

The Long-Run Effects of Individual Debt Relief*

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Abstract

Individuals with extensive debt may be granted debt relief in court. We provide the first long-run evaluation of a debt relief program with data from court records linked to nationwide Danish registers. Using 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 at least 25 years after a court ruling. The net transition of workers into employment accounts for two thirds of the increase in earned income.

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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 the first long-run evaluation of a debt relief program. 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 25 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 outcomes that have not been studied previously. We are the first to 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 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.

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 more likely to own a house or an apartment, accumulate more assets, own more real estate, 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 25 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.

Our paper is the first study of a debt relief program outside of the US. 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](#)). In Denmark and other Continental European countries, 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 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 \(1999\)](#) and [Ramsay \(2017\)](#)).

The best available evidence on the impact of debt relief in the US 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 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 \(2020\)](#) 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. 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 ([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) ([Bang-Pedersen, 2018](#)). Eventually, Finland and Norway (1993), Austria and Sweden (1994), and other countries in Europe would adopt similar laws ([Alexopoulos and Domowitz \(1998\)](#)).

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 would never repay all their debt and could get a share of outstanding debt back through partial repayment, and the government would not have to pay benefits to debtors who received public income support because of high interest payments.

The commission described the everyday life of overburdened debtors (pages 73-74 of the commission report). 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 moral hazard.

2.2 Law on debt relief

The 1984 regulations on debt relief in Denmark¹ 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 at length how the requirements should be interpreted.²

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. 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

¹Lov 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).

²The 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 practise 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.

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 Danish Bankruptcy Law, an investigation should only be 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 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 treated equally). 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 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 part of their 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 City 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 ("Fogedretten" in Danish).

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.

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.³

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

3.1 Data collection *Statstidende*

All issues of the newspaper *Statstidende* are stored on microfilm in the Danish Royal Library (Det Kongelige Bibliotek). We extracted information from announcements about debt relief (investigations, hearings, granted applications, and revoked decisions) using optical character recognition, from the start of the debt relief program July 1st 1984 until October 14th 2005. At that time, the debt relief program was reformed. We also went through all announcements manually to correct mistakes and to ensure that the information in *Statstidende* was recorded accurately. 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 official names and addresses of all Danish residents together with their unique individual personal identification number. In a

³Danish law permits public agencies to remit 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).

first round, we matched applicants for debt relief with listings in the CPR Register using exact full name, municipality, street name, street number, floor, and door number. In subsequent matchings we relaxed these conditions and accepted fuzzy matches with some minor spelling errors or inaccuracies in the available information (more details about the matching procedure can be found in the Supplementary Appendix). Overall, we were able to identify a unique person in the Danish Central Person Register for 97.0% of the announcements in Statstidende (see Table A1 in the Supplementary Appendix), corresponding to an initial sample of 49,306 individuals in the Danish population.⁴

We divided all individuals in our sample into those who were granted and those who were denied debt relief. According to Danish law, the City Court is obliged to publish a public announcement in Statstidende when an individual is granted debt relief. We classify an individual as having been granted debt relief if we have an announcement of this type in our database, and classify an individual as having been denied debt relief if there is no such announcement.

We refer to the year when the City Court publishes a first announcement about an applicant as the year of application. Figure A1 in the Supplementary Appendix plots the fraction of applicants who were granted debt relief as a function of time. The fraction was fairly constant from 1984 until 2003 and then fell 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 A2 in the Supplementary Appendix). 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).

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 A3 in the Sup-

⁴There are typically several announcements for each applicant (investigations, hearings, granted applications, and withdrawn applications).

plementary Appendix). For the period 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 these variables and their definitions is available in Table A4 in the Supplementary Appendix.

We create a measure of earned income which is the sum of earnings for employed individuals and business income for individuals who are self-employed on an annual basis.⁵ We also classify all 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. To capture dependence on welfare payments, we create a dummy variable for an individual receiving disability insurance, a dummy variable for an individual receiving social assistance benefits, and two variables describing the amount of disability insurance and social benefits respectively in monetary units.

The Danish Income Tax Register contains information on the taxable wealth holdings of all Danish residents, which originates from the period when Denmark had a wealth tax ([Jakobsen et al. \(2020\)](#)). 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 wealth tax was

⁵Monetary 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 be interpreted carefully.

abolished in 1997 but the registers still contain updated information on assets and debt.

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) and Kreiner, Leth-Petersen and Willerslev-Olsen (2020)).⁶ The measure of taxable debt 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 unsecured taxable debt in banks and other financial institutions (see Table A4 in the Supplementary 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 A4 in the Supplementary 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

⁶Since the information is collected for tax purposes we do not have separate information on the type of loans, maturity, interest rates, etc.

year of application, and a slight 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.⁷ 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 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 A5 in the Supplementary 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.

⁷The 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).

3.4 Repayment statistics

In order to describe the repayment terms for applicants who receive debt relief, we hand-collected additional information from a subsample of 200 randomly selected pages in *Statstidende* published between 1984 and 2005 (the repayment sample). These are repayment terms that we chose not to extract from all 150,944 announcements in *Statstidende* in our original data collection. 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 debtors. 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. This calculation can only be performed for a selected sample of debtors.

Table A6 in the Supplementary Appendix presents summary statistics for the repayment sample. Across 827 announcements, the mean dividend was 10.2% and three quarters of debtors paid a dividend of 13.6% 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 373 announcements with information about the length of the repayment period with a mean of 4.6 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 246 announcements (when the dividend was positive), with a mean monthly repayment of 2190 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 262 cases with a mean debt of 1.55 million DKK. This estimate from the repayment sample is larger than the estimate from the Tax Register, perhaps because the City Court considers a wider range of debt than what is recorded in the Tax Register (for example unpaid taxes).

4 Results

We present a series of results on the impact of receiving debt relief in Denmark. We first discuss descriptive 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 A2 in the Supplementary Appendix show mean outcomes for applicants for debt relief, from 4 years before to 16 years after the year of application. Applicants who were granted debt relief had lower mean earned income prior to application compared with denied applicants. Afterwards, these positions reversed and applicants who were granted debt relief had higher mean earned income up to 16 years after application. The employment rate was similar or slightly lower for granted applicants prior to application but also reversed gradually, so that granted applicants had a higher employment rate towards the end of the follow-up period.

Mean taxable assets, the fraction of debtors who owned real estate, and mean taxable real estate fell prior to application. Mean taxable debt also fell, due to a reduction in secured debt. Unsecured debt, on the other hand, continued to rise up until the year of application. These time patterns are consistent with a process of deleveraging where applicants were selling of financial assets and real estate to reduce their debt, and perhaps clarify what fraction of the debt they could repay ([Kilborn \(2009\)](#)).

After the year of application, there was a divergence in balance sheets depending on court

decision. Applicants who were granted debt relief accumulated taxable assets at a higher rate and became more likely to own real estate compared with applicants who were denied debt relief. Applicants who were granted debt relief also experienced a large, immediate, and persistent reduction in unsecured debt, but accumulated more secured debt relative to denied applicants, consistent with the steady increase in real estate ownership. The net effect of these changes was that granted applicants accumulated more taxable wealth than denied applicants during the whole follow-up period.

4.2 Event-study regressions

We further describe changes in outcome variables over time by estimating event-study regressions of the form

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

where Y_{it} is the outcome of interest for applicant i in year t , 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,⁸ and X_{it} are exogenous covariates (calendar year fixed effects, age, and age squared). 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 A3 and A4 in the Supplementary Appendix display event-study graphs for all outcomes (Figure 2 displays event-study graphs for a sub-sample permitting a 25-year study horizon, we return to these results in Section 4.7.2). These graphs show that receiving debt relief is associated with significant and persistent effects throughout our sixteen-year follow-up period, in the form of an increase in earned income, an increase in taxable assets, an increase in the

⁸Since our event-study design is centered around year of application (which we observe for both granted and denied applicants) we do not make "forbidden comparisons" in the terminology of [Borusyak, Jaravel and Spiess \(2022\)](#). If standard DiD assumptions hold (e.g. parallel trends), δ_s identifies a relevant policy parameter.

likelihood of owning real estate, a decrease in unsecured debt, an increase in secured debt, and an increase in taxable wealth. Many of the estimated event-study coefficients are statistically significant, but the interpretation of some estimates is not straightforward since the outcomes (e.g. taxable debt) exhibit non-parallel trends prior to application. This is why we turn next to instrumental variable estimation to better understand the impact of receiving debt relief.

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.⁹ 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

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

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

where we refer to equations (2) and (3) as the first and second stage. 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 court

⁹In 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.

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

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 so that an applicant could potentially have been assigned to a different trustee. 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 relief cases.¹⁰ 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 wealth, 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 year of observation. We also include court-by-year fixed effects for the court and year of application 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

¹⁰These 515 trustee identifiers correspond to 502 different trustees 13 of whom work in two courts.

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 nonparametric 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 A7 in the Supplementary 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 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, suggesting that we do not have a weak instrument problem. 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 and that a trustee only affects the assigned applicant through the probability that the applicant is granted debt relief. Table A8 in the Supplementary 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 A8, 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 A8 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.¹¹

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.¹² 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 A5 in the Supplementary Appendix shows a scatter plot of the instrument and the dividend, with no apparent relationship between the two variables. In Table A9 in the Supplementary Appendix, 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 A9 suggest that our IV results are driven by exogenous entry into debt relief and not contaminated by variations in the conditions under which applicants receive debt relief.¹³

¹¹See 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.

¹²Trustees 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).

¹³The 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.

4.3.3 Monotonicity of instrument

In the presence of heterogenous 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 conduct two tests of monotonicity previously implemented by [Bhuller et al. \(2020\)](#) and [Norris, Pecenco and Weaver \(2021\)](#). 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 A10 in the Supplementary 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 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 A11 in the Supplementary Appendix shows results from the first stage regression in the same subsamples as in Table A10. 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 A12 in the Supplementary 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.

Being granted debt relief also leads to a significant increase in employment of about ten percentage points, or equivalently 20.7% of the follow-up mean. 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 \(2011\)](#).

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

$$I_{it} = P_{it} \cdot E_{it} \quad (5)$$

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

$$\Delta I = \Delta P \cdot E + P \cdot \Delta E \quad (6)$$

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 Supplementary

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.

The right column of Table 2 further indicates that the higher employment rate 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 A13 in the Supplementary 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.

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 A6 in the Supplementary Appendix). We also take a first step towards assessing the fiscal impact of the Danish debt relief program using these long-term estimates on earnings together with the evidence on debt relief and welfare dependency (see Section G in the Supplementary Appendix).

4.5 Financial status IV results

The estimated IV coefficients for financial status and real estate ownership in Table 3 show a significant increase in taxable assets for applicants who are granted debt relief corresponding

to about 200% of the follow-up mean (see Table A12 in the Supplementary Appendix). 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, also 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. 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). The net effect, operating through all these changes in 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.

4.6 IV results by follow-up period

We further split the follow-up period into three subperiods (1 to 5 years, 6 to 10 years, and 11 to 16 years) and estimate the two-stage least squares model with outcome data for one of the subperiods at a time. These results are presented in Table 4.

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 slightly lower in the first period (years 1 to 5) and then increases further in later periods. The impact of debt relief on assets, real estate, and the fraction of real estate owners is positive and significant in the first subperiod and continues to grow over time. Interestingly, the estimated impact on taxable debt is first negative but turns positive in

the second and third subperiods. Further disaggregation 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 follow-up period (11 to 16 years), taxable wealth has increased by 261,000 DKK which corresponds to around one and a half years of earned income for applicants in the year prior to application.

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 Supplementary Appendix. We start by varying the number of required cases per trustee from 20 to 50 and then 100 cases. Table A14 shows that by and large the estimated impact of receiving debt relief is similar across cutoff levels.

Instead of using the admission rate of trustees as a single instrument, one can use many fixed effects instruments, one for each collection of cases handled by the same trustee (Norris, Pecenco and Weaver (2021)). A caveat is that this alternative method can suffer from weak instrument bias if trustees are handling too few cases. Table A15 shows that using trustee fixed effects instruments produces lower estimates relative to our main IV results when we require that each trustee handles 20 cases, consistent with weak instrument bias towards OLS. If we require that each trustee handles 100 cases, the two IV methods produce similar results, reinforcing our choice of the single trustee admission rate as our main instrument.

In Table A16, we present robustness checks where we vary the construction of the admission rate of trustees. 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

similar across specifications.

Attrition in our sample is mainly due to emigration or death (see Table A17 for descriptive statistics and IV results). When we estimate our model on a balanced panel with no attrition (Table A18), 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 A19 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

To further understand the impact of debt relief, we estimate our IV model in subsamples based on sex, age, education, and earned income and present these estimates in Tables A20 and A21 in the Supplementary Appendix. The main effect of debt relief that we observe in our full sample is also present in most of these subsamples. Expressed in relative terms, the impact of debt relief on earned income is largest among women as opposed to men (29% vs 18%), and among applicants with low as opposed to high income measured over four years prior to application (39% vs 22%).

Our sample includes applicants for debt relief from 1984 to 2003 giving us 16 years of follow-up data for all applicants. We also conduct a subgroup analysis with early applicants who applied for debt relief from 1984 up until 1994, giving us 25 years of follow-up data for a smaller subsample. Figure 2 presents event-study graphs for earned income, employment, taxable assets, taxable wealth, unsecured debt, and secured debt for this early subsample. These graphs show that the impact of debt relief is persistent and present for at least 25 years after the year of application for debt relief.

Table A22 in the Supplementary Appendix presents the full set of IV results for the early subsample during 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 and sizeable 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 Supplementary Appendix E for details). Table A23 in the Supplementary Appendix 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. This similarity is also visible in follow-up means presented in Table A12 in the Supplementary 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 two mechanisms can 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 but we cannot rule out the presence of some measurement error. One type of error is that we may

have matched a person appearing in a court announcement to the wrong person in the Central Person Register. Another type of error is that we may have misclassified an applicant as having been granted as opposed to denied debt relief (and vice versa).

Both these types of errors are likely to induce a downward bias in the event-study estimates (attenuation bias). If the errors in the data collection process are uncorrelated across applicants handled by the same trustee (classical measurement error), our IV estimates are still consistent. The fact that the number of granted applicants in our database corresponds to 97.6% of the granted applicants in official Danish statistics, suggests that we have not severely under- or overestimated the fraction of applicants who are granted debt relief. Taking this argument one step further, there is no indication that our instrument is distorted due to inflated or deflated admission rates for individual trustees.

A second 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 small 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 is the main reason why our IV estimates are larger than the corresponding OLS estimates.

A similar type of bias has been discussed in the literature on the labor supply effects of disability insurance. In an influential paper, [Bound \(1989\)](#) argued that OLS estimates comparing granted and denied applicants for disability insurance overestimate the negative impact of disability insurance on employment and earnings, as individuals who are granted disability

insurance have worse work prospects than rejected applicants (negative selection into program participation based on expected future outcomes). Consistent with this interpretation, [Maestas, Mullen and Strand \(2013\)](#) use quasi-random assignment of US applicants for disability insurance to examiners with varying allowance rates and obtain IV estimates for the impact of disability insurance on earnings and employment that are smaller than corresponding OLS estimates. In our setting where program participation improves labor market outcomes, a similar selection mechanism leads to a downward bias in OLS estimates.

For all the reasons mentioned in this section, we think that the event-study estimates provide a lower bound on the impact of receiving debt relief in Denmark. We also think that the IV estimates are closer to a causal effect of debt relief and provide a better guide to the impacts of the Danish debt relief program.

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 the Supplementary Appendix F.

Debt relief is a transfer of wealth from creditors to debtors. This transfer can be described as a stock of wealth, or a flow of payments with a present discounted value equal to the transfer. If we regard the wealth transfer as a stock, debtors who are granted debt relief should work less since they experience a positive wealth shock (a reduction of their debt). This is arguably not the way to think about debt relief since applicants who are granted debt relief work more both in the US and Denmark. We believe instead that the transfer should be analyzed as a cancelled flow of payments from debtors to creditors. These payments act as an (implicit) tax on the earned income of debtors and make debt relief equivalent to a tax cut. This view is consistent with previous evidence from the US showing that the impact of debt relief is larger in states

with higher wage garnishment rates ([Dobbie and Song \(2015\)](#) and [Dobbie, Goldsmith-Pinkham and Yang \(2017\)](#)).

When debt is public and debtors are subject to wage garnishments proportional to earned income, the required payments from debtors to creditors are (more or less) identical to a tax. When debt is private, additional income generated by the debtor will increase the likelihood that creditors ask for partial repayment of the debt, and will raise the amount of required payments (see Section 2.3 for examples on how creditors can make claims on additional income). The more additional income the debtor receives, the larger will be the demands from creditors, similar to a tax that rises with income.

In this setting, we consider an example of an applicant for debt relief who has the mean disposable income, earned income, and debt of individuals in our sample. We assume that 20% of the applicant's earned income is withheld as wage garnishments prior to the court decision (the maximum). If denied debt relief, wage garnishments will be withheld from the applicant for the remaining life. 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 3.4). For completeness, an applicant who is granted debt relief in our example has to pay a mean dividend to creditors. 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, and ii) the applicant has to pay a dividend to the creditors.

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.¹⁴ Adopting this elasticity in our example,

¹⁴This estimate is in the higher end of published estimates based on Danish data.

the removal of wage garnishments produces a 12.2% annual increase in taxable earnings for an applicant who is granted debt relief.

To assess the impact of the dividend (a payment from the debtor to creditors that is fixed by the court and independent of future earned income), we treat the dividend as a lump-sum payment with a wealth effect. [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 dividend corresponds to a 1.0% increase in annual earned income for an applicant who is granted debt relief.

According to these numbers, the main impact of debt relief on earned income is due to the tax cut as opposed to the payment of the dividend. This pattern is consistent with our empirical estimates showing a large increase in earned income for granted applicants that persists *beyond* the repayment period (earnings are higher 16 years after a court decision whereas the repayment of the dividend typically lasts for five years only). The pattern is also consistent with the dividend being around 10% of total debt on average. It is the permanent removal of the debt (with associated persistent demands on debtors from creditors) that leads to an increase in earned income, not the repayment of the smaller dividend.

If the impact of debt relief is the sum of the tax cut and the required dividend, the total impact on earned income is an increase of 13.2%. 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 A5 in the Supplementary 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 "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 tax cut. 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 includes outcome variables that have not been studied previously 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. 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.

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 for the first time 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 that we document 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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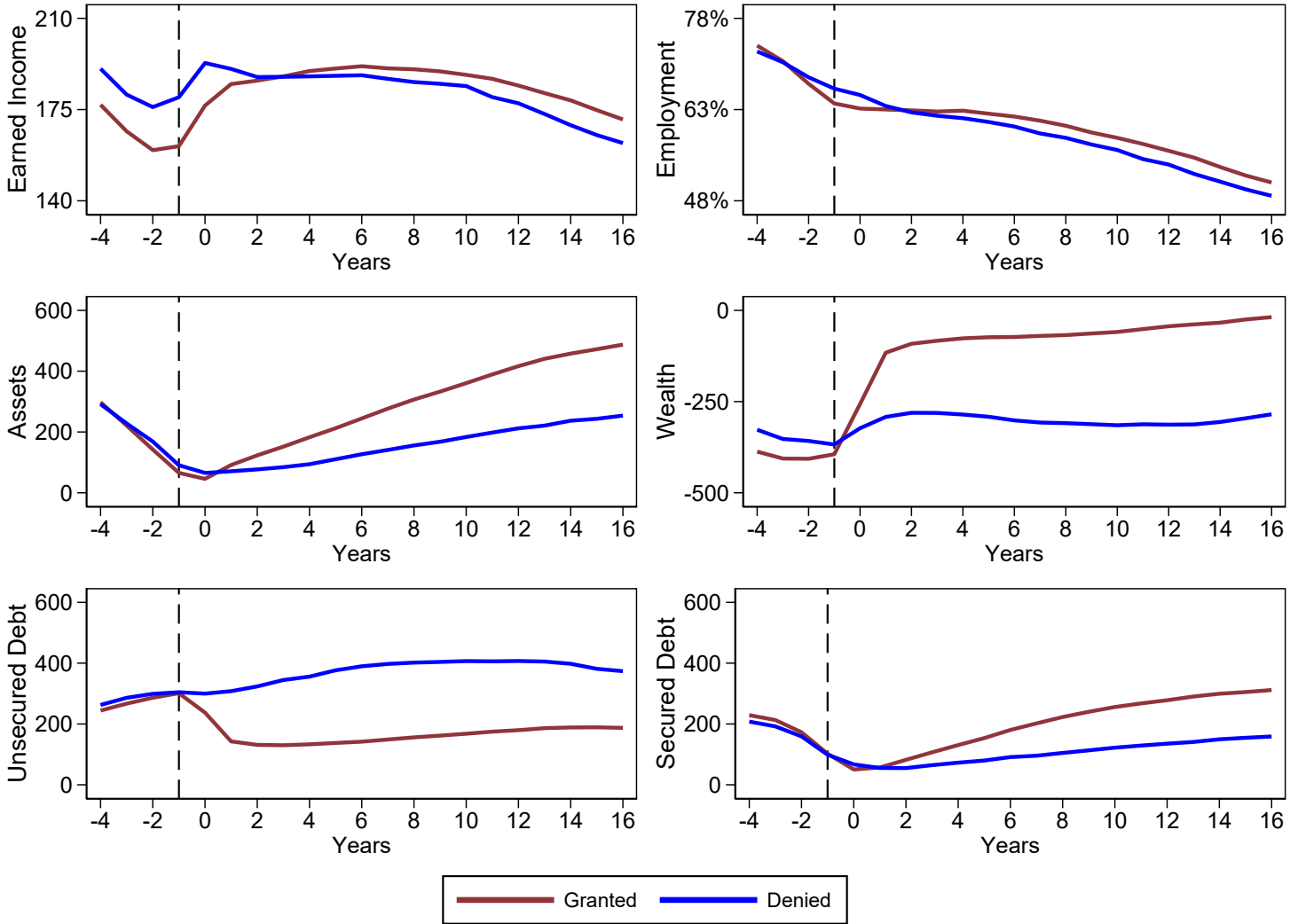
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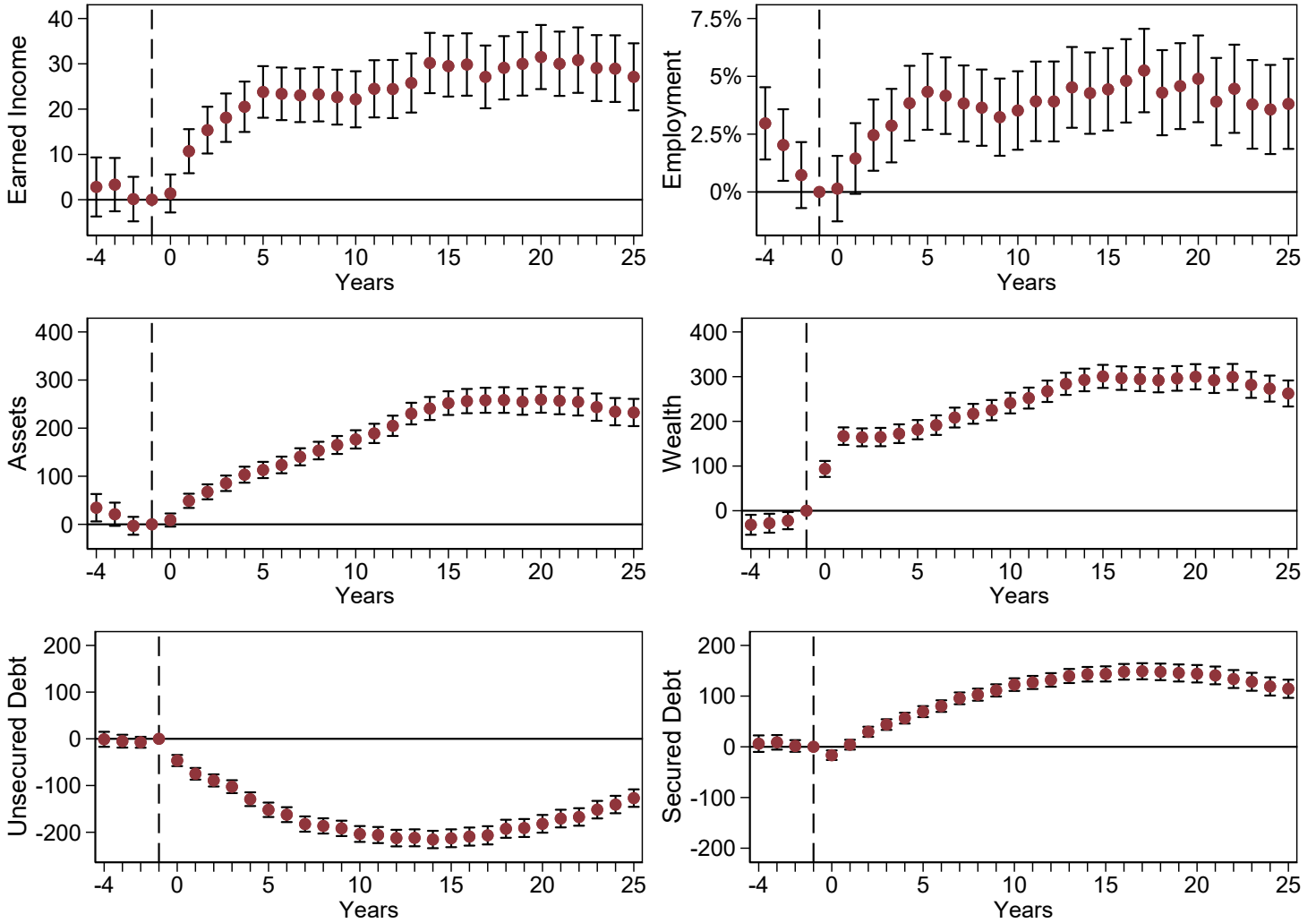
FIGURE 1.

Mean Outcomes Before and After Application for Debt Relief



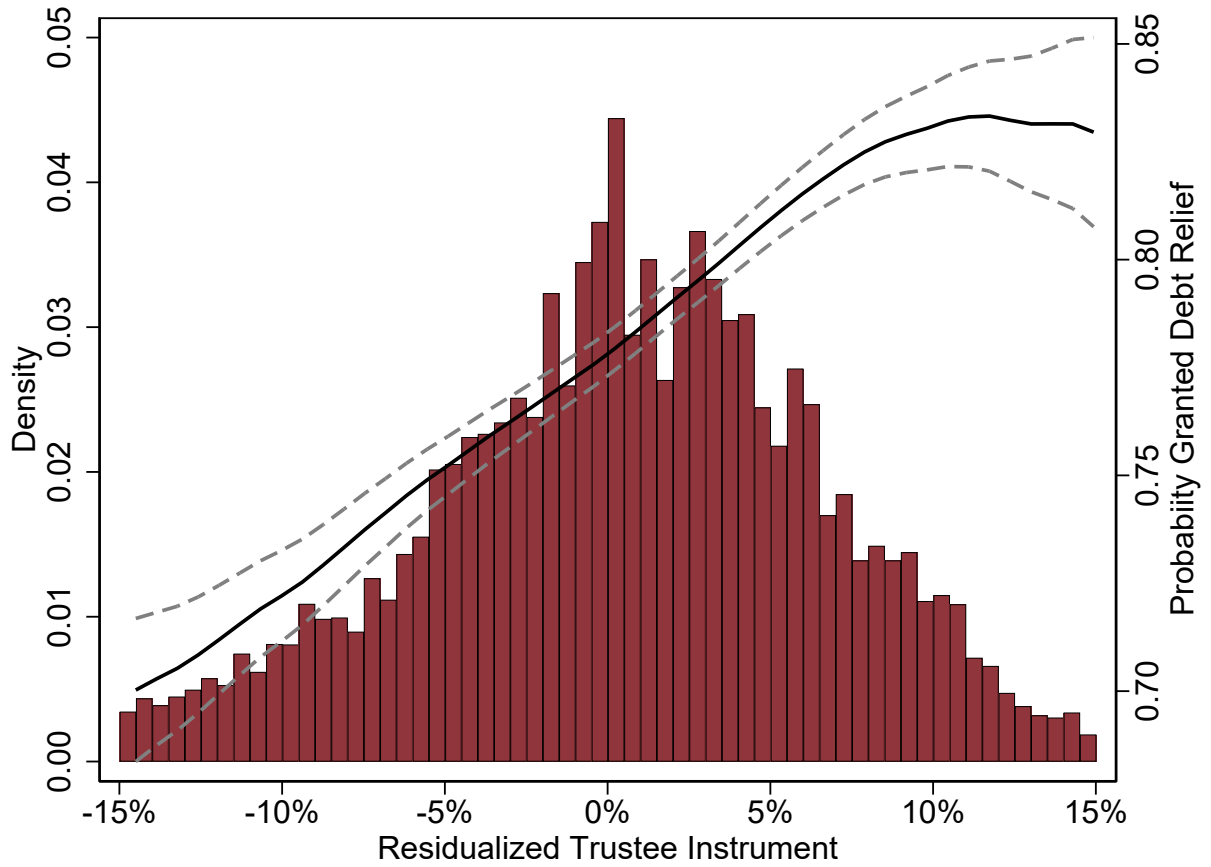
Note: This graph shows mean outcomes for granted and denied applicants for debt relief from 4 years before to 16 years after the year of application. The outcome variables are earned income (top left), employment (top right), taxable assets (middle left), taxable wealth (middle right), unsecured taxable 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.

FIGURE 2.
Long-Run Event-Study Graphs



Note: 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.

FIGURE 3.
Properties of Instrumental Variable



Note: 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).

TABLE 1.
Summary Statistics for Applicants of Debt Relief

	All	Granted	Denied
Mean age	44.2 (10.5)	44.3 (10.6)	43.8 (10.2)
Fraction men	63.3%	62.7%	65.2%
Fraction married	58.4%	58.2%	59.3%
Mean persons in household	2.5 (1.4)	2.5 (1.4)	2.6 (1.4)
Mean years of schooling	11.0 (2.9)	11.0 (2.9)	11.1 (2.9)
Mean earned income	165 (172)	161 (170)	180 (176)
Fraction employed	64.4%	63.9%	66.3%
Fraction unemployed	12.1%	12.5%	10.8%
Mean taxable wealth	-389 (635)	-394 (640)	-368 (616)
Mean taxable assets	71 (435)	66 (443)	91 (405)
Mean taxable debt	458 (700)	457 (702)	461 (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 for the year prior to application, further divided into applicants who were granted or denied debt relief. Monetary unit is thousands of 2020 DKK. Numbers in parentheses are standard errors.

TABLE 2.
Impact of Debt Relief on Labor Market Outcomes

Earned Income (DKK)	46,800** (15,200)
Employed (y/n)	0.117** (0.039)
Unemployed (y/n)	0.0050 (0.012)
Out of Labor Force (y/n)	-0.122** (0.040)
Hourly Wage (DKK)	11.6 (8.25)
Observations (individuals)	32,794

Notes: This table shows the estimated impact of debt relief on labor market outcomes using instrumental variable regression. 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 3.
Impact of Debt Relief on Household Finances and Real Estate Ownership

Taxable Wealth (DKK)	282,500** (46,400)
Taxable Assets (DKK)	309,300** (54,400)
Taxable Debt (DKK)	7,870 (66,000)
Taxable Secured Debt (DKK)	201,400** (38,000)
Taxable Unsecured Debt (DKK)	-188,100** (42,600)
Owns Real Estate (y/n)	0.248** (0.044)
Taxable Real Estate (DKK)	260,800** (47,700)
Observations (individuals)	32,794

Notes: This table shows the estimated impact of debt relief on household finances and real estate ownership using instrumental variable regression. 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). Numbers in parentheses are standard errors clustered at the level of the trustee identifier. ** p<0.01, * p<0.05.

TABLE 4.
Impact of Debt Relief by Follow-up Period

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

Supplementary Appendix

to

The Long-Run Effects of Individual Debt Relief

(for Online Publication)

August 1, 2023

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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 birthdate.

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 most common reason for a name change in our data appears to be that married women switch to the last name of their husband. 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 A1 in the Supplementary Appendix 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 A3 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.¹⁵ 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.¹⁶

In Figure A6 in the Supplementary Appendix, 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. In 2005, the debt relief program was reformed with the intention of making it easier to apply for debt relief. The 2005 reform appears to have triggered an increase in the number of applicants for debt relief (with a slight delay), but the increase only lasted for a relatively short period of time.

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 Efterretninger, Social Sikring og Retsvæsen) with publication numbers 1986:6, 1987:6, 1988:7,

¹⁵We exclude data for 1984 from the calculation since the debt relief program was initiated on July 1st 1984.

¹⁶This 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).

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, Kriminalitet 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

$$\Delta I = \Delta P \cdot E + P \cdot \Delta E \quad (8)$$

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:

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

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

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):

$$S_{E0} = \frac{\Delta P \cdot E_0}{\Delta I} \quad (11)$$

$$S_{E1} = \frac{\Delta P \cdot E_1}{\Delta I} \quad (12)$$

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

$$\Delta I = 46,800 \quad (13)$$

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

$$\Delta P = 0.117 \quad (14)$$

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

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

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

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 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). Complifiers 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

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

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

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

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

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

To estimate these shares in our sample, we follow [Dahl, Kostøl and Mogstad \(2014\)](#) and 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:

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

$$\hat{\pi}_a = \hat{\chi} + \hat{\eta} \cdot \underline{z} \quad (21)$$

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

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 conditional probability and the assumption of monotonicity implies that

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

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 A23.

F 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.

F.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

$$\Delta \log(1 - \tau) = \log(1 - 0.445 - 0.2) - \log(1 - 0.445) \quad (24)$$

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%.

F.2 Wealth effect due to dividend

The mean debt of individuals in the repayment sample is 1.55 million DKK and the mean dividend is 10.2% which implies that the average applicant who is granted debt relief has to repay 158,100 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 1685 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 1.0% for applicants who are granted debt relief.

G 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.¹⁷ 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 A13).

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 A13) 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

$$288 + 34 + 15 = 337 \quad (25)$$

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 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 (1550) and dividend (10.2%) in the repayment sample, and assume that financial institutions pay a corporate income tax of 37.8% (the mean from 1986 to 2003),¹⁸ the cost in terms of lower tax revenue is

¹⁷We 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.

¹⁸Retrieved from the homepage of the Danish Tax Ministry at www.skm.dk/skattetal/satser/tidsserier.

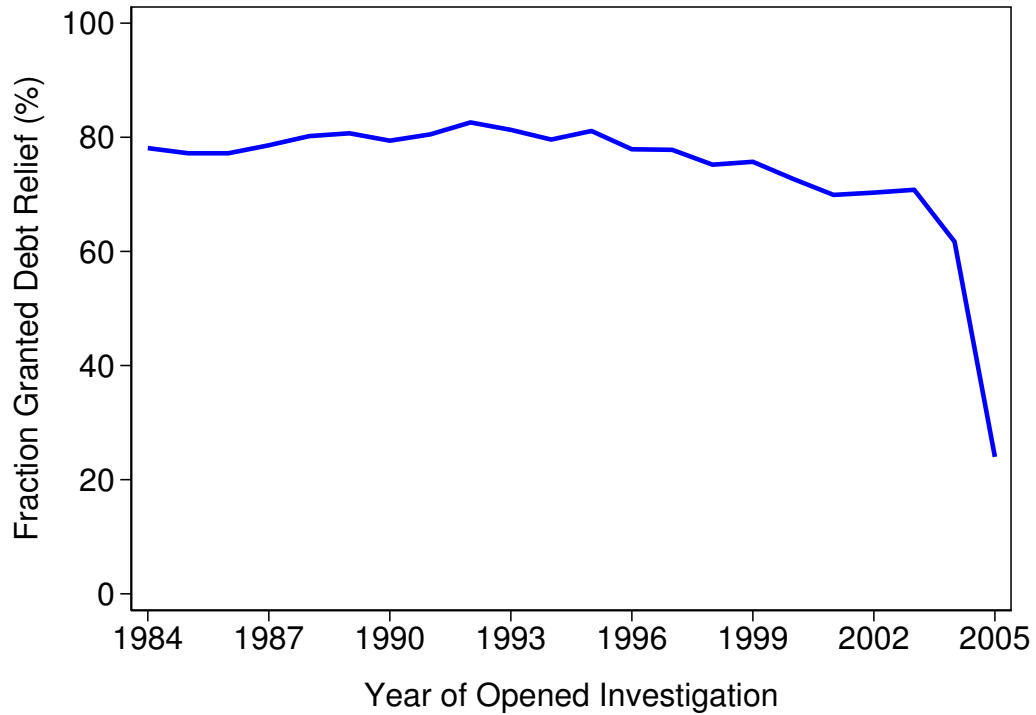
$$1550 \cdot (1 - 0.102) \cdot 0.378 \approx 526 \quad (26)$$

Subtracting our estimated benefits from costs gives a net fiscal cost per granted applicant for debt relief of $526 - 337 = 189$ (one hundred eighty-nine thousand DKK or approximately twenty-eight 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 $(1 - 0.102) \cdot 0.378 \approx 0.34$ 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.

FIGURE A1.

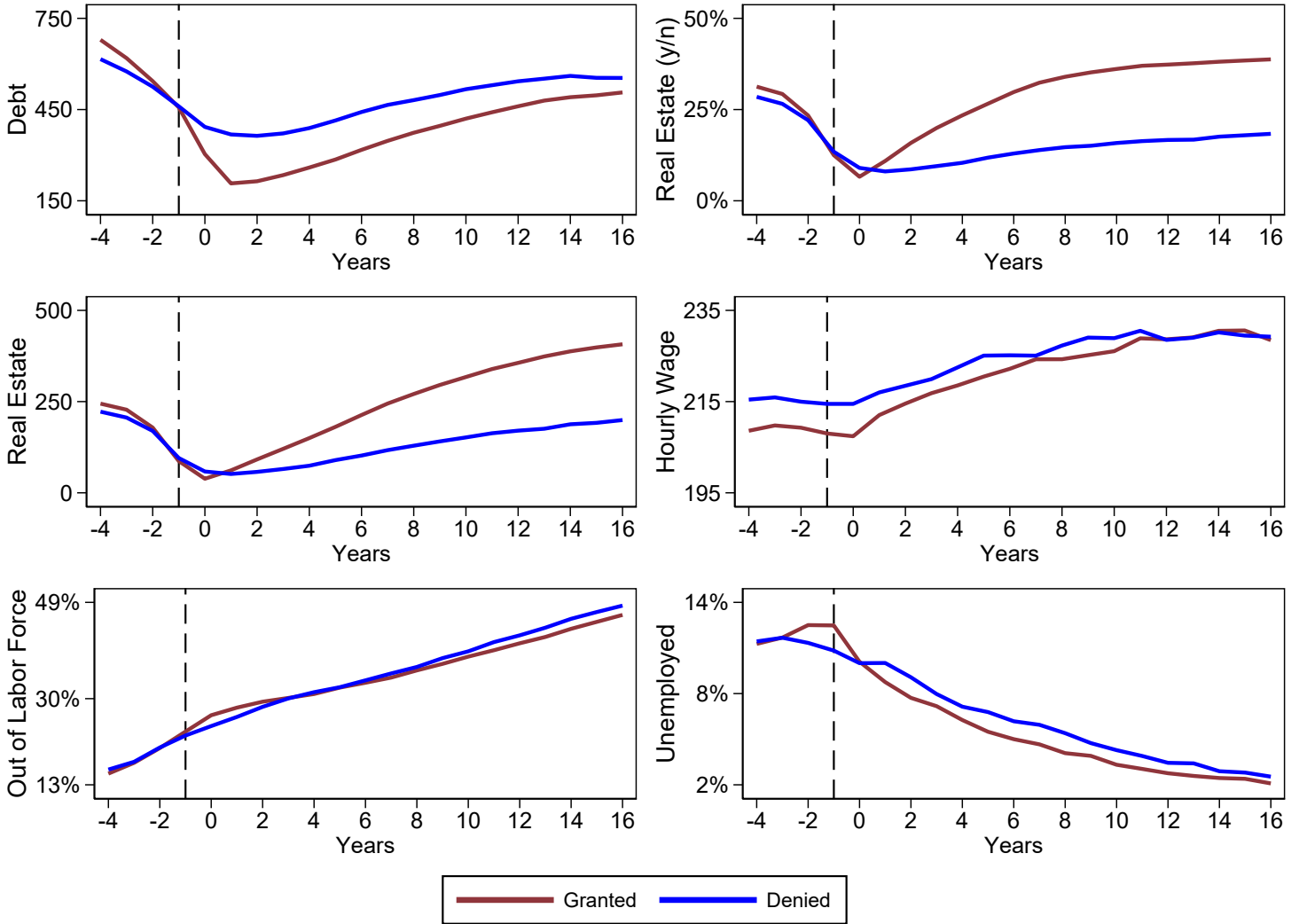
Fraction Granted Debt Relief in Initial Sample



Note: 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).

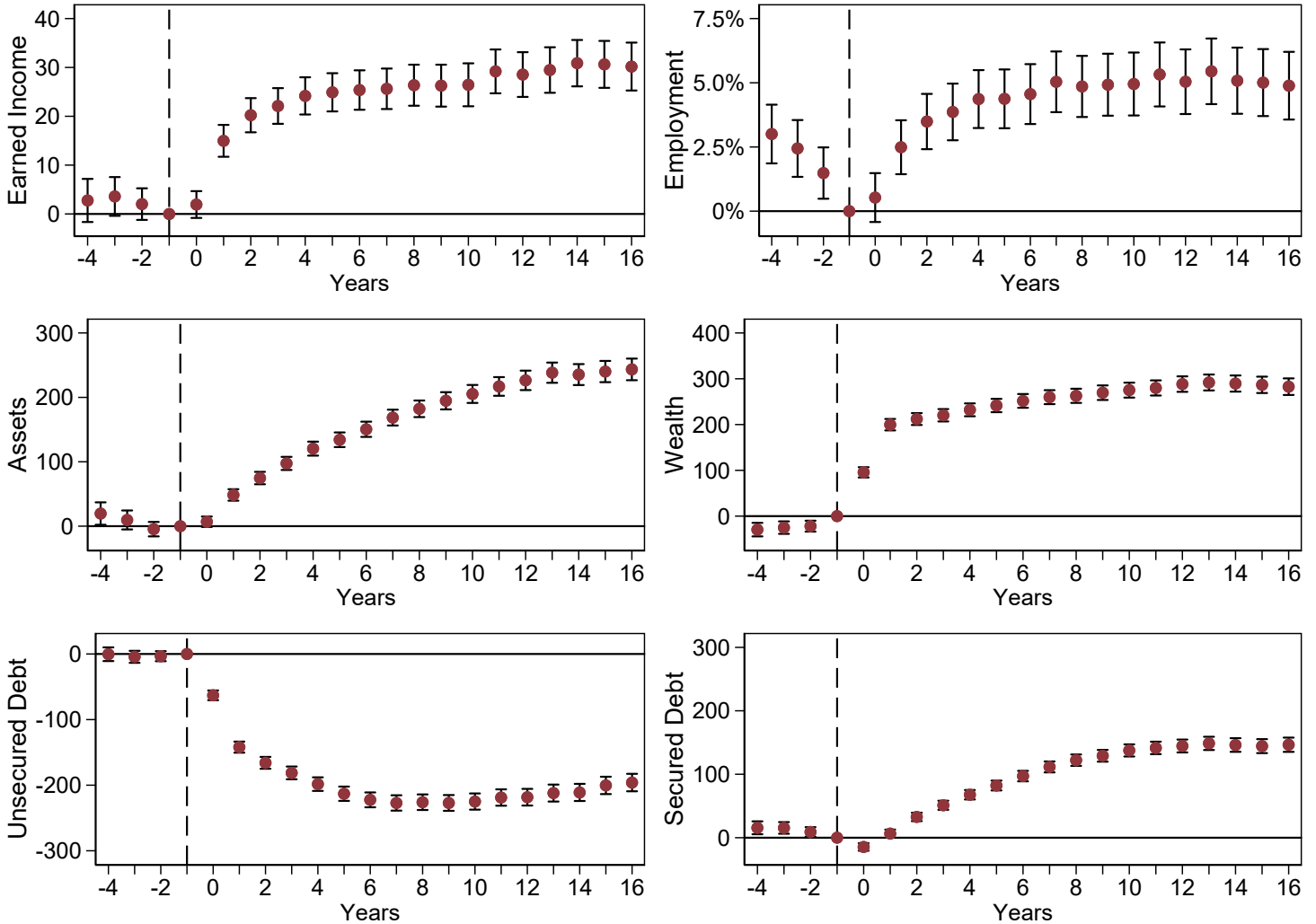
FIGURE A2.

Mean Outcomes Before and After Application for Debt Relief



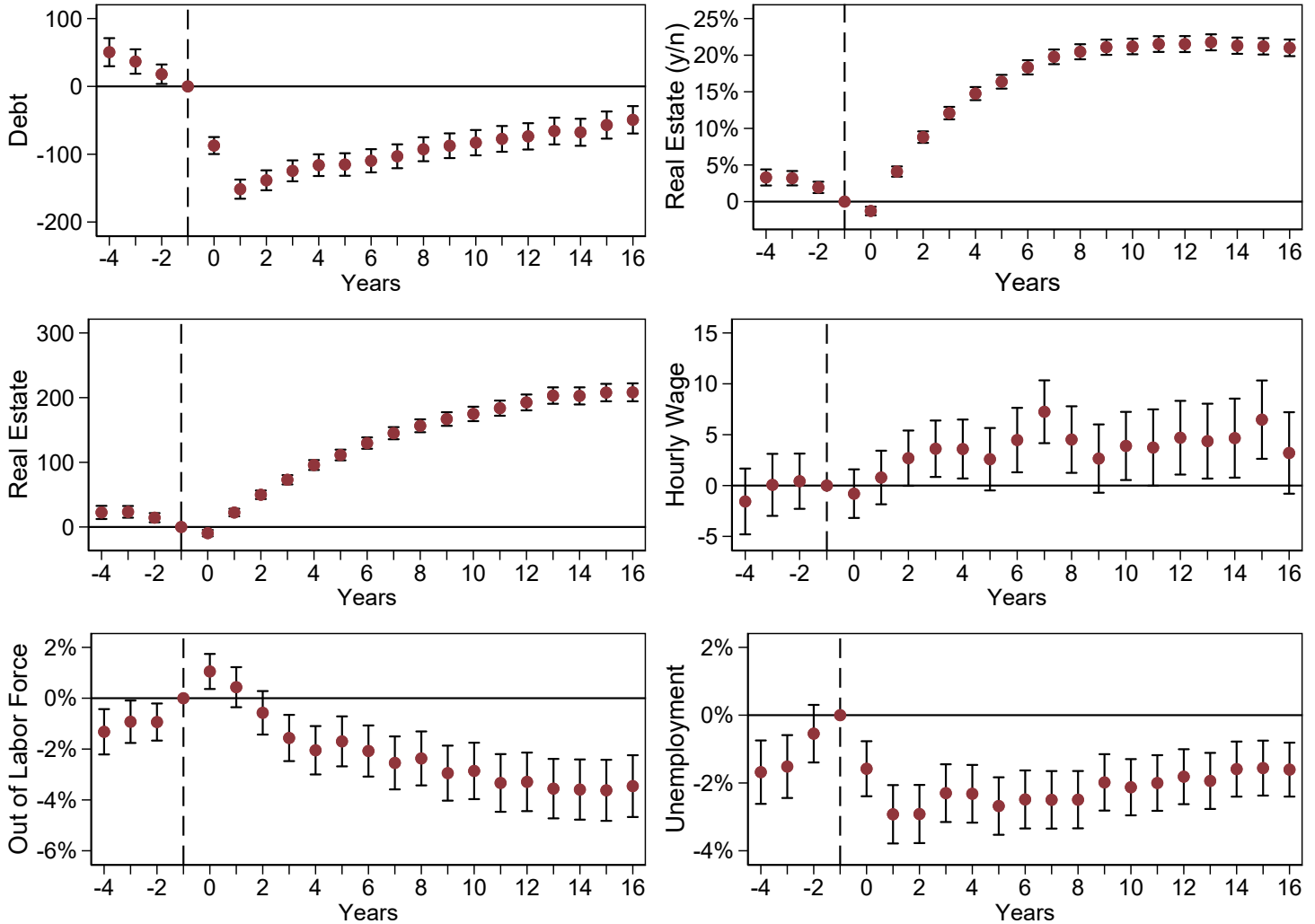
Note: This graph shows mean outcomes for granted and denied applicants for debt relief from 4 years before to 16 years after the year of application. The outcome variables are taxable debt (top left), the fraction of real estate owners (top right), taxable real estate (middle left), the hourly wage rate 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 A3.
Event-Study Graphs



Note: 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.

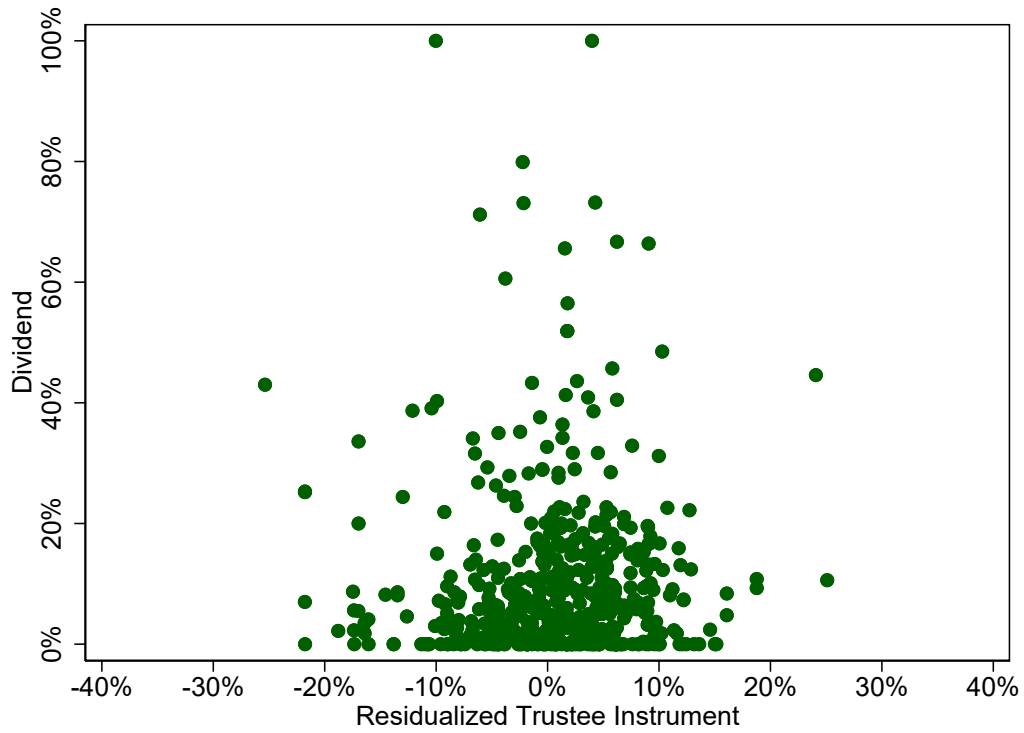
FIGURE A4.
Event-Study Graphs



Note: 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 A5.

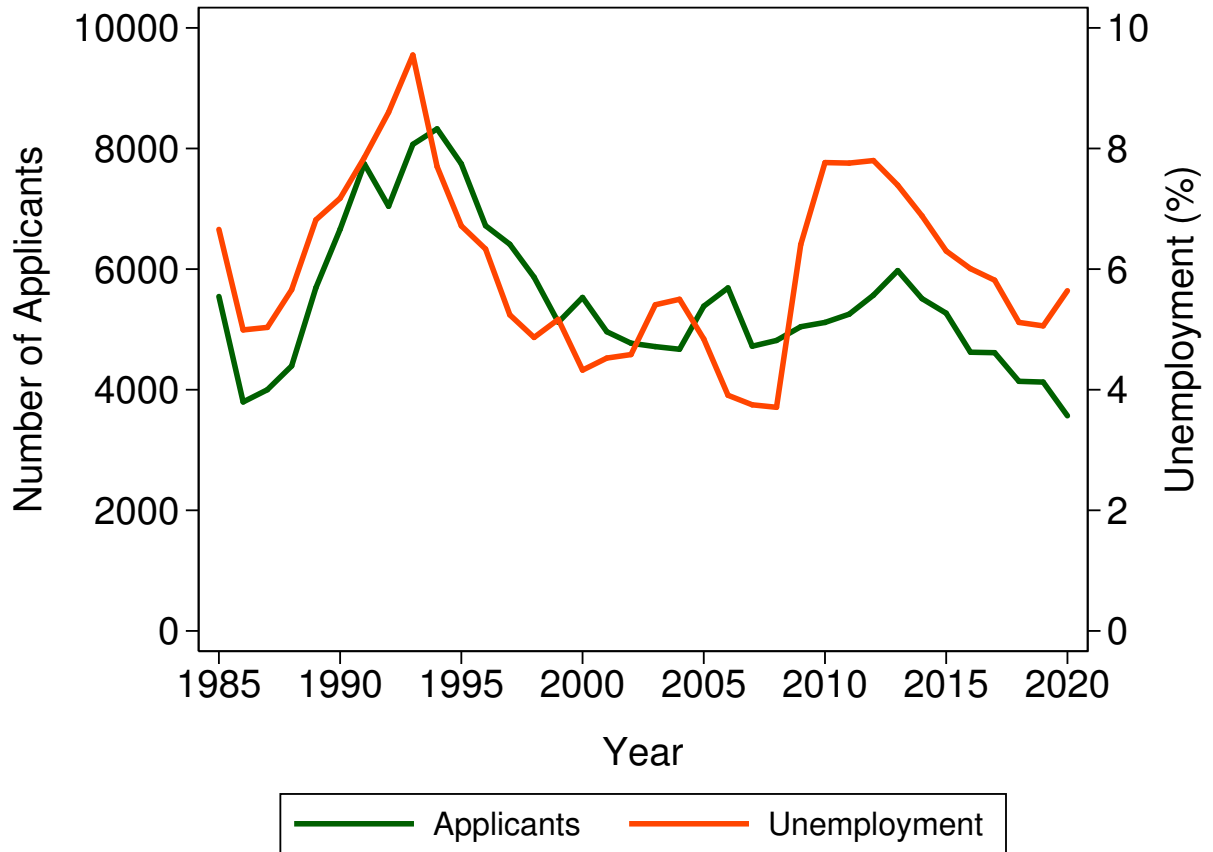
Dividend and Trustee Instrument



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=510).

FIGURE A6.

Number of Applicants for Debt Relief and the Unemployment Rate



Note: 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 in this Supplementary Appendix) 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.

TABLE A1.
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

Note: 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 of this Supplementary Appendix.

TABLE A2.
Number of Cases per City Court

Court	Cases	Court	Cases
Aabenraa	326	Naksoy	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	Rødning	361
Gråsten	257	Rønne	473
Haderslev	399	Silkeborg	795
Helsingø	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 A3.
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

Note: 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 of this Supplementary Appendix.

TABLE A4.
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	1987-1994 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 A5.
Applicants for Debt Relief versus General Population

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

TABLE A6.
Debt and Repayment Statistics

<u>Dividend (%)</u>	
Mean (SD)	10.2 (13.6)
Median (Interquartile range)	6.0 (1.4–13.5)
Observations	827
<u>Repayment period (yrs)</u>	
Mean (SD)	5.0 (0.8)
Median (Interquartile range)	5.0 (5.0–5.0)
Observations	373
<u>Monthly repayment</u>	
Mean (SD)	2,190 (2330)
Median (Interquartile range)	1,630 (930–2,830)
Observations	246
<u>Unsecured debt (millions)</u>	
Mean (SD)	1.55 (1.73)
Median (Interquartile range)	1.08 (0.64–1.9)
Observations	262

Note: This table shows debt and repayment statistics for a random sample of individuals who were granted debt relief between 1984 and 2005. The data was 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 A7.
IV First Stage Regression

	Without covariates	With covariates
Instrument	0.532 (0.037) **	0.535 (0.037) **
Male		-0.029 (0.0041) **
Age 0-40		-0.068 (0.021) **
Age 41-50		-0.058 (0.020) **
Age 51-60		-0.057 (0.020) **
Age 61-70		4.26E-04 (0.021)
Single Household (y/n)		0.029 (0.0064) **
Earned Income (DKK)		-1.13E-07 (2.41E-08) **
Employment		0.032 (0.011) **
Unemployment		0.0055 (0.014)
Married (y/n)		-0.016 (0.0056) **
Immigrant (y/n)		0.0042 (0.015)
Real Estate Ownership (y/n)		-0.035** (0.0091) **
Taxable Debt (DKK)		1.16E-08 (4.06E-09) **
Taxable Assets (DKK)		-6.25E-09 (6.59E-09)
Highschool (y/n)		0.023 (0.0053) **
University (y/n)		-0.0041 (0.0078)
Education Missing (y/n)		-0.012 (0.013)
Social Assistance (y/n)		-9.08E-04 (0.0093)
Wage Quartile 1		0.0066 (0.0082)
Wage Quartile 2		0.0069 (0.0089)
Wage Quartile 3		-0.013 (0.0086)
Wage Quartile 4		-0.020 (0.0087) *
Observations (individuals)	33,186	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 A8.
Instrument Balance Test

	Instrument	Granted Debt Relief
Male	-1.72E-04 (8.53E-04)	-2.88E-02 (4.14E-03) **
Age 0-40	8.88E-04 (3.81E-03)	-6.72E-02 (2.10E-02) **
Age 41-50	8.66E-04 (3.72E-03)	-5.75E-02 (2.08E-02) **
Age 51-60	-2.29E-03 (3.86E-03)	-5.78E-02 (2.03E-02) **
Age 61-70	5.05E-04 (3.68E-03)	6.96E-04 (2.14E-02)
Single Household (y/n)	-1.38E-03 (1.15E-03)	2.77E-02 (6.34E-03) **
Earned Income (DKK)	-2.76E-09 (4.23E-09)	-1.15E-07 (2.42E-08) **
Employment	-1.00E-03 (1.99E-03)	3.15E-02 (1.12E-02) **
Unemployment	1.32E-03 (2.30E-03)	6.20E-03 (1.36E-02)
Married (y/n)	3.98E-04 (1.09E-03)	-1.55E-02 (5.65E-03) **
Immigrant (y/n)	3.06E-03 (2.77E-03)	5.79E-03 (1.46E-02)
Real Estate Ownership (y/n)	2.20E-03 (1.52E-03)	-3.36E-03 (9.13E-03) **
Taxable Debt (DKK)	4.75E-10 (7.25E-10)	1.19E-08 (4.11E-09) **
Taxable Assets (DKK)	-2.66E-10 (1.32E-09) *	-7.67E-09 (6.70E-09)
Highschool (y/n)	2.81E-04 (8.97E-04)	2.29E-02 (5.34E-03) **
University (y/n)	3.45E-03 (2.72E-03)	-2.30E-03 (8.26E-03)
Education Missing (y/n)	1.86E-03 (2.61E-03)	-1.08E-02 (1.34E-02)
Social Assistance (y/n)	-3.18E-03 (1.61E-03) *	-2.61E-03 (9.31E-03)
Wage Quartile 1	1.32E-03 (1.36E-03)	7.28E-03 (8.20E-03)
Wage Quartile 2	2.15E-03 (1.41E-03)	8.05E-03 (8.90E-03)
Wage Quartile 3	9.44E-04 (1.43E-03)	8.05E-03 (8.90E-03)
Wage Quartile 4	2.23E-03 (1.58E-03)	-1.84E-02 (8.73E-03) *
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 A9.
Dividend and Trustee Instrument

DIVIDEND NOT WINSORIZED		
	Without Covariates	With Covariates
Instrument	-4.42E.04 (0.098)	-0.0043 (0.095)
Observations (individuals)	510	504
R2	0.000	0.124
DIVIDEND WINSORIZED		
	Without Covariates	With Covariates
Instrument	0.0091 (0.094)	0.0059 (0.090)
Observations (individuals)	510	504
R2	0.000	0.132

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 A10.
IV First Stage Regression in Subsamples

	Men	Women
Instrument	0.502 ** (0.037)	0.591 ** (0.055)
Observations (individuals)	20,411	12,520
R2	0.096	0.142
	Young	Old
Instrument	0.535 ** (0.050)	0.532 ** (0.051)
Observations (individuals)	17,581	15,350
R2	0.120	0.142
	Low education	High education
Instrument	0.571 ** (0.056)	0.513 ** (0.044)
Observations (individuals)	13,784	19,147
R2	0.139	0.115
	Low income	High income
Instrument	0.510 ** (0.041)	0.566 ** (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 A11.

IV First Stage Regression in Subsamples with Reverse-sample Instrument

	Men	Women
Instrument	0.435 ** (0.042)	0.591 ** (0.053)
Observations (individuals)	18,912	13,250
R2	0.106	0.148
	Young	Old
Instrument	0.320 ** (0.050)	0.320 ** (0.052)
Observations (individuals)	17,205	15,778
R2	0.124	0.143
	Low education	High education
Instrument	0.450 ** (0.054)	0.366 ** (0.043)
Observations (individuals)	14,289	18,812
R2	0.142	0.118
	Low income	High income
Instrument	0.418 ** (0.038)	0.424 ** (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 A12.
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 A13.
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 A14.
Instrumental Variable Estimates by Required Cases per Trustee

	20 Cases	50 Cases	100 Cases
Earned Income (DKK)	46,800** (15,200)	45,500** (15,900)	51,500** (16,800)
Employed (y/n)	0.117** (0.039)	0.108* (0.045)	0.163** (0.051)
Unemployed (y/n)	0.0050 (0.012)	4.69E-04 (0.013)	0.015 (0.014)
Out of Labor Force (y/n)	-0.122** (0.040)	-0.111* (0.046)	-0.182** (0.051)
Hourly Wage (DKK)	11.6 (8.25)	11.0 (9.59)	5.52 (9.60)
Taxable Wealth (DKK)	282,500** (46,400)	347,600** (49,700)	330,500** (61,400)
Taxable Assets (DKK)	309,300** (54,400)	240,100** (58,100)	287,300** (61,800)
Taxable Debt (DKK)	7,870 (66,000)	-136,700* (66,800)	-77,900 (81,900)
Taxable Secured Debt (DKK)	201,400** (38,000)	163,900** (41,400)	196,900** (39,500)
Taxable Unsecured Debt (DKK)	-188,100** (42,600)	-267,600** (37,000)	-302,400** (35,200)
Owns Real Estate (y/n)	0.248** (0.044)	0.207** (0.041)	0.238** (0.044)
Real Estate (DKK)	260,800** (47,700)	218,300** (48,800)	252,700** (49,300)
Observations (individuals)	32,794	23,113	11,065

Notes: This table shows the estimated impact of debt relief using the admission rate of the assigned trustee as an instrumental variable, 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 A15.
Trustee Fixed Effects as Instrumental Variables

	20 Cases	50 Cases	100 Cases
Earned Income (DKK)	25,200** (8,190)	32,600** (10,500)	41,700** (14,900)
Employed (y/n)	0.073** (0.021)	0.087** (0.029)	0.149** (0.041)
Unemployed (y/n)	0.0035 (0.0061)	0.0012 (0.0082)	0.0099 (0.012)
Out of Labor Force (y/n)	-0.077** (0.021)	-0.090** (0.030)	-0.163** (0.040)
Hourly Wage (DKK)	4.49 (3.62)	4.81 (5.01)	-1.731 (7.22)
Taxable Wealth (DKK)	249,100** (25,300)	285,900** (34,600)	304,400** (50,700)
Taxable Assets (DKK)	214,700** (29,400)	190,700** (37,600)	252,400** (52,600)
Taxable Debt (DKK)	-53,700 (34,800)	-114,600** (42,600)	-78,200 (63,200)
Taxable Secured Debt (DKK)	140,900** (21,200)	126,100** (27,400)	178,500** (33,300)
Taxable Unsecured Debt (DKK)	-185,000** (22,500)	-227,500** (30,000)	-257,200** (39,700)
Owns Real Estate (y/n)	0.194** (0.024)	0.207** (0.041)	0.225** (0.039)
Real Estate (DKK)	182,800** (26,000)	168,500** (31,800)	221,600** (41,600)
Observations (individuals)	32,794	23,113	11,065

Notes: This table shows the estimated impact of debt relief using trustee fixed effects as instrumental variables, 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 A16.
Alternative Specifications of Instrumental Variable

	Vary by year	Leave out year	Split sample
Earned Income (DKK)	56,300* (26,500)	48,800* (19,000)	76,700** (25,300)
Employed (y/n)	0.145* (0.064)	0.121* (0.049)	0.201** (0.065)
Unemployed (y/n)	-0.022 (0.019)	0.0097 (0.015)	0.0030 (0.020)
Out of Labor Force (y/n)	-0.124 (0.065)	-0.131** (0.049)	-0.205** (0.064)
Hourly Wage (DKK)	30.5* (14.8)	9.52 (9.95)	12.8 (14.3)
Taxable Wealth (DKK)	71,900 (79,800)	316,200** (55,400)	299,300** (69,500)
Taxable Assets (DKK)	246,600** (85,400)	344,800** (68,900)	369,100** (87,900)
Taxable Debt (DKK)	157,600 (104,200)	8,720 (82,100)	75,400 (90,400)
Taxable Secured Debt (DKK)	191,100** (58,400)	212,400** (47,600)	220,800** (57,600)
Taxable Unsecured Debt (DKK)	-136,700** (20,400)	-170,000** (36,700)	-161,300** (22,400)
Owns Real Estate (y/n)	0.217** (0.072)	0.268** (0.055)	0.298** (0.072)
Real Estate (DKK)	221,900** (75,200)	287,600** (60,500)	307,200** (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 A17.
Attrition

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

Note: 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 A18.
Balanced Panel Results

	With Attrition	Balanced Panel
Earned Income (DKK)	46,800** (15,200)	42,200* (16,500)
Employed (y/n)	0.117** (0.039)	0.106* (0.042)
Unemployed (y/n)	0.0050 (0.012)	0.0029 (0.013)
Out of Labor Force (y/n)	-0.122** (0.040)	-0.109** (0.042)
Hourly Wage (DKK)	11.6 (8.25)	8.56 (8.84)
Taxable Wealth (DKK)	282,500** (46,400)	294,200** (50,400)
Taxable Assets (DKK)	309,300** (54,400)	328,200** (60,600)
Taxable Debt (DKK)	7,870 (66,000)	10,600 (71,600)
Taxable Secured Debt (DKK)	201,400** (38,000)	212,700** (42,900)
Taxable Unsecured Debt (DKK)	-188,100** (42,600)	-196,400** (46,700)
Owns Real Estate (y/n)	0.248** (0.044)	0.272** (0.048)
Real Estate (DKK)	260,800** (47,700)	275,700** (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 A19.
Alternative Levels of Clustering

	Court	Individual	Court-by-year	Trustee-by-year
Earned Income (DKK)	46,800** (17,400)	46,800** (16,300)	46,800** (18,500)	46,800** (17,900)
Employed (y/n)	0.117** (0.043)	0.117** (0.039)	0.117** (0.041)	0.117** (0.040)
Unemployed (y/n)	0.0050 (0.0092)	0.0050 (0.012)	0.0050 (0.0096)	0.0050 (0.013)
Out of Labor Force (y/n)	-0.122** (0.041)	-0.122** (0.039)	-0.122** (0.041)	-0.122** (0.045)
Hourly Wage (DKK)	11.6 (11.5)	11.6 (8.82)	11.6 (10.6)	11.6 (7.78)
Taxable Wealth (DKK)	282,500** (38,700)	282,500** (45,200)	282,500** (42,600)	282,500** (55,200)
Taxable Assets (DKK)	309,300** (74,800)	309,300** (56,300)	309,300** (58,400)	309,300** (39,400)
Taxable Debt (DKK)	7,870 (83,100)	7,870 (60,900)	7,870 (79,600)	7,870 (70,000)
Taxable Secured Debt (DKK)	201,400** (50,900)	201,400** (38,500)	201,400** (38,100)	201,400** (27,100)
Taxable Unsecured Debt (DKK)	-188,100** (36,200)	-188,100** (36,200)	-188,100** (45,800)	-188,100** (54,900)
Owens Real Estate (y/n)	0.248** (0.056)	0.248** (0.044)	0.248** (0.047)	0.248** (0.037)
Real Estate (DKK)	260,800** (64,800)	260,800** (49,700)	260,800** (48,800)	260,800** (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 A20.
Subgroup Analysis by Sex and Age

	Men	Women	Young	Old
Earned Income (DKK)	35,000 (21,700)	46,600* (18,400)	58,000** (22,300)	31,600 (19,600)
Employed (y/n)	0.102* (0.050)	0.118* (0.053)	0.103* (0.050)	0.114 (0.060)
Unemployed (y/n)	0.0071 (0.015)	0.0036 (0.018)	-0.0040 (0.016)	0.017 (0.015)
Out of Labor Force (y/n)	-0.109* (0.051)	-0.123* (0.053)	-0.101* (0.049)	-0.129* (0.061)
Hourly Wage (DKK)	-1.11 (12.8)	21.4 (12.0)	18.8 (9.23)	-17.1 (16.0)
Taxable Wealth (DKK)	358,600** (67,800)	151,800** (47,800)	197,700** (59,600)	401,400** (70,100)
Taxable Assets (DKK)	303,700** (71,500)	310,600** (75,300)	388,400** (81,300)	248,200** (76,500)
Taxable Debt (DKK)	-97,200 (90,700)	171,600* (72,400)	174,800* (85,200)	-175,300* (88,900)
Taxable Secured Debt (DKK)	199,600** (48,500)	212,900** (51,500)	275,800** (57,400)	129,400* (50,200)
Taxable Unsecured Debt (DKK)	-265,600** (50,700)	-86,100* (33,500)	-117,100** (38,100)	-279,500** (52,200)
Owens Real Estate (y/n)	0.224** (0.057)	0.273** (0.059)	0.290** (0.061)	0.238** (0.063)
Real Estate (DKK)	262,400** (61,000)	253,100** (67,900)	354,500** (71,400)	177,700** (68,000)
Observations (individuals)	20,308	12,486	17,545	15,249

Notes: This table shows the estimated impact of debt relief using the admission rate of the assigned trustee as an instrumental variable, in subgroups defined by sex and age (below or above 45 years of age). Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

TABLE A21.
Subgroup Analysis by Education and Earned Income

	Low Education	High Education	Low Income	High Income
Earned Income (DKK)	32,400 (21,200)	59,200* (27,900)	39,300 (20,400)	54,700* (21,900)
Employed (y/n)	0.126* (0.060)	0.095 (0.060)	0.161 (0.062)	0.076 (0.045)
Unemployed (y/n)	0.0060 (0.016)	-3.74E-04 (0.019)	0.024 (0.020)	-0.011 (0.014)
Out of Labor Force (y/n)	-0.135* (0.061)	-0.094 (0.061)	-0.184*** (0.062)	-0.065 (0.045)
Hourly Wage (DKK)	2.97 (9.96)	14.6 (14.1)	15.7 (13.7)	12.5 (9.67)
Taxable Wealth (DKK)	190,300** (56,100)	371,200** (71,400)	310,000** (62,800)	257,900** (61,200)
Taxable Assets (DKK)	194,400** (72,000)	437,700** (94,300)	263,000** (75,600)	367,100** (75,500)
Taxable Debt (DKK)	922 (75,300)	39,000 (103,800)	-55,700 (86,300)	85,300 (87,200)
Taxable Secured Debt (DKK)	124,700** (47,500)	324,100** (68,500)	139,100** (47,200)	271,300** (55,800)
Taxable Unsecured Debt (DKK)	-114,600** (40,500)	-249,600** (50,900)	-212,900** (46,900)	-170,400** (40,800)
Owns Real Estate (y/n)	0.148* (0.059)	0.392** (0.073)	0.232** (0.061)	0.279** (0.062)
Real Estate (DKK)	190,300** (64,800)	379,300** (80,400)	210,000** (64,300)	323,400** (68,300)
Observations (individuals)	13,725	14,502	16,340	16,454

Notes: This table shows the estimated impact of debt relief using the admission rate of the assigned trustee as an instrumental variable, in subgroups defined by education and earned income. Monetary unit is 2020 DKK. Numbers in parentheses are standard errors clustered by trustee identifier. ** p<0.01, * p<0.05.

TABLE A22.
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 A23.
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 E in this Supplementary Appendix for more details).