Essays in Bankruptcy and Financial Distress

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Essays in Bankruptcy and Financial Distress

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Department of Finance

A thesis presented for the degree of
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Copenhagen Business School

Donghyun Kang
Essays in Bankruptcy and Financial Distress

Executive abstract

Abstract

This dissertation is the final product of my PhD studies at the Department of Finance at Copenhagen Business School. The thesis consists of three chapters, which study how the bankruptcy institution—bankruptcy law and bankruptcy judges—affects the ex-post resolution of financial distress. The chapters are self-contained and can be read independently.

In the first chapter, "Wealth Protection in Bankruptcy and Serial Entrepreneurship," I study whether wealth protection in personal bankruptcy provides a second chance to failed entrepreneurs. I exploit windfall wealth from inheritances to proxy for exogenous variation in personal wealth after bankruptcy. Windfall wealth increases reentry to business *only* among entrepreneurs who did not experience severe losses in personal income or wealth before bankruptcy. Those who respond to windfall wealth by starting new businesses have lower profits, indicating their lower entrepreneurial quality. Overall, the findings suggest that bankruptcy policies increasing wealth protection can promote serial entrepreneurship, but their effectiveness is limited by low entrepreneurial quality and personal experience of severe losses.

In the second chapter, "Biased Judges? Judge Characteristics and Bankruptcy Outcomes," exploiting the random assignment of judges to corporate bankruptcy filings, I examine the effect of judge characteristics on outcomes. First, I find that cases assigned to judges who grew up during the Great Depression are more likely to emerge from bankruptcy, whereas those assigned to judges with economics training and conservative political ideology are more likely to result in liquidation. Second, I show that the case duration is shorter (longer) when the potential case outcome is consistent (inconsistent) with judges' preferences. Third, the judge characteristics do not correlate with post-emergence outcomes. Overall, the findings suggest that the effect of judge characteristics may be concentrated in marginal cases where the economic benefits of liquidation versus emergence are not significantly different.

In the third chapter, "Disqualifying Managerial Misconduct in Corporate Bankruptcy" (with S. Lakshmi Naaraayanan and Kasper Meisner Nielsen) we examine the introduction of bankruptcy quarantines that disqualifies managers engaging in negligent business practices for up to 3 years. Using administrative register data from Denmark, we document that disqualifications discourage future business activity: after the quarantine, individuals are 15% less likely to be managers or business owners. Disqualified individuals are also less likely to be involved in future bankruptcies or future criminal activities. At the same time, the fraction of family members of disqualified individuals who are active in a management role increases from 10% to 30%. We also find changes to the managerial labor pool, resulting in more CEOs with a criminal record and those relying solely on social transfers. Overall, our findings provide the first systematic evidence on the governance consequences of disqualifying managerial misconduct in corporate bankruptcies.

Resumé

Denne afhandling er det endelige produkt af mine ph.d.-studier ved Institut for Finansiering på Copenhagen Business School. Afhandlingen består af tre kapitler, der undersøger, hvordan konkursinstitutioner – konkurslovgivning og dommere – påvirker den efterfølgende løsning af økonomisk nød. Kapitlerne er uafhængige og kan læses separat.

I det første kapitel, "Wealth Protection in Bankruptcy and Serial Entrepreneurship", undersøger jeg, om formuebeskyttelse ved personlig konkurs giver en ny chance til konkursramte iværksættere. Jeg udnytter arv fra uventede dødsfald til at undersøge eksogen variation i personlig formue efter konkurs. Uventet arv øger tilbagevenden til erhvervslivet *kun* blandt iværksættere, der ikke oplevede alvorlige tab i personlig indkomst eller formue før konkurs. De, der reagerer på uventet arv ved at starte nye virksomheder, har lavere overskud, hvilket indikerer deres lavere iværksætterkvalitet. Samlet set tyder resultaterne på, at konkurslovgivning, der øger formuebeskyttelse, kan fremme serieiværksætteri, men deres effektivitet er begrænset af konkursramte iværksætteres lave kvalitet og personlige erfaringer med alvorlige tab.

I det andet kapitel, "Biased Judges? Judge Characteristics and Bankruptcy Outcomes", udnytter jeg den tilfældige tildeling af dommere til virksomhedskonkurser til at undersøge effekten af dommerkarakteristika på udfaldene. For det første finder jeg, at sager tildelt dommere, der voksede op under den store depression, er mere tilbøjelige til at ende med rekonstruktion, hvorimod sager tildelt dommere med økonomisk uddannelse og konservativ politisk ideologi er mere tilbøjelige til at resultere i likvidation. For det andet viser jeg, at sagsvarigheden er kortere (længere), når det potentielle sagsresultat er i overensstemmelse (ikke i overensstemmelse) med dommernes præferencer. For det tredje korrelerer dommerkarakteristika ikke med resultater efter konkursen. Samlet set tyder resultaterne på, at effekten af dommerkarakteristika kan være koncentreret i marginale sager, hvor de økonomiske fordele ved likvidation versus rekonstruktion ikke er væsentligt forskellige.

I det tredje kapitel, "Disqualifying Managerial Misconduct in Corporate Bankruptcy" (med S. Lakshmi Naaraayanan og Kasper Meisner Nielsen), undersøger vi indførelsen af konkurskarantæner, der diskvalificerer ledere, der har udøvet uagtsom forretningspraksis i op til 3 år. Ved hjælp af administrative registerdata fra Danmark dokumenterer vi, at diskvalifikation afskrækker fremtidig erhvervsaktivitet: efter karantænen er enkeltpersoner 15% mindre tilbøjelige til at være ledere eller virksomhedsejere. Diskvalificerede personer er også mindre tilbøjelige til at være involveret i fremtidige konkurser eller fremtidig kriminelle aktivitet. Samtidig stiger andelen af familiemedlemmer til diskvalificerede personer, der er aktive i en ledelsesrolle, fra 10% til 30%. Vi finder også ændringer i ledelsesarbejdsstyrken, hvilket resulterer i flere administrerende direktører med en plettet straffeattest og der udelukkende er afhængige af sociale overførsler. Samlet set er vores resultater det første systematiske bevis for styringseffekterne af diskvalificerende ledelsesadfærd ved virksomhedskonkurser.

Acknowledgements

Introduction

The three chapters in this thesis study how the bankruptcy institution—bankruptcy law and bankruptcy judges—affects the ex-post resolution of financial distress. I provide summaries of the individual chapters that highlight each chapter's contribution.

Wealth Protection in Bankruptcy and Serial Entrepreneurship

I evaluate whether policies that increase the level of wealth protection in personal bankruptcy help failed entrepreneurs to start a new business. The goal of such policies is explicitly mentioned, for example, in the Directive on Restructuring and Insolvency that the European Union adopted in 2019: "[the Directive aims that] *over-indebted entrepreneurs benefit from a full discharge of debt ..., thereby allowing them a second chance.*" To my knowledge, this is the first study examining the impact of protecting post-bankruptcy wealth on *serial* entrepreneurship.

I evaluate the policy goal by using an identification strategy that exploits windfall wealth from inheritances received by failed entrepreneurs in Denmark. The underlying idea is that windfall wealth after bankruptcy can serve as a proxy for greater amounts of wealth being protected in bankruptcy. To answer the research question, I construct a dataset on the universe of bankrupt individuals in Denmark based on court documents, linked with administrative register data.

I find that windfall wealth increases business ownership of failed entrepreneurs but has no effect on entrepreneurs who experienced *severe* losses in personal income or wealth before bankruptcy. Moreover, those who respond to windfall wealth by starting new businesses have lower income from post-bankruptcy entrepreneurship, indicating their lower entrepreneurial quality. Overall, the findings suggest that bankruptcy policies increasing wealth protection can promote serial entrepreneurship, but their effectiveness is limited by low entrepreneurial quality and personal experience of severe losses.

Biased Judges? Judge Characteristics and Bankruptcy Outcomes

The bankruptcy institution governs the reallocation of resources of distressed firms. In a frictionless bankruptcy system, the identity of a judge ruling on a bankruptcy case should not affect its outcome. On the contrary, a growing literature suggests that there is significant variation in the application of the bankruptcy law across judges. Despite such variation, evidence on why judges exhibit such differences is limited.

In this study, I examine the effect of judicial heterogeneity on bankruptcy outcomes in the US by focusing on a specific set of judge characteristics related to personal experiences and ideologies. The identification strategy exploits random assignment of judges to bankruptcy filings. First, I find that cases assigned to judges who grew up during the Great Depression are more likely to emerge from bankruptcy, whereas those assigned to judges with economics training and conservative political ideology are less likely to. Second, I show that the case duration is shorter (longer) when the potential case outcome is consistent (inconsistent) with judges' preferences. Third, the judge characteristics do not correlate with post-emergence outcomes. Overall, the findings suggest that the effect of judge characteristics may be concentrated in marginal cases

where the economic benefits of liquidation versus emergence are not significantly different.

Disqualifying Managerial Misconduct in Corporate Bankruptcy

Corporate bankruptcy law aims to balance protecting creditors with offering entrepreneurs a second chance. A key, yet understudied, concern is that managers might abuse the protection of limited liability to engage in irresponsible business conduct or even fraud. To deter such misconduct, policymakers around the world have implemented disqualification rules that bar managers of bankrupt companies from management positions for a specified period. Despite its policy relevance, empirical evidence on the impact of disqualification on corporate bankruptcy is limited.

This study examines the introduction of bankruptcy quarantines, which disqualify managers engaged in negligent business practices for up to three years. Using administrative register data from Denmark, we document that disqualifications discourage future business activity: after the quarantine, individuals are 15% less likely to be managers or business owners. Disqualified individuals are also less likely to be involved in future bankruptcies or future criminal activities. At the same time, the fraction of family members of disqualified individuals who are active in a management role increases from 10% to 30%. We also find changes to the managerial labor pool, resulting in more CEOs with a criminal record and those relying solely on social transfers. Overall, our findings provide the first systematic evidence on the governance consequences of disqualifying managerial misconduct in corporate bankruptcies.

Contents

Chapter 1

Wealth Protection in Bankruptcy and Serial Entrepreneurship

Abstract

I study whether wealth protection in personal bankruptcy provides a second chance to failed entrepreneurs. I exploit windfall wealth from inheritances to proxy for exogenous variation in personal wealth after bankruptcy. Windfall wealth increases reentry to business *only* among entrepreneurs who did not experience severe losses in personal income or wealth before bankruptcy. Those who respond to windfall wealth by starting new businesses have lower profits, indicating their lower entrepreneurial quality. Overall, the findings suggest that bankruptcy policies increasing wealth protection can promote serial entrepreneurship, but their effectiveness is limited by low entrepreneurial quality and personal experience of severe losses.

1.1 Introduction

Policymakers around the world have increased the level of wealth protection in personal bankruptcy laws to reduce the cost of entrepreneurial failure and foster entrepreneurship. For example, in 2019, the European Union adopted the Directive on Restructuring and Insolvency with the explicit aim to help "*over-indebted entrepreneurs benefit from a full discharge of debt ..., thereby allowing them a second chance.*"[1] Despite the importance of these policy changes, evidence on whether wealth protection in bankruptcy law is effective in providing a second chance to *failed* entrepreneurs is scant. In this study, I evaluate the effect of wealth protection on serial entrepreneurship using high-quality administrative data from Denmark.

A priori, whether failed entrepreneurs respond to a higher level of wealth protection is ambiguous. On one hand, greater wealth protection might promote serial entrepreneurship by protecting wealth from seizure by creditors, thereby relaxing financial constraints of failed entrepreneurs. Consistent with the financial constraints channel, Cahn et al. (2021) and Herkenhoff et al. (2021) find that public information on past bankruptcy limits access to financing, which deters entrepreneurship. On the other hand, regardless of their wealth, failed entrepreneurs' negative personal experiences might decrease their willingness to start another business. Prior studies document that negative personal experiences, such as corporate bankruptcy, decrease managerial risk-taking (Malmendier et al. 2011; Dittmar and Duchin 2016; Schoar and Zuo 2017). Collectively, whether failed entrepreneurs start another venture after increased wealth protection is an empirical question.

The key empirical challenge in answering this question is to generate random variation in the level of protected wealth to entrepreneurs in the event of bankruptcy. Prior studies that rely on cross-state or state-level variation in wealth protection (in the US) are subject to potential concerns about whether such variation is confounded by state-specific investment opportunities (Hynes et al. 2004). Moreover, more debtor-friendly laws may reduce credit supply, exacerbating the financial constraints that failed entrepreneurs face. Such indirect general equilibrium channels would counteract the direct effect of wealth protection on serial entrepreneurship.[2]

In this study, I address these challenges by using an identification strategy that exploits windfall wealth from inheritances received by failed entrepreneurs in Denmark.[3] The underlying idea is that variation in windfalls after bankruptcy serves as a proxy for variation in the wealth protected in bankruptcy. The research design has two advantages. First, the timing of inheritance is random relative to that of bankruptcy, which is supported by both the institutional features of Danish bankruptcy law and the data. Second, because these windfalls are restricted to individuals receiving inheritances, my results are unlikely to be explained by shifts in the overall credit supply. I exploit this random variation to estimate the effect of greater wealth protection on serial entrepreneurship by using a matched sample. I match failed entrepreneurs who receive inheritances with those who do not receive inheritances but have similar characteristics. I then compare the reentry rates of the two groups of failed entrepreneurs.

I first trace the effect of wealth protection on serial entrepreneurship without conditioning on past entrepreneurial experiences. I find that failed entrepreneurs are not more likely to own a business despite receiving windfall wealth after bankruptcy. This result holds even for those who receive inheritances above the median size, approximately 15,000 EUR (or equivalent to 22% relative to the median debt). The result implies that wealth protection in bankruptcy is not a sufficient condition for failed entrepreneurs to

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[1]See the Directive (EU) 2019/1023.

[2]Other indirect general equilibrium channels may confound the inference by causing changes in the composition of failed entrepreneurs. For example, more debtor-friendly laws reduce the expected costs of failure, which may incentivize more distressed entrepreneurs to file for bankruptcy (Agarwal et al. 2005).

[3]Several studies examine the effect of windfall wealth on first-time entrepreneurship by using different sources of wealth shocks. These include inheritances (Holtz-Eakin et al. 1994; Andersen and Nielsen 2012; Naaraayanan 2019), cash windfalls (Bellon et al. 2021; Cespedes et al. 2021; Bermejo et al. 2022), and increased access to credit via housing collateral (Adelino et al. 2015; Schmalz et al. 2017; Jensen et al. 2022).

restart. Moreover, the muted response contrasts with prior findings that document a positive effect of wealth protection on first-time entrepreneurship (e.g., Fan and White 2003; Armour and Cumming 2008; Cerqueiro et al. 2019), suggesting that the experience of failure and its severity may be another important determinant of serial entrepreneurship among bankrupt entrepreneurs.

To investigate why greater wealth protection alone does not spur serial entrepreneurship, I examine the role of past entrepreneurial experiences. Specifically, I use three measures of experiencing severe losses in personal income or wealth from business failure: (i) experiencing negative income from entrepreneurship, (ii) accumulating large business debts, and (iii) being fully personally liable (as opposed to partially liable) for business debts. I find that such experiences of severe losses deter restarting despite windfall wealth. On the other hand, those with less severe experiences are about 10 percentage points more likely to become serial entrepreneurs after receiving windfall wealth. This heterogeneous response to windfall wealth persists across different inheritance sizes. Overall, these findings suggest that the propensity to start a new business after bankruptcy is jointly determined by the amount of protected wealth and the personal experience of past failures.

If second-chance policies foster high-quality serial entrepreneurship, failed entrepreneurs who restart after receiving inheritances should outperform entrepreneurs who start for the first time. To test this premise, I compare the level of entrepreneurial profits between serial entrepreneurs who receive post-bankruptcy inheritances and matched first-time entrepreneurs who start in the same year and have similar characteristics as the serial entrepreneurs. I find that the former group earns about 20% less profits than the latter. This finding of lower profits suggests that failed entrepreneurs who respond to greater wealth protection are, on average, unlikely to be of high quality.

This study contributes to several strands of the literature. I provide the first empirical evidence on the effect of wealth protection in personal bankruptcy on *serial* entrepreneurship. Theoretical studies posit that wealth protection could foster overall entrepreneurship (Landier 2005; Ayotte 2007; Jia 2015; Mankart and Rodano 2015). Consistent with this theoretical prediction, empirical evidence shows that greater wealth protection increases entrepreneurship across US states and across countries (Fan and White 2003; Armour and Cumming 2008; Cerqueiro et al. 2019).[4] In comparison to these studies, I specifically test whether *failed* entrepreneurs start a new business. Considering that an important goal of bankruptcy law is to enable failed entrepreneurs to "start fresh" by discharging business debts (White 2016), I fill this gap in the literature. I find that failed entrepreneurs do not unconditionally respond to increases in wealth protection.[5]

The second contribution of this study relates to research examining the impact of removing public information about past bankruptcy or delinquency on entrepreneurship. These studies document that removing such information has either positive, negative, or no impact on entrepreneurship (Bos et al. 2018; Dobbie et al. 2020; Cahn et al. 2021; Herkenhoff et al. 2021). I complement these findings by showing that the effect of wealth protection in bankruptcy on entrepreneurship depends on whether the individual experiences severe losses from business failure.

My study also contributes to the broader literature on entrepreneurship. A large body of research documents the positive effect of wealth shocks on first-time entrepreneurship (e.g., Holtz-Eakin et al. 1994; Lindh and Ohlsson 1996; Andersen and Nielsen 2012; Adelino et al. 2015; Schmalz et al. 2017; Bellon et al. 2021; Cespedes et al. 2021; Bermejo et al. 2022). Compared with these studies, I find that bankrupt entrepreneurs respond to wealth windfalls by starting new businesses if they experienced less severe losses.

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[4]Recent evidence finds that downside protection against entrepreneurial failure *outside* the formal bankruptcy system can stimulate entrepreneurial activities (Hombert et al. 2020; Koudijs and Salisbury 2020; Ersahin et al. 2021; Gottlieb et al. 2022). Conversely, Akyol and Athreya (2011), Cumming and Li (2013), Paik (2013), and Traczynski (2019) observe either a negative or no correlation between wealth protection in personal bankruptcy and entrepreneurship in the US.

[5]Other studies examine the effect of personal bankruptcy law, as well as managers' personal costs associated with corporate bankruptcy, on firm-level outcomes other than entrepreneurship, such as borrowing and investments (Berkowitz and White 2004; Berger et al. 2011; Cerqueiro et al. 2017; Cerqueiro and Penas 2017; Chen et al. 2020; Cespedes et al. 2022; Celentani et al. 2022; Damm et al. 2022; Schoenherr and Starmans 2022). In a related study, Baird and Morrison (2005) argue that reorganizations in *corporate* bankruptcies delay entrepreneurs' transition to new ventures that may better match their skills.

Another strand of the literature documents that serial entrepreneurs outperform first-time ones (Gompers et al. 2010; Lafontaine and Shaw 2016; Shaw and Sørensen 2019). In comparison to these studies, my findings indicate that marginal entrepreneurs who restart following increased wealth protection in bankruptcy underperform.[6]

The final contribution relates to the literature documenting that negative personal experiences deter individual risk-taking (e.g., Malmendier et al. 2011; Dittmar and Duchin 2016; Koudijs and Voth 2016; Knüpfer et al. 2017; Schoar and Zuo 2017; Andersen et al. 2019). Consistent with the literature, my study finds that bankrupt entrepreneurs who experience severe losses from their businesses are less willing to take risks in the labor market.

My study has implications for policies that aim to provide a second chance to failed entrepreneurs by increasing wealth protection. First, such policies may be insufficient to foster serial entrepreneurship because personal experiences of severe losses deter restarting regardless of the level of wealth protected by bankruptcy law. Second, failed entrepreneurs who do respond to such policies may, on average, generate lower profits compared to first-time entrepreneurs or bankrupt entrepreneurs who restart without the policy support. Moreover, while greater wealth protection induces a subset of failed entrepreneurs to restart, prior research documents that these policies may simultaneously reduce businesses' access to credit in the economy, which might in turn deter entry and growth of other aspiring entrepreneurs (Berkowitz and White 2004; Berger et al. 2011; Fossen 2014).[7] In sum, my findings underscore the limited effectiveness of wealth protection policies in fostering high-quality serial entrepreneurship.

The study proceeds as follows. Section 1.2 introduces the institutional setting in Denmark, providing details about personal bankruptcies for entrepreneurs and about inheritances for bankrupt individuals. Section 1.3 describes the data and how I construct the main sample by a matching procedure. I then present summary statistics of bankrupt entrepreneurs and the distribution of inheritances in the sample. Section 1.4 provides evidence that inheritances significantly increase wealth after bankruptcy. I then analyze whether and how larger wealth protected in bankruptcy affects serial entrepreneurship, focusing on the role of past entrepreneurial experiences. Section 1.5 presents robustness checks. I address the possibility that experiencing severe losses may correlate with low entrepreneurial quality. I also discuss the potential role of age of those who inherit. Section 1.6 concludes.

1.2 Institutional setting

This section describes the institutional setting relevant to this study. First, I provide a brief overview of the personal bankruptcy system in Denmark. Second, I introduce the institutional background regarding inheritance.

1.2.1 Personal bankruptcy for entrepreneurs in Denmark

The Bankruptcy Act (*Konkursloven*) governs insolvency proceedings in Denmark. Individual debtors who are insolvent, i.e., who cannot fulfill their debt obligations, have three filing options: liquidation for private individuals (*personlig konkurs*), reorganization (*rekonstruktion*), and bankruptcy (*gældssanering*).[8] I briefly

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[6]In a related study, Cesarini et al. (2017) find that winning larger prizes is associated with lower entrepreneurial income among lottery winners, consistent with my finding of lower profits among serial entrepreneurs who receive inheritances.

[7]Related studies further find that greater wealth protection in personal bankruptcy is associated with higher interest rates for both secured and unsecured credit (e.g., Gropp et al. 1997; Livshits et al. 2007; Severino and Brown 2017; Chakrabarti and Pattison 2019; Gross et al. 2021).

[8]Agrawal et al. (2022) note that in Danish parlance, *personlig konkurs* is often referred to as "personal bankruptcy." However, *personlig konkurs* does not entail debt discharge, while *gældssanering* does. Therefore, I refer to *personlig konkurs* as "liquidation" and *gældssanering* as "bankruptcy" throughout this study.

describe two proceedings that are empirically relevant for individual debtors: liquidation and bankruptcy.[9]

Under liquidation proceedings, insolvent debtors liquidate their assets to pay the debt. Liquidation can be filed by either a debtor or creditor. Importantly, liquidation does not automatically discharge the remaining unpaid debt. To receive the discharge, debtors need to apply separately for bankruptcy proceedings.

Under bankruptcy proceedings, debtors can receive a debt discharge by committing to a repayment plan, which typically lasts three to five years. Bankruptcy proceedings begin when the debtor files with the court in the local jurisdiction. Once the court confirms the filing meets all requirements, it formally opens the bankruptcy case and publicly announces it on the State Gazette, an official government gazette, which is the source of data for this study. The announcement on the State Gazette also specifies the deadline for creditors to submit their claims. After the case is opened, the debtor presents a repayment plan to the court. The plan requires the debtor to use all disposable income (defined as predicted income minus predicted necessary expenses) to pay part of the unsecured debt.[10] If the court deems the plan feasible, it approves it and issues a bankruptcy ruling, detailing the discharge ratio (the proportion of debt discharged in bankruptcy to total unsecured debt) and the repayment terms, such as installment amounts and duration. At the issuance of the bankruptcy ruling, the portion of the debt that cannot be paid from disposable income is discharged. Only under special circumstances, such as permanent illness leaving the debtor incapable of repayment, the court may grant a full, immediate discharge. The average duration between case opening and ruling is about 9.5 months in my main sample. I provide more details on bankruptcy proceedings in Appendix 1.A.1.

Two types of procedures: Ordinary versus business debt chapters   Denmark has two different personal bankruptcy procedures, defined under Chapters 25–28 and Chapter 29 of the Bankruptcy Act (hereafter referred to as the "ordinary chapter" and the "business debt chapter," respectively).[11] The two chapters follow similar legal procedures to discharge debt and require liquidation of all assets, as previously described.[12] The two types of bankruptcy have three main differences that may make the business debt chapter preferable for failed entrepreneurs who are eligible (Bang-Pedersen 2018).[13] First, the business debt chapter is only available for individuals with large business debt. According to case law, the threshold for eligibility is set at 75% of the debt being business-related (Hindborg 2017, p. 281). Second, under the business debt chapter, the debtor can be unemployed or without stable income at filing, whereas under the ordinary chapter, the debtor must have a stable income from regular employment. This relaxed condition allows failed entrepreneurs who have recently reopened a business to file for bankruptcy, even without stable income. Third, the duration of the repayment period differs between the two, lasting three years under the business debt chapter and five years under the ordinary chapter. The shorter repayment period under the business debt chapter is intended to facilitate a faster return to business after bankruptcy.

Ability to borrow and to own a business after bankruptcy   Bankruptcy effectively restricts an individual's ability to borrow, but not business ownership. When liquidation or bankruptcy proceedings begin, debtors are registered with bankruptcy flags in the credit register, called RKI. These flags are removed after completion of the repayment period (three to five years), but while they are present, they effectively make it impossible to obtain new loans or credit (Kreiner et al. 2020). Given that all bankrupt individuals are

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[9]Reorganization proceedings, which became available in 2011, are commonly used by large corporate debtors (Bang-Pedersen 2018). These proceedings represent less than 5% of all insolvency proceedings between 2011 and 2016, with approximately 100 filings each year.

[10]Secured debts, like mortgages or car loans, cannot be discharged.

[11]The business debt chapter was introduced in October 2005 following a reform to the Bankruptcy Act. Another reform in 2022, which is outside my sample period, consolidated the two chapters and reduced the repayment period to three years. For details about the 2005 reform, see Kilborn (2009), Kilborn (2011), and Bang-Pedersen (2018).

[12]Formally, the business debt chapter requires the debtor to be under liquidation proceedings, whereas the ordinary chapter does not; in practice, even in the ordinary chapter, the court requires the liquidation of debtors' assets of value, such as a house or a car.

[13]For other minor differences between the two chapters, see Appendix 1.A.1.

flagged regardless of inheritance events, my research design isolates the effect of windfall wealth, distinct from that of bankruptcy flags. Importantly, neither bankruptcy flags nor bankruptcy itself restrict an individual's ability to own a business.[14]

1.2.2 Windfall wealth from inheritances after bankruptcy

To estimate the effect of wealth protection on post-bankruptcy reentry into entrepreneurship, my research design exploits windfall wealth from inheritances that debtors receive after bankruptcy. Identifying these inheritances is facilitated by administrative registers provided by Statistics Denmark. Specifically, I use population registers to link parents and their children and wealth registers to obtain individual-level asset and liability information, which is sourced from official tax records.[15] According to Danish inheritance law, inheritances are by default equally divided among children. Legal provisions require that the transfer of the estate to heirs should be completed within 12 months after the death. An estate tax of 15% is levied on estates exceeding a net wealth of Danish Kroner (DKK) 242,400 as of 2006. This threshold is adjusted annually by a price index.

Inheritance events provide an ideal setting to study the effect of wealth protection in bankruptcy on serial entrepreneurship, due to two institutional features. First, the timing of inheritance is unrelated to that of bankruptcy. Danish case law has established that, when inheritance is anticipated at filing, the court rejects the application for bankruptcy (Hindborg 2017, p. 59, and Petersen and Ørgaard 2022, p. 125). In such cases, the court considers that the expected inheritance will improve the debtor's financial situation, reducing the need for bankruptcy protection. Moreover, debtors are required to disclose all relevant information about their financial situation, including any prospect of inheritance; concealing such information is considered fraudulent and can later result in the cancellation of the bankruptcy order (Hindborg 2017, pp. 213–215, and Hansen and Petersen 2022, p. 337). Therefore, the institutional environment suggests that the timing of an inheritance is likely to be exogenous to the timing of a bankruptcy ruling.

Second, unexpected windfalls, such as lottery winnings or inheritance, that occur after the ruling do not change the repayment terms (Hindborg 2017, p. 314, and Petersen and Ørgaard 2022, note 931).[16] Therefore, inheritances that occur after bankruptcy belong solely to the debtor, as creditors do not have claims to them. In Section 1.4.1, I provide empirical evidence that is consistent with these two institutional features. Together, these institutional features give me confidence that windfall wealth from inheritance after bankruptcy can serve as a proxy for greater amounts of wealth being protected in bankruptcy.

1.3 Data and sample selection

1.3.1 Data

I use four data sources to construct a panel dataset of entrepreneurs who go personally bankrupt. I begin with a list of bankrupt individuals and then attach to each name the corresponding unique individual identifier (CPR) assigned to every Danish citizen. The CPR identifiers, equivalent to Social Security numbers in the US, allow me to link the list of bankrupt individuals to the administrative registers and business ownership datasets. Using these linked data, I identify individuals' business ownership before and after bankruptcy and whether they receive inheritances. I describe each source in detail below.

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[14]During liquidation proceedings, the bankruptcy court may impose a bankruptcy quarantine (*konkurskarantæne*) on managers who operated their company in a grossly irresponsible manner. This quarantine prohibits them from owning a limited liability company for a three-year period but does not prohibit owning an unlimited liability company.

[15]Following Andersen and Nielsen (2012), I restrict the sample to cases where all beneficiaries are children of the deceased.

[16]Some heirs with significant debt may waive the rights to inheritance, possibly to avoid the inheritance being used to pay creditors. However, waiving inheritance before bankruptcy can lead the court to reject the application for bankruptcy, according to the Danish case law (Hindborg 2017, p. 61, and Hansen and Petersen 2022, pp. 127–129).

1. The State Gazette of Denmark (*Statstidende*): The State Gazette is a government gazette that publicly announces statutory notices on court proceedings. I parse the State Gazette documents into notices on bankruptcy rulings and identify bankrupt individuals. I start with about 2,800 issues of the State Gazette from 2006 through 2016. Each issue contains a document index, which I use to locate sections on liquidation and bankruptcy proceedings. (See a sample page in Appendix Figure 1.A.1.) Each section on liquidation (bankruptcy) proceedings contains, on average, 50 (22) notices, which leaves me with a total of about 150,000 (66,000) notices on different stages of court proceedings (e.g., whether a case opens, a ruling is made, or a ruling is cancelled). Each document groups notices by their stage, which is demarcated by subheadings. Notices on bankruptcy rulings are grouped under the subheading "Kendelse" (Ruling). See Appendix Figure 1.A.2 for a representative Kendelse page.

   Each bankruptcy notice contains structured, textual information on the court and the debtor. The court-side information includes the unique case identifier, the date of court decision, the discharge ratio, and the court that makes the decision. The debtor-side information includes the debtor's name, date of birth, and full address (either residential or associated with an owned company). If the debtor has owned a company, its unique identifier, known as the CVR-number, is also included. Because the notice on the bankruptcy ruling does not indicate the bankruptcy chapter, I infer it from the closest preceding notice on case opening, which contains such information. Combining the notices produces the list of approximately 18,000 individuals who receive bankruptcy rulings between 2006 and 2016.[17] Next, I assign CPR identifiers to the debtors listed in the State Gazette. To achieve this, I use a combination of debtor-side information from the State Gazette, such as the debtors' full name, date of birth, address, and the unique identifiers of firms they own. After excluding debtors with insufficient details in the State Gazette, I successfully assign CPR identifiers to 77% of the debtors from the State Gazette.[18]

2. Statistics Denmark: I use administrative data from Statistics Denmark, which comprise several registers containing comprehensive information on income, wealth, education, labor supply, family (parents, spouse, and children), and parental death. These registers cover the entire population of Denmark and provide individual-level data on an annual basis, using a CPR identifier for each person. The data are considered highly reliable. For instance, information on income, wealth, and employment status is directly sourced from official records from the Danish Tax and Customs Administration. Furthermore, the registers remain robust against attrition unless an individual either dies or emigrates from Denmark. Due to the high quality of the data, several studies on the drivers of business entry and exit have used these data sources (e.g., Nanda and Sørensen 2010; Andersen and Nielsen 2012; Hanspal 2018; Agrawal et al. 2022; Jensen et al. 2022). I extract data from the registers for the years 1980 to 2021. I adjust variables denominated in Danish Kroner (DKK) to the 2015 price level and winsorize them at the first and ninety-ninth percentiles for each year. To identify inheritance events and their magnitudes using these administrative registers, I follow the methodology of Andersen and Nielsen (2012). The detailed procedure is outlined in Appendix 1.A.2.

3. The Central Business Register (CVR: *Centrale Virksomhedsregister*): The Central Business Register contains firm-level information on all companies in Denmark.[19] The relevant information includes incorporation status (either unlimited or limited liability company), industry (NACE codes), number of full-time equivalent employees, business address, founders, managers, and owners. The coverage of ownership information is more detailed from 2017 onward, when limited liability companies were mandated to report their beneficial owners. The dataset covers the period from 1990 to 2021.

4. Experian: To supplement the CVR data on ownership of limited liability companies, I use the Experian

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[17]From October 2005, the State Gazette transitioned to digital publication (accessible at https://www.statstidende.dk), making 2006 the first full calendar year for which digital forms are available.

[18]The most common reason for non-assignment is when the debtor's date of birth and full name from the State Gazette matches multiple records in the administrative registers (due to having commonly used names). I exclude such multiple matches from the sample.

[19]Companies are required to report statutory information to the CVR.

dataset. It assembles data from companies' annual reports, which list ownership for individuals or entities holding more than 5% of the share capital. The dataset is available between 2000 and 2019.

1.3.2 Definition of entrepreneurs

I define individuals as entrepreneurs if they own an unlimited or limited liability company (hereafter ULC and LLC, respectively).

To determine ULC ownership, I extract information from the matched employer-employee panel dataset supplied by Statistics Denmark.[20] Statistics Denmark classifies individuals as ULC entrepreneurs if their primary occupation is at a personally owned business, either a sole proprietorship (*enkeltmandsvirksomhed*) or a partnership (*interessentskab*). Focusing on primary occupation ensures that I capture full-time entrepreneurs.[21]

Second, to determine LLC ownership, I combine datasets from the CVR and Experian. The combined dataset provides a list of legal owners (those holding more than 5% of ownership or voting rights directly) and beneficial owners (those holding more than 25% of ownership or voting rights, either directly or indirectly) for each LLC at year-end.[22] I classify individuals as LLC entrepreneurs who are either legal or beneficial owners of LLCs. By focusing on individuals with significant control rights, I capture business owners who are more likely to be entrepreneurs rather than financial investors.

1.3.3 Sample selection

To examine serial entrepreneurship after bankruptcy, the first step in the sample selection is to identify entrepreneurs who go bankrupt. Given that the State Gazette does not consistently specify whether a bankruptcy results from business failure, I use two criteria to identify former entrepreneurs. First, I classify all bankruptcies under the business debt chapter as those of former entrepreneurs, given that this chapter is exclusively available to individuals primarily indebted from business activities. Second, for ordinary chapter bankruptcies, I consider individuals to be former entrepreneurs if they owned either a ULC or LLC within the seven years leading up to their bankruptcy. Using these criteria, I identify 5,894 entrepreneurs who go bankrupt, with 3,358 and 2,536 from the ordinary and business debt chapters, respectively.

In the second step, I refine the sample to avoid spurious correlations. First, I remove 53 individuals whose rulings are subsequently cancelled (due to failures like noncompliance with the repayment plan). Second, to eliminate those who inherit too long after bankruptcy, I exclude 314 individuals whose inheritance events occur more than four calendar years after their year of bankruptcy. Third, for individuals with multiple bankruptcy rulings, I only consider the first ruling.[23]

In the third step, I retain entrepreneurs aged between 18 and 60 at the time of the ruling, thereby excluding 676 older entrepreneurs who might simply retire from the labor market after bankruptcy. After this step in sample selection, I am left with 4,851 unique failed entrepreneurs who go bankrupt between 2006 and 2016.

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[20]Unlike LLCs, ULCs are not required to submit annual reports to the CVR, resulting in potential delays or omissions in reporting the closure or reopening of ULCs. Therefore, to accurately track serial entrepreneurship in ULCs, I use the matched employer-employee panel dataset from Statistics Denmark.

[21]Specifically, Statistics Denmark classifies individuals as ULC entrepreneurs if they meet either of the following criteria: (1) they own a ULC that employs at least one other individual, or (2) they are self-employed and derive over 50% of their total income from a ULC, or their business turnover exceeds 50,000 DKK.

[22]I include three types of LLCs in Denmark: public limited liability companies (*aktieselskab*), private limited liability companies (*anpartsselskab*), and entrepreneurial companies (*iværksætterselskab*).

[23]Refiling is a rare event: only about 3% of the bankrupt individuals in my sample receive a second or, even more rarely, a third ruling. Moreover, a review of such subsequent rulings from the State Gazette suggests that they primarily adjust terms from the initial ruling, rather than indicating a separate spell of financial distress.

Matching entrepreneurs who receive inheritances to those who do not My objective is to estimate the impact of windfall wealth on serial entrepreneurship after bankruptcy. To control for the general propensity to restart a business absent windfall wealth, I match bankrupt entrepreneurs who receive inheritances after their ruling (referred to as the "treated" group) with those of similar characteristics who do not (the "control" group).

To begin, I identify the treated group from inheritance events.[24] To focus on windfalls that occur soon after bankruptcy, I only look at inheritance events between the year of bankruptcy and three years after. The procedures yield 230 entrepreneurs with inheritance events.

For each bankrupt entrepreneur in the treated group, I look for an entrepreneur in the control group, and I match with replacements. The matching takes the following steps:

1. I require that the entrepreneur in the control group has the same year of bankruptcy, bankruptcy chapter (either ordinary or business debt chapters), and gender and is of a similar age (± 1 year) as the treated entrepreneur.
2. Among potential matches, I select the nearest neighbor based on pre-bankruptcy wealth (measured at one year before the bankruptcy).[25] I further refine the accuracy of matching by excluding matched pairs with substantial differences in wealth levels (an absolute difference exceeding 1,000,000 DKK and a relative difference exceeding 50%). Additionally, I exclude individuals lacking information on their years of education, which is a control variable in my empirical specification.

After matching, my main matched sample consists of 214 unique individuals in the treated group and 205 in the control group. I observe them from the year of bankruptcy, denoted as year 0, through the five subsequent years, extending up to year +5.

1.3.4 Summary statistics

Table 1.1 reports the characteristics of all bankrupt entrepreneurs and the main matched sample (the treated and control groups), measured at the year of bankruptcy. The treated group is broadly similar to the full sample of bankrupt entrepreneurs on the observable characteristics shown in Panels A–D. Panel A shows that bankrupt entrepreneurs have, on average, large negative net wealth at one year before bankruptcy, which leads them to seek a debt discharge. Panel B reports that the treated group is four years older than the full sample. This age difference is not surprising, as one enters the treatment group when their last living parent dies.[26] Panel C shows that bankrupt entrepreneurs, on average, discharge more than 90% of total unsecured debt. Panel D reports that about 20% of bankrupt entrepreneurs owned LLCs during the seven-year period before bankruptcy. The presence of former LLC owners under personal bankruptcy suggests that, like ULC owners, they are personally liable for some business debts. Their presence is consistent with prior studies documenting the importance of personal credit (and thus personal bankruptcy) among small business owners (e.g., Hvide and Møen 2010; Robb and Robinson 2014; White 2016; Wang et al. 2022; Chava et al. 2023; Fonseca and Wang 2023).[27] In the last column, I examine the difference between the treatment and matched control groups. None of the differences in characteristics between the two groups are statistically significant. This absence of significant differences, particularly among those not used in the matching process, implies that both groups are similar on observable entrepreneurial characteristics.

[Table 1 about here.]

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[24]I provide detailed procedures on identifying inheritances in Appendix 1.A.2.

[25]My results are robust to choosing other years before bankruptcy.

[26]A potential concern is that the older mean age of the treated group, compared to all bankrupt entrepreneurs, may dampen the impact of windfall wealth on serial entrepreneurship. I address this issue in Section 1.5.

[27]Specifically, LLC owners may have personal liability if they use personal loans to finance their companies or have personal guarantees on company loans.

Inheritance amounts The main explanatory variable in my analysis is whether an individual receives an inheritance. I provide descriptive evidence about the magnitude of inheritances to show that they are economically significant for these distressed entrepreneurs. The top panel of Figure 1.1 plots the distribution of inherited wealth in six bins. The bins group different sizes of inheritances in DKK: 1 to 10,000, 10,001 to 25,000, 25,000 to 100,000, 100,001 to 250,000, 250,001 to 500,000, and those exceeding 500,000 DKK (which approximately correspond to EUR: 0.1 to 1,300, 1,301 to 3,400, 3,401 to 13,000, 13,001 to 34,000, 34,001 to 67,000, and those exceeding 67,000 EUR, respectively). The distribution of inherited wealth shows substantial variation, similar to Andersen and Nielsen (2012), who find a positive effect of windfall wealth from inheritances on first-time entrepreneurship. To put these results into perspective, the average (median) size of inheritances in my study is 308,000 DKK (115,000 DKK), which is of similar magnitude to the average found in Andersen and Nielsen (2012).[28] Therefore, the amounts that the bankrupt entrepreneurs in my sample inherit appear to be sufficiently large to relax the financial constraints of failed entrepreneurs who want to restart.

In the bottom panel of Figure 1.1, I plot the ratio of inherited wealth to dischargeable debt as an alternative way to quantify the magnitude of windfalls. This panel plots the ratio using four bins. I define dischargeable debt as unsecured debt measured at one year before bankruptcy. Similar to the top panel, these relative sizes of inheritances are economically large.

Inheritance timing In Appendix Figure 1.A.3, I report the distribution of inheritance timing relative to bankruptcy ruling. I group inheritance events into seven bins, each spanning six months. For instance, the first bin represents the share of individuals who receive an inheritance within the first six months following their bankruptcy ruling date. Subsequent bins group people by intervals of six months. The plot shows that the timing of inheritances does not exhibit bunching within the first six or 12 months following bankruptcy. For instance, inheritances that occur within 12 months of bankruptcy account for 25.7% (15.0% + 10.7%) of total inheritance occurrences, a figure lower than the 35% (19.6% + 15.4%) for those occur within the last 12 months. The relatively uniform distribution of inheritances across the event window supports that the timing of inheritances is exogenous once a bankruptcy ruling is issued (as discussed in Section 1.2.2).

[Figure 1 about here.]

1.4 Windfall wealth and serial entrepreneurship

1.4.1 Inheritance as a proxy for wealth protected in bankruptcy

In my research design, I exploit windfall wealth from inheritances to approximate the amount of wealth protected in bankruptcy. Before the main analysis, I assess the validity of the research design by testing (1) whether the treated and control groups show a similar trend in the level of wealth before the inheritance event and (2) whether inheritance increases net wealth (i.e., the difference between total assets and total debt) of the recipients.

Figure 1.2 illustrates the dynamics of average net wealth for both the treated (solid line) and control (dashed line) groups. The horizontal axis shows the years since bankruptcy, with year 0 denoting the year in which the court issues a bankruptcy ruling. The shaded area between years 0 and 3 represent the treatment window, during which inheritance events occur. The plot supports the validity of the research design in two ways.

First, Figure 1.2 shows that the treated and control groups show a similar evolution of wealth before bankruptcy. Despite constructing the matched control group based on individuals' characteristics measured

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[28]Compliant with Statistics Denmark's data policy, this median value represents the average of five values around the median.

at year -1, the overall trend in wealth is similar between the two groups throughout the event years -7 and -1.[29] This parallel pre-trend supports the main identifying assumption in the difference-in-differences design that the treated and control groups' wealth would have trended similarly in the absence of windfall wealth. In particular, the parallel pre-trend is inconsistent with opportunistic filing behavior by debtors in my sample, such as debtors systematically accumulating debt in anticipation of post-bankruptcy inheritances. Therefore, Figure 1.2 supports the main identifying assumption that the timing of inheritance is random relative to that of bankruptcy.

Second, while both groups experience a sharp jump in net wealth around the year of bankruptcy due to debt discharge, the treated group exhibits a higher level of wealth throughout the post-bankruptcy period. The persistent difference in wealth (statistically significant at the 10% level at each event year, except year 0) suggests that inheritance significantly increases the wealth of recipients. In particular, the post-bankruptcy level of wealth for the treated group remains close to zero or positive, while that of the control group remains negative. The negative level of wealth suggests that financial constraints may be particularly binding for failed entrepreneurs in the control group, who intend to restart.

Overall, Figure 1.2 lends support to (1) the parallel trend in wealth before bankruptcy, and (2) the legal feature that unanticipated inheritances belong to the debtor and are thus not subject to creditors' seizure. These two results are consistent with the institutional environment discussed in Section 1.2.

After observing that inheritances increase net wealth, I run regressions to quantify the magnitude of the increase. Specifically, I estimate the following OLS regression:

$$Y_{it} = \alpha_i + \alpha_y + \beta_1 After\ bankruptcy\ discharge_{it} + \beta_2 After\ inheritance_{it} + \gamma X'_{it} + \varepsilon_{it}, \qquad (1.1)$$

where the dependent variable, Y_{it}, is an outcome variable (net wealth and its components) of individual i in event year t, where $t = 0$ is the year of bankruptcy. In the following analysis, I use five outcome variables for Y_{it}: the level of net wealth, total debt, and total assets, or alternatively, the log-transformed values of total assets and total debts. *After bankruptcy discharge* is an indicator variable equal to one in the years following bankruptcy and zero otherwise. *After inheritance* is an indicator variable equal to one in the years following an inheritance event and zero otherwise. I include individual fixed effects (α_i) to control for unobserved time-invariant determinants of wealth for individuals, and calendar-year fixed effects (α_y) to rule out time effects, such as economic conditions. The inclusion of the individual fixed effects implies that I benchmark the post-bankruptcy and post-inheritance levels of the outcome variables to their pre-bankruptcy levels. The control variables X'_{it} include age-group fixed effects (dummies for every five-year bin of age) to control for time-varying determinants of wealth or its components. Standard errors are clustered at the individual level. The coefficient β_1 estimates the impact of bankruptcy and debt discharge on wealth. The main coefficient of interest, β_2, captures the impact of inheritance on wealth.

Table 1.2 reports the results from estimating Equation 1.1. The sample comprises individuals in the treated and control groups, observed from seven years before until five years after bankruptcy. Columns 1–5 shows results for five separate regressions in which outcome variables are the level of net wealth, total debt, total assets, the log of total assets, and the log of total debts, respectively.[30] The results in Table 1.2 are consistent with Figure 1.2. Column 1 shows that the level of net wealth increases by about 909,200 DKK in the years after bankruptcy. Moreover, the effect of inheritance is also significant, amounting to approximately 30% of the magnitude of the bankruptcy's effect on wealth. Columns 2–5 further decompose the effect on net wealth into its components, total debt and assets.[31] Columns 2 and 4 show that bankruptcy reduces a large amount of debt (by about 1,303,100 DKK or by 81%, respectively), whereas inheritance does

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[29]None of the event years between -7 and -1 show statistically significant differences in wealth between the two groups.

[30]I do not use the logarithm of net wealth because the level of net wealth is often negative.

[31]Columns 4 and 5 omit observations for which the value of total debts or assets is equal to zero. The results are robust when using *Log(total debts+1)* or *Log(total assets+1)* as alternative specifications.

not have statistically significant effects (by about 142,000 DKK or by 15%, respectively), as most of the debt is already discharged through bankruptcy. Instead, Columns 3 and 5 show that inheritances increase total assets (by about 143,900 DKK or by 90%, respectively). The increase in assets supports the research design, which uses variation in windfall wealth as a proxy for variation in the amount of protected wealth in bankruptcy.

[Figure 2 about here.]

[Table 2 about here.]

1.4.2 Wealth protected in bankruptcy and serial entrepreneurship

In the previous section, I established that windfalls from inheritance increase net wealth, which is consistent with the idea of using them as a proxy for greater wealth protection in bankruptcy. In this section, I test whether more wealth protected in bankruptcy increases the probability of starting a new business, without conditioning on past entrepreneurial experiences. If greater wealth protection, which relaxes the financial constraints of failed entrepreneurs, is a sufficient condition for restarting, I expect to see a positive relation between windfall wealth and serial entrepreneurship.

I begin with a descriptive analysis. Figure 1.3 shows the dynamics of the share of individuals who own a business. The horizontal axis shows the years since bankruptcy, with year 0 denoting the year in which the court issues a bankruptcy ruling. The entrepreneurship rates for the treated and control groups are illustrated with solid and dashed lines, respectively. The shaded area between event years 0 and 3 represents the treatment window, during which inheritance events occur.

Figure 1.3 provides several stylized facts about the dynamics of entrepreneurship around bankruptcy. First, many entrepreneurs exit businesses as they approach bankruptcy, suggesting that their business is failing. Second, in the five years following the bankruptcy, entrepreneurship rates increase by only about 14 percentage points, from approximately 10% to 24%. Specifically, the treated group shows an increase of 14 percentage points, while the control group exhibits a similar increase of 13 percentage points.[32] Finally, the treated and control groups show a parallel trend before bankruptcy. Although the treated group exhibits a marginally higher share of entrepreneurship, the difference is both economically and statistically insignificant. Moreover, even after bankruptcy, the difference between the two groups remains insignificant. The trends shown in Figure 1.3, therefore, suggest that wealth protection by bankruptcy laws may be insufficient to induce serial entrepreneurship.

[Figure 3 about here.]

To test this result more formally, I regress the indicator for post-bankruptcy business ownership on three measures of windfall wealth after bankruptcy. Specifically, to examine serial entrepreneurship after bankruptcy, I limit my sample to individuals observed in the year of bankruptcy and the following five-year periods. With this sample, I estimate the following linear probability model:

$$Owner_{it} = \alpha_y + \beta \, After \ inheritance_{it} + \gamma X'_{it} + \varepsilon_{it}, \tag{1.2}$$

where the dependent variable, $Owner_{it}$, is an indicator variable equal to one if individual i owns a business in event year t. In this specification, I use three measures to capture the effects of windfall wealth: (i) *After inheritance* is an indicator variable equal to one in the years following the inheritance event and zero otherwise; (ii) *After inheritance* × *Inherited wealth* is equal to the amount of inherited wealth (measured

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[32]These trends hold when examining the stock of serial entrepreneurs, as opposed to the flow, as in Figure 1.3. Additionally, extending the event window to seven years post-bankruptcy, as long as the data allow, does not materially change the findings. The differences between the two groups remain statistically insignificant.

in millions of 2015 DKK) in the years following the inheritance event and zero otherwise; and (iii) *After inheritance × Large inheritance* is an indicator variable equal to one for individuals receiving an above-median inheritance in the years following the inheritance event and zero otherwise. The latter two variables capture potential linear or non-monotonic effects of the size of the inheritance. For control variables X'_{it}, I include bankruptcy case characteristics (an indicator for the bankruptcy chapter and the discharge ratio) and individual characteristics. For individual characteristics, I follow Andersen and Nielsen (2012) and control for the individual's propensity to start a business: the levels of wealth and income (measured at one year before bankruptcy), age, an indicator for gender, and years of education. I include calendar-year fixed effects (α_y) to control for time effects, such as changes in investment opportunities over time. Standard errors are clustered at the individual level. The main coefficient of interest is β, which estimates the effect of receiving an inheritance (or the size of the inheritance) on the probability of owning a business after bankruptcy.

Table 1.3 shows the results from the linear probability model regressions in Equation 1.2. The results are consistent with the univariate comparison in Figure 1.3, which suggests a small effect of inheritance on serial entrepreneurship. In Column 1, I find that the probability of owning a business after receiving an inheritance is positive (4.8 percentage points), but not statistically significant. Next, in Columns 2 and 3, to address the potential concern that only substantial windfalls may impact the likelihood of restarting, I exploit variation in the amount of inheritance, as well as variation in the timing of inheritance. However, both columns suggest that the null effect is unlikely to be driven by variations in inheritance size. In Column 2, the coefficient on *After inheritance × Inherited wealth* is economically small, implying that increasing the inheritance by 1,000,000 DKK ($\approx$ 134,000 EUR) would increase the probability of restarting by only 5.5 percentage points in each post-inheritance year. The economic magnitude of additional protected wealth is small, given that average sizes of inherited wealth and dischargeable debt are 308,000 DKK and 1,100,000 DKK, respectively. In addition, the estimate is not statistically significant, suggesting there is no linear effect of inherited wealth. Similarly, Column 3 shows null effects for both above- and below-median sizes of inheritance, where the median is about 115,000 DKK (15,000 EUR).[33]

Overall, the results in Table 1.3 suggest that wealth protection in bankruptcy alone has no significant effect on serial entrepreneurship. This null result contrasts with prior findings of a positive relationship between greater wealth protection and first-time or overall entrepreneurship (Fan and White 2003; Armour and Cumming 2008; Cerqueiro et al. 2019), as well as a positive relation between wealth shocks outside bankruptcy and entrepreneurship (e.g., Holtz-Eakin et al. 1994; Andersen and Nielsen 2012; Bellon et al. 2021; Cespedes et al. 2021). The contrasting responses between failed and first-time entrepreneurs suggest that the experience of failure and its severity may discourage failed entrepreneurs from reentering entrepreneurship.

[Table 3 about here.]

1.4.3 Experience of severe losses and serial entrepreneurship

Next, I investigate the role of past entrepreneurial experience on serial entrepreneurship. Personal entrepreneurial experience may affect the probability of starting a second venture, particularly given that prior literature shows that managers' negative experiences — such as corporate bankruptcy — decrease their risk-taking (Malmendier et al. 2011; Dittmar and Duchin 2016; Schoar and Zuo 2017). While bankruptcy itself can be a distressing experience (e.g., due to the stigma attached to managers who go bankrupt, as in Grindaker et al. 2021; Bernstein et al. 2023), I focus on entrepreneurial experiences that have a *severe* impact on personal income and wealth. I identify three measures of such experiences.

First, I define severe losses based on whether an individual experiences negative personal income from entrepreneurship at any point during the pre-bankruptcy periods.[34] By construction, this measure is defined

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[33]I obtain similar results if I instead use the indicator for inheritances in the largest quartile.

[34]In Denmark, entrepreneurial losses can be used for a deduction in taxable income in the same year if an entrepreneur has

only for those who have owned unlimited liability companies before bankruptcy. Second, I classify whether over-indebtedness, which leads to subsequent bankruptcy, originates primarily from entrepreneurship. An ideal measure would be to classify each type of debt as business-related or not, but such a granular level of data is unavailable to me. Nevertheless, the institutional feature of the Danish bankruptcy system allows me to approximate the ideal measure. As discussed in Section 1.2, failed entrepreneurs with large business debts are eligible to file under the business debt chapter, a more lenient procedure than the ordinary chapter. Thus, bankrupt entrepreneurs under the business debt chapter are more likely to have accumulated large debt due to entrepreneurial failure than those under the ordinary chapter. In addition, this second measure supplements the first because it is applicable for every entrepreneur in my sample. Third, I define whether an individual solely owned an unlimited liability company before bankruptcy, implying the individual is *fully* liable for the business debts.[35] While some entrepreneurs may enter bankruptcy due to personal guarantees on their limited liability company's debt, generally, owners of limited liability companies are less likely to be held *fully* liable for business debt compared to owners of unlimited liability companies. Conversely, owners of unlimited liability companies are more likely to have severely negative experiences from business failure than owners of limited liability companies.[36] Using these three measures of adverse shocks to income or wealth, I test whether experiences of severe losses hold back bankrupt entrepreneurs from restarting despite windfall wealth.

To explore the effect of experiences of severe losses, I begin with a descriptive analysis. Figure 1.4 plots the dynamics of the share of individuals who own a business among individuals in the treated group. The top and bottom panels plot the dynamics, split by the first and second measures of severe losses, respectively.[37] Figure 1.4 highlights two stylized facts about how experiences of severe losses influence entrepreneurship before and after bankruptcy, among those who receive inheritances. First, during the pre-bankruptcy period, those with (solid lines) and without (dashed lines) severe losses show differential pre-trends. This is expected given that the groupings are determined by entrepreneurial experiences before bankruptcy. A closer look reveals that the drop in the entrepreneurial rate is both sharper and deeper for the solid lines, indicating that severe income losses or large business debts are associated with a faster and worse decline in business. Second, during the post-bankruptcy period, although every individual in the sample had been an entrepreneur before bankruptcy, the solid and dashed lines show a differential rate of serial entrepreneurship. Specifically, in both panels, roughly 30% of those without severely negative experiences (dashed lines) restart, compared to fewer than 20% of their counterparts with such experiences (solid lines). The visual evidence thus indicates that bankrupt entrepreneurs who experienced severe losses are less likely to respond to windfall wealth by starting a new business, at least in the first few years after bankruptcy.

[Figure 4 about here.]

Next, I investigate the results in Figure 1.4 more formally by estimating the following linear probability model, in which I add an interaction term from Equation 1.2:

$$
\begin{aligned}
Owner_{it} = \alpha_y &+ \beta_1 After\ inheritance_{it} + \beta_2 Severe\ losses_i \\
&+ \beta_3 After\ inheritance_{it} \times Severe\ losses_i + \gamma X'_{it} + \varepsilon_{it},
\end{aligned} \tag{1.3}
$$

where *Severe losses*$_i$ is an indicator variable equal to one if individual i experiences severe losses from

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[35] I use an indicator for "solely ULC owner" rather than for "all ULC owner" because the latter does not distinguish between entrepreneurs who had owned both an LLC and a ULC versus those who were exclusively ULC owners.

[36] Although this third measure partially captures the protection from negative personal income, it complements the first measure because it is defined for both LLC and ULC owners.

[37] To save space, I report similar figures based on the third measure of severe losses (*Full personal liability*) and those based on the control group in Appendix Figures 1.A.4 and 1.A.5, respectively. For the third measure of severe losses, I observe similar stylized facts as for the two other measures.

business before bankruptcy, which is one of the following three measures: *Severe income loss$_i$*, *Business debt chapter$_i$*, or *Full personal liability$_i$*. Standard errors are clustered at the individual level. The coefficients of interest are β_1 and β_3, which capture the heterogeneous effect of inheritance on the probability of restarting, depending on experiencing severe losses.

Table 1.4 reports the results estimating Equation 1.3 using the first measure of severe losses, *Severe income loss*. I find that experiencing severe income losses from entrepreneurship deter restarting despite larger wealth protected in bankruptcy. In Column 1, the coefficient on *After inheritance* suggests that inheritances increase the probability of owning a business after bankruptcy by 13.1 percentage points per year. The magnitude is economically large compared to the baseline probability of owning a business of 18.7% (16.5%) for the treated (control) group. However, the negative coefficient on *After inheritance × Severe income loss* is of similar magnitude, indicating that the positive effect is concentrated among entrepreneurs who do not experience severe losses. In Columns 2 and 3, I further test if the positive effect of inheritances depends on their size. In Column 2, the coefficient on *After inheritance × Inherited wealth* suggests a positive linear effect of inherited wealth on the probability of restarting. On the other hand, in Column 3, while the positive coefficient on *After inheritance* suggests that inheritances of any size have positive effects, the small coefficient on *After inheritance × Large inheritance* indicates inheritances of above-median size do not have an incremental effect. Nevertheless, in both Columns 2 and 3, when interacted with the measure of severe losses, *Severe income loss*, the positive effects are offset. These results suggest that even a larger amount of wealth protected in bankruptcy does not offset the effect of experiencing severe losses from past business.

Table 1.5 reports the results estimating Equation 1.3 using the second measure of severe losses, *Business debt chapter*. I find that experiencing severe indebtedness from failed businesses has similar negative effects on the probability of restarting. In Column 1, the positive coefficient on *After inheritance* suggests that entrepreneurs who go bankrupt without significant business debt are more likely to restart. Conversely, the negative coefficient on *After inheritance × Business debt chapter* suggests the experience of accumulating large debt from business deters serial entrepreneurship. In Columns 2 and 3, I use information on inheritance sizes and find similar experience effects, although the estimates are less precise. Importantly, the experience effect holds after expanding the sample to include entrepreneurs who did not own ULCs (i.e., solely LLC owners) before bankruptcy. This result suggests that the experience effect is insensitive to former incorporation choice of entrepreneurs.

Lastly, Table 1.6 reports the results estimating Equation 1.3 using the third measure of experiencing severe losses, *Full personal liability*. I find that holding *full* personal liability from failed businesses has negative effects on the probability of restarting. In Column 1, the large positive coefficient on *After inheritance* suggests that, after receiving inheritances, former LLC owners are more likely to restart by 12.6 percentage points per year. Compared with this positive effect, the negative coefficient on *After inheritance × Full personal liability* suggests that entrepreneurs who owned only ULCs prior to bankruptcy do not restart in response to inheritances. While these two coefficients are not statistically significant at conventional levels, their economic magnitudes are large given that the sample mean of serial entrepreneurship is about 17%. In Columns 2 and 3, I use information on inheritance sizes and find similar effects with more precise estimates: the positive effect of inheritances on serial entrepreneurship is concentrated among former LLC owners, who are less likely to be fully personally liable for business debt.

[Table 4 about here.]

[Table 5 about here.]

[Table 6 about here.]

Overall, these findings suggest that the null result in Table 1.3 is driven by entrepreneurs who experience severe losses. Although wealth windfalls relax financial constraints, my results indicate that the experience

of severe losses may decrease failed entrepreneurs' willingness to start another business. Those without such experience respond to windfall wealth and restart a business. These different responses to windfall wealth suggest that the propensity to start a new business after bankruptcy is a joint function of the amount of protected wealth and the personal experience of severe losses from failed entrepreneurship.

1.4.4   Returns to post-bankruptcy serial entrepreneurship

In previous sections, I find that entrepreneurs respond to greater wealth protected in bankruptcy only if they experienced less severe losses. In this section, I test whether the entrepreneurs who do respond are of high quality by assessing their performance in the new business relative to that of a comparison group. Given that second-chance policies rely on the premise that serial entrepreneurs outperform first-time business owners, I test the premise by using a comparison group consisting of first-time entrepreneurs. I measure performance by the business survival rate and the level of entrepreneurial income.

   To this end, I construct a matched sample consisting of failed entrepreneurs who restart after bankruptcy from my main sample and those of similar characteristics who become entrepreneurs for the first time. For each failed entrepreneur who restarts after bankruptcy, I look for a first-time entrepreneur from the Danish population. To control for the individual's entrepreneurial quality, I match individuals who restart after bankruptcy (referred to as the "serial entrepreneurs") with those of similar characteristics who become entrepreneurs for the first time (the "first-time entrepreneurs"). First-time entrepreneurs are of similar age (± 1 year) and the same gender, years of education, and incorporation choice. They have similar pre-entrepreneurial labor income, and they start their businesses at the same time that the serial entrepreneurs *re*start. I describe the matching process in detail in Appendix Section 1.A.4 and present summary statistics of the matched sample in Appendix Table 1.A.1. After matching, the sample consists of 110 unique serial entrepreneurs and 110 first-time entrepreneurs. I observe them from the year of (re)starting a business through the five subsequent years. In the matched sample, around 50% of serial entrepreneurs experience inheritance events. They are marginal entrepreneurs who respond to greater wealth protection by restarting a business and are, therefore, the focus of second-chance policies.

   To compare the performance of these two types of entrepreneurs, I use two measures: survival rate and entrepreneurial profit. Specifically, I estimate the following OLS regression:

$$Y_{it} = \alpha_y + \beta_1 After\ inheritance_{it} + \beta_2 Past\ bankruptcy_i + \gamma X'_{it} + \varepsilon_{it}, \tag{1.4}$$

where the dependent variable, Y_{it}, is either *Owner*$_{it}$ or *Labor and entrepreneurial income*$_{it}$: the former is an indicator variable equal to one if individual i owns a business in event year t; the latter measures the level of entrepreneurial profit for individual i in event year t. The main independent variables are *After inheritance* and *Past bankruptcy*. *After inheritance* is defined as an indicator variable equal to one in the years following the inheritance event and zero otherwise, identical to previous specifications. *Past bankruptcy* is an indicator equal to one for individuals who experienced bankruptcy (thus, equal to one for all serial entrepreneurs in this matched sample). For control variables X'_{it}, I use individual characteristics (age, gender, years of education) as well as wealth and labor income before entrepreneurship, incorporation choice, and years of business experience. These variables help control for an individual's general entrepreneurial ability. I include calendar-year fixed effects (α_y) to control for time effects. Standard errors are clustered at the individual level. The coefficients of interest are β_1 and β_2. β_2 estimates the difference in business ownership and entrepreneurial profit between first-time and serial entrepreneurs. β_1 captures the effect on bankrupt entrepreneurs who restart after receiving an inheritance. Collectively, a negative β_1 would suggest that, controlling for the average quality of serial entrepreneurs, the marginal quality of entrepreneurs who respond to greater wealth protection is lower than average first-time entrepreneurs.

   I first examine the difference in the survival likelihood of the two types of entrepreneurs. Appendix Table

1.A.2 reports results estimating Equation 1.4, where the outcome variable is business ownership. The results suggest that *average* serial entrepreneurs do not survive longer than first-time ones: coefficients on *Past bankruptcy* are positive but statistically not significant. Regarding *marginal* serial entrepreneurs' survival likelihood, the effect of inheritance is not monotonic in the size of inheritance: while coefficients on *After inheritance* are positive in Column 1, Columns 2 and 3 imply that serial entrepreneurs who receive large inheritances are not more likely to survive. Therefore, the results suggest that the effect of inheritance on the survival likelihood of marginal entrepreneurs is ambiguous.

After documenting that serial entrepreneurs do not survive longer in their businesses than first-time entrepreneurs, I compare levels of income between the two groups. I first provide a descriptive analysis. Figure 1.5 plots averages of labor and entrepreneurial income that are (1) measured over three years before entrepreneurship and (2) measured over all years during entrepreneurship, split by first-time and serial entrepreneurs. To illustrate the income difference between marginal and average entrepreneurs, I limit the sample to serial entrepreneurs who receive an inheritance and first-time entrepreneurs who are matched to them. Figure 1.5 shows that the two groups of entrepreneurs have similar labor income before (re)starting a business. However, after (re)starting, they have an income difference of about 44,000 DKK for each year of entrepreneurship. The difference suggests that, compared to similar first-time entrepreneurs, serial entrepreneurs who restart after receiving an inheritance earn significantly less.

[Figure 5 about here.]

I next test this descriptive result more formally. Table 1.7 reports results estimating Equation 1.4, where the outcome variable is labor and entrepreneurial income. The results in Table 1.7 are consistent with Figure 1.5. For example, in Column 1, the small, negative coefficient on *Past bankruptcy* suggests that average serial entrepreneurs earn less from their business than first-time entrepreneurs. However, the estimate is not statistically significant in all specifications, suggesting large variation in profits among average serial entrepreneurs. In contrast, the negative coefficient on *After inheritance* is both economically and statistically significant. It suggests that the serial entrepreneurs who receive an inheritance have on average 64,000 DKK lower profits for each year of entrepreneurship. The economic magnitude is large and represents about 20% lower profits, given that the average entrepreneurial profit is 280,000 DKK for all entrepreneurs in the sample. In Columns 4–6, I find similar results when I limit the sample to observations where the business survives.

The results in Appendix Table 1.A.2 suggest that survival likelihood is not always higher for serial entrepreneurs who restart after receiving an inheritance, and Table 1.7 shows that they earn significantly lower profits than first-time entrepreneurs. Together, these results do not support the idea that second-chance policies facilitate reentry of serial entrepreneurs who outperform first-time business owners.

[Table 7 about here.]

Comparison *within* bankrupt entrepreneurs who restart   In the previous section, I assess post-bankruptcy income levels of serial entrepreneurs by using a comparison group consisting of first-time entrepreneurs with similar characteristics. An alternative way to evaluate the marginal quality of serial entrepreneurs is to compare the income levels *within* only bankrupt entrepreneurs. To this end, I return to my main sample that consists of treated and control groups of bankrupt entrepreneurs. Using all bankrupt entrepreneurs, I compare entrepreneurial income between those who restart with and without receiving an inheritance. In this within-group analysis, I find that serial entrepreneurs who receive an inheritance earn about 20–30% less entrepreneurial income relative to those who do not inherit. I provide a more detailed description of the analysis in Appendix Section 1.A.6, along with the results in Appendix Figure 1.A.6 and Appendix Table 1.A.3.

Overall, the results in Table 1.7 and Appendix Table 1.A.3 suggest that the entrepreneurs that greater wealth protection is more likely to induce to restart end up earning significantly less. The low performance

of marginal entrepreneurs who respond to changes in the bankruptcy regime is consistent with prior studies. For instance, Cerqueiro et al. (2019) find that first-time entrepreneurs who start after increases in state-level wealth protection are less likely to survive than those who enter before. In addition, Cahn et al. (2021) find that bankrupt entrepreneurs who restart after removal of corporate bankruptcy flags are more likely to go bankrupt again. I complement these studies by providing direct evidence that marginal entrepreneurs earn lower profits, which may eventually lead them to exit. Overall, these findings indicate that the marginal entrepreneurs who respond to a more lenient bankruptcy regime are of low quality.

1.5   Robustness checks

In this section, I perform additional tests to assess the robustness of the baseline findings. I first address concerns that the entrepreneurs experiencing severe losses may be a proxy of their low ability or merely an artifact of long tenure in business. I then explore if the older age of the treated group is dampening the response to inheritances.

Severe losses versus low entrepreneurial quality   I address potential concerns that experiencing severe losses before bankruptcy may correlate with low entrepreneurial quality. To address this concern, I refine the tests in Table 1.4 to control for low relative performance during prior entrepreneurship. To this end, I compute the relative performance of entrepreneurs by collecting annual labor and entrepreneurial income data for the population of entrepreneurs (i.e., including non-bankrupt entrepreneurs). Next, I split entrepreneurs' income into deciles based on the 88-industry-year level.[38] The resulting variable, *Low past performance*, is an indicator variable equal to one if an individual's entrepreneurial income persistently falls within the bottom decile throughout his or her entrepreneurship before bankruptcy. In other words, by focusing on persistent low performance, this measure separates entrepreneurs who have low skill from those who experience "bad luck." This measure is only defined for entrepreneurs with available information about the industry of the company they own.

Table 1.8 reports the results. In Column 1, I first examine the effect of low relative performance, not controlling for severe losses. The positive coefficient on *After inheritance* suggests that entrepreneurs who do not experience persistently low performance are more likely to restart after receiving windfall wealth.[39] In comparison, the negative coefficient on *After inheritance × Low past performance* indicates that experiencing persistently low performance decreases the probability of owning a business after bankruptcy by about 17 percentage points per year. The differential response across past performance is consistent with the model of entrepreneurs in which they learn about their entrepreneurial ability upon entry and failure (e.g., Jovanovic 1982; Ayotte 2007; Dillon and Stanton 2017).

In Column 2, I examine the effect of severe losses after controlling for low relative performance. The coefficient on *After inheritance × Severe income loss* suggests that experiencing severe losses still has similar negative effects on serial entrepreneurship for those who inherit, while the lack of such experience offsets them. This result implies that severe losses are a personal experience distinct from low relative performance. In Columns 3 and 4, I find similar results when I use the alternative measures of severe losses, *Business debt chapter* or *Full personal liability*. Overall, these results confirm that experiencing severe losses is distinct from low entrepreneurial quality.

[Table 8 about here.]

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[38]The median (average) number of members in 88-industry-year cells is, for example, 547 (2,965) in year 2006. I then exclude observations that have fewer than 25 members in an industry-year cell. The industry classification is based on the two-digit code from NACE Rev. 2. I obtain similar results if I use the one-letter code, which splits the economy into 22 industries.

[39]To save space, I only report results using the simple binary indicator for inheritances, *After inheritance*.

Severe losses versus long tenure in business   I address the potential concern that experiencing severe losses may be positively correlated with tenure in the failed business. In Appendix Table 1.A.4, I find that having above-median tenure in ULCs before bankruptcy (greater than or equal to four years) does not subsume the effect of severe losses. This finding suggests that experiencing severe losses is not a mechanical result from having a long entrepreneurial spell.

The role of age at bankruptcy   As shown in Table 1.1, the treated group in my matched sample is on average four years older than the full sample of bankrupt entrepreneurs. This age difference is not surprising, as one enters the treatment group when their last living parent dies. Nevertheless, a potential concern is that the older age of the matched sample might lead to an underestimation of the true effect of wealth protection in bankruptcy for the broader sample. For instance, if older individuals have a lower propensity to become entrepreneurs, they may be less likely to respond to increased wealth protection by starting new businesses, which could explain the null effect of inheritances in Table 1.3.

I address this concern in two ways. First, I find that age, as a control variable in Table 1.3, does not significantly affect the probability of restarting in the main sample. Second, I conduct additional tests to evaluate whether the response to inheritance depends on age. Specifically, I employ two approaches: First, I test for a potential negative linear effect of age by interacting the binary indicator for post-inheritance events with the individual's age at bankruptcy. Second, I test for a potential non-linear effect by using the indicator variable *Above median age*, which equals one for individuals above the median age at bankruptcy (48.5 years old) and interact it with the post-inheritance indicator. I then repeat the baseline estimation using these two measures. The results, presented in Appendix Table 1.A.5, show that age does not have a statistically significant effect on the probability of restarting among entrepreneurs who receive inheritances. This finding suggests that, within my sample of bankrupt entrepreneurs, receiving an inheritance at an older age is not associated with a lower propensity to restart. Taken together, the evidence indicates that the age composition of my matched sample is unlikely to be driving the null effect in Table 1.3.

Effect of severe income loss across bankruptcy chapters   In Appendix Table 1.A.6, I address the possibility that the effect of severe income loss may differ across bankruptcy chapters. One may worry that failed entrepreneurs under the business debt chapter may have different unobservable characteristics from those under the ordinary chapter, resulting in a null effect of severe income loss on serial entrepreneurship. To tackle this concern, I split the sample by bankruptcy chapters and estimate Equation 1.3, separately for each sample. I find that the effect of severe income loss is similar in economic magnitude across both samples. The similar effect in both chapters suggests that the experience of severe income loss is not subsumed by that of severe indebtedness.

1.6   Conclusion

Existing studies document that a higher level of wealth protection in personal bankruptcy is associated with an increase in first-time entrepreneurship (Fan and White 2003; Armour and Cumming 2008; Cerqueiro et al. 2019). In comparison to these studies, I find that such a relation does not hold among *failed* entrepreneurs, exploiting windfall wealth from inheritances to generate exogenous variation in the wealth protected in bankruptcy. My results further show that the muted response to increased wealth protection is driven by entrepreneurs who experienced severe losses from business failure. Additionally, the entrepreneurs who do respond to increases in wealth protection tend to earn less in their new business.

My study has implications for policy discussions about providing a second chance for failed entrepreneurs after bankruptcy. My findings suggest increasing wealth protection in personal bankruptcy provides entrepreneurs a second chance, but the effect is limited by personal experience of severe losses. Moreover,

prior research documents the potential costs of such policies, such as credit rationing and higher interest rates for other entrepreneurs in the economy. Collectively, my findings inform the policy debate on whether increasing wealth protection is effective at fostering high-quality serial entrepreneurship.

Table 1.1: Summary statistics

This table presents the mean and standard deviation of the main variables for the sample of entrepreneurs who go bankrupt between 2006 and 2016. Each column refers to one of the three groups of individuals in my sample: (a) all bankrupt entrepreneurs, (b) the "treated group" of entrepreneurs who receive an inheritance between the year of bankruptcy and three years afterward, and (c) the "control group," which consists of entrepreneurs who do not receive an inheritance after bankruptcy and are matched to the treated group using procedures outlined in Section 1.3.3. For every variable, I compute the difference in average characteristics between the treated and control groups and test whether this difference is statistically different from zero. The variables in this table are measured at the year of bankruptcy, except for those in Panel A. Panel A reports net wealth and total income, each measured at one year before bankruptcy and in thousands of 2015 DKK (1 Euro $\approx$ DKK 7.45). Panel B presents demographic data. Panel C reports the ruling bankruptcy chapter (business debt or ordinary chapters) and the discharge ratio (the share of debt discharged in bankruptcy to total unsecured debt). Panel D reports business experience from seven years to one year before bankruptcy, such as indicators for ownership of limited or unlimited liability companies. *Severe income loss* is an indicator variable equal to one if the individual experiences negative entrepreneurial income before bankruptcy. *Low past performance* is an indicator variable equal to one if the individual's annual entrepreneurial income has always remained in the bottom decile in the industry before bankruptcy. Standard deviations are in parentheses, and t-statistics are in brackets.

	All	Matched sample		Difference
		Treated (1)	Control (2)	(1)-(2)
A. Wealth and income (1,000 DKK)				
Pre-bankruptcy wealth	-1,200.7	-1,114.3	-1,110.5	-3.8
	(2,152.0)	(1,780.2)	(1,806.6)	[-0.0]
Pre-bankruptcy income	282.4	280.3	282.4	-2.1
	(137.9)	(126.4)	(134.6)	[-0.2]
B. Individual characteristics				
Age	43.8	47.6	47.4	0.2
	(8.4)	(6.9)	(6.8)	[0.3]
Male (%)	73.1	80.4	80.0	0.4
	(44.4)	(39.8)	(40.1)	[0.1]
Years of education	11.7	11.7	11.5	0.2
	(1.8)	(1.9)	(1.9)	[1.2]
C. Bankruptcy case characteristics				
Business debt chapter (%)	45.7	41.1	40.0	1.1
	(49.8)	(49.3)	(49.1)	[0.2]
Discharge ratio (%)	91.0	90.3	92.1	-1.8
	(13.5)	(14.6)	(11.3)	[-1.4]
D. Pre-bankruptcy personal business experience				
Pre-bankruptcy LLC owner (%)	18.4	21.0	19.0	2.0
	(38.7)	(40.8)	(39.3)	[0.5]
Pre-bankruptcy ULC owner (%)	82.8	82.2	81.0	1.3
	(37.8)	(38.3)	(39.3)	[0.3]
Severe income loss (%)	61.6	55.7	62.7	-7.0
	(48.6)	(49.8)	(48.5)	[-1.3]
Low past performance (%)	4.4	2.2	2.5	-0.2
	(20.6)	(14.8)	(15.5)	[-0.1]
Number of individuals	4,851	214	205	

Table 1.2: Inheritance as a proxy for wealth protected in bankruptcy

This table reports estimates from OLS regressions, examining the effect of post-bankruptcy inheritances on the levels of net wealth, total debt, and total assets (measured in thousands of 2015 DKK, where 1 Euro ≈ DKK 7.45), and the logarithms of total debt and of total assets, respectively. The control group consists of individuals of the same bankruptcy year, bankruptcy chapter, gender, and similar age (± 1 year) and net wealth. The sample includes observations from seven years before bankruptcy until five years after. *After bankruptcy discharge* (*After inheritance*) is an indicator variable equal to one for years following the individual's bankruptcy (inheritance event). I use inheritance events that occur between the year of bankruptcy and three years after. Other control variables are defined in Table 1.1. All columns include individual, calendar-year, and age-group (dummies for every five-year bin of age) fixed effects. Standard errors are clustered at the individual level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Net wealth	(2) Total debt	(3) Total assets	(4) Log(total debt)	(5) Log(total assets)
After bankruptcy discharge	909.2***	-1,303.1***	-337.1***	-1.65***	-0.16
	(10.29)	(-9.81)	(-5.02)	(-17.09)	(-1.35)
After inheritance	269.3**	-142.0	143.9	-0.16	0.64***
	(2.57)	(-0.60)	(0.88)	(-1.15)	(3.84)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Age group fixed effects	Yes	Yes	Yes	Yes	Yes
R^2	0.11	0.15	0.10	0.31	0.07
Individual-year observations	5,400	5,400	5,400	5,001	5,193

Table 1.3: Wealth protected in bankruptcy and serial entrepreneurship

This table reports the regression results from a linear probability model examining the effect of post-bankruptcy inheritances on the probability of owning a business. The dependent variable, *Owner*, is an indicator variable equal to one if the individual owns any company in the year. The primary independent variable, *After inheritance*, is an indicator variable equal to one for years following the individual's inheritance event. *After inheritance × Inherited wealth* is equal to the amount of inherited wealth (measured in millions of 2015 DKK) in post-inheritance years, and zero otherwise. *After inheritance × Large inheritance* is an indicator variable equal to one for individuals receiving an above-median inheritance in the years following the inheritance event, and zero otherwise. The sample consists of individuals from the treated and control groups, from the year of bankruptcy until five years afterward. Control variables are defined in Table 1.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
After inheritance	0.048		0.054
	(1.64)		(1.39)
After inheritance × Inherited wealth		0.055	
		(1.03)	
After inheritance × Large inheritance			-0.012
			(-0.24)
Business debt chapter	-0.062*	-0.060*	-0.061*
	(-1.83)	(-1.79)	(-1.82)
Discharge ratio	0.002**	0.002**	0.002**
	(2.39)	(2.32)	(2.41)
Pre-bankruptcy wealth	-0.000*	-0.000*	-0.000*
	(-1.78)	(-1.73)	(-1.79)
Pre-bankruptcy income	-0.000	-0.000*	-0.000
	(-1.61)	(-1.66)	(-1.60)
Age	-0.000	-0.001	-0.000
	(-0.18)	(-0.23)	(-0.16)
Male	0.101***	0.104***	0.100***
	(3.14)	(3.25)	(3.09)
Years of education	0.019**	0.020**	0.020**
	(2.29)	(2.34)	(2.29)
Year fixed effects	Yes	Yes	Yes
R^2	0.06	0.06	0.06
Individual-year observations	2,480	2,480	2,480

Table 1.4: Experiencing severe income losses and serial entrepreneurship

This table reports the regression results from a linear probability model examining the effect of experiencing severe income losses on the probability of owning a business after bankruptcy. The dependent variable, *Owner*, is an indicator variable equal to one if the individual owns any company in the year. The primary independent variable, *After inheritance*, is an indicator variable equal to one for years following the individual's inheritance event. The interacted variable, *Severe income loss*, is an indicator variable equal to one if the individual experiences negative entrepreneurial income before bankruptcy. By construction, this measure is defined only for those who have owned unlimited liability companies before bankruptcy. *After inheritance × Inherited wealth* is equal to the amount of inherited wealth (measured in millions of 2015 DKK) in post-inheritance years, and zero otherwise. *After inheritance × Large inheritance* is an indicator variable equal to one for individuals receiving an above-median inheritance in the years following the inheritance event, and zero otherwise. The sample consists of individuals from the treated and control groups, from the year of bankruptcy until five years afterward. Control variables are defined in Table 1.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
After inheritance	0.131***		0.149**
	(2.78)		(2.38)
After inheritance × Severe income loss	-0.136**		-0.145*
	(-2.31)		(-1.81)
After inheritance × Inherited wealth		0.190***	
		(6.01)	
After inheritance × Inherited wealth × Severe income loss		-0.206***	
		(-5.87)	
After inheritance × Large inheritance			-0.040
			(-0.43)
After inheritance × Large inheritance × Severe income loss			0.023
			(0.20)
Severe income loss	0.056*	0.030	0.055*
	(1.68)	(0.94)	(1.68)
Business debt chapter	-0.027	-0.027	-0.026
	(-0.71)	(-0.69)	(-0.68)
Discharge ratio	0.001	0.001	0.001
	(1.56)	(1.42)	(1.53)
Pre-bankruptcy wealth	-0.000	-0.000	-0.000
	(-1.03)	(-1.04)	(-1.04)
Pre-bankruptcy income	-0.000**	-0.000**	-0.000**
	(-2.46)	(-2.50)	(-2.47)
Age	0.000	0.000	0.000
	(0.07)	(0.12)	(0.11)
Male	0.051	0.055	0.050
	(1.28)	(1.38)	(1.24)
Years of education	0.019*	0.021**	0.019*
	(1.93)	(2.20)	(1.93)
Year fixed effects	Yes	Yes	Yes
R^2	0.05	0.07	0.05
Individual-year observations	2,018	2,018	2,018

Table 1.5: Experiencing severe indebtedness and serial entrepreneurship

This table reports the regression results from a linear probability model examining the effect of experiencing severe indebtedness from entrepreneurship on the probability of owning a business after bankruptcy. The dependent variable, *Owner*, is an indicator variable equal to one if the individual owns any company in the year. The primary independent variable, *After inheritance*, is an indicator variable equal to one for years following the individual's inheritance event. The interacted variable, *Business debt chapter*, is an indicator variable equal to one if the individual files for bankruptcy under the business debt chapter, which is available for entrepreneurs with large business debts. *After inheritance × Inherited wealth* is equal to the amount of inherited wealth (measured in millions of 2015 DKK) in post-inheritance years, and zero otherwise. *After inheritance × Large inheritance* is an indicator variable equal to one for individuals receiving an above-median inheritance in the years following the inheritance event, and zero otherwise. The sample consists of individuals from the treated and control groups, from the year of bankruptcy until five years afterward. Control variables are defined in Table 1.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
After inheritance	0.103***		0.099**
	(2.75)		(1.98)
After inheritance × Business debt chapter	-0.132**		-0.111
	(-2.31)		(-1.42)
After inheritance × Inherited wealth		0.060	
		(1.04)	
After inheritance × Inherited wealth × Business debt chapter		-0.091	
		(-0.56)	
After inheritance × Large inheritance			0.007
			(0.10)
After inheritance × Large inheritance × Business debt chapter			-0.040
			(-0.41)
Business debt chapter	-0.014	-0.053	-0.014
	(-0.37)	(-1.50)	(-0.36)
Discharge ratio	0.002**	0.002**	0.002**
	(2.35)	(2.31)	(2.35)
Pre-bankruptcy wealth	-0.000*	-0.000*	-0.000*
	(-1.77)	(-1.73)	(-1.78)
Pre-bankruptcy income	-0.000	-0.000	-0.000
	(-1.54)	(-1.63)	(-1.54)
Age	-0.000	-0.000	-0.000
	(-0.15)	(-0.18)	(-0.11)
Male	0.101***	0.104***	0.100***
	(3.15)	(3.23)	(3.10)
Years of education	0.020**	0.020**	0.020**
	(2.30)	(2.35)	(2.29)
Year fixed effects	Yes	Yes	Yes
R^2	0.06	0.06	0.06
Individual-year observations	2,480	2,480	2,480

Table 1.6: Experiencing *full* personal liability from business failure and serial entrepreneurship

This table reports the regression results from a linear probability model examining the effect of experiencing *full* personal liability from business failure on the probability of owning a business after bankruptcy. The dependent variable, *Owner*, is an indicator variable equal to one if the individual owns any company in the year. The primary independent variable, *After inheritance*, is an indicator variable equal to one for years following the individual's inheritance event. The interacted variable, *Full personal liability*, is an indicator variable equal to one if the individual solely owned an unlimited liability company prior to bankruptcy, as opposed to owning a limited liability company. *After inheritance × Inherited wealth* is equal to the amount of inherited wealth (measured in millions of 2015 DKK) in post-inheritance years, and zero otherwise. *After inheritance × Large inheritance* is an indicator variable equal to one for individuals receiving an above-median inheritance in the years following the inheritance event, and zero otherwise. The sample consists of individuals from the treated and control groups, from the year of bankruptcy until five years afterward. Control variables are defined in Table 1.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
After inheritance	0.126		0.217**
	(1.64)		(2.12)
After inheritance × Full personal liability	-0.102		-0.194*
	(-1.27)		(-1.80)
After inheritance × Inherited wealth		0.131***	
		(3.84)	
After inheritance × Inherited wealth × Full personal liability		-0.121***	
		(-2.79)	
After inheritance × Large inheritance			-0.155
			(-1.21)
After inheritance × Large inheritance × Full personal liability			0.159
			(1.17)
Full personal liability	-0.216***	-0.234***	-0.216***
	(-3.85)	(-4.68)	(-3.85)
Business debt chapter	-0.052	-0.051	-0.052
	(-1.62)	(-1.59)	(-1.62)
Discharge ratio	0.002**	0.001*	0.002**
	(1.98)	(1.77)	(1.98)
Pre-bankruptcy wealth	-0.000	-0.000	-0.000
	(-1.16)	(-1.15)	(-1.13)
Pre-bankruptcy income	-0.000**	-0.000**	-0.000*
	(-2.05)	(-2.15)	(-1.92)
Age	-0.002	-0.002	-0.002
	(-1.05)	(-0.91)	(-1.02)
Male	0.090***	0.090***	0.086***
	(2.83)	(2.90)	(2.72)
Years of education	0.014	0.015*	0.013
	(1.56)	(1.68)	(1.54)
Year fixed effects	Yes	Yes	Yes
R^2	0.13	0.13	0.13
Individual-year observations	2,480	2,480	2,480

Table 1.7: Returns to post-bankruptcy serial entrepreneurship

This table reports estimates from OLS regressions examining the effect of post-bankruptcy inheritances on entrepreneurial profit in the matched sample. The matched sample consists of two types of individuals: (1) those who restart a business after bankruptcy (referred to as "serial entrepreneurs") and (2) the matched sample of individuals who start a business for the first time and do not experience bankruptcy (referred to as "first-time entrepreneurs"). First-time entrepreneurs are of similar age (± 1 year) and have the same gender, years of education, and incorporation choice; they have similar pre-entrepreneurial labor income; and they start their businesses at the same time as the serial entrepreneurs restart. I observe individuals from the year of (re)starting a business to five years afterward. The dependent variable, *Labor and entrepreneurial income*, is the level of labor and entrepreneurial income (measured in thousands of 2015 DKK, where 1 Euro $\approx$ DKK 7.45). In Columns 1–3, I include all individual-year observations, and in Columns 4–6, I limit the sample to observations where the business survives. The primary independent variable, *After inheritance*, is an indicator variable equal to one for years following the individual's inheritance event. *After inheritance $\times$ Inherited wealth* is equal to the amount of inherited wealth (measured in millions of 2015 DKK) in post-inheritance years, and zero otherwise. *After inheritance $\times$ Large inheritance* is an indicator variable equal to one for individuals receiving an above-median inheritance in the years following the inheritance event, and zero otherwise. *Past bankruptcy* is an indicator variable equal to one for all serial entrepreneurs in the matched sample. Control variables are defined in Appendix Table 1.A.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Labor and entrepreneurial income			Labor and entrepreneurial income (conditional on survival)		
	(1)	(2)	(3)	(4)	(5)	(6)
After inheritance	-63.8***		-34.8	-59.8**		-31.9
	(-2.64)		(-1.20)	(-2.28)		(-1.02)
After inheritance $\times$ Inherited wealth		-43.1***			-41.1***	
		(-2.91)			(-2.75)	
After inheritance $\times$ Large inheritance			-61.7**			-60.4*
			(-2.00)			(-1.91)
Past bankruptcy	-10.4	-31.2	-11.0	-5.5	-26.3	-6.0
	(-0.30)	(-1.00)	(-0.32)	(-0.14)	(-0.76)	(-0.15)
Pre-entrepreneurial wealth	0.0	0.0	0.0	0.0	0.0	-0.0
	(0.39)	(0.43)	(0.12)	(0.22)	(0.26)	(-0.08)
Pre-entrepreneurial labor income	0.7***	0.7***	0.7***	0.8***	0.8***	0.8***
	(10.90)	(10.73)	(11.16)	(10.48)	(10.30)	(10.75)
Age	-1.1	-1.2	-1.0	0.2	0.1	0.3
	(-0.72)	(-0.79)	(-0.68)	(0.11)	(0.06)	(0.17)
Male	17.1	14.8	16.4	19.8	18.3	18.7
	(0.66)	(0.58)	(0.63)	(0.72)	(0.67)	(0.68)
Years of education	0.6	0.1	0.6	0.1	-0.5	-0.0
	(0.14)	(0.01)	(0.14)	(0.02)	(-0.10)	(-0.00)
Starting a limited liability company	51.0**	55.0**	53.2**	50.5**	56.3**	54.0**
	(2.37)	(2.52)	(2.49)	(2.08)	(2.30)	(2.24)
Years of business experience	7.0**	7.0**	7.0**	6.4*	6.6*	6.3*
	(2.15)	(2.12)	(2.15)	(1.73)	(1.77)	(1.72)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.43	0.43	0.44	0.44	0.43	0.44
Individual-year observations	1,213	1,213	1,213	998	998	998

Table 1.8: Comparing low entrepreneurial quality versus experience of severe losses

This table reports the regression results from a linear probability model examining the effects of experiencing low relative performance in the past and severe losses from prior entrepreneurship on the probability of owning a business after bankruptcy. The dependent variable, *Owner*, is an indicator variable equal to one if the individual owns any company in the year. The primary independent variable, *After inheritance*, is an indicator variable equal to one for years following the individual's inheritance event. The first interacted variable, *Low past performance*, is an indicator variable equal to one if the individual's annual entrepreneurial income has always remained at the bottom decile in the industry before bankruptcy. The second interacted variable, *Severe income loss*, is an indicator variable equal to one if the individual experiences negative entrepreneurial income before bankruptcy. The third interacted variable, *Business debt chapter*, is an indicator variable equal to one if the individual files for bankruptcy under the business debt chapter. The fourth interacted variable, *Full personal liability*, is an indicator variable equal to one if the individual solely owned an unlimited liability company prior to bankruptcy, as opposed to owning a limited liability company. The sample consists of individuals from the treated and control groups, from the year of bankruptcy until five years afterward. Control variables are defined in Table 1.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
After inheritance	0.054*	0.126***	0.103***	0.117
	(1.69)	(2.69)	(2.63)	(1.22)
After inheritance × Low past performance	-0.167*	-0.162	-0.217**	-0.145
	(-1.81)	(-1.60)	(-2.31)	(-1.59)
After inheritance × Severe income loss		-0.129**		
		(-2.19)		
After inheritance × Business debt chapter			-0.125*	
			(-1.95)	
After inheritance × Full personal liability				-0.086
				(-0.86)
Low past performance	-0.049	-0.037	-0.030	-0.021
	(-0.59)	(-0.40)	(-0.37)	(-0.24)
Severe income loss		0.058*		
		(1.65)		
Business debt chapter	-0.049	-0.040	-0.002	-0.048
	(-1.25)	(-1.00)	(-0.05)	(-1.30)
Full personal liability				-0.258***
				(-3.63)
Discharge ratio	0.002**	0.001	0.002*	0.001*
	(2.03)	(1.58)	(1.95)	(1.73)
Pre-bankruptcy wealth	-0.000	-0.000	-0.000	-0.000
	(-1.47)	(-1.07)	(-1.45)	(-0.63)
Pre-bankruptcy income	-0.000***	-0.000**	-0.000***	-0.000***
	(-2.69)	(-2.46)	(-2.62)	(-2.87)
Age	-0.000	0.000	-0.000	-0.002
	(-0.15)	(0.03)	(-0.17)	(-0.80)
Male	0.071*	0.053	0.071*	0.063
	(1.71)	(1.25)	(1.72)	(1.56)
Years of education	0.024**	0.018*	0.024**	0.014
	(2.55)	(1.79)	(2.50)	(1.45)
Year fixed effects	Yes	Yes	Yes	Yes
R^2	0.06	0.05	0.07	0.13
Individual-year observations	2,022	1,902	2,022	2,022

Figure 1.1: Distribution of inheritance amounts

This figure reports the distribution of inheritance amounts. The top panel shows the distribution of inherited wealth according to six bins. The first bin represents the share of individuals who receive an inheritance of a positive amount, but less than 10,000 DKK. The second bin comprises of those receive 10,001 and 25,000 DKK. Similarly, the third, fourth, fifth bins represent those who receive 25,001 to 100,000 DKK, 100,001 to 250,000 DKK, and 250,001 to 500,000 DKK, respectively. The sixth bin comprises those who receive more than 500,000 DKK. Inherited wealth is measured in year-2015 DKK. One Euro is equivalent to DKK 7.45. The bottom panel reports the distribution of the ratio of inherited wealth to dischargeable debt according to four bins. Dischargeable debt is defined as unsecured debt measured at one year before the year of bankruptcy.
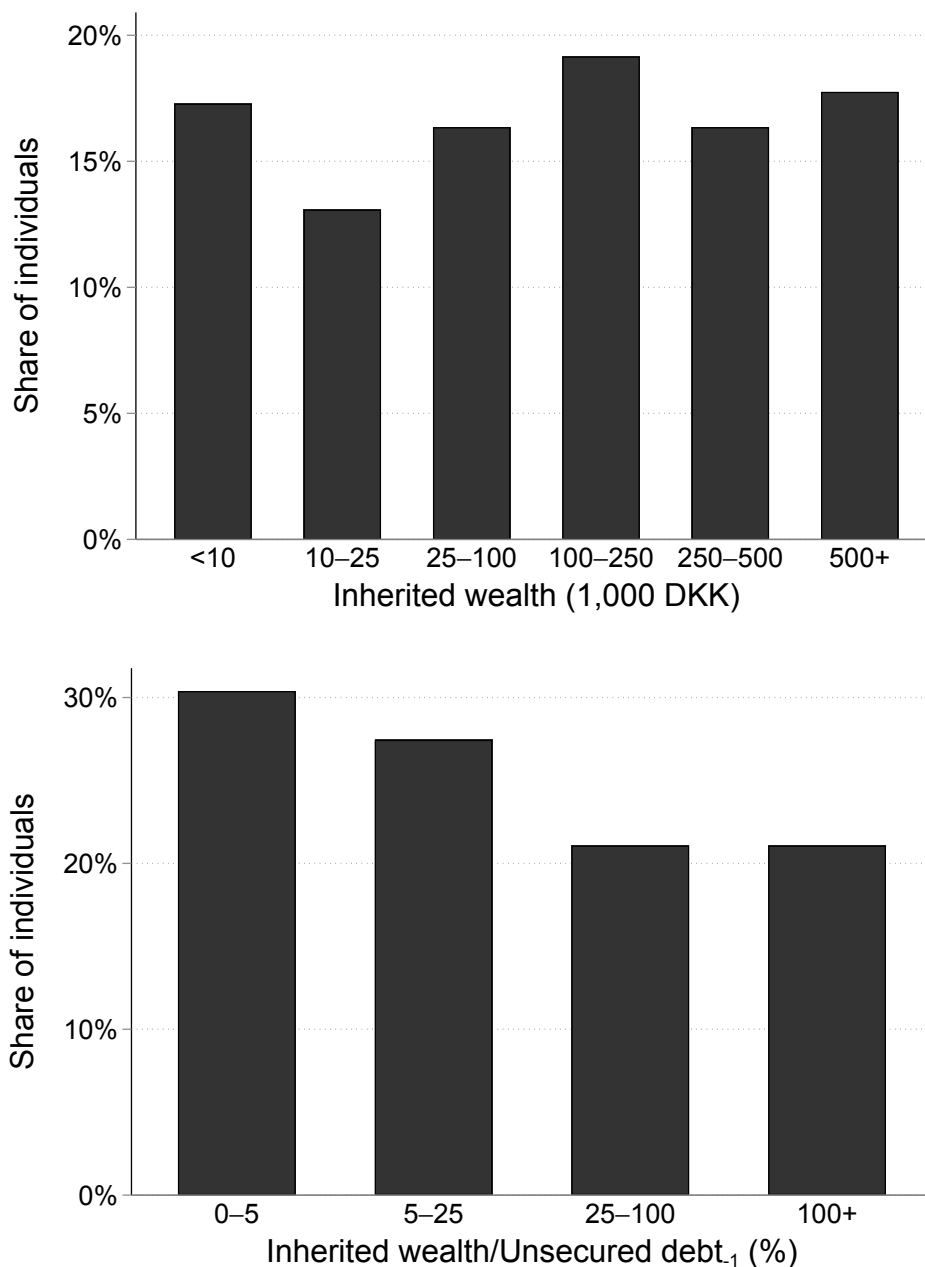
Figure 1.2: Net wealth around bankruptcy and inheritance

This figure plots the dynamics of average net wealth, defined as the difference between total assets and total debts. The horizontal axis shows the years since bankruptcy, with year 0 denoting the year in which the court issues a bankruptcy ruling. The solid line refers to bankrupt individuals who receive an inheritance between event years 0 and 3 (referred to as the 'treated group'). The dashed line refers to a control group of bankrupt individuals who do not receive an inheritance; this group is matched to the treated group based on the following characteristics: the same bankruptcy year, bankruptcy chapter, and gender, similar age at the time of the ruling (±1 year), and net wealth at event year -1. The shaded area between event years 0 and 3 represents the treatment window, during which inheritance events occur. Net wealth is measured in thousands of 2015 DKK (1 Euro ≈ DKK 7.45).
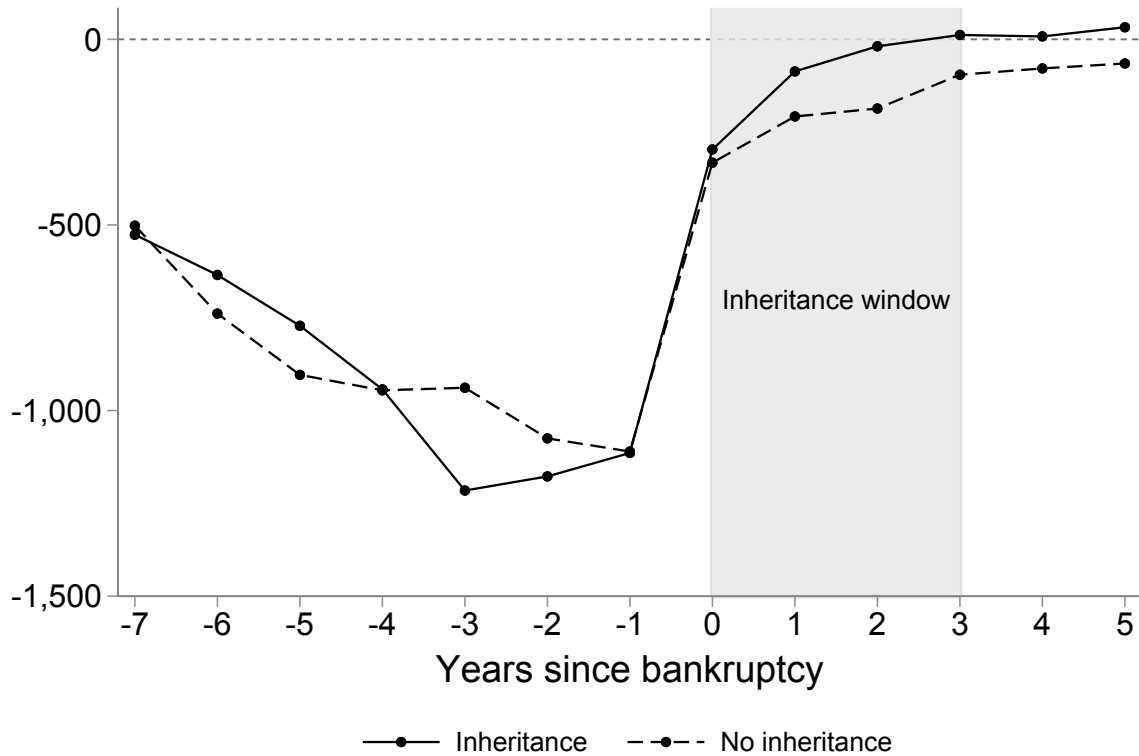
Figure 1.3: Entrepreneurship around bankruptcy

This figure plots the dynamics of the share of individuals who own a business. The horizontal axis shows the years since bankruptcy, with year 0 denoting the year in which the court issues a bankruptcy ruling. The solid line represents bankrupt individuals who receive an inheritance between event years 0 and 3 (the "treated group"). The dashed line refers to a control group of bankrupt individuals who do not receive an inheritance; this group is matched to the treated group based on the following characteristics: the same bankruptcy year, bankruptcy chapter, and gender and is of a similar age at the time of the ruling (± 1 year) with similar net wealth at event year -1. The shaded area between event years 0 and 3 represents the treatment window, during which inheritance events occur.
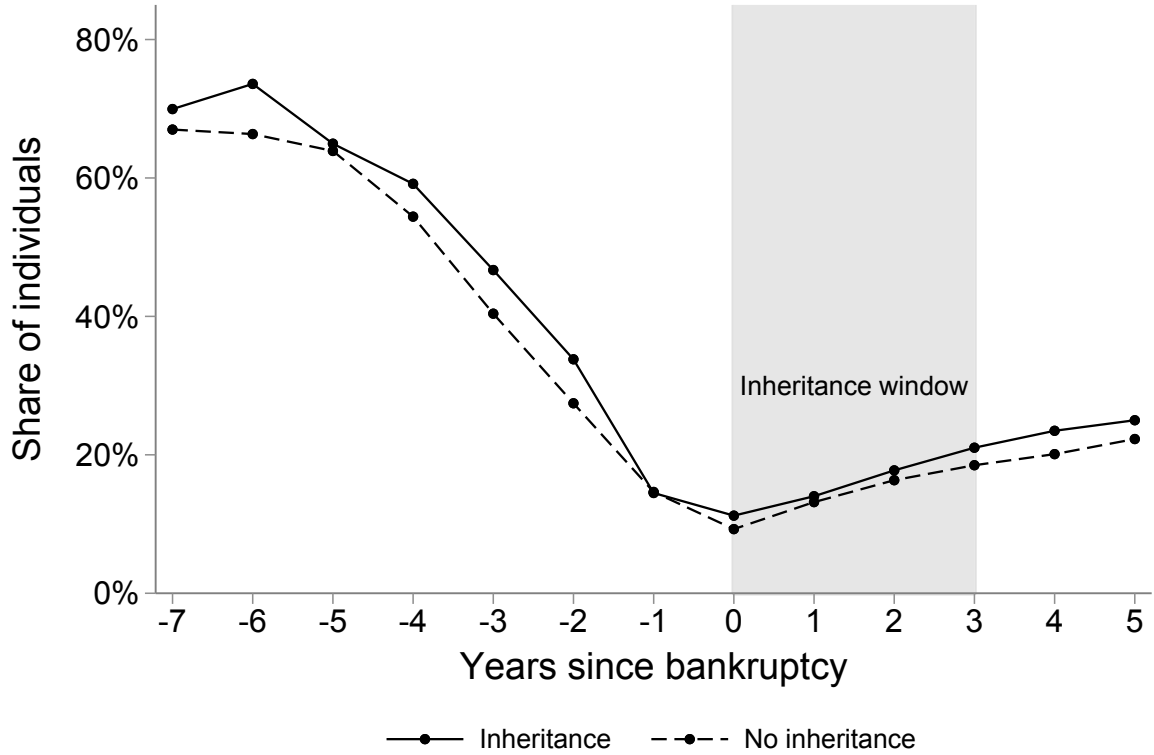
Figure 1.4: Severe losses and entrepreneurship around bankruptcy

This figure plots the dynamics of the share of individuals who own a business, focusing on those who receive an inheritance between event years 0 and 3 (the treated group). The top panel plots the dynamics split by whether the individual in the treated group experiences negative entrepreneurial income before bankruptcy. The bottom panel plots the dynamics split by whether the individual in the treated group files for bankruptcy under the business debt chapter, which is available for entrepreneurs with large business debts. Similar figures based on the third measure of severe losses (*Full personal liability*) and those based on the control group are reported in Appendix Figures 1.A.4 and 1.A.5, respectively.
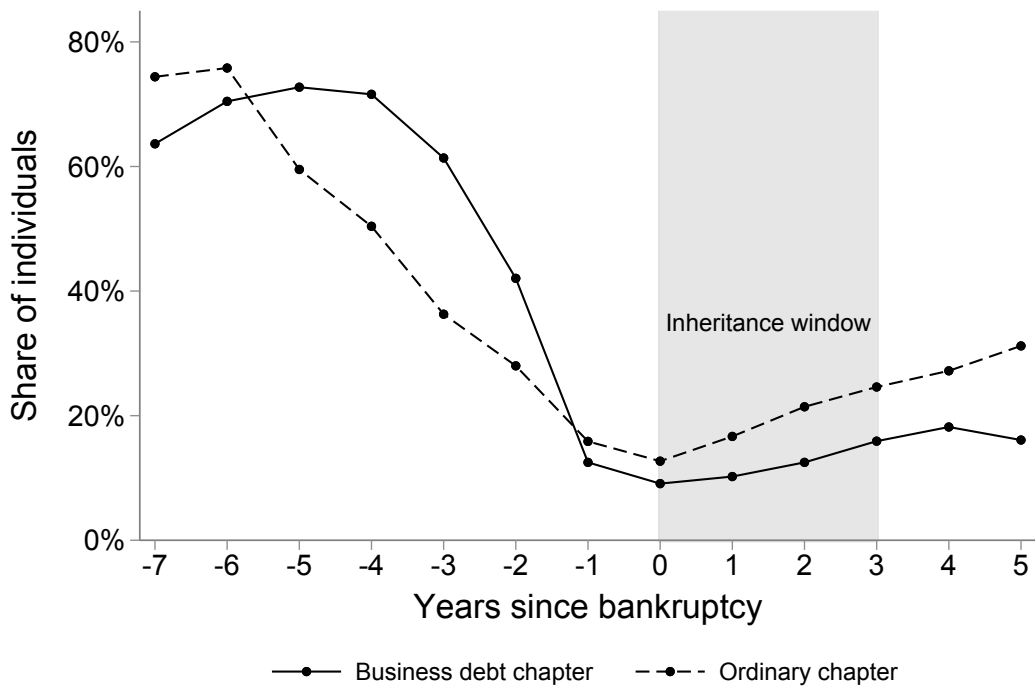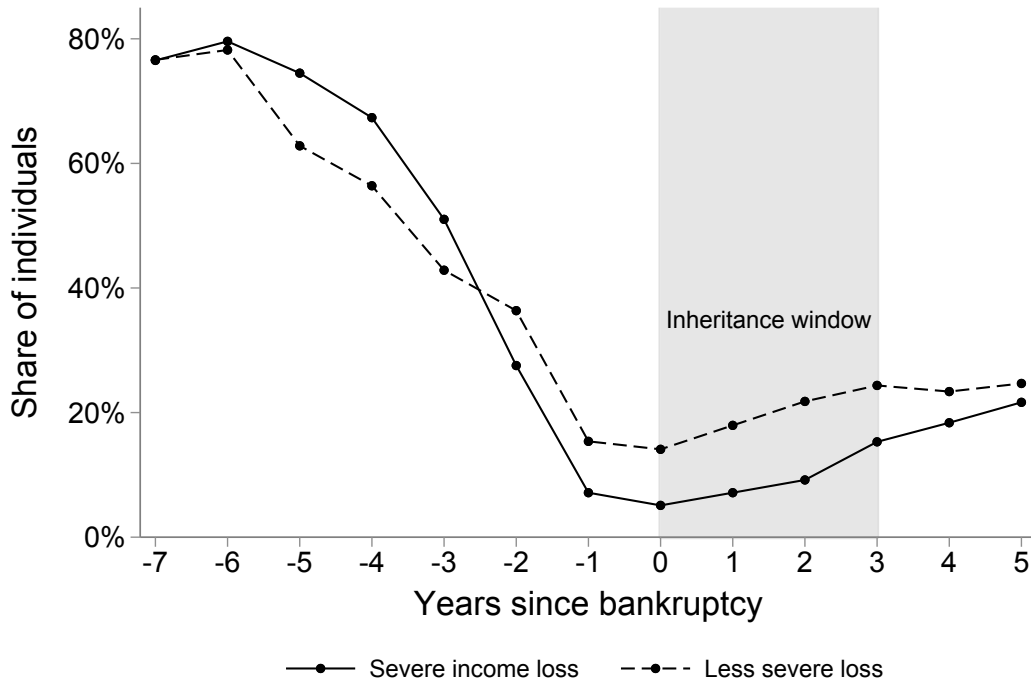
Figure 1.5: Income before and during entrepreneurship

This figure plots averages of labor and entrepreneurial income for the matched sample. The sample consists of two types of individuals: (1) those who restart a business after bankruptcy, referred to as "serial entrepreneurs," and (2) the matched sample of individuals who start a business for the first time and have not experienced bankruptcy, referred to as "first-time entrepreneurs." First-time entrepreneurs are of a similar age (± 1 year) and have the same gender, years of education, and incorporation choice; they have similar pre-entrepreneurial labor income; and they start their businesses at the same time that the serial entrepreneurs restart. I observe individuals from the year of (re)starting a business up to five years afterward. For each type of entrepreneur, the figure reports averages of labor and entrepreneurial income that are (a) measured over the three years before entrepreneurship (labelled "Before entrepreneurship") and (b) measured over all years during entrepreneurship (labelled "During entrepreneurship"). One Euro is equivalent to DKK 7.45. The light grey bars represent serial entrepreneurs who receive an inheritance, and the dark grey bars correspond to first-time entrepreneurs matched to them. Black bars indicate the difference in income levels between the light and dark grey bars.

Labor and entrepreneurial income (1,000 DKK)

| (a) Before entrepreneurship | 234 / 238 / -3 |
| (b) During entrepreneurship | 216 / 260 / -44 |

Legend:
- Serial entrepreneurs who inherit
- First-time entrepreneurs
- Difference
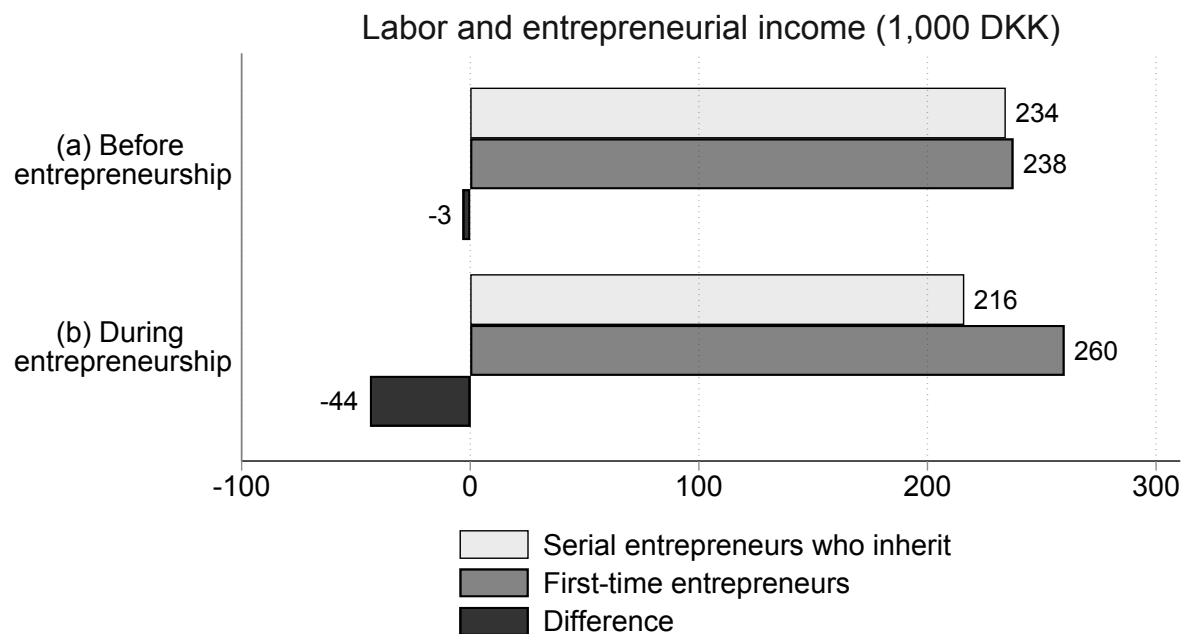
Appendix

1.A Appendix

1.A.1 Detailed procedures in bankruptcy proceedings

Liquidation proceedings for individual debtors (*personlig konkurs*) Under liquidation proceedings for private individuals, debtors liquidate their assets to pay the debt. Liquidation proceedings can be filed by either a debtor or creditor. During my sample period, it costs DKK 750 to file for bankruptcy. In addition, the filer must provide security of DKK 30,000 to cover the administrative costs. If the court decides that the debtor is insolvent, i.e., has no ability to meet debt obligations (of either secured or unsecured debt), it issues a liquidation decree. The court then appoints a trustee who collects and sells the assets owned by the debtor. All assets that are deemed not necessary to have a modest home and live a modest life will be liquidated. The proceeds from the sale are used to repay debt. Importantly, liquidation does not automatically discharge the remaining unpaid debt. To receive the discharge, debtors need to apply separately for bankruptcy proceedings.

Bankruptcy proceedings (*gældssanering*) Bankruptcy proceedings begin when the debtor files with the court in the jurisdiction where he or she lives. There is no fee associated with filing. Upon submission, the debtor is mandated to provide a comprehensive disclosure of their financial situation to the court, including any prospective inheritances or renunciations thereof (Hindborg 2017, pp. 59–60). The court reviews the filing and will dismiss it if it fails to meet the requirements for bankruptcy protection. Key requirements include the following: the majority of debt should *not* consist of debts for private consumption purposes; the debtor must not have engaged in financially irresponsible behavior, such as accumulating debt without attempts at repayment; and the debtor must have a stable financial situation, characterized by a reliable income stream from regular employment and no expected wealth gains. According to Kilborn (2009), about 60% of filings are dismissed at this stage. Filing dismissals are not announced on the State Gazette.

If the requirements are met, the court formally opens a bankruptcy case and announces the opening on the State Gazette. After opening the case, the court summons creditors to submit claims and asks the debtor to submit a repayment plan. Under the repayment plan, the debtor uses all disposable income to pay part of the unsecured debt. The installments are paid monthly and last three to five years. If the plan is feasible, the court accepts it and issues the ruling for bankruptcy, which discharges any remaining unpaid debt.[40] In my sample, about 10% of cases are rejected at this stage. The ruling is announced on the State Gazette. After the ruling, debtors are allowed to shorten the repayment duration by paying all or part of the installments at once. The source of such down payment can be, for example, a loan from a third party or cash windfalls. In addition, a ruling can be cancelled if the debtor grossly neglects the duty (e.g., failing to repay installments) or if the court or a creditor discovers that the debtor failed to disclose material

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[40]If the court considers the debtor to have no ability to pay, such as due to a permanent illness, then the debtor may receive an immediate discharge from the entire unsecured debt.

information to creditors (e.g., concealing information about anticipated increases in wealth or income at filing). The cancellation of a ruling is announced on the State Gazette.

Other differences between ordinary and business debt chapters While bankruptcies under either ordinary or business debt chapters follow similar court procedures, they differ in both filing requirements and consequences after ruling.[41] Here, I list other differences between them that are not mentioned in Section 1.2 of the main text. First, following a ruling under the business debt chapter, the debtor is prohibited from filing for bankruptcy under the same chapter for a subsequent 10-year period. Second, if a debtor under the business debt chapter did not have stable income at the time the plan was approved, upon subsequent acquisition of a stable income source (due to a new job or business comeback), the court may modify the repayment terms to increase the repayment amount.

1.A.2   Identifying inheritances from administrative registers

I outline the procedure for identifying inheritance events. I follow Andersen and Nielsen (2012) and Larsen et al. (2023), who capture inheritance events by combining several administrative registers in Denmark. The only difference from Andersen and Nielsen (2012) is that I examine all types of parental deaths, rather than sudden deaths.

The starting point is to identify deceased parents who cause a termination of the household. Terminations occur when the deceased has no spouse (a widow, widower, divorced, or never married) or both parents die in the same calendar year. For measuring the size of inheritances, I use wealth of the deceased parent(s) at the end of the year preceding death. I aggregate both parents' wealth if they die in the same year. This procedure generates the size of the estate.

Next, I only retain estates with positive wealth, discarding ones with zero or negative values. Estates with net worth exceeding DKK 242,400 in 2006 are subject to a 15% estate tax for children. This threshold is adjusted annually by a price index. Then, I link the estate to the children of the deceased who are over age 18 (beneficiaries). Following Andersen and Nielsen (2012), I restrict the sample to cases where all beneficiaries are children of the deceased. According to Danish inheritance law, inheritances are by default equally divided among children. I therefore equally divide the amount of the estate to yield the size of an inheritance.

In the final step, I link these inheritance events to the bankrupt entrepreneurs in my sample. I include parental deaths that occur after the bankruptcy ruling and up to three years after the year of ruling (event years 0, 1, 2, and 3).

1.A.3   Example of the State Gazette of Denmark

[Figure 1.A.1 about here.]

[Figure 1.A.2 about here.]

1.A.4   Matching serial entrepreneurs to first-time entrepreneurs

I start the matching procedure by identifying 123 failed entrepreneurs who restart after bankruptcy in my main sample (of which 65 individuals experience inheritance events). Next, for each failed entrepreneur who restarts after bankruptcy, I look for a first-time entrepreneur from the Danish population. To control for the individual's entrepreneurial quality, I match failed entrepreneurs who restart after bankruptcy (referred to as the "serial entrepreneurs") with those of similar characteristics who become entrepreneurs for the *first time* (the "first-time entrepreneurs"). The matching takes the following steps:

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[41]Bankruptcy under business debt chapter is commonly referred to as bankruptcy in connection with liquidation (*gældssanering i forbindelse med konkurs)* in Denmark, as it requires the debtor to be under liquidation proceeding before filing for bankruptcy.

1. I require that the first-time entrepreneur starts a business in the same year and has the same incorporation choice (starting an unlimited or limited liability company), years of education, and gender, and is of a similar age (±1 year) as the matched serial entrepreneur.
2. Among potential matches, I select the nearest neighbor based on pre-entrepreneurial income (averaged over three years before the entrepreneurship). I further refine the accuracy of matching by excluding matched pairs with substantial differences in income levels (an absolute difference exceeding 100,000 DKK and a relative difference exceeding 25%).

After matching, my matched sample consists of 110 unique serial entrepreneurs and 110 first-time entrepreneurs. I observe them from the year of starting a business through the five subsequent years.

I provide summary statistics of the matched sample in Appendix Table 1.A.1. The variables are reported at the year of starting the business, except Panel A. Panel A reports levels of wealth and labor income before the start of entrepreneurship. Serial entrepreneurs have lower wealth than first-time business owners, which is expected because serial entrepreneurs are typically in bankruptcy proceedings before restarting. In addition, in Panel C, the two types of entrepreneurs have different years of business experience. This difference in experience is because, by definition, first-time entrepreneurs have no prior business experience. Finally, Panel D shows that around 50% of serial entrepreneurs experience inheritance events. They are marginal entrepreneurs who respond to greater wealth protection and restart a business, and they are, therefore, the focus of second-chance policies.

[Table 1.A.1 about here.]

[Table 1.A.2 about here.]

1.A.5 Additional descriptive figures

[Figure 1.A.3 about here.]

[Figure 1.A.4 about here.]

[Figure 1.A.5 about here.]

[Figure 1.A.6 about here.]

1.A.6 Compare *within* bankrupt entrepreneurs who restart

In Section 1.4.4, I assess post-bankruptcy income levels of serial entrepreneurs by using a comparison group consisting of first-time entrepreneurs with similar characteristics. An alternative way to evaluate the marginal quality of serial entrepreneurs is to compare income levels *within* only bankrupt entrepreneurs. To this end, I return to my main sample that consists of treated and control groups of bankrupt entrepreneurs. Using all bankrupt entrepreneurs, I compare entrepreneurial profits for those who restart with and without receiving an inheritance. This within-group analysis helps answer whether greater wealth protection relaxes financial constraints of high-quality entrepreneurs.

I start with a descriptive analysis. Appendix Figure 1.A.6 plots averages of the labor and entrepreneurial income of individuals who own or do not own a business, split by whether the individual receives an inheritance. The figure shows that, outside of entrepreneurship, individuals who receive an inheritance and those who do not have only a small difference in income. However, when they become entrepreneurs, the two groups exhibit a large difference, driven by decreased income among the treated group.

To more formally compare the returns to serial entrepreneurship between the treated and control groups, I estimate the following OLS regression:

$$Income_{it} = \alpha_y + \beta_1 After\ inheritance_{it} + \beta_2 Owner_{it} + \beta_3 After\ inheritance_{it} \times Owner_{it} + \gamma X'_{it} + \varepsilon_{it}, \quad (1.5)$$

where the dependent variable, $Income_{it}$, is the level of income of individual i in event year t. $Income_{it}$ is measured in two ways: *Labor and entrepreneurial income* and *Total income*. Similar to previous equations, I use three measures to capture the effects of wealth shocks: *After inheritance*, *After inheritance × Inherited wealth*, and *After inheritance × Large inheritance*. The interacted variable, $Owner_{it}$, is an indicator variable equal to one if individual i owns a business in event year t. As in previous equations, I include calendar-year fixed effects (α_y) and control variables X'_{it}. The coefficient of interest is β_3, which estimates the difference in income levels between serial entrepreneurs who receive inheritances and those who do not. Standard errors are clustered at the individual level.

Appendix Table 1.A.3 reports the results estimating Equation 1.5. The results show that the marginal entrepreneurs earn less relative to entrepreneurs who do not receive inheritances. For example, in Column 1, the coefficient on *After inheritance × Owner* suggests that, individuals who own a business after inheritance events earn on average 65,000 DKK less entrepreneurial income for each year of entrepreneurship. In Columns 4–6, I find similarly lower total income. The economic magnitude represents about 20–30% less income, given that the average labor and entrepreneurial income or total income is 222,000 or 272,000 DKK for serial entrepreneurs who receive inheritances.

1.A.7 Additional robustness tests

In this subsection, I present results from additional robustness tests.

[Table 1.A.3 about here.]

[Table 1.A.4 about here.]

[Table 1.A.5 about here.]

[Table 1.A.6 about here.]

Table 1.A.1: Summary statistics for the matched sample of serial and first-time entrepreneurs

This table presents the mean and standard deviation of the main variables for two types of individuals in the sample: (1) those who restart a business after bankruptcy (referred to as "serial entrepreneurs") and (2) the matched sample of individuals who start a business for the first time and do not experience bankruptcy (referred to as "first-time entrepreneurs"). First-time entrepreneurs are of a similar age (±1 year) and have the same gender, years of education, and incorporation choice as the matched serial entrepreneurs. They also have similar pre-entrepreneurial labor income and start their businesses at the same time as the serial entrepreneurs restart. For every variable, I compute the difference in average characteristics between the two types of entrepreneurs and test whether this difference is statistically different from zero. The variables in this table are measured at the year of (re)starting the business, except for those in Panel A. Panel A reports net wealth (measured at one year before the start of entrepreneurship) and labor income (averaged over three years before the entrepreneurship). Both are reported in thousands of 2015 DKK (1 Euro ≈ DKK 7.45). Panel B presents demographic data. Panel C reports entrepreneurial characteristics, such as the incorporation choice and past experience as a business owner. Panel D reports the fraction of entrepreneurs who experience an inheritance event. The last row reports the number of individuals in each group. Standard deviations are in parentheses, and t-statistics are in brackets.

	Matched sample		Difference
	Serial entrepreneurs (1)	First-time entrepreneurs (2)	(1)-(2)
A. Pre-entrepreneurship wealth and income (1,000 DKK)			
Pre-entrepreneurial wealth	-608.4	591.7	-1,200.1***
	(2,633.9)	(1,531.7)	[-4.1]
Pre-entrepreneurial labor income	251.0	254.5	-3.4
	(180.4)	(183.7)	[-0.1]
B. Individual characteristics			
Age	49.2	49.1	0.1
	(6.4)	(6.4)	[0.1]
Male	0.9	0.9	0.0
	(0.3)	(0.3)	[0.0]
Years of education	11.9	11.9	0.0
	(2.0)	(2.0)	[0.0]
C. Entrepreneurial characteristics			
Starting a limited liability company (%)	55.5	55.5	0.0
	(49.9)	(49.9)	[0.0]
Years of business experience	6.4	0.0	6.4***
	(4.0)	(0.0)	[16.7]
D. Experiencing windfall wealth after bankruptcy			
Inheritance event (%)	50.9	0.0	50.9***
	(50.2)	(0.0)	[10.6]
Number of individuals	110	110	

Table 1.A.2: Survival likelihood

This table reports the regression results from a linear probability model, examining the effect of post-bankruptcy inheritances on the probability of remaining as a business owner in the matched sample. The matched sample consists of two types of individuals: (1) those who restart a business after bankruptcy (referred to as "serial entrepreneurs") and (2) the matched sample of individuals who start a business for the first time and do not experience bankruptcy (referred to as "first-time entrepreneurs"). First-time entrepreneurs are of a similar age (±1 year) and have the same gender, years of education, and incorporation choice as the serial entrepreneurs. They also have similar pre-entrepreneurial labor income and start their businesses at the same time as the serial entrepreneurs restart. I observe individuals from the year of (re)starting a business to five years afterward. The dependent variable, *Owner*, is an indicator variable equal to one if the individual owns any company in the year. The primary independent variable, *After inheritance*, is an indicator variable equal to one for years following the individual's inheritance event. *After inheritance × Inherited wealth* is equal to the amount of inherited wealth (measured in millions of 2015 DKK) in post-inheritance years, and zero otherwise. *After inheritance × Large inheritance* is an indicator variable equal to one for individuals receiving an above-median inheritance in the years following the inheritance event, and zero otherwise. *Past bankruptcy* is an indicator variable equal to one for individuals who experienced bankruptcy (and is thus equal to one for all serial entrepreneurs in the sample). Control variables are defined in Appendix Table 1.A.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
After inheritance	0.086*		0.144***
	(1.88)		(2.72)
After inheritance × Inherited wealth		0.008	
		(0.36)	
After inheritance × Large inheritance			-0.124*
			(-1.92)
Past bankruptcy	0.040	0.075	0.039
	(0.78)	(1.60)	(0.75)
Pre-entrepreneurial wealth	-0.000*	-0.000*	-0.000*
	(-1.66)	(-1.78)	(-1.89)
Pre-entrepreneurial labor income	0.000***	0.000***	0.000***
	(3.87)	(3.85)	(3.98)
Age	0.000	0.001	0.001
	(0.12)	(0.15)	(0.18)
Male	-0.030	-0.025	-0.031
	(-0.52)	(-0.42)	(-0.53)
Years of education	0.007	0.008	0.007
	(0.83)	(0.91)	(0.84)
Starting a limited liability company	0.130***	0.128***	0.135***
	(3.18)	(3.11)	(3.27)
Years of business experience	-0.004	-0.004	-0.004
	(-0.58)	(-0.53)	(-0.59)
Year fixed effects	Yes	Yes	Yes
R^2	0.11	0.11	0.12
Individual-year observations	1,213	1,213	1,213

Table 1.A.3: Returns to post-bankruptcy serial entrepreneurship

This table reports estimates from OLS regressions examining the effect of inheritances on serial entrepreneurs' income. The dependent variables, *Labor and entrepreneurial income* and *Total income*, are the levels of labor and entrepreneurial and total income (measured in thousands of 2015 DKK, where 1 Euro $\approx$ DKK 7.45). The primary independent variable, *After inheritance*, is an indicator variable equal to one for years following the individual's inheritance event. The interacted variable, *Owner*, is an indicator variable equal to one if the individual owns any company in the year. *After inheritance $\times$ Inherited wealth* is equal to the amount of inherited wealth (measured in millions of 2015 DKK) in post-inheritance years, and zero otherwise. *After inheritance $\times$ Large inheritance* is an indicator variable equal to one for individuals receiving an above-median inheritance in the years following the inheritance event, and zero otherwise. The sample consists of individuals from the treated and control groups, from the year of bankruptcy until five years afterward. Control variables are defined in Table 1.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Labor and entrepreneurial income			Total income		
	(1)	(2)	(3)	(4)	(5)	(6)
After inheritance	-17.3		-26.2	-8.2		-14.3
	(-1.31)		(-1.56)	(-0.89)		(-1.26)
After inheritance $\times$ Owner	-65.3**		-67.1*	-57.2**		-69.6**
	(-2.15)		(-1.80)	(-2.05)		(-2.14)
After inheritance $\times$ Inherited wealth		4.2			3.3	
		(0.49)			(0.54)	
After inheritance $\times$ Inherited wealth $\times$ Owner		-27.3			-26.3	
		(-1.37)			(-1.53)	
After inheritance $\times$ Large inheritance			16.9			11.5
			(0.74)			(0.71)
After inheritance $\times$ Large inheritance $\times$ Owner			5.0			26.7
			(0.12)			(0.72)
Owner	22.7	-1.9	22.6	-3.5	-24.1	-3.7
	(0.92)	(-0.10)	(0.91)	(-0.16)	(-1.41)	(-0.16)
Business debt chapter	61.8***	64.4***	61.3***	19.9*	21.9*	19.4*
	(4.18)	(4.36)	(4.15)	(1.72)	(1.89)	(1.68)
Discharge ratio	-0.9*	-0.8	-0.9*	-0.5	-0.4	-0.5
	(-1.84)	(-1.64)	(-1.86)	(-1.22)	(-1.07)	(-1.26)
Pre-bankruptcy wealth	-0.0	-0.0	-0.0	-0.0	-0.0	-0.0
	(-0.98)	(-0.96)	(-0.95)	(-0.67)	(-0.65)	(-0.63)
Pre-bankruptcy income	0.6***	0.6***	0.6***	0.5***	0.5***	0.5***
	(8.68)	(8.50)	(8.68)	(8.60)	(8.43)	(8.63)
Age	-4.0***	-4.0***	-4.1***	-2.1***	-2.2***	-2.2***
	(-4.13)	(-4.11)	(-4.19)	(-2.94)	(-2.95)	(-2.99)
Male	58.8***	56.7***	60.0***	42.7***	41.5***	43.5***
	(3.83)	(3.71)	(3.86)	(4.53)	(4.44)	(4.56)
Years of education	0.7	0.2	0.7	0.5	0.1	0.6
	(0.21)	(0.05)	(0.21)	(0.19)	(0.03)	(0.22)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.32	0.31	0.32	0.31	0.30	0.31
Individual-year observations	2,480	2,480	2,480	2,480	2,480	2,480

Table 1.A.4: Experiencing severe income loss versus tenure in business

This table reports the regression results from a linear probability model examining the effects of experiencing low relative performance and severe income loss from prior entrepreneurship on the probability of owning a business after bankruptcy. The dependent variable, *Owner*, is an indicator variable equal to one if the individual owns a company in the year. The main independent variable, *After inheritance*, is an indicator variable equal to one if the year is after the inheritance event for the individual. The first interacted variable, *Severe income loss*, is an indicator variable equal to one if the individual experiences negative entrepreneurial income before bankruptcy. The second interacted variable, *Long tenure in ULC*, is an indicator variable equal to one for the individual whose tenure in ULCs before bankruptcy is above the median, which is 3.5 years. By construction, these two measures are defined only for those who have owned unlimited liability companies before bankruptcy. The sample consists of individuals from the treated and control groups, from the year of bankruptcy until five years afterward. Control variables are defined in Table 1.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)
After inheritance	0.169***
	(3.19)
Severe income loss	0.056*
	(1.73)
Long tenure in ULC	-0.025
	(-0.69)
After inheritance × Severe income loss	-0.129**
	(-2.17)
After inheritance × Long tenure in ULC	-0.090
	(-1.55)
Business debt chapter	-0.006
	(-0.15)
Discharge ratio	0.001
	(1.41)
Pre-bankruptcy wealth	-0.000
	(-1.11)
Pre-bankruptcy income	-0.000***
	(-2.73)
Age	0.001
	(0.24)
Male	0.053
	(1.32)
Years of education	0.019*
	(1.95)
Year fixed effects	Yes
R^2	0.06
Individual-year observations	2,018

Table 1.A.5: The role of age

This table reports the regression results from a linear probability model examining the effect of inheritances after bankruptcy on the probability of owning a business. The dependent variable, *Owner*, is an indicator variable equal to one if the individual owns a company in the year. The main independent variable, *After inheritance*, is an indicator variable equal to one if the year is after the inheritance event for the individual. The first interacted variable, *Age at bankruptcy*, is the individual's age at bankruptcy. The second interacted variable, *Above median age*, is an indicator variable equal to one for individuals whose age at bankruptcy is above the median, which is 48.5 years old. The sample consists of individuals from the treated and control groups, from the year of bankruptcy until five years afterward. Control variables are defined in Table 1.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
After inheritance	0.097	0.011
	(0.51)	(0.28)
Age at bankruptcy	-0.001	
	(-0.45)	
After inheritance × Age at bankruptcy	-0.001	
	(-0.27)	
Above median age		-0.030
		(-0.81)
After inheritance × Above median age		0.074
		(1.31)
Business debt chapter	-0.063*	-0.062*
	(-1.86)	(-1.87)
Discharge ratio	0.002**	0.002**
	(2.42)	(2.34)
Pre-bankruptcy wealth	-0.000*	-0.000*
	(-1.82)	(-1.81)
Pre-bankruptcy income	-0.000*	-0.000
	(-1.66)	(-1.63)
Male	0.101***	0.101***
	(3.15)	(3.15)
Years of education	0.020**	0.019**
	(2.32)	(2.28)
Year fixed effects	Yes	Yes
R^2	0.06	0.06
Individual-year observations	2,480	2,480

Table 1.A.6: Effect of severe income loss across bankruptcy chapters

This table reports the regression results from the linear probability model in equation 1.3 examining the effect of experiencing severe income losses on the probability of owning a business after bankruptcy in two separate samples split by bankruptcy chapter. The dependent variable, *Owner*, is an indicator variable equal to one if the individual owns a company in the year. The main independent variable, *After inheritance*, is an indicator variable equal to one if the year is after the inheritance event for the individual. The interacted variable, *Severe income loss*, is an indicator variable equal to one if the individual experiences negative entrepreneurial income before bankruptcy. By construction, this measure is defined only for those who have owned unlimited liability companies before bankruptcy. The sample consists of individuals from the treated and control groups, from the year of bankruptcy until five years afterward. Control variables are defined in Table 1.1. All columns include calendar-year fixed effects. Standard errors are clustered at the individual level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Ordinary chapter	(2) Business debt chapter
After inheritance	0.128**	0.157*
	(2.31)	(1.89)
Severe income loss	0.051	0.093
	(1.30)	(1.36)
After inheritance × Severe income loss	-0.124*	-0.200*
	(-1.75)	(-1.93)
Discharge ratio	0.002	0.001
	(1.53)	(0.84)
Pre-bankruptcy wealth	0.000	-0.000
	(0.02)	(-1.02)
Pre-bankruptcy income	-0.000***	-0.000
	(-2.76)	(-1.15)
Age	-0.001	0.003
	(-0.30)	(0.52)
Male	0.075*	0.011
	(1.80)	(0.10)
Years of education	0.033***	-0.010
	(2.82)	(-0.65)
Year fixed effects	Yes	Yes
R^2	0.09	0.05
Individual-year observations	1,290	728

Figure 1.A.1: Example of a front page of the State Gazette

This figure displays a sample front page from the State Gazette. The document index located in the bottom-right corner (outlined by a dotted line for emphasis) specifies the page numbers for various sections. For example, notices on bankruptcy and liquidation proceedings (*Gældssanering* and *Konkursboer*) are listed from pages 31 and 39, respectively.

STATSTIDENDE

NR. 174   09.09.2016   113 . ÅRGANG
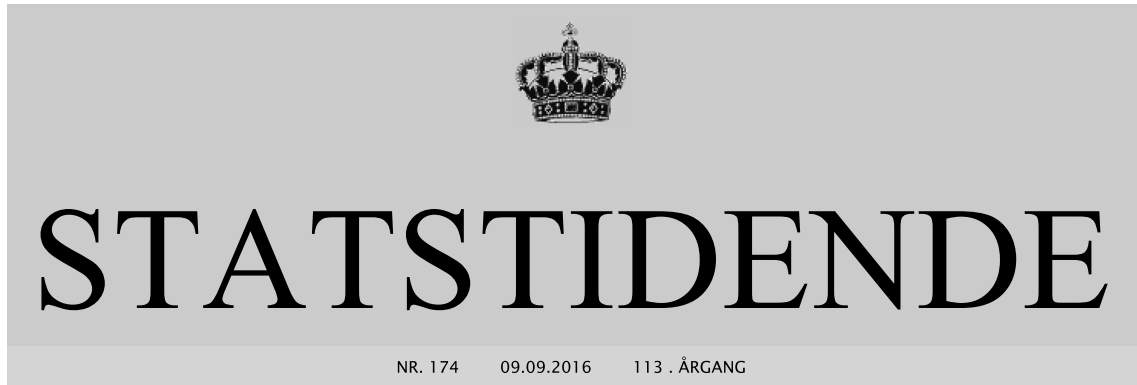
Udgivet i henhold til lov nr. 419 af 31. maj 2000.

STATSTIDENDE

www.statstidende.dk

Udkommer hverdage kl. 04 undtagen mandage, dagen efter 2. påskedag, dagen efter 2. pinsedag, og dagen efter 2. juledag. Statstidende indeholder oversigt over retslige kundgørelser, forskellige kundgørelser fra stat, kommuner og virksomheder, samt mortifikationer i det første nummer i hvert kvartal.

Priser er pr. 15. august 2014 og forefindes på: www.statstidende.dk/priser.html.

Indrykning faktureres i den måned de offentliggøres. Fakturaen vedlægges ikke særskilt dokumentation. På www.statstidende.dk kan meddelelser fremsøges direkte online og i PDF-version i et år.

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CIVILSTYRELSEN

Figure 1.A.2: Example of bankruptcy notices published in the State Gazette

This figure displays a sample page of bankruptcy notices from the State Gazette. Notices on bankruptcy rulings are grouped under the subheading *Kendelse* (Ruling), which is outlined by a dotted line for emphasis. Personally identifiable information — such as full names, dates of birth, CVR-numbers, and street addresses — has been modified to preserve anonymity.

09.09.2016	STATSTIDENDE	38

torerne ikke får mulighed for at øve indflydelse på skifterettens afgørelse om gældssanering.

John Mikkel Jensen har drevet virksomheden JollyShopping.com v/ John Jensen, CVR-nr. 12345678.

Retten i Randers, den 07.09.2016.

Retskreds
Randers

....
S07092016-304
I gældssaneringssagen for

Thomas Thomasen

Fødselsdato 10.02.1945
Møllebakken 9
3400 Hillerød

Sagsnr. 1608-773

indkaldes kreditorerne til møde i Retten i Hillerød, Lokale B, Søndre Jernban-evej 18 B, stuen, 3400 Hillerød,

mandag den 10.10.2016, kl. 13.00,

til behandling af skyldnerens forslag til gældssanering.

Forslaget med bilag ligger til eftersyn i skifteretten.
Udeblivelse medfører, at kreditorerne ikke får mulighed for at udøve indfly-delse på skifterettens afgørelse om gældssanering.

Hillerød, den 07.09.2016.

Advokat Kristian Kristiansen

Retskreds
Hillerød

Kendelse   (Ruling)

S07092016-37
Skifteretten i Hjørring har den 06.09.2016 afsagt kendelse om gældssanering for

Alice Mette Jakobsen

Fødselsdato 10.01.1964
Sønderparken 20
9800 Hjørring

Sagsnr. SKS SKIF-261/2016

på følgende vilkår:

Usikret gæld stiftet af Alice Mette Jakobsen før den 12.05.2016 nedsættes til en dividende på 0,6890%, der betales med en engangsudlodning, når kendelse om gældssanering er en-delig.

Hjørring, den 06.09.2016.

Som skifterettens medhjælper:
Jens Jensen, advokat
Advokaterne Jens and Jesper
Vestergade 3
9800 Hjørring

Retskreds
Hjørring

....
S07092016-92
Sø- og Handelsrettens skifteret har den 06.09.2016 afsagt kendelse om gældssanering for

Jesper Rasmus Jespersen

Fødselsdato 19.10.1938
Waargrethevej 2, 4 th
2300 København S

Sagsnr. G 46/16-K

på følgende vilkår:

Skyldnerens gæld stiftet inden den 12.04.2016 nedsættes til 34,94%, som afdrages over en fem-årige periode.

Sø- og Handelsretten, Skifteretten, den 07.09.2016.

Retskreds
Sø- og Handelsretten

....

S07092016-99
Sø- og Handelsrettens skifteret har den 06.09.2016 afsagt kendelse om gældssanering for

Ali Hesam Amadu

Fødselsdato 14.07.1957
Vestergade 14, 4 th.
2100 København Ø

Sagsnr. G 76/16-K

på følgende vilkår:

Skyldnerens gæld stiftet inden den 13.04.2016 nedsættes til 7,15952% og afdrages over en fem-årig periode.

Sø- og Handelsretten, Skifteretten, den 07.09.2016.

Retskreds
Sø- og Handelsretten

....
S07092016-106
Sø- og Handelsrettens skifteret har den 06.09.2016 afsagt kendelse om gældssanering for

Annie Rasmussen

Fødselsdato 25.12.1980
Christian 8.s Vej 2B, 1 tv.
1805 Frederiksberg C

Sagsnr. G 36/16-K

på følgende vilkår:

Skyldnerens gæld stiftet inden den 08.03.2016 nedsættes til 4,65628% og afdrages over med 4,18564% 4 uger efter endelig kendelse om gældssaner-ing og 0,47064% den 01.05.2017.

Sø- og Handelsretten, Skifteretten, den 07.09.2016.

Retskreds
Sø- og Handelsretten

....

Figure 1.A.3: Inheritance timing

This figure reports the distribution of inheritance timing over event time, grouped by seven bins. The first bin represents the share of individuals who receive an inheritance within the first six months following their bankruptcy ruling date. Similarly, subsequent bins group people by intervals of six months.
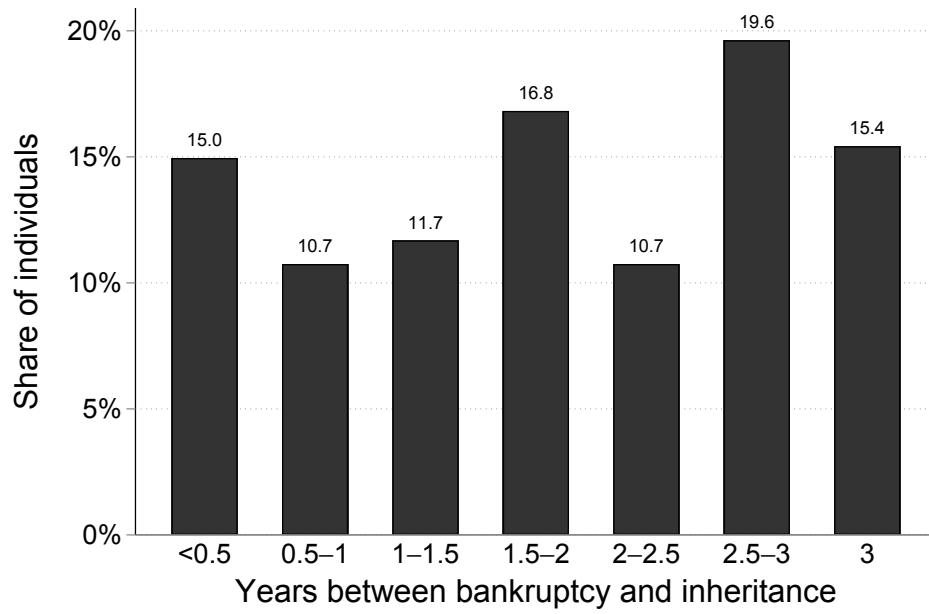
Figure 1.A.4: Being fully personally liable and entrepreneurship around bankruptcy

This figure plots the dynamics of the share of individuals who own a business. The top (bottom) panel displays the dynamics for the treated (control) group. Within each panel, the dynamics are further split by whether the individual solely owned an unlimited liability company before bankruptcy.
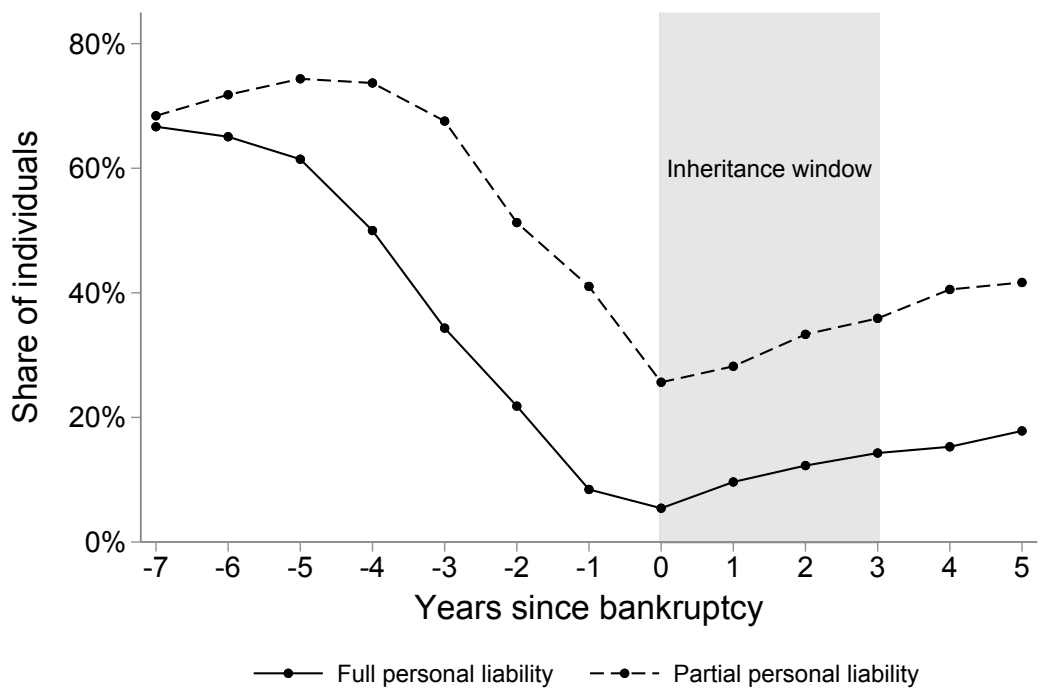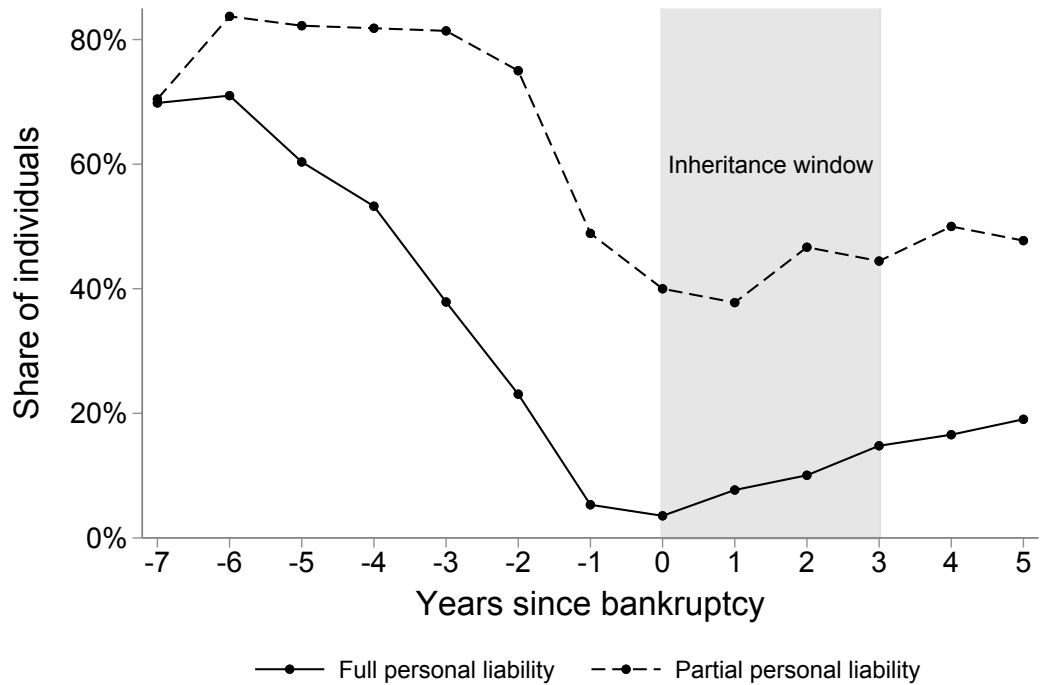
Figure 1.A.5: Severe losses and entrepreneurship around bankruptcy: Control group

This figure plots the dynamics of the share of individuals who own a business among those who do not receive an inheritance and are matched to the treated group ("control group"). The top panel plots the dynamics split by whether the individual in the control group experiences negative entrepreneurial income before bankruptcy. The bottom panel plots the dynamics split by whether the individual in the control group files for bankruptcy under the business debt chapter, which is available for entrepreneurs with large business debts. The shaded area between event years 0 and 3 represents the treatment window, during which inheritance events occur. Figure 1.4 provides a similar visualization based on the treated group.
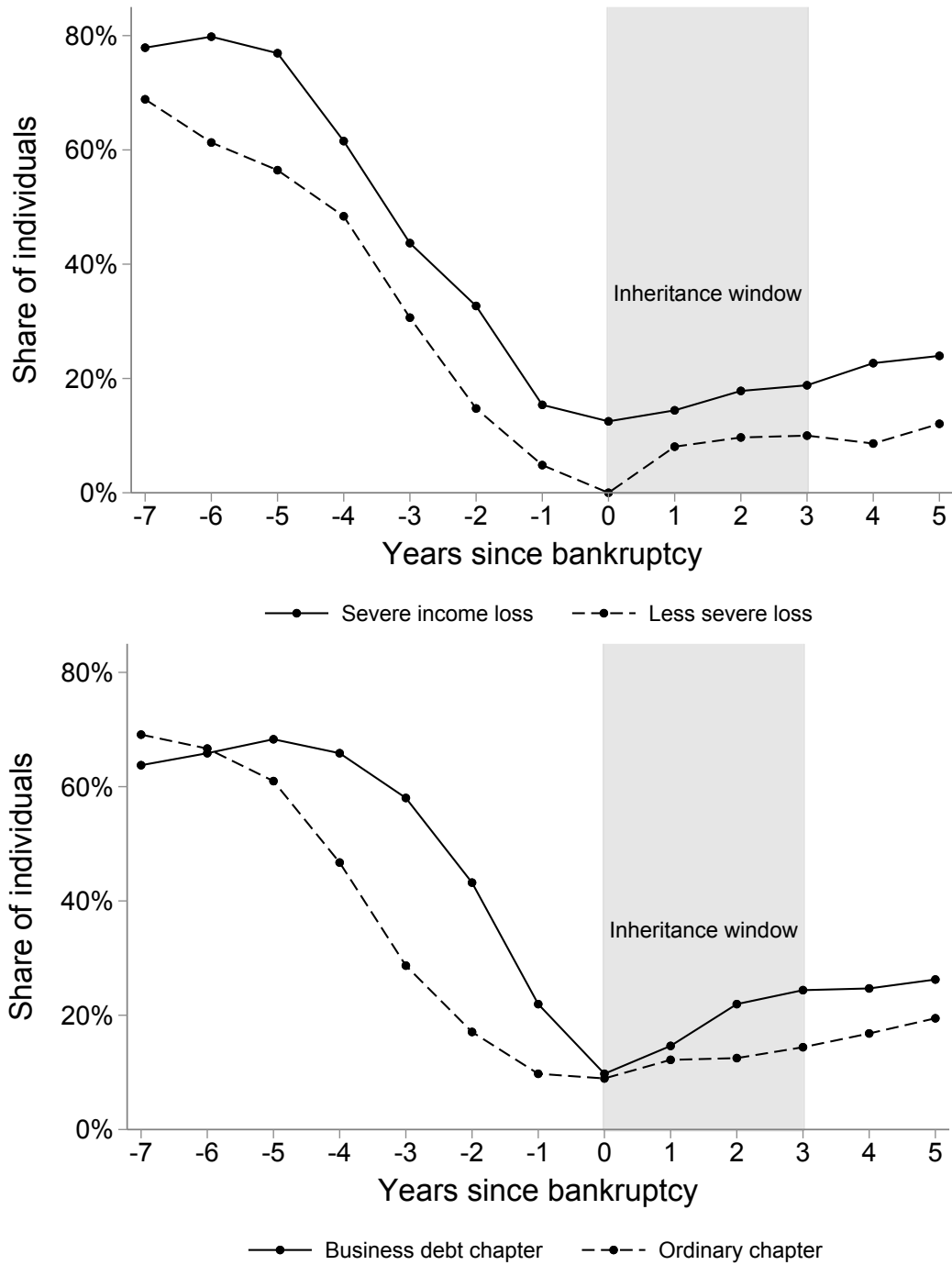
Figure 1.A.6: Income of bankrupt entrepreneurs who restart or do not

This figure plots average income within the main sample, which consists of bankrupt entrepreneurs who receive an inheritance after bankruptcy (the "treated group") and entrepreneurs who do not receive an inheritance after bankruptcy and are matched to the treated group (the "control group"). For both groups, I report averages of labor and entrepreneurial income for individuals (a) who do not own a business after bankruptcy (labelled "Non-entrepreneur") and (b) who start a new business after bankruptcy (labelled "Entrepreneur"). One Euro is equivalent to DKK 7.45. Light (dark) grey bars represent the treated (control) group. I report the difference in income levels between light and dark grey bars in black bars.

Post-bankruptcy income (1,000 DKK)
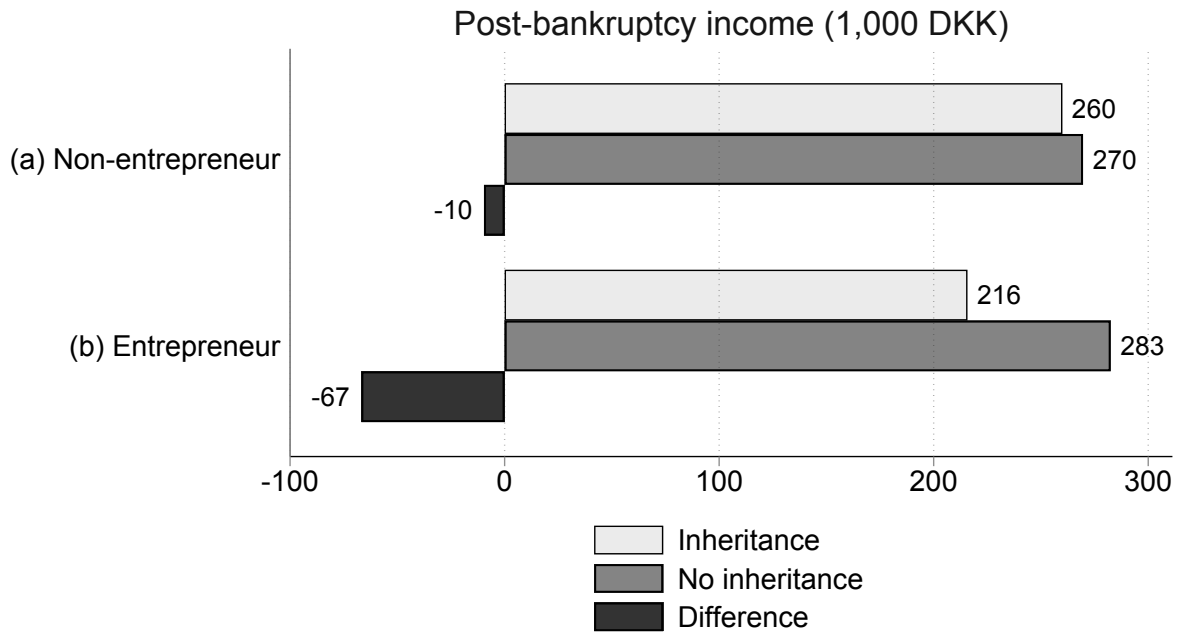
Chapter 2

Biased Judges? Judge Characteristics and Bankruptcy Outcomes

Abstract

Exploiting the random assignment of judges to corporate bankruptcy filings, I examine the effect of judge characteristics on outcomes. First, I find that cases assigned to judges who grew up during the Great Depression are more likely to emerge from bankruptcy, whereas those assigned to judges with economics training and conservative political ideology are more likely to result in liquidation. Second, I show that the case duration is shorter (longer) when the potential case outcome is consistent (inconsistent) with judges' preferences. Third, the judge characteristics do not correlate with post-emergence outcomes. Overall, the findings suggest that the effect of judge characteristics may be concentrated in marginal cases where the economic benefits of liquidation versus emergence are not significantly different.

2.1 Introduction

The bankruptcy institution governs the reallocation of resources of distressed firms. In a frictionless system, the identity of a judge ruling on a bankruptcy case should not affect its outcome. However, a growing literature suggests significant variation in the application of bankruptcy law across judges (Bris et al. 2006; Chang and Schoar 2013; Bernstein et al. 2019). These prior studies document significant judge fixed effects in bankruptcy outcomes, but evidence on why judges exhibit such differences is scant. In this study, I examine the effect of judicial heterogeneity on bankruptcy outcomes by focusing on a specific set of judge characteristics related to personal experiences and ideologies.

To measure the effect of judge characteristics on bankruptcy outcomes, I follow the literature and exploit the random assignment of bankruptcy judges to cases within the filing district (Chang and Schoar 2013; Dobbie and Song 2015; Bernstein et al. 2018; Iverson et al. 2022). The main advantage of random assignment is that it mitigates concerns that judge characteristics may correlate with case characteristics. Moreover, by including court-by-time fixed effects, my identification strategy exploits within-court variation in judge characteristics to address concerns about forum shopping.

I examine the effect of judicial heterogeneity by looking at a specific set of judge characteristics. First, I consider experiences of growing up during the Great Depression and exposure to unemployment as in Malmendier and Tate (2005). I hypothesize that experiencing macroeconomic shocks may induce judges to overestimate the costs of job losses, thus making judges more liquidation-averse.

Second, I consider experiences with economics training. Economics has influenced legal scholars through the field of law and economics. This field emphasizes the importance of competition and economic efficiency in judicial decision-making (Posner 1987). For example, it criticizes labor law and union protections that are not taking economic efficiency into account (Epstein 1983; Posner 1984).[1] I hypothesize that bankruptcy judges with economics training are less sympathetic to labor protections, inducing them to be more liquidation-inclined.

Third, I consider political ideology. Prior studies report that the Republican Party tends to support pro-creditor legislation (Nash and Pardo 2012; Skeel 2014). Moreover, Rachlinski et al. (2006) and Rachlinski et al. (2017) suggest that Republican judges tend to be more pro-creditor than Democratic ones in consumer bankruptcies. Based on this evidence, I expect that Republican judges are more liquidation-inclined.

Lastly, I consider home state experiences. Existing evidence documents home bias, i.e., economic agents show preference or biased beliefs toward hometown-related assets.[2] In a bankruptcy context, judges may show home bias if they are presiding over a bankrupt firm headquartered in states where they grew up (henceforth, their "home states"). I hypothesize that the direction of home bias, either a pro-debtor or pro-creditor proclivity, would depend on preserving whichever stakeholders' interests the judges deem are better for their home states. On the one hand, the home judges could be more debtor-friendly if they are more sympathetic to protecting local jobs, making them more liquidation-averse.[3] On the other hand, they could be more creditor-friendly when the majority of creditors are from the same home state, making them more liquidation-inclined.

The results suggest that my set of specific judge characteristics explains heterogeneity in bankruptcy outcomes. First, I find that firms in cases assigned to judges who experienced the Great Depression in their formative years are 11 percentage points more likely to emerge from bankruptcy. On the other hand, cases assigned to economics-trained and Republican judges are 15 and 13 percentage points more likely to result

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[1]Consistent with the law-and-economics agenda, Ash et al. (2020) find that circuit judges with law-and-economics training are more likely to vote against labor regulatory agencies.

[2]For a related literature on home bias, see Pool et al. (2012) for mutual fund managers' portfolio decisions; Yonker (2017) for CEOs' employment decisions; Jiang et al. (2018) for CEOs' choice of acquisition targets; Cornaggia et al. (2020) for credit analysts' rating behavior; and Lim and Nguyen (2020) for bank CEOs' lending decisions.

[3]Throughout this study, "home judge" refers to a judge assigned to a case in which the bankrupt firm is headquartered in the state where that judge grew up. I also refer to such a case as a "home case".

in liquidation, respectively. Also, firms in cases assigned to home judges have a 17 percentage point lower likelihood of emergence, while holding constant judges' overall tendency toward emergence. These effects are economically large given that the unconditional likelihood of emergence is 60%.

Second, I find that case duration is shorter (longer) when the potential case outcome is consistent (inconsistent) with judges' preferences. When the case outcome is emergence, cases assigned to economics-trained and Republican judges spend more time in court. When it is liquidation, the effect is the opposite. On the other hand, when I examine cases assigned to Democratic judges, they take longer when the outcome is liquidation. The results on duration support my earlier finding that judge characteristics affect case outcomes. The analysis implies that judges may give less (more) scrutiny when the potential case outcome is consistent (inconsistent) with their preferences.

Finally, I also examine whether the different liquidation tendencies are linked to better or worse post-emergence outcomes. Although firms in cases assigned to pro-debtor judges are more likely to emerge from bankruptcy, the emerging firms do not show significantly higher refiling rates or worse post-bankruptcy operating performance. I find similar results when I repeat the same analysis with pro-creditor judges. However, I suggest caution when interpreting the latter result because I do not observe the asset deployment of liquidated firms, making it hard to assess whether viable firms are excessively liquidated when assigned to pro-creditor judges. Overall, the results suggest that the effect of judge characteristics may be concentrated in marginal cases, where the economic benefits of liquidation versus emergence are not significantly different.

The lack of correlation between liquidation tendency and future firm performance is surprising. For instance, Chang and Schoar (2013) examine judge fixed effects and find that firms assigned to pro-debtor judges show significantly lower future sales and survival rates. On the other hand, this study evaluates specific characteristics, as opposed to the aggregate effect. I supplement their findings in that judges with strong overall biases may be driving the correlation.

This study contributes to the growing literature on how bankruptcy outcomes vary across individual judges by documenting significant judge fixed effects (Bris et al. 2006; Chang and Schoar 2013; Dobbie and Song 2015; Bernstein et al. 2018; Bernstein et al. 2019). I add to this literature by asking more generally which individual characteristics drive judicial heterogeneity, which has been previously aggregated into the judge fixed effects.

This study is most closely related to Iverson et al. (2022), who show that bankruptcy judges' on-the-bench experience affects case duration, while their political-party affiliation does not. The main difference between the two studies is that I examine a broad set of judge characteristics that are distinct from on-the-job experience and analyze how they affect both the probability of a firm emerging from bankruptcy and the duration of the case. Moreover, my analysis on the impact of political ideology differs from that of Iverson et al. (2022) in several aspects. First, I classify political ideology into three categories rather than two, accounting for judges who are neither strictly Republicans or Democrats. Second, I expand the sample on political ideology by using political donation data in addition to voter registration data. Third, I examine differential effects of judge preferences on case duration by splitting the sample into cases that end in liquidation versus emergence. When I look at the sample unconditionally, I also find no evidence that case duration differs across political ideology.

My second contribution to the literature is to highlight the effect of judge-level heterogeneity on outcomes. Recent literature shows that bankruptcy courts differ in the level of expertise and efficiency (Ayotte and Skeel 2004; Ponticelli and Alencar 2016; Iverson 2017; Ellias 2018; Li and Ponticelli 2022; Müller 2022), while others further argue that courts have different levels of bias toward debtors or workers (LoPucki 2005; Araujo et al. 2023). In contrast to these studies, I exploit within-court variation in judicial characteristics and show how it affects bankruptcy outcomes.

Lastly, this study contributes to the broader literature on the effect of judicial biases on case outcomes outside of bankruptcy courts, such as criminal sentencing and the amount of fines. The documented biases

include loss aversion (Sharfman 2005), anchoring effects (Rachlinski et al. 2006; Bordalo et al. 2015; Spamann et al. 2020), racial and in-group biases (Shayo and Zussman 2011; Abrams et al. 2012), and political affiliation (Huang et al. 2019; Gormley et al. 2021; Canayaz and Gustafson 2021). I add to this body of work by examining possible judicial bias in bankruptcy courts.

My findings have policy implications for case assignment to judges within the bankruptcy system. While random assignment does not eliminate the possibility that different preferences or beliefs of judges influence marginal cases, I find no evidence that judges allow clearly viable (unviable) firms to liquidate (emerge). An alternative system without random assignment, however, would generate problems such as judge shopping.[4] Random assignment, in comparison, mitigates the friction from judicial bias in the bankruptcy system.

The study proceeds as follows. Section 2.2 introduces the institutional background, providing details about Chapter 11 bankruptcies and the role of judges. Section 2.3 describes the data and presents summary statistics. Section 2.4 outlines the identification strategy and analyzes the effect of judge characteristics on bankruptcy outcomes. Section 2.5 further investigates various channels through which the judge characteristics might affect outcomes by exploiting heterogeneity in case characteristics. Section 2.6 presents placebo tests and results with alternative specifications. Section 2.7 concludes.

2.2 Background on Chapter 11 and bankruptcy judges

2.2.1 Chapter 11 filings

In the U.S., bankruptcy formally begins with a firm filing a voluntary petition (or by creditors filing an involuntary petition) in a bankruptcy court. Large firms typically file a voluntary petition under Chapter 11 of the U.S. Bankruptcy Code. Chapter 11 allows corporations to reorganize under court supervision, whereas under Chapter 7, they expect to liquidate all assets. Corporations can file in a bankruptcy court in a judicial district where they (1) are incorporated; (2) have their principal place of business; (3) have principal assets; or (4) have an affiliate that filed for bankruptcy. Large firms may exploit this latitude to choose a favorable court among 94 bankruptcy courts, a practice referred to as "forum shopping." (LoPucki and Whitford 1991; Eisenberg and LoPucki 1998)[5]

Appendix Table 2.A.2 presents summary statistics of filings for my sample in 62 (out of 94) bankruptcy courts in which at least one large Chapter 11 case was filed between 1980 and 2020. The table reports, for each court, the total number of filings and unique judges as well as the share of cases in which firms file outside the state in which they are headquartered. The table shows that many firms file in the District of Delaware and the Southern District of New York. These two courts have a high number of judges, which includes visiting judges, to manage the caseload. In addition, the two courts show a higher share of cases in which firms file outside their headquarters state, suggesting that debtors may prefer these courts.

The outcome of a Chapter 11 filing can be broadly classified as either emergence from bankruptcy or liquidation. Under emergence, a reorganization plan is confirmed and the bankrupt firm continues as a going concern. Under liquidation, it shuts down or stops operating as a separate business. Its assets are sold or acquired by another firm, and the employees lose their jobs. Emergence (liquidation) is economically justified when the going concern value of the business is greater than (less than) the value of the assets sold individually (Altman et al. 2019, p.43).

The main focus of this study is on regular "free fall" Chapter 11 filings, which comprise approximately 70% of large corporate bankruptcies since 1990. The remaining 30% of cases are prepackaged or prenegotiated (Altman et al. 2019, p.59). In prepackaged cases, the debtor has reached agreement on a reorganization plan with most of its creditors prior to the filing. In prenegotiated cases, which are similar to prepackaged cases,

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[4]Case law states that random assignment "avoids public perception or appearance of favoritism in assignments, and reduces opportunities for judge-shopping" (*Tripp v. Executive Office of President*, 196 F.R.D. 201, 202 (D.D.C. 2000)).

[5]The number of courts is larger than the number of states because a state can have more than one court.

the formal agreement on the plan is reached after the filing. Prepackaged and prenegotiated cases are much more likely to emerge from bankruptcy and have far shorter duration than the free fall cases (Altman et al. 2019, p.60). Since the reorganization plan is already agreed on and the outcome is effectively determined around the time of filing, there is not much room for judges to affect the probabilities of emergence in these prepackaged or prenegotiated cases (Bris et al. 2006; Chang and Schoar 2013; Antill 2022). Therefore, I focus on the free fall cases to study the effect of judge characteristics, and I later confirm that such an effect is insignificant in the prepackaged and prenegotiated cases.

2.2.2 The role of judges

A growing literature shows that judges play an important role in the bankruptcy process. They can influence it by, for instance, deciding which motions (filed by the debtor, creditors, etc.) to approve. Consistent with judges playing an active role, Bris et al. (2006) and Iverson et al. (2022) find that case duration differs across judges, and Chang and Schoar (2013), Bernstein et al. (2019), Antill (2022), and Hüther and Kleiner (2022) all show that judges have different tendencies toward liquidation. Moreover, Goyal et al. (2021) suggest that bankruptcy lawyers tailor their practices to judge-specific preferences, thereby reducing case duration. While judges can affect bankruptcy outcomes, Iverson (2017, p. 5373) argues that they face negligible monetary costs from allowing unviable firms to emerge since "in practice it is nearly impossible to determine when this occurs."

Bankruptcy judges are appointed to specific bankruptcy courts and handle all types of bankruptcy filings within that court. They occasionally visit other courts to preside over cases filed there, when the other courts have large workloads or need to avoid conflicts of interest among their own judges (Iverson et al. 2022).[6] An average (median) judge in my sample presides over 3.1 (1) cases. In addition, 83% (62%) of the cases are assigned to judges who preside over more than 1 (3) case(s). The judge with the highest number of cases in my sample is Judge Peter J. Walsh with the District of Delaware, the most popular bankruptcy court, who presides over 51 cases.

An important institutional feature in the bankruptcy system is that judges are randomly assigned to cases within filing districts, i.e., courts.[7] The random assignment ensures that, within a court, judge assignments are uncorrelated with firm characteristics. Empirical findings also support that the random assignment holds in Chapter 11 bankruptcies (Chang and Schoar 2013; Bernstein et al. 2018; Bernstein et al. 2019). In particular, Iverson et al. (2022), Antill (2022), and Seth (2022) document that the lack of correlation between judge assignment and firm characteristics holds even for bankruptcies of large public firms, which are the focus of this study. In some cases, after initial assignment, a new judge is reassigned to the case. Such judge reassignments typically occur when the initially assigned judge leaves the case (due to recusal, retirement, or medical leave) or when a case is transferred to another court. In keeping with the prior literature (e.g., LoPucki and Doherty 2015), I look at the judge who signs the final disposition order, i.e., the presiding judge.[8]

The random assignment makes it implausible that some judges systematically receive more cases warranting liquidation. Exploiting this feature, the literature has documented significant judge fixed effects, such as differentiating pro-debtor versus pro-creditor judges. However, there is scant evidence on whether and which judge characteristics affect this judicial heterogeneity. In this study, rather than aggregating all individual characteristics into the judge fixed effects, I look at specific characteristics. By showing that these judge

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[6]In my sample, 35 cases are assigned to "visiting judges," most of which are filed in Delaware.

[7]If a court has multiple divisions, a case is randomly assigned to a judge within the division in which it is filed. However, this difference in the level of randomization is unlikely to invalidate the main identifying assumption because Iverson et al. (2022) find that nearly all large corporate bankruptcies are filed in the main division of each district.

[8]For 640 cases, I have information on both (1) the judge who is initially assigned to the case and (2) the judge who presides over the case (that is, who signs the disposition order). In 90 of those cases have difference between (1) and (2) are different judges.

characteristics can explain a significant amount of the variation in outcomes, I provide evidence that helps disentangle the elements within judge fixed effects that influence the outcomes.

2.3 Data

2.3.1 Data and variable construction

I combine several databases to study the effect of judge characteristics on bankruptcy outcomes. My sample covers large, public U.S. companies that filed for Chapter 11 bankruptcy between 1980 and 2020.[9]

Bankruptcy cases

The starting point to obtain large corporate bankruptcies is the UCLA-LoPucki Bankruptcy Research Database (BRD). To be included in the sample, I require that a firm file an annual report (form 10-K) with the Securities and Exchange Commission within three years prior to the bankruptcy filing and that it have assets worth $100 million or more, measured in 1980 dollars. This gives me 1,207 cases. I drop 25 cases that are actually Chapter 7 at filing and another six cases that are still pending. When the final outcome (emergence or liquidation) is missing in the BRD, I supplement it by inspecting related court documents from Public Access to Court Electronic Records (PACER). I drop 14 cases for which I still cannot determine the outcome. The remaining 1,162 cases are split in two: the main and placebo samples. The main sample consists of 771 unique bankruptcy cases that are neither prepackaged nor prenegotiated. The placebo sample consists of 391 cases that are prepackaged or prenegotiated. I do not expect to find the effect of judge characteristics in these latter cases since the literature argues that their outcomes are largely predetermined at the time of filing (Bris et al. 2006; Chang and Schoar 2013; Antill 2022). The predetermined characteristic of prepackaged or prenegotiated cases is useful, however, for a later placebo test.

I obtain case characteristics from various sources. The BRD contains the following detailed information on each case: the date of filing, operational information (e.g., assets, liabilities, industry of the filer), the presiding judge, case duration, and the final outcome (emergence versus liquidation). When accounting information is not available on BRD, I supplement with data from Compustat. I retrieve additional information on capital structure, such as the ratio of secured debt to total assets, from Capital IQ.

Bankruptcy judges

I collect biographical information on a total of 250 presiding judges in the BRD sample. The sources include official court websites, legal databases (e.g., American College of Bankruptcy, Martindale-Hubbell Law Directory, LexisNexis, Ballotpedia.com), and web searches (which return obituaries, judges' autobiographical interviews, etc.). This process yields a comprehensive dataset on judges' personal attributes such as birth year, home state, gender, educational background, military service status, and professional experience before and after becoming a judge. I provide more details on the data-collection procedure in Appendix 2.A.2.

I focus on four specific types of judicial characteristics. First, I identify judges born between 1920 and 1939 as Depression-baby judges (Malmendier and Tate 2005; Malmendier et al. 2011). I obtain birth year information for all 244 U.S.-born judges in the BRD sample and identify 58 as Depression-baby judges.[10]

Second, I identify judges as having had economics training if they meet either or both of the following two conditions: (1) following Ash et al. (2020), judges who have ever attended the Manne program, which is an intensive economics course that ran between 1976 and 1999, or (2) following Cao (2020), judges who

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[9]When analyzing post-bankruptcy performance measures, such as the refiling rate, I use the sample between 1980 and 2017 to ensure adequate time to observe post-bankruptcy performance.

[10]For the remaining 6 judges born outside the U.S., I treat the indicator for Depression-baby judges as missing.

received a J.D. degree after 1970 from law schools that have a strong law and economics culture.[11] I obtain the list of entire attendants to the Manne program attendees from Butler (1999).[12] Among 248 judges with non-missing law school information, I identify 13 judges with economics training.

Third, I identify judges' political ideology by combining voter registration and political donation data. For the voter registration data, I follow the methodology of Kuvvet (2021) and obtain records from 20 U.S. states that are publicly accessible at VoterRecords.com and Stephen P. Morse's "One-Step Search Tool" generator.[13] I obtain political donation data from the U.S. Federal Election Commission (FEC), available from 1979. I link the judges in my sample with the FEC donors using the name and occupation following Hong and Kostovetsky (2012) and Lee et al. (2014).

I classify judges' political ideology into one of three categories: Democrats, Republicans, and "nonpartisans." Accounting for nonpartisans can incorporate more nuanced information about political ideology than a binary classification. I code judges as nonpartisan when they do not show consistent voting or donating behavior for a single party between either the Democratic or the Republican parties.[14] The primary classification of political ideology is based on the voter record; I supplement it with the donation record when the voter record is missing.[15] I am able to determine political preference categories for 121 judges, of which 77 are Democrats, 25 are Republicans, and 19 are nonpartisans.

Fourth, I classify a case as having a "home judge" when judges are assigned to the case in which the firm headquarters state is identical to the judge's home state. The primary definition of home state is the birth state. If information on the birth state is missing, I supplement it with the state where judges "grew up" until 18 years old (such as the state of the high school they attended). I obtain home state information for 183 judges.

2.3.2 Summary statistics

[Table 2.1 about here.]

Table 2.1 presents summary statistics for my sample. The top panel shows judge characteristics for the 771 firms in my sample. Twenty-seven percent of cases are assigned to judges who experienced the Great Depression and 5% to judges who have economics training. In addition, 12% and 78% of cases are assigned to Republican and Democratic judges, respectively, while the remaining 10% are assigned to nonpartisan judges. In 24% of the cases, the judge's home state and the firm headquarters states are the same. Notably, information on the political ideology and home state of judges has more missing values than other characteristics, which reduces the sample in tests examining these two characteristics. The middle panel reports firm characteristics, measured by the closest available data to the filing date. The average firm in my sample is large, with assets of $5,196 million in 2020 US dollars. It is also highly levered and not profitable, with a leverage ratio of 0.97 and a return on assets of -0.05, on average. The bottom panel reports bankruptcy outcomes. Sixty percent of firms in my sample emerge from bankruptcy, with an average of 21 months in bankruptcy. Conditional on emergence, recidivism is relatively rare since only 9% of the firms file again for bankruptcy within 3 years of emergence. I provide detailed variable definitions in Appendix Table

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[11]These law schools are the University of Chicago, the University of Virginia, the University of Southern California, the University of Miami, Emory University, and George Mason University. For more details on the definition, see Cao (2020).

[12]I cross-check the list with the annual reports that contain attending judges, available at a public repository. See https://www.muckrock.com/foi/virginia-128/judge-attendance-at-events-conferences-and-seminars-36073/

[13]These states are Alaska, Arkansas, Colorado, Connecticut, Delaware, District of Columbia, Florida, Idaho, Louisiana, Michigan, Mississippi, Nevada, New Jersey, New York, North Carolina, Ohio, Oklahoma, Rhode Island, Utah, and Washington. Notable omissions are California, Pennsylvania, and Texas.

[14]Specifically, a nonpartisan judge is the one who either (1) has a voter record without any party affiliation ("unaffiliated"), (2) has a voter record with a party other than the Democratic or the Republican parties (e.g., the Independence Party), or (3) has no voter record in the state that the judge works ("unregistered"), or (4) makes donations to both parties, while the voter record is either unaffiliated or unregistered.

[15]The voter records take priority over the donation records since Kempf and Tsoutsoura (2021) argue that the former is better at isolating political ideology from the intention of political influence and social pressure.

2.A.1. Appendix Table 2.A.3 additionally reports summary statistics for judge characteristics at the judge level.

2.4 Judge characteristics and bankruptcy outcomes

2.4.1 Identification strategy

In this section, I introduce the identification strategy to estimate the effect of judicial characteristics on bankruptcy outcomes. My main identifying assumption relies on the institutional feature that bankruptcy judges are randomly assigned to cases within a filing court (Chang and Schoar 2013; Bernstein et al. 2019; Antill 2022; Iverson et al. 2022). Random assignment of judges mitigates the concern that judge and firm characteristics are correlated.

To examine the impact of judicial characteristics on bankruptcy outcomes, I estimate the following ordinary least squares (OLS) regression:

$$Y_{ircjt} = \alpha_r + \alpha_{ct} + \beta\, JudgeChar_j + \gamma X'_{ijt} + \varepsilon_{ircjt}, \tag{2.1}$$

where the dependent variable Y_{ircjt} is the bankruptcy outcome of firm i in industry r, which filed for bankruptcy in court c and is assigned to judge j in year t.

In the analysis of bankruptcy outcomes, Y_{ircjt} is an indicator that takes a value of one if firm i emerges from bankruptcy. In the analysis of the bankruptcy duration, Y_{ircjt} is the log of the number of months the case spends in bankruptcy.

The main coefficient of interest is β, which captures the impact of judicial characteristics on bankruptcy outcomes. $JudgeChar_j$ is a set of time-invariant characteristics of judge j. Depending on the test, I examine one of the following indicator or categorical variables: judges' birth cohort, experience with economics training, or political ideology. Furthermore, in all tests except that on Depression-baby judges, I control for gender, military service status, and judges' on-the-bench experience, the last of which Iverson et al. (2022) find relevant for explaining case duration.[16]

I include two sets of fixed effects in all specifications. First, I include court-by-filing year fixed effects (α_{ct}).[17] Despite the within-court random assignment of judges, a remaining issue is that bankrupt firms may prefer filing in a favored court, a behavior known as forum shopping.[18] The court-by-year fixed effects ensure that I isolate judge-level variation in bankruptcy outcomes, which is the subject of this study, not court-level variation. Second, I include industry fixed effects (α_r), based on the Fama-French 12 industry classification, to ensure that β does not capture variation in the probability of emergence and bankruptcy duration at the industry level. I cluster standard errors at the filing court level following Iverson et al. (2022) and Antill (2022). Equation (2.1) ensures that the identification strategy compares the bankruptcy outcomes of firms that filed in the same court and year, but are assigned to different judges.

Following Iverson et al. (2022), I include the following firm-level control variables in X'_{ircjt}, measured by available data closest to filing year t: (1) the log of the book value of assets (in 2020 dollars); (2) the log of the number of subsidiaries filing together; (3) the leverage ratio; and (4) the return on assets.

When I examine home bias, I modify the baseline specification, Equation (2.1), and estimate the following

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[16]When I test the effect of Depression-baby judges, I drop on-the-bench experience from the control variable since it is collinear to the dummy for older cohorts.

[17]As a robustness check, Section 2.6.2 shows that my results are similar when using less restrictive fixed effects, that is, separate court and year fixed effects.

[18]For example, LoPucki (2005) argues that bankruptcy courts such as Delaware and the Southern District of New York are more debtor-friendly.

OLS regression:

$$Y_{ircjt} = \alpha_r + \alpha_{ct'} + \alpha_j + \beta\, HomeJudge_{ij} + \gamma X'_{ijt} + \epsilon_{ircjt}, \tag{2.2}$$

where $HomeJudge_{ij}$ is an indicator variable equal to one if firm i's case is assigned to judge j whose home state is identical to the firm headquarters state. The main coefficient of interest is β_1, which estimates the effect of the bankruptcy case being assigned to a home judge.

In Equation (2.2), I add judge fixed effects (α_j) to control for judges' overall liquidation tendency. The judge fixed effects ensure that I isolate within-judge variation.[19] In addition, different from Equation (2.1), I replace court-by-year fixed effects with court-by-decade fixed effects ($\alpha_{ct'}$) to ensure I have sufficient variation in the data: court-by-year fixed effects are highly restrictive in that they require a judge to be assigned to both home and non-home cases within the same court and year.[20] Equation (2.2) ensures that the identification strategy on home bias compares the bankruptcy outcomes of firms that filed in the same court and decade and are assigned to the same judge, but whose headquarters states may or may not be the same as the judge's home state.

2.4.2 Main results

I begin with a descriptive analysis of how emergence probabilities differ for cases assigned to judges with different characteristics. Figure 2.1 plots average emergence probabilities conditional on assignment to Depression-baby, economics-trained, Republican, or home judges. The dashed horizontal lines show the average emergence probabilities for the full sample. All subplots show significant differences in emergence probabilities depending on the characteristics of the assigned judge. In particular, the emergence patterns on Depression-baby, and economics-trained, and Republican judges are consistent with my predictions. In addition, lower emergence likelihood under home judge suggests that they may be more liquidation-inclined.

[Figure 2.1 about here.]

Table 2.2 shows results from the linear probability model regressions in Equation (2.1), where the dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. The results from the multivariate regression analysis are consistent with the univariate comparison in Figure 2.1, showing that judge characteristics are associated with different probabilities of emergence from bankruptcy. Columns 1, 3, 5, and 7 do not include judge-level controls, whereas Columns 2, 4, 6, and 8 do. Columns 1 and 2 show that cases assigned to judges who grew up during the Great Depression have a 9- and 11-percentage- point higher probability of emergence, respectively.[21] The results in Columns 3–6, on the other hand, indicate that firms in cases assigned to economics-trained and Republican judges are 15 and 13 percentage points less likely to emerge from bankruptcy, respectively. In Columns 7 and 8, I add judge fixed effects to isolate within-judge variation, and I substitute court-by-year with court-by-decade fixed effects. I find that firms in cases assigned to home judges are 17 percentage points less likely to emerge from bankruptcy, supporting the hypothesis that home judges may be more pro-creditor. The effects of judge characteristics are economically large, considering that the unconditional likelihood of emergence in the sample is 60%. For instance, firms in cases assigned to Depression-baby judges are 18.3% ($= 0.11/0.60$) more likely to emerge. The estimates in Table 2.2 are consistent with the hypotheses that judge characteristics correlate with different liquidation tendencies across judges.

Regarding magnitude, my estimates may represent an upper bound on the effects of judge characteristics if judges' liquidation tendency and the ex-ante probability of emergence are negatively correlated. In

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[19]Dobbie and Song (2015) and Bernstein et al. (2019) find that judge leniency with respect to emergence are not time varying.

[20]An average judge is assigned to three cases in my sample.

[21]In Columns 1 and 2, since on-the-bench experience and the indicator for older cohorts are strongly positively correlated, I exclude $Log(Months\ as\ judge)$ to avoid multicollinearity with $Depression\ baby$.

contemporaneous work, Hüther and Kleiner (2022) suggest that liquidation-averse judges are more likely to be assigned to cases in which hedge funds are unsecured creditors. If such assignment patterns occur, the correlation between judge characteristics and emergence is a combination of two effects: (1) judges' liquidation tendency affecting the probability of emergence, and (2) firms with a high ex-ante probability of emergence, which hedge funds often target (Jiang et al. 2012), being more likely to be assigned to liquidation-averse judges. While the second effect only influences a subset of bankruptcy cases where hedge funds participate as unsecured creditors, my results complement Hüther and Kleiner (2022) in that different stakeholders in bankruptcy, such as hedge funds, may prefer or avoid judges with certain characteristics.

[Table 2.2 about here.]

Next, I examine whether these judge characteristics that are associated with different liquidation tendencies affect case duration. I use the same specification, Equation (2.1), where the dependent variable is the log number of months cases spend in bankruptcy. First, I test the relation using the whole sample. Appendix Table 2.A.5 shows that the effect of judge characteristics on duration is economically small and statistically indistinguishable from zero.[22] However, this specification may fail to capture the underlying relation between judge characteristics and case duration if the effects have opposing directions across case outcomes. For example, one might expect that judges with a strong preference toward liquidation may be quicker (slower) to decide when a potential outcome is consistent (inconsistent) with their preference.

To explore these opposing effects, I also begin with a descriptive analysis of how duration differs for cases assigned to judges with different characteristics when I consider case outcomes. Figure 2.2 plots the average months in bankruptcy by judge assignment. Each panel, from (a) to (d), compares the effect of case assignment to Depression-baby, economics-trained, Republican, or home judges, respectively. In each panel, I split the sample by outcome (liquidation versus emergence) and, in each subsample, measure the average duration by judge assignment. The dashed horizontal lines show the average months in bankruptcy from the whole sample.

Panels (a), (b), and (c) suggest that the average duration may vary among cases assigned to Depression-baby, economics-trained, and Republican judges when I consider outcomes. In particular, panel (b) shows that, among liquidation cases, those assigned to economics-trained judges spend about 5 fewer months in bankruptcy. Panel (c) shows that, among emerging cases, those assigned to Republican judges spend about 10 months more in bankruptcy compared to those assigned to Democratic judges. The results in panels (b) and (c) are consistent with the idea that judges may give less (more) scrutiny when a potential outcome is consistent (inconsistent) with their preference.

[Figure 2.2 about here.]

Table 2.3 investigates the opposing effects more formally by splitting the sample by whether the case ends in liquidation or emergence. The results show that some judge characteristics affect case duration when I consider case outcomes. While Columns 1 and 2 show that the effect of Depression-baby judges is statistically insignificant under both case outcomes, Columns 3 to 8 show that those of economics training and political ideology are economically and statistically significant. For example, Columns 3 and 5 show that, within cases ending in liquidation, those assigned to economics-trained and Republican judges spend 36% and 55% less time in bankruptcy, respectively. In economic terms, that is equivalent to a reduction of 7.5 and 11.6 months in bankruptcy compared to their sample means, respectively. Columns 4 and 6, on the other hand, report the opposite effects for cases ending in emergence. In other words, the results indicate that cases assigned to judges with liquidation-inclined characteristics spend less time in bankruptcy when

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[22]This lack of correlation between judge characteristics and unconditional case duration is consistent with Iverson et al. (2022), who show that military service status and political ideology do not affect unconditional duration (see their table A1). Similarly, Antill (2022) shows that judges' liquidation tendencies do not affect unconditional duration (see his table B.4).

they end in liquidation (and vice versa if they end in emergence). Moreover, Columns 7 and 8 support the results on Republican judges by showing that the effect among Democratic judges has opposite signs. The estimates in Table 2.3 suggest that judges may give less (more) scrutiny when the potential case outcome is consistent (inconsistent) with their preferences.

[Table 2.3 about here.]

Finally, I examine whether the judge characteristics are linked to better or worse post-emergence outcomes. The results so far show that specific judge characteristics affect the emergence probabilities and duration split by outcomes. Given the presence of these judge effects, it is natural to ask if they are a form of judicial *bias* by evaluating bankrupt firms' performance. For example, if judges with liquidation-averse characteristics allow clearly unviable firms to emerge, I expect that overall post-emergence outcomes of firms assigned to such judges should be worse.[23]

Table 2.4 shows that the judge characteristics do not have significant effects on post-emergence outcomes. The dependent variables are an indicator equal to one if a firm refiles within 3 years of emergence (*Refile*) and return on assets after emergence (*ROA (post)*). In most columns, the estimated coefficients are statistically indistinguishable from zero.[24] The lack of correlation is consistent with Antill (2022), who argues that judges' liquidation tendency is more likely to affect the outcomes of cases where multiple forms of bankruptcy exits can be legally justified. Overall, the evidence in Table 2.4 suggests that the effect of judge characteristics may be concentrated in marginal cases, where the economic benefits of liquidation versus emergence are not significantly different.

[Table 2.4 about here.]

To illustrate the results in Table 2.4 that do not support the bias hypothesis, Figure 2.3 plots average post-emergence outcomes by judge assignment. Panel (a) shows that the average refiling rate does not differ significantly by judge assignment. Panel (b) shows that the average return on assets after emergence is also similar irrespective of judge assignment, with the exception of assignment to economics-trained judges, though the difference is statistically insignificant in the multivariate analysis.

[Figure 2.3 about here.]

2.5 Heterogeneity in case characteristics

In Section 2.4.2, I find evidence that judge characteristics affect the probabilities of emergence for bankrupt firms. Next, I test whether the effects of judge characteristics on the probabilities of emergence are more pronounced in some cases than in others. I perform cross-sectional tests to investigate whether judge preferences correlate with firm characteristics in determining case outcomes, in line with my earlier findings.

2.5.1 Labor channel

To better understand the channel driving the results, I analyze whether cross-sectional variation in the number of employees affects the probabilities of emergence of cases assigned to Depression-baby and economics-trained judges. Particularly, the predictions on these two characteristics are based on judges' attitude toward preserving employment in bankrupt firms. If the labor-based channel is driving the liquidation

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[23]The ideal test on the post-bankruptcy outcomes should also explore asset utilization of liquidating firms. However, due to data limitations, I observe outcomes of emerging firms only.

[24]Exceptions are Columns 3 and 8, where the estimates are statistically significant. However, I take caution in interpreting them. In Column 3, the sign of the coefficient contradicts the bias hypothesis. In Column 8, although the effect of home judge is positive on *ROA (post)*, its effect on *Refile* in Column 7 is economically and statistically insignificant. This inconsistency does not support the bias hypothesis.

tendency of Depression-baby and economics-trained judges, I expect to see stronger effects of the two judge characteristics in firms with a large number of employees. Specifically, for Depression-baby judges, the perceived costs of liquidating firms with a large number of employees will be higher than for other firms, whereas for economics-trained judges, the perceived benefits will be higher for the same type of firms. To test this labor-based channel, I split the sample into two groups at the median number of employees and re-estimate the emergence regression from Table 2.2 separately for each subsample.

In line with the labor-based channel, Table 2.5 shows that the effects of Depression-baby and economics-trained judges are concentrated among firms with a large number of employees. For each characteristic, columns Low and High denote the sample with the below- and above-median number of employees, respectively. Columns 2 and 4 show that the effects of cases being assigned to Depression-baby and economics-trained judges are observed only when bankrupt firms have an above-median number of employees. Moreover, the heterogeneous effect of the number of employees is absent among cases assigned to Republican or home judges, for which my predictions on emergence versus liquidation are not related to workers. Overall, these cross-sectional findings support the labor-based hypotheses on the source of liquidation tendency among economics-trained judges and those who grew up during the Great Depression.

[Table 2.5 about here.]

2.5.2 Survival likelihood

Next, I examine whether judges' liquidation tendency is sensitive to the survival likelihood of the bankrupt firms. If judges have an outcome preference, I expect that the effect of judges on emergence would be stronger when the firm's survival likelihood is contrary to their preferred outcome. For the measure of survival likelihood, I use the return on assets at filing (ROA). Higher ROA suggests that the bankrupt firm is experiencing financial distress, rather than economic distress. Consistent with this expectation, Tables 2.2 and 2.4 show that bankrupt firms with higher ROA are significantly more likely to emerge and less likely to refile for bankruptcy within 3 years (though the latter result is less statistically robust). To test the sensitivity of judges' liquidation tendency to the survival likelihood, I split the sample into two groups at the median ROA and re-estimate the emergence regression from Table 2.2 separately for each subsample.

Table 2.6 presents the results. For each characteristic, columns Low and High denote the sample with the below- and above-median ROA, respectively. Columns 1 and 2 show that the positive effect of cases assigned to Depression-baby judges on emergence is concentrated among less profitable firms. On the other hand, across Columns 3 to 8, the negative effects of cases assigned to economics-trained, Republican, and home judges on emergence are mostly concentrated among more profitable firms. These findings suggest that judges with liquidation-inclined characteristics are more stringent in regard to the profitability of the firms they allow to emerge from bankruptcy. Overall, the evidence in Table 2.6 implies that the effect of judge characteristics is more pronounced in cases where judges expect to see an outcome that contrasts to their liquidation tendency.

[Table 2.6 about here.]

2.5.3 Creditor channel

If Republican judges are pro-creditor, and thus more liquidation-inclined, then I expect their liquidation tendency to be amplified when creditors have higher stakes, that is, when the bankrupt firm is highly levered. In particular, as secured creditors tend to prefer liquidation over emergence (Baird and Rasmussen 2003; Jiang et al. 2012), a higher level of secured debt might further amplify Republican judges' pro-creditor tendency.

To test this conjecture, I modify the baseline specification into the following form:

$$Emerge_{ircjt} = \alpha_r + \alpha_{ct} + \beta_1 Republican_j + \beta_2 Debt\ ratio_i$$
$$+ \beta_3 Republican_j \times Debt\ ratio_i + \gamma X'_{ijt} + \varepsilon_{ircjt}, \qquad (2.3)$$

where *Debt ratio* is either *Leverage ratio*, the ratio of total liabilities to total assets, or *Secured debt ratio*, the ratio of secured debt to total assets. To estimate the heterogeneous effect of Republican judges with precision, I use an interaction term because the number of cases with non-missing political ideology of judges is relatively small.

Table 2.7 shows the results. Column 1 examines whether a greater creditor stake amplifies Republican judges' liquidation tendency. The negative coefficient on the interaction term between the indicator for Republican judges and leverage ratio indicates that higher leverage indeed strengthens Republican judges' liquidation tendency. Next, Column 2 tests whether Republican judges' liquidation tendency becomes stronger when secured creditors, who tend to prefer liquidation, have a greater stake. The negative coefficient on the interaction term between the indicator for Republican judges and the secured debt ratio shows similar results. Lastly, Column 3 includes both interaction terms to separate the different effects across types of debt. The coefficient estimate is positive (negative) on the interaction term between the indicator for Republican judges and the leverage (secured debt) ratio. These results indicate that the stronger liquidation tendency of Republican judges comes more from the amount of secured debt than from the amount of total debt. The dominant effect of secured debt is consistent with secured creditors' liquidation bias.

[Table 2.7 about here.]

2.5.4   Why do home judges rule differently?

While the baseline results in Table 2.2 suggest that home judges may be more pro-creditor, the economic channel through which this occurs remains unclear. Moreover, one might still suspect that judges care about home state employees, yet this concern is dominated by concerns for home state creditors. In this section, I study whether judges care more about creditors (or employees) in their home state by exploiting variation in the intensity of local stakeholders' interests.

Based on the motivating hypotheses, I expect that the pro-creditor tendency of home judges will be stronger, and thus more liquidation-inclined, when local creditors have a high stake in the firm. Conversely, I expect that they will be strongly liquidation-averse when the firm has a large number of local employees. The ideal measure for this cross-sectional analysis would be the intensity of home state stakeholders' interests, such as the amount of claims held by home state creditors or the number of home state–based employees. Unfortunately, that level of data is not available. To approximate this ideal measure, I define the degree of geographic concentration of the firm around its headquarters state, or "localness," and interact it with the level of stakeholders' interests.

To test how home judges' liquidation tendency varies with local stakeholders' interests, I estimate the following model:

$$Emerge_{ircjt} = \alpha_r + \alpha_{ct'} + \alpha_j + \beta_1 HomeJudge_{ij} + \beta_2 Localness_i + \beta_3 StakeholderInterest_i$$
$$+ \beta_4 HomeJudge_{ij} \times Localness_i + \beta_5 HomeJudge_{ij} \times StakeholderInterest_i$$
$$+ \beta_6 Localness_i \times StakeholderInterest_i$$
$$+ \beta_7 HomeJudge_{ij} \times Localness_i \times StakeholderInterest_i + \gamma X'_{ijt} + \varepsilon_{ircjt}, \qquad (2.4)$$

where *Localness$_i$* is the number of times the headquarters state is mentioned in the annual report (form 10-K) of firm i divided by the total number of mentions of all U.S. states in the same report. The data on

Local ness$_i$ are calculated from García and Norli (2012).[25] Following García and Norli (2012), I interpret this measure as the degree of geographic concentration of the firm's business within the headquarters state.[26] Another interacted variable, *StakeholderInterest$_i$*, captures the the level of two major stakeholders' interests in firm i: creditors' and workers'. To measure creditors' interests, I use *Leverage ratio$_i$* as in Table 2.7. To measure workers' interests, I use *Log(No. of employees)$_i$* as in Table 2.5. Because state names are often mentioned in 10-K reports when describing the location of stores or manufacturing facilities (García and Norli 2012) and because banks that rely heavily on local financing are more likely to lend to geographically proximate businesses (Petersen and Rajan 2002; Becker 2007), these triple interaction terms approximate the ideal measure of local stakeholders' interests.

Table 2.8 shows the results. To save space, I report only the coefficients of interest, that is, the estimates that include *HomeJudge* or *Local ness*. The full table is reported in Appendix Table 2.A.4. Among cases assigned to home judges, I find negative and positive effects of local creditors and of local labor, respectively, on the probabilities of emergence. In Column 1, I start the analysis by looking simply at variation in localness. I confirm that localness alone does not have a significant effect on home judges' liquidation tendency. This insignificance may stem from conflating the opposite effects of local creditors and local labor. Therefore, in the remaining columns, I separate these effects. In Columns 2 and 3, I examine the interaction between home judges and leverage. In Column 2, the positive coefficient on *Home judge × Leverage ratio* implies that a higher leverage ratio—independent of its localness—does not make home judges more liquidation-inclined. Hence, in Column 3, I isolate local creditors' interests from those of general creditors. The negative coefficient on the triple interaction term (*Home judge × Localness × Leverage ratio*) suggests that home judges' liquidation inclination becomes stronger for highly levered local firms.

In Columns 4 and 5, I turn to examine the interaction between home judges and labor. In Column 4, the positive coefficient on *Home judge × Log(No. of employees)* shows preliminary evidence that home judges care about workers in home state firms, as seen in the higher likelihood of emergence. However, this estimate does not discriminate between different degrees of firm localness. In Column 5, I address this issue by estimating a triple interaction term, *Home judge × Localness × Log(No. of employees)*. The coefficient on this triple interaction term is positive and statistically significant, while that on *Home judge × Log(No. of employees)* becomes statistically insignificant. This result provides strong evidence that the positive effect of a large number of workers is present only in highly local firms, rather than in firms with simply have a large number of workers.

Finally, in Column 6, I simultaneously control for the intensity of local workers' and creditors' interests. This joint specification examines which local stakeholders' interests home judges care more about. The estimates on the two triple interaction terms suggest that the pro-creditor tendency of home judges dominates their pro-labor tendency, while the economic magnitude of the latter remains similar to that in Column 5. Overall, the results confirm the cross-sectional predictions: while home judges may worry about the loss of local employment from liquidation, the concern is outweighed by the concern for local creditors' interests.

[Table 2.8 about here.]

Taken together, the cross-sectional analyses in this section bolster the earlier results that judge characteristics affect the probability that firms will emerge from bankruptcy. The analyses also mitigate the concern that the effects of judge characteristics are simply a combination of other characteristics. For example, an alternative explanation should be able to jointly explain why cases assigned to judges who experienced the

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[25]I thank Diego Garcia for sharing the data on the state counts in 10-K filings.

[26]To extend coverage of *Local ness*, which is only available between 1994 and 2008 in García and Norli (2012), I match a value that is measured up to 10 years before the bankruptcy filing year. This limits the sample to bankruptcies filed after 1994. Relatedly, Parsons et al. (2020) report that state counts from García and Norli (2012) are very stable over time. However, I do not match values after bankruptcy since the degree of geographic concentration may change significantly through the major restructuring of assets.

Great Depression are more likely to result in emergence from bankruptcy when the firms have a large number of employees or low-profitability.

2.6 Placebo tests and robustness checks

In this section, I perform additional tests to assess the robustness of the baseline evidence.

2.6.1 Placebo tests

I conduct a placebo test using prepackaged and prenegotiated cases, where a judge's liquidation tendency is unlikely to influence outcomes. Prior studies, such as Chang and Schoar (2013) and Antill (2022), report that outcomes of such cases are largely predetermined prior to filing. I re-run the analysis in Tables 2.2 and 2.3 using the sample that includes only prepackaged and prenegotiated cases.

Table 2.9 shows that in the placebo sample, most judge characteristics have insignificant effects on the probabilities of emergence, both in economic and statistical terms. While the effect of economics-trained judges is statistically significant, its economic magnitude is too small (1 to 3 percentage points increase), relative to the base rate of emergence of 60%, to argue that economics-trained judges play a large role even in prepackaged cases.

I perform a similar placebo test on case duration. Appendix Table 2.A.6 reports the results. Columns 1, 3, 5, and 7 first test whether a judge characteristic correlates with duration, irrespective of case outcomes. The coefficient estimates generally show that there is no significant relation between judge characteristics and duration in the placebo sample. Columns 2, 4, 6, and 8 further test heterogeneous effects of case outcomes on judges' liquidation tendency, in the spirit of Table 2.3.[27] The results again show that no judge characteristics, except economics training, affect duration in prepackaged cases. The positive effect of economics-trained judges on duration is concentrated in emerging cases (Column 4), which is still consistent with the baseline results; this result implies that economics-trained judges may be strongly liquidation-inclined such that they exercise extra scrutiny even for prepackaged cases that are highly likely to emerge on average. Overall, the results of tests using the placebo sample show that among cases where the influence of judges on outcomes is limited, the effect of judge characteristics is mostly absent.

[Table 2.9 about here.]

2.6.2 Alternative specifications

This subsection addresses two potential concerns with the main specification in Equations 2.1 and 2.2. First, Appendix Table 2.A.7 shows that the baseline results on emergence in Table 2.2 are robust to using a logit model rather than a linear probability model.[28] Second, one might worry that court-by-time fixed effects are too restrictive, despite having advantages, such as addressing concerns about forum shopping. In particular, including court-by-time fixed effects absorbs variation in outcomes in smaller courts that have only one or two cases during the sample period. I address this concern by replacing court-by-time fixed effects with separate court and time fixed effects, which allows me to exploit more variation in outcomes in smaller courts.

Appendix Table 2.A.8 reports the results. This table is identical to Table 2.2, except that I use alternative specifications. In Columns 1–6 (7–8), I substitute court-by-year (court-by-decade) fixed effects with separate court and year (decade) fixed effects. The estimates show that the effects of judge characteristics are still

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[27]Since there are few liquidations among prepackaged cases, I use an interaction term between a judge characteristic and the indicator for emergence to estimate the heterogeneous effects with precision.

[28]In an unreported regression, I find that the results on refiling are robust to using a logit model. The sample size is substantially smaller since the logit model excludes observations that are perfectly predicted.

economically significant, but the statistical relations become less robust. While these results help shed light on the robustness of the relation between judge characteristics and emergence, my main specification gives more economically meaningful results. This difference occurs because, while court-by-time fixed effects absorb variation in smaller courts, I am interested in studying the relation in larger courts that attract far more filings.

I repeat similar exercises on duration and post-emergence performance in Appendix Tables 2.A.9 and 2.A.10, respectively. They show largely similar results to previous corresponding Tables 2.3 and 2.4.

2.7 Conclusion

In this study, I examine the effects of judge characteristics on bankruptcy outcomes, exploiting the random assignment of judges to corporate bankruptcy cases. First, I find that cases assigned to judges who grew up during the Great Depression are more likely to emerge from bankruptcy, whereas those assigned to judges with economics training and conservative political ideology are more likely to result in liquidation. Second, I show that case duration is shorter (longer) when the potential case outcome is consistent (inconsistent) with judges' preferences. Third, I find that cases are less likely to result in emergence when the firm is headquartered in the judge's' home state. This effect is stronger when local creditors' interests are high, suggesting that judges may care about creditors from the same home state. Finally, I find that judge characteristics do not correlate with post-emergence outcomes. This lack of correlation implies that the effect of the judge characteristics may be concentrated in marginal cases, where the economic benefits of liquidation versus emergence are not significantly different.

The findings highlight the importance of judge random assignment in designing a bankruptcy system. An alternative system without random assignment would generate problems such as judge shopping, whereby bankrupt firms pick individual judges to hear their cases. Random assignment mitigates the friction from judicial bias in the bankruptcy system.

Table 2.1: Summary statistics

This table presents the number of observations, mean, standard deviation, 25% percentile, median, and 75% percentile values of the main variables for the sample of 771 large U.S. Chapter 11 cases from 1980 to 2020. The sample excludes prepackaged or prenegotiated cases. Detailed variable definitions are in Appendix Table 2.A.1.

Variable	Obs	Mean	SD	P25	Median	P75
Judge characteristics at filing						
Depression baby	759	0.27	0.45	0.00	0.00	1.00
Economics-trained	769	0.05	0.21	0.00	0.00	0.00
Republican	439	0.12	0.32	0.00	0.00	0.00
Democrat	439	0.78	0.41	1.00	1.00	1.00
Home judge	617	0.24	0.43	0.00	0.00	0.00
Male	771	0.76	0.43	1.00	1.00	1.00
Military	771	0.19	0.39	0.00	0.00	0.00
Months as judge	771	108.6	84.5	41.1	95.1	162.9
Firm characteristics at filing						
Total assets (in $ millions)	771	5,196	35,085	525	933	2,646
Leverage ratio	759	0.97	0.45	0.76	0.90	1.06
Return on assets	755	-0.05	0.23	-0.07	-0.01	0.04
No. of filings	771	9.08	20.30	1.00	3.00	9.00
No. of employee	769	8,275	18,653	957	2,956	8,000
Secured debt ratio	527	0.30	0.30	0.05	0.24	0.47
Bank debt ratio	364	0.27	0.26	0.08	0.22	0.36
Localness	386	0.33	0.24	0.13	0.29	0.50
Bankruptcy outcomes						
Emerge	771	0.60	0.49	0.00	1.00	1.00
Months in Ch.11	768	21.11	17.24	10.45	16.42	25.61
Refile	442	0.09	0.28	0.00	0.00	0.00
Return on assets (post)	216	0.02	0.37	-0.11	0.00	0.13

Table 2.2: Judge characteristics and emergence from bankruptcy

This table presents the effect of judge characteristics on the probability that a firm will emerge from bankruptcy. The estimates come from linear probability models in specifications that are provided below. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. Across columns, I use indicator variables for four different judge characteristics. In Columns 1 and 2, the main explanatory variable is *Depression baby*, defined as judges born between 1920 and 1939. In Columns 3 and 4, it is *Economics-trained*, defined as judges who either have attended the Manne program or graduated from law schools with a strong law and economics culture. In Columns 5 and 6, it is *Republican*, defined as judges who have consistently voted for or donated to the Republican Party. In Columns 7 and 8, it is *Home judge*, defined as whether the firm is headquartered in the judge's home state. All columns include firm-level controls and industry (Fama-French 12) fixed effects. In addition, Columns 1–6 include court-by-year fixed effects, and Columns 7–8 include court-by-decade and judge fixed effects. Standard errors are clustered at the filing court level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

$$\text{Columns 1–6: } Emerge_{ircjt} = \alpha_r + \alpha_{ct} + \beta\,JudgeChar_j + \gamma\,X'_{ijt} + \varepsilon_{ircjt}$$

$$\text{Columns 7–8: } Emerge_{ircjt} = \alpha_r + \alpha_{ct'} + \alpha_j + \beta\,HomeJudge_{ij} + \gamma\,X'_{ijt} + \varepsilon_{ircjt}$$

	Depression baby		Economics-trained		Republican		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judge characteristic	0.09	0.11**	-0.14**	-0.15***	-0.13***	-0.13***	-0.17*	-0.17*
	(1.17)	(2.23)	(-2.15)	(-3.36)	(-5.77)	(-4.62)	(-2.00)	(-1.88)
Male		-0.05		0.01		-0.03		
		(-0.62)		(0.13)		(-0.32)		
Military		0.03		0.02		-0.04		
		(0.33)		(0.31)		(-0.61)		
Log(Months as judge)				0.07***		0.06***		0.02
				(4.18)		(10.85)		(0.61)
Log(Total assets)	0.04**	0.04**	0.05**	0.04*	0.03***	0.03**	0.06**	0.06**
	(2.07)	(2.05)	(2.22)	(2.00)	(4.23)	(2.25)	(2.65)	(2.39)
Leverage ratio	0.17***	0.17***	0.17***	0.18***	0.13***	0.13***	0.20***	0.20***
	(3.25)	(3.35)	(3.58)	(4.05)	(4.46)	(4.63)	(3.45)	(3.25)
Return on assets	0.24***	0.24***	0.22**	0.23**	0.24***	0.25**	0.16	0.15
	(2.88)	(2.77)	(2.36)	(2.28)	(2.82)	(2.57)	(1.37)	(1.36)
Log(No. of filings)	-0.02*	-0.02*	-0.02*	-0.02	-0.04***	-0.04***	-0.01	-0.01
	(-1.79)	(-1.94)	(-1.84)	(-1.49)	(-6.02)	(-5.61)	(-0.49)	(-0.47)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	737	737	746	746	431	431	602	602
R^2	0.562	0.562	0.559	0.571	0.606	0.615	0.503	0.503

Table 2.3: Judge characteristics and duration in bankruptcy: split by case outcome

This table presents the effect of judge characteristics on case duration, splitting the sample by whether the case ends in liquidation or emergence. The estimates are from OLS regressions in the specification provided below. The dependent variable is the log number of months a case spends in bankruptcy. Across columns, I use indicator variables for four different judge characteristics. In Columns 1 and 2, the main explanatory variable is *Depression baby*, defined as judges born between 1920 and 1939. In Columns 3 and 4, it is *Economics-trained*, defined as judges who either attended the Manne program or graduated from a law school with a strong law and economics culture. In Columns 5 and 6 (7 and 8), it is *Republican* (*Democrat*), defined as judges who have consistently voted for or donated to the Republican (Democratic) Party. All columns include firm-level controls and industry (Fama-French 12) and court-by-year fixed effects. Standard errors are clustered at the filing court level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

$$Log(Months\ in\ Ch.11)_{ircjt} = \alpha_r + \alpha_{ct} + \beta\,JudgeChar_j + \gamma X'_{ijt} + \varepsilon_{ircjt}$$

	Depression baby		Economics-trained		Republican		Democrat	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Case outcome	Liquidate	Emerge	Liquidate	Emerge	Liquidate	Emerge	Liquidate	Emerge
Judge characteristic	-0.19	0.03	-0.36**	0.29**	-0.55**	0.25***	0.43**	-0.11
	(-1.54)	(0.43)	(-2.52)	(2.29)	(-2.15)	(3.00)	(2.47)	(-0.73)
Male	0.01	-0.27***	-0.02	-0.25***	-0.05	0.01	-0.19	0.02
	(0.08)	(-3.50)	(-0.13)	(-6.32)	(-0.29)	(0.13)	(-1.00)	(0.14)
Military	0.20	0.10	0.14	0.10	-0.05	-0.03	-0.06	-0.04
	(0.75)	(0.75)	(0.66)	(0.85)	(-0.30)	(-0.63)	(-0.24)	(-0.84)
Log(Months as judge)			-0.16	0.00	-0.27**	0.01	-0.29***	0.02
			(-1.64)	(0.05)	(-2.29)	(0.25)	(-3.11)	(0.31)
Log(Total assets)	0.09	0.23***	0.09	0.24***	0.02	0.17***	0.02	0.17***
	(1.21)	(3.28)	(0.98)	(3.02)	(0.20)	(3.86)	(0.19)	(3.95)
Leverage ratio	0.29**	0.19**	-0.04	0.20**	-0.06	0.16	-0.08	0.17*
	(2.30)	(2.26)	(-0.41)	(2.07)	(-0.26)	(1.59)	(-0.28)	(1.76)
Return on assets	0.23	-0.01	0.04	-0.01	0.21*	0.03	0.25***	0.06
	(0.75)	(-0.08)	(0.11)	(-0.09)	(1.74)	(0.20)	(2.97)	(0.38)
Log(No. of filings)	-0.06	-0.01	-0.07	0.00	-0.01	0.03	0.01	0.02
	(-0.72)	(-0.42)	(-1.00)	(0.12)	(-0.18)	(1.34)	(0.18)	(1.21)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	296	438	299	444	180	249	180	249
R^2	0.644	0.711	0.660	0.710	0.642	0.718	0.645	0.715

Table 2.4: Judge characteristics and post-emergence outcomes

This table presents the effect of judge characteristics on post-emergence outcomes. Columns 1–6 and 7–8 report estimates from linear probability models in specifications that are provided below. The dependent variable is either an indicator equal to one if a firm refiles within 3 years of emergence (*Refile*) or return on assets after emergence (*ROA (post)*). Across columns, I use indicator variables for four different judge characteristics. In Columns 1 and 2, the main explanatory variable is *Depression baby*, defined as judges born between 1920 and 1939. In Columns 3 and 4, it is *Economics-trained*, defined as judges who either attended the Manne program or graduated from law schools with a strong law and economics culture. In Columns 5 and 6, it is *Republican*, defined as judges who have consistently voted for or donated to the Republican Party. In Columns 7 and 8, it is *Home judge*, defined as whether the firm is headquartered in the judge's home state. All columns include firm-level controls and industry (Fama-French 12) fixed effects. In addition, Columns 1–6 include court-by-year fixed effects and Columns 7–8 include court-by-decade and judge fixed effects. Standard errors are clustered at the filing court level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

Columns 1–6: $PostOutcomes_{ircjt} = \alpha_r + \alpha_{ct} + \beta\, JudgeChar_j + \gamma X'_{ijt} + \varepsilon_{ircjt}$

Columns 7–8: $PostOutcomes_{ircjt} = \alpha_r + \alpha_{ct'} + \alpha_j + \beta\, HomeJudge_{ij} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

	Depression baby		Economics-trained		Republican		Home judge	
	(1) Refile	(2) ROA (post)	(3) Refile	(4) ROA (post)	(5) Refile	(6) ROA (post)	(7) Refile	(8) ROA (post)
Judge characteristic	-0.01	0.10	0.09**	0.31	0.05	0.08	0.01	0.16*
	(-1.04)	(0.46)	(2.51)	(0.70)	(0.28)	(0.68)	(0.26)	(1.79)
Male	-0.01	-0.10	-0.01	-0.10	0.08	-0.07***		
	(-0.37)	(-0.85)	(-0.33)	(-1.36)	(0.86)	(-3.40)		
Military	0.07	0.19*	0.07	0.20*	-0.01	0.26***		
	(0.49)	(2.01)	(0.48)	(1.87)	(-0.87)	(6.00)		
Log(Months as judge)			-0.01	0.01	-0.03	0.07**	-0.04*	0.07***
			(-0.20)	(0.09)	(-0.76)	(2.83)	(-1.71)	(2.84)
Log(Total assets)	0.01	0.09***	0.01	0.08***	0.00	0.08**	0.01	0.04
	(0.22)	(3.96)	(0.25)	(3.88)	(0.20)	(2.13)	(0.49)	(0.75)
Leverage ratio	0.00	0.21	0.00	0.26	0.05***	0.12**	0.01	0.05
	(0.30)	(1.30)	(0.52)	(0.97)	(4.34)	(2.17)	(0.38)	(0.37)
Return on assets	-0.12	-0.02	-0.13	0.02	-0.12	-0.08	-0.40**	0.07
	(-0.79)	(-0.65)	(-0.75)	(0.21)	(-0.56)	(-0.46)	(-2.03)	(0.30)
Log(No. of filings)	0.00	-0.05**	-0.00	-0.04	0.01	-0.07**	0.02	-0.01
	(0.09)	(-2.05)	(-0.03)	(-1.51)	(0.47)	(-2.15)	(1.23)	(-0.31)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	416	203	422	208	236	117	351	178
R^2	0.682	0.757	0.662	0.729	0.636	0.756	0.551	0.727

Table 2.5: Cross-sectional splits by the number of employees

This table presents results on the effect of judge characteristics on the probabilities of emergence when I split the sample at the median number of employees. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. In each split sample, the specification is identical to those in Table 2.2, which may be consulted for details. Standard errors are clustered at the filing court level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

	Depression baby		Economics-trained		Republican		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Split by median employee	Low	High	Low	High	Low	High	Low	High
Judge characteristic	0.07	0.14***	-0.05	-0.20***	-0.19	-0.07	-0.09	-0.14
	(0.72)	(3.60)	(-1.15)	(-3.32)	(-0.65)	(-0.47)	(-0.77)	(-0.58)
Male	-0.01	-0.17	0.03	-0.09	-0.16*	-0.06		
	(-0.13)	(-0.94)	(0.26)	(-0.61)	(-1.91)	(-0.56)		
Military	0.17	-0.01	0.15	0.01	0.22	-0.09**		
	(0.69)	(-0.09)	(0.65)	(0.05)	(1.27)	(-2.16)		
Log(Months as judge)			0.05	0.08	0.03	0.07***	0.07	0.06
			(1.43)	(1.67)	(1.14)	(3.10)	(0.66)	(1.35)
Log(Total assets)	0.03	0.03**	0.04	0.02**	0.04	0.02	0.03	0.03
	(0.39)	(2.24)	(0.46)	(2.32)	(0.66)	(0.21)	(0.47)	(1.52)
Leverage ratio	0.23*	0.14***	0.24*	0.18***	0.12	0.16*	0.32***	0.14
	(1.74)	(4.45)	(1.79)	(6.51)	(1.03)	(1.88)	(2.97)	(1.39)
Return on assets	0.21	1.03**	0.22	0.92**	0.21	0.92	0.24***	0.16
	(1.20)	(2.66)	(1.14)	(2.42)	(1.41)	(1.27)	(2.78)	(0.64)
Log(No. of filings)	0.01	-0.07***	0.01	-0.06**	-0.02	-0.07***	0.07*	-0.05
	(0.73)	(-2.82)	(0.92)	(-2.62)	(-0.81)	(-5.55)	(1.73)	(-1.43)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	371	365	374	371	210	220	301	300
R^2	0.660	0.661	0.661	0.673	0.737	0.722	0.658	0.582

Table 2.6: Cross-sectional splits by profitability

This table presents results on the effect of judge characteristics on the probabilities of emergence when I split the sample at the median return on assets (ROA). The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. In each split sample, the specification is identical to those in Table 2.2, which may be consulted for details. Standard errors are clustered at the filing court level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

	Depression baby		Economics-trained		Republican		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Split by median ROA	Low	High	Low	High	Low	High	Low	High
Judge characteristic	0.21*	0.06	-0.07	-0.18***	-0.13	-0.14***	-0.03	-0.22**
	(1.72)	(0.45)	(-1.42)	(-7.42)	(-0.94)	(-5.77)	(-0.12)	(-2.11)
Male	-0.07	-0.07	0.03	-0.03	0.14	-0.03		
	(-0.55)	(-0.87)	(0.15)	(-0.37)	(0.64)	(-1.36)		
Military	0.16	-0.07	0.13	-0.07	0.19**	-0.12		
	(1.09)	(-0.35)	(0.87)	(-0.32)	(2.73)	(-0.88)		
Log(Months as judge)			0.09**	0.03	0.08***	0.04**	0.08**	-0.03
			(2.49)	(1.38)	(13.57)	(2.10)	(2.21)	(-0.33)
Log(Total assets)	0.08	0.03**	0.07	0.04**	0.02	0.06	0.10**	0.04
	(1.18)	(2.05)	(0.92)	(2.24)	(0.27)	(0.80)	(2.61)	(0.67)
Leverage ratio	0.13***	0.23***	0.16***	0.25***	0.13***	0.19*	0.18***	0.24***
	(6.14)	(5.11)	(5.43)	(5.42)	(4.29)	(1.94)	(3.04)	(4.32)
Return on assets	0.21**	-0.82	0.25**	-0.89	0.20	-0.11	0.17	-1.05
	(2.50)	(-1.06)	(2.40)	(-1.14)	(1.48)	(-0.11)	(0.79)	(-1.03)
Log(No. of filings)	-0.00	-0.08***	-0.00	-0.07***	-0.02**	-0.12**	0.01	-0.05
	(-0.00)	(-4.14)	(-0.20)	(-3.29)	(-2.12)	(-2.11)	(0.22)	(-1.61)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	374	363	376	370	220	211	305	297
R^2	0.664	0.723	0.666	0.725	0.710	0.790	0.548	0.612

Table 2.7: Heterogeneous effects of Republican judges: Debt ratios

This table explores the heterogeneous effects of Republican judges on the probability of emergence based on different levels of debt ratios. The table reports estimates from linear probability models in the specification provided below. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. For firm i, *Debt ratio$_i$* is either *Leverage ratio$_i$*, the ratio of total liabilities to total assets, or *Securd debt ratio$_i$*, the ratio of secured debt to total assets. Standard errors are clustered at the filing court level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

$$Emerge_{ircjt} = \alpha_r + \alpha_{ct} + \beta_1 Republican_j + \beta_2 Debt\ ratio_i$$
$$+ \beta_3 Republican_j \times Debt\ ratio_i + \gamma X'_{ijt} + \varepsilon_{ircjt}$$

	(1)	(2)	(3)
Republican	-0.06	-0.13***	-0.27***
	(-1.17)	(-4.13)	(-6.34)
Leverage ratio	0.14***	0.11*	0.10
	(4.46)	(1.99)	(1.66)
Secured debt ratio		0.07	0.07
		(1.31)	(1.34)
Republican × Leverage ratio	-0.08**		0.22***
	(-2.07)		(4.73)
Republican × Secured debt ratio		-0.17*	-0.40***
		(-1.80)	(-5.74)
Male	-0.03	0.06	0.05
	(-0.30)	(0.48)	(0.39)
Military	-0.04	0.04	0.05
	(-0.69)	(1.06)	(1.02)
Log(Months as judge)	0.06***	0.09***	0.09***
	(9.35)	(5.08)	(5.12)
Log(Total assets)	0.03**	0.02***	0.02***
	(2.09)	(3.83)	(4.24)
Return on assets	0.25**	0.22**	0.22**
	(2.61)	(2.40)	(2.31)
Log(No. of filings)	-0.04***	-0.06**	-0.05**
	(-5.67)	(-2.62)	(-2.72)
Industry fixed effects	Yes	Yes	Yes
Court-year fixed effects	Yes	Yes	Yes
Observations	431	310	310
R^2	0.615	0.630	0.631

Table 2.8: Heterogeneous effects of home judges: Stakeholders' "localness"

This table explores the heterogeneous effects of home judges on the probabilities of emergence based on different levels of local stakeholders' interests. The table reports estimates from linear probability models in specification 2.4. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. *Local ness* denotes the headquarters state concentration ratio, constructed from the dataset on geographic dispersion of business operations from García and Norli (2012). *Leverage ratio* is the ratio of total liabilities to total assets. To save space, only the coefficients of interest are reported. In all specifications, other non-interacted variables and two-way interacted terms are included but not reported; the full table is reported in Appendix Table 2.A.4. *Judge controls* include the judge's on-the-bench experience. *Firm controls* include the log of total assets, leverage ratio, return on assets, and the log of the number of filings. Standard errors are clustered at the filing court level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

	(1)	(2)	(3)	(4)	(5)	(6)
Home judge	-0.38*	-0.36***	-1.04**	-0.85**	0.06	-0.65
	(-1.83)	(-2.82)	(-2.20)	(-2.13)	(0.07)	(-0.66)
Localness	-0.18		-0.29		0.28	-0.31
	(-1.07)		(-1.20)		(1.36)	(-1.56)
Home judge × Localness	0.59		4.36**		-2.54	1.58
	(1.07)		(2.16)		(-1.40)	(0.87)
Home judge × Leverage ratio		0.20*	0.60**			0.54**
		(1.91)	(2.40)			(2.11)
Localness × Leverage ratio			0.11			0.54**
			(0.60)			(2.41)
Home judge × Localness × Leverage ratio			-4.02*			-4.79**
			(-1.99)			(-2.18)
Home judge × Log(No. of employee)				0.09*	-0.05	-0.03
				(1.72)	(-0.48)	(-0.24)
Localness × Log(No. of employee)					-0.06**	-0.05
					(-2.27)	(-1.47)
Home judge × Localness × Log(No. of employee)					0.39*	0.41
					(1.74)	(1.37)
Judge controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Court-decade fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	307	602	307	601	306	306
R^2	0.603	0.507	0.613	0.513	0.628	0.644

Table 2.9: Placebo test: Judge characteristics and emergence from bankruptcy

This table presents the results of a placebo test. I repeat the analysis in Table 2.2 using a placebo sample that includes only prepackaged and prenegotiated cases. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. Standard errors are clustered at the filing court level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

	Depression baby		Economics-trained		Republican		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judge characteristic	-0.03	-0.02	0.01**	0.03**	0.01	0.03	0.02	0.02
	(-0.56)	(-0.31)	(2.14)	(2.50)	(0.06)	(0.27)	(0.16)	(0.16)
Male		-0.02		-0.04		-0.12***		
		(-0.80)		(-1.66)		(-3.23)		
Military		-0.19		-0.21		-0.13*		
		(-1.53)		(-1.56)		(-1.76)		
Log(Months as judge)				0.03**		0.04*		-0.03
				(2.12)		(1.97)		(-0.68)
Log(Total assets)	0.02	0.02	0.02	0.01	0.02	0.02	0.01	0.01
	(0.74)	(0.63)	(0.55)	(0.47)	(0.56)	(0.78)	(0.28)	(0.30)
Leverage ratio	0.05*	0.05**	0.05**	0.05**	0.05	0.05	0.02*	0.02**
	(2.01)	(2.13)	(2.16)	(2.03)	(1.37)	(1.35)	(1.80)	(2.08)
Return on assets	-0.04	-0.04*	-0.04	-0.04	-0.04***	-0.05***	-0.01	-0.01
	(-1.40)	(-1.84)	(-1.21)	(-1.66)	(-3.43)	(-8.76)	(-0.48)	(-0.44)
Log(No. of filings)	-0.02***	-0.02***	-0.02***	-0.02***	-0.01***	-0.01***	-0.02	-0.02
	(-3.57)	(-3.86)	(-3.37)	(-3.04)	(-3.55)	(-2.97)	(-1.56)	(-1.60)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	376	376	382	382	280	280	315	315
R^2	0.421	0.431	0.414	0.430	0.445	0.477	0.361	0.363

Figure 2.1: Emergence by assigned judges' characteristics

This figure plots the average emergence probabilities conditional on assignment to Depression-baby, economics-trained, Republican, or home judges. The dashed horizontal lines show the average emergence probabilities from the whole sample. *Depression baby* indicates assigned judges are born between 1920 and 1939. *Economics-trained* indicates judges have either attended the Manne program or graduated from law schools with a strong law and economics culture. *Republican* and *Democrat* indicates judges who have consistently voted for or donated to the Republican or Democratic parties, respectively, while *Nonpartisan* indicates judges without consistent support for either party. *Home judge* indicates whether a judge's home state is the same as the state where the firm is headquartered.
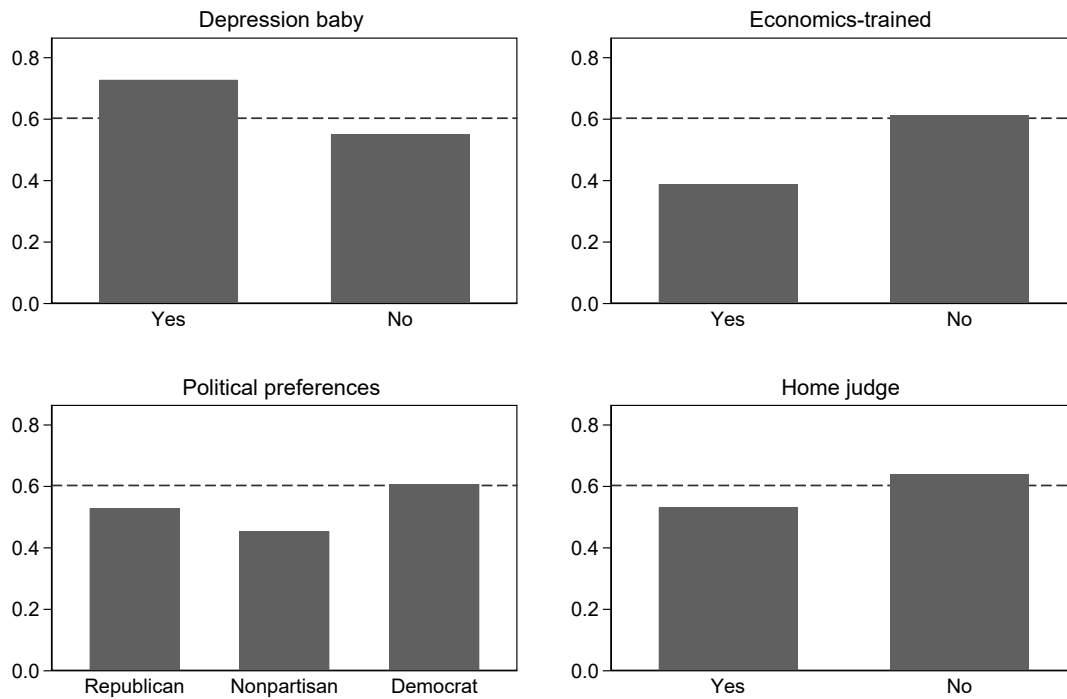
Figure 2.2: Months in bankruptcy by assigned judges' characteristics and by case outcomes

This figure plots the average months in bankruptcy by judge assignment. Each panel (a)–(d) compares the effect of case assignment to Depression-baby, economics-trained, Republican, or home judges, respectively. In each panel, I split the sample by case outcome (liquidation versus emergence) and, in each subsample, measure the average duration by judge assignment. The dashed horizontal lines show the average months in bankruptcy across the whole sample. See Figure 2.1 for variable definitions.
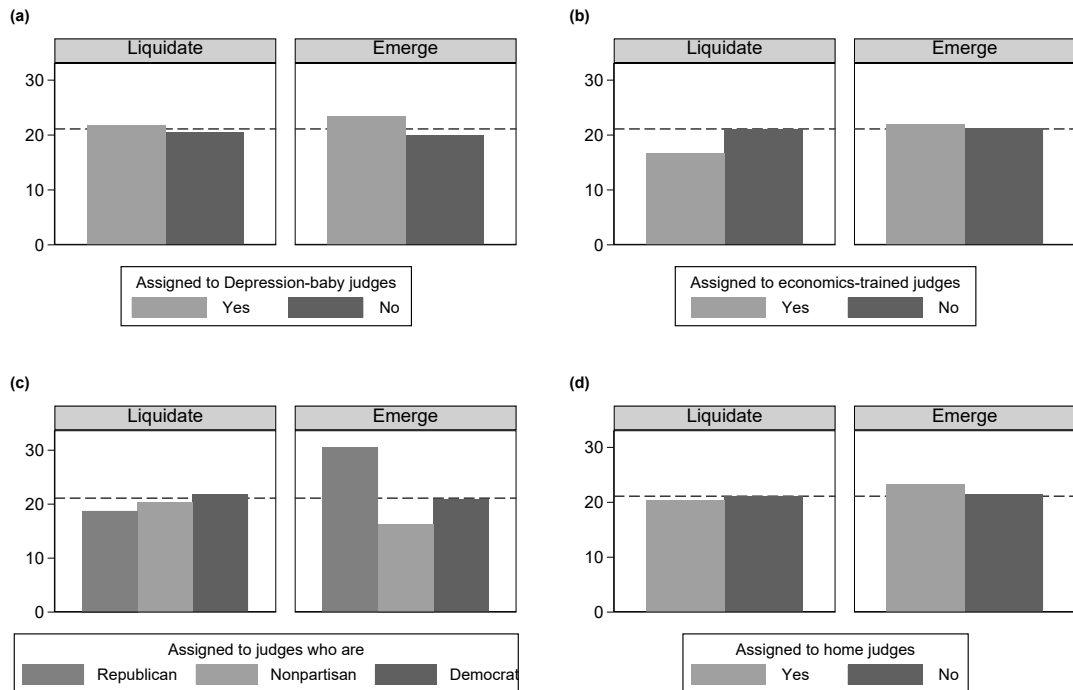
Figure 2.3: Post-emergence outcomes by assigned judges' characteristics

These figures plot average post-emergence outcomes conditional on assignment to Depression-baby, economics-trained, Republican, or home judges. Panels (a) and (b) report the average refiling rate and return on assets after emergence, respectively. The dashed horizontal lines show the average values for the full sample. See Figure 2.1 for variable definitions.

(a) Refiling rate
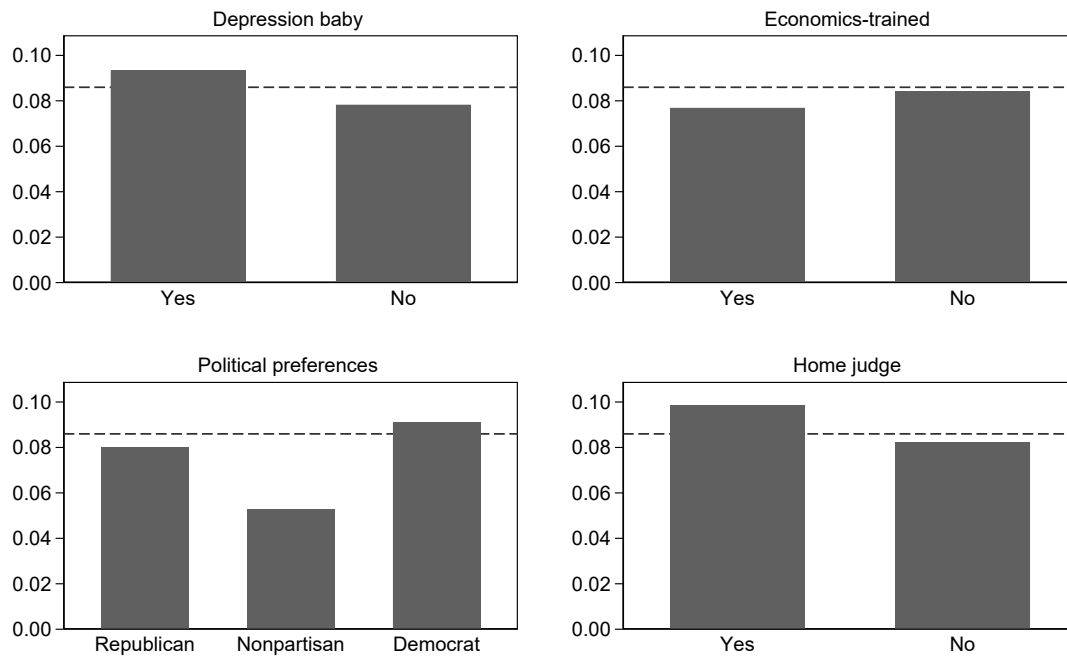
(b) Return on assets after emergence
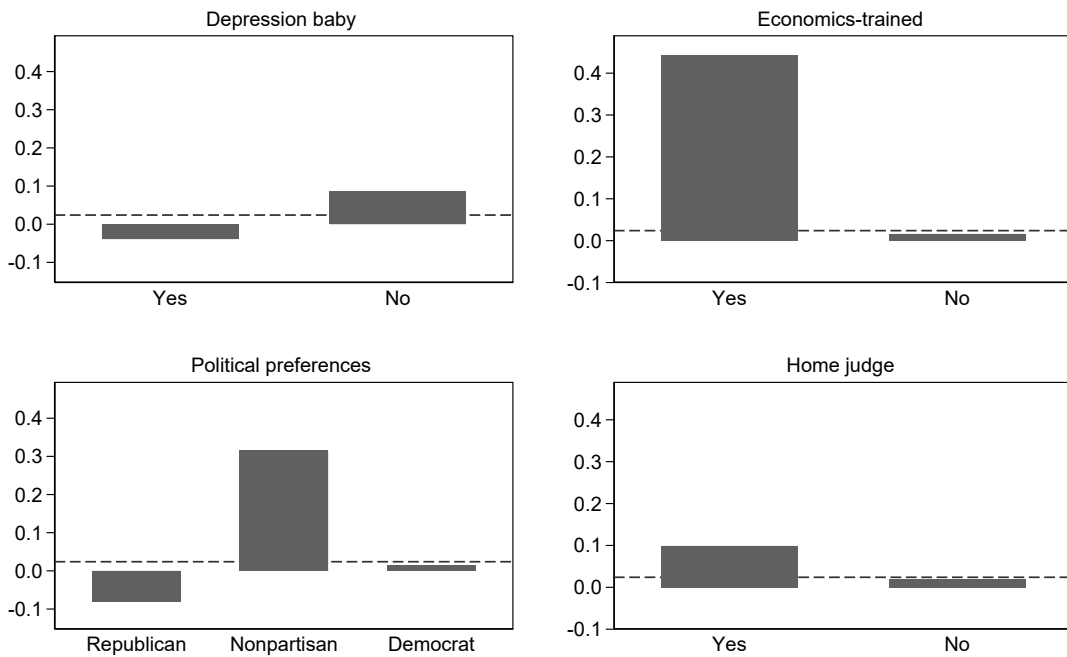
Appendix

2.A Appendix

2.A.1 Variable definitions

[Table 2.A.1 about here.]

2.A.2 Collecting biographical information of judges

I collect judges' biographical information from several sources. First, I obtain from official court websites biographical information (e.g., home state, educational background, and years of judge appointment and retirement). If such information is not available on court websites, I search legal databases, such as the American College of Bankruptcy, Martindale.com, LexisNexis, Westlaw Profiler, Bloomberg Law's People Search, Almanac of the Federal Judiciary available via Westlaw, Ballotpedia.com, LinkedIn, Marquis' Who's Who Biographies, and other online resources. In addition, for judges born before 1940, I identify a judge's birthplace using 1940 Census records.[29] I also contact judges for home state information.

Second, to supplement home state information that is not available from the previous steps, I conduct web searches. Specifically, I combine the name of a judge with common keywords that identify a person's hometown, such as *native*, *hometown*, *birth*, *born*, *grew*, *childhood*, or *high school*. Common sources include obituaries, judges' autobiographical interviews, and newspaper articles. To ensure I identify the correct person and the correct home state, I read each article and verify the match.

2.A.3 Additional summary statistics

[Table 2.A.2 about here.]

[Table 2.A.3 about here.]

2.A.4 Full tables showing all coefficients

[Table 2.A.4 about here.]

2.A.5 Additional robustness tests

[Table 2.A.5 about here.]

[Table 2.A.6 about here.]

[Table 2.A.7 about here.]

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[29]For details on using the Census data, I refer to Appendix B in Chuprinin and Sosyura (2018).

[Table 2.A.8 about here.]

[Table 2.A.9 about here.]

[Table 2.A.10 about here.]

Table 2.A.1: Variable definitions

Variable	Definition
A. Judge-level characteristics	
Depression baby	Indicator variable equals one if the judge is born between 1920 and 1939.
Economics-trained	Indicator variable equals one if the judge has ever attended the Manne program or has received a J.D. degree after 1970 from law schools that have a strong law and economics culture.
Republican (Democrat)	Indicator variable equals one if the judge has consistently voted for or donated to the Republican (Democratic) Party.
Home judge	Indicator variable equals one if the firm headquarter state and the judge's hometown state are identical.
Male	Indicator variable equals one if the judge is male.
Military	Indicator variable equals one if the judge has served in the military.
Months as judge	Number of months from the judge's appointment date to the case filing date.
Log(Months as judge)	Log of *Months as judge*.
B. Firm-level characteristics (measured at filing)	
Total assets	Total assets (in millions of 2020 dollars).
Log(Total assets)	Log of *Total assets*.
Leverage ratio	Liabilities / Total assets.
Return on assets	Net income / Total assets.
No. of employee	Number of employees.
No. of filings	Number of subsidiaries associated with the case at filing.
Secured debt ratio	Secured debt / Total assets
Bank debt ratio	Bank debt / Total assets
Localness	Number of times the headquarter state is mentioned in the firm's annual report (form 10-K), relative to the total number of mentions of all 50 states. The data are from García and Norli (2012).
C. Case outcomes	
Emerge	Indicator variable equals one if the bankrupt firm emerges from bankruptcy.
Months in Ch.11	Number of months in bankruptcy, from the filing date to the plan confirmation date.
Log(Months in Ch.11)	Log of *Months in Ch.11*.
Refile	Indicator variable equals one if the firm refiles for bankruptcy within three years of emergence.
Return on assets (post)	Net income after emergence / Total assets after emergence, winsorized at $\pm$ 100%.

Table 2.A.2: Court-level summary statistics

This table presents summary statistics for Chapter 11 filings between 1980 and 2020 by 62 bankruptcy courts in my sample. Each row refers to a bankruptcy court, where *No. of filings* denotes the total number of filings over the sample period, *No. of judges* denotes the number of unique judges who preside over filings made in that court, and *% Outside* denotes the share of cases in which firms file outside their headquarters state. When calculating *No. of judges*, I include visiting judges who preside outside the court to which they ordinarily belong; hence, a single judge may be counted multiple times across courts.

Court	No. of filings	No. of judges	% Outside	Court	No. of filings	No. of judges	% Outside
AL MD	2	1	0	MO ED	7	1	29
AL ND	2	2	0	MO WD	4	2	25
AL SD	1	1	100	MS SD	3	2	0
AR ED	1	1	0	NC ED	4	1	0
AZ	10	6	30	NE	1	1	100
CA CD	34	22	8.8	NH	3	2	0
CA ED	1	1	0	NJ	15	9	53
CA ND	19	8	5.3	NV	7	3	43
CA SD	3	2	0	NY ED	1	1	100
CO	9	6	0	NY ND	1	1	0
CT	1	1	0	NY SD	139	18	65
DE	254	21	99	OH ND	12	7	25
FL MD	10	4	10	OH SD	6	5	17
FL SD	8	3	13	OK WD	3	2	33
GA ND	11	7	45	OR	1	1	0
GA SD	1	1	0	PA MD	1	1	0
HI	1	1	0	PA WD	4	4	0
ID	1	1	100	TN ED	1	1	0
IL ND	22	11	14	TN MD	3	3	0
IN ND	1	1	100	TX ND	34	9	21
IN SD	4	2	0	TX SD	42	7	14
KS	4	4	0	TX WD	9	5	11
KY ED	1	1	100	UT	4	3	25
LA ED	5	3	20	VA ED	19	5	32
LA MD	1	1	0	VA WD	1	1	0
LA WD	1	0	0	VT	1	1	0
MA	10	7	20	WA ED	2	1	0
MD	6	4	17	WA WD	2	2	50
ME	1	1	100	WI ED	1	1	0
MI ED	12	5	0	WV ND	1	1	0
MN	1	0	0	WV SD	1	1	0

Table 2.A.3: Summary statistics at the judge level

This table presents the number of observations, mean, standard deviation, 25% percentile, median, and 75% percentile values of the main variables for 250 judges in my sample. Detailed variable definitions are in Appendix Table 2.A.1.

Variable	Obs	Mean	SD	P25	Median	P75
Depression baby	244	0.24	0.43	0.00	0.00	0.00
Economics-trained	248	0.05	0.22	0.00	0.00	0.00
Republican	121	0.21	0.41	0.00	0.00	0.00
Democrat	121	0.64	0.48	0.00	1.00	1.00
Home judge	183	0.34	0.48	0.00	0.00	1.00
Male	250	0.79	0.41	1.00	1.00	1.00
Military	250	0.27	0.45	0.00	0.00	1.00
Months as judge	250	103.35	80.18	39.06	96.03	149.39

Table 2.A.4: Heterogeneous effects of home judges: Stakeholders' "localness"

This table presents the heterogeneous effects of home judges on the probabilities of firms emerging from bankruptcy based on different levels of local stakeholders' interests. This table is identical to Table 2.8 but displays all coefficient estimates.

	(1)	(2)	(3)	(4)	(5)	(6)
Home judge	-0.38*	-0.36***	-1.04**	-0.85**	0.06	-0.65
	(-1.83)	(-2.82)	(-2.20)	(-2.13)	(0.07)	(-0.66)
Localness	-0.18		-0.29		0.28	-0.31
	(-1.07)		(-1.20)		(1.36)	(-1.56)
Home judge × Localness	0.59		4.36**		-2.54	1.58
	(1.07)		(2.16)		(-1.40)	(0.87)
Home judge × Leverage ratio		0.20*	0.60**			0.54**
		(1.91)	(2.40)			(2.11)
Localness × Leverage ratio			0.11			0.54**
			(0.60)			(2.41)
Home judge × Localness × Leverage ratio			-4.02*			-4.79**
			(-1.99)			(-2.18)
Home judge × Log(No. of employee)				0.09*	-0.05	-0.03
				(1.72)	(-0.48)	(-0.24)
Localness × Log(No. of employee)					-0.06**	-0.05
					(-2.27)	(-1.47)
Home judge × Localness × Log(No. of employee)					0.39*	0.41
					(1.74)	(1.37)
Log(No. of employee)				-0.05**	-0.12***	-0.12***
				(-2.25)	(-3.55)	(-3.28)
Log(Months as judge)	0.04	0.02	0.04	0.03	0.05	0.06*
	(1.23)	(0.58)	(1.16)	(0.70)	(1.68)	(1.97)
Log(Total assets)	0.06*	0.06**	0.07*	0.09***	0.15***	0.16***
	(1.69)	(2.54)	(1.82)	(3.78)	(5.07)	(5.25)
Leverage ratio	0.18*	0.17**	0.13	0.24**	0.24	0.13
	(1.78)	(2.44)	(0.88)	(2.50)	(1.41)	(0.86)
Return on assets	0.46*	0.21***	0.44*	0.17	0.60*	0.60**
	(1.84)	(2.85)	(2.01)	(1.30)	(1.78)	(2.18)
Log(No. of filings)	-0.02	-0.02	-0.02	-0.02	-0.03	-0.02
	(-0.40)	(-0.57)	(-0.27)	(-0.61)	(-0.53)	(-0.37)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Court-decade fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	307	602	307	601	306	306
R^2	0.603	0.507	0.613	0.513	0.628	0.644

Table 2.A.5: Judge characteristics and duration in bankruptcy

This table explores the effect of judge characteristics on case duration, irrespective of case outcome. The estimates are from OLS regressions in the specification provided below. The dependent variable is the log of the number of months a case spends in bankruptcy. Across columns, I use indicator variables for four different judge characteristics. In Column 1, the main explanatory variable is *Depression baby*, defined as judges born between 1920 and 1939. In Column 2, it is *Economics-trained*, defined as judges who either attended the Manne program or graduated from law schools with a strong law and economics culture. In Column 3, it is *Republican*, defined as judges who have consistently voted for or donated to the Republican Party. In Column 4, it is *Home judge*, defined as whether a judge's home state and the firm's headquarters state are identical. All columns include firm-level controls, and industry (Fama-French 12) and court-by-year fixed effects. Standard errors are clustered at the filing court level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

$$Log(Months\ in\ Ch.11)_{ircjt} = \alpha_r + \alpha_{ct} + \beta\,JudgeChar_j + \gamma X'_{ijt} + \varepsilon_{ircjt} \qquad (2.5)$$

	(1) Depression baby	(2) Economics-trained	(3) Republican	(4) Home judge
Judge characteristic	0.00	-0.05	-0.05	-0.05
	(0.04)	(-1.06)	(-0.16)	(-0.48)
Male	-0.18***	-0.16***	-0.05	
	(-3.11)	(-4.73)	(-0.45)	
Military	0.19	0.18	0.05	
	(1.33)	(1.37)	(0.54)	
Log(Months as judge)		-0.04**	-0.07***	-0.15***
		(-2.11)	(-18.04)	(-6.46)
Log(Total assets)	0.16***	0.17***	0.12***	0.18***
	(5.33)	(5.48)	(16.30)	(7.64)
Leverage ratio	0.14***	0.13***	0.13***	0.05
	(8.09)	(7.36)	(7.38)	(0.92)
Return on assets	0.04	0.03	0.14**	0.16**
	(0.52)	(0.39)	(2.40)	(2.02)
Log(No. of filings)	-0.01	-0.01	0.01	-0.01
	(-0.66)	(-0.85)	(0.82)	(-0.58)
Industry fixed effects	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	Yes
Observations	734	743	429	599
R^2	0.599	0.600	0.567	0.552

Table 2.A.6: Placebo test: Judge characteristics and duration in bankruptcy

This table presents the results of a placebo test. The sample includes only prepackaged or prenegotiated cases. The dependent variable is the log number of months a case spends in bankruptcy. I estimate the equation below using an OLS regression. Standard errors are clustered at the filing court level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

$$Log(Months\ in\ Ch.11)_{irc\,j\,t} = \alpha_r + \alpha_{ct} + \beta_1 JudgeChar_j + \beta_2 JudgeChar_j \times Emerge_i$$
$$+ \beta_3 Emerge_i + \gamma X'_{ij\,t} + \varepsilon_{irc\,j\,t}$$

	Depression baby		Economics-trained		Republican		Democrat	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judge characteristic	0.25	-0.01	0.29***	-0.26	-0.18	-0.01	0.24	0.29
	(1.19)	(-0.06)	(10.54)	(-1.09)	(-0.76)	(-0.05)	(1.41)	(0.79)
Emerge		-0.60*		-0.58**		-0.35		-0.36
		(-1.86)		(-2.67)		(-1.19)		(-1.24)
JudgeChar × Emerge		0.29		0.67**		-0.19		-0.03
		(0.60)		(2.34)		(-0.39)		(-0.06)
Male	-0.16	-0.15	-0.13	-0.15	0.29**	0.25**	0.28***	0.24**
	(-1.38)	(-1.53)	(-1.01)	(-1.27)	(2.69)	(2.10)	(3.85)	(2.82)
Military	0.38	0.26	0.41	0.29	0.21	0.16	0.21	0.15
	(1.46)	(1.26)	(1.44)	(1.27)	(1.31)	(1.17)	(1.09)	(0.91)
Log(Months as judge)			-0.05***	-0.03**	-0.04*	-0.03	-0.05**	-0.03
			(-4.91)	(-2.70)	(-1.77)	(-1.19)	(-2.48)	(-1.25)
Log(Total assets)	0.17**	0.19*	0.16**	0.17*	0.19**	0.19**	0.18**	0.19**
	(2.28)	(2.01)	(2.18)	(1.90)	(2.36)	(2.23)	(2.30)	(2.17)
Leverage ratio	0.05	0.07	0.06	0.09	0.10	0.12	0.10	0.12
	(0.56)	(0.73)	(0.94)	(1.20)	(0.99)	(1.16)	(1.00)	(1.15)
Return on assets	0.11*	0.08	0.14**	0.12*	0.20***	0.19***	0.19***	0.17***
	(1.95)	(1.22)	(2.27)	(1.93)	(3.44)	(3.35)	(4.21)	(4.02)
Log(No. of filings)	0.10*	0.09	0.10	0.08	0.09	0.08	0.09	0.08
	(1.69)	(1.49)	(1.61)	(1.44)	(1.51)	(1.39)	(1.50)	(1.35)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	376	376	382	382	280	280	280	280
R^2	0.504	0.524	0.507	0.530	0.527	0.536	0.530	0.539

Table 2.A.7: Judge characteristics and emergence from bankruptcy: Logit model

This table explores the effect of judge characteristics on the probabilities of emergence. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. This table is identical to Table 2.2, except that I use logit models. All columns include firm-level controls and industry (Fama-French 12) fixed effects. Standard errors are clustered at the filing court level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

	Depression baby		Economics-trained		Republican		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judge characteristic	0.61	0.78***	-0.66***	-0.77***	-0.70***	-0.67***	-1.24***	-1.33***
	(1.61)	(2.82)	(-3.26)	(-6.21)	(-4.80)	(-6.65)	(-2.61)	(-2.87)
Male		-0.39*		-0.02		-0.28		
		(-1.89)		(-0.05)		(-1.13)		
Military		0.13		0.04		-0.42		
		(0.38)		(0.11)		(-1.08)		
Log(Months as judge)				0.53***		0.57***		0.35
				(4.79)		(9.17)		(1.52)
Log(Total assets)	0.26***	0.25***	0.27***	0.25***	0.20*	0.17**	0.41***	0.40***
	(2.81)	(2.75)	(3.21)	(3.15)	(1.74)	(2.45)	(3.00)	(2.77)
Leverage ratio	1.49***	1.47***	1.43***	1.80***	1.16***	1.68***	2.06***	2.10***
	(3.75)	(3.75)	(3.49)	(4.32)	(3.69)	(4.32)	(4.39)	(4.50)
Return on assets	1.88*	1.92*	1.63	1.84	2.61*	2.91*	1.50***	1.51***
	(1.72)	(1.71)	(1.57)	(1.56)	(1.84)	(1.69)	(4.45)	(4.62)
Log(No. of filings)	-0.16***	-0.16***	-0.17***	-0.15**	-0.34***	-0.30***	-0.10	-0.09
	(-2.82)	(-2.70)	(-2.94)	(-2.46)	(-3.37)	(-4.32)	(-0.65)	(-0.61)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	425	425	432	432	233	233	407	407
Pseudo R^2	0.238	0.240	0.236	0.266	0.262	0.291	0.283	0.288

Table 2.A.8: Judge characteristics and emergence from bankruptcy: Alternative specification

This table explores the effect of judge characteristics on the probabilities of emergence. Columns 1–6 and 7–8 report estimates from linear probability models in specifications that are provided below. The dependent variable is an indicator variable equal to one if a firm emerges from bankruptcy. This table is identical to Table 2.2, except that I use alternative specifications. In Columns 1–6 (7–8), I substitute court-by-year (court-by-decade) fixed effects with separate court and year (decade) fixed effects. All columns include firm-level controls and industry (Fama-French 12) fixed effects. Standard errors are clustered at the filing court level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

Columns 1–6: $Emerge_{ircjt} = \alpha_r + \alpha_c + \alpha_t + \beta\,JudgeChar_j + \gamma X'_{ijt} + \varepsilon_{ircjt}$

Columns 7–8: $Emerge_{ircjt} = \alpha_r + \alpha_c + \alpha_{t'} + \alpha_j + \beta\,HomeJudge_{ij} + \gamma X'_{ijt} + \varepsilon_{ircjt}$

	Depression baby		Economics-trained		Republican		Home judge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Judge characteristic	0.05	0.06	-0.15***	-0.15***	-0.07*	-0.06	-0.15	-0.15
	(1.10)	(1.53)	(-5.46)	(-5.31)	(-1.75)	(-1.66)	(-1.50)	(-1.47)
Male		-0.06**		-0.03		-0.06		
		(-2.50)		(-0.98)		(-1.06)		
Military		0.04		0.02		0.03		
		(0.82)		(0.43)		(0.28)		
Log(Months as judge)				0.04***		0.04***		0.03
				(2.97)		(4.15)		(0.78)
Log(Total assets)	0.06***	0.06***	0.06***	0.06***	0.06***	0.06***	0.06**	0.06**
	(5.11)	(5.04)	(5.37)	(5.02)	(4.34)	(4.22)	(2.62)	(2.46)
Leverage ratio	0.16***	0.16***	0.16***	0.16***	0.14***	0.15***	0.20***	0.20***
	(4.17)	(4.20)	(4.33)	(4.35)	(4.78)	(4.83)	(3.95)	(3.77)
Return on assets	0.13	0.13	0.12	0.12	0.09	0.09	0.09	0.09
	(1.43)	(1.42)	(1.27)	(1.28)	(0.91)	(0.83)	(1.10)	(1.09)
Log(No. of filings)	-0.02	-0.02	-0.02	-0.02	-0.03**	-0.03**	-0.02	-0.02
	(-1.17)	(-1.27)	(-1.22)	(-1.14)	(-2.49)	(-2.50)	(-0.72)	(-0.70)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	737	737	746	746	431	431	602	602
R^2	0.306	0.308	0.302	0.309	0.350	0.358	0.452	0.452

Table 2.A.9: Judge characteristics and duration in bankruptcy: Alternative specification

This table explores the effect of judge characteristics on case duration when I split the sample by whether the case ends in liquidation or emergence. The estimates are from OLS regressions in the specification provided below. The dependent variable is the log of the number of months a case spends in bankruptcy. This table is identical to Table 2.3, except that I use alternative specifications. In all Columns, I substitute court-by-year fixed effects with separate court and year fixed effects. All columns include firm-level controls, industry (Fama-French 12). Standard errors are clustered at the filing court level, and t-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

$$Log(Months\ in\ Ch.11)_{ircjt} = \alpha_r + \alpha_c + \alpha_t + \beta\,JudgeChar_j + \gamma X'_{ijt} + \varepsilon_{ircjt}$$

	Depression baby		Economics-trained		Republican		Democrat	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Case outcome	Liquidate	Emerge	Liquidate	Emerge	Liquidate	Emerge	Liquidate	Emerge
Judge characteristic	-0.22*	0.01	-0.03	0.28**	-0.59**	0.19***	0.43***	-0.04
	(-2.01)	(0.12)	(-0.17)	(2.08)	(-2.67)	(2.77)	(2.78)	(-0.39)
Male	-0.00	-0.24***	-0.07	-0.23***	0.16	-0.03	0.08	-0.03
	(-0.01)	(-4.79)	(-0.42)	(-5.77)	(0.99)	(-0.56)	(0.55)	(-0.37)
Military	0.16	0.09	0.22**	0.09	0.13	0.06	0.17	0.04
	(1.44)	(1.48)	(2.28)	(1.36)	(0.84)	(0.73)	(0.92)	(0.44)
Log(Months as judge)			-0.12***	-0.04**	-0.23***	-0.03	-0.24***	-0.03
			(-2.82)	(-2.27)	(-3.65)	(-1.43)	(-4.15)	(-1.41)
Log(Total assets)	0.14***	0.18***	0.14***	0.18***	0.08	0.17***	0.08	0.17***
	(3.28)	(4.27)	(3.38)	(4.35)	(1.51)	(6.48)	(1.26)	(6.51)
Leverage ratio	0.23**	0.09	0.10	0.10	-0.00	0.13**	0.01	0.13**
	(2.08)	(1.16)	(1.28)	(1.23)	(-0.01)	(2.41)	(0.07)	(2.65)
Return on assets	0.38**	-0.03	0.30	-0.01	0.22	-0.02	0.23	-0.02
	(2.17)	(-0.49)	(1.49)	(-0.17)	(1.28)	(-0.37)	(1.38)	(-0.36)
Log(No. of filings)	-0.05	-0.00	-0.07*	-0.01	-0.04	0.00	-0.03	0.00
	(-1.12)	(-0.16)	(-1.99)	(-0.28)	(-1.03)	(0.00)	(-0.60)	(0.04)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	296	438	299	444	180	249	180	249
R^2	0.424	0.497	0.439	0.495	0.521	0.566	0.518	0.561

Table 2.A.10: Judge characteristics and post-emergence outcomes: Alternative specification

This table explores the effect of judge characteristics on post-emergence outcomes. Columns 1–6 and 7–8 report estimates from linear probability models in specifications provided below. The dependent variable is either an indicator equal to one if a firm refiles within 3 years of emergence (*Refile*) or return on assets after emergence (*ROA (post)*). This table is identical to Table 2.4, except that I use alternative specifications. In Columns 1–6 (7–8), I substitute court-by-year (court-by-decade) fixed effects with separate court and year (decade) fixed effects. All columns include firm-level controls and industry (Fama-French 12) fixed effects. Standard errors are clustered at the filing court level, and *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Definitions of variables are in Appendix Table 2.A.1.

Columns 1–6: $PostOutcomes_{ircjt} = \alpha_r + \alpha_c + \alpha_t + \beta\,JudgeChar_j + \gamma X'_{ijt} + \varepsilon_{ircjt}$

Columns 7–8: $PostOutcomes_{ircjt} = \alpha_r + \alpha_c + \alpha_{t'} + \alpha_j + \beta\,HomeJudge_{ij} + \gamma X'_{ijt} + \epsilon_{ircjt}$

	Depression baby		Economics-trained		Republican		Home judge	
	(1) Refile	(2) ROA (post)	(3) Refile	(4) ROA (post)	(5) Refile	(6) ROA (post)	(7) Refile	(8) ROA (post)
Judge characteristic	-0.06	0.05	0.09***	0.36	-0.02	-0.06	-0.00	0.22**
	(-1.34)	(0.76)	(3.80)	(0.98)	(-0.21)	(-0.18)	(-0.01)	(2.18)
Male	-0.04	-0.05	-0.06*	-0.05	-0.04	0.05		
	(-1.39)	(-0.59)	(-1.96)	(-0.65)	(-0.55)	(1.07)		
Military	0.03	0.02	0.03	0.04	-0.03	0.13		
	(0.53)	(0.18)	(0.47)	(0.36)	(-1.19)	(0.95)		
Log(Months as judge)			0.00	0.00	-0.02	0.03	-0.02	0.05
			(0.20)	(0.08)	(-0.81)	(1.11)	(-0.78)	(0.85)
Log(Total assets)	-0.02	0.01	-0.02	0.00	-0.00	0.08**	-0.01	0.02
	(-0.75)	(0.32)	(-0.83)	(0.15)	(-0.09)	(2.74)	(-0.34)	(0.51)
Leverage ratio	0.01	0.08	0.01	0.12	0.04	0.12	0.01	0.16
	(0.78)	(0.81)	(0.67)	(0.95)	(1.27)	(1.11)	(0.55)	(1.37)
Return on assets	-0.05	-0.03	-0.05	-0.01	-0.04	0.01	-0.17	0.04
	(-0.66)	(-0.55)	(-0.65)	(-0.09)	(-0.51)	(0.09)	(-0.88)	(0.17)
Log(No. of filings)	0.00	0.00	0.00	0.01	0.00	-0.03	0.01	0.01
	(0.08)	(0.18)	(0.03)	(0.29)	(0.21)	(-0.70)	(0.40)	(0.32)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge fixed effects	No	No	No	No	No	No	Yes	Yes
Observations	416	203	422	208	236	117	351	178
R^2	0.339	0.469	0.325	0.452	0.389	0.593	0.430	0.664

Chapter 3

Disqualifying Managerial Misconduct in Corporate Bankruptcy

with S. Lakshmi Naaraayanan and Kasper Meisner Nielsen

Abstract

This study examines the introduction of bankruptcy quarantines that disqualifies managers engaging in negligent business practices for up to 3 years. Using administrative register data from Denmark, we document that disqualifications discourage future business activity: After the quarantine, individuals are 15% less likely to be managers or business owners. Disqualified individuals are also less likely to be involved in future bankruptcies or future criminal activities. At the same time, the fraction of family members of disqualified individuals who are active in a management role increases from 10% to 30%. We also find changes to the managerial labor pool, resulting in more CEOs with a criminal record and those that rely solely on social transfers. Overall, our findings provide the first systematic evidence on the governance consequences of disqualifying managerial misconduct in corporate bankruptcies.

3.1 Introduction

Corporate bankruptcy law aims to strike a balance between protecting creditors and relieving debtors of financial obligations. Seminal studies document the importance of creditor protection for the well-being of financial markets and economic growth (e.g., La Porta et al. 1997; La Porta et al. 1998; Levine 1998; Levine 1999; Levine et al. 2000), while others document that relieving debtors of financial obligations gives them a second chance (e.g., Aghion et al. 1992; Thorburn 2000; Baird and Morrison 2005; Ayotte 2007; Bernstein et al. 2019; Waldock 2020). A less studied, but key feature in many bankruptcy regimes, is the role of disqualifying individuals from serving as managers, directors or owners if they have engaged in gross managerial misconduct. Such disqualifications aim at restricting second-chance policies for individuals who engaged in managerial misconduct and lower the incidence of future bankruptcy to reduce misallocation of resources. Despite the importance of disqualifications among policymakers, empirical evidence and its concomitant implications for corporate bankruptcy remain scant.

From a theoretical perspective bankruptcy law has important implications for aspiring entrepreneurs, credits markets that facilitate entrepreneurial activities as well as the public. For entrepreneurs, the bankruptcy law determines their exposure to downside risk by providing wealth protection through limited liability, while simultaneously limiting creditors' exposure to business failures. A major concern is that individuals might misuse the protection of limited liability under bankruptcy laws to engage in irresponsible business conduct or even fraud. The main purpose of managerial disqualifications is to protect creditors and the public from individuals who seek to abuse their position, as a manager, of a limited liability company. Managerial misconduct extends from breaches of statutory duties within the Companies Act legislation, to not paying taxes or VAT, or to at the other end of the spectrum involving fraud or other acts of a criminal nature. Therefore, the aim of the disqualification is to make managers personally liable for misconduct by restricting their future ability to manage a business.

In this study, we evaluate the introduction of managerial disqualifications using administrative register data from Denmark.[1] To identify disqualifications, we rely on three lists published by regional newspapers in 2017. These regional newspapers, owned by the same media holding company, obtained the information from local bankruptcy courts by filing a freedom of information request. Although the bankruptcy quarantine law explicitly specifies that the register is not accessible to the public, the courts decided to comply with the request and provide a year-to-date overview of disqualification decisions by their court. The lists were subsequently published by the newspapers, and cover disqualifications of 69 managers of bankrupt firms who engaged in managerial misconduct during the first three quarters of 2017 in six out of 24 bankruptcy court districts in Denmark. For 58 of these managers, we obtain administrative register data on their business position, income, wealth and criminal records and match them to a control group of comparable bankrupt managers.

Our empirical specification compares individuals identified from these newspapers to a matched control group of managers of bankrupt firms who were not disqualified, operated their business in the same location and went bankrupt at the same time as the firm of the disqualified manager. Specifically, the control group is formed by exact matching on location (court district), time of bankruptcy, age cohort, and total income. This matching criterion mitigates concerns regarding selection on observables while the inclusion of individual fixed effects additionally controls for time-invariant individual characteristics. The main strength of our empirical strategy is that it exploits variation within individuals, while the main caveat is that it cannot rule out concerns related to time-varying individual differences that emanate around managerial disqualifications.

We begin by examining the consequences of bankruptcy quarantines for disqualified individuals. We find that disqualified managers do not return as managers and owners of companies after the expiry of the quarantine. As a result, they have lower incomes both during and after the quarantine. The effects are

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[1]For ease of reference, we interchangeably refer to bankruptcy quarantines and managerial disqualifications.

economically large: during the quarantine income is 32,000 DKK lower, equivalent to a 25 percent relative to the average income two years before the quarantine. Income drops further, relative to the control group, such that the treatment group earns 122,700 DKK less than the control group after expiry of the quarantine. The drop in income during the quarantine is driven by labor income, while only a fraction of the drop in income after the quarantine results from lower labor income. However, the drop in income does not significantly affect their wealth. Further, we examine and find that disqualified managers respond by switching their activities towards family members, especially spouses but not children, to avoid being penalized in the future. Lastly, focusing on the business network of disqualified managers, we do not find that they become more active – if anything they become less active – relative to the business network of the control group.

Next, we study the effect of disqualifications on the managers' future business formation. Disqualification orders do not prevent individuals from founding new companies, allowing them to potentially remain involved in business activities. We find that after bankruptcy, the treatment group is 5.8 percent less likely to form a new company. We note that both during and after disqualification, the treatment group is less likely to found by 6.0 and 4.9 percentage points, respectively: the economic magnitude is significant given that pre-bankruptcy level of business formation is about 15%. Further tests suggest that family members of disqualified individuals are more likely to register new businesses after the disqualification period expires. This increase in family member business formation is consistent with the possibility of them acting as strawman managers or owners. Therefore, the evidence suggests that disqualifications discourage new business formation by the disqualified individuals themselves, but this effect seems to be mitigated by the increased involvement of family members.

We next examine the effect of disqualifications on the managers' future business misconduct. Given that disqualification orders are imposed when managers use bankruptcy protection to commit misconduct, we expect that disqualified individuals would have a higher likelihood to refile for bankruptcies or commit similar crimes again if not disqualified. We find that after bankruptcy, the treatment group is 19 percentage points less likely to refile for bankruptcy and 7-10 percentage points less likely to commit a crime (depending on type of crimes). Even after disqualification expires, the effect persists, with a reduction of 11 percentage points in refiling, and 9-14 percentage points in crimes (depending on type of crimes) compared to the control group. The effect is statistically and economically significant. For instance, the relative reduction in fraud after quarantine is about 48 percent, when comparing to pre-quarantine levels. These findings suggest that disqualifications effectively reduce recidivism among managers with a higher propensity to commit business misconduct.

Lastly, we study the implications of bankruptcy quarantines for the managerial labor pool. If managerial disqualifications are costly for individuals, then one could expect individuals with criminal intentions to anticipate these costs and avoid having formal managerial responsibilities. We capture variation in individual characteristics that are plausibly related to the exposure to the labor market, by identifying the fraction of foreigners and individuals receiving public (welfare) transfers in the managerial labor pool. The main idea is that these individuals have lower personal costs of being disqualified. Following the introduction of bankruptcy quarantines, we observe an increase in the share of managers who are foreign citizens, those of foreign origin, and those relying on public transfers. The increase is statistically as well as economically significant. The fraction of foreigners increases by 1.3 percentage points relative to an average level of 6.4 percent. These results suggest that the labor pool shifts towards individuals with lower personal costs of being disqualified.

Moreover, if disqualifications discourage gross business neglect, one expects the reform to have significant effects on the composition of the managerial labor pool with respect to managers with past bankruptcy experiences and criminal records. To gauge this effect, we examine whether the fraction of managers with a past bankruptcy and the fraction of managers with a criminal record change after the reform. Following the introduction of bankruptcy quarantines there is a decline in the fraction of managers with a past bankruptcy.

Thus, the reform appears to slow the tendency of managers to be involved in bankruptcies. Specifically, we study the fraction of managers with any criminal record, fraction of managers convicted of white-collar crime and the fraction of managers convicted of fraud. For all three measures, we note that the fraction of the labor pool with criminal records increases prior to the reform and tends to decrease after the introduction of the reform. Importantly, the decline in the fraction of managers with a criminal record is not immediate, suggesting that it takes time for the effect to kick in. Interestingly, the timing of the decline seems to coincide with the increase in the number of disqualifications. Collectively, these results suggest that the managerial disqualifications discourage individuals from becoming repeat offenders.

Our first contribution is to the literature on personal costs of bankruptcy. Focusing on top executives several studies report a large decline in CEO income after bankruptcy fillings (Gilson 1989; Gilson and Vetsuypens 1993; Eckbo and Thorburn 2003; Eckbo et al. 2016) and a small decline in CEO income after liquidations (Grindaker et al. 2021). In comparison to these studies, we document that the personal costs are larger for managers engaging in gross misconduct compared to managers of bankrupt firms that did not engage in misconduct.

Our second contribution is to inform the literature examining the frictions impeding the efficiency of the bankruptcy process. Prior work has focused on the role of asymmetric information, conflicts of interest among creditors, judicial process, and intermediary frictions documenting that these impose large economic costs (Iverson 2017; Dou et al. 2021; Antill 2022; Antill 2024; Iverson et al. 2022; Antill and Clayton 2024). Another strand of literature has focused on the role of downside protection in the bankruptcy law. Theoretical studies posit that downside protection fosters overall entrepreneurship (Landier 2005; Ayotte 2007; Jia 2015; Mankart and Rodano 2015). Consistent with this theoretical prediction, empirical evidence shows that downside protection increases entrepreneurship (Fan and White 2003; Armour and Cumming 2008; Cerqueiro et al. 2019; Kang 2024), while other studies find no effect of downside protection in personal bankruptcy and entrepreneurship (Akyol and Athreya 2011; Cumming and Li 2013; Paik 2013; Traczynski 2019). Relatedly, several studies show that downside protection against entrepreneurial failure in the labor market can stimulate entrepreneurial activities (Hombert et al. 2020; Koudijs and Salisbury 2020; Ersahin et al. 2021; Gottlieb et al. 2022). In comparison to these studies, we examine whether managerial disqualifications affect the likelihood of starting a new business relative to failed entrepreneurs, who did not get disqualified. To this end, we find that disqualified managers are less likely to start new businesses, less likely to go bankrupt again in the future, and less likely to be convicted of fraud in the future.

Relatedly, our study also contributes to research examining the impact of removing information about past bankruptcy or delinquency on entrepreneurship. These studies document that removing such information has either positive, negative, or no impact on entrepreneurship (Bos et al. 2018; Dobbie et al. 2020; Cahn et al. 2021; Herkenhoff et al. 2021). In comparison to these studies, we show that disqualifications are effective in limiting the business activity of individuals who engaged in misconduct, even if the disqualification is not public knowledge.

Lastly, our results speak to the literature on personal cost of misconduct for managers and directors. Prior literature documents significant personal costs in terms of future employment for managers associated with financial misrepresentation (Karpoff et al. 2008), price fixing (Combe and Monnier 2011; Connor and Lande 2012; González et al. 2019), and tax avoidance (Gallemore et al. 2014). Similarly, a large body of evidence suggest that directors are replaced following lawsuits and SEC enforcement action (Romano 1991; Farber 2005; Ferris et al. 2007), financial irregularities (Gilson 1989; Srinivasan 2005; Fich and Shivdasani 2007; Ertimur et al. 2012), or departure from value-maximizing decisions (Coles and Hoi 2003; Harford 2003; Jiang et al. 2016). In comparison to these studies, we examine the effect of managerial disqualifications due to misconduct in corporate bankruptcies. We find that disqualified managers are less likely to start a business, less likely to go bankrupt in the future, and less likely to be convicted of fraud in the future. We also find evidence of significant personal costs as personal income after the expiry of the disqualification

remains low relative to bankrupt managers that were not disqualified.

Finally, our results have implications for policy makers and the working of bankruptcy law. Our findings highlight the importance of balancing the incentive to take risks by providing aspiring entrepreneurs with downside protection (i.e., limited liability) while maintaining personal liability for managerial misconduct. Our findings suggest that managerial disqualification limits individuals' ability to continue their misconduct. Our findings also stress some limitation of disqualifications by providing evidence of strawman appointments of family members, and potential consequences for the managerial labor pool. To this end, we provide the first evidence on the effect of managerial disqualifications for individuals as well as the managerial labor market.

Our study proceeds as follows. Section 3.2 details the institutional setting and the changes to the bankruptcy law that introduces disqualifications for managerial misconduct. In Section 3.3, we describe the construction and sources of our data. Section 3.4 details our research design and difference-in-differences analysis that compares outcomes for disqualified managers of bankrupt firms to those of comparable managers of bankrupt firms who did not get disqualified. In Section 3.5 we examine the personal costs of managerial disqualifications. We analyze the effect on disqualified individuals' positions as manager, owner, and board members, as well as their personal income during and after the disqualification period. Section 3.5 also provides evidence on possible "straw man" appointments, involving family members or business associates of disqualified managers. In Section 3.6, we study whether disqualified managers start new businesses as well as the likelihood of future bankruptcies and criminal activity. Section 3.7 examines the effect of the introduction of bankruptcy quarantines on the managerial labor pool. We then conclude. An online appendix provides additional evidence and alternative specifications.

3.2 Changes to the bankruptcy law and introduction of managerial disqualifications

On January 1, 2014, Denmark implemented a law introducing "bankruptcy quarantines" that disqualify managers of bankrupt firms who engaged in negligent business practices. Examples of negligent business practices range from fraud, failing to pay taxes or VAT, shareholder loans, hiding assets from the bankruptcy court, omitting to file annual reports, or inappropriately using the bankruptcy process to a creditor's detriment.

The maximum disqualification period is three years, during which the individual cannot hold management positions in limited liability companies. Notably, the disqualification does not apply to ownership or directorships in limited liability firms, nor to positions in firms with unlimited liability (i.e. sole proprietorships or partnerships).

Disqualification is ordered by the bankruptcy court based on evidence and findings presented by the court-appointed liquidator (*kurator*) and a response from the manager's lawyer. If a disqualification is imposed, the court notifies the Central Business Register, which deregisters the individual from managerial roles in other limited liability companies. Article 157 of the Danish Bankruptcy Law specifies that negligent business conduct must be a result of "gross managerial misconduct" to qualify for disqualification. Case law suggests that several violations can stand alone as grounds for disqualification. Examples of these are a) lack of or misleading bookkeeping, b) misuse of company funds, including transfer or loans to management or shareholders, c) failure to settle taxes or VAT, and d) tax evasion or fraud. Less serious violations typically require multiple occurrences alongside other violations to trigger disqualification.[2] In practice, most disqualifications result from a combination of violations.

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[2]Examples of less serious violations are: a) non-payment of the contributed capital, b) non-cooperation in the disclosure of the bankruptcy case and liquidation, c) not pay salary to the employees, d) withholding of pension for the employees, e) continuation of the company's operations beyond what is economically reasonable, and f) introducing a straw man.

A key feature of the reform of the bankruptcy law is that disqualification information is not publicly available. Article 12 of the Law on Bankruptcy Quarantines explicitly prohibits sharing the registry of disqualified managers with third parties, including the public, except in legally mandated situations. This lack of transparency makes it difficult for market participants to identify managers associated with negligent business practices. The lack of transparency probably also implies that our estimates provide a lower bound for the personal costs of disqualifications.

Case evidence suggests that bankruptcy judges have followed the legislative intent, ordering disqualifications in 72% of cases where the liquidator recommends them. In most convictions, the maximum disqualification period of three years is imposed (95% of cases). A small percentage (around 5%) receive a two-year or one-year disqualification.

3.3 Data and descriptive statistics

3.3.1 Data

We construct a dataset with 98,738 corporate bankruptcies between 2010 and 2021 in limited liability firms (both publicly and privately held) in Denmark. Our dataset contains information on managers, owners, and board members, as well as personal and family information about them. Individuals are identified by the *Central Personal Registration number* (CPR number), the equivalent of the social security number in the United States, which ensures that we have unique identification across individuals and data sources. The dataset is constructed based on five different sources, as explained below.

We obtain access to data on firms, managers, owners, and boards members from the official records of the Central Business Register (*CVR registeret*) at the Danish Business Authorities (*Erhvervsstyrelsen*). Danish corporate law mandates that all firms register with the Business Authorities, who track firm status (active or bankrupt). The law also requires firms to report changes in management, ownership, and board membership within two weeks of occurrence. We use this data to identify bankruptcies and associated managers, owners, and board members.

Income and wealth data are from the official records at the Danish Tax and Customs Administration (*SKAT*). This dataset includes personal income and wealth information for the Danish population, linked by CPR number. SKAT receives this information directly from relevant sources, such as financial institutions reporting customer deposits and security investments and employers reporting employee wages. We access income and wealth data from 2010 to 2022 through Statistics Denmark to evaluate the personal costs associated with managerial disqualifications.

Data on criminal offences come from the Danish Central Crime Register (*Det Centrale Kriminalregister*) at the Danish National Police (*Rigspolitiet*). The data contain records of all criminal offences, legal charges, convictions, and fines exceeding DKK 1,500. All records are linked to individuals by CPR number and include details about the nature of crime, police district, and legal outcome. Within the Danish Crime Registers there are several datasets which we exploit in our analysis: Criminal Charges (*Kriminalstatistik*) gives us all individuals charged with a crime, the date of the crime they are being charged with and a 7-digit code which describes the criminal activity. Each record includes a 16-digit journal number assigned by the police district at the time of the charge. This code, along with the CPR number, allows us to link between crime datasets (e.g., charged crimes to convictions) and across datasets (e.g., crimes committed to financial and demographic data). Convictions (*Kriminalstatistik afgørelser*) inform us of the legal decisions of the criminal activity. From this database we also exclude individuals whose charges were subsequently dropped, withdrawn, were acquitted, or received a written warning.

Individual and family data originate from the official Danish Civil Registration System. These records include the personal identification number (CPR), gender, date of birth, CPR numbers of family members

(parents, children, and siblings), and their marital histories (number of marriages and divorces). In addition to providing individual characteristics, such as age, gender, and marital status, these data enable us to identify family members of bankrupt managers.

We supplement the administrative register data with information about individuals who were disqualified from being managers by the bankruptcy court. Ideally, we would have access to all disqualifications. However, Danish bankruptcy quarantine law restricts public access to the disqualification registry and prohibits information sharing with third parties except in legally mandated situations. To address this limitation, we rely on three lists of disqualifications published by regional newspapers in 2017. These newspapers, owned by the same media holding company, obtained the information through freedom of information requests submitted to local bankruptcy courts. While the law restricts public access, the courts complied with these requests, providing a year-to-date overview of disqualification decisions of 69 managers for the first three quarters of 2017 in six out of 24 Danish bankruptcy court districts (see Appendix Figure 3.A.1 for an example newspaper article and Appendix Figure 3.A.2 for a map of covered jurisdictions). We note that while these cases are a small subsample of the number of bankruptcies and cases involving disqualifications between 2014 and 2021 as shown in Appendix Figure 3.A.3, the timing and geography of our sample is close to random. We further note that our results remain qualitatively unaffected if we benchmark outcomes for disqualified managers to all bankrupt managers in 2017. Finally, more than 90% of the disqualifications are for a period of three years, as shown in Appendix Figure 3.A.4, which allows us to simplify the empirical specification.

3.3.2 Sample and descriptive statistics

As mentioned in the previous section, the data on managerial disqualifications cover 69 managers of bankrupt firms during the first three quarters of 2017 in six out of 24 court districts. To ensure that our results are not driven by regional differences or macro-economic conditions, we form a matched control group of managers of bankrupt firms who were not disqualified, operated their business in the same location and went bankrupt at the same time as the firm of the disqualified manager. As a result of the matching criteria, the sample of disqualified managers in the current version of our study is reduced to 58.

Specifically, the control group is formed by exact matching on location (court district), time of bankruptcy, and age cohort (± 5 years). Among the managers that match these criteria, we select *all*-nearest neighbors based on total income, with distance less than 200,000 DKK, and assign the date of disqualification to managers in control group within each treated-control pair.[3] This matching procedure ensures that the treatment and control groups are comparable.

Table 3.1 summarizes the individual characteristics of the treatment and control groups two years prior to the bankruptcy ruling. The treatment group comprises individuals that are managers of a bankrupt company and are subsequently disqualified, while the control group are managers of a bankrupt company who did are not disqualified.

The table shows that the groups are statistically indistinguishable on most characteristics, except for pre-bankruptcy criminal activity (Panel B). Panel A shows that both groups have similar levels of income. They also have negative net wealth, suggesting financial distress at the personal level, while that of the control group is lower. Panel B shows that the treatment group is significantly more likely to have committed a crime before bankruptcy, particularly white-collar crimes and frauds. Such higher criminal propensity reflects the rationale behind disqualification orders, which target managers who engage in misconduct leading to bankruptcy. Panels C and D show that both groups have similar levels of business activities measured by the number of managerial positions held or the likelihood of being a manager or an owner.

[Table 3.1 about here.]

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[3]In unreported results, we find our results remain unaffected, both quantitatively and qualitatively, even when using all managers of bankrupt firms as the alternative control group.

3.4 Empirical strategy

To estimate the personal cost of disqualification for managers, we follow the labor economics literature on earnings losses of displaced workers (Jacobson et al. 1993). We estimate Equation 3.1 for individuals, where the dependent variable is either indicators for business positions (manager, owner, or board member), income, or net wealth (defined as assets minus liabilities) in year 2000 DKK, y_{it}, for manager i in year t:

$$y_{it} = \alpha_i + \beta \text{ Treated }_i * \text{ Post }_{i,t} + \gamma_t + \epsilon_{it}, \tag{3.1}$$

where the parameters α_i represent individual fixed effects, α_t represent time fixed effects, $Treated_i$ is an indicator for disqualified individuals, $Post_{it}$ is an indicator for individuals during and after the disqualification period. The parameter β captures the personal cost of disqualifications, while ϵ_{it} is the error term. To further explore the short-term and long-term effects of disqualification on individuals, we separate the $Post_{it}$ into two periods and estimate Equation 3.2 as follows:

$$y_{it} = \alpha_i + \beta_1 \text{ Treated }_i * \text{ During }_{i,t} + \beta_2 \text{ Treated }_i * \text{ After }_{i,t} + \gamma_t + \epsilon_{it}, \tag{3.2}$$

$During_{i,t}$ is an indicator for disqualified individuals during the disqualification period, and $After_{i,t}$ is an indicator for disqualified individuals after the expiry of the disqualification period. The parameter β_1 captures the short-term effect of disqualification, and β_2 captures the long-term effect after the expiry of disqualification. In all specifications, we interact the fixed effects with pair identifiers that distinguish specific treated-control manager pairs. This interaction ensures that β_1 and β_2 are estimated within the tightly defined treated-control pairs of individuals. Standard errors are clustered at the individual level.

The matching procedure reduces concerns about selection bias on observable characteristics. The inclusion of individual fixed effects further controls for time-invariant individual characteristics. While our empirical strategy mitigates concerns related to time-invariant characteristics, however, we are unable to rule out concerns related to time-varying individual differences that may arise around managerial disqualifications.

3.5 Personal costs of managerial disqualifications

We begin by examining changes to disqualified individuals' involvement in businesses relative to a matched control group of bankrupt managers that are not disqualified. Subsequently, we study the effect on managerial positions, directly affected by the disqualification, as well as positions as owners and directors that are likely (indirectly) affected by the disqualification. Additionally, we also examine the effect of disqualifications on managerial income and net wealth.

3.5.1 Effect on disqualified managers.

Figure 3.1 shows the fraction of individuals that are managers (top panel) and owners (bottom panel) of a company for the two groups: the treatment group (solid gray line) consisting of individuals that managed a bankrupt company and were disqualified, and the control group (black dotted line) consisting of individuals that managed a bankrupt company but did not get disqualified. The dashed vertical lines at event time 0 and 3 show the individual-specific start and end of the disqualification period, respectively. This divides the event window into three periods: a period *before*, a period *during* and a period *after* the disqualification.

We note that before the disqualification, the fraction of managers and owners in both groups follows a similar pattern, initially increasing and then decreasing. However, the decline is larger for disqualified individuals implying that they are less likely to be a manager or owner of any firm both during and after the expiry of the disqualification. After the disqualification, we note a slight increase in the probability of

being a manager for the treatment group. That said, less than 20 percent of disqualified managers manage to stage a comeback.

Table 3.2 reports the result from a linear probability model where the dependent variables are indicators for being a manager, owner, or a board member. For each outcome, we first run a specification in Equation 3.1. We note that after quarantine, the treatment group is 20 percentage points less likely to be owners. The effect is economically as well as statistically significant. We then run the specification in Equation 3.2 where we separate the Post × Treated effect into indicators for during quarantine, and after quarantine to differentiate between the shorter-term direct and longer-term indirect effects of the disqualifications. We note that the disqualified individuals are 11 percent less likely to be a manager after the expiry of the disqualification, and 15 percent less likely to be an owner relative to the tightly matched control group. We also note that there is no effect on directorships as shown in columns 5 and 6 of Table 3.2. Overall, disqualified individuals are less likely to be managers or owners after the expiry of the disqualification period.

Next, to gauge the financial consequences for disqualified individuals, Figure 3.2 shows the time-series evolution of average income and net wealth for the treatment and control group separately. The top panel focuses on total personal income which is the sum of labor income, entrepreneurial income, financial income, and other sources of income. The personal income of bankrupt managers is low before the quarantine and increases thereafter. More interestingly, we find that the relative difference between the treatment and control group is small before the disqualification but increases both during and after the disqualification. The bottom panel shows the average net wealth (assets minus liabilities). We note that net wealth is negative before the bankruptcy for both groups, indicating that liabilities exceed assets. As with income, net wealth tends to increase for both groups after the bankruptcy.

More formally, Table 3.3 shows results from OLS regressions with income, labor income, net wealth, assets and liabilities as dependent variables. All dependent variables are measured in 1,000 DKK to accommodate negative values. Negative income values may occur for individuals with loss-making businesses, while negative net wealth reflects liabilities exceeding assets. Columns 1 and 2 of Table 3.3 show that income is lower for the treatment group during and after disqualifications. These effects are economically significant: income during disqualification is 32,000 DKK lower (25% relative to pre-disqualification income), and it falls further after disqualification, with the treatment group earning 122,700 DKK less than the control group. Columns 3 and 4 show that the drop in income during disqualification is driven by labor income, while only a fraction of the drop in income after disqualification results from lower labor income.

Table 3.3 also shows results for net wealth, assets and liabilities. For net wealth, we find no significant difference between the treatment and control group. All coefficients are positive, but statistically insignificant. Columns 7 to 10 show that both assets and liabilities decline for the treatment group relative to the control group during and after quarantine. The difference is economically as well as statistically significant in after the quarantine. Interestingly, the reduction in assets and liabilities are of similar magnitude, resulting in an effect on net wealth close to zero. In essence, while the balance sheet of disqualified managers shrinks, it does not significantly affect their net wealth.

Overall, these results suggest that disqualified managers do not return as managers and owners of companies after the expiry of the disqualification. As a result, their income falls during and after disqualification, although the drop in income does not significantly affect their wealth. One important caveat with interpreting these results as evidence of significant personal costs associated with disqualification is that individuals might respond by switching their (illegal) activities towards family members or business associates to avoid being penalized in the future. We therefore proceed by examining the effect of disqualifications on outcomes for family members and the business network related to the disqualified individual.

[Figure 3.1 about here.]

[Table 3.2 about here.]

3.5.2 Effect on disqualified managers' family members.

To examine the effects on family members of the disqualified mangers, we use data from the Danish social security system to construct family trees. Specifically, we identify spouses, children, parents and siblings of the disqualified managers to evaluate whether differential effects emanate from family members associated with individuals in the treatment and control group.

Figure 3.3 shows the fraction of family members that are active as managers and owners. We focus on spouses and children, because parents and siblings are rarely active as managers and owners. For spouses, around disqualifications, we observe a large and sudden increase in the fraction that are managers while we see a decline in the fraction that are managers for spouses of matched managers in the control group. The fraction increases from below 1 percent before the quarantine to 7.5 percent during the quarantine. Interestingly, only less than a quarter of the increase disappears in the period after the quarantine, suggesting a longer-term shift in the business formation patterns among spouses of disqualified managers. At the same time, for children of disqualified managers, we only observe a gradual increase in their participation as managers, relative to the children of matched mangers who went bankrupt but were not disqualified.

In contrast, the bottom panel of Figure 3.3 shows a smaller increase in the fraction of owners that are spouses. The fraction of owners for spouses of disqualified managers increases from 20 percent to 29 percent at the end and after the expiry of the disqualification. At the same time, we do not see any changes in the fraction of owners among spouses of the control group. For children, we see a gradual decline in the fraction that are owners prior to the disqualification while this fraction rises temporarily above 15 percent during quarantine period and stabilizes to slightly below 15 percent after the expiry of quarantine.

More formally, Table 3.4 presents these results in a regression framework. Consistent with the figures, we find that the spouses of disqualified managers are more likely to be a business owner and a board member. Coefficients are generally positive, statistically significant, and economically meaningful. When we separate the effects, we find that spouses are more likely to be owners both during and after the expiry of the disqualification period while they are only more likely to be board members during the quarantine period. Further, we also do not find a significant difference in the propensity to be a manager between the treatment and control group spouses. Notably, we do not find any such patterns among children of the disqualified managers relative to the children of individuals in the control group.

Overall, these results suggest that disqualified managers respond by switching their activities towards family members, especially spouses but not children, to avoid being penalized in the future.

3.5.3 Effect on disqualified managers' business network.

Lastly, to gauge the effect of disqualifications on the managers' business network, we identify individuals that have served as managers, owners, or board members at the same firm as the disqualified manager. We then follow the business networks' positions as managers and owners to understand whether they become more active when managers in their network are disqualified. Figure 3.4 shows the result of this exercise. Before bankruptcy quarantines, we observe that the individuals in the business network of disqualified managers have an increasing tendency to be managers of firms: One year before the quarantine more than half of the business network is active as managers. However, after quarantine, the fraction declines for both groups,

albeit by more for the network of treatment than for the network of control groups. For ownership, we observe an increasing fraction of owners among bankrupt managers that are not disqualified, while there is no increase for the business network of disqualified managers. Thus, there appears to be no support for the concern that the business network becomes more active when one of the individuals in the network is disqualified from being a manager.

Table 3.5 presents the result for a formal test of whether the business network of quarantined individuals become more active during and after the quarantine. We note that the business network generally becomes less active during the quarantine compared to the business network of the control group. The fraction of the business network that are managers and board members decline by 8 and 7 percent, respectively. Both differences are statistically significant at the 10 percent level. After the quarantine, we find that the business network is 10 percent less likely to serve as board members. Collectively these results suggest that the business network of disqualified managers does not become more active – if anything they become less active – relative to the business network of the control group.

[Figure 3.4 about here.]

[Table 3.5 about here.]

3.6   New business formation and business activities

3.6.1   New business formation

We examine the effect of disqualifications on the managers' future business formation. Disqualification orders do not prevent individuals from founding new companies, allowing them to potentially remain involved in business activities. Figure 3.5 shows the fraction of managers founding new limited liability companies. The figure shows similar pre-bankruptcy business formation rates for both groups (four years preceding the bankruptcy). However, after bankruptcy, business formation drops more significantly for the treatment group. Notably, no manager in the treatment group forms a new company two years after the disqualification period begins. This decline aligns with Table 3.2, which shows a lower likelihood of company ownership among disqualified individuals despite legal permission to own companies.

We test this descriptive result more formally in a regression. Table 3.6 reports the result from a linear probability model where the dependent variable is an indicator for founding a new limited liability company. We find that after bankruptcy, the treatment group is 5.8 percent less likely to form a new company. We note that both during and after disqualification, the treatment group is less likely to found by 6.0 and 4.9 percentage points, respectively: the economic magnitude is significant given that pre-bankruptcy level of business formation is about 15%. However, Appendix Figure 3.A.5 and Table 3.A.1 suggest that family members of disqualified individuals become more likely to form new businesses after the disqualification period expires. This increase in family member business formation is consistent with the possibility of them acting as strawman managers or owners, as indicated in Table 3.4. The combined evidence suggests that disqualifications discourage new business formation by the disqualified individuals themselves, but this effect might be mitigated by the involvement of family members.

[Figure 3.5 about here.]

[Table 3.6 about here.]

3.6.2   Future bankruptcies and criminal activities

We next examine the effect of disqualifications on the managers' future business misconduct. Given that disqualification orders are imposed when managers use bankruptcy protection to commit misconduct, we

expect that disqualified individuals would have a higher likelihood to refile for bankruptcies or commit similar crimes again if not disqualified.

Figure 3.6 shows the fraction of managers filing for bankruptcy (top panel) and committing a fraud (bottom panel). In the top panel, we see that the treatment group has a higher fraction of managers who file for bankruptcy than the control group when combining 5 years preceding the bankruptcy (event years -4 and 0). This difference suggests a higher tendency for multiple bankruptcies by the year of quarantine. However, after a disqualification, the filing rate drops significantly and remains below pre-quarantine levels. This reduced likelihood aligns with the results from Table 3.2, where disqualified individuals are less likely to return to managerial roles. Similarly, the bottom panel shows a substantially higher pre-quarantine fraud rate for the treatment group. Following disqualification, the fraud rate drops and converges with the control group.

We test this descriptive result more formally in a regression. Table 3.7 reports the result from a linear probability model where the dependent variables are indicators for filing for bankruptcy and committing a crime (whether any crime, a white-collar crime, or a fraud). We find that after bankruptcy, the treatment group is 19 percent less likely to refile for bankruptcy and 7-10 percent less likely to commit a crime (depending on type of crimes). Even after disqualification expires, the effect persists, with a reduction of 11 percent in refiling, and 9-14 percent in crimes (depending on type of crimes) compared to the control group. The effect is statistically and economically significant. For instance, the relative reduction in fraud after quarantine is about 48 percent, when comparing to pre-quarantine levels. These findings suggest that disqualifications effectively reduce recidivism among managers with a higher propensity to commit business misconduct.

[Figure 3.6 about here.]

[Table 3.7 about here.]

3.7   Effects on the managerial labor pool

If managerial disqualifications are costly for individuals, then one could expect individuals with criminal intentions to anticipate these costs and avoid having formal managerial responsibilities. Thus, one potential response to the introduction of disqualification is to appoint "straw men" i.e. managers or owners who are being controlled by the actual owner or controller of the company. In this section, we formally test whether that are changes to managerial labor pool that are suggestive of "straw men" appointments.

One obvious way of evading the consequences of managerial disqualification is to appoint individuals for whom the cost of being disqualified from managing a business in Denmark is negligible. If the cost of disqualifications is related to the labor market as shown in Figure 3.2 and Table 3.3, then the personal cost of disqualifications is lower for individuals that are less exposed to the labor market in Denmark. We capture variation in individual characteristics that are plausibly related to the exposure to the labor market, by identifying the fraction of foreigners and individuals receiving public (welfare) transfers in the managerial labor pool. The main idea is foreigners and individuals on public transfers are less reliant on labor market outcomes in Denmark, and therefore have lower personal costs of being disqualified. Thus, to measure the effect of bankruptcy quarantines, we examine whether the managerial labor pool experience a shift towards foreigners and individuals receiving public welfare. The top panel in Figure 3.7 show the fraction of non-Danish citizens and individuals of non-Danish origin. For both measures me observe a modest increase year on year from 2009 to 2013, followed by a larger increase year on year after the introduction of bankruptcy quarantines. Thus, the growth in the fraction of foreigners in the managerial labor pool seems to accelerate after the reform. The bottom panel shows that the fraction of managers receiving public transfers declined from 5 to 4 percent before the reform, while it increased to between 5 and 6 percent after the introduction of

the bankruptcy quarantines. Thus, the managerial labor pool seems to shift towards individuals for whom the cost of disqualifications might be lower.

Table 3.8 presents results from a regression of managerial characteristics in the labor pool from a specification with regional fixed effects and industry fixed effects. The dependent variables are indicators taking the value one for foreign citizens, immigrants or individuals receiving public transfers. The variable of interest is the post-reform indicator which tests how the labor pool changes after the introduction of bankruptcy quarantines. Across the six specifications, we note significant changes to the managerial labor pool. The fraction of managers that are foreign citizens increase, the fraction of managers of foreign origin, and the fraction of managers receiving public transfers increase. The increase is statistically as well as economically significant. The fraction foreigners increase by 1.3 percentage points relative to an average level of 6.4 percent in Column 2. The fraction of immigrants increases by 2.7 percentage points relative to an average level of 6.6 percent, and the fraction of managers receiving public transfers increase by 0.6 percentage point relative to an average level of 4.6 percent. Thus, overall results in Table 3.8 suggest that the labor pool shifts towards individuals with lower personal costs of being disqualified.

If disqualifications discourage gross business neglect, one would expect the reform to have significant effects on the composition of the managerial labor pool with respect to managers with past bankruptcy experiences and criminal records. Evidence from Section IV and V suggests that disqualified individuals are less likely to be managers or owners after the disqualification, less likely to go bankrupt in the future, and less likely to be involved in criminal activities. If the reform is effective in targeting individuals that commit gross business negligence, one would also expect a positive long-term effect on the managerial labor pool. To gauge this effect, we examine whether the fraction of managers with a past bankruptcy and the fraction of managers with a criminal record changes after the reform. Figure 3.8 plots the fraction of managers with a bankruptcy in the past five years (top panel), and the fraction of managers with a criminal conviction in the past five years (bottom panel). For both panels, we note interesting changes. Before 2014, the top panel of Figure 3.8 shows a strong positive time trend in the fraction of managers with a past bankruptcy. The fraction increases from 4 percent in 2009 to 7 percent in 2013. Following the introduction of bankruptcy quarantines there is a decline in the fraction of managers with a past bankruptcy. Thus, the reform seems to slow the tendency of managers to be involved in bankruptcies. The bottom panel of Figure 3.8 shows a similar effect for the fraction of managers with a criminal record. We report the fraction of managers with any criminal record, fraction of managers convicted of white-collar crime and the fraction of managers convicted of fraud. For all three measures we note that the fraction of the labor pool with criminal records increases prior to the reform, and tends to decrease after the introduction of the reform. Importantly, the decline in the fraction of managers with a criminal record is not immediate, suggesting that it takes time for the effect to kick in. Interestingly, the timing of the decline seems to coincide with the increase in the number of disqualifications as shown in Figure 3.8.

[Figure 3.7 about here.]

[Figure 3.8 about here.]

[Table 3.8 about here.]

3.8   Concluding remarks

Bankruptcy law attempts to strike a balance between providing downside protection for failing entrepreneurs, while limiting the losses for creditors and the public. A major, but understudied, concern with the provision of downside protection is if individuals use the protection of limited liability and bankruptcy law to commit irresponsible business conduct or outright fraud.

In this study we use evidence from the introduction of managerial disqualifications in Denmark to provide evidence on the working of disqualifications for managerial misconduct in corporate bankruptcy. We find that individuals that are disqualified are less likely to be managers and owners of firms with limited liability – even after the expiry of the disqualification. The lower level of involvement in businesses manifests itself in a lower likelihood of founding a business, a lower likelihood of future bankruptcies and a lower likelihood of future criminal activities. Collectively, these results suggest that the managerial disqualifications discourage individuals from becoming repeat offenders.

Our results also speak to possible side effects of managerial disqualifications. We find that family members, spouses and children, of disqualified individuals become more active as managers and owners during and after the disqualification period. This tokenism might be indicative of straw men appointments by which disqualified individuals attempt to evade the consequences of managerial misconduct. In spirit of straw men appointments, we find that an increasing number of managers with low personal costs of disqualifications enter the managerial labor pool. In particular, we find an increased number of foreign managers and managers that receive public welfare after the introduction of the reform.

Overall, out study provides the first empirical study of the workings of managerial disqualifications, providing evidence of interest to market participants and policy makers. To this end, our results highlight that disqualifications on one side might discourage repeat offenders, our results also suggest that criminal syndicates might appoint straw men to avoid being personally exposed to the personal costs of disqualifications.

Table 3.1: Descriptive statistics

This table summarizes the observable characteristics of the treatment and control groups two years prior to the bankruptcy ruling. Panel A presents personal income and wealth, Panel B details past criminal activities, Panel C shows the number of managerial positions held, and Panel D reports active management and ownership positions. All amounts are in year 2000 1,000 DKK (1 euro equals 7.45 DKK). For every variable, we compute the difference in average characteristics between the treatment and control groups and test whether this difference is statistically different from zero. Standard deviations are in parentheses, and t-statistics are in brackets. ***, **, and * denote significance at the one, five and ten percent level.

	Bankrupt and quarantined (1)		Bankrupt but not quarantined (2)		Difference (2)-(1)	
A. Income and wealth (1,000 DKK)						
Total income	128.2	(168.9)	134.3	(119.8)	6.2	[0.3]
Labor income	141.8	(120.8)	136.4	(125.9)	-5.4	[-0.3]
Net wealth	-152.9	(620.4)	-396.2	(2,257.4)	-243.2	[-0.8]
Assets	779.6	(1,194.3)	575.6	(1,354.5)	-203.9	[-1.1]
Liabilities	932.5	(943.4)	971.8	(2,609.8)	39.3	[0.1]
B. Crimes within past 5 years (%)						
Any crime	37.9	(48.9)	22.6	(41.9)	-15.4*	[-2.6]
White-collar crime	22.4	(42.1)	11.2	(31.5)	-11.2*	[-2.4]
Fraud	19.0	(39.5)	7.0	(25.6)	-11.9**	[-3.1]
C. Number of managerial positions						
In limited liability companies	1.5	(1.2)	1.5	(1.7)	0.0	[0.1]
In unlimited liability companies	0.4	(0.5)	0.4	(0.5)	-0.0	[-0.5]
D. Management, ownership, and board membership (%)						
Manager in limited liability companies	87.9	(32.9)	78.9	(40.9)	-9.0	[-1.6]
Owner-manager in unlimited liability companies	39.7	(49.3)	33.3	(47.2)	-6.4	[-1.0]
Owner in limited liability companies	65.5	(47.9)	63.3	(48.2)	-2.2	[-0.3]
Board membership in limited liability companies	5.2	(22.3)	12.4	(33.0)	7.2	[1.6]
Observations	58		412			

Table 3.2: Effect of disqualifications on position as manager, owners and board member

This table reports the estimated effect of managerial disqualifications on the likelihood of being a manager, an owner or a board member. The dependent variables are indicators for being a manager, an owner, or a board member, respectively. *Treated* is an indicator for being a disqualified manager. *Post* is an indicator for years after an individual is disqualified from being a manager. *During* and *After* are indicators equal to one during the managerial disqualification and after managerial disqualification, respectively. The specification includes individual fixed effects and year fixed effects. The unit of observation is individual year. ***, **, and * denote significance at the one, five and ten percent level.

Dependent variable	Manager		Owner		Board member	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.07		-0.20***		0.02	
	(0.07)		(0.05)		(0.02)	
Treated × During		-0.06		-0.21***		0.02
		(0.08)		(0.06)		(0.02)
Treated × After		-0.11*		-0.15**		0.03
		(0.06)		(0.06)		(0.02)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.78	0.78	0.78	0.78	0.77	0.77
N	13,054	13,054	13,054	13,054	13,054	13,054

Table 3.3: Personal costs of managerial disqualifications

This table estimates the personal costs of managerial disqualifications. In Columns 1 and 2 the dependent variable is total income. In Column 3 and 4 the dependent variable is labor income. In Column 5 the dependent variables are net wealth (assets – liabilities), while it is assets in Columns 7 and 8, and liabilities in Columns 9 and 10, respectively. *Treated* is an indicator for being a disqualified manager. *Post* is an indicator for years after an individual is disqualified from being a manager. *During* and *After* are indicators equal to one during the managerial disqualification and after managerial disqualification, respectively. All amounts are measured in year 2000 1,000 DKK. One euro equals 7.45 DKK. The specification includes individual fixed effects and year fixed effects. The unit of observation is individual year. ***, **, and * denote significance at the one, five and ten percent level.

Dependent variable	Total income		Labor income		Net wealth		Assets		Liabilities	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated × Post	-49.79***		-33.16**		41.69		-61.46		-103.15	
	(17.59)		(15.82)		(57.23)		(49.96)		(62.81)	
Treated × During		-32.08*		-34.07**		50.54		-46.98		-97.52
		(13.56)		(15.73)		(57.47)		(47.46)		(62.82)
Treated × After		-122.69**		-29.41		5.23		-121.08*		-126.31*
		(50.58)		(18.52)		(67.69)		(68.87)		(73.38)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.65	0.66	0.81	0.81	0.91	0.91	0.90	0.90	0.92	0.92
N	12,840	12,840	12,840	12,840	12,840	12,840	12,840	12,840	12,840	12,840

Table 3.4: Effect of managerial disqualifications on family members

This table reports the estimated effect of managerial disqualifications on the likelihood of a bankrupt managers' family member being a manager, an owner or a board member. In panel A the sample consists of spouses of individuals that are involved as a manager in a bankruptcy, whereas the sample in panel B consists of children of individuals that are involved as a manager in a bankruptcy. The dependent variables are indicators for being a manager, an owner, or a board member, respectively. *Treated* is an indicator for being a disqualified manager. *Post* is an indicator for years after an individual is disqualified from being a manager. *During* and *After* are indicators equal to one during the managerial disqualification and after managerial disqualification, respectively. The specification includes individual fixed effects and year fixed effects. The unit of observation is individual year. ***, **, and * denote significance at the one, five and ten percent level.

(a) Panel A. Spouse

Dependent variable	Manager		Owner		Board member	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.07**		0.09		0.03**	
	(0.03)		(0.06)		(0.01)	
Treated × During		0.07**		0.08		0.03**
		(0.03)		(0.06)		(0.01)
Treated × After		0.05*		0.15*		0.02
		(0.03)		(0.09)		(0.02)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.74	0.76	0.76	0.76	0.91	0.91
N	7,771	7,771	7,771	7,771	7,771	7,771

(b) Panel B. Children

Dependent variable	Manager		Owner		Board member	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.07		-0.04		0.00	
	(0.06)		(0.06)		(0.01)	
Treated × During		0.06		-0.04		-0.00
		(0.06)		(0.06)		(0.01)
Treated × After		0.11		-0.03		0.00
		(0.07)		(0.07)		(0.02)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.78	0.78	0.73	0.73	0.78	0.78
N	6,974	6,974	6,974	6,974	6,974	6,974

Table 3.5: Effect of managerial disqualifications on professional network

This table reports the estimated effect of managerial disqualifications on the likelihood of a business network member of being a manager, an owner or a board member. The sample consists of business network members of individuals that are involved in a bankruptcy. Business networks are defined as individuals that overlapped with the individuals involved in the bankruptcy either as founders, managers, owners, or board members of the same firm. The dependent variables are indicators for being a manager or an owner, respectively. *Treated* is an indicator for being a disqualified manager. *Post* is an indicator for years after an individual is disqualified from being a manager. *During* and *After* are indicators equal to one during the managerial disqualification and after managerial disqualification, respectively. The specification includes individual fixed effects and year fixed effects. The unit of observation is individual year. ***, **, and * denote significance at the one, five and ten percent level.

Dependent variable	Manager		Owner		Board member	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.06		0.00		-0.08*	
	(0.04)		(0.04)		(0.04)	
Treated × During		-0.08*		-0.01		-0.07*
		(0.04)		(0.04)		(0.04)
Treated × After		0.01		0.06		-0.10**
		(0.06)		(0.05)		(0.05)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.82	0.82	0.82	0.82	0.83	0.83
N	24,324	24,324	24,324	24,324	24,324	24,324

Table 3.6: New business formation around disqualifications

This table estimates the effect of bankruptcy quarantines on the likelihood of forming a new business. The dependent variable is an indicator for founding a new limited liability company. *Treated* is an indicator for being a disqualified manager. *Post* is an indicator for years after an individual is disqualified from being a manager. *During* and *After* are indicators equal to one during the managerial disqualification and after managerial disqualification, respectively. The specification includes individual fixed effects and year fixed effects. The unit of observation is individual year. ***, **, and * denote significance at the one, five and ten percent level.

Dependent variable	Founding a new company	
	(1)	(2)
Treated × Post	-0.058**	
	(0.029)	
Treated × During		-0.060**
		(0.029)
Treated × After		-0.049
		(0.030)
Individual fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
R^2	0.60	0.60
N	12,944	12,944

Table 3.7: Effect of managerial disqualifications on criminal activity

This table estimates the effect of managerial disqualifications on future criminal charges and convictions. In column 1 and 2 the dependent variable is an indicator for filing for bankruptcy. In columns 3 to 6 the dependent variable is an indicator for being convicted of a crime. Columns 3 and 4 includes any crime, whereas columns 5 and 6 focuses on white-collar crime and columns 7 and 8 on fraud, respectively. White-collar crime is defined by using the FBI criminal code definitions. *Treated* is an indicator for being a disqualified manager. *Post* is an indicator for years after an individual is disqualified from being a manager. *During* and *After* are indicators equal to one during the managerial disqualification and after managerial disqualification, respectively. The specification includes individual fixed effects and year fixed effects. The unit of observation is individual year. ***, **, and * denote significance at the one, five and ten percent level.

Dependent variable	Filing for bankruptcy		Convicted of crime					
			Any crime		White-collar crime		Fraud	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post	-0.191***		-0.099***		-0.069**		-0.082***	
	(0.025)		(0.035)		(0.032)		(0.029)	
Treated × During		-0.210***		-0.089***		-0.062*		-0.080***
		(0.028)		(0.035)		(0.033)		(0.029)
Treated × After		-0.113***		-0.138***		-0.097***		-0.091***
		(0.019)		(0.037)		(0.033)		(0.033)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.68	0.68	0.66	0.65	0.65	0.65	0.67	0.67
N	12,936	12,936	13,054	13,054	13,054	13,054	13,054	13,054

Table 3.8: Effects on the managerial labor pool

This tables shows the changes to the composition of the managerial labor pool. In column 1 and 2 the dependent variable is an indicator for foreign citizens. In columns 3 and 4 the dependent variable is an indicator for immigrants, while the dependent variable in columns 5 and 6 is an indicator for individuals who receive public transfers. *Post reform* is an indicator for years after the introduction of managerial disqualifications in 2014. The sample include active managers, and the unit of observation is individual year.

Dependent variable	Foreign citizen		Foreign origin		Receiving public transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.063***	0.064***	0.065***	0.066***	0.047***	0.046***
	(0.008)	(0.006)	(0.009)	(0.003)	(0.003)	(0.001)
Post reform	0.014***	0.013***	0.028***	0.027***	0.005***	0.006***
	(0.004)	(0.004)	(0.006)	(0.005)	(0.002)	(0.002)
Regional fixed effects	No	Yes	No	Yes	No	Yes
Industry fixed effects	No	Yes	No	Yes	No	Yes
R^2	0.00	0.06	0.00	0.07	0.00	0.01
N	4,101,455	4,101,455	4,101,455	4,101,455	3,994,627	3,994,627

Figure 3.1: Positions as managers and owners around disqualifications

This figure shows the fraction of individuals that are active managers or owners in a window from five years before to five years after going bankrupt. The top (bottom) figure show on whether individuals are managers (owners) in limited liability companies. The dashed line shows the fraction that are managers for individuals that are disqualified from being a manager for up to 3 years. The solid gray line shows the fraction that are active among individuals that are not disqualified from being a manager.
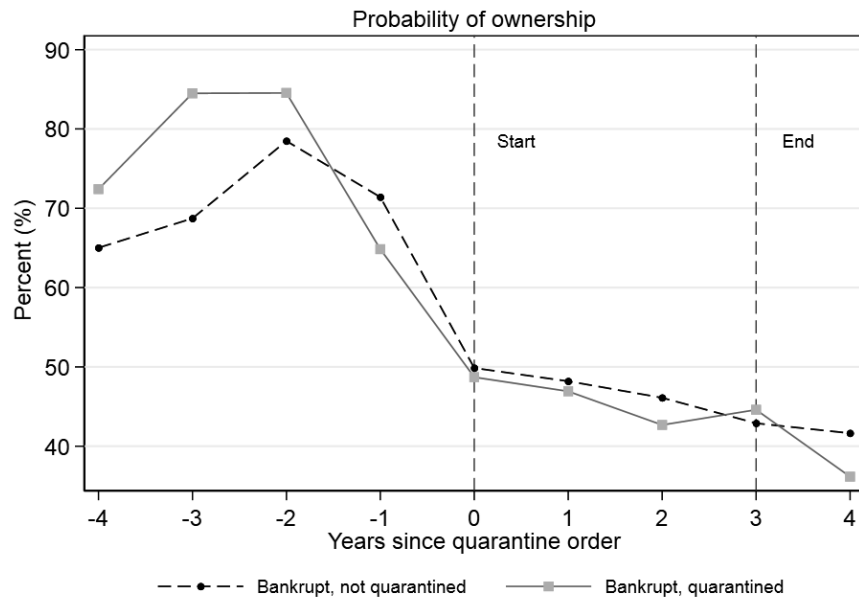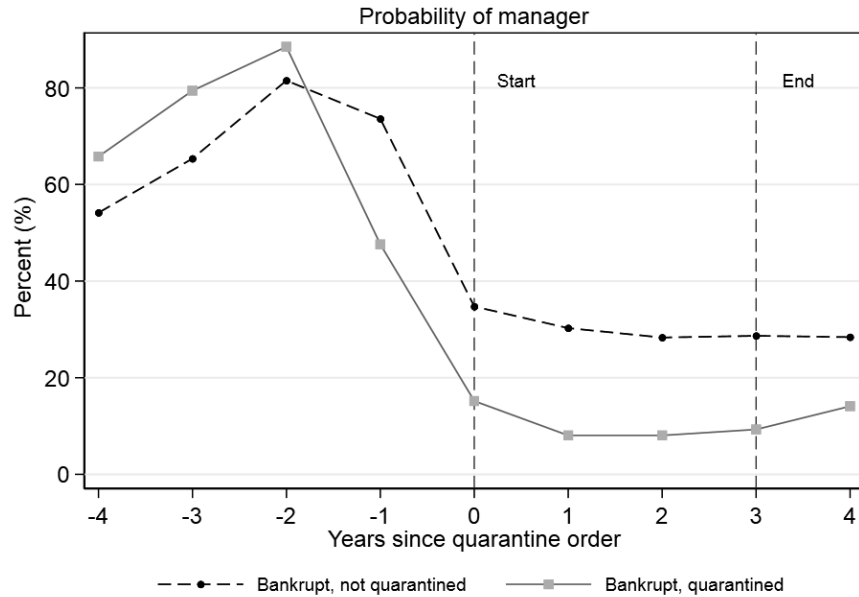
Figure 3.2: Personal income and wealth around disqualifications

This figure shows average personal income in a window from five years before to five years after going bankrupt. In the top (bottom) panel, the figure reports average income (net wealth) for bankrupt managers. The dashed line shows the average for individuals that are disqualified from being a manager for up to 3 years. The solid gray line shows the average for individuals that are not disqualified from being a manager. All amounts are in year 2000 1,000 DKK. One euro equals 7.45 DKK.
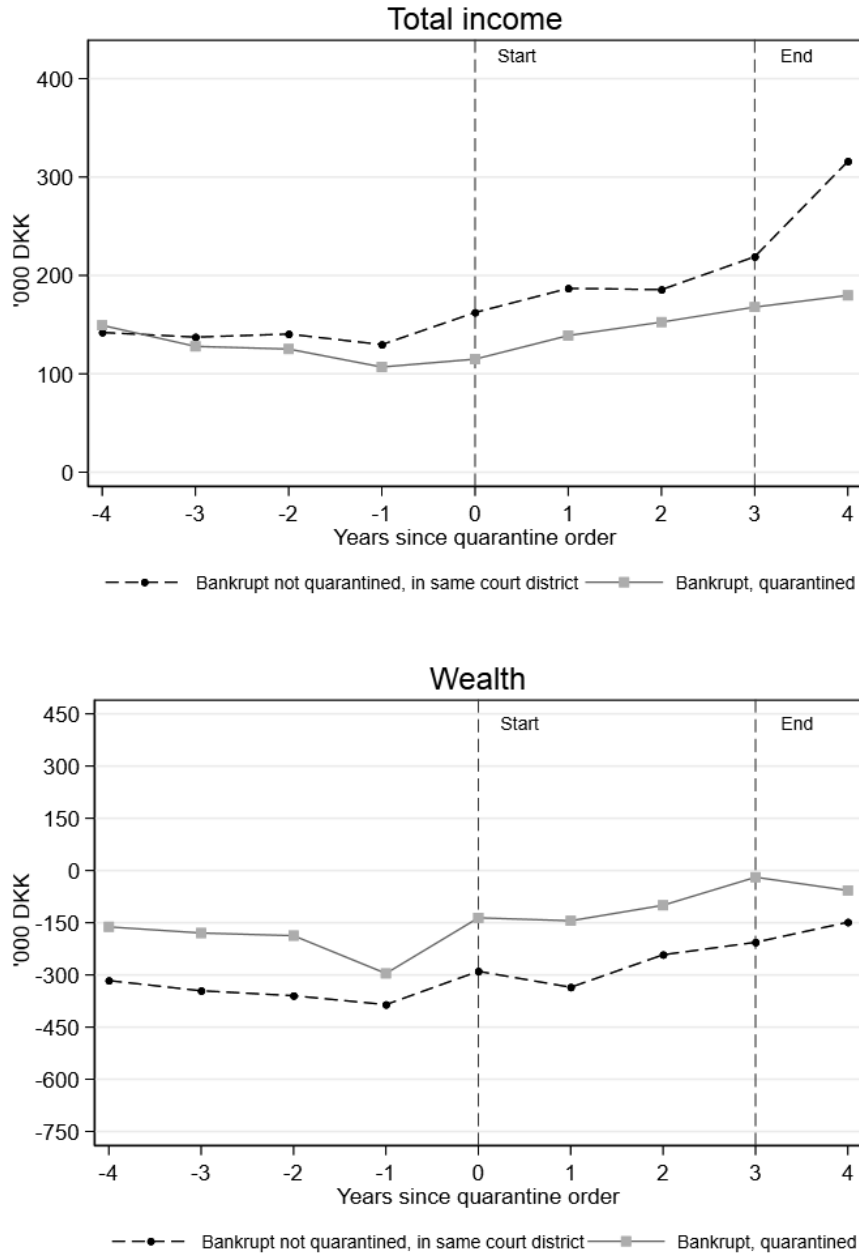
Figure 3.3: Family members' position as managers and owners around disqualifications

This figure shows whether family members of individuals involved in bankruptcies are active as managers or owners in a window from five years before to five years after going bankrupt. The top (bottom) panels show the fraction of managers (owners) for spouses and children of the individuals involved in the bankruptcy. The dashed line shows the average for spouses and children of individuals that are disqualified from being a manager for up to 3 years. The solid gray line shows the average for spouses and children of individuals that are not disqualified from being a manager.

(a) Spouse

(b) Children

(c) Spouse

(d) Children
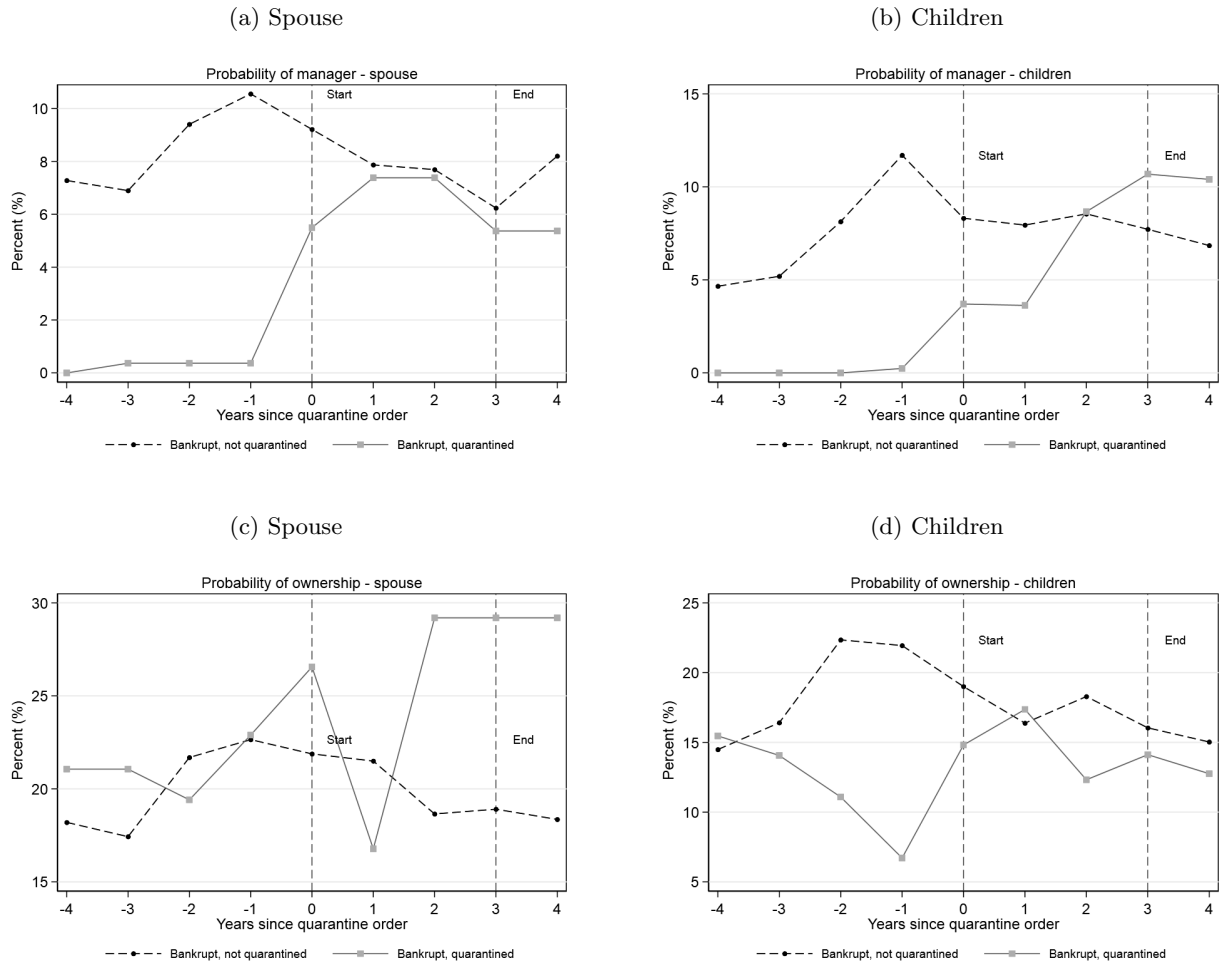
Figure 3.4: Business networks' positions as manager and owner around disqualifications

This figure shows whether business networks of individuals involved in bankruptcies are active as managers in a window from five years before to five years after going bankrupt. Business networks are defined as individuals that overlapped with the individuals involved in the bankruptcy either as founders, managers, owners, or board members of the same firm. The top (bottom) panel shows the fraction of managers (owners) for the business network of individuals involved in the bankruptcy. The dashed line shows the average for the business network of individuals that are disqualified from being a manager for up to 3 years. The solid gray line shows the average for the business network of individuals that are not disqualified from being a manager.
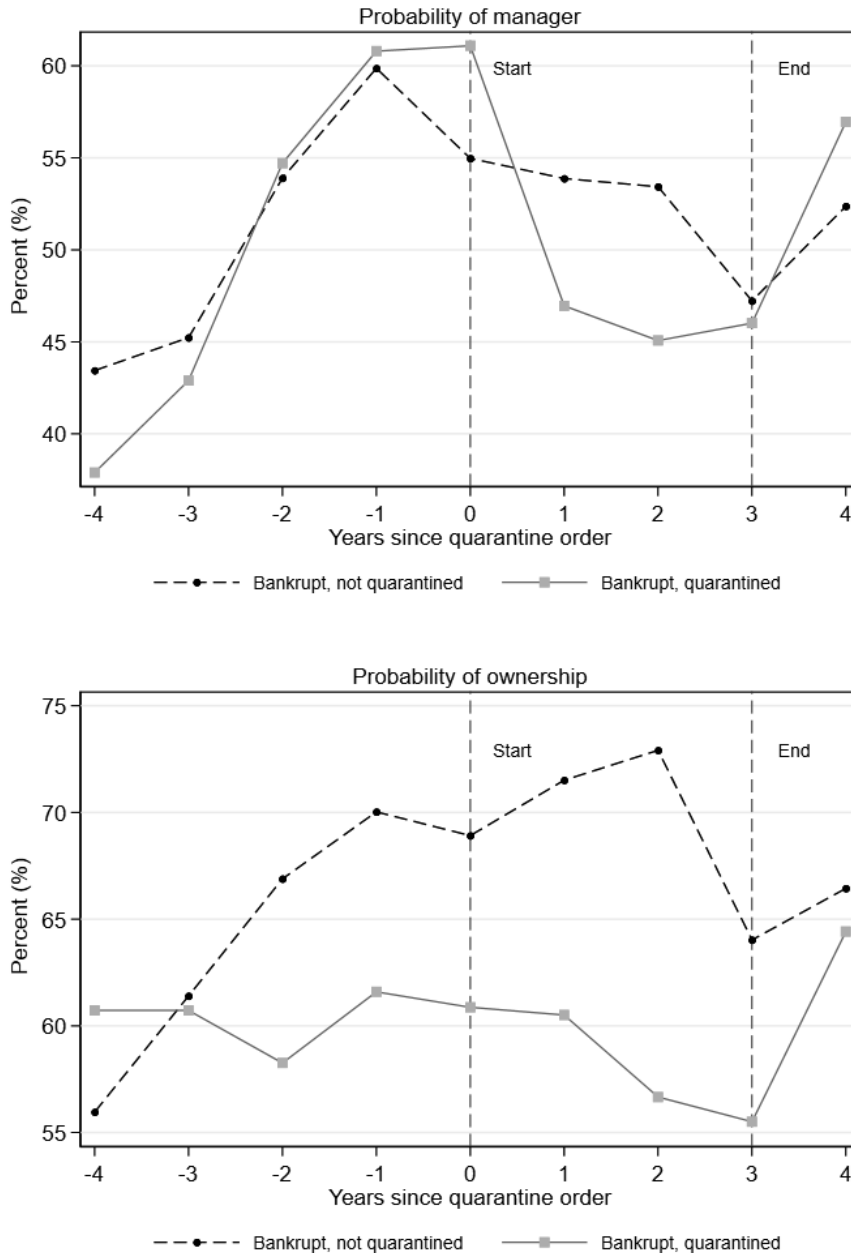
Figure 3.5: New business formation around disqualifications

This figure shows the likelihood of founding a new limited liability company in a window from five years before to five years after going bankrupt. The dashed line shows the average for individuals that are disqualified from being a manager for up to 3 years. The solid gray line shows the average for individuals that are not disqualified from being a manager.

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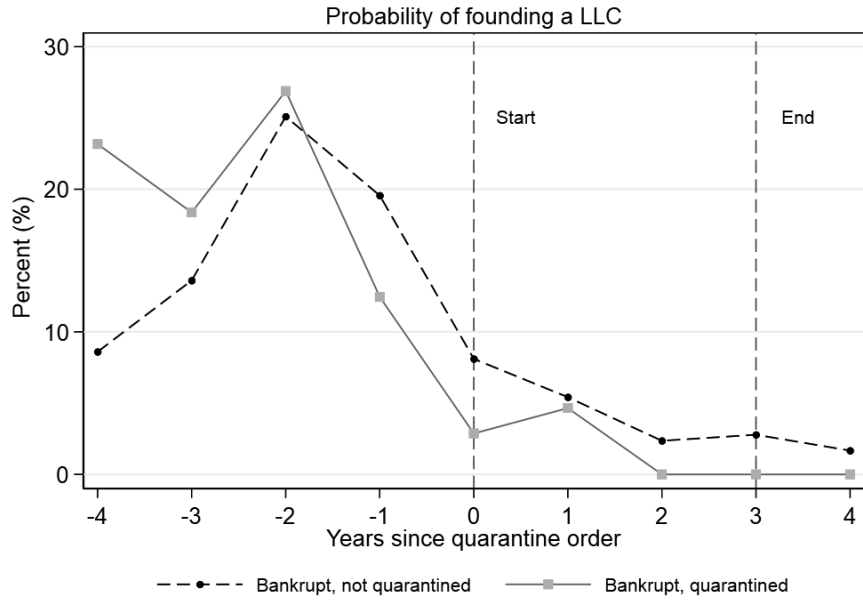
Figure 3.6: Bankruptcy filings and criminal activity around disqualifications

This figure shows the incidence of bankruptcy filings (top panel) and criminal convictions (bottom panel) in a window from five year before to five years after bankruptcy. The dashed line shows the fraction for individuals that are disqualified from being a manager for up to 3 years. The solid gray line shows the fraction for individuals that are not disqualified from being a manager.
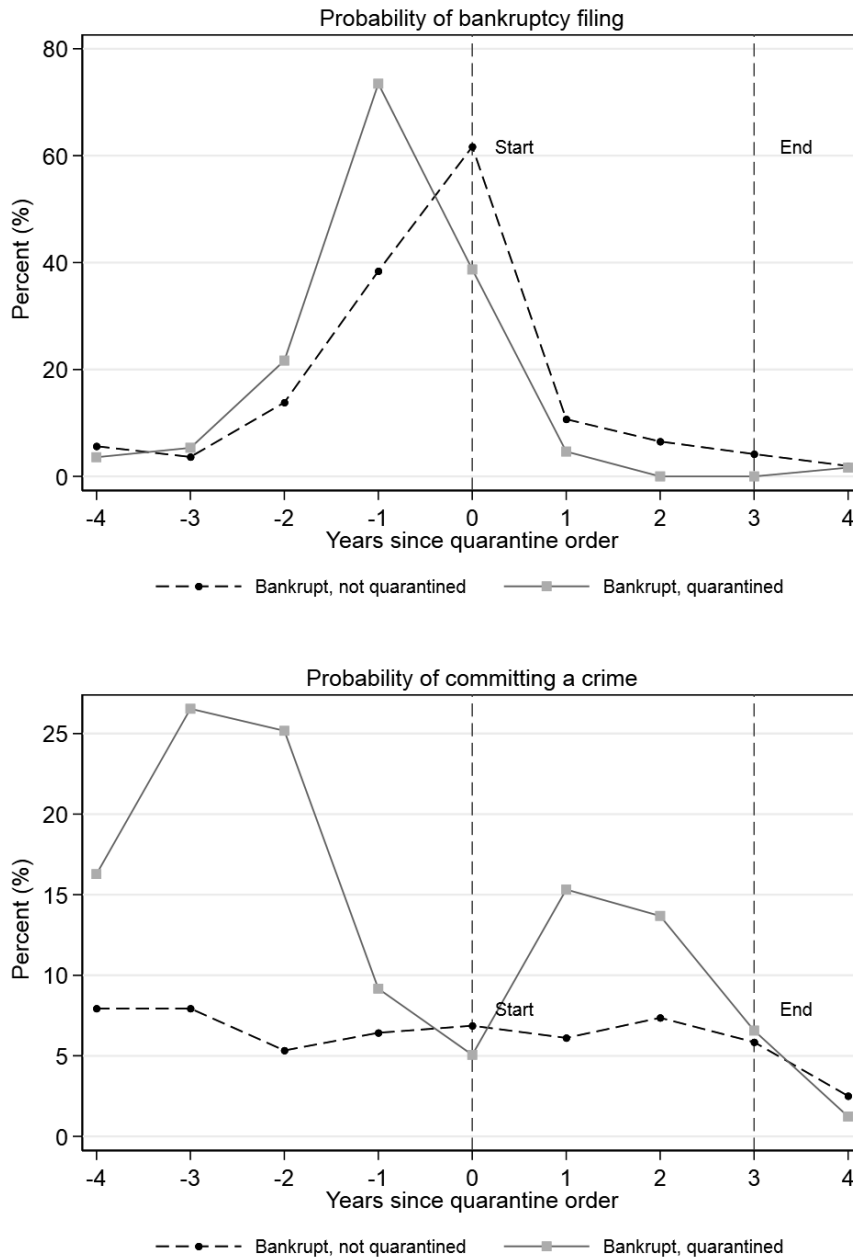
Figure 3.7: Effect on the managerial labor pool

This figure reports the fraction of the managerial labor pool that are plausible strawmen appointments. The top panel reports the fraction of active managers of limited liability companies that are foreigners (i.e. non-Danish citizen, non-Danish origin or not fully tax liable). The bottom panel reports the fraction of active managers that are living off public transfers (i.e. social welfare). The introduction of managerial disqualifications in 2014 is marked by the dotted line.

Foreigners in the managerial labor pool

Receiving public transfers
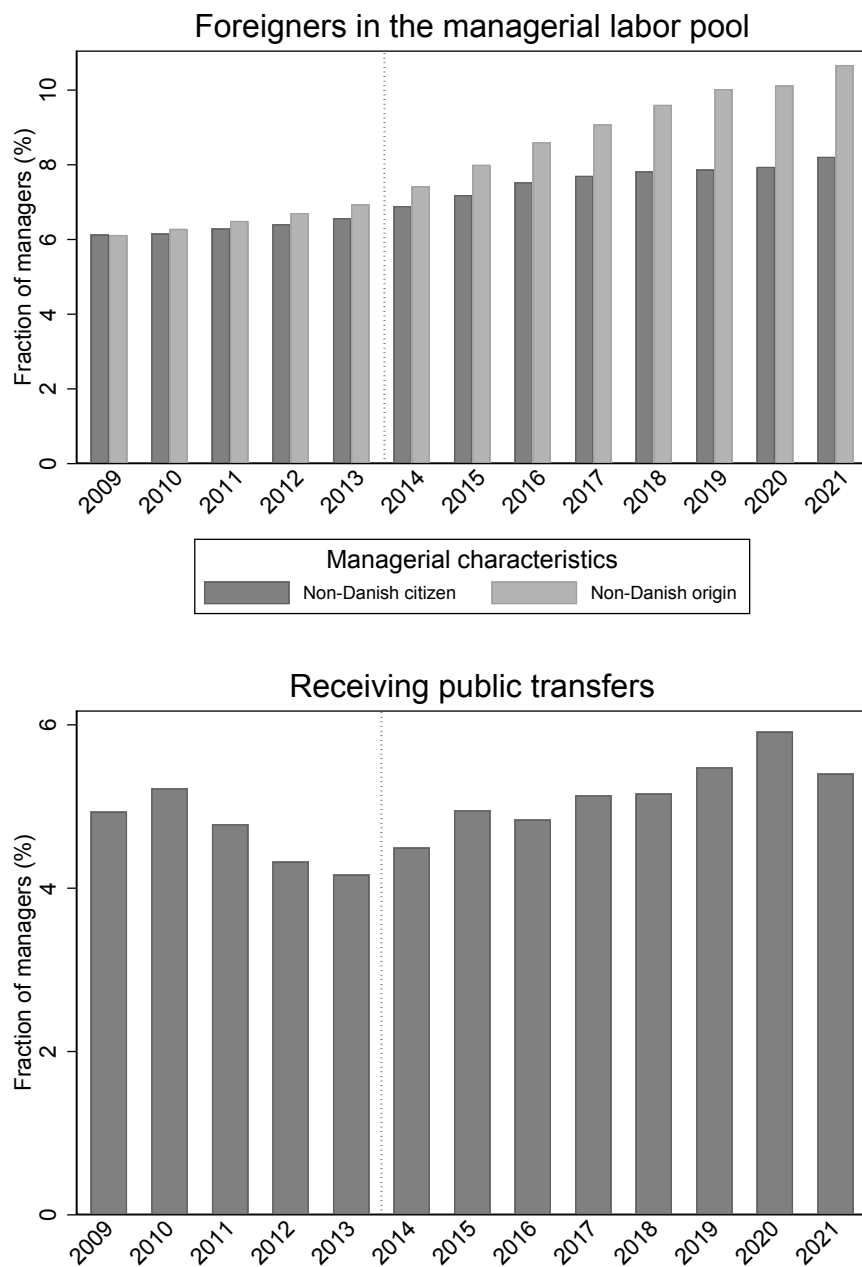
Figure 3.8: Effect on the managerial labor pool

This figure reports the fraction of the managerial labor pool that have a bankruptcy filing or a criminal conviction. The top panel reports the fraction of active managers of that have filed for bankruptcy in the pr 5 years. The bottom panel reports the fraction of active managers have a criminal conviction in the prior 5 years. The introduction of managerial disqualifications in 2014 is marked by the dotted line.

Bankrupt in past 5 years

Criminals in the managerial labor pool

Appendix

3.A   Appendix

[Table 3.A.1 about here.]

[Figure 3.A.1 about here.]

[Figure 3.A.2 about here.]

[Figure 3.A.3 about here.]

[Figure 3.A.4 about here.]

[Figure 3.A.5 about here.]

Table 3.A.1: New business formation by family members around disqualifications

This table reports the estimated effect of bankruptcy quarantines on the likelihood of a family member of forming a new business. The sample consists of family members of individuals that are involved in a bankruptcy. The dependent variable is an indicator for founding a new limited liability company. *Treated* is an indicator for being a disqualified manager. *Post* is an indicator for years after an individual is disqualified from being a manager. *During* and *After* are indicators equal to one during the managerial disqualification and after managerial disqualification, respectively. The unit of observation is individual year. The specification includes individual fixed effects and year fixed effects. ***, **, and * denote significance at the one, five and ten percent level.

Dependent variable	Founding a new company	
	(1)	(2)
Treated × Post	0.018	
	(0.012)	
Treated × During		0.007
		(0.009)
Treated × After		0.062**
		(0.030)
Individual fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
R^2	0.75	0.75
N	19,679	19,679

Figure 3.A.1: Excerpt from a newspaper article listing disqualified managers

This figure is an excerpt from a newspaper article listing disqualified managers in Southern Jutland during the first three quarters of 2017. The geographic coverage corresponds to the following three bankruptcy courts: *Skifteretten i Esbjerg*, *Skifteretten i Kolding*, and *Skifteretten i Sønderborg*. To preserve anonymity, personally identifiable information (names of individuals and companies) has been blurred. The original list includes individual names, associated bankrupt companies, and disqualification periods (*de konkursramte*, *konkursbo*, and *så mange år må den konkursramte ikke drive virksomhed*).

Figure 3.A.2: Bankruptcy court districts in Denmark

This map illustrates the 24 bankruptcy court districts in Denmark. The six districts where newspaper articles published disqualification lists (Horsens, Kolding, Esbjerg, Sønderborg, Odense, and Svendborg) are shaded with diagonal lines.
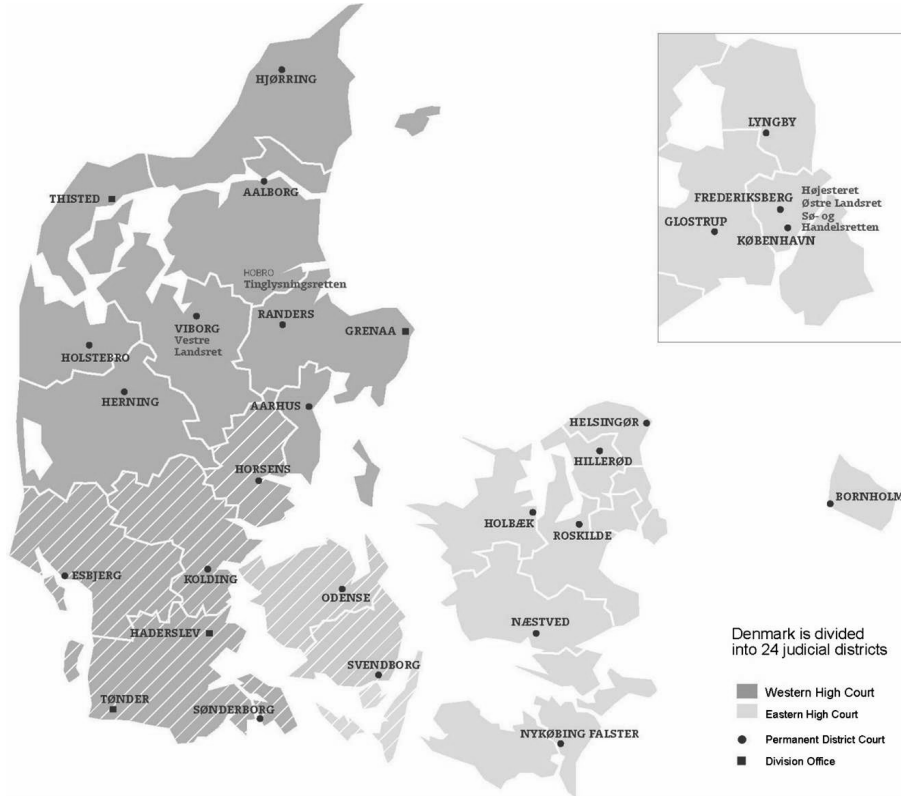
Figure 3.A.3: Number of bankruptcies and disqualifications, 2009–2021

This figure reports the number of bankrupt companies, bankrupt managers, and misconduct cases brought to the bankruptcy court in each year. Misconduct cases are bankruptcy filings where the liquidator recommends a quarantine to the bankruptcy court. The introduction of managerial disqualifications in 2014 is marked by the dashed line.
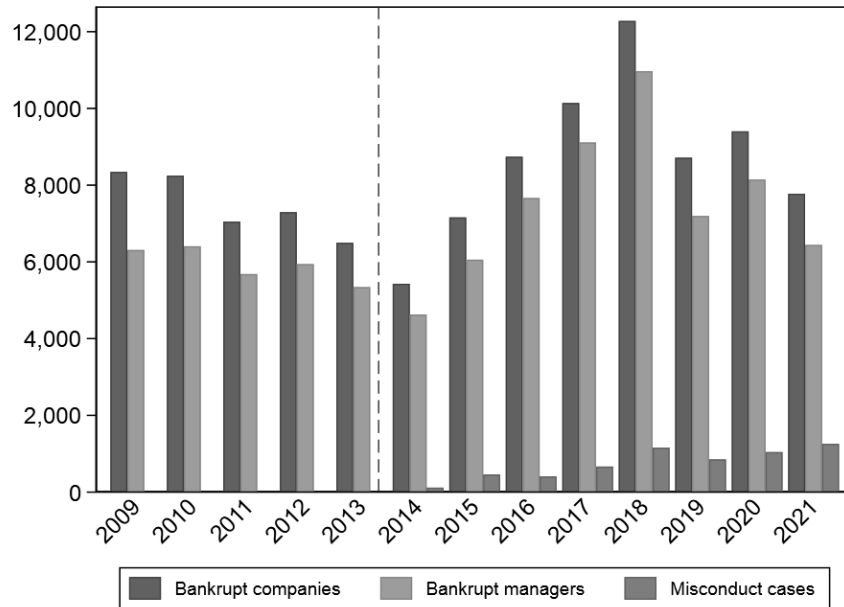
Figure 3.A.4: Length of disqualification period

This figure plots the distribution of the length of disqualification period in our sample. The horizontal axis shows the disqualification period in years, and the vertical axis indicates the percentage of individuals disqualified for that period.
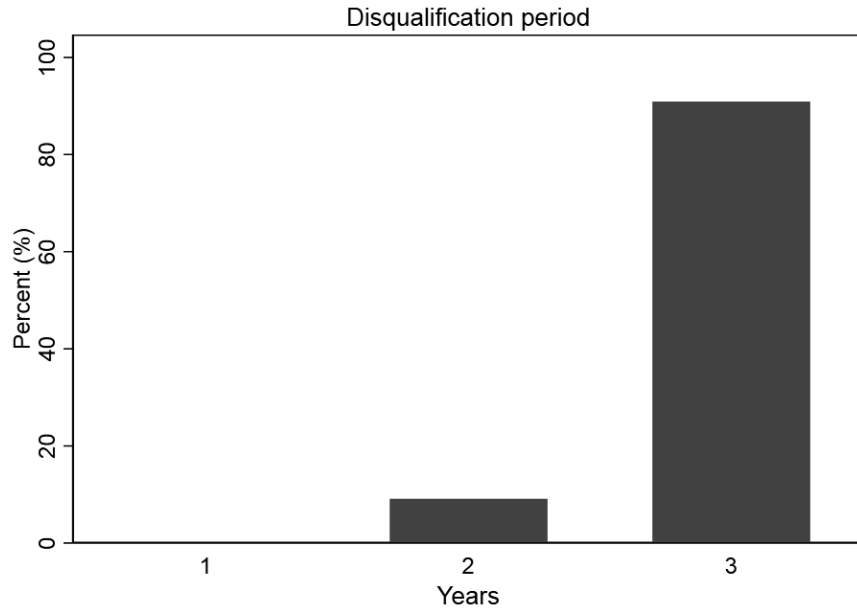
Figure 3.A.5: New business formation by family members around disqualifications

This figure shows whether any of the family member of individuals involved in bankruptcies founding a new limited liability company in a window from five years before to five years after going bankrupt. Families are defined as a spouse and children of the individuals involved in bankruptcy. The dashed line shows the average for family members of individuals that are disqualified from being a manager for up to 3 years. The solid gray line shows the average for family members of individuals that are not disqualified from being a manager.

Probability of founding a LLC -- family
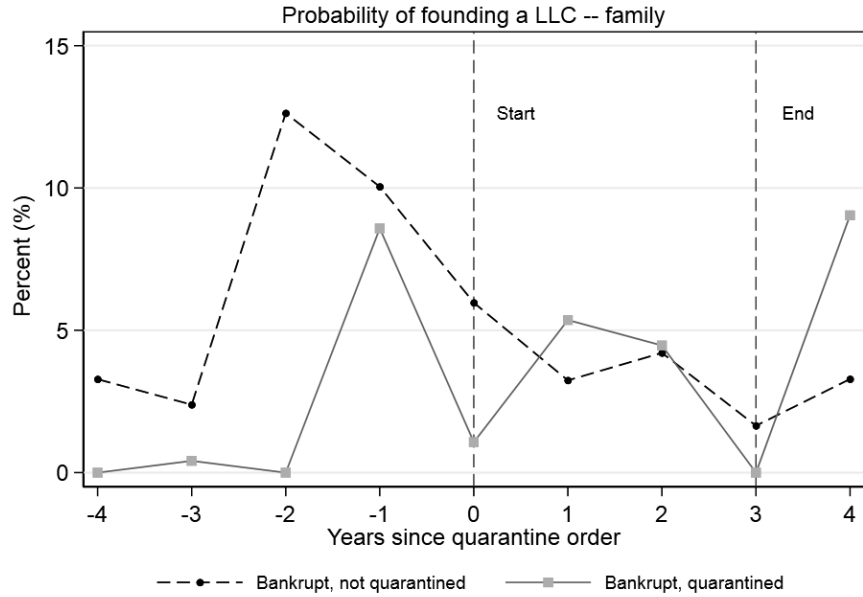
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