NBER WORKING PAPER SERIES

LABOR LAWS AND INNOVATION

Viral V. Acharya
Ramin P. Baghai
Krishnamurthy V. Subramanian

Working Paper 16484
http://www.nber.org/papers/w16484

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2010

We are grateful to Amit Seru and Vikrant Vig for helpful comments and to Anusha Chari, Rich Mathews, Amalia R. Miller, and Radha Iyengar for their insightful discussions. Furthermore, we would like to thank seminar and conference participants at the American Law and Economics Annual Meeting (2009), Western Finance Association Annual Meeting (2009), NBER Summer Institutes on Innovation Policy and the Economy (2009) and Law and Economics (2009), Summer Research Conference in Finance (2009) at the Indian School of Business (ISB), Cambridge University (Centre for Business Research), Emory University, London Business School, NYU Microeconomics seminar, and NYU Stern for valuable comments and suggestions. We thank Hanh Le and Chandrasekhar Mangipudi for excellent research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Labor Laws and Innovation
Viral V. Acharya, Ramin P. Baghai, and Krishnamurthy V. Subramanian
NBER Working Paper No. 16484
October 2010
JEL No. F30,G31,J08,J5,K31

ABSTRACT

Stringent labor laws can provide firms a commitment device to not punish short-run failures and thereby spur their employees to pursue value-enhancing innovative activities. Using patents and citations as proxies for innovation, we identify this effect by exploiting the time-series variation generated by staggered country-level changes in dismissal laws. We find that within a country, innovation and economic growth are fostered by stringent laws governing dismissal of employees, especially in the more innovation-intensive sectors. Firm-level tests within the United States that exploit a discontinuity generated by the passage of the federal Worker Adjustment and Retraining Notification Act confirm the cross-country evidence.

Viral V. Acharya
Stern School of Business
New York University
44 West 4th Street, Suite 9-84
New York, NY 10012
and NBER
vacharya@stern.nyu.edu

Ramin P. Baghai
London Business School
rbaghai.phd2006@london.edu

Krishnamurthy V. Subramanian
Indian School of Business
Gachibowli
Hyderabad (A.P.)
India 500032
kvsubramania@gmail.com

1 Introduction

Do legal institutions of an economy affect the pattern of its real investments, and, in turn, its economic growth? In this paper, we focus on one specific aspect of this overarching theme. In particular, we investigate whether the legal framework governing the relationships between employees and their employers affects the extent of innovation in an economy.

While the inefficiencies and rigidities associated with stringent labor laws — laws that prevent employers from seamlessly negotiating and/or terminating labor contracts with employees — are much celebrated in the academic literature[1] and the media, this discussion is generally centered around the *ex post* effects of labor laws.[2] In particular, it is not difficult to see that once the situation to renegotiate or terminate an employment contract has arisen, tying down an employer's hands from doing so can lead to ex post inefficient outcomes. Much less studied, however, is the *ex ante* incentive effect of such strong labor laws. Might stringent labor laws, even if as an unintended consequence, provide firms a commitment device to not punish short-run failures and thereby spur their employees to undertake activities that are value-maximizing in the long-run? In this paper, we focus on one specific dimension of labor laws. We provide empirical evidence that dismissal laws – laws that make it difficult for firms to discharge employees – indeed appear to have an ex ante *positive* incentive effect by encouraging firms and their employees to engage in more successful, and more significant, innovative pursuits.

To provide this evidence, we use data on patents issued by the United States Patent and Trademark Office (USPTO) to U.S. and foreign firms as well as citations to these patents as constructed by Hall, Jaffe and Trajtenberg (2001). The "industry" level classification we employ pertains to the patent classes in this data. We measure innovation for an industry in a given year by the number of patents applied for (and subsequently granted), the number of all subsequent citations to these patents, and the number of firms filing for patents in that year and industry.

We use the index of labor laws developed by Deakin et al. (2007). They construct this index by analyzing in detail the evolution of differences in employment protection legislation in five coun-

[1] Botero et al. (2004), for example, claim that heavier regulation of labor leads to adverse consequences for labor market participation and unemployment.

[2] For example, strong labor market regulation is often blamed to be one of the reasons for Europe's economic under-performance compared to the U.S. For a recent study articulating this theme, see the study of France and Germany by the McKinsey Global Institute (1997).

tries — U.S., U.K., France, Germany, and India — over the period 1970–2006. They analyze *forty* dimensions of labor laws and group them into five components that correspond to the regulation of: (i) alternative forms of labor contracting; (ii) working time; (iii) dismissal; (iv) employee representation; and (v) industrial action. The index takes into account not just the formal or positive law but also the self-regulatory mechanisms that play a functionally similar role to laws in certain countries. While using the Deakin et al. index forces us to limit our cross-country analysis to only the five countries mentioned above, these countries account for 72% of the patents filed with the USPTO during our sample period. Given our focus on laws that govern dismissal of employees, we mainly employ their dismissal law sub-index in our tests.

To obtain sharp empirical predictions that we test using this data, we develop a theoretical model in an addendum to the paper. The model considers an incomplete contracts setup in which the firm is unable to reward innovative pursuits sufficiently since it cannot separate bad luck from poor effort. Absent such separation, it may be ex post efficient for the firm to dismiss employees after their pursuits fail, even though this weakens ex ante incentives to innovate. Stringent dismissal laws alleviate this commitment problem and thereby spur innovation. Hence, we test

HYPOTHESIS 1: *Stronger dismissal laws lead to greater innovation.*

Since the ex ante incentive effect should matter more in the innovative sectors of the economy, we also test

HYPOTHESIS 2: *Stronger dismissal laws lead to relatively more innovation in the innovation-intensive industries than in the traditional industries.*

Since other aspects of labor laws do not have this ex ante incentive effect, we test

HYPOTHESIS 3: *Laws governing dismissal of employees influence innovation more than other aspects of labor laws.*

Further, endogenous growth theory (see Aghion and Howitt, 2005, for example) informs us that country-level laws and institutions that encourage innovation should accelerate country-level economic growth. Hence, we also investigate

HYPOTHESIS 4: *Stronger dismissal laws lead to greater country-level economic growth, particularly in the more innovation-intensive industries.*

Finally, our theoretical argument implies that stringent dismissal laws makes innovation value-enhancing to the firm. Thus, stringent dismissal laws should lead to (i) an increase in R&D

investment; and (ii) better firm performance. Therefore, we examine

HYPOTHESIS 5: *Stronger dismissal laws increase R&D investment and lead to better performance at the firm-level.*

To test Hypothesis 1, we employ panel regressions of our proxies for innovation on the Deakin et al. (2007) dismissal law index, where we include fixed effects for country, industry (i.e., patent class) and application year. As Imbens and Wooldridge (2009) suggest, these regressions enable us to estimate a "difference-in-difference" effect in a setting with multiple treatment countries and multiple time periods. In these tests, we find that more stringent dismissal laws positively influence the innovative activity in a country. This effect is statistically and economically significant: an increase in the dismissal law index by one standard deviation, ceteris paribus, results in a rise in the annual number of patents, number of patenting firms, and citations by 6.1%, 7.0% and 9.2% respectively. In estimating this effect, we also control for (i) a country's creditor rights, its rule of law, efficiency of judicial system, and anti-director rights; (ii) a country's bilateral trade with the U.S. in each of its industries, which is necessitated by our use of U.S. patents to proxy innovation in these countries; (iii) a measure of the country's comparative advantage in an industry in a given year; and (iv) the GDP per capita of the country.

A key concern in the above tests stems from the endogeneity of the dismissal law changes: other factors that accompany these law changes may be accounting for our results. Specifically, changes in a country's government, such as a change in its political leanings, may confound our results. We examine robustness to such concerns through two separate tests. First, we augment our fixed effects specification with country-specific and industry-specific trends. This enables us to identify the effect of dismissal law changes using deviations (at the patent class level) from the average time trends for each country and each industry. Since some of the above confounding effects would manifest in country-specific and industry-specific time trends, we isolate better the pure effect of dismissal law changes on innovation. Second, we examine directly the endogeneity introduced by a change in government by including a time-varying proxy for the political leanings of a country's government. We find that the main effect of the dismissal laws on innovation stays positive and significant even after accounting for the government's political leanings.

Next, to test Hypothesis 1 for each country that underwent a significant dismissal law change, we study the before-after effect of a change in dismissal laws in the affected country (the "treatment group") vis-à-vis the before-after effect in a country where such a change was not effected (the "control group") around the period of change. We examine the effects of changes in dismissal laws in the U.S. through the passage of the Worker Adjustment and Retraining Notification Act (WARN) in 1989 and similar changes in the U.K. and France in the 1970s. Our results remain similar to those using the full sample.

Having found support for Hypothesis 1 linking dismissal laws to innovation, we investigate Hypothesis 2. To conduct these tests, we follow Acharya and Subramanian (2009) in ranking patent classes by their patenting intensity in the U.S. We interact this proxy for innovation intensity with the dismissal law index in the fixed effects panel regressions. We find that the coefficient on this interaction term is significantly positive, which implies that the effect of dismissal laws is more pronounced in industries that have a greater propensity to innovate. We also shed light on Hypothesis 3. To this end, we line up the five dimensions of the labor laws of Deakin et al.'s index and find that the "regulation of dismissal" component is the only one which has a consistently positive and significant effect on innovation.

In other tests, we confirm that the direction of causality runs from labor laws to innovation rather than vice versa. As a final robustness check, we investigate the effect of dismissal law changes on innovation undertaken by individuals (as opposed to firm employees) and find no effect on the same. Since dismissal laws governing employment by firms should have no effect on innovation undertaken by individuals, these results provide evidence that the postulated effect of dismissal laws on employee incentives for innovation applies inside firms only.

In our next piece of cross-country evidence, we investigate if the positive effect of dismissal laws on innovation translates into a positive effect on economic value at the country level (Hypothesis 4). In regressions using growth rates in value added for each industry in a country, we find evidence that the passage of stronger dismissal laws indeed led to enhanced economic growth within the country; this effect is robust to including the Rajan and Zingales (1998) measures of financial development as well as their interactions with external financial dependence. Interestingly, among other dimensions of labor laws, we find that laws that encourage industrial action by workers in the form of strikes dampen economic growth in the country; the detrimental effect of such labor laws is almost double the salutary effect of dismissal law passages. Also, we do not find any growth effects before the passage of the dismissal laws, which mitigates concerns about the reverse causal effects of economic growth on law passage.

In our final piece of cross-country evidence, we find that at the firm-level, the passage of dismissal laws has a contemporaneous positive effect on R&D investment, leads to higher annual sales growth and (weakly) increases the firm's return on assets, confirming Hypothesis 5.

Our difference-in-difference design and the extensive controls for alternative interpretations should alleviate concerns that time-varying country-level unobserved factors may be influencing innovation. Nevertheless, we also complement our cross-country results with firm-level tests focusing on the U.S. alone by analyzing the change in dismissal laws in the form of the passage of the WARN Act in 1989. In these tests, we exploit the discontinuity introduced by the fact that the WARN Act was applicable *only to firms with 100 or more employees.* The left panel of Figure 1 illustrates our identification strategy, where we plot the before-after difference in patents (due to the passage of WARN) for firms having employees in the range [95,105] in 1987. Firms with employees in the range [95,99] form the control sample while those in the range [100,105] form the treatment group; we classify firms based on the number of employees in 1987 (two years before the law change) to avoid any endogeneity stemming from the classification itself. A break at the cutoff point of 100 employees in the before-after difference in innovation is evident in the left panel of Figure 1. In the right panel of the same figure, we perform a similar visual examination around a placebo cutoff-point of 50 employees for firms with employees in the range [45,55]; as expected, the absence of an effect on firm innovation around the placebo cutoff is easily discernible.[3]

In formal tests, we first confirm that WARN did indeed bind by studying its effects on employee layoffs. Then, we undertake tests that formalize the visual effect in the left panel of Figure 1. We find that compared to firms that were unaffected by the passage of WARN, those affected file more patents post WARN; also, they file patents that are more widely cited. According to our theoretical motivation, the positive effect of dismissal laws on innovation results from the positive effect that these laws have on employee effort. We therefore also investigate whether the passage of WARN has an effect on employee effort in innovative projects; we find supportive evidence in that both patents and citations *per employee* increase significantly after the passage of WARN for

[3]Each plot point in Figure 1 is the average before-after difference for each firm and subsumes a maximum of 16 observations for a given firm into one; thus, what may appear like an outlier in the figure is consistent behavior for certain firms over several years. Our regression discontinuity results are robust to winsorization of the innovation measures at the 1% as well as 5% levels.

the "treatment" group of firms with more than 100 employees. Apart from showing the presence of the discontinuous effect at the cut-off of 100 employees, we show in placebo tests the absence of the same at cut-offs of 50 and 150. Finally, using a difference-in-difference specification, we show that our results are obtained using the entire sample as well.

Together, the regression-discontinuity and difference-in-difference tests provide robust support of our hypotheses. In particular, the tests based on WARN enable us to shut out any unobserved heterogeneity that may affect our cross-country examinations. Furthermore, while WARN was applicable selectively to some firms but not others (based on their size), other federal laws that may have been contemporaneous did not have such a discriminatory effect.

The rest of the paper is organized as follows. Section 2 presents the theoretical motivation. Section 3 describes the cross-country empirical results. Section 4 discusses the results based on the WARN Act in the U.S. Section 5 reviews additional related literature. Section 6 concludes.

2 Theoretical motivation

We present a theoretical motivation for our primary tests using a model developed fully in the theoretical addendum. The model features an all-equity firm choosing between two projects that differ mainly in their degree of innovation. For instance, in the case of a pharmaceutical company these two projects can be thought of as inventing and launching a new drug, or manufacturing and launching a generic substitute for an existing drug. Launching a generic substitute involves uncertainties due to customer demand and competition. In contrast, inventing and launching a new drug, while resulting in higher terminal payoffs in the case of success, entails additional uncertainties associated with the process of exploration and discovery, and thus involves significantly more risk.

The firm, which is risk-neutral, hires a risk-averse employee to work on the project. The employee is particularly averse to the risk of being dismissed from employment. A key friction in the model is that contracts are incomplete in the spirit of Grossman and Hart (1986), Hart and Moore (1990), and Hart (1995). Specifically, we assume that the firm cannot commit through a contract that it will not fire its employee in those states where project failure occurs due to sheer bad luck. This inability to commit not to replace the employee stems from (i) the non-verifiability of investment and, in turn, the cause for project failure; and (ii) the fact that the firm finds it advantageous ex post to replace the original employee after the project fails.

In such a setting, dismissal laws ameliorate the lack of commitment. Even though the firm may decide to replace its original employee at an intermediate date before cash-flows from the project are realized, dismissal laws impose limits on the firm's ability to do so. Among others, the model generates the prediction that the lower threat of termination created by stronger dismissal laws acts as a commitment device for the firm to not punish the employee when the project is unsuccessful. Since innovative projects are riskier, the insurance effect stemming from this lower threat of termination matters more for the innovative project than for the routine project: The insurance effect leads the employee to increase his investment relatively more with the innovative project than with the routine project. Since an increase in the employee's investment increases the likelihood of project success, a disproportionate increase in the employee's investment in the innovative project (relative to the routine project) leads to a similar increase in the value of the project. Therefore, the firm finds innovative projects to be more value-enhancing than routine projects. Thus, more generally, stringent dismissal laws lead to more innovation, particularly in the more innovation-intensive industries.

3 Cross-country analysis

First, we describe the data, our proxies for innovation and the changes in dismissal laws. Then, we describe our empirical results.

3.1 Proxies for Innovation

Our theoretical argument implies that the passage of dismissal laws should lead to greater employee effort in innovative projects, thereby enhancing the likelihood of successful innovation that is value-enhancing to the firm. Therefore, we employ the number of patents, citations and patenting firms as proxies for innovation. While patents proxy successful innovation, the simple count of patents does not distinguish breakthrough innovations from less significant or incremental technological discoveries.[4] In contrast, citations capture the economic *importance* and drastic nature of innovation. Intuitively, the rationale behind using patent citations to identify important innovations is that if firms are willing to further invest in a project that is building upon a previous patent, it implies that the cited patent is influential and economically significant. In addition, patent

[4] Pakes and Shankerman (1984) show that the distribution of the importance of patents is extremely skewed, i.e., most of the value is concentrated in a small number of patents. Hall, Jaffe and Trajtenberg (2005) among others demonstrate that patent citations are a good measure of the value of innovations.

citations tend to arrive over time, suggesting that the importance of a patent may be revealed later in its life and may be difficult to evaluate at the time the innovation occurs. We also employ the number of patenting firms as a third proxy for successful innovation.[5]

To construct these proxies for innovation, we use data on patents filed with the U.S. Patent Office (USPTO) and the citations to these patents, compiled in the NBER Patents File (Hall, Jaffe and Trajtenberg, 2001). The NBER patent dataset provides among other items: annual information on patent assignee names, the number of patents, the number of citations received by each patent, the technology class of the patent and the year that the patent application is filed. The dataset covers all patents filed with the USPTO by firms from around 85 countries. We exploit the technological dimension of the data generated by *"patent classes"* in our cross-country tests. Over the years, the USPTO has developed a highly elaborate classification system for the technologies to which the patented inventions belong, consisting of about 400 patent classes. During the patent examination process, patents are assigned to detailed technologies as defined by the patent class. The USPTO performs these assignments with care to facilitate future searches of the prior work in a specific area of technology (Kortum and Lerner, 1999).

We date our patents according to the year in which they were applied for. This avoids anomalies that may be created due to the lag between the date of application and the date of granting of the patent (Hall, Jaffe and Trajtenberg, 2001). Note that although we use the application year as the relevant year for our analysis, the patents appear in the database only after they are granted. Hence, we use the patents actually granted (rather than the patent applications) for our analysis.

Patents have long been used as an indicator of innovative activity in both micro- and macroeconomic studies (Griliches, 1990). Although patents provide an imperfect measure of innovation, there is no other widely accepted method which can be applied to capture technological advances. Nevertheless, using patents has its drawbacks. Not all firms patent their innovations, because some inventions do not meet the patentability criteria and because the inventor might rely on secrecy or other means to protect its innovation. In addition, patents measure only successful innovations. To that extent, our results are subject to the same criticisms as previous studies that use patents to measure innovation (e.g., Griliches, 1990; Kortum and Lerner, 1999).

[5]The USPTO defines "assignee" as the entity to which a patent is assigned. A simple count of the number of assignees in a patent class in a given application year provides a measure of the number of patenting entities.

A note about the use of U.S. patents to proxy innovation done by international firms is in order. To compare innovation done by firms across countries, it is crucial to employ *patents filed in a single jurisdiction* by firms from these countries.[6] Given its status as the technological leader, the U.S. is the natural single jurisdiction of choice.[7] However, using patents filed with the USPTO introduces potential biases since it is likely that foreign firms file patents with the USPTO because they need to sell their products in the U.S. Hence, we control for such systematic biases stemming from comparative advantages and bilateral trade patterns.

3.2 Dismissal Law Changes

In order to analyze the impact of dismissal laws on innovation, we exploit the time-series variation generated by changes of these laws within countries. We use a comprehensive list of dismissal law changes from Deakin et al. (2007), who analyze in detail the evolution of employment protection legislation across five countries for each year from 1970 to 2006 and generate a labor law index.[8] The Deakin et al. index offers several advantages. First, the long time-series, which captures comprehensively all the country level changes in dismissal laws, enables us to conduct tests that alleviate econometric concerns that may otherwise be a problem in a cross-country setting.

Second, their categorization of labor laws into different components – dismissal laws being one of them – allows us to assess the impact on innovation of dismissal laws vis-à-vis other categories of labor laws. Deakin et al. analyze forty dimensions of labor and employment law and group

[6]Since enforcement of intellectual property protection may vary across jurisdictions, comparing domestic patents filed in the various countries would not accurately measure differences in ex post innovation or the ex ante incentives for innovation in these countries. In contrast, comparing patents granted in one jurisdiction alleviates such concerns of heterogeneity and provides standardization across patents in (i) the strength of patent protection; (ii) the duration of protection; (iii) the penalties for patent infringement and therefore the nature of patent enforcement; and (iv) the patenting practices followed by the jurisdiction's patent office for all firms filing in the jurisdiction.

[7]Lall (2003, p.1664) recommends using U.S. patent data "for two reasons. First, practically all innovators who seek to exploit their technology internationally take out patents in the USA, given its market size and technological strength. [...] Second, the data are readily available and can be taken to an extremely detailed level." Furthermore, the U.S. has the most advanced patenting system in the world (Kortum and Lerner, 1999) and most innovating firms internationally file patents in the U.S. (Cantwell and Hodson, 1991). Finally, U.S. patents are a high quality indicator of international technological activity (Cantwell and Anderson, 1996).

[8]The Botero et al. (2004) index presents an alternative to the Deakin et al. (2007) index that we use. Although Botero et al. (2004)'s index is constructed for 85 countries, the index is available only for the year 1997. Therefore, it is not suitable to investigate the *causal* impact of labor laws on innovation, which necessitates controlling for observable and unobservable *time-varying* heterogeneity. Another alternative is the EPL measure constructed by Nicoletti and Scarpetta (2001) for a set of OECD countries for the years 1990-1998. However, this index neither offers the cross-sectional comprehensiveness of the index constructed by Botero et al. (2004), nor the full extent of the longitudinal advantages of the index developed by Deakin et al. (2007). Furthermore, the EPL index only measures the aggregate stringency of a country's labor laws, while in this study we are interested in one particular dimension of these laws, namely dismissal rules.

them into five categories: (i) the regulation of alternative forms of labor contracting (e.g. self-employment, part-time work, and contract work); (ii) regulation of working time; (iii) regulation of dismissal; (iv) employee representation; and (v) rules governing industrial action. By averaging the sub-components for each category per country and year, Deakin et al. (2007) obtain sub-indices for the five aspects of labor and employment law (see Appendix A for details about each of the components of these five sub-indices).

Third, Deakin et al. (2007) take into account not only formal laws but also self-regulatory mechanisms, which makes their index particularly comprehensive with respect to the range of rules analyzed. For example, in certain legal systems, collective bargaining agreements – which do not constitute formal law – play a functionally similar role to formally enacted laws. Finally, the values reported in their index are complemented by a detailed country-level description of *all the law changes* in each country. Though the Deakin et al. index is available only for five countries – U.S., U.K., France, Germany and India – focusing on these five countries does not represent a substantial omission in our analysis as these five countries account for 72% of patents filed with the USPTO.

To examine the effect of laws governing dismissal of employees on innovation, we focus on the "Regulation of Dismissal" sub-index. This sub-index (hereafter "the Dismissal Law index"or "Regulation of Dismissal index") is made up of the following components: the legally mandated notice period; the amount of mandatory redundancy compensation; constraints on dismissal imposed by the law (such as dismissal being lawful only in case of misconduct or serious fault of the employee); parties to be notified in case of dismissal (this ranges from a formal communication to a state body to a simple oral statement to the employee); redundancy selection (e.g. priority rules based on seniority, marital status etc.); applicability of priority rules in re-employment; and rules governing unjust dismissal (i.e. the extent of procedural constraints on dismissal imposed by the law; whether reinstatement is the normal remedy for unfair dismissal; the period of service required for an employee to qualify for protection against unjust dismissal).

Figure 2 shows the evolution of the dismissal law index for the five countries in our sample while Figure 3 shows the variation in each of its components; in each case, higher values represent stricter laws governing dismissal. Table 1 details each dismissal law change during the time period 1970-2006; these law changes generate the variation observed in Figures 2 and 3. As an illustration, consider a few specific law changes. In France, before 1973, the employer was not required to notify an employee in case of a dismissal. In 1973, this aspect of dismissal law was strengthened by requiring the employer to provide the employee with written reasons for the dismissal. This change is reflected as an increase of 0.33 in the "Notification of Dismissal" component and a corresponding increase of 0.0367 in the "Regulation of Dismissal" index. In 1975, the law was further strengthened and the employer had to obtain the permission of a state/ local body prior to any individual dismissal; this law change results in an increase of 0.67 and 0.074 in the "Notification of Dismissal" component and "Regulation of Dismissal" index respectively. In 1986, this law was weakened; now the employer only had to notify the state/ local body prior to an individual dismissal (in contrast to requiring their permission earlier), which resulted in a decrease of 0.33 and 0.0367 in the "Notification of Dismissal" component and "Regulation of Dismissal" index respectively.

Examining Figures 2 and 3 together with Table 1 indicates that the numerous legal changes provide substantial time-series variation, which we exploit in our cross-country tests.

3.3 Summary Statistics

Panel A of Table 2 lists the summary statistics for the cross-country sample. For each of the five countries in our sample, this table lists the mean, median, standard deviation, minimum, and maximum for the number of patents filed, citations received by these patents, the number of firms filing patents, as well as the dismissal law index. Since the Deakin et al. index is available from 1970 to 2006 while the patent data ends in 2002, we terminate our cross-country sample in 2002.

3.4 Empirical Results

We investigate whether stronger dismissal laws lead to greater innovation. Inferring a causal relationship between country-level dismissal laws and innovation presents the challenge that country-level dismissal laws are expected to be largely correlated with other country-level unobserved factors. To infer this causal relationship, we utilize the fact that the dismissal law index exhibits substantial time-series variation as described above.

3.4.1 Fixed-effects panel regressions

To start with, we employ fixed-effects panel regressions of the innovation proxies on the dismissal law index, where we include fixed effects at the country, time and industry (i.e. patent class) levels:

$$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \beta \cdot X_{ict} + \varepsilon_{ict} \tag{1}$$

where y_{ict} is the natural logarithm of a measure of innovation for the USPTO patent class i from country c applied for in year t (and eventually granted). Since the application year captures better the timing of the innovation (Hall et al., 2001), we date the patent by its application year. $\beta_i, \beta_c, \beta_t$ denote patent class, country and application year fixed effects respectively. $DismissalLaws_{ct}$ denotes the stringency of dismissal laws based on the index value for country c in year t. X_{ict} denotes the set of control variables. The country fixed effects control for time-invariant unobserved factors at the country level. The application year fixed effects control for global technological shocks; further, they allow us to control for the problem stemming from the truncation of citations, i.e., citations to patents applied for in later years would on average be lower than citations to patents applied for in earlier years. Similarly, the patent class fixed effects control for average differences in technological advances across the different industries as well as time-invariant differences in patenting and citation practices across industries.

As explained by Imbens and Wooldridge (2009), in (1) , β_1 estimates the "difference-in-difference" in a generalized multiple treatment groups, multiple time periods setting. Intuitively, given the country and time fixed effects, β_1 estimates the *within-country* differences *before* and *after* the dismissal law change vis-à-vis similar before-after differences in countries that did not experience such a change during the same period (see Appendix B for a formal proof). Therefore, these tests are less subject to the criticism that country or industry level unobserved factors influencing innovation are correlated with the level of dismissal laws in a country. Since the primary variable of interest, $DismissalLaws_{ct}$, varies at the country level, we cluster standard errors by country.

Table 3, Columns 1-3, shows the results of the test of equation (1) using the logarithm of the number of patents, number of patenting firms, and number of citations to patents as the dependent variables. For each of the three dependent variables, we find the coefficient on the dismissal law index to be positive and significant. This result indicates that strong dismissal laws are positively correlated with innovation, as suggested by Hypothesis 1.

In these regressions, we also control for other variables that may affect innovation:

Creditor rights Acharya and Subramanian (2009) provide empirical evidence that when a country's bankruptcy code is creditor-friendly, excessive liquidations cause levered firms to shun innovation, whereas by promoting continuation upon failure, a debtor-friendly code induces greater innovation. Therefore, first, we control for the extent of creditor protection in a country by using the time-varying Djankov et al. (2007) index of creditor rights, available for 1970-2002.[9] We find the coefficient on creditor rights to be negative and, except in one specification, significant.

Other country-level laws Since the labor laws in a country may be correlated with its other laws, we employ the set of (by construction time-invariant) legal variables highlighted by the law and finance literature (La Porta et al., 1997, 1998): Rule of Law, Antidirector Rights Index and the Efficiency of Judicial System (all from La Porta et al., 1998). The Rule of Law and the Efficiency of the Judicial System are positively correlated with innovation while the Antidirector Rights Index appears with a negative sign in two out of the three specifications.[10]

A related concern is that the contracting and legal environments in India might be very different from other countries in our sample. Given the relatively limited number of observations from India, it is unlikely that India may be driving our results. Nevertheless, as a robustness check, we performed all the tests by excluding observations for India; the results stay almost identical.

Bilateral Trade Using U.S. patents to proxy innovation in non-U.S. countries avoids concerns of heterogeneity stemming from employing patents filed under each country's patenting system. However, this strategy introduces potential biases. Note that since we include country, patent class and application year fixed effects in our regressions, the coefficient β_1 in equation (1) would be biased only if time-varying omitted variables at the country/ patent class level that affect these biases are also correlated with changes in dismissal laws.

Nevertheless, we employ non-U.S. countries' bilateral trade with the U.S. in a given industry to account for this bias. Countries that export to the U.S. would file more patents with the

[9]Since there were no creditor rights changes in the five sample countries from 1970 to 1978 (Armour et al. 2006), we extend the Djankov et al. (2007) index from 1978 to 1970.

[10]Since these country-level law indices do not vary over time, we estimate their effect by aggregating the country fixed effects. In omitted tests, we also controlled for Logarithm of days to enforce a contract, Estimated Cost of Insolvency Proceedings, and legal origin in these regressions. These variables were dropped due to multi-collinearity.

USPTO, particularly in their export-intensive industries. MacGarvie (2006) finds that citations to a country's patents are correlated with the level of exports and imports that the country has with the U.S. Therefore, in our regressions, we add for each country the logarithm of the level of imports and the level of exports that the country has with the U.S. in each year at each 3-digit ISIC industry level, using data from Nicita and Olarreaga (2006).[11] While imports have no consistent effect, exports are negatively correlated with innovation, although this effect is only significant in some specifications.

Comparative Advantage and Economic Development A key determinant of innovation is the comparative advantage that a country possesses in its different industries, which could affect our interpretation of β_1. As our proxy for industry level comparative advantage, we employ the ratio of value added in a 3-digit ISIC industry in a particular year to the total value added by that country in that year. The data for these measures come from the United Nations Industrial Development Organization (UNIDO)'s statistics. Relatedly, since richer countries may innovate more and may also file more patents with the U.S., we also include the logarithm of real GDP per capita. We find in Columns 1-3 of Table 3 that neither the ratio of value added nor economic development have a significant effect on innovation.

Crucially, in all these specifications, we find that the overall effect of dismissal laws stays positive and significant for all three innovation proxies.

Economic magnitudes In addition to being statistically significant, the economic magnitude of the impact of dismissal laws on innovative activity is also large. Since we identify the effect of dismissal laws primarily using within-country variation, the appropriate standard deviation to use for estimating the economic magnitude is the within-country standard deviation. As seen in Table 2, the U.S. has the lowest standard deviation among countries that underwent a significant dismissal law change. Using Columns 1-3 of Table 3, we find that an increase in the dismissal law index by one standard deviation, ceteris paribus, results in a rise in the annual number of patents, number of patenting firms, and citations by 6.1%, 7.0% and 9.2% respectively.

As for the effect of specific law changes, consider, for example, the effect of the law change

[11] We match the patent classes to the 3-digit ISIC using a two-step procedure: first, the updated NBER patent dataset (patsic02.dta on Brownwyn Hall's homepage) assigns each patent to a 2-digit SIC. We then employed the concordance from 2-digit SIC to 3-digit ISIC codes. Since every patent is already assigned to a patent class in the original NBER patent dataset, this completes our match from the patent class to the 3-digit ISIC code.

relating to procedural constraints on dismissal in the U.K. in 1987. Due to a decision of the House of Lords (Polkey v. A.E. Dayton Services Ltd.) in 1987, it was less easy for employers to avoid a finding of unjust dismissal in case of a lack of due process. This law change corresponds to an increase from 0.33 to 0.67 in the 'Procedural Constraints on Dismissal' component of the dismissal law index. Since this is one of the nine components of the dismissal index, the change corresponds to an increase of 0.0378 in the dismissal law index. This law change leads to an increase in annual number of patents, number of patenting firms, and citations by 2.8%, 3.1% and 4.1% respectively.

3.4.2 Endogeneity of dismissal law changes

We now examine concerns relating to the possible endogeneity of the dismissal law changes.

Panel regressions with country-specific and industry-specific trends To examine whether other country/ industry level changes accompanying the dismissal law change account for our results, we incorporate country-specific and industry-specific time trends in our test design:

$$y_{ict} = t\beta_{j \leftarrow i} + t\beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \beta \cdot X_{ict} + \varepsilon_{ict} \tag{2}$$

where $t\beta_{j \leftarrow i}$ denotes a time trend for the industry (patent category[12]) j to which patent class i belongs; $t\beta_c$ denotes a time trend for country c; the other variables are as defined in equation (1). By accounting for these country-specific and industry-specific time trends, we identify the intended effect using deviations (at the patent class level) from the average time trend for each country and that for each industry. Since other country or industry level changes accompanying the dismissal law changes could lead to country-specific as well as industry-specific time trends, these tests enable us to isolate better the *pure* effect of dismissal law changes on innovation. The results of these tests are shown in Columns 4–6 of Table 3. After accounting for country and industry-specific time trends, the coefficient of the dismissal law index remains positive and significant. Comparing the coefficients in Columns 4–6 to columns 1–3 suggests that controlling for country- and industry-specific trends even strengthens the effect of dismissal laws.

Correlation of dismissal law changes with changes in government An important concern stems from the fact that changes in a country's labor laws are likely to be correlated with changes in

[12] A patent category encompasses several patent classes. There are six patent categories.

elected governments in a country. In particular, to cater to their political constituencies, more left-leaning governments may be inclined to strengthen labor laws. Botero et al. (2004) find evidence that labor market regulation is often driven by political considerations: countries with a longer history of leftist governments have more stringent labor regulation. Deakin et al. (2007) also document that the primary motivation for labor market (de)regulation is political. They find that a rapid decline in the intensity of labor market regulation in the U.K. coincided with the election of a Conservative government committed to a policy of labor market deregulation. Similarly, a limited revival of regulation of the labor markets in the U.K. coincided with the return to office in 1997 of a Labor government which ended U.K.'s opting out of the EU Social Charter. Furthermore, they find that in France, the election of the socialist government in 1981 led to a series of labor law reforms – the 'Auroux laws'. These laws, which were enacted in 1982, affected a wide range of aspects in both individual and collective labor law. Since that time, French labor law has tracked the changing political fortunes of the main parties.

If leftist governments are more likely to invest in education and other public services, which may have a positive impact on innovation in a country, it is possible that the effect of dismissal laws on innovation documented above is, in fact, caused by other factors coinciding with changes in government rather than changes in dismissal laws. We examine this concern by using a time-varying proxy for the political leanings of a country's government. We use the variable *Government* from Armingeon et al. (2008), which captures the balance of power between left and right-leaning parties in a given country's parliament.[13] This variable takes on values from one to five, with one denoting a hegemony of right-wing (and center) parties, and five denoting a hegemony of social-democratic and other left parties. The variable Government is available for all countries in our sample, except for India. As expected, it is strongly positively correlated with the dismissal law index (the correlation is 0.52), which implies that stricter dismissal laws are indeed enacted in a country when the government is leftist in its political leanings.

Columns 1-3 of Table 4 show the result of including Government as an additional control variable to the basic tests described in Equation (1). We find that the coefficient of Government is positive and statistically significant in two out of three specifications. Thus, *within* a country, innovation is

[13] Armingeon et al. (2008) construct a Comparative Political Data Set, which is a collection of annual political and institutional data for 23 democratic countries for the period of 1960 to 2006. Our variable *Government* is denoted "govparty" in Armingeon et al. (2008).

greater under more left-leaning governments, possibly because leftist governments may emphasize investments in education and other basic public services, which may in turn be positively correlated with innovation.

Crucially, however, we observe that the coefficient on the dismissal law index remains positive and significant (at the 5% level or above) for all three innovation proxies. Comparing the coefficient of dismissal laws in Columns 1–3 of Table 4 to Columns 1–3 of Table 3 (note that columns 4 to 6 of Table 3 do not provide the appropriate comparison since they include country- and industry-specific trends) shows that accounting for the endogenous law changes (due to changes in government) does not materially affect the economic magnitude of the effect of dismissal laws. In fact, the coefficients in Columns 1 to 3 of Table 4 are uniformly greater than those in Columns 1 to 3 of Table 3. In unreported tests, we examine whether the coefficient on dismissal laws changes materially upon adding Government as an additional control variable by including the interaction of dismissal laws with the Government variable. We find that for each of the three innovation proxies, the coefficient of the interaction is statistically indistinguishable from zero (with the p-values being 23%, 35% and 14% respectively for patents, firms and citations). This implies that once we control for the political leanings of a country's government, unobserved factors that coincide with the dismissal law changes appear to be uncorrelated with innovation.

Thus, we conclude that our results are not affected by possible endogeneity stemming from (i) other country/ industry level confounding factors that coincided with the dismissal law changes or (ii) specifically, the political considerations that may have driven the law changes.

Other robustness checks In Table 4 we also address two additional concerns. Is it the case that our results are driven by the dismissal law change in the U.S.? Second, are the results driven by a possible increase in German patenting activity owing to the re-unification of East and West Germany in 1990? To examine these alternative stories, we restrict our sample to the ten-year period from 1993 to 2002. The three year time lag after the German re-unification and the four year time lag after the U.S. WARN Act became effective ensure that the effect of either event is minimal during this sample period. Columns 4-6 of Table 4 provide evidence that identification in our tests of Equation (1) does not rely on the 1988 WARN Act alone or on the effect of the German re-unification.

3.4.3 Traditional difference-in-difference tests

Given the five countries in our analysis, a pertinent question is whether the overall effects of labor laws hold in the time-series for *each* of the five countries. However, in country-specific regressions, we would not be able to control for general macroeconomic factors and technological shocks through year fixed effects since the year dummies soak up all the variation in the index for a country. This would represent a severe omission since technological shocks have historically arrived at common times in different countries (Kortum and Lerner, 1999). Given the importance of such global technological shocks, we cannot draw any meaningful inference from such country-by-country regressions.

Instead, we use *each* country which underwent a significant dismissal law change to undertake traditional difference-in-difference tests, where we examine the before-after effect of a change in dismissal laws in the affected country (the "treatment group") vis-à-vis the before-after effect in a country where such a change was not effected (the "control group"). By including another country as a control group, these difference-in-difference tests largely neutralize the effect of global technology shocks. Examining Figure 2 makes it clear that laws affecting dismissal underwent changes primarily in three different instances: in the U.K. and France in the early 1970s and in the U.S. in 1989.[14] We therefore examine the effect of each of these three changes. Figure 4 (a) illustrates the difference-in-difference for the change in laws governing dismissal in the U.S. in 1989 with Germany as the control group since Germany did not undergo any dismissal law changes during this period. In this figure, we plot across time the ratio of realized number of patents and citations in a particular year to that in 1989 – the year of the U.S. dismissal law change. We find that while the number of patents and citations are relatively in sync for U.S. and Germany until 1989, post 1989, these measures for the U.S. break ahead of those for Germany. Figure 4 (b) further depicts this break for the U.S. by plotting a linear fit of the number of patents and citations across time for U.S. and Germany before and after the law change.

The econometric variant of this visual test is identical to that in Equation (1), except that we restrict the sample to a treatment and a control country:

[14] India was the only other country which had significant changes in dismissal laws during our sample period. However, given the small number of observations for India, we cannot undertake such tests for India.

$$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \varepsilon_{ict} \quad (3)$$

Note that $DismissalLaws_{ct}$ is constant for the "control" group. As shown in Appendix B, given the country and year dummies, the coefficient β_1 estimates the difference-in-difference.

Notice that compared to the usual difference-in-difference specification, which contains dummies for treatment groups and treatment periods only, including dummies for all the application years as well as the patent classes leads to a much stronger test since we are able to control for time-invariant country and patent class specific determinants of innovation as well as time-varying effects that are common to all countries and all patent classes. As in Equation (1), the application year fixed effects enable us to also control for the problem stemming from the truncation of citations. Similarly, the patent class fixed effects allow us to control for average differences in technological advances as well as time-invariant differences in patenting and citation practices across industries.

Table 5 shows the results of these tests. In the first test, we examine the impact of dismissal law changes in the U.K. in the early 1970s; the "control group" is the U.S., which did not experience such a law change in that time interval (see Figure 5 (a)). Columns 1-3 of Panel A of Table 5 report the results from this test. In the second set of tests, we investigate the impact of dismissal law changes in France in the early 1970s; the "control group" is again the U.S. (see Figure 5 (b)). Results are reported in Columns 4-6 of Panel A of Table 5. We infer from both these tests that the coefficient β_1, which captures the causal effect of the dismissal law changes, is positive and significant (at the 1% level) for all specifications.

Next, we exploit the dismissal law change in the U.S. in 1989 where the "control group" is Germany, which did not experience such a major law change in the sample period (see Figure 5 (c)). Columns 1-3 in Panel B of Table 5 show that β_1 is positive and significant, which corroborates the hypothesis that tougher dismissal laws have a favorable impact on innovation.

Overall, the evidence presented in Table 5 lends strong support to the hypothesis that tougher dismissal laws lead ex ante to greater innovation. The economic effects of these law changes are substantial. In the U.S., for example, the dismissal index increased from 0 to 0.167 in 1989. The quantitative effect of this strengthening in employment protection was an increase in the number of patents by 15.3%. The effect is similar in the case of the other two innovation proxies.

Discussion These two-country difference-in-difference tests have several attractive features. First, apart from providing evidence using specific "natural experiments," these tests also have the advantage of easier interpretation due to the existence of specific treatment and control groups.

Second, the difference-in-difference tests address concerns that the results obtained in Section 3.4.1 are a spurious combination of (i) a general trend of labor laws, in particular laws governing dismissal, becoming stricter over time; and (ii) a rising trend in USPTO patent applications (and grants) since the year 1985 (see, for example, Kortum and Lerner, 1999). As seen in Columns 1-6 of Panel A in Table 5, the difference-in-difference tests for U.K.-vs-U.S. and France-vs-U.S. employ samples until 1978 and 1985 respectively. Given these time periods, the sample excludes years containing the rising trend in USPTO patent applications.

Finally, by examining the effect of changes in one particular law in one particular country, the difference-in-difference tests provide point estimates of the effect of specific changes in labor laws on innovation using experiments of greatest relevance to policies concerned with promoting innovation.

3.4.4 Causality or reverse causality?

It is important to further examine the direction of causality from dismissal laws to innovation. As we discussed in Section 3.4.2, political factors were a key determinant for the dismissal law changes in the countries in our sample. Since these political reasons were largely orthogonal to the objective of promoting country-level innovation, our evidence above can be interpreted truly as a causal effect of the dismissal law change on innovation. Nevertheless, by examining the dynamic aspects of the effect of the law change, we investigate reverse causality in our tests below. For example, was it the case that the dismissal law changes were effected to provide an extra boost to innovation already occurring due to some other changes in the economy? In this case, we might see an "effect" of the change even prior to the change itself. Also, did the dismissal law changes occur due to lobbying by innovative industries in these countries (in order to gain a further competitive advantage over their international competitors)? Since lobbying firms would try to gain a competitive advantage by anticipating the change and responding to them in advance, in this case as well, we might see an "effect" of the change even prior to the change itself.

To examine such possibilities of reverse causality, we use the dismissal law change in the U.S. in 1989. As this change occurred at a point in time, it is ideal to address such a concern. We follow

Bertrand and Mullainathan (2003) in decomposing the change in dismissal laws into three separate time periods: (i) Dismissal Law Change (-2,0), which captures any effects from two years before to the year of the change; (ii) Dismissal Law Change (1,2), which captures the effects in the year after the change and two years after the change; and (iii) Dismissal Law Change ($\geq$3), which captures the effect three years after the change and beyond.

Columns 4-6 of Panel B in Table 5 show the results of these regressions. A positive and significant coefficient on Dismissal Law Change (-2,0) would be symptomatic of reverse causation. However, we find that the coefficient is *negative* and statistically significant in Columns 4 and 5, and it is statistically insignificant in Column 6. In contrast, the coefficients of Dismissal Law Change (1,2) and Dismissal Law Change ($\geq$3) in Columns 4-6 show that while the dismissal law change has a *positive* effect on the innovation proxies in the first two years, the effect of the law change lasts three years and beyond; in fact, this "long-run" effect is larger than the effect in the first two years. The effect in the first two years of the law change is consistent with evidence in Kondo (1999) that there is about a one-and-a-half year lag between patent applications and R&D investment. Furthermore, the long gestation periods involved with innovative projects also contribute to the effect of the dismissal law change being economically larger for the period after three years.

3.4.5 Inter-industry differences based on Innovation Intensity

We now examine our Hypothesis 2 that the effect of dismissal laws should be disproportionately stronger in industries that exhibit a greater propensity to innovate than in other industries. To provide intuition for the design of this test, consider two industries: "surgical and medical instruments" and "textiles". Firms in surgical and medical instruments have a higher propensity to innovate and have riskier cash flows than firms in the textile industry. Therefore, surgical and medical instruments serves as an example of a more-innovative industry while textiles serves as a benchmark less-innovative industry. Hypothesis 2 predicts that the effect on innovation of the U.S. dismissal law change in 1989 would be disproportionately higher in surgical and medical instruments when compared with that in textiles. Figure 6 illustrates this interaction effect. In this figure, we plot across time the ratio of realized number of patents and citations for surgical and medical instruments relative to textiles and apparel for the U.S. vis-à-vis Germany. To examine the effect of the U.S. law change in 1989, we normalize this ratio to be one in 1989. We find that while the ratios for the U.S. and Germany overlap with each other until 1990, after 1990, the ratio for the U.S. surges ahead of that for Germany.

In the econometric variant of this visual test, we investigate the effect of the interaction of the dismissal law index with a proxy for the innovation intensity of an industry:

$$\begin{aligned} y_{ict} &= \beta_i + \beta_c + \beta_t + \beta_1 \cdot (DismissalLaws_{ct} * InnovationIntensity_{i,t-1}) \\ &\quad + \beta_2 \cdot DismissalLaws_{ct} + \beta_3 \cdot InnovationIntensity_{i,t-1} + \beta X_{ict} + \varepsilon_{ict} \ , \end{aligned} \tag{4}$$

where $InnovationIntensity_{i,t-1}$ denotes the Innovation Intensity for patent class i in year $(t-1)$. We follow Acharya and Subramanian (2009) in measuring $InnovationIntensity_{i,t-1}$ as the median number of patents applied for by U.S. firms in patent class i in year $(t-1)$. Since the proxy for Innovation Intensity is time-varying, it captures the inter-temporal changes in the propensity to innovate caused by technological shocks. Note that the interaction term $(DismissalLaws_{ct} * InnovationIntensity_{i,t-1})$ varies at the level of patent class i in country c in application year t. Since our dependent variable, y_{ict}, exhibits equivalent variability, the coefficient β_1 is well-identified and measures the relative effect of dismissal laws across industries that vary in their innovation intensity. Note further that despite the country fixed effects, the coefficient on dismissal laws (β_2) is identified too since the dismissal law index exhibits variation across time. Similarly, innovation intensity exhibits time variation as well, and therefore its coefficient (β_3) can be identified despite the presence of patent class fixed effects.

The principal coefficient of interest is that of the interaction between country level dismissal laws and industry (i.e. patent class) level patenting intensity – β_1. Hypothesis 2 predicts that $\beta_1 > 0$. As the variable $InnovationIntensity$ is constructed using U.S. patents, we avoid mechanical correlation between this variable and our dependent variables by using only the number of patenting firms and the number of citations as innovation proxies.

The results of the basic tests are reported in Columns 1-2 of Table 6. As in our previous tests, we control for other determinants of innovation in Columns 3-4. Across these specifications, we find that the coefficient of the interaction term stays positive, as well as statistically and economically significant, indicating that the positive impact of dismissal laws on innovation is more pronounced in innovation intensive industries.

3.4.6 Effect of Other Dimensions of Labor Laws

Next, we test our Hypothesis 3 that labor laws that affect the ex post likelihood of an employee being dismissed from employment matter more for innovation than other categories of labor laws. For this purpose, we run the following regression:

$$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 * lA_{ct} + \beta_2 * lB_{ct} + \beta_3 * lC_{ct} + \beta_4 * lD_{ct} + \beta_5 * lE_{ct} + \beta X_{ict} + \varepsilon_{ict} \quad (5)$$

where β_1 - β_5 measure the impact on innovation of the five components of the Deakin et al. (2007) labor law index: Alternative employment contracts (lA_{ct}), Regulation of working time (lB_{ct}), Regulation of dismissal (lC_{ct}) – our "dismissal law index", Employee representation (lD_{ct}), and Industrial action (lE_{ct}).[15]

Columns 1-3 of Table 7 present results of these tests; the only dimension of labor laws which has a consistently positive and significant impact on innovation is the "regulation of dismissal" component.

3.4.7 Placebo tests using innovation by individuals as "control group"

As a final robustness check, we undertake placebo tests by employing innovation done by individuals as a control group, since our theoretical argument implies that labor laws should only have an effect on the innovation done by employees, not by individuals. For this purpose, we run the following regression:

$$y_{ict,\text{ individuals}} = \beta_i + \beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \varepsilon_{ict} \quad (6)$$

where $y_{ict,\text{ individuals}}$ denotes the innovation done by individuals in patent class i, country c and application year t (corresponding to assignee codes 4 and 5 which denote U.S. individuals and non-U.S. individuals respectively). The coefficient β_1 therefore measures the difference-in-difference effect of changes in dismissal laws for the innovation done by individuals. Columns 4-5 of Table 7 present results of these tests using the number of patents and the number of citations as innovation proxies. For both these variables, we find no effect on innovation done by individuals, which reinforces the fact that dismissal laws only matter for innovation incentives inside firms.

[15]Note that while the correlation between different labor law components is positive and significant, the tests do not encounter any multi-collinearity problem.

In Columns 6-7, we verify the effect of dismissal laws on innovation done by firms using triple-difference tests as follows:

$$y_{ict,\text{ firms}} - y_{ict,\text{ individuals}} = \beta_i + \beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \varepsilon_{ict} \quad (7)$$

We find the effect of dismissal laws on innovation by firms vis-à-vis individuals to be positive and statistically as well as economically significant.

These placebo and triple-difference tests enable us to control for any extraneous country-level omitted factors that may have coincided with the passage of dismissal laws. For example, in Section 3.4.2, we had investigated if the endogenous correlation of dismissal law passages with leftist governments drove our results. These tests provide further evidence confirming that such endogeneity does not account for our findings. Since greater investments in education and other public services by left-leaning governments should manifest as an increase in innovation by individuals as well, these tests enable us to control for such endogenous factors. In general, any endogenous country-level variable that affects the passage of dismissal laws and affects innovation performed by all agents in the economy should be accounted for in the above tests.

In sum, we can conclude with a reasonable degree of certainty that country level changes in dismissal laws did indeed foster innovation by firms in that country.

3.4.8 Dismissal laws and country-level economic growth

As our next cross-country inquiry, we ask how dismissal laws affect country-level growth rates. Since innovation is essential to sustain high levels of growth in an economy (see the pioneering work on endogenous growth theory of Romer, 1990; Grossman and Helpman, 1991; and Aghion and Howitt, 1992), examining whether the increased innovation stemming from the passage of dismissal laws eventually leads to higher country-level economic growth can shed crucial light on the value implications of dismissal laws at the country level.

We follow Rajan and Zingales (1998) in employing as our dependent variable the growth rate in real value added over the period 1970-2002 for each ISIC (manufacturing) industry in a country (obtained from the UNIDO Industrial Statistics database). Since other dimensions of labor laws could matter for country-level economic growth, we run the following regression:

$$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \beta_2 * lA_{ct} + \beta_3 * lB_{ct} + \beta_4 * lD_{ct} + \beta_5 * lE_{ct} + \beta X_{ict} + \varepsilon_{ict} \quad (8)$$

where i now denotes the 3-digit ISIC industry and y_{ict} denotes the continuously compounded growth rate in value added in industry i in country c in year t. β_1 remains the coefficient of interest since it captures the difference-in-difference effect of the passage of dismissal laws on country-level economic growth. The other dimensions of labor laws are: Alternative employment contracts (lA_{ct}), Regulation of working time (lB_{ct}), Employee representation (lD_{ct}), and Industrial action (lE_{ct}).[16] In these regressions, the robust standard errors are clustered by country.

Table 8 reports the results of these regressions. In Column 1, we include as control variables the creditor rights index, log of imports and exports by the country to the U.S. in the given 3-digit ISIC, the log of GDP per capita as well as the proxy for comparative advantage of a 3-digit ISIC industry in a country and year (measured as the ratio of value added in a 3-digit ISIC, country, year to the total value added in that country in that year). Since we had included these variables as controls in our tests on innovation earlier, their motivation remains the same as before. We find that the passage of dismissal laws within a country has a positive and statistically significant effect on economic growth in that country. In addition to being statistically significant, the economic magnitude of the impact of dismissal laws on economic growth is also large. We find that, ceteris paribus, a one-standard deviation increase in the dismissal law index results in an approximately 2.2% increase in the growth in value added in a typical industry.

Among the other dimensions of labor laws, we find that laws relating to industrial action (e.g., the ability to organize strikes) have a strong negative effect on economic growth. By comparing the coefficients, we can infer that the detrimental effect of enacting laws that enable industrial action by workers is approximately *double* the beneficial effect of dismissal laws on economic growth.

In Column 2, we replace the separate country and industry fixed effects with fixed effects for each 3-digit ISIC industry in each country (country * industry fixed effects) to reassure ourselves that the results stay unchanged even with this very robust specification that controls for all time-invariant unobserved factors at the level of each industry in each country. In Column 3, we include our proxy

[16] Note that while the correlation between different labor law components is positive and significant, the tests do not encounter any multi-collinearity problem.

for the political leanings of the country's government to account for any possible endogeneity in law changes due to political reasons. We find that while the effect of dismissal laws stays unaffected (the economic effect marginally increases), the negative effect of industrial action laws now disappears. This seems to suggest that the possible political-economic factors that lead left-leaning governments to enact laws favoring industrial action could also be the ones that affect country-level economic growth negatively. From the perspective of this study, however, this test reassures once more that endogeneity of dismissal law changes (due to changes in government) is unlikely to be accounting for our results on innovation or economic growth.

In Column 4, we examine whether the effect of dismissal laws on economic growth is disproportionately greater in more innovation intensive industries. For this purpose, we interact dismissal laws with the proxies for innovation intensity discussed earlier. Since Acharya and Subramanian (2009) find that weaker creditor rights lead to economic growth in particularly the more innovative industries, we also include the interaction of the creditor rights index with the proxy for innovation intensity. Furthermore, as in Rajan and Zingales (1998), we interact the industry's external financial dependence with a measure of the country's financial development (accounting standards); we also include its interaction with innovation intensity. We test whether the coefficient of the interaction between dismissal laws and innovation intensity accounts for growth over and above these effects. We find that the coefficient of the interaction is positive and statistically significant, although its economic magnitude is relatively small, possibly because we are examining this relative effect within manufacturing industries only (owing to data limitations).

In Columns 5 and 6 we examine the dynamic effect of the passage of dismissal laws on country-level economic growth. In Column 5, we include $DismissalLaws_{ct}$ and $DismissalLaws_{c,t+1}$ to decompose the total effect of dismissal laws into any potential effects *before* the passage of dismissal laws and the effect *after* their passage. Since the coefficient of $DismissalLaws_{c,t+1}$ captures the correlation between growth in time t and dismissal law changes in time $t+1$, it tests for any growth effects before the law change itself. As seen in the coefficient of $DismissalLaws_{c,t+1}$, there appears no evidence of such reverse causality driving the effects on economic growth. Furthermore, the posited positive effect on economic growth after the passage of dismissal laws is robustly evident.

In Column 6, we examine the short and long run effects of the passage of dismissal laws on economic growth. For this purpose, we decompose the aggregate effects into: (i) an effect before the passage of the dismissal laws as captured by the coefficient of $DismissalLaws_{c,t+1}$; (ii) an effect from year of passage to one year after passage of the dismissal laws, as captured by the coefficient of $DismissalLaws_{c,t}$; and (iii) the effect two years and after, as captured by the coefficient of $DismissalLaws_{c,t-2}$. We find in Column 6 that when we break the total effect into prior, contemporaneous and long-run effects, the positive effect of dismissal laws on economic growth is largely felt in the long-run.

In sum, we conclude that innovation fostered by stringent dismissal laws manifests as enhancements in economic value at the country level in the form of accelerated country- (and industry-) level economic growth.

3.4.9 Dismissal laws and firm-level outcomes

As our final cross-country inquiry, we examine whether the effect of dismissal laws on innovation translates into concomitant effects on firm-level outcomes. The sample for our firm-level analysis in this section includes the firm-level information provided in Compustat Global. Since coverage by Compustat Global starts in 1987 and the Deakin et al. (2007) labor law index ends in 2005, our sample extends from 1987 to 2005. The identification of the effect of dismissal laws on various firm level outcomes comes from the *post-1987* changes in dismissal laws that occurred in the U.S., Germany and U.K.[17]

Table 9 shows the results of the tests of

$$y_{ict} = \beta_i + \beta_t + \beta_1 * DismissalLaws_{ct} + \varepsilon_{ict} \tag{9}$$

where y_{ict} measures a firm-level outcome for firm i that operates in country c in time t. In these tests, the country-level variables that we had included in our previous tests as well as firm-level variables such as asset tangibility, firm size, market-to-book value of assets and firm leverage are used as control variables. In these regressions as well, we estimate standard errors that are clustered by the country of the firm.

[17] The dismissal law change in the U.S. after 1987 occurred due to the passage of the WARN Act in 1989. In U.K., post 1987, the minimum period of service required to qualify for normal case of unjust dismissal was decreased from 2 years to 1 year in 1999. In Germany, there were three different legal changes in dismissal laws post 1987. First, the legally mandated notice period for all dismissals was increased in 1993 to one month. In 2000, the dismissal law was changed from no procedural hurdles being imposed on dismissal to one where dismissal has to be in writing. In 1997, the law relating to priority in re-employment was changed to allow precedence to previously fired employees in re-employment.

R&D investment First, we examine the effect of the passage of dismissal laws on R&D investment. Our theoretical argument shows that the increase in innovative effort by each individual employee due to the passage of dismissal laws makes innovation value-enhancing to the firm. Thus, the passage of dismissal laws should incentivize the firm's management to choose innovative projects over routine ones, which should manifest as a positive effect on firm-level R&D investment. Column 1 shows a positive and statistically significant effect of the passage of dismissal laws on R&D investment. In terms of economic magnitude, a one standard deviation change in the dismissal law index leads to a 1.3% increase in firm-level R&D investment. In Column 2, we examine whether the effect of dismissal laws on R&D investment manifests before the law change itself by including $DismissalLaws_{ct}$ and $DismissalLaws_{c,t+1}$ as in Section 3.4.8 above. We find that the coefficient of $DismissalLaws_{c,t+1}$ is statistically indistinguishable from zero, which implies there was plausibly no effect on R&D investment prior to the passage of dismissal laws, allowing us to rule out reverse causality. However, the effect after the passage of dismissal laws is positive and robust as seen in the coefficient of $DismissalLaws_{ct}$.

In Column 3, we include $DismissalLaws_{c,t}$ and $DismissalLaws_{c,t-1}$ to examine any long run effect on R&D investment. However, we find only a contemporaneous effect of the passage of dismissal laws on R&D investment, as seen in the positive and significant coefficient of $DismissalLaws_{c,t}$ and the lack of the same for $DismissalLaws_{c,t-1}$. This contemporaneous effect on R&D investment, which is an input to innovation, contrasts to the long-run effects on the outputs of innovation such as patents, citations as well as country-level economic growth.

Firm performance Columns 4 and 5 present the results of tests that examine the effect of dismissal laws on firm performance as measured by yearly sales growth as well as the return on assets (as measured by EBITDA/ total assets). To avoid problems stemming from questions about the appropriate asset pricing model, we focus on these accounting measures of firm performance. As seen in Table 9, dismissal laws have a strong, positive and significant effect on sales growth. The impact on profitability is positive, but statistically not significant.

4 Within-country evidence using the WARN Act

In the previous sections, we provided evidence of the impact of labor laws on innovation in a cross-country setting. In this section, we present tests of our main hypothesis based on U.S. data alone. These tests exploit a *discontinuity* introduced by the passage of the federal U.S. WARN Act and do not rely on labor law index data. Our tests in this section are aimed at removing any concerns about: (i) time-varying country-level unobserved factors driving our results thus far; and (ii) potential measurement error arising from the use of U.S. patents to proxy innovation by international firms.

4.1 An Overview of the WARN Act

The WARN Act is a federal law that was enacted by the U.S. Congress on August 4, 1988, and became effective on February 4, 1989.[18] The WARN Act requires employers to give written notice 60 days before the date of a mass layoff or plant closing to: (i) affected workers; (ii) chief elected official of the local government where the employment site is located; and (iii) the State Rapid Response Dislocated Worker Unit. Subject to the law are private employers with 100 or more full-time employees, or with 100 or more employees who work at least a combined 4,000 hours a week. Only layoffs classified as "mass layoffs" or "plant closings," or layoffs of 500 or more full-time workers at a single site of employment, are covered.[19] In the case of non-compliance, employees, their representatives, and units of local government can bring individual or class action suits in federal district courts against employers. Employers who violate the WARN Act are liable for damages in the form of back pay and benefits to affected employees.

The requirement of prior notification to local government together with penalties for non-compliance imply that the WARN passage increases the hurdles faced by employers when dismissing employees. This effect is in line with the effect of dismissal laws as discussed in our theoretical motivation. Therefore, we expect WARN to have the predicted positive effect on innovation.

To show the diversity of companies affected by the WARN Act, we obtained WARN Act notices received by the Employment Development Department in California in 2009. These included the following companies: Adobe Systems Incorporated; American Airlines, Inc.; AT&T company; Circuit City Stores, Inc.; Comcast Cable; FOX Interactive Media, Inc.; Genentech, Inc.; Henkel

[18]The details on the WARN Act reported in this section are drawn from the following two sources, unless otherwise noted: United States Department of Labor – Employment & Training Administration (*http://www.doleta.gov/layoff/warn.cfm*); and Levine (2007).

[19]A "plant closing" is defined as a closure of a facility within a single site of employment involving layoffs of at least 50 full-time workers. In the case of a "mass layoff," an employer lays off either between 50 and 499 full-time workers at a single site of employment, or 33% of the number of full-time workers at a single site of employment. For further details, see Levine (2007).

Corporation; Hilton Hotels Corporation; HSBC; JPMorgan Chase & Co.; National Semiconductor Corporation; NEC Electronics America, Inc.; Palm, Inc.; San Francisco Chronicle; SAP America, Inc.; Seagate Technology LLC; Siemens; Stanford University; Sun Microsystems, Inc.; Symantec; The Boeing Company; The McGraw-Hill Companies; Valeant Pharmaceuticals International; Virgin Mobile USA; Walt Disney World Co.; Yahoo! Inc.; and many others.[20] Clearly, this list encompasses a broad range of firms including the very innovative ones.

The range of firms issuing WARN Act notices illustrates the fact that dismissal presents a distinct threat to researchers. As an example of this threat, consider the following passage from a January 2009 Wall Street Journal article:[21]

> "Pfizer Inc. is laying off as many as 800 researchers in a tacit admission that its laboratories have failed to live up to the tens of billions of dollars it has poured into them in recent years. [...] While the new cuts will only dent Pfizer's overall work force of 83,400, they strike at the company's lifeblood: the labs charged with discovering lucrative new drugs."

After discussing our data and test methodology, we will provide evidence of the importance of the WARN Act by showing the impact of its passage on employment fluctuations, before documenting its impact on innovation.

4.2 Data and Sample

In order to examine the effect of the passage of the WARN Act on innovation, we match the NBER patents file to Compustat data. Each assignee in the NBER dataset is given a unique and time-invariant identifier. We match the U.S. assignee names in the NBER patent dataset to the names of divisions and subsidiaries belonging to a corporate family from the Directory of Corporate Affiliations. We then match the name of the corporate parent to Compustat. Additionally, we augment our match of the U.S. assignee names to the Compustat parent with the recent gvkey-assignee match developed by NBER.[22] As before, to construct proxies for innovation, we employ patents filed with the USPTO and citations to these patents, compiled in the NBER Patents File

[20] Source: *http://www.edd.ca.gov/Jobs_and_Training/warn/eddwarnlwia09.pdf*

[21] "Corporate News: Pfizer Plans Layoffs in Research – Drug Maker Has Little in Pipeline to Show for Its $7.5 Billion R&D Budget," *The Wall Street Journal*, 14 January 2009.

[22] See https://sites.google.com/site/patentdataproject/Home/downloads for the details about this new match.

(Hall, Jaffe and Trajtenberg, 2001). The summary statistics for the main variables used in these firm-level tests are displayed in Panel C of Table 2.

4.3 Regression-discontinuity tests

In these U.S. based tests, we exploit the *discontinuity* introduced by the fact that the WARN Act was applicable *only to firms with 100 or more employees*. Our identification strategy in these tests is based on comparing U.S. firms that were affected by the law change (firms with 100 or more employees) to U.S. firms that were not (firms with less than 100 employees). To fully exploit the discontinuity due to the WARN Act and thereby provide the most robust evidence in support of our hypotheses, we focus on the firms in the range of [95,105] employees.[23] As placebo tests, we also test for any spurious effects on innovation by using cutoffs of 50 and 150 employees and a sample of firms with employees in the range [45, 55] and [145, 155] respectively.

4.3.1 WARN Act and Innovation

As discussed in Section 1, the left panel of Figure 1 shows the break in the before-after difference in innovation due to the passage of the WARN Act at the cutoff point of 100 employees. To undertake tests that formalize this visual effect, we use the following specification:

$$y_{it} = \beta_i + \beta_t + \beta_1 * (Over100)_{i,1987} * (After1988)_t + \epsilon_{it} \tag{10}$$

where y_{it} is a proxy for innovation by firm i in year t.[24] The sample covers twelve years around the passage of the WARN Act (from 1983-1994).

$(Over100)_{i,1987}$ is a dummy taking the value of one if a firm has $\geq$ 100 employees in the year *1987*, i.e., two years before the passage of the WARN Act, and 0 otherwise. As explained above, we focus on the firms where $95 \leq (Over100)_{i,1987} \leq 105$. We use employment information from the year 1987 only to avoid any endogeneity stemming from group classification due to the layoffs themselves. This is a good instrument for the following reasons. First, it is unlikely that the WARN Act had an impact on employment two years prior to its passage. Second, whether a firm had more than 100 employees in 1987 is a good predictor for the other years (including after 1987) as well.

[23] Since the number of firms in the [99,101] range is very limited, we employ the expanded range [95,105].

[24] The regression-discontinuity results are robust to winsorization of the innovation measures at the 1% as well as 5% level.

$(After1988)_t$ is a dummy taking the value of one after the passage of the WARN Act (i.e., for the years 1989-1994). The firm dummies (β_i) control for any residual time-invariant heterogeneity of firms and the year dummies (β_t) control for general macro-economic factors. In all the regressions, we cluster standard errors at the firm level.

4.3.2 WARN Act and Employee Layoffs

When examining the effect of the WARN Act on innovation, a key question that arises is whether the WARN Act indeed binds for innovative firms. To answer this question, we formally test whether the passage of the WARN Act had a significant impact on employee layoffs in the affected firms. We define employee layoffs to have occurred in firm i in year t if the number of employees in that year are lower than those in the previous year. We then estimate the following linear probability model for the twelve years surrounding the passage of the WARN Act (1983-1994):

$$Ind(Emp_{i,t} - Emp_{i,t-1} < 0) = \beta_t + \beta_1 \cdot (Over100)_{i,1987} * (After1988)_t + \beta_2 \cdot (Over100)_{i,1987} + \epsilon_{it} \quad (11)$$

where $Ind(Emp_{i,t} - Emp_{i,t-1} < 0)$ is a binary variable taking on a value of one in case of a net employment reduction in firm i from year $t-1$ to year t.

Since employee layoffs due to the WARN Act do not exhibit much within-firm variation, we do not include firm fixed effects. However, to control for average differences in employee layoffs across years, we include the year fixed effects (β_t).

4.3.3 Regression-discontinuity Results

We now discuss the results of our regression-discontinuity tests investigating the impact of WARN on employment fluctuations and innovation.

Column 1 in Panel A of Table 10 reports the results of the tests of (11) for firms having employees in the range [95,105] in 1987. We find that the passage of WARN *decreased* the likelihood of layoffs in the affected firms. Compared to the control firms in the range [95,99], the before-after difference in the likelihood of employee layoffs decreased by 33% for the treated firms in the range [100,105].

Columns 2-5 in Panel A of Table 10 show the results for the effect of WARN on innovation; again we use only firms that have employees in the range [95,105] in 1987. In Columns 2-3 of Panel A, we report the results of tests examining the effect on overall firm-level innovation by using the log of the number of patents and citations respectively as dependent variables. In line with Hypothesis 1, we find that the strengthening of dismissal laws via the WARN Act had a positive and significant impact on U.S. firm-level innovation. The economic magnitude of the discontinuity is significant as well with firms affected by WARN experiencing increases in patents and citations by 43% and 71% respectively when compared to similar firms that were not affected by WARN. Given the median firm filing one patent per year, this implies an increase of about one additional patent in two years after the passage of WARN.

As discussed in Section 2, according to our model, the positive effect of dismissal laws on innovation results from the positive effect that these laws have on employee effort. Unlike our cross-country set-up, our sample here is constructed at the firm level. Therefore, we can investigate whether the passage of WARN had an effect on employee effort in innovative projects. For this purpose, we normalize our proxies for innovation using the number of employees in a firm. In Columns 4-5 of Panel A, we report the results using $ln(patents/employees)$ and $ln(citations/employees)$ as the dependent variables. Here, we find that both patents and citations per employee increase significantly after the passage of WARN for the "treatment" group of firms. This finding is important because it shows that the theoretical backdrop finds empirical support not only in its result linking dismissal laws and innovation, but also in the specific mechanism we conjecture to be at play, which is that the positive effect of dismissal laws on innovation results from the positive effect that these laws have on employee effort.

Panel B and C of Table 10 show the results for the placebo tests using only firms that have employee numbers in the range [45, 55] and [145, 155] respectively in 1987. In each of these panels, Column 1 shows the effect on employee layoffs while Columns 2-3 show the results for the log of the number of patents and citations respectively; as before, Columns 4-5 report the results using log of the number of patents and citations per employee respectively. In both these panels, we can infer that there was no differential effect at the corresponding cutoffs that is consistent with our hypotheses. This provides reassurance that the positive effect of WARN on innovation documented in Panel A is not spurious.

4.4 WARN Act and Innovation: Difference-in-difference Tests

Having convinced ourselves that the discontinuous effect of the WARN Act on employee layoffs and innovation is indeed strong for firms in the vicinity of the cut-off, we now investigate the effect of the WARN Act on innovation for the entire sample of firms. Figure 7 illustrates the difference-in-difference effect, where we compare the U.S. firms that were affected by the law change (firms with 100 or more employees) to U.S. firms that were not (firms with less than 100 employees); it shows the linear fit of the number of patents and citations across time for the treated and control firms before and after 1989. The presence of a break for the treated firms and its absence for the control group of firms in 1989 is quite clear from the figure. To undertake tests that formalize this visual effect, we implement the equivalent of equation (10) for the entire sample:

$$y_{it} = \beta_i + \beta_t + \beta_1 * (Over100)_{i,1987} * (After1988)_t + \beta \cdot X_{it} + \epsilon_{it} \tag{12}$$

where all the variables are as defined above. X_{it} represents the set of control variables which include $Size$ and *Market-to-Book* ratio and, in some specifications, the interaction of $Size$ with the $(After1988)_t$ dummy.[25]

Table 11 shows the results for the difference-in-difference effect of the WARN Act on innovation. In Columns 1-2 of Panel A, we run the basic specification using logs of the number of patents and citations respectively as the dependent variables. In Columns 3-4, we also include firm size to account for the possibility that larger firms might innovate more on average. Second, we include *Market-to-Book* to control for investment opportunities, as these may also have an impact on a firm's innovation policies. In Columns 5-6, we include the interaction of firm size with a dummy for the period following the WARN passage. This interaction controls for the possibility that after the passage of WARN, larger firms may have systematically differed from smaller firms in their innovation outcomes.

In line with Hypothesis 1, we find that overall, the strengthening of dismissal laws via the WARN Act had a positive and significant impact on U.S. firm-level innovation. Compared to the

[25] *Market-to-Book* is the market value of assets to total book assets. Market value of assets is total assets plus market value of equity minus book value of equity. The market value of equity is calculated as common shares outstanding times fiscal-year closing price. Book value of equity is defined as common equity plus balance sheet deferred taxes. *Size* is the natural logarithm of sales. In order to eliminate the impact of outliers, we winsorize the variables *Market-to-Book* and *Size* at 1% and 99%.

control group, annual patents increased by 18% (Column 1) for the treatment group of firms, with an even larger effect for citations.

In Panel B of Table 11, we report the results using the log of the number of patents and citations per employee respectively as the dependent variables. Again, we find overall the effect on these proxies for employee effort at innovation to be strongly positive and statistically significant.

Finally, in Columns 5-6 (Panel A and B of Table 11), we interact firm size with the $(After1988)_t$ dummy. The positive and significant coefficient on the interaction indicates that after 1988, large firms innovated more for reasons that may be unrelated to WARN. However, the fact that the difference-in-difference coefficient in the specification with citations (Column 6 in both panels) remains significant at the 5% level shows that even if larger firms had systematically different innovation outcomes compared to smaller firms in the period post 1988, WARN caused employees to focus on innovation that was more important, i.e., more highly cited.

4.5 Discussion

Apart from not suffering from concerns relating to unobserved factors at the country level, the above tests based on WARN offer other attractive advantages. Since our sample for the WARN tests ended in 1994, they enable us to conclude that our results on the positive effect of dismissal laws on innovation are not driven by any spurious effects that patent reforms motivated by the General Agreement on Tariffs and Trade (GATT) may have had. Under the GATT changes, an unexpired issued patent or a patent application pending on June 8, 1995, has a term of protection that is the longer of 17 years from the date of issuance of the patent or 20 years from the filing date of the patent application. For applications filed on or after June 8, 1995, the patent life is now twenty years, measured from the earliest patent application. However, since our sample for the WARN tests is terminated in 1994, our results are not caused by GATT related changes.

Also, the tests based on the WARN Act mitigate effects of any other contemporaneous factors that may confound our results. This strength of the WARN based tests stems from a combination of three factors. First and foremost, since the firms are separated into treatment and control groups based on the number of employees, any unobserved factor that affects all firms uniformly (i.e. irrespective of employment figures) cannot be driving our results. Nevertheless, as a second line of defense, we have included firm-fixed effects to account for time-invariant effects of unobserved factors, in general, and firm size and investment opportunities (market-to-book), in particular. Second, we have performed pure regression-discontinuity tests to focus on firms just above and below the employment cut-off relevant for WARN. Third, in the difference-in-difference tests, we have included firm size and its interaction with the post-WARN period to account for any *time-varying* correlation of any unobserved factors with firm size. Therefore, laws or policy changes or any other unobserved factor that may influence innovation cannot affect the results unless they resemble WARN in discriminating based on workforce-size.

Related to the above, the WARN tests also alleviate concerns that our results may be affected by the coinciding of the post WARN period with the recession in the early 1990s. To the extent that this recession slowed down the average pace of innovation, the application year fixed effects should capture the same. If, instead, the recession affected innovation by smaller or larger firms disproportionately more, the interaction of firm size with the dummy for the time period post 1988 should capture some of these effects. Finally, since firms of similar sizes should have felt the effect of the recession similarly, the regression-discontinuity specification provides confirmation that our results are not affected by the recession in the 1990s.

Finally, the WARN Act was not intended to specifically encourage innovation or economic growth. Brügemann (2007) examines various articles in the business press that document the events preceding and following the WARN Act. He does not find any evidence arguing that the Act was aimed at improving a specific aspect of the U.S. economy. Thus, our tests above can be interpreted as a truly causal, even if an unintended, effect of the WARN Act passage on innovation.

5 Related Literature

In examining the effect of laws on employee stability and thereby the real investments made by a firm, our work is closest to that of Garmaise (2007). Using legal enforcement of employee non-compete agreements as a proxy for laws that limit human capital mobility, he finds that such laws enhance executive stability. However, in contrast to our results, such limits on human capital mobility reduce firm-level investments in Research and Development in his study. One way to rationalize our findings with his is that non-compete agreements induce employee stability by restricting their freedom from departure when they wish to leave firms; in contrast, dismissal laws induce employee stability by restricting the ability of firms to fire employees. Thus, in the setting of Garmaise (2007), lower human capital mobility is associated with less firm-specific or skill-intensive investments by employees such as in Research and Development since these are more likely ex post to lead to invocation of non-compete clauses; however, in our setting, lower mobility encourages innovative pursuits since employees are less likely to be fired in case innovative projects fail (including due to sheer bad luck), thereby making innovation more profitable from an ex ante standpoint for firms too.

Our cross-country tests together with the WARN-based results complement the findings in Acharya, Baghai, and Subramanian (2009), who show that the staggered adoption of common-law exceptions to the "employment-at-will" principle (so-called "Wrongful-Discharge Laws") in several U.S. states resulted in more innovation by U.S. firms. Apart from the different setting (cross-country and U.S. federal law changes vis-à-vis law changes within U.S. states), this study differs from Acharya, Baghai, and Subramanian (2009) in other key ways. Since the cross-country setting provides variation stemming from passage of different labor laws, we are able to confirm here that dismissal laws are salient in engendering positive incentives for innovation and spurring economic growth. Other dimensions of labor laws do not have this salutary effect; in fact, we find that laws favoring industrial action may be quite detrimental to economic growth at the country level. Also, the rich variation induced by the numerous dismissal law changes enables us to control for possible country-specific (and industry-specific) time trends, which we cannot do in the other study.

More generally, our paper contributes to the earlier literature that examines the effect of laws governing the employer-employee relationship. Botero et al. (2004) find that heavier regulation of labor leads to adverse consequences for labor market participation and unemployment. Atanassov and Kim (2007) examine the interaction between labor laws and investor protection laws and find that rigid employment laws lead to higher likelihood of value-reducing major asset sales, particularly when investor protection is weak. They find that assets are sold to forestall layoffs, even if these asset sales hurt performance. Besley and Burgess (2004) conclude from their study of manufacturing performance in Indian states that pro-worker labor laws are associated with lower levels of investment, productivity, and output. Bassanini, Nunziata and Venn (2009) also show that mandatory dismissal regulations in OECD countries have a depressing effect on productivity growth in industries where layoff restrictions are more likely to be binding.

In contrast to these studies which document the negative effects of labor laws, our study finds that stringent labor laws can motivate a firm and its employees to pursue value-enhancing innovative activities. Menezes-Filho and Van Reenen (2003) focus on a specific aspect of labor laws — the extent to which unions are allowed to operate — and survey the existing literature for their effects on innovation. They note that while U.S. studies find a negative impact of unions on innovation, European studies do not uniformly support these findings. While Menezes-Filho and Van Reenen (2003) focus on laws governing unions, we examine all dimensions of labor laws and pay particular attention to laws governing dismissal of employees.

Also related to our study is the work by MacLeod and Nakavachara (2007) who develop a theoretical model and provide empirical evidence that the passage of wrongful discharge laws across several U.S. states enhances (reduces) employment in industries requiring high (low) relationship specific-investment. Acharya, Baghai, and Subramanian (2009) find that the passage of these wrongful discharge laws across U.S. states also led to increased innovation.

The stance that strong dismissal laws may be efficient is in line with the view taken by many human resource (HR) management scholars who deem stable employer-employee relationships characterized by low employee turnover, as well as a corporate culture in which failure is tolerated, and risk taking and learning are actively encouraged, important catalysts for innovation (see Hailey, 2001, and the literature cited therein). "Indeed, the HR literature tends, on the whole, to suggest that secure, permanent employment contracts allied to a 'high-commitment' management approach will be necessary or at least advantageous in those cases where a business strategy aspires towards innovation" (p.3, Storey et al., 2002).

In less directly related work, Simon (1951) and Williamson, Wachter and Harris (1975) argue that stronger labor laws may also have an ex post efficiency aspect to them. While the former study argues that strong labor laws provide insurance to employees against risks associated with loss of income and employment, the latter claims that strong labor laws reduce transaction costs derived from the incompleteness of the employment contract. Finally, Lerner and Wulf (2007) examine U.S. publicly listed firms with centralized R&D units and find that long-term incentives provided to corporate R&D heads are associated with greater firm-level innovation.

6 Conclusion

In this paper, we presented empirical evidence that firm-level innovation is causally determined by laws governing the ease with which firms can dismiss their employees. Using patents and citations as proxies for innovation and a time-varying index of dismissal laws, we found both in a cross-country and within-U.S. setting that stringent dismissal laws seem to foster innovation.

The robustness and strength of our results begs the question whether such laws are in fact necessary to promote innovation. Can firm-level contracts not suffice to provide employees the incentives to innovate? One possibility is that innovation may have externalities and thus institutions supporting innovation might be desirable to obtain socially efficient investments in innovation (Romer, 1986; Aghion and Howitt, 1992). Another possibility is that firm-level contracts lack the force of commitment that laws offer. Since the outcomes of innovation are unpredictable, they are difficult to contract ex ante (Aghion and Tirole, 1994), which renders private contracts to motivate innovation susceptible to renegotiation. Such possibility of renegotiating contracts dilutes their ex ante incentive effects. Since laws are considerably more difficult for private parties to alter than firm-level contracts, legal protection of employees in the form of stringent dismissal laws can introduce the time-consistency in firm behavior absent with only private contracts.

Another reason why the law might be necessary to protect employee dismissals and promote innovation is that firms may be run by short-termist or myopic top management. In such firms, poor firm-level governance of top management actions might prevent efficient long-term contracts being written with employees. The law can improve the so-called "internal governance" of firms (Acharya, Myers and Rajan, 2010) by effectively lengthening the horizon of employees and indirectly inducing the top management to provide better incentives to employees by investing for the long run. Assessing whether labor laws are indeed efficient is an important topic for future research. Our results highlight one important positive effect of dismissal laws, namely their ability to spur innovation and economic growth, that must be factored into such an assessment.

Appendix A – Description of the Labor Law Index

This section briefly describes the components of the labor law index as detailed in Deakin et al. (2007).

Alternative Employment Contracts. This sub-index measures the cost of using alternatives to the "standard" employment contract, computed as an average of the eight following variables: 1. Stringency as to the determination of the legal status of the worker (equal 1 if the law mandates such a status; 0.5 if the law allows the status to be determined by the contract nature; and 0 if the parties have complete freedom in stipulating the status); 2. Equal treatment of part-time workers relative to full-time ones (equal 1 if part-time workers are legally recognized a right to equal treatment with full-time workers; 0.5 if this right is more limited; and 0 otherwise); 3. Cost of dismissing part-time workers relative to that for full-time workers (equal 1 if part-time workers enjoy proportionate rights to full time workers regarding dismissal protection; and 0 otherwise); 4. Substantive constraints on the conclusion of a fixed-term contract (equal 1 if there is such a constraint; and 0 otherwise); 5. The right to equal treatment of fixed-term workers relative to permanent workers (equal 1 if such a right is present, 0.5 if such a right is more limited, and 0 otherwise); 6. Maximum duration of fixed-term contracts before the employment is deemed permanent (taking scores between 0 and 1, with higher scores indicating a lower allowed duration); 7. Stringency as to the use of agency work (equal 1 if the use of agency labor is prohibited, 0.5 if this use is limited and 0 otherwise); and 8. Equal treatment of agency workers relative to permanent ones (equal 1 if the right to this equal treatment is legally recognized, an intermediate score between 0 and 1 if this right is limited, and 0 otherwise).

Regulation of Working Time. This sub-index measures how employee-focused the law on working time is. The sub-index is as an average of the following seven variables: 1. Annual leave entitlements, which measures the standardized normal length of annual paid leave (taking values between 0 and 1, with higher values indicating longer leave entitlements); 2. Public holiday entitlements (taking values between 0 and 1, with higher values indicating longer public holiday entitlements); 3. Overtime premia (equal 1 if the premium if double time, 0.5 if it is time and a half, and 0 if there is no overtime premium); 4. Weekend working (equal 1 if the normal premium for weekend working is double time, or if weekend working is prohibited or strictly controlled, 0.5 if it is time and a half, and 0 if there is no premium); 5. Limits to overtime working (equal 1 if there is a limit to the number of weekly working hours, including overtime, 0.5 if such limits can be averaged out over a period longer than a week, and 0 if there is no such a limit); 6. Duration of the weekly normal working hours, exclusive of overtime (equal 1 for 35 hours or less, 0 for 50 hours or more, and intermediate values between 0 and 1 for the rest); and 7. Maximum daily working time (scores are normalized to be on a 0-1 scale, with a limit of 8 hours scoring 1, and a limit of 18 hours or more scoring 0).

Regulation of Dismissal. This sub-index measures the extent to which the regulation of dismissal favors the employee; note that this sub-index corresponds to the "dismissal law index" used in this paper. The sub-index is an average score of the following nine variables: 1. Legally mandated notice period (values are normalized to be between 0 and 1, with 12 weeks = 1 and 0 weeks = 0); 2. Legally mandated redundancy compensation made to a worker who is made redundant after 3 years of employment (values are normalized to be between 0 and 1, with 12 weeks = 1 and 0 weeks = 0); 3. Minimum qualifying period of service for normal case of unjust dismissal (values are normalized to be between 0 and 1, with 0 months = 1 and 3 years or more = 0); 4. Procedural constraints on dismissal (taking values of 1, 0.67, 0.33 and 0; the higher of which suggests higher costs of the employer's failure to follow procedural requirements prior to dismissal); 5. Substantive constraints on dismissal (taking values of 1, 0.67, 0.33 and 0; the higher of which suggests stricter requirements on the part of the employer to establish reasons for dismissal); 6. Reinstatement as a normal remedy for unfair dismissal (taking values of 1, 0.67, 0.33 and 0; which suggest, as the remedy for unfair dismissal, respectively reinstatement, a choice of reinstatement or compensation, compensation, no remedy); 7. Notification of dismissal (taking values of 1, 0.67, 0.33 and 0; higher values of which imply more complicated procedure for dismissal notification); 8. Redundancy selection (equal 1 if redundancy dismissal must be based on priority rules, and 0 otherwise); and 9. Priority in re-employment (equal 1 if re-employment must be based on priority rules, 0 otherwise).

Employee Representation. This sub-index measures the strength of employee representation. The sub-index is an average score of the following seven variables: 1. Right to Unionization (taking values of 1, 0.67, 0.33 and 0; higher values indicate better protection of the right to form trade unions); 2. Right to collective bargaining (taking values of 1, 0.67, 0.33 and 0; higher values indicate better protection of the right to collective bargaining); 3. Duty to bargain (equal 1 if the employer has the legal duty to reach an agreement with worker organizations; and 0 otherwise); 4. Extension of collective agreements (equal 1 if collective agreements are legally extended to third parties at the national or sectoral level, and 0 otherwise); 5. Closed shops (equal 1 if both pre-entry and post-entry closed shops are permitted, 0.5 if pre-entry closed shops are prohibited but post-entry ones are permitted; and 0 if neither type of closed shops is permitted); 6. Codetermination via board membership (equal 1 if unions/ workers have the legal right to nominate directors in companies of a certain size; and 0 otherwise); and 7. Codetermination and information/ consultation of workers (taking values of 1, 0.67, 0.5, 0.33 and 0; higher values of which suggest higher degree of participation by workers in the determination process through work councils and enterprise committees).

Industrial Action. This sub-index measures the strength of legal protection for industrial action. The sub-index is calculated as the average of the following nine variables: 1. Unofficial industrial action (equal 1 if strikes are conditionally not unlawful, and 0 otherwise); 2. Political industrial action (equal 1 if political-oriented strikes are permitted, and 0 otherwise); 3. Secondary industrial action (taking values of 1, 0.5 and 0 if secondary or sympathy strike action is respectively unconstrained, permitted under certain conditions, and prohibited); 4. Lockouts (equal 1 if permitted and 0 otherwise); 5. Right to industrial action (taking values of 1, 0.67, 0.33 and 0; higher values of which suggest better protection of the right to industrial action); 6. Waiting period prior to industrial action (equal 1 if strikes can occur without mandatory prior notification/waiting period, and 0 otherwise); 7. Peace obligation (equal 1 if existence of a collective agreement does not render a strike unlawful, and 0 otherwise); 8. Compulsory conciliation or arbitration (equal 1 if alternative dispute resolution mechanisms before the strike are not mandatory, and 0 otherwise); and 9. Replacement of striking workers (equal 1 if employers are prohibited from dismissing striking workers engaging in a non-violent or non-political strike, and 0 otherwise).

Appendix B – "Difference-in-Difference" Interpretation for the Fixed Effect Panel Regressions

In this Appendix, we show that the fixed effects panel regressions employed in equation (1) estimate a "difference-in-difference" in a generalized multiple treatment groups, multiple time period setting.

We begin with the model specification used in equation (1):

$$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 \cdot DismissalLaws_{ct} + \varepsilon_{ict} \tag{B-1}$$

During the sample period 1970-2002, suppose the Dismissal Law Index for country c, $DismissalLaws_{ct}$, changes n times in years $t_1, ..., t_n$, where $1 < ... < n$ and t_l denotes the year in which the l^{th} change occurred for country c. Denote $m_l = [t_l + 1, t_{l+1}]$ as the time interval during which the l^{th} change has occurred but not the $(l+1)^{th}$. Let $DismissalLaws_c(m_l)$ denote the value of the Dismissal Law Index during the period m_l. Thus, $DismissalLaws_{ct} = DismissalLaws_c(m_l)$ for any $t \in m_l$.

Therefore,

$$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 \cdot DismissalLaws_c(m_l) + \varepsilon_{ict}, t \in m_l \tag{B-2}$$
$$y_{ict'} = \beta_i + \beta_c + \beta_{t'} + \beta_1 \cdot DismissalLaws_c(m_{l+1}) + \varepsilon_{ict'}, t' \in m_{l+1} \tag{B-3}$$

Subtracting $(B-2)$ from $(B-3)$, we obtain

$$y_{ict'} - y_{ict} = (\beta_{t'} - \beta_t) + \beta_1 \cdot \Delta DismissalLaws_{cl} + \varepsilon_{ict'} - \varepsilon_{ict} \tag{B-4}$$

where

$$\Delta DismissalLaws_{cl} = DismissalLaws_c(m_{l+1}) - DismissalLaws_c(m_l)$$

denotes the magnitude of the l^{th} change in the Dismissal Law Index in country c.

Let c' denote a country that did not change its dismissal laws over the time intervals m_l or m_{l+1} or equivalently the time period $[t_l + 1, t_{l+2}]$.

$$\begin{aligned} y_{ic't} &= \beta_i + \beta_c + \beta_t + \beta_1 \cdot DismissalLaws_{c'}(m_l) + \nu_{ict}, t \in m_l & \text{(B-5)} \\ y_{ic't'} &= \beta_i + \beta_c + \beta_{t'} + \beta_1 \cdot DismissalLaws_{c'}(m_{l+1}) + \nu_{ict'}, t' \in m_{l+1} & \text{(B-6)} \end{aligned}$$

Because the Dismissal Law Index is unchanged over the time period $[t_l + 1, t_{l+2}]$,

$$DismissalLaws_{c'}(m_l) = DismissalLaws_{c'}(m_{l+1}) \tag{B-7}$$

Subtracting $(B-5)$ from $(B-6)$ and using $(B-7)$, we obtain

$$y_{ic't'} - y_{ic't} = (\beta_{t'} - \beta_t) + \nu_{ict'} - \nu_{ict} \tag{B-8}$$

Subtracting $(B-8)$ from $(B-4)$, we obtain

$$[y_{ict'} - y_{ict}] - [y_{ic't'} - y_{ic't}] = \beta_1 \cdot \Delta DismissalLaws_{cl} + [(\varepsilon_{ict'} - \nu_{ict'}) - (\varepsilon_{ict} - \nu_{ict})]$$

Assuming that

$$E\left[\{(\varepsilon_{ict'} - \nu_{ict'}) - (\varepsilon_{ict} - \nu_{ict})\} \, | \Delta DismissalLaws_{cl}\right] = 0 \tag{B-9}$$

we get after taking expectations

$$\beta_1 \cdot \Delta DismissalLaws_{cl} = \underbrace{E\left[y_{ict'} - y_{ict}\right]}_{\text{Before-after difference for Treatment}} - \underbrace{E\left[y_{ic't'} - y_{ic't}\right]}_{\text{Before-after difference for Control}}$$

Thus, β_1 estimates the difference-in-difference in a multiple treatment groups, multiple time periods setting.

References

[1] Acharya, V., R. Baghai and K. Subramanian, 2009, "Wrongful Discharge Laws and Innovation," Working paper, New York University Stern School of Business.

[2] Acharya, V., S. Myers and R. G. Rajan, 2010, "The Internal Governance of Firms, " *Journal of Finance*, forthcoming.

[3] Acharya, V., K. Subramanian, 2009, "Bankruptcy Codes and Innovation," *Review of Financial Studies*, 22(12), 4949–4988.

[4] Aghion, P. and P. Howitt, 1992, "A Model of Growth Through Creative Destruction," *Econometrica*, 60(2), 323–352.

[5] Aghion, P. and P. Howitt, 2005, "Growth with Quality-Improving Innovations: An Integrated Framework," *Handbook of Economic Growth* (by P. Aghion and S.N. Durlauf, eds.), Vol. 1A, Chapter 2, pp. 67–110, Elsevier, Amsterdam.

[6] Aghion, P. and J. Tirole, 1994, "The Management of Innovation," *The Quarterly Journal of Economics*, 109(4), 1185–1209.

[7] Armingeon, K., M. Gerber, P. Leimgruber, and M. Beyeler, 2008, "Comparative Political Data Set 1960–2006, " Institute of Political Science, University of Berne.

[8] Atanassov, J., and E. H. Kim, 2007, "Labor Laws and Corporate Governance: International Evidence from Restructuring Decisions," Working Paper, Ross School of Business.

[9] Bassanini, A., L. Nunziata and D. Venn, 2009, "Job protection legislation and productivity growth in OECD countries, " *Economic Policy* 24(58), 351–402.

[10] Bertrand, M. and S. Mullainathan, 2003, "Enjoying the Quiet Life? Corporate Governance and Managerial Preferences," *Journal of Political Economy*, 111, 1043–1075.

[11] Besley, T. and R. Burgess, 2004, "Can Labor Regulation Hinder Economic Performance? Evidence from India," *Quarterly Journal of Economics*, 119(1), 91–134.

[12] Botero, J., Djankov, S., La Porta, R., F. Lopez-De-Silanes and A. Shleifer, 2004, "The Regulation of Labor," *Quarterly Journal of Economics*, 119(4), 1339–1382.

[13] Brügemann, B., 2007, "Employment Protection: Tough to Scrap or Tough to Get?," *Economic Journal*, 117, 386–415.

[14] Cantwell, J. and B. Andersen, 1996, "A Statistical Analysis of Corporate Technology Leadership Historically," *Economics of Innovation and New Technology*, 4(3),211-234.

[15] Cantwell, J. and C. Hodson, "Global R&D and U.K. Competitiveness," in MC Casson, ed, *Global Research Strategy and International Competitiveness*, Basil Blackwell, 1991.

[16] Deakin, S., P. Lele, and M. Siems, 2007, "The evolution of labour law: Calibrating and comparing regulatory regimes," *International Labour Review* 146(3-4), 133–162.

[17] Djankov, S., C. McLiesh and A. Shleifer, 2007, "Private Credit in 129 Countries," *Journal of Financial Economics*, 85(2), 299–329.

[18] Garmaise, M., 2007, "Ties that Truly Bind: Non-competition Agreements, Executive Compensation and Firm Investment," UCLA Anderson working paper.

[19] Griliches, Z., 1990, "Patent statistics as economic indicators: A survey," *Journal of Economic Literature*, 28, 1661–1707.

[20] Grossman, G. and O. Hart, 1986, "The Costs and Benefits of Ownership: A Theory of Vertical and Lateral Integration," *The Journal of Political Economy*, 94(4), 691–719.

[21] Grossman, G. and E. Helpman, 1991, "Innovation and Growth in the Global Economy," Cambridge: MIT Press.

[22] Hailey, V., 2001, "Breaking the mould? Innovation as a strategy for corporate renewal," *The International Journal of Human Resource Management*, 12(7), 1126–1140.

[23] Hall, B. H., A. Jaffe and M. Trajtenberg. 2001. "The NBER Patent Citations Data File: Lessons, Insights and Methodological Tools," NBER working paper.

[24] Hall, B., A. Jaffe, and M. Trajtenberg, 2005, "Market value and patent citations," *RAND Journal of Economics* 32, 101–128.

[25] Hart, Oliver. 1995. Firms, Contracts, and Financial Structure. *Oxford: Clarendon Press.*

[26] Hart, O. and J. Moore, 1990, "Property Rights and the Nature of the Firm," *Journal of Political Economy* 98(6), 1119–1158.

[27] Imbens, Guido M. and Jeffrey M. Wooldridge, 2009, "Recent Developments in the Econometrics of Program Evaluation," *Journal of Economic Literature*, 47(1), 5–86.

[28] Kondo M., 1999, "R&D dynamics of creating patents in the Japanese industry," *Research Policy*, 28(6), 587-600.

[29] Kortum, S. and J. Lerner, 1999, "What is behind the recent surge in patenting?," *Research Policy*, 28, 1-22.

[30] Lall, Sanjaya, 2003, "Indicators of the relative importance of IPRs in developing countries," *Research Policy,* 32(9), 1657-1680.

[31] La Porta, Rafael, F. Lopez-de-Silanes, A. Shleifer, and R. W. Vishny, 1997, "Legal determinants of external finance," *Journal of Finance*, 52, 1131-1150.

[32] La Porta, Rafael, F. Lopez-de-Silanes, A. Shleifer, and R. W. Vishny, 1998, "Law and finance," *Journal of Political Economy*, 101, 678-709.

[33] Lerner, Josh, and Julie Wulf, 2007, "Innovation and Incentives: Evidence from Corporate R&D," *The Review of Economics and Statistics,* 89(4), 634–644.

[34] Levine, L., 2007, "The Worker Adjustment and Retraining Notification Act (WARN)," Working Paper, Congressional Research Service.

[35] MacGarvie, 2006, "Do firms learn from international trade?" *The Review of Economics and Statistics*, 88(1), 46-60.

[36] MacLeod W.B. and V. Nakavachara, 2007, "Can Wrongful Discharge Law Enhance Employment?," *The Economic Journal*, 117, 218—278.

[37] McKinsey Global Institute, 1997, *France and Germany*, Washington, DC: McKinsey.

[42] Menezes-Filho, N., and J. Van Reenen, 2003, "Unions and Innovation: A Survey of the Theory and Empirical Evidence," in John T. Addison and Claus Schnabel, eds., *International Handbook of Trade Unions,* Edward Elgar Publishing Ltd, 293–334.

[39] Nicita A. and M. Olarreaga, 2006, "Trade, Production and Protection: 1976-2004," *World Bank Economic Review,* 21(1).

[40] Nicoletti, G. and S. Scarpetta, 2001, "Interaction between Product and Labour Market Regulations: Do They Affect Employment? Evidence from the OECD Countries," *Conference on "Labour Market Institutions and Economic Outcomes."* Lisbon: Bank of Portugal, June 3–4, 2001.

[41] Pakes, A., and M. Shankerman, 1984, "The rate of obsolescence of patents, research gestation lags, and the private rate of return to research resources," in Zvi Griliches, ed., *R&D, Patents and Productivity,* University of Chicago Press, 98–112.

[42] Rajan, R. and L. Zingales, 1998, "Financial dependence and growth," *American Economic Review*, 88,559–586.

[43] Romer, P., 1986, "Increasing Returns and Long-Rung Growth," *Journal of Political Economy*, 94(5), 1002–1037.

[44] Romer, P., 1990, "Endogenous Technological Change," *Journal of Political Economy*, 98(5), pages S71–102.

[45] Simon, H.A., 1951, "A Formal Theory of the Employment Relationship," *Econometrica*, 19, 293–305.

[46] Storey, J., P. Quintas, P. Taylor, and W. Fowle, 2002, "Flexible employment contracts and their implications for product and process innovation," *The International Journal of Human Resource Management*, 13(1), 1–18.

[47] Williamson, O. E., M. L. Wachter, and J. E. Harris, 1975, "Understanding the Employment Relation: The Analysis of Idiosyncratic Exchange," *The Bell Journal of Economics*, 6(1), 250–278.

Figure 1: Effect of WARN Act on Innovation by US Firms.

The figure shows the discontinuous effect of the passage of WARN on innovation as measured by the log of patents. We plot the number of firm employees in 1987 (horizontal axis) against the difference in innovation before and after the passage of WARN (vertical axis). Each point in the graph is the average before-after difference for each firm and subsumes a maximum of 16 observations for a firm into one; multiple plot points for different firms may coincide and appear as one point in the graph. The WARN Act only applies to firms with more than 100 employees. In the left panel, we show the effect on innovation around the actual legal cutoff of 100 employees, while in the right panel, we show the (absence of the) effect around a placebo cutoff of 50 employees.

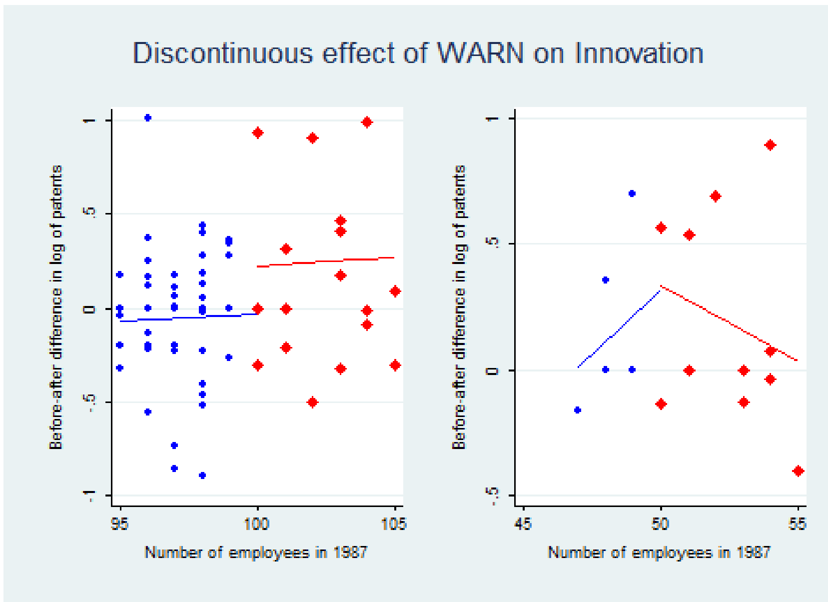


Figure 2: Regulation of Dismissal.

The figure shows the strength of the "Regulation of Dismissal" for a given country and year. Higher values indicate more employment protection / stricter laws. The dismissal index data is from Deakin et al. (2007).

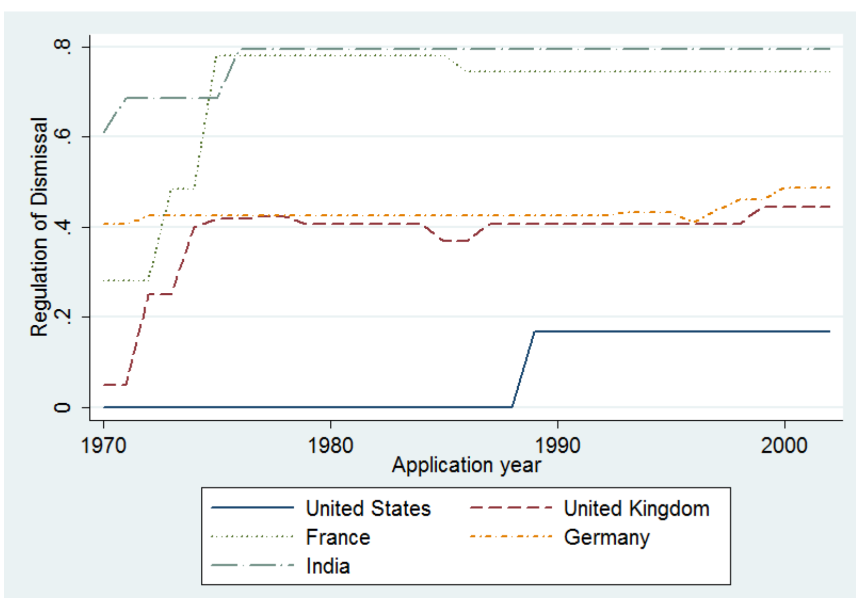


Figure 3: Components of the Dismissal Law Index.

The figure shows the nine sub-components of the "Dismissal Law Index" for a given country and year. Higher values indicate more employment protection / stricter laws. Each line represents one country (France, Germany, India, UK, or US). The sub-components of the "Dismissal Index" are: *v16* (Legally mandated notice period); *v17* (Legally mandated redundancy compensation); *v18* (Minimum qualifying period of service for normal case of unjust dismissal); *v19* (Law imposes procedural constraints on dismissal); *v20* (Law imposes substantive constraints on dismissal); *v21* (Reinstatement normal remedy for unfair dismissal); *v22* (Notification of dismissal); *v23* (Redundancy selection); *v24* (Priority in re-employment). These index components are described in more detail in Appendix A. The index data is from Deakin et al. (2007).

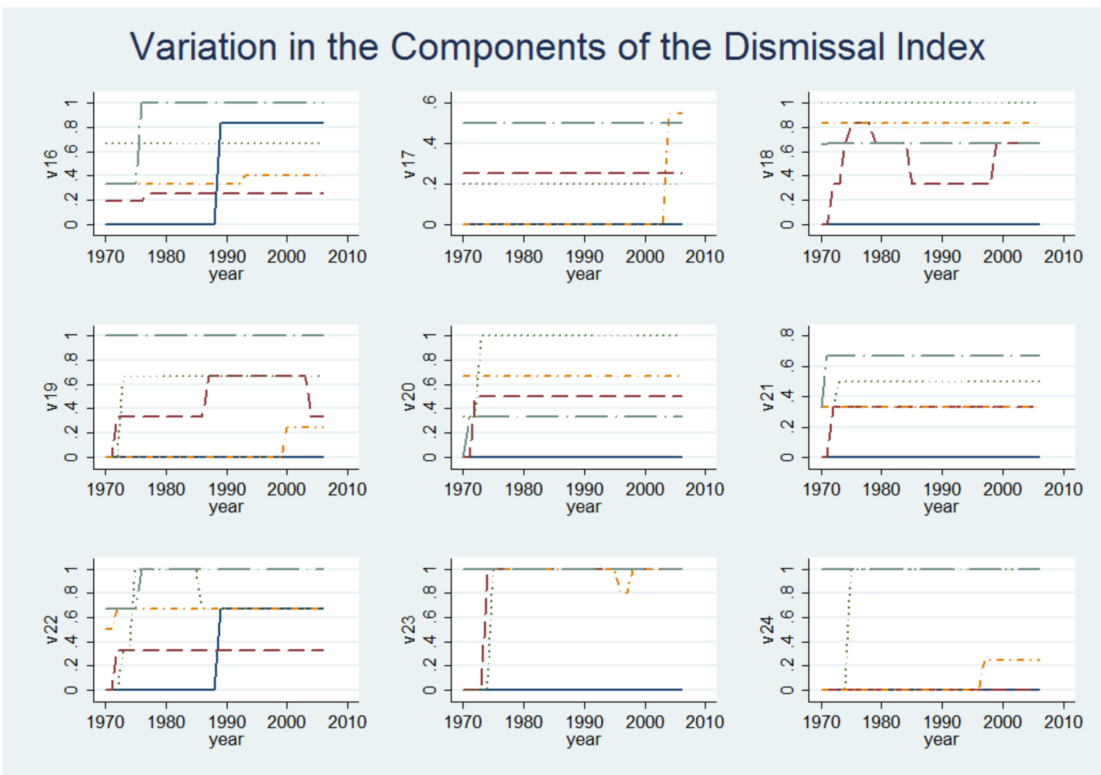
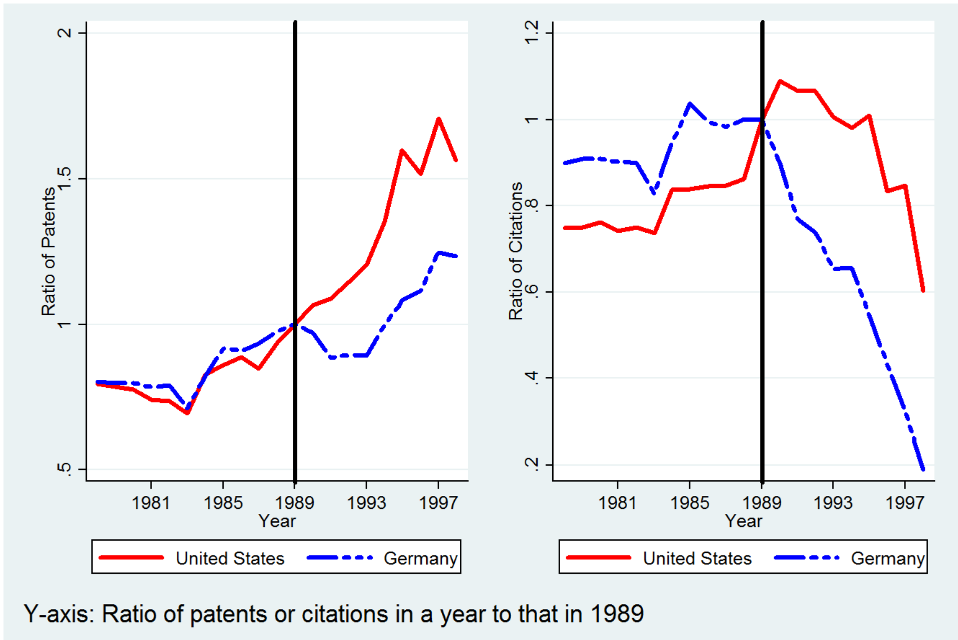
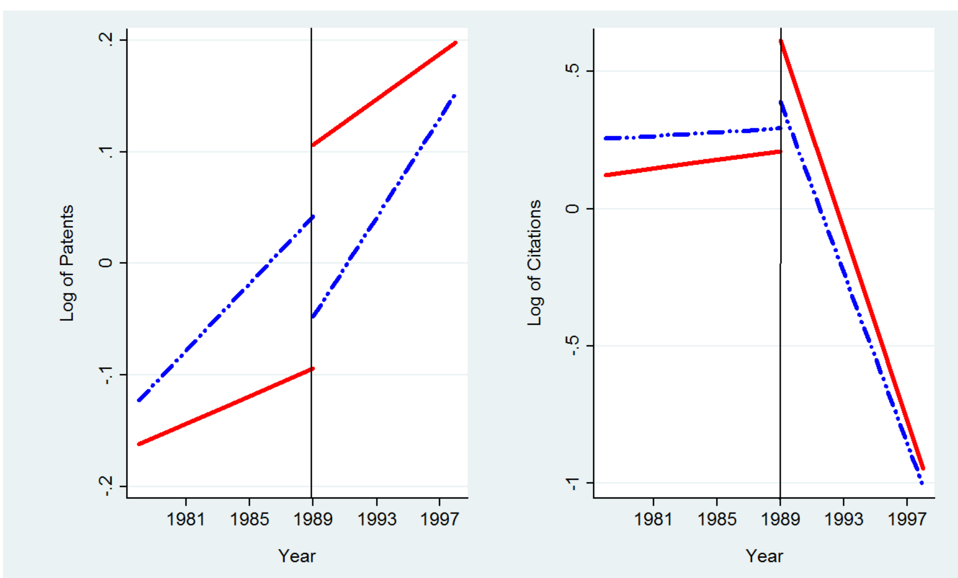


Figure 4: Aggregate Innovation: U.S. vs Germany.

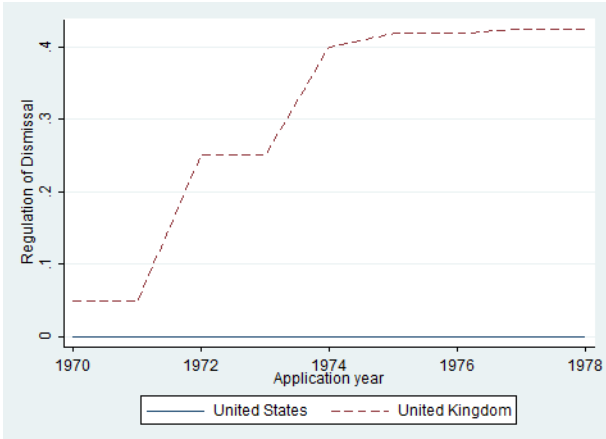


(a) This figure shows a plot across time of the *ratio* of the realized number of patents and citations in a particular year to that in 1989, the year the U.S. WARN Act became effective. The continuous line shows the ratio for the U.S. while the discontinuous line shows the same for Germany, which experienced no dismissal law change in the time interval examined. The vertical line indicates the year the U.S. WARN Act became effective (1989).

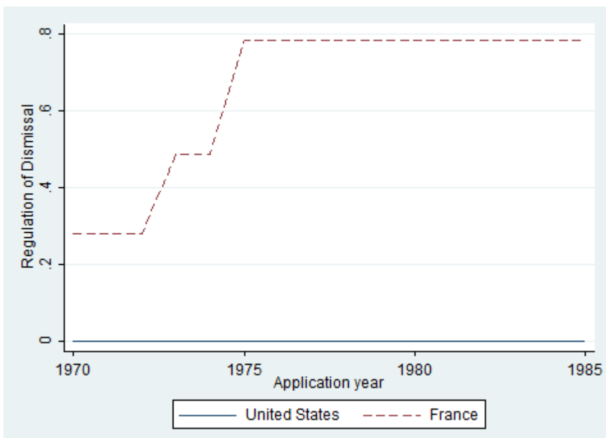


(b) This figure shows the linear fit of the two innovation measures patents and citations for the treated (U.S.; continuous line) and control (Germany; discontinuous line) groups before and after the WARN Act became effective.

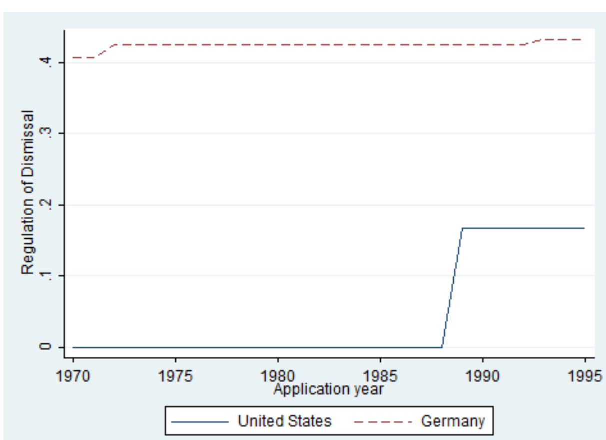
Figure 5: Regulation of Dismissal.



(a) **Regulation of Dismissal, U.S. and U.K.** The figure shows the index representing the regulation of dismissal for the U.S. and U.K. from 1970-1978.



(b) **Regulation of Dismissal, U.S. and France.** The figure shows the index representing the regulation of dismissal for the U.S. and France from 1970-1985.



(c) **Regulation of Dismissal, U.S. and Germany.** The figure shows the index representing the regulation of dismissal for the U.S. and Germany from 1970-1995.

Figure 6: Differences in Innovation between Innovation-intensive and Non-intensive Industries for U.S. vis-à-vis Germany.

This figure plots the time series of the *ratio* of the realized number of patents and citations in an innovation-intensive sector (Surgery and Medical Instruments) relative to a non-intensive sector (Textiles and Apparel) for the U.S. vis-a-vis Germany. The continuous line shows the trend for the U.S. while the discontinuous line shows the same for Germany. The vertical line indicates the year 1989, when the U.S. WARN Act became effective. For each country, the ratio is normalized to 1 in 1989.

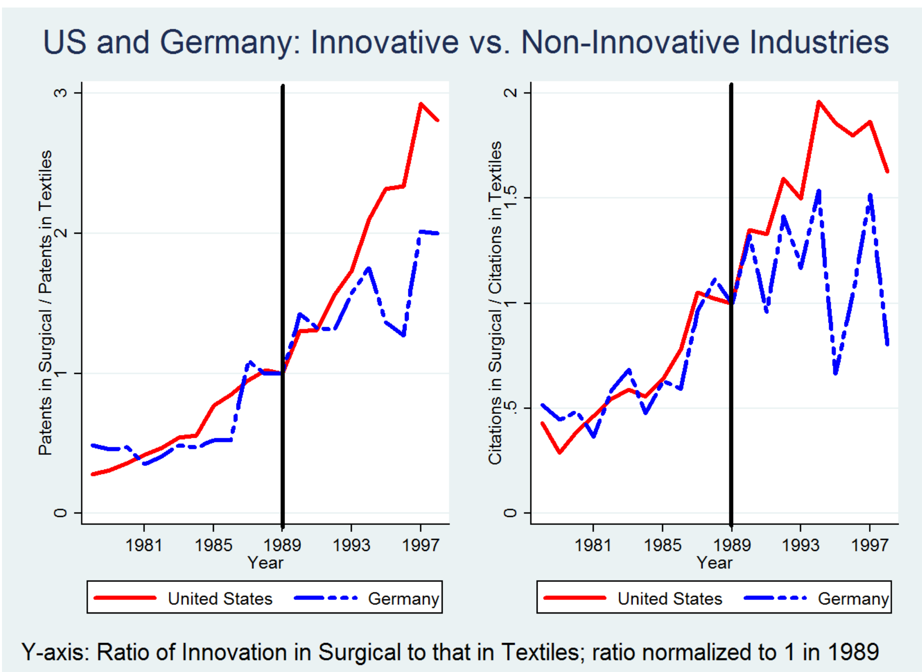


Figure 7: WARN Act and Innovation by U.S. Firms.

This figure shows the linear fit of the number of patents and citations for the treated (firms with $\geq$ 100 employees; continuous line) and control (firms with $<$ 100 employees; discontinuous line) groups before and after the WARN Act became effective (1989). Specifically, the dependent variable is the residual from a regression of the log of patents/ citations on firm dummies.

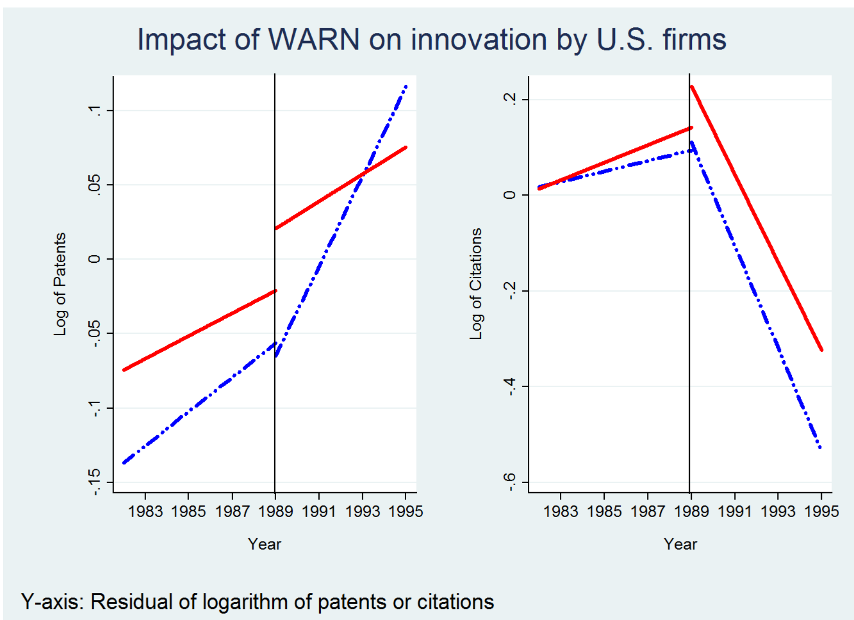


Table 1: Dismissal Law Changes - Detailed Description.

This table shows the sub-components of the Deakin et al. (2007) dismissal law index, and discusses the *changes* that the dismissal laws underwent in the respective countries and years. For more details see Deakin et al. (2007); cited passages are taken from the index description in the latter source.

Law	France	Germany	India	U.K.	U.S.
Legally mandated notice period for all dismissals	No change	No change	Increased from 4 weeks to 12 weeks in 1976	No change	Before 1989, there was no notice period required. In 1989, the notice period was increased to 60 days
Legally mandated redundancy compensation	No change	No change	No change	No change	No change
Minimum qualifying period of service for normal case of unjust dismissal	No change	No change	No change	Before 1972, only workers with $\geq$ 3 years of service qualified for general protection against unjust dismissal. This qualification was progressively reduced to 2 years in 1972, to 1 year in 1974 and 6 months in 1975. Then, this qualification was progressively increased to 1 year in 1979 and to 2 years in 1985. However, it was brought back to 1 year in 1999.	No change
Law imposes procedural constraints on dismissal	Before 1973, there were no procedural constraints on dismissal. In 1973, this law was strengthened to "if the procedural requirements were not followed, the dismissal would be found to be unjust."	Before 2000, there were no procedural constraints on dismissal. Since 2000, dismissal has to be in writing, otherwise the dismissal is void. Failure to follow procedural requirements is one of the factors taken into account in determining whether the dismissal is unjust or not.	No change	Before 1972, there were no procedural constraints on dismissal. In 1972, this law was strengthened to "failure to follow procedural requirement was one of the factors taken into account in determining whether the dismissal was unjust or not." In 1987, the law was further strengthened to "if the procedural requirements were not followed, the dismissal would be found to be unjust."	No change
Law imposes substantive constraints on dismissal	Before 1973, dismissal was permissible if it is 'just' or 'fair' as defined by case law. After 1973, dismissal is justified only in the case of serious misconduct or fault of the employee	No change	Before 1971, there were no substantive constraints on dismissal. After 1971, dismissal was permissible if it is 'just' or 'fair' as defined by case law	Before 1972, there were no substantive constraints on dismissal. After 1972, dismissal is justified only in the case of misconduct, lack of capability, redundancy, etc.	No change
	(continued)	*(continued)*	*(continued)*	*(continued)*	*(continued)*

Table 1: —*continued*

Law	**France**	**Germany**	**India**	**U.K.**	**U.S.**
Notification of dismissal	Before 1973, the law did not require the employer to notify the employee for dismissal. In 1973, the law was strengthened by requiring the employer to provide the employee written reasons for the dismissal. In 1975, the law was further strengthened by requiring the employer to obtain the permission of a state/local body prior to any individual dismissal. In 1986, the law was weakened; now the employer had to only notify the state/ local body prior to an individual dismissal (in contrast to requiring their permission earlier)	Before 1972, the law required the employer to provide the employee written reasons for the dismissal. In 1972, the law was strengthened by requiring the employer to notify the state/ local body prior to an individual dismissal	Before 1976, the law required the employer to notify the state/ local body before an individual dismissal. In 1976, the law was further strengthened by requiring the employer to obtain the permission of a state/ local body prior to any individual dismissal	Before 1972, the law did not require the employer to notify the employee for dismissal. After 1972, the law requires the employer to provide the employee with written reasons for the dismissal	Before 1989, no notification of dismissal was required. In 1989, the law was strengthened to require notification to the state/ local body prior to mass dismissals in the case of firms with more than 100 full-time employees.
Redundancy selection	Before 1975, the law did not require the employer to follow any priority rules in dismissing an employee on grounds of redundancy. After 1975, the law requires the employer to follow priority rules based on seniority, marital status, number or dependants, etc., prior to dismissing an employee for reasons of redundancy.	No change	No change	Before 1974, the law did not require the employer to follow any priority rules in dismissing an employee on grounds of redundancy. After 1974, the law requires the employer to follow priority rules based on seniority, marital status, number or dependants, etc., prior to dismissing an employee for reasons of redundancy	No change
Priority in re-employment	Before 1975, the law did not require the employer to follow any priority rules in re-employing a dismissed employee. After 1975, the law requires the employer to follow priority rules based on seniority, marital status, number or dependants, etc., when re-employing a dismissed employee.	Before 1997, the employer did not have to follow any priority rules in re-employing a dismissed employee. After 1997, the law required the employer to follow priority rules based on seniority when re-employing a dismissed employee.	No change	No change	No change

Table 2: Summary Statistics.

Panel A (Cross-Country Sample) of the table gives summary statistics for the following variables per country: number of patents, number of patenting firms, number of citations, and the dismissal law index. The data span the years 1970–2002.
Panel B (Cross-Country, Firm-Level Sample) shows the summary statistics for the firm-level, cross-country sample. *Tangibility* is Net Property, Plant and Equipment/Total Assets, *Size* is Log(Total Assets), *Market-to-book* is (Total Assets - Book value of Equity + Market value of Equity)/Total Assets, and *Leverage* is Long-Term Debt / Total Assets. The sample spans the years 1987 to 2005.
Panel C (U.S. WARN Sample) of the table gives summary statistics for the main variables used in the single-country U.S. WARN tests. *Market-to-Book* ratio is the market value of assets to total book assets. Market value of assets is total assets plus market value of equity minus book value of equity. The market value of equity is calculated as common shares outstanding times fiscal-year closing price. Book value of equity is defined as common equity plus balance sheet deferred taxes. *Size* is the natural logarithm of sales. The sample spans 1983–1994.
Patent data is from the NBER Patents File (Hall, Jaffe and Trajtenberg, 2001). The labor law index data is from Deakin et al. (2007). Firm-level data is from Compustat.

Panel A: Cross-Country Sample						
United States						
	Obns.	*Mean*	*Median*	*Std. Devn.*	*Min.*	*Max.*
Number of patents	13,291	120.518	72	168.881	1	3,172
Number of patenting firms	13,291	49.122	31	59.590	1	728
Number of citations	13,291	820.045	375	1317.006	0	16,726
Dismissal Law Index	13,291	0.070	0	0.082	0	0.167
United Kingdom						
	Obns.	*Mean*	*Median*	*Std. Devn.*	*Min.*	*Max.*
Number of patents	10,383	8.152	5	12.630	1	297
Number of patenting firms	10,383	5.501	4	6.090	1	90
Number of citations	10,383	44.474	19	72.760	0	1,353
Dismissal Law Index	10,383	0.377	0.407	0.094	0.049	0.444
Germany						
	Obns.	*Mean*	*Median*	*Std. Devn.*	*Min.*	*Max.*
Number of patents	11,722	18.615	10	24.462	1	365
Number of patenting firms	11,722	9.550	6	9.931	1	113
Number of citations	11,722	83.339	39	121.727	0	1,360
Dismissal Law Index	11,722	0.431	0.425	0.018	0.407	0.488
France						
	Obns.	*Mean*	*Median*	*Std. Devn.*	*Min.*	*Max.*
Number of patents	10,277	8.085	5	11.700	1	262
Number of patenting firms	10,277	5.157	3	5.366	1	64
Number of citations	10,277	38.271	17	57.678	0	767
Dismissal Law Index	10,277	0.699	0.746	0.150	0.281	0.782
India						
	Obns.	*Mean*	*Median*	*Std. Devn.*	*Min.*	*Max.*
Number of patents	661	1.852	1	2.222	1	20
Number of patenting firms	661	1.390	1	1.088	1	10
Number of citations	661	4.080	1	8.125	0	88
Dismissal Law Index	661	0.782	0.797	0.040	0.61	.797
Panel B: Cross-Country, Firm-Level Sample						
	Obns.	*Mean*	*Median*	*Std. Devn.*	*Min.*	*Max.*
EBITDA/Total Assets	191,046	-0.629	0.088	63.829	-23956.5	55.345
Log(S_t/S_{t-1})	164,863	0.116	0.082	0.600	-9.369	15.008
Log(R&D Expense)	64,217	1.517	1.497	2.310	-6.908	9.408
Tangibility	192,250	0.270	0.194	0.251	0	2.439
Size	195,928	4.851	4.874	2.703	-6.908	14.278
Market-to-Book	144,373	8.974	1.322	649.688	-0.389	222,021
Leverage	159,717	0.537	0.128	66.625	0	26,246.67
Panel C: U.S. WARN Sample						
	Obns.	*Mean*	*Median*	*Std. Devn.*	*Min.*	*Max.*
Number of patents	13,968	16.473	2	58.933	1	1,612
Number of citations	13,968	172.115	25	700.363	0	21,042
Number of employees (thsd.)	12,822	14.056	2.113	42.476	0	876.8
Market-to-Book	11,648	2.014	1.376	1.870	0.599	12.623
Size	13,142	5.281	5.431	2.557	-1.415	10.274

Table 3: Fixed Effects Regressions using Dismissal Law Index.

The OLS regressions in Columns (1)–(3) implement the following model:
$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \beta X_{ict} + \varepsilon_{ict}$
where y_{ict} is the natural logarithm of a measure of innovation for the USPTO patent class i from country c applied for in year t. $\beta_i, \beta_c, \beta_t$ denote patent class, country and application year fixed effects. β_1 measures the impact of dismissal laws on our innovation proxies. X_{ict} denotes a set of control variables.
The OLS regressions in Columns (4)–(6) implement the following model:
$y_{ict} = t\beta_{j \leftarrow i} + t\beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \beta \cdot X_{ict} + \varepsilon_{ict}$ where $t\beta_{j \leftarrow i}$ denotes a time trend for the industry (patent category) j to which patent class i belongs; $t\beta_c$ denotes a time trend for country c.
The *Creditor Rights Index* is from Djankov, McLiesh, and Shleifer (2007). *Rule of Law*, *Antidirector Rights Index* and the *Efficiency of Judicial System* are time-invariant legal variables (all from La Porta et al., 1998). *Log Imports* is the log of a country's imports from the US in a given 3-digit ISIC industry in a given year; *Log Exports* is the log of a country's exports to the US in a given 3-digit ISIC industry in a given year (export and import data are from Nicita and Olarreaga, 2006). *Ratio of Value Added* is the ratio of value added in the 3-digit ISIC in a year to the total value added by that country in that year (from UNIDO). *Log of per capita GDP* is the logarithm of real GDP per capita. The dismissal index data is from Deakin et al. (2007). Robust standard errors (clustered by country) are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable is Nat. Logarithm of	(1) Number of Patents	(2) Number of Patenting Firms	(3) Number of Citations	(4) Number of Patents	(5) Number of Patenting Firms	(6) Number of Citations
Dismissal Law Index	0.719*	0.820***	1.070**	1.006**	0.875*	1.459**
	(0.396)	(0.283)	(0.513)	(0.504)	(0.329)	(0.726)
Creditor Rights Index	-0.076*	-0.057*	-0.068	-0.038*	-0.021	-0.052**
	(0.031)	(0.021)	(0.035)	(0.015)	(0.016)	(0.011)
Rule of Law	0.128	0.115	0.518	15.462	10.302	14.022
	(0.557)	(0.363)	(0.571)	(7.259)	(5.744)	(7.604)
Antidirector Rights Index	-0.352**	-0.277**	0.145*	-1.924	-0.892	-3.369
	(0.094)	(0.060)	(0.056)	(1.078)	(0.697)	(1.946)
Efficiency of Judicial System	0.623***	0.562***	1.625***	1.782	-2.677	10.541
	(0.057)	(0.039)	(0.188)	(6.069)	(3.959)	(7.954)
Log Imports	-0.016	-0.026	0.011	-0.008	-0.020	0.020
	(0.023)	(0.017)	(0.013)	(0.025)	(0.019)	(0.013)
Log Exports	-0.030*	-0.024	-0.057*	-0.027	-0.023	-0.056*
	(0.011)	(0.015)	(0.023)	(0.016)	(0.019)	(0.025)
Ratio of Value Added	0.020	0.007	0.007	0.026	0.014	0.016
	(0.024)	(0.031)	(0.028)	(0.024)	(0.032)	(0.028)
Log of per capita GDP	0.099	0.119	-0.139	-0.112	-0.103	0.103
	(0.957)	(0.626)	(0.999)	(0.924)	(0.689)	(1.016)
Constant	-8.530	-8.106**	-14.508**	-171.231**	-79.494*	-249.113**
	(4.515)	(2.882)	(4.008)	(44.177)	(28.675)	(55.677)
Patent Class, Country, Application Year FE	Y	Y	Y	Y	Y	Y
Patent Category and Country Trends	N	N	N	Y	Y	Y
Observations	34,381	34,381	31,516	34,279	34,279	31,479
R^2	0.836	0.844	0.820	0.840	0.848	0.822

Table 4: Fixed Effects Regressions using Dismissal Law Index - Robustness.

The OLS regressions below implement the following model:
$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \beta X_{ict} + \varepsilon_{ict}$
where y_{ict} is the natural logarithm of a measure of innovation for the USPTO patent class i from country c applied for in year t. $\beta_i, \beta_c, \beta_t$ denote patent class, country and application year fixed effects. β_1 measures the impact of dismissal laws on our innovation proxies. X_{ict} denotes a set of control variables. *Government*, from the Comparative Political Data Set by Armingeon et al. (2008), captures the balance of power between left and right-leaning parties in a given country's parliament (variable denoted "govparty" in Armingeon et al., 2008). The *Creditor Rights Index* is from Djankov, McLiesh, and Shleifer (2007). *Rule of Law*, *Antidirector Rights Index* and the *Efficiency of Judicial System* are time-invariant legal variables (all from La Porta et al., 1998). *Log Imports* is the log of a country's imports from the US in a given 3-digit ISIC industry in a given year; *Log Exports* is the log of a country's exports to the US in a given 3-digit ISIC industry in a given year (export and import data are from Nicita and Olarreaga, 2006). *Ratio of Value Added* is the ratio of value added in the 3-digit ISIC in a year to the total value added by that country in that year (from UNIDO). *Log of per capita GDP* is the logarithm of real GDP per capita. The dismissal index data is from Deakin et al. (2007). Robust standard errors (clustered by country) are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Data from 1970-2002			Data from 1993-2002		
Dependent Variable is Nat. Logarithm of	(1) Number of Patents	(2) Number of Patenting Firms	(3) Number of Citations	(4) Number of Patents	(5) Number of Patenting Firms	(6) Number of Citations
Dismissal Law Index	0.806**	0.875***	1.088**	1.885**	1.778**	4.991**
	(0.319)	(0.243)	(0.404)	(0.798)	(0.851)	(1.986)
Government	0.017**	0.009	0.033*	0.038***	0.024***	0.088***
	(0.003)	(0.004)	(0.013)	(0.005)	(0.004)	(0.012)
Creditor Rights Index	-0.045	-0.039	-0.036	0.367**	0.333**	0.706**
	(0.021)	(0.018)	(0.023)	(0.083)	(0.087)	(0.201)
Rule of Law	2.360***	1.863***	2.765***	3.298***	2.816***	4.667***
	(0.278)	(0.237)	(0.314)	(0.403)	(0.462)	(0.756)
Antidirector Rights Index	0.004	-0.001	0.060	0.222**	0.214*	0.397**
	(0.044)	(0.037)	(0.049)	(0.067)	(0.073)	(0.088)
Efficiency of Judicial System	0.662***	0.610***	0.771***	0.000	0.000	0.000
	(0.057)	(0.041)	(0.098)	(0.000)	(0.000)	(0.000)
Log Imports	-0.011	-0.022	0.018	0.031	0.054	-0.007
	(0.023)	(0.017)	(0.013)	(0.067)	(0.052)	(0.084)
Log Exports	-0.028	-0.024	-0.057	-0.071	-0.098	-0.040
	(0.015)	(0.018)	(0.026)	(0.054)	(0.042)	(0.042)
Ratio of Value Added	0.025	0.014	0.006	0.107***	0.069*	0.136*
	(0.022)	(0.031)	(0.032)	(0.010)	(0.024)	(0.050)
Log of per capita GDP	-0.934	-0.487	-1.212	-0.226	-0.276	0.330
	(0.611)	(0.503)	(0.697)	(1.513)	(1.371)	(1.094)
Constant	-19.540**	-18.912***	-23.446**	-27.893	-23.251	-53.189**
	(3.918)	(3.067)	(4.710)	(13.371)	(11.623)	(14.571)
Patent Class, Country, Application Year FE	Y	Y	Y	Y	Y	Y
Observations	34,029	34,029	31,333	12,454	12,454	9,999
R^2	0.838	0.845	0.821	0.850	0.861	0.832

Table 5: Difference-in-Difference Tests using the Dismissal Law Index.

The OLS regressions in **Panel A** implement the following model:
$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \varepsilon_{ict}$
where y_{ict} is the natural logarithm of a measure of innovation for the USPTO patent class i from country c applied for in year t. $\beta_i, \beta_c, \beta_t$ denote patent class, country and application year fixed effects. $DismissalLaws_{ct}$ denotes the index of laws governing dismissal in country c in year t. β_1 measures the difference-in-difference effect of the change of the Dismissal Law Index. In this table, we focus on regressions examining "large" changes in dismissal laws in three countries. Columns 1-3 report the results examining the impact of dismissal law changes in the U.K. in the early 1970s; the "control group" is the U.S. Columns 4-6 report the results examining the impact of dismissal law changes in France in the early 1970s; the "control group" is again the U.S., which did not experience such a law change in that time interval.
Panel B, Columns 1-3, reports the results examining the impact of the dismissal law change in the U.S. in 1989; the "control group" is Germany, which did not experience such a law change in the sample period (from 1970-1995). Columns 4-6 of Panel B examine the possibility of reverse causality by following Bertrand and Mullainathan (2003) in decomposing the change in dismissal laws into three separate time periods: *Dismissal Law Change (-2,0)* is a dummy that takes the value of one for the years 1987-1989 for the U.S., zero otherwise; *Dismissal Law Change (1,2)* is a dummy that takes the value of one for the years 1990-1991 for the U.S., zero otherwise; finally, *Dismissal Law Change (≥3)* is a dummy that takes the value of one for the years 1992 and thereafter for the U.S., zero otherwise.
The labor law index data is from Deakin et al. (2007). Robust standard errors (clustered by country) are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

PANEL A

	(1)	(2)	(3)	(4)	(5)	(6)
	UK & US; UK dismissal law changes in early 1970s; data from 1970-1978			**France & US; France dismissal law change in early 1970s; data from 1970-1985**		
Dependent Variable is Nat. Logarithm of	Number of Patents	Number of Patenting Firms	Number of Citations	Number of Patents	Number of Patenting Firms	Number of Citations
Dismissal Law Index	0.149***	0.222***	0.187***	0.376***	0.422***	0.339***
	(0.019)	(0.012)	(0.008)	(0.003)	(0.006)	(0.006)
Constant	1.397**	1.135**	3.013***	4.307***	3.524***	2.665***
	(0.046)	(0.053)	(0.045)	(0.023)	(0.018)	(0.014)
Patent Class, Country, Application Year FE	Y	Y	Y	Y	Y	Y
Observations	6,633	6,633	6,568	11,623	11,623	11,474
R^2	0.923	0.922	0.886	0.913	0.911	0.876

PANEL B

	(1)	(2)	(3)	(4)	(5)	(6)
	Germany & US; US dismissal law change in 1989; data from 1970-1995					
Dependent Variable is Nat. Logarithm of	Number of Patents	Number of Patenting Firms	Number of Citations	Number of Patents	Number of Patenting Firms	Number of Citations
Dismissal Law Index	0.854**	0.692**	1.619***			
	(0.030)	(0.033)	(0.017)			
Dismissal Law Change (-2,0)				-0.132**	-0.119**	-0.014
				(0.003)	(0.004)	(0.007)
Dismissal Law Change (1,2)				0.071**	0.071**	0.218***
				(0.003)	(0.004)	(0.002)
Dismissal Law Change (≥3)				0.173**	0.144**	0.309**
				(0.008)	(0.007)	(0.006)
Constant	4.119**	3.362**	6.188***	4.278***	3.492***	5.695***
	(0.132)	(0.111)	(0.091)	(0.013)	(0.008)	(0.029)
Patent Class, Country, Application Year FE	Y	Y	Y	Y	Y	Y
Observations	20,039	20,039	19,875	20,039	20,039	19,875
R^2	0.848	0.864	0.834	0.848	0.864	0.834

Table 6: Relative Impact of Dismissal Laws on Aggregate Innovation in Different Industries based on their Innovation Intensity.

The OLS regressions below implement the following model:
$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 \cdot DismissalLaws_{ct} * InnovationIntensity_{i,t-1} + \beta_2 \cdot DismissalLaws_{ct} + \beta_3 \cdot InnovationIntensity_{i,t-1} + \beta X_{ict} + \varepsilon_{ict}$
where y_{ict} is the natural logarithm of a measure of innovation for the USPTO patent class i from country c applied for in year t. $\beta_i, \beta_c, \beta_t$ denote patent class, country and application year fixed effects. $DismissalLaws_{ct}$ denotes the index of laws governing dismissal in country c in year t. The Innovation Intensity for patent class i in year $(t-1)$, $InnovationIntensity_{i,t-1}$, is measured as the median number of patents applied by US firms in patent class i in year $(t-1)$. X_{ict} denotes a set of control variables. The *Creditor Rights Index* is from Djankov, McLiesh, and Shleifer (2007). *Rule of Law*, *Antidirector Rights Index* and the *Efficiency of Judicial System* are time-invariant legal variables (all from La Porta et al., 1998). *Log Imports* is the log of a country's imports from the US in a given 3-digit ISIC industry in a given year; *Log Exports* is the log of a country's exports to the US in a given 3-digit ISIC industry in a given year (export and import data are from Nicita and Olarreaga, 2006). *Ratio of Value Added* is the ratio of value added in the 3-digit ISIC in a year to the total value added by that country in that year (from UNIDO). *Log of per capita GDP* is the logarithm of real GDP per capita. The labor law index data is from Deakin et al. (2007). Robust standard errors (clustered by country) are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable is Logarithm of	(1) Number of Patenting Firms	(2) Number of Citations	(3) Number of Patenting Firms	(4) Number of Citations
Dismissal Law Index * Innovation Intensity	0.336***	0.195*	0.338***	0.183**
	(0.052)	(0.116)	(0.038)	(0.051)
Dismissal Law Index	-0.126	0.138	0.452	0.926
	(0.252)	(0.218)	(0.268)	(0.446)
Innovation Intensity	-0.124***	-0.040	-0.123**	-0.067
	(0.021)	(0.034)	(0.044)	(0.057)
Creditor Rights Index			-0.057	-0.054
			(0.030)	(0.042)
Rule of Law			0.311	0.808
			(0.427)	(0.553)
Antidirector Rights Index			0.078	0.170*
			(0.049)	(0.063)
Efficiency of Judicial System			1.224***	1.552***
			(0.111)	(0.182)
Log Imports			-0.035	0.009
			(0.030)	(0.018)
Log Exports			-0.027	-0.055*
			(0.019)	(0.021)
Ratio of Value Added			0.024	0.022
			(0.035)	(0.013)
Log of per capita GDP			0.002	-0.645
			(0.764)	(0.989)
Constant	-3.088***	-2.545**	-14.881**	-12.155**
	(0.545)	(0.613)	(4.412)	(4.145)
Patent Class, Country, Application Year FE	Y	Y	Y	Y
Observations	41,609	38,890	32,265	29,874
R^2	0.831	0.806	0.842	0.823

Table 7: Effect of Dismissal Laws vis-à-vis Other Dimensions of Labor Laws and Triple-Difference Tests.

The OLS regressions in Columns 1–3 below implement the following model:
$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 * lA_{ct} + \beta_2 * lB_{ct} + \beta_3 * lC_{ct} + \beta_4 * lD_{ct} + \beta_5 * lE_{ct} + \beta X_{ict} + \varepsilon_{ict}$
where y_{ict} is the natural logarithm of a measure of innovation for the USPTO patent class i from country c applied for in year t. β_1 - β_5 measure the impact on measures of innovation of the respective labor law for the five components of the labor law index: Alternative employment contracts (lA_{ct}), Regulation of working time (lB_{ct}), Regulation of Dismissal / Dismissal Law Index (lC_{ct}), Employee representation (lD_{ct}), and Industrial action (lE_{ct}). The labor index data is from Deakin et al. (2007).
The regressions in Columns 4 & 5 focus on innovation done by individuals. Tests in Columns 6 & 7 estimate a triple-difference: in Column 6, the dependent variable is the difference between the log of patents filed by firms and the log of patents filed by individuals; the dependent variable in Column 7 is defined analogously, but employs citations.
X_{ict} denotes the usual set of control variables. The *Creditor Rights Index* is from Djankov, McLiesh, and Shleifer (2007). *Rule of Law*, *Antidirector Rights Index* and the *Efficiency of Judicial System* are time-invariant legal variables (all from La Porta et al., 1998). *Log Imports* is the log of a country's imports from the US in a given 3-digit ISIC industry in a given year; *Log Exports* is the log of a country's exports to the US in a given 3-digit ISIC industry in a given year (export and import data are from Nicita and Olarreaga, 2006). *Ratio of Value Added* is the ratio of value added in the 3-digit ISIC in a year to the total value added by that country in that year (from UNIDO). *Log of per capita GDP* is the logarithm of real GDP per capita. Robust standard errors (clustered by country) are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Innovation by Firms			Innovation by Individuals		Innovation by Firms – Innovation by Individuals	
Dependent variable is *ln* of:	Patents	Firms	Citations	Patents	Citations	Patents	Citations
Dismissal Law Index	0.803*	0.839**	1.011*	0.002	0.232	0.805*	0.888**
	(0.403)	(0.280)	(0.504)	(0.036)	(0.232)	(0.346)	(0.293)
Regulation of working time	-0.436	-0.374*	0.359				
	(0.267)	(0.152)	(0.448)				
Alternative employment contracts	-0.070	-0.170*	0.228				
	(0.115)	(0.072)	(0.159)				
Employee representation	0.706*	0.610**	-0.448				
	(0.313)	(0.190)	(0.503)				
Industrial action	-0.393	-0.361	0.833*				
	(0.298)	(0.187)	(0.387)				
Creditor Rights Index	-0.062	-0.046*	-0.022	0.005	0.038	-0.115	-0.158**
	(0.031)	(0.021)	(0.032)	(0.005)	(0.033)	(0.063)	(0.053)
Rule of Law	0.041	0.061	0.424	0.009	-0.004	0.147	0.363
	(0.588)	(0.385)	(0.643)	(0.046)	(0.201)	(0.602)	(0.633)
Antidirector Rights Index	-0.304**	-0.237**	0.143**	-0.031**	-0.057	-0.334**	-0.283*
	(0.080)	(0.053)	(0.036)	(0.009)	(0.040)	(0.112)	(0.115)
Efficiency of Judicial System	0.412**	0.340***	1.962***	0.015	0.013	0.701***	0.832***
	(0.114)	(0.069)	(0.194)	(0.009)	(0.053)	(0.096)	(0.106)
Log Imports	-0.015	-0.026	0.013	0.025	0.058	0.009	-0.102
	(0.023)	(0.017)	(0.013)	(0.028)	(0.051)	(0.047)	(0.129)
Log Exports	-0.030*	-0.024	-0.057*	0.004	0.053	-0.079	-0.277
	(0.013)	(0.015)	(0.024)	(0.029)	(0.071)	(0.063)	(0.131)
Ratio of Value Added	0.020	0.007	0.008	-0.012	-0.018	0.032	0.027
	(0.024)	(0.031)	(0.027)	(0.006)	(0.017)	(0.030)	(0.030)
Log of per capita GDP	0.341	0.296	-0.019	-0.013	0.020	0.032	-0.276
	(1.052)	(0.690)	(1.187)	(0.076)	(0.346)	(1.010)	(1.060)
Constant	-8.441	-7.417*	-18.180***	0.017	-0.328	-6.341	-9.455
	(4.145)	(2.757)	(3.412)	(0.387)	(1.951)	(5.038)	(5.099)
Patent Class, Country, Application year FE	Y	Y	Y	Y	Y	Y	Y
Observations	34,381	34,381	31,516	32,941	32,941	32,941	30,159
R^2	0.837	0.844	0.820	0.146	0.110	0.830	0.795

Table 8: Effect of Labor Laws on Economic Growth.

The OLS regressions below implement the following model:
$y_{ict} = \beta_i + \beta_c + \beta_t + \beta_1 * DismissalLaws_{ct} + \beta_2 * lA_{ct} + \beta_3 * lB_{ct} + \beta_4 * lD_{ct} + \beta_5 * lE_{ct} + \beta X_{ict} + \varepsilon_{ict}$ where i now denotes the 3-digit ISIC industry and y_{ict} denotes the continuously compounded growth rate in value added in industry i in country c in year t; β_1 captures the difference-in-difference effect of the passage of dismissal laws on economic growth. X_{ict} denotes the set of control variables. The specification in Column 5 uses the variable Accounting Standards as a measure of the country's financial development and the variable Financial Dependence, a measure of an industry's external financial dependence; both variables are from Rajan and Zingales (1998). The value added data is obtained from the UNIDO Industrial Statistics database. The description of the other explanatory variables can be found in the previous tables, and will be omitted here to conserve space.
Robust standard errors (clustered by country) are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable is continuously compounded growth in real value added at the 3-digit ISIC level						
Dismissal Law Index$_t$	0.260*	0.266*	0.313*	0.277**	0.395*	-0.251
	(0.106)	(0.105)	(0.100)	(0.078)	(0.168)	(0.360)
Dismissal Law Index$_t$* Innovation intensity				0.015*		
				(0.005)		
Dismissal Law Index$_{t+1}$					-0.152	-0.078
					(0.150)	(0.329)
Dismissal Law Index$_{t-2}$						0.733**
						(0.156)
Alternative employment contracts	-0.054	-0.056	-0.085	-0.080	-0.059	0.003
	(0.107)	(0.110)	(0.125)	(0.118)	(0.047)	(0.090)
Regulation of working time	-0.037	-0.040	-0.160**	-0.140**	-0.190*	0.034
	(0.163)	(0.166)	(0.047)	(0.025)	(0.074)	(0.158)
Employee representation	0.358	0.365	0.454	0.438	0.269***	0.278
	(0.202)	(0.203)	(0.224)	(0.217)	(0.024)	(0.184)
Industrial action	-0.536**	-0.535*	-0.165	-0.183	-0.055	-0.628**
	(0.191)	(0.200)	(0.196)	(0.176)	(0.249)	(0.218)
Creditor Rights Index	-0.053	-0.053	0.051	0.050	0.007	-0.077
	(0.078)	(0.080)	(0.077)	(0.075)	(0.038)	(0.079)
Log of per capita GDP	0.103	0.086	0.115	0.096	0.488	0.091
	(0.223)	(0.219)	(0.344)	(0.286)	(0.339)	(0.263)
Log of imports	0.007***	0.014**	0.013	0.007	0.007	
	(0.001)	(0.005)	(0.017)	(0.003)	(0.004)	
Log of exports	-0.005**	-0.010*	-0.003	-0.003	-0.003	
	(0.002)	(0.004)	(0.006)	(0.003)	(0.005)	
Ratio of value added	0.587***	1.019**	0.676**	0.547***	0.552***	
	(0.084)	(0.291)	(0.151)	(0.065)	(0.080)	
Government			-0.003	-0.003	0.000	
			(0.003)	(0.003)	(0.005)	
Creditor rights index * Innovation intensity				-0.001		
				(0.001)		
Accounting Standards * Financial dependence				-0.001		
				(0.001)		
Accounting Standards * Innovation intensity				0.001		
				(0.001)		
Innovation intensity					0.000	
					(0.000)	
Constant	-0.937	-0.592	-1.285	-1.236	-5.122	-0.905
	(2.425)	(2.299)	(3.585)	(3.052)	(3.667)	(2.772)
Country FE	Y	N	N	Y	Y	Y
Country x ISIC FE	N	Y	Y	N	N	N
ISIC FE	Y	N	N	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	3,252	3,252	2,577	2,577	2,481	3,250
R^2	0.304	0.316	0.476	0.467	0.473	0.310

Table 9: Fixed Effects Regressions using Dismissal Law Index: Firm-Level Outcomes.

The OLS regressions below implement the following model:
$y_{ict} = \beta_i + \beta_t + \beta_1 * DismissalLaws_{ct} + \beta X_{ict} + \varepsilon_{ict}$
where y_{ict} is the dependent variable for firm i in country c and year t. β_i, β_t denote firm and year fixed effects. β_1 measures the impact of dismissal laws on the dependent variables (*Log of R&D Expense* in Columns 1–3; *Sales Growth* in Column 4; *EBITDA / Total Assets* in Column 5). X_{ict} denotes a set of control variables.
The *Creditor Rights Index* is from Djankov, McLiesh, and Shleifer (2007). *Rule of Law*, *Antidirector Rights Index* and the *Efficiency of Judicial System* are time-invariant legal variables (all from La Porta et al., 1998). *Log of per capita GDP* is the logarithm of real GDP per capita. The *Dismissal Law Index* is from Deakin et al. (2007). The firm-level control variables, obtained from Compustat Global, are: *Tangibility* (Net Property, Plant and Equipment/Total Assets), *Size* (Log(Total Assets)), *Market-to-book* (Total Assets - Book value of Equity + Market value of Equity)/Total Assets), and *Leverage* (Long-Term Debt / Total Assets). The dependent variables are also obtained from Compustat Global. Robust standard errors (clustered by country) are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Dependent variable is:	Log(R&D Expense)			$\text{Log}(S_t/S_{t-1})$	EBITDA/Assets
Dismissal Law Index$_t$	0.158**	0.198*	0.143*	0.842***	22.217
	(0.053)	(0.104)	(0.058)	(0.153)	(18.279)
Dismissal Law Index$_{t-1}$			0.415		
			(0.518)		
Dismissal Law Index$_{t+1}$		-0.782			
		(1.378)			
Creditor Rights Index	-0.367*	-0.294	-0.379*	0.043	2.048
	(0.143)	(0.141)	(0.144)	(0.030)	(1.975)
Rule of Law	-0.447	-0.361	-0.423	0.185	6.451
	(0.317)	(0.351)	(0.309)	(0.095)	(5.189)
Antidirector Rights Index	-0.010	-0.028	-0.000	0.098***	1.612
	(0.017)	(0.032)	(0.009)	(0.013)	(0.878)
Efficiency of Judicial System	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log of per capita GDP	0.826	0.822	0.830	0.048	-0.799
	(0.469)	(0.480)	(0.466)	(0.114)	(6.399)
Tangibility	1.322***	1.321***	1.322***	-0.071***	7.811***
	(0.020)	(0.020)	(0.020)	(0.013)	(0.466)
Size	0.631***	0.631***	0.631***	0.157***	4.922***
	(0.014)	(0.014)	(0.014)	(0.006)	(0.339)
Market-to-book	0.000***	0.000***	0.000***	0.000***	-0.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Leverage	0.000***	0.000***	0.000***	-0.003***	-0.209***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
Constant	-5.385**	-6.117***	-5.681**	-3.278***	-93.064**
	(1.503)	(1.291)	(1.489)	(0.247)	(29.288)
Firm and Year FE	Y	Y	Y	Y	Y
Observations	45,707	45,369	45,707	103,910	115,475
R^2	0.933	0.933	0.933	0.149	0.426

Table 10: Regression Discontinuity Test Design: Impact of WARN Act on U.S. Firm-Level Innovation and Employment.

The regressions below implement the following model:
$y_{it} = \beta_i + \beta_t + \beta_1 * (Over100)_{i,1987} * (After1988)_t + \beta X_{it} + \epsilon_{it}$
β_i and β_t are firm and year fixed effects, respectively. Across all three panels, the dependent variables (y_{it}) are: In Column 1, $Ind(Emp_{i,t} - Emp_{i,t-1} < 0)$ is a binary variable taking on a value of one in case of a net employment reduction in firm i from year $t-1$ to year t. In Columns 3 & 4, the dependent variables are (the log of) patents and citations, and in Columns 4 & 5, (the log of) patents and citations scaled by the number of employees. $(Over100)_{i,1987}$ is a dummy variable taking the value of one in each year if a given firm has $\geq$ 100 employees *in 1987*, and zero otherwise; as, for a given firm, this variable does not vary over time, its effect is subsumed in the firm dummies. $(After1988)_t$ is a dummy taking the value of one after the passage of the WARN Act (i.e. the years 1989-1994); this coefficient is subsumed by the year dummies. *Market-to-Book* ratio is the market value of assets to total book assets. *Size* is the natural logarithm of sales. Patent data is from the NBER Patents File (Hall, Jaffe and Trajtenberg, 2001). Firm-level data is from Compustat.
In **Panel A**, the sample is restricted to firms whose 1987 employment is just below or just above the relevant WARN cutoff, i.e. firms with employment between 95 and 105 employees. In **Panel B**, the sample is restricted to firms whose 1987 employment is between 45 and 55 employees. Finally, in **Panel C**, only firms whose employment in the year 1987 is between 145 and 155 are included in the sample.
Robust standard errors (clustered at the firm level) are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

PANEL A: $95 \leq Employment_{i,1987} \leq 105$					
Dependent variable is	(1) $Ind(Emp_{i,t} - Emp_{i,t-1} < 0)$ Linear probability model	(2) Log (Patents)	(3) Log (Citations)	(4) Log(Patents/ Employees)	(5) Log(Citations/ Employees)
$(Over100)_{i,1987} * (After1988)_t$	-0.336**	0.357*	0.536*	1.051**	1.088**
	(0.159)	(0.212)	(0.289)	(0.513)	(0.510)
$(Over100)_{i,1987}$	0.314***				
	(0.118)				
Constant	0.187	1.313***	3.399***	-3.323***	-1.205***
	(0.127)	(0.092)	(0.179)	(0.394)	(0.391)
Firm FE	N	Y	Y	Y	Y
Application Year FE	Y	Y	Y	Y	Y
Observations	279	618	618	511	511
R^2	0.087	0.761	0.704	0.671	0.725

PANEL B: $45 \leq Employment_{i,1987} \leq 55$					
Dependent variable is	(1) $Ind(Emp_{i,t} - Emp_{i,t-1} < 0)$ Linear probability model	(2) Log (Patents)	(3) Log (Citations)	(4) Log(Patents/ Employees)	(5) Log(Citations/ Employees)
$(Over100)_{i,1987} * (After1988)_t$	-0.222	0.054	1.165**	-0.437	0.721
	(0.364)	(0.217)	(0.471)	(0.352)	(0.484)
$(Over100)_{i,1987}$	0.193				
	(0.232)				
Constant	0.205	0.862***	2.695***	-3.454***	-1.657***
	(0.299)	(0.198)	(0.343)	(0.296)	(0.196)
Firm FE	N	Y	Y	Y	Y
Application Year FE	Y	Y	Y	Y	Y
Observations	63	143	143	129	129
R^2	0.204	0.532	0.663	0.799	0.726

PANEL C: $145 \leq Employment_{i,1987} \leq 155$					
Dependent variable is	(1) $Ind(Emp_{i,t} - Emp_{i,t-1} < 0)$ Linear probability model	(2) Log (Patents)	(3) Log (Citations)	(4) Log(Patents/ Employees)	(5) Log(Citations/ Employees)
$(Over100)_{i,1987} * (After1988)_t$	0.658**	-0.397	0.100	-0.581	-0.383
	(0.278)	(0.427)	(0.684)	(0.762)	(1.074)
$(Over100)_{i,1987}$	-0.273				
	(0.191)				
Constant	-0.000	1.273***	3.258***	-2.542***	-0.626
	(0.000)	(0.106)	(0.376)	(0.259)	(0.601)
Firm FE	N	Y	Y	Y	Y
Application Year FE	Y	Y	Y	Y	Y
Observations	43	79	79	73	73
R^2	0.383	0.712	0.811	0.630	0.807

Table 11:
Difference-in-difference Tests: Impact of WARN Act on U.S. Firm-Level Innovation.

The regressions below implement the following model:
$y_{it} = \beta_i + \beta_t + \beta_1 * (Over100)_{i,1987} * (After1988)_t + \beta X_{it} + \epsilon_{it}$
β_i and β_t are firm and year fixed effects, respectively. $(Over100)_{i,1987}$ is a dummy variable taking the value of one in each year if a given firm has $\geq$ 100 employees *in 1987*, and zero otherwise; as, for a given firm, this variable does not vary over time, its effect is subsumed in the firm dummies. $(After1988)_t$ is a dummy taking the value of one after the passage of the WARN Act (i.e. the years 1989-1994); this coefficient is subsumed by the year dummies. *Market-to-Book* ratio is the market value of assets to total book assets. *Size* is the natural logarithm of sales. Patent data is from the NBER Patents File (Hall, Jaffe and Trajtenberg, 2001). Firm-level data is from Compustat.
In **Panel A**, the dependent variables are the natural logarithm of patents, as well as the natural logarithm of citations.
In **Panel B**, the patents and citations are scaled by the number of employees (in thousand) before taking the log.
Robust standard errors (clustered at the firm level) are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

PANEL A

Dependent variable is *ln* of:	(1) Patents	(2) Citations	(3) Patents	(4) Citations	(5) Patents	(6) Citations
$(Over100)_{i,1987} * (After1988)_t$	0.165***	0.435***	0.214***	0.446***	0.060	0.272**
	(0.047)	(0.084)	(0.053)	(0.103)	(0.067)	(0.125)
Size			0.176***	0.148***	0.168***	0.139***
			(0.025)	(0.038)	(0.025)	(0.037)
Size * $(After1988)_t$					0.032***	0.036**
					(0.010)	(0.016)
Market-to-Book			-0.002	-0.003	-0.003	-0.003
			(0.004)	(0.007)	(0.004)	(0.007)
Constant	1.557***	3.415***	0.728***	2.729***	0.777***	2.785***
	(0.021)	(0.038)	(0.130)	(0.200)	(0.129)	(0.198)
Firm & Application Year FE	Y	Y	Y	Y	Y	Y
Observations	13,038	13,038	10,321	10,321	10,321	10,321
R^2	0.879	0.787	0.896	0.810	0.897	0.810

PANEL B

Dependent Variable is *ln* of	(1) Patents/ Employees	(2) Citations/ Employees	(3) Patents/ Employees	(4) Citations/ Employees	(5) Patents/ Employees	(6) Citations/ Employees
$(Over100)_{i,1987} * (After1988)_t$	0.644***	0.929***	0.327***	0.554***	0.164	0.372**
	(0.133)	(0.132)	(0.092)	(0.114)	(0.123)	(0.146)
Size			-0.397***	-0.429***	-0.405***	-0.438***
			(0.028)	(0.040)	(0.029)	(0.040)
Size * $(After1988)_t$					0.034***	0.039**
					(0.012)	(0.016)
Market-to-Book			-0.004	-0.004	-0.005	-0.004
			(0.006)	(0.008)	(0.006)	(0.008)
Constant	-5.950***	-4.098***	-3.998***	-1.978***	-3.945***	-1.919***
	(0.032)	(0.045)	(0.151)	(0.213)	(0.152)	(0.213)
Firm & Application Year FE	Y	Y	Y	Y	Y	Y
Observations	11,687	11,687	10,255	10,255	10,255	10,255
R^2	0.924	0.858	0.939	0.864	0.939	0.864