The Economic Journal, **117** (*June*), F189–F217. © The Author(s). Journal compilation © Royal Economic Society 2007. Published by Blackwell Publishing, 9600 Garsington Road, Oxford OX4 2DQ, UK and 350 Main Street, Malden, MA 02148, USA.

DOES EMPLOYMENT PROTECTION REDUCE PRODUCTIVITY? EVIDENCE FROM US STATES*

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Theory predicts that mandated employment protection may reduce productivity by distorting production choices. We use the adoption of wrongful-discharge protection by state courts in the US from 1970 to 1999 to evaluate the empirical link between dismissal costs and productivity. Drawing on establishment-level data from the Census Bureau, our estimates suggest that wrongful-discharge protection reduces employment flows and firm entry rates. Moreover, plants engage in capital deepening and experience a decline in total factor productivity, indicative of altered production techniques. Evidence of strong contemporaneous growth in employment, however, leads us to view our findings as suggestive but tentative.

An extensive literature explores the impact of dismissal costs – also frequently called firing costs or employment protection – on the operation of labour markets. Beginning with the seminal work of Lazear (1990), much research has focused on assessing how dismissal costs affect employment levels. Theory suggests, however, that dismissal costs may have ambiguous effects on employment levels. Dismissal costs act as a tax on firing, which reduces dismissals but also reduces hiring. The net effect of these offsetting factors is ambiguous, at least in the short run. It is perhaps not surprising therefore that the empirical literature has found widely varying effects of dismissal costs on employment levels.

By contrast, theory makes a clear prediction about the impact of dismissal costs on the efficiency of hiring and firing. Provided that dismissal protection is not undone by Coasean bargaining, dismissal protection raises firms' adjustments costs. Consequently, firms will find it optimal not to hire workers whose short-term marginal product exceeds their market wage and will choose to retain unproductive workers whose wage exceeds their productivity (Blanchard and Portugal, 2001). These distortions in production choices unambiguously reduce worker flows. They are also likely to cause firms to substitute capital for labour and have the potential to reduce productivity by distorting production choices.

This article evaluates whether, and to what extent, the introduction of dismissal costs affects firms' production choices and, ultimately, their productivity. The source of variation in dismissal costs that we exploit is the adoption of wrongful discharge protection by US state courts from the late 1970s to the early 1990s. These common-law

* The research in this article was conducted while the authors were Special Sworn Status researchers of the US Census Bureau at the Boston Census Research Data Center (BRDC). Support for this research from NSF grant (ITR-0427889) is gratefully acknowledged. Research results and conclusions expressed are those of the authors and do not necessarily reflect the views of the Census Bureau. This article has been screened to ensure that no confidential data are revealed. We are grateful to seminar participants at the IZA Conference on Employment Protection and Labor Markets, the Census Bureau RDC Conference, MIT, NBER Labor and Productivity Groups, SOLE, and AEA and, especially, to Daron Acemoglu, Josh Angrist, Giuseppe Bertola, Björn Brügemann, and Paul Oyer for their comments. Autor acknowledges generous support from the National Science Foundation (CAREER SES-0239538) and the Alfred P. Sloan Foundation. Kugler acknowledges support from a GEAR grant from the University of Houston.

protections against wrongful discharge generated a flood of litigation in adopting states and increased the uncertainty and potential cost of discharging workers. As has been established in prior work using both household survey data and aggregate state-level employment data, adoption of wrongful discharge laws had measurable effects on state employment levels, unemployment-to-employment flows, and the outsourcing of jobs to temporary help employers (Miles, 2000; Schanzenbach, 2003; Autor, 2003; Autor *et al.*, 2004, 2006; Kugler and Saint Paul, 2004). Yet, these aggregate effects have rarely been explored using representative microdata on firms, nor have their consequences for productivity been assessed.[1]

In this article, we simultaneously analyse the consequences of employment protection for establishment-level employment flows and productivity. We first test whether dismissal costs reduce employment volatility – a necessary implication of any standard non-Coasean model – both at the extensive (entry/exit) margin and intensive (within-plant) margin. We next assess whether any reduction in employment volatility is accompanied by a reduction in productivity.

Our analysis exploits detailed, comprehensive establishment-level data from two Census Bureau surveys: the Longitudinal Business Database (LBD) and the Annual Survey of Manufacturers (ASM). Sourced from US tax records and Census surveys, the LBD provides annual employment and payroll information on all US private establishments in most lines of business. The LBD is thus an exceptional resource for identifying the effects of dismissal costs on how firms adjust their labour inputs; its employment and wage records cannot, however, facilitate a further study of the concomitant adjustments of other factors of production and the consequences for productivity. We thus complement the LBD with a balanced panel of 'ongoing' manufacturing plants continuously surveyed by the ASM. We first demonstrate that the impact of dismissal costs on employment adjustment within this panel mirrors the LBD manufacturing universe, and then turn to the ASM's detailed operating data (e.g., output, capital investment, employment) to study extensively the important productivity outcomes.

We find that one of the three dismissal protections adopted during this period, the Covenant of Good Faith and Fair Dealing ('good faith' hereafter), reduced annual employment fluctuations and the entry of new establishments in adopting states. Consistent with the apparent rise in adjustment costs, we document that firms in adopting states engaged in capital deepening, leading to a concurrent rise in labour productivity. Notably, we find evidence of a decline in total factor productivity following adoption of the good faith exception. Our effects are strongest in the short run, peaking around three years after the adoption and declining afterwards. These results suggest that adoption of dismissal protections altered short-run production choices and caused employers to retain unproductive workers, leading to a reduction in technical

[1] In contemporaneous work, Bird and Knopf (2005) analyse the effects of wrongful-discharge protections on the earnings, profitability and efficiency of the US banking sector from 1980 to 1990. They conclude that adoption of wrongful-discharge protections raised wages, reduced profits and lowered productivity in this sector. Petrin and Sivadasan (2006) introduce and implement a novel framework for estimating the effects of employment protection legislation on productivity, focusing on its impact on the gap between workers' marginal revenue product and the wage. Using data from Chile, they find that increases in firing costs raise this gap. Prieger (2005) examines the impact of the Americans with Disabilities Act on the entry and exit of firms in retail.

efficiency. Clouding the interpretation of these results, however, is the finding that adoption of the good faith exception is associated with implausibly large subsequent growth in manufacturing employment. This pattern suggests that our results may be partly contaminated by confounding economic shocks. Thus, while our analysis provides novel direct evidence that employment protections may reduce firm-level productivity, the results must be viewed as tentative. It is our hope that future studies will provide further exploration of these initial results.

1. Wrongful Discharge Protection in the US

The US has long had a legal presumption that workers and employers may freely terminate their employment relationships 'at will,' that is without notification, financial penalty or requirement to demonstrate good (or any) cause. This legal doctrine, referred to as employment-at-will, was first articulated by the Tennessee Supreme Court in 1884 and was subsequently adopted into the common law by almost all US state courts by the mid-1930s (Morriss, 1994).[2]

Beginning in the 1970s, the legal consensus supporting employment-at-will eroded rapidly. In a series of precedent-setting cases between 1972 and 1992, the vast majority of US state courts adopted one or more common-law exceptions to the employment-at-will doctrine. These exceptions constrained the ability of employers operating in adopting states to terminate workers 'at will.' These common-law exceptions are typically classified into three categories:

(1) the implied covenant of good faith and fair dealing ('good faith' exception);
(2) the tort of wrongful discharge in violation of public policy ('public policy' exception); and
(3) the implied-in-fact contract not to terminate without good cause ('implied contract' exception).[3]

We summarise these exceptions here and refer the reader to Autor *et al.* (2006) for an extended discussion.

Read broadly, the good faith exception prohibits employers from firing workers for 'bad cause.' The definition of 'bad cause,' however, varies greatly by state and over time. The California Court of Appeals' famous 1980 good faith ruling in *Cleary v. American Airlines*[4] – probably the most influential of all good faith cases – was initially understood to bar California employers from terminating *any* worker without good cause. However, the California Supreme Court's 1988 ruling in *Foley v. Interactive Data Corp* vastly reduced the scope of the *Cleary* decision and limited the financial remedies

[2] Idaho, New Jersey and New Mexico adopted employment-at-will in 1948, 1953 and 1968, respectively. Prior to Idaho, the most recent was Wyoming in 1937. Montana is the only state to have implemented exceptions to the employment-at-will doctrine by statute rather than common law (Ewing *et al.*, 2005).

[3] For detailed discussion of the evolution of the employment-at-will doctrine, see Morriss (1994, 1995), Autor (2003), Kugler and Saint Paul (2004) and Autor *et al.* (2006). Our discussion draws particularly on the latter work, which contains (at present) the most current legal analysis. Legal scholars, including most notably Dertouzos and Karoly (1992), also categorise these exceptions according to whether they allow for tortious damages (i.e., pain, suffering, and possibly punitive damages) in addition to contractual damages (i.e., exclusively economic losses). Recent work has not found that this distinction is empirically relevant (Autor *et al.*, 2006), however, and hence we focus on the three categories of legal exception.

[4] 168 Cal. Rptr. 722 (Cal. Ct. App. 1980 October).

available to plaintiffs.[5] At present, all eleven state courts that recognise the good faith exception (including California) primarily limit awards to 'timing' cases in which the employer intentionally terminates a worker to deprive her of a promised benefit (e.g., a sales commission or non-vested pension). Hence, 'bad cause' under the good faith exception is currently construed narrowly, though this was not always the case.

The public policy exception, recognised by 43 states as of 1999, provides workers with protections against discharges that would inhibit them from acting in accordance with public policy. In states recognising the public policy exception, workers may, for example, litigate if they are fired for performing jury duty, filing a worker's compensation claim, reporting an employer's wrongdoing, or refusing to commit perjury on behalf of the employer. Because courts typically limit public policy cases to clear violations of explicit legislative commands, rather than violations of a vaguer sense of public obligation, the public policy exception is not generally thought to impose substantial constraints on employer behaviour.

The implied contract exception, also recognised by 43 states in 1999, comes into force when an employer implicitly promises not to terminate a worker without good cause. Such implicit promises may include, for example: personnel manuals stating that the employer's policy is to terminate employees only for just cause; expectations arising from a worker's longevity of service or history of promotions and salary increases; and usual company practices that preclude terminating workers without good cause. The expected economic impact of the implied contract exception is hard to gauge. On the one hand, employers can potentially 'contract around' this exception simply by rewording personnel manuals and adding explicit language to employment contracts to state that all employees remain 'at will'.[6] On the other hand, firms without sophisticated human resources staff may be unaware of the implied contract exception or lack the expertise to insulate themselves fully from its reach. Additionally, the implied contract exception can potentially reclassify an employer's entire workforce as not 'at will,' which may impose significant costs.

To assess the effects of these employment-at-will exceptions on productivity and employment outcomes, we adopt a difference-in-difference approach that contrasts state-level changes in outcomes in adopting states to contemporaneous changes in outcomes in non-adopting states. This treatment-control contrast identifies the average causal effect of the exceptions on the outcomes of interest under the assumption that these outcomes would have otherwise evolved similarly in adopting and non-adopting states. We take a number of steps to buttress the robustness of this statistical approach. All econometric models include industry or industry-by-year fixed effects (in addition to state fixed effects) to absorb industry-wide shocks that may be correlated across states. In addition, most specifications include state-specific linear time trends to

[5] 765 P.2d 373 (Cal. 1988). Whereas the *Cleary* decision permitted plaintiffs to recover tortious damages for violations of the good faith doctrine, *Foley* reduced these damages to contractual losses (Jung and Harkness, 1989).

[6] And indeed, large employers took such steps. The Bureau of National Affairs (1985) found that 63% of large employers surveyed in the early 1980s had recently 'removed or changed wording in company publications to avoid any suggestion of an employment contract', and 53% had 'added wording to applications and handbooks specifying that employment may be terminated for any reason'. Sutton and Dobbin (1996) report that the percentage of firms using 'at will' clauses in employment contracts increased from 0% to 29% between 1955 and 1985.

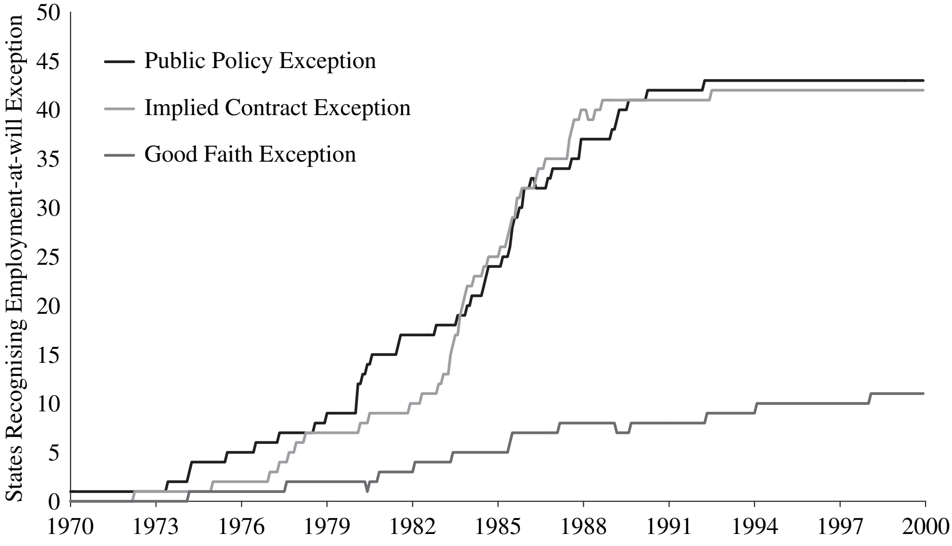

Fig. 1. *Count of States Recognising Exceptions to Employment-at-will, 1970–1999 at Monthly Frequency*

account for possible pre-existing trends that may predate the adoption of employment-at-will exceptions and could otherwise be confounded with adoption. Some specifications further include plant fixed effects, where identification comes from contrasts of within-plant changes in outcomes in adopting relative to non-adopting states. As a falsification test, we also estimate dynamic models that contrast changes in outcomes in years prior to and following adoption of exceptions to provide a check on the possibility that adoption of employment-at-will exceptions are caused by changes in outcomes rather than *vice versa*.

Figure 1 plots the number of states recognising each of the three exceptions during the time period of 1970 to 1999 (at monthly frequency).[7] Two main points are visible. First, the public policy and implied contract exceptions are far more widely recognised than the good faith exception. Second, adoption of each exception appears to follow something of a contagion pattern, with a large number of adoptions occurring in rapid succession between 1976 and 1988, followed by near-stasis from 1988 forward. This pattern suggests that adoptions cannot be viewed as fully independent, but that a widespread change in legal thinking in the 1970s and 1980s led many state courts to amend the long-standing doctrine of employment-at-will at around the same time. This potentially presents a challenge for identification in that businesses might react in advance to anticipated changes in the legal environment, thus blurring the pre-post contrast. However, the date at which a state adopts a given exception is an idiosyncratic function of the cases brought before state high courts and the disposition of the sitting judges. Many states never adopt exceptions and others reverse or amend these exceptions after adoption. Accordingly, precedent-setting cases that generate exceptions to employment-at-will typically will provide a discrete element of surprise. This is

[7] The dips in the series reflect court reversals of doctrines that were previously recognised.

particularly likely to be true for the good faith exception, which was adopted more slowly and less extensively than either the public policy or implied contract exceptions.

As emphasised by Autor *et al.* (2006), it is likely that a substantial component of the economic cost of the employment-at-will exceptions emanates from the uncertainty they introduced into the employment relationship. When most exceptions were adopted in the late 1970s to late 1980s, the volume and cost of wrongful discharge litigation that would ultimately ensue was unknown to firms and potential litigants. Adding to the uncertainty, personnel and professional law journals (i.e., the trade publications relied upon by personnel managers and corporate attorneys) published numerous articles that appeared to overstate the scope of the protections afforded to workers and the penalties that firms would incur for violating them (Edelman *et al.*, 1992). Because employers were potentially led to anticipate greater constraints and costs than ultimately materialised, Autor *et al.* (2006) argue that the short-term and medium-term effects of these dismissal protections may have exceeded their 'steady-state' effects, and they present evidence consistent with this hypothesis.

Several prior studies have analysed the effects of employment-at-will exceptions on labour market outcomes. The first study in this vein, Dertouzos and Karoly (1992), found using aggregate state-level data that adoption of common-law dismissal protections reduced state employment levels by as much as 7%. Subsequent analyses by Miles (2000), Schanzenbach (2003) and Autor *et al.* (2004, 2006) using industry-level and household-level data do not confirm these results, however. These more recent studies find either modest negative effects (Autor *et al.*, Schanzenbach) or no effects of dismissal protections on employment levels (Miles). As noted above, however, theory makes ambiguous predictions about the impact of dismissal costs on employment levels.

A number of studies also provide evidence that states' adoption of dismissal protections raised hiring and firing costs. Miles (2000) and Autor (2003) show that employers in adopting states substituted temporary help agency workers for direct-hire employees, presumably in an effort to minimise litigation risks.[8] Kugler and Saint-Paul (2004) find using the National Longitudinal Survey of Youth that these protections (especially the good faith exception) reduced the re-employment probability of unemployed relative to employed workers, suggesting that dismissal protections exacerbated adverse selection into non-employment. Both sets of findings are significant for our work because they demonstrate that the adoption of dismissal protections raised firms' adjustment costs – a necessary condition for them to have had productivity impacts.

Our study builds on this prior work in two major respects. First, use of establishment-level data provides direct evidence on the effects of dismissal protections on firms' employment adjustments at both the extensive (plant opening/closing) and intensive (job flows) margins. Second, we directly evaluate the consequences of dismissal protections for establishment-level production choices and realised productivity.

[8] The implied contract exception in particular confers a comparative advantage on temporary help agencies since these firms are universally understood to offer only short-term employment. It is the implied contract exception that appears primarily responsible for the growth of temporary help agency employment (Autor, 2003).

2. Theoretical Considerations

In a standard competitive model of the labour market, employment protection is economically equivalent to mandated employment benefits. Benefit mandates raise the cost of employing workers, leading to an inward shift in labour demand. If, however, workers value the mandated benefit at its marginal cost of provision – that is, the mandate is efficient – then the Coase theorem applies. Labour supply shifts outward to offset the inward shift in labour demand, employment levels are unchanged and wages fall to cover exactly the cost of the benefit (Summers, 1989; Lazear, 1990). There are no productivity or employment consequences.[9]

Mandatory dismissal protection can impose efficiency costs in the competitive model, however. If workers value dismissal protection at less than its marginal cost of provision – or, equivalently, if some share of the termination benefit accrues to a third-party, such as an attorney – the benefit mandate drives a wedge between the private and social cost of job separations, yielding a deadweight loss. Because dismissal costs are only paid when workers and firms separate, the deadweight loss component of the dismissal cost functions as a tax on separations – an adjustment cost. Consider, for example, a case where a worker's marginal product falls below his wage and the wage cannot drop sufficiently to compensate the firm (either due to a non-negativity constraint or due to downward wage rigidities). If the worker values the dismissal benefit at its marginal cost, both the worker and the firm will agree to terminate the job. If the payment of the dismissal benefit incurs a deadweight loss, however, both the worker and the firm will find it optimal to continue the employment relationship so long as the present value of the productivity shortfall is less than the deadweight loss. Consequently, inefficient dismissal protection – that is, protection that workers value at less than cost – inhibits efficient job separations (and, indirectly, reduces efficient accessions as well).

In the competitive model, inefficient dismissal protection unambiguously reduces allocative efficiency – that is, they are welfare reducing. Their implications for the technical efficiency of production are less clear cut. If dismissal protection causes firms to retain (some) unproductive workers, this will cause a decline in labour productivity, *ceteris paribus*. Offsetting this factor, firms may screen new hires more stringently, leading to a favourable compositional shift in the productivity of the employed workforce. Moreover, because inefficient dismissal protection provides firms with an incentive to substitute from labour to other factors of production, capital deepening may also raise the marginal product of labour. Hence, the net impact on technical efficiency (as opposed to allocative efficiency) is ambiguous.

[9] Aghion and Hermalin (1990) and Levine (1991) present models in which dismissal protection is under-provided by the private market due to adverse selection. Bertola (2004) also presents a model in which dismissal costs are under-provided due to risk-aversion. Agell (1999) discusses why eliminating dismissal protection may not be desirable when labour markets are subject to fairness considerations and market imperfections, while Wasmer (2006) and Macleod and Nakavachara (2007) focus on human capital investment. In all these cases, dismissal protection mandates can be efficiency-enhancing since workers may value this protection above its cost of provision. In the Coasean model, this would imply that imposing the mandate would raise employment levels. See Saint-Paul (2002) and Brügemann (2007) for theories on the political economy of employment protection.

While many labour economists use this competitive model as a benchmark, much of the macroeconomic literature views employment protection through the lens of the Diamond-Mortensen-Pissarides equilibrium unemployment model (Mortensen and Pissarides, 1994; Kugler *et al.*, 2003). As in the competitive model, dismissal costs in the equilibrium unemployment model curtail efficient separations by reducing the threshold productivity at which firms are willing to dismiss workers, thus reducing productivity. In contrast to the competitive model, however, worker-firm matches in the equilibrium unemployment setting generate quasi-rents, and the allocation of rents between firms and workers is typically determined through Nash bargaining. Nash bargaining exacerbates the deadweight loss from inefficient employment protections.[10] In the Nash bargain, dismissal costs reduce the firms' outside options or 'threat points,' causing workers' wage demands to rise even as profits fall. Facing lower profits and higher wage demands, firms curtail job creation and increase the threshold productivity at which they are willing to hire. The induced rise in reservation productivity potentially leads to an *increase* in firm-level productivity since less productive matches are not realised.[11] Hence, the net productivity effect is again ambiguous.

Although the competitive and equilibrium unemployment models differ in their details, both imply that dismissal protection dampens employment adjustments but has ambiguous effects on firms' productivity. On the other hand, both models indicate that if dismissal protection does *not* reduce job flows (perhaps because it satisfies Coasean equivalence), this protection should not affect productivity. These theoretical observations motivate our empirical approach. We begin by assessing whether adoptions of exceptions to the employment-at-will doctrine reduce job flows. We next turn to an analysis of their consequences for firm productivity. Because of the many possible avenues of adjustment noted above, our empirical work examines the impacts of dismissal protection on multiple plant-level production outcomes including capital investment, capital intensity, labour productivity and total factor productivity.

3. Data Description

Establishment-level data are essential for characterising how firms and their associated establishments respond to the passage of dismissal protection. This project draws such data from two confidential surveys collected by the Census Bureau – the Longitudinal Business Database (LBD) and the Annual Survey of Manufacturers (ASM). Each survey is described below, and Table 1 provides descriptive statistics.

3.1. *Longitudinal Business Database*

The LBD is a unique source for studying employment dynamics across manufacturing and non-manufacturing sectors. Sourced from IRS tax data and Census surveys, the LBD annually covers approximately 3.9 million establishments with positive

[10] Nash bargaining amplifies inefficiencies because it is non-Coasean; the initial allocation of property rights affects both the distribution of resources *and* the efficiency of bargained outcomes (Grout, 1984).

[11] Although productivity impacts are ambiguous, welfare consequences are generally negative, as in the competitive case above. If the search equilibrium is not initially constrained efficient, however, it is possible for policy interventions to improve aggregate efficiency (Pissarides, 2000, chapter 8).

Table 1

Descriptive Statistics for LBD and ASM, 1976–99

	Covered by Exceptions				
	Good faith	Public policy	Implied contract	Never covered	All states
Annual means of variable	(1)	(2)	(3)	(4)	(5)
(*a*) LBD State-SIC2 panel					
Employment change	13%	11%	11%	10%	11%
% Positive change	57%	58%	58%	60%	58%
Surveyed employment	15,078,526	55,598,270	56,151,488	5,387,148	68,091,479
% Manufacturing	25%	28%	28%	21%	28%
% Mining	1%	1%	1%	0%	1%
% Construction	7%	7%	7%	8%	7%
% Wholesale trade	8%	8%	8%	9%	8%
% Retail trade	26%	26%	26%	29%	26%
% Services	32%	29%	30%	32%	30%
% in Entering establishments	8%	7%	7%	8%	7%
% in Exiting establishments	7%	6%	6%	7%	6%
% Part of Multi-unit firms	53%	56%	55%	53%	55%
Surveyed establishments	869,860	3,106,760	3,188,694	333,504	3,871,392
Establishment entry rate	14%	13%	13%	15%	13%
Establishment exit rate	12%	11%	11%	12%	11%
% Part of Multi-unit firms	21%	23%	22%	23%	22%
Maximum states	12	43	43	3	50
(*b*) ASM manufacturing plant panel					
Plant employment change	11%	11%	11%	10%	11%
% Positive change	49%	49%	49%	50%	49%
Plant employment	790	747	766	658	746
% Non-production workers	32%	26%	27%	25%	26%
% Part of Multi-unit firm	98%	98%	98%	97%	98%
Total Installed capital (m)	\$33	\$30	\$31	\$27	\$31
Total Investment (m)	\$1.9	\$1.7	\$1.7	\$1.5	\$1.7
Labour productivity (k)	\$88	\$86	\$87	\$78	\$85
Maximum plants	794	4,848	4,601	323	5,666

Notes: Annual means for Columns 1-3 are calculated over all observations covered by the listed exception during the sample period, including any pre-coverage and post-coverage periods. Figures employ NBER deflators as described in text and are presented in 1999 dollars.

employment, representing over 68 million employees, in most US private industries. Panel (*a*) of Table 1 highlights that most of the LBD's surveyed employees are in the manufacturing, retail trade and services sectors. These percentages are fairly similar for states passing dismissal protections and those not doing so.[12]

The microdata first facilitate the development of complete state-industry-year panels of employment by summing employment counts across individual establishments. Publicly available series normally do not provide employment counts by state-industry; even when they do so, the Census Bureau is required to suppress values that compromise the confidentiality of individual establishments. Building from the microdata

[12] The LBD's sample frame during the 1976 to 1999 period includes mining; construction; manufacturing; wholesale trade; retail trade; and services (except hospitals, education services, social services, and private households). Sectors not included for the full panel are agriculture, forestry and fishing; transportation and public utilities; finance, insurance and real estate; and public administration. Jarmin and Miranda (2002) describe the construction of the LBD.

overcomes these limitations and a full employment panel is developed for the 1976 to 1999 sample frame.

From this state-industry-year panel, we can estimate absolute year-over-year employment changes. The mean absolute employment change over the sample is approximately 11%. This absolute job turnover metric aggregates over employment adjustments on the intensive margin (i.e., the hiring and firing of workers by continuing establishments) and the entry/exit margin. In the LBD, establishments are assigned unique and time-invariant identifiers that further afford longitudinal estimation of these two dimensions of adjustment. The entry and exit rates for establishments are approximately 13% and 11%, respectively. As many entering and exiting establishments are very small in size, only 7% and 6% of employees are working in entering or exiting establishments, respectively.[13] Finally, the survey's reporting structure affords the linkage of establishments to their parent firms. Approximately 22% of establishments and 55% of employees are part of multi-unit firms.

3.2. *Annual Survey of Manufacturers*

While the LBD provides a comprehensive view of employment dynamics across manufacturing and non-manufacturing sectors, reported data are limited to total employment and payroll only. To evaluate the impact of reduced job turnover for capital and productivity outcomes, we turn to two detailed surveys of manufacturers undertaken by the Census Bureau. The Census of Manufacturers (CM) collects operating data on all US manufacturing plants at five-year intervals (i.e., 1972, 1977, and so on). In between the CMs, the Census Bureau conducts the Annual Survey of Manufacturers (ASM). The ASM is a probability sampled subset of the CM, with the panel redrawn two years after each CM. Plants with more than 250 employees in the previous CM are sampled with certainty.

We extract from the ASM a balanced panel of all plants continuously monitored from 1972 to 1999. This restriction focuses on intensive adjustments in large plants operating in stable business climates; by conditioning on survival, the extensive margin is suppressed. While the approximately 5,700 plants represent less than 2% of all US manufacturing establishments, they account for over a quarter of total manufacturing activity in terms of employments and shipments. Almost all of these plants are part of multi-unit firms, although not all of the plants have sister establishments within this balanced panel.

Year-over-year employment changes are again studied. While the average annual employment change is again 11%, a larger fraction of these changes are negative, reflecting the trend decline in manufacturing employment from 1976 to 1999. In addition, the more detailed employment data for manufacturers allow us to examine production and non-production workers separately; the mean non-production worker employment share is 26%.

The continuous monitoring of this ASM panel affords the calculation of detailed capital stocks and productivity metrics. Capital stocks are calculated with the perpetual

[13] Dunne *et al.* (1989) and Kerr and Nanda (2006) provide additional descriptive statistics on entry and exit patterns in the manufacturing and non-manufacturing sectors, respectively.

inventory method, as explained below. The mean plant-level capital stock for the 1976 to 1999 sample is \$31m in 1999 dollars. Labour productivity is defined as deflated total value of shipments divided by total plant employment. Finally, we estimate total factor productivity (TFP) as the residual from a production function of value-added on four factors:production workers, non-production workers, machinery capital, and structures capital.

4. Consequences of Employment Protection

In this Section we discuss the impact of wrongful discharge protections on firm behaviour. We begin by examining the first-order effect of employment protection on employment fluctuations, both at the intensive (within-establishment) and extensive (entry/exit) margins. If wrongful discharge protections increase adjustment costs, this should lead to a reduction in hiring and dismissals, resulting in an overall dampening of employment fluctuations. We next test the impact of employment protection on employment levels, a margin along which prior research has obtained mixed results. Finally, we turn to the important question of whether the possibly restricted ability of businesses to adjust employment due to the introduction of employment protections has productivity consequences.

4.1. *Effects on Employment Fluctuations*

We estimate the effects of the wrongful discharge exceptions (i.e., good faith, public policy and implied contract) described in Section 1 on employment fluctuations using both the LBD and ASM. We begin by estimating the following equation using the LBD:

$$ABS_{sjt} = \boldsymbol{\lambda}_s + \boldsymbol{\kappa}_j + \boldsymbol{\tau}_t + \beta_{GF} GF_{st-1} + \beta_{PP} PP_{st-1} + \beta_{IC} IC_{st-1} + \varepsilon_{sjt}, \tag{1}$$

where ABS_{sjt} is the absolute year-to-year employment change of a two-digit SIC sector j, in state s, at time t,

$$ABS_{sjt} = \frac{|E_{sjt} - E_{sjt-1}|}{(E_{sjt} + E_{sjt-1})/2}.$$

$\boldsymbol{\lambda}_s$, $\boldsymbol{\kappa}_j$ and $\boldsymbol{\tau}_t$ are vectors of state, industry and time effects, respectively.[14] GF_{st-1}, PP_{st-1} and IC_{st-1} are indicators of whether the good faith, public policy and implied contract exceptions were in place in state s at time $t-1$.[15] Thus, the coefficients β_{GF}, β_{PP} and β_{IC} capture the effects of employment protection on annual net employment flows.[16]

[14] *ABS* is closely linked to Davis *et al.*'s (1996) job reallocation measure estimated at the sector level, which adds the average positive changes in employment in a sector to the average negative changes in employment in a sector. *ABS* is defined to be zero if both employments are zero. *ABS* is bounded between [0, 2], thereby minimising the influence of outliers.

[15] The one-year lag from the survey date is due to employment counts in the LBD and ASM usually being measured as of March 1st.

[16] In addition to the *ABS* measure, we estimated models that distinguish between positive and negative adjustments. Since we were unable to reject the hypothesis that the results are symmetric on both margins, we suppress tabulation of these results.

Table 2

Effects of Employment-at-will Doctrines on LBD Annual Employment Changes, 1976–99

Legal exception	State FE, SIC2 FE, YR FE (1)	Col. 1 plus State trends (2)	Col. 1 plus SIC2-YR FE (3)	Col. 1 plus State trends, SIC2-YR FE (4)
(*a*) LBD Absolute annual employment changes, full sample				
Good Faith	−0.005	−0.006	−0.004	−0.005
	(0.003)	(0.004)	(0.003)	(0.004)
Public Policy	0.002	0.003	0.002	0.004
	(0.002)	(0.003)	(0.002)	(0.003)
Implied Contract	0.000	0.001	0.001	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Obs.	51,074	51,074	51,074	51,074
(*b*) LBD Absolute annual employment changes, mfg. only				
Good Faith	−0.009	−0.016	−0.006	−0.013
	(0.005)	(0.007)	(0.005)	(0.007)
Public Policy	0.006	0.010	0.006	0.011
	(0.003)	(0.003)	(0.003)	(0.003)
Implied Contract	0.000	0.003	0.002	0.005
	(0.003)	(0.004)	(0.003)	(0.004)
Obs.	21,418	21,418	21,418	21,418

Notes: Huber-White robust standard errors clustered on state reported in parentheses.

Our core battery of specifications also includes two estimations of greater stringency. First, we consider models with state-specific time trends. These require that identification come from the discontinuity surrounding the passage of the wrongful discharge exception. These specifications can provide reassurance that our coefficients are not reflecting smoothly trending omitted variables that are potentially correlated with the adoption of the exceptions. A benefit of the state-industry panel is that we can also control for industry-specific trends using the non-parametric form of two-digit SIC industry and year interactions. These latter estimations allow us to control for employment shifts due to national trends in a state's industries, again providing confidence in the identification strategy.

Panels (*a*) and (*b*) of Table 2 report estimates of the effects of the wrongful discharge exceptions on employment fluctuations for the Full Sample and for manufacturing only. Panel (*a*) includes all LBD sectors: manufacturing, mining, construction, wholesale trade, retail trade and services. The reported standard errors account for possible error correlations across firms within a state and within states over time. We weight the samples using the mean employment level in the state-industry-year cells during the early 1976 to 1985 period. The results for the Full Sample show a decline in employment fluctuations following the introduction of the good faith exception, though the results are not statistically significant. By contrast, the results for the public policy and implied contract exceptions show insignificant positive impacts on employment fluctuations.

When we estimate these models for manufacturing alone in Panel (*b*), we find a negative, and in the majority of cases, significant, effect of the good faith exception on employment fluctuations. This result is robust to the inclusion of state-specific and industry-specific trends. It suggests a reduction in employment fluctuations of 5% to

12% after the introduction of the good faith exception (i.e., dividing the -0.006 to -0.016 coefficients by the average annual employment change of 13% in Table 1 for states adopting the good faith exception). The results for the implied contract exception remains insignificant. Surprisingly, we find a positive and significant impact of the public policy doctrine on employment fluctuations in the LBD data. This latter result, however, is not supported in the upcoming analysis of the more accurate, establishment-level ASM panel.

The initial LBD analysis suggests a significant effect of the good faith exception on employment fluctuations in manufacturing. To test whether this finding is consistent with a causal relationship, we evaluate the relationship between the adoption of the good faith exception and employment fluctuations using a dynamic specification:

$$\begin{aligned} ABS_{sjt} = \boldsymbol{\lambda}_s + \boldsymbol{\kappa}_j + \boldsymbol{\tau}_t + \sum_{q=-5}^{2} \beta_{GFt+q} \Delta GF_{st+q} + \sum_{q=-5}^{2} \beta_{PPt+q} \Delta PP_{st+q} + \sum_{q=-5}^{2} \beta_{ICt+q} \Delta IC_{st+q} \\ + \beta_{GFt-6} GF_{st-6} + \beta_{PPt-6} PP_{st-6} + \beta_{ICt-6} IC_{st-6} + \varepsilon_{sjt}, \qquad (2) \end{aligned}$$

where ΔGF_{st+q}, ΔPP_{st+q} and ΔIC_{st+q} indicate whether adoption occurred at year $t+q$. These dynamic variables capture the transitory effects of the reforms. GF_{st-6}, PP_{st-6} and IC_{st-6} estimate long-term outcomes by indicating adoptions that occurred at year $t-6$ or before. These coefficients are relative to the period three years prior to the reform, and their pattern indicates whether the earlier pre-post results (1) are consistent with a causal interpretation. In particular, we would be concerned if there are large and statistically significant coefficients on the lead indicators, regardless of whether they are positive or negative. The specification also helps identify whether the largest impacts of the exceptions occur over the short run or long run.[17] To conserve space, we only tabulate coefficients for the good faith exception, with full results for the public policy and implied contract exceptions given in the companion working paper version of this article (Autor *et al.*, 2007).

Appendix Table 1 presents results from this dynamic specification estimated for the manufacturing sector, as well as additional specifications including state-specific and industry-specific trends. The basic specification shows negative coefficients for the good faith lags but mostly insignificant and weakly positive coefficients for the leads, thus supporting a causal interpretation of our results. That is, the introduction of the exception precedes employment changes and not *vice versa*. By contrast, the public policy and implied contract leads and lags have typically positive coefficients, as is shown in Appendix Table 1 of Autor *et al.* (2007). The pattern for the public policy doctrine is particularly noteworthy since it suggests that the unexpected positive estimate for the impact of this doctrine on employment fluctuations found in Table 2 is likely to be spurious. These patterns are robust to the inclusion of state-specific and industry-specific trends. The results using the LBD suggest that the impact of the good faith doctrine peaks approximately three years following adoption and that the long-

[17] The dynamic estimation also includes a second set of lead and lag variables to account for the four cases in which legal exceptions were formally abandoned. The inclusion or exclusion of these additional regressors does not materially influence the reported results.

Table 3

Effects of Employment-at-will Doctrines on ASM Annual and Quarterly Employment Changes, 1976–99

Legal Exception	State FE, SIC2 FE, YR FE (1)	Col. 1 plus State trends (2)	Col. 1 plus SIC2-YR FE (3)	Col. 1 plus State trends, SIC2-YR FE (4)	Plant FE, YR FE (5)	Col. 5 plus State trends, SIC2-YR FE (6)
(*a*) ASM Absolute annual employment changes						
Good Faith	−0.004	−0.005	−0.003	−0.005	−0.006	−0.002
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Public Policy	0.000	0.001	−0.001	0.000	0.000	−0.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Implied Contract	0.000	0.004	0.001	0.003	−0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Obs.	135,937	135,937	135,937	135,937	135,937	135,937
(*b*) ASM Production worker quarterly churn						
Good Faith	−0.005	0.005	−0.003	0.006	−0.004	0.004
	(0.009)	(0.006)	(0.008)	(0.005)	(0.009)	(0.003)
Public Policy	0.005	0.003	0.003	0.003	0.005	0.004
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Implied Contract	−0.004	−0.003	−0.002	−0.003	−0.004	−0.002
	(0.005)	(0.004)	(0.003)	(0.003)	(0.005)	(0.003)
Obs.	135,976	135,976	135,976	135,976	135,976	135,976

Notes: Huber-White robust standard errors clustered on state reported in parentheses.

term effect is insignificant (i.e., six or more years following adoption). This pattern is comparable to Autor *et al.* (2006), who report that the near-term effects of adoption of wrongful discharge doctrines dissipate within approximately five years, perhaps because the initial market uncertainty about the potentially vast – but ultimately modest scope – of the protection offered is resolved (Edelman *et al.*, 1992).[18]

Table 2's results from the LBD suggest that manufacturing was particularly affected by the introduction of wrongful discharge exceptions, likely because manufacturing employment is highly seasonal and highly cyclical, making dismissal protections particularly costly.[19] We use plant-level data from the ASM to further examine the effects of employment protection in manufacturing. Panel (*a*) of Table 3 presents analogous results to those using the LBD in Table 2. Because our ASM sample uses a balanced panel of ongoing plants, we can now add plant fixed effects to the prior specification, leading to the following estimating equation:

$$ABS_{pt} = \mu_p + \tau_t + \beta_{GF} GF_{st-1} + \beta_{PP} PP_{st-1} + \beta_{IC} IC_{st-1} + \varepsilon_{pt}. \tag{3}$$

The dependent variable is the absolute year-to-year employment change in plant p from $t-1$ to t; μ_p is a plant fixed effect. As before, we include state-specific and industry-

[18] Only 13 states introduced good faith exceptions during the period studied. California introduced the first good faith exception in a highly visible court ruling. Though our basic and dynamic LBD results on employment changes are strongest for the full sample of states, the results are qualitatively similar but less precise when California is excluded.

[19] The mean year-to-year turnover in manufacturing was 12%, compared to 10% in construction, 6% in wholesale trade, 7% in retail trade, and 8% in services. Only mining had a higher annual turnover (27%). Regressions examining the mining sector also find a substantial dampening of annual employment volatility following the adoption of the good faith exception.

specific trends. The estimated standard errors again allow for error correlations across plants within states and within states over time.[20]

Consistent with the LBD, the results using the ASM suggest that the good faith exception reduces employment fluctuations. We do not find evidence, however, in the ASM sample that the implied contract or public policy doctrines impact employment fluctuations. We estimate in Table 3 that the good faith exception reduces employment fluctuations by 1.5% to 4.5%, which is about half the size of the estimate using the LBD data. While this finding is only marginally statistically significant, supporting evidence from dynamic specifications below suggest that the effect is likely to be causal.

The difference between the LBD and ASM results is explained in part by the fact that in the ASM we can control for additional unobservable factors affecting a plant's employment fluctuations. Contrasting Columns (4) and (6), with and without plant fixed effects, we can see that excluding plant effects using our ASM sample implies a reduction of 5% in employment fluctuations as opposed to 2% with plant effects. As is shown in the next Sections, the remaining differences between the estimates in the LBD and the ASM samples are likely due to the fact that the LBD includes entering and exiting business while the ASM sample is composed of a balanced sample of ongoing plants. The ASM analysis therefore excludes any effect of wrongful discharge protections on employment fluctuations occurring through entry and exit.

As with the LBD, we also estimate a dynamic specification using ASM data. Panel (*a*) of Appendix Table 2 presents these estimates. Similar to the patterns found with the LBD, leads of the good faith exception are found to have weakly positive and insignificant effects on employment fluctuations while lags of the good faith exception have negative effects on employment changes. The maximum dampening is again attained three years following adoption. As with the LBD estimates, the long-term impacts are insignificant and, in the case of the ASM, weakly positive.[21]

Since employment protection may also affect seasonal employment fluctuations (Wolfers, 2005), we also study a *quarterly* employment churn measure to complement the year-over-year changes. In particular, we estimate (3) using as a dependent variable the following measure of quarterly churn for production workers:[22]

$$PWChurn_{pt} = \frac{PW_{pt}^{\max} - PW_{pt}^{\min}}{(PW_{pt}^{\max} + PW_{pt}^{\min})/2},$$

where $PW_{pt}^{\max}$ and $PW_{pt}^{\min}$ are the maximum and the minimum quarterly production-worker employment in plant p in year t, respectively. As before we allow for state-specific and industry-specific trends and cluster the standard errors on state.

[20] We have also estimated analogous models using an unbalanced panel of ASM plants (i.e., not limited to those continually operating). Findings from these models, which are qualitatively similar, are available from the authors on request.

[21] As for the LBD results, the ASM findings are qualitatively similar but somewhat less precise when we exclude California from our sample. As a complement to the panel estimations, similar results are found with lagged dependent variable specifications that test for mean reversion.

[22] The ASM does not collect quarterly employment for non-production workers. The plant-level ASM does not allow us to estimate employment effects separately by demographic group, as is feasible with data from household surveys as in Kahn (2007).

Panel (*b*) of Table 3 shows results for these specifications. Estimates without state trends show negative effects of the good faith exception on seasonal adjustments of production workers, but the results are not significant and the effects become positive when controlling for state trends. Looking deeper, however, the results from the underlying dynamic specifications reported in Panel (*b*) of Appendix Table 2 consistently show weakly positive coefficients on the leads and negative coefficients on the lags. Moreover, the dampening is again most significant three years after the adoption. Thus, the dynamic specifications appear most consistent with a significant causal effect of the good faith exception on seasonal employment fluctuations over the short-run to medium-run.

4.2. *Effects on Entry and Exit*

The difference in the magnitudes of the estimated effects of wrongful discharge exceptions on employment fluctuations in the LBD and the ASM samples suggests that part of the reduction in employment fluctuations observed following adoption of the good faith exception is explained by changes in firm entry and exit (i.e., the extensive employment margin). To evaluate the importance of external adjustment, we use the LBD to estimate regressions similar to (1), where the dependent variable is the log of the average count of plants over five-year intervals among continuing, entering and exiting businesses. We use five-year averages to minimise the possibility of capturing spurious entry and exit due to 'ghosting' and reporting bumps observed surrounding Census years. The wrongful discharge indicators take the value of one if the exceptions had been adopted as of the midpoint of the five-year intervals.[23]

Panels (*a*)–(*d*) of Table 4 report results of these regressions for total, continuing, entering and exiting plants, respectively. Panel (*a*) shows little change in the total count of plants in response to the introduction of any of the exceptions. However, Panels (*b*)–(*d*) show that in the case of the good faith exception, this reflects countervailing forces among continuing and other plants. Panel (*b*) shows an increase in plant survival after the introduction of good faith exceptions, though this effect is only marginally significant. Panel (*c*) shows that entry is substantially reduced in manufacturing after the introduction of good faith exceptions, though exit is unaffected. These results, controlling for state-specific and industry-specific trends, suggest a reduction of 7.7 log points in the number of entering plants, where log points refer to 100 times the coefficient in the log-linear specification (thus roughly corresponding to percentage point changes).[24] This translates into a reduction of about 9,000 establishments. By contrast, the public policy and implied contract exceptions do not appear to affect entry.

[23] Annual regressions of entry and exit yield quantitatively similar results, though the magnitudes are smaller. Studying entry and exit at five-year intervals avoids spurious peaks of entry and exit rates surrounding Census years, when additional manpower is devoted to updating the business registry. This updating has a noticeable effect on establishment counts but not on summed employment levels used for year-to-year employment changes. Entry and exit are defined as the first and last year an establishment is observed in the LBD, respectively, with the end years of the sample excluded. Establishments alive for a single year are recorded as both entering and exiting. This procedure ignores potential exit and re-entry by establishments, but more importantly avoids spurious entry and exit from 'ghosting' establishments with poor longitudinal linkages.

[24] This result is consistent with Kugler and Pica (forthcoming), who find that increased dismissal costs in Italy after the 1990 labour market reform reduced entry of small firms.

Table 4

Effects of Employment-at-will Doctrines on LBD Plant Counts, 1978–97

Legal exception	State FE, SIC2 FE, YR FE (1)	Col. 1 plus State trends (2)	Col. 1 plus SIC2-YR FE (3)	Col. 1 plus State trends, SIC2-YR FE (4)
(*a*) LBD Mfg. log count of all plants				
Good Faith	0.027	0.026	0.003	0.020
	(0.023)	(0.013)	(0.032)	(0.026)
Public Policy	0.061	0.008	0.068	0.006
	(0.021)	(0.006)	(0.026)	(0.023)
Implied Contract	−0.012	−0.017	−0.019	−0.017
	(0.019)	(0.007)	(0.025)	(0.016)
Obs.	3,911	3,911	3,911	3,911
(*b*) LBD Mfg. log count of continuing plants				
Good Faith	0.047	0.040	0.021	0.035
	(0.027)	(0.015)	(0.036)	(0.027)
Public Policy	0.068	0.008	0.076	0.007
	(0.024)	(0.007)	(0.029)	(0.024)
Implied Contract	−0.013	−0.015	−0.021	−0.015
	(0.022)	(0.008)	(0.027)	(0.017)
Obs.	3,891	3,891	3,891	3,891
(c) LBD Mfg. log count of entering plants				
Good Faith	−0.117	−0.068	−0.131	−0.077
	(0.031)	(0.028)	(0.032)	(0.034)
Public Policy	0.016	−0.011	0.015	−0.019
	(0.028)	(0.028)	(0.030)	(0.033)
Implied Contract	−0.021	−0.030	−0.016	−0.030
	(0.024)	(0.031)	(0.028)	(0.034)
Obs.	3,846	3,846	3,846	3,846
(*d*) LBD Mfg. log count of exiting plants				
Good Faith	0.011	0.005	−0.012	−0.002
	(0.026)	(0.034)	(0.029)	(0.043)
Public Policy	0.063	0.048	0.068	0.042
	(0.018)	(0.022)	(0.023)	(0.032)
Implied Contract	−0.006	−0.022	−0.009	−0.021
	(0.020)	(0.022)	(0.023)	(0.025)
Obs.	3,862	3,862	3,862	3,862

Notes: Five-year blocks. Huber-White robust standard errors clustered on state-year reported in parentheses.

In combination with the findings in Tables 2 and 3, these results suggest that the dampening effect of the good faith exception on employment fluctuations operates through two channels: a reduction in net employment flows in ongoing plants and a reduction in the entry of new plants. The reduced rate of establishment creation helps reconcile the smaller decline in employment fluctuations evident in the ASM panel compared to the LBD.[25]

4.3. *Effects on Employment Levels*

Here we explore the effects of wrongful discharge exceptions on employment levels. As discussed, the effect of these dismissal protections on net employment is theoretically ambiguous (at least in the short run) since both dismissals and hiring are affected.

[25] See Koeniger and Prat (2007) for analysis of product market regulation and the extensive margin.

Table 5

Effects of Employment-at-will Doctrines on ASM Employment Levels, 1976–99

Legal exception	State FE, SIC2 FE, YR FE (1)	Col. 1 plus State trends (2)	Col. 1 plus SIC2-YR FE (3)	Col. 1 plus State trends, SIC2-YR FE (4)	Plant FE, YR FE (5)	Col. 5 plus State trends, SIC2-YR FE (6)
(*a*) ASM Log total employment						
Good Faith	0.025	0.079	0.015	0.068	0.033	0.020
	(0.033)	(0.036)	(0.030)	(0.032)	(0.031)	(0.025)
Public Policy	0.015	−0.002	0.014	−0.001	0.016	0.010
	(0.016)	(0.013)	(0.014)	(0.012)	(0.016)	(0.013)
Implied Contract	−0.012	−0.011	−0.004	−0.005	−0.012	−0.009
	(0.018)	(0.012)	(0.015)	(0.011)	(0.018)	(0.015)
Obs.	135,976	135,976	135,976	135,976	135,976	135,976
(*b*) ASM Log production worker employment						
Good Faith	−0.002	0.083	−0.008	0.070	0.003	−0.002
	(0.034)	(0.039)	(0.030)	(0.034)	(0.032)	(0.025)
Public Policy	0.019	0.000	0.017	0.000	0.021	0.014
	(0.017)	(0.013)	(0.014)	(0.012)	(0.017)	(0.013)
Implied Contract	−0.010	−0.011	0.001	−0.003	−0.009	−0.004
	(0.020)	(0.013)	(0.016)	(0.012)	(0.020)	(0.016)
Obs.	135,932	135,932	135,932	135,932	135,932	135,932
(*c*) ASM Log non−production worker employment						
Good Faith	0.052	0.079	0.040	0.071	0.065	0.048
	(0.035)	(0.026)	(0.033)	(0.025)	(0.037)	(0.035)
Public Policy	0.012	−0.009	0.014	−0.005	0.009	0.008
	(0.018)	(0.013)	(0.016)	(0.013)	(0.018)	(0.015)
Implied Contract	−0.014	−0.009	−0.008	−0.005	−0.018	−0.012
	(0.017)	(0.011)	(0.016)	(0.011)	(0.018)	(0.015)
Obs.	135,232	135,232	135,232	135,232	135,232	135,232

Notes: Huber-White robust standard errors clustered on state reported in parentheses.

We start by estimating similar regressions to (3) using the ASM data, but where the dependent variable is the log of employment in plant p at time t. Table 5 presents results of these regressions for total employment as well as for production and non-production employment separately. Panel (*a*) shows that total employment increases with the introduction of the good faith and public policy exceptions, though the public policy results are insignificant. The implied contract exception has a negative though insignificant effect on employment, which is consistent in sign and magnitude – though not statistical significance – with the findings in Autor *et al.* (2004, 2006).

When employment is disaggregated into production and non-production workers, we find that the increase in total employment following the introduction of the good faith exception is driven primarily by the increase in employment of non-production workers. For example, the final columns of Panels (*b*) and (*c*) suggest that production employment does not react to the introduction of the good faith exception while non-production employment in the typical plant increases by 4.8 log points following the introduction of this exception.[26] This differential rise in non-production demand may be explained by capital–skill complementarity (Griliches, 1969; Berman *et al.*, 1994), as

[26] These results are robust to various specifications and to the exclusion of California and Arizona, even though Arizona had unusually high employment growth during the 1980s and 1990s.

Section 4 will show, the adoption of the good faith exception may have spurred capital-deepening in firms.

As before we also estimate dynamic specifications to test whether our findings are consistent with a causal interpretation. In these specifications, found in Appendix Table 2 of our working paper (Autor *et al.*, 2007), the estimated impact of the good faith exception on employment levels commences a year prior to adoption and becomes puzzlingly large in subsequent years when state-specific trends are included, exceeding 10 log points six years ahead. This pattern is very unlikely to reflect a causal relationship and suggests the presence of confounding shocks. A potential explanation is that California and Arizona, the two largest states that adopted a good faith exception, experienced unusually strong employment growth in the late 1980s, likely for reasons unrelated to this particular legal doctrine.[27] The results for the public policy exception have inconsistent signs across specifications and show no evidence of a trend break after adoption of the doctrine. By contrast, results for the implied contract exception show consistently negative effects for both leads and lags, though the lead effects are smaller.

Table 6 shows results from regressions similar to (1) using LBD data, but where the dependent variable is the log of average manufacturing employment in state *s* and industry *j* over five-year intervals and where the wrongful discharge indicators take the value of one if the exceptions had been adopted as of the midpoint of the five-year intervals. Panel (*a*) presents results for all plants, while Panels (*b*)–(*d*) present results for continuing, entering and exiting plants, respectively.[28]

Consistent with the results from the ASM, which includes only continuing plants, we find that total employment in the LBD sample increased by about 7.8 log points following the adoption the good faith exception. Examining employment separately for continuing, entering and exiting plants in Panels (*b*)–(*d*) shows that this growth is driven by continuing plants. Panel (*b*) shows a significant increase in employment of 8.3 log points in continuing plants, while Panels (*c*) and (*d*) show a marginal decline in employment created by plant entry and an increase in employment lost due to plant exit, although these two effects are not statistically significant. Note also the close comparability of the estimated effects of the good faith exception on employment levels in the ASM sample (Table 5, (*a*)) and on employment levels in ongoing plants in the LBD (Table 6, (*b*)). This pattern is expected since the ASM sample is composed exclusively of ongoing plants. In summary, the net growth of employment that we observe after adoption of the good faith exception is accounted for by reduced job creation in entering plants and increased job destruction in exiting plans – both of which led to reduced employment – accompanied by more than offsetting employment growth in ongoing plants.

As with the ASM, dynamic specifications (found in Appendix Table 3 of our companion working paper) show positive coefficients on the good faith exception's leads

[27] In fact, excluding California from the estimates largely eliminates the estimated positive employment effects of the good faith exception. However, California is arguably the strongest test-case for evaluating the labour market impact of this exception since the *Cleary* decision is *the* landmark case among good faith rulings. We are accordingly reluctant to remove California from the sample. Excluding Arizona reduces but does not eliminate the estimated positive employment effect.

[28] Annual employment regressions yield quantitatively similar results, though the magnitudes of the coefficients are smaller. We use employment at five-year intervals here to keep consistency with the results on the counts of entering and exiting plants presented in the previous Section.

Table 6

Effects of Employment-at-will Doctrines on LBD Employment Levels, 1978–97

Legal exception	State FE, SIC2 FE, YR FE (1)	Col. 1 plus State trends (2)	Col. 1 plus SIC2-YR FE (3)	Col. 1 plus State trends, SIC2-YR FE (4)
(*a*) LBD Mfg. log empl. in all plants				
Good Faith	0.061	0.092	0.045	0.078
	(0.044)	(0.028)	(0.053)	(0.044)
Public Policy	0.066	0.015	0.064	0.011
	(0.025)	(0.016)	(0.031)	(0.032)
Implied Contract	−0.027	−0.009	−0.032	−0.012
	(0.028)	(0.018)	(0.033)	(0.027)
Obs.	3,911	3,911	3,911	3,911
(*b*) LBD Mfg. log empl. in continuing plants				
Good Faith	0.067	0.096	0.051	0.083
	(0.045)	(0.029)	(0.054)	(0.044)
Public Policy	0.067	0.008	0.065	0.005
	(0.026)	(0.016)	(0.032)	(0.032)
Implied Contract	−0.030	−0.015	−0.034	−0.017
	(0.028)	(0.018)	(0.033)	(0.028)
Obs.	3,891	3,891	3,891	3,891
(*c*) LBD Mfg. log empl. in entering plants				
Good Faith	−0.004	0.014	−0.023	−0.001
	(0.048)	(0.056)	(0.053)	(0.068)
Public Policy	0.035	0.127	0.029	0.106
	(0.046)	(0.047)	(0.047)	(0.052)
Implied Contract	0.021	0.123	0.020	0.113
	(0.045)	(0.058)	(0.046)	(0.056)
Obs.	3,846	3,846	3,846	3,846
(*d*) LBD Mfg. log empl. in exiting plants				
Good Faith	0.073	0.075	0.048	0.066
	(0.048)	(0.093)	(0.056)	(0.112)
Public Policy	0.080	0.140	0.070	0.111
	(0.038)	(0.050)	(0.040)	(0.056)
Implied Contract	0.019	0.133	0.016	0.124
	(0.040)	(0.044)	(0.044)	(0.045)
Obs.	3,862	3,862	3,862	3,862

Notes: Five-year blocks. Huber-White robust standard errors clustered on state-year reported in parentheses.

and implausibly large positive coefficients on the lags, making questionable a causal interpretation of the effects on employment. The results for the public policy and implied contract doctrines are comparable to the prior estimates.[29]

4.4. *Effects on Productivity*

The finding that the good faith exception reduces job flows is consistent with the expectation that this discharge protection raises firms' adjustments costs. Here we explore the consequences of this rise in adjustment costs on other margins of non-labour adjustment. One such margin is capital substitution; if discharge protections

[29] Table 6 also shows economically large, although inconsistent, effects of the public policy exception on employment levels. Similar to earlier studies, this pattern raises puzzles about the interpretation between the public policy exception and employment.

raise the effective price of labour by making it more expensive to hire and fire, firms may substitute towards other inputs. Second, given the restrictions on firms' ability to adjust, we also may expect total factor productivity to be affected – though as noted in Section (2), compositional shifts in worker hiring following the adoption of dismissal protections may generate countervailing effects on labour productivity.

We begin by examining whether productivity was affected by employment protections due to changes in input composition. In particular, we ask whether the introduction of employment protections affected capital investment and, subsequently, capital–labour ratios. Panels (*a*) and (*b*) of Table 7 report results of specifications similar to (1) and (3) without and with state-specific and industry-specific trends but where the dependent variables are the log of total capital investment and the log of the capital–labour ratio.

Capital stocks are measured at the beginning-of-year and constructed using the perpetual inventory method. Capital stocks are separately calculated for machinery and structures and then aggregated for total capital metrics. The capital stock of plant p in industry j at time t is:

$$K_{pt} = \left(1 - \delta_{jt-1}\right) K_{pt-1} + \frac{I^N_{pt-1}}{P^N_{Ijt-1}} + \frac{I^U_{pt-1}}{P^N_{Ijt-3}},$$

where initial capital stocks in 1972 are obtained by deflating book values of capital by BEA two-digit SIC deflators for installed capital. New equipment investments, I^N_{pt-1}, are deflated with NBER four-digit SIC new-capital deflators, P^N_{Ijt-1}. Used equipment purchases, I^U_{pt-1}, employ the NBER four-digit SIC deflators lagged three periods. The annual depreciation rates, δ_{jt-1}, are obtained from the BEA by two-digit SIC industries.

Table 7

Effects of Employment-at-will Doctrines on ASM Capital Investment, 1976–99

Legal exception	State FE, SIC2 FE, YR FE (1)	Col. 1 plus State trends (2)	Col. 1 plus SIC2-YR FE (3)	Col. 1 plus State trends, SIC2-YR FE (4)	Plant FE, YR FE (5)	Col. 5 plus State trends, SIC2-YR FE (6)
(*a*) ASM Log total capital investment						
Good Faith	0.087	0.146	0.059	0.122	0.092	0.065
	(0.037)	(0.057)	(0.034)	(0.050)	(0.035)	(0.031)
Public Policy	0.019	0.001	0.023	0.005	0.022	0.024
	(0.025)	(0.027)	(0.025)	(0.024)	(0.025)	(0.025)
Implied Contract	0.012	−0.002	0.011	0.003	0.009	0.005
	(0.020)	(0.024)	(0.020)	(0.021)	(0.021)	(0.022)
Obs.	133,608	133,608	133,608	133,608	133,608	133,608
(*b*) ASM Log total capital−labour ratio						
Good Faith	0.056	−0.022	0.034	−0.021	0.045	0.046
	(0.027)	(0.024)	(0.021)	(0.024)	(0.026)	(0.012)
Public Policy	−0.028	0.000	−0.025	−0.003	−0.027	−0.020
	(0.019)	(0.013)	(0.014)	(0.011)	(0.019)	(0.012)
Implied Contract	0.024	0.016	0.011	0.008	0.020	0.011
	(0.021)	(0.011)	(0.015)	(0.010)	(0.021)	(0.014)
Obs.	119,181	119,181	119,181	119,181	119,181	119,181

Notes: Huber-White robust standard errors clustered on state reported in parentheses.

Panel (*a*) of Table 7 shows a positive and significant effect of the introduction of the good faith exception on total investment (machinery and structures) of 6.5 log points (Column 6) but show no effects from the introduction of the public policy and implied contract exceptions. Dynamic specifications, found in Appendix Table 4 of the companion working paper, indicate that capital investment peaks several years after adoption of the good faith exception and then declines somewhat thereafter (a pattern similar to the results for employment fluctuations). However, leads of the good faith adoption variable in the dynamic specifications are notably negative (though statistically insignificant), suggesting that part of the post-adoption rise in capital investment may reflect an investment rebound from an earlier downturn.

Not surprisingly given the increase in employment levels, Panel (*b*) of Table 7 shows mixed – albeit generally positive – effects of the good faith exception on capital-labour ratios. For example, estimated effects are negative when controlling for state-specific trends but become positive when controlling for plant effects. Dynamic specifications, given in Appendix Table 5 of the working paper, find negative coefficients on both leads and lags, raising the question of whether the introduction of the good faith exception followed rather than preceded increases in the capital-labour ratio.

To explore effects on productivity, Panel (*a*) of Table 8 presents results of specifications like (1) and (3), but where the dependent variables is a TFP measure estimated using a production function residual methodology. For the residuals methodology, we

Table 8

Effects of Employment-at-will Doctrines on ASM Labour Productivity and Total Factor Productivity, 1976–99

Legal xception	State FE, SIC2 FE, YR FE (1)	Col. 1 plus State trends (2)	Col. 1 plus SIC2-YR FE (3)	Col. 1 plus State trends, SIC2-YR FE (4)	Plant FE, YR FE (5)	Col. 5 plus State trends, SIC2-YR FE (6)
(*a*) ASM Total factor productivity						
Good Faith	−0.019	−0.023	−0.017	−0.022	−0.020	−0.014
	(0.014)	(0.009)	(0.014)	(0.009)	(0.015)	(0.008)
Public Policy	0.006	0.004	0.005	0.004	0.004	0.002
	(0.008)	(0.007)	(0.008)	(0.007)	(0.009)	(0.008)
Implied Contract	−0.005	−0.003	−0.005	−0.003	−0.004	−0.006
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
Obs.	110,881	110,881	110,881	110,881	110,881	110,881
(*b*) ASM Log labour productivity						
Good Faith	0.050	−0.004	0.051	0.002	0.039	0.044
	(0.018)	(0.008)	(0.015)	(0.008)	(0.017)	(0.011)
Public Policy	−0.004	0.004	−0.008	0.005	−0.003	−0.008
	(0.010)	(0.005)	(0.009)	(0.006)	(0.011)	(0.009)
Implied Contract	0.001	0.005	0.001	0.006	−0.002	−0.002
	(0.011)	(0.007)	(0.010)	(0.007)	(0.011)	(0.009)
Obs.	135,972	135,972	135,972	135,972	135,972	135,972

Notes: Huber-White robust standard errors clustered on state reported in parentheses. TFP is the establishment-level residual from a regression of value-added on four factors of production (production employment, non-production employment, machinery capital and structures capital) at the industry-year level.

first estimate the following production function in logs for each two-digit SIC industry and year using ordinary least squares:

$$\log(Y_{pt}) = \alpha + \varsigma_{jt}\log(L_{pt}) + \gamma_{jt}\log(H_{pt}) + \theta^M_{jt}\log(K^M_{pt}) + \theta^S_{jt}\log(K^S_{pt}) + \xi_{pt},$$

where Y_{pt} is value added (i.e., total value of shipments net of materials/fuels costs and inventory adjustments) in plant p at time t deflated using the NBER four-digit SIC shipments deflators, L_{pt} is the count of production or unskilled workers, and H_{pt} is the count of non-production or skilled workers. K^M_{pt} and K^S_{pt} are the separated machinery and structures capital stocks, respectively. The residuals from the regression above provide our TFP measure:

$$TFP_{pt} = \log(Y_{pt}) - \hat{\varsigma}_{jt}\log(L_{pt}) - \hat{\gamma}_{jt}\log(H_{pt}) - \hat{\theta}^M_{jt}\log(K^M_{pt}) - \hat{\theta}^S_{jt}\log(K^S_{pt}) - \alpha.$$

The results in Table 8 show a uniformly negative and generally significant effect of the introduction of the good faith exception on TFP, though the effect is slightly attenuated when we control for plant effects. By contrast, the public policy exception appears to have a positive effect and the implied contract exception appears to have a negative effect, though neither is significant in any specification.[30] However, results from dynamic specifications, found in Appendix Table 3, show mostly negative coefficients for both leads and lags of the good faith exception, though the coefficients on the lags are substantially larger. The dynamic specification thus raises some question about a causal interpretation of the good faith effects on productivity, though the available evidence is mostly consistent with a reduction in TFP in the four years following adoption of the good faith exception.

Finally, Panel (*b*) of Table 8 explores whether the increase in capital investment following the introduction of the good faith exception found in Table 7 affected labour productivity. We estimate that labour productivity rose substantially (by 1 to 4 log points) following adoption of the good faith exception. This measured rise in labour productivity follows from the fact that both capital investment and non-production worker employment (Tables 5 and 7) rose after adoption of the good faith exception. Since our labour productivity measure does not adjust for the quality of labour inputs, the rise in raw labour productivity is likely to reflect a mixture of capital deepening and compositional shifts in labour quality. Thus, this finding is not at odds with the conclusion that the good faith exception reduced TFP. Results from dynamic specifications for labour productivity that control for plant effects (Appendix Table 7 of the working paper) show negative coefficients on the leads and positive though

[30] We also examined a cost-shares methodology to analyse TFP. Cost shares were estimated for three-digit SIC industries from the NBER productivity database (Bartelsman and Gray, 1996). Production worker, non-production worker, and materials and fuels cost shares are calculated relative to TVS; the cost share of capital is a residual such that the cost shares sum to one. The results of the TFP measure obtained using a cost-shares methodology are generally declining but more mixed. However, the cost-shares methodology presents several disadvantages:
(1) the coefficients on the shares are out-of-sample estimates obtained using NBER data;
(2) we cannot disaggregate between equipment and structures since the capital share is obtained as a residual; and
(3) the cost-shares methodology assumes constant returns-to-scale in the production function and perfectly competitive input markets.

insignificant coefficients on the lags, suggesting that the good faith exception preceded the increase in labour productivity.

5. Conclusions

This article makes two contributions. The first is to exploit microdata to examine the effect of dismissal protection on establishment-level outcomes in a representative sample of employers. The second is to consider simultaneously the effects of this protection on job flows — where there are unambiguous theoretical implications – and on several other important (and mostly unstudied) margins of firm behaviour, including capital investment, labour productivity and total factor productivity, where the predictions of theory are less clear cut. We believe that the power of the analysis derives from the evidence that the adoption of one particular dismissal protection, the good faith exception to employment-at-will, reduced employment fluctuations in adopting states. These effects were largest in the first three years following adoption and diminished thereafter. This finding indicates that adjustment costs rose in the short run, a necessary condition for there to be an impact on economic efficiency.

The finding on employment fluctuations motivates us to analyse how this short-run rise in adjustment costs impacted firms' choices of capital and labour inputs, and ultimately, their productivity. The most surprising result of our analysis is that the increase in adjustment costs appears to have spurred capital and skill deepening – that is, firms raised capital investment and increased non-production worker employment. These changes in input choices led to a rise in measured (non-quality-adjusted) labour productivity and a decline in total factor productivity. This last finding is potentially quite important because, if correct, it provides confirmation that exogenous increases in adjustment costs reduce efficiency.

Our findings also present two unresolved puzzles. First, the adoption of the good faith exception appears to follow (likely by coincidence) a major investment downturn. This pattern reduces our confidence in the causal interpretation of the rise in capital investment following adoption of the good faith exception. The second puzzle is that the estimated positive effect of the good faith exception on employment levels is larger than appears plausible (albeit often imprecisely estimated). In light of these puzzles, we view our findings as suggestive but inconclusive. Though our data support the hypothesis that adoption of the good faith exception raises adjustment costs, the anomalous results for employment levels suggests a cautious interpretation of the findings until further evidence accumulates.

Our results have interesting parallels with those of a recent study by Acemoglu and Finkelstein (2005) of firm-level responses to changes in labour costs in the US hospital industry. Responding to a change in Medicare reimbursement policy in the 1980s that effectively increased the cost of labour relative to capital, Acemoglu and Finkelstein document that hospitals raised both their capital-labour ratios and the skill composition of their workforces. Acemoglu and Finkelstein suggest that this result may be explained by either capital-skill complementarity or technology-skill complementarity (assuming that new capital investments embed recent technologies), as in our discussion above. While the Acemoglu and Finkelstein findings are drawn from a distinctly different economic context than our study (i.e., a heavily regulated sector versus a relatively competitive sector) and exploit a different source of policy variation (i.e., employment subsidies rather than dismissal costs), the parallels with our findings for the effect of dismissal protection on the US manufacturing sector are nonetheless intriguing and deserving of further consideration.

Appendix

Table 1

Dynamic Effects of Employment-at-will Doctrines on LBD Manufacturing Absolute Annual Employment Changes, 1976–99

Legal exception	State FE, SIC2 FE, YR FE (1)	Col. 1 plus State trends (2)	Col. 1 plus SIC2-YR FE (3)	Col. 1 plus State trends, SIC2-YR FE (4)
ΔGF^{t+2}	0.009 (0.010)	0.008 (0.011)	0.008 (0.009)	0.007 (0.010)
ΔGF^{t+1}	0.017 (0.010)	0.016 (0.011)	0.017 (0.009)	0.016 (0.010)
ΔGF	0.003 (0.009)	0.003 (0.010)	0.002 (0.009)	0.002 (0.010)
ΔGF^{t-1}	−0.007 (0.009)	−0.009 (0.010)	−0.005 (0.008)	−0.006 (0.010)
ΔGF^{t-2}	−0.004 (0.010)	−0.005 (0.011)	−0.002 (0.009)	−0.003 (0.010)
ΔGF^{t-3}	−0.026 (0.011)	−0.027 (0.014)	−0.021 (0.011)	−0.023 (0.012)
ΔGF^{t-4}	−0.006 (0.011)	−0.007 (0.013)	−0.005 (0.011)	−0.007 (0.012)
ΔGF^{t-5}	−0.011 (0.008)	−0.012 (0.009)	−0.006 (0.008)	−0.008 (0.008)
GF Lag t−6	−0.002 (0.008)	−0.006 (0.010)	0.000 (0.007)	−0.006 (0.009)
Obs.	21,418	21,418	21,418	21,418

Notes: Huber-White robust standard errors clustered on state reported in parentheses. Coefficients for the leads and lags of the implied contract and public policy exceptions are reported in the working paper.

Appendix

Table 2

Dynamic Effects of Employment-at-will Doctrines on ASM Annual and Quarterly Employment Changes, 1976–99

Legal exception	State FE, SIC2 FE, YR FE (1)	Col. 1 plus State trends (2)	Col. 1 plus SIC2-YR FE (3)	Col. 1 plus State trends, SIC2-YR FE (4)	Plant FE, YR FE (5)	Col. 5 plus State trends, SIC2-YR FE (6)
(*a*) ASM Absolute annual employment changes						
ΔGF^{t+2}	0.011 (0.011)	0.014 (0.010)	0.008 (0.011)	0.013 (0.010)	0.009 (0.010)	0.010 (0.011)
ΔGF^{t+1}	0.006 (0.011)	0.010 (0.011)	0.005 (0.011)	0.010 (0.010)	0.004 (0.011)	0.007 (0.010)
ΔGF	0.004 (0.006)	0.009 (0.007)	0.001 (0.005)	0.006 (0.006)	0.001 (0.006)	0.003 (0.005)
ΔGF^{t-1}	0.004 (0.006)	0.010 (0.007)	0.002 (0.006)	0.009 (0.007)	0.001 (0.006)	0.004 (0.005)
ΔGF^{t-2}	−0.001 (0.005)	0.005 (0.007)	−0.003 (0.005)	0.004 (0.006)	−0.004 (0.005)	0.000 (0.005)
ΔGF^{t-3}	−0.017 (0.006)	−0.010 (0.007)	−0.016 (0.007)	−0.008 (0.008)	−0.021 (0.006)	−0.013 (0.006)
ΔGF^{t-4}	−0.013 (0.007)	−0.005 (0.007)	−0.012 (0.007)	−0.004 (0.006)	−0.016 (0.007)	−0.009 (0.005)
ΔGF^{t-5}	−0.007 (0.007)	0.003 (0.006)	−0.005 (0.007)	0.004 (0.005)	−0.010 (0.007)	−0.002 (0.006)
GF Lag t–6	0.001 (0.006)	0.015 (0.008)	0.002 (0.006)	0.015 (0.006)	−0.002 (0.006)	0.005 (0.005)
(*b*) ASM Production worker quarterly churn						
ΔGF^{t+2}	−0.003 (0.011)	0.008 (0.007)	−0.005 (0.010)	0.006 (0.007)	−0.001 (0.011)	0.003 (0.007)
ΔGF^{t+1}	0.009 (0.015)	0.024 (0.017)	0.006 (0.014)	0.020 (0.017)	0.011 (0.015)	0.015 (0.014)
ΔGF	−0.026 (0.014)	−0.011 (0.013)	−0.027 (0.013)	−0.013 (0.012)	−0.025 (0.014)	−0.019 (0.008)
ΔGF^{t-1}	−0.017 (0.015)	0.000 (0.011)	−0.018 (0.013)	−0.002 (0.010)	−0.015 (0.015)	−0.009 (0.007)
ΔGF^{t-2}	−0.015 (0.014)	0.005 (0.011)	−0.011 (0.014)	0.006 (0.010)	−0.013 (0.014)	0.000 (0.008)
ΔGF^{t-3}	−0.029 (0.015)	−0.008 (0.011)	−0.025 (0.013)	−0.007 (0.010)	−0.027 (0.016)	−0.014 (0.007)
ΔGF^{t-4}	0.014 (0.017)	0.036 (0.011)	0.020 (0.016)	0.039 (0.013)	0.016 (0.018)	0.032 (0.010)
ΔGF^{t-5}	−0.002 (0.029)	0.021 (0.019)	0.003 (0.027)	0.022 (0.017)	0.000 (0.030)	0.015 (0.021)
GF Lag t–6	−0.011 (0.014)	0.013 (0.013)	−0.009 (0.013)	0.011 (0.013)	−0.008 (0.014)	0.002 (0.007)
Obs.	135,937	135,937	135,937	135,937	135,937	135,937

Notes: Huber-White robust standard errors clustered on state reported in parentheses. Coefficients for the leads and lags of the implied contract and public policy exceptions are reported in the working paper.

Appendix

Table 3

Dynamic Effects of Employment-at-will Doctrines on ASM Total Factor Productivity, 1976–99

	State FE, SIC2 FE, YR FE	Col. 1 plus State trends	Col. 1 plus SIC2-YR FE	Col. 1 plus State trends, SIC2-YR FE	Plant FE, YR FE	Col. 5 plus State trends, SIC2-YR FE
Legal Exception	(1)	(2)	(3)	(4)	(5)	(6)
ΔGF^{t+2}	−0.006 (0.020)	−0.010 (0.016)	−0.005 (0.019)	−0.010 (0.015)	−0.008 (0.019)	−0.008 (0.011)
ΔGF^{t+1}	−0.007 (0.017)	−0.010 (0.022)	−0.003 (0.016)	−0.008 (0.021)	−0.010 (0.019)	−0.008 (0.019)
ΔGF	−0.006 (0.022)	−0.011 (0.026)	−0.008 (0.020)	−0.014 (0.025)	−0.007 (0.022)	−0.009 (0.022)
ΔGF^{t-1}	−0.026 (0.020)	−0.032 (0.021)	−0.023 (0.020)	−0.031 (0.021)	−0.027 (0.022)	−0.027 (0.019)
ΔGF^{t-2}	−0.016 (0.021)	−0.022 (0.020)	−0.012 (0.022)	−0.019 (0.020)	−0.019 (0.023)	−0.015 (0.019)
ΔGF^{t-3}	−0.032 (0.018)	−0.040 (0.019)	−0.031 (0.019)	−0.039 (0.019)	−0.039 (0.020)	−0.037 (0.018)
ΔGF^{t-4}	−0.036 (0.029)	−0.046 (0.027)	−0.035 (0.029)	−0.046 (0.026)	−0.042 (0.030)	−0.039 (0.023)
ΔGF^{t-5}	−0.011 (0.031)	−0.022 (0.025)	−0.013 (0.031)	−0.025 (0.024)	−0.020 (0.033)	−0.019 (0.022)
GF Lag $t-6$	−0.016 (0.024)	−0.041 (0.022)	−0.014 (0.024)	−0.040 (0.021)	−0.014 (0.025)	−0.012 (0.017)
Obs.	110,881	110,881	110,881	110,881	110,881	110,881

Notes: Huber-White robust standard errors clustered on state reported in parentheses. Coefficients for the leads and lags of the implied contract and public policy exceptions are reported in the working paper. TFP is the establishment-level residual from a regression of value-added on four factors of production (production employment, non-production employment, machinery and structures capital) at the industry-year level.

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