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Abstract. Evidence from a fairly extensive literature in economics concludes that the adoption by state courts of exceptions to the common law *employment-at-will* doctrine has had far-reaching unintended impacts on firm behavior and labor market outcomes on several behaviorally distinct margins. We extend this literature by examining their unintended effects on union membership. Additionally, we examine the unintended effects on union membership that resulted from government adoption of union-like social welfare programs. Our results strongly suggest that these legal and policy interventions, which provide just-cause protections, social insurance and health and safety protections, have had the unintended effects of contributing to the long-run decline in union density.

JEL Classification: J51, J58, K3

Keywords: Employment-at-Will, Unionization, Union Density, Social Welfare Programs, Just Cause, Wrongful Discharge

I. Introduction

For most of the twentieth century employment relationships in the United States were largely governed by the common law doctrine of *employment-at-will*, which meant employers were free to terminate employees at any time whether for good cause, bad cause or no cause. Until the latter half of the twentieth century, only the federal government had created limitations to the at-will doctrine. Arguably, the National Labor Relations Act of 1935, which prohibits terminations on the basis of union activity, was the first major limitation placed on the employment-at-will doctrine. Another important exception occurred with passage of the Civil Rights Act of 1964, which prohibits discharge based on race, religion, sex, age and national origin. Both were explicitly intended to protect the rights of those defined class of workers by providing members of an identified class the right to sue employers for wrongful discharge. For all other workers, however, employment relationships remained governed by the at-will doctrine.

During the 1970s and 1980s however, a number of state courts began carving out one or more exceptions to the at-will doctrine that had historically governed employment relationships within their jurisdictions. Also referred to as wrongful discharge protections, these state court decisions place substantive constraints on a firm's ability to terminate employees absent evidence for just cause. Unlike federal legislation, however, state courts have created exceptions that are broader in scope because they apply to *all* workers and *all* firms (large and small)[1] within that state. Consequently, as Miles (2000) points out, state exceptions are potentially more far-reaching in their impacts on firm behavior and labor market outcomes than existing federal protections.

State courts have created three major classes of exceptions to the at-will doctrine. The first is referred to as a *public-policy* exception, under which a worker is considered wrongfully discharged if the termination breaches an explicit and well-defined policy of the state. For example, a worker cannot be terminated for filing a worker's compensation claim following an injury on the job. Also, firms cannot fire workers

[1] Federal legislation typically exempts small firms, say firms with less than 50 employees.

because they refuse to violate laws, blow the whistle on other workers or management, or exercise a legally protected right such as taking time off to serve on a jury or vote.[2] Muhl (2001) finds that while the definition of public policy varies from state to state, most state courts narrowly limit a public policy claim to clear statements found in that state's constitution or state statutes. Currently, courts in 43 states have adopted this exception.

A second exception is when an *implied-contract* can be inferred from past actions of the firm, even though no expressed or written contract exists. That is, even though an employee may be employed without an explicit contract, an employer may have made oral or written assurances concerning job security or representations about the process to be followed when an adverse employment action is taken. Generally, the existence of an implied contract might be inferred from recruiting statements, company handbooks, oral representations, past personnel decisions, performance evaluations or promotion decisions. The implied-contract at-will exception is currently recognized in 41 states.

Finally, courts in 10 states have created at-will exceptions based on the existence of a *covenant of good faith and fair dealing*. This exception requires that all employees be treated fairly, which means termination must be for good cause. Since courts in adopting states assume that *every* employment relationship is subject to a covenant of good faith and fair dealing, *good-faith* exceptions go well beyond the more narrowly defined public-policy and implied-contract exceptions.[3] As Dertouzos, et al. (1988) point out, a good-faith exception is particularly applicable to those employees with long job tenure, a record of promotions and a record of good job performance. Wyoming was the last state to adopt this exception in 1994.[4]

Numerous studies have evaluated the effects of these discharge protections on several behaviorally distinct labor market outcomes. Interestingly, these studies show that the constraints placed on firm behavior by at-

[2] There are currently 30 states with laws that require employers to allow employees to take time off to vote including some that impose criminal penalties on an employer who fires or otherwise punishes an employee for taking time away from work in order to vote.
[3] See Muhl (2001).
[4] More detailed discussions of employment-at-will exceptions can be found in Autor, Donohue III and Schwab (2006), Dertouzos, Holland and Ebener (1988), Krueger (1991), Muhl (2001) and Stieber (1984).

will exceptions operate on several different unintended margins; with potential impacts on employment, unemployment, earnings, job flows, capital investment, labor productivity, total factor productivity, employment outsourcing and innovative activities.

Dertouzos and Karoly (1992; DK)[5] carried out one of the first rigorous tests of the potential impact on firms operating in states that have adopted one or more at-will exceptions. The authors found that the adoption of an at-will exception results in a significant increase in labor costs for firms within adopting states. Their estimates suggests that the adoption of an at-will exception was equivalent to a 10 percent tax on wages, leading to a 3 percent reduction in aggregate employment. In contrast, Miles (2000) found no evidence that at-will exceptions affect either aggregate employment or unemployment. But, it appeared that the *implied-contract* exception caused firms to increase their utilization of temporary employment by about 15 percent.[6] In a more recent study by Bird and Knopf (2009), the adoption of wrongful discharge laws, specifically the *implied-contract* exception, was found to increase labor expenses and negatively affect profitability within the commercial banking industry.

Autor, Donohue and Schwab (2006; ADS) reevaluated the potential effects at-will exceptions might have on employment and earnings "using richer data and a more complete coding of the case law." The authors found that the *implied-contract* exception had a modestly sized negative impact on the employment-to-population ratio. Their estimated impact was substantially smaller than what DK reported, but considerably larger than the zero impact found by Miles.[7] In their follow-up study, ADS (2006) sought to reconcile the discrepancies in the results of the DK, Miles and ADS studies. Based on their reanalysis of the data, the authors concluded that while the DK study significantly overestimated the negative employment effects of at-will exceptions, the Miles study underestimated the effects.

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[5] We follow the convention in the literature of referring to each study with an author acronym.

[6] Autor (2003) also found a significant growth in temporary employment following a state's adoption of at-will exceptions.

[7] ADS (2004) also reevaluated the evidence and found the same middle ground position with respect to the estimated effects of at-will exceptions on employment.

On yet another margin, Autor, Kerr and Kugler (2007; AKK) provided convincing evidence that following the creation of a *good-faith* exception, firms altered their short-run production choices. Since at-will exceptions act as a tax on firing, firms tended to retain unproductive workers (workers whose short-run marginal products are lower than their market wage), which in turn, lead to reductions in technical efficiency.[8] AKK also found that the increased input adjustment costs imposed by discharge protections lead firms to engage in capital and skill deepening; that is, firms increased capital investment and increased employment of non-production workers. Similarly, MacLeod and Nakavachara (2007; MN) found that while the employment effect of at-will exceptions was negative for low skilled workers, it was positive for employment of skilled workers, particularly for *good-faith* exceptions.

In a follow up to the MN study, Acharya, Baghai and Subramanian (2013; ABS) also found positive effects of *good-faith* on the employment of skill workers. Since the adoption of a *good-faith* exception has the potential of eliminating a firm's incentive to engage in ex post opportunistic behavior, it acts to guarantee innovative employees will reap contractual rewards from engaging in innovating activities. ABS found that the adoption of a *good-faith* exception enhanced innovative efforts by employees and encouraged firms to invest in risky but potentially ground-breaking projects.

In this paper we contribute to this on-going debate by examining the potential unintended consequences of court-created employment at-will exceptions on *union membership*. Historically, one of the fundamental benefits provided by union collective bargaining agreements, in addition to higher wages, has been the contractual guarantees of discharge for just cause only. Muhl (2001) noted that when workers first began organizing unions, one of the chief provisions contained in their collective bargaining agreements was a requirement of just cause for adverse employment actions by the firm, as well as the specification of procedures for arbitrating worker grievances. The adoption of at-will exceptions has in effect created "publically" available remedies for unjust dismissal claims; substituting for the provision of those

[8] Morriss (1995) found that the adoption of a public-policy exception resulted in a reduction in the likelihood of employees being discharged in that state.

protections through collective bargaining and thus, reducing the net benefits to workers of union membership. The potential negative effects on union density from the provision by government of certain union-like services has been referred to as the *government substitution hypothesis*.[9]

Neumann and Rissman (1984; NR) were the first to address this substitution hypothesis systematically. As NR note, union density had been on a steady decline since the early 1950s when it peaked at over 30 percent. By 1980 union membership had dropped to roughly 22 percent of nonagricultural employment; almost identical to its level in 1939. The conventional argument attributed the decline to the changing composition of employment in the U.S.[10] However, NR found that, while structural changes had been important, less than 50 percent of the decline in union density could be accounted for by changing employment composition in the U.S. economy. This obviously left room for other explanations, including the possible effects of the increased supply of union-like services resulting from court-created at-will exceptions.

Even though, at the time their study was conducted, departures from the at-will doctrine were of very recent vintage and were not yet widely adopted, there existed a sufficient number of adoptions for two classes of exceptions—*implied-contract* and *public-policy*—to attempt an assessment of their effects.[11] Employing biennial cross-sectional state level data for 1964 through 1980, NR estimated the effects of these two types of exceptions on union density.[12] For each exception, two alternative measures were constructed. The first was a simple indicator variable equal to one if a state had an exception. The alternative was a time trend variable equal to unity in the year the exception was adopted, two in the following year and so on. NR found that while the coefficients on both *public-policy* variables were negative, neither achieved statistical significance. The results for the implied-contract exception, however, told an entirely different story. Each

[9] A review of the literature on this topic is provided by Coombs (2008).

[10] This is the central message of was referred to as the "saturationnist" argument; that is, structural factors were the driving force in union growth (or decline). See Moore and Newman (1975) for a test of this hypothesis.

[11] By the end of their sample period, 12 states had adopted *Implied-contract* exceptions. *Public-policy* exceptions existed in 19 states.

[12] In footnote 11 the authors make reference to a third class of exceptions, but do not indicate what class of exceptions it is. They refer to this third class as "legal minutiae, not relevant to the questions examined here." Since during their sample period only 4 states had adopted *good-faith* exceptions, we suspect this may be the class they were referring to.

measure was negative and highly significant, indicating that union density declines after the adoption of an *implied-contract* exception. At least with respect to the *implied-contract* exception, their take-away is that public provision of employment security competes with the private provision by unions and is consistent with the substitution hypothesis.

In contrast, Block, Mahoney and Corbitt (1987; BMC) found no support for the at-will substitution hypothesis. BMC addressed the question in a slightly different way. BMC tested whether at-will exceptions negatively impact NLRB representation elections, which they argue are the primary choice mechanisms for establishing collective bargaining within a firm (i.e. joining a union). Using pooled election data from January 1978 through August 1985, estimates from their probit model revealed that both the *public-policy* and *good-faith* exceptions had no significant impacts on election outcomes. Surprisingly, the implied-contract exception was significant, but the wrong sign—the authors provided no explanation for this counter-intuitive result.

Our study represents a substantial extension of this limited literature. Both the NR and BMC studies use data prior to the mid-1980s when the growth in state adoptions of at-will exceptions accelerated. In our repeated state cross-sectional time-series data we include a period of time well before states started adopting these at-will exceptions, as well as observations from the entire decade during which most of these exceptions were being created and a substantial period of time thereafter. Thus, our study has the distinct advantage of being able to more fully exploit the variation across states in both the extent and the timing of the adoption of employment-at-will exceptions.

Additionally, we revisit another unsettled question in the government substitution hypothesis debate: Has the adoption and expansion of union-like services provided by social welfare benefit programs for workers contributed to the decline of unions?[13] Since many of the major programs such as worker's compensation, unemployment compensation and health and safety laws found their origin in union contracts, it is certainly

[13] One can find conjectures of this hypothesis as early as Reder (1951; p. 517).

conceivable that these government programs substitute for their private provision by unions; thereby further reducing the attractiveness of union membership.

Again, the study by NR (1984) represents the first attempt to subject this variant of the substitution hypothesis to a rigorous test. To do this, the authors examined aggregate time-series data on union density in the U.S. for the period 1904 through 1980. Using a regression model similar to the reduced-form model developed by Ashenfelter and Pencavel (1969; AP), NR introduced a measure of the government supply of union-like services.[14] NR represented the effects of substitution possibilities by the inclusion of federal spending on social welfare, expressed as a fraction of GDP. Their measure of social welfare included expenditures on obvious substitutes like unemployment compensation and workmen's compensation, as well as the less obvious substitutes such as education and veteran's benefits.

In their first set of regressions over the period 1904-1960, the coefficient on welfare spending was negative, but not significant. However, when NR used the estimated coefficients to generate forecasts of union density in the 1961-1980 post sample period, their forecasts compared well with actual unionization only with the inclusion of social welfare expenditures. Estimating the model for the entire sample period (1904-1980) NR found that the coefficient on the welfare variable had a significant negative value. Based on the two alternative tests, the authors concluded that welfare expenditures substitute for the services supplied by unions, which suggests that the expansion of social welfare programs during this period contributed, at least in part, to the long-run decline in union density.

Freeman (1986) challenged NR's interpretation of the evidence. He argued that the negative effect of welfare spending on union density was weak and consequently, any change in the model specification, sample period or measure of welfare spending would produce insignificant results. Freeman used cross-country data for 1970 and 1980 to test the substitution hypothesis. Defining two different measures of social

[14] NR's specification was not fully equivalent to the AP model, however. The dependent variable in the AP model was defined as the rate of growth in union membership from the preceding period, while NR examined the percentage of the work force unionized at time t.

welfare spending, Freeman found a significant *positive* correlation between the level of welfare spending and changes in union density. Freeman thus, asserts that unions have actually managed better in countries with larger welfare programs.

In light of the conflicting evidence, Moore et al. (1989; MNS) reevaluated the government substitution hypothesis with respect to the relationship between government welfare spending and union density. MNS used three alternative time-series union growth models: the AP (1969) model; the Bain and Elsheikh (1975; BE) model; and the NR (1984) model. The authors restricted the definition of social welfare spending to include only those types of programs that clearly compete with traditional union services.[15] Further, MNS allowed for the possibility that social welfare spending is endogenous by implementing a 2SLS model.[16] Arguably, using a more restricted definition of welfare spending along with a correction for the possibility of endogeneity, allowed MNS to provide a more precise test of the substitution hypothesis.

Also, because evidence suggested there was a major structural change in U.S. labor markets following the passage of the Wagner Act in 1935, MNS estimated the three alternative time-series models for two different periods: the entire period (1929-1983) and the post Wagner Act period (1936-1983).[17] For the six regressions, MNS found relatively weak support for the substitution hypothesis. Over the 1929-1983 period MNS found no support for the substitution hypothesis. However, results from the 1936-1983 period did find support for the hypothesis in two of the time-series models—the BE and NR models. While the welfare coefficient had the correct sign, it failed to achieve significance in the AP model. The authors concluded that overall, time-series evidence on impact of welfare spending on union density was mixed.

[15] Specifically, MNS excluded expenditures on general education and expenditures on health and medical programs from the veterans programs. The authors added total spending by the Occupational Safety and Health Administration.
[16] As noted by many scholars, beginning in the 1930s labor unions started playing an increasingly larger role in supporting and advocating social welfare policies that protect workers. This would suggest that the direction of causality may run in both directions. For a general discussion of organized labor's views on these types of policies see BMC (1987) and Freeman and Medoff (1984).
[17] See Sheflin et al. (1981).

In summary, existing evidence for both variants of the government substitution hypothesis is mixed and hence, the debate remains unsettled. Our paper represents a substantive extension of the extant literature in at least three ways. First, both the at-will exceptions and social welfare variables are evaluated within a single empirical model. Previous studies examined these issues separately utilizing different estimation strategies, outcome variables and data (mixtures of cross-section and time-series). Second, we use data from a 47-year repeated cross-section of states. As mentioned earlier, this has the distinct advantage of permitting us to more fully exploit the variation across states in both the extent and the timing of the adoption of employment-at-will exceptions. Third, rather than use expenditures on an amalgamation of several different social welfare programs, we disentangle these behaviorally distinct programs and instead, introduce three welfare program variables separately into the regression model. The empirical question addressed in this paper is whether these court created at-will exceptions and the growth in state level social welfare benefit programs for workers have substituted for their private provision by unions thereby reducing the attractiveness of union membership and contributing in part to the long-run decline in union density.

II. Background

***Decline in Union Density*.**

Union density in the U.S. reached its peak in the early fifties and then began a steady seven-decade decline.[18] In 1953 union membership represented 32.5 percent of the workforce. By 2018 the percent of wage and salary workers who were union members had dropped to 10.5 percent. The long-run decline in union density has occurred within almost all major industries and across all demographic groups of workers.[19]

[18] Reder (1951, p. 518) offered this prescient warning, "In the United States, the heroic period of union founding is over."

[19] Union density within the public-sector represents the one remarkable exception, however. Unlike the steady decline that was occurring within the private-sector, union membership within the public-sector expanded rapidly from the early 1960s to the mid-1970s; with union density increasing from 10.6 percent in 1961 to 40.2 percent in 1976. After a dip ending in the early 1980s, public-sector union density was relatively stable for about 3 decades, then declined again from 2011 to the present. Nevertheless, the membership rate of public-sector workers in 2018 was 33.9 percent; more than five times higher than that of private-sector workers (6.4 percent). Union density for local government workers was 40.3 percent—the highest among all public-sector workers.

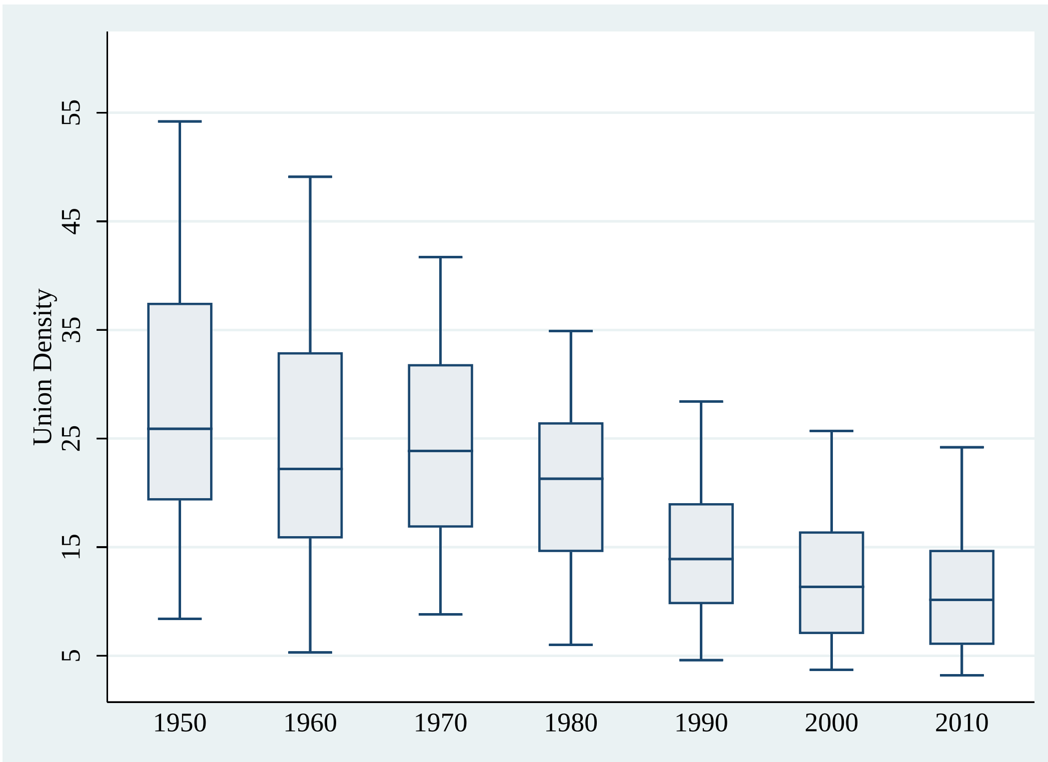
While union membership varies significantly among states, currently ranging from a low of 2.7 percent in North Carolina and South Carolina to a high of 22.3 percent in New York, without exception union density in every state has steadily declined for decades. Using box plots, Figure 1 shows the distribution of union density between the 48 contiguous states (not including the District of Columbia) for each census year starting in 1950 through 2010. Box plots provide convenient and efficient mechanisms for summarizing the changes in the distribution of union density across 48 states over time. Each box plot displays a five-number summary statistic of the distribution of union density across states in that year.[20] These summary statistics include the minimum, 25th quartile, the median, 75th quartile, and the maximum. The box indicates the lower and upper quartiles of state union density and the median value (in this case, the average of the union densities for the two states ranked 24 and 25) is represented by a line subdividing the box.[21] The length of the box (difference between the upper and lower quartiles) represents the interquartile range (IQR) and is used to generate the lines (whiskers) spanning all observations within 1.5(IQR) of the nearer quartile. Additionally, since we have no observations on state union density that fall outside 3(IQR), the adjacent lines on the whiskers represent the minimum and maximum values.

Figure 1 shows that all five summary measures have decreased from 1950 through 2010. The maximum value for state union density has decreased from Washington's 54.2 percent in 1950 to New York's 24.2 percent in 2010. Additionally, the minimum value for state union density has also decreased from 8.4 percent in 1950 (North Carolina) to 3.2 percent in 2010 (North Carolina). Note also that the boxes get tighter over the sample period. This implies that state union densities in the upper- and lower-quartiles are converging. Moreover, even though the lower quartile is getting smaller, the tightening of the IQR is expounded by a decrease in the upper quartile. For example, state level union density for the 75th quartile was 37.5 percent in 1950 and 14.75 percent in 2010. In comparison, state level union density for the 25th quartile was 19.3 percent in 1950 and 6 percent in 2010. In other words, whereas 50 percent of all state

[20] See Spear (1952) or Tukey (1972) for details on how these box plots are constructed.
[21] Interesting to note that these are also called "Tukey's Hinges."

level union densities in 1950 were between 37.5 and 19.3 percent, in 2010 50 percent fall between 14.75 and 6 percent. Finally, although the median shows a slight uptick in 1970 at 23.85 percent relative to the median in 1960 (22.2 percent), the trend in state level union density was strongly decreasing.

Figure 1. Decline in State-Level Union Density



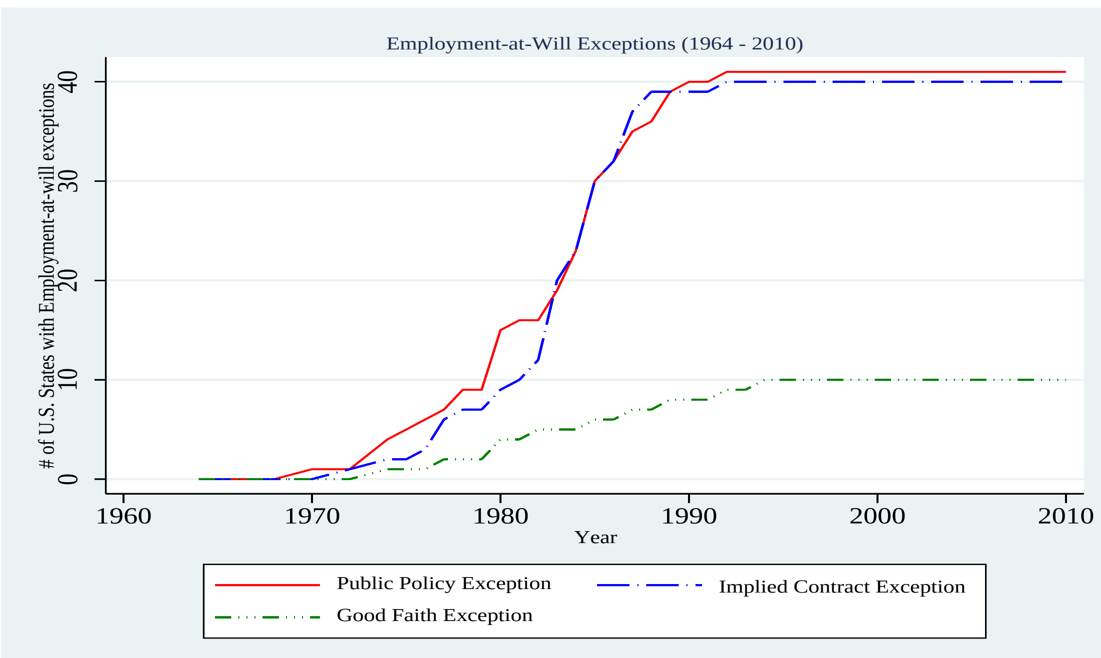
Source: Authors' calculations.

Surge in Adoptions of EAW Exceptions.

Prior to the 1980s only a few states recognized any at-will exceptions other than exceptions contained in federal legislation. Then beginning in the early 1980s state court rulings creating at-will exceptions proliferated. It was during this decade that an extensive discussion occurred within the legal profession about whether employment relationships should remained governed by the at-will doctrine or move to a just-cause standard.[22] According to Lopathka (1984) this debate became "the labor law issue of the 80s" or as Hoerr (1985) put it, "the hottest topic in the field of law."[23]

The phenomenal growth in the adoption of at-will exceptions during the 1980s can clearly be seen in Figures 2 and 3. Prior to 1980, only 9 states had adopted a *public-policy* exception. By the end of the 1980s, an additional 21 states did so. There was an equally impressive upsurge in adoptions of the *implied-contract* exception. Only 7 states recognized this exception prior to 1980, but by the end of 1989 a total of 32 states had created *implied-contract* exceptions. Finally, of the 10 states that currently recognize *good-faith* exceptions, 6 were adopted during the decade of the 80s. Only 5 more exceptions were adopted after the 1980s; coming to an end with Wyoming's adoption of a *good-faith* exception in 1994. Four states, Florida, Georgia, Louisiana and Rhode Island, continue to adhere to the at-will doctrine—that is, courts in these states have declined to adopt any of the three classes of exceptions.

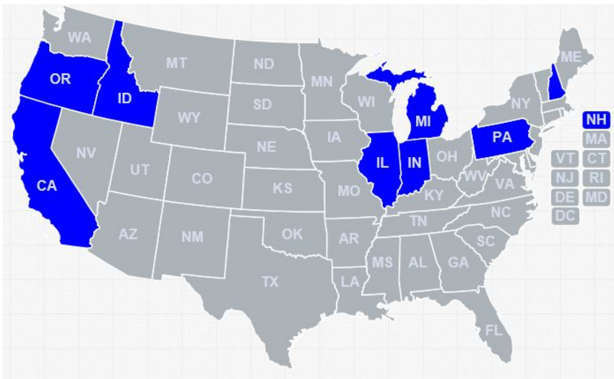
Figure 2. State Adoptions of Employment-at-Will Exceptions



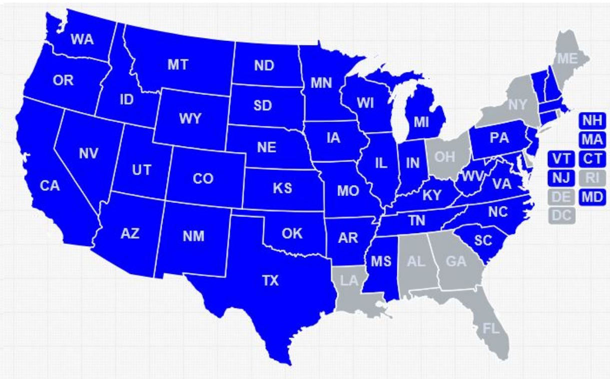
Source: EAW exception plots calculated from ADS (2006) coding.

[22] See Miles (2000).
[23] This emerging issue was the subject of Professor Stieber's Presidential Address delivered at the *Industrial Relations Research Association* meetings in 1984. See Stieber (1984).

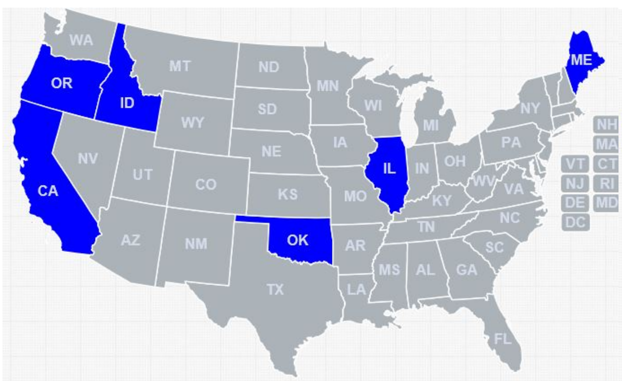
Figure 3. Status of At-Will Exceptions (Blue) in the 1970s and 1980s



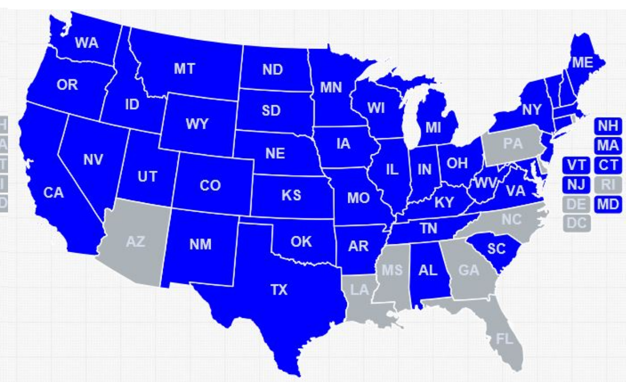
(a) ***Public-Policy*** **Exceptions 1970s**



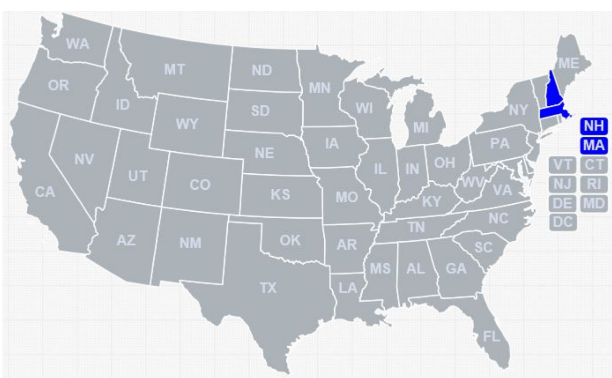
(b) ***Public-Policy*** **Exceptions 1980s**



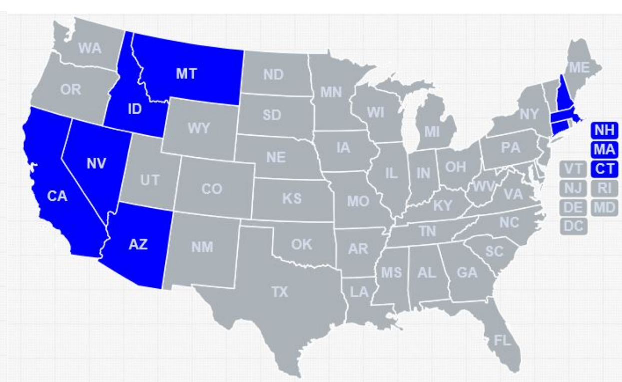
(c) ***Implied-Contract*** **Exceptions 1970s**



(d) ***Implied-Contract*** **Exceptions 1980s**



(e) ***Good-Faith*** **Exceptions 1970s**



(f) ***Good-Faith*** **Exceptions 1980s**

Source: EAW exception maps constructed form ADS (2006) coding.

III. Data and Variable Definitions

The panel data set used in this study contains 47 years of repeated annual cross sections for the forty-eight contiguous states from 1964 through 2010. Our data set includes observations from the two decades prior to and two decades subsequent to the surge in the adoption of at-will exceptions during the decade of the 1980s. A distinctive feature of this data set is that it contains multiple natural experiments created by state court adoptions of at-will exceptions that were staggered over time and across states. Since a large number of the exceptions were adopted in different states in different years, we are able to examine the before-after effect of an unexpected change in the at-will environment in the adopting state (treatment group) relative to the before-after effect in those states in which no change occurred in their at-will status (control group).

Union density in each state is defined as the percentage of nonagricultural wage and salary employees who are union members.[24] For data on the employment-at-will exceptions, we use the ADS (2006) coding, which identifies the year and type of exception adopted in each state. [25] Their coding links a change in the law to the year in which a precedent-setting court decision was made, and thus, allows us to capture the effect on union membership of an unexpected change in the law. These exceptions were not adopted for the purpose of creating just-cause protections to substitute for those protections provided by unions. That is, these adoptions evolved through precedent-setting court decisions that were highly unlikely to have been a response to trends in union membership.[26]

To measure the effect of the three at-will exceptions, we define a simple indicator variable for each exception, taking the value zero up to the year prior to a state's adoption of that exception and unity every

[24] Union membership data by state are reported in *unionstats.com*. The data base is constructed by Barry Hirsch and David Macpherson and is updated annually. Their annual state level union membership data begins in 1964. A description of the methodology used to construct a consistent time series on union membership can be found in Hirsch, Macpherson and Vroman (2001) and also Hirsch and Macpherson (2003).

[25] We consulted Johnson (2017) to determine whether there were additional adoptions subsequent to the ADS coding. We could find none.

[26] Autor (2003) addressed this issue by estimating probability models to determine whether states in which unions were growing or declining faster relative to other states were more likely to adopt exceptions. He found no evidence to support that hypothesis.

year thereafter. The indicator variable remains zero for states that never adopt that particular exception. Let *PP* represent the indicator variable for a *public-policy* exception. Likewise for an *implied-contract* exception, we have *IC* and for the *covenant of good-faith* we have *CGF*.

To examine the second variant of the government substitution hypothesis, we consider the effects on union density of state-level union-like social welfare programs. Our social welfare programs include benefits provided by two social insurance programs and benefits derived from health and safety inspections by the Occupational Health and Safety Administration (OSHA). These programs clearly compete with the benefits traditionally provided to unionized workers in collective bargaining agreements. One form of social insurance is *workers' compensation*; state-run programs which provide workers with payments to cover lost wages and expenses for medical treatment as a result of a workplace injury. These programs are state regulated and the level of benefits, along with the administrative mechanisms used to provide these benefits, vary significantly from state to state.[27] We define worker's compensation (*WC*) as total state level program spending as a fraction of the civilian labor force.[28] A second program variable included in our model is *unemployment insurance*. This is a federal-state program which provides unemployment benefits to eligible workers who become unemployed through no fault of their own. While the provisions that govern the coverage and financing are uniform nationally, the provisions that govern the eligibility, timing and *level* of benefit payments vary among states. The program is funded through taxes paid by employers and the tax rate varies between firms based on the number of employee claims an employer has had to pay out (experience rating). The experience rating system provides firms with an incentive to avoid laying off workers or cutting positions. We define unemployment insurance (*UI*) as total program spending within each state as a fraction of its civilian labor force. The third social welfare program variable (*OSHA*) measures the intensity of state-level activities intended to define and enforce *safety and health requirements* in the workplace. OSHA was established by an act of Congress in 1970 and was charged with setting and

[27] See Clayton (2003/2004) for a detailed discussion of the history of workers' compensation laws in the U.S. along with examples of the state-to-state variations in program structure and benefit levels.
[28] All expenditure measures are expressed in constant dollars.

enforcing health and safety standards in the workplace. Among the types of inspections performed by OSHA, the most common are programmed inspections, aimed at reducing hazards on jobsites and based on factors such as rate of occurrence of injury and citation history, but also includes random selection.[29] In principle, OSHA regulations apply uniformly across states, but states were permitted and encouraged to independently administer their own occupational safety and health programs. Currently, there are 23 states in our sample with their own independent OSHA-approved programs; 19 of these states cover both private and public sectors, while 4 states cover only the public sector.[30] While OSHA-approved state plans must set workplace safety and health standards that are "at least as effective as" OSHA standards, each state can set their own standards for hazards not addressed by federal OSHA standards, but the state is required to conduct inspections to enforce its standards. Further, state plans can impose higher fines or stricter penalties than OSHA. State plans also have the option to set standards covering hazards not addressed by Federal OSHA standards. *OSHA* is defined as a simple indicator variable for each state with an OSHA-approved plan. *OSHA* takes a value of zero up to the year in which a state creates its own plan. *OSHA* then takes a value of unity in the year the state plan was approved and every year thereafter. The indicator variable remains zero for states that never adopt their own plan.

Finally, we include the following set of state level control variables that previous studies have shown influence union density: (1) the percent of females in labor force (*FLF*); (2) the percent of the labor force age 55 and older (*SLF*); (3) percent of the state's population living in urban areas (*URB*); (4) the proportion of a state's real GDP from manufacturing to its real GDP from services (*MANU*); (5) a regional dummy

[29] See https://www.osha.gov/laws-regs/oshact/section_8.

[30] A fifth state, Maine, received initial approval in 2015 for its own state plan covering state and local government workers, only.

variable taking a value of one for states in the South and zero otherwise (*SOU*);[31] and (6) a dummy variable taking a value of one for states with a *Right-to-Work* law (*RTW*) and zero otherwise.[32]

IV. Empirical Design

We follow the convention in the literature of analyzing the determinants of union membership within a demand and supply framework *ál la* Ashenfelter and Pencavel (1969; AP). So, consider the following reduced-form equation:[33]

$$U_{st} = \Upsilon_s + \alpha_t + \beta_{PP}PP_{st} + \beta_{IC}IC_{st} + \beta_{CGF}CGF_{st} + \beta_{WC}WC_{st} + \beta_{UC}UC_{st} + \beta_{OSHA}OSHA_{st} + \sum \beta_i X_{ist} + \mu_{st} \quad (1)$$

where U_{st} is union density in state s at time t. Vectors Υ_S and α_t represent time-invariant state effects and state-invariant time effects, respectively, and μ_{st} is an unobserved idiosyncratic error-term.[34] Matrix X contains the control variables defined above that have been found to influence union membership. This empirical design exploits the variation across states in both the extent and timing of the adoption of employment-at-will exceptions.[35]

[31] The South is defined as the 11 states of the southern Confederacy—Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas and Virginia—plus Oklahoma and Kentucky.

[32] A RTW law prohibits collective bargaining agreements that include a union shop provision; that is, employees operating in a unionized firm are banned from negotiating contracts that require all employees join the union or pay union dues. Currently 27 states have right-to-work laws. At the beginning of our sample period 19 states had RTW laws; with an additional 3 states adopting RTW laws during our sample period.

[33] Because data on the relative prices of union membership are not easily attainable, estimating structural equations that describe the demand for and supply of union services is not possible. Further, obtaining accurate measures of the relative prices of union membership would be enormously complicated due to the fact that the "price" of union membership is multidimensional and would include such things as initiation fees, monthly dues and lost income from work stoppages.

[34] We include a dummy variable for each state in the models to control for the estimated state-invariant effects. Because $S(48) \approx T(47)$, and to support our boxplots, we use decade-dummy variables to allow for aggregate time-invariant effects. See Wooldridge (2002, Ch.7).

[35] Table 3A in the appendix provides the means and standard deviations for the dependent and independent variables included in the models. The within- and between- variation is also provided in the table.

Since a plausible argument can be made that causation between union density and social welfare runs in both directions, our empirical design considers each welfare variable as endogenous.[36] Further, to the extent organized labor has played an instrumental role in the development, design and expansion of social welfare policies that protect workers, the coefficient on each welfare variable in a single equation model will likely be biased toward zero because the negative effect of each welfare variable on union density will be at least partially offset by the positive effect running in the opposite direction. Fitting a model such as a single-equation instrumental variables regression estimated via two-stage least squares requires that there must be one or more variables that are correlated with each endogenous variable but uncorrelated with the disturbance μ_{st}. Consider the following reduced form equations:[37]

$$WC_{st} = \varepsilon_s + \rho_t + \Lambda_{EC}EC_{st} + \Sigma\Lambda_i X_{sti} + \upsilon_{st} \tag{1a}$$

$$UC_{st} = \eta_s + o_t + Y_{IMB}IMB_{st} + \Sigma Y_i X_{sti} + \varsigma_{st} \tag{1b}$$

$$OSHA_{st} = \omega_s + \sigma_t + \Psi_{STPLAN}OSHAINSP_{st} + \Sigma\Psi_i X_{sti} + \varphi_{st} \tag{1c}$$

where the matrix X contains the exogenous control variables defined above. *EC*, *IMB*, and *OSHAINSP* are exogenous variables that are excluded from equation (1). Similar to equation (1), the first two vectors in each equation are time-invariant state effects and state-invariant time effects, respectively. The instruments included in the reduced form models but excluded from the regression equation are employer contributions for government social insurance (*EC*), income maintenance benefits (*IMB*), and the number of planned OSHA inspections in a state as a ratio of the number of establishments in the state (*OSHAINSP*). Contributions by employers for government social insurance (*EC*) includes employer payments under Old-

[36] Since adoption of at-will exceptions result from court-created exceptions and were not the outcome of legislation, we have minimal concerns about the endogeneity of at-will exceptions. We agree with NR (1984) who contend that a reverse causality argument would be "very strained." Also, as ADS (2004) recognize, using a two-stage procedure to "instrument for legal variation may be a cure worse than the disease." Also, recall Autor (2003) formally addressed this issue and found no evidence of simultaneity.
[37] Equations 1a – 1c are examples of reduced form equations in the sense that we have written an endogenous variable in terms of exogenous variables. Following the general terminology used in simultaneous equations models, equation (1) is therefore the structural or regression equation.

age, Survivors', and Disability Insurance (i.e., Social Security) and Hospital Insurance (also known as Medicare). These components of *EC* are plausibly similar to the safety net provided by workers' compensation. Additionally, Income maintenance benefits (*IMB*) consists of Supplemental Security Income (SSI), Earned Income Tax Credit (EITC), Supplemental Nutritional Assistance (SNAP), and other income maintenance benefits aimed at offsetting some of the adverse effects from short- and long-term unemployment. Finally, the number of planned OSHA inspections per establishment (*OSHAINSP*) in a state is expected to have a direct relationship with whether the state has an OSHA approved state plan. It is assumed that in states with relatively more OSHA inspections, economies of scale would lead to state's running their own plans in order be more efficient. If these variables are correlated with the endogenous variables - *WC*, *UC*, and *OSHA* - uncorrelated with μ_{st}, 2SLS, and the idiosyncratic error terms, υ_{st}, ς_{st}, and φ_{st}, are each assumed to be pairwise uncorrelated with the elements of μ_{st}., 2SLS will produce consistent estimates. Our interests lie in the results of the following structural equation.[38]

$$U_{st} = \gamma_s + \alpha_t + \beta_{PP}PP_{st} + \beta_{IC}IC_{st} + \beta_{CGF}CGF_{st} + \beta_{WC}\widehat{WC}_{st} + \beta_{UC}\widehat{UC}_{st} + \beta_{OSHA}\widehat{OSHA}_{st} + \sum \beta_i X_{ist} + \mu_{st} \qquad (2)$$

IV. New Estimates of the Substitution Hypothesis

The Pooled OLS results from simultaneously testing both variants of the government substitution hypothesis in one model are reported in Table 1.[39] Overall, our results provide strong evidence consistent with the predictions from both variants of the *government substitution hypothesis*.

Estimates of the effects of each court-created at-will exception on union density are contained in the first panel. Models 1 through 3 test each at-will exception separately. Based on these results, we can infer that

[38] To test the correlation, we estimated each reduced form equation by ordinary least squares (OLS) including state- and decade-fixed effects. The coefficients and robust standard errors (clustered at state level, in parentheses) for *EC*, *IMB*, and *OSHAINSP* 102.081 (41.643), 0.330 (0.045), and 0.003 (0.000), respectively. Moreover, the coefficients on the obtained residuals from estimating each reduced form equation and including them in the regression equation are statistically significant at the 10-percent level or higher (see Hausman [1978]).
[39] See Mundlak (1978) for estimating pooled cross-sectional time series data.

all three classes of court-created at-will exceptions have produced *negative* unintended effects on union membership. Consistent with the substitution hypothesis, all the at-will exception measures have negative coefficients and all are statistically significant. Model 4 includes all three indicator variables.[40] Each coefficient is negative and statistically significant. The results suggest that union density in states that have adopted a *public-policy* exception is a little less than .73 percent lower than in states that have not adopted this particular exception. Similarly, within states that have adopted *implied-contract* exceptions, union density is roughly 1.5 percent lower than states without such an exception. Finally, the adoption of a *covenant of good-faith* exception reduces union density by 1.5 percent below states without such an exception. Our interpretation is that the evidence strongly supports the hypothesis that government provision of just-cause protections for workers represent relatively strong substitutes for those protections traditionally included only within collective bargaining agreements thus, reducing the net benefits to workers of union membership. The result is that these court-created at-will exceptions have had a demonstrable negative impact on union density in the states where they have been adopted.[41]

It is important to note that while the three exceptions provide just-cause protections for disparate reasons, *each* acts as a substitute (at least partially) for the provision of the same protections embodied in collective bargaining agreements; the hallmark of union's appeal to workers. Further, since the criteria for a wrongful discharge claim under each class of exceptions do not overlap, it is reasonable to assume that a state's adoption of multiple exceptions substantially expands the scope of wrongful discharge protections, thereby creating a stronger substitute for the just-cause protections provided by unions. We address the issue of multiple adoptions in the next section.

[40] Also, included in the appendix is Model 4 without a treatment for the endogenous variables.

[41] The pattern of results for the at-will exceptions in Table 1 is robust to alternative specifications, particularly with respect to the inclusion/exclusion of sub-sets of control variables. We also introduced a time trend variable for each exception, taking the value of one in the year that a state adopted that exception, a value of two in the subsequent year and so on. The coefficients on the time trend for *PP* and *IC* were each negative and significant. While coefficient on the time trend for *CGF* was also negative, it was not statistically significant.

With respect to the second variant of the substitution hypothesis, our results suggest that government activities associated with the provision of workers compensation programs and the regulation of health and safety standards in the workplace also substitute, at least partially, for similar services provided by unions. In the full model, coefficients on two social welfare programs, (*WC*) and (*OSHA*), have the anticipated negative signs and are statistically significant---consistent with the *government substitution hypothesis*. This appears to be particularly the case for the effects on union density resulting from implementation of independent state OSHA-approved safety and inspection programs. States with their own OHSA-approved plans cause union density to be roughly 9.8 percent lower than states relying on the federal OHSA inspection standards.

One unexpected result is the estimated *positive* and significant coefficient for unemployment insurance (*UI*). At first blush, this appears to be a counter-intuitive result; especially in light of previous studies that included spending on unemployment compensation as one component of total social welfare spending under the explicit assumption that unemployment insurance programs "clearly" compete with traditional union services.[42] Even though it is empirically small, the positive and statistically significant coefficient on *UI* is clearly inconsistent with this hypothesis. One plausible explanation might be found in Ashenfelter and Pencavel's influential time-series study of union growth. The authors assumed that the stock of labor grievances at any point in time and the resultant positive impact on union membership is a function of the unemployment rate; specifically, the unemployment rate in the preceding trough of the business cycle (U^P).[43] To the extent that the level of spending on unemployment insurance really serves as a proxy for the unemployment rate, *UI* might thus, be viewed as a proxy for a pool of labor grievances, in which case, our results make sense. In AP's basic model, the coefficient on U^P was positive and statistically significant,

[42] For examples, see NR (1984) and MNS (1989).
[43] The authors allowed for the possibility that the positive effect on union membership of a given pool of grievances at the trough decays with time. Since they found no evidence of decay, their grievance variable amounted to a step function, with the unemployment rate constant from one trough to the next.

lending empirical support for this hypothesis.[44] Additional support can also be found in the NR (1984) study. In their time-series model of union growth (percent organized), NR included a contemporaneous unemployment rate (U_t), which was positive and significant in all of their specifications.

One important implication of this re-interpretation of the *UI* program is that it may explain, at least partially, why MNS found mixed evidence for welfare spending's effect on union membership. Unemployment insurance was included in their restricted definition of welfare spending, possibly diluting its estimated effect on union membership.

Overall, our results go a long way toward resolving the debate about whether at-will exceptions and social welfare programs have, on the margin, contributed to the decline in union density. The results strongly suggest that these government interventions, which provide just-cause protections, social insurance and health and safety protections, have resulted in the unintended effects of offering close substitutes for the same protections historically the exclusive domain of union collective bargaining agreements.

[44] As Hirsch and Addison (1986) point out, the underlying assumption for this relationship to hold is that workers expect joining a union will provide job security, particularly over the business cycle.

Table 1. 2SLS Results for State Union Density (1964-2010)

	Model 1	Model 2	Model 3	Model 4
EAW Exceptions:				
PP	-1.393***			-0.723*
	(0.430)			(0.364)
IC		-1.643**		-1.456**
		(0.686)		(0.697)
CGF			-1.654*	-1.549*
			(0.843)	(0.817)
Social Welfare:				
UI	0.005***	0.004***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
WC	-0.023**	-0.019*	-0.028***	-0.017*
	(0.009)	(0.010)	(0.010)	(0.010)
OSHA	-9.461***	-9.870***	10.736***	-9.848***
	(1.845)	(2.037)	(1.953)	(1.977)
Adjusted R-squared	0.93	0.93	0.93	0.93
N	2,256	2,256	2,256	2,256

Notes: Dependent Variable = (Fraction Unionized) x 100. In addition to the control variables, all models include both state and decade fixed effects. The robust standard errors clustered at the state level are reported in parentheses; * *p<0.1; ** p<0.05; ***p<0.01*. Social welfare variables are the predicted values from their respective reduced-form regressions. Complete results are reported in the Appendix, Table 1A. OLS results are also reported in the Appendix, Table 4A.

V. Strength in Numbers: Multiple Adoptions and the Strength of the Substitution Hypothesis

Since the three at-will exceptions are disparate in their criteria and the scope of coverage for just-cause protections, our hypothesis is that a state's adoption of more than one exception represents a substantive expansion in the scope of that state's wrongful discharge protections, producing a closer substitute for the just-cause protections provided by unions; that is, the substitution effects become stronger. Our hypothesis implies that as a state expands the scope of its legal just-cause protections by adopting more than one at-will exception, the marginal impact (negative) on union density will increase. Once again we exploit the variation that exists across states in both the extent and the timing of their adoption of at-will exceptions.

Table 2 shows how the move from a single at-will exception to a second exception was staggered across states and time. Thirty-nine states ultimately adopted a second at-will exception. On average, 3.4 years separated the adoption of the first exception from the adoption of the second exception; with a range of zero to 14 years. In 1972 California became the first state to adopt a second at-will exception. In 1992, Delaware and Mississippi were the last states to adopt a second at-will exception.

Table 3 reports the same staggered nature of adoptions for states that adopted a third exception. A total of eight states adopted all three exceptions; with an average of 8 years separating the adoption of the second and third at-will exceptions; with a range from 5 to 12 years. In 1980 California became the first state to adopt a third exception, while Wyoming in 1994 became the last state to adopt a third exception.

To test whether the strength of the substitution effect increases as the number of adoptions increases, we define four indicator variables measuring step-wise and discrete movements from a state's initial employment-at-will environment (*NONE*), to its adoption of the first at-will exception (*FIRST*), its second (*SECOND*) and then possibly a third (*THIRD*). The indicator variables are defined in the following way:

$$NONEst = \begin{cases} 1 & if\ PPst + ICst + CGFst = 0 \\ 0 & otherwise \end{cases}$$

$$FIRSTst = \begin{cases} 1 & if\ PPst + ICst + CGFst = 1 \\ 0 & otherwise \end{cases}$$

$$SECONDst = \begin{cases} 1 & if\ PPst + ICst + CGFst = 2 \\ 0 & otherwise \end{cases}$$

$$THIRDst = \begin{cases} 1 & if\ PPst + ICst + CGFst = 3 \\ 0 & otherwise \end{cases}$$

The variable *NONE* represents the initial condition for all states; that is, when employment relations in states were governed by the employment-at-will doctrine. All forty-eight states begin with *NONE* = 1. This variable takes the value of unity up to the year a state adopts its first exception and then zero thereafter.[45] The variable *FIRST* takes the value zero up to the year a state adopts its first exception and then unity until it adopts its second exception, at which time it reverts to zero.[46] The indicator variable *SECOND* takes the value of zero up to the year a state adopts its second at-will exception. It remains at unity thereafter unless, at some future point, the state adopts a third exception, at which time *SECOND* reverts to a value of zero. Of the 39 states that adopted two exceptions (Table 2), only six adopted two exceptions in the same year.

The last indicator variable *THIRD* takes the value of zero up to the year that a state adopts its third at-will exception and unity thereafter. A total of 31 states adopted two at-will exceptions and declined to adopt a third. Of the eight states that ultimately adopted a third exception, none adopted all three exceptions in a single year. Therefore, each state in this group will, for some period of time, have a value of unity for the indicator variable *SECOND*. But, beginning in the year that the third exception is adopted the value of *SECOND* reverts to zero.

Finally, we also define an alternative measure to test for the impact of multiple adoptions on union density. To do this, we define a discrete variable equal to the number of exceptions that exist in each state in each year. It is defined as:

$$TOTAL_{st} = PP_{st} + IC_{st} + CGF_{st}$$

TOTAL ranges in value from zero to three. During the period when each state's employment relations were governed by the at-will doctrine, *TOTAL* = 0.

[45] In 1970 California became the first state to adopt an at-will exception.
[46] Six states adopted two at-will exceptions in a single year: Arizona (1985); Connecticut (1985); Delaware (1992); Idaho (1977); Kentucky (1983); and New Hampshire (1974). Thus, for these states *TWO* remains zero for the entire sample period.

Table 2. Year of First and Second Adoptions of At-Will Exceptions and Years between Adoptions

	PP	*IC*	*CGF*	Year of Second	Years Between
Arizona	1985		1985	1985	0
Arkansas	1980	1984		1984	4
California	1970	1972		1972	2
Colorado	1985	1983		1985	2
Connecticut	1980		1980	1980	0
Delaware	1992		1992	1992	0
Illinois	1978	1974		1978	4
Idaho	1977	1977		1977	0
Indiana	1973	1987		1987	14
Iowa	1985	1987		1987	2
Kansas	1981	1984		1984	3
Kentucky	1983	1983		1983	0
Maryland	1983	1985		1985	2
Massachusetts	1980		1977	1980	3
Michigan	1976	1980		1980	4
Minnesota	1986	1983		1986	3
Mississippi	1987	1992		1992	5
Missouri	1985	1983		1985	2
Montana	1980		1982	1982	2
Nebraska	1987	1983		1987	4
Nevada	1984	1983		1984	1
New Hampshire	1974		1974	1974	0
New Jersey	1980	1985		1985	5
New Mexico	1983	1980		1983	3
North Dakota	1987	1984		1987	3
Ohio	1990	1982		1990	8
Oklahoma	1989	1976		1989	13
Oregon	1975	1978		1978	3
South Carolina	1985	1987		1987	2
South Dakota	1988	1983		1988	5
Tennessee	1984	1981		1984	3
Texas	1984	1985		1985	1
Utah	1989	1986		1989	3
Vermont	1986	1985		1986	1
Virginia	1985	1983		1985	2
Washington	1984	1977		1984	7
West Virginia	1978	1986		1986	8
Wisconsin	1980	1985		1985	5
Wyoming	1989	1985		1989	4
					AV=3.4

Source: Calculated from ADS (2006) coding.

Table 3. States Adopting All Three Exceptions and Number of Years between Adoptions

				Years Between		
	PP	*IC*	*CGF*	1 & 2	2 & 3	1 & 3
California	1970	1972	1980	2	8	10
Connecticut	1980	1985	1980	0	5	5
Idaho	1977	1977	1989	0	12	12
Massachusetts	1980	1988	1977	3	8	11
Montana	1980	1987	1982	2	5	7
Nevada	1984	1983	1987	1	3	4
New Hampshire	1974	1988	1974	0	14	14
Wyoming	1989	1985	1994	4	6	9
			AV =	2	8	9

Source: Calculated from ADS (2006) coding.

Table 4 displays the results obtained by introducing variables designed to measure the effects on union density from a step-wise and discrete expansion in the scope of a state's just-cause protections produced by multiple adoptions. While Models 1 through 3 are the same, we alternate the excluded indicator variable in order to more clearly illustrate the marginal impacts resulting from the step-wise adoption of at-will exceptions. The results reported in Model 1 are based on *NONE* as the reference group; that is, states with employment relations still governed by the employment-at-will doctrine at time *t*. The coefficient on *FIRST* is negative, as expected, but it is not significant, implying that the adoption of only a single at-will exception may not provide a significantly strong substitute for the provision of just-cause protections by unions. Our estimations suggest that it takes more than one at-will exception before the substitution effect has a significant negative impact on union density. Once a state adopts its second at-will exception, however, the expanded scope in just-cause protections provides a closer substitute for union services and hence, produces a larger and statistically significant impact on union density. Our results suggest that union density is roughly 2.03 percent lower in states that recognize two exceptions relative to states governed by EAW. It should be pointed out that not only does this include states that adopt no more than two exceptions, but also includes the effect on states that ultimately adopt their third employment-at-will exception. Moreover, in states with all three exceptions, union density is estimated to be 3.75 percent lower relative to EAW states.

The specification of Model 2 allows for a direct test of the hypothesis that states with two and three exceptions will have lower union densities compared to states with only one exception. As reported in the second column, union density in states with two exceptions is roughly 1.56 percent lower than states with one exception and for states that recognize all three exceptions union density is approximately 3.28 percent lower.

To the extent that states with all three exceptions provide just-cause protections that are closer substitutes for union services than states with only two exceptions, we expect the negative impact on union density of *THIRD* will be significantly larger than *SECOND.* A direct test of this is provided by Model 3. Using *SECOND* as the reference group, the coefficient on *THIRD* is -1.715 and is statistically significant. These results imply that states with all three exceptions are expected to observe a 1.72 percent lower union density, on average, compared to states with two exceptions.[47]

Finally, to test the hypothesis that the negative effect on union density increases as the number of exceptions that states recognize increase, we include *TOTAL* in Model 4. The coefficient is negative and statistically significant, which implies a 1.19 percent decrease in union density, on average, for each exception that a state recognizes. Whereas the Models 1 – 3 allowed us to compare individually the specific changes from EAW to states with one exception, and then states with two exceptions compared to one, and so on, the incremental effect of adding another exception to the basket of substitutes, Model 4, provides an average effect from each exception (-1.193). If we consider again the first three models, the coefficients are -0.471, -1.562, and -1.715, for the comparisons of *FIRST* to *NONE*, *SECOND* to *FIRST*, and *THIRD* to *SECOND*, respectively. Collectively, these results imply the negative effect on union membership increases as states adopt multiple exceptions to the employment at will doctrine.[48]

[47] An indirect test of the hypothesis that states with three exceptions will observe lower union densities than states with two exceptions is that the coefficient on *THIRD* minus the coefficient on *SECOND* is equal to zero in Models 1 and 2. The test statistics and standard errors are identical to the coefficient on *THIRD* in Model 3.

[48] We also specified a model that includes a quadratic term for *TOTAL* and included this (*TOTALSQ*) variable with *TOTAL* in the model. The coefficients (absolute value of roust standard errors in parentheses) on *TOTAL* and *TOTALSQ* are -.0.727 (0.585) and -0.183 (0.238), respectively and although neither coefficient was statistically

Table 4. 2SLS Impact on Union Density from Multiple Adoptions of Employment-at-Will Exceptions (1964-2010)

	Model 1	Model 2	Model 3	Model 4
EAW Exceptions:				
NONE		0.471	2.032***	
		(0.485)	(0.592)	
FIRST	-0.471		1.562**	
	(0.485)		(0.676)	
SECOND	-2.032***	-1.562**		
	(0.592)	(0.676)		
THIRD	-3.746***	-3.276***	-1.715**	
	(0.993)	(1.021)	(0.763)	
TOTAL				-1.168***
				-0.299
Social Welfare:				
UI	0.003**	0.003**	0.003**	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
WC	-0.018*	-0.018*	-0.018*	-0.017*
	(0.009)	(0.009)	(0.009)	(0.009)
OSHA	-9.996***	-9.996***	-9.996***	-9.558***
	(1.870)	(1.870)	(1.870)	(1.990)
Adjusted R-squared	0.93	0.93	0.93	0.93
N	2,256	2,256	2,256	2,256

Note: In Model 1, the *F*-statistic and *p*-value are 6.04 and 0.0178, respectively, for the test that $\beta_{SECOND} - \beta_{FIRST} = 0$. In the same model, the *F*-statistic and *p*-value are 7.32 and 0.0095, respectively, for the test that $\beta_{THIRD} - \beta_{FIRST} = 0$. All models include state- and decade-fixed effects; Robust (clustered at the state-level) standard errors in parentheses; * p<0.1; ** p<0.05; *** p<0.01; Social welfare variables are the predicted values from their respective reduced-form regressions.

significant, the joint test that both coefficients are equal to zero was rejected; $F(2,47) = 8.48$, p-value = 0.0007. These results are available upon request.

V. *Lagniappe* – A Counterfactual Exercise

Our results strongly suggest that all three at-will exceptions and two of the social welfare variables have contributed to the steady long run decline in union density. Further, because the scope of just-cause protections differ substantively for each class of at-will exceptions, our analysis also revealed that the negative impact on union density became stronger as states adopted multiple exceptions. Clearly, these results are consistent with both variants of the *government substitution hypothesis*.

Another way to frame the question is to ask what union density would have been during our sample period had state courts not adopted employment-at-will exceptions; had states not adopted their own OSHA plans and finally; had there been no worker's compensation programs. Our simple counterfactual exercise is intended to provide an approximation of the relative contribution to the decline in union density made by the government's provision of these particular union-like services.[49]

To perform this counterfactual, we add the absolute value of the negative coefficient for each of the exceptions (*PP*, *IC*, and *CGF*) reported in Table 1 to union density for states that recognized these exceptions. We also do this with the coefficient on *OSHA* for states with federally approved state plans. Finally, we multiply the absolute value of the coefficient on *WC* by the change in worker's compensation expenditures for each state between each of the decades in question, which is then also added to a state's union density.[50] The counterfactual offers a glimpse at what union density might be had these interventions never occurred, holding all else constant.

[49] The coefficient on state unemployment compensation in the last column in Model 1 is not statistically significant and the magnitude is close to zero, therefore, we did not include it in the counterfactual analysis.
[50] For example, WC in 1970 is subtracted from WC in 1980 for each state and is multiplied by 0.032 to be added to the counterfactual union density for each state in 1980. In this case, we are evaluating the counterfactual example with respect to workers' compensation by the difference in expenditures between each decade. In auxiliary counterfactual exercises, the effect of workers' compensation was evaluated using the difference between each state's WC in 1964 and each decade. There were no noticeable differences in the boxplots. These results are available upon request.

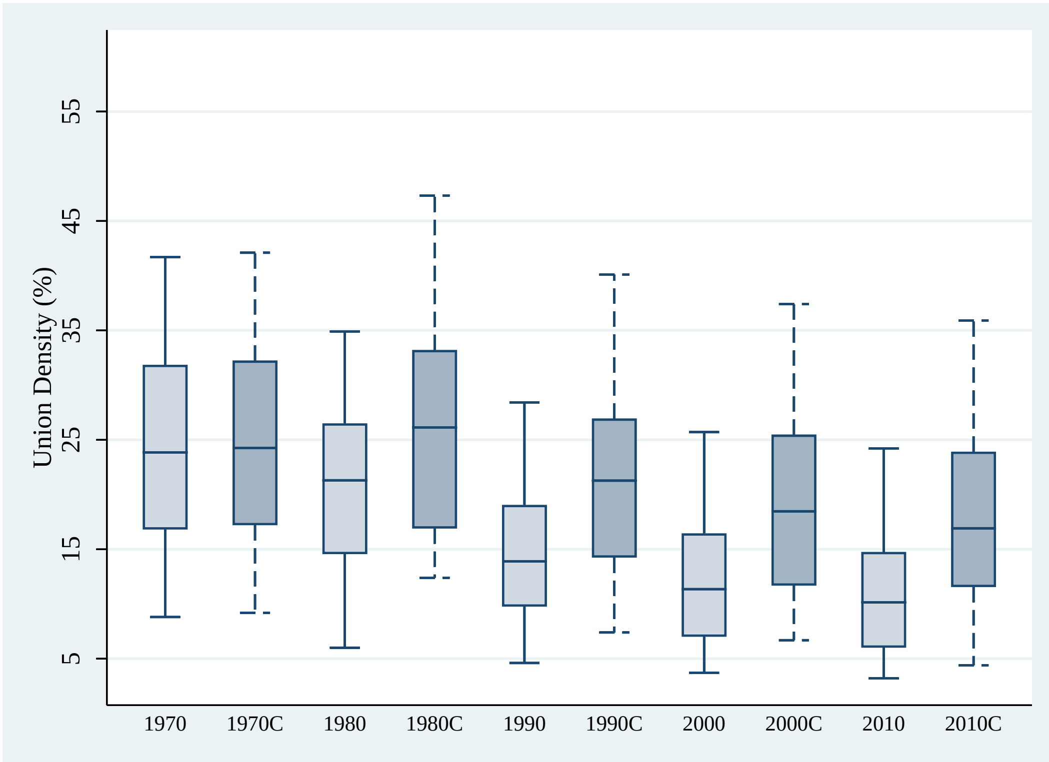
Results for the counterfactual calculations are presented in Table 5. Our starting point for the counterfactuals is 1970, a decade before the surge in the adoption of at-will exceptions. A few interesting patterns emerge. First, the counterfactual results for 1970 suggest that the median level of union density across states would have been about 24.2 percent; only about .3 percentage points higher than it actually was. Second, as expected, the differential between actual union density and the counterfactual increased following the wide-spread adoption of at-will exceptions during the 1980s so by 1990 the differential had grown to 7.6 percentage points (a 54.7 percent differential). In 2010 our counterfactual suggests the median level of union density would have been about 16.6 or 6.4 percentage points higher than it actually was in that year. Finally, over this period, the median level of union density fell by roughly 57 percent. The counterfactuals suggest a far more modest decline of about 31 percent. The results in Table 5 are displayed in Figure 4, which reproduces box plots from Figure 1 and adds the associated box plots for each counterfactual exercise (dark blue boxes).

Table 5. Union Density: Actual and Counterfactual

	Median Union Density			Percent
Year	Actual	Counterfactual	Difference	Difference
1970	23.9	24.2	0.3	1.2%
1980	21.3	25.4	4.1	19.2%
1990	13.9	21.5	7.6	54.7%
2000	11.4	17.8	6.4	56.1%
2010	10.2	16.6	6.4	62.7%
%Δ 1970-2010	-57.3%	-31.4%		

Source: Authors' calculations.

Figure 4. Decline in State-Level Union Density: Actual and Counterfactuals



Source: Authors' calculations.

VI. Discussion

Evidence from a fairly extensive literature in economics concludes that the adoption by state courts of exceptions to the common law employment-at-will doctrine has had far-reaching unintended impacts on firm behavior and labor market outcomes on several behaviorally distinct margins. We have extended this literature by examining their unintended effects on union density. While economists have long recognized the potential for government policies to *directly* alter the costs and benefits of union membership, these changes in employment law, resulting from precedent-setting court decisions, represent *indirect* unintended links between government activity and the demand for union membership. Issues like employment security and just cause protections, commonly found in union contracts as far back as the late 1800s, are thought to be fundamental determinants of workers' demand for union membership. The provision of just cause protections for adverse discharge under these at-will exceptions provide workers with many of the benefits they receive through union contracts thus, providing a close substitute for the benefits derived from union membership. Our evidence supports the hypothesis that government provision of just-cause protections for workers represent relatively strong substitutes for those protections traditionally included within collective bargaining agreements thus, reducing the net benefits to workers of union membership. While the evidence from previous studies was mixed, we find that each of the three classes of court-created at-will exceptions has had a demonstrable negative impact on union density in the states where they have been adopted.

Further, since the three classes of at-will exceptions are disparate in their criteria and the scope of coverage for just-cause protections, our hypothesis is that a state's adoption of more than one exception represents a substantive expansion in the scope of that state's wrongful discharge protections, producing a closer substitute for the just-cause protections provided by unions; that is, the substitution effects become stronger. To test this hypothesis we introduced a collection of indicator variables designed to measure the effects on union density from a step-wise and discrete expansion in the scope of a state's just-cause protections produced by multiple adoptions. We find strong evidence that the negative effect on union membership increases as a state adopts multiple exceptions to the employment at will doctrine.

We also examine a second variant of the substitution hypothesis. Has the adoption of major union-like social welfare programs—employment insurance programs and health and safety laws—negatively impacted union growth? With respect to this variant of the substitution hypothesis, our results suggest that government activities associated with the provision of workers compensation programs and the regulation of health and safety standards in the workplace also substitute, at least partially, for similar services provided by unions. This appears to be particularly the case for the effects on union density of the creation and implementation of independent state OSHA-approved safety and inspection programs. States with their own OHSA-approved plans cause union density to be roughly 9.8 percent lower than states relying on the federal OHSA inspection standards.

One unexpected result was the estimated *positive* and significant coefficient for unemployment insurance, which appears to be a counter-intuitive result; especially in light of previous studies that included spending on unemployment compensation as one component of total social welfare spending under the explicit assumption that the unemployment insurance program clearly competes with traditional union services. One plausible explanation is that the level of spending on unemployment insurance really serves as a proxy for the unemployment rate, which in the time-series union growth models by AP (1969), BE (1976) and NR (1984) was assumed to measure the stock of labor grievances and its positive effect on a worker's propensity to join a union. All three studies found empirical support for this proposition.

This re-interpretation of the role of unemployment insurance may partially account for the mixed results found in the MNS (1989) study, since spending on UI was included in their more restricted social welfare variable. The inclusion of UI spending in both the NR and MNS papers may have diluted the estimated impact of social welfare spending on union density.

Finally, we address a counterfactual question; what would union density have been during our sample period had state courts not adopted employment-at-will exceptions; had states not adopted their own OSHA plans and finally; and had there been no worker's compensation programs? Our simple counterfactual exercise is intended to provide an approximation of the relative contribution to the decline in union density made by the government's provision of these particular union-like services. Between 1970 and 2010, the median level of union density across states fell from 23.9 percent to 10.2 percent, or roughly 57 percent. Absent the adoption of at-will exceptions and the expansion of union-like social welfare programs, our counterfactual exercise suggests a far more modest decline of about 35 percent. Our counterfactual estimate for the median union density in 2010 is 16.6 percent or roughly 63 percent higher than the actual level of union density in that year.

Overall, our results may go a long way toward resolving the debate over the *government substitution hypothesis*; that is, whether at-will exceptions and social welfare programs have, on the margin, contributed to the decline in union density. Our results reveal that these legal and policy interventions, which provide just-cause protections, social insurance and health and safety protections, have had the unintended effects of offering union-like services that are close substitutes for the same protections historically found exclusively within union collective bargaining agreements. Thus, the effect of these interventions has been to transform government into a partial substitute for collective bargaining as a mechanism for providing workers with protections from the undesirable behavior of employers, which in turn, reduced the net benefits to workers of union membership.

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Table 1A. Full 2SLS Results for State Union Density (1964 - 2010)

	Model 1	Model 2	Model 3	Model 4
EAW Exceptions:				
PP	-1.393***			-0.723*
	(0.430)			(0.364)
IC		-1.643**		-1.456**
		(0.686)		(0.697)
CGF			-1.654*	-1.549*
			(0.838)	(0.817)
Social Welfare:				
UI	0.005***	0.004***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
WC	-0.023**	-0.019*	-0.028***	-0.017*
	(0.009)	(0.013)	(0.010)	(0.010)
OSHA	-9.461***	-9.87***	-10.736***	-9.848***
	(1.845)	(2.037)	(1.953)	(1.977)
Controls:				
FLF	-0.155	-0.129	-0.196*	-0.165*
	(0.096)	(0.095)	(0.101)	(0.096)
SLF	0.249***	0.220**	0.268***	0.226**
	(0.085)	(0.097)	(0.082)	(0.095)
URB	-0.004	-0.034	-0.010	-0.023
	(0.073)	(0.075)	(0.069)	(0.069)
MANU	0.517***	0.562***	0.551***	0.542***
	(0.183)	(0.183)	(0.176)	(0.177)
SOUTH	-12.037***	-13.844***	-13.098***	-13.001***
	(2.563)	(2.643)	(2.425)	(2.471)
RTW	0.775	0.258	0.883	0.083
	(0.975)	(0.913)	(0.589)	(1.016)
Constant	41.792***	44.608***	45.156***	44.959***
	(7.141)	(7.067)	(6.829)	(6.725)
Adjusted R-squared	0.93	0.93	0.93	0.93
N	2,256	2,256	2,256	2,256

Notes: Dependent Variable = (% Unionized) x 100. In addition to the control variables, all models include both state and decade fixed effects. Robust standard errors clustered at the state level are reported in parentheses; * *p<0.1; ** p<0.05; ***p<0.01*. Social welfare variables are the predicted values from their respective reduced-form regressions.

Table 2A. 2SLS Results for the Impact on State Union Density from Cumulative Adoptions of At-Will Exceptions (1964 - 2010)

	Model 1	Model 2	Model 3	Model 4
EAW Exceptions:				
NONE		0.471	2.032***	
		(0.485)	(0.592)	
FIRST	-0.471		1.562**	
	(0.485)		(0.676)	
SECOND	-2.032***	-1.562**		
	(0.592)	(0.676)		
THIRD	-3.746***	-3.276***	-1.715**	
	(0.993)	(1.021)	(0.763)	
TOTAL				-1.168***
				(0.299)
Social Welfare:				
UI	0.003**	0.003**	0.003**	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
WC	-0.018*	-0.018*	-0.018*	-0.017*
	(0.009)	(0.009)	(0.009)	(0.009)
OSHA	-9.996***	-9.996***	-9.996***	-9.558***
	(1.870)	(1.870)	(1.870)	(1.990)
Controls:				
FLF	-0.175*	-0.175*	-0.175*	-0.160*
	(0.094)	(0.094)	(0.094)	(0.092)
SLF	0.241***	0.241***	0.241***	0.228**
	(0.088)	(0.088)	(0.088)	(0.088)
URB	-0.011	-0.011	-0.011	-0.017
	(0.065)	(0.065)	(0.065)	(0.068)
MANU	0.534***	0.534***	0.534***	0.529***
	(0.171)	(0.171)	(0.171)	(0.175)
SOUTH	-12.079***	-12.079***	-12.079***	-12.546***
	(2.401)	(2.401)	(2.401)	(2.383)
RTW	0.365	0.365	0.365	0.171
	(1.028)	(1.028)	(1.028)	(1.069)
Constant	43.756***	43.756***	43.756***	43.851***
	(6.448)	(6.448)	(6.448)	(6.833)
Adjusted R-squared	0.93	0.93	0.93	0.93
N	2,256	2,256	2,256	2,256

Note: In Model 1, the *F*-statistic and *p*-value are 6.04 and 0.0178, respectively, for the test that $\beta_{Two} - \beta_{One} = 0$. In the same model, the *F*-statistic and *p*-value are 7.32 and 0.0095, respectively, for the test that $\beta_{Three} - \beta_{One} = 0$. All models include state- and decade-fixed effects; Robust (clustered at the state-level) standard errors in parentheses; * p<0.1; ** p<0.05; *** p<0.01; Social welfare variables are the predicted values from their respective reduced-form regressions.

Table 3A. Variable Means and Dispersions

		Standard Deviations								
Variable	Mean	Overall	Min	Max	Between	Min	Max	Within	Min	Max
Dependent										
Union Density	17.08	8.51	2.30	44.80	6.47	5.52	30.06	5.62	3.28	33.58
EAW Exceptions										
PP	0.52	0.50	0	1	0.24	0	0.87	0.44	-0.36	1.11
IC	0.50	0.50	0	1	0.24	0	0.83	0.44	-0.33	1.09
CGF	0.13	0.33	0	1	0.24	0	0.79	0.23	-0.66	0.96
Social Welfare										
UI	131.71	98.66	13.01	760.43	54.58	39.64	261.53	82.55	-31.85	630.61
WC	46.65	71.48	1.06	874.75	59.04	3.50	246.35	41.18	-184.74	675.05
OSHA	0.34	0.47	0	1	0.38	0	0.79	0.29	-0.45	1.30
Controls										
FLF	43.59	4.08	4.60	49.90	1.32	39.27	45.79	3.87	7.51	52.12
SLF	11.78	2.35	0.80	18.70	0.82	9.87	13.23	2.20	1.20	18.27
URB	68.41	14.70	32.20	94.90	14.45	34.12	92.49	3.38	58.64	80.26
MANU	1.87	2.42	0.05	15.47	0.91	0.14	4.06	2.25	-1.66	13.64
SOUTH	0.27	0.44	0	1	0.45	0	1	0	0.27	0.27
RTW	0.42	0.49	0	1	0.49	0	1	0.09	-0.33	1.20

Table 4A. Full OLS Results for State Union Density (1964 - 2010)

	Model 4
EAW Exceptions:	
PP	-1.066**
	(0.415)
IC	-1.397**
	(0.641)
CGF	-1.335*
	(0.801)
Social Welfare:	
UI	0.002*
	(0.001)
WC	-0.006**
	(0.003)
OSHA	-0.668
	(0.747)
Controls:	
FLF	-0.149*
	(0.081)
SLF	0.231**
	(0.096)
URB	0.068
	(0.073)
MANU	0.538***
	(0.164)
SOUTH	4.689***
	(1.288)
RTW	-0.773
	(1.123)
Constant	15.401***
	(3.934)
Adjusted R-squared	0.93
N	2,256

Notes: Dependent Variable = (% Unionized) x 100. In addition to the control variables, all models include both state and decade fixed effects. Robust standard errors clustered at the state level are reported in parentheses; * $p<0.1$; ** $p<0.05$; *** $p<0.01$.