Internet Appendix to
"The Economic Impact of Right-to-Work Laws:
Evidence from Collective Bargaining Agreements and Corporate Policies"*

A. Robustness tests

A.1. What predicts the introduction of RTW laws?

Our difference-in-differences methodology reduces the likelihood that omitted variables, such as globalization or anti-union sentiment, are driving our results. However, we want to investigate, to the extent possible, what these omitted variables might be. Also, we want to understand what leads to the introduction of RTW laws. Therefore, we follow the approach of Simintzi, Vig, and Volpin (2015) among others, and estimate a predictive regression using several state-level political and economic variables.

One of our predictive variables is the political orientation of a state's governor. It is plausible that the political party in power has an effect on this particular type of law. Of the five RTW introductions that we focus on in our BNA sample period, Oklahoma (2001), Indiana (2012), Michigan (2013), Wisconsin (2015), and West Virginia (2016); all occurred under either a Republican governor, a Republican state legislature, or both. Moreover, in the aftermath of the 2010 midterm elections, the Indiana state legislature tipped from an even split to Republican, the Michigan governorship and legislature shifted to Republican control, and the Wisconsin governorship and legislature flipped from Democratic to Republican. Over the next election cycle, all three states introduced RTW laws. For West Virginia, the state legislature has been controlled by the Republican party since 2014. Jim Justice was elected as the governor of West Virginia in 2016, and he switched party affiliation from Democratic to Republican as soon as he took office. Later in the same year, West Virginia joined the ranks of right-to-work states. Taken together, it is plausible to hypothesize that political party control at the state level might influence the likelihood of RTW adoption.

We use Carl Klarner's political data repository for data on governors up to 2010, and we manually extend the data up to 2016.[1] The *Governor democrat* variable takes the value of 1 if the governor is a Democrat and 0 in the case of a Republican. Independent governors are coded as 0.5.

Second, we use state-level imports from China as a proxy for the effect of globalization.[2] The data are from the USA Trade Online database (State of Destination) of the Census Bureau. We scale this variable by the nominal gross state product. Third, we include the state-level union membership rate in the regressions as a proxy for union strength. The real growth rate in the gross state product is also incorporated as a predicting variable. Other variables are (a) the change in

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[1] `http://klarnerpolitics.com/kp-dataset-page.html`

[2] To further ease the concern that globalization, in particular trade with China, is simultaneously driving RTW introduction and low real wage growth, we check and verify that none of the commuting zones (with the exception of Milwaukee, WI) classified as high import exposure per worker by Autor, Dorn, and Hanson (2013) is in an RTW adoption state in our sample period.

the union membership rate over the previous five years and (b) the annual change in imports from China. Due to the limitations of the trade data, which are only available at the state level from 2008, we perform the predictive regressions from 2009 to 2017, which allows us to capture the five most recent RTW introductions (Indiana, Michigan, Wisconsin, West Virginia, and Kentucky). We start in 2009 because all predictors are lagged by one year. The dependent variable is ΔRTW, the dummy variable that indicates the year of the law's introduction. For the five treated states, we remove observations after the introduction of the law. Also, as in our other regressions, we remove RTW states that introduced the law before the beginning of the sample period, which is 2009 in this case.

The regression results are documented in Table A13. It shows that the political orientation of the governor is an important predictor of RTW laws. That variable is statistically significant in all columns. RTW legislation is more likely to be passed when the governor is Republican. Interestingly, the other variables are not statistically significant. Therefore, it does not seem likely that globalization or union strength is responsible for RTW laws, although we cannot rule out that possibility.

In Table A14, we estimate our base case specification from Eq. (5), while controlling for the *Governor democrat* variable.[3] The table shows that the governor's party affiliation is significant in the most stringent specification, in Column (4). A Democratic governor has a positive but small effect on wage growth, with a coefficient of 0.001. Most importantly, the coefficients of the ΔRTW dummy are very similar to Table 3, both in magnitude and in significance. This suggests that our results are not driven by the most obvious predictors of RTW laws.

A.2. Measuring wage growth over the second year of each contract

In all our tests using data from collective bargaining agreements, we measure wage growth by considering the first year of each contract, as explained in Section 4. Since wage growth over the first year is an imperfect measure of the overall value provided by a CBA to workers, this raises the question of whether our results are driven by our particular variable definition.

To see how sensitive our results are to this assumption, we collect the wage increase for the second year of each contract. This is more difficult to do than collecting data for the first year, and it results in fewer observations and a potentially noisier measure of wage growth. We collect this data by adopting our textual analysis algorithm introduced in Section 4.

We make a couple of assumptions to create a measure that is as accurate as possible. First, if the contract length is only one year, then we omit the observation from the sample. Second, if the contract length is more than a year, and if the wage increase for the second year is not mentioned explicitly, then we assume that the second-year wage increase is zero. Third, if a contract includes a wage reopener clause for the second year, we omit the observation from the sample. Fourth, if a CBA specifies that wages in the second year are conditional on future inflation, which is unknown ahead of time, then we also omit the data point from the sample. Finally, for a few observations, we must convert an absolute wage increase into a relative increase without knowing the exact level of wages at the beginning of the second year. For these observations, we use the wage level at the beginning of the first year, obtained as in Section 4, as an approximation. As a result of these assumptions, the final sample contains second-year wage information for 14,585 contracts, slightly below the 15,125 observations in Table 3.

We estimate similar difference-in-differences specifications as in Table 3, but with the second-year wage growth as the dependent variable. The results are shown in Table A15. Most importantly,

[3]The number of observations is a bit lower than in Table 3 because the District of Columbia does not have a governor.

the ΔRTW dummy is negative and highly significant across all specifications. The magnitude of the coefficients is very similar to those in Table 3. This is reassuring because it suggests that our results are not driven by measuring wage growth only over the first year of each contract.

A.3. Using only RTW states

One of the potential concerns with our difference-in-differences specification in Eq. (5) is that non-RTW states are not a good control group for those states that introduce an RTW law. Also, one might criticize the addition of non-RTW states to the sample as an artificial increase in the sample size, which could lead to an excessive reduction of standard errors. To alleviate these concerns, we omit all non-RTW states from the sample, and we repeat our base case regressions from Table 3, while using only observations from the five RTW states. In the resulting difference-in-differences regressions, the control group consists solely of observations in RTW states before the introduction of the RTW law.

Table A16 contains the results of the regressions using the reduced sample. They are very similar to the results in Table 3. The coefficients of ΔRTW are negative and highly significant in all columns. Even the magnitudes of the coefficients are similar. For example, in Column (4), the most conservative specification, the coefficient of ΔRTW is -0.006 in both tables. This highlights the robustness of our estimates, especially since the sample size has dropped from 15,125 to 2,278. Taken together, these findings suggest that adding non-RTW states to our control group does not affect our main conclusions.

We also construct a robustness test where we use the neighboring states of our five treated states as the control group. The list of neighboring states, within the subset of non-RTW states, contains Colorado, Illinois, Kentucky, Maryland, Minnesota, Missouri, New Mexico, Ohio, and Pennsylvania. We estimate our base case regression from Table 3 using only these neighboring states and our five treated states, and find qualitatively very similar results. We omit this table for brevity, but the results are available from the authors.

A.4. Drop multistate observations from the sample

As explained in Appendix B, the raw CBA data set contains some observations in which the contract covers workers in multiple states. Since the state variable in the raw data contains the value *multistate* for these observations, we divide each of these observations into multiple observations, using the information in the city variable. This raises the potential concern that the treatment of the multistate contracts artificially inflates the sample size, or that multistate contracts affect the estimation in some other special way. To account for this possibility, we remove all multistate observations from the sample, and we re-estimate our base case regressions from Table 3 using the smaller sample.

Table A17 shows that the resulting regression results are very similar to the results shown in Table 3. The ΔRTW coefficients are negative and highly significant in all specifications. The coefficient magnitudes are similar as well.

A.5. Drop early years with no RTW introductions

The sample period of our contract-level tests is 1988–2016. However, as can be seen in Table 1, all five RTW introductions that are used as the treated states occurred towards the end of the sample period. This possibly raises the concern that the early years of the sample period bias some coefficients in our regression specification in Eq. (5). Therefore, we shorten the sample period to 2001–2016, and re-estimate our base case regressions from Table 3. The results are shown in Table

A18, and they are very similar to the findings in Table 3. In the most conservative specification, in Column (4), the coefficient of ΔRTW drops slightly to -0.004, but is still significant at the 5% level.

A.6. Early contract renegotiation

One might be concerned about whether our results are biased by premature contract renegotiation. This is not likely to be the case. In the US, a contracting party cannot unilaterally change the terms of a contract. Also, a contract would be invalid if it is written in a way that allows one party not to perform its duties.[4] A rare exception to cancel an existing contractual obligation would be a Chapter 11 bankruptcy filing. However, since our results suggest that RTW laws help firms rather than hurting them, it is unlikely that RTW laws lead to more bankruptcies.

The typical way to prematurely renegotiate a contract would be a voluntary renegotiation, where both parties agree to change the key contractual terms, like the wage rate. Again, it is very unlikely that both parties have an incentive to renegotiate the contract. Our results suggest that RTW laws benefit employers and hurt the covered employees. Therefore, a renegotiation request by the employer will most likely be declined by the union.

Even though we believe that premature renegotiations are very unlikely, we examine how such renegotiations would affect our results. As an example, consider the possibility that all CBAs are prematurely renegotiated in the years prior to the passage of the RTW law, and that the newly negotiated wage rate reflects the shift in bargaining power to the employer. This might lead us to underestimate the true treatment effect after the introduction of the law, i.e., in years 0, +1, +2, etc. If this is the case, the parallel trends assumption in our main wage regression should be violated. Also, we should see a significant increase in the number of contracts in years -1 and -2. However, we know from Table 3 that the parallel trends assumption is not violated. Also, we check whether the number of contacts significantly increases in year -1 and -2 in Table A19, and this turns out not to be the case. The corresponding variables, ΔRTW^{-1} and ΔRTW^{-2}, are insignificant.

A second possibility would be that contracts are prematurely renegotiated in the year of the law's introduction, i.e., in year 0. If a random sample of contracts is renegotiated this way, that should not bias the treatment effects in years 0, +1, or +2. To get a bias that explains our findings, one must further assume that high wage growth contracts are more likely to be renegotiated early than low wage growth contracts. In that case, the treatment effect in the year of the law might be overestimated, while the treatment effect in years +1 and +2 might be underestimated (in absolute values). However, in this case, we should see a significant increase in the number of contracts in year 0. We test for such an increase in year 0 in Table A19, but corresponding coefficient, ΔRTW, is insignificant.

A.7. Higher-order fixed effects

Our base case regressions in Table 3 already include year, industry, and state fixed effects. They control for a lot of unobservable variation in wage growth. However, we cannot rule out the possibility that there are omitted variables that are correlated with both our RTW dummy and with wage growth. To further alleviate such concerns, we take the spline regression specification in Column (4) in Table 3 and add higher-order fixed effects. In Table A20, Column (1) contains state and industry-year fixed effects, Column (2) contains year and state-industry fixed effects, and Column (3) combines industry-year and state-industry fixed effects. In all columns, the coefficient

[4] See the concept of an "illusory promise" in Black (1990), among others.

of ΔRTW is negative and has a similar magnitude as in Table 3, although the statistical significance is slightly lower.

A.8. Triple-difference estimation with unemployment rate

The bargaining power of unions is not only affected by RTW laws, but also by characteristics of the firm, the state, and the overall economy. In particular, it is likely that the state of the local economy has an influence on negotiations between employers and unions. We control for state-level economic growth in our regressions, but we do not explicitly control for the condition of the local labor market. If the local unemployment rate is high, it is plausible that the labor negotiations are affected. Also, it is possible that high unemployment interacts with the effect of RTW laws. For example, it could be that during times of high local unemployment, the bargaining power of unions is already low, and that the introduction of RTW laws hurts them even more.

To test this hypothesis, we obtain unemployment data at the state-year level from the Local Area Unemployment Statistics (LAUS) database of the Bureau of Labor Statistics. We then estimate an augmented version of Eq. (5). It is a triple-difference regression where we add the change in the local unemployment rate, $\Delta Unemp$, as an additional control variable and also interact it with the ΔRTW dummy variable.

The results of this regression are shown in Table A21. The coefficient of ΔRTW is still negative and significant at the 1% level in all columns. This is reassuring, because it helps us to rule out an additional omitted variable concern, according to which RTW laws are just a reaction to developments in the local labor market. Interestingly, the coefficient of $\Delta Unemp$ is significantly positive in all columns. This means that an increase in the unemployment rate is associated with higher wage growth for unionized workers. More importantly, however, the interaction term $\Delta RTW \times \Delta Unemp$ is negative. This suggests that during times with increasing unemployment, the negative effect of RTW laws on wage growth is even stronger, which is consistent with our hypothesis. However, the coefficient is only significant in Column (3), which is the most conservative specification.

A.9. Results without controlling for GSP growth

In our regression specification in Eq. (5) we control for local economic conditions with GSP growth. This regression could be biased if GSP growth is itself affected by the introduction of an RTW law. In Table A22, we omit GSP growth and show that the results are both qualitatively and quantitatively very similar to Table 3.

A.10. Results without using CBP data

As explained in Appendix B, for a subset of contracts we need to convert the dollar increase in wages to a percentage increase. For some of these contracts we know the level of wages, which helps us with the conversion. For contracts where the level of wages is not available, we rely on County Business Patterns (CBP) data for an estimate of wage levels. In Table A23, we omit all contracts for which we use CBP data, and show that our results are very similar to Table 3.

A.11. Dynamic effect of RTW laws on firm policies between 1988 to 2016

The dynamic response of investment, employees growth, and leverage are unchanged compared with the full sample results documented in Table 6. Parallel trend assumptions are satisfied in all three columns in Table A7. The most significant rise in investment and employees growth

materializes in year 3 after RTW introduction, consistent with the full sample. Moreover, leverage declines significantly in almost all years following RTW introduction in Column (3), but especially in RTW^{+2}. From this exercise, we are more confident that the five RTW adoptions in the short sample are similar to the fourteen RTW adoptions in the full sample between 1950 and 2016.

A.12. Merging large contract expirations with Compustat

The main CBA dataset in our paper only contains about 19,000 successful contract renegotiations, most of which are private firms, so merging with Compustat would produce a small sample size. However, we were able to use another Bloomberg BNA database, called Contract Listings, which contains a much larger number of contracts. While these contracts do not have wage data, they do contain the employer name and the expiration date. Under the assumption that a contract expiration is typically followed by a contract renegotiation, we are able to identify a subsample of Compustat firms that experience a contract renegotiation.

The Contract Listings database is the same database that we have used to show that our sample of collective bargaining agreements is not biased relative to the population of CBAs (see Table A5). In this robustness test, we manually match a subset of large contracts in the Contract Listings database to Compustat.

Our initial sample contains 433,082 contract expirations with a non-missing expiration date, covering 1995–2017. This sample is too large to be manually matched to Compustat. The matching is made difficult by the fact that the employer name in the Contract Listings database can either denote a parent company name, a subsidiary name, a plant name, or even a trade association name.

To reduce the sample size while focusing on the most important contract expirations, we drop observations with a bargaining unit size of less than 500 workers as well as contract expirations where the bargaining unit size is missing. The same threshold for the number of workers is used in Yi (2016). This reduces the number of observations to 18,390.

We only keep the five states that introduce RTW during our sample period (Oklahoma, Indiana, Michigan, Wisconsin, and West Virginia), which results in 1,983 contract expirations. After dropping all years prior to RTW passage and years later than five years after passage, the number goes further down to 324. For these contract expirations, we manually match each employer name (parent company, subsidiary, or plant name) using a Wikipedia search and a Google search to a publicly traded firm, if it exists, and then to a gvkey. The match is done not only based on name, but also based on year. Out of the 324 contract expirations, we have found gvkeys for 121.

Some firms have multiple contract expirations in the same year. After removing duplicate gvkey-expiration year pairs, the number of observations is reduced to 74. After merging with Compustat (based on gvkey and calendar year) and after dropping foreign firms, we end up with 57 expirations.

Within the 57 observations, some firms have multiple contract expirations after the introduction of an RTW law. For example, Kroger has four expirations: 2012 in Indiana, 2014 in Michigan, 2016 in Wisconsin, and 2017 in Michigan. For each firm, we focus on the first contract expiration after an RTW introduction. In the case of Kroger, this is 2012. As a result, we end up with 40 unique firms (gvkeys) with at least one large contract expiration after the introduction of RTW in one of our five RTW states.

We then perform an event study using these 40 firms, where the event year is the year of the contract expiration. We calculate average operating profitability (oibdp divided by lagged total assets) for each of the three years before and the three years after the introduction of RTW, as well as for the year of the introduction. Profitability is winsorized at the 1% level and we drop firm-years where operating profitability is below -1000%.

The results of the event study are presented in Fig. A3. It shows that profitability is relatively

constant in the years leading up to the event year, which is defined as the first contract expiration after the introduction of RTW. Following the event year, there is a steady increase in operating profitability. The vertical bars denote 95% confidence intervals for the mean. As can be seen from the figure, the increase in profitability is not statistically significant, which is not surprising, given the small sample of 40 firms. In year +3, the confidence interval is particularly wide, because most of our RTW introductions are towards the end of the sample period, so fewer than 40 firms remain in the sample by that year. However, the $p-$value of a two-sided $t-$test that compares the mean of year -3 to the mean of year $+2$ is $p = 10.3\%$. In other words, the increase in profitability is almost statistically significant.

We also construct a placebo test to support the view that the increase in profitability in Fig. A3 is not a consequence of concurrent shocks. For each of the 40 gvkeys in Fig. A3, we randomly select a firm-year observation in Compustat with the same 4-digit SIC code and the same fiscal year as the event year. By definition, firms in this control group do not experience a large contract expiration in one of our five RTW states immediately after the passage of the law. Fig. A4 shows that for this control group, profitability is mostly flat over time. Similarly to Fig. A3, the confidence intervals are relatively wide due to the small sample size.

In spite of the limitations of a small sample size, Figs. A3 and A4 provide suggestive evidence that profitability does increase after the introduction of RTW, especially if one focuses on those firms that are unionized and that experience a contract expiration in the years after RTW passage. The event study evidence presented here leads us to believe that two conditions are necessary for RTW laws to induce firm-level adjustment: RTW enactment and union contract renegotiation, which is consistent with the shift in union-firm bargaining power channel we propose.

B. Sample construction for collective bargaining agreements

Within the text extraction algorithm, we separately search for absolute and relative wage changes. We transform all absolute wage changes to relative changes by scaling them by the level of wages, if this is available in the text. If the level of wages is not available, we use the average wage from the Census Bureau's County Business Patterns (CBP) data set. To approximate the actual wage of the covered workers more precisely, we calculate the average wage for each year and for each industry, where industries are defined using 2-digit SIC codes until 1997 and 2-digit NAICS codes afterward. Average wages are calculated as total payroll in Q1 divided by the total number of workers at the end of Q1. In a robustness test in Appendix A we show that our results are very similar if we do not use the CBP data.

If the absolute change in wages is reported in weekly, monthly, or yearly amounts, we normalize them to hourly wage increases. If the BNA data set contains a range of wage increases, we use the midpoint of the range. In some cases, the *State* variable of the BNA data set specifies that the workers are located in multiple states, which is coded as *Multistate*. In those cases, we manually extract the states of the covered workers, if possible, by using the information in the *City* variable of the data set. For example, if a CBA covers workers in Maine and Tennessee, then we replace the single observation with two observations, one for each of the two states. In our robustness tests, we show that removing these Multistate observations from the sample does not affect our results.

We verify the accuracy of our text extraction algorithm by manually collecting the wage increases for a random sample of 500 contracts and then comparing the wage variable to the wage variable collected using the algorithm. We remove observations from the sample if the change in the first-year wage is higher than 100% or lower than −100%. Also, we remove observations from the sample if the contract length exceeds 10 years, which is very rare.

The regression in Eq. (5) includes the variable ΔGSP_{st}, which is the real growth rate of the gross state product of state s in year t. To calculate ΔGSP_{st}, we use the nominal gross state products provided by the Bureau of Economic Analysis, and we convert them to real annual growth rates using the GDP implicit price deflator from the Federal Reserve Bank of St. Louis website.

C. Sample construction for executive compensation

In Section 6.1, we merge our Compustat panel data set with the ExecuComp Annual Compensation database. After merging based on fiscal year and GVKEY, we end up with a shorter sample period of 1992–2016, due to the late starting date of ExecuComp. We restrict the executive compensation data to the CEO of each firm-year, and construct four variables: Salary, Options, Stocks, and OthComp. For Salary, we use the ExecuComp variable SALARY. For Options, we use OPTION_AWARDS_BLK_VALUE until 2005 and OPTION_AWARDS_FV after 2005. For Stocks, we use RSTKGRNT up to 2005 and STOCK_AWARDS_FV after 2005. Finally, for OthComp, we use the ExecuComp variable OTHCOMP.

To adjust for inflation, we use the Consumer Price Index for All Urban Consumers (All Items, Not Seasonally Adjusted) from the Federal Reserve Bank of St. Louis and express all amounts in 2016 dollars. To remove duplicate observations, we order the data set based on GVKEY, fiscal year, and executive name, and select the first observation (only one duplicate observation removed). For some of our variables, there are a few observations with negative values. In those cases, we set the value of the variable to "missing". All four variables are transformed as $\log(1 + x)$ and are winsorized at the 1% and 99% levels.

Table A1

Summary statistics for change in log wage growth broken down by state.

This table presents summary statistics for log wage growth in a sample of collective bargaining agreements (CBAs) from Bloomberg BNA, broken down by state. The sample period is 1988–2016. CBAs are matched to states through the location of the establishment at which contracts are negotiated. Each count in Column (1) represents a contract.

	(1) count	(2) mean	(3) sd	(4) min	(5) p25	(6) p50	(7) p75	(8) max
Alaska	62	0.024	0.025	0.000	0.005	0.017	0.034	0.103
California	1,580	0.036	0.033	−0.062	0.020	0.030	0.045	0.262
Colorado	128	0.031	0.053	−0.062	0.010	0.027	0.039	0.565
Connecticut	541	0.030	0.036	0.000	0.017	0.027	0.034	0.394
Delaware	37	0.037	0.024	0.000	0.029	0.032	0.041	0.127
District of Columbia	139	0.034	0.033	0.000	0.021	0.032	0.039	0.310
Hawaii	126	0.033	0.026	−0.041	0.017	0.030	0.049	0.113
Illinois	1,250	0.030	0.026	−0.069	0.017	0.029	0.038	0.223
Indiana	433	0.026	0.025	−0.128	0.010	0.027	0.034	0.193
Kentucky	125	0.028	0.026	0.000	0.017	0.025	0.033	0.161
Maine	149	0.026	0.023	0.000	0.011	0.025	0.032	0.157
Maryland	241	0.033	0.033	0.000	0.015	0.030	0.040	0.215
Massachusetts	882	0.028	0.026	−0.030	0.015	0.025	0.034	0.278
Michigan	985	0.020	0.027	−0.105	0.000	0.019	0.030	0.269
Minnesota	535	0.024	0.028	−0.163	0.007	0.020	0.030	0.326
Missouri	287	0.033	0.022	0.000	0.021	0.030	0.039	0.186
Montana	91	0.037	0.049	0.000	0.021	0.030	0.037	0.280
New Hampshire	84	0.026	0.022	0.000	0.011	0.025	0.034	0.122
New Jersey	776	0.032	0.030	0.000	0.020	0.030	0.039	0.323
New Mexico	69	0.038	0.033	0.000	0.022	0.034	0.049	0.191
New York	1,815	0.030	0.027	0.000	0.019	0.030	0.039	0.320
Ohio	1,166	0.025	0.027	−0.030	0.010	0.025	0.030	0.441
Oklahoma	71	0.034	0.075	0.000	0.015	0.020	0.034	0.635
Oregon	436	0.028	0.027	−0.051	0.013	0.025	0.035	0.231
Pennsylvania	1,449	0.028	0.025	−0.111	0.017	0.030	0.036	0.195
Rhode Island	225	0.028	0.023	0.000	0.016	0.030	0.034	0.144
Vermont	130	0.029	0.025	−0.030	0.017	0.029	0.039	0.165
Washington	524	0.029	0.027	−0.057	0.013	0.027	0.039	0.219
West Virginia	127	0.033	0.027	0.000	0.021	0.029	0.037	0.165
Wisconsin	662	0.027	0.022	−0.223	0.020	0.028	0.031	0.178
Total	15,125	0.029	0.029	−0.223	0.015	0.027	0.037	0.635

Table A2

Summary statistics for change in log wage growth broken down by year.

This table presents summary statistics for log wage growth in a sample of collective bargaining agreements (CBAs) from Bloomberg BNA, broken down by year. The sample period is 1988–2016. CBAs are aggregated by the year of the effective date of each contract. Each count in Column (1) represents a contract.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	count	mean	sd	min	p25	p50	p75	max
1988	284	0.045	0.042	0.000	0.020	0.039	0.061	0.336
1989	173	0.071	0.064	0.000	0.032	0.050	0.094	0.565
1990	146	0.074	0.048	0.000	0.039	0.058	0.102	0.221
1991	143	0.057	0.046	0.000	0.030	0.044	0.077	0.306
1992	77	0.068	0.044	0.000	0.033	0.057	0.105	0.170
1993	97	0.045	0.037	0.000	0.023	0.034	0.059	0.179
1994	165	0.032	0.027	0.000	0.021	0.030	0.039	0.184
1995	337	0.033	0.046	0.000	0.010	0.030	0.039	0.394
1996	305	0.030	0.035	−0.010	0.018	0.028	0.032	0.296
1997	575	0.027	0.018	−0.062	0.020	0.030	0.034	0.138
1998	557	0.032	0.022	0.000	0.022	0.030	0.038	0.211
1999	623	0.034	0.025	0.000	0.025	0.030	0.039	0.320
2000	650	0.038	0.029	0.000	0.027	0.032	0.044	0.441
2001	642	0.037	0.021	0.000	0.029	0.034	0.045	0.219
2002	536	0.036	0.028	−0.051	0.023	0.030	0.041	0.265
2003	672	0.030	0.026	−0.051	0.017	0.030	0.038	0.205
2004	629	0.030	0.026	0.000	0.017	0.030	0.039	0.262
2005	758	0.032	0.024	0.000	0.020	0.030	0.039	0.186
2006	646	0.034	0.023	0.000	0.022	0.030	0.039	0.195
2007	720	0.034	0.025	−0.051	0.023	0.030	0.039	0.326
2008	776	0.032	0.021	−0.062	0.020	0.030	0.039	0.183
2009	842	0.016	0.018	−0.128	0.000	0.016	0.030	0.138
2010	767	0.013	0.020	−0.223	0.000	0.010	0.025	0.189
2011	841	0.015	0.033	−0.102	0.000	0.010	0.022	0.635
2012	755	0.016	0.020	−0.105	0.000	0.020	0.025	0.269
2013	725	0.019	0.017	−0.064	0.010	0.020	0.026	0.168
2014	626	0.022	0.021	−0.163	0.015	0.020	0.028	0.253
2015	582	0.024	0.020	−0.101	0.015	0.023	0.030	0.162
2016	476	0.026	0.021	−0.111	0.020	0.023	0.030	0.183
Total	15,125	0.029	0.029	−0.223	0.015	0.027	0.037	0.635

Table A3

Summary statistics for change in log wage growth broken down by industry.

This table presents summary statistics for log wage growth in a sample of collective bargaining agreements (CBAs) from Bloomberg BNA, broken down by industry. The sample period is 1988–2016. CBAs are aggregated by the industry of the establishment at which contracts are negotiated. Industry is defined by the 2-digit SIC code. Each count in Columns (1) and (4) represents a contract.

		(1)	(2)	(3)			(4)	(5)	(6)
SIC	Name	count	mean	sd	SIC	Name	count	mean	sd
10	Metal	18	0.031	0.026	50	Wholesale-Durable	37	0.036	0.026
12	Coal	19	0.038	0.022	51	Wholesale-Non-Durable	37	0.031	0.022
14	Mining	14	0.038	0.046	53	General Merchandise	22	0.051	0.037
15	Building	55	0.041	0.030	54	Food Stores	499	0.033	0.035
16	Heavy Construction	471	0.034	0.023	55	Automotive Dealers	9	0.041	0.042
17	Contractors	414	0.042	0.037	56	Apparel Stores	19	0.046	0.017
20	Food and Kindred	433	0.027	0.021	58	Restaurants	44	0.061	0.043
21	Tobacco	2	0.027	0.000	59	Misc. Retail	61	0.044	0.036
22	Textile	46	0.038	0.040	60	Depository Inst.	11	0.041	0.041
23	Apparel	60	0.035	0.027	62	Brokers	8	0.020	0.013
24	Lumber	50	0.021	0.029	63	Insurance Carriers	35	0.028	0.011
25	Furniture	38	0.053	0.039	64	Insurance Agents	5	0.033	0.009
26	Paper	301	0.023	0.014	65	Real Estate	25	0.031	0.013
27	Printing	323	0.029	0.044	70	Hotels	111	0.042	0.038
28	Chemicals	195	0.031	0.026	72	Personal Services	38	0.038	0.035
29	Petroleum	36	0.024	0.011	73	Business Services	196	0.038	0.029
30	Rubber	103	0.034	0.031	75	Auto Repair	29	0.052	0.07
31	Leather	16	0.045	0.024	76	Misc. Repair	6	0.026	0.008
32	Stone	97	0.027	0.021	78	Motion Pictures	52	0.026	0.011
33	Primary Metal	196	0.022	0.026	79	Amusement Parks	145	0.033	0.054
34	Fabricated Metal	178	0.032	0.029	80	Health Services	1,453	0.034	0.031
35	Industrial Machinery	194	0.030	0.028	81	Legal Services	2	0.016	0.023
36	Electronic Equip.	185	0.032	0.032	82	Education	506	0.030	0.037
37	Tranportation Equip.	393	0.024	0.020	83	Social Services	68	0.029	0.025
38	Measuring Instruments	54	0.035	0.028	84	Museums	10	0.056	0.036
39	Misc. Manufacturing	31	0.044	0.026	86	Membership Org.	41	0.033	0.018
40	Railroad Transportation	83	0.034	0.027	87	Engineering	50	0.051	0.048
41	Local Transit	966	0.038	0.034	89	Misc. Services	6	0.031	0.015
42	Motor Freight	41	0.040	0.036	90	Government	2,378	0.022	0.019
43	USPS	1	0.025	.	91	General Government	17	0.043	0.056
44	Water Transportation	47	0.037	0.036	92	Justice	931	0.019	0.016
45	Air Transportation	75	0.047	0.055	93	Public Finance	1	0.026	.
47	Transportation Services	35	0.048	0.068	94	Human Resource	2	0.027	0.038
48	Communications	456	0.027	0.017	96	Economic	1	0.000	.
49	Electric Services	453	0.031	0.018	99	Nonclassifiable	2191	0.023	0.024

Table A4

Simple RTW dummy, private vs public sector employees.

This table presents estimation results for the difference-in-differences specification in Eq. (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Columns (1)–(3) are based on the subsample of private sector CBAs, in which the 2-digit SIC codes are < 90, and Columns (4)–(6) are focused on the public sector, in which the SIC codes are ≥ 90. Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:					
	$\Delta log(w)$					
	Private	Private	Private	Public	Public	Public
	(1)	(2)	(3)	(4)	(5)	(6)
ΔRTW	-0.009^{***}	-0.006^{***}	-0.004^{**}	-0.013^{***}	-0.013^{***}	-0.004
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)
GSP growth	0.069^{**}	0.059^{**}	0.046	0.100^{**}	0.100^{**}	0.075^{***}
	(0.032)	(0.029)	(0.031)	(0.040)	(0.040)	(0.027)
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry FE		*Yes*	*Yes*		*Yes*	*Yes*
State FE			*Yes*			*Yes*
Observations	9,604	9,604	9,604	5,521	5,521	5,521
Adjusted R^2	0.118	0.165	0.170	0.185	0.185	0.234
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A5

Comparison of contract distribution across states.

This table presents the comparison of two data sets of collective bargaining agreements (CBAs), both provided by Bloomberg BNA. The Contract Listings data set approximately covers the entire universe of private-sector CBAs between 1990 and 2017. The Settlement Summaries data set is a smaller set of CBAs, and is the sample used in this paper. The table shows the distribution across US states for both data sets. Columns (1) and (2) present the number of contracts in each state and their percentages as a fraction of the total number of observations in the Contract Listings data set. Columns (3) and (4) contain the same two variables for Settlement Summaries data set.

	Contract Listings (universe)		Settlement Summaries (sample)	
	(1)	(2)	(3)	(4)
	Count	Percent of Total	Count	Percent of Total
Alaska	942	0.35%	62	0.41%
California	34,794	12.85%	1,580	10.45%
Colorado	2,444	0.90%	128	0.85%
Connecticut	4,607	1.70%	541	3.58%
District of Columbia	1,645	0.61%	139	0.92%
Delaware	860	0.32%	37	0.24%
Hawaii	1,819	0.67%	126	0.83%
Illinois	30,856	11.40%	1,250	8.26%
Indiana	8,935	3.30%	433	2.86%
Kentucky	3,323	1.23%	125	0.83%
Massachusetts	8,520	3.15%	882	5.83%
Maryland	4,497	1.66%	241	1.59%
Maine	727	0.27%	149	0.99%
Michigan	16,499	6.09%	985	6.51%
Minnesota	11,913	4.40%	535	3.54%
Missouri	11,703	4.32%	287	1.90%
Montana	1,547	0.57%	91	0.60%
New Hampshire	577	0.21%	84	0.56%
New Jersey	16,392	6.05%	776	5.13%
New Mexico	1,010	0.37%	69	0.46%
New York	31,948	11.80%	1,815	12.00%
Ohio	18,123	6.69%	1,166	7.71%
Oklahoma	1,247	0.46%	71	0.47%
Oregon	5,693	2.10%	436	2.88%
Pennsylvania	22,649	8.36%	1,449	9.58%
Rhode Island	1,716	0.63%	225	1.49%
Vermont	492	0.18%	130	0.86%
Washington	12,710	4.69%	524	3.46%
Wisconsin	10,493	3.88%	662	4.38%
West Virginia	2,097	0.77%	127	0.84%
Total	270,778	100.00%	15,125	100.00%

Table A6

Comparison of contract distribution across industries.

This table presents the comparison of two data sets of collective bargaining agreements (CBAs), both provided by Bloomberg BNA. The Contract Listings data set approximately covers the entire universe of private-sector CBAs between 1990 and 2017. The Settlement Summaries data set is a smaller set of CBAs, and is the sample used in this paper. The table shows the distribution across major SIC industries for both data sets. Columns (1) and (2) present the number of contracts in each major industry and their percentages as a fraction of the total number of observations in the Contract Listings data set. Columns (3) and (4) contain the same two variables for Settlement Summaries data set. The Contract Listings data set covers the period 2012–2017 as SIC industry classification is missing prior to 2012. In the Settlement Summaries data set, agreements coded with SIC industry 99 are dropped. In the bottom panel, we eliminate public sector agreements (SIC code 9), and recalculate the fraction of contracts in each major industry.

	Contract Listings 2012-2017		Settlement Summaries	
	(1)	(2)	(3)	(4)
SIC Major	Count	Percent of Total	Count	Percent of Total
1	0	0.00%	991	7.66%
2	18,347	21.97%	1,484	11.47%
3	18,566	22.23%	1,447	11.19%
4	14,913	17.86%	2,157	16.68%
5	6,680	8.00%	728	5.63%
6	10,796	12.93%	84	0.65%
7	7,411	8.87%	577	4.46%
8	4,747	5.68%	2,136	16.51%
9	2,049	2.45%	3,330	25.75%
Total	83,509	100.00%	12,934	100.00%

Without Public Sector

	Contract Listings 2012-2017		Settlement Summaries	
	(1)	(2)	(3)	(4)
SIC Major	Count	Percent of Total	Count	Percent of Total
1	0	0.00%	991	10.32%
2	18,347	22.52%	1,484	15.45%
3	18,566	22.79%	1,447	15.07%
4	14,913	18.31%	2,157	22.46%
5	6,680	8.20%	728	7.58%
6	10,796	13.25%	84	0.87%
7	7,411	9.10%	577	6.01%
8	4,747	5.83%	2,136	22.24%
Total	81,460	100.00%	9,604	100.00%

Table A7

Dynamic effect of RTW laws on firm investment, employment growth, and leverage—post-1988 sample.

We use a sample of firm-year observations from the CRSP-Compustat merged database. The sample period is 1988–2016. This table reports the coefficient estimates of spline regressions on firm policies. The explanatory variables are dummies denoting each year in the 11-year (±5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. Treated observations beyond ΔRTW^{+5} are omitted. The dependent variable in Column (1) is investment, defined as capital expenditure (CAPX) divided by lagged assets. The dependent variable in Column (2) is employees growth, defined as employees (emp) divided by lagged employees minus 1. The dependent variable in Column (3) is book leverage, defined as debt in current liabilities plus long-term debt (dlc + dltt) divided by lagged assets. All regressions include controls (not shown) and year, industry as well as firm fixed effects. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1)	(2)	(3)
	Inv/A	EmpGr	Debt/A
$\Delta RTW^{<(-5)}$	0.00390	0.0127	−0.00153
	(0.92)	(0.82)	(−0.11)
ΔRTW^{-5}	0.00144	−0.0292	−0.00162
	(0.57)	(−1.53)	(−0.08)
ΔRTW^{-4}	0.00287	0.00762	−0.00139
	(0.96)	(0.23)	(−0.07)
ΔRTW^{-3}	0.00209	0.0217	0.00214
	(0.37)	(1.14)	(0.16)
ΔRTW^{-2}	−0.00338	0.0153	−0.00410
	(−1.20)	(0.57)	(−0.41)
ΔRTW	0.00440*	0.0353	−0.00948
	(1.81)	(1.35)	(−0.98)
ΔRTW^{+1}	0.00457	−0.00572	−0.0227*
	(1.25)	(−0.44)	(−2.02)
ΔRTW^{+2}	−0.000779	0.0211	−0.0321***
	(−0.12)	(0.76)	(−3.02)
ΔRTW^{+3}	0.0111***	0.0675***	−0.0232*
	(2.82)	(3.92)	(−1.82)
ΔRTW^{+4}	−0.00325	0.0173	−0.0466**
	(−0.62)	(0.57)	(−2.12)
ΔRTW^{+5}	0.0109	−0.0111	−0.0187
	(0.48)	(−0.42)	(−0.73)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	55,685	55,685	55,685
Adjusted R^2	0.585	0.139	0.677

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8

The effect of RTW laws on firm investment, employment growth, and leverage: Labor-intensive firms only.

We use a sample of firm-year observations from the CRSP-Compustat merged database. This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1950 to 2016. The RTW law indicator (RTW) is the main explanatory variable. The dependent variable in Column (1) is investment, defined as capital expenditure (CAPX) divided by lagged assets. The dependent variable in Column (2) is employees growth, defined as employees (emp) divided by lagged employees minus 1. The dependent variable in Column (3) is book leverage, defined as debt in current liabilities plus long-term debt (dlc + dltt) divided by lagged assets. All regressions include controls and year, industry as well as firm fixed effects. State-level year-over-year real GSP growth ($GSP\ growth$) is the only control variable not measured at the firm level. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1)	(2)	(4)
	Inv/A	EmpGr	Debt/A
RTW	0.00450***	0.0172*	−0.0291***
	(2.80)	(1.83)	(−3.86)
LogAsset	−0.00865***	−0.0699***	0.0311***
	(−12.12)	(−15.34)	(10.09)
Tobin Q	0.00325***	0.0127***	−0.000421
	(5.84)	(5.14)	(−0.60)
Cash flow	0.00673**	0.0276**	
	(2.46)	(2.61)	
GSP growth	0.0432**	0.129	−0.0171
	(2.47)	(1.13)	(−0.48)
Profitability			−0.113***
			(−10.38)
Tangiblity			0.0234**
			(2.51)
Constant	0.0975***	0.397***	0.0502**
	(29.00)	(16.19)	(2.73)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	58,464	58,464	58,464
Adjusted R^2	0.527	0.151	0.648

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9

The effect of RTW on unions, using an alternative measure of the free-rider problem.

This table shows spline regressions to estimate the timing of the effect of RTW laws on unions. The sample is based on union membership data from unionstats.com, and the unit of observation is a state-year. The sample period is 1983–2016. The dependent variable is the difference between the union coverage rate and the union membership rate. Column (1) is based on the entire workforce, Column (2) focuses on private sector unions, and Column (3) is based on the public sector. The main explanatory variables are a set of dummies that indicate when a right-to-work (RTW) law is introduced. ΔRTW^{+3} denotes three years after the introduction of the law, ΔRTW^{+2} denotes two years after the law, ΔRTW^{+1} denotes one year after the law, ΔRTW is the year of the introduction, ΔRTW^{-2} is two years before the introduction, and $\Delta RTW^{<(-2)}$ stands for all years before then. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:		
	Coverage membership absolute difference		
	Total	Private	Public
	(1)	(2)	(3)
$\Delta RTW^{<(-2)}$	0.027	0.087	0.086
	(0.153)	(0.146)	(0.309)
ΔRTW^{-2}	0.068	0.037	0.423
	(0.088)	(0.125)	(0.537)
ΔRTW	0.090	0.015	0.917*
	(0.140)	(0.102)	(0.508)
ΔRTW^{+1}	0.414*	0.196	1.786***
	(0.211)	(0.132)	(0.532)
ΔRTW^{+2}	0.392***	0.354***	0.835
	(0.132)	(0.088)	(0.868)
ΔRTW^{+3}	0.101	0.148**	0.366
	(0.093)	(0.074)	(0.761)
GSP growth	−0.421	0.285	−0.670
	(0.424)	(0.599)	(1.270)
Year FE	*Yes*	*Yes*	*Yes*
State FE	*Yes*	*Yes*	*Yes*
Observations	1,014	1,014	1,014
Adjusted R^2	0.677	0.537	0.660
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A10

Dynamic effect of RTW laws on firm dividends, repurchases, and cash holdings: Labor-intensive firms only.

We use a sample of firm-year observations from the CRSP-Compustat merged database. This table reports the coefficient estimates of spline regressions on firm policies. The explanatory variables are dummies denoting each year in the 11-year (± 5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. Treated observations beyond ΔRTW^{+5} are omitted. The dependent variable in Column (1) is dividends (dv) divided by lagged assets. The dependent variable in Column (2) is repurchases (prstkc) divided by lagged assets. The dependent variable in Column (3) is cash and short-term investments (che) divided by total assets. All regressions include controls (not shown) and year, industry as well as firm fixed effects. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1) Div/A	(2) Repur/A	(3) Cash/A
$\Delta RTW^{<(-5)}$	0.000337	0.00255	0.00535
	(0.14)	(0.49)	(0.98)
ΔRTW^{-5}	0.00115	−0.00379	0.00551
	(0.74)	(−0.72)	(0.68)
ΔRTW^{-4}	0.000203	−0.0106***	0.00345
	(0.11)	(−2.87)	(0.34)
ΔRTW^{-3}	−0.00105	−0.00773	0.00370
	(−0.40)	(−1.43)	(0.48)
ΔRTW^{-2}	0.000384	−0.000329	0.00113
	(0.15)	(−0.06)	(0.18)
ΔRTW	−0.00174	−0.00158	0.00279
	(−0.84)	(−0.35)	(0.30)
ΔRTW^{+1}	−0.000115	−0.00779*	0.00302
	(−0.05)	(−1.74)	(0.36)
ΔRTW^{+2}	0.00104	−0.00994	0.0121
	(0.27)	(−1.54)	(0.57)
ΔRTW^{+3}	0.00476***	−0.00403	−0.0178
	(3.10)	(−0.58)	(−1.29)
ΔRTW^{+4}	0.00479**	−0.00589	−0.0107
	(2.10)	(−1.51)	(−0.63)
ΔRTW^{+5}	0.00310	−0.00665	−0.0610***
	(1.24)	(−0.81)	(−3.42)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Observations	58,464	52,567	58,464
Adjusted R^2	0.622	0.296	0.719

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11
The effect of RTW laws on executive compensation: Labor-intensive firms.
We use a sample of firm-year observations from the CRSP-Compustat merged database, combined with ExecuComp. This table presents estimation results for Eq. (7). The sample period is 1992–2016. The explanatory variables are dummies denoting each year in the 11-year (±5) window around the RTW adoption plus one dummy denoting if a particular observation is more than five years before the enactment of the law ($\Delta RTW^{<(-5)}$). Observations in the one year immediately before the RTW law implementation do not have a RTW dummy and serve as the benchmark. The dependent variables are various measures of CEO compensation: base salary (Salary), options granted (Options), stocks granted (Stocks), and other compensation (OthComp). All dependent variables are in logs of thousand dollars. Control variables that are not displayed are lagged cash flow over assets, lagged Tobin's Q, lagged log of assets, and the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered by state and year.

	Salary	Options	Stocks	OthComp
	(1)	(2)	(3)	(4)
$\Delta RTW^{<(-5)}$	−0.041	0.243	−0.277	−0.045
	(0.034)	(0.586)	(0.416)	(0.121)
ΔRTW^{-5}	0.035	0.614	0.073	−0.120
	(0.045)	(0.458)	(0.379)	(0.095)
ΔRTW^{-4}	0.035	0.599	−0.045	−0.006
	(0.043)	(0.491)	(0.411)	(0.167)
ΔRTW^{-3}	−0.001	−0.135	−0.318	0.022
	(0.057)	(0.488)	(0.440)	(0.154)
ΔRTW^{-2}	0.012	−0.139	−0.462	0.053
	(0.055)	(0.450)	(0.395)	(0.121)
ΔRTW	−0.006	0.266	−0.052	0.292***
	(0.039)	(0.818)	(0.530)	(0.088)
ΔRTW^{+1}	−0.017	0.174	−0.128	0.277*
	(0.029)	(0.808)	(0.570)	(0.156)
ΔRTW^{+2}	0.041	0.180	0.263	0.147
	(0.041)	(0.592)	(0.755)	(0.124)
ΔRTW^{+3}	0.077**	−0.169	−0.208	0.270*
	(0.032)	(0.451)	(0.437)	(0.154)
ΔRTW^{+4}	0.113**	0.386	−0.160	0.083
	(0.053)	(0.292)	(0.560)	(0.160)
ΔRTW^{+5}	0.006	0.875*	0.428	1.288
	(0.026)	(0.490)	(0.350)	(0.911)
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
Firm FE	*Yes*	*Yes*	*Yes*	*Yes*
Observations	14,906	14,805	14,895	14,903
Adjusted R^2	0.736	0.379	0.510	0.562
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A12

The effect of RTW laws on firm unemployment provision: Labor-intensive firms only.

We use a sample of firm-year observations from the CRSP-Compustat merged database. This table reports the coefficient estimates of panel regressions by pooling all firm-year observations from 1950 to 2016. The RTW indicator and its interaction with industry sales growth excluding a given firm's own sale ($RTW \times IndSalesGrEx$) is the main explanatory variable. All regressions include controls and year, industry as well as firm fixed effects. State-level year-over-year real GSP growth ($GSP\ growth$) is the only control variable not measured at the firm level. Robust standard errors with double clustering at the state and year level are used in reporting the t-statistics in parentheses.

	(1)	(2)
	EmpGr	EmpGr
RTW	−0.0338	−0.0351
	(−1.39)	(−1.52)
$Ind.\ Sales\ Growth\ Ex.$	0.00679***	0.00655***
	(3.57)	(2.84)
$RTW \times IndSalesGrEx$	0.0491***	0.0481***
	(3.07)	(3.42)
LogAsset	−0.0664***	−0.0732***
	(−14.49)	(−11.73)
Tobin Q	0.0136***	0.0139***
	(5.30)	(4.57)
Cash flow	0.0279**	0.0259**
	(2.56)	(2.49)
GSP growth	0.122	0.0728
	(1.06)	(0.52)
StateUI		−0.000679
		(−1.42)
Constant	0.369***	0.415***
	(15.41)	(13.78)
Year FE	Yes	Yes
Industry FE	Yes	Yes
Firm FE	Yes	Yes
Observations	56,326	48,284
Adjusted R^2	0.148	0.150

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13

Predicting the introduction of RTW laws.

This table contains the results of predictive regressions for the introduction of right-to-work (RTW) laws. The sample period is 2009–2017. The unit of observation is a state-year. The dependent variable is a dummy that takes the value of 1 in the year when a RTW law is introduced. The predictors are the political orientation of the state's governor, the ratio of a the state's imports from China to the state's gross state product, the average union membership rate of the state, the growth rate of the gross state product, the change in the state's union membership rate, the change in the ratio of imports from China and the gross state product, and a constant. Columns (1)–(3) contain OLS regressions, and Columns (4)–(6) present logistic regressions. All predicting variables are lagged by one year. Observations in RTW states after the introduction of a RTW law are omitted from the sample.

	Dependent variable:					
	ΔRTW					
	OLS			*logistic*		
	(1)	(2)	(3)	(4)	(5)	(6)
Governor democrat	−0.050**	−0.049**	−0.055**	−2.298**	−2.232*	−2.211*
	(0.020)	(0.020)	(0.023)	(1.139)	(1.151)	(1.158)
Imports from China	0.101	0.071	0.084	8.553	2.747	2.543
	(0.730)	(0.731)	(0.830)	(40.690)	(45.342)	(45.477)
Union membership	−0.002	−0.002	−0.002	−0.115	−0.113	−0.104
	(0.002)	(0.002)	(0.002)	(0.126)	(0.129)	(0.128)
GSP growth	0.013	−0.002	−0.001	−0.060	−2.403	−2.879
	(0.373)	(0.373)	(0.450)	(19.681)	(19.684)	(19.564)
Union mem. chg.		−0.006	−0.006		−0.361	−0.314
		(0.006)	(0.007)		(0.355)	(0.355)
Chg. in imports from China			−0.278			4.751
			(4.155)			(218.561)
Constant	0.084**	0.078**	0.086**	−1.440	−1.779	−1.731
	(0.037)	(0.037)	(0.042)	(1.596)	(1.654)	(1.648)
Observations	240	240	212	240	240	212
Adjusted R^2	0.012	0.013	0.007			
Log Likelihood				-21.127	-20.566	-20.153
Akaike Inf. Crit.				52.254	53.132	54.306

Note: *p<0.1; **p<0.05; ***p<0.01

Table A14
The effect of RTW laws on wage growth, controlling for governorship.
This table presents estimation results for an augmented version of Eq. (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. We control for the political orientation of a state's governor, as well as for the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	-0.019^{***}	-0.011^{***}	-0.010^{***}	-0.005^{***}
	(0.002)	(0.001)	(0.001)	(0.001)
Governor democrat	-0.002	0.001	0.001	0.001^{**}
	(0.001)	(0.001)	(0.001)	(0.001)
GSP growth	0.099^{***}	0.086^{***}	0.072^{**}	0.057^{**}
	(0.029)	(0.031)	(0.029)	(0.025)
Constant	0.028^{***}			
	(0.002)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	14,986	14,986	14,986	14,986
Adjusted R^2	0.013	0.151	0.193	0.202
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A15
The effect of RTW laws on wage growth, using the second year of each contract.
This table presents estimation results for the difference-in-differences specification in Eq. (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages, calculated for the second year of each contract. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. We control for the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	-0.016^{***}	-0.013^{***}	-0.011^{***}	-0.006^{***}
	(0.001)	(0.002)	(0.002)	(0.002)
GSP growth	0.069^{***}	0.050^{*}	0.038^{*}	0.026
	(0.023)	(0.028)	(0.023)	(0.018)
Constant	0.027^{***}			
	(0.001)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	14,585	14,585	14,585	14,585
Adjusted R^2	0.004	0.038	0.082	0.089
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A16
The effect of RTW laws on wage growth, using only RTW states.
This table presents estimation results for the difference-in-differences specification in Eq. (5), using only the subsample of observations in an RTW state. The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	-0.016^{***}	-0.006^{***}	-0.006^{***}	-0.006^{***}
	(0.001)	(0.001)	(0.002)	(0.002)
GSP growth	0.128^{***}	0.171^{***}	0.199^{***}	0.167^{***}
	(0.027)	(0.036)	(0.040)	(0.039)
Constant	0.023^{***}			
	(0.001)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	2,278	2,278	2,278	2,278
Adjusted R^2	0.030	0.188	0.238	0.243
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A17
The effect of RTW laws on wage growth, without multistate observations.
This table presents estimation results for the difference-in-differences specification in Eq. (5), using only the subsample of observations in which the collective bargaining agreement (CBA) does not cover multiple states. The unit of observation is a CBA. The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	-0.020^{***}	-0.012^{***}	-0.011^{***}	-0.006^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
GSP growth	0.110^{***}	0.092^{***}	0.081^{***}	0.061^{**}
	(0.028)	(0.031)	(0.029)	(0.025)
Constant	0.027^{***}			
	(0.001)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	14,066	14,066	14,066	14,066
Adjusted R^2	0.013	0.156	0.197	0.206
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A18
The effect of RTW laws on wage growth, with a short sample period.
This table presents estimation results for the difference-in-differences specification in Eq. (5), where the sample period is shortened to 2001–2016. The unit of observation is a collective bargaining agreement (CBA). The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:			
	$\Delta log(w)$			
	(1)	(2)	(3)	(4)
ΔRTW	-0.016^{***}	-0.012^{***}	-0.010^{***}	-0.004^{**}
	(0.001)	(0.001)	(0.001)	(0.002)
GSP growth	0.094^{***}	0.098^{***}	0.085^{***}	0.042^{**}
	(0.028)	(0.033)	(0.029)	(0.019)
Constant	0.024^{***}			
	(0.001)			
Year FE		*Yes*	*Yes*	*Yes*
Industry FE			*Yes*	*Yes*
State FE				*Yes*
Observations	10,993	10,993	10,993	10,993
Adjusted R^2	0.010	0.113	0.172	0.193
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A19

The timing of the effect of RTW on the number of CBAs.

This table presents estimation results for a difference-in-differences regression, using the sample of collective bargaining agreements (CBAs) from Bloomberg BNA. The unit of observation is a state-year. The sample period is 1988–2016. The dependent variable is the number of CBAs per state-year. The main explanatory variables are a set of dummies that indicate when a right-to-work (RTW) law is introduced. ΔRTW^{+3} denotes three years after the introduction of the law, ΔRTW^{+2} denotes two years after the law, ΔRTW^{+1} denotes one year after the law, ΔRTW is the year of the introduction, ΔRTW^{-1} is one year before the introduction, and ΔRTW^{-2} stands for two years before the introduction of the law. $\Delta RTW^{[1,3]}$ indicates the first three years after the introduction of the law. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:	
	Number of CBAs	
	(1)	(2)
ΔRTW^{-2}	−6.317	−6.341
	(5.814)	(5.804)
ΔRTW^{-1}	−5.118	−5.068
	(8.317)	(8.318)
ΔRTW	−2.520	−2.496
	(10.352)	(10.350)
$\Delta RTW^{[1,3]}$	−8.684***	
	(2.923)	
ΔRTW^{+1}		−13.248***
		(3.110)
ΔRTW^{+2}		−5.745
		(4.973)
ΔRTW^{+3}		−5.291*
		(2.750)
GSP growth	12.262	11.834
	(26.740)	(26.860)
Year FE	*Yes*	*Yes*
State FE	*Yes*	*Yes*
Observations	857	857
Adjusted R^2	0.735	0.735
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table A20

Dynamic regressions with higher-order fixed effects.

This table presents estimation results for a modified version of the difference-in-differences specification in Eq. (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variables are a set of dummies that indicate when a right-to-work (RTW) law is introduced. ΔRTW^{+2} denotes two years after the introduction of the law, ΔRTW^{+1} denotes one year after the law, ΔRTW is the year of the introduction, ΔRTW^{-2} is two years before the introduction, and $\Delta RTW^{<(-2)}$ stands for all years before then. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:		
	$\Delta log(w)$		
	(1)	(2)	(3)
$\Delta RTW^{<(-2)}$	0.003	0.001	0.0002
	(0.002)	(0.001)	(0.001)
ΔRTW^{-2}	−0.001	0.001	0.0002
	(0.002)	(0.001)	(0.002)
ΔRTW	−0.003*	−0.002*	−0.004*
	(0.002)	(0.001)	(0.002)
ΔRTW^{+1}	−0.001	0.0001	−0.001
	(0.002)	(0.002)	(0.002)
ΔRTW^{+2}	−0.002	−0.001	−0.002
	(0.002)	(0.004)	(0.002)
GSP growth	0.063***	0.041*	0.046***
	(0.020)	(0.022)	(0.018)
Year FE		*Yes*	
State FE	*Yes*		
Industry-Year FE	*Yes*		*Yes*
State-Industry FE		*Yes*	*Yes*
Observations	15,026	15,026	15,026
Adjusted R^2	0.308	0.273	0.368
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A21
The effect of RTW laws on wage growth, interacted with unemployment rate.
This table presents estimation results for an augmented version of Eq. (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy that indicates the year of the introduction of a right-to-work (RTW) law. We control for the change in the unemployment rate at the state-year level, denoted as $\Delta Unemp$, the interaction of the latter with ΔRTW, as well as for the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:		
	$\Delta log(w)$		
	(1)	(2)	(3)
ΔRTW	-0.011^{***}	-0.010^{***}	-0.009^{***}
	(0.002)	(0.002)	(0.002)
$\Delta Unemp$	0.002^{***}	0.002^{***}	0.002^{***}
	(0.0005)	(0.0005)	(0.0005)
$\Delta RTW \times \Delta Unemp$	0.001	0.001	-0.005^{***}
	(0.001)	(0.002)	(0.002)
GSP growth	0.101^{***}	0.088^{***}	0.072^{***}
	(0.034)	(0.032)	(0.027)
Year FE	*Yes*	*Yes*	*Yes*
Industry FE		*Yes*	*Yes*
State FE			*Yes*
Observations	15,125	15,125	15,125
Adjusted R^2	0.152	0.194	0.203
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A22
The effect of RTW laws on wage growth, without controlling for GSP growth.
This table presents estimation results for Eq. (5). The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy variable that indicates the year when a right-to-work (RTW) law is introduced. In contrast to Table 3, we do not control for the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:		
	$\Delta log(w)$		
	(1)	(2)	(3)
ΔRTW	-0.011^{***}	-0.010^{***}	-0.005^{***}
	(0.001)	(0.001)	(0.001)
Year FE	Yes	Yes	Yes
Industry FE		Yes	Yes
State FE			Yes
Observations	15,125	15,125	15,125
Adjusted R^2	0.148	0.192	0.201
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A23
The effect of RTW laws on wage growth, without CBP data.
This table presents estimation results for Eq. (5). In contrast to Table 3, we omit all contracts where wage growth had to be estimated using auxiliary data from the County Business Patterns (CBP) data set. The unit of observation is a collective bargaining agreement (CBA). The sample period is 1988–2016. The dependent variable is the change in the log of wages. The main explanatory variable is ΔRTW, a dummy variable that indicates the year when a right-to-work (RTW) law is introduced. An additional control variable is the growth rate of the gross state product (GSP). Standard errors are shown in parentheses and are clustered at the state level.

	Dependent variable:		
	$\Delta log(w)$		
	(1)	(2)	(3)
ΔRTW	-0.011^{***}	-0.010^{***}	-0.005^{***}
	(0.001)	(0.001)	(0.001)
GSP growth	0.079^{***}	0.068^{**}	0.053^{**}
	(0.030)	(0.027)	(0.022)
Year FE	Yes	Yes	Yes
Industry FE		Yes	Yes
State FE			Yes
Observations	13,381	13,381	13,381
Adjusted R^2	0.189	0.226	0.237
Note:			*p<0.1; **p<0.05; ***p<0.01

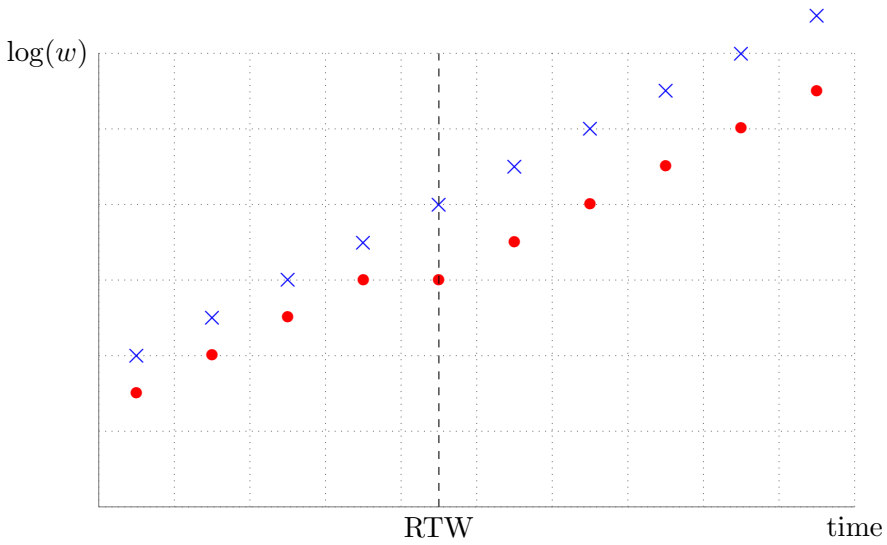

Figure A1. Stylized plot illustrating the identification strategy. The vertical axis shows log wages and the horizontal axis shows time. The dots represent contracts in a state that introduces an RTW law during the sample period. The crosses indicate observations in a state that does not pass such a law.

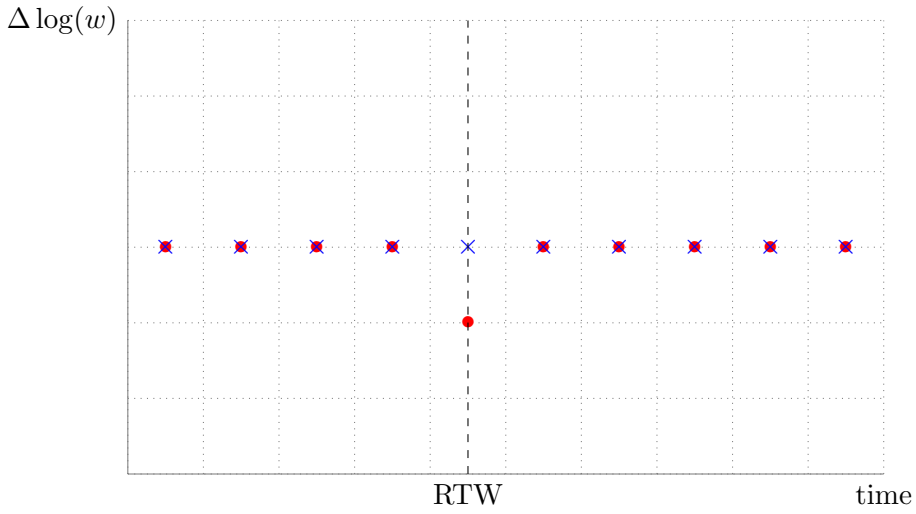

Figure A2. Stylized plot illustrating the identification strategy. The vertical axis shows the *change* in log wages and the horizontal axis shows time. The dots represent contracts in a state that introduces an RTW law during the sample period. The crosses indicate observations in a state that does not pass such a law.

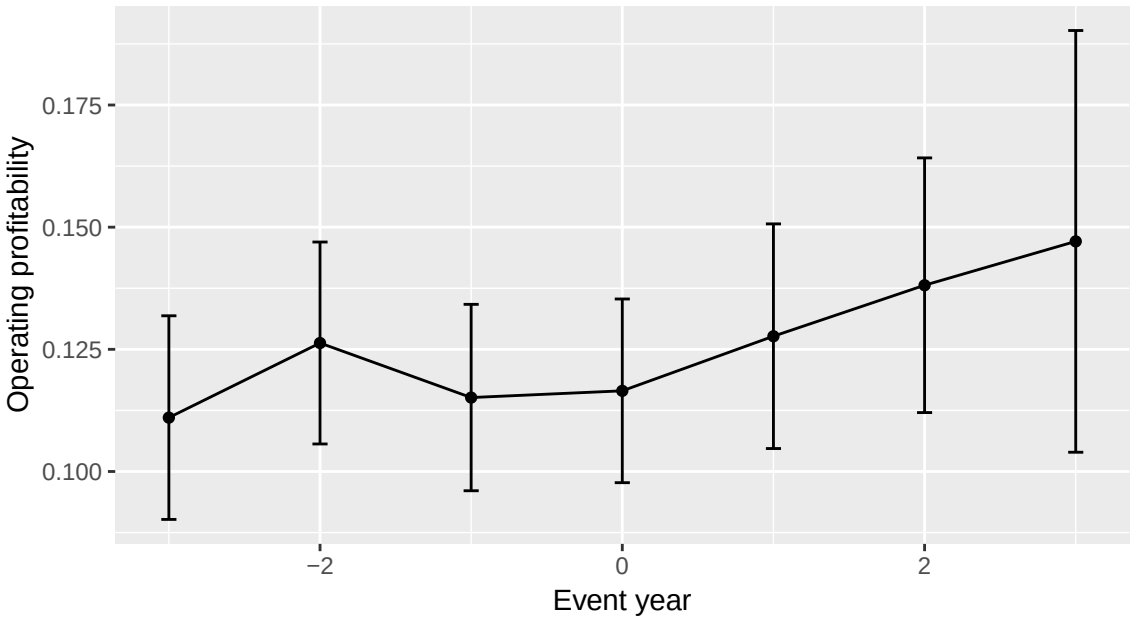

Figure A3. Event study for profitability after a post-RTW contract expiration. We identify firm-year observations in Compustat that experience a union contract expiration immediately after the introduction of RTW. We focus on large contracts which cover at least 500 workers and we limit our analysis to the five states that introduce RTW between 1995 and 2016. The contract expirations are from the Contract Listings database of Bloomberg BNA. The event year is defined as the year of the first contract expiration after the passage of RTW. The solid dots represent average operating profitability in a particular event year. The vertical lines denote 95% confidence intervals for the mean.

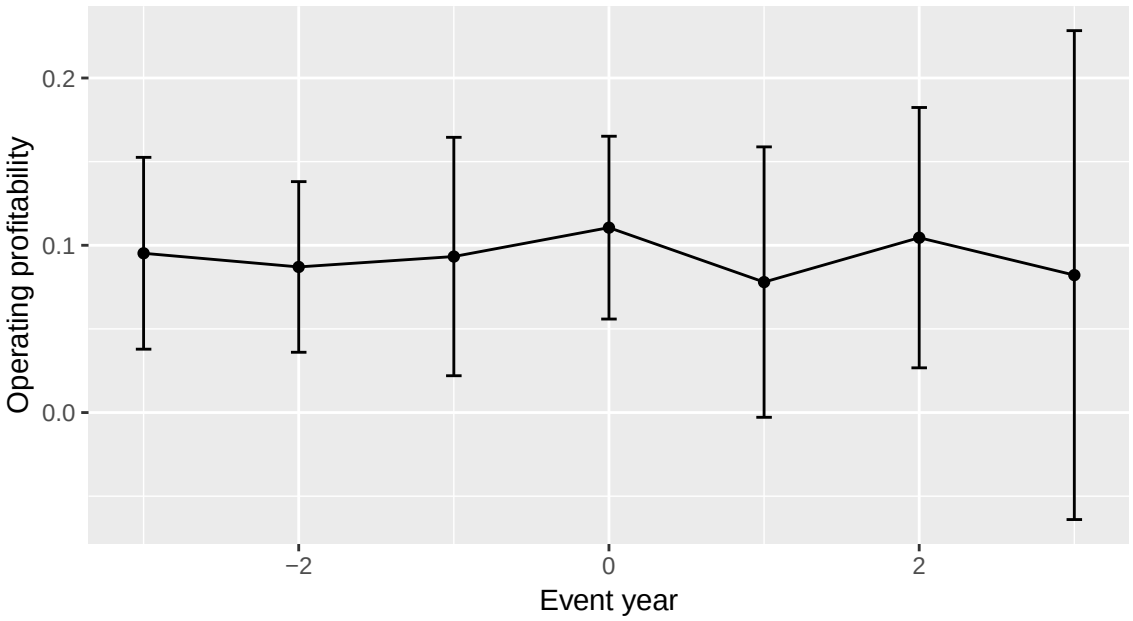

Figure A4. Placebo event study for profitability. This event study is an extension of Fig. A3 and shows the evolution of profitability for a matched control group of firms. For each of the 40 firms in our treatment group in Fig. A3, we randomly select a matched control firm in the same 4-digit SIC code and the same fiscal year as the event year. The solid dots represent average operating profitability in a particular event year. The vertical lines denote 95% confidence intervals for the mean.

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