# Employment Protection Laws and Privatization*

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**Abstract**

Do employment protection laws hinder privatization? Using privatization deals in fourteen European countries from 1977-2003 and within-country variation in employment protection laws, we find that stringent employment protection laws significantly deter privatization. The fear of job cuts apparently leads organized labor in the state-owned enterprises to vehemently oppose privatization. We find that stringent employment protection laws inhibit privatization *disproportionately* more in industries that are less productive and require lower level of job skill, consistent with the fear of retrenchment being greater in the less-productive and low-skilled sectors. We also find that stringent employment protection laws inhibit privatization *disproportionately* more in unionized industries, consistent with the fact that workers in these industries exert considerable political pressure. To obtain these results exploiting inter-industry differences, we use industry-level measures for the United States as an instrument to alleviate potential endogeneity concerns. We employ panel regressions that include fixed effects to control for unobserved factors at the country, industry and year levels. We also examine specifications including country-specific and industry-specific trends to account for spurious correlations stemming from such trends in privatization and in enacting employment protection laws. Our results are also robust to controlling for endogenous changes in employment protection laws due to: (i) changes in a country's government, specifically adopting or rejecting a left-of-center political stance; (ii) trade liberalization; and (iii) country-level economic growth.

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# Employment Protection Laws and Privatization

## 1. Introduction

Privatization – broadly defined as the deliberate sale by a government of state-owned enterprises (SOEs) or assets to private economic agents – has been a centerpiece of national government policy over the last three decades. Since 1977, 190+ national governments around the world have raised almost $2.0 trillion by selling state-owned enterprises to private entities, mostly through public share offerings. Countries could raise a similar or even larger amount by selling stock currently held in fully or partially state-owned companies. For example, European governments could raise more than $450 billion by offloading their remaining stakes in partially privatized firms (Ng and Bartsch, 2009). The Chinese, Russian, and Saudi governments alone retain stakes worth almost $1.7 trillion in partially privatized, publicly traded firms (Megginson, 2010). These amounts do not include entire industries — most notably electric utilities and oil and gas production — that remain 100% state-owned in several countries.

Despite this potential revenue windfall, and the desperate fiscal needs of many national budgets, governments the world over are extremely reluctant to renew large-scale privatization programs. A key reason for this hesitancy is that state-owned enterprises (SOEs) often employ excess workers, and employ them relatively inefficiently (see Banerji and Sabot (1994), Schleifer and Vishny (1994) and Dinç and Gupta (2011)). As a result, national governments fear that privatization will result in large scale labor force restructuring either before or after divestiture, though evidence supporting such labor force restructuring is conflicting (see section 2 for details).

Since labor retrenchment is regulated in most countries through employment protection laws, the above reluctance to privatize raises a fundamental question: Do employment protection laws hinder privatization? Using a sample of privatization deals in fourteen OECD countries from 1977-2003 and within-country variation in employment protection laws, we find that stringent employment protection laws indeed deter privatization significantly. Furthermore, employment protection laws inhibit privatization *disproportionately* more in industries that are less productive, require lower level of job skill and are more unionized.

To produce this evidence, we use data on privatizations from the Privatization Barometer, which contains a comprehensive list of privatization transactions from 1977 onwards for all European countries. For our empirical analysis, we use two different samples for privatization: one aggregated at the country, year level and the other aggregated at the country, industry, year level. We examine the number of privatizations as well as the total value of the privatization deals (in $ billions).

We use the Employment Protection Law (EPL) index sourced from Allard (2005), who analyzes in detail the evolution of employment protection legislation across the OECD countries for each year

from 1950 to 2003 to generate the EPL index. Apart from the long time-series, which captures comprehensively all country-level changes in employment protection laws, the EPL index covers comprehensively all aspects of employment protection legislation for the OECD countries. This index has been constructed by surveying existing laws and regulations in OECD countries; the final scores have been reviewed and corrected by each of the national governments. The index covers eighteen aspects of employment protection legislation grouped into three broad categories: (i) laws protecting workers with regular contracts; (ii) laws affecting workers with fixed-term/temporary contracts; and (iii) regulations concerning collective dismissals.

To examine if stringent employment protection laws discourage privatization, first, we employ fixed effects panel regressions using the country, year sample, where we include fixed effects for the country and year of privatization. Second, since industries in which privatization is undertaken may be correlated with unobserved country-level factors, we employ fixed effects panel regressions using the country, industry, year sample, where we include fixed effects for the country, industry and year of privatization. In these tests, we identify the intended effect as a difference-in-difference *within* a given industry in a given country: the before-after difference in privatization in a given industry in a country where employment protection laws changed vis-à-vis this difference in the same industry in a country where employment protection laws did not change. In both set of tests, we find that stringent employment protection laws reduce privatization in a country. This effect is statistically and economically significant: an increase in the EPL index by one standard deviation, *ceteris paribus*, reduces the number of privatization deals in any given country in any given year by 0.62 and the total value of privatization deals by about $820 million. Since the average value of a privatization deal is $1.6 billion in our sample, the $820 million decrease is economically meaningful.

A key concern in the above tests stems from the endogeneity of the employment protection law changes: other factors that accompany these law changes may be accounting for our results. For example, slowing economic growth in a country may increase the political support for employment protection laws (Saint-Paul, 2002) and spur national governments to privatize more. Second, changes in a country's government, specifically its left-of-center political stance, may lead to passage of employment protection laws while also slowing privatization. Third, in countries in which the government share of production is high, political-economic factors may lead governments to enact stringent employment protection laws, on the one hand, and avoid privatization, on the other. Fourth, since trade liberalization in a country may result in job losses, governments may enact stringent employment protection laws following trade

liberalization.[1] Furthermore, trade reforms may be correlated with other structural changes including privatization.

We examine robustness to such concerns in two separate set of tests. First, we augment our fixed effects specification using the country, year sample with country-specific trends. In addition, we augment our fixed effects specification using the country, industry, year sample with country-specific as well as industry-specific trends. Including these trends enables us to identify the effect of employment protection law changes using deviations at the country, year level (at the country, industry, year level) from the average time trends for each country (as well as for each industry). Since the above confounding effects would manifest in these country-specific (and industry-specific) time trends, these tests enable us to isolate better the pure effect of employment protection law changes on privatization. Including these trends increases the economic magnitude of employment protection laws on privatization.

Second, we examine directly the endogeneity introduced by the above mentioned factors by including: (i) yearly GDP growth rate; (ii) a time-varying proxy for the political leanings of a country's government; (iii) the share of the government in total production in the country in each year; and (iv) the sum of imports to and exports from the country as well as the country's exchange rate versus the US dollar. We find that privatization is lower under left-wing governments, which is consistent with such governments deriving their political support from worker groups opposed to privatization. We also find that the magnitude of the country's imports and exports is positively correlated with privatization, which suggests that trade reforms are correlated with the additional structural reform of privatization. Crucially, however, we find that the main effect of the employment protection laws stays negative and significant even after accounting for these sources of endogeneity. We also control for the level of stock market development in the country, its creditor rights, GDP per capita, and the growth rate of its population and find our results to be unchanged.

Next, to test for the effect of employment protection laws for each country that privatized significantly, we study the before-after effect of a change in employment protection laws in a country where employment protection laws change (the "treatment group") vis-à-vis the before-after effect in a country where no employment protection law change occurs around the period of change (the "control group"). Specifically, we examine the effects of changes in employment protection laws in Germany, Italy, Spain and Sweden using United Kingdom as the control group country. We then repeat these tests using France as the control group country since employment protection laws do not change in France and United Kingdom around the period of change in these countries. Our results remain unchanged.

[1] The OECD Employment Outlook in 2005 estimated that international trade accounted for up to 4 % of all permanent layoffs in Canada, the US and the EU in 2000.

Having found evidence supporting the hypothesis that stringent employment protection laws deter privatization, we investigate if stringent employment protection laws discourage privatization *disproportionately* more in the less productive industries and in industries that require low level of job skill since the fear of job cuts should be disproportionately greater in these industries. For this purpose, we use the industry-level measures in a given year for an industry in the United States. We use the industry-level measure for the U.S. to avoid endogeneity associated with the productivity/job skill measured for a particular industry in a particular country. Since organized labor in a country's less productive/lower skilled industries may exert political pressure on the government to enact stringent employment protection laws, as well as influence the government to stop privatization, the productivity measured for a particular industry in a particular country will be endogenous. In contrast, the productivity measured for an industry in the U.S. is unlikely to be correlated with the employment protection laws in the country. We interact these measures with the EPL index and find that the effect of the EPL index is more pronounced in the less productive industries as well as in industries that require a lower level of job skill.

Next, we examine if stringent employment protection laws discourage privatization *disproportionately* more in the more unionized industries since workers would exert considerable political pressure in these industries. Using the percentage of the workforce that is unionized in an industry in the United States as an instrument for the level of unionization in that industry, we find that stringent employment protection laws indeed dampen privatization *disproportionately* more in the more unionized industries than the less unionized ones. In other tests, we confirm that the effect of employment protection law changes does not manifest before the change itself, which reassures us that the direction of causality runs from employment protection laws to privatization rather than vice versa.

In sum, across the plethora of tests, we find that employment protection laws discourage privatization significantly. Since labor restructuring is the most sensitive issues with respect to privatization, our study highlights that national governments must ease the rigidities in their labor markets before embarking on a privatization exercise. Such labor market reforms not only increase the likelihood of privatization but also enable the government to generate greater proceeds from the privatization itself.

This study is organized as follows. We review the related literature in section 2 while the hypotheses we develop and test and presented in section 3. Section 4 presents the data and proxies examined. Section 5 presents empirical results and robustness checks, while section 6 concludes.

## 2. Related Literature

Our study contributes to two streams of the financial economics literature. First, our study relates to the literature examining the impact of privatization on the labor force. Some studies show that that

privatization increases the likelihood of labor force restructuring. Chong, Guillen, and Lopez-de-Silanes (2009) show that 78% of the SOEs in their sample reduce their labor-force before privatization. In their multi-national study, D'Souza and Megginson (1999) document significant declines in employment post-privatization. The following single-country/ single-company studies also document significant declines in employment post-privatization: Ramamurti (1997, Ferrocarilla Argentinos), LaPorta and Lopez-de-Silanes (1999, Mexico), Laurin and Bozec (2000, Canadian National railroad), Omran (2001, Egypt), Arin and Okten (2002, Turkey), and Boardman, Laurin, and Vining (2003, Canada). On the other hand, the multi-national studies of Galal, Jones, Tandon, and Vogelsang (1992), Megginson, Nash, and van Randenborgh (1994), Boubakri and Cosset (1998), and Brown, Earle, and Teledgy (2006) show that employment increases significantly after privatization, at least relative to a benchmark of comparable firms, as does Gupta's (2005) study of partial privatization in India. Many other studies--summarized in Megginson and Netter (2001), Djankov and Murrell (2002), Megginson (2005), and Estrin, Hanousek , Kocenda , and Svejnar (2009)--document insignificant changes in divested firm employment levels, despite very large increases in sales and efficiency. Chong, et al (2009) show that the new private owners of divested companies re-hire laid off workers in almost 44% of all the cases examined. [2]

Second, our study relates to the literature examining the detrimental effects of employment protection laws. Lazear (1990) and Ljungqvist and Sargent (1998) argued that employment protection laws hinder job destruction and thereby lead to less job creation and higher unemployment. Botero, et al. (2004) find empirically that heavier regulation of labor leads to adverse consequences for labor market participation and unemployment. Atanassov and Kim (2009) examine the interaction between labor laws and investor protection laws and find that rigid employment laws lead to higher likelihood of value-reducing major asset sales, particularly when investor protection is weak. They find that assets are sold to forestall layoffs, even if these asset sales hurt performance. Besley and Burgess (2004) conclude from their study of manufacturing performance in Indian states that pro-worker labor laws are associated with lower levels of investment, productivity, and output. Bassanini, Nunziata and Venn (2009) also show that mandatory dismissal regulations in OECD countries have a depressing effect on productivity growth in industries where layoff restrictions are more likely to be binding. Our study contributes to this literature by documenting the discouraging effect of employment protection laws on privatization.

## 3. Empirical Hypotheses

Compared to profit-maximizing privately-owned firms, public enterprises employ excess labor which renders them relatively inefficient (Schleifer and Vishny (1994)). Upon privatization, the newly

[2] Studies documenting insignificant post-privatization employment changes include Macquieira and Zurita (1996, Chile), Sun and Tong (2003, China), D'Souza, Megginson, and Nash (2005, developed countries), and Boubakri, Cosset, and Guedhami (2005, developing countries).

privatized public enterprise needs to shed its excess labor in order to enhance efficiency and thereby create shareholder value. By imposing restrictions on the retrenchment of employees, stringent employment protection laws impose hurdles on the management of a newly privatized public enterprise in moving towards the efficient level of employment. Anticipating the difficulties in shedding excess labor and thereby becoming more efficient, private entities would be less willing to bid for the public enterprises that the government wants to privatize. Furthermore, conditional on the privatization being carried out, private parties would price in the difficulties in achieving the efficient level of employment and thereby provide a lower value for the publicly run enterprise when employment protection laws are more stringent. Therefore, we predict that:

*HYPOTHESIS 1:* Stringent employment protection laws in a country lead to (a) lower number of privatizations; and (b) lower value of privatization deals in the country.

The fear of job losses upon privatization leads organized labor in the state-owned enterprises to vehemently oppose privatization. These organized workers exert considerable political influence since politicians often derive their power by showering their patronage on these groups of workers. The fear of retrenchment would be greater in the less productive industries and in industries that require low levels of skill on the job. Yet, the efficiency gains from shedding excess labor force would be the greatest in these industries. However, stringent employment protection laws should inhibit cutbacks in the labor force and disproportionately obstruct privatization in the less productive industries, in industries that require low levels of skill on the job, and in more unionized industries. Therefore, we also predict that:

*HYPOTHESIS 2:* Compared to the more productive industries, stringent employment protection laws in a country lead to (a) disproportionately lower number of privatizations; and (b) disproportionately lower value of privatization deals in the less productive industries.

*HYPOTHESIS 3:* Compared to industries that require a high level of job skill, stringent employment protection laws in a country lead to (a) disproportionately lower number of privatizations; and (b) disproportionately lower value of privatization deals in industries requiring low level of job skill.

*HYPOTHESIS 4:* Compared to the less unionized industries, stringent employment protection laws in a country lead to (a) disproportionately lower number of privatizations; and (b) disproportionately lower value of privatization deals in the more unionized industries.

## 4. Data and Proxies

In this section, we describe the data, our proxies for privatization, and the changes in employment protection laws.

### *4.1. Proxies for Privatization*

Our privatization data comes from Privatization Barometer (www.privatizationbarometer.net), which is the principal online database of privatization transactions in Europe. Privatization Barometer is a fully searchable database that contains a comprehensive list of privatization transactions from 1977 onwards for all European countries. Privatization Barometer is the official provider of privatization data to the OECD (Organisation for Economic Co-operation and Development) and the World Bank.

We conduct our empirical analysis using two different privatization samples: one aggregated at the country, year level and the other aggregated at the country, industry, year level. We employ two different proxies for privatization: (i) the number of privatization deals; and (ii) the total value of privatization deals (in US$ billions). Table 1 shows the summary statistics for privatization in the 14 countries that form our sample. For each of these countries, we show the number of deals and the total value of deals in dollar billions. We notice that during the time period 1977-2003, Italy undertook the highest number of privatization transactions while the dollar value of these privatization transactions was highest for France.

****** Insert Table 1 about here ******

### *4.2. Employment protection laws*

In order to analyze the impact of employment protection laws on privatization, we exploit the time-series variation generated by changes in these laws within countries. We use the Employment Protection Law (EPL) index sourced from Allard (2005), who analyzes in detail the evolution of employment protection legislation across the OECD countries for each year from 1950 to 2003 to generate the EPL index. The EPL index offers an important advantage in the context of our study. [3] First, the long time-series, which captures comprehensively all country-level changes in employment protection laws, enables us to conduct tests that alleviate econometric concerns that would otherwise be a problem in a cross-country setting.

Second, the EPL index covers comprehensively all aspects of employment protection legislation for the OECD countries. This index has been constructed by surveying existing law and regulations in OECD countries and by assigning numerical scores for each and every aspect of employment protection

[3] The Botero, et al. (2004) index presents an alternative to the EPL index. Although Botero, et al. (2004)'s index is constructed for 85 countries, the index is available only for the year 1997. Therefore, it is not suitable to investigate the causal impact of labor laws on privatization, which necessitates controlling for observable and unobservable time-varying heterogeneity. The Deakin, et al. (2007)'s index presents another alternative to the EPL index. While Deakin, et al. (2007) index provides substantial time-series variation, it is available only for five countries: the United States, the United Kingdom, France, Germany and India. Since there were no privatization deals in the United States during the time period before 2008 and privatization data for India is not available in the privatization database, using the Deakin, et al. index would restrict our sample to only three countries. Therefore, we choose to use the EPL index.

legislation. The final scores have been obtained after necessary reviews and corrections by each of the national governments. The EPL index covers eighteen aspects of employment protection legislation grouped into three broad categories: (i) laws protecting those workers who have signed regular contracts with their employers ("Regular Contracts"); (ii) laws affecting workers with fixed-term/temporary contracts or contracts with temporary work agencies ("Temporary Contracts"); and (iii) regulations applying to collective dismissals ("Collective Dismissals").

The "Regular Contracts" index focuses on the procedural requirements that need to be followed once a decision is taken to fire an employee who has been provided a regular employment contract, the notice period that needs to be given to such an employee, the severance pay requirements, and the prevailing standards of and penalties for "unfair" dismissals. Employment protection laws protect workers covered under "Regular Contracts" from redundancies resulting from economic factors. Such economic factors include bankruptcy, complete or partial liquidation of the enterprise, staff cuts due to changes in the production technology or the structure of the enterprise as well as due to financial problems of the employer. In such a case the redundant worker enjoys protection in the form of a notice period combined with severance pay. Other reasons for employment termination with notice include long-term absence from work due to health reasons, unsatisfactory work performance due to health problems or inadequate qualifications, and refusal to move to another locality in connection with the relocation of the enterprise or of one of its parts. In some countries, age and eligibility for old-age pension are also valid reasons for employment termination with notice by employer while in other countries such a termination is unlawful.

The "Temporary Contracts" index evaluates the conditions under which these types of contracts can be offered, the maximum number of successive renewals and the maximum cumulated duration of a temporary employment contract. The "Collective Dismissals" index defines a collective dismissal and specifies the notification requirements provided by law and the associated delays and costs for the employers.

Figure 1 shows the evolution of the EPL index for the 14 OECD countries for which both the privatization data and the EPL index are available: Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Portugal, Spain, Sweden, and the United Kingdom. As can be seen by examining Figure 1, there is considerable time-series variation among the 14 countries that form our sample.

****** Insert Figure 1 about here ******

This time-series variation within a country is generated by specific law changes relating to employment protection. For example, in France, the employment protection laws relating to the notification of employee dismissals were weakened in 1986. Before this law change, an employer was required to provide the employee with written reasons for his/her dismissal. Furthermore, the employer

had to obtain the permission of a state/local body prior to any individual dismissal. In 1986, this law was changed so that the employer only had to notify the state/local body prior to an individual dismissal. Consistent with this law change, in Figure 1, we see the EPL index for France decreasing in 1986. Table 2 provides a comprehensive list of all the major changes in EPL in our sample of countries. This list of changes is derived from the changes described in Bertola, et al. (1999) and the OECD Employment Outlook (1999).

****** Insert Table 2 about here ******

Table 3 shows that the EPL index for a sample of countries ranges from a minimum of 1.3 to a maximum of 4.1. The average value of the EPL index is 2.6 with the median being a very similar 2.5. The time-series variation within countries witnessed in Figure 1 as well as variation among countries results in a standard deviation of 0.8. Thus, the EPL index in our sample exhibits considerable variation, which we can exploit to identify the effect of employment protection on privatization.

****** Insert Table 3 about here ******

Panel A of Table 3 displays the summary statistics for the country, year sample while Panel B displays the same for the country, industry, year sample. Each panel lists the mean, median, standard deviation, minimum, and maximum for both the proxies for privatization as well as the explanatory variables. We note that there is considerable variation in the number of privatization deals as well as the dollar value of the privatization deals both at the country year level as well as the country industry year level samples.

## 5. Empirical Results

We investigate whether the passage of employment protection laws leads to lower privatization. Inferring a causal relationship between country-level employment protection laws and privatization presents the challenge that country-level employment protection laws are expected to be largely correlated with other country-level unobserved factors. For example, Saint-Paul (2002) asserts that "one expects employment protection to prevail in countries with more rigid labor markets. In other words, there exists a "complementarity" between firing costs and other labor market rigidities." To overcome the econometric challenges posed by such complementarity and thereby infer a causal relationship between EPL and privatization, we utilize the fact that the EPL index exhibits substantial time-series variation as described above.

Figure 2 depicts a visual difference-in-difference examining the effect of the passage of the employment protection law in Germany in 1990: this change lowered the EPL index for Germany from 2.9 to 2.6. The left panel in the graph plots on the y-axis the average number of deals in Germany over the years 1986-1996 versus the same variable for the United Kingdom, since there was no change in

employment protection laws in United Kingdom during this period. The right panel of the graph plots on the y-axis the average value of deals over the years 1986-1996 for Germany and the United Kingdom. To enable comparison, we normalize the measure to 1 in 1990 for Germany and United Kingdom. This figure clearly illustrates that after the employment protection law changes in 1990, lowering employment protection, the average number of privatization deals and the average value of these deals increases in Germany compared to the change over the same period in the United Kingdom.

****** Insert Figure 2 about here ******

As the employment protection law changes occur in the sample countries in different years, we can implement the econometric variant of the above visual difference-in-difference. We start with a basic specification employing country and year fixed effects and progressively employ tighter specifications to control for various sources of endogeneity.

### *5.1. Basic test: Fixed-effects panel regressions using the country, year sample.*

In our basic test, we implement a fixed-effects panel regression using the privatization proxies at the country, year level. These panel regressions implement a difference-in-difference in a multiple treatment groups, multiple time periods setting as employed by Imbens and Wooldridge (2009). Thus, we examine the before-after effect of a change in employment protection laws in the affected country (the "treatment group") vis-à-vis the before-after effect in a country where such a change is not effected (the "control group") in the same year. This difference-in-difference test is implemented through the following panel regression:

$$y_{ct} = \alpha_c + \alpha_t + \beta_1 * EPL_{ct} + \beta X_{ct} + \varepsilon_{ct} \qquad (1)$$

where $y_{ct}$ is the measure of privatization for country c in year t, and $\alpha_c$ and $\alpha_t$ denote country and year fixed effects, respectively. $EPL_{ct}$ denotes the stringency of employment protection laws based on the EPL index value for country c in year t. The country fixed effects control for time-invariant unobserved factors at the country-level while the year fixed effects control for common global trends in privatization. $X_{ct}$ denotes the set of control variables. Table 4 shows the complete set of variables used in the regressions, as well as their description and the source for the variable.

****** Insert Table 4 about here ******

As explained by Imbens and Wooldridge (2009), $\beta_1$ in regression equation (1) estimates the "difference-in-difference" in a generalized multiple treatment groups, multiple time periods setting. Intuitively, given the country and time fixed effects, $\beta_1$ estimates the within-country differences in privatization before and after the EPL change vis-à-vis similar before-after differences in countries that do not experience such a change during the same period. Therefore, these tests are less subject to the criticism that country-level unobserved factors influencing privatization are correlated with the level of

employment protection laws in a country. Since the primary variable of interest $EPL_{ct}$ varies at the country-year level, in all our regressions, we estimate robust standard errors clustered by country-year.

Table 5 shows the results of the test of equation (1) using the total value of privatization deals in a country, year (US$ billions) and using the number of privatization deals in a country, year. Columns 1 and 2 show the results using the value of the deals as the dependent variable while columns 3 and 4 show the results using the number of deals as the dependent variable. For each of the two dependent variables, we find the coefficient on the EPL index to be negative and significant at the 1% or 5% level. This result indicates that strong employment protection laws are negatively correlated with privatization. In these regressions, we also control for other country-level variables that may affect privatization.

****** Insert Table 5 about here ******

*5.1.1. GDP growth*

In examining the effect of EPL on privatization, controlling for the economic growth in a given country is crucial. This is because privatization of state-owned enterprises may be a government response to a slowdown in economic growth in a country. First, when governments become too large or overextended and build up unnecessary layers of bureaucracy, privatization may be a response to reduce the size of the existing government. This downsizing aspect of privatization is important since bad government policies and government corruption can have a large, negative impact on economic growth. Thus, when economic growth slows down, countries may resort to privatization to reduce the negative impact of government ownership on economic growth. Second, privatization through public share offerings contribute to stock market development (Boutchkova and Megginson, 2001). Since stock market development has been shown in the literature to have a positive effect of economic growth (Demirgüç-Kunt and Maksimovic, 1998, among others), privatization can contribute to growth through its positive impact on stock market development. Third, studies on post-privatization performance suggest that newly privatized firms significantly increase their investment expenditures, which in turn boosts national investment spending and economic growth. Finally, additional revenues to the government from privatization proceeds (and reduced subsidies) lead to an increase in investments on infrastructure, a reduction in budget deficits, and a generally positive effect on aggregate productivity and economic growth.

A more critical concern is that a country's growth rate could not only influence privatization outcomes but also affect the passage of EPL. For instance, Saint-Paul (2002) asserts that a higher economic growth rate reduces the political support for EPL because it increases the cost in the form of lower wages. However, since incumbent workers are most fearful of losing jobs during periods of slow economic growth, the political support for EPL would be high in such periods. As empirical support for

his thesis, Saint-Paul (2002) points out that in many European countries employment protection increased in the early 1970s and proved very difficult to reduce in the 1980s since this was a period of slow growth. Thus, from our perspective, slowing economic growth may lead both to greater privatization as well as increases in the stringency of EPL. Therefore, we control for the GDP growth rate of the economy, but find its effect to be statistically insignificant.

### *5.1.2. Creditor rights*

Since publicly owned enterprises may have an implicit government guarantee that private enterprises do not possess, weaker rights provided to creditors in bankruptcy may enable the public sector to raise cheaper debt financing compared to privately owned enterprises and thereby crowd out financing to the private sector. Privatization may be an optimal response to such crowding out of the private sector in countries with weaker creditor rights. Therefore, privatization may be more likely in countries with weaker creditor rights. In contrast, Acharya and Subramanian (2009) provide empirical evidence that weaker creditor protection countries foster economic growth by encouraging more firm-level innovation. Since privatization may be less likely when economic growth in a country is quite robust, weaker creditor rights in a country could also lead to less privatization. To control for these effects of creditor rights on privatization, we include the time-varying index of creditor rights developed by Djankov, et al. (2007); this index is available for the time period 1970-2002. We find the coefficient on creditor rights to be insignificant, possibly suggesting that the conflicting effects of creditor rights may be offsetting each other.

### *5.1.3. GDP per capita*

Poor countries tend to grow more rapidly than rich industrialized countries (Barro, 1991). Thus, the initial level of economic development in a country can impact the decision to privatize public enterprises by influencing economic growth in the country. We therefore include the logarithm of GDP per capita to control for the effect of a country's initial conditions on its privatization propensity. We find this variable to be insignificant as well.

### *5.1.4. Population*

Bornschier, et al. (1978) argue that a higher growth rate of a country's population may dilute the benefits of economic policies. Moreover, the findings of Savvides (1995) and Adams (2006) indicate that a high national population growth rate reduces economic growth. Since the decision to privatize public enterprises may be influenced significantly by the growth path of economy, we include the logarithm of a country's population to control for the growth rate in the population. We find that a higher rate of population growth in a country is negatively correlated with the decision to privatize public enterprises.

### *5.1.5. Other country-level variables*

The country fixed effects employed in the regressions in Table 5 control for the possibility that the employment protection laws in a country may be correlated with its laws and institutions. For example, Botero, et al. (2004) find that the country's legal origin is strongly correlated with the stringency of the nation's labor laws. Specifically, civil law countries are more likely to have stringent labor laws than common law ones. Since the country's legal origin is time invariant, the country fixed effects control for this. Similarly the country fixed effects subsume the effect of other time-invariant legal variables highlighted by the law and finance literature (La Porta, et al., 1997, 1998): rule of law, anti-director rights index, efficiency of the judicial system, days to enforce a contract, estimated cost of insolvency proceedings, and others (see La Porta, et al., 1998).

### *5.1.6. Economic magnitudes*

Using the coefficient of the EPL index in columns 2 and 4 of Table 5, we estimate the economic magnitude of the effect of EPL on privatization. A one standard deviation increase in the EPL index, which equals 0.78 from Table 2, reduces the number of privatization deals in any given country in any given year by 0.62 and the total value of privatization deals by about $820 million. Using the average value of privatization deals, which equals $1.6 billion from Table 3, the reduction in the number of privatization deals in any given country in any given year by 0.62 translates into a decrease of $994 million, which is very close to the $820 million estimated using the coefficient in column 4 of Table 5.

## ***5.2. Fixed-effects panel regressions using the country, industry, year sample***

After undertaking these difference-in-difference tests by aggregating the privatization deals at the country, year level, we repeat these tests using a more disaggregated country, industry, year sample:

$$y_{ict} = \alpha_c + \alpha_i + \alpha_t + \beta_1 * EPL_{ct} + \beta X_{ct} + \varepsilon_{ct} \qquad (2)$$

where $y_{ict}$ above is the measure of privatization for 2-digit SIC industry i in country c in year t. If there is no privatization undertaken for a given 2-digit SIC industry, country, year, $y_{ict}$ equals zero. $\alpha_i$ , $\alpha_c$ and $\alpha_t$ denote industry, country and year fixed effects, respectively. The industry fixed effects control for time-invariant unobserved factors at the industry-level.

Table 6 reports the results of these tests. In columns 1 and 2, we employ the number of deals as the dependent variable with column 1 including country and year fixed effects while column 2 includes country, year, and industry fixed effects. In columns 3 and 4, we repeat the same specifications using the value of deals as the dependent variable. Across columns 1 to 4, we find that the coefficient of the EPL index is negative and statistically significant. Thus, the effect of employment protection laws on privatization is seen to be robust using this disaggregated, industry-level sample as well.

****** Insert Table 6 about here ******

If certain industries are more subject to political exploitation in the form of patronage through excess employment, then privatization is more likely to be employed in such industries. In the above tests employing the industry-level sample, we control for country, industry and year specific effects through our fixed effects. Thus, the above fixed effects panel regressions effectively exploit variation across time in the choice of privatization *within a given industry in a given country*. These tests thus compare privatization outcomes within similar assets in the same country before and after a change in EPL against the control group of country, industry pairs that did not undergo such a legal change.

### *5.3. Additional sources of endogeneity in Employment Protection Law Changes*

We have already examined the possibility that EPL changes may be endogenous to the country-level economic growth and find that accounting for the same does not alter the robustness of the relationship between EPL and privatization. We now undertake additional tests to further examine whether or not our results are an outcome of such endogeneity.

#### *5.3.1. Panel regressions with country-specific time trends*

To examine whether other country-level changes accompanying the EPL changes account for our results, we incorporate country-specific time trends into tests employing the main (country, year) sample:

$$y_{ct} = \alpha_c + \alpha_t + \alpha_c{*}t + \beta_1{*}EPL_{ct} + \beta X_{ct} + \varepsilon_{ct} \qquad (3)$$

where $\alpha_c{*}t$ denotes a time trend specific to each country c in our sample and the other variables are as defined in equation (1). By accounting for these country-specific time trends, we identify the intended effect using deviations (at the country, year level) from the average time trend for each country. Since other country-level changes accompanying the employment protection law passages could lead to country-specific time trends, these tests enable us to isolate better the pure effect of employment protection law changes on privatization.

As can be seen in Figure 1 and in Table 2, there has been no secular trend of either relaxing or tightening EPLs across our sample of countries. Changes in EPL among our sample of countries have indeed been bidirectional, becoming more stringent in some countries and more lenient in others. In fact, even within the same country, we observe in Figure 1 that EPL changes have typically been bidirectional: becoming more stringent at some points and then more lenient at others. Thus, given the lack of a secular trend in the EPL changes within a country, even a secular trend of increasing privatization in each country in our sample cannot lead to a spurious correlation. Nevertheless, the country-specific trends allow the remote possibility that our results are due to a spurious correlation stemming from unidirectional changes in employment protection laws in a few countries. Columns 1 and 2 of Table 7 show the results of the tests of equation (3). After accounting for country-specific time trends, the coefficient of the EPL index

remains negative and significant at the 5% level. Comparing the coefficients in columns 1 and 2 of Table 7 to columns 2 and 4 of Table 5, respectively, suggests that controlling for country-specific trends strengthens the effect of employment protection laws on privatization.

****** Insert Table 7 about here ******

*5.3.2. Panel regressions with country- and industry-specific time trends*

We now use the more disaggregated industry-level (i.e. country, industry, year) sample to incorporate both industry-specific and country-specific time trends into our tests:

$$y_{ict} = \alpha_i + \alpha_c + \alpha_t + \alpha_c{*}t + \alpha_i{*}t + \beta_1{*}EPL_{ct} + \beta X_{ct} + \varepsilon_{ict} \qquad (4)$$

where $\alpha_c{*}t$ and $\alpha_i{*}t$ denote a time trend specific to each country c and each 2-digit SIC industry i in our sample, and the other variables are as defined in equation (1). By accounting for these country-specific and industry-specific time trends, we identify the intended effect using deviations (at the country, industry, year level) from the average time trend for each country and that for each industry.

These tests incorporating country- and industry-specific time trends also enable us to control for the possibility that *unobserved* country-level changes accompanying the passage of employment protection laws have a differential effect on privatization in different industries. Columns 3 and 4 of Table 7 show the results of the tests of equation (4). After accounting for country- and industry-specific time trends, the coefficient of the EPL index remains negative and significant at the 1% level. Comparing the coefficients in columns 3 and 4 of Table 7 to columns 3 and 6 of Table 6, respectively, suggests that controlling for country-specific trends considerably strengthens the effect of employment protection laws on privatization.

As mentioned in section 5.2 above, if certain industries are more subject to political exploitation in the form of patronage through excess employment, then privatization is more likely to be employed in such industries. In the above tests, we control for any country- and industry-specific trends due to such unobserved factors.

*5.3.3. Correlation of employment protection law passages to changes in government*

An important concern stems from the fact that changes in a country's employment protection laws are likely to be correlated with changes in elected governments. In particular, to cater to their political constituencies, more left-leaning governments may be inclined to strengthen labor laws. Botero, et al. (2004) find evidence that labor market regulation is often driven by political considerations: countries with a longer history of leftist governments have more stringent labor regulation. Deakin, et al. (2007) also document that the primary motivation for labor market (de)regulation is political. They find that a rapid decline in the intensity of labor market regulation in the United Kingdom coincided with the election of a Conservative government committed to a policy of labor market deregulation. Similarly, a

limited revival of regulation of the labor markets in the United Kingdom coincided with the return to office in 1997 of a Labor government which ended United Kingdom's opting out of the EU Social Charter. Furthermore, they find that in France, the election of a socialist government in 1981 led to a series of labor law reforms – the "Auroux laws". These laws, which were enacted in 1982, affected a wide range of aspects in both individual and collective labor law. Since that time, French labor law has tracked the changing political fortunes of the main parties.

Since leftist governments are less likely to privatize public enterprises, it is possible that the effect of employment protection laws on privatization documented above is, in fact, caused by other factors coinciding with changes in government rather than changes in employment protection laws. We examine this concern by including time-varying proxies for the political leanings of a country's government. These variables are constructed using the variable *Government* from Armingeon, et al. (2008), which captures the balance of power between left and right-leaning parties in a given country's parliament.[4] This variable takes on values from one to five, with one denoting a hegemony of right-wing (and centre) parties, and five denoting a hegemony of social-democratic and other left parties. As expected, it is strongly positively correlated with the EPL index (the correlation is 0.62), which implies that stricter employment protection laws are indeed enacted in a country when the government is leftist in its political leanings.

Panel A of Table 8 shows the result of tests adding this variable to the basic tests described in Tables 4 and 5. We find that privatization is lower under leftist governments, which is consistent with such governments deriving their political support from worker groups and such groups being opposed to privatization. Crucially, however, we observe that the coefficient on the employment protection law index remains negative and significant (at the 5% level) in all the specifications in Panel A of Table 8.

****** Insert Table 8 about here ******

*5.3.4. Correlation of employment protection law passages to government share in production*

The economic growth literature suggests that a measure of government involvement in economic production serves as a proxy for the level of political corruption in the country (Cook and Uchida, 2003; Filipovic, 2005). Greater political corruption may lead governments to enact stringent employment protection laws in order to cater to their political constituencies. Similarly, political pressure exerted by influential groups may force the government to avoid privatization. We control for this source of endogeneity by including the share of government production in a country's GDP each year. Panel B of Table 8 shows the results of including this variable along with the other control variables and the EPL index. We find that while greater government involvement in production is consistently negatively

[4] Armingeon, et al. (2008) construct a Comparative Political Data Set, which is a collection of annual political and institutional data for 23 democratic countries for the period of 1960 to 2006.

correlated with privatization, this correlation is not significant. Crucially, however, we observe that the coefficient on the employment protection law index remains negative and significant (at the 5% level) in all specifications in Panel B of Table 8.

#### *5.3.5. Correlation of employment protection law passages to trade reforms*

Since trade liberalization in a country may result in job losses, governments may enact stringent EPL following trade liberalization. For example, the OECD Employment Outlook in 2005 estimates that international trade accounts for up to 4 % of all permanent layoffs in Canada, the US and the EU in 2000. Furthermore, trade reforms may coincide with other structural reforms such as privatization. Thus, omitting the effect of trade reforms may cause the effect of EPL on privatization to appear endogenous. To control for this source of endogeneity, we include the aggregate level of imports into and exports from a country. Since trade reforms should result in an increase in exports and imports, this variable proxies trade reforms. Furthermore, we include the exchange rate for the country's domestic currency with respect to US dollars as another proxy for the trade related openness of the country's economy. Panel C of Table 8 shows the results of including both these variables. We notice that increasing openness in a country's trade, as captured by its imports and exports, is positively correlated with the level of privatization in the country. Furthermore, the nation's exchange rate, which also reflects the country's trade openness, is positively correlated with the level of privatization in the country. Importantly, we observe that the coefficient on the employment protection law index remains negative and significant (at the 5% level) in all the specifications in Panel C of Table 8.

#### *5.3.6. Controlling for the effect of stock market development*

Since privatization might be easier in countries where stock markets are well established, we also control for the effect of stock market development on privatization using the Stock Market Turnover Ratio as well as the Ratio of Value Traded in the Stock Market to the GDP in the country. Panel D of Table 8 shows the results of controlling for stock market development. We find that stock market development, as measured by the Ratio of Value Traded in the Stock Market to GDP, is weakly positively correlated with the privatization. Importantly, we find the effect of EPL index to still be negative and significant except in column 2.

### *5.4. Traditional difference-in-difference tests*

Having documented the negative effect of employment protection laws on privatization using the full sample, we now examine this relationship using traditional two-country difference-in-difference tests. For each country that undertakes a significant degree of privatization, we study the before-after effect of a change in employment protection laws in the country where employment protection laws changes (the

"treatment group") vis-à-vis the before-after effect in a country where no employment protection law change occurs around the period of change (the "control group"). As seen in Table 1, France, Germany, Italy, Spain, Sweden and the United Kingdom are the countries with the largest privatization programs – with respect to both the number and dollar value of privatization transactions during our sample period.

As seen in Figure 1, employment protection laws in Germany, Italy, Spain and Sweden change over the period 1986 to 2002. However, the United Kingdom does not change its employment protection laws during the time period 1986 to 1997 while France does not change its employment protection laws over the time period 1991 to 2002. Therefore, we conduct two separate sets of two country difference-in-difference tests: (i) using United Kingdom as the control group to examine the effect of employment protection changes in Germany, Italy, Spain and Sweden over the time period 1986 to 1987; and (ii) using France as the control group to examine the effect of employment protection changes in Germany, Italy, Spain and Sweden over the time period 1991 to 2002.

To ensure enough variation in these two-country settings, we use only the disaggregated country, industry, year sample and employ the following specification, which is identical to that in equation (2) except that it only includes the treatment country and the control country:

$$y_{ict} = \alpha_c + \alpha_i + \alpha_t + \beta_1 * EPL_{ct} + \beta X_{ct} + \varepsilon_{ct} \qquad (5)$$

Table 9 shows the results of these two-country difference-in-difference tests. Panel A of Table 9 shows the difference-in-difference tests using the United Kingdom as the control group for EPL changes that occur during the time period 1986 to 1997. Panel B of Table 9 shows the difference-in-difference tests using France as the control group for EPL changes that occurred during the time period 1991-2002.

****** Insert Table 9 about here ******

We notice that the effect of EPL on privatization is negative and significant using both the number of privatization deals and the dollar value of the privatization transactions. Out of the 16 specifications used in Table 9, five are not significant in these tests. This lack of significance in some cases is expected since the two country tests do not offer the necessary degree of freedom to power the statistical tests. However, the others are statistically significant at the 5% level.

### *5.5. Causality or reverse-causality?*

It is important to further examine the direction of causality from employment protection laws to privatization. As we discussed in section 3.3.3, political factors are a key determinant of employment protection law changes in the countries in our sample. By examining the dynamic aspects of the effect of the law change, we investigate reverse causality in our tests below. For example, is it that employment protection law changes are effected to provide an extra boost to privatization already occurring due to

some other structural reforms in the economy? In this case, we might see an "effect" of the change even prior to the change itself.

To address the above concern, we run specifications in which we include $EPL_{c,t+2}$ together with $EPL_{ct}$ as independent variables to decompose the total effect of employment protection laws into any potential effects before and after the passage of the laws:

$$y_{ict} = \alpha_i + \alpha_c + \alpha_t + \alpha_c{*}t + \alpha_i{*}t + \beta_1{*}EPL_{ct} + \beta_2{*}EPL_{c(t+2)} + \beta X_{ct} + \varepsilon_{ict} \qquad (6)$$

The coefficients of the regression of $y_{ict}$ on $EPL_{c(t+2)}$ ($\beta_2$) capture the relation between privatization in year t and employment protection laws passed in year t+2, or the effect on privatization two years before the law passage. Since we include $EPL_{ct}$ in the above regression specification, which captures the effect of employment protection laws passed at time t on privatization in time t and beyond, $EPL_{c(t+2)}$ captures the effect of employment protection laws passed at time t on privatization in time t-2 and t-1. Therefore, the coefficient $\beta_2$ captures any reverse causal effects of the passage of employment protection laws on privatization.

Table 10 shows the results from estimating equation (6). In columns 1 and 2, we estimate equation (6) using the dollar value of privatization transactions as the dependent variable. Column 1 shows the specification without any control variables while columns 2 shows the specification after including all the control variables described in earlier tests in section 3.3. We notice that the coefficient of $EPL_{c(t+2)}$ is negative and significant in column 1 but loses its statistical significance in column 2 when we include the entire set of control variables that may be affecting the relationship between EPL and privatization. This suggests that the omitted variable bias in column 1 manifests as a reverse causal relationship. Crucially, however, once we include all the relevant control variables, the coefficient of $EPL_{c(t+2)}$ becomes insignificant, which suggests the absence of any reverse causal effect of EPL on privatization once all the determinants of the relationship are controlled for. Also, the coefficient of $EPL_{ct}$ remains negative and significant.

In columns 3 and 4, we estimate equation (6) using the number of privatization transactions as the dependent variable with and without the control variables, respectively. For the number of privatization transactions, we find that the coefficient of $EPL_{c(t+2)}$ is insignificant in both columns 3 and 4, which further confirms the absence of any reverse causal effect of EPL on privatization. The coefficient of $EPL_{ct}$ again is negative and significant.

****** Insert Table 10 about here ******

### *5.6. Triple-difference tests exploiting Inter-industry differences*

#### *5.6.1. Inter-industry differences based on labor productivity*

We now examine our Hypothesis 2 that the effect of employment protection laws should be disproportionately stronger in industries where labor productivity is lower. For this purpose, we use the industry-level productivity in a given year for an industry in the United States in that particular year. We use the industry-level measure for the U.S. to avoid endogeneity associated with the productivity measured for a particular industry in a particular country. Since less productive industries in a country may exert political pressure on the government to enact stringent employment protection laws as well as influence the government to stop privatization, measured productivity for a particular industry in a particular country would be endogenous. In contrast, the productivity measure for an industry in the U.S. is unlikely to be correlated with the employment protection laws in the foreign country. We interact these proxies for productivity with the EPL index:

$$y_{ict} = \alpha_i+\alpha_c+\alpha_t+\alpha_c*t+\alpha_i*t + \beta_1*EPL_{ct} + (\beta_2* EPL_{ct} + \beta_3)*LaborProductivity_{it} + \beta X_{ct} + \varepsilon_{ict} \qquad (7)$$

We use the output per hour as well as the output per worker as two different measures of productivity. Table 11 shows the results of estimating equation (7) using output per hour as the proxy for productivity of labor. We find that the coefficient of the interaction term is consistently and significantly positive. Since the coefficient of the EPL index variable is significantly negative, this implies that the marginal effect of the EPL index is greater in an industry where employee productivity is lower than in industry where employee productivity is greater. In an industry where labor productivity is zero, columns 1 and 4 show the coefficients of the EPL index to be -0.381 and -0.516 for the value of deals and the number of deals, respectively. For an industry in which the output per hour is one standard deviation greater (which equals 12.1 from panel B of table 2), the coefficient of the EPL index is -0.341 and -0.524, respectively. Therefore, a one standard deviation decrease in the productivity of labor increases the marginal effect of employment protection laws by approximately 11%.

****** Insert Table 11 about here ******

Table 12 shows the results of estimating equation (7) using output per worker as the proxy for productivity of labor. As in Table 11, we find that the coefficient of the interaction term is significantly positive. The economic magnitude of the interaction effect in Table 12 is similar to that in Table 11. We thus conclude that the effect of employment protection laws is more pronounced in the less productive industries than in the more productive industries, which is consistent with Hypothesis 2.

****** Insert Table 12 about here ******

*5.6.2. Inter-industry differences based on degree of skill required on the job*

To examine Hypothesis 3, we use the level of labor compensation for an industry in the US as a proxy for the level of skill required in the job in that industry. Since compensation offered to labor increases with the level of skill required in the job, the level of labor compensation serves as a useful proxy for the level of skill required in the job. For reasons similar to those mentioned in section 5.6.1 above, we use the comparable measure for the industries in the U.S. to avoid any concerns of potential endogeneity. We employ a similar specification to that in equation (7) except that we replace labor productivity with labor compensation. Table 13 shows the results of these tests.

****** Insert Table 13 about here ******

We find that the coefficient of the interaction term is significantly positive. Since the main effect of EPL index is negative, this implies that the marginal effect of the EPL index is greater in magnitude in an industry where employee skill required on the job is lower, which is consistent with Hypothesis 3.

*5.6.3. Inter-industry differences based on union density*

To examine Hypothesis 4, we use the percentage of workforce that is unionized in an industry in the U.S. as a proxy for the level of unionization in that industry in the treatment country. For reasons similar to those mentioned in section 5.6.1 above, we use the variable for the industries in the U.S. to avoid any concerns of potential endogeneity. We employ a similar specification to that in equation (7) except that we replace labor productivity with union density. Table 14 shows the results of these tests.

****** Insert Table 14 about here ******

We find that the coefficient of the interaction term is significantly negative. Since the main effect of EPL index is negative, this implies that the marginal effect of the EPL index is greater in magnitude in an industry where workers are more unionized, which is consistent with Hypothesis 4.

*5.6.4. Discussion*

The above triple-difference tests exploiting inter-industry differences in the propensity to privatize provide the strongest piece of evidence of the causal effect of employment protection laws on privatization. Despite the plethora of tests that we have undertaken to ameliorate concerns relating to endogeneity, a residual concern could be that the pattern of industries in different countries is potentially correlated systematically with country-wide unobserved factors. Since privatization outcomes within a country could be driven by the pattern of industries in a country, the choice of privatization could also be driven by such omitted variables.

For instance, Saint-Paul (2002) argues theoretically that unemployed workers typically oppose the enactment of EPL since it reduces both their likelihood of finding a job and the average wage that this job will pay. Thus, EPL is typically supported by workers at older plants whereas workers at younger plants typically oppose EPL. This is because for workers in the younger plants, the gain from EPL in the

form of extended job duration is more remote, and therefore more heavily discounted when workers are young. Since employment protection shifts the distribution of jobs toward older vintages and since more senior workers favor more employment protection, EPL creates its own constituency. This may potentially lead to multiple steady-state equilibria. An equilibrium with high employment protection may be supported by a mass of senior workers artificially maintained in unproductive matches by past employment protection legislation, whereas if such legislation had not been enacted, there would be more workers in younger vintages and no political support for EPL. Similarly, since privatization often leads to job losses, privatization creates its own constituencies for and against. Therefore, our results above may just be a manifestation of time-varying multiple equilibria within a country, with these multiple equilibria driven by country-level omitted factors.

It is precisely concerns of this variety that the triple-difference tests above help address. A key advantage of these tests is that the industry-level measures are constructed for the United States. Therefore, these measures for the U.S. are unlikely to be correlated in unobserved ways with the privatization outcomes in a country. However, due to the underlying technology in an industry being similar across different countries, the industry-level measures computed for the U.S. are correlated with the propensity for privatization in an industry. These U.S. measures therefore serve as a valid instrument. As a result, these triple-difference tests are unlikely to be driven by the concerns listed above.

## 2. Conclusion

Do employment protection laws hinder privatization? Using privatization deals in fourteen OECD countries from 1977-2003 and within-country variation in employment protection laws, we find that stringent employment protection laws significantly deter privatization. The fear of job losses following government divestment leads organized labor in the state-owned enterprises to vehemently oppose privatization. Consistent with the fear of retrenchment being greater in the less-productive and low-skilled sectors and the fact that such groups of workers exert considerable political pressure, we find that stringent employment protection laws inhibit privatization *disproportionately* more in industries that are less productive, require lower level of job skill, and are more unionized.

Since labor restructuring is one of the most difficult and sensitive issues with respect to privatization, our study highlights that national governments intending to privatize their state-owned enterprises should focus on easing the rigidities in their labor markets before or simultaneously with launching a divestiture program. Such labor market reforms not only increase the likelihood of privatization but also enable the government to generate greater proceeds from the sales.

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**Figure 1: Variation in Employment Protection Legislation (EPL) Index**

Variation in the EPL index with time for 14 countries in the time period 1977 – 2003.

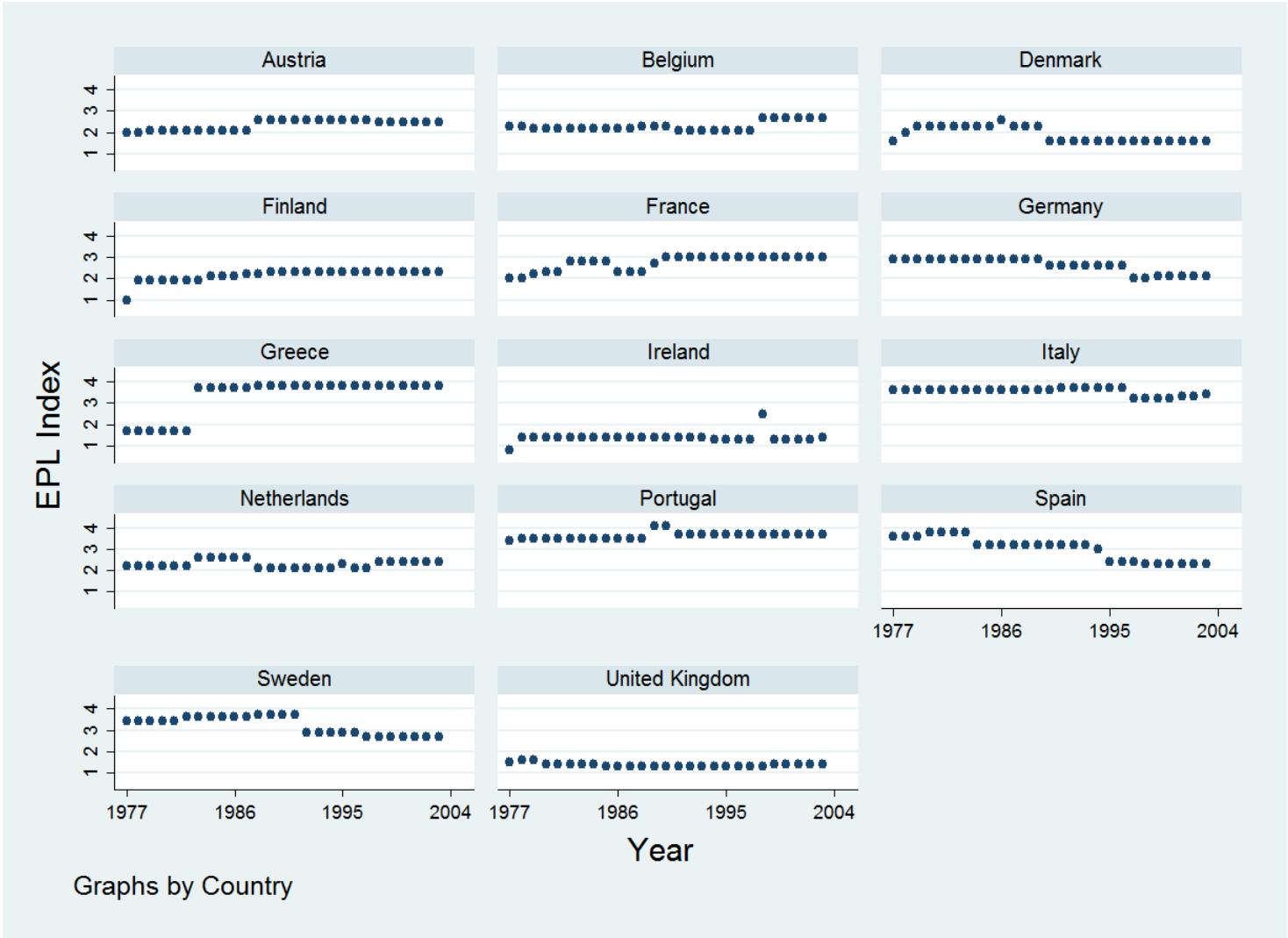

**Figure 2: Difference-in-difference effect of change in Employment Protection Legislation in Germany in 1990**
This figure shows the average number of deals and average value of deals for United Kingdom and Germany from 1987 - 1995. The EPL index decreased in Germany in 1990 as can be seen in figure 1. However, there is no change for UK in this period. The y-variable is normalized to 1 for each country in 1990. The solid line shows the trend for Germany while the dashed line shows the trend for UK.

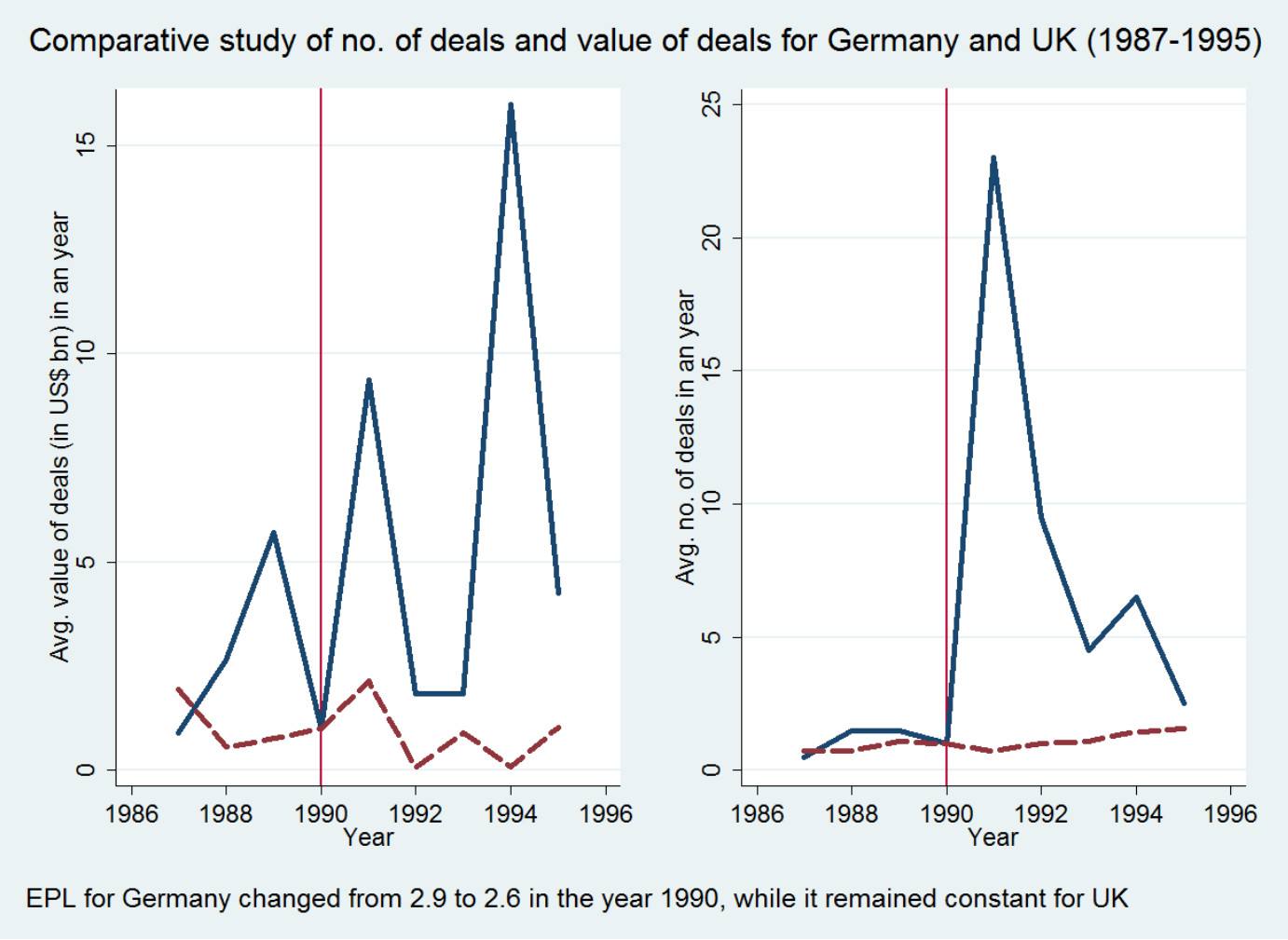

**Table 1: Country-wise summary of privatization**

Following table shows the statistics for privatization deals of 14 countries for which EPL index is available. Time period: 1977 – 2009

| Country | No. of deals | Total value of deals (US$ bn) |
|---|---|---|
| Austria | 70 | 18.8 |
| Belgium | 23 | 9.1 |
| Denmark | 20 | 8.8 |
| Finland | 80 | 30.0 |
| France | 168 | 200.6 |
| Germany | 193 | 125.2 |
| Greece | 68 | 29.2 |
| Ireland | 24 | 8.9 |
| Italy | 206 | 169.2 |
| Netherlands | 49 | 51.1 |
| Portugal | 98 | 36.1 |
| Spain | 107 | 53.7 |
| Sweden | 77 | 46.5 |
| United Kingdom | 202 | 145.5 |

**Table 2: Major changes in employment regulations**

The table shows the reasons for some major changes in employment regulations that caused a change in the value of EPL. A tightening regulation increases the value of EPL while a relaxing regulation causes a decrease in EPL.

| Country | Year | Tightening of EPL | Relaxation of EPL |
|---|---|---|---|
| Austria | 1998 | | New law on working time increased working time flexibility on the basis of collectively agreed provisions at sectoral level. |
| Belgium | 1991 | | Fixed-term contracts possible without specifying an objective reason.<br>Number of permissible renewals as well as overall duration of fixed-term and temporary agency contracts were progressively widened. |
| Denmark | 1990 | | Temporary work agencies (TWA) were deregulated. |
| France | 1982 | Reduction of the statutory working week from 40 to 39 hours | |
| | 1986 | | Administrative authorization for dismissal for economic reasons was abolished.<br>The list limiting the circumstances in which the use of fixed-term contract and temporary staffing is permissible was abolished. |
| | 1989 | Collective redundancies must be accompanied by social plan. | |
| | 1990 | The list limiting the circumstances in which the use of fixed-term contract and temporary staffing is permissible was restored. | |
| Germany | 1997 | | Number of permissible renewals as well as overall duration of fixed-term and temporary agency contracts were progressively widened. |
| | 1999 | Employment threshold for unfair dismissal protection was lowered again to 5 employees per establishment. | |
| Italy | 1991 | A law regulated collective redundancies, establishing standards relating to information and consultation. | |
| | 1997 | | In case of violation of fixed-term contracts legal discipline, a new Act limited the drastic sanction (conversion of the fixed term contract into an open-ended one) only to serious cases.<br>TWA were legalized.<br>Reduction of the statutory working week from 48 (fixed in 1923) to 40 hours. |
| Portugal | 1991 | | Firing restrictions eased through a wider range of admissible lay-off motivations and the abolition of prior authorization of collective dismissals. |
| Spain | 1984 | | The law increased the range of permissible fixed-term contracts. |
| | 1994 | Tightening of reasons under which fixed-term | Temporary work agencies permitted. |

| | | | |
|---|---|---|---|
| | | contracts are allowed. | Prior administrative authorization for dismissals for economic reasons was abolished. Objective grounds for collective redundancies extended and procedural requirements made less time-consuming. |
| | 1998 | | Maximum compensation pay for unfair dismissal was reduced from 45 to 33 days per year of service. |
| Sweden | 1997 | | Fixed-term contracts possible without specifying an objective reason, where no more than five employees are covered by such contracts simultaneously. |
| United Kingdom | 1985 | | The period of service to claim unfair dismissal increased to two years. |

**Table3: Summary Statistics**

This table presents the summary statistics for the variables used in the regression analysis. Panel A reports the statistics for the variables computed at country, year level. Labor productivity, Output per worker, and Labor compensation which are computed at country, sic, year is not reported in panel A. Panel B reports the statistics for the variables computed at country, sic, year level. The values reported are for 14 countries over the period 1978 – 2003.

| Panel A: Summary stats for the variables at Country, Year level | | | | | | |
|---|---|---|---|---|---|---|
| Variable | Obsns. | Min. | Max. | Mean | Median | Std. Dev. |
| No. of deals | 364 | 0 | 46 | 2.99 | 1 | 4.6 |
| Value of deals (US$ bn) | 364 | 0 | 26.7 | 1.6 | 0.07 | 3.4 |
| EPL | 364 | 1.3 | 4.1 | 2.6 | 2.5 | 0.79 |
| Creditor Rights | 364 | 0 | 4 | 2.1 | 2 | 1.03 |
| GDP Per Capita | 364 | 3926.5 | 34374.8 | 16336.8 | 15693.7 | 6390.3 |
| Population in 1000s | 364 | 3314 | 82398.3 | 26251.5 | 10189.4 | 25381.3 |
| Exchange rate ($ US) | 364 | 0.11 | 10.6 | 1.7 | 0.89 | 2.3 |
| Govt. share of GDP (%) | 364 | 9.5 | 26.2 | 15.9 | 14.95 | 3.5 |
| Trade Openness (Imports + Exports) (%) | 364 | 17.6 | 161.5 | 58.8 | 51.9 | 30.3 |
| Centralization Index | 299 | 1 | 5 | 2.97 | 3 | 0.99 |
| Government (Schmidt Index) | 363 | 1 | 5 | 2.7 | 3 | 1.5 |
| Stock market turnover ratio | 224 | 0 | 2.2 | 0.56 | 0.45 | 0.42 |
| Stock market total value traded/GDP | 237 | 0 | 2.7 | 0.34 | 0.17 | 0.43 |
| Panel B: Summary stats for the variables at Country, SIC, Year level | | | | | | |
| No. of deals | 9100 | 0 | 11 | 0.12 | 0 | 0.49 |
| Value of deals (US$ bn) | 9100 | 0 | 18.3 | 0.06 | 0 | 0.54 |
| EPL | 9100 | 1.3 | 4.1 | 2.7 | 2.6 | 0.78 |
| Creditor Rights | 9100 | 0 | 4 | 2.1 | 2 | 1.1 |
| GDP Per Capita | 9100 | 3926.5 | 34374.8 | 16305.1 | 15947.7 | 6172.4 |
| Population in 1000s | 9100 | 3314 | 82398.3 | 34528 | 37750.8 | 27137.2 |
| Exchange rate ($ US) | 9100 | 0.11 | 10.6 | 1.5 | 0.87 | 2.1 |
| Govt. share of GDP (%) | 9100 | 9.5 | 26.2 | 15.8 | 14.9 | 3.5 |
| Trade Openness (Imports + Exports) (%) | 9100 | 17.6 | 161.5 | 51.9 | 45.6 | 25.3 |
| Centralization Index | 7590 | 1 | 5 | 2.9 | 3 | 1.03 |
| Government (Schmidt Index) | 9063 | 1 | 5 | 2.7 | 3 | 1.6 |
| Stock market turnover ratio | 5618 | 0 | 2.2 | 0.61 | 0.49 | 0.45 |
| Stock market total value traded/GDP | 5936 | 0 | 2.7 | 0.37 | 0.21 | 0.44 |
| Labor Productivity | 4684 | 43.8 | 124.4 | 87 | 87.6 | 12.1 |
| Output Per Employee | 4684 | 46.7 | 127.1 | 87.7 | 88.7 | 12.1 |
| Labor Compensation | 4684 | 34.1 | 142.9 | 80 | 80.3 | 18.3 |

**Table 4: Description of Variables and their sources**

This table gives the list of variables used in the empirical study together with their brief description and their source.

| Variable | Description | Source |
|---|---|---|
| No. of deals | Number of privatization deals measured per country, year or country, industry and year | Privatization Barometer |
| Value of deals | Total value of privatization deals per country, year or country, industry, year in US$ billions | Privatization Barometer |
| EPL | Employment Protection Legislation Index | Gayle Allard, "Measuring Job Security Over Time: In Search of a Historical Indicator for EPL" |
| Creditor Rights | Creditor rights protection index | Simeon Djankov, Caralee McLiesh, and Andrei Shleifer, "Private Credit in 129 Countries" |
| GDP Growth % | Percentage growth of GDP at constant prices, year-on-year | Penn World Tables |
| GDP Per Capita | GDP Per Capita at current prices relative to the United States | Penn World Tables |
| Population | Country's population in 1000s | Penn World Tables |
| Exchange rate | Exchange rate of a country's currency to that of US | Penn World Tables |
| Govt. share of GDP (%) | Government share of real GDP, at current prices | Penn World Tables |
| Openness | Total of exports and imports of a country as a percentage of GDP, at constant prices | Penn World Tables |
| Centralization Index | An indicator which characterizes the degree of centralization of wage bargaining in a country. Values range from 1 to 5. | Nickell, William, 2006, "The CEP-OECD Institutions Data Set (1960-2004)" |
| Government | Government (Schmidt-Index): (1) hegemony of right-wing (and centre) parties (gov_left=0), (2) dominance of right-wing (and centre) parties (gov_left<33.3), (3) balance of power between left and right (33.3<gov_left<66.6), (4) dominance of social-democratic and other left parties (gov_left>66.6), (5) hegemony of social-democratic and other left parties (gov_left=100). | Klaus Armingeon, Sarah Engler, Panajotis Potolidis, Marlène Gerber, Phillip Leimgruber, "Comparative Political Data Set I, 1960-2008" |
| Stock market turnover ratio | Ratio of the value of total shares traded to average real market capitalization. | Thorsten Beck, Asli Demirgüç-Kunt and Ross Levine, (2000), "A New Database on Financial Development and Structure," World Bank Economic Review 14, 597-605. |
| Stock market total value traded/GDP | Ratio of total shares traded on the stock exchange to GDP. Along with stock market turnover ratio, it is used as a measure of a country's stock market liquidity. | Thorsten Beck, Asli Demirgüç-Kunt and Ross Levine, (2000), "A New Database on Financial Development and Structure," World Bank |

| | | |
|---|---|---|
| | | Economic Review 14, 597-605. |
| Labor Productivity | An index that measures of output of an industry per hour of labor expended on that output. A base value of 100 is taken for the year 2002. | Bureau of Labor Statistics |
| | | |
| Output per employee | An index measuring the output of an industry per worker using a base value of 100 in the year 2002. | Bureau of Labor Statistics |
| | | |
| Labor compensation | An index of worker compensation using a base value of 100 in 2002. | Bureau of Labor Statistics |

**Table 5: Effect of employment protection laws on privatization using fixed-effects panel regressions at the country, year level**

This table reports the regression results of value of deals and number of deals per year in a country on EPL index and a set of controls with country and year fixed effects. Standard errors reported in parentheses in parentheses are robust to heteroskedasticity. ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively. The sample includes privatization deals over the time period 1978 – 2003.

The regression equation estimated is: $y_{ct} = \alpha_c + \alpha_t + \beta_1 * EPL_{ct} + \beta X_{ct} + \varepsilon_{ct}$

| | Value of deals ($ bn) | | No. of deals | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| EPL Index | -1.258*** | -1.051*** | -1.086*** | -0.797** |
| | (0.352) | (0.343) | (0.357) | (0.367) |
| Creditor Rights | | 0.477 | | 0.049 |
| | | (0.293) | | (0.408) |
| GDP Growth (%) | | -0.096 | | 0.119 |
| | | (0.063) | | (0.144) |
| log(GDP Per Capita) | | -2.183 | | -1.797 |
| | | (1.473) | | (1.726) |
| log(Population in 1000s) | | -24.103** | | -31.789*** |
| | | (11.989) | | (11.953) |
| Country FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Observations | 364 | 364 | 364 | 364 |
| Adj R-squared | 0.459 | 0.469 | 0.596 | 0.598 |

**Table 6: Effect of employment protection laws on privatization using fixed-effects panel regressions at the country, industry, year level**

This table reports the regression results of value of deals and number of deals per sic and year in a country on EPL index and a set of controls with country, year and industry fixed effects. 2-digit SIC codes are used for computing the number of deals, value of deals, and industry fixed effects. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered by (country, year). ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively. The sample includes privatization deals over the time period 1978 – 2003.

The regression equation estimated is: $y_{ict} = \alpha_i + \alpha_c + \alpha_t + \beta_1 * EPL_{ct} + \beta X_{ct} + \varepsilon_{ict}$

| | Value of deals ($ bn) | | | No. of deals | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| EPL Index | -0.039*** | -0.039*** | -0.031*** | -0.033*** | -0.033*** | -0.019 |
| | (0.012) | (0.012) | (0.012) | (0.013) | (0.013) | (0.013) |
| Creditor Rights | | | 0.022** | | | -0.004 |
| | | | (0.010) | | | (0.013) |
| GDP Growth (%) | | | -0.003 | | | 0.009 |
| | | | (0.002) | | | (0.006) |
| log(GDP Per Capita) | | | -0.083 | | | -0.039 |
| | | | (0.063) | | | (0.072) |
| log(Population in 1000s) | | | -0.954** | | | -1.359*** |
| | | | (0.416) | | | (0.383) |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | No | Yes | Yes | No | Yes | Yes |
| Observations | 9,100 | 9,100 | 9,100 | 9,100 | 9,100 | 9,100 |
| Adj R-squared | 0.0282 | 0.0639 | 0.0643 | 0.100 | 0.135 | 0.137 |

**Table 7: Effect of employment protection laws on privatization using fixed-effects panel regressions including country- and industry-specific time trends**

This table reports the regression results of number of deals and value of deals EPL index and a set of controls with country, year, industry fixed effects and country, industry trends. Columns 1, 2 use data computed at country, year level. Columns 3, 4 use data computed at country, sic, year level. 2-digit SIC codes are used for computing the number of deals, value of deals, and Industry fixed effects. Standard errors reported in parentheses in Columns 1-2 (3-4) are robust to heteroskedasticity (and are clustered by country, year). ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively. The sample includes privatization deals over the time period 1978 – 2003. The regression equations estimated are:

Columns 1, 2: $y_{ct} = \alpha_c + \alpha_t + \alpha_c{*}t + \beta_1{*}EPL_{ct} + \beta X_{ct} + \varepsilon_{ct}$

Columns 3, 4: $y_{ict} = \alpha_i + \alpha_c + \alpha_t + \alpha_c{*}t + \alpha_i{*}t + \beta_1{*}EPL_{ct} + \beta X_{ct} + \varepsilon_{ict}$

| Data: | Country, Year level | | Country, SIC, Year level | |
|---|---|---|---|---|
| | Value of deals ($ bn) | No. of deals | Value of deals ($ bn) | No. of deals |
| | (1) | (2) | (3) | (4) |
| EPL Index | -1.341*** | -1.360** | -0.056*** | -0.079*** |
| | (0.440) | (0.525) | (0.017) | (0.021) |
| Creditor Rights | 2.204*** | -0.178 | 0.089*** | -0.003 |
| | (0.778) | (0.821) | (0.028) | (0.028) |
| GDP Growth (%) | 0.010 | 0.133 | 0.001 | 0.008 |
| | (0.070) | (0.155) | (0.003) | (0.006) |
| log(GDP Per Capita) | -3.966 | 7.378* | -0.156 | 0.172 |
| | (3.235) | (3.920) | (0.118) | (0.143) |
| log(Population in 1000s) | -9.807 | -1.114 | -0.746 | -1.365* |
| | (12.136) | (23.598) | (0.478) | (0.783) |
| Country, Year, Industry FE | Yes | Yes | Yes | Yes |
| Country FE*Year | Yes | Yes | Yes | Yes |
| Industry FE*Year | No | No | Yes | Yes |
| Observations | 364 | 364 | 9,100 | 9,100 |
| Adj R-squared | 0.558 | 0.637 | 0.0839 | 0.154 |

**Table 8: Effect of employment protection laws on privatization after controlling for potential sources of endogeneity**

Columns 1, 2 use data at country, year level with country, year FE and country trend. Columns 3, 4 use data at country, sic, year level with country, year, and industry FE, country and industry trends. Industry fixed effects are computed using 2-digit SIC. Standard errors reported in parentheses in Columns 1-2 (3-4) are robust to heteroskedasticity (and are clustered by country, year). ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively.

The regression equations estimated are:

Columns 1, 2: $y_{ct} = \alpha_c + \alpha_t + \alpha_c{*}t + \beta_1{*}EPL_{ct} + \beta X_{ct} + \varepsilon_{ct}$

Columns 3, 4: $y_{ict} = \alpha_i + \alpha_c + \alpha_t + \alpha_c{*}t + \alpha_i{*}t + \beta_1{*}EPL_{ct} + \beta X_{ct} + \varepsilon_{ict}$

**Panel A: Controlling for endogeneity due to changes in government stance (1978 – 2003)**

| Data: | Country, Year level | | Country, SIC, Year level | |
|---|---|---|---|---|
| | Value of deals ($ bn) | No. of deals | Value of deals ($ bn) | No. of deals |
| | (1) | (2) | (3) | (4) |
| EPL Index | -1.231*** | -0.957* | -0.051*** | -0.058*** |
| | (0.442) | (0.554) | (0.017) | (0.021) |
| Government | -0.190 | -0.730*** | -0.004 | -0.021*** |
| | (0.116) | (0.154) | (0.004) | (0.005) |
| Control variables as in table 4/5 | Yes | Yes | Yes | Yes |
| Country, Year FE, Country FE*Year | Yes | Yes | Yes | Yes |
| Industry FE, Industry FE*Year | No | No | Yes | Yes |
| Observations | 363 | 363 | 9,063 | 9,063 |
| Adj R-squared | 0.556 | 0.662 | 0.083 | 0.156 |

**Panel B: Controlling for endogeneity due to government share in production (1978 – 2003)**

| Data: | Country, Year level | | Country, SIC, Year level | |
|---|---|---|---|---|
| | Value of deals ($ bn) | No. of deals | Value of deals ($ bn) | No. of deals |
| | (1) | (2) | (3) | (4) |
| EPL Index | -1.339*** | -1.334** | -0.055*** | -0.077*** |
| | (0.439) | (0.523) | (0.017) | (0.021) |
| Govt. share of GDP | -0.012 | -0.176 | -0.005 | -0.017 |
| | (0.191) | (0.376) | (0.007) | (0.014) |
| Control variables as in table 4/5 | Yes | Yes | Yes | Yes |
| Country, Year FE, Country FE*Year | Yes | Yes | Yes | Yes |
| Industry FE, Industry FE*Year | No | No | Yes | Yes |
| Observations | 364 | 364 | 9,100 | 9,100 |
| Adj R-squared | 0.556 | 0.636 | 0.084 | 0.154 |

**Panel C: Controlling for endogeneity due to country's openness (1978 – 2003)**

| Data: | Country, Year level | | Country, SIC, Year level | |
|---|---|---|---|---|
| | Value of deals ($ bn) | No. of deals | Value of deals ($ bn) | No. of deals |
| | (1) | (2) | (3) | (4) |
| EPL Index | -1.477*** | -1.631*** | -0.059*** | -0.086*** |
| | (0.474) | (0.562) | (0.018) | (0.022) |
| Trade openness (Imports + Exports) | 0.081* | 0.141** | 0.004** | 0.006** |
| | (0.047) | (0.063) | (0.002) | (0.003) |
| Exchange Rate | 0.259 | 0.633** | 0.008 | 0.021* |
| | (0.200) | (0.289) | (0.008) | (0.011) |
| Control variables as in table 4/5 | Yes | Yes | Yes | Yes |
| Country, Year FE, Country FE*Year | Yes | Yes | Yes | Yes |
| Industry FE, Industry FE*Year | No | No | Yes | Yes |
| Observations | 364 | 364 | 9,100 | 9,100 |
| Adj R-squared | 0.560 | 0.643 | 0.084 | 0.155 |

**Panel D: Controlling for stock market liquidity (1989 – 2003)**

| Data: | Country, Year level | | Country, SIC, Year level | |
|---|---|---|---|---|
| | Value of deals ($ bn) | No. of deals | Value of deals ($ bn) | No. of deals |
| | (1) | (2) | (3) | (4) |
| EPL Index | -3.103** | -1.784 | -0.125** | -0.126** |
| | (1.420) | (1.500) | (0.050) | (0.049) |
| Stock Market Turnover Ratio | 1.136 | -3.869 | 0.070 | -0.125 |
| | (1.762) | (2.601) | (0.053) | (0.077) |
| Stock Market Total Value Traded / GDP | -0.406 | 4.008* | -0.019 | 0.178** |
| | (1.264) | (2.094) | (0.046) | (0.075) |
| Control variables as in table 4/5 | Yes | Yes | Yes | Yes |
| Country, Year FE, Country FE*Year | Yes | Yes | Yes | Yes |
| Industry FE, Industry FE*Year | No | No | Yes | Yes |
| Observations | 202 | 202 | 5,145 | 5,145 |
| Adj R-squared | 0.589 | 0.718 | 0.086 | 0.173 |

**Table 9: Two-country Difference-in-difference tests using EPL changes in some major countries**

Each specification includes data for the country mentioned at the top and a control country, where no change in EPL occurred in the time period considered. The first table has United Kingdom as the control country where EPL did not change in the period 1986 – 1997. The second table has France as the control country where EPL did not change in the period 1991 – 2002. In each table, panel A shows the results for value of deals in US$ billions, and panel B shows the results for no. of deals. Country, year, and industry fixed effects (at 2-digit sic code) are used in all the specifications. Standard errors are robust to heteroskedasticity and clustered by (country, year). ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively. The regression equation estimated is:

$y_{ict} = \alpha_i + \alpha_c + \alpha_t + \beta_1 * EPL_{ct} + \varepsilon_{ict}$

(i) Control country is United Kingdom. Time period: 1986 – 1997

| Panel A: Value of deals (US$ bn) per Country, SIC, Year | | | | |
|---|---|---|---|---|
| | Germany | Italy | Spain | Sweden |
| EPL Index | -0.426*** | -1.153*** | -0.382** | -0.278*** |
| | (0.087) | (0.300) | (0.142) | (0.076) |
| Country, Year, Industry FE | Yes | Yes | Yes | Yes |
| Observations | 900 | 864 | 804 | 756 |
| Adj R-squared | 0.115 | 0.125 | 0.160 | 0.115 |
| Panel B: No. of deals per Country, SIC, Year | | | | |
| | Germany | Italy | Spain | Sweden |
| EPL Index | -0.059 | -0.329*** | -0.007 | -0.103* |
| | (0.121) | (0.079) | (0.102) | (0.058) |
| Country, Year, Industry FE | Yes | Yes | Yes | Yes |
| Observations | 900 | 864 | 804 | 756 |
| Adj R-squared | 0.173 | 0.194 | 0.193 | 0.158 |

(ii) Control country is France. Time period: 1991 - 2002

| Panel A: Value of deals (US$ bn) per Country, SIC, Year | | | | |
|---|---|---|---|---|
| | Germany | Italy | Spain | Sweden |
| EPL Index | -0.082 | -0.516*** | -0.093** | -0.039 |
| | (0.147) | (0.124) | (0.044) | (0.062) |
| Country, Year, Industry FE | Yes | Yes | Yes | Yes |
| Observations | 900 | 864 | 804 | 756 |
| Adj R-squared | 0.122 | 0.125 | 0.112 | 0.0788 |
| Panel B: No. of deals per Country, SIC, Year | | | | |
| | Germany | Italy | Spain | Sweden |
| EPL Index | -0.262 | -0.409*** | -0.218*** | -0.235*** |
| | (0.219) | (0.116) | (0.029) | (0.081) |
| Country, Year, Industry FE | Yes | Yes | Yes | Yes |
| Observations | 900 | 864 | 804 | 756 |
| Adj R-squared | 0.216 | 0.291 | 0.186 | 0.183 |

**Table 10: Effect of employment protection laws on privatization using fixed-effects panel regressions including lagged and future values of EPL index**

Industry fixed effects are computed at 2-digit SIC level. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered by (country, year). ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively. The sample includes privatization deals over the time period 1978 – 2000. The regression equation estimated is:

$y_{ict} = \alpha_i + \alpha_c + \alpha_t + \alpha_c{*}t + \alpha_i{*}t + \beta_1{*}EPL_{ct} + \beta_2{*}EPL_{c(t+2)} + \beta X_{ct} + \varepsilon_{ict}$

| | Value of deals ($ bn) | | No. of deals | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $EPL_t$ | -0.046** | -0.109* | -0.095*** | -0.098* |
| | (0.019) | (0.059) | (0.027) | (0.059) |
| $EPL_{(t+2)}$ | -0.039** | -0.066 | 0.022 | 0.043 |
| | (0.019) | (0.062) | (0.023) | (0.074) |
| Centralization Indicator | | -0.029 | | -0.005 |
| | | (0.061) | | (0.063) |
| Creditor Rights | | 0.060 | | 0.027 |
| | | (0.054) | | (0.083) |
| GDP Growth (%) | | -0.005 | | 0.025* |
| | | (0.007) | | (0.013) |
| log(GDP Per Capita) | | -0.407 | | -0.164 |
| | | (0.525) | | (0.725) |
| log(Population in 1000s) | | -0.800 | | 3.442 |
| | | (2.880) | | (4.162) |
| Exchange Rate | | 0.048 | | 0.026 |
| | | (0.043) | | (0.052) |
| Govt. share of GDP | | -0.021 | | -0.006 |
| | | (0.038) | | (0.044) |
| Trade openness (Imports + Exports) | | 0.010 | | 0.030* |
| | | (0.009) | | (0.015) |
| Government | | 0.021* | | -0.033** |
| | | (0.012) | | (0.014) |
| Change in party composition | | 0.011 | | 0.024 |
| | | (0.027) | | (0.023) |
| Ideological gap bet. new & old govt. | | -0.023 | | -0.017 |
| | | (0.015) | | (0.015) |
| Stock Market Turnover Ratio | | 0.160** | | -0.279** |
| | | (0.068) | | (0.121) |
| Stock Market Total Value Traded / GDP | | 0.118 | | 0.249** |
| | | (0.083) | | (0.124) |
| Country FE, Year FE, Industry FE, Country FE*Year, Industry FE*Year | Yes | Yes | Yes | Yes |
| Observations | 8400 | 4076 | 8400 | 4076 |
| Adj R-squared | 0.097 | 0.117 | 0.160 | 0.169 |

**Table 11: Inter-industry differences due to labor productivity in effect of employment protection laws on privatization**

Industry fixed effects are computed at 2-digit SIC level. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered by (country, year). ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively. Since the data on labor productivity is available only for the time period 1987 – 2000, this sample includes privatization deals over this time period. The regression equation estimated is:

$y_{ict} = \alpha_i + \alpha_c + \alpha_t + \alpha_c{*}t + \alpha_i{*}t + \beta_1{*}EPL_{ct} + \beta_2{*}\ EPL_{ct}{*}LaborProductivity_{it} + \beta_3{*}\ LaborProductivity_{it} + \beta X_{ct} + \varepsilon_{ict}$

| | Value of deals ($ bn) | | | No. of deals | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| EPL Index | -0.381*** | -0.364*** | -0.584*** | -0.516*** | -0.537*** | -0.759*** |
| | (0.076) | (0.081) | (0.129) | (0.108) | (0.117) | (0.218) |
| EPL*Labor Productivity | 0.004*** | 0.003*** | 0.005*** | 0.005*** | 0.005*** | 0.008*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.003) |
| Labor Productivity | -0.008*** | -0.008*** | -0.013*** | -0.014*** | -0.015*** | -0.023*** |
| | (0.002) | (0.003) | (0.005) | (0.004) | (0.005) | (0.008) |
| Creditor Rights | | 0.000 | 0.005 | | -0.031 | 0.076 |
| | | (0.016) | (0.034) | | (0.026) | (0.083) |
| GDP Growth (%) | | -0.005 | -0.017** | | 0.016 | 0.032 |
| | | (0.003) | (0.007) | | (0.012) | (0.020) |
| log(GDP Per Capita) | | 0.018 | -0.758** | | 0.184 | -0.698 |
| | | (0.086) | (0.331) | | (0.137) | (0.504) |
| log(Population in 1000s) | | -0.801 | -0.836 | | -0.772 | -1.383 |
| | | (0.891) | (1.471) | | (0.860) | (1.913) |
| Centralization Indicator | | | -0.001 | | | -0.069 |
| | | | (0.034) | | | (0.072) |
| Exchange Rate | | | -0.055** | | | 0.012 |
| | | | (0.027) | | | (0.037) |
| Govt. share of GDP | | | -0.073** | | | -0.073** |
| | | | (0.029) | | | (0.032) |
| Trade openness (Imports + Exports) | | | 0.000 | | | 0.006 |
| | | | (0.003) | | | (0.006) |
| Government | | | -0.025** | | | -0.048*** |
| | | | (0.011) | | | (0.013) |
| Change in party composition | | | -0.020 | | | 0.005 |
| | | | (0.022) | | | (0.023) |
| Ideological gap bet. new & old govt. | | | -0.017 | | | -0.014 |
| | | | (0.013) | | | (0.014) |
| Stock Market Turnover Ratio | | | 0.075* | | | -0.091 |
| | | | (0.044) | | | (0.066) |
| Stock Market Total Value Traded / GDP | | | -0.151** | | | -0.019 |
| | | | (0.070) | | | (0.085) |
| Country, Year, Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 4,684 | 4,684 | 3,045 | 4,684 | 4,684 | 3,045 |
| Adj R-squared | 0.075 | 0.074 | 0.090 | 0.159 | 0.161 | 0.172 |

**Table 12: Inter-industry differences due to output per employee in effect of employment protection laws on privatization**

Industry fixed effects are computed at 2-digit SIC level. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered by (country, year). ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively. Since the data on output per employee is available only for the time period 1987 – 2000, this sample includes privatization deals over this time period. The regression equation estimated is:

$y_{ict} = \alpha_i + \alpha_c + \alpha_t + \alpha_c{*}t + \alpha_i{*}t + \beta_1{*}EPL_{ct} + \beta_2{*}\ EPL_{ct}{*}OutputPerEmployee_{it} + \beta_3{*}OutputPerEmployee_{it} + \beta X_{ct} + \varepsilon_{ict}$

| | Value of deals ($ bn) | | | No. of deals | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| EPL Index | -0.382*** | -0.364*** | -0.581*** | -0.488*** | -0.509*** | -0.688*** |
| | (0.078) | (0.081) | (0.134) | (0.111) | (0.121) | (0.218) |
| EPL*Output Per Employee | 0.004*** | 0.003*** | 0.005*** | 0.005*** | 0.005*** | 0.007*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.003) |
| Output Per Employee | -0.008*** | -0.008*** | -0.012** | -0.013*** | -0.014*** | -0.020** |
| | (0.003) | (0.003) | (0.005) | (0.005) | (0.005) | (0.008) |
| Creditor Rights | | 0.001 | 0.006 | | -0.029 | 0.076 |
| | | (0.016) | (0.033) | | (0.026) | (0.083) |
| GDP Growth (%) | | -0.005 | -0.017** | | 0.016 | 0.032 |
| | | (0.003) | (0.006) | | (0.012) | (0.020) |
| log(GDP Per Capita) | | 0.013 | -0.758** | | 0.168 | -0.709 |
| | | (0.087) | (0.330) | | (0.138) | (0.507) |
| log(Population in 1000s) | | -0.784 | -0.833 | | -0.783 | -1.297 |
| | | (0.886) | (1.457) | | (0.865) | (1.914) |
| Centralization Indicator | | | -0.004 | | | -0.067 |
| | | | (0.035) | | | (0.073) |
| Exchange Rate | | | -0.056** | | | 0.012 |
| | | | (0.027) | | | (0.037) |
| Govt. share of GDP | | | -0.073** | | | -0.074** |
| | | | (0.029) | | | (0.032) |
| Openness in current prices | | | 0.000 | | | 0.006 |
| | | | (0.003) | | | (0.006) |
| Government | | | -0.025** | | | -0.048*** |
| | | | (0.011) | | | (0.013) |
| Change in party composition | | | -0.021 | | | 0.004 |
| | | | (0.022) | | | (0.023) |
| Ideological gap bet. new & old govt. | | | -0.017 | | | -0.014 |
| | | | (0.013) | | | (0.014) |
| Stock Market Turnover Ratio | | | 0.074* | | | -0.086 |
| | | | (0.044) | | | (0.066) |
| Stock Market Total Value Traded / GDP | | | -0.149** | | | -0.024 |
| | | | (0.070) | | | (0.085) |
| Country, Year, Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 4,684 | 4,684 | 3,045 | 4,684 | 4,684 | 3,045 |
| Adj R-squared | 0.075 | 0.074 | 0.091 | 0.159 | 0.161 | 0.171 |

**Table 13: Inter-industry differences due to labor compensation in effect of employment protection laws on privatization**

Industry fixed effects are computed at 2-digit SIC level. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered by (country, year). ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively. Since the data on labor compensation is available only for the time period 1987 – 2000, this sample includes privatization deals over this time period. The regression equation estimated is:

$y_{ict} = \alpha_i + \alpha_c + \alpha_t + \alpha_c{*}t + \alpha_i{*}t + \beta_1{*}EPL_{ct} + \beta_2{*}EPL_{ct}{*}LaborCompensation_{it} + \beta_3{*}LaborCompensation_{it} + \beta X_{ct} + \varepsilon_{ict}$

| | Value of deals ($ bn) | | | No. of deals | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| EPL Index | -0.236*** | -0.221*** | -0.271*** | -0.300*** | -0.303*** | -0.349*** |
| | (0.069) | (0.077) | (0.073) | (0.057) | (0.061) | (0.112) |
| EPL*Labor Compensation | 0.002** | 0.002** | 0.001* | 0.003*** | 0.003*** | 0.003** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Labor Compensation | -0.004 | -0.004 | -0.002 | -0.003 | -0.004* | -0.002 |
| | (0.003) | (0.003) | (0.004) | (0.002) | (0.002) | (0.005) |
| Creditor Rights | | 0.001 | 0.005 | | -0.027 | 0.079 |
| | | (0.016) | (0.034) | | (0.026) | (0.084) |
| GDP Growth (%) | | -0.006 | -0.018*** | | 0.016 | 0.032 |
| | | (0.003) | (0.007) | | (0.012) | (0.020) |
| log(GDP Per Capita) | | 0.008 | -0.799** | | 0.158 | -0.737 |
| | | (0.092) | (0.336) | | (0.140) | (0.512) |
| log(Population in 1000s) | | -0.895 | -0.746 | | -0.969 | -1.525 |
| | | (0.916) | (1.478) | | (0.876) | (1.908) |
| Centralization Indicator | | | 0.008 | | | -0.071 |
| | | | (0.037) | | | (0.075) |
| Exchange Rate | | | -0.053* | | | 0.009 |
| | | | (0.027) | | | (0.037) |
| Govt. share of GDP | | | -0.077** | | | -0.076** |
| | | | (0.030) | | | (0.033) |
| Trade openness (Imports + Exports) | | | -0.000 | | | 0.006 |
| | | | (0.003) | | | (0.006) |
| Government | | | -0.026** | | | -0.049*** |
| | | | (0.011) | | | (0.013) |
| Change in party composition | | | -0.022 | | | 0.002 |
| | | | (0.022) | | | (0.024) |
| Ideological gap bet. new & old govt. | | | -0.016 | | | -0.012 |
| | | | (0.013) | | | (0.014) |
| Stock Market Turnover Ratio | | | 0.090* | | | -0.083 |
| | | | (0.047) | | | (0.068) |
| Stock Market Total Value Traded / GDP | | | -0.171** | | | -0.034 |
| | | | (0.075) | | | (0.087) |
| Country, Year, Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 4,684 | 4,684 | 3,045 | 4,684 | 4,684 | 3,045 |
| Adj R-squared | 0.074 | 0.074 | 0.088 | 0.159 | 0.160 | 0.170 |

**Table 14: Inter-industry differences due to union density in effect of employment protection laws on privatization**

Industry fixed effects are computed at 2-digit SIC level. Standard errors reported in parentheses are robust to heteroskedasticity and are clustered by (country, year). ***, **, and * denote statistical significance at 1%, 5%, and 10% respectively. Since the data on union density is available only for the time period 1987 – 2000, this sample includes privatization deals over this time period. The regression equation estimated is:

$y_{ict} = \alpha_i + \alpha_c + \alpha_t + \alpha_c{*}t + \alpha_i{*}t + \beta_1{*}EPL_{ct} + \beta_2{*}EPL_{ct}{*}UnionDensity_{it} + \beta_3{*}UnionDensity_{it} + \beta X_{ct} + \varepsilon_{ict}$

| | Value of deals ($ bn) | | | No. of deals | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| EPL Index | 0.016 | 0.024 | -0.022 | 0.012 | 0.020 | 0.049 |
| | (0.027) | (0.025) | (0.062) | (0.027) | (0.026) | (0.064) |
| EPL*Union Density | -0.005*** | -0.005*** | -0.008** | -0.005*** | -0.005*** | -0.010*** |
| | (0.001) | (0.001) | (0.003) | (0.001) | (0.001) | (0.002) |
| Union Density | 0.005 | 0.005 | -0.001 | 0.011** | 0.011** | 0.034** |
| | (0.006) | (0.006) | (0.020) | (0.004) | (0.004) | (0.013) |
| Creditor Rights | | 0.010 | 0.036 | | -0.007 | -0.017 |
| | | (0.013) | (0.038) | | (0.018) | (0.060) |
| GDP Growth (%) | | -0.004 | 0.037 | | 0.012 | 0.017 |
| | | (0.003) | (0.045) | | (0.008) | (0.083) |
| log(GDP Per Capita) | | -0.027 | -0.015* | | -0.012 | 0.026* |
| | | (0.077) | (0.009) | | (0.094) | (0.013) |
| log(Population in 1000s) | | -1.038* | -0.858** | | -1.462*** | -0.147 |
| | | (0.569) | (0.375) | | (0.563) | (0.722) |
| Centralization Indicator | | | -1.030 | | | 3.136 |
| | | | (1.996) | | | (4.129) |
| Exchange Rate | | | -0.033 | | | 0.033 |
| | | | (0.039) | | | (0.053) |
| Govt. share of GDP | | | -0.041 | | | -0.005 |
| | | | (0.030) | | | (0.044) |
| Trade openness (Imports + Exports) | | | 0.005 | | | 0.029* |
| | | | (0.004) | | | (0.015) |
| Government | | | -0.006 | | | -0.033** |
| | | | (0.014) | | | (0.014) |
| Change in party composition | | | -0.016 | | | 0.021 |
| | | | (0.027) | | | (0.023) |
| Ideological gap bet. new & old govt. | | | -0.007 | | | -0.018 |
| | | | (0.017) | | | (0.015) |
| Stock Market Turnover Ratio | | | 0.089 | | | -0.280** |
| | | | (0.061) | | | (0.121) |
| Stock Market Total Value Traded / GDP | | | -0.099 | | | 0.270** |
| | | | (0.087) | | | (0.126) |
| Country, Year, Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 7,224 | 7,224 | 3,913 | 7,224 | 7,224 | 3,913 |
| Adj R-squared | 0.079 | 0.079 | 0.107 | 0.150 | 0.152 | 0.176 |