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The Long-Run Effects of Individual Debt Relief

Abstract

Individuals with extensive debt may be granted debt relief in court. We provide a comprehensive evaluation of the Danish debt relief program with data from court records linked to nationwide register data. Using event-study methods and quasi-random assignment of applicants to court trustees with varying admission rates, we show that debt relief leads to a large increase in earned income, employment, assets, real estate, secured debt, home ownership, and wealth that persists for more than 25 years after a court ruling. The net transition of workers into employment accounts for two thirds of the increase in earned income.

JEL-Codes: D140, D310, K350.

Keywords: debt relief, personal bankruptcy, household finance.

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1 Introduction

Many governments provide debt relief programs that allow granted applicants to reduce their unsecured debt. Debt relief programs are motivated by a concern that high levels of debt are self-reinforcing, induce economic distress, and lead to a poor quality of life (World Bank, 2013). Since individuals with large debt often pay a substantial fraction of their income to creditors (similar to a tax), there is also a presumption that reducing debt raises the incentives for debtors to improve their economic situation (debt relief provides a fresh start).

Despite the wide availability of debt relief programs for several decades now, there is limited evidence on the impact of these programs on debtors. The simple reason is a shortage of records on applicants for debt relief and a limited ability to combine such records with panel data that tracks individuals over time. In this paper, we overcome previous data constraints by hand-collecting data on the universe of granted and non-granted applicants for debt relief in Denmark from 1984 to 2003 and linking these records to nationwide Danish registers.

An important contribution of our paper is that we provide a long-run evaluation of an under-studied debt relief program using administrative register data. In our main analysis, we follow applicants for 16 years after a court ruling but we also conduct subgroup analysis where we extend the follow-up period to 34 years. Applicants who are granted debt relief in Denmark typically repay a small part of their debt during 5 years, meaning that we study applicants far beyond the repayment period. A longer follow-up period provides a more complete program evaluation and helps us understand if debt relief gives permanent help to debtors, or if debtors eventually fall back into old habits such as excessive consumption and accumulation of unsustainable debt. A comparison of short- and long-run effects (during and after the repayment period) is also informative about the incentive structure of debt relief programs, and is relevant for policy questions regarding the optimal design of relief programs (e.g. repayment conditions).

A second contribution of our paper is that we describe the impact of debt relief on a wide range of measures related to labor market outcomes and financial status. We evaluate the impact of debt relief on assets, home ownership, real estate, and wealth, and we also use comprehensive data on secured and unsecured debt from nationwide registers covering all interest-bearing loans in banks and other financial institutions. In addition, we study labor market outcomes such as earned income, employment status, and public income support. We find that debt relief increases earned income, and our combined data on income, debt, assets, real estate, and wealth reveals that much of this increase in income translates into an accumulation of assets and wealth. A large fraction of the increase in assets can be accounted for by home ownership and real estate which is partly financed through an increase in secured debt (applicants who are granted debt relief regain access to mortgage loans).

We start our empirical analysis by examining the income and wealth trajectories of applicants for debt relief, and by performing event-study regressions that compare applicants who were granted versus denied debt relief. Visual inspection of the event-study graphs shows that there are large changes in outcomes for granted versus denied applicants at the time of a court decision. The mean outcomes of granted and denied applicants also suggest that the impact of debt relief arises primarily due to improvements for granted applicants.

To improve the identification of causal effects, we estimate the impact of debt relief using an IV design based on the quasi-random assignment of applicants for debt relief to court trustees with varying admission rates. In the Danish system, the trustee serves as an impartial assistant to the court and prepares the debt relief case for final assessment by the judge. As a consequence, the trustee can indirectly affect the verdict of the court through e.g. individual differences in the interpretation and implementation of admission requirements. Consistent with such heterogeneity, we observe large variations in admission rates across trustees within the same court and year of application. We conduct several tests of our instrument and show that the observable characteristics of applicants for debt relief do not predict the admission rates of trustees handling applicant cases (conditional independence of the instrument). We also find that the admission rate of trustees (the instrument) is not correlated with the repayment terms of granted applicants, suggesting that our IV results are driven by exogenous entry into debt relief as opposed to variations in the conditions under which applicants receive debt relief.

Before describing our main results, we briefly discuss how debt relief may affect granted applicants. A key policy concern is that individuals with very large debt have weak incentives to work (Dobbie and Song (2015)), since future payments to creditors act as an (implicit) tax on the earned income of debtors (sometimes referred to as debt overhang, see Bernstein (2021); Donaldson, Piacentino and Thakor (2019)). The more additional income the debtor receives, the larger will be the demands from creditors, similar to a tax that rises with income. Reducing this high implicit tax rate through debt relief should increase the labor supply and earnings of granted applicants (a substitution effect). Debt relief also improves the financial status of granted applicants and should allow them to accumulate assets and gain access to mortgage loans. Counteracting these potentially favorable gains from debt relief is the concern that granted applicants will eventually fall back into excessive debt accumulation, that debt relief reduces labor supply and earnings through a wealth effect, and that the labor demand for granted applicants is low due to e.g. their credit history. Ultimately, it is therefore an empirical question whether there are long-run effects from debt relief.

Our results show that individuals who are granted debt relief have 26% higher earned income, are 11.7 percentage points more likely to be employed, are 12.2 percentage points less likely to be out of the labor force, are 25 percentage points more likely to own a house or an apartment, accumulate more assets corresponding to an increase of about 200% of the follow-up mean, have less unsecured debt, have more secured debt, and accumulate more wealth relative to debtors who are denied debt relief. The impact of debt relief is highly persistent and is present in our data for at least 34 years after a court decision. About two-thirds of the increase in earned income for granted applicants can be attributed to a net transition of workers who are out of the labor force into employment. This decomposition suggests that high levels of debt discourage individuals from entering the labor market.

Debt relief programs are often divided into Anglo-Saxon programs (the US and the UK) and more recent programs in Continental Europe. Among the Continental European countries, Denmark has the oldest debt relief program (Niemi-Kiesilainen (1999)). Debt relief in the US is considered more debtor-friendly than in Continental Europe (Ramsay (2012)) where admission requirements are stricter and relief programs primarily target individuals who have been exposed to a series of unfortunate events. In the US, out-of-pocket medical expenses are considered pivotal in about a quarter of personal bankruptcies among low income households (Gross and Notowidigdo, 2011). See also e.g. Keys (2018); Indarte (2023) for other evidence on the selection into personal bankruptcies in the US. In Denmark, universal health care coverage implies that direct health care costs are not a major cause of applications for debt relief. In the US, indebted individuals can choose to apply for Chapter 7 or Chapter 13 bankruptcy, whereas Denmark and most other Continental European countries offer a single debt relief program with no applicant choice. Finally, unsecured debt in Continental Europe is seldomly fully discharged. Individuals who are granted debt relief often repay a small (fixed) part of their debt during a fixed period (typically 5 years). After the repayment period, debtors are free of all unsecured debt included in the relief program (Niemi-Kiesilainen (1999)).

The best available evidence on the impact of debt relief is found in a series of papers that use quasi-random assignment of judges to court cases to estimate the effect of receiving Chapter 13 bankruptcy protection in the US relative to the best outside option (no protection or protection from Chapter 7 bankruptcy). In a first paper, Dobbie and Song (2015) find that 5 to 10 years after a court ruling, Chapter 13 bankruptcy leads to higher earnings, lower mortality, and a lower foreclosure rate. In additional work, Dobbie, Goldsmith-Pinkham and Yang (2017) use data from a credit bureau and show that debt relief decreases the likelihood of future financial distress and increases the likelihood of retaining a mortgage. Other related studies include Gross, Notowidigdo and Wang (2020), Musto (2004) and Dobbie et al. (2020) who analyze the consequences of bad credit reports (bankruptcy flags) and find that the removal of such reports leads to an increase in available credit and borrowing, Di Maggio, Kalda and Yao (2019) who study the effects of debt relief for student loans, and Indarte (2022) who presents an overview of recent papers on the costs and benefits of household debt relief. Our study broadens the available evidence on the impact of debt relief by providing longer follow-up, additional outcomes based on nationwide administrative register data (assets, home ownership, real estate, and wealth) and a new counterfactual (due to institutional differences across countries) that compares applicants who are granted versus denied debt relief in court.

Our paper is also related to a literature in quantitative macroeconomics that analyzes the aggregate implications of debt relief and evaluates the costs and benefits of debt relief programs in equilibrium models (e.g. Livshits, MacGee and Tertilt (2007), Chatterjee et al. (2007), Mitman (2016), and Auclert, Dobbie and Goldsmith-Pinkham (2019)). Among the benefits that have been studied are the ability of debtors to smooth consumption across states and time. Costs that have been analyzed include a reduction in credit supply and moral hazard among debtors.

The rest of this paper is structured as follows. Section 2 presents the institutional setting for debt relief in Denmark. Section 3 describes our data collection and the database that we create. Section 4 presents estimates from our event-study and IV regressions, and Section 5 concludes.

2 Institutional Setting

2.1 Background

In 1977, the Danish government appointed a committee which proposed a new law on debt relief (Danmarks Justitsministerie, 1982). Two years later, Danish parliament approved a slightly modified version of the law which came into effect on July 1st, 1984. At that time, Denmark was the only country in Continental Europe with a legal procedure for debt relief (the UK and the US already had such procedures).

The commission noted that Denmark had a legal procedure for adjusting secured debt when the debtor owned assets (personal bankruptcy), but lacked a procedure for adjusting unsecured debt held by debtors with no major assets. The commission argued that debtors with unsustainable debt, creditors in the financial industry, and the government would all benefit from a law on debt relief. Debtors would be allowed a fresh start, creditors would not devote resources to collect payments from debtors who could never repay all their debt and would instead get a share of outstanding debt back through partial repayment. In addition, the government would not have to pay benefits to debtors who received public income support because of high interest and debt payments.

According to the commission, the prospect that every improvement in the financial situation of the debtor above the subsistence level will only benefit creditors, implies that the debtor is in a hopeless financial situation, with a potential strong and negative impact on the debtor and his or her family. These financial circumstances will often trigger apathy, resentment, self-blame, and/or a feeling of inferiority and failure. As a consequence, the debtor no longer tries to improve his or her situation. The debtor lacks the motivation to hold on to an existing job, to find a new job (if unemployed), or to start a new business, and will often try to support him- or herself through government benefits. According to the commission, there are strong humanitarian arguments for helping overburdened debtors and their families get out of an unsustainable economic situation. The commission acknowledged that a law on debt relief could lead to some debtors taking on more debt with little regard for future risks, but concluded that the gains from the law outweighed the costs associated with such moral hazard.

2.2 Law on debt relief

The 1984 regulations on debt relief in Denmark[1] were introduced as new chapters in the existing Danish Bankruptcy Law (Konkursloven). The law states that there are two requirements that have to be met for an individual to receive debt relief. The first requirement is that the debtor can show that he or she is unable to repay the debt today and in the foreseeable future. The second requirement is that the personal circumstances of the debtor speak in favor of granting debt relief. The law provides few details about these requirements, but the government commission discussed how the requirements should be interpreted.[2]

[1]Lov 1984-05-09 nr 187 om ændring af konkursloven, gældsbrevsloven og lov om retsafgifter (Law 1984-05-09 nr 187 about changes in the Bankruptcy Law, the Debt Instruments Law, and the Law on Court Fees).

[2]The law was intentionally vague when specifying admission requirements to allow courts to make case-by-case assessments of applicants, implicitly acknowledging that debt relief was "new territory" and court practice had to develop along the way (Kilborn (2009)). This decentralized approach of implementing the law may be *one* source of the variation in admission rates across trustees that we exploit in our empirical design.

Concerning the debtor being unable to repay the debt today or in the future, the commission wrote that the debt should be sufficiently large and that the current and future economic situation of the debtor should prevent repayment of the debt. Typical conditions under which debt relief will not be granted is if the debt is small or if the debtor is only temporarily in financial hardship with better economic conditions expected in the future, for example due to temporary unemployment. The value of assets is expected to be low for applicants, who should have used existing funds to pay down outstanding debt. If applicants nevertheless own significant assets, they generally do not qualify for debt relief. Concerning personal circumstances, the manner in which the debt was acquired, the age of the debt, and the stability of the debtor's economic situation are relevant factors for decisions on debt relief. Debtors who have ended up with large debt due to a business that failed in a recession or due to a general fall in housing prices, have a more justifiable case for receiving debt relief than debtors who have acquired debt to finance extensive private consumption. Debtors with older debt who have tried to repay their debt for a long time also have more valid reasons for debt relief than debtors who recently acquired debt. A stable economic situation with little uncertainty regarding the debtor's income and expenditures also speaks in favor of debt relief.

A debtor applies for debt relief in the local City Court. In general, applicants do not pay a fee (costs that arise during the legal process are borne by the government). The applicant must declare all his or her assets and liabilities and all sources of household income. The court calls the applicant to a first meeting to collect more information and to verify that the information provided by the applicant is correct. The court then makes a decision to dismiss the application or to initiate an investigation about debt relief. According to the law, an investigation is only initiated if there is a reasonable chance that the application will be successful. If an investigation is initiated, the court will typically appoint a court trustee ("medhjælper" in Danish), often a private lawyer, who will serve as an assistant to the court throughout the legal process.

The court trustee collects additional information from the applicant and prepares a repayment plan for a part of the applicant's unsecured debt. When the plan is ready, the applicant and the court trustee are called to a public hearing where the trustee presents the repayment plan. The creditors are also invited to the hearing and can pose questions to the applicant, but they often choose not to be present. After the hearing, the court decides whether or not the applicant is granted debt relief, and if so, what percentage of the debt the applicant should repay and over what period of time. A granted applicant is responsible for making payments according to the repayment plan to a designated bank account and the trustee transfers these payments to the creditors in proportion to the debt that is owed to them (all creditors are as a general rule treated equally regardless of the type of debt). Importantly, once the court has made its decision, the nominal repayment plan is *fixed* and repayments do not vary as a function of the debtor's future income.

The court can remove all or part of the *unsecured* debt of the debtor at the time of the application for debt relief. Secured debt (e.g. a mortgage) is as such not affected by an applicant receiving debt relief. There is no legal restriction on how long the repayment period must be, but many debtors who are granted debt relief repay remaining unsecured debt over a period of five years. There is also no restriction on how many times an individual can apply for debt relief. If the applicant does not follow the repayment plan or if it is discovered that the applicant provided false/inaccurate information during the application process, the court can revoke its decision on debt relief at any later point in time. The creditors may also appeal a decision on debt relief to a higher court, which can modify or cancel the previous decision from the City Court. In practice, appeals and modified verdicts are rare (fewer than 1% of granted applicants in our data have their debt relief revoked).

2.3 Creditor rights in Denmark

Danish creditors have different ways of collecting unsecured debt from debtors who do not fulfill their obligations. A creditor can approach the debtor directly or through a debt collection agency to secure payments and negotiate a repayment plan. If the debtor fails to make payments or does not acknowledge the debt, the creditor can also initiate a debt collection process in the Court of Bailiffs. In court, the creditor can petition to have the debtor placed under personal bankruptcy. If the court decides in favor of bankruptcy, the debtor's assets are liquidated and the proceeds are distributed to the creditors. The creditor can also ask for a bailiff to enter the debtor's home and secure assets that can be sold off to cover the debt. Claims on the debtor's assets (e.g. real estate, cars, large savings, valuable furniture, or future inheritances) can be liquidated as long as the debtor can sustain living conditions at or above the legal poverty level.[3]

Public creditors have additional privileges and may use wage garnishments to retain a portion of the debtor's earnings and public transfers in order to settle outstanding public debt. During our study period, wage garnishments could not exceed 20% of the debtor's income and the debtor was allowed to keep income necessary to sustain living conditions at the legal poverty level. Public institutions in Denmark have their own bailiff system and do not have to appeal to the Court of Bailiffs to make claims on the debtor's assets.[4]

The Danish rules generally allow credit scoring companies to keep information about the opening of an investigation into debt relief for a maximum of 5 years for non-granted applicants, and up until the end of the repayment period for granted applicants. For granted applicants who receive a full discharge of debt, credit scoring companies must immediately remove all information about the debt relief process (including information on discharged debt). As a consequence, creditors who are contacted by these granted applicants after the court verdict will not know that the applicants recently received debt relief.

3 Data

The Danish law on debt relief stipulates that the City Court has to make a public announcement in the newspaper Statstidende if and when an investigation into debt relief is initiated, if and when the applicant and the creditors are called to a hearing at the court, and if and when the court decides to grant debt relief to an applicant. An announcement about an applicant who is granted debt relief

[3] An application for debt relief may be preceded by a personal bankruptcy (a forced liquidation of the debtor's assets in court). The bankruptcy procedure enables creditors with secured debt to obtain as much of their debt as possible. Since loans are full recourse in Denmark, unrealized remaining claims are then converted to unsecured debt that may be discarded if the individual applies for and is granted debt relief.

[4] Danish law permits public agencies to remit a part of public debt. According to the Danish Withholding Tax Act and the VAT Act, debt remission can primarily be granted if a debtor owes the public unpaid taxes and toll fees only, but not if the debtor also has other public debt or debt to private creditors (see https://www.retsinformation.dk/eli/mt/1995/91 (in Danish) for more information).

has to specify the percentage reduction of the applicant's unsecured debt.

3.1 Data collection Statstidende

All issues of Statstidende are stored on microfilm in the Danish Royal Library. We extracted information from announcements about debt relief, from the start of the debt relief program July 1st 1984 until October 14th 2005 when there is a change in the way data is stored. This process gave us an initial database with 150,944 announcements listing the date, court, type of announcement, name and address of the applicant, and the name of the appointed court trustee.

We then merged our data on applicant names and addresses with the Danish Central Person Register (CPR) which contains the current and historical names and addresses of all Danish residents together with their unique personal identification number. More details about the matching procedure can be found in the Online Appendix 3.1. Overall, we were able to identify a unique person in the Danish Central Person Register for 97.0% of the announcements in Statstidende, corresponding to an initial sample of 49,306 individuals in the Danish population.[5]

We divided all individuals in our sample into those who were granted or denied debt relief. We classify individuals as granted if we have an announcement of this type in our database. We refer to the year when the City Court publishes a first announcement about an applicant as the year of application. We restrict our final sample to applicants from the start of the debt relief program in 1984 up until 2003, in order to have sufficient follow-up time after an application to capture later announcements granting applicants debt relief. This process results in a final sample with 46,571 individuals handled by 71 different City Courts (see Table A.2 in the Online Appendix). In the final sample, the median time for granted applicants from a first announcement in Statstidende to the granting of debt relief is 7 months (the 90th percentile is 1 year and 9 months).

Statistics Denmark and the Courts of Denmark ("Danmarks Domstole") publish official statistics on the number of court cases for debt relief in Denmark (see Table A.3 in the Online Appendix).

[5] There are typically several announcements for each applicant (investigations, hearings, granted applications, and withdrawn applications).

For the period from 1988 to 2003 when data is available, the number of granted cases of debt relief in Denmark was 32,565 according to official statistics. In our hand-collected database, we have 31,768 unique individuals who were granted debt relief during that same period (97.6% of the cases in the official statistics). The similarity of these two numbers suggests that our database includes nearly all investigated applicants for debt relief in Denmark from 1984 to 2003.

3.2 Register data

We link our full sample to several nationwide Danish registers and use the register data to describe the background and outcomes of applicants for debt relief. We briefly describe the most important variables in our study and how we construct them. A complete list of the outcome variables and their definitions is available in Table A.4 in the Online Appendix.

We create a measure of earned income which includes earnings for employed individuals and business income for individuals who are self-employed.[6] We also classify individuals as being employed (by a firm or self-employed), unemployed, or out of the labor force based on their employment status in November each year. The Danish Income Tax Register contains yearly information on the taxable wealth holdings of all Danish residents, which originates from the period when Denmark had a wealth tax. Due to the tax, banks and other financial institutions had to report the taxable assets and taxable debt of Danish residents directly to the tax authorities. The measure of taxable assets in the Tax Register includes holdings of bank deposits, bonds, and stocks in banks and other financial institutions, as well as the official government appraisal of each Danish resident's real estate. From this appraisal, we create a dummy variable describing whether or not an individual owns some real estate. The measure of taxable debt in the Tax Register includes interest-bearing mortgages and other secured debt as well as interest-bearing unsecured debt in banks and other financial institutions (Leth-Petersen (2010)).[7] The measure of taxable debt

[6]Monetary numbers are CPI-adjusted throughout the paper to 2020 DKK (1 USD was approximately 6.5 DKK in 2020). We also use a measure of hourly wages calculated by Statistics Denmark. By construction, our results for wages are conditional on employment. Selection into employment for applicants who receive debt relief could affect changes in wages and the results for wages should therefore be interpreted carefully.

[7]Since the information is collected for tax purposes we do not have separate information on the type of loans,

does not include all types of debt to public institutions (e.g. unpaid taxes), and does not include debt between private individuals. There is no simple way of decomposing total taxable debt into subcategories that are consistent over our whole sample period, but we can construct subcategories for taxable secured debt in banks and other financial institutions (mortgages and other secured debt) and taxable unsecured debt in banks and other financial institutions (see Table A.4 in the Online Appendix). Henceforth, we refer to these two variables as secured and unsecured debt.

The majority of the register variables that we use are available in our linkage on an annual basis from 1980 to 2019, giving us at least 4 years of data on all applicants prior to the year of application, and at least 16 years of data after the year of application (see Table A.4 in the Online Appendix). To remove the effect of outliers, we winsorize earned income, taxable assets, taxable debt, taxable secured and unsecured debt, taxable wealth, taxable real estate, and the hourly wage rate at the 1st and 99th percentile by calendar year.

3.3 Summary statistics

In the left column of Table 1, we present summary statistics for our full sample for the year prior to the year of application. Applicants for debt relief are on average 44.2 years old in the year of application, and a majority are men (63.3%). The average years of schooling are 11.0 and 64.4% of applicants are employed. The fraction of real estate owners is 12.0%.

The measure of taxable assets in Table 1 shows that applicants for debt relief have essentially no assets. This measure confirms that the debt relief program successfully targets individuals who cannot repay their debt by selling off existing assets.[8] The measure of taxable debt is considerably larger than the measure of taxable assets implying that applicants for debt relief have negative taxable wealth.

The center and right columns of Table 1 divide applicants for debt relief into those who were

maturity, interest rates, etc.

[8]The number of individuals who are granted debt relief in Denmark each year corresponds to less than 5% of the individuals who are registered by the Danish tax authorities as being late with payments on a loan greater than 100,000 DKK (approximately 15,000 USD), reinforcing that debt relief is restricted to debtors with severe financial problems (Kreiner, Leth-Petersen and Willerslev-Olsen, 2020).

granted versus denied debt relief. The pattern that emerges from the table is that these two groups are similar in terms of observable characteristics. Individuals who are granted debt relief have lower earned income, are less likely to be employed, and are less likely to own real estate compared to applicants who are denied debt relief, suggesting that there is a slight negative selection of individuals into debt relief by the City Court.

We also make a comparison of individuals who are applying for debt relief in Denmark with the general population in Denmark conditional on age and sex. For that purpose, we randomly draw five individuals with the same birth year and sex as each applicant for debt relief and describe the characteristics of these comparators in Table A.5 in the Online Appendix. The table reveals that individuals who apply for debt relief have lower socioeconomic status relative to the general Danish population. Applicants for debt relief have fewer years of schooling on average, have lower earned income, lower taxable assets, higher taxable debt, and are less likely to own real estate. The magnitude of these differences is quite large.

3.4 Repayment statistics

In order to describe the repayment terms for applicants who receive debt relief, we hand-collected additional information from a sample of 1000 randomly selected days where Statstidende was published between 1984 and 2005 (the repayment sample). Information on the fraction of the debt that had to be repaid (the dividend) is mandatory by law and is therefore available for close to all granted applicants. Some announcements contain information about the total unsecured debt, or information about the length of the repayment period and the monthly repayment from the debtor to the creditors. When the dividend is positive, we can use information on the repayment per month, the length of the repayment period, and the dividend to infer the total unsecured debt.

Table A.6 in the Online Appendix presents summary statistics for the repayment sample. Across 3968 announcements, the mean dividend was 10.3% and three quarters of debtors paid a dividend of 14.3% or less. These estimates show that debtors who are granted debt relief in Denmark typically repay a small fraction of their debt to the creditors. We also found 2181 an-

Table 1: Summary Statistics for Applicants of Debt Relief

	All	Granted	Denied
Mean age	44.2	44.3	43.8
	(10.5)	(10.6)	(10.2)
Fraction men	63.3%	62.7%	65.2%
Fraction married	58.4%	58.2%	59.3%
Mean persons in household	2.5	2.5	2.6
	(1.4)	(1.4)	(1.4)
Mean years of schooling	11.0	11.0	11.1
	(2.9)	(2.9)	(2.9)
Mean earned income	165	161	180
	(172)	(170)	(176)
Fraction employed	64.4%	63.9%	66.3%
Fraction unemployed	12.1%	12.5%	10.8%
Mean taxable wealth	-389	-394	-368
	(635)	(640)	(616)
Mean taxable assets	71	66	91
	(435)	(443)	(405)
Mean taxable debt	458	457	461
	(700)	(702)	(692)
Fraction real estate owners	12.0%	11.8%	12.7%
Observations	46,571	36,404	10,167

Notes: This table shows summary statistics for the full sample (and separately for granted and denied applicants) for the year prior to application. Monetary unit is thousands of 2020 DKK. Numbers in parentheses are standard deviations.

nouncements with information about the length of the repayment period with a mean of 4.5 years. Among these announcements, more than 85% described debtors who received a repayment period of exactly 5 years.

The repayment sample has information on the required repayment in 1145 announcements (when the dividend was positive), with a mean monthly repayment of 2500 DKK. To assess the magnitude of these repayments, we combined data on the fraction of granted applicants who pay a positive dividend, their mean repayment, and the estimated mean disposable income of granted applicants in our full sample according to the Income Tax Register. This comparison indicates that the mean repayment corresponds to 14.4% of mean disposable income for granted applicants.

Using direct information, or indirect information in the form of the required repayment and the length of the repayment period, we were able to infer the total debt in 1389 cases with a mean debt of 1.71 million DKK. This estimate from the repayment sample is larger than the estimate from the Tax Register, likely because the City Court considers a wider range of debt (for example unpaid taxes) than what is recorded in the Tax Register.

4 Results

We present a series of results on the impact of receiving debt relief in Denmark. We first discuss evidence in the form of mean outcomes and event-study regressions, and then turn to results from IV regressions using quasi-random assignment of applicants to court trustees.

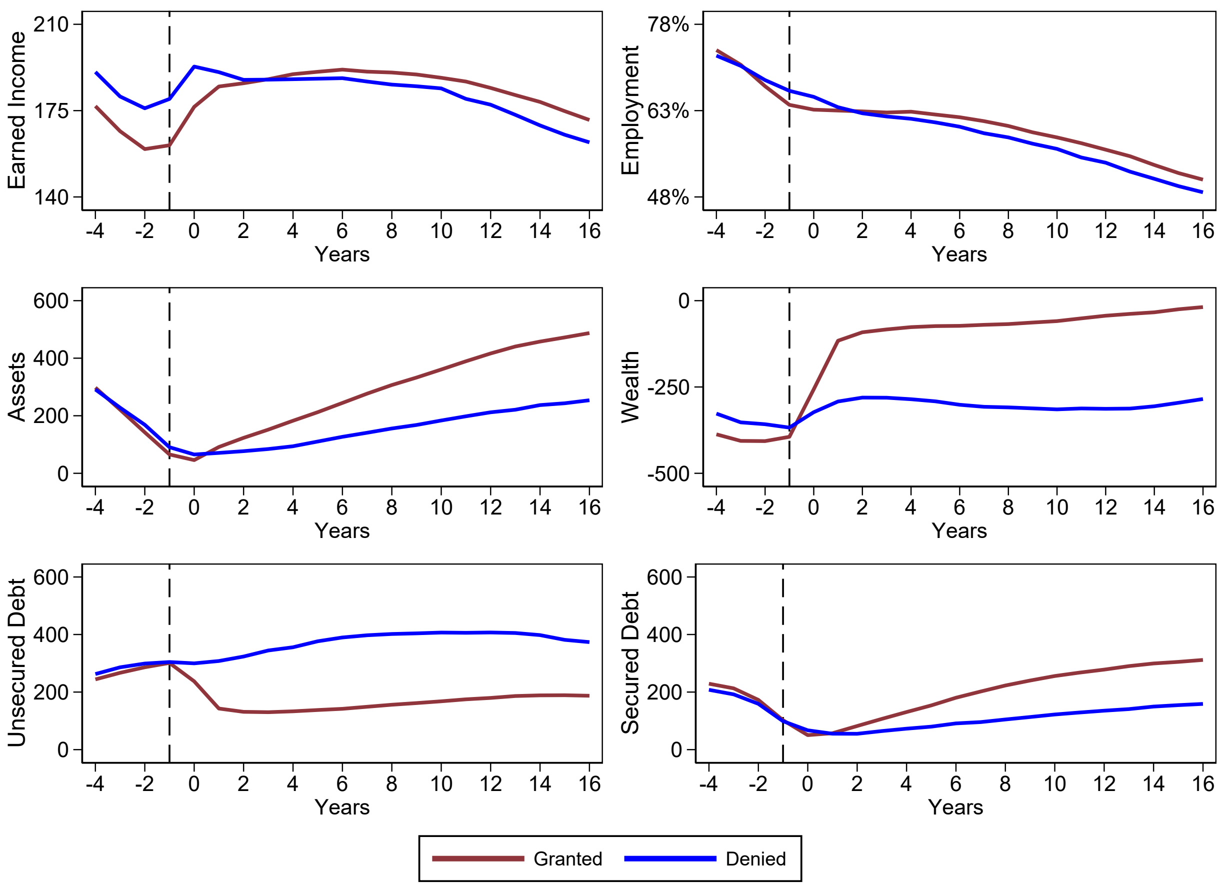
4.1 Graphical evidence

Figure 1 and Figure A.2 (Online Appendix) show mean outcomes for applicants for debt relief, from 4 years before to 16 years after the year of application. Applicants who are granted debt relief have lower mean earned income prior to application compared to denied applicants. Afterwards, these positions reverse and applicants who are granted debt relief have higher mean earned income up to 16 years after application. The employment rate is similar or slightly lower for granted applicants prior to application but also reverses gradually, so that granted applicants have a higher employment rate towards the end of the follow-up period.

Mean taxable assets, the fraction of debtors who own real estate, and mean taxable real estate falls prior to application. Mean taxable debt also falls, due to a reduction in secured debt. Unsecured debt, on the other hand, continues to rise up until the year of application. These time patterns are consistent with a process of deleveraging where applicants are selling off financial assets and real estate to reduce their debt, and perhaps clarify what fraction of the debt they can repay (Kilborn (2009)).

After the year of application, there is a divergence in balance sheets depending on court decision. Applicants who are granted debt relief accumulate taxable assets at a higher rate and become more likely to own real estate compared to applicants who are denied debt relief. Applicants who are granted debt relief also experience a large, immediate, and persistent reduction in unsecured debt, but accumulate more secured debt relative to denied applicants, consistent with the steady increase in real estate ownership. The net effect of these changes is that granted applicants accumulate more taxable wealth than denied applicants during the whole follow-up period.

Figure 1: Mean Outcomes Before and After Application for Debt Relief



Notes: This graph shows mean outcomes for granted and denied applicants for debt relief from 4 years before to 16 years after the year of application. The outcome variables are earned income (top left), employment (top right), taxable assets (middle left), taxable wealth (middle right), unsecured taxable debt in banks and other financial institutions (bottom left), and secured taxable debt in banks and other financial institutions (bottom right). Monetary unit is thousands of 2020 DKK.

4.2 Event-study regressions

Next, we estimate event-study regressions of the form

$$Y_{it} = \alpha_i + \psi_s + X_{it}\theta + \sum_{s \neq -1} \delta_s \cdot 1[t - A_i = s] \cdot D_i + \varepsilon_{it} \quad (1)$$

where Y_{it} is the outcome of interest for applicant i in year t, α_i is an individual fixed effect, D_i is a dummy for applicants who were granted debt relief, A_i is the year of application, ψ_s are period fixed effects (in years relative to year of application), δ_s is the parameter of interest and measures the impact of debt relief s years after application,[9] and X_{it} are exogenous covariates (calendar year fixed effects). In all event-study regressions (1), we cluster standard errors at the level of the applicant and normalize δ_s to zero in the year prior to application.

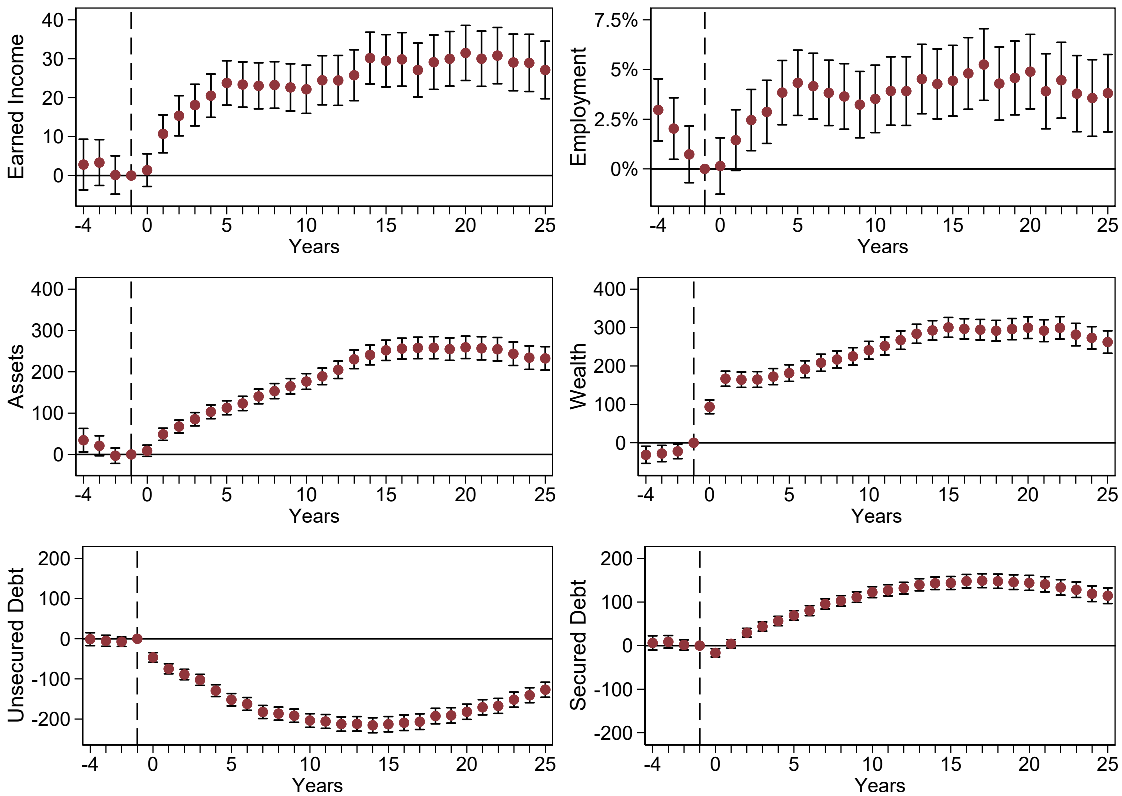
Figures A.3 and A.4 in the Online Appendix display event-study graphs for all outcomes over a 16-year horizon using all application cohorts. In addition, Figure 2 displays event-study graphs for a sub-sample permitting a 25-year study horizon while still requiring that our data is balanced wrt event time (i.e. using applicants for debt relief from 1984 up until 1994). Overall, these graphs show that receiving debt relief is associated with significant and persistent effects throughout our 16-year follow-up period, extending all the way up to 25 years after application. To go even further, Figure A.5 (Online Appendix) shows impacts for a 34-year horizon where we use all applicant observations and do not require that we have a balanced sample wrt event time. This last figure suggests that, especially for financial outcomes, the impacts of debt relief persist over 34 years.

Overall, the event-study graphs show that the effects of debt relief include an increase in earned income, an increase in taxable assets, an increase in the likelihood of owning real estate, a decrease in unsecured debt, an increase in secured debt, and an increase in taxable wealth. For all outcomes, the event-study coefficients are statistically significant but the interpretation of some of the estimates may not be straightforward since the outcomes (e.g. employment) exhibit non-parallel trends

[9] Using the estimator developed in Callaway and Sant'Anna (2021) we obtain very similar results to what we show below (see Figure A.6). Note that our event-study is balanced and centered around *event* time (year of application), and that there is a control group for each application cohort – i.e. non-granted individuals who applied for debt relief in the same year.

prior to application. This is why we turn next to instrumental variable estimation to better understand the impact of receiving debt relief and compare the event-study estimates with alternative estimates from an IV setup.

Figure 2: Long-Run Event-Study Graphs



Notes: This graph shows estimated event-study coefficients from 4 years before to 25 years after the year of application comparing granted and denied applicants for debt relief. The outcome variables are earned income (top left), employment (top right), taxable assets (middle left), taxable wealth (middle right), unsecured taxable debt in banks and other financial institutions (bottom left), and secured taxable debt in banks and other financial institutions (bottom right). Standard errors are clustered at the level of the debtor. Monetary unit is thousands of 2020 DKK. The sample consists of applicants for debt relief from 1984 up until 1994.

4.3 Instrumental variables model

Our main econometric method is an IV study design that builds upon previous work by Doyle (2007), Dahl, Kostøl and Mogstad (2014), and Dobbie and Song (2015) among others. These authors exploit the random assignment of individuals to judges or investigators with varying leniency. Unlike Dobbie and Song (2015), we do not know the identity of judges deciding debt relief cases in our data. Instead, we construct an instrument based on the quasi-random assignment of applicants to court trustees. Trustees are appointed as assistants to the court and are influential in preparing and assessing a debt relief case for the judge and can indirectly affect the verdict of the court.[10] This institutional feature creates exogenous variation in the admission of applicants into debt relief.

Our econometric IV framework is a two-stage least squares model

$$D_{ijc} = \chi + \eta Z_{ijc} + \Gamma W_{it} + u_{it} \tag{2}$$

$$Y_{it} = \mu + \beta D_{ijc} + \Theta W_{it} + v_{it} \tag{3}$$

where we refer to equations (2) and (3) as the first and second stage. D_{ijc} is an indicator for being granted debt relief for applicant i assigned to trustee j in court c. Similar to previous studies, the instrument Z_{ijc} for applicant i assigned to trustee j in court c is the mean leave-out admission rate of the trustee in the court minus the mean leave-out admission rate of all trustees in the same court (n_{jc} is the number of cases handled by trustee j while n_c is the number of cases in court c across all trustees)

$$Z_{ijc} = \frac{1}{n_{jc}-1} \cdot \left(\sum_{k=1}^{n_{jc}} (D_k) - D_i \right) - \frac{1}{n_c - 1} \cdot \left(\sum_{k=1}^{n_c} (D_k) - D_i \right) \tag{4}$$

To reduce noise in our instrument, we limit our IV sample to trustees handling 20 cases or more in a court. We also require that there are 2 trustees or more in a given year in a court. We calculate the value of the instrument across all observations in our data (not only the estimation sample) and refer to the combination of a trustee and a court as a trustee identifier. In the IV estimation sample, we have 32,794 observations with 515 trustee identifiers handling an average of 64 debt

[10]In his extensive survey of the Danish debt relief system, Kilborn (2009) notes *"Though reliable statistical figures on the rate of plan non-confirmation are not available, one suspects that nearly all plans submitted to hearing by the trustees are confirmed by the courts"*, further highlighting the importance of trustees in the court decision process.

relief cases.[11] In all IV regressions, we cluster standard errors by the trustee identifier.

We include exogenous covariates for an applicant, W_{it}, that are available for the whole study period. These covariates are demographic variables (sex, age at application in four categories, a dummy for a single-person household, legal marital status, immigrant status), education (in three categories), the hourly wage in the year prior to application (dummies for quartiles and missing data), the balance sheet of the applicant in the year prior to application (taxable assets, taxable debt, and a dummy for real estate ownership), proxies for the permanent income of the applicant (the mean over four years prior to application of earned income, employment, unemployment, and social assistance), and fixed effects for the (calendar) year of observation. We also include court-by-year-of-application fixed effects to adjust for variations in the quality of applicants and the behavior of trustees across locations and time.

4.3.1 Relevance of instrument

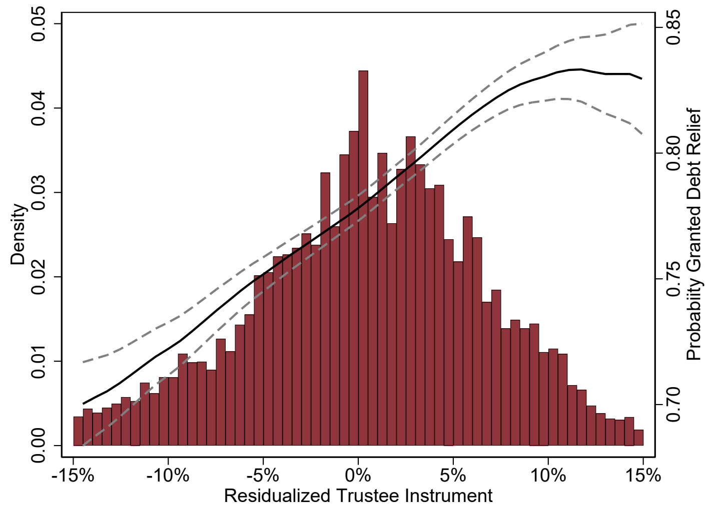
A valid instrument requires relevance, independence, exclusion, and monotonicity. In this and the following two sections, we present evidence regarding each of these requirements.

Figure 3 shows a histogram of the residualized instrument conditional on court-by-year fixed effects, indicating that there is considerable variation in the admission rate of trustees. Moving from the 10th to the 90th percentile of trustees corresponds to a 17 percentage point increase in admission rates. The solid line in Figure 3 shows the fitted values from a non-parametric regression of the debt relief court verdict (whether or not the applicant was granted debt relief) on the instrument. Consistent with the first stage of the IV regression model, this relationship is monotonically increasing across the distribution of the instrument and close to linear.

Table A.7 in the Online Appendix shows results from the first stage regression (equation (2)). The first column in the table presents estimates from the regression with court-by-year fixed effects but no other covariates. The estimated coefficient for the instrument is 0.532, meaning that a 10 percentage point increase in the admission rate of the assigned trustee (a change not uncommon

[11] These 515 trustee identifiers correspond to 502 different trustees 13 of whom work in two courts.

Figure 3: Properties of Instrumental Variable



Notes: This graph displays properties of the instrumental variable used for 2SLS estimation. The bar chart shows the distribution of the instrument (the normalized mean trustee admission rate) conditional on court-by-year fixed effects (y-axis to the left). The black line shows the non-parametric regression of a dummy for a debtor being granted debt relief on the instrument with 95% confidence intervals (y-axis to the right).

in the data, see Figure 3) increases the probability of the applicant being granted debt relief by 5.32 percentage points on average. The instrument coefficient is significant with an associated F-statistic of 206. The second column of the table adds the full set of covariates that we use in the two-stage regression model. Including these covariates hardly affects the estimated coefficient for the instrument, consistent with the instrument being at most weakly correlated with the observable characteristics of applicants for debt relief.

4.3.2 Independence and exclusion of instrument

Independence and exclusion require that the assignment of applicants to trustees is random (within court and year) and that a trustee only affects the assigned applicant through the probability that the applicant is granted debt relief. Table A.8 (Online Appendix) shows a balance test for the instrument and the exogenous covariates, W_{it}, in the two-stage least squares model. In the right column, we present results from a regression of the court verdict (whether an applicant is granted debt relief or not) on the covariates. Twelve out of twenty-two coefficients are significant and the

F-statistic for joint significance is 12.94 ($p < 0.001$). These results imply that the covariates are highly predictive of whether an applicant is granted debt relief or not.

In the left column of Table A.8, we present the results from a regression of the instrument on the same set of exogenous covariates. Only two out of twenty-two coefficients are significant and small in magnitude, and a test that the coefficients are jointly equal to zero is not rejected ($p = 0.252$). The implication of the results in Table A.8 is that the same observable applicant characteristics that predict the verdict in debt relief cases, do not jointly predict the admission rate of the trustees handling the cases, consistent with the assignment of trustees to cases being random within a court and year. Representatives for the courts and experts in the field have confirmed this practice.[12]

A potential threat to our identification strategy is that trustees may affect not only who gets debt relief but also the conditions under which debt relief is granted.[13] We therefore examine the association between the (residualized) trustee instrument (i.e. the normalized admission rate of trustees in equation (4) conditional on court-by-year fixed effects) and the dividend that granted applicants pay to their creditors, using data from the repayment sample (see Section 3.4). Figure A.8 (Online Appendix) shows a scatter plot of the instrument and the dividend, with no apparent relationship between the two variables. In Table A.9, we present results from a regression of the dividend on the instrument. When we exclude covariates, there is no association between the two variables (the R-squared rounded to three decimals is 0.000). Including the exogenous covariates, W_{it}, from the two-stage least squares model and/or winsorizing the dividend at the 1st and 99th percentiles (to reduce the impact of outliers) does not change our conclusion that there is no stable relationship between the instrument and the dividend. The results in Table A.9 suggest that our IV results are driven by exogenous entry into debt relief and not contaminated by variations in the

[12]See e.g. https://www.domstol.dk/aarhus/raadgivning/ for the current list of trustees in the Aarhus City Court where the head of the court (Preben Veng) confirmed that cases are assigned to trustees by court administrative personnel on a rolling basis, i.e. random assignment.

[13]Trustees are assistants to the court and are required to be impartial and prepare debt relief cases according to the law. If trustees assist debtors or creditors beyond their legal duties, trustees face the risk of losing their authorization to work for the court (and hence lose their salary).

conditions under which applicants receive debt relief.[14]

4.3.3 Monotonicity of instrument

In the presence of heterogeneous treatment effects, the instrument must satisfy monotonicity if the estimated impact of receiving debt relief is to identify a positively weighted average of individual treatment effects. Monotonicity means that an applicant who was granted debt relief when handled by a trustee with a low admission rate would also have been granted debt relief by a trustee with a higher admission rate (and vice versa). An example of a violation of monotonicity would be if some trustees were more supportive of male applicants while other trustees were more supportive of female applicants.

We first implement the joint test of strict monotonicity and exclusion developed by Frandsen, Lefgren and Leslie (2023*a*). Following the arguments in Sigstad (2023), we implement the test within courts and we focus on the 9 largest courts in our sample (close to 50 % of the sample) to ensure reasonable sample sizes.[15] Table A.17 (Online Appendix) reports p-values and test statistics. In the majority of cases, the p-value is above conventional levels implying that we cannot reject the null hypothesis of strict monotonicity and exclusion. There are also cases where we do reject the null hypothesis with a p-value close to zero, but these cases are scattered across courts and time periods and we cannot rule out that individual rejections reflect random noise as there are relatively few cases per trustee (see the discussion in Frandsen, Lefgren and Leslie (2023*b*)). Re-estimating our IV model and excluding the courts where we most often reject the null hypothesis does not fundamentally change any of the findings.

We also conduct two tests of (average) monotonicity previously implemented by Bhuller et al. (2020) and Norris, Pecenco and Weaver (2021). Under the weaker assumption of average monotonicity and exclusion, the IV estimator still identifies a convex combination of treatment effects

[14]The similarity of our IV results for labor market outcomes both during the repayment period as well as in the longer run is also consistent with the estimated impact arising due to entry into debt relief as opposed to differences in repayment conditions across trustees.

[15]Focusing on excluded courts tends to generate similar findings but suffers from a limited sample size within courts. Further, re-estimating our IV model using the 9 largest courts does not fundamentally change any of our results below.

(Frandsen, Lefgren and Leslie (2023*a*)). The first test is that the coefficient for the instrument in the first stage (the impact of the instrument on the probability that applicants are granted debt relief) should have the same sign in different subsamples. Table A.10 (Online Appendix) shows results from the first stage regression in subsamples based on sex, age, education, and earned income. The estimated coefficients for the instrument in all these subsamples are positive and significant.

A second test of (average) monotonicity is that trustees who had a high admission rate (relative to other trustees) in one subsample should also have a high admission rate (relative to other trustees) in other subsamples. We conduct this test by estimating the first stage in subsamples with an instrument that is constructed from cases outside the subsample (a reverse-sample instrument). When we estimate the first stage among men, for example, the instrument is constructed among women. Table A.11 (Online Appendix) shows results from the first stage regression in the same subsamples as in Table A.10. The estimated coefficients for the instrument are still positive and significant using the reverse-sample version of the instrument.

4.4 Labor market IV results

We now present our IV estimates for labor market outcomes in Table 2. The estimated two-stage least squares model indicates that applicants who are granted debt relief have significantly higher earned income, with an annual increase of 46,800 DKK. This rise corresponds to 26.0% of the mean earned income of applicants who were denied debt relief measured across all 16 years of follow-up (follow-up means for denied applicants are reported in Table A.13 in the Online Appendix). This is a large effect, albeit similar in magnitude to the estimated effect in Dobbie and Song (2015) who find that Chapter 13 bankruptcy protection increases earnings for US applicants by 25.1% relative to the pre-filing mean. This similarity in magnitude arises even though the Danish institutional setting is different from the US in several ways, something we discuss further below.

Being granted debt relief also leads to a significant increase in employment of more than ten percentage points, or equivalently 20.7% of the follow-up mean. The higher employment rate

Table 2: Impact of Debt Relief on Labor Market Outcomes

	(1) OLS	(2) IV
Earned Income (DKK)	20,183**	46,800**
	(1,576)	(15,200)
Employed (y/n)	0.0230**	0.117**
	(0.0038)	(0.039)
Unemployed (y/n)	-0.0123**	0.0050
	(0.0023)	(0.012)
Out of Labor Force (y/n)	-0.0110**	-0.122**
	(0.0036)	(0.040)
Hourly Wage (DKK)	4.264**	11.6
	(0.915)	(8.25)
Observations (individuals)	46,391	32,794

Notes: This table shows the estimated impact of debt relief on labor market outcomes. Column (1) reports estimates based on our event-study regression (equation 1) assuming a time-invariant treatment effect. Column (2) reports estimates using our instrumental variable regression. Monetary unit is 2020 DKK. The number of observations in Column (1) refers to the number of individuals with non-missing outcome data (the maximum across outcomes). The number of observations in Column (2) refers to individuals with non-missing outcome data and a valid instrument. Note that the difference in estimates across columns is not explained by differences in samples (see Table A.12 in Appendix). Numbers in parentheses are standard errors clustered at the level of the individual (Column 1) or clustered at the level of the trustee identifier (Column 2). ** p<0.01, * p<0.05.

of granted applicants is almost completely offset by a corresponding decrease in the fraction of individuals who are out of the labor force. Our interpretation of the results in Table 2, is that high debt discourages some individuals from entering the labor market, most likely due to demands that would be placed on them from creditors if they acquired additional income above the legal poverty level (see also Section 2.3 and Section 4.7.5). We also try to further decompose the decrease in the fraction of applicants who are out of the labor force. Suggestive evidence is pointing towards a decrease in the fraction of applicants who receive disability insurance and a decrease in the fraction who receive social assistance (see Table A.14 in the Online Appendix). None of these two estimated effects on welfare dependency are significant though. As can be seen in Table 2, we find no significant effect of debt relief on wages or unemployment.

To understand how much this increase in employment contributes to the increase in earned income, we use a statistical decomposition based on previous work by Blundell, Bozio and Laroque (2011) and outlined in Online Appendix D. The earned income I_{it} of individual i in year t can be written as the product

$$I_{it} = P_{it} \cdot E_{it} \tag{5}$$

where P_{it} is an indicator for individual i working in year t, and E_{it} is the earned income of the individual in that year if he or she is working. Using a linear decomposition, the change in earned income, ΔI, is

$$\Delta I = \Delta P \cdot E + P \cdot \Delta E \tag{6}$$

The first term in the decomposition, $\Delta P \cdot E$, is the contribution of the extensive margin (employment) and the second term, $P \cdot \Delta E$, is the contribution of the intensive margin (hours worked and the hourly wage). If we set the changes in earned income and employment, ΔI and ΔE, equal to our IV estimates and implement the simple decomposition discussed in the Online Appendix D, we find that the increase in employment accounts for approximately two thirds of the increase in earned income for applicants who are granted debt relief in Denmark.

An important contribution of our study is the long study horizon which enables us to quantify long-lasting impacts of debt relief and cumulative gains in earnings and employment. If we sum over all 16 years of follow-up, the accumulated increase in earned income for applicants who are granted debt relief in Denmark amounts to close to half (48.3%) of the mean debt they owe to creditors (Table A.6 in the Online Appendix). Finally, in Section I (Online Appendix) we use our estimates from above and take a first step towards assessing the fiscal impact of the Danish debt relief program by accounting for increased tax revenues, reduced public income transfers, and lost tax revenue from creditors who qualify for tax reductions when they write off their credit losses.

Table 3: Impact of Debt Relief on Household Finances and Real Estate Ownership

	(1) OLS	(2) IV
Taxable Wealth (DKK)	255,898**	282,500**
	(5,610)	(46,400)
Taxable Assets (DKK)	155,504**	309,300**
	(5,315)	(54,400)
Taxable Debt (DKK)	-110,386**	7,870
	(6,870)	(66,000)
Taxable Secured Debt (DKK)	93,587**	201,400**
	(3,512)	(38,000)
Taxable Unsecured Debt (DKK)	-191,849**	-188,100**
	(4,323)	(42,600)
Owns Real Estate (y/n)	0.156**	0.248**
	(0.004)	(0.044)
Taxable Real Estate (DKK)	124,318**	260,800**
	(4,099)	(47,700)
Observations (individuals)	46,391	32,794

Notes: This table shows the estimated impact of debt relief on household finances and real estate ownership. Column (1) reports estimates based on our event-study regression (equation 1) assuming a time-invariant treatment effect. Column (2) reports estimates using our instrumental variable regression. Monetary unit is 2020 DKK. The number of observations in Column (1) refers to the number of individuals with non-missing outcome data (the maximum across outcomes). The number of observations in Column (2) refers to individuals with non-missing outcome data and a valid instrument. Note that the difference in estimates across columns is not explained by differences in samples (see Table A.12 in Appendix). Numbers in parentheses are standard errors clustered at the level of the individual (Column (1)) or clustered at the level of the trustee identifier (Column (2)). ** p<0.01, * p<0.05.

4.5 Financial status IV results

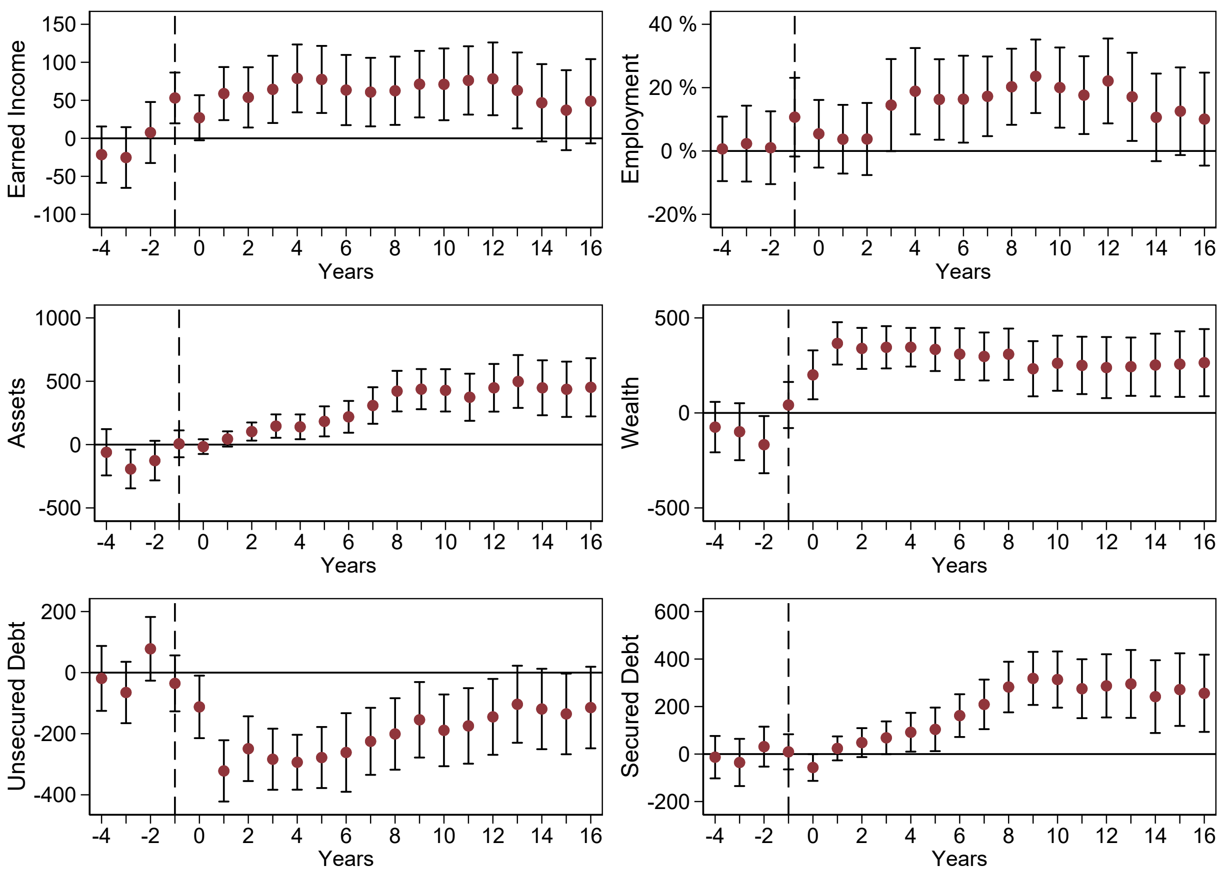
Table 3 shows IV estimates for outcomes related to financial status. The estimates show a significant increase in taxable assets for applicants who are granted debt relief corresponding to about 200% of the follow-up mean. After debt relief, there is a reduction in unsecured debt and an increase in secured debt, and these two changes are roughly equal in size (there is no significant change in total taxable debt when all years of follow-up are pooled together). The IV estimates reveal a large increase in the fraction of granted applicants who own real estate of around 25 percentage points, in the order of 200% of the follow-up mean. There is also a significant increase in real estate wealth which is slightly larger than the increase in secured debt.

Linking these patterns to our results on earned income and employment, we see that debt relief increases earned income and that much of this increase in income translates into an accumulation of assets (the estimated increase in assets amounts to close to two years' earned income in the year prior to application, see Table 1). A large fraction of the increase in assets (around 84 percent) can be accounted for by home ownership and real estate which is partly financed through an increase in secured debt (applicants who are granted debt relief regain access to mortgage loans). The net effect of debt relief, operating through all these changes in earnings, assets, home ownership, real estate, unsecured and secured debt, is an estimated long-lasting increase in taxable wealth for applicants who are granted versus denied debt relief. These improvements in financial status are in alignment with the findings in Dobbie, Goldsmith-Pinkham and Yang (2017) for the US, except that we find a (long-lasting) decline in unsecured debt in Denmark for granted applicants. Danish applicants who are granted debt relief should gain easier access to both secured and unsecured debt due to a better credit score and lower risk of flagging (see Section 2.3). We therefore believe that the reduction in unsecured debt reflects a preference among debtors for secured and (likely) cheaper debt.

4.6 IV results by follow-up period

In Figure 4 we present the IV results for our six main outcomes when we estimate the IV model separately for each year in the follow-up period. In Table A.15 (Online Appendix), we further present sub-period estimates for all our outcomes when we split the follow-up periods into three intervals (1 to 5 years, 6 to 10 years, and 11 to 16 years). The IV estimates for earned income are more or less stable across follow-up time, suggesting that the increase in earned income caused by debt relief is permanent. The estimated effect of debt relief on employment is lower in the first periods, then increases, and eventually falls again in the last periods. The impact of debt relief on assets, real estate, and the fraction of real estate owners grows over time. Interestingly, the estimated impact on taxable debt is first negative but turns positive in later sub-periods. Figure 4 shows that this pattern is driven by a large and initial (mechanical) reduction in unsecured debt, followed by a gradual increase in secured debt that is likely linked to the increase in home ownership and real estate wealth. In the last periods (11 to 16 years after application), taxable wealth has increased by 261,000 DKK which corresponds to around one and a half years' earned income for applicants in the year prior to application.

Figure 4: IV Event-Study Estimates



Notes: This figure plots IV estimates (equations 2 and 3) for each year in the follow-up period after application. The set of control variables is identical to the set of control variables in Tables 2 and 3. Since we also include estimates for years prior to an application for debt relief, we need to restrict our sample to applicants for debt relief between 1988 and 2003, to ensure that we can appropriately lag our covariates in these pre-periods.

4.7 Extended results

4.7.1 Robustness checks

To examine the sensitivity of our IV estimates, we conduct a series of robustness checks and present these results in the Online Appendix. We start by varying the number of required cases per trustee from 20 to 50 and then 100 cases. Table A.16 shows that the estimated impact of receiving debt relief is quite similar across cutoff levels.

Instead of using the admission rate of trustees as a single instrument, one can also use "many fixed effects" instruments, one for each collection of cases handled by the same trustee (see e.g. Norris, Pecenco and Weaver (2021)). To test the robustness of our estimates wrt this alternative specification, we implement the UJIVE estimator of Kolesár (2013) and show the estimated coefficients for a restriction of 20 or 50 cases per trustee in Table A.18. We see that the effects are quite similar and statistically indistinguishable from the results based on our main IV specification. In Table A.19, we present further robustness checks where we vary the construction of our instrument. The first column uses an instrument that is calculated by calendar year, the second column leaves out court cases in the same calendar year when computing the mean admission rate, and the third column randomly splits the sample in two halves and uses the admission rate calculated in one half to estimate the model in the other half. Overall, results are again similar across specifications.

Attrition in our sample is mainly due to emigration or death (see Table A.20 for descriptive statistics and IV results). When we estimate our model on a balanced panel with no attrition (Table A.21), results are close to our main IV estimates. Finally, we consider different ways of clustering standard errors. In our main analysis, we cluster standard errors at the level of the trustee (similar to Bhuller et al. (2020)). Table A.22 presents results when we use other methods. Standard errors change only slightly if we cluster at the debtor level, court level, court-by-year level, or trustee-by-year level. In sum, none of the robustness checks challenges the main findings above.

4.7.2 Subgroup analysis

We estimate our IV model in subsamples based on marital status, sex, age, education, income, wealth, debt, and economic conditions at the time of application (recessions versus ordinary conditions). The impacts of debt relief that we estimate in the full sample are also present in most of these subsamples. Due to high imprecision in the estimates, we should interpret potential differences across subsamples cautiously. Below we briefly discuss results from a few subgroups where differences in impacts are particularly large. For a complete set of estimates and a full discussion of all subgroup effects, see Online Appendix Section E and Tables A.23, A.24.

We find that workers with low education have larger employment impacts than higher educated individuals (a 10 percentage points difference in impacts relative to the non-granted means). Workers with high education, on the contrary, display larger earnings impacts. Workers with high education also have larger increases in wealth, mostly driven by larger reductions in unsecured debt. Individuals with below median income prior to application experience a larger increase in earnings on average than individuals with above median income (a 17% difference relative to the non-granted mean), and much of this difference can be explained by a 16 percentage points higher impact on employment. For wealth, we find larger increases in employment (a 16 percentage points difference in impacts) and earnings (a 15 percentage points difference in impacts) relative to non-granted means, for individuals with above versus below median wealth prior to application. On the contrary, we see the largest increases in wealth for the low wealth group largely explained by larger reductions in unsecured debt. Finally, we also find that individuals applying in a recession have larger employment impacts compared to individuals applying in other years, potentially due to more positive selection into applying for debt relief during recessions. For earnings, impacts are smaller both in absolute and relative terms for individuals applying in a recession.

Lastly, we also present IV estimates beyond our baseline estimation period of 16 years. Table A.25 in the Online Appendix presents the full set of IV results for early applicants who applied for debt relief from 1984 up until 1994 (i.e. similar cohorts as in Figure 2), for the follow-up period from 17 to 25 years after application. The estimated coefficients for labor market outcomes are similar in magnitude to the estimates for the earlier periods, but less precise due to a smaller sample size. The results for financial status and real estate ownership are typically larger than for earlier periods, with an estimated increase of 46.6 percentage points for home ownership at 25 years, and sizable effects on wealth, assets, and secured debt.

4.7.3 Compliers

Our IV model estimates the average impact of being granted debt relief for compliers, that is applicants who would be granted debt relief if assigned to the least strict trustee, but not granted debt relief if assigned to the strictest trustee. Using the method of Dahl, Kostøl and Mogstad (2014), we find that our sample can be split into 22% compliers, 63% always takers (who would be granted debt relief with all trustees), and 15% never takers (who would be denied debt relief with all trustees).

We also describe the distribution of observable characteristics among compliers by estimating the share of compliers in subsamples (see the Online Appendix F for details). Table A.26 shows that compliers resemble (in terms of observable characteristics before application) applicants in the full sample and the subsample of applicants who were granted debt relief. Reweighting our event-study to match the compliers on observable characteristics therefore has only a very small effect on the event-study estimates (see Figure A.7 in the Online Appendix).

4.7.4 Discussion of results

Broadly speaking the results from our event-study regressions are similar to our IV results in qualitative terms. In general, however, the IV estimates in Tables 2 and 3 are in the order of twice as large as the event-study estimates (with some variations across outcomes). We believe that at least three mechanisms may explain this discrepancy.

The first mechanism is that our collection of data from court announcements on microfilm may have induced errors in our database. We have been conservative when collecting data, and we match aggregate statistics such as the fraction granted applicants well, but we cannot rule out the presence of some measurement error in e.g. grantee status. Measurement error is likely to induce a downward bias in the event-study estimates (attenuation bias). If errors in the data collection process are uncorrelated across applicants handled by the same trustee (classical measurement error), our IV estimates are still consistent.

A second mechanism is that our IV estimates and event-study estimates measure different things. Assuming that the identifying assumptions are satisfied, our IV estimates are local average treatment effects (LATE) while our event-studies measures the average treatment effect on the treated (ATT). We found very limited evidence of selection of compliers in terms of observables suggesting that heterogeneity in treatment effects related to observables cannot explain differences between the IV and event-study estimates. However, this does not rule out selection on unobservable gains for compliers.

To move beyond the LATE parameter, we estimate marginal treatment effects (Heckman and Vytlacil (2005)) for granted and non-granted filers with a similar (ex ante) probability of being granted debt relief. In the Online Appendix G, we discuss our implementation of the MTE framework in more detail. Figure A.9 displays the estimates for our six main outcomes and shows upward sloping MTE curves for earnings, employment, assets, and secured debt. These are also the outcomes for which we observe the largest differences between IV and event-study estimates. An upward sloping MTE curve implies that individuals who are only granted debt relief when assigned to the trustee with the highest admission rate have larger treatment effects on average. This analysis suggests that one reason why the IV and event-study estimates differ (why the LATE parameter is larger than the ATT parameter) may be because compliers in our setting are individuals with larger gains from debt relief relative to the average granted individual. An important caveat is that the identification of marginal treatment effects requires additional assumptions, such as strict monotonicity, and we therefore think of this analysis as mainly exploratory (see Online Appendix G for further discussion).

A third and final mechanism is due to the manner in which the City Court selects applicants who are granted debt relief. According to Danish law, the court should consider the current and future ability of applicants to repay their debt when making its decision. This forward-looking nature of the court implies that applicants who are granted debt relief are likely to have worse future economic prospects than applicants who are denied debt relief. In particular, it is likely that granted applicants have worse expected future earnings trajectories, worse expected future health trajectories, and worse expected future work capacity. A simple comparison of the changes in outcomes of applicants who are granted versus denied relief is therefore likely to produce a smaller estimated impact of receiving debt relief. This selection mechanism induces a downward bias in the event-study estimates, and we believe that the forward-looking selection of applicants who are granted debt relief may also be an important reason why our IV estimates are larger than the corresponding event-study estimates.[16]

For all the reasons mentioned in this section, we think that the event-study estimates may provide a lower bound on the average impact of receiving debt relief in Denmark. We also think that the IV estimates are closer to the causal effect of debt relief for a marginal applicant and therefore provide a better guide to what would happen if the Danish debt relief program was (slightly) expanded.

Finally, we briefly discuss our results in relation to previous results from the US and highlight some differences in the institutional setting. First, Figure 1 suggests that the impact of debt relief in Denmark arises because granted applicants experience improvements in outcomes relative to non-granted applicants. Dobbie and Song (2015) show that their results for the US are largely driven by a further deterioration of outcomes for non-granted applicants over time. Relative to US applicants, individuals who apply for debt relief in Denmark are on a worse (downward) trajectory prior to their application, likely reflecting that an application for debt relief is only initiated after a longer spell of over-indebtedness and financial distress (contrast pre-application trajectories in Figure 2 in Dobbie and Song (2015) to Figure 1 above). Applicants in Denmark are in this sense at the rock

[16]A similar type of bias has been discussed in the literature on the labor supply effects of disability insurance (DI), see e.g. Bound (1989) and Maestas, Mullen and Strand (2013) who argue that due to negative selection into DI program participation, OLS estimates overestimate the negative impact of disability insurance. In our setting where program participation improves labor market outcomes, a similar selection mechanism leads to a downward bias in OLS estimates.

bottom. We believe that these dynamics prior to the court process could explain why the outcomes for non-granted applicants in Denmark do not deteriorate further after application. In addition, there are differences in treatment across countries (the incentive to work is stronger in Denmark since the repayment plan is fixed irrespective of future applicant earnings) and differences in the counterfactual that is used for estimation, since that there are two main routes to debt relief in the US setting (Chapter 7 and Chapter 13) but only one route in Denmark.[17]

4.7.5 Mechanism

In this section, we provide a simple calculation to assess why debt relief is associated with a large increase in earned income. We base our discussion on a neoclassical framework and consider how debt relief affects a debtor's budget constraint. A limitation is that we lack individual data on payments from debtors to creditors before and after a court decision. All details of our calculation are presented in Appendix 4.7.5.

It has long been recognized that individuals with large debt have weaker incentives to work, since future claims from creditors can act as an implicit tax on earnings.[18] If a debtor in Denmark has unpaid public debt and is subject to wage garnishments proportional to earned income, the required payments from the debtor to creditors are (more or less) identical to a tax. If the debtor instead has unpaid private debt, additional income generated by the debtor will increase the likelihood that creditors ask for partial repayment of the debt, and will raise the required payments. The more additional income the debtor receives, the larger will be the demands from creditors, similar to a tax that rises with income. In both these circumstances, the main effect of debt relief (a reduction in current and future claims on the debtor) is similar to a reduction in the marginal tax rate on earnings.

We apply this reasoning to a prototypical applicant for debt relief who has the mean disposable

[17] Dobbie and Song (2015) note that around 20 % of dismissed Chapter 13 filers refile under Chapter 7. Similarly Dobbie, Goldsmith-Pinkham and Yang (2017) note that 27 % of dismissed Chapter 13 filers convert to Chapter 7 with a success rate of around 95%.

[18] See for example the 1934 verdict from the US Supreme Court in the case of the Local Loan Company versus Hunt (292 U.S. 234) in which the court argued that claims from the creditor (the Local Loan Company) on the debtor (Mr Hunt) reduced the incentives of the debtor to work.

income, earned income, and debt of individuals in our sample. We also assume that 20% of the applicant's earned income is withheld as wage garnishments (the maximum) for the rest of life if the applicant is not granted debt relief. We adopt this assumption since the ratio of debt to disposable income is so large that a typical applicant cannot arguably repay all debt through wage garnishments alone (see Section 2.3). For completeness, an applicant who is granted debt relief in our example also has to pay a mean dividend to creditors. In our example, the impact of debt relief on the applicant's budget constraint is then two-fold: i) the applicant is no longer subject to 20% wage garnishments (a tax cut), and ii) the applicant has to pay a fixed dividend to the creditors that is independent of future earnings.

We assume that wage garnishments act like a tax on earned income on top of regular taxes, and that removing wage garnishments is equivalent to a permanent 20% tax cut. There is a range of estimates describing the elasticity of taxable earnings with respect to the tax rate. For this example, we use the elasticity from Kleven and Schultz (2014) who study how taxable earnings at the intensive margin responded to changes in the Danish tax code in the 1980's. They obtain an uncompensated elasticity of taxable earnings with respect to the net-of-tax rate of 0.26 for large tax cuts that affected a wide group of tax payers.[19] Adopting this elasticity in our example, the removal of wage garnishments produces a 12.2% annual increase in taxable earnings for an applicant who is granted debt relief.

To asses the impact of the fixed dividend (a payment from the debtor to creditors that is fixed by the court and independent of the debtor's future income), we treat the dividend as a lump-sum payment. Cesarini et al. (2017) study lottery winners in Sweden and estimate the impact of wealth on taxable earnings. Using their estimate in our example, the negative wealth effect from the lump-sum dividend corresponds to a 1.2% increase in annual earned income for an applicant who is granted debt relief.[20]

[19]This estimate is in the higher end of published estimates based on Danish data. Note that this estimate incorporates both the substitution and the income effects from a tax cut.

[20]We are implicitly assuming that there is no direct transfer of wealth from creditors to debtors, other than the transfer that takes place through a reduction in wage garnishments (a tax cut) and the payment of the dividend. The underlying rationale for this setup is that the prototypical debtor who is denied debt relief in our example i) will never be able to pay back all of the outstanding debt in his or her lifetime through wage garnishments no matter how much

If the impact of debt relief is the sum of the tax cut and the fixed dividend, the total impact on earned income is an increase of 13.4%. In comparison, our IV estimate shows that Danish applicants who are granted debt relief experience an increase in earned income of 26%. One way of describing our IV estimate is to ask how large the elasticity of earned income has to be to explain the observed increase in earned income. In our example with a removal of 20% wage garnishments, the required elasticity is 0.50. Another way of describing the IV estimate is to ask how large the tax cut has to be to explain the increase in earned income. In our example with an elasticity of taxable income of 0.26, the required tax cut is 32%.

While the estimated elasticity of taxable earnings that we use for our example is somewhat higher than other available estimates from Denmark, there are also good reasons to think that the response in earned income is high for applicants who receive debt relief. Debt relief is typically granted to individuals with a low employment rate and low wages (see Table A.5 in the Appendix). Previous research has shown that the response in labor supply to changes in the effective wage rate is higher at the extensive margin, especially for people at the lower end of the earnings distribution (Heckman (1993), Eissa and Liebman (1996)). There is also evidence suggesting that the elasticity of labor supply is bigger when workers are exposed to large wage shocks (Chetty et al. (2011)), as is the case for applicants who are granted debt relief and face a permanent and potentially large reduction in the effective tax rate.

Notwithstanding these arguments, our estimated response in earned income is so large that a mechanism operating through changes in taxes (demands on debtors from creditors) requires a large elasticity of taxable income. We therefore mention an additional mechanism that could influence applicants for debt relief. A literature in behavioral economics has considered how stress and financial worries reduce attention, cognitive capabilities, and executive powers (Mullainathan and Shafir (2013)). According to this argument, the financial problems of overindebted individuals limit their capacity to engage with creditors and improve their economic situation (they are in a

the debtor works and how little the debtor consumes (there is no wealth effect from the part of the debt that the debtor will never repay if denied debt relief), and ii) wage garnishments are at the maximum level (20% in our example) and will remain there irrespective of how much the debtor works.

"psychological poverty trap"). The permanent removal of all unsecured debt reduces the mental strain on applicants for debt relief and gives them the impetus to start working, work more hours, and/or switch to a job with better prospects. This propagation mechanism reinforces the behavioral response due to changes in the budget constraint and contributes to a labor supply elasticity that is especially high for applicants who are granted debt relief. This line of thought is also consistent with a long-lasting (permanent) impact of debt relief, reaching far beyond the repayment period.

5 Conclusion

Debt relief programs allow granted applicants to reduce their unsecured debt in court. There is limited evidence regarding the impact of these programs on individual debtors due to many data constraints. We overcome previous obstacles by hand-collecting data from court records and by linking these records to nationwide Danish registers.

The Danish debt relief commission wrote 40 years ago that individuals with excessive debt no longer try to improve their economic situation, lack the motivation to hold on to an existing job or find a new job, and rarely move out of welfare dependency. The results of our evaluation are consistent with this view and show that debt relief leads to a substantial increase in earned income and employment. A statistical decomposition shows that the rise in employment accounts for two-thirds of the increase in earned income, suggesting that high levels of debt can discourage individuals who do not work from entering the labor market.

The estimates for earned income and employment are large but similar in magnitude to previous results from the US, potentially due to a common mechanism that affects labor market behavior. We argue that debt relief strengthens the incentives to work and that being granted debt relief resembles a large and permanent decrease in the marginal tax rate. We further argue that the impact of debt relief on the budget constraint goes far (but maybe not all the way) towards explaining the rise in earned income for granted applicants.

Our evaluation of the Danish debt relief program also includes outcome variables such as assets, home ownership, real estate, and wealth. Combining income and wealth data shows that debt relief increases earned income and that much of this increase in income leads to an accumulation of assets and wealth. There is a strong impact of debt relief on secured debt, home ownership, and real estate which accounts for much of the increase in assets. For example, the IV estimates show an increase in home-ownership of 25 percentage points. After debt relief, there is a reduction in unsecured debt and an increase in secured debt, and these two changes are roughly equal in size (there is no significant change in total taxable debt when all years of follow-up are pooled together). Our interpretation of these patterns is that granted applicants regain access to mortgage loans and can purchase a new home, accumulate assets, and build net wealth. Such clear and long-lasting improvement in financial status may also trigger further future benefits well beyond simply owning a house or having positive net wealth (see e.g. Sodini et al. (2023)).

The most striking of our results is the long-run impact of debt relief. A natural concern is that debtors who have ended up with excessive unsecured debt and have been unable to resolve their economic issues by themselves, may benefit temporarily from debt relief but will eventually fall back into old habits of unsustainable debt accumulation. Expressed differently, debt relief programs do not resolve the fundamental issues that caused debtors to borrow too much. We are able to address these concerns with extensive follow-up data reaching as far as 25 years after a debt relief court decision. Given that applicants are on average 44 years old when they apply for debt relief in Denmark, our evaluation period covers the whole remaining phase in the life-cycle when a typical applicant for debt relief can be expected to be active in the labor market.

Both our event-study graphs and IV estimates show that the impact of debt relief on earnings and employment persists for at least 25 years, and also show that the impact on assets, real estate, and home ownership continues to grow over time. The net effect is that applicants who are granted debt relief accumulate more wealth throughout our entire follow-up period, with little to no indication of an eventual deterioration in economic status.

Altogether, our study strengthens the case for debt relief as an important escape route for over-indebted individuals, in the short- and long-run. We acknowledge that an overall assessment of debt relief programs has to compare the benefits with the costs of providing debt relief such as potential moral hazard, increases in interest rates, and a reduction in the supply of credit.

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Online Appendix to

The Long-Run Effects of Individual Debt Relief

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A Sample Selection

We dropped applicants who at any point in time were listed in Statstidende with a foreign address or an address on Greenland or the Faroe Islands. After the match with the CPR register, we also removed 21 persons with death dates prior to the first announcement in Statstidende and 4 persons with no known birth date. Figure A.1 plots the fraction of applicants who were granted debt relief as a function of time. The fraction is fairly constant from 1984 until 2003 and then falls quickly. The mechanical explanation for this pattern is that near the end of our initial sample period, we lack sufficient follow-up time to capture announcements stating that an applicant was granted debt relief. We therefore restrict our final sample to applicants from the start of the debt relief program in 1984 up until 2003, corresponding to 46,571 persons at 71 different City Courts (see Table A.2). In the final sample, the median time for granted applicants between a first announcement in Statstidende and the granting of debt relief is 7 months (the 90th percentile is 1 year and 9 months).

B Match with Danish Central Person Register

We have information on the name and address of applicants for debt relief from announcements in Statstidende. In order to add information on applicants from official Danish registers, we matched applicants to the Danish Central Person Register (the CPR register) containing the official names, addresses, and identification numbers of all Danish residents.

The CPR register lists the current name of residents and previous names of residents who have changed names. The CPR register also lists the full address of residents, consisting of municipality, street name, street number, street letter, floor, and side of floor (for apartment buildings only).

We performed the matching with a large subset of the CPR register made up of residents on any one of the streets mentioned in announcements on debt relief in Statstidende. Our subset of the CPR register did not include residents with protected addresses, and was restricted to residents on a street from two years before to five years after an announcement in Statstidende.

Overall, we were able to match 97.0% of announcements in Statstidende to unique individuals in the CPR register. We conducted the matching process in steps, first identifying individuals using complete information and then gradually relaxing the matching criteria. Table A.1 describes the overall match rate and lists the main types of matches that we used.

The first and largest category of matches are exact matches on full name and address (68.7% of announcements in Statstidende). These matches were achieved with the full name (no spelling error allowed) and the address (always municipality, street name, and street number, and possibly also street letter, floor, and side of floor). The second category are exact matches on full previous name and address (10.8% of announcements in Statstidende).

The third category of matches are what we refer to as comprehensive matches on name and address (5.4% of announcements in Statstidende). One type of match in this category are cases where the name in Statstidende was "contained" in the official name. A hypothetical person in Statstidende JENS ANDERS PEDERSEN might have been matched to a person on the same address with the official name JENS PREBEN ANDERS PEDERSEN. Another type of match involved a change in the order of names. A hypothetical person JENS ANDERS PEDERSEN in Statstidende might have been matched to a person on the same address with the official name ANDERS JENS PEDERSEN.

The fourth and final category are fuzzy matches on name and address (12.2% of announcements in Statstidende). One type of match in this category are cases where the spelling of a name in Statstidende deviated slightly from the spelling of the official name. A hypothetical person in Statstidende JENS ANDERS PEDERSEN might have been matched to a person on the same address with the official name JENS ANDERS PETERSEN. More formally, we allowed for a maximum distance of 15 between a name in Statstidende and the official name as defined by the SPEDIS function in SAS. Another type of match are cases where the name of a person in Statstidende and the official name of the matched person in the CPR register agreed fully, but there was a slight deviation between the addresses. A hypothetical person JENS ANDERS PEDERSEN listed in Statstidende as living in a given municipality on a given street on street number 67, for example, could be matched to a person in the CPR register living in the same municipality, on the same street, but on street number 57 (a one-digit deviation between the street number in Statstidende and the official street number in the CPR register).

C Official Statistics on Debt Relief

Statistics Denmark (Danmarks Statistik) and the Courts of Denmark ("Danmarks Domstole") publish annual official statistics on the number of applicants for debt relief, the number of opened investigations on debt relief, and the number of granted applications for debt relief. Table A.3 list these statistics from 1984 to 2020 (the number of investigations and the number of approved applications are not available in all years).

Over the period from 1985 to 2020, an average of about 5500 individuals in Denmark applied for debt relief each year according to the official statistics.[21] The average adult population (between 18 and 80 years of age) in Denmark from 1985 to 2000 was 4.1 million, meaning that about 1 in 750 adult Danes (or 0.13%) applied for debt relief each year. Out of the total number of applicants from 2002 to 2020 (when data is available), 46% of applicants were investigated by the local City Court. From 1988 to 2020 (when data is available), approximately 32% of all original applicants were granted debt relief.[22]

In Figure A.11 we plot the number of applicants for debt relief from 1985 to 2020 and the unemployment rate in Denmark (from the OECD main economic indicators). As found in previous studies from the US, there is a strong relationship in Denmark between the state of the labor market and the number of applications for debt relief.

C.1 Data Sources for the Official Statistics

The statistics on applicants from 1984 to 1997 and granted applications from 1991 to 1997 are available in a series of statistical messages from Statistics Denmark (Statistiske Efterrettninger,

[21] We exclude data for 1984 from the calculation since the debt relief program was initiated on July 1st 1984.

[22] This fraction of granted applications is an approximation since the people who are granted debt relief in a year are not necessarily the same people who applied for debt relief in that year (there is a time lag from application to decision which we disregard in this approximation).

Social Sikring og Retsvæsen) with publication numbers 1986:6, 1987:6, 1988:7, 1989:10, 1990:5, 1991:8, 1992:6, 1993:6, 1994:5, 1995:8, 1996:9, 1997:8, and 1998:11.

The statistics on applicants and granted applications from 1998 to 2001 are available in annual publications from Statistics Denmark (Kriminalitet 1998, Kriminalitet 1999, Kriminaliet 2000, and Kriminalitet 2001).

The statistics on debt relief from 2002 to 2020 are available in annual statistical messages published by the Courts of Denmark on their webpage (www.domstol.dk). The number of applicants and the number of opened investigations are published in a series on the number of insolvency cases handled by the Danish City Courts (Statistik for skiftesager: Modtagne sager om insolvensskifte m.v.). The number of approved applicants is published in a different series (Statistik for skiftesager: Afsluttede sager om insolvensskifte m.v.).

We have not found official statistics on the number of granted applications for debt relief for the period prior to 1991. Statistics for the years 1988 to 1990 are available in the proposed Swedish law on debt relief, introduced by the government to parliament in 1994 (Regeringens proposition 1993/94:123, Skuldsaneringslag). The text in the proposed bill cites sources in the Danish Ministry of Justice but does not refer to a specific publication.

D Decomposition of Change in Earned Income

We conduct a simple decomposition of the impact of debt relief on earned income into an effect on employment (the extensive margin) and an effect on the earned income of individuals who are employed (the intensive margin). The framework we use is based on a previous study by Blundell, Bozio and Laroque (2011) who decompose changes in labor supply along extensive and intensive margins.

The earned income I_{it} of individual i in year t can be written as the product

$$I_{it} = P_{it} \cdot E_{it} \tag{7}$$

where P_{it} is an indicator for individual i working in year t, and E_{it} is the earned income of the individual in that year if he or she is working. We perform a linear decomposition where the change in earned income, ΔI, is

$$\Delta I = \Delta P \cdot E + P \cdot \Delta E \tag{8}$$

The first of the terms in the decomposition, $\Delta P \cdot E$, is defined as the extensive margin change and the second of the terms, $P \cdot \Delta E$, is defined as the intensive margin change.

In our application, earned income is changing from an initial time period (before debt relief) which we denote by $t = 0$, to a later time period (after debt relief) which we denote by $t = 1$. There are two exact decompositions of the change in earned income over this time period:

$$\Delta I = I_1 - I_0 = (P_1 - P_0) \cdot E_0 + P_1 \cdot (E_1 - E_0) \tag{9}$$

$$\Delta I = I_1 - I_0 = (P_1 - P_0) \cdot E_1 + P_0 \cdot (E_1 - E_0) \tag{10}$$

The first decomposition (9) weights the change in the employment rate by the earned income of those who work in the initial time period (before debt relief), and the second decomposition (10) weights the change by the earned income of those who work in the later time period (after debt relief). As a consequence, there are two possible expressions for the share, S_E, of the change in earned income that can be attributed to changes in employment (the extensive margin):

$$S_{E0} = \frac{\Delta P \cdot E_0}{\Delta I} \tag{11}$$

$$S_{E1} = \frac{\Delta P \cdot E_1}{\Delta I} \tag{12}$$

To implement the decomposition method above, we set the change in earned income, ΔI, from before to after debt relief equal to our instrumental variable estimate for the impact of debt relief on earned income

$$\Delta I = 46,800 \tag{13}$$

Similarly, we set the change in employment, ΔP, equal to our instrumental variable estimate for the impact of debt relief on employment

$$\Delta P = 0.117 \tag{14}$$

Finally, we weight the change in the employment rate by the mean earned income of those who work during the four years prior to the year of application for debt relief, or the mean earned income of those who work during the 16 years after the year of application. The shares that we obtain are then

$$S_{E0} = \frac{\Delta P \cdot E_0}{\Delta I} = \frac{0.117 \cdot 228,200}{46,800} \approx 0.57 \tag{15}$$

$$S_{E1} = \frac{\Delta P \cdot E_1}{\Delta I} = \frac{0.117 \cdot 306,900}{46,800} \approx 0.77 \tag{16}$$

The mean of these two estimated shares is 0.67, indicating that the impact of debt relief on employment (the extensive margin) accounts for in the order of two thirds of the impact of debt relief on earned income.

E Subgroup Effects

To further understand the impact of debt relief, we estimate our IV model in subsamples based on marital status, sex, age, education, income, wealth, debt, and economic conditions (applying in a recession). These variables are all measured prior to application. We present the estimates for our main outcomes in Tables A.23 and A.24. The impacts of debt relief that we estimate based on the full sample are also present in most of these subsamples. Due to high uncertainty in the estimates across subgroups, we should interpret any differences cautiously. Further keep in mind that the division into subgroups is based on relative comparisons of applicants, such that e.g. high income individuals are still individuals with relative low income compared to the general Danish population, see Table A.5.

Our estimates suggest that women have larger earnings and employment impacts compared to men. Relative to non-granted means, the impact on earnings corresponds to an increase of 29% compared to 18% for men. The impact of debt relief on wealth is around twice as high for men compared to women. This is primarily explained by a larger reduction in unsecured debt for men. For age, we find that workers below the age of 45 have larger earnings impacts, but there is no difference relative to non-granted means. Workers above age 45 have larger impacts for wealth which is largely explained by larger declines in unsecured debt compared to workers below age 45. We do not find a lot of heterogeneity in impacts based on marital status.

Workers with low education have larger employment impacts than workers with high education (a 25% compared to a 15% increase relative to non-granted means). At the same time, workers with high education display larger earnings impacts both in absolute and relative terms. Workers with high education also have larger increases in wealth largely explained by larger reductions in unsecured debt. Workers with high education experience larger increases in assets offset by larger increases in secured debt. Relative to non-granted means, we find that individuals with below median income prior to applying have larger impacts (39%) than individuals with high income (22%). A part of this difference comes from a larger increase in employment among the low income group where the estimated impact is a 16 percentage points increase in employment. For wealth, the difference in impacts is small, but we do find that the high income group has a larger increase in assets and secured debt.

Distinguishing individuals based on the amount of debt prior to application, we find limited heterogeneity in employment and earnings impacts. Unsurprisingly, we find larger increases in wealth and assets, and a larger decrease in unsecured debt for individuals with large amounts of debt prior to applying. In terms of wealth, we find larger increases in employment (a 16 percentage points difference) and earnings for individuals with above median wealth (non-granted means show that these are still individuals with negative wealth). Relative to non-granted means, the earnings impacts correspond to an increase of 38% for the above median wealth group and 23% for the below median wealth group. On the contrary, we see the largest increases in wealth for the low wealth group, mostly explained by larger reductions in unsecured debt. Finally, we also find that individuals applying in a recession have larger employment impacts than applicants in other years, potentially reflecting more positive selection into debt relief during recessions (consistent with non-granted means). For earnings, impacts are smaller (in absolute and relative terms) for individuals applying in a recession.

F Characteristics of Compliers

We use the method of Dahl, Kostøl and Mogstad (2014) to describe compliers in the context of a continuous instrument (the trustee admission rate). Compliers are, by definition, those applicants who would be granted debt relief if assigned to the least strict trustee but not granted debt relief if assigned to the strictest trustee.

Let $\bar{z}$ be the admission rate of the least strict trustee and let $\underline{z}$ be the admission rate of the strictest trustee, and let D_i be an indicator for treatment status. The share of compliers in the population, π_c, is then

$$\pi_c = Pr(D_i = 1|z_i = \bar{z}) - Pr(D_i = 1|z_i = \underline{z}) = Pr(D_i(\bar{z}) > D_i(\underline{z})) \tag{17}$$

Because of monotonicity, the share of always takers who receive debt relief for all values of the instrument, π_a, is

$$\pi_a = Pr(D_i = 1|z_i = \underline{z}) = Pr(D_i(\bar{z}) = D_i(\underline{z}) = 1) \tag{18}$$

and the share of never-takers who never receive debt relief regardless of the value of the instrument, π_n, is

$$\pi_n = Pr(D_i = 0|z_i = \bar{z}) = Pr(D_i(\bar{z}) = D_i(\underline{z}) = 0) \tag{19}$$

To estimate these shares in our sample, we let the strictest and least strict trustee correspond to the bottom and top 1 percentiles of the trustee admission rate. The estimated first stage linear regression equation gives the predicted relationship between debt relief status and the instrument (see equation 2). Based on the estimated first stage equation, we set the share of compliers equal to the predicted fraction receiving debt relief at the top percentile of the trustee admission rate minus the predicted fraction at the bottom percentile, the share of always takers to the predicted fraction receiving debt relief at the bottom percentile of the admission rate, and the share of never takers to the predicted fraction not receiving debt relief at the top percentile of the admission rate:

$$\hat{\pi}_c = \hat{\eta} \cdot (\bar{z} - \underline{z}) \tag{20}$$

$$\hat{\pi}_a = \hat{\chi} + \hat{\eta} \cdot \underline{z} \tag{21}$$

$$\hat{\pi}_n = 1 - \hat{\chi} - \hat{\eta} \cdot \bar{z} \tag{22}$$

Implementing these formula gives us an estimated 22% compliers, 63% always takers, and 15% never takers.

The distribution of observable characteristics among compliers can be obtained by estimating the share of compliers in subsamples (Abadie, 2003). For a binary characteristic $X \in 0,1$, the definition of a conditional probability and the monotonicity assumption implies that

$$\begin{gathered} \frac{Pr(X_i = 1 | D_i(\bar{z}) > D_i(\underline{z}))}{Pr(X_i = 1)} = \\ \frac{Pr(D_i(\bar{z}) > D_i(\underline{z}) | X_i = 1)}{Pr(D_i(\bar{z}) > D_i(\underline{z}))} = \\ \frac{\mathbb{E}(D_i | Z_i = \bar{z}, X_i = 1) - \mathbb{E}(D_i | Z_i = \underline{z}, X_i = 1)}{\mathbb{E}(D_i | Z_i = \bar{z}) - \mathbb{E}(D_i | Z_i = \underline{z})} \end{gathered} \tag{23}$$

The nominator in this right-hand expression is the share of compliers in the subsample with $X = 1$, and the denominator is the share of compliers in the whole sample. We estimate these shares (as above) using the predicted values from the first stage (in the whole sample and in subsamples) at the top and bottom 1 percentiles of the trustee admission rate. We then multiply the estimated ratio (23) by the marginal probability, $Pr(X_i)$, to obtain the distribution of the characteristic, $Pr(X_i|D_i(\bar{z}) > D_i(\underline{z}))$, among compliers. These numbers are presented in Table A.26.

In Figure A.7, we reweigh our event-study estimates to match the sample of compliers based on observable characteristics. We follow Dahl, Kostøl and Mogstad (2014); Bhuller et al. (2020); Agan et al. (2023) and estimate propensity scores as a function of baseline covariates and split our sample into quintiles based on the propensity score. We then estimate the proportion of compliers separately for each quintile (like Table A.26). Lastly, we use the quintile specific share of compliers relative to the full estimation sample share and reweight our event-study regressions accordingly.

G Marginal Treatment Effects

To explore treatment effects heterogeneity by unobserved characteristics, we use the Marginal Treatment Effect (MTE) framework (Heckman and Vytlacil (2005, 2007)). Modeling observed outcomes in the framework of potential outcomes (and following Bhuller et al. (2020)) we can write

$$Y_i = D_i * Y_i(1) + (1 - D_i) * Y_i(0)$$

where D_i is a dummy equal to one if individual i is granted debt relief. The decision to grant debt relief is determined by a choice function given as $D_i = \mathbb{1}\{\upsilon(X_i, Z_i) - V_i\}$, where X_i are observable characteristics of the applicant, Z_i is the acceptance rate of the trustee assigned to individual i, $\upsilon()$ is an unknown function, and V_i is an unobserved continuous variable. Applicants are granted debt relief if $\upsilon(X_i, Z_i) \geq V_i \Longrightarrow F_V(\upsilon(X_i, Z_i)) \geq F_V(V_i)$ where F_V is the cumulative distribution of V. Let $F_V(\upsilon(X_i, Z_i)) = P(Z_i, X_i)$ where $P(Z_i, X_i)$ is the propensity score of being granted debt relief conditional on the trustee acceptance rate Z_i and observed characteristics X_i. $F_V(V_i)$ can then be defined as the unobserved resistance to getting debt relief. The Marginal Treatment Effect is defined as $E(Y(1) - Y(0)|X = x, F_V(V) = F_v)$, which can be interpreted as the treatment effect for individuals at the margin $P(Z, X) = F_v$.

In our preferred setting, we trim observations at 5 % of the common support range of treatment propensities to remove noise in the tails when estimating the MTE curve. We estimate the MTE using a quadratic polynomial for the control functions $k_j(P)$, which capture heterogeneity in the outcome as a function of the unobserved resistance evaluated at $F_V = P$ (we use the STATA package mtefe by Andresen (2018)).

Figure A.9 shows the estimated MTEs for our six main outcomes using our implementation of the MTE framework. For earnings, employment, assets and (un)secured debt, the MTE curve is upward sloping although statistical uncertainty implies that we cannot rule out that the MTE curves could also have other shapes.[23] An interpretation of the upward-sloping MTE curves is that individuals on the margin who are pushed into treatment by trustees with a high acceptance rate have the largest treatment effects from getting debt relief. This suggests that applicants who have the highest benefits from debt relief are the least likely to get through the system. Such impact heterogeneity would be consistent with the LATE parameter being larger than the ATT, and could therefore explain why our event-study estimates are lower than IV estimates in Tables 2 and 3. Tables 2 and 3 do in fact show that IV estimates are larger for all these outcomes except for unsecured debt. In addition, the MTE curve is flat for wealth consistent with our event-study and IV estimates for wealth being of similar magnitude as we report in Table 3.

An advantage of the MTE framework is that we can express other treatment effects as weighted averages of the MTEs. Through the MTE framework, we can also calculate the ATT and the LATE within our region of common support, following Carneiro (2011). Performing these calculations shows that the LATE estimate is higher than the equivalent ATT estimate for earnings. This pattern is also consistent with our prior findings that the event-study yields an estimated ATT parameter which is lower than the LATE parameter estimated from the IV model. The ATU for earned income is 36,861, while the ATT is 17,062. Hence, the difference we observe between our event-study and

[23]The upward-sloping shape of the MTE curve is consistent across specifications. In Appendix Figure A.9, we show robustness of different functional forms for the outcomes of earnings and employment. We show robustness regarding the degrees of the polynomial (third or fourth degree), different ranges of trimming at 1 % and 2.5 %, estimation by the local IV approach, and a semi-parametric approach using splines. Our results are also in alignment with Dobbie and Song (2015) who also find an upward-sloping MTE for earnings.

IV estimates can potentially be attributed to the different parameters that these two econometric models identify.

H Labor Supply Mechanism

We consider an example where the impact of debt relief on an applicant's budget constraint is two-fold: i) the applicant is no longer subject to 20% wage garnishments, and ii) the applicant has to pay a dividend to the creditors.

H.1 Tax effect due to wage garnishments

Kleven and Schultz (2014) present marginal income tax rates for tertiles of Danish tax payers in their Table 2. If we assume that applicants for debt relief belong to the lowest tertile, the mean marginal tax rate for these applicants over the period from 1986 to 2003 was 44.5%. The removal of 20% wage garnishments leads to the following change in the log net-of-tax rate when an applicant is granted debt relief

$$\Delta log(1-\tau) = log(1-0.445-0.2) - log(1-0.445) \tag{24}$$

Using the estimated elasticity of earnings with respect to the net-of-tax rate from Kleven and Schultz (2014) of 0.257 gives an implied change in log earnings of 0.115. Converting this log change to a percentage change gives us an increase in earned income for applicants who are granted debt relief of 12.2%.

H.2 Wealth effect due to dividend

The mean debt of individuals in the repayment sample is 1.71 million DKK and the mean dividend is 10.3% which implies that the average applicant who is granted debt relief has to repay 176,100 DKK to the creditors (a negative wealth effect). Cesarini et al. (2017) estimate that an increase in wealth of 100 SEK leads to an annual decrease in taxable earnings of 1.07 SEK. This estimate translates into an increase in annual earned income for an applicant who is granted debt relief of 1878 DKK. The mean earned income of granted applicants in the year before application is 161,000. Combining these numbers produces an increase in earned income due to the dividend of 1.2% for applicants who are granted debt relief.

I Fiscal Consequences of Debt Relief

We take the first steps towards assessing the fiscal impact of the Danish debt relief program. We consider only direct effects on the government budget and ask what the consequences are if one more applicant is granted rather than denied debt relief.[24] We do not consider equilibrium effects such as the impact of the debt relief program on interest rates and the supply of credit. We base our assessment on the IV estimates that describe the long-run effect of debt relief over our sixteen-year follow-up period (Tables 2 and A.14).

The first fiscal benefit from granting debt relief is the increase in tax revenue that follows from higher earned income (Table 2). We assume that applicants for debt relief belong to the lowest tertile income bracket and use the mean marginal tax rate of 44.5% from 1986 to 2003 (Kleven and Schultz (2014)). The second fiscal benefit are lower costs for social assistance and disability insurance (Table A.14) which we assume are not taxed. We discount all flows at a rate of 2% and express all numbers in thousands of DKK. The sum of the present discounted value of higher tax revenues, lower social assistance, and lower disability insurance payments is

$$288 + 34 + 15 = 337 \tag{25}$$

It is difficult to evaluate the fiscal cost of granting one more applicant debt relief, as the cost likely depends on whether debt is private or public. In the case of private debt, financial institutions can

[24]We ignore administrative costs associated with the handling of cases (e.g. trustee salary) as these costs are largely independent of the outcome of the debt relief decision process.

deduct the credit loss they incur when an applicant is granted debt relief at the full book value of the debt and reduce their corporate income tax. If we use the mean size of the debt (1710) and dividend (10.3%) in the repayment sample, and assume that financial institutions pay a corporate income tax of 37.8% (the mean from 1986 to 2003),[25] the cost in terms of lower tax revenue is

$$1710 \cdot (1 - 0.103) \cdot 0.378 \approx 580 \tag{26}$$

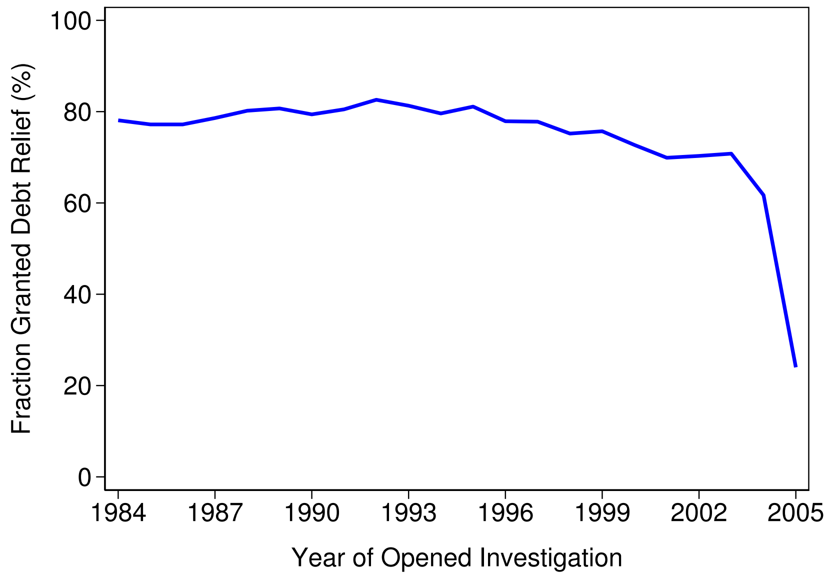
Subtracting our estimated benefits from costs gives a net fiscal cost per granted applicant for debt relief of $580 - 337 = 243$ (two hundred fourty-three thousand DKK or approximately thirty-seven thousand USD).

To assess the fiscal consequences when debt is public, we need to know what fraction of debt is repaid by applicants who are denied debt relief (information which we do not have) in order to assess the "true" value of the outstanding debt. Our calculation above is valid if the present discounted value of future repayments made by denied applicants with public debt, equals the loss to the government when debt is private (denied applicants repay a fraction $(1 - 0.103) \cdot 0.378 \approx 0.34$ of their public debt). The fiscal cost of debt relief is then independent of whether debt is private or public. We leave it to future investigations to determine if this is a reasonable assumption.

[25]Retrieved from the homepage of the Danish Tax Ministry at www.skm.dk/skattetal/satser/tidsserier.

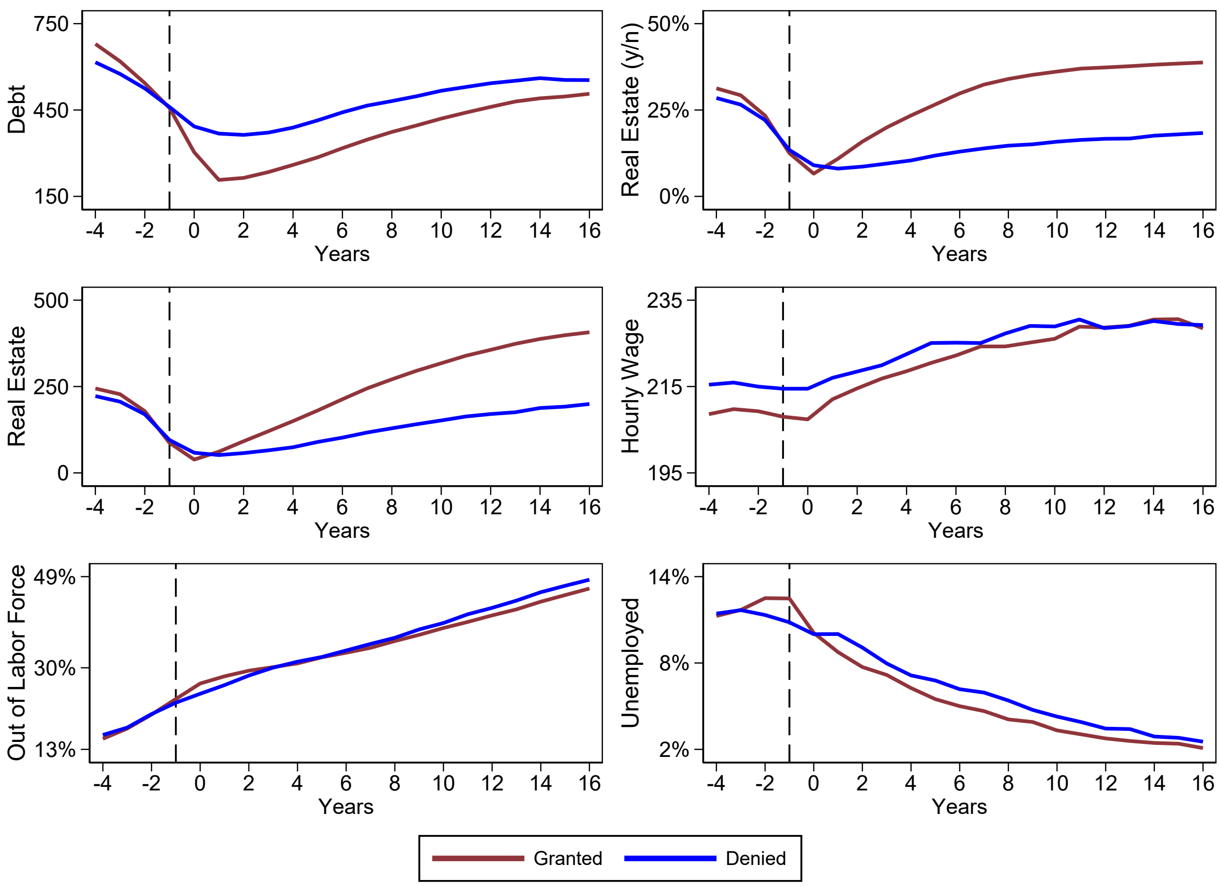
J Figures

Figure A.1: Fraction Granted Debt Relief in Initial Sample



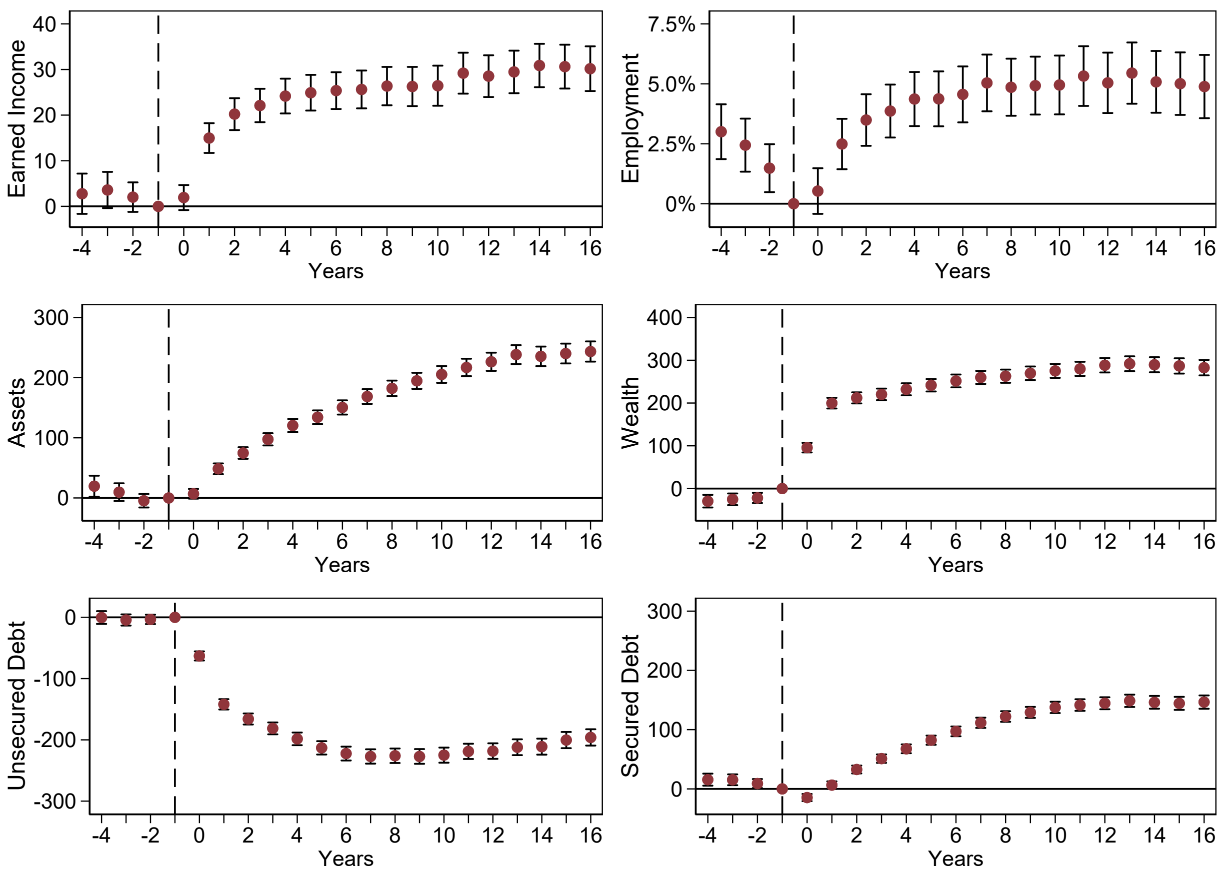
Notes: This graph shows the fraction of applicants in our initial sample from 1984 to 2005 who were eventually granted debt relief (the number of granted applicants divided by the number of applicants for which the City Court opened an investigation).

Figure A.2: Mean Outcomes Before and After Application for Debt Relief



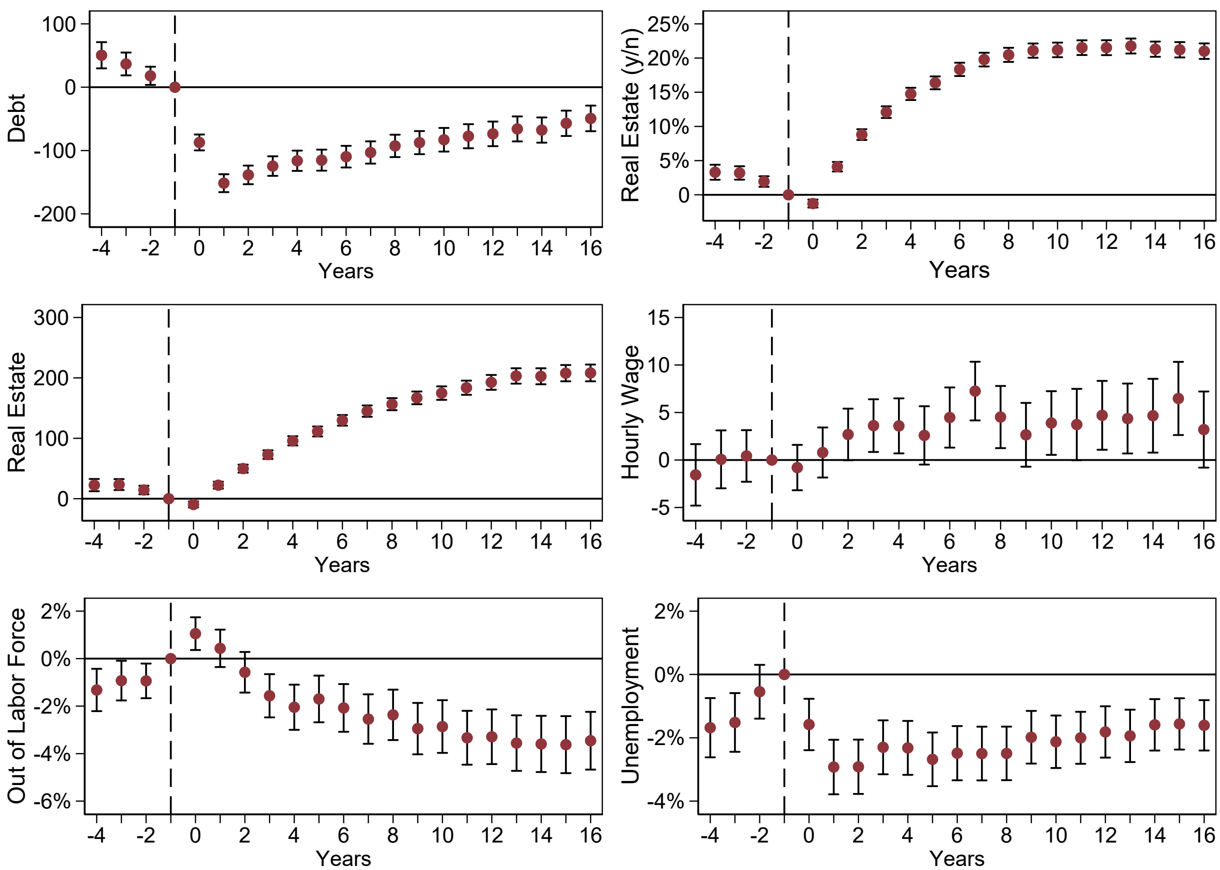
Notes: This graph shows mean outcomes for granted and denied applicants for debt relief from 4 years before to 16 years after the year of application. The outcome variables are taxable debt (top left), the fraction of real estate owners (top right), taxable real estate (middle left), the hourly wage rate among those who are employed (middle right), the fraction out of the labor force, (bottom left), and the fraction unemployed (bottom right). Monetary unit is thousands of 2020 DKK.

Figure A.3: Event-Study Graphs



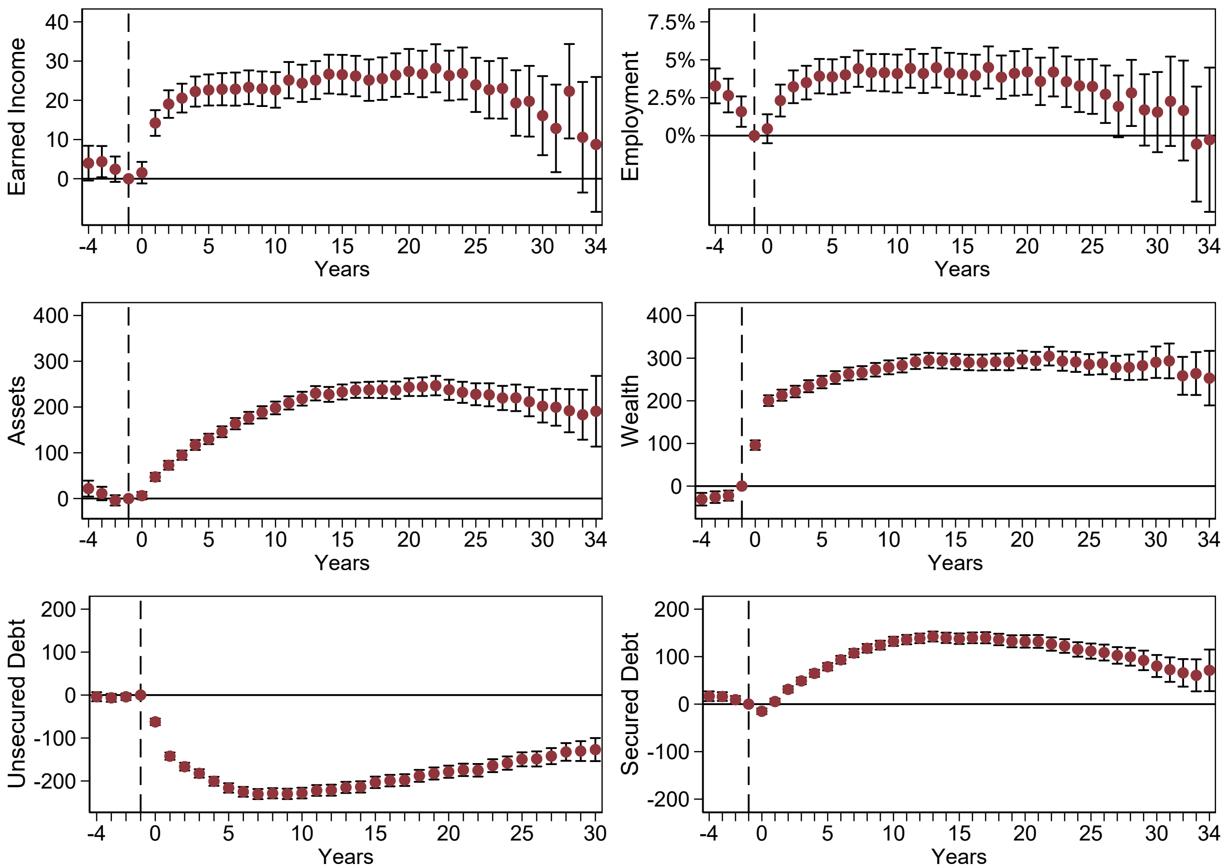
Notes: This graph shows estimated event-study coefficients from 4 years before to 16 years after the year of application comparing granted and denied applicants for debt relief. The outcome variables are earned income (top left), employment (top right), taxable assets (middle left), taxable wealth (middle right), unsecured taxable debt in banks and other financial institutions (bottom left), and secured taxable debt in banks and other financial institutions (bottom right). Standard errors are clustered at the level of the debtor. Monetary unit is thousands of 2020 DKK.

Figure A.4: Event-Study Graphs



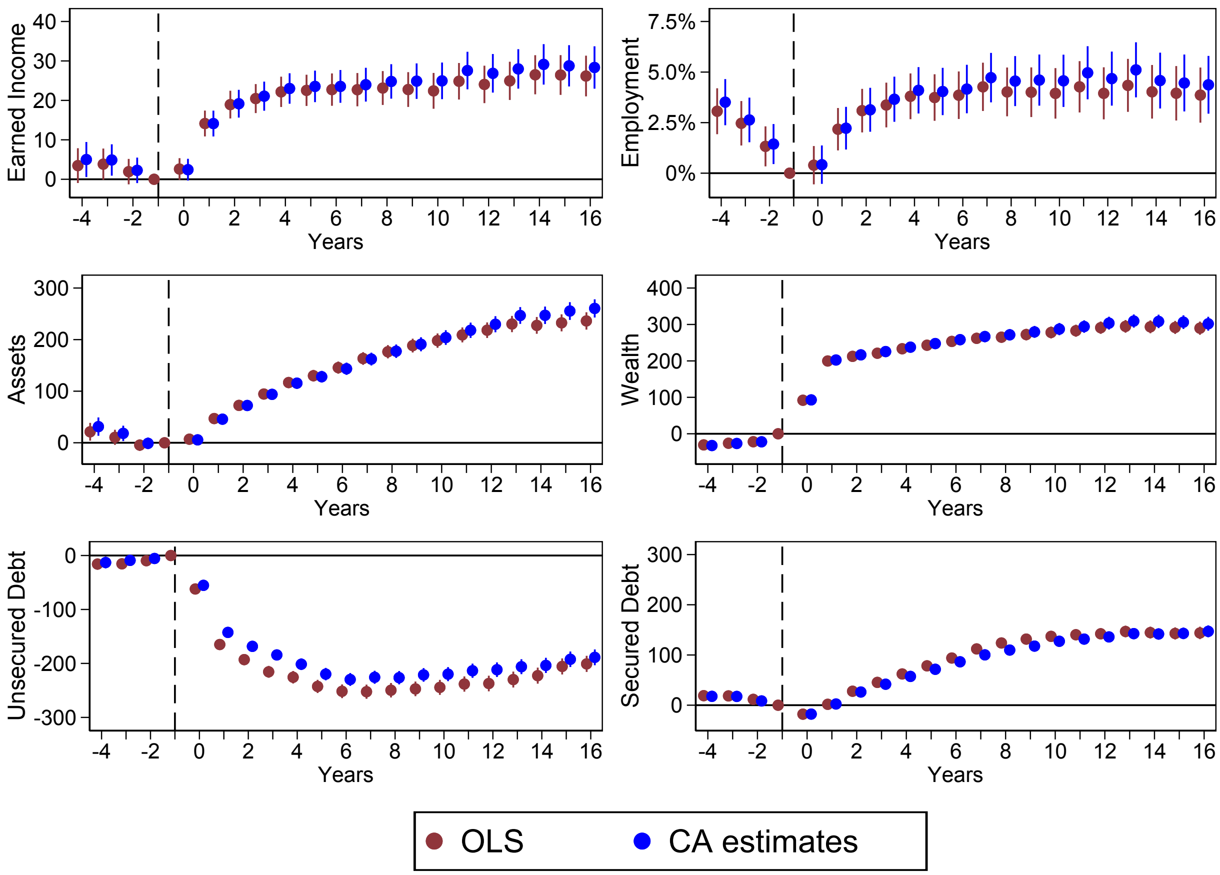
Notes: This graph shows estimated event-study coefficients from 4 years before to 16 years after the year of application comparing granted and denied applicants for debt relief. The outcome variables are taxable debt (top left), the fraction of real estate owners (top right), taxable real estate (middle left), the hourly wage rate among those who are employed (middle right), the fraction out of the labor force, (bottom left), and the fraction unemployed (bottom right). Standard errors are clustered at the level of the debtor. Monetary unit is thousands of 2020 DKK.

Figure A.5: Event-Study Graphs for 34 Years



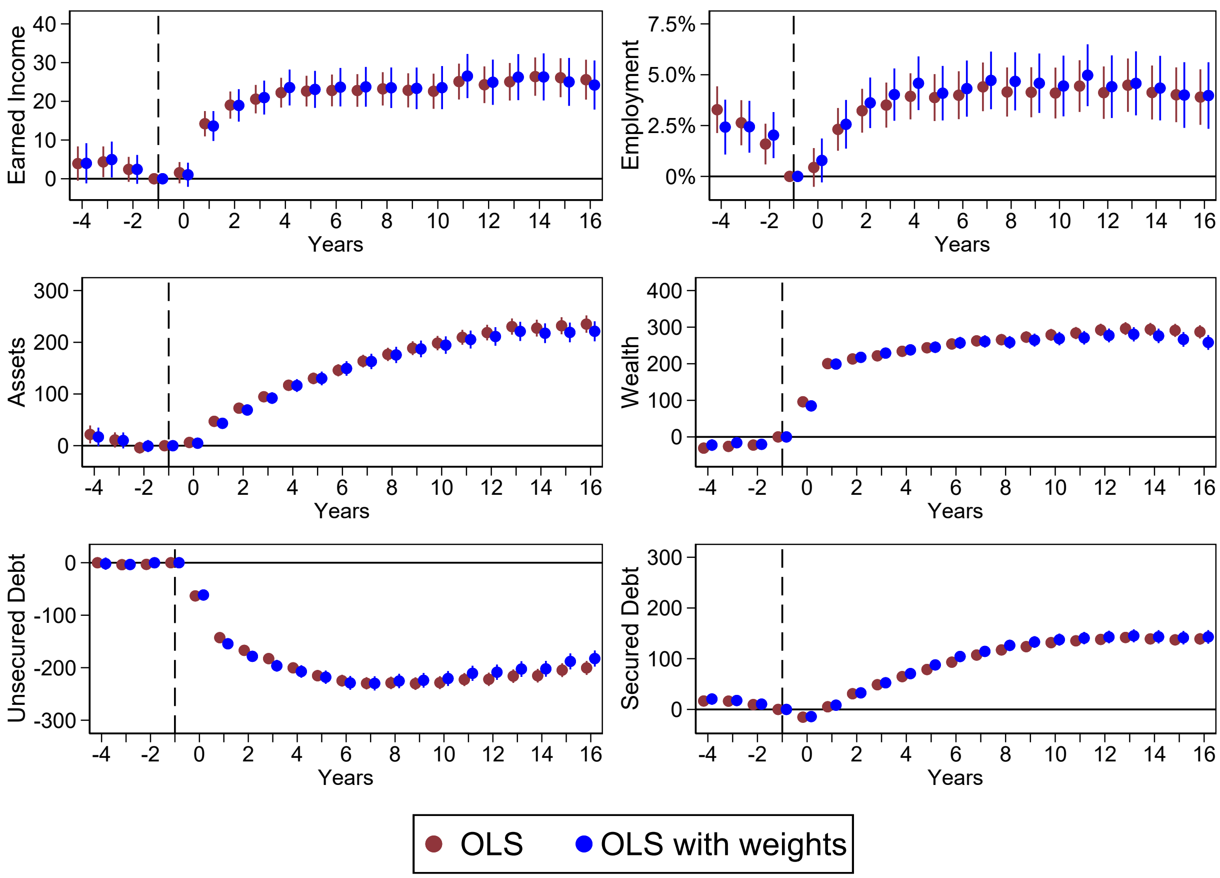
Notes: This graph shows estimated event-study coefficients from 4 years before to 34 years after the year of application comparing granted and denied applicants for debt relief. The outcome variables are earned income (top left), employment (top right), taxable assets (middle left), taxable wealth (middle right), unsecured taxable debt in banks and other financial institutions (bottom left), and secured taxable debt in banks and other financial institutions (bottom right). The panel is unbalanced in event time to extend the observation window as far as possible. Unsecured debt is extended to 30 years after application because this variable is available from 1987 only (see Table A.4). Standard errors are clustered at the level of the debtor. Monetary unit is thousands of 2020 DKK.

Figure A.6: Regular versus Callaway and Sant'Anna Event-Study



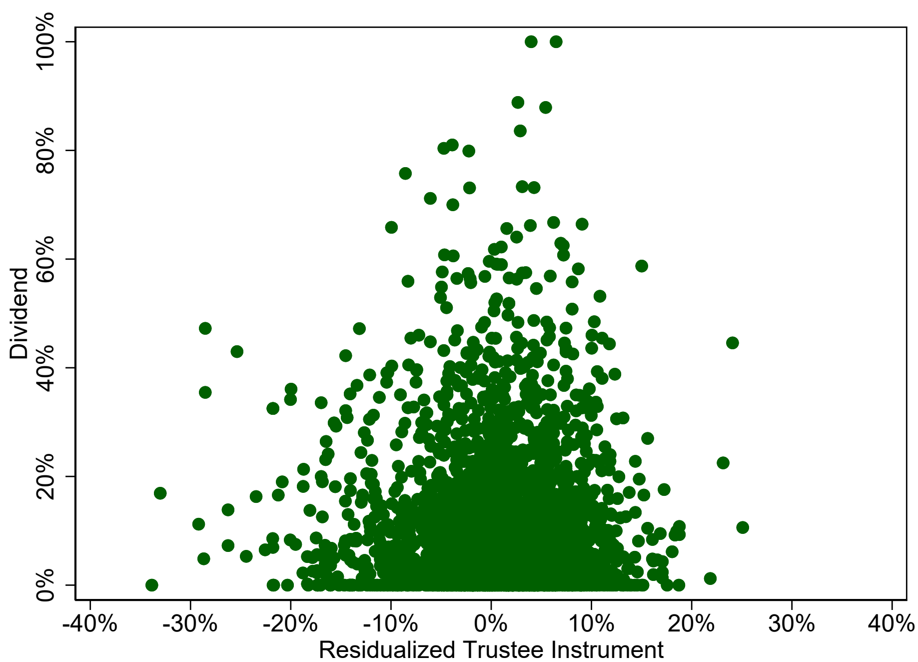
Notes: This figure contrasts event-study estimates obtained via the standard two-way fixed effects model (Equation 1 in the paper) to event-study estimates obtained using the estimator in Callaway and Sant'Anna (2021). All estimates were constructed via the "csdid" package in Stata (Rios-Avila et al. (2023)).

Figure A.7: Complier-Weighted versus Unweighted Event-Study



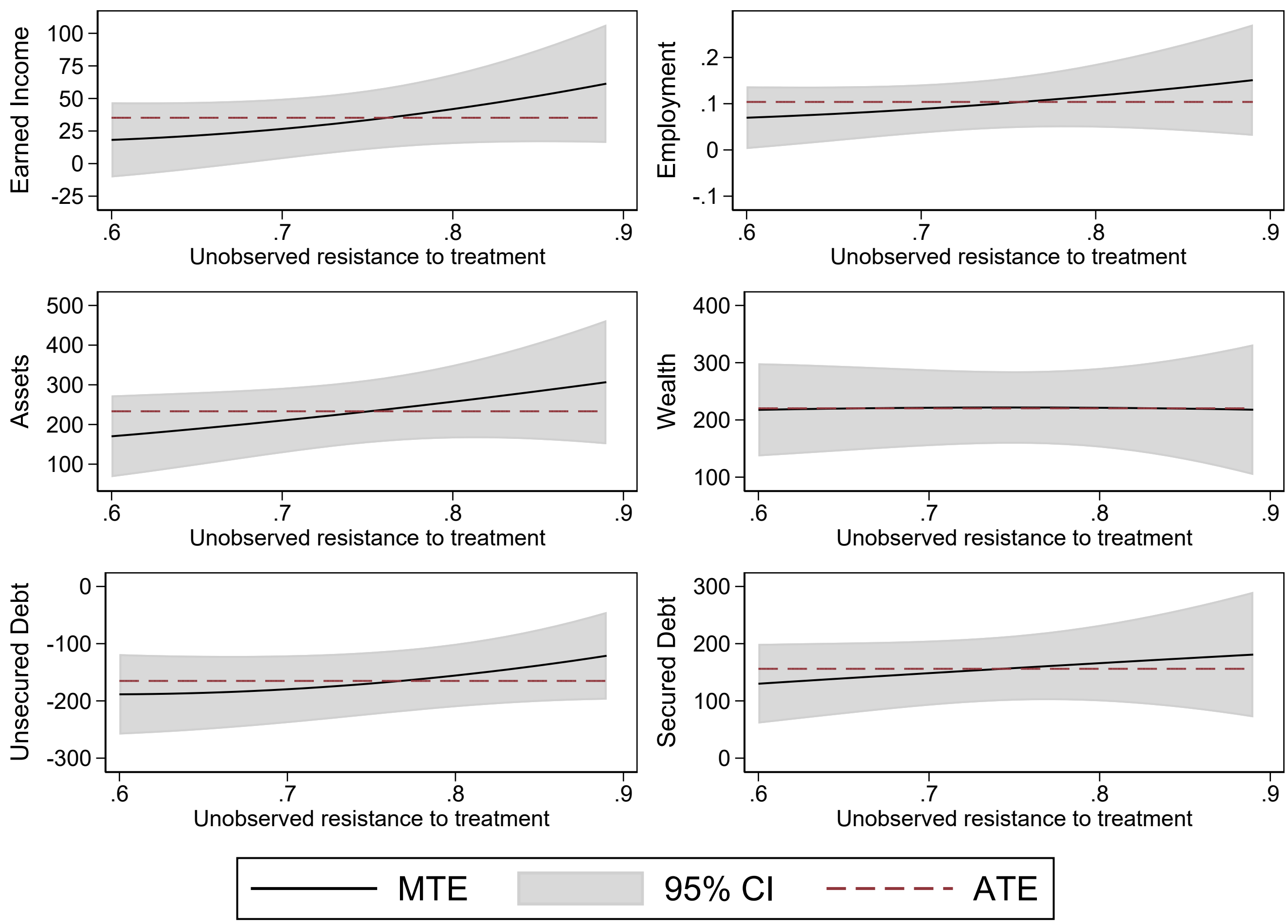
Notes: This figure plots our standard event-study estimates compared to similar estimates when using "complier weights" in the regression as in Dahl, Kostøl and Mogstad (2014); Bhuller et al. (2020). We estimate propensity scores based on our baseline covariates and split our sample into quintiles based on the propensity score. We then estimate the proportion of compliers separately for each quintile (as in Table A.26). Finally, we reweight our event-study regressions such that the share of compliers in each quintile matches the share of compliers in the full sample.

Figure A.8: Dividend and Trustee Instrument



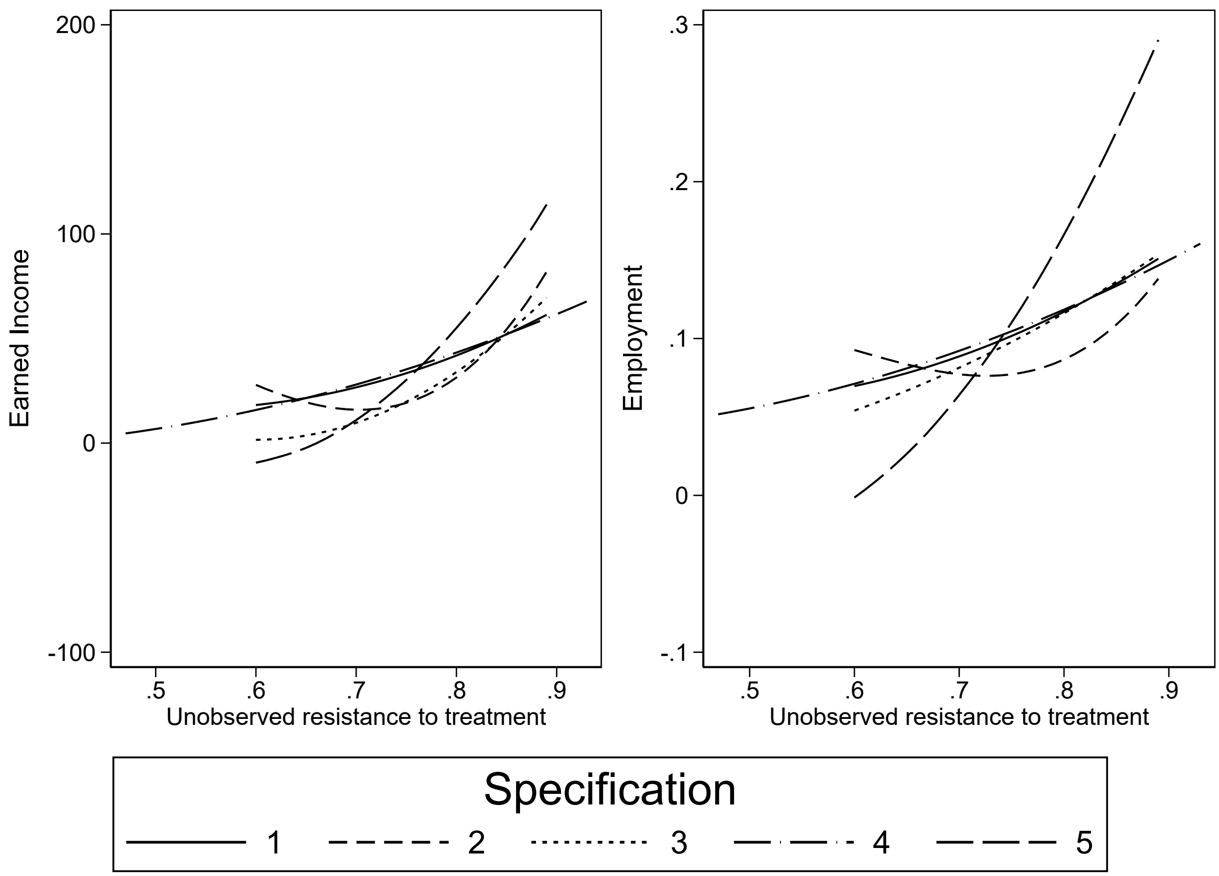
Notes: This graph shows a scatter plot of the dividend among applicants who were granted debt relief and the residualized trustee instrument (the normalized admission rate of trustees conditional on court-by-year fixed effects). Data is from the repayment sample (n=2591).

Figure A.9: MTE Estimates



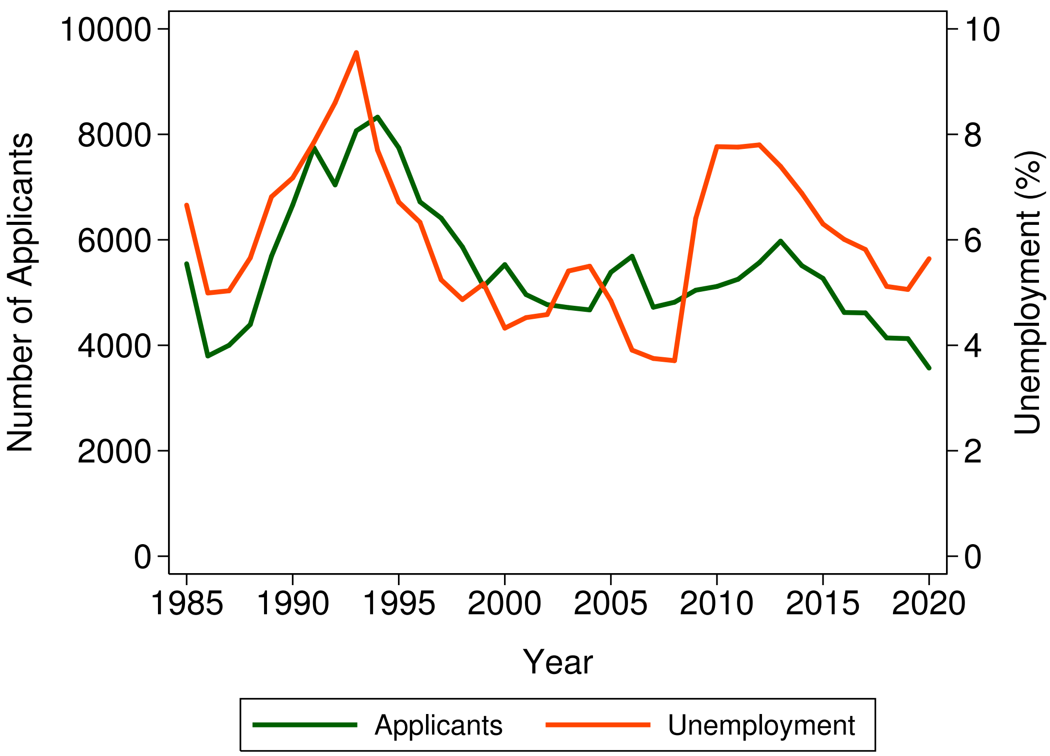
Notes: This figure shows marginal treatment effects (MTEs) for our six main outcomes in the 16 years after application. Propensity scores are predicted using a logit regression, including our baseline covariates and court-by-time fixed effects. We trim observations at the 5% level to remove noise in the tails of the distribution. The MTEs are estimated using the separate approach with a second-order polynomial. We use the STATA package mtefe by Andresen (2018). The outcome variables are earned income (top left), employment (top right), taxable assets (middle left), taxable wealth (middle right), unsecured taxable debt in banks and other financial institutions (bottom left), and secured taxable debt in banks and other financial institutions (bottom right). Monetary unit is thousands of 2020 DKK.

Figure A.10: MTE:s with Different Functional Forms



Notes: This figure shows the estimated MTEs for five different specifications: 1 is our baseline specification (as in A.9), 2 changes the order of polynomials to three compared to the baseline specification, 3 uses the local IV approach to estimate our baseline specification, 4 trims observations at the 1% level as opposed to 5% in the baseline specification, and 5 is a semi-parametric specification. We use the STATA package mtefe by Andresen (2018). The outcome variables are earned income (left), and employment (right). Monetary unit is thousands of 2020 DKK.

Figure A.11: Number of Applicants for Debt Relief and the Unemployment Rate



Notes: This graph shows the annual number of applicants for debt relief in Denmark (left axis) and the annual unemployment rate (right axis). Data on applicants is from the official statistics of Denmark (see section C) and data on the unemployment rate is from OECD (main economic indicators). Data for 1984 is excluded since the debt relief program was only introduced in July of that year.

K Tables

Table A.1: Match with Central Person Register

Type of Match	Frequency	Share (%)
Exact Match Name and Address	103,640	68.7
Exact Match Previous Name and Address	16,246	10.8
Comprehensive Match	8162	5.4
Fuzzy Match	18,376	12.2
No Match	4520	3.0
Total Announcements	150,944	100.0

Notes: This table presents the different types of matches that were used when merging data on applicants for debt relief in Statstidende from 1984 to 2005 with unique individuals in the Danish Central Person Register, their frequencies, and their shares in the total number of announcements on debt relief in Statstidende. More details about this procedure and further definitions can be found in Section B.

Table A.2: Number of Cases per City Court

Court	Cases	Court	Cases
Aabenraa	326	Naksov	337
Aalborg	1842	Nibe	459
Aarhus	3519	Nyborg	377
Assens	423	Nykøbing Falster	680
Brædstrup	425	Nykøbing Mors	243
Brønderslev	459	Nykøbing Sjælland	303
Ebeltoft	220	Næstved	374
Esbjerg	775	Odense	1994
Faaborg	530	Randers	1166
Fjerritslev	446	Ribe	432
Fredericia	537	Ringkøbing	374
Fredrikshavn	641	Ringsted	366
Frederikssund	642	Roskilde	1316
Grenå	531	Rudkøbing	270
Grindsted	476	Rødding	361
Gråsten	257	Rønne	473
Haderslev	399	Silkeborg	795
Helsinge	282	Skanderborg	684
Helsingør	502	Skive	485
Herning	1193	Skjern	534
Hillerød	613	Slagelse	522
Hjørring	1279	Sorø	329
Hobro	460	Store Heddinge	522
Holbæk	308	Struer	277
Holstebro	305	Svendborg	969
Holsted	512	Sæby	465
Horsens	945	Sø- og Handelsretten	4556
Kalundborg	383	Sønderborg	486
Kjellerup	510	Terndrup	376
Kolding	625	Thisted	503
Korsør	244	Tønder	493
Køge	716	Varde	472
Lemvig	278	Vejle	548
Mariager	422	Viborg	831
Maribo	351	Vordingborg	422
Middelfart	401		

Table A.3: Official Statistics on Number of Applicants for Debt Relief

Year	Applied	Investigated	Granted
1984	2760		
1985	5546		
1986	3797		
1987	4000		
1988	4394		1415
1989	5690		1363
1990	6661		2016
1991	7745		2161
1992	7042		2406
1993	8069		2390
1994	8326		2864
1995	7745		3085
1996	6720		2646
1997	6412		2249
1998	5866		2188
1999	5118		1813
2000	5530		1650
2001	4962		1547
2002	4771	1967	1373
2003	4715	1985	1399
2004	4671	2138	1439
2005	5385	2232	1168
2006	5688	2891	1988
2007	4722	2265	1637
2008	4817	1993	1397
2009	5045	1946	1189
2010	5116	2046	1320
2011	5253	2337	1514
2012	5568	2514	1669
2013	5975	2914	2046
2014	5511	2723	2051
2015	5269	2492	1961
2016	4622	2271	1747
2017	4614	2435	1654
2018	4139	2071	1504
2019	4127	1903	1330
2020	3568	1832	1231

Notes: This table shows official statistics on the annual number of applicants, the number of opened investigations, and the number of granted applications for debt relief in Denmark. The sources of these statistics are presented in Section C.

Table A.4: Outcome Variables

Outcome	Register Variable	Years	Definition
Earned Income (DKK)	ERHVERVSINDK_13	1980-2019	
Employed (y/n)	PSTILL	1980-2012	1-37, 71-77
	JOB_P_SOCIO_KODE	2013-2019	110, 120, 131-136
Unemployed (y/n)	PSTILL	1980-2012	40
	JOB_P_SOCIO_KODE	2013-2019	200
Out of Labor Force (y/n)	PSTILL	1980-2012	41-57, 90-98
	JOB_P_SOCIO_KODE	2013-2019	311-517
Hourly Wage (DKK)	TIMELON	1980-2010	
	JOB_TIME_LOEN_SMAL	2011-2019	
Taxable Wealth (DKK)	QAKTIVF-QPASSIV	1980-1996	
	QAKTIVF_NY05-QPASSIVN	1997-2019	
Taxable Assets (DKK)	QAKTIVF	1980-1996	
	QAKTIVF_NY05	1997-2019	
Taxable Debt (DKK)	QPASSIV	1980-1996	
	QPASSIVN	1997-2019	
Taxable Secured Debt (DKK)	PRIGALD	1984-1994	
	OBLGAELD	1995-2019	
Taxable Unsecured Debt (DKK)	BANKGAELD	1987-1993	
	BANKGAELD	1995-2019	
Owns Real Estate (y/n)	KOEJD	1983-2019	KOEJD $>$ 0
Real Estate (DKK)	KOEJD	1983-2019	
Disability Pension (y/n)	TILBTOT	1984-2019	TILBTOT $>$ 0
Disability Pension (DKK)	TILBTOT	1984-2019	
Social Assistance (y/n)	KONT_GL	1980-1993	KONT_GL $>$ 0
	KONTANTHJ_13	1994-2019	KONTANTHJ_13 $>$ 0
Social Assistance (DKK)	KONT_GL	1980-1993	
	KONTANTHJ_13	1994-2019	

Table A.5: Applicants for Debt Relief versus General Population

	All	Comparators
Mean age	44.2	44.2
	(10.5)	(10.5)
Fraction men	63.3%	63.3%
Fraction married	58.4%	64.5%
Mean persons in household	2.5	2.7
	(1.4)	(1.3)
Mean years of schooling	11.0	11.7
	(2.9)	(3.1)
Mean earned income	165	264
	(172)	(195)
Fraction employed	64.4%	77.4%
Fraction unemployed	12.1%	6.1%
Mean taxable wealth	-389	114
	(635)	(437)
Mean taxable assets	71	569
	(435)	(730)
Mean taxable debt	458	407
	(700)	(593)
Fraction real estate owners	12.0%	50.4%
Observations	46,571	232,855

Notes: This table shows summary statistics for our sample (left column) and five comparators from the general Danish population (right column) matched on sex and birth year, for the year before application for debt relief. Monetary unit is thousands of 2020 DKK. Numbers in parentheses are standard deviations.

Table A.6: Debt and Repayment Statistics

Dividend (%)	
Mean (SD)	10.3 (12.7)
Median (Interquartile range)	6.1 (1.4–14.3)
Observations	3968
Repayment period (yrs)	
Mean (SD)	4.5 (1.7)
Median (Interquartile range)	5.0 (5.0–5.0)
Observations	2181
Monthly repayment	
Mean (SD)	2,500 (5490)
Median (Interquartile range)	1,800 (1000–2,970)
Observations	1145
Unsecured debt (millions)	
Mean (SD)	1.71 (1.88)
Median (Interquartile range)	1.10 (0.68–2.10)
Observations	1389

Notes: This table shows debt and repayment statistics for a random sub-sample of individuals who were granted debt relief between 1984 and 2005 (see Section 3.4). The data were collected from public court announcements in Statstidende. The dividend is the total payment from the debtor to the creditors divided by the total outstanding unsecured debt. According to Danish law, announcements in Statstidende have to contain information about the dividend. Statistics on the length of the repayment period and the monthly repayment are presented for cases where the dividend was positive. Similarly, information about the amount of debt is typically only available when the dividend is positive. Monetary unit is 2020 DKK.

Table A.7: IV First Stage Regression

	Without covariates	With covariates
Instrument	0.53200 (0.03730) **	0.53500 (0.03730) **
Male		-0.02870 (0.00413) **
Age 0-40		-0.06770 (0.02060) **
Age 41-50		-0.05800 (0.02040) **
Age 51-60		-0.05660 (0.01990) **
Age 61-70		0.00043 (0.02100)
Single Household (y/n)		0.02850(0.00640) **
Earned Income (in 10,000s DKK)		-0.001130 (0.00024)**
Employment		0.03200 (0.01120) **
Unemployment		0.00549 (0.01350)
Married (y/n)		-0.01570 (0.00564) **
Immigrant (y/n)		0.00415 (0.01450)
Real Estate Ownership (y/n)		-0.034800** (0.00910) **
Taxable Debt (in 10,000s DKK)		0.00012 (0.00004)**
Taxable Assets (in 10,000s DKK)		-0.00006 (0.00006)
Highschool (y/n)		0.02280 (0.00533) **
University (y/n)		-0.00414 (0.00783)
Education Missing (y/n)		-0.01170 (0.01340)
Social Assistance (y/n)		-0.00091 (0.0093)
Wage Quartile 1		0.00657 (0.00816)
Wage Quartile 2		0.00690 (0.00888)
Wage Quartile 3		-0.01270 (0.00860)
Wage Quartile 4		-0.01960 (0.00873) *
Observations (individuals)	32,931	32,931
R2	0.079	0.087
F-statistic (instrument)	206	205

Notes: This table shows results from the first stage IV regression without (left column) and with (right column) exogenous covariates. Both regressions include court-by-year fixed effects and a constant. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.8: Instrument Balance Test

	Instrument	Granted Debt Relief
Male	-0.00017 (0.00085)	-0.02880 (0.00414) **
Age 0-40	0.00089 (0.00381)	-0.06720 (0.02100) **
Age 41-50	0.00087 (0.00372)	-0.05750 (0.02080) **
Age 51-60	-0.00229 (0.00386)	-0.05780 (0.02030) **
Age 61-70	0.00051 (0.00368)	0.00070 (0.02140)
Single Household (y/n)	-0.00138 (0.00115)	0.02770 (0.00634) **
Earned Income (in 10,000s DKK)	-0.00003 (0.00004)	- 0.00115 (0.00024) **
Employment	-0.00100 (0.00199)	0.03150 (0.01120) **
Unemployment	0.00132 (0.00230)	0.00620 (0.01360)
Married (y/n)	0.00040 (0.00109)	-0.01550 (0.00565) **
Immigrant (y/n)	0.00306 (0.00277)	0.00579 (0.01460)
Real Estate Ownership (y/n)	0.00220 (0.00152)	-0.03360 (0.00913) **
Taxable Debt (in 10,000s DKK)	0.000005 (0.000007)	0.00012 (0.00004)**
Taxable Assets (in 10,000s DKK)	-0.000027 (0.000001) *	-0.00008 (0.0007)
Highschool (y/n)	0.00028 (0.00090)	0.02290 (0.00534) **
University (y/n)	0.00345 (0.00272)	- 0.00230 (0.00826)
Education Missing (y/n)	0.00186 (0.00261)	-0.01080 (0.01340)
Social Assistance (y/n)	-0.00318 (0.00161) *	-0.00261 (0.00931)
Wage Quartile 1	0.00132 (0.00136)	0.00728 (0.00820)
Wage Quartile 2	0.00215 (0.00141)	0.00805 (0.00890)
Wage Quartile 3	0.00094 (0.00143)	- 0.01220 (0.00862)
Wage Quartile 4	0.00223 (0.00158)	- 0.01840 (0.00873) *
Observations (individuals)	32,931	32,931
Joint F-statistic (p-value)	1.192 (0.252)	12.94 (<0.001)

Notes: This table shows results from regressing the instrumental variable (left) and a dummy for applicant being granted debt relief (right) on applicant characteristics, court-by-year fixed effects, and a constant. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.9: Dividend and Trustee Instrument

DIVIDEND NOT WINSORIZED		
	Without Covariates	With Covariates
Instrument	0.0028	0.0041
	(0.047)	(0.042)
Observations (individuals)	2,591	2,574
R2	0.000	0.106

DIVIDEND WINSORIZED		
	Without Covariates	With Covariates
Instrument	0.00059	0.0018
	(0.046)	(0.042)
Observations (individuals)	2,591	2,574
R2	0.000	0.112

Notes: This table shows results from linear regressions of the dividend among applicants who were granted debt relief on the residualized trustee instrument (i.e. the normalized admission rate of trustees in equation (4) conditional on court-by-year fixed effects). Data on the dividend is from the repayment sample. The regressions with covariates include all exogenous covariates, W_{it}, from the second stage of the IV 2SLS regression model. The two bottom regressions have a winsorized dividend as dependent variable (winsorized at the 1st and 99th percentile). The dividend and the residualized trustee instrument are both measured on a scale from 0 to 100 (in percentage points). Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.10: IV First Stage Regression in Subsamples

	Men	Women
Instrument	0.502 **	0.591 **
	(0.037)	(0.055)
Observations (individuals)	20,411	12,520
R2	0.096	0.142
	Young	**Old**
Instrument	0.535 **	0.532 **
	(0.050)	(0.051)
Observations (individuals)	17,581	15,350
R2	0.120	0.142
	Low education	**High education**
Instrument	0.571 **	0.513 **
	(0.056)	(0.044)
Observations (individuals)	13,784	19,147
R2	0.139	0.115
	Low income	**High income**
Instrument	0.510 **	0.566 **
	(0.041)	(0.051)
Observations (individuals)	16,440	16,491
R2	0.117	0.132

Notes: This table shows results from the first stage IV regression in subsamples. All regressions include exogenous covariates, court-by-year fixed effects, and a constant. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.11: IV First Stage Regression in Subsamples with Reverse-sample Instrument

	Men	Women
Instrument	0.435 **	0.591 **
	(0.042)	(0.053)
Observations (individuals)	18,912	13,250
R2	0.106	0.148

	Young	Old
Instrument	0.320 **	0.320 **
	(0.050)	(0.052)
Observations (individuals)	17,205	15,778
R2	0.124	0.143

	Low education	High education
Instrument	0.450 **	0.366 **
	(0.054)	(0.043)
Observations (individuals)	14,289	18,812
R2	0.142	0.118

	Low income	High income
Instrument	0.418 **	0.424 **
	(0.038)	(0.054)
Observations (individuals)	16,384	16,798
R2	0.121	0.136

Notes: This table shows results from the first stage IV regression in subsamples, using an instrument constructed from the reverse subsample (instrument for cases with male applicants was constructed from cases with female applicants etc.). All regressions include exogenous covariates, court-by-year fixed effects, and a constant. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.12: Event-Study Estimates for the IV Sample

	(1) Full Sample	(2) IV sample
Earned Income (DKK)	20,183**	19,515**
	(1,576)	(1,848)
Employed (y/n)	0.0230**	0.0228**
	(0.0038)	(0.0045)
Unemployed (y/n)	-0.0123**	-0.0121**
	(0.0023)	(0.0025)
Out of Labor Force (y/n)	-0.0110**	-0.0112**
	(0.0036)	(0.0043)
Hourly Wage (DKK)	4.264**	5,439**
	(0.915)	(1.061)
Taxable Wealth (DKK)	255,898**	252,887
	(5.610)	(6.457)
Taxable Assets (DKK)	155,504**	149,184
	(5.315)	(6.104)
Taxable Debt (DKK)	-110,386**	115,531**
	(6.870)	(7.652)
Taxable Secured Debt (DKK)	93,587**	91,357**
	(3,512)	(4.117)
Taxable Unsecured Debt (DKK)	-191,849**	-191,125**
	(4,323)	(4.895)
Owns Real Estate(y/n))	0.156**	0.148*
	(0.004)	(0.005)
Taxable Real Estate (DKK)	124,318**	120,227**
	(4,099)	(4,845)
Observations (individuals)	46,390	32,794

Notes: This table shows the estimated impact of debt relief based on our event-study regression for the full sample (Column (1)) and our IV sample (Column (2)). Monetary unit is 2020 DKK. In Column (1) the number of observations refers to the number of individuals with non-missing outcome data (the maximum across outcomes). Column (2) further requires a valid instrument. Numbers in parentheses are standard errors clustered at the level of the individual (Column (1)) or clustered at the level of the trustee identifier (Column (2)). ** p<0.01, * p<0.05.

Table A.13: Mean Outcomes During Follow-Up

	Denied	Denied Compliers
Earned Income (DKK)	180,300	170,400
Employed (y/n)	0.565	0.558
Unemployed (y/n)	0.052	0.048
Out of Labor Force (y/n)	0.383	0.393
Hourly Wage (DKK)	225	214
Taxable Wealth (DKK)	-299,900	-313,000
Taxable Assets (DKK)	156,800	145,100
Taxable Debt (DKK)	471,200	466,500
Taxable Secured Debt (DKK)	102,700	88,900
Taxable Unsecured Debt (DKK)	344,000	350,400
Owns Real Estate (y/n)	0.135	0.134
Real Estate (DKK)	124,100	116,700

Notes: This table shows the means for our outcome variables across individuals and across the sixteen-year follow-up period. Means for denied compliers are computed using the method of Dahl, Kostøl and Mogstad (2014). Monetary unit is 2020 DKK.

Table A.14: Impact of Debt Relief on Welfare Dependency

	IV
Receives Disability Pension (y/n)	-0.054
	(0.039)
Disability Pension (DKK)	-1,050
	(1,180)
Receives Social Assistance (y/n)	-0.029
	(0.018)
Social Assistance (DKK)	-2,450
	(1,670)
Observations (individuals)	32,794

Notes: This table shows the estimated impact of debt relief on welfare dependency using instrumental variable regression. Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered at the level of the trustee identifier. ** p<0.01, * p<0.05.

Table A.15: Impact of Debt Relief by Follow-up Period

	Years 1-5	Years 6-10	Years 11-16
Earned Income (DKK)	48,600**	51,500**	47,800*
	(16,800)	(19,200)	(21,500)
Employed (y/n)	0.093	0.140**	0.135*
	(0.049)	(0.050)	(0.054)
Unemployed (y/n)	0.019	-0.018	0.014
	(0.022)	(0.017)	(0.015)
Out of Labor Force (y/n)	-0.113*	-0.122*	-0.150**
	(0.047)	(0.048)	(0.056)
Hourly Wage (DKK)	5.36	11.6	23.2
	(8.89)	(11.3)	(12.8)
Taxable Wealth (DKK)	335,800**	253,700**	261,000**
	(44,500)	(59,800)	(66,500)
Taxable Assets (DKK)	125,700**	337,600**	469,300**
	(37,800)	(67,600)	(91,200)
Taxable Debt (DKK)	-225,200**	59,500	184,100
	(53,300)	(80,400)	(99,400)
Taxable Secured Debt (DKK)	77,300**	244,100**	290,600**
	(29,500)	(46,600)	(62,300)
Taxable Unsecured Debt (DKK)	-262,100**	-189,500**	-128,600*
	(41,800)	(52,800)	(55,300)
Owns Real Estate (y/n)	0.141**	0.279**	0.338**
	(0.041)	(0.056)	(0.063)
Real Estate (DKK)	105,300**	284,900**	395,300**
	(33,100)	(58,700)	(81,100)
Observations (individuals)	32,794	31,289	29,481

Notes: This table shows the estimated impact of debt relief using instrumental variable regression. The follow-up period is divided into three subperiods (1-5 years, 6-10 years, 11-16 years). Monetary unit is 2020 DKK. The number of observations refers to the number of individuals with a valid instrument and outcome data (the maximum across outcomes). The number of observations is, for example, lower for wages with missing observations for the non-employed. Numbers in parentheses are standard errors clustered at the level of the trustee identifier. ** p<0.01, * p<0.05.

Table A.16: Instrumental Variable Estimates by Required Cases per Trustee

	20 Cases	50 Cases	100 Cases
Earned Income (DKK)	46,800**	45,500**	51,500**
	(15,200)	(15,900)	(16,800)
Employed (y/n)	0.117**	0.108*	0.163**
	(0.039)	(0.045)	(0.051)
Unemployed (y/n)	0.0050	0.0005	0.015
	(0.012)	(0.013)	(0.014)
Out of Labor Force (y/n)	-0.122**	-0.111*	-0.182**
	(0.040)	(0.046)	(0.051)
Hourly Wage (DKK)	11.6	11.0	5.52
	(8.25)	(9.59)	(9.60)
Taxable Wealth (DKK)	282,500**	347,600**	330,500**
	(46,400)	(49,700)	(61,400)
Taxable Assets (DKK)	309,300**	240,100**	287,300**
	(54,400)	(58,100)	(61,800)
Taxable Debt (DKK)	7,870	-136,700*	-77,900
	(66,000)	(66,800)	(81,900)
Taxable Secured Debt (DKK)	201,400**	163,900**	196,900**
	(38,000)	(41,400)	(39,500)
Taxable Unsecured Debt (DKK)	-188,100**	-267,600**	-302,400**
	(42,600)	(37,000)	(35,200)
Owns Real Estate (y/n)	0.248**	0.207**	0.238**
	(0.044)	(0.041)	(0.044)
Real Estate (DKK)	260,800**	218,300**	252,700**
	(47,700)	(48,800)	(49,300)
Observations (individuals)	32,794	23,113	11,065

Notes: This table shows our IV estimates of the impact of debt relief across different specifications where we vary the minimum required number of cases per trustee. Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.17: Joint Test of Exclusion and Monotonicity Assumption

	Copenhagen	Aarhus	Aalborg	Odense	Roskilde	Hjorring	Randers	Herning	Horsens
Earnings t+1									
Test Stat	23.760	32.355	29.422	19.294	12.952	15.748	16.162	13.862	16.459
Pvalue	*0.126*	*0.499*	*0.001*	*0.037*	*0.012*	*0.151*	*0.240*	*0.008*	*0.087*
Earnings t+8									
Test Stat	20.046	35.123	26.227	14.588	9.660	10.914	16.173	8.268	15.281
Pvalue	*0.272*	*0.368*	*0.003*	*0.148*	*0.047*	*0.451*	*0.240*	*0.082*	*0.122*
Earnings t+16									
Test Stat	23.203	41.390	19.853	17.383	18.584	14.923	8.759	11.077	14.017
Pvalue	*0.143*	*0.150*	*0.031*	*0.066*	*0.001*	*0.186*	*0.791*	*0.026*	*0.172*
Employment t+1									
Test Stat	42.739	44.060	8.103	18.892	8.605	12.965	9.538	4.638	20.895
Pvalue	*0.001*	*0.094*	*0.619*	*0.042*	*0.072*	*0.296*	*0.731*	*0.327*	*0.022*
Employment t+8									
Test Stat	15.898	37.582	17.182	12.647	14.880	8.091	8.283	8.102	8.659
Pvalue	*0.531*	*0.267*	*0.070*	*0.244*	*0.005*	*0.705*	*0.825*	*0.088*	*0.565*
Employment t+16									
Test Stat	19.926	52.539	14.920	19.611	6.992	9.537	16.665	7.006	13.382
Pvalue	*0.278*	*0.017*	*0.135*	*0.033*	*0.136*	*0.572*	*0.215*	*0.136*	*0.203*
Wealth t+4									
Test Stat	27.264	37.081	12.001	12.973	5.939	18.302	18.859	9.806	8.011
Pvalue	*0.054*	*0.286*	*0.285*	*0.225*	*0.204*	*0.075*	*0.128*	*0.044*	*0.628*
Wealth t+8									
Test Stat	24.062	46.855	16.970	16.665	8.599	21.491	8.544	11.490	17.376
Pvalue	*0.118*	*0.056*	*0.075*	*0.082*	*0.072*	*0.029*	*0.806*	*0.022*	*0.066*
Wealth t+16									
Test Stat	17.677	47.107	20.920	9.603	11.975	22.171	17.261	10.079	19.919
Pvalue	*0.409*	*0.053*	*0.022*	*0.476*	*0.018*	*0.023*	*0.188*	*0.039*	*0.030*
Assets t+4									
Test Stat	25.455	31.356	12.524	12.422	6.639	22.088	15.704	11.044	11.930
Pvalue	*0.085*	*0.549*	*0.252*	*0.258*	*0.156*	*0.024*	*0.265*	*0.026*	*0.290*
Assets t+8									
Test Stat	32.101	30.464	12.840	24.268	8.791	9.941	19.646	10.646	11.912
Pvalue	*0.015*	*0.594*	*0.233*	*0.007*	*0.067*	*0.536*	*0.104*	*0.031*	*0.291*
Assets t+16									
Test Stat	34.544	36.997	12.836	17.374	18.111	19.358	16.365	11.307	18.566
Pvalue	*0.007*	*0.290*	*0.233*	*0.066*	*0.001*	*0.055*	*0.230*	*0.023*	*0.046*
Degrees of freedom	17	33	10	10	4	11	13	4	10
Observations	4053	2670	1476	1365	1241	993	853	824	669

Notes: This table shows the results from the test by Frandsen, Lefgren and Leslie (2023*a*). The test is implemented separately for each of the 9 largest courts in our sample (following the arguments in Sigstad (2023)) with the same set of covariates as in our baseline model and using the Stata package testjfe. We use the default number of knots (3) in the test and we report test statistics and p-values based on the fit component of the test, see Frandsen, Lefgren and Leslie (2023*b*).

Table A.18: UJIVE as Instrumental Variable

	20 Cases	50 Cases
Earned Income (DKK)	26,930*	36,617*
	(14,276)	(15,228)
Employed (y/n)	0.0898**	0.0985*
	(0.0365)	(0.042)
Unemployed (y/n)	0.0155	0.0085
	(0.0110)	(0.0117)
Out of Labor Force (y/n)	-0.106**	-0.109**
	(0.0362)	(0.0417)
Hourly Wage (DKK)	5.968	8.182
	(7.472)	(8.178)
Taxable Wealth (DKK)	275,241**	319,281**
	(44,884)	(48,513)
Taxable Assets (DKK)	248,979**	212,833**
	(52,739)	(54,294)
Taxable Debt (DKK)	-50,073	-131,993*
	(60,876)	(60,045)
Taxable Secured Debt (DKK)	151,464**	133,597**
	(37,339)	(39,450)
Taxable Unsecured Debt (DKK)	-194,273**	-251,019**
	(39,739)	(42,008)
Owns Real Estate (y/n)	0.210**	0.188**
	(0.042)	(0.040)
Real Estate (DKK)	206,214**	189,939**
	(46,373)	(46,067)
Observations (individuals)	32,794	23,113

Notes: This table shows the estimated impact of debt relief using the UJIVE estimator (Kolesár (2013)), by required number of cases per trustee. Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.19: Alternative Specifications of Instrumental Variable

	Vary by year	Leave out year	Split sample
Earned Income (DKK)	56,300*	48,800*	76,700**
	(26,500)	(19,000)	(25,300)
Employed (y/n)	0.145*	0.121*	0.201**
	(0.064)	(0.049)	(0.065)
Unemployed (y/n)	-0.022	0.0097	0.0030
	(0.019)	(0.015)	(0.020)
Out of Labor Force (y/n)	-0.124	-0.131**	-0.205**
	(0.065)	(0.049)	(0.064)
Hourly Wage (DKK)	30.5*	9.52	12.8
	(14.8)	(9.95)	(14.3)
Taxable Wealth (DKK)	71,900	316,200**	299,300**
	(79,800)	(55,400)	(69,500)
Taxable Assets (DKK)	246,600**	344,800**	369,100**
	(85,400)	(68,900)	(87,900)
Taxable Debt (DKK)	157,600	8,720	75,400
	(104,200)	(82,100)	(90,400)
Taxable Secured Debt (DKK)	191,100**	212,400**	220,800**
	(58,400)	(47,600)	(57,600)
Taxable Unsecured Debt (DKK)	-136,700**	-170,000**	-161,300**
	(20,400)	(36,700)	(22,400)
Owns Real Estate (y/n)	0.217**	0.268**	0.298**
	(0.072)	(0.055)	(0.072)
Real Estate (DKK)	221,900**	287,600**	307,200**
	(75,200)	(60,500)	(78,200)
Observations (individuals)	31,570	32,793	16,343

Notes: This table shows the estimated impact of debt relief using the admission rate of the assigned trustee as an instrumental variable with alternative specifications. The first column uses an instrument that is calculated by calendar year, the second column leaves out court cases in the same calendar year, and the third column randomly splits the sample in two halves and uses the instrument calculated in one half to estimate the model in the other half. Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.20: Attrition

At 6 years	
IV coefficient	0.019
Standard error	(0.026)
Mean attrition	0.050
At 11 years	
IV coefficient	0.0065
Standard error	(0.036)
Mean attrition	0.105
At 16 years	
IV coefficient	-0.042
Standard error	(0.045)
Mean attrition	0.169
All years 1-16	
IV coefficient	-0.0014
Standard error	(0.024)
Mean attrition	0.080
Observations (individuals)	32,931

Notes: This table shows the rate of attrition in our sample at 6, 11, and 16 years of follow-up time, and the mean across all years 1 to 16. Coefficients and standard errors are presented for 4 separate regressions with the dependent variable being a dummy for attrition and the independent variable being whether or not the applicant was granted debt relief. Numbers in parentheses are standard errors clustered by trustee identifier. ** $p<0.01$, * $p<0.05$.

Table A.21: Balanced Panel Results

	With Attrition	Balanced Panel
Earned Income (DKK)	46,800**	42,200*
	(15,200)	(16,500)
Employed (y/n)	0.117**	0.106*
	(0.039)	(0.042)
Unemployed (y/n)	0.0050	0.0029
	(0.012)	(0.013)
Out of Labor Force (y/n)	-0.122**	-0.109**
	(0.040)	(0.042)
Hourly Wage (DKK)	11.6	8.56
	(8.25)	(8.84)
Taxable Wealth (DKK)	282,500**	294,200**
	(46,400)	(50,400)
Taxable Assets (DKK)	309,300**	328,200**
	(54,400)	(60,600)
Taxable Debt (DKK)	7,870	10,600
	(66,000)	(71,600)
Taxable Secured Debt (DKK)	201,400**	212,700**
	(38,000)	(42,900)
Taxable Unsecured Debt (DKK)	-188,100**	-196,400**
	(42,600)	(46,700)
Owns Real Estate (y/n)	0.248**	0.272**
	(0.044)	(0.048)
Real Estate (DKK)	260,800**	275,700**
	(47,700)	(53,100)
Observations (individuals)	32,794	27,353

Notes: This table shows the estimated impact of debt relief using the admission rate of the assigned trustee as an instrumental variable, in full panel with attrition (left) and in balanced panel with no attrition (right). Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.22: Alternative Levels of Clustering

	Court	Individual	Court-by-year	Trustee-by-year
Earned Income (DKK)	46,800**	46,800**	46,800**	46,800**
	(17,400)	(16,300)	(18,500)	(17,900)
Employed (y/n)	0.117**	0.117**	0.117**	0.117**
	(0.043)	(0.039)	(0.041)	(0.040)
Unemployed (y/n)	0.0050	0.0050	0.0050	0.0050
	(0.0092)	(0.012)	(0.0096)	(0.013)
Out of Labor Force (y/n)	-0.122**	-0.122**	-0.122**	-0.122**
	(0.041)	(0.039)	(0.041)	(0.045)
Hourly Wage (DKK)	11.6	11.6	11.6	11.6
	(11.5)	(8.82)	(10.6)	(7.78)
Taxable Wealth (DKK)	282,500**	282,500**	282,500**	282,500**
	(38,700)	(45,200)	(42,600)	(55,200)
Taxable Assets (DKK)	309,300**	309,300**	309,300**	309,300**
	(74,800)	(56,300)	(58,400)	(39,400)
Taxable Debt (DKK)	7,870	7,870	7,870	7,870
	(83,100)	(60,900)	(79,600)	(70,000)
Taxable Secured Debt (DKK)	201,400**	201,400**	201,400**	201,400**
	(50,900)	(38,500)	(38,100)	(27,100)
Taxable Unsecured Debt (DKK)	-188,100**	-188,100**	-188,100**	-188,100**
	(36,200)	(36,200)	(45,800)	(54,900)
Owns Real Estate (y/n)	0.248**	0.248**	0.248**	0.248**
	(0.056)	(0.044)	(0.047)	(0.037)
Real Estate (DKK)	260,800**	260,800**	260,800**	260,800**
	(64,800)	(49,700)	(48,800)	(34,000)
Observations (individuals)	32,794	32,794	32,794	32,794

Notes: This table shows the estimated impact of debt relief using the admission rate of the assigned trustee as an instrumental variable. Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by court (1st column), debtor (2nd column), court-by-year of application (3rd column), and trustee identifier-by-year of application (4th column). ** p<0.01, * p<0.05.

Table A.23: Subgroup Analysis by Sex, Age, Marital Status, and Education

	Men	Women	Young	Old	Married	Single	Low Education	High Education
Earned Income (DKK)	35,000	46,600*	58,000**	31,600	52,411**	58,070	32,400	59,200*
	(21,700)	(18,400)	(22,300)	(19,600)	(18,843)	(31,812)	(21,200)	(27,900)
	[193,018]	[158,708]	[231,122]	[112,887]	[189,600]	[163,235]	[146,025]	[203,567]
Employed (y/n)	0.102*	0.118*	0.103*	0.114	0.132**	0.155	0.126*	0.095
	(0.050)	(0.053)	(0.050)	(0.060)	(0.045)	(0.0888)	(0.060)	(0.060)
	[0.579]	[0.540]	[0.697]	[0.386]	[0.589]	[0.516]	[0.494]	[0.622]
Wealth (DKK)	358,600**	151,800**	197,700**	401,400**	294,184**	246,933**	190,300**	371,200**
	(67,800)	(47,800)	(59,600)	(70,100)	(53,897)	(88,999)	(56,100)	(71,400)
	[-354,185]	[-207,827]	[-304,008]	[294,487]	[-314,548]	[-267,038]	[-262,163]	[323,167]
Assets (DKK)	303,700**	310,600**	388,400**	248,200**	339,018**	201,079	194,400**	437,700**
	(71,500)	(75,300)	(81,300)	(76,500)	(67,468)	(105,489)	(72,000)	(94,300)
	[145,461]	[175,939]	[185,554]	[118,576]	[166,156]	[138,340]	[120,160]	[179,555]
Secured Debt (DKK)	199,600**	212,900**	275,800**	129,400*	220,285**	152,344**	124,700**	324,100**
	(48,500)	(51,500)	(57,400)	(50,200)	(48,583)	(74,076)	(47,500)	(68,500)
	[95,772]	[114,391]	[131,461]	[64,519]	[112,048]	[82,932]	[74,987]	[119,906]
Unsecured Debt (DKK)	-265,600**	-86,100*	-117,100**	-279,500**	-194,696**	-218,537**	-114,600**	-249,600**
	(50,700)	(33,500)	(38,100)	(52,200)	(45,872)	(76,002)	(40,500)	(50,900)
	[391,805]	[263,433]	[345,872]	[341,588]	[357,806]	[314,147]	[299,719]	[371,009]
Observations (individuals)	20,308	12,486	17,545	15,249	21,195	11,176	13,725	14,502

Notes: This table shows IV estimates across different subgroups. The subgroups are defined by sex, age (below or above 45 years of age), marital status, and education. Married is defined as being registered as married in any of the four years prior to applying for debt relief. The low education group is applicants who have lower secondary education or less. The high education group is applicants who have upper secondary education or more. Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by trustee identifier. Numbers in hard brackets are non-granted means in the follow-up period. ** p<0.01, * p<0.05.

Table A.24: Subgroup Analysis by Entry Conditions (Economic Outcomes Prior to Application)

	Recession	Non-Recession	Low Income	High Income	Low Debt	High Debt	Low Wealth	High Wealth
Earned Income (DKK)	41,604	55,224**	39,300	54,700*	50,225*	58,346*	48,632*	58,460**
	(26,697)	(20,254)	(20,400)	(21,900)	(22,392)	(24,349)	(22,557)	(20,949)
	[194348]	[172,290]	[101,014]	[246,043]	[149,115]	[212,241]	[209,535]	[153,003]
Employed (y/n)	0.166*	0.0986	0.161*	0.076	0.129*	0.131*	0.0535	0.210**
	(0.0674)	(0.0518)	(0.062)	(0.045)	(0.0585)	(0.0553)	(0.0550)	(0.0576)
	[0.603]	[0.542]	[0.395]	[0.704]	[0.503]	[0.628]	[0.629]	[0.505]
Wealth (DKK)	259,942**	295,437**	310,000**	257,900**	120,927*	407,084**	331,374**	166,455**
	(77,802)	(57,198)	(62,800)	(61,200)	(53,561)	(77,874)	(77,177)	(46,419)
	[-273,034]	[-315,229]	[262,746]	[-330,811]	[-200,670]	[-401,745]	[-439,400]	[-169,902]
Assets (DKK)	284,677**	330,291**	263,000**	367,100**	205,754**	409,841**	282,009**	304,636**
	(90,579)	(70,288)	(75,600)	(75,500)	(71,097)	(91,431)	(84,377)	(75,741)
	[183,854]	[141,328]	[115,344]	[191,158]	[109,577]	[205,185]	[161,040]	[152,811]
Secured Debt (DKK)	159,127*	235,167**	139,100**	271,300**	158,509**	254,205**	189,564**	226,106**
	(64,149)	(48,286)	(47,200)	(55,800)	(48,759)	(62,308)	(56,836)	(53,200)
	[118,956]	[93,419]	[74,672]	[125,900]	[67,073]	[139,204]	[114,622]	[91,556]
Unsecured Debt (DKK)	-134,631*	-224,337**	-212,900**	-170,400**	-89,277*	-270,599**	-264,797**	-98,434**
	(62,873)	(52,537)	(46,900)	(40,800)	(42,909)	(70,020)	(66,053)	(37,528)
	[333,321]	[349,928]	[296,422]	[383,444]	[236,290]	[454,181]	[472,288]	[224,148]
Owns Real Estate (y/n)	0.167**	0.294**	0.232**	0.279**	0.219**	0.258**	0.161*	0.353**
	(0.0674)	(0.0587)	(0.061)	(0.062)	(0.0592)	(0.0685)	(0.0640)	(0.0645)
	[0.157]	[0.122]	[0.107]	[0.157]	[0.099]	[0.171]	[0.135]	[0.]
Observations (individuals)	13,217	19,577	16,340	16,454	16,361	16,433	16,437	16,357

Notes: This table shows IV estimates across different subgroups. The subgroups are defined by income, debt or wealth prior to application or whether the individual is applying in a recession. Recession years are defined as 1984-1985 and 1991-1995 (following the definition from Andersen and Rasmussen (2011)). Income groups are defined as being above/below median income (averaged over four years prior to applying) among individuals applying in the same year. Debt and wealth groups are defined in a similar way. Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by trustee identifier. Numbers in hard brackets are non-granted means in the follow-up period. ** $p<0.01$, * $p<0.05$.

Table A.25: Instrumental Variable Estimates for Years 17 to 25

Earned Income (DKK)	40,300
	(39,800)
Employed (y/n)	0.160
	(0.089)
Unemployed (y/n)	0.023
	(0.014)
Out of Labor Force (y/n)	-0.169
	(0.088)
Hourly Wage (DKK)	56.6*
	(25.7)
Taxable Wealth (DKK)	390,200**
	(144,000)
Taxable Assets (DKK)	811,300**
	(195,300)
Taxable Debt (DKK)	396,600*
	(177,200)
Taxable Secured Debt (DKK)	383,700**
	(127,400)
Taxable Unsecured Debt (DKK)	-44,300
	(83,700)
Owns Real Estate (y/n)	0.466**
	(0.123)
Real Estate (DKK)	688,100**
	(173,500)
Observations (individuals)	13,927

Notes: This table shows the estimated impact of debt relief during follow-up years 17 to 25 using the admission rate of the assigned trustee as an instrumental variable. Sample consists of applicants for debt relief from 1984 up until 1994. Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

Table A.26: Characteristics of Compliers

	All	Granted	Compliers
Men	0.620	0.614	0.585
Age 45 or above	0.534	0.532	0.539
Employed	0.636	0.631	0.669
Unemployed	0.115	0.119	0.090
Married	0.568	0.564	0.655
Owns real estate	0.259	0.264	0.168
Low education	0.417	0.415	0.433
Low earned income	0.500	0.492	0.524

Notes: This table shows the share of compliers with various observable characteristics (right column) together with the corresponding shares in our full sample (left column) and the subsample of applicants who were granted debt relief (middle column). Compliers are defined as those applicants who would be granted debt relief if assigned to the least strict trustee, but not granted debt relief if assigned to the strictest trustee. We estimate the share of compliers and the distribution of characteristics among compliers using the predicted fraction receiving debt relief from the first stage regression, treating the top and bottom one percentiles of the predicted admission rate as the least strict and strictest trustees (see Section F for more details).

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