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Agrawal, Ashwini; Gonzalez-Uribe, Juanita; Martínez-Correa, Jimmy

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Measuring the Ex-Ante Incentive Effects of Creditor Control Rights during Bankruptcy Reorganization

Ashwini Agrawal
London School of Economics

Juanita González-Uribe
London School of Economics

Jimmy Martínez-Correa
Copenhagen Business School

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Abstract

A large theoretical literature studies the effects of creditor control during bankruptcy proceedings on firm outcomes. Empirical work in this area mainly examines reforms to creditor control rights during liquidation. In this paper, we use administrative microdata and exploit a legal reform in Denmark to provide the first causal estimates of creditor empowerment in reorganization—the complementary bankruptcy procedure to liquidation. We find that the Danish reform led to a sharp decline in liquidations. Although few insolvent firms make use of the new reorganization procedures, we show that solvent firms improved their financial management and increased employment and investment. The findings illustrate the empirical importance of reorganization rules on the incentives of stakeholders outside of bankruptcy.

Keywords: Bankruptcy, Reorganization, Liquidation, Creditors, Debtors

JEL Classification Codes: G33, G34, G18

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Corresponding author: Ashwini Agrawal (a.agrawal14@lse.ac.uk).
Mailing Addresses: Ashwini Agrawal, LSE, Houghton Street, London WC2A 2AE, UK
Juanita González-Uribe, LSE, Houghton Street, London WC2A 2AE, UK
Jimmy Martínez-Correa, Copenhagen Business School, Porcelænshaven 16A, 2.69, 2000 Frederiksberg, Denmark

1. Introduction

A large theoretical literature in corporate governance examines the incentives of creditors during bankruptcy proceedings (see White, 1989 and La Porta et. al, 1998 for an overview). This literature has been accompanied by a wide body of empirical work that measures the effects of creditor control rights on firm outcomes. Most of these studies, however, investigate changes to liquidation procedures or creditors' ability to seize and liquidate assets (e.g., Vig, 2013; Visaria, 2009). This paper provides the first causal estimates of the effects of greater creditor control rights during reorganization—the complementary procedure to liquidation in bankruptcy.

Our focus on reorganization is essential because the lessons learned from studies of liquidation cannot be readily applied to reorganization, as the two procedures feature vastly different incentive problems between creditors and debtors (Hart, 1995). For example, creditors use the threat of liquidation to deter debtor strategic default while trying to limit inefficient liquidations. In contrast, creditors in reorganization must incentivize managers to continue operating a business if they possess valuable skills or information while limiting the scope for debtors to behave opportunistically.

We analyze recent reforms to Denmark's bankruptcy code, which granted creditors greater control over the bankruptcy reorganization process. In 2010, Danish creditors were given the newfound ability to pass their own court-enforced restructuring plans. Creditors also gained the power to replace management during reorganization (Bang-Pedersen, 2017). Prior to the reform, debtors were significantly more powerful than creditors during reorganization: only debtors could propose a restructuring plan, and existing management would essentially remain in charge of the firm's assets during bankruptcy (International Insolvency Institute, n.d.).

Denmark's reform is useful to study for several reasons. First, the reform offers a rare opportunity to study plausibly exogenous variation in creditor control rights

during reorganization while keeping liquidation rules unchanged. Second, we are able to obtain detailed administrative microdata collected by the Danish government, as well as credit registry data from Experian A/S, to precisely measure the reform's impact on a variety of firm outcomes. Third, the setting we examine is informative because Denmark has relatively well-developed financial markets, and evidence of contracting frictions in our setting is likely to be of relevance to other economies.

We test the hypothesis that the increase in creditor control rights during Danish bankruptcy reorganization led to an overall reduction in corporate liquidations. Prior to the reform, debtors remained in control of the firm's assets during reorganization, which enabled managers to take opportunistic actions that would benefit debtors at the expense of creditors, such as risk-shifting and inefficient continuation (Jensen and Meckling, 1976; Harris and Raviv, 1991). Creditors and debtors could not write enforceable contracts to prevent such behavior because actions such as risk-shifting are inherently unobservable and nonverifiable in court (Aghion, Hart, and Moore, 1992; Hart, 1995). Instead, creditors would avoid these agency costs during bankruptcy by liquidating, rather than restructuring, insolvent firms.

By shifting control rights from debtors to creditors during reorganization, the Danish reform enabled creditors to prevent managers from incurring such agency costs. Creditors could now remove managers who took actions that were inconsistent with creditors' restructuring plans. We therefore hypothesize that the reform would reduce corporate liquidation rates through two complementary channels.

First, because creditors are given greater control over the firm during reorganization and now face lower debtor agency costs, they will have greater incentive to restructure, rather than liquidate, insolvent firms in bankruptcy. We call this channel the ex-post incentive effect of the reform on liquidations. Second, because managers have less scope for opportunistic behavior during reorganization that benefits debtors

at the expense of creditors, solvent debtors will have greater incentive to avoid entering financial distress to begin with. We call this channel the ex-ante incentive effect of the reform on liquidations.

We present a number of empirical findings that support our hypothesis. First, we note that anecdotally, Danish reorganization rarely took place prior to the reform, as creditors almost always chose to liquidate, rather than restructure, distressed firms (International Insolvency Institute, n.d.). Creditors reported being concerned that managers would "abuse the firm's assets" during bankruptcy (Danish Bankruptcy Council, 2009). These anecdotes are consistent with the view that debtor opportunism was a major concern for creditors during reorganization.

Second, we exploit unique institutional features of Denmark's reform to devise an identification strategy that measures the causal impact of the reform on corporate liquidations. Denmark's new rules only applied to limited liability companies (LLC's); sole proprietorships were unaffected by the new bankruptcy procedures. These two types of firms, which comprise approximately 99% of all firms in Denmark, share similar trends in liquidation rates prior to the reform. However, after the reform goes into effect, only limited liability firms—but not sole proprietorships—show a significant decline in liquidation rates. These findings are reinforced by a battery of regression and propensity score matching analyses that control for numerous firm, industry, and macroeconomic characteristics. Across all specifications that we examine, we find that the reform has a negative effect on the liquidation rates of "treated" limited liability firms.

We present evidence to support the underlying mechanisms of our hypothesis. For example, we find that solvent firm managers show marked improvements in their debt repayment patterns following the reform. Using data on repayment delinquencies, we show that firms pay a higher fraction of their outstanding loans on time after the law

passed. The effects are especially salient for firms with a single owner—as compared to firms with dispersed shareholders—where the manager and owner are likely to be the same person; such managers otherwise have stronger incentives to take opportunistic actions that transfer value from debt to equity during reorganization.

We also present evidence that the reform triggers an increase in credit supply. We find that firms hold greater leverage, and pay lower interest rates for a given amount of leverage, after the reform. Although debtors may reduce their demand for credit following the reform (ceteris paribus), the observed net increase in leverage, and lower interest rates conditional on leverage, suggest that creditors' willingness to offer credit on more generous terms outweighs any potential reductions in credit demand.[1] The results thus support our hypothesis because they show that creditors increase their willingness to provide capital when they are less exposed to opportunistic behavior by debtors (see Broadie, Chernov, and Sundaresan, 2007; Antill and Grenadier, 2019 for related theories).

We then show how the reform impacts firms' real investment decisions. The data indicate that firms increase their employment and physical capital stock following the reform. The findings are consistent with the view that increased credit supply, triggered by the new reorganization rules, enables firms to raise additional capital to expand their operations and grow in size. The results thus illustrate that the reform was successful at mitigating unemployment and helping viable businesses continue operating through adverse financial conditions—key objectives of bankruptcy design.

We then describe the governance decisions of firms to illustrate potential mechanisms by which debtors and creditors influence managerial behavior. We document higher managerial turnover among the executives and the directors of solvent

[1] See Vig (2013) for a prominent example of debtors reducing their demand for credit in response to an increase in creditor control rights during liquidation.

companies after the new law is passed. These findings are consistent with debtors responding to the reform by more frequently replacing managers who expose their firms to high risks of financial distress.

To supplement our statistical findings, we also present a case study of the Danish reform at work. Top-Toy, a leading toy retailer, entered 2018 with significant debts due to poor Christmas sales and a failed enterprise resource planning implementation (Mölne, 2018; Norman, 2018). In January 2018, the owners of Top-Toy replaced the CEO, Søren Torp Laursen, with Per Sigvardsson, just months after Laursen had claimed that Top-Toy was not "concerned about international rumors on bankruptcy" and in fact planned on opening new stores in the coming year (Östgren, 2017; Lindgren, 2017). Top-Toy was unable to climb out of financial distress, though after entering reconstruction in November 2018, creditors voted to keep the company afloat provided that the firm close its most unprofitable stores and streamline its operations in accordance with creditor recommendations (Philipsen, 2018). Top-Toy embodies our hypothesis by showing how the reform impacted the incentives of solvent debtors and creditors to avoid distress and continue operating through reconstruction.

We present several analyses to consider alternative explanations for our empirical findings. First, we argue the unobservable changes in economic conditions during the sample period are unlikely to explain our results. We observe discrete changes in liquidations in the immediate months surrounding the implementation of the law. It is unlikely that economic conditions dramatically improve in a similarly discrete manner, particularly for limited liability firms relative to sole proprietorships. Second, we show that our results are not driven by simple changes in sample composition around the reform. Third, we present excerpts from Danish parliamentary debate to show that political economy considerations are unlikely to explain our findings.

We also note that competing models of creditor control during reorganization appear to be of second-order empirical importance in our setting. For example, models in which stronger creditor control rights incentivize managerial entrenchment (Bebchuk and Picker, 1993; Berkovitch, Israel, and Zender 1997, 1998), or worsen coordination among creditors (Gertner and Scharfstein, 1991), suggest that the Danish reform could increase liquidation rates. Our findings to the contrary suggest that our hypothesis is the most plausible explanation for the Danish reform's effects.

The findings in our paper contradict the views of many observers who panned the Danish reform. Critics noted that less than 5% of bankruptcy filings from 2011 to 2016 involved reorganization, which led them to conclude that the Danish act was ineffective at curbing liquidations since so few firms used the new reorganization procedure (Bariatti and von Galen, 2014; Bang-Pedersen, 2017). Our results, however, show that the impact of the reform cannot be fully understood solely by looking at insolvent firms that use reorganization. Instead, the data show the need to consider the reform's ex-ante incentive effects on solvent firms that operate outside of bankruptcy.

To that end, we perform back-of-the-envelope calculations to quantify the relative sizes of the Danish reform's incentive effects on liquidations. We use the pre-reform sample data to estimate a probit model of corporate liquidations and then use the fitted model to predict the number of liquidations that would have occurred had the reform never been passed. Our estimates suggest that the ex-ante incentive effects of the reform on solvent firms are approximately three times larger than the ex-post incentive effects of the reform on insolvent firms that enter reconstruction.

Our paper provides a unique contribution to the literature on bankruptcy resolution: we present the first causal estimates of the ex-ante incentive effects of greater creditor control rights during reorganization. The impact of creditor control rights on corporate liquidations is a major concern in reorganization design (Aghion,

Hart, and Moore, 1992). Isolated instances of reorganization reforms are rare; many papers study simultaneous changes to reorganization and liquidation, such as Scott and Smith (1986), Araujo, Ferreira, and Funchal (2012), and Hackbarth, Haselmann, and Schoenherr (2015). Our work illustrates that contracting frictions between creditors and debtors during reorganization are significant barriers to restructuring—even in well-developed economies such as Denmark.

This study complements two related strands of the literature on creditor empowerment in corporate governance. First, our estimates show that fully characterizing the empirical effects of creditor control during reorganization requires understanding both its ex-post effects on insolvent firms and its ex-ante effects on solvent firms (studies of the ex-post effects include Hotchkiss, 1995; Strömberg, 2000; Franks and Sussman, 2005; Davydenko and Franks, 2008; and Bharath, Panchapagesan, and Werner, 2014). Second, our study demonstrates that increased creditor control during reorganization may have different effects than increased creditor control during liquidation. For example, Vig (2013) presents evidence that stronger creditor control during liquidation can lead to lower overall levels of secured borrowing by debtors.[2] Our findings illustrate that the impact of increased creditor control depends on the procedure being considered as well as the market context in which creditors and debtors are interacting.

2. Institutional background, theoretical framework, and related literature

2.1. The Danish Bankruptcy Reorganization Reform of 2010

[2] Our study also relates to Becker and Strömberg (2012), who find that explicit changes in managers' fiduciary duties toward debt holders during solvency mitigate debt-equity conflicts. Our findings complement their work by showing that greater debt holder control during reorganization also affects the behavior of managers, even without explicitly changing fiduciary responsibilities during solvency.

During the financial crisis, Denmark witnessed a wave of corporate liquidations and high unemployment. Regulators seized the opportunity to revise reorganization rules that were deemed to be in need of overhaul. After seeking advice from a panel of policymakers, academics, and practitioners (Bang-Pedersen, 2017), the Danish parliament bankruptcy reorganization reforms in June 2010 with the aim of helping viable businesses stay afloat during financial distress (Barfoed, 2010). We describe the essential features of the reform in this section and provide a detailed description of the reform and the Danish Bankruptcy Code in the Appendix (Section A).

Prior to the reform, the Danish reorganization procedure was relatively favorable toward debtors (International Insolvency Institute, n.d; Gullitz-Wormslev and Levin, 2010). For example, only management was allowed to file for restructuring, and creditors could only ratify management's plan; creditors were unable to file reorganization plans themselves. Additionally, management would essentially remain in possession of the firm during bankruptcy. Creditors had limited ability to replace management during reorganization.

In practice, management's restructuring plans were rarely approved by creditors, and filing for reorganization was essentially just a precursor toward liquidation (International Insolvency Institute n.d, Danish Bankruptcy Council 2009). Anecdotes from the Danish Bankruptcy Council that was tasked with reforming the reorganization procedure acknowledged creditors' concerns that management would "abuse the firm's assets" during reorganization (Danish Bankruptcy Council, 2009). This view suggests that creditors were concerned about opportunistic behavior by debtors if left unchecked.

The 2010 reform introduced a new reorganization procedure, called Rekonstruktion, which gave creditors significantly more power during restructuring. The reform enabled creditors to implement their own restructuring plans without requiring management consent. To verify that creditor plans would be implemented

properly, managers would be required to report to creditors through a court-appointed administrator (Sjørslev and Højslet, 2019). Creditors were also given the right to replace management and have the firm's activities overseen by the administrator if they deemed that management failed to implement creditors' plans. The new rules only applied to LLC's; the reorganization rules for sole proprietorships remained unchanged. Additionally, the Danish liquidation procedure for all firms remained the same as before (Bang-Pedersen, 2017).

Initial support for Denmark's reform was unanimous across Denmark's political party spectrum. In the Appendix (Section B), we present official excerpts of parliamentary debate surrounding the act by various political party representatives before the passage of the reform in 2010.[3] The Danish minister of justice, Lars Barfoed, summarized these views before the reform's official adoption by stating

> I am pleased that there is broad support for the proposal, as it is, although there are of course things that we must work with during the committee process. There is an overall very positive backing for the proposal, I think, and not least of the intentions, namely that we should improve the possibility of insolvent but viable companies being reconstructed instead of [liquidated]. Because it is basically what the proposal is about (Danish Parliament Documents Collection Bill L 199 2009-10).[4]

Following the passage of the reform, however, the Danish act was widely subjected to criticism:

> Despite the fact that the 2010 [reform] introduced a modern reorganization regime into the Danish Bankruptcy Act, in practice it has been quite a limited success. In the period 2011-2016, less than 5% of all insolvency proceedings concerning businesses were reorganization proceedings, whereas the remaining, more than 95% [of all insolvency *proceedings] were liquidation proceedings.* (Bang-Pedersen, 2017)

This view echoes criticisms leveled at other bankruptcy procedures that grant creditors significant control rights during reorganization: critics argue that too few insolvent

[3] The reforms were passed in June 2010, but were put into effect in April 2011. In our empirical analysis, we use the data of passage as the relevant event date to account for anticipatory behavior that preempts the effective date (any anticipatory effects are part of the treatment effect of the reform). Our regression results hold if we use the effective date instead of the passage date.

[4] Original Danish text translated using Google Translate.

firms make use of such procedures, thereby increasing total liquidations since economically viable but financially distressed debtors do not wish to transfer control to creditors during reorganization. For example, Kaiser (1996) criticizes the UK reorganization procedure—which is considered relatively creditor friendly (Tollenaar, 2017)—on these grounds. As another example, the Swedish government introduced bankruptcy restructuring reforms in 1996 because it was deemed that too few firms were entering restructuring; Strömberg (2000) finds that restructuring filings were less than 1% of all bankruptcy filings from 1988 to 1991. These anecdotes illustrate how the impact of increased creditor control rights during bankruptcy reorganization are often evaluated by examining insolvent firms exclusively.

2.2. Theoretical Framework

We propose the hypothesis that increases in creditor control rights during reorganization not only have effects on insolvent firms but also have important effects on solvent firms. In this section, we describe our hypothesis in the context of the 2010 Danish reform. We also discuss alternative models and explain how our empirical tests enable us to test competing views of the reform.

We hypothesize that prior to the Danish reform, creditors were limited in their ability to write enforceable contracts with debtors that would prevent managers from taking actions that benefit debtors at the expense of creditors during reorganization (Aghion, Hart, and Moore, 1992; Hart, 1995; Becker and Strömberg, 2012). Prominent examples of such opportunistic actions are risk-shifting (Jensen and Meckling, 1976) and inefficient continuation (Harris and Raviv, 1991)—agency costs that were alluded to in the Danish Bankruptcy Council's review of the prevailing bankruptcy procedure (Danish Bankruptcy Council, 2009). The inability to write such contracts stemmed from the fact that managerial actions are inherently difficult to observe and verify in

court (Hart, 1995). Because creditors lacked the control rights to dismiss management, they would prefer to avoid exposure to these agency costs by liquidating, rather than restructuring, insolvent firms.

The 2010 Danish reform shifted control rights from debtors to creditors during bankruptcy, as creditors became empowered to implement their own restructuring plans and replace management during reorganization. These changes effectively enabled creditors to prevent managers from taking opportunistic actions that benefited debtors at the expense of creditors. Creditors now had the power to unilaterally dismiss managers who took actions that were inconsistent with creditors' restructuring plans.

The reform, therefore, would have a negative impact on corporate liquidations through two complementary channels. First, because creditors are given greater control over the firm during reorganization and now face lower debtor agency costs, they will have greater incentive to restructure, rather than liquidate, insolvent firms already in bankruptcy. We call this channel the ex-post incentive effect of the reform on liquidations. Second, because managers have less scope to take opportunistic actions during bankruptcy that benefit debtors at the expense of creditors, solvent debtors will have greater incentive to avoid entering financial distress to begin with. We call this channel the ex-ante incentive effect of the reform on liquidations.

To test our hypothesis, we examine the following empirical predictions:

Prediction 1: Corporate liquidation rates will decline following the increase in creditor control rights triggered by the Danish reorganization reform. This prediction reflects the net sum of our hypothesized ex-ante and ex-post incentive effects of the reform on managerial behavior. The null hypothesis is that the reform does not have a significant, negative impact on corporate liquidation rates; this contrasting prediction is reflective of critical views toward creditor control in reorganization discussed earlier.

Prediction 2: Debt repayment rates for solvent firms will improve following the increase in creditor control rights caused by the reorganization reform. This prediction reflects the ex-ante incentive effects of the law, as solvent firm managers will make greater efforts to reduce the risk of financial distress. One corollary of this prediction is that the debt repayment rates of firms with a single owner should especially improve (compared to firms with dispersed shareholders); prior to the reform, managers of single-owner firms are more prone to engage in risk-shifting, since they are more likely to internalize the gains that benefit debtors at the expense of creditors.

Prediction 3: Credit supply will increase, ceteris paribus, following the increase in creditor control rights spurred by the reorganization reform. Our hypothesis suggests that creditors will be more willing to offer a given amount debt at a lower cost when they are less concerned about debtor agency costs (see Broadie, Chernov, and Sundaresan, 2017 and Antill and Grenadier, 2019 for models of debt pricing and debtor agency costs).

Testing this prediction is complicated because empirical measures of borrowing amounts and borrowing costs will reflect the net effects of both credit supply and credit demand responses to the reform. Debtors may reduce their demand for credit in response to the reform, for example, because managers become more constrained in reorganization to take actions that benefit debtors at the expense of creditors. Vig (2013), for example, shows that debtors reduce their demand for secured debt when creditors are given stronger control rights in liquidation (see also Benmelech, Kumar, and Rajan, 2020). Additionally, a further complication arises when one considers the joint relationship between borrowing costs and borrowing quantities. In equilibrium, a credit supply-induced increase in firm borrowing could actually translate into higher overall borrowing costs.

To test our prediction about credit supply, we draw inference from equilibrium changes in firm leverage decisions and interest rates on debt—controlling for leverage—around the reform. If we observe both a net increase in leverage and a net reduction in interest rates on debt for a given level of leverage, then the evidence supports our hypothesis irrespective of the demand-induced effects triggered by the reform. For example, increased leverage and lower interest expenses (controlling for leverage) would indicate that any reductions in debtor demand for credit are outweighed by the effects of credit supply increases, as such joint effects of the reform on debt quantities and prices could not be observed otherwise.

Prediction 4: Firms will increase investment in production inputs following the reform. The reduction in borrowing costs triggered by the reform will increase the profitable investment opportunity set facing firms. Thus, the new reform would enable firms to invest in greater amounts of production inputs such as labor and capital. We test this prediction by studying changes in firm-level employment and physical capital stocks after the reform.

Finally, we evaluate potential governance levers that debtors might use to influence managers' behavior after the reform.

Prediction 5: Debtors will exert greater monitoring effort over managers due to the reform. Since debtors have greater incentives to avoid financial distress after the reform, we predict that debtors will exert greater monitoring efforts over management to ensure they are taking actions to avoid insolvency. Debtor intervention may be explicit or implicit; we examine data on managerial turnover around the reform to assess whether there is evidence of explicit discipline imposed on management by debtors.

2.2.1. Alternative theories and welfare

In contrast to our hypothesis, alternative theories suggest that increased creditor control rights spurred by the Danish reform could actually raise corporate liquidation rates. For example, because the reform enables creditors to dismiss managers, one potential response to the reform is that managers of solvent firms may select inefficient investment projects that require their specific human capital, thereby deepening potential managerial entrenchment and making it costlier for creditors to fire them during bankruptcy (Bebchuk and Picker, 1993; Berkovitch, Israel, and Zender, 1997). Greater creditor control rights during reorganization may also worsen creditor coordination during bankruptcy (Gertner and Scharfstein, 1991).

Because these alternative theories make predictions that are the opposite of our hypothesis, any evidence that we find consistent with our hypothesis suggests our model is empirically dominant relative to these alternative models. In other words, the evidence would indicate that our paper's empirical findings are primarily driven by our hypothesis. While these alternative theories may be relevant in other contexts outside of Denmark, the data will enable us to assess whether our hypothesis is the most important explanation of the Danish reform's effects on firm outcomes.

One important caveat to our analysis is that we are unable to make normative statements about the welfare impact of increased creditor control rights. First, welfare analysis requires detailed measures of surplus for all stakeholders within the firm. We only have limited data, however, on the surplus realized by stakeholders such as workers and suppliers. Second, we do not observe the counterfactual value that would be created by debtors overseeing insolvent firms without creditor control. In some cases, debtors may have (nontransferable) expertise that enables them to create more value from insolvent firms than creditors that oversee the firm during reorganization.

Nevertheless, we believe our findings are of interest because the general shift in control rights from debtors to creditors during reorganization is a major concern in

bankruptcy design (Aghion, Hart, and Moore, 1992; Becker and Josephson, 2016; Bharath, Panchapagesan, Werner, 2014). Isolated instances of reorganization reforms are rare; many papers study simultaneous changes to reorganization and liquidation, such as Scott and Smith (1986), Araujo, Ferreira, and Funchal (2012), and Hackbarth, Haselmann, and Schoenherr (2015).[5] If the ex-ante effects of the Danish reform are sufficiently large, then our findings show that exclusively focusing on the ex-post effects of bankruptcy rules can be misleading, and our estimates can help us distinguish the empirical relevance of competing views of bankruptcy reorganization.

2.3. Related literature

Our paper argues that the ex-ante effects of creditor control during restructuring need to be considered alongside the ex-post effects to fully characterize the impact of reorganization rules on firms. Studies of the ex-post effects of reorganization rules on firms already in financial distress include Davydenko and Franks (2008) and Franks and Sussman (2005), who study distressed firms across various codes in the UK, France, and Germany. Franks and Torous (1992, 1994), Gilson, John, and Lang (1990), Hotchkiss (1995), and Bharath, Panchapagesan, and Werner (2014) study insolvent firms in US Chapter 11.

Our study of reorganization also complements studies of liquidation—the main bankruptcy alternative to reorganization. Because the incentive problems between debtors and creditors differ between liquidation and reorganization, it is difficult to extrapolate lessons from studies of liquidation to reorganization (Hart, 1995). Moreover, different studies often feature different economic contexts. For example, Vig (2013)

[5] Rodano, Serrano-Velarde, and Tarantino (2016) is an exception, though they study an Italian reform that eases loan renegotiation during bankruptcy rather than unambiguous shifts in creditor control during reorganization.

presents evidence that stronger creditor control rights during liquidation in India lead to a reduction in the use of secured debt, as debtors' reduced demand for secured debt outweighs any supply-side effects of the reform on credit availability. In Denmark, we argue that stronger creditor control during reorganization leads firms to increase their overall borrowing; the credit supply-side effects are dominant. The data thus show that the impact of increased creditor control rights on firm outcomes can vary, depending on the procedure being studied, the level of financial development in a market, and the ability of debtors and creditors to "contract" around the law.

3. Data

We use three data sources to construct a panel dataset of detailed information on Danish firms. The first source of data is firm-year level administrative records maintained by the Danish government agency Statistics Denmark. We obtain these data through a nondisclosure agreement with the agency. The information covers nearly all publicly traded and privately held companies in Denmark.[6] The records contain annual firm accounting information such as balance sheet and income statement data. We use these data to measure firm characteristics such as operating performance, financial leverage, and implied interest rates on debt.[7] The data also include information on firm incorporation status, such as limited liability or sole proprietorship classifications, geographic locations of operations, bankruptcy dates, and standard European industrial

[6]Danish companies that earn between 0.3 and 100 million DKK are required to report their standardized accounting information to the Danish tax authorities; this information, along with survey questionnaire responses, comprise the administrative data that we examine.

[7] Statistics Denmark does not contain data on loan interest rates; however, we approximate implied interest rates on debt by computing the ratio of interest payments to outstanding long-term debt for a given firm-year.

classification codes (NACE).[8] The sample years for which we obtain data are from 2000 to 2013.

The second source of data describes the annual operating status of each company in the sample; these data come from Statistics Denmark and the Danish Business Authority (DBA). The DBA monitors whether firms are operating as independent entities, involved in reconstruction, or liquidated. For firms that are liquidated, we observe the specific dates when firms cease to operate; these data span the years 2009 to 2016.

The third source of data is from Experian A/S, a credit bureau that provides detailed data on the financial liabilities of firms in Denmark. These data supplement the information available from Statistics Denmark. Unlike Statistics Denmark, however, it only has data for limited liability firms; Experian does not contain information on sole proprietorships. Experian enables us to observe the characteristics of the debt liabilities facing sample firms, such as the amounts of outstanding debts as well as the debt repayment histories of firms in our sample. For many firms in Experian, we also observe managerial turnover and governance characteristics such as whether a firm has a single owner or dispersed equity ownership.[9]

We combine these data sources to create two firm-year panel datasets from 2009-2012, one created with Statistics Denmark data, the other created with Experian. In both datasets, for every firm-year observation, we observe balance sheet and income statement information as well as information about the region and industry of operation. We use our (primary) dataset with Statistics Denmark information to examine liquidation probabilities, financial decisions, and investment policies for limited

[8] Geographic location in the Statistics Denmark data is at the municipality ("Kommune") level; there are 98 distinct municipalities in the sample.
[9] The geographic locations of firms in Experian are at the zip code level; there are 1,106 distinct locations in the Experian sample.

liability firms and sole proprietorships around the passage of the reform. We use the (secondary) Experian data to examine debt repayment patterns and managerial turnover for limited liability firms around the passage of the reform. The definitions for all the variables that we use in these analyses are presented in the Appendix (Section C) along with the data sources corresponding to each variable.

3.1. Sample descriptive statistics

Table 1 contains descriptive statistics for the firms in our primary sample. There are approximately 132,070 firms in the sample; 72,505 of these firms are limited liability in their incorporation status, while the remaining 59,565 firms in our sample are sole proprietorships.[10] The average firm size is 4,562,000 DKK ($680,000 USD) in total assets, with annual average revenues of 6,273,000 DKK ($0.935M USD). As to be expected, limited liability firms are larger in size and generate higher revenues than sole proprietorships. In terms of financing patterns, the average net financial leverage (defined as long-term debt minus current assets divided by total assets) of sample firms is 9%, with limited liability firms taking on less debt than sole proprietorships on average. In the Appendix (Section D), we also include summary statistics for the limited liability firms that we observe in Experian to illustrate that we are able to measure debt repayment patterns and managerial turnover for a significant fraction of limited liability firms covered in Statistics Denmark's administrative records.

Fig. 2 depicts the industry distribution of firms across incorporation statuses, as per NACE classifications. Among LLC's, trade and transport represent approximately 38% of sample firms, while construction and knowledge-based industries each cover 20% of sample firms. Among sole proprietorships, trade and transport comprises 50%

[10] In the entire population of Danish firms, sole proprietorships and limited liability firms comprise 99.1% of all firms; the remaining firms, such as partnerships, cooperatives, and commercial funds, are excluded from our analysis for simplicity.

of sample firms, while construction comprises 24% of sample firms and knowledge-based services cover 10% of sample firms.

Table 2 describes the operating statuses of firms in our sample. Specifically, the table depicts the numbers of firms that are either operating or liquidated in our sample, across the years for which we observe overlapping accounting and operating status data. Across sample years, the rates of liquidation for limited liability firms range between approximately 2%-4%. Sole proprietorships show a lower propensity to become liquidated, with liquidation probabilities ranging between 0.4%-0.7%.

There are several key points on display in Tables 1 and 2. First, we observe a broad cross-section of firms across industries in Denmark; our data contain companies that span different ages, sizes, and performance metrics. Second, there are clear sampling differences between limited liability firms and nonlimited liability firms. As expected, LLC's tend to be larger firms, and they are more likely to appear in industries such as knowledge-based and information technology services. Third, we see that the number of corporate liquidations in Denmark prior to the passage of the reform is economically large in magnitude, illustrating the potential importance of legislation aimed at reducing rates of company dissolutions.

3.2. Sampling properties

There are various strengths and limitations of our data. One of the advantages of our dataset is that we observe nearly the entire population of firms in Denmark; our data are not subject to sample selection biases that might otherwise plague similar regression analysis in other contexts. Firms in Denmark are required by law to register with the Danish government and report their financial status to the authorities on an annual basis. The Danish government maintains and verifies the veracity of the records; these data have been increasingly used by researchers in economics and finance.

A second advantage of our data is that there are a significant number of firms that belong to various classes of incorporation status in Denmark. This variation enables us to exploit key institutional features of the reform for identification and increase the statistical power of our tests. We are also able to provide estimates that are ostensibly less subject to omitted variable biases that otherwise affect the interpretation of alternative empirical strategies such as cross-country analyses.

One of the limitations of our data is that we have limited time-series data on firms prior to 2009, as the Danish government's records on liquidations and changes in firm status are incomplete for previous years. As a result, we are unable to perform standard analyses of pretrends of firm behavior prior to the reform. To circumvent this problem, we perform a number of alternative analyses to assess the likely importance of differential trends in firm behavior prior to the reform that could explain differences in firm behavior after the law is passed. Our ability to perform these tests is enabled by the granularity of the microdata, and we present various pieces of evidence to argue that our findings are unlikely to be driven by preexisting trends in firm behavior between limited liability firms and nonlimited liability firms.

4. Empirical findings

In this section, we present a number of new empirical findings that provide support for our hypothesis.

4.1. Corporate liquidations

The first step of our empirical analysis is to measure the net impact of the Danish Bankruptcy Reorganization reform on corporate liquidations in the economy. As described earlier, many observers of Denmark's bankruptcy code criticized the reform as being ineffective at reducing corporate liquidations because few insolvent firms actually made use of the new bankruptcy procedures established by the Danish act.

Table 2 shows that the maximum number of limited liability firms that enter reorganization in any given sample year is 87, which is less than 0.05% of the total firms in the sample and less than 7% of the number of limited liability firms that are liquidated in the same year. Consistent with critics' views, the data indicate that few insolvent firms make use of the new reconstruction procedure.

These data, however, only indicate that the ex-post effects of the reform are limited. To measure the full causal impact of the reform—which includes both its ex-ante and the ex-post effects—the ideal experiment would be to measure the firm outcomes that materialize after the passage of the reform and compare these measures with the counterfactual outcomes that would have materialized in the absence of the reform. The difference in these outcomes would represent the true impact of the law.

The problem with performing this comparison in practice, however, is that the counterfactual outcomes of interest are unobservable. To circumvent this problem, we conduct several sets of analyses using observable data. We exploit legal features of the reform to construct our identification strategy, and we argue that the collective evidence closely approximates the causal effects that we wish to estimate.

4.1.1. Full sample estimates

Fig. 1 presents a time-series plot of corporate liquidations in Denmark. The figure depicts liquidations that occur each month around the reform's passage in June 2010 and its implementation in April 2011, both for limited liability firms impacted by the reform ("treated" firms) and for sole proprietorships unaffected by the reform ("control" firms). The plot shows similar trends in liquidation rates across limited liability firms and sole proprietorships in the months leading up to the reform. Immediately following the implementation of the reform, however, there is a persistent decline in limited liability liquidations; among sole proprietorships, in contrast, we see

a slight upward trend in liquidations. The raw data therefore depict a postreform divergence in liquidation rates between limited liability firms and sole proprietorships that is consistent with our hypothesis.

To control for additional factors that influence liquidation rates, we estimate several econometric models, where the dependent variable of interest is a binary indicator of whether a firm enters into liquidation in a given year. In Table 3, we present our findings from a probit model with the following specification:[11]

$$Pr(Liquidation_{it+1} = 1) = \phi[\beta_1 * Reform_t * LLC_{it} + \beta_r * Reform_t + \beta_l * LLC_{it} + \beta_c * Controls_{it} + B_{rc} * Reform_t * Controls_{it} + B_{lc} * LLC_{it} * Controls_{it} + e], \quad (1)$$

where the dependent variable, $Liquidation_{it+1}$, is a binary indicator for whether firm *i* enters liquidation in year *t+1*. Liquidation in year *t+1* is modeled as a function of firm and industry characteristics in year *t*. The main independent variable, $Reform_t$, is a binary indicator of whether the reorganization reform is passed by year *t*. LLC_{it} is an indicator for the limited liability status for firm *i* at time *t*.

The $Controls_{it}$ include industry growth at the NACE one-digit level, firm turnover rate, profitability, size, liability ratio, workforce size, and age; we also interact each of these controls with *LLC* and with *Reform*.[12] We include fixed effects for location to control for differences in liquidation rates across geographic segments. To avoid inconsistency in the probit coefficient estimates (i.e., the incidental parameters problem, which arises as the number of firms approaches infinity with a fixed number of time periods T), we do not include firm fixed effects (Wooldridge, 2002). We report

[11] We find nearly identical results using alternative econometric models such as logit and linear probability models; these results are available upon request. Probit models are most appropriate in our setting given the theoretical arguments presented in Judge et. al (1985) and Amemiya (1981). For example, probit is preferred to OLS because standard assumptions underlying OLS significance tests are violated with dichotomous dependent variables.

[12] For more details on the exact definitions of the control variables, refer to Section C (variable definitions) in the Appendix.

results with increasing numbers of controls to show the robustness of our treatment estimates across specification choices.

The identification assumption that underlies the causal interpretation of the regression estimates in Specification 1 is that the reform is uncorrelated with unobserved determinants of corporate liquidation. Under the identification assumption, the coefficient for *Reform*LLC* tells us whether the reform has a positive or negative effect on the probability of liquidation, controlling for observable factors such as firm performance and macroeconomic conditions.

Table 3 presents the regression estimates. For the sake of brevity, Table 3 in the paper presents coefficient estimates for the treatment effect estimator (i.e., the interaction term of $Reform_t*LLC_{it}$). The coefficients for all the control variables are presented in the Online Appendix to this paper. The coefficient estimates for *Reform*LLC* across all columns of Table 3 indicate that the reform has a negative effect on the propensity for firms to get liquidated, even after controlling for aggregate economic conditions and idiosyncratic firm characteristics. In the Online Appendix, we show that the treatment effect is also robust to the inclusion of location fixed effects. Overall, the stability of the coefficient estimates across the table columns illustrates the robustness of the treatment effect to the specification choice; the treatment effect estimates range between -0.06 and -0.10 across columns 1 to 4.

In addition to full sample regression analysis, we also conduct propensity score analysis. This analysis helps ensure that our findings in Table 3 are not explained by differences between limited liability firms and sole proprietorships that might otherwise explain changes in liquidation rates following the reform (such as differential trends in liquidation rates across firms of different sizes). We summarize the procedure and the results here and provide a more detailed description of the procedure and the matching statistics in the Appendix (Section E).

The first step in the propensity score matching analysis is using nearest-neighbor matching to pair each limited liability firm with the most similar sole proprietorship in the sample, based on probit models of firms' total assets and workforce sizes over the 2007-2009 period (Panel A of Table E1 contains the probit results). We require that successful matches fall in the common support of estimated propensity scores; we otherwise exclude sample firms for which we are unable to identify close matches (Panel B of Table E1 and Table E2 present matched sample statistics).

The second step of the analysis is to use the matched sample to estimate a linear probability model of corporate liquidations with the explanatory variables in Specification 1.[13] The treatment effect coefficient estimates are presented in Table E3 (all other coefficient estimates are presented in the Online Appendix to this paper). The results are very similar to the findings presented in Table 3: the treatment effect is negative and statistically significant across columns. The results illustrate that even when we analyze matched-firm samples, the reform is estimated to have a negative effect on liquidation rates.

Collectively, the raw data depicted in Fig. 1 and the regression estimates presented in Table 3 and Table E3 show that the Danish reform reduced limited liability liquidations. The findings are consistent with our hypothesis that an increase in creditor control rights during bankruptcy reorganization leads to an overall reduction in the total number of corporate liquidations observed in the economy. Although the estimates in Table 2 indicate that very few insolvent firms use the new reconstruction procedure to restructure—consistent with a small ex-post effect of the reform—the results in Fig. 1

[13] Because the matched sample has significantly fewer observations than the full sample (and in particular, significantly fewer liquidation events), OLS is more appropriate than probit analysis in this setting (Amemiya, 1981).

and Table 3 and Table E3 imply that the ex-ante incentive effects of the reform are significant, as the reform is estimated to have a large negative impact on corporate liquidation rates overall. The findings thus illustrate that solely focusing on the ex-post effects of creditor control rights during bankruptcy reorganization can lead to wildly misleading views of the full effects of these control allocations on liquidation rates.

4.1.2. Heterogeneity across firms

The full sample estimates presented in columns 1 through 4 of Table 3 encompass rich heterogeneity in the effects of the reform on Danish firms. To examine the impact of the reform across firms of varying workforce sizes, we present probit estimates for Specification 1 across firms that are either below or above the sample median workforce size. The results depicted in columns 5 and 6 of Table 3 indicate that the reform appears to be especially impactful for firms with large workforces. Similar results using propensity score matching analysis are depicted in columns 5 and 6 of Table E3. The results suggest that the reform is indeed helpful in curtailing unemployment driven by liquidations, as the firms with the largest workforces show the greatest reductions in liquidation risk after the law change.

We also estimate Specification 1 for firms operating in different industries. For some industries, there are too few liquidation events to estimate probit coefficients. Thus, we present coefficient estimates for *Reform*LLC* using a linear probability model. Fig. 3 depicts these estimates by industry and illustrates that the reform has a negative impact on liquidation rates in high employment sectors such as manufacturing, information and communications, knowledge-based services, and trade and transportation. The point estimates in other industries such as construction, real estate, and arts and entertainment are also negative in magnitude but statistically indistinguishable from zero given the large standard error bands.

4.2. Debt repayment

Given the empirical importance of the Danish reform's ex-ante incentive effects on liquidation rates, we analyze solvent firm behavior in further detail to reveal the reform's underlying mechanisms. Our hypothesis predicts that creditor control during reorganization leads solvent firms to take actions that reduce the risk of entering financial distress. We test this prediction by analyzing solvent firm debt repayment patterns from Experian and estimate the following OLS regression specification:

$$Debt\ repayment\ fraction_{it} = \beta_1 * Reform_t + \beta_c * Controls_{it} + e, \quad (2)$$

where the dependent variable, $Debt\ repayment\ fraction_{it}$, is the percentage of outstanding loans (in total and by amount) that are paid on time by firm *i* in year *t*.

We estimate Specification 2 for all limited liability firms, as Experian does not cover sole proprietorships. We then separately estimate Specification 2 for firms with a single owner and for firms with dispersed equity ownership. The controls in Specification 2 correspond to the same industry and firm level controls used in Specification 1. The only exception is workforce size since Experian's measures of employment consist of indicators of different workforce sizes (e.g., *Employees 1-9;* see Appendix Sections C and D for more details).

Table 4 presents regression results using the fraction of total outstanding loans paid on time as the dependent variable. Column 1 indicates that the fraction of outstanding debts paid on time increases by roughly three percentage points after the passage of the reform, relative to a base rate of debt repayment of 75%. When we control for additional factors such as aggregate and idiosyncratic measures of performance, we observe similar estimates in columns 2 and 3.[14] Columns 4 and 5 show

[14] The Online Appendix shows that the results are also robust to controlling for regional differences in corporate debt repayment rates.

that the impact of the reform on outstanding debt payment is relevant both for firms with dispersed ownership and for firms with a single owner, though the coefficient estimates are larger for single-owner firms.[15]

We then estimate Specification 2 for different outstanding debt amounts, as defined by Experian (see Table C1 in the Appendix) to see whether the debt repayment behavior that we observe in Table 4 is relevant across debt positions of various sizes. We estimate Specification 2 with the full set of controls for each type of debt amount reported in Experian, and we present the regression coefficients and standard error bands for *Reform* in Fig. 4. The figure illustrates that the reform has a positive impact on loan repayment rates across different debt amounts.

The results in Table 4 and Fig. 4 indicate that the reform leads solvent firms to pay a higher fraction of their outstanding debt obligations on time. The evidence supports our hypothesis by showing that solvent firms take demonstrable actions to reduce the risk of entering financial distress after the reform is passed. The large effects observed for single-owner firms further supports our hypothesis because managers in these firms likely have the strongest incentives to shift value from equity to debt during bankruptcy, as they are more likely to internalize the equity surplus of their actions.

4.3. Credit supply

Our hypothesis predicts that the reform will trigger an expansion in credit supply because creditors can better protect themselves from debtor agency costs in reorganization. We study this prediction by examining changes in financial leverage and borrowing costs for solvent firms around the reform, as the equilibrium changes in these outcomes can help us identify any shifts in credit supply driven by the reform. Specifically, we estimate the following OLS regression specification:

[15] The difference in coefficients of 2.85 is statistically significant with a *p*-value less than 0.05.

$$Financial\ outcome_t = \beta_1 * Reform_t * LLC_{it} + \beta_r * Reform_t + \beta_l * LLC_{it} + \quad (3)$$

$$\beta_c * Controls_i + B_{rc} * Reform_t * Controls_{it} + B_{lc} * LLC_{it} * Controls_{it} + e,$$

where the dependent variable, $Financial\ outcome_{it}$, takes on one of two values.

The first financial outcome that we examine is the change in net financial leverage, which is defined as the firm-specific percentage change in long-term debt minus current assets divided by total assets of firm *i* in year *t*. The second financial outcome that we study is the average implied interest rate on debt, which is defined as the ratio of total interest payments to long-term debt for firm *i* in year *t*.[16] All other controls are the same as in Specification 1 to maintain consistency across tables, except we exclude the liability ratio, and we include financial leverage as a control when we examine interest rates on debt to assess how the cost of debt changes for a given level of leverage.

The regression results are presented in Table 5. In Panel A, the coefficient estimates of the interaction term *Reform*LLC* range from 0.61% to 3.4%, which indicates a relative increase of approximately 2.1% in financial leverage for limited liability firms around the reform. In Panel B, the coefficient estimates indicate that implied interest rates for a given amount of debt decrease for limited liability firms following the reform. The coefficient of the interaction term *Reform*LLC* ranges from -2.16% to -6.81%, implying a five percentage point reduction in the average implied interest rate on debt paid by solvent firms.

The results in Panels A and B are robust to including location fixed effects (shown in the Online Appendix). The results in Panel B are also robust to controlling for other measures of borrowing such as the liability ratio and the net leverage ratio

[16] Because we do not directly observe interest rates on debt in our data, we approximate these values using observed interest payments divided by outstanding debt amounts. This measure of interest rates is inherently noisy and subject to measurement error and likely attenuates our treatment effect estimates.

(these results are presented in the Online Appendix). Finally, we supplement our regression analysis with propensity score matching analysis, and we document even stronger results in Table E4.

The evidence shows that firms hold greater leverage, and pay lower interest rates for a given amount of leverage, after the reform. The evidence is consistent with our hypothesis' predicted increase in credit supply following the reform. Although debtors may reduce their demand for credit following the reform (ceteris paribus), the observed net increase in leverage, and lower interest rates conditional on leverage, suggest that creditors' willingness to offer credit on more generous terms outweighs any potential reductions in credit demand.[17]

4.4. Real investment

We measure the Danish reform's impact on corporate investment by estimating the following regression specification:

$$Investment_{it} = \beta_1 * Reform_t * LLC_{it} + \beta_r * Reform_t + \beta_l * LLC_{it} + \beta_c * Controls_{it} + \quad (4)$$
$$B_{rc} * Reform_t * Controls_{it} + B_{lc} * LLC_{it} * Controls_{it} + e,$$

where the dependent variable, $Investment_{it}$, is defined as either the annual change in employment (Panel A) or the change in the logarithm of physical capital stock scaled by 100 (Panel B) for firm *i* as of year *t*.[18] All controls are defined in the same way as in Specification 1 to maintain consistency across tables (though we exclude workforce size from both panels and total assets from Panel B to avoid collinearity with the

[17] See Vig (2013) for an example of Indian firms reducing their demand for credit in response to an increase in creditor control rights during liquidation. The results from this study and our analysis illustrate that the net impact of creditor control rights on equilibrium borrowing will depend on the procedures being examined as well as other context-dependent factors such as the level of financial development and the ease of contracting between debtors and creditors.

[18] This measure of investment is imperfect since it does not account for depreciation expenses; however, it is a useful approximation for investment in physical capital, as capital expenditures are not explicitly recorded by Statistics Denmark.

dependent variables). It is worth noting that Specification 4 implicitly accounts for firm-level heterogeneity in employment and investment because the dependent variables are computed as firm-specific changes over time.

The results are presented in Table 6. In Panel A, the statistically significant coefficient on the interaction term *Reform*LLC* ranges between 0.26 and 0.55 across all columns. In Panel B, the weakly significant coefficient on the interaction term *Reform*LLC* ranges between 3.61 and 4.27 across all columns. The estimates indicate that limited liability firms increase their employment and physical capital stock after the passage of the reform relative to sole proprietorships. The results in both panels are robust to controls for various characteristics such as industry growth, firm performance, and location fixed effects. Additionally, when we examine employment and investment using propensity score matching analysis, as shown in Table E5, we find similar results (similar to Table 6, the results are stronger for employment than physical capital). These findings support the view that firms increase investment in labor and physical capital because of the net reductions in borrowing costs triggered by the reform.

4.5. Governance

According to our hypothesis, increased creditor control rights during reorganization reduce the ability of debtors to benefit from opportunistic actions taken by managers on their behalf during bankruptcy. Our hypothesis thus suggests that debtors have incentive to ensure that managers take stronger actions to avoid financial distress. One way in which debtors can influence managers is to monitor and dismiss poorly performing executives. To explore this hypothesis, we examine Experian data on executive turnover and estimate the following probit specification[19]:

[19] We report that our results are also robust to alternative statistical models such as logit and linear probability models.

$$Pr(Managerial\ turnover_{it} = 1) = \phi[\beta_1 \times Reform_t + \beta \times Controls_{it} + e], \quad (5)$$

where the dependent variable, *Managerial turnover*$_{it}$, is a binary indicator for whether firm *i* experiences managerial turnover in year *t*. We observe executive turnover across several different managerial positions within the firm, such as chief executive officer, chairman of the board, and plant manager. All controls are defined the same way as in Specification 2.

The results are presented in Table 7. Columns 1 through 3 depict the impact of the reform on CEO turnover. The coefficient on *Reform* is positive and significant across the different specifications controlling for various firm characteristics. The results indicate that sample firms experience a higher likelihood of chief executive turnover following the passage of the reform. Columns 4 through 6 indicate that other executives within the firm, such as plant managers and directors, also experience higher frequencies of turnover following the reform.

The findings suggest that one channel by which debtors are able to influence managerial behavior and avoid financial distress is through dismissal. Under Danish corporate law, managers have a fiduciary responsibility toward shareholders during solvency, so debtors have a legal means by which they can replace managers who may take excessive risks with the firm's assets. The evidence in Table 7 suggests that debtors engage with management to alter solvent firm decision-making to be in line with their new incentives following the reform.

4.6. Alternative explanations

We conduct a number of analyses to test alternative explanations for our main findings. Specifically, we assess the likely importance of violations of our identification assumption in the form of unobservable investment opportunities, sample selection criteria, and political economy forces surrounding the reform.

4.6.1. Unobservable investment opportunities

One important concern for our analysis is the extent to which our findings reflect changes in unobservable investment opportunities around the time of the reform. Like many other countries, Denmark's economy improved in 2010 following the global financial crisis, and this improvement alone likely mitigated the frequency of bankruptcy events across the economy. Creditors may have therefore found it easier to avoid liquidation irrespective of their ability to manage insolvent firm assets during reconstruction.

There are numerous pieces of evidence that indicate that changes in unobservable investment opportunities are unlikely to fully account for our empirical findings. First, as seen in Fig. 1, the observed changes in liquidation events in our data occur precisely in the immediate months surrounding the implementation of the law. It is unlikely that economic conditions dramatically improved in a similarly discrete manner. Moreover, the reduction in liquidation probabilities is relevant only for LLC's covered by the reform; liquidations of non-LLC's actually increase following the passage of the reform. Improvements in general economic conditions are difficult to reconcile with the contrasting trends in liquidations for LLC's versus non-LLC's.

Second, our regression analysis explicitly controls for lagged industry-specific GDP growth (Tables 3-7). The estimated impact of the reform on bankruptcy probabilities is robust to the inclusion of these controls and is also robust to controls for other firm characteristics that are likely to highly covary with unobservable investment opportunities, such as operating performance and location fixed effects.

Third, the effects that we observe in the full sample do not pertain to firms in the construction and real estate sectors, as illustrated in Fig. 3. These industries were two of the sectors that were most subject to changes in economic conditions during the

financial crisis, so if firms in these industries exhibited marked improvements in liquidation rates, then one might safely assume it was because these firms were benefiting from improvements in their industry conditions following the nadir of the crisis. The absence of such effects suggests that the main effects that we document are not driven by improvements in sectors that were most subject to economic recovery around the passage of the reform.

4.6.2. Sample selection biases

A second important concern is assessing the extent to which our regression estimates might be influenced by sample selection bias. If the Danish Bankruptcy Act coincided with a reduction in firm creation rates, then it is possible that the observed reduction in liquidations is simply due to a mechanical reduction in the number of new firms that are formed after the passage of the reform.

There are two pieces of evidence that reject this alternative hypothesis. First, when we reestimate Specification 1 and restrict our sample to firms that are incorporated before the passage of the reform (thereby eliminating new firms that are created after the reform), we observe similar results as our full sample estimates. These results are presented in Table 8: the coefficient on the interaction term for *Reform*LLC* is negative and statistically significant across all columns.

Second, in unreported analysis, we observe no significant reduction in the number of new limited liability firms that enter the sample around the passage of the reform. Instead, we observe a slight increase in the number of limited liability firms that are created, consistent with our earlier findings that the reform lowers the cost of capital for firms who raise debt financing. These results suggest that our findings are unlikely to simply reflect changes in firm composition during the sample period.

4.6.3. Political economy of the Danish bankruptcy reform

As described in Section 2 and illustrated in official excerpts from parliamentary debate surrounding the Danish bankruptcy reform (see Appendix, Section B), changes to the Danish Bankruptcy Act were broadly supported by all the major political parties in Denmark. The unanimous support for the law appears to have been driven by the collective view that the rate of corporate liquidations in Denmark was unsustainably high in 2010 and reforms were needed to combat the liquidations of insolvent but otherwise viable businesses. There is little evidence to suggest that the reforms were motivated by market participants attempting regulatory capture; such behavior would likely result in disjointed political support for the law. Moreover, to the extent that different industries have varying strengths of ties to specific political parties, the unanimous support for the reform suggests that political favoritism toward any one particular industry within Denmark is unlikely to be relevant in our setting.

4.7. Relative magnitudes of ex-ante versus ex-post effects of the reorganization reform

We provide a back-of-the-envelope calculation of the relative magnitudes of the ex-ante incentive effects versus the ex-post incentive effects of the Danish Reform on corporate liquidation rates. We begin by estimating Specification 1 with the full set of controls (but no indicator for *Reform*) using Experian data for limited liability firms prior to the passage of the reform in 2010. The regression estimates capture the marginal effects of various firm characteristics on the probability of liquidation prior to the reform.

We then use the estimated coefficients to predict the number of liquidations that would have taken place in the absence of the reform by multiplying the estimated coefficients by the respective postperiod sample values for the explanatory variables. The difference between the predicted number of liquidations and the observed number

of liquidations following the reform provide an estimate of the net effects of the law, expressed in terms of the number of firms that were saved from liquidation.

We decompose these estimated net effects into ex-ante and ex-post incentive effects by treating the observed number of firms that enter reconstruction as an (upper bound) estimate of the law's ex-post effects. The underlying assumption is that all of the firms in reconstruction would have become liquidated under the old regime. The difference between the estimated net effects of the reform and these ex-post incentive effects of the reform provide a (lower-bound) measure of the law's ex-ante incentive effects.

The relevant figures used in our calculations are provided in Table 9. Panel A presents the regression coefficients from the baseline model specification estimated using limited liability firms in 2009. Based on these coefficients, Panel B shows that the predicted number of firms that would have entered into liquidation in 2012 is approximately 2,074.[20] The actual number of observed liquidations in 2012 was 1,731, implying that the net effect of the reform was a reduction of 343 liquidations. Given that there were 83 reorganizations in 2012, the ratio of the ex-ante incentive effects (260 fewer firms entering bankruptcy and becoming liquidated) divided by the ex-post incentive effects (83 fewer liquidations due to reconstruction) is 3.13. Our estimates thus suggest that the ex-ante incentive effects of the reform are at least three times larger than their ex-post incentive effects.

5. Conclusion

This paper presents the first causal estimates of the ex-ante incentive effects of creditor control rights during bankruptcy reorganization. Denmark's 2010 reform to its

[20] We limit the postsample analysis to 2012 to minimize measurement error in our calculations, as the reform became effective in April, 2011.

bankruptcy laws, which empowered creditors to implement their own restructuring plans and remove managers, led to a significant decline in corporate liquidations. Even though few insolvent firms used the newly established reorganization procedures, solvent firms improved their debt repayment patterns dramatically following the reform. The findings show that characterizing the impact of bankruptcy rules solely by their ex-post effects—a perspective frequently heard in debates about insolvency codes around the world—can be wildly misleading. Instead, our results show that reducing debtor agency costs during reorganization can significantly alter solvent firm behavior and trigger an overall expansion of credit supply.

Our paper studies a critical issue in corporate governance: the assignment of control rights between creditors and debtors during bankruptcy reorganization. Our paper is distinct from a large literature that examines creditor control rights during liquidation. The findings we document highlight various issues related to creditor control during reorganization that require further study. For example, the impact of creditor control rights likely depends on unique characteristics of the financial markets in which firms operate; creditors operating in highly competitive sectors may behave differently from creditors that are able to earn monopoly rents. The impact of creditor control rights during reorganization may also depend on their control rights during liquidation. Dissecting the full range of effects that stem from creditor control is an important topic for future research and policy considerations.

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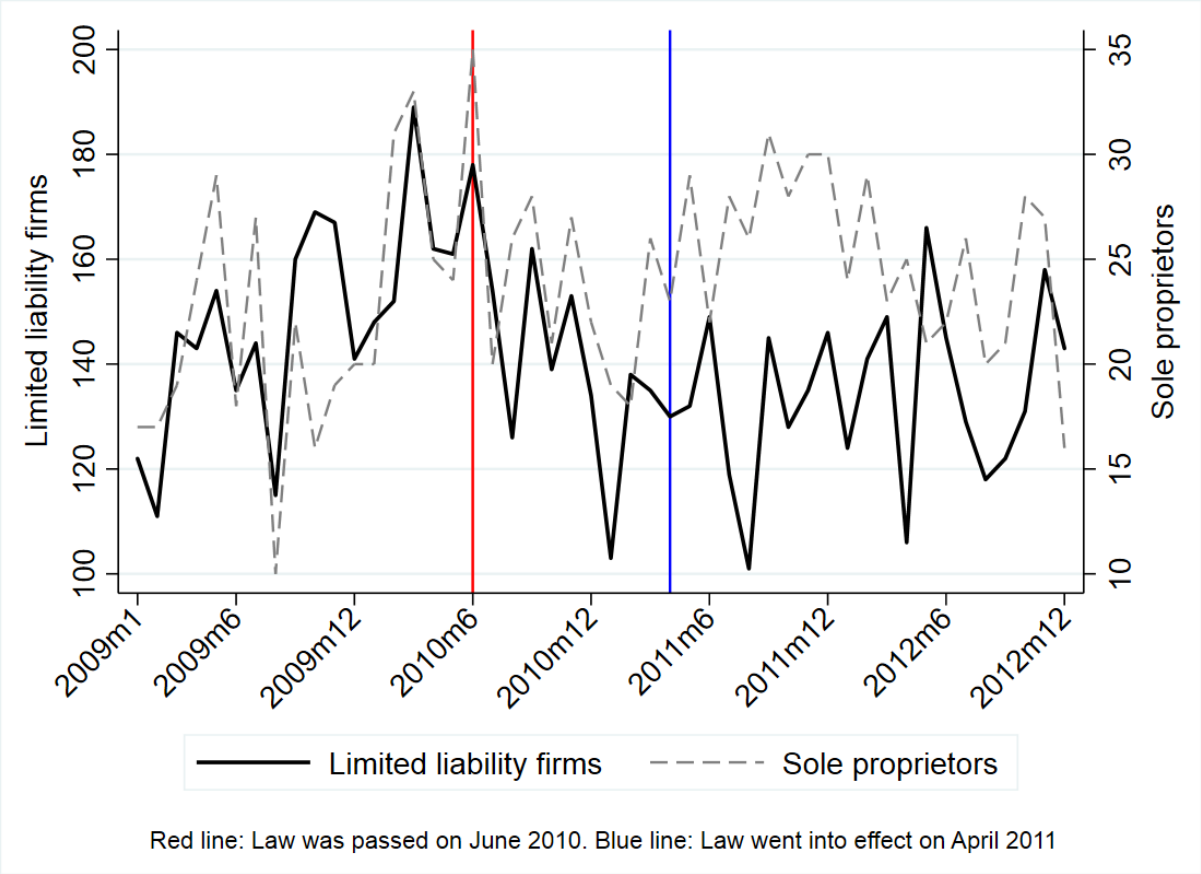
Figures and Tables

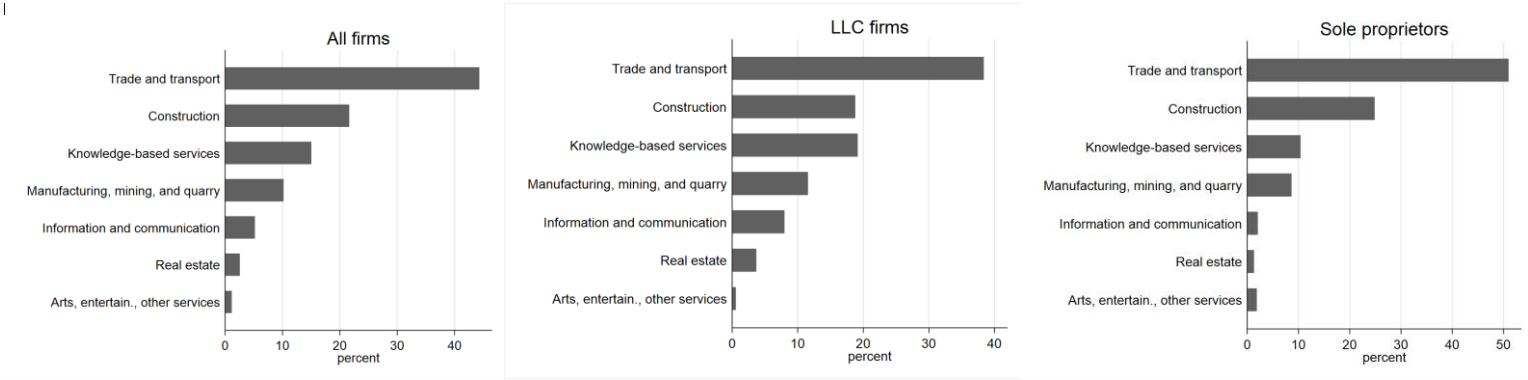
Fig. 1: Corporate liquidations around the bankruptcy reorganization reform



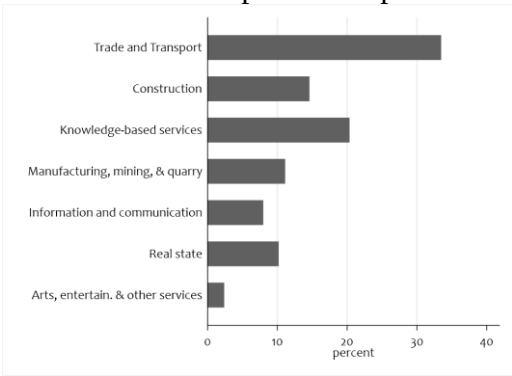
This figure depicts the number of liquidations of Danish firms with different incorporation statuses each month between 2009 and 2012. The Danish reorganization reform was passed by Parliament in June 2010 and went into effect in April 2011. The reform increased creditor control rights for limited liability firms; creditor control rights remained unchanged for sole proprietors. The sample consists of the universe of limited liability firms and sole proprietors available in the Statistics Denmark database.

Fig. 2: Industry distribution of sample firms

Panel A: Statistics Denmark sample

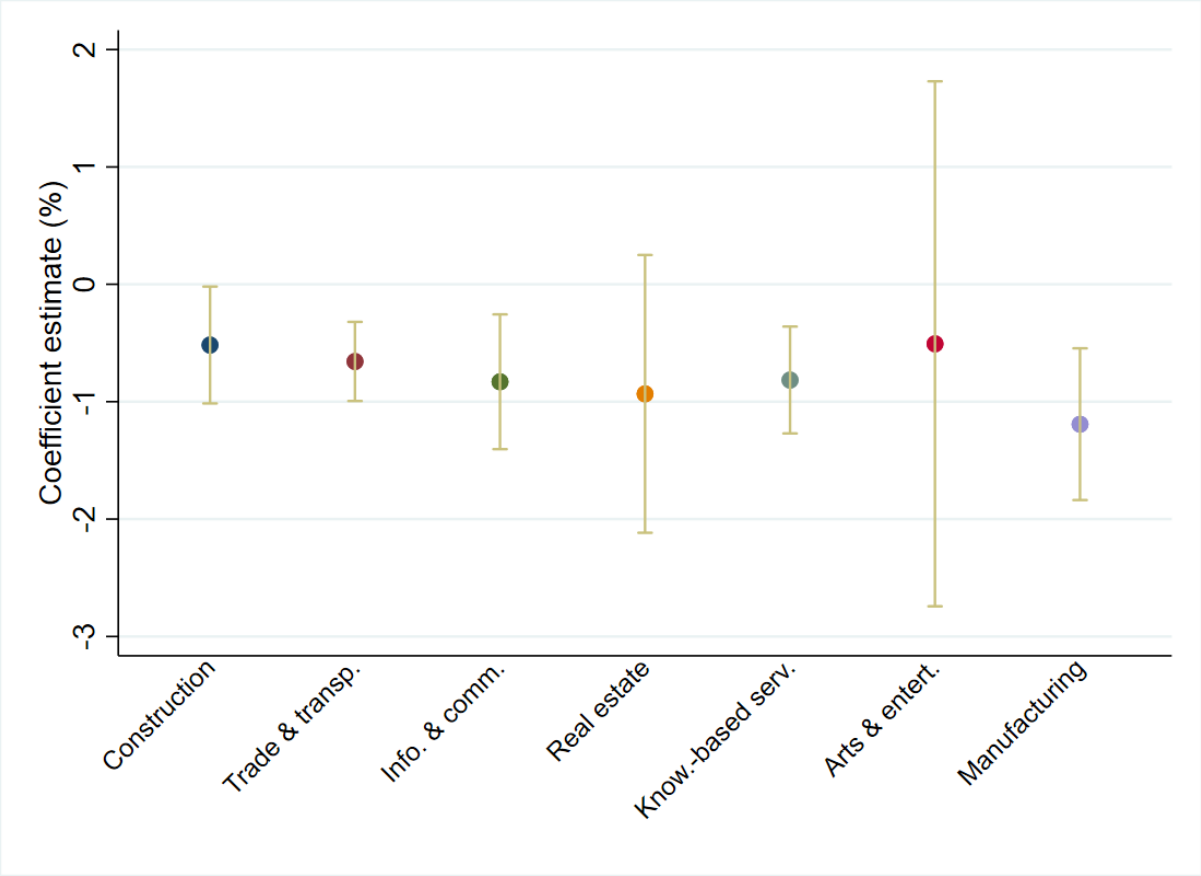


Panel B: Experian sample



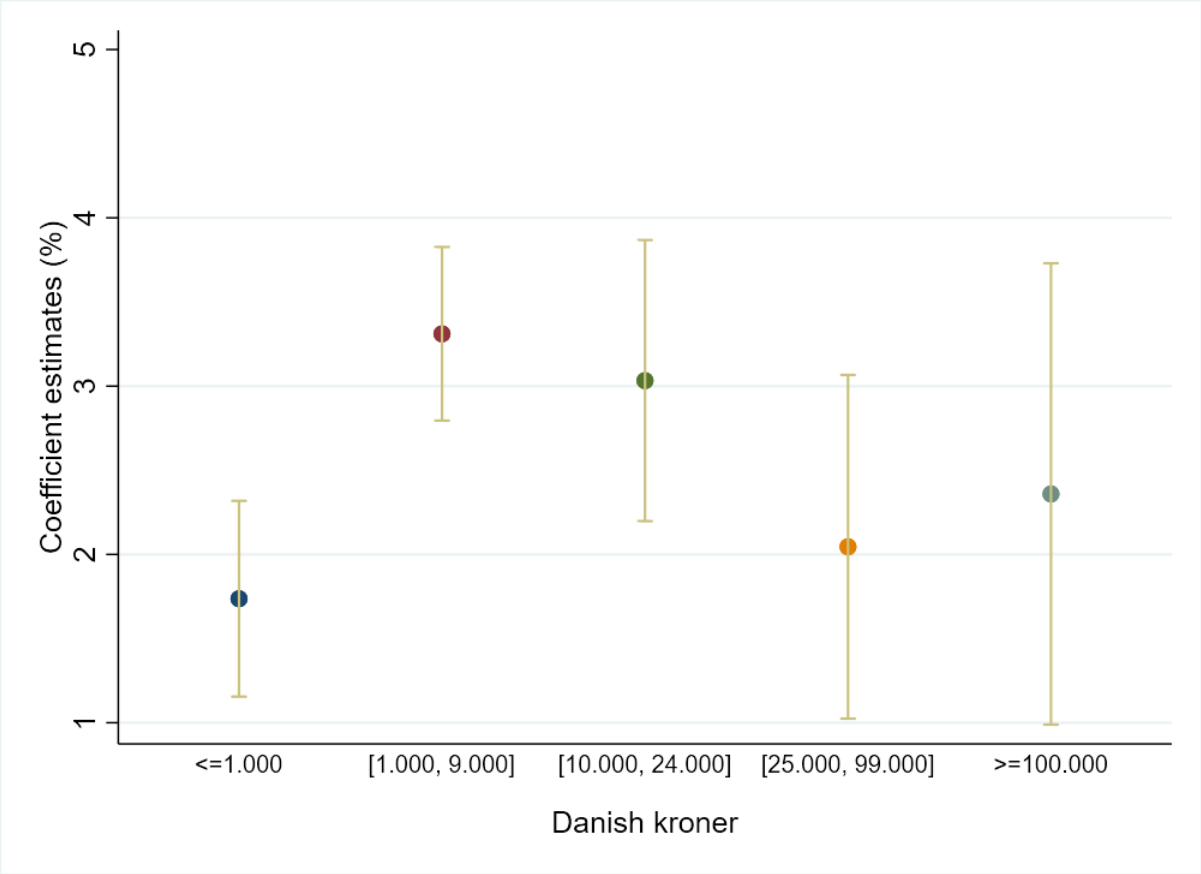
These figures show the industry distributions of sample firms during the 2009-2012 period, for the Statistics Denmark sample (Panel A) and the Experian sample (Panel B). The industry classification is the Dansk Branchekode 2007 (DB07), which is based on the European industry standard classification system NACE (Nomenclature des Activités Économiques dans la Communauté Européenne). Our sample includes information on firms in all industries except those in the following sectors: agriculture, finance and insurance, and public administration.

Fig. 3: Liquidation effects of the bankruptcy reorganization reform across industries



This figure presents the marginal effect estimates of the Danish bankruptcy reorganization reform on corporate liquidation probabilities across different industries, using a linear probability model (Specification 1). Confidence intervals (95%) are shown around each coefficient estimate; standard errors are heteroskedasticity robust and clustered at the firm level.

Fig. 4: Changes in debt repayment patterns
following the bankruptcy reorganization reform



This figure presents the coefficient estimates of the effects of the Danish bankruptcy reorganization reform on debt repayment rates (vertical axis) for limited liability firms across different outstanding debt sizes (horizontal axis), as per Specification 2. The sample includes all firms in the Experian database. Standard errors are heteroskedasticity robust and clustered at the firm level.

Table 1: Sample descriptive statistics

This table shows descriptive statistics of sample firms contained in the Statistics Denmark database. The sample consists of all firms tracked by Statistics Denmark during the 2009-2012 sample period. There are a total of 353,155 observations divided between 187,318 observations of limited liability firms and 165,837 observations of sole proprietors. There are a total of 132,070 unique firms, of which 59,565 are sole proprietors and 72,505 are limited liability firms. Monetary values are expressed in terms of thousands of Danish kroner (DKK). All variables are winsorized at the top and bottom 1% (the only exceptions are employees and firm age, which are not winsorized, and leverage, net leverage, and implied interest rates, which are winsorized at the top and bottom 2%). Variable definitions are presented in Table C1 in the Appendix.

	Full sample		Limited liability firms		Sole proprietors	
Variable name	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Revenue	6,273	12,437	10,698	20,105	2,132	2,686
Gross profit	2,755	4,799	4,578	7,943	1,087	1,208
Total assets	4,562	10,695	7,136	13,607	1,654	4,303
Fixed assets	1,863	5,358	3,261	10,314	843	1,741
Current assets	2,427	5,434	4,504	9,441	561	941
Total liabilities	2,974	6,765	5,220	12,088	1,049	1,752
Long term (LT) debt	623	1,743	1,066	3,217	278	612
Total equity	1,466	4,213	2,765	7,647	380	1,218
Revenue ratio (Revenue/Assets)	3.45	4.14	2.54	2.59	4.47	5.19
Profitability ratio (Gross profit/Assets)	1.87	2.46	1.25	1.32	2.57	3.16
Tangibility ratio (Fixed assets/Assets)	0.35	0.33	0.29	0.29	0.42	0.35
Liability ratio (Total liab./Assets)	0.97	1.06	0.81	0.76	1.15	1.29
Leverage ratio (LT debt /Assets)	0.20	0.27	0.14	0.16	0.28	0.36
Net leverage ratio (LT debt-current assets/Assets)	0.09	0.30	0.01	0.21	0.18	0.39
Equity ratio (Total equity/Assets)	0.05	1.02	0.23	0.67	-0.15	1.27
Implied interest rate (Interest payable/Long term debt)	0.63	1.44	0.74	1.82	0.53	1.09
Number of employees	5.32	64.88	8.90	88.88	1.27	3.18
Firm age	12.53	10.80	11.25	10.80	13.97	10.61

Table 2: Sample liquidations and bankruptcy reorganizations

This table shows the number of liquidations and bankruptcy reorganizations (reconstructions) during the 2009-2012 period. The table depicts the number of liquidations by incorporation status—limited liability or sole proprietorships—using administrative data from Statistics Denmark. The liquidation rate is calculated as the percentage of firms that are liquidated, out of the total number of firms observed in a given year with the same incorporation status. The data source for firms in reconstruction following the reform is the Danish Business Authority (DBA).

Sample	Liquidations		Liquidation rate		Reconstruction		
Firm type	Limited liability	Sole proprietors	Limited liability	Sole proprietors	Total	Limited liability	Sole proprietors
2009	1,790	254	3.53%	0.67%	0	0	0
2010	1,989	331	3.21%	0.64%	0	0	0
2011	1,671	335	2.69%	0.59%	87	87	0
2012	1,731	306	1.97%	0.42%	83	83	0

Table 3: Corporate liquidations and the bankruptcy reorganization reform

This table presents probit model estimates of the impact of the Danish bankruptcy reorganization reform on corporate liquidation probabilities. In columns 1-4, the sample includes all firms in the Statistics Denmark database over the 2009-2012 period. In columns 5-6, the sample consists of firms ranked by either below or above median employment size (as measured prereform in 2009). The dependent variable across all columns is an indicator of whether a given firm becomes liquidated in a given year. The variable ***Reform*** is an indicator of whether the observation is made following the passage of the reform in June 2010, and the variable *LLC* is an indicator of limited liability status. *Industry controls* refer to lagged year-to-year changes in GDP measured within an industry, and *Firm controls* include the following variables: liability ratio, revenue ratio, profitability ratio, log(total assets), log(employees), and firm age. "Yes" for *Industry controls* × *LLC* indicates that interactions between the *Industry controls* and the variable *LLC* are included in the regression. "Yes" for *Industry controls* × *Reform* indicates that interactions between the *Industry controls* and the variable *Reform* are included in the regression. "Yes" for *Firm controls* × *LLC* and *Firm controls* × *Reform* are defined analogously. We report the coefficients on *Reform* × *LLC* and also include the variables *Reform* and *LLC* in the specifications as well as control variables as specified in the bottom rows of each column. For brevity, the coefficients on these control variables are reported in the Online Appendix to this paper. Standard errors are heteroskedasticity robust and clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	All	All	All	< Median employees	> Median employees
Reform × LLC	-0.06**	-0.07**	-0.10***	-0.09***	-0.05	-0.13***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.07)	(0.05)
Obs.	353,155	353,145	353,145	353,145	129,869	141,042
Pseudo-R^2	0.045	0.046	0.116	0.116	0.166	0.089
Industry controls	No	Yes	Yes	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes	Yes	Yes
Industry controls × LLC	No	Yes	Yes	Yes	Yes	Yes
Firm controls × LLC	No	No	Yes	Yes	Yes	Yes
Industry controls × Reform	No	No	No	Yes	Yes	Yes
Firm controls × Reform	No	No	No	Yes	Yes	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Debt repayment patterns and the bankruptcy reorganization reform

This table presents linear regression estimates of the impact of the Danish bankruptcy reorganization reform on debt repayment patterns for limited liability firms. The sample in columns 1-3 consists of all limited liability firms in Experian; the sample in column 4 (5) consists of firms with a single owner (multiple owners). The dependent variable in the regression specifications is ***Repayment percentage***, which is the percentage of total outstanding loans that are paid on time by a firm in a given year. The variable $Reform$ is an indicator of whether the observation is made following the passage of the reform in June 2010. *Industry controls* refers to lagged year-to-year changes in GDP measured within an industry, and *Firm controls* include the following variables: liability ratio, revenue ratio, profitability ratio, log(total assets), firm age, and indicator variables for firm workforce sizes (*Employees 1-9, Employees 10-19, and Employees >20;* see Appendix A1*)*. We report the coefficient on *Reform*, and we include control variables in the regressions as specified in the bottom rows of each column. For brevity, the coefficients on these control variables are reported in the Online Appendix to this paper. Standard errors are heteroskedasticity robust and clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)
Sample	All	All	All	Single owner	Dispersed equity ownership
Reform	3.57***	3.29***	3.08***	4.56***	1.71**
	(0.225)	(0.234)	(0.237)	(0.655)	(0.777)
Obs.	89,388	84,363	83,291	12,737	7,147
R^2	0.002	0.002	0.013	0.017	0.024
Industry controls	No	Yes	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Financial policy effects of the bankruptcy reorganization reform

This table presents linear regression estimates of the impact of the Danish bankruptcy reorganization reform on firm financial policies. The dependent variable in Panel A is *Change in net leverage*, defined as the year-to-year change in long term debt minus current assets as a percentage of the firm's assets; in Panel B the dependent variable is the *Interest rate on debt*, defined as interest payments divided by total long-term debt outstanding. Both variables are scaled by 100. ***Reform*** is an indicator of whether the observation is made following the passage of the reform in June 2010, and the variable *LLC* is an indicator of limited liability status. *Industry controls* refer to lagged year-to-year changes in GDP within an industry, and *Firm controls* include the following variables: revenue ratio, profitability ratio, log(total assets), log(employees), and firm age; Panel B additionally includes the leverage ratio. "Yes" for *Industry controls* × *LLC* indicates that interactions between the *Industry controls* and the variable *LLC* are included in the regression. "Yes" for *Industry controls* × *Reform* indicates that interactions between the *Industry controls* and the variable *Reform* are included in the regression. "Yes" for *Firm controls* × *LLC* and *Firm controls* × *Reform* are defined analogously. We report the coefficients on *Reform* × *LLC* and also include the variables *Reform* and *LLC* in the specifications as well as control variables as specified in the bottom rows of each column. For brevity, the coefficients on these control variables are reported in the Online Appendix to this paper. Standard errors are heteroskedasticity robust and clustered at the firm level.

Panel A: Change in net leverage

	(1)	(2)	(3)	(4)
Reform × LLC	3.40***	2.62***	2.65***	0.61**
	(0.22)	(0.22)	(0.22)	(0.25)
Obs.	261,588	261,588	261,588	261,588
R^2	0.001	0.006	0.011	0.013
Industry controls	No	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes
Industry controls ×LLC	No	Yes	Yes	Yes
Firm controls ×LLC	No	No	Yes	Yes
Industry controls ×Reform	No	No	No	Yes
Firm controls ×Reform	No	No	No	Yes

Panel B: Interest rate on debt

	(1)	(2)	(3)	(4)
Reform × LLC	-6.13***	-4.32***	-2.16**	-6.81***
	(0.96)	(0.99)	(0.97)	(1.26)
Obs.	337,016	337,007	337,007	337,007
R^2	0.003	0.004	0.058	0.059
Industry controls	No	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes
Industry controls ×LLC	No	Yes	Yes	Yes
Firm controls ×LLC	No	No	Yes	Yes
Industry controls ×Reform	No	No	No	Yes
Firm controls ×Reform	No	No	No	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Employment and investment effects of the bankruptcy reorganization reform

This table presents linear regression estimates of the impact of the Danish bankruptcy reorganization reform on firm employment and capital investment. The dependent variable in Panel A is the year-to-year change in the number of employees for a given firm. The dependent variable in Panel B is the year-to-year change in the logarithm of physical capital stock for a given firm (scaled by 100). ***Reform*** is an indicator of whether the observation is made following the passage of the reform in June 2010, and the variable *LLC* is an indicator of firm limited liability status. The specification also include *Industry controls*, which refer to lagged year-to-year changes in GDP measured within an industry, and *Firm controls* include the following variables: liability ratio, revenue ratio, profitability ratio, log(total assets), and firm age (Panel B excludes the log of total assets). "Yes" for *Industry controls* × *LLC* indicates that interactions between the *Industry controls* and the variable *LLC* are included in the regression. "Yes" for *Industry controls* × *Reform* indicates that interactions between the *Industry controls* and the variable *Reform* are included in the regression. "Yes" for *Firm controls* × *LLC* and *Firm controls* × *Reform* are defined analogously. We report the coefficients on *Reform* × *LLC* and also include the variables *Reform* and *LLC* in the specifications as well as control variables as specified in the bottom rows of each column. For brevity, the coefficients on these control variables are reported in the Online Appendix to this paper. Standard errors are heteroskedasticity robust and clustered at the firm level.

Panel A: Employment

	(1)	(2)	(3)	(4)
Reform ×LLC	0.50***	0.55***	0.53***	0.26***
	(0.03)	(0.03)	(0.02)	(0.02)
Obs.	261,588	261,588	261,588	261,588
R^2	0.003	0.004	0.008	0.010
Industry controls	No	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes
Industry controls × LLC	No	Yes	Yes	Yes
Firm controls × LLC	No	No	Yes	Yes
Industry controls × Reform	No	No	No	Yes
Firm controls × Reform	No	No	No	Yes

Panel B: Capital investment

	(1)	(2)	(3)	(4)
Reform ×LLC	3.61*	3.84*	3.73*	4.26*
	(2.15)	(2.16)	(2.14)	(2.21)
Obs.	261,588	261,588	261,588	261,588
R^2	0.000	0.000	0.015	0.015
Industry controls	No	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes
Industry controls × LLC	No	Yes	Yes	Yes
Firm controls × LLC	No	No	Yes	Yes
Industry controls × Reform	No	No	No	Yes
Firm controls × Reform	No	No	No	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Managerial turnover and the bankruptcy reorganization reform

This table presents probit model estimates of the impact of the Danish bankruptcy reorganization reform on management and board turnover for limited liability firms. The dependent variable in each column is an indicator variable for whether a firm experiences turnover for a specific position in a given year. The variable $Reform$ is an indicator of whether the observation is made following the passage of the reform in June 2010. *Industry controls* refer to lagged year-to-year changes in GDP measured within an industry, and *Firm controls* include the following variables: liability ratio, revenue ratio, profitability ratio, log(total assets), firm age, and indicator variables for firms with different workforce sizes (*Employees 1-9, Employees 10-19, and Employees >20;* see Appendix A1). We report the coefficients on *Reform* and also include control variables as specified in the bottom rows of each column. For brevity, the coefficients on these control variables are reported in the Online Appendix to this paper. Standard errors are heteroskedasticity robust and clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)	(6)
	CEO turnover	CEO turnover	CEO turnover	Plant manager turnover	Chairman of the board turnover	Director turnover
Reform	0.05***	0.05***	0.06***	0.04***	0.02	0.03*
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)
Obs.	314,657	293,225	275,845	275,845	275,845	275,845
Pseudo-R^2	0.00	0.00	0.03	0.01	0.05	0.03
Industry controls	No	Yes	Yes	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes	Yes	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Liquidation effects of the bankruptcy reorganization reform on firms already operating prior to the reform

This table presents probit model estimates of the impact of the 2010 Danish bankruptcy restructuring reform on corporate liquidation probabilities for firms that were already incorporated prior to the reform (i.e., prior to June 2010). In columns 1-4, the sample includes all firms in the Statistics Denmark database over the 2009-2012 period. In columns 5-6, the sample consists of firms ranked by either below or above median employment size (as measured prereform in 2009). The dependent variable across all columns is an indicator of whether a given firm becomes liquidated in a given year. The variable ***Reform*** is an indicator of whether the observation is made following the passage of the reform in June 2010, and the variable *LLC* is an indicator of limited liability status. *Industry controls* refer to lagged year-to-year changes in GDP measured within an industry, and *Firm controls* include the following variables: liability ratio, revenue ratio, profitability ratio, log(total assets), log(employees), and firm age. "Yes" for *Industry controls* × *LLC* indicates that interactions between the *Industry controls* and the variable *LLC* are included in the regression. "Yes" for *Industry controls* × *Reform* indicates that interactions between the *Industry controls* and the variable *Reform* are included in the regression. "Yes" for *Firm controls* × *LLC* and *Firm controls* × *Reform* are defined analogously. We report the coefficients on *Reform* × *LLC* and also include the variables *Reform* and *LLC* in the specifications as well as control variables as specified in the bottom rows of each column. For brevity, the coefficients on these control variables are reported in the Online Appendix to this paper. Standard errors are heteroskedasticity robust and clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	All	All	All	< Median employees	> Median employees
Reform × LLC	-0.14***	-0.17***	-0.17***	-0.17***	-0.05	-0.13***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.07)	(0.05)
Obs.	320,681	320,672	320,672	320,672	129,869	141,042
R^2	0.046	0.048	0.124	0.125	0.166	0.089
Industry controls	No	Yes	Yes	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes	Yes	Yes
Industry controls × LLC	No	Yes	Yes	Yes	Yes	Yes
Firm controls × LLC	No	No	Yes	Yes	Yes	Yes
Industry controls × Reform	No	No	No	Yes	Yes	Yes
Firm controls × Reform	No	No	No	Yes	Yes	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Comparison of ex-ante and ex-post effects of the reorganization reform

This table presents estimates of the relative magnitudes of the ex-ante and ex-post incentive effects of the 2010 Danish reorganization reform. Panel A presents coefficient estimates from a probit model of liquidation outcomes estimated using our sample of limited liability firms in 2009. Standard errors are clustered at the firm level.*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Panel B depicts the number of liquidations that would be predicted by the model estimates in Panel A in 2012 using control variable values from 2012 as well as the actual number of liquidations and observed reorganizations in 2012.

Panel A: Liquidation probit model estimates

Liquidation probability	(1)
Liability ratio	0.17***
	(0.00)
ΔGDP sector	-0.75***
	(0.14)
Log (Total assets)	0.01***
	(0.00)
Revenue ratio	-0.00
	(0.02)
Profitability ratio	0.00*
	(0.00)
Employees 1-9	0.13***
	(0.02)
Employees 10-19	0.21***
	(0.03)
Employees >20	0.14***
	(0.03)
Firm age	-0.01***
	(0.00)
Constant	-2.21***
	(0.04)
Obs.	181,197
R^2	0.0451
Sample	Limited liability firms (2009)

Panel B: Ex-Ante and Ex-Post Effect Estimates

Year	Predicted liquidations	Actual liquidations	Predicted – actual (liquidations)	Reorganizations	Ratio of ex-ante / ex-post effects
2012	2,074	1,731	343	83	3.13

Appendix

A. Description of Denmark Insolvency Code

Danish insolvency law is governed by the Danish Bankruptcy Act, called Konkursloven, which was passed in 1977. The 1977 act replaced the existing insolvency code that had been first established in 1872. There are essentially two procedures in the Danish insolvency code: reorganization and liquidation (in Danish parlance, "liquidation" is often referred to as "bankruptcy"). Like many other countries, there have been numerous revisions to Denmark's procedures over the past 40 years. Perhaps the largest reform, however, was the 2010 amendment, which introduced a new procedure for bankruptcy reorganization called Rekonstruktion (Bang-Pedersen, 2017).

Prior to the 2010 reform, only management could petition for reorganization. Debt holders did not have the ability to file for restructuring or initiate reorganization plans without the consent of management (Gullitz-Wormslev and Levin, 2011; International Insolvency Institute, n.d.). The process of reorganization consisted of several steps. Management would typically first file for an automatic stay (in Danish parlance, a "suspension of payments"), that would last at most 12 months. Debt holders would not be allowed to take actions against the firm during this period, while management would continue to operate the firm. During those 12 months, management could propose a restructuring plan (in Danish parlance: "compulsory composition") that would need debt holder and court approval (Gullitz-Wormslev and Levin, 2011; International Insolvency Institute, n.d.).

If a restructuring plan was approved, then it would be binding across all stakeholders in the firm. Existing managers of the firm would continue to oversee the firm's operations but would have to report to a court-appointed administrator to ensure that the firm was abiding by any approved restructuring plan. In practice, debt holders rarely approved reorganization plans proposed by management. Instead, liquidation proceedings were much more common than reorganization.

The 2010 reform introduced Rekonstruktion, a reorganization procedure designed to help economically viable companies survive as going concerns during times of financial distress (Barfoed, 2010). In contrast to the prior code, a petition for Rekonstruktion can be filed by either the insolvent firm's management or by the insolvent firm's debt holders if the firm is an LLC. If the firm is not an LLC and the managers(s) are personally liable for any debts, such as the case for sole proprietorships, then only the managers (and not the debt holders) can file for restructuring (Bang-Pedersen, 2017). The legal condition of insolvency is established by the firm's inability to meet debt obligations on time.

As before, during the time that an insolvent firm is under restructuring proceedings, an automatic stay is typically issued to prevent debt holders from taking actions against the firm. Restructuring proceedings can last up to 12 months; during this time, debt holders and the court work to determine a viable restructuring plan. Debt holders vote on the plan under a variety of guidelines. Perhaps the most important guideline is that debt holders can vote for a plan that does not receive management approval; management consent is not required for the debt holders of limited liability firms to pass a court-enforceable reorganization plan.

Another consideration is that only debt holders that will be affected by the restructuring plan—that is, debt holders that can expect to receive some form of dividend from the firm—are eligible to cast votes. Additionally, secured creditors can only cast votes if the value of their secured debt is greater than the value of their collateral. Finally, a plan is approved as long as a majority (50%) of the outstanding creditor claims does not cast votes against the plan, where claims are determined by the relevant monetary amounts of debt due to each voting debt holder.

The court's primary role in approving the restructuring plan is to ensure that unsecured creditor interests are protected. The court does not have the power to implement its own restructuring plan unless creditors vote in support of it. Once a plan is approved, then the plan is binding for all stakeholders in the firm, including all debt holders that may not have participated in the court meeting as well as all debt holders that may have voted against the plan.

The execution of the restructuring plan is overseen by management. However, to ensure these plans are carried out properly and that debt holders are protected from abuse, management is supervised by a court-appointed administrator and is required to report all material information about the business to the court. Furthermore, debt holders are also given the right to replace management and have the firm's activities overseen by the administrator if deemed necessary. The decision to replace management is conditional upon a majority vote among debt holders (Bang-Pedersen, 2017).

If an insolvent firm does not enter into reorganization proceedings, then the firm can enter liquidation proceedings. The procedures for liquidation have largely stayed the same over time; the 2010 reforms did little to change these rules (Sjørslev and Højslet, 2018). Both before and after the reform, a petition to liquidate the firm could be filed by either the managers or the firm's debt holders. During liquidation proceedings, the management and the board of directors of the insolvent firm are relieved of their duties, and the court oversees the liquidation of the assets so as to maximize proceeds to debt holders in order of their priority and size of relevant claims.

B. Political views surrounding Danish bankruptcy reform of 2010

Political support for the Danish bankruptcy reform of 2010 was unanimous across the political spectrum of Denmark. In this section, we present excerpts of parliamentary debate prior to the voting and passage of the reform from Denmark's three largest political parties. The source of these excerpts is Section 8 of Consideration of Bill No. L 199: Proposal for a law amending the bankruptcy act and various other laws (reconstruction, etc.) by Justice Minister Lars Barfoed. The original Danish text is translated to English using Google Translate.

The proposal we are here with is actually a real, very good proposal. That's also considering the time we are currently in. Unfortunately, we are in the situation in Denmark that there have actually been no such bankruptcies in the past. We have to go back to 1979 before we reach the same number of bankruptcies among companies. It requires action. And from Ventre's side, we think that the government has come up with a good initiative, just this bill.

You have asked the Bankruptcy Council to come forward with some suggestions and constructive feedback on how this proposal can be designed so that we ensure [the survival of] companies as much as possible, but of course, with a reasonable balance, not just saying that a company should at all costs survive. However, you go in and look at certain types of companies. They give them an opportunity to make a reconstruction rather than closing and liquidating a company that might have a good production potential or a good service that is much needed and as the employees in the company, has a great know-how, so they might be able to move on.

- Irene Simonsen, spokesperson for Venstre, Denmark's Conservative-Liberal Party

Unfortunately, it is obvious to us all that the crisis is far from over and we see that a lot of companies still experience falling sales figures. In Denmark, we have not experienced in recent times as many companies succumb to, as we unfortunately see now, companies that usually work well, but who now have to turn the key and send their employees home to an uncertain future in the unemployment queue. Not long ago, it was announced that more than 650 companies had to shut down already here in March. It is 35 per cent more bankruptcies than at the same time in 2009 and the highest rate we have seen in Denmark for a long time…

However, we are also pleased that the Minister, with this bill, takes a positive step in the right direction, a small and delayed step, we think, but it is one step and we look forward to being laid to improve opportunities for economically-troubled companies to be continued in order to be declared bankrupt, for example, as proposed by the bill to introduce more reconstruction opportunities.

- Maja Panduro, spokesperson for Socialdemokratiet, Denmark's Social Democratic Party

If a company that is in financial difficulties is otherwise viable, it is a shame if the legislation and the rules we have today can only lead one place, namely to bankruptcy of the company, termination of the company, firing of employees. It would be advisable if, in the context of reconstruction – i.e. with a company that is in financial difficulties but otherwise it is viable and where a recruiter can be appointed - there is an opportunity to come up with a proposal for how to get the economy up so that the company can go on with what the company really is best at, for example. We therefore think that the proposal here is a very good proposal that tries to solve the situation that if you are in financial difficulties, the only option is such bankruptcy. It

should not be, and it should not be either. There should be opportunities there where there is hope that it may get better.

- Tom Behnke, spokesperson for Det Konservative Folkeparti, Denmark's Conservative People's Party

C. Variable definitions

Table C1: Variable definitions
This table presents the full list of all variables, along with their definitions, used in the analysis. Availability of the data item in Statistics Denmark (DST) and Experian is denoted in the last two columns.

Variable name	Definition	In DST?	In Experian?
Liquidation status	Indicator variable if company is liquidated the next year	Yes	Yes
Revenue	Sales	Yes	Yes
Operational costs	Operational costs	No	Yes
Gross profit	Revenue - Operational costs	Yes	Yes
Total assets (or Assets)	Fixed assets+ Current assets	Yes	Yes
Fixed assets	Fixed assets	Yes	Yes
Current assets	Current assets	Yes	Yes
Total liabilities	Short term debt +Long term debt+ Long term liabilities Providers	Yes	Yes
Short term debt	Short term debt	No	Yes
Long term debt	Long term debt	Yes	Yes
Long term liabilities providers	Long term liabilities providers	Yes	No
Interest rate of debt	Interest payments / Long term debt	Yes	No
Total equity	Total equity	Yes	Yes
Revenue ratio	Revenue/Total assets	Yes	Yes
Profitability ratio	Gross profit/Total assets	Yes	Yes
Tangibility ratio	Fixed assets/Total assets	Yes	Yes
Liability ratio	Total liabilities/ Total assets	Yes	Yes
Leverage ratio	Long term debt/Total assets	Yes	Yes
Net leverage ratio	(Long term debt - Current assets)/Total assets	Yes	Yes
Equity ratio	Total equity/Total assets	Yes	Yes
Number of employees	Number of full-time employees	Yes	No
Firm age	Years since incorporation	Yes	Yes
$\Delta\text{GDP sector}_{t-1}$	Lagged year-to-year change in sector GDP (1-digit NACE)	Yes	Yes
Repayment fraction	Fraction of the number of nonequity outstanding liabilities paid on time in the past 12 months	No	Yes
Repay value 0-1	Fraction of the number of nonequity liabilities with outstanding balance below 1K DKK paid on time in the past 12 months.	No	Yes
Repay value 1-9	Fraction of the number of nonequity debts with outstanding balance between 1K and 9K DKK paid on time in the past 12 months.	No	Yes
Repay value 10-24	Fraction of the number of nonequity debts with outstanding balance between 10K and 24K DKK paid on time in the past 12 months.	No	Yes
Repay value 25-99	Fraction of the number of nonequity debts with outstanding balance between 25K and 99K DKK paid on time in the past 12 months.	No	Yes
Repay value >100	Fraction of the number of nonequity liabilities with outstanding balance above 100K DKK paid on time in the past 12 months	No	Yes
Employees 0	Indicator variable for firms with 0 employees	No	Yes
Employees 1-9	Indicator variable for firms with 1-9 employees	No	Yes
Employees 10-19	Indicator variable for firms with 10-19 employees	No	Yes
Employees >20	Indicator variable for firms with more than 20 employees	No	Yes
Administrative director turnover	Indicator variable if administrative director leaves the firm	No	Yes
Plant manager turnover	Indicator variable if plant manager leaves the firm	No	Yes
Chair turnover	Indicator variable if chair leaves the board	No	Yes
Board turnover	Indicator variable if board member leaves the board	No	Yes

D. Experian sample summary

Table D1: Experian sample descriptive statistics

This table presents descriptive statistics for all sample firms contained in the Experian database during the 2009-2012 sample period. There are 142,551 unique limited liability (AS/APS) firms in the database (Experian does not maintain information for sole proprietorships). Monetary values are expressed in terms of thousands of Danish kroner (DKK). All variables are winsorized at the 1% level (aside from the employment indicator variables (e.g., Employees 0) and firm age). Variable definitions are presented in Table C1 in the Appendix.

Variable name	Obs.	Mean	Std. dev.
Revenue	313,160	7,636	24,512
Gross profit	298,181	4,549	12,636
Total assets	314,380	16,250	56,616
Fixed assets	274,568	9,252	37,817
Current assets	313,160	7,636	24,512
Total liabilities	314,357	20,823	634,267
Long term debt	117,205	7,473	25,724
Total equity	314,357	6,226	25,334
Revenue ratio (Revenue / Total assets)	311,399	0.68	0.33
Profitability ratio (Gross profit / Total assets)	296,425	0.63	0.82
Tangibility ratio (Fixed assets / Total assets)	273,480	0.36	0.32
Liability ratio (Total liabilities / Total assets)	312,197	0.97	2.05
Equity ratio (Total equity / Total assets)	312,197	0.12	1.42
Employees 0	314,657	0.33	0.47
Employees 1-9	314,657	0.49	0.50
Employees 10-19	314,657	0.09	0.28
Employees >20	314,657	0.10	0.29
Firm age	314,657	14.20	17.56
Debt repayment fraction	314,657	0.01	0.10
Administrative director turnover	314,657	0.03	0.16
Plant manager turnover	314,657	0.01	0.12
Chair turnover	314,657	0.02	0.15
Board turnover	89,388	0.75	0.31

E. Propensity score matching analysis: procedure description and results

We conduct propensity score matching analysis to supplement our full sample analysis of the impact of the Danish bankruptcy reorganization reform on firm outcomes. The main goal of the matching analysis is to remove any potentially meaningful differences (along observable dimensions) between LLC's and sole proprietorships that might otherwise explain differences in liquidation rates across firms (at the expense of lower statistical power). We employ well-known matching procedures to create matched samples of firms that are similar across observable dimensions, and we show that our main regression results hold for these matched samples.

We use the nearest-neighbor matching algorithm originally developed by Rosembaum and Rubin (1983) (see Roberts and Whited, 2012 for a summary). The first step in our propensity score matching analysis is to estimate a probit regression at the company level, where the dependent variable is a binary indicator of whether a particular company in the sample is an LLC or a sole proprietorship, and the explanatory variables include the logarithmic transformation of firm assets and the logarithmic transformation of the number of firm employees for the years 2007, 2008, and 2009—the three years immediately preceding the passage of the reform. The probit is estimated using the cross-section of companies that have nonmissing assets and employees for the 2007-2009 period; this sample includes 26,157 LLC's and 26,843 sole proprietorships. We present the coefficient estimates of the probit regression, the R^2, and the *p*-value of the chi-squared test in the first column of Panel A in Table E1 (we label this column "prematch"). The coefficients and statistics reveal that the independent variables explain a significant amount of variation in limited liability status (the R^2 of the regression is 0.257).

The second step in the procedure is to use the predicted probabilities from the probit estimation—that is, the propensity scores—to perform a single nearest-neighbor match with a replacement. That is, each LLC is paired with a sole proprietorship whose propensity score is closest, in an L^1-norm sense. We require that successful matches fall in the common support of estimated propensity scores. We also require that the difference in propensity scores between LLC firms and their matched sole proprietorships is less than 2.20×10^{-5}. Finally, following Smith and Todd (2005), we match sole proprietorships to LLCs with replacement (i.e., a given sole proprietorship may appear more than once in the sample) to improve the accuracy of the matches (though this comes at the expense of lower statistical power).

Panel B of Table E1 shows that these requirements result in 9,202 limited liability firms for which we are able to find a corresponding sole proprietorship match. The average number of LLC matches per sole proprietorship in the matched sample is 1.438; the median number of matches is 1, and the maximum is 8. To illustrate the similarities in firm characteristics between matched LLCs and sole proprietorships, we present the R^2 and the *p*-value of the chi-squared test of the probit model estimated on the matched sample in column 2 of Panel A. This column shows that, relative to the prematched column, the R^2 falls to 0 and the *p*-value of the chi-squared test dramatically increases to 0.223, implying that the null hypothesis that the coefficient estimates of the independent variables are jointly zero cannot be rejected. In other words, observable firm characteristics such as size and number of employees cannot otherwise explain differences in LLC and sole proprietorship firms in the matched sample. To support this conclusion further, column 2 in Panel B of Table E1 shows that the differences in the estimated propensity scores between matched limited liability firms and sole proprietorships are negligible. The mean difference between the matched propensity scores is 9.52×10^{-6}. Finally, we report summary statistics of the matched sample in Table E2 and show that various firm characteristics within the matched sample are similar across treatment and control groups.

We present our main regression results using the matched sample in Tables E3-E5. For example, Table E3 presents linear probability model estimates of liquidation rates following the reform using the matched sample. Table E4 examines changes in net financial leverage (Panel A) and implied interest rates (Panel B) following the reform using the matched sample. Table E5 examines changes in employees (Panel A) and changes in physical capital stock (Panel B) following the reform using the matched sample. For brevity, we report coefficient estimates for the main variables of interest in Tables E3-E5; we report the coefficient estimates for all control variables in the Online Appendix.

Table E1: Propensity score matching diagnostic tests

This table presents propensity score matching diagnostics. The first step in the matching process is a probit regression at the company level, of a binary indicator variable for whether a firm is a limited liability company (LLC) or sole proprietorship, regressed on the logarithm of the firm's assets and the logarithm of the firm's workforce size for the years 2007, 2008, and 2009. The probit is estimated on the cross section of companies that have no missing assets and employees in the 2007-2009 period. Panel A presents the coefficient estimates, and the R^2 and the *p*-value of the chi-squared test for joint parameter significance of the probit model under the column "prematch." The second step in the propensity score matching procedure is to use the predicted probability from the probit estimation, that is, the propensity score, to perform single nearest-neighbor matching with replacement. Panel B presents the distribution of differences in the estimated propensity scores between limited liability firms and matched sole proprietorships. The coefficient estimates, the R^2 and the *p*-value of the chi-squared test for joint parameter significance of the probit model estimated on the subsample of matched treatment and control observations after matching are presented in Panel A under the column "postmatch." Panel B presents descriptive statistics of the matched sample. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Probit regressions

	(1) Prematch	(2) Postmatch
Dependent variable	LLC	LLC
Log (Total assets 2007)	0.184**	0.007
	(0.019)	(0.027)
Log (Total assets 2008)	0.171**	0.021
	(0.022)	(0.033)
Log (Total assets 2009)	-0.056**	-0.039
	(0.017)	(0.024)
Log (Employees 2007)	0.237**	-0.067
	(0.033)	(0.044)
Log (Employees 2008)	-0.031	0.027
	(0.042)	(0.055)
Log (Employees 2009)	0.518**	0.054
	(0.029)	(0.038)
Constant	-2.980**	0.066
	(0.045)	(0.066)
Observations	42,457	18,404
Pseudo R-squared	0.257	0.000
Chi-squared p-value	0.000	0.233

Panel B: Matched sample statistics

Number of LLC firms	26,157
Number of matched LLC firms	9,202
Number of sole proprietorships	26,843
Number of matched sole proprietorships	6,398
Distribution of matches per sole proprietorship	
Mean	1.438
Standard deviation	0.747
Max	8
p50	1
p99	4
Sample differences in propensity scores	
Mean	$9.52 \times 10\text{-}6$
Standard deviation	$6.25 \times 10\text{-}6$

Table E2: Matching sample descriptive statistics

This table shows descriptive statistics of the matched sample. There are a total of 61,111 observations divided between 29,549 observations of limited liability companies and 31,562 observations of sole proprietorships. There are 15,600 unique firms, of which 9,202 are limited liability firms and 6,398 are sole proprietorships. Monetary values are expressed in terms of thousands of Danish kroner (DKK). All variables are winsorized at the top and bottom 1% (the exceptions are the log employees and age, which are not winsorized, and the variables: leverage ratio, net leverage ratio, and implied interest rate, which are winsorized at the top and bottom 2%). Variable definitions are presented in Table C1 in the Appendix.

	Full sample		Limited liability firms		Sole proprietors	
Variable name	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Liquidation status	0.01	0.11	0.02	0.14	0.01	0.09
Reform	0.70	0.46	0.69	0.46	0.71	0.45
Revenue	4,520	5,139	5,021	6,420	4,069	3,874
Gross profit	2,075	1,907	2,168	2,185	1,988	1,632
Total assets	3,395	6,279	3,453	6,655	3,339	5,905
Fixed assets	1,442	2,592	1,054	2,554	1,805	2,603
Current assets	1,583	2,207	2,028	2,633	1,143	1,560
Total liabilities	2,046	2,747	1,970	2,870	2,106	2,594
Long term (LT) debt	473	874	386	841	556	905
Total equity	1,020	2,128	1,153	2,219	884	1,993
Revenue ratio (Revenue/Total assets)	2.65	3.02	2.43	2.29	2.85	3.57
Profitability ratio (Gross profit/Assets)	1.39	1.75	1.19	1.15	1.58	2.16
Tangibility ratio (Fixed assets/Assets)	0.40	0.33	0.26	0.27	0.53	0.32
Liability ratio (Total liab./Total assets)	0.85	0.80	0.75	0.68	0.95	0.88
Leverage ratio (LT debt /Total assets)	0.19	0.22	0.13	0.14	0.24	0.27
Net leverage ratio (LT debt-current assets/Total assets)	0.08	0.26	-0.01	0.19	0.16	0.29
Equity ratio (Total equity/Total assets)	0.15	0.79	0.26	0.67	0.06	0.87
Implied interest rate (Interest payable/ Long term debt)	0.63	1.37	0.76	1.83	0.50	0.93
Number of employees	2.98	3.15	3.06	3.00	2.91	3.28
Firm age	16.40	10.22	13.22	9.68	19.36	9.81

Table E3: Corporate liquidations and the bankruptcy reorganization reform

This table presents linear probability model estimates of the impact of the 2010 Danish bankruptcy reorganization reform on corporate liquidation probabilities for the subset of limited liability companies (LLC) that are matched to sole proprietorships using the size of firm assets and workforces (prereform). The dependent variable is an indicator of whether a given firm becomes liquidated in a given year. The variable $Reform$ is an indicator of whether the observation is made following the passage of the reform in June 2010, and the variable *LLC* is an indicator of limited liability status. Industry controls refers to lagged ΔGDP sector, and firm controls includes the following variables: liability ratio, revenue ratio, profitability ratio, log(total assets), log(employees), and firm age. "Yes" for *Industry controls* × *LLC* indicates that interactions between the *Industry controls* and the variable *LLC* are included in the regression. "Yes" for *Industry controls* × *Reform* indicates that interactions between the *Industry controls* and the variable *Reform* are included in the regression. "Yes" for *Firm controls* × *LLC* and *Firm controls* × *Reform* are defined analogously. We report the coefficients on *Reform* × *LLC* and also include the variables *Reform* and *LLC* in the specifications as well as control variables as specified in the bottom rows of each column. For brevity, the coefficients on these control variables are reported in the Online Appendix. Standard errors are heteroskedasticity robust and clustered at the firm level.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	All	All	All	< Median employees	> Median employees
Reform × LLC	-0.55**	-0.72***	-0.62***	-0.46*	-0.37	-0.63*
	(0.23)	(0.24)	(0.23)	(0.25)	(0.38)	(0.33)
Observations	61,111	61,111	61,111	61,111	19,354	41,757
R^2	0.003	0.003	0.023	0.023	0.018	0.028
Industry controls	No	Yes	Yes	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes	Yes	Yes
Industry controls ×LLC	No	Yes	Yes	Yes	Yes	Yes
Firm controls ×LLC	No	No	Yes	Yes	Yes	Yes
Industry controls ×Reform	No	No	No	Yes	Yes	Yes
Firm controls ×Reform	No	No	No	Yes	Yes	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table E4: Financing effects of the bankruptcy reorganization reform

This table presents linear regression estimates of the impact of the 2010 Danish bankruptcy reorganization reform on firm financing for the subset of limited liability companies (LLC) that are matched to sole proprietorships using the size of firm assets and workforces (prereform). The dependent variable in Panel A is Change in net leverage, defined as the year-to-year change in long-term debt minus current assets as a percentage of the firm's assets; in Panel B the dependent variable is the Implied interest rate on debt, defined as interest payments as a percentage of long term debt. Both variables are scaled by 100. The variable $Reform$ is an indicator of whether the observation is made following the passage of the reform in June 2010, and the variable *LLC* is an indicator of limited liability status. Industry controls refer to lagged ΔGDP sector, and firm controls include the variables: revenue ratio, profitability ratio, log(total assets), log(employees), and firm age; Panel B additionally includes the leverage ratio. "Yes" for *Industry controls* × *LLC* indicates that interactions between the *Industry controls* and the variable *LLC* are included in the regression. "Yes" for *Industry controls* × *Reform* indicates that interactions between the *Industry controls* and the variable *Reform* are included in the regression. "Yes" for *Firm controls* × *LLC* and *Firm controls* × *Reform* are defined analogously. We report the coefficients on *Reform* × *LLC* and also include the variables *Reform* and *LLC* in the specifications as well as control variables as specified in the bottom rows of each column. For brevity, the coefficients on these control variables are reported in the Online Appendix. Standard errors are heteroskedasticity robust and clustered at the firm level.

Panel A: Change in net leverage

	(1)	(2)	(3)	(4)
Reform × LLC	3.60***	2.25***	2.11***	0.98**
	(0.41)	(0.40)	(0.40)	(0.41)
Observations	58,711	58,711	58,711	58,711
	0.002	0.009	0.012	0.015
Industry controls	Yes	Yes	Yes	Yes
Firm controls	No	Yes	No	Yes
Industry controls × LLC	Yes	Yes	Yes	Yes
Firm controls × LLC	No	Yes	No	Yes
Industry controls × Reform	No	No	Yes	Yes
Firm controls × Reform	No	No	No	Yes

Panel B: Interest rate on debt

	(1)	(2)	(3)	(4)
Reform × LLC	-10.76***	-8.24***	-11.12***	-20.48***
	(2.32)	(2.42)	(2.39)	(2.76)
Observations	59,362	59,362	59,362	59,362
	0.005	0.006	0.079	0.082
Industry controls	Yes	Yes	Yes	Yes
Firm controls	No	Yes	No	Yes
Industry controls × LLC	Yes	Yes	Yes	Yes
Firm controls × LLC	No	Yes	No	Yes
Industry controls × Reform	No	No	Yes	Yes
Firm controls × Reform	No	No	No	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table E5: Investment and the bankruptcy reorganization reform

This table presents linear regression estimates of the impact of the 2010 Danish bankruptcy reorganization reform on firm investment for the subset of the LLC firms that are matched to sole proprietorships by asset and employee size prereform. The dependent variable in Panel A is the year-to-year change in number of employees for a given firm. The dependent variable in Panel B is the year-to-year change in the logarithm of physical capital stock for a given firm (scaled by 100). The variable $Reform$ is an indicator of whether the observation is made following the passage of the reform in June 2010, and the variable *LLC* is an indicator of limited liability status. Industry controls refers to lagged ΔGDP sector, and firm controls includes the following variables in Panel A: liability ratio, revenue ratio, profitability ratio, log(assets), and firm age. In Panel B, the firm controls exclude the variable log(assets). "Yes" for *Industry controls* × *LLC* indicates that interactions between the *Industry controls* and the variable *LLC* are included in the regression. "Yes" for *Industry controls* × *Reform* indicates that interactions between the *Industry controls* and the variable *Reform* are included in the regression. "Yes" for *Firm controls* × *LLC* and *Firm controls* × *Reform* are defined analogously. We report the coefficients on *Reform* × *LLC* and also include the variables *Reform* and *LLC* in the specifications as well as control variables as specified in the bottom rows of each column. The coefficients on these control variables are not reported for the sake of brevity but are available in the Online Appendix. Standard errors are heteroskedasticity robust and clustered at the firm level.

Panel A: Employment

	(1)	(2)	(3)	(4)
Reform × LLC	0.08***	0.09***	0.09***	0.18***
	(0.03)	(0.03)	(0.03)	(0.03)
Observations	58,711	58,711	58,711	58,711
	0.022	0.024	0.035	0.050
Industry controls	Yes	Yes	Yes	Yes
Firm controls	No	Yes	No	Yes
Industry controls × LLC	Yes	Yes	Yes	Yes
Firm controls × LLC	No	Yes	No	Yes
Industry controls × Reform	No	No	Yes	Yes
Firm controls × Reform	No	No	No	Yes

Panel B: Capital investment

	(1)	(2)	(3)	(4)
Reform × LLC	3.27	5.16	3.55	2.74
	(4.18)	(4.22)	(4.14)	(4.41)
Observations	58,711	58,711	58,711	58,711
	0.001	0.001	0.026	0.026
Industry controls	Yes	Yes	Yes	Yes
Firm controls	No	Yes	No	Yes
Industry controls × LLC	Yes	Yes	Yes	Yes
Firm controls × LLC	No	Yes	No	Yes
Industry controls × Reform	No	No	Yes	Yes
Firm controls × Reform	No	No	No	Yes

*, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.