Investor Protection and Corporate Control*

Borja Larrain† Matías Tapia‡ Francisco Urzúa I.§

July 2017

Abstract

We argue that investor protection changes the relative importance of productivity and scale as drivers of corporate control transfers. Using a large sample of European firms we find that control transfers are more correlated with increasing profitability and less correlated with increasing size when investor protection is strong. This suggests that improving productivity is more important as a driver of acquisitions when investor protection is strong, and alleviating financial constraints or empire building are more important when investor protection is weak. Our evidence is consistent with the idea that good investor protection promotes a more productive use of corporate assets.

*We thank comments and suggestions from seminar participants at the Federal Reserve Bank of Saint Louis, University of Minnesota, Midwest Macro Meetings, the Central Bank of Chile, CEA, Universidad de Chile, USACH, PUC-Chile, the 5th FinanceUC conference, Gary Becker, Henrik Cronqvist (discussant), Nicolás Figueroa, Francisco Gallego, Rafael La Porta, Alexander Monge-Naranjo, Alberto Naudon, Derek Neal, Francisco Parro, Víctor Ríos Rull, Pablo Serra, Stanislav Sokolinski, José Tessada, and particularly Andrei Shleifer. José Ignacio Llodrá, Francisco Muñoz, and Ignacio Venezian provided excellent research assistance. Tapia acknowledges financial support from CONICYT/Programa de Investigacion Asociativa (Proyecto SOC 1102) and VRI-Inicio. Larrain and Tapia acknowledge partial financial support from CGC-UC. The usual disclaimer applies.

†Pontificia Universidad Católica de Chile, Escuela de Administración and FinanceUC, e-mail: borja.larrain@uc.cl (corresponding author)

‡Central Bank of Chile and EH-Clio Lab, e-mail: mtapia@bcentral.cl

§Rotterdam School of Management, Erasmus University, the Netherlands, email: urzuainfante@rsm.nl

Legal investor protection is a key determinant of the development of financial markets (see, among others, La Porta, López-de-Silanes, Shleifer, and Vishny (1997), and Djankov, La Porta, López-de-Silanes, and Shleifer (2008)). Markets with better investor protection allocate more capital to firms with good investment opportunities (see Mclean, Zhang, and Zhao (2012), and Wurgler (2000)), and invest more in R&D and innovation (Brown, Martinsson, and Petersen 2013). Part of this improvement in the allocation of resources can be attributed to the market for control. For instance, Rossi and Volpin (2004) show that better investor protection increases the frequency of mergers and acquisitions. Transfers of control –through the purchase and sale of controlling equity stakes– can render important gains in scale and productivity. Yet, how exactly these gains arise and what is the precise role for investor protection is still a matter of ongoing research (see, for example, Burkart, Gromb, Mueller, and Panunzi (2014)).

In this paper we empirically study the impact of investor protection on control transfers. This is a challenging task in the international context that is required to have cross-country variation in investor protection. Control transfers in public firms are relatively infrequent, and among private firms, where control transfers are more frequent, data are seldom available. In order to overcome these obstacles we study a unique sample of private firms in Europe that have experienced transfers of control. Our sample results from the combination of two datasets: *Zephyr*, which identifies deals such as mergers and acquisitions (M&As), and *Amadeus*, which gives access to financial data on European private firms. Our sample is close to the one in Erel, Jang, and Weisbach (2015), who study the impact of acquisitions on financial constraints, although they do not examine the role of investor protection. We provide direct micro-level evidence on the relationship between control transfers and real outcomes such as firm size and profitability, and how this interplay is affected by investor protection.

Productivity and scale are two drivers of control transfers in corporations. The productivity channel refers to acquisitions where control is transferred to a shareholder that can run

the firm more efficiently, even if the firm remains at the same scale, because the shareholder has access to better technology or better managerial practices. The scale channel refers to two sub-cases. On the one hand, by relaxing financial constraints the new controlling shareholder can solve an underinvestment problem at the target firm. On the other hand, control transfers can be related to overinvestment when acquisitions are motivated by empire building (Jensen 1986).

The model in Burkart, Gromb, Mueller, and Panunzi (2014) shows that stronger investor protection positively affects the productivity channel. Better investor protection makes it more likely that control is transferred to more productive owners. Therefore, our empirical setting can be understood as a test for their theoretical prediction. We add the consideration of firm scale, which is absent from their model because scale is exogenously fixed. We are motivated to do so because the results in Erel, Jang, and Weisbach (2015) suggest that financial constraints are crucial in transfers of control, and hence that the scale channel is a first-order concern.

We argue that investor protection has different effects on these two drivers of control transfers. When investor protection is strong, under and overinvestment problems are less severe. Hence, scale becomes less important as a motive for control transfers. In a market with perfect investor protection (i.e., without financial frictions), increasing productivity is the only valid reason behind a transfer of control. Therefore, control transfers should be more correlated with productivity improvements in high investor-protection markets. In low investor-protection markets control transfers should be more correlated with increasing firm size, for good or bad reasons. In those markets financial constraints are more binding for target firms affected by underinvestment. At the same time, empire-building acquirers face fewer constraints in low protection markets. Both reasons suggest increasing firm scale as a result of control transfers.

Empirically we compare each firm in the data before and after the transfer of control. This feature of our data is quite unique since it is often hard to observe firms after the transfer

of control (i.e., post-transfer data for acquired firms is hard to get). In regression terms, we rely on within-firm time-series variation by using firm-level fixed effects, which alleviate concerns about many unobservables. Our measure of investor protection to compare across markets is the anti-self-dealing index of Djankov, La Porta, López-de-Silanes, and Shleifer (2008). We find that profitability (return on assets, ROA) improvements are more likely and stronger in magnitude after transfers of control when investor protection is high. This finding is consistent with the prediction of Burkart, Gromb, Mueller, and Panunzi (2014). At the same time, and also when investor protection is high, we find that increasing firm size (total book assets) is less likely and weaker in magnitude after transfers of control. This suggests that scale is less relevant as a motive for transferring control in countries with high investor protection.

Can we really attach these results to investor protection? We perform a horse race between investor protection and other country characteristics to see where the differential effect on control transfers is coming from. Alternative explanations have to account for the simultaneous effects on profitability and firm size that we find, which is not obvious. For example, we do not find that per capita GDP or accounting standards (for example, the adoption of IFRS) absorb the effect of investor protection. Typical governance indicators such as the rule of law, the effectiveness of the government, or regulation do not explain our results either. At the same time, it is hard to empirically differentiate investor protection from unobserved characteristics that vary simultaneously along country and time (i.e., country-year fixed effects). Overall, the empirical findings are consistent with our predictions, but we cannot rule out all other explanations.

We perform auxiliary tests that complement our previous results. First, we decompose the effects of control transfers on profitability, scale, and capital structure. We find that profitability increases are achieved mostly through cost-cutting rather than increasing sales. In this sense, our results are in line with the literature on the relationship between acquisitions and productivity (see Li (2013), and Maksimovic and Phillips (2001) among others). In

terms of scale, the negative differential effect of investor protection in control transfers is seen both in tangible assets and working capital, but the results are stronger for tangible assets. The negative effect of investor protection is also seen in leverage, which points towards the stronger alleviation of financial constraints in poor protection countries. Second, we find that the relative size of the acquirer and the target interacts with investor protection in the expected way. Large acquirers are more likely to relax financial constraints (Hadlock and Pierce 2010), and hence to increase target size, but this effect is moderated by investor protection for the same reason as before, namely that everyone is less financially constrained when investor protection is high. Finally, we find that our results are robust to focusing on serial acquirers as Fuller, Netter, and Stegemoller (2002), which allows us to control for acquirer fixed effects, and to different subsamples according to the type of acquisition (diversifying or not) and the ownership structure of targets and acquirers (e.g., comparing listed and non-listed acquirers).

The paper that is closest to ours from a theoretical standpoint is Burkart, Gromb, Mueller, and Panunzi (2014), who study the relation between investor protection and takeovers in a variety of contexts. Our empirical setup of small firms is similar to their analysis of a takeover with a pivotal large shareholder, which avoids the classic problem of free-riding in dispersed ownership structures (Grossman and Hart 1980). In this case investor protection is enough to increase the efficiency of the takeover market. A contribution relative to their setup is that we also consider financial frictions at the firm level.[1] In their model, financial constraints affect potential bidders at the moment of paying for the target, but the target itself is not affected by frictions in the investment process. Frictions at the firm level can affect target value endogenously by changing the scale of the firm.

On the empirical side, and since the seminal work of La Porta, López-de-Silanes, Shleifer,

[1] In a sense our paper follows the theoretical literature that studies the relationship between the allocation of control and pledgeable income (see Tirole (2006), chapter 10). A unifying theme in this literature is that different allocations of control can increase the funding capacity of a firm. Other papers study the effect of investor protection on the allocation of control from particular angles, for example, for succession decisions in family firms (see Burkart, Panunzi, and Shleifer (2003), and Caselli and Gennaioli (2013)), or the organization of pyramidal business groups (Almeida and Wolfenzon 2006).

and Vishny (1998), many papers have studied the impact of legal investor protection on financial markets. For example, the depth of stock markets, firm valuation, and the level of ownership concentration are all related to investor protection (see La Porta, López-de-Silanes, Shleifer, and Vishny (2002), and Djankov, La Porta, López-de-Silanes, and Shleifer (2008) among others). Shleifer and Wolfenzon (2002) provide a unifying model that accounts for many of these features, but it does not include the market for control. Probably because of data limitations, the market for control is less studied in empirical work. There are a few exceptions, although none of these papers studies the predictions that we study. Rossi and Volpin (2004) study the frequency of mergers and acquisitions as a function of investor protection. Foley and Greenwood (2010) study the evolution of the ownership structure of firms after the IPO as a function of investor protection in the country. Finally, Bris and Cabolis (2008) study the gains from "importing" good investor protection through cross-border mergers. Along these lines we study the relative importance of the investor protection of the country of the target and of the acquirer. We find that the investor protection of the country of the target explains most of our results.

The structure of the paper is as follows. Section 1 develops the main testable predictions. Section 2 shows the empirical evidence with the sample of European firms. Section 3 concludes.

1 Hypotheses Development

We are empirically interested in how investor protection affects the outcomes of control transfers, in particular firm productivity and scale. As a starting point we consider the sales of controlling blocks described in Burkart, Gromb, Mueller, and Panunzi (2014). In their model a firm with a controlling shareholder faces a single potential acquirer. Transfers of control may not go through if the acquirer is financially constrained, even if she is a more productive user of the assets. Better investor protection eases the acquirer's financial

constraints and makes transfers more likely. Hence, our first prediction follows directly from Corollary 2 in Burkart, Gromb, Mueller, and Panunzi (2014):

Empirical Prediction 1 (Transfers of Control and Productivity): *Transfers of control are more correlated with improvements in productivity in high investor-protection markets than in low investor-protection markets.*

In Burkart, Gromb, Mueller, and Panunzi (2014)'s setup investor protection leads to a more efficient market of control, although firm scale is constant. In their model financial constraints affect the ability of acquirers to bid aggressively for a firm, but not the firm itself when it needs to invest. However, recent evidence suggests that acquisitions are also related to lifting financial constraints at the firm level so firms do not forgo valuable investment opportunities. Acquirers can often increase target size because of both their internally generated cash flow or their ability to raise funds from capital markets (Erel, Jang, and Weisbach 2015).

In a general setting we can decompose total firm value in two components: productivity times total capital invested (scale). Acquirers can increase firm value if they are more productive users of the same capital. For example, if they bring better technology or better managerial practices. At the same time, and not mutually exclusive with being more productive, acquirers can affect firm value by modifying the scale of the operation. According to Erel, Jang, and Weisbach (2015) scale has received less attention than productivity in the M&A literature. If the firm is initially constrained and investing at a second-best level, then relaxing financial constraints and increasing scale can create value. Almeida, Campello, and Hackbarth (2011) present a formal model along these lines, where a distressed firm is acquired by another firm with financial slack. Even if there are no operational synergies between the two firms, such a merger creates value by avoiding the underinvestment problem that affects the distressed firm.[2]

[2]Liao (2014) shows evidence consistent with the idea that minority acquisitions (where control is not transferred unlike the deals we study in this paper) also alleviate financial constraints in targets.

Financial frictions can lead to underinvestment as suggested above, but also to overinvestment. In particular, agency problems can lead to empire building and bloated firm size (Jensen 1986). Increasing the scale of the firm in this case creates *private* value for managers or controlling shareholders (e.g., status, career concerns), but not for minority shareholders. The empirical evidence regarding the relationship between acquisitions and firm scale suggests that acquisitions often correct overinvestment in the target firm. For instance, Li (2013) shows that U.S. acquirers cut capital expenditures in the target and also reduce scale along other dimensions (e.g., employment). Combined (target plus acquirer) announcement returns of takeovers are in line with these scale reductions. Devos, Kadapakkam, and Krishnamurthy (2008) also show that synergies in mergers are mainly produced by cutbacks in investment expenditures. This evidence is focused on the U.S. market, and therefore a pending challenge is to extend it to other markets with poorer investor protection.

In a world with scale considerations investor protection directly affects the value of firms by relaxing financial constraints in the case of underinvestment, or by alleviating agency problems in the case of overinvestment. For example, better investor protection increases the equity multiplier of distressed firms and allows for higher investment (Shleifer and Wolfenzon 2002). When investor protection is *perfect*, there are no financial constraints or agency problems so all firms invest at their first-best level. In such a market, acquirers cannot add value by relaxing financial constraints, since firms are already unconstrained. Agency problems are also under control in a market with perfect investor protection, and hence empire building is not a motive for acquisitions. By extension of this logic to markets with good (although not perfect) investor protection, changing the scale of the target should be less relevant than increasing productivity as a motive for control transfers.

On the other hand, when investor protection is low, increasing the scale of distressed targets can be a key driver of acquisitions. Also when investor protection is low, agency problems and entrenched managers face fewer constraints. Hence, acquisitions can be motivated by empire building and they can lead to overinvestment. Both of these mechanisms

suggest a strong correlation between control transfers and increasing firm size when investor protection is low.

These considerations lead to our second empirical prediction:

Empirical Prediction 2 (Transfers of Control and Firm Size): *Transfers of control are more correlated with changes in firm size in low investor-protection markets than in high investor-protection markets.*

Our predictions 1 and 2 distinguish the impact of investor protection on two drivers of control transfers (productivity and scale). Better investor protection changes the *relative* importance of these drivers. However, this does not have to be understood as saying that productivity and scale are always substitutes, quite the contrary. Both productivity and scale can move in the same direction. The point is that the relative intensity of the scale motive wears down as investor protection improves.

There can be particular cases in which scale and productivity are substitutes. For example, imagine a low-productivity acquirer, but with a financing capacity that more than compensates for her low productivity. Transferring control to this low-productivity acquirer can be efficient if the new scale is large enough once financial constraints are relaxed. In other words, total firm value (i.e., the size of the pie to be distributed among shareholders) can be higher under this acquirer. In Burkart, Gromb, Mueller, and Panunzi (2014) productivity increases in all transfers of control. In their model the scale of the firm is fixed and therefore there is no other margin that can compensate for a productivity loss. In our setup, productivity can fall, but the transfer can still be efficient if the larger scale more than compensates for the productivity loss. This potential trade-off between productivity and scale, which is certainly not present in all transfers, has been described in previous literature on control and financial constraints (see Tirole (2006), chapter 10)

Notice that the predictions that we study correspond to the *differential* effect of investor protection on control transfers. A negative effect of investor protection on firm size does

not necessarily imply that the average transfer shows a decrease in size. It is only necessary that the average change in size is smaller in high protection countries than in low protection countries. The same is true for productivity. A positive effect of investor protection on productivity does not necessarily imply that the average transfer shows an increase in productivity. It is only necessary that the average change in productivity is larger in high protection countries than in low protection countries. We do not have a prediction for the *unconditional* average effects of control transfers on productivity and firm size, because they depend on the relative strength of the scale and productivity motives for the average firm. The average effect also depends on whether the potential trade-off between productivity and scale that we describe above is present in the average firm or not.

Investor protection also interacts with firm characteristics during control transfers. For example, large acquirers are more likely to increase firm size as financial constraints are relaxed. Hadlock and Pierce (2010) show that firm size is a particularly good predictor of financial constraints. The financial advantage of large acquirers can be particularly important in a market with low investor protection, therefore we predict a stronger scale motive in a transfer of control between a large acquirer and a small target in a low protection market. Similarly, the productivity motive is more likely to be behind the transfer of control between a low-productivity target and high-productivity acquirer (Maksimovic and Phillips 2001), and more so if this transfer occurs in a market with high investor protection. Similarly, firm characteristics interact with investor protection in the incentives of acquirers to search for targets. It is arguably more likely that large acquirers search for small targets in poor protection markets since their size advantage makes them more competitive in comparison to other acquirers in those markets.

Our third prediction summarizes these intuitions:

Empirical Prediction 3 (Triple Interactions): *In high investor-protection markets compared to low investor-protection markets, transfers of control are:*

i) less correlated with changes in firm size, especially if the size difference between the

acquirer and the target is large.

ii) more correlated with improvements in productivity, especially if the productivity difference between the acquirer and the target is large.

Our label of Prediction 3 as "triple interactions" becomes clear in the empirical section since it refers to the regression specification that we use to test this prediction.

2 Empirical Results

2.1 Data Description

We use a combination of two datasets, *Zephyr* and *Amadeus*, from Bureau van Dijk (BvD). *Zephyr* provides data on public and private deals like M&As, IPOs, acquisitions of minority stakes, and others. It provides information on the type of deal and the characteristics of the deal such as the stake that was acquired, legal status (listed or private firm), and industry classification. From *Zephyr* we select all M&As of European firms completed between 1997 and 2012. We consider that control is transferred when the acquirer has less than 50% of the target's shares before the deal and more than 50% after the deal.[3] We focus on private targets, which account for the vast majority of control transfers in *Zephyr*. We exclude from our sample the private-to-public deals (e.g., IPOs) or public-to-private deals (e.g., LBOs) because the type of ownership structure and the informational environment in those cases are changing simultaneously with the identity of the controlling shareholder. Our purpose is to focus solely on changes of the controlling shareholder.

We then match target firms from *Zephyr* to *Amadeus*, the database that provides financial information on public and private firms in Europe. The match is necessary to have information on financial variables before and, particularly, after the deal. We are able to

[3]We exclude all targets that participate in more than one deal during our sample period, with different acquirers or with the same acquirer. For instance, if within this period the acquirer buys 20% from the target and then two years later increases its stake to 60%, we don't consider this target in our sample. The reason for excluding these observations is that it is difficult to pin down the effect of each individual deal in these cases.

observe target firms after the deal if they remain as independent legal entities and are not fully absorbed by acquirers.[4] Matching both databases is possible given that firms have a common identifier in BvD. However, the match is not perfect because many private firms do not appear in *Amadeus*.[5] Also, in order to include a control transfer in our sample we require that the target firm appears in *Amadeus* for at least one year before the deal, and at least two years after the deal (e.g., for a deal in 2012 we require financial data up to 2014). The effects of control (e.g., increased fund raising) can take time to materialize, hence the importance of having a relevant post-transfer window.[6] Overall, after matching *Zephyr* with *Amadeus* we lose approximately three-quarters of the control transfers initially identified with basic information in *Zephyr*. In Table A.1 in the appendix we describe in more detail the number of deals we lose in each step of our matching procedure.[7]

Following Erel, Jang, and Weisbach (2015) we apply several restrictions to our sample. We truncate the distribution of annual growth in total book assets at 100%.[8] We use ROA (=EBITDA/Total assets) as our main proxy of productivity, which although crude is the best we can do given data availability.[9] We truncate the distribution of ROA at

[4] We are able to observe the target firm basically if it continues to pay taxes. The firm can continue to exist as an independent legal entity even if the ultimate owner changes. In some cases the target firm is absorbed by another firm, in which case we do not observe the target firm after the transfer and it is excluded from our data. All firms (private and public) in Europe are required to disclose their financial information to local tax authorities and other regulators. Amadeus collects this information from local sources and standardizes it.

[5] There are other problems in the match. First, *Amadeus* keeps a rolling window of 10 years of data for most companies. Thus, almost all deals prior to 2005 are lost in the matching. Second, we cannot match targets that change their BvD identifier, those that are legally absorbed by the acquirer after a control acquisition, or those that have several BvD identifiers (BvD has been in the process of homogenizing the codes among its databases).

[6] Our results are robust to focusing on a 2-year window around the acquisition. We prefer using the entire time series available for each firm because it allows us to get better estimates of firm and year fixed effects, and therefore to see whether the transfer of control really produces a change in each firm.

[7] It is possible that our sample selection procedure leads to a selection bias that affects the results (Netter, Stegemoller, and Wintoki 2011). Out of a universe of close to 46,000 deals with target information in *Zephyr* (see Table A.1) we end up with only 10,989 deals for our main tests. We tackle the selection bias by estimating a Heckit model. The first stage of this model is a probit (cross-sectional) equation where we model the probability that a firm appears in our sample as a function of target assets, ROA, leverage, and country and year dummies. We then construct the inverse of Mill's ratio (see Wooldridge (2002)), and we include it in our baseline regressions. Our results survive this adjustment, and Mill's ratio is never significant, which suggests that selection bias is not affecting our results.

[8] Our results are robust to using lower cutoffs for asset growth, for example, 25% or 50%.

[9] EBITDA is earnings before interest, taxes, depreciation and amortization.

−100% and 100%. We also drop observations that imply annual differences in ROA between two consecutive years that are larger than 100 percentage points in absolute value. These restrictions are there only to make sure that firms before and after the control transfer are relatively comparable entities, and that our regression results are not driven by outliers. For instance, growth rates higher than 100%, or very violent changes in ROA, can be the product of a combination of the target with some of the parent's other firms. Although potentially interesting situations, these outliers are hard to identify precisely in the data and they can heavily distort regressions and statistical inference.

Table 1 shows the distribution of the 10,989 deals in our sample by country of the target firm. Erel, Jang, and Weisbach (2015) have a sample of close to 5,100 deals for a similar set of countries in the years 2001-2008. Most of the deals we add to their sample correspond to control transfers in the years 2009-2012. Our sample is much larger than what one can gather if considering only public firms. For example, Dyck and Zingales (2004) show data of control premia for 393 transfers in 39 countries. If we restrict the attention to European countries in their sample we would have only 149 deals. In *Zephyr* we find fewer than 200 transfers of control in public firms in the same time period and countries as our main sample.

The anti-self-dealing index of Djankov, La Porta, López-de-Silanes, and Shleifer (2008) is our main proxy for investor protection. This index captures the legal protection available to non-controlling investors against the expropriation from corporate insiders through related-party transactions.[10,11] This index is particularly applicable to the environment of small firms and large controlling shareholders that characterize our sample. Countries of the common law tradition, such as the U.K. or Ireland, have the highest levels of investor protection.

[10] Although the focus of this index is on public firms, it covers the key issue of corporate governance that is pervasive in public and private firms alike, namely the existence of self-dealing or tunneling. For example, Enriques and Volpin (2007) conclude in their study of corporate reform in Europe that "far too little has been done to resolve the problem of related-party transactions, which is the most common form of self-dealing for dominant shareholders in Europe (p. 138, Enriques and Volpin (2007))." Moreover, as Djankov, La Porta, López-de-Silanes, and Shleifer (2008) show, the laws that regulate this problem are deeply ingrained in a country's legal tradition (e.g., common or civil law) and, hence, they represent principles at work in the legislation applicable to all types of corporations in the country.

[11] Notice that debtholders, and not only minority shareholders, are affected by related party transactions if these represent cases of risk-shifting or fraudulent conveyance.

Scandinavian countries (e.g., Finland or Norway) are somewhat in the middle, while other big European countries such as Germany, France, or Spain have relatively low levels of investor protection. As can be seen in Table 1, the cross-country variation of the anti-self-dealing index is significant in our sample given that the index, by construction, ranges between zero and one.

The anti-self-dealing index is the preferred measure of investor protection of Djankov, La Porta, López-de-Silanes, and Shleifer (2008), who study a series of related indexes to measure this multidimensional country characteristic. The authors argue that this is a better measure than the anti-director index originally presented in La Porta, López-de-Silanes, Shleifer, and Vishny (1998). We also use the creditor rights index, which is an index related to debtholders' rights in bankruptcy (Djankov, McLiesh, and Shleifer 2007). In our sample the correlation between the anti-self-dealing index and the creditor rights index is quite high (0.63), showing that the protection of minority shareholders and the protection of debtholders tend to go hand in hand, at least among these countries.

Table 2 shows the distribution of deals across time. The years 2006-2011 account for the lion's share of our sample. The financial crisis (2008-9) might make our sample period not representative of other periods. However, the crisis might also be an advantage if it makes financial frictions more prevalent, and the type of mechanisms suggested in this paper more relevant.

Targets are relatively small firms, with median assets of 3.3 million EUR, although there are some large targets as shown by the difference between average and median assets. Target size is comparable to the one in Erel, Jang, and Weisbach (2015). Target age is on average 22 years, which shows that these are mostly well-established firms and not start-ups or new entrepreneurial ventures. The average stake after acquisitions is 95.3%, hence this is an environment of large controlling shareholders and not dispersed ownership structures. This also shows that control is typically not shared, for instance, among two large shareholders.

2.2 Preliminary evidence

Table 3 shows changes around control transfers for the main variables in our analysis. For each firm we compute the average of a variable before and then after the transfer of control. We compute the difference between these averages (after minus before) for each firm, and then we average these differences across firms. The average change in ROA is -0.036 while the average change in (log) total assets is 0.066. Table 3 shows raw averages, without adjusting, for example, for time fixed effects. The recessions in the latter part of our sample (2008-9, 2011-12) can explain in part the average fall in ROA after transfers.[12] However, an aggregate macro effect does not necessarily explain the difference between high and low protection countries, which is the focus of our empirical prediction. We consider the investor protection of the country of the target firm as our measure of investor protection. The average fall in ROA in high investor protection countries is less pronounced than in low protection countries (-0.022 vs. -0.039), which implies a positive difference of 0.017 in favor of high protection countries. At the same time, assets decrease in high protection countries while they increase in low protection countries (-0.544 vs. 0.255), which implies a negative difference of -0.799 against high protection countries. Differences between high and low investor protection are significant at the 1% level.

Other variables reported in Table 3 include the main components of EBITDA, which is equal to sales, minus the costs of goods sold, and minus other operating expenses. This helps us to better understand the behavior of ROA. Despite a relative fall in sales when comparing high and low protection countries (0.002 vs. 0.148), high protection countries are better than low protection countries at controlling costs (0.077 vs. 0.421) and cutting other operating expenses (-0.088 vs. 0.570). We also report the main components of EBITDA as a fraction of sales, where we find that the main decrease in costs in high protection countries compared to low protection countries comes from other operating expenses (-0.017 vs. 0.178).

[12] One could argue that the decision to transfer control is based on expected productivity, but that ex-post productivity includes unpredictable macro shocks such as the crisis of 2008. In the data we get to see only ex-post or realized ROA.

In a similar way as with the profitability decomposition we split total assets into fixed assets and current assets. Fixed assets include tangible assets and intangible assets (e.g., patents or software), plus depreciation. We find a negative and significant difference between high and low protection countries in both fixed and current assets. When looking at ratios of fixed and current assets over total assets we find that the negative difference between high and low protection countries is focused on fixed assets. Finally, leverage (=book debt over book assets) also decreases more strongly in high protection countries.[13]

In Table 4 we split the raw data by type of deal, depending on whether ROA increased or decreased after the transfer of control, and similarly for total assets.[14] We find that deals with an increase in ROA are relatively more frequent in high protection countries than in low protection countries (42% vs. 38%), while the opposite is true for deals with an increase in assets (42% vs. 61%). The higher frequency of productivity-increasing deals in high protection countries explains in part the positive effect of investor protection on ROA that we see in Table 3. While the lower frequency of size-increasing deals explains in part the negative effect of investor protection on firm size.

Table 4 also shows that our findings do not solely rely on the relative frequency of different types of deals. We find that *within deal-type* the difference in ROA changes between countries with high and low protection is generally positive (see the next-to-last column in Table 4). The exception corresponds to deals with a decrease in ROA, although the difference (only -0.005) is smaller in magnitude than the positive differences seen in other deals. Again *within deal-type*, the difference in asset changes between countries with high and low protection is always negative (see the last column of Table 4). This implies that, even holding constant the relative frequency of deals, transfers of control in high protection countries tend to be empirically associated with a relative increase in ROA and a relative decrease in assets compared to deals in low protection countries.

[13] The overall drop in target leverage during this period can be attributed to the financial crisis, although not necessarily the intensity of the drop according to investor protection.

[14] There are fewer deals in Table 4 than in Table 1 because we do not have data simultaneously for ROA and assets in every deal.

2.3 Main results on the consequences of control transfers

We test our predictions with the following panel regression:

$$\text{Firm Outcome}_{j,t} = \beta \text{Control}_{j,t} + \gamma(\text{Control}_{j,t} \times \text{Investor Protection}_c) + \mu_t + \mu_j + \epsilon_{j,t}. \quad (1)$$

Firm outcome can be ROA or log-assets of firm j in year t. $\text{Control}_{j,t}$ is a dummy variable that takes the value of one on the year of the control transfer and subsequent years for each firm, and zero otherwise. Since financial data is recorded at the end of the year, data for the same year of the deal already contains effects related to the change of controlling shareholder. The interaction of $\text{Control}_{j,t}$ with the level of investor protection in country c where firm j is domiciled ($\text{Investor Protection}_c$) captures variation in the effect of control transfers as a function of investor protection. The regression above is basically a differences-in-differences setup in the sense that we first compute before-and-after differences for each firm, and we then study how these differences vary according to investor protection.

Our sample selection and regression specification follows closely Erel, Jang, and Weisbach (2015), except for the new interaction term. All regressions include year fixed effects (μ_t) that capture any aggregate time-varying effects such as the crisis of 2008. Perhaps more important is the inclusion of firm fixed effects (μ_j), which has several advantages. First, firm fixed effects imply that we are focusing on the time series variation within each firm as control is transferred, or that each firm is compared to its own history. Any time-invariant firm characteristic is absorbed by the fixed effect. Second, and as a corollary of the previous point, the firm fixed effects absorb any time-invariant industry or country effects. For this reason we do not add investor protection as an independent variable to regressions. Standard errors are clustered at the country level in all of our regressions since investor protection is a country variable.[15]

Table 5 shows results for the main panel regressions. The average control transfer im-

[15] Our results are robust to using firm-clustered standard errors.

plies a fall in ROA ($\beta = -0.013$, t-stat$= -2.60$) and an increase in log-assets ($\beta = 0.101$, t-stat$= 5.61$). These results are already conditional on time fixed effects, so we cannot blame aggregate macro effects for the average fall in profitability. The average fall in ROA after transfers of control has not been documented before, but it is reminiscent of the well-established post-IPO drop in profitability (see Pástor, Taylor, and Veronesi (2009) and references herein).[16] At least according to our setup the prediction for the average effect is ambiguous, since it depends on the relative strength of the scale motive and the productivity motive for the average firm. The results in Table 5 suggest that in our sample financial constraints are strong since the average firm is gaining scale and giving up profitability at the same time.[17]

Our main interest is on how investor protection modifies the average effect of transferring control. This effect is captured by the interaction of the control dummy with investor protection (γ coefficient). Although the coefficient on the control dummy in the ROA regression is negative, the positive coefficient on the interaction ($\gamma = 0.038$, t-stat$= 2.71$) implies that the productivity effect of control transfers becomes positive (or less negative) as investor protection increases. The interaction is negative in the case of log assets ($\gamma = -0.744$, t-stat$= -5.06$), which means that the average increase in assets after control transfers is smaller or even negative as investor protection increases.

In order to illustrate the magnitudes of the coefficients in Table 5 we consider the following thought experiment. We compare the results of a control transfer in a market at the 25th percentile of investor protection in our sample (Czech Republic) with a control transfer in a market at the 75th percentile of investor protection (Finland). Focusing on the second column for each dependent variable in Table 5, we find that the average control transfer in the Czech Republic would show a fall in ROA of 0.017, and an increase in (log) assets

[16] Transfers of control and IPOs are different corporate events, but analogous explanations can be drawn in each case. The IPO literature has explored rational and behavioral theories for the average negative effect on profitability, ranging from real options to adverse selection and market timing. We do not take a stand regarding these theories when applied to control transfers.

[17] Identifying the average effect of control on productivity and scale is an important, yet unresolved issue in the literature (see, for example, Maksimovic and Phillips (2001) and Pérez-González (2007)).

of 0.218. A control transfer in Finland would show a 50 basis-point gain in productivity (through a smaller fall in ROA of 0.012) vis-à-vis the same transfer in the Czech Republic, but an almost 10% smaller change in scale (through an increase in log assets of 0.121)

2.4 Is it really investor protection?

One concern with our results is whether they are derived from investor protection itself or from other country characteristics that are correlated with investor protection. We tackle this omitted variables problem by making a horse race between our main interaction with investor protection and interactions with other country characteristics.

For example, Rajan and Zingales (1998) study the impact of per capita GDP and accounting quality in the context of finance and growth. The interaction with per capita GDP is interesting because a naive view of investor protection is that it is just a proxy for income levels. However, the results in columns 1 and 5 of Table 6 show that the interaction of control and per capita GDP does not drive away our main interaction.

Accounting practices can explain differences in the informational environment between countries. We proxy for accounting practices in two ways. First, similarly to Rajan and Zingales (1998), we use the index of accounting quality taken from the Global Competitiveness Index (1-7 index; 7 is best). Second, we use a dummy for the period after the adoption of IFRS in each country. IFRS was adopted in 2005 in most western European countries, but later than 2005 in Russia, Romania, and other eastern European countries in our sample. If the ROA results are related to informational asymmetries we can expect the interaction of the control dummy and accounting practices to have a positive coefficient in the regression (i.e., lower fall in ROA when information is relatively good). We do not find evidence for this hypothesis in Table 6 (see columns 2 and 3). Even after controlling for accounting practices the interaction of control and investor protection remains significant, and of similar magnitude when compared to baseline results.

We also incorporate several controls from the World Bank database of governance indi-

cators, in particular, measures of the rule of law, government effectiveness, and regulatory quality. As seen in columns 4 and 8 of Table 6, the interactions of these variables with the control dummy are never statistically significant, nor do they drive away our main interaction with investor protection.

A related concern that we study in Table 7 is the robustness of our results to alternative measures of investor protection. The anti-self dealing index can be a noisy proxy for investor protection, although this measurement error would only bias the results against our predictions. One of the disadvantages of the anti-self dealing index is that it does not have time-series variation. As an alternative we explore the enactment of the European Takeover Directive, which provides a time-varying measure of investor protection within the European Union. Different countries incorporated the board neutrality rule to different extents, and starting from different levels of compliance before the directive. Davies, Schuster, and van de Walle de Ghelcke (2010) provide details on the implementation of this rule (see their Table 3). As seen in column 1 of Table 7, the interaction of the enactment of the board neutrality rule with the control dummy has a positive sign, which implies that ROA goes up after control transfers particularly so in countries that implemented the rule more strictly. The coefficient is, however, not statistically significant. One potential explanation for this result is that, although the change in regulation is well identified in this case, it only refers to one particular aspect of investor protection.

The creditor rights index is based on the rights of debtholders in the event of bankruptcy (Djankov, McLiesh, and Shleifer 2007). The results with creditor rights in the regression for ROA (column 2 of Table 7) are comparable to the anti-self dealing index in sign and significance. The coefficient in the regression for assets is still negative, although significance is reduced (column 6 of Table 7).

La Porta, López-de-Silanes, Shleifer, and Vishny (1998) argue that the protection of non-controlling investors is ultimately a function of the legal origin of the country, which provides the principles that define commercial law. Countries with a common law tradition (U.K.,

U.S.A., former British colonies in Africa and Asia) fare better in almost all dimensions of investor protection. On average, Scandinavian and German countries are somewhat below common law countries in terms of investor protection, and civil law countries (i.e., French legal origin) are at the bottom. In Table 7 we interact our control variable with dummies for the different legal origins (civil law is the excluded category). We find that common law countries have the strongest positive impact on ROA, and the largest reduction in assets. Scandinavian countries have smaller reductions in assets than common law countries, but stronger than German countries. We do not find a meaningful difference between German countries and civil law countries. Finally, following La Porta, López-de-Silanes, Shleifer, and Vishny (1998) and Rajan and Zingales (1998), we instrument for the interaction of control and investor protection with the previous interactions of control and legal origin dummies (see the last column for ROA and for assets in Table 7). The results using 2SLS confirm the previous OLS results.

2.5 Decomposing the effects on productivity and scale

In order to better understand the relative increase in productivity allowed by investor protection we run separate regressions with the main elements of EBITDA.[18] As can be seen in the first column of Table 8, there is no differential effect of investor protection on sales after control is transferred. However, in columns 2 and 3 there is a significant reduction in the costs of goods sold and other operating expenses when investor protection is high (see Li (2013) for similar evidence among U.S. takeover targets). This suggests that productivity increases are achieved through cost-cutting while retaining similar sales, instead of increasing sales aggressively.

In Table 8 we also dig deeper into the scale results. The differential effect of investor protection is highly significant for both fixed assets and current assets (see columns 4 and 5). The effect is larger in magnitude in the case of fixed assets (-0.835 vs. -0.407). This

[18] The number of observations varies for the different elements of EBITDA. Often times the decomposition is incomplete although the final value of EBITDA is available in the dataset.

suggests that transfers of control have more to do with the divestiture of tangible assets (i.e., property, machines, etc.) than with an efficient use of working capital. Our results are in line with Devos, Kadapakkam, and Krishnamurthy (2008) and Li (2013) who find that acquisitions in the U.S. are related to cutting investment expenditures.

The last column in Table 8 shows the regression with leverage as dependent variable, which reflects the capital structure of the firm. We find that the control dummy has a positive coefficient (coeff. 0.066, t-stat 2.53), although the interaction of control and investor protection has a negative impact on leverage (coeff. -0.171, t-stat -5.7). In other words, although leverage increases after transfers of control, which is a sign of relaxing financial constraints, the increase is less pronounced (or leverage goes down) in countries with good investor protection. This is consistent with the idea that, in high protection markets, financial constraints are less binding even before control transfers.

2.6 Acquirer and target characteristics

Acquirer and target characteristics relative are important for the outcomes of control transfers (Erel, Jang, and Weisbach 2015). Large acquirers are more likely to increase target size as financial constraints are relaxed. Highly profitable acquirers are more likely to increase target profitability (Maksimovic and Phillips 2001). Investor protection probably affects the selection process involved in acquisitions. For example, large acquirers are more likely to target small firms in an environment of poor investor protection since their relative size is a stronger advantage in those markets in comparison to markets with good investor protection.

In Table 9 we show regressions where we interact the control dummy with the relative

size (and also profitability) of acquirers and targets:

$$\begin{aligned}\text{Firm Outcome}_{j,t} &= \beta\text{Control}_{j,t} + \gamma(\text{Control}_{j,t} \times \text{Investor Protection}_c) \\ &\quad +\delta(\text{Control}_{j,t} \times \text{Relative Size}_j) \\ &\quad +\lambda(\text{Control}_{j,t} \times \text{Relative Size}_j \times \text{Investor Protection}_c) \\ &\quad +\mu_t + \mu_j + \epsilon_{j,t}. \end{aligned} \tag{2}$$

Relative size is the difference in (log) assets between the acquirer and the target. Relative profitability (not shown above) is the difference in ROA between the acquirer and the target. Both differences are measured in the year before the acquisition with data from *Zephyr*.

As expected, the interaction with relative profitability has a positive and significant coefficient in the ROA regressions (columns 1-3 Table 9), and the interaction with relative size has a positive and significant coefficient in the assets regressions (columns 4-6 Table 9). It is harder to predict a sign for the cross-effects, for example, the effect of relative size on ROA.

We also include triple interactions of the control dummy, acquirer-target characteristics, and investor protection.[19] We find significant triple interactions in the regression for assets (column 6 Table 9). As in our Prediction 3, the triple interaction of control, relative size, and investor protection is negative (coeff. -0.113, t-stat. -2.56), which suggests that large acquirers do not have such a big impact on scale in high protection markets because target firms are initially less constrained. We do not find evidence in favor of the second part of Prediction 3 since the triple interaction of control, relative profitability, and investor protection is not significant in the ROA regression (column 3 Table 9).

After controlling for acquirer-target characteristics the significance of our main double interaction of control and investor protection survives in the regressions for assets (columns 5-6 in Table 9). The coefficients on this interaction are comparable in magnitude to the

[19] Other double interactions are absorbed by firm fixed effects given that acquirer-target relative characteristics are constant for each firm.

main regressions in Table 5. In the ROA regressions, however, the double interaction is no longer significant at conventional levels (columns 2-3 in Table 9). The coefficients are of similar magnitude compared to the baseline results, but the standard errors increase by a factor of two. Part of this can be attributed to a potential case of over-controlling when we include acquirer-target characteristics. By controlling for acquirer-target relative characteristics we give up an important margin of influence of investor protection, namely that investor protection modifies the set of transfers that occur in equilibrium (the "extensive margin"). As investor protection improves, relatively fewer transfers are made towards larger acquirers simply to gain scale, and relatively more transfers are made towards more profitable acquirers. In other words, by controlling for acquirer-target characteristics we are focusing on the effect of investor protection only within a given pair of firms (the "intensive margin") that is matched for supposedly exogenous reasons.

Implicit in our analysis so far is that the relevant variation in investor protection is cross-country. We do this for two reasons. First, for identification purposes: there is more hope that this component is exogenous to the firm-level decision to transfer control, at least when compared to firm variables associated with corporate governance. Second, the available empirical evidence suggests that the country component dominates differences in corporate governance (see Doidge, Karolyi, and Stulz (2007)). Our approach does not rule out the existence of a firm-level component in investor protection. However, it does rest on the idea that local investor protection cannot be perfectly substituted for other firm characteristics, such as the origin of the acquirer. Previous research shows that acquirers from good investor protection countries may help targets in poor protection countries to "import" investor protection (see Rossi and Volpin (2004) and Bris and Cabolis (2008)).

One way to test the substitution hypothesis is to include in our regression the interaction with the investor protection of the acquirer's country. As seen in Table 10, the interaction with the investor protection of the acquirer's country has a negative sign in the ROA regression unlike our main interaction with the investor protection of the target's country. In

the assets regression the interaction with the investor protection of the acquirer's country is not statistically significant. The significance of our main interaction survives the inclusion of the interaction with the acquirer's investor protection in both the ROA and the assets regressions.

Another alternative is to include the difference in investor protection between the country of the target and the acquirer. As seen in Table 10, we find a positive coefficient in the ROA regression for this interaction which means that the effect of the target's country dominates. The interaction with the difference in investor protection is not significant in the assets regression, while the interaction with the target's investor protection is still significant (see last column in Table 10). Overall, the results in Table 10 are consistent with the idea that the local legal environment cannot be perfectly substituted with other governance mechanisms. In other words, local investor protection matters even if foreign acquirers can ameliorate some of the deficiencies of the local market by "importing" investor protection.

Acquisitions across industries are more likely to be motivated by managerial entrenchment and empire building (Devos, Kadapakkam, and Krishnamurthy 2008). Almeida, Campello, and Hackbarth (2011) argue that mergers due to financial distress ("liquidity mergers") are more likely within industries. Therefore, it is interesting to see if investor protection has a differential effect in deals where the target and the acquirer operate in different industries or in the same industry. We label acquisitions as "diversifying" when the acquirer and the target are in different industries.[20] In Table 11 we find that the effect of investor protection can be seen both in diversifying and non-diversifying acquisitions. We test formally if there is a statistically significant difference in the coefficients across samples by including an interaction with a dummy for diversifying acquisitions in the regression that includes all deals (columns 3 and 6 of Table 11). The interaction with diversifying acquisitions is never significant. One interpretation of these results is that investor protection is equally effective in reining in empire-building acquisitions across industries, while reducing the need

[20] We define an acquisition as "diversifying" when the target and the acquirer operate in two different SIC-2 codes.

for liquidity mergers within industries. Our result that ROA improvements are not limited to non-diversifying acquisitions is consistent with the finding of Schoar (2002) about the productivity improvements of targets in diversifying acquisitions.

In our sample we have cases of a single acquirer buying several targets. The presence of a single acquirer in multiple deals allows us to control for time-invariant acquirer characteristics through acquirer fixed effects, on top of the already included target-firm fixed effects. Therefore, any variation that we find in outcomes must to be due to changes in the target through time and cannot be attributed to the acquirer itself. This focus on "serial acquirers" (i.e., acquirers for which we observe more than one acquisition) follows the empirical strategy of Fuller, Netter, and Stegemoller (2002). Close to a third of the observations in our sample correspond to acquisitions by serial acquirers. In Table 12 we run regressions separately for serial and non-serial acquirers and we find that the effects, although statistically significant in each sub-sample, are stronger in magnitude among serial acquirers. This reinforces the idea that the our results are related to changes in the target as it falls under a new controlling shareholder, and not simply a function of acquirer characteristics.

Finally, in Table 13 we explore potential variation in our results across different ownership structures of targets and acquirers. The ownership data has to be put together from annual *Amadeus*' DVDs, since it is not readily available for download from WRDS. The ownership data includes the names of controlling shareholders and their ownership stakes, together with the listing status of the company. First, we split acquirers into publicly listed and non-listed. (All targets are private companies.) Then, we split acquirers and targets into those with and without a strong controlling shareholder before the deal. For the case of private companies this implies that there is a shareholder who owns at least 50% of the shares, while this threshold is 20% for public companies. Although with varying magnitude of the coefficients, the results are almost uniformly robust across sub-samples. All of the coefficients have the predicted signs, and all except for one are statistically significant. This suggests that the type of ownership structure before the deal is not crucial for our main findings.

2.7 Assorted robustness checks

In Table 14 we show our results after controlling for alternative sets of fixed effects. Industry-year fixed effects can capture merger waves or particular technological changes at the industry level. If industrial composition is correlated with investor protection, then our interaction of interest can be related to these effects instead of the mechanism we propose. As seen in columns 1 and 2 of Table 14, our results remain robust after we include industry-year fixed effects.

Next we include country-year fixed effects on top of the baseline firm fixed effects (see columns 3 and 4 in Table 14). The coefficient on the interaction in the ROA regression is smaller than our baseline estimate (0.028 vs. 0.038), but it is still significant at the 10% level. The interaction in the assets regressions is still negative, but it is no longer significant. Country-year fixed effects are tough controls, since they absorb potentially a big chunk of the variation that we actually want to explain. These fixed effects absorb cross-sectional differences in investor protection, like country dummies, but also the interaction of these country effects with time. Our control dummy is basically a "before-and-after" variable for each firm. Since, by construction, early years in our sample are more likely to be "before" years and later years are more likely to be "after" years, the country-year dummies capture a relevant piece of the variation that allows us to identify the interaction of investor protection and control. Despite the strictness of these fixed effects, the overall message survives, and in particular the ROA result that is the key prediction of Burkart, Gromb, Mueller, and Panunzi (2014). It is still a word of caution when interpreting our results since we cannot claim that we can perfectly separate our results from unobserved country-year effects. This is specially true in a relatively short sample like the one we have (mainly 2006-2011).

3 Conclusions

In this paper we study the consequences of investor protection for control transfers in corporations. We argue that investor protection changes the relative importance of productivity and scale as two drivers of control transfers. When investor protection is strong the main driver of acquisitions is the possibility to transfer control to a more productive acquirer. Since financial constraints are relaxed for all firms when investor protection is high, the possibility to transfer control to an acquirer with a financial advantage simply to gain scale is less relevant. Also, good investor protection limits the possibility of empire-building type of acquisitions and hence increasing scale through overinvestment. Consistent with these predictions we find that, when investor protection is high, control transfers are decreasingly correlated with changes in scale, and increasingly correlated with profitability improvements. The results come from a large sample of private European firms that experienced transfers of control mainly between 2006 and 2011.

References

Almeida, Heitor, Murillo Campello, and Dirk Hackbarth, 2011, Liquidity mergers, *Journal of Financial Economics* 102, 526–558.

Almeida, Heitor, and Daniel Wolfenzon, 2006, A theory of pyramidal ownership and family business groups, *Journal of Finance* 61, 2637–2681.

Bris, Arturo, and Christos Cabolis, 2008, The value of investor protection: firm evidence from cross-border mergers, *Review of Financial Studies* 21, 605–648.

Brown, James R., Gustav Martinsson, and Bruce C. Petersen, 2013, Law, stock markets, and innovation, *Journal of Finance* 68, 1517–1549.

Burkart, Mike, Denis Gromb, Holger Mueller, and Fausto Panunzi, 2014, Legal investor protection and takeovers, *Journal of Finance* 69, 1129–1165.

Burkart, Mike, Fausto Panunzi, and Andrei Shleifer, 2003, Family firms, *Journal of Finance* 58, 2167–2201.

Caselli, Francesco, and Nicola Gennaioli, 2013, Dynastic management, *Economic Inquiry* 51, 971–996.

Davies, Paul, Edmund-Philipp Schuster, and Emilie van de Walle de Ghelcke, 2010, The takeover directive as a protectionist tool?, in Ulf U. Bernitz, and Wolf-Georg W. Ringe, ed.: *Company Law and Economic Protectionism: New Challenges to European Integration* . chap. 6 (Oxford University Press).

Devos, Erik, Palani-Rajan Kadapakkam, and Srinivasan Krishnamurthy, 2008, How do mergers create value? A comparison of taxes, market power, and efficiency improvements as explanations for synergies, *Review of Financial Studies* 22, 1179–1211.

Djankov, Simeon, Rafael La Porta, Florencio López-de-Silanes, and Andrei Shleifer, 2008, The law and economics of self-dealing, *Journal of Financial Economics* 88, 430–465.

Djankov, Simeon, Caralee McLiesh, and Andrei Shleifer, 2007, Private credit in 129 countries, *Journal of Financial Economics* 12, 77–99.

Doidge, Craige, Andrew Karolyi, and Rene Stulz, 2007, Why do countries matter so much for corporate governance?, *Journal of Financial Economics* 86, 1–39.

Dyck, Alexander, and Luigi Zingales, 2004, Private benefits of control: An international comparison, *Journal of Finance* 59, 533–596.

Enriques, Luca, and Paolo Volpin, 2007, Corporate governance reform in continental europe, *Journal of Economic Perspectives* 21, 117–140.

Erel, Isil, Yeejin Jang, and Michael S. Weisbach, 2015, Do acquisitions relieve target firms' financial constraints?, *Journal of Finance* 70, 289–328.

Foley, Fritz, and Robin Greenwood, 2010, The evolution of corporate ownership after IPO: The impact of investor protection, *Review of Financial Studies* 23, 1231–1260.

Fuller, Kathleen, Jeffry Netter, and Mike Stegemoller, 2002, What do returns of acquiring firms tell us? Evidence from firms that make many acquisitions, *Journal of Finance* 57, 1763–1793.

Grossman, Sanford J., and Oliver D. Hart, 1980, Takeover bids, the free-rider problem, and the theory of the corporation, *The Bell Journal of Economics* 11, 42–64.

Hadlock, C., and J. Pierce, 2010, New evidence on measuring financial constraints: moving beyond the KZ index, *Review of Financial Studies* 23, 1909–1940.

Jensen, Michael C., 1986, Agency costs of free cash flow, corporate finance, and takeovers, *American Economic Review* 76, 323–329.

La Porta, Rafael, Florencio López-de-Silanes, Andrei Shleifer, and Robert Vishny, 1997, Legal determinants of external finance, *Journal of Finance* 52, 1131–1150.

——— , 1998, Law and finance, *Journal of Political Economy* 106, 1113–1155.

——— , 2002, Investor protection and corporate valuation, *Journal of Finance* 57, 1147–1170.

Li, Xiaoyang, 2013, Productivity, restructuring, and the gains from takeovers, *Journal of Financial Economics* 109, 250–271.

Liao, Rose C., 2014, What drives corporate minority acquisitions around the world? The case for financial constraints, *Journal of Corporate Finance* 26, 78–95.

Maksimovic, Vojislav, and Gordon Phillips, 2001, The market for corporate assets: Who engages in mergers and asset sales and are there efficiency gains, *Journal of Finance* 56, 2019–2065.

Mclean, David, Tianyu Zhang, and Mengxin Zhao, 2012, Why does the law matter? Investor protection and its effects on investment, finance, and growth, *Journal of Finance* 67, 313–350.

Netter, Jeffry, Mike Stegemoller, and M. Babajide Wintoki, 2011, Implications of data screens on merger and acquisition analysis: A large sample study of mergers and acquisitions from 1992 to 2009, *Review of Financial Studies* 24, 2316–2357.

Pástor, Luboš, Lucian Taylor, and Pietro Veronesi, 2009, Entrepreneurial learning, the IPO decision, and the post-IPO drop in firm profitability, *Review of Financial Studies* 22, 3005–3046.

Pérez-González, Francisco, 2007, The impact of acquiring control on productivity, Working paper Stanford University.

Rajan, Raghuram, and Luigi Zingales, 1998, Financial dependence and growth, *American Economic Review* 88, 559–586.

Rossi, Stefano, and Paolo Volpin, 2004, Cross-country determinants of mergers and acquisitions, *Journal of Financial Economics* 74, 277–304.

Schoar, Antoinette, 2002, The effect of diversification on firm productivity, *Journal of Finance* 62, 2379–2403.

Shleifer, Andrei, and Daniel Wolfenzon, 2002, Investor protection and equity markets, *Journal of Financial Economics* 66, 3–27.

Tirole, Jean, 2006, *The Theory of Corporate Finance* (Princeton University Press: Princeton, NJ).

Wooldridge, Jeffrey, 2002, *Econometric Analysis of Cross Section and Panel Data* (The MIT Press: Cambridge, MA).

Wurgler, Jeffrey, 2000, Financial markets and the allocation of capital, *Journal of Financial Economics* 58, 187–214.

Table 1
Country-level Characteristics

The table shows the number of control transfers for a sample of European private targets between 1997 and 2012. The table also shows country characteristics such as the anti-self-dealing index that measures investor protection, creditor rights and GDP per capita (log).

Country	Number of Deals	Investor Protection (Anti-Self Dealing)	Creditor Rights	GDP per Capita (log)
Austria	156	0.21	3	10.08
Belgium	384	0.54	2	10.01
Bulgaria	72	0.65	2	7.36
Croatia	42	0.25	3	8.34
Czech Republic	182	0.33	3	8.52
Denmark	87	0.46	3	10.3
Finland	541	0.46	1	10.05
France	977	0.38	0	10.01
Germany	703	0.28	3	10.03
Greece	28	0.22	1	9.24
Hungary	102	0.18	1	8.45
Iceland	9	0.26		10.3
Ireland	89	0.79	1	10.12
Italy	509	0.42	2	9.83
Latvia	64	0.32	3	8.01
Lithuania	48	0.36	2	8.09
Luxemburg	19	0.28		10.71
Netherlands	653	0.2	3	10.06
Norway	555	0.42	2	10.52
Poland	323	0.29	1	8.37
Portugal	133	0.44	1	9.25
Romania	128	0.44	2	7.41
Russia	1,186	0.44	2	7.49
Slovakia	37	0.29		8.23
Spain	696	0.37	2	9.54
Sweden	731	0.33	1	10.2
Turkey	1	0.43	2	7.99
Ukraine	210	0.08	2	6.45
United Kingdom	2,324	0.95	4	10.1
Total	10,989	0.49	2.24	9.52

Table 2
Target and Deal Characteristics by Year

The table shows the annual frequency of control transfers for a sample of European private targets between 1997 and 2012. The table also shows the mean (median) of targets' assets in thousands of Euros, the mean of targets' age in years, as well as the initial stake (pre-acquisition) and the final stake (post-acquisition) the acquirer has in the target.

	Number of Deals	Target's Assets before Acquisition (EUR '000)		Target's Age before Acquisition (Years)	Initial Stake (%)	Final Stake (%)
		Mean	Median	Mean	Mean	Mean
1997-2004	234	45,797	3,883	28.5	2.9	94.0
2005	927	48,280	4,494	26.4	3.5	94.8
2006	1,421	45,261	3,667	24.5	2.4	95.8
2007	1,859	30,590	3,791	24.0	2.7	95.4
2008	1,703	26,499	3,556	22.6	2.1	95.6
2009	1,373	39,778	2,959	22.1	3.0	93.9
2010	1,713	35,927	3,106	21.0	2.7	94.6
2011	1,577	41,033	2,897	19.5	2.3	96.2
2012	182	21,644	1,074	16.3	1.4	98.2
Total	10,989	36,992	3,386	22.7	2.6	95.3

Table 3
Summary Statistics for Transfers of Control

The table shows summary statistics for a sample of European private targets between 1997 and 2012. For each target we compute the average of each characteristic (ROA, assets, etc.) after and before the deal, and then take the difference between these two averages (after minus before). The Table shows the mean and the standard deviation of this difference for all observations, and then for subsamples in countries with high and low investor protection. High and low investor protection are defined according to the average investor protection in Table 1. Firm characteristics include return over assets (ROA) defined as EBITDA over assets; the log of total assets (Assets); sales (both in log and as a fraction of assets); cost of goods sold and other operating expenses (both in log and as a fraction of sales); fixed and current assets (both in log and as a fraction of total assets); and leverage, which we define as debt over assets.

	Full Sample		High Investor Protection (a)		Low Investor Protection (b)		(a) - (b)	(a) - (b)
After-minus-Before Changes	Mean	SD	Mean	SD	Mean	SD	Mean dif.	p-values
ROA	-0.036	0.188	-0.022	0.207	-0.039	0.183	0.017	0.01
Assets (log)	0.066	1.979	-0.544	2.418	0.255	1.749	-0.799	0.00
Sales (log)	0.128	1.503	0.002	1.192	0.148	1.552	-0.145	0.00
Sales over Total Assets	-0.161	1.165	0.011	1.419	-0.197	1.104	0.208	0.00
Cost of Goods Sold (log)	0.306	1.514	0.077	1.289	0.421	1.609	-0.344	0.00
Cost of Goods Sold over Sales	-0.018	0.161	0.000	0.145	-0.028	0.169	0.027	0.00
Other Operating Expenses (log)	0.293	1.563	-0.088	1.481	0.570	1.574	-0.658	0.00
Other Operating Expenses over Sales	0.003	0.193	-0.017	0.213	0.017	0.178	-0.034	0.00
Fixed Assets (log)	-0.107	2.012	-0.676	1.841	0.110	2.036	-0.786	0.00
Fixed Assets over Total Assets	-0.009	0.200	-0.033	0.197	-0.001	0.201	-0.031	0.00
Current Assets (log)	0.268	1.566	0.003	2.172	0.355	1.276	-0.352	0.00
Current Assets over Total Assets	0.019	0.210	0.064	0.218	0.004	0.205	0.060	0.00
Leverage	-0.052	0.258	-0.129	0.287	-0.027	0.243	-0.102	0.00

Table 4

Types of Deals in High and Low Investor Protection Countries

The table shows the number of deals, their frequency and characteristics for a sample of European private targets between 1997 and 2012. The table splits deals in high and low investor protection countries (of targets), and also according to deal outcomes: deals with increases or decreases in ROA or assets relative to before the deal. Change in ROA (assets) is the difference in average ROA (assets) after minus before the deal. ROA and assets are defined as in Table 3.

	High Investor Protection				Low Investor Protection				High-Low Investor	
	Number of deals	Freq.	Change in ROA	Change in Assets (log)	Number of deals	Freq.	Change in ROA	Change in Assets (log)	Change in ROA	Change in Assets (log)
By ROA:										
Increase in ROA	469	0.42	0.134	-0.705	2238	0.38	0.112	0.238	0.022	-0.94
Decrease in ROA	636	0.58	-0.137	-0.76	3591	0.62	-0.132	0.24	-0.005	-1.00
By Assets:										
Increase in Assets	464	0.42	-0.015	0.73	3527	0.61	-0.035	0.99	0.020	-0.25
Decrease in Assets	641	0.58	-0.027	-1.80	2302	0.39	-0.044	-0.91	0.017	-0.89

Table 5

Investor Protection and the Consequences of Control Transfers: Panel Regressions

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. Dependent variables are return on assets (ROA), and assets (log) as defined in Table 3. Independent variables are a dummy that takes the value of one after the transfer of control and the interaction between this dummy and investor protection in the target's country proxied with the anti-self-dealing index developed by Djankov et al. (2008). All regressions include firm and year fixed effects (not reported). Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	ROA		Assets (log)	
	(1)	(2)	(3)	(4)
Control	-0.013***	-0.030***	0.101***	0.464***
	(0.005)	(0.007)	(0.018)	(0.127)
Control x Investor Protection		**0.038*** **		**-0.744*** **
		(0.014)		**(0.147)**
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	67,260	66,174	100,228	98,959
R-squared	0.018	0.019	0.014	0.018

Table 6
Other Country Characteristics and the Consequences of Control Transfers

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. Dependent variables are return on assets (ROA) assets (log). Independent variables are a dummy that takes the value of one after the transfer of control and the interaction between this dummy and the following variables: investor protection proxied with the anti-self-dealing index developed by Djankov et al. (2008), GDP per capita (log), a proxy for accounting quality, a dummy for the period after the adoption of IFRS in each country, and proxies for government effectiveness, regulatory quality and rule of law from the World Bank. All regressions include firm and year fixed effects (not reported). Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	ROA				Assets (log)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Control	-0.036	-0.021	-0.024**	-0.027***	3.202***	2.299***	0.890*	0.621***
	(0.029)	(0.021)	(0.011)	(0.005)	(1.101)	(0.590)	(0.442)	(0.177)
Control x Investor Protection	**0.037****	**0.040****	**0.038****	**0.038****	**-0.391****	**-0.441*****	**-0.730*****	**-0.614*****
	(0.015)	**(0.016)**	**(0.014)**	**(0.014)**	**(0.162)**	**(0.144)**	**(0.140)**	**(0.198)**
Control x GDP per Capita (log)	**0.001**				**-0.306****			
	(0.003)				**(0.114)**			
Control x Accounting Quality		**-0.002**				**-0.373*****		
		(0.005)				**(0.114)**		
Control x IFRS Adoption			**-0.008**				**-0.493**	
			(0.009)				**(0.472)**	
Control x Government Effectiveness				**-0.017**				**-0.126**
				(0.015)				**(0.529)**
Control x Regulatory Quality				**0.008**				**0.190**
				(0.014)				**(0.266)**
Control x Rule of Law				**0.008**				**-0.256**
				(0.018)				**(0.421)**
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	66,174	66,174	66,174	65,003	98,959	98,959	98,959	97,344
R-squared	0.019	0.019	0.019	0.018	0.030	0.027	0.021	0.025

Table 7
Other Financial Characteristics and the Consequences of Control Transfers

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. Dependent variables are return on assets (ROA) assets (log). Independent variables are a dummy that takes the value of one after the transfer of control and the interaction between this dummy and the following variables: an index for Board Neutrality Rule from Davies, Schuster and van de Walle de Ghelcke (2010) following Europe's Takeover Directive in 2004, a proxy for creditor rights from Djankov, McLiesh, and Shleifer (2007), and a set of dummy variables that take the value of one for countries with Common law, Scandinavian, and German legal origin respectively (French legal origin is the excluded category). The instrumented version of investor protection corresponds to a 2SLS regression where the previous dummy variables for legal origin are instruments for the anti-self-dealing index. All regressions include firm and year fixed effects (not reported). Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	ROA				Assets (log)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Control	-0.020***	-0.028***	-0.017***	-0.037***	-0.265	0.310*	0.283*	0.473***
	(0.007)	(0.007)	(0.006)	(0.007)	(0.287)	(0.173)	(0.156)	(0.140)
Control x Board Neutrality Rule	**0.003**				**0.126**			
	(0.002)				**(0.102)**			
Control x Creditor Rights		**0.008****				**-0.092**		
		(0.003)				**(0.059)**		
Control x Common Law Dummy			**0.032*****				**-0.528****	
			(0.004)				**(0.215)**	
Control x Scandinavian Law Dummy			**-0.001**				**-0.316**	
			(0.010)				**(0.232)**	
Control x German Law Dummy			**0.001**				**-0.063**	
			(0.008)				**(0.277)**	
Control x Instrumented Investor Protection				**0.054*****				**-0.772*****
				(0.007)				**(0.214)**
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58,810	65,959	66,174	66,136	90,242	98,630	98,959	99,575
R-squared	0.017	0.019	0.019	0.019	0.010	0.016	0.020	0.018

Table 8

Decomposing the Effects of Control Transfers on Productivity, Scale, and Capital Structure

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. Dependent variables are sales, cost of goods sold, other operating expenses, fixed assets, current assets (all in log), and leverage as defined in Table 3. Independent variables are a dummy that takes the value of one after the transfer of control and the interaction between this dummy and investor protection proxied with the anti-self-dealing index developed by Djankov et al. (2008). All regressions include firm and year fixed effects (not reported). Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	Decomposing EBITDA			Decomposing Assets		Capital Structure
	Sales (log)	Cost of Goods Sold (log)	Other Operating Expenses (log)	Fixed Assets (log)	Current Assets (log)	Leverage
	(1)	(2)	(3)	(4)	(5)	(6)
Control	0.059	0.247***	0.606**	0.394***	0.308**	0.066**
	(0.070)	(0.060)	(0.216)	(0.103)	(0.117)	(0.026)
Control x Investor Protection	**-0.106**	**-0.330*** **	**-0.825*** **	**-0.835*** **	**-0.407** **	**-0.171*** **
	(0.121)	**(0.086)**	**(0.246)**	**(0.244)**	**(0.150)**	**(0.030)**
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	75,252	22,541	24,724	64,120	98,487	86,642
R-squared	0.009	0.035	0.041	0.022	0.025	0.043

Table 9
Regressions with Acquirer-Target Relative Size and Profitability

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. Dependent variables are return on assets (ROA) and assets (log) as defined in Table 3. Independent variables are a dummy that takes the value of one for control acquisitions after the deal, the interaction between this dummy and investor protection proxied with the anti-self-dealing index developed by Djankov et al. (2008); the acquirer-target relative size (profitability), which we measure as the difference between the acquirer's assets (ROA) and the target's assets (ROA) before the transfer of control. All regressions include firm and year fixed effects (not reported). Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	ROA			Assets (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
Control	-0.005	-0.022*	-0.020	-0.091**	0.360**	0.222
	(0.004)	(0.012)	(0.012)	(0.038)	(0.165)	(0.183)
Control x Relative Size	**-0.001**	**-0.001**	**-0.002**	**0.078*** **	**0.082*** **	**0.132*** **
	(0.001)	**(0.001)**	**(0.003)**	**(0.012)**	**(0.011)**	**(0.016)**
Control x Relative Profitability	**0.180*** **	**0.180*** **	**0.207*** **	**-0.228****	**-0.225****	**-0.494****
	(0.013)	**(0.013)**	**(0.030)**	**(0.105)**	**(0.108)**	**(0.221)**
Control x Investor Protection		**0.041**	**0.035**		**-1.050*** **	**-0.733***
		(0.027)	**(0.028)**		**(0.339)**	**(0.424)**
Control x Relative Size x Investor Protection			**0.001**			**-0.113****
			(0.005)			**(0.044)**
Control x Relative Profitability x Investor Protection			**-0.064**			**0.569***
			(0.069)			**(0.306)**
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	47,155	47,155	47,155	57,699	57,699	57,699
R-squared	0.040	0.040	0.040	0.027	0.032	0.032

Table 10
Investor Protection in the Acquirer's Country

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. Dependent variables are return on assets (ROA) and assets (log) as defined in Table 3. Independent variables are a dummy that takes the value of one after the transfer of control and the interaction between this dummy and investor protection in the target's country proxied with the anti-self-dealing index developed by Djankov et al. (2008), the investor protection from the acquirer's country and the difference between the target's and the acquirer's investor protection. Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	ROA			Assets (log)		
	(1)	(2)	(3)	(4)	(5)	(6)
Control	-0.022**	-0.013**	-0.022**	0.479***	0.096***	0.479***
	(0.009)	(0.005)	(0.009)	(0.170)	(0.024)	(0.170)
Control x Target's Investor Protection	**0.048*** (0.016)**		**0.021 (0.019)**	**-0.794** (0.338)**		**-0.790*** (0.203)**
Control x Acquirer's Investor Protection	**-0.027** (0.012)**			**0.004 (0.393)**		
Control x Difference in Investor Protection		**0.035*** (0.011)**	**0.027** (0.012)**		**-0.375 (0.295)**	**-0.004 (0.393)**
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	59,946	59,946	59,946	89,143	89,143	89,143
R-squared	0.020	0.020	0.020	0.019	0.015	0.019

Table 11
Diversifying Deals

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. In the Table we split the sample based on whether the deals are diversifying or not, which we define as those where the target and the acquirer are from different 2-digit SIC codes. Dependent variables are return on assets (ROA) and assets (log) as defined in Table 3. Independent variables are a dummy that takes the value of one after the transfer of control and the interaction between this dummy and investor protection proxied with the anti-self-dealing index developed by Djankov et al. (2008), as well as a dummy for diversifying deals. All regressions include firm and year fixed effects (not reported). Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	ROA			Assets (log)		
	Type of Deal			Type of Deal		
	Diversifying	Non-Diversifying	All	Diversifying	Non-Diversifying	All
	(1)	(2)	(3)	(4)	(5)	(6)
Control	-0.029***	-0.031***	-0.028***	0.471***	0.434***	0.388***
	(0.008)	(0.010)	(0.010)	(0.139)	(0.116)	(0.124)
Control x Investor Protection	**0.039*** **	**0.036***	**0.036***	**-0.657*** **	**-0.784*** **	**-0.788*** **
	(0.012)	**(0.018)**	**(0.018)**	**(0.162)**	**(0.142)**	**(0.141)**
Control x Diversifying Acquisitions			**-0.004**			**0.125**
			(0.009)			**(0.082)**
Control x Investor Protection x Diversifying Acquisitions			**0.003**			**0.136**
			(0.014)			**(0.101)**
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	35,553	30,621	66,174	51,184	47,775	98,959
R-squared	0.021	0.016	0.019	0.020	0.019	0.019

Table 12
Serial Acquirers

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. The Table splits the sample between deals with serial acquirers, which we defined as those acquirers that make more than one acquisition during our sample period. Dependent variables are return on assets (ROA) and assets (log) as defined in Table 3. Independent variables are a dummy that takes the value of one after the transfer of control and the interaction between this dummy and investor protection proxied with the anti-self-dealing index developed by Djankov et al. (2008). All regressions include firm and year fixed effects (not reported). Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	ROA		Assets (log)	
	Serial Acquirers		Serial Acquirers	
	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Control	-0.028***	-0.040**	0.428***	0.563***
	(0.007)	(0.015)	(0.104)	(0.187)
Control x Investor Protection	**0.027****	**0.071****	**-0.653*****	**-1.006*****
	(0.010)	**(0.027)**	**(0.123)**	**(0.230)**
Target Fixed Effects	Yes	Yes	Yes	Yes
Acquirer Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	45,566	20,608	69,835	29,124
R-squared	0.017	0.026	0.016	0.026

Table 13
Ownership Structure of Acquirers and Targets

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. The Table splits the sample based on acquirers listed status and also on whether acquirers and targets have controlling shareholders, which we define as having a shareholder with more than 50% (20%) of the shares in private (listed) firms. Dependent variables are return on assets (ROA) and assets (log) as defined in Table 3. Independent variables are a dummy that takes the value of one after the transfer of control and the interaction between this dummy and investor protection proxied with the anti-self-dealing index developed by Djankov et al. (2008). All regressions include firm and year fixed effects (not reported). Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	ROA						Assets (log)					
	Listed Acquirer		Target w/Controlling Shareholder		Acquirer w/Controlling Shareholder		Listed Acquirer		Target w/Controlling Shareholder		Acquirer w/Controlling Shareholder	
	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(9)	(10)	(11)	(12)	(13)	(14)
Control	-0.038***	-0.028***	-0.020**	-0.030***	-0.016*	-0.036***	0.582***	0.424***	0.322***	0.496***	0.498***	0.454***
	(0.011)	(0.008)	(0.008)	(0.007)	(0.009)	(0.011)	(0.119)	(0.128)	(0.091)	(0.112)	(0.114)	(0.082)
Control x Investor Protection	**0.083*** **	**0.026** **	**0.024*** **	**0.039** **	**0.016**	**0.041*** **	**-0.831*** **	**-0.714*** **	**-0.517*** **	**-0.693** **	**-0.855*** **	**-0.615*** **
	(0.027)	**(0.011)**	**(0.007)**	**(0.014)**	**(0.017)**	**(0.013)**	**(0.209)**	**(0.161)**	**(0.158)**	**(0.241)**	**(0.174)**	**(0.144)**
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,261	56,913	19,905	12,905	15,013	10,073	13,800	85,159	33,401	22,215	24,138	16,362
R-squared	0.013	0.020	0.016	0.014	0.018	0.021	0.025	0.018	0.039	0.038	0.037	0.051

Table 14
Robustness Checks

The table shows fixed effect panel regressions for a sample of European private targets between 1997 and 2012. Dependent variables are return on assets (ROA) and assets (log) as defined in Table 3. Independent variables are a dummy that takes the value of one after the transfer of control and the interaction between this dummy and investor protection proxied with the anti-self-dealing index developed by Djankov et al. (2008). The Table shows regressions with industry-year (Columns 1-2) and country-year dummies (Columns 3-4). All regressions include firm fixed effects (not reported). Standard errors are robust and clustered at the country level. Significance: * 10%, ** 5%, *** 1%.

	Industry-Year Dummies		Country-Year Dummies	
	ROA	Assets (log)	ROA	Assets (log)
	(1)	(2)	(3)	(4)
Control	-0.032***	0.344***	-0.026***	0.109***
	(0.007)	(0.097)	(0.008)	(0.031)
Control x Investor Protection	**0.041*** **	**-0.492*** **	**0.028***	**-0.018**
	(0.014)	**(0.154)**	**(0.015)**	**(0.043)**
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	65,046	95,191	66,174	98,959
R-squared	0.024	0.024	0.026	0.250

Table A.1
Number of Deals in the Sample after Cleaning and Matching *Zephyr* with *Amadeus*

Zephyr	
Total number of deals	145,834
Deals where both the target and acquirer have Bureau van Dijk identifiers	106,481
Deals where we know the stake acquired and the final stake	81,900
Deals with targets' accounting data	46,239
Deals with both target and acquirers' accounting data	33,060
Matching with Amadeus	
Number of deals matched	21,904
Deals for which we have accounting data both before and after the deal	10,989