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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 their role in individuals' perceptions of 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 those of low socioeconomic status, including non-college-educated residents and current or former blue-collar job holders, but has no discernible impact on their high-status counterparts. Of affected residents, workers stand to reap the most benefit. We also find that the favorable effect of unionization is transmitted through the assessment of improved financial situation, personal health, and workplace quality. This finding highlights the role of pecuniary and nonpecuniary benefits (for example, on-the-job safety, work-life balance, interpersonal trust, and workers' autonomy) that unions afford to protect society's most marginalized groups.

1. Introduction

Until the COVID-19 pandemic hit the world in early 2020, the United States had been experiencing a stretch of robust economic growth and prosperity since the great recession. Unemployment was at its lowest since 1970, as the economy added jobs for nearly 8 years in a row between 2010 and 2018 (US Bureau of Labor Statistics 2020). For working people, therefore, the postrecession 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, an increasing number of newer low-wage jobs, rising automation in certain manufacturing in-

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dustries, 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, p. 397), 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 individuals' well-being by advocating for favorable contracts and working conditions for workers (see, for example, Freeman and Kleiner 1999); establishing generalized norms of labor practices by shaping local, state, and national politics; lobbying on labor-friendly policies (see, for example, Feigenbaum, Hertel-Fernandez, and Williamson 2018); and fostering a sense of solidarity among coworkers that helps insulate against work-related stress (see, for example, Flavin, Pacek, and Radcliff 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 individuals' perceptions of, or psychological reactions to, actual circumstances (see Section OA1 of the Online Appendix for a more detailed discussion). The vast majority of the existing literature focuses on the psychological well-being of the unionized (see Blanchflower and Bryson [2020] for a review). The few studies that extend attention to nonmembers and/or nonworkers arrive at mixed conclusions (for example, Redcliff 2005; Flavin, Pacek, and Radcliff 2010; Keane, Pacek, and Radcliff 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 effect of unionization on likely determinants of happiness, such as wages and income inequality (see, for example, Western and Rosenfeld 2011), health care provision (Olson 2019), occupational safety and workplace hazard protections (Zoorob 2018), and working-class representation in state legislatures and Congress (Feigenbaum, Hertel-Fernandez, and Williamson 2018). On the other hand, the economic costs associated with union membership can lead to the counterprediction 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 individuals' 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

[1] In an interview with the *Guardian* (Bible 2017), economist Anne Case explained the significance of labor market conditions on individuals' lives: "The quality of the labor market may affect whether a person marries, and the stability of their personal lives, and whether they risk their health at work."

[2] Radcliff (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, Pacek, and Radcliff (2010) and Keane, Pacek, and Radcliff (2012) report a similarly beneficial effect of unionization on aggregate levels of life satisfaction when considering union members and nonmembers. Makridis (2019), on the other hand, finds that the passage of right-to-work (RTW) laws in the United States, which generally weakens unions, positively affect individuals' current life satisfaction and economic sentiment, although these effects are restricted to union members and are estimated with larger standard errors when variations in state borders are exploited for identification.

contracts can increase workers' distress (see, for example, Brochu and Morin 2012). The decline in the value of merit may also create frustration among new and/or high-performing employees by discounting education and experience and discouraging individuals' creativity (Freeman 1978; Borjas 1979). These considerations leave open the questions of whether and how unionization may affect subjective well-being on net.

This study attempts to address both questions using the longest-running public opinion survey in the United States, the 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 because they reduce union density. This creates a unique opportunity to bring a better understanding of causality to the extant literature about unionization and subjective well-being. Given that RTW-adopting states may differ from non-RTW-adopting states in unobservable ways that account for the differences in well-being (for example, labor market opportunities), we examine the individuals living in contiguous counties that straddle a state border—weighted by their probabilities of RTW exposure (see, for example, Hirano, Imbens, and Ridder 2003)—and then compare changes in their responses in a difference-in-differences (DiD) framework. Meanwhile, RTW statutes vary across states and in scope (for example, with respect to the coverage of workers), which potentially causes the treatment intensity or the extent of the decline in unionization rates to vary by state. The self-reported union status and/or union density estimates could also be subject to well-known measurement errors, whose effects on union wage differentials have been extensively explored in the literature (for example, Card 1996). To correct for these potential biases, we further adopt an instrumental-variable (IV) approach, exploiting the varied implementation of RTW laws as an instrument for changes in union density, given that the adoption of RTW laws reduces unionization and is plausibly unrelated to happiness except through its effect on union density.

We find that a 1-percentage-point (ppt) increase in union density boosts the overall level of happiness among low-socioeconomic-status residents by .04 SD while having a negligible impact on their higher-status counterparts.[3] This translates into an overall well-being cost of .14 SD imposed by RTW legislation as a whole against low-status individuals. In addition, the unionization effect is primarily driven by workers, consistent with recent discoveries for the United States and many European countries that union memberships increase job satisfaction and employees' well-being (Davis 2013; Donegani and McKay 2012; Blanch-

[3] For classifying social status, instead of using the more conventional income- and wealth-based approach, we adopt an education- and occupation-based classification, as those areas are more stable across one's lifetime than income and wealth and therefore are less affected by an RTW change in legislation.

flower and Bryson 2020).[4] This may appear to run counter to the proposition that union members express greater job dissatisfaction than nonmembers in the seminal works Freeman (1978) and Borjas (1979), but it is plausible that rising uncertainty after the recession enhanced the appreciation for a secure work environment facilitated by labor unions, especially among individuals born between the 1960s and the 1990s or, in other words, cohorts younger than Freeman's and Borjas's samples in the 1970s (Blanchflower and Bryson 2020).[5]

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 adoptions of RTW laws prior to the 2010s. The expanded time horizon allows us to account for potentially delayed responses to RTW laws among individuals who might be only indirectly affected (namely, nonunion workers and/or nonworkers) and speak to a longer-term well-being effect of RTW laws. It takes a certain period of time for collective agreements to be renegotiated[6] and for unions to contribute to an improvement in living conditions for the society at large. In this sense, studies that primarily rely on data after 2010 are able to measure only short-term outcomes, and what remains unknown is whether there might be longer-run repercussions of decreased union density, given that the majority of RTW adoptions in recent decades occurred after 2012.[7] Furthermore, because the underlying mechanisms for short- and long-term effects may be entirely different, the additional subgroup analysis undertaken in the current study, relative to prior research (for example, Makridis 2019), may uncover variability in the direction and magnitude of the average treatment effects for individuals in a population and unmask aspects of RTW laws that predominantly affect society's most marginalized groups.

Second, besides the intent-to-treat (ITT) analysis conventionally performed in existing RTW studies using border-county designs (Holmes 1998; Feigenbaum, Hertel-Fernandez, and Williamson 2018; Makridis 2019), we provide the first local average treatment effect (LATE) estimate of unionization on well-being, ac-

[4] Using the same data from the General Social Survey (GSS) and the Gallup Daily Tracker Pool for the United States, for example, Blanchflower and Bryson (2020) find that union workers report higher levels of life satisfaction, happiness, and job satisfaction than nonunion workers and that they were also less likely to be stressed, worried, depressed, sad, or lonely after the great recession.

[5] Besides the exit-versus-voice hypothesis posited in Freeman and Medoff (1984), in which union workers might be more likely than nonunion workers to complain about working conditions to rectify dissatisfying circumstances, many studies that find a negative association of union membership and job satisfaction also attribute the observed differences, at least in part, to differences in the objective characteristics of the job (for example, the nature of the tasks and working conditions), workers' preferences, and/or the propensity to unionize among the already dissatisfied (Berger, Olson, and Boudreau 1983; Hersch and Stone 1990; Bender and Sloane 1998; Bryson, Cappellari, and Lucifora 2004; Hammer and Avgar 2005).

[6] While the National Labor Relations Act does not specify a length of time for a labor contract, in practice all collective agreements have a specified length. The normal term of a contract is 3 years, although in recent years many contracts have moved to longer terms, 4 or 5 years, for example (Compa 2014).

[7] In Makridis (2019), the average time elapsed between the enactment of RTW laws and the measurement of well-being is 3 years, whereas the average length of the postintervention period in the present study is 6 years.

knowledging that neither the legal language nor compliance with RTW laws may be uniform across states and noting 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, Diamond, and Hainmueller 2010). Owing to data limitations, this exercise is performed for Oklahoma, an RTW-adopting state that offers the most post-RTW information and largest donor pool in the sample. While not necessarily generalizable to other RTW-adopting states at present, this method allows for time-varying unobserved heterogeneity (for example, antiunion sentiment) and, to some extent, spillover effects of RTW laws across state borders (for example, through competition) and thus provides supporting evidence that, with the exception of the adoption of RTW laws, border-adjacent counties are politically, culturally, and economically similar.

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

2. Data

2.1. Measures of Subjective Well-Being and Unionization

To study the relationship between unionization and happiness, we obtained data from the 1988–2018 GSS and focus on 1993–2018 for the border-county analysis on the county of an individual's residence.[8] The GSS is a nationally representative survey that tracks social characteristics and attitudes of American adults over time. It was conducted every year from 1972 to 1994 (except in 1992) and biennially beginning in 1994, surveying approximately 3,000 individuals in each wave. 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 (for example, financial and employment satisfaction) and job experiences (for example, perceptions of interactions between employees and employers) in selected years. The presence of these variables enables us to infer the implications of unionization on well-being in a more comprehensive fashion than is possible with other more traditional labor data sources such as the Current Population Survey (CPS).

While the phrasing of the GSS questions has been modified to some extent over the years, we are able to identify three essential indicators of well-being for all individuals: general happiness ("Taken all together, how would you say things are these days—would you say that you are very happy, pretty happy, or not too

[8] The GSS unfortunately does not provide information about the location of an individual's job(s). This omission could result in an over- or underestimate of the true unionization effect for individuals who live and work in different states with different RTW statuses. Additional investigations in this regard could be important extensions of the current study, especially in light of the surge in remote work during and potentially after the COVID-19 pandemic.

happy?"), financial satisfaction ("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 job satisfaction ("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 equal to 1 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 0 and an SD of 1, so that all resulting regression coefficients can be interpreted in terms of changes in standard deviations.

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 US Bureau of Labor Statistics uses for publishing estimates at the national level (Hirsch, MacPherson, and Vroman 2001).[9] To best capture state-level variations in union strength, we consider six 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 union members and workers who report no union affiliation but whose jobs are covered by a union or an employee association contract. Since nonunion 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. Right-to-Work Legislation

Right-to-work laws are a response to the National Labor Relations Act 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 act granted states the power to permit workers in a unionized workplace to opt out of paying membership fees, even if those workers enjoyed benefits from collective bargaining and union representation. Before the passage of the 1947 Taft-Hartley Act, five states had already passed such laws (Arkansas and Florida in 1944 and Arizona, Nebraska, and South Dakota in 1946). Since then, 21 additional states have passed

[9] With w_{ij} representing the annualized Current Population Survey sample weight for individual i in group j (where group can be state, metropolitan area, industry, or occupation), employment for group j is Employment$_j = \sum w_{ij}$. Let M_{ij} equal one if individual i is a union member in group j and zero otherwise and C_{ij} equal one if individual i in group j is covered. Union membership and coverage density estimates then measure the percentage of employees who are members or covered, respectively, defined as %Mem$_j = (w_{ij}M_{ij}/w_{ij}) \times 100$, and %Cov$_j = (w_{ij}C_{ij}/w_{ij}) \times 100$. See Hirsch and MacPherson (2003) for a detailed discussion of the methodology.

RTW laws, with the greatest concentration of them in the West and Southeast. The states and the enactment dates of their laws as of December 31, 2018, are presented in Table OA1.[10,11]

3. Methodology

3.1. Identification Strategy

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

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

where Y_{isdt} denotes happiness for individual i who lives in state s and census division d in survey year t. State fixed effects (π_{sd}) capture state-level determinants of happiness that are stable over time. The census-division–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 $\boldsymbol{X}$ includes individual-level attributes that are likely correlated with the enactment of RTW laws and that affect well-being: gender, age (five categories), years of schooling, highest degree obtained (four categories), race (a dummy for white), household size, marital status, the presence of children under the age of 6, and the interaction terms between white and other covariates.[12]

Since switcher states might be systematically different from non-RTW states, we replicate the analysis for individuals in neighboring counties across an RTW border. Because a county may be located on the border of multiple states, we follow the existing literature (Dube, Lester, and Reich 2010; Feigenbaum, Hertel-Fernandez, and Williamson 2018) and allow counties bordering other counties from multiple states to pair with each other and stack the data accordingly.[13] Owing to the stacked nature of the data, we use a slightly different DiD model:

$$Y_{icst} = \beta_1 \mathrm{RTW}_{st} + \boldsymbol{X}'_{icst}\theta + \rho_{cs} + \tau_{bt} + \varepsilon_{icst}, \tag{2}$$

where ρ_{cs} is county fixed effects and τ_{bt} is border-pair–year fixed effects account-

[10] Data are collected from National Right to Work Committee, Right to Work States Timeline (https://nrtwc.org/facts/state-right-to-work-timeline-2016/).

[11] The RTW law in Texas was originally passed in 1947 and then modified to its current form in 1993. Given that there is a controversy regarding whether the 1947 legislation provided any specific means of enforcement (see, for example, Meyers 1955), we exclude Texas from the analysis to obtain the cleanest estimates. Considering Texas as an always-RTW state does not fundamentally affect our conclusions.

[12] Socioeconomic characteristics likely affected by RTW laws, such as weekly work hours and household income, are excluded to avoid potential mechanical endogeneity. From this perspective, the analyses in Section 4.3 will underestimate the true happiness effect of unionization if adoption of RTW laws promotes job growth (see, for example, Holmes 1998) and if employment is associated with a higher level of subjective well-being (see, for example, Frey and Stutzer 2002).

[13] For example, individuals living in a county straddling three state borders will appear in the data set three times each year.

ing for 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, specification (2) ignores potential state differences in RTW statutes and the degree of compliance. A close comparison of the legal language used after 1980 reveals that individual statutes vary in their coverage of workers, penalties, and remedies (for example, invoking civil versus criminal laws or differences in the magnitude of fines or penalties), though the legal effect of the laws is not significantly different across states (Feigenbaum, Hertel-Fernandez, and Williamson 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, individuals' decisions about whether to pay union dues or join a union can vary depending on, for example, the prevalence of antiunion sentiment, which results in a divergence between the LATE effect and the ITT effect of RTW laws.

In addition, the well-documented measurement errors in the estimates of union density can bias the estimated unionization effect upward or downward (see, for example, Card 1996; Olson 2019). These measurement errors can be caused by variability in the sampling of households in the CPS or erroneous and/or biased responses from individuals. Such measurement errors have long been suspected to account for, at least partially, the wide range of values for the union wage gap 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:

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

where $\widehat{\text{Density}}_{st}$ 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-reweighting (PSW) technique to balance the distribution of observable characteristics between switcher and non-RTW states. While dimensionality is not a concern in our context, this procedure eliminates the assumption of linearity and therefore allows the results to be more robust to misspecification than parametric DiD models (Hirano, Imbens, and Ridder 2003). We estimate the conditional probability that a switcher state passes RTW legislation ($\widehat{p(X_l)}$) using the covariates in the parametric model to identify the states that are, on average, the most similar over the duration of the observation period and then use those estimates to weight outcome values (Y_i):

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

where $W \in (0, 1)$ denotes treatment status and n is the proportion of treated units. In this way, observations with large values for $\widehat{p(X_l)}$ will be weighted down when treated and weighted up when untreated (and vice versa for small values). Because the inverse of these propensity scores may overinflate the influence of observations at the ends of the distribution, we further calculate the fifth and 95th percentiles of the propensity score and remove the observations that fall outside these limits (Imbens 2015).[14]

The reliability of these estimates rests on the fact that the counterfactual happiness of border-county individuals in switcher states exhibits patterns similar to those in non-RTW states. To assess the validity of this identifying assumption, we perform a series of robustness checks. First, we conduct an event-study analysis to evaluate the possibility that the enactment of RTW laws is correlated with preexisting differences in happiness in switcher and non-RTW states. Failure to find differential trends in happiness immediately prior to the implementation of an RTW law would suggest that the common-trends assumption is likely satisfied. Second, we employ a synthetic control method (SCM) to create a weighted control group by matching moments of key variables in the pre-RTW period in Oklahoma and in the non-RTW states. While acknowledging that the 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 switcher and non-RTW states is unlikely a result of unobserved differences across states. Finally, we address issues related to preintervention unionization.

3.2. 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 the border-county sample, consisting of approximately 84,000 individuals residing in 290 border counties, which is over half of the 452 counties observed in the GSS (see Figure 1).[15] In the raw sample, individuals exposed to RTW legislation report a lower level of happiness. They are significantly more likely to be male, be older, and have a higher level of education, in particular associate's degrees, though once trimmed and reweighted by the estimated propensity scores, these differences disappear.

Figure 2 plots corresponding probabilities of adopting an RTW law for switcher and non-RTW states for the border-county sample after adjusting for covariates. While switcher states tend to have a higher density of high propensity scores than non-RTW states, the PSW procedure brings them much closer, as is formally confirmed by the insignificant Hosmer-Lemeshow statistic. We obtain similar results for the low-status and high-status samples.[16] Thus, the PSW tech-

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

[15] Throughout the analysis, we employ cross-sectional survey weights to reflect representativeness and adjust for nonresponse.

[16] For the sake of brevity, these results are not included but are available from the authors on request.

Table 1

Key Variables for Border-County Analysis

	Raw Sample		Propensity-Score-Weighted Sample	
	Non-RTW	RTW	Non-RTW	RTW
Happiness	2.20	2.18	2.20	2.18
Covariates:				
White	.81	.85	.85	.85
Male	.45+	.50	.49	.50
Married	.55	.57	.57	.57
Child under 6	.19	.15	.15	.15
Schooling:	13.57	13.68	13.71	13.68
Less than high school	.13	.10	.10	.10
High school	.51	.54	.55	.54
Associate's degree or junior college	.07*	.10	.09	.10
Bachelor's degree	.19	.17	.17	.17
Graduate school	.10	.09	.09	.09
Under age 25	.12	.11	.11	.11
Ages 25–44	.39	.35	.34	.35
Ages 45–65	.33*	.38	.38	.38
Over age 65	.15	.16	.17	.16
Household size	2.75	2.68	2.67	2.68
N	79,421	4,744	77,703	3,730

Note. Values are means or frequencies. RTW = right to work.
* $p < .05$ (t-test of state-level group difference).

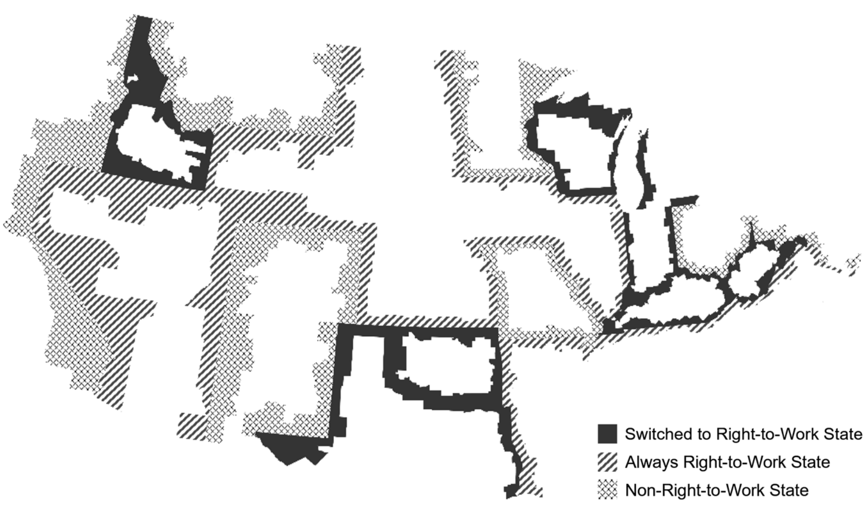

Figure 1. Geographic distribution of border counties, 2018

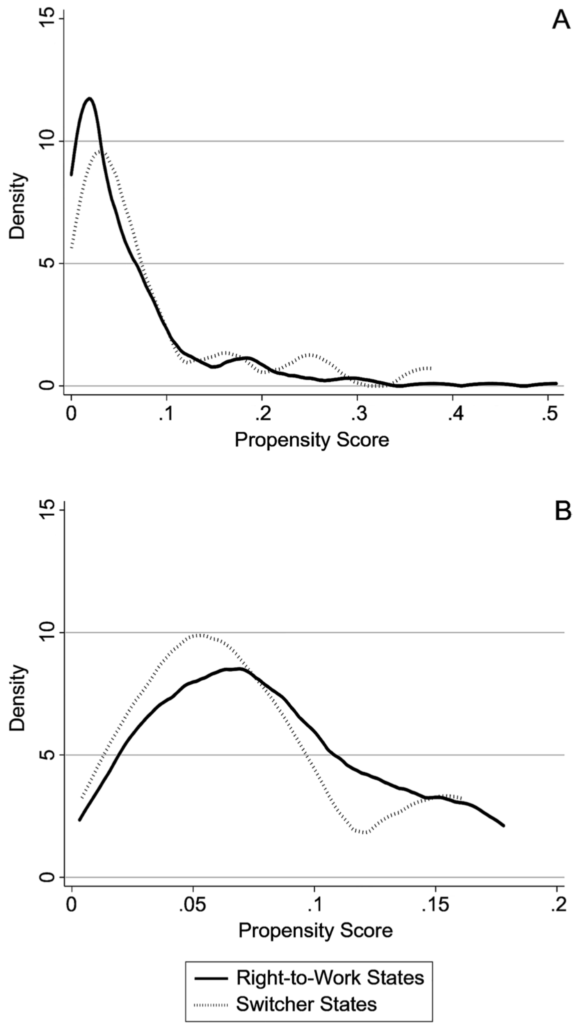

Figure 2. Probability of adopting a right-to-work law. *A,* Raw sample; *B,* propensity-score-weighted sample.

nique seems 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 effect of RTW laws on happiness for all individuals and those in border counties are presented in Table 2, where standard

Table 2
Effect of Right-to-Work Laws on Happiness

			Border-County Pairs			
	All Counties		Raw Sample			Propensity-Score-Weighted Sample
	(1)	(2)	(3)	(4)	(5)	(6)
RTW	−.056*	−.115*	−.066**	−.081**	−.052	−.078*
	(.026)	(.056)	(.016)	(.024)	(.030)	(.039)
Mean	2.195	2.195	2.186	2.186	2.186	2.204
SD	.626	.626	.628	.628	.628	.625
N	19,236	19,236	84,165	84,165	84,165	81,433
State fixed effects	Yes	Yes	Yes	No	No	No
Census division time trends	No	Yes	No	No	No	No
County fixed effects	No	No	No	Yes	Yes	Yes
Border fixed effects	No	No	No	Yes	Yes	Yes
Border-county-pair time trends	No	No	No	No	Yes	Yes
Clustering	State	State	State-border pair	State-border pair	State-border pair	State-border pair

Note. Data are from difference-in-differences (DiD) and propensity-score-weighted-DiD estimates for individuals in the 1993–2018 General Social Survey. All regressions control for gender, age (five categories), years of schooling, highest degree attained (five categories), race (a dummy for white), household size, marital status, the presence of children under 6, and the interaction terms between white and other covariates. Happiness is standardized to have a mean of 0 and an SD of 1. All regressions include year fixed effects.

* $p < .05$.
** $p < .01$.

errors are clustered by state and border pair in the preferred model specification (column 6) to account for potential serial correlations among the observations of the same border pair over time. Across samples and specifications, the RTW coefficient fluctuates in magnitude and statistical significance, which suggests that pre-RTW differences in happiness between switcher and non-RTW states—while bearing no substantive implications for our results—exist. Conditional on county, year, and border-pair fixed effects, the estimated RTW effect becomes weaker when unrestricted border-specific time trends are considered, which implies 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. This is consistent with our expectation of seeing 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 with those for any arbitrary counties in the United States. Most important, even in the most flexible model specification in which confounding biases are removed through the PSW procedure, an adverse effect of RTW laws on well-being is still apparent (at .08 SD).

4.2. Propensity-Score-Weighted Instrumental-Variable Approach

The PSW IV estimates of the effect of unionization on happiness using our preferred specification (column 6 in Table 2; see also the DiD specification in Table 3), for six definitions of union density, are presented in Table 3. Focusing on results from the first stage, we see that 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. With union membership rate as an example, the passage of RTW laws is associated with reductions in the membership rate of all sectors, private sectors, and the manufacturing sector. Since the association between RTW laws and unionization appears to be strongest for the manufacturing sector ($F = 61$) of the six measures, we treat it as our preferred specification.

In terms of the second-stage results, 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 association between happiness and RTW laws. The coefficient for union density in the preferred specification indicates that a 1 ppt increase in the union workforce leads to a .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 RTW laws on happiness of .077 SD, nearly identical to the ITT estimate in column 1.

Table 3

Effect of Unionization on Happiness: Propensity-Score-Weighted Estimates

	Union Membership				Union Coverage		
	Difference in Differences (1)	All Sectors (2)	Private Sector (3)	Manufacturing (4)	All Sectors (5)	Private Sector (6)	Manufacturing (7)
Density (second stage)		.060+	.015+	.024*	.061+	.015+	.026*
		(.032)	(.009)	(.011)	(.035)	(.009)	(.012)
RTW (reduced form)	−.078*						
	(.039)						
RTW (first stage)		−1.312**	−5.106**	−3.220**	−1.286**	−5.337**	−2.976**
		(.276)	(1.212)	(.412)	(.322)	(1.182)	(.408)
Test of excluded instruments		22.57	17.75	61.01	18.35	20.40	53.24
p-Value		.00	.00	.00	.00	.00	.00

Note. Estimates in columns 2–7 are from instrumental-variable specifications. Standard errors, in parentheses, are two-way clustered by state and border pair. For the raw sample, mean = 2.204, SD = .625. $N = 81{,}433$.

+ $p < .1$.

* $p < .05$.

** $p < .01$.

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 with high-status individuals, county residents of low socioeconomic status, especially labor market participants, are more likely to be the main beneficiaries of union efforts. First, because they hamstring 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 workplaces (wages, benefits, and so on). If the marginal utility of time and money is greater for individuals at the lower end of the income distribution, there should be 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 owing 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 primarily affecting blue-collar and less-educated individuals. In the context of the wide-ranging protections against labor market volatility, unions can be particularly critical for unskilled laborers, 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, and the like.

Defining low-status residents to be individuals who never attended college and/or used to or were working in blue-collar occupations at the time of the survey[17] and separately estimating our preferred PSW IV model for different status groups confirms this hypothesis. Unionization increases the average level of happiness for low-status individuals by .04 SD—double the unionization effect for the full sample—but has no discernible impact on their high-status counterparts (see Table 4).[18] Moreover, the observed effect is concentrated on workers, or in

[17] The occupational classification variable in the GSS is based on 2010 US Bureau of the Census three- or four-digit occupation classifications and responses to the following questions: "What kind of work do you (did you normally) do? That is, what (is/was) your job called?" "What (do/did) you actually do in that job? Tell me, what (are/were) some of your main duties?" and "What kind of place (do/did) you work for?" Following the existing literature (for example, Brochu and Morin 2012), we make a distinction between professional and service occupations and blue-collar occupations. Professional occupations include those in management; business and financial operations; computers and mathematics; architecture and engineering; life, physical, and social sciences; community and social services; law; education and training; library services; arts; design; entertainment; sports; media; and health care. Service occupations include those in health care support, protective services, food preparation and service, building and grounds cleaning and maintenance, personal care, sales, office and administrative support, and the military. Blue-collar occupations include those in farming, fishing, forestry, construction and extraction, maintenance and repair, production, transportation, and material moving.

[18] We also estimate the unionization effect separately for non-college-educated and current or former blue-collar job holders and obtain qualitatively similar results across the two groups.

Table 4
Differential Effects of Unionization

	Full Sample	Low-Status Individuals	High-Status Individuals
All individuals:			
Density	.024*	.038**	−.020
	(.011)	(.013)	(.017)
Raw sample mean	2.204	2.163	2.288
Raw sample SD	.625	.628	.607
N	81,433	48,743	28,645
Workers:			
Density	.004	.044**	−.026
	(.013)	(.013)	(.017)
Raw sample mean	2.228	2.176	2.300
Raw sample SD	.600	.602	.590
N	52,128	29,713	20,952

Note. Estimates are from a propensity-score instrumental-variable specification for the manufacturing sector. Since a separate propensity-score-weighting procedure is applied to each subsample, the observations for low-status and high-status individuals do not sum to that of the full sample. First-stage *F*-statistics range from 40 to 61. Standard errors, in parentheses, are two-way clustered by state and border pair.
* $p < .05$.
** $p < .01$.

other words labor market participants. This is plausible if unionization results in improved working conditions and the increased job satisfaction, in turn, contributes to greater overall well-being. This points to workplace quality as a potential mechanism through which unionization may affect happiness.

In unreported results, we investigated whether the unionization effect is conditioned on union membership. Rerunning the original model separately for union and nonunion members reveals a favorable effect of unionization for both groups, though the magnitude for union members is two to five times greater than that for nonunion members, depending on whether the spouse's union status 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 for nonunion workers are presumably weakened by the probability of spillover. However, we need to be cautious in interpreting these results. Unlike the CPS, the GSS provides information about individuals' 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 the estimates. On the one hand, if free riders report a lower level of happiness due to lower incomes (which indicates a higher marginal utility of money) than fee-paying members before the passage of RTW laws, then the omission of this group from the post-RTW period will result in an understatement of the true effect those laws for union members. On the other hand, since free riders receive the benefits but do not pay fees, the boost in income could increase their well-being, which would imply an overstatement of the true effect of RTW laws on union members. As neither the direction nor the magnitude of this bias is obvious in this context, we do not place much emphasis on this set of results, although they are available on request.

4.4. Robustness Checks

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 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 forces non-RTW states to respond to the possible exit of firms to RTW states by adopting RTW laws. Then there could be a similar happiness gap after the adoption of RTW laws. To investigate this possibility, we first conduct an event-study analysis by focusing on the three switcher states—Oklahoma, Indiana, and Michigan—that provide at least 3 survey years (6 calendar years) of post-RTW 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 occurs and therefore relaxes the standard assumption of DiD models that treatment is associated with a one-time shift in the outcome. The observed lead effects also provide an important falsification test for any differential, preexisting trends in the switcher states that may confound the estimates. We estimate a variant of the DiD equation:

$$Y_{icst} = \sum_{k=9}^{-11} \delta_k \mathrm{RTW}_{st}^{k} + \boldsymbol{X}_{icst}' \theta + \rho_{cs} + \tau_{bt} + \varepsilon_{icst}, \tag{5}$$

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

Owing to the smaller sample sizes and limited identifying variations, we do not employ the PSW technique in this exercise, and as demonstrated in Figure 3 the estimated 95 percent confidence intervals of the key regression coefficients from equation (5) tend to be larger for both all individuals and low-status individuals than those in the main analysis. Despite these differences, a familiar pattern emerges. There is a clear downward shift in the average level of happiness for low-status individuals, the primary beneficiaries of union efforts, which begins in

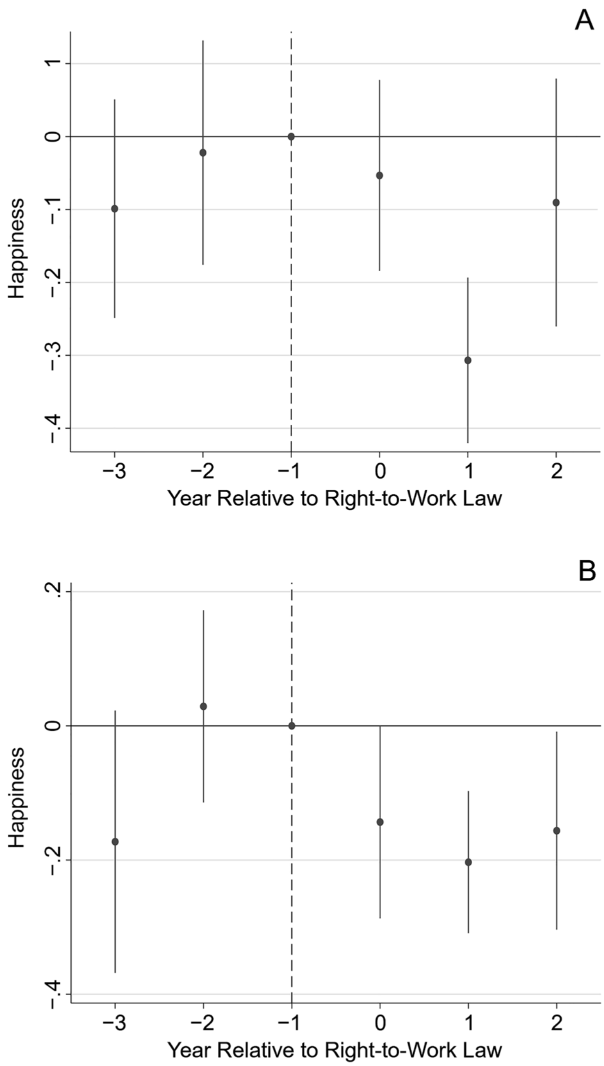

Figure 3. Event-study analysis of average levels of happiness. *A,* All individuals; *B,* low-status individuals.

the adoption year of RTW laws and persists over the postintervention window. Importantly, the estimated values for δ_k in the 3 preadoption waves are statistically insignificant, exhibiting no particular upward or downward pretreatment trends. Thus, the event-study analysis suggests that even if the decisions to adopt RTW laws were driven by factors such as increased competition caused by some switcher states, they had no material impact on well-being during the observation window, at least as far as Oklahoma, Indiana, and Michigan are concerned.

4.4.2. Synthetic Control Analysis

As a more systematic assessment of hidden factors that vary over time, we implement an SCM (Abadie and Gardeazabal 2003; Abadie, Diamond, and Hainmueller 2010) to estimate the ITT effect of 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 preintervention pattern of happiness in the switcher state. Since it allows the effects of unobserved characteristics to vary over time and for potential interstate spillover effects of the RTW laws to exist, results obtained that are consistent with those under a standard DiD specification serve as an indication that the observed RTW effect is not an artifact.

To smooth stochastic variation and produce results that are as generalizable as possible, ideally we would adopt a method that aggregates multiple events into a single treatment effect (see, for example, Ben-Michael, Feller, and Rothstein 2021). However, close to half of the non-RTW states are observed inconsistently (namely, 10 of 24 states)—since the primary sampling unit of the GSS is a region rather than a state—which prevents a satisfactory preintervention fit, an important practical requirement for an SCM (Abadie 2021). Furthermore, the vast majority of switcher states in the sample did not adopt RTW laws until after 2012, which provides relatively little postintervention data. For these reasons, we focus on the earliest adopter of an RTW law, Oklahoma, so that later adopters (Indiana and Wisconsin) can be utilized to construct the synthetic controls. This approach effectively boosts the size of the donor pool by nearly 30 percent while permitting a reasonably long postintervention window (from 1988 to 2010) to examine whether the RTW effect precipitates after a considerable delay.[19]

Following the notation used in Abadie, Diamond, and Hainmueller (2010) and indexing units $j = 1, \ldots, J + 1$ such that the first unit is Oklahoma and the others are donor states, we compute the SCM estimate of the RTW effect by subtracting a linear combination of happiness in the donors (Y_{jt}) from actual happiness (Y_{1t}) in Oklahoma for the post-RTW period:

$$Y_{1t} - \sum_{j=2}^{J+1} w_j^{*} \times Y_{jt},$$

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 to minimize the preintervention root mean square prediction error (RMSPE) with a regression-based method. Since the standard approach maximizes in-sample fit, which likely results in a prediction that would perform poorly out of sample, a data-driven alternative or cross-validation technique that exploits the first half of the preintervention trend to form the synthetic match and reserves the second half for out-of-sample validation and weight selection has also been proposed (see, for

[19] We restrict the start date to 1988, as some questions were not asked consistently because of the rotational design of the GSS in earlier years.

example, Cavallo et al. 2012). We utilize results from both methods to check sensitivity.

For inference, we implement the classic permutation test by examining whether the estimated 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 in the state of interest. Thus we compare the postintervention and preintervention RMSPE (post-RMSPE/pre-RMSPE) of actual happiness less the synthetic control predictions for Oklahoma with ratios for the distribution of the donors. The ranking of Oklahoma relative to the donors for those ratios determines the significance level for the estimated effect of RTW laws.

Figure 4*A* shows the SCM estimates for all individuals in Oklahoma using the standard and cross-validation methods, where the pretreatment period is divided roughly in the middle into a 1988–93 training period and a 1994–2000 validation period.[20] (Corresponding donor pools are listed in Table OA3.) The vertical line indicates the enactment year of RTW legislation. The outcomes from both approaches before RTW passage are remarkably similar, 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 8 years immediately before the passage of RTW laws.[21] Owing to the volatility of the data, the overall measures of fit for the full sample are relatively poor compared with those for low-status individuals, with 72–89 percent of placebos having a preintervention RMSPE at least as large as Oklahoma's. Perhaps because of this, despite a lower average actual happiness in the post-RTW period (by .08 SD), the standardized placebo-based p-value in Table OA2 ranges from .11 to .55, which indicates that more than 10 percent of the placebo effects from donors have a postintervention RMSPE at least as great as Oklahoma's, after taking into consideration the preintervention match quality.

Repeating the exercise for individuals of low status in Figure 4*B* produces an improved preintervention fit. Oklahoma has a better preintervention match than 93–94 percent of placebos, which allows the synthetic units to mimic the rises and falls in actual happiness over the entire pre-RTW period. In the posttreatment period, a clear decline in actual happiness relative to its synthetic counterparts emerges (placebo-based $p = .00$), averaging .20 SD. This is 8 ppt larger than the RTW effect obtained from the border-county analysis using the DiD model

[20] Since the split between training and validation is arbitrary, Figure OA3 displays actual happiness and five versions of synthetics that result from different partitions of the pretreatment period from 1993 through 2000. As shown, in spite of small variations in the ability of the synthetic control to replicate the counterfactual trajectory in the original model, there is no systematic upward bias associated with the choice of threshold.

[21] A set of predictors perceived to contribute to the average level of happiness in Oklahoma are included in the synthetic control models, such as lagged values of happiness, share of population in blue-collar occupations, share of population 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 particular combination varies with the sample.

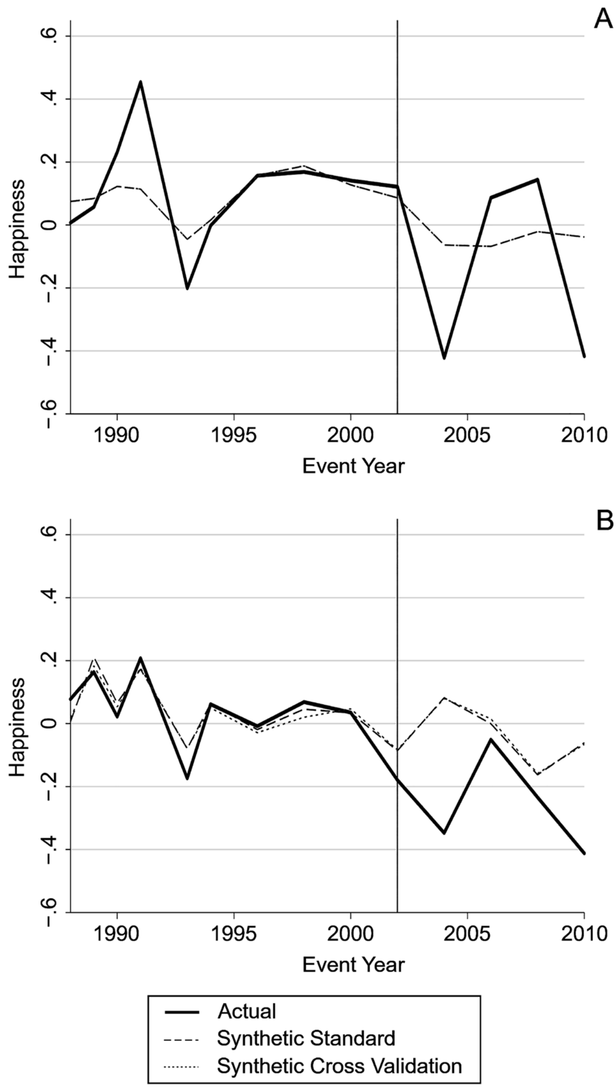

Figure 4. Synthetic control analysis for happiness in Oklahoma. *A*, All individuals; *B*, low-status individuals.

for the same set of states over the same time period (column 6 in Table 2), which translates to a difference of .12 SD.

To evaluate the credibility of these results, several robustness checks suggested in Abadie (2021) are presented in Section OA2 of the Online Appendix. In particular, we examine whether the observed gap in happiness is driven by a lack of predictive power of the SCM models, the choice of donor pool, and interstate spillover effects. These results, cross-checked with those obtained through the cross-validation method (shown in Figures 4 and OA3) confirm our original conclusion that RTW laws reduce self-reported happiness. It is also worth noting that no significant decrease in the happiness gap is observed when the states bordering Oklahoma are excluded from the analysis. If we assume that the well-being of a non-RTW state is more likely to adversely react to the adoption of an RTW law by neighbors than nonneighbors, then this result corroborates findings from the event-study analysis that even if confounders such as increased competition from an RTW state drive the decision to adopt an RTW law in a non-RTW state, they are not important correlates of happiness in this context.

4.4.3. Antiunion Sentiment and the Political Orientation of Governments

A distinct but related concern about increased competition is a state's preexisting opposition to unions, which potentially affects both the state's probability of adopting an RTW law (Bryson et al. 2019) and residents' rating of subjective well-being (Okulicz-Kozaryn, Holmes, and Avery 2014). A similar argument can be made for a change in the political orientation of state government that occurs simultaneously or immediately before the passage of an RTW law. 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 (for example, Michigan and Wisconsin) rather than an RTW law per se. While the SCM results obtained for Oklahoma provide indirect proof that an adverse effect of RTW laws on well-being exists in the absence of drastic changes in antiunion sentiment and/or partisan composition of government on 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.

Table OA4 first repeats the border-county analysis separately for states whose membership or coverage rate was at or above the national average 1 year prior to the adoption of RTW laws (Michigan, Wisconsin, West Virginia, and Kentucky) and states whose unionization rates fell below the average (Oklahoma and Indiana). If antiunion sentiment is responsible for explaining the results, then there should be a greater unionization effect for the former. The evidence, however, suggests the opposite. While a positive association between unionization and happiness is present for both groups, the size of the unionization effect is greater for states with weak union support. Given that the states with weak union support also tend to be early adopters of RTW laws, allowing for an equal follow-up window by focusing on the states that have at least 3 survey waves of postintervention data (Oklahoma, Indiana, and Michigan) does not lead to substantively different conclusions.

To explicitly evaluate the role of electoral outcomes, Table OA4 adds the partisan composition of each state government to the original border-county regressions, measured by the percentage of the state legislature controlled by the

Republican Party in the senate and house or assembly and the party affiliation of the governor at the beginning of a calendar year.[22] There is mixed evidence for whether Republican control of governments is associated with a lower level of well-being, although no fundamental meaning is attached to these estimates owing to unobserved heterogeneity. Importantly, conditional on these variables, the relationship between unionization and happiness remains qualitatively unchanged. To the extent that the sustained decline of organized labor may hurt Democrats in elections (Feigenbaum, Hertel-Fernandez, and Williamson 2018), this approach could lead to an underestimate 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 of our identifying assumption. The results provide little support that the estimated unionization or effect of RTW laws is biased by unobserved differences between switcher and non-RTW states. It is, however, important to note that even if the estimates survive a battery of indirect tests, unobserved heterogeneity cannot be completely ruled out as a possible explanation owing to data limitations and methods employed.

5. Exploring the Mechanisms

5.1. A Domain Satisfaction Approach

The domain satisfaction model in 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 finances and health. Tables 5 and 6 decompose happiness into domain satisfaction for low-status individuals, where the unionization effect is concentrated, by utilizing responses to questions about three relevant areas consistently asked in the GSS—financial satisfaction, self-rated health, and job satisfaction—to understand which component or components of happiness drive the observed differentials in well-being.[23,24] The estimated unionization effects using the preferred model specification (column 4 in Table 3) for all individuals and workers are presented in Table 5. Overall, labor unions contribute to both financial and job satisfaction, and the size of the effect is roughly the same in magnitude (.05–.06 SD). Personal health also is favorably affected by unionization, but a significant effect is present only among workers. This is likely if the health-related benefits are primarily conferred through workplace safety rather

[22] These variables are constructed from data from Council of State Governments (1994–2018) and documents at National Conference of State Legislatures, About State Legislatures (https://www.ncsl.org/about-state-legislatures/state-partisan-composition). Owing to their unique legislative organization, Nebraska and the District of Columbia are excluded from this exercise.

[23] The respondents in the GSS were also asked about 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.

[24] Subjective health is modeled as a binary indicator for a report of excellent or fair health, the top two categories (and zero otherwise), to account for potential nonlinearity. Coding it as a continuous scale generates less pronounced but qualitatively similar results.

Table 5

Unionization Effects on Satisfaction by Domain

	Finance	Health	Job
Full sample ($N = 48{,}743$):			
Density	.055**	.011	
	(.017)	(.009)	
Raw sample mean	1.956	.181	
Raw sample SD	.737	.385	
Workers ($N = 29{,}713$):			
Density	.057**	.018+	.050*
	(.015)	(.010)	(.020)
Raw sample mean	1.943	.209	3.237
Raw sample SD	.724	.407	.811

Note. Estimates are from a propensity-score instrumental-variable specification for the manufacturing sector for low-status individuals. The first-stage F-statistics range from 43 to 47. Standard errors, in parentheses, are two-way clustered by state and border pair.
+ $p < .1$.
* $p < .05$.
** $p < .01$.

Table 6

Satisfaction by Domain and the Relationship between Unionization and Happiness

	Full Sample ($N = 48{,}743$)		Workers ($N = 29{,}713$)			
	(1)	(2)	(3)	(4)	(5)	(6)
Density	.038**	.023*	.044**	.029*	.026*	.019
	(.013)	(.011)	(.013)	(.012)	(.013)	(.014)
Finance		.281**		.274**	.259**	.219**
		(.003)		(.006)	(.006)	(.006)
Health					.355**	.331**
					(.013)	(.013)
Job						.179**
						(.007)

Note. Estimates are from a propensity-score instrumental-variable specification for the manufacturing sector for low-status individuals. First-stage F-statistics range from 43 to 47. Standard errors, in parentheses, are two-way clustered by state and border pair.
* $p < .05$.
** $p < .01$.

than other public health initiatives that extend to nonworkers (such as the Affordable Care Act).

Table 6 further investigates the plausibility of finance, health, and employment as channels of transmission by including them in the original models. If the unionization effect operates through these channels, then values of the estimated density coefficient should decline. Across samples and model specifications, the domain satisfaction variables are positive and significant on their own, which indicates that an individual's more favorable perception of his or her financial situation, personal health, and work environment strongly predicts more happiness. When all three measures are controlled for (columns 3–6 in Table 6), the observed unionization effect completely vanishes, which implies that it is fully mediated by improvements in these domains of happiness. In particular, the inclusion of financial satisfaction is associated with the biggest decline in the estimated density coefficient (34 percent), followed by job satisfaction (27 percent) and personal health (10 percent). While these measures are highly correlated and are potentially endogenous in the equation, results from this exercise provide suggestive evidence that both economic and noneconomic factors may be at play in mediating the observed relationship between unionization and happiness.

5.2. A Closer Examination of Workers

Given the disproportionate effect of unionization observed for workers, Table OA5 provides more evidence for how individuals' quality of life evolved after passage of RTW laws by focusing on a group of randomly selected workers in the Quality of Work Life module of the GSS who provided opinions on topics including financial reality, workplace safety, work-life balance, discrimination, interactions between employees and employers, 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 satisfaction in each domain—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 effect of unionization on financial satisfaction observed in Section 5.1, Table OA5 provides corroborating evidence that union density positively affects workers' assessments of current finances. More workers report that their financial situation is staying the same or improving rather than getting worse as union density rises. For personal health, increased unionization lessens workplace safety concerns by .1–.2 SD and reduces the incidence of hand, wrist, arm, and/or shoulder pain by .03 SD. To the extent that work-life balance and job predictability may affect an employee's perspective on his or her work, Table OA5 indicates that a strong union presence leads to fewer work hours beyond the usual schedule (increase of .18 SD) and more time for workers to relax or pursue enjoyable activities outside work (increase of .16 SD, which is about the same in magnitude). While there is no evidence that unionization alleviates the degree of gender or race discrimination, there is a positive effect on relationship between employees and employers: workers report a higher level of trust in management, a greater likelihood of taking part in making decisions that affect them, and an overall happier relationship with their employer. Finally, to the extent that labor unions may restrict individuals' creativity by generating group thinking, Table OA5 reveals a deleterious effect of unionization on perceived fairness in promotion, though the impact of unionization on overall productivity is positive.

6. Discussion and Conclusion

The current study investigates the implications of unionization on well-being 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 unionized workers but everyone in society. Individuals toward the lower end of the income distribution, including those without a college education and current or former blue-collar job holders, are the primary beneficiaries of union efforts. Considering that the estimated effect of unionization is also concentrated on the employed, blue-collar workers with low levels of education thus may be at double jeopardy for the adverse effect of declining unionization.

What is noteworthy is that subjective evaluations of financial situation, personal health, and work environment are all tied to the favorable effect of unionization. Conditional on these measures of satisfaction, there is no significant effect of unionization for low-status workers, the group that drives the differential in well-being. This result points toward the value of nonpecuniary aspects of labor market experiences such as on-the-job safety, work-life balance, interpersonal trust, and workers' autonomy as mediators through which unionization benefits the most economically vulnerable. This finding is further supported by 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, the health channel is significant only for low-status workers. This finding has particular significance when considered with the evidence that a more pronounced adverse effect of reduced union presence on well-being is felt by these individuals. Typically employed in more physical occupations, these workers may be more vulnerable to injuries or chronic health conditions such as back or shoulder pain and thus more likely to have poor health. Therefore, union efforts to create safe workplaces by advocating for regulations enforced by public health entities such as the Occupational Safety and Health Administration, by investing in programs to educate workers about on-the-job hazards, and/or by working with employers to reduce workers' injuries and the time lost due to injury may be particularly fruitful avenues to protect the well-being of this socioeconomically 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 (such as productivity, profitability, and growth). Rich research from related fields (such as economics, political science, and psychology) has made substantial progress toward understanding the economic value and role of unions. However, our finding that average happiness decreases in response to the decline in unionization raises interesting questions about whether antiunionization policies may be detrimental to individuals' well-being, which can in turn carry long-term repercussions not just for individuals but also for firms' productivity and the economy overall (Oswald et al. 2015).

Rapid skill-based technical advancement is changing the economic landscape. Abstract, nonroutine jobs are replacing fixed-wage routine jobs, which widens income inequality and increases job polarization. Structural changes in the economy have led to a shift from the production of durable goods to service occupations in which cognitive skills enjoy higher returns, and artificial intelligence is replacing routine manual jobs in manufacturing sectors. Such a shift has been a boon for firms and the economy in the aggregate but perhaps has not been a boon for the well-being of those who make up the economy—the working class. While evidence suggests that policies such as RTW laws increase competition and may lead to firms' improved profitability, one ultimately needs to weigh the potential benefits of increased efficiency (for example, from maximizing workers' motivation by protecting the freedom not to join a union) against potential costs to individuals' well-being. Particularly important are the potential effects on health, especially in the context of increased rates of suicide and substance abuse over the last decade that have often been attributed to the despair or emotional pain and suffering of the less-educated, blue-collar working class. Thus, this topic merits further in-depth scrutiny.

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