

Three Essays in Empirical Corporate Finance

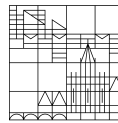
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Contents

Summary	1
Zusammenfassung	3
1 Empirical Evidence on Debt Governance	5
1.1 Introduction	6
1.1.1 Related literature	10
1.2 Data and summary statistics	13
1.2.1 Data	13
1.2.1.1 Data on board independence	14
1.2.1.2 Data on bankruptcy penalties	15
1.2.1.3 Control variables	15
1.2.2 Summary statistics	17
1.3 Cross-country study	19
1.3.1 Average relation	19
1.3.2 Difference in default probability	22
1.3.3 Difference in default probability and causal inference	24
1.4 Changes in board independence around bankruptcy reforms	25
1.4.1 Germany's bankruptcy reform of 2012	26
1.4.2 The 2005–2006 bankruptcy reforms of Italy	30
1.4.3 US Bankruptcy Reform Act of 1978	32
1.4.4 Changes in board structure of firms in other countries	34
1.5 Alternative explanations and additional discussions	35
1.5.1 Alternative explanations	35
1.5.1.1 Recovery rates of creditors	35
1.5.1.2 Changes in other firm characteristics	37
1.5.2 Additional discussions	38
1.5.2.1 Governance role of bankruptcy penalties at earlier levels	38
1.5.2.2 Relevance of bankruptcy penalties in the presence of covenants	39
1.5.2.3 Endogeneity of corporate governance structure	40
1.5.2.4 Debt governance and firm value as an outcome variable	41
1.6 Conclusions	42
Tables and figures for Chapter 1	44
Appendix for Chapter 1	59
2 Dual-Class Firms, Agency Problems, and Debt Governance: Evidence from Korea	70
2.1 Introduction	71
2.2 Dual-class firms, debt governance, and Korea's bankruptcy reform	77
2.2.1 Dual-class firms	77
2.2.2 Debt governance	78
2.2.3 Korea's bankruptcy reform	79
2.2.3.1 Changes to bankruptcy law	79
2.2.3.2 History of the reform	80
2.3 The voting premium as a measure of agency problems	81
2.3.1 Theoretical and empirical background	81
2.3.2 Potential problems of the voting premium	83
2.3.3 The voting premium and takeover regulations in Korea	84
2.3.4 Debt governance and firm value as a measure of agency problems	85

2.4 Data, summary statistics, and empirical method.....	86
2.4.1 Data	86
2.4.2 Summary statistics	87
2.4.3 Empirical method	88
2.5 Results	89
2.5.1 Main results	89
2.5.2 Parallel trends.....	90
2.5.3 Heterogeneity	91
2.6 Confounding factors.....	92
2.6.1 The 2007–2009 financial crisis	93
2.6.2 Changes in the voting premium of firms in other countries.....	93
2.6.3 Cross-country analysis and different measures of agency problems	95
2.7 Conclusions	98
Figures and tables for Chapter 2	100
Appendix for Chapter 2.....	111
3 Creditor Rights, Corporate Leverage and Investments, and the Firm Type	114
3.1 Introduction	115
3.2 Germany’s bankruptcy law reform of 2012	121
3.2.1 Prereform bankruptcy law	121
3.2.2 The ESUG Act.....	122
3.2.3 Brief history of the bankruptcy law reform.....	125
3.3 Data and empirical strategy	126
3.3.1 Data	126
3.3.2 Empirical strategy	126
3.3.3 Summary statistics and preliminary results on the average effect	129
3.4 Average effect on leverage and investments	130
3.4.1 Corporate leverage	130
3.4.2 Corporate investments.....	132
3.5 Heterogeneity in the firm type	133
3.5.1 Heterogeneity in size, profitability, and maturity.....	134
3.5.1.1 Additional analysis: heterogeneity in firm size alone	136
3.5.2 Implications for bankruptcy literature.....	137
3.6 Remeasuring the effect by variation in default risk	138
3.7 Conclusions	140
Tables and figures for Chapter 3	142
Appendix for Chapter 3.....	154
Eigenabgrenzung	155
References	156

Summary

This thesis consists of three chapters on empirical corporate finance. The first chapter is titled “Empirical Evidence on Debt Governance” and is single-authored work. A large theoretical literature suggests that debt can mitigate agency problems, yet empirical evidence is limited due to identification problems. To provide evidence, I examine whether firms implement independent directors as a substitute when debt governance becomes ineffective. Key to my analysis are two central features of debt as a governance device: first, it is only effective with bankruptcy penalties, second, it only matters for firms close to default. Consistent with debt inducing discipline, across countries, board independence is negatively related to bankruptcy penalties, and especially so among risky firms. For them, a change from strictest to softest penalties is associated with a 46% increase in the number of independent directors. Comparing changes in board independence of risky firms to those of safe firms around bankruptcy reforms confirms the cross-country results. My findings underline the role of agency problems in explaining corporate capital structure, and can further account for several unresolved issues in corporate governance (e.g., differences in bankruptcy penalties can explain 37% of the 37 percentage point “board independence gap” between the US and the UK). Moreover, my findings have important implications for optimal bankruptcy design: in the absence of bankruptcy penalties, management may have difficulties to credibly commit to forgo inefficient actions and thus to secure financing.

The second chapter is titled “Dual-Class Firms, Agency Problems, and Debt Governance: Evidence from Korea” and is single-authored work. I examine whether debt (or, more generally, a performance threshold) can reduce agency problems between controlling shareholders and minority shareholders in dual-class firms. To do so, I use two key features of debt governance. First, it is only effective with bankruptcy penalties, second, it only matters for firms close to default. I measure agency problems by the voting premium. Using a Korean bankruptcy reform that reduced penalties, I find that risky firms exhibited a 59% increase in the voting premium after the reform relative to safe firms. My findings have important implications for the heated debate on dual-class share structure.

The third chapter is titled “Creditor Rights, Corporate Leverage and Investments, and the Firm Type” and is joint work with Axel Kind, Jiri Tressl, and Sabine Wende. Stronger creditor rights reduce credit costs and thus may allow firms to increase leverage and investments. Yet, they also increase distress costs and thus may prompt firms to lower leverage and undertake risk-reducing but unprofitable investments. Using a German bankruptcy reform, on average,

we find evidence consistent with the latter hypothesis. We also hypothesize and find evidence that the effect of creditor rights on corporate leverage and investments depends on the firm type (particularly, firm size), as it influences the effect that creditor rights have on credit costs and distress costs and thus which effect dominates. Our understanding reconciles mixed empirical evidence and has important implications for optimal bankruptcy design. In particular, it points to a menu of procedures in which a debtor-friendly and creditor-friendly procedure co-exist and thus allow different types of firms to utilize the procedure that suits them best.

Zusammenfassung

Diese Arbeit besteht aus drei Kapiteln über empirische Unternehmensfinanzierung. Das erste Kapitel hat den Titel „Empirical Evidence on Debt Governance“ und ist eigene Arbeit. Eine umfangreiche theoretische Literatur legt nahe, dass Schulden Agency-Probleme mildern können, empirische Beweise dazu sind jedoch begrenzt aufgrund von Identifizierungsproblemen. Um Beweise anzubieten, untersuche ich, ob Unternehmen unabhängige Direktoren als Ersatz implementieren, wenn Governance durch Schulden unwirksam wird. Der Schlüssel zu meiner Analyse sind zwei zentrale Merkmale von Schulden als Governance-Instrument: Erstens, ist es nur wirksam mit Insolvenzstrafen, zweitens, ist es nur von Bedeutung für Unternehmen mit hoher Ausfallwahrscheinlichkeit. Im Einklang mit der Hypothese, dass Schulden disziplinieren, steht über Länder hinweg die Unabhängigkeit des Aufsichtsrates in einem negativen Zusammenhang mit Insolvenzstrafen, und zwar insbesondere bei Unternehmen mit hoher Ausfallwahrscheinlichkeit. Für diese Unternehmen, ist ein Wechsel von den strengsten zu den mildesten Strafen verbunden mit einem Anstieg der Anzahl unabhängiger Direktoren um 46%. Ein Vergleich von Veränderungen der Unabhängigkeit des Aufsichtsrates von Unternehmen mit hoher Ausfallwahrscheinlichkeit mit denen von Unternehmen mit geringer Ausfallwahrscheinlichkeit rund um Insolvenzreformen bestätigt die länderübergreifenden Resultate. Meine Ergebnisse unterstreichen die Rolle von Agency-Problemen bei der Erklärung der Kapitalstruktur von Unternehmen, und können zudem mehrere ungelöste Fragen in Corporate Governance erklären (z. B. können Unterschiede bei Insolvenzstrafen 37% des 37 Prozentpunkte Unterschieds bezüglich der Unabhängigkeit des Aufsichtsrates zwischen den USA und Großbritannien erklären). Darüber hinaus haben meine Ergebnisse wichtige Implikationen für die optimale Gestaltung des Insolvenzrechts: Ohne Insolvenzstrafen, kann das Management Schwierigkeiten haben sich glaubhaft zu verpflichten auf ineffiziente Handlungen zu verzichten und daher Schwierigkeiten haben die Finanzierung zu sichern.

Das zweite Kapitel hat den Titel „Dual-Class Firms, Agency Problems, and Debt Governance: Evidence from Korea“ und ist eigene Arbeit. Ich untersuche, ob Schulden (oder, allgemeiner, eine Leistungsschwelle) Agency-Probleme zwischen kontrollierenden Aktionären und Minderheitsaktionären in Unternehmen die Aktien mit unterschiedlichen Stimmrechten haben reduzieren können. Zu diesem Zweck, nutze ich zwei wesentliche Merkmale von Schulden als Governance-Instrument. Erstens, ist es nur wirksam mit Insolvenzstrafen, zweitens, ist es nur von Bedeutung für Unternehmen mit hoher Ausfallwahrscheinlichkeit. Ich messe Agency-Probleme durch die Stimmrechtsprämie. Anhand einer koreanischen Insolvenzreform, die

Strafen reduzierte, stelle ich fest, dass Unternehmen mit hoher Ausfallwahrscheinlichkeit nach der Reform einen Anstieg der Stimmrechtsprämie um 59% aufwiesen im Vergleich zu Unternehmen mit geringer Ausfallwahrscheinlichkeit. Meine Ergebnisse haben wichtige Implikationen für die hitzige Debatte über die Aktienstruktur mit unterschiedlichen Stimmrechten.

Das dritte Kapitel hat den Titel „Creditor Rights, Corporate Leverage and Investments, and the Firm Type“ und ist gemeinsame Arbeit mit Axel Kind, Jiri Tressl, und Sabine Wende. Stärkere Gläubigerrechte reduzieren Kreditkosten und können daher Unternehmen erlauben, den Verschuldungsgrad und die Investitionen zu erhöhen. Sie erhöhen jedoch auch Insolvenzkosten und können daher Unternehmen dazu veranlassen, den Verschuldungsgrad zu verringern und risikoreduzierende aber unrentable Investitionen zu tätigen. Anhand einer deutschen Insolvenzreform finden wir Beweise, die im Durchschnitt mit der letztgenannten Hypothese übereinstimmen. Wir stellen auch die Hypothese auf und finden Beweise dafür, dass der Effekt von Gläubigerrechten auf den Verschuldungsgrad und die Investitionen von der Unternehmensart (insbesondere der Unternehmensgröße) abhängt, da dieser den Effekt von Gläubigerrechten auf Kreditkosten und Insolvenzkosten beeinflusst und daher beeinflusst welcher Effekt überwiegt. Unser Verständnis bringt uneinheitliche empirische Beweise in Einklang und hat wichtige Implikationen für die optimale Gestaltung des Insolvenzrechts. Insbesondere deutet es auf ein Menü von Verfahren hin, in dem ein schuldnerfreundliches und gläubigerfreundliches Verfahren nebeneinander bestehen und es daher unterschiedlichen Arten von Unternehmen erlauben, das Verfahren zu nutzen, das am besten für sie geeignet ist.

Chapter 1

Empirical Evidence on Debt Governance*

* I thank Axel Kind, Edward Morrison, and Jiri Tressl.

“for debt to be an effective source of discipline it must be backed by an appropriate bankruptcy (or insolvency) procedure, i.e. there must be an appropriate ‘penalty’ in the event of default. A bankruptcy mechanism that is ‘soft’ on management – e.g. one that, like Chapter 11, keeps creditors at bay for a long period – may have the undesirable property that it reduces management’s incentive to avoid default, thus undermining the bonding or disciplinary role of debt.”

—Oliver Hart (1995, p. 685)

1.1 Introduction

A large theoretical literature gives debt a fundamental role in corporate governance. In particular, it is argued that debt gives creditors control and liquidation rights when management fails to make a promised payment. This threat to its private benefits of control encourages management to reduce the probability of bankruptcy by pursuing high profits rather than less profitable personal goals. As such, debt can be a disciplinary source that allows investors (both shareholders and creditors) to restrict adverse managerial actions, and can further be a bonding device that allows management to commit to a sufficient level of efficiency in order to ensure corporate financing.¹

The notion that debt can alleviate agency problems has a number of important implications. First, it suggests that agency problems might play a key role in determining corporate capital structure (e.g., Grossman and Hart, 1982; Zwiebel, 1996). Second, it helps explaining several unresolved issues in corporate governance. For example, debt governance can explain the substantial cross-sectional variation in corporate governance structure, given that firms typically substitute between alternative governance mechanisms. Likewise, debt governance can be useful in understanding mixed empirical evidence on the effectivity of other governance mechanisms, as improved governance may be beneficial for firms with weak debt governance but has no effect for firms with strong debt governance.² Third, and probably most important, the possible ability of debt to reduce agency problems has important implications for optimal bankruptcy design. As detailed below, debt governance is only effective with bankruptcy penalties. This suggests that the absence of bankruptcy penalties may prevent firms from good governance and thus from obtaining financing (Aghion, Hart, and Moore, 1992; Hart, 2000).

¹ On the role of debt in constraining management from diverting funds, see Grossman and Hart (1982); in inducing managerial effort, see Dewatripont and Tirole (1994); and in curbing managerial empire building, see Zwiebel (1996). For surveys and studies providing discussions on debt as a disciplinary and bonding mechanism, see Aghion, Hart, and Moore (1992), Hart (1995, 2000, 2001, 2017), Shleifer and Vishny (1997), and Benmelech and Bergman (2008).

² For the argument that improvements in corporate governance may have a different effect for firms with weak and strong governance, see Bertrand and Mullainathan (2003), Giroud and Mueller (2010, 2011), and Nini, Smith, and Sufi (2012).

Despite the large volume of theories suggesting that debt induces discipline and the importance of this hypothesis, evidence on the empirical relevance of debt governance is limited due to identification problems (summarized further below). In this paper, I attempt to provide evidence. To do so, I examine whether firms implement independent directors as a substitute governance mechanism when debt governance becomes ineffective.³ Key to my analysis are two central features of debt as a governance device. First, debt governance is only effective with bankruptcy penalties, given that the absence of penalties undermines management's incentive to avoid bankruptcy, and, hence, reduces the incentive effect of debt to pursue high profits (Grossman and Hart, 1982; Aghion, Hart, and Moore, 1992; Hart, 1995, 2000). Second, debt governance only matters for firms close to bankruptcy, given that proximity to default ensures bankruptcy when performance is poor, and, therefore, increases the incentive to be efficient in order to avoid bankruptcy (Zwiebel, 1996; Hart, 2001).⁴ Building on these features, the central test of my study is whether firms implement independent directors as a substitute when bankruptcy legislations that are soft on management make debt governance ineffective, and whether this effect is stronger for firms with high default probability.

My empirical analysis is based on a sample of 11,965 firms from 50 countries. I measure managerial bankruptcy penalties using the creditor rights index of La Porta et al. (1998), which contains bankruptcy costs to managerial private benefits of control by including data regarding management dismissal during bankruptcy, and regarding the likelihood of a firm liquidation as the outcome of the bankruptcy process (mainly determined by provisions that cover the ability of creditors to grab assets, and the inability of management to seek firm reorganization without creditors' consent).

To begin, I study the average relation between board independence and bankruptcy penalties. Consistent with the hypothesis that debt mitigates agency problems, I find a negative relation between board independence and managerial bankruptcy penalties. The effect is sizable. A change from strictest to softest penalties is associated with a 34% increase in the number of independent directors (or 18 percentage point increase in the fraction of independent directors

³ In studying the interaction between alternative governance mechanisms, my study follows the notion that governance mechanisms are endogenously determined within the broader system of a firm's governance structure. That is, firms substitute between different governance mechanisms in choosing an optimal governance structure in which the demand for one mechanism depends on the strength of alternative mechanisms (Demsetz and Lehn, 1985; Hermalin and Weisbach, 2003, 2012; Adams, Hermalin, and Weisbach, 2010; Avedian, Cronqvist, and Weidenmier, 2015; Bharath and Hertz, 2019). See Section 1.5.2.3 for a more detailed discussion on this. Additionally, see Section 1.5.2.4 for why it is not meaningful to evaluate the governance role of debt by studying the relation between debt governance and firm value (as a proxy for agency problems).

⁴ Note that proximity to default increases the likelihood of bankruptcy in case of low performance not only because it increases the likelihood that profits are lower than debt dues, but also because for firms with high default probability such situations lead to actual default states, i.e., firms cannot pay off current debt dues by issuing more debt in order to avert default (Hart and Moore, 1995; Zwiebel, 1996; Hart, 2001).

to board size), suggesting that debt is viewed as a powerful tool for disciplining management. Notably, as the effect between board independence and bankruptcy penalties is of great economic significance, an important by-product of my findings is that they can explain large parts of the substantial cross-sectional variation in corporate board structure that is left unexplained by previously found determinants of board independence. In this sense, differences in bankruptcy penalties can, e.g., explain about 37% of the 37 percentage point difference in the average fraction of independents to board size between the US and the UK.

Next, I examine whether the negative relation between board independence and bankruptcy penalties differs for firms with high and low default probability. Further supporting the hypothesis that debt mitigates agency problems, I find that the negative relation is stronger among firms with high default probability relative to firms with low default probability. In fact, the difference in the negative relation increases monotonically by comparing more risky firms with more safe firms. Economically, for firms with high default probability, a change from strictest to softest bankruptcy penalties is associated with an increase in the number of independent directors by 46%, or 18% relative to firms with low default probability.

The difference in the negative relation across firms with high and low default probability not only provides further support on the empirical relevance of debt governance, but also improves the causal interpretation of my findings. A potential concern when looking at the average relation between board structure and bankruptcy penalties is that bankruptcy law is endogenous to board independence, i.e., that for most countries an omitted country characteristic simultaneously determines both variables such that bankruptcy penalties do not cause board dynamics. Looking at the difference in the negative relation across firms with high and low default probability mitigates this concern by reducing the identification problem to one in which for most countries an omitted country attribute not only determines bankruptcy law and board structure, but also affects board structure differently for firms with high and low default probability. Note that endogeneity problems are unlikely to be the case. Across countries, bankruptcy law is typically determined by a different reason—e.g., the 2007–2009 financial crisis for Germany; a corporate scandal for Italy; the Asian financial crisis for South Korea; an attempt to promote lending via weaker creditor rights for Brazil, and via stronger creditor rights for India; and the political power of competing interest groups for the US.⁵ Additionally, these reasons should not

⁵ The findings by Djankov, McLiesh, and Shleifer (2007) regarding the stability of creditor rights scores over time (the correlation between the 1978 and 2003 creditor rights indices is 0.95), the absence of convergence in bankruptcy law across countries (differences persist over the 25-year period), and the difficulty to find determinants of creditor rights (80% of the cross-sectional variation in creditor rights remains unexplained by important country characteristics, including legal origin, the level of economic development, and culture) are broadly

affect board structure, or at least not differently across firms with high and low default probability.

To further improve the causal interpretation of my results, I study changes in board independence around three bankruptcy reforms.⁶ The objective is to support the cross-country results by focusing on cases for which the determinant of the bankruptcy law is known, and should not be correlated with corporate board structure. The first reform is Germany's reform of 2012 that, prompted by the 2007–2009 financial crisis, was aimed at facilitating the restructuring of distressed firms (Canipek, Kind, and Wende, 2021). The reform reduced bankruptcy penalties by introducing a debtor-in-possession proceeding (accessible without creditors' consent) that allows management to stay in corporate control during bankruptcy and moreover imposes automatic stay on the assets. The other two reforms are the 2005–2006 reforms of Italy, prompted by a corporate scandal (Rodano, Serrano-Velarde, and Tarantino, 2016; see also Favara et al., 2017). The 2005 reform, designed to facilitate firm continuation to rescue the firm, reduced bankruptcy penalties by removing strict restrictions for a debtor to enter a reorganization procedure. Afterwards, the 2006 reform increased penalties by making several changes to speed up liquidation procedures (which, through higher expected recovery rates, increases creditors' incentives to press for liquidation).

For each of the three reforms, I compare changes in board independence of firms with high default probability around the reform to those of firms with low default probability. Consistent with the cross-country results, I find that firms with high default probability increased (decreased) the number of independent directors after a decrease (increase) in bankruptcy penalties relative to firms with low default probability. Also the economic effects are similar to those of the cross-country analysis.

A number of additional results further support my findings. First, I get (qualitatively) similar results when (i) using, instead of the number of independent directors, the ratio of independents to board size as a measure of the governance intensity by the board, (ii) using, instead of Altman's Z-score, the interest coverage ratio as a measure of default probability, and (iii) excluding US and UK firms from the analysis, which account for about 60% of the sample observations. Second, I find that the negative relation between board independence and bankruptcy penalties is strongest for individual creditor rights components that typically correspond

consistent with the view that, for each country, bankruptcy law is typically determined by a different reason, and not a single reason that applies across countries.

⁶ The empirical approach is similar to that used by Favara et al. (2017), who confirm their cross-country results regarding the effect of bankruptcy law design on investments and risk taking by studying changes in these variables of firms with high default probability relative to those of firms with low default probability around bankruptcy reforms.

to strong bankruptcy penalties (such as management dismissal during bankruptcy, the inability of management to seek reorganization unilaterally, and the ability of creditors to grab assets) and weakest for the component that typically corresponds to weak penalties (i.e., the right of creditors to be paid out first).

Third, a potential concern with my results from the bankruptcy reforms in Germany and Italy is that confounding factors affected board structure of high and low default probability firms differently. I find that board independence of high and low default probability firms followed parallel trends prior to the reforms, suggesting that board independence of both groups of firms generally experience similar shocks. Additionally, I find that in other countries firms with high default probability did not change board independence relative to firms with low default probability after the reforms in Germany and Italy, suggesting that the observed changes in Germany and Italy are not driven by confounding events that were common to all countries. These results, together with the fact that board structure is sticky, i.e., that board structure is determined by static factors that typically do not change over time, make it unlikely that confounding factors affect my results. Fourth, using detailed summary statistics on board structure of US firms based on hand-collected data from Graham, Kim, and Leary (2020), I provide event-study evidence on the 1979 US bankruptcy reform that reduced bankruptcy costs (Hackbarth, Haselmann, and Schoenherr, 2015; Favara et al., 2017). While board independence was constant at about 50% for four decades beginning in the 1930s, it increased to about 60% from the late 1970s to mid-1990s. Finally, I find that my results are unlikely to be driven by alternative explanations, and provide several discussions that may be helpful in gaining further insights into the findings of this paper.

1.1.1 Related literature

My study contributes to the financial contracting literature. A central hypothesis of this literature is that debt can mitigate agency problems. I contribute by providing empirical evidence on this hypothesis. Despite the large number of financial contracting theories and the importance of this hypothesis, there is little evidence on the empirical relevance of debt governance. Existing evidence comes from studies that find a negative relation between managerial entrenchment and debt and suggest that this is consistent with entrenched managers avoiding the disciplinary role of debt (Berger, Ofek, and Yermack, 1997; Garvey and Hanka, 1999), or from studies that find a positive relation between debt and firm value and suggest that this is consistent with debt is valued as a governance device (e.g., Jung, Kim, and Stulz, 1996; Harvey, Lins, and Roper, 2004). However, these papers examine the governance role of debt in general,

that is, the incentive effect of debt that comes from various mechanisms, including, besides the threat of managerial bankruptcy penalties, also forcing management to disgorge free cash flow (e.g., Jensen, 1986), increased monitoring by creditors (Harvey, Lins, and Roper, 2004), or the threat of managerial restrictions following covenant violation (see below). Additionally, given their dependence on cross-sectional or event-study analyses, these studies are restricted by the potential endogeneities in the relations between debt and the other variables, making it difficult to draw reliable conclusions.⁷⁸ Different than these studies, I exploit two key features of debt as a governance device that allow me to provide direct and credible empirical evidence on the incentive effect of debt that comes through bankruptcy penalties.

My paper also contributes to the literature on optimal bankruptcy design. In particular, my findings in support of debt as an effective source of discipline provide empirical evidence consistent with the notion that “a good bankruptcy procedure is one that (...) preserves the (ex ante) bonding role of debt by penalizing management adequately in bankruptcy states.” (Aghion, Hart, and Moore, 1992, pp. 532–533). Without bankruptcy penalties, management may have difficulties to credibly commit to refrain from inefficient actions and thus to secure financing ex ante (Aghion, Hart, and Moore, 1992; Hart, 2000). To the best of my knowledge, this aspect is not considered by the current bankruptcy literature, which almost exclusively focuses on how firm financing might be affected by bankruptcy law through collateral value and distress costs.⁹ It is important to note that while my findings are important in general, they may

⁷ For the little empirical evidence on the governance role of debt (in general) and the different mechanisms through which debt can induce discipline, see also Shleifer and Vishny (1997) and Dey, Nikolaev, and Wang (2016). For the endogeneity problems regarding the empirical evidence on debt governance and other corporate governance mechanisms, see Berger, Ofek, and Yermack (1997), Garvey and Hanka (1999), Hermalin and Weisbach (2003), Harvey, Lins, and Roper (2004), Adams, Hermalin, and Weisbach (2010), John and Litov (2010), and Wintoki, Linck, and Netter (2012).

⁸ Notably, revisiting the analyses of Berger, Ofek, and Yermack (1997) and Garvey and Hanka (1999) with a larger sample and more robust econometric methods, John and Litov (2010) find that the relationship between managerial entrenchment and leverage is actually positive, and not negative, consistent with their hypothesis that entrenched managers receive better access to debt financing as a response to a more conservative investment policy that is aligned with creditors’ interests. For a detailed discussion on the conflicting “managerial entrenchment” and “creditor alignment” hypotheses, see Ji, Mauer, and Zhang (2020), who also provide a brief review of the mixed empirical evidence in the literature on the entrenchment-leverage relation.

⁹ The current bankruptcy literature almost exclusively focuses on two channels when asking whether stronger creditor rights facilitate or hamper corporate financing (Acharya, Amihud, and Litov, 2011; Vig, 2013; Rodano, Serrano-Velarde, and Tarantino, 2016; Mann, 2018; Canipek, Kind, and Wende, 2021; Schoenherr and Starmans, 2021). Stronger creditor rights can reduce credit costs through higher collateral value and thus may allow firms to increase debt financing. In contrast, stronger creditor rights can increase distress costs and thus may prompt firms to reduce debt financing. My study contributes to this debate by pointing to another channel through which creditor rights can affect firm financing: as noted above, stronger creditor rights can enable debt as a powerful mechanism to discipline management, which can reduce capital costs of equity (by limiting managerial expropriation) as well as debt (by lowering default probability) and thus may allow firms to increase financing. Note that this channel offers alternative explanations to existing findings. For example, as a response to stronger creditor rights, a documented increase in debt financing may not be because of lower interest rates through higher collateral value, but because of lower interest rates as a result of better debt governance (or, as another possibility, simply because firms set debt as a governance mechanism, as argued by the financial contracting literature). Likewise, an observed

be of particular importance in the context of the recent crises during which many countries have significantly softened their bankruptcy law.¹⁰

I also contribute to the literature that examines the determinants of board independence—a variable that varies strongly across countries and across firms (Doidge, Karolyi, and Stulz, 2007; Ferreira and Kirchmaier, 2013). While it is recognized that country characteristics have a greater explanatory power than firm characteristics, “much of the cross-sectional variation in board independence remains unexplained.” (Ferreira and Kirchmaier, 2013, p. 212; see also Doidge, Karolyi, and Stulz, 2007).¹¹ Additionally, it has been difficult to provide credible causal effects of country and firm characteristics on board structure, and moreover to find characteristics that have an economically rather than only statistically significant effect on the composition of the board (Ferreira, Ferreira, and Mariano, 2018).¹² My findings suggest a new determinant of board independence: the availability of debt governance, which depends on country-level bankruptcy law (whether debt governance is effective) and firm-specific bankruptcy risk (whether debt governance matters). My results help to explain the existence of the independence gap across countries with different bankruptcy penalties (e.g., my findings can explain more than one third of the difference in board structure between the US and the UK) and variation in board independence across firms with different default probabilities. The effect of debt governance on board independence is of great economic significance. Given the experimental design, it is unlikely that the results are affected by endogeneity problems.

Finally, my paper is also related to a growing body of empirical research that shows that creditors can constrain managerial discretion in situations of covenant violation. Taken together, these studies find that violation rarely results in termination. Instead, creditors use the

decrease in corporate leverage may not be because of higher distress costs, but because firms increased equity financing by lower capital costs of it, while their demand for debt remained unchanged.

¹⁰ In an attempt to protect distressed firms from liquidation, e.g., Germany and the UK have softened their laws as a response to the 2007–2009 financial crisis and the Covid-19 crisis, respectively (Canipek, Kind, and Wende, 2021; Payne, 2021). While under some circumstances suspending bankruptcy penalties temporarily may be useful, my findings suggest that repealing them permanently may result in substantial costs.

¹¹ Boone et al. (2007) and Linck, Netter, and Yang (2008) respectively show that important firm characteristics can only explain a maximum of 31% and 22% of the variation in board independence of US firms. In contrast, Doidge, Karolyi, and Stulz (2007) show that up to 72% of the variation in firm-level governance can be explained by country-level dummy variables. However, when the authors try to explain the country effects with country characteristics that the literature believes to be associated with corporate governance, such as the levels of economic and financial development, shareholder rights, or the quality of legal enforcement, they find that these variables can only explain 18% of the variation in corporate governance.

¹² As noted by Ferreira, Ferreira, and Mariano (2018), the economic significance of the effects of important firm characteristics on board independence is low in virtually all regressions in the literature. For example, Boone et al. (2007) show that a one-standard-deviation change in firm size, firm age, number of business segments, CEO tenure, and CEO ownership is associated with a 1.8, 2.4, 1.3, 1.9, and 4.8 percentage point change in board independence, respectively (see also Ferreira, Ferreira, and Raposo, 2011, who report similar results, and additionally obtain an effect of 1.2 percentage points for changing leverage by one standard deviation). The study by Ferreira, Ferreira, and Mariano (2018) is an important exception. The authors show that the number of independent directors increases by 24% following financial covenant violations.

threat of accelerating the loan to renegotiate tighter restrictions on corporate financial, investment, and governance policy (Chava and Roberts, 2008; Nini, Smith, and Sufi, 2009, 2012; Roberts and Sufi, 2009a, 2009b; Roberts, 2015; Ferreira, Ferreira, and Mariano, 2018). As a result, it has been argued that the threat of managerial restrictions can have a disciplinary function (see Bharath and Hertz, 2019, who find evidence consistent with this view). As noted, unlike these studies, I do not consider the incentive effect of debt that comes from managerial restrictions following covenant violation but the incentive effect of debt that comes from bankruptcy penalties. However, like these studies, my analysis provides empirical evidence consistent with a rising view in the empirical corporate governance literature that debt plays an important role in the governance of firms. This fact has been largely ignored by the traditional literature that primarily focuses on the ability of equity-based governance mechanisms to mitigate agency problems (Nini, Smith, and Sufi, 2012; Bharath and Hertz, 2019).

The remainder of the paper proceeds as follows. Section 1.2 describes the data, discusses the measures for board independence and bankruptcy penalties, and provides summary statistics. Section 1.3 presents the main findings from the cross-country analysis. Section 1.4 presents the findings from the bankruptcy reforms. Section 1.5 discusses the robustness of the findings with regard to alternative explanations and offers discussions that may provide further insights into the findings of this paper. Section 1.6 concludes.

1.2 Data and summary statistics

1.2.1 Data

I study the relation between board independence, bankruptcy penalties, and default probability, after controlling for country and firm characteristics. In doing so, I use data from various sources. I obtain board data from the BoardEx database.¹³ The data on creditor rights (as a country-specific measure of managerial bankruptcy penalties) are from 2003 and come from Djankov, McLiesh, and Shleifer (2007)—who updated and enlarged the data of La Porta et al. (1998). Other country-level variables are from Djankov, McLiesh, and Shleifer (2007), Djankov et al. (2008), and the World Bank databases. Other firm-level variables (including the necessary variables to construct Altman’s Z-score as a firm-specific measure of default probability) are sourced from Worldscope. Table A.1.1 in the Appendix presents detailed variable definitions and data sources.

¹³ I thank the Columbia University for providing data access to WRDS and BoardEx during my research stay.

After merging the databases (I match firms in BoardEx to their accounting data in Worldscope using ISIN codes), I start the sample construction with choosing the sample period 2002–2016. I choose this sample period to allow for time-series data that cover the years around the bankruptcy reforms that are used as natural experiments, although my results are unaffected when focusing on other periods. Following the conventions in the board literature and international studies (e.g., Ferreira, Ferreira, and Mariano, 2018; and, e.g., Brockman and Unlu, 2009; Favara et al., 2017), I exclude firms from financial industries (SIC codes 6000–6999). Additionally, I exclude each firm-year observation without complete data on all dependent and control variables used in the main empirical analysis, without data on Altman’s Z-score, and with negative or zero total assets or sales.¹⁴ Finally, I winsorize all firm-level control variables at the first and ninety-ninth percentiles to account for outliers. The final sample consists of 86,913 firm-year observations from 11,965 firms in 50 countries.¹⁵

1.2.1.1 Data on board independence

By monitoring management and pushing for CEO dismissal in case of poor performance, independent directors can induce an incentive effect on management quality similar to that one typically associated with debt governance and can thus serve as a substitute for solving agency problems. Following Ferreira, Ferreira, and Mariano (2018), I consider the actual number of independent directors as the primary measure of the governance intensity by the board and not the ratio of independent directors to board size for two reasons. First, independent directors can exert pressure on management regardless of the board size or their fraction in the board. The more independents, the higher is the probability that there is someone who is “unfriendly”, that is, someone who asks the management the right questions, or initiates a CEO dismissal by making the first move in expressing doubts on management performance to other board members or by even making a motion of CEO dismissal (Anderson, Mansi, and Reeb, 2004). Second, ratios are not informative about what happens to the number of independent directors and the board size after changes in boards, while focusing on levels allows for such inference (Ferreira, Ferreira, and Mariano, 2018). However, for robustness, I also present the results for the

¹⁴ Following Ferreira, Ferreira, and Mariano (2018), I drop financial industries, but keep regulated industries (SIC codes 4800–4949). Following Favara et al. (2017), I drop firm-years with negative or zero total assets or sales. My results remain unaffected when also excluding regulated industries and/or also including firm-years with negative or zero total assets or sales.

¹⁵ Data coverage of BoardEx improved over the course of the 2000s (Ferreira and Kirchmaier, 2013). However, my findings are unaffected by possible composition effects of the BoardEx database, as analyses for individual years (e.g., 2002, 2009, or 2016) lead to similar results as those from the main analysis for the years 2002–2016. Note that also the findings of the natural experiments are unaffected by possible composition effects, given that the analyses require data availability prior to the reforms.

ratio of independents to board size as an alternative measure of the board's governance intensity, which is also indicative regarding the influence of independent directors on board decisions (Anderson, Mansi, and Reeb, 2004). Additionally, I present the results for board size, which allows to infer whether independents replace outgoing directors or are added to the board after possible changes in the board (Ferreira, Ferreira, and Mariano, 2018).

1.2.1.2 Data on bankruptcy penalties

I exploit that for debt to be an effective source of discipline, there must be a bankruptcy procedure that sufficiently penalizes management in default (Grossman and Hart, 1982; Aghion, Hart, and Moore, 1992; Hart, 1995, 2000). I use the creditor rights index of La Porta et al. (1998) as a measure of managerial bankruptcy penalties. The creditor rights index, CRIGHTS, is comprised by four binary components that capture the power of secured creditors against management in bankruptcy: AUTOSTAY, which equals one if secured creditors are able to seize their collateral after the reorganization petition is approved, that is, if there is no automatic stay on the assets, and zero if automatic stay prevents the debtor from secured creditors; REORG, which equals one if there are restrictions, such as creditors' consent, when a debtor files for reorganization, and zero if management can seek protection from creditors by filing for reorganization unilaterally; SECURED, which equals one if secured creditors are paid first out of the proceeds of liquidating the firm, and zero if other claimants, such as the government or workers, are given absolute priority; and MANAGES, which equals one if an administrator is automatically appointed for running the business during the reorganization procedure, that is, management is dismissed in the reorganization procedure, and zero if management retains corporate control during reorganization (see Table A.1.1 in the Appendix for details). The index ranges from zero to four and higher values indicate stricter penalties on management in bankruptcy.¹⁶

1.2.1.3 Control variables

To mitigate the concern that the relation between board independence and bankruptcy penalties is driven by a factor that simultaneously determines both variables, I control for a number of country and firm characteristics that are possible determinants of board structure and

¹⁶ For a description of the index, see also Djankov, McLiesh, and Shleifer (2007), Brockman and Unlu (2009), and Acharya, Amihud, and Litov (2011). See Acharya, Amihud, and Litov (2011), who use the index as a measure of firms' distress costs.

bankruptcy law.¹⁷ Specifically, the country characteristics include legal origin, which has been shown to be an important determinant of creditor rights for some countries, given that for these countries bankruptcy law is rather not written from the scratch but adopted through conquest, imperialism, or imitation from some legal families (La Porta et al., 1998; Djankov, McLiesh, and Shleifer, 2007). Furthermore, I include shareholder rights (SHRIGHTS), because firms in countries with weak shareholder rights may implement alternative instruments as substitute governance mechanisms (La Porta et al., 1998; Dahya, Dimitrov, and McConnell, 2008). I also control for the quality of legal enforcement (rule of law), which may substitute with board independence (see La Porta et al., 1998, who argue that well-functioning courts can step in and rescue investors expropriated by management), or, as another possibility, may increase the productivity of other governance mechanisms (see Doidge, Karolyi, and Stulz, 2007; Aggarwal et al., 2009, who argue that commitments of other governance mechanisms are more credible with stronger enforcement). Finally, I consider the levels of financial development (market cap) and economic development (GDP per capita), following Doidge, Karolyi, and Stulz (2007) and Aggarwal et al. (2009), who argue that in countries with better financial development it may be more beneficial to invest in firm-level governance (since firms can receive more funding from the capital markets, thus benefiting more from governance related reductions in the cost of financing) and that in countries with better economic development it may be less costly to invest in firm-level governance (as it is less expensive to find individuals with the skills necessary to perform good governance).

Following Boone et al. (2007) and Ferreira, Ferreira, and Raposo (2011), the firm characteristics include size, because size can involve more agency problems and thus requires more board monitoring, and because firms grown into different business lines require additional monitoring by specialized board members; free cash flow and profitability (return on assets), because these variables may be related to agency conflicts and private benefits available to management and thus to the advantages of monitoring; and the market-to-book ratio as well as return volatility, because for high growth firms and firms with more volatility outsider monitoring may be more costly and less effective.¹⁸ Additionally, I control for insider ownership, because higher ownership increases managers' and controlling shareholders' cost of extracting

¹⁷ Note that considering firm and country characteristics also allows to compare firms with similar characteristics (for more, see footnote 22 below), and to account for possible alternative explanations of the independence-penalty relation (for more, see Section 1.5 below).

¹⁸ Note that including return volatility (standard deviation of return on assets) as a control variable requires that firms have at least two observations to be included in the sample, i.e., the analysis excludes firms with only one observation (there are about 500 such firms). The findings remain unaffected when dropping return volatility as a control variable and including firms with only one observation.

private benefits and hence reduces the need to mitigate agency conflicts by stronger board monitoring (Doidge, Karolyi, and Stulz, 2007);¹⁹ and for leverage, which is related to the effectivity of debt governance (Zwiebel, 1996; Hart, 2001) and hence may affect the need of independent directors as a substitute governance device.

For robustness, in Table A.1.2 in the Appendix, which repeats the main analyses of Tables 1.2 and 1.3 below, I consider a number of other country-level control variables. These variables include accounting standards (which may substitute or compliment with board independence, see La Porta et al., 1998; Anderson, Mansi, and Reeb, 2004), average institutional ownership in a country (which is shown to affect corporate governance, see Aggarwal et al., 2011), the board structure of a country (one-tier, two-tier, or mixed), and board rules on whether a country requires or encourages firms to increase board independence (rule-based, comply-or-explain, or no rule). While the inclusion of each of these controls reduces sample size (i.e., the number of countries) considerably, the relation between board independence, creditor rights, and bankruptcy risk remains unaffected.²⁰

1.2.2 Summary statistics

Table 1.1 presents summary statistics for the main variables at the country level.²¹ Three facts are worth mentioning. First, board independence varies significantly across countries, and the variation is persistent over time (and seems to be related to creditor rights). For example, the US (CRIGHTS=1), Canada (1), the UK (4), and Germany (3) are countries which are relatively similar in economic and financial development (Acharya, Amihud, and Litov, 2011), but

¹⁹ For many sample firms, information on insider ownership is not available in Worldscope for some years (while being available for other years). In case of missing data, I use the average ownership value computed by the years for which data is available. My results remain unaffected when excluding firm-years without ownership data, or when extending the sample by also including firms for which ownership data is never available.

²⁰ There are also other firm-level variables that might affect board independence such as firm age, number of business segments, or R&D expenditures (Boone et al., 2007; Ferreira, Ferreira, and Raposo, 2011); or the strength of other governance mechanisms that can substitute for board independence such as incentive compensation or takeover provisions (Demsetz and Lehn, 1985; Hermalin and Weisbach, 2003, 2012; Adams, Hermalin, and Weisbach, 2010; Avedian, Cronqvist, and Weidenmier, 2015; Bharath and Hertz, 2019). I do not control for these variables because of missing data or because their inclusion typically considerably reduces sample size. However, note that possible endogeneity problems due to the omission of variables are mitigated by the fact that the central test of the paper is to study the interaction between board independence, bankrupt penalties, and bankruptcy risk, and there is no obvious reason why omitted variables should be related differently to countries with different bankruptcy penalties as well as firms with different default probabilities. Also note that I employ firm fixed effects in the natural experiments in Section 1.4, which allow to account for time-invariant variables. Finally, in unreported results, I repeat the main analyses in this paper using Worldscope data on firm age and R&D expenditures and find that the results remain virtually unaffected.

²¹ For comparison, Ferreira and Kirchmaier (2013) report similar numbers on board characteristics for a sample of 29 countries in 2010 using BoardEx. For example, for the US and the UK, they report an average board size of 8.42 and 6.50, respectively, and an average ratio of independent directors to board size of 74% and 34%, respectively. The numbers for the US are also similar to those of other studies that focus on US firms and use the Investor Responsibility Research Center (IRRC) database for data on boards (e.g., Ferreira, Ferreira, and Raposo, 2011).

different in board independence. Columns 1 and 5 show that, in the US and Canada, the average number of independent directors/ratio of independents to board size is 6.2/75% and 6.1/73%, respectively, while, in the UK and Germany, it is only 2.7/38% and 1.8/13%, respectively. Columns 2 and 6 show the average within-firm yearly growth in the number of independent directors and in the ratio of independents to board size. As can be seen, there is a trend to more independent boards, probably resulting from the worldwide board reforms within the last two decades that require or encourage firms to increase board independence (Ferreira and Kirchmaier, 2013; Fauver et al., 2017). However, the “independence gap”—defined by Ferreira and Kirchmaier (2013) as the variation in board independence across countries—is persistent over time, as countries with low levels of board independence do not exhibit a more aggressive increase in independent board monitoring than countries with high levels of board independence. Taken together, this suggests an important role for static country variables, such as creditor rights, in shaping the independence gap.²²

Second, there is little within-firm variation in board independence. Column 3 shows that the average firm changes its number of independent directors only 0.37 times per year (or 0.25 times per year when subtracting the trend component of column 2), i.e., only about every 2.7 years (or 4.0 years). Column 4 shows that the within-firm standard deviation of the number of independents is small as well, indicating that the rare changes in board independence lead only to minor modifications of the board.²³ This sticky feature of board structure suggests that board structure is determined by static firm factors that typically do not change over time (such as industry affiliation or bankruptcy risk), which provides a good environment for studying changes in board structure as a response to a bankruptcy reform since possible adjustments in board independence can be better attributed to the change in the bankruptcy law and not to possible confounding shocks.²⁴

²² Note that the summary statistics suggest that board independence and creditor rights seem to be negatively related, particularly when comparing countries that roughly contain firms with similar characteristics in terms of firm-level and country-level properties (which is important, as characteristics such as firm size are typically related to board independence so that by comparing firms with different characteristics the observed relation between board independence and creditor rights may simply be the reflection of the size effect). For example, while the US and Canada have high board independence and low creditor rights, the UK and Germany have low board independence and high creditor rights. Nordic countries provide another example. Despite the similar macroeconomic characteristics, the countries are dissimilar in board structure and creditor rights. In the countries with weak creditor rights, Finland (CRIGHTS=1), Sweden (1), and Norway (2), the average ratio of independent directors to board size is 85%, 54%, and 43%, respectively, while it is 41% for Denmark (3) with strong creditor rights.

²³ From the 74,948 possible within-firm changes in the number of independent directors in my sample, a change took place in only 27,753 cases. For these cases, in only 6,461 cases more than one independent was added to or removed from the board, and in only 1,535 cases more than two independents were added or removed.

²⁴ On the stickiness of board structure, see also Graham, Kim, and Leary (2020).

Finally, note that observations from the US and the UK respectively account for 45% and 15% of the full sample observations. I will show that the findings are similar when excluding US and UK firms.

1.3 Cross-country study

1.3.1 Average relation

As described in the Introduction, the central analysis in this paper is whether firms implement independent directors as a substitute governance mechanism when bankruptcy laws that are soft on management make debt governance ineffective, and whether this effect is stronger for firms with high default probability. In a first step, I study the average relation between board independence and bankruptcy penalties. To do so, I estimate the following ordinary least squares (OLS) panel regression:

$$\text{Dependent variable}_{i,j,c,t} = \alpha + \beta \text{CRIGHTS}_c + \lambda \mathbf{X}_{i,j,c,t} + \psi \mathbf{y}_t + \theta \mathbf{a}_j + \epsilon_{i,j,c,t}, \quad (1.1)$$

where the dependent variable is either the logarithm of the number of independent directors, the ratio of the number of independent directors to board size, or the logarithm of board size; i indexes firms, j indexes industries, c indexes countries, and t indexes time; CRIGHTS_c is the creditor rights index as a measure of managerial bankruptcy penalties; $\mathbf{X}_{i,j,c,t}$ is a vector of the firm and country characteristics discussed above; \mathbf{y}_t is a vector of year fixed effects that control for time-varying factors common to all firms; and \mathbf{a}_j is a vector of industry fixed effects (based on the two-digit SIC code) that control for possible differences across firms from different industries. Standard errors are clustered by country; although conclusions are identical when the errors are clustered by firm.²⁵²⁶

²⁵ The OLS specification follows those employed by Benmelech and Bergman (2008) and Favara et al. (2017). Note that, following Ferreira, Ferreira, and Mariano (2018), I use the logarithm of the number of independent directors to estimate percentage changes in the number of independents.

²⁶ I cluster standard errors at the country level because observations may be correlated within countries (corporate governance is likely to be correlated within a country because some governance rules are mandated by country-level regulations (Aggarwal et al., 2011)). This method is in line with other cross-country studies that examine the effect of firm and country characteristics on firm-level corporate governance (Dojige, Karolyi, and Stulz, 2007; Aggarwal et al., 2009; Aggarwal et al., 2011). However, the summary statistics above show that board structure is persistent within a firm, suggesting that within a firm observations may not be independent over time (on this, see also Aggarwal et al., 2011). In unreported results, I mitigate this concern in two ways. First, I cluster standard errors at the firm level (Aggarwal et al., 2011). I find that the errors are lower than the errors clustered at the country level, suggesting that clustering errors by country presents a more moderate method. Second, I estimate standard errors using country-level clustering, but only including observations from one individual year (e.g., 2002, 2009, or 2016). I find that the results are similar to those of the main analysis.

Panel A of Table 1.2 reports the results for the logarithm of the number of independent directors (plus one to also consider observations with zero independents) as the primary measure of the governance intensity by the board. Consistent with the hypothesis that debt mitigates agency problems, columns 1 (without controls) and 2 (with controls) show that the coefficient on $CRIGHTS_c$ is negative and statistically significant, i.e., board independence is negatively related to managerial bankruptcy penalties. The economic effect is sizable. Column 2 shows that a change from strictest (when debt governance is most effective) to softest (when debt governance is least effective) penalties is associated with an approximately 34% increase in the number of independent directors (or an increase of $0.34 \times 4.8 = 1.6$ independents, based on the sample average of the number of independents presented in Table 1.1 above), suggesting that debt is valued as a powerful instrument to discipline management.²⁷

Importantly, as the effect between board independence and bankruptcy penalties is of great economic significance, an important by-product of my results is that they can explain large parts of the substantial cross-sectional variation in board independence that is left unexplained by previously found determinants of board independence.²⁸ In this sense, differences in management penalties in bankruptcy between the US ($CRIGHTS=1$) and the UK ($CRIGHTS=4$) can explain about $(1.6 \times \frac{3}{4}) / (6.2 - 2.7) = 34\%$ of the difference in the average number of independent directors between both countries.

In columns 3 to 6, I also examine the relation between the number of independents and individual components of the creditor rights index. As shown, the negative relation is strongest for all three components that typically correspond to strong management penalties in bankruptcy—i.e., provisions that impose costs on managerial private benefits of control either within the bankruptcy procedure by mandating the dismissal of management during bankruptcy (MANAGES), or via inducing firm liquidation as the outcome of the bankruptcy procedure by precluding management to file for firm reorganization unilaterally (REORG) or by not allowing

²⁷ Note that in describing my findings I follow the formulations of the bankruptcy literature (Acharya, Amihud, and Litov, 2011; Vig, 2013; Ponticelli and Alencar, 2016; Rodano, Serrano-Velarde, and Tarantino, 2016; Favara et al., 2017; Mann, 2018; Schoenherr and Starmans, 2021), e.g., in this section with regard to “columns 1 (without controls) and 2 (with controls) show that the coefficient on $CRIGHTS_c$ is negative and statistically significant, i.e.,” or in the following sections with regard to “as shown in columns 1 (without controls) and 2 (with controls), the coefficient on $Post_t \times HighRisk_i$ is positive and statistically significant, that is, treated firms changed firm characteristics after/around the reform relative to control firms”. Also note that I borrow the term “debt governance” from Whitehead (2011), who use this term for describing the general governance role of debt. Finally, note that other wordings follow the formulations of the general finance literature, e.g., “the effect is sizable” (Benmelech and Bergman, 2008), “calculated by/based on the sample average” (Ferreira, Ferreira, and Mariano, 2018), and “and especially/only so for some firms” (Giroud and Mueller, 2010, 2011). The same applies for Chapter 2 of this doctoral thesis.

²⁸ On the low economic effects of other determinants of board independence, see Boone et al. (2007), Doidge, Karolyi, and Stulz (2007), Linck, Netter, and Yang (2008), Ferreira, Ferreira, and Raposo (2011), and Ferreira, Ferreira, and Mariano (2018).

automatic stay on assets (AUTOSTAY). In contrast, the negative relation is weakest and statistically insignificant for the component that typically corresponds to weak bankruptcy penalties—i.e., the provision that gives secured creditors the right to be paid out first (SECURED), which might impose penalties only indirectly via higher incentives of creditors to press for liquidation.²⁹ These results lend further support to the notion that firms implement independent directors as a substitute when insufficient bankruptcy penalties make debt governance ineffective.

Columns 7 and 8 respectively show the estimates when US firms and both US as well as UK firms are excluded. As can be seen, also when excluding firms of both countries which account for about 60% of the full sample observations, I find a negative relation between the number of independent directors and managerial penalties in bankruptcy (the economic and statistical effects are lower compared to those of the full sample, however, Section 1.3.2 below shows that the effects appear large when focusing on those firms that actually drive the negative relation, i.e., firms with high default probability for which debt governance is most effective).³⁰

For robustness, in Panel B, I repeat the analysis for the fraction of independent directors to board size as an alternative measure of the board's governance strength. As can be seen, the findings are (qualitatively) similar to those of Panel A. Column 2 implies that switching off debt governance by moving from the strictest to softest management penalties in default is associated with an approximately 18 percentage point increase in the fraction of independents to board size. In terms of explaining cross-sectional variation, the results imply that differences in bankruptcy penalties can, e.g., explain about $(0.18 \times \frac{3}{4}) / (0.75 - 0.38) = 37\%$ of the 37 percentage point difference in the average ratio of independents to board size between the US and the UK. Note that the results not only ensure the robustness of the findings, but also indicate that substantial board and voting power is given to independent directors when debt governance is ineffective.

Finally, to infer whether independent directors replace dependents or are added to the board when debt governance becomes ineffective, Panel C presents the estimates for the logarithm of board size as the dependent variable. As shown, in general, there is no statistically significant relation between board size and bankruptcy penalties, suggesting that independents replace

²⁹ Notably, Section 1.3.2 below shows that for firms with high default probability, i.e., when debt governance matters, there is actually a statistically significant negative relation between board independence and SECURED; although the negative relation is weaker compared to those of the other components of the creditor rights index.

³⁰ Note that parts of the designs of the tables for this section (specifically, for Tables 1.2 and 1.3 and Tables A.1.2 and A.1.3), the description of the economic effect based on a change from highest to lowest creditor rights, the analysis of the individual creditor rights components to further support the findings, and the exclusion of US and UK firms to ensure the robustness of the findings follow the cross-country studies of Brockman and Unlu (2009), Acharya, Amihud, and Litov (2011), and Favara et al. (2017), who study the effect of bankruptcy law design on other firm-level characteristics, such as dividend payouts, leverage, investments, or risk taking. Also parts of the descriptions of the tables for this section follow these papers as well as Vig (2013).

dependents instead of being added to the board. The result is, on the one hand, consistent with the notion that small boards of directors are more effective in monitoring management (Yermack, 1996), and, on the other hand, suggests that the fraction of independent directors on the board is increased at minimal costs.

1.3.2 Difference in default probability

Next, I study whether the negative relation between board independence and bankruptcy penalties differs for firms with high and low default probability.³¹ To this end, I expand the regression in equation (1.1) by the interaction term $HighRisk_{i,j,c,t} \times CRIGHTS_c$ (as well as $HighRisk_{i,j,c,t}$), where $HighRisk_{i,j,c,t}$ is a dummy variable that equals one if a firm has high default probability (e.g., if it has bankruptcy risk above the median bankruptcy probability distribution of its country), and zero if it has low default probability.³² I measure bankruptcy risk with Altman's Z-score (Altman, 1968). Table A.1.3 in the Appendix repeats the analysis of Table 1.3 below using the interest coverage ratio as a proxy for the probability of default. The findings are virtually identical.

Panel A of Table 1.3 shows the results for the logarithm of the number of independent directors. In column 1, $HighRisk_{i,j,c,t}$ equals one if a firm is in the top half of the bankruptcy probability distribution of its country for a given year (low Z-score), and zero otherwise (high Z-score). Further supporting the empirical relevance of debt governance, the coefficient on the interaction between high default probability and creditor rights is negative and statistically significant. That is, the negative relation between board independence and creditor rights is stronger among firms with high default probability relative to firms with low default probability. The estimates imply that the marginal effect between creditor rights and the number of independent directors is 7% among firms with low default probability, whereas the marginal effect is 10% ($0.073 + 0.024$) among firms with high default probability.

In column 2, I separate firms into three terciles, and define firms in the highest tercile as firms with high default probability and firms in the lowest tercile as firms with low default

³¹ For studies that measure the effects of bankruptcy law design on other firm characteristics using variation in the effect of the law in default probability, see, e.g., Rodano, Serrano-Velarde, and Tarantino (2016), Favara et al. (2017), Gross et al. (2021), and Schoenherr and Starmans (2022).

³² The expansion of the regression by an interaction term follows the regressions employed by Favara, Schroth, and Valta (2012), Favara et al. (2017), and Albuquerque et al. (2019). The separation of firms at the median, tercile (and cutting off the middle 33% of the distribution), or quartile (and cutting off the middle 50% of the distribution) based on the distribution of a specific variable follows the approaches by Chava and Roberts (2008), Giroud and Mueller (2010, 2011), Ferreira, Ferreira, and Raposo (2011), Favara, Schroth, and Valta (2012), Vig (2013), Rodano, Serrano-Velarde, and Tarantino (2016), Favara et al. (2017), and Schoenherr and Starmans (2021).

probability (cutting off the middle 33% of the bankruptcy probability distribution). In column 3, I separate firms into four quartiles, and define firms in the highest quartile as firms with high default probability and firms in the lowest quartile as firms with low default probability (cutting off the middle 50% of the bankruptcy probability distribution). Strikingly, across columns 1 to 3, the negative coefficient on the interaction term increases monotonically (both in terms of economic and statistical significance), implying that the difference in the negative relation increases the more particularly risky firms are compared to particularly safe firms.³³ Regarding the economic significance, column 3 shows that, for firms with high default probability, a change from the strictest to the softest bankruptcy regime is associated with an increase in the number of independent directors by 46%, or 18% (4×0.045) relative to firms with low default probability.³⁴

Columns 4 to 7 show the results when interacting $HighRisk_{i,j,c,t}$ (quartile separation) with the individual creditor rights components, i.e., when studying differences across high and low default probability firms in the relations between board independence to the individual components. As shown, the difference in the negative relation across firms with high and low default probability is strongest for the provisions that typically impose strong bankruptcy costs (MANAGES, REORG, AUTOSTAY), and weakest for the provision that typically imposes the weakest costs (SECURED). Finally, columns 8 (median separation) and 9 (quartile separation) present the estimates when excluding firms from the US and the UK. As can be seen, all findings are supported. A notable difference to the full sample is that, in the more general case here, no statistically significant relation between board independence and bankruptcy penalties exists among firms with low default probability (e.g., column 9 shows that, among safe firms, the coefficient on $CRIGHTS_c$ has a t -value of -0.91).³⁵

³³ In unreported results, I study the relation between the number of independents and creditor rights separately for the four quartiles. I find that the negative relation is weakest among firms with low default probability, increasing in default probability, and strongest among firms with high default probability. Specifically, the coefficients on $CRIGHTS_c$ for the first (low default probability), second, third, and fourth (high default probability) quartiles respectively are -0.070 (t -value=-2.80), -0.073 (t -value=-3.57), -0.080 (t -value=-4.61), and -0.117 (t -value=-6.55).

³⁴ Note that, to be considered in the analysis, for a given year a country needs to have at least two firms in column 1, three firms in column 2, and four firms in column 3, which explains changes in the number of countries across Tables 1.2 and 1.3 or across columns 1 to 3 in Table 1.3. Also note that dividing firms by the bankruptcy probability distribution of their country for a given year may lead to an insufficient differentiation between high and low default probability firms in countries with few firms for a given year. Alternative possibilities are to divide firms by the distribution over all years, by the distribution over all countries, or to set a specific threshold of Altman's Z -score that identifies high and low default probability firms (a commonly used rule is, e.g., to define firms with a Z -score below 1.8 as risky firms and firms with a Z -score above 3.0 as safe firms). In unreported results, I find that my findings remain virtually identical when using the alternative possibilities.

³⁵ There are at least two reasons why also among safe firms board independence and creditor rights can be negatively related. First, as discussed in Section 1.3.3 below, it might be the case that creditor rights are endogenous to board structure, i.e., an omitted country characteristic simultaneously drives both variables. While this is rather unlikely for most countries as across countries bankruptcy law is typically determined by a different reason (and these reasons typically should not affect board structure), it is possible when considering only two countries.

Panel B of Table 1.3 presents the results for the ratio of independents to board size as the dependent variable. As can be seen, the findings remain (qualitatively) unaffected. Column 3 shows that, for firms with high default probability, a change from strictest to softest bankruptcy penalties is related to an increase in the ratio by 22 percentage points, or 5 percentage points relative to firms with low default probability. Panel C presents the estimates for board size as the outcome variable. As shown, board size tends to be more negatively related to creditor rights for firms with high default probability relative to firms with low default probability, yet the difference in the negative relation is statistically insignificant for the more general case in which the US and the UK are excluded.

1.3.3 Difference in default probability and causal inference

The fact that the negative relation between board independence and bankruptcy penalties differs for firms with high and low default probability not only provides further support on the empirical relevance of debt governance, but also improves the causal interpretation of my findings. Specifically, the main concern when identifying the empirical role of debt governance by looking at the average relation between board structure and bankruptcy penalties is that bankruptcy law might be endogenous to board independence, that is, that for most countries an omitted country characteristic simultaneously determines both variables such that bankruptcy penalties do not cause board dynamics. Identifying the empirical relevance of debt governance by looking at the difference in the negative relation across firms with high and low default probability mitigates this concern by reducing the identification problem to one in which for most countries an omitted country attribute not only determines bankruptcy law and board structure, but also affects board structure differently for firms with high and low default probability (put differently, the feature that debt governance matters only for firms with high default probability gives something of a “natural control group” in firms with low default probability that control for country characteristics common to all firms).³⁶

The result that, among safe firms, board independence and creditor rights are not correlated when considering most countries (i.e., when excluding the US and the UK) but are correlated when considering the US and the UK (which account for more than half of the observations) seems to be consistent with this view. Put differently, for the US and the UK, there might be an omitted country variable that simultaneously determines board independence and bankruptcy law. Second, as discussed in Section 1.5 further below, bankruptcy penalties may make other governance mechanisms for which bankruptcy risk is not necessary to function more effective, thereby possibly also playing a governance role for safe firms.

³⁶ See Favara et al. (2017), who argue that heterogeneity in default risk mitigates potential endogeneity problems in the relations between bankruptcy law and different firm variables. See also Chava and Roberts (2008), Ferreira, Ferreira, and Raposo (2011), Vig (2013), and Schoenherr and Starmans (2021), who argue that heterogeneity across firms improves the causal interpretation of their findings.

Note that endogeneity problems are unlikely to affect my findings. Across countries, bankruptcy law is typically determined by a different reason—e.g., for Germany in 2012, the 2007–2009 financial crisis that led to weaker creditor rights to rescue distressed firms (Canipek, Kind, and Wende, 2021); for Italy in 2005, a corporate scandal leading to weaker creditor rights to rescue the firm without violating European laws (Rodano, Serrano-Velarde, and Tarantino, 2016); for South Korea in 2006, the 1997–1998 Asian financial crisis that led to a management-friendly law as demanded by the IMF and World Bank in exchange for financial support (Schoenherr and Starmans, 2021); for Brazil in 2005, an attempt to promote lending via weaker creditor rights (Ponticelli and Alencar, 2016; Favara et al., 2017); for India in 2002, an attempt to promote lending via stronger creditor rights (Vig, 2013); for India before 2002, political considerations on electoral outcomes leading to lower creditor rights as layoffs could have a negative effect on electoral outcomes (Vig, 2013); for the UK in 2020, the Covid-19 crisis that led to weaker creditor rights to rescue distressed firms (Payne, 2021); and for the US in 1979, the political power of competing interest groups that resulted in a debtor-friendly law (Posner, 1997).³⁷ Additionally, these reasons should not affect board structure, or at least not differently across firms with high and low default probability.

1.4 Changes in board independence around bankruptcy reforms

To further bolster the causal interpretation of my findings, I study changes in board structure of firms with high default probability relative to those of firms with low default probability around bankruptcy law reforms.³⁸ The objective of the analysis is to support the cross-country findings by focusing on cases for which the determinant of the bankruptcy law is known, and should not be correlated with corporate board structure. There are six bankruptcy reforms that have been comprehensively studied by the bankruptcy literature, namely, the 2012 reform in Germany, the 2005–2006 reforms in Italy, the 1979 reform in the US, the 2006 reform in Korea, the 2005 reform in Brazil, and the 2002 reform in India.³⁹ I study the reforms in Germany and

³⁷ While the process of the US bankruptcy reform of 1979 was triggered by increasing dissatisfaction with the bankruptcy law (the law was considered unnecessarily complicated and intolerably ambiguous), the final statute was just a reflection of the relative political power of competing interest groups, such as debtors, managers, creditors, lawyers, judges, and politicians (Posner, 1997; Hackbarth, Haselmann, and Schoenherr, 2015).

³⁸ The empirical strategy is similar to that used by Favara et al. (2017), who support their cross-country findings regarding the effect of bankruptcy law design on investments and risk taking by studying changes in these variables of risky firms relative to those of safe firms around bankruptcy reforms. For studies that use bankruptcy reforms to examine the effect of bankruptcy design on different firm characteristics, see, besides Favara et al. (2017), also, e.g., Vig (2013), Rodano, Serrano-Velarde, and Tarantino (2016), Canipek, Kind, and Wende (2021), and Schoenherr and Starmans (2021).

³⁹ As mentioned earlier, the studies in the bankruptcy literature are typically concerned with the effect of creditor rights on corporate financial and investment policy.

Italy, as those are the only cases for which data availability of BoardEx allows for a meaningful analysis.⁴⁰ Additionally, I provide event-study evidence on the US reform using detailed summary statistics on board structure of US firms from 1933 to 2011 based on hand-collected data provided by Graham, Kim, and Leary (2020).

1.4.1 Germany's bankruptcy reform of 2012

As a response to the 2007–2009 financial crisis, in 2012, Germany introduced a bankruptcy reform that was aimed at facilitating the restructuring of distressed firms (Canipek, Kind, and Wende, 2021).⁴¹ In particular, the reform reduced managerial bankruptcy penalties by introducing a debtor-in-possession proceeding of about six months—accessible unilaterally, i.e., without creditors' consent—that allows management to stay in corporate control during the bankruptcy procedure and moreover imposes automatic stay on the assets.^{42,43} In terms of the creditor rights index, the reform removed the individual creditor rights components MANAGES, REORG, and AUTOSTAY (only secured creditors' right to be paid first is left unchanged), thus reducing Germany's creditor rights index by three points from four to one.⁴⁴ According to the cross-country results of Section 1.3, this is associated with an increase in the

⁴⁰ As noted, BoardEx improved data coverage over the course of the 2000s (BoardEx started collecting data in 1999) and at different speeds across countries. For the year of the reform, board data (in combination with the control variables and Altman's Z-score) are available for only two firms in Brazil and no firms in Korea, India, and the US.

⁴¹ The intention to reform the law initially dates back to the coalition agreement of 2009, and was originally based on the idea to compete with other restructuring laws, such as that of the US, that have been increasingly favored by investors in their investment decisions. However, ultimately no major changes have been made, until, in June 2010, an oppositional party submitted a letter of request demanding the government to implement a restructuring law in order to protect thousands of the by the 2007–2009 financial crisis distressed firms from inefficient liquidation by the old law. (Similarly, the 2020 bankruptcy reform in the UK initially originates from several proposals in 2016, and was actually based on the idea to compete with other restructuring laws; however, ultimately no major changes have been made, until the Covid-19 crisis and the economic consequences it generated brought the proposals back to the legislative agenda (Payne, 2021)). For my analysis, the most important point is that the determinant of the bankruptcy reform, be it the 2007–2009 financial crisis or to compete with other reorganization laws, should not be correlated with corporate board structure, or at least not differently across firms with high and low default probability. For the history of the reform, see Canipek, Kind, and Wende (2021).

⁴² Similar to US Chapter 11, the law also offers the possibility of a cram down option that allows the court to impose a restructuring plan on dissenting creditors (Canipek, Kind, and Wende, 2021).

⁴³ Under the old law, management was significantly penalized in bankruptcy. In particular, immediately after filing for bankruptcy, management was dismissed, and corporate control was transferred to an administrator. Moreover, while the task of the administrator was to continue the firm until the voting date on which different classes of claimants voted on whether to continue or liquidate the firm (scheduled six months after filing), his/her overriding objective to protect secured creditors' security value together with the threat of contingent liabilities towards creditors when violating this objective essentially gave secured creditors the power to seize their assets during the bankruptcy procedure, which in turn led to firm liquidation. Empirically, 86% of bankrupt firms got liquidated before the voting date, i.e., by the administrator. Overall, bankrupt firms experienced a firm liquidation at the end of the bankruptcy procedure in about 89% of cases. For the pre-reform law in Germany, see Canipek, Kind, and Wende (2021).

⁴⁴ Empirically, Canipek, Kind, and Wende (2021) show that, in 2017, the acceptance rate when a firm filed for the debtor-in-possession proceeding was about 88%. Additionally, the continuation rate of firms that filed for a reorganization procedure was about 90%. These results document the empirical relevance of the changes introduced by the reform.

number of independent directors by approximately 26%, or 14% when measuring the effect by comparing firms with high and low default probability.

I examine changes in board independence of firms with high default probability relative to those of firms with low default probability around the bankruptcy reform. Relative to event-study evidence, this has the advantage that firms with low default probability serve as a control group that account for confounding factors common to all firms.⁴⁵ If debt governance matters, the expectation is that firms with high probability of default increased board independence after the reform relative to firms with low probability of default.

The data are the same as for the cross-country analysis, described in Section 1.2. The sample period is from 2007 to 2016, that is, I study five years before and after the enactment of the reform in 2011.⁴⁶ Based on the four-year average pre-reform (2008 to 2011) measure of default probability (Altman's Z-score), I divide firms into four quartiles and define firms in the highest quartile as high default probability firms and firms in the lowest quartile as low default probability firms.⁴⁷ The full sample with all quartiles consists of 221 firms. The final sample, i.e., when only considering firms in the highest and lowest quartiles, consists of 778 firm-year observations from 110 firms (55 firms with high default probability and 55 firms with low default probability). Table A.1.4 in the Appendix reports summary statistics, separately for firms with high and low default probability, and for the periods before and after the reform.

To test for the expected effect, I estimate the following difference-in-differences regression:

$$\begin{aligned}
 & \textit{Dependent variable}_{it} \\
 & = \alpha_i + \lambda_t + \eta \textit{Post}_t + \gamma \textit{HighRisk}_i + \delta (\textit{Post}_t \times \textit{HighRisk}_i) + \beta \mathbf{X}_{it} \\
 & + \varepsilon_{it},
 \end{aligned} \tag{1.2}$$

where the dependent variable is one of the board variables of interest (either log independents, ratio of independents to board size, or log board size); i indexes firms, and t indexes time; α_i are firm fixed effects that control for time-invariant differences across firms; λ_t are year fixed

⁴⁵ Note that although the determinant of the reform should not be correlated with corporate board structure, another advantage of comparing high and low default probability firms is that, to affect the results, the reform's determinant not only needs to be correlated with board independence, but also needs to affect board independence to a different extent for firms with high and low default probability.

⁴⁶ The reform was passed in 2011, and became effective in 2012. For details on the timing of the reform, see Canipek, Kind, and Wende (2021).

⁴⁷ I use the four-year pre-reform average (2008 to 2011) of default probability, and not, e.g., the five-year pre-reform average, to remain consistent with the analysis of the Italian reforms for which I use the four-year pre-reforms average from 2002 to 2005, with 2002 being the first year of the sample period. Note that the separation of firms based on the average pre-reform measure of a specific variable follows Vig (2013).

effects that control for time-varying factors that are common to all firms; $Post_t$ is an indicator variable that equals one for the years after the bankruptcy reform was passed (2012 to 2016), and zero for the years before the reform was passed (2007 to 2011); $HighRisk_i$ is an indicator variable that equals one for firms with high default probability (highest quartile of the bankruptcy probability distribution), and zero for firms with low default probability (lowest quartile); X_{it} are the same time-varying firm characteristics as in the cross-country analysis;⁴⁸ ε_{it} is the firm-specific error term; and δ is the coefficient of interest, which measures the change in the board variable of firms with high default probability after the reform relative to firms with low default probability (the difference-in-differences effect). I cluster standard errors at the firm level, as corporate board structure should be correlated over time within a firm (see Section 1.2.2 above).⁴⁹

Columns 1 and 2 of Table 1.4 report the results for the logarithm of the number of independent directors as the primary measure of the board's governance strength. Consistent with the cross-country results, columns 1 (without controls) and 2 (with controls) show that the coefficient on $Post_t \times HighRisk_i$ is positive and statistically significant, i.e., firms with high default probability increased the number of independents after the reform relative to firms with low default probability. Also the economic effect is consistent with that of the cross-country analysis. Column 2 shows that, relative to firms with low default probability, firms with high default probability increased the number of independent directors by 29% after the reform (which implies an increase of $0.29 \times 1.8 = 0.5$ independents, calculated by the sample average of the number of independents of high default probability firms, see Table A.1.4).

In columns 3 and 4, I present the estimates for the ratio of independents to board size as an alternative measure of the governance strength by the board. As shown, the findings are (qualitatively) similar. Column 4 shows that firms with high default probability increased the ratio by 8 percentage points relative to firms with low default probability. Finally, columns 5 and 6 present the estimates for the logarithm of board size as the outcome variable. As can be seen, firms with high default probability did not change board size after the reform relative to firms with low default probability, which is again consistent with the cross-country analysis and suggests that independents replaced dependents instead of being added to the board.

⁴⁸ Note that, compared to the cross-country analysis, return volatility drops out as a control variable, given that it is defined as the average standard deviation of return on assets across all years and thus does not vary over time.

⁴⁹ The difference-in-differences specification follows that employed by Vig (2013) and Favara et al. (2017). Note that also parts of the table descriptions and table designs in this section (particularly for Tables 1.4–1.6) follow Vig (2013) and Favara et al. (2017).

As mentioned, comparing high and low default probability firms has the advantage of a control group in firms with low default probability that account for confounding factors common to all firms. Yet, a potential concern with my results is that a confounding factor affected high and low default probability firms differently.⁵⁰ Before proceeding, recall that board structure is sticky.⁵¹ That is, board structure is determined by static variables that typically do not change over time, which already mitigates the concern of possible confounding shocks.

To further mitigate the concern of confounding events, in Figure 1.1, I plot changes in the number of independent directors for firms with high and low default probability over time (for an extended period from 2002–2017 to get a broader picture). Specifically, Panel A shows the time series of the average number of independent directors for high and low default probability firms. As can be seen, high default probability firms increased the number of independents relative to low default probability firms after the reform. In contrast, high and low default probability firms followed parallel trends in the number of independents several years before the reform, suggesting that both groups of firms generally experience similar shocks and thus would have followed the same trend in the absence of the reform. To examine the statistical significance of possible changes, Panel B plots the yearly differences in the number of independent directors between high and low default probability firms relative to the difference in the year prior to the reform, 2011.⁵² As shown, for the years after the reform, the point estimates are positive and statistically significant, implying that the number of independents of firms with high default probability increased relative to firms with low default probability after 2011. In contrast, for the years before the reform, the point estimates are statistically insignificant, implying that the difference in the number of independents between high and low default probability firms was not different to that of 2011.

Notably, adjustments in boards were made over a number of years after the reform. This is consistent with the fact that specific dates of the annual shareholders' meeting (on which directors are typically replaced), staggered board provisions, or existing contracts can lead to lags in replacing directors as well as to changes in directors at different points in time across firms.^{53,54}

⁵⁰ On the problem of confounding shocks, see Vig (2013) and Schoenherr and Starmans (2021).

⁵¹ As shown in Section 1.2.2, firms change the number of independents on average only about every 2.7 years, and, if a change takes place, typically only one independent director is added to or removed from the board.

⁵² The empirical method here follows that used by Favara et al. (2017) and Jensen and Johannesen (2017). Note that Tables A.1.5 and A.1.7 in the Appendix follow Table A4 of Jensen and Johannesen (2017).

⁵³ On possible lags in replacing directors, see also Ferreira, Ferreira, and Mariano (2018).

⁵⁴ The figures presented in this section graphically reflect the first two facts of the summary statistics discussed in Section 1.2.2 above. Taken together, the figures show that (i) there is an independence gap across countries, (ii) the independence gap persists over time, (iii) there is a general trend towards more independent boards, and (iv) the independence gap seems to be related to creditor rights (e.g., Germany has low board independence with pre-reform CRIGHTS=3, Italy has medium to high board independence with pre-reform CRIGHTS=2, other countries (including Austria, Belgium, France, Netherlands, and Switzerland) have medium to high board

For robustness, Figure A.1.1 in the Appendix repeats the analysis of Figure 1.1 for the ratio of independents to board size as well as for board size. As can be seen, all findings are supported, i.e., risky firms did not change board structure or board size relative to safe firms prior to the reform.

1.4.2 The 2005–2006 bankruptcy reforms of Italy

Prompted by the Parmalat scandal, one of the largest European corporate bankruptcies, the Italian government introduced the 2005 and 2006 bankruptcy law reforms (Rodano, Serrano-Velarde, and Tarantino, 2016; see also Favara et al., 2017).⁵⁵ The 2005 reform, aimed at rescuing the firm, was created to facilitate firm continuation and reduced bankruptcy penalties by allowing debtors to enter a reorganization procedure unilaterally, thereby removing strict credit reimbursement requirements that previously applied (prior to the reform, debtor's plan had to provide the full repayment of secured creditors' claims along with at least 40% of unsecured creditors' claims).⁵⁶ Afterwards, the 2006 reform increased bankruptcy penalties by making several changes to the liquidation procedure that speeded up the previously lengthy process of firm liquidations (which, due to higher expected recovery rates, increases creditors' incentives to press for liquidation).⁵⁷ Empirically, Rodano, Serrano-Velarde, and Tarantino (2016) show that the share of reorganization procedures increased from about 2% before 2005 to about 10% in 2009. Additionally, the share of liquidation procedures that lasted longer than 24 months decreased from about 95% before 2005 to about 60% after 2005. With regard to the creditor rights index, while the first reform eliminated the individual component REORG (and partly eliminated MANAGES and AUTOSTAY through facilitating access to the protected

independence with average CRIGHTS=1.8, and the US has high board independence with CRIGHTS=1). Additionally, the figures show that there is little within-firm variation in board independence around the trend component (it seems that not even the 2007–2009 financial crisis led to major changes in board independence).

⁵⁵ Parmalat was an Italian dairy and food company. At the end of 2003, the firm collapsed with total assets of about 20 billion dollars. To rescue the firm without violating European regulations on government bailouts, the Italian government introduced the 2005 reform (Rodano, Serrano-Velarde, and Tarantino, 2016).

⁵⁶ Following US Chapter 11, the 2005 reform also made other changes that additionally reduced bankruptcy penalties. In particular, the reform lowered the share of votes (from two-thirds to one-half) required for a reorganization plan to be accepted. Furthermore, the reform introduced a cram down option that allows the court to impose a reorganization plan despite objections from creditors (Rodano, Serrano-Velarde, and Tarantino, 2016).

⁵⁷ In Italy, a liquidation procedure is in general carried out by a court-appointed trustee. Prior to the reform, the combined effect of poor trustee incentives to speed up liquidation—the trustee's salary was independent of creditors' recovery rates or the duration of the procedure—and the lack of creditor rights to monitor the trustee—creditors could neither veto the trustee's decisions nor ask for the trustee to be replaced—made liquidations a lengthy affair. In contrast, the new law allows creditors to ask for the trustee to be replaced and moreover makes all trustee decisions dependent on their approval. As a result, creditors effectively gain control over the liquidation procedure, who, in order to preserve asset value, are typically interested in speeding up liquidation. For the pre-reform law in Italy, again see Rodano, Serrano-Velarde, and Tarantino (2016).

reorganization procedure under management), the second reform affected the component SECURED (by higher expected recovery rates in liquidation).

As for the reform in Germany, I study changes in board independence of firms with high default probability relative to those of firms with low default probability around the reforms. If debt governance matters, we should see that, relative to firms with low probability of default, firms with high probability of default increased board independence after the 2005 reform, and decreased board independence after the 2006 reform. The data are the same as for the cross-country study. The time period is from 2002 to 2011 to study the ten years around the 2005–2006 reforms. For both reforms, based on the four-year average measure of default probability prior to the first reform (2002 to 2005) (thus, comparing the same set of firms over time), I divide firms into four quartiles and define high default probability firms as firms in the highest quartile of the bankruptcy probability distribution and low default probability firms as firms in the lowest quartile. The full sample consists of 53 firms, while the final sample, i.e., when only considering firms in the highest and lowest quartiles, consists of 217 firm-year observations from 27 firms (14 firms with high default probability and 13 firms with low default probability). Note that the sample size is relatively small, which is why the findings from Italy may be seen as supplementary evidence to the findings from Germany. Table A.1.6 in the Appendix reports summary statistics, separately for firms with high and low default probability, and for the periods before and after the reforms.

I test for the expected effects employing the difference-in-differences regression of equation (1.2) to each reform. For the 2005 reform, $Post_t$ equals one for the year after the reform was passed (2006), and zero for the years before the reform was passed (2002 to 2005), that is, I compare changes in board structure after the first reform was passed, but before the second reform was passed. For the 2006 reform, $Post_t$ equals one for the years after the reform was passed (2007 to 2011), and zero for the year before the reform was passed (2006), that is, I compare changes in board structure after the second reform relative to the year after the first reform.

Panels A and B of Table 1.5 respectively show the results for the 2005 and 2006 reforms. Confirming the cross-country findings, columns 1 and 2 show that firms with high default probability increased (decreased) the number of independent directors relative to firms with low probability of default after the 2005 (2006) reform. Also the economic effects are similar to those found in the cross-country analysis. Columns 3 and 4 present the results for the ratio of independents to board size as the outcome variable and support the findings. Finally, columns

5 and 6 show that firms with high default probability did not change board size relative to low default probability firms after the reforms.

As for the reform in Germany, Figure 1.2 shows changes in the number of independent directors for firms with high and low default probability over time. Most importantly, while high default probability firms changed the number of independents relative to low default probability firms after the reforms, they did not change board independence relative low default probability firms prior to the reforms. This is consistent with the assumption that both groups of firms would have followed the same trend without the reforms.⁵⁸

1.4.3 US Bankruptcy Reform Act of 1978

Probably the most prominent bankruptcy reform is the US Bankruptcy Reform Act of 1978, which became effective in October 1979. While the process of the reform was triggered by increasing dissatisfaction with the bankruptcy law (the law was considered unnecessarily complicated and intolerably ambiguous), the final outcome reflected the relative political power of competing interest groups, such as debtors, managers, creditors, lawyers, judges, and politicians. Ultimately, the reformation process resulted in a law that was created to facilitate debt renegotiation in bankruptcy (Posner, 1997; Hackbarth, Haselmann, and Schoenherr, 2015; Favara et al., 2017).

In particular, the reform made three major changes to debtor-in-possession reorganization under Chapter 11 (Hackbarth, Haselmann, and Schoenherr, 2015; Favara et al., 2017). First, it reduced the share of votes that are required for a reorganization plan to be accepted. Second, it introduced a cram down option that allows the court to impose reorganization if a plan cannot be agreed. Third, it lowered the conditions under which firms can voluntarily file for Chapter 11, i.e., the new law does not require a firm to be insolvent to file for reorganization (which

⁵⁸ As a response to corporate scandals such as the Parmalat scandal, and following worldwide board reforms aimed at improving corporate governance, simultaneously to the bankruptcy reforms, the Italian government passed board reforms in 2006 that require and encourage a certain level of board independence (Kim and Lu, 2013; Fauver et al., 2017). In particular, the board reforms in Italy require that firms have at least one independent director and furthermore encourage firms to form remuneration and control committees that are made up with a majority of independents, that is, recommending at least two independents. For my empirical analysis, the most important point here is that the findings should not be affected by the board reforms, given that the reforms apply to all firms, while I study changes in board structure of high default probability firms relative to those of low default probability firms (low default probability firms serve as a control group). A potential concern might be that the board reforms affected high and low default probability firms disproportionately, i.e., that the reforms were binding for one of the groups of firms, while they were not binding for the other group. However, note that in the year of the reforms, 2006, almost all of the sample firms had at least 2 independents (more precisely, only one firm had only one independent director), suggesting that the reforms were not binding for any of the considered groups. Finally, note that while the board reforms were aimed at increasing board independence, the bankruptcy reform of 2006 leads to the expectation that firms decrease board independence, suggesting that, if anything, the board reforms actually downward bias the results.

may enable distressed firms to get rid of debt obligations by strategically filing for Chapter 11, or to use the threat of voluntary Chapter 11 filing to achieve concessions, such as debt relief or less covenant restrictions, from creditors outside of bankruptcy⁵⁹). Overall, the reform reduced liquidation risk in distress by shifting bargaining power in reorganization to debtors, thereby reducing managerial bankruptcy penalties.

While data limitations of conventional board databases such as BoardEx prevent a more refined analysis of the reform, based on hand-collected data, among others (e.g., Hermalin and Weisbach, 1988; Lehn, Patro, and Zhao, 2009), Graham, Kim, and Leary (2020) provide detailed information on annual means of board structure of US firms around the reform that allow for event-study evidence. For the period 1920 to 2011, the database of the authors contains about 87,000 firm-year observations of NYSE/AMEX firms, largely hand-collected from Moody's Industrial Manuals.

Figure 1.3, which is a copy of Panel C of Figure 1 from Graham, Kim, and Leary (2020), shows average board structure from 1933 to 2011 (blue solid line). Additionally, the figure shows average regression residuals after controlling for several firm characteristics, including firm age, board size, book assets, Tobin's Q, industry-adjusted return on assets, and asset tangibility (red dashed line).⁶⁰ Consistent with the previous findings, and as described by the authors, "Board independence was stable at about 50% for four decades starting in the 1930s (...) [while] From the late 1970s to mid-1990s, average board independence increased to around 60%." (p. 618).⁶¹ Note that the large sample size suggests that the observed changes are statistically significant. Importantly, and to put this matter differently, the analysis here not only provides further support on the empirical relevance of debt governance, but also provides a natural explanation for the increase of independent directors in the US between the 1970s and 1990s, a puzzle that is often documented and discussed in the board literature but remains largely unresolved to this date.^{62,63}

⁵⁹ See Benmelech and Bergman (2008), who show that firms are able to renegotiate their contracted obligations downward outside of bankruptcy states when liquidation values are low.

⁶⁰ Note that permission to reprint Panel C of Figure 1 from Graham, Kim, and Leary (2020) is in general given by the fact that the paper by the authors is an open access article. Additionally, permission was also obtained by asking the publisher Elsevier directly. The granted permission can be shown upon request. See Graham, Kim, and Leary (2020) for a detailed description of data sources and sample construction.

⁶¹ Likewise, Hermalin and Weisbach (1988, p. 593) find in their sample: "Board composition has changed through time. A striking feature of our data is that between 1971 and 1983 there has been a substantial movement toward replacing insiders with outsiders. The average percentage of outsiders rose from 37.6% to 53.9%, while the average percentage of insiders fell from 49.1% to 34.3%."

⁶² For more on the puzzling rise of independents in the US, see, besides Hermalin and Weisbach (1988), also Gordon (2007).

⁶³ Note that while changes in board structure until the mid-90s may be caused by the bankruptcy reform, changes thereafter may be caused by other factors, such as international trends in corporate governance (Ferreira

1.4.4 Changes in board structure of firms in other countries

As noted, a potential concern with my results is that a confounding shock affected high and low default probability firms differently. The sticky feature of board structure, and the parallel trends between high and low default probability firms prior to the reform already mitigate this concern. In this subsection, I study changes in board structure of firms in other countries. Specifically, I examine whether in other countries firms with high default probability have changed board independence relative to firms with low default probability around the reforms in Germany and Italy. This analysis further addresses the concern of confounding events by controlling for the set of possible confounding shocks commonly experienced by all countries, such as economic crises or worldwide trends in corporate governance.

As control countries, I consider a group of countries that are similar to Germany and Italy in terms of economic and financial development. These countries include Austria, Belgium, France, Netherlands, and Switzerland.⁶⁴ Additionally, I also present the results for the US, which might be seen as the benchmark case for international shocks and trends on board independence. The empirical analysis is the same as for Germany and Italy. Specifically, for each of the control countries, I divide firms into four quartiles (based on the average Z-score from 2008–2011 regarding the German reform and based on the average Z-score from 2002–2005 regarding the Italian reforms) and define firms in the highest quartile as risky firms and firms in the lowest quartile as safe firms, and study changes in board structure for the ten years around the reforms.

Panel A of Table 1.6 presents the results regarding Germany's reform. The number of independent directors is the dependent variable in columns 1 (similar countries) and 4 (US). As can be seen, in the similar countries, firms with high default probability did not increase the number of independents relative to firms with low default probability after the reform in 2011. The same applies for the US. Columns 2 and 5 show that the findings are similar when using the ratio of independents to board size as the outcome variable. Finally, columns 3 and 6 show that firms with high default probability did not exhibit changes in board size after the reform relative to firms with low default probability in the control countries. In Figure 1.4, I plot changes in the number of independent directors for high and low default probability firms of the control countries over time. As shown, there were no changes in the number of independents

and Kirchmaier, 2013; Fauver et al., 2017) or NYSE/Nasdaq amendments and the Sarbanes-Oxley Act that both required and encouraged firms to increase board independence (Graham, Kim, and Leary, 2020).

⁶⁴ For Italy, Spain might be an additional option for a country with similar macroeconomic characteristics. For Germany, Nordic countries (including Denmark, Finland, Norway, and Sweden) might have similar macroeconomic characteristics. In unreported results, I include Spain and Nordic countries as control countries to the analyses of the German and Italian reforms and obtain similar results.

of firms with high default probability relative to firms with low default probability, both before and after the reform.

Panels B and C of Table 1.6 respectively report the results for the 2005 reform and 2006 reform in Italy. Most importantly, in the similar countries and in the US, firms with high default probability did not change the number of independents relative to firms with low default probability after the reforms in 2005 and 2006. Figure 1.5 shows that high and low default probability firms exhibited parallel trends in the number of independents, both before as well as after the reforms.⁶⁵

Overall, the results suggest that the findings in Germany and Italy are not affected by confounding shocks that were common to all countries. Ultimately, the results suggest that, to affect one of the difference-in-differences analyses, confounding events not only needed to have different effects on high and low default probability firms, but also needed to be country-specific. Note that such a country-specific shock appears unlikely, given that in all countries high and low default probability firms followed the same trend.

1.5 Alternative explanations and additional discussions

In the previous sections, I find a negative relation between board independence and bankruptcy penalties, and especially so for firms with high default probability. The results suggest that firms implement independent directors as a substitute when bankruptcy legislations that are soft on management make debt governance ineffective, thus providing empirical support on debt governance. In this section, I study the robustness of my findings with regard to alternative explanations. Additionally, I provide some discussions that may be helpful in gaining further insights into the findings of this paper.

1.5.1 Alternative explanations

1.5.1.1 Recovery rates of creditors

At a first sight, a concern with my findings might be that the negative relation between board independence and creditor rights may not be driven by the fact that firms implement

⁶⁵ In unreported results, I also directly compare changes in board independence of German and Italian firms to those of firms from other countries. Specifically, I compare risky firms from Germany and Italy to all firms from other countries as well as safe firms from Germany and Italy to all firms from other countries. I find that board independence of risky German and Italian firms increased (decreased) after the reforms relative to firms from other countries. In contrast, board independence of safe German and Italian firms in general did not change after the reforms relative to firms from other countries. Notably, also on average, i.e., when considering all firms, German and Italian firms changed board independence after the reforms relative to firms from other countries, implying changes in the independence gap across countries.

independents as a substitute when the absence of bankruptcy penalties makes debt governance ineffective, but may be driven by an alternative explanation that is based on creditors' recovery rates. Specifically, it might be that because creditors expect lower recovery rates when their rights in bankruptcy are weak, they demand independent directors in order to improve management quality and to thereby reduce the probability of default. Management, in turn, agrees to such a demand to raise capital by low credit costs.⁶⁶

A closer examination, however, suggests that this alternative story is unlikely to be relevant in my setting. From a theoretical perspective, there are at least two reasons for this. First, note that in each of the two bankruptcy regimes, i.e., creditor- or debtor-friendly, or, put differently, high or low recovery rates, management may bond itself to the optimal amount of independents (the amount after which an additional independent brings no more additional value on management quality and thus has no effect on credit costs) to ensure financing by low credit costs. In other words, in both regimes, i.e., regardless of creditors' recovery rates, management adds the same number of independents, since for both regimes this is the optimal amount of governance pressure and thus allows for the maximum amount of capital. The only reason why a shift from a creditor-friendly to a debtor-friendly regime may motivate management to increase independents is when such a shift changes the value of corporate governance, which is not given in the "recovery rates story", but central in the "governance substitution story" (additional independents restore management discipline that was previously accomplished by debt governance). Second, John and Litov (2010) argue that while good corporate governance may be desirable for stockholders by aligning the interests of managers with those of equityholders, it might be harmful for creditors by encouraging value-enhancing risk taking that leaves them with downside risk. As such, creditors may actually not be interested in good governance, but in contrast may consider a firm with weak governance as being less risky and hence provide better terms for debt financing.⁶⁷

Besides the theoretical inconsistencies, there are various pieces of empirical evidence that are inconsistent with the recovery rates story, while being consistent with the governance

⁶⁶ See, e.g., Davydenko and Franks (2008), Brockman and Unlu (2009), Vig (2013), and Rodano, Serrano-Velarde, and Tarantino (2016) for a discussion of lower recovery rates of creditors when creditor rights are weak. See Brockman and Unlu (2009), who argue that creditors may demand and managers agree to more restrictive firm policies, such as lower dividend payouts, when creditor rights are weak in order to reduce the potential losses of granting a loan and the costs of credit.

⁶⁷ Empirically, John and Litov (2010) find a positive relationship between weak corporate governance and leverage, consistent with their hypothesis that entrenched managers receiving better access to debt financing as a response to a more conservative investment policy that is aligned with creditors' interests (on this, see also Ji, Mauer, and Zhang, 2020). Additionally, Bradley and Chen (2015) find that board independence can increase the cost of debt and that independent directors typically set corporate policies that increase firm risk. The authors argue that their findings suggest that independent directors act in the interest of shareholders and are costly to bondholders with the intensification of agency problems between these two stakeholder groups.

substitution story. First, if the recovery rates story applies, we should see that the negative relation between board independence and the individual components of the creditor rights index is strongest for the provision that determines whether secured creditors are paid out first after the bankruptcy procedure (SECURED), as a direct proxy for creditors' expected recovery rates in bankruptcy. In contrast, as shown in Tables 1.2 and 1.3 in Section 1.3, the effects are weakest for this provision, while being strongest for provisions that imply strong management penalties in default (MANAGES, AUTOSTAY, REORG).

Second, while both stories have the same prediction of a negative relation between board independence and bankruptcy penalties, they have mutually exclusive predictions on the relation between leverage and board independence. The governance substitution story predicts that higher leverage reduces board independence (for levered firms, debt governance matters). In contrast, the recovery rates story predicts that higher leverage increases board independence (for levered firms, banks demand more independent directors to avoid default). Table 1.2 in Section 1.3.1 shows the relation between leverage and board independence. As shown, an increase in leverage is related to a reduction in board independence. This result is inconsistent with the recovery rates story, while being consistent with the governance substitution story.⁶⁸

Third, in ongoing research, Canipek (2023a) studies the relation between agency problems and bankruptcy penalties, and finds that agency problems are negatively related to bankruptcy penalties. This finding is consistent with the governance substitution story, which is based on the notion that debt governance mitigates agency problems. In contrast, the recovery rates story cannot explain the negative relation. In fact, the recovery rates story predicts a positive relation between agency problems and bankruptcy penalties, as stronger creditor rights reduce creditors' demand for good corporate governance.

1.5.1.2 Changes in other firm characteristics

Another alternative explanation for the negative relation is possible changes in other firm characteristics. The bankruptcy literature suggests that creditor rights can change credit costs and distress costs (Acharya, Amihud, and Litov, 2011; Vig, 2013; Rodano, Serrano-Velarde, and Tarantino, 2016; Mann, 2018; Canipek, Kind, and Wende, 2021; Schoenherr and Starmans,

⁶⁸ Similar to this, in unreported results, I find that an increase in default probability (based on Altman's Z-score) is related to a reduction in board independence, and only so in countries with high creditor rights (see also Maier and Yurtoglu, 2022, who find a negative relation between bankruptcy risk and board independence). This result is consistent with the governance substitution story, which predicts that higher default probability decreases board independence, and only so in countries with strict management penalties (when debt governance is effective). The result cannot be explained by the recovery rates story, which can only explain a positive relation between bankruptcy risk and board independence but not a negative one.

2021). Such changes may affect important firm characteristics, such as corporate leverage, investments, risk taking, or profitability, which in turn may affect board structure. However, it is important to note that this concern is mitigated by the fact that I control for a large number of firm characteristics that have been found to be affected by creditor rights, and that have been shown to be possible determinants of board structure. Additionally, it should be noted that the economic significance of the effects of important firm characteristics on board independence is low in essentially all regressions in the board literature (Ferreira, Ferreira, and Mariano, 2018). For example, Boone et al. (2007) and Ferreira, Ferreira, and Raposo (2011) show that a one-standard-deviation change in firm size, number of business segments, and corporate leverage is related to a 1.8, 1.3, and 1.2 percentage point change in board independence, respectively. This suggests that possible changes in firm characteristics should not explain the observed large economic effect between board structure and creditor rights.⁶⁹

1.5.2 Additional discussions

1.5.2.1 Governance role of bankruptcy penalties at earlier levels

Bankruptcy penalties may not only present the lower point of managerial inefficiency, but may also play a governance role at earlier levels (Hart and Moore, 1995; Hart, 2001, 2017). As mentioned in the Introduction, besides the threat of bankruptcy penalties, there are also other mechanisms through which debt can induce an incentive effect on management quality such as forcing management to disgorge free cash flow that might otherwise have been used for making unprofitable empire building investments (Jensen, 1986; Stulz, 1990; Hart and Moore, 1995) or the threat of managerial restrictions on financial and investment policy following covenant violation (Chava and Roberts, 2008; Nini, Smith, and Sufi, 2009, 2012; Roberts and Sufi, 2009a, 2009b; Roberts, 2015; Bharath and Hertzfel, 2019). Importantly, while bankruptcy risk is not necessary to validate the incentive effect of these mechanisms, bankruptcy penalties may be important by making them more effective. For example, Hart and Moore (1995) emphasize that it is actually “hard” debt that curbs overinvestment by its nonpostponable nature such that obligations are effectively enforced when they are due. Similarly, bankruptcy law should be the basis in the renegotiations following a covenant violation by determining the bargaining power between creditors and managers. Whereas creditor-friendly bankruptcy procedures give creditors a credible threat (by high liquidation values for creditors and high bankruptcy costs for management) to put the company into bankruptcy when denying the demanded amendments,

⁶⁹ Also note that there is no obvious reason how possible changes in the firm characteristics should explain the negative relation between agency problems and creditor rights found by Canipek (2023a).

more management-friendly laws improve managers' bargaining position (in fact, management can default strategically, with automatic stay provisions preventing foreclosures and lowering creditors' liquidation value) and thus reduce the extent of new restrictions that creditors can renegotiate.⁷⁰

Note that the possibility that bankruptcy penalties induce an incentive effect at earlier levels provides a natural explanation for the negative relation between board independence and bankruptcy penalties among safe firms documented in Section 1.3.2 (at least for the full sample). Additionally, note that, provided that governance by covenants or by the limitation of funds affects risky and safe firms equally,⁷¹ one might interpret the difference in the negative relation between board independence and bankruptcy penalties across risky and safe firms as the result of the direct incentive effect that managerial bankruptcy penalties have on management quality (for which default risk is relevant), while the negative relation among safe firms might be interpreted as the result of the indirect incentive effect that managerial bankruptcy penalties produce by making the other mechanisms (for which default risk is not relevant) more effective.

1.5.2.2 Relevance of bankruptcy penalties in the presence of covenants

From a theoretical perspective, a large volume of studies shows that the threat of bankruptcy penalties can mitigate agency problems. Yet, a concern with these theories might be whether debt governance is practically relevant in the presence of covenants. As outlined, covenant violations can trigger tighter restrictions on corporate financial and investment policy.⁷²

⁷⁰ For renegotiation-based incomplete contracting models predicting that liquidation values affect renegotiation and determine the bargaining power between creditors and debtors, see Berglöf and von Thadden (1994), Bolton and Scharfstein (1996), Hart and Moore (1998), and Benmelech and Bergman (2008). Empirically, Benmelech and Bergman (2008) show that firms can renegotiate their contracted obligations downward (in and outside of bankruptcy states) when liquidation values are low. In addition, Davydenko and Franks (2008) show that recovery rates of secured creditors are positively correlated with their rights in bankruptcy. Specifically, the recovery rates in the UK (CRIGHTS=4), Germany (3), and France (0) are 94%, 72%, and 57%, respectively.

⁷¹ It seems unclear whether governance by covenants or by the limitation of funds affects risky and safe firms equally or differently. For example, on the one hand, governance through covenants may be particularly relevant for risky firms because here violations may occur more often or banks may be more interested to intervene and thus press for tighter restrictions after violations. However, on the other hand, Billett, King, and Mauer (2007) find that covenant protection is less present in risky firms (consistent with the notion that covenants restricting financing flexibility are especially costly for risky firms, as risky firms may need to borrow additional funds and/or restructure existing liabilities to survive (McDaniel, 1986; Begley, 1994; Billett, King, and Mauer, 2007)), suggesting that the governance role of covenants may be more relevant for safe firms.

⁷² Note that in case of covenant violation, firms are typically not unable to repay the loan (Nini, Smith, and Sufi, 2012). Also note that there are a number of different scenarios that may occur when a firm violates a covenant. For example, the firm just repays the loan (and may take out a new loan), creditors allow a covenant waiver, creditors impose tighter constraints, or bankruptcy is declared (either strategically if the firm is able to repay the loan but wants to get rid of obligations or forced if the firm is unable to repay the loan).

As a result, covenants can serve as an upstream disciplining mechanism in comparison to debt governance, possibly making the incentive effect of debt governance irrelevant.

However, bankruptcy law should be critical for at least two reasons. First, while covenant violations may put restrictions on management regarding financial and investment decisions and thus may give management incentives to avoid violation, they virtually never lead to firm liquidation or management dismissal. For example, Nini, Smith, and Sufi (2012) show that only about 1% of violations are associated with a resolution by bankruptcy and/or liquidation. Additionally, they show that, despite violating firms exhibit several quarters of declines in operating performance and market valuation before violation, the probability of a forced CEO turnover is only about 1.5% during the four quarters before the violation and only about 2.5% during the four quarters after the violation.⁷³ The threat of management dismissal and firm liquidation should present the most dominant incentive effect on management to perform (Hart, 1995), and should not be made irrelevant by possible restrictions after covenant violation.

Second, as noted above, bankruptcy law may not only define the lower point of managerial inefficiency, but may also affect management at earlier levels by influencing the effectiveness of covenants and other governance mechanisms for which default risk is not necessary to discipline management. Overall, whether the governance role of bankruptcy penalties is practically relevant or not is ultimately an empirical question. Addressing this question is the main goal of this paper.

1.5.2.3 Endogeneity of corporate governance structure

In studying the interaction between alternative governance mechanisms, my study follows the notion that governance mechanisms are endogenously determined within the broader system of a firm's governance structure. That is, firms substitute between different governance mechanisms in forming an optimal governance structure in which the demand for one governance mechanism depends on the strength of alternative governance mechanisms (Demsetz and Lehn, 1985; Hermalin and Weisbach, 2003, 2012; Adams, Hermalin, and Weisbach, 2010; Avedian, Cronqvist, and Weidenmier, 2015; Bharath and Hertz, 2019).⁷⁴ Two points are

⁷³ Nini, Smith, and Sufi (2012) show that whereas changes in the investment and financing behavior of violators coincide with the renegotiated credit agreements that include stricter restrictions, changes in the management of violators can occur by informal influence, e.g., creditors suggest actions to the board that can be made to increase the probability of receiving a covenant waiver.

⁷⁴ For studies that provide empirical evidence consistent with the idea that firms substitute between independent boards and alternative governance mechanisms, see, e.g., Ferreira, Ferreira, and Raposo (2011), who support the governance role of stock price informativeness by finding a substitution effect between price informativeness and board independence; Gillan, Hartzell, and Starks (2006), who find that board independence can be a substitute for the market for corporate control; and Coles, Lemmon, and Wang (2008), who report a negative relation between board independence and equity-based compensation. For empirical evidence consistent with a

worth noting in this context. First, in contrast to the endogeneity theory, it might be that board independence and debt governance are complements rather than substitutes (see, e.g., Doidge, Karolyi, and Stulz, 2007; Aggarwal et al., 2009, who argue that some governance mechanisms, such as strong legal enforcement, may complement others). However, there is no obvious reason why this should be the case. For board independence to induce discipline, debt governance is not needed. Likewise, for debt governance to be effective, board independence is not needed. Additionally, note that both stories have mutually exclusive predictions, suggesting that if board independence and debt governance are compliments, we should actually see a positive relation between them and not a negative relation. Second, endogeneity of corporate governance structure suggests that the hypothesis that debt mitigates agency problems cannot be rejected if there is no negative relation between board independence and bankruptcy penalties, given that firms may have substituted debt governance with other governance mechanisms than independent directors, such as incentive compensation, takeover provisions, or outside block-holding. Conversely, the finding that firms implement independent directors when debt governance becomes ineffective presents a strong case that debt is valued as a governance device.

1.5.2.4 Debt governance and firm value as an outcome variable

I evaluate the empirical relevance of debt to mitigate agency problems by studying the relation between debt governance and board independence, and not the relation between debt governance and firm value. Most studies evaluate the empirical relevance of a governance mechanism by looking at changes in the firm value, given that potential changes in agency problems should be reflected in the valuation of the firm (e.g., Grossman and Hart, 1982; Zwiebel, 1996; Harvey, Lins, and Roper, 2004; Fauver et al., 2017). However, in the setting of debt governance this approach is not very meaningful. The reason is that bankruptcy law not only affects the effectivity of debt governance, but may also affect credit costs and distress costs.⁷⁵ As a result, possible changes in the firm value may not come through a change in debt governance, but may come through changes in the other two variables. Studying the relation of

substitution effect among various other governance mechanisms, see, e.g., La Porta et al. (1998), who find that companies in countries with poor shareholder protection have more concentrated ownership; Bharath and Hertzl (2019), who provide evidence that ex post creditor governance by bank borrowing can substitute for pressure from the takeover market and product market; and Giroud and Mueller (2010), who find a substitution effect between competition and the takeover market.

⁷⁵ As suggested by the bankruptcy literature (Acharya, Amihud, and Litov, 2011; Vig, 2013; Rodano, Serano-Velarde, and Tarantino, 2016; Mann, 2018; Canipek, Kind, and Wende, 2021; Schoenherr and Starman, 2021), stronger creditor rights can reduce credit costs through higher collateral value and thus may increase firm value by allowing firms to increase financing and investments. In contrast, stronger creditor rights can also increase distress costs and thus may reduce firm value by prompting firms to lower leverage and undertake risk-reducing but unprofitable investments.

debt governance to board independence gets around this problem, and largely mitigates the problem of alternative stories (see above).

1.6 Conclusions

A large body of theoretical research suggests that debt can mitigate agency problems. Yet, empirical evidence is limited due to identification problems. In this paper, I attempt to provide evidence. To do so, I examine whether firms implement independent directors as a substitute when debt governance becomes ineffective. Key to my analysis are two central features of debt as a governance device. First, it is only effective with bankruptcy penalties, second, it only matters for firms close to default (Grossman and Hart, 1982; Aghion, Hart, and Moore, 1992; Hart, 1995, 2000, 2001; Zwiebel, 1996). Consistent with debt inducing discipline, across countries, board independence is negatively related to bankruptcy penalties, and especially so among firms with high default probability. For these firms, a change from strictest to softest penalties is associated with a 46% increase in the number of independent directors, suggesting that debt is valued as a powerful source of discipline. Comparing board independence of high and low default probability firms around bankruptcy reforms confirms the cross-country results.

My findings have many important implications. First, my findings emphasize the role of agency problems as an important determinant of corporate capital structure. While the notion that debt is implemented to solve agency problems is theoretically comprehensively established (e.g., Grossman and Hart, 1982; Zwiebel, 1996), empirical evidence is scarce. Although my analysis does not directly consider financing decisions, the documented importance of debt to mitigate agency problems suggests that this aspect plays an important role in firms' capital structure. Second, my findings can account for several unresolved issues in corporate governance. One example is that debt governance can help to understand the mixed empirical evidence on the effectivity of other governance mechanisms. Whereas improvements in corporate governance may have a positive effect for firms in which debt governance is unavailable (or samples in which such firms are more present), they may have no effect for firms in which debt governance already mitigates agency problems.⁷⁶ Another example is that debt governance can be key in understanding the substantial cross-sectional variation in corporate governance structure. In my study, I find that debt governance can explain large parts of the variation in corporate

⁷⁶ Similarly, Giroud and Mueller (2010, 2011) provide evidence that firms in noncompetitive industries benefit more from good governance relative to firms in competitive industries where competitive pressure already enforces discipline on managers. For the notion that improvements in corporate governance may have a different effect for firms with weak and strong governance, see, besides Giroud and Mueller (2010, 2011), Bertrand and Mullainathan (2003) and Nini, Smith, and Sufi (2012).

board structure (e.g., my results can explain about 37% of the difference in board structure between the US and the UK, or provide an explanation for the increase of independent directors in the US between the 1970s and 1990s). It seems likely that debt governance also plays a similarly important role in shaping the use of other governance mechanisms, such as incentive compensation, corporate takeover provisions, or outside block-holding (see the theory on endogeneity of corporate governance structure). I look forward to seeing research on this.

Finally, and probably most important, my study has important implications for optimal bankruptcy design. In particular, my findings present empirical evidence consistent with the notion that bankruptcy law should preserve the governance function of debt by penalizing management adequately in bankruptcy states. A law that fails to do so, has the effect that management may have difficulties to credibly commit to forgo inefficient actions and thus to secure financing *ex ante* (Aghion, Hart, and Moore, 1992; Hart, 2000).⁷⁷ It is important to note that this aspect is not considered by the current bankruptcy literature. Moreover, it is worth noting that while my findings are important in general, they may be particularly relevant in the context of the recent crises during which many countries have significantly softened their bankruptcy law.

⁷⁷ As noted earlier, stronger creditor rights may also be costly by increasing distress costs and thus prompting firms to reduce financing (Acharya, Amihud, and Litov, 2011; Vig, 2013; Mann, 2018; Canipek, Kind, and Wende, 2021; Schoenherr and Starmans, 2021). However, note that a debtor-friendly law is not necessarily incompatible with the “debt governance channel”. One possibility to account for the channel in a debtor-friendly law is by including provisions that make bankruptcy penalties contingent on managerial misconduct (which can be determined, for example, by the performance of comparable firms). Consistent with this, for some firm types, like unlisted large firms, the new debtor-friendly law in Korea stipulates that the incumbent management may not be allowed to stay in control during bankruptcy if insolvency was caused by material mismanagement (Park, 2008).

Tables and figures for Chapter 1

Table 1.1

Summary statistics

This table presents summary statistics by country. The columns show: column 1, average of the number of independent directors; column 2, average of the within-firm absolute growth in the number of independent directors per year (e.g., the average firm increases the number of independent directors by 0.12 per year, i.e., by 1.2 over ten years); column 3, average of the within-firm number of changes in the number of independent directors per year (e.g., the average firm changes the number of independent directors 0.37 times per year, i.e., every 2.7 years); column 4, average of the within-firm standard deviation in the number of independent directors; column 5, average of the ratio of the number of independents to board size; column 6, average of the within-firm absolute growth in the ratio of the number of independents to board size per year; column 7, average of the within-firm number of changes in the ratio of the number of independents to board size per year; column 8, average of the within-firm standard deviation in the ratio of independents to board size; column 9, average of board size; column 10, creditor rights index; column 11, average of Altman's Z-score; column 12, average of the standard deviation of Altman's Z-score; column 13, number of observations; and column 14, number of firms. Refer to Table A.1.1 in the Appendix for variable definitions.

	Number of independent directors				Ratio of independents to board size				Board size (9)	Creditor rights (10)	Altman's Z-score		Observations (13)	Number of firms (14)
	Mean (1)	Within-firm			Mean (5)	Within-firm					Mean (11)	SD (12)		
		Yearly growth (2)	Yearly changes (3)	SD (4)		Yearly growth (6)	Yearly changes (7)	SD (8)						
Argentina	4.60	0.383	0.467	1.142	0.470	0.021	0.617	0.085	9.79	1	3.94	6.44	70	10
Australia	3.57	0.088	0.383	0.631	0.592	0.012	0.514	0.088	5.92	3	6.50	11.31	4,146	670
Austria	6.54	0.137	0.309	0.954	0.487	0.012	0.465	0.075	12.71	3	2.93	2.44	292	36
Belgium	4.05	0.030	0.340	0.704	0.445	0.007	0.515	0.078	9.32	2	3.68	5.28	668	76
Brazil	2.99	0.085	0.378	0.781	0.378	0.013	0.558	0.086	8.21	1	2.46	3.23	512	89
Canada	6.09	0.102	0.447	0.732	0.727	0.011	0.528	0.059	8.30	1	3.01	6.46	4,084	657
Chile	2.10	-0.036	0.270	0.373	0.266	-0.005	0.387	0.050	8.07	2	2.43	1.11	131	20
China	3.57	-0.006	0.152	0.273	0.442	0.003	0.333	0.034	8.52	2	3.31	3.96	1,162	193
Colombia	4.20	0.243	0.514	0.709	0.552	0.026	0.486	0.071	7.63	0	2.41	1.49	46	9
Croatia	0.95	0.118	0.059	0.316	0.067	0.008	0.471	0.023	14.26	3	4.47	2.20	19	2
Czech Republic	0.70	-0.056	0.222	0.823	0.035	-0.002	0.333	0.043	17.40	3	3.38	1.64	20	2
Denmark	4.02	0.183	0.359	0.999	0.410	0.014	0.618	0.099	10.51	3	5.41	5.36	341	35
Egypt	1.70	0.143	0.143	0.363	0.180	0.015	0.286	0.035	10.63	2	2.59	2.16	27	6
Finland	6.22	0.100	0.428	0.786	0.854	0.014	0.346	0.061	7.36	1	3.18	2.04	482	54
France	3.64	0.163	0.340	0.780	0.345	0.016	0.509	0.079	9.98	0	2.63	3.14	3,400	430
Germany	1.77	0.175	0.147	0.624	0.134	0.011	0.282	0.049	13.39	3	3.39	4.17	2,299	290
Greece	2.94	0.049	0.192	0.493	0.321	0.004	0.304	0.048	9.91	1	2.23	3.18	320	34
Hong Kong	3.75	0.060	0.194	0.272	0.394	0.009	0.418	0.031	9.88	4	4.18	6.35	1,697	373
Hungary	3.43	0.136	0.227	0.467	0.417	0.022	0.364	0.049	9.50	1	3.34	2.75	30	8
India	4.72	0.033	0.408	0.564	0.507	0.006	0.559	0.047	9.36	2	5.01	5.86	1,467	406
Indonesia	2.51	-0.018	0.224	0.361	0.219	-0.003	0.606	0.032	11.67	2	4.24	5.19	218	53
Ireland	5.02	0.067	0.417	1.006	0.527	0.010	0.552	0.082	9.12	1	3.72	6.48	564	68
Israel	3.93	0.080	0.275	0.674	0.534	0.014	0.445	0.079	7.55	3	3.39	5.83	825	99
Italy	4.78	0.136	0.334	0.921	0.448	0.011	0.475	0.077	10.77	2	2.38	2.62	934	113
Japan	1.97	0.335	0.309	0.627	0.200	0.032	0.551	0.063	10.81	2	3.43	3.44	1,795	386
Kenya	5.90	0.235	0.235	1.526	0.544	0.024	0.471	0.156	10.70	4	3.33	3.89	20	3
Malaysia	3.78	0.050	0.282	0.417	0.494	0.005	0.419	0.044	7.82	3	4.16	5.45	700	144
Mexico	5.67	-0.004	0.318	0.585	0.494	0.005	0.500	0.044	11.59	0	3.78	4.60	284	48

Morocco	0.25	0.000	0.000	0.000	0.026	0.000	0.143	0.003	12.62	1	4.04	2.29	16	2
Netherlands	4.48	0.146	0.373	0.783	0.532	0.016	0.527	0.092	8.36	3	2.88	3.73	921	115
New Zealand	5.00	0.109	0.429	0.524	0.726	0.020	0.440	0.054	6.91	4	5.01	8.34	223	48
Nigeria	2.48	0.328	0.358	0.787	0.237	0.031	0.642	0.080	10.11	4	3.53	2.41	85	18
Norway	3.07	0.203	0.325	1.013	0.427	0.026	0.400	0.135	7.35	2	3.36	5.10	802	97
Pakistan	1.00	0.000	0.000	0.000	0.101	0.000	0.000	0.000	10.50	1	1.07	0.43	4	2
Panama	2.92	0.182	0.273	0.515	0.270	0.015	0.364	0.042	10.75	4	2.65	0.58	12	1
Peru	3.58	-0.053	0.474	0.548	0.445	-0.003	0.526	0.061	8.25	0	2.36	2.41	24	5
Philippines	2.82	0.000	0.158	0.284	0.274	0.001	0.250	0.027	10.41	1	2.94	3.49	147	27
Poland	2.62	0.179	0.348	0.916	0.202	0.010	0.589	0.074	12.88	1	3.05	1.81	137	25
Portugal	2.80	0.110	0.278	0.892	0.235	0.007	0.498	0.073	10.65	1	1.46	1.15	296	33
Russia	3.54	-0.014	0.466	0.944	0.346	-0.001	0.591	0.095	10.38	2	3.48	4.05	328	49
Singapore	3.92	0.003	0.252	0.394	0.519	0.002	0.416	0.047	7.54	3	2.89	4.34	1,064	200
South Africa	5.40	0.174	0.460	0.826	0.528	0.017	0.622	0.071	10.15	3	4.18	3.84	1,062	166
South Korea	2.48	-0.049	0.262	0.500	0.300	-0.007	0.390	0.057	8.05	3	3.41	2.94	210	46
Spain	4.22	0.082	0.380	0.792	0.369	0.009	0.581	0.073	11.72	2	2.35	3.36	881	110
Sweden	4.60	0.238	0.413	1.115	0.537	0.028	0.543	0.130	8.91	1	4.74	6.40	1,391	149
Switzerland	3.16	0.149	0.261	0.600	0.388	0.018	0.331	0.071	7.61	1	4.53	4.49	1,027	142
Thailand	5.93	0.009	0.346	0.464	0.443	-0.000	0.477	0.032	13.25	2	4.26	5.92	140	33
Turkey	2.60	0.135	0.177	0.300	0.279	0.012	0.271	0.032	9.61	2	2.82	2.51	121	25
United Kingdom	2.65	0.052	0.301	0.544	0.375	0.009	0.467	0.077	6.59	4	3.61	6.90	12,595	1,680
United States	6.18	0.140	0.425	0.821	0.746	0.014	0.516	0.068	8.20	1	4.13	7.03	38,874	4,678
Mean/Total	4.77	0.119	0.370	0.700	0.576	0.013	0.494	0.068	8.35	1.84	3.91	6.64	86,913	11,965

Table 1.2**Board independence and bankruptcy penalties**

This table presents estimates of OLS regressions. The dependent variable is the logarithm of the number of independent directors (plus one) in Panel A, the ratio of the number of independent directors to board size in Panel B, and the logarithm of board size in Panel C. Columns 1 to 6 present the results for the full sample. Column 7 presents the results when US firms are excluded, and column 8 shows the results when US and UK firms are excluded. *t*-values are reported in parentheses and are based on standard errors clustered at the country level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.1.1 in the Appendix for variable definitions.

	All countries						Exclude US	Exclude US & UK
	(1)	(2)	(3)	(4)	(5)	(6)		
CRIGHTS	-0.201*** (-3.75)	-0.085*** (-4.21)					-0.072*** (-3.64)	-0.052** (-2.06)
AUTOSTAY			-0.142** (-2.27)					
REORG				-0.191*** (-3.47)				
SECURED					-0.071 (-0.66)			
MANAGES						-0.173*** (-3.19)		
Size		0.116*** (6.74)	0.117*** (6.67)	0.118*** (6.54)	0.120*** (6.28)	0.118*** (6.51)	0.139*** (8.92)	0.116*** (8.32)
Leverage		-0.101*** (-2.90)	-0.097*** (-2.82)	-0.099*** (-2.83)	-0.095*** (-2.77)	-0.104*** (-2.88)	-0.134*** (-3.24)	-0.102** (-2.20)
Return on assets		-0.104 (-1.27)	-0.104 (-1.26)	-0.094 (-1.06)	-0.087 (-0.93)	-0.089 (-0.98)	-0.002 (-0.02)	-0.128 (-1.14)
Return volatility		-0.064*** (-3.00)	-0.079*** (-3.30)	-0.066*** (-2.98)	-0.085*** (-3.56)	-0.068*** (-3.08)	-0.074** (-2.53)	-0.076 (-1.58)
Market-to-book		0.011* (1.98)	0.011* (1.97)	0.012** (2.02)	0.013* (2.00)	0.012* (2.00)	0.021*** (3.60)	0.013** (2.39)
Free cash flow		0.038 (0.63)	0.034 (0.56)	0.027 (0.39)	0.017 (0.24)	0.023 (0.34)	-0.012 (-0.15)	0.091 (1.03)
Insider ownership		-0.370*** (-6.90)	-0.374*** (-7.25)	-0.369*** (-6.92)	-0.391*** (-6.84)	-0.379*** (-6.83)	-0.450*** (-7.20)	-0.493*** (-7.14)
SHRIGHTS		-0.162*** (-4.22)	-0.190*** (-4.90)	-0.174*** (-4.57)	-0.226*** (-5.18)	-0.167*** (-4.91)	-0.177*** (-4.23)	-0.164*** (-3.86)
Rule of law		-0.020 (-0.23)	-0.044 (-0.48)	0.002 (0.02)	-0.004 (-0.04)	-0.059 (-0.68)	-0.020 (-0.23)	-0.016 (-0.19)
English legal origin		0.612*** (4.36)	0.547*** (3.94)	0.618*** (4.95)	0.613*** (3.88)	0.524*** (3.53)	0.653*** (4.52)	0.570*** (4.22)
French legal origin		0.040 (0.29)	0.039 (0.28)	0.117 (0.81)	0.082 (0.56)	0.037 (0.25)	0.062 (0.45)	0.033 (0.24)
German legal origin		-0.407*** (-3.13)	-0.511*** (-4.39)	-0.410*** (-3.37)	-0.445*** (-3.19)	-0.467*** (-3.60)	-0.441*** (-3.34)	-0.507*** (-4.23)
Nordic legal origin		0.255 (1.23)	0.314 (1.53)	0.245 (1.15)	0.360 (1.64)	0.181 (0.77)	0.288 (1.37)	0.260 (1.22)
GDP per capita		-0.047 (-0.89)	-0.071 (-1.37)	-0.038 (-0.71)	-0.066 (-1.14)	-0.000 (-0.01)	-0.037 (-0.70)	-0.043 (-0.80)
Market cap		-0.018 (-0.90)	0.004 (0.16)	-0.017 (-0.75)	0.005 (0.20)	-0.012 (-0.55)	-0.008 (-0.24)	0.011 (0.30)
Year fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,913	86,913	86,913	86,913	86,913	86,913	48,039	35,444
Number of countries	50	50	50	50	50	50	49	48
R ²	0.171	0.534	0.528	0.532	0.523	0.530	0.418	0.377

(continued)

Table 1.2 (continued)

Panel B. Dependent variable: ratio of the number of independent directors to board size								
	All countries						Exclude US (7)	Exclude US & UK (8)
	(1)	(2)	(3)	(4)	(5)	(6)		
CRIGHTS	-0.093*** (-2.92)	-0.045*** (-4.86)					-0.039*** (-4.01)	-0.030** (-2.27)
AUTOSTAY			-0.097*** (-4.83)					
REORG				-0.074*** (-2.76)				
SECURED					-0.020 (-0.46)			
MANAGES						-0.095*** (-3.95)		
Size		0.020*** (3.17)	0.020*** (3.21)	0.021*** (3.04)	0.021*** (2.96)	0.021*** (3.07)	0.026*** (3.50)	0.015*** (2.93)
Leverage		-0.040*** (-4.66)	-0.038*** (-4.42)	-0.039*** (-4.08)	-0.038*** (-3.80)	-0.042*** (-4.60)	-0.034** (-2.07)	-0.022 (-1.18)
Return on assets		0.003 (0.08)	0.000 (0.01)	0.010 (0.23)	0.013 (0.28)	0.011 (0.25)	0.067** (2.34)	0.031 (0.85)
Return volatility		-0.022*** (-2.91)	-0.029*** (-3.58)	-0.026*** (-3.20)	-0.033*** (-4.26)	-0.024*** (-3.01)	-0.020* (-1.81)	-0.011 (-0.42)
Market-to-book		0.001 (0.62)	0.001 (0.41)	0.002 (0.85)	0.002 (0.89)	0.002 (0.80)	0.004 (1.65)	0.001 (0.23)
Free cash flow		-0.015 (-0.51)	-0.014 (-0.52)	-0.023 (-0.68)	-0.027 (-0.77)	-0.023 (-0.68)	-0.052* (-1.88)	-0.015 (-0.52)
Insider ownership		-0.230*** (-14.68)	-0.230*** (-16.87)	-0.232*** (-13.15)	-0.241*** (-12.23)	-0.235*** (-13.72)	-0.246*** (-10.04)	-0.266*** (-10.30)
SHRIGHTS		-0.090*** (-5.41)	-0.101*** (-7.17)	-0.103*** (-6.38)	-0.123*** (-6.05)	-0.092*** (-5.65)	-0.092*** (-5.20)	-0.086*** (-5.10)
Rule of law		0.021 (0.54)	0.005 (0.14)	0.029 (0.65)	0.025 (0.56)	-0.001 (-0.01)	0.023 (0.64)	0.025 (0.68)
English legal origin		0.224*** (3.61)	0.186*** (3.38)	0.220*** (3.47)	0.213*** (2.93)	0.177*** (2.97)	0.233*** (3.83)	0.196*** (3.47)
French legal origin		-0.098* (-1.73)	-0.106* (-1.94)	-0.062 (-0.82)	-0.075 (-1.04)	-0.100* (-1.74)	-0.088 (-1.54)	-0.100* (-1.85)
German legal origin		-0.220*** (-3.59)	-0.278*** (-5.76)	-0.232*** (-3.68)	-0.250*** (-3.59)	-0.251*** (-4.48)	-0.225*** (-3.63)	-0.251*** (-4.35)
Nordic legal origin		0.058 (0.70)	0.088 (1.10)	0.064 (0.65)	0.105 (1.09)	0.017 (0.19)	0.067 (0.84)	0.056 (0.70)
GDP per capita		-0.010 (-0.40)	-0.025 (-1.07)	-0.007 (-0.25)	-0.017 (-0.54)	0.016 (0.61)	-0.007 (-0.30)	-0.010 (-0.43)
Market cap		-0.010 (-1.21)	0.001 (0.16)	-0.006 (-0.56)	0.003 (0.32)	-0.007 (-0.80)	-0.009 (-0.78)	-0.000 (-0.02)
Year fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,913	86,913	86,913	86,913	86,913	86,913	48,039	35,444
Number of countries	50	50	50	50	50	50	49	48
R ²	0.188	0.575	0.573	0.566	0.559	0.571	0.398	0.422

(continued)

Table 1.2 (continued)

Panel C. Dependent variable: logarithm of board size								
	All countries						Exclude US (7)	Exclude US & UK (8)
	(1)	(2)	(3)	(4)	(5)	(6)		
CRIGHTS	-0.055*	-0.027					-0.012	0.002
	(-1.80)	(-1.54)					(-0.57)	(0.07)
AUTOSTAY			-0.048					
			(-1.05)					
REORG				-0.080				
				(-1.33)				
SECURED					-0.082			
					(-1.42)			
MANAGES						-0.019		
						(-0.33)		
Size		0.107***	0.107***	0.107***	0.108***	0.108***	0.118***	0.115***
		(13.27)	(13.45)	(13.12)	(12.92)	(12.85)	(18.57)	(14.49)
Leverage		-0.039	-0.037	-0.038	-0.036	-0.038	-0.088***	-0.081**
		(-1.37)	(-1.34)	(-1.37)	(-1.33)	(-1.37)	(-3.60)	(-2.43)
Return on assets		-0.147***	-0.147***	-0.145***	-0.143***	-0.141***	-0.137**	-0.167***
		(-5.20)	(-5.55)	(-5.06)	(-4.76)	(-4.73)	(-2.61)	(-2.74)
Return volatility		-0.023	-0.028	-0.022	-0.029	-0.028	-0.041*	-0.073**
		(-1.43)	(-1.46)	(-1.51)	(-1.61)	(-1.63)	(-1.90)	(-2.58)
Market-to-book		0.013***	0.013***	0.014***	0.014***	0.014***	0.018***	0.012***
		(3.20)	(3.30)	(3.15)	(3.08)	(3.09)	(3.61)	(2.82)
Free cash flow		0.078***	0.077***	0.076***	0.073***	0.072***	0.074	0.092
		(3.12)	(3.48)	(2.99)	(2.94)	(2.83)	(1.55)	(1.53)
Insider ownership		0.034	0.033	0.036	0.025	0.029	0.050	0.008
		(1.23)	(1.14)	(1.35)	(0.91)	(1.05)	(1.33)	(0.26)
SHRIGHTS		0.008	-0.000	0.009	-0.017	-0.005	0.033	0.037
		(0.29)	(-0.02)	(0.30)	(-0.65)	(-0.16)	(1.08)	(1.20)
Rule of law		0.022	0.014	0.031	0.043	0.017	0.037	0.035
		(0.28)	(0.17)	(0.44)	(0.54)	(0.21)	(0.45)	(0.43)
English legal origin		0.025	0.004	0.033	0.064	0.006	0.021	0.010
		(0.20)	(0.03)	(0.27)	(0.50)	(0.05)	(0.16)	(0.08)
French legal origin		0.068	0.066	0.095	0.076	0.077	0.108	0.103
		(0.56)	(0.53)	(0.83)	(0.65)	(0.65)	(0.88)	(0.87)
German legal origin		0.113	0.079	0.120	0.136	0.089	0.163	0.134
		(0.92)	(0.62)	(1.02)	(1.06)	(0.69)	(1.30)	(1.03)
Nordic legal origin		0.013	0.032	0.002	0.076	0.020	-0.001	-0.001
		(0.08)	(0.19)	(0.01)	(0.46)	(0.12)	(-0.01)	(-0.00)
GDP per capita		-0.056	-0.064	-0.052	-0.073*	-0.052	-0.069	-0.070
		(-1.19)	(-1.40)	(-1.08)	(-1.69)	(-1.00)	(-1.32)	(-1.32)
Market cap		-0.001	0.006	-0.003	0.004	0.005	-0.031*	-0.019
		(-0.06)	(0.34)	(-0.22)	(0.26)	(0.32)	(-1.88)	(-0.96)
Year fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,913	86,913	86,913	86,913	86,913	86,913	48,039	35,444
Number of countries	50	50	50	50	50	50	49	48
R ²	0.036	0.467	0.465	0.468	0.465	0.464	0.497	0.432

Table 1.3**Board independence and bankruptcy penalties: difference in default probability**

This table presents estimates of OLS regressions. The dependent variable is the logarithm of the number of independent directors (plus one) in Panel A, the ratio of the number of independent directors to board size in Panel B, and the logarithm of board size in Panel C. Columns 1 to 7 show the results for the full sample. Columns 8 and 9 show the results when US and UK firms are excluded. For columns 1 and 8, HighRisk is a dummy variable that equals one if a firm has a default probability that is above the median bankruptcy probability distribution of its country for a given year (low Altman's Z-score), and zero if a firm has a default probability that is below the median bankruptcy probability distribution (high Altman's Z-score). For column 2, HighRisk is a dummy variable that equals one if a firm has a default probability that is in the highest tercile of the bankruptcy probability distribution, and zero if a firm has a default probability that is in the lowest tercile of the bankruptcy probability distribution (cutting off the middle 33% of the distribution). For columns 3–7 and 9, HighRisk is a dummy variable that equals one if a firm has a default probability that is in the highest quartile of the bankruptcy probability distribution, and zero if a firm has a default probability that is in the lowest quartile of the bankruptcy probability distribution (cutting off the middle 50% of the distribution). *t*-values are reported in parentheses and are based on standard errors clustered at the country level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.1.1 in the Appendix for variable definitions.

Panel A. Dependent variable: logarithm of the number of independent directors									
	All countries							Exclude US & UK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CRIGHTS x HighRisk	-0.024*** (-5.57)	-0.036*** (-6.55)	-0.045*** (-8.23)					-0.032*** (-3.92)	-0.052*** (-4.12)
CRIGHTS	-0.073*** (-3.41)	-0.072*** (-3.17)	-0.070*** (-3.08)					-0.036 (-1.30)	-0.029 (-0.91)
AUTOSTAY x HighRisk				-0.099*** (-5.63)					
AUTOSTAY				-0.108 (-1.68)					
REORG x HighRisk					-0.096*** (-4.68)				
REORG					-0.163*** (-2.85)				
SECURED x HighRisk						-0.086** (-2.15)			
SECURED						-0.053 (-0.45)			
MANAGES x HighRisk							-0.094*** (-4.54)		
MANAGES							-0.137** (-2.30)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,867	57,840	43,356	43,356	43,356	43,356	43,356	35,398	17,623
Number of countries	49	45	44	44	44	44	44	47	42
R ²	0.536	0.531	0.525	0.518	0.521	0.510	0.519	0.380	0.358

(continued)

Table 1.3 (continued)

Panel B. Dependent variable: ratio of the number of independent directors to board size									
	All countries							Exclude US & UK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CRIGHTS x HighRisk	-0.006*** (-3.78)	-0.010*** (-5.46)	-0.012*** (-7.43)					-0.008** (-2.18)	-0.014*** (-3.61)
CRIGHTS	-0.042*** (-4.48)	-0.042*** (-4.19)	-0.042*** (-4.15)					-0.026* (-1.97)	-0.023 (-1.62)
AUTOSTAY x HighRisk				-0.018** (-2.28)					
AUTOSTAY				-0.094*** (-4.46)					
REORG x HighRisk					-0.028*** (-4.97)				
REORG					-0.071** (-2.65)				
SECURED x HighRisk						-0.019* (-1.76)			
SECURED						-0.009 (-0.18)			
MANAGES x HighRisk							-0.030*** (-5.22)		
MANAGES							-0.086*** (-3.44)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,867	57,840	43,356	43,356	43,356	43,356	43,356	35,398	17,623
Number of countries	49	45	44	44	44	44	44	47	42
R ²	0.576	0.565	0.556	0.552	0.546	0.537	0.551	0.423	0.387
Panel C. Dependent variable: logarithm of board size									
	All countries							Exclude US & UK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CRIGHTS x HighRisk	-0.014*** (-3.52)	-0.021*** (-4.17)	-0.027*** (-4.84)					-0.009 (-0.91)	-0.016 (-1.27)
CRIGHTS	-0.020 (-1.16)	-0.015 (-0.92)	-0.011 (-0.71)					0.006 (0.21)	0.009 (0.31)
AUTOSTAY x HighRisk				-0.096*** (-9.55)					
AUTOSTAY				0.002 (0.06)					
REORG x HighRisk					-0.052** (-2.08)				
REORG					-0.049 (-0.89)				
SECURED x HighRisk						-0.035 (-0.90)			
SECURED						-0.061 (-1.04)			
MANAGES x HighRisk							-0.040 (-1.51)		
MANAGES							0.005 (0.09)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,867	57,840	43,356	43,356	43,356	43,356	43,356	35,398	17,623
Number of countries	49	45	44	44	44	44	44	47	42
R ²	0.467	0.469	0.472	0.472	0.472	0.468	0.468	0.432	0.456

Table 1.4**Bankruptcy reform in Germany and changes in corporate boards**

This table presents estimates of difference-in-differences regressions for the bankruptcy reform in Germany. The dependent variable is the logarithm of the number of independent directors (plus one) in columns 1 and 2, the ratio of the number of independent directors to board size in columns 3 and 4, and the logarithm of board size in columns 5 and 6. HighRisk is a dummy variable that equals one if the firm has a high probability of default (highest quartile of the bankruptcy probability distribution), and zero if it has a low probability of default (lowest quartile). Post is a dummy variable that equals one for the years after the bankruptcy reform was passed (2012 to 2016), and zero for the years before the reform was passed (2007 to 2011). *t*-values are reported in parentheses and are based on standard errors clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.1.1 in the Appendix for variable definitions.

	log(Number of independent directors)		Ratio of independents to board size		log(Board size)	
	(1)	(2)	(3)	(4)	(5)	(6)
Post x HighRisk	0.270** (2.54)	0.286*** (2.78)	0.075** (2.49)	0.075** (2.56)	-0.025 (-0.62)	0.010 (0.23)
Post	0.402*** (4.21)	0.381*** (3.88)	0.101*** (3.63)	0.098*** (3.41)	-0.032 (-0.69)	-0.060 (-1.19)
Size		0.085 (1.20)		0.003 (0.11)		0.139*** (2.73)
Leverage		-0.128 (-0.73)		0.017 (0.29)		0.143 (1.19)
Return on assets		-0.303 (-0.42)		-0.040 (-0.20)		-0.109 (-0.61)
Market-to-book		-0.006 (-0.26)		-0.009 (-1.26)		0.011 (1.22)
Free cash flow		0.294 (0.41)		0.043 (0.22)		0.078 (0.43)
Insider ownership		-0.257** (-2.09)		-0.051* (-1.73)		-0.042 (-0.77)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	778	778	778	778	778	778
Number of firms	110	110	110	110	110	110
R ² (within)	0.212	0.230	0.181	0.190	0.017	0.080

Table 1.5**Bankruptcy reforms in Italy and changes in corporate boards**

This table presents estimates of difference-in-differences regressions for the bankruptcy reforms of 2005 (Panel A) and 2006 (Panel B) in Italy. The dependent variable is the logarithm of the number of independent directors (plus one) in columns 1 and 2, the ratio of the number of independent directors to board size in columns 3 and 4, and the logarithm of board size in columns 5 and 6. HighRisk is a dummy variable that equals one if the firm has a high probability of default (highest quartile of the bankruptcy probability distribution), and zero if it has a low probability of default (lowest quartile). For the 2005 reform (Panel A), Post is a dummy variable that equals one for the year after the bankruptcy reform was passed (2006), and zero for the years before the reform was passed (2002 to 2005). For the 2006 reform (Panel B), Post is a dummy variable that equals one for the years after the reform was passed (2007 to 2011), and zero for the year before the reform was passed (2006). *t*-values are reported in parentheses and are based on standard errors clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.1.1 in the Appendix for variable definitions.

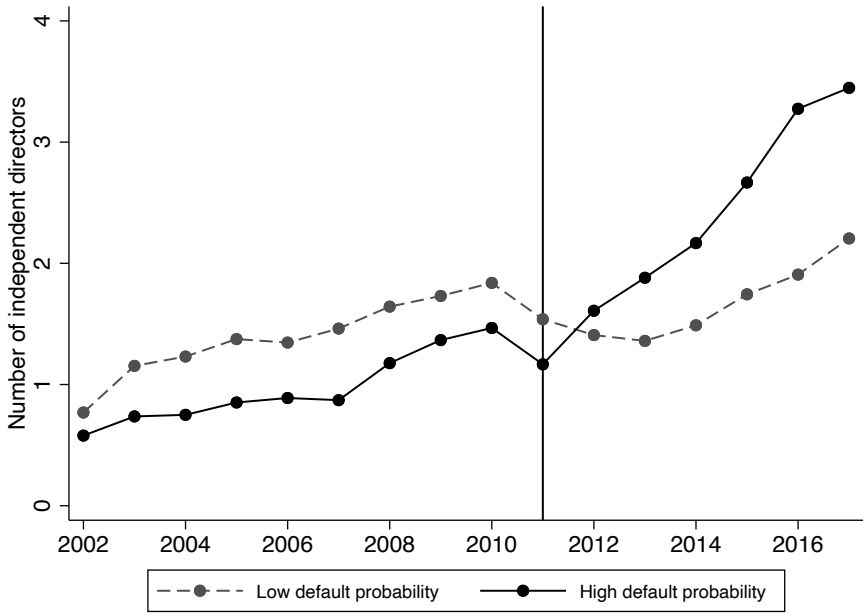
Panel A. 2005 reform						
	log(Number of independent directors)		Ratio of independents to board size		log(Board size)	
	(1)	(2)	(3)	(4)	(5)	(6)
Post x HighRisk	0.266** (2.06)	0.251** (2.10)	0.065 (1.39)	0.068 (1.37)	0.102 (1.07)	0.083 (0.99)
Post	-0.167 (-1.32)	-0.273** (-2.13)	-0.055 (-1.62)	-0.057 (-1.26)	-0.045 (-0.64)	-0.134 (-1.60)
Size		0.107 (1.45)		-0.017 (-0.39)		0.148** (2.23)
Leverage		-0.108 (-0.22)		0.153 (0.54)		-0.271 (-0.73)
Return on assets		0.496 (0.85)		0.296 (1.20)		0.018 (0.05)
Market-to-book		0.219** (2.54)		0.052 (1.52)		0.052 (0.64)
Free cash flow		0.162 (0.64)		-0.070 (-0.51)		0.206 (0.81)
Insider ownership		0.270 (1.45)		0.124 (1.39)		-0.099 (-0.47)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	107	107	107	107	107	107
Number of firms	27	27	27	27	27	27
R ² (within)	0.092	0.260	0.066	0.133	0.036	0.203
Panel B. 2006 reform						
	log(Number of independent directors)		Ratio of independents to board size		log(Board size)	
	(1)	(2)	(3)	(4)	(5)	(6)
Post x HighRisk	-0.268** (-2.12)	-0.275** (-2.51)	-0.090** (-2.47)	-0.098*** (-3.11)	0.038 (0.38)	0.015 (0.18)
Post	0.149 (1.50)	0.137 (1.22)	0.045 (1.49)	0.063* (1.85)	0.029 (0.69)	0.049 (0.79)
Size		0.215* (1.76)		-0.060 (-1.33)		0.261*** (2.80)
Leverage		0.343 (1.05)		0.219** (2.29)		-0.368 (-1.33)
Return on assets		-0.628 (-0.32)		0.310 (0.45)		0.879 (0.94)
Market-to-book		0.020 (0.80)		0.006 (0.65)		0.018 (1.33)
Free cash flow		0.546 (0.28)		-0.254 (-0.38)		-1.121 (-1.22)
Insider ownership		-0.119 (-1.38)		-0.085* (-1.82)		0.136 (1.11)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	135	135	135	135	135	135
Number of firms	25	25	25	25	25	25
R ² (within)	0.086	0.140	0.074	0.151	0.018	0.197

Table 1.6**Bankruptcy reforms in Germany and Italy and changes in corporate boards of firms in other countries**

This table presents estimates of difference-in-differences regressions for firms in other countries for the bankruptcy reform of 2012 in Germany (Panel A) and the bankruptcy reforms of 2005 (Panel B) and 2006 (Panel C) in Italy. The dependent variable is the logarithm of the number of independent directors (plus one) in columns 1 and 4, the ratio of the number of independent directors to board size in columns 2 and 5, and the logarithm of board size in columns 3 and 6. Columns 1 to 3 present the results for firms in Austria, Belgium, France, Netherlands, and Switzerland, and columns 4 to 6 present the results for US firms. HighRisk is a dummy variable that equals one if the firm has a high probability of default (highest quartile of the bankruptcy probability distribution), and zero if it has a low probability of default (lowest quartile). For the 2012 reform in Germany (Panel A), Post is a dummy variable that equals one for the years after the bankruptcy reform was passed (2012 to 2016), and zero for the years before the reform was passed (2007 to 2011). For the 2005 reform in Italy (Panel B), Post is a dummy variable that equals one for the year after the bankruptcy reform was passed (2006), and zero for the years before the reform was passed (2002 to 2005). For the 2006 reform in Italy (Panel C), Post is a dummy variable that equals one for the years after the reform was passed (2007 to 2011), and zero for the year before the reform was passed (2006). *t*-values are reported in parentheses and are based on standard errors clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.1.1 in the Appendix for variable definitions.

Panel A. 2012 reform in Germany						
	Similar countries			United States		
	Independents (1)	Ratio (2)	Board size (3)	Independents (4)	Ratio (5)	Board size (6)
Post x HighRisk	0.014 (0.32)	0.016 (0.85)	-0.018 (-0.66)	-0.004 (-0.47)	-0.001 (-0.16)	-0.004 (-0.52)
Post	0.204*** (4.45)	0.084*** (4.36)	0.024 (0.97)	0.050*** (5.52)	0.047*** (8.93)	-0.008 (-0.96)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,029	2,029	2,029	11,268	11,268	11,268
Number of firms	248	248	248	1,558	1,558	1,558
R ² (within)	0.133	0.138	0.069	0.076	0.066	0.049
Panel B. 2005 reform in Italy						
	Similar countries			United States		
	Independents (1)	Ratio (2)	Board size (3)	Independents (4)	Ratio (5)	Board size (6)
Post x HighRisk	0.017 (0.38)	0.019 (1.20)	0.007 (0.23)	-0.015 (-1.57)	0.004 (0.86)	-0.024*** (-2.95)
Post	0.136** (2.23)	0.057** (2.49)	-0.068** (-2.04)	0.191*** (15.87)	0.124*** (18.77)	0.023*** (2.60)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	743	743	743	6,402	6,402	6,402
Number of firms	197	197	197	1,680	1,680	1,680
R ² (within)	0.159	0.179	0.023	0.183	0.229	0.044
Panel C. 2006 reform in Italy						
	Similar countries			United States		
	Independents (1)	Ratio (2)	Board size (3)	Independents (4)	Ratio (5)	Board size (6)
Post x HighRisk	-0.032 (-0.80)	-0.019 (-1.31)	0.017 (0.75)	0.004 (0.41)	-0.001 (-0.30)	0.006 (0.70)
Post	0.160*** (4.58)	0.069*** (5.20)	-0.002 (-0.11)	0.036*** (4.40)	0.036*** (8.60)	-0.007 (-0.86)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	985	985	985	6,896	6,896	6,896
Number of firms	190	190	190	1,481	1,481	1,481
R ² (within)	0.093	0.078	0.068	0.061	0.061	0.039

Panel A. Number of independent directors



Panel B. did estimate relative to 2011

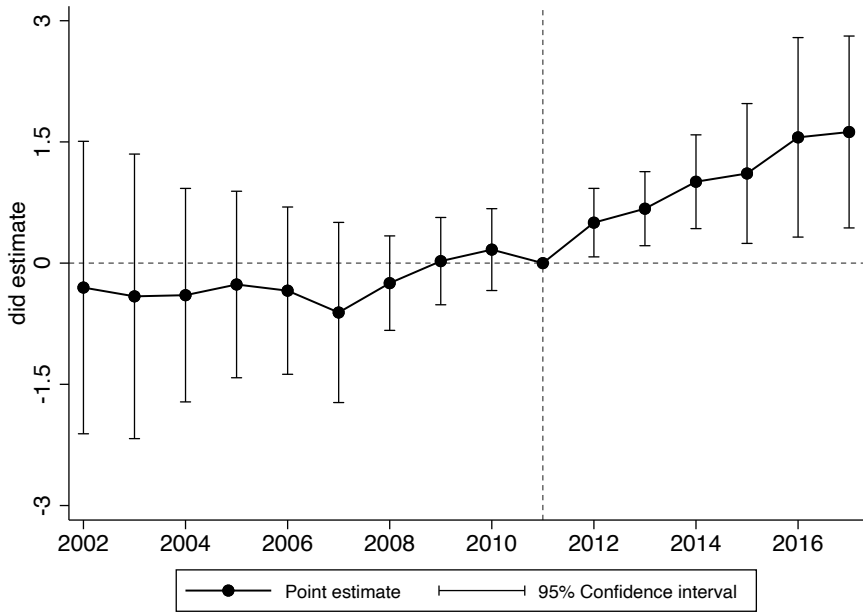
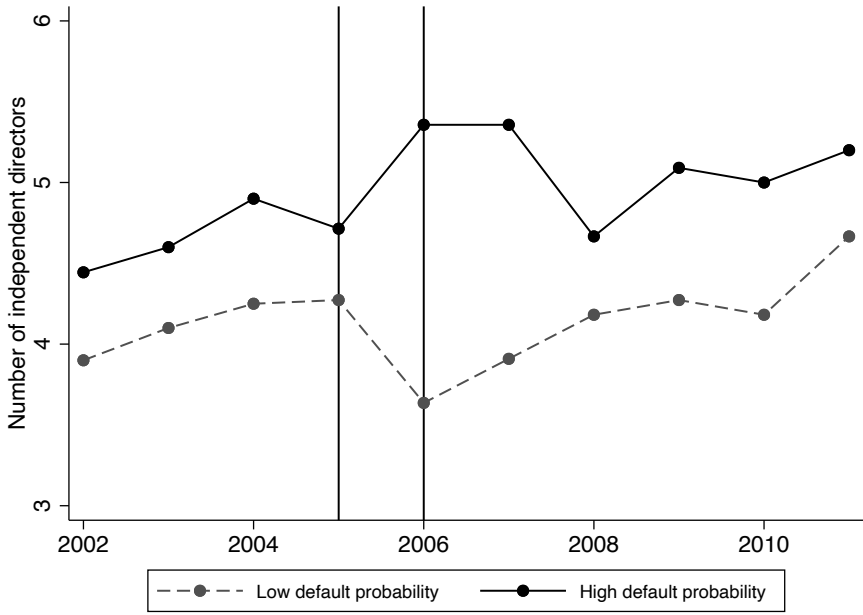


Figure 1.1
 Bankruptcy reform in Germany and the number of independent directors
 Panel A plots the average number of independent directors for high and low default probability firms. Panel B plots the point estimates (black dots) and the 95% confidence intervals (grey bars) on the interaction terms between a set of time dummies and a dummy indicating firms with high default probability. The point estimates are relative to the year prior to the reform, 2011. The dependent variable is the number of independent directors. The regression is specified as in column 2 of Table 1.4, that is, with firm fixed effects, year fixed effects, controls, and standard errors clustered on the firm level. Detailed point estimates and *t*-values can be found in Table A.1.5 in the Appendix.

Panel A. Number of independent directors



Panel B. did estimate relative to 2005

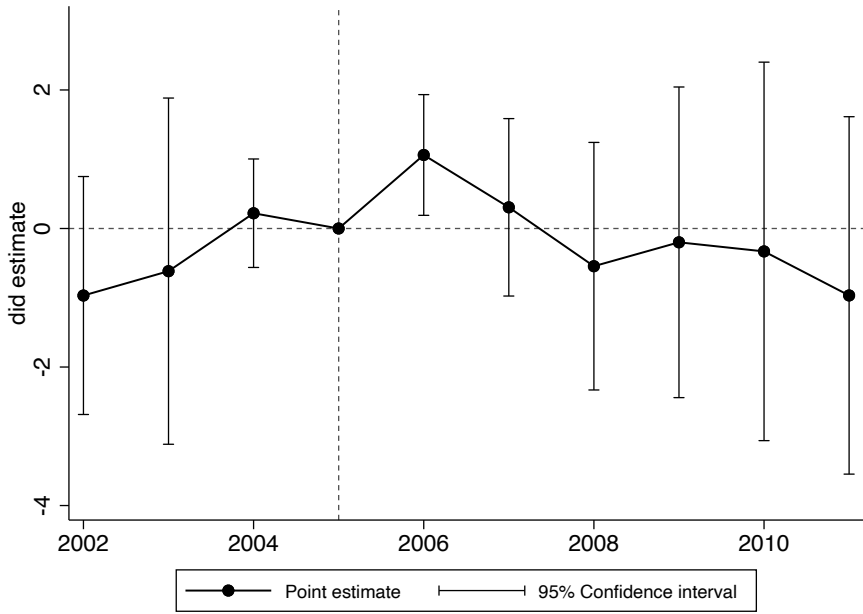


Figure 1.2
Bankruptcy reforms in Italy and the number of independent directors

Panel A plots the average number of independent directors for high and low default probability firms. Panel B plots the point estimates (black dots) and the 95% confidence intervals (grey bars) on the interaction terms between a set of time dummies and a dummy indicating firms with high default probability. The point estimates are relative to the year prior to the reforms, 2005. The dependent variable is the number of independent directors. The regression is specified as in column 2 of Table 1.5, that is, with firm fixed effects, year fixed effects, controls, and standard errors clustered on the firm level. Detailed point estimates and *t*-values can be found in Table A.1.7 in the Appendix.

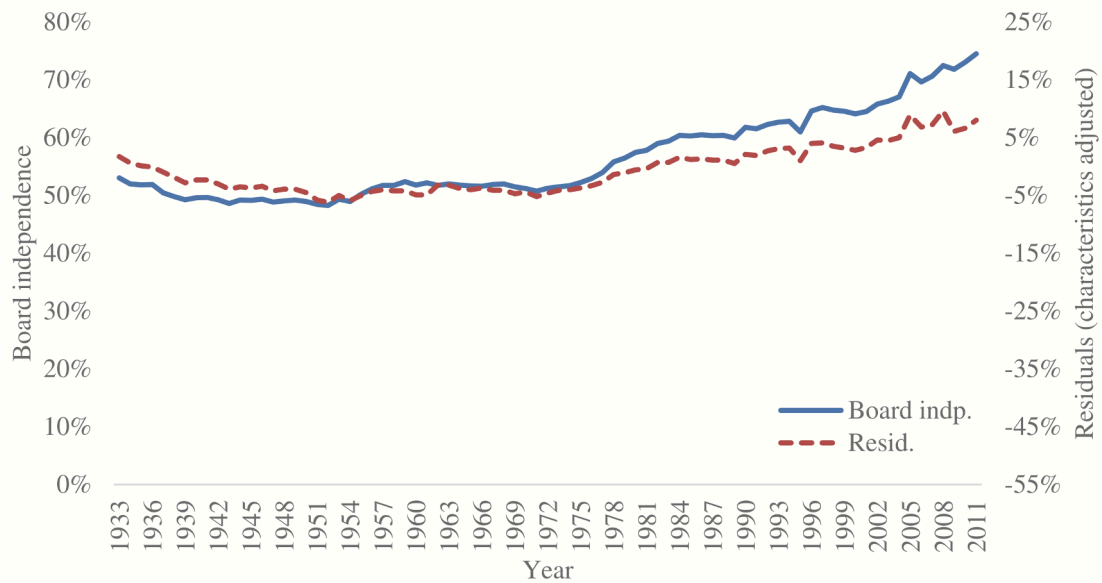
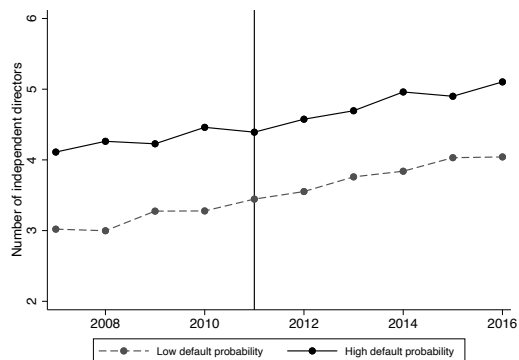


Figure 1.3

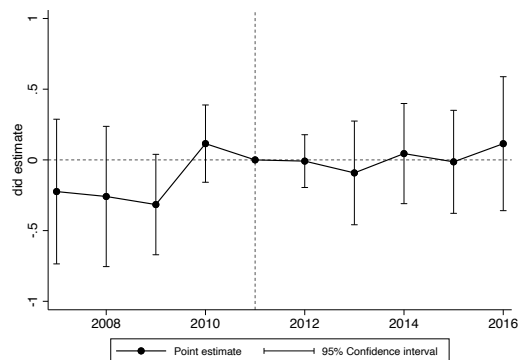
US bankruptcy reform and the ratio of the number of independent directors to board size

This figure is a copy of Panel C of Figure 1 from Graham, Kim, and Leary (2020). The solid blue line shows annual means of the ratio of the number of independent directors to the number of directors. The dashed red line shows average regression residuals after controlling for several firm characteristics, including firm age, board size, book assets, Tobin's Q, industry-adjusted return on assets, and asset tangibility. Based on hand-collected data from Moody's Industrial Manuals, from 1920 to 2011, the database of the authors contains about 87,000 firm-year observations of NYSE/AMEX firms.

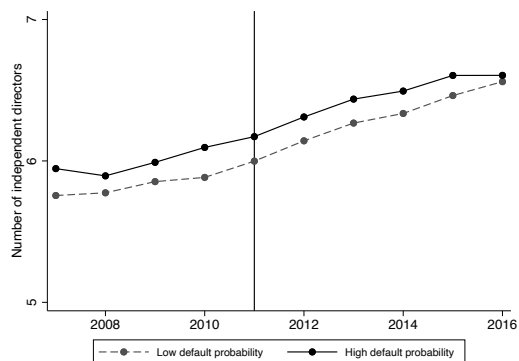
Panel A. Number of independent directors (similar countries)



Panel B. did estimate relative to 2011 (similar countries)



Panel C. Number of independent directors (US)



Panel D. did estimate relative to 2011 (US)

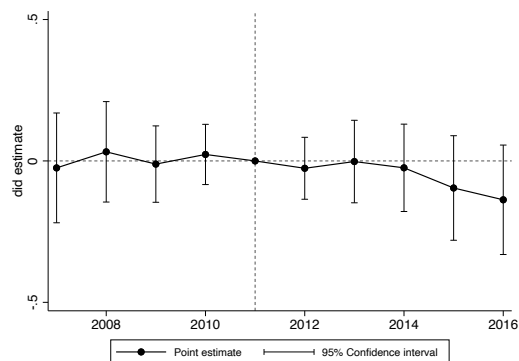
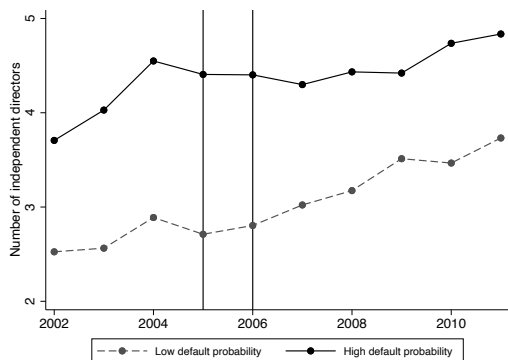


Figure 1.4

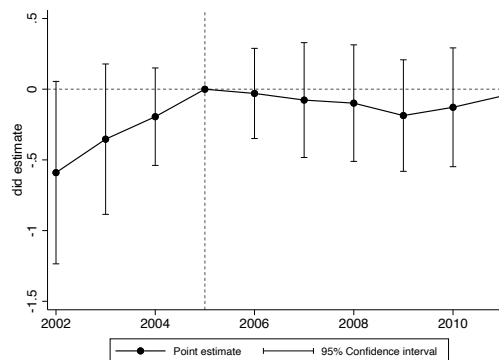
Bankruptcy reform in Germany and the number of independent directors of firms in other countries

Panels A (similar countries) and C (US) plot the average number of independent directors for high and low default probability firms. Panels B (similar countries) and D (US) plot the point estimates (black dots) and the 95% confidence intervals (grey bars) on the interaction terms between a set of time dummies and a dummy indicating firms with high default probability. The point estimates are relative to the year prior to the reform, 2011. The dependent variable is the number of independent directors. The regression is specified as in column 1 of Table 1.6, that is, with firm fixed effects, year fixed effects, controls, and standard errors clustered on the firm level. Detailed point estimates and *t*-values can be found in Table A.1.5 in the Appendix.

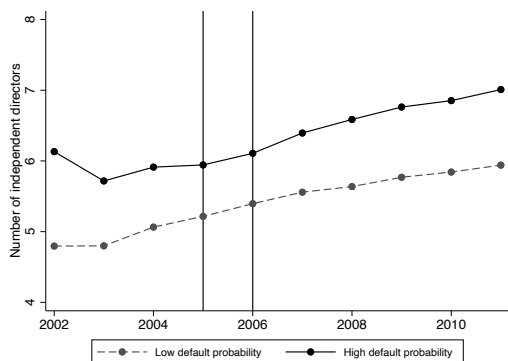
Panel A. Number of independent directors (similar countries)



Panel B. did estimate relative to 2005 (similar countries)



Panel C. Number of independent directors (US)



Panel D. did estimate relative to 2005 (US)

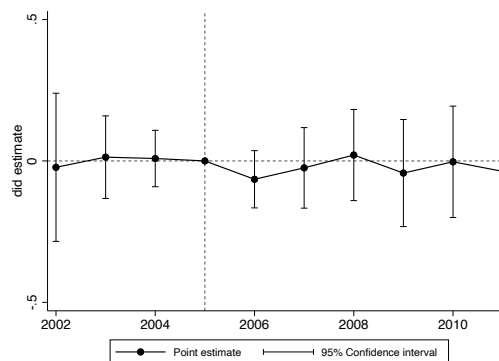


Figure 1.5

Bankruptcy reforms in Italy and the number of independent directors of firms in other countries

Panels A (similar countries) and C (US) plot the average number of independent directors for high and low default probability firms. Panels B (similar countries) and D (US) plot the point estimates (black dots) and the 95% confidence intervals (grey bars) on the interaction terms between a set of time dummies and a dummy indicating firms with high default probability. The point estimates are relative to the year prior to the reforms, 2005. The dependent variable is the number of independent directors. The regression is specified as in column 1 of Table 1.6, that is, with firm fixed effects, year fixed effects, controls, and standard errors clustered on the firm level. Detailed point estimates and *t*-values can be found in Table A.1.7 in the Appendix.

Appendix for Chapter 1

Table A.1.1
Variable definitions

Variable name	Variable definition	Source
<i>Board characteristics</i>		
log(Number of independent directors)	Logarithm of the number of independent board members.	BoardEx
Ratio of the number of independents to board size	Ratio of the number of independent board members to the number of board members.	BoardEx
log(Board size)	Logarithm of the number of board members.	BoardEx
<i>Country characteristics</i>		
Creditor rights (CRIGHTS)	Aggregate index of creditor rights, following La Porta et al. (1998). The index is the sum of the four individual components described below. The index ranges from zero to four and higher values indicate stricter bankruptcy penalties on management in default. The data are from 2003 and come from Djankov, McLiesh, and Shleifer (2007), who updated and enlarged the data of La Porta et al. (1998).	Djankov, McLiesh, and Shleifer (2007)
No automatic stay (AUTOSTAY)	Equals one if secured creditors are able to seize their collateral after the reorganization procedure is approved, that is, if there is no automatic stay on the assets of the firm. Equals zero if the law imposes automatic stay on the assets, thereby protecting the debtor against secured creditors.	Djankov, McLiesh, and Shleifer (2007)
Reorganization (REORG)	Equals one if the reorganization procedure imposes restrictions, such as secured creditors' consent or minimum dividends, for a debtor to file for reorganization. Equals zero if management can seek protection from creditors unilaterally by filing for reorganization.	Djankov, McLiesh, and Shleifer (2007)
Secured creditors first (SECURED)	Equals one if secured creditors are ranked first in the distribution of the proceeds that result from the disposition of the assets of a bankrupt firm. Equals zero if other claimants, such as the government or workers, are given absolute priority.	Djankov, McLiesh, and Shleifer (2007)
No management stay (MANAGES)	Equals one if an administrator is automatically appointed for running the business during the reorganization procedure, that is, management is dismissed in the reorganization procedure. Equals zero if management retains corporate control during reorganization.	Djankov, McLiesh, and Shleifer (2007)
Revised shareholder rights (SHRIGHTS)	Aggregate index of shareholder rights, following La Porta et al. (1998). The index is formed by adding one when: (1) shareholders are allowed to mail their proxy vote to the firm, (2) shareholders are not required to deposit their shares prior to the general shareholders' meeting, (3) cumulative voting or proportional representation of minorities (shareholders owning 10% or less of the capital) in the board of directors is allowed, (4) an oppressed minorities mechanism is in place, (5) the minimum percentage of share capital that entitles a single shareholder to call for an extraordinary shareholders' meeting is less than or equal to 10%, and (6) shareholders have preemptive rights. The index ranges from zero to six and higher values indicate better legal protection of minority shareholders. The data are from 2003 and come from Djankov et al. (2008), who revised, updated, and enlarged the data of La Porta et al. (1998).	Djankov et al. (2008)
Legal origin	Dummy variables that identify the legal origin of the company law or commercial code of each country. The five origins are English, French, German, Nordic, and Socialist.	Djankov, McLiesh, and Shleifer (2007)
Rule of law	"captures perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence." The index ranges from -2.5 (weak) to 2.5 (strong). Details on the data sources, the aggregation method, and the precise interpretation of the index can be found in Kaufmann, Kraay, and Mastruzzi (2010).	Quotation and source are from World Bank (Worldwide Governance Indicators)
GDP per capita	Logarithm of the real GDP per capita. The data are in constant 2010 US dollars.	World Bank
Market cap	Logarithm of the average stock market capitalization in US dollars.	World Bank
<i>Firm characteristics</i>		
Altman's Z-score	Sum of 1.2 times net working capital divided by total assets, 1.4 times retained earnings divided by total assets, 3.3 times EBIT divided by total assets, 0.6 times market value of equity divided by total liabilities, and 0.99 times sales divided by total assets. Firms with low Z-score have high default probability and firms with high Z-score have low default probability.	Worldscope
Size	Logarithm of total assets in millions of US dollars.	Worldscope
Leverage	Ratio of total debt to total assets.	Worldscope
Return on assets	Ratio of EBITDA to total assets.	Worldscope
Return volatility	Average standard deviation of return on assets.	Worldscope
Market-to-book	Ratio of total assets plus market value of equity minus book value of equity to total assets.	Worldscope
Insider ownership	Ratio of the number of closely held shares by insiders (shareholders who hold at least 5% of the outstanding shares, such as officers and directors and immediate families, other corporations, or individuals) to the number of shares outstanding.	Worldscope
Free cash flow	Ratio of EBITDA minus capital expenditures to total assets.	Worldscope

<i>Appendix</i>		
Interest coverage ratio	Ratio of EBIT to interest expenses.	Worldscope
Board rules	Dummy variables that identify whether a country requires or encourages a certain level of board independence. The three board rules are rule-based, comply-or-explain, and no rule.	Fauver et al. (2017)
Board structures	Dummy variables that identify the board structure of each country. The three board structures are one-tier, two-tier, and mixed (choice over board structure).	Adams and Ferreira (2007) and Ferreira and Kirchmaier (2013)
Accounting standards	Index of accounting standards, created by the Center for International Financial Analysis and Research (CIFAR, 1995). The index is constructed by examining and rating companies' annual reports for their inclusion or omission of 85 items. The index ranges from zero to 100 and higher values indicate better accounting standards. The data come from Bhattacharya, Daouk, and Welker (2003) and Hope, Kang, and Zang (2007).	Bhattacharya, Daouk, and Welker (2003) and Hope, Kang, and Zang (2007)
Institutional ownership	Average institutional ownership in a country in 2010. The data are from Dyck et al. (2019). Because Dyck et al. (2019) do not report results for the US, the data for the US are from Aggarwal et al. (2011) and are from 2007.	Dyck et al. (2019) and Aggarwal et al. (2011).

Table A.1.2**Board independence and bankruptcy penalties (additional controls)**

This table presents estimates of OLS regressions. The dependent variable is the logarithm of the number of independent directors (plus one) in Panel A, the ratio of the number of independent directors to board size in Panel B, and the logarithm of board size in Panel C. Columns 1 to 7 show the results for the full sample. Columns 8 to 10 show the results when US and UK firms are excluded. For columns 6 and 9, Risk is a dummy variable that equals one if a firm has a default probability that is above the median bankruptcy probability distribution of its country for a given year (low Altman's Z-score), and zero if a firm has a default probability that is below the median bankruptcy probability distribution (high Altman's Z-score). For columns 7 and 10, Risk is a dummy variable that equals one if a firm has a default probability that is in the highest quartile of the bankruptcy probability distribution, and zero if a firm has a default probability that is in the lowest quartile of the bankruptcy probability distribution (cutting off the middle 50% of the distribution). *t*-values are reported in parentheses and are based on standard errors clustered at the country level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.1.1 in the Appendix for variable definitions.

Panel A. Dependent variable: logarithm of the number of independent directors										
	All countries							Exclude US & UK		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CR x Risk						-0.023***	-0.046***		-0.032**	-0.057***
						(-5.12)	(-7.25)		(-2.49)	(-3.29)
CR	-0.119***	-0.124***	-0.084***	-0.091***	-0.143***	-0.132***	-0.135***	-0.168**	-0.152**	-0.142*
	(-3.25)	(-6.34)	(-3.77)	(-4.45)	(-4.36)	(-4.04)	(-4.02)	(-2.76)	(-2.45)	(-2.03)
Comply-exp.	0.228				-0.217	-0.216	-0.196	-0.287	-0.286	-0.278
	(1.44)				(-1.10)	(-1.10)	(-0.94)	(-1.25)	(-1.26)	(-1.14)
Rule-based	0.203				0.004	0.006	-0.010	-0.003	-0.003	-0.007
	(1.56)				(0.02)	(0.03)	(-0.05)	(-0.02)	(-0.02)	(-0.03)
Mixed		-0.038			-0.278*	-0.278*	-0.323*	-0.362*	-0.362*	-0.419*
		(-0.39)			(-1.91)	(-1.92)	(-2.03)	(-1.87)	(-1.87)	(-1.95)
Two-tier		0.264**			0.413***	0.412***	0.362***	0.429**	0.429**	0.359*
		(2.44)			(3.86)	(3.85)	(3.41)	(2.59)	(2.60)	(2.06)
Inst. owner			-0.000		-0.001	-0.001	-0.000	-0.004	-0.004	-0.003
			(-0.02)		(-0.46)	(-0.46)	(-0.04)	(-1.19)	(-1.20)	(-0.72)
Accounting				-0.003	0.022***	0.022***	0.021***	0.027***	0.027***	0.027***
				(-0.62)	(3.52)	(3.51)	(3.15)	(3.71)	(3.71)	(3.66)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	84,584	83,476	86,613	84,815	79,909	79,907	39,935	28,440	28,438	14,202
No. countries	41	37	40	37	27	27	27	25	25	25
R ²	0.546	0.552	0.538	0.544	0.567	0.568	0.556	0.435	0.436	0.411

(continued)

Table A.1.2 (continued)

Panel B. Dependent variable: ratio of the number of independent directors to board size										
	All countries							Exclude US & UK		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CR x Risk						-0.005***	-0.012***		-0.008	-0.017***
						(-3.44)	(-6.27)		(-1.37)	(-2.96)
CR	-0.064***	-0.054***	-0.046***	-0.046***	-0.071***	-0.068***	-0.068***	-0.016	-0.012	-0.010
	(-3.84)	(-5.74)	(-4.20)	(-4.33)	(-3.77)	(-3.60)	(-3.51)	(-0.61)	(-0.44)	(-0.35)
Comply-exp.	0.085				-0.030	-0.029	-0.033	-0.055	-0.054	-0.065
	(1.32)				(-0.24)	(-0.23)	(-0.27)	(-0.43)	(-0.42)	(-0.49)
Rule-based	0.060				0.007	0.008	0.001	0.029	0.030	0.024
	(1.06)				(0.06)	(0.07)	(0.01)	(0.28)	(0.29)	(0.23)
Mixed		-0.015			-0.070	-0.070	-0.095	-0.043	-0.043	-0.074
		(-0.23)			(-0.71)	(-0.71)	(-0.94)	(-0.40)	(-0.40)	(-0.65)
Two-tier		0.076			0.124*	0.123*	0.112	0.062	0.061	0.048
		(1.54)			(1.74)	(1.72)	(1.54)	(0.75)	(0.74)	(0.57)
Inst. owner			-0.001		-0.001	-0.001	-0.000	0.001	0.001	0.002
			(-0.74)		(-0.73)	(-0.73)	(-0.24)	(0.64)	(0.64)	(1.01)
Accounting				-0.001	0.005	0.005	0.005	0.008*	0.008*	0.008*
				(-0.27)	(1.17)	(1.16)	(1.11)	(1.82)	(1.81)	(1.90)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	84,584	83,476	86,613	84,815	79,909	79,907	39,935	28,440	28,438	14,202
No. countries	41	37	40	37	27	27	27	25	25	25
R ²	0.584	0.583	0.578	0.577	0.590	0.591	0.573	0.467	0.468	0.429

Panel C. Dependent variable: logarithm of board size										
	All countries							Exclude US & UK		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CR x Risk						-0.014**	-0.028***		-0.006	-0.014
						(-2.73)	(-4.19)		(-0.47)	(-0.80)
CR	-0.057**	-0.066***	-0.009	-0.009	-0.052*	-0.045	-0.042	-0.166***	-0.163***	-0.147**
	(-2.35)	(-4.04)	(-0.42)	(-0.49)	(-1.98)	(-1.68)	(-1.57)	(-2.99)	(-2.89)	(-2.50)
Comply-exp.	0.284***				0.158	0.157	0.179	0.232*	0.231*	0.244
	(3.02)				(1.00)	(1.00)	(1.17)	(1.74)	(1.73)	(1.71)
Rule-based	0.325***				0.178	0.177	0.191	0.132	0.131	0.153
	(3.18)				(1.03)	(1.03)	(1.19)	(1.05)	(1.04)	(1.23)
Mixed		-0.065			0.001	0.001	-0.004	-0.029	-0.029	-0.039
		(-0.72)			(0.01)	(0.01)	(-0.04)	(-0.28)	(-0.28)	(-0.38)
Two-tier		0.224**			0.164**	0.165**	0.136	0.308**	0.310**	0.253**
		(2.44)			(2.09)	(2.10)	(1.61)	(2.77)	(2.79)	(2.20)
Inst. owner			0.005**		0.002	0.002	0.002	-0.003	-0.003	-0.002
			(2.39)		(0.88)	(0.88)	(0.95)	(-0.66)	(-0.66)	(-0.41)
Accounting				-0.007	0.002	0.002	0.003	-0.004	-0.004	-0.002
				(-1.24)	(0.29)	(0.30)	(0.42)	(-0.66)	(-0.66)	(-0.34)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	84,584	83,476	86,613	84,815	79,909	79,907	39,935	28,440	28,438	14,202
No. countries	41	37	40	37	27	27	27	25	25	25
R ²	0.490	0.493	0.476	0.476	0.509	0.509	0.506	0.523	0.523	0.531

Table A.1.3

Board independence and bankruptcy penalties: difference in default probability (interest coverage ratio)

This table presents estimates of OLS regressions. The dependent variable is the logarithm of the number of independent directors (plus one) in Panel A, the ratio of the number of independent directors to board size in Panel B, and the logarithm of board size in Panel C. Columns 1 to 7 show the results for the full sample. Columns 8 and 9 show the results when US and UK firms are excluded. For columns 1 and 8, HighRisk is a dummy variable that equals one if a firm has a default probability that is above the median bankruptcy probability distribution of its country for a given year (low interest coverage ratio), and zero if a firm has a default probability that is below the median bankruptcy probability distribution (high interest coverage ratio). For column 2, HighRisk is a dummy variable that equals one if a firm has a default probability that is in the highest tercile of the bankruptcy probability distribution, and zero if a firm has a default probability that is in the lowest tercile of the bankruptcy probability distribution (cutting off the middle 33% of the distribution). For columns 3–7 and 9, HighRisk is a dummy variable that equals one if a firm has a default probability that is in the highest quartile of the bankruptcy probability distribution, and zero if a firm has a default probability that is in the lowest quartile of the bankruptcy probability distribution (cutting off the middle 50% of the distribution). *t*-values are reported in parentheses and are based on standard errors clustered at the country level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.1.1 in the Appendix for variable definitions.

Panel A. Dependent variable: logarithm of the number of independent directors									
	All countries							Exclude US & UK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CRIGHTS x HighRisk	-0.025*** (-8.20)	-0.035*** (-9.96)	-0.039*** (-9.36)					-0.029*** (-5.72)	-0.037*** (-4.28)
CRIGHTS	-0.072*** (-3.42)	-0.071*** (-2.99)	-0.070*** (-2.73)					-0.038 (-1.42)	-0.021 (-0.69)
AUTOSTAY x HighRisk				-0.075*** (-3.39)					
AUTOSTAY				-0.139** (-2.25)					
REORG x HighRisk					-0.090*** (-4.80)				
REORG					-0.143** (-2.24)				
SECURED x HighRisk						-0.065** (-2.03)			
SECURED						-0.014 (-0.11)			
MANAGES x HighRisk							-0.084*** (-4.13)		
MANAGES							-0.129** (-2.08)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,867	57,840	43,326	43,326	43,326	43,326	43,326	35,398	17,593
Number of countries	49	45	44	44	44	44	44	47	42
R ²	0.536	0.515	0.507	0.503	0.503	0.493	0.502	0.379	0.348

(continued)

Table A.1.3 (continued)

Panel B. Dependent variable: ratio of the number of independent directors to board size									
	All countries							Exclude US & UK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CRIGHTS x HighRisk	-0.007*** (-5.94)	-0.009*** (-6.89)	-0.010*** (-5.25)					-0.009*** (-3.72)	-0.011** (-2.32)
CRIGHTS	-0.041*** (-4.30)	-0.043*** (-3.98)	-0.043*** (-3.74)					-0.026* (-1.85)	-0.022 (-1.42)
AUTOSTAY x HighRisk				-0.013 (-1.62)					
AUTOSTAY				-0.107*** (-5.22)					
REORG x HighRisk					-0.023*** (-4.04)				
REORG					-0.065** (-2.18)				
SECURED x HighRisk						-0.024* (-1.72)			
SECURED						0.005 (0.09)			
MANAGES x HighRisk							-0.023*** (-3.67)		
MANAGES							-0.089*** (-3.22)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,867	57,840	43,326	43,326	43,326	43,326	43,326	35,398	17,593
Number of countries	49	45	44	44	44	44	44	47	42
R ²	0.576	0.560	0.557	0.557	0.546	0.538	0.551	0.423	0.387
Panel C. Dependent variable: logarithm of board size									
	All countries							Exclude US & UK	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CRIGHTS x HighRisk	-0.010*** (-5.04)	-0.015*** (-6.41)	-0.019*** (-7.08)					-0.007 (-1.30)	-0.011** (-2.32)
CRIGHTS	-0.022 (-1.25)	-0.015 (-0.89)	-0.011 (-0.67)					0.005 (0.17)	0.013 (0.45)
AUTOSTAY x HighRisk				-0.059*** (-7.53)					
AUTOSTAY				-0.009 (-0.21)					
REORG x HighRisk					-0.046*** (-4.10)				
REORG					-0.049 (-0.85)				
SECURED x HighRisk						-0.000 (-0.00)			
SECURED						-0.062 (-1.14)			
MANAGES x HighRisk							-0.033** (-2.28)		
MANAGES							0.013 (0.23)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	86,867	57,840	43,326	43,326	43,326	43,326	43,326	35,398	17,593
Number of countries	49	45	44	44	44	44	44	47	42
R ²	0.467	0.447	0.430	0.430	0.432	0.428	0.428	0.432	0.429

Table A.1.4

Summary statistics for the bankruptcy reform in Germany

This table presents summary statistics regarding the bankruptcy reform in Germany, separately for firms with high default probability (columns 1–3) and firms with low default probability (columns 4–6). The columns show: columns 1 and 4, average for all years (2007 to 2016); columns 2 and 5, average for the years before the reform (2007 to 2011); and columns 3 and 6, average for the years after the reform (2012 to 2016). Columns 7, 8, and 9 present p -values of t -tests of difference in means between high and low default probability firms for all years, for the years before the reform, and for the years after the reform, respectively. Refer to Table A.1.1 in the Appendix for variable definitions.

	High default probability			Low default probability			p -all (7)	p -before (8)	p -after (9)
	All (1)	Before (2)	After (3)	All (4)	Before (5)	After (6)			
Number of independents	1.81	1.20	2.30	1.60	1.63	1.57	0.28	0.08	0.01
Ratio independents to board size	0.133	0.078	0.179	0.146	0.143	0.148	0.40	0.00	0.15
Board size	13.75	15.25	12.53	10.30	11.01	9.79	0.00	0.00	0.00
Size	14.29	14.89	13.81	13.12	13.32	12.97	0.00	0.00	0.00
Leverage	0.326	0.340	0.315	0.089	0.093	0.087	0.00	0.00	0.00
Return on assets	0.032	0.010	0.049	0.148	0.175	0.129	0.00	0.00	0.00
Market-to-book	1.67	1.34	1.94	2.36	2.27	2.42	0.00	0.00	0.00
Free cash flow	-0.013	-0.040	0.009	0.112	0.138	0.093	0.00	0.00	0.00
Insider ownership	0.347	0.353	0.342	0.452	0.468	0.440	0.00	0.00	0.00
Altman's Z-score	0.55	0.50	0.59	6.79	6.71	6.85	0.00	0.00	0.00

Table A.1.5Detailed point estimates and *t*-values for figures on Germany's bankruptcy reform

This table reports the point estimates and *t*-values regarding Figures 1.1, A.1.1, and 1.4. *t*-values are reported in parentheses and are based on standard errors clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Figure 1.1 (Panel B) (1)	Figure A.1.1 (Panel B) (2)	Figure A.1.1 (Panel D) (3)	Figure 1.4 (Panel B) (4)	Figure 1.4 (Panel D) (5)
2002 x HighRisk	-0.302 (-0.33)	0.026 (0.50)	1.978 (1.58)	-	-
Number of observations	132	132	132	-	-
Number of firms	104	104	104	-	-
R ² (within)	0.477	0.504	0.398	-	-
2003 x HighRisk	-0.411 (-0.46)	0.019 (0.34)	1.278 (1.56)	-	-
Number of observations	132	132	132	-	-
Number of firms	104	104	104	-	-
R ² (within)	0.506	0.517	0.388	-	-
2004 x HighRisk	-0.396 (-0.59)	0.003 (0.08)	2.095 (2.20)	-	-
Number of observations	133	133	133	-	-
Number of firms	105	105	105	-	-
R ² (within)	0.567	0.588	0.378	-	-
2005 x HighRisk	-0.264 (-0.45)	-0.003 (-0.09)	1.112 (1.24)	-	-
Number of observations	151	151	151	-	-
Number of firms	108	108	108	-	-
R ² (within)	0.404	0.412	0.193	-	-
2006 x HighRisk	-0.341 (-0.65)	0.002 (0.07)	1.047 (1.15)	-	-
Number of observations	153	153	153	-	-
Number of firms	108	108	108	-	-
R ² (within)	0.410	0.432	0.247	-	-
2007 x HighRisk	-0.611 (-1.09)	-0.023 (-0.79)	0.963 (1.40)	-0.224 (-0.86)	-0.025 (-0.25)
Number of observations	157	157	157	408	2,527
Number of firms	110	110	110	243	1,521
R ² (within)	0.362	0.402	0.282	0.146	0.076
2008 x HighRisk	-0.248 (-0.84)	0.010 (0.54)	-0.673 (-1.23)	-0.259 (-1.03)	0.032 (0.36)
Number of observations	162	162	162	420	2,592
Number of firms	110	110	110	246	1,541
R ² (within)	0.364	0.357	0.233	0.138	0.071
2009 x HighRisk	0.025 (0.09)	0.014 (1.08)	0.182 (0.22)	-0.316 (-1.75)	-0.011 (-0.16)
Number of observations	156	156	156	402	2,496
Number of firms	104	104	104	224	1,436
R ² (within)	0.316	0.336	0.142	0.120	0.041
2010 x HighRisk	0.167 (0.66)	0.013 (0.85)	-0.083 (-0.27)	0.115 (0.83)	0.023 (0.42)
Number of observations	161	161	161	402	2,491
Number of firms	102	102	102	218	1,331
R ² (within)	0.142	0.138	0.081	0.060	0.036
2012 x HighRisk	0.501** (2.35)	0.056 (1.74)	-0.296 (-1.06)	-0.009 (-0.09)	-0.026 (-0.47)
Number of observations	195	195	195	414	2,345
Number of firms	100	100	100	219	1,246
R ² (within)	0.189	0.188	0.038	0.068	0.044
2013 x HighRisk	0.674*** (2.92)	0.056** (2.26)	-0.208 (-0.59)	-0.092 (-0.49)	-0.002 (-0.03)
Number of observations	192	192	192	408	2,251
Number of firms	100	100	100	220	1,248
R ² (within)	0.189	0.150	0.103	0.089	0.067
2014 x HighRisk	1.007*** (3.44)	0.087*** (2.94)	-0.319 (-0.80)	0.045 (0.25)	-0.024 (-0.31)
Number of observations	187	187	187	405	2,212
Number of firms	100	100	100	220	1,247
R ² (within)	0.254	0.216	0.146	0.109	0.094
2015 x HighRisk	1.109** (2.54)	0.086* (2.05)	-0.443 (-0.83)	-0.014 (-0.07)	-0.096 (-1.02)
Number of observations	185	185	185	399	2,139
Number of firms	100	100	100	220	1,247
R ² (within)	0.253	0.209	0.123	0.150	0.087
2016 x HighRisk	1.557*** (2.50)	0.105* (2.18)	0.156 (0.34)	0.115 (0.48)	-0.137 (-1.39)
Number of observations	183	183	183	395	2,055
Number of firms	100	100	100	218	1,244
R ² (within)	0.256	0.249	0.107	0.158	0.094
2017 x HighRisk	1.623*** (2.71)	0.124** (2.43)	0.037 (0.07)	-	-
Number of observations	177	177	177	-	-
Number of firms	100	100	100	-	-
R ² (within)	0.309	0.294	0.142	-	-
Firm controls	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes

Table A.1.6

Summary statistics for the bankruptcy reforms in Italy

This table presents summary statistics regarding the bankruptcy reforms in Italy, separately for firms with high default probability (columns 1–3) and firms with low default probability (columns 4–6). The columns show: columns 1 and 4, average for all years (2002 to 2011); columns 2 and 5, average for the years before the reforms (2002 to 2005); and columns 3 and 6, average for the years after the reforms (2006 to 2011). Columns 7, 8, and 9 present p -values of t -tests of difference in means between high and low default probability firms for all years, for the years before the reforms, and for the years after the reforms, respectively. Refer to Table A.1.1 in the Appendix for variable definitions.

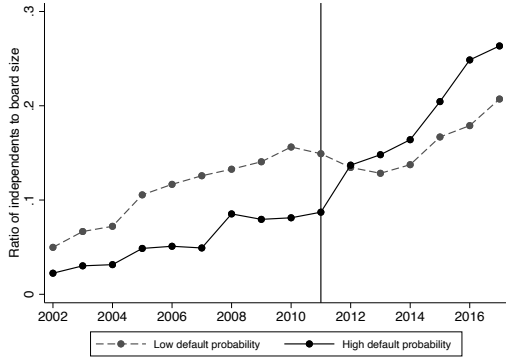
	High default probability			Low default probability			p -all (7)	p -before (8)	p -after (9)
	All (1)	Before (2)	After (3)	All (4)	Before (5)	After (6)			
Number of independents	4.96	4.67	5.13	4.13	4.13	4.13	0.01	0.29	0.01
Ratio independents to board size	0.453	0.440	0.461	0.432	0.405	0.448	0.46	0.47	0.71
Board size	11.50	10.77	11.94	10.19	10.87	9.78	0.01	0.90	0.00
Size	15.63	15.55	15.69	14.41	14.61	14.29	0.00	0.01	0.00
Leverage	0.335	0.342	0.331	0.164	0.165	0.162	0.00	0.00	0.00
Return on assets	0.071	0.047	0.085	0.152	0.197	0.124	0.00	0.00	0.10
Market-to-book	1.17	1.21	1.14	1.98	2.29	1.79	0.00	0.00	0.00
Free cash flow	0.013	-0.030	0.039	0.107	0.144	0.085	0.00	0.00	0.05
Insider ownership	0.419	0.419	0.418	0.540	0.531	0.546	0.00	0.00	0.00
Altman's Z-score	1.07	0.90	1.17	4.55	5.59	3.91	0.00	0.00	0.00

Table A.1.7Detailed point estimates and *t*-values for figures on the bankruptcy reforms in Italy

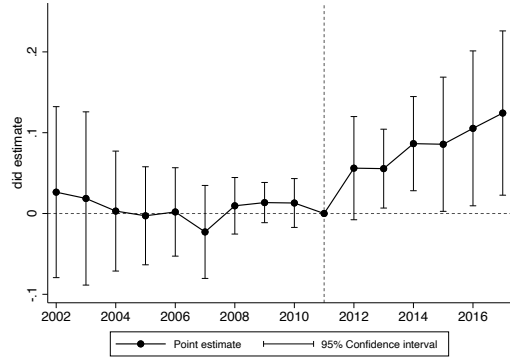
This table reports the point estimates and *t*-values regarding Figures 1.2 and 1.5. *t*-values are reported in parentheses and are based on standard errors clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Figure 1.2 (Panel B) (1)	Figure 1.5 (Panel B) (2)	Figure 1.5 (Panel D) (3)
2002 x HighRisk	-0.968 (-1.16)	-0.590* (-1.80)	-0.023 (-0.17)
Number of observations	44	286	2,186
Number of firms	27	195	1,622
R ² (within)	0.306	0.291	0.387
2003 x HighRisk	-0.616 (-0.51)	-0.353 (-1.31)	0.013 (0.18)
Number of observations	45	313	2,872
Number of firms	27	196	1,670
R ² (within)	0.218	0.240	0.151
2004 x HighRisk	0.221 (0.58)	-0.195 (-1.11)	0.009 (0.17)
Number of observations	43	331	3,061
Number of firms	26	192	1,652
R ² (within)	0.524	0.138	0.053
2006 x HighRisk	1.062** (2.51)	-0.030 (-0.19)	-0.065 (-1.26)
Number of observations	50	377	3,008
Number of firms	25	192	1,600
R ² (within)	0.304	0.106	0.053
2007 x HighRisk	0.306 (0.49)	-0.077 (-0.37)	-0.025 (-0.34)
Number of observations	50	366	2,855
Number of firms	25	191	1,603
R ² (within)	0.450	0.072	0.082
2008 x HighRisk	-0.544 (-0.63)	-0.099 (-0.47)	0.021 (0.25)
Number of observations	48	357	2,734
Number of firms	25	191	1,600
R ² (within)	0.533	0.049	0.120
2009 x HighRisk	-0.199 (-0.18)	-0.187 (-0.93)	-0.043 (-0.44)
Number of observations	47	342	2,639
Number of firms	25	190	1,604
R ² (within)	0.230	0.157	0.159
2010 x HighRisk	-0.331 (-0.25)	-0.128 (-0.60)	-0.003 (-0.03)
Number of observations	46	339	2,590
Number of firms	25	191	1,605
R ² (within)	0.331	0.124	0.177
2011 x HighRisk	-0.966 (-0.77)	-0.048 (-0.18)	-0.037 (-0.34)
Number of observations	44	332	2,520
Number of firms	25	190	1,604
R ² (within)	0.358	0.201	0.165
Firm controls	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

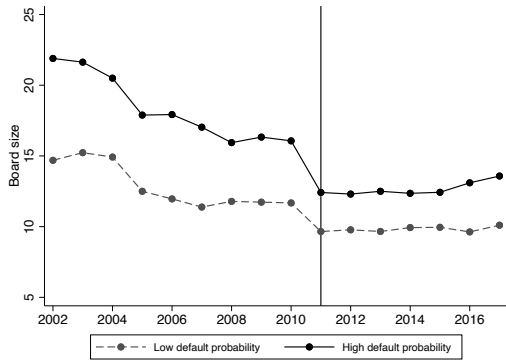
Panel A. Ratio of independents to board size



Panel B. did estimate relative to 2011 (ratio)



Panel C. Board size



Panel D. did estimate relative to 2011 (board size)

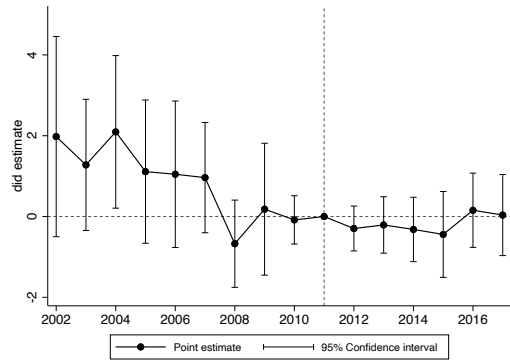


Figure A.1.1

Bankruptcy reform in Germany, the ratio of independents to board size, and the board size

Panels A and C respectively plot the average ratio of independent directors to board size and the average board size for high and low default probability firms. Panels B and D plot the point estimates (black dots) and the 95% confidence intervals (grey bars) on the interaction terms between a set of time dummies and a dummy indicating firms with high default probability. The point estimates are relative to the year prior to the reform, 2011. The dependent variable is the ratio of independent directors to board size in Panel B and the board size in Panel D. The regression is specified as in columns 4 and 6 of Table 1.4, that is, with firm fixed effects, year fixed effects, controls, and standard errors clustered on the firm level. Detailed point estimates and *t*-values can be found in Table A.1.5 in the Appendix.

Chapter 2

Dual-Class Firms, Agency Problems, and Debt Governance: Evidence from Korea*

* I thank Joonkyu Choi, Kyung-Hoon Chun, Merritt Fox, Lawrence Glosten, Edward Greene, Terence Haliday, Jens Jackwerth, Axel Kind, Haksoo Ko, Jiri Tressl, Umakanth Varottil, and Wai Yee Wan.

2.1 Introduction

Dual-class firms have become increasingly important in recent years. For example, in the US, the average share of dual-class IPOs in all IPOs was 25% for 2017–2021 (see Figure 2.1). Prominent examples of dual-class firms around the world are Google and Facebook in the US, Samsung and Hyundai in Korea, BMW and Volkswagen in Germany, or Volvo and Hennes & Mauritz in Sweden. As a reaction to the increasing popularity, dual-class shares have become the subject of an extensive debate in academia and politics. Dual-class share structure can be costly because the separation of control and ownership generates potentially extreme agency problems between controlling shareholders and minority shareholders. Yet, dual-class share structure can also be beneficial because it allows insiders flexibility towards less-informed or short-termed outside shareholders. Among researchers and regulators, the central question in this context is how to allow dual-class firms, while at the same time offering solutions to effectively limit possible expropriation of minority shareholders by controlling shareholders. Note that finding solutions is difficult, given that the high degree of control makes controlling shareholders typically immune to traditional governance mechanisms, such as shareholder activism, incentive compensation, or board independence.¹

In this paper, I study whether a performance threshold can mitigate agency problems. In particular, I examine whether debt can reduce agency problems between controlling shareholders and minority shareholders in dual-class firms. A large body of theoretical research gives debt a central role in corporate governance. Specifically, it is argued that debt gives creditors control and liquidation rights when the firm fails to make a prespecified payment. This threat of a loss in private control benefits motivates controlling shareholders to reduce the probability of bankruptcy by pursuing high profits rather than less profitable personal objectives. As such, debt can serve minority shareholders as a disciplinary source to limit adverse managerial actions, and can further serve controlling shareholders as a bonding device to commit to a sufficient level of efficiency in order to secure financing.²

¹ For the debate in academia and politics on dual-class firms, see Bebchuk and Kastiel (2017), Berger and Hodrick (2018), Govindarajan et al. (2018), Sharfman (2018, 2019), Fisch and Solomon (2019), Fleming (2019), Kim and Michaely (2019), Papadopoulos (2019), Aggarwal et al. (2021), Lel et al. (2021), and Cremers, Lauterbach, and Pajuste (2022). For empirical evidence on the costs and benefits of dual-class share structure, see, e.g., Gompers, Ishii, and Metrick (2010), Kim and Michaely (2019), Lel et al. (2021), and Cremers, Lauterbach, and Pajuste (2022). For the limitations of other governance mechanisms to mitigate agency problems between controlling shareholders and minority shareholders, see Dey, Nikolaev, and Wang (2016), and also Masulis, Wang, and Xie (2009), Bebchuk and Hamdani (2017), Bebchuk and Kastiel (2017), and Aggarwal et al. (2021).

² On the role of debt in restricting diversion of funds, see Grossman and Hart (1982); in inducing effort, see Dewatripont and Tirole (1994); and in curbing empire building, see Zwiebel (1996). For surveys and studies providing discussions on debt as a disciplinary and bonding mechanism, see Aghion, Hart, and Moore (1992), Hart (1995, 2000, 2001, 2017), Benmelech and Bergman (2008), and Canipek (2023b).

To provide empirical evidence, I use two key features of debt as a governance device (Canipek, 2023b). First, debt governance is only effective with bankruptcy penalties, given that the absence of penalties undermines controlling shareholders' incentive to avoid bankruptcy (Grossman and Hart, 1982; Aghion, Hart, and Moore, 1992; Hart, 1995, 2000). Second, debt governance only matters for firms close to bankruptcy, given that proximity to default ensures bankruptcy when performance is poor (Zwiebel, 1996; Hart, 2001). Based on these two features, the central test of my paper is whether firms with high default probability exhibit an increase in agency costs relative to firms with low default probability when a bankruptcy law that is soft on controlling shareholders makes debt governance ineffective.

As a change in penalties, I use Korea's bankruptcy reform of 2006 which replaced a creditor-friendly law with a debtor-friendly law. The reform significantly reduced controlling shareholders' cost of bankruptcy, thereby changing debt governance from effective to ineffective. In particular, before the reform, the incumbent management was dismissed during the bankruptcy proceeding and ownership of the firm was transferred to new owners. In contrast, after the reform, the incumbent management remains in corporate control during bankruptcy and ownership is retained by the existing owners (Ko, 2007; Schoenherr and Starmans, 2021).

Measuring changes in agency problems is challenging, and perhaps even more challenging in the context of debt governance. Typically, changes in agency problems are inferred by changes in the firm value, given that potential agency problems should be reflected in the valuation of the company (e.g., Grossman and Hart, 1982; Zwiebel, 1996; Gompers, Ishii, and Metrick, 2003; Harvey, Lins, and Roper, 2004). However, in the setting of debt governance this approach is not very meaningful. The reason is that bankruptcy law not only affects the effectiveness of debt governance, but may also affect credit costs and distress costs.³ As a result, possible changes in the firm value might not be because of a change in debt governance, but because of changes in the other two variables.

I measure private benefits of control by the price difference of high- and low-voting shares in dual-class firms. The idea here is that if control is valuable, this should be reflected in the price of votes. Because minority shareholders do not have access to private benefits of control, the voting premium reflects the expected price a large shareholder is willing to pay when accumulating shares to form a controlling coalition (Zingales, 1994, 1995; Chung and Kim, 1999; Nenova, 2003; Doidge, 2004; Guadalupe and Pérez-González, 2010; Hong, 2013). Note that

³ As suggested by the bankruptcy literature (Acharya, Amihud, and Litov, 2011; Vig, 2013; Mann, 2018; Canipek, Kind, and Wende, 2021; Schoenherr and Starmans, 2021), stronger creditor rights can reduce credit costs through higher collateral value and thus may increase firm value by allowing firms to increase financing and investments. In contrast, stronger creditor rights can also increase distress costs and thus may reduce firm value by prompting firms to lower leverage and undertake risk-reducing but unprofitable investments.

the voting premium not only depends on the size of private control benefits, but also on the probability of a control contest (Zingales, 1994, 1995; Chung and Kim, 1999; Nenova, 2003). As a result, changes in the voting premium in response to changes in agency problems should be concentrated among firms for which a control contest is more possible, which is typically the case when ownership is dispersed. I will use this feature to examine cross-sectional variation in possible changes of the voting premium. Finally, note how looking at shares with dissimilar voting rights but similar cash flow rights gets around the measurement problem of agency costs. While the potential effect of changes in credit costs and distress costs affects high- and low-voting shares similarly by the cash flow component, the potential effect of changes in debt governance affects both share classes differently (presumably, a lower effectivity of debt governance is beneficial for the voting share, but costly for the nonvoting share).

Consistent with the hypothesis that debt reduces agency problems, I find that firms with high probability of default exhibited an increase in the voting premium after the reform relative to firms with low probability of default. The economic effect is sizable. Relative to firms with low default probability, the voting premium of firms with high default probability increased by 59% after the reform, suggesting that debt is valued as a powerful mechanism for solving agency problems. Importantly, given that prices are forward-looking, the results also suggest that firms cannot fully substitute debt governance with other governance mechanisms, consistent with the notion that debt may be special in solving the agency problems in dual-class firms. This is further confirmed by the fact that the increase in the voting premium continues for many years after the reform was introduced.

My findings are further supported by an analysis across firms with high default probability. In particular, I find that the increase in the voting premium is stronger for those high default probability firms in which ownership is more dispersed (when the possibility of a control contest is higher) and in which potential agency problems are extreme (specifically, when firm age and firm size is higher). In fact, while risky firms with concentrated ownership and low agency problems still experienced an increase in the voting premium, for risky firms with dispersed ownership and high agency problems the increase is more than one third larger. Notably, the effect varies especially across firm age, consistent with a raising view in the dual-class literature that the potential benefits of dual-class share structure tend to decrease while the potential costs tend to increase as time passes (Bebchuk and Kastiel, 2017; Kim and Michaely, 2019; Cremers, Lauterbach, and Pajuste, 2022).

A potential concern with my results is that a confounding factor affected the voting premium of high and low default probability firms differently. Various pieces of evidence mitigate

this concern. First, I find that the voting premium of high and low default probability firms followed parallel trends several years prior to the reform, suggesting that the voting premium of both groups of firms would have followed the same trend in the absence of the reform. Second, the heterogeneities discussed above suggest that a confounding factor must not only have had a different effect on firms with high and low default probability, but also on firms with different levels of ownership concentration and agency problems.⁴ Third, one specific confounding factor might be the 2007–2009 financial crisis. I find that the documented changes occurred shortly after the enactment of the law in 2005, and not in 2008 when the financial crisis began to affect the Korean economy (Schoenherr and Starmans, 2021). Fourth, I find that in other countries firms with high default probability did not exhibit an increase in the voting premium relative to firms with low default probability after the reform in Korea, suggesting that the observed changes in Korea are not driven by confounding shocks that were common to all countries. Finally, I find that agency problems are negatively related to bankruptcy penalties also across countries, further confirming the results from Korea.

My paper contributes to the financial contracting literature. In particular, I provide empirical evidence on a central hypothesis of this literature, namely, that debt can mitigate agency problems. Despite the large volume of financial contracting theories and the importance of this hypothesis, there is little evidence on the empirical relevance of debt governance. Most closely related evidence comes from the ongoing work by Canipek (2023b), who finds that firms appoint independent directors as a substitute when bankruptcy legislations that are soft on management make debt governance ineffective, consistent with the notion that debt induces discipline. My paper is complementary to this study in numerous ways. First, I do not consider the relation between debt governance and board independence, but between debt governance and agency problems. This provides an alternative way to identify the governance role of debt, thereby offering additional evidence on an important subject for which only limited evidence exists. Second, my analysis does not include the agency problem between management and minority shareholders, but focuses on a specific agency problem, namely, the conflict between controlling shareholders and minority shareholders. This allows me to provide evidence on the helpfulness of debt to mitigate an agency problem that is commonly viewed as being immune to traditional governance mechanisms (Dey, Nikolaev, and Wang, 2016; see also Masulis, Wang, and Xie, 2009; Bebchuk and Hamdani, 2017; Bebchuk and Kastiel, 2017; Aggarwal et al., 2021). Finally, by studying changes in agency problems, my analysis not only provides

⁴ For the problem of confounding shocks and the possibility to alleviate the problem by examining cross-sectional heterogeneity, see Vig (2013) and Schoenherr and Starmans (2021).

evidence on the hypothesis that debt can solve agency problems, but also on the hypothesis that, in certain situations, debt governance is not perfectly substitutable by other governance mechanisms. This evidence is important as it has key implications on bankruptcy design.⁵

My study also contributes to the literature on optimal bankruptcy design. In particular, my findings provide empirical evidence consistent with the view that bankruptcy law should preserve the governance role of debt by adequately penalizing management and controlling shareholders in bankruptcy states. In the absence of bankruptcy penalties, firms may have difficulties to commit to refrain from inefficient actions and thus to secure financing *ex ante* (Aghion, Hart, and Moore, 1992; Hart, 2000). It is important to note that this aspect is not considered by the current bankruptcy literature.⁶ Moreover, while my findings are important in general, they may be of particular importance in the context of the recent crises during which many countries have significantly softened their bankruptcy law.⁷⁸

Finally, and most important, my paper contributes to the heated debate on dual-class share structure. Among academics and policymakers, perhaps the core question in this context is how to allow dual-class firms, while at the same time offering solutions to alleviate potential

⁵ More broadly related evidence comes from studies that find a negative relation between managerial entrenchment and debt and suggest that this is consistent with entrenched managers avoiding the disciplinary role of debt (Berger, Ofek, and Yermack, 1997; Garvey and Hanka, 1999), or from studies that find a positive relation between debt and market value and suggest that this is consistent with debt is valued as a governance device (e.g., Harvey, Lins, and Roper, 2004; Dey, Nikolaev, and Wang, 2016). However, these papers examine the governance role of debt in general, i.e., the incentive effect of debt that comes, besides the threat of bankruptcy penalties, also through other mechanisms, such as forcing management to disgorge free cash flow (e.g., Jensen, 1986), additional monitoring by lenders (Harvey, Lins, and Roper, 2004), or the threat of managerial restrictions following covenant violation (e.g., Nini, Smith, and Sufi, 2012). Moreover, given their dependence on cross-sectional or event-study analysis, a restriction of these studies is the potential endogeneities in the relations between debt and the other variables, making it difficult to draw reliable inferences. Different than these studies, I exploit two key features of debt as a governance device that allow me to provide direct and credible evidence on the incentive effect of bankruptcy penalties. On the broadly related evidence, see also Canipek (2023b).

⁶ Currently, the bankruptcy literature focuses on two channels when asking whether stronger creditor rights facilitate or hamper firm financing (Acharya, Amihud, and Litov, 2011; Vig, 2013; Mann, 2018; Canipek, Kind, and Wende, 2021; Schoenherr and Starmans, 2021). The first channel is that stronger creditor rights may allow firms to increase debt financing by lowering credit costs through higher collateral value. In contrast, the second channel is that stronger creditor rights may prompt firms to reduce debt financing by increasing distress costs. As described above, my findings point to another channel through which stronger creditor rights can affect financing, namely, by increasing the effectivity of debt governance, which may reduce capital costs of equity (by reducing managerial expropriation) as well as debt (by lowering default probability) and thus may allow firms to obtain financing. Note that this channel is not necessarily incompatible with a debtor-friendly law. One way to combine the channels is by making bankruptcy penalties contingent on managerial misconduct (e.g., determined by the performance of comparable firms). Consistent with this, for some cases, the new debtor-friendly law in Korea stipulates that the incumbent management may not be allowed to remain in control during bankruptcy if insolvency was caused by material mismanagement (Park, 2008). As noted below, this rule does not affect my empirical analysis, given that my sample consists of listed firms for which the rule does not apply.

⁷ To rescue distressed firms from liquidation, for example, Germany and the UK have softened their laws respectively as a response to the 2007–2009 financial crisis and the Covid-19 crisis (Canipek, Kind, and Wende, 2021; Payne, 2021). While in some situations suspending bankruptcy penalties temporarily may be desirable, my findings suggest that repealing them permanently may cause substantial costs.

⁸ For the implications of the finding that debt can mitigate agency problems on optimal bankruptcy law design, see Canipek (2023b).

expropriation of minority shareholders by controlling shareholders. I contribute to this debate by providing empirical evidence that debt governance, or, more broadly, a mechanism that takes away control when a prespecified performance threshold cannot be achieved, can serve as a solution to effectively mitigate agency problems in dual-class firms. For policymakers, my findings suggest that implementing provisions that allow for such mechanisms may be desirable. This can be done by providing a bankruptcy law for dual-class firms that sufficiently penalizes the controlling shareholder in default. An alternative possibility, that may also provide an effective solution for safe firms, is to include “control change provisions” that allow controlling shareholders to maintain control when performance is good but transfer control when performance is bad. Kaplan and Strömberg (2003) analyze contracts between venture capitalists and entrepreneurs and show that, for solving agency problems, venture capitalists write contracts that are consistent with such a provision, i.e., they implement shifts in control that are contingent on observable measures of financial performance.^{9,10} In the context of dual-class shares, the provision should be relatively easy to design. As the control shift, one could use the change of the dual-class share structure into a single-class share structure (or as a softer option, the approval by minority shareholders to maintain control) (as in sunset provisions; Bebchuk and Kastiel, 2017). As the performance threshold, one could use the performance of comparable single-class firms (perhaps complemented by a prespecified tolerance range).¹¹

The rest of the paper is organized as follows. Section 2.2 provides information on dual-class firms, debt governance, and the Korean bankruptcy reform. Section 2.3 discusses the voting premium as a measure for private control benefits. Section 2.4 describes the data, provides

⁹ Note that such control change provisions follow the spirit of financial contracting and control change theories (Aghion and Bolton, 1992; Hart, 2001, 2017). Whereas debt contracts allow to shift control when collective action problems make control change provisions difficult to implement, such as in mature single-class firms, control change provisions can shift control when they are easier to implement, such as in the relation between venture capitalists and entrepreneurs or in the setting of dual-class firms (see below).

¹⁰ Note that the relationship between minority shareholders and controlling shareholders is comparable to the relationship between venture capitalists and entrepreneurs. While minority shareholders can be seen as venture capitalists that typically want to maximize firm value and mitigate agency problems, controlling shareholders can be seen as entrepreneurs who enter capital markets to finance their projects (Kaplan and Strömberg, 2003).

¹¹ Bebchuk and Kastiel (2017) offer another solution to the dual-class problem. Specifically, the authors argue that the potential benefits of dual-class shares tend to decrease and the potential costs tend to increase over time; and propose sunset provisions that, after a predetermined period of time, require companies to convert the dual-class structure into a single-class structure unless their extension is approved by minority shareholders. Consistent with this, Kim and Michaely (2019) and Cremers, Lauterbach, and Pajuste (2022) find that, at the time of the IPO, dual-class firms have higher valuation than single-class firms, while, over time, this valuation premium tends to disappear. Such sunset provisions are increasingly common in the US (51% of dual-class IPOs had sunset provisions in 2021, see https://www.cii.org/dualclass_stock) and are even under discussion in some countries to become mandatory. Note that sunset provisions are not without controversy, in particular, because the mandated conversion eliminates the possibility to maintain dual-class structure and thus the potential benefits associated with it (Berger and Hodrick, 2018; Sharfman, 2018, 2019; Berger, 2019; Fisch and Solomon, 2019).

summary statistics, and discusses the empirical method. Sections 2.5 and 2.6 present the empirical findings. Section 2.7 concludes.

2.2 Dual-class firms, debt governance, and Korea's bankruptcy reform

2.2.1 Dual-class firms

Dual-class firms are widespread worldwide and moreover have become increasingly important in recent years. In 2016, the share of dual-class firms in listed firms was about 4% worldwide, while dual-class firms accounted for about 10% of the worldwide market capitalization (Kim, Matos, and Xu, 2018). Some examples of dual-class firms around the world are Google and Facebook in the US, Samsung and Hyundai in Korea, BMW and Volkswagen in Germany, or Volvo and Hennes & Mauritz in Sweden.¹² Figure 2.1 shows that, in the US, the share of dual-class IPOs in all IPOs increased from 10% for 2003–2007 to 25% for 2017–2021 (Panel A), while dual-class firms accounted for about 37% of the total IPO market capitalization for 2017–2020 (Panel B).¹³

As a response to the increasing importance, dual-class firms have become the subject of an intense debate in the literature and politics. On the one hand, dual-class share structure can be value-reducing as the separation of ownership and control generates potentially large agency problems between controlling shareholders and minority shareholders. However, on the other hand, dual-class share structure can also be value-enhancing because it gives controlling shareholders freedom from less-informed or short-termed minority shareholders. Under scholars and regulators, the central question in this context is how to allow dual-class firms, while at the same time providing mechanisms to effectively limit possible expropriation of minority shareholders by controlling shareholders.¹⁴ Finding mechanisms is notoriously difficult, given that traditional governance mechanisms, such as shareholder activism, incentive compensation, outside block-holders, the market of corporate control, corporate charter, or board independence, typically do not apply for controlling shareholders due to their high degree of control (Dey,

¹² Other examples are Nike, Alibaba, The New York Times, Berkshire Hathaway, Viacom/CBS, LG, Nokia, Korean Air Lines, Snap, Levi Strauss & Co., Pinterest, Lyft, Ericsson, LinkedIn, Hershey's, Ralph Lauren, Ford, Under Armour, Dropbox, or Zoom.

¹³ On the importance and rise of dual-class share structure, see also Bebchuk and Kastiel (2017), Fisch and Solomon (2019), Kim and Michaely (2019), Papadopoulos (2019), Sharfman (2019), the Committee on Capital Markets Regulation report (2020), Aggarwal et al. (2021), Lel et al. (2021), and Cremers, Lauterbach, and Pajuste (2022).

¹⁴ For the debate in research and politics about dual-class firms, see Bebchuk and Kastiel (2017), Berger and Hodrick (2018), Govindarajan et al. (2018), Sharfman (2018, 2019), Fisch and Solomon (2019), Fleming (2019), Kim and Michaely (2019), Papadopoulos (2019), Aggarwal et al. (2021), Lel et al. (2021), and Cremers, Lauterbach, and Pajuste (2022). For empirical evidence that dual-class share structure can be beneficial in some cases but costly in other cases, see, e.g. Gompers, Ishii, and Metrick (2010), Kim and Michaely (2019), Lel et al. (2021), and Cremers, Lauterbach, and Pajuste (2022).

Nikolaev, and Wang, 2016; see also Masulis, Wang, and Xie, 2009; Bebchuk and Hamdani, 2017; Bebchuk and Kastiel, 2017; Aggarwal et al., 2021).

2.2.2 Debt governance

In this study, I examine whether debt can mitigate agency problems between controlling shareholders and minority shareholders in dual-class firms. A large theoretical literature gives debt a fundamental role in corporate governance. In particular, it is argued that debt provides control and liquidation rights to creditors when the company is unable to make a previously agreed payment. This potential loss in control and ownership encourages the controlling shareholders to reduce the probability of bankruptcy by pursuing high profits rather than less profitable personal goals, such as diverting funds, empire building, or shirking. Hence, debt can be a disciplinary source that allows minority shareholders to restrict adverse managerial actions, and can moreover be a bonding device that allows controlling shareholders to commit to a sufficient level of efficiency in order to ensure corporate financing (Grossman and Hart, 1982; Aghion, Hart, and Moore, 1992; Dewatripont and Tirole, 1994; Hart, 1995, 2000, 2001, 2017; Zwiebel, 1996; Benmelech and Bergman, 2008; Canipek, 2023b).¹⁵ Note that bankruptcy presents a specific performance threshold that may induce discipline. In the Conclusions, I will discuss the possibility of alternative thresholds that might also be applicable for dual-class firms.

To provide evidence, I follow Canipek (2023b) and exploit two core features of debt as a governance device. The first feature is that debt governance is only effective with bankruptcy penalties, given that the absence of penalties undermines the incentive of controlling shareholders to avoid bankruptcy, and, hence, reduces the incentive effect of debt to pursue high profits (Grossman and Hart, 1982; Aghion, Hart, and Moore, 1992; Hart, 1995, 2000). The second feature is that debt governance only matters for firms close to bankruptcy, given that proximity to default ensures bankruptcy when performance is low, and, therefore, increases the incentive

¹⁵ Note that bankruptcy may not only present the lower point of managerial inefficiency, but may also affect debtors earlier by determining the effectivity of other, typically, more upstream mechanisms through which debt can induce an incentive effect, such as forcing management to disgorge free cash flow (Jensen, 1986; Stulz, 1990; Hart and Moore, 1995) or the threat of managerial restrictions following covenant violation (e.g., Nini, Smith, and Sufi, 2012). For example, Hart and Moore (1995) argue that it is “hard” debt that curbs overinvestment by its nonpostponable nature such that debt is effectively enforced when due. Similarly, among others, Hart and Moore (1998) and Benmelech and Bergman (2008) suggest that, by determining bargaining power between creditors and debtors, bankruptcy law may be the basis in the renegotiation process following a covenant violation. Whereas creditor-friendly bankruptcy procedures give creditors a credible threat to put the company into bankruptcy (by high liquidation values for creditors and high bankruptcy costs for controlling shareholders) when denying the demanded amendments, debtor-friendly procedures improve firms’ bargaining position (in fact, firms may default strategically, which typically allows to shake off debt and lowers creditors’ liquidation value) and thus may reduce the extent of new restrictions that creditors can renegotiate.

to be efficient in order to avoid bankruptcy (Zwiebel, 1996; Hart, 2001).¹⁶ Building on these two features, the central test of my study is whether firms with high default probability experience an increase in agency problems relative to firms with low default probability when a bankruptcy reform that abolishes penalties makes debt governance ineffective. Importantly, comparing changes in agency problems of firms with high default probability to those of firms with low default probability has the advantage that it controls for factors, common to all firms, that might have changed together with a change in bankruptcy law.¹⁷

2.2.3 Korea's bankruptcy reform¹⁸

2.2.3.1 Changes to bankruptcy law

As a change in penalties, I use Korea's bankruptcy reform of 2006 (the Unified Bankruptcy Act) that replaced a creditor-friendly law with a debtor-friendly law. The reform significantly reduced bankruptcy penalties for controlling shareholders, thereby changing debt governance from effective to ineffective. Specifically, under the old law, the incumbent management (which is typically the controlling shareholders or their representatives) was dismissed during the bankruptcy procedure. Moreover, corporate control was transferred to a court-appointed receiver who had the standard practice to engage in a sales process that typically led to an ownership transfer to new investors. In contrast, under the new law, the incumbent management stays in corporate control during bankruptcy and negotiates firm restructuring with the creditors (Ko, 2007; Schoenherr and Starmans, 2021).^{19,20}

¹⁶ Notably, proximity to default increases the likelihood of bankruptcy in case of low performance not only because it increases the likelihood that profits are lower than debt dues, but also because for firms with high default probability such situations lead to actual default states, that is, firms cannot pay off current debt dues by issuing more debt in order to avert default (Hart and Moore, 1995; Zwiebel, 1996; Hart, 2001).

¹⁷ For studies that use bankruptcy reforms to study the effects of bankruptcy law design on other firm characteristics, such as corporate financial and investment policy, see, e.g., Vig (2013), Rodano, Serrano-Velarde, and Tarantino (2016), Favara et al. (2017), Canipek, Kind, and Wende (2021), and Schoenherr and Starmans (2021). For studies that measure the effects of a bankruptcy reform using variation in the impact of the reform in default probability, see, e.g., Rodano, Serrano-Velarde, and Tarantino (2016), Favara et al. (2017), Gross et al. (2021), and Schoenherr and Starmans (2022).

¹⁸ I am indebted to Joonkyu Choi, Terence Halliday, and Haksoo Ko for all their comments and advice on the bankruptcy law in Korea.

¹⁹ The law does not provide an automatic stay of assets. However, the court can issue a comprehensive stay order that prohibits all creditors from seizing debtor's assets. Before the reform, a stay order was not comprehensive and was issued on a case by case basis separately for individual creditors (Ko, 2007; Park, 2008).

²⁰ Note that the incumbent management may not be allowed to remain control during bankruptcy if insolvency was caused by material mismanagement, creditors make a request for a reasonable cause, or it appears necessary for firm restructuring to appoint a third party receiver (Park, 2008). However, in practice, management maintained control in most situations (Park, 2008; Schoenherr and Starmans, 2021). Moreover, the exceptions noted above do not apply for many cases, most importantly for my study, when the company is listed on the stock exchange or registered on KOSDAQ (Park, 2008). Given that my study is based on firms listed on KOSDAQ, management of my sample firms stays in control by law.

Empirically, Schoenherr and Starmans (2021) show that, under the pre-reform law, management remained in corporate control during bankruptcy in 5% of the bankruptcy cases and ownership transfer occurred in 91% of cases that do not result in liquidation. In contrast, under the post-reform law, management remains in control in 88% of cases and ownership transfer occurs in 19% of reorganization cases.²¹

2.2.3.2 History of the reform

The reformation process was prompted by the 1997–1998 Asian financial crisis. To receive financial support, in 1998, the Korean government agreed to the IMF and World Bank to modernize the bankruptcy law according to global norms. In particular, the IMF and World Bank demanded the unification of the existing system of three separate bankruptcy laws (a creditor-friendly law for large firms, a debtor-friendly law for small firms, and a private law) into a single statute, which was considered to be a more efficient and predictable solution to bankruptcy (Halliday and Carruthers, 2009; Schoenherr and Starmans, 2021).²²

The reform process was hallmarked by different opinions on the actual usefulness of a reform, as well as conflicting opinions between various participants about the actual design of the reform (particularly, whether the law should be debtor- or creditor-friendly), leading to a back and forth between alternative sites of the reform through 1998 to 2005 (Halliday and Carruthers, 2009).^{23,24} From 1998 to 2001, the government sought opinions from the various interest

²¹ Larger or listed firms in general were not bailed-out by the government in case of bankruptcy. See, e.g., Nam and Oh (2000), Kim (2006), Halliday and Carruthers (2007), and Lee (2011), who discuss bankruptcy cases of large Korean chaebols and suggest that management and controlling shareholders were dismissed and lost corporate ownership. Also note that the bankruptcy data by Schoenherr and Starmans (2021) are based on a sample of large firms. In fact, the Korean public rather had little tolerance to rescue large firms in bankruptcy, which is also reflected in the fact that, before the reform, small firms typically had the opportunity to recover under a debtor-friendly law, while large firms were typically subject to a creditor-friendly law (Halliday and Carruthers, 2009).

²² Note that, given that my sample consists of large listed firms, the relevant pre-reform bankruptcy law in Korea for my analysis is the creditor-friendly law for large firms described in Section 2.2.3.1 above. Schoenherr and Starmans (2021) show that the debtor-friendly law for small firms applied primarily for firms with assets below 7 billion KRW (about 6 million USD). For comparison, the average asset size in my sample is 7.074 billion KRW (about 6 billion USD).

²³ Although the Korean government agreed to the IMF and World Bank to adjust its bankruptcy law with regard to the international norm of a uniform law, much disagreement existed over the actual usefulness of a reform, given that the existing system was seen by many as a suitable solution for the Korean economy (Halliday and Carruthers, 2009). Additionally, there was a major conflict between the various participants in the reform process about the design of the bankruptcy law. While a debtor-friendly law was preferred by the IMF, World Bank, economists, and firms, a creditor-friendly law was preferred by the lawyers, banks, judges, and the Korean public (who had little tolerance for a law that appears to coddle incompetent managers and owners) (Halliday and Carruthers, 2009). As noted by Halliday and Carruthers (2009, pp. 223–224), “The struggle can be observed in the switching back and forth between alternative sites of reform (. . .) From early 1998 until 2005 the protagonists in Korea’s bankruptcy reforms offered one solution after another”

²⁴ The IMF also demanded a modernization of the law in other countries that were affected by the Asian financial crisis, such as Indonesia. In contrast to Korea, the reform in Indonesia was passed closely after the crisis in 1998 (Halliday and Carruthers, 2009; Schoenherr and Starmans, 2021). This further emphasizes the conflicting opinions during the reformation process in Korea.

groups. From 2001 to 2003, a Ministry of Justice drafting team worked on several drafts, which were sent out to the participants for review and comments. In 2003, the Ministry of Justice submitted a unified law to the National Assembly. “With the change of government in March 2004, and in response to many critical comments from the Judiciary Committee, the draft was further revised and introduced again in 2004. After navigating its way through subcommittees of the National Assembly, amid some considerable controversy over several provisions, the unified law was enacted by the National Assembly in March 2005 to become effective twelve months later.” (Halliday and Carruthers, 2009, p. 235).²⁵

For my empirical analysis, I define the date after which changes in the voting premium should have occurred by the enactment of the reform in 2005. Because of the different opinions, until the end there was uncertainty about whether the reform would be passed at all, and about important provisions of the new law (such as automatic stay or management stay) that essentially determine whether a bankruptcy procedure is creditor- or debtor-friendly, making it unlikely that participants made major adjustments before the reform was passed—in my case, to buy voting shares with a significant premium. Even if some adjustments were made in anticipation of the reform, major changes should have occurred when the enactment of the reform eliminated uncertainties. Later, I will show that high and low default probability firms followed parallel trends in the voting premium before the reform was passed, while firms with high default probability exhibited an increase in the voting premium relative to firms with low default probability closely around the enactment of the reform in 2005.

2.3 The voting premium as a measure of agency problems

2.3.1 Theoretical and empirical background

I measure private benefits of control by the voting premium in dual-class firms. The idea behind the voting premium as a measure for control benefits is that if control is valuable, this should be reflected in the price of voting shares, which allocate control. Because minority shareholders will never access private benefits of control, the voting premium reflects the expected price minority shareholders will receive when a large shareholder accumulates shares to form a controlling block (Zingales, 1994, 1995; Chung and Kim, 1999; Nenova, 2003; Doidge, 2004; Guadalupe and Pérez-González, 2010; Hong, 2013). Note that the voting premium not only depends on the size of control benefits but also on the probability of a control contest, as for a

²⁵ Overall, the final outcome of the reform is a reflection of the demands of the different parties (as with the 1979 US bankruptcy reform, see Posner, 1997). While the law in general is a debtor-friendly law as demanded by some groups, unlike their demands, the law did not include automatic stay or unconditional management stay provisions which reflects the demands of other groups.

given control benefit the voting premium increases with the possibility of a control contest (Zingales, 1994, 1995; Chung and Kim, 1999; Nenova, 2003). As a result, possible changes in the voting premium as a reaction to changes in agency problems should be concentrated among firms for which a control contest is more possible, which typically is the case when existing ownership structure is more dispersed.²⁶²⁷ Later, I will exploit this feature to study cross-sectional variation in possible changes of the voting premium.

The relation between the voting premium and the value of private benefits of control is not only theoretically given, but also empirically supported. Among others, Nenova (2003), Doidge (2004), Guadalupe and Pérez-González (2010), Hong (2013), and Gao and Zhang (2019) find that the voting premium is negatively related to improvements in corporate governance mechanisms, such as law enforcement, minority shareholder rights, competition, reporting standards, and board independence. Additionally, Doidge (2004) and Kim and Michaely (2019) find that the voting premium increases in firm characteristics that are typically associated with potentially extreme agency problems, such as higher firm age and a larger separation of ownership and control. Finally, Zingales (1994, 1995) and Chung and Kim (1999) show that the voting premium is closely related to a firm's ownership structure, consistent with the underlying mechanism of the voting premium as a measure for control value.

Following Zingales (1995) and Doidge (2004), I define the voting premium by the ratio of the price of a voting right to the price of a cash flow right:

$$VP = \frac{P_H - P_L}{P_L - rv * P_H}, \quad (2.1)$$

where P_H is the price of the high-voting share, P_L is the price of the low-voting share, and rv is the number of votes of a low-voting share relative to the number of votes of a high-voting share. Note that when the high-voting share has one vote and the low-voting share has no vote ($rv =$

²⁶ Note that market prices reflect prices among minority shareholders. Large shareholders typically trade their blocks outside the market. This is why possible changes in the voting premium as a reaction to changes in agency problems should be stronger for firms in which the possibility to acquire control within the market is higher (although prices of voting shares held by insiders may increase as well). On this, see Chung and Kim (1999).

²⁷ Note that it is not necessary to have a majority ownership (more than 50% of the voting shares) to be the controlling shareholder. Typically, control is achieved with far less ownership (La Porta, Lopez-De-Silanes, and Shleifer, 1999). Also, it is not necessary to be a single controlling shareholder to access control benefits, as control benefits can be shared with other owners. Moreover, even in situations in which a majority owner exists, voting shares should be valuable, given that one can share parts of the control benefits or because future allocation of majority ownership is uncertain and votes are important to gain control in the future (see Zingales, 1995; Doidge, 2004, who also discuss events, such as family disputes or death of the owner, in which majority ownership breaks open). Overall, the probability of a control contest should generally increase in ownership dispersion, as dispersed ownership makes the acquisition of a controlling stake more possible and more valuable (as one does not need to share the control benefits).

0), the voting premium is simply given by $(P_H - P_L)/P_L$. In fact, this is the case for the analysis in Korea, where dual-class firms have ordinary shares with one vote and preference shares with no vote. In Section 2.6.2, to mitigate the concern of confounding events, I will also study changes in the voting premium of firms in other countries around the reform in Korea. Here, the refined measure of equation (2.1) allows to compare the voting premium across countries with different voting arrangements.

2.3.2 Potential problems of the voting premium

The voting premium as a measure of private benefits of control has two potential problems. The first problem is that the voting premium may be subject to measurement errors. That is, the voting premium is not only determined by control benefits, but also by other factors that are unrelated to control benefits.²⁸ However, this problem is less relevant in my study, given that I am not primarily interested in the size of control benefits, but rather in changes of control benefits as a reaction to changes in corporate governance. Moreover, the problem is alleviated by the fact that I compare the voting premium of high and low default probability firms, and there is no obvious reason to expect that a possible measurement error is disproportional across both groups of firms. Finally, in all regressions, I use firm fixed effects that account for unobservable time-invariant factors, and I furthermore control for firm and security variables that have been shown to be important determinants of the voting premium (Zingales, 1994, 1995; Nenova, 2003; Doidge, 2004). These variables include ownership structure, firm size, voting power, and differences in dividends and liquidity between high- and low-voting shares. For certain analyses, I extend the list of controls by variables that are commonly used in the corporate finance literature, such as leverage, profitability, or Tobin's Q.

The second problem is that an analysis of dual-class firms might be subject to a selection bias problem. Specifically, my results may not be applicable for single-class firms if dual-class firms differ from single-class firms with respect to the relation between private control benefits and debt governance. One specific concern here is that agency problems in dual-class firms may be higher than in single-class firms, because in dual-class firms the wedge between control and ownership might be larger, and because especially firms with potentially large agency problems may adapt dual-class share structures to exploit this wedge.^{29,30} However, two points are

²⁸ See Zingales (1994, 1995), Nenova (2003), Doidge (2004), and Levit, Malenko, and Maug (2021) for detailed discussions on possible determinants of the voting premium.

²⁹ On the possible selection bias problem regarding studies of dual-class firms, see Doidge (2004), Gompers, Ishii, and Metrick (2010), and Kind and Poltera (2013).

³⁰ As indicated earlier, another reason is that debt may be special for solving agency problems in dual-class firms, given that traditional governance mechanisms may not apply for controlling shareholders due to their high

important to note in this context. First, the primary purpose of my study is to provide evidence on dual-class firms. As noted above, dual-class firms are widespread worldwide and additionally have become increasingly important in recent years. Given this, research on dual-class share structure is of great importance. Second, while my results are particularly useful for dual-class firms, they still may also be relevant for single-class firms. The potential upward bias in agency problems may be alleviated given that also single-class firms can separate control rights from cash flow rights by pyramid structures (Doidge, 2004). Furthermore, more recently, several studies suggest that firms may not adopt a dual-class share structure because of agency problems, but rather because such a structure allows firms protection from less-informed or short-termed outsiders (Bebchuk and Kastiel, 2017; Kim and Michaely, 2019; Aggarwal et al., 2021; Lel et al., 2021; Cremers, Lauterbach, and Pajuste, 2022), which additionally mitigates the potential upward bias.³¹ In Section 2.6 below, I will show that the main results of this paper are supported when using alternative measures of control benefits that are also not based on the voting premium in dual-class firms, thereby further mitigating concerns of potential measurement errors and allowing for better inference on single-class firms.

2.3.3 The voting premium and takeover regulations in Korea³²

Takeover regulations can influence the validity of the voting premium as a measure of control benefits. For example, there can be a provision that restricts outsiders to acquire more than a certain percentage of voting shares. For a given level of control benefits, such a provision would reduce the voting premium by reducing the possibility of a control contest (Chung and Kim, 1999; Nenova, 2003) (similar to concentrated ownership). Likewise, there can be a provision mandating that an outsider who acquires control must make a bid to the remaining shareholders to acquire their shares at the same price. Such equal price provisions (both between share classes and across share classes) can hamper takeover activity by making acquisitions more costly (Chung and Kim, 1999; Nenova, 2003; Varottil and Wan, 2021) and can moreover lead to zero voting premium between high- and low-voting shares even if the value of control

degree of control (Dey, Nikolaev, and Wang, 2016; see also Masulis, Wang, and Xie, 2009; Bebchuk and Hamdani, 2017; Bebchuk and Kastiel, 2017; Aggarwal et al., 2021).

³¹ Consistent with these arguments, Dyck and Zingales (2004, p. 566), who estimate control benefits in single-class firms, compare their findings to those of Nenova (2003), who estimate control benefits in dual-class firms, and conclude: “In spite of the different method used, there is a remarkable similarity in findings. Our estimates for countries like Mexico and Germany are identical, and the overall correlation between our measures is 0.59 for the raw measure and 0.62 for the refined measure (statistically different from zero at the 2 percent level).” Moreover, Kim and Michaely (2019) and Cremers, Lauterbach, and Pajuste (2022) find that dual-class firms on average do not have lower valuation and performance than comparable single-class firms.

³² I am indebted to Kyung-Hoon Chun for his invaluable detailed comments and advice on the takeover market in Korea. I am also grateful to Umakanth Varottil and Wai Yee Wan for all their comments and suggestions.

is positive (Nenova, 2003). However, no such provisions existed in Korea during the sample period.³³³⁴

In fact, Korean takeover regulations offer many additional features that further support the assumptions of the voting premium as a measure of control benefits. For example, the Korean takeover market is comparable to the market rule of the US, where takeovers are encouraged by refraining from regulations that may restrict takeover activity (Varotttil and Wan, 2021).³⁵ Moreover, the Korean law stipulates that any acquirer who wants to acquire more than 5% of shares in a company outside the securities market may only do so through a tender offer at the same price to other shareholders, which makes off-market acquisitions costly and thus enables takeovers especially by forming coalitions within the security markets (Chung and Kim, 1999; Varotttil and Wan, 2021). Based on this, Chung and Kim (1999, pp. 37–38) argue: “The Korean market for dual class equity closely approximates the assumptions behind theoretical models of voting premium. In Korea, there are general provisions that make it difficult for acquirers to offer unequal takeover bids to different classes of securities of target companies. Therefore, the value of the vote arises primarily from the opportunity to form coalitions without any formal takeover bid. Because of these unique features, the Korean securities market provides an excellent opportunity to study the determinants of voting premiums and to appraise the control value of a block of shares.”

2.3.4 Debt governance and firm value as a measure of agency problems

Before proceeding, it might be instructive to explain why I use the voting premium to measure changes in agency costs, and not the firm value. Most studies infer changes in agency problems by looking at changes in the firm value, because potential agency problems should be

³³ A law that prohibited the acquisition of more than 10% of votes by an outsider existed until 1997, but was repealed to enhance managerial efficiency by facilitating control changes (Chung and Kim, 1999). A law requiring outsiders who acquire more than 25% of shares to make a tender offer at the same price to other shareholders was introduced in 1997, but was repealed shortly thereafter in 1998, as a response to the 1997–1998 Asian financial crisis in order to facilitate control changes (Varotttil and Wan, 2021).

³⁴ Like country-level takeover regulations, also corporate charter provisions can influence the validity of the voting premium as a measure of private benefits of control. In particular, firms can include voting cap provisions that stipulate an upper limit of votes that an outsider can acquire, or coattail provisions that mandate equal treatment of shares in control changes (Nenova, 2003). I do not control for corporate charter provisions in my analysis. However, for ten randomly selected firms of my sample (five firms with high default probability and five firms with low default probability), I searched for such or similar rules in the annual reports. I could not find any. Note that, to affect my results, the possible omission of takeover provisions needs to be disproportionate across high and low default probability firms. Also note that while agency problems of low default probability firms should not increase as a reaction to reduced bankruptcy penalties, agency problems of high default probability firms should increase, which suggests that omitting takeover provisions rather lead to a downward bias of my results.

³⁵ For further reading on the Korean takeover market, see also Kim (2009), who provides empirical support that the takeover market in Korea is well functioning by studying Korean takeover regulations in the context of four case studies (SK Corporation’s fight against Sovereign Asset Management, the contest for control over Hyundai, KT&G’s fight against Carl Icahn, and LG and Carlyle’s proxy contest against Hanaro Telecom).

reflected in the valuation of the firm (e.g., Grossman and Hart, 1982; Zwiebel, 1996; Gompers, Ishii, and Metrick, 2003; Harvey, Lins, and Roper, 2004). However, in the context of debt governance this method may not be very meaningful, as bankruptcy law not only affects the effectiveness of debt governance but might also affect credit costs and distress costs, which in turn might affect firm value (Acharya, Amihud, and Litov, 2011; Vig, 2013; Mann, 2018; Canipek, Kind, and Wende, 2021; Schoenherr and Starmans, 2021). As such, possible changes in the firm value may not be because of a change in debt governance, but may come through changes in credit costs and distress costs. Note how looking at the voting premium addresses this problem. While the potential effect of changes in credit costs and distress costs affects high- and low-voting shares similarly by the cash flow component, the potential effect of changes in debt governance affects both share classes differently (presumably, less debt governance has a positive effect on the voting share, but a negative effect on the nonvoting share). The voting premium, therefore, allows to measure changes in agency costs without potential influences by one of the other two channels.

2.4 Data, summary statistics, and empirical method

2.4.1 Data

I begin the sample construction by identifying all firms with dual-class shares provided by Datastream during the sample period. For these firms, to be included in the sample, the following restrictions are required (Nenova, 2003; Doidge, 2004; Hong, 2013): (1) firms have at least two classes of shares with different voting rights, (2) both shares are publicly traded and listed on a domestic stock exchange, (3) the low-voting class is not convertible into the high-voting class, and (4) neither share class receives a fixed dividend independent of the other class nor is redeemable or callable at the option of the firm at a preset price.

The sample period is from 2000 to 2009, that is, I study five years before and after the enactment of the reform in 2005. For certain analyses, I extend the sample period to 2012. For each share class, I obtain Wednesday-to-Wednesday weekly data from Datastream for closing stock price, dividend yield, number of shares outstanding, and turnover. Following the voting premium literature (Doidge, 2004; Hong, 2013), for a given year, a firm is included in the sample only if it has at least 20 weekly observations. The data on other firm characteristics, including ownership and size (both as firm-level control variables) as well as the necessary variables to construct Altman's Z-score (as a firm-specific measure of default probability), come from Worldscope and are based on annual data. I exclude firms from financial (SIC codes 6000–6999) and regulated industries (SIC codes 4800–4949), and I exclude each firm-year

observation without complete data on all dependent and control variables used in the main empirical analysis. Additionally, I exclude firms without data on Altman's Z-score, and I require each firm to have observations for the pre-reform and post-reform periods (Hong, 2013).³⁶ Finally, I winsorize all variables at the first and ninety-ninth percentiles to account for outliers. The final sample consists of 663 firm-year observations from 77 firms. For robustness, in certain analyses, I additionally present results when also including firms without ownership data, which increases the sample size to 939 firm-year observations coming from 99 firms.

2.4.2 Summary statistics

The voting premium of equation (2.1) is computed by the weekly price data for each firm and averaged annually for each year. As for the voting premium, other variables that are based on weekly Datastream data are calculated weekly and averaged annually (Nenova, 2003; Doidge, 2004). The variables are defined in detail in Table A.2.1 in the Appendix. Table 2.1 provides summary statistics, separately for firms with high and low default probability, and for the periods before and after the reform. Based on the two-year average pre-reform (2003 to 2004) measure of default probability (Altman's Z-score; Altman, 1968), I divide firms at the median and define firms above the median bankruptcy probability distribution as high default probability firms and firms below the median as low default probability firms.³⁷

Three points are worth mentioning. First, the average voting premium in Korea is 68% (see columns 1 and 6). In other words, corporate control is worth more than two thirds of the cash flow value (without controlling for differences in security characteristics), consistent with the notion that private benefits of control can be large in dual-class firms.³⁸

Second, before the reform, the voting premium was 51% for firms with high default probability and 65% for firms with low default probability (see columns 4 and 9). In contrast, after the reform, the voting premium was 87% for firms with high default probability and 70% for

³⁶ For most of the sample firms, ownership information for 2008 are not available in Worldscope. In case of missing data for 2008, I use the ownership value from 2009 also for 2008. My results remain unaffected when excluding firm-years with missing ownership data for 2008.

³⁷ The separation of firms at the median (or any other point) based on the (average pre-event) distribution of a specific variable follows the approaches by Chava and Roberts (2008), Giroud and Mueller (2010, 2011), Ferreira, Ferreira, and Raposo (2011), Favara, Schroth, and Valta (2012), Vig (2013), Rodano, Serrano-Velarde, and Tarantino (2016), Favara et al. (2017), and Schoenherr and Starmans (2021).

³⁸ My estimate of the average voting premium in Korea is similar to the estimates of existing studies (e.g., Nenova, 2003; Doidge, 2004; Hong, 2013). Also the estimates of the voting premium in the other countries of Section 2.6.2 are similar. Note that I generally have a lower number of firms than existing studies. The reason is that my sample restrictions are somewhat stricter (e.g., different than other studies, I exclude financial and regulated industries), in particular, with regard to data availability on ownership structure. For example, for a similar time period as in my paper, from 2002 to 2007, Hong (2013) identifies 100 dual-class firms in Korea without controlling for ownership. As noted above, I obtain 99 firms when also including firms without ownership data, which is similar to the analysis by Hong (2013).

firms with low default probability (see columns 5 and 10). This suggests that firms with high default probability exhibited an increase in the voting premium after the reform relative to firms with low default probability by 31 percentage points. In unreported results, I subtract the before-after reform difference in the voting premium of firms with high default probability from that of firms with low default probability, that is, I estimate a simplified version of the difference-in-differences regression of equation (2.2) below (i.e., without firm and year fixed effects and without controls). The increase in the voting premium of high default probability firms relative to low default probability firms is statistically significant at 5% level.

Third, on average, 35% of Korean shares are held by insiders (see columns 1 and 6), implying that ownership in Korea is relatively dispersed and thus providing the possibility for outsiders to accumulate control. In addition, the within-firm standard deviation of ownership suggests that ownership varies noticeably over time (see columns 3 and 8), which is broadly consistent with the view that there are no major takeover regulations in Korea that may restrict takeover activity.

2.4.3 Empirical method

If debt curbs agency problems in dual-class firms, the expectation is that the voting premium of firms with high probability of default increased after the reform in Korea relative to firms with low probability of default. I test for the expected effect with the following difference-in-differences regression:

$$VP_{it} = \alpha_i + \lambda_t + \eta Post_t + \gamma HighRisk_i + \delta(Post_t \times HighRisk_i) + \beta X_{it} + \varepsilon_{it}, \quad (2.2)$$

where VP_{it} is the voting premium as specified in equation (2.1); i indexes firms, and t indexes time; α_i are firm fixed effects that account for time-invariant differences across firms; λ_t are year fixed effects that account for time-varying factors common to all firms; $Post_t$ is a dummy variable that equals one for the years after the bankruptcy reform was passed (2005 to 2009), and zero for the years before the reform was passed (2000 to 2004); $HighRisk_i$ is a dummy variable that equals one if the firm has a high probability of default (above the median of the bankruptcy probability distribution), and zero if it has a low probability of default (below the median); X_{it} are the above discussed time-varying firm and security characteristics that are possible determinants of the voting premium; ε_{it} is the firm-specific error term; and δ is the coefficient of interest, which measures the change in the voting premium of firms with high probability of default after the reform relative to firms with low probability of default (the

difference-in-differences effect). Standard errors are clustered at the firm level, as the voting premium should be correlated over time within a firm.³⁹

2.5 Results

2.5.1 Main results

Columns 1 and 2 of Table 2.2 present the main results. Consistent with the hypothesis that debt induces discipline, columns 1 (without controls) and 2 (with controls) show that the coefficient on $Post_t \times HighRisk_i$ is positive and statistically significant. That is, firms with high default probability exhibited an increase in the voting premium after the reform relative to firms with low default probability. The economic effect is notable. Column 2 shows that, relative to firms with low default probability, the voting premium of firms with high default probability increased by 41 percentage points after the reform. This implies an increase in the voting premium by $0.41 / 0.69 \times 100 = 59\%$ (calculated by the full sample average voting premium of high default probability firms, see Table 2.1), suggesting that performance pressure by debt is viewed as a powerful instrument for solving agency problems.

It is important to note that, given that pricing is forward-looking, the results also suggest that debt governance is not fully substitutable by other incentive schemes, consistent with the hypothesis that debt is special in solving agency problems in dual-class firms (see Dey, Nikolaev, and Wang, 2016, who provide a detailed discussion on the specialness of debt). To further confirm this, I study the voting premium over a longer time period. In column 3, I extend the analysis by three years, from 2009 to 2012. Additionally, in column 4, I maintain the extended sample but also drop the three years right after the reform was passed, 2005 to 2007. As shown, the increase in the voting premium remains for many years after the introduction of the reform.

For robustness, in columns 5 to 10, I present the estimates of several alternative specifications. Specifically, column 5 presents the estimates when additionally controlling for corporate leverage, profitability, and Tobin's Q. Columns 6 and 7 respectively show the estimates when excluding penny stocks from the analysis and when considering a balanced panel in which firms have observations throughout the entire sample period. Column 8 reports the results when using the log of the voting premium as the dependent variable.⁴⁰ In column 9, I report the estimates

³⁹ The difference-in-differences specification follows those employed by Vig (2013) and Favara et al. (2017). Note that also parts of the table descriptions (particularly for Tables 2.2–2.4) follow Vig (2013) and Favara et al. (2017).

⁴⁰ Note that high and low default probability firms have about the same average voting premium (see the summary statistics in Table 2.1). This largely excludes the possibility that the voting premium of high default probability firms have increased relative to low default probability firms in terms of a percentage points change, but not in terms of a percentage change. As shown in column 8, using the log of the voting premium as the

for an extended sample that also includes firms without ownership data. Finally, in column 10, I show the results when repeating the empirical analysis by dividing firms into high and low default probability firms based on their default probability in the year before the reform (2004), instead of the two-year pre-reform average (2003 to 2004). As can be seen, throughout all specifications, the results are (qualitatively) similar to those of the main analysis in column 2.

2.5.2 Parallel trends

Comparing changes in the voting premium of firms with high default probability to those of firms with low default probability allows to account for confounding events common to all firms. However, a potential concern with my results is that a confounding factor affected the voting premium of high and low default probability firms differently. To mitigate this concern, in Figure 2.2, I plot changes in the voting premium for high and low default probability firms over time. Specifically, Panel A shows the evolution of the average voting premium for high and low default probability firms. To examine the statistical significance of possible changes, Panel B plots the yearly differences in the voting premium between high and low default probability firms relative to the difference in the year prior to the reform, 2004.⁴¹ As shown, while high default probability firms exhibited an increase in the voting premium relative to low default probability firms after the reform, high and low default probability firms followed parallel trends in the voting premium several years prior to the reform, suggesting that both groups of firms generally experience similar shocks to the premium and thus would have followed the same trend without the reform.

Notably, the voting premium increased right after the enactment of the reform from 2004 to 2005, and additionally from 2005 to 2006. This is consistent with the timing of the reform, given that the reform was only passed in March 2005 and thus parts of the 2005 voting premium may contain pre-reform levels (recall that the yearly voting premium is calculated by weekly data).⁴²

dependent variable to measure percentage changes in the voting premium yields similar results (the effect in column 8 is 55% and the implied effect from column 2 is 59%). In unreported results, I repeat the analysis using the log of the voting premium plus one to also consider observations with negative value. The estimated did coefficient and *t*-value respectively are 0.252 and 3.28. The number of firms and observations are as in column 2. Note that adding one to the voting premium considerably reduces the economic effect relative to the true size, given that the average voting premium is about 0.7.

⁴¹ The empirical method follows that used by Favara et al. (2017) and Jensen and Johannesen (2017). Note that Table A.2.2 in the Appendix follows Table A4 of Jensen and Johannesen (2017).

⁴² The voting premium also increased from 2008 to 2009. One explanation for this is that for some firms ownership structure may have changed. While changes in ownership might not affected the voting premium before the reform (when the effectivity of debt governance reduced agency problems and thus the influence of ownership structure on the validity of the voting premium as a measure of agency problems), changes in ownership might affect the voting premium after the reform (when the ineffectivity of debt governance increases agency problems).

2.5.3 Heterogeneity

To further support my results, I study variation in the increase of the voting premium across firms with high default probability. As noted earlier, changes in the voting premium as a response to changes in private benefits of control should be concentrated in firms in which the probability of a control contest is higher. Moreover, the effect should be stronger in firms in which potential agency problems are extreme.⁴³ Following the voting premium literature (Zingales, 1994, 1995; Chung and Kim, 1999; Nenova, 2003), I use the existing ownership structure of a firm to proxy for the possibility of a control contest. The possibility of a control contest is high when ownership is dispersed and low when ownership is concentrated.⁴⁴ I measure agency problems by firm age and firm size. Firm age is based on the current dual-class literature, which suggests that the potential benefits of dual-class share structure tend to decrease and the potential costs tend to increase over time (Bebchuk and Kastiel, 2017; Kim and Michaely, 2019; Cremers, Lauterbach, and Pajuste, 2022). Firm size is based on the notion that controlling a large empire may entail private control benefits (e.g., Hart, 1995; Zwiebel, 1996).

To test for the expected heterogenous effects, I divide firms with high default probability into two groups of firms with high and low ownership concentration/agency problems, and then

Although I control for ownership, the employed ownership data may not fully capture all relevant information (see footnotes 44 and 45 below).

⁴³ See, e.g., Hart (1995) and Harvey, Lins, and Roper (2004), who argue that corporate governance is more important for firms in which agency problems are stronger.

⁴⁴ As data for ownership concentration, I use the ratio of the number of closely held shares by insiders (shareholders who hold at least 5% of the outstanding shares, such as officers and directors and immediate families, other corporations, or individuals) to the number of shares outstanding (Worldscope item 08021). This data item should offer the information needed, namely, the proportion of shares that are typically not traded on the market, and whether there is a majority owner in the firm (see footnotes 26 and 27). Note that an alternative method that is often used in the literature is to look at the ownership stake of the three largest shareholders. In unreported results, I compare country means of my ownership data (for the list of countries in Table 2.6 below and based on the period 2002–2016) to those provided by La Porta et al. (1998) who apply the alternative method. The correlation coefficient is 0.689, suggesting that both measures lead to similar results. An additional note on the ownership data used in my study is that the item indicates ownership not solely regarding voting shares but regarding to all shares. This makes it possible that the indicated ownership concentration is low, although ownership concentration with regard to voting shares is actually high. However, the concern is mitigated by the fact that insiders typically hold voting shares. Furthermore, in Korea, there are typically more voting shares than nonvoting shares, which additionally reduces the concern. Finally, the goal of my analysis is to examine whether possible changes in the voting premium are stronger for firms with dispersed ownership as compared to firms with concentrated ownership, and to the extent that possible measurement errors are similar across firms the ownership data should allow for a meaningful analysis.

⁴⁵ The probability that shares of minority holders will be demanded in a control contest might be nonlinear in ownership concentration. The probability may be lowest for high ownership concentration, highest for a medium concentration, and medium for dispersed ownership because here it might be that not all votes are pivotal in allocating control (Zingales, 1994, 1995; Nenova, 2003; Kind and Poltera, 2013). Additionally, the probability may also depend on other factors than ownership concentration. For example, Zingales (1994, 1995) and Nenova (2003) use the Shapley value which, besides ownership concentration, also accounts for the number, compensation, and interaction between the vote-holders. However, note that Zingales (1994) shows that a simple dummy for majority control yields almost identical results as the Shapley value. Finally, note that the goal of my cross-sectional analysis is to examine whether possible changes in the voting premium are stronger when a control contest is more possible, which should be achieved by comparing firms with dispersed ownership to firms with concentrated ownership.

compare each group to the control group of firms with low default probability. Risky firms are divided at the median, based on the two-year pre-reform average measures of ownership concentration and agency problems. Columns 1 to 6 of Table 2.3 present the results. Consistent with the cross-sectional predictions, the increase in the voting premium relative to firms with low default probability is stronger for those high default probability firms in which ownership is more dispersed (columns 1 and 2) and in which potential agency problems are stronger (columns 3 to 6). In fact, while the increase in the voting premium is still existing for risky firms with concentrated ownership and low agency problems, the increase is more than one third larger for risky firms with dispersed ownership and high agency problems. Notably, heterogeneity in the effect occurs particularly across firm age, consistent with the notion that the potential benefits and costs of dual-class share structure change over time.

In columns 7 to 9, I examine whether the differential effects among risky firms are also statistically significant. Specifically, among risky firms, I directly compare firms with high and low ownership concentration/agency problems. As can be seen, risky firms with low ownership concentration (column 7)/high agency problems (columns 8 and 9) experienced an increase in the voting premium after the reform relative to risky firms with high ownership concentration/low agency problems.

Finally, in unreported tests, I repeat the analysis in Table 2.3 for safe firms, that is, I study variation in the voting premium across safe firms with differences in ownership concentration and agency problems. I find no differential changes in the voting premium among safe firms.

2.6 Confounding factors

As noted, a potential concern with my findings is that a confounding factor affected high and low default probability firms differently. The parallel trends between high and low default probability firms prior to the reform already mitigates this concern. Moreover, it is important to note that a confounding factor not only had to have a different effect on firms with high and low default probability, but also a different effect across firms with different levels in ownership concentration, size, and age, given the heterogeneities discussed above.⁴⁶ In this section, I present additional evidence that further mitigate the concern of confounding events.

⁴⁶ On the problem of confounding factors and the possibility to mitigate the problem by studying heterogeneity in firm characteristics, see Vig (2013) and Schoenherr and Starmans (2021).

2.6.1 The 2007–2009 financial crisis

One specific confounding factor might be the 2007–2009 financial crisis. Figure 2.2 above shows when the observed changes in the voting premium occurred. As can be seen, the increase in the voting premium of high default probability firms relative to low default probability firms occurred shortly after the enactment of the law in 2005, and not in 2008 when the financial crisis began to affect the economy of Korea (Schoenherr and Starmans, 2021).⁴⁷ To further confirm this, column 1 of Table 2.4 presents the estimates of a shortened sample period from 2000 to 2007, which excludes the years after the financial crisis. As shown, changes occurred after the reform, but before the crisis. Finally, in column 2, from the main sample period 2000–2009, I exclude the 2008–2009 period during which Korea was affected by the crisis, and furthermore extend the sample period to 2012. As can be seen, the results remain unaffected when excluding the period of the financial crisis and persist for several years after the crisis.⁴⁸ Taken together, the results suggest that my findings are not driven by the financial crisis.

2.6.2 Changes in the voting premium of firms in other countries

Next, I study whether in other countries firms with high default probability have exhibited a change in the voting premium relative to firms with low default probability around the reform in Korea. This analysis further mitigates the concern of confounding events by accounting for the set of possible confounding shocks commonly experienced by all countries, such as economic crises or international trends in corporate governance.

For simplicity, I only consider countries where the voting arrangement is the same across companies. The information regarding the voting arrangements come from Doidge (2004).⁴⁹ Additionally, I exclude Brazil and Italy from the analysis, as these countries have also

⁴⁷ As noted by Schoenherr and Starmans (2021), while the Korean economy was not directly affected by the subprime exposure, as an export-dependent economy Korea experienced lower growth rates in 2008 and 2009. Growth rates were back to pre-2008 levels in 2010.

⁴⁸ The empirical approach here is similar to that used by Schoenherr and Starmans (2021), who, by examining different sample periods, show that their results regarding the effect of the Korean bankruptcy reform on corporate financing and investment are not affected by the 2007–2009 financial crisis.

⁴⁹ I exclude countries where firms have different voting arrangements. For example, in Canada, firms can have high-voting shares with voting rights from 10 to 500 times the voting rights of low-voting shares (Doidge, 2004). The excluded countries are Australia, Canada, Finland, Mexico, South Africa, the US, and the UK. Given that there is no obvious reason why changes in the voting premium of high default probability firms relative to low default probability firms should be different across countries with different voting arrangements, my analysis should not be affected by the exclusion of these countries. Note: as shown in Table 2.5, my simplified analysis includes 61 dual-class firms. For a similar time period as in my analysis, over the period 2002 to 2007, Hong (2013) also identifies dual-class firms for the excluded countries and shows that the total number of these firms is 109 (the number of firms is 3, 34, 14, 3, 11, 36, and 8, respectively, for the above listed excluded countries), indicating that my simplified analysis accounts for more than one third of the expected full sample.

introduced bankruptcy reforms around 2005.⁵⁰ For each country, the data collection and construction is as for Korea, described in Section 2.4.1. In an additional specification, I also control for economic development (GDP per capita), which may differ across countries and may affect the voting premium.⁵¹ The data are sourced by the World Bank. As for the firms in Korea, based on the two-year average pre-reform default probability, I divide firms into high and low default probability firms at the median of the bankruptcy probability distribution of all firms. Because the median Z-score here is 2.40, while it is 1.62 in Korea, I also present the results when dividing firms into high and low default probability firms at the 1.62 threshold. Table 2.5 provides the list of countries, number of dual-class firms, the voting arrangements for each country, as well as different summary statistics.⁵²

Columns 3 (with controls) and 4 (with economic development as an additional control) of Table 2.4 report the results. As can be seen, in other countries firms with high default probability did not exhibit an increase in the voting premium relative to firms with low default probability after the Korean reform. Columns 5 and 6 present the estimates when dividing firms at the 1.62 threshold. As shown, the results remain unaffected.⁵³ In Panels A (2.40 threshold) and B (1.62 threshold) of Figure 2.3, I show changes in the voting premium for high and low default probability firms of other countries over time. As visible, high and low default probability firms followed parallel trends in other countries, both before and after the reform.

For robustness, I also directly compare changes in the voting premium of Korean firms to those of firms from other countries. Specifically, columns 7 and 8 respectively present the estimates when comparing risky firms from Korea to all firms from other countries and when

⁵⁰ Around 2005, Brazil and Italy introduced bankruptcy reforms with ambiguous predictions on changes in bankruptcy penalties. Specifically, Brazil introduced a reform in 2005 that allowed for a reorganization procedure similar to US Chapter 11 and thus may have reduced penalties, but also gave creditors a higher ranking on proceedings in case of firm liquidation and thus may have increased penalties by motivating creditors to press for liquidation. Similarly, Italy introduced a reform in 2005 that removed strict restrictions to enter reorganization and thus may have reduced penalties, but subsequently introduced a reform in 2006 that made several changes to speed up the previously lengthy process of firm liquidation and thus may have increased penalties (again, higher expected recovery rates may increase creditors' incentive to press for liquidation). In unreported results, I also include dual-class firms from Brazil and Italy to the analysis. The results remain unaffected. For the Brazilian reform, see Ponticelli and Alencar (2016) and Favara et al. (2017). For the Italian reforms, see Rodano, Serrano-Velarde, and Tarantino (2016) and Favara et al. (2017).

⁵¹ For comparability, for all countries, including Korea, here size is measured by the log of assets in USD, instead of the log of assets in the respective national currency as done for the single country analysis in Korea. Summary statistics for the other countries are given in Table 2.5. As a supplement, for Korea, the average log of assets in USD is 14.05.

⁵² Note that some countries, such as Japan, China, and Singapore, banned dual-class firms during my sample period. Other countries, such as Indonesia, Malaysia, Philippines, Taiwan, and Thailand, are not included by conditions (3) and (4) of Section 2.4.1 (on country-specific rules of dual-class firms, see Nenova, 2003; Doidge, 2004). Finally, some countries, like India, are not included due to the data restrictions imposed in Section 2.4.1.

⁵³ In unreported results, I repeat the analysis in columns 3 to 6 of Table 2.4 using the log of the voting premium (and the log of the premium plus one) as the dependent variable to measure percentage changes in the premium. The results remain (qualitatively) unaffected.

comparing safe firms from Korea to all firms from other countries. As can be seen, the voting premium of risky Korean firms increased after the reform relative to firms from other countries. In contrast, the voting premium of safe Korean firms did not change after the reform relative to firms from other countries. Because Korean firms have on average a higher voting premium than firms from other countries (see Tables 2.1 and 2.5), it might be the case that the voting premium of Korean firms have changed relative to firms from other countries in terms of a percentage points change, but not in terms of a percentage change. To address this, in columns 9 and 10, I repeat the analysis using the log of the voting premium as the dependent variable to measure percentage changes in the premium. As shown, also in terms of percentage changes the voting premium of Korean firms increased relative to firms from other countries, and especially so for risky Korean firms.⁵⁴

Taken together, the results suggest that the findings in Korea are not driven by confounding factors common to all countries. Ultimately, the results boil down the identification problem to one in which a confounding factor not only needed to affect high and low default probability firms differently, but also needed to be a country-specific phenomenon in Korea.

2.6.3 Cross-country analysis and different measures of agency problems

Finally, I study whether the negative relation between agency problems and bankruptcy penalties also exists across countries, using the existing country estimates of three studies that apply different methods to measure the value of control. The goal of this analysis is to further support that the results in Korea are actually caused by the bankruptcy reform, and not by a confounding shock. Because two of the measures are not based on the voting premium in dual-class firms (see below), an important byproduct of the analysis is that it also mitigates concerns of potential measurement errors of the voting premium as a measure of agency problems and moreover allows for improved inference on the relevance of my findings for single-class firms.

The first estimate is by Morellec, Nikolov, and Schürhoff (2018), who measure the value of control by first using a dynamic model that identifies an optimal target leverage in the absence of agency conflicts, and then backing out agency conflicts from the difference between optimal target leverage and observed leverage.⁵⁵ They study 12,652 firms across 14 countries

⁵⁴ In unreported results, I repeat the analysis using the log of the voting premium plus one as the dependent variable to also consider observations with negative value. The corresponding did estimates for columns 9 and 10 respectively are 0.262 (*t*-statistic of 4.30) and 0.056 (*t*-statistic of 1.15). The number of firms and observations are as in columns 7 and 8.

⁵⁵ In Morellec, Nikolov, and Schürhoff's model, the controlling shareholder derives utility from its equity stake in the firm and can additionally capture parts of free cash flow as private benefits. Debt is beneficial for the controlling shareholder because it increases company value (modelled by the deductibility of interest payments) and thus the controlling shareholder's utility through the equity stake. Yet, debt also constraints the controlling

from 1997 to 2011, and find that on average private benefits of control represent 4.4% of free cash flows. The second estimate is by Nenova (2003), who, as in my study, measures private benefits of control by the voting premium in dual-class firms. Based on a sample of 661 dual-class firms across 18 countries in 1997, she finds that on average the value of control is 12.7% of the company value. The final estimate comes from Dyck and Zingales (2004), who measure control benefits using the difference between the price per share an acquirer pays for a controlling block and the market price of a share after the control change is announced. Based on a sample of 393 controlling block sales across 39 countries from 1990 to 2000, they find that on average corporate control is worth 14 percent of the firm value. Notably, all three studies conclude that control benefits vary widely across countries, suggesting an important role for country-specific factors (such as bankruptcy penalties) in shaping private benefits of control.

I measure bankruptcy penalties by following Canipek (2023b) and using the creditor rights index of La Porta et al. (1998), which contains penalties by covering data regarding management dismissal during the bankruptcy procedure, and regarding the likelihood of a firm liquidation as the outcome of the bankruptcy procedure (e.g., determined by provisions that cover the ability of creditors to seize assets, and the inability of firms to seek reorganization without creditors' consent). As for the country-analysis above, I control for economic development. Additionally, I control for other variables that might affect agency problems or the validity of the voting premium as a measure of private benefits of control (Nenova, 2003; Dyck and Zingales, 2004; Fauver et al., 2017; Morellec, Nikolov, and Schürhoff, 2018). These variables include shareholder rights, rule of law, average board independence, and average ownership structure. Because the analysis uses country averages of the control benefits and of the explanatory variables, I only consider countries for which the dependent as well as independent variables are calculated by at least five observations to obtain meaningful results (La Porta et al., 1998). Table 2.6 provides the list of countries, the values for each measure of private benefits

shareholder by reducing free cash flow available for cash diversion. As a result, the controlling shareholder faces a debt trade-off, and as noted above, private benefits are defined as the difference between optimal target leverage (without agency costs, i.e., without cash diversion) and the observed choice of debt. Accordingly, private benefits can be reduced by regulations that mitigate the possibility of cash diversion (such as stronger shareholder rights or better law enforcement) or institutions that increase the positive impact of debt on company value. In my analysis, I take a financial contracting theory perspective (Grossman and Hart, 1982; Aghion, Hart, and Moore, 1992; Dewatripont and Tirole, 1994; Hart, 1995, 2000, 2001, 2017; Zwiebel, 1996; Benmelech and Bergman, 2008; Canipek, 2023b). In particular, I interpret higher observed debt levels as a commitment that allows the controlling shareholder to increase firm value (or to secure financing ex ante). Importantly, the commitment role of debt is only valid if bankruptcy is penalized (Grossman and Hart, 1982; Aghion, Hart, and Moore, 1992; Hart, 1995, 2000). Hence, I expect that stronger creditor rights increase the positive impact of debt on company value and thus motivate the controlling shareholder for higher debt levels, which in turn reduces the difference between optimal and observed leverage (or, in other words, private benefits of control).

of control, and the values for the control variables. The variable definitions are again in Table A.2.1 in the Appendix.

Table 2.7 reports the results. For each of the three measures, I focus only on creditor rights as an explanatory variable in the first specification. In the second specification, I add the control variables. In columns 1 and 2, I start with the measure of Morellec, Nikolov, and Schürhoff. As expected, firms in countries with stronger creditor rights have lower agency problems. The effect is not only statistically but also economically significant. In column 2, going from the softest to the toughest bankruptcy regime is associated with a reduction of private control benefits by $4 \times 0.009 = 3.6$ percentage points.⁵⁶ Notably, column 1 suggests that creditor rights can explain about 55% of the cross-country variation in agency conflicts. Figure 2.4 shows graphically how dominant creditor rights seem to shape the value of control.

Next, columns 3 and 4 report the results for Nenova's (2003) measures that are based on the voting premium. As can be seen, stronger creditor rights are associated with lower agency problems (although statistically insignificant in the first specification). The magnitude of the effect is similarly important to that from my analysis for Korea. In column 4, a change in the creditor rights index from the lowest to the highest value is related to a reduction of the value of control by 23 percentage points. Column 3 shows that creditor rights can explain about 8% of the variation in private benefits of control across countries.

Finally, columns 5 and 6 present the estimates for the Dyck and Zingales (2004) measures. While the negative relation between private control benefits and creditor rights is statistically significant in the first specification, the introduction of control variables leads to a loss of the statistical significance. However, all coefficients have the expected sign. Moreover, it is important to note that debt is a governance device especially for firms with high default probability (Zwiebel, 1996; Hart, 2001). The use of country averages, therefore, undermines the importance of bankruptcy penalties as a device for curbing agency problems by also including firms with low probability of default.

In sum, the findings here confirm the results from Korea. Additionally, the results mitigate concerns that the voting premium as a measure for private benefits of control may be subject to measurement errors and moreover suggest that the findings of this paper may also be important for single-class firms.⁵⁷

⁵⁶ The description of the economic effect based on a change from highest to lowest creditor rights follows the cross-country studies by Brockman and Unlu (2009) and Acharya, Amihud, and Litov (2011).

⁵⁷ It is important to note that the analysis here is only intended to offer a simple additional robustness test, and is not meant to present a comprehensive cross-country analysis of the relation between agency problems and bankruptcy penalties. Econometrically, the drawback of the analysis is that the relation between agency problems and penalties may be endogenous, given that firms with low default probability do not serve as a control group

2.7 Conclusions

Dual-class firms have gained increasing importance in recent years. Dual-class share structure can be costly by generating potentially large agency problems between controlling shareholders and minority shareholders. Yet, dual-class share structure can also be beneficial by allowing insiders flexibility regarding less-informed or short-termed outside shareholders. Under academics and regulators, the central question in this context is how to allow dual-class firms, while at the same time finding ways to effectively limit expropriation of minority shareholders by controlling shareholders (Bebchuk and Kastiel, 2017; Kim and Michaely, 2019; Aggarwal et al., 2021; Lel et al., 2021; Cremers, Lauterbach, and Pajuste, 2022). In this paper, I study whether debt can reduce agency problems between controlling shareholders and minority shareholders in dual-class firms. To do so, I use two key features of debt as a governance device. First, it is only effective with bankruptcy penalties, second, it only matters for firms close to default (Grossman and Hart, 1982; Aghion, Hart, and Moore, 1992; Hart, 1995, 2000, 2001; Zwiebel, 1996; Canipek, 2023b). I measure agency problems by the voting premium. Using a Korean bankruptcy reform that reduced bankruptcy penalties, I find that firms with high default probability exhibited an increase in the voting premium by 59% after the reform relative to firms with low default probability. This result suggests that debt is valued as a powerful mechanism for reducing agency problems.

My findings have several important implications. First, my findings underline the role of agency problems in shaping firm's capital structure. Theoretically, the view that capital structure can be used to reduce agency costs is largely established (e.g., Grossman and Hart, 1982; Zwiebel, 1996). However, empirical evidence is limited. Second, my findings have key implications for the design of optimal bankruptcy law. In particular, my findings suggest that bankruptcy law should retain the governance role of debt by sufficiently penalizing management and controlling shareholders in default, as otherwise companies may have difficulties to commit to forgo inefficient actions and thus to obtain financing (Aghion, Hart, and Moore, 1992; Hart, 2000). Notably, while this aspect is important in general, it may be particularly relevant at

(however, note that, across countries, bankruptcy law is typically determined by a different reason—such as the 2007–2009 financial crisis for Germany, a corporate scandal for Italy, the 1997–1998 Asian financial crisis for Korea, an attempt to promote lending for Brazil, or the political power of competing groups for the US (Canipek, 2023b)—, which mitigates the concern that an omitted country variable simultaneously determines bankruptcy law and agency problems for most countries). Additionally, in unreported results, I find that the statistical significance of the negative relation for the Nenova measure dissipates when also controlling for legal origin as a determinant of bankruptcy law (La Porta et al., 1998) (note that an obvious explanation for this is that the combination of five legal origins with only 17 observations may make the analysis meaningless). Finally, the observed findings for the Morellec, Nikolov, and Schürhoff measure may not be because firms increase debt as a result of an increase in the effectivity of debt governance (see footnote 55), but because firms increase debt as a result of changes in credit costs and distress costs (see Section 2.3.4). I look forward to any comments to my analysis.

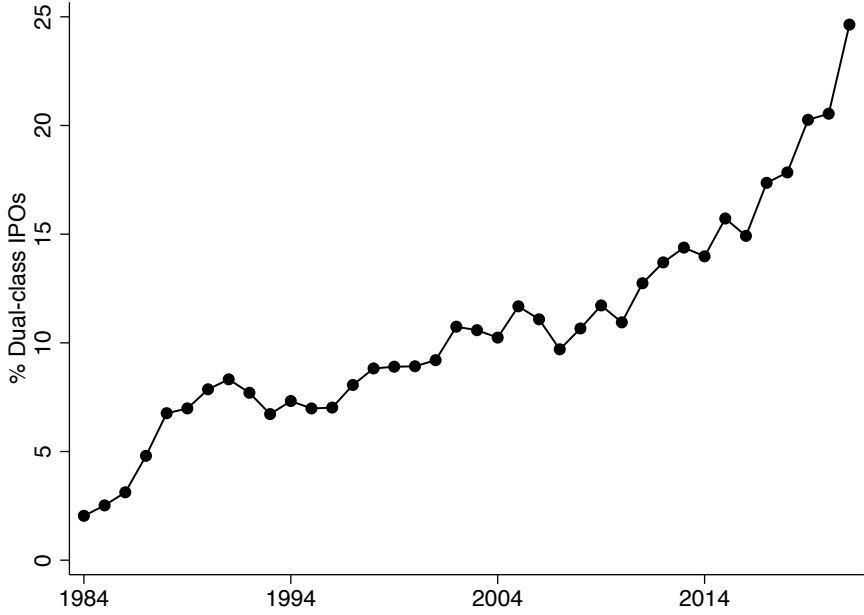
present, given that many countries have significantly softened their bankruptcy law as a response to the recent crises.⁵⁸

Finally, and most important, my findings have important implications for the current debate in academia and politics on dual-class firms. My results suggest that debt governance, or, more generally, mechanisms that transfer control when performance is poor, can offer a solution to effectively mitigate the agency problems associated with dual-class firms. For policymakers, my findings imply that providing provisions that enable such mechanisms may be desirable. This can be done by subjecting dual-class firms to a bankruptcy law that removes control from the controlling shareholder in default. An alternative way, that may also be applicable for safe firms, is to include “control change provisions” that allow controlling shareholders to stay in control as long as performance is good but shift control when performance is bad (as in financial contracting and control change theories, see Aghion and Bolton, 1992; Hart, 2001, 2017; Kaplan and Strömberg, 2003). In the context of dual-class shares, the design of the provision should be simple. The control shift could be the change of the dual-class share structure into a single-class share structure (as in sunset provisions, see Bebchuk and Kastiel, 2017). The performance threshold could be the performance of comparable single-class firms.

⁵⁸ On the implications of the finding that debt can mitigate agency problems regarding corporate capital structure and optimal bankruptcy law design, see Canipek (2023b).

Figures and tables for Chapter 2

Panel A. Share of dual-class IPOs (five-year moving average)



Panel B. Share of dual-class IPO market capitalization

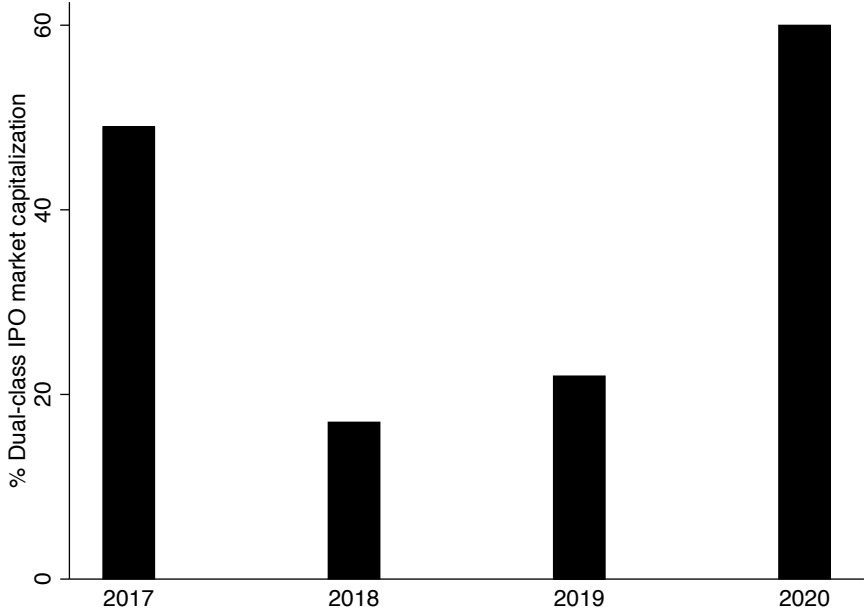
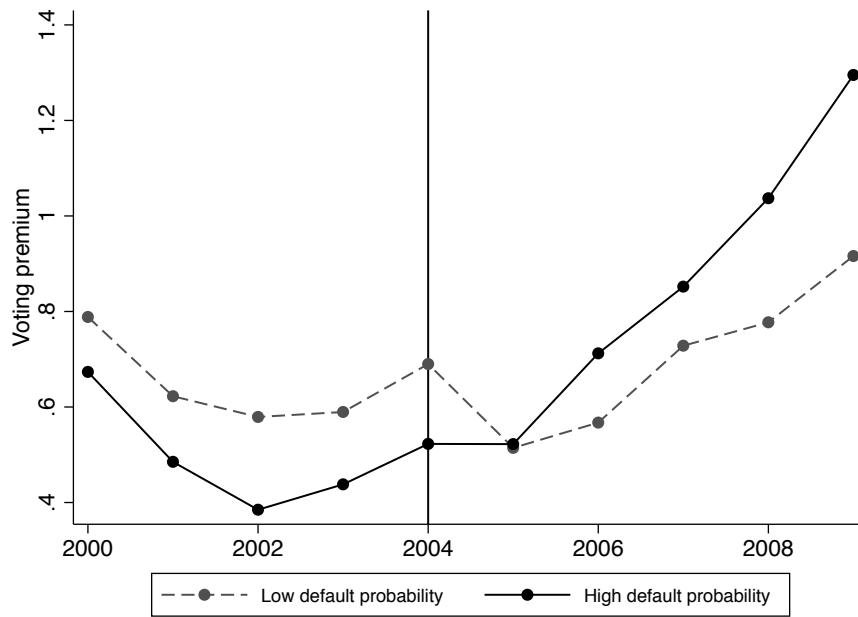


Figure 2.1
Dual-class IPOs in the US

Panel A shows the five-year moving average of the share of dual-class IPOs in all IPOs in the US. Panel B shows the share of dual-class IPO market capitalization in total IPO market capitalization in the US. The data on the share of dual-class IPOs come from Jay Ritter’s website (<https://site.warrington.ufl.edu/ritter/ipo-data/>). The data on the share of dual-class IPO market capitalization are from the CII website (<https://www.cii.org/dualclass stock>).

Panel A. Voting premium



Panel B. did estimate relative to 2004

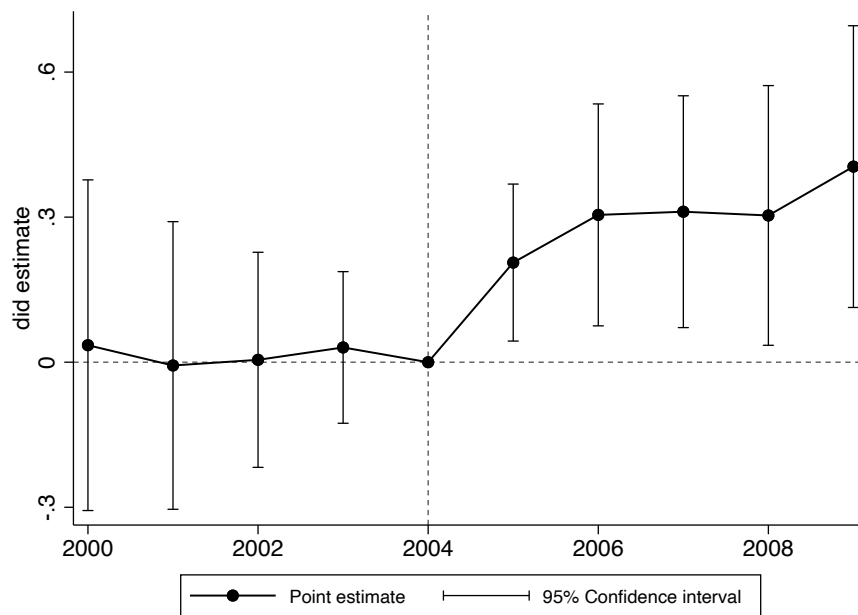
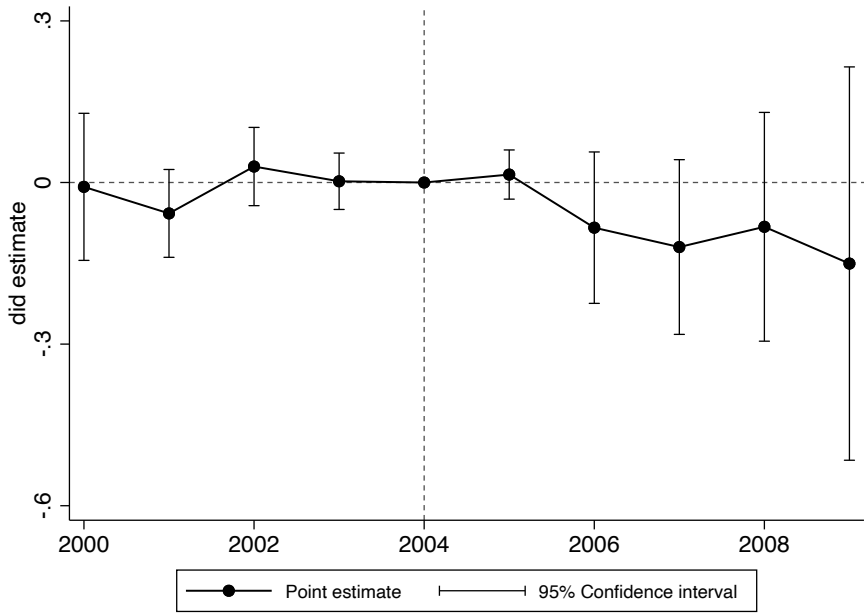


Figure 2.2
Bankruptcy reform in Korea and the voting premium

Panel A plots the average voting premium for high and low default probability firms. In Panel B, the black dots show the point estimates on the interaction terms between a set of time dummies and a dummy indicating firms with high default probability. The point estimates are related to the year prior to the reform, 2004. The grey bars present 95% confidence intervals for the point estimates. The dependent variable is the voting premium. The regression is specified as in column 2 of Table 2.2, i.e., with firm fixed effects, year fixed effects, controls, and standard errors clustered on the firm level. Detailed point estimates and *t*-values can be found in Table A.2.2 in the Appendix.

Panel A. did estimate relative to 2004 for the 2.40 threshold



Panel B. did estimate relative to 2004 for the 1.62 threshold

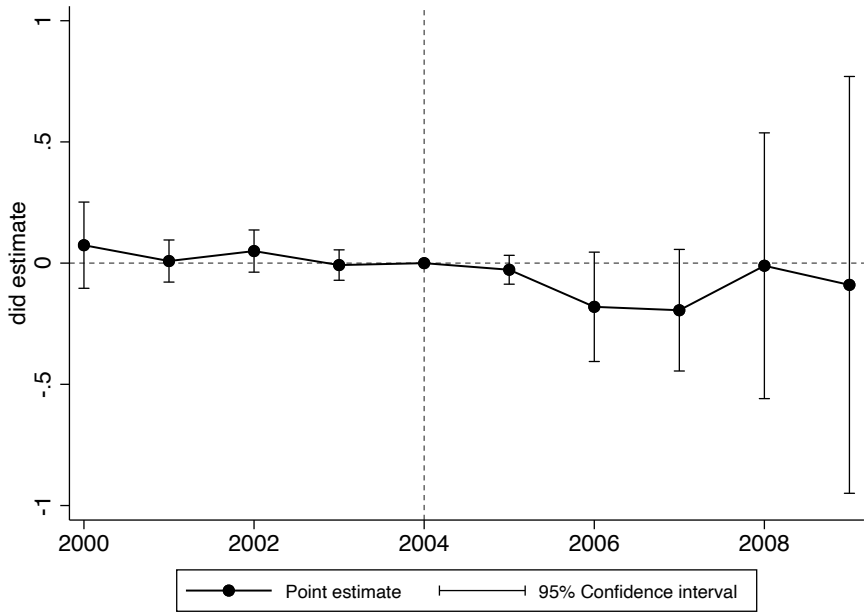


Figure 2.3
Bankruptcy reform in Korea and the voting premium of firms of other countries

In Panels A (2.40 threshold) and B (1.62 threshold), the black dots show the point estimates on the interaction terms between a set of time dummies and a dummy indicating firms with high default probability. The point estimates are related to the year prior to the Korean bankruptcy reform, 2004. The grey bars present 95% confidence intervals for the point estimates. The dependent variable is the voting premium. The regression is specified as in column 2 of Table 2.2, i.e., with firm fixed effects, year fixed effects, controls, and standard errors clustered on the firm level. Detailed point estimates and *t*-values can be found in Table A.2.2 in the Appendix.

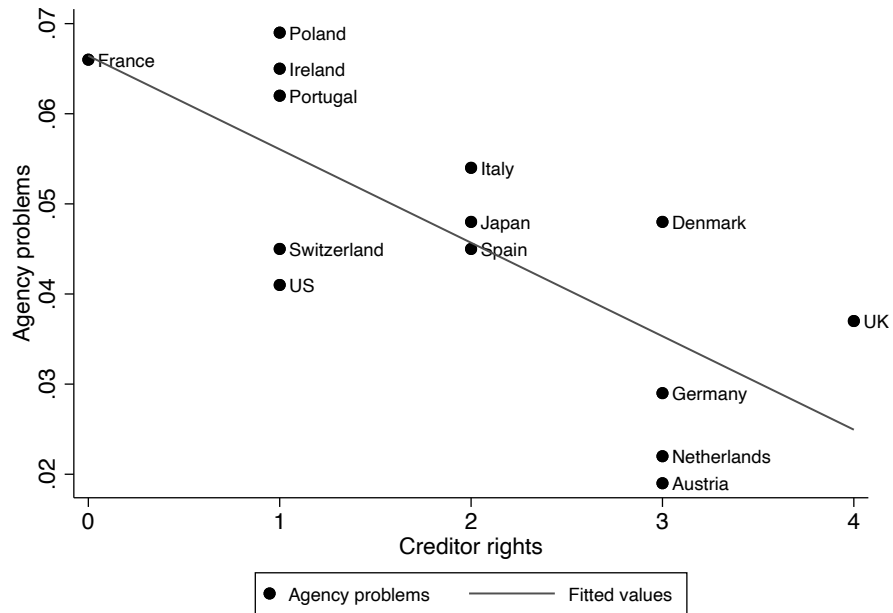


Figure 2.4

Agency problems and creditor rights across countries

This figure plots the relation between agency problems and creditor rights across countries. The black dots show the country estimates of agency problems from Morellec et al. (2018). The fitted line shows the slope of a regression of the control benefits on creditor rights (as shown in Table 2.7, the slope coefficient is -0.010 with a *t*-value of -3.72).

Table 2.1

Summary statistics

This table presents summary statistics for firms with high default probability (columns 1–5) and firms with low default probability (columns 6–10). The columns show: columns 1 and 6, average for all years (2000 to 2009); columns 2 and 7, standard deviation for all years; columns 3 and 8, within-firm standard deviation for all years; columns 4 and 9, average for the years before the reform (2000 to 2004); and columns 5 and 10, average for the years after the reform (2005 to 2009). Columns 11, 12, and 13 present p -values of t -tests of difference in means between high and low default probability firms for all years, for the years before the reform, and for the years after the reform, respectively. Refer to Table A.2.1 in the Appendix for variable definitions.

	High default probability					Low default probability					p -all	p -before	p -after
	All years			Before	After	All years			Before	After			
	Mean	SD	Within-SD	Mean	Mean	Mean	SD	Within-SD	Mean	Mean			
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	
Voting premium	0.688	0.982	0.420	0.510	0.871	0.677	0.798	0.358	0.647	0.704	0.87	0.15	0.10
Ownership	0.318	0.192	0.116	0.301	0.335	0.378	0.197	0.117	0.367	0.388	0.00	0.00	0.02
Size	21.18	1.817	0.346	21.07	21.30	20.97	1.787	0.309	20.74	21.17	0.13	0.09	0.55
Turnover	-0.510	1.157	0.839	-0.194	-0.834	-0.229	1.347	0.792	0.114	-0.548	0.00	0.01	0.05
Dividend dummy	0.490	0.501	0.243	0.401	0.581	0.716	0.452	0.308	0.647	0.780	0.00	0.00	0.00
Voting power	1.080	0.086	0.015	1.085	1.075	1.073	0.077	0.008	1.074	1.072	0.25	0.23	0.71
Leverage	0.379	0.173	0.104	0.395	0.362	0.231	0.168	0.081	0.255	0.209	0.00	0.00	0.00
Profitability	0.036	0.075	0.056	0.032	0.040	0.081	0.068	0.053	0.093	0.070	0.00	0.00	0.00
Tobin's Q	0.981	0.274	0.157	0.898	1.065	1.029	0.376	0.209	0.978	1.077	0.06	0.01	0.77
Altman's Z-score	1.138	0.886	0.551	0.967	1.322	2.660	1.317	0.819	2.391	2.912	0.00	0.00	0.00
Age	42.42	9.803	2.884	40.19	44.78	42.21	17.28	2.907	40.05	44.23	0.85	0.93	0.73

Table 2.2**Bankruptcy reform in Korea and changes in the voting premium**

This table presents estimates of difference-in-differences regressions for the bankruptcy reform in Korea. The dependent variable is the voting premium in columns 1–7 and 9–10, and the logarithm of the voting premium in column 8. Post is a dummy variable that equals one for the years after the bankruptcy reform was passed (from 2005), and zero for the years before the reform was passed (before 2005). HighRisk is a dummy variable that equals one if the firm has a high probability of default (above the median of the bankruptcy probability distribution), and zero if it has a low probability of default (below the median). Columns 1 and 2 present the results for the main analysis. Column 3 shows the results for an extended time period from 2000 to 2012, and column 4 reports the results when maintaining the extended period but excluding the years right after the reform, 2005–2007. Columns 6 and 7 respectively show the estimates when excluding penny stocks and when considering a balanced panel. Column 9 reports the estimates for an extended sample that also includes firms without ownership data. Column 10 shows the results when dividing firms into high and low default probability firms based on their default probability in the year before the reform (2004). *t*-values are reported in parentheses and are based on standard errors clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.2.1 in the Appendix for variable definitions.

	Main results		Extended period		Robustness					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post x HighRisk	0.395*** (3.67)	0.413*** (3.94)	0.408*** (3.42)	0.434*** (2.69)	0.383*** (3.74)	0.436*** (3.65)	0.488*** (4.00)	0.546*** (3.39)	0.229** (2.55)	0.411*** (3.85)
Post	0.181 (1.61)	0.162 (1.34)	0.272 (1.64)	0.244 (1.40)	0.098 (0.76)	0.169 (1.23)	0.175 (1.20)	0.136 (0.70)	0.077 (0.80)	0.169 (1.38)
Ownership		-0.267* (-1.75)	-0.187 (-0.97)	-0.213 (-0.87)	-0.152 (-1.00)	-0.356** (-2.07)	-0.276 (-1.40)	-0.112 (-0.41)		-0.329** (-2.14)
Size		0.090 (1.00)	0.132 (1.26)	0.192* (1.79)	0.080 (0.87)	0.046 (0.47)	0.190* (2.00)	0.108 (1.04)	0.100 (1.16)	0.066 (0.76)
Turnover		-0.009 (-0.24)	-0.029 (-0.66)	-0.039 (-0.85)	-0.021 (-0.56)	0.001 (0.02)	-0.008 (-0.22)	0.007 (0.13)	-0.034 (-1.09)	-0.021 (-0.58)
Dividend dummy		0.024 (0.36)	-0.053 (-0.58)	-0.087 (-0.93)	0.039 (0.58)	0.053 (0.73)	-0.019 (-0.18)	0.014 (0.16)	0.039 (0.72)	0.040 (0.58)
Voting power		1.114 (0.85)	2.604 (1.47)	2.911* (1.67)	0.812 (0.61)	0.110 (0.09)	3.385* (1.82)	1.459 (1.11)	1.006 (1.38)	0.872 (0.67)
Leverage					0.154 (0.70)					
Profitability					0.065 (0.26)					
Tobin's Q					0.401*** (3.30)					
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	663	663	865	668	663	561	300	535	939	641
Number of firms	77	77	77	77	77	65	30	67	99	74
R ² (within)	0.240	0.257	0.222	0.252	0.295	0.249	0.359	0.193	0.166	0.262

Table 2.3**Bankruptcy reform in Korea and changes in the voting premium: heterogeneity**

This table presents estimates of difference-in-differences regressions for the bankruptcy reform in Korea. The dependent variable is the voting premium in all columns. Post is a dummy variable that equals one for the years after the bankruptcy reform was passed (from 2005), and zero for the years before the reform was passed (before 2005). Columns 1–6 report the results when comparing high default probability firms with different levels of ownership dispersion, age, and size to low default probability firms. Columns 7–9 report the results when comparing high default probability firms with high ownership dispersion/age/size to high default probability firms with low ownership dispersion/age/size. For columns 1–6, High is a dummy variable that equals one if the firm has a high probability of default (above the median of the bankruptcy probability distribution), and zero if it has a low probability of default (below the median). For columns 7–9, High is a dummy variable that equals one if the firm has high ownership dispersion/age/size (above the median ownership dispersion/age/size among high default probability firms), and zero if it has low ownership dispersion/age/size (below the median). *t*-values are reported in parentheses and are based on standard errors clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.2.1 in the Appendix for variable definitions.

	Ownership dispersion		Age		Size		Among risky firms		
	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)	Owner (7)	Age (8)	Size (9)
Post x High	0.524*** (3.82)	0.279** (2.43)	0.600*** (4.75)	0.203* (1.74)	0.530*** (3.92)	0.283** (2.61)	0.234* (1.70)	0.329** (2.29)	0.242* (1.71)
Post	0.149 (1.11)	0.100 (0.76)	0.192 (1.49)	0.076 (0.55)	0.294** (2.14)	-0.040 (-0.30)	0.509** (2.60)	0.458** (2.35)	0.512** (2.66)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	505	482	501	468	506	481	339	339	339
Number of firms	58	57	56	55	58	57	39	39	39
R ² (within)	0.269	0.146	0.307	0.130	0.293	0.149	0.397	0.412	0.399

Table 2.4**Bankruptcy reform in Korea and changes in the voting premium: confounding factors**

This table presents estimates of difference-in-differences regressions for the bankruptcy reform in Korea. The dependent variable is the voting premium in columns 1–8, and the logarithm of the voting premium in columns 9–10. Post is a dummy variable that equals one for the years after the bankruptcy reform was passed (from 2005), and zero for the years before the reform was passed (before 2005). Columns 1–2 are on Korean firms. Column 1 shows the results for the time period 2000 to 2007, and column 2 reports the results for the time period 2000 to 2012 but without the years 2008 to 2009. Columns 3–6 are on firms from other countries, and columns 7–10 are on Korean firms and firms from other countries. Columns 7 and 9 report the results when comparing risky Korean firms to firms from other countries, and columns 8 and 10 report the results when comparing safe Korean firms to firms from other countries. For columns 1–6, Exposed is a dummy variable that equals one if the firm has a high probability of default (above the median bankruptcy probability distribution of Korean firms for columns 1–2, above the median bankruptcy probability distribution of firms in other countries for columns 3–4, and below an Altman's Z-score of 1.62 for columns 5–6), and zero if it has a low probability of default (below the median bankruptcy probability distribution of Korean firms for columns 1–2, below the median bankruptcy probability distribution of firms in other countries for columns 3–4, and above an Altman's Z-score of 1.62 for columns 5–6). For columns 7–10, Exposed is a dummy variable that equals one for Korean firms, and zero for firms from other countries. *t*-values are reported in parentheses and are based on standard errors clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.2.1 in the Appendix for variable definitions.

	Voting premium in other countries									
	2007–2009 financial crisis		Other countries				Korea and other countries			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post x Exposed	0.333*** (3.22)	0.357*** (3.00)	-0.004 (-0.08)	-0.004 (-0.08)	0.005 (0.04)	-0.003 (-0.03)	0.364*** (4.25)	0.026 (0.40)	1.356*** (4.78)	0.780*** (2.80)
Post	-0.044 (-0.39)	0.280 (1.61)	-0.127 (-0.86)	-0.060 (-0.35)	-0.130 (-0.92)	-0.060 (-0.39)	-0.081 (-0.58)	-0.095 (-0.57)	-0.324 (-0.93)	-0.398 (-1.10)
GDP				-0.645 (-1.35)		-0.648 (-1.49)	0.750 (1.38)	0.517 (0.93)	-1.155 (-0.64)	-0.897 (-0.51)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	525	727	547	547	547	547	886	871	637	640
No. of firms	77	77	61	61	61	61	100	99	84	85
R ² (within)	0.145	0.192	0.076	0.079	0.076	0.079	0.187	0.046	0.143	0.105

Table 2.5

Summary statistics for firms in other countries

This table presents summary statistics for firms in other countries. Panel A reports summary statistics by country. Panel B reports summary statistics for the 2.40 threshold (columns 1–6) and 1.62 threshold (columns 8–13), separately for firms with high default probability (columns 1–3 and 8–10) and firms with low default probability (columns 4–6 and 11–13). In Panel B, the columns show: columns 1, 4, 8, and 11, average for all years; columns 2, 5, 9, and 12, standard deviation for all years; and columns 3, 6, 10, and 13, within-firm standard deviation for all years. Columns 7 and 14 present *p*-values of *t*-tests of difference in means between high and low default probability firms for all years on the 2.40 and 1.62 thresholds, respectively. Refer to Table A.2.1 in the Appendix for variable definitions.

Panel A. Summary statistics by country												
	Number of firms	Observations	Voting premium	Ownership	Size	Turnover	Dividend dummy	Voting power	Z-score	GDP	Voting arrangements	
Austria	2	14	-0.513	0.627	13.59	0.432	0.643	1.482	1.684	10.71	Ordinary shares one vote, preferred shares nonvoting	
Chile	5	49	0.080	0.610	13.92	1.285	0.490	1.637	2.600	9.30	A shares elect 6/7 directors, B shares elect 1/7 directors	
Denmark	3	26	0.109	0.527	12.83	2.067	0.346	1.909	3.801	10.96	A shares ten votes, B shares one vote	
France	3	25	-0.121	0.543	15.39	-1.217	0.600	1.129	1.719	10.59	Ordinary shares one vote, preferred shares nonvoting	
Germany	24	216	0.198	0.468	14.40	1.253	0.736	1.704	2.422	10.58	Ordinary shares one vote, preferred shares nonvoting	
Norway	2	15	0.105	0.530	14.37	-0.602	0.133	1.421	1.615	11.37	A shares one vote, B shares nonvoting	
Sweden	16	153	0.042	0.291	15.10	3.157	0.131	7.267	2.851	10.80	A shares one vote, B shares 0.1 votes	
Switzerland	6	49	0.225	0.379	13.93	0.040	0.429	3.483	3.023	11.17	Ordinary shares one vote, preferred shares nonvoting	
Total/Mean	61	547	0.107	0.435	14.46	1.534	0.474	3.383	2.605	10.62	-	

Panel B. Summary statistics for firms with high and low default probability															
	2.40 threshold							1.62 threshold							
	High default probability			Low default probability				<i>p</i>	High default probability			Low default probability			
	Mean	SD	Within-SD	Mean	SD	Within-SD	Mean		SD	Within-SD	Mean	SD	Within-SD	<i>p</i>	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)		
Voting premium	0.179	0.879	0.189	0.036	0.219	0.089	0.01	0.119	0.804	0.314	0.103	0.587	0.088	0.81	
Ownership	0.477	0.233	0.127	0.395	0.212	0.095	0.00	0.493	0.256	0.125	0.419	0.214	0.106	0.00	
Size	14.62	2.069	0.276	14.30	1.839	0.294	0.06	14.22	2.362	0.268	14.53	1.826	0.290	0.12	
Turnover	1.229	1.911	0.548	1.833	2.139	0.633	0.00	1.199	1.543	0.610	1.631	2.167	0.586	0.04	
Dividend dummy	0.487	0.501	0.124	0.460	0.499	0.174	0.53	0.642	0.481	0.145	0.425	0.495	0.151	0.00	
Voting power	2.369	2.580	0.259	4.379	6.495	0.458	0.00	2.522	3.565	0.440	3.633	5.389	0.336	0.03	
Altman's Z-score	1.708	0.643	0.484	3.482	1.276	0.959	0.00	1.432	0.669	0.556	2.948	1.302	0.772	0.00	

Table 2.6

Summary statistics for the cross-country analysis

This table presents summary statistics by country for the relation between agency problems and bankruptcy penalties across countries. Refer to Table A.2.1 in the Appendix for variable definitions.

	Morellec et al. (2018)	Nenova (2003)	Dyck & Zingales (2004)	Creditor rights	Board independence	Shareholder rights	Rule of law	Ownership	GDP
Argentina	.	.	0.27	1	0.470	4	5.35	0.53	8.91
Australia	.	.	0.02	3	0.592	4	10.00	0.28	10.75
Austria	0.019	.	.	3	0.487	2	10.00	0.58	10.67
Brazil	.	0.2319	0.65	1	0.378	3	6.32	0.57	9.10
Canada	.	0.0276	.	1	0.727	5	10.00	0.40	10.72
Chile	.	0.2314	0.18	2	0.266	5	7.02	0.45	9.22
Colombia	.	.	0.27	0	0.552	3	2.08	0.63	8.52
Denmark	0.048	0.0084	0.08	3	0.410	2	10.00	0.45	10.94
Finland	.	-0.0503	0.02	1	0.854	3	10.00	0.37	10.66
France	0.066	0.2805	.	0	0.345	3	8.98	0.34	10.57
Germany	0.029	0.0950	0.10	3	0.134	1	9.23	0.48	10.55
Hong Kong	.	-0.0288	0.00	4	0.394	5	8.22	0.54	10.09
Ireland	0.065	.	.	1	0.527	4	7.80	0.39	10.78
Israel	.	.	0.27	3	0.534	3	4.82	0.51	10.18
Italy	0.054	0.2936	0.37	2	0.448	1	8.33	0.58	10.51
Japan	0.048	.	-0.04	2	0.200	4	8.98	0.18	10.66
Malaysia	.	.	0.07	3	0.494	4	6.78	0.54	8.91
Mexico	.	0.3642	0.34	0	0.494	1	5.35	0.64	9.10
Netherlands	0.022	.	0.02	3	0.532	2	10.00	0.39	10.75
New Zealand	.	.	0.03	4	0.726	4	10.00	0.48	10.37
Norway	.	0.0583	0.01	2	0.427	4	10.00	0.36	11.34
Philippines	.	.	0.13	1	0.274	3	2.73	0.57	7.43
Poland	0.069	.	.	1	0.202	3	8.70	0.47	9.12
Portugal	0.062	.	.	1	0.235	3	8.68	0.52	9.98
South Africa	.	0.0672	.	3	0.528	5	4.42	0.52	8.74
South Korea	.	0.2894	0.16	3	0.300	2	5.35	0.23	9.75
Spain	0.045	.	0.04	2	0.369	4	7.80	0.51	10.31
Sweden	.	0.0104	0.07	1	0.537	3	10.00	0.28	10.76
Switzerland	0.045	0.0544	0.06	1	0.388	2	10.00	0.41	11.12
Thailand	.	.	0.12	2	0.443	2	6.25	0.47	8.29
Turkey	.	.	0.37	2	0.279	2	5.18	0.59	9.03
United Kingdom	0.037	0.0957	0.01	4	0.375	5	8.57	0.19	10.55
United States	0.041	0.0201	0.01	1	0.746	5	10.00	0.20	10.74

Table 2.7

Agency problems and bankruptcy penalties across countries

This table presents estimates of OLS regressions. The dependent variable is the estimate of agency problems from Morellec et al. (2018) in columns 1–2, the estimate of agency problems from Nenova (2003) in columns 3–4, and the estimate of agency problems from Dyck and Zingales (2004) in columns 5–6. *t*-values are reported in parentheses and are based on robust standard errors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Refer to Table A.2.1 in the Appendix for variable definitions.

	Morellec et al. (2018)		Nenova (2003)		Dyck and Zingales (2004)	
	(1)	(2)	(3)	(4)	(5)	(6)
Creditor rights	-0.010*** (-3.72)	-0.009*** (-5.01)	-0.029 (-1.20)	-0.058*** (-3.89)	-0.056** (-2.75)	-0.027 (-1.23)
Board independence (country average)		-0.006 (-0.33)		-0.319** (-2.77)		-0.051 (-0.39)
Shareholder rights		-0.002 (-0.50)		-0.009 (-0.69)		-0.021 (-1.23)
Rule of law		-0.008* (-1.95)		-0.067* (-2.21)		-0.027 (-1.56)
Ownership (country average)		-0.025 (-0.67)		-0.045 (-0.23)		0.417* (2.02)
GDP		-0.006 (-0.88)		0.072 (0.87)		0.027 (0.61)
Number of countries	14	14	17	17	26	26
R ²	0.549	0.773	0.079	0.778	0.166	0.529

Appendix for Chapter 2

Table A.2.1
Variable definitions

Variable name	Variable definition	Source
<i>Security characteristics</i>		
Voting premium	Ratio of the price of a voting right to the price of a cash flow right. Or $(P_H - P_L)/(P_L - rv * P_H)$, where P_H is the price of the high-voting share, P_L is the price of the low-voting share, and rv is the number of votes of a low-voting share relative to the number of votes of a high-voting share. As the other security characteristics (following below) that are based on weekly Datastream data, for each firm the voting premium is computed weekly and averaged annually for each year. The security characteristics are defined following Doidge (2004).	Datastream and Doidge (2004)
Turnover	Logarithm of the ratio of the total turnover of the low-voting share to the total turnover of the high-voting share.	Datastream
Dividend dummy	Dummy variable that equals one if the low-voting share receives a larger dividend than the high-voting share.	Datastream
Voting power	Ratio of total votes attached to the high-voting share class relative to the ratio of total cash flow rights attached to the high-voting share class. Or $(NS_H + NS_L)/(NS_H + rv * NS_L)$, where NS_H is the number of high-voting shares outstanding and NS_L is the number of low-voting shares outstanding.	Datastream
<i>Firm characteristics</i>		
Altman's Z-score	Sum of 1.2 times net working capital divided by total assets, 1.4 times retained earnings divided by total assets, 3.3 times EBIT divided by total assets, 0.6 times market value of equity divided by total liabilities, and 0.99 times sales divided by total assets. Firms with low Z-score have high default probability and firms with high Z-score have low default probability.	Worldscope
Ownership	Ratio of the number of closely held shares by insiders (shareholders who hold at least 5% of the outstanding shares, such as officers and directors and immediate families, other corporations, or individuals) to the number of shares outstanding.	Worldscope
Size	Logarithm of total assets in thousands of KRW. For the analysis of firms in other countries in Section 2.6.2, total assets are in thousands of USD.	Worldscope
Leverage	Ratio of total debt to total assets.	Worldscope
Profitability	Ratio of EBITDA to total assets.	Worldscope
Tobin's Q	Ratio of total assets plus market value of equity minus book value of equity to total assets.	Worldscope
Firm age	Number of years since the date of firm foundation, or, in case of missing data, number of years since the date of firm incorporation. Note that more data are available for firm foundation than for firm incorporation, which is why the date of firm foundation is used as the default option.	Worldscope
<i>Country characteristics</i>		
Morellec et al. (2018) estimate of agency problems	Average agency problems in a country from Morellec et al. (2018), who measure agency problems by first using a dynamic model that identifies an optimal target leverage in the absence of agency conflicts, and then backing out agency conflicts from the difference between optimal target leverage and observed choice of debt. The data are based on the period 1997–2011.	Morellec et al. (2018)
Nenova (2003) estimate of agency problems	Average agency problems in a country from Nenova (2003), who measures agency problems by the price difference of shares with different voting rights in dual-class firms. The data are from 1997.	Nenova (2003)
Dyck and Zingales (2004) estimate of agency problems	Average agency problems in a country from Dyck and Zingales (2004), who measure agency problems by the difference between the price per share an acquirer pays for a controlling block and the market price of a share after the control change is announced. The data are based on the period 1990–2000.	Dyck and Zingales (2004)
GDP	Logarithm of the real GDP per capita. The data are in constant 2010 USD.	World Bank
Creditor rights	Aggregate index of creditor rights, following La Porta et al. (1998). The index is formed by adding: (1) one if secured creditors are able to seize their collateral after the reorganization procedure is approved, i.e., if there is no automatic stay on the firm's assets, and zero if the law imposes automatic stay on the assets; (2) one if the reorganization procedure imposes restrictions, such as secured creditors' consent, for a debtor to file for reorganization, and zero if debtor can seek reorganization unilaterally; (3) one if secured creditors are paid first out of the proceeds of liquidating the firm, and zero if other claimants, such as the government or workers, are given absolute priority; and (4) one if an administrator is automatically appointed for running the firm during the reorganization procedure, i.e., management is dismissed in the reorganization procedure, and zero if management retains corporate control during reorganization. The index ranges from zero to four and higher values indicate stricter penalties in bankruptcy. The data are from 2003 and come from Djankov, McLiesh, and Shleifer (2007), who updated and enlarged the data of La Porta et al. (1998).	Djankov, McLiesh, and Shleifer (2007)
Board independence (country average)	Average of the ratio of the number of independent board members to the number of board members in a country based on the period 2002–2016.	Canipek (2023b)
Shareholder rights	Aggregate index of shareholder rights, created by La Porta et al. (1998). The index is formed by adding one if: (1) shareholders are allowed to mail their proxy vote to the firm, (2) shareholders are not required to deposit their shares prior to the	La Porta et al. (1998) and Pistor, Raiser, and Gelfer

	<p>general shareholders' meeting, (3) cumulative voting or proportional representation of minorities (shareholders owning 10% or less of the capital) in the board of directors is allowed, (4) an oppressed minorities mechanism is in place, (5) the minimum percentage of share capital that entitles a single shareholder to call for an extraordinary shareholders' meeting is less than or equal to 10%, and (6) shareholders have preemptive rights. The index ranges from zero to six and higher values indicate better legal protection of minority shareholders. The data are from 1993. The data for Poland are from Pistor, Raiser, and Gelfer (2000).</p>	(2000)
Rule of law	<p>"Assessment of the law and order tradition in the country produced by the country risk rating agency International Country Risk (ICR). Average of the months of April and October of the monthly index between 1982 and 1995." The index ranges from zero to ten and higher scores indicate more tradition for law and order. The data are from La Porta et al. (1998). The data for Poland are from Pistor, Raiser, and Gelfer (2000).</p>	<p>Quotation from La Porta et al. (1998, p. 1124). Data from La Porta et al. (1998) and Pistor, Raiser, and Gelfer (2000)</p>
Ownership (country average)	<p>Country average of the percentage of common shares owned by the three largest shareholders in the ten largest, nonfinancial, privately owned, publicly traded, firms in a country. The data are from 1993 and come from La Porta et al. (1998). The data for Poland are based on the ownership data from Worldscope (defined above) and are based on the period 2002–2016.</p>	<p>La Porta et al. (1998) and Worldscope</p>

Table A.2.2Detailed point estimates and *t*-values for figures

This table reports the point estimates and *t*-values regarding Figures 2.2 and 2.3. *t*-values are reported in parentheses and are based on standard errors clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Figure 2.2 (Panel B) (1)	Figure 2.3 (Panel A) (2)	Figure 2.3 (Panel B) (3)
2000 x HighRisk	0.035 (0.20)	-0.008 (-0.12)	0.074 (0.83)
Number of observations	131	111	111
Number of firms	77	61	61
R ² (within)	0.278	0.367	0.377
2001 x HighRisk	-0.007 (-0.05)	-0.057 (-1.41)	0.009 (0.20)
Number of observations	141	116	116
Number of firms	77	61	61
R ² (within)	0.077	0.224	0.197
2002 x HighRisk	0.005 (0.04)	0.030 (0.82)	0.050 (1.14)
Number of observations	140	119	119
Number of firms	77	61	61
R ² (within)	0.208	0.139	0.148
2003 x HighRisk	0.031 (0.39)	0.002 (0.09)	-0.008 (-0.25)
Number of observations	144	119	119
Number of firms	77	61	61
R ² (within)	0.211	0.147	0.148
2005 x HighRisk	0.206* (2.53)	0.015 (0.65)	-0.027 (-0.92)
Number of observations	147	121	121
Number of firms	77	61	61
R ² (within)	0.193	0.168	0.173
2006 x HighRisk	0.305*** (2.64)	-0.084 (-1.19)	-0.180 (-1.60)
Number of observations	145	114	114
Number of firms	77	61	61
R ² (within)	0.296	0.191	0.222
2007 x HighRisk	0.311** (2.59)	-0.120 (-1.48)	-0.194 (-1.55)
Number of observations	133	113	113
Number of firms	77	61	61
R ² (within)	0.339	0.271	0.282
2008 x HighRisk	0.304** (2.25)	-0.082 (-0.77)	-0.011 (-0.04)
Number of observations	145	114	114
Number of firms	77	61	61
R ² (within)	0.349	0.176	0.172
2009 x HighRisk	0.405*** (2.77)	-0.151 (-0.65)	-0.090 (-0.19)
Number of observations	145	108	108
Number of firms	77	61	61
R ² (within)	0.435	0.231	0.225
Firm controls	Yes	Yes	Yes
Country controls	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Chapter 3

Creditor Rights, Corporate Leverage and Investments, and the Firm Type*

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3.1 Introduction

What is the effect of creditor rights on corporate leverage and investments? While there is substantial debate on this question, the answer is unclear. Stronger creditor rights can reduce credit costs of secured debt and thus may allow firms to increase leverage and investments. Yet, they also increase distress costs and thus may prompt firms to reduce the risk of default by lowering leverage and undertaking risk-reducing but unprofitable investments. The existing empirical evidence is mixed, which is puzzling (and unsatisfying to guide policymakers) given that the answer, i.e., whether the effect of creditor rights on credit costs or that on distress costs dominates, should ultimately be an empirical issue.¹

In this paper we have two goals. First, we want to provide additional empirical evidence to the ongoing debate on the effect of creditor rights on corporate financial and investment policy. And, second, we try to understand the reason for the puzzling mixed empirical evidence in the bankruptcy literature. Specifically, we will argue that the effect of creditor rights depends on the firm type, as it influences the effect that creditor rights have on credit costs and distress costs and thus which effect dominates. As described below, our understanding has important implications for optimal bankruptcy design.

To address these goals, we use Germany's bankruptcy law reform of 2012 that, prompted by the 2007–2009 financial crisis, was designed to facilitate the restructuring of distressed firms. In particular, the reform reduced creditor rights by introducing a debtor-in-possession proceeding of about six months (which can be entered unilaterally, i.e., without creditors' consent) that allows management to stay in corporate control during the bankruptcy procedure and moreover imposes automatic stay on the assets. The new law, therefore, reduced secured creditors' power to recover their collateralized claims, but also reduced distress costs for shareholders and management.

We measure the effect of the bankruptcy law reform by a difference-in-differences analysis. Specifically, we use variation in the effect of the reform in asset tangibility and probability of default. In our main analysis, we follow Vig (2013) and compare changes in leverage and investments of firms with high asset tangibility around the reform to those of firms with low asset tangibility, exploiting that firms with high asset tangibility should be more affected by a

¹ For the debate on the effect of creditor rights on corporate leverage and investments, see Acharya, Amihud, and Litov (2011), Vig (2013), Ponticelli and Alencar (2016), Mann (2018), and Schoenherr and Starmans (2021). For empirical studies that find a positive effect of creditor rights on corporate financial and investment policy, see, e.g., Haselmann, Pistor, and Vig (2010), Campello and Larrain (2016), Ponticelli and Alencar (2016), Rodano, Serrano-Velarde, and Tarantino (2016), Calomiris et al. (2017), and Aretz, Campello, and Marchica (2020); and for studies that find a negative effect, see, e.g., Acharya, Amihud, and Litov (2011), Vig (2013), Cho et al. (2014), and Schoenherr and Starmans (2021).

change in creditor rights since higher levels of collateralizable assets should make the trade-off between the effect that creditor rights have on credit costs and that on distress costs more important (i.e., whether to increase secured debt and investments as a result of lower credit costs, or to reduce the risk of default as a result of higher distress costs related with existing secured debt).² To mitigate the concern that our conclusions are driven by confounding shocks that coincided with the introduction of the bankruptcy law reform and affected firms with high asset tangibility differently than firms with low asset tangibility, we redo our empirical analysis, but by comparing changes of firms with high default probability, which should be more affected by the design of the bankruptcy law, to those of firms with low default probability.³ The idea is that if we obtain results consistent with those of our main analysis, our conclusions should not be affected by such confounding shocks. Put differently, for confounding events to affect our understandings, they must not only have had a different effect on high and low asset tangibility firms, but also on high and low default probability firms.

To begin, we examine the average effect on corporate financial and investment policy. We find that firms with high asset tangibility increased corporate leverage after the reform relative to firms with low asset tangibility (using both secured debt and unsecured debt). Moreover, after the reform firms with high asset tangibility relatively discarded investments that can reduce the risk of default but are typically unprofitable—such as the accumulation of assets with high recovery value that allows firms to avoid default in situations of distress by liquidating these assets, or diversifying activities. Finally, after the reform firms with high asset tangibility exhibited relatively higher levels of risk and profitability, which provides support on the mechanisms underlying the findings regarding investments.⁴ Taken together, our results provide evidence consistent with the notion that stronger creditor rights impose a cost on firms by increasing distress costs and thus prompting firms to reduce the risk of default by undertaking risk-reducing but unprofitable financial and investment decisions. In other words, our findings suggest that, on average, the effect of creditor rights on distress costs outweighs that on credit costs.

² In fact, our summary statistics show that while the secured-debt-to-debt ratio is 26% for firms with low asset tangibility, it is 48% for firms with high asset tangibility, consistent with the identification strategy that firms with high asset tangibility are more affected by a change in creditor rights since higher levels of collateralizable assets allow for a larger increase in secured debt (and thus make the effect of stronger creditor rights on credit costs of secured debt more important) and, in general, imply more secured debt (and thus make the effect of stronger creditor rights on distress costs related with the use of secured debt more important).

³ For studies that use variation in default probability to measure the effects of bankruptcy reforms, see Rodano, Serrano-Velarde, and Tarantino (2016), Favara et al. (2017), Gross et al. (2021), and Schoenherr and Starmans (2021).

⁴ Economically, after the reform high asset tangibility firms exhibited relative changes in, e.g., leverage, the capex-to-sales ratio, and profitability of about 1.2, 10.3, and 1.4 percentage points, respectively, suggesting that creditor rights play an important role in corporate financial and investment policy.

After documenting the average effect, we attempt to understand why existing empirical studies find mixed evidence on the effect of creditor rights on corporate financial and investment policy, which seems puzzling given that whether the effect of creditor rights on credit costs or that on distress costs outweighs should ultimately be answered empirically. Our starting point here is to exploit the idea that the effect of creditor rights should vary across groups of firms (Rasmussen, 1992; Schwartz, 1997; Hart, 2000).⁵ In particular, we hypothesize that the effect of creditor rights on corporate leverage and investments depends on the firm type, as it influences the effect that creditor rights have on credit costs and distress costs and thus which of the two effects dominates. As such, stronger creditor rights can be beneficial in some firms (in which the effect of creditor rights on credit costs outweighs the effect on distress costs) or samples (in which such firms are more present), but can impose a cost in other firms or samples.

To test our hypothesis, we define firm types for which creditor rights should have a different effect by the firm characteristics size, profitability, and maturity, which should not only influence the effect that creditor rights have on credit costs but also that on distress costs. In particular, for large, profitable, and mature firms, the effect of creditor rights on credit costs may be relatively less important, as better information sharing (e.g., via long-lasting bank relations or more comprehensive disclosure obligations) may serve as a substitute for collateral value (Djankov, McLiesh, and Shleifer, 2007; Haselmann, Pistor, and Vig, 2010; Vig, 2013; Campello and Larrain, 2016; Aretz, Campello, and Marchica, 2020).⁶ At the same time, for these firms the effect of creditor rights on distress costs should be relatively more important, as distress costs should be more relevant. Intuitively, size implies higher bankruptcy costs for management and controlling shareholders by higher private benefits of control; profitability implies higher inefficient liquidation costs for claimholders; and maturity implies higher bankruptcy costs for claimholders through undiversified human capital. In fact, our bankruptcy data show that while the share of bankrupt firms that used a firm restructuring under the new law

⁵ Rasmussen (1992), Schwartz (1997), and Hart (2000) argue that diversity among firms implies that different firms might wish to be subject to different bankruptcy regimes. For example, Rasmussen (1992, p. 102) states: “A limited partnership whose sole asset is an apartment building, the construction of which is financed by a loan from a bank, might be the type of firm that would select the no-bankruptcy [creditor-friendly] option. (. . .) Their sole concern is receiving the lowest interest rate possible. The lower the interest rate, the greater the likelihood that the owners of the building will be able to service the debt load and have profits left over.”

⁶ Also Haselmann, Pistor, and Vig (2010), Vig (2013), Campello and Larrain (2016), and Aretz, Campello, and Marchica (2020) argue and find evidence that the positive effect of increased collateral value may be stronger for some firm types, in particular for small firms for which collateral typically compensates for information problems. Note that different than these papers, our paper studies differences in the firm type to understand the mixed empirical evidence in the bankruptcy literature, and moreover discusses the implications of the mixed evidence and our understandings on optimal bankruptcy design. Additionally, we argue that the firm type not only influences the positive effect of creditor rights on collateral value but also the negative effect of creditor rights on distress costs. Finally, our paper specifically studies the effects of bankruptcy law, and not the effects of collateral law as done by most of the mentioned papers.

was less than 1% among small firms, it increased monotonically in firm size, and was 31% among large firms, consistent with the view that the effect that creditor rights have on distress costs should be relatively more important for some firm types, such as large firms, for which distress costs are more relevant.

Consistent with our hypothesis, we find that the effect of creditor rights on corporate financial and investment policy strongly depends on the firm type. In particular, we find that the positive changes in leverage and investments of high asset tangibility firms relative to low asset tangibility firms are stronger among large, profitable, and mature firms (in which the effect of creditor rights on distress costs should outweigh that on credit costs) compared to those among small, unprofitable, and young firms. Indeed, while among small, unprofitable, and young firms high asset tangibility firms did not change leverage and investments statistically significant relative to low asset tangibility firms, the positive changes in leverage and investments of high asset tangibility firms relative to low asset tangibility firms are strong and significant among large, profitable, and mature firms.

In additional analysis, we also define firm types by firm size alone. Firm size not only seems to be theoretically and, based on our bankruptcy data, empirically an effective way to identify firms for which creditor rights should have a different effect, but also seems to be a particularly practicable way, thereby possibly offering a helpful guide to policymakers in choosing an appropriate bankruptcy law if a menu approach is not possible (detailed below). Strikingly, and closely echoing our bankruptcy data, we find that while the positive changes in leverage and investments of high asset tangibility firms relative to low asset tangibility firms are not present among small firms, they increase monotonically in firm size, and are strongest among large firms.

A causal interpretation of our results depends on the validity of the assumption that high and low asset tangibility firms would have followed the same trend in the absence of the reform. Although we provide credibility to this assumption by documenting that both groups of firms exhibited parallel trends in the absence of changes in the bankruptcy law, it is still possible that confounding shocks coincided with the introduction of the reform and affected firms with high asset tangibility differently than firms with low asset tangibility. We mitigate the concern that our conclusions are affected by confounding events in two ways. First, we present additional results in which we limit the possibility that both groups have experienced different shocks by comparing high and low asset tangibility firms within the same industry, e.g., an automobile manufacturer with an automobile retailer. Our results remain virtually unchanged, suggesting that our findings are not driven by different industry shocks that high and low asset tangibility

firms might have experienced.⁷ Second, we remeasure the effect of the reform, but by comparing changes in leverage and investments of firms with high probability of default, for which the design of the bankruptcy law should be more relevant, to those of firms with low probability of default. We obtain results that are (qualitatively) similar to those of our main analysis, suggesting that our findings should not be affected by confounding shocks. Overall, given the parallel trends between high and low asset tangibility firms as a reaction to shocks other than changes in the bankruptcy law, together with the heterogeneity across firm types and the results discussed here, confounding events are unlikely to affect our conclusions.

We make three contributions to the bankruptcy literature. First, we provide additional empirical evidence to the ongoing debate on the effect of creditor rights on corporate financial and investment policy. In particular, we provide evidence consistent with the notion that stronger creditor rights impose a cost on firms by increasing distress costs and thus prompting firms to reduce the risk of default by undertaking risk-reducing but unprofitable financial and investment decisions (or, in other words, that, on average, the effect of creditor rights on distress costs outweighs that on credit costs). Our findings that stronger creditor rights can have a negative effect complement those from few existing studies that provide such evidence (see Vig, 2013; Cho et al., 2014; Schoenherr and Starmans, 2021, who find a negative effect on leverage; and Acharya, Amihud, and Litov, 2011, who find a negative effect on investments). Moreover, and to the best of our knowledge, we are the first to provide more credible evidence that creditor rights can induce firms to undertake investments that reduce the risk of default but are however unprofitable, thereby mitigating endogeneity concerns of the cross-country findings by Acharya, Amihud, and Litov (2011).

Second, our understanding that the effect of creditor rights on corporate financial and investment policy depends on the firm type helps to reconcile the puzzling mixed empirical evidence of existing studies. Specifically, stronger creditor rights can have a positive effect in some firms (in which the effect of creditor rights on credit costs outweighs that on distress costs) or samples/countries (in which these firms are more important), but can have a negative effect in other firms or samples/countries. Notably, the suggestion that the effect of creditor rights should depend on the firm size, or, more specifically, that stronger creditor rights may be beneficial for small firms but costly for large firms, is closely consistent with the mixed evidence of the existing studies that typically find a positive effect of creditor rights in SMEs (e.g., Haselmann, Pistor, and Vig, 2010; Campello and Larrain, 2016; Ponticelli and Alencar, 2016; Rodano,

⁷ On the problem of confounding shocks and the possibility to mitigate the problem by comparing firms within the same industry, see Vig (2013).

Serrano-Velarde, and Tarantino, 2016; Calomiris et al., 2017; Aretz, Campello, and Marchica, 2020) but a negative effect in listed or large firms (Acharya, Amihud, and Litov, 2011; Vig, 2013; Cho et al., 2014; Schoenherr and Starmans, 2021).⁸

Finally, and perhaps most important, our understanding that the effect of creditor rights depends on the firm type also has important implications for optimal bankruptcy design. In particular, our findings are contrary to a widely held opinion that bankruptcy law should be uniform and balance the effect of creditor rights on credit costs and distress costs. For example, Acharya, Amihud, and Litov (2011, p. 165) conclude: “The optimal level of creditor rights should balance their positive effect on the supply of credit against their negative effect on corporate risk-taking and on operating performance, as well as on the demand for debt. In future work, it would be interesting to assess directly this important tradeoff.” Rather, our findings (in combination with the mixed empirical evidence) point to a menu of procedures in which a debtor-friendly and creditor-friendly procedure co-exist and thus allow different types of firms to utilize the procedure that suits them best.^{9,10} Importantly, our argument that for each firm type one of the two effects that creditor rights have should dominate gives rise to a practically feasible yet effective solution of a menu that consists only of two sets of procedures; a particularly creditor-friendly procedure that allows, e.g., SMEs to utilize the prevailing dominance of credit costs over distress costs, and a particularly debtor-friendly procedure that allows, e.g., large firms to utilize the prevailing dominance of distress costs over credit costs.¹¹ If such a menu is not possible, our analysis suggests that countries should choose a debtor-friendly or creditor-friendly procedure, depending on an important firm type in the country (on this point, see

⁸ After we posted the relevant version of our paper, including the role of the firm type, on SSRN in January 2019, Schoenherr and Starmans (2021) followed a similar logic to ours in explaining the mixed empirical evidence (while another version of their paper circulated earlier, to the best of our knowledge, the relevant version of their paper, including the role of the firm type, was uploaded in November 2019). Specifically, while we argue that the effect of creditor rights depends on the firm type, and as one specific firm type, on the firm size, they identify another specific firm type, namely, how owners’ and managers’ personal costs of bankruptcy influence the effect of creditor rights. Their findings further highlight the important role of the firm type in understanding the mixed empirical evidence in the bankruptcy literature. Note that Schoenherr and Starmans (2021) also suggest similar implications for optimal bankruptcy design as we do (specifically, a menu approach or choosing the law in dependence of an important firm type in the country, see below).

⁹ On the menu approach, see Rasmussen (1992), Schwartz (1997), and Hart (2000).

¹⁰ In fact, although we do not see a menu approach, it seems that leading economies avoid a bankruptcy law that balances creditor rights. Instead, they tend to choose an extreme case, see, e.g., the U.S., Canada, France, Germany (after the reform in 2012), and the U.K. (after the reform in 2020) for particularly debtor-friendly laws, and Germany (before 2012) and the U.K. (before 2020) for particularly creditor-friendly laws.

¹¹ Some concerns about a menu approach—which are actually directed to the idea of a menu within a firm type—are, for example, that a firm typically borrows at different times with different preferences over the precise formulation of the bankruptcy procedure or that a firm often has many creditors and that different procedures on creditors are practically not feasible (see Schwartz, 1997, who discusses possible obstacles in detail and provides useful solutions to them). However, our focus on firm types, that is, that for each firm type one of the two effects that creditor rights have should outweigh, largely mitigates such concerns as desired changes in the bankruptcy procedure (i.e., changes in the overweight) should not or only rarely occur, e.g., when a firm changes its firm type by evolving from a small to a large firm.

Schoenherr and Starmans, 2021). Here, our suggestion of using profitability, maturity, and, especially, size not only seems to provide an effective, but also a particularly practical solution to define firm types for which creditor rights should have a different effect, thereby possibly offering a helpful guide to policymakers in selecting an appropriate bankruptcy system.

The rest of the paper is organized as follows. Section 3.2 describes the German bankruptcy law reform of 2012. Section 3.3 describes the data and discusses the empirical strategy. Section 3.4 presents the results on the average effect and Section 3.5 presents the results on heterogeneity in the firm type. Section 3.6 presents the results when measuring the effect of the reform by comparing firms with high and low default probability. Section 3.7 concludes.

3.2 Germany's bankruptcy law reform of 2012¹²

3.2.1 Prereform bankruptcy law¹³

The prereform bankruptcy law in Germany was characterized by strong creditor rights, which allowed secured creditors to recover their collateralized claims, but also imposed significant distress costs for shareholders and management.¹⁴ In particular, immediately after submitting the bankruptcy petition, management was dismissed, and corporate control was transferred to an insolvency administrator.¹⁵ The task of the administrator was to continue the firm until the voting date (on which different classes of claimants, such as secured creditors, unsecured creditors, the workforce, and suppliers, vote on firm continuation or liquidation), which was scheduled about six months after submission. However, the administrator's overriding objective to protect secured creditors from adverse changes in their asset value (i.e., to liquidate the firm if a threat to the asset value was recognized) together with the threat of contingent liabilities towards creditors when violating this objective essentially gave secured creditors the power to seize their collateralized assets during the bankruptcy procedure, which in turn led to

¹² In describing the bankruptcy reform in Germany, we closely follow the structures and wordings of other studies that describe bankruptcy legislations or bankruptcy reforms including Posner (1997), La Porta et al. (1998), Djankov, McLiesh, and Shleifer (2007), Acharya, Amihud, and Litov (2011), Vig (2013), Hackbarth, Haselmann, and Schoenherr (2015), Ponticelli and Alencar (2016), Rodano, Serrano-Velarde, and Tarantino (2016), Favara et al. (2017), and Schoenherr and Starmans (2021).

¹³ On the prereform and postreform laws, see the German bankruptcy law and the ESUG bill (Gesetzentwurf der Bundesregierung: Entwurf eines Gesetzes zur weiteren Erleichterung der Sanierung von Unternehmen) of, e.g., Mai 2011 (available at <https://dserver.bundestag.de/btd/17/057/1705712.pdf>).

¹⁴ On the strong creditor rights of the prereform bankruptcy law in Germany, see, e.g., also Djankov, McLiesh, and Shleifer (2007) and Davydenko and Franks (2008).

¹⁵ Theoretically, after three months, it was possible for management to regain corporate control from the insolvency administrator to then continue the firm until the voting date under self-administration. However, this possibility was virtually never employed (in only about 1% of cases), presumably because it was only possible with creditors' consent. Moreover, 55% of bankrupt firms got liquidated by the insolvency administrator already within the first three months of the procedure, which made this option also in theory unfeasible for many firms.

automatic firm liquidation. Empirically,¹⁶ 86% of firms in bankruptcy got liquidated before the voting date (in fact, 55% got liquidated already within the first three months of the procedure), that is, by the insolvency administrator. Overall, while secured creditors could have expected a recovery rate of more than 70%, bankrupt firms experienced a firm liquidation at the end the bankruptcy procedure in about 89% of cases (continuation was either in the form of an accepted reorganization plan or an asset deal).¹⁷

3.2.2 The ESUG Act

As a response to the 2007–2009 financial crisis (and to compete with other reorganization jurisdictions, see below), in 2012, Germany reformed its bankruptcy law by introducing the Act for Further Facilitation of the Restructuring of Companies (ESUG Act henceforth). The main objective of the reform was to facilitate the restructuring of distressed firms, and to achieve this objective, several provisions similar to U.S. Chapter 11 provisions were adopted.¹⁸ In particular, the ESUG Act introduced a debtor-in-possession proceeding of about six months (i.e., until voting date), which allows management to stay in unrestricted corporate control during the bankruptcy procedure.^{19,20} Additionally, the proceeding imposes automatic stay on the assets, which prevents secured creditors from seizing their assets, and hence protects debtors from automatic liquidation. Importantly, management can enter the reorganization procedure unilaterally, that is, there are no strict restrictions, such as creditors' consent, when management files

¹⁶ The data on the prereform law come from Icks and Kranzusch (2009, 2010) and are based on bankruptcy cases from 2002 to 2008. The reported data exclude sole proprietorships and micro-sized firms (sales less than 2 million euro), which both had even higher liquidation rates (Icks and Kranzusch, 2009, 2010).

¹⁷ As under the new law, on the voting date, the law provided the possibility of a cram down option that allowed the court to impose a restructuring plan on dissenting groups if the majority of the claimant groups confirmed the plan. However, because major decisions were already made by the insolvency administrator before the voting date, this possibility was hardly ever an option (on this, see the ESUG bill besides Icks and Kranzusch, 2009, 2010).

¹⁸ It is noteworthy that we restrict our discussion on the changes made by the ESUG Act to the debtor-in-possession proceeding. The reform introduced several other restructuring-friendly mechanisms, such as the possibility of a debt-equity swap, extending the classes of claimants by the group of equity holders (thereby increasing the voting power of groups that are typically interested in a firm continuation), and the possibility of an umbrella proceeding for firms that are not yet in payment default.

¹⁹ As U.S. Chapter 11, upon filing, a trustee is appointed who has the task to help management and to inform creditors about ongoing developments of the process. The trustee has no control or liquidation rights.

²⁰ Theoretically, after three months, creditors can ask the court to terminate the debtor-in-possession proceeding. However, this is only possible if concrete circumstances can be proved—with any uncertainty or ambiguity going to the expense of the complaining party—that would result in substantial disadvantages for all claimant groups (according to the bill, this formulation is used to guarantee that there are no obstacles that might limit management's ability to remain in the proceeding, which is intended to end up in a firm continuation as the outcome of the bankruptcy procedure). Based on anecdotal evidence (see footnote 22 below), the debtor-in-possession proceeding was terminated only in exceptional cases, which occurred either because creditors were able to prove such circumstances or because firms voluntarily initiated a firm liquidation. Additionally, as shown in Panel D of Figure 3.1 below, almost 90% of firms that filed for ESUG reorganization experienced a firm continuation as the outcome of the bankruptcy procedure, empirically documenting the effectivity of the law to protect debtors against liquidation attempts of creditors.

for the debtor-in-possession procedure (the only conditions are that the reorganization procedure is not obviously unfeasible and that firm's assets are sufficient to cover the legal costs). Finally, to facilitate firm continuation as the outcome of the process, the law furthermore provides a cram down option that allows the court to impose a restructuring plan on dissenting groups if the majority of the claimant groups confirms the restructuring plan.

By introducing a debtor-in-possession proceeding of about six months (accessible without creditors' consent) that allows management to stay in corporate control during the bankruptcy procedure and moreover imposes automatic stay on the assets, the ESUG Act reduced secured creditors' power to recover their collateralized claims, but also reduced distress costs for shareholders and management. To get a better understanding on the strength of the changes made by the ESUG Act, it is useful to consider the well-known creditor rights index by La Porta et al. (1998), which captures the power of secured creditors against the debtor in bankruptcy. The index ranges between four (strong creditor rights, or, equivalently, weak debtor rights) and zero (weak creditor rights, or, equivalently, strong debtor rights), and is formed by adding one when (i) the reorganization procedure does not impose an automatic stay on the assets of the firm, i.e., secured creditors are able to seize their asset after the reorganization petition is approved; (ii) the reorganization procedure imposes restrictions, such as secured creditors' consent, for a debtor to file for it; (iii) secured creditors are ranked first in the distribution of the proceeds that result from the disposition of the assets of a bankrupt firm; and (iv) an administrator is appointed for running the firm in bankruptcy, i.e., management is dismissed during bankruptcy.²¹ As such, ESUG reduced Germany's creditor rights index from four to one (only secured creditors' right to be paid first is left unchanged), that is, by three points. Based on the cross-country study by Djankov, McLiesh, and Shleifer (2007), this is associated with a reduction in the private credit to GDP ratio of about 18 percentage points.

Ample anecdotal evidence exists on the empirical relevance of the changes made by the ESUG Act.²² In short, most of the comments and surveys run under the notion that the new law marks the beginning of a new restructuring culture in Germany, thereby ending decades of a by liquidation hallmarked bankruptcy law. Moreover, it is commonly concluded that the main goals of the bankruptcy law reform were achieved. Specifically, the debtor-in-possession proceeding is easily accessible, a firm restructuring is facilitated, and the course of the bankruptcy

²¹ On the creditor rights index of La Porta et al. (1998), see also Djankov, McLiesh, and Shleifer (2007), Brockman and Unlu (2009), and Acharya, Amihud, and Litov (2011).

²² For surveys based on standardized questions or interviews with relevant market participants, such as managers, creditors, judges, or consultants, see the Creditreform survey (2014), the Boston Consulting Group study (2018), and the ESUG evaluation report commissioned by the German government (2018).

procedure has become more predictable (by getting around the arbitrary decisions of the insolvency administrator).

In Figure 3.1, we present empirical evidence on ESUG. In particular, Panel A shows the filing rate of bankrupt firms for a reorganization procedure under ESUG, separately for all bankrupt firms and for the 50 largest annual bankruptcy cases (measured by sales). While the filing rate for an ESUG reorganization is not more than 3% among all bankrupt firms (i.e., 97% voluntarily filed for a liquidation procedure under an insolvency administrator), in 2017, it is 62% among the 50 largest bankrupt firms. In fact, Panel B, which outlines the share of bankrupt firms that used a firm restructuring under ESUG for different firm sizes, shows that while the share was less than 1% among small firms, it increased monotonically in firm size, and was 31% among large firms.^{23,24} These data are consistent with our hypothesis in Section 3.5 below that the effect that creditor rights have on distress costs should be relatively more important for some firm types, such as large firms, for which distress costs are more relevant. Panel C documents the acceptance rate when a firm files for ESUG reorganization. As can be seen, in 2017, about 90% of firms that filed for ESUG reorganization received the approval to do so, empirically reflecting that there are no strict restrictions, such as creditors' consent, for a debtor to enter the reorganization procedure. Finally, Panel D shows the continuation rate (either in the form of an accepted reorganization plan or an asset deal) of firms that filed for an ESUG

²³ Besides introducing a reorganization procedure, the ESUG Act also made changes to the liquidation procedure. In particular, the new law stipulates that, in liquidation, the groups of claimants set up a creditors' committee and appoint the insolvency administrator, who was appointed by the insolvency court prior to the reform. See Closset and Urban (2019), who study the effect of this in a sample of small firms by comparing firms just above a size threshold for which setting up a creditors' committee and appointing an insolvency administrator is mandatory with firms just below the threshold for which this rule is optional (the size threshold is met if at least two of the following conditions are given: assets greater than €4.84m, sales greater than €9.68m, or average number of employees greater than 50). Notably, the authors find that firms above the threshold reduced leverage after the reform relative to firms below the threshold and argue that this might be because the stronger creditor rights to influence the appointment of the administrator increased distress costs and thus induced firms to reduce risk of default by avoiding debt. To us, this interpretation seems incomplete, given that the considered small firms typically voluntarily file for liquidation in bankruptcy (see Panels A and B of Figure 3.1) and thus should not be concerned about changes in distress costs, or, alternatively, can use the debtor-in-possession procedure when seeking firm restructuring. In our view, a more natural explanation of their finding of reduced leverage might be that the new law did not increase creditor rights in liquidation, but actually reduced creditor rights in liquidation, given that problems of creditor coordination together with weak incentives of the other claimant groups to cooperate can increase the length of liquidation procedures, thereby reducing creditors' power to preserve the value of their assets and thus increasing credit costs (Rodano, Serrano-Velarde, and Tarantino, 2016) (note that, prior to ESUG, in two thirds of the cases the appointment of the administrator took place already within a week after filing (Icks and Kranzusch, 2009)). See Rodano, Serrano-Velarde, and Tarantino (2016), who argue and provide evidence that lengthy liquidation procedures in Italy reduced creditor rights in liquidation and led to higher credit costs and tighter credit constraints for SMEs.

²⁴ On the fact that ESUG reorganization procedures are especially used by large firms, see also the ESUG evaluation report and the Boston Consulting Group study. Note that the data for Panel A are from The Boston Consulting Group and JUVE legal information, for Panel B are from the ESUG evaluation report, for Panel C are from The Boston Consulting Group, and for Panel D are from Finance-Magazin/Falkensteg.

procedure.²⁵ As shown, almost 90% of firms that filed for ESUG experienced a firm continuation as the outcome of the bankruptcy procedure, which empirically documents the effectiveness of the new law to prevent secured creditors from seizing their collateralized assets and to cram down dissenting groups on the voting date.²⁶

3.2.3 Brief history of the bankruptcy law reform

Before proceeding, it might be helpful to briefly outline the history of the bankruptcy law reform. To compete with other reorganization laws that have been increasingly favored by investors in their investment decisions, within the coalition agreement of 2009, political parties agreed to modernize Germany's bankruptcy law. However, ultimately no major changes have been made on this, until, in June 2010, an oppositional party put forward a letter of request demanding the government to immediately implement a restructuring law on the ground that one became aware of the full extent of the economic consequences that the 2007–2009 financial crisis had and that under the existing law it needs to be expected that thousands of distressed but viable firms will be inefficiently liquidated.²⁷ Following this, in September 2010, the parliament presented a discussion paper to ESUG (which formed the basis for the first ESUG draft in February 2011) in which for the first time specific amendments were proposed. After rounds of modifications and reform voting, in December 2011, the government passed ESUG, and set its effective date for March 2012.²⁸

There are two important points here for our empirical analysis. First, the bankruptcy law reform should present a reasonably exogenous change in creditor rights, given that the reason for the reform, be it the 2007–2009 financial crisis or the intention to compete with other reorganization laws, should not be related to firms' financial and investment policy or at least not

²⁵ It should be noted that the continuation rates on ESUG procedures are primarily intended to empirically confirm the effectiveness of the new law in preventing automatic firm liquidation by reducing secured creditors' power to seize the assets; and not to compare changes in the continuation rates to the prereform law. In particular, while the continuation rates on ESUG procedures only include firms that seek for firm reorganization and thus exclude firms that voluntarily apply for liquidation, the above presented liquidation rates on the prereform law include such firms, which might naturally lead to higher liquidation rates.

²⁶ To our knowledge, there are no available data on secured creditors' recovery rates within debtor-in-possession procedures in Germany.

²⁷ Similarly, the 2020 bankruptcy reform in the U.K. initially had its foundation in a number of proposals in 2016, and was actually based on the idea to compete with other restructuring laws. Yet, ultimately no major changes have been made, until the Covid-19 crisis and its economic consequences brought the proposals back on the legislative agenda (Payne, 2021).

²⁸ Most importantly, the request of the oppositional party (Antrag der Fraktion BÜNDNIS 90/DIE GRÜNEN: Insolvenzrechtsreform unverzüglich vorlegen – Außergerichtliche Sanierungsverfahren stärken – Insolvenzplanverfahren attraktiver gestalten) of June 2010 is available at <https://dserver.bundestag.de/btd/17/020/1702008.pdf>. For an overview of the history of the bankruptcy reform provided by beck-aktuell (Gesetzgebung), see [https://rsw.beck.de/aktuell/gesetzgebung/gesetzgebungsvorhaben-zusaetzliche-materialien/erleichterung-der-unternehmenssanierung-\(esug\)](https://rsw.beck.de/aktuell/gesetzgebung/gesetzgebungsvorhaben-zusaetzliche-materialien/erleichterung-der-unternehmenssanierung-(esug)).

differently across firms with high and low asset tangibility (in fact, we will provide empirical evidence on this by showing that both groups of firms followed parallel trends before the reform). Second, given that the introduction of the law was prompted by the financial crisis, the reform progressed quickly, which arguably rules out major adjustments of firms and creditors in anticipation of the legal changes before the reform was passed (in December 2011).²⁹

3.3 Data and empirical strategy

3.3.1 Data

Our data come from the Bureau van Dijk database, which includes besides public firms also private firms, thereby allowing us to measure the effect of the reform for a broad variety of different types of firms. We begin the sample construction by excluding sole proprietorships and micro sized firms (sales less than two million euro), as these firms are typically not subject to mandatory disclosure rules. Following convention (e.g., Favara et al., 2017), we also exclude firms from financial (SIC codes 6000–6700) and public administration industries (SIC codes 9100–9900), and drop firm-years with negative values in sales or assets. Moreover, we exclude each firm-year observation without complete data on all dependent and control variables used in the main empirical analysis, and firms without data on asset tangibility (as a measure of variation in the effect of the reform). Finally, we require that each firm has at least two observations, given that we use firm fixed effects in our empirical analysis. For the sample period 2009 to 2015, the final sample consists of 7,478 firms, or 4,985 firms when employing our empirical strategy, i.e., when only considering high and low asset tangibility firms. Our data offer detailed information on firms' financial and investment policies (our dependent variables), and on other important firm characteristics that serve as control variables or are useful in addressing the main goals of our paper. In Table 3.2 below, we will discuss summary statistics for the main variables.

3.3.2 Empirical strategy

To measure the effect of the bankruptcy law reform on firms' financial and investment policy, we use a difference-in-differences analysis. In particular, we use variation in the effect of the reform in asset tangibility and probability of default. In our main analysis, we follow Vig (2013) and compare changes in leverage and investments of firms with high asset tangibility

²⁹ Likewise, Rodano, Serrano-Velarde, and Tarantino (2016) argue that the 2005–2006 reforms in Italy were prompted by a corporate scandal and thus were not caused by trends in corporate performance. Additionally, the authors note that the reform process went quickly, mitigating concerns about anticipation.

around the reform to those of firms with low asset tangibility, exploiting that firms with high asset tangibility should be more affected by a change in creditor rights since higher levels of collateralizable assets make the effect of creditor rights on credit costs (by enabling to increase secured debt) and on distress costs related with secured debt (by implying more secured debt) more important.

Our empirical analysis uses that firms with high asset tangibility should be more affected by the bankruptcy law reform, which has the advantage of a control group in firms with low asset tangibility that account for other factors (common to all firms) that may have changed simultaneously with the reform. However, a causal interpretation of our results (i.e., that changes in leverage and investments of firms with high asset tangibility after the reform relative to those of firms with low asset tangibility are caused by the reform) depends on the assumption that both groups of firms would have followed the same trend in the absence of the reform, i.e., that both groups of firms react to shocks other than changes in the bankruptcy law in an identical way. Although we will give credibility to this assumption by showing that high and low asset tangibility firms exhibited parallel trends in the absence of a bankruptcy reform, it is still possible that confounding shocks coincided with the introduction of the reform and affected firms with high asset tangibility differently than firms with low asset tangibility.

We mitigate the concern of confounding events in two ways. First, we present additional results in which we limit the possibility that both groups have experienced different shocks by comparing high and low asset tangibility firms within the same industry, e.g., an automobile manufacturer with an automobile retailer.³⁰ And second, in Section 3.6 below, we remeasure the effect of the reform, but by comparing changes of firms with high probability of default, which should be more affected by the design of the bankruptcy law, to those of firms with low probability of default.³¹ The idea is that if we obtain results consistent with those of our main analysis, our conclusions should not be affected by confounding shocks. Put differently, for confounding events to affect our understandings, they must not only have had a different effect on high and low asset tangibility firms, but also on high and low default probability firms.

For our main analysis, we follow Vig (2013) and divide our sample into three terciles (top 33%, middle 33%, and bottom 33%) based on firms' average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), and define firms in the highest tercile as our treated group and firms in the lowest tercile as our control group

³⁰ For the problem of confounding shocks and the possibility to mitigate the problem by comparing firms within the same industry, see Vig (2013).

³¹ For studies that exploit variation in probability of default to measure effects of bankruptcy law reforms, see Rodano, Serrano-Velarde, and Tarantino (2016), Favara et al. (2017), Gross et al. (2021), and Schoenherr and Starmans (2021).

(cutting off firms that are in the middle 33% of the asset tangibility distribution). We estimate the effect of the bankruptcy law reform with the following difference-in-differences regression:

$$y_{it} = \alpha_i + \lambda_t + \eta POST_t + \gamma TREATED_i + \delta(POST_t \times TREATED_i) + \beta X_{it} + \varepsilon_{it}, \quad (3.1)$$

where i indexes firms, and t indexes time; y_{it} is one of our dependent variables of interest on financial and investment policy; α_i are firm fixed effects that control for time-invariant differences between firms; λ_t are year fixed effects that control for time-varying factors that are common to all firms; $POST_t$ is a dummy variable that equals one for the years after the ESUG Act was passed (2012 to 2015), and zero for the years before the bankruptcy law reform was passed (2009 to 2011); $TREATED_i$ is an indicator variable that equals one if the firm belongs to the treated group (top 33% firms), and zero if it belongs to the control group (bottom 33% firms); X_{it} are the time-varying covariates $EBIT/Assets$ and $Log(Sales)$ that might affect our dependent variables; ε_{it} is the firm specific error term; and δ is the coefficient of interest which measures the difference-in-differences effect.³²

As mentioned, to mitigate the concern that our findings are driven by confounding events that coincided with ESUG and affected high and low asset tangibility firms differently, we present additional results in which we limit the possibility that both groups have experienced different shocks by comparing high and low asset tangibility firms within the same industry. To do so, we follow Vig (2013) and expand the baseline specification in equation (3.1) with the interaction term $\omega_j \times \lambda_t$ (where j indexes industries; and ω_j are industry fixed effects), which effectively compares firms within the same industry by absorbing time-varying industry shocks. We measure this specification by first generating 13 suitable industries (aimed at ensuring that firms within one industry experience the same industry shock), to which we then assign firms based on a 4-digit SIC code.³³ To ensure that firms within one industry were exposed to the

³² The difference-in-differences specification follows that employed by Vig (2013) and Favara et al. (2017).

³³ An industry classification based on a 2-digit SIC code is rather unsuitable to ensure that treated and control firms experience the same industry shocks. The reason is that this classification leads to industries, such as manufacturing or retail, that contain firms from different branches. For example, the manufacturing industry contains firms from food and kindred, furniture and fixtures, or transportation, while the retail industry contains firms from food stores, furniture stores, or automobile retailer. As a result, one might compare firms from different branches within one industry, e.g., within the manufacturing industry a high tangibility automobile manufacturer with a low tangibility food production firm, which violates our goal that both firms should experience the same industry shock. To achieve our goal, we first generate suitable industries, such as food, furniture, or automobile, to which we then assign firms across the value chain of these industries based on a 4-digit SIC code (e.g., an automobile manufacturer and automobile retailer to the automobile industry). As such, one compares, e.g., the high tangibility automobile manufacturer with the low tangibility automobile retailer, ensuring that both firms experience the same industry shock. The 4-digit SIC codes assigned to the 13 industries are available upon request. Our results remain virtually unchanged when employing an industry classification based on a 2-digit SIC code, suggesting that industry shocks were rather unimportant.

same industry shock, we only consider firms for which the 4-digit SIC code clearly indicates to which industry the firm belongs, resulting in a reduction of the sample size from 4,985 to 3,152 firms.³⁴ Table 3.1 provides an industry distribution of our sample.

Finally, we cluster standard errors at the firm level, with possible correlations of the errors over time being addressed by the time dummies (Petersen, 2009). Our findings remain similar when the errors are double clustered at the firm and year levels (as done by Vig, 2013).

3.3.3 Summary statistics and preliminary results on the average effect

In columns 1 and 4 of Table 3.2, we present prereform summary statistics for our main variables, separately for firms with high and low levels of asset tangibility. Most importantly, comparing firms in the highest tercile of the asset tangibility distribution with firms in the lowest tercile of the asset tangibility distribution gives strong variation in the ability to collateralize assets (specifically, asset tangibility is 74% for firms in the highest tercile, whereas it is 17% for firms in the lowest tercile) and thus should give strong variation in the effect of the reform. Indeed, while the Secured-Debt-to-Debt ratio is 26% for firms with low asset tangibility, it is 48% for firms with high asset tangibility, which is consistent with the identification strategy that firms with high asset tangibility are more affected by a change in creditor rights as more collateralizable assets allow for a stronger increase in secured debt (and hence make the effect of stronger creditor rights on secured borrowing costs more important) and typically imply more secured debt (and hence make the effect of stronger creditor rights on distress costs associated with the use of secured debt more important) (Vig, 2013).

In Table 3.2, we furthermore present preliminary results on the average effect of the reform. In particular, in columns 2 and 5, we respectively document the before-after reform differences in the main variables for high and low asset tangibility firms; while, in column 7, we subtract the before-after reform differences of high asset tangibility firms from those of low asset tangibility firms, thus essentially reporting the results of a simplified version of the difference-in-differences regression of equation (3.1) (i.e., without firm and year fixed effects, and without controls).³⁵ As can be seen, firms with high levels of asset tangibility increased corporate leverage after the reform relative to firms with low levels of asset tangibility (rows 1 and 2), using both secured debt and unsecured debt (as evident by an unaffected debt composition in row 3). Moreover, relative to firms with low asset tangibility, after the reform high asset tangibility

³⁴ For instance, we neglect firms from the 4-digit SIC code 3069 Fabricated Rubber Products as it is unclear whether these firms produce products, e.g., for the automobile industry or furniture industry.

³⁵ Note that our analyses here basically follow the analyses regarding summary statistics and preliminary evidence of Vig (2013). Also note that the descriptions of the tables and figures in this paper in general follow those of Vig (2013).

firms discarded risk-reducing but typically unprofitable investments—such as accumulating high recovery assets (as measured by investments in fixed assets in rows 4 and 5), or diversifying activities (as measured by enlarging actions in row 6). Finally, firms with high asset tangibility exhibited relatively higher levels of profitability (row 7) and risk (row 8) after the reform, supporting the results on investments. Overall, the preliminary results are consistent with the hypothesis that stronger creditor rights impose a cost on firms in that they increase distress costs and thus prompt firms to undertake financial and investment decisions that reduce the risk of default but are however less profitable. In other words, the results suggest that, on average, the effect of creditor rights on distress costs outweighs that on credit costs.

3.4 Average effect on leverage and investments

Stronger creditor rights can reduce credit costs of secured debt through higher collateral value and thus may allow firms to increase leverage and investments. However, stronger creditor rights also increase distress costs and thus may prompt firms to reduce the risk of default by lowering leverage and undertaking risk-reducing but unprofitable investments (Acharya, Amihud, and Litov, 2011; Vig, 2013; Ponticelli and Alencar, 2016; Mann, 2018; Schoenherr and Starmans, 2021). In this section, we study the average effect of Germany’s bankruptcy law reform of 2012 that reduced creditor rights.

3.4.1 Corporate leverage

Columns 1 to 3 of Table 3.3 present the results on corporate leverage, as measured by the Debt-to-Capital ratio. As shown in columns 1 (without controls) and 2 (with controls), the coefficient on $POST_t \times TREATED_i$ is positive and statistically significant. That is, firms with high levels of asset tangibility increased corporate leverage after the bankruptcy law reform relative to firms with low levels of asset tangibility (economically, by about 1.2 percentage points with regard to column 2). The result is consistent with the notion that higher distress costs of stronger creditor rights induce firms to reduce the risk of default by lowering leverage. To mitigate the concern of possible confounding events that affected high and low asset tangibility firms differently, in column 3, we present the results of a regression that expands the baseline specification in equation (3.1) with the interaction term $\omega_j \times \lambda_t$, thereby comparing high and low asset tangibility firms within the same industry (Vig, 2013). As can be seen, our results remain virtually unchanged, suggesting that our findings are not affected by different industry shocks that high and low asset tangibility firms might have experienced. Finally, for robustness, in columns

4 to 6, we repeat the analysis using the Debt-to-Assets ratio as an alternative measure of corporate leverage. As shown, our findings remain unaffected.

Our empirical analysis uses variation in the effect of the reform in asset tangibility, which gives a treatment group in firms with high asset tangibility and allows for a control group in firms with low asset tangibility that accounts for other factors, common to all firms, that may have changed together with the reform. However, a causal interpretation of our results hinges on the assumption that both groups of firms would have followed the same trend in the absence of the reform. In Panel A of Figure 3.2, we provide credibility to this assumption by presenting the de-meaned time series of corporate leverage for firms with high and low levels of asset tangibility (following Vig, 2013). As visible, while high asset tangibility firms increased corporate leverage relative to low asset tangibility firms in the two years following the introduction of the bankruptcy law reform (2012 and 2013), they exhibited parallel trends to low asset tangibility firms before the introduction of the reform (2009 to 2011), as well as in the years after the two-year adjustment phase (2013 to 2015). This supports the assumption that both groups of firms typically receive the same shocks and thus would have followed the same trend in the absence of the ESUG Act.

In Table 3.4, we confirm this graphical evidence statistically by reporting annual changes in leverage and investments of firms with high asset tangibility relative to those of firms with low asset tangibility. More specifically, in columns 1 and 2, we define 2009 as the pre-treatment year and 2010 as the fictive post-treatment year, that is, we examine changes of high asset tangibility firms relative to those of low asset tangibility firms from 2009 to 2010; in columns 3 and 4, we define 2010 as the pre-treatment year and 2011 as the fictive post-treatment year; and so on. As can be seen, while high asset tangibility firms increased corporate leverage (rows 1 and 2) statistically significant relative to low asset tangibility firms in the two years following the reform, corporate leverage of high asset tangibility firms did not change relative to low asset tangibility firms in the absence of changes in the bankruptcy law, i.e., from 2009 to 2011, and from 2013 to 2015.

Finally, in columns 7 to 9 of Table 3.3, we present additional results on the debt composition. This allows to infer whether firms with high asset tangibility used secured debt (although credit costs of secured borrowing should have increased) or/and unsecured debt to increase corporate leverage; or, as another possibility, decreased secured debt (as a result of increased credit costs) but increased corporate leverage by unsecured debt.³⁶ As can be seen, high asset tangibility firms did not change their debt composition after the reform relative to low asset

³⁶ On this, see also Vig (2013).

tangibility firms, suggesting that they increased leverage using both secured debt as well as unsecured debt.

3.4.2 Corporate investments

Firms can also reduce the risk of default by investments. In particular, Acharya, Amihud, and Litov (2011) argue that the higher distress costs of stronger creditor rights may induce firms to reduce default risk by undertaking risk-reducing but unprofitable investments. The authors suggest that such investments can be the accumulation of assets with high recovery value that allows firms to defer default in situations of distress by liquidating these assets (and using the proceedings to pay debts due), or diversifying activities. In this subsection, we test these predictions. We measure investments in high recovery assets by investments in fixed assets, specifically, by the CAPEX-to-Sales ratio. To proxy for diversifying activities, we use enlarging actions, specifically, $\text{Log}(\text{Assets})$. Note that firm size might only be a rough measure of diversification, as changes in size may also come from growth within business lines. However, firm size should still be informative about corporate diversification, given that large changes in size should to some extent be the result of growth across different business lines. Below, we will additionally study the effect of the reform on profitability and risk, thereby providing evidence on the underlying mechanisms of our investment policy findings.

In Table 3.5, we present the results on corporate investment policy. As shown in columns 1 to 3, relative to firms with low asset tangibility, after the reform firms with high asset tangibility reduced high recovery assets that allow firms to defer default in distress (by about 10.3 percentage points). For robustness, in columns 4 to 6, we use the Fixed-Assets-to-Assets ratio as an alternative measure of investments in high recovery assets. As can be seen, our findings remain (qualitatively) unaffected. Finally, columns 7 to 9 show that, relative to low asset tangibility firms, after the reform high asset tangibility firms reduced enlarging actions that can reduce the risk of distress but are typically associated with lower levels of profitability (by about 10.7%). Taken together, our results provide empirical evidence consistent with the notion that the higher distress costs of stronger creditor rights induce firms to undertake risk-reducing but less profitable investment decisions.

In Figure 3.3, we plot the CAPEX-to-Sales ratio for firms with high and low asset tangibility over time. As visible, while high asset tangibility firms reduced the ratio relative to low asset tangibility firms in the two years following the reform, they did not change investment policy relative to low asset tangibility firms without changes in the bankruptcy law (i.e., from 2009 to 2011, and from 2013 to 2015), providing credibility to the parallel trends assumption.

Also here, Table 3.4 (row 4) confirms this visual impression statistically. Indeed, the table shows that, besides leverage and investments, both groups of firms exhibited parallel trends also in profitability.

To provide support on the mechanisms underlying our results regarding investment policy, in Table 3.6, we study the effects of the reform on profitability and risk. In particular, if the observed changes in investments are caused by the fact that firms eliminated risk-reducing but unprofitable investments that were previously implemented to avoid high bankruptcy costs, we should see an increase in profitability and risk after the reform.³⁷ We measure profitability by the EBITDA-to-Assets ratio and risk by the standard deviation of profitability (calculated separately for the prereform and postreform periods). As can be seen, firms with high asset tangibility exhibited higher levels of profitability (columns 1 to 3) (by about 1.4 percentage points) and risk (columns 4 to 6) after the bankruptcy reform relative to firms with low asset tangibility.

Overall, our results on the average effect support the notion that stronger creditor rights impose a cost on firms by increasing distress costs and thus prompting firms to reduce the risk of default by undertaking risk-reducing but unprofitable financial and investment decisions. In other words, our findings suggest that, on average, the effect of creditor rights on distress costs dominates that on credit costs.

3.5 Heterogeneity in the firm type

After studying the average effect, in this section we ask why existing empirical studies find mixed evidence on the effect of creditor rights on firms' financial and investment policy. This is puzzling (and unsatisfying to guide policymakers), given that whether the effect of creditor rights on credit costs or that on distress costs outweighs should ultimately be an empirical issue. Our starting point is to use the idea that the effect of creditor rights should vary across groups of firms.³⁸ Specifically, we hypothesize that the effect of creditor rights on corporate leverage and investments depends on the firm type, as it influences the effect of creditor rights on credit costs and distress costs and thus which effect dominates. Therefore, stronger creditor rights can be beneficial in some firms (for which the effect of creditor rights on credit costs outweighs

³⁷ Our analyses here are similar to those by Acharya, Amihud, and Litov (2011), who argue and find evidence that risk-reducing but unprofitable investments due to stronger creditor rights should also be reflected by lower profits. Additionally, the authors argue and provide evidence that stronger creditor rights should reduce cash flow risk.

³⁸ For example, Rasmussen (1992), Schwartz (1997), and Hart (2000) suggest that diversity among firms implies that different firms may wish to be subject to different bankruptcy systems.

that on distress costs) or samples (in which such firms are more dominant), but can be costly in other firms or samples.³⁹

3.5.1 Heterogeneity in size, profitability, and maturity

To test our hypothesis, we define firm types for which creditor rights should have a different effect by the firm characteristics size, profitability, and maturity. These characteristics should influence the effect that creditor rights have on credit costs as well as that on distress costs. In particular, for large, profitable, and mature firms, the effect of creditor rights on credit costs may be relatively less important, as better information sharing (e.g., by long-lasting bank relations or more comprehensive disclosure obligations) may serve as a substitute for collateral value (Djankov, McLiesh, and Shleifer, 2007; Haselmann, Pistor, and Vig, 2010; Vig, 2013; Campello and Larrain, 2016; Aretz, Campello, and Marchica, 2020). At the same time, for these firms the effect of creditor rights on distress costs should be relatively more important, as distress costs should be more relevant. Intuitively, size implies higher bankruptcy costs for management and controlling shareholders by higher private control benefits; profitability implies higher inefficient liquidation costs for claimholders; and maturity implies higher bankruptcy costs for claimholders through undiversified human capital. In fact, our bankruptcy data from Panel B of Figure 3.1 above show that while the share of bankrupt firms that used a firm restructuring under ESUG was less than 1% among small firms, it increased monotonically in firm size, and was 31% among large firms, which is consistent with the view that the effect that creditor rights have on distress costs should be relatively more important for some firm types, such as large firms, for which distress costs are more relevant.

To identify our defined firm types, we use the Hadlock and Pierce (2010), Whited and Wu (2006), and Kaplan and Zingales (1997) indices of financial constraints. In doing so, we follow Farre-Mensa and Ljungqvist (2016), who show that prominent measures of financial constraints rather do not identify financially constraint firms, but rather identify special firm characteristics such as size, profitability, and maturity that are typically associated with information problems

³⁹ Note that the structure, formulation, and explanation of our hypothesis follows the structures, formulations, and explanations of the hypotheses by Han (2013) and Ji, Mauer, and Zhang (2020), who respectively explain conflicting theories and mixed empirical evidence on the risk-return relation for housing and on the relation between managerial entrenchment and leverage by variation across certain types. Also note that we borrow the term “weakest, increase monotonically, and strongest” from Giroud and Mueller (2010, 2011), who use this term in the context of competition and corporate governance. Finally note that in describing our findings, we follow the wordings of the bankruptcy literature (Acharya, Amihud, and Litov, 2011; Vig, 2013; Rodano, Serrano-Velarde, and Tarantino, 2016; Favara, 2017; Mann, 2018; Schoenherr and Starmans, 2021), e.g., with regard to “as shown in columns 1 (without controls) and 2 (with controls), the coefficient on $POST_t \times TREATED_i$ is positive and statistically significant. That is, treated firms changed leverage/investments after/around the reform relative to control firms”.

(indeed, the indices are constructed by strongly loading on these firm characteristics, see the description of Table 3.7 below).⁴⁰ Specifically, firms typically classified as constrained present small, unprofitable, and young firms, while firms classified as unconstrained present large, profitable, and mature firms.

In Table 3.7, we examine heterogeneity in the firm type regarding corporate leverage. Following Farre-Mensa and Ljungqvist (2016), for all three indices, we divide our initial sample into three terciles (based on firms' average prereform index value) and define firms in the lowest tercile as large, profitable, and mature firms and firms in the highest tercile as small, unprofitable, and young firms (cutting off firms in the middle 33% of the distribution). Subsequently, among each of the two subsamples of firms, we compare changes in leverage and investments of high asset tangibility firms to those of low asset tangibility firms. The first (baseline specification) and second (with industry fixed effects) columns on each index report the results for the subsample of large, profitable, and mature firms, while the last two columns report the results for the subsample of small, unprofitable, and young firms.

As can be seen, for all three indices, the increase in leverage of high asset tangibility firms relative to low asset tangibility firms is stronger among the subsample of large, profitable and, mature firms (in which the effect of creditor rights on distress costs should outweigh that on credit costs) compared to the subsample of small, unprofitable, and young firms. Indeed, while among small, unprofitable, and young firms high asset tangibility firms did not change leverage statistically significant relative to low asset tangibility firms, the increase in leverage of high asset tangibility firms relative to low asset tangibility firms is strong and significant among large, profitable, and mature firms.⁴¹

⁴⁰ Farre-Mensa and Ljungqvist (2016) find that firms typically classified as constrained do not behave as if they were constrained. They have no problems to increase debt when their demand for debt increases exogenously as a result of a tax increase. In fact, they react just as strongly as typically unconstrained firms. Additionally, the authors apply the test to firms that should be "truly" financially constrained, namely, firms that are close to default. They find that in contrast to low default probability firms who increase leverage to take advantage of tax shields, firms with high default probability are unresponsive to tax increases. Overall, the authors conclude that bankruptcy risk measures seem to do a good job in detecting financial constraint firms, whereas popular measures of financial constraints rather identify special firm characteristics that are typically associated with information problems. (Note that the findings of Farre-Mensa and Ljungqvist (2016) also mitigate possible concerns that the indices might proxy for default risk, by which we would not estimate heterogeneity in the defined firm types, but, as in Section 3.6 below, heterogeneity in the effect of the reform in risk of default. In fact, as we will show in this section, this concern is also empirical inconsistent since the observed effects are not driven by the typically as constrained classified firms, but by the typically as unconstrained classified firms.)

⁴¹ While our finding that the effect of creditor rights varies across firm types is sufficient to confirm the hypothesis that the effect of creditor rights should depend on the firm type, one might ask why we do not find a negative effect of the reform, e.g., a reduction in leverage among small, unprofitable, and young firms. Intuitively, there are at least two reasons for this. First, our sample simply may not include sufficiently small, unprofitable, and young firms in which the effect of creditor rights on credit costs outweighs that on distress costs (recall that we also exclude sole proprietorships and micro sized firms). Second, besides firm characteristics, also country characteristics may influence the effect of creditor rights on credit costs and distress costs. For example, mitigated information problems through better country-level accounting standards or other information sharing systems

In Table 3.8, we study cross-sectional heterogeneity on investments (CAPEX-to-Sales ratio) and profitability. As shown, the positive changes in investments and profitability (i.e., the reduction in high recovery assets and the increase in profitability) of high asset tangibility firms relative to low asset tangibility firms are stronger among large, profitable, and mature firms compared to those among small, unprofitable, and young firms.⁴²

3.5.1.1 Additional analysis: heterogeneity in firm size alone

In additional analysis, we also define firm types for which creditor rights should have a different effect by firm size alone. Firm size not only seems to be theoretically and, based on our bankruptcy data, empirically an effective solution to define firm types, but moreover seems to be a particularly practicable solution, which may offer a helpful guide to policymakers in selecting an appropriate bankruptcy system if a menu method is not feasible (detailed below).

To provide evidence, as in Panel A of Figure 3.2, in Panels B and C of Figure 3.2, we plot the de-meaned time series of corporate leverage for firms with high and low levels of asset tangibility. Different than in Panel A, which is based on the full sample, in Panel B, we consider a subsample of firms with sales larger than 10 million euros (specifically, we first drop firms with sales less than 10 million euros (based on firms' average prereform sales), and then divide firms into high and low asset tangibility firms), while in Panel C, we consider a subsample of firms with sales larger than 100 million euros. As visible, the increase in leverage of high tangibility firms relative to low tangibility firms after the reform increases from Panel A to Panel B, as well as from Panel B to Panel C. In other words, the positive effect on leverage increases in firm size.

In Panel D of Figure 3.2, we support this visual impression statistically by reporting the difference-in-differences coefficients of regressions (with controls and industry fixed effects) employed on different subsamples of firms with different firm sizes. More specifically, the coefficient of the first category is from a subsample of firms with sales less than 10 million euros (i.e., we first drop firms with sales more than 10 million euros, and then divide firms into high and low asset tangibility firms), the coefficient of the second category is from a subsample

might serve as a substitute for collateral value (Djankov, McLiesh, and Shleifer, 2007), such that reductions in creditor rights have only weak effects on credit costs. See Dyck and Zingales (2004) and Djankov, McLiesh, and Shleifer (2007) who show that Germany has relatively comprehensive accounting standards and other information sharing systems.

⁴² Although stronger than the effect among constrained firms, the positive effect among unconstrained firms is statistically insignificant when identifying the firm types with the Kaplan and Zingales (1997) index. This could be because, different than the Whited and Wu (2006) and Hadlock and Pierce (2010) indices, the Kaplan and Zingales measure does not consider firm size—a characteristic that seems to be particularly powerful in influencing the effect of creditor rights on credit costs and distress costs—and therefore might be less effective in identifying firm types for which creditor rights have a different effect.

of firms with sales more than 10 million euros, the coefficient of the third category is from a subsample of firms with sales more than 30 million euros, and so on. Strikingly, and closely reflecting our bankruptcy data from Panel B of Figure 3.1 above, the panel shows that while the increase in corporate leverage of high asset tangibility firms relative to low asset tangibility firms is not present among small firms, it increases monotonically in firm size, and is strongest among large firms.

3.5.2 Implications for bankruptcy literature

Taken together, the results provide evidence consistent with our hypothesis that the effect of creditor rights on corporate leverage and investments depends on the firm type, as it influences the effect that creditor rights have on credit costs and distress costs and thus which of them dominates. We believe that our understandings here shed new light on two critical issues in the bankruptcy literature. First, our findings help to reconcile the mixed empirical evidence of existing studies on the effect of creditor rights on firms' financial and investment policy. Specifically, stronger creditor rights can be beneficial in some firms (in which the effect of creditor rights on credit costs outweighs that on distress costs) or samples/countries (in which these firms are more important), but can impose a cost in other firms or samples/countries. Markedly, the suggestion that the effect of creditor rights should depend on the firm size, or, more specifically, should be more beneficial for smaller firms compared to larger firms, is closely consistent with the mixed evidence of the extant studies that typically find a positive effect of creditor rights in SMEs but a negative effect in listed or large firms.⁴³ Table A.3.1 in the Appendix presents an overview of these studies, together with the average firm size in the study and the found effect.

And second, our findings have important implications for optimal bankruptcy design. In particular, our findings are inconsistent with a widely accepted opinion that an optimal bankruptcy law should be uniform and balance the effect of creditor rights on credit costs and distress costs (e.g., Acharya, Amihud, and Litov, 2011). Instead, they point to a menu of procedures in which the two extremes of a particularly debtor-friendly and particularly creditor-friendly procedure co-exist and thus allow different types of firms to utilize the prevailing overweight to the full extent.⁴⁴ To be more specific, when credit costs outweigh distress costs (distress costs outweigh credit costs), an optimal bankruptcy law should increase (decrease) creditor

⁴³ Note that also the average effect of our study is consistent with this, given that the average firm in our study is considerably large (the average prereform size of high asset tangibility firms is about 138 million euros, see Table 3.2).

⁴⁴ On a menu approach, see Rasmussen (1992), Schwartz (1997), and Hart (2000).

rights to the maximum (minimum) level, thereby allowing firms to fully utilize this overweight. Importantly, our argument that for each firm type one of the two effects that creditor rights have should outweigh gives rise to a practically feasible yet effective solution of a menu that consists only of two sets of procedures: a particularly creditor-friendly procedure that allows, e.g., SMEs to utilize the overweight of credit costs over distress costs, and a particularly debtor-friendly procedure that allows, e.g., large firms to utilize the overweight of distress costs over credit costs.

3.6 Remeasuring the effect by variation in default risk

The causal interpretation of our results depends on the validity of the assumption that high and low asset tangibility firms would have followed the same trend in the absence of the reform. That is, that both groups of firms react to shocks other than changes in the bankruptcy law in the same way. Although we give credibility to this assumption by documenting that high and low asset tangibility firms exhibited parallel trends in the absence of changes in the bankruptcy law, it is still possible that confounding shocks coincided with the introduction of the ESUG Act and affected firms with high asset tangibility differently than firms with low asset tangibility. In this section, we mitigate the concern of possible confounding events by redoing our empirical analysis, but now by comparing changes in leverage and investments of firms with high default probability to those of firms with low default probability. In doing so, we use that firms with high default probability should be more affected by the design of the bankruptcy law (Rodano, Serrano-Velarde, and Tarantino, 2016; Favara et al., 2017; Gross et al., 2021; Schoenherr and Starmans, 2021). The idea of our analysis is that if we obtain results that are consistent with those of our main analysis, our findings should not be affected by confounding shocks.

We measure default probability by the interest coverage ratio (Schoenherr and Starmans, 2021), which is defined as the ratio of EBIT to interest payments. As in the main analysis, we divide our initial sample into three terciles, but now based on firms' average prereform (2009 to 2011) measure of default probability. Firms in the lowest tercile (high probability of default) are our treated group, whereas firms in the highest tercile (low probability of default) are our control group (neglecting firms that are in the middle 33% of the bankruptcy probability distribution). Table A.3.2 in the Appendix presents summary statistics for the main variables, separately for firms with high and low probability of default.⁴⁵

⁴⁵ Most importantly, although there is still some difference in asset tangibility between risky and safe firms (asset tangibility for risky firms is about 50% whereas it is 38% for safe firms), the difference is considerably

In Table 3.9, we present the average effect on corporate financial and investment policy obtained by employing our difference-in-differences analysis (augmented by the industry dummies) on firms with high and low probability of default. As can be seen, relative to firms with low probability of default, after the reform firms with high probability of default increased corporate leverage (columns 1 and 2), using both secured and unsecured debt (column 3). Moreover, after the reform risky firms relatively discarded investments that reduce the risk of default but are typically less profitable (columns 4 to 6), which is related to subsequently higher levels of profitability (column 7) and risk (column 8).

In Panel A of Figure 3.4, we present a graphical analysis to one of our main contributions in this paper, which is to provide credible evidence that stronger creditor rights can impose a cost on firms by prompting them to undertake investments that reduce the risk of default but are however unprofitable (Acharya, Amihud, and Litov, 2011). As visible, firms with high and low probability of default exhibited parallel trends on investments (CAPEX-to-Sales ratio) in the absence of changes in the bankruptcy law, suggesting that this would have also been the case in the absence of the ESUG Act. Finally, in Panel B of Figure 3.4, we report the differential effects on leverage for subsamples of firms with different firm sizes. As shown, the effects vary considerably across groups of firms (specifically, the increase in leverage of risky firms relative to safe firms is stronger in subsamples of larger firms), which is consistent with our maintained hypothesis that the effect of creditor rights should depend on the firm type. Altogether, the results here are consistent with those of our main analysis, suggesting that our findings should not be affected by confounding shocks. Put differently, to affect our findings, confounding events must not only have had a different effect on high and low asset tangibility firms, but also on high and low default probability firms.

Before proceeding, it is worth noting that when measuring the effect of the reform by comparing high and low asset tangibility firms, we only consider reductions in distress costs related to the use of secured debt. That is, we only measure reductions in distress costs to shareholders and management coming from reduced inefficient liquidation costs when secured debt is used, but not reductions in distress costs for management during bankruptcy (in other words, prior to

smaller than that between high and low asset tangibility firms (74% and 17%). This, in combination with the stronger economic effect obtained when measuring the effect of the reform by comparing risky and safe firms (see below), ensures that the measured effect here is not or at least not entirely driven by the same possible problem as when measuring the effect by comparing high and low asset tangibility firms, i.e., that a confounding shock affected firms with high and low asset tangibility differently (if this were the case, we should actually see a lower economic effect here by a lower difference in asset tangibility). Additionally, in unreported results, we estimate the effect of the reform by comparing risky and safe firms that have about the same levels of asset tangibility. Our results remain virtually unchanged, suggesting that the results presented in this section are not driven by possible confounding shocks that affected firms with different levels of asset tangibility differently.

ESUG, while firms with low asset tangibility may have had no inefficient liquidation costs because they had no secured debt, management was still dismissed during bankruptcy). In contrast, when measuring the effect by comparing risky and safe firms, we also consider reductions in distress costs that management experiences during bankruptcy. Consistent with this, the economic effects are in general stronger when comparing risky and safe firms relative to those when comparing high and low asset tangibility firms (e.g., while here the economic effects for leverage, CAPEX-to-Sales, Log(Assets), and profitability respectively are 2.5, 7.0, 14.2%, and 4.7, in our main analysis the economic effects respectively are 1.4, 10.6, 10.9%, and 0.9), further strengthening the causal interpretation of our findings.

Overall, given the parallel trends between high and low asset tangibility firms as a reaction to shocks other than changes in the bankruptcy law, the heterogeneity across groups of firms (which additionally strengthens the causal interpretation of our results), and the results from this section, it appears unlikely that confounding events affect our conclusions.

3.7 Conclusions

Stronger creditor rights can reduce credit costs and thus may allow firms to increase leverage and investments, but also increase distress costs and thus may induce firms to reduce the risk of default (Acharya, Amihud, and Litov, 2011; Vig, 2013; Ponticelli and Alencar, 2016; Mann, 2018; Schoenherr and Starman, 2021). Using a German bankruptcy law reform that reduced creditor rights, we find that on average firms increased corporate leverage after the reform. Moreover, firms discarded investments that can reduce bankruptcy risk but are typically unprofitable, and additionally exhibited higher levels of risk and profitability. Our results, therefore, provide evidence consistent with the notion that stronger creditor rights impose a cost on firms by increasing distress costs and thus prompting firms to undertake risk-reducing but unprofitable financial and investment decisions. Put differently, our findings suggest that, on average, the effect of creditor rights on distress costs outweighs that on credit costs.

After reporting the average effect, we try to understand the mixed empirical evidence of existing studies on the effect of creditor rights on corporate financial and investment policy. We hypothesize and provide evidence that the effect of creditor rights depends on the firm type—e.g., on firm characteristics such as size, profitability, and age—, as it influences the effect that creditor rights have on credit costs and distress costs and thus which of them dominates. Hence, stronger creditor rights can be beneficial in some firms or samples/countries, but can impose a cost in other firms or samples/countries. Notably, the suggestion that the effect of creditor rights should depend on firm size, or, more specifically, that stronger creditor rights

may be beneficial for small firms but costly for large firms, is closely consistent with the mixed evidence of extant studies that, in general, report a positive effect of creditor rights in SMEs while documenting a negative effect in listed or large firms.

Our understanding that the effect of creditor rights depends on the firm type also has important implications for optimal bankruptcy design. In particular, it is contrary to a widely accepted opinion that an optimal bankruptcy law should be uniform and balance the effect of creditor rights on credit costs and distress costs (e.g., Acharya, Amihud, and Litov, 2011). Instead, it points to a menu of procedures in which the two extremes of a debtor-friendly and creditor-friendly procedure co-exist and thus allow different types of firms to choose the procedure that suits them best.⁴⁶ Importantly, because for each firm type one of the two effects that creditor rights have should outweigh, one arrives at a practically feasible yet effective solution of a menu that consists only of two sets of procedures; a particularly creditor-friendly procedure that allows, e.g., SMEs to utilize the prevailing dominance of credit costs over distress costs, and a particularly debtor-friendly procedure that allows, e.g., large firms to utilize the dominance of distress costs over credit costs. If such a menu is not possible, our results suggest that countries should choose one of the two extreme cases, depending on an important firm type in the country (on this, see Schoenherr and Starman, 2021). Here, our way of using profitability, maturity, and, especially, size not only seems to provide an effective, but also a particularly practical solution to define firm types for which creditor rights should have a different effect, thereby possibly offering a helpful guide to policymakers in choosing an appropriate bankruptcy law.

⁴⁶ Consistent with this, Hart (2000, p. 1) notes: “One point I will stress is that it is unlikely that “one size fits all.” (. . .) One can also imagine a country choosing a menu of procedures and allowing firms to select among them.” For the menu approach, see also Rasmussen (1992) and Schwartz (1997).

Tables and figures for Chapter 3

Table 3.1
Industries

This table provides an industry distribution of our sample. We generated 13 industries (aimed at ensuring that firms within one industry experience the same industry shock) to which we assigned firms based on the 4-digit SIC code.

Industries	Number of firms
Tobacco	32
Construction	670
Food and Beverages	273
Textiles, Apparel, and Leather	95
Furniture, Fixtures, and other Housing Supply	41
Paper (including Printing and Publishing)	100
Other Consumer Goods (such as Watches, Jewelry, Toys, etc.)	49
Chemicals, Pharmaceuticals, and Health	681
Automobiles and Ancillaries	185
Other Transportation Products	150
Service	712
Industrial Electronics	111
Household Electronics and Communication	53
Total	3,152

Table 3.2**Summary statistics and preliminary results on the average effect**

Columns 1 and 4 present prereform summary statistics for the main variables, separately for firms with high and low asset tangibility. As in Vig (2013), we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%) based on firms' average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), with firms in the highest tercile defined as high asset tangibility firms (our treatment group) and firms in the lowest tercile defined as low asset tangibility firms (our control group) (cutting off firms that are in the middle 33% of the asset tangibility distribution). Columns 2 and 5 respectively present before-after reform differences for high and low asset tangibility firms. Column 7 reports the difference-in-differences coefficient δ for the regression $y_{it} = \beta + \eta POST_t + \gamma TREATED_i + \delta(POST_t \times TREATED_i) + \varepsilon_{it}$. Here, i indexes firms, and t indexes time; y_{it} is the dependent variable of interest; $POST_t$ is a dummy variable that equals one for the years after the ESUG Act was passed (2012 to 2015), and zero for the years before the bankruptcy reform was passed (2009 to 2011); $TREATED_i$ is an indicator variable that equals one if the firm belongs to the treated group, and zero if it belongs to the control group; and ε_{it} is the firm specific error term. Standard errors are reported in parentheses. ***, **, and * indicate significance at the 1% level, 5% level, and 10% level, respectively.

Variables	High asset tangibility			Low asset tangibility			Difference-in-differences	
	Before 1	Difference 2	Observations 3	Before 4	Difference 5	Observations 6	DID 7	Observations 8
Debt/(Debt+Equity)	0.653*** (0.004)	-0.024*** (0.002)	15,524	0.715*** (0.004)	-0.032*** (0.003)	14,712	0.007** (0.004)	30,236
Debt/Assets	0.583*** (0.005)	-0.021*** (0.002)	15,534	0.713*** (0.004)	-0.033*** (0.003)	14,741	0.012*** (0.004)	30,275
Secured Bank Debt/Debt	0.478*** (0.024)	-0.029 (0.024)	11,699	0.263*** (0.005)	0.005 (0.004)	10,148	-0.033 (0.024)	21,847
CAPEX/Sales	0.172*** (0.018)	-0.059*** (0.020)	14,445	0.018*** (0.003)	0.020* (0.011)	13,361	-0.079*** (0.023)	27,806
Fixed Assets/Assets	0.739*** (0.002)	-0.013*** (0.002)	15,872	0.174*** (0.002)	0.029*** (0.002)	15,312	-0.042*** (0.003)	31,184
Log(Assets)	10.378*** (0.028)	0.172*** (0.010)	15,881	9.479*** (0.026)	0.336*** (0.012)	15,335	-0.164*** (0.016)	31,216
EBITDA/Assets	0.112*** (0.002)	0.001 (0.001)	15,482	0.118*** (0.003)	-0.009*** (0.002)	14,888	0.010*** (0.003)	30,370
SD(EBITDA/Assets)	0.029*** (0.001)	0.001 (0.001)	4,671	0.054*** (0.001)	-0.008*** (0.002)	4,648	0.009*** (0.002)	9,319
Sales (in MM €)	137.478*** (16.904)	19.173 (22.313)	14,764	99.773*** (5.305)	14.580** (7.050)	13,701	4.592 (24.114)	28,465

Table 3.3

Average effect on corporate leverage and debt composition

This table reports results for the regression $y_{it} = \alpha_i + \lambda_t + \eta POST_t + \gamma TREATED_i + \delta(POST_t \times TREATED_i) + \beta X_{it} + \varepsilon_{it}$. The dependent variable is the Debt-to-(Debt+Equity) ratio in columns 1 to 3, the Debt-to-Assets ratio in columns 4 to 6, and the Secured-Bank-Debt-to-Debt ratio in columns 7 to 9. Here, i indexes firms, and t indexes time; y_{it} is the dependent variable of interest; α_i are firm fixed effects; λ_t are year fixed effects; and $POST_t$ is a dummy variable that equals one for the years after the ESUG Act was passed (2012 to 2015), and zero for the years before the bankruptcy reform was passed (2009 to 2011). $TREATED_i$ is an indicator variable that equals one if the firm belongs to the treated group, and zero if it belongs to the control group. As in Vig (2013), we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%) based on firms' average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), with firms in the highest tercile forming our treated group and firms in the lowest tercile forming our control group (cutting off firms that are in the middle 33% of the asset tangibility distribution). X_{it} are the time-varying covariates $EBIT/Assets$ and $\text{Log}(Sales)$; and ε_{it} is the firm specific error term. The coefficient of interest is δ , which measures the difference-in-differences effect. The last column of each dependent variable expands the baseline specification above with the interaction term $\omega_j \times \lambda_t$, where j indexes industries, and ω_j are industry fixed effects. Standard errors are reported in parentheses and are clustered at the firm level. ***, **, and * indicate significance at the 1% level, 5% level, and 10% level, respectively.

Variables	Debt/(Debt+Equity)			Debt/Assets			Secured Bank Debt/Debt		
	1	2	3	4	5	6	7	8	9
POST x TREATED	0.010*** (0.003)	0.012*** (0.003)	0.014*** (0.005)	0.016*** (0.003)	0.019*** (0.003)	0.019*** (0.005)	-0.045 (0.029)	-0.049 (0.032)	-0.108 (0.084)
EBIT/Assets		-0.204*** (0.021)	-0.217*** (0.025)		-0.198*** (0.020)	-0.209*** (0.024)		-0.118*** (0.023)	-0.131*** (0.047)
Log(Sales)		0.024*** (0.005)	0.026*** (0.007)		0.023*** (0.005)	0.025*** (0.007)		-0.017* (0.009)	-0.018 (0.021)
Number of firms	4,985	4,985	3,152	4,985	4,985	3,152	4,985	4,985	3,152
Observations	30,236	27,898	17,725	30,275	27,926	17,742	21,847	20,688	13,178
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x year	No	No	Yes	No	No	Yes	No	No	Yes
Within R-sq.	0.039	0.081	0.087	0.033	0.073	0.083	0.001	0.001	0.010

Table 3.4

Placebo tests around the introduction of the ESUG Act

This table reports the difference-in-differences coefficient δ for the regression $y_{it} = \alpha_i + \lambda_t + \eta POST_t + \gamma TREATED_i + \delta(POST_t \times TREATED_i) + \beta X_{it} + \varepsilon_{it}$. The dependent variables in the rows are the Debt-to-(Debt+Equity) ratio, the Debt-to-Assets ratio, the Secured-Bank-Debt-to-Debt ratio, the CAPEX-to-Sales ratio, and the EBITDA-to-Assets ratio. Here, i indexes firms, and t indexes time; y_{it} is the dependent variable of interest; α_i are firm fixed effects; and λ_t are year fixed effects. $POST_t$ is a dummy variable that, in columns 1 to 2, takes on a value of one for the year 2010 and zero for the year 2009, in columns 3 to 4, takes on a value of one for the year 2011 and zero for the year 2010, in columns 5 to 6, takes on a value of one for the year 2012 and zero for the year 2011, in columns 7 to 8, takes on a value of one for the year 2013 and zero for the year 2012, in columns 9 to 10, takes on a value of one for the year 2014 and zero for the year 2013, and, in columns 11 to 12, takes on a value of one for the year 2015 and zero for the year 2014. $TREATED_i$ is an indicator variable that equals one if the firm belongs to the treated group, and zero if it belongs to the control group. As in Vig (2013), we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%) based on firms' average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), with firms in the highest tercile forming our treated group and firms in the lowest tercile forming our control group (cutting off firms that are in the middle 33% of the asset tangibility distribution). X_{it} are the time-varying covariates $EBIT/Assets$ and $\log(Sales)$; and ε_{it} is the firm specific error term. The coefficient of interest is δ , which measures the difference-in-differences effect. The last column of each of the time periods expands the baseline specification above with the interaction term $\omega_j \times \lambda_t$, where j indexes industries, and ω_j are industry fixed effects. Standard errors are reported in parentheses and are clustered at the firm level. ***, **, and * indicate significance at the 1% level, 5% level, and 10% level, respectively.

Variables	2009 to 2010		2010 to 2011		2011 to 2012		2012 to 2013		2011 & 2013		2013 to 2014		2014 to 2015	
	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Debt/(Debt+Equity)	0.000 (0.002)	0.004 (0.004)	0.001 (0.002)	0.003 (0.003)	0.007*** (0.002)	0.005* (0.003)	0.007*** (0.002)	0.010*** (0.003)	0.013*** (0.003)	0.013*** (0.004)	-0.001 (0.002)	0.000 (0.003)	0.001 (0.002)	0.001 (0.004)
Debt/Assets	0.003 (0.002)	0.007* (0.003)	0.002 (0.002)	0.004 (0.003)	0.008*** (0.002)	0.006** (0.003)	0.009*** (0.002)	0.012*** (0.003)	0.016*** (0.003)	0.016*** (0.004)	0.001 (0.002)	0.002 (0.003)	0.004 (0.002)	0.004 (0.004)
Secured BD/Debt	0.007 (0.005)	0.005 (0.007)	0.009 (0.090)	-0.146 (0.228)	-0.068 (0.066)	-0.072 (0.080)	-0.011** (0.006)	-0.014 (0.009)	-0.080 (0.075)	-0.077 (0.083)	-0.004 (0.006)	0.005 (0.010)	0.001 (0.005)	0.004 (0.006)
CAPEX/Sales	-0.061 (0.039)	0.020 (0.038)	-0.060 (0.041)	-0.047 (0.039)	-0.052*** (0.020)	-0.051*** (0.016)	-0.058** (0.025)	-0.083** (0.040)	-0.076*** (0.020)	-0.097*** (0.024)	-0.027 (0.024)	-0.028 (0.061)	0.000 (0.019)	0.021 (0.048)
EBITDA/Assets	-0.006* (0.003)	-0.008 (0.006)	0.003 (0.003)	0.004 (0.005)	0.010*** (0.003)	0.004 (0.004)	0.004 (0.002)	0.001 (0.004)	0.013*** (0.003)	0.006 (0.005)	0.003 (0.002)	0.007 (0.004)	0.003 (0.003)	0.005 (0.004)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x year	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Table 3.5

Average effect on corporate investments

This table reports results for the regression $y_{it} = \alpha_i + \lambda_t + \eta POST_t + \gamma TREATED_i + \delta(POST_t \times TREATED_i) + \beta X_{it} + \varepsilon_{it}$. The dependent variable is the CAPEX-to-Sales ratio in columns 1 to 3, the Fixed-Assets-to-Assets ratio in columns 4 to 6, and the Log of Assets in columns 7 to 9. Here, i indexes firms, and t indexes time; y_{it} is the dependent variable of interest; α_i are firm fixed effects; λ_t are year fixed effects; and $POST_t$ is a dummy variable that equals one for the years after the ESUG Act was passed (2012 to 2015), and zero for the years before the bankruptcy reform was passed (2009 to 2011). $TREATED_i$ is an indicator variable that equals one if the firm belongs to the treated group, and zero if it belongs to the control group. As in Vig (2013), we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%) based on firms' average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), with firms in the highest tercile forming our treated group and firms in the lowest tercile forming our control group (cutting off firms that are in the middle 33% of the asset tangibility distribution). X_{it} are the time-varying covariates $EBIT/Assets$ and $Log(Sales)$; and ε_{it} is the firm specific error term. The coefficient of interest is δ , which measures the difference-in-differences effect. The last column of each dependent variable expands the baseline specification above with the interaction term $\omega_j \times \lambda_t$, where j indexes industries, and ω_j are industry fixed effects. Standard errors are reported in parentheses and are clustered at the firm level. ***, **, and * indicate significance at the 1% level, 5% level, and 10% level, respectively.

Variables	CAPEX/Sales			Fixed Assets/Assets			Log(Assets)		
	1	2	3	4	5	6	7	8	9
POST x TREATED	-0.094*** (0.022)	-0.103*** (0.022)	-0.106*** (0.038)	-0.044*** (0.003)	-0.039*** (0.003)	-0.049*** (0.004)	-0.141*** (0.011)	-0.107*** (0.007)	-0.109*** (0.011)
EBIT/Assets		0.018 (0.314)	-0.206 (0.547)		-0.073*** (0.018)	-0.054** (0.025)		-0.298*** (0.065)	-0.235*** (0.064)
Log(Sales)		-0.499*** (0.166)	-0.304* (0.159)		-0.016*** (0.006)	-0.016** (0.007)		0.404*** (0.029)	0.370*** (0.030)
Number of firms	4,985	4,985	3,152	4,985	4,985	3,152	4,985	4,985	3,152
Observations	27,806	27,806	17,621	31,184	28,446	18,034	31,216	28,465	18,042
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x year	No	No	Yes	No	No	Yes	No	No	Yes
Within R-sq.	0.002	0.029	0.016	0.031	0.040	0.043	0.137	0.327	0.289

Table 3.6

Average effect on profitability and risk

This table reports results for the regression $y_{it} = \alpha_i + \lambda_t + \eta POST_t + \gamma TREATED_i + \delta(POST_t \times TREATED_i) + \beta X_{it} + \varepsilon_{it}$. The dependent variable is the EBITDA-to-Assets ratio in columns 1 to 3, and the standard deviation of the EBITDA-to-Assets ratio (calculated separately for the prereform and postreform periods) in columns 4 to 6. Here, i indexes firms, and t indexes time; y_{it} is the dependent variable of interest; α_i are firm fixed effects; λ_t are year fixed effects; and $POST_t$ is a dummy variable that equals one for the years after the ESUG Act was passed (2012 to 2015), and zero for the years before the bankruptcy reform was passed (2009 to 2011). $TREATED_i$ is an indicator variable that equals one if the firm belongs to the treated group, and zero if it belongs to the control group. As in Vig (2013), we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%) based on firms' average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), with firms in the highest tercile forming our treated group and firms in the lowest tercile forming our control group (cutting off firms that are in the middle 33% of the asset tangibility distribution). X_{it} is the time-varying covariate Log (*Sales*) in columns 1 to 3, and the means of *EBIT/Assets* and Log (*Sales*) (calculated separately for the prereform and postreform periods) in columns 4 to 6; and ε_{it} is the firm specific error term. The coefficient of interest is δ , which measures the difference-in-differences effect. The last column of each dependent variable expands the baseline specification above with the interaction term $\omega_j \times \lambda_t$, where j indexes industries, and ω_j are industry fixed effects. Standard errors are reported in parentheses and are clustered at the firm level. ***, **, and * indicate significance at the 1% level, 5% level, and 10% level, respectively.

Variables	EBITDA/Assets			SD(EBITDA/Assets)		
	1	2	3	4	5	6
POST x TREATED	0.012*** (0.002)	0.014*** (0.002)	0.009*** (0.003)	0.009*** (0.002)	0.009*** (0.002)	0.008*** (0.003)
Log(Sales)		0.061*** (0.006)	0.061*** (0.007)			
Mean(EBIT/Assets)					0.058 (0.054)	0.081 (0.062)
Mean(Log(Sales))					0.001 (0.009)	0.004 (0.012)
Number of firms	4,985	4,985	3,152	4,980	4,980	3,149
Observations	30,370	28,375	17,991	9,319	9,049	5,725
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry x year	No	No	Yes	No	No	Yes
Within R-sq.	0.005	0.050	0.063	0.008	0.014	0.024

Table 3.7

Heterogeneity in the firm type on corporate leverage

This table reports results for the regression $y_{it} = \alpha_i + \lambda_t + \eta POST_t + \gamma TREATED_i + \delta(POST_t \times TREATED_i) + \beta X_{it} + \varepsilon_{it}$, employed separately on subsamples of large, profitable, and mature firms and small, unprofitable, and young firms. The dependent variable is the Debt-to-(Debt+Equity) ratio. Here, i indexes firms, and t indexes time; y_{it} is the dependent variable of interest; α_i are firm fixed effects; λ_t are year fixed effects; and $POST_t$ is a dummy variable that equals one for the years after the ESUG Act was passed (2012 to 2015), and zero for the years before the bankruptcy reform was passed (2009 to 2011). $TREATED_i$ is an indicator variable that equals one if the firm belongs to the treated group, and zero if it belongs to the control group. As in Vig (2013), we divide firms (within each of the subsamples) into three terciles (top 33%, middle 33%, and bottom 33%) based on their average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), with firms in the highest tercile forming our treated group and firms in the lowest tercile forming our control group (cutting off firms that are in the middle 33% of the asset tangibility distribution). X_{it} are the time-varying covariates $EBIT/Assets$ and $\text{Log}(Sales)$; and ε_{it} is the firm specific error term. The coefficient of interest is δ , which measures the difference-in-differences effect. The last column of each subsample expands the baseline specification above with the interaction term $\omega_j \times \lambda_t$, where j indexes industries, and ω_j are industry fixed effects. We identify our subsamples of firms using the Hadlock and Pierce (2010), Whited and Wu (2006), and Kaplan and Zingales (1997) indices of financial constraints. Following Farre-Mensa and Ljungqvist (2016), for all three indices, we divide our initial sample into three terciles (based on firms' average prereform index value) and define firms in the lowest tercile as large, profitable, and mature firms and firms in the highest tercile as small, unprofitable, and young firms (cutting off firms that are in the middle 33% of the distribution). The Kaplan and Zingales Index is originally constructed as (the following variables are Compustat data items) $-1.001909[(ib + dp)/lagged ppent] + 0.2826389[(at + pcc_f \times csho - ceq - txdb)/at] + 3.139193[(dltt + dlc)/(dltt + dlc + seq)] - 39.3678[(dvc + dvp)/lagged ppent] - 1.314759[che/lagged ppent]$; and, due to data limitations, we approximate the Index by $-1.001909[EBITDA/lagged fixed assets] + 3.139193[(long-term debt + short-term debt)/(long-term debt + short-term debt + equity)] - 1.314759[cash and cash equivalents/lagged fixed assets]$. The Whited and Wu Index is originally constructed as $-0.091[(ib + dp)/at] - 0.062[\text{indicator set to one if } dvc + dvp \text{ is positive, and zero otherwise}] + 0.021[dltt/at] - 0.044[\log(at)] + 0.102[\text{average industry sales growth, estimated separately for each three-digit SIC industry and each year, with sales growth defined as } (sales_{it}/sales_{it-1}) - 1] - 0.035[\text{sales growth, defined as } (sales_{it}/sales_{it-1}) - 1]$; and we approximate the Index by $-0.091[(EBITDA)/assets] + 0.021[\text{long-term debt}/assets] - 0.044[\log(assets)] + 0.102[\text{average industry sales growth, estimated separately for each four-digit SIC industry and each year, with sales growth defined as } (sales_{it}/sales_{it-1}) - 1] - 0.035[\text{sales growth, defined as } (sales_{it}/sales_{it-1}) - 1]$. The Hadlock and Pierce Index is originally constructed as $-0.737size + 0.043size^2 - 0.040age$, where size equals the log of Compustat item at and age is the number of years the firm is listed; and we approximate the Index by $-1Assets$. Standard errors are reported in parentheses and are clustered at the firm level. ***, **, and * indicate significance at the 1% level, 5% level, and 10% level, respectively.

Variables	Dependent variable: Debt/(Debt+Equity)											
	Hadlock-Pierce				Whited-Wu				Kaplan-Zingales			
	Large, profitable, and mature		Small, unprofitable, and young		Large, profitable, and mature		Small, unprofitable, and young		Large, profitable, and mature		Small, unprofitable, and young	
	1	2	3	4	5	6	7	8	9	10	11	12
POST x TREATED	0.022*** (0.005)	0.019** (0.008)	-0.003 (0.006)	0.002 (0.007)	0.030*** (0.007)	0.030*** (0.009)	0.002 (0.007)	-0.002 (0.008)	0.028*** (0.005)	0.041*** (0.008)	0.003 (0.005)	0.005 (0.007)
EBIT/Assets	-0.198*** (0.047)	-0.190*** (0.061)	-0.224*** (0.026)	-0.222*** (0.028)	-0.271*** (0.043)	-0.274*** (0.045)	-0.140*** (0.041)	-0.138*** (0.041)	-0.212*** (0.029)	-0.236*** (0.038)	-0.244*** (0.043)	-0.218*** (0.052)
Log(Sales)	0.026** (0.012)	0.048*** (0.015)	0.027*** (0.007)	0.026*** (0.009)	0.028*** (0.011)	0.027** (0.011)	0.044*** (0.015)	0.044*** (0.015)	0.047*** (0.008)	0.050*** (0.010)	0.004 (0.006)	0.010 (0.008)
Number of firms	1,324	886	1,735	1,102	899	899	850	850	1,613	1,058	1,513	989
Observations	8,320	5,541	8,597	5,456	5,754	5,754	4,858	4,858	9,244	6,063	8,314	5,492
Firm & year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x year	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Table 3.8

Heterogeneity in the firm type on corporate investments and profitability

This table reports results for the regression $y_{ijt} = \alpha_i + \lambda_t + \eta POST_t + \gamma TREATED_i + \delta(POST_t \times TREATED_i) + \beta X_{ijt} + \omega_j \times \lambda_t + \varepsilon_{ijt}$, employed separately on subsamples of large, profitable, and mature firms and small, unprofitable, and young firms. The dependent variable is the CAPEX-to-Sales ratio in columns 1, 3, 5, 7, 9, and 11, and the EBITDA-to-Assets ratio in the other columns. Here, i indexes firms, t indexes time, and j indexes industries; y_{ijt} is the dependent variable of interest; α_i are firm fixed effects; λ_t are year fixed effects; and $POST_t$ is a dummy variable that equals one for the years after the ESUG Act was passed (2012 to 2015), and zero for the years before the bankruptcy reform was passed (2009 to 2011). $TREATED_i$ is an indicator variable that equals one if the firm belongs to the treated group, and zero if it belongs to the control group. As in Vig (2013), we divide firms (within each of the subsamples) into three terciles (top 33%, middle 33%, and bottom 33%) based on their average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), with firms in the highest tercile forming our treated group and firms in the lowest tercile forming our control group (cutting off firms that are in the middle 33% of the asset tangibility distribution). X_{ijt} are the time-varying covariates $EBIT/Assets$ and $\log(Sales)$; ω_j are industry fixed effects; and ε_{ijt} is the firm specific error term. The coefficient of interest is δ , which measures the difference-in-differences effect. We identify our subsamples of firms using the Hadlock and Pierce (2010), Whited and Wu (2006), and Kaplan and Zingales (1997) indices of financial constraints. Following Farre-Mensa and Ljungqvist (2016), for all three indices, we divide our initial sample into three terciles (based on firms' average prereform index value) and define firms in the lowest tercile as large, profitable, and mature firms and firms in the highest tercile as small, unprofitable, and young firms (cutting off firms that are in the middle 33% of the distribution). The Kaplan and Zingales Index is originally constructed as (the following variables are Compustat data items) $-1.001909[(ib + dp)/lagged pment] + 0.2826389[(at + prcc_f \times csho - ceq - txdb)/at] + 3.139193[(dltt + dlc)/(dltt + dlc + seq)] - 39.3678[(dvc + dvp)/lagged pment] - 1.314759[che/lagged pment]$; and, due to data limitations, we approximate the Index by $-1.001909[EBITDA/lagged fixed assets] + 3.139193[(long-term debt + short-term debt)/(long-term debt + short-term debt + equity)] - 1.314759[cash and cash equivalents/lagged fixed assets]$. The Whited and Wu Index is originally constructed as $-0.091[(ib + dp)/at] - 0.062[indicator set to one if dvc + dvp is positive, and zero otherwise] + 0.021[dltt/at] - 0.044[\log(at)] + 0.102[average industry sales growth, estimated separately for each three-digit SIC industry and each year, with sales growth defined as $(sales_{it}/sales_{it-1}) - 1] - 0.035[sales growth, defined as $(sales_{it}/sales_{it-1}) - 1]$; and we approximate the Index by $-0.091[(EBITDA)/assets] + 0.021[long-term debt/assets] - 0.044[\log(assets)] + 0.102[average industry sales growth, estimated separately for each four-digit SIC industry and each year, with sales growth defined as $(sales_{it}/sales_{it-1}) - 1] - 0.035[sales growth, defined as $(sales_{it}/sales_{it-1}) - 1]$. The Hadlock and Pierce Index is originally constructed as $-0.737size + 0.043size^2 - 0.040age$, where size equals the log of Compustat item at and age is the number of years the firm is listed; and we approximate the Index by $-1Assets$. Standard errors are reported in parentheses and are clustered at the firm level. ***, **, and * indicate significance at the 1% level, 5% level, and 10% level, respectively.$$$$

Variables	Hadlock-Pierce				Whited-Wu				Kaplan-Zingales			
	Large, profitable, and mature		Small, unprofitable, and young		Large, profitable, and mature		Small, unprofitable, and young		Large, profitable, and mature		Small, unprofitable, and young	
	CAPEX/ Sales	EBITDA/ Assets	CAPEX/ Sales	EBITDA/ Assets	CAPEX/ Sales	EBITDA/ Assets	CAPEX/ Sales	EBITDA/ Assets	CAPEX/ Sales	EBITDA/ Assets	CAPEX/ Sales	EBITDA/ Assets
1	2	3	4	5	6	7	8	9	10	11	12	
POST x TREATED	-0.196** (0.087)	0.021*** (0.005)	-0.035** (0.014)	0.005 (0.006)	-0.117*** (0.044)	0.016*** (0.006)	-0.171 (0.121)	0.013** (0.006)	-0.149 (0.098)	0.006 (0.006)	-0.045 (0.028)	-0.020*** (0.005)
EBIT/Assets	-2.167 (2.112)		0.139 (0.206)		0.714 (0.587)		-1.261 (1.475)		-1.111 (1.278)		0.479* (0.275)	
Log(Sales)	-0.111 (0.160)	0.032*** (0.012)	-0.320 (0.293)	0.058*** (0.014)	-0.533 (0.433)	0.060*** (0.012)	-0.120 (0.145)	0.044*** (0.011)	-0.239 (0.225)	0.053*** (0.011)	-0.619 (0.444)	0.059*** (0.010)
Number of firms	886	886	1,102	1,102	899	899	850	850	1,058	1,058	989	989
Observations	5,417	5,550	5,515	5,620	5,693	5,784	4,873	4,976	6,061	6,154	5,473	5,589
Firm & year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.9

Remeasuring the effect of the reform by variation in default risk

This table reports results for the regression $y_{ijt} = \alpha_i + \lambda_t + \eta POST_t + \gamma TREATED_i + \delta(POST_t \times TREATED_i) + \beta X_{ijt} + \omega_j \times \lambda_t + \varepsilon_{ijt}$. The dependent variable is the Debt-to-(Debt+Equity) ratio in column 1, the Debt-to-Assets ratio in column 2, the Secured-Bank-Debt-to-Debt ratio in column 3, the CAPEX-to-Sales ratio in column 4, the Fixed-Assets-to-Assets ratio in column 5, the Log of Assets in column 6, the EBITDA-to-Assets ratio in column 7, and the standard deviation of the EBITDA-to-Assets ratio (calculated separately for the prereform and postreform periods) in column 8. Here, i indexes firms, t indexes time, and j indexes industries; y_{ijt} is the dependent variable of interest; α_i are firm fixed effects; λ_t are year fixed effects; and $POST_t$ is a dummy variable that equals one for the years after the ESUG Act was passed (2012 to 2015), and zero for the years before the bankruptcy reform was passed (2009 to 2011). $TREATED_i$ is an indicator variable that equals one if the firm belongs to the treated group, and zero if it belongs to the control group. As in our main analysis, we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%), but now based on firms' average prereform (2009 to 2011) measure of default probability (as measured by the interest coverage ratio (ratio of EBIT to interest payments)). Firms in the lowest tercile (high probability of default) form our treated group and firms in the highest tercile (low probability of default) form our control group (cutting off firms that are in the middle 33% of the bankruptcy probability distribution). X_{ijt} are the time-varying covariates $EBIT/Assets$ and $\text{Log}(Sales)$ in columns 1 to 6, the covariate $\text{Log}(Sales)$ in column 7, and the means of $EBIT/Assets$ and $\text{Log}(Sales)$ (calculated separately for the prereform and postreform periods) in column 8; ω_j are industry fixed effects; and ε_{ijt} is the firm specific error term. The coefficient of interest is δ , which measures the difference-in-differences effect. Standard errors are reported in parentheses and are clustered at the firm level. ***, **, and * indicate significance at the 1% level, 5% level, and 10% level, respectively.

Variables	Debt/ (Debt+Equity) 1	Debt/ Assets 2	SBD/ Debt 3	CAPEX/ Sales 4	FA/ Assets 5	Log (Assets) 6	EBITDA/ Assets 7	SD(EBITDA/ Assets) 8
POST x TREATED	0.025*** (0.004)	0.021*** (0.004)	-0.017*** (0.006)	-0.070*** (0.023)	-0.027*** (0.004)	-0.142*** (0.010)	0.047*** (0.003)	0.007*** (0.003)
EBIT/Assets	-0.274*** (0.040)	-0.261*** (0.036)	-0.073*** (0.024)	-0.112 (0.464)	-0.047** (0.021)	-0.122** (0.053)		0.025 (0.035)
Log(Sales)	0.026*** (0.006)	0.026*** (0.006)	-0.020*** (0.008)	-0.313* (0.161)	-0.024*** (0.006)	0.372*** (0.030)	0.067*** (0.007)	0.004 (0.014)
Number of firms	3,128	3,128	3,128	3,128	3,128	3,128	3,128	3,127
Observations	17,705	17,722	13,068	17,585	18,003	18,009	17,967	5,726
Firm & year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry x year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

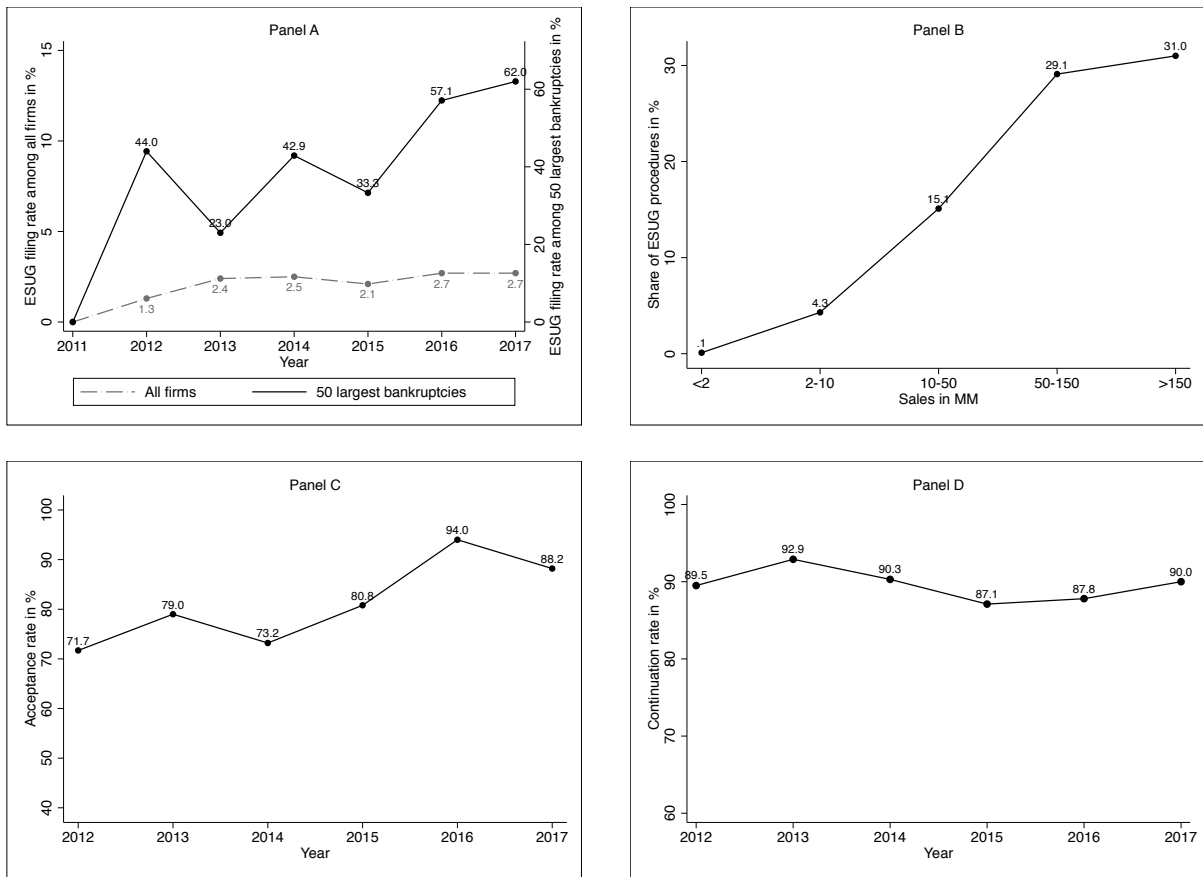


Figure 3.1 Empirical evidence on ESUG. Panel A shows the filing rate of bankrupt firms for a reorganization procedure under ESUG, separately among all bankrupt firms (dashed line) and among the 50 largest annual bankruptcy cases (solid line) (measured by sales; average sales here is 210 million euro). Panel B outlines the share of bankrupt firms that used a firm restructuring under ESUG for different firm sizes. Panel C documents the acceptance rate when a firm files for ESUG reorganization. Panel D shows the continuation rate (either in the form of an accepted reorganization plan or an asset deal) of firms that filed for an ESUG procedure. Sources: JUVE legal information, The Boston Consulting Group, Finance-Magazin/Falkensteg, ESUG evaluation report.

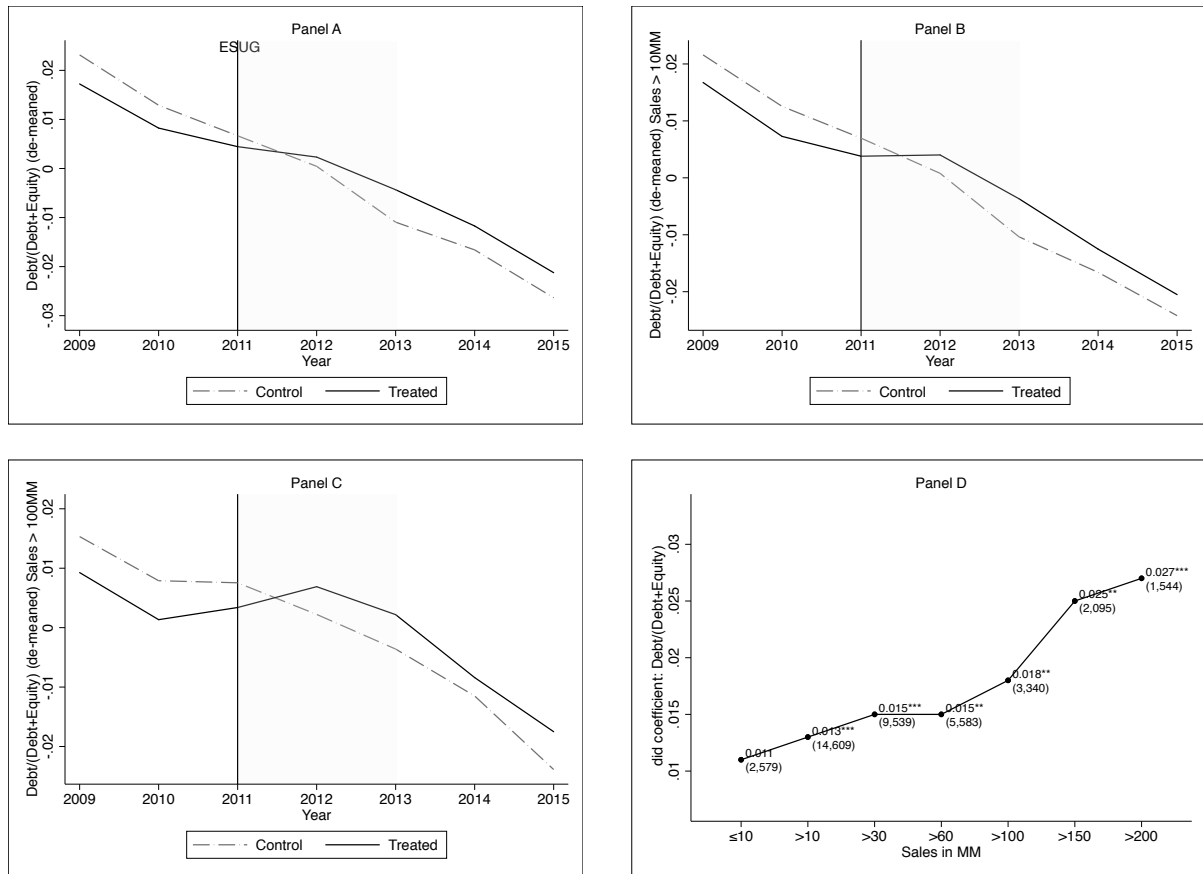


Figure 3.2 Effect on corporate leverage. Panel A plots the de-meaned time series of the Debt-to-(Debt+Equity) ratio for firms with high and low levels of asset tangibility. Following Vig (2013), we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%) based on firms' average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), with firms in the highest tercile are defined as high asset tangibility firms (our treatment group) and firms in the lowest tercile are defined as low asset tangibility firms (our control group) (cutting off firms that are in the middle 33% of the asset tangibility distribution). As in Panel A, in Panels B and C, we plot the de-meaned time series of the Debt-to-(Debt+Equity) ratio for firms with high and low levels of asset tangibility. Different than in Panel A, which is based on the full sample, in Panel B, we consider a subsample of firms with sales larger than 10 million euros (specifically, we first drop firms with sales less than 10 million euros (based on firms' average prereform sales), and then divide firms into high and low tangibility firms), while, in Panel C, we consider a subsample of firms with sales larger than 100 million euros. Panel D reports difference-in-differences coefficients of regressions employed on different subsamples of firms with different firm sizes. Specifically, the coefficient of the first category (from left to right) is from a subsample of firms with sales less than 10 million euros (i.e., we first drop firms with sales more than 10 million euros (based on firms' average prereform sales), and then divide firms into high and low tangibility firms), the coefficient of the second category is from a subsample of firms with sales more than 10 million euros, the coefficient of the third category is from a subsample of firms with sales more than 30 million euros, and so on. The dependent variable is the Debt-to-(Debt+Equity) ratio. The regressions are specified as in column 3 of Table 3.3, i.e., with covariates and industry fixed effects. The number of observations for each category is reported in parentheses.

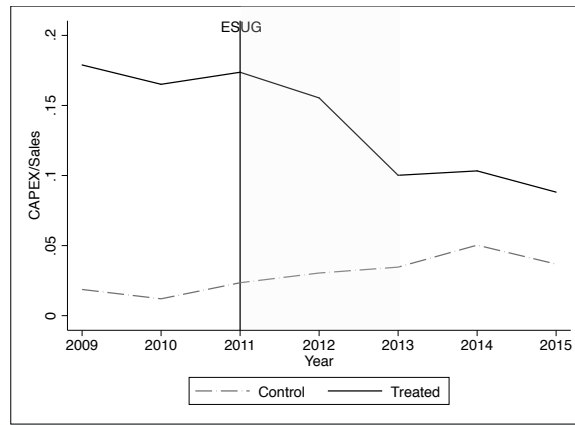


Figure 3.3 Effect on corporate investments. This figure plots the time series of the CAPEX-to-Sales ratio for firms with high and low levels of asset tangibility. Following Vig (2013), we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%) based on firms' average prereform (2009 to 2011) measure of asset tangibility (as measured by the ratio of fixed assets to total assets), with firms in the highest tercile are defined as high asset tangibility firms (our treatment group) and firms in the lowest tercile are defined as low asset tangibility firms (our control group) (cutting off firms that are in the middle 33% of the asset tangibility distribution).

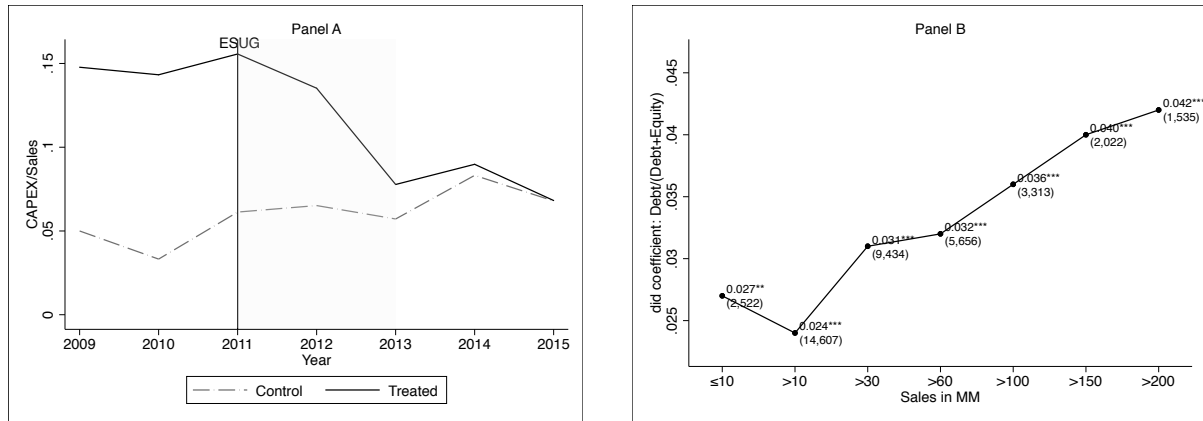


Figure 3.4 Treatment by risk. Panel A plots the time series of the CAPEX-to-Sales ratio for firms with high and low probability of default. As in our main analysis, we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%), but now based on firms' average prereform (2009 to 2011) measure of default probability (as measured by the interest coverage ratio (ratio of EBIT to interest payments)). Firms in the lowest tercile (high probability of default) form our treated group and firms in the highest tercile (low probability of default) form our control group (cutting off firms that are in the middle 33% of the bankruptcy probability distribution). Panel B reports difference-in-differences coefficients of regressions employed on different subsamples of firms with different firm sizes. Specifically, the coefficient of the first category (from left to right) is from a subsample of firms with sales less than 10 million euros (i.e., we first drop firms with sales more than 10 million euros (based on firms' average prereform sales), and then divide firms into high and low probability of default firms), the coefficient of the second category is from a subsample of firms with sales more than 10 million euros, the coefficient of the third category is from a subsample of firms with sales more than 30 million euros, and so on. The dependent variable is the Debt-to-(Debt+Equity) ratio. The regressions are specified as in column 3 of Table 3.3, i.e., with covariates and industry fixed effects. The number of observations for each category is reported in parentheses.

Appendix for Chapter 3

Table A.3.1 Creditor rights, leverage and investments, and size: empirical literature

This table presents an overview of the empirical literature regarding the effect of creditor rights on corporate leverage and investments, together with the average firm size in the study and the found effect. Other studies in the literature, such as Djankov, McLiesh, and Shleifer (2007) or Qian and Strahan (2007), are not included as they are not directly concerned with how firms adjust corporate leverage and investments with regard to bankruptcy law (the papers are, e.g., rather concerned with private credit or bank lending).

Study	Average firm size	Effect of stronger creditor rights
Haselmann, Pistor, and Vig (2010)	SME	positive
Campello and Larrain (2016)	SME	positive
Ponticelli and Alencar (2016)	SME	positive
Rodano, Serrano-Velarde, and Tarantino (2016)	SME	positive
Calomiris et al. (2017)	SME	positive
Aretz, Campello, and Marchica (2020)	SME	positive
Acharya, Amihud, and Litov (2011)	Listed/Large	negative
Vig (2013)	Listed/Large	negative
Cho et al. (2014)	Listed/Large	negative
Schoenherr and Starmans (2021)	Listed/Large	negative
Our study (2023)	Listed/Large	negative

Table A.3.2 Summary statistics for firms with high and low default probability

This table reports prereform summary statistics on the main variables, separately for firms with high and low probability of default. As in our main analysis, we divide our sample into three terciles (top 33%, middle 33%, and bottom 33%), but now based on firms' average prereform (2009 to 2011) measure of default probability (as measured by the interest coverage ratio (ratio of EBIT to interest payments)). Firms in the lowest tercile (high probability of default) form our treated group and firms in the highest tercile (low probability of default) form our control group (cutting off firms that are in the middle 33% of the bankruptcy probability distribution).

Variables	High probability of default	Low probability of default
Debt/(Debt+Equity)	0.767	0.568
Debt/Assets	0.728	0.548
Secured Bank Debt/Debt	0.398	0.254
CAPEX/Sales	0.127	0.050
Fixed Assets/Assets	0.501	0.377
Log(Assets)	10.027	9.864
EBITDA/Assets	0.053	0.182
SD(EBITDA/Assets)	0.047	0.049
Sales in MM €	114.827	143.374

Eigenabgrenzung

Das erste Kapitel „Empirical Evidence on Debt Governance“ ist eigene Arbeit. Alle Arbeiten wurden eigenständig von mir durchgeführt. Das zweite Kapitel „Dual-Class Firms, Agency Problems, and Debt Governance: Evidence from Korea“ ist eigene Arbeit. Alle Arbeiten wurden eigenständig von mir durchgeführt. Das dritte Kapitel „Creditor Rights, Corporate Leverage and Investments, and the Firm Type“ ist gemeinsame Arbeit mit Prof. Dr. Axel Kind, Jun.-Prof. Dr. Jiri Tressl, und Dr. Sabine Wende. Alle Arbeiten wurden gemeinschaftlich durchgeführt. Das dritte Kapitel entstammt aus meiner Masterarbeit an der Universität zu Köln. Die vorliegende Version beinhaltet wesentliche Anpassungen und Erweiterungen. Die Masterarbeit wurde zusammen mit der Dissertation bei dem Prüfungskomitee eingereicht. Nennenswerte Programme, die bei der Erstellung der Dissertation von mir benutzt wurden, sind Word, Stata, EconLit und Google Scholar, und Google Übersetzer und DeepL (insbesondere für die Übersetzung der englischen Zusammenfassung in die deutsche Zusammenfassung).

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