Surprising Causes:

Propensity-Adjusted Treatment Scores for Multi-method Case Selection

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Abstract:

Scholarship on multi-method case selection in the social sciences has developed rapidly in recent years, but many possibilities remain unexplored. This essay introduces an attractive and advantageous new alternative, involving the selection of extreme cases on the treatment variable, net of the statistical influence of the set of known control variables. Cases that are extreme in this way are those in which the value of the main causal variable is as surprising as possible, and thus this approach can be referred to as seeking "surprising causes." There are practical advantages to selecting on surprising causes, and there are also advantages in terms of statistical efficiency in facilitating case-study discovery. We first argue for these advantages in general terms and then demonstrate them in an application regarding the dynamics of U.S. labor legislation.

The literature on case selection has, in recent years, begun to emphasize the perhaps surprising value of selecting cases that are extreme on the treatment, i.e., the main independent variable. This case-selection strategy has important advantages when scholars, pursuing discovery rather than confirmation, seek to uncover reasons for measurement error in the treatment, to learn about as-yet unknown confounding variables, or to test or build new hypotheses about the nature of the causal pathway connecting the treatment with the outcome (Koivu and Damman 2015; Seawright 2016). The emergent argument that extreme cases on the treatment are unusually valuable offers a potentially transformative intervention into the long-standing qualitative practice of selecting extreme cases on the dependent variable. Yet in light of recent arguments that deviant cases offer a more efficient solution to the same case-selection problems as extreme cases on the outcome, one is left to wonder whether there might be a parallel improvement to be had with respect to extreme cases on the treatment.

This essay introduces such an improvement, involving the selection of extreme cases on propensity-adjusted X. Intuitively, such cases are those in which the score of the main causal variable is as surprising as possible, given available knowledge about background variables. The shorthand label "surprising causes" captures this feature of the propensity-adjusted X case selection strategy.

The goal of discovery-oriented multi-method research is to learn about a not-fully-known data-generating process. This approach falls on the side of what process-tracing scholars often call theory-building rather than theory-testing research (Beach and Pedersen 2016: 14-28) or what people working in machine-learning call the training rather than testing phase (James et al. 2013: 21-23) of model development. As in those literatures, the purpose of case selection is to facilitate new discoveries that improve understanding and open avenues of research rather than to

confirm ideas already under discussion; some form of subsequent confirmatory research may well be necessary. The case selection strategy is regarded successful if plausible new ideas about potential limitations with existing theory are brought into the discussion, because bringing such ideas into the scholarly conversation constitutes a form of intellectual progress in its own right. For this discussion, we will focus on three concrete forms of discovery: new possible confounding variables, new ideas about sources of problems in measurement, and new hypotheses about mediators, i.e., intervening causal steps between the treatment and the outcome.

This essay begins by situating the surprising causes case-selection strategy in the existing literature on multi-method case selection. It then points toward a range of practical advantages of selecting on surprising causes and argues that this case-selection strategy should be more efficient in facilitating discovery in comparison with selecting extreme cases on the unadjusted X variable. It then transitions to an applied example in which interesting substantive insights emerge into the dynamics of U.S. labor legislation from a process of repeatedly selecting on surprising causes and then revising a regression model to capture the new insights that emerge from qualitative analysis. This extended application thus serves both as a demonstration of the practical value of a novel case-selection procedure and as an unusually explicit example of a full, iterative application of multi-method research with the goal of discovery.

1. Introducing Surprising Causes Case Selection

The best approach to case selection in qualitative and multi-method research in the social sciences has long been a topic of debate (Achen and Snidal 1989; Eckstein 1975; Herron and Quinn 2016; Lijphart 1971, 1975; Teune and Przeworski 1970; Seawright and Gerring 2008).

Among the many options discussed by these authors, methodologists within sociology have emphasized the importance of deviant (alternately anomalous or exceptional) cases (Pearce 2002, Ermakoff 2014), as well as extreme or "pointy" cases (Pacewicz 2020). Deviant cases have been operationalized as extreme cases on the residual of a regression predicting the outcome based on the treatment and any control variables (Seawright and Gerring 2008). Thus, deviant cases involve extreme selection on a regression-adjusted version of the dependent variable. Seawright (2016) has argued that deviant cases are better than extreme cases on the dependent variable because residualizing removes variance explained by already-measured confounders. Could a similar logic apply to the independent variable?

Extreme cases on the main causal variable are attractive for in-depth study for three key reasons, explained in depth in Seawright (2016). First, because we typically expect causal effects to increase in response to dosage, extreme cases on the independent variable should on average have unusually large causal effects, thereby facilitating qualitative study of the causal pathway between the independent and dependent variables. Second, if there is measurement error on the independent variable, the sources of error are likely to be most extreme and hence most qualitatively visible in cases with very high or very low scores. Third, confounding variables by definition have to be statistically connected to the main independent variable, and are therefore most likely to be prominent in cases with unusual values on X. It seems plausible that residualizing the treatment variable would maintain and perhaps enhance these advantages by removing already-known factors from the analysis.

Before we can consider this possibility, however, we must first clarify what it would mean to residualize the independent variable. A standard regression does not produce anything like a residual for the treatment variable. However, the literature on statistical causal inference

has long discussed a relevant idea: the propensity score (Rosenbaum and Rubin 1983). For a binary treatment variable, the propensity score is the probability that the treatment takes on a value of one, given the observed scores on all the control variables. For continuous treatment variables, using a density function in place of a simple probability yields the generalized propensity score (Hirano and Imbens 2004).

The mean of a generalized propensity score is the center of probability mass for the treatment variable — as is the propensity score itself for a binary treatment. Thus, X_i minus the mean of the (generalized or simple) propensity score measures the distance of the actually-observed treatment for case *i* and the expected value of the treatment for that case. This distance, which we will refer to as propensity-adjusted *X*, measures how surprising a case's observed treatment is, and in which direction the surprise occurred.

A propensity-adjusted version of the independent variable is a close analogue, for case-selection purposes, of the residual from a regression of the main dependent variable in the independent variable and the controls. That is to say, it captures the component of the variable that is surprising and novel in light of the available information. Extreme cases on the propensity-adjusted version of the *X* variable are cases that have unexpectedly high or low values, given the available information about all the measured confounders.[1]

To make things concrete, suppose that the main *X* variable has a positive correlation with each of the measured control variables. Then, cases with surprising causes will be extreme cases on the propensity-adjusted version of *X*, i.e., those in which the *X* variable and the control variables are all simultaneously far from their means — but in opposite directions. If *X* is unusually positive in a given case and the control variables are all unusually negative, then

(given a set of positive correlations across the entire sample) that case will be extreme on the propensity-adjusted version of *X*, and hence counts as having a surprising cause.[2]

There is some discussion of paired selection of case studies based on propensity scores, with the intention of achieving something like statistical control (Weller and Barnes 2014; Nielsen 2016). The case-selection proposal analyzed here differs fundamentally from the existing alternatives. The goal of case selection is essentially different. Existing approaches use propensity scores and matching to directly facilitate causal inference by reducing or eliminating the influence of known confounding variables on pairwise comparisons. By contrast, the propensity-adjusted approach discussed here helps with discovery by identifying cases that are surprising in certain ways given current knowledge. This has important implications. Because they are trying to help achieve direct causal inference, matching-based methods of case selection are vulnerable to criticism on the basis of imperfect matching, omitted variables, and other specification problems. Discovery-based case selection using propensity scores, in contrast, does not assume a fully correct specification and therefore does not face these potential critiques. In particular, the overall research design assumes that there probably are issues of measurement error and confounding in the original regression model. The process could potentially go wrong if post-treatment variables are included in the starting regression – a potential issue that will receive additional attention below.

Rather, the assumption here is simply that the model generating the propensity scores adequately captures the current state of knowledge. If the propensity scores do a good job of representing what is currently known about the causes of the independent variable, then anything odd or unexpected about the selected cases is likely to represent a step forward in terms of knowledge; if the model leaves out important ideas that are already known, there is a significant

risk that the cases will simply reemphasize those already-known ideas. In other words, for the present approach, omitted variables and other misspecifications are fine, so long as they reflect scholarly consensus. The goal here is to select cases that make it easy to discover such mistakes – and as long as case selection highlights cases where previously unknown confounders and so forth are important, then the project is a success. This has an important further implication: scholars engaged in this kind of research will be most productive if they enthusiastically search for findings that violate their prior expectations.

2. Practical Advantages of Selecting Cases with Surprising Causes

In comparison with selecting extreme cases directly on *X*, the propensity-adjusted version involved in the surprising-causes strategy has some intuitive attractions. First, it is less likely to select cases where all variables are simultaneously extreme in the same direction. Cases that suffer from over-determination of the outcome — in which essentially every possible causal process related to the outcome is active to the utmost degree — can be messy to sort out via process tracing and other forms of within-case analysis (George and Bennett 2005).

Second, the set of cases with surprising causes adjusts dynamically in response to accumulated knowledge as part of a multi-method research cycle. Cases with the most extreme positive and negative scores on the independent variable remain fixed throughout a research trajectory, unless cases are added to or subtracted from the relevant population or major sources of measurement error are discovered. In particular, it is a disappointing feature of extreme cases on *X* that finding out about and incorporating a measure of a new confounding variable does nothing to change which cases are extreme on *X*. Thus, after a single round of case-study analysis, this case-selection method becomes somewhat inert. This is a practical advantage that

cases with surprising causes share with deviant cases, and that sets these two methods apart from most competitors within the case-selection literature.

The list of cases with surprising causes, by contrast, can change as important new control variables are added. After all, those variables may render predictable cases whose score on the independent variable was previously surprising, letting new cases emerge at the top of the case-selection hierarchy. Thus, using a propensity-adjusted version of X for case selection facilitates multi-stage multi-method research cycles in which new confounders are iteratively discovered and successive case studies incrementally clarify the nature of the relationship. This allows multi-case studies to develop sequentially, thereby increasing the probability that each additional case adds new information to the overall study.

Third, this case-selection strategy meets the goal of pathway cases (Gerring 2007) without the uncomfortable necessity to engage in simultaneous selection on the cause and the outcome. Pathway cases are intended to be cases where "the causal effect of X_1 [the treatment] on Y [the outcome] can be isolated from other potentially confounding factors" (Gerring 2007: 238). That is to say, in proposing this case-selection rule, Gerring sought to meet a need for finding cases in which the causal pathway from the treatment to the outcome would stand in as much contrast as possible to any causal pathways linked to confounders or other measured causes. Specifically, the goal is to select cases for which, in a standard mediation framework (Imai et al. 2010), the mediation effect of the treatment through an as-yet-unknown pathway is maximally different from the effect of any effects from known confounders. By selecting cases in which the treatment is as unrelated to known patterns of confounding as possible, surprising causes provide just this, ensuring that any causal pathway connected with the treatment is as extreme as possible

while also selecting for situations in which pathways linked to other causes are working in the opposite causal direction.

The case-selection technique proposed here also has some conceptual resemblance to George and Bennett's (2005: 121) concept of "least-likely cases" as cases where "the independent variables in a theory are at values that only weakly predict an outcome or predict a low-magnitude outcome." The major distinction is that cases with surprising causes are those in which the independent variables connected with rival theories and other confounders only weakly predict the main causal variable, rather than the outcome.

Separating the effects of known alternative explanations from those of the main causal variable of interest helps process tracing by making the observable implications of those hypotheses as different as possible. In the language of Bayesian process tracing, when alternative explanations point in the opposite direction from the main explanation, then observations predicted by the main explanation will have an unusually large likelihood ratio for the main explanation (Fairfield and Charman 2017) – and thereby will increase the amount that can be learned by close analysis of the case.

3. An Argument for the Efficiency of Cases with Surprising Causes

While the above considerations provide a set of practical and intuitive rationales for selecting cases with surprising causes in multi-method research, a brief direct argument about the efficiency of this case-selection strategy as a tool for discovery is nonetheless necessary. Specifically, it is important to ask whether there are reasons to believe that propensity-adjusted extreme cases on X are better than unadjusted extreme cases for the common case-study goals of discovering sources of measurement error, learning about the causal pathway between the

treatment and the outcome, and discovering as-yet unmeasured confounders. These discoveries should not be expected to generalize statistically to some broader population; surprising cases are, after all, unusual by design. However, it is reasonable to generalize discoveries from case selection to a broader universe through theoretical generalization (Maxwell and Chmiel 2014), in which additional cases are tested (or regressions conducted, etc.) to see if they reinforce the new discovery, gradually leading to a revised theoretical framework.

Furthermore, discovery-oriented research of the kind added here can serve important complementary roles as a robustness check or as a means of refining a regression. Regression-type research, after all, typically assumes that none of the kinds of issues discussed here are present in the data and model being analyzed. Discovering one or more problems points to avenues for strengthening the statistical model, and thus offers a way of improving the statistical analysis that is hard to achieve in purely quantitative research. By contrast, seeking new discoveries of this sort and failing to find them adds some confirmatory weight to the original statistical analysis. Thus, case-based research focused on discovery can serve a wide range of other multi-method research purposes.

Following the lead of Seawright (2016), we will briefly consider each of these scenarios in light of standard econometric models. We will measure the efficacy of a case-selection procedure in terms of its correlation with the quantity that we seek to discover. The thought is that the more predictably related a case-selection process is to a given unknown factor, the more likely a scholar is to uncover that factor via the case-selection process. An errors-in-variables model will be used for considering measurement error, a mediation model for causal pathways, and an omitted-variable model for unmeasured confounders.

The extent to which a particular case has a surprising cause will be formalized as follows. Suppose that an auxiliary regression has been run with X as the dependent variable and all the measured potential confounders in the main regression as control variables. From this auxiliary regression, form the fitted values for X by the ordinary procedure, and label those fitted values as $\hat{X}$. Then, the propensity-adjusted version of X is $X - \hat{X}$, and the most extreme cases are those with an absolute value as far as possible from zero on that scale. Thus, surprising causes are found in cases with a large absolute value of $X - \widehat{X}$. We will refer to $X - \widehat{X}$ as X^*

The correlation between X and an unknown quantity W is thus the covariance of ($\hat{X} + X^*$) and W, divided by the standard deviation of ($\hat{X} + X^*$) times the standard deviation of W. Because covariance is a linear operator, the numerator is equivalent to the covariance of $\hat{X}$ and W plus the covariance of X^* and W.

By comparison, the correlation between X^* and W is the ratio of the covariance of X^* and W to those two variables' standard deviations. As long as the auxiliary regression used to form $\hat{X}$ has non-zero explanatory power, the denominator will be smaller than for unadjusted X, because $\hat{X}$ is removed from the standard deviation. Hence, the correlation between X^* and W will definitely be bigger than the correlation between X and W when the covariance of $\hat{X}$ and W is zero and the auxiliary regression has non-zero explanatory power; there are other, more complicated circumstances in which the X^* correlation is stronger, but they are not needed for this essay.

In other words, whether it is better to choose extreme cases on propensity-adjusted X or on unadjusted X, everything comes down to the properties of $X\hat{}$. When $X\hat{}$ contains no information about the problem of interest, then the adjusted version of X which discards $X\hat{}$ will be superior because it will select cases based only on the relevant information; unadjusted X can

only be superior for case selection in circumstances where $\hat{X}$ can meaningfully predict the problem of interest.

Thus, for measurement error, the key question is whether sources of measurement error in X are likely to be related to, or unrelated to, $\hat{X}$. Under the standard assumption that measurement error is independent of other observed covariates, it is evident that $\hat{X}$ is also independent of the measurement error. After all, $\hat{X}$ is a linear combination of the other observed covariates — so if they are independent of the measurement error, then $\hat{X}$ must be, as well. We can therefore conclude that the relevant portion of X is X^*, and thus that the covariance between $X^*$and the measurement error in X will in expectation equal the covariance between unadjusted X and its measurement error. Hence, nothing is lost in the numerator of the correlation by using the propensity-adjusted version of X, and therefore any reduction in the denominator will produce a stronger correlation for propensity-adjusted X. Therefore, selecting extreme cases on the propensity-adjusted version of X is at least as good in expectation, and often better, than unadjusted X for discovering sources of measurement error in the treatment variable. In the more general case where the direction and magnitude of measurement error may depend on the values of the control variables, nothing definitive can be said; scholars should thus pay special attention to the prospect of systematic measurement error related to the controls. Such error will not always defeat case selection based on surprising causes, and sometimes will be easily discovered via this approach. However, certain patterns of systematic measurement error may be able to hide from this method.

For causal pathways, let us follow a standard mediation analysis framework by suggesting that the causal pathway of interest is the causal effect of X on Y via a mediating variable M, net of any effect on M from confounding variables. Let us define the unconfounded

portion of M, caused only by X and perhaps random influences, as M_X. Further, let us suppose that the goal of case-study research is to discover M_X, given that it is the purest representation of how X causes Y independent of other variables. Given that M_X is by construction independent from the confounding variables in the causal scenario, it will also have zero covariance in expectation with $\hat{X}$, which is after all constructed as a linear combination of those confounding variables. In qualitative language, this shared variance involves situations in which the outcome is over-determined, thereby offering little advantage for case-study research inquiring into causal pathways.

Because $\hat{X}$ has zero expected covariance with M_X, we can conclude that the covariance with X^* and M_X will in expectation capture the full covariance between X and M_X. Because, once again, the variance of X^* is smaller than the variance of X, we can conclude that the correlation of X^* and M_X is at least as good as the correlation involving unadjusted X. That is to say, the propensity-adjusted version of X is by construction statistically independent of the measured confounding variables, and therefore extreme cases on this propensity-adjusted version of the treatment will be more strongly connected with the variables on the pathway between X and Y (and less strongly connected with spurious pathways involving confounders) than extreme cases on unadjusted X. This argument critically depends on the idea that the control variables are not post-treatment, i.e., caused by X. Hence, theoretical care on this point is critical, as is qualitative attention to verify this key assumption.

Finally, when the goal is to discover as-yet unknown confounders, the propensity-adjusted version of X involved in the surprising-causes strategy is once again superior. This is because $X\hat{}$ reflects the connection between X and *already-measured* confounders; new confounders, if they exist, must be related to $X - X\hat{}$, or else they are simply redundant to the

existing set of control variables. This argument will go wrong to a greater or lesser extent if some of the control variables are post-treatment and constitute part of the causal pathway connecting *X* to *Y*; in that kind of situation, conditioning on the control variables throws away part of the relevant variance of *X* and can thus make it more difficult to discover confounding variables. Hence, it is important to devote theoretical attention to eliminating post-treatment control variables, and also to probably spend some qualitative time exploring causal pathways to eliminate post-treatment controls. With that caveat, there is indeed a statistical argument in favor of using extreme cases on propensity-adjusted *X* in place of unadjusted *X* for multi-method designs that seek information about sources of measurement error, the nature of the causal pathway from *X* to *Y*, or new control variables that could improve a causal inference by eliminating sources of confounding.

In each of these scenarios, it is worth emphasizing the epistemological goals and requirements at hand. It is entirely implausible that the regression which serves as a starting place for case selection will be known to be causally correct. As many scholars have argued, causally true regressions are exceptionally rare at best in social-science observational studies (Freedman 1997; Berk 2004). Furthermore, even if the initial regression were in fact a faithful representation of the data-generating process, it is hard to see how scholars would come to know that fact.

Hence, it is unreasonable to adopt an epistemological stance in which case selection and subsequent analysis are seen as testing the hypothesis that the initial regression is true. It is more reasonable to start with the assumption that the initial regression is distorted by potentially many analytic errors and sources of bias. The goal, then, is to discover and at least partially resolve one or a few such errors. While this will not in itself deliver a true and final causal inference, it will represent a contribution toward the eventual construction of more stable knowledge. A cycle

from error-laden regression analysis to selecting cases with surprising causes, onward to qualitative case analysis, and finally back to a new, error-laden but improved regression represents a strategy of "causal inference for mortals." That is to say, it offers the potential to contribute to a long-term dialogue that may eventually stabilize causal inference in observational contexts, without requiring superhuman levels of prior causal knowledge.

4. Application: Labor Unions and the Rise of State Employment Laws

How does surprising-causes case selection and the concomitant cyclical version of multi-method research work in practice? This section offers an application that demonstrates the utility of this case-selection approach in particular and an iterated, integrated multi-method design more generally. While the findings of this analysis regarding the politics of employment law are, in our view, of interest in their own right, this section puts unusual textual emphasis on the overall structure of the research cycle behind those findings. After all, the nature of integrative multi-method research, as well as the specific advantages of selecting cases with surprising causes, stands out most clearly in such a research process-oriented discussion.

There is no shortage of scholarship on the "ossification" of American labor law and the decline of labor unions in the U.S. (e.g., Goldfield 1987; Estlund 2002; Rosenfeld 2014). Less discussed, but no less important, is the burgeoning of state-level *employment law* during precisely the same period (Galvin 2019).[3] Indeed, it is a remarkable coincidence that while labor law withered, employment law flourished: at the same time that national union density declined from 30 percent to 11 percent, state legislatures enacted thousands of new employment laws

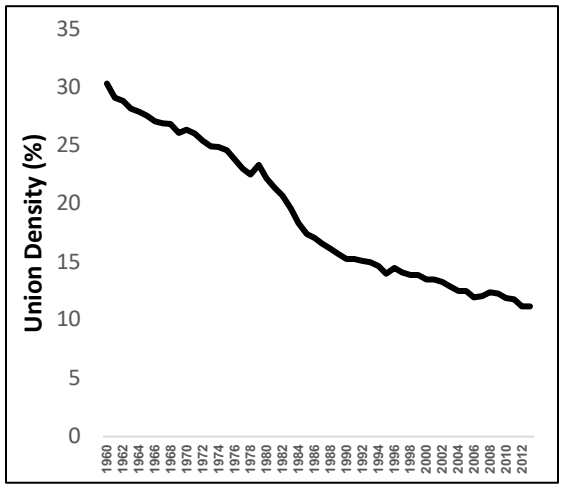
regulating the same areas of workplace relations that might have otherwise been addressed through collective bargaining (see **Figures 1** and **2**).

Scholars have hypothesized a causal connection between these trends. Legal scholar Theodore St. Antoine, for example, posits that "part of the growth we have seen in employment law, as distinguished from labor law, is attributable to the decline of organized labor. Government has had to step in to fill the vacuum" (2004, 495-6). Just as the Wagner Act was designed to "rectify the failings" of the prior *Lochner*-era system, the growth of employment law has been depicted as "a response to the decline of collective bargaining and the regulatory vacuum it has left behind" (Estlund 2002, 1529; Sachs 2008; Stone 1992; Summers 1988).

The notion that state legislatures turned to employment laws to protect an increasingly non-unionized, vulnerable workforce has a certain intuitive appeal. But upon closer inspection, the relationship between the two developments appears considerably more complex. Moving from national-level trends to within-state relationships, a simple initial regression analysis shows the opposite relationship: contrary to the hypothesis that employment laws were enacted as *substitutes* for union protections, we find that the steeper the state's union density decline, the *fewer* the employment laws enacted (**Table 1**). Union density plummeted everywhere, but in states where unions were better able to withstand the forces of decline, legislatures enacted more employment laws. For every percentage point of union density lost, states enacted about 6 fewer employment laws (**Figure 3**).

Figure 1

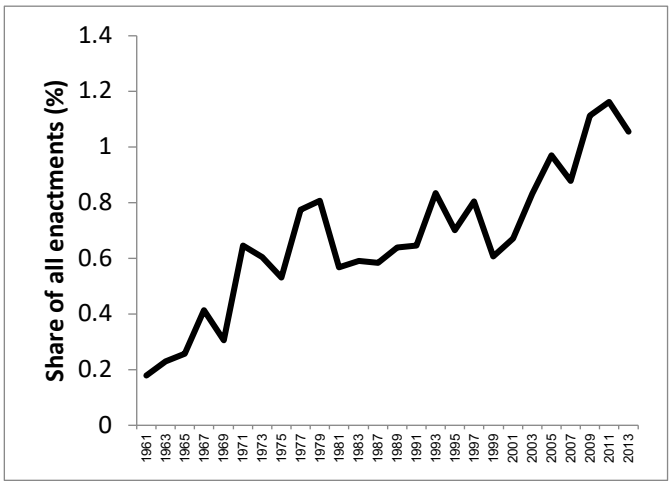
National Union Density Decline, 1960-2013



Source: Hirsch and Macpherson 2017

Figure 2

State Employment Law Growth, 1960-2013



Source: Galvin (2019)

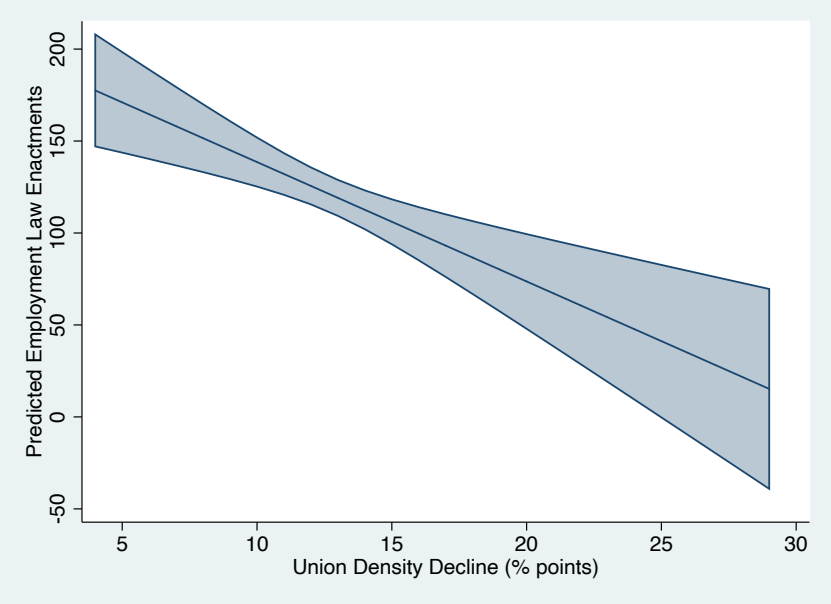
Table 1: Relationship between State-level Employment Laws and Union Density, 1974-2014

	(1)
Union Density Decline	-6.491***
	(1.634)
Baseline Union Density	5.916***
	(1.024)
Legislative Productivity	0.00475***
	(0.000690)
CA	173.1***
	(40.63)
Constant	-20.59
	(21.61)
Observations	50
Adj R-squared	0.7781

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Dependent variable is the total number of employment laws enacted by state. *Union Density Decline* is the percentage point change in state-level union density, where a higher value indicates greater decline. *Base Union Density* is average union density in the five years prior; *Legislative Productivity* is the total number of state laws enacted over the same period; *CA* dummies for the extreme outlier California.

Figure 3: Predicted Employment Law Enactments (by Union Density Decline)



Within states, then, it appears that the deeper the unions' penetration of the workforce and the more stable the rate of union density, the larger the role for the state in regulating the employment relationship. While this, too, makes some intuitive sense, the nature of this relationship is a mystery. In which direction do the causal arrows point? Is the relationship spurious or an artifact of measurement? Were declining unions mere bystanders as state legislators enacted employment laws to compensate for the "regulatory vacuum" left in the unions' wake (Estlund 2002, 1529)? Or were unions instrumental in the laws' enactment, such that stronger unions helped to construct more robust employment law regimes? And what of reverse causation: did the enactment of employment laws contribute to union decline by providing for free what workers might have otherwise obtained through their unions, thereby reducing the incentive to unionize? Or was the relationship endogenous: did unions contribute to their own decline by pushing for employment laws that reduced their own necessity? Or did unobserved factors cause both union decline and employment law's growth? Is the raw unionization rate even the appropriate measure for the causal factors at work here? At present, we cannot say.

To begin clarifying the nature of the relationship between union decline and employment law growth, case studies are particularly useful. But how to select cases? An unadjusted extreme-case selection strategy would be problematic, since factors ranging from each state's industrial mix to employment trends, to demographic change, to the political strength of business might explain both a state's union density rate and its legislative attention to employment law. Eeconomic confounders threaten to muddy the waters further still. Can we do better by selecting cases with surprising causes? As discussed above, the propensity-adjusted case-selection strategy can help to highlight problems with conceptualization and measurement of the treatment variable, identify confounding variables, and explore causal pathways.

Conceptualization and Measurement

The model we use to generate cases with surprising causes need not be complex or even creative: we simply want to begin with a reasonable representation of existing theory and start adding innovations from there. As such, let us start with the basic "government substitution" model examined by preeminent labor economist Richard Freeman (1986). First proposed by Neumann and Rissman (1984), the government substitution hypothesis posits that the state's direct provision of benefits, protections, and rights to workers undermines their incentive to unionize by providing for free what workers might otherwise get through their unions (Neumann and Rissman 1984; Moore and Newman 1988; Moore et al. 1989; Coombs 2008; Freeman 1986; Hauserman and Maranto 1988; Bennett and Taylor 2001). Freeman used cross-sectional data to estimate the relationship between union density and protective employment legislation enacted in all 50 states. Although he found no support for the notion that protective legislation had "adverse feedback effects on union density," he found strong empirical support for the "reverse causal

link," that "more highly unionized states are, indeed, more likely to pass protective legislation" (265). Building on Freeman's model and adding the two controls from our initial descriptive model above, we estimate:

(1) Employment Laws = $a + b$ *Base Union Density* $+ c$ *Union Density Decline* $+ d$ *MFG* $+ e$ *logMFGWage* $+ f$ *logEMPLCHG* $+ g$ *Unemployment* $+ h$ *RTW* $+ i$ *Legislative Productivity* $+ j$ *CA* $+ u$[4]

As shown in **Table 2**, less union density decline and a higher baseline do indeed predict more employment law enactments, even when controlling for key economic and political variables. Right-to-work states are associated with fewer employment law enactments, while greater legislative productivity and a lower unemployment rate predict more employment laws.

Table 2: Government Substitution Hypothesis Model

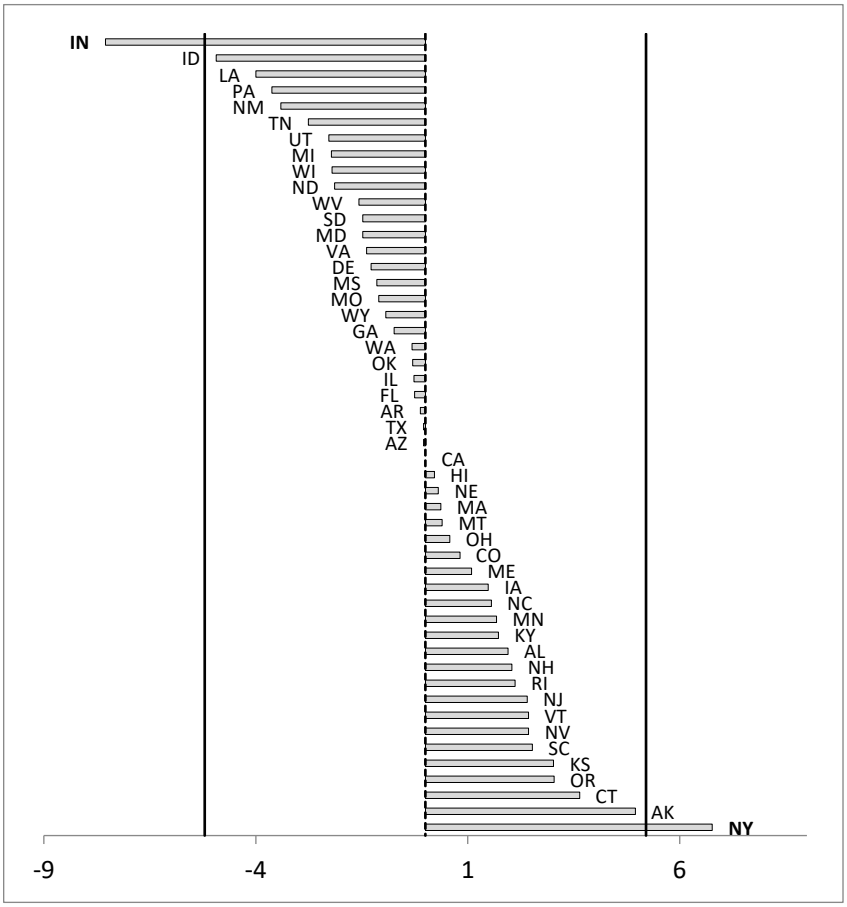
	(1)
Base Union Density	4.707***
	(1.198)
Change in Union Density	-4.546**
	(1.836)
MFG	66.41
	(97.81)
logMFGWage	-0.759
	(5.449)
logEMPLCHG	14.70
	(12.27)
Unemployment	-9.904**
	(4.896)
RTW	-34.07**
	(13.34)
Legislative Productivity	0.00547***
	(0.000788)
CA	159.2***
	(40.05)
Constant	-22.34
	(91.17)
Observations	50
Adj R-squared	0.798

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

These results lend additional support to Freeman's findings and suggest a robust relationship between the two trends. For our purposes, however, the more important point is that the control variables in this model can be used to select cases with surprising causes. This step produces the following values (**Figure 4**):

Figure 4: Propensity-Adjusted Extreme Values on Change in Union Density



Note: vertical lines represent +/- 2*SD from mean

Union density declines in Indiana (-7.6 residual) and New York (+6.8 residual) are least-well explained by the covariates: the residuals on both states are more than 2 standard deviations (5.1) from the mean (zero). Indiana's union density declined far more than expected and New York's

declined far less than expected. Why might these two states have emerged as our propensity-adjusted extreme cases? At first blush, the answer is not obvious. Let us therefore look more closely at the data generation process for our key treatment variable and consider its relation to unionism in Indiana and New York.

Estimated annually using the monthly household Current Population Survey (CPS), the standard measure of union density reports each state's share of the nonagricultural workforce belonging to a union (Hirsch and Macpherson 2017). As Hirsch and Macpherson point out, however, this commonly used measure combines private and public sector union members in a single estimate of union membership in each state. The authors also report private and public union density measures separately. Can those more fine-grained measures of union density illuminate why Indiana and New York exhibit unusual values? Examining these disaggregated measures, we see that although every state saw significant declines in private sector union density between 1983-2014,[5] Indiana hemorrhaged private sector union members (second-greatest decline in the nation) while New York's decline was about average **(Figure 5)**. New York, meanwhile, had the highest public sector union density in the nation – indeed, its *baseline level* of public sector union density was more than twice the national average, and did not vary much over the years – while Indiana's public sector union density was only slightly below average **(Figure 6).** Indiana, then, appears "extreme" in its private sector union density decline and New York appears "extreme" in its baseline level of public sector union density. By pulling out these two cases for further examination, the propensity-adjustment procedure has pointed us toward measurement issues with potentially important theoretical and empirical implications. It still leaves open the question, however, of where we should we direct our attention: to private

sector union density, public sector union density, or both? And to the state's *change* in union density or to its *baseline* level?

Figure 5: Private Sector Union Density

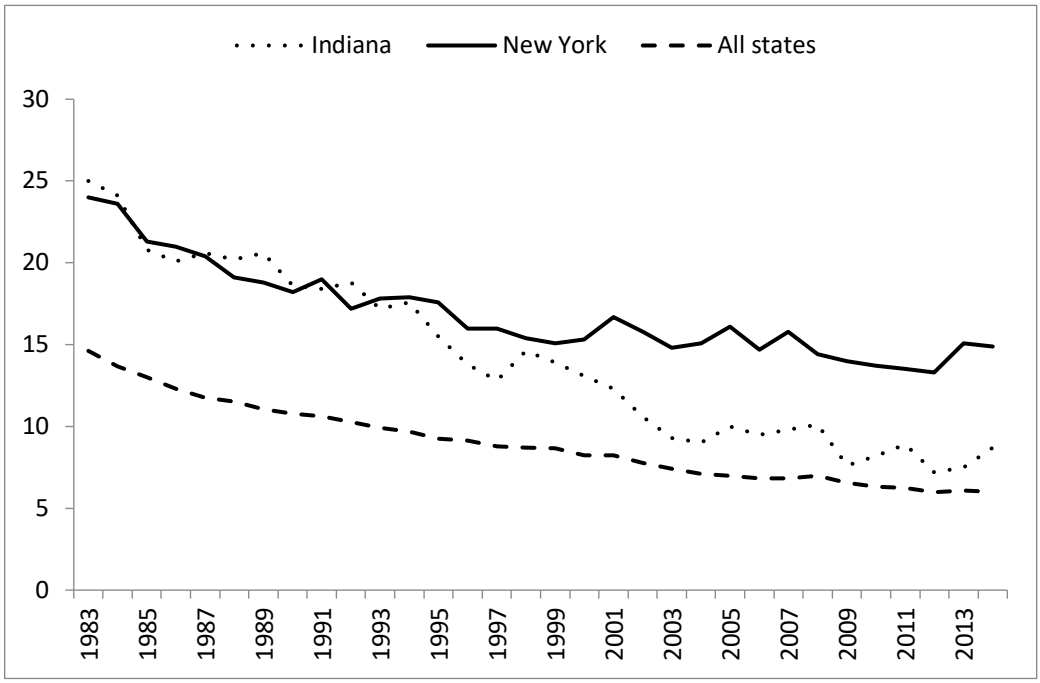
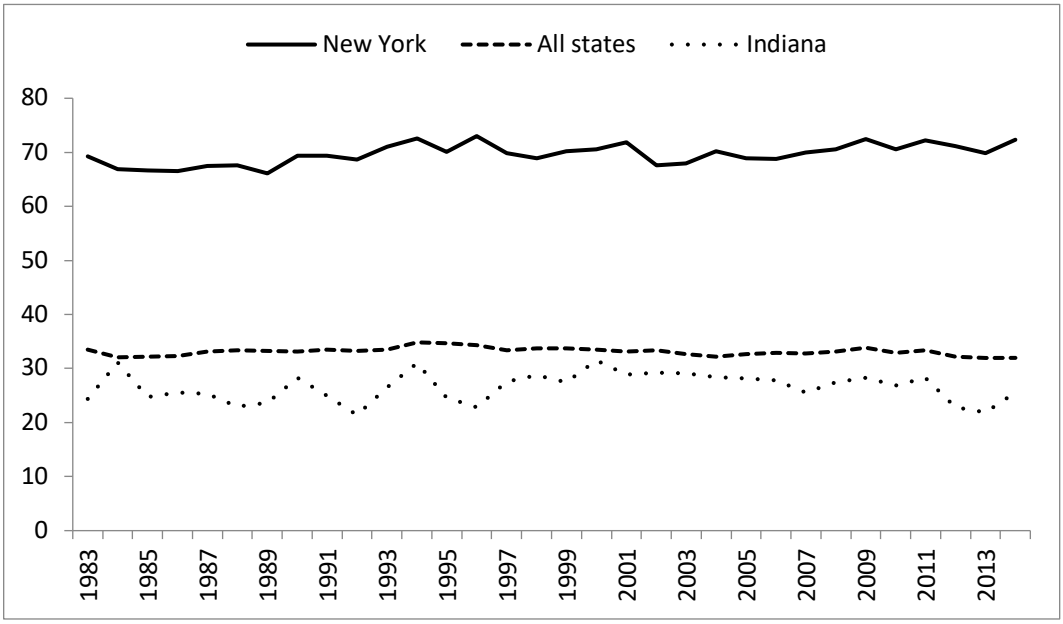


Figure 6: Public Sector Union Density



Rather than forge ahead with our initial, noisy measure of union density, let us disaggregate the measure into its private and public-sector component parts, incorporate them both into a revised regression analysis, and take our cues from the results. In **Table 3,** union density is disaggregated into four parts: baseline (1983) and percentage point change in both private and public sector union density. As Models 1 and 2 reveal, when examined independently, both private and public union density rates are positive and significantly related to employment law enactments. Including the two measures together (Model 3), however, yields a curious result: while the baseline level of public sector union density in 1983 is highly significant, the statistical significance of private sector union density washes out. Public sector union density, it would appear, better predicts state employment law enactments.

Table 3: Disaggregated Union Density Measures

	(1)	(2)	(3)
Baseline Private Sector Union Density	3.336*		-0.979
	(1.940)		(2.324)
Change in Private Sector Union Density	-3.815		1.227
	(2.855)		(3.172)
Baseline Public Sector Union Density		1.189***	1.307***
		(0.344)	(0.451)
Change in Public Sector Union Density		-0.709	-0.796
		(0.689)	(0.738)
MFG	30.79	-31.38	-47.87
	(105.4)	(90.57)	(102.6)
logMFGWage	-1.887	-1.914	-1.783
	(5.324)	(4.481)	(5.013)
logEMPLCHG	11.48	16.13	16.39
	(12.80)	(11.40)	(12.10)
Unemployment	-7.443	-2.990	-2.265
	(4.806)	(4.189)	(4.819)
RTW	-41.41***	-23.62*	-23.17
	(12.65)	(13.05)	(13.82)
Legislative Productivity	0.00638***	0.00575***	0.00567***
	(0.000938)	(0.000824)	(0.000903)
CA	129.6***	132.1***	133.5***
	(38.87)	(35.28)	(36.29)
Constant	38.44	-19.65	-23.65
	(84.40)	(78.76)	(81.28)
Observations	50	50	50
Adj R-squared	0.709	0.762	0.750

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

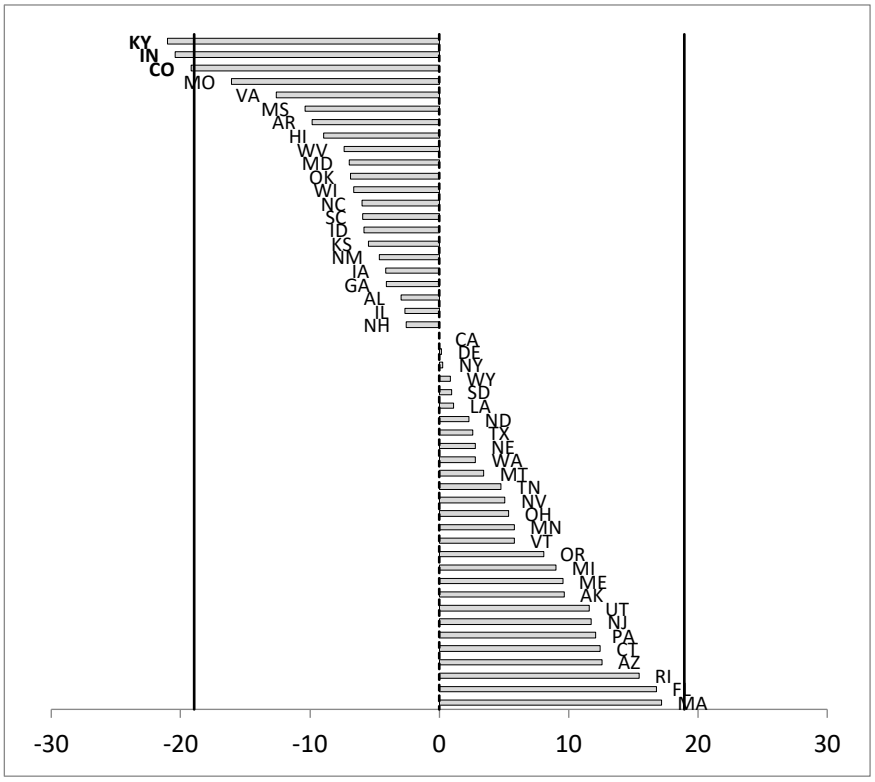
In sum, looking at the two cases with the most surprising causes has pointed us toward making a simple but important change to the measurement of the key explanatory variable. It prompted us to examine more closely the composite measure of union density, which revealed a potentially important distinction between public sector and private sector union density. The improved model also revealed that the *change* in union density matters less than the baseline *level* of union density. These findings are particularly useful for case study purposes, as they suggest that more value may be gained by focusing attention on public sector unions like AFSCME, NEA, and AFT, for example, than on private sector unions like the Teamsters, UFCW, and USW. They also prompt us to examine a wider range of potentially relevant institutional factors in subsequent analyses: for whereas private-sector unions are governed by national labor law's uniform collective bargaining rules, public-sector unions are governed by state-level collective bargaining laws, which vary substantially across states. Thus, variation in states' collective bargaining laws, the activities of public sector unions, and the baseline rate of public sector union density are now matters of substantial theoretical interest in the investigation.

It is worth noting that the potential significance of state labor laws and public sector unions completely escaped the attention of scholarship on the "government substitution" hypothesis discussed above, which relied on standard regression analysis to test existing theory. Using the propensity-adjustment procedure to refine our conceptualization and measurement of the union density treatment variable, in other words, has not only altered our model, but it has opened new lines of inquiry.

To identify the highest-leverage case studies for exploring these questions further, let us run the propensity-adjustment procedure again—this time, changing the main "treatment"

variable to *Baseline Public Density* while keeping the measures of private sector union density measures on the right-hand side. As shown in **Figure 7**, this procedure reveals that New York is no longer extreme, but Kentucky (-21.00) and Colorado (-19.19) join Indiana (-20.40) as extreme propensity-adjusted cases that have lower than expected public sector union densities.

Figure 7: Propensity-Adjusted Extreme Values on Baseline Public Sector Union Density



Note: vertical lines represent +/- 2*SD from mean

Confounders

Although the conceptualization and measurement of our main treatment variable has progressed, we are still left with considerable uncertainties about the theoretical significance of the regression model. Are the results meaningful, or are they hopelessly distorted by confounding variables or even reverse causation? Next, let us scrutinize our cases with surprising

levels of public sector unionization (Kentucky, Indiana, and Colorado) to look for potential answers to these questions.

Although the growing political science literature on public sector unions has sparked a number of debates, there is little disagreement that state-level labor laws – especially right-to-work and collective bargaining laws – powerfully determine the size and political clout of public sector unions (Anzia and Moe 2016; Hertel-Fernandez 2018; Flavin and Hartney 2015). Right-to-work laws, authorized by the federal Taft Hartley Act of 1947, allow nonunion members to avoid paying union representation fees despite receiving union representation. These laws have been criticized as creating free-rider problems that can bankrupt unions. Eighteen states passed right-to-work laws in the 1940s and 1950s, all in the South, Southwest, and Great Plains regions. By 2019, eight more states had become"right-to-work," including several in the Midwest (see **Table 4**). The *RTW* dummy variable in the model above indicates whether a state had a right to work law on the books prior to the period under examination (1983-2014).

Of our three extreme states, neither Colorado, Indiana, nor Kentucky had enacted a right to work law before 1983. However, Colorado passed a strikingly similar law in 1943 (Labor Peace Act) which has been called a "precursor" to Taft Hartley that "clearly influenced" the latter's design (Hogler and Shulman 1999, 884-7), and both Indiana (2012) and Kentucky (2017) are among a handful of "late adopters" (**Table 4**). Whatever caused these three states to be unusual – unusually early (CO) or unusually late (IN and KY) -- may also explain their extreme propensity-adjusted values.

Collective bargaining laws come in all shapes and sizes: collective bargaining can be prohibited; authorized but employers not required to bargain; workers can have the right only to present proposals; employers can be required only to meet and confer; the employers' duty to

bargain can be implied or explicit (Valletta and Freeman 1988). Complicating matters further still, different rights and duties can be extended to different categories of workers (state employees, local police, local firefighters, local teachers, and/or other local employees). Let us consider each state's experience with both right-to-work laws and collective bargaining laws.

Kentucky. Although "right to work" was a frequent topic of debate and a number of counties enacted local right-to-work laws, Democratic control of at least one legislative chamber from 1866-2016 prevented any statewide law from passing (Gerth 2015). When Republicans gained unified control of state government for the first time in the state's history in 2017, right to work was one of the first laws enacted (Wilson 2017). Partisan control of the state legislature thus appears to have been pivotal.

With respect to collective bargaining laws, despite its relatively large public sector, Kentucky is "one of a handful of states—almost exclusively in the old Jim Crow South—that denies the right of collective bargaining for state employees" (Hennen 2015). Although the Kentucky Supreme Court has ruled that public employees are permitted to unionize, employers are not compelled to recognize those unions or bargain with them. The only exceptions include local police and firefighters, with whom local governments must bargain (Valletta and Freeman 1988). It is perhaps unsurprising, then, that Kentucky's average public sector union density ranked 37th out of 50 states.

Table 4: Timing of State Adoption of Right to Work Laws

Early adopters (1940s-1950s)	Middle adopters (1960s-1980s)	Late adopters (2000s)
Alabama (1953, 2016[c])	Idaho (1985)	Kentucky (2017)
Arizona (1946[c], 1947)	Louisiana (1976)	Indiana (2012)
Arkansas (1944[c], 1947)	Wyoming (1963)	Michigan (2012)
Florida (1943, 1968[c])		Oklahoma (2001[c])
Georgia (1947)		Wisconsin (2015)
Iowa (1947)		
Kansas (1958[c])		

Mississippi (1954, 1960[c])
Nebraska (1946[c], 1947)
Nevada (1952)
North Carolina (1947)
North Dakota (1947)
South Carolina (1954)
South Dakota (1946[c], 1947)
Tennessee (1947)
Texas (1947, 1993)
Utah (1955)
Virginia (1947)

Note: c-superscript indicates constitutional provision rather than statute

Indiana. Although Indiana enacted a right-to-work law in 1956, it was repealed in 1965 when Democrats regained control of state government. Indiana did not enact another right-to-work law until 2012, when the "Tea Party surge" of 2010 delivered Indiana Republicans their largest legislative majorities in decades (Cuarino 2012; Hansen 2010). As part of a nationwide conservative "revival" in which Republican lawmakers were said to be "dusting off the decades-old right-to-work legislation for a new millennium," Indiana became the first state in over a decade, and the first in the industrial Midwest, to enact a right-to-work law (Pugh 2012). This move did not surprise observers who viewed Indiana as the "most conservative of the Rust Belt States, and one where residents identify more strongly with those in southern and western states where right-to-work laws have been in place" (Grimm 2012).

Most public workers in Indiana have the right to bargain with their employers, but their employers are not required to bargain with them (except for public school districts, since 1974). In 1990, Democratic Governor Evan Bayh extended collective bargaining rights to state employees by executive order, and over the next fifteen years, almost 2/3 of state workers became union members (Dvorak 2010). Democrats introduced bills every session to enshrine these rights in law, but continuous Republican majorities in the state Senate blocked them from advancing. On Republican Governor Mitch Daniels's second day in office in 2005, he rescinded

Bayh's executive order (Greenhouse 2011). In the ensuing years, the share of state employees paying union dues dropped from 66 to 7 percent (Erickson 2011). As in Kentucky, then, partisan control of Indiana's state government seemed to play an important role in shaping the relevant laws in the context of the conservative ideological "revival."

Colorado. Ideological conservatism and GOP legislative control also helps to explain Colorado's peculiar labor laws. Although Colorado still technically lacks a right-to-work law, its Labor Peace Act of 1943 rendered the state effectively right-to-work years before Taft-Hartley – indeed, it has been called a "precursor" to Taft-Hartley. Mandating two separate elections, the Act requires a 75% supermajority in the second stage to establish agency fees after a union has already been elected by a majority vote. If the supermajority threshold is not met, the workplace effectively becomes right-to-work. The Act was enacted by Republican supermajorities evidencing a clear "ideological bent…vot[ing] conservatively, fairly consistently." At the time, union leaders called the Act "the most vicious piece of antilabor legislation passed by any legislature in the history of the state" (Hogler and Shulman 1999, 885-6).

Collective bargaining rights in Colorado are weak as well: although public teachers and firefighters can bargain collectively, employers are not required to negotiate with them and strikes are prohibited. Colorado has not enacted any other laws governing collective bargaining in the public sector, thus leaving most public workers without collective bargaining rights.

In sum, in all three states, opposition by Republicans controlling important veto pivots appears to have kept collective bargaining laws weak or nonexistent while propelling Kentucky, Indiana, and Colorado toward right-to-work (or quasi-right-to-work) status. The ideological conservatism of the three states also appears to have contributed to their relatively weak state labor laws. Two potential confounders thus emerge from the preceding analyses: partisan control

of state government and the states' ideological leanings. Given these states' peculiar histories with right-to-work, we might also consider recoding the RTW variable to include the adoption of right-to-work laws after 1983 as well as before, since states that have become right-to-work may be distinctly different from those that have not.[6] And given the fact that Colorado's unique labor law served as the model for the standard right-to-work laws enacted thereafter, perhaps it should be included in the RTW variable as well. Let us therefore include these two confounders, recode the right-to-work variable, and re-estimate the following model:

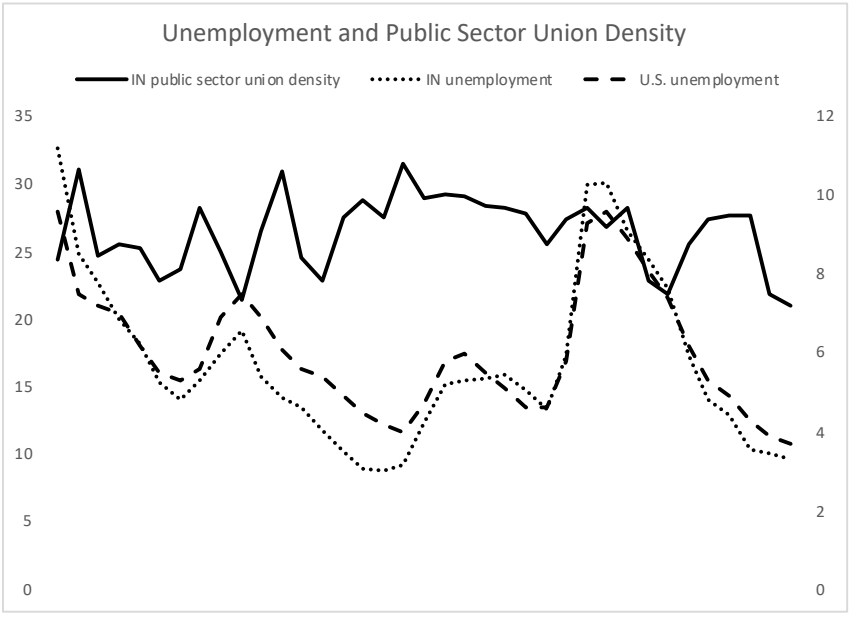
(2) Employment Laws = $a + b$ *Base Public Density* $+ c$ *Change in Public Density* $+ d$ *MFG* $+ e$ *logMFGWage* $+ f$ *logEMPLCHG* $+ g$ *Unemployment* $+ h$ *RTW* $+ i$ *Legislative Productivity* $+ j$ *CA* $+ k$ *MassEconLib* $+ l$ *DemLeg* $+ u$

where *MassEconLib* is the Caughey and Warshaw (2018) measure of state public opinion on economic matters as a proxy for ideology and *DemLeg* measures the total number of years of Democratic control of state legislative chambers.

The next iteration of the model **(Table 5)** continues to reveal a positive and statistically significant relationship between baseline public sector union density and the enactment of employment laws. The coefficient on our main treatment variable is only slightly larger but the RTW coefficient has increased in magnitude and is now significant at $p<0.10$ – as is the new proxy for ideology. *DemLeg* is not statistically significant, falling just under the $p<0.10$ level, perhaps reflecting the persistence of conservative state Democratic parties in the South throughout the first half of this period. The two new variables also appear to be confounders: regressing baseline public sector union density on *MassEconLib*, *DemLeg,* and covariates reveals that both are statistically significant at $p<0.05$ and $p<0.10$, respectively.

One question that may arise is whether any covariates are post-treatment, meaning they are an effect rather than a cause of *X*. If so, they are likely to be good predictors of *X* despite not being part of the assignment process, thereby potentially leading the propensity-adjustment process to inadvertently fail to identify cases that are good candidates for further discovery. As noted above, this calls for careful theoretical attention and qualitative analysis of causal pathways. So let us briefly consider whether this applies in one of the states considered above. First, take unemployment. Indiana's baseline public sector union density rate in 1983 was 24.5%. Thereafter, it hovered around its period average of 26.5%. Meanwhile, the unemployment rate in Indiana rose and fell in tandem with national recessions (**Figure 8**) but bore no apparent relationship to changes in Indiana's public sector union density ($p<0.66$).

Figure 8



Next, consider right-to-work (RTW). Could it be a consequence of the baseline rate of public sector union density? This seems unlikely too. As discussed above, Indiana's 2012 right-to-work law resulted from the "Tea Party surge" of 2010, which delivered Republican legislative majorities for the first time in decades. There is no evidence that Indiana's public sector union

density rate had anything to do with it. The reverse causal link is much more apparent: after the state became "right-to-work" in January 2012, its public sector union density rate fell from 28.3 in 2011 to 22.8 in 2012. When a superior court judge subsequently struck down the law, membership ticked up several points, only to fall again after the Supreme Court's *Abood* decision deemed all agencies fees unconstitutional. A similar pattern played out in Kentucky, another late RTW adopter. In other words, right-to-work appears to predict public sector union density, but not vice versa, which is what we want to see. As a potential confounder, it should be included in the model. Although space is limited here, each variable in the model could be examined in a similar manner to identify any potential post-treatment effects.

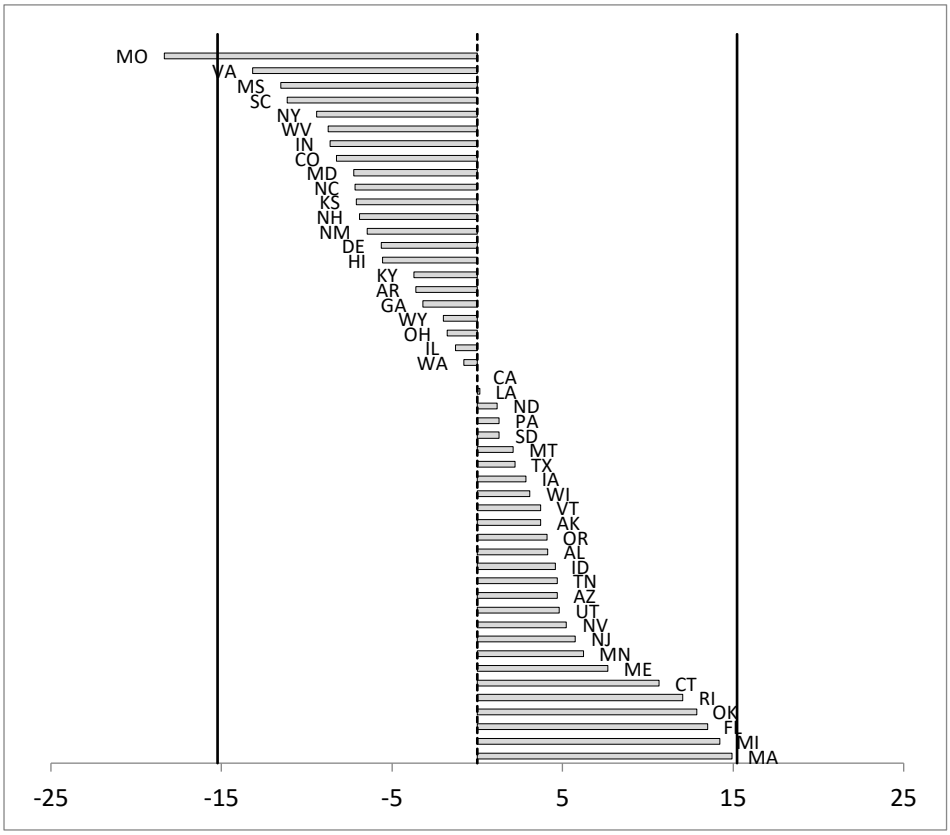
Table 5: Including Confounders

	(1)
Baseline Private Sector Union Density	0.272
	(2.553)
Change in Private Sector Union Density	-0.682
	(3.558)
Baseline Public Sector Union Density	1.317**
	(0.548)
Change in Public Sector Union Density	-0.342
	(0.884)
MFG	-21.77
	(106.0)
logMFGWage	3.093
	(5.291)
logEMPLCHG	12.38
	(12.72)
Unemployment	-2.130
	(4.885)
RTW	-33.22*
	(18.51)
Legislative Productivity	0.00547***
	(0.00100)
CA	115.5***
	(37.22)
MassEconLib	-97.05*
	(51.42)
DemLeg	1.004
	(0.620)
Constant	-107.4
	(87.01)
Observations	49
Adj R-squared	0.767

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

To identify surprising cases for further study, let us implement our propensity-adjustment procedure a final time **(Figure 9)**. Now, all previously identified extreme cases (New York, Indiana, Kentucky, Colorado) are no longer extreme, and Missouri emerges as the sole extreme case, with by far the most surprising (lower than expected) value of public sector union density (-18.36).

Figure 9: Propensity-Adjusted Extreme Values on Improved Model



Note: vertical lines represent +/- 2*SD from mean

Historically, public sector union density in Missouri has indeed been very low. In 1983, it ranked 42nd out of 50 states. Through 2014, it averaged 12 percentage points below the nationwide average. Furthermore, among all unionized workers in Missouri, only 24 percent worked in the public sector, the lowest share in the nation. Only Indiana is comparable; in the

remaining 48 states, public sector workers averaged 45 percent of states' unionized workforces between 1983 and 2014.

One need not look far for an explanation. In 1947, the Missouri Supreme Court ruled that the state's constitutional right to bargain collectively applied only to workers in the private sector. Public sector workers could form unions, but public employers were only obligated to "meet and confer" with employee representatives. Agreements were non-binding, and fair-share agency fees were prohibited. A ruling in 1982 further affirmed that public employers were free to change the terms of any agreement "unilaterally" at any time. A Court ruling in 2007 finally extended collective bargaining rights to public employees and required government employers to abide by agreements reached, but employers were not required to bargain in "good faith" until 2012. Strikes remained illegal, and it was left to the legislature to design procedural frameworks for carrying out negotiations—which the legislature never did (Moon 2011).

For the vast majority of Missouri's history, then, the public sector has effectively been right-to-work and its workers have lacked collective bargaining rights. In 2016, Missouri state employees were found to be the lowest-paid government workers in the nation and public school teachers' salaries ranked 43rd out of 50 (Ibid.). In multiple ways, then, public sector union workers in Missouri have long operated from a highly disadvantaged position. But does that mean they have lacked political influence? What is the relationship, if any, between public sector unionization in Missouri and the state's enactment of employment laws?

Causal Pathways

By selecting a case in which the treatment is as unrelated to known confounders as possible, we have set ourselves up for a case analysis in which the causal pathway connecting the

treatment to the outcome is as extreme as possible, satisfying more efficiently the goal of a "pathway case" and improving our chances of discovering the nature of the causal pathway leading from labor unions to state employment law regimes, should one exist. Many questions remain: Were labor unions integral to the enactment of employment laws? If so, which unions, and specifically what role did they play? Were they consistently important political players, did they play more of a supportive role, or were they actually absent from most legislative campaigns to strengthen workers' rights? For it remains possible that the statistical relationship revealed above is spurious. Close examination of case evidence cannot conclusively resolve the causal puzzle, but it can provide evidentiary support that helps to move the research process forward incrementally.

Case Study: Missouri

How to tackle the case study in a systematic way? The Missouri state legislature enacted 72 employment laws between 1960 and 2014 (and likely debated hundreds more)—far too many policy campaigns to examine in comprehensive detail. Although it is tempting to randomly select a subset of these employment laws and examine the role of unions in their enactment, looking only at successful cases threatens to bias our inferences (e.g., unions may have only pushed for laws they knew had a good chance of success). We therefore set out to examine any and all policy campaigns we could find—irrespective of whether they resulted in legislative enactments—within a predetermined set of categories of employment law.

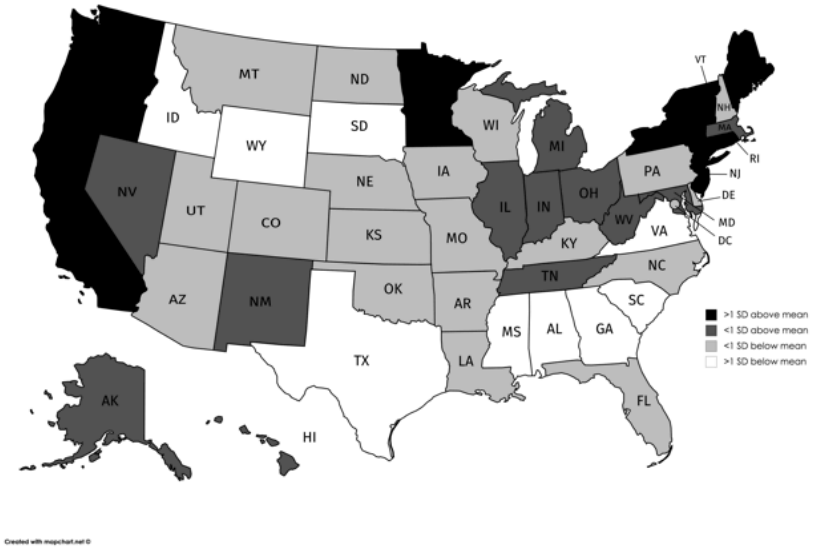
But which categories? We construct a simple and transparent measure of 11 prominent areas of employment law, grouped into three major categories, as tracked by two authoritative sources: the National Conference of State Legislatures (NCSL) and the Department of Labor

(DOL) (**Table 6**). States receive a 1 if they had a major law on the books governing each particular policy area in 2014 and 0 if there was no law. Data from each source was cross-checked against other data sources to confirm accuracy, and three other variables, constructed independently but theoretically measuring the same phenomenon of interest, are used as validity checks (**Appendix A**). We call each state's total score their "Employment Law Regime Score."[7] **Figure 10** illustrates the geographic variation.

Table 6: Components of the State Employment Law Regime Score

Major Category	**Policy Area**
Wages, Hours, and Leave	Minimum wage (DOL) Overtime (NCSL) Prevailing wage (DOL) Meal/Rest periods (DOL) Parental Leave/Time Off (NCSL)
Discrimination and Retaliation	Workplace Discrimination (NCSL) Drug and Alcohol Testing (NCSL) Whistleblower (NCSL)
Terms and Conditions of Employment	Child Labor (NCSL) OSHA State Plans (DOL) Employee Misclassification (NCSL)

Figure 10: State Employment Law Regime Scores: Geographic Variation



Thoroughly examining legislative campaigns across several policy areas, we can begin to flesh out the nature of the relationship between labor unions and state employment law regimes. Local newspapers serve as the primary sources for the analysis. Newspapers are fallible, of course, since journalists may not have detected the involvement of unions, not all relevant legislative campaigns may have been covered, and the extent of coverage may not be equivalent in all cases. Although this means that union involvement may have occurred and we cannot detect it, at least this potential problem biases the analysis *against* the hypothesis that unions were involved. Although it makes "type 2 errors" more likely (falsely inferring the absence of labor unions when they were in fact involved), we can live with that: our primary concern is to avoid "type 1 errors" in which we falsely claim union involvement when none existed (for example, by interpreting the statistical associations reported above as causal). That is precisely what we are trying to avoid through case study analysis.

We seek answers to two main questions: *Were unions integral to the policy process?* If so, which unions? And, *how integral were they*: were they considered key actors in all legislative

campaigns, or just some? How much leadership did they exert? Did they only donate money, or did they also testify, gather signatures, mobilize letter-writing campaigns, and so on? In other words, what does the causal pathway actually look like? Unions need not have won every legislative campaign: our interest is simply in discovering whether unions were integral and consistently involved in trying to enact worker-friendly employment laws.

Our sources include 114 full-text searchable Missouri newspapers with coverage extending from the late 1980s to the present (see **Appendix B**). Using broad search terms, we examined hundreds of newspaper articles for each policy area. Because we were choosing policy campaigns blindly, we opted to examine the most emblematic policy type within each of the three major categories (and two from the second category): the minimum wage, workplace discrimination laws, whistleblower laws, and child labor laws. Each policy arena has been a site of ongoing political contestation and legislative debate, featuring multiple efforts over the years to establish, amend, or alter the legal status quo in each policy area. Within each policy area, we identified multiple policy campaigns, thus multiplying our number of "causal process observations" many times over (**Table 7** below) (Collier et al 2004). A summary table follows brief case narratives.

Minimum Wage

Prior to 1991, Missouri did not have its own minimum wage. Although most Missouri workers were covered by the federal minimum, almost 90,000 employees fell through the cracks (Mosley 1990). In 1989-1990, Republican Governor John Ashcroft vetoed two bills raising the state minimum to match the federal rate before signing the third (Missouri 1990). The state AFL-CIO was widely reported to be the primary group advocating for all three bills and its officers

were widely quoted in newspapers. For example, responding to the governor's first veto, the secretary-treasurer of the Missouri AFL-CIO said: "John Ashcroft swung his ax at not only organized labor but at all labor in the state of Missouri" (Young 1989b). The president of the state AFL-CIO told the *St. Louis Post-Dispatch* "he was considering the formation of a coalition to place the issue on the ballot in November 1990" (Young 1989a). This threat appeared to convince Ashcroft to sign the third bill (Linsalata and Lindecke 1990; Mosley 1990).

A few years later, in what was called the "biggest union-business confrontation since the 1978 fight over 'right to work,'" a labor-led coalition succeeding in placing on the November 1996 ballot a proposition to make Missouri's wage floor the highest in the nation (Gallagher 1996; Mannies 1996b). The "Campaign to Reward Work" coalition, formed by the Missouri AFL-CIO, SEIU, UNITE, and others, collected the necessary signatures, penned op/eds, and provided numerous quotes to newspapers (Winslow and Paone 1996; Mannies 1996b). After a vigorous campaign, the proposition was voted down—likely in part because Bill Clinton's federal minimum wage increase in August preempted the issue and because Missouri's Democratic governor Mel Carnahan opposed the proposition (Mannies 1996a; Heaster 1996).

The political dynamics flipped a decade later when voters overwhelmingly approved a similar ballot initiative in 2006. The state AFL-CIO was credited with "launching" the campaign, collecting 210,000 signatures, holding multiple rallies around the state, and generating steady publicity for the increase (Douglas 2006). AFL-CIO and SEIU leaders were quoted widely in newspapers and unions were described as the primary supporters along with ACORN. "Organized labor and the community activist group ACORN raised almost $2 million for door-to-door campaigning, field work and radio and newspaper ads," the *St. Louis Dispatch* reported (Young 2006). Opponents were reportedly "hurt by the lack of participation by the state's largest

business organization, the Missouri Chamber of Commerce," which focused instead on preserving Republican legislative majorities (Mannies 2006; Dine 2006; Young 2006).

Organized labor continued to support living-wage campaigns in subsequent years and won city-level increases in St. Louis and Kansas City (Stafford 2013b; Bott 2017). In 2013, unions representing UAW, CWA, AFT, and other AFL-CIO member unions founded the "Workers Organizing Committee" along with nonprofit advocacy groups to press for higher wages in the greater Kansas City fast-food industry (Stafford 2013b, 2013a). Multiple bills were introduced in the legislature between 2014 and 2018 but all were rejected by Republican majorities. In 2017, Republicans successfully enacted a law preempting cities and counties from raising their local minimum wages higher than the statewide rate.

Given Republican dominance at the state level, pursuing another ballot initiative was seen as an attractive strategy in 2018 (Campbell 2018). In this second ballot campaign, the concerted efforts of organized labor were again plainly evident. After successfully leading the campaign to repeal the state's right-to-work law in the summer of 2018 by a two-to-one margin, labor unions helped to mobilize support for a November 2018 ballot proposition to raise the minimum wage to $12 an hour by 2023, affecting an estimated quarter of the state's workforce (Suntrup 2018). "Labor groups collected more than 120,000 signatures to add the question to the November ballot" and the "Raise Up Missouri" raised coalition raised almost $2 million from "state labor groups, prominent Democrats and left-leaning organizations" including "the Laborers International Union of North America Laborers and the SEIU Local 1 Missouri Division PAC," which donated tens of thousands of dollars as the campaign got underway (Campbell 2018). The president of the Missouri AFL-CIO noted that organized labor

was actively mobilizing its membership in support of the initiative: "We look out for the best interests of working-class people" (Suntrup 2018).

In sum, in each campaign -- from the establishment of the state's first-ever minimum wage in 1990 to the most recent ballot initiative in 2018 -- the prominent and vigorous support of Missouri labor unions was clearly evident. In every case, unions worked collectively through the state AFL-CIO, and after its disaffiliation from the national federation, the SEIU as well, to influence policy and politics at the state level.

Workplace Discrimination and Whistleblower Laws

The Missouri Human Rights Act prohibits discrimination in the workplace (as well as in housing and public accommodations) on account of race, color, religion, national origin, ancestry, sex, disability, and age. Numerous amendments have been made over the years. In the more recent period covered by searchable newspapers, two major efforts to roll back protections stand out, both spearheaded by emboldened Republican legislative majorities in 2011-12 and 2017-18. In the first period, three bills designed to make discrimination lawsuits more difficult to file while eliminating some of the existing protections for whistleblowers were passed by the legislature, and all three were vetoed by Democratic Governor Jay Nixon. A nearly identical bill was passed and signed by Republican Governor Eric Greitens in 2017. There is substantial evidence that labor unions worked to prevent the enactment of all four bills: they lobbied the governor, collected signatures, and testified against the bills.

The 2010 elections brought Republican supermajorities to both houses of the state legislature. A central part of the GOP's agenda involved fostering a more "pro-business environment" by rewriting workplace discrimination laws and limiting whistleblower claims to

reduce employers' liability and legal costs (Lieb 2010; Rhetoric 2011). Proposed bills required employees to prove that discrimination was a "motivating factor" rather than a "contributing factor" in their termination, weakened protections for whistleblowers, and capped the amount of damages available to employees against whom an employer retaliated. Although the Missouri AFL-CIO's top legislative priority in 2011 was to prevent a right-to-work law from passing, it was visibly active in opposing these changes. The state AFL-CIO secretary-treasurer said: "I can't understand how you can create any (new) employment by limiting a person's right to speak out and blow the whistle on something that's going wrong or their protections under discriminatory law" (Watson 2011). During the 14 days in which Nixon had to decide whether to veto the bill, labor unions worked in coalition with women's groups, the NAACP, trial attorneys, and advocacy groups for the disabled to lobby the governor to veto it—which he did (Rivas 2011a, 2011b). The president of the St. Louis NAACP noted that organized labor's help was critical: "We got with our friends in labor, collected a good 20,000 signatures and made sure the governor knew" that opposition to the law was broad and deep (Rivas 2011b).

The state AFL-CIO considered the first legislative term in 2011 a success and CWA leader Chere Chaney celebrated labor's ability to stymie most of the Republicans' agenda (Stafford 2011). In the second session, however, business groups intended to revise the vetoed laws "to make them acceptable to labor groups" and the governor. Although the AFL-CIO signaled its willingness to "talk with business groups to find middle ground," virtually identical (but separate) discrimination and whistleblower bills were passed in early 2012. Nixon vetoed the discrimination bill. The whistleblower bill, facing another certain veto, died in the Senate (Duplantier 2012a, 2012b; Whistleblower 2012; Missouri 2011; Young 2012). Missouri AFL-CIO's Herb Johnson praised the governor at a rally at the Capitol, saying that the discrimination

bill "attacks the civil rights of Missourians and would make it easier for corporations to discriminate against workers for their age, race or gender. Governor Nixon realizes that workers deserve to have their human rights preserved on the job" (Missouri 2012; Thousands 2012).

Five years later, with even larger Republican legislative majorities and a conservative Republican in the governor's mansion, GOP lawmakers resurrected their discrimination and whistleblower reforms, this time in a single bill. The legislation was swiftly passed and signed by Greitens in the spring of 2017 (Editorial 2017; Hancock 2017). Labor unions testifying against the bill included the AFT-Missouri, the state SEIU, Missouri AFL-CIO, Missouri NEA, the CWA, and the nonprofit Missouri Jobs with Justice (State Personnel Law 2017).

In sum, Republican efforts to roll back workplace discrimination and whistleblower protections were vigorously opposed by organized labor. In coalition with other groups, public and private sector unions engaged in high-profile lobbying, testifying, and grassroots campaigns. Although the coalition met with success in 2011 and 2012 when Democratic Governor Jay Nixon vetoed the bills, a Republican governor signed both provisions into law only five years later. Union activities in this policy area provide further evidence that organized labor was indeed an integral political player in legislative campaigns in this crucial area of employment law.

Child Labor Laws

The earliest child labor laws in Missouri date back to the 1930s, but numerous rules have been added and changes proposed in the years since. Labor unions were consistently named in newspaper accounts as advocates of stronger child labor laws and appear to have been the primary opponents of those who sought to weaken those laws.

When reports in the early 1990s showed a rise in child labor abuses, the state's limited enforcement capacities were fingered as part of the problem (Eardley 1992; Illegal 1992). In addition to being understaffed, the state enforcement agency had no authority to impose fines on violators. "Without a hammer, there's only so much we can do," the agency's director said in 1992. Missouri only had two inspectors working full-time on child labor in the state, preventing the agency from covering "the kind of territory and businesses we would like to" (Eardley 1992).

Labor unions sought to help monitor and report instances of noncompliance (Dine 1991). In part, this stemmed from their concern that violations of child labor laws would cost union members jobs. One private sector union leader said: "Naturally, we have a big concern about it. Labor would naturally want this work drawing prevailing wage. What happens if every school starts letting students do its work?" Another building and trades union official said: "High school students shouldn't be doing work of people who are trying to make a living...it's just a way of undercutting the working people in this community." Such jobs would "normally" be done by workers from the Teamsters or other labor unions (Dine 1991).

As additional problems were identified, the issue rose to the "forefront" of Democratic Governor Mel Carnahan's policy agenda (Dine 1991; Hardwick 1992; Illegal 1992; Eardley 1992; Heaster 1995). In 1995, at a conference on labor-management relations, Carnahan signed into law a number of changes that gave the state agency rulemaking authority, implemented stricter hours restrictions, authorized civil penalties, and more (Heaster 1995). The Missouri AFL-CIO continued to mention the preservation and enforcement of child labor laws as one of their core concerns in subsequent years (Ganey 2004; Hoedel 2014).

In 2011, Republican state Senator Jane Cunningham introduced a bill to abolish most of the child labor protections in Missouri, arguing that parents, rather than government, should be

allowed to decide labor issues for their children (Shelly 2011a, 2011b). Organized labor opposed the bill and the president of the Greater Kansas City AFL-CIO argued that the bill was of a piece with a broader anti-worker, anti-union agenda that "takes away workers' rights" (Hoedel 2014). Others ridiculed it and the bill failed to advance in the Senate (Missouri 2011). In sum, whenever child labor issues periodically surfaced in Missouri, the state labor federation and specific private sector unions were cited as prominent advocates for stronger laws and regulatory authorities.

Discussion

Evidence from the Missouri case thus supports the notion that unions have indeed been integral political players in employment-law policy campaigns. While other types of groups played important roles in specific policy campaigns – e.g., ACORN in support of a higher minimum wage in 2006; faith-based and other community groups in the municipal minimum wage campaigns of 2013; women's groups and NACCP in defending discrimination protections in 2011; nonprofits advocating for child labor laws – the Missouri AFL-CIO was the only group that was *consistently* on the front lines in each of the dozen policy campaigns we examined across four major policy areas. Win or lose, and irrespective of the particular substantive area or final policy outcome, organized labor was visibly active, engaged, and integral to the policy debate and legislative process **(Table 7)**.

Not surprisingly, labor's success was conditional on partisan control of state government. During periods of unified Republican control, the unions' only successes came through ballot initiatives that bypassed the state legislature to advance bills that large majorities of voters supported but Republican lawmakers opposed (minimum wage increases in 2006 and 2018). The ballot initiative process was also used to pressure a Republican governor to sign Missouri's first

minimum wage in 1990. Otherwise, organized labor needed either unified Democratic control to enact stronger laws (child labor in 1995) or a Democratic governor to veto Republican bills it opposed (three discrimination and whistleblower bills in 2011-2012, child labor bill in 2011).

But the purpose of the case study is not to explain policy outcomes or to make definitive claims about causal mechanisms—it is to use a high-leverage case to explore the nature of the causal pathway and assess the likelihood that the statistical relationship identified above is spurious. As we have seen, the evidence renders the argument of spuriousness difficult to make.

The case study also helps to identify another omitted variable. In the second stage of the analysis above we added measures of party strength and citizen ideology and recoded the right-to-work variable to account for laws governing collective bargaining and union organizing—but none of those changes captured the kinds of Court-imposed limits on collective bargaining that appear to have limited the growth of public sector unionism in Missouri. A finer-grained measure of collective bargaining rules in each state may therefore be needed in future iterations.

We also discovered further insights into the nature and composition of our main treatment variable, public sector union density. We dove into the case study looking for evidence of public-sector union involvement in the policymaking process. Given that public sector union workers in Missouri are relatively few and seemingly weak, we were uncertain as to what, if anything, we might find. What emerges most clearly from the case study is that the Missouri AFL-CIO is the most prominent and powerful labor organization involved in state-level politics. As a federation of unions, the state AFL-CIO represents all affiliated Missouri unions and their members—public and private sector alike—at the state level.[8] This organizational structure enables unions representing workers across a variety of different sectors and industries to magnify their influence and speak with a single voice in state-level policy campaigns. Moreover,

it provides a mechanism through which smaller groups, like public sector workers, can have their concerns addressed in policy campaigns that they could not likely run on their own.

Table 7: Causal Process Observations

Major Category	Policy Area	Year	Policy Type	Objective	Outcome	Evidence of Union Involvement?	Outcome Favorable to Unions?
Wages, Hours, and Leave	Minimum Wage	1989-1990	Legislation	Raise minimum wage	Vetoed	Yes	No
Wages, Hours, and Leave	Minimum Wage	1989-1990	Legislation	Raise minimum wage	Vetoed	Yes	No
Wages, Hours, and Leave	Minimum Wage	1989-1990	Legislation	Raise minimum wage	Enacted	Yes	Yes
Wages, Hours, and Leave	Minimum Wage	1996	Ballot initiative	Raise minimum wage	Failed	Yes	No
Wages, Hours, and Leave	Minimum Wage	2006	Ballot initiative	Raise minimum wage	Passed	Yes	Yes
Wages, Hours, and Leave	Minimum Wage	2018	Ballot initiative	Raise minimum wage	Passed	Yes	Yes
Discrimination and Retaliation	Discrimination and Whistleblower	2011	Legislation	Limit litigation, eliminate whistleblower protections	Vetoed	Yes	Yes
Discrimination and Retaliation	Discrimination	2012	Legislation	Limit discrimination litigation	Vetoed	Yes	Yes
Discrimination and Retaliation	Whistleblower	2012	Legislation	Limit whistleblower protections	Died in Senate	Yes	Yes
Discrimination and Retaliation	Discrimination and Whistleblower	2017	Legislation	Limit litigation, eliminate whistleblower protections	Enacted	Yes	No
Terms and Conditions of Employment	Child Labor	1995	Legislation	Enhance state's enforcement capacity	Enacted	Yes	Yes
Terms and Conditions of Employment	Child Labor	2011	Legislation	Eliminate child labor laws	Failed in Senate	Yes	Yes

For a reality check, we interviewed the president of the Missouri AFL-CIO, seeking information about the federation's mix of members and its relationship to the federation's policy priorities. State federation president Mike Louis confirmed that despite their meager numbers, public sector workers work reciprocally with private sector workers to advance one another's issues and concerns at the state level. During the extended federal government shutdown in early 2019, for example, the state AFL-CIO devoted substantial resources to bring public attention to the burdens imposed on locked-out federal employees working in Missouri. He said: "It was a very pleasant feeling to see how many private sector members were eager to make phone calls

and write letters in support of the workers. We orchestrated as much of that as we could" (Louis 2019). With unified Republican control of the state government, Louis noted that state and municipal workers had become targets of union-curbing legislation:

> "We are very involved with lobbying on behalf of those workers. Currently we are partnered with AFSCME as well as unions that represent teachers, construction workers, firefighters, police officers, etc. in fighting HB 1413 (paycheck deception)…So yes, public workers still have a voice and yes the State AFL-CIO works hard to represent them in Jefferson City."

What is more, in 2019 the #1 "core issue" on the state federation's policy agenda was the enactment of "enabling legislation to provide the process of collective bargaining for public employees," and it was noteworthy that the largest public sector unions in the state—AFT, AFSCME, AFGE—served on the state AFL-CIO Executive Board (AFL-CIO 2019).

The *within-state* federated structure of the AFL-CIO thus appears to be a critically important, though often overlooked, mechanism through which both public- and private-sector unions exert policy influence in state-level politics. From a research standpoint, this discovery helpfully raises still more questions about internal power dynamics and the relative influence of unions within the state federation. In subsequent research, the nature of these relationships, as well as the federation's internal rules and decision-making processes, deserve greater attention. For now, it suffices to note that even in our propensity-adjusted case featuring extremely weak public sector unions, there is plenty of evidence to suggest that a causal pathway leading from unions to state employment law regimes does indeed exist, and that along this pathway we should expect to see considerable political activism by the state federation of labor on behalf of public- and private-sector workers alike.

5. Conclusion

Selecting cases with surprising causes is an efficient new tool for case selection in multi-method research. It allows scholars to focus on aspects of a potential cause that are surprising and empirically distinct from the set of known control variables. As such, it facilitates discovery regarding problems such as measurement error, confounding, and causal pathways. Furthermore, by allowing the set of cases for in-depth study to adjust in response to new discoveries, it facilitates an integrative multi-method research cycle. In the context of a methodological debate in which non-experimental research is represented as having to approach these issues as brute assumptions, an approach that pivots toward empirically-guided qualitative discovery of new problems and their potential statistical solutions offers a novel and empowering way forward.

Applying this technique to the problem of understanding the political dynamics of state-level labor and employment law in the U.S. demonstrates the practical value of both this specific case-selection strategy and of integrative multi-method research cycles more generally. Starting from a standard regression model of the prevalence of employment laws in the states, we use case studies to discover: that the treatment can productively be partitioned, with a focus on public-sector unions being the most informative; that state-level partisan dynamics and ideological leanings are relevant control variables; and that public-sector unions may well exercise some or much of their influence on legislation through broader coalitions, in particular through state labor federations such as the state AFL-CIO. Each of these discoveries suggests new, richer statistical analyses relative to the starting point, and each step of the analysis deepens our understanding of the overall relationship.

Space limitations prevent a full comparison to the alternative approach of selecting extreme cases on the unadjusted *X* variable. But it is worth noting that this more conventional approach would have selected Michigan and North Carolina as our extreme cases on unadjusted *X* (i.e., the composite union density measure). Neither state, however, would have suggested any reason to partition the union density treatment variable into its private and public sector components -- Michigan's baseline private sector union density was extremely high (second highest in the nation in 1983) *and* its public sector union density was extremely high (fourth highest). North Carolina, likewise, exhibited extremely low values on both private and public sector union density (50th and 42nd, respectively). Selecting cases on unadjusted *X* would have caused us to completely overlook what turned out to be important variation in the composition of the treatment variable across states. The more conventional approach would have also obscured the relevance of state-level collective bargaining and right-to-work laws, the examination of which led us to discover important confounders; and it would have concealed entirely the significance of intrastate union alignments and power dynamics within state federations, which were revealed to be important issues for further study. The propensity-adjusted case selection procedure enabled these substantive discoveries to surface incrementally, deepening our understanding and clarifying the nature of relationship between unions and state employment law regimes further at each stage of the research cycle. Finally, whereas the unadjusted approach would have kept our attention fixed on only two states throughout the research process, the propensity-adjusted version prompted us to examine, and extract valuable information from, five different states, thereby increasing the likelihood that each additional case would add new information to the overall study.

Does the research cycle end at this point? The best methodological guidance is to continue such cycles until something novel and interesting has been learned. However, if the rate of learning slows down, scholars may productively pivot to other designs, such as multi-method research focused on deviant cases, machine-learning tools for data exploration, and so forth.

The reader need not agree with all, or indeed any, of these discoveries on substantive grounds in order to see the value of undertaking and clearly reporting such multi-method research cycles. After all, each step forward from the initial regression is taken here on the basis of some kind of explicit argument and empirical evidence. This fact opens our decisions to productive debate regarding the qualitative evidence, our interpretations, and the resulting modeling choices—a debate that is much harder to have in a meaningful way if the model specification and revision process is suppressed or lacks empirical inputs. Furthermore, the reader need not believe that the end result of the reported process captures causal truth; the authors are not persuaded of this, either. Rather, there is value in this and similar research cycles because the process provides reason to believe that progress has been made toward a more causally meaningful analysis, even if that final goal remains out of grasp.

APPENDIX A:

Model 1

Description of variables: Employment Laws is total number of employment laws enacted in each state between 1974 and 2014. The key explanatory (treatment) variable is *Union Density Decline*, which is the percentage point change in union density from 1974 to 2014 (sign reversed for ease of interpretation). Covariates include: *Base Density,* which is the average statewide union density during the five years prior to the period of steep decline, to account for different baseline levels; *MFG* is the share of the state's nonagricultural workforce in manufacturing in 1973, to account for the tendency of high-manufacturing states to have more unionized workforces (Bureau of Economic Analysis); *logMFGWage* takes the log of the average manufacturing wage in each state in 1973, since higher-wage states may pass more protective legislation (Bureau of Economic Analysis); *logEMPLCHG* is the log change in nonagricultural employment in the state during the same period, which may capture another motor of union density decline and control for another potential incentive for enacting employment protections (Bureau of Economic Analysis). *Unemployment* is the average unemployment rate in the state during the period, accounting for the tendency of loose labor markets to dampen enthusiasm for both unionization and greater state regulation of the workplace (Bureau of Labor Statistics). *RTW* is a dummy variable for whether the state passed a right-to-work law prior to 1974 (National Conference of State Legislatures), sometimes used as a proxy for business strength. *Legislative Productivity* is the total number of laws passed by the state as recorded in the Book of the States annual volumes, to control for each state's propensity to legislate (Council of State Governments

1935-2017); and *CA,* which dummies for California (far more legislatively productive than any other state).

Employment Law Regime Measure

States receive a 1 if they had a major law on the books governing each particular policy area in 2014 and 0 if there was no law. Exceptions include: *Meal period*: 1=state law covers all workers, 0.5=law applies only to minors, and 0=no law; *Child labor*: 1=employment certificate is issued by the state or a school, 0.5=employer must register the child worker or keep records, and 0=no law; *Drug and alcohol testing*: 1=state regulates broadly, 0.5=regulation of public sector only; 0=no law; *Whistleblower*: 1=public and private sector workers are covered, 0.5=either public or private sector (but not both), and 0=no law; *Employee Misclassification*: states receive 0.33 for each of the following: interagency taskforces and studies; clear and objective tests for determining employee status; sector-specific laws.

Sources: the National Conference of State Legislatures (NCSL) and the Department of Labor (DOL)

Three Alternative Measures

Three other variables, each constructed independently but theoretically measuring the same phenomenon of interest, are used as validity checks. The first is the tally of employment laws used above: it measures all employment laws enacted in each state between 1973 and 2014 as a share of all state laws passed.[9] States that pay relatively more attention to employment laws are expected to have more comprehensive employment law regimes. The other two measures

provide validity checks from the opposite direction, capturing the extent to which state laws are considered by employers and business advocates to be hospitable environments for investment and growth. One uses a Chamber of Commerce study in which researchers examined 34 types of laws on the books in 2009 and graded each state on the extent to which its laws increased the regulatory burden on business and opened the door to litigation (Johnson and Hollenbeck 2009b). The other is the Index of Worker Freedom (IWF) compiled by the conservative Alliance for Worker Freedom in 2009, which tracked 15 laws that it claimed obstructed workers' freedom and drove away high-quality workers (Johnson and Hollenbeck 2009a). Note that the sign is flipped on this measure. Despite their differences, the three other variables are strongly correlated with the first (see Measure 1 in **Table 3**), suggesting that they do generally capture the same phenomenon of interest.

Correlation Matrix

Dependent variables	Measure 1	Measure 2	Measure 3	Measure 4
Measure 1: Employment Law Regime Score	1.00			
Measure 2: Attention to Employment Laws	0.72	1.00		
Measure 3: Chamber of Commerce Score	0.79	0.67	1.00	
Measure 4: Index of Worker Freedom	-0.83	-0.64	-0.80	1.00

Appendix B

Missouri Sources

Broad search terms included, e.g., "minimum wage AND Missouri AND (bill OR law OR legislation OR amendment OR ballot OR policy)"

Using the database *Newsbank,* sources include 114 full-text searchable Missouri newspapers with coverage extending from the late 1980s to the present. They include:

Source Name	Dates	Location	Type	Format
St. Louis Post-Dispatch (MO)	1988 - Current	USA - MO - St. Louis	Newspaper	Text
Kansas City Star, The (MO)	1991 - Current	USA - MO - Kansas City	Newspaper	Text
Maneater, The: University of Missouri (Columbia, MO)	1995 - Current	USA - MO - Columbia	College/University Newspaper	Text
Hannibal Courier-Post (MO)	1997 - Current	USA - MO - Hannibal	Newspaper	Text
Associated Press: Springfield Metro Area (MO)	1998 - Current	USA - MO - Springfield	Newswire	Text
Columbia Daily Tribune (MO)	1998 - Current	USA - MO - Columbia	Newspaper	Text
Examiner, The (Independence-Blues Springs-Grain Valley, MO)	1998 - Current	USA - MO - Independence, Blue Springs, Grain Valley	Newspaper	Text
Riverfront Times (St. Louis, MO)	1998 - Current	USA - MO - St. Louis	Newspaper	Text
Monett Times, The (MO)	1999 - 2009	USA - MO - Monett	Newspaper	Text
Boonville Daily News (MO)	1999 - Current	USA - MO - Boonville	Newspaper	Text
Jefferson City News-Tribune (MO)	1999 - Current	USA - MO - Jefferson City	Newspaper	Text
Daily Journal (Park Hills, MO)	2000 - Current	USA - MO - Park Hills	Newspaper	Text
St. Joseph News-Press (MO)	2000 - Current	USA - MO - St. Joseph	Newspaper	Text
St. Charles Business Record (MO)	2001 - 2009	USA - MO - St. Charles	Journal	Text
St. Louis Countian/St. Louis Daily Record (MO)	2001 - 2009	USA - MO - St. Louis	Journal	Text
Macon Chronicle-Herald (MO)	2001 - 2014	USA - MO - Macon	Newspaper	Text
Longview Current, The: Metropolitan Community College - Longview (Lee's Summit, MO)	2001 - 2015	USA - MO - Lee's Summit	College/University Newspaper	Text
Carthage Press, The (MO)	2001 - 2018	USA - MO - Carthage	Newspaper	Text
Daily Guide (Waynesville, MO)	2001 - 2018	USA - MO - Waynesville	Newspaper	Text
Constitution-Tribune (Chillicothe, MO)	2001 - Current	USA - MO - Chillicothe	Newspaper	Text
Kirksville Daily Express (MO)	2001 - Current	USA - MO - Kirksville	Newspaper	Text
Maryville Daily Forum, The (MO)	2001 - Current	USA - MO - Maryville	Newspaper	Text
Mexico Ledger (MO)	2001 - Current	USA - MO - Mexico	Newspaper	Text

Moberly Monitor-Index (MO)	2001 - Current	USA - MO - Moberly	Newspaper	Text
Neosho Daily News (MO)	2001 - Current	USA - MO - Neosho	Newspaper	Text
Rolla Daily News (MO)	2001 - Current	USA - MO - Rolla	Newspaper	Text
Daily Record, The (Kansas City, MO)	2002 - 2009	USA - MO - Kansas City	Journal	Text
California Democrat, The (MO)	2003 - Current	USA - MO - California	Newspaper	Text
Democrat News (Fredericktown, MO)	2003 - Current	USA - MO - Fredericktown	Newspaper	Text
Farmington Press (MO)	2003 - Current	USA - MO - Farmington	Newspaper	Text
Fulton Sun, The (MO)	2003 - Current	USA - MO - Fulton	Newspaper	Text
Oakville-Mehlville Journal (MO)	2004 - 2007	USA - MO - Oakville	Newspaper	Text
South City Journal (St. Louis, MO)	2004 - 2007	USA - MO - St. Louis	Newspaper	Text
Citizen Journal (St. Louis, MO)	2004 - 2008	USA - MO - St. Louis	Newspaper	Text
Hazelwood-Bridgeton Journal (MO)	2004 - 2008	USA - MO - Hazelwood	Newspaper	Text
North Side Journal (St. Louis, MO)	2004 - 2008	USA - MO - St. Louis	Newspaper	Text
Northeast County Journal (Bellefontaine Neighbors, MO)	2004 - 2008	USA - MO - Bellefontaine Neighbors	Newspaper	Text
Overland-St. Ann Journal (MO)	2004 - 2008	USA - MO - Overland	Newspaper	Text
Chesterfield Journal (MO)	2004 - 2009	USA - MO - Chesterfield	Newspaper	Text
Kirkwood-Webster Journal (MO)	2004 - 2009	USA - MO - Kirkwood	Newspaper	Text
Meramec Journal (Byrnes Mill, MO)	2004 - 2009	USA - MO - Byrnes Mill	Newspaper	Text
News Democrat Journal (Festus, MO)	2004 - 2009	USA - MO - Festus	Newspaper	Text
Press Journal (St. Louis, MO)	2004 - 2009	USA - MO - St. Louis	Newspaper	Text
Southwest City Journal (St. Louis, MO)	2004 - 2009	USA - MO - St. Louis	Newspaper	Text
Southwest County Journal (St. Louis, MO)	2004 - 2009	USA - MO - St. Louis	Newspaper	Text
Tri-County Journal (St. Louis, MO)	2004 - 2009	USA - MO - St. Louis	Newspaper	Text
Wentzville Journal (MO)	2004 - 2009	USA - MO - Wentzville	Newspaper	Text
Jefferson County Journal (Festus, MO)	2004 - 2011	USA - MO - Festus	Newspaper	Text
Mid-County Journal (Town and Country, MO)	2004 - 2011	USA - MO - Town and Country	Newspaper	Text
North County Journal - Northwest Edition (Florissant-Black Jack, MO)	2004 - 2011	USA - MO - Florissant, Black Jack	Newspaper	Text

North County Journal - Overland Edition (St. Louis, MO)	2004 - 2011	USA - MO - St. Louis	Newspaper	Text
O'Fallon Journal (MO)	2004 - 2011	USA - MO - O'Fallon	Newspaper	Text
South Side Journal (St. Louis, MO)	2004 - 2011	USA - MO - St. Louis	Newspaper	Text
St. Peters Journal (MO)	2004 - 2011	USA - MO - St. Peters	Newspaper	Text
South County Journal (St. Louis, MO)	2004 - 2013	USA - MO - St. Louis	Newspaper	Text
Warrenton Journal (MO)	2004 - 2013	USA - MO - Warrenton	Newspaper	Text
West County Journal (St. Louis, MO)	2004 - 2013	USA - MO - St. Louis	Newspaper	Text
St. Charles County Journal (MO)	2004 - Current	USA - MO - St. Charles	Newspaper	Text
Boone County Journal, The (Ashland, MO)	2005 - Current	USA - MO - Ashland	Newspaper	Text
Joplin Globe, The (MO)	2005 - Current	USA - MO - Joplin	Newspaper	Text
Kansas City Star, The: Blogs (MO)	2005 - Current	USA - MO - Kansas City	Blog	Text
University News: St. Louis University (MO)	2005 - Current	USA - MO - St. Louis	College/University Newspaper	Text
University News: University of Missouri - Kansas City (MO)	2005 - Current	USA - MO - Kansas City	College/University Newspaper	Text
West Side Star (Laurie, MO)	2006 - 2010	USA - MO - Laurie	Newspaper	Text
Star Herald, The (Belton, MO)	2006 - 2011	USA - MO - Belton	Newspaper	Text
Pulaski County Mirror (St. Robert, MO)	2006 - 2017	USA - MO - St. Robert	Newspaper	Text
Griffon News: Missouri Western State College (St. Joseph, MO)	2006 - Current	USA - MO - St. Joseph	College/University Newspaper	Text
Independent News (Florissant, MO)	2006 - Current	USA - MO - Florissant	Newspaper	Text
Jackson County Advocate (Grandview, MO)	2006 - Current	USA - MO - Grandview	Newspaper	Text
Laclede County Record, The (MO)	2006 - Current	USA - MO - Lebanon	Newspaper	Text
St. Louis American (MO)	2006 - Current	USA - MO - St. Louis	Newspaper	Text
Raytown Post, The (MO)	2007 - 2008	USA - MO - Raytown	Newspaper	Text

Democrat-Missourian, The (Harrisonville, MO)	2007 - Current	USA - MO - Harrisonville	Newspaper	Text
Lee's Summit Journal (MO)	2007 - Current	USA - MO - Lee's Summit	Newspaper	Text
Northeast News, The (Kansas City, MO)	2007 - Current	USA - MO - Kansas City	Newspaper	Text
Sedalia Democrat, The (MO)	2007 - Current	USA - MO - Sedalia	Newspaper	Text
Blue Springs Journal (MO)	2008 - 2011	USA - MO - Blue Springs	Newspaper	Text
Kansas City Examiner (MO)	2008 - 2016	USA - MO - Kansas City	Web-Only Source	Text
St. James Leader Journal (MO)	2008 - 2016	USA - MO - St. James	Newspaper	Text
St. Louis Examiner (MO)	2008 - 2016	USA - MO - St. Louis	Web-Only Source	Text
Maneater, The: University of Missouri: Blogs (Columbia, MO)	2008 - 2017	USA - MO - Columbia	Blog	Text
Linn County Leader (Brookfield, MO)	2008 - Current	USA - MO - Brookfield	Newspaper	Text
People's Tribune, The (Bowling Green, MO)	2008 - Current	USA - MO - Bowling Green	Newspaper	Text
Student Life, The: Washington University-St. Louis (MO)	2008 - Current	USA - MO - St. Louis	College/University Newspaper	Text
Vedette, The (Greenfield-Miller, MO)	2009 - 2014	USA - MO - Greenfield, Miller	Newspaper	Text
Joplin Examiner (MO)	2009 - 2016	USA - MO - Joplin	Web-Only Source	Text
Lake Today, The (Lake Ozark, MO)	2009 - 2016	USA - MO - Lake Ozark	Newspaper	Text
Springfield Examiner (MO)	2009 - 2016	USA - MO - Springfield	Web-Only Source	Text
Aurora Advertiser (MO)	2009 - Current	USA - MO - Aurora	Newspaper	Text
CBS - 4 KMOV (St. Louis, MO)	2009 - Current	USA - MO - St. Louis	Web-Only Source	Text
Daily Star-Journal, The (Warrensburg, MO)	2009 - Current	USA - MO - Warrensburg	Newspaper	Text
McDonald County Press, The (MO)	2009 - Current	USA - MO - Noel, Lanagan	Newspaper	Text

Missouri Lawyers Media (MO)	2009 - Current	USA - MO - St. Louis	Web-Only Source	Text
Ozark County Times, The (MO)	2009 - Current	USA - MO - Gainesville	Newspaper	Text
Webster County Citizen (Seymour, MO)	2009 - Current	USA - MO - Seymour	Newspaper	Text
ABC - 33 KSPR (Springfield, MO)	2010 - Current	USA - MO - Springfield	Web-Only Source	Text
FOX - 4 WDAF (Kansas City, MO)	2010 - Current	USA - MO - Kansas City	Web-Only Source	Text
Journal, The: Webster University (St. Louis, MO)	2010 - Current	USA - MO - St. Louis	College/University Newspaper	Text
NBC - 3 KYTV (Springfield, MO)	2010 - Current	USA - MO - Springfield	Web-Only Source	Text
Stephens Life: Stephens College (Columbia, MO)	2011 - 2012	USA - MO - Columbia	College/University Newspaper	Text
Saint Joseph Telegraph, The (MO)	2011 - 2015	USA - MO - St. Joseph	Newspaper	Text
Associated Press State Wire: Missouri (MO)	2011 - Current	USA - MO	Newswire	Text
Bethany Republican-Clipper (MO)	2011 - Current	USA - MO - Bethany	Newspaper	Text
FOX - 2 KTVI (St. Louis, MO)	2011 - Current	USA - MO - St. Louis	Web-Only Source	Text
KBIA - 91.3 FM: Web Edition Articles (MO)	2011 - Current	USA - MO - Columbia	Web-Only Source	Text
Community Free Press (Springfield, MO)	2012 - 2015	USA - MO - Springfield	Newspaper	Text
Omnibus, The: Southwest Baptist University (Bolivar, MO)	2013 - 2016	USA - MO - Bolivar	College/University Newspaper	Text
Capaha Arrow, The: Southeast Missouri State University (Cape Girardeau, MO)	2013 - Current	USA - MO - Cape Girardeau	College/University Newspaper	Text

Crowder Sentry, The: Crowder College (Neosho, MO)	2013 - Current	USA - MO - Neosho	Web-Only Source	Text
Current, The: University of Missouri - St. Louis (MO)	2013 - Current	USA - MO - St. Louis	College/University Newspaper	Text
Northwest Missourian: Northwest Missouri State University (Maryville, MO)	2013 - Current	USA - MO - Maryville	College/University Newspaper	Text
Riverfront Times: Blogs (St. Louis, MO)	2014 - 2015	USA - MO - St. Louis	Blog	Text
70 West Sentinel (Wentzville, MO)	2014 - Current	USA - MO - Wentzville	Web-Only Source	Text

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[1] Often operationalized as at least two standard deviations away from the population mean.
[2] The "surprising causes" term is intended to capture the idea that the value which is assumed by the main causal variable is surprising in a given case in light of the available control variables. It does not intend to suggest that the causal variable itself is necessarily theoretically surprising.
[3] *Employment laws* establish minimum workplace standards and grant individual rights and protections via statute rather than through collective bargaining. Laws pertaining to collective bargaining and unionization are considered *labor laws*.
[4] Variables are described in **Appendix A.**
[5] State-level data on union density by sector begins in 1983.

[6] As discussed above, recoding independent variables as such can raise post-treatment bias and deserves further qualitative inquiry of causal pathways.
[7] Components are described in **Appendix A.**
[8] The SEIU, which disaffiliated from the AFL-CIO in 2005, similarly combines public and private sector workers in a single organization. The labor federations in the state's two largest cities, the Greater Kansas City AFL-CIO and the St. Louis Labor Council, likewise represent both public- and private-sector unions.
[9] Data and methods in Galvin (2019).