

Employment Protection and Takeovers

Olivier Dessaint, Andrey Golubov and Paolo Volpin*

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Abstract

We show that labor restructuring is a key driver of corporate takeovers around the world. In a difference-in-differences setting, major increases in employment protection result in reduced takeover activity. Consistent with workforce restructuring being a prime source of synergies, tighter employment protection more than halves the combined firm gains and reduces takeover premiums by a third. In line with the labor channel behind these effects, the reforms do impede layoffs, and triple difference effects *within* affected country-years show a greater reduction in synergies in deals with greater potential for workforce restructuring. Bidders fail to adjust offer prices enough and exhibit lower returns.

JEL classification: J30; G14; G34; K31

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*Olivier Dessaint and Andrey Golubov are from Rotman School of Management, University of Toronto, 105 St. George Street, Toronto, Ontario, M5S 3E6, Canada (olivier.dessaint@rotman.utoronto.ca and andrey.golubov@rotman.utoronto.ca). Paolo Volpin is from Cass Business School, City University London, 106 Bunhill Row, London, EC1Y 8TZ, United Kingdom (paolo.volpin.1@city.ac.uk). We thank Kenneth Ahern, Pat Akey, Eric de Bodt, François Derrien, Craig Doidge, David Goldreich, Johan Hombert, Dimitris Petmezas, Antoinette Schoar, David Thesmar, Alfred Yawson, as well as seminar participants at City University London (Cass) and University of Toronto (Rotman) for useful comments and suggestions. Any errors are our own.

I. Introduction

Cost reductions in the pursuit of economies of scale and scope are commonly believed to be a major driver – and a key source of synergies – in corporate takeovers (see, e.g., Houston, James, and Ryngaert (2001) and Devos, Kadapakkam, and Krishnamurthy (2009)). Restructuring the workforce (in particular, in the form of laying off redundant white- and blue-collar workers at the target firm) is presumed to be one of the primary channels through which such cost reductions are obtained. In fact, employment considerations are a contentious issue in many takeovers and mergers. For example, when the U.S. pharmaceutical firm Pfizer made a takeover bid for its British-based rival AstraZeneca in 2014, the deal fell through in part due to the U.K. government’s opposition based on concerns that the acquisition would result in a significant reduction in the firm’s research and development personnel in the U.K.¹

While labor force issues are widely viewed as central to mergers and acquisitions (M&A), there is no systematic empirical evidence on the importance of labor restructuring as a driver of the market for corporate control and as a source of merger synergies. This is partly because labor regulations are largely uniform within countries, and any cross-country variation comes with a host of other pertinent differences. However, understanding the drivers of the market for corporate control is important because takeovers effect massive reallocations of resources both within and across industries and countries. For instance, the year 2014 alone has seen \$3.5 trillion worth of M&A activity globally, amounting to almost 5% of the world gross domestic product (GDP).² Our paper fills this void and provides the first systematic evidence on the link between labor regulation and takeovers.

Intuitively, restrictions to labor force restructuring are expected to affect takeover dynamics in several ways. First, if workforce restructuring represents an important

¹ “[In Drug Mergers, There’s One Sure Bet: The Layoffs](#)”, *The Wall Street Journal*, 29 April 2014.

² Source: Thomson Reuters SDC and the International Monetary Fund (IMF).

consideration in takeovers, then fewer takeover attempts are likely to materialize when employment is highly protected and redundancies are costlier. Second, where bids are made, the rigidity of labor regulation is expected to reduce the synergy gains from mergers and acquisitions, and bidders are expected to pay lower premiums (assuming rational behavior on their part). Finally, there may be further distributional effects on bidder performance depending on the offer price adjustment. If offer prices do not fully adjust for the lower expected synergies (e.g. price-insensitive empire-builders), rational bidders may be crowded out from the market, resulting in more overpayment, lower average bidder returns, and potentially a dead-weight loss in efficiency.

In this paper, we exploit the cross-country and time-series variation in employment protection to identify the causal impact of labor market rigidity on takeover activity and related economic outcomes. As a prequel to our main analysis, we begin with a simple cross-country test and show that the national level of employment protection explains a large part of cross-country differences in M&A activity and takeover premiums. We then turn to a difference-in-differences research design exploiting major employment protection reforms across a panel of 21 developed economies and show that employment protection changes have statistically significant and economically large effects on the market for corporate control. We begin by showing that the number of takeover deals drops by almost 15% in response to major employment protection increases. Similarly, deal volume drops by almost 30%. These effects are consistent with workforce restructuring being a major driver of corporate mergers and acquisitions, in line with the neoclassical, efficiency-seeking motive of takeovers (Gort (1969), Jensen (1993), Mitchell and Mulherin (1996), Andrade, Mitchell, and Stafford (2001)).

We then show that, following major employment protection increases, the combined firm cumulative abnormal returns (CAR) around merger announcements decline by 2 percentage points, relative to the unconditional combined firm CAR of 2.4%. In an efficient stock market,

the value change of the merging firms in response to deal announcement can be interpreted as the expected synergy gain brought about by the combination. The magnitude of our estimate indicates that labor force restructuring represents over 80% of the typical efficiency gain in takeover deals.³ Consistent with the synergies result, we further find that, in response to tighter labor regulation, takeover premiums drop by about 11 percentage points, relative to the unconditional takeover premium of 33.8% in the overall sample. Thus, if bidders fully adjusted the takeover premium for the changes in synergies, these results would suggest that workforce restructuring generates roughly a third of merger gains.

We further break down the combined firm CAR into the bidder CAR and the target CAR components (the latter largely reflecting the premium). While the target CARs decline in line with the premiums result, the bidder CAR is also significantly reduced. This suggests that, although bidders reduce offer prices following increases in employment protection, they do not adjust them enough. We present corroborating evidence by showing that i) the dollar value of the premium does not decline one-for-one with the synergies, with the difference roughly adding up to the decline in bidder dollar gains; ii) the expected synergy gain per dollar of premium offered declines; and iii) bidders are more likely to overpay, as manifested in a higher incidence of negative bidder returns. These results are consistent with price-insensitive empire-building or hubristic bidders crowding out rational efficiency-seekers. To support this interpretation, we show that the negative impact of tighter employment protection on bidder gains is less pronounced – and the offer price adjustment is greater – for bidders in countries with better governance.

³ An alternative and, perhaps, superior benchmark for evaluating the economic magnitude of the effect of the reforms is the average combined firm CAR in the pre-treatment period for the group of countries experiencing reforms. This value is 4.0%, implying that a two-percentage points reduction in the combined firm gain corresponds to a half of the synergy gains.

Our main results are robust to the usual methodological concerns, such as pre-treatment differences between treated and control firms, omitted variables (for instance, contemporaneous reforms in areas other than employment protection), and reverse causality. We show that, for each of the outcomes, there are no discernible effects of the labor reforms in the years prior to their passage, and a permanent effect immediately following the reform. To address the omitted variables concern we exploit cross-sectional differences *within country-years* and establish heterogeneous treatment effects that are in line with the labor channel. We show that, for bidder-target combinations with a high degree of business overlap (domestic intra-industry deals), increases in employment protection are associated with a further reduction in the combined CAR (-3.2%) as compared to deals with little business overlap in the same country and year. We also show that increases in employment protection are associated with a greater reduction in takeover gains for targets with poorer productivity relative to the bidders, in sectors with greater average workforce turnover following mergers, and in mature sectors. These latter tests include country-year fixed effects, which eliminates any time-varying heterogeneity between the reforming and non-reforming countries.⁴ Finally, to cement the labor force channel interpretation of the documented effects, we show that stronger employment protection reforms are, indeed, associated with a smaller reduction in the combined firm workforce following mergers.

The results we document survive a series of further robustness checks. The effect of employment reforms on the market for corporate control does not change significantly with the inclusion of the post-2008 period characterized by diverging economic fundamentals. Similarly, the results are unchanged when we exclude U.S. firms: the phenomenon we

⁴ Reverse causality, whereby takeover activity affects employment protection, is also an unlikely explanation for our findings. The most plausible reverse causality story, whereby politicians anticipate increased takeover activity and tighten labor protection to preserve employment, predicts a positive association between employment protection and takeover activity. Our results show the opposite.

document is a worldwide one. The results are also robust to the use of alternative measures of employment protection and to inclusion of a number of political economy controls. Finally, both positive and negative changes in employment protection move the outcomes in the expected direction; the effect of increases in employment protection is particularly large. We close with a tentative discussion of the implications of our findings for welfare. To the extent that mergers and acquisitions have positive externality effects, for example, through disciplining governance effects or creative destruction, the likely impact of stronger employment protection on welfare via the takeover channel is negative.

Our paper belongs to the growing literature on labor economics and finance. This literature was propelled by Botero et al. (2004) who study the regulation of labor around the world and its effects on various economic outcomes such as labor force participation and unemployment. Among the more recent work, Hombert et al. (2014) analyze the effect of unemployment insurance on entrepreneurial activity. Ellul, Pagano, and Schivardi (2014) show that family ownership and unemployment insurance are substitutes in providing job stability to workers. Giroud and Mueller (2015) identify the effect of leverage on employment via the corporate balance sheet channel. Tate and Yang (2015) show that inter-industry human capital transferability explains corporate diversification patterns and labor productivity gains in diversifying firms.

Closer to our setting, Tian and Wang (2014) use union elections in U.S. firms to study the effect of unionization on takeover outcomes; they show that unionized firms are less likely to attract bids, and receive lower premiums and exhibit longer bid durations when targeted. John, Knyazeva, and Knyazeva (2015) study the effect of employee rights on bidder performance and show that bidders (but not targets) from states with right-to-work statutes exhibit lower bidder returns, which they attribute to poorer deal selection and integration due to greater employee-shareholder conflicts at the bidder. We contribute to this literature by

providing the first evidence on the effects of employment protection regulation on the global market for corporate control and the importance of labor force restructuring as a motive and source of synergy gains in corporate takeovers.⁵

The paper proceeds in the following way. Section II discusses related literature. Our data and the research design are described in Section III. We present our main results and discuss their implications in Section IV. Finally, Section V concludes the paper with a summary of our findings.

II. Related Literature

The paper belongs to the literature on the effects of labor regulation on economic and financial performance, as well as to the voluminous M&A literature in corporate finance.

II.A Labor and Financial Economics

The evidence on stock prices overwhelmingly indicates that employment protection reduces firms' market value. Ruback and Zimmerman (1984), Abowd (1989), and Hirsch (1991) document that labor union coverage has a negative association with U.S. firms earnings' and market values. Chen, Kacperczyk, and Ortiz-Molina (2011) find that the cost of equity is higher in more unionized industries. Lee and Mas (2012) study the impact of firm-level union elections on firm performance and find that union wins are associated with stock price losses, as well as decreases in firm profitability and growth.

There is a large literature on the relation between employment and leverage. Bronars and Deere (1991) use industry-level data to document a positive correlation between leverage and

⁵ A related study by Alimov (2015) focuses exclusively on cross-border takeovers and shows that tighter employment protection in the target firm country is associated with *higher* levels of cross-border M&A activity, particularly when the bidder's country has a less rigid labor market.

the degree of unionization as a proxy for labor bargaining power. Matsa (2010) uses changes in labor laws in the U.S. to identify the causal relation (if any) between labor bargaining power and leverage and finds a positive relationship between increases in labor bargaining power and firm leverage. Similarly, Lin, Schmid, and Xuan (2015) show that German firms subject to the employee board representation mandate have higher leverage than similar firms not subject to the mandate. On the other hand, using international data, Simintzi, Vig, and Volpin (2015) find that reforms increasing employment protection are associated with a significant reduction in leverage.

Among the papers focusing on real economic variables, Botero et al. (2004) show that more stringent labor regulation is associated with lower labor force participation and higher unemployment. Besley and Burgess (2004) find that more pro-worker regulation is associated with lower investment and economic growth. Sraer and Thesmar (2007) and Mueller and Philippon (2011) show that family firms provide implicit employment insurance to their employees. Atanassov and Kim (2009) provide international evidence that strong unions reduce the scope for firms' financial and economic restructuring. On the positive side, Acharya, Baghai, and Subramanian (2013) find that pro-labor laws can have an ex-ante positive effect on firms' innovation.

II.B Labor and Takeovers

Early studies of employment changes following takeovers rely on relatively small samples. For instance, Bhagat, Shleifer, and Vishny (1990) study 60 hostile takeovers taking place during the 1980s and find that layoffs can explain at most 10-20% of takeover premiums. Similarly, Kaplan (1989) studies 76 management buyouts and finds only limited evidence of employment declines following takeovers. Evidence from plant-level studies is also mixed. On the one hand, Li (2013) studies productivity changes following takeovers and shows that new

owners reduce wages by 0.5% and employment by 2.1% at the target plants. On the other hand, Ouimet and Zarutskie (2015) argue and find that acquisitions can be used as a means of efficiently *increasing* the labor force.

Recent studies of labor protection and takeovers include Tian and Wang (2014) and John, Knyazeva, and Knyazeva (2015). Tian and Wang (2014) focus on the union status of target firms in the U.S. Exploiting close unionization ballots in a regression discontinuity design, they show that target firms that narrowly pass unionization ballots are less likely to receive takeover bids, attract lower offer premiums when they do become targets, and exhibit longer bid durations than firms that narrowly lose the unionization ballots. However, they do not find any differences in the combined firm value and performance, suggesting that the overall gains to takeovers of unionized targets are comparable to those of non-unionized firm takeovers.

John, Knyazeva, and Knyazeva (2015) focus on state-level employee protection in the U.S. and its effect on bidder returns. They show that bidding firms from stronger employee protection states, defined as states that have passed the right-to-work statutes, experience 0.5% lower announcement returns. Combined firm returns are also reduced by about 0.8%, suggesting there are lower synergy gains in deals involving bidders from strong employee right states. However, they further show that the higher bidder returns for bidders from weaker employee right states cannot be explained by workforce reductions. Instead, they argue that stronger bidding firm employee rights results in greater employee-shareholder agency conflicts, manifesting itself in poorer deal selection and subsequent integration. Furthermore, they show that the *target* firm state employee rights do not matter.

III. Data and Research Design

III.A Sample Composition and Data Sources

Our sample covers 21 developed countries for which we have data on major employment protection reforms over the 1985-2007 period. Our sample stops in 2007, because the global financial crisis that followed represents a severe structural shock for both employment protection and takeovers. In addition, because the crisis has affected the different economies differently – the recovery period is characterized by diverging economic fundamentals – the parallel trends assumption during this period is likely to be violated.⁶

The employment protection reforms data come from OECD and from Simintzi, Vig, and Volpin (2015), who build on the Employment Protection Legislation (EPL) indicators from OECD and manually identify major changes in labor market rigidity. The M&A data come from the Thomson Reuters SDC M&A database. We impose the following sample selection criteria:

- 1) The target is from one of the 21 developed OECD countries for which we have the employment protection data, namely Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the U.K., and the U.S.
- 2) The transaction value is at least \$50 million.
- 3) The acquisition represents a transfer of control, meaning that the bidder aims to bring its ownership in the target to more than 50%.

The sample includes the most active takeovers markets in the world (the U.S., the U.K., Canada, and Japan). We supplement the M&A data with country-level economic conditions

⁶ Nevertheless, we have experimented with extending the sample period to 2013. The results, reported in Appendix A, continue to hold.

from the IMF and the OECD, firm-level stock return and market index data from Datastream (as well as CRSP for U.S. firms), and firm-level fundamental characteristics from Global Compustat and Worldscope. The final sample size differs across tests due to availability of the requisite dependent and control variables. For instance, while the country-level tests include public and private bidders and targets, most of our deal-level tests require that the target firm be public, and tests on the combined firm CARs further require that the bidding firm also be listed. Sample descriptive statistics are presented in Table 1. Table 2 presents a country-level breakdown for the number and volume of M&A deals, combined firm CARs, and offer premiums. We refrain from a detailed discussion of sample statistics, apart from noting that the most salient features are in line with prior work using international M&A data (see, e.g. Rossi and Volpin (2004), Erel, Liao, and Weisbach (2012) and Lel and Miller (2015)). For instance, the average takeover premium is 33.8%, the average combined firm CAR is 2.4%, the average bidder CAR is 0.7%, consistent with the existing evidence that mergers generate moderate synergy gains, with targets gaining substantially from offer premiums and with bidders roughly breaking even. The average bid completion rate in the sample is 90%. All variables definitions are given in Appendix B.

[Please Insert Table 1 about Here]

III.B Preliminary Evidence

As a preliminary look at the data, in Figure 1 we plot the relation between the average M&A volume (scaled by GDP) for each of the 21 countries over the 1985-2007 period and the average EPL indicator, as published by the OECD, over the same period.⁷ The graph clearly displays a strong negative association between these two variables: on the one hand, M&A

⁷ We do not impose any data filters and use all takeovers from SDC in these macro country-level tests. The results are identical using only the deals satisfying our main sample selection criteria.

activity is high (at about 8% of GDP) in countries like the U.S. and New Zealand, where the EPL score is low (at about 1); on the other hand, M&A activity is low (at 4% of GDP) in countries like Italy or Spain, where EPL score is high (at 3.3 in Italy and 3.1 in Spain). The cross-country explanatory power of employment protection for M&A activity is high: the R^2 of this simple regression is 0.34.

[Please Insert Figure 1 about Here]

In Figure 2, we plot the relation between the average offer premium as a percentage of the pre-bid price for the 21 countries and the average EPL indicator over the 1985-2007 period. This graph also displays a strong negative correlation: the offer premium is high (at about 30%) in countries like the U.S. and the U.K., where the EPL score is low (at about 1 in the U.S. and 1.8 in the U.K.); while the offer premium is low (at about 15%) in countries like Italy or Spain, where EPL score is high. Again, the explanatory power of this regression is quite high: the level of employment protection explains 24% of the cross-country variation in takeover premiums.

[Please Insert Figure 2 about Here]

These associations are consistent with our premise that workforce restructuring represents an important motive for corporate takeovers and a major source of synergy gains. There appears to be fewer takeovers in countries in which labor is highly protected. Similarly, offer premiums are lower in countries with stronger employment protection. Of course, the simple cross-country correlation is not evidence of a causal relationship and can reflect other relevant differences across countries. Nevertheless, these associations are consistent with our predictions. Moreover, they also suggest that labor market rigidity can help explain the substantial differences in the levels of takeover activity and takeover premiums across countries. We now turn to a difference-in-differences methodology that exploits intertemporal variation in employment protection *within* countries.

III.C Research Design

To identify the causal impact of labor market rigidity on the market for corporate control we exploit intertemporal variations in employment protection in a difference-in-differences research design. A canonical example of the difference-in-differences application in financial economics is the Bertrand and Mullainathan (2003) study of the effects of antitakeover laws on managerial behavior.

The key identifying assumption in a difference-in-differences design is that of parallel trends. Specifically, identification relies on the assumption that the outcome variable would have behaved in similar way across treated and control groups absent treatment. In our setting, this translates into maintaining that the market for corporate control would have evolved in a similar fashion across treated and control countries in the absence of labor reforms. In other words, to make causal claims, we need to ensure that employment protection reforms are not endogenous to takeover dynamics. This could be the case if an omitted variable is driving both employment protection reforms and takeover dynamics. In our model specifications, we will control for country-level changes that are possibly correlated with both. We will also explore triple difference effects in specifications that entirely absorb any country-year heterogeneity.

Another endogeneity concern is that of reverse causality, whereby the link goes from takeovers to employment protection. However, the most plausible reverse causality story produces an opposite association to that we hypothesize. Specifically, if politicians anticipate increased takeover activity and tighten labor protection to preserve employment, we should observe a *positive* association between employment protection and takeover activity, whereas we predict a negative one. Further, the existing work on the political economy of labor regulation shows that the most important determinants of employment protection are legal origin and economic development (Botero et al. (2004)), electoral rules (Pagano and Volpin (2005)), and wealth concentration (Perotti and Von Thadden (2006)). We will check for

robustness to the inclusion of these controls. We will also explore the dynamics of the effect (if any) in order to more closely establish the causal impact of the reforms.

As our major shocks to employment protection, we utilize 21 labor market reforms identified by Simintzi, Volpin, and Vig (2015). These reforms track changes to the national rules and regulations governing regular and fixed-term employment contracts, as well as collective dismissals. This includes procedural requirements on firing, standards for “unfair dismissals”, conditions on the use of temporary contracts, notice periods and severance pay requirements, delays and costs associated with collective dismissals. The reader can refer to Appendix B of their paper for the detailed description of each of the reforms in each of the countries.

Of these reforms, nine have tightened employment protection: Austria (1988), Belgium (1998), France (1990, 1993), Greece (1988), Italy (1991), Portugal (1989), Switzerland (1994), and the U.S. (1989). The other twelve reforms have reduced employment protection: Australia (2005), Austria (2003), Denmark (1990), Germany (1997), Italy (1998), Netherlands (1988, 1999), Norway (1994), Portugal (1991), Spain (1994), and Sweden (1993, 1997). Figure 3 graphically depicts the distribution of the two types of reforms over time.

[Please Insert Figure 3 about Here]

Note that not every country in our sample has experienced a major employment protection reform during the sample period: Canada, Finland, Ireland, Japan, New Zealand, and the U.K. did not have any such reform. As a consequence, these countries will always be in the control group. On the contrary, some of the sample countries have experienced two major reforms (namely, Austria, France, Italy, Netherlands, Portugal, and Sweden). The staggered nature of the reforms allows the same country to be in both treated and control groups at different points in time, further alleviating the concerns that the results could be picking up unobservable differences across countries.

Specifically, the treatment indicator $EPL_{k,t}$ (where k indexes countries and t indexes years) is set to zero for all countries as of 1985 ($EPL_{k,1985} = 0$). In each of the subsequent years, the prior year's value remains constant if there were no major employment protection reforms in that country in that year ($EPL_{k,t} = EPL_{k,t-1}$). It increases by one if there is a major reform increasing employment protection in that country and year ($EPL_{k,t} = EPL_{k,t-1} + 1$). Finally, it decreases by one if there is a major reform decreasing employment protection in that country and year ($EPL_{k,t} = EPL_{k,t-1} - 1$). By construction, this index treats all employment protection reforms equally. It is designed to capture large, long-run changes in employment protection regulation over time, and is not comparable across countries.

For the country-level tests (deal numbers and volumes), we perform weighted-least squares (WLS) regressions of the following specification:

$$y_{kjt} = \beta \times EPL_{k,t} + \theta \times X_{kt} + \gamma_j \times \alpha_k + \gamma_j \times \delta_t + \varepsilon_{kjt}, \quad (1)$$

where y_{kjt} is the number (value) of deals in an industry j in a country k in the year t , $EPL_{k,t}$ is a reform indicator for country k in the year t as defined above, X_{kt} is a vector of country-level controls, $\gamma_j \times \alpha_k$ is an industry-country fixed effect, $\gamma_j \times \delta_t$ is an industry-year fixed effect, and ε_{kjt} is the error term. We aggregate the deal numbers and volumes at the country-industry-year level, and weight by the average number of listed firms in the pre-treatment period. As the effect of the reform is more precisely estimated in countries with larger takeover markets, the weighting ensures that we give more weight to the more accurate estimations. In addition, this maintains consistency with the deal-level tests described below, where more weight is naturally put on the more active takeover markets with a greater number of deals entering the estimation.⁸

⁸ Weighting by the sample size is common practice in other parts of the literature that work more frequently with aggregate data. See, for example, Mian and Sufi (2014).

For the deal-level tests (combined CAR, offer premium, bidder and target CAR), we run the following regression specification estimated by ordinary least squares (OLS):

$$y_{it} = \beta \times EPL_{k,t} + \theta \times X_{it} + \gamma_j \times \alpha_k + \gamma_j \times \delta_t + \varepsilon_{it}, \quad (2)$$

where y_{it} is a deal-level outcome for deal i in country k in the year t , $EPL_{k,t}$ is a reform indicator for country k in the year t as defined above, X_{it} is a vector of deal-level controls, $\gamma_j \times \alpha_k$ is an industry-country fixed effect, $\gamma_j \times \delta_t$ is an industry-year fixed effect, and ε_{it} is the error term. That is, we make within-industry comparisons of the change in deal-level outcomes in countries passing an employment protection reform (treatment group) and the same change in countries not passing a reform (control group).

In all cases, statistical inferences are based on heteroskedasticity-consistent standard errors double-clustered by the two dimensions of the panel (country and year in deal-level tests; industry-country and industry-year in the country-level tests since we are collapsing the data at these levels).⁹

IV. Empirical Results

IV.A Country-Level Tests

We begin our analysis by examining the effect of employment protection reforms on the overall activity of the takeover market. To that effect, we examine deal numbers and deal volumes in countries passing employment protection reforms and compare them to deal numbers and volumes in countries with no changes to employment protection in that year. We aggregate deal numbers and volumes at the industry level. These country-level tests allow us to include both public and private bidders and targets. We use the thirteen SDC macro

⁹ We have experimented with alternative clustering levels. Double clustering by country and year used in the reported results produces the most conservative standard errors.

industries: keeping the categories broad enough avoids having zero or near zero values for certain industries in countries with relatively less active takeover markets, which would make the impact of the reforms (if any) look larger. We estimate the country-level tests using WLS, weighting by the average number of listed firms in that country measured over the 1985-1987 period – i.e., before the first reform in our sample.¹⁰ The weighting is done to ensure that smaller countries do not have a disproportionately large influence on the overall treatment effect. The results are reported in Table 3.

Column (1) of Table 3 estimates the baseline effect on deal volumes controlling for time-varying country-level economic conditions, namely GDP per capita, GDP growth, and a set of dummies for the level of creditor rights protection. We estimate that the average effect of labor reforms is -0.27, significant at the 1% level, suggesting that deal volumes drop by 27% in countries that tighten employment protection relative to deal volumes in the same industry in non-reforming countries. Column (2) examines the dynamics of the effect, with the coefficient estimates also graphed in Figure 4. There is no statistically significant effect in the years prior to the reforms, and there is a permanent decline in every year subsequent to the reforms. Finally, Column (3) examines separately positive and negative changes to employment protection. We find that each of them moves the outcome variable in the predicted direction: tighter labor protection has a negative impact, while lighter labor protection has a positive impact on M&A volumes.

Note however that the results based on deal volumes will somewhat overstate the true effect if there is also an impact on the pricing of the deals – that is, if offer premiums, which are included in deal values, are also reduced because of the reforms. We will examine this question in Section IV.C. An alternative way to circumvent this issue is to examine deal

¹⁰ We fix the value at the average of the pre-treatment period to avoid the weight being potentially affected by the reform. The results are identical if we allow the weights to update.

numbers. Columns (4), (5), and (6) therefore repeat the tests with deal numbers as the dependent variable. The average effect estimated in Column (4) is -0.14, again significant at the 1% level. The dynamics analysis reported in Column (5) and presented graphically in Figure 5 confirms that there is no effect prior to the passage of the reforms and that there is a permanent decline in deal numbers in all the years following the reforms. The magnitude of the coefficient suggests that deal volumes drop by about 14% in response to tighter employment protection. Column (6) reveals that the effect is large and significant for increases in employment protection, but not so for decreases.

[Please Insert Table 3 and Figures 4 & 5 about Here]

IV.B. Deal-Level Tests: Combined CAR

We now examine the effect of employment protection on the expected takeover synergies. If stock market participants correctly anticipate the costs and benefits of the merger, the change in the market value of the combined firm (combined firm CAR) provides an estimate of the synergy gains brought about by the combination (see, e.g. Bradley, Desai, and Kim (1988), Andrade, Mitchell, and Stafford (2001), Devos, Kadapakkam, and Krishnamurthy (2009)). Hence, these tests are based on a sample of deals where both the bidder and the target are listed. Table 4 presents the estimation results for the combined firm CAR analysis.

Column (1) shows that the average baseline effect is -1.99 percentage points, significant at the 1% level. Column (2) explores the dynamics of the effect and shows once again that there is no impact of the reforms on the combined firm gains prior to their passage, while there is a persistent decline in the years following the change (see also Figure 6). Both increases and decreases in employment protection move the takeover synergy in the predicted direction as shown in Column (3). Finally, in Column (4) we add target, bidder, and deal-level controls (namely the size of the target, whether it employs any defensive tactics, whether the bidder has

a toehold, whether the deals is in same industry, cross-border, paid for with cash, hostile, and includes competing bidders). We find that the magnitude of the effect is unchanged and is still significant at the 1% level. The coefficient estimate of -2.06 suggests that tighter employment protection reduces synergy gains by 83% of its unconditional average of 2.4%. To put a dollar value on this estimate, a 2.06 percentage points reduction in combined firm value is equivalent to a loss of \$235 million (\$45.4 million) in shareholder value for a mean(median)-sized bidder-target combination.

Note however that, insofar as the reforms do not completely shut down the ability to restructure the labor force but only make it costlier, this result is a lower-bound estimate on the importance of labor restructuring as a source of efficiency gains in takeovers. At the same time, if the costs associated with more onerous labor regulation prevent combinations of firms that would have created value through channels other than those related to workforce optimization (e.g., resource complementarities, technological innovation, cross-selling opportunities) then the above is an overestimate of the importance of employee restructuring as a source of takeover gains. It is not clear which of the above two forces dominates, and therefore it is difficult to conclude exactly how much of the estimate above represents a transfer from employees to shareholders.

[Please Insert Table 4 and Figure 6 about Here]

One concern with our analysis so far is that some omitted variable (for instance, a contemporaneous reform in areas other than employment protection) may be behind both the change in the combined CAR and employment protection. To address this concern we explore cross-sectional, or, heterogeneous, treatment effects. If the results we document are attributable to the labor channel, we should expect to observe stronger effects on deals with greater potential for workforce restructuring. To that effect, we employ four proxies for the potential for workforce optimization.

First, we construct a business overlap indicator, which is a dummy variable that takes the value of one if the bidder and the target belong to the same country and same industry, and zero otherwise. Changes in employment protection should have a greater effect when bidder and target have a high degree of business overlap since these deals are more likely to be motivated by cost-cutting objectives. Second, we proxy for the labor productivity gap between the bidder and the target using the difference between the sales-per-employee ratios of the two firms. This measure captures the potential for labor efficiency improvements at the deal level and we therefore expect a greater impact of the reforms on deals with greater such potential. Third, we estimate the importance of workforce reduction at the industry-level as the sample average workforce reduction one-year after the merger. We expect a greater decline in takeover synergies for industries with higher levels of post-merger employee turnover. Finally, we employ industry maturity measured by sales growth as an (inverse) proxy for consolidation needs. Higher values of this variable imply higher growth potential and, thus, less need for consolidation. Table 5 reports the results of these cross-sectional tests.¹¹

Columns (1) and (2) use the business overlap indicator. In Column (1), we augment the specification estimated in Table 4 with the business overlap indicator interacted with EPL, as well as with country and year fixed effects (to ensure a correct triple difference interpretation).¹² In Column (2) we further add country-year fixed effects, such that the EPL indicator is absorbed and only the interaction effects between the labor reform indicator and our proxies for workforce optimization potential are identified and can be interpreted as a triple

¹¹ Note that we replace industry-year and industry-country fixed effects with country and year fixed effects in these tests – this is because cross-industry differences is precisely the variation we wish exploit in two out of the four proxies.

¹² The main effect of the conditioning variable is omitted when fully interacted with industry-year and industry-country fixed effects.

difference effect. There is a more negative effect of employment protection reforms on combined CAR for deals with greater potential for workforce synergies.

Columns (3) and (4) repeat these tests using the relative labor productivity of the bidder and the target. Labor productivity is defined as turnover scaled by the number of employees. The higher the values of this ratio, the higher is the potential for improving workforce utilization at the target firm. Consistent with this prediction, we find that the reduction in combined firm gains is greater for deals in which the potential for labor productivity improvements is greater.

Columns (5) and (6) use post-merger workforce reduction at the industry level as our measure of the importance of labor restructuring. There is a stronger negative effect of labor reforms on the combined CAR in industries that exhibit higher levels of post-merger employee turnover. Finally, Columns (7) and (8) use target firm industry growth (computed as the weighted average sales growth of all firms in that four-digit SIC industry in the 3-years following the deal) as a proxy for the need for consolidation. The coefficient on the interaction terms is positive, suggesting that the effect of employment protection is significantly *less* negative when the target industry is characterized by greater growth opportunities.

Overall, there is strong evidence of heterogeneous treatment effects that is consistent with the labor force channel being the driver of the established link between labor regulation and takeover gains.

[Please Insert Table 5 about Here]

IV.C. Deal-Level Tests: Offer Premium

Another way to gauge the importance of labor force restructuring as a source of synergy gains in takeovers is to look at the offer premium. If restructuring the labor force at the target firm is an important part of the value of the target to a potential bidder, then we should expect

reforms leading to more rigid labor markets to be associated with declines in takeover premiums (assuming that target shareholders extract at least a portion of the takeover gains). These tests are based on a sample of listed targets, but the bidder can be either a private or a public firm. This prediction is tested in Table 6.

Column (1) reports the baseline estimate of the effect of labor reforms on premiums (in percentage points). The coefficient is -10.95, significant at the 1% level. This indicates that, following the reforms, takeover premiums decline by about 11 percentage points relative to countries that have not experienced a reform in that year. Column (2) examines the dynamics of the effect. Once again, there is no effect on takeover premiums before the reforms take place, and there is permanent decline in premiums in each of the years after the reform is passed. This result is also presented graphically in Figure 7.

Column (3) separately examines increases and decreases in employment protection. We find that both types of reforms move the outcome variable in the predicted direction. Finally, Column (4) incorporates target, bidder, and deal-level controls, namely the size of the target, whether it employs any defensive tactics, whether the bidder has a toehold, whether the bidder is a listed firm, whether the deals is same industry, cross-border, paid for with cash, is hostile, and includes competing bidders. The inclusion of these controls leaves the magnitude of the coefficient virtually unchanged, at -11.24, and still statistically significant at the 1% level. An almost 11-percentage points reduction in takeover premiums corresponds to about a third of the unconditional premium of 33.8%. Once again, these results suggest that the ability to restructure the labor force at the target firm represents a large proportion of the target firm value to the potential bidder.

[Please Insert Table 6 and Figure 7 about Here]

IV.D. Deal-Level Tests: Other Tests

So far we have established that the takeover gains are reduced by tighter labor protection. It is interesting to examine whether bidders act rationally and fully incorporate the consequences of employment protection reforms and adjust offer premiums enough to compensate for lower potential synergy gains. To that effect, we break down the combined firm CAR into its components, namely the bidder CAR and the target CAR. If bidders make full adjustment to the offer price, we should see no effect of employment protection reforms on bidder returns. However, if bidders do not fully adjust their offers for the scarcer workforce optimization opportunities, then we should see a negative impact of employment protection reforms on bidder returns. As for the target CARs, we expect them to be negatively affected by increases in employment protection. This is because target CARs largely reflect offer premiums, and we have already established that offer premiums decline following employment protection increases. Table 7 presents the results of these and related tests. Odd-numbered columns are for all available observations, and even-numbered columns are for the subset of deals for which we have bidder-, target- and deal-level controls.

Panel A, Columns (1) and (2) replicate the baseline results for the combined CAR shown in Table 4. Columns (3) and (4) replace the dependent variable with the target firm CAR. As expected, the results mirror those for offer premiums, namely that the target firm gains decline by about 4 percentage points in response to regulation making labor restructuring costlier.¹³ Columns (5) and (6) report the results for the bidding firm CAR. The estimates indicate that bidder returns decline in response to employment protection increases by about 0.45 percentage points, or by about 1.16 percentage points when control variables are included (which in this

¹³ The magnitude of the effect on target CARs is somewhat smaller than that on the offer premium. This can be due to several reasons. First, the CAR incorporates market expectation about completion probability. Second, offer premiums are measured relative to an undisturbed share price four weeks prior to the announcement, whereas the target CAR misses any run-up prior to the immediate window around the announcement day.

case amounts to focusing on the subset of listed targets). These results suggest that, on average, bidders do not fully adjust offer prices in response to labor reforms that make workforce restructuring costlier.

We explore the idea of insufficient adjustment of the offer price in more detail by focusing on the subsample of deals between listed firms, which allows us to compare the effects on the bidder and the target. In Panel B, we estimate the distribution of synergy gains between the bidder and the target by computing the dollar values of the synergy, target gain, bidder gain, and offer premium scaled by combined market value before the announcement (we scale by the combined market value and not the synergy as the latter is sometimes negative). Column (1) produces the same estimate as that for percentage combined firm gains, since the two variables are identical by construction. Columns (2) and (3) show both targets and bidders bear part of the decline in the combined dollar gains. Finally, Column (4) shows that the offer premium does not reduce one-for-one with the synergy gain, suggesting that bidders do not adjust prices enough. Moreover, this estimate helps reconcile the preceding analysis. Specifically, this estimate indicates that the offer premium relative to the combined firm value is reduced by 1.31, while the synergy gain is reduced by 2.06. Adjusting the premium estimate by the expected probability of payment (completion) of 81% in this subsample, the difference between 2.09 and $1.31 \times 0.81 = 1.06$ is 1.03, which roughly equals the decline in the dollar gain of the bidder (-0.97).

Finally, Panel C provides two additional corroborating results. Column (1) focuses on the subsample with positive dollar synergy gains and, again, shows that the expected synergy gain per dollar of offer premium is reduced after the reforms. Lastly, if bidders do not adjust offer prices enough, we should expect to see more overpayment and thus higher incidence of value destruction for the bidder, or negative bidder returns. Column (2) establishes that the

probability of observing negative bidder CARs indeed increases significantly following the labor reforms.¹⁴

[Please Insert Table 7 about Here]

While the average effect of employment protection on bidder CAR is negative, the M&A literature distinguishes between value-maximizing mergers initiated by efficiency-seeking managers and agency-driven deals proposed by empire-building managers. We would expect the former category of bidders to fully adjust their offers and not experience declines in their performance, while the latter bidders should be the ones affected the most. To capture the heterogeneity in bidder motives and rationality, we use the cross-country differences in corporate governance quality: we expect bidders from better governance regimes to have a larger fraction of efficiency-seeking types, and bidders from weaker governance regimes to have a higher proportion of empire-builders. Specifically, we condition the effect of EPL reforms on bidder CARs and takeover premiums on the anti-self-dealing index of Djankov et al. (2008) as our proxy for governance quality at the country level.¹⁵ We also perform this analysis on the combined firm gains to distinguish between price adjustment and overall deal quality effects in strong versus weak governance countries. This again requires that the tests be based on a sample of deals where both the bidder and the target are listed. These results are reported in Table 8.

¹⁴ If targets capture a larger portion of synergies following the reforms, it would be rational for targets to accept bids more often. We perform analysis of deal completion probabilities and find that increased employment protection is associated with a modest increase in deal completion; however the effect is not robust across specifications.

¹⁵ On the one hand, a firm-level governance quality measure could provide more direct evidence, but such measures are not widely available for a panel of countries. On the other hand, use of a country-level governance measure alleviates the concern that firm-level governance structures are endogenous choices.

Column (1) shows the effect of EPL reforms on bidder CARs conditional on the governance quality in the bidder's country. As expected, the interaction term is positive and statistically significant at the 1% level, suggesting that bidders from better governance regimes are much less affected by the labor reforms. Column (2) repeats this test on offer premiums. Here, we find that the interaction term between the EPL reform indicator and governance quality is *negative* and statistically significant at the 1% level, suggesting that bidders from better governance regimes adjust offer prices *more*. Finally, Column (3) shows that the effect of the reforms on the overall deal quality as captured by the combined firm CAR does not vary with the bidding firm's country governance quality. This confirms that the less negative effect of EPL reforms on bidder performance in better governance regimes works through offer price adjustment and is not due to differences in overall value creation across strong/weak governance environments.

Overall, these results are consistent with both types of bidders being present in the cross-section of deals, with the efficiency-seeking type being crowded out from the market by the price-insensitive empire-builders following the reforms.¹⁶ If this entails suboptimal bidder-target pairing, then the implication of this effect is an overall dead-weight loss in efficiency.

[Please Insert Table 8 about Here]

¹⁶ A limitation of this analysis is that we only observe bidder behavior conditional on participating in the bidding contest and thus appearing in our data. It is possible that certain rational bidders do not join the contest altogether, either in anticipation of being outbid or after observing a bid that is already too high. A quick inspection of the data reveals that the EPL reforms have a negative but statistically insignificant impact on the probability of observing competing bidders or upward price revisions. These results also imply that the negative effect of EPL reforms on bidder CARs cannot be explained by improvements in target firm bargaining power.

IV.E. Robustness Checks

In this section, we comment on a number of further robustness tests that, for the sake of exposition, are reported in Appendix A. First, we extend the sample period to include the post-crisis (post-2008) period. Second, as the U.S. represents a large portion of the sample and is also the most active takeover market, we re-estimate our results excluding the U.S. Third, we switch our measure of employment protection to the original OECD EPL index used in Figures 1 and 2; this index does not focus only on the large reforms but is comparable across countries. Fourth, we incorporate additional country-level political economy controls, namely income inequality (Gini coefficient), left/right governments, union density, voting rules (proportionality), and the corporate tax rate. Fifth, we exclude countries that have not experienced any labor reforms throughout the sample period. In all cases, we find that our main results, namely a negative effect of stronger employment protection on M&A activity, synergy gains, and premiums continue to hold.

Another potential concern with our analysis so far is that the composition of deals may change following the reforms. If this is the case, composition effects may bias our estimates of the true effect of the reforms on takeover gains. For example, suppose that, following the reforms, deals with low labor force synergies disappear from the market and only the very profitable deals remain. This would lead us to underestimate the importance of workforce restructuring for takeover gains. Alternatively, suppose that bidders are financially constrained and the additional burden of labor market rigidity forces them to divert financial resources away from the takeover towards other, more profitable uses. In this case, stronger employment protection can prevent otherwise value-creating deals from happening, causing us to overestimate the importance of workforce restructuring as a source of synergies. In order to address the above concern, ideally, one should compare the *same* deal before and after the reforms; however, this is clearly impossible. Alternatively, one can attempt to compare deals

with very similar characteristics before and after the reform to control for as much of deal heterogeneity as possible.

To the extent that takeover gains are industry-specific, within-industry comparison helps alleviate concerns regarding deal composition. Note that our tests already include industry-year and industry-country fixed effects. By comparing the outcomes of deals within the same industry, we somewhat mitigate the concern that what we are capturing is a re-composition effect. Throughout our paper we have kept our industry definitions relatively broad (the 13 industries classification provided by SDC) in order to ensure sufficient number of deals per industry in the country-level tests. However, for the deal-level tests we can introduce more granularity into our industry definitions. We have experimented with using 814 SIC4 industries and further saturating the combined firm CAR regression specifications with 1,558 SIC4-year and SIC4-country dummies. Here, the point estimate of the effect of EPL reforms is -2.9. Relative to the average combined firm CAR in the pre-treatment period of 4.0%, the results suggest that workforce restructuring represents about 73% of the typical synergy gain. Note, however, that this precision comes at the cost of losing roughly 30% of the sample due to single observations for certain SIC4-year or SIC4-country combinations being absorbed by the fixed effects.

Perhaps an even more conservative way to control for deal heterogeneity before and after the reform is to examine the effects of the reform on deals involving the *same target company*. This test is made possible by the presence of withdrawn bids in our sample, with those target firms being targeted again after the EPL reforms are enacted. To the extent that synergy gains are target-specific, such a comparison again helps alleviate deal composition concerns. In our sample, there are 1,142 deals involving 537 distinct target firms being targeted both before and after the EPL reforms. Here, we find that the combined firm CAR is reduced by 4.0% in response to the reforms. Relative to the average combined firm CAR in the pre-treatment period

of 5.1% in this subsample, one can argue that roughly 78% of the gains priced by the market at the time of the first bid announcement (before the EPL reform is passed) come from workforce restructuring. Again, note that the sample size in this test is very low. Nevertheless, the aforementioned results help alleviate the concerns that our estimates of the importance of workforce restructuring for takeover gains may be biased by deal composition effects.

IV.F. The Labor Channel

Finally, a natural extension of our analysis is to examine the effect of labor reforms on post-merger layoffs themselves, with the prediction being that tighter employment protection is, indeed, associated with lower levels of post-merger workforce restructuring. Note however that this analysis is complicated by several data limitations. First, firm-level employment data is only available for 20% of the sample. Second, any changes in employee headcount reflect both firing and hiring, while we expect the mechanism for the effects we document to work largely through the former. And third, we can only observe changes in employment at the combined firm relative to the bidder and the target before the deal, whereas we expect most of the layoffs to occur at the target firm, and the latter typically represents a smaller part of the combined firm.

With these caveats in mind, we proceed to examining the effect of mergers on employment, and then evaluate the effect of EPL reforms on this association. These tests are performed at the deal-year level, whereby we expand our initial dataset of deals by adding the information on the number of employees at the bidder and the target prior to the deal, as well as at the combined entity following the merger. We perform this procedure on completed deals only and restrict our analysis to a five-year window around the year of deal completion.

With this deal-year panel at hand, we estimate the change in the (log) number of employees at the combined firm after the merger relative to the combined number of employees

in the bidder and the target prior to the deal (denoted “*Post Merger*”)¹⁷. Inclusion of industry-year fixed effects in this specification turns the coefficient on the *Post Merger* indicator into an estimate of the abnormal change in employment following takeovers, with the benchmark being the change in employment at our sample firms in the same industry that have not merged in that year. We then test whether this effect is different across reforming and non-reforming country-years by interacting the *Post Merger* indicator with the *EPL* indicator and including EPL-industry-year and EPL-industry-country fixed effects; the latter ensures that we are not capturing the effect of the reforms on employment (if any) that is independent of mergers. Finally, we verify that the effect of EPL reforms on post-merger restructuring does not arise because the laws are passed at a time when post-merger restructuring becomes more difficult across countries, or because the reforms are passed in countries where the magnitude of post-merger restructuring is always low. This is achieved by augmenting the previous specification with Post Merger-industry-year and Post Merger-industry-country fixed effects. As with all tests, we cluster standard errors at the two dimensions of the panel, which in this cases amounts to double clustering by deal and by year. Table 9 reports the estimation results.

Column (1) shows the baseline estimate of the effect of takeovers on employment (*Post Merger*), with the coefficient indicating that, on average, following takeovers, employment at the combined firm is reduced by about 6% relative to the employment at the bidder and the target prior to the deal. Column (2) estimates the effect of EPL reforms on this association (*EPL x Post Merger*) and shows that, following the reforms, the reduction in employment following takeovers is moderated by about 5%, relative to an unconditional reduction in employment of about 9%.¹⁸

¹⁷ This is achieved by the inclusion of deal fixed effects, such that we always compare post-merger employment to the pre-merger employment at the same pair of firms.

¹⁸ Note that the main effect of labor reforms (*EPL*) on employment at the combined firm is absorbed when fully interacted with industry-year and industry-country fixed effects.

We also explore the dynamics of the effect of employment protection reforms on workforce restructuring in the post-merger years (Column (3)). We find that there is significant reduction in the combined firm employment in all years following the merger, and that there is an offsetting effect in all of those years when a country passes an EPL reform. Figure 8 presents this result graphically and reveals that, prior to employment protection tightening or after a reduction in employment protection, the reduction in post-merger employment is statistically significant for every post-merger year; this is not the case following employment protection tightening or before the adoption of a less protective reform.

Finally, in Column (4) we estimate the difference-in-differences effect of EPL reforms on post-merger workforce restructuring and we find that EPL reforms are associated with preservation of roughly 7% of the combined firm workforce.¹⁹ Overall, these results further cement the labor force channel interpretation for the effects on deal outcomes that we documented above.

[Please Insert Table 9 and Figure 8 about Here]

IV.G. Discussion and Implications

A question that follows naturally from our analysis is whether the net economic impact of stronger employment protection via the takeover channel is positive or negative. Thus, in this section we discuss the broader implications of our findings, namely, the likely effect of employment protection on efficiency and welfare. Conceptually, to the extent that mergers and acquisitions have positive externality effects, impediments to takeover activity from labor market rigidity represent a cost to the society.

¹⁹ Once again, the main effect of takeovers (*Post Merger*) on employment at the combined firm is omitted when fully interacted with industry-year and industry-country fixed effects.

First, if mergers facilitate efficient restructuring of industries and firms (Maksimovic and Phillips (2001); Andrade and Stafford (2004)) and contribute towards technological innovation (see Bena and Li (2014)), then higher labor force burden prevents some of these value-increasing mergers from happening. The fact that mergers create private value, as evidenced by an uptick in the value of the combined firm, is widely established, including our own evidence of positive combined firm gains. Moreover, existing literature suggests that these gains do not simply come at the expense of customers, suppliers or competitors. For instance, Shahrur (2005) shows that privately value-increasing horizontal mergers are associated with *positive* wealth gains for corporate customer, supplier and competitor firms. Similarly, Sheen (2014) shows that mergers between product market competitors result in cost savings that are passed on to the consumer in the form of lower prices while maintaining average product quality. And in vertical integration type-deals, Shenoy (2012) finds no evidence of collusion effects that would hurt customers or foreclosure effects that would hurt rivals. Finally, the evidence in Devos, Kadapakkam, and Krishnamurthy (2009) suggests that little-to-none of the takeover gains come at the expense of the tax authorities.

Secondly, to the extent that takeovers act as an external governance mechanism, reduction in takeover activity is expected to lead to more managerial slack and thereby adversely affect productivity and output. For instance, using state-level business combination law passage in the U.S., Bertrand and Mullainathan (2003) show that greater insulation from the market for corporate control is associated with a “quieter life” for managers, manifesting itself in lower productivity and profitability, and lower plant creation and destruction. In a similar setting, Atanassov (2013) shows that greater insulation from the disciplining effect of takeovers is associated with reduced and less influential innovation activity by firms. In an international setting, Lel and Miller (2015) show that less takeover-friendly environments are

associated with fewer disciplining takeovers and with weaker CEO turnover-performance sensitivity.

Thus, the extent to which the burden of labor market rigidity imposes additional costs on bidders and prevents efficiency-enhancing mergers, as well as the extent to which it reduces managerial discipline and encourages slack, reduced takeover activity because of tighter employment protection is likely to have a detrimental effect on social welfare.

V. Conclusion

In this paper, we have set out to establish the importance of labor restructuring as a motive and as a source of synergy gains in corporate takeovers. Using cross-country and time-series variation in the degree of employment protection afforded by national laws and regulations, we have shown that employment protection has a profound effect on the market for corporate control. Passage of major labor regulation reforms that increase employment protection is associated with a marked decline in the number and volume of mergers and acquisitions. These reforms also reduce total synergy gains by 50-80% and takeover premiums by a third. These results are consistent with workforce restructuring being a significant source of cost synergies. To buttress this interpretation we show that i) the reforms are indeed associated with the extent of post-merger employment changes, and ii) within country-years subject to the reforms, the decline in takeover gains is stronger for deals with greater potential for workforce reduction. Bidders do not fully adjust offer prices for the changes in synergies and experience a decline in their returns, but these latter effects are mitigated by bidder governance quality. We interpret this as potential crowding-out of rational bidders by price-insensitive empire-builders or hubristic bidders, leading to a dead-weight loss through suboptimal bidder-target matching and/or less effective management of target assets.

Overall, our findings suggest that labor restructuring is a major driver of the market for corporate control and a key source of merger synergies. It appears that labor market rigidity can explain much of the differences in the levels of takeover activity and takeover premiums around the world. Finally, mergers and acquisitions is an additional channel through which employment protection regulation affects productivity and output. While a complete general equilibrium analysis is beyond the scope of our study, if takeovers exhibit positive externalities on the economy, for example through disciplining effects or by contributing to creative destruction, the likely effect of increased employment protection on welfare is negative.

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Table 1***Sample Descriptive Statistics***

The table presents summary statistics for the main variables used in the analysis. The sample includes 45,696 M&A deals from SDC over the 1985-2007 period. To be included in the sample, the target company must be located in one the 21 OECD countries considered in our study, the deal value must be higher than \$50M, and the stake sought by the bidding firm must be greater than 50%. All variables are defined in Appendix B. All continuous variables are trimmed at the 1% level in each tail.

	Count	Mean	Stdv	p10	p50	p90
<i>Deal-level Variables</i>						
Anti-Self	44,738	0.64	0.18	0.38	0.65	0.95
Bidder Market Value (in million \$)	24,724	8,479	29,434	142	1,372	16,276
Bidder Market Value (log)	24,724	7.3	1.8	5.0	7.2	9.7
Business Overlap	45,696	22.3%	41.6%	0.0%	0.0%	100.0%
CAR Bidder [-3,+3]	23,806	0.7%	8.2%	-8.3%	0.1%	10.3%
CAR Combined [-3,+3]	7,129	2.4%	7.7%	-6.2%	1.5%	12.3%
CAR Target [-3,+3]	11,949	20.6%	22.4%	-2.0%	16.9%	49.4%
Combined Gains by \$ of Premium Offered	5,560	0.3	2.3	-2.2	0.3	2.6
Completed	45,696	90.8%	28.9%	100.0%	100.0%	100.0%
Cross Border	45,696	24.5%	43.0%	0.0%	0.0%	100.0%
Deal Value (in million \$)	45,696	648	3,048	60	157	1,073
Deal Value (log)	45,696	5.4	1.2	4.1	5.1	7.0
Defense	45,696	3.6%	18.7%	0.0%	0.0%	0.0%
Hostile	45,696	3.5%	18.5%	0.0%	0.0%	0.0%
Multiple Bidders	45,696	5.3%	22.4%	0.0%	0.0%	0.0%
Offer Premium	9,906	33.8%	30.0%	2.8%	28.6%	72.2%
Public Bidder	45,696	57.5%	49.4%	0.0%	100.0%	100.0%
Public Target	45,696	29.6%	45.6%	0.0%	0.0%	100.0%
Productivity Gap (in thous. \$)	7,539	12.6	401.4	-247.0	3.9	255.3
Same Industry	45,696	41.2%	49.2%	0.0%	0.0%	100.0%
Stock Payment	30,569	17.0%	37.6%	0.0%	0.0%	100.0%
Target Market Value (in million \$)	12,591	1,133	4,169	45	196	2,158
Target Market Value (log)	12,591	5.5	1.5	3.8	5.3	7.7
Toehold	45,696	1.5%	7.1%	0.0%	0.0%	0.0%
<i>Industry-level Variables</i>						
Post Merger Workforce Reduction	559	-15.8%	37.3%	-51.7%	-8.1%	15.2%
Growth	4,437	8.1%	11.5%	-4.1%	7.1%	22.0%
<i>Country-level Variables</i>						
Corporate Tax Rate	460	37.0%	8.7%	28.0%	35.0%	50.0%
Creditors Rights	460	2.1	1.1	1.0	2.0	3.0
GDP Growth	460	2.7%	1.7%	0.5%	2.8%	5.0%
GDP per Capita (in thous. \$)	460	25.8	11.2	12.6	24.4	39.7
Gini	460	31.6	4.6	26.0	31.4	37.0
Left Government	460	40.9%	49.2%	0.0%	0.0%	100.0%
Proportionality	460	1.7	1.2	0	2	3
Right Government	460	46.3%	49.9%	0.0%	0.0%	100.0%
Union Density	460	36.8%	20.3%	15.1%	31.3%	74.0%

Table 2***M&A Activity by Country***

The table reports statistics on M&A activity by country. The sample includes 45,696 M&A deals from SDC over the 1985-2007 period. To be included in the sample, the target company must be located in one of the 21 OECD countries considered in our study, the deal value must be higher than \$50M, and the stake sought by the bidding firm must be greater than 50%. All variables are defined in Appendix B.

Country	M&A Activity		Combined CAR (%)		Premium (%)	
	Number	Volume (Bn\$)	Count	Mean	Count	Mean
Australia	1,897	768	278	3.9%	418	25.3%
Austria	126	61	9	2.6%	12	32.9%
Belgium	261	172	23	3.2%	29	31.6%
Canada	2,492	1,247	471	1.7%	537	30.4%
Denmark	284	125	25	4.6%	55	28.8%
Finland	287	103	16	6.0%	35	26.2%
France	1,583	1,112	123	1.4%	223	21.7%
Germany	1,410	1,122	71	1.3%	84	24.0%
Greece	84	46	27	2.4%	26	11.7%
Ireland-Rep	196	75	12	2.3%	31	39.2%
Italy	1,095	882	24	-0.2%	94	20.1%
Japan	1,066	781	227	1.7%	299	16.6%
Netherlands	730	746	60	3.8%	98	32.2%
New Zealand	269	86	16	6.3%	38	21.0%
Norway	414	196	77	3.3%	108	33.1%
Portugal	161	106	13	4.2%	9	25.2%
Spain	801	492	45	3.2%	67	19.4%
Sweden	834	414	77	4.3%	151	30.7%
Switzerland	344	388	39	3.4%	54	24.2%
United Kingdom	6,074	3,813	707	3.7%	1,252	37.2%
United States	25,288	16,855	4,789	2.2%	6,286	36.2%
Total	45,696	29,590	7,129	2.4%	9,906	33.8%

Table 3

Industry-Level M&A Activity and EPL Reforms

This table presents difference-in-differences estimates of the effect of EPL reforms on the volume (Columns 1 to 3) and the number (Columns 4 to 6) of M&A deals at the industry-level. Industries are the 13 macro industries defined by SDC. Country control variables include *GDP per Capita*, *GDP Growth*, and *Creditor Rights Index* dummies. $EPL^{y-(+i)}$ is a dummy equal to one if the year is the i^{th} year before (after) the reform, and zero otherwise (y^{++} denotes year +4 and beyond). All variables are defined in Appendix B. The specification "WLS" is weighted least squares where the weight is the average number of listed firms in the country over the pre-treatment period. Standard errors are adjusted for heteroskedasticity and double-clustered by industry-country and industry-year. t-statistics are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: M&A Activity by Industry						
Measure of M&A activity WLS	Deal Volume in M\$ (log)			Deal Number (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
EPL	-0.27*** (-3.33)			-0.14*** (-3.73)		
EPL ^{y-2}		0.01 (0.06)			-0.05 (-0.64)	
EPL ^{y-1}		0.06 (0.34)			0.04 (0.49)	
EPL ^{y+0}		-0.17 (-1.35)			-0.03 (-0.48)	
EPL ^{y+1}		-0.43*** (-2.93)			-0.26*** (-3.26)	
EPL ^{y+2}		-0.45*** (-3.19)			-0.25*** (-3.59)	
EPL ^{y+3}		-0.37** (-2.38)			-0.24*** (-3.47)	
EPL ^{y++}		-0.22** (-2.25)			-0.13*** (-2.63)	
EPL ^{Positive}			-0.25** (-1.98)			-0.23*** (-3.72)
EPL ^{Negative}			0.29*** (2.68)			0.04 (0.90)
Country Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	3,646	3,646	3,646	3,646	3,646	3,646

Table 4

Combined CAR and EPL Reforms

This table presents difference-in-differences estimates of the effect of EPL reforms on the combined CAR [-3,+3] expressed in percentage points. Industries are the 13 macro industries defined by SDC. Country control variables include *GDP per Capita*, *GDP Growth*, and *Creditor Rights Index* dummies. Control variables for target characteristics include *Target Market Value* and *Defense*. Control variables for bidder characteristics include *Toehold*. Control variables for deal characteristics include *Same Industry*, *Cross Border*, *Stock Payment*, *Multiple Bidders* and *Hostile*. $EPL^{y-(+)i}$ is a dummy equal to one if the year is the i^{th} year before (after) the reform, and zero otherwise ($^{y++}$ denotes year +4 and beyond). All variables are defined in Appendix B. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year. t-statistics are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Combined CAR [-3,+3] (in percentage points)				
OLS	(1)	(2)	(3)	(4)
EPL	-1.99*** (-4.20)			-2.06*** (-4.37)
EPL ^{y-2}		-0.74 (-1.22)		
EPL ^{y-1}		0.74 (0.94)		
EPL ^{y+0}		-2.59* (-1.77)		
EPL ^{y+1}		-2.13** (-2.01)		
EPL ^{y+2}		-2.04* (-1.71)		
EPL ^{y+3}		-2.01** (-2.12)		
EPL ^{y++}		-1.76*** (-2.92)		
EPL ^{Positive}			-2.50*** (-4.29)	
EPL ^{Negative}			1.62*** (3.43)	
Country Controls	Yes	Yes	Yes	Yes
Target Controls				Yes
Bidder Controls				Yes
Deal Controls				Yes
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes
N	7,129	7,129	7,129	7,129

Table 5

Combined CAR and EPL Reforms – Triple Differences Analysis

This table presents the results of a triple-differences analysis of the effect of EPL reforms on the combined CAR conditional on deal and industry characteristics. Four cross-sectional dimensions are examined: the degree of geographical (same country) and business (same SIC4) overlap between the target and the bidder (Columns 1 and 2), the extent to which the target company has excess capacity relative to the acquiring firm (Columns 3 and 4), the average post-merger workforce reduction by industry (Columns 5 and 6), and the type of deal: consolidation-type vs. growth-type (Columns 7 and 8). *Business Overlap* is a dummy equal to one if the target and the bidder are located in the same country and operate in the same industry, and zero otherwise. *Productivity Gap* is the difference between the productivity of the bidder and the productivity of the target. *Industry Post-Merger Restructuring* is a time-invariant variable equal to the average workforce reduction one year after the merger is completed by SIC4. *Industry Growth* is the weighted average growth in revenues over the next three years by SIC4. Country control variables include *GDP per Capita*, *GDP Growth*, and *Creditor Rights Index* dummies. All variables measuring a cross-sectional contrast (*Business Overlap*, *Productivity Gap*, *Post-Merger Restructuring*, and *Growth*) are interacted with the year fixed effects and the country fixed effects. Base line effects are omitted when absorbed by the fixed effects. All variables are defined in Appendix B. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year. t-statistics are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Combined CAR [-3,+3] (in percentage points)								
Cross-sectional Contrasts by	Deal Characteristics				Industry Characteristics			
	Business Overlap		Productivity Gap		Post-Merger Workforce Reduction		Growth	
OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EPL x Business Overlap	-3.21*** (-2.84)	-3.53** (-2.50)						
EPL x Productivity Gap			-2.47** (-2.79)	-2.69*** (-4.32)				
EPL x Post-Merger Restructuring					-6.64*** (-3.90)	-6.64*** (-3.24)		
EPL x Growth							12.10** (2.17)	17.63** (2.80)
EPL	-0.87** (-2.55)		-1.15*** (-4.63)		-2.55*** (-4.00)		-2.44** (-2.70)	
Country x Year Fixed Effects		Yes	Yes	Yes		Yes		Yes
Year dummies (Interacted)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country dummies (Interacted)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country controls	Yes		Yes		Yes		Yes	
N	7,129	7,129	5,674	5,674	6,958	6,958	5,467	5,467

Table 6

Offer Premium and EPL Reforms

This table presents difference-in-differences estimates of the effect of EPL reforms on the offer premium expressed in percentage points. Country control variables include *GDP per Capita*, *GDP Growth*, and *Creditor Rights Index* dummies. Control variables for target characteristics include *Target Market Value* and *Defense*. Control variables for bidder characteristics include *Toehold* and *Public Bidder*. Control variables for deal characteristics include *Same Industry*, *Cross Border*, *Stock Payment*, *Multiple Bidders* and *Hostile*. $EPL^{y-(+)i}$ is a dummy equal to one if the year is the i^{th} year before (after) the reform, and zero otherwise (y^{++} denotes year +4 and beyond). All variables are defined in Appendix B. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year. t-statistics are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Offer Premium (in percentage points)				
OLS	(1)	(2)	(3)	(4)
EPL	-10.95*** (-3.62)			-11.24*** (-4.52)
EPL ^{y-2}		-4.79 (-0.96)		
EPL ^{y-1}		4.61 (0.61)		
EPL ^{y+0}		-4.34 (-0.85)		
EPL ^{y+1}		-7.16** (-2.43)		
EPL ^{y+2}		-9.23** (-2.26)		
EPL ^{y+3}		-10.95* (-1.93)		
EPL ^{y++}		-13.04*** (-2.93)		
EPL ^{Positive}			-14.81*** (-3.77)	
EPL ^{Negative}			8.00*** (3.13)	
Country Controls	Yes	Yes	Yes	Yes
Target Controls				Yes
Bidder Controls				Yes
Deal Controls				Yes
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes
N	9,906	9,906	9,906	9,906

Table 7

The Division of Takeover Gains and EPL Reforms

This table presents difference-in-differences estimates of the effect of EPL reforms on the size and division of takeover gains to bidder and target firms. In Panel A, the dependent variable is the cumulative abnormal return of the combined firm, target firm, and bidding firm. In Panel B, the dependent variable is the dollar value of expected gains scaled by the combined market value. In Panel C, Column 1, the dependent variable is the amount of dollar expected synergies relative to the dollar premium paid. In Panel C, Column 2, the dependent variable is a dummy equal to one if the bidder CAR is negative, and zero otherwise. Country controls include *GDP per Capita*, *GDP Growth*, and *Creditor Rights Index* dummies. Target, bidder and deal controls include *Target Market Value*, *Defense*, *Toehold*, *Same Industry*, *Cross Border*, *Stock Payment*, *Multiple Bidders*, *Hostile*, *Public Target*, and *Public Bidder*. The specification "Clogit" is conditional logit (marginal effects are reported). Standard errors are adjusted for heteroskedasticity and double-clustered by country and year. t-statistics are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A						
Dependent Variable	Combined CAR [-3,+3]		Target CAR [-3,+3]		Bidder CAR [-3,+3]	
	(1)	(2)	(3)	(4)	(5)	(6)
OLS						
EPL	-1.99*** (-4.20)	-2.06*** (-4.37)	-4.06** (-2.37)	-4.00*** (-2.95)	-0.45* (-1.86)	-1.16*** (-2.86)
Country Controls	Yes	Yes	Yes	Yes	Yes	Yes
Target, Bidder & Deal Controls		Yes		Yes		Yes
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	7,129	7,129	11,949	11,949	23,806	7,129
Panel B						
Dependent Variable: Dollar Values / Combined Market Value at t-4						
OLS	Combined	Target	Bidder	Premium Paid		
	(1)	(2)	(3)	(4)		
EPL	-2.06*** (-4.37)	-1.08*** (-3.31)	-0.97*** (-3.69)	-1.31** (-2.75)		
Country Controls	Yes	Yes	Yes	Yes		
Target, Bidder & Deal Controls	Yes	Yes	Yes	Yes		
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes		
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes		
N	7,129	7,129	7,129	5,826		
Panel C						
Dependent Variable	Combined CAR in Dollar Value / Premium Paid in Dollar Value		Negative Bidder CAR			
	OLS (1)		Clogit - Margins (2)			
EPL	-0.30*** (-3.17)		0.09*** (3.11)			
Country Controls	Yes		Yes			
Target, Bidder & Deal Controls	Yes		Yes			
Industry x Year Fixed Effects	Yes		Yes			
Industry x Country Fixed Effects	Yes		Yes			
N	5,516		7,129			

Table 8

Deal Outcomes, EPL Reforms, and Governance Quality

This table shows the effect of EPL reforms on takeover outcomes conditional on the quality of corporate governance in the bidding firm's country. In Column 1, the dependent variable is the bidding firm CAR (in percentage points). In Column 2, the dependent variable is the offer premium (in percentage points). In Column 3, the dependent variable is the combined CAR (in percentage points). *Anti-Self* is the bidding firm's country anti-self-dealing index proposed by Djankov et al. (2008). Controls include country control variables (*GDP per Capita*, *GDP Growth*, *Creditor Rights Index* dummies) and control variables for target, bidder and deal characteristics (*Target Market Value*, *Defense*, *Toehold*, *Same Industry*, *Cross Border*, *Stock Payment*, *Multiple Bidders*, *Hostile*). Standard errors are adjusted for heteroskedasticity and double-clustered by country and year. t-statistics are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	Bidder CAR [-3,+3] (1)	Offer Premium (%) (2)	Combined CAR [-3,+3] (3)
OLS			
EPL	-2.14*** (-3.81)	-6.46** (-2.12)	-2.21** (-2.70)
EPL x Anti-Self	1.70*** (3.84)	-8.49*** (-3.80)	0.28 (0.29)
Anti-Self	-1.08 (-1.59)	-2.01 (-0.80)	0.57 (0.40)
Country Controls	Yes	Yes	Yes
Target Controls	Yes	Yes	Yes
Bidder Controls	Yes	Yes	Yes
Deal Controls	Yes	Yes	Yes
Industry x Year Fixed Effects	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes
N	7,044	9,728	7,044

Table 9

Post-Merger Workforce Restructuring and EPL Reforms

This table presents estimates of the effect of EPL reforms on the combined number of employees following mergers. All deals are followed over a five-year window around the completion of the transaction, which allows to identify the dynamics of the total number of employees at the bidder and the target in the years surrounding the merger. The dependent variable is the log-number of employees of the bidder and the target in year $y+i$, where y is the year of completion of the merger, and $+i$ ($-i$) is the number of years after (before) the merger. *Post_merger* is a dummy equal to one if i is positive, and zero otherwise. *Post_merger* ^{$y+i$} is a dummy equal to 1 if the year is the i^{th} year after the completion of the merger. Country controls include *GDP per Capita*, *GDP Growth*, and *Creditor Rights Index* dummies. Base line effects are omitted from the regression when absorbed by the fixed effects. Industries are the 13 macro industries defined by SDC. All variables are defined in Appendix B. Standard errors are adjusted for heteroskedasticity and double-clustered by deal and by year. t-statistics are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: Combined Number of Employees (Log)				
OLS	(1)	(2)	(3)	(4)
Post Merger	-0.06*** (-4.87)	-0.09*** (-6.82)		
EPLx Post Merger		0.05*** (4.12)		0.07** (2.06)
Post Merger ^{y-1}			0.01 (0.92)	
Post Merger ^{y-0}			-0.09*** (-4.58)	
Post Merger ^{y+1}			-0.09*** (-3.11)	
Post Merger ^{y+2}			-0.12*** (-3.33)	
EPL x Post Merger ^{y-1}			0.01 (1.00)	
EPL x Post Merger ^{y-0}			0.06*** (2.86)	
EPL x Post Merger ^{y+1}			0.06** (2.28)	
EPL x Post Merger ^{y+2}			0.06* (1.79)	
Country Controls	Yes	Yes	Yes	Yes
Deal Fixed Effects	Yes	Yes	Yes	Yes
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes
EPL x Industry x Country Fixed Effects		Yes	Yes	Yes
EPL x Industry x Year Fixed Effects		Yes	Yes	Yes
Post Merger x Industry x Country Fixed Effects				Yes
Post Merger x Industry x Year Fixed Effects				Yes
N	24,775	24,775	24,775	24,775
# Deals	5,053	5,053	5,053	5,053

Figure 1

M&A Volume / GDP vs. OECD EPL Index

This figure presents the average volume of M&A deals (scaled by total GDP) by country over the 1985-2007 period relative to the average EPL index (as published by the OECD) over the same period. The analysis is based on all deals reported in SDC. The slope and the R-squared correspond to a regression of the mean M&A volume to GDP on the mean EPL index across countries. The gray-shaded areas represent the 90% confidence interval for the fitted value from this regression.

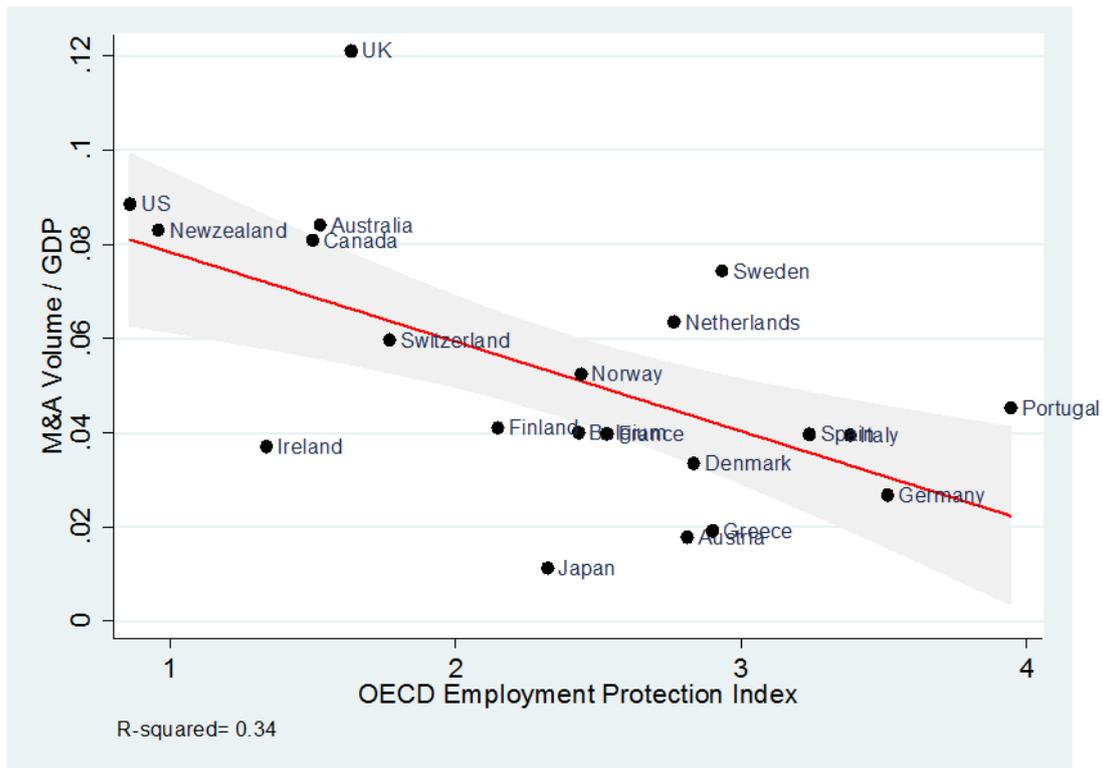


Figure 2

Offer Premium vs. OECD EPL Index

This figure presents the offer premium by country (median over the 1985-2007 period) relative to the average EPL index (as published by the OECD) over the same period. The analysis is based on all deals reported in SDC. The slope and the R-squared correspond to a regression of the median offer premium on the mean EPL index across countries. The gray-shaded areas represent the 90% confidence interval for the fitted value from this regression.

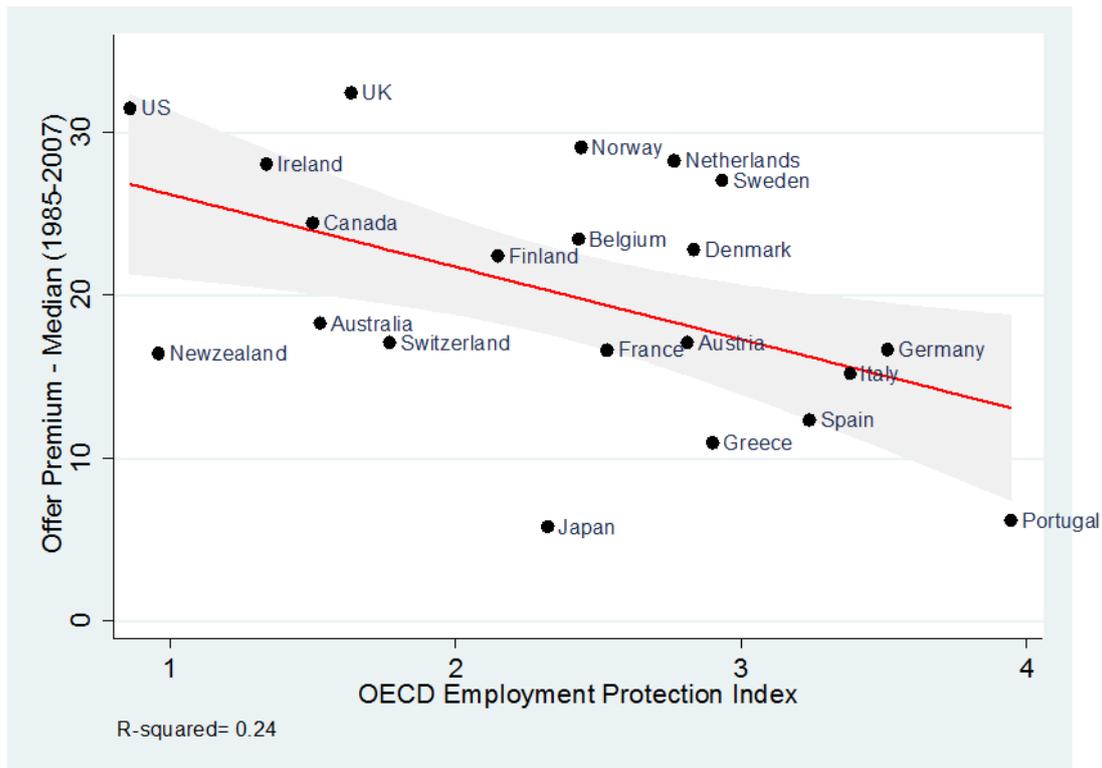


Figure 3

Major EPL Reforms over the 1985-2007 period

This graph shows the timing of the twenty-one major employment protection reforms occurred in the OECD countries over the 1985-2007 period. Of these twenty-one major reforms, nine have tightened employment protection (*More Protection*) and twelve have reduced employment protection (*Less Protection*). Data are from Simintzi, Vig and Volpin (2015)

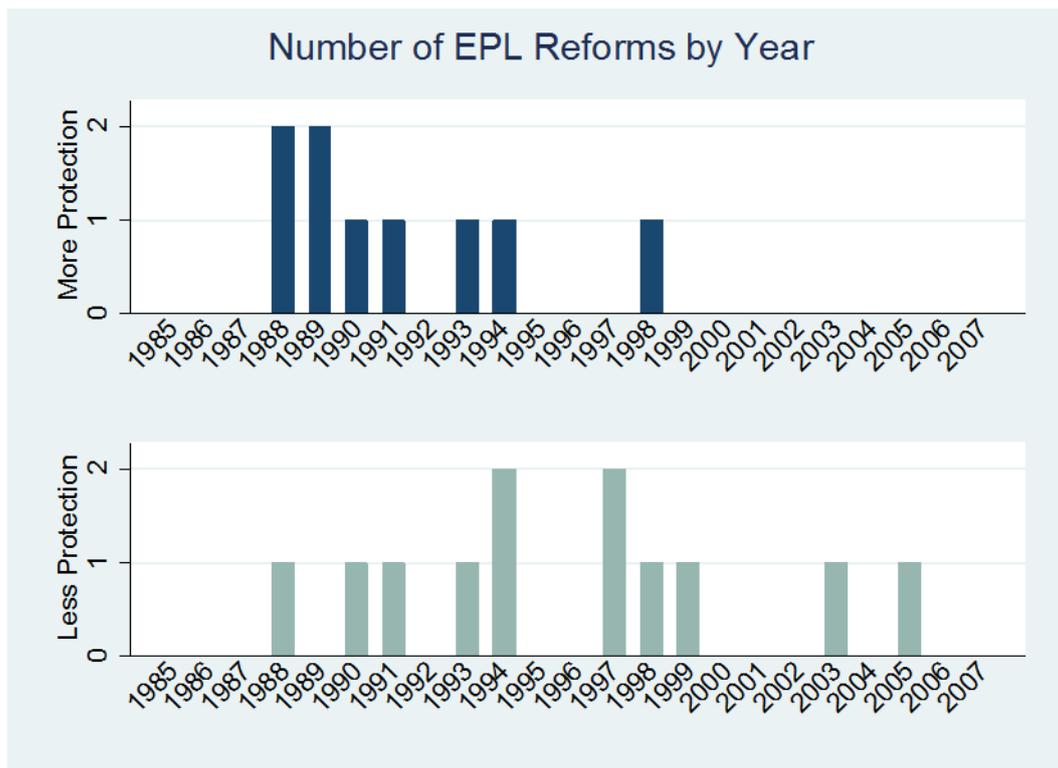


Figure 4

Industry-Level Volume of M&A Deals after EPL reforms

This figure shows the average effect of the EPL reforms on the volume of M&A deals at the industry-level in a country affected by the reform relative to the same industry in unaffected countries. The graph plots the regression coefficients from Column (2) of Table 3. The horizontal bars represent standard error bounds of the estimate at the 90% confidence interval.

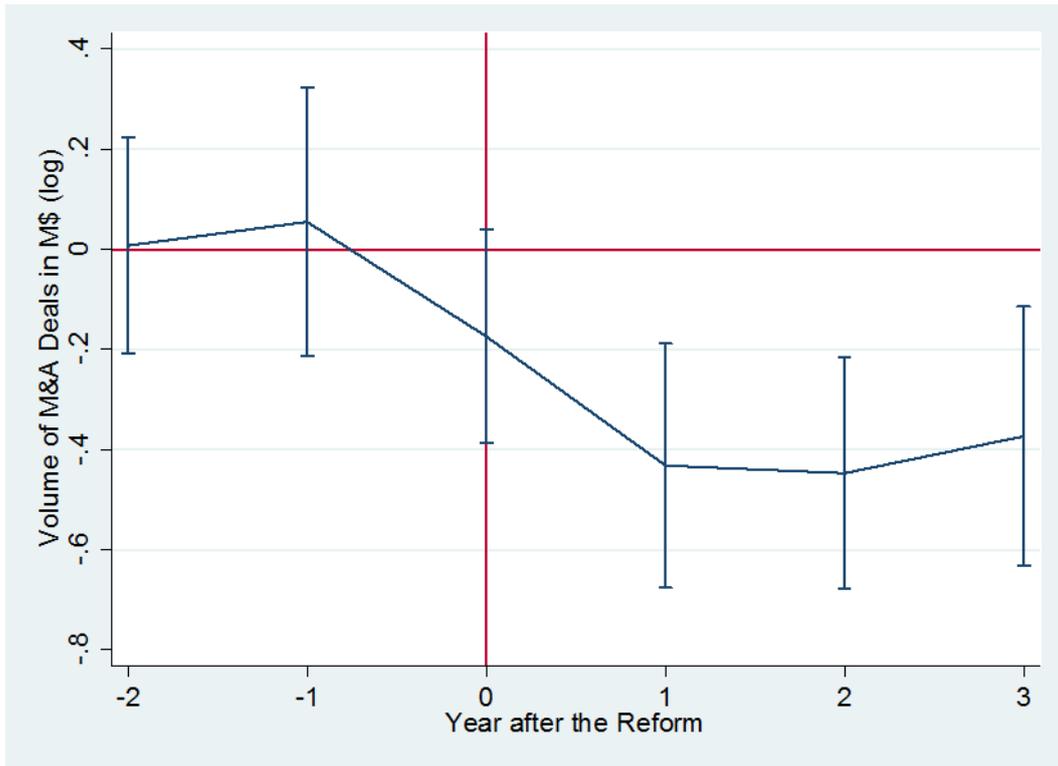


Figure 5

Industry-Level Number of M&A Deals after EPL reforms

This figure shows the average effect of the EPL reforms on the number of M&A deals at the industry-level in a country affected by the reform relative to the same industry in unaffected countries. The graph plots the regression coefficients from Column (5) of Table 3. The horizontal bars represent standard error bounds of the estimate at the 90% confidence interval.

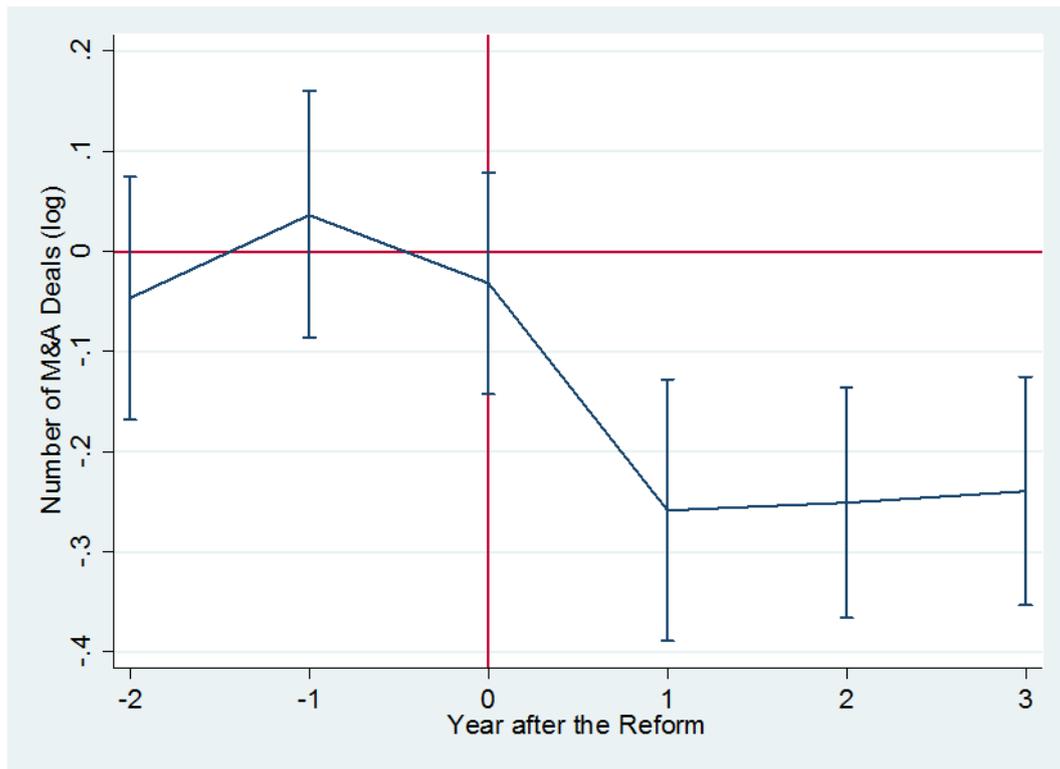


Figure 6

Combined CAR after EPL reforms

This figure shows the average effect of the EPL reforms on takeover gains (Combined CAR) in a given industry in a country affected by the reform relative to the same industry in unaffected countries. The graph plots the regression coefficients from Column (2) of Table 4. The horizontal bars represent standard error bounds of the estimate at the 90% confidence interval.

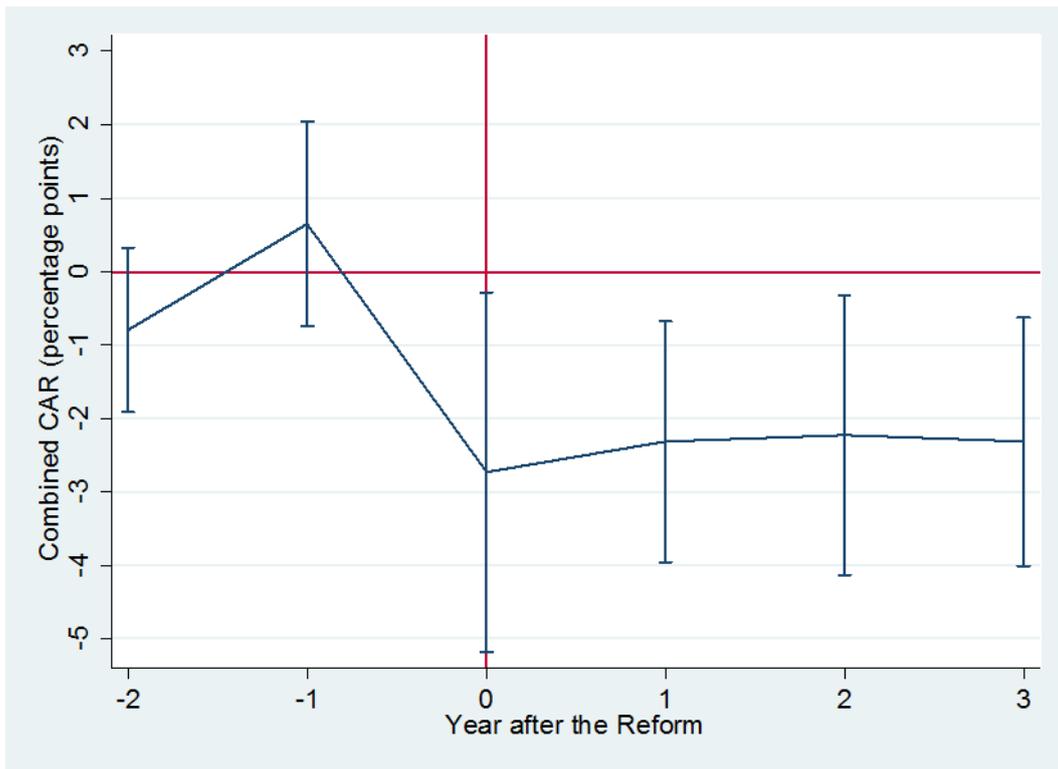


Figure 7

Offer Premium after EPL reforms

This figure shows the average effect of the EPL reforms on offer premiums in a given industry in a country affected by the reform relative to the same industry in unaffected countries. The graph plots the regression coefficients from Column (2) of Table 6. The horizontal bars represent standard error bounds of the estimate at the 90% confidence interval.

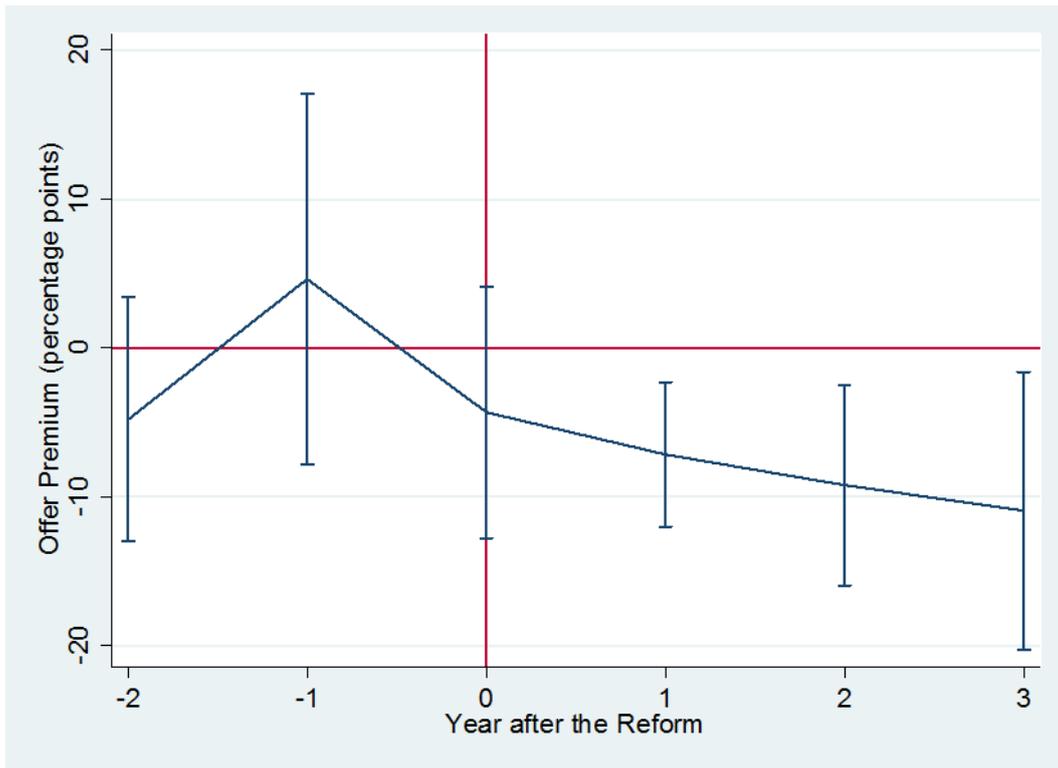
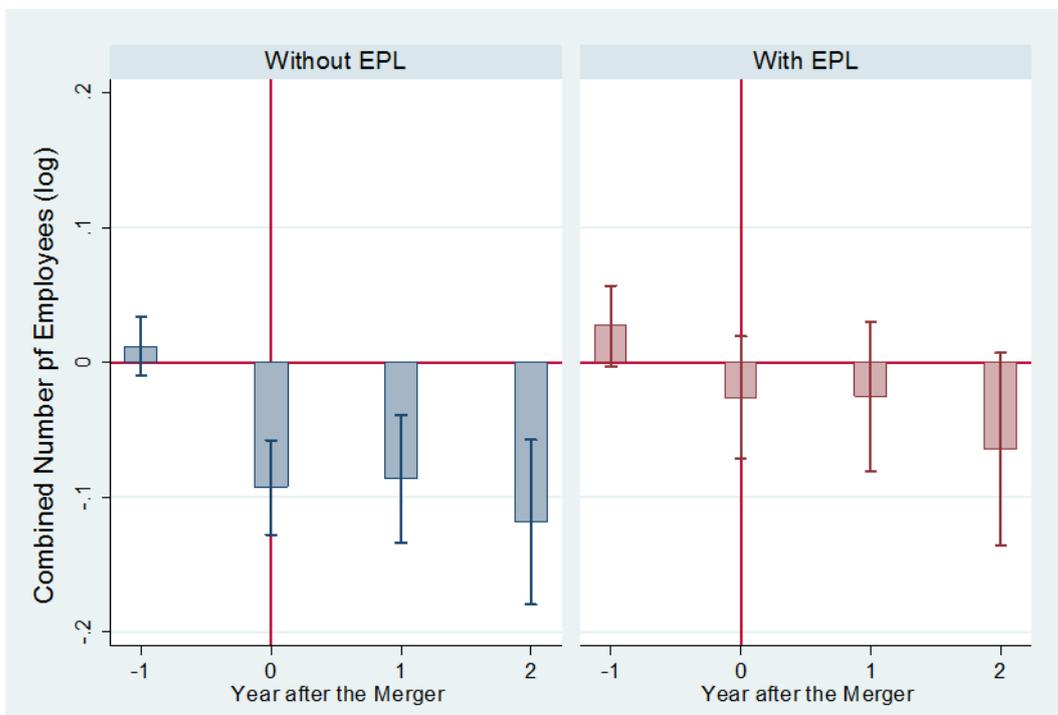


Figure 8

Post-Merger Workforce Restructuring after EPL reforms

This figure shows the effect EPL reforms on the dynamic of the post-merger workforce restructuring. The two graphs plot the regression coefficients from Column 3 of Table 9. The graph on the left-hand side shows the change in the combined number of employees in the years following the completion of the merger before a more protective reform is passed or after the adoption of a reform that reduces employment protection (“*Without EPL*”). The graph on the right-hand side shows the change in the combined number of employees in the years following the completion of the merger after a more protective reform is passed or before the adoption of a less protective reform (“*With EPL*”). The combined number of employees is the total number of employees of the bidder and the target (in log). The horizontal bars represent standard error bounds of the estimate at the 90% confidence interval.



Appendix A - Robustness Tests

This table reports the estimation results of several robustness tests. In Panel A.1, the tests are performed on a larger sample including the post-2008 period. In Panel A.2, the tests are performed excluding U.S. deals. In Panel A.3, the tests are performed using the continuous EPL index defined by the OECD. In Panel A.4, the tests are performed including additional control variables for the political / macro-economic environment at country level. In Panel A.5, the tests are performed excluding countries with no EPL reforms during the sample period. The specification "WLS" is weighted least squares where the weight is the average number of listed firms in the country over the pre-treatment period. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year for deal-level tests, and by industry-country and industry-year for country-level tests. t-statistics are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

<i>A.1 - Including Post-2008 period</i>					
Dependent Variable	Comb CAR [-3;+3]	Offer Premium	Deal Number		Deal Volume
	OLS	OLS	WLS		WLS
EPL	-1.51** (-2.43)	-9.14*** (-3.74)	-0.14*** (-3.58)		-0.27*** (-3.65)
Country Controls	Yes	Yes	Yes		Yes
Target Controls	Yes	Yes			
Bidder Controls	Yes	Yes			
Deal Controls	Yes	Yes			
Industry x Year Fixed Effects	Yes	Yes	Yes		Yes
Industry x Country Fixed Effects	Yes	Yes	Yes		Yes
N	8,634	12,375	4,837		4,837

<i>A.2 - Excluding USA</i>						
Dependent Variable	Comb CAR [-3;+3]	Offer Premium	Deal Number		Deal Volume	
	OLS	OLS	WLS	OLS	WLS	OLS
EPL	-1.59** (-2.80)	-11.14*** (-4.03)	-0.15** (-2.36)	-0.14*** (-3.04)	-0.37*** (-2.58)	-0.35*** (-3.00)
Country Controls	Yes	Yes	Yes	Yes	Yes	Yes
Target Controls	Yes	Yes				
Bidder Controls	Yes	Yes				
Deal Controls	Yes	Yes				
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	2,266	3,577	2,708	2,708	2,708	2,708

<i>A.3 - Using OECD EPL Index</i>					
Dependent Variable	Comb CAR [-3;+3]	Offer Premium	Deal Number		Deal Volume
	OLS	OLS	WLS		WLS
EPL ^{OECD}	-3.22*** (-4.35)	-13.60** (-2.27)	-0.14*** (-2.68)		-0.32*** (-3.05)
Country Controls	Yes	Yes	Yes		Yes
Target Controls	Yes	Yes			
Bidder Controls	Yes	Yes			
Deal Controls	Yes	Yes			
Industry x Year Fixed Effects	Yes	Yes	Yes		Yes
Industry x Country Fixed Effects	Yes	Yes	Yes		Yes
N	7,129	9,906	3,646		3,646

Appendix A (Cont'd)- Robustness Tests

This table reports the estimation results of several robustness tests. In Panel A.1, the tests are performed on a larger sample including the post-2008 period. In Panel A.2, the tests are performed excluding U.S. deals. In Panel A.3, the tests are performed using the continuous EPL index defined by the OECD. In Panel A.4, the tests are performed including additional control variables for the political / macro-economic environment at country level. In Panel A.5, the tests are performed excluding countries with no EPL reforms during the sample period. The specification "WLS" is weighted least squares where the weight is the average number of listed firms in the country over the pre-treatment period. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year for deal-level tests, and by industry-country and industry-year for country-level tests. t-statistics are in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

A.4 - With additional controls for political / macro-economic factors

Dependent Variable	Comb CAR [-3;+3]	Offer Premium	Deal Number	Deal Volume
	OLS	OLS	WLS	WLS
EPL	-2.52*** (-4.65)	-10.37*** (-3.08)	-0.12*** (-3.15)	-0.25*** (-3.17)
Gini	0.1 (0.94)	-0.16 (-0.42)	0.00 (-0.63)	-0.01 (-1.21)
Left Government	-3.79*** (-4.47)	10.52** (2.71)	0.15** (2.07)	-0.1 (-0.67)
Right Government	-3.74*** (-4.03)	11.90*** (3.35)	0.05 (0.68)	-0.31** (-2.12)
Union Density	0.06 (0.34)	0.03 (0.05)	-0.01 (-0.96)	0.00 (-0.22)
Proportionality	-1.77 (-0.76)	-3.45 (-1.48)	-0.09 (-1.53)	-0.30** (-2.29)
Corporate Tax Rate	-10.45 (-1.13)	-50.2 (-0.99)	-0.56 (-1.51)	-0.63 (-0.89)
Country Controls	Yes	Yes	Yes	Yes
Target Controls	Yes	Yes		
Bidder Controls	Yes	Yes		
Deal Controls	Yes	Yes		
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes
N	7,129	9,906	3,646	3,646

A.5 - Excluding countries with no EPL Reforms over the 1985-2007 period

Dependent Variable	Comb CAR [-3;+3]	Offer Premium	Deal Number	Deal Volume
	OLS	OLS	WLS	WLS
EPL	-1.35* (-2.09)	-8.12** (-2.61)	-0.16*** (-3.70)	-0.31*** (-3.59)
Country Controls	Yes	Yes	Yes	Yes
Target Controls	Yes	Yes		
Bidder Controls	Yes	Yes		
Deal Controls	Yes	Yes		
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes
N	5,650	7,690	2,549	2,549

Appendix B - List of Variables (in alphabetical order)

Variable	Definition
<i>Anti-Self</i>	Bidding firm country anti-self-dealing index from Djankov et al. (2008)
<i>Business Overlap</i>	Dummy equal to one if the bidder and the target are located in the same country and operate in the same SIC4 industry, zero otherwise
<i>Bidder Market Value (log)</i>	Natural logarithm of <i>Bidder Market Value (M\$)</i>
<i>Bidder Market Value (M\$)</i>	Bidder market value prior to deal announcement in M\$
<i>CAR Bidder [-3;+3]</i>	Bidder cumulative abnormal return over a seven-day window around the deal announcement. Abnormal returns are calculated using the market model relative to a local equity market index (CRSP for U.S. stocks, the local stock market equity index reported by Datastream for non-U.S. stocks)
<i>CAR Combined [-3;+3]</i>	Weighted average of the target and the bidder cumulative abnormal returns. The weights are the market values of the target and the bidder four days prior to the announcement
<i>CAR Target [-3;+3]</i>	Target cumulative abnormal return over a seven-day window around the deal announcement. Abnormal returns are calculated using the market model relative to a local equity market index (CRSP for U.S. stocks, the local stock market equity index reported by Datastream for non-U.S. stocks)
<i>Combined Gains per Dollar of Premium Offered</i>	Dollar amount of expected synergies over the dollar amount of premium offered. $(CAR\ Combined\ [-3,+3] \times Combined\ Market\ Value) / (Offer\ Premium \times Target\ Market\ Value)$
<i>Combined Number of Employees</i>	Total number of employees of the target and the bidding company (in log)
<i>Completed</i>	Dummy equal to one if the deal was completed, and zero otherwise
<i>Corporate Tax Rate</i>	Official corporate tax rate in the target country
<i>Creditor Rights</i>	Creditor Rights Index as from Djankov, McLiesh, and Shleifer (2007)
<i>Cross Border</i>	Dummy equal to one if the target and the bidder involved have their headquarters located in different countries, and zero otherwise
<i>Deal Value (log)</i>	Natural logarithm of <i>Deal Value (M\$)</i>
<i>Deal Value (M\$)</i>	Deal value in M\$
<i>Defense</i>	Dummy equal to one if a defense mechanism was used by the target, and zero otherwise
<i>EPL</i>	EPL is an indicator variable that increments (decrements) by one whenever a major reform aimed at increasing (reducing) employment protection is adopted during the year. This variable is defined recursively as in Simintzi, Vig and Volpin (2015). In 1985, the EPL score is set to zero for all countries
<i>EPL^{Positive}</i>	$EPL^{Positive}$ is an indicator variable that increments by one whenever a major reform aimed at increasing employment protection is adopted during the year. In 1985, the $EPL^{Positive}$ score is set to zero for all countries
<i>EPL^{Negative}</i>	$EPL^{Negative}$ is an indicator variable that increments by one whenever a major reform aimed at reducing employment protection is adopted during the year. In 1985, the $EPL^{Negative}$ score is set to zero for all countries
<i>GDP Growth</i>	GDP growth as reported by the IMF
<i>GDP Per Capita</i>	Natural logarithm of the GDP per capita as reported by the IMF
<i>Gini</i>	Gini coefficient by country as reported by the OECD
<i>Growth</i>	Weighted average growth in revenues over the next three years by SIC4
<i>Hostile</i>	Dummy equal to one if the initial bid was hostile, and zero otherwise
<i>Left Government</i>	Dummy equal to one if the governing party is a left-wing party, and zero otherwise
<i>Multiple Bidders</i>	Dummy equal to one if more than one bidder is involved, and zero otherwise
<i>Offer Premium</i>	Offer price relative to target stock price four weeks prior to deal announcement as reported by SDC

<i>Post Merger Workforce Reduction</i>	Average post-merger change in the number of employees by SIC4 multiplied by -1. The change in the number of employees is the number of employees at the bidding firm one year after the completion of the deal relative to the combined number of employees at the bidder and the target one year prior
<i>Proportionality</i>	Proportionality index measuring the degree of proportionality of the electoral system in the country
<i>Public Bidder</i>	Dummy equal to one if the bidder is a public company, and zero otherwise
<i>Public Target</i>	Dummy equal to one if the target is a public company, and zero otherwise
<i>Productivity Gap</i>	Bidder productivity ratio minus target productivity ratio one year prior to deal announcement. Productivity ratio is defined as total revenues in thousands of dollars divided by the total number of employees.
<i>Right Government</i>	Dummy equal to one if the governing party is a right-wing party, and zero otherwise
<i>Same Industry</i>	Dummy equal to one if the bidder and the target and both operate in one of the 89 mid-industries defined by SDC
<i>Stock Payment</i>	Dummy equal to one if 100% of the proposed payment is in stock, and zero otherwise
<i>Target Market Value (log)</i>	Natural logarithm of the <i>Target Market Value (M\$)</i>
<i>Target Market Value (M\$)</i>	Target market value prior to deal announcement in M\$
<i>Toehold</i>	Percentage ownership of the target by the bidder prior to initiating the bid
<i>Union Density</i>	Trade union density reported by the OECD. Percentage of employees who are members of a trade-union
