

Do Right-to-Work Laws Work? Evidence on Individuals' Well-Being and Economic Sentiment

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Abstract

This paper investigates the effects of state right-to-work (RTW) laws on individuals' well-being and economic sentiment. Using licensed microdata from Gallup between 2008 and 2017, this paper finds that the adoption of RTW laws is associated with a .029 SD and a .041 SD increase in individuals' life satisfaction and economic sentiment, respectively. A difference-in-differences estimator suggests that these improvements are concentrated among union workers. These results are robust to entropy balancing and border-pair approaches. Moreover, these improvements in well-being are consistent with an increase in competition among unions, which prompts them to provide higher-quality services that are valued by their members.

1. Introduction

The share of states with right-to-work (RTW) laws has grown from 20 percent in 1960 to a slight majority as of 2018 (Figure 1). These laws prohibit union security agreements between companies and unions, which means that employees in unionized workplaces are not required to pay for union representation, even though they may still receive the benefits that paying members receive.

Despite mixed evidence that RTW laws have a positive effect on employment and wages (Warren and Strauss 1979; Hirsch 1980; Ellwood and Fine 1987), there is even more controversy about how they affect employees' subjective sense of well-being, especially among union workers who are most affected by the recent

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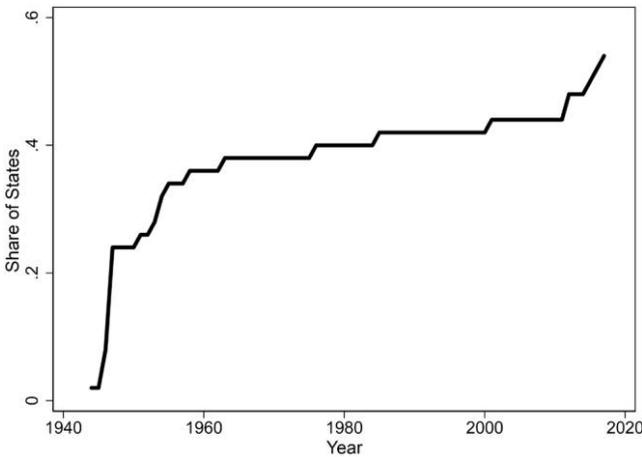


Figure 1. Right-to-work laws across states, 1944–2018

Supreme Court decision in *Janus v. American Federation of State, County, & Municipal Employees, Council 31* (138 S. Ct. 2448 [2018]), which concluded that public unions cannot force members to pay dues. These laws have been subject to significant criticism, particularly from labor unions.¹ The primary purpose of this paper is to examine the effects of these laws on individuals' well-being and economic sentiment.

Unfortunately, there have been at least two difficulties in empirically researching the effects of RTW laws on individuals' well-being. The first difficulty is in finding comprehensive data on subjective well-being over a sufficiently long time period. The second difficulty is in controlling for the unobserved ways that individuals in states with RTW laws may vary from their counterparts in other states. For example, states might adopt RTW laws because they are on an upward economic trend and are trying to attract more business. This paper overcomes these challenges by leveraging Gallup's US daily poll, which surveys 1,000 individuals per day since 2008 on topics relating to well-being (for example, life satisfaction) and their sentiments about both current and future economic activity; these polls also include respondents' information about demographic characteristics. Gallup's infrastructure and specialization in survey methodology make it uniquely capable of implementing large surveys with comparable questions over time.

To explore the effects of RTW laws on well-being, my baseline empirical specification compares outcomes among observationally equivalent individuals before and after the adoption of RTW laws in a state. The adoption of RTW laws is associated with a .029 SD increase in current life satisfaction, a .012 SD increase in expected future life satisfaction, and a .041 SD increase in economic sentiment

¹ For example, the American Federation of Labor and Congress of Industrial Organizations says that the "real purpose of right to work laws is to tilt the balance toward big corporations and further rig the system at the expense of working families" (AFL-CIO, Right to Work [<https://aflcio.org/issues/right-work>]).

about current and future economic activity. To address the concern that these estimates reflect other time-varying shocks in a state, I estimate a difference-in-differences (DD) specification that compares union and nonunion workers before and after the adoption of RTW laws.² Interestingly, the positive association between RTW laws and well-being is concentrated among union workers. These results are robust to an entropy-balancing estimator that reweights treated and control units on the basis of, for example, state employment growth and demographics (Hainmueller 2012). Moreover, the results are robust to a border-pair analysis that compares outcomes among individuals on opposite sides of state borders that differ in their RTW status, as in Holmes (1998).

What explains the positive effects of RTW laws on well-being, given prior literature on unions as a platform for expressing a collective voice (Freeman 1976; Freeman and Kleiner 1990)? Using additional data from the Current Population Survey (CPS; 1990–2017), County Business Patterns (CBP; 1990–2016), American Time Use Survey (ATUS; 2003–17), and the Panel Study of Income Dynamics (PSID; 1970–2016), I rule out three possible answers: income effects (that is, free riding without paying union dues allows for a higher disposable income), composition effects (that is, the marginal worker in the union has a greater sense of well-being), and correlated state economic policy shocks (that is, a series of probusiness legislation that more broadly improves economic conditions). Instead, I find that the adoption of RTW laws is associated with an approximate 1-percentage-point rise in the probability that workers report that their boss is more trusting and is more likely to treat them as partners. While not conclusive, the results are consistent with the view that RTW laws increase competition and, in turn, encourage unions to provide higher-quality services. Moreover, these results are consistent with recent empirical contributions that find a causal effect of competition on productivity in health care (Gaynor, Moreno-Serra, and Propper 2013; Bloom et al. 2015a), manufacturing (Syverson 2004; Schmitz 2005; Bloom et al. 2019), and retail (De Loecker 2011; Matsa 2011). My results also contribute to a larger literature on the effects of state labor market regulations—such as the minimum wage, occupational licensing, noncompete enforcement agreements, and wrongful-discharge laws—on economic outcomes, which uses similar methodological approaches.³

This is the first paper to formally examine how RTW laws affect measures of individuals' well-being, related to the broader literature on the effects of RTW laws on employment and wages. While there has been some mixed evidence that RTW laws are mainly symbolic and do not reduce union density (Lumsden and

² In addition to providing evidence that the parallel-trends assumption for well-being outcomes holds in the 2008–17 sample, I also examine the potential for pretrends using a longer sample of employment and establishment-level growth between 1990 and 2016 from the Census Bureau's County Business Patterns data. I do not find evidence of pretrends either at the state level or between union versus nonunion workers in the same state.

³ Since these literatures are too large to merit their own survey here, I refer readers to relevant recent work that sufficiently summarizes the latest results, including Jardim and van Inwegen (2017) and Jardim et al. (2017) about the minimum wage; Kleiner (2006) about occupational licensing; Starr (2015) and Starr, Balasubramanian, and Sakakibara (2018) about noncompete contracts; and Autor, Donohue, and Schwab (2006) about wrongful discharge laws.

Petersen 1975; Farber 1984), most studies find a negative effect (Warren and Strauss 1979), at least on union membership (Hirsch 1980), that subsequently decays over time (Ellwood and Fine 1987). There is also a large literature that finds a positive effect of RTW laws on employment and wages, arguing that it is necessary to control for unobserved location-specific factors to recover causal effects (Reed 2003; Kalenkoski and Lacombe 2006).⁴ These laws are more broadly representative of probusiness policies that are associated with increases in the manufacturing employment share (Holmes 1998). However, the difficulty of exploiting sources of exogenous variation remains a recurring challenge in this literature, so scholars have applied synthetic control methods (Eren and Ozbeklik 2016).

The results also relate directly to the literature on the causal effects of unions on both workers and firms. On the firm side, Klasa, Maxwell, and Ortiz-Molina (2009) and Matsa (2010) show that unions alter the capital structure of firms by encouraging managers to undertake greater debt as a way of increasing bargaining power against union leaders. Similarly, Lee and Mas (2012) show that unions have a negative effect on firm value of roughly \$40,500 per unionized worker (10 percent of the average firm market value), which is consistent with early evidence that unionized companies had roughly 10 percent lower market values and earnings between 1972 and 1980 compared with their counterparts (Hirsch 1991). On the employee side, Freeman (1976) and Freeman and Kleiner (1990) show that unions allow employees to express a collective voice (for example, regarding grievances), in addition to allowing them to earn higher wages for comparable nonunion jobs (Freeman 1982; Card 1996). Moreover, unions have traditionally helped mitigate inequality by conferring benefits toward blue-collar households (Card 1996; Farber et al. 2018).

The structure of the paper is as follows. Section 2 introduces background and the theoretical framework for understanding how RTW laws might impact well-being. Section 3 describes the data and measurement strategy. Section 4 presents the empirical model and results. Section 5 examines the potential mechanisms. Section 6 concludes.

2. Background and Theoretical Framework

In 1935, the National Labor Relations Act (NLRA) made it possible for private-sector workers to unionize and enter collective-bargaining agreements; the agreements required every employee covered by the contract to pay dues to the negotiating labor organization (Collins 2014). However, the Taft-Hartley Act of 1947 later amended the NLRA, allowing states to supersede the union security agreements by enacting RTW laws. Since then, 27 states have enacted RTW laws (Figure 1) despite significant pushback from unions.⁵

⁴ Rinz (2018) also exploits historical variation in the 1947 Taft-Hartley Act, finding a generally positive 3–4 percent increase in wages concentrated among middle-skill workers and those outside highly unionized industries.

⁵ See Leef (2010) for a comprehensive and detailed historical discussion. There is, however, one state, Missouri, that passed right-to-work (RTW) legislation in early 2017 and subsequently rolled it back in August 2018. This is possible when a newly passed state law is overturned by a referendum based on the responses of a sufficient number of residents who petition for a vote.

The literature on RTW laws has largely focused on two main sets of outcomes: union activity and both employment and wages. While there is generally strong evidence that RTW laws have a negative effect on union activity that decays over time (Warren and Strauss 1979; Hirsch 1980; Ellwood and Fine 1987), evidence of their effects on employment and wages has been more mixed.⁶ For example, using RTW laws as a proxy for probusiness policy and county variation on different sides of state borders, Holmes (1998) finds that states with RTW laws have much higher levels and growth of manufacturing activity. Similarly, Reed (2003) and Kalenkoski and Lacombe (2006) find that RTW laws have positive effects on wages and employment once important location-specific factors are introduced as controls. However, focusing on Oklahoma's adoption of RTW laws using a synthetic control approach, Eren and Ozbeklik (2016) do not find any significant effects on employment or wages but do find an effect on union density.

Despite this well-developed literature that analyzes the effects of RTW laws on employment and wages, there has been no investigation of the impact of RTW laws on workers' well-being. This paper argues that there are four possible mechanisms. One is that RTW laws allow workers to free ride on union services, simultaneously reaping union benefits and holding greater disposable income to spend on other things, like consumption. This is a prominent concern among union leadership, who warn about the impact of RTW laws on the long-term health of unions and collective-bargaining agreements. Second, workers who really like the union and report high life satisfaction may remain part of it, while marginal workers may exit, thereby reflecting a composition effect in the types of workers who participate. Third, RTW laws might force unions to become more competitive and improve the services they offer to their members. Since states without RTW laws provide a steady and guaranteed stream of income to unions by fiat, the incentives for unions to provide valuable services to workers are quite weak.⁷ Fourth, since RTW laws are highly controversial among unions, states that adopt the laws might pass them at the same time that they pass a broader set of probusiness policies. Through a series of diagnostics, I examine the evidence in support of these potential mechanisms.

It is not a new idea that greater competition may increase productivity by promoting improvements in management (Bloom et al. 2015a) and workplace practices (Schmitz 2005). For example, Schmidt (1997) develops a theoretical model in which greater competition increases managerial effort as competitors enter a market, but effort declines once competition becomes too intense. Barring exceptions where competition becomes too extreme, greater competition creates greater incentives for managers to increase productivity (Raith 2003).⁸ By allow-

⁶ Some early studies argue that the passage of these laws was more symbolic than causal of any real declines in union activity (Lumsden and Petersen 1975; Farber 1984).

⁷ Although there is no literature on the effect of such competitive forces on union services, the theoretical channel builds on a mountain of theoretical and empirical evidence in industrial organization linking competition with consumer welfare (for example, see Ho and Lee [2017] for evidence from the health care sector).

⁸ The positive effects of competition on productivity have been confirmed in many sectors, such as health care (Gaynor, Moreno-Serra, and Propper 2013; Bloom et al. 2015a), manufacturing (Syverson 2004; Schmitz 2005; Bloom et al. 2019), and retail (De Loecker 2011; Matsa 2011).

ing workers the option not to pay union dues, RTW laws can generate similar competitive effects that encourage unions to offer more valuable services to win over prospective future members.⁹

3. Data and Measurement

3.1. Gallup Daily Polling Repeated Cross Section

The primary source consists of newly licensed data from Gallup Inc. Gallup is the United States' premier polling service and conducts daily surveys of 1,000 US adults on various political, economic, and well-being topics. In particular, 200 Gallup interviewers conduct computer-assisted telephone interviews with randomly sampled respondents (age 18 or over) from all 50 states and the District of Columbia. Detailed location data, such as the zip code and metro area, are also collected, along with corresponding sample weights. These data are used by Kahneman and Deaton (2010) to study the relationship between well-being and income, by Deaton (2012) to study well-being over the Great Recession, and by me (Makridis 2018b) to study the effects of economic sentiment on real activity and the effects of mortgage foreclosures and housing prices on well-being (Makridis and Ohlrogge 2018).¹⁰

Gallup's polling relies on live (not automated) interviews with dual-frame sampling (including a random-digit-dial method) of landline and wireless phones. Half of the respondents receive the well-being track version (with a 9 percent survey response) of the survey questions, whereas the other half receives the politics and economy track version (with a 12 percent survey response). The surveys contain different topical questions, but both contain the same identifying demographic information. Gallup also conducts the survey in Spanish to record replies from Spanish speakers who do not also speak English. The sampling methodology uses a three-call design to reach respondents who do not answer the original attempted call. The primary measures of well-being are on a 0–10 scale of perceived current and expected future (in 5 years) life satisfaction from the Gallup-Sharecare well-being index, and the measure of economic sentiment is the sum of a 1–4 index of the respondent's perception of the current state of the economy and a 1–3 index of his or her perception of the future state of the economy. Table 1 compiles the relevant survey questions.

Table 2 presents descriptive statistics on the Gallup data, focusing on two (arbitrary) partitions of the sample: between 2008 and 2011 and between 2012 and

⁹ However, unlike traditional forms of competitive policy, RTW laws also may generate nonpecuniary benefits. In particular, since RTW laws allow employees to choose whether to pay union dues, they implicitly expand employee choice and autonomy. Even if an employee decides to pay union dues, the choice may affect his or her well-being. Unfortunately, distinguishing between the causal effect of competition and these nonpecuniary effects is challenging; this paper provides only suggestive evidence consistent with the role of competition.

¹⁰ As Deaton (2012) discusses, the measurement of life satisfaction and economic sentiment was slightly different in 2008 relative to other years. It is therefore possible that the inclusion of the year introduces some measurement error. In practice, results are insensitive to dropping 2008.

Table 1
Main Gallup Survey Questions

	Question	Scale
Life satisfaction	Please imagine a ladder with steps numbered from 0 at the bottom to 10 at the top. The top of the ladder represents the best possible life for you, and the bottom of the ladder represents the worst possible life for you. On which step of the ladder would you say you personally feel you stand at this time?	0–10
Expected future life satisfaction	On which step do you think you will stand about 5 years from now?	0–10
Perception of current economic activity	How would you rate economic conditions in this country today: as excellent, good, only fair, or poor?	1–4
Perception of future economic activity	Right now, do you think that economic conditions in this country, as a whole, are getting better or getting worse?	1–3
Hiring	Now thinking more generally about the company or business you work for, including all of its employees, based on what you know or have seen, would you say that, in general, your company or employer is (a) hiring new people and expanding the size of its workforce, (b) not changing the size of its workforce, or (c) letting people go and reducing the size of its workforce?	1–3

Table 2
Descriptive Statistics for Right-to-Work (RTW) and Non-Right-to-Work States

	RTW, 2008–11		Non-RTW, 2008–11		RTW, 2012–17		Non-RTW, 2012–17	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Demographics:								
Average age	48.0	17.7	47.9	17.6	47.4	18.2	47.2	18.2
Male	.48	.50	.49	.50	.49	.50	.49	.50
Average number of children	.87	3.55	.85	3.55	.94	4.44	.92	4.60
No high school	.13	.34	.11	.31	.12	.32	.11	.31
High school	.30	.46	.30	.46	.30	.46	.29	.45
Technical school	.06	.24	.06	.24	.04	.21	.04	.20
Some college	.23	.42	.22	.41	.25	.43	.24	.43
College	.16	.37	.17	.38	.16	.37	.18	.38
Postgraduate	.12	.32	.14	.35	.12	.32	.15	.36
White	.69	.46	.74	.44	.68	.47	.69	.46
Public sector	.19	.39	.17	.37	.16	.37	.16	.36
Employed	.59	.49	.60	.49	.60	.49	.62	.49
Union	.06	.25	.16	.37	.07	.25	.15	.35
Sentiment:								
Current life satisfaction	6.87	2.07	6.80	2.01	6.98	2.02	6.98	1.96
Future life satisfaction	7.71	2.32	7.66	2.23	7.81	2.25	7.78	2.17
Believes economy is good	.14	.35	.12	.32	.23	.42	.22	.42
Believes economy is improving	.28	.45	.28	.45	.41	.49	.43	.50
Uses strengths at work	.85	.36	.84	.36	.86	.35	.86	.35
Boss treats like a partner	.63	.48	.62	.48	.61	.49	.61	.49
Boss creates environment of trust	.81	.39	.79	.41	.82	.39	.80	.40
N	529,662		847,495		993,502		1,056,734	

Note. Current life satisfaction is scaled on a 0–10 index, where 10 is the best possible life on the Cantril life ladder; expected future life satisfaction is 5 years in the future. Observations are weighted using the survey's sample weights.

2017. There are few statistically significant differences between RTW and non-RTW states with respect to demographic characteristics such as age, gender, and number of children. However, there are some differences in race and education. For example, RTW states tend to have a lower share of whites compared with non-RTW states between 2008 and 2011 (69 percent versus 74 percent), although these differences vanish between 2012 and 2017, which potentially reflects the aftermath of the Great Recession. Moreover, while there are few differences in low to medium levels of educational attainment between RTW and non-RTW states, RTW states tend to have a slightly lower share of individuals with a postgraduate degree (12 percent versus 14–15 percent). However, there are stark differences in union density across states, which is consistent with the early empirical evidence (Warren and Strauss 1979; Hirsch 1980).

Turning toward differences in subjective well-being and economic sentiment, I consider current and expected future life satisfaction. The RTW states tend to have slightly higher levels of life satisfaction relative to non-RTW states, both between 2008 and 2011 and between 2012 and 2017, as well as greater dispersion in life satisfaction. These differences in well-being could reflect differences in labor market institutions. However, differences in perceptions of the current and future state of the economy tend to be more minor, although the evidence suggests that individuals in RTW states tend to be more optimistic about the current state of the economy. Finally, focusing on differences in workplace practices, I find evidence that RTW states have better work environments. For example, 81–82 percent of individuals in RTW states report that their boss creates an environment of trust, whereas non-RTW states have slightly lower rates (79–80 percent). These differences are not driven by composition effects arising from differences in occupational concentration.

Do these measures of well-being and sentiment reflect genuine changes in real economic activity? First, my companion work (Makridis 2018b) shows that a 1-percentage-point rise in employment and housing-price growth is associated with a .12-percentage-point and a .27-percentage-point rise, respectively, in the probability that an individual reports that the national state of the economy is growing, which reflects the fact that the arrival of good news improves economic sentiment. Similarly, a 1-percentage-point rise in employment growth is also associated with a .34-percentage-point rise in the probability that an individual reports that his or her firm is expanding, which suggests that self-reported information about economic sentiment reflects authentic improvements in local economic activity. Second, changes in subjective well-being are also correlated with workplace practices. For example, individuals answering that trust exists at their workplace and that their boss treats them like a partner report a .268 SD and a .15 SD higher current life satisfaction, respectively, conditional on controls.¹¹

¹¹ For more comparisons, see Deaton (2018), who shows that there is an inverse U-shape in the age profile of reported life satisfaction that matches the patterns in the Germany socioeconomic panel; moreover, Aghion et al. (2016) show that these measures of current and expected future life satisfaction are associated with measures of creative destruction and churn in the labor market.

Of course, there are limitations to survey questions about subjective well-being. First, there is the halo effect, which refers to the tendency for recipients to answer different questions with the same mental state of mind, producing answers that spill over from one survey question to another. However, Oswald (2008) develops a proxy for a more objective measure of well-being on similar data and finds a correlation of .80 between this kind of data and the commonly reported measure of subjective well-being; these measures have also been used in a number of empirical exercises (Di Tella, MacCulloch, and Oswald 2001; Oswald and Wu 2010, 2011; Deaton 2012). Moreover, the halo effect is mainly a concern when the primary independent variable of interest is also a survey question, whereas in my setting the primary coefficient of interest varies at the state-year-month level.

Second, Bond and Lang (2019, p. 1638) argue that “[i]t is essentially impossible to rank two groups on the basis of their mean happiness using the types of survey questions prevalent in the literature.” While the use of subjective well-being measures remains an ongoing debate (for example, see Krueger and Schkade 2008), this paper presents two series of robustness exercises. First, instead of relying on the ordinal index, I create binary indicator variables denoting that an individual is highly satisfied with his or her life and/or optimistic about the economy. Second, following the recommendation that “we would require a functional form assumption that survived the joint test of the parametric functional form and common reporting function across groups” (Bond and Lang 2019, p. 1639), I also show robustness across several common functional forms (for example, standardized z-score and logarithms); see Online Appendix OA.

3.2. *State Right-to-Work Laws*

The microdata from the Gallup polls were matched with an indicator for state adoption of RTW laws made available through the National Conference of State Legislatures. Figure 2 documents the states with RTW laws as of 2018. Figure 3 plots the distribution of six state-level variables between 2008 and 2016 across states with and without RTW laws. States with RTW laws vary in a number of ways. For example, they have an annual population growth rate of 1.6 percentage points, whereas non-RTW states have a growth rate of .91 of a percentage point. They also have an employment growth rate of 1.8 percentage points, compared with 1.1 percentage points for their counterparts (unemployment rates are similar), and a larger manufacturing share, consistent with Holmes (1998). However, states with RTW laws tend to have a lower share of residents with college degrees—26 percent in comparison with the 30 percent of their counterparts.

3.3. *Supplemental Panels and Repeated Cross Section*

Additional data come from the CBP (1990–2016), CPS (1990–2017), ATUS (2003–17), and PSID (1970–2016). Data from these sources are used to disentangle the potential mechanisms that can explain the main results for well-being. The CBP is an annual series from the Census Bureau and provides data on em-

ployment, wages, and payroll expenditures by geography and sector. The CPS Outgoing Rotation Group (accessed through the Integrated Public Use Microdata [IPUMS] data portal) provides complementary data for individuals, tracking individuals' earnings and hours worked with information on union membership and state.

The ATUS is conducted on individuals who are sampled about 3 months after completing the final CPS of the year (Hamermesh, Frazis, and Stewart 2005). It is a three-stage stratified sample: after taking a subsample of CPS households, the ATUS sample is distributed equally across states on the basis of each state's population share; households are stratified by race, presence and age of children, and number of adults in the household; and, finally, an eligible individual in the household who is at least 15 years old is randomly selected to participate. Each wave is based on 24-hour time diaries in which individuals report their activities from the previous day. To harmonize the observations, the survey personnel assign activities reported by individuals to categories relating to time use that the Bureau of Labor Statistics has established. Finally, the PSID (collected by the University of Michigan) differs from the CPS and ATUS in that it tracks a panel of individuals over time. The sample is restricted to able-bodied heads of households between ages 25 and 65, which produces waves of approximately 1,000–3,000 individuals per year (every other year from 1997 onward). For each data set, nominal variables are deflated by the 2010 personal consumption expenditure price index, and individuals working fewer than 500 hours per year or earning less than \$5,000 annually are dropped.

4. Right-to-Work Laws and Well-Being

4.1. Identification

To understand the relationship between measures of individual well-being and economic sentiment, I begin by considering a standard fixed-effects regression of the form

$$y_{ist} = \gamma \text{RTW}_{st} + \beta \mathbf{X}_{it} + \eta_s + \lambda_t + \varepsilon_{ist}, \quad (1)$$

where y denotes the individual outcome, RTW denotes an indicator for whether the state has an RTW law, \mathbf{X} denotes a vector of individual covariates, and η and λ denote state and year fixed effects.¹² Standard errors in equation (1) are clustered at the state level to allow for arbitrary degrees of autocorrelation in the same location over time (Bertrand, Duflo, and Mullainathan 2004).

Individual covariates include fixed effects on the highest degree earned, gender, age, and race.¹³ The inclusion of state and time fixed effects removes time-

¹² I also experimented with a wide array of other controls, but they do not produce statistically different gradients on the RTW coefficient. For example, I included zip-code housing-price growth to control for the potential impact of the Great Recession on states during their potential decision-making and adoption of RTW laws. I also controlled for state and county industry composition to control for potential reallocation.

¹³ I also experimented with party affiliation to control for the potentially symbolic nature of RTW laws but omit these data from the main results since they are not measured in the full sample.

invariant characteristics across location that potentially make states with RTW laws systematically distinct from their counterparts. These fixed effects address the identification concern that RTW states vary in other probusiness ways that also attract more economic opportunity. In this sense, equation (1) identifies the causal effect of RTW laws on the basis of within-state comparisons of observationally equivalent workers before and after the adoption of RTW laws. Although 27 states have RTW laws, only five states adopted them within the 2008–17 sample period: Michigan and Indiana (2012), Wisconsin (2015), West Virginia (2016), and Kentucky (2017).¹⁴

Recognizing the potential nonrandom nature of these laws, I now consider an additional formulation of equation (1), focusing more specifically on the individuals who are directly affected by RTW laws through a DD estimator comparing the outcomes of union workers with their counterparts before versus after the adoption of RTW laws in the state:

$$y_{ist} = \gamma \text{RTW}_{st} + \xi u_{it} + \zeta (\text{RTW}_{st} \times u_{it}) + \beta \mathbf{X}_{it} + \eta_s + \lambda_t + \varepsilon_{ist}, \quad (2)$$

where u denotes an indicator for working in a union job. The primary coefficient of interest in equation (2) is ζ , which characterizes how individual well-being changes for union workers after the adoption of RTW laws. The identifying assumption is that union workers would have trended similarly to nonunion workers in RTW states had RTW laws not passed.

One limitation with the DD implementation is that the union indicator is available only between 2009 and 2016, which reduces the sample size and identifying variation, especially since Kentucky only adopted an RTW law in 2017. An additional concern with these specifications is that they fail to account for other sources of time-varying unobserved heterogeneity. I therefore also adopt a balancing method from Hainmueller (2012) by reweighting on the basis of state monthly employment growth and individuals' age, college attainment, and race.¹⁵ While this approach potentially overcontrols since RTW laws may directly affect employment, it allows me to purge variation in economic activity that might also be driving variation in individuals' well-being.

A final concern arises from potential time-varying selection into unions. First, union elections could be correlated with the timing of the laws. However, if they were, then unions would be more likely to talk negatively about the prospective

¹⁴ Missouri is not included because the law was defeated in 2018 via a referendum before it could take effect.

¹⁵ I implement the approach using a package developed by Hainmueller and Xu (2013). The method improves on the classic synthetic control approach in Abadie (2005) and Abadie, Diamond, and Hainmueller (2010). The entropy-balancing approach works by constructing weights such that the covariate distributions of the control group in the preprocessed data match on all the prespecified moments (although I use only the first moment for simplicity). The approach has several advantages over conventional matching methods. First, it allows for matching on higher-order moments, not just the first moment. Second, although the weights are chosen to ensure balancing, they are kept as close as possible to the base weights to avoid the loss of information, and it is, therefore, a generalization of the propensity-score-weighting approach in Hirano, Imbens, and Ridder (2003).

RTW law under consideration and its subsequent passage, which would prompt employees in the union to be more pessimistic, not more optimistic, about well-being and economic sentiment (that is, downward bias). Second, union workers might be more likely to report higher levels of well-being. While union workers have a 1.7 percent higher reported current life satisfaction in the cross section, the fact that union density is on the decline would bias against my results since RTW laws have a negative effect on union density. Despite these realities, the results that follow deal with the potential for selection effects.

4.2. *Main Results*

Table 3 documents the results associated with equation (1). I focus on three outcomes: self-reported current life satisfaction, expected future life satisfaction in 5 years, and economic sentiment. Whereas the first two are measured on an ordinal scale of 0–10, the third is measured on a scale of 2–7 by taking the sum of the 1–4 index of perceptions of the current state of the economy and the 1–3 index of perceptions of the future state of the economy. Beginning with the conditional correlations without fixed effects, I find that individuals in states with RTW laws exhibit .027–.031 SD higher current and expected future life satisfaction and .032 SD higher economic sentiment. Turning to the demographic characteristics, I find that age is positively associated with current life satisfaction but negatively associated with economic sentiment and expected future life satisfaction. Moreover, while education (for example, college attainment) is positively associated with life satisfaction and economic sentiment, whites experience substantially lower levels of all three outcomes, consistent with existing evidence on the declining well-being of noncollege white males (Case and Deaton 2017).

These conditional correlations, however, could be biased because of unobserved differences between states with and without RTW laws. Turning to the regressions with state and time fixed effects, I find that the adoption of RTW laws is associated with a .029, .012, and .041 SD rise in current life satisfaction, expected future life satisfaction, and economic sentiment, although the gradient on economic sentiment is not statistically significant at conventional levels. The fact that the fixed-effect results are marginally lower in magnitude for the life satisfaction outcomes suggests the potential for upward bias in the cross section; that is, states adopting RTW laws may also experience an unobserved positive productivity shock that jointly affects well-being. To partially address the concern in Bond and Lang (2019) that the ordinal scales of well-being are not comparable across individuals, Table OA1 in Online Appendix OA presents robustness using alternative functional forms.

Since the outcomes are all measured as an index, it might be hard to interpret whether the magnitudes are economically significant at face value. While the estimated elasticity between RTW laws and well-being is surprisingly similar to the elasticity between management scores and RTW laws in Bloom et al. (2019, table 5), I put the results in perspective using the following two exercises. First,

Table 3
 Baseline Estimates of Right-to-Work Laws on Well-Being and Economic Sentiment: 2008–17 Gallup Data

	Current Life Satisfaction		Future Life Satisfaction		Economic Sentiment	
	(1)	(2)	(3)	(4)	(5)	(6)
RTW Law	.031* (.014)	.029** (.005)	.027** (.007)	.012+ (.007)	.032 (.021)	.041 (.036)
Age	.002** (.000)	.002** (.000)	-.015** (.000)	-.015** (.000)	-.003** (.000)	-.003** (.000)
Male	-.126** (.003)	-.127** (.003)	-.128** (.004)	-.129** (.004)	.084** (.005)	.081** (.005)
No high school	-.147** (.007)	-.141** (.007)	-.279** (.013)	-.275** (.013)	-.058** (.019)	-.040** (.015)
High school or technical school	-.052** (.003)	-.045** (.004)	-.095** (.004)	-.090** (.004)	-.059** (.007)	-.042** (.006)
College	.203** (.004)	.201** (.004)	.108** (.003)	.106** (.003)	.204** (.005)	.204** (.004)
White	-.064** (.008)	-.048** (.007)	-.143** (.014)	-.130** (.016)	-.233** (.019)	-.218** (.019)
R ²	.04	.05	.12	.12	.03	.09
N	2,451,437	2,451,437	2,346,887	2,346,887	1,703,839	1,703,839
State fixed effects	No	Yes	No	Yes	No	Yes
Time fixed effects	No	Yes	No	Yes	No	Yes

Note. Coefficients are from regressions of standardized (*z*-score) individual current and expected future life satisfaction (0–10 scale) and perceptions about the current and future state of the economy (2–7 scale) on an indicator for whether the state has right-to-work laws and controls (education fixed effects [no high school, high school or technical school, or college; some college is the omitted group], gender, marital status, number of children, and race [white]). Standard errors are clustered at the state level, and sample weights are used.

+ $p < .10$.
 * $p < .05$.
 ** $p < .01$.

consider the conditional correlation on college attainment in column 2 of Table 3, which suggests that workers with college degrees have .201 SD higher life satisfaction relative to their counterparts. Then the marginal effect on RTW laws is roughly 14.4 percent of the marginal effect of college attainment ($= .029/.201$). Second, consider the fact that workplace practices are only one determinant of overall life satisfaction. In particular, regressions of standardized life satisfaction on an indicator for whether an individual feels like there is trust at work or whether the boss treats him or her like a partner produce gradients of .268 and .15, respectively. Given that the earlier elasticity with respect to well-being is .029, or 11–20 percent of the gradient on workplace practices, then, coupled with the fact that the share of union workers is 12 percent between 2008 and 2017 in this sample, the treatment effect could account for a standard deviation increase in life satisfaction among union workers—that is, from 6/10 to 8/10.¹⁶

While these results point toward the aggregate effects and popularity of RTW laws, they are vulnerable to the concern that states adopting them vary in potentially other unobserved ways; that is, they might be trending up in economic activity and, therefore, implying a spurious positive association. To deal with this concern more explicitly, Table 4 documents the results associated with equation (2) from the DD estimator under two specifications. Columns 1 and 4 present a canonical DD estimator based on the survey sample weights. Columns 2 and 5 present a reweighted DD estimator along the lines of Abadie (2005) by estimating the weights through entropy balancing as in Hainmueller (2012).¹⁷ Under the preferred DD and reweighted estimator, the adoption of RTW laws is associated with a .021 SD and a .076 SD increase in current life satisfaction and economic sentiment among union workers, and these values are significant at the 5 percent and 1 percent levels, respectively. Consistent with the potential for upward bias, reweighting reduces the gradient on economic sentiment from .093 to .076 SD.

However, one concern is that reweighting does not fully control for heterogeneous state trends. Columns 3 and 6 control for state-year-month fixed effects, exploiting variation in reported well-being among union versus nonunion workers after controlling for shocks that are common among workers in the same state at a point in time, thereby leveraging variation in the share of union workers. Two concerns emerge. First, is selection into unions nonrandom and time varying? If anything, declining union density should imply that the remaining workers are more pessimistic about economic prospects, which would create down-

¹⁶ Moreover, since the measure of life satisfaction is a categorical variable, changes in policy might not influence the marginal worker. That is, an individual might choose to report the same 6/10 score because the policy change did not push him or her over the threshold for giving a 7/10, for example. If, in contrast, the variable is continuous, more individuals would likely report changes in reported well-being, which would generate a larger treatment effect. Unfortunately, this is a limitation inherent in these data.

¹⁷ Compared with other propensity-score estimators, like synthetic controls (Abadie, Diamond, and Hainmueller 2010), entropy balancing is more flexible because any transformation of the covariates (for example, not necessarily a polynomial function) is used to achieve a balancing. In practice, I use state-level employment growth, an indicator for college attainment, age, and an indicator for being white to achieve balancing between states with and without RTW laws.

Table 4
 Difference-in-Differences Estimates of Right-to-Work Laws on Well-Being and Economic Sentiment: 2009–16 Gallup Data

	Current Life Satisfaction			Economic Sentiment		
	(1)	(2)	(3)	(4)	(5)	(6)
RTW Law	.005 (.007)	-.003 (.008)		-.003 (.036)	-.038 (.025)	
Union	.036** (.006)	.024** (.005)	.024** (.005)	-.012 (.011)	-.002 (.016)	-.000 (.016)
Union × RTW Law	.019+ (.011)	.021* (.008)	.021* (.008)	.093** (.015)	.076** (.018)	.074** (.018)
R ²	.05	.06	.06	.09	.09	.10
N	1,034,569	1,034,569	1,034,566	712,831	712,831	712,827
State fixed effects	Yes	Yes	Yes	No	Yes	Yes
Time fixed effects	Yes	Yes	Yes	No	Yes	Yes
Balancing weight	No	Yes	Yes	No	Yes	Yes
State-time fixed effects	No	No	Yes	No	No	Yes

Note. Coefficients are from regressions of standardized (z -score) individual current life satisfaction (0–10 scale) and perceptions about the current and future state of the economy (2–7 scale) on indicators for whether the state has right-to-work laws and whether the individual is in a union job, their interaction, and controls (education fixed effects [no high school, high school or technical school, or college, normalized to some college as the omitted group], gender, marital status, number of children, and race [white]). Standard errors are clustered at the state level, and sample weights are used unless an entropy-balancing weight is indicated (Hainmueller 2012). Balancing weights are constructed by regressing RTW Law on state monthly employment growth, age, an indicator for college attainment, and race (white) using the package from Hainmueller and Xu (2013).

+ $p < .10$.

* $p < .05$.

** $p < .01$.

ward bias. Second, is there enough variation in union density? If limited variation is a concern, it would attenuate the results. Nonetheless, the gradients in columns 3 and 6 are statistically indistinguishable from their counterparts.

In addition to validating the baseline results that RTW laws are associated with improvements in life satisfaction and economic sentiment, these results highlight two important corollaries. First, the fact that the effects are concentrated exclusively among the union-RTW interaction, rather than the direct effect of the RTW indicator, suggests that these states were not trending in potentially unobserved ways. Union workers are precisely the ones who are expected to experience a change in well-being and sentiment since RTW laws affect them directly. If, for example, a doctor were to be heavily affected, either the short-term general equilibrium effects would have to be large or it would indicate a potentially omitted variable. Second, the estimates on economic sentiment are especially strong and large—roughly half the magnitude on the marginal effect of college attainment in Table 3. These estimates, therefore, suggest that the passage of RTW laws fundamentally increases the optimism that union workers have about their economic prospects.

Given that these results are identified on the basis of within-state variation, one concern is that they are not externally valid since only six states adopted RTW laws between 2008 and 2017. One way to gauge the robustness of these results and the degree of within-state variation is to exploit the cross section using a more carefully weighted entropy-balancing estimator. Since the primary concern is that states with RTW laws tend to have greater employment growth (1.8 percentage points versus 1.1 percentage points per year; see Figure 3) and more manufacturing workers (12 percent versus 11 percent; see Figure 3), I estimate new weights using entropy balancing over state-level employment and population growth and employment shares in construction, manufacturing, and retail trade. Using these estimated weights, I find that RTW laws are associated with .028 SD higher life satisfaction ($p = .041$), which compares with .038 SD ($p = .014$) under an ordinary least squares model. In this sense, the reweighted cross-sectional estimates imply gradients within the confidence intervals of the baseline results.

4.3. Robustness Exercises

To the extent that endogeneity concerns associated with the previous estimates remain, they would require stories about time-varying differences between union and nonunion workers in states that adopted RTW laws. In other words, the passage of RTW laws must not be correlated with preexisting differences in either the treatment or control groups.

4.3.1. Other Time-Varying Shocks

Related to the fourth potential mechanism behind these effects, states that pass RTW laws may have passed other probusiness policies that increased well-being. Although an internal review of these policies suggests that they cannot account

for the effects,¹⁸ I provide more evidence that these potential confounders are not biasing my estimate; these results are summarized below (see Online Appendix OB for details).

First, I introduce an array of time-varying state characteristics, including state population growth; the age, education, and race distributions (bins of the share of individuals within different ranges); and, most important, state employment growth (and its lags). Their inclusion does not alter the baseline, consistent with the identifying assumption that time-varying shocks to well-being are uncorrelated with the adoption of these laws. Second, I exploit plausibly exogenous daily variation in the staggered adoption of RTW laws among eight counties in Kentucky between 2014 and 2015. Since Gallup began distinguishing between five types of well-being in 2014, I examine the association between RTW laws and both overall well-being (the sum of five well-being indices) and purpose-specific (workplace-related) well-being. Consistent with the view that the positive effects on subjective well-being are moderated through workplace practices, I find a noisy increase in overall well-being but a statistically significant increase in purpose-related well-being.

4.3.2. Parallel-Trends Assumption

The DD estimator relies on the assumption that union workers would have trended similarly to nonunion workers had states not adopted RTW laws (parallel trends). To examine the potential for pretrends, I estimate regressions of the form

$$y_{ist} = \gamma_1 \text{RTW}_{s,t-2} + \gamma_2 \text{RTW}_{s,t-1} + \gamma_3 \text{RTW}_{s,t} + \gamma_4 \text{RTW}_{s,t+1} + \gamma_5 \text{RTW}_{s,t+2} + \beta \mathbf{X}_{it} + \eta_s + \lambda_t + \varepsilon_{ist}, \quad (3)$$

where γ_1 and γ_2 provide an indication of the potential presence of pretrends and γ_4 and γ_5 provide an indication of the longer-term effects of RTW laws. Unfortunately, there is only limited time-series variation in the introduction of RTW laws. For example, the identifying variation is effectively coming only from Michigan and Indiana, which introduced their RTW laws in 2012, which provides ample pre-RTW and post-RTW samples for the inclusion of lag and forward variables. Equation (3) also focuses on comparisons of y_{ist} rather than of the treatment (union) and control (nonunion) groups, because doing so would require eliminating 2009 and 2017 from the samples, which would prevent me from including a sufficient number of lags and leads. Figure 4 nonetheless plots these estimated coefficients for both outcome variables. In both cases, there is a rise in outcomes for $t = 0$ with a subsequent increase in $t + 1$, although for life satisfaction the $t + 2$ coefficient is close to 0. Moreover, these results are consistent with the lack of pretrends in employment and establishment growth (see Online Appendix OB).

¹⁸ The list of policies is available from the author on request.

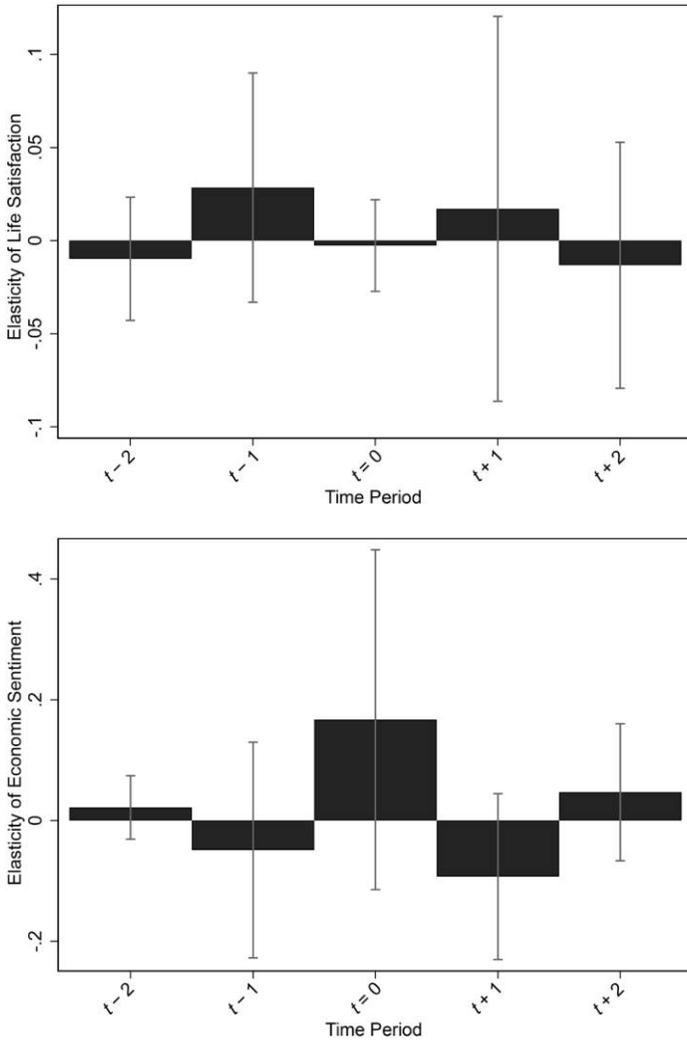


Figure 4. Potential pretrends in states adopting right-to-work laws

To further guarantee that states that adopted RTW laws are not on systematically different trends, I implement a variant of the balancing test that has desirable econometric properties (Pei, Pischke, and Schwandt 2019). I use state employment growth as a proxy for a potential confounder since the concern is that states adopting RTW laws are positively selected; that is, they are growing more than their counterparts. I subsequently regress this on an indicator for a state having RTW laws. While the unconditional correlation implies that states with RTW laws have .46-percentage-point higher employment growth ($p = .050$) as in

Figure 3, once demographics are introduced as controls, the conditional correlation becomes statistically insignificant at conventional levels ($p = .123$). In other words, observed demographic covariates appear to be a proxy for any potential differential trends that could be a threat to identification.

Is the lack of evidence of pretrends a function of not having enough power? Although that concern is inconsistent with the fact that the baseline results are precisely estimated, I now implement an additional border-pair exercise that compares subjective well-being among individuals who reside in counties on different sides of state borders, that is, between RTW and non-RTW states (see Holmes [1998] for an early application).¹⁹ Similar to recent methodological contributions that have exploited variation in state borders to understand the effects of labor market regulation (for example, minimum wages in Dube, Lester, and Reich [2010]), the intuition behind the comparison is that individuals in counties on different sides of a state border are unlikely to differ in unobservable ways and, therefore, are more comparable. The challenges are that the treatment effect is much more local, and the sample size declines significantly. Fortunately, however, differences—at least among observed demographic characteristics—are minor. For example, the average worker is 47.31 years old on a state border but 47.64 years old in other areas; 48.85 percent of individuals are male on a state border, but 48.66 percent are male in other areas; and 30.39 percent of individuals have a college degree on a state border, but 30.43 percent have a college degree in other areas.

Figure 5 plots the counties along borders between states with and without RTW laws; there are 438 such counties contained in the sample, and 364 of them have at least 100 respondents in them. I regress the usual measures of subjective well-being on the indicator for RTW laws, controlling for not only individual characteristics but also the border-pair, county, and time fixed effects. Table 5 documents the results. Consistent with the main results, adoption of RTW laws is associated with a .025 and a .040 SD rise in current life satisfaction and economic sentiment, respectively, although the latter is not statistically significant at conventional levels, and the former is significant only at the 10 percent level. One reason for the larger standard error emerges from the smaller sample, especially along only state borders in a given year.

¹⁹Unfortunately, Gallup asks only for the location of the residence, not where the individual works. It is, therefore, possible that an individual commutes across the border to work. While this may generate an attenuation bias only in the treatment effect, one way to gauge the scope of the concern involves drawing on census data on the average commute time and share of people who live and work in the same metropolitan and micropolitan area. I regress both of these outcome variables on an indicator for whether an individual's residence is in a county on a state border and find no statistically or economically meaningful associations. Although this is only a proxy for individuals in the data, it suggests that the individuals along a state border are not systematically more likely to have long commutes across state borders.

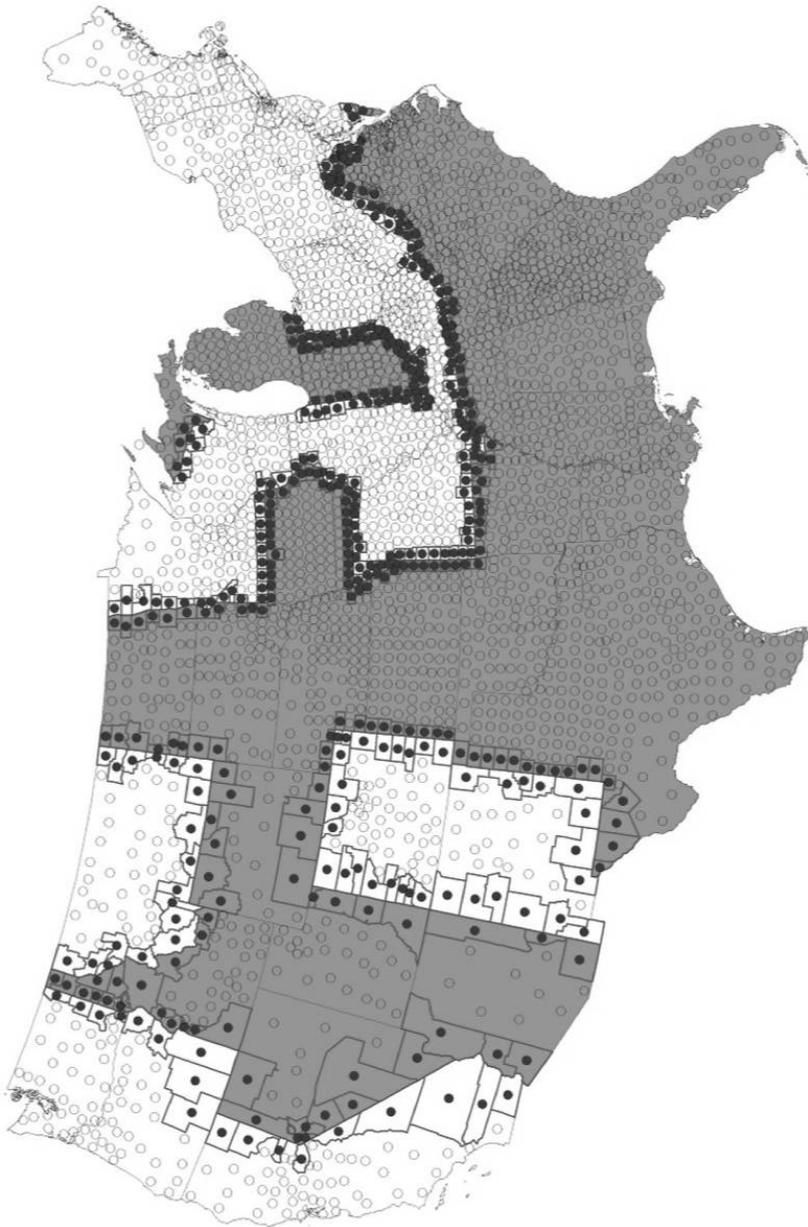


Figure 5. Counties along state borders with right-to-work laws

Table 5
Robustness Using a Border-Pair Approach
to Identification: 2008–17 Gallup Data

	Current Life Satisfaction	Future Life Satisfaction	Economic Sentiment
RTW Law	.025 ⁺ (.014)	.027* (.010)	.040 (.040)
R ²	.08	.15	.13
N	316,711	303,603	221,304

Note. Coefficients are from regressions of individuals' standardized (*z*-score) current and expected future life satisfaction (0–10 scale) and economic sentiment (2–7 scale) on indicators for whether the state has right-to-work (RTW) laws and controls. The sample is restricted to counties that fall along a border of a state with an RTW law and one without. County, time, and county pair fixed effects are included; county pairs can include, for example, three counties that neighbor each other. Controls include education fixed effects (no high school, high school or technical school, or college; some college is the omitted group), gender, marital status, number of children, and race (white). Standard errors are clustered at the state level, and sample weights are used.

⁺ $p < .10$.

* $p < .05$.

5. Understanding the Mechanisms

The main result—that union workers experience an increase in their subjective well-being and economic sentiment after the adoption of RTW laws—may appear puzzling in light of the conventional wisdom that unions provide a way for employees to voice grievances (Freeman 1976, 1980; Freeman and Kleiner 1990). In particular, since RTW laws are associated with declines in union density (Warren and Strauss 1979; Hirsch 1980; Ellwood and Fine 1987), then one may expect RTW laws to adversely affect union workers. To understand the rationale behind the main results, this section examines the evidence behind the potential mechanisms outlined in Section 2. Although there is no conclusive evidence for one mechanism over another, an examination of the microdata allows me to rule out some and provide evidence consistent with another.

5.1. Income Effects and Free Riding

One possibility is that the main results reflect an income effect. If union workers receive the benefits but do not pay dues, then the boost in disposable income could raise well-being through, for example, a consumption channel. Indeed, the prospect of free riding was the main contention that the Supreme Court ruled out in 2018 in *Janus v. American Federation of State, County, and Municipal Employees*.

The Gallup microdata contain several pieces of information that help uncover the scope for income effects. First, since individuals are surveyed on daily consumption of nondurables between 2008 and 2014, I can conduct both a coefficient

comparison and a balancing test along the lines of Pei, Pischke, and Schwandt (2019).²⁰ For example, introducing consumption as a control in equation (1) produces a gradient of .0406 ($p = .019$) on RTW status, which is slightly larger than the baseline result of .0302 ($p = .00$) on the sample from 2008 to 2014. Similarly, replacing sentiment with logged consumption expenditures in equation (1) produces a gradient of $-.0426$ ($p = .025$) on RTW status. This suggests that any potential correlation between consumption expenditures and the adoption of RTW laws runs in the opposite direction required by an income-effects story.²¹

Second, I can bound the magnitude of potential income effects. Consider the following two empirical regularities. First, according to Deaton (2008, p. 57), “[E]ach doubling of GDP is associated with a constant increase in life satisfaction.” Second, average union dues are roughly \$400 per year. Since average household income was \$73,298 in 2014 according to the census, an additional \$400 amounts to an increase of .55 percent per year. Moreover, replacing the outcome variable in equation (1) with the level of life satisfaction (on the 0–10 scale) produces a gradient of .057 ($p = .00$) on RTW status. This means that income would need to rise by over \$4,000 per year to account for the baseline effect, which is over 10 times as large as the average worker’s union dues. While these diagnostics are not necessarily inconsistent with prior evidence on the potential for free riding (Zax and Ichniowski 1991; Ichniowski and Zax 1991), they suggest that income effects cannot account for the main result.

5.2. Composition Effects

A second possibility is that the change in reported life satisfaction simply reflects a change in the composition of workers who remain in unions following the adoption of RTW laws. For example, if those who remain in unions following the adoption of RTW laws also report higher levels of life satisfaction, then the exodus of the more marginal union workers may drive the overall treatment effect. While the Gallup data are only a repeated cross section, and thus do not allow me to track the same worker before and after the adoption of an RTW law, there are at least two ways to home in on the potential selection effects.

First, using the PSID data for 1970–2016 (see Blundell, Pistaferri, and Preston [2008] for a summary of the data), I can examine how unobserved productivity among union workers correlates with the adoption of RTW laws. To proxy for unobserved productivity, I take the residual from a regression of logged labor income on person and year fixed effects, together with other time-varying individual covariates such as experience and age. I subsequently regress this measure

²⁰ See Makridis (2018b) for validation of the Gallup consumption data with the Bureau of Economic Analysis consumption data.

²¹ How to treat respondents who report no consumption expenditures is an open question. The aforementioned results are based on generating logged consumption by taking $\log(c + 1)$. If, however, logged consumption is simply defined as $\log(c)$, then there is no statistical association between the adoption of RTW laws and consumption, nor is there a change in the coefficient on RTW status when consumption is included as a control.

of unobserved productivity on an indicator for RTW status, union membership, and their interaction, obtaining a slightly positive, but statistically insignificant, gradient of .014 ($p = .21$) on the interaction. Since unobserved productivity is positively correlated with reported well-being, this suggests that composition effects are unlikely to account for the main result since there is no statistically significant evidence of positive selection.

Second, given an elasticity between RTW laws and union density, another bounding exercise can be undertaken to understand how large composition bias would need to be to account for the observed rise in life satisfaction following the adoption of RTW laws among union workers. The Gallup data indicate that, between 2008 and 2017, 6.87 percent of workers in an RTW state and 15.56 percent of workers in a non-RTW state are in a union. Suppose that workers who stay in the union are similar to union workers with .021 SD higher life satisfaction and those who leave are similar to the nonunion workers. Then, since the adoption of RTW laws leads to a decrease of approximately half in the union share, composition effects can account for at most .01 SD of the overall effect. In this sense, even in the most generous case in which the elasticity is identified off the cross section, composition effects would account for only 50 percent of the treatment effect.

5.3. Competition

A third possibility is that RTW laws encourage unions to become more competitive in the services that they offer their members. Absent RTW laws, unions are guaranteed a steady income stream from union dues since workers do not have a choice about whether to pay. This guaranteed income stream, in turn, may reduce the incentive for unions to provide value-added services to its members (for example, improvements in workplace practices). However, greater competition following the adoption of RTW laws may encourage more thoughtful collaboration between employers and union leaders.

While union investments and managerial changes are not directly observed, the Gallup polls contain several survey questions that provide information about workplace outcomes. Three survey questions, coded as binary variables, are relevant: whether their boss creates an open and trusting work environment, whether their boss treats them like a partner, and whether they get to use their strengths at work. Using these as outcome variables in a logit regression, conditional on state and time fixed effects, together with the usual individual covariates, I find that the adoption of RTW laws in a state is associated with a 1-percentage-point rise in the probability that an individual reports that he or she feels like the boss treats them as a partner and a .7-percentage-point rise in the probability that the boss creates an open and trusting work environment. To put this in perspective, approximately 36 percent and 20 percent of individuals report that their boss does not treat them like a partner and does not create a trusting work environment, which means that a 1 percent increase in the probability is nontrivial, especially in light of the share of union workers in the labor force.

Additional diagnostics suggest that these perceptions take roughly a year to set in. Although management practices tend to be sticky (Bloom et al. 2015b), which means that they can take years to adjust, my outcome variable is not a measure of physical investment or organizational prices but rather perception. In this sense, as long as unions signal, and follow the signals with some credible investments, then attitudes among workers should adjust fairly rapidly. Moreover, my results are consistent with not only early literature that finds that RTW laws do not lead to increased labor law violations (Elliott and Huffman 1984) but also more recent empirical work on the important role of competition and workplace practices. First, Schmitz (2005) examines the increase in competition for iron ore in Chicago and finds a significant increase in productivity moderated by workplace practices. Second, Bloom et al. (2019) compare 5,143 plants that are 50 km from state borders that vary in their adoption of RTW laws and find that plants in an RTW state have .014–.027 SD higher management practice scores. In both cases, an increase in competition reduces rent sharing by either employers or employees.

Although neither piece of evidence is a silver bullet, I examine two additional diagnostics to test the view that competitive forces are at play. First, I estimate equation (2) separately for private- and public-sector workers. When the outcome variable is current life satisfaction, I find that the passage of RTW laws has a slightly larger effect on union workers among private-sector workers, which is equal to .0273 ($p = .04$), than among public-sector workers, which is equal to .0198 ($p = .226$). Given differences in the degree of flexibility that unionized organizations in the public versus private sector retain, examining this dimension of heterogeneity is not only uniquely interesting but also comforting since the results are consistent with theory. Second, using data from the ATUS for 2003–17, Online Appendix OC shows that the adoption of RTW laws is associated with a 10–16-minute per day increase in time allocated to work activities. To the extent that RTW laws improve workplace practices and the disutility of labor supply declines, employees will be willing to allocate more time to work (Makridis 2018a).

5.4. *Correlated Probusiness Policies*

A fourth possibility is that RTW laws are simply passed along with other probusiness policies that positively affect economic activity and well-being. Moreover, a related possibility is that, since RTW laws lead to declines in union density, and unions are costly for firms (Chen, Kacperczyk, and Ortiz-Molina 2011; Lee and Mas 2012) and reduce firm investment (Fallick and Hassett 1999), then RTW laws could indirectly increase economic activity and, in turn, well-being.

While the fact that the main results are concentrated among union workers is counterfactual to this prediction, since probusiness policies should benefit non-union workers more, consider the following diagnostics (see Online Appendix OB for details). First, if probusiness policies are passed together with RTW laws, then there should be some expectation among businesses manifested by higher employment or establishment growth leading up to the passage of the laws. How-

ever, using panel data on counties for 1990–2016 from County Business Patterns, I find no evidence of pretrends in either employment or establishment growth. Second, although RTW laws tend to be passed at a state level, Kentucky is unique in that eight of its counties passed their own variants. Using county-level variation in the Gallup sample, I replicate the main result, which rules out the concern that other state-level policies coincide with the adoption of state RTW laws. Finally, an additional qualitative review of legislation passed during the adoption of RTW laws in states between 2008 and 2017 finds little evidence of policies that could otherwise account for these patterns in well-being.

5.5. Discussion

What do these results imply about the future of unions and labor market institutions in the United States? At least in the 2008–17 Gallup sample, unions are associated with worse workplace practices, whereas RTW laws are associated with improvements. One reason for this may be the fact that unions were unable to deliver on their promise of high wages and job security during the financial crisis, which saw a decline in routine employment (Jaimovich and Siu, forthcoming) and a rise in greater skill requirements, or “upskilling” (Hershbein and Kahn 2016), and automation (Autor and Dorn 2013). In fact, fixed-wage jobs exhibited much higher employment volatility than their counterparts in jobs with performance-related pay (Makridis and Gittleman 2018). Performance-related-pay jobs also offer more opportunity for career advancement and human capital accumulation (Makridis 2019), and workers in those jobs report higher levels of engagement and corporate culture (Makridis 2018a). While these descriptive pieces of evidence are not causal, they suggest that, at least as currently designed, unions might be increasingly unable or ineffective to fulfill their stated objectives.

6. Conclusion

There has been a fundamental transformation in the way employees and employers contract with one another in the labor market over the past 4 decades as performance-related pay has become increasingly common (Lemieux, MacLeod, and Parent 2009) and union density has declined (Hirsch 2012). Although there is unambiguous evidence that these moves toward stronger incentives are associated with improvements in productivity (Paarsch and Shearer 1999, 2000; Lazear 2000; Shearer 2004; Bandiera, Barankay, and Rasul 2005) and human capital formation (Shaw and Lazear 2008; Makridis 2019) among employees and greater flexibility among firms (Makridis and Gittleman 2018), it remains an open question whether these changes have also positively affected the well-being of workers or if the gains in productivity have simply gone toward firms. The answer will play a major role in determining the optimal policy response to increasing automation and technological adoption among firms in the emerging gig economy.

This paper provides the first evidence, to my knowledge, that the adoption of RTW laws has increased individual well-being and economic optimism, even af-

ter controlling for a wide array of time-varying state and individual factors and time-invariant differences across location and time. Using microdata collected by Gallup's US daily poll between 2008 and 2017, I show that the adoption of RTW laws increases current and future expected life satisfaction and economic optimism. Using a DD estimator and entropy reweighting, I find that these gains are concentrated among union workers, which suggests that those who have benefited most are precisely those whom the legislation targeted. The results are robust to controlling for a wide array of time-varying state characteristics, including contemporaneous and lagged employment growth, to testing for the presence of pretrends among states adopting RTW laws, and to comparing individuals on the borders of states with and without RTW laws.

These results affect how one thinks about the role of unions in the modern economy. Dating back to early work such as Freeman (1976, 1980) that formalizes a hypothesis in Hirschman (1970), the conventional wisdom was that unions provide employees a way of expressing their voice without requiring them to exit from undesirable employment situations. While the data do not enable me to unambiguously pinpoint the mechanism behind the main effects, they allow me to rule out several potential mechanisms, including income effects arising from not having to pay union dues, composition effects arising from the change in union membership, and contemporaneous probusiness policies accompanying the passage of RTW laws. However, I find some evidence that the passage of RTW laws is associated with improvements in workplace practices and that the main effects are concentrated in the private sector. Although not conclusive, these results are consistent with the view that RTW laws increase competition and, in turn, encourage unions to provide greater value to their members. Further research is needed to see whether these patterns in well-being continue among union workers in RTW states in years to come.

This paper opens several routes for additional inquiry. First, how do unions and/or labor market regulations affect the returns that firms face to offer non-wage amenities, such as benefits and corporate culture? To the extent that firms pay unionized employees above the market wage, they have less money to spend on other areas—in particular, developing employees' human capital. My results suggest that RTW laws grant employees greater autonomy and increase the employee-employer surplus that is on the table for negotiation. Second, given the result that RTW laws increase individuals' well-being, how can unions be improved or transformed so that they grant employees autonomy and ownership and simultaneously help and support them in reasonable ways? For example, Blasi, Freeman, and Kruse (2013) argue that employee ownership and profit sharing are possible approaches for improving and transforming unions. More work is needed to understand how labor market institutions in the 21st century should deal with major technological disruption, like automation, and the changing nature of work, like the gig economy.

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