Post-Bankruptcy Wealth and Serial Entrepreneurship

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Abstract

I examine whether greater personal wealth helps failed entrepreneurs return to business, and which of them it helps. Exploiting inheritances received after personal bankruptcy in Denmark as plausibly exogenous variation in post-bankruptcy wealth, I find that, on average, the entrepreneurial response to these windfalls is positive but imprecisely estimated. The response is stronger among entrepreneurs who did not experience negative entrepreneurial income before bankruptcy and those with a smaller share of personal debt arising from business activity. This pattern is consistent with the experience of severe personal financial losses weakening the response to greater wealth.

Keywords: personal bankruptcy, entrepreneurship, second-chance policy

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1 Introduction

Entrepreneurs rely on their personal balance sheets to finance their businesses (e.g., Robb and Robinson 2014; Adelino et al. 2015; Fonseca and Wang 2026). Because entrepreneurs often bear personal liability for business debts, business failure can lead to personal bankruptcy (White 2016; Kalda et al. 2026).[1] Although debt discharge in personal bankruptcy improves entrepreneurs' balance sheets, they may still have little net wealth and face restricted access to credit for several years (Bos et al. 2018; Dobbie et al. 2020; Cahn et al. 2021; Herkenhoff et al. 2021). Their own wealth may therefore be particularly important for financing a restart. Helping failed entrepreneurs return to business is one rationale for bankruptcy provisions that discharge debt and protect wealth from creditors.[2] In this study, I ask whether an increase in net wealth after bankruptcy leads failed entrepreneurs to start another business, and which of them respond.

Financial constraints and the experience of failure may jointly shape this response. If failed entrepreneurs remain financially constrained, greater net wealth may enable them to finance a new business. Consistent with the financial constraints channel, Cahn et al. (2021) and Herkenhoff et al. (2021) find that removing public information about past bankruptcy improves access to financing and increases business creation. However, the personal financial losses associated with business failure may reduce an entrepreneur's willingness to take risks again, even when her finances improve. More broadly, prior studies document that negative personal experiences, such as corporate bankruptcy or personal investment losses, can reduce subsequent risk-taking (Malmendier et al. 2011; Dittmar and Duchin 2016; Koudijs and Voth 2016; Schoar and Zuo 2017; Andersen et al. 2019). If severe personal losses discourage starting another business even when financing becomes

[1] Using the 2007 U.S. Consumer Bankruptcy Project, Lawless (2012) reports that about 14% of personal bankruptcy filers were self-employed at filing or during the preceding two years.

[2] For example, the European Union's Directive on Restructuring and Insolvency of 2019 aims to ensure that "over-indebted entrepreneurs can benefit from a full discharge of debt ..., thereby allowing them a second chance" (Directive (EU) 2019/1023).

available, the restart response to greater wealth may be weaker among entrepreneurs who experienced those losses. I therefore examine both the average restart response and how it varies with the losses that failure imposed.

Estimating these responses from changes in bankruptcy law, which governs how much wealth debtors keep, is difficult: reforms can alter not only post-bankruptcy wealth but also credit supply and the composition of debtors who file for bankruptcy (Gropp et al. 1997; Berkowitz and White 2004; Berger et al. 2011; Gross et al. 2021). I therefore exploit inheritances that failed entrepreneurs receive after personal bankruptcy in Denmark as plausibly exogenous variation in post-bankruptcy wealth. Two institutional features support this design. First, debtors must disclose anticipated inheritances at filing, and courts reject applications for debt relief when such a windfall is expected. This screening helps limit the scope for timing bankruptcy around an anticipated inheritance. Consistent with this screening, I find no bunching of inheritance events immediately after the bankruptcy ruling. Second, inheritances received after the ruling belong to the debtor and leave the repayment terms unchanged. The design therefore estimates the restart response to an increase in net wealth among entrepreneurs who have already been granted debt relief.

I combine Danish administrative data on personal bankruptcy, wealth, family links, and business ownership to trace entrepreneurial activity around inheritance. I compare entrepreneurs who receive inheritances after their bankruptcy ruling (the treated group) with otherwise similar bankrupt entrepreneurs who do not receive inheritances (the control group). The groups are matched on bankruptcy year, demographic characteristics, pre-bankruptcy net wealth, and the share of debt discharged, with exact matching on three measures of personal losses associated with business failure. In this matched sample, I use a stacked difference-in-differences design that compares each recipient only with her matched controls, before and after the calendar year of parental death. Identification relies on the assumption that, absent inheritance, recipients and their matched controls would have experienced similar changes in entrepreneurial activity. Event stud-

ies centered on inheritance show no statistically detectable difference in entrepreneurial activity between the two groups in the preceding years.

I first document that inheritance raises recipients' net wealth by about 157,000 Danish kroner (DKK), or approximately 21,000 euros, relative to matched controls.[3] The estimated average entrepreneurial response to inheritance is positive but imprecise. The point estimate is an increase of 3.1 percentage points in the annual probability of being an entrepreneur. For comparison, the average annual entrepreneurship rate across both the treated and control groups is 16.7% from the bankruptcy year through six years afterward. The 95% confidence interval ranges from about –2 to +8 percentage points, encompassing both zero and economically meaningful increases. I also find no clear evidence that the response increases with the size of the inheritance, with no statistically detectable difference between recipients of above- and below-median inheritances.

The response to inheritance does, however, vary with the personal losses associated with prior failure. I measure the severity of these losses in three ways: (i) experiencing negative entrepreneurial income before bankruptcy, (ii) having a large share of personal debt arise from business activity, and (iii) being fully rather than partially personally liable for business debts. The estimated increase in the annual probability of being an entrepreneur is 7.5 percentage points for those without negative entrepreneurial income, and the response is about 10.8 percentage points lower for those with negative entrepreneurial income. Similarly, the estimated increase is 8.6 percentage points for those with a smaller business-related share of personal debt, and the response is about 12.6 percentage points lower for those with a larger share. Both differences in responses are statistically significant. The main estimates also show a stronger response among entrepreneurs with partial rather than full personal liability, although this difference is less precisely estimated and more sensitive to specification. A weaker response could also arise if entrepreneurs with more severe losses remained more financially constrained after in-

[3] Monetary amounts are expressed in 2015 DKK (1 Euro $\approx$ DKK 7.45).

heritance. I find no robust evidence that larger inheritances narrow the differences in responses.[4] Overall, these findings are consistent with the experience of severe personal losses weakening the response to greater wealth.

A potential concern about the differences in entrepreneurial responses is that the loss measures also capture persistent differences in entrepreneurial ability or industry composition. To address this concern, I account for persistently low past performance and the industry of the failed business; the differences in entrepreneurial responses remain. A broader concern for interpreting the inheritance estimates is that they may also reflect caregiving before parental death or bereavement afterward. In particular, bereavement may reduce labor supply and offset a positive entrepreneurial response to inherited wealth. Employment and labor supply show no statistically detectable changes before death and decline persistently afterward, while household outcomes remain broadly stable. The absence of changes before death provides little support for anticipatory caregiving, and the persistence of the decline is more consistent with a wealth effect on labor supply than with transitory bereavement.

Finally, the economic benefits of starting another business depend in part on whether it leads to lasting activity and income rather than to entry followed by exit. I therefore compare whether recipients and matched controls restart within three and five years of inheritance, whether they are operating a business at each horizon, and their cumulative income during years of entrepreneurship. To capture restarts rather than businesses already operating when the inheritance arrives, the comparison excludes recipients and controls who were operating a business in the year before inheritance. It includes those who never restart, so it does not condition on the decision to restart. The estimates are imprecise: none is statistically distinguishable from zero, and they provide no clear evidence that inheritance increases entry followed by exit or lowers longer-run business

[4]For each of the three measures, I also compare the net wealth of entrepreneurs with more and less severe personal losses. I find no evidence that those with more severe losses are financially worse off before inheritance, and no statistically significant difference between the two groups in the increase in net wealth following inheritance.

activity or income.

This study contributes to the literature on bankruptcy and entrepreneurship. To my knowledge, I provide the first empirical evidence on the effect of an increase in post-bankruptcy wealth on *serial* entrepreneurship. Theoretical studies posit that protecting entrepreneurs' wealth from creditors in personal bankruptcy could foster overall entrepreneurship (Landier 2005; Ayotte 2007; Jia 2015; Mankart and Rodano 2015). Empirical studies exploit either variation in ex-ante wealth protection, which is generally associated with more entrepreneurship (Fan and White 2003; Armour and Cumming 2008; Cerqueiro et al. 2019), or the removal of public information about past bankruptcy or delinquency, with mixed effects on entrepreneurship (Bos et al. 2018; Dobbie et al. 2020; Cahn et al. 2021; Herkenhoff et al. 2021).[5] In comparison to these studies, I examine an increase in wealth that arrives *after* bankruptcy, among entrepreneurs who have already failed.[6]

My study also contributes to the broader literature on entrepreneurship and on experience-based risk-taking. A large body of research documents the positive effect of windfall wealth on entrepreneurship (e.g., Holtz-Eakin et al. 1994; Andersen and Nielsen 2012; Adelino et al. 2015; Schmalz et al. 2017; Bellon et al. 2021; Cespedes et al. 2021; Bermejo et al. 2024). Compared with these studies, I study entrepreneurs who have already failed and find that their response to windfall wealth varies with the personal losses that failure imposed. This pattern is consistent with evidence that negative personal experiences deter subsequent 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).

[5] 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.

[6] 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, investments, bankruptcy filing, and post-bankruptcy survival (Berkowitz and White 2004; Agarwal et al. 2005; Berger et al. 2011; Cerqueiro et al. 2017; Cerqueiro and Penas 2017; Chen et al. 2020; Cole et al. 2024; Schoenherr and Starmans 2022; Hotchkiss et al. 2026). 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.

My study has implications for policies that aim to provide a second chance to failed entrepreneurs by increasing their post-bankruptcy wealth.[7] The estimated entrepreneurial response to greater wealth is stronger among entrepreneurs whose failure imposed less severe personal losses. This suggests that a policy's effect on restarts may depend on the personal losses experienced by the entrepreneurs it reaches. The potential increase in restarts must be weighed against the effects of wealth protection on credit supply: prior research shows that greater wealth protection may restrict access to credit for other entrepreneurs, discouraging business entry and growth (Berkowitz and White 2004; Berger et al. 2011; Fossen 2014).[8] My estimates inform the restart side of this trade-off separately from its credit-supply side.

The study proceeds as follows. Section 2 describes the Danish institutional setting. Section 3 introduces the data, sample construction, and empirical strategy. Section 4 presents the effects of inheritance on post-bankruptcy wealth, serial entrepreneurship, and longer-run entrepreneurial outcomes. Section 5 reports robustness checks, and Section 6 concludes.

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.

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 fil-

[7] The European Commission discusses both financing difficulties and the personal consequences of bankruptcy when motivating support for entrepreneurs seeking to restart (European Commission 2007).

[8] 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; Chakrabarti and Pattison 2019; Gross et al. 2021; Severino et al. 2026).

ing options: liquidation for private individuals (*personlig konkurs*), reorganization (*rekonstruktion*), and bankruptcy (*gældssanering*).[9] I briefly describe two proceedings that are empirically relevant for individual debtors: liquidation and bankruptcy.[10]

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. 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.[11] 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. I provide more details on bankruptcy proceedings in Appendix IA.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

[9] 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.

[10] 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.

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

debt chapter," respectively).[12] The two chapters follow similar legal procedures to discharge debt and require liquidation of all assets, as previously described. The two types of bankruptcy have two 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, 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 restart 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 severely restrict access to new loans or credit (Kreiner et al. 2020). Treated and control entrepreneurs are flagged alike and, because I match exactly on the bankruptcy year and chapter, face the same statutory repayment period. The comparison between them therefore does not reflect initial differences in access to credit. Importantly, neither bankruptcy flags nor bankruptcy itself restrict an individual's ability to own a business.[14]

[12] 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).

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

[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 managing a limited liability company for a three-year period but does not prohibit managing an unlimited liability company.

2.2 Windfall wealth from inheritances after bankruptcy

To estimate the effect of post-bankruptcy wealth on serial 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.

Two institutional features make inheritances received after bankruptcy a source of plausibly exogenous variation in post-bankruptcy wealth. First, courts screen for anticipated inheritances at filing. Danish case law requires debtors to disclose any prospective inheritance, and the court rejects applications in which an inheritance is anticipated, because the expected windfall is deemed to improve the debtor's financial situation (Hindborg 2017, p. 59, and Petersen and Ørgaard 2022, p. 125). Concealing such a prospect is treated as fraudulent and can later result in cancellation of the bankruptcy order (Hindborg 2017, pp. 213–215, and Hansen and Petersen 2022, p. 337). The screening limits the scope for timing a filing around an anticipated inheritance, so the timing of an inheritance is plausibly 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 be-

[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

long solely to the debtor, as creditors have no claim to them. Together, these two features imply that a post-bankruptcy inheritance improves the debtor's balance sheet while the repayment terms set by the court, and the legal environment in which she restarts, remain unchanged. In Sections 3.4 and 4.1, I provide empirical evidence consistent with both features.

3 Data and empirical strategy

3.1 Data

I construct a panel of entrepreneurs who go personally bankrupt by linking four data sources through the unique individual identifier (CPR) assigned to every Danish citizen. First, I obtain bankruptcy rulings from the State Gazette of Denmark (*Statstidende*), a government gazette that publicly announces statutory notices on court proceedings. Parsing the notices yields approximately 20,000 individuals who receive bankruptcy rulings between 2006 and 2017.[17] Second, I use administrative registers from Statistics Denmark, which cover the entire population of Denmark annually and contain information on income, wealth, education, labor supply, family (parents, spouse, and children), and parental death. Information on income, wealth, and employment status is directly sourced from official records from the Danish Tax and Customs Administration. I extract data from the registers for the years 1980 to 2023 and use them to identify inheritance events and their magnitudes, following the methodology of Andersen and Nielsen (2012). Third, the Central Business Register (CVR: *Centrale Virksomhedsregister*) contains firm-level information on all companies in Denmark from 1990 to 2023, including incorporation status (either unlimited or limited liability company), industry (NACE codes), founders, managers, and

Petersen 2022, pp. 127–129).

[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. Bruze et al. (2024) use the same source to study the long-run effect of personal bankruptcy in Denmark (*gældssanering*), hand-collecting announcements from microfilm through 2005; my sample begins with the digital edition in 2006.

owners. Fourth, I supplement the CVR data on ownership of limited liability companies with the Experian dataset, which assembles ownership information from companies' annual reports between 2000 and 2019. I adjust variables denominated in Danish Kroner (DKK) to the 2015 price level and winsorize them at the 2.5 and 97.5 percentiles for each event year. Appendix IA.2 describes each source in detail, and Appendix IA.3 the procedure for identifying inheritances.

3.2 Definition of entrepreneurs

I define individuals as entrepreneurs if they are owner-managers: either (i) owners of an unlimited liability company (hereafter ULC), or (ii) owners of a limited liability company (hereafter LLC) who also serve as a manager of that company. For ULCs the two roles are inseparable by construction; for LLCs the additional manager condition restricts the definition to entrepreneurs actively managing their businesses rather than passive financial investors.

First, to determine ULC entrepreneurship, I extract information from the matched employer-employee panel dataset supplied by Statistics Denmark.[18] 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.[19]

Second, to determine LLC entrepreneurship, I combine datasets from the CVR and Experian. The combined dataset provides a list of managers, 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

[18] 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.

[19] 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.

LLC at year-end.[20] I classify individuals as LLC entrepreneurs who are managers and, simultaneously, legal or beneficial owners of LLCs.

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 were either a ULC or LLC entrepreneur within the seven years leading up to their bankruptcy. Using these criteria, I identify 6,210 entrepreneurs who go bankrupt, with 3,454 and 2,756 from the ordinary and business debt chapters, respectively.

In the second step, I apply four sample restrictions. First, I remove 60 individuals whose rulings are subsequently cancelled (due to failures like noncompliance with the repayment plan). Second, I exclude 363 individuals who inherited within the seven years preceding their bankruptcy. This exclusion keeps the expected post-bankruptcy inheritance comparable across the remaining bankrupt entrepreneurs, since pre-inheritors have already received some or all of their potential inheritance and would otherwise differ systematically from the rest of the sample. Third, I exclude 4 individuals who receive multiple inheritance events after bankruptcy. Such cases typically arise from divorced parents, where the bankrupt entrepreneur can inherit separately from each biological parent (and any stepparent), and their inheritance patterns differ from those in the typical two-parent household. Fourth, for individuals with multiple bankruptcy rulings, I

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

only consider the first ruling.[21]

In the third step, I retain entrepreneurs aged between 18 and 60 at the time of the ruling, thereby excluding 708 older entrepreneurs who might simply retire from the labor market after bankruptcy. After this step in sample selection, I am left with 5,075 unique failed entrepreneurs who go bankrupt between 2006 and 2017.

Matching entrepreneurs who receive inheritances to those who do not To control for the propensity to restart a business absent windfall wealth, I match bankrupt entrepreneurs who receive inheritances after their ruling (the "treated" group) with those of similar characteristics who do not (the "control" group).

To begin, I identify the treated group from inheritance events.[22] I date each inheritance to the calendar year of parental death, which I refer to as the *inheritance year*. 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 235 entrepreneurs with inheritance events. None of these treated entrepreneurs inherits an operating business from a parent, which alleviates the concern that an inherited business, rather than windfall wealth, drives the decision to restart after bankruptcy.

For each bankrupt entrepreneur in the treated group, I look for up to five entrepreneurs in the control group, and I match with replacement. The matching takes the following steps:

1. I require that the entrepreneur in the control group has the same year of bankruptcy and gender and is of a similar age ($\pm$5 years) and has a similar debt discharge ratio ($\pm$10 percentage points) as the treated entrepreneur.
2. I also require that the control entrepreneur match the treated entrepreneur exactly on

[21] 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.

[22] I provide detailed procedures on identifying inheritances in Appendix IA.3.

the three measures of personal losses associated with business failure: the bankruptcy chapter (ordinary or business debt), whether the entrepreneur experienced negative entrepreneurial income before bankruptcy, and whether she had been a ULC entrepreneur before bankruptcy.[23]

3. Among potential matches, I select up to five nearest neighbors by minimizing the Mahalanobis distance to the treated entrepreneur with respect to three variables: pre-bankruptcy wealth at one, two, and three years before bankruptcy. I further refine the accuracy of matching by excluding matched pairs whose pre-bankruptcy wealth at one year before bankruptcy differs by more than 500,000 DKK in absolute value.

After matching, my main matched sample consists of 185 unique individuals in the treated group and 589 in the control group. I observe them from seven years before bankruptcy through six years after. Each treated entrepreneur and her matched controls together form a *cohort*, which serves as the building block of the stacked difference-in-differences design described in Section 3.5. The main findings are qualitatively similar in an alternative sample that is additionally matched on expected inheritance measured in the bankruptcy year (Section 5 and Appendix Tables IA.10–IA.11).

3.4 Summary statistics

Table 1 reports the characteristics of all bankrupt entrepreneurs and the main matched sample (the treated and control groups), measured at the year of bankruptcy. Panel A shows that bankrupt entrepreneurs have, on average, large negative net wealth at one year before bankruptcy, which prompts them to seek a debt discharge.[24] Panel B reports that the treated group is 3.8 years older than the full sample, as expected, since an entrepreneur enters the treated group only when her last living parent dies. Panel C shows

[23] I define these measures in Section 4.3.

[24] The mean is less negative in the treated group than in the full sample, reflecting a small number of extremely negative outliers in the full sample; the medians are closer, at -560,000 DKK in the full sample and -470,000 DKK in the treated group. Compliant with Statistics Denmark's data policy, the median represents the average of five values around the median. For readability, I refer to them as the median throughout.

that bankrupt entrepreneurs, on average, discharge more than 90% of total unsecured debt. Panel D reports that about 84% of bankrupt entrepreneurs bore full personal liability from being ULC entrepreneurs. The remaining 16% filed for personal bankruptcy despite having managed only LLCs, which suggests that, like ULC entrepreneurs, they were 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., Robb and Robinson 2014; White 2016; Fonseca and Wang 2026; Kalda et al. 2026).[25] In the last column, I examine the difference between the treated and matched control groups. With the exception of age and the discharge ratio (both economically small in magnitude), none of the differences are statistically significant. Overall, this balance, particularly on characteristics not used in the matching procedure, suggests that the two groups are similar on observable entrepreneurial characteristics.

Inheritance amounts The main explanatory variable in my analysis is whether an individual receives an inheritance. To gauge whether the inheritances are economically significant for these distressed entrepreneurs, the top panel of Appendix Figure IA.3 plots the distribution of inherited wealth. The distribution shows substantial variation, and the average (median) inheritance of 232,000 DKK (108,000 DKK) is of similar magnitude to the average in Andersen and Nielsen (2012).

The bottom panel scales inherited wealth by dischargeable debt, defined as unsecured debt measured at one year before bankruptcy. Relative to the debt discharged in bankruptcy, the windfalls are likewise economically large.

To benchmark these amounts against an explicit wealth protection policy, I compare them to the homestead exemptions that protect home equity from liquidation in US personal bankruptcy. In 2015, the average (median) homestead exemption across US states

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

was about USD 95,000 (USD 72,900) (Indarte 2023). By comparison, the average (median) inheritance in my sample is about USD 39,300 (USD 18,300).[26] The inheritances are thus of the same order of magnitude as the wealth that an explicit protection policy leaves with debtors, though at its lower end, roughly two-fifths (one-quarter) of the corresponding exemption. The amounts that bankrupt entrepreneurs in my sample inherit therefore appear large enough to matter for the financing of a restart.

Inheritance timing Appendix Figure IA.4 reports the distribution of inheritance timing relative to the bankruptcy ruling in six-month bins. Inheritances do not bunch in the months immediately after the ruling: those within 12 months of bankruptcy account for 27.0% (14.6% + 12.4%) of inheritance events, compared with 31.9% (18.4% + 13.5%) in the last 12 months of the window. This pattern is consistent with the screening of anticipated inheritances at filing (Section 2.2): debtors do not appear to time their filings to precede an imminent windfall.

3.5 Empirical strategy

To estimate the effect of post-bankruptcy wealth on serial entrepreneurship, I employ a stacked difference-in-differences design in the spirit of Gormley and Matsa (2011), which avoids the well-documented biases of two-way fixed-effects estimators in staggered settings with heterogeneous treatment effects (Goodman-Bacon 2021; Baker et al. 2022).[27]

For each treated entrepreneur, I construct an event-specific dataset, which I refer to as a *cohort*, consisting of the treated entrepreneur and up to five matched controls assigned to her by the procedure described in Section 3.3. By construction, all members of cohort c share the same year of bankruptcy. I observe each individual from seven years before

[26] I convert the inheritance amounts to US dollars at the average US dollar–DKK exchange rate over my sample period, 5.9 DKK per US dollar, with annual averages ranging from 5.1 to 6.7 across 2006–2017.

[27] In my setting, treated entrepreneurs receive inheritances at different post-bankruptcy horizons, from the year of bankruptcy and until three years after. A pooled two-way fixed-effects specification would use already-inherited entrepreneurs as implicit controls for those who inherit later, contaminating the estimated treatment effect. The stacked design restricts each treated entrepreneur to be compared only with her own matched controls within a common observation window.

bankruptcy (t = –7) through six years afterward (t = +6), and stack all cohorts into a single panel.

Centering the observation window on bankruptcy follows all failed entrepreneurs over a common horizon through failure and recovery, so that the treated entrepreneur and her matched controls are compared at the same stage of the post-bankruptcy process. Within this window, treatment begins in the inheritance year, which varies across treated entrepreneurs, and the baseline specification below pools the post-inheritance response relative to matched controls. In Section 4, I complement the pooled estimates with event studies centered on the inheritance year, which assess anticipation and trace the response by time since inheritance.

For an outcome Y_{ict} of individual i in cohort c at event year t (where $t = 0$ is the year of bankruptcy), the baseline regression on the stacked panel is:

$$Y_{ict} = \beta \, \textit{After inheritance}_{ict} + \delta \, X'_{ict} + \alpha_{ic} + \kappa_{ct} + \gamma_a + \varepsilon_{ict}, \tag{1}$$

where $\textit{After inheritance}_{ict}$ is an indicator variable equal to one for the treated entrepreneur in cohort c in event years on or after her inheritance year, and zero otherwise; it is therefore zero for every control observation throughout the cohort.

The vector X'_{ict} collects time-varying individual characteristics: an indicator for being married in year t and the number of children under the age of 18 living in the household in year t.[28]

The specification includes three sets of fixed effects. First, α_{ic} is an individual-by-cohort fixed effect: it absorbs all time-invariant characteristics specific to each individual-cohort cell and, because matching is with replacement, treats a control appearing in more than one cohort as a separate unit in each cohort she enters. Second, κ_{ct} is a cohort-by-event-year fixed effect, which absorbs any shock common to all members of the same

[28] I obtain similar results when I omit these time-varying control variables, addressing the concern that they may themselves respond to the windfall. The results are reported in Appendix Table IA.4.

cohort at the same event time, including the mechanical post-bankruptcy recovery dynamics shared by individuals who file in the same year. Third, γ_a is an age-bin fixed effect, where the age bin a is defined in five-year intervals based on the individual's age in event year t; this fixed effect controls for general life-cycle variation in the outcome. With these fixed effects, the coefficient of interest, β, is identified from within-cohort, within-event-year contrasts: it captures the post-inheritance change in the outcome for the treated entrepreneur relative to her matched controls in the same cohort and event year.

To assess whether the entrepreneurial response varies with the size of the windfall, I extend Equation 1 by interacting $\textit{After inheritance}_{ict}$ with a measure of inheritance size:

$$Y_{ict} = \beta_1 \, \textit{After inheritance}_{ict} + \beta_2 \, \textit{After inheritance}_{ict} \times \textit{Size}_i + \delta \, X'_{ict} + \alpha_{ic} + \kappa_{ct} + \gamma_a + \varepsilon_{ict}, \quad (2)$$

where $\textit{Size}_i$ is one of two measures, each entering a separate regression: $\textit{Inherited wealth}_i$, the inherited amount in millions of 2015 DKK, which permits a linear relation between the inherited amount and the outcome; and $\textit{Large inheritance}_i$, an indicator equal to one for above-median inheritances, which permits a non-monotonic response. The main effect of $\textit{Size}_i$ is absorbed by the individual-by-cohort fixed effect α_{ic}. The coefficient β_2 captures how the response varies with the size of the windfall.

I estimate Equations 1 and 2 by weighted least squares, with each treated entrepreneur assigned weight one and each of her k matched controls assigned weight $1/k$, so that each cohort contributes equally to identification. Standard errors are clustered at the individual level, allowing residuals to be correlated across the cohorts and event years in which a control appears. For the cross-sectional comparisons in Tables 1 and 5, which use one observation per individual in a cohort, I instead cluster at the matched-cohort level to account for the within-cohort dependence introduced by matching (Abadie and Spiess 2022).

4 Windfall wealth and serial entrepreneurship

4.1 Inheritance as a source of post-bankruptcy wealth

Before turning to the main analysis, I assess the research design by testing (1) whether the treated and control groups follow a similar trend in net wealth before bankruptcy and (2) whether inheritance increases the net wealth (i.e., total assets minus total debt) of recipients.

Figure 1 illustrates the dynamics of average net wealth for both the treated (solid line) and control (dashed line) groups. The plot supports the validity of the research design in two ways.

First, the treated and control groups follow a similar evolution of net wealth before bankruptcy. Although the matching uses net wealth only at event years –3 to –1, the two groups track each other over the full pre-bankruptcy window from event year –7, and the pre-period differences are not jointly statistically significant ($p = 0.32$). This parallel pre-trend supports the identifying assumption that, absent the windfall, the net wealth of the two groups would have evolved similarly.

Second, while both groups experience a sharp jump in net wealth around the year of bankruptcy due to debt discharge,[29] the treated group exhibits a higher level of net wealth than the control group throughout the post-bankruptcy period. The persistent difference in net wealth (jointly statistically significant at the 1% level for event years 0 through 6) suggests that inheritance increases the net wealth of recipients.

Figure 1 is thus consistent with the second institutional feature in Section 2.2: inheritances received after the ruling belong to the debtor rather than to creditors. To quantify the increase in net wealth, Table 2 reports estimates from the stacked specification in Equation 1 on the treated and control groups observed from seven years before until six

[29] Net wealth remains negative on average even after the bankruptcy ruling because Danish personal bankruptcy discharges unsecured debt while leaving secured obligations, primarily mortgages, on the debtor's balance sheet.

years after bankruptcy. Columns 1–3 report separate regressions with net wealth, total debt, and total assets as outcomes.

The results in Table 2 are consistent with Figure 1. Column 1 shows that inheritance increases net wealth by about 157,200 DKK in the years following the inheritance event. Columns 2 and 3 examine the two components of net wealth.[30] Inheritance reduces both total debt (by about 405,500 DKK) and total assets (by about 188,500 DKK), with the reduction in debt exceeding the reduction in assets. Rather than holding the windfall, treated entrepreneurs appear to use it to repay the secured obligations that survive the bankruptcy discharge, so the inheritance shows up as lower debt rather than higher assets.

Nonwealth effects of parental death Because the inheritance follows a parental death, the post-inheritance coefficient could also reflect nonwealth changes around the death: deteriorating parental health and caregiving could reduce labor supply beforehand, and bereavement or changes in household circumstances could affect behavior afterward. To examine these channels, Appendix Figure IA.5 plots dynamic responses of labor-supply and household outcomes around the inheritance event, estimated with the matched stacked specification of Equation 1 in event time relative to inheritance. Because the inheritance is dated to the year of parental death, event year 0 denotes both the death and the inheritance. The window runs from five years before to six years after inheritance; I restrict the inheritance-centered analyses to cohorts whose inheritance year is 2017 or earlier to allow six years of follow-up for every cohort.[31] I use event year –2 as the omitted category so that the coefficient at –1 captures any change in the year immediately preceding the death.

Employment and labor supply show no statistically detectable changes before death

[30] Because the three outcomes are estimated separately and each is winsorized by event year (Section 3.1), the coefficients need not satisfy the identity, net wealth = total assets – total debt, exactly.

[31] The registers end in 2023, so inheritances occurring in 2018 or later lack six years of potential follow-up. The restriction retains 162 of the 185 treated entrepreneurs, together with their matched controls. The inheritance-centered regressions use 8,101 observations; the difference from the main sample reflects both the cohort restriction and the change in the observation window.

and decline persistently afterward. Employment (Panel (a)) is statistically flat before the event, with an imprecise decline of about 2.6 percentage points at event year –1 whose confidence interval includes zero, and then falls by about 12 percentage points from event year 2 through 6, against a baseline employment share of 71.7%. Total labor supply (Panel (b)), which proxies for hours worked using ATP pension contributions following Kleven et al. (2019), shows no pre-event movement, drops sharply in event year 0, and remains lower throughout the post-event window.[32] The absence of changes before death provides little support for anticipatory caregiving, and the persistence of the decline is more consistent with a wealth effect on labor supply than with transitory bereavement. Marital status (Panel (c)) shows no statistically detectable change throughout the event window. The number of children (Panel (d)) is also largely stable, apart from an economically small decline in year +6.

4.2 Post-bankruptcy wealth and serial entrepreneurship

In the previous section, I established that windfalls from inheritance increase net wealth after bankruptcy. In this section, I estimate the average entrepreneurial response to this increase, before examining in Section 4.3 how the response varies with the personal losses associated with prior failure. If failed entrepreneurs remain financially constrained, greater net wealth should increase their subsequent entrepreneurial activity.

I begin with a descriptive analysis. Figure 2 plots the share of individuals who are entrepreneurs for the treated and control groups in the same event-time layout as Figure 1. Figure 2 provides three stylized facts about entrepreneurship around bankruptcy. First, many entrepreneurs exit their businesses as they approach bankruptcy, consistent with the businesses failing. Second, in the six years following bankruptcy, the share of entrepreneurs recovers by about 18 percentage points, from approximately 5% to 23%, with similar increases of 19 and 17 percentage points in the treated and control groups. Third,

[32] This pattern is in line with Vihriälä and Virkola (2025), who find little evidence of anticipatory adjustment in spending and labor supply before inheritances due to non-sudden parental deaths.

the two groups move in parallel before bankruptcy, and the treated group's marginally higher share is neither economically nor statistically significant. The raw comparison thus shows no clear divergence between the groups after the inheritance window.

To test this more formally, I estimate Equations 1 and 2 with the indicator for entrepreneurship as the outcome. The dependent variable, $Entre\,preneur_{ict}$, is an indicator variable equal to one if individual i in cohort c is an entrepreneur in event year t. Table 3 shows the results. The results are consistent with the univariate comparison in Figure 2. In Column 1, the point estimate is an increase of 3.1 percentage points in the annual probability of being an entrepreneur. For comparison, the average annual entrepreneurship rate across both the treated and control groups is 16.7% from the bankruptcy year through six years afterward. The 95% confidence interval ranges from about –2 to +8 percentage points, encompassing both zero and economically meaningful increases. Next, in Columns 2 and 3, I examine whether this average conceals a response concentrated among recipients of substantial windfalls by exploiting variation in the size of inheritance. Neither column provides statistically clear evidence that the entrepreneurial response increases with inheritance size. In Column 2, the coefficient on *After inheritance* $\times$ *Inherited wealth* implies a marginal effect of about 2.4 percentage points at the mean inheritance of 232,000 DKK, but the estimate is imprecise and does not establish a linear relation between inherited wealth and the response. Similarly, in Column 3, I find no statistically detectable difference in the response between recipients of above- and below-median inheritances, where the median is about 108,000 DKK (14,500 EUR).[33]

I next examine the timing of the response relative to the windfall itself. Because Figure 2 and Table 3 are organized in bankruptcy time, I complement them with an event study centered on the inheritance year, which assesses anticipation and traces the response by time since inheritance. The specification retains the controls and fixed effects of Equation 1 and replaces *After inheritance* with indicators for event time relative to in-

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

heritance, estimated on the window and sample of Appendix Figure IA.5, with year –1 as the reference period. Appendix Figure IA.6 reports the estimates. The pre-inheritance coefficients are negative and rise toward the reference period, from about –4 percentage points four to five years before inheritance. They are individually indistinguishable from zero and jointly so ($p = 0.72$): entrepreneurship shows no statistically detectable adjustment before the windfall arrives. The post-inheritance coefficients are positive in the years immediately after the windfall and negative in years 5 and 6; however, none is statistically distinguishable from zero. Their average over years 0–6 is about 0.6 percentage points ($t = 0.23$), compared with a static estimate of about 3 percentage points on the same sample. The static estimate is larger because it is measured relative to the average of all pre-inheritance years, when the difference between the treated and control groups was lower than in year –1. The point estimate is therefore positive under either specification but imprecisely estimated, and its magnitude depends on the reference period.

Overall, the results in Table 3 indicate that the average entrepreneurial response to a sizable increase in post-bankruptcy net wealth is positive but imprecisely estimated. For context, a large body of research documents a positive effect of windfall wealth on entrepreneurship outside bankruptcy (e.g., Holtz-Eakin et al. 1994; Andersen and Nielsen 2012; Bellon et al. 2021; Cespedes et al. 2021). The confidence interval above is too wide to establish whether failed entrepreneurs respond differently on average. I next examine how the response varies with the personal losses associated with prior failure.

4.3 Experience of severe losses and serial entrepreneurship

Next, I examine how the entrepreneurial response to inheritance varies with the personal losses associated with prior failure. These losses may reduce an entrepreneur's willingness to take risks again, even when her finances improve. More broadly, prior studies document that negative personal experiences, such as corporate bankruptcy or personal investment losses, can reduce subsequent risk-taking (Malmendier et al. 2011;

Dittmar and Duchin 2016; Koudijs and Voth 2016; Schoar and Zuo 2017; Andersen et al. 2019). 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. 2024), I focus on losses that have a *severe* impact on personal income and wealth. I measure the severity of these losses in three ways.

First, I define severe losses based on whether an individual experiences negative personal income from entrepreneurship at any point from seven years to one year before bankruptcy.[34] The measure equals zero for entrepreneurs who managed only an LLC, because limited liability keeps business losses inside the company rather than passing them through to personal income. Second, I use the bankruptcy chapter to identify entrepreneurs with a large share of personal debt arising from business activity. As discussed in Section 2, the business debt chapter is available only to entrepreneurs whose debt is predominantly business-related, so filers under this chapter are more likely to have a large business-related share of personal debt than filers under the ordinary chapter. Third, I define whether an individual was a ULC entrepreneur before bankruptcy — rather than only managing an LLC — implying the individual is *fully* liable for the business debts. Although some entrepreneurs enter bankruptcy because of personal guarantees on their LLC's debt, entrepreneurs who managed a ULC bear the full losses of that business personally, whereas those who managed only LLCs generally do not. Using these three measures, I test whether the entrepreneurial response to windfall wealth differs between failed entrepreneurs who did and did not experience severe losses.

Figure 3 plots the share of individuals who are entrepreneurs around bankruptcy, separately for entrepreneurs with less severe losses (left subplots) and more severe losses (right subplots) under each of the three measures. In each subplot, the share falls steeply as bankruptcy approaches and recovers afterward in both the treated and control groups.

[34] In Denmark, entrepreneurial losses can be used for a deduction in taxable income in the same year if an entrepreneur has another salaried job or earns positive capital income. However, the tax deduction is unlikely to significantly offset the loss in total income in the year given that my sample consists of full-time entrepreneurs who have little financial wealth.

After bankruptcy, the treated group's share rises above the control group's among entrepreneurs with less severe losses, whereas no comparable gap opens among those with more severe losses. For instance, in panel (a), about 30% of treated entrepreneurs who experienced *no* severe income loss are entrepreneurs six years after bankruptcy, compared with about 20% of their matched controls, whereas fewer than 20% of treated entrepreneurs who experienced severe income loss are, no more than their matched controls.

Next, I investigate the results in Figure 3 more formally by estimating the following linear probability model. I extend Equation 1 by interacting $\textit{After inheritance}_{ict}$ with an indicator for experiencing severe losses:

$$Y_{ict} = \beta_1 \textit{After inheritance}_{ict} + \beta_2 \textit{After inheritance}_{ict} \times \textit{Severe losses}_i + \delta X'_{ict} + \alpha_{ic} + \kappa_{ct} + \gamma_a + \varepsilon_{ict}, \tag{3}$$

where $\textit{Severe losses}_i$ is one of the three indicators defined above—$\textit{Severe income loss}_i$, $\textit{Business debt chapter}_i$, or $\textit{Full personal liability}_i$—each entering a separate regression. The main effect of $\textit{Severe losses}_i$ is absorbed by the individual-by-cohort fixed effect α_{ic}. The coefficients of interest are β_1, the entrepreneurial response to inheritance among entrepreneurs without severe losses, and $\beta_1 + \beta_2$, the response among those with severe losses; β_2 therefore measures how the response differs between the two groups.

Table 4 reports the results from estimating Equation 3 using the three measures of severe losses. In Column 1, the estimated increase in the annual probability of being an entrepreneur is 7.5 percentage points for entrepreneurs without negative entrepreneurial income before bankruptcy, compared with the average annual entrepreneurship rate of 16.7%. The response is 10.8 percentage points smaller for entrepreneurs with negative entrepreneurial income.[35] In Column 2, the estimated increase is 8.6 percentage points

[35] Because limited liability keeps business losses inside the company, *Severe income loss* is zero for LLC-only entrepreneurs. Restricting Column 1 to entrepreneurs who operated a ULC before bankruptcy, for whom negative entrepreneurial income is observed, yields a difference between the severe- and less-severe-loss groups of about –8.4 percentage points, compared with –10.8 percentage points in the main sample; the estimate is less precise in the smaller subsample ($p = 0.12$) but similar in magnitude (Appendix Table IA.3).

for entrepreneurs who filed under the ordinary chapter, and the response is 12.6 percentage points smaller for those who filed under the business debt chapter. Both differences are statistically significant at the 5% level. In Column 3, the estimated increase is 14.2 percentage points for entrepreneurs with partial personal liability (those who managed only LLCs), and the response is 12.8 percentage points smaller for those with full personal liability. This difference is less precisely estimated and is not statistically significant at conventional levels.

Financial position and differences in responses The weaker response among entrepreneurs with more severe losses could arise if they remained more financially constrained after inheritance. I examine this possibility in three ways.

First, I compare net wealth across the severity groups in the year of bankruptcy (event year 0), restricting the sample to observations before inheritance. For each of the three measures of personal losses, I regress net wealth on the severe-loss indicator, controlling for individual characteristics and for calendar-year and age-bin fixed effects. Appendix Table IA.5 reports the results. None of the estimated differences is statistically significant: I find no evidence that entrepreneurs with more severe losses are financially worse off before inheritance.

Second, I examine whether the increase in net wealth following inheritance differs by the severity of losses. Specifically, I estimate Equation 3 using each of the three measures of severe losses, with the level of net wealth as the dependent variable. Appendix Table IA.6 reports the results. The main coefficient *After inheritance* is consistently positive and statistically significant, confirming the baseline effect of inheritance on post-bankruptcy wealth. None of the interaction terms (*After inheritance* $\times$ *Severe losses*) is statistically significant, so I find no statistically detectable difference across the severity groups in the increase in net wealth following inheritance.

Third, I examine whether larger inheritances narrow the difference in responses be-

tween entrepreneurs with more and less severe losses. To address this, I exploit variation in inheritance size, as in Columns 2 and 3 of Table 3, by extending Equation 2 to interact *Severe losses*$_i$ with *Size*$_i$. Appendix Tables IA.7 and IA.8 report the results for the two measures of inheritance size, respectively. In both tables, the coefficients on *After inheritance*$_{ict}$ × *Severe losses*$_i$ remain negative, though they reach significance only in the continuous-size specification (Appendix Table IA.7). The triple-interaction terms, which would capture any narrowing of the difference with a larger windfall, are statistically insignificant in both tables: they are positive in the continuous-size specification and small with mixed signs in the above-median specification. I therefore find no robust evidence that larger inheritances narrow the differences in responses.

Pre-inheritance trends and differences in responses Finally, I assess the identifying assumption behind the differences in Table 4, which is the same as for the baseline: absent the windfall, entrepreneurs in each subgroup and their matched controls would have experienced similar changes in entrepreneurial activity. Because the aggregate pre-inheritance test in Appendix Figure IA.6 could conceal offsetting movements across subgroups, I re-estimate the inheritance-centered event study separately for each measure of severe losses (Appendix Figure IA.7). Panel A of Appendix Table IA.1 reports joint tests of the pre-inheritance coefficients within each subgroup and of their differences across subgroups. For *Severe income loss* and *Business debt chapter*, no test within a subgroup rejects, and the tests of differences between subgroups yield p-values of 0.147 and 0.112. For *Full personal liability*, both the test within the subgroup with partial personal liability ($p = 0.021$) and the test of differences between subgroups ($p = 0.030$) reject. The subgroup with partial personal liability exhibits a particularly large negative coefficient at event year –5.

Panels B and C compare static estimates with average dynamic coefficients over inheritance-relative years 0–6 on the same 8,101 observations, so that any difference reflects specifica-

tion rather than sample composition. The static specification pools the pre-inheritance years, whereas the dynamic coefficients are measured relative to year –1. For *Severe income loss*, the difference in responses between entrepreneurs with more and less severe losses changes little, from –9.1 to –9.7 percentage points, and remains statistically significant at the 10% level. For *Business debt chapter*, it attenuates from –14.5 to –10.1 percentage points and remains significant at the 5% level. For *Full personal liability*, it attenuates from –13.7 to –4.1 percentage points and is not statistically significant. The responses within subgroups are more sensitive to the reference period than the differences between them: among entrepreneurs without severe income loss and among those who filed under the ordinary chapter, the average dynamic coefficients are 4.8 and 4.6 percentage points, compared with static estimates of 7.0 and 8.9 percentage points.

Interpretation Overall, the entrepreneurial response to inheritance varies with the personal losses associated with prior failure. The response is stronger among entrepreneurs without negative entrepreneurial income before bankruptcy and among those with a smaller business-related share of personal debt, and both differences remain when the response is measured relative to the year before inheritance. The main estimates also show a stronger response among entrepreneurs with partial rather than full personal liability, although this difference is less precisely estimated and more sensitive to specification. I find no robust evidence that differences in financial position account for these patterns. The findings are consistent with the experience of severe personal losses weakening the response to greater wealth, in line with evidence that negative personal experiences can reduce subsequent risk-taking (Malmendier et al. 2011; Dittmar and Duchin 2016; Koudijs and Voth 2016; Schoar and Zuo 2017; Andersen et al. 2019).

4.4 Longer-run entrepreneurial outcomes

The economic benefits of starting another business depend in part on whether it leads to lasting activity and income. I therefore compare, over the three and five years after inheritance, whether treated and control entrepreneurs restart, whether they are operating a business at each horizon, and their cumulative income during years of entrepreneurship. Because restarting is itself a response to the windfall, I measure these outcomes for the matched sample rather than for restarters only, so the comparison does not condition on the decision to restart. To capture restarts rather than businesses already in operation when the windfall arrives, I exclude individuals who were entrepreneurs one year before inheritance.[36]

To test this formally, I estimate the following cross-sectional model separately for horizons $h = 3$ and $h = 5$:

$$Y_i^h = \beta \, Treated_i + \gamma' X_i + \mu_c + \theta_a + \varepsilon_i, \tag{4}$$

where the dependent variable, Y_i^h, is one of four outcomes: an indicator equal to one if individual i is an entrepreneur in any year from the inheritance year through h years after ($Entered_i^h$, Column 1); an indicator equal to one if she is an entrepreneur h years after inheritance ($Operating_i^h$, Column 2); the difference between the two, which equals one for individuals who restart within the window but have exited by year h (Column 3); and cumulative labor and entrepreneurial income from the inheritance year through year h, assigning zero in years without entrepreneurship (Column 4). $Treated_i$ is an indicator equal to one for treated entrepreneurs and zero for their matched controls. For the control vector X_i, I include the matching variables that vary within cohort—wealth measured one year before bankruptcy and the discharge ratio. The cohort fixed effects (μ_c) absorb the characteristics matched exactly within cohort, namely gender and the three severe-losses indicators, and age-bin fixed effects (θ_a, based on age at bankruptcy in five-year

[36] The exclusion is applied symmetrically: for control individuals, the reference year is the inheritance year of the treated entrepreneur in their cohort.

bins) absorb lifecycle effects. Regressions are weighted so that each cohort contributes equally, and standard errors are clustered at the matched-cohort level.[37] The coefficient of interest, β, measures the difference in entrepreneurial activity or income between the treated and control groups over the h-year window. In Column 3, a negative β indicates that a smaller share of the treated group has restarted and exited by year h.

Table 5 reports the estimates for the three-year horizon (Panel A) and the five-year horizon (Panel B). At neither horizon is any coefficient statistically distinguishable from zero. In Panel A, the treated group is about 1 percentage point more likely to have restarted by year 3 and about 2 percentage points more likely to be operating at year 3 (Columns 1 and 2), against control-group means of 19.5% and 16.3%. The entry–operation gap in Column 3 is correspondingly small, about –1 percentage point relative to a mean of 3.3%, and it changes sign at the five-year horizon in Panel B, where the entry coefficient is again about 1 percentage point and the operation coefficient is zero. Cumulative income during years of entrepreneurship is about 35,100 DKK lower over three years (Column 4), against a control-group mean of 115,900 DKK. These income estimates are economically non-trivial, about 30% of the three-year mean, but imprecise: the data neither reject zero nor rule out a meaningful shortfall.[38] The estimates therefore provide no clear evidence that inheritance increases entry followed by exit or lowers longer-run business activity or income.

Prior work on lenient bankruptcy regimes finds that the businesses started under them are less durable: Cerqueiro et al. (2019) find that first-time entrepreneurs who start after increases in state-level wealth protection are less likely to survive, and Cahn et al. (2021) find that bankrupt entrepreneurs who restart after the removal of corporate bank-

[37] Clustering at the individual level instead yields similar results.

[38] Appendix Table IA.2 restricts the sample to entrepreneurs who did not experience severe losses, under each of the three definitions, at both horizons. The entry and operation coefficients are larger than in the full sample, between about 4 and 9 percentage points for the two larger subsamples, but remain imprecise. The entry–operation gap is small throughout; the one estimate that is significant, for the ordinary-chapter subsample at three years, is negative and does not persist at five years. The income estimates vary in sign across subsamples and horizons.

ruptcy flags are more likely to fail again. My setting differs from these studies in two ways. They examine changes in *expected* post-bankruptcy wealth or in external financing conditions, whereas I examine an increase in realized post-bankruptcy wealth. They also follow entrepreneurs who start a business, whereas my comparison includes individuals who never restart. On average, I find no clear evidence of a shift toward less durable or less profitable businesses after a windfall, although the estimates, particularly for income, are too imprecise to rule one out.

5 Robustness checks

In this section, I perform additional tests to assess the robustness of the baseline findings. I first address the concern that the treated and control groups may differ in the inheritance they expect to receive, which could shape behavior even before any windfall is realized. Next, I examine whether experiencing severe losses merely proxies for low entrepreneurial ability. Finally, I test whether the weaker response among entrepreneurs with severe losses reflects the industry composition in which their businesses failed.

Alternative matched sample with additional matching by expected inheritance One concern is that the treated and control groups differ in *expected* inheritance, the family wealth they stand to receive, even though matching already balances their own pre-bankruptcy wealth. Such differences could bias the comparison through two channels: entrepreneurs with wealthier parents may have a higher baseline propensity to start a business, or may behave differently before bankruptcy (in filing, default, or risk-taking). To address this concern, I match on expected inheritance directly.[39] The alternative sample is matched identically, except that expected inheritance enters the Mahalanobis criteria (alongside net wealth at event years –3 to –1) and matches differing by more than

[39] I construct expected inheritance as for realized inheritance (Appendix IA.3)—parental wealth net of tax, split equally among prospective heirs—but evaluated in the year of the bankruptcy ruling rather than in the year a parent actually dies.

250,000 DKK are dropped. This yields a smaller sample of 149 treated and 393 control individuals, closely balanced on expected inheritance (about 183,000 vs. 161,000 DKK), so any residual difference in restart behavior is unlikely to run through the prospect of a windfall.

Appendix Tables IA.10 and IA.11 reproduce the baseline and severe-losses results (Tables 3 and 4) and the longer-run outcomes (Table 5) in this alternative sample.[40] The results are qualitatively consistent with the main sample: the average entrepreneurial response is positive but imprecisely estimated, the response is stronger among entrepreneurs with less severe losses, and the longer-run entrepreneurial outcomes of the treated group show no consistent shortfall relative to the control group. Differences in expected inheritance between the treated and control groups therefore do not drive my findings.

Severe losses versus low entrepreneurial ability Experiencing severe losses before bankruptcy may correlate with low entrepreneurial ability. To address this concern, I add to the specifications in Table 4 the interaction of *After inheritance* with *Low past performance*, an indicator equal to one if an entrepreneur's labor and entrepreneurial income remains in the bottom decile of her industry and year in every year of entrepreneurship before bankruptcy.[41] By requiring persistence, the measure distinguishes entrepreneurs of low ability from those who merely had a bad year. It is defined only for entrepreneurs whose industry-year cell is large enough, so the tests below run on a slightly smaller sample than Table 4.

Table 6 reports the results. Column 1 includes only low past performance, without the severe-losses measures. The positive coefficient on *After inheritance* (about 4.7 per-

[40] The alternative-sample entrepreneurship table reproduces the baseline specification (Column 1 of Table 3) and the three severe-losses measures; I omit the two inheritance-size columns (Columns 2 and 3), whose results are likewise unchanged.

[41] I compute the deciles within 88-industry-year cells over the population of entrepreneurs, including those who do not go bankrupt. The median (average) number of members in these cells is, for example, 547 (2,965) in year 2006. I exclude observations that have fewer than 25 members in an industry-year cell. The industry classification is based on the two-digit NACE Rev. 2 division code. I obtain similar results if I use the one-letter code, which divides the economy into 22 industries.

centage points) is the estimated response among entrepreneurs without persistently low performance.[42] The negative interaction with *Low past performance* more than offsets this response for persistent low performers. This pattern is consistent with models in which entrepreneurs learn about their ability through entry and failure (e.g., Jovanovic 1982).

Columns 2 through 4 add the severe-losses measures. Even after controlling for low past performance, the interaction of *After inheritance* with *Severe income loss* remains negative and significant (Column 2, about –10.9 percentage points), as do the interactions with *Business debt chapter* (Column 3) and *Full personal liability* (Column 4). The differences in entrepreneurial responses therefore remain after accounting for persistently low past performance, whose own interaction stays negative and significant throughout.

Severe losses versus industry composition A potential concern is that the weaker response among entrepreneurs with severe losses reflects industry composition rather than the losses themselves: severe failures may concentrate in sectors whose entrepreneurs restart more slowly after bankruptcy. To address this, I augment the severe-losses specifications in Table 4 with fixed effects for the industry of the pre-bankruptcy business interacted with the event year, so that the interaction is identified only from entrepreneurs who failed in the same industry (two-digit NACE Rev. 2 division code) and are observed at the same event year, comparing those who did and did not experience severe losses. Appendix Table IA.9 shows that the pattern in Table 4 is unchanged. Industry composition of failed businesses therefore does not account for the differences in responses.

6 Conclusion

Helping failed entrepreneurs return to business is one rationale for bankruptcy provisions that discharge debt and protect wealth from creditors. In this study, I examine whether an increase in net wealth after bankruptcy leads failed entrepreneurs to start an-

[42] To conserve space, I report only the binary inheritance indicator, *After inheritance*.

other business, and which of them respond, using inheritances received after personal bankruptcy in Denmark as plausibly exogenous variation in post-bankruptcy wealth. I find that the average entrepreneurial response to inheritance is positive but imprecisely estimated. The point estimate is an increase of 3.1 percentage points in the annual probability of being an entrepreneur, against an average annual entrepreneurship rate of 16.7% across the treated and control groups, and the 95% confidence interval ranges from about –2 to +8 percentage points. The response varies with the personal losses associated with prior failure. The estimated increase is 7.5 percentage points for entrepreneurs without negative entrepreneurial income before bankruptcy, and the response is about 10.8 percentage points lower for those with negative entrepreneurial income. The estimated increase is 8.6 percentage points for those with a smaller business-related share of personal debt, and the response is about 12.6 percentage points lower for those with a larger share. These findings are consistent with the experience of severe personal losses weakening the response to greater wealth.

My study has implications for policies that aim to provide a second chance to failed entrepreneurs by increasing their post-bankruptcy wealth. The estimated entrepreneurial response to greater wealth is stronger among entrepreneurs whose failure imposed less severe personal losses. This suggests that a policy's effect on restarts may depend on the personal losses experienced by the entrepreneurs it reaches. 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 how greater post-bankruptcy wealth affects serial entrepreneurship.

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Figure 1: Net wealth around bankruptcy and inheritance

This figure plots the dynamics of average net wealth, defined as the difference between total assets and total debt. 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 ("Inheritance") refers to the treated group of bankrupt individuals who receive an inheritance between event years 0 and 3. The dashed line ("No inheritance") refers to the matched control group of bankrupt individuals who do not receive an inheritance. The two groups are matched as described in Section 3.3. The shaded area between event years 0 and 3 represents the inheritance window, during which inheritance events occur. Each yearly mean is computed as a weighted average, with each treated entrepreneur assigned weight one and each of her k matched controls weight $1/k$, so that each cohort contributes equally to the plotted line. Net wealth is measured in thousands of 2015 DKK (1 Euro $\approx$ DKK 7.45).

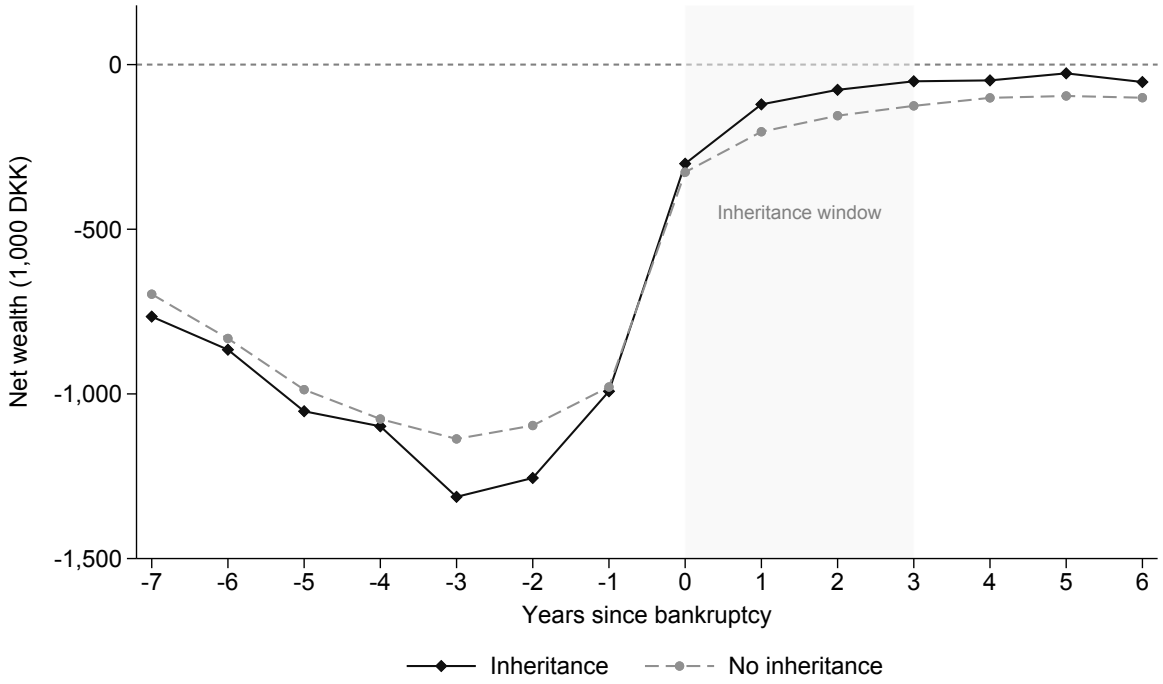


Figure 2: Entrepreneurship around bankruptcy

This figure plots the dynamics of the share of individuals who are entrepreneurs. The solid line ("Inheritance") refers to the treated group and the dashed line ("No inheritance") to the matched control group. The two groups, event time, the shaded inheritance window, and the weighting are defined as in Figure 1.

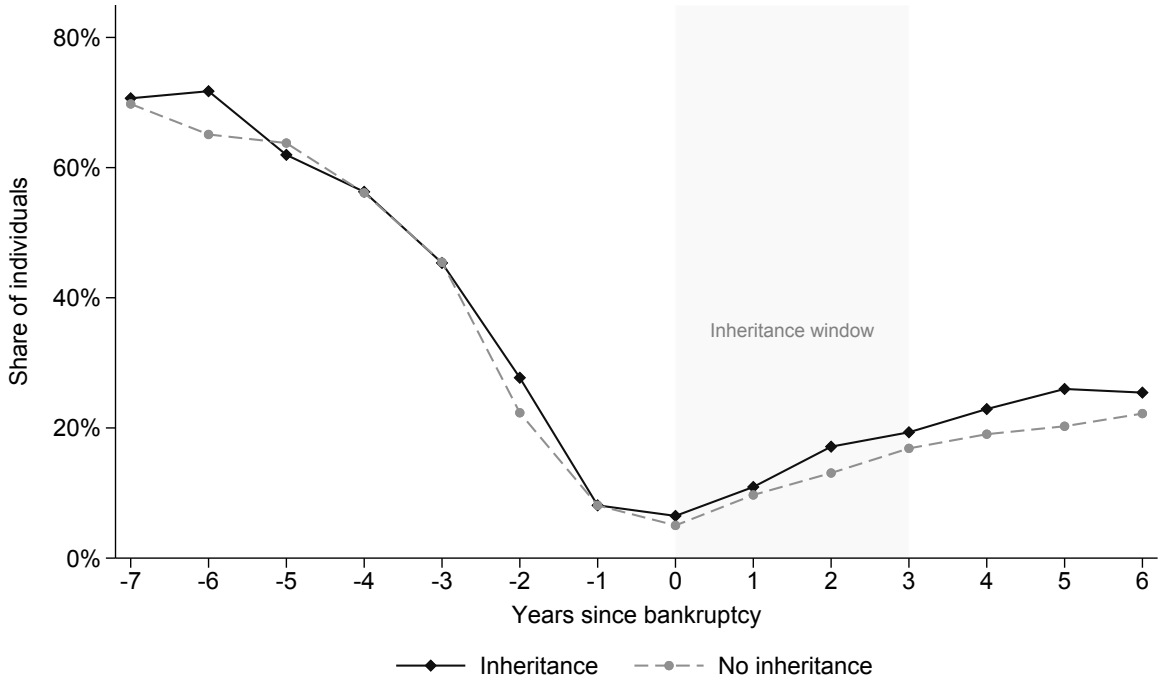
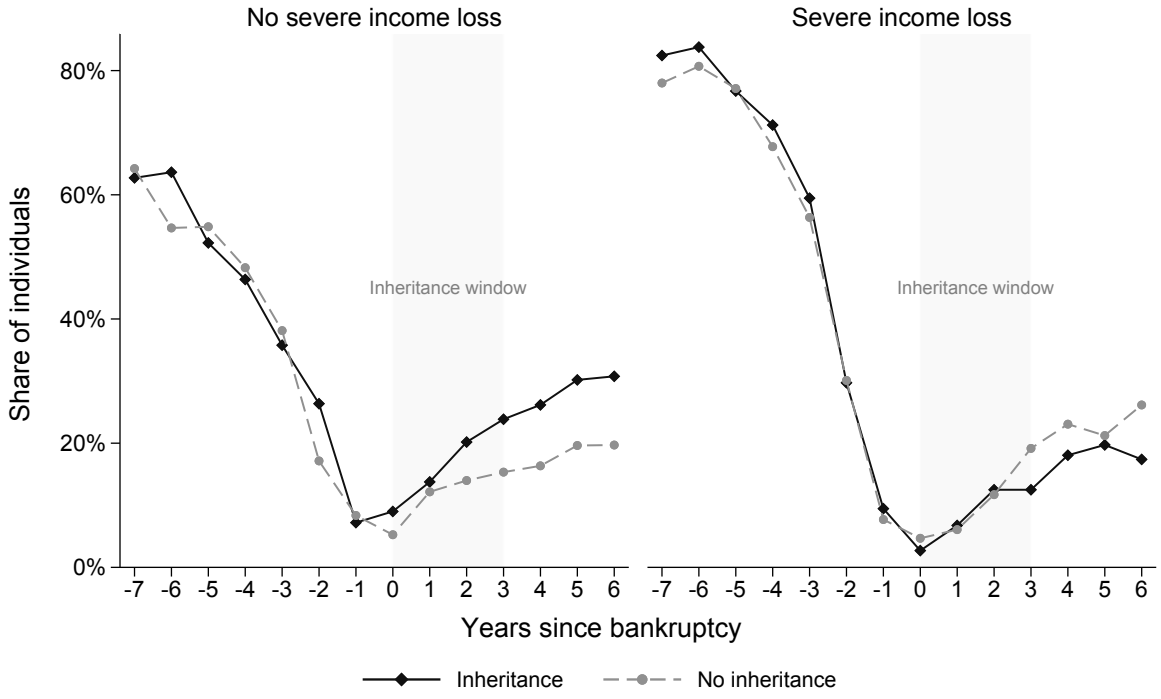


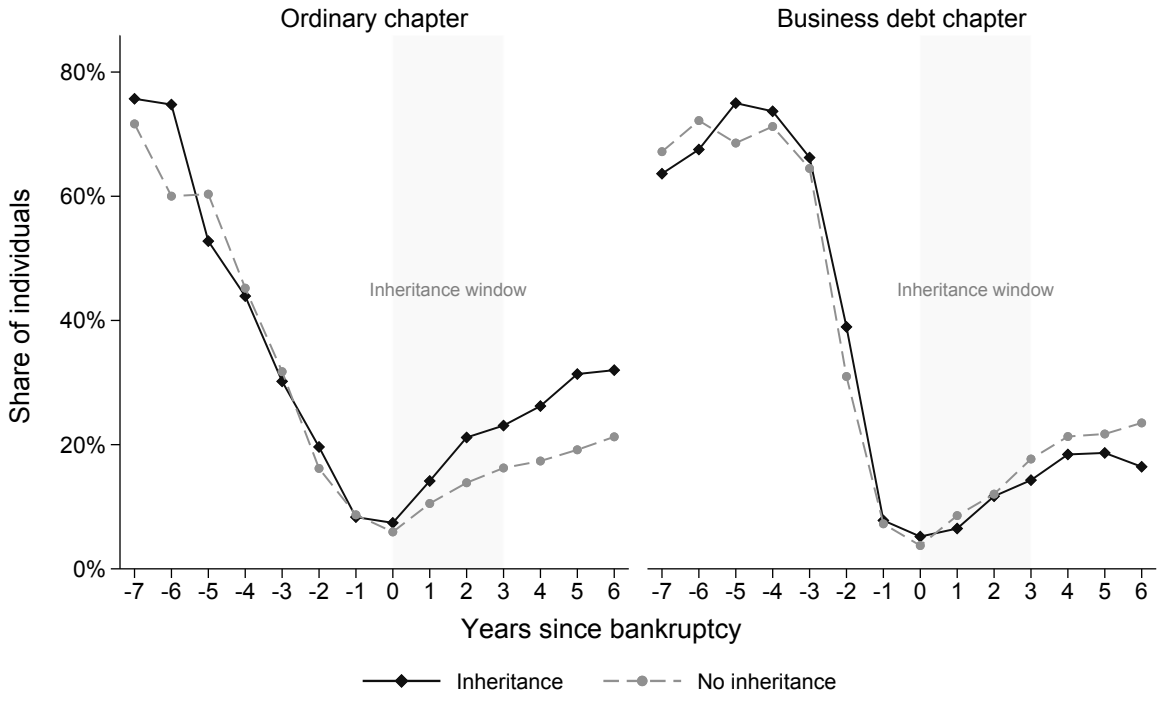
Figure 3: Severe losses and entrepreneurship around bankruptcy

This figure plots the dynamics of the share of individuals who are entrepreneurs, by three measures of the severity of personal losses. Within each panel, the left subplot shows the less-severe-loss subgroup and the right subplot shows the severe-loss subgroup. In all panels, the solid black lines ("Inheritance") refer to the treated group and the dashed grey lines ("No inheritance") to the matched control group. Panel (a) plots the dynamics split by whether the individual experiences negative entrepreneurial income before bankruptcy. Panel (b) plots the dynamics split by whether the individual files for bankruptcy under the business debt chapter, which is available to debtors whose debt is at least 75% business-related. Panel (c) plots the dynamics split by whether the individual experiences full personal liability (i.e., whether the individual managed any unlimited liability company before bankruptcy). The two groups, event time, the shaded inheritance window, and the weighting are defined as in Figure 1.

(a) Severe income loss



(b) Business debt chapter filing



(c) Full personal liability

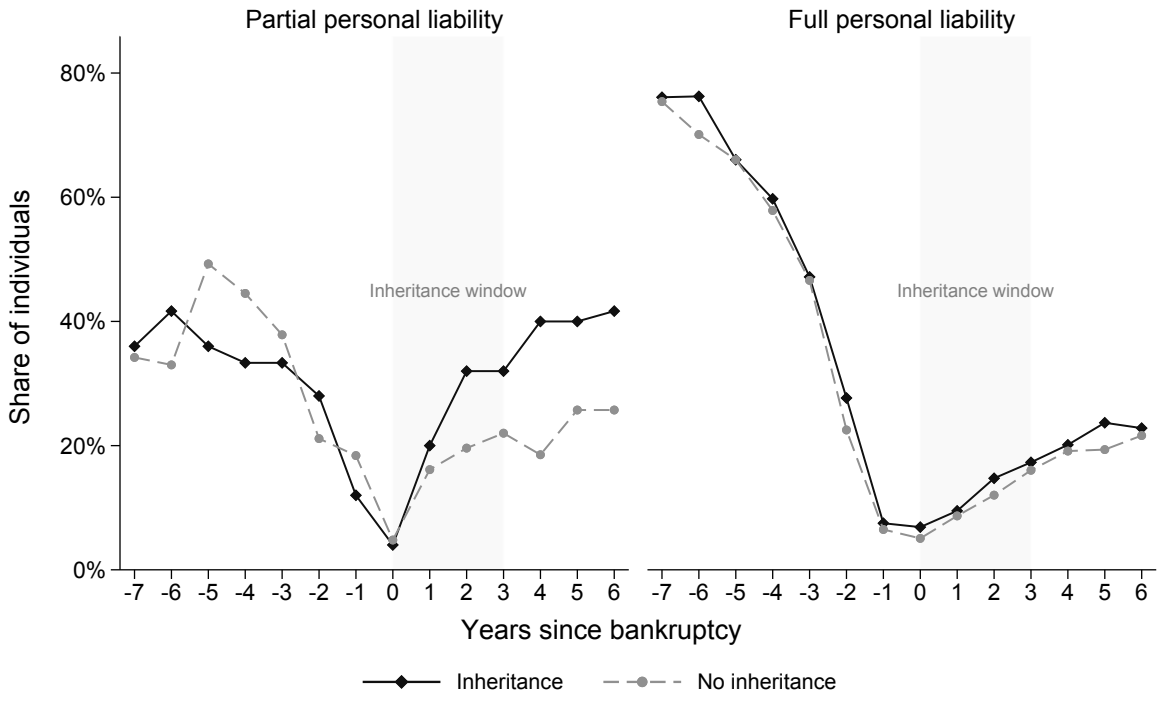


Table 1: Summary statistics

This table presents the mean and standard deviation of the main variables for the sample of entrepreneurs who go bankrupt between 2006 and 2017. Each column refers to one of the three groups of individuals in my sample: (1) all bankrupt entrepreneurs, (2) the "treated group" of entrepreneurs who receive an inheritance between the year of bankruptcy and three years afterward, and (3) 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 3.3. Each treated entrepreneur is matched to up to five controls, which together form a cohort. The last column reports the difference in means between the treated and control groups, estimated by a weighted least-squares regression of the variable on a treatment indicator. Each treated entrepreneur is assigned weight one and each of her k matched controls weight $1/k$, so that each cohort contributes equally to the comparison. Standard errors are clustered at the matched-cohort level to account for the within-cohort dependence introduced by matching, following Abadie and Spiess (2022). The variables in this table are measured at the year of bankruptcy, except for those in Panels A and D. 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: indicators for severe income loss, full personal liability, and low past performance. *Severe income loss* is an indicator variable equal to one if the individual experiences negative entrepreneurial income before bankruptcy. *Full personal liability* is an indicator variable equal to one if the individual has been an unlimited liability company entrepreneur before bankruptcy. *Low past performance* is an indicator variable equal to one if the individual's annual labor and 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	All (1)	Treated (2)	Control (3)	Difference (2)-(3)
A. Wealth and income (1,000 DKK)				
Pre-bankruptcy wealth	-1,220.6 (1,884.8)	-992.1 (1,635.4)	-978.9 (1,628.1)	-13.2 [-0.9]
Pre-bankruptcy total income	284.0 (112.9)	281.7 (106.5)	285.4 (113.9)	-3.6 [-0.4]
B. Individual characteristics				
Age	43.7 (8.3)	47.5 (6.6)	46.9 (6.6)	0.6*** [3.5]
Male (%)	73.4 (44.2)	82.7 (37.9)	82.7 (37.9)	-0.0 [-0.1]
Years of education	11.7 (1.8)	11.8 (1.9)	11.6 (2.0)	0.2 [1.1]
C. Bankruptcy case characteristics				
Business debt chapter (%)	46.1 (49.9)	41.6 (49.4)	41.6 (49.3)	-0.0 [-0.0]
Discharge ratio (%)	91.1 (13.1)	92.8 (8.0)	93.3 (7.4)	-0.5* [-1.7]
D. Pre-bankruptcy personal business experience				
Severe income loss (%)	44.3 (49.7)	40.0 (49.1)	40.0 (49.0)	0.0 [0.0]
Full personal liability (%)	84.4 (36.3)	86.5 (34.3)	86.5 (34.2)	-0.0 [-0.1]
Low past performance (%)	3.8 (19.2)	1.8 (13.2)	2.3 (14.9)	-0.5 [-0.4]
Number of individuals	5,075	185	589	

Table 2: Inheritance as a source of post-bankruptcy wealth

This table reports estimates from the stacked difference-in-differences specification in Equation 1, examining the effect of post-bankruptcy inheritance on the levels of net wealth, total debt, and total assets (measured in thousands of 2015 DKK, where 1 Euro $\approx$ DKK 7.45). The treated group consists of bankrupt individuals who receive an inheritance between the year of bankruptcy and three years after; the control group consists of bankrupt individuals who do not receive an inheritance. The two groups are matched as described in Section 3.3. The sample includes observations from seven years before bankruptcy until six years after. *After inheritance* is an indicator variable equal to one for the treated entrepreneur in event years on or after her inheritance year, and zero for every control observation throughout the cohort. The cohort-by-event-year fixed effects absorb the effect of the bankruptcy ruling, which occurs at the same event year for every member of a given cohort. All columns include time-varying individual controls, individual-by-cohort fixed effects, cohort-by-event-year fixed effects, and age-bin fixed effects (dummies for every five-year bin of age). Time-varying individual controls are an indicator for being married and the number of children under the age of 18 living in the household. Regressions are weighted by assigning each treated entrepreneur weight one and each of her k matched controls weight $1/k$, so that each cohort contributes equally. 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
After inheritance	157.2***	-405.5***	-188.5*
	(3.06)	(-3.20)	(-1.86)
Time-varying individual controls	Yes	Yes	Yes
Individual $\times$ cohort fixed effects	Yes	Yes	Yes
Cohort $\times$ event-year fixed effects	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes
R^2	0.89	0.92	0.87
Observations	10,705	10,705	10,705

Table 3: Post-bankruptcy wealth and serial entrepreneurship

This table reports estimates from the stacked difference-in-differences specifications in Equations 1 and 2, examining the effect of post-bankruptcy inheritance on the probability of entrepreneurship. The dependent variable, *Entrepreneur*, is an indicator variable equal to one if the individual is an entrepreneur in the year. Column 1 reports the baseline specification of Equation 1. Columns 2 and 3 report the size-interacted specification of Equation 2, with $Size_i$ set to *Inherited wealth*$_i$ in Column 2 and to *Large inheritance*$_i$ in Column 3. *After inheritance* is an indicator variable equal to one for the treated entrepreneur in event years on or after her inheritance year, and zero for every control observation throughout the cohort. *Inherited wealth*$_i$ is the amount of inherited wealth received by treated entrepreneur i, measured in millions of 2015 DKK. *Large inheritance*$_i$ is an indicator equal to one if individual i receives an above-median inheritance. Both are zero for control entrepreneurs. The treated and control groups are defined and matched as in Table 1. The sample includes observations from seven years before bankruptcy until six years after. All columns include time-varying individual controls, individual-by-cohort fixed effects, cohort-by-event-year fixed effects, and age-bin fixed effects (dummies for every five-year bin of age). Time-varying individual controls are an indicator for being married and the number of children under the age of 18 living in the household. Regressions are weighted by assigning each treated entrepreneur weight one and each of her k matched controls weight $1/k$, so that each cohort contributes equally. 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	0.031	0.007	0.052
	(1.23)	(0.23)	(1.42)
After inheritance × Inherited wealth		0.104	
		(1.29)	
After inheritance × Large inheritance			-0.042
			(-0.82)
Time-varying individual controls	Yes	Yes	Yes
Individual × cohort fixed effects	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes
R^2	0.68	0.68	0.68
Observations	10,705	10,705	10,705

Table 4: Severe losses and serial entrepreneurship

This table reports estimates from the stacked difference-in-differences specification in Equation 3, examining the effect of post-bankruptcy inheritance on the probability of entrepreneurship by the experience of severe losses. The dependent variable, *Entrepreneur*, is an indicator variable equal to one if the individual is an entrepreneur in the year. The primary independent variable, *After inheritance*, is an indicator equal to one for the treated entrepreneur in event years on or after her inheritance year, and zero for every control observation throughout the cohort. In each column, the coefficient on *After inheritance* is the response among entrepreneurs without the indicated severe loss, and the coefficient on the interaction term is the difference in the response for those with it. The interacted variables are defined in Section 4.3: in Column 1, *Severe income loss* is an indicator variable equal to one if the individual experiences negative entrepreneurial income before bankruptcy; in Column 2, *Business debt chapter* is an indicator variable equal to one if the individual files for bankruptcy under the business debt chapter; in Column 3, *Full personal liability* is an indicator variable equal to one if the individual was an unlimited liability company entrepreneur before bankruptcy, and zero if she managed only limited liability companies. The treated and control groups are defined and matched as in Table 1. The sample includes observations from seven years before bankruptcy until six years after. All columns include time-varying individual controls, individual-by-cohort fixed effects, cohort-by-event-year fixed effects, and age-bin fixed effects (dummies for every five-year bin of age). Time-varying individual controls are an indicator for being married and the number of children under the age of 18 living in the household. Regressions are weighted by assigning each treated entrepreneur weight one and each of her k matched controls weight $1/k$, so that each cohort contributes equally. 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	0.075**	0.086**	0.142*
	(2.26)	(2.43)	(1.68)
After inheritance × Severe income loss	-0.108**		
	(-2.04)		
After inheritance × Business debt chapter		-0.126**	
		(-2.46)	
After inheritance × Full personal liability			-0.128
			(-1.44)
Time-varying individual controls	Yes	Yes	Yes
Individual × cohort fixed effects	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes
R^2	0.68	0.68	0.68
Observations	10,705	10,705	10,705

Table 5: Post-bankruptcy wealth and longer-run entrepreneurial outcomes

This table reports estimates from the cross-sectional model in Equation 4, examining the effect of post-bankruptcy inheritance on longer-run entrepreneurial outcomes in the main matched sample. The sample excludes individuals who were entrepreneurs one year before inheritance; for control individuals, the reference year is the inheritance year of the treated entrepreneur in their cohort. It retains individuals who never restart, so the estimates are unconditional on entry. Panel A measures outcomes over the three years after inheritance ($h = 3$) and Panel B over the five years ($h = 5$). The dependent variable is an indicator for being an entrepreneur in any year from the inheritance year through year h (Column 1), an indicator for being an entrepreneur in year h (Column 2), the difference between the two, equal to one for individuals who restart within the window but have exited by year h (Column 3), and cumulative labor and entrepreneurial income from the inheritance year through year h, with zero in years without entrepreneurship, in thousands of 2015 DKK (Column 4). The Column 3 coefficient equals the difference between the Column 1 and 2 coefficients, and its t-statistic tests their equality. *Treated* equals one for entrepreneurs who receive an inheritance after bankruptcy and zero for their matched controls, who do not inherit. Individual controls are wealth measured one year before bankruptcy and the discharge ratio. All columns include cohort fixed effects and age-bin fixed effects defined by age at bankruptcy in five-year bins. Regressions are weighted by assigning each treated entrepreneur weight one and each of her k matched controls weight $1/k$. Standard errors are clustered at the matched-cohort level, and t-statistics based are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Three years after inheritance ($h = 3$)

	Entered by year 3 (1)	Operating at year 3 (2)	Entry–operation gap (1) – (2) (3)	Cumulative entrepreneurial income (4)
Treated	0.010	0.018	-0.009	-35.1
	(0.24)	(0.48)	(-0.71)	(-1.26)
Individual-level controls	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes	Yes
R^2	0.30	0.31	0.22	0.33
Observations	641	641	641	641

Panel B: Five years after inheritance ($h = 5$)

	Entered by year 5 (1)	Operating at year 5 (2)	Entry–operation gap (1) – (2) (3)	Cumulative entrepreneurial income (4)
Treated	0.013	0.000	0.012	-45.9
	(0.27)	(0.01)	(0.56)	(-1.08)
Individual-level controls	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes	Yes
R^2	0.32	0.33	0.21	0.34
Observations	598	598	598	598

Table 6: Severe losses versus low entrepreneurial ability

This table augments the severe-losses specifications in Table 4 with the interaction of *After inheritance* and *Low past performance*; Column 1 includes this interaction alone. The dependent variable, *Entrepreneur*, is an indicator equal to one if the individual is an entrepreneur in the year. *After inheritance* is an indicator equal to one for the treated entrepreneur in event years on or after her inheritance year, and zero for every control observation. *Low past performance* is an indicator equal to one if the individual's labor and entrepreneurial income remained in the bottom decile of its industry-by-year cell in every year of entrepreneurship before bankruptcy, as described in Section 5. Because this measure requires the industry-year cell to contain at least 25 entrepreneurs, the sample is slightly smaller than in Table 4. The three severe-losses measures—*Severe income loss*, *Business debt chapter*, and *Full personal liability*—are defined in Section 4.3 and entered one per column in Columns 2–4. The estimating equation, controls, fixed effects, weighting, and clustering are as in the notes to Table 4. *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
After inheritance	0.047*	0.096**	0.109***	0.480***
	(1.69)	(2.52)	(2.92)	(2.85)
After inheritance × Low past performance	-0.371**	-0.345**	-0.435***	-0.354**
	(-2.47)	(-2.47)	(-2.86)	(-2.35)
After inheritance × Severe income loss		-0.109*		
		(-1.92)		
After inheritance × Business debt chapter			-0.151***	
			(-2.67)	
After inheritance × Full personal liability				-0.456***
				(-2.67)
Time-varying individual controls	Yes	Yes	Yes	Yes
Individual × cohort fixed effects	Yes	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes	Yes
R^2	0.68	0.68	0.68	0.68
Observations	9,750	9,750	9,750	9,750

Internet Appendix to

"Post-Bankruptcy Wealth and Serial Entrepreneurship"

Donghyun Kang[1]

Internet Appendix Contents

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IA.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 of residence. There is no fee associated with filing. Upon submission, the debtor is mandated to provide the court with a comprehensive financial disclosure, 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.[1] In my sample, about 10% of cases are rejected at this stage. The ruling is announced on the State Gazette. The average duration between case opening and ruling is about 9.5 months in my main sample. 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 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.[2] Here, I list other differences between them that are not mentioned in Section 2 of the main text. First, 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. Second, 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. Third, if a debtor

[1] 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.

[2] 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. 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.

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.

IA.2 Data description

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 3,000 issues of the State Gazette from 2006 through 2017. Each issue contains a document index, which I use to locate sections on liquidation and bankruptcy proceedings. (See a sample page in Figure IA.1.) Each section on liquidation (bankruptcy) proceedings contains, on average, 50 (22) notices, which leaves me with a total of about 160,000 (71,000) 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 Figure IA.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 20,000 individuals who receive bank-

ruptcy rulings between 2006 and 2017.[3] 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.[4]

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; Agrawal et al. 2022; Jensen et al. 2022). I extract data from the registers for the years 1980 to 2023. I adjust variables denominated in Danish Kroner (DKK) to the 2015 price level and winsorize them at the 2.5 and 97.5 percentiles for each event 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 IA.3.
3. The Central Business Register (CVR: *Centrale Virksomhedsregister*): The Central Business

[3] 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. Bruze et al. (2024) use the same source to study the long-run effect of personal bankruptcy in Denmark (*gældssanering*), hand-collecting announcements from microfilm through 2005; my sample begins with the digital edition in 2006.

[4] 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.

Register contains firm-level information on all companies in Denmark.[5] 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 2023.

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

IA.3 Identifying inheritances from administrative registers

I outline the procedure for identifying inheritance events. I follow Andersen and Nielsen (2012), 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

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

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).

One potential concern is that some parents may have owned a business at the time of death, in which case the child could inherit a going-concern business rather than purely financial wealth. Such cases would confound the interpretation of inheritances as windfall wealth unrelated to entrepreneurial opportunities. I verify, however, that none of the treated individuals in the final estimation sample inherit a business from their parent, alleviating this concern.

IA.4 Example of the State Gazette of Denmark

Figure IA.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.

Brugerservice, tlf. 70 80 77 80 (man. - tors. kl. 8.30-16, fre. Kl. 8.30-14.30) og redaktionen@statstidende.dk. Redaktionen, tlf. 43 43 33 11 (man. - tors. kl. 9-15, fre. Kl. 9-14) og redaktionen@statstidende.dk. Bogholderi, tlf. 3395 1089 og bogholderi-statstidende@statens-adm.dk.

Indrykning foregår via statstidende.dk og kan ske frem til kl. 07 dagen før offentliggørelse. Deadlines forudsætter fejlfrit indhold/skema. Fristen for mortifikationer og indkaldelse til ejendomsdom offentliggøres på forsiden af statstidende.dk.

Statstidende ISSN 1399-8714

Statstidende hører til Justitsministeriets område og varetages af Civilstyrelsen, tlf.: 33 92 33 34, civilstyrelsen@civilstyrelsen.dk.

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JUSTITSMINISTERIET
CIVILSTYRELSEN

Figure IA.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 Jernbanevej 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 indflydelse 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 endelig.

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 gældssanering og 0,47064% den 01.05.2017.

Sø- og Handelsretten, Skifteretten, den 07.09.2016.

Retskreds
Sø- og Handelsretten

.....

NR. 174 113. Årgang

IA.5 Distribution of inheritance amounts and timing

Figure IA.3: Distribution of inheritance amounts

This figure reports the distribution of inheritance amounts. The top panel shows the distribution of inherited wealth across six mutually exclusive bins: positive amounts up to 10,000 DKK; 10,001–25,000 DKK; 25,001–100,000 DKK; 100,001–250,000 DKK; 250,001–500,000 DKK; and more than 500,000 DKK. Inherited wealth is measured in 2015 DKK (1 Euro $\approx$ 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.

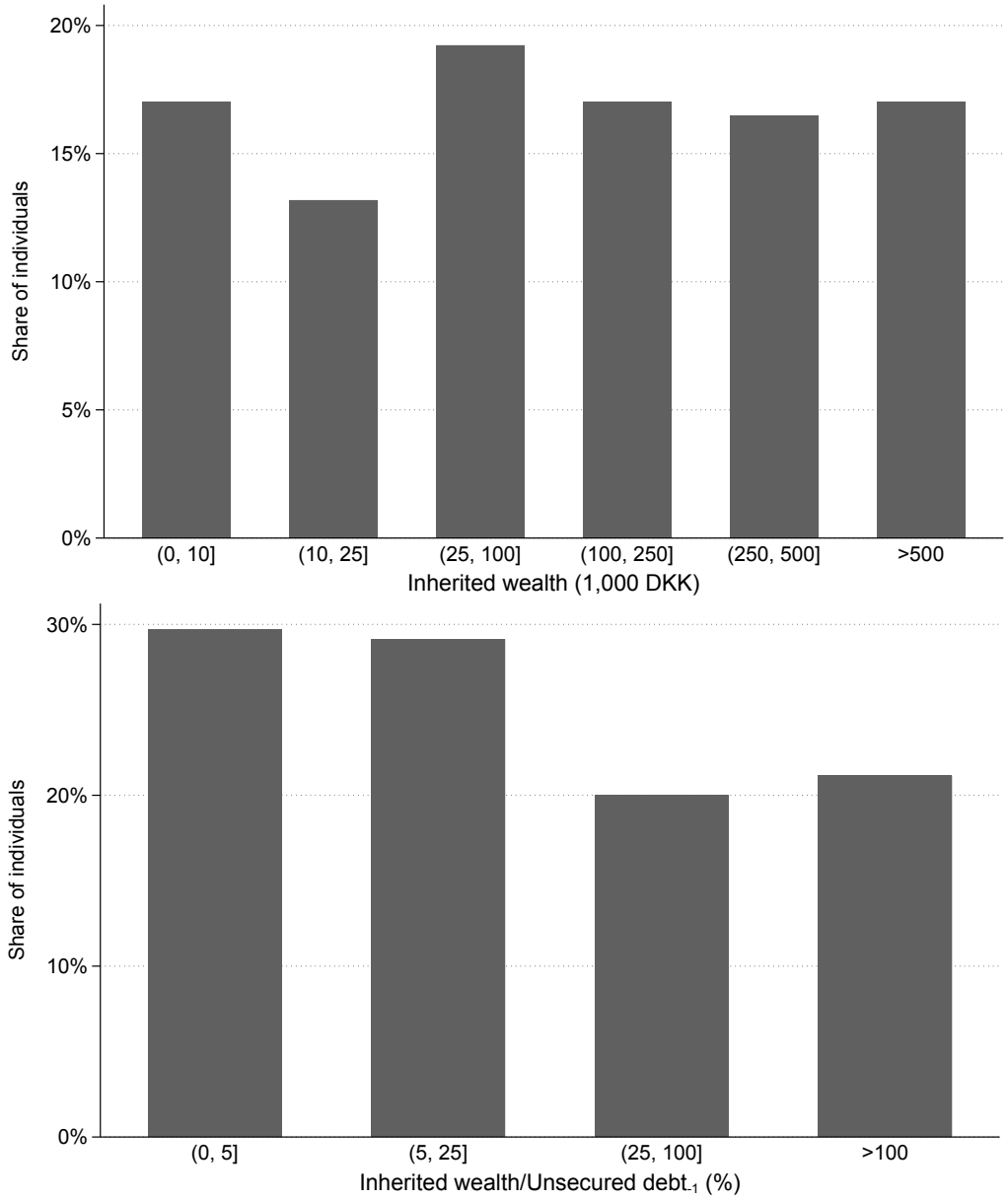
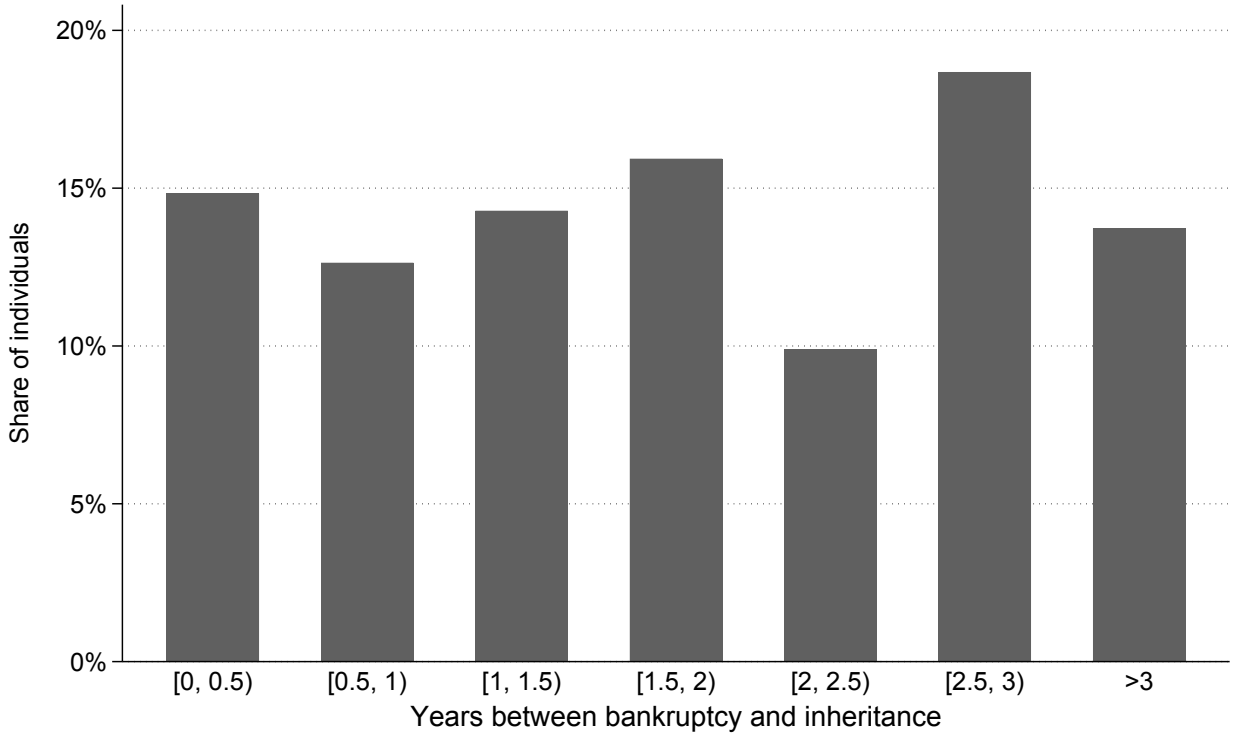


Figure IA.4: Distribution of inheritance events over event time

This figure reports the distribution of inheritance events over event time, grouped into seven six-month bins. The first bin reports the share of treated entrepreneurs who receive an inheritance within six months of the bankruptcy ruling; each subsequent bin covers a further six months.

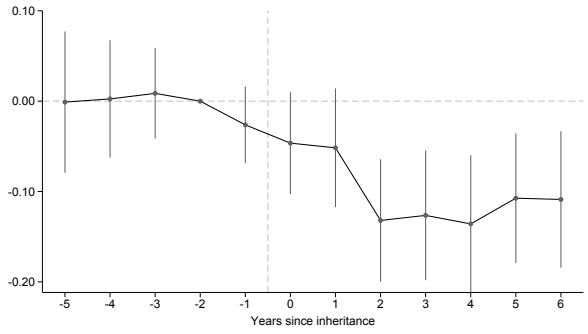


IA.6 Inheritance-centered event study

Figure IA.5: Labor supply and household outcomes around the inheritance event

This figure plots dynamic responses of labor-supply and household outcomes around the inheritance event. The coefficients are estimated with the matched stacked specification in Equation 1, extended by fully interacting inheritance-relative event-year indicators and treatment status. The inheritance year is defined as the year of parental death, so event year 0 denotes both the year of death and the year of inheritance, and matched controls are assigned the inheritance year of the treated entrepreneur in the same cohort. The sample comprises the 162 treated entrepreneurs in the main matched sample who inherit in 2017 or earlier, together with their matched controls. Observations span inheritance-relative years –5 through +6, with the inheritance-year restriction allowing six years of follow-up before the registers end in 2023. Event year –2 is the omitted reference period, rather than year –1, so that the coefficient at event year –1 captures any anticipatory change in the year immediately preceding parental death. All panels exclude the time-varying individual controls, because marital status and the number of children are themselves the outcomes in Panels (c) and (d). All panels include individual-by-cohort, cohort-by-event-year, and age-bin fixed effects, and regressions are weighted by assigning each treated entrepreneur weight one and each of her k matched controls weight $1/k$. Vertical bars report 95% confidence intervals based on standard errors clustered at the individual level. Panels (a) and (b) measure labor supply using contributions to the Danish labor-market supplementary pension (ATP), following Kleven et al. (2019). Panel (a) plots *Employment*, an indicator equal to one if annual ATP contributions are positive. Panel (b) plots *Total labor supply*, a proxy for hours worked measured by annual ATP contributions, with missing or negative values set to zero. Panel (c) plots *Married*, an indicator equal to one if the individual is married or in a registered partnership. Panel (d) plots the *Number of children*.

(a) Employment



(b) Total labor supply

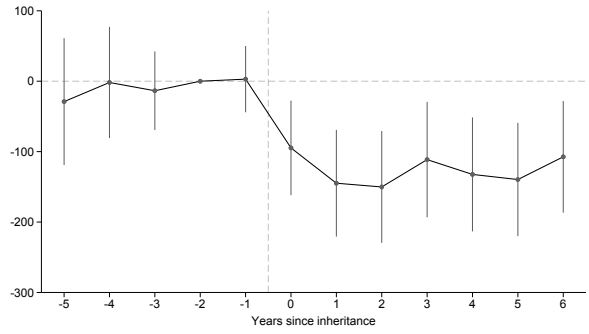
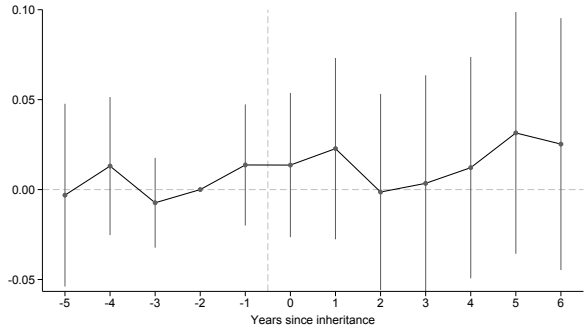


Figure IA.5: Labor supply and household outcomes around the inheritance event (continued)

(c) Married



(d) Number of children

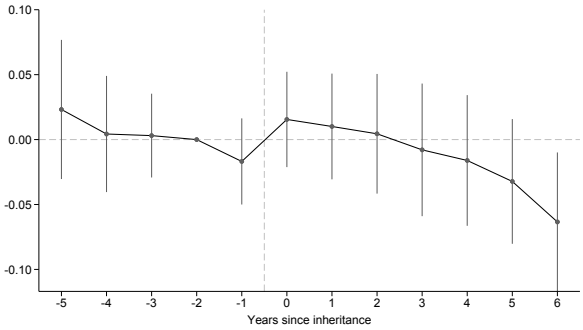


Figure IA.6: Inheritance-centered event study: entrepreneurship

This figure plots dynamic estimates of the entrepreneurship response to inheritance. The dependent variable is an indicator equal to one if the individual is an entrepreneur in the year. The coefficients are estimated with the matched stacked specification in Equation 1, extended by fully interacting inheritance-relative event-year indicators and treatment status. The inheritance year is defined as the year of parental death, so event year 0 denotes both the year of death and the year of inheritance, and matched controls are assigned the inheritance year of the treated entrepreneur in the same cohort. The sample comprises the 162 treated entrepreneurs in the main matched sample who inherit in 2017 or earlier, together with their matched controls. Observations span inheritance-relative years –5 through +6, with the inheritance-year restriction allowing six years of follow-up before the registers end in 2023. Year –1 is the omitted reference period. The specification includes the baseline time-varying individual controls, individual-by-cohort, cohort-by-event-year, and age-bin fixed effects. Each treated entrepreneur receives weight one, and each of the k matched controls receives weight $1/k$. Vertical bars indicate pointwise 95% confidence intervals based on standard errors clustered at the individual level. A joint Wald test that the pre-inheritance coefficients are zero does not reject ($p = 0.72$).

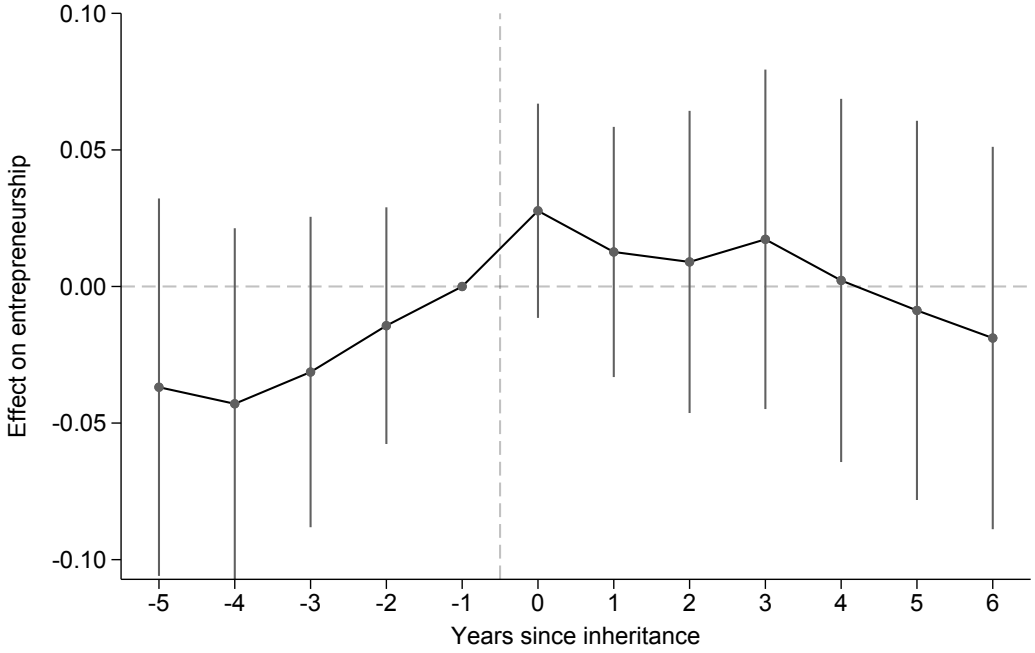
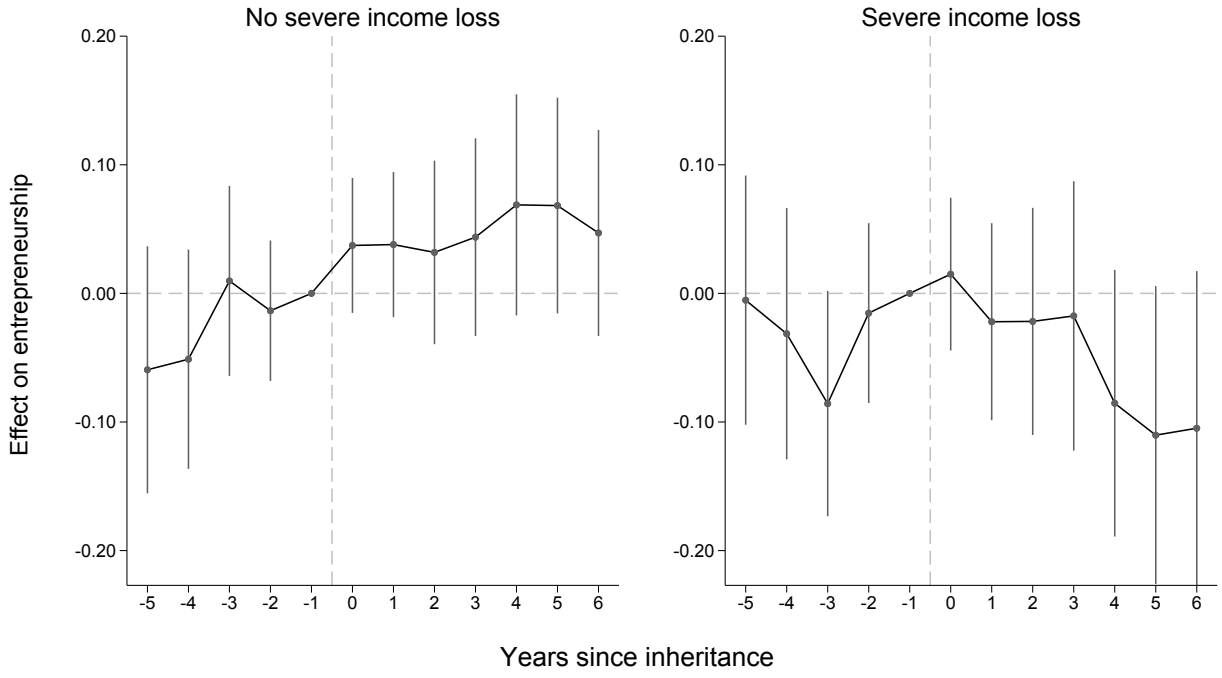


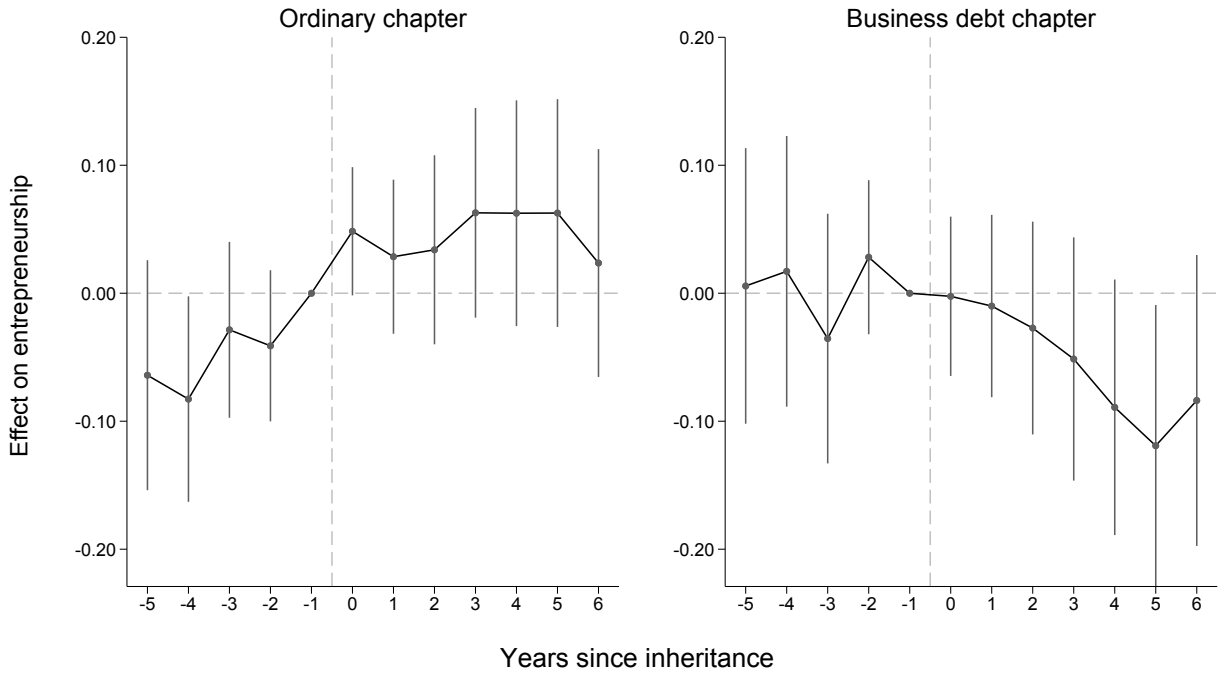
Figure IA.7: Inheritance-centered event study: entrepreneurship by severity of personal losses

This figure reports dynamic estimates of the entrepreneurship response to inheritance by three measures of the severity of personal losses. Within each panel, the left subplot reports estimates for the less-severe-loss subgroup and the right subplot reports estimates for the severe-loss subgroup. Panel (a) distinguishes individuals classified as having no severe income loss from those who experienced negative entrepreneurial income before bankruptcy. Panel (b) compares ordinary chapter filings with business debt chapter filings. Panel (c) compares partial with full personal liability, where full personal liability identifies individuals who managed an unlimited liability company before bankruptcy. For each measure, the matched stacked specification in Equation 1 is extended by fully interacting inheritance-relative event-year indicators, treatment status, and the relevant severity indicator. The left subplot plots the treatment-by-event-year coefficients; the right subplot plots the sums of those coefficients and the corresponding triple-interaction coefficients. Each subplot therefore measures changes in the treated–control difference in the probability of entrepreneurship within the relevant subgroup, relative to year –1. The inheritance year is defined as the year of parental death, and matched controls are assigned the inheritance year of the treated entrepreneur in the same cohort. The sample comprises the 162 treated entrepreneurs in the main matched sample who inherit in 2017 or earlier, together with their matched controls. Observations span inheritance-relative years –5 through +6, with the inheritance-year restriction allowing six years of follow-up before the registers end in 2023. All specifications include the baseline time-varying individual controls, individual-by-cohort, cohort-by-event-year, and age-bin fixed effects. Each treated entrepreneur receives weight one, and each of the k matched controls receives weight $1/k$. Vertical bars indicate pointwise 95% confidence intervals based on standard errors clustered at the individual level. Panel A of Appendix Table IA.1 reports joint tests of the pre-inheritance coefficients plotted here.

(a) Severe income loss



(b) Business debt chapter filing



(c) Full personal liability

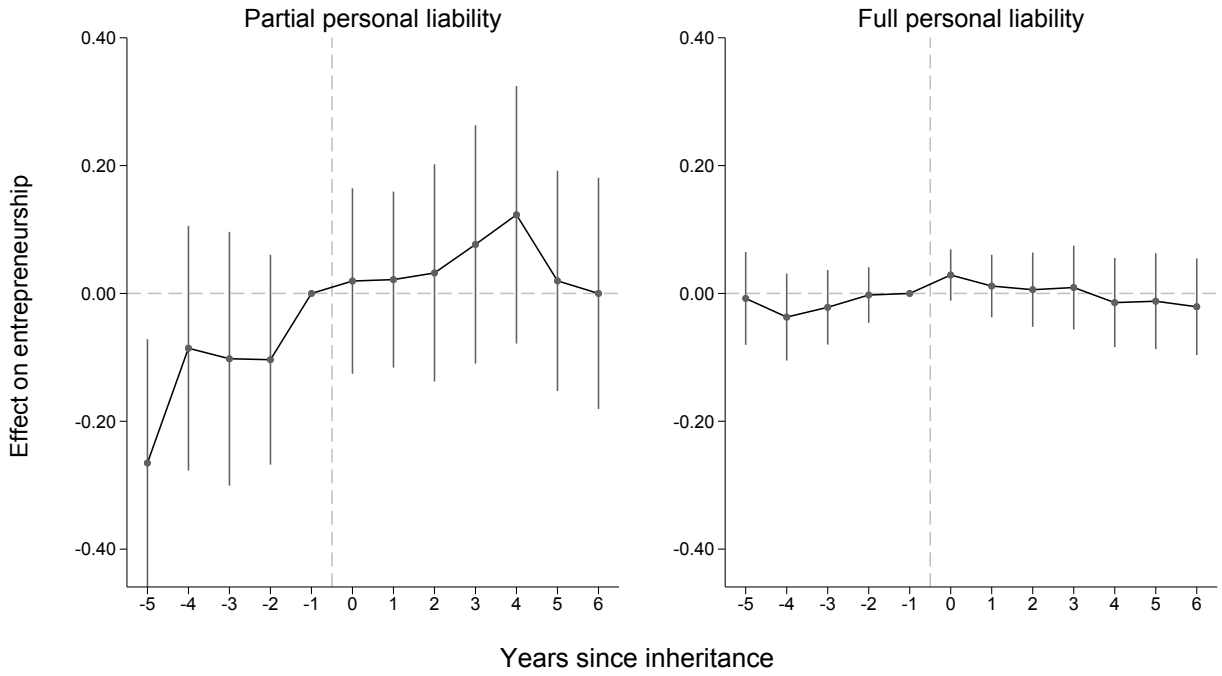


Table IA.1: Inheritance-centered event study: pre-event tests and static versus dynamic estimates

This table reports pre-event tests and static versus dynamic estimates of the entrepreneurship response to inheritance, by three measures of the severity of personal losses; the dynamic coefficients are plotted in Appendix Figure IA.7. The dependent variable is *Entrepreneur*, defined in Table 4. The sample comprises the 162 treated entrepreneurs in the main matched sample who inherit in 2017 or earlier, together with their matched controls. Observations span inheritance-relative years –5 through +6, with the inheritance-year restriction allowing six years of follow-up before the registers end in 2023. The severe-loss subgroup comprises entrepreneurs with severe income losses (Column 1), business debt chapter filings (Column 2), and full personal liability (Column 3); each difference is the severe-loss estimate minus the less-severe-loss estimate. Panel A reports joint Wald tests that the dynamic coefficients for years –5 through –2 are zero, within each subgroup and for the subgroup differences. Panel B reports static estimates for each subgroup and the difference between subgroups, obtained from Equation 3; Panel C reports equally weighted averages of the dynamic coefficients for years 0–6, measured relative to year –1. Within each column, the three panels use identical observations, weights, controls, and fixed effects. In Panels B and C, *t*-statistics based on standard errors clustered at the individual level are in parentheses, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

	(1) Severe income loss	(2) Business debt chapter	(3) Full personal liability
Panel A: Joint pre-period tests (*p*-values)			
Less-severe-loss subgroup	0.385	0.169	0.021
Severe-loss subgroup	0.328	0.501	0.848
Difference between subgroups	0.147	0.112	0.030
Panel B: Static difference-in-differences estimates			
Less-severe-loss subgroup	0.070**	0.089**	0.152
	(2.06)	(2.56)	(1.59)
Severe-loss subgroup	-0.021	-0.056	0.015
	(-0.49)	(-1.37)	(0.57)
Difference between subgroups	-0.091*	-0.145***	-0.137
	(-1.65)	(-2.70)	(-1.38)
Panel C: Average dynamic coefficients, years 0–6			
Less-severe-loss subgroup	0.048	0.046	0.042
	(1.53)	(1.39)	(0.54)
Severe-loss subgroup	-0.050	-0.055	0.001
	(-1.19)	(-1.42)	(0.05)
Difference between subgroups	-0.097*	-0.101**	-0.041
	(-1.86)	(-1.98)	(-0.50)
Time-varying individual controls	Yes	Yes	Yes
Individual × cohort fixed effects	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes
Observations	8,101	8,101	8,101

IA.7 Longer-run entrepreneurial outcomes: additional results

Table IA.2: Post-bankruptcy wealth and longer-run entrepreneurial outcomes: entrepreneurs without severe losses

This table re-estimates Table 5 on the subsamples of entrepreneurs who did not experience severe losses, defined by each of the three indicators in Section 4.3. Each sample row reports the *Treated* coefficient. Specifications, weighting, clustering, and inference are as in the notes to Table 5.

Panel A: Three years after inheritance ($h = 3$)

Sample/group	Entered by year 3 (1)	Operating at year 3 (2)	Entry–operation gap (1) – (2) (3)	Cumulative entrepreneurial income (4)
No severe income loss	0.041	0.046	-0.005	-21.7
	(0.74)	(0.86)	(-0.42)	(-0.55)
Observations	396	396	396	396
R^2	0.29	0.31	0.21	0.34
Ordinary chapter	0.062	0.085	-0.023**	24.3
	(1.13)	(1.60)	(-2.48)	(0.66)
Observations	386	386	386	386
R^2	0.30	0.32	0.23	0.33
Partial personal liability	-0.058	-0.049	-0.009	-172.3
	(-0.40)	(-0.35)	(-0.82)	(-1.54)
Observations	67	67	67	67
R^2	0.49	0.47	0.20	0.50
Individual-level controls	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes	Yes

Panel B: Five years after inheritance ($h = 5$)

Sample/group	Entered by year 5 (1)	Operating at year 5 (2)	Entry–operation gap (1) – (2) (3)	Cumulative entrepreneurial income (4)
No severe income loss	0.062	0.041	0.021	3.3
	(0.98)	(0.70)	(0.81)	(0.05)
Observations	365	365	365	365
R^2	0.31	0.32	0.23	0.35
Ordinary chapter	0.076	0.083	-0.007	22.5
	(1.26)	(1.47)	(-0.28)	(0.41)
Observations	369	369	369	369
R^2	0.31	0.34	0.22	0.38
Partial personal liability	-0.013	-0.059	0.046	-102.7
	(-0.08)	(-0.35)	(1.56)	(-0.43)
Observations	57	57	57	57
R^2	0.50	0.49	0.47	0.43
Individual-level controls	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes	Yes

IA.8 Robustness tests

IA.8.1 Alternative specifications

Table IA.3: Severe income loss and serial entrepreneurship: restricting to prior unlimited liability company entrepreneurs

This table re-estimates the *Severe income loss* specification in Column 1 of Table 4 on the subsample of entrepreneurs who operated an unlimited liability company before bankruptcy. *Severe income loss* is zero by construction for entrepreneurs who managed only limited liability companies, because limited liability keeps business losses inside the company rather than passing them through to personal income. Restricting the sample therefore retains only entrepreneurs for whom negative entrepreneurial income is possible and observed. The definition of severe losses is unchanged. The dependent variable, *Entrepreneur*, is an indicator variable equal to one if the individual is an entrepreneur in the year. The primary independent variable, *After inheritance*, is an indicator equal to one for the treated entrepreneur in event years on or after her inheritance year, and zero for every control observation throughout the cohort. *Severe income loss* is defined in Section 4.3. The treated and control groups, the sample window, the time-varying individual controls, fixed effects, weighting, and clustering are as in the notes to Table 4. *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)
After inheritance	0.055
	(1.60)
After inheritance × Severe income loss	-0.084
	(-1.58)
Time-varying individual controls	Yes
Individual × cohort fixed effects	Yes
Cohort × event-year fixed effects	Yes
Age-bin fixed effects	Yes
R^2	0.68
Observations	9,563

Table IA.4: Severe losses and serial entrepreneurship: excluding time-varying controls

This table reports estimates from the stacked difference-in-differences specifications in Equations 1 and 3, examining the effect of post-bankruptcy inheritance on the probability of entrepreneurship by the experience of severe losses. The dependent variable, *Entrepreneur*, is an indicator variable equal to one if the individual is an entrepreneur in the year. The primary independent variable, *After inheritance*, is an indicator equal to one for the treated entrepreneur in event years on or after her inheritance year, and zero for every control observation throughout the cohort. The three severe-losses measures—*Severe income loss* (Column 2), *Business debt chapter* (Column 3), and *Full personal liability* (Column 4)—are defined in Section 4.3 and entered one per column. The treated and control groups, the sample window, the definition of *After inheritance*, fixed effects, weighting, and clustering are as in the notes to Table 4. The only difference is the exclusion of time-varying individual controls, which are an indicator for being married and the number of children under the age of 18 living in the household. *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
After inheritance	0.029	0.075**	0.080**	0.142*
	(1.12)	(2.24)	(2.24)	(1.70)
After inheritance × Severe income loss		-0.112**		
		(-2.10)		
After inheritance × Business debt chapter			-0.119**	
			(-2.29)	
After inheritance × Full personal liability				-0.131
				(-1.48)
Time-varying individual controls	No	No	No	No
Individual × cohort fixed effects	Yes	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes	Yes
R^2	0.67	0.68	0.68	0.68
Observations	10,705	10,705	10,705	10,705

Table IA.5: Post-bankruptcy wealth before inheritance, by severity of personal losses

This table reports estimates from cross-sectional regressions comparing net wealth in the year of bankruptcy (event year 0) across entrepreneurs with more and less severe personal losses. The sample is drawn from the main matched sample and restricted to observations in the year of bankruptcy, excluding observations at or after inheritance. The dependent variable is net wealth, measured in thousands of 2015 DKK (1 Euro ≈ DKK 7.45). The three measures of severe losses—*Severe income loss* (Column 1), *Business debt chapter* (Column 2), and *Full personal liability* (Column 3)—are defined in Section 4.3 and entered one per column. Each reported coefficient measures the conditional difference in net wealth between entrepreneurs with more and less severe losses. All columns include individual-level controls measured in the year of bankruptcy—an indicator for being married, the number of children under the age of 18 living in the household, and the discharge ratio—as well as calendar-year and age-bin fixed effects. Cohort fixed effects are omitted because matching is exact on the severe-loss indicators, which are constant within each matched cohort and would therefore be absorbed by these fixed effects. Weighting and clustering are as in the notes to Table 2. *t*-statistics are in parentheses.

	Net wealth		
	(1)	(2)	(3)
Severe income loss	13.7		
	(0.22)		
Business debt chapter		-89.6	
		(-1.30)	
Full personal liability			-47.8
			(-0.39)
Individual-level controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes
R^2	0.07	0.07	0.07
Observations	751	751	751

Table IA.6: Inheritance as a source of post-bankruptcy wealth, by severity of personal losses

This table reports estimates from the stacked difference-in-differences specification in Equation 3, examining the effect of post-bankruptcy inheritance on the level of net wealth (measured in thousands of 2015 DKK, where 1 Euro ≈ DKK 7.45) by the experience of severe losses. The three severe-losses measures—*Severe income loss* (Column 1), *Business debt chapter* (Column 2), and *Full personal liability* (Column 3)—are defined in Section 4.3 and entered one per column. The treated and control groups, the sample window, the definition of *After inheritance*, the time-varying individual controls, fixed effects, weighting, and clustering are as in the notes to Table 2. *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
After inheritance	124.0**	177.1***	301.5**
	(2.05)	(3.47)	(2.30)
After inheritance × Severe income loss	81.3		
	(0.77)		
After inheritance × Business debt chapter		-45.9	
		(-0.42)	
After inheritance × Full personal liability			-166.7
			(-1.16)
Time-varying individual controls	Yes	Yes	Yes
Individual × cohort fixed effects	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes
R^2	0.89	0.89	0.89
Observations	10,705	10,705	10,705

Table IA.7: Severe losses and serial entrepreneurship, by inheritance amount

This table extends the heterogeneity analysis of Table 4 by allowing the difference in responses to vary with the size of the inheritance. I estimate the stacked difference-in-differences specification of Equation 2, additionally interacting each measure of severe losses with *Inherited wealth*$_i$. The dependent variable, *Entrepreneur*, is an indicator variable equal to one if the individual is an entrepreneur in the year. *Inherited wealth*$_i$ is the amount of inherited wealth received by treated entrepreneur i, measured in millions of 2015 DKK, and is zero for control entrepreneurs, as in Table 3. The three severe-losses measures—*Severe income loss* (Column 1), *Business debt chapter* (Column 2), and *Full personal liability* (Column 3)—are defined in Section 4.3 and entered one per column. The treated and control groups, the sample window, the definition of *After inheritance*, the time-varying individual controls, fixed effects, weighting, and clustering are as in the notes to Table 4. In each column, the triple-interaction term *After inheritance* × *Severe losses* × *Inherited wealth* captures whether a larger windfall narrows the difference in responses between entrepreneurs with more and less severe losses. *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
After inheritance	0.073*	0.076*	0.187
	(1.75)	(1.66)	(1.61)
After inheritance × Inherited wealth	0.014	0.042	-0.154
	(0.16)	(0.45)	(-0.76)
After inheritance × Severe income loss	-0.166**		
	(-2.48)		
After inheritance × Severe income loss × Inherited wealth	0.252		
	(1.43)		
After inheritance × Business debt chapter		-0.159**	
		(-2.42)	
After inheritance × Business debt chapter × Inherited wealth		0.166	
		(0.86)	
After inheritance × Full personal liability			-0.205*
			(-1.70)
After inheritance × Full personal liability × Inherited wealth			0.301
			(1.36)
Time-varying individual controls	Yes	Yes	Yes
Individual × cohort fixed effects	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes
R^2	0.68	0.68	0.68
Observations	10,705	10,705	10,705

Table IA.8: Severe losses and serial entrepreneurship, by above-median inheritance

This table extends the heterogeneity analysis of Table 4 by allowing the difference in responses to vary with the size of the inheritance. I estimate the stacked difference-in-differences specification of Equation 2, additionally interacting each measure of severe losses with *Large inheritance*$_i$. The dependent variable, *Entrepreneur*, is an indicator variable equal to one if the individual is an entrepreneur in the year. *Large inheritance*$_i$ is an indicator equal to one if individual i receives an above-median inheritance, and zero for control entrepreneurs, as in Table 3. The three severe-losses measures—*Severe income loss* (Column 1), *Business debt chapter* (Column 2), and *Full personal liability* (Column 3)—are defined in Section 4.3 and entered one per column. The treated and control groups, the sample window, the definition of *After inheritance*, the time-varying individual controls, fixed effects, weighting, and clustering are as in the notes to Table 4. In each column, the triple-interaction term *After inheritance* × *Severe losses* × *Large inheritance* captures whether a larger windfall narrows the difference in responses between entrepreneurs with more and less severe losses. *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
After inheritance	0.094*	0.100*	0.201
	(1.95)	(1.85)	(1.40)
After inheritance × Large inheritance	-0.039	-0.028	-0.112
	(-0.59)	(-0.39)	(-0.64)
After inheritance × Severe income loss	-0.107		
	(-1.44)		
After inheritance × Large inheritance × Severe income loss	0.002		
	(0.02)		
After inheritance × Business debt chapter		-0.108	
		(-1.49)	
After inheritance × Large inheritance × Business debt chapter		-0.038	
		(-0.38)	
After inheritance × Full personal liability			-0.170
			(-1.15)
After inheritance × Large inheritance × Full personal liability			0.079
			(0.43)
Time-varying individual controls	Yes	Yes	Yes
Individual × cohort fixed effects	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes
R^2	0.68	0.68	0.68
Observations	10,705	10,705	10,705

Table IA.9: Severe losses and serial entrepreneurship: controlling for pre-bankruptcy industry

This table re-estimates the severe-losses specifications in Table 4, adding pre-bankruptcy-industry-by-event-year fixed effects. The dependent variable, *Entrepreneur*, is an indicator equal to one if the individual is an entrepreneur in the year. *After inheritance* is an indicator equal to one for the treated entrepreneur in event years on or after her inheritance year, and zero for every control observation. The three severe-losses measures—*Severe income loss*, *Business debt chapter*, and *Full personal liability*—are defined in Section 4.3 and entered one per column. Pre-bankruptcy industry is the industry (based on the two-digit code from NACE Rev. 2) of the individual's pre-bankruptcy business; interacting it with event year absorbs any restart trajectory common to an industry at a given event year. The estimating equation, controls, remaining fixed effects, weighting, and clustering are as in the notes to Table 4. Adding these fixed effects drops a small number of singleton industry-by-event-year cells, which provide no within-cell variation for identification. *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
After inheritance	0.068**	0.084**	0.135
	(2.02)	(2.31)	(1.58)
After inheritance × Severe income loss	-0.098*		
	(-1.80)		
After inheritance × Business debt chapter		-0.125**	
		(-2.38)	
After inheritance × Full personal liability			-0.125
			(-1.38)
Time-varying individual controls	Yes	Yes	Yes
Individual × cohort fixed effects	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes
Pre-bankruptcy industry × event-year fixed effects	Yes	Yes	Yes
R^2	0.71	0.71	0.71
Observations	10,593	10,593	10,593

IA.8.2 Alternative sample matched on expected inheritance

Table IA.10: Alternative sample matched on expected inheritance: severe losses and serial entrepreneurship

This table reproduces Table 3 (Column 1) and Table 4 using an alternative matched sample. The sample is constructed exactly as the main sample, except that expected inheritance is added to the matching criteria and matches differing in expected inheritance by more than 250,000 DKK are excluded, as described in Section 5. The dependent variable, *Entrepreneur*, is an indicator equal to one if the individual is an entrepreneur in the year. Column 1 reports the baseline specification (Equation 1); Columns 2–4 interact *After inheritance* with each of the three severe-losses measures (Equation 3), defined in Section 4.3. All variable definitions, controls, fixed effects, weighting, and inference are as in the notes to Tables 3 and 4. *t*-statistics are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
After inheritance	0.034	0.100**	0.109**	0.306***
	(1.04)	(2.38)	(2.49)	(2.94)
After inheritance × Severe income loss		-0.166**		
		(-2.53)		
After inheritance × Business debt chapter			-0.178***	
			(-2.79)	
After inheritance × Full personal liability				-0.308***
				(-2.81)
Time-varying individual controls	Yes	Yes	Yes	Yes
Individual × cohort fixed effects	Yes	Yes	Yes	Yes
Cohort × event-year fixed effects	Yes	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes	Yes
R^2	0.68	0.68	0.68	0.68
Observations	7,498	7,498	7,498	7,498

Table IA.11: Alternative sample matched on expected inheritance: longer-run entrepreneurial outcomes

This table reproduces Table 5 using an alternative matched sample. The sample is constructed exactly as the main sample, except that expected inheritance is added to the matching criteria and matches differing in expected inheritance by more than 250,000 DKK are excluded, as described in Section 5. Specifications, weighting, clustering, and inference are as in the notes to Table 5.

Panel A: Three years after inheritance ($h = 3$)

	Entered by year 3 (1)	Operating at year 3 (2)	Entry–operation gap (1) – (2) (3)	Cumulative entrepreneurial income (4)
Treated	0.030	0.027	0.003	-21.5
	(0.66)	(0.62)	(0.20)	(-0.66)
Individual-level controls	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes	Yes
R^2	0.38	0.40	0.29	0.40
Observations	435	435	435	435

Panel B: Five years after inheritance ($h = 5$)

	Entered by year 5 (1)	Operating at year 5 (2)	Entry–operation gap (1) – (2) (3)	Cumulative entrepreneurial income (4)
Treated	0.025	0.001	0.025	-21.0
	(0.48)	(0.01)	(1.04)	(-0.40)
Individual-level controls	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
Age-bin fixed effects	Yes	Yes	Yes	Yes
R^2	0.37	0.38	0.33	0.38
Observations	401	401	401	401