STATE LEGAL VARIATION AND ECONOMIC OUTCOMES[†]

The Employment Consequences of Wrongful-Discharge Laws: Large, Small, or None at All?

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Uniquely in the industrialized world, the United States has long had the presumption that employers may legally fire workers "at will," that is, "for good cause, bad cause, or no cause at all."[1] During the 1970's and 1980's, 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.[2]

Briefly summarized: the "public policy" exception prevents employee discharges that would thwart an important public policy, for example, performing jury duty, filing a worker's compensation claim, reporting an employer's wrongdoing, or refusing to commit perjury. The "good faith" exception prohibits employers from firing workers to deprive them of earned benefits, such as sales commissions or pension bonuses. The "implied contract" exception makes informal employer assurances of ongoing employment, such as those found in personnel manuals or promotion letters, legally enforceable. Under the implied-contract exception, an employer implicitly offering ongoing employment can only terminate a worker for good cause.

Understanding the economic consequences of these doctrines is essential to an evaluation of the costs of using litigation to protect "employment rights." Fortunately for empirical analysis, states vary greatly in the timing and extent of adoption of wrongful-discharge laws. Most state courts have adopted at least one wrongful-discharge law in the last three decades. Three states (Florida, Georgia, and Rhode Island) have never adopted an exception, while 10 states recognize all three exceptions.[3] One may potentially use this cross-state, over-time variation to analyze how wrongful-discharge laws affected employment and earnings in state labor markets.

We are not the first authors to recognize this opportunity. In an influential paper, James N. Dertouzos and Lynn A. Karoly (1992; DK hereafter) estimated that the adoption of wrongful-discharge laws was economically equivalent to a *10-percent employer-side tax on wages,* leading to a 3-percent reduction in aggregate employment, in states that allow workers to sue for punitive ("tort") damages for wrongful discharge, as is typically true under the good-faith and public-policy exceptions. Moreover, DK found that states that adopted a doctrine under which plaintiffs may sue only for economic ("contract") losses (typically the implied-contract

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[1] Quotation from *Payne* v. *Western & Atlantic Railroad, Supreme Court of Tennessee, 1884.* Andrew P. Morriss (1994) documents the history of the employment-at-will doctrine.

[2] To date, only Montana (in 1987) has passed a statute establishing a good-cause standard for employment terminations. All other employment-at-will exceptions are common-law (case law) doctrines.

[3] The legal appendix of Autor et al. (2003) classifies the case law used for this analysis.

exception) suffered an additional 1–2-percent employment decline.

For over a decade, scholars and politicians have widely cited the DK paper as evidence that the costs of adopting exceptions to the doctrine of employment at will is very high.[4] DK's results have been challenged, however. In a careful recent analysis, Thomas J. Miles (2000) concluded that wrongful-discharge laws had "no statistically significant effects on either employment or unemployment." Notably, Miles does not analyze why his findings differ from those of DK.[5]

Most recently, in Autor et al. (2003; ADS hereafter), we comprehensively reevaluated the impacts of wrongful-discharge doctrines on employment and earnings using richer data and a more complete coding of the case law. Exploiting legal, employment, and wage data observed at monthly intervals, ADS found a robustly negative, but modestly sized, effect of one wrongful-discharge doctrine, the implied-contract exception, on the employment-to-population ratio in state labor markets. This reduction, which averaged 0.8–1.6 percent, is only *one-third to one-fifth* as large as the impact DK reported, yet considerably larger than the estimate of zero reported by Miles.[6]

This paper seeks to reconcile the substantial discrepancies among the DK, Miles, and ADS studies. Substantively, we evaluate whether one may have confidence in any one set of estimates (including our own) for the impact of wrongful-discharge laws on state labor markets. Methodologically, we draw general lessons for economists and legal scholars attempting to evaluate the impact of common-law doctrines on economic outcomes.

[4] For example, during his gubernatorial campaign, former California Governor Pete Wilson prominently cited Dertouzos and Karoly's (1993) conclusions in support of tort reform (Martyn Hopper, 1995).

[5] Autor (2003) and Miles (2000) find strong evidence that employers increased demand for temporary help agency employment after states adopted wrongful-discharge laws. Adriana Kugler and Gilles 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. Morriss (1995) finds mixed evidence that wrongful-discharge doctrines reduced the job-separation rates of nonunion relative to unionized workers.

[6] ADS found no evidence that wrongful-discharge laws had a significant impact on wage levels.

I. The Dertouzos and Karoly (1992) Study

The Miles and ADS papers analyze the impact of wrongful-discharge doctrines on labor markets by contrasting contemporaneous employment and wage changes in adopting versus non-adopting states (a difference-in-difference estimate). DK eschews this source of variation, arguing that the "supply and demand" for legal doctrines may drive the adoption of state common law. To correct for this postulated endogeneity, DK uses instrumental variables (IV) to predict states' adoption of wrongful-discharge doctrines and replace the actual laws with these predicted values. Specifically, DK estimates an equation of the form

$$(1) \qquad \ln(\text{emp}_{st}) = \boldsymbol{\beta}_1 \hat{\mathbf{L}}_{jt} + \gamma_s + \delta_t + \varepsilon_{st}$$

where the dependent variable is log total employment in state s and year t, and $\hat{\mathbf{L}}$ is a vector of predicted probabilities of the presence of each doctrine in the state and year. These probabilities were estimated from a log odds regression of the observed law variables, $\mathbf{L}$, on a set of instrumental variables, $\mathbf{Z}$, and vector of year dummies $\boldsymbol{\delta}$.

Among a large set of instrumental variables used by DK, two most strongly predict a state's propensity to adopt a wrongful-discharge law (Dertouzos and Karoly, 1992 tables 3.3 and 3.4): the fraction of contiguous states recognizing a similar doctrine, and whether in 1980 a state had a "right-to-work" law (permitting nonunion employees at unionized establishments). Unfortunately, both measures are likely to have a direct correlation with employment levels that does not arise from their indirect effects on law adoption.

Figures 1 and 2 illustrate the source of our concern. As Olivier Blanchard and Lawrence B. Katz (1992) discuss and Figure 1 shows, Southern employment growth has exceeded non-Southern growth for six decades, substantially predating the adoption of wrongful-discharge laws. These differential trends stem from factors such as the advent of air conditioning, which increased habitability and manufacturing productivity in the South, and civil-rights-era

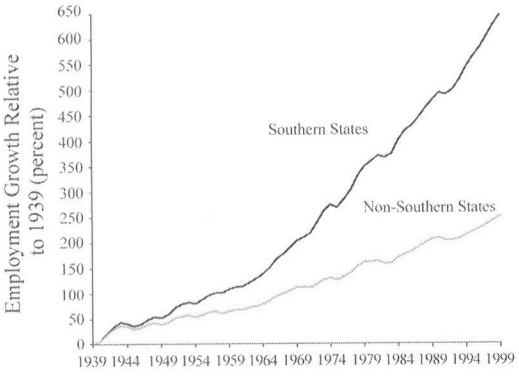

FIGURE 1. EMPLOYMENT GROWTH IN SOUTHERN VERSUS NON-SOUTHERN STATES, 1939–1999

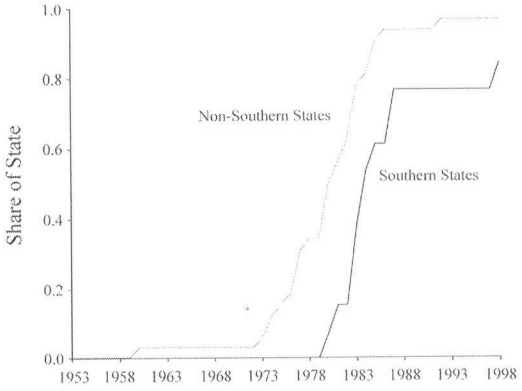

FIGURE 2. SHARE OF STATES RECOGNIZING ONE OR MORE WRONGFUL-DISCHARGE LAWS, 1953–1998

legislation that increased wages and employment of Southern blacks (Raymond Arsenault, 1984; Donohue and James J. Heckman, 1991).[7] As Figure 2 shows, Southern states were also last and least likely to adopt wrongful-discharge doctrines, meaning that their fraction of contiguous states adopting exceptions was also much lower. Moreover, 85 percent of Southern states had a right-to-work law in 1980, compared to 25 percent of non-Southern states.

Given the positive correlation between Southern region and employment growth and the negative correlation between Southern region and adoption of wrongful-discharge doctrines, it is a near certainty that equation (1), a regression of state employment growth on predicted law adoption, will indicate that wrongful-discharge laws reduce employment ($\hat{\boldsymbol{\beta}}_1 < \mathbf{0}$). Yet, this correlation may arise simply because wrongful-discharge laws were more likely to be adopted in states that had been experiencing slower employment growth for decades.

To explore this possibility, we made a substantial effort to replicate DK's core results using that paper's cited data sources, classification of case law, and empirical methods. While we could not reproduce the findings exactly, we believe our results are sufficiently comparable to allow us to explore the main substantive issues.[8] Table 1 summarizes the findings. The first column replicates DK's basic instrumental-variables specification. This specification estimates the impact of three wrongful-discharge laws on the log of state employment during 1980 through 1987: the implied-contract exception or good-faith exception with contract remedy (IC/GF), the broad public-policy or good-faith exception with tort remedy (PP/GF), and the narrow public-policy exception (NPP). The impact of these doctrines on state employment appears to be significant and large. The IC/GF doctrine is estimated to reduce employment by close to 4.5 percentage points, and the PP/GF doctrine reduces it by an additional 3.0 percentage points.[9]

[7] Scholars vary on the definition of the "Southern" states. In Figure 1, we use Alabama, Arizona, Florida, Georgia, Kentucky, Louisiana, Mississippi, New Mexico, North and South Carolina, Tennessee, Texas, and Virginia. Non-Southern states include all other U.S. states (excluding the District of Columbia) and, due to lack of employment data extending to 1939, Alaska, Hawaii, Illinois, Michigan, and Minnesota.

[8] DK did not provide the original data or programs, but the authors kindly shared their mapping of contiguous states developed for the IV estimates. Disconcertingly, we uncovered more than a dozen coding errors in this mapping. In addition, DK somewhat unconventionally coded Alaska as bordering Idaho, Oregon, Montana, and Washington state, and Hawaii as bordering California, Nevada, and Oregon. In the service of replication, we employ their original mapping.

[9] As noted, our replication results differ from DK. While DK (table 5.2) finds that the PP/GF doctrine has the largest negative impact on employment (2.1 percent), we find a larger impact due to IC/GF. Our estimates for the NPP doctrine are comparable. Following DK, we do not use a true IV procedure, but insert predicted values from the first-stage (predictive) equation into the second-stage esti-

To the degree that the instrumented state-law variables primarily proxy for persistent employment trends, adding state time trend variables will reduce the bias. As column (ii) shows, including trend variables reduces the estimated impacts of the state laws by approximately 60 percent and renders the coefficients insignificant. Notably, the standard errors of the law variables are little affected, indicating that the trend variables are not simply introducing collinearity that reduces the precision of the estimates. These results suggest that the DK estimates are substantially biased.

The next two columns of Table 1 provide an additional specification check. DK includes one highly problematic control variable in equation (1): the log of state gross product (GSP) and its change. If wrongful-discharge laws reduce employment, they are likely to reduce state output as well. Column (iii) drops the GSP variable from the basic DK model, yielding coefficients on the instrumented law variables that are large and difficult to interpret. When, in column (iv), state trends are added to the model excluding GSP, the estimated impacts of the laws return to reasonable magnitudes and are in this case insignificant. These findings again suggest that the DK IV approach is non-robust.

To contrast DK's IV approach to our difference-in-difference (DD) methodology, we estimate in panel B of Table 1 a series of models in which we use the actual law changes as independent variables rather than their predicted values. In column (v) of the second panel, we find a positive and significant effect of the IC/GF doctrine on state employment levels, opposite to DK's IV estimates. However, when we condition on state specific trends, this effect reverses sign and becomes negative and significant at 0.8 percentage points.

Because of our doubts about the validity of conditioning on state output in employment models, we drop the GSP measures in the final two columns. This yields more stable estimates.

TABLE 1—REPLICATION OF DERTOUZOS & KAROLY (1992), THE ESTIMATED IMPACT OF WRONGFUL DISCHARGE LAWS ON LOG STATE EMPLOYMENT, 1980–1987
(DEPENDENT VARIABLE: 100 × LN[STATE EMPLOYMENT])

	A) Replication of DK's instrumental-variables estimates			
Independent variable	(i)	(ii)	(iii)	(iv)
Implied-contract or good-faith (contract) doctrine	−4.44 (1.26)	−1.45 (0.94)	−14.53 (2.39)	−1.13 (1.70)
Broad public-policy or good-faith (tort) doctrine	−3.00 (1.50)	−1.26 (1.58)	3.48 (2.91)	−5.83 (2.86)
Narrow public-policy doctrine	0.03 (0.94)	−0.55 (0.96)	−5.19 (1.82)	0.05 (1.73)
100 × log(gross state product)	0.73 (0.02)	0.77 (0.03)		
100 × Δlog(gross state product)	−0.42 (0.04)	−0.40 (0.03)		
State trends	no	yes	no	yes
F test (state trends):		0.00		0.00
R^2:	1.000	1.000	0.999	1.000
	B) Replication of DK using a difference-in-difference estimator			
Independent variable	(v)	(vi)	(vii)	(viii)
Implied-contract or good-faith (contract) doctrine	0.97 (0.44)	−0.75 (0.36)	−0.42 (0.91)	−1.46 (0.67)
Broad public-policy or good-faith (tort) doctrine	−1.13 (0.60)	0.46 (0.58)	−0.15 (1.22)	0.00 (1.05)
Narrow public-policy doctrine	0.16 (0.40)	0.43 (0.32)	0.27 (0.74)	−0.73 (0.56)
100 × log(gross state product)	0.76 (0.02)	0.77 (0.03)		
100 × Δlog(gross state product)	−0.42 (0.04)	−0.40 (0.03)		
State trends	no	yes	no	yes
F test (state trends):		0.00		0.00
R^2:	1.000	1.000	0.999	1.000

Notes: For the results reported, $n = 400$ (50 states × 8 years). Columns (i)–(iv) tabulate our replication and specification checks of Dertouzas and Karoly's (1992) table 5.2, column 2. Columns (v)–(viii) reestimate these models using a difference-in-difference estimator. Following DK, instruments included in columns (i)–(iv) of panel (A) are: whether a state had a right-to-work statute in 1980, whether a state has a Republican governor, the percentage change in lawyers per capita, the share of neighboring states recognizing each doctrine and the square of this measure, the percentage unionized, the change in percentage unionized, and the change in the percentage unemployed.

mates. This procedure is likely to exaggerate the precision of the estimates since it does not account for the fact that the independent variables are predicted values. In addition, we follow DK in excluding from the first-stage models certain variables used in the second-stage estimates (e.g., the first-stage estimates exclude state dummies). This method again violates standard practice.

The initial estimate finds a small negative impact of the IC/GF doctrine on state employment. Adding trends, the magnitude increases to −1.5 percent, roughly one-third the size of the DK instrumental-variables estimate, but closely comparable to our high-end estimates in ADS.

Our reanalysis of DK underscores the hazards of using smoothly evolving economic variables, such as trends in neighboring states, as instrumental variables for discrete events, such as the timing of specific court cases. This strategy turns discontinuous events into continuous non-events, which may readily correlate with other smooth trends in the data. While DK rightfully stresses that legal rules may in part be endogenous, employers are unlikely to wholly foresee the timing of a change to the common law. This unanticipated component of the law allows the difference-in-difference estimator to potentially identify discontinuous employment impacts.

II. The Importance of Legal Classification: The Miles Study

Contrary to DK's analysis (and our ADS study), Miles's (2000) recent study of wrongful-discharge laws found no effects on aggregate employment or unemployment during the years 1964–1995. With generous assistance from Miles, we fully replicated these findings and located the source of the discrepancy: the classification of case law developed by David J. Walsh and Joshua L. Schwarz (1996; WS hereafter) and used in the Miles study. Simply using the Miles data and specifications but replacing the WS legal variables with our own showed that the implied-contract exception reduced employment in adopting states.

While this finding resolves the proximate puzzle, it raises a deeper concern. Do our findings hinge on technical points of case law about which other scholars could readily disagree? To answer this question, we compare in Table 2 the findings using the ADS legal classification to the results we would have obtained using the classifications developed by all previous authors: Dertouzos and Karoly (1992), Morriss (1995), and Walsh and Schwarz (1996). Following these earlier analyses, we aggregate the data from the ADS paper to annual (rather than monthly) observations and apply the long-panel difference-in-difference approach to estimate the model,

$$(2)\quad \ln(\text{emp/pop})_{srt} = \boldsymbol{\beta}_3 \mathbf{L}_{st} + \gamma_s + \delta_t + (\gamma_s \times t) + (\delta_t \times \lambda_r) + \varepsilon_{srt}$$

where the dependent variable is the log employment of the state employment-to-population ra-

TABLE 2—COMPARISONS OF ESTIMATED IMPACT OF WRONGFUL-DISCHARGE LAWS ON EMPLOYMENT USING VARIOUS LEGAL CLASSIFICATIONS (DEPENDENT VARIABLE: 100 × LN(EMPLOYMENT/POPULATION)

	A) Morriss, 1978–1989		B) DK, 1980–1987	
Independent variable	ADS (i)	Morriss (ii)	ADS (iii)	DK (iv)
Implied contract	−1.01	−1.11	−1.28	−0.65
	(0.32)	(0.32)	(0.38)	(0.34)
Public policy	−0.01	−0.03	−0.27	−0.01
	(0.31)	(0.30)	(0.39)	(0.40)
Good faith	−1.12	−0.65	−0.74	−0.89
	(0.47)	(0.45)	(0.55)	(0.57)
R^2:	0.96	0.96	0.98	0.97
n:	600		400	

	C) WS, 1978–1994		
		WS	
Independent variable	ADS (v)	Original (vi)	Corrected (vii)
Implied contract	−1.00	−0.17	−0.65
	(0.28)	(0.27)	(0.31)
Public policy	−0.17	−0.40	0.02
	(0.27)	(0.30)	(0.30)
Good faith	0.35	0.85	0.80
	(0.40)	(0.46)	(0.50)
R^2:	0.95	0.95	0.95
n:	850		

Notes: The table compares our findings reported in Autor et al. (2003; "ADS") to those we would have obtained using the legal classifications of (A) Morriss (1995), (B) Dertouzos and Karoly (1992; "DK"), and Walsh and Schwarz (1996; "WS" [both original and corrected]). Huber-White robust standard errors are reported in parentheses. All models include state and year main effects, linear state trends, and interactions between four Census geographic region dummies and individual year dummies. All regressions are weighted by states' share of total population ages 14–64 in each year. Dummies for adoption of legal doctrines are lagged by one year.

tio. Our concern about regional trends led us to include linear state time trends and interactions between year dummies and a vector of four geographic region dummies (λ_r). To provide a pure test, we use the same time periods used by each of the other scholars.

Panel A of Table 2 compares the ADS classification to the scheme Morriss developed for 1978 through 1989. For both classifications, the implied-contract exception lowers the employment rate significantly. The good-faith doctrine is also associated with reduced employment in these models, but as subsequent panels show, this is not robust across time periods. The public-policy doctrine is never significant.

Panel B compares the ADS and DK classifications for the DK period of 1980–1987. Under either classification, we find a negative employment effect of the implied-contract exception (and other wrongful-discharge doctrines, although not all are significant).

In the final panel, we compare our results with the Walsh and Schwarz scheme (used by Miles) for the years 1978–1994 (the beginning of the ADS sample to the end of the WS sample). Using the WS classification, we find that the implied-contract exception has a weakly negative, but never significant, effect on employment levels.

To understand why the findings using the WS scheme differ notably from the other two classifications considered, we compared the WS and ADS classifications case-by-case. Of a potential 150 instances (50 states × 3 doctrines), we found 29 discrepancies. These discrepancies appear to stem from a substantive difference in how cases were selected for each classification. To maximize the chance of detecting economic effects of changes to the common law, in ADS we attempt to locate 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 (1996 p. 646) select cases that best articulate courts' rationales for promulgating a new doctrine. Because of the unique nature of common law, these differing objectives yield distinct results. Often, as the common law develops, the courts make an initial holding, such as identifying a new employment right, but do not fully articulate the extent and limits of the new doctrine until subsequent cases arise, often several years later. One might therefore expect that the cases selected by WS would tend to follow the cases that declare new doctrine without offering great elaboration.

TABLE 3—COMPARISON OF CLASSIFICATION OF LEADING WRONGFUL-DISCHARGE CASES: ADS VS. WS

Case type	Number of discrepancies	ADS case precedes WS case by 1+ years	WS case precedes ADS case by 1+ years	Same year, different case
Implied contract	16 of 43	13	2	1
Public policy	11 of 43	6	3	2
Good faith	1 of 11	1	0	0

The pattern of discrepancies shown in Table 3 bears this out. The ADS and WS classifications of implied-contract cases differ in 16 instances. In 13 situations WS picks a later case, ranging from one to 13 years later (and for one state, Indiana, they find no case at all). Only for two states is an earlier case picked in WS. A similar pattern emerges for public-policy cases: in six of 11 instances, ADS identifies an earlier case, while in three of 11 cases WS identifies an earlier case. (There are far fewer good-faith cases, and we differ in only one state.)

To reconcile with WS, we modified only what we view as the six leading discrepancies in the WS classification vis-à-vis the ADS classification. As the final column of Table 2 reveals, these modifications account for a substantial share of the empirical discrepancy and, importantly, restore the conclusion that the implied-contract exception caused a small but significant employment reduction.

III. Conclusion

Our reanalysis supports the conclusions of Autor et al. (2003) that the Dertouzos and Karoly (1992) paper significantly overestimates the disemployment effects of wrongful-discharge laws while the Miles (2000) study underestimates these effects. Our study also offers two methodological points relevant to the growing body of work using panel data to evaluate the effects of state legislative and judicial pronouncements. First, we caution that using

two-stage estimation techniques to instrument for legal variation may be a cure worse than the disease. While it is always a concern that legal rules may in part be endogenous, instrumenting for legal changes with anything other than a discontinuous, exogenous forcing variable is hazardous. In addition to risking spurious inference, this approach often discards usable discrete variation induced by unanticipated changes in the law. Second, our analysis reveals that detailed legal evaluation of the *innovations* in legal doctrine is essential to estimating their economic impacts. By the time the courts issue a decision that fully elaborates the reasoning behind a new common-law doctrine, employers may already have responded to the initial precedent-setting decision.

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