ESSAYS ON EMPIRICAL CORPORATE FINANCE

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ABSTRACT

Given the growing economic and strategic importance of human capital, examining the relationship between firms and labor is crucial for academics, practitioners and policymakers. Unlike capital, labor has unique features. Employees not only make strategic decisions and allocate or withdraw effort, but they also have the freedom to move across firms. This mobility, along with the strategic roles employees play, contrasts with the more static nature of capital. Moreover, a unique set of frictions induced by employment protection laws is present in labor markets, which in turn impacts firms' decisions and their behaviors in the product market.

The labor and finance literature starts with recognizing the interdependency between firms and labor. On the one hand, firm decisions can significantly affect their workers. For instance, financing projects with excessive debt may heighten the risk of financial distress, potentially leading to layoffs. Additionally, firms' decisions to invest in technology can directly affect workers by altering the demand for labor and changing compensation structures. On the other hand, labor market attributes also influence firm decisions, affecting product market behavior and impeding their capacity to expand. In this thesis, I include three empirical essays that provide new insights into the intersection of labor and finance from different angles.

Chapter 1 provides insights into how corporate social responsibility (CSR) affects firms' behavior, with a focus on the interaction of labor and CSR. Using a unique panel of target firms in European countries, I investigate post-merger employment policies of socially responsible firms. Surprisingly, I find that acquirers with greater CSR performance are more likely to lay off employees in target firms. My findings are primarily driven by the Social

component of the CSR rating. I further document a positive impact of acquirers' social performance on target firms' labor productivity, technical efficiency, and staff costs. In addition, I show that socially responsible firms enjoy higher announcement returns, especially when they do more layoffs. These results are consistent with the cost-saving channel: higher labor costs induced by the implementation of CSR policies decrease the optimal level of employment in acquired targets. Overall, my paper suggests that socially responsible firms do not lose sight of value maximization when making layoff decisions.

Chapter 2 contributes to the literature on business groups by connecting it to labor market issues. In particular, we provide evidence that business groups are partially insulated from the impact of employment protection legislation due to their ability to operate internal labor markets. A major 2012 reform lowering dismissal costs in Spain stimulated employment growth in stand-alone firms significantly more than in comparable group-affiliated firms. Response to the reform was most muted in group firms that were in a position to better access their internal labor market, e.g., due to geographic proximity to their affiliates. We also provide causal evidence that group affiliation became less pervasive in Spain following the reform, in line with longstanding claims that labor market frictions shape the organizational structure and favor the emergence of business groups.

Chapter 3 examines how ownership changes in response to the presence of labor market issues. We provide evidence suggesting that common owners play an important role in bypassing frictions to employee mobility that hinder information sharing across firms. Based on this, we introduce a different perspective to the literature and ask: What are the driving forces behind endogenous common ownership? We study the effects of knowledge protection on common ownership by exploiting the staggered adoption of the Inevitable Disclosure Doctrine (IDD), which is a trade secret law that limits knowledgeable workers' mobility across firms. We find that the recognition of IDD causes an increase in firm-level common ownership, suggesting that common ownership helps overcome labor market frictions via facilitating information flows. The effect is mainly led by long-term investors, and it is stronger for firms that rely more on human capital and for firms with more mobile employees. Our findings shed light on the efficiency-improving hypothesis of common ownership - in the presence of IDD, an exogenous increase in common ownership can help firms conduct more innovation activities and enjoy better operating performance. In summary, this paper provides direct evidence that the need for information sharing serves as an important motive for the emergence of common ownership.

Chapter 1

Corporate Social Responsibility and Post-merger Labor Restructuring

1.1 Introduction

Corporate social responsibility (CSR) has received increasing attention from investors, corporate executives, researchers, and policymakers during the last two decades. According to a recent survey by KPMG (2020), 96% of the world's largest 250 companies now report CSR activities, which is up from 35% in 1999. Prior literature considers CSR engagement as a stakeholder-oriented behavior, which reflects a commitment to behave ethically and to invest in activities that benefit various stakeholders (McWilliams and Siegel, 2001; Edmans, 2011; Deng et al., 2013; Guiso et al., 2015; Flammer, 2015). The debate over CSR revolves around whether such activities are value-enhancing or whether they are the value-destroying manifestation of agency conflicts.[1] For a more in-depth insight into CSR, a natural question arises: How does CSR affect firms' behavior? In this paper, I shed light on this question by studying the post-merger labor restructuring decisions of acquirers with varying degrees of CSR engagement.

[1]A large number of studies provide evidence that CSR can enhance firm value: firms with higher employee satisfaction realize superior long-term stock returns (Edmans, 2011); the adoption of CSR proposals improves firms' labor productivity and sales growth (Flammer, 2015); high CSR firms enjoy a lower cost of capital (El Ghoul et al., 2011; Chava, 2014; Gao et al., 2021); and perform better during financial crises (Lins et al., 2017). In contrast, others such as Cheng et al. (2013), Krüger (2015), Masulis and Reza (2015), and Cai et al. (2021) view CSR activities as the result of agency problems within the firm.

As one of the most important corporate investment decisions, mergers and acquisitions (M&A) offers an excellent platform to better understand the nature of CSR activities. This is because M&As are largely unanticipated events that can potentially mitigate reverse causality problems (Deng et al., 2013). While there is considerable research on the links between CSR and M&A, most of it examines the impact of CSR on M&A performance (e.g., Deng et al., 2013; Gomes and Marsat, 2018; Arouri et al., 2019), paying little attention to the issue of how social performance influences post-merger strategies in target firms.[2]

In this study, I construct a unique panel of target firms and investigate whether socially responsible acquirers manage targets differently after acquisitions. By focusing on post-acquisition restructuring strategies, I can potentially avoid some endogeneity concerns that are common to the literature (e.g., Deng et al., 2013; Flammer, 2015; Lins et al., 2017).[3] Given the centrality of human capital, the restructuring process after acquisitions inevitably involves decisions associated with the workforce of target firms. Previous studies suggest that corporate mergers could "hurt" workers, documenting a significant decline in employment within target firms after acquisitions (Li, 2013; Dessaint et al., 2017; Lagaras, 2020; Gehrke et al., 2021).[4] Thus, I focus on employees – a crucial group of internal stakeholders and arguably the firms' most valuable asset – and conjecture that the way a company treats its stakeholders (e.g., CSR) should play a role in post-merger labor restructuring. This raises the following question: Are socially responsible acquirers more likely to protect targets' employees from restructuring after acquisitions?

According to the different views on CSR, the relationship between CSR and post-merger restructuring is ambiguous. Socially responsible acquirers may engage in less post-merger labor restructuring due to two very different reasons. Under the *agency view*, CSR can be a manifestation of agency problems (Bénabou and Tirole, 2010; Cheng et al., 2013; Masulis

[2]One potential reason this question has not been investigated is deficient data. First, most target firms are private firms. Second, in the United States, acquirers often integrate targets with their existing assets, and thus it is hard to observe financial statements of targets both before and after acquisitions. To overcome these obstacles, I study a unique sample of private firms in Europe and use the Amadeus database in this paper.

[3]This is a question that is different from *"Does CSR affect corporate performance?"*, which presumably suffers more from endogeneity problems. For example, it is possible that only well-performing firms can afford to engage in CSR activities, which is commonly referred to as "doing good by doing well".

[4]This is because eliminating occupational overlap is often the key channel to obtaining synergy gains.

and Reza, 2015): investments in CSR are made to satisfy management's personal preferences at the expense of shareholders. Thus, managers in high CSR acquirers are more likely to overinvest to "build empires", and more employment growth after acquisitions should be expected. Furthermore, inefficient managers can strategically engage in socially responsible activities and protect employees from restructuring as an entrenchment strategy (Cespa and Cestone, 2007). Alternatively, CSR engagement can be viewed as a not-for-profit (i.e., genuinely altruistic) behavior (Bénabou and Tirole, 2010; Borghesi et al., 2014; Liang and Renneboog, 2020). Managers (and their companies) may personally believe that they have a moral obligation to engage in CSR activities. When a firm commits to social good, it fosters a corporate culture of trust and cooperation that takes into account the social, environmental, and other externalized impacts of its decisions (Hoi et al., 2013; Gao et al., 2014). Such an altruistic motive would likely encourage firms to care more about their stakeholders (e.g., employees) and limit downsizing decisions.[5]

By contrast, a cost-saving argument would predict that CSR engagement promotes labor restructuring after takeovers. CSR activities entail substantial costs, many of which are employee-related (Accenture and UNGC, 2010). Expenses aimed to improve the work-life balance (e.g., childcare, flexitime), health and safety, and employee involvement, add up to the wage bill, increasing the labor costs per employee. Hence, costs per worker in target firms are likely to increase once they are acquired and managed by socially responsible acquirers, which in turn decreases the optimal level of employment in targets (See Figure 1.1 for an illustration). In line with this view, Liang et al. (2020) argue that when acquirers' employment policies are more generous, cost savings from eliminating overlapping or redundant workers are greater, leading to higher announcement returns. If this is the case, high CSR acquirers will have greater incentives to operate larger employee layoffs in target firms, especially for the redundant or overlapping workforce.

Whether socially responsible acquirers are more or less likely to restructure the target's labor force is ultimately an empirical question. To test this question, I use data from European countries for the 2000-2018 period. The sample in this paper is from the combination of two

[5] Matsa and Miller (2013, 2014) show that women-owned companies undertake fewer workforce reductions, increasing relative labor costs, and they argue that female leaders may be more stakeholder-oriented and altruistic.

datasets, *Zephyr* and *Amadeus*, which provide detailed M&A information and give access to financial data on European private firms. The unique feature of these databases is that I can observe acquired firms (including private firms) both before and after the deal. Moreover, I obtain data on CSR ratings from the Refinitiv ESG database, which covers more than 10,000 publicly listed companies worldwide. The panel structure of the data enables me to include target firm fixed effects, thereby controlling for all time-invariant characteristics at the firm level. I compare the employment levels of target firms before and after the acquisition, and investigate how this interplay is related to the acquirers' CSR performance.

Using a sample of 921 target firms from 14 European countries, I find that acquirers with superior CSR performance are *more* likely to lay off employees in target firms after the acquisition. The results are economically significant: a one-point increase in the CSR rating (with a standard deviation of 0.71) is associated with a decrease in the target's post-merger employment of 10%. This finding holds after controlling for various target, acquirer, and deal-level characteristics. One concern is that recent evidence indicates that CSR activities are often adopted by firms with good governance or with greater institutional ownership (Ferrell et al., 2016; Dyck et al., 2019; Chen et al., 2020b). Therefore, I also provide evidence that my findings persist after controlling for factors related to corporate governance and institutional ownership. As an additional test, I use the country's legal origin as a proxy for the acquirer's CSR level. I find that acquirers from Scandinavian countries operate larger employee layoffs in target firms, when compared with other acquirers.[6] This confirms my main finding that socially responsible firms are more prone to engage in post-acquisition labor restructuring in target firms.

I next explore the mechanisms underlying the documented effects of CSR, focusing first on the cost-saving motive. I show that my results are mainly driven by the *Social* score, which covers a firm's relationship with its employees, and less so by the *Environmental* score. More importantly, I find that the acquiring firm's CSR policies providing monetary benefits to employees (e.g., monetary CSR dummy, acquirers' staff benefits) have a negative effect on the post-merger employment of target firms. These findings are consistent with my

[6] Liang and Renneboog (2017) find that a firm's CSR contribution and its country's legal origin are strongly correlated and firms from the Scandinavian legal regime obtain the highest scores on most of the CSR ratings.

conjecture that greater CSR policies, by increasing the cost per worker, lead acquirers to do more layoffs following the acquisition.

To provide further evidence of the cost-saving motive, I focus on the *Social* score and apply a triple difference-in-differences approach. I first investigate whether the relation is more pronounced for firms acquiring targets in highly-skilled industries. Employee-related CSR programs are likely to be more "expensive" in these industries, thus inducing higher labor costs in target firms after takeovers. Building on this conjecture, I indeed find that the effect of *Social* score on employment mainly comes from human-capital-intensive industries. Second, I examine the targets that are more financially constrained, for which the cost-saving motive is more relevant. As expected, my results are stronger for targets in financially dependent industries and targets with more cash holdings. Third, I examine whether my findings are affected by deal types. If cost savings from laying off redundant or overlapping employees are larger for high CSR acquirers, more pronounced results should be expected for same-industry or domestic deals, which have more opportunities for eliminating redundancy.[7] Consistent with this logic, I find that the relation between the *Social* score and employment is more pronounced for the same-industry and domestic deals. Finally, I show that my results are also stronger for targets with more inefficient employees, as redundant workers are more likely in firms with lower labor productivity. In sum, I conclude that these results provide evidence in support of the cost-saving view.

I then turn to investigate how acquirers' social performance affects other target firms' outcome variables. My findings suggest that targets acquired by acquirers with greater social performance experience higher staff costs (which include not only wages but also other monetary benefits) after M&As. These results indirectly support my cost-saving argument: higher post-acquisition labor costs, driven by socially responsible acquirers, increase the likelihood of targets laying off employees. Further, I document a positive effect of social performance on the target firm's labor productivity and technical efficiency, which is also in line with my argument that socially responsible firms have greater incentives to fire redundant or overlapping workforce. However, given that firm resources are allocated towards CSR

[7] By contrast, in cross-industries or cross-countries deals, opportunities for eliminating overlap could be limited due to skill gaps and geographical distance.

activities after acquisitions, I find some evidence that these social accomplishments might be achieved at the expense of the targets' capital expenditures.

In addition, I conduct an event study to investigate market reactions toward acquisitions by socially responsible firms. If socially responsible acquirers enjoy greater cost-saving benefits by firing more employees in target firms, the market should react more positively to deal announcements. As expected, I find that the acquirer's social performance is positively related to shareholder returns around deal announcements. In particular, I observe that socially responsible firms also enjoy higher announcement returns when they do more layoffs in target firms. Overall, these results are consistent with my main argument that acquirers with better social performance can realize greater cost-saving benefits from labor restructuring after the acquisition.

I also investigate several alternative explanations for my results but find little support for these explanations. For example, previous literature asserts that CSR performance enhances corporate reputation and social capital, gaining trust from investors and other stakeholders (Godfrey et al., 2009; Goss and Roberts, 2011; Elfenbein et al., 2012; Lins et al., 2017; Hong et al., 2019; Barrage et al., 2020). Moral capital, however, can provide insurance by moderating the negative assessment of stakeholders when firms suffer a negative event. Given the negative externalities of layoffs on various internal and external stakeholders, large-scale workforce reductions after the acquisition may incur reputational penalties. As such, CSR engagement serves to protect firms from adverse reputational consequences of corporate downsizing. In this respect, acquirers with a better CSR image may be able to engage in more post-merger layoffs. A second alternative hypothesis, the managerial entrenchment channel, argues that it is possible that engagements in CSR and protecting employees from restructuring are substitute ways of forming an alliance with stakeholders. If high CSR firms have built solid support from other stakeholders, they have less to lose from engaging in layoffs after acquisitions. However, I do not find strong evidence pointing to these two channels as major explanations for my main findings.

To further pin down my results, I perform a battery of additional tests and robustness checks. First, I incorporate subsidiary-level data into my analysis and find that when targets are acquired by a high CSR acquirer, the subsidiaries of these target firms also engage in more labor restructuring after acquisitions. Second, I address the concern that targets differ along many dimensions by showing that my results are robust to using a matched sample.[8] I match targets acquired by high *Social* acquirers with those by low *Social* acquirers on industry, country, and other control variables. My analyses of the matched sample again show that targets in the high CSR group engage in more labor restructuring after acquisitions. Third, I follow previous studies (Goss and Roberts, 2011; Cai et al., 2016; Bae et al., 2019; Cheung et al., 2020) and further address the endogeneity concern by using two sets of instrumental variables: 1) a country's egalitarian culture; 2) 5-year lagged CSR. The results from the two-stage least squares (2SLS) estimation confirm my main results. Finally, I find that my results are robust to (i) an alternative ESG database (e.g., Sustainalytics), (ii) the use of different dependent variables (e.g., employee layoffs), (iii) controlling for the acquirer's management practices, and (iv) the exclusion of US acquirers or targets in financial industries.

This study contributes to two strands of literature. First, it is related to the work on corporate social responsibility (e.g., Edmans, 2011; Flammer, 2015; Ferrell et al., 2016; Lins et al., 2017). By examining the post-merger labor restructuring decisions of socially responsible acquirers, I provide insights into how CSR affects firms' behavior. In the M&A context, existing evidence shows that CSR creates value for acquiring firms' shareholders (Deng et al., 2013), impacts bid premiums (Gomes and Marsat, 2018), and affects M&A completion uncertainty (Arouri et al., 2019). While previous studies show that CSR is associated with M&A performance, the impact of CSR on post-acquisition strategies has been relatively unexplored. My paper fills this gap by providing evidence that socially responsible acquirers manage target firms differently after acquisitions. In particular, I examine the employment policies of socially responsible acquirers.

In the context of CSR and M&As, I also answer the following questions: How do managers in socially responsible firms balance the interests of stakeholders and shareholders when making post-merger layoff decisions? Whose interests to serve first? While prior studies consider CSR as a voluntary commitment to be responsible for a broader group of stakeholders and even beyond the interests of firms (McWilliams and Siegel, 2001; Vogel, 2005), I find no evi-

[8] One specific concern is that acquisition decisions are not random, as employment dynamics may vary across targets for reasons that are unrelated to the social performance of their acquirers.

dence that high CSR firms are willing to sacrifice profits to protect workers from post-merger restructuring. By contrast, high CSR acquirers seem more prone to realize cost savings by engaging in labor restructuring after acquisitions. My findings do not support the argument that firms with great CSR performance might lose sight of value maximization (Friedman, 1970; Cheng et al., 2013; Borghesi et al., 2014; Masulis and Reza, 2015; Cai et al., 2021) and suggest that socially responsible firms also act in the best interests of their shareholders.

Second, this paper contributes to the research that examines the employment effects of mergers. Prior studies have shown that takeovers are associated with a significant decline in target firms' employment, and this employment decline reflects efficiency-seeking restructuring (Li, 2013; Dessaint et al., 2017; Lagaras, 2020; Gehrke et al., 2021). However, Geurts and Van Biesebroeck (2019) provide evidence of substantial heterogeneity and show that mergers motivated by market power experience a strong workforce reduction, but mergers motivated by efficiency gains lead to employment expansions. In this paper, I build upon the existing studies and examine one firm-specific characteristic, CSR engagement, as a determinant of labor restructuring after M&As. My study provides novel insights into how this corporate policy plays a significant role in exacerbating or mitigating workforce reductions after the M&A. To the best of my knowledge, this study is the first to investigate the interaction between acquirers' CSR performance and post-merger restructuring with a focus on employment outcomes.

The rest of the paper proceeds as follows. Section 2 presents the data and the sample construction. Empirical methodology and results are presented in Sections 3 - 5. Section 6 concludes.

1.2 Data and Summary Statistics

1.2.1 Sample selection and panel structure

My sample consists of European mergers between 2003 and 2016. The initial sample of mergers comes from *Zephyr*, which contains information on public and private deals like IPOs, M&As, acquisitions of minority stakes, and others. Accounting and employment data are accessible through the Amadeus database for public and, crucially, private firms in Europe because most European countries require all firms (private and public) to report their unconsolidated financial accounts publicly (Erel et al., 2015). I then match target firms from *Zephyr* to *Amadeus* using the common firm identifier in BvD. The match is necessary to have information on financial variables before and, particularly, after the deal. I can therefore observe target firms after the deal if they remain as independent legal entities and are not fully absorbed by acquirers.

To be included in my sample, the transactions should meet the following four selection criteria: (1) the deal was announced after 2002, and the Zephyr database contains detailed information on this transaction; (2) the acquiring firm has less than 50% of the target's shares before the deal and more than 50% after the deal; (3) the acquiring firm has data available in Refinitiv for the fiscal year before the deal; (4) the target firm has non-missing financial and employment data for at least one year before and two years after the deal (e.g., for a deal in 2010 I require employment data up to 2012).[9] These restrictions result in a final sample of 921 deals made by 586 acquiring firms. In Table A.1.1, I describe in more detail the number of deals I lose in each step of my sample construction procedure. In addition, I get year-end financial information from three years before the deal to three years after the deal. This gives me a 7-year event window from T - 3 to T + 3, where the year T is the year of the transaction for each firm.[10]

1.2.2 CSR measure

I obtain CSR data from the Refinitiv ESG database (formerly ASSET4) that has been employed in previous CSR studies (Ferrell et al., 2016; Liang and Renneboog, 2017; Dyck et al., 2021; Tsang et al., 2021). The sample includes more than 10,000 companies around the world and provides history up to the fiscal year 2002 for approximately 1,000 companies (mainly U.S. and European). All Refinitiv ESG data is refreshed on products every week, including

[9]Following Larrain et al. (2017), I also exclude all targets that participate in more than one deal during my sample periods, with different acquirers or with the same acquirer. The reason for excluding these observations is that it is difficult to pin down the effect of each deal transaction for these cases.

[10]In Table A.1.2, I also define the event window from T - 2 to T + 2, and find that my results remain the same.

the recalculation of the ESG scores. The Refinitiv ESG database evaluates a firm's ESG performance, commitment and effectiveness based on publicly reported information (e.g., annual reports, stock exchange filings, non-governmental organizations' websites, and news sources). It captures and calculates over 450 company-level ESG measures, of which a subset of 186 of the most comparable and material per industry power the overall company assessment and scoring process. Each measure goes through a careful process to standardize the information and guarantee it is comparable across the entire range of companies. These underlying measures are grouped into 10 categories that form the three pillar scores: environmental, social, and corporate governance. Following prior studies (e.g., Dyck et al., 2019; Cheung et al., 2020; Tsang et al., 2021), I compute a firm's overall CSR score by averaging the scores assigned to the environmental and social dimensions, which are closely connected with the traditional notion of CSR.

1.2.3 Summary statistics

In Panel A of Table 1.1, I present the distribution of my sample mergers according to the target industry and year. The number of mergers increases more or less monotonically until the year 2007. It then decreases significantly during the financial crisis and rebounds in 2011. Most of the targets are in manufacturing (36.08%), services (32.71%), and wholesale and retail trade (11.30%).[11] Panel B reports the characteristics and distribution of acquisitions across countries. Targets in the UK have more employees, with a mean of 591, more than eight times the targets in Denmark. The United Kingdom is also the country with more activities, with almost one-third of the deals (32.24%), followed by Germany (12.81%), Sweden (11.40%) and Spain (10.75%). More than two-thirds (73.37%) of deals are diversified and cross-border, and the vast majority (93.05%) of the acquisitions involve private targets.

Table 1.2 presents summary statistics for financial variables of the acquirers and targets for the year prior to the acquisition. Most acquisitions are small, with a median target asset size of around €15.83 million. Not surprisingly, acquirers are much larger than targets,

[11] To keep a sufficiently large number of observations, I do not exclude the targets in the financial and utility industries. However, my conclusions remain unaffected after excluding these from the sample (results are shown in the section on robustness tests).

with a mean asset size of about €34,364.02 million, compared to a mean target asset size of €210.49 million. Acquiring firms also have more employees, with a mean of 42,412, compared to the mean of 375 for the targets. Acquirers have a lower leverage ratio (mean of 0.25) than targets (mean of 0.66). Further, I divide acquirers into high and low CSR firms according to the sample median of their CSR. Firms with high CSR scores have significantly lower Tobin's q and ROA than firms with low CSR scores, suggesting that CSR engagement might be driven by agency problems (Cheng et al., 2013; Masulis and Reza, 2015). Compared to acquirers with low CSR scores, those with high CSR scores are larger in total assets, have more employees (Liang and Renneboog, 2017), maintain higher leverage, and spend more on employee expenses (although insignificantly so).[12] As for deal characteristics, I find that compared to firms with low CSR scores, firms with high CSR scores prefer to acquire larger targets, targets with lower labor productivity, and targets whose industries are different from theirs. All variables' definitions are available in Appendix A.

1.3 Empirical Methodology and Results

1.3.1 Main results

I now investigate how CSR affects acquirers' employment policies after acquisitions, and, specifically, I examine whether socially responsible acquirers engage in more or less labor restructuring in target firms. To explore the relation between the CSR performance and the post-merger employment level, I adopt a difference-in-differences design and estimate the following panel regression model:

$$Employment_{i,t} = \alpha_i + \beta_2 Post \cdot CSR_i + \gamma Post \cdot X_i + \delta_i + \zeta_t + \lambda_r + \epsilon_{i,t} \tag{1.1}$$

Where *CSR* is the log of acquirer's initial CSR score (measured in the year prior to the deal announcement) and *Post* is a dummy variable that takes a value of one for observations

[12] A large part of SG&A consists of expenses related to labor and IT investments (e.g., white collar wages, employee training, consulting, and IT expenditures) (Eisfeldt and Papanikolaou, 2013).

in the years after the deal, and zero otherwise.[13] My dependent variable is the target's employment at the firm level in logs. The key estimate is the interaction term *Post* with acquirers' CSR performance.

One of the advantages of the panel structure is that I can include target firm (δ_i) fixed effects to control for time-invariant firm-level characteristics that may be correlated with omitted variables. All estimations also include year (ζ_i) and event-time (λ_r) fixed effects. These fixed effects absorb the *Post* dummy while allowing me to control for changing macroeconomic conditions and economic trends that are common to all acquisitions. In addition, targets of high and low CSR acquirers could differ along with a number of dimensions that may be correlated with the dependent variable. For example, as mentioned in Section 2.3, high CSR acquirers prefer larger targets or targets that are from different industries. To further mitigate the sources of confounding variation, I control for firms' initial characteristics for both acquirers and targets and deal characteristics, as well as their interaction with a *Post* dummy. In Section 5.2, I also employ a propensity score matching analysis to mitigate the concern that whether high CSR acquirers may manage targets differently or they buy different targets. I do not include time-varying firm-level controls because they are endogenous to the deal decision. X_i is a vector of firm-level control variables measured in the year before the deal, including acquirer size, acquirer leverage, acquirer ROA, acquirer Tobin's Q, target size, and target leverage. These controls ensure that the results are not driven by pre-deal differences among acquirers with different levels of social performance. Note that X_i does not enter separately in the baseline regression because it is absorbed by firm fixed effects.

I also implement an event study DiD analysis and estimate the following dynamic specification:

$$Employment_{i,t} = \alpha_i + \sum_{k=-3,\neq -1}^{+3} \beta_k W_{ki} \cdot CSR_i + \gamma Post \cdot X_i + \delta_i + \zeta_t + \lambda_r + \epsilon_{i,t} \qquad (1.2)$$

[13] Following Dyck et al. (2019), I use logs of CSR scores to obtain better distributional properties and to reduce the impact of outliers. My main results are unaffected if I use the raw scores instead of the scores in logs.

Where W_{ki} is a dummy equal to one if in year t firm i is k years away from the completion of the deal, with $k \in [-3, +3]$. The effects on year $t-1$ are normalized to zero. In all specifications, standard errors are corrected for clustering of observations at the acquirer level.

Table 1.3 presents the regression results from these analyses. In column (1), I show the baseline estimate of the effect of acquisitions on employment (*Post*), with the coefficient indicating that, on average, following acquisitions, employment at the target firm decreases by 11.6%.[14] In column (2), I interact the *Post* dummy with acquirers' CSR investment to study how CSR performance modifies the average effect of takeovers on employment. After controlling for various target and acquirer initial characteristics, I estimate a negative and significant coefficient for the interaction term, which indicates that the decline in employment after the deal is significantly more pronounced as the acquirer's CSR engagement increases. The results regarding CSR are economically significant. A one-point increase in CSR (with a standard deviation of 0.71 points) is associated with a 10.1% decrease in targets' post-merger employment. In column (3), I add event-time fixed effects, such that the *Post* dummy itself is absorbed and only the interaction effects are identified. I find that the magnitude of the effect is unchanged and is still significant at the 5% level. I obtain qualitatively similar results: each extra point on the CSR decreases employment by 10 percentage points, ceteris paribus. Column (4) explores the dynamics of the effect of CSR on labor restructuring in the post-merger years. No statistically significant effect exists in the years before the deals, and a persistent stronger workforce reduction for acquirers with superior CSR performance is evident in every year subsequent to the mergers (See Figure 1.2). These findings suggest that my results do not suffer from reverse causality. Finally, in columns (5) - (6), I additionally control for deal-specific characteristics and country-level (target firm) economic conditions. I continue to find a negative and significant coefficient on the interaction between *CSR* and *Post*.

[14] My finding appears to be dissimilar to Boucly et al. (2011) and Erel et al. (2015). The possible reasons for this are related to the following: First, the size of the target firms in my sample is much larger (more than three times larger) than that of Erel et al. (2015); Second, nearly a third of the targets are concentrated in the UK, where capital and credit markets are large and well-functioning. Thus, relaxing credit constraints is less likely to be the motive for mergers and acquisitions in my sample.

I also ensure that my findings persist after controlling for measures of corporate governance and institutional ownership. Recent evidence shows that well-governed firms or firms with higher institutional ownership are more likely to be socially responsible (Ferrell et al., 2016; Dyck et al., 2019; Chen et al., 2020b). As institutional investors act as effective monitors of corporate behavior and can discourage firms' overinvestment (Aggarwal et al., 2011; Crane et al., 2016), managers will move quickly to undertake post-acquisition restructuring. If governance or institutional ownership is correlated with my CSR measure, it is possible that CSR is simply proxying for governance, resulting in an omitted variable bias. To address this concern, I first measure governance by using the *Governance* score from the Refinitiv ESG database.[15]. I also construct a firm's entrenchment index (*E-index*) following Bebchuk et al. (2009) and Liang and Renneboog (2020).[16] In addition, I gather acquirers' institutional ownership data from the Factset Stock Ownership Summary database by Ferreira and Matos (2008). In Table 1.4, I repeat the analyses from Table 1.3, but I now add the governance and institutional ownership controls. All models include the full set of other control variables employed in Table 1.3. Consistent with my predictions, columns (2) – (5) show that the *Governance* score and the *Institutional Ownership* are negative and significant, which provides some evidence that well-governed firms do more labor restructuring after acquisitions.[17] No significant results can be found for the *E-index*. Most importantly, I again find that the effect of CSR on targets' post-merger employment persists. These results suggest that my main results documented above are not fully driven by firm governance.

Overall, I document a negative relation between acquirers' CSR performance and employment (in targets) after acquisitions. This evidence is consistent with the cost-saving view that CSR increases labor costs per employee, and thus, high CSR acquirers are more likely to fire workers, especially the redundant or overlapping workforce. I acknowledge that many departures may not be due to layoffs. Instead of being eliminated, these employees or jobs are simply transferred from the target firm to the acquirer (Gehrke et al., 2021). To investi-

[15] I have excluded corporate governance components from the measure of CSR when estimating main regressions.

[16] The *E-index* include a list of governance provisions: poison pills, golden parachutes, staggered boards/classified boards and supermajority requirements.

[17] Foreign institutional investors are in a better position than domestic institutional investors to monitor firms (Aggarwal et al., 2011). I additionally control for both domestic and foreign institutional ownership in Table A.1.3 and find that my results remain the same.

gate this, I examined changes in employment for acquirers around the time of the deal. As shown in Table A.1.4, I do not observe any significant changes in the employment levels of acquirers after the deal. This suggests that the movement of employees between targets and acquirers is unlikely to drive the findings documented in this paper.[18]

1.3.2 Legal origin and employment

As the main purpose of this paper is to evaluate how CSR affects firms' employment policies after acquisitions, I also turn to the regulatory context of CSR at the country level.[19] Using CSR ratings for 23,000 firms from 114 countries, Liang and Renneboog (2017) find that a firm's CSR performance and its country's legal origin are strongly correlated, and the level of CSR is highest under the Scandinavian legal regime. I therefore use legal origin as a proxy for firm-level CSR and explore the relation between acquirers' legal origin and targets' post-merger employment. Moreover, since all of the acquirers in my sample are in the Refinitiv ESG database, a potential concern is that my results may be subject to sample selection bias, if the decision on whether to include a firm in the database is not random. This test could mitigate such bias and give me more observations, even including many private acquirers.[20] Following Porta et al. (1998), Djankov et al. (2008), and Liang and Renneboog (2017), I classify legal traditions into five categories, as denoted by the following dummy variables: *English Common Origin, French Civil Origin, German Civil Origin, Scandinavian Civil Origin, and Socialist Origin.* As reported in Table 1.6, I regress employment on the legal origin dummy and show that the results are mostly consistent with my predictions. In column (1), I find a negative coefficient on the interaction between *Scandinavian* and *Post*, implying that acquirers from Scandinavian countries are more likely

[18]I exclude acquirers involved in multiple deals during my sample period, because it is difficult to isolate the effect of each individual deal in these cases.

[19]In the context of CSR, a country's legal regime determines how "public goods" should be provided by firms: through regulations and rules, firm discretion, or government involvement in business (Kitzmueller and Shimshack, 2012). As such, the explicit or implicit contracts between firms' shareholders and their stakeholders can be shaped by a country's legal regime through its effect on governance structures and the decision-making process. For example, in Germany, large firms are legally required to take into account the interests of employees through the system of *co-determination*, which requires that employees and shareholders have an equal number of seats on the supervisory board of the company (Allen et al., 2015).

[20]Refinitiv mainly covers large firms included in the major global equity indices, so most (small) firms do not receive a rating from the Refinitiv ESG database.

to fire workers after takeovers. Column (2) also shows that the *Civil * Post* coefficient is negative though generally statistically insignificant. These results confirm my main findings that socially responsible acquirers are more likely to do labor restructuring in target firms after acquisitions.

1.3.3 Union strength and investor protection

To further help address the concern of omitted correlated variables, I next estimate triple-difference regression models by testing whether the negative relation between CSR and employment is stronger for targets that operate in countries with weak union laws or targets in countries with strong investor protection. Low union strength in the target's country indicates the relative ease with which acquirers can undertake labor restructuring. By the same token, if the target country has strong labor unions, local employees have more bargaining power to resist lay-offs and the implementation of various employment policies. In addition, when investors have greater influence, higher priority is given to enhancing firm value (Atanassov and Kim, 2009). That is to say, if the employee layoff after acquisitions increases shareholder wealth, one would expect targets in countries with stronger shareholder protection to make more employee layoffs. Therefore, the negative relation between the acquirer's CSR and post-acquisition employment should be stronger for targets in countries with weak labor unions or strong investor protection.

Table 1.5 presents the results from tests examining the effect of union strength and investor protection on the relation between CSR and post-merger employment. The data for labor regulations comes from Botero et al. (2004), which has been widely used in previous studies (Atanassov and Kim, 2009; Levine, 2017). The first index, *Union*, measures the statutory protection and power of unions. The second index, *CRL*, assesses the legal protection of labor unions and the regulation of collective disputes. My main proxy for investor protection is the anti-self-dealing index (*ASDI)* developed by (Djankov et al., 2008), which captures a country's legal protection of shareholder rights. Moreover, I also use the Djankov et al. (2007) creditor index, *Creditor*, for legal protection of creditor rights. Overall, the results are consistent with my predictions. In columns (1) - (4) of Panel A, I find that the coefficient on the triple interaction term is positive and significant at the 5% level, suggesting that the negative relation between CSR and targets' post-merger employment becomes more (less) pronounced in countries with weak (strong) union power. As reported in Panel B, the coefficient on the triple interaction term is negative and significant, which indicates that the negative relation is more pronounced for targets in countries with stronger investor protection. This result suggests that the observed workforce reductions are more likely driven by shareholder value maximization.

1.4 Potential Mechanisms

In this section, I investigate the mechanisms underlying the documented effects of CSR, focusing on the cost-saving motive.

1.4.1 Unbundling CSR

Social vs Environmental CSR – I first disentangle the different dimensions of an acquiring firm's CSR contribution. Specifically, the Environmental (E) dimension measures a firm's impact on the natural environment. The Social (S) dimension covers a firm's relation with its employees, customers, and society. Firms with higher *Social* scores are more likely to treat their employees well and provide generous employment benefits. An overly generous labor policy for employees (especially the redundant workers) in target firms may be perceived by acquirer shareholders as money not well spent. If the cost-saving view is the underlying channel, my results are expected to be mainly driven by the *Social* score. I extend the main regressions by examining the two individual components of the CSR rating in Table 1.7. As expected, in columns (1) – (3), I show that my findings are mainly driven by the acquirer's *Social* score, and less so by the *Environmental* score. The magnitude of the coefficient on the *Social* score is also much larger (more than ten times larger than that of the *Environmental* score). The coefficient estimate of -0.157 implies that a one-point increase in the *Social* score is associated with a 15.7% decrease in employment of target firms after acquisitions. Figure A.1.1 presents the estimated coefficients together with 95% confidence bands, focusing on the specification including target, year and event-time fixed effects. These results also rule out an alternative explanation that green acquirers are more likely to close the polluting plants or departments in target firms, which decreases employment after acquisitions. If this was the case, I should observe a negative and significant coefficient on the interaction term of *Environmental * Post*. Finally, for robustness tests, I follow Fauver et al. (2018) and use an equally weighted employee-friendliness index, which is defined as the equal weighting of the workforce and human rights sub-scores from the Refinitiv database. The result using the measure of employee-friendliness in column (4) is also negative and significant at the 5% level.

Provision of costly benefits to employees – Some employee-related CSR programs that affect employee welfare can contribute to the *Social* performance (e.g., work-life balance benefits, health and safety policies, employee involvement, etc.), imposing additional labor costs on firms. This suggests that a high CSR acquirer has to pay additional labor costs per worker in target firms. Thus, I dig deeper into the Refinitiv ESG database and following Liang et al. (2020), I construct a monetary CSR dummy in which I consider several forms of monetary policies: (i) *Day Care Services:* Does the company claim provide day care services (including services such as vouchers, referrals, allowances, etc.) to employees? (ii) *Policy Employee Health & Safety:* Does the company have a policy to improve employee health & safety? (iii) *Health & Safety Training:* Does the company train its executives or key employees on health & safety? (iv) *Policy Skills Training:* Does the company have a policy to improve the skills training of its employees? As before, I interact the monetary CSR dummy with the *Post* dummy. The results are presented in column (5). The coefficient on *Monetary CSR dummy * Post* is negative and significant at the 1% level, which indicates that the acquiring firm's provision of costly benefits to employees has a negative effect on the post-merger employment of target firms.

I also gather information on acquirers' staff benefits from the Refinitiv ESG database, which measures the total value of salaries, wages, and all other benefits to workers, as reported by the company in its CSR reporting. Similar to the monetary CSR dummy above, this variable is also directly associated with the extent to which an acquirer has to pay an extra premium to workers in target firms. Specifically, it contains all monetary benefits, such as social security, pension, allowances, commissions, share-based payments, etc. I thus measure the acquirer's labor costs per employee by using the ratio of staff benefits to the total number of employees, and explore whether acquirers with greater employee welfare engage in more labor restructuring. In line with my prediction, I find in column (6) that acquirers' staff benefits per employee are negatively related to post-merger employment in target firms. Overall, these results imply that my findings are driven by the cost-saving channel.

1.4.2 Cross-sectional variation analysis

To provide further evidence that the effects of CSR on post-merger labor restructuring are tied to the cost-saving channel, I then focus on the *Social* component and implement triple difference-in-differences tests to examine the heterogeneous treatment effects.

Intense labor cost pressure

Human-capital intensive industries – First, I investigate whether the effects of CSR are stronger for firms acquiring targets in highly-skilled, human capital-intensive industries. Firms in these industries (e.g., Apple, Amazon, Google, Facebook, Microsoft, etc.) are well known for providing their employees with generous perks in addition to competitive salaries. In this case, I expect that employee-related CSR programs should be more "expensive" as well, thus inducing higher labor costs in target firms after acquisitions by high CSR acquirers. As such, I expect that my main results are more pronounced for targets in highly-skilled industries. Following Ghaly et al. (2015) and Cao and Rees (2020), I first define the *High Skill* indicator as taking the value of one if the industries belong to telecommunications, high-tech, and healthcare industries, and zero otherwise.[21] I next define the *High R&D* indicator as taking the value of one if the industry-level R&D expenditure is above the sample median, and zero otherwise, as firms in R&D intensive industries are more likely to depend on highly educated or skilled workers.[22] Finally, I follow Chen et al. (2021) and measure skilled occupation intensity as the proportional of skilled occupations with respect to all occupations in each industry. I obtain employment data from the Integrated Public Use Microdata Series

[21] I include the following two-and three-digit SIC codes: 283, 357, 36, 384, 48, and 80.

[22] The industry-level R&D measure is the average of the firm-level R&D intensity, calculated as the ratio of R&D expenditure to total sales.

(IPUMS) database, which provides Current Population Survey (CPS) Data on individual worker's occupational code, industry, state, etc. Based on the IPUMS occupational code book, I define skilled workers as those with an occupational code between 37 and 200, which includes occupations such as scientists, engineers, computer programmers, IT professionals, etc.[23] I then define the *High skilled employment* indicator as equal to one if the proportion of skilled workers among all workers in the firm's 2-digit SIC industry is above the sample median, and zero otherwise. The results are reported in Panel A of Table 1.8. Consistent with my predictions, coefficients on triple interaction terms are all negative and significant, indicating that the negative relation between acquirers' social performance and post-merger employment is more pronounced for targets in human-capital-intensive industries.

Financial constraints – Second, I examine the targets that are more financially constrained, for which the cost-saving motive is more relevant. As financially distressed firms value financial flexibility with more urgency and thus are more sensitive to increased labor costs induced by CSR programs. If the cost-saving story can explain my findings, the results should be more pronounced when targets face greater financial pressure. I thus use the industry-level financial dependence and the level of cash holdings (normalized by a firm's assets) as measures of financial constraints.[24,25] *High financial dependence* equals one if the target operates in a 2-digit SIC industry with financial dependence above the sample median, and zero otherwise. *High cash* equals one if the target's cash holdings is above the sample median, and zero otherwise. Inspection of the results in Panel B shows that coefficients of triple interaction terms are negative and significant, which suggests that the results are indeed stronger among targets in financially dependent industries and targets with higher cash holdings.

[23] Since the CPS data does not provide SIC industry information directly, I manually link the 1990 industry code to the two-digit SIC code.

[24] Industry financial dependence is Rajan and Zingales (1998) measure of external financial dependence, computed at the 2-digit SIC code using U.S. data. This measure is arguably more exogenous than other firm-level traditional measures of financial constraints (e.g., leverage, size, age, etc.).

[25] Cash holdings are higher when managers believe they face greater financial constraints (Opler et al., 1999; Erel et al., 2015). Given that the target firms in my sample are mostly privately held and are very small, I can not use measures of financial constraints (e.g., KZ index, or WW index) that can be calculated for larger or public firms.

More opportunities for redundancy

I expect that the effect of CSR should be affected by the deal type. According to my arguments, cost savings from eliminating redundant or overlapping workers are greater for high *Social* acquirers. However, relative to same-industry deals, diversifying deals offer fewer opportunities for eliminating redundant resources in the workforce due to the lack of occupation overlap (i.e., similar job duties and skills among acquirer and target workforces). Similarly, when acquiring a foreign target, opportunities for eliminating overlap are also limited due to geographical distance and regulatory concerns (Liang et al., 2020). Hence, my results should be more pronounced for same-industry or domestic deals, which have more opportunities for eliminating redundancy. I then label acquisitions as same-industry (domestic) when the acquirer and the target are from the same industry (country). I define an acquisition as "same-industry" when the target and the acquirer operate in the same three-digit SIC code.[26] The results reported in column (1) of Panel C are largely consistent with my premise. I find that the coefficient on the triple interaction term (*Same-industry * Social * Post*) is negative and significant at the 5% level. In column (2), the coefficient on the triple interaction is again negative, although not statistically significant at conventional levels. Taken together, these results are consistent with the notion that due to more opportunities for eliminating workforce overlap, the negative relationship between CSR and targets' post-merger employment becomes stronger.

Second, I test the target firms with lower labor productivity. The rationale is that redundant resources in the workforce are more likely for low-quality or inefficient workers. As such, targets with lower labor productivity provide more opportunities to eliminate workforce redundancy, and my results should be stronger for these target firms. I measure labor productivity by using the ratio of firm sales to employment. I then define the *Low labor productivity* indicator as equal to one if the target's average labor productivity (3 years before the deal) is below the sample median, and zero otherwise. As shown in column (3), the triple interaction term (*Low labor productivity * Social * Post*) is negative and significant at the

[26] My results remain qualitatively unchanged when I define the same-industry deal using the two-digit SIC code.

5% level, suggesting that the negative effect of social performance on employment becomes stronger when targets have more inefficient workers.

Overall, the negative relationship between acquirers' social performance and post-merger employment (in targets) is more pronounced for targets in human-capital-intensive industries, targets that are more financially constrained, and deals or targets with more opportunities for eliminating workforce redundancy. These results provide further support for the cost-saving explanation.

1.4.3 Effects on other target firm outcomes

Next, I examine how acquirers' *Social* performance affects target firms in other outcome variables. First, I investigate the channel through which social performance may impact labor expenditure. The cost-saving channel argues that CSR can increase the expenditure per worker and, thus, target firms of high CSR acquirers will implement a larger post-merger workforce reduction (as those laid-off employees would otherwise receive a larger additional payment). If this channel exists, I should expect more expenditure on human capital in target firms acquired by acquirers with greater social performance. To explore this hypothesis, I conduct additional tests by using two proxies for expenditure in human capital: 1) Staff costs to assets and 2) Staff costs per employee. The staff costs not only contain wages and salaries but also include social security costs, pension costs and other employee-related costs. Figure A.1.2 presents the estimated coefficients together with 95% confidence bands, focusing on the specification including target, year, and event-time fixed effects. The coefficients and standard errors are reported in Table 1.9. Consistent with my predictions, in columns (1) - (2), I find that *Social * Post* coefficient is positive and significant, suggesting that acquirers with greater social performance increase the expenditure on workers in target firms after acquisitions. Taken together, these results are largely consistent with the cost-saving story.

Second, I examine the impact of CSR on the target firm's labor productivity and technical efficiency. If socially responsible firms are more inclined to undertake layoffs, especially for the redundant or overlapping workforce, then an improvement in the target firms' labor productivity should be expected. As a result of this enhanced productivity, targets can also improve their earnings potential and technical efficiency by delivering better services or making better products. To examine these issues, I examine the impact using several proxies for labor productivity and technical efficiency: 1) Sales per employee; 2) Added value per employee; 3) Material costs per employee; 4) Sales to assets.[27] As expected, column (3) - (6) do indeed show that CSR has a positive impact on labor productivity and technical efficiency after acquisitions.[28]

Finally, I examine the capital expenditures in target firms. Engagements in CSR - that is, meeting the needs of various corporate stakeholders - may draw limited financial and physical resources from other investment opportunities, which leads to a decline in capital expenditures. I use asset growth as the proxy for capital expenditure because *CAPEX* is rarely reported in my sample. As shown in columns (7) - (8), there is a significant negative relation between the acquirer's social performance and asset growth in target firms after acquisitions and the magnitude of the coefficient on *Asset growth (fixed)* is larger. These results suggest that the allocation of scarce corporate resources to CSR activities could decrease targets' capital expenditures, which provides additional insights into the drivers of my main findings.[29]

1.4.4 Announcement effects

In this section, I provide further evidence supporting the cost-saving view by investigating the impact of an acquirer's social performance on merger announcement returns. If acquirers with greater social performance can efficiently restructure the labor force in target firms and realize higher cost savings, I expect to observe positive shareholders' reactions to M&A announcements. To assess market reactions and thus draw inferences on shareholder value, I calculate cumulative abnormal stock returns (CARs) for the acquiring firm in the T days surrounding the deal announcement. These abnormal returns are obtained using the market

[27]The number of observations declines substantially because, in the UK, firms are not required to report sales data (Erel et al., 2015). Data on material costs is missing for firms from the UK, and I use the cost of sales to replace the missing value.

[28]It is also possible that CSR improves the firm-employee relationships, thus increasing labor productivity (Edmans, 2011, 2012; Flammer, 2015).

[29]I also explore innovation activities in target firms in Table A.1.5. Due to the data limitation, I focus only on the number of patents of target firms. I find that higher levels of social performance are negatively related to the number of patents, which indicates that socially responsible acquirers could also reduce their innovation investments in target firms after acquisitions.

model over a period starting 120 days before the announcement date until 30 days before this date. I focus only on the acquirers' CARs because most of the targets in my sample are private firms.

In Panel A of Table 1.10, I report the CARs for the full sample of acquirers as well as the subsamples of high and low *Social* acquirers.[30] Acquirers are divided into high and low *Social* acquirers according to the sample median of their social performance. The mean CAR (-1, 1), CAR (-2, 2), and CAR (-3, 3) for the full sample are positive and significant. The subsample results show that these positive returns are mostly driven by high *Social* acquirers. The mean CARs for high *Social* acquirers are positive and significant at the 1% level. In contrast, the respective CARs for low *Social* acquirers are much smaller and not significant. The median CARs show a similar pattern. The equality in mean and median CARs between the high and low *Social* subsamples is rejected significantly.

In Panel B, I present estimates from multivariate regressions using the CAR (-1, 1) as the dependent variable. In addition to including acquirer controls specified in Section 3.1, I also control for acquirer industry and year fixed effects. Column (1) indicates that a higher level of social performance is positively related to shareholder returns around deal announcements. This is consistent with my main story that socially responsible acquirers can realize greater cost savings from eliminating workforce redundancy, and thus, these CSR policies are regarded favorably by shareholders. To mitigate omitted variable concerns, in columns (2) – (4), I consistently find that higher levels of social performance are positively related to acquirer CARs, and this effect is not eroded by the inclusion of target and deal-specific characteristics, and target industry, acquirer and target country fixed effects.

However, it is possible that the positive effect of CSR on merger performance is due to CSR activities leading to greater stakeholder satisfaction, which ultimately benefits shareholders (Deng et al., 2013), rather than efficient post-merger labor restructuring. Next, I take a further step to investigate how the market responds to workforce reductions after the acquisition. If acquirers with superior social performance can achieve greater cost-saving benefits through post-merger labor restructuring, then I should expect to see higher announcement returns for these socially responsible acquirers, especially when they implement more layoffs

[30] The results for high and low CSR acquirers are reported in Table B.4.

after the acquisition. Panel C of Table 1.10 presents the results. The independent variable of interest is the interaction term *Social * Large* $\Delta log(Emp)$, where *Large* $\Delta log(Emp)$ is an indicator that equals one if the pre-to-post decrease in log-employment is above the sample median, and zero otherwise. Similarly, the specifications include various fixed effects, and firm and deal-specific characteristics. I find that the interaction coefficient is positive and significant at the 5% level.[31] These results are consistent with the notion that investors anticipate increased shareholder wealth due to workforce reductions by acquirers with greater social performance, which is also in line with my main argument.

1.4.5 Alternative explanations

Moral capital

Another potential channel for the observed findings could be related to the moral capital story. Existing literature suggests that CSR activities can help build social capital and enhance stakeholder trust, and there are potential halo effects of being charitable or good (Godfrey et al., 2009; Goss and Roberts, 2011; Elfenbein et al., 2012; Lins et al., 2017; Hong et al., 2019; Barrage et al., 2020). Firms with stronger CSR credentials (i.e., larger moral capital reserves) are more likely to be seen in a positive light, and stakeholders are more likely to temper their negative judgement of the firm. This positive moral capital, in turn, can provide a form of insurance by moderating the negative assessment of stakeholders when firms suffer a negative event. In other words, CSR can help firms window-dress their image and reputation to pursue self-interest or economic egoism in the organization. Given the negative externalities of layoff on various internal and external stakeholders, large-scale workforce reductions after the takeover may incur reputational penalties. As such, CSR engagement serves to protect firms from adverse reputational consequences of corporate downsizing, and acquirers with a better CSR image may engage in more post-merger layoffs.

To test the moral capital channel, I begin by testing large acquirers. Large firms always face greater scrutiny from media, special interests, and stakeholders because they have higher profiles than small firms. Simply put, firms with a larger market preference always incur

[31] As a placebo test, Table A.1.7 shows that "green acquirers" can not enjoy higher announcement returns when they do more labor restructuring.

more risk. If the moral capital story is the underlying mechanism, stronger results should be found for larger acquirers. Second, I expect the moral capital benefits to be less prevalent in industries with high labor volatility or when the economy turns downward. In these cases, labor adjustment occurs more often, and layoff decisions can be seen as a more common practice. I define the *High labor volatility indicator* as one if the target's industry-level labor volatility is above the sample median, and zero otherwise.[32] I then use change in GDP to proxy for economic conditions. *Negative GDP change* is the 1-year percentage decrease in the target's country GDP, with positive changes set to zero. Third, I obtain data on the country-level *"Responsibility is really important"* and *"Work is really important"* from the World Value Survey, and consider the case in which people in the target's country have a higher predilection for responsibility and work. Employee layoffs could have greater negative social implications in countries with higher values of these two variables ("High" is defined as being above the sample median), and thus the moral capital of high CSR firms becomes more important, and my results should also be more pronounced in these countries. The results of the analysis are reported in columns (1) – (5) of Table A.1.8. However, I do not find any evidence to support the moral capital channel.

If the moral capital channel plays a role in post-merger labor restructuring, it is also interesting to investigate the acquirers with a prior history of mass layoffs. Godfrey et al. (2009) argue that whether CSR activities can generate moral capital mainly depends on the stakeholders' evaluations of the firm's motives. The moral capital arising from socially responsible activities comes from the signal of non-self-serving intentions. However, engagement in activities with negative effects on stakeholders may signal an intention to act self-interestedly rather than considering the needs of others or society at large. As such, for firms with repeated violations, investments in CSR may be perceived as a window-dressing behavior for their negative behaviors. If CSR engagements are viewed as an ingratiating attempt to win favor, firms are less likely to gain and may even generate a negative moral evaluation. Thus, prior mass layoff practices may deplete firms' moral capital and result in a dramatically less forgiving stakeholder set, and my results should be less pronounced for acquirers with a prior

[32] The industry-level labor volatility is the average of the firm-level labor volatility, measured as the standard deviation of the number of employees relative to the value of plant, property, and equipment (PPE) assets over time.

history of mass layoffs. Following Atanassov and Kim (2009), the mass layoff is measured as taking the value of one if a firm experiences more than a 15% drop in the number of employees from year t–1 to year t or $t + 1$. Then, I define a *Prior mass layoff* indicator as one if the acquirer had undertaken a large-scale employee layoff in the 5 years before the deal, and zero otherwise. The specification in column (6) shows a positive and significant triple interaction term (significant at the 10% level), the only instance supporting the moral capital channel. Overall, I find very limited evidence that the moral capital story is driving my main findings.

Managerial entrenchment

The agency view of CSR suggests that CSR activities are linked to the pursuits of managers' self-interests. Inefficient CEOs can use CSR activities strategically to build relations with stakeholders to receive favorable treatment during future turnover decisions (Cespa and Cestone, 2007). In line with that, Cai et al. (2021) provide empirical evidence that CEOs are unlikely to be replaced for poor performance when firms donate to charities affiliated with a large fraction of the board or when they donate large amounts. If protecting employees from post-merger restructuring can be used as an entrenchment strategy for managers, it is also possible that engagements in CSR activities and reluctance to layoff are substitute ways of forming an alliance with stakeholders. When managers in high CSR firms have built solid support from other stakeholders, they have less to lose from engaging in layoffs. Therefore, high CSR acquirers are expected to take more layoffs after acquisitions.

However, I do not find evidence in support of this channel. First, when I include corporate governance proxies to capture agency concerns, the negative relationship between acquirers' CSR performance and post-merger employment in target firms continues to hold (Table 1.4). Second, if my results are primarily driven by the agency channel, I would expect my results to be more pronounced in countries with weak legal protection, where shareholders' and managers' incentives are less likely to be aligned, and agency problems are likely to be higher. However, the results are not consistent with my predictions. In contrast, the negative relationship between CSR and employment (in targets) is more pronounced in countries with better investor protection (Table 1.5), which suggests that the observed workforce reductions are more likely to be motivated by shareholder value maximization rather than influenced by manager entrenchment. Finally, if CSR is a manifestation of agency problems, one expects that the main findings should be driven by acquirers' environmental and social performance simultaneously. Again, as shown above, my results are mainly driven by the social score, and less so by the environmental score (Table 1.7). Overall, these results suggest that the entrenchment channel is unlikely to be the main channel through which socially responsible acquirers operate larger workforce reductions after the acquisition.

1.5 Additional Analyses and Robustness Tests

1.5.1 Subsidiary-level evidence

In this section, I utilize the subsidiary-level data and examine how acquirers' social performance affects the labor restructuring in the targets' subsidiaries. I rely on the Amadeus database to extract ownership information of target firms. The minimum ownership stake I require to consider a target firm as a controlling shareholder is 50%. Similarly, I require that the subsidiaries have nonmissing employment data for at least one year before and two years after the merger. Then, I am able to find 427 subsidiaries meeting these criteria. Table 1.11 reports results from subsidiary-level regressions using Equation (1), and results are mostly consistent with those at the parent level. The estimates in columns (1) - (3) show that when targets are acquired by a high CSR acquirer, the subsidiaries of these target firms also engage in more labor restructuring after acquisitions. In line with my previous findings, the results are primarily driven by the social component of the CSR rating, rather than by the environmental component. In addition, I can observe a positive effect of the acquirer's social performance on subsidiaries' staff costs and labor productivity in columns (4) - (7), although insignificantly so. Finally, column (8) shows that targets managed by acquirers with greater social performance operate fewer subsidiaries after acquisitions. This result is consistent with my previous findings that the allocation of scarce corporate resources to CSR activities could decrease targets' capital expenditures.

1.5.2 Propensity score matching

One specific concern is that acquisition decisions are not random, as employment dynamics may vary across targets for reasons that are unrelated to the social performance of their acquirers. For example, do socially responsible acquirers manage targets differently, or rather they buy different targets? [33] Moreover, one could wonder whether my results are driven by the target's social performance, rather than the acquirer's. However, empirically this is a difficult question because most targets in my sample are private firms and do not receive a rating from the Refinitiv ESG database. To control for observable differences in firm and industry attributes, I next perform a one-to-one propensity score matching analysis. Acquirers are divided into high and low CSR subgroups according to the sample median of their *Social* score. I match targets in the high CSR group with those in the low CSR group on their size and employment in the year before the deal, and I ensure targets in these two groups are in the same two-digit SIC code and the same country.[34,35] By matching on industry and country, I can also remove unobserved industry and country heterogeneity that may be correlated with the employment in target firms. In Figure 1.3, I present the estimated coefficients together with 95% confidence bands for the matched sample. The results in Table 1.12 show that targets lay off more employees following the acquisition by acquirers with higher *Social* performance.[36] I further plot employment levels before and after the event for each group, and the deal completion year is defined as time zero. Following Vig (2013) and Buchuk et al. (2014), I compute the yearly rescaled average values of employment for each subsample. For each year, rescaling is done by deducting the 3-year average before the deal (i.e. $T-3$ to $T-1$) from each annual average figure of employment. In Figure A.1.3, I can observe that these two subsamples have similar trends in terms of employment before the

[33] For example, it might be possible that socially responsible firms are more inclined to pursue acquisitions, seeking synergies from these transactions, which could lead to an expectation of more employee layoffs for acquirers with higher social performance. However, I do not find a significant positive effect of social performance on the likelihood of a firm being an acquirer in Table A.1.9.

[34] I exploit a probit model to estimate the probability of being a target in the high CSR group based on their size and employment in the year before the deal.

[35] In Table A.1.10, I additionally match targets from these two groups based on acquirers' characteristics and deal characteristics, and my results remain unaffected.

[36] I also perform a Mahalanobis matching analysis in Table A.1.11, and find that our results remain the same.

deal. However, targets in the high CSR group experience a strong reduction in employment level after the completion of takeovers at time 0. In contrast, a similar downward trend is not seen for targets in the low CSR group. To further address selection concerns, I use placebo tests to compare targets acquired by "green acquirers" with that acquired by "non-green acquirers". Acquirers are divided into two subgroups according to the sample median of their *Environmental* score. If the above results were due solely to selection, then similar employment dynamics should also be observed among targets of "green acquirers". However, no results can be found in Table A.1.12.

1.5.3 Instrumental variable test

To further alleviate potential endogeneity problems, I also estimate instrumental variable regressions. My first instrument is a measure of national culture, egalitarianism. National culture can be considered as a critical informational institution that significantly affects the behaviors of corporations and their stakeholders, which is expected to shape firms' CSR practices (Schwartz, 2004). In particular, egalitarian cultures seek to induce societal members to recognize one another as moral equals who share basic interests as human beings. The most important values in egalitarian cultures include equality, social justice, responsibility, and mutual help. People are socialized to internalize a commitment to cooperate and feel concerned for everyone's welfare. A firm's CSR investment, especially efforts to promote the welfare of employees and society, is more likely to be valued in egalitarian cultures (Schuler and Cording, 2006). Thus, in countries with a higher egalitarianism value, firms are expected to maintain high CSR performance, treat their employees well and, more generally, act for the benefit of all their stakeholders as a matter of choice (Cai et al., 2016; Cheung et al., 2020). Second, I add the lagged social score from 5 years before the deal. It is unlikely that the social score assigned to firms 5 years before the deal is going to be influential in the labor restructuring of target firms.

To support my choice of instrumental variables, in the 2SLS regression I perform the following two tests: (1) a weak instruments test to confirm the relevance of the instruments (i.e., high correlations between instruments and CSR); (2) An overidentification test to examine the exogeneity of the instruments (i.e., no significant correlation between the instruments and the error terms in the regressions). Results are reported in Table A.1.13. In the first-stage model reported in column (1), I see that both instruments are statistically significant, which seems to validate their use. In column (2), I report results from the second-stage model and I find that the coefficient of instrumented *Social* is still negative and significant. Taken together, these results confirm my main findings that high CSR acquirers are more likely to engage in post-acquisition labor restructuring in target firms.

1.5.4 Other robustness tests

I also conduct a few other specific robustness tests. First, it is possible that my main finding of the relation between CSR and labor restructuring is attributed primarily to the Refinitiv ESG database used in my study.[37] The coverage of Refinitiv ESG data is fairly extensive, and the database is also widely employed in a large number of previous studies. However, it is arguable that the assignment of individual firm ratings may be biased toward the methodology Refinitiv adopts. To address this possible bias, I employ an alternative ESG rating database, the Sustainalytics database, which is also widely used in the literature.[38] I then repeat the main estimation using firm-level CSR ratings assigned by the Sustainalytics database (ranging between 0 and 100). As the Sustainalytics database in WRDS is available from 2009, there is a significant loss of data, reducing the sample size to 2600 observations. Results are reported in column (1) of Table 1.13 and confirm that my key findings remain materially unaffected.

Second, I replicate my main regressions using the *Layoff* indicator as the dependent variable. I follow Atanassov and Kim (2009) and define a *Layoff* indicator as one if the firm experiences a decrease in the number of employees greater than 15% over one year or two years, and zero otherwise.[39] The results in column (2) show that the coefficient on the interaction term is positive and significant, confirming my previous conclusions.

[37] Chatterji et al. (2016) argue that one should cross-validate the results using several different ESG data sources for every CSR research.

[38] Similar to Refinitiv ESG, CSR ratings in the Sustainalytics database are also industry adjusted, that is, companies are rated on their CSR engagement (both voluntary initiatives and mandatory compliance), relative to industry peers, on a global scale. Firm coverage in the Sustainalytics database is comprised mostly of constituents of major global equity indices.

[39] For robustness I use a different cutoff level for *Layoff* (20% decline in the number of employees) and find similar results.

Third, I control for the acquirer's management practices. CSR investments are set by a firm's management team. Doukas and Zhang (2021) show that acquirers with talented managers are more inclined to engage in CSR activities to shape corporate social culture. Hence, if CSR is simply proxying for managerial ability, acquirers with greater social performance could change the management practices in target firms, resulting in a change in the type of workforce after acquisitions. I then use the Demerjian et al. (2012) proxy of managerial practices, which estimates the proportion of firm efficiency attributable to managers. Since Demerjian et al. (2012) obtain their sample from Compustat, the additional requirement of having managerial ability data for the acquirer reduces my sample size to 1784 observations. As reported in column (3), our results remain qualitatively unchanged after controlling for the acquirer's management practices.

Fourth, based on my sample distribution, one could argue that results may be driven by US acquirers, as they make up 26% of my sample. I repeat my results for a sample excluding US acquirers in column (4). The exclusion of these target firms reduces the sample size to 4519 observations. I find that my results remain the same, suggesting that I am identifying a global phenomenon.

Fifth, recent studies (Deng et al., 2013; Erel et al., 2015) remove financial firms from their investigations as financial industries have different reporting policies and are subject to different regulations. In order to rule out this potential bias, I also remove all targets in financial industries from my sample. Results are reported in column (5). Again, the acquirer's CSR appears to bear a negative and statistically significant relationship with post-merger employment in target firms.

Sixth, another potential concern is that acquirers with greater social performance might close some of the plants because of the social violations in target firms (e.g., child labor, gender diversity, etc.), which leads to a significantly higher likelihood of layoffs. If this were the case, I should observe more asset sales in target firms once they are acquired by socially responsible firms. To address this possibility, my dependent variable is replaced as the *Asset sales* indicator, which equals one if the firm experiences more than a 15% drop in its fixed assets over one year or two years, and zero otherwise. Results are presented in column (6) of

Table 1.13. I do not observe any significant results on asset sales of target firms, suggesting that my findings are less likely to be driven by targets' asset sales after acquisitions.

Finally, I include more fixed effects to alleviate the concern that some unobservable omitted variables can potentially drive our results. In column (7), I control for both industry-by-year fixed effects and country-by-year fixed effects. I find my results are still robust after incorporating these fixed effects.

1.6 Conclusion

Despite the plethora of studies on the relations between CSR and M&A performance, the impacts of CSR on post-merger strategies remain under-explored. In this paper, I conduct the first study to investigate whether high CSR acquirers manage targets differently, and in particular, I examine the employment policies of socially responsible acquirers. Using a sample of 921 deals announced between 2003 and 2016 in Europe, I find that acquirers with greater CSR performance lay off more employees in target firms. This empirical result is consistent with the cost-saving story. The underlying idea is that CSR activities increase the labor costs per employee in target firms, which in turn decreases the optimal level of employment. Hence, target firms operate larger employee layoffs after acquisitions, especially for the redundant or overlapping workforce.

In line with the cost-saving view, my findings are mainly driven by the *Social* score rather than by the *Environmental* score. More importantly, I show that the acquiring firm's CSR policies providing monetary benefits to employees are negatively associated with targets' employment after acquisitions. The relationship between acquirers' social performance and post-merger labor restructuring is more pronounced for targets in human-capital-intensive industries, targets that are more financially constrained, and deals or targets with more opportunities for eliminating redundancy. Further, I document consistent evidence that acquirers with greater social performance increase staff costs, labor productivity, and technical efficiency in target firms. I also show that socially responsible acquirers can realize higher announcement returns, especially when they do more layoffs. Finally, my results are robust to correct for potential endogeneity problems and a battery of other potential econometric issues. I find very limited evidence that the moral capital and the managerial entrenchment stories drive my main findings.

Overall, this paper contributes to the CSR literature. Anecdotal evidence suggests that socially responsible firms may not act in the best interests of shareholders – either because of their pure altruistic motivations (i.e., sacrifice money for a good cause) or because managers in these firms perceive a personal benefit from the investment. However, in this paper, I do not find any evidence in support of this view. In contrast, I show that socially responsible acquirers are managed to maximize shareholder interests by engaging in more post-merger labor restructuring to realize cost-saving benefits.

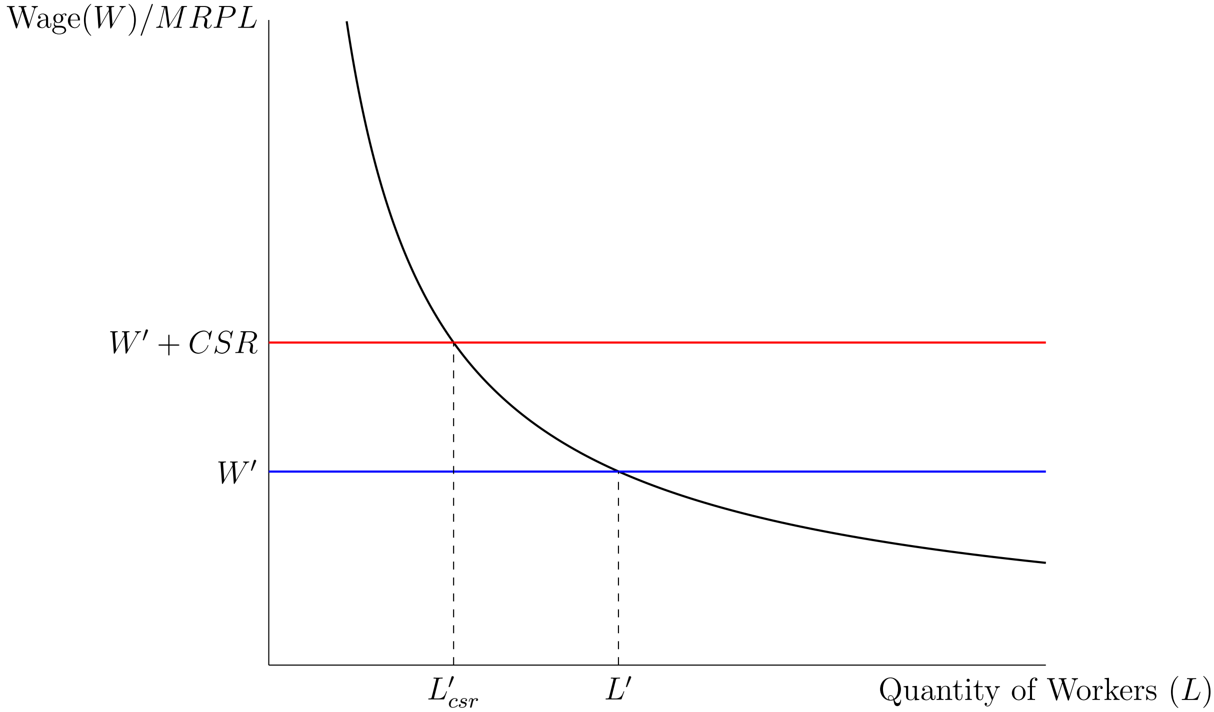


Figure 1.1: Optimal labor demand decreases with labor-related CSR expenses

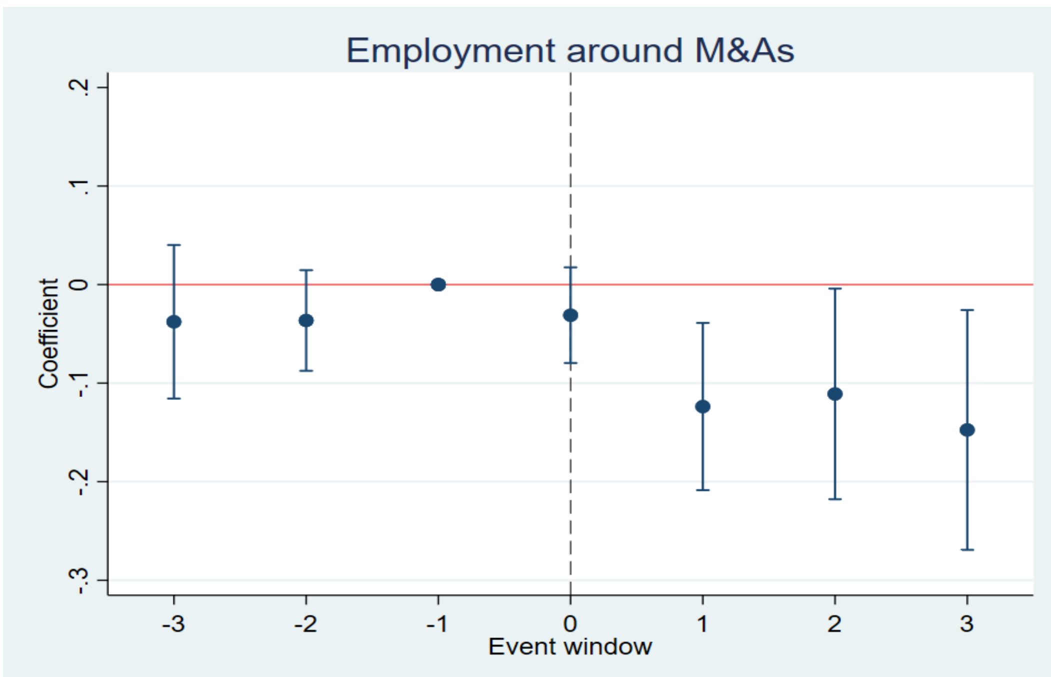


Figure 1.2: Employment coefficients around M&As
The figure displays coefficient estimates of the fixed effects model for employment in target firms, with 95% confidence intervals. I include target, year and event-time fixed effects in my specification.

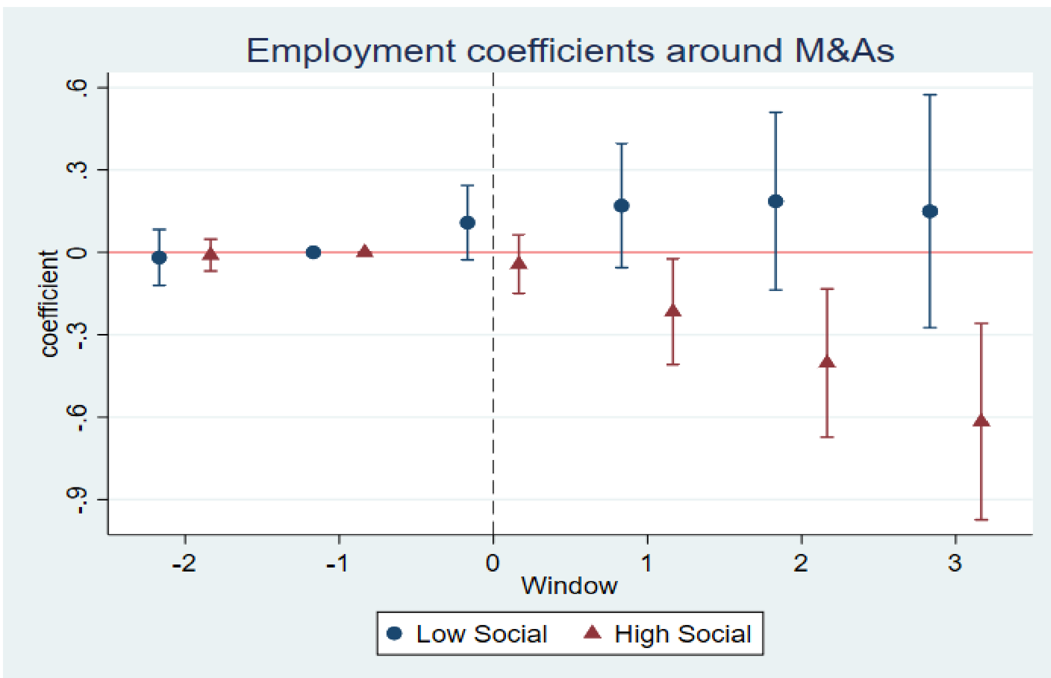


Figure 1.3: Employment by event year for the matched sample
The figure displays coefficient estimates for employment in a matched sample, with 95% confidence intervals. The match is based on industry, country, size and employment the year before the deal. The regressions include firm and year fixed effects.

Table 1.1: Sample distribution

This table shows a sample distribution of European targets from 2003 to 2016. Panel A shows the sample distribution by year and industry. Panel B shows the characteristics of acquisitions across targets' countries.

Panel A: Sample Distribution by Year and Industry

Target primary US SIC code (two-digit)	Agriculture, Forestry, and Fishing (01-09)	Mining and Construction (10-17)	Manufacturing (20-39)	Transportation and Communications (40-48)	Wholesale Trade and Retail Trade (50-59)	Finance Insurance and Real Estate (60-67)	Services and Public Administration (70-97)	Total
2003	1	1	9	4	3	4	5	27
2004	0	1	12	6	6	1	10	36
2005	1	4	23	6	6	1	15	56
2006	0	8	29	10	10	4	20	81
2007	0	5	25	12	12	6	28	88
2008	0	3	28	4	6	4	35	80
2009	0	2	13	11	4	2	12	44
2010	0	3	22	6	10	7	24	72
2011	0	4	39	1	7	3	28	82
2012	0	6	23	8	11	5	34	87
2013	1	1	22	1	5	0	21	51
2014	0	3	25	4	4	4	17	57
2015	0	2	35	6	12	6	21	82
2016	1	2	27	3	8	5	31	77
Total	4	45	332	82	104	52	301	920

Panel B: Target and Deal Characteristics by Country

Country	No. of Deals	Target's employment		Cross-border Deals (%)	Diversified Deals (%)	Private target (%)
		Mean	Median			
AT	10	94.20	91.50	80.00%	80.00%	100.00%
BE	58	96.64	38.50	82.76%	74.14%	94.83%
DE	118	357.14	79.50	76.27%	72.03%	88.98%
DK	2	76.00	76.00	100.00%	100.00%	100.00%
ES	99	341.23	76.00	74.75%	71.72%	96.97%

FI	20	75.00	30.50	80.00%	65.00%	90.00%
FR	72	305.40	77.00	61.11%	77.46%	83.33%
GB	297	591.01	143.00	59.19%	72.39%	95.29%
GR	1	362.00	362.00	0.00%	100.00%	100.00%
IE	8	123.88	25.50	88.89%	75.00%	100.00%
IT	76	298.25	93.00	82.89%	78.95%	97.37%
NL	50	387.64	25.00	76.00%	72.00%	98.00%
PT	5	95.20	37.00	100.00%	60.00%	80.00%
SE	105	185.09	26.00	78.10%	73.33%	87.62%
Total	921	374.55	78.00	70.61%	73.37%	93.05%

Table 1.2: Summary statistics

This table shows summary statistics for the firm and deal-level variables (in the year before the acquisition). Acquirers are divided into high and low CSR acquirers according to the sample median of CSR. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

		Full sample				High CSR		Low CSR		Differnce
	Variable	Obs	Mean	Median	SD	Mean	Median	Mean	Median	High - Low
Acquirer	CSR (Log)	921	3.52	3.69	0.71	4.08	4.07	2.97	3.13	1.11***
	Social (log)	921	3.69	3.75	0.57	4.07	4.11	3.31	3.40	0.76***
	Environmental (log)	921	2.99	3.67	1.53	4.06	4.11	1.91	2.42	2.15***
	Total assets (EUR Million)	920	34364.02	4531.64	134000.00	55210.44	6718.96	13517.61	3190.84	41692.82***
	Leverage	920	0.25	0.25	0.15	0.26	0.25	0.24	0.24	0.02*
	Employment	910	42412.13	18533.50	67164.42	60385.26	34625.00	24280.30	10664.00	36104.96***
	ROA	910	0.13	0.12	0.07	0.12	0.12	0.13	0.12	-0.01*
	Tobin Q	918	1.83	1.52	1.04	1.69	1.44	1.96	1.59	-0.028***
	Staff_empl (EUR Million)	290	56.10	47.21	47.87	56.58	52.23	55.77	41.42	0.82
	SG&A(log)	691	13.46	13.43	1.46	14.03	13.88	12.96	13.00	1.06***
	SG&A_toas	691	0.20	0.17	0.15	0.21	0.17	0.19	0.17	0.01
Target	Total assets (EUR Million)	921	210.49	15.83	949.82	274.21	17.35	146.91	14.93	127.29**
	Employment	921	374.55	78.00	1105.17	412.62	77.50	336.55	82.00	76.07
	Leverage	912	0.66	0.62	0.42	0.68	0.62	0.65	0.62	0.03
	Public target	921	0.07	0.00	0.25	0.07	0.00	0.07	0.00	0.00
	Labour productivity	502	5.15	0.23	31.48	2.07	0.23	8.35	0.24	-6.28**
	Labour productivity2	644	0.17	0.07	0.41	0.17	0.08	0.16	0.07	0.01
Deal	Diversify deal	920	0.73	1.00	0.44	0.80	1.00	0.67	1.00	0.13***
	Cross-border deal	921	0.70	1.00	0.46	0.68	1.00	0.72	1.00	-0.04

Table 1.3: Main results

This table presents estimates of the effect of acquirers' CSR performance on the post-merger employment in target firms. The dependent variable is the natural logarithm of employment plus one. *CSR* is the natural logarithm of the acquirer's CSR score. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. $Win(+k)$ is a dummy equal to one if the year is the kth year after the acquisition. Control variables include *Acquirer Size*, *Acquirer Leverage*, *Acquirer ROA*, *Acquirer Tobin's Q*, *Target Size*, *Target Leverage*, *Diversify*, *Cross*, *GDP per capital* and *GDP growth*. Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.115***	0.991***				
	(0.03)	(0.37)				
CSR * Post		-0.092**	-0.101**		-0.100**	-0.102**
		(0.05)	(0.05)		(0.05)	(0.05)
CSR*Win (-3)				-0.038		
				(0.04)		
CSR*Win (-2)				-0.036		
				(0.03)		
CSR*Win (0)				-0.031		
				(0.02)		
CSR*Win (1)				-0.124***		
				(0.04)		
CSR*Win (2)				-0.111**		
				(0.05)		
CSR*Win (3)				-0.147**		
				(0.06)		
Acquirer size * Post		0.064**	0.066***	0.065***	0.072***	0.072***
		(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Acquirer leverage * Post		-0.169	-0.172	-0.171	-0.172	-0.162
		(0.21)	(0.21)	(0.21)	(0.21)	(0.21)
Acquirer ROA * Post		1.048*	1.061*	1.062*	1.055*	1.047*
		(0.60)	(0.60)	(0.60)	(0.62)	(0.62)
Acquirer Q * Post		-0.020	-0.022	-0.022	-0.020	-0.020
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Target size * Post		-0.105***	-0.105***	-0.104***	-0.107***	-0.108***
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Target leverage * Post		-0.117*	-0.118*	-0.117*	-0.122*	-0.125*
		(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Diversify * Post					-0.107	-0.110*
					(0.07)	(0.07)
Cross * Post					-0.033	-0.033
					(0.07)	(0.07)
GDP Growth						-0.016
						(0.01)
GDP per Capita						-0.878
						(1.03)
Target FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Event FE	No	No	Yes	Yes	Yes	Yes
N	5,958	5,804	5,804	5,804	5,799	5,799
r2	0.873	0.879	0.879	0.879	0.879	0.880

Table 1.4: Controlling for governance and institutional ownership

This table presents the results that controlling for corporate governance and institutional ownership. The dependent variable is the natural logarithm of employment plus one. *CSR* is the natural logarithm of the acquirer's CSR score. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. *Governance* is the natural logarithm of the acquirer's governance score. *E-index 1* is the sum of the following dummy variables from Datastream: the presence of a poison pill, a golden parachute, a supermajority requirement and a staggered board. *E-index 2* has the same composition as *E-index 1*, except that staggered board is replaced by classified board. *IO_Total* is the percentage of total institutional ownership in the acquiring firm. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)				
	(1)	(2)	(3)	(4)	(5)
CSR * Post	-0.101**	-0.075*	-0.161**	-0.165**	-0.108**
	(0.05)	(0.05)	(0.07)	(0.07)	(0.05)
Governance * Post		-0.104**			
		(0.04)			
E-index 1 * Post			-0.023		
			(0.04)		
E-index 2 * Post				-0.042	
				(0.05)	
IO_Total * Post					-0.250**
					(0.10)
Control * Post	Yes	Yes	Yes	Yes	Yes
Target FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes
N	5,804	5,804	2,566	2,566	5,592
r2	0.879	0.880	0.887	0.887	0.886

Table 1.5: Effects of labor unions and investor protection

This table reports the triple difference-in-differences tests to examine the heterogeneous effects. The dependent variable is the natural logarithm of employment plus one. *CSR* is the natural logarithm of the acquirer's CSR score. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. *Union* is the country-level index which assesses the legal protection of labor unions. *CRL* is the country-level index which measures the protection of collective relations laws. *Source:* Botero et al. (2004). Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Effects of labour union

	Number of Employees (log)			
	(1)	(2)	(3)	(4)
CSR * Post	-0.185**	-0.230***	-0.251**	-0.311***
	(0.07)	(0.07)	(0.11)	(0.10)
Union * Post	-0.840*	-0.901**		
	(0.45)	(0.43)		
CRL * Post			-1.12	-1.304*
			(0.72)	(0.69)
Union * CSR * Post	0.326**	0.325**		
	(0.13)	(0.13)		
CRL * CSR * Post			0.448**	0.480**
			(0.21)	(0.21)
Control * Post	No	Yes	No	Yes
Target FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Window FE	Yes	Yes	Yes	Yes
N	5,945	5,804	5,945	5,804
r2	0.874	0.880	0.874	0.880

Panel B: Effects of investor protection

	Number of Employees (log)			
	(1)	(2)	(3)	(4)
CSR * Post	0.140*	0.089	0.061	0.039
	(0.08)	(0.08)	(0.08)	(0.09)
ASDI * Post	0.916*	0.981**		
	(0.47)	(0.44)		
Creditor * Post			0.113	0.164*
			(0.10)	(0.09)
ASDI * CSR * Post	-0.348**	-0.341**		
	(0.14)	(0.13)		
Creditor * CSR * Post			-0.043	-0.055*
			(0.03)	(0.03)
Control * Post	No	Yes	No	Yes
Target FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Window FE	Yes	Yes	Yes	Yes
N	5,945	5,804	5,945	5,804
r2	0.874	0.880	0.874	0.880

Table 1.6: Legal origin and employment

This table presents the effects of the acquirer's legal origin on post-merger labor restructuring. The dependent variable is the natural logarithm of employment plus one. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. *Scandinavian* is a dummy variable that equals one if the acquirer's headquarter is located in a Scandinavian civil law country and zero otherwise. *Civil* is a dummy variable that equals one if the acquirer's headquarter is located in a civil law country and zero otherwise. *English* is a dummy variable that equals one if the acquirer's headquarter is located in an English common law country and zero otherwise. *French* is a dummy variable that equals one if the acquirer's headquarter is located in a French civil law country and zero otherwise. *German* is a dummy variable that equals one if the acquirer's headquarter is located in a German civil law country and zero otherwise. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)		
	(1)	(2)	(3)
Scandinavian * Post	-0.134***		-0.161**
	(0.04)		(0.07)
Civil * Post		-0.023	
		(0.03)	
English * Post			-0.040
			(0.06)
French * Post			0.003
			(0.06)
German * Post			-0.055
			(0.07)
Control * Post	Yes	Yes	Yes
Target FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Event FE	Yes	Yes	Yes
N	10,327	10,327	10,327
r2	0.934	0.934	0.934

Table 1.7: Unbundling CSR

This table presents the results for unbundling CSR. The dependent variable is the natural logarithm of employment plus one. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. In columns (1)-(3), *Social* is the natural logarithm of the acquirer's social score. *Environmental* is the natural logarithm of the acquirer's environmental score. In column (4), *EF index* is defined as the equal weighting of the workforce and human rights sub-scores from the Refinitiv database. In column (5), *Monetary CSR dummy* is a dummy ranging from 0 to 4, which adds one if the acquirer provides day care services for its employees, has the policy to improve employee health & safety, trains its employees on health & safety, or has the policy to improve the skills training of its employees. In column (6), *Staff_empl* is measured as the acquirer's staff benefits divided by the total number of employees. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Social * Post	-0.158***		-0.177**			
	(0.06)		(0.07)			
Environmental * Post		-0.014	0.016			
		(0.02)	(0.03)			
EF index * Post				-0.081**		
				(0.04)		
Monetary CSR dummy * Post					-0.064***	
					(0.02)	
Acquirer Staff_empl * Post						-1.611*
						(0.95)
Control * Post	Yes	Yes	Yes	Yes	Yes	Yes
Target FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes	Yes
N	5,804	5,804	5,804	5,672	5,608	1,836
r2	0.879	0.879	0.879	0.882	0.880	0.846

Table 1.8: Cross-sectional variation analysis

This table reports the triple difference-in-differences tests to examine the cost-saving story. The dependent variable is the natural logarithm of employment plus one. *Social* is the natural logarithm of the acquirer's social score. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. In Panel A, the indicator variable *High Skill* takes the value of one if the target belongs to telecommunications, high-tech, and healthcare industries, and zero otherwise. The indicator variable *High R&D* takes the value if the industry-level R&D expenditure (of target firms) is above the sample median, and zero otherwise. The indicator variable *High Skilled employment* takes the value of one if the proportion of skilled workers among all workers is above the sample median, and zero otherwise. In Panel B, the indicator variable *High Financial dependence* takes the value of one if the industry-level financial dependence (of target firms) is above the sample median, and zero otherwise. The indicator variable *High Cash* takes the value if the target's cash holdings is above the sample median, and zero otherwise. In Panel C, the indicator variable *Same-industry* takes the value of one if the target is in the same industry as the acquirer. The indicator variable *Domestic* takes the value of one if the target is in the same country as the acquirer. *labor productivity* is measured as the ratio of sales (in thousands) to the number of employees. The indicator variable *Low labor Productivity* takes the value of one if the target's average labor productivity (before the deal) is below the sample median, and zero otherwise. Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Targets in human-capital-intensive industries

	Number of Employees (log)		
	(1)	(2)	(3)
Social * Post	-0.132**	-0.051	-0.066
	(0.06)	(0.07)	(0.07)
High Skill * Post	0.943		
	(0.61)		
High R&D * Post		0.784**	0.653*
		(0.37)	(0.38)
High Skill per * Post			
High Skill * Social * Post	-0.292*		
	(0.17)		
High R&D * Social * Post		-0.220**	
		(0.10)	
High Skill per * Social * Post			-0.189*
			(0.11)
Control * Post	Yes	Yes	Yes
Target FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Event FE	Yes	Yes	Yes
N	5,804	5,642	5,786
r2	0.880	0.880	0.880

Panel B: Targets that are more financially constrained

	Number of Employees (log)	
	(1)	(2)
Social * Post	-0.057	-0.041
	(0.06)	(0.07)
High Financial dependence * Post	0.741*	
	(0.38)	
High Cash * Post		0.836**
		(0.39)
High Financial dependence * Social * Post	-0.228**	
	(0.11)	
High Cash* Social * Post		-0.222**
		(0.11)
Control * Post	Yes	Yes
Target FE	Yes	Yes
Year FE	Yes	Yes
Event FE	Yes	Yes
N	5,642	5,699
r2	0.880	0.881

Panel C: More opportunities for eliminating redundancy

	Number of Employees (log)		
	(1)	(2)	(3)
Social * Post	-0.088	-0.138**	-0.047
	(0.06)	(0.06)	(0.10)
Same-industry * Post	0.924**		
	(0.41)		
Domestic * Post		0.304	
		(0.45)	
Low Labour productivity * Post			1.188**
			(0.53)
Same-industry * Social * Post	-0.229**		
	(0.11)		
Domestic * Social * Post		-0.076	
		(0.12)	
Low Labour productivity * Social * Post			-0.344**
			(0.14)
Control * Post	Yes	Yes	Yes
Target FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Event FE	Yes	Yes	Yes
N	5,799	5,804	3,705
r2	0.880	0.879	0.880

Table 1.9: Effects on other target firm outcomes

This table presents the estimates of the effect of *Social* score on targets' performance. *Social* is the natural logarithm of the acquirer's social score. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. In columns (1) - (2), the dependent variables are *Staff costs to assets* and *Staff costs per employee*. In columns (3) - (4), the dependent variables are *Sales per Employee* and *Added value per employee*. In columns (5) - (6), the dependent variables are *Material costs per employee* and *Sales to assets*. In columns (7) (8), the dependent variable are *Asset growth* and *Fixed asset growth*. Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Staff_assets	Staff_empl	Sales_empl	Added value_empl	Material_empl	Sales_assets	Asset_growth	Asset_growth(fixed)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Social * Post	0.051***	0.008*	6.005**	0.035*	-0.074*	32.623**	-0.044*	-0.076*
	(0.02)	(0.00)	(2.35)	(0.02)	(0.04)	(13.13)	(0.03)	(0.04)
Control * Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	4,269	4,198	2,751	2,778	2,840	3,139	4,858	4,563
r2	0.832	0.617	0.444	0.618	0.875	0.437	0.195	0.175

Table 1.10: Announcement effects

This table examines the effects of social performance on announcement returns for acquiring firms. In Panel A, I report the CARs for the full sample of acquirers as well as the subsamples of high and low *Social* acquirers. Acquirers are divided into high and low *Social* acquirers according to the sample median of their social performance. In Panel B, the dependent variable is the acquirer's three-day CAR around a M&A announcement. *Social* is the natural logarithm of the acquirer's social score. In Panel C, the dependent variable is the acquirer's three-day CAR around a M&A announcement. Large $\Delta log(Emp)$ is an indicator that equals one if the pre-to-post decrease in log-employment is above the sample median, and zero otherwise. Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Univariate tests

	Full sample		High Social		Low Social		Test of difference	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
CAR(-1, 1)	0.371***	0.077**	0.624***	0.202***	0.12	-0.047	0.504**	0.249**
CAR(-2, 2)	0.467***	0.259***	0.816***	0.412***	0.119	-0.007	0.697***	0.419**
CAR(-3, 3)	0.548***	0.314***	0.825***	0.481***	0.271	0.227	0.554*	0.254*

Panel B: Regressions of CAR (-1, 1)

	Acquirer CAR (-1, 1)		
	(1)	(2)	(3)
Social	0.613**	0.602**	0.563*
	(0.28)	(0.28)	(0.30)
Acquirer controls	Yes	Yes	Yes
Deal controls	No	Yes	Yes
Target controls	No	Yes	Yes
Year FE	Yes	Yes	Yes
Acquirer industry FE	Yes	Yes	Yes
Target industry FE	No	No	Yes
Acquirer and target country FE	No	No	Yes
N	796	786	786
r2	0.190	0.211	0.310

Panel C: Employment and CAR (-1, 1)

	Acquirer CAR (-1, 1)		
	(1)	(2)	(3)
Social	0.197	0.237	0.092
	(0.33)	(0.34)	(0.37)
Large Diff	-3.121**	-2.999*	-3.450*
	(1.55)	(1.55)	(1.76)
Social * Large Diff	0.853**	0.770*	0.933**
	(0.41)	(0.42)	(0.47)
Acquirer controls	Yes	Yes	Yes
Deal controls	No	Yes	Yes
Target controls	No	Yes	Yes
Year FE	Yes	Yes	Yes
Acquirer industry FE	Yes	Yes	Yes
Target industry FE	No	No	Yes
Acquirer and target country FE	No	No	Yes
N	796	786	786
r2	0.194	0.215	0.314

Table 1.11: Subsidiary-level evidence

This table presents the estimates at the subsidiary level. *CSR* is the natural logarithm of the acquirer's CSR score. *Social* is the natural logarithm of the acquirer's social score. *Environmental* is the natural logarithm of the acquirer's environmental score. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. In columns (1) - (3), the dependent variable is the natural logarithm of employment plus one. In columns (4) - (5), the dependent variables are *Staff costs to assets* and *Staff costs per employee*. In columns (6) - (7), the dependent variables are *Sales per Employee* and *Added value per employee*. In column (8), the dependent variable is the natural logarithm of the number of subsidiaries plus one. Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)			Staff_assets	Staff_empl	Sales_empl	Av_empl	No.subsidiaries
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CSR * Post	-0.117*							
	(0.06)							
Social * Post		-0.092*		0.006	0.000	4.414	0.226	-0.042*
		(0.05)		(0.02)	(0.00)	(4.04)	(0.16)	(0.02)
Environmental * Post			-0.044					
			(0.03)					
Control * Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target FE	No	No	No	No	No	No	No	Yes
Sub FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,446	2,446	2,446	1,865	1,828	1,437	1,363	9,047
r2	0.933	0.933	0.933	0.843	0.703	0.555	0.827	0.849

Table 1.12: Propensity score matching

This table reports the results using a propensity score matched sample. Acquirers are divided into high and low CSR subgroups according to the sample median of their *Social* score. I match targets in the high CSR group with those in the low CSR group on their size and employment in the year before the deal, and I ensure targets in these two groups are in the same two-digit SIC code and the same country.

	Number of Employees (log)	
	(1)	(2)
High Social * Post	-0.148*	-0.175**
	(0.08)	(0.09)
Control * Post	No	Yes
Target FE	Yes	Yes
Year FE	Yes	Yes
Event FE	Yes	Yes
N	4,206	4,157
r2	0.875	0.871

Table 1.13: Robustness tests

This table reports the results using a propensity score matched sample. Acquirers are divided into high and low CSR subgroups according to the sample median of their *Social* score. I match targets in the high CSR group with those in the low CSR group on their size and employment in the year before the deal, and I ensure targets in these two groups are in the same two-digit SIC code and the same country.

	Sustainalytics	Layoff	Managerial practices	Exclude US acquirers	Exclude financial targets	Asset sales	More fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Social * Post	-0.460*	0.058**	-0.199*	-0.163**	-0.128**	0.021	-0.160**
	(0.27)	(0.03)	(0.11)	(0.07)	(0.06)	(0.03)	(0.06)
Management * Post			-0.403				
			(0.50)				
Control * Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	No	No	No	No	No	No	Yes
Country-Year FE	No	No	No	No	No	No	Yes
N	2,600	4,437	1,784	4,519	5,510	4,480	5,542
r2	0.870	0.377	0.861	0.876	0.877	0.375	0.908

Appendix

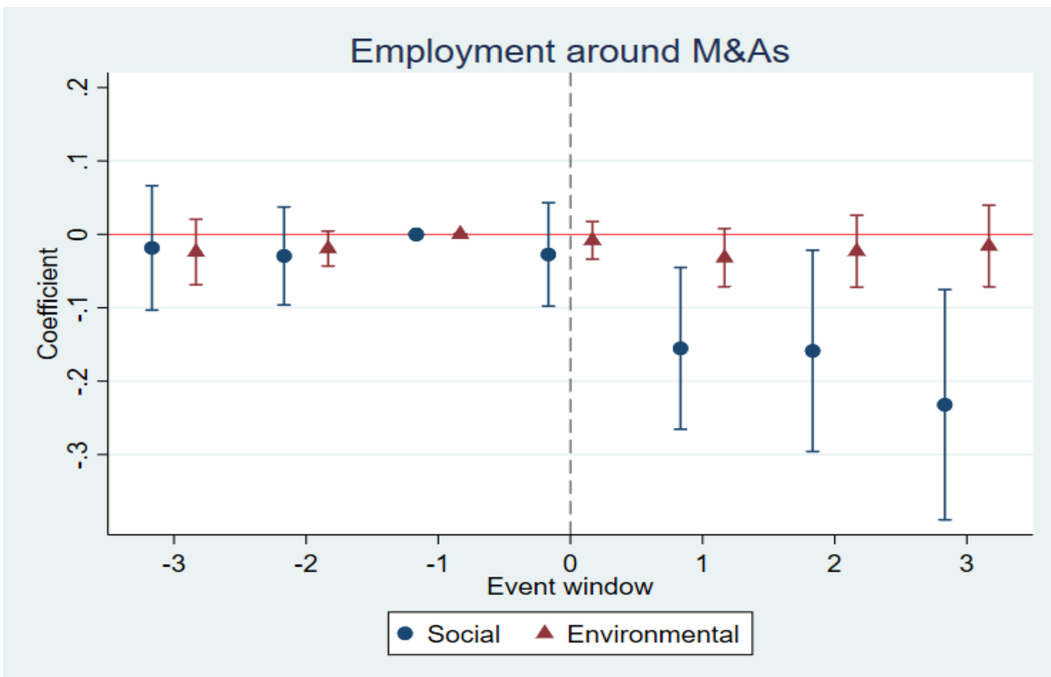
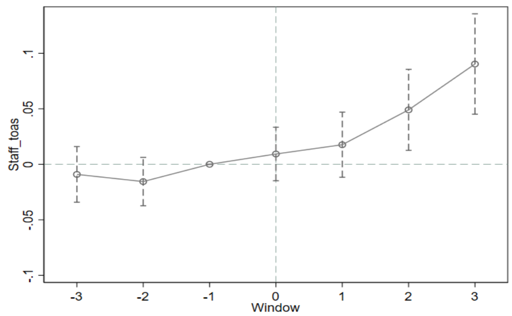
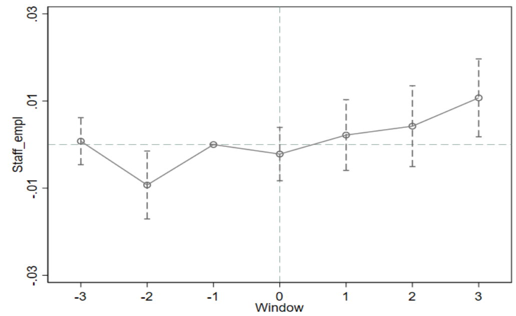


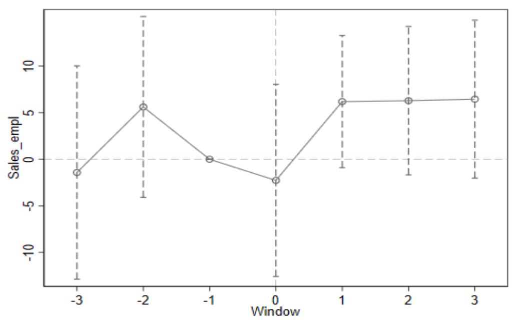
Figure A.1.1: Employment coefficients around M&As (Unbundling CSR)
The figure displays coefficient estimates of the fixed effects model for employment in target firms, with 95% confidence intervals. I include target, year, and event-time fixed effects in my specification.



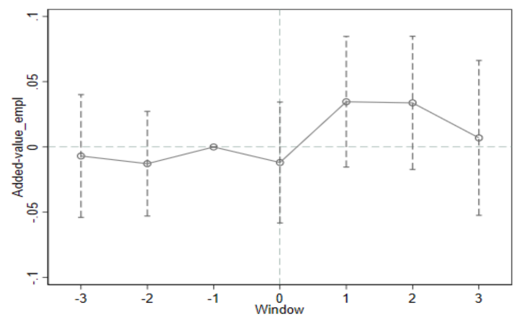
(a) Staff-costs_asset



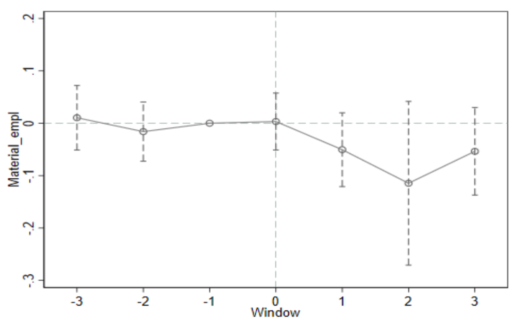
(b) Staff-costs_empl



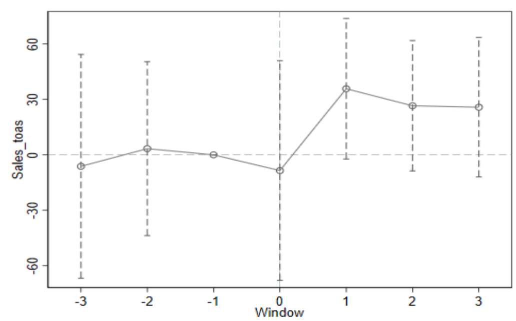
(c) Sales_empl



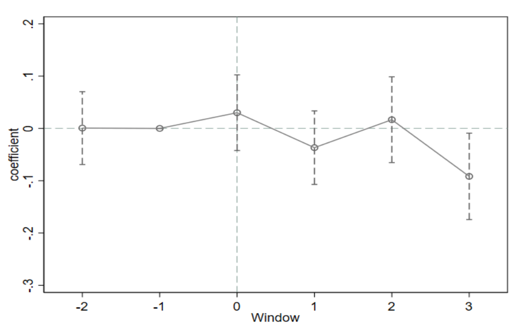
(d) Added-value_empl



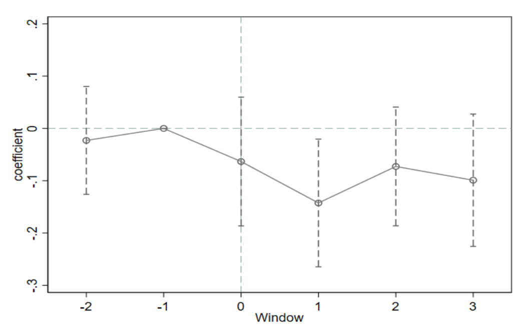
(e) Material-costs_empl



(f) Sales_asset



(g) Asset_growth



(h) Asset_growth(fixed)

Figure A.1.2: Effects on other target firm outcomes
The figure displays coefficient estimates of the fixed effects model for labor productivity, technical efficiency, staff costs, and asset growth in target firms, with 95% confidence intervals. I include target, year and event-time fixed effects in my specification.

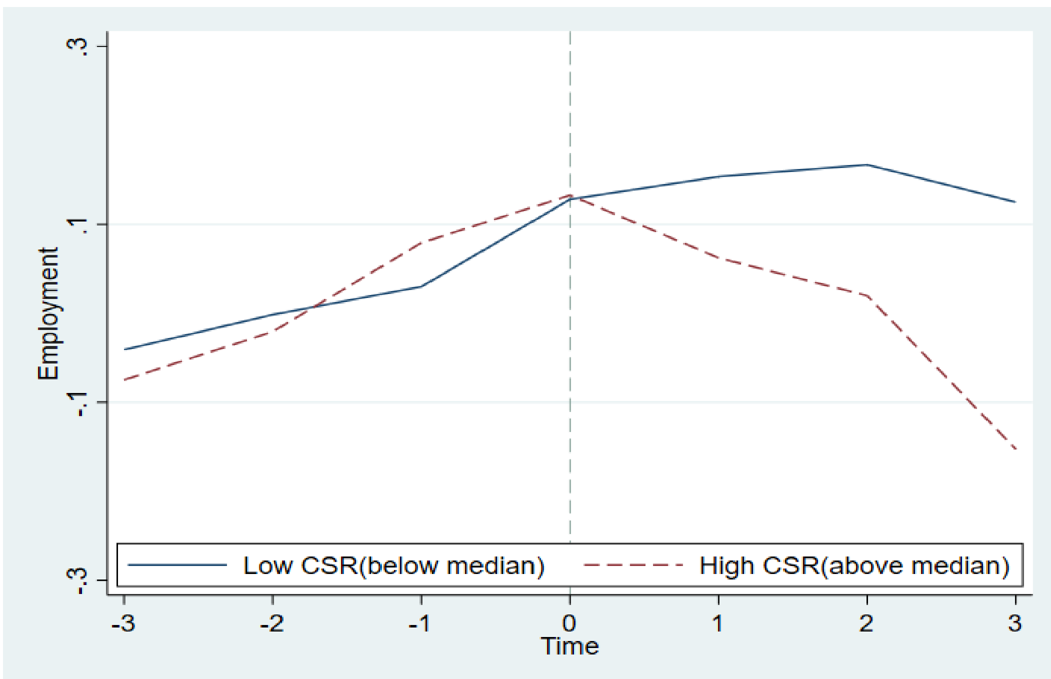


Figure A.1.3: Rescaled average value of employment
The figure presents the yearly rescaled average values of employment for targets in high and low CSR groups. For each year, the rescaling is done by deducting the three-year average before the deal (i.e. $T-3$ to $T-1$) from each annual average figure of employment.

Table A.1.1: Sample construction process

This table describes the sample construction process of the deals in this paper.

Total Number of deals	401,156
Deals with targets in Europe	200,116
Deals where the acquirer has less than 50% of the target's shares before the deal and more than 50% after the deal	163,099
Deals for which I have acquirers' CSR data in the year before the deal	6,851
Deals for which I have acquirers' accounting data in the year before the deal	6,813
Deals for which I have targets' accounting data both before and after the deal	921

Table A.1.2: 5-year event window

This table presents the results for a 5-year event window. The dependent variable is the natural logarithm of employment plus one. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. In columns (1)-(3), *Social* is the natural logarithm of the acquirer's social score. *Environmental* is the natural logarithm of the acquirer's environmental score. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)			
	(1)	(2)	(3)	(4)
CSR * Post	-0.094**			
	(0.04)			
Social * Post		-0.133**		-0.136**
		(0.05)		(0.06)
Environmental * Post			-0.020	0.002
			(0.02)	(0.02)
Control * Post	Yes	Yes	Yes	Yes
Target FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
N	4,385	4,385	4,385	4,385
r2	0.896	0.896	0.896	0.896

Table A.1.3: Controlling for domestic and foreign institutional ownership

This table presents the results that controlling for domestic and foreign institutional ownership. The dependent variable is the natural logarithm of employment plus one. *CSR* is the natural logarithm of the acquirer's CSR score. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. *IO_Dom* is the percentage of total domestic institutional ownership in the acquiring firm.*IO_For* is the percentage of total foreign institutional ownership in the acquiring firm. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)		
	(1)	(2)	(3)
CSR * Post	-0.109**	-0.082*	-0.103**
	(0.05)	(0.05)	(0.05)
IO_Dom * Post	-0.191**		-0.263**
	(0.09)		(0.11)
IO_For * Post		-0.122	-0.410
		(0.26)	(0.30)
Control * Post	Yes	Yes	Yes
Target FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Event FE	Yes	Yes	Yes
N	5,592	5,592	5,592
r2	0.886	0.885	0.886

Table A.1.4: Effects on the employment of acquirers

This table presents estimates of the effect of acquirers' CSR performance on the employment of the acquirers after the deal. The dependent variable is the employment of the acquirers. *CSR* is the natural logarithm of the acquirer's CSR score. *Post* is a dummy variable that takes a value of one for observations in the years after the deal, and zero otherwise. Control variables include *Acquirer Size*, *Acquirer Leverage*, *Acquirer ROA* and *Acquirer Tobin's Q*. Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Acquirer number of employees (log)	
	(1)	(2)
CSR * Post	-0.028	-0.008
	(0.03)	(0.03)
Acquirer size * Post		-0.017
		(0.01)
Acquirer leverage * Post		-0.152
		(0.10)
Acquirer ROA * Post		-0.236
		(0.23)
Acquirer Q * Post		0.060***
		(0.01)
Control * Post	No	Yes
Year FE	Yes	Yes
Acquirer FE	Yes	Yes
Window FE	Yes	Yes
N	3,582	3,458
r2	0.966	0.969

Table A.1.5: Effects on target firm innovation

This table presents the estimates of the effect of *Social* score on other targets' innovation activities. The dependent variable is the natural logarithm of one plus number of patents. *Social* is the natural logarithm of social scores plus one. *Post* is a dummy variable that equals one for the years after an acquisition, and zero otherwise. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Patents	
	(1)	(2)
Social * Post	-0.193***	-0.195**
	(0.06)	(0.08)
Control * Post	No	Yes
Year FE	Yes	Yes
Target FE	Yes	Yes
Event FE	Yes	Yes
N	1,727	1,129
r2	0.747	0.762

Table A.1.6: Announcement effects

This table examines the effects of CSR on announcement returns for acquiring firms. In Panel A, I report the CARs for the full sample of acquirers as well as the subsamples of high and low CSR acquirers. Acquirers are divided into high and low CSR acquirers according to the sample median of their social performance. In Panel B, the dependent variable is the acquirer's three-day CAR around a M&A announcement. *CSR* is the natural logarithm of the acquirer's social score. Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Univariate tests

	Full sample		High CSR		Low CSR		Test of difference	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
CAR(-1, 1)	0.371***	0.077**	0.560***	0.162***	0.183	-0.007	0.377*	0.169*
CAR(-2, 2)	0.467***	0.259***	0.693***	0.440***	0.241	0.000	0.452*	0.440**
CAR(-3, 3)	0.548***	0.314***	0.599***	0.328***	0.497*	0.293*	0.102	0.035

Panel B: Regressions of CAR (-1,1)

	Acquirer CAR (-1, 1)			
	(1)	(2)	(3)	(4)
CSR	0.646***	0.655***	0.583**	0.598**
	(0.21)	(0.21)	(0.24)	(0.24)
Acquirer controls	Yes	Yes	Yes	Yes
Deal controls	No	Yes	No	Yes
Target controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Acquirer industry FE	Yes	Yes	Yes	Yes
Target industry FE	No	No	Yes	Yes
Acquirer and target country FE	No	No	Yes	Yes
N	796	786	795	786
r2	0.154	0.178	0.268	0.286

Table A.1.7: Employment and announcement return (placebo test)

This table examines the effects of environmental performance on announcement returns for acquiring firms. The dependent variable is the acquirer's three-day CAR around a M&A announcement.*Environmental* is the natural logarithm of the acquirer's environmental score. Large $\Delta log(Emp)$ is an indicator that equals one if the pre-to-post decrease in log-employment is above the sample median, and zero otherwise. Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Acquirer CAR (-1, 1)		
	(1)	(2)	(3)
Environmental	0.141	0.162	0.167
	(0.12)	(0.13)	(0.15)
Large $\Delta log(Emp)$	-0.342	-0.499	-0.213
	(0.55)	(0.57)	(0.58)
Environmental * Large $\Delta log(Emp)$	0.130	0.119	0.069
	(0.16)	(0.16)	(0.17)
Acquirer controls	Yes	Yes	Yes
Deal controls	No	Yes	Yes
Target controls	No	Yes	Yes
Year FE	Yes	Yes	Yes
Acquirer industry FE	Yes	Yes	Yes
Target industry FE	No	No	Yes
Acquirer and target country FE	No	No	Yes
N	796	786	786
r2	0.190	0.212	0.310

Table A.1.8: Moral capital channel

This table reports the triple difference-in-differences tests to examine the moral capital channel. In column (1), the indicator variable *Large Acquirer* takes the value of one if the acquirer's size is above the sample median, and zero otherwise. In column (2), *Negative GDP change* is the 1-year percentage decrease in the target's country GDP, with positive changes set to zero. In column (3), the indicator variable *High labor volatility* takes the value of one if the industry-level labor volatility (of target firms) is above the sample median, and zero otherwise. In column (4), the indicator variable *High Responsibility* takes the value of one if the country level "Responsibility is really important" is above the sample median, and zero otherwise. In column (5), the indicator variable *High Work* takes the value of one if the country level "Work is really important" is above the sample median, and zero otherwise. In column (6), *Prior mass layoff* is an indicator variable that takes the value of one if the acquirer has a mass employee layoff in the 5 years before the deal, and zero otherwise. Standard errors are robust and clustered at the acquirer level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)					
	(1)	(2)	(3)	(4)	(5)	(6)
Social * Post	-0.149**	-0.166**	-0.170***	-0.230***	-0.047	-0.220***
	(0.07)	-0.07	(0.06)	(0.08)	(0.09)	(0.07)
Acquirer size * Post	-0.015					
	(0.41)					
High Labour volatility * Post		-0.049				
		(0.38)				
Negative GDP change * Post			8.304			
			(6.33)			
High Responsibility * Post				-0.362		
				(0.41)		
High Work * Post					0.343	
					(0.37)	
Prior mass layoff * Post						-0.845**
						(0.40)
Social * Acquirer size * Post	-0.022					
	(0.11)					
Social * High Labour volatility * Post		-0.011				
		(0.11)				
Social * Negative GDP change * Post			-2.719			
			(1.81)			
Social * High Responsibility * Post				0.124		
				(0.11)		
Social * High Work * Post					-0.138	
					(0.11)	
Social * Prior mass layoff * Post						0.195*
						(0.11)
Negative GDP change			1.237			
			(2.05)			
Control * Post	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Target FE	Yes	Yes	Yes	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes	Yes	Yes
N	5,804	5,642	5,804	5,374	5,374	5,465
r2	0.880	0.879	0.880	0.878	0.878	0.882

Table A.1.9: Social performance and the likelihood of acquisition

This table reports the tests that examine the effects of social performance on the likelihood of being acquirers. The dependent variable *Acquisition* is an indicator variable, which takes the value of one if the firm does the acquisition in a given year, and zero otherwise. *CSR* is the natural logarithm of the acquirer's CSR score. *Social* is the natural logarithm of the acquirer's social score. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Acquisition			
	(1)	(2)	(3)	(4)
Social	-0.003		-0.007*	
	(0.00)		(0.00)	
CSR		-0.000		-0.006
		(0.00)		(0.00)
Acquirer size			0.054***	0.054***
			(0.01)	(0.01)
Acquirer leverage			-0.051**	-0.050**
			(0.02)	(0.02)
Acquirer ROA			0.086***	0.086***
			(0.03)	(0.03)
Acquirer Q			0.008***	0.008***
			(0.00)	(0.00)
Control	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Acquirer FE	Yes	Yes	Yes	Yes
N	38,307	38,307	37,155	37,155
r2	0.406	0.406	0.410	0.410

Table A.1.10: Propensity score matching (with deal characteristics)

This table reports the results using a propensity score matched sample. Acquirers are divided into high and low CSR subgroups according to the sample median of their *Social* score. I match targets in the high CSR group with those in the low CSR group on their size, employment, acquirers' characteristics, and deal characteristics in the year before the deal and I ensure targets in these two groups are in the same two-digit SIC code and the same country. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)	
	(1)	(2)
Social * Post	-0.191**	-0.174*
	(0.10)	(0.09)
Control * Post	No	Yes
Year FE	Yes	Yes
Target FE	Yes	Yes
Event FE	Yes	Yes
N	3,610	3,610
r2	0.871	0.876

Table A.1.11: Mahalanobis matching

This table reports the results using a Mahalanobis matched sample. Acquirers are divided into high and low CSR subgroups according to the sample median of their *Social* score. I ensure targets in these two groups are in the same two-digit SIC code and the same country. In column (1), I match targets in the high CSR group with those in the low CSR group on their size and employment in the year before the deal. In column (2), I match targets in the high CSR group with those in the low CSR group on their size, employment, acquirers' characteristics, and deal characteristics in the year before the deal. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)	
	(1)	(2)
Social * Post	-0.183**	-0.191**
	(0.09)	(0.10)
Control * Post	Yes	Yes
Year_FE	Yes	Yes
Target_FE	Yes	Yes
Event FE	Yes	Yes
N	3,383	2,464
r2	0.872	0.868

Table A.1.12: Propensity sore match (Environmental score)

This table reports the results using a propensity score matched sample. Acquirers are divided into high and low CSR subgroups according to the sample median of their *Environmental* score. I match targets in the high CSR group with those in the low CSR group on their size, and employment in the year before the deal and I ensure targets in these two groups are in the same two-digit SIC code and the same country. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Number of Employees (log)	
	(1)	(2)
Environmental * Post	0.045	0.060
	(0.09)	(0.12)
Control * Post	No	Yes
Year FE	Yes	Yes
Target FE	Yes	Yes
Event FE	Yes	Yes
N	4,075	3,994
r2	0.856	0.858

Table A.1.13: Instrumental variable test

This table presents my two-stage least square estimations. In the first sage, the social score is regressed on two instruments, which are the country's egalitarianism value and the 5-year lagged social score. In the second stage, the dependent variable is the natural logarithm of employment plus one and *Social_adj* is the predicted value of the social score. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	First	Second
	Social * Post	Number of Employees (log)
	(1)	(2)
Egalitarianism * Post	0.225***	
	(0.08)	
Lagged Social * Post	0.473***	
	(0.04)	
Social_adj * Post		-0.299**
		(0.12)
Control * Post	Yes	Yes
Target FE	Yes	Yes
Year FE	Yes	Yes
Event FE	Yes	Yes
Undertidentification test (p-value)	0.000	
K-P F-stat	90.90	
Overidentification test (p-value)		0.132
Observations	3,301	3,301
R-squared		0.035

Chapter 2

Employment Protection and Organizational Structure

2.1 Introduction

Business groups are widespread organizations that operate internal capital and labor markets, a feature that may present important advantages when external markets are plagued by frictions (Khanna and Palepu, 1997; Khanna and Yafeh, 2007). A related recent literature has shown that the ability to rely on internal labor markets (ILMs) allows group-affiliated firms to mitigate labor adjustment costs in the presence of shocks. Faced with growth opportunities, group-affiliated (BG) firms can bypass hiring frictions and expand rapidly (Cestone et al., 2023a); faced with adverse shocks, they can reallocate workers to affiliated units, thus avoiding firing costs (Cestone et al., 2023b). This suggests that labor market regulations that exacerbate, or alleviate, frictions should have a less pronounced impact on BG firms as compared to their stand-alone peers. It also suggests that group-affiliation should become less valuable, hence less prevalent, following major liberalizations of labor markets.

To test these two hypotheses, we exploit a labor market reform that substantially reduced the direct and indirect costs of employee dismissals for Spanish firms. This allows us to provide evidence that group-affiliated firms are shielded from the impact of employment protection legislation (EPL hereafter) and that ILMs are a key factor behind this resilience. By studying the evolution of business groups around the reform, we also provide causal evidence for the longstanding claim that labor market frictions are a major factor behind the emergence of group structures (Khanna and Yafeh, 2007).

In 2012, the Spanish government implemented a structural reform of the labor market, which aimed at reshaping Employment Protection Legislation. The *Real Decreto Ley 3/12* contained various provisions to mitigate the costs of labor adjustment in the face of economic shocks. First, it broadened the definition of fair dismissal. Second, it significantly reduced the monetary compensation for unfair dismissal. Third, it also eliminated the need for an administrative authorization for collective dismissals. Early assessments propose that this legislation substantially reduced dismissal costs and promoted hiring in the Spanish economy (OECD, 2013). These features of the reform make it an ideal setting to study the impact of EPL changes on business groups.

We first focus on the question of whether group-affiliated firms are less sensitive to EPL changes. To guide our empirical analysis, we use a simple model studying how optimal hiring should respond to a change in firing costs in a two-firm business group operating an ILM versus a stand-alone firm. Firing costs induce firms to postpone hiring in the presence of uncertainty about firm profitability; however, business groups can transfer part of the redundant workers within the ILM and avoid such costs. The model generates two main predictions. First, in the presence of an active ILM operated by the group headquarters, a change in firing costs has a smaller impact on the labor demand of BG-firms versus the labor demand of a stand-alone firm. Second, the response to changes in firing costs is more muted in business groups with less frictional ILMs and in groups whose units are exposed to uncorrelated profitability shocks (thus offering more opportunities for internal employee reallocation).

To perform our analysis, we use data from Bureau van Dijk's *Amadeus* database for both public and private firms. The unique feature of this database is that, besides firms' balance sheets and profit and loss statements, it provides information on ownership structure: this allows us to identify pairs of firms with a common controlling shareholder and reconstruct the structure of business groups in Spain.

In our baseline analysis, we rely on a difference-in-difference empirical strategy to study the employment growth response to the 2012 reform in BG and stand-alone firms. More specifically, we compare the evolution of employment growth in firms affiliated with small *two-firm* groups versus their stand-alone peers (Larrain et al., 2019). Comparing stand-alone legal entities with small BGs alleviates the concern that firms affiliated with large groups might respond differently to shocks because of a multitude of unobservable factors, including managerial talent and political connections. However, affiliation to a small group may still provide mechanisms to alleviate the impact of frictions in external factor markets.[1] We find that following the reform, BG firms experience a significantly smaller increase in employment growth when compared to their stand-alone peers: the employment growth rate is about 2.8 percentage points smaller in (*two-firm*) BG firms. We also study the evolution of employment growth in a 7-year window around the reform using a dynamic specification. While we do not observe any pre-reform differential, employment growth (as compared to the pre-reform baseline) in BG firms two and three years after the reform is 3.7 and 4.1 percentage points smaller than in stand-alone firms.

In sum, a major EPL reform aimed at reducing the costs of labor adjustments was not as effective in stimulating hiring by BG firms as it was for stand-alone firms. This suggests that prior to 2012, group-affiliated firms in Spain were partly shielded from the burdensome EPL provisions that the reform set out to soften.

To investigate the mechanisms that may explain this muted response to changes in employment protection legislation, we exploit heterogeneity in firm and group characteristics that can proxy for ILM frictions. In line with our model prediction, we find that the muted response to EPL is concentrated in diversified groups, i.e. those whose affiliated firms operate in different 2-digit SIC industries. We also show that the muted response to EPL is concentrated in geographically focused groups, i.e. those whose affiliated firms are located within the same city. This finding is in line with our model prediction that low ILM frictions (captured here by geographical proximity between subsidiaries) allow groups to bypass employment protection legislation. These results align with previous work showing that firms in

[1] Small *two-firm* groups have been shown to mitigate credit constraints for member firms thanks to collateral and cash-flow cross-pledging (Larrain et al., 2019, 2023). Cestone et al. (2023a) show that industry diversification and geographical focus of groups, rather than group size *per se*, facilitate internal labor reallocation within groups.

diversified but geographically focused groups are in a better position to rely on the Internal Labor Market (Tate and Yang, 2015; Cestone et al., 2023a).

We then focus our analysis on groups with $n \geq 2$ subsidiaries, and compare the employment response to the reform in BG firms with high versus low *ILM Access* (with different measures of *ILM Access*). In particular, we follow Cestone et al. (2023a) and measure the *ILM Access* of a BG firm as the workforce employed in affiliates of the same group (i) located within the same city as the firm, but (ii) active in different 2-digit industries. In line with our ILM story, we find that the employment growth response to the EPL reform is significantly less pronounced in BG firms that enjoy higher ILM Access. Our evidence thus suggests that Internal Labor Markets are a critical feature of group affiliation, allowing groups to bypass labor market frictions and partly insulating them from the impact of EPL legislation.

Do ILMs, and thus group affiliation, become less valuable when major reforms mitigate labor market frictions? We address this question by studying the impact of the reform on firm incorporation and the dynamics of group affiliation. Our empirical strategy is to compare business group affiliation in neighboring Spanish and French regions, with the latter being a valid control as they were not subject to the reform (the *treatment* in our setting). We first analyze the incorporation of new firms as in Bena and Ortiz-Molina (2013): after controlling for firm characteristics and a host of fixed effects, we find that the reform reduces by 10.1 percentage points the likelihood that new firms are established as group-affiliated. We then focus on firms that are classified as stand-alone in the year before the reform, and ask whether they are more or less likely to become group-affiliated afterward. After matching Spanish and French firms and controlling for key firm characteristics, we find that the reform reduces by 3.4 to 4.1 percentage points the likelihood of Spanish stand-alone firms becoming part of a group. Finally, we turn to the group-level analysis, which also provides consistent evidence. We show that the expansion of group affiliation significantly slows down following this large EPL liberalization.

We also investigate alternative explanations for our results. For instance, the observed differential response to the labor reform might simply be a byproduct of internal capital markets (ICMs). Capital reallocation within business groups could allow group firms to bypass costs arising in frictional labor markets, making them less sensitive to changes in

EPL. To investigate this, we first explore the heterogeneity of ICMs within groups, with a particular focus on group cash holdings and tangibility. Secondly, we exploit cross-sectional variation across industries with varying levels of external financial dependence, where access to ICMs is more likely to influence group firm dynamics (Belenzon et al., 2013). However, we do not find strong evidence supporting the ICM channel as a major explanation for our findings.

Our paper builds a bridge between the recent literature on Internal Labor Markets and earlier work attempting to document and explain the prevalence of business groups across periods and countries.

Within the business group literature, many papers have argued that groups are more resilient to shocks than stand-alone firms, due to their ability to reallocate production factors internally (Khanna and Palepu, 1997; Khanna and Yafeh, 2007). This view is supported by the observation that groups are more prevalent where labor and financial markets display important frictions, as is the case in emerging markets. While a large literature has analyzed internal capital markets in groups, evidence that these organizations operate internal labor markets is more recent (Huneeus et al., 2021; Cestone et al., 2023a). Cestone et al. (2023b) provide evidence that more stringent regulations on employee dismissal encourage BG firms to reallocate more workers to their group affiliates when faced with adverse shocks. This suggests that the ability to redeploy workers via the ILM can partly insulate group-affiliated firms from the impact of employment protection regulations. In line with this hypothesis, we provide novel evidence suggesting that stand-alone firms, rather than BG affiliates, are the main beneficiaries of labor market liberalization.

Our paper is also related to the literature studying how labor market frictions affect corporate policies (see Matsa, 2018). While the corporate finance and governance literature has focused on the impact of EPL on leverage (Simintzi et al., 2015) and acquisitions (Dessaint et al., 2017), there is no causal evidence on EPL as a driver of organizational structure. Belenzon and Tsolmon (2015) analyze the relation between group-affiliation and EPL across countries, and show that group affiliation is more prevalent in countries with more rigid EPL, especially in high-turnover industries, pointing to ILMs as the main explanation. By exploiting the 2012 Spanish labor market reform within a DiD setting, we complement this work by providing causal evidence that legislation limiting the burden of dismissal costs for employers reduces the prevalence of business group affiliation.

The rest of the paper proceeds as follows. Section 2 presents the theoretical framework. Section 3 describes the institutional background and the 2012 Spanish labor reform. Section 4 presents the data and the sample construction. Empirical methodology and results are discussed in Sections 5 - 8. Section 9 concludes.

2.2 Theoretical Background

We illustrate our empirical predictions with the aid of a simple model where firms make hiring decisions in the presence of firing costs due to employment protection. A formal solution of the model is provided in the Appendix.

At $t = 0$, each firm chooses to hire L_i workers. At $t = 1$, the firm learns whether it is productive, i.e. there is demand for its product (this has probability $x \in (\frac{1}{2}, 1)$), or in distress, i.e. there is no demand (this has probability $1 - x$). After learning the state of demand, the firm has two options: it can continue production, possibly hiring additional workers ℓ_i, or stop production. Late hiring of additional workers at $t = 1$ entails a training cost $h(\ell) = h\ell^2/2$, hence $h' > 0$, $h'' > 0$ and $h(0) = 0$.[2] Workers hired at $t = 0$ receive on-the-job training at no cost. After $t = 1$, actual production takes place and a productive firm pays wages $w \times (L_i + \ell_i)$ to the workforce and generates revenues $g(L_i + \ell_i)$, where the function g satisfies $g' > 0$ and $g'' < 0$. We also assume $g''' > 0$ as e.g. in a Cobb-Douglas production function. Being in distress at $t = 1$ means that the firm will be unable to generate any revenues, hence it is better to liquidate the assets for proceeds C, fire workers and stop production. If the firm fires workers, it bears a separation cost $F < w$ per worker.

There are two types of organizations in our model, business groups and stand-alone firms. A stand-alone firm faces the following hiring decision at $t = 0$:

$$\max_{L_{sa}} x[g(L_{sa} + \ell_{sa}) - w(L_{sa} + \ell_{sa}) - h\frac{\ell_{sa}^2}{2}] + (1 - x)[C - FL_{sa}],$$

[2] Blatter et al. (2012) provide evidence that marginal costs of hiring skilled workers increase in the number of hires, supporting a convex cost assumption. With linear hiring costs, firms would either hire all workers either at $t = 0$ or at $t = 1$ depending on the relative size of the hiring and firing cost per worker.

anticipating that after learning it is productive, the firm hires ℓ_{sa} additional workers so as to maximize $g(L_{sa}+\ell_{sa})-w(L_{sa}+\ell_{sa})-h(\ell_{sa})$, i.e. optimal late hiring after good news ℓ_{sa} satisfies $g'(L_{sa}+\ell_{sa})=w+h\ell_{sa}$ for a given initial stock of employees L_{sa}. If in distress, the firm optimally fires all its workers at cost FL_{sa} and liquidates its assets.

In the Appendix, we show that if h is "large enough" relative to F, the firm chooses to hire in both periods: $L_{sa}>0$ and $\ell_{sa}>0$.[3] The firm equates the expected marginal cost of late training to the expected marginal cost of firing: $h\ell^*_{sa}=\frac{1-x}{x}F$, which together with

$$g'(L_{sa}+\ell^*_{sa})=w+\frac{1-x}{x}F \tag{2.1}$$

defines the optimal hiring policy for a stand-alone firm. Equation 2.1 implies that $\partial L^*_{sa}/\partial F<0$: in the presence of uncertainty about future profitability, firing costs discourage hiring.

A business group consists of two firms (k and j) controlled by a common parent. Hiring and firing decisions at both $t=0$ and $t=1$ are made at the firm level; however, the $t=1$ decision whether to adjust labor resorting to the external or the internal labor market (ILM) pertains to the group headquarters that has total group value as its objective function. For simplicity, we assume that the ILM is frictionless: workers can be reallocated at no extra cost across the two units. However, following Cestone et al. (2023a), we assume that only a fraction $\mu\in(0,1]$ of a subsidiary's existing stock of workers can be redeployed to the other subsidiary. This assumption captures, for instance, the fact that some workers employed in a group affiliate may not agree to be reallocated to another affiliate if this falls outside a reasonable commuting distance from the current job.

We assume that the shocks affecting the two group units are not perfectly correlated. When firm j is in distress, the conditional probability of k being productive is $1-\nu>0$. When firm j is productive, the conditional probability that k is in distress is $1-\lambda>0$. In the presence of firing costs, whenever unit j is in distress while unit k is productive (hence with probability $(1-x)(1-\nu)$), the group headquarters reallocates $i\leq\mu L_j$ workers from j to

[3] In the Appendix, we show that when instead late-training costs are small relative to firing costs, the firm adopts a "wait-and-see" approach and postpones all hiring after uncertainty is resolved.

k. We focus here on the symmetric equilibrium where both BG units hire at $t = 0$.[4] In this equilibrium, when firm j is profitable while firm k is not (which happens with probability $x(1-\lambda)$, firm j hires $\ell_j^I > \mu L_k$ workers externally at $t = 1$, where ℓ_j^I solves:

$$\max_{l_j} g(L_j + \ell_j) - w\ell_j - \frac{h}{2}(\ell_j - \mu L_k)^2. \tag{2.2}$$

If instead both firm j and k are profitable at $t = 1$ (which happens with probability $x\lambda$), firm j hires ℓ_j^E workers externally, where ℓ_j^E solves:

$$\max_{l_j} g(L_j + \ell_j) - w\ell_j - \frac{h}{2}(\ell_j)^2. \tag{2.3}$$

With this in mind, BG-firm j's hiring problem at $t = 0$ can be written as:

$$\max_{L_j} \lambda g(L_j + \ell_j^E) + (1-\lambda)g(L_j + \ell_j^I) - w(L_j + \ell_j^I + \ell_j^E) + \frac{(1-x)}{x}[C - FL_j + (1-\nu)\mu F L_j].$$

The optimal hiring policy for a BG-affiliated firm takes into account the fact that ILM reallocation across units is feasible at $t = 1$. When firm j is profitable and firm k is not, firm j can draw on the redeployable workers of firm k (μL_k) and reduce its hiring costs. When firm j is unprofitable and firm k is healthy, firm j can bypass firing costs by redeploying a fraction μ of its workers to k. This generates two intuitive results that we formally derive in the Appendix and present here.

Result 1: The impact of a change in firing costs on $t = 0$ hiring is larger in absolute value for the stand-alone firm than a group-affiliated firm: $|\frac{\partial L_{sa}}{\partial F}| > |\frac{\partial L_j}{\partial F}|$.

Result 2: The sensitivity of $t = 0$ hiring to firing costs for a BG-firm is: (i) lower in absolute value, the lower the correlation of shocks across group subsidiaries: $\frac{\partial^2 L_j}{\partial F \partial \nu} < 0$; (ii) lower in absolute value, the more redeployable are its workers via the Internal Labor Market: $\frac{\partial^2 L_j}{\partial F \partial \mu} > 0$.

[4] As each unit does not internalize the externality it exercises on the affiliate by hiring a pool of workers available on the ILM, there are also two equilibria where one BG unit postpones all hiring until $t = 1$ and absorbs all its workforce from the other unit at $t = 1$, whenever possible: $L_k^* = 0$, $\ell_k^* \leq \mu L_j$.

2.3 Institutional Background and 2012 Spanish Labor Reform

2.3.1 Institutional background

Over the last half-century, the Spanish government has implemented legislation aimed at protecting workers from unfair redundancies. These regulations detail the procedures for redundancy to be followed, including provisions for notice periods, the involvement of third parties (e.g. courts, workers' representatives, labor unions, etc.), and the process for workers to sue their employers (Bassanini et al., 2009). They also define the conditions for fair dismissal and the sanctions for breaching these provisions, enabling workers to claim substantial monetary compensation after a wrongful termination.

In addition to contractual losses, workers may also receive compensation for punitive damages and emotional distress. Notably, punitive damages often comprise a large percentage of settlement awards, significantly increasing an employer's liability. Furthermore, since a jury determines these damage awards without a clear formula, the final settlement amount becomes unpredictable for employers. Consequently, when firms discharge employees, they face substantial firing costs. These costs encompass not only the severance payment itself but also legal fees and lawsuit settlements resulting from violations of employment protection legislation.

Compared to other countries, the dismissal cost in Spain is high. For example, an employee with 20 years of tenure could receive 30 months' wages for an unfair dismissal (and 12 months' wages for a fair dismissal). In contrast, the average severance payment for unfair dismissal in other OECD countries is much lower, at only 13.7 months' wages in total (OECD, 2013). Moreover, temporary contract workers are entitled to severance payment equivalent to 12 days of salary per year of service at the end of the contract. They can also receive the same severance payment as workers with permanent contracts in cases of unfair dismissal.

Moreover, in the Spanish labor market, the vast majority of firms declare dismissals as unfair (Bentolila et al., 2012). This is primarily due to the difficulty employers faced in proving an economically justified dismissal before 2012. When dismissed workers sue the company, firms lose in three out of four cases in court (Bentolila et al., 2012). As a result, employers often avoid court proceedings (only 2% of dismissals are taken to court), preferring instead to declare dismissals as unfair. This strategy involves disbursing a 45-day severance payment upfront, regardless of the contract type.

2.3.2 Spanish 2012 labor reform

The global financial crisis has a big impact on the Spanish economy. Since the beginning of the crisis, the number of unemployed workers in Spain increased by more than 4 million. In 2010, the country's GDP per capita fell by 4.9%, and its labor market experienced the third-highest rise in unemployment levels in Europe, behind only Greece and Cyprus. The unemployment rate in Spain peaked at 26.7% in October 2013, nearly three times the European average level of 10.8%. Even more worryingly, the proportion of long-term unemployment, those who are unemployed for more than 12 months, also raised dramatically and accounted for 50.4% share of all unemployment in 2013 (versus 19.1% in 2007).

In response to the continued economic decline and surge in unemployment following the crisis, the Spanish government adopted the 2012 labor market reform. This was introduced as Royal Decree-Law 3/2012 in February 2012 and was ratified, without substantial modifications, by the Spanish Parliament in July 2012. The government hoped that the reform would make firms more prone to adjust to changing business conditions, increasing their internal flexibility. This, in turn, was expected to reduce the overall rate of job destruction and stimulate job creation. The reform comprises several main components shown as follows (OECD, 2013).

Firstly, the reform prioritized collective bargaining agreements at the firm level over those at the sector or regional level. It simplified the process for firms to opt out of a collective agreement and implement flexibility measures to curtail job losses. For instance, the reform allows employers to unilaterally change working conditions - such as working hours, schedules, and wages - provided there are objective economic or technical reasons.

Secondly, the reform reshaped the definition of fair economic dismissal, establishing objective circumstances that could justify the termination of a contract. For instance, a dismissal could be justified if a firm experienced three consecutive quarters of declining revenues or ordinary income. Moreover, the firm no longer needed to prove that the termination was essential for its future profitability. Thus, the justification of fair economic dismissals became broader and more straightforward than before. With respect to collective dismissals, the requirement of administrative authorization for collective redundancies was eliminated after 2012.

Thirdly, the reform reduced the amount of monetary compensation for unfair dismissal to 33 days' wages per year of service, capped at a maximum of 24 months. Previously, it was 45 days' wages per year of service with a maximum limit of 42 months. Additionally, the reform relieved employers from paying workers' interim wages between the dismissal's effective date and the final court ruling. However, despite the significant reduction in compensation for unfair dismissal brought about by the reform, the severance pay in Spain remains high compared to other OECD countries.

Finally, the reform introduced a new permanent contract for full-time employees in small firms (those with fewer than 50 employees), known as the *Contrato de Apoyo a Emprendedores*. This contract offers several hiring incentives and fiscal rebates. Most importantly, it extends the trial period to one year, during which employers do not need to compensate any dismissals. The longer probationary period enhances employment flexibility, enabling employers to better experiment with and screen new workers. This reduces the risk of mismatches and decreases potential costs.

Overall, the 2012 labor market reform effectively reduced dismissal costs and infused greater dynamism into the Spanish labor market.

2.4 Data and Summary Statistics

2.4.1 Sample selection

Data on group affiliation are constructed using Bureau van Dijk's (BvD) Amadeus ownership and financial database, covering the period from 2009 to 2015. The key advantage of these data is their comprehensive and representative coverage of both private and public firms in Europe thanks to mandatory disclosure regulations for private firms (Bernard et al., 2018).

The accounting information is obtained from Amadeus and includes balance sheet and profit and loss figures. The ownership data detail the names of shareholders, their ownership stakes, and information regarding whether they are individuals (families), non-financial companies, banks or other types of corporations.

Following Belenzon et al. (2013) and Belenzon and Tsolmon (2015), we define a business group as a collection of at least two legally distinct firms controlled by an ultimate shareholder. Specifically, a firm is classified as group affiliated if it meets at least one of the following criteria: (1) the firm itself is a parent company of another firm in our sample (i.e., it has a subsidiary); (2) its controlling shareholder is a corporation (i.e., it is a subsidiary); (3) it shares the same controlling shareholder with at least one other firm in our sample. The minimum ownership stake required to be considered as a controlling shareholder is 50%. Any firm that does not meet at least one of the three criteria above is a stand-alone. Since some groups have thousands of affiliated firms (Larrain et al., 2021), comparing BG and standalone firms might not be appropriate as affiliated firms enjoy financial and managerial support from the rest of the group. Therefore, a crucial step for our identification strategy is to focus on business groups with only two firms (*two-firm BGs*) where the differences with stand-alone firms are likely to be minimized (Larrain et al., 2019).

To further ensure the quality and homogeneity of our data, we apply several restrictions. First, since the main target of this reform is small and medium-sized employers, we limit the sample to firms with fewer than 100 employees (OECD, 2013).[5] Second, we exclude firms whose affiliation status changes more than once during the sample period to avoid potential endogeneity issues. Third, we require that a firm appears in our sample for at least one year before the reform, and at least two years afterwards. Finally, in order to minimize endogeneity concerns due to omitted variable bias, we need to ensure that business group and stand-alone firms are as similar as possible. To do this, we perform a one-to-one propensity score matching, based on the three-digit SIC code, size (book assets) and the number of employees in the year before the reform.[6]

[5] This report suggests that the effect of the reform is concentrated in small and medium firms (those below 100 employees). Our results remain unchanged if we include firms with more than 100 employees.

[6] Figure A.2.1 displays the distribution of size and employment for both sets of firms. As expected from the matching procedure, both distributions are basically overlapping.

2.4.2 Summary statistics

Table 2, panel A, provides detailed definitions and summary statistics for the main variables in our analysis. To minimize the effects of outliers, we winsorize all continuous variables at the 1st and 99th percentiles. Our main dependent variable is *Employment Growth*, calculated as the change in the log of firm employment between years t and $t-1$. Employment growth has a mean value of -3%. The rest of the panel summarizes our control variables. For example, the average firm in our sample has a book value of assets of €2.72 million, a leverage ratio of 50%, and tangibility of 41%. In terms of performance, median ROA is 1.07%, and sales growth of -2%. Panel A also presents industry characteristics. For example, industry labor intensity is the average ratio of employment relative to sales, computed at the two-digit SIC code using U.S. data. Following Rajan and Zingales (1998), we measure the external financial dependence of an industry as the average fraction of investment not financed by internal cash flows, and external equity dependence as the average fraction of investment financed with equity.

Panel B summarizes the characteristics of business group firms and their matched stand-alone firms. As expected from the matching procedure, the average size and employment are similar. However, BG firms have higher leverage and tangibility, lower cash holdings, a lower sales-to-assets ratio, and greater sales growth, although most differences are not economically relevant. In Panel C, we show the characteristics of business groups, focusing on those for which we have information on their geographical location. This reduces the sample size to 6,530 observations. We define a *BG (same city)* indicator, which equals 1 if both firms are located in the same city, and 0 otherwise. The *BG (same city)* indicator has a value of 0.73, showing that most groups in our sample are geographically close. The *BG (same sic2)* indicator is equal to 1 if both firms belong to the same two-digit SIC industry, and 0 otherwise. Finally, the *BG (ILM access)* indicator is defined as taking a value of 1 if both of the following criteria are met: (i) both firms are located in the same city, and (ii) belong to different two-digit SIC industries.

2.5 Empirical Methodology and Main Results

2.5.1 Employment response to the EPL reform: BG vs stand-alone firms

We employ a DiD approach to compare the employment growth of firms belonging to a two-firm business group with that of stand-alone firms. Specifically, we run the following regression:

$$Employment\ Growth_{i,t} = \alpha + \beta BG \times Post_{i,t} + \lambda Controls_{i,t-1} + \delta_i + \zeta_t + \epsilon_{i,t} \qquad (2.4)$$

where i identifies firms, t years. $Employment\ Growth_{i,t}$ is the growth in employment for firm i in year t. BG_i is a dummy that equals 1 if firm i is affiliated with a two-firm business group, and 0 otherwise. $Post$ is a dummy equal to 1 if the observation year is after 2012, and 0 otherwise. All specifications include firm (δ_i) and year fixed effects (ζ_t), which control for firm-level time invariant characteristics and country-level economic conditions. $Controls_{i,t-1}$ is a vector of lagged firm-level control variables, including size (logarithm of total assets), leverage, tangibility, cash holdings, cash flow, and ROA. We cluster the standard errors at the firm level.

Table 2.2 reports the regression results, with our focus on the interaction term $BG \times Post$, and β being our coefficient of interest. That is, we want to understand whether there is a differential employment growth response to the 2012 reform across group-affiliated versus stand-alone firms. Our results in columns (1) and (2) show that β is -0.028 and significant at the 1% level, indicating that the post-reform increase in employment growth is significantly less pronounced for group-affiliated firms. In terms of economic significance, the drop in employment growth for business groups is sizable both when compared to the sample mean (-0.03) and the standard deviation (0.296). Controlling for $Post$ in column (1) or adding year-dummies in column (2) seems not to change our results. In column (3), we additionally control for firm-level lagged variables, with β remaining virtually unchanged.

Next, we substantiate the validity of our difference-in-differences estimates by showing the differential pre-trends between group and stand-alone firms.[7] Specifically, we set up a dynamic specification by replacing the $Post$ dummy in Equation 2.4 with six year dummies, $Reform_t$, where t ranges from -3 to 3. $Reform_t$ means t years since the 2012 labor reform. t = -1 indicates the year 2011 and is omitted from the regression to form the benchmark year. The estimates are reported in column (4). We do not observe any statistically significant differences in employment growth in the years before the reform, suggesting that our results are not driven by pre-reform trends. However, afterwards group firms exhibit a significantly lower employment growth (see Figure 2.2).

Since our results can be driven by industry and/or regional shocks, in columns (5)-(6), we include industry-year and region-year fixed effects, yet the coefficient estimates of $BG \times Post$ remain negative and significant. Last, we deal with the concern that some specific provisions in the reform apply only to firms below certain size thresholds, notably 50 and 25 employees (OECD, 2013).[8] This might explain a differential response to the reform if group firms and stand-alone firms belong to different size categories. To mitigate this concern, we re-estimate Equation 2.4 after ensuring that group and their matched stand-alone firms have exactly the same number of employees in 2011 (a restriction that results in a significant reduction in the number of observations). As shown in column (7), our results remain qualitatively unchanged.

Overall, the results in Table 2.2 show that group-affiliated firms experience lower employment growth after the reform relative to stand-alone firms. As the reform reduced firing costs, our results are in line with our model prediction: if group-affiliated firms can bypass labor market frictions (including firing costs) by using Internal Labor Markets, they are less affected by changes to employment protection regulation. In the next section, we set to test other predictions from our model.

[7] Figure 2.1 illustrates the results in an event-study fashion.

[8] For example, the reform extended an existing subsidy, equivalent to 40% of ordinary severance pay, to all cases of fair dismissal for firms employing fewer than 25 workers.

2.5.2 Alternative empirical strategy: comparing firms in neighboring Spanish and French regions

To check the robustness of our findings we adopt an alternative empirical strategy. Specifically, we compare the evolution of employment growth for group and stand-alone firms in Spain and France, as the latter were not affected by the reform. The idea is as follows: if our results are driven by unobservable characteristics common to business groups, then they should also be present among French groups. To explore this, we identify firms located in neighboring Spanish and French regions, with the former being the treated group and the latter the control.

In Figure 2.3, we illustrate the trend of employment growth for both Spanish and French groups and stand-alone firms for the period from 2009 to 2015. This figure displays the yearly re-scaled average values of employment growth for the entire sample period. We observe that, compared with firms in France, both group and stand-alone firms in Spain (treated) exhibit a positive trend in employment growth following the reform. More importantly, firms affiliated with a Spanish business group experience a smaller increase in employment growth relative to stand-alone ones, whereas there are no differences among French groups and stand-alone firms. A more formal regression analysis in Table A.2.3 in the Appendix shows that our results are similar when controlling for firm characteristics as well as firm and year fixed effects. Both stand-alone and business group firms' employment growth is higher in Spain, and the differential is larger for stand-alone ones. Overall, our results seem to confirm the finding that groups' employment grows less following the reform, consistent with the idea that affiliation helps firms evade some of the effects of employment protection legislation.

2.6 Evidence on the Importance of Groups' Internal Labor Markets

In the previous section we show that BG firms are less sensitive to changes in EPL when compared to stand-alone firms. This is compatible with our model, where internal labor markets allow BG-affiliated firms to bypass frictions generated by employment protection regulations. However, an alternative explanation for our baseline result could be that BG firms are less responsive to both adverse and positive shocks due to their ability to access the group's internal capital market(Buchuk et al., 2014; Almeida et al., 2015). By relying on our model, in this section, we focus on business groups only and test additional predictions that are specific to a theory of internal labor markets.

2.6.1 BG firms with greater ILM access

Our model predicts that the sensitivity of BG firms' hiring to firing costs is smaller, the more redeployable are its workers via the group's ILM. In Table 2.3, we test this prediction using different measures of ILM's (lack of) frictions inspired by Huneeus et al. (2021) and Cestone et al. (2023a).

First, we exploit heterogeneity in geographical concentration among our two-firm business groups. The geographical distance between group firms is potentially a significant determinant of frictions within the internal labor market. This is because moving employees across firms is likely to incur substantial costs, both tangible (such as relocation expenses) and intangible (employee resistance to relocation tends to increase with the distance of the move). Moreover, geographical proximity between different subsidiaries could promote prior communication, thereby reducing information asymmetry regarding workers' characteristics. Therefore, ILM frictions should be less pronounced for business groups located in the same city. We expect firms affiliated with these groups to enjoy fewer benefits from the EPL reform.

We interact *BG (same city)* and *BG (different city)* with *Post*, showing the results in Table 2.3. Column (1) shows that the coefficient on *BG (same city)* $\times$ *Post* is -0.029 and significant at the 1% level, while that on *BG (different city)* $\times$ *Post* is -0.009 and statistically insignificant. This indicates that the diminished sensitivity of employment growth to the labor reform is more pronounced among same-city groups. We then exploit heterogeneity in industry diversification across our two-firm groups. Tate and Yang (2015) document more active ILM within diversified firms. Industry diversification allows different units in a group to be exposed to unrelated sectoral shocks, thus creating more scope for workforce reallocation across divisions. Thus, if diversified groups have more opportunities for ILM reallocation, BG firms operating in different industries should be less sensitive to the EPL reform.

To test this prediction, we interact *BG (same sic2)* and *BG (different sic2)* with *Post* in column (2). The results show that the coefficient of *BG (different sic2)* $\times$ *Post* is negative and significant, while that on *BG (same sic2)* $\times$ *Post* is not. This result suggests that our results are stronger among diversified groups, which is in line with the ILM mechanism.

Finally, we follow Huneeus et al. (2021) and Cestone et al. (2023a) and use our *BG (ILM access)* dummy for groups that are located in the same city and diversified across industries. Our results are again consistent with our predictions in column (3), as only the coefficient of *BG (ILM access)* $\times$ *Post* is negative and statistically significant. This indicates that our results are more pronounced for group firms wither better ILM access.

Taken together, the results in this section provide support for the ILM mechanism: the diminished sensitivity of BG employment growth to the 2012 labor reform is mainly exhibited by groups with greater ILM access.

2.6.2 Exploiting industry labor intensity

Next, we exploit cross-sectional variation in labor dependence across industries. If business groups can effectively reallocate employees across firms through ILM, this ability should be particularly valuable for firms operating in labor-intensive industries. This is due to the high frequency of labor adjustments in these industries. If group firms benefit less from EPL liberalization due to their internal labor markets - leading to diminished sensitivity of employment growth to the reform - we should expect this effect to be more pronounced for firms in industries that heavily rely on labor.

Following the previous literature, we construct three indicators based on U.S. data to measure the degree to which an industry is labor-dependent (Levine et al., 2020; Belenzon and Tsolmon, 2015). The first measure, '*labor intensity*', is the ratio of employment relative to sales. The second measure is '*labor volatility*', defined as the standard deviation of employment relative to fixed assets. The third measure is '*labor turnover*', defined as the absolute change in employment relative to lagged employment.

To test our predictions, we split the sample along the median of these three industry labor-dependence measures. As reported in columns (1)-(2) of Table 2.4, the coefficient on the interaction is only significant in the high labor-intensive sample. In columns (3)-(6), we also find that the results are stronger, both in terms of magnitude and statistical significance, in firms that belong to labor dependent industries. Overall, we show that the results do indeed come mostly from industries that heavily depend on labor, which is in line with our ILM argument.

2.6.3 Large and small business groups

To provide further evidence in support of ILM we now include larger business groups in our sample. That is, we do not limit our sample of groups to '*two-firm*' business groups, comparing employment growth across groups with a much more dispersion in access to the ILM.[9] We predict that firms affiliated to a business group with easier access to the ILM should be less influenced by the labor reform, experiencing lower employment growth afterwards. We estimate the following specification:

$$Employment\ Growth_{i,t} = \alpha + \beta ILM\ Access \times Post_{i,t} + \lambda Controls_{i,t-1} + \delta_i + \zeta_t + \epsilon_{i,t} \quad (2.5)$$

where $ILM\ Access_i$ represents different measures of internal labor market access (measured in the year 2011) for the BG firm i. *Post* is a dummy variable equal to 1 if the observation year is after 2012, and 0 otherwise. $Controls_{i,t}$ is a vector of control variables used in Equation 2.4. All specifications include firm (δ_i) and year fixed effects (ζ_t).

We show the results in Table 2.5. In column (1), we use group size (the number of firms affiliated to the group) as our first measure of the potential internal labor market. We argue that a larger group has access to more human capital. In line with this intuition, the coefficient estimate on the interaction term of *Group Size* × *Post* is -0.001 and significant at the 1% level. This indicates less employment growth after the reform for firms affiliated with larger groups where access to the internal labor market is presumably easier. Furthermore, we follow Huneeus et al. (2021) and Cestone et al. (2023a) and take geographical proximity

[9] In Table A.2.1, we present summary statistics for the sample that only consists of BG firms.

and industry diversification within the group's workforce into consideration. Specifically, our second measure, *ILM Access (Employment)*, is equal to the sum of employment of all other group affiliates that are located in the same city but do not belong to the same 2-digit industry as the focal firm. In column (2), the coefficient on the interaction term, *ILM (Employment)* $\times$ *Post*, is negative and significant, which indicates that the increased employment growth in response to the EPL liberalization is less pronounced the larger the ILM access. Finally, in column (3), we define our third measure of *ILM Access* as the number of other group affiliates within the same group that are located in the same city but not in the same 2-digit industry as the focal firm. In line with our previous findings, we continue to find a negative and significant coefficient on the interaction term between *ILM Access* and *Post*. Taken together, these results suggest that BG firms with greater ILM access take less advantage of EPL liberalization, thus enjoying lower employment growth after the reform.[10]

It is worth noting that there are certain limitations to the above test. In an ideal experiment, BG firms with varying levels of access to the ILM should be identical in all other respects once controlling for firm characteristics and fixed effects. However, this is nearly impossible to achieve. In particular, BG firms with greater *ILM Access* might belong to groups that also have more scope for using internal capital markets. For example, our ILM measure, *Group Size*, is also likely to be positively associated with a more active internal capital market. In subsection 2.8.1, we investigate whether internal capital markets drive the differential post-reform employment growth, failing to find strong evidence suggesting that this is the case.

2.6.4 Group-level analysis

In this section, we aggregate the data at the group level to examine the evolution of group-level employment growth in business groups with different levels of ILM access. The previous firm-level analyses highlight the importance of ILMs, showing that BG firms experience lower employment growth after the EPL liberalization. However, differences in employment

[10] In Table A.2.2, we include only firms affiliated with a two-firm business group, and repeat the similar analysis. We find consistent evidence that BG firms with greater ILM access are insulated from this labor reform.

growth across firms within a group do not necessarily imply changes in aggregate group-level employment growth. Given that labor adjustment costs are different across firms, some BG firms may be less impacted by the labor reform, while others within the same group might be reactive, leading to greater employment growth. In such a scenario, we might not find ILM evidence in terms of the aggregate employment growth of a group.

To empirically evaluate the impact of ILM on group-level employment growth, and in line with the previous section, we construct three measures capturing the extent of a group's capability to utilize the internal labor markets. Our first ILM access measure is group size, which is measured by the total number of firms within a group. Second, we consider the geographic proximity of group firms and compute the total number of duplicated firms within a business group located in the same city. Third, we focus on group diversification by calculating the number of firms belonging to distinct two-digit SIC industry codes. Importantly, all these ILM measures are defined based on data from 2011. We then aggregate firm-level employment to the group level and conduct the following regression, controlling for group and year fixed effects:

$$Group\ Emplgro_{g,t} = \alpha + \beta ILM\ Access_{g,t} \times Post + \lambda Controls_{g,t-1} + \delta_g + \zeta_t + \epsilon_{j,t} \tag{2.6}$$

where g identifies groups, t identifies years. $Group\ Emplgro_{g,t}$ is employment growth for group g in year t. $ILM\ Access_g$ represents different measures of ILM access (measured in the year 2011) for the group g. $Post$ is a dummy variable equal to 1 if the observation year is after 2012, and 0 otherwise. $Controls_{g,t}$ is a vector of lagged group-level control variables. All specifications include group (δ_g) and year fixed effects (ζ_t).

Table 2.6 displays the group-level regression results. The coefficient on $Group\ Size \times Post$ ranges from -0.013 to -0.016 and is statistically significant at the 1% level. This indicates that larger groups experience a smaller increase in employment growth following the reform. In Table 2.7, we further investigate other group characteristics with varying levels of ILM access by considering geographical proximity and industry diversification within the group. As expected, the coefficients on the interaction term are negative and significant, suggesting that groups with greater ILM access are less sensitive to changes in external labor market frictions.

As discussed previously, we acknowledge that the measures of ILM access (measured by the number of group units) here do not entirely eliminate the effects of ICM. In Table A.2.5, we provide evidence suggesting that the ICM is not driving the differential response to the labor reform.

2.7 Does EPL Liberalization Affect Group Affiliation?

2.7.1 New firm incorporation

A corollary of our previous results is that EPL reforms that mitigate firing costs remove one of the advantages that business groups hold over stand-alone firms. To the extent that groups emerge as organizational forms that allow firms to bypass frictions (Khanna and Yafeh, 2007), we expect the 2012 EPL reform to drive down group affiliation.

For this purpose, we borrow the setting from Bena and Ortiz-Molina (2013), who study the role of pyramidal ownership structures in the incorporation of new firms. Here we also focus on new firm incorporation and test whether new firms are less likely to be part of groups after the reform, when access to their internal labor markets is less beneficial. We use the year of incorporation to calculate firms' age, defining new firms as those between 1-3 years old (some firms only first appear in our data when they are 2 or 3 years old). The sample of new firms only contains the first year a firm is observed in the data, which means we keep only one observation per firm. We then replicate the approach in section 5.2 by adding French data and focusing on firms in neighboring Spanish-French regions. As before, Spanish firms are treated and French are controls. We then estimate the following equation:

$$BG_i = \alpha + \beta_1 Treat + \beta_2 Post + \beta_3 Treat \times Post + \lambda Controls_i + \delta_j + \zeta_t + \epsilon_{i,t} \tag{2.7}$$

where BG_i is a dummy that equals 1 if the new firm i is affiliated to a business group, and 0 otherwise. $Treat$ is a dummy variable equal to 1 if the firm is located in Spain, and 0 for French firms. $Post$ is a dummy variable equal to 1 if the observation year is after 2012, and 0 otherwise. $Controls_i$ is a vector of control variables used in Equation 2.4. All specifications include industry (δ_j) and year fixed effects (ζ_t).

Table 2.8, panel A presents the estimation results. In column (2), we find that the coefficient is -0.051 and significant at the 1% level, suggesting that the 2012 labor reform decreases the likelihood of new firms being affiliated with a business group by approximately 5.1 percentage points. Columns (3)-(4) further show that our results are robust even when we only consider new firms that are one year old. Overall, these results are consistent with the prediction that EPL liberalization, by reducing labor market frictions, makes group affiliation less valuable and less common.

2.7.2 Changes to group affiliation

Next, we examine how EPL liberalization affects the dynamics of group affiliation. Specifically, we investigate whether stand-alone firms are less likely to change their affiliation and become part of a group after the reform. We estimate a firm panel specification and compare the probability of group affiliation between Spanish versus French firms, before and after the regulation takes effect:

$$BG_{i,t} = \alpha + \beta Treat \times Post + \lambda Controls_{i,t} + \delta_i + \zeta_t + \epsilon_{i,t} \tag{2.8}$$

where $BG_{i,t}$ is a dummy that takes a value of one for group affiliated firms, and zero for stand-alone. As before, our focus lies on the interaction *Treat* $\times$ *Post*. All specifications include firm (δ_i) and year fixed effects (ζ_t). Panel B of Table 2.8 reports the results from our analysis. We find that the coefficient estimates on the interaction term are negative and significant in all columns. In column (1), the coefficient is -0.036, indicating a 3.6% lower probability of Spanish stand-alone firms becoming affiliated with a business group when compared to neighboring French firms after the passing of the reform. For robustness, we match each Spanish firm with a French control based on size and employment the year before the reform, ensuring that they also belong to the same industry. As reported in columns (3)-(4), we still find a negative and significant coefficient on the interaction term of $Treat \times Post$. Taken together, the likelihood of transitioning from stand-alone to business group decreases following the reform, confirming that labor market frictions are a key factor behind group formation (see Khanna and Yafeh, 2007).

2.7.3 Group expansion

Lastly, we turn to group-level analysis. We are interested in how changes in EPL relate to the expansion of groups. As labor rigidities decrease following the reform, business groups may become less attractive, slowing their expansion. To test this prediction, we compare business group expansion for Spanish and French groups, regardless of their location within the country. We then conduct the following regression at the group level, controlling for group and year fixed effects:

$$Group\ Size_{g,t} = \alpha + \beta Treat \times Post + \lambda Controls_{g,t} + \delta_j + \zeta_t + \epsilon_{j,t} \tag{2.9}$$

where $GroupSize_{g,t}$ is the number of firms within group g. $Treat$ is a dummy variable equal to 1 if the group is located in Spain, and 0 for control groups. $Post$ is a dummy variable equal to 1 if the observation year is after 2012, and 0 otherwise. $Controls_{g,t}$ is a vector of lagged group-level control variables. All specifications include group (δ_g) and year fixed effects (ζ_t).

Panel C reports the results. In line with previous findings, the coefficient estimates for the interaction $Treat \times Post$ are significant and negative, indicating that the labor reform negatively affects the expansion of business groups. These results consistently suggest a lower prevalence of groups following the EPL liberalization. Overall, our findings in this section provide new insights into the role that employment protection regulation and internal labor markets play in the emergence and attractiveness of business groups.

2.8 Robustness and Additional Analyses

2.8.1 Alternative explanation: Internal Capital Markets

An alternative explanation for our findings could rely on the role of internal capital markets (ICMs) within business groups. The existing literature provides empirical evidence supporting the benefits of reallocating capital among group firms (Buchuk et al., 2014; Almeida et al., 2015). This reallocation might enable capital to flow more freely, thereby mitigating the negative effects of external labor frictions. Therefore, firms affiliated with such groups are less likely to be impacted by changes in employment protection. Following this line of reasoning, the observed diminished sensitivity of labor to the EPL reform could simply be the byproduct of capital reallocation within business groups.

To understand whether the ICM channel plays a role in explaining our main findings in Table 2.2, we first exploit the heterogeneity of internal capital markets within two-firm business groups, with a focus on group cash holdings and tangibility (we exclude assets as these are correlated with employment). Greater internal resource reallocation is plausible when a firm is affiliated with a group possessing higher cash holdings or higher tangibility, which facilitates access to credit (Larrain et al., 2019). If the employment dynamics we observe are indeed a byproduct of ICMs, our results should be more pronounced for firms affiliated with a financially stronger group. In Table 2.9 panel A, *BG (High group cash/tangibility)* is equal to 1 if the firm is affiliated with a two-firm business group with tangibility/cash above the median, and 0 otherwise. *BG (Low group cash/tangibility)* is equal to 1 if the firm is affiliated with a two-firm business group with cash/tangibility below the median, and 0 otherwise. We interact these variables with Post and find that the coefficients of both interactions are negative and significant: both financially stronger and weaker groups display a muted response to the reform, when compared to stand-alone firms. For robustness, we further categorize firms within two-firm groups based on their partner's cash/tangibility. However, as shown in columns (3) - (4), we still do not find strong differences.

Our second approach is to divide our sample according to different levels of external financial (equity) dependence, as ICMs are more likely to play a role in group firm dynamics when external financial dependence is high.[11] If our main findings in Table 2.2 were explained by internal capital markets, then we would expect stronger results for firms in industries with high external financial (equity) dependence. However, as shown in Panel B of Table 2.9, the $BG \times Post$ coefficient remains similar across firms from industries with high financial (equity) dependence and low financial (equity) dependence.

[11] Boutin et al. (2013) show that ICMs are particularly beneficial in environments where raising external capital is difficult.

Finally, in panel C, we focus on all group-affiliated firms (as opposed to *two-firm* business groups only) to investigate whether the ICM channel provides an alternative explanation. We first exploit group-level measures of cash holdings and tangibility measured in the year before the reform. For the next two specifications, we also calculate the average cash holdings and tangibility of the firm's partners within the same group (i.e., the rest of the group's cash/tangibility). If ICM was the underlying mechanism, then group firms with resource-rich firms/partners should be less affected by the labor reform, experiencing lower employment growth thereafter. However, we do not find significant results, except in column (1), which shows significance only at the 10% level.[12] Overall, the results of Table 2.9 provide limited evidence in support of the ICM channel.

2.8.2 Further effects of the reform

Noting that groups do not seem to benefit from the reform as much as stand-alone firms, in this section, we investigate the differential impact of the reform on other firm characteristics. Stringent employment protection regulations often impose rigidities that can make labor adjustments more difficult and costlier for firms (i.e., firing costs, keeping redundant workers, etc.). In such an environment, internal labor markets may offer a competitive advantage, which is likely to diminish after the reform. Therefore, we expect that group affiliates underperform stand-alone firms afterwards.

To test this, we focus on two measures of operating performance: return on assets (ROA) and sales growth. *ROA* is the ratio of net income to book value of assets. *Sales growth* is the growth in sales from one fiscal year to the next. Table 2.10 presents the regression results when repeating our specification from Table 2 with firm controls as well as firm and year fixed effects. In panel A, we compare firms affiliated with two-firm groups versus their stand-alone peers. As shown in columns (1) - (4), the coefficient of *Post* is positive, in line with the overall positive effects of the reform (OECD, 2013). However, we do not find a very strong drop in performance for group affiliated firms as compared to stand-alones.

[12] In Table A.2.4, we re-estimate Equation 2.5 and further control for the interactions of *Post* and various ICM measures here. We can see that our results remain qualitatively unchanged.

In Table 2.10 panel B, we repeat the previous analysis in our sample of group affiliated firms and compare the evolution of performances across those with varying levels of ILM access (similar to our analysis in Table 2.5). Unlike our results in panel A, we now find a significant drop in performance for groups with greater ILM access.[13]

2.9 Conclusion

In this paper, we investigate the interaction between business groups and employment protection. We predict that firms affiliated with a business group should be partly insulated from the impact of employment protection legislation, as they can bypass labor market frictions through their internal labor markets. To investigate this question, we exploit a major Spanish labor reform in 2012, which substantially reduced dismissal costs.

Our findings confirm that firms affiliated with a business group are less affected by employment protection legislation. In particular, when compared with similar stand-alone peers, BG firms experience a smaller increase in employment growth following the reform. In line with the ILM story, this differential response is more pronounced in firms affiliated with groups that have greater access to ILMs. Furthermore, we provide consistent evidence when we focus exclusively on group-affiliated firms and examine how our results vary with different group characteristics. Finally, we show that following this large employment liberalization, group affiliation becomes less prevalent. This finding provides the first causal evidence for the longstanding claim that external labor market frictions are a key determinant of organizational structure.

[13] In Table 2.11, we do not observe any differential effects of the labor reform on other firm outcomes, such as leverage, investment, and cash holdings, between BG firms and stand-alone firms. All in all, these results show that the effects of the reform are focused, as intended, on easing labor market frictions which means that groups lose some of their advantages. They also provide indirect evidence suggesting that our main findings are not driven by the internal capital market channel.

Figure 2.1: Employment growth for BG and SA firms

Notes: The figure shows the yearly rescaled average values of firms' employment growth in the 7-year window around the labor reform in 2012 for group firms and stand-alone firms. The rescaling is done by deducting the three-year average before reform (2009-2011) from each annual average figure of employment growth.

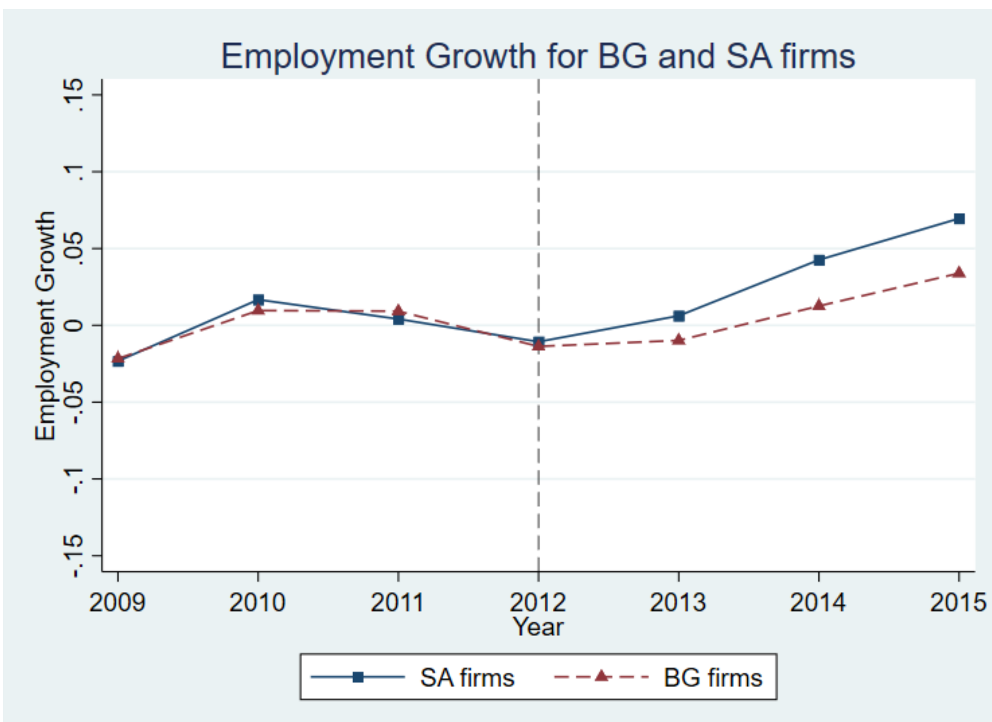
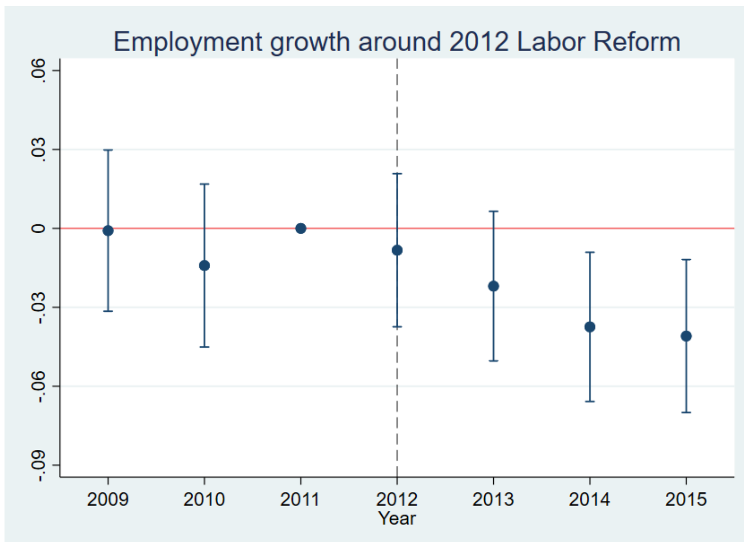
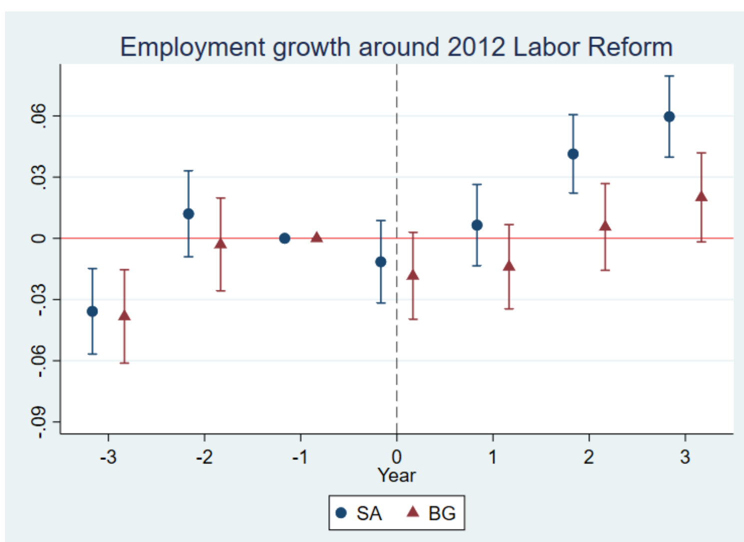


Figure 2.2: Dynamics of employment growth

Notes: Figure 3(a) displays coefficient estimates for equation (1). We include firm and year fixed effects in our specifications. Figure 3(b) displays coefficient estimates of employment growth for BG and SA firms. We include firm fixed effects to account for time-invariant firm characteristics. The confidence intervals in both figures are at 95% level.



(a)



(b)

Figure 2.3: Employment growth for firms in Spain and France

Notes: The figure shows the yearly rescaled average values of firms' employment growth in the 7-year window around the labor reform in 2012 for firms in Spain and France. The rescaling is done by deducting the three-year average before reform (2009-2011) from each annual average figure of employment growth.

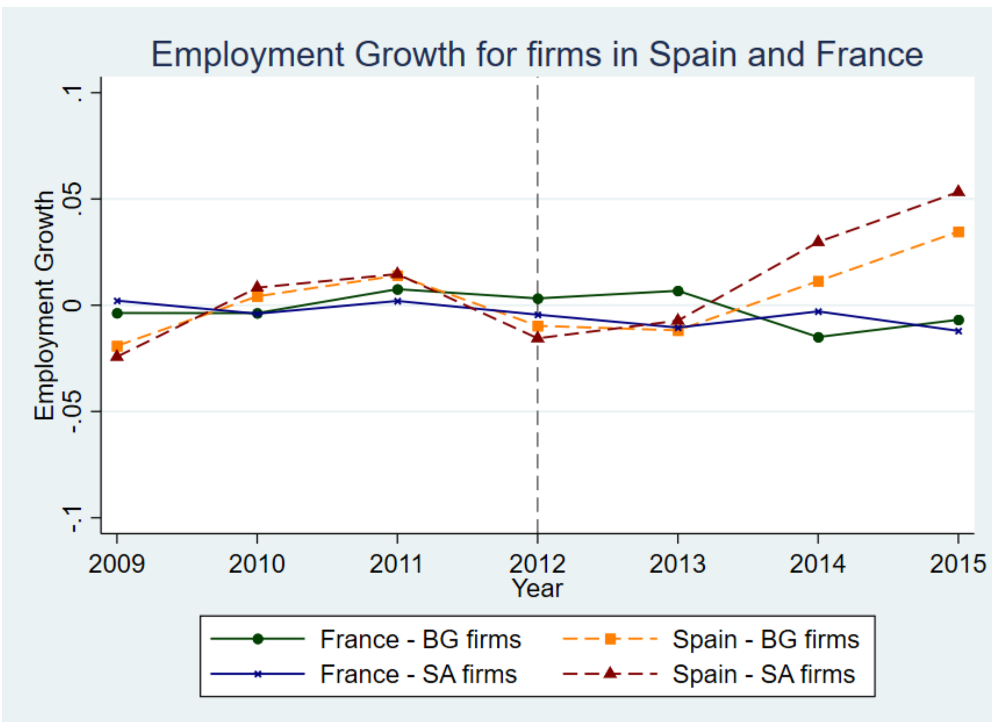


Table 2.1: Summary statistics

This table presents summary statistics for our sample. Panel A provides summary statistics. *Employment growth* is the difference between firms' logarithm of number of employees and its lag. *BG* is a dummy that equals 1 if the firm is affiliated with a two-firm business group, and 0 otherwise. *Employment* is the logarithm of one plus the number of employees. *Total Assets* is measured in millions of euros. *Size* is the logarithm of book value of assets. *Tangibility* is fixed assets over assets. *Leverage* is book value of debt over assets. *Cash* is cash holdings over assets. *Cash flow* is cash flow over assets. *ROA* is return on assets. *Sales/assets* is sales over assets. *Sales growth* is the growth in sales from one fiscal year to the next. *Industry labor intensity* is the average ratio of employment relative to sales, computed at the two-digit SIC code using U.S. data. *Industry labor dependence* is the average standard deviation of employment relative to fixed assets over time, computed at the two-digit SIC code using U.S. data. *Industry labor turnover* is the average labor-turnover rate, computed at the two-digit SIC code using U.S. data. Industry financial and equity dependence are Rajan and Zingales (1998) measures of external financial dependence, computed at the two-digit SIC code using U.S. data. Panel B summarizes the characteristics of BG and stand-alone firms. Panel C presents the characteristics of the two-firm business groups in our sample. *BG (same city)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where the member firms are located in the same city, and 0 otherwise. *BG (different city)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where the member firms are located in different cities, and 0 otherwise. *BG (same sic2)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where the member firms have the same two-digit SIC industry code, and 0 otherwise. *BG (different sic2)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where the member firms have different two-digit SIC industry codes, and 0 otherwise. *BG (ILM access)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where both of the following criteria are met: (i) firm members of a two-firm business group are located in the same city, and (ii) firm members of a two-firm business group have different two-digit SIC industry codes. *BG (non-ILM access)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group but either of these criteria is not met.

Panel A: Summary statistics

VarName	Obs	Mean	SD	Median
Employment growth	22,311	-0.03	0.296	0.00
Employment (log+1)	23,191	1.96	0.952	1.79
BG dummy	24,241	0.50	0.500	0.00
Total Assets (millions)	24,241	2.72	5.217	1.02
Size	24,241	13.81	1.424	13.83
Leverage	24,240	0.50	0.280	0.52
Tangibility	24,241	0.41	0.303	0.36
Cash holding	23,258	0.13	0.164	0.06
Cash flow	23,800	0.04	0.086	0.03
ROA	24,148	1.47	7.982	1.07
Sales/Assets	22,946	1.17	1.087	0.91

Sales growth	22,195	0.00	0.364	-0.02
Industry labor intensity	23,868	0.00	0.004	0.00
Industry labor volatility	23,868	0.01	0.007	0.01
Industry labor turnover	23,868	0.12	0.034	0.12
Industry external fin. dep.	23,868	-1.77	1.917	-1.12
Industry external equity dep.	23,868	0.25	0.701	0.00

Panel B: Summary statistics for BG and SA firms

VarName	BG		SA		
	N	Mean	N	Mean	Mean difference
Employment growth	11,043	-0.03	11268	-0.04	0.01
Employment (log+1)	11,504	1.96	11687	1.95	0.01
Size (log)	12,105	13.82	12136	13.80	0.02
Leverage	12,105	0.51	12135	0.49	0.02***
Tangibility	12,105	0.42	12136	0.40	0.02***
Cash holding	11,633	0.12	11625	0.13	-0.01***
Cash flow	11,872	0.04	11928	0.04	0.00
ROA	12,055	1.54	12093	1.40	0.14
Sales/Assets	11,441	1.13	11505	1.21	-0.08***
Sales growth	11,050	0.00	11145	-0.01	0.01***

Panel C: Group characteristics

VarName	Obs	Mean	SD	Median
BG (same city)	6,530	0.73	0.443	1.00
BG (different city)	6,530	0.27	0.443	0.00
BG (same sic2)	6,530	0.38	0.486	0.00
BG (different sic2)	6,530	0.62	0.486	1.00
BG (ILM access)	6,530	0.43	0.495	0.00
BG (Non-ILM access)	6,530	0.57	0.495	1.00

Table 2.2: Main results

This table reports the results of OLS regressions where the dependent variable is *Employment Growth*. *BG* is an indicator equal to 1 if the firm belongs to a two-firm business group, and 0 otherwise. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. Control variables are defined in Table 1. In column (7), we further pair group-affiliated firms with stand-alone firms based on an exact match in terms of employment count in 2011. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Employment Growth						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BG × Post	-0.028***	-0.028***	-0.027***		-0.026***	-0.026***	-0.018*
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Post	0.042***						
	(0.00)						
BG × $Reform_{-3}$				-0.001			
				(0.02)			
BG × $Reform_{-2}$				-0.014			
				(0.02)			
BG × $Reform_{0}$				-0.008			
				(0.01)			
BG × $Reform_{1}$				-0.022			
				(0.01)			
BG × $Reform_{2}$				-0.037***			
				(0.01)			
BG × $Reform_{3}$				-0.041***			
				(0.01)			
Control	No	No	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	No	No	No
Industry × Year	No	No	No	No	Yes	Yes	Yes
Region × Year	No	No	No	No	No	Yes	Yes
N	22,291	22,291	21,332	21,332	21,332	21,319	14,791
r2	0.140	0.143	0.153	0.153	0.153	0.173	0.177

Table 2.3: BG firms with greater ILM access

This table reports the results of OLS regressions where the dependent variable is *Employment Growth*. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. *BG (same city)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where the member firms are located in the same city, and 0 otherwise. *BG (different city)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where the member firms are located in different cities, and 0 otherwise. *BG (same sic2)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where the member firms have the same two-digit SIC industry code, and 0 otherwise. *BG (different sic2)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where the member firms have different two-digit SIC industry codes, and 0 otherwise. *BG (ILM access)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group where both of the following criteria are met: (i) firm members of a two-firm business group are located in the same city, and (ii) firm members of a two-firm business group have different two-digit SIC industry codes. *BG (non-ILM access)* is a dummy that equals 1 if the firm is affiliated with a two-firm business group but either of these criteria is not met. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Employment Growth		
	(1)	(2)	(3)
BG(same city) × Post	-0.029***		
	(0.01)		
BG(different city) × Post	-0.009		
	(0.02)		
BG(same sic2) × Post		-0.010	
		(0.01)	
BG(different sic2) × Post		-0.033***	
		(0.01)	
BG(ILM access) × Post			-0.040***
			(0.01)
BG(non-ILM access) × Post			-0.012
			(0.01)
Control	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	11,570	11,570	11,570
r2	0.151	0.151	0.151

Table 2.4: Cross-sectional analysis: labor intensive industries

This table reports the results of OLS regressions where the dependent variable is *Employment Growth*. *BG* is an indicator equal to 1 if the firm belongs to a two-firm business group, and 0 otherwise. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. We split the sample using above and below the median industry labor intensity, labor dependence, and labor turnover, computed using industrial U.S. data. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Employment Growth					
	Labour intensity		Labour dependence		Labour turnover	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
BG × Post	-0.016	-0.036***	-0.017	-0.032***	-0.015	-0.033***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Control	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	11,316	9,582	8,266	12,601	8,326	12,575
r2	0.155	0.152	0.140	0.160	0.152	0.156

Table 2.5: BG firms *vs* BG firms: Large and small business groups

This table reports the results of OLS regressions where the dependent variable is *Employment Growth*. The sample consists of all firms that are group-affiliated in 2011. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. *Group Size* is the number of firm units within the business group. *ILM access (employment/1000)* is the sum of employment of all other group units that are located in the same city but in a different two-digit industry. *ILM access (firm units)* is the sum of firm units that are located in the same city but in a different two-digit industry within the same business group. All these ILM measures are defined based on data from 2011. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Employment Growth			
	(1)	(2)	(3)	(4)
Group Size × Post	-0.001***			-0.001***
	(0.00)			(0.00)
ILM Access (employment/1000) × Post		-0.030**		-0.021
		(0.01)		(0.02)
ILM Access (firm units) × Post			-0.002**	0.001
			(0.00)	(0.00)
Control	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	94,882	93,082	94,882	93,082
r2	0.187	0.179	0.187	0.179

Table 2.6: Group-level analysis

This table reports the results of OLS regressions where the dependent variable is *Group Employment Growth*. The sample consists of all business groups defined in 2011. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. *Group Size* is the number of firm units within the business group (measured in 2011). Standard errors are robust and clustered at the firm level. In column (1), the sample includes only stand-alone firms, whereas column (2) includes only group-affiliated firms. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Group employment Growth		
	(1)	(2)	(3)
Group Size × Post	-0.013***	-0.013***	-0.016***
	(0.00)	(0.00)	(0.00)
Post	0.025***		
	(0.01)		
Group Assets			-0.308***
			(0.01)
Group Leverage			-0.234***
			(0.03)
Group Cash			-0.270***
			(0.05)
Group Cash flow			0.365***
			(0.10)
Group Tangible			0.216***
			(0.04)
Group ROA			0.073
			(0.10)
Control	No	No	Yes
Group FE	Yes	Yes	Yes
Year FE	No	Yes	Yes
N	49,768	49,768	49,768
r2	0.117	0.117	0.181

Table 2.7: Group-level analysis: other group characteristics

This table reports the results of OLS regressions where the dependent variable is *Group Employment Growth*. The sample consists of all business groups defined in 2011. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. *Group Size* is the number of firm units within the business group. *Group(No. of diff sic2)* is the number of firms with different two-digit SIC codes within a group. *Group(No. of same cities)* is the maximum number of firms in the same city within a group. All these ILM measures are defined based on data from 2011. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Group employment growth			
	(1)	(2)	(3)	(4)
Group Size × Post	-0.016***			-0.020***
	(0.00)			(0.01)
Group(No. of diff sic2) × Post		-0.020***		0.005
		(0.00)		(0.01)
Group(No. of same cities) × Post			-0.017***	0.002
			(0.00)	(0.01)
Control	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	49,768	49,768	43,453	43,453
r2	0.181	0.180	0.177	0.177

Table 2.8: Effects of EPL liberalization on group affiliation

This table reports the difference-in-differences tests that examine the effects of EPL liberalisation on corporate group affiliation. *Treat* is an indicator equal to 1 if the firm is located in Spain, and 0 otherwise. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. In Panel A, we study the effects of EPL on the creation of new firms (firms with age 1 - 3 years since incorporation), where the dependent variable is the dummy for group affiliation (*BG*). In Panel B, we consider only stand-alone firms, as defined in 2011, and study the effects of EPL on the likelihood of transitioning from stand-alone to group affiliation, where the dependent variable is the dummy for group affiliation (*BG*). In Panel C, we conduct a group-level analysis and study the effects of EPL on the number of firm units within a group, where the dependent variable is *Group Size*. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. Firm level: new firm creation

	BG			
	Age (1-3)		Age (1)	
	(1)	(2)	(3)	(4)
Treat × Post	-0.101***	-0.051***	-0.124***	-0.082**
	(0.02)	(0.02)	(0.04)	(0.04)
Treat	-0.114***	-0.059***	-0.160***	-0.094***
	(0.01)	(0.01)	(0.03)	(0.03)
Control	No	Yes	No	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	23,373	19,756	3,501	2,981
r2	0.132	0.190	0.164	0.245

Panel B. Firm level: BG dynamics

	BG			
	Full sample		Matched sample	
	(1)	(2)	(3)	(4)
Treat × Post	-0.036***	-0.030***	-0.041***	-0.034***
	(0.00)	(0.00)	(0.01)	(0.01)
Control	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	386,906	346,542	53,732	49,121
r2	0.663	0.663	0.508	0.529

Panel C. Group level: Group Size

	Group Size	
	(1)	(2)
Treat × Post	-0.102***	-0.066***
	(0.02)	(0.02)
Control	No	Yes
Group FE	Yes	Yes
Year FE	Yes	Yes
N	142,648	132,864
r2	0.810	0.832

Table 2.9: Alternative explanation: Internal Capital Market

This table reports the results of OLS regressions where the dependent variable is *Employment Growth*. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. Panel A and B consist of all firms that are from Equation 2.4. In panel A, *BG (High group cash)* is an indicator equal to 1 if the firm is affiliated with a two-firm business group with cash above the median, and 0 otherwise. *BG (Low group cash)* is an indicator equal to 1 if the firm is affiliated with a two-firm business group with cash below the median, and 0 otherwise. *BG (High group tangi)* is an indicator equal to 1 if the firm is affiliated with a two-firm business group with tangibility above the median, and 0 otherwise. *BG (Low group tangi)* is an indicator equal to 1 if the firm is affiliated with a two-firm business group with tangibility below the median, and 0 otherwise. *BG (High partner tangi)* is an indicator equal to 1 if the firm is affiliated with a two-firm business group and its partner with tangibility above the median, and 0 otherwise. *BG (Low partner tangi)* is an indicator equal to 1 if the firm is affiliated with a two-firm business group and its partner with tangibility below the median, and 0 otherwise. In panel B, we split the sample using above and below the median industry external dependence. *BG* is an indicator equal to 1 if the firm belongs to a two-firm business group, and 0 otherwise. In panel C, the sample consists of all firms that are group affiliated in 2011. *Other Cash* is the average cash of other firms within the same business group (measured in 2011). *Other Tangibility* is the average tangibility of other firms within the same business group (measured in 2011). *Group Cash* is the cash holdings of the group that the firm is affiliated with (measured in 2011). *Group Tangibility* is the tangibility of the group that the firm is affiliated with (measured in 2011). *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: BGs and SAs - Cash and Tangibility

	Employment Growth			
	(1)	(2)	(3)	(4)
BG(High group cash) × Post	-0.029**			
	(0.01)			
BG (Low group cash) × Post	-0.035***			
	(0.01)			
BG(High group tangibity) × Post		-0.024*		
		(0.01)		
BG(Low group tangibity) × Post		-0.039***		
		(0.01)		
BG(High partner cash) × Post			-0.029**	
			(0.01)	
BG(Low partner cash) × Post			-0.035***	
			(0.01)	
BG(High partner tangibity) × Post				-0.023*
				(0.01)
BG(Low partner tangibity) × Post				-0.040***
				(0.01)
Control	Yes	Yes	Yes	Yes

Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	10,876	10,876	10,876	10,876
r2	0.152	0.152	0.152	0.152

Panel B: BGs and SAs - External Dependence

	Employment Growth			
	External financial dependence		External equity dependence	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
BG × Post	-0.024***	-0.027**	-0.026**	-0.026**
	(0.01)	(0.01)	(0.01)	(0.01)
Control	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	13,063	7,821	9,526	11,342
r2	0.161	0.145	0.148	0.160

Panel C: BG firms vs BG firms

	Employment Growth			
	(1)	(2)	(3)	(4)
Group Cash × Post	-0.014*			
	(0.01)			
Group Tangibility × Post		0.002		
		(0.02)		
Other Cash × Post			-0.007	
			(0.01)	
Other Tangibility × Post				-0.009
				(0.02)
Control	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	94,882	94,882	89,847	91,166
r2	0.187	0.187	0.184	0.186

Table 2.10: Firm performance

This table presents the results of OLS regressions where dependent variables are other firm outcome variables. In column (1) - (4), we construct two measures of operating performance: *return on assets (ROA)* and *Sales growth*. In panel A, the sample consists of all firms that are from Table 2.2. *BG* is an indicator equal to 1 if the firm belongs to a two-firm business group, and 0 otherwise. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. In panel B, the sample consists of all firms that are group-affiliated in 2011. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%.

Panel A. BG firms and SA firms

	Operating Performance			
	ROA		Sales growth	
	(1)	(2)	(3)	(4)
BG × Post	-0.178	-0.171	-0.017*	-0.016*
	(0.20)	(0.20)	(0.01)	(0.01)
Post	0.824***		0.055***	
	(0.14)		(0.01)	
Control	Yes	Yes	Yes	Yes
Firm_FE	Yes	Yes	Yes	Yes
Year	No	Yes	No	Yes
N	22,586	22,586	21,177	21,177
r2	0.447	0.454	0.199	0.213

Panel B. BG firms and BG firms

	Operating Performance			
	ROA		Sales growth	
	(1)	(2)	(3)	(4)
ILM × Post	-0.821*	-0.749*	-0.092***	-0.092***
	(0.45)	(0.45)	(0.03)	(0.03)
Post	0.388***		0.017***	
	(0.07)		(0.00)	
Control	Yes	Yes	Yes	Yes
Firm_FE	Yes	Yes	Yes	Yes
Year	No	Yes	No	Yes
N	92,640	92,640	87,843	87,843
r2	0.501	0.507	0.204	0.208

Table 2.11: Other firm outcomes

This table presents the results of OLS regressions where dependent variables are other firm outcome variables. In columns (1)-(2), the dependent variable is *Leverage*. In columns (3)-(4), the dependent variable is *Asset growth*. In columns (5)-(6), the dependent variable is *Cash holdings*.In panel A, the sample consists of all firms that are from Table 2.2. *BG* is an indicator equal to 1 if the firm belongs to a two-firm business group, and 0 otherwise. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. In panel B, the sample consists of all firms that are group-affiliated in 2011. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%.

Panel A. BG firms and SA firms

	Other Firm Outcomes					
	Leverage		Asset growth		Cash	
	(1)	(2)	(3)	(4)	(5)	(6)
BG × Post	-0.002	0.002	0.005	0.005	-0.001	-0.001
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
Post	-0.041***		-0.008**		0.006***	
	(0.00)		(0.00)		(0.00)	
Control	Yes	Yes	Yes	Yes	Yes	Yes
Firm_FE	Yes	Yes	Yes	Yes	Yes	Yes
Year	No	Yes	No	Yes	Yes	Yes
N	22,611	22,611	22,611	22,611	22,636	22,636
r2	0.895	0.898	0.365	0.369	0.770	0.771

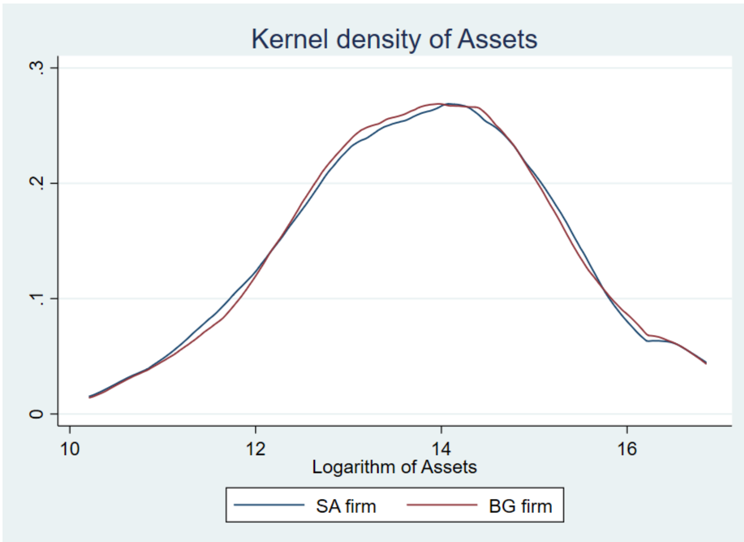
Panel B. BG firms and BG firms

	Other Firm Outcomes					
	Leverage		Asset growth		Cash	
	(1)	(2)	(3)	(4)	(5)	(6)
ILM × Post	0.011	0.011	-0.014	-0.013	0.001	0.002
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Post	-0.021***		-0.022***		0.006***	
	(0.00)		(0.00)		(0.00)	
Control	Yes	Yes	Yes	Yes	Yes	Yes
Firm_FE	Yes	Yes	Yes	Yes	Yes	Yes
Year	No	Yes	No	Yes	No	Yes
N	92,794	92,794	92,794	92,794	92,790	92,790
r2	0.867	0.867	0.340	0.342	0.744	0.745

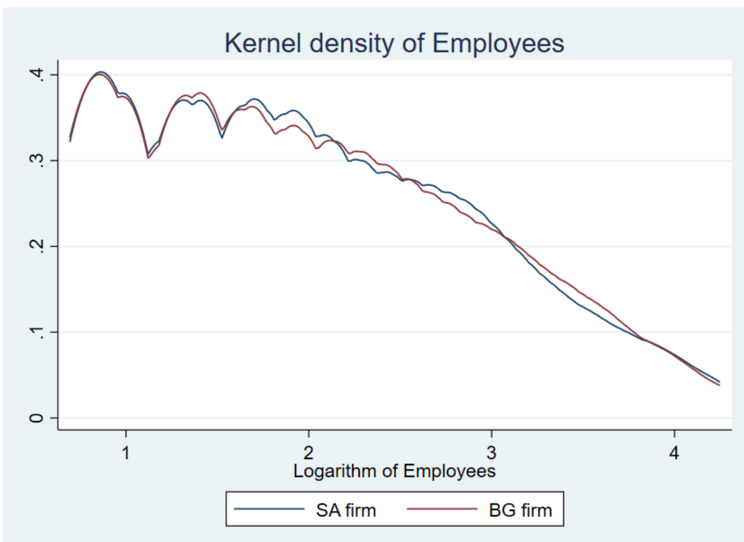
Appendix

Figure A.2.1: Size and employment densities of BG and SA firms

Notes: This figure shows the kernel distribution of size and employment in 2011 for two sets of firms: firms that belong to a two-firm business group and stand-alone firms.



(a)



(b)

Table A.2.1: Summary statistics(BG firms only)

This table presents summary statistics for the sample that only consists of BG firms. *Employment growth* is the difference between firms' logarithm of number of employees and its lag. *BG* is a dummy that equals 1 if the firm is affiliated with a two-firm business group, and 0 otherwise. *Employment* is the logarithm of one plus the number of employees. *Total Assets* is measured in millions of euros. *Size* is the logarithm of book value of assets. *Tangibility* is fixed assets over assets. *Leverage* is book value of debt over assets. *Cash* is cash holdings over assets. *Cash flow* is cash flow over assets. *ROA* is return on assets. *Sales/assets* is sales over assets. *Sales growth* is the growth in sales from one fiscal year to the next. *Group Size* is the number of firm units within the business group. *ILM access (employment/1000)* is the sum of employment of all other group units that are located in the same city but in a different two-digit industry. *ILM access (firm units)* is the sum of firm units that are located in the same city but in a different two-digit industry within the same business group.

VarName	Obs	Mean	SD	Min	Median	Max
Employment growth	99,086	-0.03	0.300	-1.16	0.00	1.10
Employment (log+1)	108,363	2.33	1.137	0.69	2.30	4.60
Total Assets (millions)	135,207	11.19	30.800	0.02	2.39	235.28
Size	135,207	14.65	1.822	10.00	14.69	19.28
Leverage	135,193	0.57	0.451	0.00	0.53	2.92
Tangibility	135,207	0.48	0.339	0.00	0.46	1.00
Cash holding	129,557	0.09	0.149	0.00	0.03	0.81
Cash flow	131,827	0.03	0.135	-0.66	0.03	0.46
ROA	131,881	0.04	0.140	-0.62	0.04	0.51
Sales/Assets	121,526	1.03	1.218	0.00	0.65	6.63
Sales growth	111,484	0.12	0.961	-0.93	-0.01	7.41
Group size	135,207	4.99	6.316	2.00	3.00	42.00
ILM (employment/1000)	109,531	0.05	0.188	0.00	0.00	1.48
ILM (firm units)	135,207	1.32	2.317	0.00	1.00	15.00

Table A.2.2: BG firms *vs* BG firms (two-firm BGs)

This table reports the results of OLS regressions where the dependent variable is *Employment Growth*. The sample consists of firms affiliated with the business group with only two firms. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. *Same City* is an indicator equal to 1 if all firm members of a two-firm business group are located in the same city, and 0 otherwise. *Different Sic2* is an indicator equal to 1 if firm members of a two-firm business group have different two-digit SIC industry codes, and 0 otherwise. *ILM access* is an indicator equal to 1 when both of the following criteria are met: (i) firm members of a two-firm business group are located in the same city, and (ii) firm members of a two-firm business group have different two-digit SIC industry codes. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Employment Growth		
	(1)	(2)	(3)
Same City × Post	-0.011		
	(0.02)		
Different Sic2 × Post		-0.026*	
		(0.01)	
ILM Access × Post			-0.023*
			(0.01)
Control	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	6,654	6,654	6,654
r2	0.160	0.160	0.160

Table A.2.3: Effects of EPL liberalization on employment growth

This table reports the results of OLS regressions where the dependent variable is *Employment Growth*. *Treat* is an indicator equal to 1 if the firm is located in Spain, and 0 otherwise. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. In column (1), the sample consists of only stand-alone firms. In column (2), the sample consists of only business group firms. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	Employment growth	
	SA firms	BG firms
	(1)	(2)
Treat × Post	0.035***	0.013***
	(0.00)	(0.00)
Control	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
N	231,138	65,858
r2	0.176	0.210

Table A.2.4: Alternative explanation: Internal Capital Market - BG firms *vs.* BG firms

This table reports the results of OLS regressions where the dependent variable is *Employment Growth*. The sample consists of all firms that are group-affiliated in 2011. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. *Group Size* is the number of firm units within the business group. *ILM access (employment/1000)* is the sum of employment of all other group units that are located in the same city but in a different two-digit industry. *ILM access (firm units)* is the sum of firm units that are located in the same city but in a different two-digit industry within the same business group. *Other Cash* is the average cash of other firms within the same business group. *Other Tangibility* is the average tangibility of other firms within the same business group. *Group Cash* is the cash holdings of the group that the firm is affiliated with. *Group Tangibility* is the tangibility of the group that the firm is affiliated with. All these measures of group characteristics are defined based on data from 2011. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Employment Growth											
Group Size × Post	-0.001***	-0.001***	-0.001***	-0.001***								
	(0.00)	(0.00)	(0.00)	(0.00)								
ILM Access (employment/1000) × Post					-0.029**	-0.032***	-0.028**	-0.030**				
					(0.01)	(0.01)	(0.01)	(0.01)				
ILM Access (firm units) × Post									-0.002**	-0.002**	-0.002*	-0.002**
									(0.00)	(0.00)	(0.00)	(0.00)
Other Tangibility × Post	-0.004				-0.006				-0.005			
	(0.01)				(0.01)				(0.01)			
Other Cash × Post		-0.017				-0.014				-0.014		
		(0.02)				(0.02)				(0.02)		
Group Tangibility × Post							-0.015*				-0.014	
							(0.01)				(0.01)	
Group Cash × Post			-0.010	-0.010				-0.002				-0.005
			(0.02)	(0.02)				(0.02)				(0.02)
Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	91,001	89,684	91,001	91,001	89,316	88,048	89,316	89,316	91,001	89,684	91,001	91,001
r2	0.186	0.184	0.186	0.186	0.179	0.177	0.179	0.179	0.186	0.184	0.186	0.186

Table A.2.5: Alternative explanation: Internal Capital Market - group level analysis

This table reports the results of OLS regressions where the dependent variable is *Group Employment Growth.* The sample consists of all business groups defined in 2011. *Post* is an indicator equal to 1 if the observation year is after 2012, and 0 otherwise. *Group Size* is the number of firm units within the business group. *Group(No. of diff sic2)* is the number of different two-digit SIC code firms within a group. *Group(No. of same cities)* is the maximum number of firms in the same city within a group. *Group Cash* is the cash holdings of the group. *Group Tangibility* is the tangibility of the group. All these measures of group characteristics are defined based on data from 2011. Standard errors are robust and clustered at the firm level. *, ** and *** stand for statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Group level analysis - ICM

	Group Employment Growth	
	(1)	(2)
Group Cash × Post	0.144***	
	(0.05)	
Group Tangibility × Post		0.022
		(0.02)
Control	Yes	Yes
Group FE	Yes	Yes
Year FE	Yes	Yes
N	49,768	49,768
r2	0.180	0.180

Panel B: Group level analysis - further control for ICM

	Group Employment Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Group Size × Post	-0.016***	-0.016***				
	(0.00)	(0.00)				
Group(No. of diff sic2) × Post			-0.019***	-0.021***		
			(0.00)	(0.00)		
Group(No. of same cities) × Post					-0.017***	-0.018***
					(0.00)	(0.00)
Group Cash × Post	0.078		0.112**		0.084*	
	(0.05)		(0.05)		(0.05)	
Group Tangibility × Post		0.036*		0.036*		0.032
		(0.02)		(0.02)		(0.02)
Control	Yes	Yes	Yes	Yes	Yes	Yes
Group FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	43,453	49,768	49,768	49,768	43,453	43,453
r2	0.177	0.181	0.180	0.180	0.177	0.177

Chapter 3

Does Knowledge Protection Spur Common Ownership? Evidence from the Inevitable Disclosure Doctrine

3.1 Introduction

Common ownership has experienced a considerable increase over the last few decades. This secular increase has challenged the conventional view that firms act as independent decision-makers, and has spurred a welter of recent studies investigating its implications on product market and other corporate strategies (e.g., Azar et al., 2018, 2022; He and Huang, 2017).

Considering the profound implications of firms' ownership structure, it is important to understand the driving forces behind the rise of common ownership. Although empirical work provides evidence that investor indexing, the holdings of the Big Three and consolidation in the asset management industry are the three main driving forces (Backus et al., 2021; Amel-Zadeh et al., 2022), more work is called for regarding the endogenous motive of common ownership.[1] Therefore, a natural question is: do shareholders or managers actively seek to increase common ownership as a deliberate corporate strategy? In this paper, we aim to answer this question and provide evidence that the quest for information sharing acts as an important motive for common ownership.

To examine how firms' incentive of information-sharing triggers common ownership, our paper exploits the staggered adoption of the Inevitable Disclosure Doctrine (IDD) in a difference-in-difference setting. The IDD is a trade secret law passed by U.S. state courts, which prevents firms'

[1] Amel-Zadeh et al. (2022) emphasizes the importance of activist, non-financial blockholder, or insider when measuring universal and common ownership of firms.

workers with the knowledge of trade secrets from working for their rivals, regardless of the rivals' locations. The adoption of IDD therefore enhances knowledge protection and introduces obstacles to share information across firms and markets. Previous studies suggest that common owners can facilitate information flows by forming strategic alliances and sharing resources among common-held firms (e.g., He and Huang, 2017). We argue that this feature of common ownership can help firms bypass the obstacles introduced by IDD and share valuable knowledge capital resources. In other words, we conjecture that a state's adoption of IDD could lead to an increase in firms' common ownership.

Using a large panel of public firms listed on major U.S. exchanges from 1994 to 2015, we find a significant increase in firms' common ownership level following the adoption of IDD in their headquarter states. Specifically, our baseline results show that common ownership increases by 7.7% relative to the sample average once the IDD is enacted, which is both statistically significant and economically important. Overall, these results suggest that when IDD is adopted, firms increase their common ownership level, potentially due to their incentive to bypass the information obstacles induced by IDD and share knowledge capital.

While the adoption of IDD provides a clean setting to explore whether the incentive to share information across firms induces the rise of common ownership, there are still some residual issues that require careful consideration. First, to mitigate the sources of confounding variations, we exploit panel regression techniques and control for firm and industry-year fixed effects. Our results are also robust to including time-varying firm characteristics as well as state economic environment. The second concern is that the treated and control firms differ along dimensions that might affect firms' common ownership. We address this issue by performing a Mahalanobis matching using the firm size and its 3-digit SIC industry to ensure the comparability between the treated and controls - our results are both quantitatively and qualitatively similar to our baseline findings. Third, we take extra care when examining the dynamic effects of IDD to rule out the concerns that our results are driven by pre-treatment trends. Specifically, we exploit both the conventional dynamic regressions as well as the newly developed approaches (Sun and Abraham, 2021; Cengiz et al., 2019), and find no pre-trends in the evolution of firms' common ownership level.

We next conduct cross-sectional analyses to explore the information-sharing channel through which IDD laws increase common ownership. The recognition of IDD, by protecting firms' trade secrets, increases frictions on labor mobility and information flows across firms and markets. Considering the features of IDD, we argue that the increase in labor market frictions should be more pronounced in industries that depend more heavily on human capital and knowledge capital, as well as in industries with higher labor mobility. As a result, firms operating in these industries should be more inclined to increase their common ownership as a way to bypass frictions to value-enhancing information flows. We exploit industry-level variations and construct proxies for the importance of human (knowledge) capital and labor mobility in our cross-sectional analyses. As expected, we find that firms in industries with higher dependence on human (knowledge) capital and those with greater labor mobility experience a more drastic increase in common ownership following the IDD adoption.

Given the heterogeneity in institutional shareholders' incentive to monitor, we expect that the increase in common ownership would be mainly driven by shareholders that "care" the most. To test this, we harness the varied interests of different types of institutional investors and incorporate them into our analysis. Specifically, we argue that as long-term investors are more likely to benefit from their monitoring efforts, they should have a stronger incentive to manage their cross-holdings as a way to expedite value-enhancing information sharing. Consistent with this line of reasoning, we find that the IDD adoption mainly causes an increase in common ownership among long-term investors. These results indicate that to the extent of their capability of influencing portfolio firms, institutional shareholders adjust their cross-holdings as a means of compensating for the negative shock to information sharing imposed by IDD.

As common ownership can be used to mitigate the frictions to information flows caused by the IDD recognition, the next question to ask is to what extent common ownership overcomes such frictions on the circulation of human and knowledge capital. To investigate this, we explore the shareholder value implications of the increased common ownership in states that have adopted IDD. If common ownership effectively improves information sharing after the adoption of IDD, we should observe an enhancement in corporate outcomes, especially in innovation activities, for firms that have experienced an increase in cross-holdings. Using mergers between financial institutions as an exogenous shock to common ownership, we find that an increase in common ownership improves corporate innovation outcomes and operating performance when IDD is enacted. This confirms the information-sharing-facilitating role of common ownership, and more importantly, shows that the IDD-induced common ownership improves efficiency.

Our paper contributes to several strands of literature. First, we provide more insight into the sprouting common ownership literature. Despite the heated debate over the impact of common ownership, academics have not yet reached a consensus. On the one hand, a growing body of literature testifies how common ownership may reduce the product market competition (Azar et al., 2018, 2022) and emphasizes the necessity of carrying out effective antitrust enforcement to undo the harm (Elhauge, 2016; Posner et al., 2017). On the other hand, some recent studies cast doubt on the validity of common ownership's influence on firm behaviors and further on competition (Lewellen and Lowry, 2021; Gilje et al., 2020; Koch et al., 2020). More notably, contrary to notoriously hindering competition, common ownership affects corporate strategies also through the efficiency channel, which would in turn increase innovation and improve welfare (He and Huang, 2017; López and Vives, 2019; Anton et al., 2021; Kini et al., 2021).

It is important to understand the causes of the rise of common ownership. Our paper contributes to the literature by providing evidence on how common ownership helps overcome frictions in the circulation of knowledge capital across firms and markets. Previous studies focus on how an exogenous rise in firms' common ownership reshapes corporate activities. We take a different angle by examining how the information-sharing and collaboration-facilitating role of common ownership can be utilized by common owners to facilitate innovation among firms in their portfolios. Our findings bring more insight into the understanding of common ownership, not only about its impacts, but also about its potential causes.

Our paper also connects to the prior work on trade secret protection laws and regulations. A number of studies have investigated the influence of the Inevitable Disclosure Doctrine (IDD) on various corporate outcomes. Evidence has been documented that the adoption of IDD increases firms' leverage (Klasa et al., 2018), raises the probability of being targeted in M&A deals (Chen et al., 2020a), strengthens firms' anti-takeover provisions (Dey and White, 2021), increases tax avoidance activities (Ding et al., 2021), and adversely affects firms' innovation and upward earnings management (Contigiani et al., 2018; Gao et al., 2018). Studies also find that the rejection of IDD has a profound influence on corporate strategies such as corporate social responsibility (Flammer and Kacperczyk, 2019) and executive compensation (Na, 2020). We add to this literature by turning to the effect of trade secret laws on firms' ownership structure. By preventing employees with the knowledge of firms' trade secrets from working for rival firms, the IDD adoption reduces labor mobility (Png and Samila, 2013) and thus raises labor market frictions. Our evidence suggests that common ownership can be used as a means to bypass such frictions and help firms access valuable human and knowledge capital.

Our paper is closely related to Chen et al. (2020a), which demonstrates that firms may engage in strategic M&A to acquire targets from the states that recognize IDD in an effort to gain valuable human capital resources. We complement their paper by exploring other approaches firms can employ with regard to obtaining human and knowledge capital resources. Though launching an M&A deal helps the acquirer to almost fully intake the target's human capital, the procedure can be lengthy. The initiation of the deal may be obstructed by the targets' anti-takeover provisions as well as anti-competitive concerns from competition authorities. We argue that common owners also play an important role in bypassing labor market frictions as they can help firms share resources (e.g., human capital related resources) with each other without doing an acquisition. Therefore, our paper bridges the labor and ownership structure literature by documenting a positive link between the recognition of IDD and firm-level common ownership.

3.2 The Inevitable Disclosure Doctrine and Hypothesis Development

3.2.1 Background on Inevitable Disclosure Doctrine

A trade secret, defined by the U.S. Patent and Trademark Office, (1) is information that has either actual or potential independent economic value by virtue of not being generally known; (2) has value to others who cannot legitimately obtain the information, and; (3) is subject to reasonable efforts to maintain its secrecy. Although national-level trade secret acts have been approved and recommended (e.g., the Uniform Trade Secrets Act of 1979; the Economic Espionage Act of 1996; and the Defend Trade Secrets Act of 2016), U.S. trade secret protection is mainly governed by the state jurisdiction. Under principles of common law, the owner of a trade secret would be able to take legal actions when the trade secret is misappropriated.[2]

The Inevitable Disclosure Doctrine is a common law principle that enhances the protection of trade secrets. It allows a court to impose a preliminary injunction prohibiting a departing employee from taking a similar position in rival firms merely based on threatened future misappropriation. More specifically, the injunction does not need to be backed with evidence of employee misconduct or the disclosure of the underlying trade secrets. Under the IDD, former employers can rest with an immediate relief provided that (1) the departing employee had access to its trade secrets, (2)

[2] Misappropriation of trade secrets occurs when the trade secret is acquired by improper means (e.g., theft or breach of a duty to maintain secrecy), or by disclosure without consent by a person who acquired the trade secret under situations, giving rise to a duty to maintain its secrecy or limit its use.

the employee's duties in the new firm would inevitably make her use or disclose the trade secrets, and (3) the disclosure of the trade secrets would produce irreparable economic harm to the firm.

It should be noted that many employment contracts contain a non-disclosure agreement (NDA) and a covenant not to compete (CNC), which are designed to protect the firm's trade secrets when its employees wish to switch jobs or start competing firms. Literature has documented a thorough comparison between IDD and the non-disclosure agreement (NDA) as well as the covenant not to compete (CNC) (e.g., Klasa et al., 2018; Chen et al., 2020a). In particular, IDD provides stronger protection in three aspects. First, IDD removes the geographic restriction under NDA and CNC and works in a wider scope (e.g., inter-state job switching).[3] Second, the implementation of IDD fills the evidentiary void (courts can impose the injunction based on a future misconduct) and thus increases the enforceability of NDA and CNC.[4] Last but not least, in states that have adopted the strongest form of IDD, courts can grant an injunction without the NDA/CNC.

3.2.2 The IDD as an Exogenous Shock

Our IDD indicators are drawn upon recent literature studying the impact of the Doctrine (Png and Samila, 2013; Castellaneta et al., 2016; Klasa et al., 2018). Existing papers may identify the timing of the adoption/rejection of IDD by a state differently. We therefore collect all the legal cases used as the identification of IDD adoption/rejection in the literature and carefully read through the case scripts and make our judgment based on (1) the year of the earliest case in a state that used IDD, and (2) if there are multiple cases with different extent of adoption/rejection regarding one state, we choose the year of the case that indicates a more thorough adoption/rejection with wider and stronger influence.

For instance, in Png and Samila (2013) and Castellaneta et al. (2016), the case that identifies the adoption of IDD in Illinois is the famous PepsiCo case in 1995, whilst in Klasa et al. (2018), the identifying case is Teradyne Inc. v. Clear Communications Corp., 707 F. Supp. 353 (N.D. 111. 1989). The PepsiCo case is cited by many states that adopted IDD in a later stage and is viewed

[3]Note that if an employee is changing job across states, the lawsuit on trade secret protection should be under the jurisdiction where the former employer locates (Garmaise, 2011).

[4]Some states apply the Inevitable Disclosure Doctrine only when the departing employee is bound by a covenant not to compete (CNC), such as Connecticut (Branson Ultrasonics Corp. v. Stratman, 921 F. Supp. 909 (D. Conn. 1996)). As noted by Klasa et al. (2018), "...the IDD is also a powerful tool to establish a key element in any legal action to enforce CNCs, i.e., a significant likelihood of irreparable harm to the firm if the employee is allowed to work for the rival..." Following the literature, we include these states as the IDD-adopted states in our sample.

as a seminal case in the law literature (Treadway, 2002; Wiesner, 2012). As argued by Png and Samila (2013), "It was decided by a federal Court of Appeals interpreting a UTSA provision, set a standard for injunctions on the basis of inevitable disclosure, and broadened the definition of a trade secret." We therefore classify the PepsiCo, Inc. v. Redmond, 54 F.3d 1262, 1272 (7th Cir. 1995) case as the official adoption of IDD in Illinois.[5] Table A.3.1 lists the year and specific cases in which a state adopts/rejects the Inevitable Disclosure Doctrine.

The exogeneity of IDD recognition is crucial to our empirical difference-in-difference setting and can be supported by the following reasons. First, the intention of state courts when adopting IDD rests mainly on the protection of trade secrets. The major externality that courts are concerned about is labor mobility, which is the predominant reason why some state courts rejected the Doctrine.[6] We henceforth argue that the change in firms' common ownership is an unintended consequence by the policy change. Second, finance researchers are often plagued by the likelihood that the passage of laws they exploit as a natural shock is driven by lobbying. However, the adoption/rejection of IDD depends on the judicial decisions made by state judges that are considered to be independent of the government; and their decisions are built on the merits of cases, unlikely to be swayed by political pressure or by lobbying (Klasa et al., 2018).

Apart from the verbal reasoning, we also provide statistical evidence on the exogeneity of IDD adoption. To address the concern that unobservable factors might affect the recognition of IDD, we start by examining whether the IDD adoption in a state is determined by state-level characteristics and, specifically, whether it is driven by the common ownership averaged at the state level. This test utilizes panel data at the state and year levels, and the results are reported in Table 3.1. We start with the most parsimonious test by including only the average level of common ownership in a state as the explanatory variable in column (1). The coefficient of common ownership is positive but insignificant, which indicates that state-level common ownership does not predict the adoption of IDD. In column (2), we further control for a number of state-level characteristics, such as state GDP growth and unemployment rate, etc. Reassuringly, none of the coefficients of these variables

[5] As a robustness check, we also run our regressions using the IDD indicators compiled by Klasa et al. (2018), both the coefficient sign and the significance level remain unchanged.

[6] California explicitly rejected the Inevitable Disclosure Doctrine because it restricts employee mobility. See Schlage Lock Co. v. Whyte, 101 Cal. App. 4th 1443 (2002), "...No published California decision has accepted or rejected the inevitable disclosure doctrine. [101 Cal. App. 4th 1447] In this opinion, we reject the inevitable disclosure doctrine. We hold this doctrine is contrary to California law and policy because it creates an after-the-fact covenant not to compete restricting employee mobility...".

are significant. We therefore argue that the adoption of IDD can be viewed as an exogenous shock, which contributes to the validity of our analysis.

3.2.3 Hypothesis Development

The recognition of IDD prevents knowledgeable workers from working for rival firms of their former employers. The keywords here, "knowledgeable workers", indicate that the emphasis of this doctrine is on skilled labor, that has the knowledge of a firm's trade secrets and is a critical production factor for firms. The value of a firm's human capital is not necessarily contained within a specific individual, and it also includes the subsequent resources that the individual can bring (e.g., knowledge capital, networking, relationships, etc.). The recognition of IDD therefore places frictions not only on employee turnover, but also on the circulation of information across firms and markets.

In our paper, we argue that common ownership can be used as an approach to help firms share and access valuable knowledge capital, thus bypassing information frictions caused by IDD. Empirical evidence suggests that common ownership by institutional shareholders plays a bridge-building role by facilitating information spillovers among portfolio firms, which leads to an improvement in product market performance and efficiency (He and Huang, 2017; López and Vives, 2019; Kini et al., 2021).[7] To be more specific, common owners can spur the information flows among cross-held firms either through communication, or by facilitating some form of product market agreements (e.g., strategic alliances, joint ventures, etc.). Most importantly, common owners allow firms to indirectly share their human capital without the need to engage in M&A deals. By doing this, the IDD-induced frictions to information flows can be effectively mitigated, which leads to our first testable prediction:

Prediction 1: *Firm-level common ownership increases following the recognition of IDD as a way to bypass the obstacles in information sharing.*

The positive effect of IDD on common ownership may vary by industry. If the increased common ownership after the IDD adoption is due to the quest for human capital and information sharing, we expect this treatment effect to be stronger for firms in industries with greater importance on human capital. In addition, given that the effect of IDD on firm-level common ownership stems

[7] There are two main reasons why common ownership improves information sharing: first, common owners can help align the incentives of the contracting parties, mitigating frictions associated with incomplete contracts; second, common owners can reduce information asymmetry and facilitate cooperation among same-industry firms. See He and Huang (2017) for a detailed discussion.

from difficulties in directly hiring talent due to increased labor market frictions, we expect the treatment effects to be more prominent for firms whose employees have higher *ex ante* mobility in the labor market. These arguments lead to our second prediction:

Prediction 2a: *After the recognition of IDD, the increase in common ownership should be more pronounced in firms that operate in industries that are human capital and knowledge capital intensive.*

Prediction 2b: *After the recognition of IDD, the increase in common ownership should be more pronounced in firms that operate in industries with greater labor market mobility .*

Although common ownership may alleviate the obstacles in inter-firm information flows in states with an active IDD, it is *a priori* unclear whether this IDD-induced increase in common ownership would exert anti-competitive effects on the market. If the increase in common ownership solely offsets the hurdles firms face in accessing valuable human capital, regulators do not need to be alerted as it may improve welfare and efficiency through promoting overall innovation activities of the industry. However, as the IDD adoption increases a firm's proprietary costs of disclosure, firms reduce their voluntary disclosure subsequently (Li et al., 2018; Kim et al., 2021), which makes tacit collusion through financial disclosures more difficult to sustain (Bourveau et al., 2020). Consequently, common owners may be used to exchange collusive information among cross-held firms. Although we do not have direct data on firms' collusive behaviors (e.g., the price information), we can test the implications of the IDD-induced increase in common ownership on competition via indirectly investigating its impact on corporate innovation activities and performance. This leads to our third prediction.

Prediction 3a: *If the IDD-induced increase in common ownership is efficiency-improving, firms that experience an increase in common ownership in the presence of IDD should enjoy an improvement in innovation activities as well as corporate performance.*

Prediction 3b: *If the IDD-induced increase in common ownership is anti-competitive and facilitates collusion, firms that experience an increase in common ownership in the presence of IDD should go through a reduction in innovation activities, but enjoy an improvement in corporate performance.*

3.3 Data and Sample

3.3.1 Measurement of Common Ownership

We rely on the Refinitiv 13F Institutional Holdings dataset to obtain data on institutional shareholders' holdings. To construct our main dependent variable, we exploit the firm-level measure of common ownership used in Lewellen and Lowry (2021),

$$CO_j = \sum_{k=1}^{K} \sum_{i=1}^{I} \omega_k \beta_{i,j} \beta_{i,k} \tag{3.1}$$

where j and k are industry peers that are cross-held by the same institutional investor i. For each firm pair j and k, $\beta_{i,j}$ is the ownership stake investor i has in firm j while $\beta_{i,k}$ is the ownership stake held by i in firm k. The product of the two ownership stakes measures how much interest i has in the joint value of the firm pair. The measure is higher when i's interest is more symmetrically spread between j and k. We calculate this product of ownership stakes for all the firm pairs that j can form with its industry peers k's. The firm-level common ownership for the focal firm j is measured as the weighted average of the product of ownership stakes. ω_k denotes the weight and is measured as the market capitalization weight of firm k in its industry. We exploit the value weighting in our variable construction in order to capture investors' incentives. For example, for each focal firm j, suppose investor i has same fractional equity stakes in two rival firms of j, denoted as k_1 and k_2. Assume that k_1 has a larger market cap than k_2, investor i's ownership holdings in k_1 can translate into a larger dollar stake, which gives investor i more incentive to consider k_1's interests. Finally, to construct the above measure, we only consider ownership stakes that are above the threshold of 0.5%. This is because stakes below this threshold are unlikely to have an effective influence on corporate decisions (Azar et al., 2018).

3.3.2 Sample Construction and Summary Statistics

Our sample consists of all the U.S. publicly listed firms traded on the New York Stock Exchange, NYSE Amex, and NASDAQ-NMS Stock Market from 1994 to 2015 covered by the database. Our sample begins in 1994 because this is the first year electric filings on the EDGAR system are available, which enables data on firms' historical headquarters to be accurately extracted.[8] The sample ends in 2015 to ensure we have two years data after the last state-level IDD policy change in Georgia. We exclude firms from heavily regulated industries to avoid any potential confounding impact (we exclude firms with SIC code in $6000 - 6999$, $4900 - 4999$, and ≥ 9900). This sample selection process yields $56,562$ firm-year observations in our baseline analysis.

All the financial data in our sample is from the Compustat and CRSP databases. We include the log value of total assets, leverage, ROA, Tobin's Q, CAPEX, cash flow, and cash holding as our firm-level control variables. Following the IDD literature, state-level controls are also included in our regressions. We obtain the GDP growth data from the Bureau of Economic Analysis, unemployment rate from the Bureau of Labor Statistics, business entry and exit rate from the Business Dynamics Statistics database of the U.S. Census Bureau. We also control for the CNC Index developed by Garmaise (2011), as well as the Business Combination Law indicator from Giroud and Mueller (2010). All the continuous variables are winsorized at the 1st and 99th percentiles to alleviate the influence of outliers.

Table 3.2 provides summary statistics of the variables used in our empirical analyses. The mean and median of the firm-level common ownership in our sample are 2.97 and 1.27, with a 3.86 standard deviation. Most firms are large, with a median asset size of roughly \$315.94 million. The average ratio of debt to assets is about 0.20, and the median of ROA and Tobin's Q are 0.11 and 1.60. As for state characteristics, the average state has an unemployment rate of 5.93%, and a GDP growth rate of 4.86%. On average, the state establishment entry and exit rates are 11.12% and 9.99%, respectively. More than half of the states have adopted business combination laws.

3.4 Empirical Methodology and Main Results

3.4.1 IDD Adoption Increases Common Ownership

Following the IDD literature, we use firms' states of headquarters to identify the treated sample in lieu of firms' actual employment locations due to the data limitation. As argued by Klasa et al. (2018), workers with access to trade secrets are more likely to be higher-level employees that work at firms' headquarters, which supports our variable selection. To examine whether state

[8] Firms' historical headquarter data is extracted from the Augmented 10-X Header Data provided by Bill McDonald. See https://sraf.nd.edu/data/augmented-10-x-header-data/.

courts' recognition of IDD affects common ownership, we follow the approach used in Bertrand and Mullainathan (2003) and employ a generalized difference-in-difference design. Our baseline regression model is defined as below:

$$\begin{aligned} CO_{i,t} = \alpha + \beta_1 * IDD_{i,t} + \beta_2 * Firm\,Characteristics_{i,t} + \beta_3 \\ * State\,Characteristics_{s,t} + \delta_i + \eta_k \times \zeta_t + \epsilon_{i,t} \end{aligned} \tag{3.2}$$

Where i indexes the firm, s indexes the state where a firm's headquarter is located, and t indexes the year. The dependent variable is CO, the common ownership measure for firm i in year t. The variable IDD is an indicator variable that equals one if the firm is headquartered in a state whose courts recognize the IDD, and zero otherwise. For states that reversed the IDD adoption decisions during our sample period, we change the IDD indicator back to zero after the rejection in that particular state. We include a comprehensive set of firm and state characteristics that can potentially affect a firm's common ownership. All of our models include firm fixed effects (δ_i) and industry-year fixed effects defined at the 3-digit SIC level ($\eta_k \times \zeta_t$). Given that our treatment is a state-level policy shock, we cluster the standard errors in all regressions at the state level.

Table 3.3 presents the baseline results of our difference-in-difference approach discussed in Section 3.4. Column (1) in Table 3.3 reports the results of our baseline regression model without any control variable. Our results suggest that the common ownership of treated firms increases in response to the adoption of IDD in states where they headquarter compared with that of the control firms. In terms of economic significance, the coefficient of the *IDD* indicator implies that firms increase common ownership by 20.7 percentage points following the adoption of the IDD. Given that the sample mean of common ownership in our sample is 2.97, this finding indicates a 7.0% increase in common ownership due to the IDD adoption. Columns (2) and (3) further control for various firm and state characteristics. The inclusion of these control variables strengthens the results from the previous panel - the DiD coefficients barely change and are still positive and highly significant at the 1% level. The magnitude of the coefficient in column (3) suggests that firms increase their common ownership by 23% following the adoption of IDD even after controlling for potential confounding firm-level and state-level factors, which is very similar to our results without controls and is economically meaningful.

The *IDD* indicator in our setting captures both the recognition and the rejection of IDD. To explore the impact of adoption and rejection separately, we follow Chen et al. (2020a) and replace the *IDD* indicator with the *IDD adoption* and *IDD rejection* indicators. The *IDD adoption* indicator is defined as one if the IDD is adopted, and zero otherwise; and the *IDD rejection* indicator equals one if the state reverses its previously adopted IDD, and zero otherwise. As reported in column (4), the coefficient of the *IDD adoption* indicator is positive and significant at the 5% level, while the coefficient on the *IDD rejection* indicator is negative, although insignificant. This evidence suggests that our main results are mainly driven by the adoption of the IDD.

3.4.2 Dynamic Effects

The causality of our difference-in-difference approach rests on the assumption that outcomes in treated and control firms would move in parallel in the absence of IDD adoption. While this cannot be tested directly, the leading terms in dynamic regressions will provide some useful indication of its plausibility. We follow Bertrand and Mullainathan (2003) and replace the IDD indicator with relevant time indicators. The coefficients on the variables IDD^{-2} and IDD^{-1} are particularly important because their significance would suggest whether there is any difference between the treated and control groups prior to the adoption of IDD. The results in Table 3.4 show that these two indicators are not significantly different from zero, which lends support to the plausibility of the parallel trend assumption.

Recent development in the econometrics field has shown that the conventional TWFE estimators could be biased in the presence of heterogeneous treatment effects (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021). This could cast doubt on our dynamic regression results. To confirm the validity of our baseline results and further provide evidence on the plausibility of the parallel trend assumption, we exploit two newly developed methods, the "interaction-weighted" method proposed by Sun and Abraham (2021) and the stacked-by-event approach proposed by Cengiz et al. (2019), to re-examine our different-in-difference estimates. The event study plots produced using these two methods are shown in the Appendix. As there are cases that the adoption of IDD takes place before 1994, or gets reversed during our sample period, we exclude firms operating in those states to guarantee a clean setting. Figure A.3.2 depicts the coefficients of the "interaction-weighted" estimates.

To implement the stacked-by-event approach introduced by Cengiz et al. (2019), we follow their methodology and trim our data to keep an 8-year event window (-4, +4). In particular, when stacking all the cohorts together, we follow the steps in Baker et al. (2022) using a full exclusion as well as a partial exclusion method. In the full exclusion sample, we stack cohort-specific data using observations from firms that are headquartered in states that recognized IDD in the cohort treatment year as the treated, and include all firms in states that never adopt IDD or adopt IDD more than four years after the cohort treatment year. In the partial exclusion sample, we use firms receiving IDD treatment for the first time in the cohort treatment year as the treated, and use all other firm-year observations without active IDD in place in the 8-year event window as the control. We then perform an event study on the stacked sample and plot the estimates in Figure A.3.3.

It is reassuring that when using these two methods, all the leading terms are not significantly different from zero and the firm-level common ownership increases after the IDD adoption. This mitigates the concern over the conventional DiD estimates and further verifies our baseline findings.

3.4.3 Matched Sample Analysis

Although we have accounted for the potential confounding impact of observable firm/state characteristics on common ownership, some *unobservable* factors may cause the treated and control firms to be intrinsically different and thus contaminate our results. To mitigate this concern, we perform a Mahalanobis matching using firm size and its 3-digit SIC industry to ensure the comparability between the treated and controls.

Specifically, we identify firms that are headquartered in states that have recognized IDD in that year as the treated and categorize firms in states that do not have an active IDD as the potential controls. As we match firms using their size one year before the treatment, we exclude all the states that have passed IDD before 1994. This further assuages the potential bias introduced by treatments before our sample period begins. To capture unobservable factors at the industry level that might affect common ownership, we assign a firm as a control only if it operates within the same 3-digit SIC industry as the treated firm. This procedure leads to a matched sample with 459 treated firms and 416 control firms. The firm size between our treated and controls is not significantly distinguishable with a p-value of 0.344.

Performing our analysis on the matched sample, we continue to find a significant increase in common ownership following the recognition of IDD, and the economic magnitude is similar to our baseline findings. Table 3.5 presents the results. The first three columns report the baseline results and the last three columns show the dynamic regression outcomes. Similar to our previous findings, we do not find evidence on pre-trends in common ownership prior to the IDD adoption. In untabulated analyses, we find both qualitatively and quantitatively similar results using two recently-developed staggered DiD methods proposed by Cengiz et al. (2019) and Sun and Abraham (2021)[9]. This suggests that our results obtained using the full sample are not contaminated by the unobservable factors and provides further evidence on the plausibility of the causality of our findings.

3.4.4 Alternative Common Ownership Measures

We further consider alternative measures for common ownership. One potential concern is that the main measure cannot accurately capture the influence and incentives of common owners given the shareholder heterogeneities. We therefore look at the common ownership of the firm's top 5 largest shareholders, who have the strongest controlling power and incentive to influence management. We follow the literature define the top 5 shareholders' common ownership as below:

$$Top5CO_j = \sum_{i=1}^{5} \sum_{k \neq j} \omega_k \beta_{i,k} \tag{3.3}$$

where j and k are industry peers that are cross-held by the same institutional investor i. $\beta_{i,k}$ is the ownership stake held by i in firm k. ω_k denotes the weight and is measured as the market capitalization weight of firm k in its industry. This measure captures the stakes the focal firm j's 5 largest institutional shareholders have in its industry peers.

As a robustness check, we regress this alternative measure of common ownership on our IDD indicator and report the results in the first column of Table 3.6. Our findings show a significant increase in firms' top 5 shareholders' cross-holdings. More interestingly, the magnitude of the coefficient is larger than that of the baseline findings. This suggests that large shareholders have a stronger incentive to increase their cross-holding stakes as a way to compensate for the obstructed information flows across their portfolio firms.

On another note, we also explore the cross-holding measures $CrossDummy$, $NumConnected$, $NumCross$, $AvgNum$, and $TotalCrossOwn$ developed in He and Huang (2017).[10] These measures provide more insights on a firm's relation with its industry peers through cross-holders. Of

[9] Results are available upon request.

[10] $CrossDummy$ is a dummy variable that equals one if the firm is cross-held in a fiscal year and zero otherwise. $NumConnected$ is the number of peer firms within the same SIC4 industry that share any common holders with the firm. $NumCross$ is the number of unique institutions that cross-hold the firm. $AvgNum$ is calculated as the number of same-industry firms cross-held by each cross-holding institution averaged by the number of all such institutions. $TotalCrossOwn$ is the sum of all cross-owners' percentage holdings in a firm's shares. For detailed measure constructions, please see He and Huang (2017).

all the five measures, we find a significant positive relation between common ownership and the IDD adoption in three of them, *NumConnected*, *NumCross*, and *TotalCrossOwn*. Note that *CrossDummy* is an indicator capturing whether a firm is cross-held, which we believe is a much rougher measure for common ownership and cannot reveal the extent of cross-holdings. It is thus not surprising that no result is found - firms can be cross-held beforehand, and IDD may increase the level of firms' cross-holdings which cannot be captured by *CrossDummy*. As for *AvgNum*, it is likely that the number of cross owners also increases following the IDD adoption, which mitigates the increase in this measure. Nevertheless, the coefficients of these two measures are both positive, though not significant.

3.5 Economic Mechanisms and Further Analyses

3.5.1 Information-Sharing Channel

To investigate what drives the impact of IDD on firm-level common ownership, we exploit cross-sectional analyses and ask whether the information-sharing-facilitating effect of common ownership contributes to the increase in cross-holdings.

If common ownership is used as a strategic way to offset the information frictions brought by IDD and to access valuable human capital resources, institutional shareholders should arguably have greater incentives to enhance their cross-holdings in industries that rely more on human and knowledge capital.

To investigate this, we use several proxies to measure the importance of human capital. We first construct the R&D intensity measure using the ratio of R&D expenditure to total assets because human capital is particularly important for high R&D firms. We interact *IDD* with *High R&D*, an indicator variable that equals to one if the industry-level R&D expenditure is above the sample median, and zero otherwise. Table 3.7 column (1) shows the results. We find that the coefficient on the interaction of *IDD * High R&D* is significantly positive at the 5% level, which suggests that the treatment effect is stronger for firms that operate in high R&D industries.

Second, we use the ratio of intangible assets to total assets to identify firms with more human and knowledge capital, as intangible assets include items such as copyrights, patents and trademarks. We define a *High Intangible* indicator as one if the industry-level intangible assets are above the sample median and add the interaction of IDD and *High Intangible* into our regression model. The coefficient on the interaction term is positive and significant, indicating a more pronounced effect of IDD on firms operating in intangible-assets-intensive industries.

Third, following the previous studies (e.g., Ghaly et al., 2015; Ben-Nasr and Ghouma, 2018; Cao and Rees, 2020), we define a firm as human-capital-intensive if the firm belongs to telecommunications, high-tech, and healthcare industries.[11] We re-estimate the main regression by adding the interaction term of *IDD * High Human Capital Intensity*. As reported in column (3), the coefficient of the interaction term is positive and significant at the 5% level, indicating that the adoption of the IDD has a significantly larger impact on common ownership in human-capital-intensive industries.

Finally, we follow Chen et al. (2020a) and measure knowledge occupation intensity as the proportion of knowledge occupations with respect to all occupations in each industry. Based on the IPUMS occupational code book, we define knowledge workers as those with an occupational code below 200, which includes occupations such as managers, scientists, and engineers, etc.[12] We then define the *High Knowledge Occupation Intensity* indicator as one if the proportion of knowledgeable workers among all workers in the firm's 2-digit SIC industry is above the sample median, and zero otherwise. In column (4), we interact *IDD* with *High Knowledge Occupation Intensity*. Supporting our prediction, we find that the recognition of IDD leads to a significantly greater increase in common ownership for industries that rely more on knowledge occupations.

Taking into account that the influence of the IDD on firm-level common ownership results from heightened labor market frictions during the direct hiring of talent, we anticipate that the treatment effects will be more significant for firms within industries characterized by greater labor market mobility.

To examine this, we construct several proxies to capture the level of labor mobility of an industry. Following Levine et al. (2020), we first construct the labor volatility measure, and we define a *High Labor Volatility* indicator as one if the standard deviation of the number of employees relative to the value of plant, property, and equipment (PPE) assets over time for firms in an industry is greater than the sample median, and zero otherwise. In column (1) of Table 3.8, we interact *IDD* with *High Labor Volatility*, and find that the coefficient of the interaction term is significantly positive.

[11] We include the following two- and three-digit SIC codes: 283, 357, 36, 384, 48 and 80.

[12] We obtain employment data from the Integrated Public Use Microdata Series (IPUMS) database, which provides Current Population Survey (CPS) data on individual worker's occupational code, industry, state, etc. Since the CPS data does not provide SIC industry information directly, we manually link the 1990 industry code to the two-digit SIC code.

This indicates that the effect of IDD is more pronounced for firms operating in industries with greater labor volatility.

Second, Deng and Gao (2013) and Gao et al. (2015) show that employees enjoy greater employment mobility in areas where a significant number of industry peer firms are present in the local labor market. Following their studies, we use the number of peer firms and industry entry rates as two additional proxies for labor market mobility. We then define the *Many Rivals* indicator as one if the number of firms in the same two-digit SIC industry is above the sample median, and zero otherwise. We also use the industry entry rates constructed by Klapper et al. (2006) and define the *High Entry Rate* indicator as one if the industry entry rate is above the sample median. As reported in columns (2) and (3), the coefficients of the interaction terms are significantly positive, which further confirms our conjecture that the effect of IDD is more pronounced for firms operating in industries with many peers (and thus their employees are more likely to switch jobs ex ante).

Finally, Oyer and Schaefer (2005) argue that employee stock options help retain employees. Consequently, employees with fewer stock options are anticipated to exhibit higher mobility in the labor market, leading to a more pronounced increase in the common ownership level for firms in industries with a lower prevalence of employee stock options. We define the *Low option grant* indicator equal to one if the industry-level employee stock option grant is below the sample median, and zero otherwise. As expected, the interaction term is positive and significant at the 1% level, suggesting that the treatment effect is significant for firms in industries characterized by a low level of employee stock options.

Overall, we find that the effect of IDD adoption on a firm's common ownership is more pronounced for firms that rely more on human capital and operate in industries with greater employment mobility. These results provide evidence in support of our proposed information-sharing channel.

3.5.2 Different Types of Common Ownership

As shareholders' incentives to monitor and influence corporate issues are of different degrees, the extent of the increase in common ownership should vary according to the type of institutional investors. As argued by Gaspar et al. (2005) and Chen et al. (2007), institutional shareholders with longer investment horizons tend to be better monitors, whereas short-term institutions focus more on information gathering and trading and are less likely to influence a firm's management decisions. Therefore, long-term institutional investors with stable investment strategies would have a greater incentive to enhance the long-run growth of invested firms in order to boost profits. If this is the case, long-term investors would be more likely to play a bridge-building role by facilitating information spillovers among their portfolio firms after the IDD adoption. We therefore expect a stronger treatment effect on the level of common ownership of long-term investors.

To identify the investment horizon of institutional investors, we follow Gaspar et al. (2005) and calculate each institutional investor i's churn rate at quarter t, which measures how frequently each institutional investor rotates their portfolio stocks. The churn rate is calculated as,

$$CR_{i,t} = \frac{\sum_{j \in Q} |N_{j,i,t}P_{j,t} - N_{j,i,t-1}P_{j,t-1} - N_{j,i,t-1} \triangle P_{j,t}|}{\sum_{j \in Q} \frac{N_{j,i,t}P_{j,t} + N_{j,i,t-1}P_{j,t-1}}{2}} \tag{3.4}$$

where P and N represent the price and number of shares of company j held by institutional investor i in quarter t. Q is the set of companies held by investor i. As our analysis is conducted at year level, we annualize the quarterly churn rates by calculating institutional investor i's average churn rate over the past four quarters using the formula below,

$$AV\_CR_{i,t} = \frac{1}{4} \sum_{r=1}^{4} CR_{i,t-r+1}. \tag{3.5}$$

As a higher average churn rate indicates a shorter investment horizon, we classify long-term (short-term) institutional investors as the ones whose average churn rate is in the bottom (top) tertile.

Additionally, institutional shareholders with strong incentives to influence corporate strategies are likely to hold the shares for a longer period (Kini et al., 2021). We therefore rely on the holding periods of institutional investors and identify those that hold a firm's shares for at least four consecutive quarters as long-term investors. This method has the advantage of categorizing investors based on individual portfolio firms, which allows for the flexibility that an investor may have heterogeneous monitoring incentives towards different firms in its portfolio.[13] Finally, we construct measures of common ownership by different investor types using the same method in Section 3.3.1.

In Table 3.9, we re-estimate Equation (1) by separately examining the common ownership of long-term and short-term investors. In columns (1) - (2), the dependent variable measures a firm's

[13]For instance, if an investor A holds firm i's share for more than four consecutive quarters, and holds firm j's share for only one quarter, it is identified as a long-term investor when calculating firm i's common ownership level, and is identified as a short-term investor when it comes to firm j.

percentage of shares commonly owned by long-term institutional investors. Similarly, in columns (3) - (4), the dependent variable measures a firm's percentage of shares commonly owned by short-term institutional investors. In panel A, we report the results where churn rates are used to classify long-term investors; whilst in panel B, we use the holding periods of a firm by institutional investors to categorize different investors. In both panels, the coefficients on IDD are significantly positive for long-term investors, indicating that the recognition of IDD causes an increase in common ownership by long-term institutional investors. On the contrary, we do not find such an effect when it comes to short-term institutional investors.

Overall, our findings in this section show that the treatment effect of IDD adoption is mainly driven by the common ownership of long-term investors. This is in line with the hypothesis that the increased common ownership following the IDD adoption is due to firms' incentive to facilitate information-sharing.

3.5.3 Possible Product Market Gains

Efficiency Gains or Collusion?

The increase in common ownership can also be driven by firms' motives towards collusion. To collude, firms need to communicate with other colluding partners in order to coordinate their market behaviors. However, direct communication (e.g. explicit collusion) between firms is generally easy to be detected by antitrust authorities and can cause expensive legal costs. As such, public disclosure with no direct communication is often seen as an indispensable method, which is normally legal and can still facilitate collusion by aiding coordination and monitoring for defections. For instance, Awaya and Krishna (2016) find that sales-related communication improves monitoring and increases collusive profits. Bourveau et al. (2020) show that as cartel enforcement becomes more effective, U.S. firms share more detailed information about customers, contracts, and products via financial disclosures, enabling peers to tacitly coordinate product market actions. More recently, Bertomeu et al. (2021) provide combined theoretical and empirical support that firms do use their public discourse to tacitly collude.

However, IDD may impede firms' voluntary disclosure: previous studies find that IDD recognition increases the proprietary costs of disclosure, and in response, corporate managers withhold more information (Li et al., 2018; Kim et al., 2021).[14]. As it might be costlier to collude through public disclosure after the IDD adoption, firms may seek common owners as an alternative. Corporate managers in common-owned firms could continue to tacitly collude, because common ownership can enhance information transfer between firms via the owners themselves, facilitating anti-competitive behaviors without the need for public disclosure. Therefore, to sustain collusion, common ownership becomes more important after the adoption of IDD and an increase in common ownership is thus expected.

While our data does not allow us to explore this possibility directly (e.g., whether firms are coordinating on prices), we examine firms' operating performance as well as innovation activities to identify firms' true motive. Specifically, if the IDD-induced increase in common ownership is efficiency-improving, we should observe not just improved operating performance, but also intensified corporate innovation activities.

Identification: Mergers between Financial Institutions

To answer the question of whether the IDD-induced increase in common ownership brings about efficiency effects, we examine whether and how it affects firms' R&D activities and operating performance. As the firm-level common ownership is endogenous in our setting, we cannot conduct a conventional cross-sectional analysis by adding its interaction term with the IDD indicator into our baseline regressions.[15] We henceforth employ another difference-in-difference setting following the common ownership literature and use the mergers between financial institutions as exogenous shocks to firm-level common ownership (He and Huang, 2017; Lewellen and Lowry, 2021; Kini et al., 2021).

We use the M&A deals summarized in Table A.3.2 that took place during our sample period as the quasi-natural experiments following Lewellen and Lowry (2021).[16] To construct the treated sample, we identify firms that are likely to experience an increase in common ownership as a result

[14]The adoption of IDD increases a firm's proprietary costs of disclosure. That is, with less access to trade secrets through employee mobility, public disclosure could contain more valuable information, and rival firms would rely more heavily on a firm's public disclosure to discover its proprietary information (Li et al., 2018; Kim et al., 2021)

[15]To examine the association between the choice to increase common ownership after IDD and subsequent innovation activities, we first add the interaction term of *IDD * Common Ownership* and provide preliminary results. Table A.3.4 shows that the coefficients of all interactions are positive but only significant when using innovation productivity as the dependent variable.

[16]The earliest M&A deal in our analysis took place in 1997. We choose this year to ensure that firms have 3-year financial data prior to the first shock.

of financial institution mergers. Specifically, we first require that all the selected firms are blockheld by one of the merging institutions, and then exclude the ones with no same-industry peers being blockheld by the other merging party in the quarter prior to the deal announcement date.

We construct two pools of firms for the control sample. Regarding the first control sample, we require that all the chosen firms are blockheld by one of the merging partners, and the other partner does not simultaneously blockhold any same-industry competitor of these firms. This ensures that the mergers would not change the level of control firms' common ownership. The selection process implies that all the control firms are from different industries compared with the treated firms and we therefore refer to this sample as the "different-industry control sample". This leads to one potential drawback that the control firms are different from the treated firms as there might be industry-level heterogeneities, which may introduce noises to our analysis. To mitigate this issue, we follow Lewellen and Lowry (2021) and construct our second control sample, the "same-industry control sample", using firms from the same industries as the treated ones (see Figure A.3.1 for an illustration). To be selected as a same-industry control firm, we require it to be blockheld by a non-merging institution, and to be closest in market capitalization to a treated firm before the merger. As a final filtering procedure, we exclude firms that have no data one quarter before the deal completion date and impose a $(-3, +3)$ window around the event year in our analysis.[17]

Measurement of Innovation

We adopt the common measures of firms' innovation activities used in the finance and innovation literature, the number of patents and citations. The patent information is extracted from the United States Patent and Trademark Office (USPTO) website and from the patent database compiled by Michael Woeppel.[18] We calculate the number of patents produced by each firm in each year as a measure of corporate innovation output.[19] We also rely on the category-year adjusted citations to measure the quality and quantity of firms' innovative activities.[20] To mitigate the skewness of the patent data, we take the natural log value of one plus the patent/citation number to generate our main dependent variables.

[17]Our results are very similar both qualitatively and quantitatively using a $(-1, +1)$ event window.

[18]See https://www.mikewoeppel.com/data for the data details. The data was compiled and used in Stoffman et al. (2022)

[19]Following the literature (e.g., He and Tian, 2013; He and Huang, 2017), we use the application year of a patent to determine a firm's innovation output.

[20]For the detailed adjustment method, please see Hall et al. (2001).

Firms' numbers of patents and citations are reliable proxies for their innovation activities. However, they do not necessarily reveal a firm's innovation productivity. Firms with high numbers of patents or citations can be financially-unconstrained firms that spend a large part of their financial slack on R&D. If firms can alleviate the frictions in knowledge spillovers induced by IDD through common ownership, we should also see an increase in firms' innovation productivity. We exploit the innovation productivity measure used in He and Huang (2017) as our third measure of corporate innovation, which is calculated as the number of patents generated by a firm over its lagged R&D expenditures. Given the time-consuming characteristics of R&D, we follow the literature and take the one-year future value of all our innovation measures in the regression analysis.

Innovation Output and Operating Performance

The identifying assumption of our diff-in-diff setting using the M&A deals is that after the mergers take place, the level of the common ownership of portfolio firms held by the merging parties experiences a significant increase. To illustrate this, we regress our main measure of common ownership on the M&A deal indicator "*Treat*Post*", where *Treat* denotes the M&A treatment and *Post* denotes the time. As is shown in Table 3.10, portfolio firms' common ownership increases significantly following the mergers of financial institutions. Column (1) uses the "different-industry control" sample where specific characteristics of the merger events are captured, and column (2) uses the "same-industry control" sample which guarantees the comparability of the treated and controls.

We argue that institutional shareholders strategically increase their portfolio firms' common ownership after the IDD adoption in an effort to facilitate information and valuable human capital sharing. If the motive behind this is efficiency-improving, we should see an increase in firms' innovation outputs as well as an improvement in corporate performance. On the contrary, if firms do this to tacitly collude, we should see an improvement in performance, but not in innovation outcomes. The regression results examining the valuation effect of common ownership and IDD adoption are reported in Table 3.11.

As discussed in the previous section, we exploit the number of patents, innovation productivity, and the number of adjusted citations as our proxies for innovation activities. We find that corporate innovation outputs experience a significant increase for firms headquartered in IDD-adopted states following the sudden increase in common ownership. In columns (1)-(3) of Panel A, we exploit the different-industry sample and in columns (4)-(6), we use the same-industry sample. We regress our three proxies on the interaction terms of *Treat*, *Post*, and *IDD*, respectively. Our variable of interest is the triple interaction term *Treat*Post*IDD*, which captures the effect of an increase in a firm's common ownership on its innovative activities if a state has an active IDD. If the IDD-induced increase in common ownership is efficiency-improving, the coefficient of the triple interaction term should be positive and significant. Our results confirm this hypothesis.

Common owners increase their cross-holdings following the IDD adoption in pursuit of portfolio value maximization. We therefore should see better performance in the affected portfolio firms headquartered in IDD-adopted states. To test for this, we construct two measures of operating performance: ROA and NPM. ROA is the return on assets calculated as the ratio of operating income divided by total assets, and NPM is the net profit margin calculated as the ratio of net income divided by sales. In Panel B, we again employ the two samples of control firms and find an increase in firms' operating performance. To be more specific, when using the sample with control firms from different industries, we see a 1.1% increase in ROA and a 3.7% increase in NPM. Similarly, for the sample using same-industry controls, we document a 1.6% increase in ROA. Although the coefficient of the triple interaction term is not significant when NPM is used as the dependent variable, the sign is positive. Overall, our results suggest that increased common ownership in the presence of IDD improves firms' operating performance.

3.5.4 Local Economic Growth

Finally, we move beyond firm-level investigations and explore the valuation effect in a broader economic environment. As common ownership plays an important role in information sharing and inter-firm coordination, we expect the state-level common ownership occasioned by IDD to be associated with higher local economic growth.

Table A.3.5 presents the results examining whether the IDD-induced increase in common ownership would boost state-level economic growth. The dependent variable is the state-level GDP growth, and the main variable of interest is the interaction term between IDD and the average level of common ownership in that state. Following our baseline regression, we control for time-varying state characteristics as well as year and state fixed effects. Column (2) of Table A.3.5 shows that the coefficient of the interaction term of IDD and common ownership is positive and significant, which indicates that common ownership promotes local economic growth where an IDD is effective in the state. This finding again corroborates the information-sharing channel of common ownership, and suggests an efficiency-improving implication. Note that as the state-level common ownership may suffer from endogeneity issues, the results should therefore be interpreted with caution.

3.6 Robustness Tests and Additional Investigation

In this section, we carry out a battery of robustness tests to further prove the validity of our results.

First, we show that our results remain unchanged when using various subsamples. Column (1) in Table 3.12 uses a refined industry definition where we omit firms with the fourth-digit SIC codes being 0 or 9 as previous studies show that these SIC codes may not accurately define the economic market (Clarke, 1989; Kahle and Walkling, 1996; He and Huang, 2017). Column (2) drops the industry-years with fewer than five observations, and Column (3) uses only manufacturing firms (SIC codes between 2000 and 3999). The results are very similar to those in our baseline regressions, suggesting that our results are not driven by certain subsamples.

Second, given that some states had adopted the Inevitable Disclosure Doctrine before the beginning of our sample period, including those states in the sample may lead to confounding results. We therefore omit the states that recognized the Doctrine before 1994 and never reversed the decision during our sample period. It is also possible that our results are driven by some unobservable characteristics shared among the treated states. Although the staggering nature of the IDD adoption alleviates this problem, we carry out a placebo test for a further robustness check. To do this, we assign the treated states' IDD adoption years to their neighboring states and no significant results appear, which cements the validity of our results.

Third, it is challenging to accurately define a product market. We use the SIC 4-digit code as our main industry classification, upon which our common ownership measure is built on. We acknowledge that the 4-digit SIC industry classification may be too vague to define a firm and its rivals. Moreover, firms may have more than one major operating business line, which makes it difficult to classify each firm to one SIC code. The fact that firms may strategically explore new business opportunities to cope with the ever-changing market leads to a change in their rivals. Using a fixed industry classification code may cloud the results by not revealing the most up-to-date corporate relationships. To solve this issue, we use the NAICS 6-digit code and the Text-based Network Industry Classifications developed by Hoberg and Philips (2010, 2016). Similar to the SIC, NAICS is a fixed industry classification code. However, the most recent version of NAICS was updated in 2017, which is relatively newer than the SIC code and can illustrate a more up-to-date relation between firms and their counterparts within one granular industry. Regarding the time-varying characteristics of firms' competition relations, we resort to the TNIC industry classification. This classification is built from a 10K-based textual analysis, which to the largest extent captures firms' main products, and thus business lines on an annual basis. Firms' rivals are henceforth assigned based on the similarity between their main products, which mitigates the shortcomings of the traditional industry classifications. We also re-calculate the level of common ownership using another industry classification proposed by Hoberg and Philips (2010, 2016), the FIC300, and regress it on our IDD indicator. Our results are robust to exploiting different industry classifications.

Fourth, similar to the argument above, it is unlikely that all institutional shareholders are keen on shaping firms' managerial decisions. Institutional shareholders with a relatively small share of a firm may not be able to facilitate the communication, and thus this increase in common ownership is likely to be driven by the small institutional investors' intention to free-ride. To alleviate this issue, we raise the threshold of 0.5% to the blockholding level, 5% and re-calculate the measure using only these investors. We still witness a significant increase in common ownership following the adoption of IDD.

Fifth, all of the firms in our sample are from the Compustat database, which mainly covers large firms in the US. This would introduce a downward bias to our findings for two reasons. On the one hand, it is costlier for institutional shareholders to get control of multiple large firms. On the other hand, compared with small firms, it would be easier for large firms to choose corporate acquisitions as an alternative means of obtaining human capital resources. To test this conjecture, we examine whether our results are more pronounced for smaller firms. Specifically, we use the book value of total assets, sales and the number of employees to measure firm size. As reported in Table A.3.6, the treatment effect of IDD is significantly stronger for smaller firms, which confirms our hypothesis.

Sixth, Amel-Zadeh et al. (2022) provide evidence that omitting ownership by non-financial block holders and insiders can lead to erroneous measures of overlapping ownership. Moreover, they show that the holdings of the "Big Three" institutional asset managers is one of the main drivers of common ownership. To make sure our results are not biased by these potential issues, we obtain data on ownership from the FactSet database, which collects global equity ownership for institutions, funds, and non-institutional "insiders/stake holders".[21] Also, we remove the "Big Three" holdings when constructing the measure of common ownership and re-estimate our model. Results are reported in Table A.3.7 and confirm the positive and statistically significant relationship between the IDD adoption and common ownership.

3.7 Conclusion

Although there is a rich literature studying the effect of common ownership on various macro- and micro-level outcomes, questions regarding the motives behind the rise of common ownership are still open. In our paper, we investigate whether common ownership may arise as a mechanism to offset the hurdles in sharing human and knowledge capital resources among firms within the same industry.

We exploit the staggered recognition of the U.S. state-level IDD in a difference-in-difference setting. The IDD is a law that tightens trade secrets protection by reducing the mobility of workers. By disclosing a positive link between the IDD adoption and firm-level common ownership, we confirm that common owners can facilitate the sharing of information among industry peers and help them access valuable human capital resources. Specifically, firms that rely more on human capital and have employees with higher *ex ante* mobility in the labor market are more likely to experience an increase in common ownership following the IDD adoption. Consistent with the conventional belief that long-term institutional shareholders should have stronger incentives to promote information sharing across firms, our findings suggest that the IDD-induced increase in common ownership by long-term institutional shareholders is more pronounced.

Our findings contribute to a recent strand of the common ownership literature by highlighting that common ownership can bring about an efficiency-improving effect alongside its well-documented anti-competitive effect. We find that firms enjoy better innovation outcomes when there is an increase in common ownership in the presence of IDD, which lends support to our prediction that common ownership helps overcome labor market frictions via facilitating information flows among industry peers. We provide new evidence on the bright side of common ownership, which offers important policy implications given the rising attention of regulators on common ownership due to its potential impact on the overall market.

[21] As the ownership coverage in the FactSet database is only available since 1999, we perform this test on a subsample beginning in 1999.

Table 3.1: Exogeneity test for IDD

This table reports the validation test results of the Inevitable Disclosure Doctrine (IDD). We regress the *IDD Adoption* indicator on state-level common ownership to examine if the recognition of IDD is exogenous in our setting. The indicator variable *IDD Adoption* takes the value of one if IDD is recognized in a state, and zero otherwise. *State-Level CO* measures firms' common ownership collapsed at the state-level. We also include other state characteristics such as unemployment rate and GDP growth. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. State and year fixed effects are included in the regressions. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	IDD Adoption	
	(1)	(2)
State-Level CO	0.007	0.009
	(0.007)	(0.008)
Unemployment Rate		-0.004
		(0.020)
GDP Growth		-0.003
		(0.003)
Estabilishment Entry Rate		-0.027
		(0.036)
Estabilishment Exit Rate		0.019
		(0.028)
CNC Index		-0.073
		(0.114)
Observations	1077	1077
R^2	0.790	0.792
State FE	Yes	Yes
Year FE	Yes	Yes

Table 3.2: Descriptive Statistics

This table shows the descriptive statistics for our sample firms from 1994 to 2015. All continuous variables are winsorized at 1% level.

	Mean	SD	Min	P25	Median	P75	Max	Obs
Variables of Interest								
Common Ownership	2.966	3.856	0.000	0.012	1.267	4.560	16.730	56562
IDD	0.483	0.500	0.000	0.000	0.000	1.000	1.000	56562
Firm-Level Variables								
Size	5.840	1.952	1.601	4.439	5.756	7.179	10.686	56437
Leverage	0.203	0.212	0.000	0.008	0.156	0.323	0.980	56192
ROA	0.067	0.217	-1.031	0.045	0.114	0.172	0.413	56273
Cash	0.216	0.237	0.000	0.033	0.122	0.325	0.936	56421
Cash Flow	0.017	0.234	-1.243	0.016	0.077	0.125	0.328	56271
Tobin's Q	2.213	1.859	0.593	1.157	1.595	2.482	11.949	56250
CAPEX	0.054	0.058	0.000	0.018	0.035	0.067	0.323	56013
State-Level Variables								
Unemployment Rate	5.934	1.948	2.700	4.625	5.458	6.758	12.192	56562
GDP Growth	4.864	2.676	-3.600	3.500	4.900	6.600	10.100	56562
Establishment Entry Rate	11.120	1.698	7.669	9.941	10.992	12.261	15.938	56562
Establishment Exit Rate	9.987	1.194	7.596	9.135	9.955	10.804	13.048	56562
Business Combination Laws	0.578	0.494	0.000	0.000	1.000	1.000	1.000	56562
CNC Index	3.668	2.329	0.000	3.000	4.000	5.000	9.000	56562

Table 3.3: Effect of IDD on common ownership - baseline results

This table reports our baseline results regarding the change of common ownership due to the Inevitable Disclosure Doctrine (IDD) adoption. The dependent variable is *Common ownership*, which measures institutional shareholders' crossholdings at the firm level. The indicator variable IDD takes the value of one if the IDD is recognized in a state, and zero otherwise. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership			
	(1)	(2)	(3)	(4)
IDD	0.207**	0.227**	0.230***	
	(0.090)	(0.088)	(0.077)	
IDD Adopt				0.230**
				(0.087)
IDD Reject				-0.182
				(0.115)
Size		0.830***	0.830***	0.831***
		(0.049)	(0.049)	(0.049)
Leverage		-0.084	-0.084	-0.082
		(0.156)	(0.154)	(0.154)
ROA		-0.244	-0.243	-0.243
		(0.154)	(0.153)	(0.153)
Cash		-0.070	-0.069	-0.067
		(0.179)	(0.179)	(0.178)
Cash Flow		0.143	0.144	0.144
		(0.100)	(0.100)	(0.099)
Tobin's Q		0.102***	0.103***	0.102***
		(0.017)	(0.017)	(0.017)
CAPEX		0.245	0.248	0.249
		(0.363)	(0.364)	(0.363)
Unemployment Rate			0.014	0.012
			(0.042)	(0.043)
GDP Growth			-0.007	-0.007
			(0.007)	(0.007)
Establishment Entry Rate			0.020	0.017
			(0.047)	(0.048)
Establishment Exit Rate			-0.045	-0.043
			(0.055)	(0.056)
Business Combination Laws			-0.032	-0.011
			(0.181)	(0.184)
CNC Index			0.005	0.002
			(0.030)	(0.031)
Observations	55254	54216	54216	54216
R^2	0.773	0.783	0.783	0.783
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.4: Effect of IDD on common ownership - dynamic results

This table reports our dynamic regression results regarding the change of common ownership due to the Inevitable Disclosure Doctrine (IDD) adoption. The dependent variable is *Common ownership*, which measures institutional shareholders' crossholdings at the firm level. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership		
	(1)	(2)	(3)
IDD($\tau = -2$)	0.066	-0.001	-0.001
	(0.131)	(0.116)	(0.114)
IDD($\tau = -1$)	0.109	0.078	0.077
	(0.143)	(0.139)	(0.138)
IDD($\tau = 0$)	0.130	0.081	0.083
	(0.154)	(0.149)	(0.145)
IDD($\tau = +1$)	0.258**	0.224**	0.226**
	(0.114)	(0.102)	(0.102)
IDD($\tau = +2$)	0.202*	0.173	0.188*
	(0.115)	(0.105)	(0.101)
IDD($\tau \geq +3$)	0.228**	0.246**	0.250**
	(0.108)	(0.108)	(0.098)
Observations	55254	54216	54216
R^2	0.773	0.783	0.783
Firm-Level Controls	No	Yes	Yes
State-Level Controls	No	No	Yes
Firm FE	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes

Table 3.5: Effect of IDD on common ownership - matched sample

This table reports how the firm-level common ownership is affected by the IDD adoption using our matched sample. The dependent variable is *Common ownership*, which measures institutional shareholders' crossholdings at the firm level. The indicator variable IDD takes the value of one if the IDD is recognized in a state, and zero otherwise. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership					
	(1)	(2)	(3)	(4)	(5)	(6)
IDD	0.196	0.208*	0.243*			
	(0.127)	(0.120)	(0.133)			
IDD($\tau = -2$)				0.185	0.182	0.179
				(0.131)	(0.132)	(0.129)
IDD($\tau = -1$)				0.229	0.208	0.203
				(0.156)	(0.155)	(0.151)
IDD($\tau = 0$)				0.275	0.250	0.281
				(0.204)	(0.191)	(0.202)
IDD($\tau = +1$)				0.415**	0.390**	0.395**
				(0.184)	(0.174)	(0.191)
IDD($\tau = +2$)				0.415*	0.395*	0.429*
				(0.222)	(0.216)	(0.238)
IDD($\tau \geq +3$)				0.282	0.310	0.360
				(0.194)	(0.195)	(0.224)
Observations	9803	9619	9619	9803	9619	9619
R^2	0.798	0.803	0.804	0.798	0.803	0.804
Firm-Level Controls	No	Yes	Yes	No	Yes	Yes
State-Level Controls	No	No	Yes	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.6: Alternative measures of common ownership

This table reports the results using alternative measures of common ownership. Column (1) uses *Top 5* as the dependent variable, which measures the firm's top 5 largest shareholders' crossholdings. Columns (2) - (6) use the measures from He and Huang (2017) as the dependent variables. *Cross Dummy* is a dummy variable that equals one if a firm is cross-held in a given year, and zero otherwise. *Num Connected* is the number of same-industry rivals that share any common owner with the firm. *Num Cross* is the number of unique institutions that cross-hold the firm in that year. *Avg Num* is the number of same-industry peers block-held by the average cross-holding institution. *Total Cross Own* is the sum of all common owners' percentage holdings in the firm itself. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership					
	Top 5	Cross Hold	Num Connected	Num Cross	Avg Num	Total Cross Own
	(1)	(2)	(3)	(4)	(5)	(6)
IDD	1.210**	0.009	0.037**	0.018*	0.022	0.004**
	(0.550)	(0.010)	(0.018)	(0.010)	(0.015)	(0.002)
Observations	54216	54216	54216	54216	54216	54216
R^2	0.727	0.623	0.734	0.678	0.705	0.660
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.7: Cross-sectional variations - human capital

This table reports the results on the effect of IDD on common ownership conditional on the importance of human capital to the firm. The dependent variable is *Common Ownership* measured at the firm level. The indicator variable IDD takes the value of one if the IDD is recognized in a state, and zero otherwise. In the first column, we use innovation activities to measure firms' dependence on labor. *High R&D* indicator equals one if the industry-level R&D expense is above the sample median, and zero otherwise. *High Intangible* is an indicator that takes the value of one if the industry-level intangible assets is above the sample median, and zero otherwise. *High Human Capital Intensity* is an indicator of whether a firm is human capital intensive, that is, if a firm belongs to telecommunications, high-tech, and healthcare industries. *High Knowledge Occupation Intensity* indicates whether a firm operates in an industry that has an above median knowledgeable occupation intensity. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership			
	(1)	(2)	(3)	(4)
IDD	0.055	0.077	0.110*	0.048
	(0.076)	(0.109)	(0.059)	(0.099)
IDD * High R&D	0.417**			
	(0.162)			
IDD * High Intangible		0.359*		
		(0.197)		
IDD * High Human Capital Intensity			0.477**	
			(0.226)	
IDD * High Knowledge Occupation Intensity				0.355*
				(0.199)
Observations	54216	54216	54216	53469
R^2	0.783	0.783	0.783	0.780
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.8: Cross-sectional variations - labor mobility

This table reports the effect of IDD on common ownership conditional on employment mobility in the labor market. The dependent variable is *Common Ownership* measured at the firm level. The indicator variable IDD takes the value of one if the IDD is recognized in a state, and zero otherwise. *High Labor Volatility* is an indicator that takes the value of one if the industry-level labor volatility is above the sample median, and zero otherwise. *Many Rivals* is an indicator that equals one if the number of firms operating in that industry is above the sample median, and zero otherwise. *High Entry Rate* is a dummy variable indicating whether an industry's entry rate is above the sample median or not. *Low Stock Option* is an indicator that takes the value of one if the industry-level employee stock option grant is below the sample median, and zero otherwise. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership			
	(1)	(2)	(3)	(4)
IDD	0.054	0.055	-0.094	0.019
	(0.092)	(0.076)	(0.135)	(0.111)
IDD * High Labor Volatility	0.351***			
	(0.130)			
IDD * Many Rivals		0.417**		
		(0.162)		
IDD * High Entry Rate			0.487***	
			(0.173)	
IDD * Low Stock Option				0.488***
				(0.172)
Observations	54216	54216	52656	53254
R^2	0.783	0.783	0.783	0.782
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.9: The impact of IDD on common ownership: long-term vs short-term investors

This table reports the regression results using split samples based on institutional investors' investment horizons. The dependent variable is *Common ownership*, which measures institutional shareholders' crosshodings at firm level. To be specific, we categorize institutional investors based on their investment horizons and calculate the common ownership according to their investment lengths. In panel A, we classify long-term/short-term investors using their churn rates; whilst in panel B, we employ the holding period of a firm by an investor as the classification rule. The indicator variable IDD takes the value of one if the IDD is recognized in a state, and zero otherwise. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership			
	Long-Term Investors		Short-Term Investors	
	(1)	(2)	(3)	(4)
Panel A: Churn Rate				
IDD	0.142*	0.148*	0.030	0.032
	(0.082)	(0.076)	(0.025)	(0.028)
Observations	55254	54216	55254	54216
R^2	0.767	0.773	0.611	0.613
Panel B: Holding Period				
IDD	0.207**	0.224***	0.001	0.003
	(0.086)	(0.080)	(0.002)	(0.002)
Observations	55354	54287	55354	54287
R^2	0.780	0.790	0.512	0.513
Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.10: Effect of institution mergers on common ownership

This table reports the results on the effect of institution mergers on common ownership. The dependent variable is *Common Ownership*. *Treat* is a dummy variable that equals one if a firm is a treatment stock and zero if it is a control (see Section 3.5.3 for details). *Post* is a dummy that equals one for the post-event period and zero for the pre-event period. The sample consists of treated firms and different-industry control firms in the first two columns, whilst in column (3)-(4), the sample includes treated firms and same-industry controls. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by institution merger. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership			
	Different Industries		Same Industries	
	(1)	(2)	(3)	(4)
Treat*Post	0.459**	0.420**	0.723***	0.723***
	(0.190)	(0.182)	(0.152)	(0.152)
Observations	8414	8350	2034	2034
R^2	0.789	0.791	0.764	0.764
Controls	No	Yes	No	Yes
Firm*Merge*FE	Yes	Yes	Yes	Yes
Merge*Year*FE	Yes	Yes	Yes	Yes

Table 3.11: Valuation effect of increased common ownership following the IDD adoption

This table examines the valuation effect of increased common ownership after the IDD adoption. Panel A presents the results on how innovation activities are affected and the dependent variables are *Ln(1+Patent)* (log number of patents), *Inno_Prod* and *Ln(1+Citation)* (log number of citations), respectively. Panel B presents the results on operating performance and the dependent variables are *ROA* and *NPM* (net profit margin), respectively. *Treat* is a dummy variable that equals one if a firm is a treatment stock and zero if it is a control (see Section 3.5.3 for details). *Post* is a dummy that equals one for the post-event period and zero for the pre-event period. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by institution merger. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Different Industries			*Same Industries*		
	Ln(1+Patent)	Inno_Prod	Ln(1+Citation)	Ln(1+Patent)	Inno_Prod	Ln(1+Citation)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Innovation Activities						
Treat*Post*IDD	0.188***	0.072**	0.178*	0.267***	0.054*	0.149**
	(0.055)	(0.025)	(0.091)	(0.074)	(0.024)	(0.062)
Observations	7874	8096	7874	1939	1945	1939
R^2	0.938	0.782	0.898	0.953	0.776	0.927
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm*Merge*FE	Yes	Yes	Yes	Yes	Yes	Yes
Merge*Year*FE	Yes	Yes	Yes	Yes	Yes	Yes

	Different Industries		*Same Industries*	
	ROA	NPM	ROA	NPM
	(1)	(2)	(3)	(4)
Panel B: Operating Performance				
Treat*Post*IDD	0.013***	0.039***	0.016*	0.222
	(0.004)	(0.011)	(0.007)	(0.129)
Observations	8320	8300	2023	2010
R^2	0.871	0.814	0.859	0.635
Controls	Yes	Yes	Yes	Yes
Firm*Merge*FE	Yes	Yes	Yes	Yes
Merge*Year*FE	Yes	Yes	Yes	Yes

Table 3.12: Robustness check

This table reports the robustness test results. The dependent variable is *Common ownership*. Column (1) drops the industries with the fourth-digit SIC code being 0 or 9. Column (2) omits all the industry-years with fewer than 5 observations. Column (3) only includes firms operating in the manufacturing industries. Column (4) excludes all the states that have passed IDD before 1994 and have not rejected the decision during our sample period. Column (5) presents the results of our placebo test, where we assign the treated states' IDD adoption years to their neighboring states. Column (6)-(8) exploits alternative industry classifications when calculating common ownership. Column (9) includes only blockholders when measuring common ownership. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by institution merger. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership								
	Refine - Industry	Drop Obs < 5	Manufacturing	Refine - State	Placebo	NAICS	TNIC3	FIC300	Blockhold
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
IDD	0.253**	0.265**	0.236**	0.203**	-0.010	0.272***	0.636***	0.627***	0.143**
	(0.100)	(0.104)	(0.103)	(0.078)	(0.138)	(0.086)	(0.179)	(0.231)	(0.064)
Observations	36731	45534	29132	44353	55254	53888	53773	52741	54216
R^2	0.789	0.791	0.770	0.786	0.773	0.768	0.636	0.713	0.662
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes

Appendix

Figure A.3.1: Sample construction for the financial institution mergers analysis: example

In this example, we have three industries (X, Y, and Z) and two merging financial institutions (A and B). Following Lewellen and Lowry (2021), *Treated firms* are firms that are block-held by one of the merging partners with same-industry rivals being block-held by the other partner (e.g., X1, X2, X3, X4, X6). *Different-industry control sample* includes firms block-held by one merging partner with no same-industry rivals block-held by the other partner (e.g., Y1, Y2, Z1, Z2, Z3). *Same-industry control sample* consists of firms that are not held by either of the institution and are matched to *treated firms* based on industry and market capitalization (e.g., X1', X2', X3', X4').

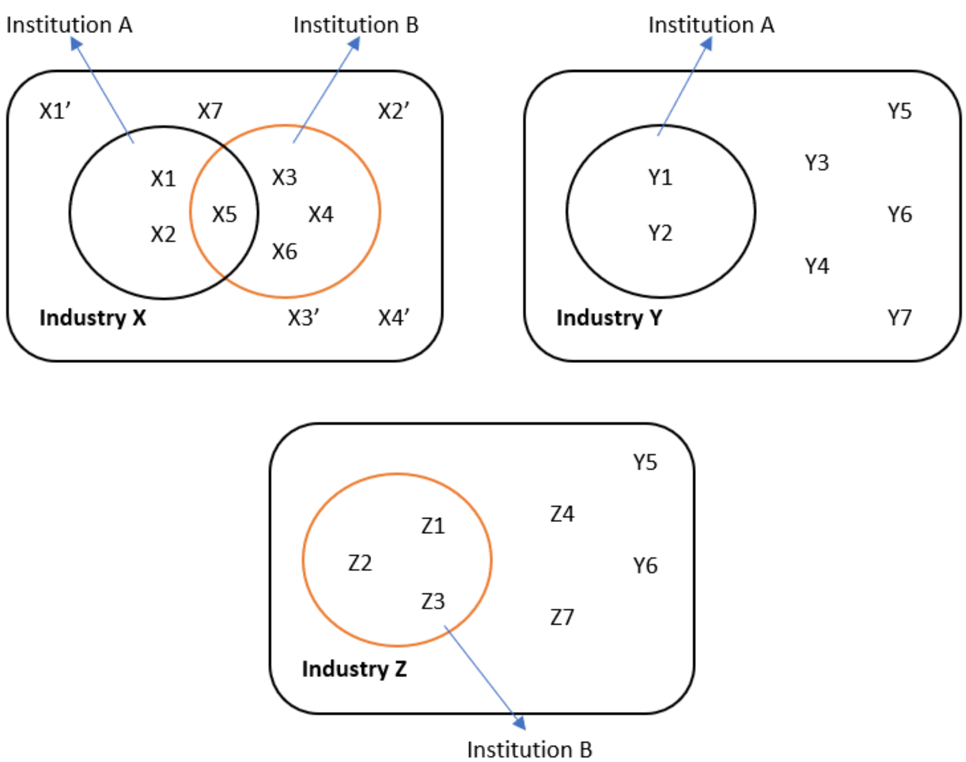


Figure A.3.2: Impact of IDD on common ownership - IW estimates

This figure plots the IW estimates for each relative period, obtained by implementing Sun and Abraham (2021) "interaction weighted" estimator. The confidence intervals in the figure are at 95% level. We include firm as well as industry*year fixed effects. Standard errors are clustered by state.

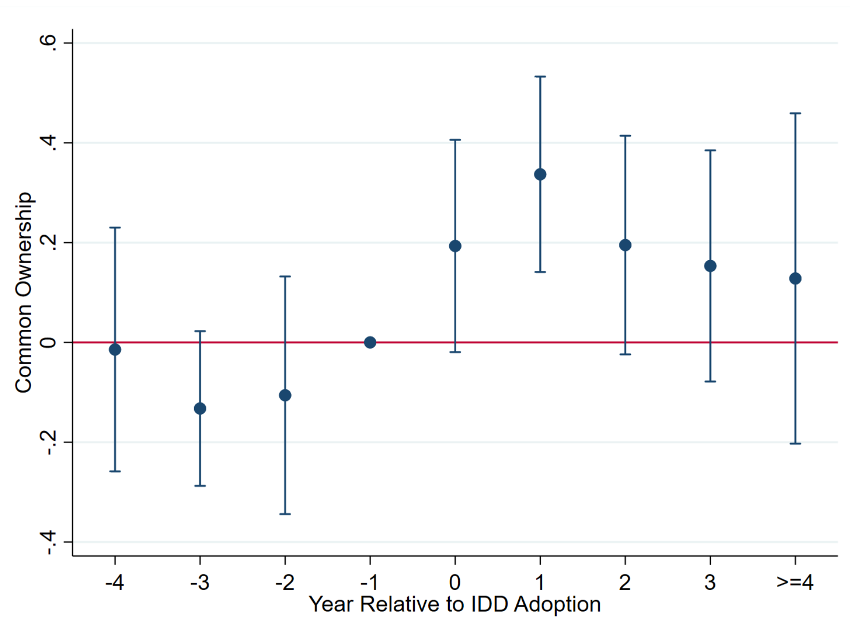
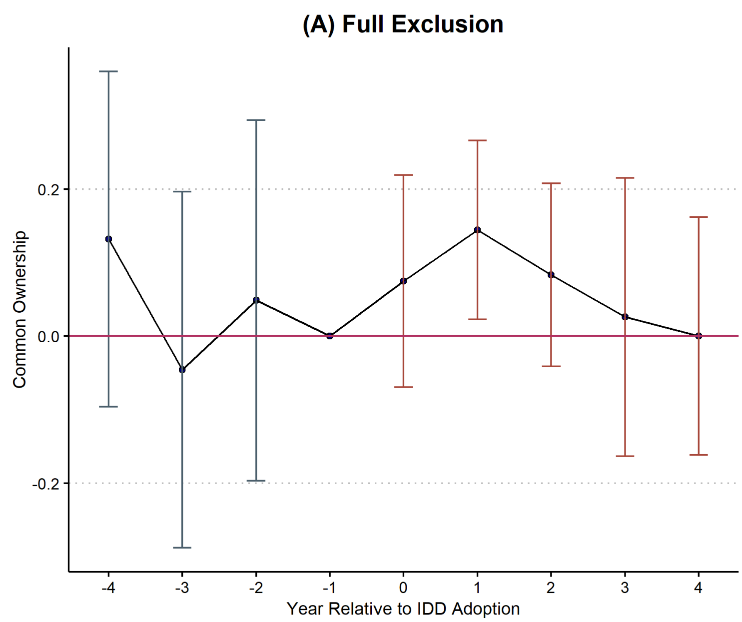


Figure A.3.3: Impact of IDD on common ownership - stacked

This figure plots the event study estimates for each relative period, obtained by implementing the stacked-by-event approach (Cengiz et al., 2019). The confidence intervals in the figure are at 95% level. To stack all the cohorts together, we trim the data to keep an 8-year event window (-4, +4) using full exclusion as well as partial exclusion following Baker et al. (2022). In the full exclusion sample, we stack cohort-specific data using observations from firms that are headquartered in states that recognized IDD in the cohort treatment year as the treated, and include all firms in states that never adopt IDD or adopt IDD more than four years after the cohort treatment year. In the partial exclusion sample, we use firms receiving IDD treatment for the first time in the cohort treatment year as the treated, and use all other firm-year observations without active IDD in place in the 8-year event window as the control. The top panel reports the results with full exclusion, whilst the bottom panel plots the estimates obtained using the sample with partial exclusion. We include firm*cohort as well as year*cohort fixed effects. Standard errors are clustered by state*cohort.



(a)

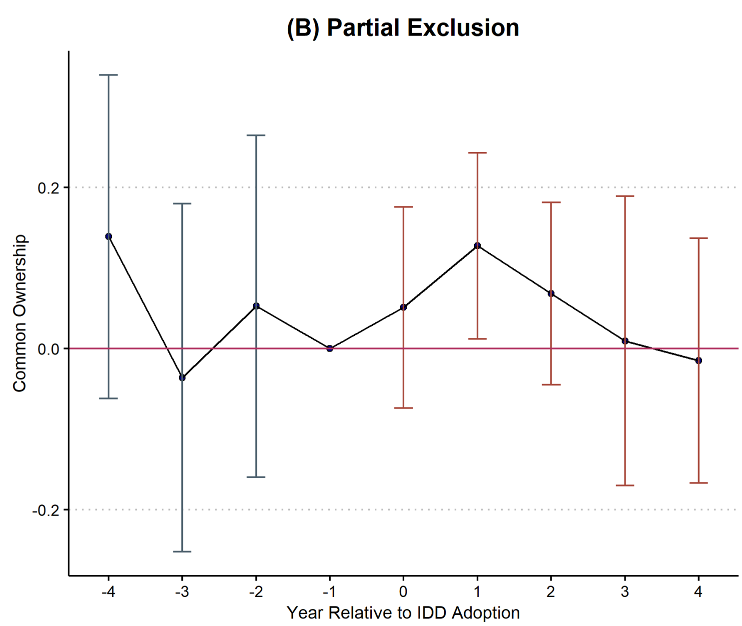


Table A.3.1: Legal cases on the Inevitable Disclosure Doctrine

This table lists all the legal cases leading to the adoption/rejection of the Inevitable Disclosure Doctrine in the U.S. states.

State	Year	Attitude	Case
AR	1997	adopt	Southwestern Energy Co. v. Eickenhorst, 955 F. Supp. 1078 (W.D. Ark. 1997)
CT	1996	adopt	Branson Ultrasonics Corp. v. Stratman, 921 F. Supp. 909 (D. Conn. 1996)
DE	1964	adopt	E.I. duPont de Nemours & Co. v. American Potash & Chem. Corp., 200 A.2d 428 (Del. Ch. 1964)
FL	1960	adopt	Fountain v. Hudson Cush-N-Foam Corp., 122 So. 2d 232 (Fla. Dist. Ct. App. 1960)
	2001	reject	Del Monte Fresh Produce Co. v. Dole Food Co. Inc., 148 F. Supp. 2d 1326 (S.D. Fla. 2001)
GA	1998	adopt	Essex Group Inc. v. Southwire Co., 501 S.E.2d 501 (Ga. 1998)
	2013	reject	Holton v. Physician Oncology Services, LP (GA. 2013)
IL	1995	adopt	PepsiCo, Inc. v. Redmond, 54 F.3d 1262, 1272 (7th Cir. 1995)
IN	1995	adopt	Ackerman v. Kimball Int'l Inc., 652 N.E.2d 507 (Ind. 1995)
IA	1996	adopt	Uncle B's Bakery v. O'Rourke, 920 F. Supp. 1405 (N.D. Iowa 1996)
KS	2006	adopt	Bradbury Co. v. Teissier-duCros, 413 F. Supp. 2d 1203 (D. Kan. 2006)
MA	1994	adopt	Bard v. Intoccia, 1994 U.S. Dist. LEXIS 15368 (D. Mass. 1994)
MD	2004	reject	LeJeune v. Coin Acceptors, Inc., 849 A.2d 451, 471 (Md. 2004)
MI	1966	adopt	Allis-Chalmers Manuf. Co. v. Continental Aviation & Eng. Corp., 255 F. Supp. 645 (E.D. Mich. 1966)
	2002	reject	CMI Int'l, Inc. v. Intermet Int'l Corp., 649 N.W.2d 808 (Mich. Ct. App. 2002)
MN	1986	adopt	Surgidev Corp. v. Eye Technology Inc., 648 F. Supp. 661 (D. Minn. 1986)
MO	2000	adopt	H&R Block Eastern Tax Servs. Inc. v. Enchura, 122 F. Supp. 2d 1067 (W.D. Mo. 2000)
NJ	1987	adopt	Nat'l Starch & Chem. Corp. v. Parker Chem. Corp., 530 A.2d 31 (N.J. Super. Ct. 1987)
NY	1919	adopt	Eastman Kodak Co. v. Powers Film Prod., 189 A.D. 556 (N.Y.A.D. 1919)
	2011	reject	International Business Machines Corp. v. Visentin,?2011 WL 672025 (S.D.N.Y. Feb. 16, 2011)
NC	1996	adopt	Merck & Co. v. Lyon, 941 F. Supp. 1443 (M.D.N.C. 1996)
OH	2000	adopt	Procter & Gamble Co. v. Stoneham, 747 N.E.2d 268 (Ohio Ct. App. 2000)
PA	1982	adopt	Air Products & Chemical Inc. v. Johnson, 442 A.2d 1114 (Pa. Super. Ct. 1982)
TX	1993	adopt	Rugen v. Interactive Business Systems Inc., 864 S.W.2d 548 (Tex. App. 1993)
	2003	reject	Cardinal Health Staffing Network Inc. v. Bowen, 106 S.W.3d 230 (Tex. App. 2003)
UT	1998	adopt	Novell Inc. v. Timpanogos Research Group Inc., 46 U.S.P.Q.2d 1197 (Utah D.C. 1998)
VA	1999	reject	Government TechnologyServices, Inc. v. IntellisysTechnology Corp., 51 Va. Cir.55 (Va. Cir. Ct. Oct. 20, 1999)
WA	1997	adopt	Solutec Corp. Inc. v. Agnew, 88 Wash. App. 1067 (Wash. Ct. App. 1997)

Table A.3.2: M&As between financial institutions

This table shows all the M&A deals used as identifications in this paper.

Date Announced	Date Effective	Acquiror Name	Target Name	Acquirer Mgrno	Target Mgrno
30/08/1996	06/01/1997	NationsBank Corp,Charlotte,NC	Boatmen's Bancshares,St Louis	62890	9480
20/01/1997	20/05/1997	Mellon Bank Corp,Pittsburgh,PA	Ganz Capital Management Inc	55390	39800
09/06/1997	01/10/1997	BankAmerica Corp	Robertson Stephens & Co	5980	74535
21/01/1997	05/11/1997	PIMCO Advisors LP	Oppenheimer Capital LP	70470	67463
05/11/1997	01/12/1997	PIMCO Advisors LP	Oppenheimer Capital LP	70470	67463
11/12/1997	01/04/1998	Mellon Bank Corp,Pittsburgh,PA	Founders Asset Management Inc	55390	38870
13/04/1998	30/09/1998	NationsBank Corp,Charlotte,NC	BankAmerica Corp	62890	5980
06/04/1998	08/10/1998	Travelers Group Inc	Citicorp	84900	16260
20/07/1998	31/12/1998	SunTrust Banks Inc,Atlanta,GA	Crestar Finl Corp,Richmond,VA	82355	21650
15/02/1999	06/07/1999	Credit Suisse Asset Management	Warburg Pincus Asset Mgmt	5720	91450
30/04/1999	20/09/1999	Firstar Corp,Milwaukee,WI	Mercantile Bancorp,St Louis,MO	38230	55510
14/03/1999	01/10/1999	Fleet Financial Group Inc,MA	BankBoston Corp,Boston,MA	38260	6000
20/06/2000	02/10/2000	Alliance Capital Mgmt Hldg LP	Sanford C Bernstein & Co Inc	25610	8650
13/09/2000	31/12/2000	Chase Manhattan Corp,NY	JP Morgan & Co Inc	15345	58835
18/10/2000	14/02/2001	Allianz AG	Nicholas-Applegate Capt Mgmt	1275	64240
25/10/2000	10/04/2001	Franklin Templeton Investments	Fiduciary Trust Co Intl	39300	28060
16/04/2001	04/09/2001	First Union Corp,Charlotte,NC	Wachovia Corp,Winston-Salem,NC	37700	91000
14/04/2003	30/04/2003	Goldman Sachs Group Inc	Ayco Co LP	41260	5500
27/10/2003	01/04/2004	Bank of America Corp	FleetBoston Financial Corp,MA	62890	38260
14/01/2004	01/07/2004	JPMorgan Chase & Co	Bank One Corp,Chicago,IL	58835	5955
26/05/2004	03/01/2005	Wells Fargo & Co	Strong Financial-Fund Asts	65850	82100
19/05/2005	04/08/2005	Transamerica Investment Mgmt	Westcap Investors LLC	84750	92160
31/10/2006	04/12/2006	Morgan Stanley	FrontPoint Partners LLC	58950	7759
16/09/2008	22/09/2008	Barclays PLC	Lehman-Invest Bkg Bus	7900	50200
23/04/2008	30/09/2008	Lehman Brothers Holdings Inc	David J Greene & Co LLC	50200	42120
07/07/2008	07/11/2008	RiverSource Investments LLC	J&W Seligman & Co	45639	78400
03/10/2008	31/12/2008	Wells Fargo & Co	Wachovia Corp,Charlotte,NC	65850	37700
14/09/2008	01/01/2009	Bank of America Corp	Merrill Lynch & Co Inc	62890	56780
16/09/2009	01/12/2009	Blackrock Inc	Barclays Global Fund Advisors	9385	7900
06/04/2010	06/04/2010	Goldman Sachs Group Inc	Level Global Investors LP	41260	10194

Table A.3.3: The impact of IDD on firm valuation conditional on intangible assets

This table reports the results on the effect of IDD on firms' valuation conditional on intangible assets. The dependent variables are *Tobin's Q* and *ROA*, respectively. The indicator variable *High Patent* in columns (1)-(2) takes the value one if a firm's number of patents is above the sample median, and zero otherwise. Similarly, *High Citation* in columns (3)-(4) indicates whether a firm's number of citations is above the sample median. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Tobin's Q		*ROA*	
	(1)	(2)	(3)	(4)
IDD	-0.125*	-0.115*	-0.001	-0.001
	(0.070)	(0.065)	(0.002)	(0.002)
IDD*High Patent	0.201**		0.006*	
	(0.092)		(0.003)	
IDD*High Citation		0.177**		0.006*
		(0.084)		(0.003)
Observations	54104	54104	54104	54104
R^2	0.623	0.623	0.909	0.909
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table A.3.4: Valuation effect of increased common ownership - preliminary test

This table examines whether increased common ownership has a positive impact on firm innovation following the IDD adoption. We conduct a cross-sectional regression exploiting the interaction term of *Common Ownership* and the *IDD* indicator, where *IDD* takes the value of one if the IDD is recognized in a state, and zero otherwise. We use a firm's number of patents, number of citations, as well as its innovation productivity to measure its innovation outcomes. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Ln(1+Patent)	*Ln(1+Citation)*	*Inno_Prod*
	(1)	(2)	(3)
Common Ownership	-3.914	-12.344***	-3.594**
	(3.532)	(4.597)	(1.696)
IDD	-0.026*	-0.054*	-0.035**
	(0.015)	(0.032)	(0.017)
IDD*Common Ownership	1.524	6.954	3.144*
	(4.408)	(6.385)	(1.645)
Observations	47987	47987	48037
R^2	0.895	0.856	0.583
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes

Table A.3.5: Valuation effect of increased common ownership - state GDP growth

This table examines whether increased common ownership has a positive impact on state-level GDP growth following the IDD adoption. We conduct a cross-sectional regression exploiting the interaction term of *State CO* and the *IDD* indicator, where *IDD* takes the value of one if the IDD is recognized in a state, and zero otherwise. We use a state's GDP growth in a given year as the outcome variable. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	State-Level GDP Growth	
	(1)	(2)
IDD	-0.670	-0.588*
	(0.430)	(0.346)
State CO	0.030	0.061
	(0.108)	(0.108)
State CO*IDD	0.091	0.136*
	(0.089)	(0.077)
Unemployment Rate		-0.463***
		(0.089)
Establishment Entry Rate		0.701***
		(0.149)
Establishment Exit Rate		-0.992***
		(0.143)
CNC Index		-0.481***
		(0.076)
Observations	1077	1077
R^2	0.467	0.546
State FE	Yes	Yes
Year FE	Yes	Yes

Table A.3.6: Effect of IDD on common ownership: small firms

This table reports the results on the effect of IDD on firm-level common ownership, conditional on the firm size. The dependent variables are *Common Ownership*, which measures firm-level degree of common ownership. We interact firm size with the IDD adoption indicator, where we use total assets, number of employees, and sales to measure firm size respectively. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership		
	(1)	(2)	(3)
IDD	1.230***	0.290***	1.084***
	(0.336)	(0.084)	(0.309)
IDD*Size	-0.168***		
	(0.055)		
IDD*log(Employees)		-0.119***	
		(0.039)	
IDD*Sales			-0.147***
			(0.050)
Observations	54172	53188	53489
R^2	0.783	0.783	0.784
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes

Table A.3.7: Effect of IDD on common ownership: Alternative sample

This table reports the results on the effect of IDD on firm-level common ownership using alternative samples. The dependent variables are *Common Ownership*, which measures firm-level degree of common ownership. In column 1, the common ownership measure is constructed using the ownership data from FactSet instead of Thomson Reuters 13F. As the ownership data from FactSet mainly begins in 1999, we omit the states that recognized the Doctrine before 1999 and never reversed the decision during our sample period. Column 3 uses the ownership data from Thomson Reuters 13F but omits the holdings by the *Big Three*. Variable definitions are provided in the appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Industry fixed effects are defined at the 3-digit SIC level. Standard errors in parentheses are clustered by state. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Common Ownership		
	FactSet	FactSet drop 1999	No Big Three
	(1)	(2)	(3)
IDD	0.165**	0.188**	0.073**
	(0.073)	(0.076)	(0.035)
Observations	42528	27900	54216
R^2	0.744	0.757	0.706
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes

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