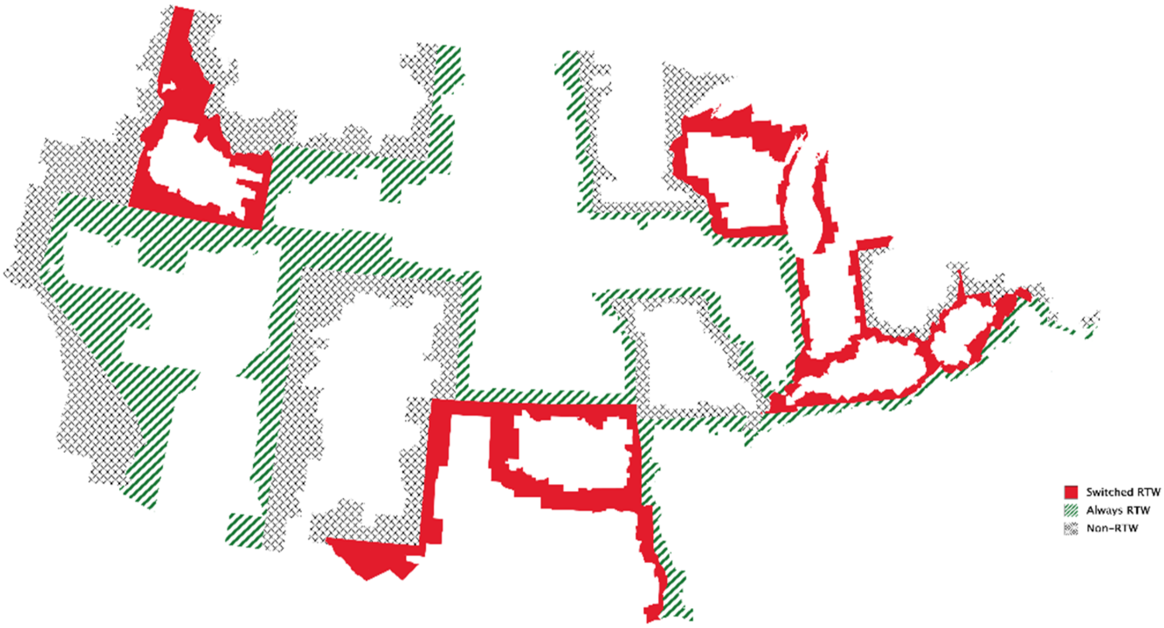
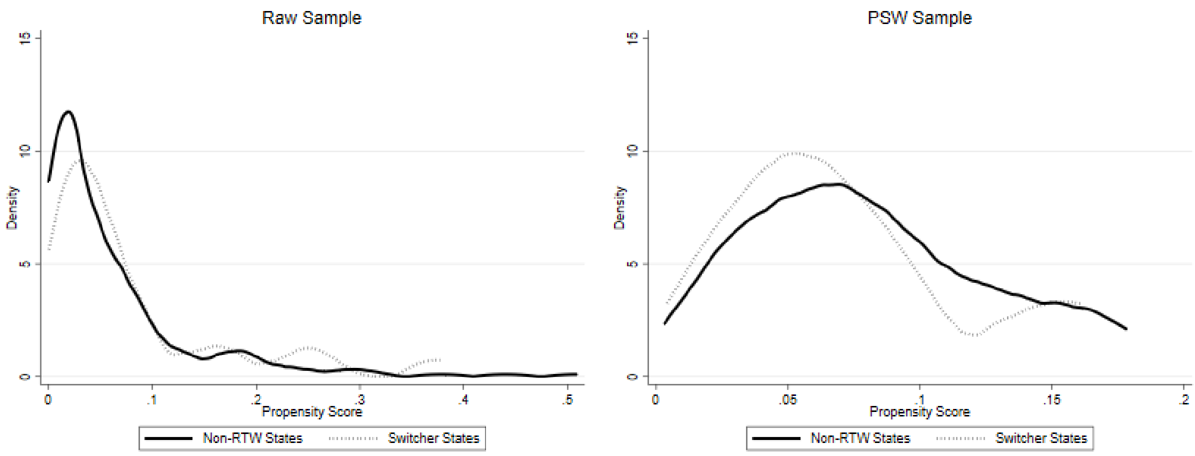
Figure 1: Geographic Disribution of Border County Pair, 2018



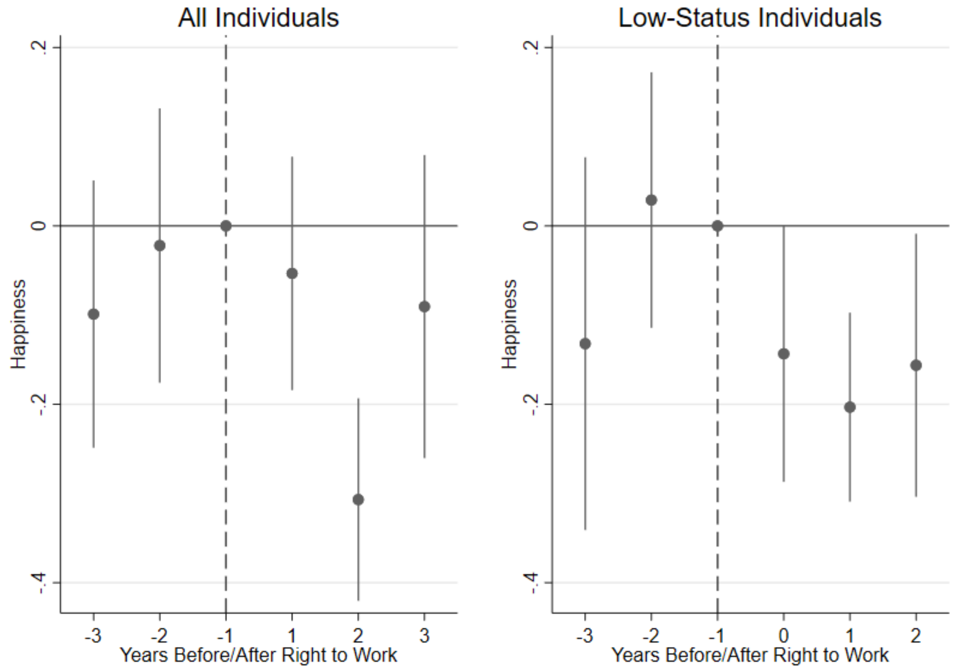
Notes: The classification of border county pairs – switched RTW, always RTW, and non-RTW – was based on the enactment date of RTW laws in each state as of December 31, 2018. Data are collected from the National Right to Work Committee ($https://nrtwc.org/facts/state-right-to-work-timeline-2016/$), retrieved on March 20, 2019.

Figure 2: Distribution of Estimated Propensity Scores



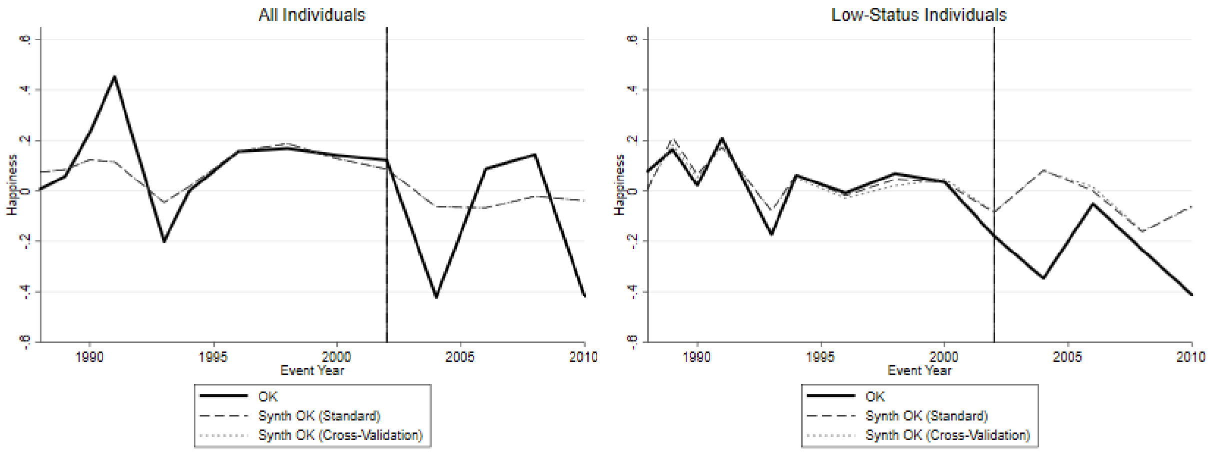
Notes: This figure illustrates the distribution of the estimated propensity scores for switcher (dashed line) and non-RTW (solid line) states for the border-county sample.

Figure 3: Event Study Analysis



Notes: This figure shows the key regression coefficients with 95% confidence intervals from Equation 4. Each coefficient is estimated relative to the year prior to RTW adoption.

Figure 4: Synthetic Control Analysis



Notes: This figure reports synthetic control estimates for Oklahoma. Corresponding donor pools are listed in Appendix Table 3.

Table 1: Key Variables for Border-County Analysis

	Raw Sample		PSW Sample	
	Non-RTW (1)	RTW (2)	Non-RTW (4)	RTW (5)
Outcome				
Happiness	2.20	2.18	2.20	2.18
Covariates				
White	0.81	0.85	0.85	0.85
Male	0.45	0.50*	0.49	0.50
Married	0.55	0.57	0.57	0.57
Presence of Child under Age 6	0.19	0.15	0.15	0.15
Years of Schooling	13.57	13.68	13.71	13.68
Less than high school	0.13	0.10	0.10	0.10
High school	0.51	0.54	0.55	0.54
Associate/Junior college	0.07	0.10**	0.09	0.10
Bachelor's	0.19	0.17	0.17	0.17
Graduate	0.10	0.09	0.09	0.09
Age under 25	0.12	0.11	0.11	0.11
Age 25 to 44	0.39	0.35	0.34	0.35
Age 45 to 65	0.33	0.38**	0.38	0.38
Age over 65	0.15	0.16	0.17	0.16
Household Size	2.75	2.68	2.67	2.68
Number of Observations	79,347	4,727	77,703	3,730

Notes: This table reports the means/frequencies of key variables used in the border-county analyses for the raw (columns 1-2) and PSW samples (columns 3-4) by exposure to the RTW legislation. Stars indicate the p-value of a t-test for group difference at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Difference-in-Differences Estimates for the RTW Effect on Happiness

	All Counties		Border-County Pairs			
	RAW Sample		Raw Sample			PSW Sample
	(1)	(2)	(3)	(4)	(5)	(6)
RTW	-0.056**	-0.115**	-0.066***	-0.081***	-0.052*	-0.078**
	(0.026)	(0.056)	(0.016)	(0.024)	(0.030)	(0.039)
Raw Sample Mean	2.195	2.195	2.189	2.189	2.189	2.204
Raw Sample SD	0.626	0.626	0.627	0.627	0.627	0.625
N	19,236	19,236	84,074	84,074	84,074	81,433
Year Fixed Effects	X	X	X	X	X	X
State Fixed Effects	X	X	X			
Census-Division Time Trends		X				
County Fixed Effects				X	X	X
Border Fixed Effects				X	X	X
Border-Specific Time Trends					X	X
SE Clustering	State	State	State by Border Pair	State by Border Pair	State by Border Pair	State by Border Pair

Notes: This table reports the DiD and PSW-DiD estimates of the RTW effect on happiness for individuals in the 1993-2018 GSS. All regressions control for individual gender, age (in five categories), years of schooling, highest degree attained (in five categories), race (a dummy for white), household size, marital status, the presence of children under the age of six, and the interaction terms between white and other covariates. Happiness is standardized for all individuals to have a mean of zero and a SD of 1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Propensity-Score Weighted Instrumental Variable Estimates for the Unionization Effect on Happiness

		Union Membership			Union Coverage		
	DD	All Sectors IV	Private Sector IV	Manufacturing IV	All Sectors IV	Private Sector IV	Manufacturing IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(N=81,433; Raw Sample Mean=2.204; Raw Sample SD=0.625)							
Union Density		0.060*	0.015*	0.024**	0.061*	0.015*	0.026**
(2^{nd} Stage)		(0.032)	(0.009)	(0.011)	(0.035)	(0.009)	(0.012)
RTW	-0.078**						
(Reduced Form)	(0.039)						
RTW		-1.312***	-5.106***	-3.220***	-1.286***	-5.337***	-2.976***
(1^{st} Stage)		(0.276)	(1.212)	(0.412)	(0.322)	(1.182)	(0.408)
F Test of Excluded		22.57	17.75	61.01	18.35	20.40	53.24
Instruments		(P=0.00)	(P=0.00)	(P=0.00)	(P=0.00)	(P=0.00)	(P=0.00)

Notes: This table reports the PSW-IV estimates for the happiness effect of unionization using the model specification in column 6 of Table 2. Standard errors are clustered two-way by state and border-pair. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Differential Effect of Unionization

	Full Sample (1)	Low Status (2)	High Status (3)
Panel A: All Individuals			
Union Density	0.024**	0.038***	-0.020
	(0.011)	(0.013)	(0.017)
Raw Y Mean	2.204	2.163	2.288
Raw Y SD	0.625	0.628	0.607
N	81,433	48,743	28,645
Panel B: Workers			
Union Density	0.004	0.044***	-0.026
	(0.013)	(0.013)	(0.017)
Raw Y Mean	2.228	2.176	2.300
Raw Y SD	0.600	0.602	0.590
N	52,128	29,713	20,952

Notes: This table reports the estimated unionization effect by individual socioeconomic status and work arrangement using the model specification in column 4 of Table 3. Since a separate PSW procedure is applied to each sub-sample, the number of observations between low and high-status individuals do not sum to that of the full sample. First-stage F statistics range from 40 to 61. Standard errors are clustered two-way by state and border-pair. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Unionization Effects on Domain Satisfaction

	Finance (1)	Health (2)	Job (3)
Panel A: Full Sample (N=48,743)			
Union Density	0.055***	0.011	–
	(0.017)	(0.009)	–
Raw Y Mean	1.956	0.181	–
Raw Y SD	0.737	0.385	–
Panel B: Workers (N=29,713)			
Union Density	0.057***	0.018*	0.050**
	(0.015)	(0.010)	(0.020)
Raw Y Mean	1.943	0.209	3.237
Raw Y SD	0.724	0.407	0.811

Notes: This table reports the unionization effect on domain satisfaction using the model specification in column 4 of Table 3 for low-status individuals. First-stage F statistics range from 43 to 47. Standard errors are clustered two-way by state and border-pair. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Domain Satisfaction and Unionization-Happiness Relation

	Full Sample (N=48,743)		Workers (N=29,713)			
	(1)	(2)	(3)	(4)	(5)	(6)
Density	0.038***	0.023**	0.044***	0.029**	0.026**	0.019
	(0.013)	(0.011)	(0.013)	(0.012)	(0.013)	(0.014)
Finance		0.281***		0.274***	0.259***	0.219***
		(0.003)		(0.006)	(0.006)	(0.006)
Health					0.355***	0.331***
					(0.013)	(0.013)
Job						0.179***
						(0.007)

Notes: This table reports the unionization effect on happiness using the model specification in column 4 of Table 3 for low-status individuals. First-stage F statistics range from 43 to 47. Standard errors are clustered two-way by state and border-pair. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$