Do Right-to-Work Laws Work? Evidence from Individual Well-being and Economic Sentiment

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For Review

Abstract

Over a majority of states have adopted right-to-work (RTW) laws. Using licensed micro-data from Gallup between 2008-2017 and within-state variation, the adoption of RTW laws is associated with systematic increases in life satisfaction and economic sentiment. The results are robust to: (i) difference-in-differences comparing union / non-union workers before / after RTW adoption, (ii) re-weighting states using entropy balancing, (iii) controlling for time-varying state confounders coinciding with RTW adoption, and (iv) comparing individuals on opposite sides of state borders. Contrary to conventional wisdom, RTW laws raise employee well-being and sentiment by improving workplace conditions and culture.

Keywords: right-to-work, sentiment, union, well-being, welfare.

JEL: I31, J28, J38, J53.

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1 Introduction

The share of states that have adopted right-to-work laws has grown from 20% in 1960 to nearly 60% in 2017 (Figure 1). These laws prohibit union security agreements between companies and unions, meaning that employees in unionized workplaces cannot be required to pay or participate in union representation, although they may still receive the benefits that paying members receive.[1]

[INSERT FIGURE 1 HERE]

While there is growing evidence that RTW laws have a positive effect on employment and wages, there is much more controversy about how they affect employee well-being and economic sentiment, especially among the individuals who are most likely to be affected by the laws.[2] These laws have been subject to significant scrutiny and criticism, particularly from labor unions. For example, the AFL-CIO 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."[3] The primary purpose of this paper is to examine whether these claims are true—what are the causal effects of these laws on measures of individual well-being and economic sentiment?

Unfortunately, empirical progress has been challenging for at least two reasons. First, comprehensive data on subjective well-being over a sufficiently long time series is difficult to find. Second, states with RTW laws vary in potentially many unobserved ways, making comparisons between individuals in different states subject to standard selection concerns. Furthermore, states might adopt RTW laws because they are on an upward economic trend trying to attract more business. This paper overcomes these empirical challenges by leveraging micro-data from Gallup's U.S. Daily Poll, which surveys 1,000 individuals per day over well-being topic (e.g., life satisfaction) and economic sentiment about both current and future economic activity, containing many demographic characteristics too. Gallup's infrastructure and specialization in survey methodology make them uniquely capable at implementing large surveys with comparable questions over time.

The baseline empirical specification compares outcomes among observationally equivalent individuals before versus after the adoption of RTW laws. Under the preferred specification, I find that adoption of RTW laws is associated with a 0.029 standard deviation increase in current life

[1] https://www.nlrb.gov/rights-we-protect/employerunion-rights-and-obligations

[2] Although there has been some mixed evidence from cross-sectional studies (Wessels, 1981; Garofalo and Malhotra, 1992), which are reviewed by Moore and Newman (1985), more recent applications that leverage plausibly exogenous variation have found positive effects of these laws. These are discussed shortly.

[3] https://aflcio.org/issues/right-work

satisfaction, a 0.014 increase in expected future life satisfaction, and a 0.054 increase in economic sentiment about current and future economic activity. As expected, these estimated marginal effects are lower in magnitude than the conditional correlations, reflecting the fact that states with RTW laws are positively selected. I also provide suggestive evidence that the channel through which RTW laws raise well-being and sentiment is through improved employee-employer relations. In particular, using the same source of within-state variation, adoption of RTW laws is associated with a 4.2% and 4% increase in the probability that employees report that their boss is treating them well and creating an open and trusting work environment, respectively.

To guarantee that these results are not driven by other time-varying unobserved factors, I implement several additional exercises. First, using a difference-in-difference estimator, combined with entropy re-weighting, I find that the adoption of RTW laws is associated with a 0.02 and 0.11 standard deviation increase in current life satisfaction and economic sentiment, respectively.[4] In fact, these results suggest that the bulk of the increase in well-being and economic sentiment is concentrated among the policy-relevant group of interest: union workers. Second, I control for a wide array of time-varying state characteristics, including contemporaneous and lagged employment growth. These results suggest that it is unlikely that state-specific trends are simultaneously correlated with the passage of RTW laws and improvements in well-being. Third, I validate the parallel trends assumption by showing that states adopting RTW laws do not display any significant differences in well-being or sentiment leading up to the passage of these laws. Fourth, I restrict the sample to individuals in counties on the border of states with and without RTW laws—individuals who are unlikely to vary in many unobserved ways—and find similar results.[5]

While this is the first study to formally examine how measures of individual well-being respond to RTW laws, this paper is most closely connected with a literature on their effects on union formation and employment and/or wages. While there has been some mixed evidence that RTW laws are mainly symbolic and do not actually reduce union density (Lumsden and Petersen, 1975; Farber, 1984), most studies have found a large negative effect (Warren and Strauss, 1979; Hirsch, 1980) that subsequently decays over time (Ellwood and Fine, 1987). There is also a large literature

[4]Related to the approach with synthetic control matching estimators (Abadie et al., 2010), entropy re-weighting as introduced by Hainmueller (2012) creates balancing across a set of measured variables between the treated and control units. I use monthly state employment growth, age, college attainment, and race as the variables to balance over between treated and control states, proxying for differences in economic trajectory, human capital, and demographics.

[5]These estimates are also robust to comparing individuals in zipcodes along different sides of the border, but doing so restricts the sample further since Gallup does not have complete coverage of every zipcode, whereas it does cover nearly every county.

that has found a positive effect of RTW laws on employment and wages and have argued that controlling for unobserved location-specific factors is important for recovering causal effects (Reed, 2003; Kalenkoski and Lacombe, 2006). These laws are more broadly representative of pro-business policies that are associated with increases in the manufacturing employment share (Holmes, 1998). A recurring challenge in this literature, however, is that it is difficult to exploit sources of exogenous variation, prompting the application of synthetic control methods (Eren and Ozbeklik, 2016). It is nonetheless important to view these results through the lens of country-specific labor market institutions, which may vary significantly across countries (e.g., see Yao and Zhong (2013)).

The results also relate directly with a literature on the causal effects of unions on both workers and firms. On the firm side, Klasa et al. (2009) and Matsa (2010) both 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% of the average firm market value), which is consistent with early evidence that unionized companies had roughly 10% lower market values and earnings between 1972 and 1980 compared to their counterparts (Hirsch, 1991a).[6] On the employee side, Freeman (1976) and Freeman and Kleiner (1990) have shown that unions allow employees to express a collective voice (e.g., voicing grievances), in addition to earning higher wages for comparable non-union jobs (Freeman, 1982; Card, 1996).

The fact that RTW laws are negatively associated with well-being and economic sentiment might appear puzzling given historical evidence that unions create a platform for collective voice (e.g., voicing grievances and improving workplace conditions) (Freeman, 1976; Freeman and Kleiner, 1990). However, while the perception may have once been that unions were a vehicle for creating voice and maintaining job security, they may be either unable and/or less effective at delivering these aims in today's economy. For example, Makridis and Gittleman (2017) show that fixed wage jobs, which are roughly 28% more likely to be unionized, exhibited much more employment volatility over the financial crisis, consistent with an early point from Friedman (1950). In this sense, even though Freeman and Rogers (2006) find that 63% of employees said they wanted more influence than they had over decisions are their workplace, unions might simply be ineffective or unable to provide such autonomy. Indeed, employees are flocking towards performance pay jobs, which contain greater opportunities for advancement and human capital accumulation (Makridis,

[6]Lee and Mas (2012) also provide a clever reconciliation with event-study earlier results from DiNardo and Lee (2004) that unions had only small effects by showing that the declines in firm value on take place roughly two years after a company becomes unionized.

2017b). My results, more broadly, contribute to a larger literature on the effects of state labor market regulations, such as the minimum wage, occupational licensing, non-compete enforcement agreements, and wrongful discharge laws, on economic outcomes, which tend to use a similar methodological approach to isolating the causal effects.[7]

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 results, containing the identification strategy, results, and robustness. Section 5 concludes.

2 Background and Theoretical Framework

Starting in 1935, the National Labor Relations Act (NLRA) made it possible for private sector workers to unionize and enter collective bargaining agreements, requiring 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 right-to-work (RTW) laws. Since then, 28 states have enacted RTW laws (Figure 1) despite significant push back from unions.

Before examining how RTW laws might affect individual well-being, it is useful to put the discussion in context of a broader literature about the effects of unions on employees and firms. While there is evidence that union workers earn a premium compared to their counterparts in low skill jobs (Freeman, 1982; Card, 1996), unions may limit the returns to human capital accumulation by muting the incentive to invest in new skills and allocate more time to work. For example, performance pay workers—even within comparable jobs in the manufacturing sector—not only earn more, but also learn more and exhibit much more dynamic career paths (Makridis, 2017b). To the extent that human capital is a function of time allocated to the job as in Shaw (1989), Imai and Keane (2004), or Makridis (2017b), then employees covered by these contracts will learn less since unions often impose limits on the amount of time employees can work.

On top of the incentive effects of unions on employee time use, unions can also impose costs on firms. For example, Lee and Mas (2012) use an event study to show that the equity value of firms

[7]Since each of these sub-literatures is too large to merit its 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 and Balasubramanian (forthcoming) about non-compete contracts, and Autor et al. (2006) about wrongful discharge laws.

declines by roughly $40,500 per worker between 15-18 months after becoming unionized. Similarly, Hirsch (1991b) finds that market value and earnings are 10-15% lower in the average unionized company. Klasa et al. (2009) and Matsa (2010) also show that unionized firms strategically hold less cash and take on more debt to gain greater bargaining power when negotiating with the unions. Chen et al. (2011) find that the cost of equity (debt) is greater in higher (lower) unionized industries. These labor market distortions and rigidities imposed by unions can account for much of the dispersion in unemployment in Europe (Ljungqvist and Sargent, 1998, 2008).

The literature on RTW laws has generally focused on two main sets of outcomes: union activity and employment and/or 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), there has been more mixed evidence on their effects on employment and wages.[8] For example, using RTW laws as a proxy for pro-business policy and county variation on different sides of state borders, Holmes (1998) finds that states with these laws have much higher levels and growth in manufacturing activity. Similarly, Reed (2003) and Kalenkoski and Lacombe (2006) both find that RTW 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) did not find any significant effects on employment or wages.

With these in mind, RTW laws can affect individual well-being in two ways. First, in the presence of compensating differentials (Rosen, 1986), which account for roughly 2/3 of the dispersion in wages (Sorkin, 2017), then firms that are required to pay higher wages because of unions necessarily will provide fewer non-wage amenities and/or development and training opportunities since these amenities are costly to provide. This follows for not only the obvious reason that non-wage amenities are costly to provide, so if a firm is already paying above-market wages, they have less margin for other expenditures, but also an indirect reason. In particular, since firms provide non-wage benefits (Liu et al., 2017) and invest in corporate culture (Makridis, 2016) in part to raise employee engagement and the time employees allocate to work activities, then these upsides are weaker in a world where unions limit the amount of time employees can work.

Second, since unions impose greater costs on firms, they are especially likely to be displaced through automation as companies look for ways to save on labor costs (Autor, 2015). Although

[8]Some early studies argued that the passage of these laws was more symbolic than actually causal of any real declines in union activity (Lumsden and Petersen, 1975; Farber, 1984).

there are many reasons for the decline in the employment share of manufacturing jobs, including the rise of China (Autor et al., 2013), one main reason is the ease of automating many of the routine jobs that were typically held by union workers (Autor et al., 2003). In this sense, if employees care about their future career and human capital is at least partially occupation-specific, then better employees will want to work in jobs that provide upward mobility and a career path. For example, Makridis (2016) shows that a standard deviation increase in the quality of development and training opportunities is associated with a 0.18 standard deviation increase in job satisfaction, even after controlling for other dimensions of corporate culture.

3 Data and Measurement

Gallup Daily Polling Repeated Cross-section.—The primary source consists of newly licensed data with Gallup Inc. Gallup is the United States' premier polling service and conducts daily surveys of 1,000 U.S. adults on various political, economic, and well-being topics. Specifically, 200 Gallup interviewers conduct computer-assisted telephone interviewers 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, is also available with corresponding sample weights. These data have been used recently 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, by Makridis (2017a) to study the effects of economic sentiment on real activity, and by Makridis and Ohlrogge (2017) to study the effects of mortgage foreclosures and housing prices on well-being.

Gallup's polling relies on live (not automated) interviews with dual-frame sampling (including random-digit-dial [RDD]) landline and wireless phone sampling. Half of the respondents receive the "well-being track" version (with a 9% survey response) of the survey questions, whereas the other half receives the "politics and economy track" (with a 12% survey response). The two surveys contain different topical questions, but both contain the same identifying demographic information. Gallup also conducts the survey in Spanish to record replies from those Spanish speakers who do not also speak English. The sampling methodology also uses a three-call design to reach respondents who do not pick up on the original attempt. The survey has changed in some dimensions since 2008 with the inclusion of detailed well-being related indices since 2014, but the main outcomes of the analysis are available throughout. The primary measures of well-being are based on a one-to-ten scale of perceived current and expected future life satisfaction from

the Gallup-Sharecare Well Being Index, and the measure of economic sentiment is the sum of a one-to-four index about the respondent's perception of the current state of the economy and a one-to-three index about their perception about the future state of the economy. Table 1 documents the survey questions with their wording in the online survey.

[INSERT TABLE 1 HERE]

State right-to-work laws.—These micro-data were matched with indicators for whether a state has passed RTW laws. These are made available through the National Conference of State Legislatures (NCSL), among other outlets (http://www.ncsl.org/research/labor-and-employment/right-to-work-laws-and-bills.aspx). Figure 2 documents the states with RTW laws as of 2017. Figure 3 plots the distribution of six state-level variables between 2008-2016 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 0.0099 percentage points (pp), whereas non-RTW states have a growth rate of 0.056pp. They also have an employment growth rate of 0.0088 compared to their counterparts with 0.0048 (similarly for their unemployment rates), as well as a larger manufacturing share, consistent with Holmes (1998). However, states with RTW laws tend to have a lower share of residents with a college degree—26% in comparison with their counterparts with 31%.

[INSERT FIGURE 2 HERE]

[INSERT FIGURE 3 HERE]

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 consider fixed effect regressions of the form

$$y_{ist} = \gamma RTW_{st} + \beta X_{it} + \eta_s + \lambda_t + \epsilon_{ist} \tag{1}$$

where y denotes the individual outcome, RTW denotes an indicator for whether the state has a RTW law, X denotes a vector of individual covariates, and η and λ denote state and year fixed effects. Standard errors in Equation 1 are clustered at the state-level to allow for arbitrary degrees of autocorrelation within the same location over time (Bertrand et al., 2004).

Individual covariates include fixed effects on the highest degree earned, gender, age, and race. I have also experimented with party affiliation controls to control for the potentially symbolic nature of RTW laws—although the state fixed effects should absorb these—but omit these party affiliation fixed effects from the main results since they are not measured in the entire sample. The inclusion of state and time fixed effects also remove time-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 pro-business ways that also attract more economic opportunity. In this sense, Equation 1 identifies the causal effect of RTW laws based on within-state comparisons between observationally equivalent workers before versus after the adoption of RTW laws. Although there are 28 states that have RTW laws, only six states adopted them within the 2008 to 2017 sample period: Michigan and Indiana (2012), Wisconsin (2015), West Virginia (2016), and Missouri and Kentucky (2017).

An additional formulation of Equation 1 that focuses more specifically on the individuals who are directly affected by these laws involves a simple difference-in-difference (DD) estimator that compares the outcomes of union workers with their counterparts before versus after the adoption of RTW laws in the same state

$$y_{ist} = \gamma RTW_{st} + \xi u_{it} + \zeta(RTW_{st} \times u_{it}) + \beta X_{it} + \eta_s + \lambda_t + \epsilon_{ist} \tag{2}$$

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

One limitation with the DD implementation is that the union indicator is only available between 2009-2016, which reduces the sample size and identifying variation since both Missouri and Kentucky adopted RTW laws 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 re-weighting based on state monthly employment growth and individual age, college attainment and race.[9] While this approach potentially "over

[9]I implement the approach using a package developed by Hainmueller and Xu (2013). The method improves upon the classic synthetic control approach in Abadie (2005) and Abadie et al. (2010). The entropy balancing approach works by constructing weights such that the covariate distributions of the control group in the pre-processed data match on all the pre-specified moments (I use only the first moment). The approach has several features over conventional matching methods. First, it allows for matching on higher order moments—not just the

controls" since RTW laws have direct effects on employment, it allows me to purge any variation in economic activity that might also be driving variation in individual well-being.

4.2 Main Results

Table 2 documents the results associated with Equation 1. I focus on three outcomes: self-reported current and expected future life satisfaction, which individuals answer on a one to ten scale about their overall outlook on life, and the sum of perceptions about the current and future state of the economy, which individuals answer on a one to four and one to three scale (and thus the sum of seven), respectively. Beginning with the conditional correlations in columns 1, 3, and 5, I find that adoption of RTW laws is associated with a 0.037 standard deviation increase in current life satisfaction, a 0.033 increase in (expected) future life satisfaction, and 0.035 increase in perceptions about current and future economic activity. Turning towards the fixed effects estimates in columns 2, 4, and 6, I find that the adoption of RTW laws is associated with a 0.029 standard deviation increase in current life satisfaction, a 0.014 increase in (expected) future life satisfaction, and 0.054 increase in perceptions about current and future economic activity. While the estimates under both well-being outcomes are significant at the 1% and 5% level, respectively, the estimates for economic sentiment are not. However, their confidence intervals are still above zero.

Since the outcomes are all measured as an index, it might be hard to interpret whether the magnitudes are big or small at face value. However, comparing the coefficients to the marginal effects of estimated demographics provides a useful heuristic. Take, for example, the marginal effect of college attainment, which is normalized to the omitted group of those with only some college education. Column 1 suggests that college degree workers have a 0.270 higher standard deviation of life satisfaction, relative to their counterparts. It follows that the marginal effect on RTW laws is roughly 10.8% of the marginal effect of college attainment. In this sense, at least from a descriptive point of view, the marginal effect of RTW laws is clearly non-trivial in light of the fact that college attainment is associated with a wide array of wage and non-wage benefits.

[INSERT TABLE 2 HERE]

The fact that Table 2 points towards such strong, positive, and precise estimates on reported well-being is remarkable in light of the fact that the majority of individuals are not directly affected

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 is, therefore, a generalization of the propensity score weighting approach in Hirano et al. (2003).

by the adoption of RTW laws. For example, a professional services worker who is not in a union does not gain more freedom after the passage of these laws. While these results speak towards the important aggregate effects and approval of these laws among state residents, they also raise the concern that states adopting RTW laws 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 3 now documents the results associated with Equation 2 from the DD estimator (columns 1 and 3) and a semi-parametric re-weighted DD estimator related to Abadie (2005) (columns 2 and 4) . Results with two outcomes are presented: current life satisfaction and economic sentiment.[10] The standard DD estimator suggests that the adoption of RTW laws is associated with a 0.02 and 0.11 standard deviation increase in current life satisfaction and economic sentiment *among union workers*, which are significant at the 10% and 1% levels, respectively. The re-weighted DD estimator suggests that the adoption of RTW laws is associated with a 0.02 and 0.08 standard deviation rise in current life satisfaction and economic sentiment, significant at the 5% and 1% levels, respectively.

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 $\times$ 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 the RTW laws affect them directly. If, for example, a doctor were heavily affected, either the short-term general equilibrium effects would have to be large or it would indicate a potential omitted variable. Second, the estimates on economic sentiment are especially strong and large—roughly half of the magnitude on the marginal effect of college attainment in Table 2. These estimates, therefore, suggest that the passage of RTW laws fundamentally transforms the optimism that union workers have about their career prospects.

[INSERT TABLE 3 HERE]

[10]Unfortunately, since this requires reducing the extent of the variation in the sample by trimming off 2009 and 2017, there is much less variation to infer about future well-being. In this sense, although not reported formally, it is not surprising that I find a statistically insignificant estimate centered around zero for the interaction between union $\times$ RTW when the outcome is future life satisfaction.

4.3 Understanding the Mechanisms

Given that the stated purpose behind unions is improving worker well-being, the result that RTW laws (and the option not to participate in unions) actually lead to improvements in life satisfaction and economic optimism is perhaps surprising.[11] The theoretical motivation discussed earlier provides two main explanations that help understand these surprising empirical results.

First, even setting aside the fact that compensating employees for productivity is more costly and tougher to do when workers are in a union,[12] these firms also face weaker incentives to provide other workplace amenities that require organizational capital, such as corporate culture and purpose-driven work.[13] To the extent dispersion in labor income is explained in part by compensating differentials (Sorkin, 2017), then policies that impose greater costs on firms reduce firm profits and, therefore, the resources they have available. While a union contract might be attractive for some individuals, it might not be for others—for these latter workers, they may still decide to incur the disutility cost of participating in the union if they sufficiently weight other job-specific features (e.g., pay).[14] In this sense, providing employees the right to voluntarily opt in or out of a union is strictly better for workers than mandating that they join it.

To provide evidence on this channel, I turn towards longitudinal micro-data from the National Longitudinal Survey of Youth (NLSY) between 1979 and 2014 drawing on the 1979 and 1997 cohorts. The relevant sample contains full-time employees between 4,300 and 7,500 individuals observed at an approximately bi-annual frequency since information on job satisfaction and union membership is not available in every year. Job satisfaction is measured on a one-to-five scale and I normalize it to have a mean of zero and standard deviation of unity. I subsequently regress the z-score on an indicator for participating in a union, conditional on a wide array of controls,

[11] For example, AFL-CIO says that they work "tirelessly to improve the lives of working people" (https://aflcio.org/about-us).

[12] For example, using the fully panel of individuals from the National Longitudinal Survey of Youth between 1984 and 2016, union workers are 24% less likely to receive any type of performance pay compensation (Makridis, 2017b).

[13] While there are different theories for explaining variation in management practices and corporate culture, Dessein and Prat (2017) provide a unified theory that focuses on the role of organizational capital. Differences in the external environment (e.g., collective bargaining agreements that impose costs on firms) can affect the returns of different organizational practices.

[14] In the theoretical motivation, I used the example of increasing automation. If workers value development and training opportunities, and these are harder to provide to employees who are in a union, then rational workers might choose not to opt into a union if they place a sufficiently high weight on human capital accumulation (or sufficiently low weight on collective bargaining agreements). The taste for acquiring additional human capital might be especially pervasive in the presence of increasing automation.

including a quadratic in age, years of schooling, and indicators on being male, white, or black. I find that union workers have 5.4% lower self-reported job satisfaction (p-value = 0.00).[15] To proxy for potential differences in human capital between union and non-union workers, I also experiment with the inclusion of additional controls: tenure and experience (in years), which reduces the coefficient down to 5.2% (p-value = 0.00), and the hourly wage, which reduces the coefficient down to 4.8% (p-value = 0.00).

Given the richness of the Gallup micro-data, I delve into the these differences in job satisfaction. For example, I find that union workers report having 2.5% lower workplace purpose, which is an index constructed based on the respondent's answers to five questions.[16] Recent research in organizational economics, for example, has highlighted the importance of workplace culture (Edmans, 2012; Makridis, 2016) and, in particular, purpose (Gartenberg et al., 2016) as a primary driver of engagement and firm value. In this sense, organizational culture is not just an amenity that employees value, but also a determinant of organizational productivity, which translates into higher wages and development opportunities for employees. Leveraging information on financial and work-related concerns about the future, I also find that individuals in RTW states report a 0.021 (p-value = 0.402) lower standard deviation of worry about not having enough money to do what the respondent would like to do, a 0.048 (p-value = 0.001) lower standard deviation of worry about not having enough money to do what the respondent perceives as needing to do, and a 0.029 (p-value = 0.009) higher standard deviation of feeling active and productive.

Second, motivated by the fact that individuals place an inherent value choice (Leotti et al., 2010), providing individuals the right to choose whether to join a union or not can reinforce individual agency. Requirements to join a union might have especially adverse effects on employee engagement and optimism given the increasing employee preferences over managing their career and autonomy in the workplace. One potential reason for these increasing preferences could be the rise of Millennials in the workplace who place a large value on developing and training opportunities, especially relative to Gen Xers and Baby Boomers. For example, companion evidence in Makridis (2016) shows that a standard deviation increase in the perceived quality of development and training opportunities is associated with a 0.18 standard deviation in job satisfaction, even

[15] Unfortunately, the public micro-data files do not contain information about the individual's state of residence. If they did, I could easily check for whether differences in job satisfaction are greater in RTW states.

[16] These questions include: (i) "There is a leader in my life who makes me enthusiastic about the future", (ii) "I like what I do everyday", (iii) "In the past 12 months, I have reached most of my goals", "(Iv) I get to use my strengths to do what I do best every day", and (iv) "I learn or do something interesting every day."

after controlling for other non-wage amenities, such as managerial quality and pay transparency.[17]

To provide evidence on this channel, I draw on additional questions in the Gallup micro-data on workplace practices, including: (i) a question about whether their boss treats them like a partner, and (ii) a question about whether their boss creates an open and trusting work environment. The shares of individuals reporting a value equal to one on these are 64%, and 79%, respectively. After estimating logit regressions of these two indicators on state RTW laws, conditional on the usual individual controls and both state and year fixed effects, I find that the adoption of RTW laws leads to a 4% and 4.2% rise in the probability that the employee reports that their boss treats them like a partner and that the boss creates a trusting and open work environment. These coefficients are robust to controlling for individual income bins, suggesting that improvements in workplace practices are likely not due to income effects arising from not having to pay union dues.

Why do these results contrast with historical evidence about the role unions played in creating a collective voice and improved workplace conditions for employees (Freeman, 1976, 1980; Freeman and Kleiner, 1990)? The above evidence suggests that unions—at least over the 2008 to 2017 period—are associated with worse workplace conditions and lower well-being measured in a wide array of ways. One reason could come from the fact that unions were not able to deliver on their promise of high wages and job security during the financial crisis with the decline in routine employment (Jaimovich and Siu, 2014), and rise of upskilling (Hershbein and Kahn, 2016) and automation (Autor and Dorn, 2013). In fact, fixed wage jobs exhibited much higher employment volatility in comparison to their performance pay job counterparts (Makridis and Gittleman, 2017). Performance pay jobs also offer more opportunity for career advancement and human capital accumulation (Makridis, 2017b) and performance pay workers report higher levels of engagement and corporate culture (Makridis, 2016). While these descriptive pieces of evidence are not causal, they suggest that unions might be increasingly unable or ineffective at fulfilling their stated objectives. I later discuss the implications for labor market institutions.

[17]Recent survey evidence from Gallup further reports that nearly twice as many Millennials report that the opportunity to learn and grow is "extremely important" to them, relative to Baby Boomers and Gen Xers (https://hbr.org/2016/05/what-millennials-want-from-a-new-job and http://news.gallup.com/reports/189830/e.aspx). PwC has found similar survey results (https://www.pwc.com/m1/en/services/consulting/documents/millennials-at-work.pdf).

4.4 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 non-union workers in states that adopted RTW laws. In other words, the passage of RTW laws must not be correlated with pre-existing differences in either the treatment or control groups.

There are two primary concerns that threaten the assumption. First, states that passed RTW laws might have passed other policies that spuriously coincide with the improvements in well-being and economic sentiment. To address this concern, I introduce an array of time-varying state characteristics, including: state population growth, state employment growth and its lag, the age, education, and race distributions (bins on the share of individuals within different ranges). Second, states that passed RTW laws might have passed them for reasons that are correlated with pre-existing trends. To address this concern, I show that the parallel trends assumption holds.

Table 4 begins by documenting the first set of these diagnostics, which follows a similar form as Table 3 with the exception that it also contains a large vector of time-varying state controls. If, for example, states are trending for other reasons and/or other economic policies were implemented because of a growing economy (e.g., employment growth) or improvements in quality of life (e.g., population growth), then these controls should make it very unlikely for me to recover a meaningful effect on union $\times$ RTW. Not surprisingly, state employment growth and its lag are both significantly positively associated with current life satisfaction and economic sentiment. However, each of the interaction effects are almost identical to those from the main results from the DD estimates in Table 3. In this sense, violations to the exclusion restriction would have to be uncorrelated with all of these time-varying state economic and demographic characteristics, but somehow correlated with well-being and sentiment.

[INSERT TABLE 4 HERE]

To examine the potential for pre-trends, I now estimate regressions of the form

$$y_{ist} = \gamma_1 RTW_{s,t-2} + \gamma_2 RTW_{s,t-1} + \gamma_3 RTW_{s,t} + \gamma_4 RTW_{s,t+1} + \gamma_5 RTW_{s,t+2} + \beta X_{it} + \eta_s + \lambda_t + \epsilon_{ist} \quad (3)$$

where γ_1 and γ_2 will provide an indication of the potential presence of pre-trends and γ_4 and γ_5 will 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 only coming from Michigan and Indiana who both introduced their RTW laws in 2012 and from Wisconsin who introduced their law in 2015. The confidence intervals are, therefore, large and marginal effects will underestimate the true long-run effect of RTW laws on well-being. Equation 4 also focuses on comparisons of y_{ist}, rather than between the treatment (union) and control (non-union) groups, because doing so would require eliminating 2009 and 2017 from the samples, which 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 an immediate rise in outcomes for $t = 0$ with a subsequent increase in $t + 1$, although for life satisfaction the $t + 2$ coefficient is close to zero.

To further guarantee that states that have adopted RTW laws are not on systematically different trends, I implement a variant of the balancing test, which has desirable econometric properties (Pei et al., 2017). 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 0.46 percentage points higher employment growth (p-value = 0.050) as in Figure 3, once demographics are introduced as controls, the conditional correlation becomes statistically insignificant at conventional levels (p-value = 0.123). In other words, observed demographic covariates appear to proxy for any potential differential trends that could be a threat to identification.

As a final robustness exercise, I focus on individuals who reside in counties on state borders between RTW and non-RTW states as in Holmes (1998). Similar to recent methodological contributions that have exploited variation in state borders to understand the effects of labor market regulation (e.g., minimum wages Dube et al. (2010)), the intuition behind the comparison is that individuals on counties on different sides of the same state border are unlikely to differ in unobservable ways and, therefore, are more comparable. The challenge is 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 47.6 years old on a border zipcode, but 48.2 years old in other areas; 48.6% of individuals are male on a border zipcode, but 48.5% are male in other areas; and, 30.1% of individuals have a college degree on a border zipcode, but 30.5% have a college degree in other areas.

Figure 5 plots the counties that are on the borders between states with and without RTW

laws. I restrict the sample to counties where at least 200 individuals are observed in my database, although the results are not very sensitive to a slightly larger cutoff of 300 individuals, for example. The sample, therefore, contains 441 counties that lay on a border of a state with RTW laws with only one county that is excluded because of my sample size restriction (which would therefore make 442 counties). Unfortunately, the sample drops from 3,391,697 to 404,755 when restricting to border counties—and the variation is even smaller when exploiting within-state variation in the adoption of RTW laws. Nonetheless, when controlling for state and year fixed effects, I find that adoption of RTW laws is associated with a 0.049 (p-value = 0.088) and a 0.038 (p-value = 0.586) standard deviation increase in current life satisfaction and economic sentiment. Moreover, I have also experimented with using border zipcodes, which produces comparable gradients of 0.01 (p-value = 0.522) and a 0.093 (p-value = 0.01). However, in both cases the precision of the estimates is larger than the baseline results because of the decline in the sample size.

[INSERT FIGURE 5 HERE]

4.5 Implications for Regulation

Are these results about the positive effects of RTW laws applicable for a broader set of regulatory policies? While it is outside the scope of this paper to provide a definitive answer about regulation in general, especially since there is so much nuance about the type of regulation in question, I conduct a simple exercise that draws on RegData—a comprehensive database of regulation that goes up to a four-digit NAICS level. Developed by Al-Ubaydli and McLaughlin (2017), RegData draws on information in the Code of Federal Regulation (CFR). Al-Ubaydli and McLaughlin (2017) use text analysis search for five strings that are associated with regulations ("shall", "must", "may not", "prohibited", and "required) to identify regulatory restrictions for each sector × year.[18]

Since the number of regulations are measured across sector × year, I create a county × year measure by exploiting a county's pre-recession exposure to different industries through a Bartik-like estimator of the form

$$r_{ct} = \sum_{k} (e_{c,k,2005}/e_{c,2005}) \Delta r_{kt} \tag{4}$$

[18]RegData makes several innovations, relative to the literature thus far. For example, Mulligan and Shleifer (2005) developed a theory of regulation based on cross-sectional differences in state population and the length of regulations.Dawson and Seater (2013) developed a measure of regulation based on the page count of the CFR, estimating its effect on productivity and other measures of economic activity.

where $e_{c,k,2005}/e_{c,2005}$ denotes a county's employment share for industry k and Δr_{kt} denotes the growth in regulations between year t and $t-1$ in industry k. By fixing the employment share to a pre-recession level (i.e., 2005), Equation 4 does not bundle local demand shocks that may otherwise manifest themselves in the year-to-year employment shares (Goldsmith-Pinkham et al., 2017). I subsequently regress measures of life satisfaction and economic optimism on the location-specific regulatory measure from Equation 4, which allows me to recover a treatment effect of regulation on sentiment under the assumption that the pre-recession employment shares are plausibly exogenous with respect to contemporaneous shocks to individual sentiment data between 2008 and 2017.

Using the entropy balancing estimator as the baseline, I find a coefficient of -0.014 (p-value = 0.729) on the regulatory shock when the outcome variable is standardized life satisfaction and a coefficient of -0.589 (p-value = 0.00) on the regulatory shock when the outcome is standardized economic optimism. In other words, a 1pp rise in employment-weighted regulatory growth in a county is associated with a large 0.589pp decline in economic optimism, conditional on controls (including contemporaneous state employment and population growth and county unemployment rates). The fact that the regulatory shock is correlated with economic optimism, but not life satisfaction, suggests that broader regulation may affect well-being primarily by influencing beliefs about the economy (and thus business prospects), rather than directly through stated life satisfaction. These results are consistent with standard concerns that regulation might adversely affect individuals by reducing economic activity (Gong and Yannelis, 2018).

5 Conclusion

There has been a fundamental transformation in the way employees and employers contract with one another in the labor market over the past four decades: the incidence of performance pay compensation has grown from 15% of the labor force to 50% and the incidence of union contracting has declined from 30% to under 10% (Makridis, 2017b). Although there is unambiguous evidence that these moves towards stronger incentives are associated with improvements in productivity (Paarsch and Shearer, 1999; Lazear, 2000; Paarsch and Shearer, 2000; Shearer, 2004; Bandiera et al., 2005) and human capital formation (Shaw and Lazear, 2008; Makridis, 2017b) among employees and greater flexibility among firms (Makridis and Gittleman, 2017), an open question remains whether these changes have also positively affected the well-being of workers—or if the gains in productivity have simply gone towards firms. The answer to this question 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 right-to-work (RTW) laws has increased individual well-being and economic optimism, even after controlling for a wide array of time-varying state and individual factors and time-invariant differences across location and time. Using micro-data from Gallup's U.S. Daily Poll between 2008 and 2017, I show that the adoption of RTW laws raises current and future expected life satisfaction and economic optimism. Using a difference-in-difference estimator, together with entropy re-weighting, that these gains are concentrated among union workers, suggesting that those who have benefited most are precisely those whom the legislation targeted. The results are also robust to controlling for a wide array of time-varying state characteristics, including contemporaneous and lagged employment growth, and testing for the presence of pre-trends among states that did versus did not adopt RTW laws. The results are also robust to comparing individuals on the border of states with and without RTW laws, which leverages variation among a more comparable set of individuals.

These results have large implications for how we think about the role of unions in the modern economy. Dating back to early work by Freeman (1976) and Freeman (1980) that formalized a hypothesis from Hirschman (1970), unions conventionally provided employees a way of expressing their voice without requiring them to exit from undesirable employment situations. While this may have been the case in the 1980s, my results suggest that it may not be the case anymore: individuals well-being rises after granted the option of not joining a union. Given the decline in union density and the negative effect of RTW laws on union formation, these results are evidence that employees no longer see unions as a benefit to their welfare. I provide suggestive evidence that the adoption of RTW laws actually raises the probability that managers treat their employees as partners and create an open/trusting work environment by 4.2% and 4%, respectively.

This paper opens up several routes for additional inquiry. First, how do unions and/or other labor market regulations affect the returns that firms face to offer non-wage amenities, such as benefits and corporate culture? To the extent firms pay unionized employees above the market wage, then the firms have less money to spend on other areas—in particular, developing their human capital. My results suggest that RTW laws have granted employees greater autonomy and increased the employee-employer surplus that is on the table for negotiation. Second, given the result that RTW laws raise individual 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 et al. (2013) argue that employee ownership and profit sharing is one candidate approach. Much 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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6 Tables and Figures

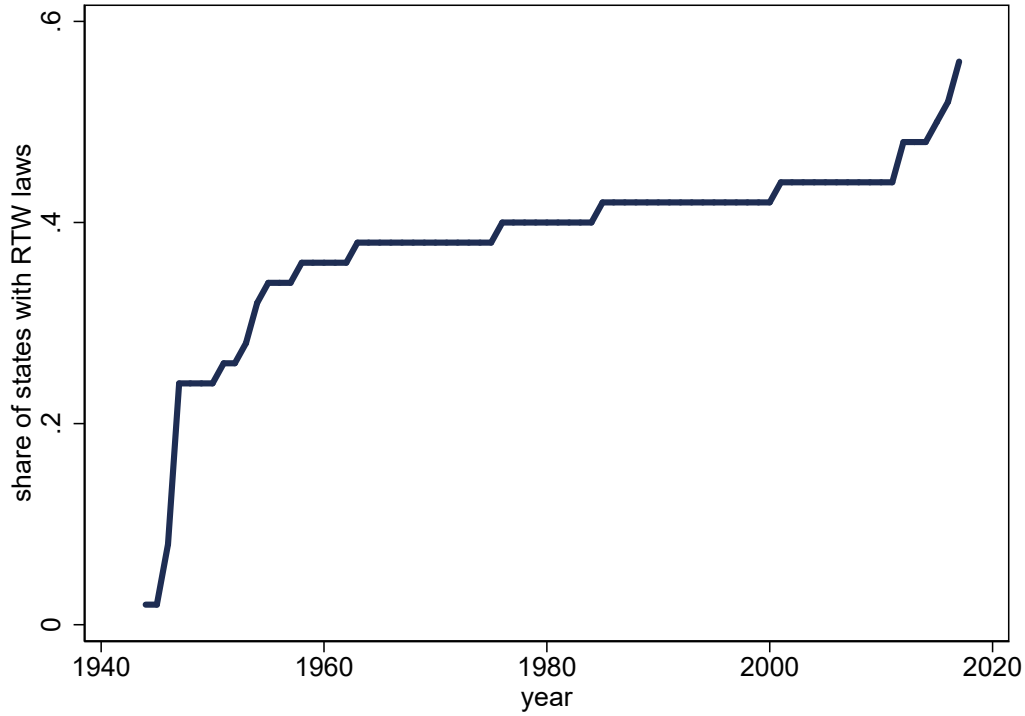

Figure 1: Incidence of Right-to-Work Laws Across States and Time

Notes.–Sources: The figure plots the share of states with right-to-work (RTW) laws since 1944.

Variable	Survey Question	Rating
Life Satisfaction	Please imagine a ladder with steps numbered from zero at the bottom to ten 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?	1-10 scale
Expected Future Life Satisfaction	On which step do you think you will stand about five years from now?	1-10 scale
Perception of Current Economic Activity	How would you rate economic conditions in this country today: as excellent, good, only fair, or poor?	1-4 scale
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 scale
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 scale

Table 1: Main Gallup Survey Questions

Notes.–Sources: Gallup. The table reports the survey questions and associated rating index used by Gallup when speaking with respondents.

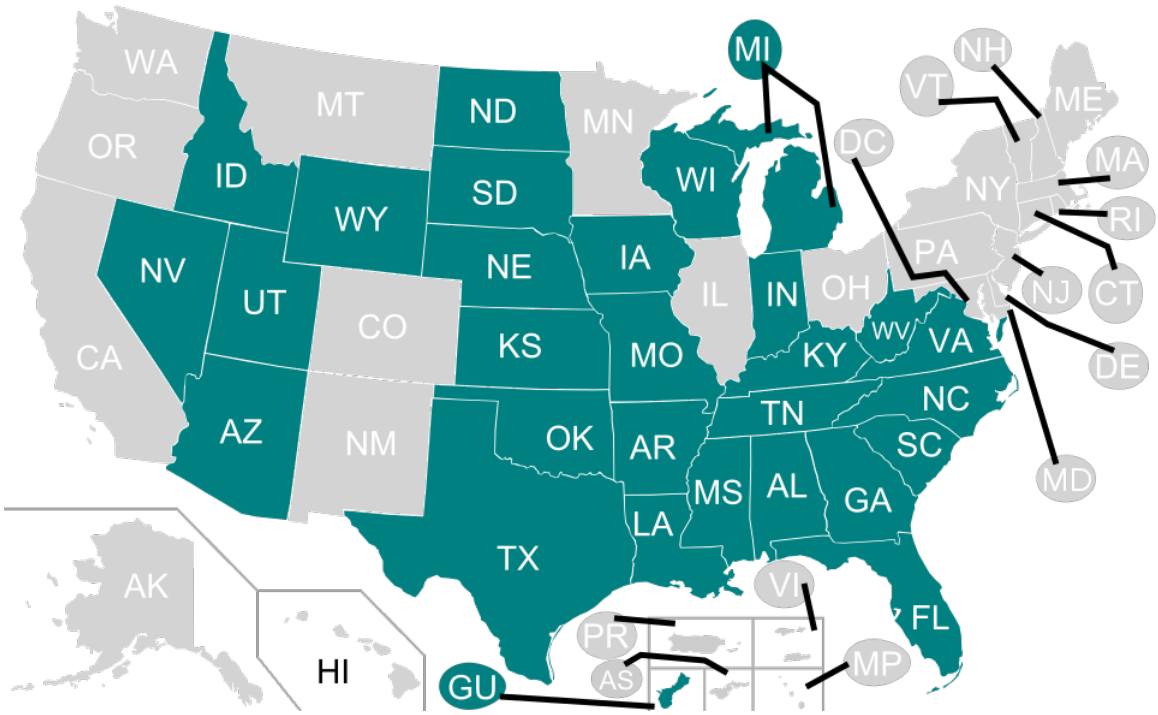

Figure 2: Spatial Variation in Right-to-Work Laws

Notes.–Sources:https://en.wikipedia.org/wiki/Right-to-work_law#/media/File:Right_to_Work_states.svg.

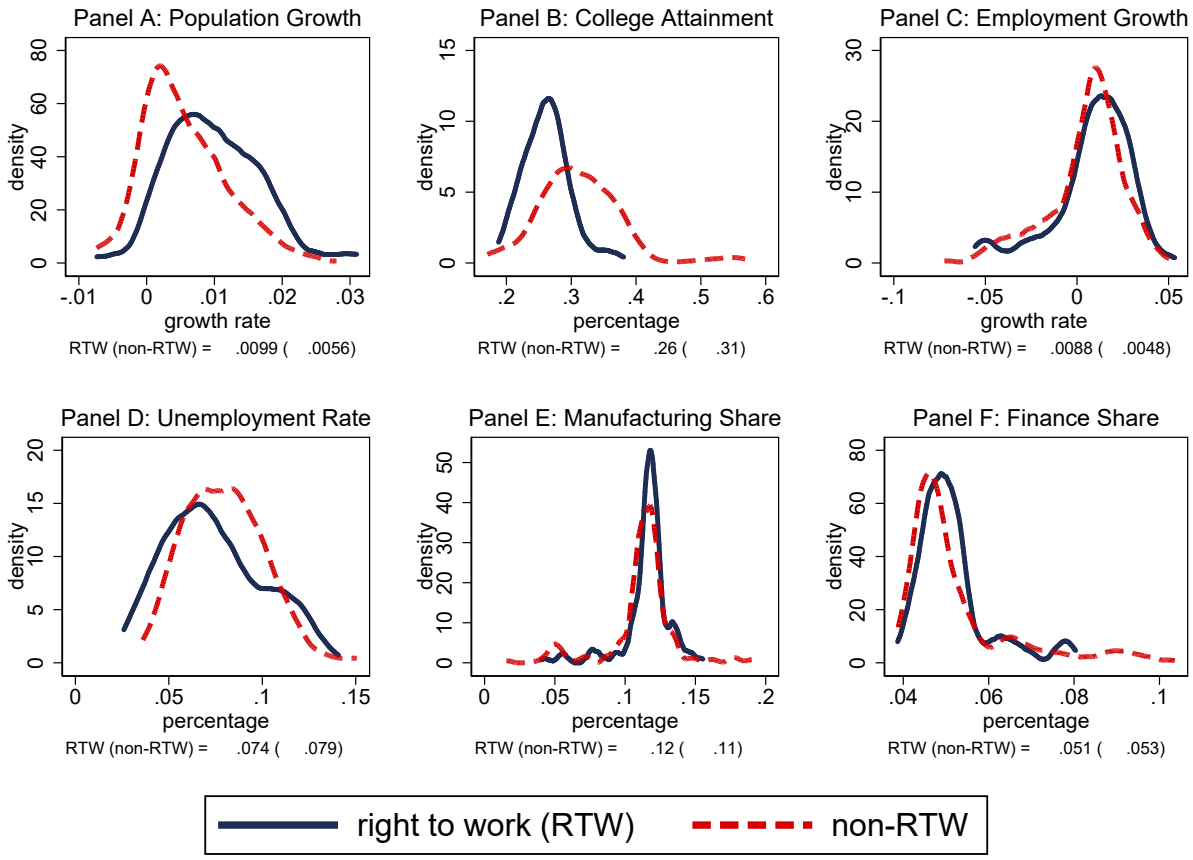

Figure 3: Comparison of Right to Work (RTW) and non-RTW States

Notes.–Sources: Gallup and American Community Survey. The figure plots the distribution of several variables between right-to-work and non-RTW states over the 2008-2016 years.

Table 2: Baseline Estimates of Right-to-Work Laws on Well-being and Sentiment

Dep. var. =	current life satisfaction		future life satisfaction		economic sentiment	
	(1)	(2)	(3)	(4)	(5)	(6)
1[RTW law]	.037**	.029***	.030***	.014**	.035	.054
	[.015]	[.005]	[.007]	[.006]	[.021]	[.040]
age	-.025***	-.024***	-.013***	-.013***	-.021***	-.019***
	[.001]	[.001]	[.001]	[.001]	[.001]	[.001]
age^2	.000***	.000***	-.000***	-.000***	.000***	.000***
	[.000]	[.000]	[.000]	[.000]	[.000]	[.000]
male	-.096***	-.097***	-.110***	-.110***	.091***	.087***
	[.003]	[.003]	[.004]	[.004]	[.005]	[.005]
1[no high school]	-.204***	-.198***	-.309***	-.305***	-.075***	-.056***
	[.015]	[.014]	[.012]	[.012]	[.021]	[.017]
1[HS or trade]	-.059***	-.053***	-.101***	-.097***	-.064***	-.047***
	[.003]	[.004]	[.004]	[.004]	[.006]	[.005]
1[college]	.270***	.268***	.131***	.130***	.223***	.221***
	[.005]	[.005]	[.004]	[.004]	[.005]	[.004]
1[white]	-.036***	-.022**	-.129***	-.116***	-.233***	-.217***
	[.011]	[.009]	[.013]	[.014]	[.019]	[.020]
R-squared	.03	.04	.11	.12	.04	.08
Sample Size	2487290	2487290	2380882	2380882	1735598	1735598
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes	No	Yes
Time FE	No	Yes	No	Yes	No	Yes

Notes.–Sources: Gallup, 2008-2017. The table reports the coefficients associated with regressions of standardized (z-score) individual current and expected future life satisfaction (one to ten scale) and perceptions about the current and future state of the economy (one to seven scale) on an indicator for whether the state has right-to-work (RTW) laws and controls. These individual controls include: day of the week (for the survey interview) fixed effects, education fixed effects (no high school, high school or technical school, or college—normalized to some college as the omitted group), and race (white). Standard errors are clustered at the state-level and sample weights are used.

Table 3: Difference-in-Difference Estimates of Right-to-Work Laws on Well-being and Sentiment

Dep. var. =	current life satisfaction		economic sentiment	
	(1)	(2)	(3)	(4)
1[RTW law]	.004	-.004	-.023	-.035
	[.008]	[.010]	[.020]	[.028]
1[union]	.042***	.027***	-.016	-.004
	[.007]	[.005]	[.012]	[.015]
× 1[RTW law]	.020*	.020**	.109***	.077***
	[.011]	[.008]	[.017]	[.018]
R-squared	.03	.03	.05	.07
Sample Size	1053037	1053037	729633	729633
Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	No	Yes
Time FE	Yes	Yes	No	Yes
Balancing Weight	No	Yes	No	Yes

Notes.–Sources: Gallup, 2009-2016. The table reports the coefficients associated with regressions of standardized (z-score) individual current life satisfaction (one to ten scale) and perceptions about the current and future state of the economy (one to seven scale) on an indicator for whether the state has right-to-work (RTW) laws, an indicator for whether the individual is in a union job, their interaction, and controls. These individual controls include: day of the week (for the survey interview) fixed effects, education fixed effects (no high school, high school or technical school, or college—normalized to some college as the omitted group), and race (white). Standard errors are clustered at the state-level and sample weights are used except for in columns 2 and 4 where an entropy balancing weight is used based on Hainmueller (2012). To construct the weight, I regress RTW on state monthly employment growth, individual age, an indicator for college attainment, and race (white) using the package from Hainmueller and Xu (2013).

Table 4: Robustness with Time-varying State Controls on the Difference-in-Difference Estimates

Dep. var. =	current life satisfaction		economic sentiment	
	(1)	(2)	(3)	(4)
1[RTW law]	-.011	-.014*	-.020	-.027
	[.007]	[.008]	[.015]	[.017]
1[union]	.042***	.027***	-.013	-.002
	[.007]	[.005]	[.011]	[.015]
× 1[RTW law]	.019*	.019**	.092***	.076***
	[.011]	[.008]	[.015]	[.018]
Δ ln(state employment)	.683***	.648***	1.783***	1.394***
	[.224]	[.186]	[.434]	[.333]
Δ ln(state employment)$_{t-1}$	.364***	.480***	1.338***	1.236***
	[.132]	[.149]	[.374]	[.380]
R-squared	.03	.03	.07	.08
Sample Size	1051851	1051851	728626	728626
Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Balancing Weight	No	Yes	No	Yes

Notes.–Sources: Gallup, American Community Survey, 2009-2016. The table reports the coefficients associated with regressions of standardized (z-score) individual current life satisfaction (one to ten scale) and perceptions about the current and future state of the economy (one to seven scale) on an indicator for whether the state has right-to-work (RTW) laws, an indicator for whether the individual is in a union job, their interaction, and controls. The individual controls include: day of the week (for the survey interview) fixed effects, education fixed effects (no high school, high school or technical school, or college—normalized to some college as the omitted group), and race (white). The state controls include state employment growth, lagged state employment growth, state population growth, the age distribution (between 0-18, 19-34, 65+—normalized to 35-64), the education distribution (no high school, high school, some college, graduate—normalized to college), the race distribution (white and black). Standard errors are clustered at the state-level and sample weights are used except for in columns 2 and 4 where an entropy balancing weight is used based on Hainmueller (2012). To construct the weight, I regress RTW on state monthly employment growth, individual age, an indicator for college attainment, and race (white) using the package from Hainmueller and Xu (2013).

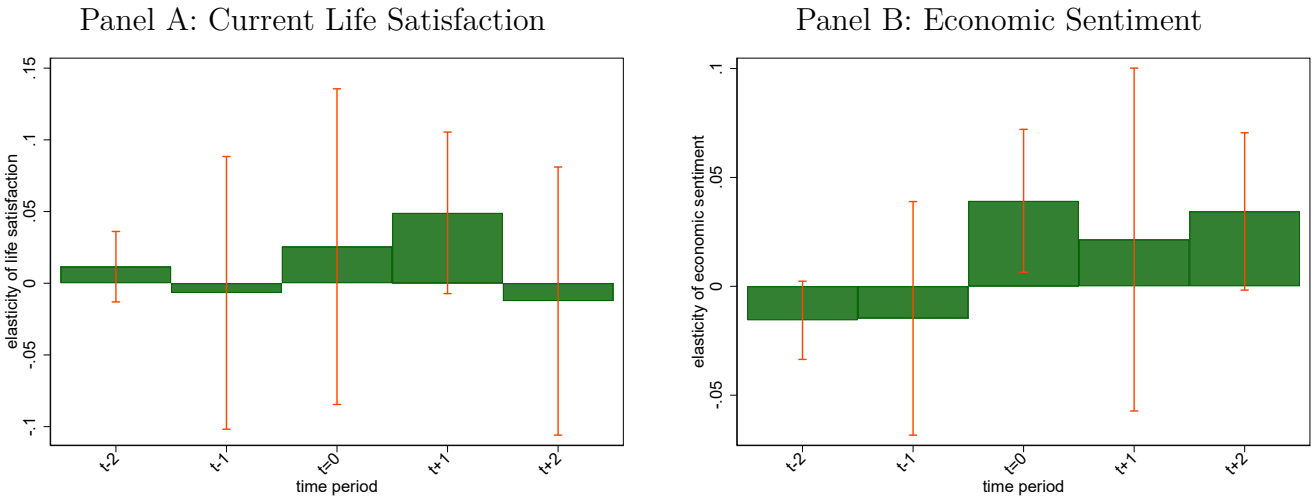

Figure 4: Examination of Potential Pre-trends in States Adopting Right-to-Work Laws

Notes.–Sources: Gallup, 2008-2017. The table reports the coefficients associated with regressions of the form

$$y_{ist} = \gamma_1 RTW_{s,t-2} + \gamma_2 RTW_{s,t-1} + \gamma_3 RTW_{s,t} + \gamma_4 RTW_{s,t+1} + \gamma_5 RTW_{s,t+2} + \beta X_{it} + \eta_s + \lambda_t + \epsilon_{ist}$$

where the outcome is standardized (z-score) individual current life satisfaction (one to ten scale) and perceptions about the current and future state of the economy (one to seven scale). The individual controls include: day of the week (for the survey interview) fixed effects, education fixed effects (no high school, high school or technical school, or college—normalized to some college as the omitted group), and race (white). Standard errors are clustered at the state-level and sample weights are used.

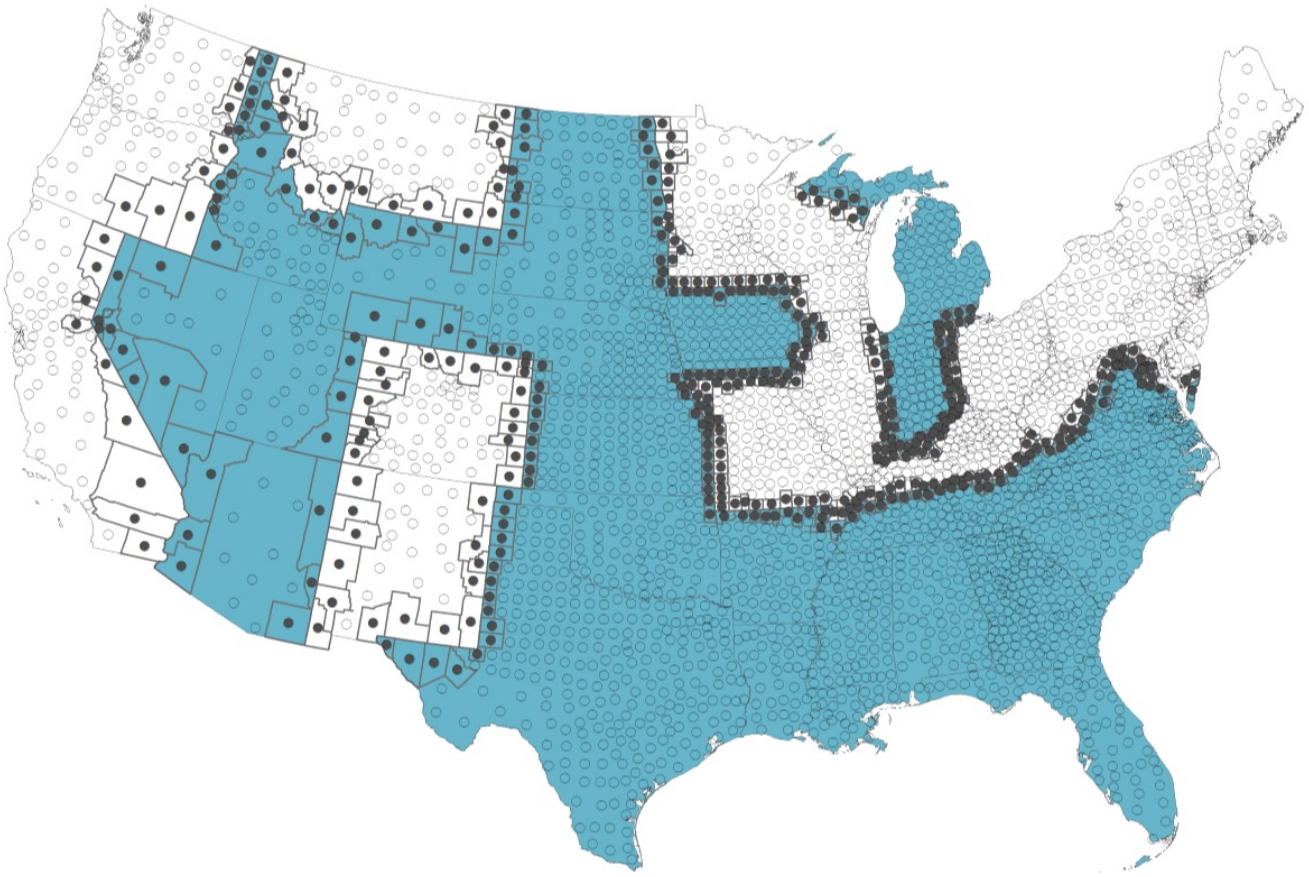

Figure 5: Comparison of Individuals in Counties Along State Borders

Notes.–Sources: Census. The figure plots the counties that are on the border of states with right-to-work (RTW) laws. The full set of counties is 442 and the Gallup micro-data contain 441 of them under the restriction of observing at least 200 respondents in the data.