Title

The Costs of Wrongful Discharge Laws

Authors
Autor, David H.
Donohue, John J., III
Schwab, Stewart

Publication Date
2001-10-02

The Costs of Wrongful Discharge Laws

David H. Autor
MIT Department of Economics and NBER

John J. Donohue III
Stanford Law School and NBER

Stewart J. Schwab*
Cornell Law School

October 2, 2001

Abstract

We estimate the effect on state-level employment, unemployment, and wages of common law wrongful-discharge protections. Over the last three decades, most state courts have adopted one or more tort, contract, or good-faith-covenant exceptions to the employment-at-will doctrine. The variation in adoptions provides dozens of "experiments," thus allowing an unusually good test of the costs of employment protection. We use a difference-in-difference econometric design and data from the CPS monthly files 1978 – 1999 to examine the effects by gender, race, age, and education levels. We find a small but robustly negative impact of the implied-contract exception on the employment to population rate in state labor markets, particularly for less educated males and younger workers. Young, less educated males face a 1.9 percentage point drop in employment rates; young, less educated females face a 1.1 percentage point drop; while older workers face no drop, particularly if better educated. We find corresponding short-term increases in unemployment rates and longer-term increases in persons not in the labor force. The adverse employment impacts for males are augmented in states with lower union presence. By contrast to the implied-contract exceptions, we find no negative effects for the tort and good-faith exceptions to employment at will. Findings in the previous literature range from no effect to huge negative effects. We reanalyze the prior studies and find the discrepancies can be readily explained by methodological choices.

* We are indebted to Lawrence Katz, Alan Krueger, Thomas Miles, and Andrew Morriss for assistance and insightful suggestions. We gratefully acknowledge the excellent research assistance of Rashida Adams, Simone Berkowitz, Douglas Bosley, Craig Estes, Rose Francis, Carolyn Heyman, Joshua Linn, Joshua Mayes, and Marci Reichbach.

Introduction

Protection has its price. This paper estimates the social costs, in terms of lesser employment, higher unemployment, and lower wages, of common law efforts to protect American workers from wrongful discharge. The cost of employment protection has been a controversial issue for some time. Many scholars have argued that stagnant employment and unemployment in many European countries – or 'Eurosclerosis,' as it is known – can be attributed to the great protection given workers (cf., Blanchard and Wolfers, 1999). While the protection may help workers retain jobs, the argument goes, it makes employers hesitant to hire new workers. Among the many problems in testing this controversial thesis is the difficulty in getting high quality data for cross-country comparisons.

Other scholars have examined the effects of American federal employment laws on employment and unemployment. Acemoglu and Angrist (forthcoming) present evidence that the Americans with Disabilities Act has decreased employment of disabled persons. Oyer and Schaefer (2000) have found that the federal Civil Rights Act of 1991 increased the frequency of mass layoffs. Hahn, Todd, and van der Klaauw (2001) have studied the costs of anti-discrimination laws. A major hurdle for these studies is that the federal statutes at issue apply all at once to the entire country. It is difficult to separate the effects of the statute from all other changes occurring at that time. See generally Donohue (1998); Donohue and Heckman (1991).

We overcome some of these methodological challenges in the present paper by looking at employment protections that arise in various states at various times, thus providing greater variation in which to attribute cause. The United States, uniquely in the industrialized world, has long had a legal presumption that workers can be fired "at will" – that is, for any time and for any reason, good or bad. In the last two or three decades, however, most state courts have adopted one or more tort, contract, or good-faith exceptions to the at-will doctrine. The states vary greatly in the timing and extent of adopting these exceptions. Three states (Florida, Georgia, and Rhode Island) have never adopted an exception. Ten states now use all three exceptions, and a few states have rejected prior adoptions. See

Appendix Table 1.

This variability in state legal doctrines allows for empirical testing of their effects on employment, unemployment, and wages by state. These effects are likely to vary for different segments of the population. Indeed, Schwab (1993) has argued that the contract erosions have an implicit life-cycle logic, designed to protect long-tenured workers while retaining the at-will rule for mid-career workers. More generally, if a wrongful-discharge protection makes employers hesitate to hire new workers, the employment of younger workers should fall (and perhaps their unemployment would rise) relative to older, long-tenured workers, who cannot be terminated as easily as before (cf. OECD, 1999, Chapter 2). For this reason, and for the first time in the U.S. literature, we break down the employment, unemployment, and wage effects by age, gender, race, and education.

Krueger (1991) was the first to use the state adoption of at-will exceptions as independent variables. His task was to predict whether a state legislature would consider statutory rejection of employment at will.[1] Dertouzos and Karoly (1992 and 1993), in a widely cited paper, tested whether adoption of these exceptions affected state-level employment. Using an instrumental-variable approach, they found surprisingly large effects. Dertouzos and Karoly calculated that states adopting a tort-based cause of action suffered a 3 percent reduction in employment, with an additional 1 or 2 percent employment decline for states also adopting contract-based protections.[2] Morriss (1995) criticized their legal variables in the course of cataloguing the challenges of using variables of this type. Autor (forthcoming) and Miles (2000) used the adoption of unjust dismissal doctrines to explain the growth in employment in the Temporary Help Services industry (THS). Their theory, largely confirmed by the data, was that employers turned to THS employment when states adopted common

[1] To date, only Montana (in 1987) has actually passed a statute establishing a good-cause standard for all employment terminations. In 1991, the Uniform Law Commissioners passed a Model Employment Termination Act similar to the Montana statute, but no state has yet adopted it. In 1996, the Arizona legislature passed a statute affirming employment at will.

law exceptions to employment at will. In addition, Miles (2000) reexamined the Dertouzos and Karoly findings using a differences-in-differences technique, but could find "no statistically significant effects on either employment or unemployment." In related work, Kugler and Saint-Paul (2000) find that a state's adoption of wrongful discharge doctrines significantly slows the job-to-job flows of unemployed relative to employed workers, suggesting that elevated firing costs raise employers' incentive to statistically discriminate against the unemployed.

Like previous studies, our major independent variables are the dates states adopted various exceptions to at-will employment. We differ from these studies, however, by analyzing the impacts of the exceptions on employment of multiple demographic subgroups, on wages as well as employment, and on employment in states with differing degrees of unionization. Our primary goal in this study, however, is not to extend the literature but to reconcile it. In particular, we wish to assess what can be reliably inferred from the data and to understand why the leading papers on this topic have reached opposite conclusions, ranging from no effect to huge negative effects.

To preview our findings, we find a small but robustly negative impact of the implied-contract exception on the employment to population rate in state labor markets, particularly for less educated males and younger workers. We find corresponding short-term increases in unemployment rates and longer-term increases in persons not in the labor force. The adverse employment impacts for males are augmented in states with lower union presence. By contrast to the implied-contract exceptions, we find no negative effects for the tort and good-faith exceptions to employment at will. We reanalyze the prior studies and find the discrepancies can be readily explained by methodological choices.

1. Wrongful discharge law

In the United States, the baseline employment contract is that most private-sector workers are

---

[2] Dertouzos and Karoly (1988) earlier examined the direct costs of wrongful-discharge litigation in California. They found these direct costs to be modest, amounting to some $100 per termination. See also Dertouzos and Karoly (1992; p. xi) (presenting findings of 1988 study).

employed at will, meaning the employer has the legal right to fire them at any time for a good reason, bad reason, or no reason at all. From the heyday of employment at will in the early part of the 20th century, a number of important restrictions on the at-will doctrine have arisen. General federal statutes prohibit employers from firing workers on the basis of union activity, race, color, religion, sex, national origin, age, or disability.[3] More particular federal or state statutes prevent employers from terminating workers for a variety of other reasons, ranging from retaliation for reporting occupational safety and health violations to preventing pension benefits from accruing to reaction to serving on a jury.[4]

a. Common law exceptions to employment at will

In addition to the federal and state statutory restrictions on employment at will, in the last 30 years state courts have created numerous common law restrictions on the at-will rule. These common law restrictions are commonly classified in three categories: (1) the tort of wrongful discharge in violation of public policy; (2) implied-in-fact contracts not to terminate without good cause; and (3) implied covenants to terminate only in good faith and fair dealing.

As the Appendix Table 1 shows, all but seven states now recognize the tort of wrongful discharge in violation of public policy. Classic examples of this tort include firing a worker for refusing to do an illegal act (such as perjuring himself when a government agency is investigating the company), for exercising a statutory right (such a filing a workers' compensation claim), or for performing a public obligation (such as serving on a jury). As Schwab (1996) has explained, courts tend to apply this tort when the termination clearly affects third parties, making the judicial interference with the at-will employment contract easy to justify. Most courts limit this tort to clear violations of express legislative commands, rather than violations of a vaguer sense of public policy articulated by the judiciary.

---

[3] National Labor Relations Act § 8(a)(3), 29 U.S.C. §158(a)(3) (enacted 1935) (prohibiting discrimination on the basis of union status); title VII of the Civil Rights Act of 1964, 42 U.S.C. §§ 2000e to 2000e-17 (prohibiting discrimination on the basis of race, color, sex, religion, or national origin); Age Discrimination in Employment Act of 1967, 29 U.S.C. §§ 621-634; Americans with Disabilities Act of 1990, 42 U.S.C. §§ 12101 – 12213.

Successful plaintiffs can recover full tort damages, including compensatory damages for lost earnings and pain and suffering and punitive damages. Multi-million-dollar judgments are often highly publicized.

A different set of 43 states recognize the implied-in-fact contract exception to at-will employment. This exception arises when the employer through words or actions implicitly promises not to terminate a worker without good cause. A well-known example comes from *Pugh v. See's Candies, Inc.*,[5] in which a worker who had risen over 32 years from dishwasher to corporate vice-president was suddenly fired. When the worker asked for an explanation, he was told "look deep within." The court recognized a cause of action for breach of an implied-in-fact contract, emphasizing the worker's longevity, the regular promotions and oral assurances of security given him over the years, and the employer's practice of not firing without good cause. Perhaps of greater practical importance in this category are the "handbook" cases, in which a court makes legally binding an employer's policy of terminating only with good cause as articulated more or less explicitly in a handbook given supervisors or all workers. Successful plaintiffs are generally restricted to contract damages, and are not entitled to punitive damages, unforeseeable damages, or compensation for pain and suffering.

Only 11 states now recognize the covenant of good faith and fair dealing as an exception to employment at will (although Oklahoma and New Hampshire recognized good faith as a distinct action in the past). A classic illustration of this category is *Fortune v. National Cash Register Co.*,[6] where the employer fired a salesperson who had done all the work for a sale just before the commission was due, thereby depriving him of the "benefit of his bargain." Most courts limit the good-faith covenant to timing cases such as *Fortune*, although California in particular recognizes a broader set of good faith

---

[4] Occupational Safety and Health Act of 1970 § 11, 29 U.S.C. 660(c) (prohibiting discrimination against employees exercising rights under OSHA); Employee Retirement Income Security Act of 1974 § 510, 29 U.S.C. § 1140; New York Judiciary Law § 519 (prohibiting discharge of employee due to absence from employment for jury service).

[5] 171 Cal. Rptr. 917 (Cal. Ct. App. 1981).

[6] 364 N.E.2d 1251 (Mass. 1977).

obligations that is sometimes hard to distinguish from a general obligation not to fire without good cause.[7] Some courts have awarded tort damages (including punitive damages) for breach of the good-faith covenant, although the trend today is to award only contract damages.

As Morriss (1995) discusses, it is not always easy to date when a state has adopted a particular at-will exception. Our concept is easy enough to state, however. We envision management-side employment lawyers reading the advance sheets and writing awareness letters to their clients when major changes occur in the law. Thus, we are interested in the first court decision in a state that would trigger a client letter warning about a change in the law. We largely agree with Morriss's criteria for identifying such decisions. Among other things, Morriss criticizes Dertouzos and Karoly for ignoring federal cases that apply state wrongful discharge doctrine.[8] In practice, while we have independently assessed the legal doctrine in this area for all 50 states, we largely agree with Morriss's list of relevant cases (although we had to update the list through the 1990s). We test the sensitivity of our findings to alternative classifications below.

Morriss also argues that the basic Dertouzos-Karoly division of wrongful discharge law into tort and contract remedies should rather be done in a three-part criterion based on legal doctrine: public policy, implied contract, or good faith covenant. This comports with our concept of the client letter as the triggering point, for these client letters from lawyers will be filtered through the lawyer's frame of reference, which is legal categories.[9]

[7] The leading California case is *Foley v. Interactive Data Corp., 765 P.2d 373 (Cal. 1988)*, in which a bank employee was fired for reporting to upper management that the FBI was investigating his immediate supervisor for embezzlement from a prior employer. The court declared these facts could amount to a breach of the covenant of good faith and fair dealing, but held that only contract damages, not full tort damages, were recoverable.

[8] The hiring and firing practices of employers in these states, argues Morriss, would be affected by federal diversity cases as well as state-court cases; thus, federal cases should be included in the analysis. Whether appellate court or supreme court cases should be used is another matter of judgment. While supreme court cases are always more authoritative and usually more visible, many appellate cases are very prominent (such as the *Pugh* case already discussed).

[9] Although we use the three-part division in the body of our analysis, we explored the relevance of the tort/contract distinction on which D&K focus. We did not find this distinction to be relevant or empirically robust. Supplemental tables are available from the authors on request.

b. Hypothesized effects of wrongful discharge law on the labor market

As is discussed by Lazaer (1990), Katz and Blanchard (1997), and Blanchard and Portugal (1998), the theoretical impact of firing restrictions on employment is ambiguous. In a frictionless labor market, the Coase theorem applies; imposition of firing costs upon employers will be fully undone by efficient worker-firm bargains in which workers post a bond equal to the firing cost and subsequently receive their payment back in wages or dismissal costs. In a situation where the Coasean invariance result does not hold, however, firing restrictions reduce employers' incentives to both hire new workers and fire incumbent workers. Accordingly, firing restrictions unambiguously dampen employment fluctuations but may either raise or lower equilibrium employment.

Not all (non-Coasean) employment protection adversely impacts labor market efficiency, however. Employment protection can be viewed as a mandated employment benefit that, while costly for employers to provide, is also valued by employees. By raising the costs of employing workers, the imposition of employment protection shifts labor demand inward. To the degree that workers value the mandated benefit, however, labor supply simultaneously shifts outward, again leaving the aggregate impact on employment ambiguous (Summers, 1989). Furthermore, as several authors have argued, the presence of adverse selection in the labor market may cause employers to provide inefficiently low levels of job security (Aghion and Hermalin, 1990; Levine, 1991). If this argument is correct, imposition of firing restrictions might actually increase employment while lowering wages.[10]

While is not clear a priori what the overall impact these erosions of the at-will doctrine should have on employment or unemployment, existing evidence suggests that the impact may differ for different groups of workers (OECD, 1999, Chapter 2; Jolls, 2000). For example, OECD (1999) provides evidence that the employment of younger, less educated workers appears most likely to be harmed by wrongful-discharge protections, while more educated, older workers appear to benefit.

[10] This would correspond to a case where, because workers value job security more at the margin than it costs employers to provide, the labor supply curve shifts outward by more than the labor demand curve shifts inward.

Turning to specific legal theories, it is not clear what the relative impact should be of the implied contract, tort of wrongful discharge, or good-faith covenant. No data exist that show the relative number or outcome of these cases, nor would this caseload data would provide a complete measure of the economic costs of wrongful discharge laws.

It is sometimes thought that the wrongful discharge in violation of public policy action is the most important exception to at-will employment, because it allows for tort damages. However, it seems likely that violations of public policy are fairly rare, particularly as limited by many courts to clear violations of express legislative commands, rather than violations of a vaguer sense of public policy by the judiciary. These cases attract attention and are accompanied by punitive damages, which increases their visibility. But scholars have suggested that their importance is exaggerated (see Edelman et al 1992), and profit-maximizing employers should discount the danger.

Good-faith cases are the least prevalent of the three exceptions, being currently recognized in only 11 states (importantly, though, including California). Some of these states allow tort damages as well (although California stopped doing so in 1988). But most states have limited the action to grossly unfair timing issues, such as firing a salesperson just before the commission is due. These cases may be more interesting in legal theory than in operation.

This leaves the implied-contract cases (which, as noted above, is the category of cases that clearly reduces employment and increases unemployment for some types of workers). On the one hand, the implied-contract cases might be *unimportant* for two reasons. First, they only lead to contract damages, so the threat of the truly spectacular jury award, complete with punitive damages, is missing. Second, an employer can usually insulate itself from an implied-contract claim by prominently disclaiming any intention that the contract be other than at will.[11] On the other hand, implied-contract cases can affect

[11] It remains a complex legal question, however, whether an employer that once issued a handbook or other promise of job security can modify it to create at-will employment. Several courts have held that such unilateral changes by the employer are not binding on incumbent employees that have previously received promises of job security.

how a firm treats its entire workforce. Consider the implied contracts based on oral reassurances, longevity, and a practice of treating employees well (such as the California *Pugh* case). One corporate response to these cases is to make hiring, evaluation, and promotion decisions more bureaucratic. Contracts will include no-modification clauses like "immediate supervisors cannot change the terms of employment; only statements in writing by the vice president for human relations can do so." Flexibility and discretion by supervisors is lost through this 'bureaucraticization.'

Consider also the other major type of implied contract cases, the handbook cases. These cases turn handbooks into legal documents affecting all workers, and may affect the firm far more pervasively than the occasional wrongful discharge in violation of public policy of a whistleblower. Top-level employees often individually negotiate their employment contracts. Firms tend to deal with rank-and-file workers with standard terms, often articulated in a handbook. If the handbook becomes legally enforceable, it becomes more costly to employ the rank and file. On the other side, if the corporate response is to abandon handbooks or include huge disclaimers, the benefits of standardization are lost and it again becomes more costly to employ the rank and file.

2. Econometric issues

Because the common law exceptions to the doctrine of employment at will were adopted (and, in some cases later repealed) in most U.S. states during the 1980s and 1990s, we effectively have dozens of 'experiments' available. This multiplicity of treatment and control groups allows us to test the validity and robustness of our statistical design. If the labor market impacts of wrongful discharge laws are in fact sizable and reliable, they should be detectable across many subsets of our 'experiments:' regions and time periods, adoptions and reversals. Our basic approach is to analyze panel data for all fifty states using state and year indicator variables, which we refer to as a difference-in-differences (or more generally, fixed effects) model.

a. Model specification

Define Y_{0it} as the natural logarithm of the *potential* employment level in U.S. state (i) in year (t) in the absence of a wrongful discharge law and Y_{1it} as the potential employment level in state (i) and year (t) with the wrongful discharge law enacted (hence, these two outcomes are counterfactuals of one another).[12] It is of course a fundamental problem of statistical inference that we can never observe both Y_{0it} and Y_{1it} for the same state and time – if we observe what happens when a state adopts a wrongful discharge law then we cannot observe would have happened if had not done so. Hence, we use comparison U.S. states to develop a plausible counterfactual scenario. Assume that:

(1) $E[Y_{0i} \mid s,t] = q_t + g_s$.

That is, in the absence of a wrongful discharge law, a state's employment level can be written as the sum of a year effect that is common across states and a state effect that is fixed over time. Suppose also that the effect of a wrongful discharge law on state (log) employment is to add a constant (which may be negative) to $E[Y_{oi} \mid s,t]$ so that:

(2) $E[Y_{1i} \mid s,t] = E[Y_{0i} \mid s,t] + d$.

Hence, (log) employment in states adopting wrongful discharge laws and comparison states not doing so in years (t-1) and (t) can be written as:

(3) $Y_s = q_t + g_s + dL_s + e_i$,

where $E[e_s \mid s,t] = 0$ and L_s is an indicator variable that is equal to 1 if state (s) had in place a wrongful discharge law in year (t) and 0 otherwise. Differencing (log) employment levels across states and years in the period before and after the 'treatment' (i.e., the law change) gives:

(4)
$$\{E[Y_s \mid s = \text{state adopting law}, t = \text{t}] - E[Y_s \mid s = \text{comparison states}, t = \text{t}] -$$
$$E[Y_s \mid s = \text{state adopting law}, t = \text{t -1}] - E[Y_s \mid s = \text{comparison states}, t = \text{t -1}]\} = d.$$

By contrasting the change in the outcome variable (log employment) between treatment and control states in the years before and after adoption, we obtain a consistent estimate of the impact of the

[12] We use natural logarithms rather than levels so that all changes in employment can be read in percentage terms rather than absolute numbers. This is a logical approach to modeling labor market phenomena since wrongful

wrongful discharge law on state employment. Or, in a regression framework, we can estimate:

(5) $$Y_{st} = a + d\,(\text{Wrongful discharge laws})_{st} + b_1(\text{Labor force demographics})_{st} + g_s + q_t + l_s t + e_{jt},$$

where the coefficient vector l_s controls for smooth trends in employment in each state. In this equation, d provides an estimate of the employment effect of the wrongful discharge law.

The difference-in-differences model requires two primary conditions for valid inference: 1) control states provide a valid 'what if' comparison for determining what would have happened to employment in treated states 'but for' adoption of the wrongful discharge law; and 2) the adoption of wrongful discharge laws are not so wholly anticipated by employers that employment in treated states has already adjusted to the new laws prior to their adoption.[13] We test both of these assumptions explicitly by exploiting multiple experimental and control states to provide a robust impact estimate, and by including 'leads' and 'lags' of our law change variables to ensure that it is law changes that lead to employment changes and not vice versa.

b. Data sources

Our employment, wage, and labor force demographic data are drawn from the combined monthly files of the Current Population Survey for the years 1978 – 1999. Unionization data are from Hirsch, McPherson and Vroman (forthcoming, 2001) and political affiliation measures are available from the Statistical Abstract of the United States. We have used these sources to develop a comprehensive database of state employment and demographic variable paired to a taxonomy of wrongful discharge law prevailing in each state and year for the two-decade period from 1978 to 1999, which we developed for this research.[14] In addition to our own taxonomy, we use classifications employed by

---

discharge laws are likely to increase or decrease employment by percentage amounts rather than by an absolute numbers of workers (particularly because U.S. state labor markets vary dramatically in size).

[13] To the degree that the wrongful discharge law is *partially* anticipated by employers and employees, the difference-in-differences estimator will be likely to underestimate the total effect of the law change on equilibrium employment. This suggests that our estimates will provide a conservative lower bound on any employment impact.

[14] Many of the demographic variables of interest are not available prior to 1978 from the Current Population Survey. Wage data is available from 1979 forward for one-quarter of the employed sample in each month.

Dertouzos and Karoly (1992), Morriss (1995), and Miles (2000).[15]

3. The impact of exceptions to employment at will on employment (and unemployment)

a. Demographic trends in employment rates

While we ultimately want to discern the impact of state law changes on the employment prospects of workers within that state, it is important to recognize that over the last two decades there have been very different trends in employment rates for males and females, as well as for more and less highly educated workers. For the period from 1978 through 1999, Figure 1 shows the employment rates for four demographic groups – men and women with more than a high school education or those with no more than a high school education. A number of patterns are revealed in this figure. First, female employment rates have risen fairly steadily over this period for both educational groups – albeit more for more educated women. In contrast, there has been no growth in the employment rate of more educated men, and a roughly 5 percent decline in the employment rate of less educated men.

Second, Figure 1 also reveals some cyclical patterns: when the economy stagnates or lapses into recession (as it did in the early 1980s and in the early 1990s), employment rates tend to stagnate or decline. For example, when the economy turned down during the 1980 recession, we see the employment rate drop most sharply for men and less educated workers. Indeed, only more educated women overcame the effect of this recession and experienced a growing employment rate. Meanwhile, less educated males experienced a roughly 8 percent drop in the employment rate over the period from 1979 through 1983. From 1983 through 1989, all four demographic groups experienced significant gains in employment rates, although the gains for women were substantially greater. With the return of a recessionary economy around 1990, the employment rates of all four demographic groups fell for the first time over our 22 year period, with the drop being most pronounced for the less educated (particularly for less educated males). Starting around 1992, however, the employment rates began

[15] Miles in turn relied on Walsh and Schwarz (1996).

rising again for all groups, with a slight tailing off in 1999.

b. Regional trends in adopting exceptions and in employment growth

Figure 2 illustrates that prior to 1973 only one state had adopted an exception to the employment at will doctrine, but that in the next quarter century virtually all states had come to recognize at least one such exception. This dramatic shift in law has proceeded somewhat more slowly in southern states, which in general have lagged the rest of the country by roughly five years in starting on the path to recognizing the legal change. But once the trend toward recognition of a legal exception begins, the speed at which judges in other states follow suit suggests a remarkable sensitivity to the decisions of other state courts.[16]

The relatively slower rate of adoption in southern states is important because it suggests that attempts to estimate the impact of these legal changes must be careful not to confuse regional trends with real effects induced by these judicial adoptions. Figure 3 illustrates that this concern is particularly important because of the substantially greater growth in employment that has occurred in the South for the last sixty years. Lest we risk harm to our libertarian readers who might strain themselves leaping back to Figure 2 to bolster the argument that the faster growth in employment was caused by the refusal to adopt the legal exceptions, we hasten to note that faster southern employment growth had proceeded for at least 35 years before as many as two non-Southern states had adopted any legal exceptions. Nonetheless, Figure 3 underscores the likely importance of varying state trends in employment as we proceed to estimate the effect of exceptions to employment at will.[17]

[16] Note that with the single exception of Montana these adoptions are the result of judicial, not legislative action. Moreover, Miles (2000: 84) finds that the speed with a state adopts one of these exceptions is not influenced by the nature of the process of judicial selection (election, appointment, etc.). Moreover, there is no correlation between speed of adoption of an at-will exception and the speed of adopting no-fault divorce, which represents a similarly abrupt state-by-state legal shift, albeit a legislatively implemented change.

[17] There is no formal definition of what constitutes a Southern state. For Figures 2 and 3, we use Alabama, Arizona, Florida, Georgia, Kentucky, Louisiana, Mississippi, New Mexico, North and South Carolina, Tennessee, Texas, and Virginia. Non-Southern states include all other U.S. states except for DC and, due to lack of employment data extending to 1939, Alaska, Hawaii, Illinois, Michigan, and Minnesota. The growth rates calculated in Figure 3 use each state's share of South or Non-South employment in 1939 as base weights.

c. Estimating the effect of the exceptions to employment at will on overall employment and unemployment

Table 1 provides the first evidence of the impact on the overall labor market of state adoption of exceptions to the employment at will doctrine. The table examines the impact of three different legal exceptions to the employment at will doctrine – the implied contract, public policy, and good faith exceptions – on three different measures of labor market performance: the employment/population ratio ("the employment rate"), the unemployment rate, and the rate at which individuals are found not in the labor market.[18]

The story that emerges most clearly from the regression results in Table 1 is that the adoption of the implied contract exception is associated with a small but meaningful reduction in employment. The first two columns represent two different regressions designed to estimate the impact of the various exceptions on the ratio of employment to population. Both regressions (indeed, all the regressions in Table 1) include dummy variables for each state, year, and each of sixteen demographic groups (male/female by high-/low-educated by ages 16 – 24, 35 – 49, 40 – 54 and 55 – 64). In addition to these three sets of fixed effects, the even columns of Table 1 also include time trends for each state and each demographic group. In both regressions 1 and 2, the implied contract exception is associated with a statistically significant decrease in the ratio of employment to population of from roughly .4 – .6 percentage points. At the same time, columns 3 and 4 reveal a corresponding *increase* in the unemployment rate of roughly the same magnitude associated with the adoption of the implied contract exception.

The Table 1 findings for the adoption of the public policy exception contrast with those for the implied contract exception. For example, the public policy exception seems to increase employment. However, this finding proves non-robust to inclusion of state and group time trends in the model. Interestingly, while the implied contract exception seems to push workers from employment to

[18] Note that the unemployment rate is the ratio of unemployed to the labor force (employed plus unemployed). The other two measures are denominated by population.

unemployment, the public policy exception seems to pull workers from being outside the labor force (note the statistically significant negative coefficients in columns 5 and 6) into employment, as the declines in the former match the estimated increase in the latter. This suggests that adopting the public policy exception leads to an outward shift in the demand curve and/or the supply curve. Since evidence we present below in section 4 suggests that the wage effects of the public policy exception are negative, this would suggest that the supply curve shift is quantitatively more important.

Finally, the estimated impact of the good faith exception is even more sensitive than that for the public policy exception to our regression specification. In all three pairs of regressions in row three, different signs are generated by the inclusion or exclusion of the state and group time trends. If one believed the regressions without these added time trends (the odd columns), one would conclude that the good faith exception drove down employment by about twice the amount of the implied contract exception, while driving up the unemployment rate and the proportion of workers outside the labor force. Given the sensitivity to specification, however, we are inclined to believe that there is no reliable inference here.[19] This may be because, despite its potential import, the courts generally construe the good faith exception narrowly. Alternatively, because there are comparatively few good faith exceptions adopted, we may simply lack the statistical power to detect their impacts.

While all of these effects are rather small, the findings from Table 1 are nonetheless somewhat surprising. The implied contract exception allows workers to overcome the normal presumption of employment at will by showing that the employer had provided them with legally sufficient reasons to think they would only be terminated for good cause or pursuant to specified procedures. In states adopting this exception, management employment lawyers often advise their clients to require new (and sometimes existing) workers to sign forms affirming that their employment was “at will.”

[19] Since the good faith exception has at least the potential to turn any discharge into a legal dispute, it would seem to be the more onerous or risky for businesses than the implied contract exception. If the good faith exception does have a less substantial quantitative impact on employment, then it is likely because courts have only applied this doctrine to a narrow range of rather exceptional circumstances.

Consequently, at rather low cost, this exception could in theory be averted by employer action, which might suggest that one would not expect any negative impact on employment.

As noted in the introduction, however, several factors mitigate against full employer avoidance. First, it remains a complex legal question whether an employer that once issued a handbook or other promise of job security can modify it to create at-will employment. Several courts have held that such unilateral changes by the employer are not binding on incumbent employees that have previously received promises of job security. A second consideration is that because the implied-contract exception may provide all employees of a company with protection from discharge other than 'for cause,' its impacts are potentially sweeping.[20] Finally, interviews with managers in Silicon Valley conducted by Donohue and Siegelman (2001) indicate that many small companies often do not seek advice of employment lawyers when establishing personnel policies and hence do not require their employees to sign 'at will' statements. During the present economic downturn and accompanying layoffs, these firms find themselves to be at greater risk of litigation concerning the nature of their previous employment contracts with the newly laid off workers.[21]

It is possible, however, that the estimated impact of the implied contract exception is spurious or exaggerated because the dummy for this exception is merely serving as a proxy for an array of factors that would be deemed to reflect greater risk or greater costs on business. Of course, if this is the case, it

[20] This is also true of the broadly construed good faith doctrine, however. It is not true for the public policy exception.

[21] The precise mechanism by which the implied contract exception could dampen employment is uncertain. Perhaps the adoption of this exception is deemed to be one more factor that warrants the hiring of legal counsel before choosing to expand employment (or locate a plant in a state with such an exception). In this event, it is the need for paying attorneys to help in circumventing the implied contract exception that constitutes a "tax" on added employment. For those firms that fail to clearly specify that that they have not departed from the presumptive rule of employment at will, the burden will come in the form of higher costs when they go to discharge or layoff employees. Contrast the case of two otherwise identical firms, one in which all the employees have signed forms indicating they are employees at will and the other in which no employees have been asked to sign such firms. The impact on employment for these two firms when the need comes to lay off a substantial part of the workforce is uncertain. If the both firms can weather the economic downturn, it is possible that the firm that has failed to protect itself will feel less inclined to lay off workers, since it will face more litigation than its companion firm that has been insulated from this expense. Conversely, the burdens of the litigation might actually push the unprotected firm into closing or relocating, thereby depressing employment. Bankruptcy might provide an option to circumvent legal liability in this context.

must be that the anti-business environment must be perceived roughly contemporaneously with the adoption of the implicit contract exception because any unchanging pro- or anti-business environment in a state would be captured in the fixed state effect. The proxy effect could operate, though, if a more liberal and hence pro-worker administration came to power and appointed judges who adopted such an exception contemporaneously with the adoption or passage of rules imposing costs on business more generally. These costs need not be limited to labor expenses but could involve anything that imposes costs on firms, such as business taxes or environmental regulations. Finally, if judges were more apt to adopt the implied contract exception after a period of prosperity in a state, the finding of a negative impact on employment could simply be reflecting a regression to the mean phenomenon as the economy subsequently slowed. We explore some of these possibilities below.

d. Does the specific doctrine matter?

Taken at face value, the results in Table 1 suggest that the implied contract doctrine has the greatest negative impact on employment of the three exceptions to employment at will. It is possible, though, that it is not the implied contract doctrine in itself that matters, but simply the fact that any exception to employment at will has been adopted (or the accumulation of multiple exceptions) that leads to the negative impact on employment. To examine this issue, we begin with the specification from column 2 of Table 1 (containing state and demographic group time trends) and introduce in Table 2 a variety of explanatory variables designed to control for the number or existence of legal exceptions in a state. For example, the second column of Table 2 shows that merely having any of the three legal exceptions to employment at will does not have an impact on the ratio of employment to population in a state. The third column of Table 2 reveals that the count of the number of legal exceptions does not correlate with a statistically significant drop in the employment rate, nor do dummies indicating the individual presence of one, two, or three exceptions alter this finding (fourth column).

Columns 5 through 8 of Table 2 reveal that the implied contract is itself the driving force behind the Table 1 results. Whether we control for the existence of any legal exception (column 5) or the count of the number of exceptions (column 6), the implied contract dummy consistently has a negative and statistically significant coefficient. Indeed, the only time the implied contract dummy loses significance (while remaining negative) is in column 7, where we simultaneously have a dummy indicating the presence of all three exceptions (in the last row of the table). Because a state with all three exceptions also has the implied contract exception, some of the explanatory power from the implied contract dummy is lost in the column 7 regression. We see this in column 8, where the estimated coefficient on the implied contract dummy is almost identical to that in column 1 and the dummy indicating all three exceptions falls to insignificance when we simply drop out the controls for the public policy and good faith exceptions. In other words, the individual legal doctrine – as opposed to the existence of a single or number of exceptions – matters, and the finding that the implied contract exception lowers the employment rate remains robust.

e. Disaggregating by gender and education

The most consistently significant effect estimated in Tables 1 and 2 was the adverse impact on employment (and unemployment) associated with the adoption of the implied contract exception. This effect was found both in the regression models that either omitted or included state and group time trends, and also in models controlling for the existence or count of exceptions to the employment at will doctrine. Table 3 explores whether these findings are robust to disaggregation by age and sex of the worker. Each of the eight regressions in Table 3 examines the impact of the three exceptions on a single sex/age group.[22] Beginning with the top panel for males, we basically see the same pattern that was observed in Tables 1 and 2: the implied contract exception is associated with lower employment rates, while the other two exceptions are not. Moreover, the strength of the effect of the implied contract exception on employment rates declines monotonically with age, and actually becomes statistically insignificant for ages 55-64. For females, none of the exceptions has any significant impact

[22] All the Table 3 regressions include state time trends as well as time trends for the two education groups (greater than high school education and high school or less).

except the implied contract exception and here only for those aged 18-24.

Once again including state time trends, we now probe further whether the implied contract exception has different effects depending upon the age, education, and sex of the worker. Table 4 provides this disaggregation by showing the impact of the implied contract exception on the employment to population ratio for each of these subgroups. This table presents the results of 16 separate regressions in which the effects on the employment of males or females, two educational categories, and four age categories are estimated. The first cell explores the impact of this exception on the employment rates for young males (aged 18-24) who have no more than a high school education. Looking at this young, less-educated, male group across 50 states over 22 years leaves us with 1,100 observations to estimate the effect of the legal change on employment. The table reveals that the implied contract exception reduces by almost 2 percentage points the employment to population ratio of young, lower educated males, and that this estimate is statistically significant.

If we look at the cell just below this, we can see the results for slightly older (age 25-39), similarly lower-educated males. Again, the implied contract exception dampens employment for this group, but the effect is now only half as large as that for the youngest males with low education. In fact, as we continue moving down that column we again see that the impact of the implied contract exception falls monotonically as the age of the low-educated male worker increases.

Looking down the first column reveals that the impact of the implied contract exception on employment falls as lower educated male workers age. We can also look across each of the four rows for male workers to see, for any age category, how greater education will influence the impact of the exception. Here we find that for any given age category of male workers, the impact of the exception falls for more educated male workers. To see this, compare the results in the first two columns of Table 4 (under Males). For each of the four age categories, as one moves from column 1 (the lower educated males) to column 2 (the higher educated males), one sees the estimated impact on employment decline.

Looking at panel B of Table 4, we see that the basic pattern that the implied contract exception dampens employment more for the young than for the old and more for the more educated than for the less educated holds up with only two exceptions.[23] Moreover, the adoption of this exception impairs the employment of lower-educated men substantially more than it impairs the employment of lower-educated women.

f. Disaggregating by race, gender, and education

The evidence presented in Table 4 prompts the conjecture that the implied contract exception tends to dampen employment more for groups with lower levels of human capital or attachment to the labor force. This might suggest that the adoption of this and other exceptions to employment at will would most adversely effect the employment of blacks. To explore this issue, we probed whether these exceptions had different impacts on employment for blacks and whites. It should be noted, though, that the power of our statistical tests is diminished by the time we have disaggregated by state, by year, by gender, by age, by education, and by race, and our results must be interpreted in light of this fact. Table 5 reveals that there are differences by race, although perhaps different from the initial speculation. While the adoption of the implied contract exception dampens employment of whites along the lines of what we have previously observed, there is almost no impact on black employment. Moreover, the absence of an impact on black employment persists whether one examines all black employment or disaggregates to examine black male and female employment separately.

One possibility is that the lack of an observed impact on black employment is merely a product of our small samples sizes. Indeed, given the large standard errors on our estimated effects on black employment, we cannot reject the possibility that the coefficients on white and black employment are the same. Moreover, as we will see in the next section, the results by race change when we estimate these effects using a lagged dependent variable model.

[23] Specifically, the oldest low-educated women and higher-educated 40-54 year old women do worse than this summary would have predicted (although the latter effect is not statistically significant for women of this age group in either education category).

Still, the ostensible puzzle of our Table 5 results may be resolved if one considers the impact of Title VII of the 1964 Civil Rights Act, which prohibits discrimination in employment across the nation. By 1978, federal employment discrimination law had largely been transformed into a mechanism for protecting covered employees, such as blacks, from being subject to wrongful discharge (Donohue and Siegelman, 1991). Thus, over the period in which we are examining the impact of state common law exceptions to employment at will blacks everywhere were already greatly protected against wrongful discharge, and therefore, we should expect that the adoption of the implied contract exception should have little or no impact on black employment. With the implied contract exception offering little or no incremental protection to black workers over what they already had under federal antidiscrimination law, the lack of any discernible impact on black employment is to be expected.

There are two caveats to this tidy story. First, just as blacks are a protected group under federal antidiscrimination law, so too are women. Thus, this rationale for why we observe no impact on the employment of blacks might suggest that we would similarly observe no impact on the employment of white women. Yet while the measured impact does appear smaller for white women than for white men, the implied contract exception is still seen to have a dampening impact on the employment of white women.

Second, if the presence of federal antidiscrimination protection is what explains the lack of impact on black employment of the implied contract exception, one would suppose that the public policy and good faith exceptions would similarly have no impact on black employment, when in fact Table 5(a) suggests that they both *enhance* black employment. Table 5(b) shows, however, that at least the ostensible *positive* impact on black employment from the good faith exception disappears if we simply drop California from our analysis, suggesting that the adoption of the good faith exception in California happened to correspond with a stimulus to black employment presumably generated from other sources. Nonetheless, we are still left with the puzzle about why the public policy exception would stimulate black (but not white) employment when federal law would seem to already provide this protection to black workers.

Table 5 reveals that the implied contract exception lowers only white, but not black employment levels. When we further disaggregate race and gender by education category in Panel A of Table 6, we find that all of the impact of this exception comes through its dampening effect on the employment of lower educated whites (and most powerfully for lower educated white males). Moreover, the dampening effect on employment levels is found across all age groups for whites, but is absent for blacks of all ages (Panel B).

g. Estimating a lagged dependent variable model

As we will see in the following section, there may be reasons for concern that exceptions to employment at will are more likely to be adopted during economic expansions. In this event, measured declines in employment following adoption could simply be reflecting a return to more customary employment levels, rather than capturing the causal influence of the legal change. To provide another test of the robustness of our results that is designed to address this issue, we estimated the impact of the legal exceptions using a lagged dependent variable model.

The benefit of using the lagged dependent variable model is in making an alternative assumption to that of the fixed effects estimator. The fixed effects estimator assumes that all level changes in the dependent variable are permanent: that is, if the ratio of employment to population in a state rises in response to a legal change, the fixed effects model implicitly assumes that it should stay that way. If it drops again, this is considered a new shock rather than mean reversion. By contrast, the lagged dependent variable model assumes that in the long run, states will return to a fixed level of employment/population (provided that the coefficient on the lag is less than 1). This means that a positive shock to employment/population this year will, in expectation, be followed by a negative shock the following year.

The use of the lagged dependent variable model responds to the concern that exceptions to employment at will are adopted at the height of booms and hence the negative impacts we find are actually the result of mean reversion. If our estimated negative impact on employment is genuine, this effect should be semi-permanent. Because the lagged model explicitly allows for mean reversion, our results would be buttressed if we were to find permanent impacts even after controlling for the fact that the employment to population ratio may have been unusually high at the time of the adoption of the implied contract exception. Table 7 shows this to be the case: both the implied contract exception and the good faith exception lower employment by at least a third of a percentage point, and the estimates are statistically significant in all three regressions. Note that in comparison with our Table 1 results, the estimates of the impact of the implied contract exception from the lagged dependent variable are 25-50 percent lower than those generated by our standard panel data regression. The results for the good faith exception, though, are roughly mid-way between the high and low estimates from our Table 1 regression.

Despite the difficulty of generating results at a very highly disaggregated level, we decided to use the lagged dependent variable model to see if our earlier Table 4 results based on disaggregations by gender, age and education would persist for the implied contract exception. Table 8 reveals that similar but generally smaller coefficients are generated using the lagged dependent variable model. Specifically, Table 8 reveals that five of the eight coefficient estimates for those under 40 showed statistically significant drops in employment associated with the adoption of the implied contract exception, while none of the eight coefficients for those above 40 were statistically significant.

Table 9 uses the lagged dependent variable model to examine the impact on the four race/gender categories that were previously examined with our standard model in Table 5. The standard model had suggested that whites but not blacks experienced employment drops because of the implied contract exception. The Table 9 results for weaken these results: for the four regressions showing the impact of the implied contract exception in which state fixed effects were included (the even columns), only one coefficient -- for white males -- reveals a statistically significant employment decline. Overall, the analysis using the lagged dependent variable model is supportive of the findings from our standard model, although it may suggest that some of the impact attributed to the implied contract exception from that analysis is in fact the product of mean reversion.

h. Exploring the timing of the impact of the implied contract exception

Thus far, we have estimated the effect of the various exceptions to employment at will by including in our regressions a dummy variable set equal to one in the year of adoption of the particular exception and continuing at that level as long as the exception remains in effect.[24] We can explore whether that model properly captures the timing of the changes in the labor market associated with the adoption of the implied contract exception by using a series of lead and lag time dummies. Ideally, from the perspective of getting a clean estimate of the impact of the legal exceptions, the lead dummies would be close to zero and statistically insignificant. It is conceivable, though, that the timing of these legal changes might be influenced at least in part by the state of the economy, which would then bias our dummy variable estimates of the effect of adopting these exceptions. To explore the possibility of this endogeneity, we estimated the impact of the implicit contract exception while introducing two lead dummies, one reflecting the state employment situation three to four years prior to adoption and the second reflecting the state of the economy one to two years prior to adoption. Other time dummies are included that capture the state of the economy in the year of and after adoption, two to three years after adoption, four to five years after, six to seven years after, and eight or more years after.

Table 10 reveals that one to two years before adoption, the state of the economy is generally strong, as reflected in the higher employment ratio and lower unemployment rate at levels that are both economically and statistically significant. The economy (measured by the employment to population ratio and the unemployment rate) softens in the year of adoption and the following year, and then drops

[24] Thus, when New Hampshire and Oklahoma first adopted the good faith exception, the dummy for this exception was set to 1 for these states, and then returned to 0 when each state rescinded this exception.

sharply two to three years after adoption, before returning to virtually no effect from the fourth year on.[25] The results in Table 10 indicate that the implied contract exception tends to be adopted when the economy is strong in the immediately preceding two years. This finding suggests that our earlier estimates in Tables 1 through 4 of the impact of the implied contract exception may have tended to exaggerate the negative impact on employment of the exception.

The lagged dummies suggest that the impact of the law on the employment to population ratio was felt immediately around passage and kept growing for the next two to three years, before fully dissipating from the fourth year on. To see this note the magnitude of the three dummies for just before, during, and after adoption, which, for both columns 1 and 2, reveal a monotonic pattern of decline. The finding that the impact of the implied contract exception on the employment rate decreases over time suggests a number of possibilities. First, it may take a few years before employers are able to find ways to dampen the negative consequences on employment of creating the implied contract exception. Second, the diminishing effect may suggest that employers over-reacted to the initial change in the law, by over-estimating the costs that the legal change would impose on employment. Support for the latter position comes in a paper by Edelman, Abraham, and Erlanger (1992), which argues that personnel journals and practice (non-academic) law journals vastly overstated the threat posed by the implied contract doctrine, drumming up employer attention and leading to excessive management changes. This argument might explain large initial employer responses even when the risks of litigation were low, followed by gradually dampening effects as the gap between the perceived and actual threat narrowed as employers gained more experience with the

[25] This pattern holds for both specifications estimating the impact on the employment to population ratio (columns 1 and 2 of Table 3) and the specification estimating the impact on the unemployment rate that does not include the state and group time trends (column 3). When the impact on the unemployment rate is estimated with state and group time trends (column 4), the unemployment rate remains lower through the time periods, but is at or near statistical significance for the following time periods: from two years prior to adoption to one year after adoption and then from the fourth year after adoption on.

new legal regime.[26] Third, as we discuss in the next subsection, the impact of any exception on employment may be muted as neighboring (or simply a higher proportion of all) states also adopt, thereby dampening the ability of employers to shift employment to perceived lower cost jurisdictions.

Table 10 examined the timing of the impact of the implied contract exception by looking at changes over time in our three labor market measures for all 16 demographic groups. Table 11 explores the timing issue with leads and lags, but now focuses on the impact of all three legal exceptions on employment in 13 industries.[27] The top panel of Table 11 again shows that the implied contract exception tends to dampen employment. When the state and industry trends are included, industry employment falls by about 2.3 percent. Note that the good faith exception is associated with a roughly 2 percent *increase* in employment.[28]

The bottom panel of Table 11 reveals coefficient estimates that are consistent in terms of sign and size with those in the top panel of the table, except that virtually none of the results (only 3 of 42 estimates) are statistically significant. Interestingly, while we saw evidence in Table 10 of endogeneity in the timing of adoption – specifically, adoptions were more likely when the employment ratio was high – Table 11 reveals virtually no evidence in support of a statistically significant effect on log industry employment in the period prior to adoption.

i. Are there cohort effects in the impact of the implied contract exception?

The earlier analysis has suggested that employment rates fall upon adoption of the implied contract exception, although this effect likely dissipates over time. It is also interesting to explore whether there

---

[26] Throughout this paper, we measure the effect of the law with either a single post-adoption dummy or with a series of dummies. An alternative specification might involve testing whether the adoption of an exception altered any pre-adoption trend. This specification was generally rejected by the data, as the results in Table 4a(12??) might lead us to expect.

[27] These thirteen aggregate industries cover employment in all sectors. All specifications include year dummies and state-industry fixed effects.

[28] When the state and industry time trends are not included, the estimated affect of the good faith exception is extremely large – almost 9 percent. Inclusion of the trends reduces this estimate from 9 percent down to about 2 percent, at least suggesting the possibility that more precise controls for existing statewide and industry-wide time trends would wipe out this effect.

is any cohort effect among the states that adopt – in other words, whether the estimated impact of adoption varies depending on the timing of adoption. Table 12 explores this issue by showing the estimated effect of the implied contract exception for four different sub-periods (in Panel A) and for two sub-periods (Panel B).[29] Panel B reveals that the 29 states that adopted the exception over the period from 1978 through 1986 showed substantial drops in their employment rates (particularly for the column 2 model including state and demographic group time trends). In contrast, the 12 states that adopted the implied contract exception from 1987 until the final adoption in 1992 experienced a drop in employment rates that is both smaller than in the earlier period and statistically insignificant in both columns 1 or 2.

Several explanations for this phenomenon are possible. First, there may be a pure cohort effect. The early adopters may be the states that are most aggressive in their pursuit of employee rights or of this right in particular and thus experience greater negative effects on employment. The late adopting states may be the states least interested in adoption (as indicated by their previous resistance to adoption) and hence more likely to interpret the exception narrowly, thereby muting its impact.

A second possibility is, as argued by Edelman, Abraham, and Erlanger (1992), that exaggerated forecasts of the consequences of unjust dismissal laws led employers in early-adopting states to anticipate a greater burden from the unjust dismissal doctrines than ultimately materialized. Benefiting from the experience of these early-adopters, employers in later adopting states might have reacted less severely to their adoption.[30]

Finally, the impact of the law may differ when few states have previously adopted the exception than when most states have already done so. If employers believe that the adoption of the implied contract exception implies greater risk or cost associated with employing labor in the state, the

---

[29] Because no implied contract exceptions were adopted after 1992, we limit our sample for this analysis to the period 1978 – 1995.

[30] Alternatively, employers in later adopting states may have anticipated the eventual adoption of the doctrines and largely responded in advance.

adoption could induce firms to re-locate to other states or to choose other states to expand their facilities. This would suggest that the adoption could indeed be seen to have a negative effect on employment in the adopting state but that the overall effect would be lower because employment would increase in other states. As more and more states adopt the exception, however, the ability to avoid this cost declines, as does the estimated impact of the exception. [31]

4. The impact of wrongful discharge laws on other employment outcomes

a. Employment of temporary workers

Autor (forthcoming) and Miles (2000) both find that the imposition of what employers perceive as burdens on the employment relationship can increase the reliance on temporary workers. The causal significance of our findings that employment rates fall when the implied contract exception is adopted is buttressed by the finding that this exception is correlated with substantial increases in temporary employment. Table 13 (from Autor, forthcoming) reveals that across all the regressions, the coefficient for the implied contract exception is always significant, while it never is for either of the other two legal exceptions. Across all seven regressions, the estimated effect of adoption of the implied contract exception is to increase the number of temporary workers in a state by 13 - 16 percent. This result remains robust to inclusion or exclusion of state specific time trends, and to controls for state employment levels, as well as state unemployment and unionization rates. Since employment in the temporary help sector is quite pro-cyclical (note the large coefficients on state employment and unemployment) these estimates buttress the case that the adoption of the implied contract exception is not primarily proxying for adverse state economic conditions but instead has real effects on the labor market.

---

[31] As noted in the introduction, in a small number of cases, state courts reversed exceptions to employment at will that they had earlier adopted (1 implied contract and 2 good faith exceptions were reversed or substantially weakened, the latter most notably in California). Although we estimated models that relaxed the constraint that adoptions and reversals have equal but opposite effects, we lack sufficient statistical power to draw inferences on the differential impact of reversals. Supplemental tables are available from the authors on request.

b. Does unionization alter the findings of the impact of legal exceptions

Because one of the primary benefits a union confers on its workers is the right not to be dismissed absent a showing of good cause, one would expect that union members would not personally benefit from the adoption of exceptions to the doctrine of employment at will (since they would already be fully protected – at least to the extent that the union tended to vigorously pursue claims of wrongful discharge). To explore this issue, we included controls for the percentage unionized in a state in estimating the Table 1 regressions of the impact of the legal exceptions on state employment rates. Column 3 of Table 14 again reveals that the implied contract exception significantly reduces employment while the other two exceptions have no systematic affect on employment. The column 4 regression reveals that the impact of the implied contract exception on overall state employment rates drops as the union share of the state's workforce rises, consistent with our expectation that this legal change will not effect the employers of already unionized workers.

Table 15 extends the analysis of the impact of unionization by disaggregating the Table 14 analysis by age and sex. While the overall impact of the implied contract exception is negative in all the regressions, it is clearly the case that the exception dampens the employment of males more than females, but that higher levels of unionization tend to dampen the effect of the exception.[32]

5. The impact of the legal exceptions to employment at will on wages

Our previous analysis has provided estimates of the impact on employment and unemployment of the adoption of the three exceptions to employment at will. We now expand this analysis to ascertain if the adoption of any of these three exceptions has an impact on the hourly wages of employed workers. A simple price theoretic framework reveals that the effect on employment and wages of the implied

[32] Because the CPS provides unionization data only after 1982, Tables 7 and 8 are both estimated over the period from 1983-99. We have recently obtained unionization data going back to 1978 from Hirsch and Macpherson. Using this more complete time series we find that during 1978-82 unionization did not interact positively with IC in limiting employment reductions. We suspect that the different results achieved during the 1978-82 period from those achieved after 1982 reflect the effects of the deep recession in the early 1980s. We will explore this issue further in the next draft of the paper.

contract exception would depend on how adoption shifts both the supply and demand curves for labor. Presumably, the demand curve would shift down as the total cost of labor would be higher and the supply curve would shift outward as workers would now have a more attractive employment package for any given prior wage level. Because both curves will be shifting, theory cannot predict the impact of adoption on employment, which can remain the same (if the demand and supply shifts are precisely offsetting), fall (if the demand curve shift dominates the supply curve shift), or rise (if the supply curve shift dominates). Theory does predict, however, that both of these predicted shifts will cause wages to fall.

Because Table 1 indicated that the implied contract exception dampened employment, this would imply that the added costs to employers exceeded the increased benefits to workers, thereby causing the demand curve to shift more than the supply curve. Table 16 explores whether the theoretical prediction of lower wages associated with the adoption of the implied contract exception has empirical support. It does not. While the inclusion of state and demographic group time trends does generate a negative coefficient (row 1, column 2), the estimate is statistically insignificant. Without such time trends, (row 1, column 1) the estimated effect of the implied contract exception on wages is *positive* (although statistically insignificant).

Conceivably, the estimated *negative* effect on wages in row 1, column 2 could be sound, but its statistical significance impaired by confounding selection effects. Obviously, if our Table 3 and 4 findings are correct that the adoption of the implied contract exception has a negative impact on the employment of lower-educated and younger workers, then a selection effect could be operating in a way that biases upward the mean wages we observe in adopting states. We attempt to correct for this impact by estimating mean wages for the sixteen age, gender, and education groups. Nonetheless, there will still be unobserved heterogeneity within each of these groups. If Tables 2 and 3 imply that lower skilled workers within any group will suffer greater declines in employment, then this selection effect will tend to elevate mean wages for the group as the lowest-paid workers within the group exit from employment.

Table 16 also shows estimates for the wage effects of the other two exceptions to employment at will. The estimated effect of the good faith exception is highly sensitive to the inclusion of the state and demographic group time trends, showing a negative (but statistically insignificant) effect when they are excluded, and a statistically significant and large positive effect when they are included. Given the strong theoretical prediction that the exceptions to employment at will tend to dampen wages, it is difficult to repose confidence in the estimated positive coefficient for the good faith exception (in column 2).

The estimated effect on wages for the public policy exception is negative and statistically significant. Coupled with the finding from Table 1, above, that the public policy exception, if anything, tends to increase employment, the combination of higher employment and lower wages suggests that the public policy exception leads to a greater supply response than demand response.[33]

Table 17 examined the impact of the three exceptions on wages by estimating separate regressions for 1) two ten-year intervals (with and without state and demographic group trends), and 2) five four-year intervals (without state and demographic group trends). The results are generally statistically insignificant, and no clear positive or negative effect is discerned from any of the three exceptions. These results lead us to conclude that there are no reliably detectable impacts of the unjust dismissal doctrines on wages.

6. Reconciling with the literature

Our findings that unjust dismissal laws – the implied contract exception in particular – have had modest but economically meaningful adverse impacts on the employment of younger, less educated, and non-union workers stand in contrast to two published papers exploring the same questions. The

[33] One can imagine circumstances under which the public policy exception might tend to *increase* wages, but it is hard to construct similar arguments for the other two exceptions. The public policy exception could actually enhance firm performance by undermining the ability of managers to act in furtherance of their own interests at the expense of their firm's interest.

first, a widely cited study by Dertouzos and Karoly (1992 and 1993), found that states' adoption of exceptions to employment at will reduced aggregate state employment by 4 to 5 percentage points, with even larger reductions in employment in the service and finance sectors. The size of these estimated employment declines – roughly an order of magnitude greater than our estimates – along with the sophistication of the econometric techniques lead this work to be widely cited both in the academic literature as well as in public policy debates.[34] More recently, Thomas Miles (2000), using a methodology more comparable to our own, found *no* effect of unjust dismissal doctrines on aggregate employment or unemployment. We believe the differences between our findings and these two analyses can be traced to key methodological choices explained below.

a. The Dertouzos and Karoly study

To estimate the impacts of wrongful discharge laws on state employment, our analysis employs a fixed effects estimator that exploits discrete cross-state variation in the timing of the adoption of unjust dismissal laws to contrast labor market outcomes in adopting and non-adopting states. Dertouzos and Karoly (D&K hereafter) eschew this source of variation, arguing that the adoption of state laws may be driven endogenously by the 'supply and demand' of legal doctrines. Accordingly, they employ a variety of fixed and time varying state characteristics as instrumental variables to predict states' adoption of doctrines, and use these predicted values in place of the actual laws. Their instruments include whether a state had a right to work law in 1980, whether it has a Republican governor, the state's level and change in union membership, the state's change in lawyers per capita, and the fraction of bordering states recognizing a similar doctrine. As their analysis demonstrates, some of these variables are indeed correlated with states' propensity to adopt common law exceptions.

Yet, there is reason to doubt whether these predictors withstand scrutiny as valid instrumental variables. Two requirements for the validity of an instrument are that it has a direct causal impact on

[34] For example, during his gubernatorial campaign, California Governor Pete Wilson prominently cited Dertouzos and Karoly's conclusions in support of tort reform (Hopper, 1995).

the potentially endogenous regressor (states' adoption of an exception to employment at will) and that it does not otherwise directly affect the dependent variable (log state employment) *except* through its influence on the endogenous regressor. It is this second stipulation that is of particular concern. While state unionization levels, governor's political party, and court activities in neighboring states *are* correlated with the propensity of state courts to amend the common law, it appears dubious that these factors do not also directly influence the business environment in the state as well. For example, a large literature documents the economic impacts of labor unions on employment and wages (cf. Farber 1986). Accordingly, we believe there is a priori reason for concern about these particular instrumental variables.[35]

A more specific concern with the D&K approach, however, lies with two particular variables they select as instruments: the fraction of neighboring states recognizing a given doctrine, and a variable indicating whether a state had a right to work law in 1980. Notably, both of these variables have a substantial regional component; the first is strongly negatively correlated with Southern geographic location and the second is strongly positively correlated with it. In particular, 85 percent of Southern states had a right to work law in 1980 versus only 25 percent of non-Southern states. Moreover, as shown earlier (Figure 2), Southern states were both later and less likely to adopt wrongful discharge laws than non-Southern states. Accordingly, for any given Southern state, the fraction of neighboring states adopting an exception is considerably lower on average than for other U.S. states.

These regional correlations are potentially problematic. As discussed by Katz and Blanchard (1992) and depicted in Figure 3, employment in Southern states grew persistently faster than other regions of the U.S. from the late 1930s onward. These trends substantially predate the adoption of unjust dismissal laws and likely stem from factors including the advent of air conditioning, which

[35] Miles (2000) also considers and rejects as invalid a number of instrumental variables, ultimately choosing to exploit the discrete timing of the law adoptions. In fact, there are a host of methodological difficulties with the Dertouzos and Karoly study that we do not treat here. Our focus is on one specific concern, trend variation in growth rates across states, which we consider of both substantive and methodological interest.

increased habitability and manufacturing productivity in the South (most notably, in tobacco and printing), and from civil rights era legislation that increased the wages and employment of Southern blacks (Arsenault, 1984; Donohue and Heckman, 1991).

To see why a correlation between these instruments and pre-existing state growth rates is potentially problematic, consider the following regression model:

(6) $$\ln emp_{jt} = a + dW_{jt} + bX_{jt} + g_j + l_t + e_{it},$$

where emp_{jt} is employment in state (j) in year (t), W_{jt} is a vector of dummy variables equal to one after a particular wrongful discharge doctrine is adopted in a given state, X_{jt} is a vector of time-varying state specific control variables, l_t is vector of year effects that absorb common macroeconomic variation affecting employment levels, and g_j is a vector of state dummies that control for fixed state-specific factors that affect the level of (log) employment. In this model, $\hat{d}$ is an estimate of the causal impact of wrongful discharges laws on the level of employment.

In estimating this equation for the years 1980 – 1987, D&K replace W_{jt} with $\hat{W}_{jt}$, where $\hat{W}_{jt}$ is the predicted presence of the wrongful discharge doctrines in each state and year from a logit regression of W_{jt} on the vector of instruments listed above. By construction, these instrumental variables will predict that southern states have lower odds of adopting wrongful discharge laws, i.e., $E[W_{jt} \mid I_j(South), t] < \overline{W}_{jt}$.[36] But as indicated above, we know that Southern states have been growing more rapidly than others for six decades, which implies that $E[e_{jt} \mid I_j(South), t] > 0$. If so, then we have that $\text{cov}(\hat{W}_{jt}, e_{jt}) < 0$, which will bias the regression towards the spurious finding that $\hat{d} < 0$, wrongful discharge laws reduce employment. Informally, the correlation between the instruments and the error

[36] $I_j(South)$ is the indicator function which takes a value of one if state (j) is in the South and zero otherwise. These patterns are abundantly evident in Tables 3.3 and 3.4 of D&K (1992), which contain their first stage IV estimates. In

term will lead to the inference that unjust dismissal laws *caused* lower state employment growth, when in fact these laws were merely adopted in states that had been experiencing slower growth for decades (see the Statistical Appendix for a formal treatment).

If this concern is well founded, the solution is also straightforward: permitting each state to assume its own time trend in the employment regression will control for smooth growth rates that are otherwise a potential source of bias.[37] To explore the relevance of this concern, we made a substantial effort to replicate D&K's core results using their cited data sources, classification of case law, and empirical methods. Despite these efforts, we were unable to reproduce their findings exactly. Yet, we believe our results are sufficiently comparable to allow us to explore the main concern raised above.[38]

Table 18 presents a summary of the main findings of our replication effort. The first column replicates D&K's basic instrumental variables specification. This specification estimates the impact of three wrongful discharge laws on the log of state employment during 1980 – 1987: the Implied Contract exception *or* Good Faith exception with contract remedy (IC/GF), the broad Public Policy *or* Good Faith exception with tort remedy (PP/GF), and the narrow Public Policy exception (NPP).[39] The estimated impact of these doctrines on state employment is large. The IC/GF doctrine is estimated to reduce employment by close to 4.5 percentage points and the PP/GF doctrine by an additional 3.0 percentage points. Both coefficients are statistically and economically significant.[40]

---

all specifications, the fraction of neighboring states adopting a given doctrine is a strong positive predictor of own-state adoption, and the presence of a right-to-work law is a strong negative predictor.

[37] Where the fixed effects estimator controls for fixed state differences in the *level* of employment, adding a linear trend controls for fixed state differences in *trends* in employment growth.

[38] Dertouzos and Karoly did not provide their original data or programs but kindly shared the mapping of state borders developed for their IV estimates. Disconcertingly, we uncovered more than a dozen coding errors in this mapping. In addition, D&K somewhat unconventionally coded Alaska as bordering Idaho, Oregon, Montana, and Washington state, and Hawaii as bordering California, Nevada, and Oregon. In the service of replication, we used their original mapping.

[39] The first stage estimates for these models (estimates available from the authors) replicate the central pattern noted above from D&K's models: the fraction of neighboring states adopting a given doctrine is a strong positive predictor of own-state adoption, and the presence of a right-to-work law is a strong negative predictor.

[40] As noted earlier, our replication results differ from D&K (1992). While they find (Table 5.2) that the PP/GF doctrine has the largest negative impact on employment (2.1 log points versus 1.4 log points), we find a larger impact on

To the degree that the instrumented state law variables are simply proxying persistent employment trends, inclusion of linear state trend variables will reduce this source of bias. Column (2) of Table 18 adds the trends. An F-test of their joint significance rejects the null hypothesis at the 1 percent level. Notably, inclusion of the trend variables reduces the magnitude of the estimated impacts of the state laws by approximately 60 percent and renders the coefficients insignificant. Tellingly, the standard errors of the law variables are little affected, indicating that the trend variables are not simply introducing collinearity that reduces the precision of the estimates. These results seem to provide strong confirmation of the hypothesis that the D&K estimates are substantially biased.

The subsequent two columns of Table 18 provide an additional specification check. One unusual control included in the D&K's estimates of equation (6) is the log of state gross product and its change. Controlling for these variables is difficult to justify in economic terms since state output is arguably an outcome measure that should be closely correlated with the dependent variable; if unjust dismissal laws reduce employment, they are likely to reduce output as well.[41] Column (3) drops the output variable from the basic D&K model, yielding large and difficult to interpret coefficients on the instrumented law variables. Apparently, the estimates are quite sensitive to inclusion of this unusual control variable. Notably, when in column (4) state trends are added to the model that excludes state output, the estimated impacts of the laws return to reasonable magnitudes and remain insignificant. These findings again suggest that the IV approach is quite non-robust.

To contrast the D&K IV methods to our far simpler fixed-effects methodology, we estimate in columns (5) – (8) a series of models in which we use the actual law changes W_{jt} as independent

---

IC/GF. Our estimates for the NPP doctrine are quite comparable. Following D&K, we do not use a true IV procedure but simply insert predicted values from the first-stage (predictive) equation into the second stage estimates. This procedure is likely to exaggerate the precision of the estimates because it does not account for the fact that the independent variables are predicted rather than observed. In addition, we follow D&K in *not* including all covariates from the second stage estimates in the first stage models (in particular, the first stage estimates exclude state dummies). This method again violates IV practice.

[41] As Alan Krueger noted on an earlier draft of this paper, D&K's finding that the unjust dismissal laws reduce state employment conditional on state output could be interpreted as indicating that the laws increase productivity.

variables rather than their predicted values. Here, we continue to use D&K's coding of the law and other dependent and independent variables. In column (5), we find a positive and significant effect of the IC/GF doctrine on state employment levels, opposite to D&K's IV estimates. However, when we condition on state specific trends, this effect reverses sign and becomes negatively significant at 0.8 percentage points. Because of our concern about the validity of conditioning on state output in these employment models, columns (7) – (8) drop the output measures. These final columns yield results that are both more stable and more consistent with our findings above. Without conditioning on state trends, we find a small negative impact on the IC/GF doctrine on state employment. Adding trends, the magnitude increases to minus 1.5 percentage points, roughly one-third the size of the D&K IV estimate and closely comparable to our estimates in Table 11 (our most comparable model because it estimates the impact of the legal changes on log employment rather than the ratio of employment to population). The comparability of these findings with our own estimates is notable given that the D&K sample uses a considerably shorter time frame, 1980 – 1987 versus 1978 – 1995, and a different coding of the relevant case law.

b. The Thomas Miles study

In a careful recent study, Thomas Miles (2000) concluded that wrongful discharge laws had no effect on aggregate employment or unemployment for the years 1964 – 1995. To understand why Miles' conclusions differ from ours, we replicated his work closely. Several differences stand out, and we explored the importance of each. The Miles study: 1) used a sample that extended from 1964 to 1994, considerably longer than our 1978 – 1999 sample; 2) employed unweighted regressions (ours are weighted by state population; 3) controlled for a number of state level demographic and policy covariates absent from our analysis, including the fraction of neighboring states recognizing a given doctrine; 4) estimated quite conservative standard errors that allow for unrestricted correlations among the errors among all states in a geographic region in all 30 years of the sample; 5) like D&K, did not control for state-specific trends in employment.

The main results from our replication effort are presented in Table 19. Column (1) of Panel A tabulates the estimated impact of a state's adoption of any unjust dismissal law on the log level of state employment during the years 1965 – 1994. Closely paralleling the findings of Miles, the point estimate for this coefficient is negative and economically large, but is also imprecisely estimated and insignificant.[42] In keeping with our concern about the bias induced by prevailing state employment trends, we add state linear time trends to the model (column (2)). As expected, an F-test of the joint significance of these variables soundly rejects the null. Notably, inclusion of these trends substantially reduces the standard error of the point estimate on the law dummy. It is now smaller in magnitude and statistically significant at the ten percent level.

Subsequent columns of Panel A replicate additional specifications from Miles (2000). These specifications add additional state covariates and leads of the law change variable. Consistent with the findings from the D&K re-analysis, we find that after controlling for differential state employment trends, adoption of unjust dismissal doctrines modestly reduced aggregate state employment.

Panel B of Table 19 displays a comparable set of estimates in which we allow the three wrongful discharge doctrines to have independent effects on employment. In the first column, which does not control for state trends, we find *widely* varying impacts of three wrongful discharge laws on state employment levels. None of the point estimates is statistically significant, however.[43] Conditioning on state employment trends in Column (2) yields more plausible results. Each exception is associated with modest employment declines, though only the Public Policy exception is statistically significant.

Subsequent columns, which add state level covariates and law change leads, continue to indicate that the Public Policy and Implied Contract exceptions reduced employment by approximately 1.5 percentage points. However, the significant pre-adoption lead dummy for the Public Policy exception

[42] Miles' point estimate for this variable (Table 4, column (1)) is –1.8. We are currently working with Thomas Miles to understand the slight discrepancies between his results and our replication of them.

[43] Consistent with our estimates in Table 5b that use log employment as the dependent variable, it appears that Good Faith exceptions were adopted in rapidly growing states.

suggests that state employment began to decline significantly just *before* adoption of this doctrine (Column 8). This was not the case for the Implied Contract exception.

In addition to the specifications tabulated, we performed a number of sensitivity tests with the replications including weighting regressions by state size, employing a time interval comparable to our primary estimates (1978 – 1999), and using employment to population rather than log employment as the dependent variable. These tests indicate that the two key difference between Miles' estimates and our own is, first, the inclusion of state-specific trends and, second, the standard errors employed.[44] The importance of the latter is shown in the final two columns of Panels A and B of Table 19. While Miles uses standard errors that allow for unrestricted error correlations among all states in each census region over *all* 30 years, we apply less conservative standard errors in columns (7) and (8). These standard errors permit unrestricted errors correlations by census region but assume that the errors are independent across years. While the choice of standard errors does not affect the qualitative conclusions of the analysis, the estimated precision of the conclusions is greatly affected. Since best practice in this area is rapidly shifting, it is difficult to say which approach yields standard errors that neither over- nor under-state the appropriate confidence interval (cf., Donald and Lang, 2001; Bertrand, Duflo, and Mullainathan, 2001). We are modestly reassured to note that even using the conservative Miles approach, the findings appear largely consistent with our primary analysis.

c. Exploring robustness to different legal classifications and time periods

Classifying the legal status concerning the existence of exceptions to the doctrine of employment at will for each state and for each year is a major undertaking and one in which numerous discretionary judgments have to be made. To explore whether our results are sensitive to the classifications that we

[44] Using overall employment to population yields results comparable to our Tables 1 – 4. One advantage that employment to population offers over log state employment is that we are able to analyze this variable by demographic group. As Tables 3 and 4 demonstrate, unjust dismissal laws have greater employment impacts on younger and less educated workers.

made, we estimated our Table 1 employment rate regressions using our own and two other classification schemes, those of D&K and Morriss.[45] The results are presented in Table 20.

Note that each classification scheme has been compiled for a different time period, reflecting the point at which the various researchers completed their studies. To get a pure test of the importance of the differences in the alternative legal classifications, Table 20 presents the results of regressions run over the same time period that the other scholars used. For example, columns 1 and 2 of Table 20 (labeled "ADS" for Autor, Donohue and Schwab) run our models using the same time period (1978 – 1989) for which we have the legal classification scheme created by Morriss. Whether we used our legal scheme or Morriss's has very little impact on the regression coefficients for the three legal exceptions. In particular, the implied contract exception is shown to have a negative and statistically significant effect on the employment rate in all four regressions. Similarly, there is very little variance in the estimates for the public policy exception, which change little across columns and are wholly insignificant in all four regressions. The only estimated coefficient that is affected by the difference in legal codings between ADS and Morriss is that for the good faith exception, where our coding actually shows a statistically negative effect for the period 1978 and 1989 for both regressions, while Morriss's coding leads to a statistically insignificant effect in one of the two (when the state and demographic group time trends are included).

The Table 20 comparisons between our legal classifications and those of D&K (now for the slightly shorter time period from 1978 - 1987) reveal a generally similar pattern. If one compares the two models that include the state and demographic group time trends (columns 2 and 4 of the middle panel of Table 20), the estimated effects always shrink in absolute value in going from our scheme to that of D&K, but the direction and statistical significance of the findings remains the same. Since one would assume that greater noise in the coding of these independent variables would bias there

[45] In a subsequent draft, we will extend Table 14 to include the Walsh and Schwarz classification of the law used by Miles (2000).

estimated effect towards zero, the finding that our effects tend to be larger may suggest that our coding was somewhat more precise.

Perhaps most importantly, Table 20 provides further assurance on the robustness of the estimated effect of the implied contract exception since it is estimated fairly stably across not only different legal classifications, but also across different time periods. Our Table 1 results estimated over the period from 1978 - 1999 look quite similar to those shown in Table 20 for three different subsets of this longer period.

7. Conclusion

Our study of the impact on employment and wages of the various exceptions to the doctrine of employment at will has led us to a number of conclusions. We find ourselves taking a middle position between those who suggest that the adoption of exceptions to employment at will has had a major negative impact on employment (particularly Dertouzos and Karoly) and those who submit that the exceptions have had no impact (Miles). We have been able to ascertain a statistically significant negative impact on employment, but it emanates from only one of the legal exceptions – the implied contract exception – and its adoption causes a decline of from .4 to .6 percentage points in the ratio of employment to population, which is an order of magnitude lower than that offered by Dertouzos and Karoly. For low-educated young male workers, however, the impact may be as high as 2 percentage points, although our analysis using a lagged dependent variable model might be taken to suggest that at least some portion of the estimated employment declines results from mean reversion rather than from the adoption of the implied contract exception.

While the matter can never be free from doubt in statistical studies of this kind, we think the robustness of our findings across a number of different specifications and dates suggests that our findings have uncovered a true causal effect of adoption of the implied contract exception. This view is buttressed by 1) the robustness of the finding to inclusion of controls for the existence and count of exceptions to employment at will in a state, as well as to analysis using a lagged dependent variable model; 2) the plausibility and stability of the findings when disaggregated by sex, race, age, and education; 3) the persistence of the effect when estimating industry impacts in employment or controlling for unionization, and 4) the presence of strong effects on the amount of temporary employment in a state.

At the same time, our findings also suggest that the impact of the implied contract exception for the nine states that adopted it after 1987 has fallen substantially when compared to the estimate effect for the 29 states adopting between 1978 and 1987. We are uncertain whether this implies that the early adopters had a more aggressive legal posture than the later adopters (a cohort effect) or whether the ability to shift employment away from an implied contract state simply dampens over time as more states adopt the exception (a substitution effect). Of course if it is the latter, then our estimates of the negative impact on employment, though already somewhat modest, would exaggerate the overall impact on employment because reductions in one state would to some extent lead to offsetting increases in employment in other states.

Those steeped in the view that low transaction costs would give rise to a Coasean invariance prediction might be surprised by the finding that the implied contract doctrine leads to any dampening of employment when it would seem that forms filled out by new employees could easily negate the legal effectiveness of this exception. Conversely, others might see the apparent inability to contract costlessly around legal rules as further confirmation that the invariance predictions of the Coase Theorem frequently do not obtain in labor markets (Donohue 1989). The dampening effect of the implied contract exception over time may suggest another possibility – that over time employers did learn how to fully circumvent the legal rule through appropriate disclaimers.

Note that our paper does not attempt to provide an overall assessment of wrongful discharge law. We have not offered any evaluation of the benefits of such laws to workers and the public, although it would seem somewhat uncontroversial that a narrow public policy exception would be welfare enhancing by undermining the ability of supervisors to act in socially harmful ways (e.g., by threatening workers with discharge if they revealed wrong-doing or cooperated with legal investigative authorities). The fact that there is some dampening of employment (assuming that any substitution effect is incomplete), especially for younger, low educated males underscores that legal protections do not come costlessly. Still, we see little evidence of a wage cost associated with the adoption of the implied contract exception (assuming it is not being disguised by an offsetting selection effect). This implies that any cost to workers is primarily operating through a modest dampening in demand of relatively low-skilled male labor. Note that if workers valued the benefit of the implied contract exception highly, one would expect to see a larger drop in wages because wage effects of the adverse demand shift caused by the burden to employers would be augmented by the benign supply shift caused by the perceived benefit to employees. The absence of a wage effect – again with the caveat for possible selection effects – may be taken as evidence against a strong supply response to the implied contract exception, suggesting that ex ante valuations of this benefit by workers are not large.[46]

[46] One explanation why workers' valuations may be low is that many workers appear to believe that they *already* possess far greater employment protection than the law affords them (Kim, 1997 and 1999).

References

Acemoglu, Daron and Joshua Angrist. Forthcoming. "Consequences of Employment Protection? The Case of the Americans with Disabilities Act." *Journal of Political Economy*.

Aghion, Phillipe and Benjamin Hermalin. 1990. "Legal Restrictions on Private Contracts can Enhance Efficiency." *Journal of Law, Economics, and Organizations.* 6(2), Fall, 381 – 409.

Arsenault, Raymond. 1994. "The End of the Long Hot Summer: The Air Conditioner and Southern Culture." *The Journal of Southern History,* 50(4), 597 – 628.

Autor, David H. Forthcoming. Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Temporary Help Employment." *Journal of Labor Economics.*

Angrist, Joshua D. and Alan B. Krueger. 1999. "Empirical Strategies in Labor Economics." In Orley C. Ashenfelter and David Card, editors, *Handbook of Labor Economics,* volume 3, Netherlands: North-Holland.

Bertrand, Marianne, Esther Duflo and Sendhil Mullainathan. 2001. "How Much Should We Trust Difference-in-differences Estimates?" mimeograph, MIT.

Blanchard, Olivier Jean and Lawrence F. Katz. 1992. "Regional Evolutions." *Brookings Papers on Economic Activity*, 1.

Blanchard, Olivier and Pedro Portugal. 2001. "What Hides Behind an Unemployment Rate: Comparing Portuguese and U.S. Unemployment." *American Economic Review*, 91(1), March, 187 – 207.

Blanchard, Olivier and Justin Wolfers. 1999. "The Role of Shocks and Institutions in the Rise of European Unemployment: The Aggregate Evidence." *The Economic Journal.* 110, March, C31 – C33.

Dertouzos, James N., Elaine Holland, and Patricia Ebener. 1988. *The Legal and Economic Consequences of Wrongful Termination*. Santa Monica, CA: Rand.

Dertouzos, James N. and Lynn A. Karoly.1992. *Labor-Market Responses to Employer Liability.* Santa Monica, CA: Rand.

Dertouzos, James N. and Lynn A. Karoly. 1993. "Employment Effects of Worker Protection: Evidence from the United States." in Christoph F. Buechtemann, editor. *Employment Security and Labor Market Behavior: Interdisciplinary Approaches and International Evidence.* Ithica, NY: ILR Press.

Donald, Stephen G. and Kevin Lang. 2001. "Inference with Difference in Differences and Other Panel Data." mimeo, Boston University, March.

Donohue, John J. III. 1989. "Diverting the Coasean River: Incentive Schemes to Reduce Unemployment Spells," 99, *Yale Law Journal,* 549.

Donohue, John J. III. 1998. "Did Miranda Diminish Police Effectiveness?" 50, *Stanford Law Review*, 1147.

Donohue, John J. III and James Heckman. 1991. "Continuous versus Episodic Change: The Impact of Civil Rights Policy on the Economic Status of Blacks." *Journal of Economic Literature*, 29, December, 1603 – 1643.

Donohue, John J. III and Peter Siegelman. 1993. "Law and Macroeconomics: Employment Discrimination Litigation over the Business Cycle," *Southern California Law Review*, 66, 709-65.

Donohue, John J. III and Peter Siegelman. ″The Changing Nature of Employment Discrimination Litigation,″ 43 Stanford Law Review 983 (1991).

Donohue, John J. III and Peter Siegelman. 2001. "The Changing Nature of Employment Discrimination Litigation Revisited," Stanford University, in progress.

Edelman, Lauren B., Steven E. Abraham, and Howard S. Erlanger. 1992. "Professional Construction of Law: The Inflated Threat of Wrongful Discharge," *Law & Society Review*, 26, 47-83.

Epstein, Richard A. 1983. "In Defense of the Contract at Will," *University of Chicago Law Review*, 51.

Farber, Henry S. 1986. "The Analysis of Union Behavior." in Orley C. Ashenfelter and Richard Layard, editors. *Handbook of Labor Economics*, Volume 2, Netherlands: North-Holland.

Hahn, Jinyong, , Petra Todd, and Wilbert van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design," *Econometrica,* 69(1), 201-209.

Heckman, James J. 1995. "Instrumental Variables: A Cautionary Tale." NBER Technical Working Paper No. 185.

Hirsch, Barry T. Hirsch, David A. Macpherson, and Wayne G. Vroman. Forthcoming 2001. "State-Level Estimates of Union Density, 1964-Present." *Monthly Labor Review*, 124(7), July.

Hopper, Martyn. 1995. "California Needs Tort Reform." *The Sacramento Bee*, June 10.

Jolls, Christine. 2000. "Accommodation Mandates," *Stanford Law Review*, 53(2), 223 – 306.

Jung, David. J. and Richard Harkness.1988. "The Facts of Wrongful Discharge," *Labor Lawyer*, 4, 257.

Kamiat, Walter. 1996. "Labor and Lemons: Efficient Norms in the Internal Labor Market and the Possible Failures of Individual Contracting," *University of Pennsylvania Law Review*, 144, 1953.

Katz, Lawrence F. and Olivier Blanchard. 1997. "What We Know and Do Not Know About the Natural Rate of Unemployment," *Journal of Economic Perspectives*, 11, 51 – 72.

Katz, Lawrence F. and Olivier Jean Blanchard. 1992. “Regional Evolutions,” *Brookings Papers on Economic Activity*, 1.

Kim, Pauline T. 1997. "Bargaining with Imperfect Information: A Study of Worker Perceptions of Legal Protection in an At-Will World," *Cornell Law Review*, 83, 105.

Kim, Pauline T. 1999. "Norms, Learning, and Law: Exploring the Influences on Workers' Legal Knowledge," *University of Illinois Law Review*, 1999, 447.

Krueger, Alan B. 1991. “The Evolution of Unjust-Dismissal Legislation in the United States,” *Industrial & Labor Relations Review*, 44(4), 644-660.

Kugler, Adriana and Gilles Saint-Paul. 2000. “How Do Firing Costs Affect Worker Flows in a World with Adverse Selection?” mimeograph, Universitat Popeu Fabra, October.

Lazear, Edward P. 1991. “Job Security Provisions and Employment.” *Quarterly Journal of Economics*, 105(3), 699 – 726.

Levine, David I. 1991. “Just-Cause Employment Policies in the Presence of Worker Adverse Selection,” *Journal of Labor Economics*, 9, 294 – 305.

Lewis, H. Gregg. 1986. *Union Relative Wage Effects: A Survey.* Chicago: University of Chicago Press).

Marshall, Alfred.1890. *Principles of Economics*. Macmillan: London.

Miles, Thomas J. 2000. “Common Law Exceptions to Employment at Will and U.S. Labor Markets.” *Journal of Law, Economics and Organizations*, 16(1), 74 – 101.

Morris, Andrew P. 1996. “Bad Data, Bad Economics, and Bad Policy: Time to Fire Wrongful Discharge Law,” *Texas Law Review*, 74, 1901.

Morriss, Andrew P. 1995. “Developing a Framework for Empirical Research on the Common Law: General Principles and Case Studies of the Decline of Employment-at-Will,” *Case Western Reserve Law Review*, 45, 999 – 1148.

OECD Employment Outlook: 1999. Paris, France: OECD, 1999.

Olson, Walter K. 1997. *The Excuse Factory: How Employment Law is Paralyzing the American Workplace*. New York: Martin Kessler.

Oyer, Paul and Scott Schaefer. 2000. “Layoffs and Litigation.” *Rand Journal of Economics*, 32(2), Summer, 345 – 358.

Peck, Cornelius J. 1979. “Unjust Discharges from Employment: A Necessary Change in the Law,” *Ohio State Law Journal*, 40, 1.

Schwab, Stewart J. 1996. “Wrongful Discharge Law and the Search for Third-Party Effects,” *Texas Law Review*, 74(7), 1943 - 1978.

Schwab, Stewart J. 1993. "Life-Cycle Justice: Accommodating Just Cause and Employment At Will," *Michigan Law Review*, 92(1), 8 - 62.

St. Antoine, Theodore J. 1988. "A Seed Germinates: Unjust Discharge Reform Heads Toward Full Flower," *Nebraska Law Review*, 67, 56.

Summers, Clyde. 1992. "Effective Remedies for Employment Rights: Preliminary Guidelines and Proposals," *University of Pennsylvania Law Review*,141, 457 – 546.

Summers, Clyde. 1976. "Individual Protection Against Wrongful discharge: Time for a Statute," *Virginia Law Review*, 62, 481.

Summers, Lawrence H. 1989. "Some Simple Economics of Mandated Benefits." *American Economic Review*, 79(2), May, 177 – 183.

Verkerke, J. Hoult. 1995. "An Empirical Perspective on Indefinite Term Employment Contracts: Resolving the Just Cause Debate," *Wisconsin Law Review*, 837.

Walsh, David J. and Joshua L. Schwarz. 1996. "State Common Law Wrongful Discharge Doctrines: Up-Date, Refinement, and Rationales." *American Business Law Journal*, 33, 645 – 689.

Willborn, Steven L., Stewart J. Schwab and John F. Burton, Jr.. 1998. *Employment Law: Cases and Materials*. Charlottesville, VA: Lexis Law Publishing (2nd. ed.).

Statistical Appendix

Consider a simple bivariate instrumental variables (i.e., Wald) estimator:

(7) $$d_{IV} = \frac{E[Y_s \mid Z_s = 1] - E[Y_s \mid Z_s = 0]}{E[L_s \mid Z_s = 1] - E[L_s \mid Z_s = 0]}$$

where Y_s is the log state employment rate, L_s is an indicator variable equal to one if a state has recognized an unjust dismissal doctrine, and Z_s is an excluded instrumental variable and time subscripts are suppressed. For concreteness, assume that Z_s is a binary variable equal to one if a neighboring state recognizes an unjust dismissal doctrine. The parameter $\hat{d}_{IV}$ is an estimate of the causal effect of adoption of an unjust dismissal doctrine on the log (percentage) change in state employment. Note that this estimate is simply the ratio of the mean difference in employment growth between states that do and do not have a neighbor recognizing an unjust dismissal doctrine divided by the mean difference in actual unjust dismissal law adoption in these same states.

A central requirement for the validity of $\hat{d}_{IV}$ is that there is no other omitted variable, X_s, which is causally responsible for differential state growth rates between adopting and non-adopting states and is also correlated with the instrumental variable Z_s. One such candidate omitted variable is Southern United States geographic location, which as above is correlated with persistent pre-existing trends in employment growth. If this variable is also correlated with Z_s, the dummy equal to one if a neighbor state recognizing a law, it can be shown that:

(8) $$plim\,\hat{d}_{iv} = d_{iv} + b'\left\{\frac{E[X_s \mid Z_s = 1] - E[X_s \mid Z_s = 0]}{E[L_s \mid Z_s = 1] - E[L_s \mid Z_s = 0]}\right\},$$

where b' is the coefficient from the bivariate regression of X_s on Z_s (cf. Angrist and Krueger, 1999). This equation indicates that if X_s is omitted from the instrumental variables estimate, the estimated impact of the common law exceptions on state employment will be biased by conventional omitted variables bias, further *magnified* by the inverse of the difference in law adoption probability between

Southern and non-Southern states. Concretely, because X_s and Z_s are negatively correlated – Southern states adopted fewer employment at will exceptions – b' will be negative. Further, because employment growth was faster in states without adopting neighbors (i.e., Southern states), the numerator of the ratio multiplying b' in (8) is also negative. And finally, because unjust dismissal law adoption was less frequent among states with non-adopting neighbors (again, Southern states), this implies that the bottom half of the ratio is also negative. Therefore, the IV estimates will be biased downwards, potentially leading to a spurious negative estimate of the impact of wrongful discharge law adoption on state employment.

Figure 1. Trends in Employment/Population by Demographic Group, 1978 - 1999 (1978 = 0)

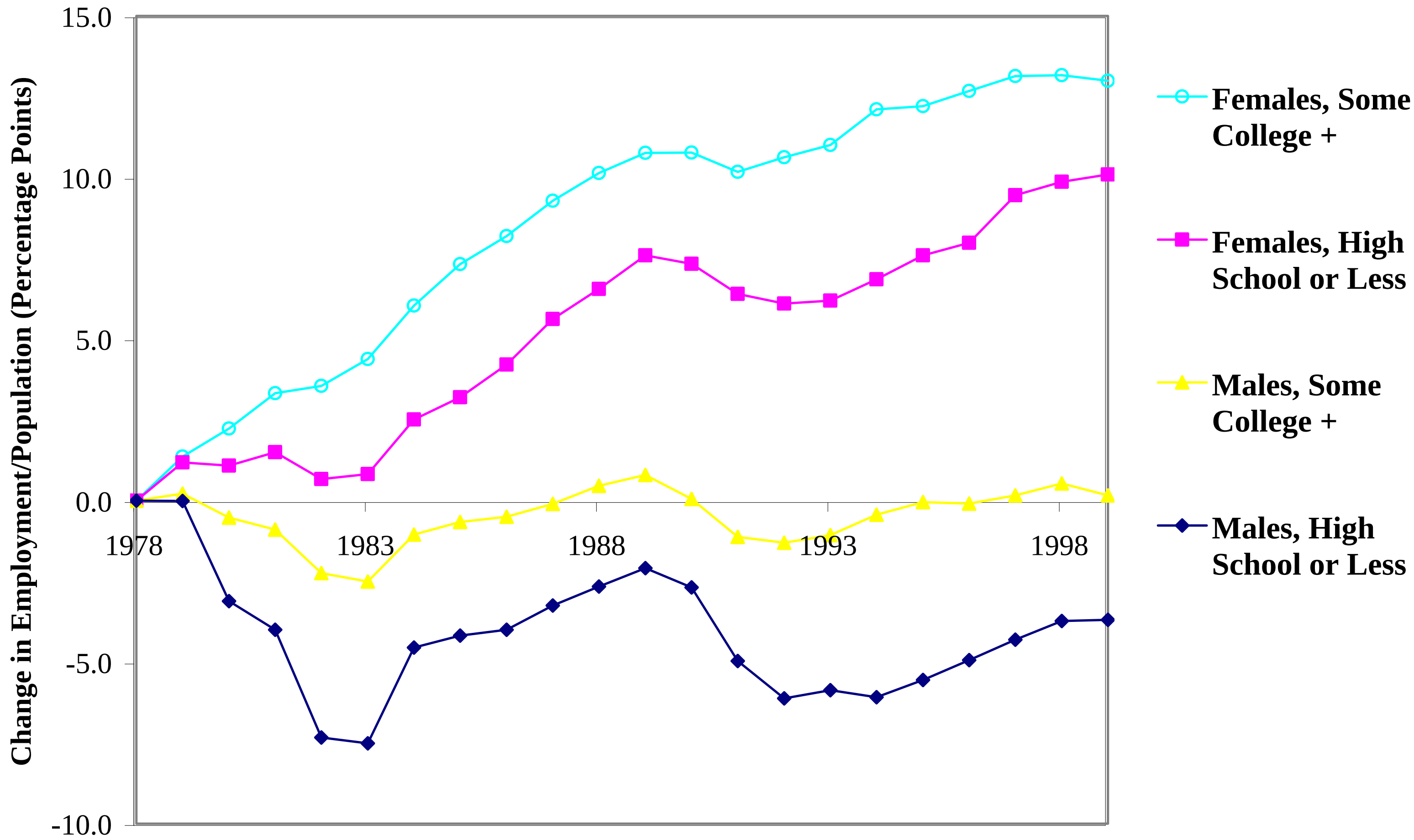


Figure 2: Fraction of States Recognizing Any Unjust Dismissal Doctrine, 1950 - 1998

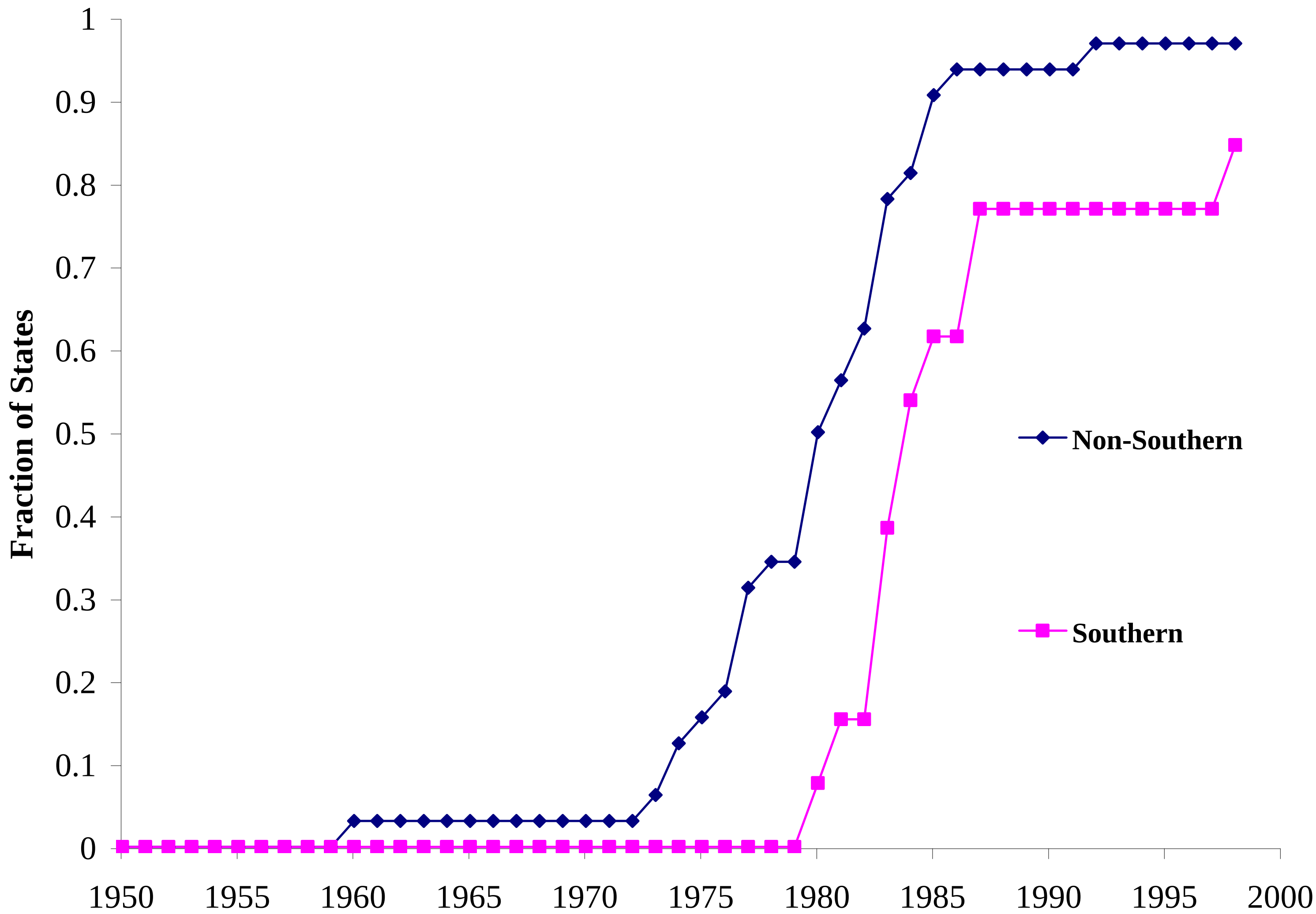
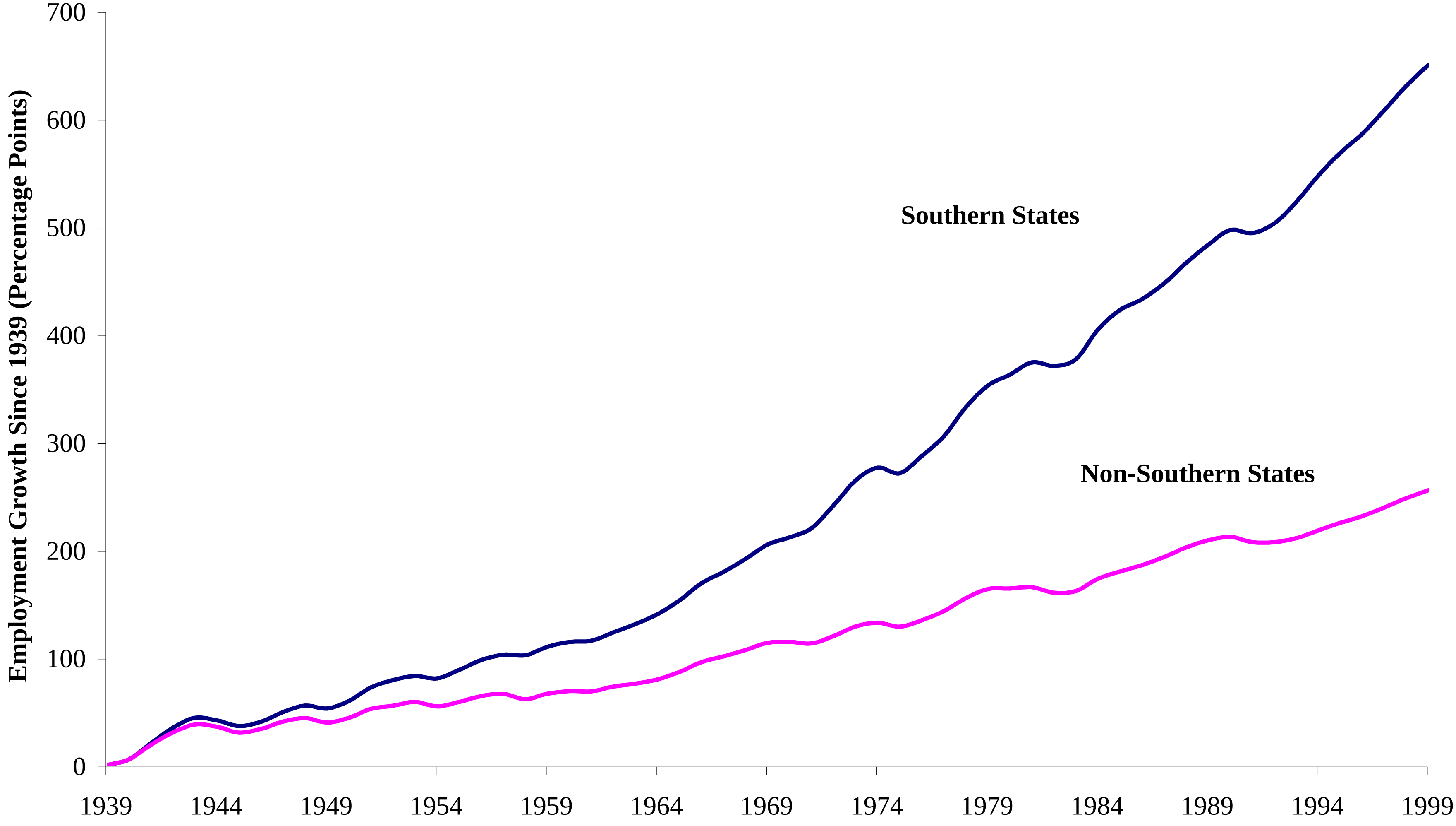


Figure 3: Employment Growth in Southern and Non-Southern States, 1939 - 1999



Number of States Recognizing Exceptions to Employment at Will, 1958 - 1999

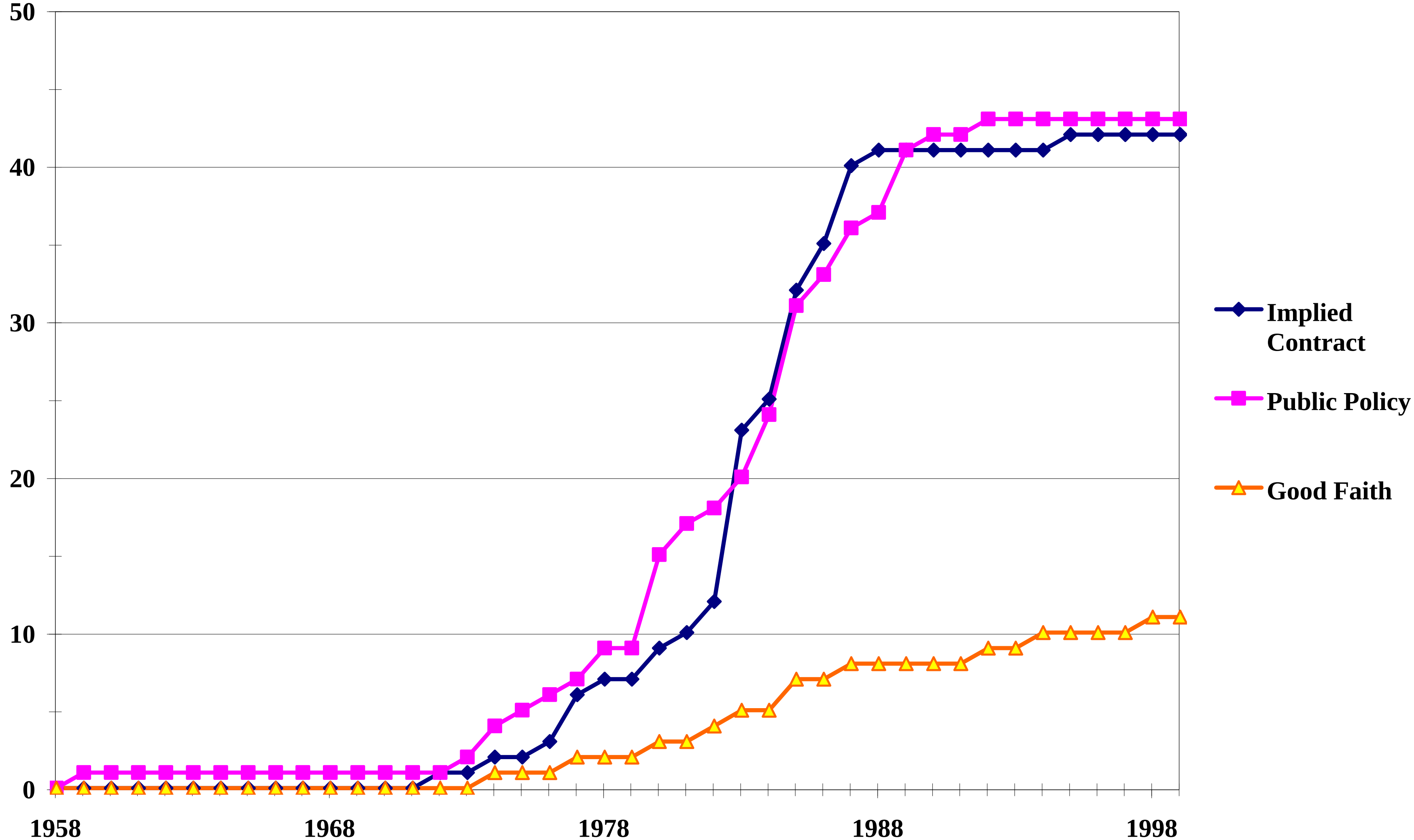


Table 1:
The Estimated Impact of State Common Law Exceptions on State Employment, Unemployment, and Labor-Force Non-Participation, 1978-1999

	Dependent Variable:					
	Employment/Population x 100		Unemployment/Labor Force x 100		Not-In-Labor-Force/Population x 100	
	(1)	(2)	(3)	(4)	(5)	(6)
Independent Variables:						
Implied Contract	-0.41	-0.62	0.36	0.59	0.13	0.17
	(0.20)	(0.22)	(0.17)	(0.23)	(0.13)	(0.11)
Public Policy	0.49	0.18	0.05	0.07	-0.53	-0.25
	(0.19)	(0.22)	(0.17)	(0.20)	(0.12)	(0.12)
Good Faith	-1.20	0.11	0.54	-0.23	0.89	0.10
	(0.33)	(0.27)	(0.23)	(0.32)	(0.29)	(0.19)
State-time and group-time trends included in	No	Yes	No	Yes	No	Yes
R^2	0.92	0.95	0.80	0.82	0.92	0.95

n= 17,600 (50 states x 22 years x 16 demographic groups (male/female x high school or less/some college or more x ages: 16-24, 25-39, 40-54, 55-64). Huber-White robust standard errors allow for clustering of errors across demographic group observations within states by year. State, year, and demographic group main effects included in all models. Models in even numbered columns also contain linear state and demographic group specific time trends. Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by gender/age/education group's share of total population in each year.

Table 2:

The Estimated Impact of Common Law Exceptions on Employment to Population: Testing the Impact of the Number of Doctrines versus the Specific Doctrines

	Dependent Variable: 100 x Employment/Population							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Implied Contract Doctrine	-0.62 (0.22)				-0.86 (0.26)	-0.73 (0.36)	-0.36 (0.27)	-0.58 (0.24)
Public Policy Doctrine	0.18 (0.22)				-0.04 (0.26)	0.07 (0.36)	0.44 (0.27)	
Good Faith Doctrine	0.11 (0.27)				0.09 (0.27)		0.66 (0.47)	
Any Doctrine		-0.02 (0.24)			0.50 (0.30)			
Count of Doctrines			-0.18 (0.15)			0.11 (0.27)		
1 Doctrine				0.01 (0.23)				
2 Doctrines				-0.33 (0.32)			-0.47 (0.31)	-0.07 (0.23)
3 Doctrines				-0.51 (0.45)			-1.09 (0.66)	-0.18 (0.37)
R^2	0.95	0.95	0.95	0.95	0.95	0.95	0.95	0.95

n= 17,600 (50 states x 22 years x 16 demographic groups (male/female x high school or less/some college or more x ages: 16-24, 25-39, 40-54, 55-64). Huber-White robust standard errors allow for clustering of errors across demographic group observations within states by year. State, year, and demographic group main effects and state and demographic linear time trends included in all models. Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by gender/age/education group's share of total population in each year.

Table 3:
The Estimated Impact of the Exceptions to Employment at Will on Employment to Population Rates by Age and Gender Group, 1978-1999

Dependent variable: 100 x Employment/Population Ratio

	Implied Contract	Public Policy	Good Faith	R^2
A. Males				
Ages 18 - 24	-1.29	-0.30	-0.28	0.71
	(0.49)	(0.49)	(0.79)	
Ages 25 - 39	-0.72	0.21	0.37	0.84
	(0.30)	(0.30)	(0.37)	
Ages 40 - 54	-0.52	-0.01	-0.20	0.88
	(0.26)	(0.26)	(0.36)	
Ages 55 - 64	-0.45	0.01	0.71	0.86
	(0.49)	(0.44)	(0.54)	
B. Females				
	Implied Contract	Public Policy	Good Faith	R^2
Ages 18 - 24	-1.00	0.50	0.03	0.86
	(0.42)	(0.43)	(0.62)	
Ages 25 - 39	-0.41	0.22	-0.19	0.93
	(0.23)	(0.23)	(0.39)	
Ages 40 - 54	-0.16	0.19	0.64	0.92
	(0.29)	(0.30)	(0.43)	
Ages 55 - 64	-0.31	0.38	0.04	0.86
	(0.35)	(0.38)	(0.56)	

n =2,200 in each regression (50 states x 22 years x 2 education groups (high school or less/some college or more). Robust standard errors in parentheses account for clustering of errors across education groups within states by year. All models include state, year, and education group main effects, and linear state and demographic group time trends. Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by state share of total U.S. population in gender/age group in each year.

Table 4:
The Estimated Impact of the Implied Contract Exception on Employment to Population Rates by Age, Gender and Education Group, 1978-1999

Dependent variable: 100 x Employment/Population Ratio

	A. Males		B. Females	
	High School or Less	Some College or Greater	High School or Less	Some College or Greater
Ages 18 - 24	-1.88	-0.44	-1.08	-0.84
	(0.65)	(0.51)	(0.53)	(0.53)
R^2	0.79	0.75	0.83	0.79
Ages 25 - 39	-0.95	-0.55	-0.49	-0.48
	(0.45)	(0.25)	(0.37)	(0.27)
R^2	0.81	0.69	0.93	0.90
Ages 40 - 54	-0.68	-0.24	-0.23	-0.31
	(0.34)	(0.28)	(0.39)	(0.37)
R^2	0.81	0.70	0.92	0.88
Ages 55 - 64	-0.67	0.04	-0.75	0.90
	(0.56)	(0.64)	(0.41)	(0.77)
R^2	0.79	0.74	0.81	0.67

n =1,100 in each regression (50 states x 22 years. Robust standard errors in parentheses. All models include state and year main effects, linear state time trends and controls for Public Policy and Good Faith exceptions. Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by state share of total U.S. population in gender/age/education group in each year.

Table 5: Impacts by Race Main

The Estimated Impact of State Common Law Exceptions on Employment by Race and Gender, 1978-1999

	Panel A: All States					
	Dependent Variable: 100 x Employment/Population					
	Males & Females		Males		Females	
	Blacks	Whites	Blacks	Whites	Blacks	Whites
Implied Contract	-0.02	-0.70	0.09	-0.87	-0.14	-0.56
	(0.55)	(0.23)	(0.61)	(0.29)	(0.59)	(0.22)
Public Policy	1.27	0.11	1.17	-0.02	1.30	0.27
	(0.53)	(0.21)	(0.62)	(0.27)	(0.60)	(0.21)
Good Faith	2.81	0.25	3.20	0.32	2.40	0.19
	(0.86)	(0.27)	(1.24)	(0.34)	(0.77)	(0.34)
R^2	0.86	0.95	0.80	0.85	0.85	0.97
n	1,100	1,100	1,100	1,100	1,056	1,056

	Panel B: All States Except California					
	Dependent Variable: 100 x Employment/Population					
	Males & Females		Males		Females	
	Blacks	Whites	Blacks	Whites	Blacks	Whites
Implied Contract	0.08	-0.74	0.30	-0.81	-0.13	-0.72
	(0.55)	(0.23)	(0.61)	(0.30)	(0.60)	(0.21)
Public Policy	1.40	0.07	1.39	0.01	1.35	0.13
	(0.54)	(0.21)	(0.64)	(0.27)	(0.60)	(0.20)
Good Faith	1.04	0.07	0.67	0.00	1.25	0.14
	(1.56)	(0.34)	(2.45)	(0.49)	(1.16)	(0.39)
R^2	0.87	0.95	0.81	0.85	0.86	0.97
n	1,078	1,078	1,078	1,078	1,034	1,034

Robust standard errors in parentheses. All models include state and year main effects, and linear state time trends. Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by state share of total U.S. population in gender/age/education group in each year. States with incomplete race-by-gender observations are dropped from the relevant columns.

Table 6: Impacts by Race by Education and Age

The Estimated Impact of State Common Law Exceptions on State Employment by Race, Age, Education and Gender, 1978-1999

Panel A: Education x Gender x Race

Dependent Variable: 100 x Employment/Population

	High School or Less				Some College or More			
	Males		Females		Males		Females	
	Blacks	Whites	Blacks	Whites	Blacks	Whites	Blacks	Whites
Implied Contract	0.06	-1.24	0.36	-0.71	0.44	-0.39	-0.95	-0.27
	(0.67)	(0.39)	(0.67)	(0.28)	(0.80)	(0.23)	(0.72)	(0.22)
R^2	0.82	0.84	0.79	0.95	0.58	0.74	0.73	0.94
n	990	990	968	968	1,056	1,056	990	990

Panel B: Age x Race

Dependent Variable: 100 x Employment/Population

	Ages 18-24		Ages 25-39		Ages 40-54		Ages 55-64	
	Blacks	Whites	Blacks	Whites	Blacks	Whites	Blacks	Whites
Implied Contract	0.00	-1.16	-1.18	-0.57	1.11	-0.50	2.27	-0.85
	(0.94)	(0.42)	(0.76)	(0.22)	(0.65)	(0.26)	(0.81)	(0.40)
R^2	0.74	0.83	0.81	0.93	0.74	0.94	0.55	0.87
n	968	968	1,078	1,078	946	946	858	858

Robust standard errors in parentheses. All models include state and year main effects, and linear state time trends. Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by state share of total U.S. population in gender/age/education group in each year. States with incomplete race-by-gender observations are dropped from the relevant columns.

Table 7: Lagged Depvar Overall

The Estimated Impact of State Common Law Exceptions on State Employment, Unemployment, and Labor-Force Non-Participation, 1979-1999: Specifications With Lagged Dependent Variable

	Employment/Population x 100			Unemployed/Labor Force x 100			Not in LF/Population x 100		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lagged Dependent Variable	0.85 (0.01)	0.79 (0.01)	0.74 (0.01)	0.70 (0.01)	0.64 (0.01)	0.59 (0.01)	0.86 (0.01)	0.82 (0.01)	0.76 (0.01)
Implied Contract	-0.32 (0.08)	-0.31 (0.11)	-0.35 (0.14)	0.23 (0.07)	0.25 (0.11)	0.31 (0.14)	0.22 (0.06)	0.18 (0.08)	0.17 (0.10)
Public Policy	0.36 (0.08)	0.19 (0.11)	0.10 (0.15)	-0.06 (0.07)	-0.07 (0.11)	0.06 (0.14)	-0.33 (0.06)	-0.12 (0.08)	-0.12 (0.11)
Good Faith	-0.34 (0.09)	-0.66 (0.20)	-0.55 (0.19)	0.12 (0.08)	0.48 (0.15)	0.38 (0.18)	0.29 (0.07)	0.33 (0.15)	0.25 (0.18)
State dummies included in model?	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
State and group trends included in model?	No	No	Yes	No	No	Yes	No	No	Yes
R^2	0.97	0.98	0.98	0.88	0.89	0.89	0.98	0.98	0.98

n= 16,800 (50 states x 21 years x 16 demographic groups (male/female x high school or less/some college or more x ages: 16-24, 25-39, 40-54, 55-64). Huber-White robust standard errors allow for clustering of errors across demographic group observations within states by year. Year and demographic group main effects included in all models. Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by gender/age/education group's share of total population in each year.

Table 8: Lagged Depvar Detailed

The Estimated Impact of the Implied Contract Exception on Employment to Population Rates by Age, Gender and Education Group, 1979-1999: Specifications with Lagged Dependent Variable

Dependent variable: 100 x Employment/Population Ratio

	A. Males		B. Females	
	High School or Less	Some College or Greater	High School or Less	Some College or Greater
Ages 18 - 24	-0.68	-0.16	-0.74	-0.26
	(0.28)	(0.33)	(0.28)	(0.28)
R^2	0.77	0.64	0.78	0.69
Ages 25 - 39	-0.40	-0.37	-0.29	-0.41
	(0.17)	(0.10)	(0.19)	(0.17)
R^2	0.81	0.68	0.91	0.84
Ages 40 - 54	-0.19	-0.12	-0.25	-0.11
	(0.16)	(0.12)	(0.18)	(0.19)
R^2	0.79	0.63	0.90	0.85
Ages 55 - 64	-0.29	-0.10	-0.19	0.43
	(0.25)	(0.32)	(0.25)	(0.34)
R^2	0.73	0.67	0.75	0.63

n =1,050 in each regression (50 states x 21 years. Robust standard errors in parentheses. All models include lagged value of the dependent variable, year dummies, and dummies for Public Policy and Good Faith exceptions (not tabulated). Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by state share of total U.S. population in gender/age/education group in each year.

Table 9: Lagged Dependent Variable by Race and Gender

The Estimated Impact of State Common Law Exceptions on State Employment by Race and Gender, 1979 - 1999: Specifications With Lagged Dependent Variable

	Dependent Variable: Employment/Population x 100							
	Panel A: Males				Panel B: Females			
	Blacks		Whites		Blacks		Whites	
Lagged Dependent Variable	0.84 (0.02)	0.45 (0.03)	0.93 (0.01)	0.71 (0.02)	0.86 (0.02)	0.58 (0.03)	0.97 (0.01)	0.81 (0.02)
Implied Contract	-0.68 (0.27)	-0.54 (0.40)	-0.14 (0.09)	-0.36 (0.15)	-0.48 (0.26)	-0.30 (0.40)	-0.13 (0.09)	-0.20 (0.14)
State dummies included in model?	No	Yes	No	Yes	No	Yes	No	Yes
State and group trends included in model?	No	No	No	No	No	No	No	No
R^2	0.76	0.81	0.89	0.90	0.85	0.87	0.97	0.97

n =1,050 in each regression (50 states x 21 years. Robust standard errors in parentheses. All models include lagged value of the dependent variable, year dummies, and dummies for Public Policy and Good Faith exceptions (not tabulated). Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by state share of total U.S. population in gender/age/education group in each year.

Table 10:
The Estimated Impact of the Implied Contract Exception on State Employment Outcomes, 1978 - 1999:
Specifications Including Leads and Lags to Year of Adoption

	Dependent Variable:					
	Employment/Population x 100		Unemployment/Labor Force x 100		Not-In-Labor-Force/Population x 100	
	(1)	(2)	(3)	(4)	(5)	(6)
Independent Variables:						
3 or 4 Years Prior	0.28	0.26	-0.14	-0.30	-0.19	-0.06
	(0.36)	(0.32)	(0.34)	(0.31)	(0.19)	(0.16)
1 or 2 Years Prior	0.68	0.85	-0.47	-0.94	-0.38	-0.20
	(0.31)	(0.30)	(0.26)	(0.27)	(0.19)	(0.19)
Year-of or Year After	0.16	0.46	0.07	-0.58	-0.25	-0.08
	(0.32)	(0.35)	(0.30)	(0.31)	(0.19)	(0.24)
2 or 3 Years After	-0.51	-0.17	0.56	-0.20	0.08	0.26
	(0.32)	(0.41)	(0.29)	(0.36)	(0.19)	(0.28)
4 or 5 Years After	-0.33	0.10	0.16	-0.72	0.19	0.37
	(0.31)	(0.43)	(0.26)	(0.35)	(0.20)	(0.31)
6 or 7 Years After	-0.13	0.25	-0.01	-0.91	0.09	0.35
	(0.32)	(0.47)	(0.24)	(0.38)	(0.23)	(0.35)
8 or More Years After	-0.20	0.09	-0.07	-0.95	0.24	0.55
	(0.34)	(0.52)	(0.24)	(0.40)	(0.25)	(0.39)
State-time and group-time trends included in model	No	Yes	No	Yes	No	Yes
H_0: Adoption$_{(t0\ -\ t8)} = 0$	0.25	0.12	0.24	0.05	0.05	0.02
R^2	0.92	0.95	0.81	0.83	0.93	0.95

n =17,600 (50 states x 22 years x 16 demographic groups). Huber-White standard errors in parentheses allow for clustering of demographic group errors within states by year. All models include state, year and demographic group fixed effects as well as leads and lags of adoption of the Public Policy and Good Faith exceptions (in addition to the those tabulated for the Implied Contract exception). Even numbered columns also include linear time trends for each state and demographic group. Each of the six dummies for adoption of the Implied Contract exception for time t-4 through t+7, are equal to 1 in two years per adopting state. The seventh dummy, for 8+ years post-adoption of Implied Contract (time t+8 forward), is equal to one in every year beginning with the 8th year after adoption. The labor force outcome variables (employment/population, unemployment/labor force, and not-in-labor-force/population) are multiplied by 100 for readability. Annual state labor force means are calculated for all adult state residents ages 16 - 64 using complete monthly labor force surveys of the Current Population Survey. Estimates are weighted by demographic groups' share of population in relevant y

Table 11:
The Estimated Impact of State Common Law Exceptions on Log Industry Employment by State, 1978 - 1999

Dependent Variable: 100 x Log State Industry Employment

	(1)			(2)		
	IC	PP	GF	IC	PP	GF
Panel A: Single Indicator Variable						
Law Main Effect	-3.77	2.28	8.75	-2.32	-1.45	2.11
	(2.14)	(2.07)	(3.05)	(0.88)	(0.76)	(1.06)
State-time and industry-time trends included in model		No			Yes	
R^2		0.988			0.995	
Panel B: Specifications Including Leads and Lags of Year of Adoption						
	(1)			(2)		
	IC	PP	GF	IC	PP	GF
Independent Variables:						
3 or 4 Years Prior	0.80	-1.74	-1.05	0.09	-2.02	2.39
	(1.97)	(1.29)	(2.90)	(0.88)	(0.73)	(1.35)
1 or 2 Years Prior	1.17	2.69	0.68	0.48	-1.07	2.12
	(2.59)	(2.33)	(3.39)	(1.29)	(1.08)	(1.75)
Year-of or Year After	-1.58	3.59	4.62	-1.29	-1.88	3.62
	(2.85)	(2.84)	(3.29)	(1.54)	(1.25)	(1.69)
2 or 3 Years After	-3.54	2.95	7.22	-2.69	-3.28	3.59
	(3.34)	(3.23)	(3.38)	(1.82)	(1.40)	(1.82)
4 or 5 Years After	-3.75	3.67	7.15	-3.17	-4.60	3.23
	(3.89)	(3.77)	(3.56)	(2.05)	(1.61)	(2.09)
6 or 7 Years After	-4.63	4.15	9.21	-3.16	-4.67	4.53
	(4.29)	(4.07)	(3.76)	(2.26)	(1.84)	(2.36)
8 or More Years After	-5.99	4.00	9.28	-3.24	-5.51	4.64
	(4.85)	(4.76)	(4.88)	(2.46)	(2.06)	(2.41)
State-time and industry-time trends included in model	No	No	No	Yes	Yes	Yes
H_0: $\text{Adoption}_{(t0\ -\ t8)} = 0$	0.31	0.70	0.04	0.37	0.03	0.15
R^2		0.988			0.995	

n=14,146 (50 states * 22 years * 13 industries, dropping state-industry observations where an industry was not present in a state in each year (154 observations)). Huber-White standard errors in parentheses allow for arbitrary forms of correlation among observations within a state-industry blocks. All models include year dummies and state x industry main effects. State x industry employment is calculated for 13 major industries encompassing all sectors using complete CPS monthly files for 1983-1999. Samples include all currently employed adults ages 18 - 64 in each calendar month. Regressions weighted by state x industry's share of national employment in each year.

Table 12:
The Estimated Impact of the Implied Contract Exception on State Labor Force Outcomes, by Sub-Periods of 1978 - 1995

A. Four Sub-Periods, 1978-1995

	States Adopting	Dependent Variable: Employment/Population x 100	Unemployment/Labor Force x 100	Not-In-Labor-Force/Population x 100
1978-1983 *(n=4,800)*	*6*	-0.77 (0.58)	0.75 (0.76)	0.28 (0.13)
R^2		0.95	0.83	0.96
1983-1987 *(n=4,000)*	*23*	-1.17 (0.34)	1.27 (0.30)	0.25 (0.18)
R^2		0.95	0.84	0.96
1987-1991 *(n=4,000)*	*8*	-0.52 (0.42)	0.73 (0.43)	-0.06 (0.18)
R^2		0.95	0.82	0.95
1991-1995 *(n=4,000)*	*1*	0.94 (0.25)	-1.20 (0.33)	-0.20 (0.22)
R^2		0.94	0.86	0.94

B. Two Sub-Periods (Early Adoptions 1978-86 and Late Adoptions 1987-95)

	States Adopting	Dependent Variable: Employment/Population x 100		Unemployment/Labor Force x 100		Not-In-Labor-Force/Population x 100	
		(1)	(2)	(3)	(4)	(5)	(6)
1978-1987 *(n=8,000)*	*29*	-0.71 (0.26)	-1.19 (0.31)	0.53 (0.28)	1.23 (0.32)	0.33 (0.13)	0.33 (0.14)
State & group time trends included in model		No	Yes	No	Yes	No	Yes
R^2		0.95	0.96	0.82	0.84	0.96	0.96
1987-1995 *(n=7,200)*	*9*	-0.37 (0.49)	-0.33 (0.33)	0.38 (0.44)	0.59 (0.30)	0.01 (0.20)	-0.14 (0.23)
State & group time trends included in model		No	Yes	No	Yes	No	Yes
R^2		0.95	0.95	0.82	0.85	0.95	0.95

Huber-White robust standard errors in parentheses account for clustering of demographic groups within states by year. Each coefficient in Panel A is from a separate OLS regression of state employment outcomes on three dummy variables equal to one in the years following adoption of an Implied Contract, Public Policy, or Good Faith exception. Only Implied Contract exception coefficients are tabulated; no Implied Contract exceptions were adopted after 1992. Count of adopting states include only cases where a state experiences a change in the law during the designated interval. Annual state labor force means are calculated for all adult state residents ages 16 - 64 in sixteen demographic groups using complete monthly labor force surveys of the Current Population Survey. The sixteen demographic groups are less-educated/more-educated where less educated is high school degree or lower and more educated is some college or greater. All models include state and year dummies and main effects for each demographic group and are weighted by demographic groups' share of population in relevant year.

Table 13:
Estimated Impact of Common Law Exceptions on Log State Temporary Help Employment, Controlling for Unemployment, Employment Growth, and Unionization.

	Dependent Variable: 100 x Log Temporary Help Employment in State						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Implied Contract	13.12 (4.53)	14.74 (4.01)	14.93 (4.02)	14.96 (4.03)	16.24 (3.93)	14.31 (3.97)	15.87 (3.90)
Public Policy	12.65 (4.41)	-4.28 (4.61)	-2.60 (4.76)	-2.30 (4.63)	-3.82 (4.62)	-4.10 (4.57)	-2.84 (4.69)
Good Faith	7.84 (6.25)	-10.62 (6.97)	-13.16 (6.80)	-13.63 (6.80)	-9.55 (6.99)	-7.93 (6.94)	-8.81 (6.80)
Log State Employment x 100			0.85 (0.33)	1.00			0.44 (0.33)
State Unemployment Rate					-4.53 (0.88)		-4.15 (0.88)
Unionization Rate x 100						1.86 (0.67)	1.49 (0.66)
State specific trends	No	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.97	0.99	0.99	0.96	0.99	0.99	0.99

n =750 (50 states x 15 years). Estimates exclude years 1979 and 1981 due to absence of union data. Huber-White robust standard errors in parentheses. Temporary help employment data is from County Business Patterns. State fraction unionized is calucatleed for eyars 1983 - 1995 from Current Population Survey merged outgoing rotation group (MORG) files, and from Troy and Shefflin *U.S. Union Sourcebook* (1985) for years 1980 and 1982. Source for this table: Autor, D. H. "Outsourcing at Will: The Contribution of Unjust Dismissal Doctrines to the Growth of Temporary Help Employment." forthcoming. *Journal of Labor Economics.*

Table 14:

The Estimated Impact of the Exceptions to Employment at Will on Employment to Population Rates: Controlling for Unionization by State, 1983 - 1999

	Dependent Variable: 100 x Employment/Population Ratio			
	(1)	(2)	(3)	(4)
Percent Unionized	-0.31	0.00	0.01	-0.07
	(0.06)	(0.05)	(0.05)	(0.06)
Implied Contract			-0.59	-2.38
			(0.25)	(0.57)
Public Policy			-0.13	-0.02
			(0.26)	(0.57)
Good Faith			-0.65	-0.49
			(0.43)	(1.15)
IC x Percent Unionized				0.12
				(0.03)
PP x Percent Unionized				0.00
				(0.03)
GF x Percent Unionized				-0.01
				(0.09)
State and Group Trends?	No	Yes	Yes	Yes
R^2	0.93	0.95	0.95	0.95
Total IC Impact				-0.36

n =13,600 (50 states x 17 years x 16 demographic groups). Robust standard errors in parentheses accounting for clustering of demographic groups within state by year. All models include state, year, and demographic group main effects are are weighted by demographic group's share of total U.S. population in each year. Samples are calculated from complete monthly files of the Curent Population Survey for 1983 - 1999. Unionization rate is calculated from the Merged Outgoing Rotation Groups of the CPS for the same years. Total IC impact is calculated as the sum of the IC coefficient and the product of the IC x percent unionized coefficient with the weighted mean unionization level in states and years where an IC exception was present.

Table 15:
The Estimated Impact of the Implied Contract Exception on Employment to Population Rates by Gender, Education and Age: 1983-1999

Dependent Variable: 100 x Employment/Population Ratio

	A. Males							
	Ages 18-24		Ages 25-39		Ages 40-54		Ages 55-64	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Unionized	-0.17	-0.25	-0.01	-0.18	0.08	-0.03	-0.08	-0.24
	(0.12)	(0.15)	(0.06)	(0.07)	(0.06)	(0.07)	(0.13)	(0.15)
Implied Contract	-1.15	-3.98	-0.88	-3.34	-0.35	-2.34	-1.35	-4.44
	(0.54)	(1.25)	(0.32)	(0.70)	(0.33)	(0.76)	(0.60)	(1.46)
IC x Frac Unionized		0.18		0.16		0.13		0.20
		(0.07)		(0.04)		(0.04)		(0.08)
Total IC Impact		-1.70		-1.29		-0.75		-2.03
R^2	0.71	0.71	0.87	0.87	0.89	0.90	0.85	0.85

	A. Females							
	Ages 18-24		Ages 25-39		Ages 40-54		Ages 55-64	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Unionized	-0.03	-0.06	0.01	-0.01	0.16	0.11	-0.11	-0.15
	(0.12)	(0.14)	(0.07)	(0.08)	(0.08)	(0.10)	(0.14)	(0.15)
Implied Contract	-0.72	-2.50	-0.28	-1.34	-0.13	-1.32	0.03	-1.04
	(0.51)	(1.27)	(0.28)	(0.57)	(0.37)	(0.79)	(0.47)	(1.04)
IC x Frac Unionized		0.11		0.07		0.08		0.06
		(0.07)		(0.04)		(0.04)		(0.06)
Total IC Impact		-1.09		-0.50		-0.37		-0.25
R^2	0.87	0.87	0.92	0.92	0.92	0.92	0.87	0.87

n=1,700 in each regression (50 states x 17 years x 2 education groups). Robust standard errors in parentheses account for clustering of education groups within states by year. All models include state, year, and age group main effects, and state and education group linear time trends and are weighted by demographic group's share of total U.S. population in each year. Samples are calculated from complete CPS monthly files for years 1983-1999. Total IC impact is calculated as the sum of the IC coefficient and the product of the IC x percent unionized coefficient with the weighted mean unionization level in states and years where an IC exception was present.

Table 16:
The Estimated Impact of the Employment at Will Exceptions on Log Hourly Wages of Employed Workers: 1979 - 1999

Dependent variable:100 x Mean Log Hourly Wages of Employed Workers		
	(1)	(2)
Independent Variables:		
Implied Contract	0.74 (0.51)	-0.32 (0.49)
Public Policy	-1.63 (0.56)	-1.08 (0.46)
Good Faith	-0.72 (0.75)	2.69 (0.75)
State-time and group-time trends included in model	No	Yes
R^2	0.97	0.98

n =16,800 (50 states x 21 years x 16 demographic groups). Huber-White robust standard errors in parentheses account for clustering of errors among demographic states within states by year. Annual state log hourly wage means are calculated for all employed wage/salary workers ages 16 - 64 using combined outgoing rotation groups of the Current Population Survey for 1979 - 1999. All models include state and year dummies and main effects for each demographic group and are weighted by each demographic groups' share of population in the relevant year. Models in column (2) also contains linear state and demographic group specific time trends.

Table 17:

The Estimated Impact of the Employment at Will Exceptions on Log Hourly Wages of Employed Workers, by Sub-Periods, 1979 - 1999

		Dependent variable: 100 x Mean Log Hourly Wages of Employed Workers						
		Implied Contract		**Public Policy**		**Good Faith**		
	Time Trends	**States Adopting**	**Point Estimate**	**States Adopting**	**Point Estimate**	**States Adopting**	**Point Estimate**	**R^2**
				A. 1979 - 1999: 10 Year Sub-Intervals				
1979 - 1989 *(n=8,800)*	No	*36*	0.02 (0.58)	*31*	-0.79 (0.60)	*8*	0.77 (0.83)	0.97
1979 - 1989 *(n=8,800)*	Yes	*36*	-0.53 (0.40)	*31*	-0.82 (0.44)	*8*	0.48 (0.47)	0.98
1989 - 1999 *(n=8,800)*	No	*1*	3.95 (0.82)	*5*	0.36 (0.48)	*5*	0.62 (0.56)	0.97
1989 - 1999 *(n=8,800)*	Yes	*1*	-0.45 (0.90)	*5*	-1.06 (0.41)	*5*	-0.37 (0.60)	0.97
				B. 1979 - 1999: 4 Year Sub-Intervals				
1979 - 1983 *(n=4,000)*	No	*5*	-1.39 (0.93)	*9*	0.77 (0.40)	*4*	1.44 (0.41)	0.92
1983 - 1987 *(n=4,000)*	No	*23*	-0.22 (0.62)	*18*	-0.85 (0.47)	*3*	-2.93 (0.87)	0.97
1987 - 1991 *(n=4,000)*	No	*8*	0.51 (0.44)	*8*	-0.59 (0.36)	*3*	0.99 (0.89)	0.98
1991 - 1995 *(n=4,000)*	No	*1*	2.30 (0.84)	*1*	-2.35 (1.33)	*2*	0.38 (0.90)	0.97
1995 - 1999 *(n=4,000)*	No	*0*	.	*0*	.	*1*	0.59 (0.82)	0.97

Huber-White robust standard errors in parentheses account for clustering of errors among demographic states within state by year. Each row is from a separate OLS regression of state mean log hourly wages of all employed workers on dummy variables equal to one in the years following adoption of an Implied Contract, Public Policy, or Good Faith exception. No Implied Contract or Public Policy exceptions were adopted during 1995 - 1999. Count of adopting states include only cases where a state experienced a change in the law during the designated interval. Annual state log hourly wage means are calculated for all employed wage/salary workers ages 16 - 64 in sixteen demographic groups using merged outgoing rotation groups of the Current Population Survey for 1979 - 1999. All models include state and year dummies and main effects for each demographic group and are weighted by each demographic groups' share of population in the relevant year. Trend models also include linear time trends for each state and demographic group.

Table 18:

Replication of Dertouzas & Karoly, 1993: The Estimated Impact of Wrongful Discharge Laws on Log State Employment, 1980 – 1987.

	Dependent Variable: 100 x Log State Employment							
	Replication of D-K's Instrumental Variables Estimates				**Replication of D-K using a Difference-in-Difference Estimator**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Implied Contract or Good Faith (Contract) Doctrine	-4.44 (1.26)	-1.45 (0.94)	-14.53 (2.39)	-1.13 (1.70)	0.97 (0.44)	-0.75 (0.36)	-0.42 (0.91)	-1.46 (0.67)
Broad Public Policy or Good Faith (Tort) Doctrine	-3.00 (1.50)	-1.26 (1.58)	3.48 (2.91)	-5.83 (2.86)	-1.13 (0.60)	0.46 (0.58)	-0.15 (1.22)	0.00 (1.05)
Narrow Public Policy Doctrine	0.03 (0.94)	-0.55 (0.96)	-5.19 (1.82)	0.05 (1.73)	0.16 (0.40)	0.43 (0.32)	0.27 (0.74)	-0.73 (0.56)
100 x Log(Gross State Product)	0.73 (0.02)	0.77 (0.03)			0.76 (0.02)	0.77 (0.03)		
100 x Δ Log(Gross State Product)	-0.42 (0.04)	-0.40 (0.03)			-0.42 (0.04)	-0.40 (0.03)		
State-Specific Time Trends included in model	No	Yes	No	Yes	No	Yes	No	Yes
F-Test of significance of state-specific time trends		0.00		0.00		0.00		0.00
R^2	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

n =400 (50 states x 8 years). Column (1) tabulates authors' (imperfect) replication of Dertouzas and Karoly (1993), Table 8-4, column (2), an instrumental variables estimate of the impact of exceptions to employment at will on log state employment. Columns (2) - (4) probe the sensitivity of the D-K findings to minor specification checks. Columns (5) - (8) estimate these models using a simple difference-in-difference estimator. Consistent with D-K, estimates are unweighted, and OLS standard errors that are not adjusted for instrumentation of employment at will exceptions are given. Instruments include: whether a state has a right to work statute, whether a state has a republican governor, the percentage change in lawyers per capita, the percentage of neighboring states recognizing each doctrine and the square of this measure, the percentage unionized, the change in percentage unionized, and the change in the percentage unemployment.

Table 19:
Replication of Thomas Miles (2000),
Common Law Exceptions to Employment at Will and U.S. Labor Markets

	Dependent Variable: 100 x Log state Non-Farm Employment							
	Legal Variables from Miles (2000)				**Autor, Donohue, Schwab Legal Variables**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy for Any Exception	-1.74	0.20			-2.19	-0.72		
	(1.71)	(1.04)			(1.45)	(0.82)		
Implied Contract			-0.81	-0.81			-1.12	-1.12
			(1.97)	(0.78)			(1.23)	(0.57)
Dummy for IC Change One Period Later			-0.19	-0.19			-0.33	-0.33
			(0.70)	(0.82)			(0.47)	(0.85)
Public Policy			-1.72	-1.72			-2.35	-2.35
			(1.21)	(0.52)			(1.11)	(0.55)
Dummy for PP Change One Period Later			-1.28	-1.28			-1.70	-1.70
			(0.55)	(0.62)			(0.52)	(0.63)
Good Faith			5.02	5.02			0.79	0.79
			(0.89)	(0.95)			(1.33)	(0.93)
Dummy for GF Change One Period Later			0.73	0.73			0.36	0.36
			(1.85)	(1.49)			(1.92)	(1.54)
Time Trends Included?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Errors Clustered by	Region	Region	Region	Region x Year	Region	Region	Region	Region x Year
R^2	0.995	0.999	0.999	0.999	0.995	0.999	0.999	0.999

n=1500 (50 states x 30 years). Huber-White standard errors in parentheses allow for correlation within nine Census geographic regions for columns 1 - 3 and 5 - 7, and within Census geographic regions by year for columns 4 and 8. State and year fixed effects are included in all models. In columns 3, 4, 7, and 8, law adoption dummies are lagged by one year. Also included are dummies for law change for each law in current year (not displayed). The following controls are also included in all models: fraction age 20 - 44, fraction age 45 - 64, fraction with high school degree, fraction with 1 - 3 years of college, fraction with 4 or more years of college, fraction of workers that are union members, fraction of employed that are black, and fraction of neighboring states recognizing each exception. Demographics and state employment are from Miles (2000).

Table 20:
Robustness Tests of the Impact of State Common Law Exceptions on Employment to Population:
Using Legal Classifications of Dertouzos and Karoly, Morriss, and Walsh and Schwarz

Dependent Variable: 100 x Employment/Population

	Comparison with Morriss, Years: 1978 - 1989				**Comparison with D&K, Years: 1980 - 1987**				**Comparison with Walsh & Schwarz, Years: 1978 - 1994**					
Classification	*ADS*		*Morriss, 1995*		*ADS*		*D&K, 1992*		*ADS*		*W&S*		*W&S Corrected*	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(5)	(6)
Implied Contract	-0.60	-0.71	-0.57	-0.76	-0.70	-0.79	0.09	-0.33	-0.38	-0.87	0.30	0.12	-0.11	-0.70
	(0.20)	(0.25)	(0.21)	(0.26)	(0.24)	(0.28)	(0.20)	(0.25)	(0.20)	(0.24)	(0.19)	(0.22)	(0.20)	(0.25)
Public Policy	-0.18	-0.22	-0.20	-0.23	-0.27	-0.35	-0.26	-0.16	0.36	-0.14	0.36	-0.13	0.38	-0.10
	(0.21)	(0.21)	(0.20)	(0.20)	(0.26)	(0.25)	(0.29)	(0.28)	(0.20)	(0.21)	(0.21)	(0.22)	(0.20)	(0.20)
Good Faith	-0.86	-0.56	-0.66	-0.31	-0.97	-0.45	-0.50	-0.31	-1.09	0.31	-0.64	0.49	-0.70	0.64
	(0.23)	(0.26)	(0.24)	(0.24)	(0.24)	(0.24)	(0.25)	(0.22)	(0.30)	(0.28)	(0.37)	(0.32)	(0.31)	(0.28)
State-time and group-time trends included?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.94	0.95	0.94	0.95	0.95	0.96	0.95	0.96	0.93	0.95	0.93	0.95	0.93	0.95
n	9,600				8,000				13,600					

16 demographic groups used in each state-year (male/female x high school or less/some college or more x ages: 16-24, 25-39, 40-54, 55-64). Huber-White robust standard errors allow for clustering of errors across demographic group observations within states by year. Controls for Public Policy and Good Faith exceptions as well as state, year, and demographic group main effects included in all models. Models in even numbered columns also contain linear state and demographic group specific time trends. Samples are calculated from complete CPS monthly files for years 1978 - 1999. All regressions are weighted by gender/age/education group's share of total population in each year.

Appendix Table 1:
Wrongful Discharge Laws by Region, State and Year

Key: C Implied Contract P Public Policy G Good Faith

	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999
New England																						
Connecticut			P G	P G	P G	P G	P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G
Maine	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C
Massachusetts	G	G	P G	P G	P G	P G	P G	P G	P G	P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G
New Hampshire	P G	P G	P	P	P	P	P	P	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Rhode Island																						
Vermont								C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Middle Atlantic																						
New Jersey			P	P	P	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
New York					C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C
Pennsylvania	P	P	P	P	P	P	P	P	P	P	P	P	P	P	P	P	P	P	P	P	P	P
East North Central																						
Illinois	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Indiana	P	P	P	P	P	P	P	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Michigan	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Ohio					C	C	C	C	C	C	C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Wisconsin			P	P	P	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
West North Central																						
Iowa								P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Kansas				P	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Minnesota						C	C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Missouri						C	C	C	C P	C P	P	P	P	P	P	P	P	P	P	P	P	P
Nebraska						C	C	C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
North Dakota							C	C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
South Dakota						C	C	C	C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
South Atlantic																						
Delaware															P G	P G	P G	P G	P G	P G	P G	P G
Florida																						
Georgia																						
Maryland				P	P	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
North Carolina								P	P	P	P	P	P	P	P	P	P	P	P	P	P	P
South Carolina								P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Virginia						C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
West Virginia	P	P	P	P	P	P	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P

Appendix Table 1:
Wrongful Discharge Laws by Region, State and Year

Key: C Implied Contract P Public Policy G Good Faith

	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999
East South Central																						
Alabama										C	C	C	C	C	C	C	C	C	C	C	C	C
Kentucky						C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Mississippi										P	P	P	P	P	P	P	P	C P	C P	C P	C P	C P
Tennessee				C	C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
West South Central																						
Arkansas			P	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Louisiana																					G	G
Oklahoma	C	C	C	C	C	C	C	C G	C G	C G	C G	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Texas							P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Mountain																						
Arizona						C	C	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G
Colorado						C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Idaho	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G
Montana			P	P	P G	P G	P G	P G	P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G
Nevada						C	C P	C P	C P	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G
New Mexico			C	C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Utah									C	C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Wyoming								C	C	C	C	C P	C P	C P	C P	C P	C P G	C P G	C P G	C P G	C P G	C P G
Pacific																						
Alaska						C G	C G	C G	C G	C G	C G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G
California	C P	C P	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G	C P G
Hawaii					P	P	P	P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Oregon	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
Washington	C	C	C	C	C	C	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P

Legal Appendix:
Cases Cited and Comparison with Dertouzos and Karoly ('Rand') Study

State	Public Policy	Implied Contract	Good Faith
Alabama		Hoffman-LaRoche v. Campbell, 512 So. 2d 725 (Ala. 1987).	
Alaska	Knight v. American Guard & Alert, 714 P.2d 788 (Alaska 1986).	Eales v. Tanana Valley Medical Surgical Group, 663 P.2d 958 (Alaska 1983).	Mitford v. Lasala, 666 P.2d 1000 (Alaska 1983).
Arizona	Wagenseller v. Scottsdale Memorial Hosp., 710 P.2d 1025 (Ariz. 1985).	Leikvold v. Valley View Community Hosp., 688 P.2d 201 (Ariz. App. 1983), vacated, 688 P.2d 170 (Ariz. 1984). **RAND uses the 1984 version of this case.**	Wagenseller v. Scottsdale Memorial Hosp., 710 P.2d 1025 (Ariz. 1985).
Arkansas	MBM Co. v. Counce, 596 S.W.2d 681 (Ark. 1980).	Jackson v. Kinark Corp., 669 S.W.2d 898 (Ark. 1984). **RAND uses Griffen v. Erickson, 642 S.W.2d 308 (Ark. 1982).**	
California	Petermann v. Int'l Brotherhood of Teamsters, 344 P.2d 25 (Cal. Ct. App. 1959).	Drzewiecki v. H&R Block, 101 Cal. Rptr. 169 (Cal. Ct. App. 1972).	Cleary v. American Airlines, 168 Cal. Rptr. 722 (Cal. Ct. App. 1980) (Tort Damages), modified to remove tort damages by Foley v. Interactive Data Corp., 765 P.2d 373 (Cal. 1988).
Colorado	Winther v. DEC Int'l Inc., 625 F. Supp. 100 (D. Colo. 1985). **RAND uses Cronk v. Inter-Mountain Rural Electric Ass'n., 765 P.2d 619 (Colo. Ct. App. 1988)**.	Brooks v. TWA, 574 F. Supp. 805 (D. Colo. 1983). **RAND uses Salimi v. Farmers Ins. Group, 684 P.2d 619 (Colo. Ct. App. 1988)**.	
Connecticut	Sheets v. Teddy's Frosted Foods, 427 A.2d 385 (Conn. 1980).	Finley v. Aetna Life, 499 A.2d 64 (Conn. Ct. App. 1985), reversed on other grounds, 520 A.2d 208 (Conn. 1987) (upholding implied contract exception) **RAND uses Magnan v. Anaconda Indus., 479 A.2d 781 (Conn. 1984)**.	Magnan v. Anaconda Indus., 479 A.2d 781 (Conn. Super. Ct. 1980), reversed on other grounds, 479 A.2d 781 (Conn. 1984). **RAND uses Cook v. Alexander & Alexander, 588 A.2d 1295 (Conn. Ct. App. 1985)**.
Delaware	Henze v. Alloy Surfaces 1992 WL 51861 (Del. 1992). **RAND uses no case**.		Merril v. Crothall-American, 606 A.2d 96 (Del. Sup. Ct. 1992). **RAND uses no case**.
Florida			
Georgia			

Hawaii	Parnar v. Americana hotels, 652 P.2d 625 (Haw. 1982).	Kinoshita v. Canadian Pacific Airlines, 724 P.2d 100 (Haw. 1986).	
Idaho	Jackson v. Minidoka Irrigation District, 563 P.2d 54 (Idaho 1977).	Jackson v. Minidoka, 563 P.2d 54 (Idaho 1977).	Metcalf v. Intermountain Gas. Co., 778 P.2d 744 (Idaho 1989).
Illinois	Kelsay v. Motorola, 384 N.E.2d 353 (Ill. 1978).	Carter v. Kaskaskia Community Action Agency, 322 N.E.2d 574 (Ill. App. Ct. 1974). **RAND uses Duldulao v. St. Mary of Nazareth Hosp. Center, 505 N.E.2d 314 (Ill. 1987)**.	
Indiana	Frampton v. Central Indiana Gas Co, 297 N.E.2d 425 (Ind. 1973).	Romak v. Public Service Co., 511 N.E.2d 1024 (Ind. 1987).	
Iowa	Northrup v. Farmland Ind., 372 N.W.2d 193 (Iowa 1985).	Young v. Cedar County Work Activity Center, 418 N.W.2d 844 (Iowa 1987).	
Kansas	Murphy v. City of Topeka, 630 P.2d 186 (Kan. Ct. App. 1981).	Allegri v. Providence-St. Margaret Health Center, 684 P.2d 1031 (Kan. Ct. App. 1984).	
Kentucky	Firestone Textile Co. v. Meadows, 666 S.W.2d 730 (Ky. 1983).	Shah v. American Synthetic Rubber Co., 655 S.W.2d 489 (Ky. 1983). **RAND has no case**.	
Louisiana			Barbe v. A.A. Harmon & Co., 719 So. 2d 462 (La. 1998). **RAND has no case**.
Maine		Terrio v. Millinocket Community Hospital, 379 A.2d 135 (Me. 1977). **RAND uses Larrabee v. Penobscot Frozen Foods, 486 A.2d 97 (1984)**.	
Maryland	Adler v. American Standard Corp., 432 A.2d 464 (Md. 1981).	Staggs v. Blue Cross, 486 A.2d 798 (Md. Ct. Spec. App.), cert. denied, 493 A.2d 349 (Md. 1985).	
Massachusetts	McKinney v. National Dairy Council, 491 F. Supp. 1108 (D. Mass. 1980). **RAND uses Cort v. Bristol Meyers Co. 431 N.E.2d 908 (Mass. 1982)**.	Hobson v. McLean Hospital Corp., 522 N.E.2d 975 (Mass. 1988). **RAND uses Jackson v. Action for Boston Comm. Dev., 525 N.E.2d 411 (Mass. 1988)**.	Fortune v. National Cash Register Co., 364 N.E.2d 1251 (Mass. 1977).
Michigan	Sventko v. Kroger, 245 N.W.2d 151 (Mich. 1976).	Toussaint v. Blue Cross, 292 N.W.2d 880 (Mich. 1980).	
Minnesota	Phipps v. Clark Oil & Refining Co., 396 N.W.2d 588 (Minn. Ct. App. 1986), aff'd 408 N.W.2d 569 (Minn. 1987).	Pine River State Bank v. Mettille, 333 N.W.2d 622 (Minn. 1983).	
Mississippi	Laws v. Aetna Finance Co., 667 F. Supp. 342 (N.D. Miss. 1987). **RAND use no case**.	Bobbitt v. The Orchard, Ltd., 603 So. 2d 356 (Miss. 1992). **RAND uses no case.**	

Missouri	Boyle v. Vista Eyewear, 700 S.W.2d 859 (Mo. Ct. App. 1985).	Arie v. Intertherm, 648 S.W.2d 142 (Mo. Ct. App. 1983). Exception overturned by Johnson v. McDonnell Douglas Corp., 745 S.W.2d 661 (Mo. Sup. Ct. 1988). **RAND includes Arie but not Johnson**.	
Montana	Keneally v. Sterling Orgain, 606 P.2d 127 (Mont. 1980).	Montana Wrongful Discharge from Employment Act, Mont. Code Ann. 39-2-901 to 914 (1987).	Gates v. Life of Montana Insurance Co., 638 P.2d 1063 (Mont. 1982). (Tort damages)
Nebraska	Ambroz v. Cornhusker Square, 416 N.W.2d 510 (Neb. 1987).	Morris v. Lutheran Medical Center, 340 N.W.2d 388 (Neb. 1983).	
Nevada	Hansen v. Harrah's, 675 P.2d 394 (Nev. 1984).	Southwest Gas Corp. v. Ahmad, 668 P.2d 261 (Nev. 1983).	K-Mart Corp. v. Ponsock, 732 P.2d 1364 (Nev. 1987). (Tort damages)
New Hampshire	Monge v. Beebe Rubber Co., 316 A.2d 549 (N.H. 1974) (only contract damages); Cloutier v. A&P, 436 A.2d 1140 (New Hamp. 1981) (allows tort damages).	Panto v. Moore Business Forms, 547 A.2d 260 (N.H. 1988). **RAND uses no case.**	*Monge v. Beebe Rubber Co., 316 A.2d 549 (N.H. 1974). Eliminated as separate cause of action in Howard v. Dorr Woolen Co., 414 A.2d 1273 (N.H. 1980).* **RAND uses Monge but not Howard**.
New Jersey	Pierce v. Ortho Pharm. Corp., 417 A.2d 505 (N.J. 1980).	Woolley v. Hoffmann-LaRoche, Inc., 491 A.2d 1257 (N.J. 1985).	
New Mexico	Vigil v. Arzola, 699 P.2d 613 (N.M. Ct. App. 1983), reversed on other grounds, 687 P.2d 1038 (N.M. 1984).	Forrester v. Parker, 606 P.2d 191 (N.M. 1980).	
New York	None. **RAND uses Chin v. AT&T, 96 Misc. 2d 1070, 410 N.Y.S.2d 737 (1978) (contract damages only), until the public policy exception was clearly rejected by NY's highest court in Murphy v. American Home Products Corp., 448 N.E.2d 86 (N.Y. 1983).**	Weiner v. McGraw-Hill, Inc., 443 N.E.2d 441(N.Y. 1982).	
North Carolina	Sides v. Duke Univ., 328 S.E.2d 818 (N.C. Ct. App. 1984).		
North Dakota	Krein v. Marian Manor Nursing Home, 415 N.W.2d 793 (N.D. 1987).	Hammond v. North Dakota State Personnel Bd., 345 N.W.2d 359 (N.D. 1984). **RAND uses Bailey v. Perkins Restaurants, Inc., 398 N.W.2d 120 (N.D. 1986).**	

Ohio	*Adopted, Goodspeed v. Airborne Express, Inc., 121 L.R.R.M. (BNA) 3216 (Ohio Ct. App. 1985); rejected, Phung v. Waste Management Inc., 491 N.E.2d 1114 (Ohio 1986);* adopted, Greely v. Miami Valley Maintenance Contractors, Inc., 551 N.E.2d 981 (Ohio 1990). **RAND does not include Greely**.	West v. Roadway Express, In.c, 115 L.R.R.M. (BNA) 4553 (Ohio Ct. App. 1982).	
Oklahoma	Burk v. K-Mart Corp., 770 P.2d 24 (Okla. 1989).	Langdon v. Saga Corp., 569 P.2d 524 (Okla. Ct. App. 1976). **RAND uses Hinson v. Cameron, 742 P.2d 24 (Okla. 1987)**.	adopted, Hall v. Farmers Insurance Exchange, 713 P.2d 1027 (Okla. 1985); *rejected, Burk v. K-Mart Corp., 770 P.2d 24 (Okla. 1989)*. **RAND gives no case here.**
Oregon	Nees v. Hocks, 536 P.2d 512 (1975).	Yartzoff v. Democrat-Herald Publ. Co., 576 P.2d 356 (Ore. 1978).	
Pennsylvania	Geary v. United States Steel Corp., 319 A.2d 174 (Pa. 1974). **RAND uses Reuther v. Fowler & Williams, Inc., 386 A.2d 119 (Pa. Super. Ct. 1978)**.		
Rhode Island	None. **RAND uses Volino v. General Dynamics, 539 A.2d 531 (R.I. 1988).**		
South Carolina	Ludwick v. This Minute of Carolina, Inc., 337 S.E.2d 213 (S.C. 1985).	Small v. Springs Industries, Inc., 357 S.E.2d 452 (S.C. 1987).	
South Dakota	Johnson v. Kreiser's Inc., 433 N.W.2d 225 (S.D. 1988) (contract damages). **RAND uses Trombollo v. Dunn, 342 N.W.2d 23 (1984)**.	Osterkamp v. Alkota Mfg, Inc., 332 N.W.2d 275 (S.D. 1983)	
Tennessee	Clanton v. Clain-Sloan Co., 677 S.W.2d 441 (Tenn. 1984).	Hamby v. Genesco Inc., 627 S.W.2d 373 (Tenn. Ct. App. 1981). **RAND cites no case.**	
Texas	Sabine Pilot Serv. Inc. v. Hauck, *672 S.W.2d 322 (Tex. Civ. App. 1984), affirmed,* 687 S.W.2d 733 (Tex. 1985).	Johnson v. Ford Motor Co., 690 S.W.2d 90 (Tex. Civ. App. 1985). **RAND cites no case.**	
Utah	Berube v. Fashion Centre, 771 P.2d 1033 (Utah 1989).	Rose v. Allied Development, 719 P.2d 83 (Utah 1986). **RAND cites no case**.	
Vermont	Payne v. Rozendaal, 520 A.2d 586 (Vt. 1986). **RAND uses Jones v. Keough, 409 A.2d 581 (Vt. 1979)**.	Sherman v. Rutland Hospital, Inc. 500 A.2d 230 (Vt. *1985*). **RAND uses Benoir v. Ethan Allen, Inc., 514 A.2d 716 (Vt. 1986)**.	
Virginia	Bowman v. State Bank of Keysville, 31 S.E.2d 797 (Va. 1985).	Frazier v. Colonial Williamsburg Foundation, 574 F. Supp. 318 (E.D. Va. 1983). **RAND cites no case**.	

Washington	Thompson v. St. Regis Paper Co., 685 P.2d 1081 (Wash. 1984). **RAND uses Roberts v. Atlantic Richfield Co., 568 P.2d 764 (Wash. 1977)**.	Roberts v. Atlantic Richfield Co., 568 P.2d 764 (Wash. 1977).	
West Virginia	Harless v. First National Bank, 246 S.E.2d 270 (W. Va. 1978).	Cook v. Heck's Inc., 342 S.E.2d 453 (W. Va. 1986).	
Wisconsin	Ward v. Frito-Lay, Inc., 290 N.W.2d 536 (Wis. Ct. App. 1980) (Contract damages). **RAND uses Brockmeyer v. Dun & Bradstreet, 325 N.W.2d 70 (Wis. Ct. App. 1982).**	Ferraro v. Koelsch, 368 N.W.2d 666 (Wis. 1985).	
Wyoming	Griess v. Consolidated Freightways, 776 P.2d 702 (Wyo. *1989*) (Tort Damages). **RAND uses Allen v. Safeway Stores, Inc., 699 P.2d 277 (Wyo. 1985) (Contract damages)**.	Mobil Coal Producing Inc., v. Parks, 704 P.2d 702 (Wyo. 1985).	Wilder v. Cody Country Chamber of Commerce, 868 P.2d 211 (Wyo. 1994). **RAND uses no case.**