THE COSTS OF WRONGFUL-DISCHARGE LAWS

David H. Autor, John J. Donohue III, and Stewart J. Schwab*

Abstract—We estimate the effects on employment and wages of wrongful-discharge protections adopted by U.S. state courts during the last three decades. We find robust evidence that one wrongful-discharge doctrine, the implied-contract exception, reduced state employment rates by 0.8% to 1.7%. The initial impact is largest for female and less-educated workers (those who change jobs frequently), while the longer-term effect is greater for older and more-educated workers (those most likely to litigate). By contrast, we find no robust employment or wage effects of two other widely recognized wrongful-discharge laws: the public-policy and good-faith exceptions.

I. Introduction

WHAT is the price of protection? This paper estimates the social costs, in possibly lower employment and wages, of common-law protections designed to protect American workers from wrongful discharge. Economic theory suggests that employment protection is a double-edged sword. It provides employment security to incumbent workers but makes employers reluctant to hire, leading to a less flexible labor market with potentially lower employment and wages. It is frequently argued that the stagnant employment performance of many European economies during the 1980s and 1990s—"Eurosclerosis"—can be attributed in part to the significant employment protection given European workers [see Lazear (1990) and Blanchard and Wolfers (1999); Krueger and Pischke (1998) provide a contrasting view]. Among the obstacles to testing this hypothesis is the difficulty of making reliable inferences using cross-country comparisons.

In this paper, we study the effects of employment protection in the United States. Numerous scholars have examined the effects of American federal employment laws on employment and unemployment. Acemoglu and Angrist (2001), DeLeire (2000), and Jolls and Prescott (2004) present evidence that the Americans with Disabilities Act (ADA) decreased employment of disabled persons. Oyer and Schaefer (2000, 2002) conclude that the federal Civil Rights Act of 1991 increased the frequency of mass layoffs and raised the returns to experience for workers who have a downward-sloping age-litigation profile. Hahn, Todd, and van der Klaauw (2001) also evaluate the costs of federal antidiscrimination laws. A major hurdle for each of these studies is that these federal statutes apply all at once to the entire country. This makes it difficult to separate the effects of the statute from all other changes occurring simultaneously (cf. Donohue, 1998; Donohue and Heckman, 1991).[1]

This paper overcomes this methodological challenge by exploiting variation in the extent and timing of adoption of employment protections across U.S. states. The United States, uniquely in the industrialized world, has long had a legal presumption that workers can be fired *at will*—that is, "for good cause, bad cause, or no cause at all."[2] During the 1970s and 1980s, this presumption eroded rapidly: most U.S. state courts created three classes of common-law restrictions that limited employers' ability to fire. These exceptions garnered media headlines, created costly litigation, and—perhaps as importantly—generated substantial uncertainty among employers about when they could terminate workers with impunity. We refer to these common-law exceptions as wrongful-discharge laws, and define their precise meaning below.

Our empirical analysis is aided by the considerable variation across states in the timing and extent of their recognition of wrongful-discharge laws. Three states—Florida, Georgia, and Rhode Island—have never altered the employment-at-will doctrine. Ten states now recognize each of three broad classes of exception to the at-will doctrine: the implied-contract, public policy, and good-faith exceptions. A few states have rejected prior adoptions (see appendix, table A1).[3] We use this variation across states and over time to analyze how wrongful-discharge laws affect employment and earnings in state labor markets.

We are not the first to explore these effects. In a widely cited line of research, Dertouzos and Karoly (1992, 1993) used an instrumental variables framework to test whether wrongful-discharge laws affected state-level employment.

Received for publication September 15, 2004. Revision accepted for publication August 2, 2005.

Supplementary table available on REStat Web site.

* MIT Department of Economics and NBER; Yale Law School and NBER; and Cornell Law School, respectively.

We are indebted to Daron Acemoglu, Joshua Angrist, Ian Ayres, David Card, Hank Farber, Dan Ho, Christine Jolls, Lawrence Katz, Alan Krueger, Thomas Miles, Andrew Morriss, Derek Neal, and seminar participants at the NBER Labor Studies Summer Institute, UC Berkeley, Columbia Law School, NYU Law School, Stanford Law School, Yale Law School, the American Economics Association, and the American Law and Economics Association for excellent suggestions. We gratefully acknowledge the research assistance of Rashida Adams, Michael Anderson, Simone Berkowitz, Sarah Bernett, Douglas Bosley, Craig Estes, Rose Francis, Chris Griffin, Tal Gross, Scott Hemphill, Carolyn Heyman, Joshua Linn, Joshua Mayes, Marci Reichbach, and Leslie West, and the research coordination of Rose Merendino.

[1] Chay (1998) circumvents this problem in looking at the impact of the Equal Employment Opportunity Act of 1972, which extended the federal prohibition on discrimination to firms with 15–24 employees, by using the variation across industries in the fraction of workers employed in firms that would become subject to federal antidiscrimination law by virtue of this legislative expansion. Jolls and Prescott (2004) use state variation in disability laws existing prior to the adoption of the federal ADA to shed light on the employment impact of the passage of the ADA.

[2] This quotation is from *Payne v. Western & Atlantic Railroad, Supreme Court of Tennessee, 1884.* Morriss (1994) provides a detailed history of the employment-at-will doctrine.

[3] To date, only Montana (in 1987) has passed a statute establishing a good-cause standard for all employment terminations. All other employment-at-will exceptions are common-law doctrines, that is, case law. In 1991, the Uniform Law Commissioners proposed 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. Krueger (1991) provides an econometric study of the factors leading state legislatures to consider statutory exceptions to the doctrine of employment at will.

The Review of Economics and Statistics, May 2006, 88(2): 211-231

They found surprisingly large impacts. Dertouzos and Karoly estimate that states adopting a tort-based cause of action (that is, one in which plaintiffs may sue employers for full compensatory and punitive damages) suffered a 3% reduction in aggregate state employment—roughly equivalent to a 10% employer-side tax on wages—with an additional 1% or 2% employment decline for states also adopting a contract-based protection, that is, one in which plaintiffs may sue only for economic losses.[4] These findings have not gone unchallenged. Morriss (1995) criticized Dertouzos and Karoly's legal variables. More recently, Miles (2000) used a difference-in-differences approach to estimate the impact of wrongful-discharge doctrines. He reports "no statistically significant effects on either employment or unemployment," but does not comment on the source of the discrepancy between his findings and those of Dertouzos and Karoly.[5]

Our paper joins this debate by comprehensively reevaluating the impacts of wrongful-discharge doctrines on employment and wages using richer data and a more complete coding of the case law than previous work. As with Dertouzos and Karoly's and Miles's studies, our key explanatory variables are the precedent-setting cases that establish the wrongful-discharge laws recognized in each state and time period. We differ from previous studies, however, in using legal and employment data observed at monthly intervals, in measuring wage as well as employment impacts, and in exploring these impacts separately by education and gender demographic subgroups over the short and the longer term. We apply robust estimation techniques throughout, and we validate our findings across time periods, outcome measures, and three distinct data sources.

Although we had anticipated that our reanalysis would reconfirm the null hypothesis accepted by Miles, we instead find a modest but robustly negative impact of one wrongful-discharge doctrine—the implied-contract exception—on the employment-to-population ratio in state labor markets. This impact, which averages -0.8% to -1.6%, exists for all education and gender groups, and is detectable among states adopting at several time intervals during the sample. The short-term impact is most pronounced for demographic subgroups that change jobs most frequently: females, and younger and less-educated workers. Over the longer term (4 to 7 years), however, the costs of implied-contract protection appear to be borne by older and more-educated workers—those most likely to litigate. We find limited evidence that the good-faith exception reduced state employment levels by a similar magnitude, but this finding is not robust. By contrast, we find no evidence that these legal doctrines had any significant impact on workers' wages. We therefore conclude that the costs of these mandates appear to accrue at the employment rather than the wage margin.[6]

Our companion paper, Autor, Donohue, and Schwab (2004; ADS hereafter) demonstrates why prior studies have reached opposing conclusions, ranging from no effect to very large negative effects. Briefly summarized, ADS shows that the exceedingly large disemployment effects estimated by Dertouzos and Karoly—3 to 5 times the magnitude of our estimates—appear driven by problematic instrumental variables that are spuriously correlated with regional employment trends that substantially predate states' adoption of wrongful-discharge laws. By contrast, the discrepancies with the methodologically similar study by Miles are explained by his reliance on a classification of case law developed by Walsh and Schwarz (1996) that differs from ours. As ADS details, Walsh-Schwarz classification neglects to code the initial precedent-setting case law in a large number of instances (20 of 94).[7] By appropriately modifying the Walsh-Schwarz classification, we find that Miles's results may be reconciled with our own.

II. Wrongful-Discharge Laws

A. *Definition and Legal Significance*

Since the heyday of employment at will in the early twentieth century, legislatures, courts, and other market institutions have repeatedly encroached on U.S. employers' discretion to terminate workers at will. First, unions have negotiated "just cause" contractual protection against firing for their members.[8] State legislatures have enacted broad statutes constraining employers' discretion to fire workers belonging to "protected classes," defined by race, color, religion, sex, national origin, age, disability, and union

[4] Dertouzos, Holland, and Ebener (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).

[5] In related work, Kugler and Saint-Paul (2004) find that a state's adoption of wrongful-discharge doctrines significantly slows the job-to-job flows of unemployed relative to employed workers. Autor (2003) and Miles (2000) find that employers increased demand for temporary-help agency employment when states adopted common-law exceptions to employment at will.

[6] A variety of studies find incomplete pass-through of employer mandates into wage levels, including Lazear (1990) and Fishback and Kantor (1995). By contrast, Gruber (1994) finds that the cost of mandated maternity benefits in the United States was entirely offset by a decline in women's wages.

[7] This discrepancy reflects differences in the intended purposes for which the legal classifications were developed. As described in section II, our legal classification attempts to identify the *first* case in a state that might trigger a client letter from attorneys warning about a change in law. By contrast, Walsh and Schwarz select cases that best articulate courts' rationales for promulgating a new doctrine. These cases often follow the initial precedent-setting decision by several years.

[8] Indeed, any employment contract for a specified term of years ordinarily cannot be terminated prior to the stipulated ending date without some particularized showing of cause. See, for example, California Labor Code §2924, which provides: "An employment for a specified term may be terminated at any time by the employer in case of any willful breach of duty by the employee in the course of his employment, or in case of his habitual neglect of his duty or continued incapacity to perform it." Increasingly, high corporate executives are also signing contracts that reward them with large severance payouts unless they are fired for gross negligence, malfeasance, or some other act of serious misconduct.

membership.[9] Additional narrow statutes also bar terminations for specific reasons, for example, to prevent pension benefits from vesting or to retaliate against employees for whistle-blowing or performing jury duty.[10]

Third, and central for this analysis, during the 1970s and 1980s the majority of U.S. state courts adopted one or more common-law exceptions to the employment-at-will doctrine that limited employers' ability to fire. These are: (1) the tort of wrongful discharge in violation of public policy (*public policy* exception); (2) the implied covenant to terminate only in good faith and fair dealing (*good-faith* exception); and (3) the implied-in-fact contract not to terminate without good cause (*implied-contract* exception). We define these exceptions in turn and discuss their significance.

First recognized by the California Supreme Court in 1959, the public policy exception gained widespread recognition in the 1980s: 34 states adopted this exception between 1979 and 1994, and a total of 43 by 1999. The public policy exception provides employees with protections against discharges that would thwart an important public policy, such as performing jury duty, filing a worker's compensation claim, reporting an employer's wrongdoing, or refusing to commit perjury.[11] In the majority of states, the public policy doctrine provides tort-based protection, meaning that plaintiffs can sue for lost earnings, pain and suffering, and punitive damages. Despite its widespread recognition, successful cases—particularly those with multimillion-dollar judgments—are rare. One reason is that courts typically limit public policy cases to clear violations of express legislative commands rather than violations of a vaguer sense of public obligation. Accordingly, some legal scholars have argued that the public policy doctrine is of minor legal and economic significance (see Edelman, Abraham, & Erlanger, 1992).

Like the public policy exception, the good-faith exception also prevents employers from firing workers for "bad cause." A leading example is the case of *Fortune v. National Cash Register Co.,* where the employer fired a salesperson just before a substantial commission was due.[12] The court found that the employer had deprived the plaintiff of the "benefit of his bargain" and awarded compensatory and punitive damages. Read broadly, the good-faith doctrine could have sweeping consequences, serving as a general prohibition against terminating any worker without just cause (that is, economic necessity or poor performance). In point of fact, the 11 state courts that currently recognize this doctrine have primarily limited good-faith awards to *timing* cases in which the employer intentionally deprives the worker of a promised benefit, such as a sales commission or pension benefit.[13] Hence, like the public policy exception, the good-faith doctrine has found relatively narrow application.

Finally, 41 states recognize the implied-contract exception. This protection comes into force when an employer implicitly promises not to terminate a worker without good cause. A landmark decision establishing the implied-contract exception was the 1980 case of *Toussaint v. Blue Cross & Blue Shield,* in which a dismissed worker successfully sued for breach of contract by citing an internal personnel policy handbook stating that it was Blue Cross's policy to terminate employees only for just cause.[14] The court held that the handbook implied a binding contract, and the worker was remunerated for breach of contract. An equally influential 1981 California case, *Pugh v. See's Candies,* expanded the implied-contract notion by finding that workers may be entitled to ongoing employment due to longevity of service, a history of promotion or salary increases, general company policies, or typical industry practices.[15] In the subsequent five years, courts in 25 other states adopted an implied-contract exception.

The expected employer costs of the implied-contract exception are difficult to assess. Two factors limit employer risk. First, implied-contract cases lead only to contractual damages (that is, economic rather than punitive or full compensatory damages), so spectacular jury awards are unlikely.[16] Second, employers can potentially insulate themselves from implied-contract claims by rewriting employment contracts and handbooks to state clearly that all employment contracts are at will.[17] On the other hand, the factors creating an implied-contract claim are vaguer than for a public policy claim, which likely contributes to

[9] 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.

[10] 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).

[11] As Schwab (1996) discusses, courts tend to apply this exception to the at-will doctrine when the termination clearly affects third parties.

[12] 364 N.E.2d 1251 (Mass. 1977).

[13] Many of the states that recognize the good-faith exception allow for full tort compensatory and punitive damages, although California prominently stopped doing so in the case *Foley v. Interactive Data Corp.,* 765 P.2d 373 (Cal. 1988). Oklahoma and New Hampshire previously recognized good faith as a distinct action, but reversed their prior decisions in 1989 and 1980, respectively. During our period of study, California recognized a very broad good-faith obligation (even with the *Foley* holding that successful plaintiffs would be limited to receiving contract damages). In *Guz v. Bechtel National, Inc.,* 8 P.3d 1089 (Cal. 2000), after our period of study, the court restricted good-faith claims primarily to timing cases.

[14] 292 N.W.2d. 880 (Michigan, 1980).

[15] 171 Cal. Rptr. 917 (Cal. Ct. App. 1981).

[16] Plaintiffs' attorneys will often append claims for fraud or defamation to their implied-contract complaints in an attempt to get before a jury on a claim for punitive damages.

[17] 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.

employer uncertainty about the litigation risks entailed.[18] Additionally, unlike the public policy and good-faith doctrines (as they have developed), the implied-contract doctrine can potentially reclassify an employer's entire workforce as not at will. In this case, the employer may terminate its employees only for good cause—which is far more likely to constrain employers than the specific "bad causes" prohibited by the public policy and good-faith exceptions.[19] Hence, paradoxically, the implied-contract doctrine is easier to 'contract around' and potentially less costly per litigant than other wrongful-discharge protections, yet is also more sweeping.

Unfortunately, no comprehensive data exist on the number of outcome of wrongful-discharge cases under these three doctrines.[20] Several findings in the literature suggest, however, that the implied-contract exception—and wrongful-discharge laws more generally—may have changed employers' hiring and termination practices. First, Miles (2000) and Autor (2003) find that employers substantially increased their use of temporary-help-agency workers shortly after their states adopted implied-contract exceptions. Second, Kugler and Saint Paul (2004) find that the hiring odds of unemployed workers declined after courts in their states recognized wrongful-discharge protections, particularly the implied-contract exception. Third, sales of employment practices liability insurance (EPLI) policies, which insure employers against litigation risk, became widespread in the 1990s. Although EPLI shields employers from liability under both federal antidiscrimination (and other) statutes and state common-law wrongful-discharge protections, an authority on EPLI interviewed for this research averred that "how protective wrongful-discharge laws are in a particular state is an important factor in setting EPLI premiums."[21] This suggests that wrongful-discharge laws impose real costs.

B. *Hypothesized Effects on the Labor Market*

As discussed by Lazear (1990) and Blanchard and Katz (1997), the theoretical effect of firing restrictions on employment levels is ambiguous. In a frictionless labor market, the Coase theorem predicts that imposition of employer-side firing costs will be fully undone by efficient worker-firm bargains; for example, workers would post a bond equal to the firing cost. Where the Coasean result does not hold, firing costs reduce employers' incentives to hire new workers and to fire incumbent workers (Donohue, 1989). This dampens employment fluctuations, which can raise or lower employment levels in the short term. Over the longer term, if employment protections raise employment costs without yielding corresponding productivity increases, a simple supply-and-demand model would predict that employment levels and/or wages are likely to fall. This effect is exacerbated if firing restrictions encourage workers to engage in rent-seeking (that is, nonmeritorious) litigation or induce employers to retain unproductive workers to avoid litigation.

Not all (non-Coasean) employment protection degrades 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 (Summers, 1989). By raising employer costs, mandated employment protection shifts labor demand inward. But to the degree that workers value the mandated benefit, labor supply simultaneously shifts outward, muting the adverse employment impact. If employees value the benefit at its full marginal cost, wages will in theory fall to cover the cost of providing the benefit, and employment levels will be unaffected (see, for example, Gruber, 1994).[22]

Although the overall impact of erosions of the at-will doctrine on employment or unemployment is not clear a priori, existing evidence suggests that the impact may differ for different groups of workers. Several studies find that the employment of younger, less-educated workers appears most likely to be harmed by wrongful-discharge protections, while older and more-educated workers appear to benefit (OECD, 1999, 2004; Jolls, 2000; Bertola, Blau, & Kahn, 2002). We examine these disparate impacts in depth below and find important differences by demographic group that depend on the time horizon examined.

III. Data Sources and Model Specification

A. *Data Sources*

To measure employment and earnings, we draw on the complete Current Population Survey (CPS) monthly files for the years 1978 to 1999. The CPS provides individual labor force data for approximately 100,000 adults per survey month starting in 1978 and contains wage data for

[18] Schwab (1993) offers a unified framework for interpreting implied-contract cases.

[19] The legal consequences of an implied contract are not always identical to those of an actual contract. For example, a worker who is covered by an explicit good-cause provision who is terminated for, say, harassing a fellow worker will prevail if the jury believes the harassment did not occur. In an implied-contract case, however, courts frequently hold that the discharged worker cannot prevail without showing that the employer did not reasonably believe the harassment occurred, thereby protecting reasonable judgments made by employers in good faith. *Cotran v. Rollins Hudig Hall International,* Inc., 17 Cal.4th 93 (1998).

[20] Nor would these caseload data provide a full measure of the economic costs of wrongful-discharge laws, for the observed caseload is an equilibrium function of employer decisions to avert or settle suits and employee incentives to file suits.

[21] Interview with Richard S. Betterley, publisher of the *Betterley Report,* a leading survey of EPLI insurance carriers (January 23, 2004).

[22] Moreover, as several authors have argued, adverse selection in labor markets may cause employers to provide inefficiently low levels of job security (Aghion & Hermalin, 1990; Levine, 1991). Restrictions on firing could therefore raise employment while reducing wages. This would correspond to a case where workers value job security more at the margin than it costs employers to provide.

one-quarter of the employed subsample beginning in 1979.[23] We calculate employment-to-population ratios by state, month, and year, and use micro data on hourly earnings in models for hourly wages. In some analyses, we also present results for eight demographic subgroups distinguished by gender, education, and age. In section VI, we verify the CPS-based employment results using independent data from the Current Employment Statistics (CES). The CES data offer a longer time series but lower precision.

To maximize usable variation in the timing of the adoption of wrongful-discharge laws, we code the legal and employment variables at monthly frequency, as done by Morriss (1995). Hence, if two states adopt a wrongful-discharge doctrine 11 months apart within the same calendar year, our estimates take accurate account of this substantial difference in timing. Because the outcome data are observed at high frequency, serial correlation is a major concern. Following the recommendations of Bertrand, Duflo, and Mullainathan (2004), we compute standard errors using the generalized Huber-White formula clustered by state. This allows for arbitrary error correlations among state-month observations.[24] In addition, we focus our analysis on relatively short pre-post intervals surrounding law adoption to isolate discrete effects on labor market outcomes.

For our legal variables, we developed a taxonomy of wrongful-discharge law prevailing in each state and month-year for the three-decade period from 1970 to 1999. As Morriss (1995) discusses, it is not always easy to date when a state has adopted a particular at-will exception. Our objective is easily stated, however. We envision management-side employment lawyers reading the advance sheets and writing awareness letters to their clients when major changes occur in the common law. Thus, we are interested in the first court decision in a state that would trigger a client letter warning about a law change. In practice, we looked for the first major appellate-court decision (either the intermediate court or the state supreme court) that signaled the sustained adoption of the particular at-will exception. Thus, a lower court decision adopting an exception that was reversed on appeal would not be counted, but a supreme court decision or lower court decision not reversed would be counted. As it turned out, our independent assessment of the legal doctrines for the 50 states largely agrees with Morriss's list of relevant cases, which we update to 1999.[25] Our companion paper ADS shows that our findings are robust to the choice of the alternative legal classifications developed by Dertouzos and Karoly (1992) and Morriss (1995).[26]

B. *Model Specification*

Because state courts adopted the common-law wrongful-discharge doctrines in different months and years during the 1980s and 1990s, we have potentially many "experiments" to exploit. Our empirical approach contrasts the change in employment and wages in states adopting a given wrongful-discharge doctrine in a given period with that in states not adopting any doctrine during the same time period.

To implement this difference-in-differences design, we must select a pre and a post period for each contrast. Although we could use the entire 1978–1999 panel to calculate these contrasts, that has two disadvantages. First, because states adopted exceptions in the first year of our 1978–1999 CPS data set and as late as 1998, the long-panel approach implies that for some states, observations from two decades before or after adoption would be used to form a pre-post contrast. This is unappealing. Second, the long-panel approach exacerbates the serial correlation problem noted above.

To mitigate these problems, we use as a baseline a five-year pre-post window: the 24 calendar months prior to adoption of a doctrine are designated as the pre period; months 13 to 36 following adoption are designated as the post period; and to allow for an adjustment interval, the first 12 months immediately following adoption are excluded from the sample. We later explore the sensitivity of our results to this set of choices by contrasting estimated short- and long-term labor market impacts. To form a control sample of nonadopting states, we include the maximal set of state-month observations for corresponding calendar months for states that did not adopt any of the three doctrines during the relevant pre- or posttreatment time interval. This design implies that some states serve as treatment states in one period and control states in another, although never within a five-year window surrounding treatment.[27]

[23] Individuals may appear up to four times in one calendar year in the employment sample (not the wage sample), though their labor force status may differ on each occasion. Our estimation procedure takes account of potential serial correlation among observations within each state sample.

[24] Specifically, the estimator for the variance-covariance matrix is given by

$$W = (V'V)^{-1}\left(\sum_{j=1}^{N} u'_j u_j\right)(V'V)^{-1},$$

where N is the total number of states, V is the matrix of independent variables, and u_j is defined for each state to be

$$u_j = \sum_{t=1}^{T} e_{jt} v_{jt},$$

where e_{jt} is the estimated residual for state i at time t, and v_{jt} is a row vector of dependent variables (including the constant). This procedure is implemented in Stata software using the "cluster" command (clustering on state).

[25] Although we use the three-part division of the at-will exceptions in the body of our analysis, we also explored the relevance of the tort contract distinction on which Dertouzas and Karoly (1992) focus. We did not find this distinction to be relevant or empirically robust.

[26] As discussed in the introduction, the Walsh-Schwarz (1996) classification used by Miles (2000) yields much weaker results. In ADS, we trace this to the fact that Walsh and Schwarz do not necessarily code the precedent-setting state cases but instead select the (typically later) cases that provide the clearest articulation of the newly adopted doctrines.

[27] For example, Maryland adopted the implied-contract exception in January of 1985, so the window of time around the commencement of treatment that enters our analysis begins at January 1983 (24 months before adoption) and continues through December 1987 (36 months after

Our basic econometric model is

$$Y_{st} = \alpha + \beta_1 Treat_{st} + \beta_2 Post_{st} + \beta_3 Treat_{st} Post_{st} + \varepsilon_{st}, \tag{1}$$

where $Treat_{st}$ is an indicator for the period from 24 months before to 36 months after adoption of a wrongful-discharge law in state s, and $Post_{st}$ is an indicator for the period 13 through 36 months after adoption. The coefficient of interest in this equation, β_3, is an estimate of the pre-post change in the outcome variable in adopting states relative to the corresponding change in nonadopting states. All estimates are weighted by the share of national residents aged 18–64 in each state-year cell.[28]

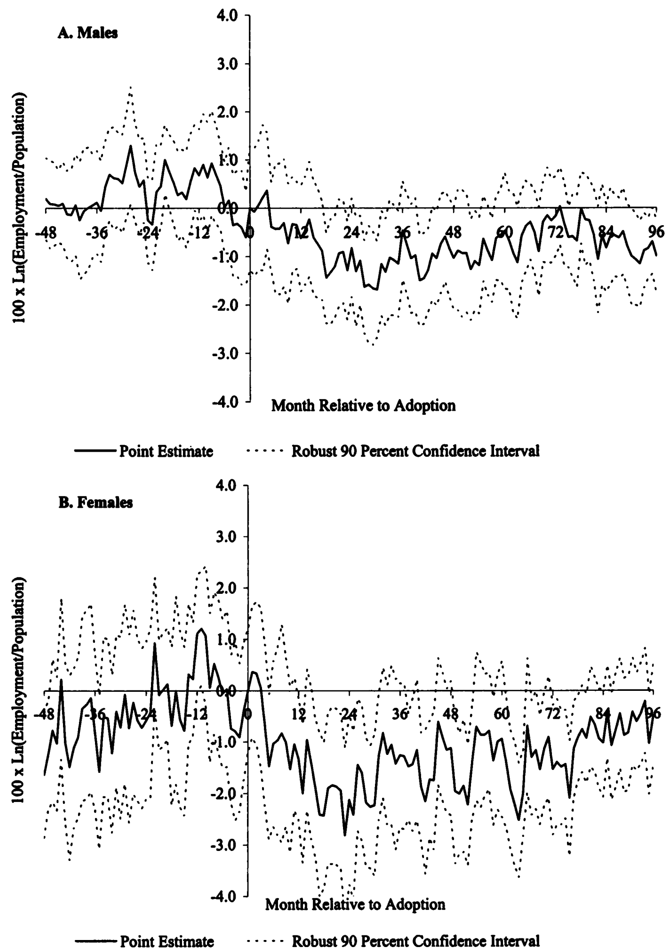
We enrich this basic model in three ways. First, in place of the common main-effect and pretreatment indicators (α and $Treat_{st}$), we add main effects for each state and their interactions with a treatment indicator variable. Second, to control flexibly for common shocks to national employment, we include an exhaustive set of time dummies, corresponding to each year and month of the sample. Finally, to allow for common regional employment shocks, we also estimate specifications that include interactions between calendar-year dummies and indicator variables denoting the four major Census geographic regions. With region controls included, the parameter β is identified by contrasting contemporaneous employment or wage outcomes in adopting versus nonadopting states located in the same geographic regions.[29]

IV. Impacts on Employment and Earnings

Before turning to estimates of equation (1), we provide a visual summary of the employment data in figures 1 through 3. These figures plot estimated log employment-to-population ratios in adopting relative to nonadopting states at monthly intervals in the 4 years prior through the 8 years following the adoption of each doctrine. Employment levels in the first full month following adoption are normalized at 0, and the dashed lines in each figure represent robust 90% confidence intervals (allowing for arbitrary within-state error correlations) for each monthly point estimate.[30]

These figures provide initial evidence that one wrongful-discharge doctrine, the implied-contract exception, did indeed affect state employment levels. As is visible in figure 1, relative (log) employment-to-population ratios for both

FIGURE 1.—STATE LOG EMPLOYMENT-TO-POPULATION RATIOS BEFORE AND AFTER ADOPTION OF IMPLIED-CONTRACT EXCEPTION: MONTHLY LEADS AND LAGS FROM 4 YEARS BEFORE TO 8 YEARS AFTER ADOPTION



adoption). Observations for January 1983 to December 1984 form the Maryland pretreatment sample, and observations from January 1986 to December 1987 form the Maryland posttreatment sample. As control states, we use all observations from other state-months that were not assigned to treatment during January 1983 through December 1987. Our model compares the change in the dependent variable in the treatment states across the pre and post periods to the change over the same years in the control states. Starting in January 1988, Maryland may reenter the control sample for later-treated states.

[28] We weight by population shares rather than population counts to avoid inadvertently placing greater weight on later observations due to growing national population.

[29] Because, as noted previously, treated states may contribute control observations 36 months after a law is adopted, the version of equation (1) that we implement is slightly richer. For each state that reenters the sample, we additionally add a post-post dummy for the posttreatment period (that is, months ≥ 37 following law adoption). Hence, the version of equation (1) implemented is

$$Y_{st} = \gamma_s + \gamma_s \cdot Treat_{st} + \beta_1 Post_{st} + \beta_2 Treat_{st} Post_{st} + \beta_3 Postpost_{st} + \delta_t + \varepsilon_{st},$$

where γ_s and δ_t are vectors of state and time dummies. As a check on this specification, we estimate in the appendix (table A2) a set of models that restrict treated states from reentering the control sample 37 months following treatment. The results in table A2 are nearly identical to those in table 1.

[30] Specifically, the figures plot the coefficient and 90% confidence bands from estimates of parameters γ_τ from the following equation:

$$Y_{st} = \delta_s + \phi_t + \sum_{\tau=-48}^{96} \gamma_\tau L_{s,t-\tau} + \varepsilon_{st},$$

where, as above, Y_{st} is the natural logarithm of the estimated employment-to-population ratio in state and time period s and t; δ_s and ϕ_t are vectors of state and time main effects; and L_{st} is a dummy variable that assumes the value of 1 (only) in the month that a state adopts a given doctrine (the impact of each doctrine is estimated simultaneously). Huber-White standard errors allow for arbitrary error correlations within states.

FIGURE 2.—STATE LOG EMPLOYMENT-TO-POPULATION RATIOS BEFORE AND AFTER ADOPTION OF PUBLIC POLICY EXCEPTION: MONTHLY LEADS AND LAGS FROM 4 YEARS BEFORE TO 8 YEARS AFTER ADOPTION

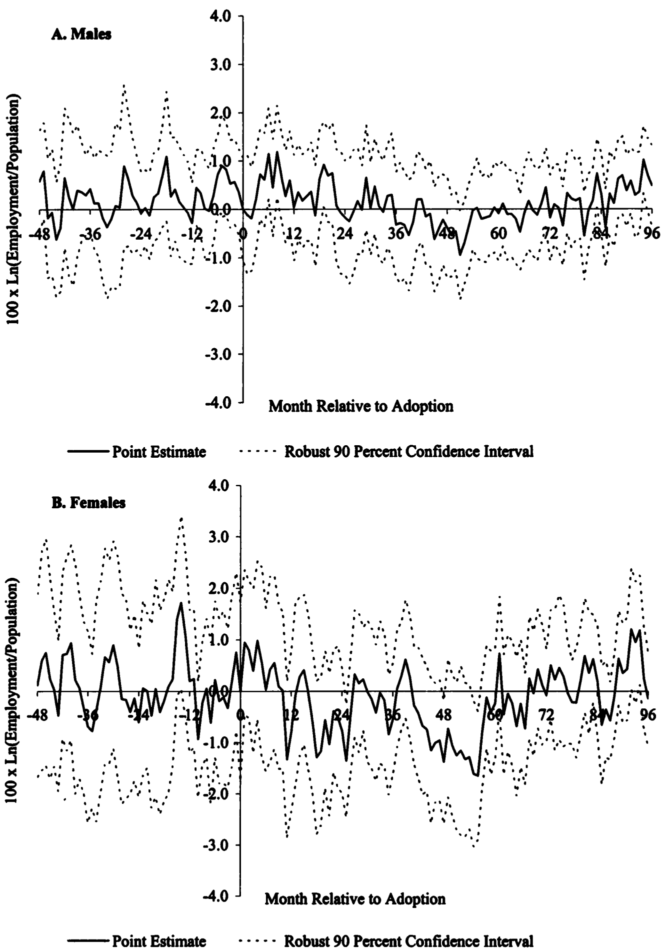
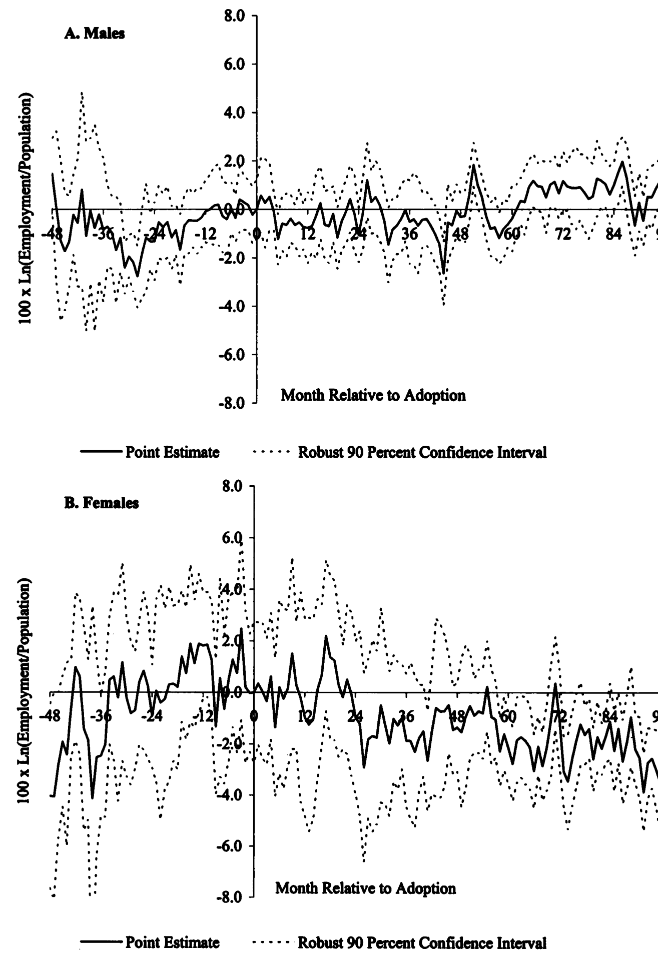


FIGURE 3.—STATE LOG EMPLOYMENT-TO-POPULATION RATIOS BEFORE AND AFTER ADOPTION OF GOOD-FAITH EXCEPTION: MONTHLY LEADS AND LAGS FROM 4 YEARS BEFORE TO 8 YEARS AFTER ADOPTION



males and females dip by approximately 1.5% to 2% over the 2 years following adoption of the implied-contract exception, reaching a nadir after approximately 24 to 30 months. By contrast, figures 2 and 3 provide little evidence that the public policy or good-faith exceptions affected employment levels. One should not make strong inferences from these figures, however. As is visible from the wide standard-error bands, the monthly point estimates are rather noisy. In addition, these models do not include the full set of controls that we later use for estimating equation (1). Nevertheless, the formal analysis of employment below largely bears out the impression given by the figures.[31]

A. Initial Estimates: Employment and Wages

The first panel of table 1 presents estimates of equation (1) for employment. What emerges clearly is that adoption of the implied-contract exception is associated with a modest but meaningful reduction in employment. In column 1 of panel A, we estimate that adoption of the implied-contract doctrine reduces the overall employment-to-population ratio by 1.7 log points in the second and third years following adoption ($t = 3.1$).[32] Adding dummies to absorb region-by-year employment shocks reduces the absolute magnitude of this point estimate only slightly, to 1.6 log points, and it remains highly significant ($t = 3.5$).

The next two rows of the table repeat these estimates for the public policy and good-faith doctrines. The public policy doctrine is associated with a small reduction in employment, but this is never significant. The point estimates for the good-faith doctrine indicate larger employment reductions—in the range of 0.4 to 0.6 log points—but these are also statistically insignificant. The low precision of the good-faith point estimates likely reflects the fact that there are fewer adoptions of the good-faith doctrine than of the

[31] We do not provide comparable plots for wage levels, because figures (and regression estimates in subsequent tables) show no evidence of a wage impact.

[32] We use the term log point to refer to a 0.01 change in the natural logarithm of the outcome measure. For the small effects measured here, log points are approximately equal to percentage points (equal to exp[log points] − 1).

TABLE 1.— DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE IMPACT OF WRONGFUL DISCHARGE LAWS ON STATE EMPLOYMENT-TO-POPULATION RATIO AND HOURLY EARNINGS: CONTRASTING OUTCOMES IN YEARS 2 AND 3 FOLLOWING ADOPTION WITH YEARS 1 AND 2 PRECEDING ADOPTION

	A. 100 × ln(Employment/Population): 1978–1999						B. 100 × ln(Hourly Wage): 1979–1999					
	All Employment		Manufacturing		Nonmanufacturing		All Employment		Manufacturing		Nonmanufacturing	
Exception	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Implied	−1.72	−1.59	−3.04	−2.89	−1.10	−1.18	0.54	0.46	0.54	0.54	0.60	0.49
contract	(0.55)	(0.45)	(1.87)	(1.54)	(0.84)	(0.504)	(0.84)	(0.76)	(0.71)	(0.65)	(0.96)	(0.84)
R^2	0.870	0.894	0.932	0.944	0.926	0.944	0.234	0.235	0.320	0.321	0.220	0.221
n	7,511		7,511		7,511		1,898,114		394,658		1,503,456	
Public policy	−0.23	−0.07	1.75	0.12	−0.65	0.01	−0.69	−0.51	0.18	0.25	−0.99	−0.84
	(0.80)	(0.59)	(1.91)	(1.62)	(0.89)	(0.60)	(0.56)	(0.57)	(0.71)	(0.53)	(0.62)	(0.63)
R^2	0.848	0.875	0.935	0.944	0.918	0.936	0.233	0.233	0.321	0.322	0.219	0.219
n	7,863		7,863		7,863		1,946,943		400,133		1,546,810	
Good faith	−0.37	−0.63	5.62	1.71	−1.88	−0.45	−1.28	−0.37	−2.22	−1.70	−1.28	−0.18
	(0.61)	(0.88)	(1.92)	(2.55)	(0.79)	(1.02)	(1.44)	(1.79)	(1.22)	(1.43)	(1.59)	(1.84)
R^2	0.852	0.883	0.929	0.941	0.916	0.935	0.229	0.230	0.310	0.311	0.216	0.217
n	7,523		7,523		7,523		1,883,260		378,217		1,505,043	
Region × year dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Panel A: Each entry is from a separate weighted OLS regression in which the dependent variable is the log of the state-month ratio of employment (in the designated sector) to population for residents aged 16–64 in 50 U.S. states. Employment is estimated from complete combined Current Population Survey monthly files for 1978–1999. All models include state main effects and indicators for each year×month in the sample. Models in even-numbered columns also include interactions between four Census-region dummies and individual calendar year dummies. Models are weighted by state's share of national population aged 16–64 in each month-year using CPS sampling weights. Huber-White robust standard errors in parentheses allow for unrestricted error correlations across observations within states.

Panel B: Each entry is from a separate weighted OLS regression of log real hourly earnings of currently employed (wage or salary) non-self-employed workers aged 16–64. Wages are calculated from the Current Population Survey Merged Outgoing Rotation Group files for 1979–1999 as the log of usual weekly earnings divided by usual weekly hours. Top-coded observations are multiplied by 1.5, and wages below $1.50 or above $100 per hour in real 2000 dollars (using the personal consumption expenditure deflator) are discarded. All models include state main effects, dummy variables for each year×month in the sample, and dummies for eight demographic groups: (male vs. female) × (high school or less vs. some college or more) × (ages 16–39 vs. 40–64). Models in even-numbered columns also include interactions between four Census-region dummies and individual calendar year dummies. Regressions are weighted by CPS earnings weights. Huber-White robust standard errors in parentheses allow for unrestricted error correlations across observations within states.

Treatment sample in each panel includes observations for 1–24 months prior to and 13–36 months following adoption of relevant doctrine in adopting states (months 0–12 following adoption are omitted). Control sample includes maximal set of observations for corresponding calendar months from states that did not adopt any of the three doctrines during the relevant pre- or posttreatment time interval. The coefficient reported is the interaction between treatment status (that is, adopting a doctrine) and an indicator for 13–36 months after adoption.

other exceptions: 10 for good faith versus 36 and 34 for implied contract and public policy.[33]

To confirm that these results are not driven by sectoral trends, subsequent columns tabulate models estimated separately for manufacturing and nonmanufacturing employment. In these models, the included time and region dummies implicitly account for sector-specific (manufacturing versus nonmanufacturing) shocks that could potentially induce bias.[34] These models find significant negative effects of the implied-contract doctrine on both manufacturing and nonmanufacturing employment. The point estimate for manufacturing employment is substantially larger than for nonmanufacturing (−3.0% versus −1.1%), but also estimated with substantially lower precision, due to the smaller scale and greater variability of manufacturing employment; hence, these point estimates are not significantly different at the 5% level. We again find no significant effect of the public-policy doctrine on employment. By contrast, the good-faith doctrine is associated with a large rise in manufacturing employment and a substantial decline in nonmanufacturing employment. These effects appear driven by regional shocks, however. Neither point estimate proves robust to inclusion of region-by-year dummies.[35]

Panel B of table 1 presents comparable estimates for the impact of wrongful-discharge doctrines on log hourly earnings of employed workers. For these models, we fit the equation

$$w_{ijst} = \beta_4 Treat_{st} + \beta_5 Post_{st} + \beta_6 Treat_{st} Post_{st} + \gamma_s + \delta_t + \pi_j + \varepsilon_{ijst}, \tag{2}$$

where w is 100 times the log hourly wage of individual i belonging to demographic group j in state s and year-month t. In addition to the state and time effects used above, these models also include a vector of dummy variables, π_j, indicating membership in each of eight demographic groups [(female vs. male) × (ages 18–39 vs. 40–54) × (education

[33] Although a total of 43, 43, and 13 implied-contract, public policy, and good-faith exceptions were adopted, not all occur in our sample window. We analyze a longer sample frame in table 6.

[34] The dependent variable in these models is the logarithm of the ratio of employment in the sector (manufacturing or nonmanufacturing) to the total state population aged 16–64. Models that instead use the logarithm of sectoral employment with no denominator yield comparable results.

[35] On the theory that costly employment protections may cause workers to substitute to the unprotected sector, we also estimated models for self-employment rates by state and month (estimates available from the authors). In contrast to expectations, the signs of the point estimates for the self-employment outcome are in most cases equal to those for overall employment, suggesting no substitution (and perhaps indicating that self-employment and formal employment are complements). However, these estimates are in all cases economically small and statistically insignificant. We are not able to estimate comparable models for wages, because self-employed workers do not report earnings in the CPS samples.

TABLE 2.—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE IMPACT OF WRONGFUL-DISCHARGE LAWS ON EMPLOYMENT AND HOURLY WAGES, 1978–1999: CONTRASTING THE IMPACT OF ANY DOCTRINE VERSUS SPECIFIC DOCTRINES

	All Industries				Manufacturing				Nonmanufacturing			
Doctrine	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	A. 100 × ln(Employment/Population), 1978–1999											
Any doctrine	−0.75	−0.63			0.53	−0.38			−0.97	−0.63		
	(0.55)	(0.40)			(1.24)	(1.06)			(0.70)	(0.43)		
Implied contract			−1.63	−1.44			−3.32	−2.52			−0.96	−1.14
			(0.55)	(0.45)			(1.57)	(1.38)			(0.73)	(0.55)
Public policy			−0.18	−0.10			1.61	−0.29			−0.58	0.07
			(0.67)	(0.46)			(1.72)	(1.47)			(0.75)	(0.49)
Good faith			−0.72	−0.73			4.98	1.88			−2.42	−0.96
			(0.56)	(0.62)			(1.69)	(1.67)			(0.75)	(0.66)
R^2	0.845	0.877	0.853	0.880	0.922	0.937	0.926	0.938	0.911	0.935	0.917	0.936
n	10,465				10,465				10,465			
Region × year dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
	B. 100 × ln(Hourly Wage), 1979–1999											
Any doctrine	0.26	0.44			0.75	0.66			0.16	0.37		
	(0.60)	(0.62)			(0.55)	(0.50)			(0.67)	(0.69)		
Implied contract			0.75	0.49			0.71	0.55			0.86	0.58
			(0.81)	(0.72)			(0.72)	(0.63)			(0.91)	(0.78)
Public policy			−1.11	−0.25			0.01	0.63			−1.47	−0.59
			(0.70)	(0.58)			(0.76)	(0.58)			(0.77)	(0.64)
Good faith			−0.76	−0.01			−1.98	−1.54			−0.63	0.29
			(1.32)	(1.67)			(1.25)	(1.15)			(1.51)	(1.78)
R^2	0.232	0.232	0.232	0.232	0.310	0.316	0.315	0.316	0.218	0.219	0.218	0.219
n	2,551,552				518,317				2,033,235			
Region × year dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Huber-White robust standard errors in parentheses allow for unrestricted error correlation within states. Dependent variables, samples, and weights are as in table 1, panels A and B. Coefficients reported are the interactions between treatment status (that is, adopting any doctrine or a specific doctrine) and an indicator for 13–36 months after adoption.

high school or less vs. at least some college)]. Standard errors are clustered by state, as above.[36]

These estimates yield no evidence that wrongful-discharge doctrines affected earnings of employed workers. For the implied-contract and public policy doctrines, point estimates are uniformly small, precisely estimated, and very far from significant. The point estimates for the good-faith doctrine are uniformly negative and in some cases large, in the range −1.2% to −2.2%. But these point estimates are also insignificant, and their magnitude is substantially reduced by inclusion of region effects.

As noted earlier, treated states—that is, those that adopted a law during our sample period—may contribute observations to the control group starting 36 months following law adoption. To provide a check on any potential bias induced by this procedure, we present in the appendix (table A2) a version of the table 1 models where treated states do not enter the control sample. These models produce near-identical estimates to our main results in table 1, suggesting that our procedure increases efficiency without inducing bias.

[36] As in the employment models, we also include a post-post dummy variable for state-month observations where a state was previously treated and reenters the sample as a control observation.

B. *Does the Specific Doctrine Matter?*

Given the generally negative estimated impact of each category of doctrine on employment levels, one potential interpretation of these results is that it is not the specific doctrine that matters, but simply whether the state has adopted any wrongful-discharge doctrine. To examine this issue, we estimate in table 2 a set of models that compares the impacts of an any-doctrine variable with a disaggregated set of three doctrine variables. As with the previous models, we specify the two-year period prior to law change as the pretreatment period and the two-year period commencing one year after law change as the posttreatment period.[37]

The first two columns of table 2, panel A, confirm that, on average, states adopting any exception to employment at will experienced an employment reduction of approximately 0.6% in the two years following adoption (not significant in either specification). Columns 3 and 4 replace

[37] An additional wrinkle in this specification is that several states adopt multiple doctrines within a five-year window and hence the pre and post periods are not unique. In estimating these models, we include all relevant pre- and posttreatment observations for a given state—meaning that some treatment and control periods overlap—and include, as in equation (1), treatment and treatment × post effects for each doctrine. Control observations are selected identically to the table 1 models.

the any-doctrine dummy with indicators for each of the three legal doctrines. When their effects are estimated jointly, only the implied-contract doctrine is statistically significant, and its point estimate is close to that in the prior table. The public policy and good-faith doctrines are insignificant in all specifications.

Subsequent columns, which repeat these estimates for manufacturing and nonmanufacturing sectors, reinforce the earlier conclusions. The any-doctrine dummy is never significant by itself, whereas the implied-contract doctrine is significant in all but one specification (column 11). The good-faith estimates are again opposite-signed for manufacturing and nonmanufacturing and, as before, are not robust to inclusion of region effects. In sum, the implied-contract doctrine is the only wrongful-discharge law that appears to have a robust negative effect on employment.

We next estimate a variant of equation (2) for wages where the effects of all three laws are estimated simultaneously. These results are found in panel B of table 2. As in table 1, the estimated effect on wages of the implied-contract exception is seen to be small and insignificant, albeit positive. When region controls are included, none of the point estimates in this table is statistically significant, suggesting that either the wrongful-discharge doctrines had no robust wage effects, or that these effects are too small to detect.[38]

C. *Estimates by Subperiod: A Consistency Check*

The preceding estimates pool all years of data to increase the precision of the estimates. The cost of this approach is that it masks any temporal heterogeneity in the economic impact of the doctrines. Table 3 studies this potential heterogeneity by tabulating the effect of each exception on employment for the following adoption cohorts: 1980 to 1983, 1984 to 1987, 1988 to 1992, and 1993 to 1998.[39]

As the first row of table 3 shows, the 15 states that adopted the implied-contract exception during 1980 to 1983 experienced a decline of −0.9% to −1.6% in employment during months 13 through 36 following adoption (the smaller estimate corresponding to the model with region-by-year controls). The 18 states that adopted this exception between 1984 and 1987 also experienced similarly large employment declines. For the final set of states that adopted the doctrine between 1988 and 1992, we also find a similarly negative employment effect (−1.8%). This point estimate is not significant at conventional levels, perhaps because only three states adopt the implied-contract doctrine in this period.

The next four columns of table 3 repeat these estimates for states adopting the public-policy and good-faith exceptions. In almost half of these regressions, the coefficient estimates are smaller than their accompanying standard errors. For the other half, the estimated effects swing wildly in sign and magnitude for each doctrine and time period. This suggests either that these doctrines affect employment inconsistently or, perhaps more plausibly, that their passage is confounded with other significant shocks to employment. By contrast, the consistency of the results for the implied-contract doctrine (across time periods and, in table 2, across sectors) increases our confidence that this doctrine did have a modest but robust causal depressing effect on state employment rates.[40]

D. *Alternative Timing Assumptions*

Thus far, we have relied on our baseline specification, which uses the 24 months prior to adoption as the pretreatment period and the months 13 to 36 following adoption as the posttreatment period. In table 4, we explore the sensitivity of our findings to alternative choices of pre and post periods, and additionally measure the longer-term impacts of the wrongful-discharge doctrines. For reference, the first two columns of table 4 repeat our baseline specification for employment from table 2 (columns 1 and 2). Columns 3 through 10 move the postadoption treatment window closer to the point of adoption by 1 year (that is, immediately thereafter) and then outward by 2, 4, and 6 years respectively.

As with prior estimates, these sensitivity tests indicate that the public policy doctrine is never significant, and that the good-faith doctrine is typically insignificant and never robust to inclusion of region effects. By contrast, varying the postadoption comparison period produces a noteworthy pattern of coefficients for the implied-contract doctrine. We

[38] One further possibility is that wage estimates may suffer from composition bias if, for example, wrongful-discharge laws price low-wage workers out of the labor market. The positive, but insignificant, wage coefficients for the implied-contract exception may be suggestive of such bias, if this legal change dampens employment in a way that disproportionately impacts low-wage workers. To evaluate this bias, we followed Neal and Johnson (1996) and Chandra (2003) in estimating models for impacts on median wages for all potential workers, including the nonemployed. To perform these estimates, we assigned nonworkers an arbitrarily low wage, thereby assuming that their potential earnings are below the median wage in their respective state-time-demographic group cell. Because this restriction excludes many female workers—and because we were not confident in the behavioral assumption that low-earnings females are least likely to participate (see Neal, 2004)—we limited our analysis of median wage estimates to males, few of whom are affected by the 50% restriction. We generally find that estimates of the effects of wrongful-discharge laws on median wages are less positive when nonearners are included in the sample than when they are excluded, suggesting that wrongful-discharge laws reduce the participation of workers in low-earnings cells. However, we found no robust negative effects of wrongful-discharge laws on wage levels in these models. A table of estimates is available from us.

[39] Adoption cohort dates refer to the year a wrongful-discharge doctrine is enacted. As with prior estimates, the pre and post periods used to form the employment contrast are the surrounding five years (two prior to adoption, three after adoption with the first omitted). To allow for the two-year pretreatment period, we do not study adoptions prior to 1980. No state adopted an implied-contract or public policy exception after 1992.

[40] Because strong regional patterns exist in adoptions of the wrongful-discharge doctrine (discussed in ADS), we also estimated the table 3 employment models separately for Southern and non-Southern states. In both regions, we find robust evidence that the implied-contract exception reduced employment-to-population ratios by 1.3 to 1.8 log points.

TABLE 3.—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE IMPACT OF WRONGFUL-DISCHARGE LAWS ON EMPLOYMENT-TO-POPULATION RATIOS: ESTIMATES BY ADOPTER COHORTS, 1978–1999*

	Implied Contract		Public Policy		Good Faith	
	(1)	(2)	(1)	(2)	(1)	(2)
1980–1983						
	−1.56	−0.94	0.35	0.69	−0.14	0.10
	(0.63)	(0.40)	(0.88)	(0.75)	(0.58)	(0.61)
R^2	0.875	0.887	0.864	0.876	0.871	0.882
n	3,334		3,180		2,564	
States adopting	15		11		4	
1984–1987						
	−1.56	−1.47	−1.39	−0.85	−3.10	−2.50
	(1.11)	(0.78)	(1.01)	(0.87)	(0.71)	(0.81)
R^2	0.905	0.914	0.896	0.904	0.905	0.912
n	3,191		3,168		1,813	
States adopting	18		17		3	
1988–1992						
	−1.79	−1.81	2.21	1.05	2.08	2.77
	(1.45)	(1.46)	(0.66)	(0.55)	(0.55)	(0.94)
R^2	0.854	0.888	0.853	0.881	0.857	0.887
n	3,160		3,850		2,944	
States adopting	3		6		2	
1993–1996						
	n/a	n/a	n/a	n/a	−0.02	0.14
					(0.20)	(0.28)
R^2					0.905	0.909
n					2,308	
States adopting	0		0		1	
Region × year dummies	No	Yes	No	Yes	No	Yes

*Dependent variable: 100 × ln(employment/population).

Huber-White robust standard errors in parentheses allow for unrestricted error correlation within states. Dependent variables, specifications, and weights are identical to table 1.

TABLE 4.— DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE IMPACT OF WRONGFUL-DISCHARGE LAWS ON LOG EMPLOYMENT-TO-POPULATION RATIOS FOR YEARS 1978–1999: TESTING SENSITIVITY TO SELECTION OF PRE- AND POST-ADOPTION TREATMENT PERIODS*

Pre period:	Baseline Yrs −2 and −1		Yrs −2 and −1		Yrs −2 and −1		Yrs −2 and −1		Yrs −2 and −1		Yrs −3 and −2		Yrs −4 and −3	
	Yrs 1 and 2		Yrs 0 and 1		Yrs 2 and 3		Yrs 4 and 5		Yrs 6 and 7		Yrs 1 and 2		Yrs 1 and 2	
Post period:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Implied	−1.63	−1.44	−1.10	−0.94	−1.59	−1.51	−1.24	−1.36	−0.52	−0.83	−1.68	−1.45	−1.58	−1.32
contract	(0.55)	(0.45)	(0.46)	(0.36)	(0.56)	(0.54)	(0.66)	(0.66)	(0.71)	(0.66)	(0.68)	(0.56)	(0.84)	(0.74)
Public policy	−0.18	−0.10	0.00	0.03	−0.08	−0.11	0.21	−0.19	0.78	0.17	−0.26	−0.19	−0.28	−0.18
	(0.67)	(0.46)	(0.43)	(0.32)	(0.80)	(0.56)	(0.87)	(0.57)	(0.87)	(0.52)	(0.81)	(0.57)	(0.87)	(0.66)
Good faith	−0.72	−0.73	−0.39	−0.38	−1.35	−1.30	−1.03	−0.71	−0.94	−0.88	−0.38	−0.59	−0.33	−0.70
	(0.56)	(0.62)	(0.40)	(0.42)	(0.58)	(0.69)	(0.69)	(0.79)	(0.96)	(1.03)	(0.66)	(0.77)	(0.97)	(1.06)
R^2	0.853	0.880	0.853	0.879	0.852	0.880	0.859	0.887	0.864	0.891	0.851	0.879	0.851	0.880
n	10,465		10,633		10,425		10,497		9,340		9,964		9,527	
Region × year dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

*Dependent variable: 100 × ln(employment/population)

Huber-White robust standard errors in parentheses allow for unrestricted error correlation within states. Samples, specifications, and weights are identical to table 2 except, as noted, varying selection of pre- and posttreatment intervals surrounding law adoption.

find that the disemployment effect of this exception appears to reach a maximum at 2 to 3 years following adoption, and then gradually decays. By years 6 and 7, the estimated employment reduction is approximately one-half the size of the baseline and is insignificant (a pattern also suggested by figure 1).

What explains this reconvergence between adopting and nonadopting states? One possibility is that it is a statistical artifact: because the vast majority of states adopted the implied-contract exception by the end of our sample, relatively few pure control states—that is, those yet to have adopted the implied-contract exception—are available to form a contrast toward the end of the sample. Alternatively, reconvergence could exist if employers either originally overestimated the costs of the implied-contract doctrine or over time learned how to minimize them. Given the initial uncertainty about the ultimate contours of the legal rules that would emerge after they were first introduced, it would not be surprising that employers would overreact to these judicial innovations, as suggested by the legal analysis of Edelman, Abraham, and Erlanger (1992). Moreover, the overreaction hypothesis is buttressed by the evidence that professional (nonacademic) law journals and personnel journals overstated the threat posed by the implied-contract doctrine, which in itself would lead employers to react excessively.[41] If, over the longer term, businesses discovered that the laws did not substantially raise employment costs, this effect would likely have abated. If, however, the initial costs were real, it is still possible that firms would learn better how to avoid creating implied contracts—perhaps by having all new employees sign forms acknowledging their at-will status—thereby reducing these costs after six or seven years.

The final columns of table 4 test the sensitivity of the employment results to the selection of the pretreatment period. By moving the pretreatment interval backward from the date of adoption, we check against the possibility that wrongful-discharge doctrines were adopted at cyclical employment peaks, thereby leading us to falsely attribute postpeak employment declines to the doctrines rather than the business cycle. Columns 11 and 12 compare employment in years 2 and 3 prior to adoption with employment in years 2 and 3 following adoption; the final two columns perform this comparison for years 3 and 4 prior to adoption. In neither case does the choice of the pretreatment comparison window substantially affect the magnitude or precision of the main results. This suggests that our findings are unlikely to be driven by spurious timing effects.[42]

V. Are All Workers Equally Affected?

Like their European counterparts, U.S. wrongful-discharge laws disproportionately protect workers with longer tenure and higher wages. Long-tenure workers can more easily make a prima facie case that their jobs provided an expectation of ongoing employment (in the case of the implied-contract doctrine), or an expectation of future benefits for current service (good-faith doctrine). In addition, damage awards tend to be roughly proportional to prior earnings, particularly in implied-contract cases. Hence high-wage workers have a greater incentive to litigate, and attorneys working on a contingency basis have a greater incentive to take their cases.[43] Because the protections offered by wrongful-discharge doctrines are not equally distributed among worker groups, we explore here whether the employment impacts also differ among demographic subgroups, defined by gender, education, and age. In table 5, we take two cuts at the estimation. Panel A presents employment impacts in years 1 and 2 following adoption (that is, 13–36 months after adoption—our baseline specification). Panel B presents longer-term results for employment effects in years 4 and 5 following adoption.

The results in panel A for short-term impacts confirm that the implied-contract doctrine appears to reduce employment rates for almost all the identified demographic groups. But the effect is not uniform across groups. The largest impacts are found for less-educated (high school or less) workers, with impacts ranging from -1.6 to -1.7 for men and from -2.2 to -2.6 for women. In addition to the implied-contract effects, we find some limited evidence (large point estimates and large standard errors) that the good-faith exception also reduces employment rates. But this impact only appears robust for older women.

These short-term results are consistent with OECD studies that find that employment protections tend to differentially harm employment of females, less-experienced workers, and less-skilled workers (Bertola, Blau, & Khan, 2002; OECD, 1999). Yet these results appear something of a puzzle in the U.S. context. Because the wrongful-discharge doctrines studied here increase the expected cost of employing high-tenure, high-wage workers, these laws should, over the longer term, lower the employment and earnings of

[41] The business press likely contributed to the sense of alarm. A 1985 *Business Week* cover article entitled "The Revolution in Employee Rights" stated, "To minimize liability, corporations have to treat each dismissal as though it were under a 'just cause' provision of a contract" (Hoerr et al., 1985). Even under the broadest reading of the case law in 1985, this statement would have been true in only the seven states that recognized the good-faith exception.

[42] One further concern is that if recent U.S. immigrants are unlikely to take advantage of employment protections, the results might be weakened by large concentrations of immigrant workers in certain states. To explore this concern, we reestimated all models in table 1 for employment and earnings excluding the six high-immigration states that contain the majority of the nation's total foreign-born population: CA, FL, IL, NY, NJ, and TX. These results are qualitatively identical to the main table 1 findings (table available from the authors).

[43] Dertouzos, Holland, and Ebener (1988) find that plaintiffs in wrongful-discharge cases typically are male (69%), hold executive or managerial positions (53%), have 6 or more years of tenure (48%), and earn considerably above the median wage.

TABLE 5.— ESTIMATES OF THE IMPACT OF WRONGFUL-DISCHARGE LAWS ON LOG EMPLOYMENT-TO-POPULATION RATIOS BY GENDER, AGE, AND EDUCATION SUBGROUPS, 1978–1999*

	Males				Females			
	≤ High School		≥ Some College		≤ High School		≥ Some College	
	18–39	40–64	18–39	40–64	18–39	40–64	18–39	40–64
Doctrine	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Years 2,3 Following Adoption Relative to 2 Years Prior to Adoption								
Implied	−1.60	−1.73	−0.63	−0.68	−2.17	−2.62	−1.39	0.23
contract	(0.81)	(0.44)	(0.40)	(0.55)	(0.89)	(0.91)	(0.55)	(0.62)
Public policy	0.20	−0.50	0.04	−0.24	−0.17	0.31	−1.22	0.24
	(0.84)	(0.65)	(0.44)	(0.59)	(0.86)	(0.89)	(0.52)	(1.04)
Good faith	−2.69	1.07	0.22	0.78	−1.88	−0.22	−1.13	−3.50
	(1.47)	(1.10)	(0.52)	(0.56)	(1.83)	(1.36)	(0.73)	(1.37)
R^2	0.702	0.643	0.621	0.505	0.782	0.761	0.697	0.684
n	10,465							
B. Years 4,5 Following Adoption Relative to 2 Years Prior to Adoption								
Implied contract	−1.09	−1.53	−0.43	−0.72	−0.70	−3.63	−0.57	−1.79
	(0.93)	(0.65)	(0.41)	(0.62)	(1.01)	(1.52)	(0.71)	(0.86)
Public policy	0.21	−0.76	0.29	−0.81	0.74	0.47	−0.83	0.94
Public policy	(0.70)	(0.75)	(0.47)	(0.73)	(1.22)	(1.25)	(0.76)	(1.00)
Good faith	−1.96	0.95	0.45	0.87	−2.04	−0.80	−1.32	−0.71
Good faith	(1.07)	(1.37)	(0.64)	(0.84)	(2.03)	(2.25)	(1.41)	(1.79)
R^2	0.667	0.614	0.614	0.511	0.782	0.776	0.705	0.719
n	10,497							

*Dependent variable: 100 × ln(employment/population).
Huber-White robust standard errors in parentheses allow for unrestricted error correlation within each state. Separate regressions in each column contrast employment of the specified demographic group in years 2 and 3 following adoption of a doctrine (panel A) or years 4 and 5 following adoption (panel B) with respect to the 2 years immediately prior to adoption of the doctrine. All models include state and year dummies and region × year dummies. Samples, specifications, and weights are identical to table 2, column 4.

protected groups and raise the demand for workers who are close substitutes—low-wage and short-tenure employees who are unlikely to (successfully) litigate.[44]

Panel B of table 5 examines the evidence for longer-term impacts. Notably, longer-term impacts for younger and less educated workers appear less negative than the short-term impacts presented in panel A, whereas longer-term impacts for older and better-educated workers appear more negative. In fact, for both sexes and both education categories, the point estimate for the employment reduction among older workers is larger than for younger workers. This suggests that the larger short-term impacts for low-wage workers seen in panel A may be explained by their high employment flow rates; reductions in hiring will first reduce employment of groups who enter and exit employment frequently.[45] But this discrepancy appears transitory. Over the longer term, negative employment consequences appear to accrue for those most protected by the wrongful-discharge doctrines. Though we lack sufficient precision to conclude that high-wage workers were differentially harmed, there is no evidence that the long-term employment costs were disproportionately borne by low-wage workers.

VI. Robustness Tests: Alternative Data Sources and Outcome Measures

Our analysis so far relies exclusively on the CPS to measure employment outcomes. This presents two limitations. One is that the CPS does not span the entire time period of interest for our study. The second is that though the CPS is ideal for measuring employment levels, it is not suitable for analyzing worker flows, which should also be affected by employment protections. We address both of these limitations here.

A. *Employment Estimates Using Establishment-Based Data*

Although most precedent-setting wrongful-discharge cases were decided in the 1980s, some state courts adopted public policy, implied-contract, and good-faith exceptions before then (in 1959, 1976, and 1974 respectively). The monthly CPS employment data series, which begins in 1978, does not cover these early adoptions. A second limitation of the CPS, as a household survey, is that it may not provide as precise an estimate of state employment levels as an establishment-based survey. To partly rectify both limitations, we supplement the CPS estimates with data from the

[44] This may indeed be what occurred with the surge in demand for temporary help employment in states adopting the implied-contract exceptions (Miles, 2000; Autor, 2003).

[45] Also notably, the point estimates for longer-term employment effects are larger for females than males. We do not believe this pattern reflects gender differences in litigiousness. A 1988 study by Dertouzos, Holland, and Ebener found that women were 31% of California wrongful-discharge plaintiffs between 1980 and 1986. Our Current Population Survey data indicate that 44% of California workers were women in those years.

Current Employment Statistics (CES) for the years 1970 to 1999.

The CES, collected by the Bureau of Labor Statistics (BLS), is drawn from a probability sample of approximately 350,000 establishments. Although these data are collected monthly, new establishments enter the data with a significant lag. To compensate for the undercount, BLS applies bias-adjustment factors in each month and rebenchmarks the CES totals to national employment in March of each year.[46] For our purposes, these bias adjustments have the potential to undermine our state-by-month estimation strategy used above if they obscure the response to the legal shock that we try to discern in the monthly data. In other words, the BLS adjustments may convey a picture of false stability in the employment data that could induce strong consistency bias over short time intervals. To address this concern, we assemble month-of-March employment data from the CES to form an annual state-by-year employment count panel for 1970 through 1999. In so doing, we lose the benefit of the monthly analysis that we employed on the CPS data, while gaining the advantage of an establishment-based data set covering a longer span of years.

Using the CES data, we estimate the following difference-in-differences model for the natural logarithm of state employment:

$$\ln Emp_{srt} = \beta_7 L_{st} + \gamma_s + \delta_t\lambda_r + \gamma_s t + \gamma_s t^2 + \varepsilon_{srt}, \qquad (3)$$

where L_{st} is a vector of wrongful-discharge law dummies that assume the value of 1 in the year following adoption and after, and δ, γ, and λ are vectors of year, state, and region dummies (indicated by subscripts t, s, and r). To allow for pronounced differential cross-state and cross-region employment trends (Blanchard & Katz, 1992), our preferred specification also controls for quadratic state trends and interactions between four region dummies and individual calendar-year dummies.

The first column of table 6 presents a model for state employment for the years 1970 through 1999 estimated with the CES data. Adoption of an implied-contract exception is associated with a reduction in state employment of 2.6%, which is statistically significant ($t = 2.2$) and almost twice as large as our main estimates (tables 1 and 2). There is reason to treat this estimate with caution, however: the same model also suggests that the good-faith doctrine raised state employment levels by an implausibly large 7.5% ($t = 4.8$). This suggests the possibility of confounding state employment trends, a point we explore in greater detail in our companion paper, ADS. To control for these trends, column 2 of the table adds quadratic state trends and region×year dummies. These variables reduce the magnitude of the implied-contract effect to -1.0 percentage points ($t = 2.5$),

[46] Details on the sampling methods of the CES are found at http://www.bls.gov/sae/790meth.htm (accessed August 21, 2004).

TABLE 6.—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE IMPACT OF WRONGFUL-DISCHARGE LAWS ON EMPLOYMENT LEVELS, 1970–1999: ANNUAL ESTIMATES FROM THE CURRENT POPULATION SURVEY AND CURRENT EMPLOYMENT STATISTICS*

	A. Current Employment Statistics							B. Comparison of Current Employment Statistics and Current Population Survey Estimates, 1978–1999							
	1970–1999				1970–1988			Current Employment Statistics				Current Population Survey			
	All		Manuf.	Nonman.	All	Manuf.	Nonman.	All		Manuf.	Nonman.	All		Manuf.	Nonman.
Doctrine	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(1)	(2)	(3)	(4)	(5)	(6)	(8)	(8)
Implied contract	−2.65	−1.03	−1.06	−1.06	−0.77	−1.18	−0.68	−2.36	−1.14	−1.31	−1.09	−3.94	−1.90	−3.62	−1.36
	(1.20)	(0.41)	(0.54)	(0.42)	(0.48)	(0.66)	(0.48)	(0.94)	(0.52)	(0.61)	(0.52)	(0.94)	(0.48)	(1.21)	(0.51)
Public policy	1.23	−0.12	0.34	−0.19	0.09	−0.18	0.14	4.85	−0.38	0.07	−0.40	3.10	−1.33	0.08	−1.68
	(1.35)	(0.48)	(0.64)	(0.47)	(0.44)	(0.66)	(0.44)	(0.92)	(0.55)	(0.66)	(0.57)	(0.97)	(0.52)	(1.22)	(0.60)
Good faith	7.49	1.59	1.52	1.39	0.14	1.41	−0.32	6.33	−1.17	−0.55	−1.62	9.33	−0.78	0.24	−1.10
	(1.56)	(1.02)	(1.16)	(1.02)	(0.64)	(0.93)	(0.66)	(1.58)	(0.94)	(1.17)	(0.95)	(1.69)	(0.98)	(2.17)	(1.17)
R^2	0.990	0.999	0.999	0.999	1.000	0.999	1.000	0.996	1.000	0.999	1.000	0.996	1.000	0.997	0.999
State trends + region × year dummies	No	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
n	1,500				950			1,100				1,100			

*Dependent variable: 100 × ln(state employment).
Huber-White robust standard errors in parentheses allow for unrestricted error correlations by state. All models include include state and year dummy variables. As indicated, models also include quadratic state × time interactions and interactions between four Census geographic regions and individual year dummies. Dependent variables is 100 × the natural logarithm of estimated state employment in the indicated years and sectors. Current Employment Statistics data are month-of-March state employment estimates. Current Population Survey data are calculated from complete CPS monthly files for January–May (centered on March) of each calendar year. Dummies for adoption of legal doctrines are lagged by 1 year. All estimates weighted by state's share of national population aged 16–64 in each year.

similar to our main estimates. The good-faith and public policy doctrines are now insignificant.[47]

We reestimate the column 2 model separately for manufacturing (column 3) and nonmanufacturing (column 4) employment. In both sectors, the implied-contract doctrine reduces employment levels by approximately 1% (significant at the 5% level). Neither of the other two wrongful-discharge laws is significant, and the sign for the public policy doctrine is inconsistent.

To further test the comparability of the CES and CPS results, we estimate a set of employment models using each for the time interval for which both are available: 1978 to 1999. To increase comparability, we form an annual state-level employment count using the CPS centered on March of each calendar year.[48] These models, in panel B of table 6, yield highly comparable effects of the impact of wrongful-discharge doctrines on state employment levels. After controlling for state trends and region effects, we find that the implied-contract doctrine is associated with an employment decline of 1.1 to 1.9 percentage points overall, but with a larger point estimate for manufacturing using the CPS data (column 8). In the specification controlling for employment trends, the good-faith doctrine is never significant, whereas the public policy doctrine is occasionally negative and significant.

We emphasize that the state × year estimation methodology in table 6 is less satisfactory than our short-panel approach in previous tables. In particular, the variation exploited has strong serial correlation and may be confounded with state and regional employment trends—issues that we addressed above by varying the pre- and posttreatment interval, contrasting short- and long-term impacts, controlling flexibly for regional effects, and examining multiple subperiods of the data. Nevertheless, the CES results increase our confidence in the main findings.

B. *Evidence on Employment Flows*

As discussed in section I, theory makes ambiguous predictions about the short-run effect of wrongful-discharge laws on employment levels. Protection that does not satisfy Coasean efficiency should lower wages or employment or both in the long run. But in the short run, firing restrictions can either raise or lower employment, because they reduce incentives to both hire and fire. Regardless of whether firing restrictions raise or lower employment levels, they should unambiguously reduce worker flows into and out of jobs. Hence, we briefly explore here how wrongful-discharge laws affect employment flows.

Our CPS data, formed from repeated cross sections of households, are not suitable for this analysis.[49] As an imperfect substitute, we exploit state-level employment flow data from the Longitudinal Research Database (Davis, Haltiwanger, & Schuh, 1996). The state-level LRD sample is available only for 1973 through 1988 and only for the manufacturing sector. A further limitation of the LRD it that it does not measure true employment flows—that is, the count of workers exiting and entering jobs. Instead, it measures the sum of job losses at contracting establishments (*job destruction*) and the sum of job gains at expanding establishments (*job creation*), each normalized by total manufacturing employment.

To examine the effects of wrongful-discharge laws on job creation and destruction, we estimate a difference-in-differences model,

$$J_{st} = \sum_{\tau=0}^{3} \xi_\tau L_{st-\tau} + \xi_4 \cdot I[t \geq (LawYR_s + 4)] + \delta_t + \gamma_s + \varepsilon_{st}, \qquad (4)$$

where the dependent variable is the job-flow measure for manufacturing employment in state s over years t to $t + 1$, and L_{st} is a vector of wrongful-discharge doctrine dummies that assume the value of 1 in the year a law is adopted, and the variable $LawYR_s$ equals the year of a state's adoption. Vectors of time and state dummies, δ and γ, control respectively for aggregate shocks and mean cross-state differences in the rate of job creation or destruction. All models are weighted by average state shares of U.S. manufacturing employment over 1973 to 1988.

Prior to estimating equation (4), we tabulate in the appendix (table A3) benchmark estimates of the state-level relationship between job creation and destruction and employment growth in manufacturing. Despite the limitations noted, the job-flow measures capture a substantial share of the variation in manufacturing employment over time: 1 percentage point of job creation predicts employment growth of 0.7 log points ($t = 16$), and 1 percentage point of job destruction predicts an employment decline of 0.8 log points ($t = 24$).[50]

Panel A of table 7 presents estimates of equation (4) for job destruction. The initial model finds some evidence that adopting a wrongful-discharge doctrine reduces manufacturing job destruction. Specifically, job destruction in the first three years following adoption of *any* wrongful-discharge law is between 0.2 and 0.6 percentage points

[47] Because of the 30-year time span in these specifications, we control flexibly for time trends using quadratic rather than (just) linear trends. If instead we only use linear trends, the implied-contract coefficients are unaffected, but the good-faith coefficients remain significantly positive in some specifications.

[48] Specifically, we form a centered average on March of each year using CPS data for January through May.

[49] Though the CPS can be used to track a subset of households over one calendar year, the matched samples are problematic: job losers are disproportionately likely to change residences and therefore exit the sample (Welch, 1993; Madrian & Lefgren, 2000).

[50] The estimates in table A3 are from a variant of equation (4) in which the dependent variable is the state-level first difference in log manufacturing employment. Job creation and destruction are included on the right side of the equation, and other control variables are as above.

TABLE 7.—ESTIMATED IMPACT OF WRONGFUL-DISCHARGE LAWS ON ANNUAL JOB FLOWS IN MANUFACTURING, 1973 TO 1988*

Time relative to (years) law adoption	A. Job Destruction				B. Job Creation				C. Gross Flows: Job Destruction + Job Creation			
	(1)	(2)			(1)	(2)			(1)	(2)		
	Any	Implied Contract	Public Policy	Good Faith	Any	Implied Contract	Public Policy	Good Faith	Any	Implied Contract	Public Policy	Good Faith
0	−0.44	0.25	−0.69	0.32	0.23	0.08	0.45	−1.29	−0.21	0.33	−0.24	−0.98
	(0.40)	(0.34)	(0.44)	(0.37)	(0.29)	(0.27)	(0.26)	(0.69)	(0.47)	(0.39)	(0.47)	(0.82)
1	−0.21	0.17	0.01	1.09	−0.19	−0.35	−0.14	−1.06	−0.39	−0.18	−0.13	0.02
	(0.48)	(0.47)	(0.58)	(0.37)	(0.46)	(0.40)	(0.34)	(0.65)	(0.53)	(0.33)	(0.65)	(0.59)
2	−0.63	−0.77	−0.17	−0.56	0.31	−0.10	0.35	−0.39	−0.32	−0.87	0.18	−0.95
	(0.47)	(0.59)	(0.51)	(0.89)	(0.45)	(0.39)	(0.36)	(0.60)	(0.41)	(0.55)	(0.46)	(0.55)
3	0.08	−0.71	1.00	0.27	−0.50	−0.50	−0.58	−1.10	−0.42	−1.22	0.41	−0.83
	(0.85)	(0.57)	(0.91)	(0.78)	(0.48)	(0.41)	(0.50)	(0.77)	(0.50)	(0.38)	(0.53)	(0.51)
R^2	0.755		0.762		0.774		0.783		0.742		0.751	

*Dependent variable: percentage-point changes in state manufacturing employment at contracting and expanding plants.
n = 752 observations (47 states × 16 years). Huber-White robust standard errors in parentheses allow for unrestricted error correlations within each state. Each numbered column is from a separate OLS regression of manufacturing employment flows by state and calendar year on leads and lags of wrongful-discharge law adoption. All models include state and year dummies and a dummy indicating ≥4 years following law adoption. Job creation (destruction) is the absolute value of the employment-weighted mean percentage-point change in employment in manufacturing plants experiencing employment increases (declines). All estimates are weighted by state mean share of national manufacturing employment over 1973–1988. Alaska, Rhode Island, Hawaii, and the District of Columbia are excluded from estimates. Data are from Davis, Haltiwanger, and Schuh (1996), and are available for download at http://www.bsos.umd.edu/econ/haltiwanger/download.htm.

lower than prior to law adoption, though these point estimates are not significant. Column 2 replaces the any-law variables with separate indicator variables for each of the three wrongful-discharge doctrines. Here, a somewhat stronger pattern emerges. Job destruction declines noticeably—by around 0.7 to 0.8 percentage points—in years 2 and 3 following adoption of the implied contract exception, though again, the point estimates are not significant at conventional levels. There is no evidence of a decline in job destruction in the years following adoption of either the public policy or good-faith doctrines.

Panels B and C of table 7 repeat these estimates for job creation and for gross job flows, the latter of which is the sum of job creation and job destruction. In years 1 through 3 following adoption of an implied-contract exception, there is some evidence of a slowdown in job creation and very strong evidence of a reduction in gross job flows. The final estimate indicates a sizable 1.2-percentage-point reduction in gross job flows in the third year following adoption of the implied-contract exception (t = 3.2). The good-faith doctrine is also associated with a significant reduction in job creation and a marginally significant decline in gross job flows—but this result did not prove robust to inclusion of four region-by-year dummy variables (not shown), and hence we are not confident of its validity.[51] For the public policy doctrine, no clear pattern emerges.

Do these estimates support the inference that wrongful-discharge laws reduced job flows? In the case of the implied-contract doctrine, the answer appears to be a qualified yes. In the years immediately following adoption of this doctrine, job creation appears to slow (albeit not significantly), followed in years 2 and 3 by a significant reduction in job destruction. Consistent with the evidence in table 4, these estimates imply a dip in employment followed by a moderate employment rebound. It bears emphasis that these job-flow results do not correspond perfectly to our main estimates; the estimated 0.5-percentage-point slowdown in job creation is not large enough to account for the 0.9-percentage-point reduction in manufacturing employment estimated for the comparable time period (table 6, column 6). Given the many sources of slippage in the LRD data, however, we believe this evidence supports the main results.[52]

VII. Conclusion

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 & Karoly, 1992) and those who find that the exceptions have had no impact (Miles, 2000). We find a statistically significant negative impact on employment, but it emanates from only one of the legal exceptions—the implied-contract doctrine—and its adoption causes a decline of 0.8% to 1.7% in the ratio of employment to population, which is between one-third and one-fifth the estimated magnitude offered by Dertouzos and Karoly (1992). Although the matter can never be free from doubt in statistical studies of this kind, the robustness of our findings across specifications, demographic groups, time periods, and data sources suggests that our findings reflect a causal effect of adoption of the implied-contract exception.

We stress that our paper does not attempt to provide an overall assessment of wrongful-discharge laws. We have not offered any evaluation of the benefits of such laws to workers and the public. The fact that there is some reduction

[51] A table of results is available from the authors.

[52] Complementing this evidence, Kugler and Saint-Paul (2004, tables 3, 4) find that adoption of wrongful-discharge doctrines—particularly the implied-contract and good-faith exceptions—significantly slowed the rate of job accession for unemployed workers in adopting states. This supports the conclusion that adoption of these doctrines dampened labor market flows.

in employment—for women, younger workers, and less-educated men in the short term, and potentially for older and more educated workers in the longer term—underscores that legal protections do not come without cost.

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 reduces employment when it would seem that simple changes to personnel policies 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 prediction of the Coase theorem frequently does not obtain in labor markets (Donohue, 1989). Still, the evidence that the depressing employment impact of the implied-contract doctrine dissipated after six or seven years may suggest that over time employers were able to circumvent the costs of the law or came to realize that these costs would be small. Part of the reason for the initial drop in employment might have been uncertainty about how far courts would push these exceptions, so that it took time for that information to be revealed and for employers to contract around the exception (which they could certainly do more readily with respect to new hires than with incumbent workers).

Our finding that the implied-contract exception generated at least short-term employment drops without corresponding drops in wages merits discussion. A simple supply-and-demand model would suggest that by raising total employment costs, adoption of the implied-contract doctrine should have caused an inward shift in labor demand, leading to lower employment *and* wage levels. Moreover, if workers valued the protection provided by this doctrine, total labor supply should have shifted outward, mitigating the employment effect but augmenting the wage effect. In other words, the observed drop in employment suggests a backward demand shift, which should have lowered wages, and any supply stimulus should only have accentuated the wage drop.

Why then did wages not fall, even during the period when employment fell? A number of possibilities must be considered. First, an outward supply shift that would accentuate a drop in wages probably did not occur, because workers did not greatly value the benefit of the implied-contract exception. This could occur if the expected benefit to the worker was in fact low, perhaps because much of the money changing hands in wrongful-discharge cases would be paid to attorneys. Alternatively, workers might not perceive a benefit from such judicial decisions because, as considerable evidence suggests, they tend to believe that they already are protected against unjust dismissal, even when they clearly are not. According to Kim (1997), "workers consistently overestimate their legal rights, with overwhelming majorities (as high as 89%) believing that they are legally protected against arbitrary and unjust discharges when in fact they can be dismissed at will."

A second possibility is that the violation of the predictions of the simple supply-and-demand framework in this context is more fundamental. In contrast to this framework, standard flow models of the labor market imply that employment protections raise wages (and reduce employment) by increasing workers' bargaining power (cf. Blanchard & Portugal, 2001). The logic of this argument is that firing costs induce employers to accept higher wage demands because the alternative of laying off workers who are pushing for higher wages would trigger the firing cost. Hence, in aggregate, employment protection creates two countervailing effects on wage levels: by shifting labor demand inward, it puts downward pressure on wages; by providing incumbent workers with enhanced bargaining power, it exerts upward pressure. According to the evidence presented here, the net effect of these two influences for recent wrongful-discharge protections adopted in the United States is to lower employment modestly while leaving overall wage levels unchanged.

REFERENCES

Acemoglu, Daron, and Joshua Angrist, "Consequences of Employment Protection? The Case of the Americans with Disabilities Act," *Journal of Political Economy* 109:5 (2001), 915–957.

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

Autor, David H., "Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing," *Journal of Labor Economics* 21:1 (2003), 1–42.

Autor, David H., John Donohue III, and Stewart J. Schwab, "The Employment Consequences of Wrongful-Discharge Laws: Large, Small, or None at All?" *American Economic Review Papers and Proceedings* 93:2 (2004), 440–446.

Bertola, Giuseppe, Francine Blau, and Lawrence Kahn, "Labor Market Institutions and Demographic Employment Patterns." European University Institute mimeograph (2002).

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, "How Much Should We Trust Difference-in-Differences Estimates?" *Quarterly Journal of Economics* 119:1 (2004), 249–275.

Blanchard, Olivier Jean, and Lawrence F. Katz, "Regional Evolutions," *Brookings Papers on Economic Activity* 1 (1992), 1–75.

——— "What We Know and Do Not Know About the Natural Rate of Unemployment," *Journal of Economic Perspectives* 11 (1997), 51–72.

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

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

Chandra, Amitabh, "Is the Convergence in the Racial Wage Gap Illusory?" NBER working paper no. 9476 (2003).

Chay, Kenneth Y., "The Impact of Federal Civil Rights Policy on Black Economic Progress: Evidence from the Equal Employment Opportunity Act of 1972," *Industrial & Labor Relations Review* 51:4 (1998), 608–632.

Davis, Steven J., John C. Haltiwanger, and Scott Schuh, *Job Creation and Destruction* (Cambridge, MA: MIT Press, 1996).

DeLeire, Thomas, "The Wage and Employment Effects of the Americans with Disabilities Act," *Journal of Human Resources* 35:4 (2000), 693–715.

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

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

——— "Employment Effects of Worker Protection: Evidence from the United States," in Christoph F. Buechtemann (Ed.), *Employment Security and Labor Market Behavior: Interdisciplinary Approaches and International Evidence* (Ithaca, NY: ILR Press, 1993).

Donohue, John J., III, "Diverting the Coasean River: Incentive Schemes to Reduce Unemployment Spells," *Yale Law Journal* 99 (1989), 549–609.

——— "Did Miranda Diminish Police Effectiveness?" *Stanford Law Review* 50 (1998), 1147–1178.

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

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

Fishback, Price V., and Shawn Everett Kantor, "Did Workers Pay for the Passage of Workers' Compensation Laws?" *Quarterly Journal of Economics* 110:3 (August 1995), 713–742.

Gruber, Jonathan, "The Incidence of Mandated Maternity Benefits," *American Economic Review* 84:3 (1994), 622–641.

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

Hoerr, John, William G. Glaberson, Daniel B. Moskowitz, Vicky Cahan, Michael A. Pollaock, and Jonathan Tasini, "Beyond Unions: A Revolution in Employment Rights Is in the Making," *Business Week* (July 8, 1985), 72–77.

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

Jolls, Christine, and J.J. Prescott, "The Effects of 'Reasonable Accommodations' Requirements and Firing Costs on the Employment of Individuals with Disabilities," Harvard Law School mimeograph (2004).

Kim, Pauline, "Bargaining with Imperfect Information: A Study of Worker Perceptions of Legal Protection in an At-Will World," *Cornell Law Review* 83:1 (1997), 105–160.

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

Krueger, Alan B., and Jörn-Steffen Pischke, "Observations and Conjectures on the U.S. Employment Miracle" (pp. 99–126), in *Third Public GAAC Symposium: Labor Markets in the USA and Germany* (Bonn: German-American Academic Council, 1998).

Kugler, Adriana, and Gilles Saint-Paul, "How Do Firing Costs Affect Worker Flows in a World with Adverse Selection?" *Journal of Labor Economics* 22:3 (2004), 553–584.

Lazear, Edward P., "Job Security Provisions and Employment," *Quarterly Journal of Economics* 105:3 (1990), 699–726.

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

Madrian, Brigitte C., and Lars Lefrgen, "An Approach to Longitudinally Matching Current Population Survey (CPS) Respondents," *Journal of Economics and Social Measurement* 26:1 (2000), 31–62.

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

Morriss, Andrew P., "Exploding Myths: An Empirical and Economic Reassessment of the Rise of Employment-At-Will," *Missouri Law Review* 59 (1994), 679–771.

——— "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 (1995), 999–1148.

Neal, Derek A. "The Measured Black-White Wage Gap among Women Is Too Small," *Journal of Political Economy* 112:1, Part 2 Supplement (2004), S1–S28.

Neal, Derek A., and William R. Johnson, "The Role of Premarket Factors in Black-White Wage Differences," *Journal of Political Economy* 104:5 (1996), 869–895.

OECD Employment Outlook: 1999 (Paris: OECD, 1999).

OECD Employment Outlook: 2004 (Paris: OECD, 2004).

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

——— "Litigation Costs and Returns to Experience," *American Economic Review* 92:3 (2002), 683–705.

Schwab, Stewart J., "Life-Cycle Justice: Accommodating Just Cause and Employment at Will," *Michigan Law Review* 95:1 (1993), 8–58.

——— "Wrongful-Discharge Law and the Search for Third-Party Effects," *Texas Law Review* 74:7 (1996), 1943–1978.

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

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

Welch, Finis, "Matching the Current Population Surveys," *Stata Technical Bulletin* 12 (1993), 7–11.

APPENDIX

TABLE A1.—WRONGFUL-DISCHARGE LAWS BY REGION, STATE, AND YEAR*

	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982
New England													
Connecticut											P1 G6	P G	P G
Maine								C11	C	C	C	C	C
Massachusetts								G7	G	G	P5 G	P G	P G
New Hampshire					P2 G2	P G	P G	P G	P G	P G	P G5	P	P
Rhode Island													
Vermont													
Middle Atlantic													
New Jersey											P7	P	P
New York													C11
Pennsylvania					P3	P	P	P	P	P	P	P	P
East North Central													
Illinois					C12	C	C	C	C P12	C P	C P	C P	C P
Indiana				P5	P	P	P	P	P	P	P	P	P
Michigan							P6	P	P	P	C6 P	C P	C P
Ohio													C4
Wisconsin											P1	P	P
West North Central													
Iowa													
Kansas												P6	P
Minnesota													
Missouri													
Nebraska													
North Dakota													
South Dakota													
South Atlantic													
Delaware													
Florida													
Georgia													
Maryland												P7	P
North Carolina													
South Carolina													
Virginia													
West Virginia									P7	P	P	P	P
East South Central													
Alabama													
Kentucky													
Mississippi													
Tennessee												C11	C
West South Central													
Arkansas											P3	P	P
Louisiana													
Oklahoma							C12	C	C	C	C	C	C
Texas													
Mountain													
Arizona													
Colorado													
Idaho								C4 P4	C P	C P	C P	C P	C P
Montana											P1	P	P G1
Nevada													
New Mexico											C2	C	C
Utah													
Wyoming													
Pacific													
Alaska													
California	P	P	C3 P	C P	C P	C P	C P	C P	C P	C P	C P G10	C P G	C P G
Hawaii													P10
Oregon						P6	P	P	C3 P	C P	C P	C P	C P
Washington								C8	C	C	C	C	C

*Key: C, implied contract; P, public policy; G, good faith. (Month of adoption or removal indicated by numbers 1–12.)
Source: Authors' analysis of case law.

TABLE A1.—(CONTINUED)

1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999
P G	P G	C10 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	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C
P G	P G	P G	P G	P G	C5 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
P	P	P	P	P	C8 P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
		C8	C P9	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
P	P	C5 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	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C	C
P	P	P	P	P	P	P	P	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	C P	C P	C P	C P
P	P	P	P	C8 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	C P	C P	C P	C P	C P	C P	C P	C P
C	C	C	C	C	C	C	C P3	C P	C P	C P	C P	C P	C P	C P	C P	C P
P	P	C6 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
		P7	P	C11 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
P	C8 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
C4	C	C	C P11	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
C1	C	C P11	C P	C P	C2 P	P	P	P	P	P	P	P	P	P	P	P
C11	C	C	C	C P11	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
	C2	C	C	C P11	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
C4	C	C	C	C	C P12	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
									P3 G4	P G	P G	P G	P G	P G	P G	P G
P	P	C1 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
		P5	P	P	P	P	P	P	P	P	P	P	P	P	P	P
		P11	P	C6 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
C9	C	C P6	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
P	P	P	C4 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
				C7	C	C	C	C	C	C	C	C	C	C	C	C
C8 P11	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
				P7	P	P	P	P	C6 P	CP	CP	C P	C P	C P	C P	C P
C	C P8	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
P	C6 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
															G1	G
C	C	C G5	C G	C G	C G	C P2 G2	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
	P6	C4 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
C6	C4	P6 G6	P G	P G	P G	P G	P G	P G	P G	P G	P G	P G	P G	P G	P G	P G
C10	C	C P9	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 G8	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
P G	P G	P G	P G	C6 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
C8	C P1	C P	C P	C P G2	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 P7	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
			C5	C	C	C P3	C P	C P	C P	C P	C P	C P	C P	C P	C P	C P
		C8	C	C	C	C P7	C P	C P	C P	C P	C P G1	C P G	C P G	C P G	C P G	C P G
C5 G5	C G	C G	C P2 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	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
P	P	P	C8 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	C P	C P	C P	C P	C P	C P	C P	C P	C P
C	C P7	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

*Key: C, implied contract; P, public policy; G, good faith. (Month of adoption indicated by numbers 1–12.)
Source: Authors' analysis of case law.

TABLE A2.—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE IMPACT OF WRONGFUL-DISCHARGE LAWS ON STATE EMPLOYMENT-TO-POPULATION RATIO AND HOURLY EARNINGS, 1978–1999: MODELS EXCLUDING PREVIOUSLY TREATED STATE-MONTH OBSERVATIONS FROM THE CONTROL SAMPLE

	A. 100 × ln(Employment/Population): 1978–1999						B. 100 × ln(Hourly Wage): 1979–1999					
	All Employment		Manufacturing		Nonmanufacturing		All Employment		Manufacturing		Nonmanufacturing	
Doctrine	(1)	(2)	(3)	(4)	(1)	(2)	(1)	(2)	(3)	(4)	(1)	(2)
Implied	−1.83	−1.57	−2.97	−2.56	−1.29	−1.20	0.55	0.55	0.45	0.53	0.65	0.57
contract	(0.58)	(0.44)	(1.75)	(1.48)	(0.84)	(0.51)	(0.84)	(0.73)	(0.72)	(0.58)	(0.95)	(0.81)
R^2	0.870	0.894	0.949	0.954	0.928	0.947	0.235	0.235	0.326	0.327	0.218	0.219
n	5,404		5,404		5,404		1,402,130		303,697		1,098,433	
Public policy	−0.30	−0.04	1.37	0.18	−0.68	−0.03	−0.71	−0.48	0.14	0.11	−1.00	−0.77
	(0.81)	(0.62)	(1.91)	(1.69)	(0.90)	(0.63)	(0.58)	(0.57)	(0.74)	(0.53)	(0.63)	(0.63)
R^2	0.827	0.865	0.947	0.955	0.915	0.938	0.236	0.237	0.327	0.327	0.221	0.221
n	5,640		5,640		5,640		1,565,684		330,649		1,235,035	
Good faith	−0.33	−0.44	5.94	2.13	−1.93	−0.29	−1.34	−0.45	−2.21	−1.67	−1.35	−0.30
	(0.63)	(0.81)	(1.93)	(2.20)	(0.83)	(1.02)	(1.43)	(1.75)	(1.19)	(1.42)	(1.59)	(1.79)
R^2	0.872	0.897	0.930	0.943	0.920	0.938	0.230	0.230	0.311	0.312	0.217	0.218
n	6,832		6,832		6,832		1,815,262		371,884		1,443,378	
Region × year dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Samples and models are identical to table 1 except that state-month observations from states that have already adopted a law during the sample period are not used in the control-state sample in months ≥37 following law adoption. For details, see notes to table 1 and footnote 29 in the text.

TABLE A3.—RELATIONSHIP BETWEEN JOB CREATION AND DESTRUCTION AND EMPLOYMENT IN MANUFACTURING, 1973 TO 1988*

	(1)	(2)	(3)	(4)	(5)	(6)
Job creation	0.74	0.73				
	(0.05)	(0.05)				
Job destruction	−0.83	−0.81				
	(0.03)	(0.04)				
Gross: creation + destruction			−0.45	−0.45		
			(0.06)	(0.06)		
Net: creation − destruction					0.79	0.78
					(0.02)	(0.02)
R^2	0.927	0.939	0.793	0.845	0.926	0.939
Region × year dummies	No	Yes	No	Yes	No	Yes

*Dependent variable: 100 × Δ ln(state manufacturing employment).

n = 752 observations (47 states × 16 years). Huber-White robust standard errors in parentheses allow for unrestricted error correlations within each state. Each numbered column is from a separate OLS regression of 100 times the annual change in log state manufacturing employment on job creation (destruction), defined as the absolute value of the employment-weighted mean percentage-point change in employment in manufacturing plants experiencing employment increases (declines). All models include state and year dummies. Models in even-numbered columns additionally contain interactions between four Census geographic regions and calendar year dummies. Estimates are weighted by state mean share of national manufacturing employment over 1973–1988. Alaska, Rhode Island, Hawaii, and the District of Columbia are excluded from estimates. Data are from Davis, Haltiwanger, and Schuh (1996).