Employment Protection and Takeovers

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Abstract

Labor restructuring is a key driver of takeovers and the associated synergy gains worldwide. In a difference-in-differences research design, we show that major increases in employment protection reduce takeover activity by 14-27% and the combined firm gains (synergies) by over half. Consistent with the labor channel behind these effects, deals with greater potential for workforce restructuring show a greater reduction in volume, number, and synergies. The reforms do impede layoffs, and the associated wage costs match the magnitude of synergy losses. Offer prices are not fully adjusted, with both bidders and targets exhibiting lower returns following the reforms.

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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)). Eliminating overlapping or inefficient operations is often the primary channel through which such gains are obtained. For instance, Maksimovic, Phillips, and Prabhala (2011) report that 19% of acquired plants are closed, and a further 27% are sold off in the three years following the merger. Restructuring the workforce, and, in particular, laying off redundant white- and blue-collar workers, should come hand in hand with such organizational changes. Anecdotally, 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]

Despite these anecdotes and the intuitive relevance of labor force issues to mergers and acquisitions (M&A), there is little 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 one does not have good ex-ante measures of the potential for workforce restructuring. For instance, actual ex-post changes in employment following mergers suffer from omitted variables and measurement problems (e.g., can capture changes in the investment opportunity set or changes in strategy independent of the deal). Our paper fills this void and provides the first systematic evidence on the link between labor restructuring and takeovers. Specifically, we exploit cross-country and time-series variation

[1] "In Drug Mergers, There's One Sure Bet: The Layoffs", *The Wall Street Journal*, 29 April 2014.

in employment protection to evaluate the importance of workforce restructuring as a driver of takeover activity and related economic outcomes.

Intuitively, the potential for labor force restructuring is expected to affect takeover dynamics in several ways. First, if workforce restructuring represents an important consideration in takeovers, then fewer takeover attempts are likely to materialize when employment is highly protected and redundancies are costlier.[2] Second, where bids are made, the rigidity of labor regulation is expected to reduce the synergy gains from mergers and acquisitions. If this is the case, offer premiums and bidder and target performance may also be affected.

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. 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 two percentage points, relative to the unconditional combined firm CAR of 2.4%. In an efficient

[2] Note that this can be either a direct consideration of the deal, i.e. pure workforce optimization, or an indirect consideration, whereby the combined firm may be consolidating or closing down a plant or other overlapping facilities as a result of the deal and needs to lay off the associated workers.

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 as much as much as 80% of the typical takeover efficiency gain immediately priced by the market (50% when compared to the average combined firm CAR in the pre-treatment period for the group of countries experiencing reforms).

We further explore how bidders and targets respond to these changes in the availability of synergy gains and examine offer premiums, target returns, and bidder returns. We find that, in response to tighter employment protection, offer premiums are reduced by about 11 percentage points, or roughly a third of the unconditional average of 33.8%. While the target CARs decline in line with the premiums results, we find that 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: both bidder and target shareholders share in the decline in 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. For instance, 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. We also demonstrate that the reforms tightening employment protection are not passed in response to deteriorating macroeconomic fundamentals that could potentially have a negative effect on the takeover market.

To further address the omitted variables concern we exploit cross-sectional differences in terms of potential for workforce restructuring and establish heterogeneous treatment effects that are consistent with the labor channel. We show that increases in employment

protection reduce the incidence of bidder-target combinations with a high degree of business overlap (domestic intra-industry deals) and lead to an incremental 3 percentage point reduction in the combined CAR 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 activity and synergy gains for targets with larger productivity gaps relative to their industry, in sectors with greater average workforce turnover following mergers, and in mature sectors. Importantly, these latter tests on combined firm CARs include country-year fixed effects, which eliminates any time-varying heterogeneity between the reforming and non-reforming countries. Moreover, any alternative explanation for our results would have to predict the same exact cross-sectional effects along these four different dimensions.[3]

Finally, to cement the labor force channel interpretation of the documented effects, we study the effect of mergers on employment, and the effect of employment protection reforms on post-merger workforce restructuring. We show that mergers do reduce combined firm employment, but stronger employment protection reforms are associated with a smaller reduction in the combined firm workforce following mergers. The difference-in-differences estimate of the effect of employment protection reforms on post-merger workforce restructuring suggests that such reforms preserve about 7% of employment at the combined firm on average. Moreover, we are able to reconcile the present value of wage savings associated with this estimated employment effect with the magnitude of the estimated changes in synergies, which further reinforces the labor channel interpretation of our main results.

[3] 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.

The results we document survive a battery of further robustness checks. The effect of employment protection 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 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.

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. John, Knyazeva and Knyazeva (2015) and Tian and Wang (2015) study the effects of employee rights and unionization on takeovers in the U.S. We contribute to this literature by providing the first evidence on the effects of national 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.[4]

[4] We review related literature in more detail in the following section.

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 empirical results and discuss their implications in Section IV. Finally, Section V concludes the paper with a summary of our findings and suggestions for future research.

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 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 effects following takeovers rely on relatively small samples and find little support for the idea that workforce restructuring motivates deals. 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. Rosett (1990) shows that union wage concessions can hardly explain the magnitude of observed premiums in a sample of 258 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. Focusing on private equity targets, Davis et al. (2014) find modest net job losses of 3% within 2 years and 6% within five years, relative to non-private-equity establishments; however there is large turnover in the form of both firing

and hiring. On the other hand, Ouimet and Zarutskie (2015) argue and find that acquisitions can also be used as a means of efficiently *increasing* the labor force.

Recent studies of labor protection and takeovers include Tian and Wang (2015), John, Knyazeva, and Knyazeva (2015), and Alimov (2015). Tian and Wang (2015) focus on the unionization status of U.S. target firms. 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 unionization does not affect the overall efficiency gains from 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 weaker employee protection states, defined as states that have passed the right-to-work statutes limiting union power, experience 0.5% higher announcement returns. Combined firm returns are also increased by about 0.8%, suggesting there are higher synergy gains in deals involving bidders from weak employee right states. However, they further show that these effects cannot be explained by workforce reductions alone. They argue that stronger employee rights results in greater employee-shareholder agency conflicts, manifesting themselves in poorer deal selection and subsequent integration. Furthermore, they show that the *target* firm state employee rights do not matter.

Finally, 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.

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 various economies differently – the recovery period is characterized by diverging economic fundamentals – the parallel trends assumption during this period is likely to be violated.[5]

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. We provide more background on these labor reforms in Section III.C. 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

[5] Nevertheless, we have experimented with extending the sample period to 2013. The results, reported in Appendix B, 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 C.

[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 and modified by Allard (2005), over the same period.[6] The graph clearly displays a strong negative correlation between these two

[6] 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.

variables: on the one hand, M&A 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]

This correlation is 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. Of course, the simple cross-country correlation is not evidence of a causal relationship and can reflect other relevant differences across countries. Nevertheless, this association is consistent with our predictions. Moreover, it also suggests that labor market rigidity can help explain the substantial differences in the levels of takeover activity 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 variation 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. Finally, we will show that the reforms are not systematically preceded by deteriorating macroeconomic fundamentals, such as economic growth and development, unemployment, consumption growth, corporate profitability, stock market return, and sovereign yield spreads.

Another potential 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 and other political economy controls. We will also explore the dynamics of the effect on M&A outcomes (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, Vig, and Volpin (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,[7] 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. Use of the discrete reform index as opposed to the continuous index such as that proposed by Allard (2005) is justified on two grounds: i) econometric (maximizing the signal-to-noise ratio), and ii) expositional (easing the interpretation of economic magnitudes). The correlation between our reform index and the Allard (2005) EPL index (in changes) is 0.51, and we will show that our results are robust to the use of this alternative index.

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).

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.

[7] We will show that our results are unchanged when we exclude reforms affecting only temporary workers.

Our 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.

A primary concern in difference-in-differences analyses is the possibility that another omitted factor that is relevant for the outcome variable of interest changes contemporaneously with the treatment indicator. Note that this concern is somewhat alleviated in our setting given that our identification strategy relies on 21 different shocks to labor market rigidity. That is, one would have to find an unobserved contemporaneous change that *systematically* accompanies labor market reforms across different countries and over time. In an attempt to alleviate these concerns even further, we examine the dynamics of various macroeconomic factors in the two years preceding the reforms. Specifically, we regress country-level GDP growth, GDP per capita, unemployment, consumption growth, average profitability (return on assets) of the corporate sector, stock market index, and sovereign yield spreads on an indicator $EPL^{(-n)}$ which takes the value of 1(-1) if a country will tighten (loosen) employment protection in *n* years. If the reforms are systematically passed in response to certain economic conditions, the coefficient on EPL^{-n} will be statistically different from zero. We use both $n=2$ and $n=1$ to capture any long-run effects. The results of this analysis, reported in Appendix A, indicate that none of these macroeconomic variables

exhibit any systematic patterns in the run-up to employment protection reforms. With such reassurance in mind, we proceed to our main analysis.

For the country-level tests (deal numbers and volumes), we perform least squares 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 target firm 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 the regressions are weighted 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.

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 target firm 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).[8]

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 that had 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 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 weighted regressions, where the importance weights are the average number of listed firms in that country measured over the 1985-1987 period – i.e., before the first reform in our sample.[9] The results are reported in Table 3.

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

[9] 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.

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. 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 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) 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: it is possible that labor restructuring is a more important consideration in large deals, so that deal volumes respond to reductions in employment protection while deal numbers do not.

[Please Insert Table 3 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. 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%, and by 52% relative to the average combined firm CAR of 4.0% in the pre-treatment period for the group of countries experiencing. 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.

[Please Insert Table 4 about Here]

VI.C. Deal-Level Tests: Cross-Sectional Effects

Even though we have shown that employment protection reforms are not passed in response to changes in macroeconomic conditions, a potential concern with our analysis so far is that some *other* omitted variable (for instance, a contemporaneous reform in areas other than employment protection) may be behind both the change in deal activity levels, 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 target's abnormal labor productivity as the difference between its sales-per-employee ratio and industry average. This measure captures (inversely) 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 or workforce reduction at the industry-level as the sample average workforce reduction one-year after the merger. We expect a greater decline in deal activity levels and takeover synergies for industries with generally higher levels of post-merger employee turnover (note that this measure uses ex-post information, so should be interpreted with caution). 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 tests for deal activity levels. We regress our proxies for deals motivated by labor restructuring on the *EPL* indicator, a set of fixed effects, and bidder, target, and deal level controls using linear probability models. Column (1) uses *Business Overlap* dummy as our first proxy. The coefficient on the *EPL* indicator is negative and significant at the 5% level, suggesting that there are fewer deals with business overlap following the reforms tightening employment protection. This is consistent with our premise that deals following such reforms are less motivated by the desire to eliminate overlapping operations. Column (2) uses a high productivity dummy as the dependent variable, which takes the value of one if the deal falls in the top tercile of the target abnormal productivity distribution. The coefficient on the *EPL* indicator is positive and statistically significant at the 5% level, suggesting that targets following the reforms tightening employment protection are more likely to have *higher* labor efficiency – consistent with these deals being less motivated by labor efficiency improvements. Column (3) uses a *High Restructuring* dummy, which

takes the value of one if the deal takes place in an SIC4 industry from the top tercile of the post-merger workforce restructuring distribution. The coefficient on the *EPL* indicator is negative and significant at the 1% level, suggesting that deals taking place after reforms that tightenen employment protection come from industries where post-merger workforce restructuring is typically low. This is again consistent with labor restructuring becoming a less important M&A driver following such reforms. Finally, column (4) uses a *High Growth* dummy, which takes the value of one if the target's industry (SIC4) is in the top tercile of past sales growth. The coefficient on the *EPL* indicator is positive and significant at the 10% level, suggesting that deals following reforms tightening employment protection are more likely to occur in growing industries, which is consistent with lesser need for consolidation. Overall, the results of these tests indicate that the types of deals observed following reforms that tighten employment protection are those in which labor restructuring is less likely to be an important motivation.

[Please Insert Table 5 about Here]

Table 6 presents similar tests for combined firm CAR using a triple-difference specification. 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 and all the control variables (to ensure a correct triple difference interpretation).[10] In Column (2) we further add country-year fixed effects, such that the *EPL* indicator itself is absorbed and only the interaction effects between the labor reform indicator and our proxies for workforce optimization potential are identified. The coefficient estimate shows a more negative effect of employment protection reforms on combined CAR for deals with greater potential for workforce synergies.

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

Columns (3) and (4) repeat these tests using the target's abnormal labor productivity (*High Productivity* dummy) interaction. The coefficient on this interaction term is positive, consistent with the prediction that the reduction in combined firm gains as a result of EPL reforms should be smaller for deals in which the potential for labor productivity improvements at the target is limited.

Columns (5) and (6) use post-merger workforce reduction at the industry level as our measure of the importance of labor restructuring. The coefficient on the *EPL X High Restructuring* interaction term is negative, suggesting that 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 as a proxy for the need for consolidation. The coefficient on the *EPL X High Growth* 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. While it is still possible that EPL reforms are correlated with changes in other variables that are relevant for takeover gains, any such omitted variable would have to generate similarly heterogeneous effects on takeover activity and gains across the four cross-sectional dimensions we explored here.[11]

[Please Insert Table 6 about Here]

IV.D. Deal-Level Tests: Division of Gains

[11] Moreover, note that the inclusion of country-year fixed effects does not change the coefficient estimates on the interaction effects, suggesting that no major omitted variable at the country-year level biases their estimation. This further validates the assumptions underlying our difference-in-differences approach.

So far we have established that takeover activity declines following reforms that make workforce restructuring more difficult, as the synergy gains from business combinations motivated by labor optimization are reduced. If bidders and targets change their offer and acceptance decisions when faced with this shock to the availability of takeover gains, employment protection reforms may affect not only the level but also the division of takeover gains between target and acquirer shareholders. We explore this possibility in this section. To that effect, we break down the combined firm CAR into its components, namely, the bidder CAR and the target CAR, and also examine the offer premium. Examining these variables also allows us to incorporate more observations into the analysis, as it does not require both parties to be listed.

If bidders make full adjustment to the offer price (premium), 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. Target CARs are expected to follow the results on offer premiums. Table 7 presents the results of these tests.

Columns (1) through (4) use offer premium and target firm CAR as the dependent variables. These tests are based on a subsample of listed targets, but the bidder can be either a private or a public firm. The specifications are otherwise identical to those in Table 4. 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) 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 11-percentage points reduction in takeover premiums corresponds to about a third of the unconditional premium of 33.8%. Columns (3) and (4) use target firm CAR as the dependent variable. 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.[12]

Recall that we estimate the effect of EPL reforms on synergy gains to be in the range of 50-80% of the unconditional mean of the combined firm CAR. Coupled with the above results showing that offer premiums are adjusted by about a third, this suggests that bidders are not able to adjust their offers one-for-one with the reduction in synergy gains. We should therefore expect to find that bidders are experiencing lower returns following reforms. Columns (5) and (6) test this prediction using the bidding firm CAR as the dependent variable. The estimates indicate that bidder returns, indeed, decline in response to employment protection increases by about 0.45 percentage points, or by about 1.16 percentage points when all deal-level control variables are included (which, in this case, also 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. The reduction in synergies is shared by both bidder and target shareholders.

[Please Insert Table 7 about Here]

[12] 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.

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 B. First, we extend the sample period to include the post-crisis (post-2008) period.[13] 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 as modified by Allard (2005) that used in Figure 1; 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. Sixth, we exclude the reforms affecting only temporary workers (8 out of 21 reforms). 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, with some minor changes to significance levels

Finally, we also perform two "placebo" tests. In the first placebo test, we assign placebo EPL reforms when a *neighboring* country experiences an EPL reform. If the effects we document are indeed driven by EPL reforms and not by general economic conditions affecting the region, we should not find significant responses to these placebo reforms. In the second placebo test, we assign placebo EPL reforms based on a predictive model using the political economy and macroeconomic characteristics. Specifically, we regress, at the country-year level, changes in EPL (separately for positive and negative reforms) on lagged GDP growth, GDP per capita, Gini coefficient, union density, voting proportionality index, corporate tax rate, and dummies for left and right governments. We then obtain fitted values

[13] The three additional reforms during this period are increases in employment protection in Greece (2007), Ireland (2007) and Japan (2011).

from this regression, and assign 2% of highest predicted values as placebo reforms – this corresponds to the number of country-year observations with EPL reforms. If the effects we document are, indeed, due to EPL reforms and not the underlying macroeconomic or political economy conditions in the country, we should find no response to these placebo reforms obeying the key patterns of the real reforms in the data. For both types of placebo tests, we find no significant effect on deal volumes, numbers, or combined firm returns.

Another issue that potentially affects the estimated magnitude of the effect is that the composition of deals may change following the reforms. Our results on deal activity show that certain deals disappear from the market. This may imply that the types of deals observed before and after the reforms are not necessarily the same. If this is the case, such composition effects may bias our estimates of the true effect of the reforms on takeover gains. 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 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. Overall, these tests suggest that further controls for deal types do not significantly alter (and even slightly increase) our estimates of the importance of workforce restructuring for takeover gains.

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 a fraction of our 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*")[14]. 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

[14] 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.

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 8 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. Interestingly, this estimate matches the 6% reduction in employment at U.S. establishments acquired by private equity as found by Davis et al. (2014). 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%.[15]

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.

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.[16] Overall, these results further cement the labor force channel interpretation for the effects on deal outcomes that we documented above.

[15] 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.

[16] 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.

[Please Insert Table 8 about Here]

The results of our analysis indicate that the ease with which the bidder can restructure the workforce of the target accounts for between 50 and 80% of the typical takeover gain in takeovers of listed firms.[17] However, it is important to note that, to the extent that burdens on workforce restructuring prevent other types of restructuring from happening (e.g., downsizing or closing down plants may not take place when the associated workers cannot be laid off), it may not be appropriate to attribute the entire magnitude of the effect to cost savings from laying off workforce. Rather, this estimate should be interpreted as the importance of the bidder's ability to implement operational efficiency improvements that directly or indirectly involve workforce restructuring.

In order to triangulate these results, we perform a back-of-the-envelope calculation of whether the estimated magnitude of the change in synergy gains matches the expected cost savings associated with workforce optimization. Recall that our estimate of the effect of EPL reforms on post-merger workforce restructuring is 7%, i.e. reforms tightening employment protection are associated with a preservation of 7% of the combined firm workforce. An average bidder-target combination in this sub-sample has 31,446 employees, such that a 7% figure corresponds to 2,201 workers. For consistency, we re-estimate the effect of EPL reforms on the combined firm CAR in the same sub-sample and find that the synergy gains are reduced by 1.8%. An average bidder-target combination in this sub-sample has a market capitalization of $15.2 billion, so 1.8% corresponds to $274.1 million reduction in synergies. Since the stock market is reflecting after-tax gains and we wish to compare them with pre-tax

[17] A caveat is in order. Given our use of event study methodology, this estimate of the importance of workforce restructuring applies to *expected* takeover gains. That is, if the market puts higher weight on the arguably more realistic cost synergies than on the more speculative revenue synergies (e.g., cross-selling etc.), then our estimate of the importance of workforce restructuring for takeover synergies may be too high. A more precise interpretation of our estimates is then that workforce restructuring represents 50-80% of the expected takeover gains priced by the market.

wages, we gross up the $274.1 million figure by the average tax rate in our sample countries over the sample period of 39%, resulting in a pre-tax change in synergy gains of $449.3 million. Treating this figure as the present value of an annuity of wage savings, we can infer what the wages should be to justify the synergy. For this calculation, we assume that i) absent the merger, the firms involved would have continued paying the 2,201 workers for a period equal to average worker tenure in OECD countries of 9.52 years[18] and ii) the wage savings are capitalized at a discount rate equal to the average equity market return in our sample countries over the sample period of 8.7%. The annual wage saving coming out of this calculation is $71.3 million, or $32,395 per employee. These are the wage savings figures that would reconcile the observed changes in synergy gains and changes in employment following the reforms. These figures appear to be in the right ballpark, as the weighted-average worker wage (weighted by the number of deals to avoid the U.S. overstating the average wage) in our sample countries over the period 1990-2007 (the earliest we could obtain data for) is $37,344. Therefore, the estimated magnitude of the effect of employment protection reforms on synergy gains squares well with the estimated cost savings associated with employment effects of the same reforms. This admittedly ad-hoc analysis nevertheless helps further alleviate omitted variables concerns that our results are reflecting some other contemporaneous changes.

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

[18] We approximate the average worker tenure with the inverse of gross annual job losses of 10.5% (Source: OECD Employment Outlook 1996, Chapter 5, Table 5.1).

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%. 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 whose magnitude matches the magnitude of the changes in synergies, and ii) within country-years subject to the reforms, the decline in takeover numbers, volumes, and synergy gains is stronger for deals with greater potential for workforce reduction On average, bidders do not fully adjust offer prices for the changes in synergies and both bidders and targets experience a decline in their announcement returns.

Overall, our findings suggest that restructuring activities directly or indirectly involving labor are a major driver of the market for corporate control and a key source of merger synergies. Labor market rigidity could explain much of the differences in activity of the takeover market around the world. Our results also suggest that mergers and acquisitions is an important channel through which employment protection regulation affects productivity and output within an economy.

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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 C. All continuous variables are trimmed at the 1% level in each tail.

	Count	Mean	Stdv	p10	p50	p90
Deal-level Variables						
Bidder Market Value (in million $)	24,724	8,479	29,434	142	1,372	16,276
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
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 milion $)	45,696	647.5	3,048.3	60.3	157.0	1,073.3
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%
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 Abnormal Productivity (in thous. $)	12,800	-97	508	-397	-40	188
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%
Industry-level Variables						
Post Merger Workforce Change	559	15.8%	37.3%	-15.2%	8.1%	51.7%
Growth	5,626	15.7%	23.8%	-2.6%	10.8%	37.2%
Country-level Variables						
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	0.0	2.0	3.0
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 and combined firm CAR 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 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 C.

Country	M&A Activity			Combined CAR (%)		
	Number	Volume (Bn$)	Average Deal Value (M$)	Count	Mean	p50
Australia	1,897	768	405	278	3.9%	3.6%
Austria	126	61	481	9	2.6%	2.4%
Belgium	261	172	658	23	3.2%	0.5%
Canada	2,492	1,247	501	471	1.7%	1.1%
Denmark	284	125	439	25	4.6%	1.3%
Finland	287	103	360	16	6.0%	4.1%
France	1,583	1,112	703	123	1.4%	0.9%
Germany	1,410	1,122	796	71	1.3%	0.3%
Greece	84	46	546	27	2.4%	0.6%
Ireland-Rep	196	75	383	12	2.3%	1.7%
Italy	1,095	882	806	24	-0.2%	-0.4%
Japan	1,066	781	733	227	1.7%	1.1%
Netherlands	730	746	1,023	60	3.8%	2.0%
New Zealand	269	86	320	16	6.3%	5.6%
Norway	414	196	474	77	3.3%	2.9%
Portugal	161	106	659	13	4.2%	1.8%
Spain	801	492	614	45	3.2%	1.7%
Sweden	834	414	496	77	4.3%	3.5%
Switzerland	344	388	1,128	39	3.4%	2.1%
United Kingdom	6,074	3,813	628	707	3.7%	2.8%
United States	25,288	16,855	667	4,789	2.2%	1.2%
Total	45,696	29,590	648	7,129	2.4%	1.5%

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. *EPL* is a reform indicator that is set to zero in 1985 and then increments by 1 (-1) each time the target firm country tightens (loosens) employment protection. $EPL^{y-(+)i}$ is a dummy equal to one if the year of the deal is the i^{th} year before (after) the reform, and zero otherwise ($^{y++}$ denotes year +4 and beyond). Country control variables include *GDP per Capita, GDP Growth,* and *Creditor Rights Index* dummies. Industry-year fixed effects are based on the 13 macro industries defined by SDC. All variables are defined in Appendix C. Regressions are weighted by the 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 in parentheses). Symbols ∗, ∗∗, ∗∗∗ indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable: M&A Activity by Industry						
Measure of M&A activity	Deal Volume in M$ (log)			Deal Number (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
EPL	-0.27***			-0.14***		
	(-3.33)			(-3.73)		
EPL^{y-2}		0.01			-0.05	
		(0.06)			(-0.64)	
EPL^{y-1}		0.06			0.04	
		(0.34)			(0.49)	
EPL^{y+0}		-0.17			-0.03	
		(-1.35)			(-0.48)	
EPL^{y+1}		-0.43***			-0.26***	
		(-2.93)			(-3.26)	
EPL^{y+2}		-0.45***			-0.25***	
		(-3.19)			(-3.59)	
EPL^{y+3}		-0.37**			-0.24***	
		(-2.38)			(-3.47)	
EPL^{y++}		-0.22**			-0.13***	
		(-2.25)			(-2.63)	
$EPL^{Positive}$			-0.25**			-0.23***
			(-1.98)			(-3.72)
$EPL^{Negative}$			0.29***			0.04
			(2.68)			(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 firm CAR [-3;+3] expressed in percentage points. *EPL* is a reform indicator that is set to zero in 1985 and then increments by 1 (-1) each time the target firm country tightens (loosens) employment protection. $EPL^{y-(+)i}$ is a dummy equal to one if the year of the deal is the i^{th} year before (after) the reform, and zero otherwise ($^{y++}$ denotes year +4 and beyond). Country control variables include *GDP per Capita, GDP Growth,* and *Creditor Rights Index* dummies. Target controls include *Target Market Value* and *Defense*. Bidder controls include *Toehold*. Deal controls include *Same Industry*, *Cross Border*, *Stock Payment*, *Multiple Bidders* and *Hostile*. Industry-year and industry-country fixed effects are based on 13 macro industries defined by SDC. All variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year (t-statistics in parentheses). Symbols ∗, ∗∗, ∗∗∗ 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***			-2.06***
	(-4.20)			(-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

Labor Restructuring Motivated Deals and EPL Reforms

This table presents difference-in-differences estimates of the effect of EPL reforms on the incidence of M&A deals motivated by labor restructuring. Four proxies for labor restructuring motivated deals are used. In Column (1), *Business Overlap* is a dummy equal to one if the bidder and the target are located in the same country and operate in the same industry (SIC4). In Column (2), *High Productivity* is a dummy equal to one if *Target Abnormal Productivity* is in the top tercile of the distribution, where *Target Abnormal Productivity* is the difference between labor efficiency of the target and the industry average prior to deal announcement. In Column (3), *High Restructuring* is a dummy equal to one if *Industry Post-Merger Restructuring* is in the top tercile of the distribution, where *Industry Post-Merger Restructuring* is the average workforce reduction in the target firm industry (SIC4) over the sample period. In Column (4), *High Growth* is a dummy equal to one if *Growth* is in the top tercile of the distribution, where *Growth* is the weighted-average growth in revenues in the target firm industry (SIC4) over prior three years. Country controls include *GDP per Capita, GDP Growth,* and *Creditor Rights Index* dummies. Target controls include *Target Market Value* and *Defense*. Bidder controls include *Toehold*. Deal controls include *Same Industry*, *Cross Border*, *Stock Payment*, *Multiple Bidders* and *Hostile*. Industry-year and industry-country fixed effects are based on 13 macro industries defined by SDC. All variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year (t-statistics in parentheses). Symbols ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Business Overlap	High Productivity	High Restructuring	High Growth
	(1)	(2)	(3)	(4)
EPL	-0.02**	0.05**	-0.02***	0.03*
	(-1.96)	(2.54)	(-3.10)	(1.95)
Industry x Year Fixed Effects	Yes	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes
Country Controls	Yes	Yes	Yes	Yes
Target Controls	Yes	Yes	Yes	Yes
Bidder Controls	Yes	Yes	Yes	Yes
Deal Controls	Yes	Yes	Yes	Yes
N	30,322	10,093	30,322	23,206

Table 6

Combined CAR and EPL Reforms – Triple Differences Analysis

This table presents the results of cross-sectional/triple-differences analysis of the effect of EPL reforms on the combined firm CAR. Four cross-sectional dimensions are examined. In columns (1) and (2), *Business Overlap* is a dummy equal to one if the bidder and the target are located in the same country and operate in the same industry, and zero otherwise. In Columns (3) and (4) *High Productivity* is a dummy equal to one if *Target Abnormal Productivity* is in the top tercile of the distribution, where *Target Abnormal Productivity* is the difference between labor efficiency of the target and the industry average prior to deal announcement. In Columns (5) and (6), *High Restructuring* is a dummy equal to one if *Industry Post-Merger Restructuring* is in the top tercile of the distribution, where *Industry Post-Merger Restructuring* is the average workforce reduction in the target firm industry (SIC4) over the sample period. In Columns (7) and (8), *High Growth* is a dummy equal to one if *Growth* is in the top tercile of the distribution, where *Growth* is the weighted-average growth in revenues in the target firm industry (SIC4) over prior three years. *EPL* is a reform indicator that is set to zero in 1985 and then increments by 1 (-1) each time the target firm country tightens (loosens) employment protection. Country control variables include *GDP per Capita, GDP Growth,* and *Creditor Rights Index* dummies. Industry-year and industry-country fixed effects are based on 13 macro industries defined by SDC. All cross-sectional contrast variables (*Business Overlap*, *High Productivity*, *High Restructuring*, *High Growth*) are interacted with all sets of fixed effects and all control variables (hence base line effects of these variables are absorbed). All variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year (t-statistics in parentheses). Symbols ∗, ∗∗, ∗∗∗ 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			
	Bursiness Overlap		Target Abnormal Productivity		Post-Merger Workforce Reduction		Growth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EPL x Business Overlap	-3.01***	-3.27**						
	(-3.67)	(-2.48)						
EPL x High Productivity			2.27**	2.76***				
			(2.13)	(3.03)				
EPL x High Restructuring					-2.69***	-2.42**		
					(-2.84)	(-2.52)		
EPL x High Growth							5.05***	4.28***
							(3.73)	(3.28)
EPL	-1.17**		-3.29***		-1.37*		-3.69***	
	(-2.09)		(-3.56)		(-1.73)		(-4.63)	
Country x Year FE		Yes		Yes		Yes		Yes
Industry x Year FE (Interacted)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Country FE (Interacted)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Controls (Interacted)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target Controls (Interacted)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bidder Controls (Interacted)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Deal Controls (Interacted)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	7,129	7,129	5,726	5,726	6,958	6,958	5,626	5,626

Table 7

Division of Gains and EPL Reforms

This table presents difference-in-differences estimates of the effect of EPL reforms on the offer premium, target CAR [-3,+3] and bidder CAR [-3,+3]. All dependent variables are expressed in percentage points. 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*. Industry-year and industry-country fixed effects are based on 13 macro industries defined by SDC. All variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year (t-statistics in parentheses). Symbols ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	Offer Premium		Target CAR [-3,+3]		Bidder CAR [-3,+3]	
	(1)	(2)	(3)	(4)	(5)	(6)
EPL	-10.95***	-11.24***	-4.06**	-4.00***	-0.45*	-1.16***
	(-3.62)	(-4.52)	(-2.37)	(-2.95)	(-1.86)	(-2.86)
Country Controls	Yes	Yes	Yes	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	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	9,906	9,906	11,949	11,949	23,806	7,129

Table 8

Post-Merger Workforce Restructuring and EPL Reforms

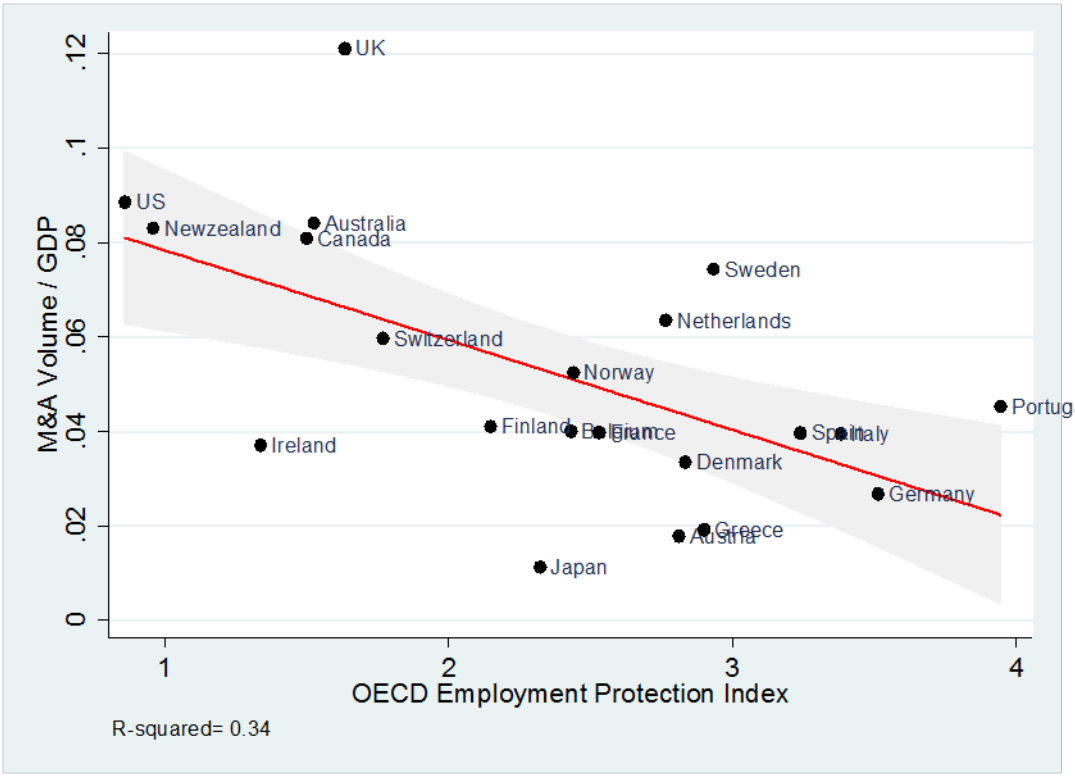
This table presents estimates of the effect of mergers on combined firm employment and the effect of EPL reforms on post-merger workforce restructuring. All deals are followed over a five-year window around completion of the transaction. The dependent variable is the (log) number of employees at the 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 deal. *EPL* is a reform indicator that is set to zero in 1985 and then increments by 1 (-1) each time the target firm country tightens (loosens) employment protection. *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. Industry-year and industry-country fixed effects are based on 13 macro industries defined by SDC. Base line effects of *EPL* in Columns (2), (3) and (4), and of both *EPL* and *Post Merger* in Column (5) are absorbed by their interactions with fixed effects. Industry-year and industry-country fixed effects are based on 13 macro industries defined by SDC. All variables are defined in Appendix C. Standard errors are adjusted for heteroskedasticity and double-clustered by deal and by year (t-statistics in parentheses). Symbols ∗, ∗∗, ∗∗∗ 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***	-0.09***		
	(-4.87)	(-6.82)		
EPLx Post Merger		0.05***		0.07**
		(4.12)		(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 GDP) by country relative to the average EPL index (as published by the OECD and corrected by Allard (2005)) over the 1985-2007 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.



Appendix A – Macroeconomic Dynamics in the Run-up to EPL Reforms

This table reports the analysis of macroeconomic dynamics in the two years prior to employment protection reforms. EPL^{y-1} is an indicator variable taking the value of 1(-1) if next year will see a reform tightening (loosening) employment protection, and zero otherwise. EPL^{y-2} is defined similarly except looking two years ahead. All other variables are expressed in percentage points (except *GDP per Capita*) and defined in Appendix C. Standard errors are adjusted for heteroskedasticity and double-clustered by country and year tests (t-statistics in parentheses). Symbols ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	GDP Per Capita	GDP Growth (%)	Unemp. Rate (%)	Public Firms RoA (%)	Private Consumption Growth (%)	Change in Stock Market Index (%)	Government Bond Yield (%)
OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)
EPL $^{y-1}$	-0.01	0.14	-0.40	0.38	-0.12	-5.18	-0.07
	(-0.29)	(0.30)	(-1.32)	(1.08)	(-0.28)	(-1.40)	(-0.39)
EPL $^{y-2}$	-0.01	0.27	-0.17	0.69	-0.15	-2.03	-0.18
	(-0.41)	(0.88)	(-0.55)	(1.09)	(-0.43)	(-0.42)	(-0.87)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	483	483	483	399	391	483	475

Appendix B - Robustness Tests

This table reports the results of several robustness tests. In Panel B.1, the tests are performed excluding U.S. deals. In Panel B.2, the tests are performed on a larger sample including the post-2008 period. In Panel B.3, the tests are performed using the continuous EPL index defined by the OECD and modified by Allard (2005). Deal volume and deal number regressions are weighted by 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 in parentheses). Symbols ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

B.1 - Excluding USA

Dependent Variable	Deal Volume	Deal Number	Comb CAR [-3;+3]
EPL	-0.35***	-0.14***	-1.59**
	(-3.00)	(-3.04)	(-2.80)
Country Controls	Yes	Yes	Yes
Target Controls			Yes
Bidder Controls			Yes
Deal Controls			Yes
Industry x Year Fixed Effects	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes
N	2,708	2,708	2,266

B.2 - Including Post-2008 period

Dependent Variable	Deal Volume	Deal Number	Comb CAR [-3;+3]
EPL	-0.27***	-0.14***	-1.51**
	(-3.65)	(-3.58)	(-2.43)
Country Controls	Yes	Yes	Yes
Target Controls			Yes
Bidder Controls			Yes
Deal Controls			Yes
Industry x Year Fixed Effects	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes
N	4,837	4,837	8,634

B.3 - Using OECD EPL Index

Dependent Variable	Deal Volume	Deal Number	Comb CAR [-3;+3]
EPL^{OECD}	-0.32***	-0.14***	-3.22***
	(-3.05)	(-2.68)	(-4.35)
Country Controls	Yes	Yes	Yes
Target Controls			Yes
Bidder Controls			Yes
Deal Controls			Yes
Industry x Year Fixed Effects	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes
N	3,646	3,646	7,129

Appendix B (Cont'd)- Robustness Tests

This table reports the results of several robustness tests. In Panel B.4, the tests are performed including additional control variables for the political / macro-economic environment in the target country. In Panel B.5, the tests are performed excluding countries with no EPL reforms during the sample period. Deal volume and deal number regressions are weighted by 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 in parentheses). Symbols ∗, ∗∗, ∗∗∗ indicate significance at the 10%, 5%, and 1% level, respectively.

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

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

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

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

Appendix B (Cont'd) - Robustness Tests

This table reports the results of several robustness tests. In Panel B.6, the tests are performed excluding EPL reforms focusing on temporary contracts. In Panel B.7 the "placebo" EPL indicator is defined based on the reforms in the neighboring country. In Panel B.8 the "placebo" EPL indicator is defined based on a statistical model predicting EPL reforms using lagged GDP growth, GDP per capita, Gini coefficient, union density, voting proportionality, corporate tax rate, and dummies for left and right governments (see Section IV.E for details). Deal volume and deal number regressions are weighted by 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 in parentheses). Symbols *, **, *** indicate significance at the 10%, 5%, and 1% level, respectively.

B.6 - Excluding EPL Reforms focusing on temporary contracts

Dependent Variable	Deal Volume	Deal Number	Comb CAR [-3;+3]
EPL	-0.29***	-0.16***	-2.00***
	(-3.40)	(-3.98)	(-4.20)
Country Controls	Yes	Yes	Yes
Target Controls			Yes
Bidder Controls			Yes
Deal Controls			Yes
Industry x Year Fixed Effects	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes
N	3,646	3,646	7,129

B.7 - Placebo using the neighboring country as the treated country

Dependent Variable	Deal Volume	Deal Number	Comb CAR [-3;+3]
EPL	0.02	-0.02	0.47
	(0.53)	(-0.28)	(1.19)
Country Controls	Yes	Yes	Yes
Target Controls			Yes
Bidder Controls			Yes
Deal Controls			Yes
Industry x Year Fixed Effects	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes
N	3,646	3,646	7,129

B.8 - Placebo using fake reforms predicted by political / macro-economic factors

Dependent Variable	Deal Volume	Deal Number	Comb CAR [-3;+3]
EPL	0.02	0.07	0.20
	(0.61)	(1.11)	(0.88)
Country Controls	Yes	Yes	Yes
Target Controls			Yes
Bidder Controls			Yes
Deal Controls			Yes
Industry x Year Fixed Effects	Yes	Yes	Yes
Industry x Country Fixed Effects	Yes	Yes	Yes
N	3,646	3,646	7,129

Appendix C - List of Variables (in alphabetical order)

Variable	Definition
Business Overlap	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
Bidder Market Value (log)	Natural logarithm of *Bidder Market Value (M$)*
Bidder Market Value (M$)	Bidder market value prior to deal announcement in M$
CAR Bidder [-3;+3]	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)
CAR Combined [-3;+3]	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
CAR Target [-3;+3]	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)
Change in Stock Market Index	Target country's main equity index return from Datastream
Combined Number of Employees	Total number of employees of the target and bidding companies (in logs)
Completed	Dummy equal to one if the deal was completed, and zero otherwise
Corporate Tax Rate	Official corporate tax rate in the target country
Creditor Rights	Creditor Rights Index as from Djankov, McLiesh, and Shleifer (2007)
Cross Border	Dummy equal to one if the target and the bidder involved have their headquarters located in different countries, and zero otherwise
Deal Value (log)	Natural logarithm of *Deal Value (M$)*
Deal Value (M$)	Deal value in M$
Defense	Dummy equal to one if a defense mechanism was used by the target, and zero otherwise
EPL	EPL is an indicator variable that increases (decreases) by one whenever a major reform aimed at increasing (reducing) employment protection is adopted in the target firm country 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
EPL^{OECD}	Continuous measure of employment protection as defined by OECD and modified for consistency by Allard (2005)
$EPL^{Positive}$	$EPL^{Positive}$ is an indicator variable that increments by one whenever a major reform aimed at increasing employment protection is adopted during the year in the target firm country. In 1985, the $EPL^{Positive}$ score is set to zero for all countries
$EPL^{Negative}$	$EPL^{Negative}$ is an indicator variable that increments by one whenever a major reform aimed at reducing employment protection is adopted during the year in the target firm country. In 1985, the $EPL^{Negative}$ score is set to zero for all countries
GDP Growth	GDP growth in the target country as reported by the IMF
GDP Per Capita	Natural logarithm of the GDP per capita in the target country as reported by the IMF
Gini	Gini coefficient in the target country as reported by the OECD
Government Bond Yield	Long-term sovereign bond yield in the target country from IMF
Growth	Weighted-average growth in revenues in the target firm industry over the last three years (by SIC4)
High Growth	Dummy equal to one if *Growth* is in the top tercile of the distribution, and zero otherwise
High Productivity	Dummy equal to one if *Target Abnormal Productivity* is in the top tercile of

	the distribution, and zero otherwise
High Restructuring	Dummy equal to one if *Industry Post-Merger Restructuring* is in the top tercile of the distribution, and zero otherwise
Hostile	Dummy equal to one if the initial bid was hostile, and zero otherwise
Left Government	Dummy equal to one if the governing party is a left-wing party, and zero otherwise
Multiple Bidders	Dummy equal to one if more than one bidder is involved, and zero otherwise
Offer Premium	Offer price relative to target stock price four weeks prior to deal announcement as reported by SDC
Post-Merger Workforce Change	Average post-merger change in the number of employees in the target industry (by SIC4). 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
Proportionality	Proportionality index measuring the degree of proportionality of the electoral system in the target country
Public Bidder	Dummy equal to one if the bidder is a public company, and zero otherwise
Public Firms RoA	Average return on assets (net income before exceptional items scaled by total assets) of all listed firms in a country-year from Global Compustat (Compustat for the U.S.)
Public Target	Dummy equal to one if the target is a public company, and zero otherwise
Private Consumption Growth	Target firm country year-on-year change in private final consumption in constant prices from OECD
Right Government	Dummy equal to one if the governing party is a right-wing party, and zero otherwise
Same Industry	Dummy equal to one if the bidder and the target and both operate in one of the 89 mid-industries defined by SDC
Stock Payment	Dummy equal to one if 100% of the proposed payment is in stock, and zero otherwise
Target Abnormal Productivity	Productivity ratio of the target relative to the industry (SIC4) average one year prior to deal announcement. Productivity ratio is defined as total revenues in thousands of dollars divided by the total number of employees.
Target Market Value (log)	Natural logarithm of the *Target Market Value (M$)*
Target Market Value (M$)	Target market value prior to deal announcement in M$
Toehold	Percentage ownership of the target by the bidder prior to initiating the bid
Unemployment Rate	Target country unemployment rate as reported by IMF
Union Density	Target country trade union density reported by the OECD. Percentage of employees who are members of a trade union