Labor Dismissal Costs and Process Innovation*

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Abstract

We show that an arguably exogenous increase in labor dismissal costs leads firms to increase their process innovation but not their product innovation. The effect is stronger in industries that are more reliant on labor that is easily replaceable by physical capital and for which labor costs account for a larger share of production costs. The increase in process innovation is accompanied by increases in capital expenditures per employee and in capital-labor ratios, as well as by a higher productivity of labor. Our evidence suggests that, by creating adjustment costs that increase operating leverage, labor dismissal costs lead firms to increase innovation efforts toward developing technologies that allow producing with higher capital-labor ratio.

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1. Introduction

Frictions in labor markets that make labor inflexible and costly, for example, labor regulations or powerful labor unions, can affect firms' operations, as well as their financing and investment decisions and, in consequence, their value. Such frictions increase firms' cost of capital and hurt firm value (Lee and Mas (2009), Chen et al. (2011), and Favilukis and Lin (2016)) and slow down economic growth (Autor et al. (2006), Besley and Burgess (2004), and Botero et al. (2004)). Responding to these frictions, firms reduce their financial leverage to undo the effect of labor rigidity on firm risk (Simintzi et al. (2015) and Serfling (2016)). Firms can also substitute physical capital for rigid labor or outsource tasks that do not require firm-specific human capital (Autor (2003) and Autor et al. (2007)). The capital for labor substitution suggests that firms need to alter their investment policies and the effect of labor market frictions on firms' investments is not well understood.

In this paper, we focus on innovation and study how firms respond to high labor dismissal costs—a key source of labor rigidity—by developing process innovations that allow changing the mix of inputs—capital and labor—used in production. Labor dismissal costs, by increasing the cost of future layoffs, create labor adjustment costs that reduce the efficiency of firms hiring and firing decisions—firms retain workers with wages above their productivity and refrain from hiring workers wages below their productivity. Further, these distortions reduce the sensitivity of firms' labor costs to changing business conditions, which increases the volatility of firms' cash flow and probability of distress. Since firms typically must fire workers during economic downturns, dismissal costs generate additional operating leverage, that is, they increase firms' cost of capital. Labor dismissal costs thus effectively increase the cost of labor relative to other inputs of production and decrease firms' expected cash flows.

For these reasons, labor dismissal costs create incentives for firms to develop novel production methods that utilize labor-saving technologies and allow to substitute capital for labor. The broad intuition dates back to John Hicks (1932), who noted that *"...a change in the relative prices of the factors of production is itself a spur to invention, and to invention of a particular kind—directed to economizing the use of a factor which has become relatively expensive...*" Jones (2005) formalizes this intuition showing that to take advantage of a substantially higher capital-labor ratio, a firm needs a new technique targeted at that capital-labor ratio, that is, the firms needs to invent a new production process.

Empirical testing of effects of labor dismissal costs on firms' investment choices is challenging as it requires detailed data on the types of projects firms invest in. To this end, we exploit a novel dataset developed by Bena and Simintzi (2017) that distinguishes *process innovations*, which refer to inventions of new production methods (Scherer (1982, 1984), Eswaran and Gallini (1996)), and *product innovations*, which refer to inventions of new goods. Specifically, we measure process (product) innovations as the number of process (product) claims contained in patents filed by a firm in a given year.[2] Bena and Simintzi (2017) show that measures of process innovation based on patent process claims capture labor-saving technologies and are useful in explaining job-displacement. The key prediction we test is that an increase in labor dismissal costs leads firms to increase their process innovation.

To estimate the causal effect of labor dismissal costs on firms' process innovation, we use a difference-in-differences methodology based on the staggered adoption of wrongful discharge laws by U.S. state courts that occurred between the late 1970s and the early 1990s. The legal framework regarding worker dismissals prevailing at the beginning of this period centered around the common law "employment at will" doctrine, which gives employers unlimited discretion to fire employees at any time. The recognition of exceptions to employment at will by courts in 46 states between 1973 and 1995, usually referred to as wrongful discharge laws, significantly limited employers' discretion to fire workers and opened them to potentially costly litigation if they lay off workers (Autor et al. (2006) discuss these laws in detail).[3]

Dertouzos and Karoly (1992), Kugler and Saint-Paul (2004), and Autor et al. (2007) highlight the impact of one of these exemptions, the Covenant of Good Faith and Fair Dealing ("the good-faith exception" hereafter). They note that the good-faith exemption places the greatest restrictions on firms' ability to fire workers because it can be interpreted as a general prohibition against firing workers without just cause. Specifically, it prevents firms from intentionally firing a worker for any "bad cause", such as to deprive her of a promised benefit (e.g., a sales commission or non-vested pension). Hence, we use the recognition of this exception to gauge an increase in the effective price of labor relative other production inputs.[4]

From an identification point of view, using the adoption of the good-faith exception has

[2] In our main tests, the counts are based on independent claims, which do not reference any other claim in the same patent, and thus identify main innovations. Independent process (product) claims account for 25% (75%) of the claims in patents filed by publicly traded firms in our data. Scherer (1990) reports that 25% (75%) of R&D investment goes to process (product) innovation.

[3] Wrongful discharge laws have been extensively used to measure labor market rigidities. For more detail on these laws and their impact on labor market outcomes see Aalberts and Seidman (1993), Walsh and Schwarz (1996), Miles (2000), Kugler and Saint-Paul (2004), Autor (2003), and Autor et al. (2004; 2006; 2007).

[4] As noted by Autor et al. (2007) and others, the other two exemptions (the Public Policy and the Implied Contract exemptions) are less restrictive of employers' firing behavior (see section 2 for a detailed discussion).

two advantages. First, prior work shows that the adoption of this exception leads to a significant increase in labor adjustment costs and operating leverage. Autor et al. (2007) find that it was followed by a significant decrease in employment volatility both in the intensive (within-plant) and the extensive (plant entry/exit) margins. Serfling (2016) shows that it led to a reduction in the volatility of employment, the elasticity of changes in earnings to changes in sales, and the likelihood that firm will fire workers when its earnings decrease, but it increased the volatility of profits. Second, the adoption of the good-faith exception is plausibly exogenous in our study of innovation. Its recognition follows from court decisions made by independent judges in specific cases rather from than changes in state legislation that are potentially contentious. It also concerns firms' firing decisions rather than innovation outcomes. Last, Serfling (2016) documents negative abnormal stock returns for firms in adopting states, suggesting that the adoption of the good-faith exception had an economically significant negative impact of firm value and was not anticipated by firms.

We find that following the recognition of the good-faith exemption innovative firms located in recognizing states increase their patenting of process innovations by about 7.4% to 11.0% relative to innovative firms in other (non-recognizing) states. This increase in process innovation is not immediate, that is, we find no effect in the year of adoption or the year after, and occurs in the medium- to long-run. Specifically, it becomes statistically and economically significant two years after the adoption and grows larger in subsequent years. Importantly, we find that the recognition of the good-faith exemption does not materially affect product innovation. This evidence supports the view that higher labor adjustment costs lead firms to increase innovation in labor-saving technologies. These results hold in specifications that control for time-invariant firm characteristics using firm fixed effects and for time-varying industry characteristics using interacted industry and year fixed effects. They also hold controlling for various time-varying firm characteristics as well as controlling for the state's business environment and political leaning, and are robust to using alternative measures of process and product innovation.

We present evidence supporting the key assumption behind the causal interpretation of our results, namely, that (conditional on the control variables) the innovation of firms in the treated group (recognizing states) and of those in the control group (unaffected states) follow parallel trends before treatment. First, the pre-treatment trends in the innovation of firms in treated and control states are statistically and economically indistinguishable, and the increase in process innovation occurs about two years after treatment. This remains true when we further control for state-level trends in innovation. Second, the results of "placebo" tests show that the recognition of the good-faith exemption in neighboring states does not affect a firm's innovation. This suggests that a correlation of the recognition of the good-faith exemption with local economic shocks is unlikely to drive our results. Furthermore, restricting the set of control states to those states that have a common border with treated states, and thus face more similar economic conditions, does not affect our results.

To shed light on the economic mechanism behind our results, we further explore the cross-sectional variation in the effect of labor dismissal costs on innovation. The impact of labor dismissal costs on labor-saving innovation likely depends on two main factors. The first is the extent to which occupations in an industry require tasks that are easily codified and replaced by technology, which we measure using the routine-task intensity measure of Autor, Levy, and Murnane (2003). The second is the importance of labor costs in the industry's cost structure, which we estimate using Compustat employment and industry wage data from the Quarterly Census of Employment and Wages. We find that the recognition of the good-faith exception leads to a larger increase in process innovation in industries that are more reliant on labor that performs routine-intensive tasks, in industries for which labor costs account for a larger share of production costs, and especially in industries with both of these characteristics. Interestingly, the recognition of the good-faith exemption does not affect product innovation in such industries. This result holds in regression models with firm fixed effects and industry fixed effects interacted with year fixed effects, as well as in models that further include state fixed effects interacted with year fixed effects.

It could be that increased job security of a firm's inventors employed at headquarters might incentivize them to exert more effort in innovation (Acharya et al. (2010)) and such effort might be concentrated in developing new processes.[5] To investigate this alternative explanation of our results, we identify inventors employed outside the firm's state of headquarters. Increased employment protection in a firm's state of headquarters incentivizes the firm to develop labor-saving innovation, but does not affect the job security of inventors it employs in other states. In our first test, we focus on process innovation developed by inventors employed outside the firm's state of headquarters, and find that the adoption of the good-faith exception also leads to an increase in this process innovation. In our second test,

[5] Job security might disproportionally affect the incentives to innovate of workers who, due to the nature of their tasks (e.g., production-line workers), are more likely to develop process rather than product innovation.

we focus on all process innovation, and show that the impact of the adoption of the good-faith exception is unrelated to whether most of the firm's inventors are outside the firm's state of headquarters or they are geographically dispersed across many states. This evidence suggests that increased inventor job security is unlikely to be the main driver our results.

Lerner and Seru (2015) caution that a stronger enforcement of patent rights since the early 1980s led to a surge in patenting activity in the 1980s and 1990s that may have differentially affected innovation across states. Specifically, stronger patent rights could have had a differential impact across geographically-clustered fields of innovation, hence causing differential trends in innovation across states that could confound our inferences. Our evidence suggests that such trends do not drive our results. First, our results hold controlling for differences in the time-trends of innovation across states measured using the total count of patent filings in the state in each year. Second, they hold controlling for the base period (beginning of sample) aggregate patenting in the state interacted with year fixed effects, which addresses the concern that the level of innovation in the pre-patenting surge period conditions the subsequent time-series evolution of innovation in the state. Third, they hold if we restrict our analyses to the period 1975-1990, which precedes the surge in patenting in the 1990s. Last, they hold if we exclude firms in the states of California and Massachusetts, which historically harbor a large fraction of patenting firms.

The introduction of new labor-saving technologies and the associated changes in production methods following an increase in labor dismissal costs are likely to be accompanied by a substitution of physical capital for labor. Such substitution is expected not only because a higher price of labor naturally makes physical capital more cost effective, but also because physical capital can embed or complement the labor savings technologies. Consistent with this view, we find that the recognition of the good-faith exemption leads firms in recognizing states to increase their capital expenditures per employee and capital-labor ratios. We also find a significant increase in the average productivity of labor (measured using sales per employee) for firms in recognizing states. The likely reason for this is that the substitution of labor-saving technology and equipment for low-skill labor leads to a shift in the composition of labor towards more high-skill workers as well as to an increase in the physical capital per worker. Both of these results are consistent with those reported by Autor et al. (2007) based on plant-level data from the Annual Census of Manufactures.[6]

Finally, we examine the association between changes in state minimum wages and process / product innovation. Like increases in employment protection, increases in minimum wages can increase labor costs that are downward rigid and thus firms' operating leverage, ultimately increasing the effective price of labor and encouraging labor-saving innovation. Supporting our arguments, we find that higher minimum wages are associated with more process innovation and that the effect is larger in industries that are more reliant on labor that performs routine-intensive tasks and for which labor costs account for a larger share of production costs. Minimum wages are unrelated to product innovation. These results reinforce our evidence based on the adoption of the good-faith exception. Further, increases in minimum wages arguably do not affect the wages or job security of a firm's inventors. This further suggests that firms' process innovation responds to changes in the effective price of labor, independently of any issues that pertain to inventors' employment situation.

Our paper advances the literature that highlights how frictions in labor markets affect firms' investment and financing decisions as well as firm value. In particular, our work is closely related to prior work that shows employment protection can generate operating leverage and through this mechanism impact corporate decisions.[7] Our key contribution is to provide direct empirical evidence that employment protection, which ultimately increases the effective price of labor primarily though the operating leverage channel as documented by Autor et al. (2007) and Serfling (2016), leads firms to tilt their innovation efforts towards developing labor-saving process technologies. In doing so, we also add to the broader economics literature that seeks to understand what drives the direction of technological change as well as the impact of public policy on innovation activities.[8]

A few prior studies examine how frictions in labor markets affect overall innovation. Acharya et al. (2010) show that the adoption of the good-faith exception leads firms to

[6] They are also consistent with those in Acemoglu and Finkelstein (2005), who show that a rise in the price of labor in the U.S. hospital industry leads to higher capital-labor ratios and to an increase in the skill composition of hospitals' workforces.

[7] We refer to the key papers in our opening paragraph. Other related work includes Matsa (2010), Agrawal and Matsa (2013), Chen et al. (2011b), Klasa et al. (2009), and Atanassov and Kim (2009), among others.

[8] How labor scarcity and high wages affect the direction of technological change and whether they encourage technological progress are central questions for economic growth with unclear answers (Acemoglu (2007, 2010)). Habakkuk (1962) notes the scarcity of labor in the 19th century obliged firms to install new labor-saving machinery and led to the continuous progress of the American industry. However, many macroeconomic models suggest that labor scarcity and high wages slow down technological progress when new technologies are embodied in capital goods. Eswaran and Gallini (1996) and Gallini (2002) discuss the impact of patent policy on process vs. product innovation and overall innovation. Jaffe and Trajtenberg (2002) discuss the empirical research on patents.

increase their patented innovation. We show that this increase in innovation is largely driven by an increase in labor-saving process innovation, which occurs as firms substitute labor-saving technology and physical capital for unskilled labor that performs routine-intensive tasks. Bradley et al. (2016) show that an increase in labor unionization leads to a reduction in innovation, because unions can hold up the firm and demand higher wages once the firm has incurred the sunk cost of innovation, reducing the firm's ex-ante incentives to innovate. This suggests that different types of frictions in labor markets (e.g., originated in labor unions or in the legal protection of employment) can have a different impact on a firm's innovation. Last, Bena and Simintzi (2017) show that large U.S. multinational firms with operations in China reduced their process innovation following an increase in their access to "cheap" offshore labor as a result of the 1999 U.S. – China agreement. We instead focus on changes in the effective price of domestic labor that affect all U.S. firms, and show that employment protection stimulates process innovation as firms reduce their reliance on domestic labor.

More generally, several other papers in the corporate finance literature study how firm characteristics, such as governance (Atanassov, 2013; Balsmeier, Fleming, and Manso, 2016), ownership structure (Aghion, Van Reenen, and Zingales, 2009; Bena, Ferreira, Matos, and Pires, 2016), organization (Lerner, Sørensen, and Strömberg, 2011; Ferrreira, Manso, and Silva, 2014; Seru, 2014), managerial compensation (Manso, 2011; Ederer and Manso, 2013), capital structure (Atanassov, Nanda, and Seru, 2007), and litigation risk (Cohen, Gurun, and Kominers, 2016) impact corporate innovation. Our paper is unique in this literature in that we focus on how labor frictions differentially affect process and product innovation.

The rest of the paper is organized as follows. Section 2 discusses our measures of process and product innovation as well as our identification strategy based on wrongful discharge laws. Section 3 discusses our main empirical results. Section 4 concludes.

2. Innovation measures, identification strategy, and data

2.1 Patented process and product innovation

Our main dependent variables are designed to separately measure a firm's patented process and product innovation output. By definition, a process innovation describes a new way to produce an existing good, while a product innovation describes a new good that did not exist before. Prior literature argues that process innovation is aimed at improving a firm's own production methods in order to lower its production cost, while product innovation is an improvement sold to others, either to consumers or to other firms (Scherer (1982), (1984);

Link (1982); Cohen and Klepper (1996); Eswaran and Gallini (1996)). As we discuss in more detail below, innovation in labor-saving technology – our main focus – is thus captured by process innovation and thus process innovation is our main outcome of interest throughout the paper.

As in Bena and Simintzi (2016), we first obtain full texts of the universe of utility patents awarded by the United States Patent and Trademark Office (USPTO) to U.S. and international companies, individuals, and other institutions from January 1976 to December 2012. We then parse the structured-texts of patent grants to identify the section that contains patent claims. Claims define – in technical terms – the scope of protection conferred by a patent and thus define what subject matter the patent protects. Since claims are critical defining elements of a patent, they are the primary subject of examination in patent prosecution and crucial in patent litigation cases. We identify and focus on independent claims, i.e., those that do not reference any other claim in the patent. These claims capture the most important and novel innovations patented by firms. They exclude minor innovations contained in dependent claims, which directly refer to an independent claim in the patent. Last, we use text analysis techniques to examine the patent claims and, aided by the very legalistic and stilted language used in these documents, unambiguously classify the independent claims within each patent as either process or product. See appendix A of Bena and Simintzi (2016) for an extensive discussion of the construction of these measures.

Through our analyses, we define *Process Innovation* (*Product Innovation*) as the natural logarithm of one plus the number of independent process (product) claims included in patents applied for by a firm in a given year. We set both claim counts to zero for firm-years with no patent filings with the USPTO. To assign patents to firms in Compustat, for each patent, we identify patent assignees listed on the patent grant document, the country of these assignees, and the indicator of whether each assignee is a U.S. corporation, a non-U.S. corporation, an individual, or a government body. Using this information, we then match patents to firms in Compustat. Our matching algorithm involves two main steps. First, we standardize patent assignee names and firm names, focusing on unifying suffices and dampening the non-informative parts of firm names. Second, we apply multiple fuzzy string matching techniques to identify the firm, if any, to which each patent belongs.

Importantly, process innovation is a good proxy for labor-saving innovation. Labor costs entail a significant part of firms' production costs and, as such, improvements in firms' production processes imply an increase in labor productivity. Further, Bena and Simintzi

(2016) empirically show that our measure of process innovation captures labor-saving innovation (see their Internet Appendix for more detail). First, industries with more process innovation subsequently decrease their reliance on labor that performs routine-intensive tasks and is thus easily substitutable by technology. Second, industries involving easily offshorable tasks, which entail flexible and cheap labor, are associated with less process innovation. Third, industries with more process innovation subsequently become more capital intensive, indicating a substitution of labor for physical capital. Fourth, they examine the abstracts and background description sections of patent documents, and document a clear positive correlation between a firm's patenting of process innovation as captured by our measure and specific mentions to labor costs reductions in these sections of the text.

2.2 Wrongful discharge laws as a source of exogenous variation in labor dismissal costs

Our empirical analyses aim to estimate the *causal* effect of labor dismissal costs on firms' process and product innovation. To this end, we use a difference-in-differences methodology based on plausibly exogenous variation in these costs associated with the staggered adoption of "wrongful discharge laws" by U.S. state courts during the late 1970s and the early 1990s. Several published papers in the legal, economics, and finance literatures have carefully discussed these laws, including their legal background, their merit and economic significance as a source of exogenous variation in labor dismissal costs, their effect on labor market outcomes, and their effect on selected corporate decisions. The key papers in the legal and labor economics literatures include Dertouzos and Karoly (1992), Miles (2000), Kugler and Saint-Paul (2004), and Autor et al. (2006; 2007), among others. Acharya et al. (2014) and Serfling (2016) summarize this literature and conduct additional empirical tests validating this setting for identification purposes. Given this, in the interest of space, we provide a brief discussion of these laws below and refer the reader to the existing literature for more detail.

The legal framework regarding worker dismissals prevailing in the United States at the beginning of the 1970s was centered around the common law doctrine of "employment at will". This doctrine sustained a legal presumption that employers can fire workers *at will*, i.e., "for good cause, bad cause, or no cause" (a quote from Payne v. Western & Atlantic Railroad, Supreme Court of Tennessee, 1884), which essentially gave employers unlimited discretion in dismissing workers. Between 1973 and 1995, courts in 46 states set new common-law legal precedents that recognized three different kinds of exceptions to employment at will usually referred to as "wrongful discharge laws". These exceptions significantly limited employers' discretion to fire workers, opened them to potentially costly litigation, and generated uncertainty about when employers could terminate workers with impunity. The survey of public firms' managers conducted by Dertouzos, Holland, and Ebener (1988) indicated that 46% of managers feared potential losses arising from such lawsuits, while Jung (1997) and Boxold (2008) document economically significant awards to plaintiffs.

The first exception to employment at will is the *"public policy exception"*, which provides employees with protection against discharges that would thwart an important public policy, such as performing jury duty, filing a worker's compensation claim, reporting an employer's wrongdoing, or refusing to commit perjury. As noted by Autor et al. (2006), despite its widespread recognition, successful cases involving significant compensation amounts are rare, because courts typically limit public policy cases to clear violations of express legislative commands rather than violations of a vaguer sense of public obligation. Hence, this exception is likely of minor legal and economic significance (see Edelman, Abraham, & Erlanger, 1992).

The second is the *implied-contract exception*, which highlights a firm's implicit promise not to terminate a workers' employment without good cause. Such implicit promise usually follows from the language used in employment contracts and internal personnel manuals, or from expectations of continuing employment based on length of service and prior promotions. Although this exemption is potentially general enough to have significant effects, in practice employers can protect themselves from these implied-contract claims by rewording personnel manuals and adding explicit language to employment contracts to state clearly that all employment contracts are at will (Miles (2000) and Autor, Kerr, and Kugler (2007)). Survey evidence indicates that large employers indeed took such steps (see The Bureau of National Affairs (1985) and Sutton and Dobbin (1996)).

The third is the *"good-faith exception"*, which represents an implied covenant to terminate employment only in good faith and fair dealing, which essential prevents an employer from firing workers for any "bad cause". Read broadly, the good-faith exception arguably represents the largest deviation from at-will employment and is the most far reaching. This exception gives employees both a contract and a tort cause of action in the event they are dismissed, allowing them to seek compensation for both contractual losses and emotional distress as well as punitive damages that imply highly uncertain amounts. Importantly, it serves as a general prohibition against terminating any worker without just cause (economic necessity or poor performance) and thus could have sweeping consequences (Dertouzos and Karoly (1992), Kugler and Saint-Paul (2004), and Autor et al. (2007)).

Prior work suggests that the adoption of wrongful discharge protection significantly increased hiring and firing costs and ultimately labor adjustment costs. Supporting this view, Miles (2000) and Autor (2003) show that employers in adopting states substituted temporary help agency workers for direct-hire employees. Further, Kugler and Saint-Paul (2004) show that this legislation (especially the good-faith exception) reduced the re-employment probability of unemployed relative to employed workers, suggesting that dismissal protection exacerbated adverse selection into non-employment. Recent work highlights the importance of the good-faith exception in particular as a source of increased labor adjustment costs and operating leverage. Autor et al. (2007) show that the adoption of the good-faith exception reduces employment volatility using plant-level data from the Census Bureau, both in the intensive (within-plant) and the extensive (plant entry/exit) margins. Further, in a sample of Compustat public firms, Serfling (2016) shows that it led to a reduction in the volatility of employment, the elasticity of changes in earnings to changes in sales, and the likelihood that firm will fire workers when its earnings decrease, but it increased the volatility of profits.

Our coding of the three wrongful discharge laws indicators follows that in Autor et al. (2007), which is given in their appendix table A1. As they note, the adoption of these exceptions occurred rapidly and mostly between 1976 and 1988, with fewer adoptions afterwards. This raises the concern that a widespread change in legal views on the employment at will doctrine around those years allowed firms to anticipate these adoptions and to adjust their behaviour accordingly. However, the are several reasons why this is unlikely to be the case. Specifically, the precedent-setting cases recognizing these exceptions to employment at will typically provide a discrete element of surprise. The adoption of an exception is an idiosyncratic function of the cases brought before state high courts and the disposition of the sitting judges. Further, many states never adopt exceptions and others reverse or amend these exceptions after adoption. Noteworthy, the good faith exception was adopted more slowly and much less extensively than the other two exceptions. Hence, the adoption of this particular exception was especially difficult to anticipate.

More generally, concerns with this type of identification relate to the possibility that firms might lobby to influence a state's decisions on employment protection or that such decisions might be driven by changes in economic or political conditions in the state that could be correlated with the outcome of interest (innovation in our case). Below we discuss several reasons why these are unlikely to be major concerns in our setting (see also Acharya et al. (2014) or Serfling (2016) for similar discussions). First, the adoption of exceptions to employment at will arises from judiciary decisions by state courts in the context of common law and not through a potentially contentious legislative process in the state. The judges serving in state courts are deemed independent of both the state and federal government, and thus are largely immune to political pressure from interested parties such as labor organizations or companies. Hence, a judge's decision to adopt these exceptions is more likely due to the merits of the specific case than to political pressure or economic factors (Walsh and Schwarz (1996)). Second, Walsh and Schwarz (1996) use published court decisions to examine why judges adopt these exceptions. They document that the three main reasons cited by judges in their rulings are enhancing fairness in employment relationships, assuring consistency with established principles of contract law, and following similar rulings in other states. This suggests that the adoption of these exceptions is unlikely driven by changing business or political conditions in the state that could affect firms' incentives to innovate.

Last, using data analogous to ours, Acharya et al. (2014) and Serfling (2016) carefully investigate what might drive a state court's decision to adopt the good-faith exception. Both studies provide direct empirical evidence that the adoption of the good-faith exception is not driven by changing political conditions, economic conditions, or in other labor regulation in the state. Further, the adoption of the good-faith exception is associated with negative abnormal stock returns around courts' final rulings, suggesting that such rulings were not anticipated. We refer the reader to the above papers for more detail on these general results.[9]

2.3 Data and variables

The sample spans the period 1975-1997 and is based on the publicly traded firms in the Compusat dataset. It includes all non-financial and non-utility firms headquartered in the U.S. that filed at least one patent with the United States Patent and Trademark Office (USPTO) during this period as in Bloom, Schankerman and Van Reenen (2013), for a total of 44,614 firm-year observations. Below we briefly define our key variables. The corresponding summary statistics are in Table 1.

The key dependent variables are *Log(1 + # Process Claims)* and *Log(1 + # Product Claims)*, respectively. Both variables are counts of independent claims, which are patent claims that do not reference any other claim in the same patent. The *Good Faith*, *Implied Contract*, and *Public Policy* indicator variables equal one if the firm's state of headquarters

[9] See section 3.3.4 of Acharya et al. (2014) and section II.D of Serfling (2016).

has adopted the corresponding exemption by year t and zero otherwise. The coding for these legal events follows Autor et al. (2006).

The control variables described next are lagged one year. *Log(1 + # Patents)* is the logarithm of one plus the number of patents filed by the firm in a given year, *Log(1 + R&D Stock)* is the logarithm of one plus a firm's R&D stock computed by adding its R&D spending (*xrd*, in $ million) since 1950 and assuming an annual depreciation rate of 15%, *Log(Sales)* is the logarithm of a firm's sales (*sale*, in $ million), *Log(Market-to-Book)* is the logarithm of a firm's market value of assets (the sum of the market value of equity, *csho*prcc_f*, the book value of long term debt, *dltt*, and the book value of debt in current liabilities, *dlc*) scaled by the book value of assets (*at*), *GDP Growth* is the one-year growth rate of state GDP in current dollars (from the Bureau of Economic Analysis), and *Political Balance* is the fraction of a state's congress members in the U.S. House of Representatives that belong to the Democratic Party (from History, Art & Archives, U.S. House of Representatives).

3. Results

3.1 Econometric specification

To identify the effect of wrongful-discharge laws on innovation outcomes, we estimate the following difference-in-differences specification:

$$y_{i,s,j,t} = \beta_1 Good\ Faith_{s,t} + \beta_2 Implied\ Contract_{s,t} + \beta_3 Public\ Policy_{s,t} \\ + \delta X_{i,s,j,t-1} + \alpha_i + \mu_{j,t} + \varepsilon_{i,s,j,t}$$

were i denotes firm, s denotes the firm's state of headquarters, j denotes the firm's two-digit SIC industry, and t denotes year.[10] The dependent variable y is either *Log(1 + # Process Claims)* or *Log(1 + # Product Claims)* and the three indicator variables for whether the state recognizes the corresponding exception to the employment at will doctrine are *Good Faith*, *Implied Contract*, and *Public Policy*, respectively. The vector X includes lagged firm-level control variables (*Log(1 + # Patents)*, *Log(1 + R&D Stock), Log(Sales)*, and *Log(Market-to-Book)*) and state-level control variables (*GDP Growth* and *Political Balance*) that are defined in section 2.3. Noteworthy, as in Bloom, Schankerman and Van Reenen (2013), we control for differences in innovation potential using a firm's lagged patent filings and lagged R&D stock,

[10] Lawsuits related to employment contracts are filed in the context of employment law, and thus the relevant jurisdiction for a wrongful discharge lawsuit is the state where the employee works. Firms often employ workers in different states, but data restrictions only allow us to identify a firm's state of headquarters, where most of a firm's workers are typically employed.

as well as for other relevant firm characteristics. Further, the inclusion of our state-level control variables alleviates concerns that changes in a state's economic conditions or political environment correlated with both the adoption of wrongful-discharge law legislation and innovation could confound our inferences. Last, α_i is a firm fixed effect, which controls for firm characteristics that do not vary over our sample period, and $\mu_{j,t}$ is a two-digit SIC industry times year fixed effect, which absorbs time-varying shocks affecting specific industries. In all specifications, we report standard errors clustered at the state level.

Our empirical approach is aided by the staggered nature of the adoption of wrongful discharge laws, which allows us for a time-varying control group that provides a counterfactual for how firms' innovations would have evolved in the treated states had they not adopted such legislation. The key coefficient of interest is β_1, which gauges the estimate of the causal effect of the adoption of the good-faith exception on innovation. Specifically, this coefficient captures the change in innovation for firms in adopting (treated) states relative to the contemporaneous change in innovation for firms in non-adopting (control) states. Of course, the causal interpretation hinges on the parallel trends assumption, i.e., that the pre-treatment trends in innovation are the same for both treated and control groups. We provide timing tests in support of this assumption in section 3.2.

3.2 Impact of labor dismissal costs on process vs. product innovation

Table 2 reports the results of pooled (panel) OLS regressions that implement our main difference-in-differences approach discussed in section 3.1. We report specifications with and without control variables. The inclusion of control variables helps control for differences between firms in treatment and control states that could confound our inferences given a lack of truly random assignment to these groups. However, it is conceivable that some of these variables (in particular the firm-level controls) could be affected by the adoption of wrongful discharge laws (i.e., be endogenous) and bias our estimates. Hence, both specifications are useful in assessing the causal effect of wrongful discharge protection on innovation.

Columns (1)-(3) report the effect of the laws on process innovations. Column 1 shows that the adoption of the good-faith exception leads to an 11% increase in the process innovation of firms in adopting states relative to that of firms in non-adopting states that is both statistically and economically significant. The estimated magnitude decreases to 8.3% when we control for the lagged number of patents filed by a firm in column 2, and to 7.4%

when we include the remaining firm-level and state-level controls. Nevertheless, the estimated coefficient remains statistically and economically significant. Columns 4-6 report the effect of the laws on product innovation. Across all specifications we consider we find no impact of wrongful discharge laws on product innovation. Further, consistent with the discussion in Section 2.2 and prior evidence that the good-faith exception places the greatest rigidities on labor markets, we find no effect of the implied-contract or public-policy exceptions on firms' process or product innovation. In sum, the results suggest that labor market rigidities that increase the effective price of labor lead firms to tilt their innovation efforts towards developing new production processes that can help them reduce labor costs.

In Table 3 we examine the dynamic response of innovation to the adoption of the good-faith exception. To this end, we estimate specifications analogous to those in Table 2 but replace *Good Faith* by *Good Faith -2*, *Good Faith -1*, *Good Faith 0, Good Faith +1*, *Good Faith +2*, and *Good Faith 3+*. These are indicator variables equal to one if the firm's state of headquarters will adopt the exemption in two years, will adopt it the following year, adopted it in the current year, adopted it the year before, adopted it two years before, or adopted it three or more years before, respectively. In addition to using our main specification in columns (1) and (4), we consider two alternative specifications designed to address the concern that our results could be driven by differential trends in innovation across states. In columns (2) and (5) we further control for the lagged logarithm of the mean number of patents filed by firms in the state (the patents of the firm itself are excluded in the calculation of this mean). In columns (3) and (6) we instead include interactions of *Log(1 + # State Patents 74-76)*, defined as the logarithm of the total number of patents filed in the state over the period 1974-1976 (preceding the first adoption of the good-faith exemption in our sample) and year indicator variables. All specifications give qualitatively similar results.

Consistent with the parallel-trends assumption behind a causal interpretation of our results, we find no statistically or economically significant difference in the process or product innovation of firms in treated and control states in the years prior to the adoption of the good-faith exception (the coefficients of *Good Faith -2* and *Good Faith -1* are statistically insignificant in all regressions reported in the table). Columns (1)-(3) show that the increase in process innovation is not immediate, as we find no significant effect in the year of adoption or one year later. The effect becomes statistically and economically significant two years following the adoption, and further increases in both magnitude and significance in subsequent years. This is consistent with a reasonable lag between the shift in the focus of firms' innovation efforts and the resulting patenting of new processes. In contrast, columns 4-6 show that the adoption of the good-faith exception has no effect on product innovation.

3.3 Underlying Economic Mechanism

We explore the cross-sectional variation in the effect of labor dismissal costs on innovation. Specifically, the impact of an increase in labor dismissal costs on labor-saving innovation depends on two main factors that affect a firm's incentives to invest in developing such innovation. The first is the extent to which occupations employed in its industry require workers to perform tasks that are easy to codify and replace by technology. Since labor-saving technologies, such as process innovation, substitute those routine-intensive tasks (Autor, Levy, and Murnane (2003); Autor and Dorn (2013)), the adoption of the good-faith exception should have a larger impact on process innovation in industries that are more reliant on labor that performs routine-intensive tasks. The second is the importance of labor costs in the industry's cost structure, i.e., the extent to which labor costs drive total costs and ultimately profitability. Firms' incentives to innovate in labor-saving production processes are greater in industries for which labor costs account for a larger fraction of production costs. In consequence, the adoption of the good-faith exception should also have a larger impact on process innovation in such industries. Importantly, the effect of increased firing costs should be strongest in industries that simultaneously exhibit *both* characteristics, i.e., are both more reliant on labor that performs routine-intensive tasks and incur labor costs that account for a larger share of total production costs.

We measure the routine-task intensity of the firm's 2-digit SIC industry (*RTI*) using total employment by occupation (based on Standard Occupational Classification codes) in each industry obtained from the Occupational Employment Statistics provided by the Bureau of Labor Statistics and the classification of occupations according to their routine task intensity of Autor, Levy, and Murnane (2003). Specifically, the *RTI* of an industry is computed as the weighted-average routine task intensity of occupations employed in the industry, weighted by the share of each occupation in the industry's total employment. For each industry, *RTI* is based on the first year the data is available (1988 for 21 industries, 1989 for 21 industries, 1990 for 27 industries, and 1992 for one additional industry).

Our measure of the labor cost share of the firm's 2-digt SIC industry (*LaborCostShr*) is computed as the median labor cost share across all firms and years in our data. Each firm's labor cost share is computed as the share of firm's wage bill on the firm's cost of goods sold (*cogs*). In doing this we face the difficulty that the variable which captures "labor and related expenses" in Compustat (*xlr*) is missing for most firms in our sample. Hence, we estimate each firm's wage bill as the number of employees reported in Compustat (*emp*) times the average wage in the firm's industry obtained from the Quarterly Census of Employment and Wages produced by the Bureau of Labor Statistics.[11]

In Table 4, we allow the effect of *Good Faith* to vary with the industry characteristics discussed above. Specifically, we augment our main regression model to include the following terms: *Good Faith* × *RTI*, *Good Faith* × *LaborCostShr*, and *Good Faith* × *RTI* × *LaborCostShr*. To facilitate the interpretation of the estimated coefficients for these interactions, both *RTI* and *LaborCostShr* are standardized to have mean zero and standard deviation of one before forming the interactions. In all regression models, we include the same control variables as in Table 2 as well as firm fixed effect and two-digit SIC industry fixed effects interacted with year fixed effects. Columns (2) and (4) further include state fixed effects interacted with year fixed effects. In these regressions, we cannot estimate the coefficients of the state-level variables.

The key results are reported in column (1) and are consistent with the intuition above. They show that the adoption of the good-faith exemption leads to a larger increase in process innovation (i) in industries that are more reliant on labor that performs routine-intensive tasks (the coefficient of *Good Faith* × *RTI* is positive and significant), (ii) in industries for which employees' wages account for a larger share of production costs (the coefficient of *Good Faith* × *LaborCostShr* is positive and significant), and (iii) especially in industries that exhibit *both* higher reliance on routine-intensive labor and a larger share of labor costs in total costs (the coefficient of *Good Faith* × *RTI* × *LaborCostShr*) is positive and significant). In column (2) we add state fixed effects interacted with year fixed effects and thus we fully control for *any* unobservable time-varying state-level factors that could confound our results. The coefficient of *Good Faith* × *RTI* slightly decreases the magnitude and becomes insignificant, but the coefficients of *Good Faith* × *LaborCostShr* and *Good Faith* × *RTI* × *LaborCostShr* which remain significant and similar in magnitude.

To assess the economic significance of the effects in column (1), note that the coefficient of *Good Faith* captures the average impact of the adoption of the good-faith exception on

[11] We use the average wage in the firm's 4-digit SIC industry if available, if not we use the average wage in the firm's 3-digit SIC industry, and if not we use the average wage in the firm's 2-digit SIC industry.

process innovation (a 6.8% increase) for a firm in an industry with mean values of *RTI* and *LaborCostShr*. The coefficient of *Good Faith* × *RTI* implies that the increase in process innovation is an additional 4.2 percentage points larger in industries whose *RTI* is one-standard deviation higher than the sample mean (while keeping *LaborCostShr* at its sample mean). The coefficient of *Good Faith* × *LaborCostShr* implies that the increase in process innovation is an additional 6.1 percentage points larger in industries whose *LaborCostShr* is one-standard deviation higher than the sample mean (while keeping *RTI* at its sample mean). Last, the coefficient of *Good Faith* × *RTI* × *LaborCostShr* implies an additional 4.1 percentage points increase in process innovation for industries in which both *RTI* and *LaborCostShr* are one-standard deviation higher than their sample means (compared to an industry for which either *RTI* or *LaborCostShr* is one-standard deviation above its sample mean, but not both). Putting together, our estimate suggest that the adoption of the good-faith exception could lead to a 21.2% increase in process innovation in industries that are both highly reliant on routine-intensive labor and have labor costs that account for large share of their total costs (6.8% + 4.2% + 6.1% + 4.1%).

The results in column (3) show that the recognition of the good-faith exemption does not affect product innovation on average, in industries with high reliance on routine-intensive labor, in industries with a large share of labor costs in total costs, or in industries with both characteristics. When we add the state fixed effects interacted with year fixed effects in column (4), the coefficient of *Good Faith* × *LaborCostShr* becomes positive and significant, but the coefficients of *Good Faith* × *RTI* and *Good Faith* × *RTI* × *LaborCostShr* remain insignificant. I particular, the magnitude of the coefficient on *Good Faith* × *RTI* × *LaborCostShr*, which captures the sharpest contrast in industry characteristics favoring the development of process innovation but not product innovation, remains close to zero.

In Table 5, we investigate an alternative explanation of why process innovation increases following the adoption of the good-faith exception in a firm's state of headquarters. Our main argument is that increased employment protection increases the effective price of labor and thus leads firms to instruct its inventors to develop labor-saving technologies. However, the adoption of this exception in the firm's state of headquarters increases job security for all of the firm's workers employed in that state, including not only a firm's production workers but also the inventors in charge of a firm's innovation activities. Hence, it could be that increased job security of a firm's inventors employed at headquarters might incentivize them to exert more effort in innovation (Acharya et al. (2010)) and such effort might be concentrated in developing new processes.[12] We thus examine whether this could drive our results.

To this end, we analyze the text of each patent filing and identify the state where the first inventor (the leading inventor) listed in the patent is employed. We then flag patents for which the main inventor listed in the patent is employed in a state other than the firm's state of headquarters. Increased employment protection in a firm's state of headquarters, where most of its production workers are typically located, incentivizes the firm to develop labor-saving innovation. It also increases job security and thus might increase incentives to innovate for inventors also employed in the state of headquarters. However, it cannot affect the job security or incentives to innovate of those inventors the firm employs in other states. Hence, our tests exploit the information contained in such cases.

In our first test, reported in columns (1)-(2), we examine how the adoption of the good-faith exception in the firm's state of headquarters affects the firm's process innovation developed by the inventors it employs outside its state of headquarters (*Log(1 + # Process Claims with Inventors Located in Other States)*). Patents that list a first inventor employed in the firm's state of headquarters are ignored in computing this variable. The regressions are otherwise analogous to those in column (4) of Table 2. Regardless of whether we include the control variables or not, we find that the adoption of the good-faith exception also leads to an increase in this process innovation is both statistically and economically significant.

In our second test, we focus on all process innovation, and examine whether the impact of employment protection on process innovation depends on the location of the firm's inventors. We use specifications analogous to those in column (4) of Table 2 augmented to include interactions of *Good Faith* with two indicator variables. The first equals one if the first inventors listed in all patents filed by the firm in a given year are located in a state other than the state of the firm's headquarters, and zero otherwise (*Inventors in Other States*). The second equals one if no state concentrates more than 50% of the first investors listed in all patents the firm applied for in a given year, and zero otherwise (*Disperse Inventors*).

In columns (3) and (4), we report the results when we include *Good Faith* × *Inventors in Other States* and *Good Faith* × *Disperse Inventors*, respectively. Our hypothesis implies that the adoption of the good-faith exception should lead to an increase in process innovation

[12] Job security might disproportionally affect the incentives to innovate of workers who, due to the nature of their tasks (e.g., production-line workers), are more likely to develop process rather than product innovation.

regardless of where inventors are located or their dispersion across states, so it implies that both coefficients should be zero. In contrast, if job security of inventors themselves was a main driver of our main results, then the coefficient of both of these interactions should be negative. We find a statistically insignificant coefficient on both of these interactions. In sum, the evidence in Table 5 suggests that increased inventor job security is unlikely to be the main driver our main results.

3.4 Additional investigation

In Table 6, we report the results of additional tests that examine whether our main results could be driven by unobserved changes in local economic conditions that could drive both the passage of wrongful discharge protection and process innovation. First, in column (1) alongside with *Good Faith* we further include the indicator variable *Good Faith Neighbor*, which is equal to one if the firm's state has not yet adopted the good-faith exception but at least one neighboring state has. The idea behind this placebo test is that, if local economic conditions that are common for bordering states drive the results, then we should observe an increase in the process of innovation for firms located in states that do not adopt the legal change but share a common border with adopting states. However, we find a negative and statistically insignificant coefficient on *Good Faith Neighbor*.

Second, in column (2), we re-estimate our baseline specification after excluding from the sample firms in states that did not adopt the good-faith exception during our sample period and do not share borders with any of the states that did adopt it. Such states are more likely to differ from adopting states on various dimensions, in particular the underlying economic conditions in the state, and thus could add noise to the control group. Nevertheless, we continue to find a statistically and economically significant impact of the adoption of the good-faith exception on firms' process innovation.

In Table 7, we address the concern that differential time trends in patent growth across states could bias our inferences. Lerner and Seru (2015) caution that a stronger enforcement of patent rights since the early 1980s led to a surge in patenting activity in the 1980s and 1990s that may have differentially affected innovation across states. Specifically, stronger patent rights could have had a differential impact across geographically-clustered fields of innovation, hence causing differential trends in innovation across states that could confound our inferences. To alleviate this concern, we estimate alternative specifications of our baseline regression reported in Table 2 in which we control for these patterns in the data.

First, in column (1) we add the (lagged) logarithm of the mean number of patents filed by firms in the state as an additional control variable (*State-Year Trend*). The estimated effect of *Good Faith* on process innovation is almost identical to that in the baseline results. Second, in column (2) we interact the (log-transformed) number of patents filed in the state during the pre-treatment period 1974-1976 (*Log(1 + # State Patents 74-76)*) with the full set of year fixed effects. The coefficient of *Good Faith* slightly increases in magnitude and remains statistically significant. Third, in column (3) we repeat the analysis over the 1975-1990 period, i.e., the period before the explosive growth in patents observed post 1990 which occurred with different intensities across states. Despite the lower statistical power associated with a smaller sample and set of events, the coefficient of *Good Faith* remains statistically and economically significant. Last, in column (4) we exclude from the sample two states, California and Massachusetts, which showed the highest patent growth over our sample period and account for a large share of patenting activity in the U.S. Again, the estimated coefficient of *Good Faith* remains positive and significant. Overall, the results in this table suggest that our results are robust to the concerns outline above.

3.5 Effect of wrongful discharge protection on physical investment and labor productivity

The introduction of new labor-saving technologies and the associated changes in production methods following an increase in labor dismissal costs are likely to be accompanied by a substitution of physical capital for labor. Such substitution is expected not only because a higher price of labor naturally makes physical capital more cost effective, but also because physical capital can embed or complement the labor savings technologies.

To this end, in Table 8 we use our difference-in-differences approach to examine whether wrongful discharge protection directly affects a firm's investment in physical capital and capital-labor ratios in addition to process innovation. In columns (1)-(3) the dependent variable is the logarithm of a firm's capital expenditures per employee and in columns (4)-(6) it is the logarithm of the capital-labor ratio. In addition to the three wrongful discharge indicators, the regressions sequentially add firm-level control variables that are relevant for investment analyses and the two state-level control variables. Consistent with capital deepening, columns (1)-(3) show that the adoption of the good-faith exemption leads firms in adopting states to increase their capital expenditures per employee by 5.2% relative to those in states that did not adopt such exception. Such changes in investment should also lead firms to increase their capital intensity. Indeed, columns (4)-(6) show that following the adoption of the good-faith exemption firms increase their capital-labor ratios by 7.2% relative to control firms.

The passage of wrongful discharge legislation can also increase employee productivity. First, it can shift the composition of a firm's labor towards using more skilled labor. The reason is that employment protection stimulates the adoption of labor-saving technologies that tend to substitute workers who perform routine tasks in the middle of the skill distribution (Autor, Levy, and Murnane (2003), Autor and Dorn (2013)) but are complementary to high skill labor (Autor and Katz (1999)). Second, because firing workers is costly, it can lead firms to more carefully select new hires and ultimately increase the quality of its workforce. Third, to the extent that capital is complementary to the firm's skilled labor, the increase in the physical capital per worker we document can also boost labor productivity. A caveat, however, is that the passage of such legislation might decrease labor productivity by making the firm retain some unproductive workers that it would dismiss if firing were costless.

With the above considerations in mind, in columns (7)-(9) we examine the impact of wrongful discharge legislation on labor productivity, measured as the logarithm of the ratio of sales per employee. The control variables are analogous to those in columns (1)-(6). We find that following the adoption of the good-faith exception firms in adopting states experience a 4% increase in the average productivity of their labor relative to firms in non-adopting states. The coefficient is both economically and statistically significant.

Overall, the results in Table 9 are consistent with those in Autor et al. (2007) based on plant-level data from the Annual Census of Manufactures, and highlight that an increase in labor adjustment costs leads firms to adopt new production methods that ultimately increase labor productivity. Importantly, they help describe the broader response of the publicly traded firms in our sample to higher dismissal costs, namely, by simultaneously developing labor-saving technology and substituting capital for labor (which are closely intertwined).

3.6 Impact of increases in state minimum wages on process and product innovation

The idea behind our tests is that firms benefit from flexibility to adjust their total wage bills when they face negative shocks to their business, because such flexibility directly affects their operating risk and thus the effective price of labor. This, in turn, might induce firms to distort their decisions on other margins, such as the allocation of R&D budgets. Importantly, the flexibility of a firm's wage bill is related not only to its ability to adjust its employment level, but also to its ability to adjust the nominal wage rate it pays to its employees. Our prior tests based on the adoption of wrongful discharge laws focus on the first aspect, i.e., on how an increase in operating leverage due to an increase in the cost of firing employees (that leaves the nominal wage rates unaffected) affects innovation. In this section, we examine the second aspect, i.e., how an increase in operating leverage due to an increase in the minimum nominal wage rate the firm must pay to its employees (that leaves the cost of firing employees unaffected) affects innovation.

To this end, we examine the association between changes in state minimum wages and our measures of process and product innovation. The premise underlying our tests is that changes in state minimum wages have a significant impact the cost of labor that performs routine-intensive tasks, i.e., of labor that can easily be replaced by new labor-saving process technologies. It is unlikely that *all* such workers perceive minimum wages. However, our identification only requires that minimum wages impact the cost of a fraction of these workers either directly (if many earn minimum wages) or indirectly (if they do not earn minimum wages but higher minimum wages ultimately lead to increases in their wages).

Panel A of Table 9 reports the results of tests in which the dependent variables are either *Log(1 + # Process Claims)* or *Log(1 + # Product Claims)* and the key independent variable is *Minimum Wage* (the minimum hourly wage prevailing in the firm's state in dollars). We construct this variable using data on state minimum wages from the Wage and Hour Division of the U.S. Department of Labor[13]. To ensure it is based on meaningful changes in minimum wages, we exclude changes that only apply to small firms (which are not in our Compustat sample) or to a selected subset of a firm's employees (such as women and minors). The sample spans the period 1975-2011 (both our measures of innovation and the minimum wage data are available for this period) and contains all firms that filed at least one patent during the sample period. The regressions include firm-level control variables (*Log(1 + # Patents)*, *Log(1 + R&D Stock), Log(Sales)*, and *Log(Market-to-Book)*) and state-level control variables (*GDP Growth* and *Political Balance*) that are defined in section 2.3. They also include firm fixed effects and two-digit SIC industry fixed effects interacted with year fixed effects.

Columns (1)-(4) show a positive association between increases in minimum wages and process innovation that is statistically significant at the 1% level in all four specifications.

[13] The data is available at https://www.dol.gov/whd/state/stateMinWageHis.htm. The average minimum hourly wage rate over our sample period is \$4.3, and increases from \$1.60 in 1975 to \$7.51 in 2011.

The estimated coefficient of *Minimum Wage* is 0.040 in column (1), which includes the fixed effects only. Column (2) highlights once again the importance of controlling for a firm's patenting history, which reduces the estimated coefficient to 0.016. In column (4), which includes all the firm-level and state-level control variables, the coefficient is 0.011. To give an idea of economic significance, note that a 50c increase in minimum wages (this is 11.6% increase relative to sample mean of $4.3) leads to a .55% increase in process innovation (0.011x0.5). In contrast, columns (5)-(8) show that changes in minimum wages have no impact on product innovation (the coefficients are much smaller in magnitude and statistically insignificant in all four specifications). Since changes in state minimum wages are unlikely to be truly exogenous, we refrain from making a causal interpretation of these results and view them as correlations. However, they are consistent with the view that rigidities in labor markets may prompt firms to increase their efforts in developing labor-saving process innovation.

In panel B, we further explore the cross-sectional variation in the effect of minimum wages, using an approach analogous to that in section 3.3 and Table 5. Specifically, we augment our regression model to include the following interaction terms: *Minimum Wage* × *RTI*, *Minimum Wage* × *LaborCostShr*, and *Minimum Wage* × *RTI* × *LaborCostShr*. To facilitate the interpretation of the estimated coefficients for these interactions, both *RTI* and *LaborCostShr* are standardized to have mean zero and standard deviation of one before forming the interactions. The variables, defined in section 3.3, capture the routine task intensity and the labor cost share of the firm's 2-digit SIC industry, respectively. All regression models include the same control variables as in Panel A as well as firm fixed effects and two-digit SIC industry fixed effects interacted with year fixed effects. Columns (2) and (4) further include state fixed effects interacted with year fixed effects.

The results in column (1) show that increases in minimum wages are associated with a larger increase in process innovation (i) in industries that are more reliant on labor that performs routine-intensive tasks (the coefficient of *Minimum Wage* × *RTI* is positive and significant at the 10% level), (ii) in industries for which employees' wages account for a larger share of production costs (the coefficient of *Minimum Wage* × *LaborCostShr* is positive and significant at the 10% level), and (iii) especially in industries that exhibit *both* higher reliance on routine-intensive labor and a larger share of labor costs in total costs (the coefficient of *Minimum Wage* × *RTI* × *LaborCostShr*) is positive and significant at the 5% level). In column (2) we add state fixed effects interacted with year fixed effects. The coefficients of *Minimum*

Wage × *RTI* and *Minimum Wage* × *LaborCostShr* slightly decrease the magnitude and become insignificant. However, the coefficient of *Minimum Wage* × *RTI* × *LaborCostShr*, which captures the sharpest contrast in industry characteristics favoring the development of process innovation but not product innovation, remains statistically significant at the 5% and similar in magnitude. The results in columns (3)-(4) consistently show, once again, no effect of changes in minimum wages on product innovation, regardless of the nature of the industry. Taken together, this evidence further highlights how labor rigidities can induce firms to tilt their innovation activities towards developing labor-saving technologies.

To assess the economic significance of the effects in column (1), note that the coefficient of *Minimum Wage* captures the average impact of minimum wages on process innovation (a 0.013x50c=0.65% increase) for a firm in an industry with mean values of *RTI* and *LaborCostShr*. The coefficient of *Minimum Wage* × *RTI* implies that the increase in process innovation is an additional 0.85% (0.017x50c) larger in industries whose *RTI* is one-standard deviation higher than the sample mean (while keeping *LaborCostShr* at its sample mean). The coefficient of *Minimum Wage* × *LaborCostShr* implies that the increase in process innovation is an additional 1.1% (0.022x50c) larger in industries whose *LaborCostShr* is one-standard deviation higher than the sample mean (while keeping *RTI* at its sample mean). Last, the coefficient of *Minimum Wage* × *RTI* × *LaborCostShr* implies an additional 1.1% (0.022x50c) increase in process innovation for industries in which both *RTI* and *LaborCostShr* are one-standard deviation higher than their sample means (compared to the case of an industry for which either *RTI* or *LaborCostShr* is one-standard deviation above its sample mean, but not both). Putting together, our estimate suggests that a 50c increase in minimum wages could lead to a 3.7% increase in process innovation in industries that are both highly reliant on routine-intensive labor and have labor costs that account for large share of their total costs (0.65% + 0.85% + 1.1% + 1.1%).

Overall, while it is difficult to compare the impact of minimum wages and employment protection we document, our evidence suggests that employment protection has a significantly larger impact on the mix between process and product innovation. A natural explanation for this is that employment protection affects all of a firm's workers but minimum wages affect fewer workers, i.e., they only affect workers that earn low wages. Hence, changes in state mandated minimum wages have a smaller effect on firms' wage bills.

More generally, this evidence further suggests that higher operating risk leads firms to increase labor-saving process innovation, and that issues pertaining to the employment conditions of inventors, such as those discussed in section xx, are unlikely to be the main channel through which labor market frictions affect innovation. Importantly, increases in minimum wages directly increase labor costs and reduce the flexibility of the firm's wage bill (increasing operating leverage), but do not affect employees' job security (of inventors or other employees). Moreover, inventors are likely to earn significantly more than minimum wages, and thus such increases are unlikely to affect their wages. Hence, the results highlight the importance of changes in the effective price of labor as a driver of process innovation.

3.7 Robustness & additional tests

We now discuss the results of additional tests that help assess the robustness of our main results in Table 2. These results are reported in tables A1-A3 of our Online Appendix.

In Table A1, we examine the robustness of our results to using three alternative definitions the process and product innovation measures we use as dependent variables. First, in columns (1)-(2) we consider dependent variables that scale the number of independent claims contained in a firm's patent filing by the number of employees. Second, in columns (3)-(4) we consider dependent variables based on the total count of claims contained in a firm's patent filings, including both independent claims and dependent claims (which refer to other claims in the same patent). Third, in columns (5)-(6) we consider dependent variables based on patent counts rather than claim counts; to this end, we define a process patent as a patent containing at least one process claim and a product patent as a patent containing at least on product claim. For all three alternative definitions of our dependent variables, the results are qualitatively similar to those reported in Table 2.

In Table A2, we repeat our analyses using Negative Binomial specifications in which the dependent variables are the raw counts of process or product claims (not log transformed). Given that our dependent variables are counts of process or product claims, these specifications are in principle well suited for our analyses. However, they might be problematic for various reasons. First, our specifications require firm fixed effects and industry-year fixed effects, but these models cannot be estimated with such a large number of dummy variables. Hence, we are unable to estimate regression models that are analogous to our preferred specification. Second, we are able to estimate specifications with state fixed effects and year fixed effects (columns (1) and (3)) as well as with state fixed effects and industry-year fixed effects (columns (2) and (4)), but Negative Binomial models can give biased estimates in the presence of a large number of fixed effects. Last, there is a large number of zeros in our dependent variables, and the Negative Binomial model is sensitive to the zero-inflated problem. Notwithstanding the caveats above, in both specifications we consider we continue to find that the passage of the good-faith exemption leads to an increase in process innovation but not in product innovation.

In Table A3, we examine the impact of wrongful discharge protection on the *composition* of innovation, measured as the share of independent process claims in the total number of independent (process and product) claims contained in the firm's patent filings. The sample is thus restricted to firm-year observations in which the firm filed at least one patent. In these specifications, we can include both firm-level and state-level controls and always include firm-fixed effects and two-digit SIC fixed effects interacted with year fixed effects. Across all specifications, we find that the adoption of the good-faith exception leads to an increase in the share of process innovation of 3.2 percentage points that is statistically significant at the 1% level. Given a sample mean share of process innovation of 25%, this implies a 12.8% increase in the share of process innovation.

4. Conclusions

Our paper highlights that frictions in labor markets are an important determinant of a firm's process / product innovation mix. We focus on labor dismissal costs, which hurt firm value in various ways and ultimately increase the effective price of labor relative to other production inputs. Everything else the same, labor dismissal costs increase a firm's expected future labor costs and, importantly, they increase the firm's operating risk and cost of capital, which can in turn crowd-out debt finance. We show empirically that higher labor dismissal costs lead firms to significantly increase their innovation efforts towards developing labor-saving processes. This, together with the increase in physical capital and the outsourcing of jobs documented in prior work, helps firms mitigate the impact of such frictions on its value.

From a public policy point of view, our findings highlight the unintended (or at least not fully understood) consequences of labor regulation that aims to benefit employees. Labor laws that increase employment protection or otherwise make labor a more costly production input ultimately lead firms to innovate in new production processes that allow them to reduce labor costs. The adoption of new labor-saving technologies as well as the associated automation of production processes leads to a substitution of capital for routine task intensive labor, which can negate some of the benefits of employment protection sought by the regulation.

Our study also calls to attention the important distinction between process and product innovation. Prior studies examine the impact of changes in regulation on overall innovation, but such changes can have differential effects on process and product innovation. The new measures of product and process innovation we use in this paper can thus aid future research. For example, Eswaran and Gallini (1996) argue that because process innovation reinforces competition, too little is done, and because product innovation relaxes competition, too much is done. They further discuss how patent policy might be structured so as to redirect technological change toward a more efficient mix of products and processes. This highlights the empirical relevance of distinguishing product and process innovation.

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Table 1: Summary Statistics

The sample spans the period 1975-1997. It includes all non-financial and non-utility firms headquartered in the U.S. that filed at least one patent with the USPTO during this period as in Bloom, Schankerman and Van Reenen (2013), for a total of 44,614 firm-year observations. The key dependent variables are *Log(1 + # Process Claims)* and *Log(1 + # Product Claims),* respectively. Both variables are counts of independent claims, which are patent claims that do not reference any other claim in the same patent. The *Good Faith*, *Implied Contract*, and *Public Policy* indicator variables equal one if the firm's state of headquarters has adopted the corresponding exemption by year *t* and zero otherwise. The coding for these legal events follows Autor et al. (2006). The control variables described next are lagged one year. *Log(1 + # Patents)* is the logarithm of one plus the number of patents filed by the firm in a given year, *Log(1 + R&D Stock)* is the logarithm of one plus a firm's R&D stock computed by adding its R&D spending (*xrd*, in $ million) since 1950 and assuming an annual depreciation rate of 15%, *Log(Sales)* is the logarithm of a firm's sales (*sale*, in $ million), *Log(Market-to-Book)* is the logarithm of a firm's market value of assets (the sum of the market value of equity, *csho*prcc_f*, the book value of long term debt, *dltt*, and the book value of debt in current liabilities, *dlc*) scaled by the book value of assets (*at*), *GDP Growth* is the one-year growth rate of state GDP in current dollars (from the Bureau of Economic Analysis), and *Political Balance* is the fraction of a state's congress members in the U.S. House of Representatives that belong to the Democratic Party (from History, Art & Archives, U.S. House of Representatives).

	Mean	Std. Dev.	Pctile 10	Median	Pctile 90
Dependent Variables					
Log(1 + # Process Claims)	0.688	1.219	0.000	0.000	2.485
Log(1 + # Product Claims)	1.173	1.537	0.000	0.000	3.434
Wrongful Discharge Laws Indicator Variables					
Good Faith	0.220	0.414	0.000	0.000	1.000
Implied Contract	0.623	0.485	0.000	1.000	1.000
Public Policy	0.650	0.477	0.000	1.000	1.000
Lagged Control Variables					
Log(1 + # Patents)	0.882	1.238	0.000	0.693	2.639
Log(1 + R&D Stock)	1.836	1.930	0.000	1.310	4.629
Log(Sales)	4.693	2.265	1.928	4.689	7.620
Log(Market-to-Book)	0.115	0.715	-0.631	-0.019	1.087
GDP Growth	0.078	0.036	0.035	0.075	0.125
Political Balance	0.603	0.167	0.417	0.600	0.833

Table 2: Wrongful Discharge Laws and Process vs. Product Innovation

The table reports the results of OLS regressions of the following form:

$$Y_{i,s,j,t} = \alpha_i + \mu_{j,t} + \beta_1 Good\ Faith_{s,t} + \beta_2 Implied\ Contract_{s,t} + \beta_3 Public\ Policy_{s,t} + \lambda Controls_{i,s,j,t-1} + \varepsilon_{i,s,j,t}$$

where *i, s, j,* and *t* denote firm, state of headquarters, two-digit SIC industry, and year, respectively. The dependent variable is *Log(1 + # Process Claims)* in columns (1)-(4) and *Log(1 + # Product Claims)* in columns (5)-(8). All variables are defined in Table 1. The variables α_i and $\mu_{j,t}$, included in all specifications, denote firm-fixed effects and industry fixed effects interacted with year fixed effects, respectively. The sample spans the period 1975-1997 and includes all non-financial and non-utility firms that filed at least one patent during this period. Standard errors are adjusted for heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Process Claims				Product Claims			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Good Faith	0.110***	0.083***	0.074***	0.074***	0.071*	0.036	0.027	0.027
	(0.027)	(0.019)	(0.016)	(0.018)	(0.036)	(0.025)	(0.024)	(0.024)
Implied Contract	-0.015	-0.018	-0.008	-0.011	-0.001	-0.005	0.007	0.005
	(0.030)	(0.021)	(0.017)	(0.017)	(0.035)	(0.022)	(0.020)	(0.020)
Public Policy	-0.023	-0.015	-0.010	-0.010	-0.019	-0.009	-0.003	-0.003
	(0.020)	(0.014)	(0.014)	(0.013)	(0.024)	(0.016)	(0.015)	(0.016)
Log(1 + # Patents)		0.371***	0.317***	0.317***		0.466***	0.403***	0.403***
		(0.025)	(0.017)	(0.017)		(0.020)	(0.019)	(0.019)
Log(1 + R&D Stock)			0.093***	0.093***			0.079***	0.079***
			(0.015)	(0.015)			(0.018)	(0.018)
Log(Sales)			-0.017	-0.017			0.026**	0.026**
			(0.011)	(0.011)			(0.012)	(0.012)
Log(Sales)^2			0.018***	0.018***			0.019***	0.019***
			(0.003)	(0.003)			(0.003)	(0.003)
Log(Market-to-Book)			0.029**	0.030***			0.048***	0.049***
			(0.011)	(0.011)			(0.012)	(0.013)
GDP Growth				-0.319				-0.246
				(0.194)				(0.272)
Political Balance				-0.056				-0.011
				(0.035)				(0.033)
Observations	44,614	44,614	44,614	44,614	44,614	44,614	44,614	44,614
Adjusted R^2	0.757	0.783	0.790	0.790	0.747	0.773	0.779	0.779

Table 3: Dynamic Effects

The table reports the results of OLS regressions analogous to those in Table 2 (columns (4) and (8)) that allow for dynamic effects of the adoption of the Good Faith exemption. The dependent variable is *Log(1 + # Process Claims)* in columns (1)-(3) and *Log(1 + # Product Claims)* in columns (4)-(6). *Good Faith -2, Good Faith -1, Good Faith 0, Good Faith +1, Good Faith +2,* and *Good Faith 3+* are indicator variables equal to one if the firm's state of headquarters will adopt the exemption in two years, will adopt it the following year, adopted it in the current year, adopted it the year before, adopted it two years before, or adopted it three or more years before, respectively. The regressions also include *Implied Contract* and *Public Policy* as well as the lagged control variables defined in Table 1 (the coefficients are omitted for brevity). All specifications include firm-fixed effects and 2-digit SIC industry fixed effects interacted with year fixed effects. Columns (2) and (5) include *State-Year Trend*, defined as the logarithm of the mean number of patents filed by firms in the state (the patents of the firm itself are excluded in the calculation of this mean), as an additional control variable (coefficient omitted). Columns (3) and (6) include interactions *of Log(1 + # State Patents 74-76)*, defined as the logarithm of the total number of patents filed in the state over the period 1974-1976 (preceding the first adoption of the Good Faith exemption in our sample), and year indicator variables. The sample spans the period 1975-1997 and includes all non-financial and non-utility firms that filed at least one patent during this period. Standard errors are adjusted for heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Process Claims			Product Claims		
	(1)	(2)	(3)	(4)	(5)	(6)
Good Faith -2	-0.005	-0.006	0.003	-0.051	-0.056	-0.049
	(0.028)	(0.029)	(0.027)	(0.042)	(0.043)	(0.043)
Good Faith -1	0.025	0.024	0.033	-0.047	-0.050	-0.048
	(0.020)	(0.021)	(0.022)	(0.043)	(0.042)	(0.041)
Good Faith 0	0.023	0.023	0.032	-0.032	-0.034	-0.028
	(0.024)	(0.024)	(0.022)	(0.046)	(0.045)	(0.043)
Good Faith +1	-0.002	-0.002	0.013	-0.024	-0.027	-0.014
	(0.024)	(0.023)	(0.027)	(0.036)	(0.035)	(0.040)
Good Faith +2	0.075**	0.074**	0.086**	0.022	0.019	0.029
	(0.035)	(0.035)	(0.035)	(0.044)	(0.042)	(0.045)
Good Faith 3+	0.098***	0.097***	0.107***	0.025	0.018	0.029
	(0.029)	(0.031)	(0.027)	(0.030)	(0.030)	(0.030)
Implied Contract	-0.006	-0.005	-0.009	0.011	0.015	0.012
	(0.015)	(0.014)	(0.014)	(0.019)	(0.018)	(0.019)
Public Policy	-0.010	-0.009	0.002	-0.000	0.002	0.003
	(0.013)	(0.012)	(0.012)	(0.016)	(0.015)	(0.015)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Trend	No	Yes	No	No	Yes	No
Log(1 + # State Patents 74-76) × Year Fixed Effects	No	No	Yes	No	No	Yes
Observations	44,293	44,286	44,293	44,293	44,286	44,293
Adjusted R^2	0.791	0.791	0.791	0.780	0.780	0.780

Table 4: Cross Sectional Variation: Routine-Task Intensity and Labor Cost Share

The table reports the results of OLS regressions analogous to those in Table 2 augmented to include an interaction between *Good Faith* and the routine task intensity of the firm's 2-digit SIC industry *(RTI)*, an interaction between *Good Faith* and the labor cost share of the firm's 2-digit SIC industry (*LaborCostShr*), and an interaction of *Good Faith*, *RTI*, and *LaborCostShr*. Both *RTI* and *LaborCostShr* are standardized to have mean zero and standard deviation of one before forming the interactions. The details on the construction of these two variables are provided in section 3.3. The dependent variables are *Log(1 + # Process Claims)* in columns (1)-(2) and *Log(1 + # Product Claims)* in columns (3)-(4). The regressions also include *Implied Contract* and *Public Policy* as well as the lagged control variables defined in Table 1 (the coefficients are omitted for brevity). All specifications include firm-fixed effects and 2-digit SIC industry fixed effects interacted with year fixed effects. Columns (2) and (4) additionally include state fixed effects interacted with year fixed effects. The sample spans the period 1975-1997 and includes all non-financial and non-utility firms that filed at least one patent during this period. Standard errors are adjusted for heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Process Claims		Product Claims	
	(1)	(2)	(3)	(4)
Good Faith	0.068***		0.031	
	(0.018)		(0.027)	
Good Faith × RTI	0.042**	0.031	0.021	0.024
	(0.020)	(0.020)	(0.036)	(0.032)
Good Faith × LaborCostShr	0.061***	0.069***	0.050	0.057**
	(0.018)	(0.021)	(0.030)	(0.028)
Good Faith × RTI × LaborCostShr	0.041**	0.041**	-0.002	0.002
	(0.017)	(0.019)	(0.024)	(0.024)
Implied Contract	-0.012		0.003	
	(0.017)		(0.020)	
Public Policy	-0.012		-0.006	
	(0.013)		(0.016)	
Control Variables	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
SIC2 × Year Fixed Effects	Yes	Yes	Yes	Yes
State × Year Fixed Effects	No	Yes	No	Yes
Observations	44,514	44,425	44,514	44,425
Adjusted R^2	0.790	0.790	0.779	0.778

Table 5: Wrongful Discharge Laws and Process Innovation: Inventors' State of Location

Columns (1)-(2) report the results of OLS regressions analogous to those in column (4) of Table 2, but using *Log(1 + # Process Claims with Inventors Located in Other States)* as the dependent variable. This dependent variable is based on the count of process claims contained in patent filings for which the first inventor listed is employed in a state other than the state of the firm's headquarters. In columns (3)-(4),we report the results of OLS regressions analogous to those in column (4) of Table 2, also using *Log(1 + # Process Claims)* as the dependent variable, but augmented to include interactions between *Good Faith* and two alternative variables. The first is *Inventors in Other States*, an indicator variable equal to one if the first inventors listed in all patents filed by the firm in a given year are located in a state other than the state of the firm's headquarters, and zero otherwise. The second is *Disperse Inventors*, an indicator variable equal to one if no state concentrates more than 50% of the first investors listed in all patents the firm applied for in a given year, and zero otherwise. The regressions in all four columns also include *Implied Contract* and *Public Policy* as well as the lagged control variables defined in Table 1 (the coefficients are omitted for brevity). All specifications include firm-fixed effects and 2-digit SIC industry fixed effects interacted with year fixed effects. The sample spans the period 1975-1997 and includes all non-financial and non-utility firms that filed at least one patent during this period. Standard errors are adjusted for heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable is:	*Log(1 + # Process Claims with Inventors Located in Other States)*		*Log(1 + # Process Claims)*	
	(1)	(2)	(3)	(4)
Good Faith	0.054***	0.032**	0.082***	0.067***
	(0.017)	(0.012)	(0.023)	(0.019)
Good Faith × Inventors in Other States			-0.011	
			(0.046)	
Inventors in Other States			0.207***	
			(0.024)	
Good Faith × Disperse Inventors				0.074
				(0.056)
Disperse Inventors				0.186***
				(0.024)
Implied Contract	-0.014	-0.008	-0.009	-0.012
	(0.021)	(0.015)	(0.017)	(0.017)
Public Policy	-0.038**	-0.029**	-0.008	-0.009
	(0.016)	(0.014)	(0.013)	(0.014)
Control Variables	No	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
SIC2 × Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	44,614	44,614	44,614	44,614
Adjusted R^2	0.748	0.773	0.792	0.791

Table 6: Wrongful Discharge Laws and Process Innovation: Local Economics Shocks

The table reports the results of OLS regressions analogous to those in column (4) of Table 2, which uses *Log(1 + # Process Claims)* as the dependent variable, but with two differences. The specification in column (1) additionally includes an indicator variable *Good Faith Neighbor* for whether a "neighboring state", i.e., a state adjacent to the firm's state of headquarters, has adopted the Good Faith exemption by year *t* and zero otherwise. The specification in column (2) is identical to that in Table 2, but is estimated after excluding from the sample firms in states that did not adopt the Good Faith exception during our sample period and do not share borders with any of the states that did adopt it. In addition to *Good Faith*, the regressions include *Implied Contract* and *Public Policy* as well as the lagged control variables defined in Table 1 (the coefficients are omitted for brevity). All specifications include firm-fixed effects and 2-digit SIC industry fixed effects interacted with year fixed effects. The sample spans the period 1975-1997 and includes all non-financial and non-utility firms that filed at least one patent during this period. Standard errors are adjusted for heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Good Faith	0.073***	0.060**
	(0.018)	(0.024)
Good Faith Neighbor	-0.008	
	(0.027)	
Implied Contract	-0.010	0.006
	(0.018)	(0.024)
Public Policy	-0.010	0.003
	(0.014)	(0.015)
Control Variables	Yes	Yes
States in Sample	All	Non-Bordering Control States Excluded
Observations	44,614	28,666
Adjusted R^2	0.790	0.793

Table 7: Wrongful Discharge Laws and Process Innovation: Time Trends in Innovation

The table reports the results of OLS regressions analogous to those in column (4) of Table 2, which uses *Log(1 + # Process Claims)* as the dependent variable, but with alternative approaches. In addition to *Good Faith*, the regressions include *Implied Contract* and *Public Policy* as well as the lagged control variables defined in Table 1 (the coefficients are omitted for brevity). Column (1) includes *State-Year Trend*, defined as the logarithm of the mean number of patents filed by firms headquartered in the state (the patents of the firm itself are excluded in the calculation of this mean), as an additional control variable (coefficient omitted). Column (2) includes interactions of *Log(1 + # State Patents 74-76)*, defined as the logarithm of the total number of patents filed by firms headquartered in the state over the period 1974-1976 (the period preceding the first adoption of the Good Faith exemption in our sample) and year fixed effects (coefficients omitted). Column (3) uses the same specification as in Table 2 but restricting the sample to the period 1975-1990. Column (4) uses the same specification as in Table 2 excluding from the sample firms headquartered in California and Massachusetts. All specifications include firm-fixed effects and 2-digit SIC industry fixed effects interacted with year fixed effects. The sample includes all non-financial and non-utility firms that filed at least one patent during the period 1975-1997. Standard errors are adjusted for heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Good Faith	0.070***	0.078***	0.053***	0.084***
	(0.019)	(0.018)	(0.017)	(0.029)
Implied Contract	-0.008	-0.015	-0.024	-0.003
	(0.015)	(0.016)	(0.015)	(0.018)
Public Policy	-0.009	0.002	0.005	-0.009
	(0.012)	(0.012)	(0.015)	(0.016)
Control Variables	Yes	Yes	Yes	Yes
State-Year Trend	Yes	No	No	No
Log(1 + # State Patents 74-76) × Year Fixed Effects	No	Yes	No	No
Sample Period	1975-97	1975-97	1975-90	1975-97
States in Sample	All	All	All	Excl. CA & MA
Observations	44,609	44,614	29,130	35,839
Adjusted R^2	0.790	0.790	0.812	0.808

Table 8: Wrongful Discharge Laws, Physical Investment, and Labor Productivity

The table reports OLS regressions of investment and productivity variables on the wrongful discharge laws indicator variables and control variables. The dependent variables are the logarithm of a firm's capital expenditures per employee (*Log(capex/emp)*), the logarithm of a firm's net property plant and equipment per employee (*Log(Capital-Labor Ratio)*), and the logarithm of a firm's sales per employee (*Log(sales/emp)*). *Good Faith*, *Implied Contract*, *Public Policy*, and the lagged control variables are defined in Table 1, except for *Profit*, which is operating income before depreciation and taxes (*oibdp*) scaled by book assets (*at*). All specifications include firm-fixed effects and 2-digit SIC industry fixed effects interacted with year fixed effects. The sample spans the period 1975-1997 and includes all non-financial and non-utility firms that filed at least one patent during this period. Standard errors are adjusted for heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Log(Capex/Emp)			Log(K/L)			Log(Sales/Emp)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Good Faith	0.047*	0.053**	0.052**	0.078***	0.073***	0.072***	0.053***	0.040**	0.040**
	(0.027)	(0.021)	(0.023)	(0.020)	(0.020)	(0.020)	(0.017)	(0.018)	(0.018)
Implied Contract	0.003	0.014	0.019	-0.005	0.001	-0.001	0.002	0.002	0.002
	(0.020)	(0.018)	(0.018)	(0.013)	(0.014)	(0.014)	(0.016)	(0.015)	(0.015)
Public Policy	-0.045	-0.042*	-0.042*	-0.047***	-0.043***	-0.044***	-0.022*	-0.018*	-0.018*
	(0.027)	(0.023)	(0.023)	(0.015)	(0.016)	(0.016)	(0.011)	(0.010)	(0.010)
Log(Market-to-Book)		0.307***	0.305***		0.033***	0.033***		0.034***	0.034***
		(0.020)	(0.020)		(0.010)	(0.010)		(0.010)	(0.010)
Profit		1.127***	1.125***		-0.111**	-0.110**		0.125**	0.125**
		(0.056)	(0.056)		(0.045)	(0.045)		(0.053)	(0.053)
Log(Sales)		-0.041***	-0.041***		0.065***	0.065***		0.240***	0.240***
		(0.015)	(0.015)		(0.017)	(0.017)		(0.017)	(0.017)
Log(sales)^2		0.014***	0.013***		0.006***	0.006***		-0.012***	-0.012***
		(0.002)	(0.002)		(0.002)	(0.002)		(0.002)	(0.002)
GDP Growth			0.518**			-0.199			-0.033
			(0.195)			(0.200)			(0.075)
Political Balance			0.109**			0.050			-0.020
			(0.043)			(0.041)			(0.032)
Observations	43,248	43,248	43,248	43,849	43,849	43,849	43,789	43,789	43,789
Adjusted R^2	0.691	0.715	0.716	0.865	0.867	0.867	0.816	0.835	0.835

Table 9: Impact of Changes in State Minimum Wages on Process and Product Innovation

Panel A of the table reports OLS regressions analogous to those in Table 2 and Panel B reports OLS regressions analogous to those in Table 5, but use the minimum hourly wage rate prevalent in the firm's state of headquarters (*Minimum Wage*) as they key independent variable. In both panels the dependent variables are *Log(1 + # Process Claims)* and *Log(1 + # Product Claims)* and the lagged control variables are defined in Table 1. Panel A reports the results of the baseline regression with firm-fixed effects and SIC2 industry fixed effects interacted with year fixed effects. Panel B augments the specifications in columns (4) and (8) of Panel A to include an interaction between *Minimum Wage* and the routine task intensity *(RTI)* of the firm's 2-digit SIC industry, an interaction between *Minimum Wage* and the labor cost share of the firm's 2-digit SIC industry (*LaborCostShr*), and an interaction of *Minimum Wage*, *RTI*, and *LaborCostShr*. Both *RTI* and *LaborCostShr* are standardized to have mean zero and standard deviation of one before forming the interactions, and are defined in Table 5. Columns (1)-(3) include firm-fixed effects and SIC2 industry fixed effects interacted with year fixed effects. Columns (2)-(4) further include state fixed effects interacted with year fixed effects. The sample spans the period 1975-2012 and includes all non-financial and non-utility firms that filed at least one patent during this period. Standard errors are adjusted by heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Main results

	Process Claims				Product Claims			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Minimum Wage	0.040*	0.016*	0.012*	0.011*	0.038	0.009	0.005	0.004
	(0.021)	(0.009)	(0.006)	(0.006)	(0.023)	(0.008)	(0.005)	(0.005)
Log(1 + # Patents)		0.518***	0.477***	0.477***		0.619***	0.574***	0.574***
		(0.019)	(0.014)	(0.014)		(0.014)	(0.013)	(0.013)
Log(1 + R&D Stock)			0.047***	0.047***			0.034**	0.034**
			(0.010)	(0.010)			(0.014)	(0.014)
Log(Sales)			-0.015	-0.015			0.031**	0.031**
			(0.010)	(0.010)			(0.012)	(0.012)
Log(Sales)^2			0.012***	0.012***			0.011***	0.011***
			(0.002)	(0.002)			(0.002)	(0.002)
Log(Market-to-Book)			0.068***	0.068***			0.070***	0.070***
			(0.015)	(0.015)			(0.012)	(0.013)
GDP Growth				-0.096				-0.039
				(0.153)				(0.182)
Political Balance				0.015				0.049
				(0.031)				(0.037)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SIC2 × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	69,130	69,130	69,130	69,130	69,130	69,130	69,130	69,130
Adjusted R^2	0.722	0.780	0.784	0.784	0.724	0.781	0.784	0.784

Panel B: Cross-Sectional Variation

	Process Claims		Product Claims	
	(1)	(2)	(3)	(4)
Minimum Wage	0.013*		0.005	
	(0.006)		(0.005)	
Minimum Wage × RTI	0.017*	0.013	0.007	0.003
	(0.010)	(0.010)	(0.007)	(0.007)
Minimum Wage × LaborCostShr	0.022*	0.021	0.013	0.008
	(0.013)	(0.015)	(0.015)	(0.017)
Minimum Wage × RTI × LaborCostShr	0.022**	0.020**	0.011	0.008
	(0.009)	(0.010)	(0.009)	(0.010)
Control Variables	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
SIC2 × Year Fixed Effects	Yes	Yes	Yes	Yes
State × Year Fixed Effects	No	Yes	No	Yes
Observations	69,011	68,913	69,011	68,913
Adjusted R^2	0.785	0.785	0.784	0.784

Internet Appendix

Table A1: Wrongful Discharge Laws and Process vs. Product Innovation - Alternative Measures of Process & Product Innovation

The table reports the results of OLS regressions analogous to those reported in Table 2 (columns (4) and (8)), but using alternative definitions of the dependent variables. In columns (1)-(2), the dependent variables are based on the count of independent claims scaled by employment (*emp*): *Log(1 + # Process Claims Per Employee)* in column (1) and *Log(1 + # Product Claims Per Employee)* in column (2). In columns (3)-(4), the dependent variables are based on the count of both independent and dependent claims: *Log(1 + # Total Process Claims)* in column (3) and *Log(1 + # Total Product Claims)* in column (4). In columns (5)-(6), the dependent variables are based on patent counts rather than counts of claims in the patent: *Log(1 + # Process Patents)* in columns (5) and *Log(1 + # Product Patents)* in columns (6), where process patents contain at least one process claim and product patents contain at least one product claim. In addition to *Good Faith*, the regressions include *Implied Contract* and *Public Policy* as well as the lagged control variables defined in Table 1 (the coefficients are omitted for brevity). All specifications include firm-fixed effects and 2-digit SIC industry fixed effects interacted with year fixed effects. The sample spans the period 1975-1997 and includes all non-financial and non-utility firms that filed at least one patent during this period. Standard errors are adjusted for heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Independent Claims Scaled by Employment		Total Claims (Dependent + Independent)		Patent Counts	
	Process	Product	Process	Product	Process	Product
	(1)	(2)	(3)	(4)	(5)	(6)
Good Faith	0.057**	0.005	0.072***	-0.007	0.050***	0.032*
	(0.021)	(0.022)	(0.025)	(0.038)	(0.015)	(0.017)
Implied Contract	-0.016	0.007	-0.018	-0.001	0.003	-0.002
	(0.013)	(0.019)	(0.024)	(0.030)	(0.011)	(0.016)
Public Policy	-0.018*	-0.005	-0.007	-0.004	-0.018*	-0.004
	(0.011)	(0.016)	(0.021)	(0.025)	(0.011)	(0.013)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	43,929	43,929	44,614	44,614	44,614	44,614
Adjusted R^2	0.513	0.480	0.740	0.707	0.827	0.838

Table A2: Wrongful Discharge Laws and Process vs. Product Innovation - Negative Binomial Specifications

The table reports the results of Negative Binomial regression models based on the count of independent claims contained in the patents. The dependent variables are # *Process Claims* and # *Product Claims*, which denote the count of independent process claims and the count of independent product claims in all patents filed by the firm, respectively. Columns (1) and (3) include state fixed effects and year fixed effects; columns (2) and (4) contain state fixed effects and 2-digit SIC industry fixed effects interacted with year fixed effects. The lagged control variables included in all columns are defined in Table 1 (the coefficients are omitted for brevity). The sample spans the period 1975-1997 and includes all non-financial and non-utility firms that filed at least one patent during this period. Standard errors are adjusted for heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Process Claims		Product Claims	
	(1)	(2)	(3)	(4)
Good Faith	0.126**	0.078*	0.029	0.039
	(0.054)	(0.043)	(0.059)	(0.058)
Implied Contract	0.101	0.039	0.045	0.036
	(0.073)	(0.050)	(0.049)	(0.038)
Public Policy	0.009	-0.012	0.044	0.001
	(0.050)	(0.043)	(0.035)	(0.032)
Control Variables	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No
SIC2 × Year Fixed Effects	No	Yes	No	Yes
Observations	45,060	45,060	45,060	45,060

Table A3: Effect of Wrongful Discharge Laws on the Share of Process Claims in Total Claims

The table reports the results of OLS regressions in which the dependent variable is the share of independent process claims in the total number of independent (process and product) claims, conditional on a firm's filing of at least one patent in that year. All specifications include firm-fixed effects and SIC2 times year fixed effects. All control variables are defined in Table 1. Standard errors are adjusted by heteroscedasticity and clustering at the state level (standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Good Faith	0.033***	0.032***	0.032***	0.032***
	(0.009)	(0.009)	(0.009)	(0.009)
Implied Contract	0.002	0.002	0.002	0.001
	(0.006)	(0.006)	(0.006)	(0.005)
Public Policy	-0.004	-0.004	-0.004	-0.004
	(0.007)	(0.007)	(0.007)	(0.006)
Log(1 + # Patents)		0.001	0.001	0.001
		(0.002)	(0.003)	(0.003)
Log(1 + R&D Stock)			0.010**	0.010**
			(0.004)	(0.004)
Log(Sales)			0.001	0.001
			(0.004)	(0.004)
Log(Sales)^2			-0.001*	-0.001*
			(0.000)	(0.000)
Log(Market-to-Book)			0.007*	0.008**
			(0.004)	(0.004)
GDP Growth				-0.101
				(0.074)
Political Balance				-0.020
				(0.017)
Firm Fixed Effects	Yes	Yes	Yes	Yes
SIC2 × Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	No	No
Year Fixed Effects	No	No	No	No
Observations	21,985	21,985	21,985	21,985
Adjusted R^2	0.385	0.385	0.385	0.385