Labor Protection and Leverage

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This paper exploits intertemporal variations in employment protection across countries and finds that rigidities in labor markets are an important determinant of firms' capital structure decisions. Over the 1985–2007 period, we find that reforms increasing employment protection are associated with a 187 basis point reduction in leverage. We interpret this finding to suggest that employment protection increases operating leverage, crowding out financial leverage. This result does not appear to be due to pretreatment differences between treated and control firms, omitted variables, unobserved changes in regional economic conditions, and reverse causality. Heterogeneous treatment effects are consistent with our economic intuition. (*JEL* J31, J51, G32, G33, K31)

Since Modigliani and Miller's (1958) irrelevance proposition, financial economists have devoted considerable efforts to understand the nature of the frictions that affect firms' financial choices. This paper attempts to further our understanding of one such friction, namely, the effect of labor regulation on the capital structure of firms.

Using firm-level data from twenty-one countries over the 1985–2007 period, we exploit intertemporal variations in labor regulation across countries, adopting a difference-in-differences (DID) research design to investigate the impact of employment protection legislation on firm financial structure. We find that firms reduce the amount of leverage in their capital structure by 187 basis points, or about 10% relative to the median leverage, following a reform that increases the employment protection, relative to a set of control firms operating

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in the same industry at the same time but which are located in countries without changes in employment protection.

Our explanation for this finding is that an increase in employment protection increases restructuring costs and thus increases the cost of financial distress for a given level of debt. Because of this, firms should react to an increase in employment protection by reducing financial leverage. This follows from a simple static trade-off model of capital structure in which employment protection increases the fixed costs that a firm has to pay independently of its performance, that is, its operating leverage. Essentially, prolabor employment legislation converts the labor contract into a debt-like contract, in turn generating a crowding out effect, namely, operating leverage crowds out financial leverage.

The empirical identification in our DID approach comes from the comparison of firms in countries that are subject to a change in employment protection (treated firms) with firms in countries that have no such change (control firms). The key identifying assumption is that, conditional on controls, treated and control firms are only randomly different. Our empirical specification controls for time-invariant firm characteristics by including firm fixed effects. Year/industry fixed effects absorb time-varying industry characteristics (e.g., investment opportunities). The traditional determinants of leverage (profitability, tangibility, size, and growth opportunities) control for time-varying firm characteristics, whereas time-varying country-level controls take care of time-varying country conditions.

The advantage of this approach, as highlighted by Bertrand and Mullainathan (2003), is that most firms over time are both in the treated and control group, thus alleviating the concern that the two sets of firms may differ along other dimensions aside from the change in employment protection. To assuage these concerns further, we show that there are no differences in pretreatment trends between treated and control firms by examining the dynamics of leverage around the passage of these laws. The dynamic analysis also allows us to investigate the presence of pretreatment trends that may be the result of, for instance, the business-cycle effects on both leverage and EPL. We find no statistically significant change in leverage prior to the passage of these laws, although we do find a significant effect on leverage one and two years after the passage of the laws.

To sharpen this analysis further, we repeat our estimations using a matched sample. We do this in two ways. We select only control firms that match the treated firms by their size and industry. We also implement a propensity score matching and select control firms that are a good match according to a larger set of observable firm characteristics (profitability, tangibility, size, growth opportunities, and industry). We find that the negative effect of an increase in employment protection on leverage is robust to these changes.

The remaining important concern is that there may be other concomitant changes in these countries that may be driving the results, that is, an omitted

variable varies within countries in a way that coincides with the change in employment protection. For example, it is plausible that other contemporaneous reforms, such as changes in tax laws, might affect firms' capital structure decisions besides labor reforms. In this case, the effect on leverage also may be incorrectly attributed to employment protection. Ideally, to control for such reforms or country-specific shocks, one needs to include country/year fixed effects. Unfortunately, this is not possible, as these fixed effects would also absorb EPL, our variable of interest.

We perform several tests (other than controlling for countries' economic conditions) to evaluate this concern. First, we show that employment protection regulations are not sensitive to economic conditions (such as economic growth, recession, or unemployment rates). Second, we restrict our sample of control firms to neighboring countries only. In this way, we can control for shocks that are common to a region, and our results become statistically stronger. Third, we control for other reforms that may be occurring at the same time as the change in employment protection. We find that there is no change in the significance of the coefficient of employment protection when we control for labor market characteristics. We also control for corporate and personal taxes using the data from Faccio and Xu (Forthcoming), and the statistical significance of our results does not change. Collectively, the results of these analyses mitigate the concern that some unobserved variables may be driving the results.

Moreover, although our indicator of employment protection is based on changes in the employment laws of countries, which are not (by their own nature) under direct control of firms, a possible concern is that changes in employment protection are endogenous to firms' economic conditions because of lobbying activity. We address this concern by studying the political economy of labor reforms. Existing political economy models have highlighted several potential determinants of employment protection. Saint-Paul (2002) suggests that employment protection should be positively correlated with employees' bargaining power. Pagano and Volpin (2005) predict that lower employment protection is likely to emerge as a political outcome in countries with majoritarian (as compared with proportional) electoral rules. Perotti and Von Thadden (2006) argue that when financial wealth is more concentrated, labor rents and labor protection are higher. Labor market characteristics, economic conditions, and characteristics of the median firm in a country are also potential determinants of employment protection legislations. To our leverage specifications, we add the variables that are significant in our political economy empirical model. The effect of employment protection on leverage is unchanged.

As an additional test, we exploit cross-sectional variation in labor turnover across different industries. Because the economic effect that we are emphasizing goes through the labor channel, it is natural to expect firms that have high labor turnover to be more likely affected by a change in EPL. So we divide the firms within a country according to the labor turnover in their

industry in the United States and evaluate the differential effect of the EPL reform across these firms. This strategy can be understood as follows: within both treatment and control groups, we are varying the treatment intensity to certain groups. Because this specification allows for country/year fixed effects, all variables at the country level are differenced out, including EPL and any omitted variables. The point of such an exercise is not to estimate the direct effect of EPL on leverage but its differential effect across firms that differ in terms of labor turnover. To the extent that the omitted variables are uncorrelated with labor turnover, the estimate can be interpreted as a triple-difference effect. We find that an increase in EPL has a greater negative effect on leverage in industries with more labor turnover.

Our findings are robust across definitions of employment protection legislation and leverage. We have constructed our own indicator of employment protection from a careful study of the labor reforms in our sample. These reforms entail significant changes in the costs of firing employees and the flexibility of hiring new ones, distinguishing between reforms that increased and those that decreased employment protection. As a measure of robustness, we also use the Employment Protection Legislation (EPL) indicator produced by Allard (2005), measuring the level of employment protection in a country as a score between zero and six. Results are very similar using these two indicators.[1] As our main measure of leverage, we use the ratio of the book value of total debt over the book value of assets and long-term debt over total assets. We also find similar results if we measure leverage as the logarithm of debt or logarithm of long-term debt, as well as other definitions of leverage.

The existing literature studying the effect of labor claims on leverage has emphasized the strategic role of debt (Baldwin 1983; Bronars and Deere 1991; Perotti and Spier 1993; Dasgupta and Sengupta 1993; Hennessy and Livdan 2009; Matsa 2010; among several others). This literature argues that debt is strategically used by firms to lower the bargaining power of labor and other suppliers of inputs, and this may be beneficial to the firm as it reduces the cost of these inputs and alleviates the underinvestment problem caused by labor holdout power. To the extent that increases in employment protection increase labor bargaining power, a prediction from these papers is that leverage should increase with employment protection. In the terminology of the trade-off theory of capital structure, this literature predicts that the marginal benefit of debt should increase with employment protection. Clearly, the negative effect of employment protection on leverage is not consistent with this prediction. Our explanation is that the increase in the marginal benefit of debt is more than offset by an increase in the marginal costs of debt due to the increase in fixed labor costs brought about by the increase in employment protection.

[1] We have also interacted our measure of employment protection with rule of law to capture the enforceable rights of employees. The results are unchanged.

Another channel in the existing literature through which employment protection may affect leverage is the human costs of bankruptcy. Berk, Stanton, and Zechner (2010) derive the optimal compensation contract for risk-averse workers in the presence of leverage and show that wages must increase with leverage to compensate workers for the costs of financial distress that they suffer. Because of the greater labor costs, an increase in the human costs of financial distress is associated with a reduction of leverage. Agrawal and Matsa (2013) find consistent evidence with this hypothesis: higher labor unemployment benefits are associated with increases in firms' leverage. It is difficult to relate our analysis to this literature because the effect of employment protection on the human cost of bankruptcy for an employee is theoretically unclear. On the one hand, employment protection may decrease the human cost of bankruptcy because employees receive higher compensation in case their employer files for bankruptcy when employment protection is higher. On the other hand, employment protection may increase the human cost of bankruptcy because it may increase the duration of unemployment in the economy.

This paper is also related to the recent literature on labor regulations and economic growth. Using industry-level data from India, Besley and Burgess (2004) find that more proworker regulation is associated with lower investment and economic growth. Botero et al. (2004) show that more stringent labor regulation is associated with lower labor force participation and higher unemployment. Contrarian evidence is offered by Acharya, Baghai, and Subramanian (2010), who find that prolabor laws can have an ex ante positive effect on firms' innovation. This paper revisits the link between labor regulation and growth using microlevel evidence and identifies the mechanism through which labor stifles growth, namely, labor crowds out external finance.

This paper also builds on the literature on labor bargaining and firm financing and real activity. Ruback and Zimmerman (1984), Abowd (1989), and Hirsch (1991) document that labor union coverage has a negative association with U.S. firms earnings' and market values. Lee and Mas (2012) study the impact of firm-level union elections on firm performance and find that union wins are associated with stock price losses, as well as decreases in firm profitability and growth. Chen, Kacperczyk, and Molina (2011) find that the cost of equity is higher in more unionized industries. Atanassov and Kim (2009) provide international evidence that strong unions play an important role in firms' financial and economic restructuring. Benmelech, Bergman, and Enriquez (2009) show that companies in financial distress extract surplus from workers by achieving substantial wage concessions.

Finally, the paper contributes to a broader body of literature arguing that firms' input markets and product markets are important factors interacting with firms' capital structure decisions. MacKay and Phillips (2005) find that the position of firms within their industries determines their financial structure. Leary and Roberts (2014) show that firms make decisions on their leverage by

responding to capital structure decisions by their peers. Kim (2012) shows that human capital specificity has a negative effect on firms' debt.

1. Data

We combine (1) firm-level financial data, (2) country-level control variables, and (3) our indicator on employment protection legislation changes. All of our variables are defined in Appendix A.

1.1 Firm-level data

Our main data source is Worldscope. Our sample contains financial information on over 30,000 nonfinancial companies in the twenty-one countries for which the EPL indicator is available. The sample spans the 1985–2007 period. We start in 1985 because we could not find systematic information about employment protection before 1985. We stop in 2007 because the financial crisis that followed represented a severe structural shock to both financial leverage and employment regulation. From our sample, we exclude financials, utilities, and public administration companies, as well as firms with missing total assets (i.e., a total of 46,034 observations). The final sample consists of 217,183 firm years, though sample size varies over time because of missing information on some variables used in the analysis.

As our main measures of leverage, we use the ratio of total debt over total assets and the ratio of long-term debt over total assets. As robustness checks, we also consider the ratio of total leverage over the market value of assets (the latter is defined as total assets, i.e., book value of equity plus market value of equity), the ratio of long-term debt over the market value of assets, net debt (defined as total debt minus cash and other marketable securities) over total assets, net debt over the market value of assets, the logarithm of one plus total debt, and the logarithm of one plus long-term debt.

In our regression analysis, we include the standard, firm-level set of explanatory variables for leverage, as identified in the literature (see, for instance, Rajan and Zingales 1995): tangibility (which is defined as net property, plant, and equipment over total assets) as a proxy for the amount of collateral that a firm can pledge, size (which is defined as the logarithm of firms' real assets) as a control for the degree of diversification and thus the risk of default, profitability (as measured by the return on assets, which is the ratio of EBIT over total assets) as a proxy for the availability of internal funds, and the market-to-book ratio (i.e. the ratio of the market value of equity plus book value of debt over the book value of debt plus equity), as an indicator of growth opportunities. In panel A of Table 1, we report mean, median, the 25th and 75th percentiles, and standard deviation of the main variables used in the analysis. For this sample, the average ratio of debt to assets is about 25%. ROA is 2.4% for the average firm in our sample; tangibility is 31.2%; and the average firm has assets of $1.4 million and trades at a market-to-book ratio of 1.88.

Table 1
Summary statistics

	Mean	Median	SD	25th percentile	75th percentile
Panel A: Firm-level variables					
Total debt/assets	0.255	0.203	0.194	0.063	0.352
Long-term debt/assets	0.172	0.119	0.155	0.004	0.218
Total debt/market value assets	0.285	0.232	0.234	0.035	0.403
Long-term debt/market value assets	0.180	0.133	0.172	0.003	0.230
Net debt/assets	0.070	0.091	0.317	−0.114	0.277
Net debt/market value assets	0.105	0.082	0.309	−0.071	0.302
Log(debt)	9.015	10.003	4.075	7.573	11.783
Log(long-term debt)	7.761	9.058	4.619	5.024	11.244
Size($ mil)	1,378	148.0	73,043	36.4	577.7
ROA	0.024	0.060	0.230	0.016	0.108
Tangibility	0.312	0.275	0.215	0.115	0.417
Market/book	1.880	1.001	5.122	0.693	1.635
Panel B: Country-level variables					
GDP growth rate (%)	2.825	2.823	1.947	1.696	3.932
GDP per capita (in thous. $)	24.115	22.888	11.567	15.502	30.574
Creditor rights	1.910	1.000	1.164	1.000	3.000
Unemployment rate (%)	5.963	5.406	2.430	4.608	7.134
Union density (%)	37.909	33.491	20.508	22.489	52.207
Bargaining centralization	2.571	3.000	1.174	2.000	3.000
Income inequality	31.480	31.400	4.441	28.000	34.225
Recession indicator	0.078	0	0.269	0	0
Miller tax index	0.317	0.321	0.165	0.235	0.388
Rule of law	0.918	1	0.116	0.833	1

This table reports summary statistics for firm-level variables and country-level controls used in the analysis. Firm-level controls are winsorized at 1% level. The sample consists of 217,183 firm-years for all nonfinancial and nonuntility firms included in Worldscope database for the twenty-one countries of our sample. It covers the period 1985–2007. All variables are defined in Appendix A.

1.2 Country-level control variables

To control for the differences in macroeconomic conditions and income across countries, we include GDP growth and GDP per capita in all of our regressions. We also control for creditor rights, as reported by Djankov, McLiesh, and Shleifer (2007), to capture differences in investor protection. The creditor rights index takes values from zero to four, with higher values indicating stronger creditor rights, and it provides time variation in creditor protection.

In our robustness analysis, we consider several possible omitted variables: unemployment rates, union density (which is obtained from the OECD), an indicator of the degree of labor bargaining centralization (from the OECD), the Gini coefficient of income inequality, corporate and personal tax reforms (as measured by the Miller index computed using the data in Faccio and Xu (Forthcoming), and an indicator of recessions. This indicator takes a value of one if there are two consecutive quarters with negative GDP growth in a country and zero otherwise. This definition of recession is standard in the macroliterature.

To take into consideration differences in the enforcement of the law across countries, we also interact our indicators of employment protection with rule of

law (from International Country Risk). Summary statistics for these variables are presented in panel B of Table 1.

1.3 Employment protection indicators

To find a measure of labor regulation that varies across countries and over time, we collected data on the major labor reforms across twenty-one OECD countries.[2] Following the Employment Protection Legislation indicator produced by the OECD, we focus on laws protecting workers with regular contracts, those affecting workers with fixed-term (temporary) contracts, and regulations applying to collective dismissals.

To create our indicator, we selected those reforms that had a significant effect on employment protection, consulting a wide range of sources. Appendix B includes descriptions of the law changes used to build our indicator for each country. Distinguishing between reforms that increased and those that decreased employment protection, we produced an indicator that takes values $R_{k,t}=+1$ (if EPL went up in country k in year t), $R_{k,t}=-1$ (if EPL went down in country k in year t), and $R_{k,t}=0$ otherwise.

There are twenty-one major labor reforms in our sample period. Some countries have no major reform (Canada, Finland, Ireland, Japan, New Zealand, and the United Kingdom), while others have two major reforms (Austria, France, Italy, the Netherlands, Portugal, and Sweden). Nine of the reforms led to an increase in employment protection, whereas twelve led to a decrease in employment protection. Table 2 shows the country split in the total number of observations and distribution of treated firms across countries. U.S. firms represent about one-third of the treated sample, followed by France (with 15%) and Germany and Australia (with 11%).

Our indicator, EPL^R, is defined recursively starting from $EPL^R_{k,1985}=0$. For any given country k in year t:

$$EPL^R_{k,t}=EPL^R_{k,t-1}+R_{k,t}. \tag{1}$$

This indicator is designed to capture the long-run effects of changes in employment regulation. By construction, it is a good indicator over time within a country, but is not comparable across countries: that is, the fact that EPL^R is greater in one country than in another does not imply that employment protection is greater in the first country compared with the second one. The variability of this index over time is shown in Figure 1.

As a robustness check, we also use the OECD indicator of employment protection as modified and extended by Allard (2005).[3] This indicator, which

[2] They are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States.

[3] The original EPL indicator was constructed by the OECD in 1985 including only regulations on regular and temporary contracts. Later on, the OECD redefined the indicator by also adding regulations on collective

Table 2
Distribution of observations by country and EPL reforms

Country	Number of firm-year observations	Number of treated firms	% of treated firms (at time of change)	EPL reforms
Australia	7,763	683	11.07	1
Austria	1,239	105	1.70	2
Belgium	1,658	117	1.90	1
Canada	11,662	0	0.00	0
Denmark	2,345	121	1.96	1
Finland	2,035	0	0.00	0
France	10,777	948	15.36	2
Germany	10,115	688	11.15	1
Greece	2,718	23	0.37	1
Ireland	990	0	0.00	0
Italy	3,254	329	5.33	2
Japan	48,072	0	0.00	0
Netherlands	3,081	334	5.41	2
New Zealand	838	0	0.00	0
Norway	2,166	97	1.57	1
Portugal	979	41	0.66	2
Spain	1,915	95	1.54	1
Sweden	3,944	399	6.47	2
Switzerland	2,915	130	2.11	1
United Kingdom	23,060	0	0.00	0
United States	75,657	2,060	33.39	1

This table reports the distribution of observations in our sample across countries (Column 1), the number of treated firms (Column 2), and the percentage of treated observations at the time of treatment (Column 3). Column 4 of the table reports employment protection reforms included in our labor indicator EPL^R.

we call EPL^A, ranges between zero and six and, unlike EPL^R, does not treat all law changes equally. A higher EPL^A score indicates stronger job security for workers. The main difference between the two indicators (EPL^R and EPL^A) is that EPL^R focuses on large changes in the law, whereas EPL^A also considers smaller changes.

2. EPL and Leverage

In this section, we investigate the relation between EPL and leverage. By employing a DID methodology that exploits the intertemporal variations in employment laws, we find that firms reduce their use of debt following legal changes that increase employment protection. We then address possible concerns with our analysis.

2.1 DID approach

Using firm-level data, we estimate the following specification:

$$y_{it} = \lambda_i + \delta \cdot EPL_{k,t-1} + \beta \cdot X_{it-1} + \alpha_j \cdot \gamma_t + \epsilon_{it}, \quad (2)$$

dismissals. This new version of EPL is available at an annual frequency only since 1998 and is based on survey questionnaires answered by government officials. Allard (2005) reconstructed the OECD employment protection legislation based on law changes of employment protection and provided a longer time series of the three components included in the new OECD indicator. In our analysis we use this longer time series of the three individual EPL components provided by Allard (2005), and we construct EPL^A as an equally weighted average of these three components.

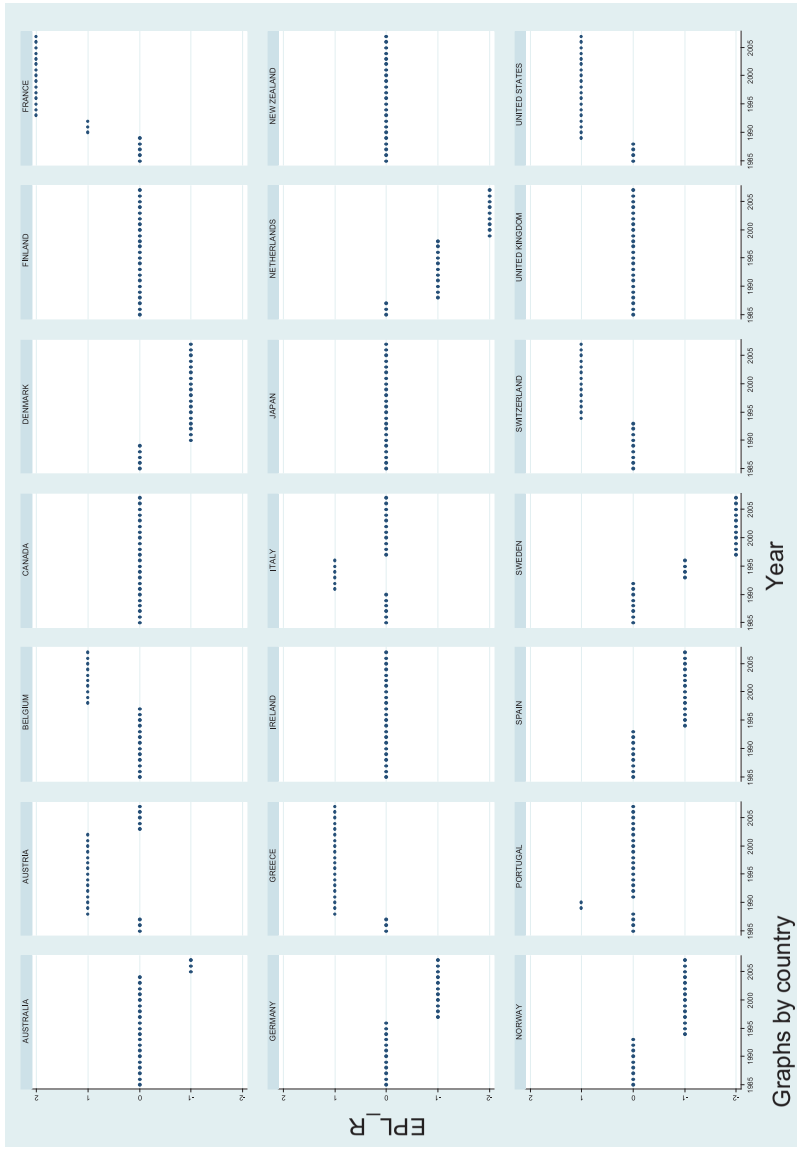

Figure 1
Employment protection legislation

where i denotes a firm, t denotes a year, j is an industry, and k is a country. The dependent variable y_{it} is our measure of leverage; EPL is measured by EPL^R; X_{it-1} is the vector of control variables; the λ_i is a firm fixed effect; $\alpha_j \cdot \gamma_t$ is an industry/year fixed effect; and ϵ_{it} is the error term. X_{it-1} includes the lagged effect of profitability, investment opportunities, size, and tangibility, all of which are the controls typically used in leverage regressions. It also includes macroeconomic variables (GDP growth, GDP per capita, and creditor rights). We cluster the standard errors at the country level, because the labor laws are changing at the country level.[4]

A similar research design has been used in several studies, particularly in labor economics. The methodology is best illustrated by the following example. Suppose there are two countries, A and B, undergoing legal changes at times $t=1$ and $t=2$, respectively. Consider $t=0$ to be the starting period in our sample. From $t=1$ to $t=2$, country B initially serves as a control group for legal change; after that it serves as a treated group for subsequent years. Therefore, most countries belong to both treated and control groups at different points in time. This specification is robust to the fact that some groups might not be treated at all or that other groups were treated prior to 1985, which is our sample's start date. It is also robust to using a continuous index (like EPL^A) or a discrete one (like EPL^R).[5]

Figure 2 presents graphically the within-firm variation in leverage, as a function of changes in employment protection legislation. It plots the average annual leverage in years $t=-3$ to $t=+3$ for the group of treated firms and for the comparison group of control firms, while controlling for firm and industry/year fixed effects that remove the effect of firm-specific characteristics and time-varying industry conditions, respectively. Because of the firm-fixed effects, the annual leverage for both treated and control firms floats around zero. The treated firms have relatively higher leverage before a change in EPL and relatively lower leverage after the change, as compared with their long-run average. No such change happens to control firms. The reduction in leverage between the year before the reform and the year after is 124 basis points for a unit increase in EPL^R.

In Table 3, we turn to our regression specifications. Leverage is defined as total debt over book value of assets in Columns 1 and 2 and as long-term debt over book value of assets in Columns 3 and 4. In Columns 1 and 3, we control for industry/year fixed effects (to control for industry-level time-varying

[4] It is important to note that clustering at the country level generates the most conservative standard errors; that is, the standard errors become much smaller when we cluster them at the firm or industry level. The results are available upon request.

[5] For instance, Angrist and Pischke (2009) write "A second advantage of regression DD is that it facilitates the study of policies other than those that can be described by dummy variables. Instead of New Jersey and Pennsylvania in 1992, for example, we might look at all state minimum wages in the United States. Some of these are a little higher than the federal minimum (which covers everyone regardless of where they live), some are a lot higher, and some are the same. The minimum wage is therefore a variable with differing treatment intensity across states and over time."

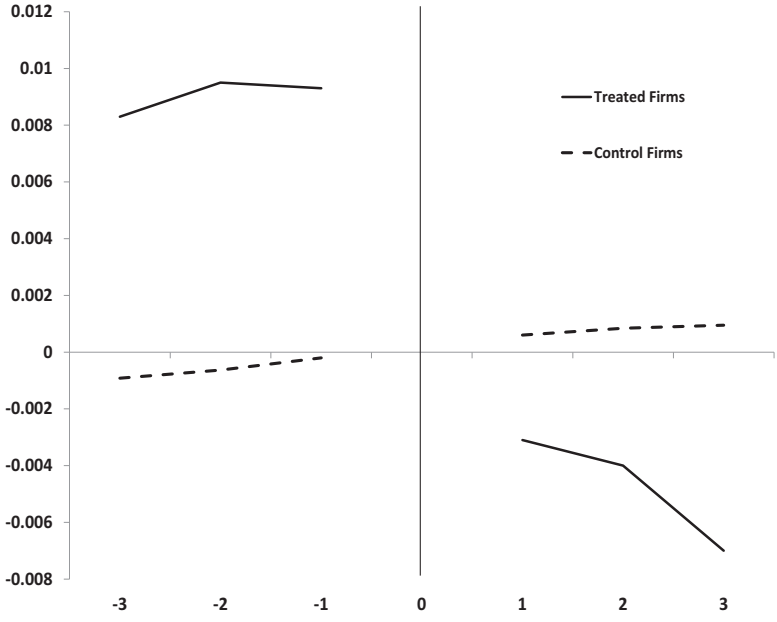

Figure 2
Firm leverage around employment protection legislation changes
The figure plots the within-firm variation in leverage, as a function of changes in employment protection legislation, net of firm-specific characteristics, and time-varying industry conditions. The average annual leverage for the group of treated firms (solid line) in years t=-3 to t=+3 is compared to the one for control firms (dotted line). t=0 corresponds to the year of the reform.

characteristics), firm fixed effects (to control for firm-specific characteristics), and country-level control variables, but we do not include the firm-level control variables. In Columns 2 and 4, we add the firm-level control variables.

Across all specifications, we find that the coefficient of interest (δ in specification 2 above) is negative and statistically different from zero at the 1% or 5% level. The effect of an increase in EPL on total debt over assets is a reduction of 173 basis points in Column 1 and 187 basis points in Column 2, or 9% relative to the median leverage. The fact that the estimated coefficient on EPL is almost identical across the two specifications indicates that EPL changes are not systematically correlated with firm-level variables; that is, they are random at the firm level. The effect of an increase in EPL on long-term debt over assets is a reduction of 106 basis points in Column 3, and 113 basis points in Column 4, or 10% relative to the median long-term leverage.

The coefficients on Tangibility, Size, ROA, and Q have the expected sign. They are all statistically significant, except for Q.[6] GDP growth has the expected

[6] The coefficient on Q is not statistically different from zero, perhaps because we capture investment opportunities, including industry/year fixed effects throughout our specifications.

Table 3
EPL and leverage: Baseline results

	Total debt/assets		Long-term debt/assets	
	(1)	(2)	(3)	(4)
$EPL^R_{t=-1}$	−0.0173	−0.0187	−0.0106	−0.0113
	(0.0084)**	(0.0080)**	(0.0028)***	(0.0037)***
$Tangibility_{t=-1}$		0.1220		0.0780
		(0.0111)***		(0.0055)***
$Size_{t=-1}$		0.0360		0.0270
		(0.0068)***		(0.0026)***
$ROA_{t=-1}$		−0.0880		−0.0516
		(0.0202)***		(0.0077)***
$Market/Book_{t=-1}$		−0.0012		−0.0004
		(0.0008)		(0.0004)
$GDPGrowth_{t=-1}$	−0.0024	−0.0020	0.0004	0.0007
	(0.0010)**	(0.0007)***	(0.0006)	(0.0006)
$GDPperCapita_{t=-1}$	0.0036	0.0028	0.0019	0.0011
	((0.0006)***	(0.0004)****	(0.0004)***	(0.0003)***
$CreditorRights_{t=-1}$	0.0073	0.0011	0.0137	0.0101
	(0.0060)	(0.0065)	(0.0046)***	(0.0050)**
Firm FE	yes	yes	yes	yes
Industry*year FE	yes	yes	yes	yes
R^2	0.72	0.75	0.70	0.72
No. of observations	217,183	178,419	223,651	178,336

This table reports the results of regressions of leverage on the EPL^R indicator. In Columns 1 and 2, we define leverage as total debt over assets, and in Columns 3 and 4, we define leverage as long-term debt over assets. The regressions control for GDP growth, GDP per capita, and creditor rights. Columns 2 and 4 also include the standard leverage firm-level controls (tangibility, size, profitability, and market to book). All controls are lagged by one year. Firm and industry/year fixed effects are included in all columns. Robust standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Standard errors are clustered at the country level. Firm-level controls are winsorized at the 1% tails. Our sample includes twenty-one countries and covers the period 1985–2007. All variables are defined in Appendix A.

negative sign in Columns 1 and 2 (although it is not significant for long-term leverage), consistent with the view that firms increase debt following macroeconomic downturns. GDP per capita is positive and significant, consistent with the idea that leverage is higher when economic development is higher. Creditor rights index is positively correlated with leverage, as demonstrated in the law and finance literature.

2.2 Econometric concerns: Pretreatment trends

The empirical identification in the DID approach presented in Table 3 comes from the comparison of the change in leverage in firms that are subject to a labor law reform (treated firms) with the change in leverage in firms that do not experience such a reform (control firms). One concern with this approach is that the estimated treatment effect could be due to pretreatment differences in the characteristics of treated and control firms. We address this concern in three ways. First, we examine the dynamics of leverage in the years around the enactment of the labor reform. Second, we control for country-specific trends, allowing countries to follow different trends. Third, we match firms according to their geographical location, their industry and size, or other observable characteristics.

2.2.1 Capital structure dynamics and country trends. To examine the dynamic effects of EPL changes on firms' capital structure, in Columns 1, 2, 5 and 6 of Table 4, we replace the EPL indicator with four variables: EPL (+2) is the 2-year forward value of EPL; EPL (+1) is the 1-year forward value of EPL; EPL (0) is the contemporaneous value of EPL; EPL (−1) is the 1-year lagged value of EPL; and EPL (−2) is the 2-year lagged value of EPL. We estimate a similar specification to those presented in Table 3, with firm fixed effects, year/industry fixed effects, firm control variables, and country control variables. Columns 1 and 2 present the results with total debt as our measure of leverage, whereas Columns 5 and 6 present results with long-term debt. The coefficients on EPL (+1) and EPL (+2) allow us to assess whether any leverage effects can be found prior to the passage of labor laws. Finding such an effect of the legislation prior to its introduction could be symptomatic of pretreatment trends in leverage or reverse causation. Across all specifications, we find that the estimated coefficients on EPL (+1) and EPL (+2) are economically and statistically insignificant. This rejects any support for pretreatment trends and reverse causality. Moreover, both EPL (−1) and EPL (−2) are statistically and economically significant across specifications. Similarly to Table 3, we exclude (in Columns 1 and 5) and include (in Columns 2 and 6) firm-level controls to control for the effect of firm-level controls on the EPL coefficient. These findings support our causal interpretation of the results. The long-run effect of EPL on leverage is captured by the coefficient on EPL(−2), which amounts to 111 basis points for total leverage and 73 basis points when leverage is defined by the ratio of long-term debt to assets.

We further check whether different country trends are driving the results. We augment our baseline with country-specific time trends. Results are shown in Columns 3 and 4 and 7 and 8 of Table 4. In this case, the identification of the effects of employment protection on firm leverage comes from whether these labor reforms lead to deviations from pretreatment country-specific trends. Despite the fact that this is a very tough specification, our results remain statistically significant at 10% level for total debt and at 5% level for long-term debt. Our results also remain significant economically.[7]

2.2.2 Matching: Geography and firm-level characteristics. To address further the concern that pretreatment differences between treated and control firms are behind the differential effect on leverage, in Table 5 we adopt a matching approach.

We start controlling for geographical differences between treated and control firms (e.g., a German firm may be quite different from a U.S. firm, even if they are in the same industry, but may be similar in many aspects to an Austrian firm). To control for geography, we define fifteen regions to include bordering

[7] We only present results using total debt going forward. However, all our results are also robust to using long-term debt as our measure of leverage and are available upon request.

Table 4
Dynamic analysis and country trends

	Total debt/assets				Long-term debt/assets			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$EPL^R_{t=+2}$	0.0053	0.0020			−0.0026	−0.0037		
	(0.0073)	(0.0069)			(0.0039)	(0.0051)		
$EPL^R_{t=+1}$	0.0113	0.0122			0.0020	0.0016		
	(0.0084)	(0.0088)			(0.0037)	(0.0039)		
$EPL^R_{t=0}$	−0.0072	−0.0116			0.0002	−0.0009		
	(0.0110)	(0.0120)			(0.0027)	(0.0033)		
$EPL^R_{t=-1}$	−0.0077	−0.0073	−0.0167	−0.0163	−0.0041	−0.0042	−0.0098	−0.0093
	(0.0037)**	(0.0036)**	(0.0092)*	(0.0093)*	(0.0017)**	(0.0019)**	(0.0038)**	(0.0040)**
$EPL^R_{t=-2}$	−0.0142	−0.0111			−0.0095	−0.0073		
	(0.0053)**	(0.0043)**			(0.0039)**	(0.0039)*		
Firm-level controls		yes		yes		yes		yes
Country-level controls	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes	yes	yes
Industry*year FE	yes	yes	yes	yes	yes	yes	yes	yes
Country year trends			yes	yes			yes	yes
R^2	0.77	0.78	0.72	0.75	0.74	0.75	0.72	0.72
No. of observations	139,110	118,420	217,183	178,419	142,843	118,306	223,651	178,336

This table reports the results of regressions of leverage on the two-year lagged, one-year lagged, the contemporaneous, and the one-year and two-year forward values of the EPL^R indicator. In Columns 1–4, we define leverage as total debt over assets, and in Columns 5–8, we define leverage as long-term debt over assets. The regressions control for GDP growth, GDP per capita, and creditor rights. Columns 2, 4, 6, and 8 also include the standard leverage firm-level controls (tangibility, size, profitability, and market to book). All controls are lagged by one year. Firm and industry/year fixed effects are included in all columns. Robust standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Standard errors are clustered at the country level. Firm-level controls are winsorized at the 1% tails. Our sample includes twenty-one countries and covers the period 1985–2007. All variables are defined in Appendix A.

Table 5
Business-cycle effects and matching

	Total debt/assets					
	(1)	(2)	(3)	(4)	(5)	(6)
$EPL^{R}_{t=-1}$	−0.0156	−0.0167	−0.0184	−0.0178	−0.0159	−0.0166
	(0.0019)***	(0.0025)***	(0.0063)***	(0.0071)**	(0.0062)**	(0.0064)**
$EPL^{Other}_{t=-1}$			−0.0006			
			(0.0145)			
Firm-level controls	yes	yes	yes	yes	yes	yes
Country-level controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Industry*year FE	yes	yes	yes	yes	yes	yes
Region*year FE	yes	yes				
R^2	0.75	0.73	0.75	0.73	0.71	0.73
No. of observations	178,419	100,660	178,419	99,449	58,611	108,697

This table regresses leverage (defined as total debt/assets) on the EPL^{R} indicator. Column 3 regresses leverage on both EPL^{R} and EPL^{Other} indicators. EPL^{Other} is computed as the average of EPL^{R} indicator in year t for the neighboring countries. We call neighboring countries those which share common borders. Columns 4–6 use a matched sample for the analysis. Columns 4 and 5 match treated and control firms by industry and size. Column 4 selects control firms using all matching firms that differ at most by a factor of two with respect to the treated firm, whereas Column 5 matches each treated firm to its nearest neighbor in terms of size and industry. Column 6 matches treated and control firms using a multivariate propensity score methodology, selecting control firms within a predefined caliper distance of $\delta = 0.01$. The regressions include the standard leverage firm-level controls (tangibility, size, profitability, and market to book). In addition, they control for GDP growth, GDP per capita, and creditor rights. All controls are lagged by one year. Firm and industry/year fixed effects are included in all columns. Columns 1 and 2 also include region/year fixed effects. Robust standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Standard errors are clustered at the country level. Firm-level controls are winsorized at the 1% tails. Our sample includes twenty-one countries and covers the period 1985–2007. All variables are defined in Appendix A.

countries (for example, the United States and Canada). In Column 1, we add region/year fixed effects to the specification estimated in Column 2 of Table 3. These fixed effects control for shocks that are common to neighboring countries. We report only the coefficient for the EPL indicator, which we find to be negative and strongly statistically significant at the 1% level. In Column 2, we estimate the same specification as in Column 1 in a smaller sample, in which we drop observations with no EPL change in a region. The coefficient of interest remains negative and strongly statistically significant at 1% level.

Interestingly, these tests also alleviate concerns that changes in local macroeconomic conditions are driving our results. Although we control for the standard macroeconomic variables in our regressions, including region-specific shocks in these specifications absorbs any variation at the regional level that might be driving our results. Following Heider and Ljungqvist (2013), we use information on geography to develop a placebo test that further addresses this concern. For each country k and year t, we compute the average of EPL^{R} indicator in year t for the neighboring countries (EPL^{Other}). To the extent that a local economic shock is also affecting neighboring countries, we would expect to find a negative and significant coefficient on EPL^{Other}. In Column 3 of Table 5, we add EPL^{Other} to the basic specification, and we find that it is not statistically different from zero, whereas the coefficient on the EPL indicator for

country k itself remains statistically and economically significant. This result suggests that the effect on leverage is not driven by business-cycle effects.

A further concern with our analysis is that treated and control firms may differ in terms of basic characteristics like size and industry. In Columns 4 and 5, we address this concern by selecting as control firms only those that match the treated firms in terms of size and industry.[8] The number of control firms varies depending on the choice of the maximum difference in size required for a match. In Column 4, we report the results selecting control firms with replacement using all matching firms that differ at most by a factor of two with respect to the treated firm.[9] In Column 5, we report the results matching each treated firms to its nearest neighbor in terms of size and industry. In both cases, the effect of labor reforms on leverage is unchanged.

In Column 6, we match treated and control firms using a multivariate propensity score methodology. We estimate a probit model of the probability of being treated as a function of firm-level characteristics (besides size and industry, we include profitability, tangibility, and growth opportunities) and country-level characteristics (GDP growth, per capita GDP, and creditor rights). For each observation in our sample, we compute a propensity score as the predicted value of the probability of being treated according to the probit model. We report the results selecting control firms within a predefined propensity score distance with caliper $\delta = 0.01$.

Across all specifications, the coefficient on the EPL indicator is virtually unchanged from the specification estimated in Table 3. This alleviates concerns that pretreatment differences may be responsible for our results.

2.3 Econometric concerns: Confounding changes

We already mentioned in the previous section that business-cycle effects might be responsible for our findings, and we have already addressed some of these concerns by controlling for GDP growth in Table 3, country-specific trends in Table 4, and geography in Table 5. A further concern with respect to a causal interpretation of our findings is that other reforms may be also happening at the same time as the change in employment protection (e.g., tax changes). In this case, the change of leverage may be incorrectly attributed to employment protection because of an omitted variables problem. We address this concern by studying the political economy of labor reforms and by controlling for corporate and personal taxation and other country-level variables. We also consider the differential impact of a change in EPL on different firms within a country.

2.3.1 Political economy of EPL. In this section, we tackle the concern that changes in labor regulation may be endogenous to firms' financial decisions.

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[8] Here, we matched by 4-digit SIC industry. Results are similar if we match at a 2-digit SIC industry.

[9] Specifically, we select only the control firms for which max $(Total\ Assets_{Treated}, Total\ Assets_{Control})$ / min $(Total\ Assets_{Treated}, Total\ Assets_{Control}) < 2$.

This concern is likely to be less severe than in the case of changes in variables that are directly affected by the choice of an individual firm, such as the unionization rate. This is because changes in EPL reflect changes in laws and are thus not directly affected by the decisions of individual firms. However, there still may be some concerns about the political economy of changes in labor protection. After all, firms or unions may lobby politicians to change labor regulations when it is in their interest to do so.

The existing empirical studies on the political economy of labor regulation do not find much support for a lobbying explanation. Botero et al. (2004) show that legal origin and economic development are the most important determinants of labor regulation, with ideology being of secondary importance. Moreover, existing political economy models have highlighted several potential determinants of employment protection that are exogenous to firms' decisions: Saint-Paul (2002) indicates that greater employment protection is likely to emerge in countries in which employees have greater bargaining power; Pagano and Volpin (2005) predict that lower employment protection is likely to emerge as a political outcome in countries with majoritarian (as compared with proportional) electoral rules; and Perotti and Von Thadden (2006) argue that when financial wealth is more concentrated, labor rents and labor protection are higher.

In untabulated regressions, we study the determinants of changes in EPL, estimating a linear probability model at the country/year level. We consider the lagged values of unemployment rate, union density, and centralized bargaining as proxies for the rigidity of the labor market, the lagged proportionality index as a measure of the proportionality of the electoral system, the lagged value of income inequality as a proxy for wealth inequality, an indicator for left-wing governments (also lagged) to capture ideology, and an indicator for technological progress at the country-year level (as proxied by a measure of multifactor productivity). Further determinants of changes in EPL may be the characteristics of the median firm in the country. If the median firm in a country is larger or older, firms may be more effective in lobbying politicians for a reduction in EPL. We also control for lagged GDP growth and a lagged indicator capturing recession years to control for business-cycle effects and GDP per capita to capture general economic development. We include year dummy and country fixed effects to capture general macroeconomic conditions and country-level heterogeneity. We cluster standard errors at the country level.

We find that union density and income inequality (as proxied by the Gini indicator) are the only variables that are statistically significant. EPL increases when a country changes toward more unionized labor markets and when income inequality increases. In the next section, we will control for these potential omitted variables.

2.3.2 Omitted variables. In Table 6, we consider several possible omitted variables. We start with those variables that were significant in our political

Table 6
EPL and leverage: Political economy

	Total debt/assets				
	(1)	(2)	(3)	(4)	(5)
$EPL^{R}_{t=-1}$	−0.0166	−0.0166	−0.0177	−0.0147	−0.0135
	(0.0078)**	(0.0077)**	(0.0074)**	(0.0035)***	(0.0030)***
$UnionDensity_{t=-1}$	−0.0020	−0.0022	−0.0019	−0.0014	−0.0012
	(0.0006)***	(0.0007)***	(0.0007)**	(0.0006)**	(0.0007)*
$Gini_{t=-1}$		0.0007	0.0008	0.0002	0.0006
		(0.0005)	(0.0004)*	(0.0004)	(0.0003)*
$BargainingCentralization_{t=-1}$			0.0171	0.0135	0.0116
			(0.0054)***	(0.0036)***	(0.0030)***
$UnemploymentRate_{t=-1}$			−0.0019	−0.0037	−0.0041
			(0.0016)	(0.0011)***	(0.0013)***
$Recession_{t=-1}$				0.0060	0.0054
				(0.0014)***	(0.0018)***
$Taxes_{t=-1}$					0.0703
					(0.0245)***
Firm-level controls	yes	yes	yes	yes	yes
Other country-level controls	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes
Industry*year FE	yes	yes	yes	yes	yes
R^2	0.75	0.71	0.71	0.76	0.72
No. of observations	178,419	171,561	169,265	150,806	145,814

This table regresses leverage (defined as total debt/assets) on the EPL^{R} indicator, controlling for potential omitted variables. Column 1 controls for union density. Column 2 adds the Gini coefficient, and Column 3 additionally controls for bargaining centralization and unemployment rates. Column 4 includes an indicator of recession to the previous set of controls, whereas Column 5 adds the miller tax indicator. The regressions include the standard leverage firm-level controls (tangibility, size, profitability, and market to book). In addition, they control for GDP growth, GDP per capita, and creditor rights. All controls are lagged by one year. Firm and industry/year fixed effects are included in all columns. Robust standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Standard errors are clustered at the country level. Firm-level controls are winsorized at the 1% tails. Our sample includes twenty-one countries and covers the period 1985–2007. All variables are defined in Appendix A.

economy analysis, namely, union density and income inequality. We then control for additional variables to capture potential confounding effects that may be biasing our findings.

In Column 1, we add union density to the specification estimated in Column 2 of Table 3. We find that union density is statistically (and economically) significant, but its addition does not change the coefficient on the EPL indicator. Here, it is worth noting that the coefficient of union density is negative, which is not consistent with the theory of debt as a bargaining tool. Column 2 adds the Gini coefficient of income inequality, which is not statistically significant and does not change the results.

In Column 3, we look at whether EPL is proxying for other employment indicators. In addition to union density, we include centralized bargaining and unemployment rates as proxies for the rigidity of the labor market. We find that leverage increases in countries in which bargaining becomes more centralized and decreases in countries in which union density increases. The coefficient on unemployment rates is not statistically significant. The coefficient on employment protection is not affected by the addition of these variables.

Hence, the EPL channel is independent of the effects of unionization or how centralized bargaining is.

In Column 4, we add an indicator of recession. This variable is strongly correlated with leverage. EPL is statistically significant at the 1% level. Finally, in Column 5, we consider tax reforms. Using data from Faccio and Xu (Forthcoming), we construct the Miller tax index, which measures the relative tax advantage of debt versus equity, including personal and corporate taxes. As expected, leverage increases in the Miller tax index. The coefficient of EPL is still significant at 1% level but is smaller in magnitude.

Although we cannot completely rule out the possibility that some unobserved variable may be driving the results, the robustness of our findings across different specifications reduces the omitted variable concerns.

2.3.3 Cross-sectional heterogeneity. As an additional test, we exploit cross-sectional variation in labor turnover across different industries. Because the economic effect we are emphasizing goes through the labor channel, it is natural to expect firms in sectors that experience high labor turnover to be more likely to be affected by a change in EPL. The assumption is that industries share common characteristics (e.g., technological) across countries. Higher values of this variable mean that these industries require higher labor turnover for their operations, and therefore increases in employment protection would impact these industries more negatively.

This test can be understood as follows: we sort firms into buckets that differ in terms of sensitivity to the treatment and test whether the impact of a change in EPL is greater in firms with higher sensitivity to the treatment. The point of such an exercise is not to estimate the direct effect of EPL on leverage, but its differential effect across firms that differ in terms of labor turnover. To the extent that the omitted variables are uncorrelated with labor turnover, the estimate can be interpreted as a triple-difference effect. In other words, the identification assumption is that omitted variables have a similar impact on leverage across buckets.

Therefore, in Table 7, we estimate the following regression specification:

$$y_{it} = \alpha_j \cdot \gamma_t + \lambda_i + \delta \cdot EPL^R_{k,t-1} + \zeta \cdot E_j + \theta \cdot \left(EPL^R_{k,t-1} \times E_j\right) + \beta \cdot X_{it-1} + \epsilon_{it}. \tag{3}$$

As before, we include firm fixed effects, industry/year fixed effects, firm-level controls, and country-level controls. E_j denotes a measure of labor turnover for industry j in year t based on data provided by Davis, Haltiwanger, and Schuh (1996) (at the 2-digit SIC industry level) for the United States. This measure is available only for manufacturing industries.

In Column 1, we find that the interaction of EPL and turnover is negative and statistically significant at the 5% level. The coefficient on EPL by itself loses statistical significance. In Column 2, we also control for country/year fixed effects. In such a specification we cannot estimate the direct effect of EPL on

Table 7
Cross-sectional heterogeneity: Labor turnover

	Total debt/assets			
	(1)	(2)	(3)	(4)
$EPL^R_{t=-1}$	0.0305		0.0075	
	(0.0245)		(0.0122)	
$EPL^R_{t=-1}$*Turnover	−0.0056	−0.0057	−0.0025	−0.0022
	(0.0027)**	(0.0027)**	(0.0012)**	(0.0011)*
Firm-level controls	yes	yes	yes	yes
Country-level controls	yes		yes	
Firm FE	yes	yes	yes	yes
Industry*year FE	yes	yes	yes	yes
Country*year FE		yes		yes
Drop U.S.			yes	yes
R^2	0.72	0.73	0.74	0.75
No. of observations	91,472	91,472	59,569	59,569

This table reports the results of regressions of cross-sectional heterogeneity. Total debt over assets is regressed on the interaction of EPL^R indicator with labor turnover. Turnover is a proxy for employment turnover calculated using data by Davis, Haltiwanger, and Schuh (1996), available for manufacturing industries in the United States. United States observations are included in Columns 1 and 2 but are not included in Columns 3 and 4. Firm and industry/year fixed effects are included in all columns. Columns 2 and 4 also include country/year fixed effects, which absorb the level EPL effect. The turnover variable is also absorbed and thus cannot be estimated. The regressions include the standard leverage firm-level controls (tangibility, size, profitability, and market to book). In addition, they control for GDP growth, GDP per capita, and creditor rights. All controls are lagged by one year. Firm and industry/year fixed effects are included in all columns. Robust standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Standard errors are clustered at the country level. Firm-level controls are winsorized at the 1% tails. Our sample includes twenty-one countries and covers the period 1985–2007. All variables are defined in Appendix A.

leverage, but we can estimate the differential effect of a change in EPL on firms in sectors that differ in their labor turnover. The estimated coefficient for this interaction term is in fact more carefully estimated in this setup because the country/year fixed effects control for other factors that vary at the country/year level. This addresses concerns that there could be changes at the country level, such as changes in the tax rates, which can impact firms' leverage and coincide with the labor regulation changes. The coefficient on the interaction of EPL and turnover is negative and statistically significant at the 5% level, which is consistent with our intuition.

As in Rajan and Zingales (1998), one concern might be that including the United States in this regression might bias our results because we define labor turnover using U.S. data. To address this concern, in Columns 3 and 4, we repeat the same analysis dropping U.S. firms. Our results remain statistically significant. In summary, across all specifications, we find that an increase in EPL is associated with a greater decrease in leverage in sectors with more labor turnover.

3. Further Tests

In this section, we show that the results are robust to alternative definitions of leverage and alternative indicators of employment protection.

Table 8
Alternative definitions of leverage

	Log (total debt)	Log (long-term debt)	Total debt/market value assets	Long-term debt/market value of assets	Net debt/book assets	Net debt/market assets
	(1)	(2)	(3)	(4)	(5)	(6)
$EPL^R_{t=-1}$	−0.109	−0.136	−0.0248	−0.0147	−0.0217	−0.0320
	(0.0334)***	(0.0469)***	(0.0161)	(0.0059)**	(0.0116)*	(0.0133)**
Firm-level controls	yes	yes	yes	yes	yes	yes
Country-level controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Industry*year FE	yes	yes	yes	yes	yes	yes
R^2	0.90	0.87	0.75	0.70	0.77	0.73
No. of observations	178,419	178,336	175,920	175,910	159,920	157,379

This table reports the results of regressions of alternative definitions of leverage on the EPL^R indicator. Leverage is defined as logarithm of one plus total debt (Column 1), logarithm of one plus long-term debt (Column 2), total debt over market value of assets (Column 3), long-term debt over market value of assets (Column 4), net debt over book value of assets (Column 5), and net debt over market value of assets (Column 6). The regressions include the standard leverage firm-level controls (tangibility, size, profitability, and market to book). In addition, they control for GDP growth, GDP per capita, and creditor rights. All controls are lagged by one year. Firm and industry/year fixed effects are included in all columns. Robust standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Standard errors are clustered at the country level. Firm-level controls are winsorized at the 1% tails. Our sample includes twenty-one countries and covers the period 1985–2007. All variables are defined in Appendix A.

3.1 Alternative definitions of leverage

Our findings are robust across definitions of leverage. As our main measure of leverage, we use the ratio of the book value of total debt over total assets and long-term debt over book value of assets. For robustness, in Table 8, we consider three alternative definitions of leverage: the logarithm of total debt (and the logarithm of long-term debt), market leverage (where we replace the book value of equity with its market value), and net debt (defined as debt net of cash).

Columns 1 and 2 of Table 8 report the coefficient for EPL of a regression similar to the one in Table 3, Column 2, where the dependent variable is the logarithm of one plus total debt (Column 1) or the logarithm of one plus long-term debt (Column 2). This specification deals with the concern that the uncovered negative effect on leverage is due to an increase in assets (at the denominator) rather than a reduction in leverage (at the numerator) following an increase in EPL. The fact that the coefficient on EPL is strongly significant (at the 1% level) and negative in both columns addresses this concern.

In Columns 3 and 4, the dependent variable is market leverage measured as the ratio of total debt over market value of assets (in Column 3) and long-term debt over market value of assets (in Column 4). The coefficient on EPL is marginally insignificant (in Column 3), significant at the 5% level (in Column 4), and negative in both cases. The weaker results with market leverage may be due to the fact that prolabor regulation is treated as negative news by the

market and thus may reduce the market value of equity. Evidence provided by Ruback and Zimmerman (1984), Abowd (1989), Hirsch (1991), and Lee and Mas (2012) shows that labor union coverage has a negative association with U.S. firms' earnings and market values. This biases against finding any results.

Columns 5 and 6 estimate the same specification using net debt over total assets (in Column 5) and net debt over market value of assets (in Column 6). This deals with concerns that an increase in EPL may lead firms to reduce cash. This reduction may more than offset the reduction in debt, making firms effectively more levered after, rather than before, the reform. Because the coefficients are significant at the 10% level (Column 5), at the 5% level (Column 6), and negative, these concerns are dismissed.

To summarize, the results in Table 8 are similar to those in Table 3: across all our specifications, increases in labor protection are associated with decreases in leverage.

3.2 Alternative measure of EPL

In Table 9, we consider alternative measures of EPL to check the robustness of our results.

In Columns 1–4, we measure employment protection with EPL^A, that is, the indicator developed by Allard (2005). The main difference between EPL^A and EPL^R is that EPL^R focuses on large changes in the law, whereas EPL^A also considers smaller changes.

In Columns 1 and 2, we replicate our baseline specifications as reported in Table 3. In Column 1, we control for industry/year fixed effects (to control for industry-level dynamics), firm fixed effects (to control for firm-specific characteristics), and country-level control variables, leaving out the firm-level control variables. In Column 2, we also add the firm-level control variables. The coefficients on the EPL indicator are identical in the two specifications, indicating that EPL changes are not systematically correlated with firm level variables; that is, they are random at the firm level. The magnitude of the coefficient is larger than in Table 3: a 1-unit increase of EPL^A is associated with a 300-basis-point decrease in leverage, or 15% relative to the median leverage. This indicates that focusing only on major reforms (as done when using EPL^R) leads to an underestimation of the true effect of employment protection on leverage.

In Columns 3 and 4, we replicate some of the dynamic analysis as reported in Table 4. Column 3 adds country-specific trends to the specification of Column 2 to address whether different country trends are driving the results. In this case, the identification of the effects of employment protection on firm leverage comes from whether these labor reforms lead to deviations from pretreatment country-specific trends. Despite the fact that this is a very tough specification, our results remain statistically significant at the 10% level. Instead, Column 4

Table 9
Employment protection: Robustness

	Total debt/assets					
	(1)	(2)	(3)	(4)	(5)	(6)
$EPL^A_{t=+2}$				−0.0096		
				(0.0083)		
$EPL^A_{t=+1}$				0.0195		
				(0.0131)		
$EPL^A_{t=0}$				−0.0217		
				(0.0158)		
$EPL^A_{t=-1}$	−0.0300	−0.0300	−0.0230	−0.0074		
	(0.013)**	(0.012)**	(0.0128)*	(0.0056)		
$EPL^A_{t=-2}$				−0.0124		
				(0.0052)**		
$EPL^A_{t=-1}$*Rule of law$_{t=-1}$					−0.0308	
					(0.0107)***	
$EPL^R_{t=-1}$*Rule of law$_{t=-1}$						−0.0174
						(0.0083)**
Firm-level controls		yes	yes	yes	yes	yes
Country-level controls	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Industry*year FE	yes	yes	yes	yes	yes	yes
Country year trends			yes			
R^2	0.73	0.76	0.76	0.80	0.76	0.75
No. of observations	217,183	178,419	178,419	118,420	178,419	178,419

This table reports the results of regressions of leverage (defined as total debt/assets) on alternative measures of the EPL indicator. Columns 1–4 use EPL^A, the EPL indicator produced by Allard (2005). Column 5 uses the EPL^A indicator scaled by the rule of law indicator to account for the quality of legal enforcement, whereas Column 6 uses the EPL^R indicator scaled by the rule of law. The regressions control for GDP growth, GDP per capita, and creditor rights. Columns 2–6 also include the standard leverage firm-level controls (tangibility, size, profitability, and market to book). All controls are lagged by one year. Firm and industry/year fixed effects are included in all columns. Robust standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Standard errors are clustered at the country level. Firm-level controls are winsorized at the 1% tails. Our sample includes twenty-one countries and covers the period 1985–2007. All variables are defined in Appendix A.

examines the dynamics of capital structure as presented in Column 2 of Table 4 to check whether there are pretreatment trends driving the analysis. We find that the estimated coefficients on EPL (+1) and EPL (+2) are economically and statistically insignificant. Therefore, there is no evidence for pretreatment trends and reverse causality. We find that the coefficient on EPL(-2) is negative and significant at the 5% level, indicating that the long-run effect on leverage is 124 basis points.

A concern with our indicators might be that they do not account for the effectiveness of litigation in different countries. If laws are not well enforced, then they are less effective. In Columns 5 and 6, we take into account the quality of legal enforcement as measured by rule of law. We interact the rule of law indicator with EPL^A in Column 5, and with EPL^R in Column 6. The results are somewhat stronger than those in the baseline specification: a unit increase in EPL^A* Rule of Law is associated with a decrease in leverage of 308 basis points, whereas an increase in EPL^R* Rule of Law is associated with a decrease in leverage of 174 basis points.

4. Conclusion

The recent financial crisis and the subsequent global recession have once again brought the contentious topic of employment protection to the fore of the policy debate. Policy makers around the world are contemplating how to reform the rules that govern industrial relations. Although some countries, such as the United States, are moving toward greater labor protection,[10] continental European countries are discussing how to amend their current labor laws in an attempt to boost potential growth.[11] These recent developments demonstrate the need to revisit the role of labor in shaping corporate behavior and pin down the economic channel through which labor may affect firms' economic activities.

Using firm-level data from twenty-one OECD countries over the 1985–2007 period, we find that leverage decreases when employment protection increases, a result that is robust across all specifications. Our theoretical argument is that prolabor regulations, by making it difficult for firms to adjust labor force, increase labor market rigidity. This makes the labor claim resemble a debt claim, where wages can be construed as coupon payments on debt. This transformation of labor claim into a senior debt-like claim crowds out financial leverage. We also find that increases in employment protection have more negative effects on firms' leverage for firms for which hiring and firing is more frequent. Our findings provide supporting evidence for the critics of employment protection laws by showing that labor-friendly regulation may have negative real effects via a finance channel.

Appendix A: Variable Definitions

A.1 Firm-level variables

Total Debt (Book)/Assets is defined as the sum of long-term debt and short-term debt over the book value of assets (source: Worldscope).

Long-term Debt (Book)/Assets is defined as long-term debt over the book value of assets (source: Worldscope).

Total Debt (Market)/Assets is defined as the sum of total debt over the sum of total debt (book) and the market value of equity (source: Worldscope).

Long-term Debt (Market)/Assets is defined as the sum of long-term debt over the sum of long-term debt (book) and the market value of equity (source: Worldscope).

Net Debt (Book)/Assets is defined as total debt minus cash over the book value of assets (source: Worldscope).

Net Debt (Market)/Assets is defined as total debt minus cash over the sum of total debt (book) and the market value of equity (source: Worldscope).

[10] The concern among some commentators and policy makers is that the flexible labor laws in the United States may have exacerbated the crisis, as the United States may have become a dumping ground for multinational firms' unemployment.

[11] For instance, a comprehensive labor market reform is one of the main requirements that were imposed on the Greek government to receive financial support from its European counterparts.

Log Total Debt is defined as the natural logarithm of one plus total debt (source: Worldscope).

Log Long-term Debt is defined as the natural logarithm of one plus long-term debt (source: Worldscope).

ROA (return on assets) is defined as earnings before interest and taxes over the book value of assets (source: Worldscope).

Firm Size is defined as the natural logarithm of total real assets (source: Worldscope).

Tangibility is defined as net property, plant, and equipment, over the book value of assets (source: Worldscope).

Market/Book is defined as the ratio of the market value of equity plus book value of debt over the book value of debt plus equity (source: Worldscope).

A.2 Country-level variables

GDP growth rate is the real annual growth rate in gross domestic product (GDP) (source: IMF Database).

GDP per capita is GDP in thousands of U.S. Dollars devided by total population (source: IMF Database).

Creditor Rights is an indicator that measures creditor power and scores between zero and four (source: Djankov, McLiesh, and Shleifer 2007).

Union Density is the ratio of union membership divided by employment (source: OECD).

Unemployment Rate is the ratio of unemployment divided by labor force (source: OECD).

Centralization Indicator is an indicator of bargaining centralization that scores between one and five. It is increasing in the degree of centralization. A value of one means that bargaining is at the company/plant level, and a value of five means that central-level agreements at the country-level are of overriding importance (source: OECD).

Income Inequality is the Gini indicator of income inequality (source: Deininger and Squire Database of Income Inequality).

Recession Indicator takes a value of one if there are two consecutive quarters with negative gdp growth in a country and zero otherwise (source: OECD).

Miller Tax Index is computed as 1 - [(1 - corporate tax rate)·(1 - personal dividend rate) /(1-personal interest rate)] (source: Faccio and Xu (Forthcoming)).

Rule of Law Indicator takes values between zero and one and characterizes legal enforcement in different countries (source: International Country Risk).

Appendix B: Labor Law Reforms

We searched a number of sources, listed below each country for changes in labor laws in the twenty-one countries of our sample over the 1985–2007 period. Mainly, we focus on changes in the procedural requirements that need to be followed when firing an employee with a regular employment contract, the notice and severance pay requirements, the prevailing standards of (and penalties for) "unfair" dismissals, the conditions under which temporary contracts can be offered, the maximum number of successive renewals and the maximum cumulated duration of these contract, the notification requirements provided by law in case of collective dismissals and the associated delays and costs for the employers.

Australia: A reform was passed in 2005 with a significant reduction of the concept and applicability of unfair dismissal. The Workplace Relations Amendment Bill removed protection against unfair dismissal for employees employed in firms with less than 100 employees. Employees of larger firms were also affected as unfair dismissal can no longer be claimed for reasons that include genuine operational reasons. Source: Vranken (2011).

Austria: There were two changes in EPL. A 1988 law increased the protection of workers with temporary contracts. In particular, the law changed the conditions under which the employees are placed at the disposal of third parties. It also outlawed agreements that are at the disadvantage of the employee. A reduction in EPL happened in 2003. The main reform was the introduction of private job placement agencies to offer services as temporary work agencies. This facilitated the development of the market for temporary workers. Sources: LABREF, OECD.

Belgium: The main change was the Employment Law of 1998. This law was prompted by the closure of a plant by Renault in 1997. Hence, it became known as the Renault law. The plant closure was sudden and unexpected. The law introduced a procedure to inform workers and enable labor negotiations in case of mass layoffs or plant closures. In particular, the law introduced the obligation of employers to consult with workers representatives and to analyze and formally respond to any proposals from workers representatives. Source: ILO.

Canada: There was no change in EPL according to all our sources and direct searches. Source: ILO.

Denmark: The main change in EPL was in 1990 with the deregulation of temporary work agencies (TWAs). Danish law now provides free entitlement to establishing and operating TWAs. This reform significantly expanded the use of temporary work contracts. Source: EIRO.

Finland: There was no major change in EPL according to our sources and direct searches. Source: fondazione Rodolfo De Benedetti (fRDB).

France: There were two reforms that increased EPL during this period. In 1990, a law imposed restrictions on firing. Collective redundancies must be accompanied by "social plans" (such as early retirement schemes, retraining contracts, etc.). The drawing of social plans is compulsory for firms with more than fifty employees, where ten or more people are to be made redundant over a 30-day period. All eligible individuals dismissed for economic reasons are offered a retraining contract. Employers are obliged to state in all cases the reason for dismissal, even when not requested by the employee. All eligible individuals dismissed for economic reasons are offered a retraining contract, jointly financed by the state and the companies. If the employer does not correctly follow the formalities, then the employer must pay the employee one month's wages in compensation. In 1993, a law transferred further powers to the public administration in drafting the social plans. If there is no social plan, or the measures proposed are inadequate, then the redundancies will be considered null and void. Workers representatives must be consulted about the plan before the notice of termination is issued. The plan must also be submitted to the Departmental Labor Inspectorate, which may propose changes if it considers the measures to be taken insufficient. Source: Bertola et al. (1999), fRDB.

Germany: EPL's main reform occured in 1997. Legal conditions covering dismissals were relaxed. The size threshold for dismissal protection was doubled. In the event of redundancies, employers have to take into consideration stricter social selection criteria: for example, age, length of service, or the family situation of the employee must be taken into account. Temporary contracts were fully liberalized. In the Labor Placement Act, the renewal period for fixed-term contracts was extended and the admissible frequency of renewals was increased. Source: OECD, fRDB.

Greece: A reform occured in 1988 to harmonize Greek legislation with the European Council Directive of 14 February 1977 (77/187/CEE). The law specified what are the rights and obligations of the new owner of the firm, which were less clear and thus less restrictive before. It also increased the protection accorded to collective contracts (Respect of collective contracts - Presidential Decree n. 572). Source: Natlex.

Ireland: A reform was passed in 2003, but we decided that the change is not of similar importance to the rest, and thus we do not report it. Source: Labref and OECD.

Italy: There were two important changes during this period. In 1991, a law on collective redundancies was passed. The employer has the duty to inform union representatives about the reasons for the proposed layoffs. In particular, the employer needs to show the unions that it is not possible to take alternative measures to the intended dismissals. The employer also needs to describe the measures that are planned to mitigate the social consequences of the collective dismissal. The unions may request an examination of the reasons for the layoffs and the possibilities of utilizing the workforce in different ways within the firm. If the parties fail to reach an agreement, the next step is a conciliation phase conducted by the Labor Office. Failure to follow the procedure properly is penalized by the obligation to reinstate the employees who have thus been dismissed unlawfully. In 1998, there was a reduction in EPL through a deregulation of temporary contracts. Agencies were created to carry out the activities of selection, employment, and placement of temporary workers. "Atypical" contracts were encouraged and the automatic transformation of fixed-term contracts into open-end ones was removed (Law 24/06/1997 n.196. "Treu Package"). Source: fRDB, EIRO.

Japan: There was no change in EPL according to all our sources and direct searches. Source: ILO.

Netherlands: There were two reforms, both of which decreased employment protection. In 1988, requirements for dismissals were eased for cases of bankruptcy. Companies no longer need to get authorization for dismissals from the Public Employment Office. In 1999, the Flexibility and Security Act was passed. The aim of the law was to provide more room to extend the duration of fixed-term contracts, to strengthen the legal position of temporary workers and stand-by workers and to allow for temporary contracts to be used over a longer period. Moreover, in 1999, procedures for dismissal on economic, technical, and organizational grounds were shortened and simplified. Once a dismissal permit has been granted, the employer can give notice of termination. The new law shortens and simplifies notice. At least one month's notice must be given, and the maximum term of notice will be four months (before it was six months). Source: EIRO, fRDB, OECD.

New Zealand: There was no change in EPL according to all our sources and direct searches. Source: OECD.

Norway: In 1994, reforms introduced the use of contracted labor and temporary employment. Temporary working agencies were also deregulated and their scope was extended. The duration of temporary work contracts was also increased. Source: OECD.

Portugal: EPL increased in 1989. Part-time employees were given the same contractual rights as full-time employees (among them, the right to claim for unfair dismissal). Moreover, regulations were introduced with respect to the conditions governing the application of fixed-term contracts. In 1991, a labor reform had the effect of decreasing EPL. Firing restrictions were eased, through a wider range of admissible layoffs. For instance, dismissals for unsuitability were authorized. Source: OECD.

Spain: The major change occured in 1994. This reform saw the amendment of many of the pre-existing labor laws (the Workers' Statute, the Labor Procedure Act and the Labor Offences and Sanctions Act) and the adoption of a number of new laws. The major aspects of the reform are the following: (1) the public monopoly of job placement was lifted to allow the operation of private employment agencies and temporary work agencies, (2) there was a reduction of the costs of individual dismissal (for instance, there was a restriction of the number of cases in which employees are awarded compensation in case of dismissal), and (3) there was also a relaxation of the rules governing collective dismissals. For example, the legally acceptable reasons for collective dismissals have been extended to include production and organizational changes in order to guarantee the future viability of the firm. Source: ILO, fRDB.

Sweden: In 1993, temporary contracts were deregulated, permitting temporary work agencies to operate. The reform also amended the Employment Protection Act, allowing firms to decide more flexibly whether to retain workers of their own choice in redundancy situations (i.e., the

last-in-first-out rule was relaxed). A further reduction in EPL happened in 1997. Since the reform, firms' rehiring obligations vis-à-vis dismissed workers expire after nine months (instead of twelve). Twelve-month fixed-term contracts are available with no restrictions and can be extended up to 18 months. The criteria determining the length of notice periods were also relaxed. Source: OECD, Natlex, fRDB.

Switzerland: An increase in EPL happened in 1994. New provisions on collective redundancies apply to an employer if at least 10% of the workforce are dismissed. The employer has an obligation to give advance notice to employees or their representatives of the collective redundancy and the reasons for it. The representatives may make proposals on how the dismissals could be avoided or reduced but cannot block the dismissals. After consultation with employees or their representatives, the employer has to notify the cantonal employment office of the intended redundancies and the results of the consultation process. The sanctions for failure to consult will be fixed by a judge and may not exceed an amount equal to twice the employee's monthly salary. Source: Practical Law.

United Kingdom: There was no major change in EPL according to our sources and direct searches. Source: fRDB.

United States: There was an increase in EPL in 1989. The WARN Act was a federal law that required employers to give written notice of 60 days, before the date of a mass layoff or plant closing, to affected workers, the chief elected official of the local government where the employment site is located, and the State Rapid Response Dislocated Worker Unit. Subject to the law are private employers with 100 or more full-time employees. In the case of noncompliance, employees, their representatives, and units of local government can bring lawsuits against employers. Employers who violate the WARN Act are liable for damages in the form of back pay and benefits to affected employees. Source: Levine (2007).

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