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Labor Law and Innovation Revisited

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Abstract

This paper examines the impact of changes in job security on corporate innovation in 20 non-U.S. OECD countries. Using a difference-in-differences approach, we provide firm-level evidence that the enhancement of labor protection has a negative impact on innovation. We then discuss possible channels and find that employee-friendly labor reforms induce inventor shirking and a distortion in labor flow. Further investigation reveals that the negative relation is more pronounced in (1) firms that heavily rely on external financing, (2) firms that have high R&D intensity, (3) manufacturing industries, and (4) civil-law countries. Our micro-level evidence indicates that enhanced employment protection impedes corporate innovation.

Keywords

Employment protection laws, Labor law reform, Corporate innovation, Inventor turnover

1. Introduction

Innovation is the engine of a country's long-run economic growth (e.g., Solow, 1957, Romer, 1986, Romer, 1990). At the firm level, Griliches (1981) and Hall (1993) show that high-patent firms are associated with significantly higher stock market valuation. At the aggregate level, Hsu (2009) shows that the growth in patents predicts future stock market returns and premiums. It is therefore vitally important, especially for policy makers, to understand the underlying factors that drive innovation and ascertain their impact. Recent studies show that labor protection is an important factor affecting firm innovation. Nevertheless, the evidence is mixed and far from conclusive. For example, Acharya et al. (2013) explore the impact of dismissal laws on innovation in the United States, the United Kingdom, France, and Germany and conclude that stringent dismissal laws spur innovation. Acharya et al. (2014) find that wrongful discharge laws in the United States that protect employees against unjust dismissal promote innovation. In contrast, Bozkaya and Kerr (2014) find that in European countries, stringent employment protection regulations hinder venture capital investment, which is critical in nurturing innovation in entrepreneurial firms. Using U.S. data, Bradley et al. (2016) find that labor unions, which provide employees with perhaps the strongest form of protection against termination, impede corporate innovation. Moreover, both the European Union and the OECD have prioritized "strengthening innovation" on their agenda and have urged member countries to support entrepreneurial and innovative activities.[1] In this paper, motivated by the ongoing debate and policy implications, we aim to gain a richer understanding of this important issue by examining the relationship between innovation and employment protection laws in 20 OECD countries.

Given the conflicting evidence in the literature, we revisit this issue with two competing hypotheses. Our first hypothesis argues that strong labor protection promotes innovation. There are at least two plausible reasons for such a relationship. First, employment protection laws protect employees against arbitrary dismissal and restrict the terms on which companies can hire workers, thereby providing employees with job security. Manso (2011) argues that the innovation-motivating incentive scheme should include both tolerance for early failure and reward for long-term success, suggesting that job security is an important factor in fostering innovation. Second, the theory on property rights (e.g., Grossman and Hart, 1986, Hart and Moore, 1990) suggests that holdup problems can arise in bilateral relationships when contracts are incomplete. For instance, firm managers could expropriate the payoffs by laying off the employee after a successful innovation. The likelihood of such ex-post holdup problems, in turn, inhibits the employee's ex ante willingness and effort to innovate. Employment protection laws constrain the firm's ability to arbitrarily discharge an employee, thereby reducing the likelihood of holdup problems and incentivizing employees to innovate. Thus, stringent labor protection helps to promote firm innovation.

An alternative hypothesis predicts that labor protection impedes innovation. There are at least four plausible reasons for such a relationship. First, Allard (2005) points out that employment stability resulting from labor protection may lead to employee immobility because labor protection places a limit on the entry of new talents, skills, and ideas into a firm. Similarly, Autor et al. (2007) find that employers are more cautious in hiring new employees and laying off current employees once wrongful discharge laws are adopted. The distorted job flow implies that employers are reluctant to terminate unproductive employees due to high dismissal costs, thereby lowering labor productivity.[2] Second, because strong labor protection lowers the probability of dismissal, it could encourage shirking, resulting in lower levels of innovation effort and output. Bradley et al. (2016) find that labor unions that prevent employees from punishment for shirking impede firm innovation. Third, the strictness of employment protection is negatively related to wage inequality among workers (e.g., Koeniger et al., 2007). The reduced wage gap could alter the landscape of the labor market by forcing out skilled workers, leading to a decline in innovation in countries with stringent labor protection. Last, strict labor regulations may hinder venture capital investment, which is the lifeblood of innovation (e.g., Chemmanur et al., 2014). Bozkaya and Kerr (2014) find that stricter employment regulations cause higher labor adjustment costs than other labor market insurance mechanisms and venture capital investors are especially sensitive to these costs. Thus, stringent labor protection impedes firm innovation.

In this study, we examine whether and to what extent labor protection affects innovation at the firm level. To measure the stringency of employment protection in a country, we create an indicator variable (*EPL_C*) that captures large changes in the employment protection legislation (EPL) index (see Allard, 2005). The EPL index captures intertemporal variations in employment protection across 21 OECD countries from 1950 to 2003. To develop proxies for innovation, we use a data set obtained from the European patent office. We measure a firm's innovation quantity by counting the number of patents applied for and its innovation quality by summing the total number of citations in each firm-year. Our measures of innovation productivity are consistent with the literature (e.g., Kamien and Schwartz, 1975, Griliches, 1990).

We employ a difference-in-differences (DID) method to investigate the impact of EPL on firm innovation. The DID method effectively controls for covariates and allows us to compare innovation outputs between treatment and control groups after a change in a country's stringency of labor protection: firms from countries that experience a change in EPL (treatment group) versus firms from countries that do not experience a change in EPL (control group). The key assumption of the DID method is that, conditional on controls, treated and control firms are only randomly different. Our empirical specification controls for relevant firm and country characteristics as in prior studies. To mitigate the omitted-variable problem, we also control for firm fixed effects to account for the time-invariant firm characteristics and for year fixed effects to absorb systematic period effects such as differences in macroeconomic conditions that may affect all sampled firms' innovation output.

We find that treatment firms experience a 5.10% decrease in innovation quantity and a 5.46% decrease in innovation quality following a major increase in employment protection relative to a set of control firms operating in the same industry at the same time but that are located in countries without changes in employment protection. Our findings support the hypothesis that labor protection could impede innovation. However, it is possible that our results are driven by the differences in pretreatment trends between treatment and control firms. In other words, differences in other dimensions, rather than changes in the stringency of employment protection, between the treatment and control firms could drive our results. To mitigate this concern, we employ a dynamics analysis to examine the timing of the relation between changes in employment protection and innovation. We find (1) there is a significant decrease in innovation one and two years after the passage of EPL; and (2) the coefficient estimates of the 1- and 2-year forward values of EPL are not significant, suggesting that there is no significant change in innovation prior to the passage of EPL.

We propose two channels for our findings. The first channel is inventor shirking. Strong labor protection could encourage shirking due to high dismissal costs, which would in turn lead to a reduction in innovation. Specifically, we find that at the inventor level, there is a significant decrease in innovation productivity following a major increase in the stringency of labor protection. The second channel is distorted job flow. Successful innovation requires new technology as well as inventors equipped with the appropriate skill sets. To maintain innovation performance, firms need to hire talented inventors in a timely manner. However, high dismissal costs distort the job market by discouraging firms from laying off unproductive inventors and hiring skilled ones. The distortion in labor flow leads to an inefficient use of corporate resources and a reduction in value added per worker (e.g., Hopenhayn and Rogerson, 1993, Cingano et al., 2010a). Using the number of new hires and new leavers as proxies for the distortion in labor flow, we find that there is a significant decrease in new hires and new leavers following the enhancement of labor protection. In addition, we find that firms are less likely to hire more productive inventors and less productive inventors are less likely to leave their current jobs after the enhancement of labor protection. Taken together, the results suggest that inventor shirking and distorted job flow after the enhancement of EPL could be the underlying channels through which labor protection affects firm innovation.

We conduct four cross-sectional tests to provide further support for our findings. First, we examine whether the negative impact varies conditional on the reliance on external financing. Rajan and Zingales (1998) show that firms with more growth opportunities are more likely to rely on external financing. Acharya and Xu (2017) find that public firms that are more dependent on external financing exhibit better innovation performance than a sample of matched private firms. Simintzi et al. (2015) find that employment protection reduces corporate financial leverage. Therefore, we predict that the negative impact of EPL on firm innovation should be more pronounced in firms with a strong reliance on external financing. We find supporting evidence for this prediction. Second, given the direct impact of R&D expenditures on corporate innovation, we conjecture that the negative impact of labor protection on innovation is stronger in R&D-intensive firms than in non-R&D-intensive counterparts. The results are consistent with our conjecture. Third, we examine whether the negative impact varies across different industries. The 2008 National Science Foundation Business R&D and Innovation Survey (BRDIS) indicates that firms in manufacturing industries are more innovative than their nonmanufacturing counterparts.[3] We expect the negative impact of labor protection on corporate innovation to be stronger in manufacturing industries. Our findings are consistent with this expectation. Fourth, we examine whether the negative impact differs in civil law versus common law countries. Botero et al. (2004) show that labor laws are generally stronger in civil law countries than common law countries. We find that the negative relation is much stronger for firms in civil law countries than their counterparts in common law countries. Our results are also robust to using an alternative subsample, alternative measures of labor protection stringency and innovation, and alternative empirical specifications. In summary, our evidence supports the hypothesis that stringent labor laws impede firm innovation.

Our paper relates broadly to three strands of the financial economics literature: (1) the literature on the real effect of EPL, (2) the literature on law and innovation, and (3) the literature on labor protection and innovation. First, our paper relates to studies examining the real effects of EPL. For example, literature has shown that stringent EPL reduces corporate investment and productivity (Besley and Burgess, 2004), affects corporate financing decisions (Simintzi et al., 2015), and impedes corporate takeover activities (Dessaint et al., 2017). Our study adds to this line of research by showing that EPL has a negative impact on corporate innovation.

Second, our paper is related to the literature on the role of laws in fostering/stifling innovation. For example, prior studies show that personal bankruptcy law (Fan and White, 2003), debtor-friendly bankruptcy laws (Acharya and Subramanian, 2009), antitakeover laws (Atanassov, 2013, Sapra, Subramanian, and Subramanian, 2014), intellectual property protection laws (Fang et al., 2017) and trade secret laws (Png, 2017) affect innovation. Our paper extends this line of research by examining the impact of labor laws on innovation. The studies closest to ours are those by Atanassov (2013) and Sapra et al. (2014). However our paper is significantly different from these studies on two accounts. First their work examine how the enhanced job security for managers due to the passage of antitakeover laws affects innovation. In contrast our paper focuses primarily on the impact of labor protection for general employees. Second, their research question is whether the threat of hostile takeover as a disciplining mechanism affects managers' incentive to innovate, whereas ours is whether the significant changes in the costs of firing employees and the flexibility of hiring new ones due to labor reforms hinder innovation.

Third, our paper adds to the literature debating the relationship between labor protection and innovation. On the positive side, existing literature finds that labor protection promotes innovation. For example, Acharya et al. (2014) use the staggered adoption of wrongful discharge laws across U.S. states and find that wrongful discharge laws, which reduce the possibility of holdup, spur innovation at the firm level. On the negative side, studies show that labor protection reduces productivity and impedes innovation. For example, Riphahn (2005) provides evidence that provisions against layoff during a probationary period reduce the productivity of new hires by dampening their efforts due to a reduced likelihood of being fired. Bradley et al. (2016) find that labor unions impede corporate innovation and attribute their findings to an ex ante underinvestment in R&D, employee shirking, and a reduction in wage inequality. Our paper contributes to the ongoing debate by documenting a negative impact of EPL on innovation.

Our paper is closely related to Acharya et al. (2013) who find that dismissal laws in the United States, the United Kingdom, France, and Germany limit employers' ability to hold up innovating employees and thereby foster innovation at the country level. Our study is distinct from their work in two important ways. First, we focus on the effect of EPL on individual firms. Given the heterogeneity at the firm level, the positive relation between dismissal laws and innovation at the country level may not generalize to the firm level. Their macro-level evidence may derive from an efficient provision of public goods. In addition, a micro-level probe enables us to exploit heterogeneities across firms to examine the conditions under which the effect of EPL is more pronounced. Second, we focus on 20 OECD countries. Using a large sample with more country-level heterogeneities, we are able to better examine the real effect EPL has on innovation in an international setting. We do not include the United States in our sample for two reasons, First, Fisher et al. (2016) show that there is a clear distinction in labor laws between the United States and the European Union. Namely, labor laws in the United States are employer-friendly while labor laws in the European Union are very protective of employees. Second, the dominance of U.S. firms in terms of the number of firms and patents would raise the concern that our findings are driven solely by U.S. firms.[4]

The remainder of the paper is organized as follows: Section 2 describes sample selection and reports summary statistics; Section 3 discusses our empirical findings; and Section 4 concludes.

2. Sample selection and summary statistics

2.1. Sample selection

We start the sample construction process with the intersection of a European patent database and the Compustat Global database. We collect patent information from the European Patent and Trademark Office (https://www.epo.org/index.html).[5] We merge the patent data with the Compustat Global database, which is the common source for global financial data from 1987 onward. We use fuzzy matching by firm names, carefully inspect all automatic matches, and perform any remaining matches manually. We then keep the observations for 20 OECD countries whose EPL index is available. Our final sample consists of 90,752 firm-year observations, including 13,105 unique firms across 20 countries for the period from 1987 through 2003.

2.2. Variable measurement

2.2.1. Measuring innovation

We extract innovation data from the latest version of the European patent database. The database covers published European patent applications as well as published international applications that are seeking patent protection via the European Patent Office.[6] It also provides detailed information including filing date, granted date, assignee's information, inventor name, inventor location, number of citations received, affiliated company name, and other patent-relevant information. Our choice is driven by two factors. The first factor is sample structure. Sixteen out of twenty OECD countries in this study are European countries, so it is more reasonable to use the patent data compiled by the European Patent Office. The second factor is the validity of patents. Jaffe and Lerner (2004) document that the U.S. Patent and Trademark Office has been issuing too many invalid patents that fail to meet the patentability requirements. Frakes and Wasserman (2017) find that although it has similar patentability requirements, the European Patent Office expends greater resources to scrutinize patent application than the United States.[7]

To measure a firm's quantity and quality of innovation, we construct two variables: (1) the natural logarithm of patents applied; and (2) the natural logarithm of number of citations received for each firm-year.[8] Patent application have on average a 2- or 3-year lag from the time of submission to the patent office until the time it is actually granted. Citations tend to accumulate over a long period of time (e.g., 50 years), but the citations we can observe at best are those received up to 2010. Therefore, we adjust our two measurements of innovation to address this truncation bias. We follow Hall et al., 2001, Hall et al., 2005) and correct the truncation problem by using a fixed-effects approach. We divide the number of patent (citation counts) for each firm-year by the mean number of patents (citation counts) of all firms for that country-year.

2.2.2. Measuring the stringency of labor protection

To measure the stringency of labor protection that varies over time and across countries, we utilize the EPL index in Allard (2005), which covers eighteen aspects of employment protection legislation grouped into three broad categories: laws protecting workers with regular contracts, laws protecting workers with temporary contracts, and regulations applying to collective dismissals.[9] Our choice of EPL index is driven by its superior performance in the following three aspects, when compared to other indices for employment protection such as Botero et al. (2004) index and Deakin et al. (2007) index. First, the EPL index comprehensively measures all country-level changes in EPL from 1950 to 2003, enabling us to explore the within-country correlation between changes in labor protection and corporate innovation. In contrast, Botero et al. (2004) index only measures the stringency of employment protection in 1997. Second, the EPL index covers all aspects of employment protection legislation across 21 OECD countries. In contrast, Deakin et al. (2007) index is only available for five countries: the United States, the United Kingdom, France, Germany, and India. Third, the EPL index has been widely used in studies that examine the economic impact of employment protection such as Alimov, 2005, Simintzi et al., 2015, Dessaint et al., 2017, and Subramanian and Megginson (2018).

To capture either a positive or negative effect of any labor law changes, we follow the spirit of Simintzi et al. (2015) and create a variable, *EPL_C*, which equals 1 (0) after (before) the EPL index increases in a country-year and equals −1 (0) after (before) the EPL index decreases in a country-year. As noted in the construction of the EPL index in Allard (2005), many countries experienced trivial changes that may not have a significant effect on labor protection. To better gauge the impact of labor protection on innovation, we only consider changes in the EPL index whose absolute value is greater than 0.2 (the absolute mean value of the change of the EPL index). If a country experiences more than two changes of EPL in the opposite direction during our sample period, we include the largest change only. As a robustness check, we also use the EPL index in Allard (2005), which is labeled as *EPL_A.* Higher values of *EPL_A* indicate more stringent labor protection for workers. It is notable that the main difference between *EPL_C* and *EPL_A* is that *EPL_C* focuses on large changes in EPL, whereas *EPL_A* considers all changes. The results for *EPL_A* are reported in Table 7, Panel D.

2.2.3. Measuring other control variables

In measuring the effect of labor protection on innovation, we include an extensive set of control variables. At the firm level, the control variables include the natural logarithm of firm assets in U.S. dollars (*LnAssets*), return on assets (*ROA*), market-to-book ratio (*MB*), property, plant, and equipment scaled by assets (*Tangibility*), firm leverage (*Leverage*), R&D expenses scaled by assets (*R&D*), and Herfindahl-Hirschman Index based on the two-digit SIC code (*HHI*). The choice of these variables is based on the existing innovation literature. For example, Beck et al., 2005, Beck et al., 2008) and find that firm size plays a critical role in shaping a firm's long-term growth. Small firms are generally more financially constrained and are therefore less likely to undertake costly innovative projects given their uncertain outcome. Therefore, we use the natural logarithm of firm assets as a proxy for the uncertainty level in conducting risky projects. Growth firms generally rely extensively on external financing (e.g., Rajan and Zingales, 1998). In addition, the success of an innovative project is highly correlated with financing ability and sustainability of the firm. We use the market-to-book ratio to control for a firm's growth opportunity. Fang et al. (2014) raise the question about the suitability of R&D expenditures as a proxy for innovation productivity. Bradley et al. (2016) further differentiates R&D expenditures as input for innovation from patents and citations that are output for innovation. Therefore, we include R&D expenditures scaled by assets in our regression. Aghion et al. (2005) provide empirical evidence that market competition and innovation exhibit a U-shaped relationship. On the contrary, Hashmi (2013) finds a mild negative relationship between competition and innovation. To account for the effect of market competition on innovation, we use the Herfindahl-Hirschman Index as a proxy for the degree of product market competition.

At the country level, the control variables include the natural logarithm of GDP per capita (*LnGDP_Capita*), the natural logarithm of country-level cumulative patents in the past 5 years (*LnPatentStock5*), public spending on secondary and tertiary education scaled by GDP (*Ed_Share*), the intellectual property protection index (*IP*), international trade (*Trade*), an indicator for the political orientation of the ruling party (*Right*), and the disproportionality of the electoral system (*Disp_Index*). The inclusion of the above macro-level variables follows the existing literature. For example, Furman et al. (2002) find that a country's knowledge stock is critical for fostering innovation. They use GDP per capita to capture the ability of a country to translate its knowledge stock into economic development. GDP per capita also captures the variations in macroeconomic conditions across countries. The stock of international patents (*LnPatentStock5*) is employed to directly measure a country's pool of new technology. Furman et al. (2002) also suggest that the intensity of human capital investment and the strength of protection for intellectual property could affect the national innovative capacity. Following their paper, we also include the fraction of GDP spent on secondary and tertiary education (*Ed_Share*) and an intellectual property protection index (IP) in our baseline regressions. MacGarvie (2006) finds that a country's international trade is a conduit for the diffusion of technological knowledge and is correlated with citations of that country's patents. We add *trade* as a control; it is constructed by taking the difference between the level of imports and the level of exports scaled by GDP in a country-year. Evidence in Krozner and Strahan (1999) suggests that political economy variables are linked to regulatory changes. Perotti and Von

Thadden (2006) argue that labor market structures are usually shaped by political decisions. To measure political environment, we use an indicator variable (Right) for the ideology of the political party in power that equals one if it is right leaning and zero otherwise. This variable essentially captures the sentiment in the country toward labor protection and the current government's leaning toward passing economic regulations that could affect innovation. In addition, Pagano and Volpin (2005) argue that weak employment protection is likely to occur in countries with majoritarian rather than proportional electoral rules. We therefore further control for the disproportionality of the electoral system (Disp_Index) in a country by using the c Index developed by Gallagher and Mitchell (2008). In our robustness analysis, we consider several possible omitted variables. Acharya et al. (2013) argue that changes in labor laws may be correlated with business cycles in a country. Ayyagari et al. (2010) find that in emerging countries, corruption is detrimental to innovation. Therefore, we control for annual GDP growth (*GDP_Growth*), unemployment rates (*Unemployment*), country-level corruption (*Corruption*), and an indicator of recessions (*Recession*).[10] This indicator equals one if a country experiences two consecutive quarters with negative GDP growth and zero otherwise. All variables, except for those normalized by the natural logarithm, are winsorized at the first and ninety-ninth percentile value. Detailed definitions of all variables are presented in Appendix B.

2.3. Summary statistics

Table 1 provides the summary statistics for all variables during our sample period. The EPL index (*EPL_A*) varies from 0.500 (lowest) to 4.100 (highest), and the mean and median values are 1.664 and 1.400, respectively. This wide range in the EPL index indicates a large variation in the strictness of labor laws across countries. The mean value of *EPL_C* is −0.172. On average, a firm files 2.464 patents and receives 6.419 citations. The mean value of total assets is about $2012 million. The proportion of R&D expenditures represents about 1.1% of total assets.

Table 1. Descriptive statistics.

Variable	**Mean**	**Median**	**Std Dev**	**Min**	**Max**
Pat (Patent)	2.464	0.000	27.600	0.000	1729.000
Cite (Citation)	6.419	0.000	69.672	0.000	3572.000
EPL_C	−0.172	0.000	0.652	−1.000	1.000
EPL_A	1.664	1.400	0.636	0.500	4.100
Assets (US $ million)	2012.670	246.371	6799.470	1.668	53053.540
ROA	0.027	0.046	0.145	−0.819	0.288
MB	1.553	0.586	3.017	0.000	20.703
Tangibility	0.315	0.280	0.236	0.000	0.937
Leverage	0.228	0.206	0.183	0.000	0.729
R&D	0.011	0.000	0.035	0.000	0.236
HHI	0.277	0.176	0.269	0.020	1.000
LnGDP_Capita	10.194	10.205	0.264	8.709	10.822
LnPatentStock5	9.744	9.924	1.424	3.135	11.563
Ed_Share	0.408	0.395	0.094	0.105	0.802
IP	4.286	4.420	0.433	1.670	4.670
Trade	1.352	1.324	2.966	−11.101	17.184
Right	0.557	1.000	0.497	0.000	1.000
Disp_Index	10.635	10.870	5.253	0.420	25.250

This table presents summary statistics for the key variables used in this study. The sample is constructed from the intersection of the European Patent Database and Compustat Global, after imposing requisite data requirements. The sample consists of 90,752 firm-year observations in 13,105 unique firms across 20 OECD countries from 1987 to 2003. Detailed definitions of all variables are provided in Appendix B. All variables, except for those normalized by natural logarithm, are winsorized at the one percent level at each tail.

In Table 2, we report the distribution of EPL stringency and innovation output by country. The most innovative country in our sample is Germany, where on average, a firm files 8.978 patents and receives 18.152 citations. Additionally, the mean values of *EPL_A* and *EPL_C* are 2.242 and −0.703, respectively. On the other hand, the least innovative country is Portugal, which reported no successful patent applications during our sample period. In general, European countries have more stringent employment protection than other OECD countries. For example, Greece has the strongest job security for workers (*EPL_A* = 3.8), whereas New Zealand has the weakest employee protection (*EPL_A* = 0.737).

Table 2. EPL indicators and innovation by Country.

Country Name	**N**	***EPL_C***	***EPL_A***	***Patent***	***Citation***
Australia (AUS)	5236	0.903	1.154	0.048	0.152
Austria (AUT)	714	0.000	2.546	0.417	0.819
Belgium (BEL)	854	0.662	2.497	1.724	4.412
Canada (CAN)	13,346	0.000	1.200	0.266	0.829
Switzerland (CHE)	1731	0.886	1.431	3.284	10.070
Germany (DEU)	5275	−0.703	2.242	8.978	18.152
Denmark (DNK)	1097	−0.981	1.613	2.006	8.237
Spain (ESP)	1314	−0.762	2.525	0.067	0.116
Finland (FIN)	893	0.000	2.300	6.727	30.010
France (FRA)	4957	0.998	2.993	2.407	5.534
U.K. (GBR)	17,086	0.000	1.343	0.680	2.493
Greece (GRC)	703	0.000	3.800	0.003	0.000
Ireland (IRL)	694	−0.412	1.425	1.784	5.376
Italy (ITA)	1653	−0.728	3.381	0.893	1.815
Japan (JPN)	29,298	−0.650	1.435	4.207	11.022
Netherland (NLD)	2035	0.532	2.275	1.267	2.973
Norway (NOR)	1045	0.000	2.716	0.810	1.858
New Zealand (NZL)	596	0.000	0.737	0.025	0.065
Portugal (PRT)	398	−0.990	3.704	0.000	0.000
Sweden (SWE)	1827	−0.948	2.787	2.031	5.186

This table presents the country distribution of EPL indicators (*EPL_C* and *EPL_A*) and firm innovation measures (*Patent* and *Citation)*. See Appendix I for all variable definitions and descriptions.

3. Empirical results

3.1. Baseline regression results

In the baseline regression model, we utilize a difference-in-differences (DID) method, which allows us to compare innovation output between treatment and control groups after a change in a country's EPL index (either tightening or relaxation). That is, firms from countries that experience a change in the EPL index (treatment group) versus firms from countries that do not experience a change in the EPL index (control group). Inspired by Simintzi et al. (2015), we specify the DID regression model as follows:

$$LnPat(orLnCite)_{i,t+N} = \alpha + \mu_i + \delta_t + \beta EPL\_C_{k,t} + \theta_i X_{i,t} + \epsilon_{i,t+N},$$

where i denotes a firm, t denotes a year, and k is a country. The dependent variable $LnPat(orLnCite)_{i,t+N}$ is the measure of a firm's innovation output in year $t + N$ ($N = 0, 1, 2$); *EPL_C* is the indicator for major changes in the EPL index; the key variable of interest; $X_{i,t}$ is the vector of control variables; α is the constant; μ_i is the firm fixed effects; δ_t is the year fixed effects; and $\epsilon_{i,t+N}$ is the error term. The vector $X_{i,t}$ includes both firm- and country-level control variables as described in the previous section. Standard errors are clustered at the country level because labor laws change at the country level.

The results of the baseline regression are reported in Table 3. The coefficients of *EPL_C* are all negative and statistically significant at least at the 5% level. Our results are also economically nontrivial. For example, in Column (1), we find that on average, patent counts for firms in the treatment group decline (increase) by 5.1% relative to those in the control group after a tightening (relaxation) in the stringency of employment protection. In Column (3), we also find consistent evidence showing that the number of citations received significantly decreases (increases) by 5.46% for the treatment group after the EPL index increases (declines). These results suggest that employee-friendly labor reform hinders corporate innovation in both quantity and quality.

Table 3. The impact of labor protection on firm innovation.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	LnPat $_{t+N}$			***LnCite*** $_{t+N}$		
	***N* = 0**	***N* = 1**	***N* = 2**	***N* = 0**	***N* = 1**	***N* = 2**
EPL_C	−0.0510**	−0.0537***	−0.0516***	−0.0546***	−0.0564***	−0.0511***
	(−2.72)	(−3.13)	(−3.05)	(−3.43)	(−3.38)	(−3.08)
LnAssets	0.0343**	0.0337**	0.0312**	0.0343**	0.0313**	0.0270**
	(2.55)	(2.80)	(2.85)	(2.75)	(2.60)	(2.68)
ROA	−0.0046	0.0270*	0.0146	−0.0064	0.0328*	0.0212
	(−0.25)	(1.74)	(0.75)	(−0.35)	(2.00)	(1.17)
MB	0.0026***	0.0028***	0.0024***	0.0022**	0.0025***	0.0021***
	(2.92)	(5.78)	(3.58)	(2.76)	(4.93)	(3.13)
Tangibility	−0.0216	−0.0254	−0.0300	−0.0094	−0.0176	−0.0231
	(−1.25)	(−1.09)	(−1.22)	(−0.53)	(−0.86)	(−1.22)
Leverage	−0.0934*	−0.0916**	−0.0813***	−0.0829**	−0.0850**	−0.0734***
	(−1.96)	(−2.81)	(−3.23)	(−2.12)	(−2.42)	(−2.87)
R&D	0.8614***	0.6356*	0.3714	0.9321***	0.6158**	0.3402
	(3.55)	(1.85)	(1.02)	(2.95)	(2.10)	(1.07)
HHI	0.0064	0.0227	0.0179	0.0153	0.0285	0.0294
	(0.24)	(0.99)	(0.76)	(0.87)	(1.45)	(1.26)
LnGDP_Capita	0.0267	0.0219	0.0316	0.0052	0.0161	0.0264
	(0.68)	(0.56)	(0.84)	(0.13)	(0.41)	(0.75)
LnPatentStock5	−0.0117	−0.0092	−0.0051	0.0071	−0.0056	0.0033
	(−0.27)	(−0.25)	(−0.14)	(0.20)	(−0.17)	(0.10)

Ed_Share	0.0410	−0.0866	−0.0810	−0.0774	−0.0922	−0.1081
	(0.56)	(−1.25)	(−0.93)	(−1.47)	(−1.56)	(−1.45)
IP	−0.0410	−0.0234	−0.0239	−0.0273	−0.0265	−0.0280
	(−1.71)	(−0.95)	(−1.03)	(−1.52)	(−1.51)	(−1.29)
Trade	−0.0021	−0.0025	−0.0024	−0.0029	−0.0025	−0.0033
	(−0.98)	(−1.21)	(−1.03)	(−1.58)	(−1.21)	(−1.54)
Right	−0.0055	0.0020	0.0038	−0.0003	−0.0012	0.0026
	(−0.68)	(0.24)	(0.45)	(−0.03)	(−0.14)	(0.33)
Disp_Index	−0.0007	0.0003	−0.0000	0.0008	0.0000	−0.0003
	(−0.45)	(0.18)	(−0.01)	(0.66)	(0.01)	(−0.17)
Constant	−0.1441	−0.1128	−0.2220	−0.1118	−0.0684	−0.2075
	(−0.38)	(−0.30)	(−0.57)	(−0.36)	(−0.20)	(−0.62)
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Observations	90,752	90,752	90,752	90,752	90,752	90,752
Adj. R-squared	0.687	0.791	0.797	0.772	0.776	0.782

This table presents the baseline results of the difference-in-differences (DID) regression model. In columns (1), (2), and (3), the dependent variable is LnPat_{t+N}, the natural logarithm of patents applied for by a firm in year t + N (N = 0, 1, 2). In columns (4), (5), and (6), the dependent variable is Ln$Cite_{t+N}$, the natural logarithm of citations received by a firm in year t + N (N = 0, 1, 2). All variable definitions are given in Appendix B. Each regression includes firm and year fixed effects. Below the coefficient estimates in parentheses are *t*-values adjusted for heteroscedasticity and country-level clustering. ***, **, and * indicate significance at the 1% 5%, and 10% levels, respectively.

Our findings contrast with those in Acharya et al. (2013). To identify what drives the inconsistency, we replicate their baseline results using European patent data. The results are reported in Panel A, Columns (1) and (2) of Appendix C. Consistent with their findings, the stringency of dismissal laws has a positive impact on innovation. For the sake of direct comparison, in Columns (3) and (4) we replace the dismissal law index with the EPL index in Allard (2005) and find that at the country level, labor protection also positively affects innovation. However, when we extend the sample by including another 17 OECD countries, as shown in Columns (5) and (6), the sign of the coefficient estimates of EPL becomes negative, suggesting that the addition of more heterogeneous countries could at least partially contribute to the reverse findings in Acharya et al. (2013). We also examine whether the country-level evidence in Acharya et al. (2013) can be generalized to the firm level. Specifically, using a large sample of firms in the United States, the United Kingdom, Germany, and France, we rerun our baseline DID regression model and find that EPL continues to have a negative impact on innovation, whereas dismissal laws are no longer effective in fostering innovation (See Appendix C, Panel B). The results suggest that 1) the dismissal law index is not as effective as the EPL index in accounting for the firm-level heterogeneity; and 2) firm-level heterogeneity within a country and across countries could also be contributing to the contrasting findings between the two studies.

3.2. Dynamic model

The advantage of the DID method in Table 3 is that it allows us to directly compare the change in innovation in firms that are subject to labor law reforms (treated firms) with the change in innovation in firms that do not experience such reforms (control firms). Nevertheless, one remaining concern is that our results are driven by pretreatment differences in the characteristics of treated and control firms. In other words, differences in other dimensions, rather than changes in labor protection between the treatment and control firms could be driving our results.

To address this concern, we employ a dynamic model that enables us to examine the dynamics of innovation in years around the changes in labor protection laws. As such, we follow Bertrand and Mullainathan (2003) and Simintzi et al. (2015) and include lead and lags of the testing variables in our DID specification. More specifically, we replace *EPL_C* with five variables: *EPL_C* (+2) is the 2-year forward value of *EPL_C; EPL_C* (+1) is the 1-year forward value of *EPL_C; EPL_C* (0) is the contemporaneous value of *EPL_C; EPL_C* (−1) is the 1-year lagged value of *EPL_C*; and *EPL_C* (−2) is the 2-year lagged value of *EPL_C*. We also include other control variables and fixed effects as in the baseline DID regression model. Standard errors are still clustered at the country level. The results in Table 4 show that there are no changes in innovation output prior to labor law reforms because the coefficient estimates of *EPL_C* (+2) and *EPL_C* (+1) are statistically insignificant. Therefore, there is no evidence suggesting that our results are driven by pretreatment trends and reverse causality. In contrast, the coefficient estimates of *EPL_C* (−2), *EPL_C* (−1) and *EPL_C* (0) are negative and significant, indicating that changes in labor protection affect innovation output. Taken together, our findings suggest that employee-friendly labor laws impede corporate innovation. This evidence is consistent with the findings of Autor et al., 2007, Bassanini et al., 2009, and Calcagnini et al. (2014), which show that strong employment protection leads to a reduction in corporate productivity.

Table 4. Dynamic analysis.

	(1)	**(2)**
	LnPat	***LnCite***
***EPL_C* (+ 2)**	0.0013	0.0015
	(0.20)	(0.27)
***EPL_C* (+ 1)**	0.0029	0.0020
	(1.18)	(0.80)
***EPL_C* (0)**	−0.0370**	−0.0393***
	(−2.26)	(−2.90)
***EPL_C* (−1)**	−0.0256***	−0.0284***
	(−3.04)	(−2.96)
***EPL_C* (−2)**	−0.0052***	−0.0050**
	(−2.93)	(−2.60)
LnAssets	0.0415**	0.0408**
	(2.41)	(2.62)
ROA	−0.0102	−0.0143
	(−0.49)	(−0.70)
MB	0.0022***	0.0014**
	(3.14)	(2.81)
Tangibility	−0.0202	−0.0060
	(−0.84)	(−0.27)
Leverage	−0.1177*	−0.1012**
	(−1.97)	(−2.26)
R&D	0.8225**	0.8611**

	(2.38)	(2.10)
HHI	−0.0022	0.0109
	(−0.08)	(0.58)
LnGDP_Capita	0.0221	−0.0013
	(0.56)	(−0.03)
LnPatentStock5	−0.0137	−0.0178
	(−0.25)	(−0.41)
Ed_Share	0.1134	0.0146
	(1.33)	(0.25)
IP	−0.0430	−0.0277
	(−1.50)	(−1.26)
Trade	−0.0036	−0.0034
	(−1.67)	(−1.59)
Right	−0.0108	−0.0059
	(−1.22)	(−0.72)
Disp_Index	−0.0018	−0.0015
	(−1.08)	(−1.14)
Constant	−0.0394	0.2726
	(−0.08)	(0.65)
Firm FE	YES	YES
Year FE	YES	YES
Observations	76,014	76,014
Adj. R-squared	0.703	0.789

This table reports the results of regressions of firm innovation on the two-year lagged, one-year lagged, the contemporaneous, and the one-year and two-year forward values of the *EPL_C* indicator. The dependent variables are Ln*Pat* and Ln*Cite* in columns (1) and 2), respectively. All variable definitions are given in Appendix B. Each regression includes firm and year fixed effects. Below the coefficient estimates in parentheses are *t*-values adjusted for heteroscedasticity and country-level clustering. ***, **, and * indicate significance at the 1% 5%, and 10% levels, respectively.

3.3. Potential channels

In this subsection, we investigate potential channels through which changes in labor protection could impact firm innovation. We first test innovation productivity before and after EPL-index changes at the individual inventor level. Riphahn (2005) shows that an “anti-layoff” clause during probationary period causes lower productivity induced by shirking. Cingano et al. (2010b) document that an increase in EPL leads to a reduction in a firm's productivity, measured as investment per worker and capital per worker. Bradley et al. (2016) also support this view by showing that labor unionization discourages employees from expending effort on innovation because of the lowered probability of dismissal. We conjecture that strong job security leads to inventor shirking and thus decreases innovation.

Following Bernstein (2015), we classify an inventor as *Stayers* if he/she does not change employment affiliation during our sample period. We then delete stayers who only produce one patent during our sample period and the stayers who only have one stayer-year observation. The stayers from the treatment (control) group must have at least a 5-year patent invention history before and after labor law reforms. We then aggregate the total number of patents invented by each stayer and the total number of citations received by those patents. By limiting our sample to *Stayers*, we are able to test the effect of labor law reforms on stayers’ innovation productivity. The model specification is the same as the baseline DID regression model except that we replace firm fixed effects with inventor fixed effects to account for the impact of inventors' characteristics on innovation. The results are reported in Table 5, Panel A. In Column (1), where the dependent variable is the natural logarithm of patents (*LnPat*), the coefficient estimate of *EPL_C* is −0.0207 and is statistically significant at the 1% level, indicating that stringent labor laws have a negative impact on innovation performance of *Stayer*. In Column (2), we replace the natural logarithm of patents (*LnPat*) with the natural logarithm of citations (*LnCite*) and rerun the regression. We find consistent results. More specifically, we find that the number of citations significantly decreases by 5.16% following the tightening of EPL. These results are consistent with those reported by Bassanini et al. (2009), which show that due to high dismissal costs, strong employment protection laws reduce labor productivity as measured by aggregate total factor productivity.

Table 5. Potential channels: inventor-level evidence.

			(1)	**(2)**
			LnPat	***LnCite***
Panel A: Innovation of *Stayers*				
EPL_C			−0.0207***	−0.0516**
			(−3.38)	(−2.23)
LnAssets			0.0025	−0.0138
			(0.25)	(−1.09)
ROA			0.0017	0.2200
			(0.02)	(1.69)
MB			0.0001	0.0013
			(0.09)	(0.52)
Tangibility			−0.0878	−0.2473
			(−1.03)	(−1.28)
Leverage			−0.0374*	−0.0403
			(−1.80)	(−0.69)
R&D			−0.2927	−0.0907
			(−1.32)	(−0.16)
HHI			0.0380	−0.0603
			(0.79)	(−0.50)
LnGDP_Capita			−0.0697***	−0.0831**
			(−4.80)	(−2.68)
LnPatentStock5			0.0766*	0.1938*
			(1.88)	(1.80)
Ed_Share			0.2752***	0.4217***
			(3.13)	(2.96)
IP			0.0003	−0.0118
			(0.03)	(−0.44)
Trade			−0.0015	0.0045*
			(−1.70)	(2.07)
Right			0.0107***	0.0209**

			(4.92)	(2.25)
Disp_Index			0.0010	0.0065
			(0.56)	(1.24)
Constant			0.2998	0.0827
			(0.87)	(0.09)
Inventor FE			YES	YES
Year FE			YES	YES
Observations			48,326	48,326
Adj. *R*-squared			0.261	0.224
	(1)	(2)	(3)	(4)
	LnNewHires	*LnLeavers*	*LnNewHires_Productive*	*LnLeavers_Unproductive*
Panel B: inventor turnover				
EPL_C	−0.1311***	−0.0287***	−0.0509**	−0.0683***
	(−3.33)	(−4.69)	(−2.80)	(−4.62)
LnAssets	0.0568	−0.0103*	0.0722*	−0.0079
	(1.46)	(−1.98)	(1.78)	(−0.63)
ROA	0.0408	0.0020	−0.0112	−0.0333**
	(1.21)	(0.20)	(−0.67)	(−2.13)
MB	0.0030**	0.0000	0.0008	−0.0015
	(2.41)	(0.07)	(0.74)	(−1.70)
Tangibility	−0.0976	−0.0271	−0.1409*	−0.0408
	(−1.14)	(−1.36)	(−1.78)	(−1.00)
Leverage	−0.0821*	0.0104	−0.0564*	0.0274
	(−1.88)	(1.23)	(−1.86)	(0.71)
R&D	0.8801*	0.0472	0.4874	−0.2324
	(1.96)	(1.01)	(1.41)	(−1.41)
HHI	0.1210**	0.0066	0.0787	−0.0181
	(2.26)	(0.97)	(1.17)	(−0.54)
LnGDP_Capita	−0.0318	0.0300	−0.1081*	−0.0568
	(−0.33)	(1.63)	(−1.92)	(−1.19)
LnPatentStock5	0.0618	−0.0115	0.0535	−0.0317
	(0.55)	(−0.73)	(0.88)	(−0.59)
Ed_Share	0.4862**	0.1413***	0.4399*	0.1771
	(2.22)	(3.80)	(1.79)	(1.09)
IP	−0.0507	0.0171	−0.0831**	−0.0321
	(−0.89)	(1.63)	(−2.72)	(−0.93)
Trade	−0.0063	−0.0014	0.0067	0.0081**
	(−1.16)	(−1.12)	(1.42)	(2.84)
Right	0.0174	−0.0015	−0.0082	−0.0152
	(0.87)	(−0.36)	(−0.48)	(−1.09)
Disp_Index	0.0108	0.0024	0.0056	0.0036
	(1.57)	(1.72)	(1.55)	(1.53)

Constant	−0.6672	−0.2519	0.5221	1.0535
	(−0.72)	(−1.42)	(0.80)	(1.61)
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	26,311	26,311	26,311	26,311
Adj. R-squared	0.454	0.039	0.562	0.048

This table reports the results for channels tests of inventor shirking and labor market distortion. In Panel A, the dependent variable is the innovation performance of the *Stayers. Stayers* are defined as inventors who have not changed their affiliation during our sample period. In column (1), the dependent variable is Ln*Pat*, which is the natural logarithm of patents applied by a firm's *Stayers* in a year. In column (2), the dependent variable is Ln*Cite*, the natural logarithm of citations received by a firm's *Stayers* in a year. Each regression includes inventor and year fixed effects. In Panel B, the dependent variables are proxies for inventor turnover in the labor market. In Column (1), the dependent variable is Ln*NewHires*, the natural logarithm of one plus the number of newly hired inventors for a firm-year. In column (2), the dependent variable is Ln*Leavers,* the natural logarithm of one plus the number of inventors left in a firm-year. In column (3), the dependent variable is Ln*NewHires_Productive*, the natural logarithm of one plus the number of newly hired productive inventors for a firm-year. In column (4), the dependent variable is Ln*Leavers_Unproductive,* the natural logarithm of one plus the number of unproductive inventors left in a firm-year. We define as productive inventors the newly hired inventors whose number of patents is above the median value of patents applied for in the past by newly hired inventors. Unproductive inventors are the new leavers whose number of patents is below the median value of patents applied for in the past by new leavers. In Panel B, each regression includes firm and year fixed effects. For both Panels, below the coefficient estimates in parentheses are *t*-values adjusted for heteroscedasticity and country-level clustering. ***, **, and * indicate significance at the 1% 5%, and 10% levels, respectively.

Second, we test whether high dismissal costs result in a decline of a firm's labor flow. Autor et al. (2007) contend that strong EPL increases firing costs that distort firms' firing and hiring decisions, leading to the inefficient use of corporate resources. In this test, we adopt the same model specification as the baseline DID regression model. Columns (1) and (2) in Panel B of Table 5 present the results of the DID regression with Ln*NewHires* and Ln*Leavers* as the dependent variable, respectively. We define Ln*NewHires* as the natural logarithm of one plus the number of inventors that firms hire and Ln*Leavers* as the natural logarithm of one plus the number of inventors that leave firms each year during our sample period.[11] To be included in the treatment group, both the new hires and leavers must invent at least one patent before and after the EPL reforms. We also limit the sample to those firms providing the information of inventors who change affiliations during our sample period. Column (1) in Table 5 Panel B shows that firms hire significantly fewer inventors due to the EPL tightening. This evidence is consistent with the findings of Autor et al. (2007) that show a decrease in state employment following the adoption of wrongful discharge laws by U.S. states. Results reported in Column (2) indicate that after tightening in the EPL, there is a decline in the likelihood of inventors' leaving their current jobs. However, given the fact that we cannot distinguish forced leavers from voluntary leavers due to the lack of relevant information in the European patent database, it is impossible to directly test whether changes in EPL result in an increase or decrease in forced layoffs. Instead, we interpret this result as evidence that enhanced EPL reduces inventor turnover.

To provide additional evidence, we examine whether firms are less likely to hire more-productive inventors and whether less-productive inventors are less likely to leave their current jobs after the enhancement of EPL. The tests are in the same spirit of those in Gao et al. (2018) who examine whether smoke-free laws promote innovation by attracting more productive inventors. Columns (3) and (4) in Panel B of Table 5 present the results of the DID regression with LnNewHires_Productive and LnLeavers_Unproductive as the dependent variable, respectively. We define Ln*NewHires_Productive* as the natural logarithm of one plus the number of productive inventors that firms hire and Ln*Leavers_Unproductive* as the natural logarithm of one plus the number of unproductive inventors that leave firms each year during our sample period. A newly hired inventor is a productive inventor if her/his total number of patents invented in the previous years (before this inventor changed her/his affiliation) is above the median value of patents invented in the previous years among all newly hired inventors.[12] Unproductive leavers are the new leavers whose number of patents invented in the previous years is below the median value of patents invented in the previous years among all leavers. The results in columns (3) and (4) in Table 5, Panel B show that high dismissal costs due to the tightening of EPL make firms less likely to hire productive inventors and more likely to retain unproductive inventors. Taken together, strong labor protection causes labor market distortion and thereby limits firms' ability to innovate.

3.4. Subsample analyses

In this subsection, we explore the conditions under which the relationship between labor protection and innovation varies. Specifically, we conduct four subsample analyses. First, we examine how reliance on external financing plays a role in our context. Rajan and Zingales (1998) show that firms with more growth opportunities are more likely to rely on external financing. Acharya and Xu (2017) find that public firms that are more dependent on external financing exhibit better innovation performance than a sample of matched private firms. Motivated by these studies, we test whether external financing reliance significantly impacts our results. Following Moshirian et al. (2014), we construct the variable of external financing reliance, which is defined as (capital expenditure + R&D expense -cash flow from operation)/capital expenditure. Based on the median value of external financing reliance sorted by industry and year, we divide our sample into two groups: high versus low external financing reliance. Consistent with the findings of prior studies, the results in Table 6, Panel A indicate that the impact of EPL on innovation is stronger for firms with higher reliance on external financing.

Table 6. Subsample analyses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$LnPat_{t+N}$		***$LnCite_{t+N}$***		***$LnPat_{t+N}$***		***$LnCite_{t+N}$***	
	***N* = 0**	***N* = 2**	***N* = 0**	***N* = 2**	***N* = 0**	***N* = 2**	***N* = 0**	***N* = 2**
	Panel A: external financing reliance							
	High				Low			
EPL_C	−0.0814**	−0.0899***	−0.0907***	−0.1005***	−0.0220*	−0.0123	−0.0191*	−0.0016
	(−2.67)	(−3.26)	(−3.41)	(−3.90)	(−1.75)	(−1.17)	(−1.88)	(−0.15)
Observations	45,367	45,367	45,367	45,367	45,385	45,385	45,385	45,385
Adj. R^2	0.692	0.794	0.771	0.785	0.677	0.799	0.774	0.776
	Panel B: R&D intensity							
	High				Low			
EPL_C	−0.0842*	−0.0914**	−0.1049**	−0.0939**	−0.0193	−0.0148	−0.0129	−0.0142
	(−1.88)	(−2.21)	(−2.79)	(−2.35)	(−1.59)	(−1.62)	(−1.70)	(−1.57)

Observations	25,864	25,864	25,864	25,864	64,888	64,888	64,888	64,888
Adj. R^2	0.739	0.815	0.795	0.800	0.591	0.754	0.721	0.734
	Panel C: Industry classification							
	Manufacturing industry				Nonmanufacturing industry			
EPL_C	−0.0656***	−0.0637***	−0.0728***	−0.0651***	−0.0029	−0.0041	−0.0029	−0.0041
	(−3.06)	(−2.90)	(−3.59)	(−2.95)	(−0.27)	(−0.44)	(−0.30)	(−0.57)
Observations	41,799	41,799	41,799	41,799	49,003	49,003	49,003	49,003
Adj. R^2	0.703	0.803	0.777	0.789	0.593	0.691	0.690	0.677
	Panel D: Legal Origins							
	Civil Law				Common Law			
EPL_C	−0.0361**	−0.0319**	−0.0322***	−0.0323**	0.0081	0.0311*	−0.0023	0.0260
	(−2.28)	(−2.34)	(−3.01)	(−2.73)	(0.61)	(2.28)	(−0.19)	(1.78)
Observations	53,794	53,794	53,794	53,794	36,958	36,958	36,958	36,958
Adj. R^2	0.685	0.831	0.806	0.815	0.700	0.716	0.686	0.695

This table reports the results for subsample analyses. $\text{Ln}Pat_{t+N}$ is the natural logarithm of patents applied for by a firm in year t + N. $\text{Ln}Cite_{t+N}$ is the natural logarithm of citations received by a firm in year t + N. In Panel A, we divide the sample into high versus low reliance on external financing subsamples based on the median value. The measure of external financing reliance is calculated as (capital expenditure + R&D expense – cash flow from operation)/capital expenditure. In Panel B, we divide the sample into high versus low R&D intensity subsamples based on the median value of R&D expenses. In Panel C, the sample is divided into two subsamples: firms in manufacturing industries versus firms in other industries. In Panel D, firms are grouped based on their home country's legal origins: civil law versus common law. Other control variables are included but omitted for brevity. All variable definitions are given in Appendix B. Each regression includes firm and year fixed effects. Below the coefficient estimates in parentheses are *t*-values adjusted for heteroscedasticity and country-level clustering. ***, **, and * indicate significance at the 1% 5%, and 10% levels, respectively.

Second, we examine whether the negative impact of labor protection on innovation is stronger in R&D-intensive firms than in their non-R&D-intensive counterparts. We conjecture that R&D-intensive firms are more sensitive to the effect of enhanced labor protection. Because of missing information regarding R&D input, the R&D intensity is highly skewed. Because over 70% of our firm-year observations lack information on R&D input, we assign a value of zero if a firm has a missing value for its R&D expenditures. Accordingly, we classify a firm as an R&D-intensive firm if its R&D input is nonzero (non-missing value of R&D input) and as a non-R&D-intensive firm otherwise. Consistent with our conjecture, the results in Table 6, Panel B show that the negative impact of EPL on innovation is concentrated in R&D-intensive firms.

Third, we investigate whether the negative impact of labor protection on innovation varies by industry. The 2008 National Science Foundation Business R&D and Innovation Survey (BRDIS) indicates that firms in manufacturing industries are more innovative than their nonmanufacturing counterparts.[13] We conjecture that the impact is more pronounced in manufacturing industries. Industries are defined as manufacturing if the firm's first digit SIC is either 2 or 3 and nonmanufacturing otherwise. The results in Table 6, Panel C are consistent with our conjecture.

Fourth, we examine whether the legal origin of the company's home country causes variations in the impact of EPL on innovation. La Porta et al. (1998) show that a country's legal origin, meaning that the country is rooted either in civil or common law, shapes the development of domestic laws. Botero et al. (2004) further show that labor laws are generally stronger in civil law countries than in common law countries. We conjecture that the impact of labor protection on innovation is stronger for firms in civil law countries than those in common law countries. The results in Table 6, Panel D provide supporting evidence for this conjecture.

3.5. Robustness checks

In this subsection, we perform several sensitivity analyses to provide evidence of the robustness of our results. Results are shown in Table 7. First, we include firms with headquarters in the United States. Inclusion of U.S. observations dramatically increases our sample size but does not alter our results, suggesting that our main finding is robust to the alternative sample. The results are in Table 7, Panel A.

Table 7. Sensitivity analyses.

	(1)	**(2)**	**(3)**	**(4)**
	$LnPat_{t+N}$		***$LnCite_{t+N}$***	
	***N* = 0**	***N* = 2**	***N* = 0**	***N* = 2**
	Panel A: Including U.S. firms			
EPL_C	−0.0480***	−0.0537***	−0.0563***	−0.0543***
	(−4.01)	(−4.91)	(−5.69)	(−4.93)
Observations	189,861	189,861	189,861	189,861
Adj. R^2	0.732	0.798	0.764	0.777
	Panel B: Country/Industry fixed effects			
EPL_C	−0.0564***	−0.0569***	−0.0590***	−0.0561***
	(−3.35)	(−4.26)	(−4.80)	(−4.30)
Observations	90,752	90,752	90,752	90,752
Adj. R^2	0.686	0.795	0.771	0.781
	Panel C: Country-specific time trends			
EPL_C	−0.0500**	−0.0480**	−0.0480***	−0.0468**
	(−2.52)	(−2.72)	(−2.88)	(−2.72)
Observations	90,752	90,752	90,752	90,752
Adj. R^2	0.687	0.797	0.772	0.782
	Panel D: EPL index in Allard (2005)			
EPL_A	−0.0838***	−0.0863***	−0.0858***	−0.0754***
	(−6.62)	(−7.19)	(−7.01)	(−6.48)
Observations	90,752	90,752	90,752	90,752
Adj. R^2	0.791	0.800	0.776	0.784
	Panel E: EPL indicators in Simintzi et al., (2015)			
EPL_S	−0.0322	−0.0323**	−0.0334**	−0.0338**
	(−1.39)	(−2.55)	(−2.12)	(−2.62)
Observations	109,882	109,882	109,882	109,882
Adj. R^2	0.677	0.791	0.764	0.775
	Panel F: EPL_C including all changes			

EPL_C	−0.0364***	−0.0439***	−0.0399***	−0.0386**
	(−3.18)	(−3.47)	(−3.26)	(−2.84)
Observations	90,752	90,752	90,752	90,752
Adj. R^2	0.687	0.796	0.772	0.782
	Panel G: Granted patents			
EPL_C	−0.0734***	−0.0554***	−0.0582***	−0.0534***
	(−3.87)	(−3.29)	(−4.32)	(−3.55)
Observations	90,752	90,752	90,752	90,752
Adj. R^2	0.714	0.778	0.758	0.762
	Panel H: Firms having at least one patent			
EPL_C	−0.0609	−0.0620*	−0.0783**	−0.0736**
	(−1.56)	(−2.01)	(−2.59)	(−2.54)
Observations	25,922	25,922	25,922	25,922
Adj. R^2	0.690	0.766	0.746	0.756

This table presents results for several robustness tests. In Panel A, we include firms with headquarters in the United States. In Panel B, we control for country/industry fixed effects. In Panel C, we add country-specific time trends. In Panels D, E and F, we use alternative measures of labor protection stringency respectively: *EPL_A* (the EPL index in Allard, 2005), *EPL_S* (EPL indicators reconstructed by Simintzi et al. 2015) and *EPL_C* that includes all changes in EPL index. In Panel G, the dependent variable is the number of patents granted (columns (1) and (2)) and number of citations received for those granted patents (columns (3) and (4)), respectively. In Panel H, we delete firm observations with zero patents. In columns (1) and (2), the dependent variable is $\text{Ln}Pat_{t+N,}$ the natural logarithm of patents applied by a firm in year t + N. In Columns (3) and (4), the dependent variable is $\text{Ln}Cite_{t+N,}$ the natural logarithm of citations received by a firm in year t + N. Other control variables are included but omitted for brevity. All variable definitions are given in Appendix B. Each regression includes firm and year fixed effects. Below the coefficient estimates in parentheses are *t*-values adjusted for heteroscedasticity and country-level clustering. ***, **, and * indicate significance at the 1% 5%, and 10% levels, respectively.

Second, we control for country/industry fixed effects to account for unobservable country/industry characteristics that could influence corporate innovation productivity. Ellison and Glaeser (1997) document that unbalanced developments exist in some industries across countries due to natural resources and geographic concentration. The results in Table 7, Panel B show that our results are robust to the addition of country/industry fixed effects.

Third, we examine whether different country trends are driving the results. We augment our DID regressions with country-specific time trends. The coefficients of *EPL_C* in Table 7, Panel C remain statistically significant at least at the 5% level.

Fourth, to examine whether our results are robust to alternative measures of labor protection stringency, we use the EPL index developed by Allard (2005), the EPL indicators reconstructed by Simintzi et al. (2015) that focus on major labor law reforms, and *EPL_C*, which includes all the changes in the EPL index. The results are reported in Table 7, Panel D, Panel E, and Panel F, respectively.[14] We find that our results continue to hold.

Fifth, we examine whether our results are robust to alternative dependent variables. The primary purpose of our study is to ascertain whether there is a casual relationship between labor protection and innovation. Therefore, to proxy for a firm's innovation tendency, we only keep the eventually granted patents and citations on those grants in our sample. In our innovation data, approximately 60% of patents applied for are eventually granted. Again, after replacing the dependent variable with the number of patents granted and number of citations received for those granted patents, we find consistent results in Table 7, Panel G (the coefficient estimates of *EPL_C* are all negative and significant at least at the 1% level).

Finally, we limit our analyses to firm observations that have at least one patent during 1987–2003. Because only about 30% of our sample firms have more than one patent, we delete firm observations with zero patents and rerun our regressions. The results in Table 7, Panel H show that the negative relation between EPL and innovation remains significant. As in the aforementioned discussion, we also consider several possible omitted macro-variables: annual GDP growth, unemployment rates, country-level corruption, and an indicator of recessions. We find that our results still hold after the addition of these variables. The results are reported in Appendix D. It is noted that in Panel D column (4), the result for LnPat becomes insignificant after controlling the corruption perception index. This is probably due to the fact that we lose more than one-third of sample observations. Additionally, in Appendix E, we employ three alternative measures of innovation to examine whether our results still hold. To account for the possible nonlinearity relation between firm size and innovation, we use as the dependent variable the natural logarithm of patent-weighted citations, Ln*CitePat*, which is calculated as the total number of citations received divided by total patent counts for each firm-year. We also follow Trajtenberg et al. (1997) and construct another two innovation measures *Generality* and *Originality. Generality* (*Originality*) is the Herfindahl index of the citing (cited) patents used to capture dispersion across technology classes. The results in Appendix E show that our results are robust to alternative innovation measures.

4. Conclusion

In this study, we investigate whether there is a causal relation between labor protection and innovation at the firm level. In contrast to the findings of Acharya et al., 2013, Acharya et al., 2014), our findings indicate that employee-friendly labor law reform causes a decline in a firm's innovation output. Further examination reveals that stringent employment protection laws encourage inventor shirking and distort labor market flow. We also find that the negative relation is more pronounced in firms with heavy reliance on external financing, with high R&D intensity, in manufacturing industries, and in civil-law countries. Our extensive micro-level evidence highlights that strong employment protection impedes corporate innovation.

Our paper contributes to at least three strands of the financial economics literature. First, our findings provide evidence on the real effect of labor protection laws and extend the literature examining whether and how the effects of labor protection laws are translated into real economy. Second, our study contributes to the literature on law and innovation. Literature has shown that legal environment has a direct impact on innovation (e.g., Fan and White, 2003, Acharya and Subramanian, 2009, Atanassov, 2013, Fang et al., 2017 and Png, 2017). Our paper extends this line of research by examining the impact of labor laws on innovation. Third, our paper contributes to the literature on labor protection and innovation by providing international firm-level evidence to the ongoing debate on the relation between labor protection and innovation. Our findings also have important policy implications, given that both the European Union and the OECD have put stimulating innovation on their agenda and urge member countries to support entrepreneurial and innovative activities.

Appendix A. A Brief Discussion of the EPL Index Construction in Allard (2005)

In 1985, the OECD created the original EPL indicator; it only included regular and temporary contracts. In late 1990 s, the OECD broadened the indicator to include collective dismissals and created a new indicator to cover all the main aspects of job security. This new version of the EPL has been available annually only since 1998 and is based on the numerical scores of surveys that cover eighteen aspects of employment protection legislation in three domains: laws protecting workers with regular contracts, laws protecting workers with temporary contracts, and regulations applying to collective dismissals. The final scores are reviewed and corrected if necessary by each of the national governments. The weighting scheme is as follows: regular contracts are assigned a weight of 5/12, temporary contracts are assigned a weight of 5/12, and collective dismissals are weighted at 1/6. To develop a long-time series for researchers to better assess the impact of labor protection on the real economy, Allard (2005) collected reliable information on legislative changes for 21 OECD countries over 50 years and reconstructed the OECD employment protection indicator. More specifically, to assign a score to a country-year that is not covered by the OECD EPL indicator, Allard reviewed volumes of legislation and dozens of other related publications and attempted to answer the questions in the OECD's surveys. After obtaining scores on the three domains, Allard created the EPL index by following the weighting scheme used in the creation of OECD EPL indicator.

Appendix B. Definition of variables.

Variable Name	**Description**
$LnPat_{t+N}$	The natural logarithm of patents applied by a firm in year t + N (N = 0,1,2) divided by the mean number of patents of all firms in that country-year. [Data Source: European Patent Office]
$LnCite_{t+N}$	The natural logarithm of citations received by a firm in year t + N (N = 0,1,2). [Data Source: European Patent Office]
$LnCitePat_{t+N}$	The natural logarithm of citations per patent applied by a firm in year t + N (N = 0,1,2), scaled by the total number of citations per patent received by all patents applied for in that country-year. [Data Source: European Patent Office]
$Generality_{+N}$	The Herfindahl Index of the citing patents used to capture dispersion across technology classes in year t + N. [Data Source: European Patent Office]
$Originality_{+N}$	The Herfindahl Index of the cited patents used to capture dispersion across technology classes in year t + N. [Data Source: European Patent Office]
EPL_C	An indicator variable that equals 1 (0) after (before) EPL index increases in a country-year, and equals −1 (0) after (before) EPL index decreases in a country-year. We only consider changes in EPL index whose absolute value is greater than 0.2 (the mean value). [Data Source: Allard (2005)]
EPL_A	EPL index in Allard (2005). [Data Source: Allard (2005)]
LnAssets	The natural logarithm of total assets. [Data Source: COMPUSTAT Global]
ROA	Returns on Assets. Net income divided by total assets. [Data Source: COMPUSTAT Global]
MB	Market-to-book ratio. Market value of common equity divided by book value of common equity. [Data Source: COMPUSTAT Global]
Tangibility	The ratio of tangible assets over total assets. Net property, plant and equipment divided by total assets. [Data Source: COMPUSTAT Global]
Leverage	Total debt divided by total assets [Data Source: COMPUSTAT Global]
R&D	R&D expenses divided by total assets. [Data Source: COMPUSTAT Global]
HHI	Herfindahl-Hirschman Index scaled by sales based on the first two digits of SIC code. [Data Source: COMPUSTAT Global]
LnGDP_Capita	The natural logarithm of GDP per capita measured by GDP in U.S. dollars divided by total population. [Data Source: World Bank]

LnPatentStock5	The natural logarithm of cumulative patents in a country over the past 5 years. [Data Source: European Patent Office]
Ed_Share	Public spending on secondary and tertiary education scaled by GDP. [Data Source: World Bank]
IP	Intellectual property protection index. [Data Source: IMD World Competitiveness Report]
Trade	International trade measured by the level of imports minus the level of exports in a country. [Data Source: IMF]
Right	An indicator for the political ideology of the ruling party, which equals one if it is right-leaning and zero otherwise. [Data Source: World Bank]
Disp_Index	Gallagher Index. The disproportionality of the electoral system in a country. [Data Source: Gallagher and Mitchell (2008)]
GDP_Growth	Annual growth rate in GDP. [Data Source: World Bank]
Unemployment	The ratio of unemployment divided by labor force. [Data Source: OECD]
Corruption	Corruption Perceptions Index. Higher values indicate less corruption. [Data Source: Transparency International]
Recession	An indicator of recessions, which equals one if there are two consecutive quarters with negative GDP growth in a country and zero otherwise. [Data Source: OECD]

Appendix C. Replication of baseline results in Acharya et al. (2013).

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	LnPat	***LnCite***	***LnPat***	***LnCite***	***LnPat***	***LnCite***
Panel A: Replication of Baseline Results in Acharya et al. (2013)— Country Level						
DSL	1.9790***	1.4078***				
	(7.82)	(5.11)				
EPL_A			0.1368***	0.1400**	−0.0932**	−0.0822
			(2.73)	(2.46)	(−2.02)	(−1.29)
Constant	5.5453***	6.8731***	6.1303***	7.2199***	6.1983***	7.2384***
	(39.04)	(41.41)	(47.61)	(46.26)	(37.28)	(28.38)
Year FE	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES
Observations	100	100	100	100	357	357
Adj. R-squared	0.987	0.985	0.986	0.987	0.987	0.973
	(1)	(2)	(3)	(4)	(5)	(6)
	$LnPat_{t+N}$			$LnCite_{t+N}$		
	$N = 0$	$N = 1$	$N = 2$	$N = 0$	$N = 1$	$N = 2$
Panel B: Replication of						

Baseline Results in Acharya et al. (2013)—Firm Level						
EPL_C	−0.1181***	−0.0809***	−0.0728***	−0.0804***	−0.0783***	−0.0765***
	(−3.60)	(−3.09)	(−2.81)	(−2.98)	(−2.91)	(−2.88)
DSL	0.2565	0.1458	0.0515	0.1264	0.1828	0.1218
	(1.26)	(0.82)	(0.29)	(0.72)	(1.07)	(0.70)
LnAssets	0.0347***	0.0347***	0.0261***	0.0320***	0.0249***	0.0173***
	(9.90)	(9.73)	(7.26)	(9.23)	(7.36)	(5.00)
ROA	−0.0068	0.0041	0.0106	−0.0014	0.0089	0.0132
	(−0.89)	(0.51)	(1.26)	(−0.16)	(1.02)	(1.52)
MB	0.0013***	0.0019***	0.0015***	0.0013***	0.0015***	0.0013***
	(4.07)	(5.43)	(4.31)	(3.71)	(4.13)	(3.58)
Tangibility	−0.0130	−0.0064	−0.0147	−0.0117	−0.0116	−0.0159
	(−1.00)	(−0.47)	(−1.07)	(−0.92)	(−0.87)	(−1.17)
Leverage	−0.0412***	−0.0585***	−0.0521***	−0.0372***	−0.0419***	−0.0420***
	(−3.92)	(−5.49)	(−5.06)	(−3.52)	(−4.14)	(−4.19)
R&D	0.1706***	0.1785***	0.0633*	0.1965***	0.1778***	0.0303
	(4.28)	(4.71)	(1.70)	(4.36)	(4.28)	(0.72)
HHI	0.0235	0.0112	−0.0015	0.0123	0.0134	0.0109
	(1.15)	(0.55)	(−0.07)	(0.65)	(0.71)	(0.55)
LnGDP_Capita	0.0252	−0.0185	−0.0391	−0.0269	−0.0217	−0.0291
	(0.57)	(−0.52)	(−1.12)	(−0.76)	(−0.62)	(−0.87)
LnPatentStock5	0.0047	−0.0010	−0.0012	0.0013	0.0010	−0.0021
	(1.09)	(−0.34)	(−0.44)	(0.43)	(0.36)	(−0.78)
Ed_Share	−0.0067	−0.1572	−0.3598**	−0.1069	−0.0575	−0.2735
	(−0.03)	(−0.93)	(−2.13)	(−0.62)	(−0.33)	(−1.58)
IP	−0.0055	0.0295	0.0111	0.0037	0.0177	0.0198
	(−0.18)	(1.02)	(0.36)	(0.14)	(0.64)	(0.64)
Trade	−0.0392	−0.0387	−0.0633	−0.0183	−0.0462	−0.0825**
	(−0.90)	(−0.91)	(−1.50)	(−0.45)	(−1.16)	(−1.99)
Right	−0.0199***	−0.0163***	−0.0195***	−0.0142***	−0.0142***	−0.0167***
	(−4.17)	(−3.85)	(−4.48)	(−3.31)	(−3.28)	(−3.79)
Disp_Index	0.0001	0.0002	0.0006	0.0017*	0.0004	−0.0001
	(0.14)	(0.19)	(0.54)	(1.74)	(0.39)	(−0.12)
Constant	0.0347***	0.0347***	0.0261***	0.0320***	0.0249***	0.0173***
	(9.90)	(9.73)	(7.26)	(9.23)	(7.36)	(5.00)
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Observations	124,594	124,594	124,594	124,594	124,594	124,594
Adj. R-squared	0.777	0.799	0.806	0.767	0.776	0.783

In Panel A, we replicate the baseline results in Acharya et al. (2013). *DSL* is the dismissal law index in Deakin et al. (2007). In columns (1)–(4), the sample includes four countries: the United States, the United Kingdom, France, and Germany. In columns (5) and (6), we extend the sample by adding another 17 OECD countries. Country and year fixed are included. In Panel B, we replicate the baseline results in Acharya et al. (2013) using firm-level data. All variable definitions are given in Appendix B. In Panel B, each regression includes firm and year fixed effects. Below the coefficient estimates in parentheses are *t*-values adjusted for heteroscedasticity and country-level clustering. ***, **, and * indicate significance at the 1% 5%, and 10% levels, respectively.

Appendix D. Additional country-level controls.

	LnPat				***LnCite***			
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
EPL_C	−0.0509**	−0.0430**	−0.0360**	−0.0217	−0.0546***	−0.0495***	−0.0390***	−0.0185*
	(−2.73)	(−2.91)	(−2.45)	(−1.34)	(−3.44)	(−3.61)	(−3.14)	(−1.81)
Recession	0.0033	−0.0014	0.0016	0.0034	−0.0000	−0.0030	0.0017	0.0025
	(1.19)	(−0.34)	(0.39)	(0.94)	(−0.01)	(−0.88)	(0.55)	(0.95)
GDP_Growth		−0.0046*	−0.0041*	0.0002		−0.0029*	−0.0022	0.0001
		(−1.86)	(−1.76)	(0.25)		(−1.81)	(−1.60)	(0.10)
Unemployment			0.4154**	0.0961			0.6349***	0.5237***
			(2.48)	(0.42)			(3.80)	(3.74)
Corruption				−0.0228**				−0.0006
				(−2.00)				(−0.05)
Constant	−0.1124	0.0460	−0.2559	0.7300**	−0.1121	−0.0103	−0.4741	0.4853
	(−0.29)	(0.15)	(−0.86)	(2.10)	(−0.36)	(−0.04)	(−1.68)	(1.62)
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	90,752	90,752	90,574	62,857	90,752	90,752	90,574	62,857
Adj. R-squared	0.687	0.687	0.687	0.764	0.772	0.772	0.773	0.833

This table presents regression results after controlling additional country characteristics All variable definitions are given in Appendix B. Each regression includes firm and year fixed effects. Below the coefficient estimates in parentheses are *t*-values adjusted for heteroscedasticity and country-level clustering. ***, **, and * indicate significance at the 1% 5%, and 10% levels, respectively.

Appendix E. Alternative Innovation Variables

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	LnCitePat$_{t+N}$		***Generality***$_{t+N}$		***Originality***$_{t+N}$	
	***N* = 0**	***N* = 2**	***N* = 0**	***N* = 2**	***N* = 0**	***N* = 2**
EPL_C	−0.0491***	−0.0466**	−0.0116***	−0.0105**	−0.0191***	−0.0175***
	(−2.89)	(−2.68)	(−3.05)	(−2.49)	(−3.09)	(−3.37)
Controls	YES	YES	YES	YES	YES	YES
Constant	−0.2195	0.0058	−0.1899	−0.1567	−0.1971	−0.1750
	(−0.67)	(0.02)	(−1.47)	(−1.43)	(−1.08)	(−1.16)

Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Observations	90,752	90,752	90,752	90,752	90,752	90,752
Adj. R-squared	0.585	0.590	0.668	0.669	0.674	0.676

This table presents the results for alternative innovation variables. In columns (1) and (2), the dependent variable is Ln$CitePat_{t+N}$, which is the natural logarithm of citations received on the firm's patents applied, scaled by the number of patents applied for in year t + N (N = 0 or 2). In columns (3) and (4), the dependent variable is $Generality_{t+N}$, the Herfindahl Index of the citing patents used to capture dispersion across technology classes in year t + N. In columns (3) and (4), the dependent variable is $Originality_{t+N}$, the Herfindahl Index of the cited patents used to capture dispersion across technology classes in year t + N. All other variable definitions are given in Appendix B. Each regression includes firm and year fixed effects. Below the coefficient estimates in parentheses are *t*-values adjusted for heteroscedasticity and country-level clustering. ***, **, and * indicate significance at the 1% 5%, and 10% levels, respectively.

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