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Designing Debt Restructuring: The Adverse Effects on Labor Market Outcomes*

Jakob Beuschlein†

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Abstract

This paper examines how the design of debt restructuring or personal insolvency programs shapes labor market outcomes using Sweden as a case study. In the Swedish debt restructuring program, debt is forgiven following a 5-year partial repayment period. I estimate the causal effects of the program using an examiner instrumental variable (IV) design and show that traditional examiner IV estimates can be biased if there is path dependency in examiner decision-making. This bias is avoided by estimating examiner leniency from past cases. On average, participation in the debt restructuring program has negative effects on labor income and employment, but these findings mask a large heterogeneity. Initially employed participants experience relative gains in income and employment, while initially unemployed participants face substantial negative effects. Both of these effects persist even after the program has ended. Combining observational data and results from a survey I conducted among debt restructuring participants, I show that the risk of substantial increases in repayment obligations disincentivizes unemployed participants from seeking employment. I then calibrate a labor supply model and show that modest changes in the repayment plan structure improve welfare for both debtors and creditors. These findings challenge the practice of substantially adjusting repayment plans in debt restructuring programs in response to income changes.

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1 Introduction

The average ratio of household debt to disposable income among OECD countries increased from 85% in 2000 to 129% in 2021. This has triggered concerns about the growing problem of household over-indebtedness. In response, many governments have adopted frameworks to provide legal structures for personal bankruptcy and to offer social insurance to individuals overwhelmed by debt.[1] In the absence of these debt relief programs, over-indebted individuals would otherwise have to rely on creditors' goodwill or face prolonged financial distress.

Although some debt relief programs allow the immediate cancellation of debt after asset liquidation, there has been a global shift toward systems that require partial repayment over an extended period (Ramsay, 2017). This creates a fundamental trade-off. On the one hand, these programs should incorporate reasonably high repayments to creditors and minimize the moral hazard of debtors. On the other hand, they should allow debtors a smooth transition into a debt-free life. At the core of this trade-off are debtors' labor supply decisions, since repayment plans are typically tied to their income. Repayments may be adjusted based on changes in debtors' financial capacity, and thus function like a tax on income. These implicit taxes can be very high and may substantially distort labor supply incentives. Despite the widespread popularity of debt relief programs, little is known about the impact of adjustable repayment plans on debtors. This paper studies how these adjustments affect the labor supply incentives of debtors in the context of the Swedish debt restructuring program, which around 7 percent of the Swedish population will undergo at some point in their lives.[2]

I estimate the treatment effects of this program using the quasi-random assignment of case examiners to debt restructuring applications at the Swedish Enforcement Authority (SEA) and an examiner instrumental variable (IV) design. I show that traditional examiner IV estimates, based on leave-one-out estimates of examiner leniency, depend on the assumption that there is no path dependency in examiner decision-making. This assumption, however, is at odds with a large literature in behavioral economics and psychology (e.g., Chen et al., 2016; Jin et al., 2023). I derive the asymptotic bias under path-dependent examiner decision-making and provide empirical evidence for distorting path dependencies. To address this issue, I propose constructing the leniency instrument based on past cases only.

Using this instrument, I find that debtors experience negative effects on labor income and employment in the 7 years after acceptance to the debt restructuring program: Labor income falls by 6.3 percent and employment decreases by 12.2 percent relative to rejected applicants. These averages, however, mask large heterogeneity, which is most pronounced with respect

[1] The number of current EU countries with debt relief programs between 2000 and 2021 increased from 9 to 25. Russia introduced its personal bankruptcy law in 2015, and India significantly reformed its laws in 2016. China followed suit by introducing a personal bankruptcy regulation in the Shenzhen Special Economic Zone in 2020. In the US, where consumer debt relief has a longer history, almost 10% of households have filed for personal bankruptcy at some point (Stavins, 2000). See Appendix Figure A1 for the time series for debt relief programs and the household debt-to-disposable income ratio among OECD countries.

[2] The Swedish debt relief program is referred to as a debt restructuring program. I will use the terms interchangeably. Other commonly used terms are consumer or personal bankruptcy.

to the initial employment status of applicants. Initially employed participants experience an *increase* in income of 13.4 percent and are 1.3 percent more likely to be employed. In contrast, participants who are initially unemployed are 31 percent *less* likely to work and earn 26.9 percent less than rejected applicants. These disparities persist after the program has ended.

In Sweden, debtors can be granted debt restructuring if they are not able to repay their debts in the foreseeable future. Debts are only discharged after successfully completing a stringent 5-year repayment plan. Importantly, repayments can be adjusted upward following significant increases in debtors' income if creditors apply for reconsideration. Regular wage increases of already employed participants do not meet these criteria, according to SEA guidelines. This generates different incentives to supply labor for participants who are initially employed and those who are unemployed.

Acceptance into the debt restructuring program shields participants from wage garnishment, whereby a part of delinquent debtors' salaries is withheld to repay creditors. Wage garnishment functions like a tax on labor income and can therefore reduce incentives to work for *rejected* applicants. I show that the increase in income among initially employed participants is larger for those who are more likely to face wage garnishment as a counterfactual to debt restructuring. Initially unemployed applicants who are rejected are empirically substantially less likely to face wage garnishment, and average garnishment rates relative to income are lower than those of initially employed applicants.

The positive effects for initially employed participants can be explained by debt collection practices outside of the debt restructuring program. To explain the adverse effects on initially unemployed participants, I zoom in on the incentive structure within the program. Employed participants can benefit from regular wage increases without risking upward adjustments to their repayment plans. In contrast, unemployed participants who begin working risk substantial increases in their repayment obligations. This allows them to keep only a small portion of their additional income, which creates strong incentives to remain unemployed.

I provide evidence for this mechanism. First, I show that the different signs in treatment effects cannot be explained by the different debtor characteristics observable in the rich Swedish data. Second, I demonstrate that initially unemployed participants, but not initially employed participants, bunch below the adjustment thresholds. To further corroborate these findings, I conducted an online survey of debtors who started debt restructuring in 2023 and show that the *perceived* probability of facing upward adjustments after large increases in income is high. Nearly half of the respondents consider these rules deterministic and perceive the probability of adjustments as 100 percent, despite actual adjustment probabilities being relatively low. Together with responses to open-ended survey questions, this suggests that participants hold misinformed views about the institutional setting (Stantcheva, 2021).

I also provide evidence that the employment effects for initially unemployed participants are not due to employer discrimination based on negative credit reports (Bos et al., 2018; Dobbie et al., 2020). Instead, unemployed participants are less likely to report actively searching for employment and are more likely to drop out of the labor force entirely.

Several mechanisms can further explain the persistence of the effects, after the program ends. Human capital depreciation (Dinerstein et al., 2022) and psychological detachment can reduce incentives to seek employment. Furthermore, employers might discriminate against job seekers with long periods of unemployment (Kroft et al., 2013; Eriksson and Rooth, 2014).

To extrapolate from my empirical setting, I develop a simple model of dynamic labor supply to simulate policy changes in the structure of the repayment plan. In the model, debtors choose working hours and repay their debts through either wage garnishment or a repayment plan within the debt restructuring program. I calibrate the model to my empirical results using the simulated method of moments (SMM). Initially employed applicants increase their labor supply relative to rejected applicants who face wage garnishment. After exogenously setting initial employment to zero for accepted applicants, the high adjustment rates prevent re-employment. To generate persistent unemployment after the program ends, I incorporate human capital depreciation for unemployed individuals.

I show that the calibrated parameters of the model imply an uncompensated labor supply elasticity of 0.27 for marginal debtors, which is similar to what Kleven and Schultz (2014) find using a large tax reform in Denmark. I then simulate policy reforms by reducing the adjustment rate, which determines how much of the additional income exceeding the adjustment threshold is added to the repayment plan. A reduction from 100 percent to 85 percent is sufficient to induce initially unemployed participants to seek part-time employment. This translates into higher employment after the program ends. I further show that lower adjustment rates are unlikely to create additional labor supply moral hazard among applicants who might otherwise be incentivized to voluntarily reduce their initial labor supply to lower repayment obligations.

This paper contributes to the literature on household debt relief or personal insolvency programs, which generally finds positive effects on the income, mortality, and financial health of participants in the US (Dobbie and Song, 2015; Dobbie et al., 2017) and Denmark (Bruze et al., 2024). Both Dobbie and Song (2015) and Bruze et al. (2024) attribute the increases in labor supply to the protection from wage garnishment. Unlike in Sweden, the Danish program does not permit any upward adjustments to the repayment plan and, therefore, does not create adverse labor supply incentives. The US system, in which upward adjustments are possible after large increases in income, is generally more lenient in terms of eligibility. This results in a substantially lower *share* of participants being initially unemployed compared with their Swedish counterparts, thereby potentially masking this group's contribution to the average estimates.

Relative to these papers, which study the consequences of debt relief compared to remaining under financial distress, my findings highlight potential pitfalls in designing debt restructuring programs with adjustable repayment plans. A feature commonly found in many countries' programs (Kadner Graziano et al., 2019, see also the EU's recommendation for adjustable repayment plans in entrepreneurial and consumer insolvency proceedings[3]). I address this by focusing on two groups of debtors that are differentially exposed to the program's incentive structure and repayment obligations outside the program. This is particularly relevant, as Indarte (2023) finds that increasing the probability of debt forgiveness in the US has only limited effects on debtor moral hazard.[4]

Hamdi et al. (2024) report positive effects on the incomes of participants' children in the US. Fraisse (2017) shows that a two-year debt suspension through the French household debt restructuring framework results in a large, but only short-lived reduction in the probability of re-default. Exler (2019) estimates a structural model with endogenous labor supply, in which households can freely choose whether to default, based on the German bankruptcy system. He finds that in the optimal system, repayment adjustment rates would be 18 percentage points lower to preserve labor supply incentives, but the repayment period would increase from 6 to 10 years to decrease default probabilities and hence interest rates.[5]

I further contribute to the literature on examiner IV regressions. Kling (2006) pioneered this approach, which, in more recent applications, typically uses the leave-one-out mean of an examiner's decisions as an instrument for the decision on the current case.[6] I evaluate the bias of the leave-one-out IV regression under path dependency.[7] Intuitively, if the instrument is constructed from future decisions that are functions of current unobserved case characteristics, it will be correlated with the regression error term. This failure of strict exogeneity of individual decisions will then result in biased estimates. Empirically, in the context of debt restructuring, the treatment effect on income rises from -12.6 percent to -6.3 percent when using the past-cases-only estimator, whereas the treatment effect on employment rises from -18.1 percent to -12.2 percent. These changes correspond to 37 percent and 61 percent of the OLS bias, respectively. I discuss further evidence for the presence of distorting path dependencies.

Although this bias magnitude is specific to the context of debt restructuring examiners in Swe-

[3]Directive (EU) 2019/1023

[4]A related, but distinct, literature investigates the effects of debt relief through discretionary policies or randomized experiments and finds generally positive effects on consumption and financial health (see e.g. Agarwal et al., 2017; Di Maggio et al., 2020; Agarwal et al., 2023; Adelino et al., 2024; Aydın, 2024; Dinerstein et al., 2024; Gyöngyösi and Verner, 2024), although the effects can depend on the specific mode of debt relief (Dobbie and Song, 2020; Ganong and Noel, 2020). Kluender et al. (2024) find zero or even negative effects on financial and mental health among participants in an experiment who receive medical debt relief. Di Maggio et al. (2020) and Gyöngyösi and Verner (2024) find an increase in labor income for recipients of debt relief in the US and Hungary. They attribute this to the distortionary effects of high levels of debt on labor supply, for example through wage garnishment. However, Dobbie and Song (2020) do not find any significant effects on labor income among recipients of debt relief in a field experiment.

[5]See, e.g., Auclert et al. (2019), Gross et al. (2021), and Auclert and Mitman (2023) for recent work on the macroeconomic and general equilibrium effects of debt restructuring programs and Exler and Tertilt (2020) for a summary of this literature.

[6]For recent applications, see, among many others, Dahl et al. (2014) on intergenerational welfare cultures; Gross and Baron (2022) on foster care; Galasso and Schankerman (2015) on cumulative innovation; Grindaker et al. (2024) on firm bankruptcy; Dobbie et al. (2018) on pre-trial incarceration; Collinson et al. (2024) on evictions and homelessness; Humlum et al. (2024) on active labor market policies; and Bakx et al. (2020) on the health effects of nursing homes. See also Chyn et al. (2024) for a recent overview of this method.

[7]Leave-one-out IV regressions produce point estimates equivalent to those generated by jackknife IV regressions with examiner fixed effects as instruments.

den, the underlying issues are likely to arise in other settings that also use similar instruments. I perform simulations and explain when the biases resulting from path dependency are more likely to cause problems. I also show a large bias in a case study on inventor mobility following patent approval decisions made by patent examiners at the US Patent Office.

Frandesen et al. (2023b) and Chao et al. (2023) propose examiner IV estimators that are robust to clustered sampling. The issues that arise from path-dependent examiner decision making are conceptually different from those that arise from clustered sampling or common shocks, and therefore require different solutions. Specifically, by imposing a causal model of examiner decision making, I introduce asymmetry in the bias such that the timing of cases matters, and the instrument can be constructed from past cases. Kolesár (2013) shows that including many covariates in two-stage least squares (2SLS) estimations using leave-one-out instruments can result in biased estimates. I extend this framework to the setting of path-dependent decision making. Intuitively, exogenous past decisions of an examiner at one point in time can be endogenous future decisions of the same examiner at an earlier point. By including many controls, the instrument can be contaminated by these endogenous cases. I propose a leave-examiner-out residualization to address this concern.[8]

The remainder of the paper is structured as follows. I describe the institutional context of debt restructuring in Sweden in Section 2. In Section 3, I derive the asymptotic bias in examiner IV designs under path dependency and develop a robust past-cases-only leniency estimator. I describe the data and how I construct my estimation sample and discuss evidence for the validity of my instruments in Section 4. In Section 5, I report my main findings. In Section 6, I discuss evidence for the proposed mechanisms and simulate policy reforms using the calibrated labor supply model. Section 7 concludes.

2 Debt restructuring in Sweden

The debt restructuring system in Sweden was initiated in 1994 by the Debt Adjustment Act (*Skuldsaneringslagen*) with the objective of allowing participants economic rehabilitation (Lennander, 1991).[9] The relevant legal framework for this context is outlined in the 2006 version of the Debt Adjustment Act.[10] Overindebted applicants seeking debt restructuring can submit their application to the Swedish Enforcement Authority (SEA, *Kronofogden*), where a case examiner will decide on each application. Applications are free of charge and can easily be submitted online. The SEA is a government agency—originally established as a tax collection authority within the Ministry of Finance—that is responsible for debt collection, distraint, and evictions,

[8] Other recent work extends the examiner IV design to allow for multiple treatments (Humphries et al., 2023; Kamat et al., 2024).

[9] Before the introduction of household debt restructuring, personal and company bankruptcies were regulated by the same law and could result in court-ordered asset seizure. However, this procedure did not include any discharge of debt for debtors whose assets did not cover the full outstanding amount (Lennander, 1991). For a more detailed overview of the history of debt restructuring in Sweden, see Ramsay (2017).

[10] Skuldsaneringslag (2006:548).

among other things. Examiners within the SEA are normally, but not always, legal professionals with a university degree who have substantial discretion in decision-making (Larsson and Jacobsson, 2013).

In 2019, roughly 20,500 individuals applied for debt restructuring and 12,200 were accepted. This implies that about 0.15 percent of the Swedish adult population entered the program in that year. Extrapolating this annual entry rate over a lifetime suggests that approximately 7.5 percent of adults will experience debt restructuring at some point.[11]

Applications are evaluated based on two main criteria. First, applicants are expected to be unable to repay their debt in the foreseeable future, and second, debt restructuring is deemed reasonable given the debtor's personal and financial circumstances. This may, for example, imply that debtors should have made their best efforts in the past to repay their debt.

Delinquent debtors can be subject to a wage garnishment scheme administered by the SEA whereby part of their salary is withheld by their employer and remitted to creditors. Once accepted to the debt restructuring program, any wage garnishment on existing debt will be replaced by an individual repayment plan that typically lasts 5 years.[12] During these years, debtors are required to repay part of their debt to their creditors. The extent of repayment depends on the debtor's repayment capacity and a reserve amount that ensures they can maintain a subsistence level, considering personal circumstances such as the number of underage children in the household. Around 40 percent of participants do not have to repay anything according to their initial repayment plan. The remaining 60 percent of applicants must repay 26 percent of their debt on average.

The repayment plan can be adjusted if the financial circumstances of the participant change substantially and permanently due to unforeseeable events, such as when transitioning from unemployment to employment. Higher income through regular wage increases does not qualify as such events. Generally, these adjustments can occur if the debtor's monthly disposable income decreases by more than 53 USD for at least three consecutive months, or if it increases by more than 423–529 USD.[13] To initiate changes, either debtors or creditors must apply to the SEA, where the application will be reviewed. The caseworker responsible for this review will typically differ from the examiner who made the initial decision. The SEA does not inform creditors about improvements in debtors' financial circumstances. However, due to Sweden's extensive transparency laws, most relevant information is publicly available to creditors. For instance, information on debtors' income can be obtained from the Swedish Tax Agency or from credit reports issued by private providers.

If the repayment plan is upward adjusted, the debtor is typically allowed to keep any increases in income up to 423-529 USD. Any additional income above this threshold will be included in

[11] The calculation assumes a constant annual entry rate of 0.15 percent and computes lifetime incidence as $1 - (1-s)^{70-18}$, where s is the annual entry probability.

[12] Shorter repayment plans may be accepted for older or very sick applicants.

[13] The underlying amounts are 500, 4,000, and 5,000 SEK, respectively, based on the average 2019 exchange rate of 9.46 SEK/USD. Throughout the paper, monetary figures are converted from SEK and reported in 2019 USD.

the initial repayment plan. Adjustments of the repayment plan can be severe if the increase in the debtor's income is large. Consider, for example, a typical working-age debtor living alone who is unemployed in 2015, but secures a job in 2016. The average monthly disposable income for such individuals is 772 USD when unemployed and rises to 1,797 USD once employed.[14] This exceeds the reserve amount the SEA considers a subsistence minimum.[15] Debtors are allowed to keep 423-529 USD of the increase of 1,025 USD which implies an implicit tax rate between 47.9 percent and 58.3 percent.[16] After the repayment plan is fulfilled, the debtor is forgiven her unsecured debt except for obligations due to family law. See Section 6.4 for a comparison between the Swedish system and systems in the US and Denmark.

Applicants are assigned to one of the SEA's units based on their place of residence.[17] The assignment mechanism for applications to examiners is based on a register of all cases within a unit. Examiners with available capacity will be assigned the oldest of the remaining cases. This quasi-random allocation of cases to examiners within units allows me to use variation in examiners' leniency to provide causal estimates. There are two steps in the decision-making process. In a first, more bureaucratic decision, applications that do not fulfill basic criteria, for example, if the applicant's debt burden is too low given their age, will be rejected.[18] Remaining applicants will then be asked to submit documentation, after which their case will be thoroughly reviewed by the examiner who then makes a final decision. I will use data on these final decisions. Note that if the first decision was also based on the examiner's leniency, this could induce a correlation between case characteristics and my instrument. However, this is unlikely to be an issue given the standardized nature of the initial decision. In Section 4.2.1, I show that the instrument is uncorrelated with observable applicant characteristics, such as age or income, that are highly predictive of being accepted to the program. If initial decisions were a function of examiner leniency, we should find imbalance in the instrument.[19]

Another threat to identification would be if examiners directly influence debtors' outcomes, and thus violate the exclusion restriction. I do not expect this to be an issue in the given setting for two reasons. First, the SEA is supposed to function as a "neutral intermediary" between debtors and creditors and should not intervene to favor either side. For this reason, the SEA, for example, does not provide advice to debtors about the validity of claims against them

[14]The numbers are derived from the Longitudinal Integrated Database for Health Insurance and Labor Market Studies which covers the entire Swedish population.

[15]The basic reserve amount is 6,090 SEK for single households, but may be higher depending on costs for accommodation. Reserve amounts can easily be computed via an online calculator on the SEA's website.

[16]This already takes into account taxes and changes in social benefits. The corresponding average gross labor income is 1,876 USD.

[17]In my analysis, I focus on the units in Östersund, Gothenburg, Kalmar, Malmö, and Sundbyberg which account for 97 percent of applications in my sample period.

[18]Applicants can self-assess whether they meet the requirements for receiving debt settlement through an online test. For example, a 45-year-old single applicant earning 2,500 USD per month, who did not accumulate debt through gambling, has a good chance of being considered for debt restructuring with a debt burden of 75,000 USD, but not with a debt burden of 25,000 USD. Since applying is free of charge, some debtors might choose to apply anyway.

[19]If leniency were correlated with case characteristics, such that more lenient examiners deal with applicants who are relatively better off, we would expect the IV estimates for income and employment to be upward biased relative to the true effect.

(Ramsay, 2017). Second, although accepted applicants do receive advice on how to improve their situation as part of the debt restructuring program, this is administered by either the respective municipality or the Swedish Consumer Agency (*Konsumentverket*) and not the SEA. Typically, examiners do not have direct contact with applicants. In Section 4.2.3, I jointly test for the exclusion restriction and the monotonicity condition (Coulibaly et al., 2024) and do not find any evidence of violations.

3 Identification: examiner IV and path dependency

3.1 Path dependency in sequential decision-making

Examiner leniency designs rely on the assumption that decision makers will inherently differ in their propensity to make decisions of a certain kind. This is often summarized by a hypothetical exogenous leniency parameter denoted L_j, where j refers to the decision maker.[20] A simple decision model consistent with classical examiner IV can be described as follows:

$$\tilde{D}_{jt} = f^c\left(L_j, U_{jt}\right). \tag{1}$$

Here, t is a chronological index for the specific case, and U_{jt} summarizes all relevant case-specific information upon which the examiner bases their decision. Importantly, the examiner considers only contemporaneous case characteristics.[21]

This model, however, is at odds with a large and growing literature in behavioral economics and psychology that suggests that, in reality, there may be path dependencies in human sequential decision-making. The literature proposes several psychological mechanisms for this phenomenon. For example, assimilation bias refers to the tendency of individuals to interpret new information in a manner consistent with previous beliefs. Bindler and Hjalmarsson (2019) report evidence for path dependencies in jury decision-making in English high-stakes criminal court cases in the 18th and 19th centuries that is consistent with assimilation bias. Englich et al. (2006) show, that legal experts' decisions in a lab experiment were similarly influenced by random anchoring. Srinivasan (2023) finds that judges in the US hand down sentences that are 97 days longer in the 10 days following a murder sentencing. Contrast effects, on the other hand, can lead to negative autocorrelation. If a particular case is perceived as exceptionally heinous, the subsequent case may appear less severe by comparison. Chen et al. (2016) find negative autocorrelation in the decisions of judges granting or denying refugees

[20] For simplicity, I will assume time-constant leniency. All results below hold when allowing for time-varying leniency as long as the sources of variation are exogenous and there is some predictive power to induce correlation between different decisions of the same examiner.

[21] An example for a simple decision process is a threshold model in which the examiner accepts an applicant if $U_{jt} > L_j$. My results do not depend on this specific functional form and hold in settings in which examiners, for example, do not agree on a common ranking of applicants, and thus violates the monotonicity condition (see, e.g., Frandesen et al. (2023a) or Sigstad (2024)). This can create additional issues under treatment effect heterogeneity. See also Appendix Section A.4.

asylum. Bhuller and Henrik (2024) provide a different channel that potentially induces path dependency. They show that Norwegian judges presiding over criminal justice cases update their decision-making if previous decisions are reversed in appeal courts. If the probability of a decision being reversed depends on the characteristics of the respective cases, then such feedback-based learning can create path dependencies.

Evidence on path dependency in decision-making is not restricted to the judicial system. Other contexts in which a decision may depend on previous alternatives are, for example, physicians' treatment decisions (Jin et al., 2023); job interviews (Radbruch and Schiprowski, 2023); TV talent shows (Page and Page, 2010); the judgment of attractiveness (Kramer et al., 2013); and refereeing in sports (Dohmen and Sauermann, 2016).

Following this literature, I propose a more general examiner decision model that also depends on the history of past case characteristics $\mathbf{U}_j^{k<t}$,

$$\tilde{D}_{jt} = f^p\left(L_j, U_{jt}, \mathbf{U}_j^{k<t}\right). \tag{2}$$

In the following section, I will show that classical examiner IV can be inconsistent under such decision models.

3.2 Asymptotic bias in examiner IV

Let $\mathcal{J} = \{1, 2, \ldots, J\}$ be the set of examiners indexed by j. Each examiner sees T cases $\mathcal{T} = \{1, 2, \ldots, T\}$ ordered chronologically, such that a case is indexed by j, t.[22] The structural model is

$$Y_{jt} = \beta D_{jt} + \alpha U_{jt} + \nu_{jt}. \tag{3}$$

Here, β denotes the constant treatment effect of an examiner's decision on an outcome of interest Y_{jt}. $D_{jt} = \tilde{D}_{jt} - \mathbf{E}[\tilde{D}_{jt}]$ is the de-meaned decision, and ν_{jt} is an iid structural error that captures causes of Y_{jt} unrelated to examiners' decisions. In Appendix B I discuss extensions to models with covariates and heterogeneous treatment effects.

I treat Y_{jt}, D_{jt}, U_{jt}, and ν_{jt} as random variables while keeping J and T fixed. Let

$$\mathcal{P}_t = [t+1, t+P_t] \tag{4}$$

be the set of future periods during which case characteristics U_{jt} potentially have an influence on decision D_{jk}. $P_t > 1$ and $P_t < T - t - 1$ depends on the underlying examiner decision-making model.

I assume that

$$\mathbf{E}[D_{jt}\nu_{jk}] = 0 \ \text{ for all } k \in \mathcal{T}, \tag{A.1}$$

[22]Many empirical applications estimate leniency by examiner and year. In these settings, j refers to such an examiner-year combination.

$$\mathbf{E}[U_{jt}D_{jk}] = 0 \text{ for } k \notin \mathcal{P}_t \text{ and } k \neq t. \tag{A.2}$$

Assumption (A.1) asserts that the structural error is exogenous. Assumption (A.2) states that current case characteristics are uncorrelated with either past decisions made without knowledge about case t or decisions sufficiently far in the future. Following the examiner decision model (2), I allow for correlation between current case characteristics U_{jt} and the contemporaneous decision, as well as decisions within the next P_t periods such that generally

$$\mathbf{E}[U_{jt}D_{jk}] \neq 0 \text{ for } k \in \mathcal{P}_t \text{ or } k = t. \tag{5}$$

Let $\varepsilon_{jt} = \alpha U_{jt} + \nu_{jt}$ be the regression error. Since case characteristics U_{jt} are unobservable to the researcher, the endogeneity of current decisions to current and past cases (5) implies that a simple OLS regression of the outcome Y_{jt} on decision D_{jt} will not recover β.

To overcome this issue, many researchers measure examiner leniency by using the leave-one-out average decision of examiner j and use this leniency as an instrument for the decision D_{jt}.[23] Excluding the current decision eliminates the influence of the current case from the instrument.

$$Z_{jt}^{\text{loo}} = \frac{1}{T-1} \sum_{k \neq t} D_{jk}. \tag{6}$$

Under the random assignment of examiners to cases, and in the absence of path dependency in the decision-making process, this instrument will fulfill the independence assumption. To ensure the instrument's relevance, other cases of the same examiner must have predictive power for the current decision

$$\mathbf{E}[D_{jt}D_{jk}] \neq 0 \text{ for all } k \in \mathcal{T}. \tag{A.3}$$

Assumption (A.3) is sufficient, but stronger than what is strictly necessary in this setting. This allows me to construct relevant instruments from any selection of other decisions an examiner makes and follows from decision models with time constant leniency such as (2).[24] The corresponding 2SLS estimator is given by

$$\hat{\beta}_{\text{loo}} = \frac{\sum_j \sum_t Z_{jt}^{\text{loo}} Y_{jt}}{\sum_j \sum_t Z_{jt}^{\text{loo}} D_{jt}}. \tag{7}$$

This estimator is numerically equivalent to the jackknife IV estimator when using examiner fixed effects as instruments (Angrist et al., 1999). I follow the literature on many instruments (Bekker, 1994) and evaluate the asymptotic biases by letting the number of examiners J grow to infinity while keeping the number of cases per examiner T fixed. If we assume that $A_j \equiv \frac{1}{T}\sum_t \frac{1}{T-1}\sum_{k\neq t} D_{jk}Y_{jt}$ and $B_j \equiv \frac{1}{T}\sum_t \frac{1}{T-1}\sum_{k\neq t} D_{jk}D_{jt}$ are iid, then when $J \to \infty$

[23] A notable exception is Farre-Mensa et al. (2019), who estimate examiner leniency at the US Patent Office using only past cases.

[24] Since I abstract from treatment effect heterogeneity, I do not need to impose a monotonicity assumption here. See also Appendix B.1.2. In the empirical part below, I will provide evidence that monotonicity holds in the context of debt restructuring.

$$\hat{\beta}_{\text{loo}} \xrightarrow{p} \beta + \alpha \frac{\sum_t \sum_{k \in \mathcal{P}_t} \mathbf{E}\left[D_{jk} U_{jt}\right]}{\sum_t \sum_{k \neq t} \mathbf{E}\left[D_{jk} D_{jt}\right]}. \tag{8}$$

See Appendix B.2 for technical details. Given path dependency (5), the second term will differ from zero since future decisions are a function of current case characteristics. Given this violation of strict exogeneity for each decision, the leave-one-out estimator will be asymptotically biased.

Since the source of bias is the inclusion of future cases $t \in \mathcal{P}_t$, we can avoid this issue by constructing an alternative instrument from past observations only:[25]

$$Z_{jt}^{\text{past}} = \frac{1}{t-1} \sum_{k<t} D_{jk}. \tag{9}$$

The respective 2SLS estimator is given by

$$\hat{\beta}_{\text{past}} = \frac{\sum_j \sum_t Z_{jt}^{\text{past}} Y_{jt}}{\sum_j \sum_t Z_{jt}^{\text{past}} D_{jt}} = \frac{\sum_j \sum_t \frac{1}{t-1} \sum_{k<t} D_{jk} Y_{jt}}{\sum_j \sum_t \frac{1}{t-1} \sum_{k<t} D_{jk} D_{jt}}. \tag{10}$$

Letting J grow to infinity, we get that

$$\hat{\beta}_{\text{past}} \xrightarrow{p} \beta + \alpha \frac{\sum_t \sum_{k<t} \mathbf{E}\left[D_{jk} U_{jt}\right]}{\sum_t \sum_{k<t} \mathbf{E}\left[D_{jk} D_{jt}\right]} = \beta \tag{11}$$

where the last step follows from assumption (A.2). The past-cases-only estimator therefore recovers the true causal effect, even under path-dependent examiner decision-making.

3.2.1 Monte Carlo simulations

In Appendix B.3 I provide simulation results for a hypothetical examiner decision model that takes six past cases into account. The bias in the leave-one-out estimation increases if the importance of past cases for the current decision increases. For a fixed number of relevant past cases, the bias decreases in the number of cases per examiner. Perhaps surprisingly at first glance, the bias grows if the relevance of examiner leniency L_j for the current decision decreases or the relevance of current case characteristics U_{jt} increases. Both of these changes weaken the first stage, which inversely scales the bias.

3.2.2 Many-controls bias

In many empirical applications, the leniency instrument is only valid conditional on controls such as court-by-year fixed effects. Kolesár (2013) shows that standard leave-one-out estimators can be biased in settings with many controls. He proposes a leave-one-out residualized

[25]This instrument will only be defined for observations for which $t > 1$.

unbiased jackknife IV estimator (UJIVE). In Appendix B.1.1, I show that this issue is exacerbated under path-dependent decision-making. Intuitively, exogenous past decisions of an examiner at one point in time can be endogenous future decisions at an earlier time. By including many controls, the instrument can be contaminated by these endogenous cases. A simple solution is to use leave-examiner-out residualization.

3.2.3 Heterogeneity in treatment effects

The general interpretation of the results derived above does not depend on the assumption of homogeneous treatment effects. However, when we allow for heterogeneity in treatment effects, 2SLS estimates will identify a weighted average of treatment effects. Differences between the classical leave-one-out and the past-cases-only estimator can then arise due to biases from path dependency or differences in weighting schemes induced by different instruments. In Appendix B.1.2 I derive regression weights for both instruments. Generally, because the past-cases-only instrument naturally relies more heavily on early cases, weighting schemes can differ if an examiner's leniency changes over time.

4 Data and instrument validity

4.1 Data

Swedish Enforcement Authority. I use data on all applications to the debt restructuring program between 2010 and 2014 that passed the initial screening conducted by the SEA. The data include identifiers for both the applicant and the assigned examiner, the examiner's unit, the dates of the initial application and the decision, and an indicator of whether the application was accepted or rejected. Applicants, but not examiners, can be linked to the administrative data sources described below through the identifiers provided. For accepted applicants, I observe both the total level of debt and the repayment obligations specified in the initial repayment plan. I also observe whether an application for an adjustment of the repayment plan was made and if this application was accepted or rejected. However, I do not observe the resulting changes in the repayment plan. I drop applicants whose debt is written off. The average acceptance rate is 61 percent.

Labor market outcomes and demographics. My data on labor market outcomes and demographics come from three administrative sources. The longitudinal integrated database for health insurance and labor market studies (LISA) contains information on labor market income, years and field of education, gender, age, and other forms of income such as pension income for all adult residents in Sweden. I use data for the years 2005 to 2021. The Structure of Earnings Survey covers all public sector employees and a random sample of around half of the private sector working population in Sweden. It is conducted annually by the Swedish National Mediation Office every September. This survey contains information on the number of hours worked and full-time equivalent wage rates. Additionally, I use data on active job market participation from the Labor Force Survey, which covers a representative sample of the Swedish population. I adjust all monetary variables to 2019 SEK prices and convert them into USD using the average 2019 exchange rate of 9.46 SEK/USD, as reported by the Swedish Riksbank. In the main estimations, I maintain a balanced sample in which every applicant can be observed in LISA for all seven years following the initiation of the debt restructuring program. I further restrict the sample to applicants of working age throughout the observation period, defined as being at least 18 and at most 56 years old in the year prior to application. Main outcomes throughout the paper are annual labor income and employment status. An individual is defined as employed if they have any labor income greater than 0 in a given year. All results are robust to adjusting the employment threshold, for instance, to an annual labor income of 5,000 USD or 10,000 USD. I further infer purchases of cars by ownership changes in the Swedish Vehicle Register from 2011 onward.

Online survey of participants. To complement the results from the administrative data sources, I also collected data on the perceptions of repayment plan adjustment probabilities through an online survey of debt restructuring participants. I obtained a random sample of 2,000 debtors who were accepted to the program in 2023 from the SEA. Of these, 589 could be contacted and provided with a link to the online survey via SMS, and 92 completed the survey. Participation in the survey was incentivized by the possibility to enter a lottery for vouchers worth 250 SEK.[26] 51 percent of applicants stated that they were currently full-time employed, 8 percent that they are part-time employed, and 40 percent that they are not currently employed. Among unemployed respondents, 41 percent stated that they are retired.

Applicant characteristics. Appendix Table A1 shows average characteristics in the year before application for all applicants and split by acceptance status. The average applicant is 44.2 years old, has less than 11 years of education, and has an annual labor income of 12,975 USD; this places them in the 23rd percentile of their birth cohort income ranking. Accepted applicants are less likely to be employed than rejected applicants (0.53 vs. 0.69), earn less (11,203 USD vs. 15,697 USD), and are more likely to be female (0.54 vs. 0.43). The average amount of debt applicable for discharge is 80,271 USD for accepted applicants, which is 7.2 times the average income of accepted applicants. On average, they repay 11,090 USD or 14.6 percent of their debt according to their initial repayment plan. 39.7 percent of accepted applicants do not have to repay anything according to their initial repayment plan.[27]

[26]The survey was fully anonymous and responses cannot be linked to the administrative data; respondents could voluntarily provide an email address only if they wished to participate in the voucher lottery.

[27]See Appendix Figure A2 for the distribution of the repayment rate.

4.2 Instrument validity

My main instrument will be the past-cases-only IV.[28] However, to compare this approach with the traditional examiner IV design, I further construct an instrument based on the leave-one-out mean of all of the examiner's other decisions. I restrict the sample to cases decided in the SEA's five main units, which together account for 97 percent of all applications in my sample period.[29] To reduce noise, I drop all observations that are estimated using 10 or fewer cases. To account for potential selection across units within the SEA, I first residualize each decision using the average acceptance rate of *other* examiners in the same unit and calendar year.[30] Unlike a standard fixed-effects approach, this procedure avoids the many-controls bias discussed in Section 3.2.2. I then construct the instruments as the average of past residuals and the leave-one-out average of residuals within examiner. My final sample contains 177 examiners. The leave-one-out instrument for the average (median) observation is estimated using 267 (234) other cases. The past-cases-only instrument for the average (median) observation is estimated using 107 (81) cases. In Appendix Table A2, I compare characteristics of compliers induced by the past-cases-only instrument to characteristics of all applicants as well as accepted applicants. Compliers are slightly older, have lower incomes, and are more likely to have children or a spouse than the overall sample of applicants. However, these differences are relatively small. For further details on how these values were constructed, see Appendix C.

4.2.1 Independence assumption

The independence assumption requires the instrument to be uncorrelated with the unobservable case characteristics the examiner bases her decision on. Failures of this assumption can arise from non-random allocation of examiners to cases. Figure 1a plots the coefficients of OLS regressions of the past-cases-only instrument on indicator variables for applicant characteristics in the year prior to application. These characteristics are highly predictive of the examiner's decision on the respective case with a joint F-Statistic of 99 and a p-value of 0. The coefficients with regard to the instrument are all small and individually and jointly insignificant, with a joint test p-value of 0.766.[31] From section 5.2 onward, I report most results split by initial employment status. The p-values for the joint tests within these sub-samples (excluding initial income as a balance variable) are 0.791 for initially employed debtors and 0.846 for initially unemployed debtors.[32] These results support the validity of the instrument.

[28] Since cases can overlap, I define past cases as all cases that are older than 30 days. My results are robust to using thresholds that are more restrictive.

[29] These units are located in Östersund, Gothenburg, Kalmar, Malmö, and Sundbyberg.

[30] Generally, for a discrete control X_{jt}, $\hat{D}_{jt} = D_{jt} - \frac{1}{N_X - N_{X,j}} \sum_{x=X_{jt}, i \neq j} D_{it}$. My results are robust to residualizing by unit and week.

[31] I follow the recommendation of Chyn et al. (2024) and do not cluster standard errors at the examiner level. Since a small share of around 6 percent of applicants apply more than once I cluster at the applicant-level.

[32] I drop indicators for high and low income from the regressions for this sample split, since they are highly collinear with initial employment status. This does not affect the results.

4.2.2 Instrument relevance

To identify treatment effects, the examiner leniency instrument must be sufficiently strongly correlated with the decision to accept an applicant to debt restructuring. Figure 1b plots the distribution of the past-cases-only instrument, residualized by year-times-office using a leave-examiner-out residualization approach. There is substantial variation in examiner leniency. To formally test the instrument relevance, I estimate the first stage by regressing the decision dummy on the instrument. Figure 1b plots this first-stage relationship, with a slope of 0.506 and a Kleibergen and Paap (2006) F-statistic of 640.5.[33] Again, splitting by initial employment status, the first stage F-statistic is 257.9 for initially employed debtors and 178.7 for initially unemployed debtors. I conclude that the instrument is strong.

4.2.3 Monotonicity condition and exclusion restriction

With treatment effect heterogeneity, we need to impose a monotonicity assumption to estimate convex combinations of treatment effects. A monotonicity assumption, such as the one stated in Appendix B.1.2, in which treatment effects are a function of the case characteristics, requires that the first stage is positive for all (or negative for all) values of case characteristics. I follow Bhuller et al. (2020) and test this assumption. I split the sample by gender, age, education, and employment, and re-estimate the first stage within each of these sub-samples. If the monotonicity assumption holds, the first stage coefficient should be positive in both subgroups. Appendix Table A4 shows results for this test using the past-cases-only instrument. As required, all coefficients are positive. To estimate marginal treatment effects (Heckman and Vytlacil, 2005) we need to impose a stricter monotonicity condition according to which treatment probability (the propensity score) must be strictly increasing in leniency. This assumption and the exclusion restriction jointly impose testable conditions. I use the sharp test developed by Coulibaly et al. (2024), using average annual income and average employment over the 7 years after the program started as outcomes. In both cases, the test does not reject the null hypothesis that the testable conditions hold with p-values of 0.997 and 0.999, respectively (see Appendix Table A5). As in Bhuller et al. (2020), I conduct an additional test. Leniency estimates for examiners from one subsample should be positively correlated with the probability of being accepted by the same examiner in another subgroup. Appendix Table A6 tests this

[33] In the single-instrument case, this is equivalent to the robust Montiel Olega and Pflueger (2013) F-statistic (Andrews et al., 2019). I follow Bhuller et al. (2020) who argue that the leniency IV should be treated as a single instrument (the instrument is the assigned leniency). Hull (2017) views the assignment of an individual examiner as instrument, implying that the F-statistic should take the number of examiners into account (although it is not clear how the latter would translate into the setting in which leniency is estimated from past cases). Angrist and Kolesár (2024) generally advise against selecting just-identified instruments based on the first stage F-statistic. In Appendix Table A3, I report Anderson-Rubin confidence intervals for my main estimations, which are robust to weak instruments (Anderson and Rubin, 1947; Andrews et al., 2019). They are almost identical to the standard Wald confidence intervals. If weak IV bias were an issue in the given setting, we would expect it to be stronger for the less precisely estimated past-cases-only instrument. This would shift the respective estimate toward the OLS estimate relative to the stronger leave-one-out estimate (Bound et al., 1995). However, Table 1 shows the opposite trend.

using the past-cases-only instrument. I split the sample by the same demographics as above, then estimate leniency for each examiner in one of the splits, and regress the examiner's decision for all cases with applicants in the other sample on the leniency instrument obtained from the first sample. Again, all regressions show positive correlations.

5 Empirical results

5.1 The impact of debt restructuring on income and employment

Table 1 reports the main results on average income and average employment for the 7 years following the start of the program. Columns 1 and 4 present results for the respective OLS estimates. Accepted applicants earn on average 4,290 USD less per year and are on average 15.4 percentage points less likely to be employed after the program started. Columns 3 and 6 report the 2SLS estimation results using the past-cases-only instrument. Both estimates drop considerably compared with the OLS estimation, which suggests negative selection into the program. Accepted applicants earn 1,250 USD, or 6.3 percent,[34] less per year than rejected applicants, but the estimate is insignificant.[35] Employment is 8.9 percentage points (12.2 percent) lower as a direct effect of being accepted into the program.[36]

5.1.1 Differences between leave-one-out and past-cases-only estimates

I report estimates for the leave-one-out instrument in columns 2 and 5. The estimate for income is 2,360 USD and -12.9 percentage points for employment, which places the two estimates between those of the OLS and the past-cases-only regressions. Two considerations are important when comparing the two instruments. First, since both instruments are estimated from similar samples, they are highly correlated with each other. Using the standard errors of each regression to gauge the variation of their difference, therefore, will overestimate the respective standard error. Appendix Figure A3 plots bootstrap distributions of both estimators as well as their difference using a Bayesian or exchangeably weighted bootstrap (Rubin, 1989; Praestgaard and Wellner, 1993). The two panels show that both distributions of the differences $\hat{\beta}_{\text{past}} - \hat{\beta}_{\text{loo}}$ have substantial mass above zero. The respective 95 percent confidence intervals are $[-547.09\ ;\ 2,761.27]$ for income and $[-0.009\ ;\ 0.083]$ for employment. Second, both instruments - and therefore their difference - are related to the size of the OLS bias. In the structural model (3) this bias is scaled by the influence of unobserved case characteristics U_{jt} on the outcome Y_{jt}, denoted by α. If α equals zero, then both the OLS and the 2SLS regressions will in expectation yield the same estimate. To separate the question of how much the instruments

[34] Relative effects for all 2SLS regressions are measured relative to the complier mean, which I obtain by estimating the respective 2SLS regression using the outcome $(D-1) * Y$, see Abadie (2002).

[35] I do not winsorize my income variable. All results are robust to winsorizing annual income at the 95$^{\text{th}}$ percentile.

[36] In Appendix Figure A6, I plot the raw means of the income and employment of accepted and rejected applicants.

differ from the question of how large the omitted variable bias in the OLS regression is, I scale the differences by the OLS bias. For this, I take the estimate of the past-cases-only IV as the ground truth and report the size of the leave-one-out IV bias compared with the OLS bias as $(|\hat{\beta}_{\text{past}} - \hat{\beta}_{\text{loo}}|)/(|\hat{\beta}_{\text{past}} - \hat{\beta}_{\text{OLS}}|)$. For income, I find a substantial difference of 37 percent compared with the OLS bias. For employment, the relative difference is 61 percent. In Appendix Figure A4, I report the difference between past-cases-only and leave-one-out estimates across 384 alternative specifications of the instruments (see also Section 5.1.2). The differences are positive for both income and employment in all specifications, and in 71 percent (income) and 62 percent (employment) of cases they are larger than the corresponding differences in my main specification.

Another potential reason the two IV estimates might differ is differential weighting of underlying heterogeneous treatment effects. I estimate marginal treatment effects using a local IV approach (Heckman and Vytlacil, 2005; Brinch et al., 2017) and report estimates of the local average treatment effect (LATE) and the average treatment effect (ATE) in Appendix Table A7. As expected, the ATE estimates are less precise, since the associated weights are not chosen to maximize efficiency. For income, the difference between ATEs based on the past-cases and leave-one-out instruments is slightly smaller than the corresponding difference between LATEs. For employment, the pattern is reversed. This suggests that the estimated bias of the leave-one-out estimation measured against the past-cases-only estimation is not driven by the underlying weighting scheme.

In Appendix Section B.4, I provide empirical evidence that strongly suggests that the differences between the leave-one-out estimation and the past-cases-only estimation are driven by path-dependent examiner decision-making. I further show that these patterns are not unique to the setting of debt restructuring in Sweden and report a bias of 113 percent relative to the OLS bias in leave-one-out IV estimates on inventor mobility after patent approval at the US Patent Office.

5.1.2 Robustness

In Appendix Table A8, I examine the robustness of the main results to alternative outcome definitions. Columns 2 and 3 winsorize income at the 99th and 95th percentiles, respectively. Columns 5 and 6 raise the annual income threshold used to define employment from 0 USD to 5,000 USD and 10,000 USD, respectively. The estimates remain very robust to these variations. Appendix Tables A9 and A10 present further robustness checks for the past-cases-only estimates of income and employment, respectively. Column 1 reports the baseline specification, which uses a leave-examiner-out residualization by unit-by-year and is restricted to observations where the instrument is based on at least ten past cases decided 30 or more days before the previous case. Column 2 controls for unit-by-week-of-application instead of unit-by-year. Columns 3 and 4 add controls for applicant characteristics, implemented by residualizing decisions with respect to year, unit, and the relevant characteristic – using the same leave-examiner-out residualization approach as in the baseline regression – before aggregating to the examiner level, in order to mitigate the many-controls bias discussed in Section 3.2.2. Column 3 includes unit-by-year-by-region-of-residence fixed effects, as geographical residence may affect applicant assignment across units. Column 4 includes unit-by-year-by-propensity-score-bin fixed effects, where the propensity score is estimated from observed applicant characteristics.[37] Columns 5-7 restrict the sample to observations for which the instrument is constructed from at least 25, 75, and 125 cases, respectively. In column 8, I report estimates from instruments in which past cases are defined as those that have been decided 60 or more days before. My main results are robust to all of these changes, with point estimates for income negative and insignificant and point estimates for employment significant at around -10 percentage points or slightly larger in magnitude. See also Appendix Figure A5, where I further explore interactions among these and additional 2SLS specifications, and compare them to different OLS estimates (both cross-sectional and exploiting the panel structure of the data). I conduct this analysis separately for initially employed and initially unemployed applicants, as this is the policy-relevant heterogeneity as emphasized in Section 6.

5.2 Heterogeneity

To explore heterogeneity in treatment effects, I split the sample by applicant characteristics and present the 2SLS estimates for income in Figure 2a and for employment in Figure 2b. There are no large differences by education or family composition (children, cohabitation).[38]

Although individual estimates are somewhat imprecise, younger, male, and initially employed applicants experience increases in labor income with no detectable effects on employment. By contrast, older, female, and initially unemployed applicants experience income losses driven by large negative effects on the extensive margin of employment.

This switch in sign for some applicants can also be seen when estimating marginal treatment effects (MTEs) for each level of unobserved resistance to treatment (Heckman and Vytlacil, 2005; Brinch et al., 2017). I plot MTEs for income and employment in Figures 2c and 2d. Resistance to treatment maps one-to-one into the examiner leniency that a marginal applicant faces. Marginal applicants with low resistance to treatment are those who face the most lenient examiners and marginal applicants with high resistance are those that face the most strict examiners. Treatment effects for marginal applicants are negative for the most lenient examiners and increase with the strictness of the examiner. This is in line with previous results split by initial employment. Since applicants are evaluated based on how likely it is that they will be

[37]To construct propensity scores, I randomly assign each examiner to one of two subsamples. Within each subsample, I estimate scores via a logit regression of the individual decision on second-degree polynomials of age and years of education; a full interaction of gender, cohabiting spouse, and presence of a child under 18; year dummies; and five income-category dummies. All characteristics are measured in the year prior to application. Scores are then predicted out of sample (using the other subsample's estimates) and grouped into 15 equally sized bins.

[38]The median applicant has 11 years of education, is 46 years old, and has an annual income of 4,191 USD.

able to repay their debt, applicants with lower income are more likely to be accepted into the debt restructuring program. The marginal applicant evaluated by a lenient examiner will, on average, have a lower income than the marginal applicant evaluated by a strict examiner.

This has implications for the overall effect of policy reforms that expand the debt restructuring program when abstracting from potential changes in the pool of applicants. We can view such an expansion as an increase in average examiner leniency. However, the overall effect will depend on where in the leniency distribution these changes occur. Examiners who are relatively lenient to begin with, will accept further applicants with negative treatment effects. Examiners who are stricter will accept additional applicants whose incomes will increase as a result of undergoing debt restructuring.

In Section 6, I will argue that the structure of the repayment plan can create different incentives to supply labor depending on an applicant's initial employment status. These incentives can give rise to treatment effects with opposite signs for employed and unemployed debtors. In Appendix Figure A7 I show that, indeed, most of the heterogeneity displayed in Figures 2a and 2b can be explained by initial employment status. I will therefore split the sample by initial employment status in the following sections.

5.3 Dynamic effects during and after the program

The outcomes of my main estimates refer to the 7 years after the start of the debt restructuring program. Figures 3a and 3b plot the dynamic effects on income and employment for each individual year. Initially employed applicants experience an average increase of 3,025.6 USD (10.4 percent) during their participation in the debt restructuring program and an increase of 5,397.3 USD (17.6 percent) after the program ends, relative to rejected applicants.[39] The estimates on employment are 2.3 percentage points (2.5 percent) during the program and 1.7 percentage points (1.8 percent) afterward. The income of initially unemployed participants is 2,496.7 USD (39.9 percent) lower than that of rejected applicants during the program and 3,443.6 USD (37.3 percent) lower after the program ends. The respective estimates for employment are -14.8 percentage points (38.2 percent) and -11.5 percentage points (27.6 percent). Although there is a slight recovery in employment – but not in income – for initially unemployed participants relative to rejected applicants after the program ends, the gap in employment remains sizable.

The precision of the IV estimations does not allow me to go further in time than 7 years. In Figure 4, however, I provide graphical evidence indicating that divergences between accepted and rejected applicants persist up to 9 years after the program started. I plot event-study graphs that compare accepted applicants with a matched group of rejected applicants for initially employed and unemployed applicants.[40] The event studies suggest that the income and em-

[39]Since I only have information on total annual income, I define initial employment as earning more than 19,000 USD year to exclude applicants who were not employed throughout the whole year. The average seven-year effects for participants with positive initial income below 19,000 USD are -2,720 USD (-14.1 percent) on income and -0.086 (-10.1 percent) on employment, respectively.

[40]I perform the matching by estimating propensity scores using a logit model and second-order polynomials of

ployment of initially employed participants increase directly after the start of the program and remain at an elevated level relative to the control group. For initially unemployed participants, employment drops with the beginning of the program while income steadily declines. After the program ends, both employment and income increase slightly, but remain at substantially lower levels.

In Appendix Figure A5, I compare several DiD specifications to a wide range of 2SLS specifications. The figure shows two main patterns. First, there is a consistent switch in the sign of treatment effects between initially employed and initially unemployed participants. Second, although the DiD estimates are smaller in magnitude than those in my main 2SLS specification, they fall within the range of plausible 2SLS estimates. I therefore conclude that the event-study estimates presented in Figure 4 provide useful corroborating evidence.[41] See also Appendix Figure A6 for raw average outcomes for both groups.

To investigate the debt restructuring program's effect on consumption dynamics, in Figure 5 I plot estimates on the probability of buying a car during and after the program for initially employed and unemployed participants.[42] For initially unemployed participants, the estimates are insignificant both during and after the program. Likewise, initially employed participants show no significant difference from rejected applicants in car purchase probability while in the program. In the two years after the program ends, however, car purchases among the initially employed increase substantially by 19.4 and 13 percentage points, which corresponds to 106 and 146 percent relative to the complier mean. This may indicate generally higher consumption due to debt repayments dropping to zero post-program, or it may reflect a relaxation of borrowing constraints. In either case, these results suggest that initially employed participants experience an improved financial situation, whereas initially unemployed participants do not.

age and years of education in the year prior to application, a fully interacted set of dummies for gender, having a spouse, and having any children below the age of 18 in the same household, and three bins of income two years before application. I then predict propensity scores and generate 20 discrete bins. Within each bin and application year, I keep the same number of accepted and rejected applicants. The event study is a regression of the outcome on event-time dummies interacted with a binary treatment indicator and event-time and applicant fixed effects. By matching on year of application and using event-time fixed effects, I circumvent issues due to treatment effect heterogeneity (Borusyak et al., 2024).

[41] Note, however, that conditioning on pre-treatment outcomes can induce bias in panel data models (Chabé-Ferret, 2017).

[42] Since I can only observe changes in the car register from 2011 onward, I restrict the sample accordingly and show only one year before application to the debt restructuring program.

6 Explaining the divergent effects of debt restructuring

In the following section, I will discuss and provide evidence for mechanisms that explain the discrepancy in outcomes for initially employed and unemployed participants. While acceptance to the program protects debtors from other debt collection practices, the program itself can impose a strong incentive structure on participants.

Initially Employed Participants. By being accepted into the debt restructuring program, initially employed applicants avoid wage garnishment.[43] Since the amount of wage garnishment is regularly adjusted based on changes in the debtor's income, it effectively functions like a tax on labor income and can, therefore, reduce incentives to work.[44] Furthermore, full-time employed participants are unlikely to face adjustments to their repayment plan since they are generally permitted to earn 423-529 USD (or 4,000-5,000 SEK) more per month, and higher incomes resulting from normal wage increases are not considered improvements in debtors' financial circumstances by the SEA. If repayment due to the debt restructuring plan is considered to be independent of labor income, an additional income effect will further increase labor supply. Below, I will show that the relative increase in income is higher for employed participants for whom high wage garnishment is a plausible counterfactual.

Initially Unemployed Participants. Unemployed participants, on the other hand, can typically only increase their labor income discretely, since most jobs require them to work a minimum amount of hours.[45] By finding a job, a participant will, therefore, likely experience an increase in income above the adjustment threshold. Since the SEA guidelines specifically state that finding new employment is a reason for upward adjustments, this may deter unemployed participants from seeking employment. For incomes above the threshold, participants can face an effective marginal adjustment rate of 100 percent. I provide three pieces of evidence for this mechanism. First, using evidence from an online survey I conducted among debt restructuring participants, I show that the perceived probability of facing an upward adjustment after a large increase in income is high. Second, the switch in signs of treatment effects by initial employment status cannot be explained by observable participant characteristics. And finally, I demonstrate that initially unemployed - but not initially employed - participants bunch below the adjustment threshold. Additionally, I show that employer discrimination is very unlikely to be a potentially alternative mechanism driving my results.

[43] See Appendix Figure A8 for the shares of accepted and rejected applicants subject to wage garnishment.

[44] This argument is also made by Dobbie and Song (2015) and Bruze et al. (2024). Dobbie and Song (2015) shows that their results vary by the strictness of state wage garnishment laws, but neither paper presents evidence based on individual-level wage garnishment data.

[45] Appendix Figure A9 Panel (a) shows the distribution of weekly contracted hours from the Structure of Earnings Survey. Most employees work full time at 40 hours per week. Very few workers work less than 20 hours. Panel (b) restricts the sample to employees who did not have any employment in the previous year. The share of employees working less than 20 hours is slightly larger than for the overall population, but still very small.

6.1 Wage garnishment for initially employed rejected applicants

I first provide evidence that wage garnishment is a relevant counterfactual for those participants who start the program with employment. Table 2 shows the effects of starting the debt restructuring program on wage garnishment and the average garnishment rate relative to income, conditional on employment. The probability of facing wage garnishment falls by 27.1 percentage points for initially employed participants, whereas it only falls by an insignificant 3.1 percentage points for initially unemployed participants. The implied average garnishment rate for those who work is 7.1 percent for initially employed participants and an insignificant 0.2 percent for initially unemployed participants.

To provide evidence that the effects on income increase with the likelihood of avoiding wage garnishment, I predict the probability that an applicant would have faced wage garnishment if they had been rejected for all applicants. I randomly split the sample of initially employed applicants, using the first subsample to predict the probability of being subject to wage garnishment for the second subsample and vise versa.[46] I then split the full sample again into a group with a below-median and an above-median predicted wage garnishment probability. Table 3 reports the 2SLS estimates on the probability of being subject to wage garnishment and income for the two subsamples of initially employed applicants. Columns 1 and 3 show that participants with higher predicted wage garnishment indeed experience a 24.6 percentage point larger drop in their wage garnishment share compared with rejected applicants. Columns 2 and 4 show that the increase in income is substantially larger and significant for the sample with high predicted garnishment in both absolute and relative terms. This provides evidence that the counterfactual does shape results for initially employed applicants.[47]

6.2 Adjustable repayment plans for initially unemployed participants

Perceptions of adjustments. Since creditors must apply for reconsideration of the repayment plan, upward adjustments do not follow a deterministic process.[48] However, the possibility of upward adjustments is made very salient to debtors, both on the SEA website and through communication with SEA staff. A key determinant of the behavioral responses of participants is therefore the perceived probability that adjustments will occur. To obtain this parameter, I conducted an online survey among debt restructuring participants (see Section 4.1 for details). Figure 6 plots the distribution of perceived adjustment probabilities among respondents to the

[46] To predict wage garnishment, I use an indicator for having been registered with the SEA by creditors in the 5 years preceding the application and an indicator for facing wage garnishment prior to the application. Most rejected applicants experience wage garnishment only after applying for debt restructuring. For this reason, I also include applicant demographics, which may be correlated with both the likelihood of delinquency and the type of debt, and potentially influence the likelihood of creditors applying for wage garnishment.

[47] Due to the lower level of wage garnishment in the initially unemployed group, the prediction exercise does not yield different rates of actual wage garnishment. I report corresponding results in Appendix Table A11.

[48] Due to Sweden's extensive transparency laws, most relevant information is publicly available to creditors. For instance, information on debtors' income can be obtained from the Swedish Tax Agency or from credit reports issued by private providers.

survey;[49] 44 percent indicate a 100 percent probability of upward adjustment following a large increase in income. The average among respondents is 77 percent. Interestingly, the *observed* adjustment probabilities are relatively low (see panel (b) in Appendix Figure A10). This may suggest that debtors severely overestimate the actual adjustment probabilities. However, it is important to note that very few unemployed participants find employment (see panel (a)). Those who do may be aware that their creditors will not apply for adjustments - for example, due to preexisting agreements between them. Adjustment probability estimates from this selected sample can, therefore, not be interpreted as causal effects. Interestingly, several respondents mentioned in the comments that they believe the SEA monitors their income and independently decides on adjustments, reflecting a misunderstanding of the actual process, which depends on creditor applications.[50]

Reweighting by applicant characteristics. The estimated effects for initially employed and unemployed participants to the debt restructuring program align very well with the respective incentive structure imposed on both groups. However, differences in estimates could also arise due to inherent differences between initially employed and unemployed debtors. I provide evidence against this argument by re-weighting the sample of initially employed debtors to match the distribution of observable characteristics of initially unemployed debtors.[51] Table 4 shows the respective estimates. Columns 1 and 2 plot coefficients in the unweighted samples of initially unemployed and initially employed debtors. To facilitate comparison, I only keep observations with overlap in the respective applicant characteristics distributions, which decreases the precision of the estimates. Column 3 reports coefficients for the sample of initially employed debtors re-weighted to match the distribution of applicant characteristics among initially unemployed debtors. For both outcomes, the re-weighted point estimates are similar to the respective unweighted estimates. In Appendix Table A12, I complement this analysis using static DiD estimates, similar to the dynamic estimates reported in Section 5.3. This approach allows me to construct 845 narrowly defined comparison groups to generate the weights.[52] The resulting estimates are precise and closely resemble the 2SLS estimates. For both income and employment, the reweighted sample of initially employed applicants exhibits even slightly larger treatment effects relative to the unweighted sample. This suggests that differences in estimates are unlikely to be driven by observable characteristics of applicants.

Bunching. In Figure 7, I show that initially unemployed, but not initially employed, participants bunch below the adjustment threshold of 423 USD per month (48,000 SEK annually).

[49] Unemployed or part-time employed respondents were asked about the likelihood of an upward adjustment occurring after finding (full-time) employment. Full-time employed respondents were asked about the adjustment probability after a substantial increase in financial capacity.

[50] E.g. “The SEA sees if your income goes up.” or “The higher your income, the more you pay to the SEA.”

[51] Specifically, I generate 4 bins of initial age, 3 bins of initial years of education, 5 bins of income 4 years before applying (by construction, there is no overlap in income in the year before applying), binary indicators for gender, having children, and having a spouse. I then take the interaction of these bins, drop all cells without overlap, and generate weights for initially employed to match the distribution of initially unemployed.

[52] Specifically, I create 10 bins of initial age, 5 bins of initial years of education, and 5 bins of income two years prior to application (instead of four years), as well as binary indicators for gender, number of children, and having a spouse.

Specifically, I create 5 bins of change in nominal (not CPI-adjusted) annual income compared with the year before applying. Then for each bin I plot the difference in shares of accepted and rejected applicants within each bin, splitting the sample by initial employment status.[53] Since acceptance to the debt restructuring program is not random, I reweight rejected applicants based on applicant characteristics to match the distribution of these characteristics among accepted applicants. The figure shows two things. Initially employed participants do not appear to bunch below the adjustment threshold.[54] However, this threshold does seem to matter for initially unemployed participants, who are more likely to remain below the adjustment threshold than rejected applicants.

Employer discrimination. An alternative explanation for the negative employment effects for initially unemployed debtors could be that potential employers discriminate against job seekers based on credit report flags (Bos et al., 2018; Dobbie et al., 2020). After being accepted to the debt restructuring program, debtors receive a flag in their credit reports indicating their participation. This flag is removed after the program ends. Dobbie et al. (2020) find that the removal of credit information in the US led to a decrease in employment by at most 0.4 percentage points. They explain this finding by showing that bad credit reports contain little additional information about job seekers. Bos et al. (2018) find that the removal of default information for Swedish borrowers defaulting on pawn shop loans reduced employment by approximately 3 percentage points among both employed and unemployed individuals. This effect size is relatively small compared with the employment effect of -13 percentage points I find for initially unemployed participants in the debt restructuring program.[55] Furthermore, bad credit reports likely affect both accepted and rejected applicants in my sample. Around 90 percent of rejected applicants who are initially unemployed face a debt collection case initiated by the SEA in the 5 years after application.

I provide two additional pieces of evidence against this channel. In Table 5 I use data from the Labor Force Survey, which contains information on the job search status of respondents. Due to the limited overlap between the survey and my sample, I report estimates for both a balanced and an unbalanced sample.[56] Columns 1 and 3 report estimates for the average probability of being out of the labor force and not searching for employment. Columns 2 and 4 report results for being unemployed and actively searching for a job. While the results are somewhat noisy, I find an insignificant increase of dropping out of the labor force of 14.6 percentage points and a significant reduction in job search of 28.9 percent for the unbalanced sample and similar point estimates for the balanced sample. This is consistent with the mechanism whereby participants

[53]The first bin includes all cases in which there is a decline in income. This bin is mechanically empty for initially unemployed participants who do not have any income in the year before application.

[54]They are less likely to experience a decrease in income compared with rejected applicants. This is likely driven by wage garnishment.

[55]Note, however, that payment defaults and debt restructuring result in different credit report flags.

[56]I also include applicants with positive but small annual labor income below 19,000 USD. These applicants may be part-time employed or not employed throughout the entire year. The average initial income of these applicants is 5,800 USD. The estimated treatment effects for this group are -2,720 USD (-14.1 percent) on income and -0.086 (-10.1 percent) on employment, respectively.

avoid job search due to high adjustment probabilities rather than facing discrimination from employers. Finally, I use the introduction of the General Data Protection Regulation (GDPR) in 2018, which limited employers' legal access to credit report information. Post-reform, employers can only access credit reports if they are directly relevant to the position affecting, for example, roles in higher financial management but not typical jobs sought by initially unemployed debtors in my sample, such as positions as waiters or hairdressers. In Appendix Figure A14, I plot average employment rates for four cohorts of rejected and accepted initially unemployed applicants who applied for debt restructuring from 2015 to 2018.[57] None of these cohorts appear visibly affected by the introduction of the GDPR in 2018, which would be expected if employer discrimination played an important role in explaining the employment gap between accepted and rejected applicants.

6.3 A model of labor supply under debt restructuring

In the next section, I develop and calibrate a dynamic model of labor supply with debt restructuring. The calibration targets a stylized version of my empirical findings: initially employed participants increase labor supply relative to rejected applicants facing wage garnishment, whereas initially unemployed participants optimally remain out of work. By focusing on debtors whose baseline labor supply depends on their pre-application employment histories, the model captures the behavior of marginal applicants.

While the model abstracts from important aspects like debtor heterogeneity, labor-market frictions, and potential effects of financial distress on productivity (Kaur et al., 2025), it provides a tractable environment for policy counterfactuals. I use the model to evaluate reductions in the marginal adjustment rate that governs how additional earnings are incorporated into the repayment schedule. These exercises show that the current repayment scheme places the program on the wrong side of the Laffer curve. Since only unemployed debtors will react to changes in the repayment plan, lowering the adjustment rate raises debtors' consumption while simultaneously increasing total repayments.

I abstract from creditor applications for repayment plan adjustments and assume that these adjustments follow income changes deterministically, given the salience of these rules for applicants to the debt restructuring program. I briefly discuss implications of this assumption below. The model generates persistence of the debt restructuring program's effects on participants by incorporating both wage growth through human capital accumulation from working and wage decreases due to human capital depreciation as in Dinerstein et al. (2022). After 5 years without work, the potential wages of participants will have declined enough to prevent labor market re-entry. Another mechanism, which I abstract from that could explain why participants remain unemployed after the program ends could be signaling and discrimination by potential employers (Kroft et al., 2013; Eriksson and Rooth, 2014).

[57] For institutional reasons, I cannot use the examiner IV design for these cohorts.

6.3.1 Model setup

Individuals choose consumption c_t and monthly working hours h_t following MaCurdy (1981) preferences

$$u(c_t, h_t) = \sum_{t=0}^{T} \delta^t \left[\frac{c_t^{1-\gamma}}{1-\gamma} - \psi \frac{h_t^{1+\chi}}{1+\chi} \right] \tag{12}$$

where δ is a discount factor. Time $t = 0$ refers to the first year after the decision on the application to the debt restructuring program has been made. See Section 6.3.5 for a discussion of how pre-application labor supply decisions can shape both repayment plans and potentially generate moral hazard and the probability of being accepted to the program. I assume throughout that individuals cannot save or borrow.[58] The budget constraint is given by

$$c_t = w_t h_t - R_D^t \tag{13}$$

$$c_t \geq c_L. \tag{14}$$

$w_t = (1-\theta)\omega_t$ is the hourly net wage rate with linear income tax θ and gross wage ω_t, R_D^t is the debt repayment schedule I describe in detail below, and c_L are social benefits for unemployed individuals paid by the government in every period. Hours can be chosen from either non-employment or a discrete set of potential contracts that require at least 20 hours of work per week. A minimum number of working hours can arise from either fixed costs of employment for potential workers arising, for example, from commuting costs or the fixed costs for employers due to hiring or training new employees (Rogerson and Wallenius, 2009). I choose 20 hours as the minimum number of hours based on the distribution of contracted hours reported in the Structure of Earnings Survey (see Appendix Figure A9).

To explain the persistence of effects and to capture dynamic returns to working, I let wages follow the law of motion in Dinerstein et al. (2022)

$$w_{t+1} = (1 + \kappa_1 \mathbf{1}\{h_t > 0\} - \kappa_2) w_t. \tag{15}$$

κ_1 captures increases in human capital accumulated through work experience in the previous year and κ_2 captures the human capital depreciation that arises every year independent of an individual's work status.

[58] Borrowing constraints are a reasonable assumption for over-indebted individuals. Not allowing for savings and borrowing for individuals after they finish the debt restructuring program is inconsequential in this setup, because consumption smoothing would require transferring income to earlier periods rather than saving during the repayment period, and there is no uncertainty requiring precautionary savings.

6.3.2 Repayment plan

I assume that rejected applicants repay their debt through wage garnishment which I model as a tax on net-of-tax labor income. This tax is linear at rate τ.

$$R^t_{D=0} = \tau w_t h_t. \tag{16}$$

For accepted applicants, initial repayment during the program is given by r. If the debtor's income increases by more than the allowance $\Delta y_t > A$ because of higher working hours, any additional income exceeding A will be taxed at the marginal adjustment rate $\rho = 1$ such that

$$R^{t<5}_{D=1} = \rho(\Delta y_t - A) + r. \tag{17}$$

Since income is purely a function of deliberate choices and there are no unforeseeable shocks to participants' financial capacities, the repayment plan cannot be adjusted downward. If a participant defaults on her repayments she exits the debt restructuring program and is again subject to wage garnishment. If she does not default, the program ends after 5 years and all remaining debt is discharged

$$R^{t\geq 5}_{D=1} = 0. \tag{18}$$

6.3.3 Calibration and choice of parameters

I follow Keane and Wasi (2016) and set $\gamma = 0.727$. The average employed participant earns a gross hourly wage of 15.6 USD. I abstract from nonlinearities of the Swedish tax schedule and deduct a linear income tax of 14 percent.[59] I set $\tau = 0.12$ to the median wage garnishment rate prior to the program. I set monthly social benefits to $c_L = 772$ USD. This amount corresponds to the disposable income of a typical unemployed single individual, as recorded in the LISA database. The minimum monthly disposable income, considered by the SEA to be the subsistence minimum, is set at 644 USD. Next, I match the median of the empirical repayment rates and set initial repayment to 12 percent of initial income for those employed and 0 for those without initial employment. The average participant is 45 years old and I set the final period $T = 20$. To match the time frame of 7 years given in my empirical analysis, I assume that there are no further changes after the seventh year and incorporate the corresponding continuation value.

I then calibrate the remaining parameters $\chi, \delta, \psi, \kappa_1$, and κ_2 to match five moments from a stylized representation of my empirical results via the simulated method of moments (SMM). Figure 8a plots this stylized representation for three types of applicants. Two applicants are initially employed. One gets rejected by the debt restructuring program and remains at initial

[59]In reality, the Swedish income tax schedule is nonlinear due to an earned income tax credit and deductions from taxable income and can vary depending on the municipality of residence. 14 percent is a reasonable approximation of the average tax paid by either part-time or full-time workers with the given hourly wage

weekly hours of 35. The second applicant gets accepted and increases working hours by 4 hours per week (corresponding to the respective 2SLS estimate, see Appendix Table A13). The third applicant is initially unemployed, gets accepted to the program and remains unemployed throughout the program and after it ends. The five moments I target are the working hours of the rejected applicant in period 0, the working hours of the initially unemployed and accepted applicant in periods 2 and 6, and the differences in working hours between the accepted and rejected initially employed applicants in periods 2 and 6. This results in parameter values of $\chi = 0.269, \delta = 0.8, \psi = 0.027, \kappa_1 = 0.04$, and $\kappa_2 = 0.011$ (see also Table 6).

Together, γ and χ imply an uncompensated static labor supply elasticity of 0.274 which is similar to what Kleven and Schultz (2014) find for a large tax increase in Denmark.[60]

6.3.4 Policy counterfactuals by decreasing the adjustment rate ρ

Next, I simulate policy reforms that reduce the marginal adjustment rate ρ and evaluate the resulting changes in the labor supply of the initially unemployed accepted applicant. Figure 8b plots weekly hours worked for different values of ρ. For $\rho = 0.6$, weekly hours increase to 25 hours per week during the program and to 40 hours per week two years after the program ends, and any disincentives induced by the adjustable repayment plan disappear. For a rate $\rho = 0.2$, labor supply during the program increases to 30 hours per week. Abolishing any adjustments and setting ρ to 0 leads to an increase of up to 35 hours per week initially and 40 hours afterward.[61]

The additional value generated by the debtor's re-entry in the labor market is split among the debtor herself, her creditors, and the government, which pays out less in social benefits and collects more in income taxes. Figure 8c shows how the additional value over 7 years is distributed for different values of the adjustment rate ρ. For a 20 percentage point lower adjustment rate at $\rho = 0.8$, debtors enjoy additional overall consumption of 69,300 USD. For a fixed repayment plan ($\rho = 0$), additional consumption increases to 110,000 USD compared with the case of $\rho = 1$. The additional debt repayment to creditors follows a Laffer curve with a modest maximum value of 6,500 USD at $\rho = 0.8$, but is positive for all positive ρ below 0.85. Government revenue increases by between 75,400 USD and 80,200 USD for adjustment rates smaller than or equal to 0.8. The overall total value over 7 years of reducing the adjustment rate by only 20 percentage points amounts to 151,400 USD per initially unemployed (marginal) applicant. This corresponds to 5.1 times the annual full-time gross salary at an hourly wage rate of 15.6 USD. Figure 8c also highlights the role of average versus marginal implicit tax rates for different adjustment margins. When ρ drops to 0.8, this is sufficient to reduce the average implied tax to induce the debtor to find part-time employment. After this decision, however, any additional hours worked will result in a substantially higher marginal implicit tax rate. To

[60] The uncompensated elasticity of the static problem is given by $\epsilon_U^L = \frac{1-\gamma}{\chi+\gamma}$; see Keane (2011).

[61] The reason labor supply does not immediately catch up with initially employed participants is a result of the wage decline due to 1 year of being unemployed.

induce further increases in employment along the intensive margin, the implicit tax rate must drop below 0.3.

6.3.5 Labor supply moral hazard and probabilistic adjustments

Since repayment plans are a function of initial income, reducing the adjustment rate ρ could induce initially employed participants to reduce initial labor supply. In Appendix D I show that, in the given setting, only very low adjustment rates ρ in combination with relatively high acceptance probabilities will result in sizable labor supply moral hazard. I also extend the model and allow for probabilistic adjustments of the repayment plan. The lowest adjustment probability after a large increase in income that is consistent with the stylized results in Figure 8a is 81 percent.

6.4 Comparison with debt restructuring programs in the US and Denmark

To understand the disparities in outcomes between the Swedish debt restructuring program and those of the US and Denmark, we have to consider both differences in the respective populations underlying the reported estimates and the institutional details. Dobbie and Song (2015) find that in the US, Chapter 13 bankruptcy increases annual earnings by 25.1 percent and employment by 6.8 percentage points. These effects are driven by a drop in the earnings and employment of rejected applicants who potentially face wage garnishment and home foreclosure. Chapter 13 bankruptcy allows the discharge of debt following a 3- to 5- year partial repayment period. As in the Swedish setting, the repayment plan can be adjusted if debtors experience a significant increase in their income.

Why do outcomes between Swedish and US debtors differ so much? A plausible explanation is the substantial difference in the underlying population of applicants. In the US, the average applicant has a 81.3 percent probability of being employed compared with 58 percent in Sweden. In a heterogeneity analysis, Dobbie and Song (2015) split the sample by initial median income.[62] Comparing high- and low-income applicants, the effect in earnings decreases from 8,650 USD to 1,691 USD and the effect on employment falls from 9.9 percentage points to an insignificant 3.9 percentage points. However, even low-income applicants in the US have a 6.8 percentage point higher probability of being initially employed compared with applicants in the overall Swedish sample. Investigating how these results change when considering only applicants without employment who can increase their income only by large discrete steps and risk adjustments in their repayment plan could shed light on the potential adverse effects of Chapter 13 bankruptcy on this vulnerable subgroup.

In Denmark, the debt restructuring program also allows for the discharge of unsecured debt after a partial repayment period of, typically, 5 years. Importantly, however, the repayment plan is fixed at the initial decision and cannot be adjusted upward. Bruze et al. (2024) report that

[62] See Table 5 in Dobbie and Song (2015)

participants in the Danish debt restructuring program have 26 percent higher earned income and are 11.7 percentage points more likely to be employed in the 16 years after the program starts. Danish applicants have a 64 percent probability of being initially employed, which renders them worse off than their US counterparts but better off than Swedish applicants. Notably, the difference in employment between accepted and rejected applicants in Denmark is -2.4 percentage points compared with -17 percentage points in Sweden. Bruze et al. (2024) also split the sample by initial median income.[63] Unlike in the US and Sweden, low-income applicants experience larger increases in labor earnings and employment, which suggests that there are no inherent reasons why low-income individuals would react worse to debt restructuring.

7 Conclusion

This paper analyzes how debt restructuring programs can shape the labor supply incentives of participating debtors using Sweden as a case study. Initially employed participants increase income and employment relative to rejected applicants. Repayment plans assigned at the onset of the program are unlikely to be adjusted upward for this group. At the same time, acceptance to the program shields participants from debt collection practices such as wage garnishment, which can reduce labor supply incentives for rejected applicants. Initially *un*employed participants, on the other hand, face large disincentives to seek employment because their repayment plans can be severely upward-adjusted following substantial increases in income. Using an online survey, I show that although observed adjustment probabilities are rather low, participants perceive these adjustments as very likely. These findings challenge the practice of substantially adjusting repayment plans in debt restructuring programs in response to income changes.

A second contribution of this paper is to show that classical examiner IV designs based on jackknife or leave-one-out estimates of examiner leniency will be biased under path-dependent decision-making, such that examiners' current decisions are influenced by past case characteristics. I present empirical evidence for the presence of such past dependencies in decision-making within the context of debt restructuring in Sweden, and demonstrate that this results in biased estimates. Similar issues might arise in other settings as well and can be avoided by constructing leniency instruments based only on past cases.

[63] See Appendix Table A.24 in Bruze et al. (2024). The authors do not provide summary statistics on the employment or income of these two groups.

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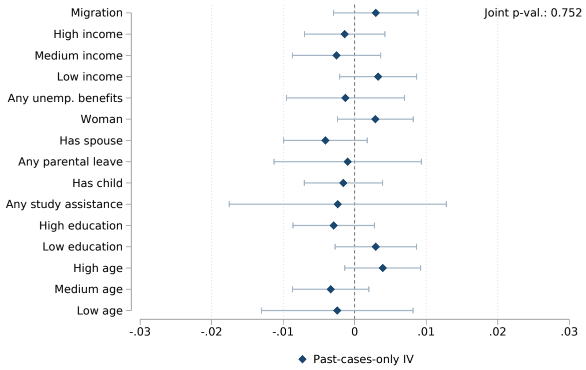
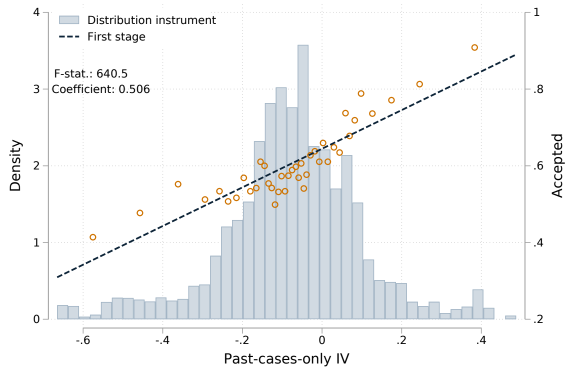
8 Tables and figures

Figure 1: Validity of instrument



(a) Balance of Instrument



(b) First Stage

Notes: This figure supports the validity of the examiner leniency design. **Panel (a)** reports coefficients from OLS regressions of the past-cases-only instruments on indicator variables for applicant characteristics measured in the year before application. The reported p-value refers to an F-test of joint significance in a regression of the respective instruments on the full set of characteristics. Characteristics include: migrant status (proxied by lacking a registered Swedish birth region), annual labor income (above USD 20,000, medium income, and zero income), receipt of unemployment benefits, gender, cohabitation with a spouse, any parental leave, presence of at least one child under 18, educational attainment (at least 12 years of schooling, or less), and age group (above 45, 30–45, below 30). The joint regressions omit the indicators for medium income, low age, and high education. Whiskers are 95% confidence intervals. **Panel (b)** shows the distribution of the past-cases-only instrument. The left y-axis presents a binscatter of the first-stage fit, plotting the average acceptance rate at each leniency level. The estimated first-stage coefficient is 0.506. Standard errors are clustered at the applicant level.

Table 1: The Effects of Debt Restructuring on Income and Employment

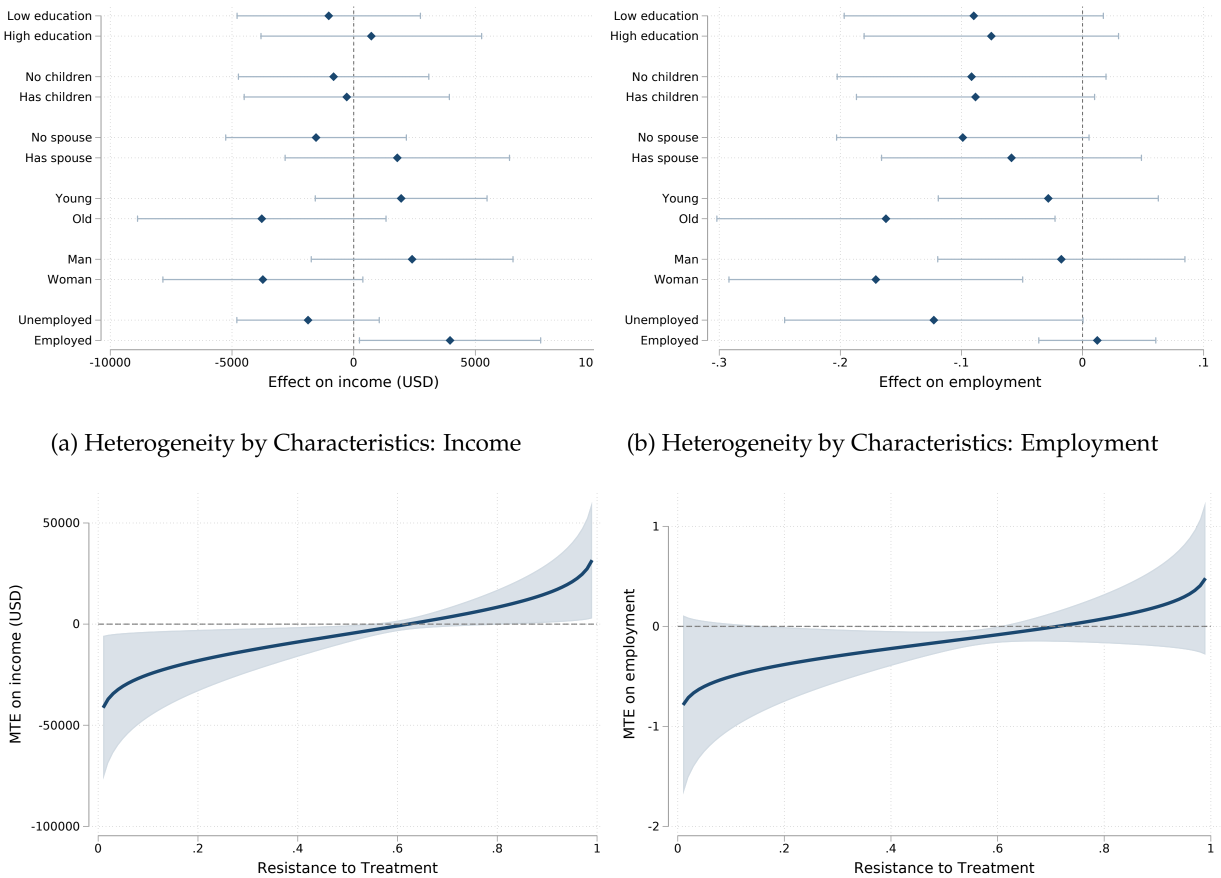
	Income			**Employment**		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Leave-one-out	Past-cases-only	OLS	Leave-one-out	Past-cases-only
Accepted	-4286.3***	-2363.7*	-1247.1	-0.154***	-0.129***	-0.0891*
	(258.3)	(1158.3)	(1410.9)	(0.00672)	(0.0309)	(0.0370)
Constant	19056.6***	17246.9***	16570.4***	0.742***	0.676***	0.651***
	(2575.3)	(714.6)	(867.4)	(0.0708)	(0.0190)	(0.0226)
Relative to control mean	-0.225	-0.126	-0.063	-0.208	-0.181	-0.122
Control for Year x Unit	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16642	16642	16642	16642	16642	16642

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents coefficients from the OLS regressions and 2SLS regressions for each of the two instruments based on leave-one-out estimates of examiner leniency and past-cases-only. The outcomes are seven-year averages of annual labor income, converted to 2019 USD, and an employment indicator variable for the years following the start of the debt restructuring program. I control for unit-by-year interactions by using fixed effects in the OLS regression and by using a leave-examiner-out residualization in the 2SLS estimations. Standard errors are clustered at the applicant level.

Figure 2: Heterogeneity in Treatment Effects



(a) Heterogeneity by Characteristics: Income

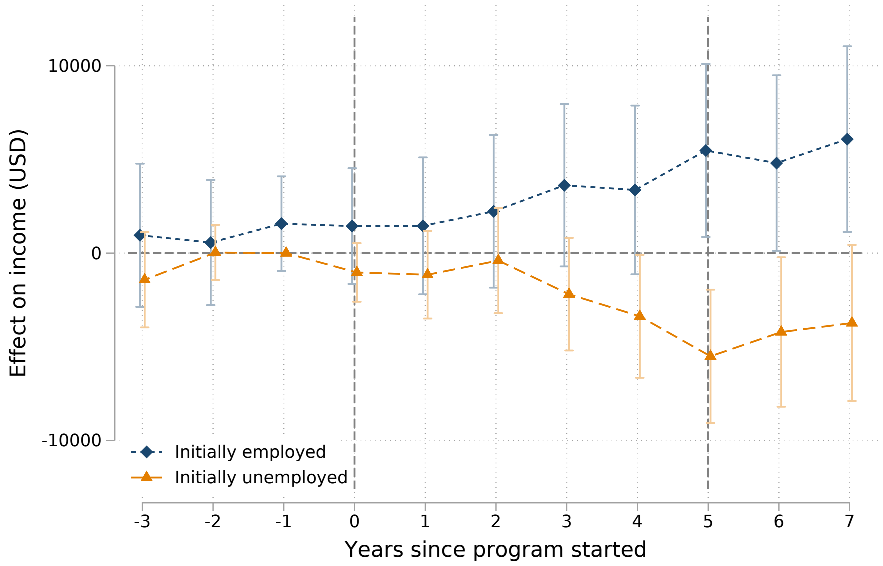
(b) Heterogeneity by Characteristics: Employment

(c) Marginal Treatment Effects: Income

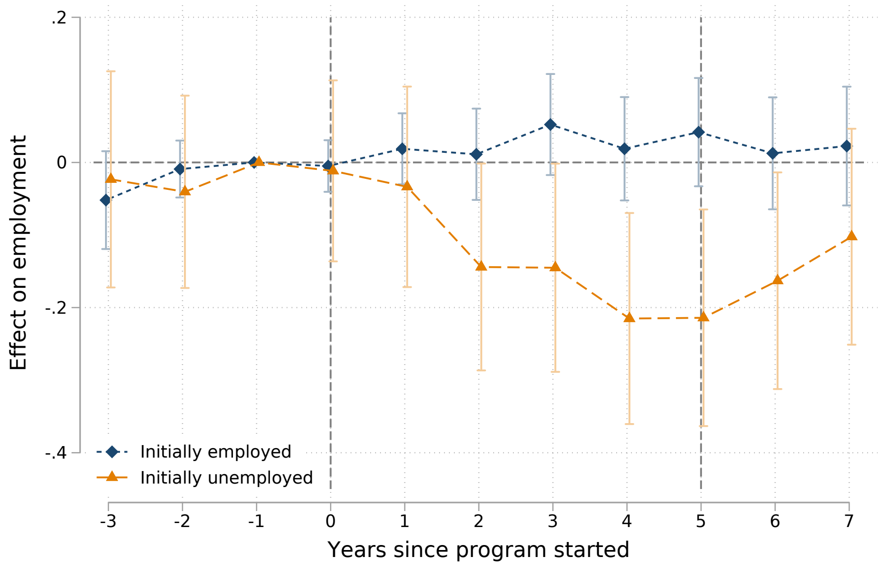
(d) Marginal Treatment Effects: Employment

Notes: This figure shows heterogeneity in the effects of acceptance into the debt restructuring program using the past-cases-only instrument. Outcomes are seven-year averages of annual labor income (in 2019 USD) and an indicator for employment in the years following program entry. **Panel (a)** reports effects on income by applicant characteristics measured in the year prior to application: years of schooling (below/above median), presence of children below the age of 18, cohabitation with a spouse, age (below/above median), gender, and baseline employment status. **Panel (b)** reports the corresponding effects on employment. **Panel (c)** plots marginal treatment effects (MTEs) for income against unobserved resistance to treatment, estimated via local IV (Andresen, 2018); **Panel (d)** presents the corresponding MTEs for employment. Confidence intervals for MTEs are computed by a bootstrap with 300 replications. In all regressions, I control for unit-by-year interactions by using a leave-examiner-out residualization. Whiskers and shaded areas indicate 95% confidence intervals. Standard errors are clustered at the applicant level.

Figure 3: Dynamic Effects of Debt Restructuring



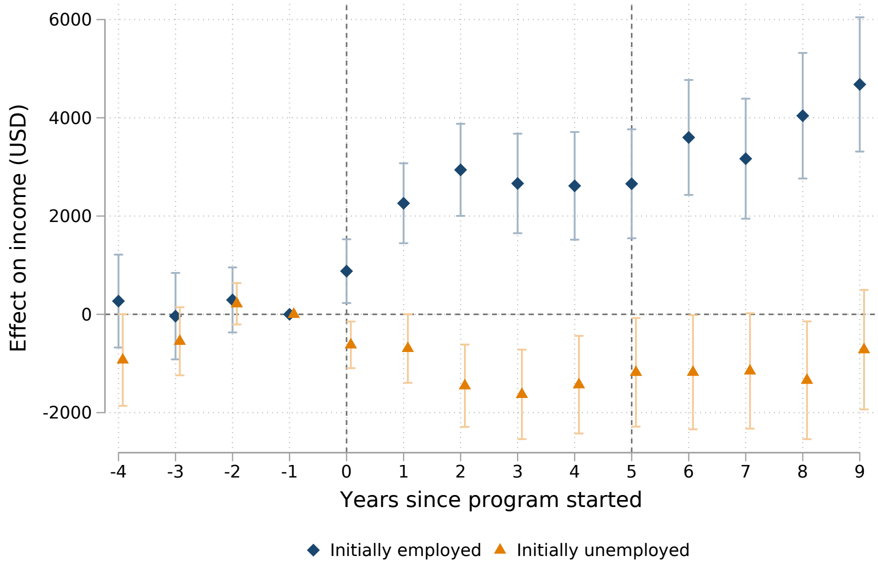
(a) 2SLS Estimates on Income



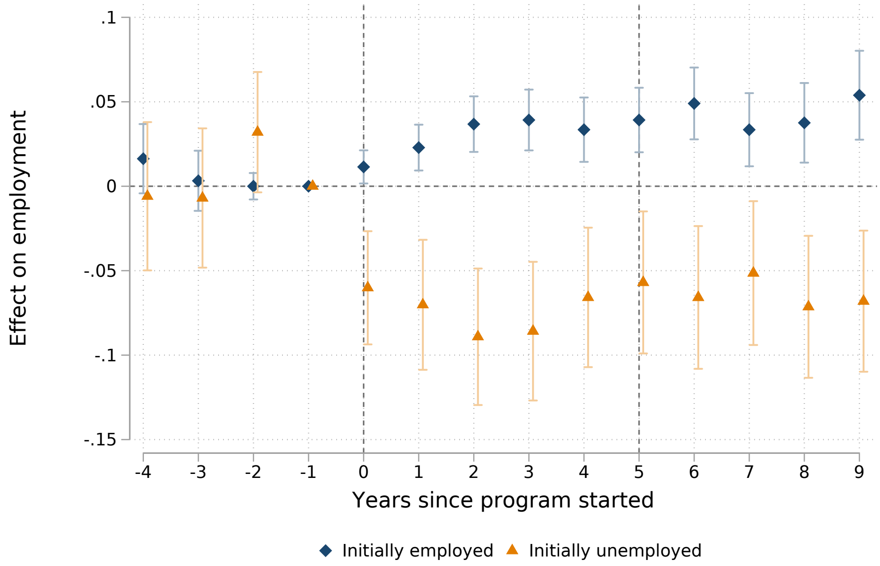
(b) 2SLS Estimates on Employment

Notes: This figure presents the effect of being accepted to the debt restructuring program for each year using the past-cases-only instrument. The sample is split into two groups: applicants with initial employment and annual labor income above 19,000 USD, and those without initial employment. Vertical dashed lines mark the application year (first) and the end of the program (second). **Panel (a)** presents results on annual labor income (in 2019 USD), and **panel (b)** on employment. The employment estimates at period $t = -1$ and the income estimates for the initially unemployed are mechanically zero by construction. I control for unit-by-year interactions by using a leave-examiner-out residualization. Whiskers are 95% confidence intervals. Standard errors are clustered at the applicant level.

Figure 4: Graphical Evidence on Long-Run Effects



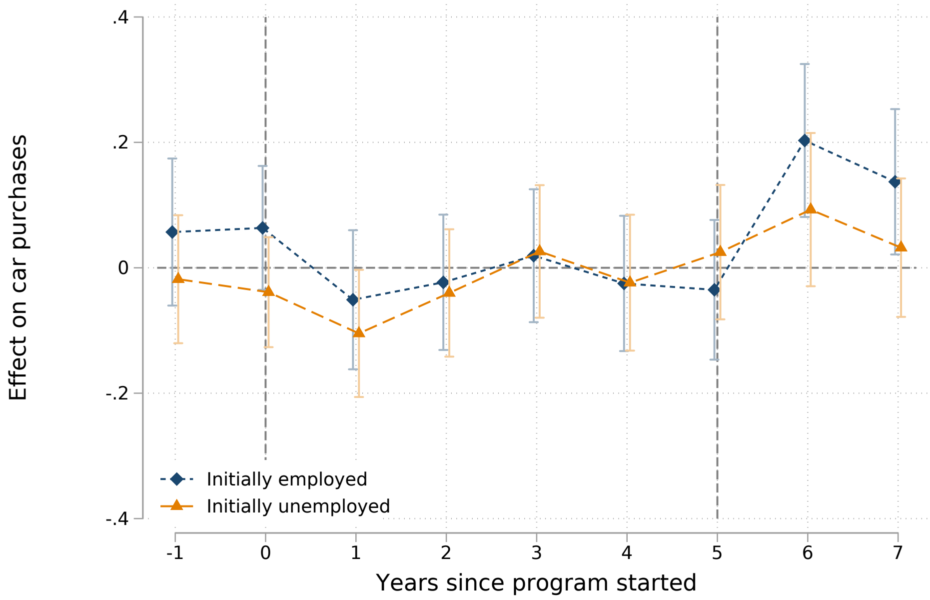
(a) DiD Estimates on Income



(b) DiD Estimates on Employment

Notes: These figures report estimates from a dynamic difference-in-differences (event-study) design comparing accepted applicants with a propensity-score–matched sample of rejected applicants. The sample is split into two groups: applicants with initial employment and annual labor income above 19,000 USD, and those without initial employment. Vertical dashed lines mark the application year (first) and the end of the program (second). The matched group is constructed by estimating a logit propensity-score model using second-degree polynomials in age and years of education (measured pre-application), a fully interacted set of indicators for gender, cohabitation with a spouse, and the presence of any child under 18 in the household, and three bins for income two years prior to application. Propensity scores are partitioned into ten bins; within each bin and application year, I retain equal numbers of accepted and rejected applicants. **Panel (a)** presents results on annual labor income (in 2019 USD), and **panel (b)** on employment. All effects are estimated relative to the year before application. Whiskers are 95% confidence intervals. Standard errors are clustered at the applicant level.

Figure 5: Dynamic 2SLS Estimates on Car Purchases



Notes: This figure presents the effect of being accepted to the debt restructuring program on car purchases for each year using the past-cases-only instrument. Car purchases can only be measured from 2011 onward. I therefore only report pre-program estimates for one year. The sample is split into two groups: applicants with initial employment and annual labor income above 19,000 USD, and those without initial employment. Vertical dashed lines mark the application year (first) and the end of the program (second). I control for unit-by-year interactions by using a leave-examiner-out residualization. Whiskers are 95% confidence intervals. Standard errors are clustered at the applicant level.

Table 2: Wage Garnishment During Program

	Has garnishment		**Garnishment rate**	
	(1) Initially employed	(2) Initially unemployed	(3) Initially employed	(4) Initially unemployed
Accepted	-0.271***	-0.0311	-0.0705***	0.0197
	(0.0376)	(0.0336)	(0.0123)	(0.0333)
Constant	0.312***	0.0932***	0.0765***	0.0193
	(0.0217)	(0.0244)	(0.00719)	(0.0202)
Relative to control mean	-0.813	-0.228	-0.887	0.447
Control for Year x Unit	Yes	Yes	Yes	Yes
Observations	5426	6495	5410	2344

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports coefficients for 2SLS regressions using the past-cases-only instrument. The outcomes are the average probability of wage garnishment within the five years following program initiation and the average wage garnishment rate relative to income, for applicants who are employed. The sample is split into two groups: applicants with initial employment and annual labor income above 19,000 USD, and those without initial employment. I control for unit-by-year interactions by using a leave-examiner-out residualization. Standard errors are clustered at the applicant level.

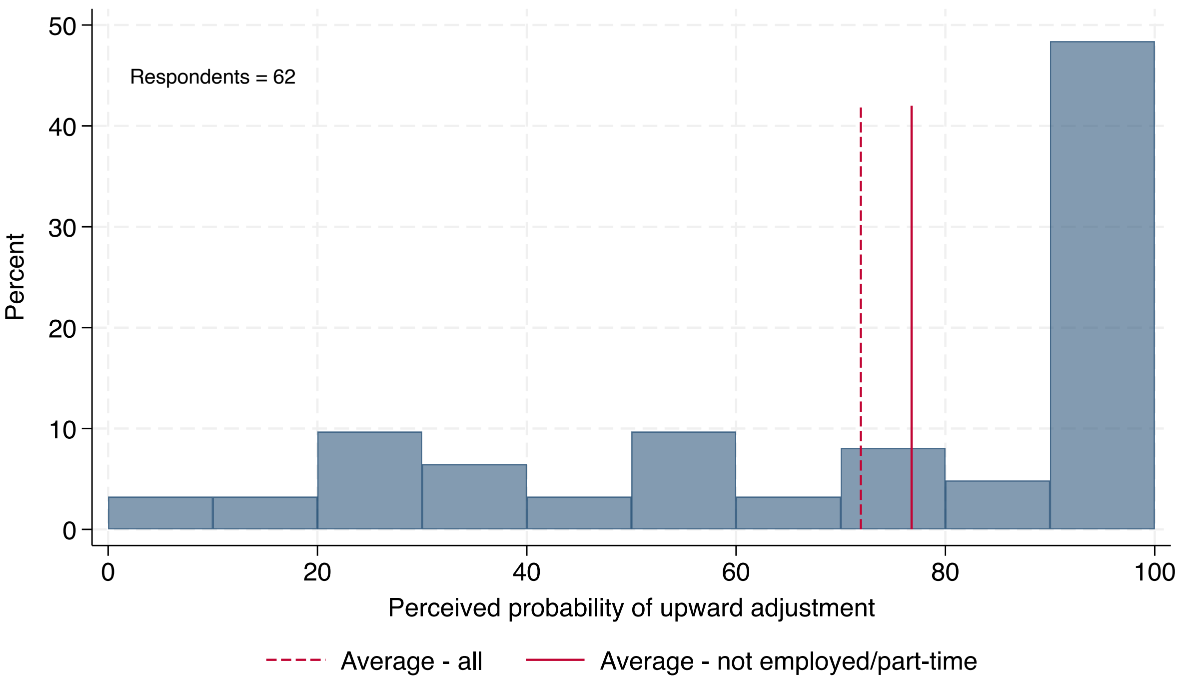
Table 3: By Predicted Wage Garnishment: Initially Employed

	Low predicted garnishment		**High predicted garnishment**	
	(1)	(2)	(3)	(4)
	Has garnishment	Income	Has garnishment	Income
Accepted	-0.100**	2370.2	-0.346***	5767.6*
	(0.0361)	(2905.8)	(0.0559)	(2576.9)
Constant	0.137***	28007.4***	0.399***	28193.2***
	(0.0238)	(1857.4)	(0.0275)	(1254.9)
Relative to control mean	-0.730	0.085	-0.867	0.205
Control for Year x Unit	Yes	Yes	Yes	Yes
Observations	2707	2707	2707	2707

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports coefficients for 2SLS regressions using the past-cases-only instrument for initially employed applicants. The outcomes are the average probability of wage garnishment within the seven years following program initiation and average income in USD, adjusted to 2019 prices, for initially employed applicants. I split the sample and use each subsample to predict the probability of wage garnishment in the other, out-of-sample. To predict wage garnishment, I estimate an OLS regression of wage garnishment status on five bins of pre-application income and age, three bins of education level in the year prior to application, dummies for gender, marital status, having children, and the application year. Additionally, I include dummies for wage garnishment status in the year before application and for SEA registration within five years before application. Columns 1 and 3 display the actual share of applicants under wage garnishment for those with low and high predicted probabilities, respectively, while columns 2 and 4 present the effects on the seven-year average of annual income in 2019 USD. Standard errors are clustered at the applicant level.

Figure 6: Perceived Adjustment Probability



Notes: This figure presents the distribution of perceived probabilities of upward adjustments to the repayment plan following a significant increase in income. The data are obtained from the online survey of debt restructuring participants. Unemployed or part-time respondents were asked about the likelihood of an upward adjustment following employment, while full-time respondents were asked about the probability of adjustment after a substantial increase in financial capacity. The dashed red line represents the overall average response of 72%, while the solid red line represents the average response of unemployed or part-time respondents, which is 77%.

Table 4: Weighted 2SLS Estimations by Initial Employment

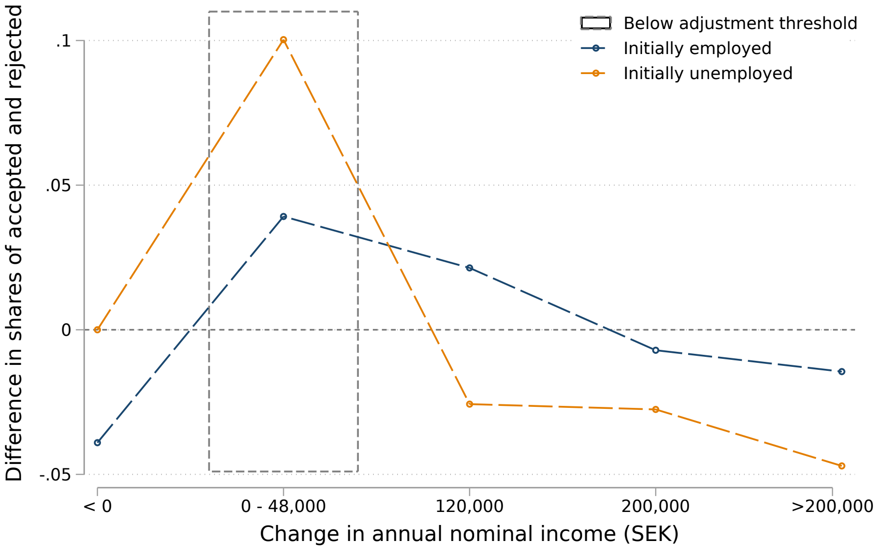
	Unweighted sample		**Weighted sample**
	(1)	(2)	(3)
	Initially unemployed	Initially employed	Initially employed
Panel A: Income			
Accepted	-2028.9	3777.2*	2307.6
	(1521.5)	(1900.0)	(2128.1)
Relative to control mean	-0.312	0.013	-0.013
Control for Year $\times$ Unit	Yes	Yes	Yes
Observations	6441	5413	5413
Panel B: Employment			
Accepted	-0.125	0.0117	-0.0122
	(0.0641)	(0.0247)	(0.0324)
Relative to control mean	-0.286	0.128	0.082
Control for Year $\times$ Unit	Yes	Yes	Yes
Observations	6441	5413	5413

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Notes: This table reports the effect of being accepted to the debt restructuring program using the past-cases-only instrument. The sample is split into two groups: applicants with initial employment and annual labor income above 19,000 USD, and those without initial employment. The first two columns report unweighted estimates from a sample with overlap in the distributions of applicant characteristics. The third column reports the estimate for initially employed applicants re-weighted to match the distribution of applicant characteristics among initially unemployed. **Panel A** reports estimates for the seven-year average of annual labor income (in 2019 USD) and **Panel B** for employment. I control for unit-by-year interactions by using a leave-examiner-out residualization. Standard errors are clustered at the applicant level.

Figure 7: Difference in Shares by Changes in Nominal Annual Income (SEK)



Notes: This figure plots the difference between the share of accepted applicants and a weighted share of rejected applicants of initially employed and unemployed participants within bins of changes in annual nominal (not CPI-adjusted) income relative to initial income. The first bin includes all participants with negative income growth relative to initial income. The share of initially unemployed participants is mechanically zero in this first bin. I split the sample into those with initial employment and annual labor income above 19,000 USD and those without initial employment. Rejected applicants are weighted using 5 bins of age, five bins of initial, and dummies for having any children initially, a college degree, a spouse, gender, and year of application to match the joint distribution of these characteristics among accepted applicants. The gray dotted line indicates the threshold below which upward adjustments should typically not occur.

Table 5: Labor Force Participation and Job Search Status

	Balanced sample		**Unbalanced sample**	
	(1) Out of labor force	(2) Job search	(3) Out of labor force	(4) Job search
Accepted	0.178	-0.237	0.146	-0.289*
	(0.181)	(0.143)	(0.178)	(0.137)
Constant	0.359**	0.288**	0.366**	0.337***
	(0.128)	(0.103)	(0.127)	(0.100)
Relative to control mean	0.495	-0.823	0.398	-0.858
Control for Year x Unit	Yes	Yes	Yes	Yes
Observations	546	546	781	781

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

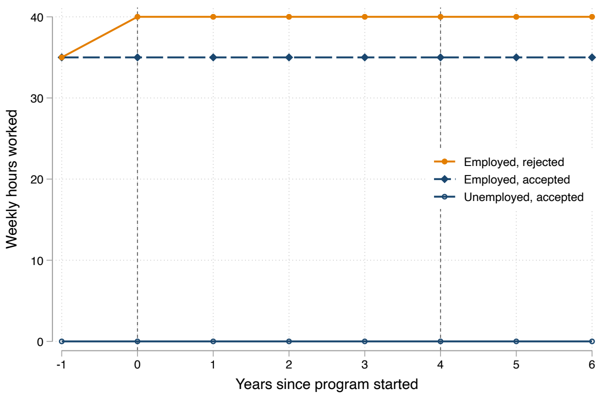
Notes: This table presents coefficients for 2SLS regressions using the past-cases-only instrument for initially unemployed participants. To increase sample size I define initial unemployment as having an initial income below 19,000 USD. Outcome measures are derived from job status data in the Labor Force Survey, indicating both the seven-year average of being out of the labor force and the seven-year average of being unemployed but actively searching for work. Columns 1 and 2 refer to the balanced sample that is also used in the main estimations. In Columns 3 and 4 I drop this restriction to increase sample size. Note that 'balanced' refers to the main sample and not to consistent participation in the Labor Force Survey, as this survey is not panel-based. Unlike in the other regressions, the control mean used to compute relative effects is based on the regression constant rather than an estimate of the respective complier mean, as the latter is infeasible due to the small sample size. Standard errors are clustered at the applicant level.

Table 6: Parameter Specification

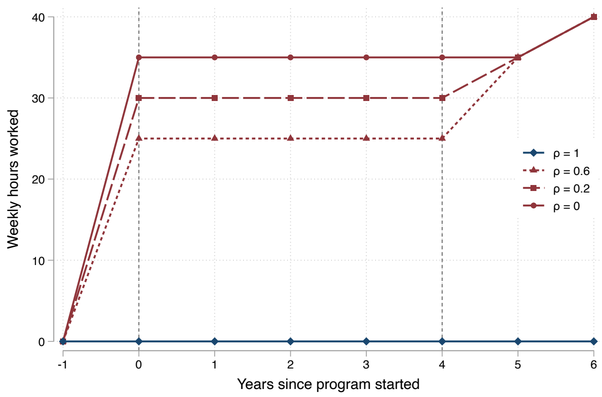
	Variable	**Value**	**Source**
Risk aversion	γ	0.727	Keane and Wasi (2016)
Frisch elasticity	χ	0.269	Calibrated via SMM
Discount factor	δ	0.8	Calibrated via SMM
Labor disutility	ψ	0.027	Calibrated via SMM
Experience effect	κ_1	0.04	Calibrated via SMM
Human capital depreciation	κ_2	0.011	Calibrated via SMM
Initial hourly gross wage	ω_{-1}	15.6 USD	Average employed participant
Initial monthly hours if employed	h_{-1}	140	Average employed participant
Income tax rate	θ	0.14	Approximation of Swedish tax schedule
Garnishment rate	τ	0.12	Median wage garnishment of employed applicants
Initial repayment rate	$r/w_{-1}h_{-1}$	0.12	Median rate of accepted employed applicants
Monthly social benefits	c_L	772 USD	Average disposable income of unemployed (LISA)

Notes: This table reports the parameter values used to calibrate the dynamic labor supply model. The parameter γ is taken from Keane and Wasi (2016). The parameters $\chi, \delta, \psi, \kappa_1$, and κ_2 are calibrated internally via SMM using five moments from the data. The remaining parameters are taken from the data.

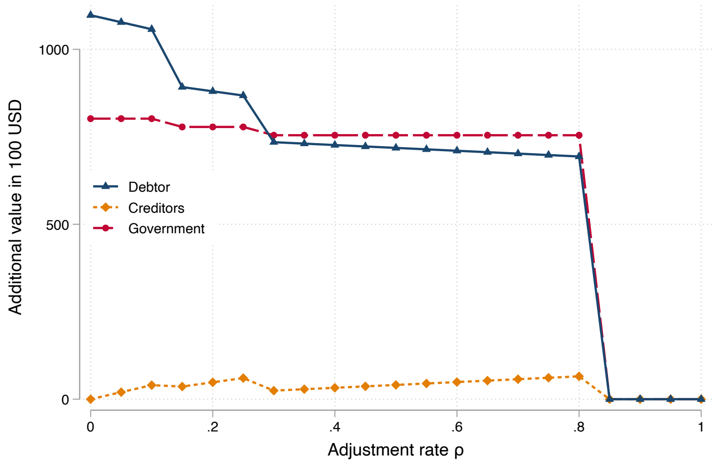
Figure 8: Counterfactual Policy Experiment



(a) Weekly Hours: Target



(b) Weekly Hours by Marginal Adjustment Rate



(c) Additional Monetary Value by Marginal Adjustment Rate

Notes: This figure reports results of the counterfactual policy experiment. **Panel (a)** reports weekly working hours following a stylized representation of my empirical results targeted by the model. Initial hours in period -1 are set exogenously. The dotted orange line plots outcomes for a rejected applicant who initially works 35 hours per week and who faces wage garnishment. The dashed blue line plots hours for an accepted applicant who initially works 35 hours per week. And the solid blue line plots outcomes for an accepted applicant who does not work in the year prior to application. The first vertical dashed line indicates the start of the debt restructuring program, and the second dashed line marks its end. **Panel (b)** report weekly hours as predicted by the model for different marginal adjustment rates ρ, as specified in equation (17). All outcomes are for accepted applicants who are initially unemployed. **Panel (c)** displays the additional value in 100 USD over seven years for different marginal adjustment rates, ρ, as specified in equation (17). The blue solid line plots additional consumption for debtors, the dashed orange line plots additional repayment to creditors, and the dashed red line additional government revenue from lower social benefits paid out and higher labor taxes. All outcomes are for accepted applicants who are initially unemployed.

Online Appendix

Designing Debt Restructuring:
The Adverse Effects on Labor Market Outcomes

Jakob Beuschlein[†]

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A1 Tables

Table A1: Applicant Characteristics

Variable	**All**	**Accepted**	**Rejected**
Age	44.17	45.09	42.75
	(8.03)	(7.54)	(8.53)
Share women	0.50	0.54	0.43
	(0.50)	(0.50)	(0.50)
Years of education	10.84	10.73	11.00
	(1.90)	(1.85)	(1.95)
Number of children	0.70	0.67	0.74
	(1.10)	(1.08)	(1.12)
Labor income (USD)	12975	11203	15697
	(15080)	(14332)	(15779)
Employed	0.59	0.53	0.69
	(0.49)	(0.50)	(0.46)
Within cohort income rank	0.23	0.20	0.28
	(0.24)	(0.22)	(0.25)
Debt (USD)		80271	
		(106390)	
Repayment (USD)		11090	
		(14979)	
Observations	16642	10082	6560

Notes: This table reports means and standard deviations (in parentheses) for characteristics in the year before application, for all applicants, accepted applicants, and rejected rejected applicants. The characteristics are age, binary gender, number of children below the age of 18 living in the same household, labor income, a dummy for being employed, and the within birth cohort labor income rank. The total amount of debt and the overall repayment according to the initial repayment plan are only observed for accepted applicants.

Table A2: Complier Characteristics

Variable	**All**	**Accepted**	**Compliers**
Woman	0.500	0.543	0.456
Young	0.459	0.418	0.531
Low education	0.397	0.418	0.369
Low income	0.500	0.560	0.434
Has children	0.378	0.365	0.440
Has spouse	0.293	0.265	0.360

Notes: This table presents the share of applicants, accepted applicants, and compilers by their respective characteristics. Here, 'young' denotes applicants below the median age, 'low education' refers to individuals with below-median years of education, and 'low income' to those with below-median income in the year prior to application. Compliers are defined as applicants who would be accepted into the debt restructuring program if assigned to an examiner at the 99th percentile of the leniency distribution but rejected if assigned to an examiner at the 1st percentile. For further details, see Appendix C.

Table A3: Weak Instrument Robust Confidence Intervals

	Income	**Employment**
Wald 95% CI: All	[-3525.1, 2319.8]	[-0.166,-0.010]
AR 95% CI: All	[-3495.6, 2290.3]	[-0.165, -0.011]
Wald 95% CI: Initially employed	[200.9, 7627.1]	[-0.036, 0.061]
AR 95% CI: Initially employed	[238.4, 7589.6]	[-0.036, 0.061]
Wald 95% CI: Initially unemployed	[-4583.9, 1091.3]	[-0.235, 0.004]
AR 95% CI: Initially unemployed	[-4555.3, 1062.6]	[-0.234, 0.003]

Notes: This table reports standard Wald 95% confidence intervals as well as Anderson-Rubin 95% confidence intervals, which are robust to weak instruments. All regressions use the past-cases-only instrument. The outcomes are annual labor income in 2019 USD and any employment in a given year, averaged over the seven years following the start of the debt restructuring program. I control for unit-by-year interactions by using a leave-examiner-out residualization. Standard errors are clustered at the applicant level. The estimates in the first two rows use the entire sample. Rows 3 and 4 include only participants who were initially employed, while the last two rows focus on participants who were initially unemployed. Standard errors are clustered at the applicant level.

Table A4: Test for Monotonicity: By Applicant Characteristics

	Split I		**Split II**		**Split III**		**Split IV**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Women	Men	Young	Old	No College	College	Unemployed	Employed
Leniency past	0.460***	0.542***	0.640***	0.450***	0.498***	0.580***	0.407***	0.561***
	(0.0277)	(0.0284)	(0.0369)	(0.0233)	(0.0210)	(0.0638)	(0.0304)	(0.0257)
Observations	8314	8328	5025	11617	14974	1584	6806	9836

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports the first stage coefficients and F-statistics for regressions of the examiner's decision on the past-cases-only instrument for different splits of the sample. The first two columns separate the sample by gender. The third and fourth columns divide the sample into applicants aged 40 or younger and those over 40. The fifth and sixth columns divide applicants based on whether they hold a college degree. The final division separates the sample by initial employment status, defined as having non-zero labor income.

Table A5: Sharp Test for Monotonicity and Exclusion Restriction

Outcome	**Test statistic**	**Critical value (5%)**	**p-value**
Income	0.0035	0.1107	0.9967
Employment	0.0001	0.0308	0.9999

Notes: This table reports test statistics, critical values, and p-values for the sharp test of the monotonicity condition and the exclusion restriction developed by Coulibaly et al. (2024) for the past-cases-only instrument. The null hypothesis H_0 is given by testable implications of the monotonicity condition and exclusion restriction. The alternative hypothesis H_1 posits that these conditions fail to hold. Following Coulibaly et al. (2024), I set Q_Y and Q_P to 5 and estimate propensity scores using a probit model. I use 300 bootstrap repetitions. The outcomes are seven-year averages of annual labor income, converted to 2019 USD, and an employment indicator variable for the years following the start of the debt restructuring program.

Table A6: Test for Monotonicity: Out-of-Sample

	Split I		**Split II**		**Split III**		**Split IV**	
	(1) Women	(2) Men	(3) Young	(4) Old	(5) No College	(6) College	(7) Unemployed	(8) Employed
Leniency past	0.239***	0.400***	0.317***	0.265***	0.341***	0.260***	0.225***	0.372***
	(0.0219)	(0.0221)	(0.0268)	(0.0197)	(0.0208)	(0.0431)	(0.0227)	(0.0228)
Observations	9604	9557	5848	12433	12935	1862	7756	11360

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports the first stage coefficients and F-statistics for regressions of the examiner's decision on the past-cases-only instrument. In each split I estimate leniency for one of the two sub-samples and then use this leniency on the other sub-sample. The first two columns separate the sample by gender. The third and fourth columns divide the sample into applicants aged 40 or younger and those over 40. The fifth and sixth columns divide applicants based on whether they hold a college degree. The final division separates the sample by initial employment status, defined as having non-zero labor income.

Table A7: Average Treatment Effects

	Income		**Employment**	
	(1) Leave-one-out	(2) Past-cases-only	(3) Leave-one-out	(4) Past-cases-only
Effects				
ATE	-5403.3**	-4846.6*	-0.213***	-0.152**
	(1690.5)	(2164.8)	(0.0457)	(0.0519)
LATE	-2533.9*	-1334.3	-0.136***	-0.0925*
	(1181.3)	(1366.2)	(0.0320)	(0.0406)
Control for Year x Unit	Yes	Yes	Yes	Yes
Observations	16642	16642	16642	16642

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates of the LATE and the ATE for the leave-one-out instrument and the instrument using past-cases-only, using a local IV marginal treatment effect estimation (Andresen, 2018). The outcomes are seven-year averages of annual labor income, converted to 2019 USD, and an employment indicator variable for the years following the start of the debt restructuring program. I control for unit-by-year interactions by using a leave-examiner-out residualization. Standard errors are clustered at the applicant level and based on 300 bootstrap repetitions.

Table A8: Robustness: Alternative Measures of Outcomes

	Income			Employment		
	(1) Main	(2) Winsorize 99th	(3) Winsorize 95th	(4) Main	(5) Threshold 5000 USD	(6) Threshold 10000 USD
Accepted	-1247.1	-1171.3	-1131.8	-0.0891*	-0.0882*	-0.0787*
	(1410.9)	(1402.7)	(1338.0)	(0.0370)	(0.0382)	(0.0381)
Constant	16570.4***	16714.3***	16355.4***	0.651***	0.589***	0.545***
	(867.4)	(860.5)	(820.5)	(0.0226)	(0.0234)	(0.0234)
Relative to control mean	-0.063	-0.059	-0.059	-0.122	-0.133	-0.127
Control for Year x Unit	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16642	16642	16642	16642	16642	16642

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports 2SLS estimates using the past-cases-only instrument. Outcomes are seven-year averages of annual labor income (in 2019 USD) and an indicator for employment in the years following entry into the debt restructuring program. Column 1 reports the baseline income estimate. Columns 2 and 3 winsorize income at the 99th and 95th percentiles, respectively. Column 4 reports the baseline employment estimate. Columns 5 and 6 increase the annual income threshold used to define non-employment to USD 5,000 and USD 10,000, respectively. Standard errors are clustered at the applicant level.

Table A9: Robustness: Income

	(1) Income	(2) Income	(3) Income	(4) Income	(5) Income	(6) Income	(7) Income	(8) Income
Accepted	-1247.1	-1368.7	-1804.8	-521.0	-473.3	-2025.5	-1123.3	-683.4
	(1410.9)	(1443.1)	(1442.8)	(1440.4)	(1564.2)	(1895.3)	(2551.3)	(1591.2)
Relative to control mean	-0.063	-0.069	-0.089	-0.026	-0.025	-0.108	-0.060	-0.036
Control for Year x Unit	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Control for Week x Unit	No	Yes	No	No	No	No	No	No
Control for Region	No	No	Yes	No	No	No	No	No
Control for PScore	No	No	No	Yes	No	No	No	No
Min. number of cases	10	10	10	10	25	75	125	10
Past cases lag	30	30	30	30	30	30	30	60
Observations	16642	16642	16642	16642	14346	8853	5402	15904

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports coefficients for 2SLS regressions for the instrument using the past-cases-only IV. The outcome is annual labor income, converted to 2019 USD, averaged over the seven years following the start of the debt restructuring program. Column 1 reports the baseline estimate. In Column 2, the instrument is residualized by week of application rather than by unit-by-year. Column 3 drops the residualization by unit-times-year omitting any controls. Columns 4 to 6 increase the minimum number of cases used to construct the past-cases-only instrument to 25, 75, and 125, respectively. In Column 6, cases are considered past if the decision occurred 60 days or more prior. Standard errors are clustered at the applicant level.

Table A10: Robustness: Employment

	(1) Employment	(2) Employment	(3) Employment	(4) Employment	(5) Employment	(6) Employment	(7) Employment	(8) Employment
Accepted	-0.0891*	-0.0933*	-0.101**	-0.0611	-0.0917*	-0.130*	-0.131	-0.0762
	(0.0370)	(0.0380)	(0.0378)	(0.0377)	(0.0416)	(0.0513)	(0.0694)	(0.0427)
Relative to control mean	-0.122	-0.127	-0.137	-0.083	-0.129	-0.182	-0.180	-0.108
Control for Year x Unit	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Control for Week x Unit	No	Yes	No	No	No	No	No	No
Control for Region	No	No	Yes	No	No	No	No	No
Control for PScore	No	No	No	Yes	No	No	No	No
Min. number of cases	10	10	10	10	25	75	125	10
Past cases lag	30	30	30	30	30	30	30	60
Observations	16642	16642	16642	16642	14346	8853	5402	15904

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports coefficients for 2SLS regressions for the instrument using the past-cases-only IV. The outcome is the seven-year average of an employment indicator variable for the years following the start of the debt restructuring program. Column 1 reports the baseline estimate. In Column 2, the instrument is residualized by week of application rather than by unit-by-year. Column 3 drops the residualization by unit-times-year omitting any controls. Columns 4 to 6 increase the minimum number of cases used to construct the past-cases-only instrument to 25, 75, and 125, respectively. In Column 6, cases are considered past if the decision occurred 60 days or more prior. Standard errors are clustered at the applicant level. Standard errors are clustered at the applicant level.

Table A11: By Predicted Wage Garnishment: Initially Unemployed

	Low predicted garnishment		**High predicted garnishment**	
	(1) Has garnishment	(2) Income	(3) Has garnishment	(4) Income
Accepted	-0.0381	-1599.5	-0.0731	-2174.1
	(0.0439)	(2544.0)	(0.0421)	(1749.2)
Constant	0.0675	4522.1*	0.161***	5908.0***
	(0.0346)	(1991.8)	(0.0280)	(1160.0)
Relative to control mean	-0.564	-0.354	-0.453	-0.368
Control for Year x Unit	Yes	Yes	Yes	Yes
Observations	3218	3218	3217	3217

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports coefficients for 2SLS regressions using the past-cases-only instrument. The outcomes are the average probability of wage garnishment within seven years after program initiation and the average income, measured in 2019 USD, for initially unemployed applicants. I split the sample and use each sub-sample to predict the probability of wage garnishment in the other, out-of-sample. To predict wage garnishment, I estimate an OLS regression of wage garnishment status on five bins of pre-application income and age, three bins of education level in the year prior to application, dummies for gender, marital status, having children, and the application year. Additionally, I include dummies for wage garnishment status in the year before application and for SEA registration within five years before application. Columns 1 and 3 display the actual share of applicants under wage garnishment for those with low and high predicted probabilities, respectively, while columns 2 and 4 present the effects on the seven-year average of annual income. Standard errors are clustered at the applicant level.

Table A12: Weighted DiD Estimations by Initial Employment

	Unweighted sample		**Weighted sample**
	(1)	(2)	(3)
	Initially unemployed	Initially employed	Initially employed
Panel A: Income			
Post × Accepted	-1063.9***	2803.1***	3519.7***
	(251.3)	(335.9)	(478.2)
Constant	3457.7***	27879.3***	26928.6***
	(75.28)	(100.5)	(138.9)
Relative to control mean	-0.308	0.101	0.131
Person FE	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes
Observations	40840	45210	45210
Panel B: Employment			
Post × Accepted	-0.0680***	0.0320***	0.0379***
	(0.0112)	(0.00563)	(0.00751)
Constant	0.237***	0.940***	0.935***
	(0.00335)	(0.00168)	(0.00218)
Relative to control mean	-0.286	0.034	0.040
Person FE	Yes	Yes	Yes
Event-time FE	Yes	Yes	Yes
Observations	40840	45210	45210

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports the effect of being accepted to the debt restructuring program using a static DiD model comparing accepted applicants to a matched group of rejected applicants. The matched control group is constructed by estimating a logit propensity-score model using second-degree polynomials in age and years of education (measured pre-application), a fully interacted set of indicators for gender, cohabitation with a spouse, and the presence of any child under 18 in the household, and three bins for income two years prior to application. Propensity scores are partitioned into ten bins; within each bin and application year, I retain equal numbers of accepted and rejected applicants. The sample is split into two groups: applicants with initial employment and annual labor income above 19,000 USD, and those without initial employment. The first two columns report unweighted estimates from a sample with overlap in the distributions of applicant characteristics. The third column reports the estimate for initially employed applicants re-weighted to match the distribution of applicant characteristics among initially unemployed. **Panel A** reports estimates for the seven-year average of annual labor income (in 2019 USD) and **Panel B** for employment. I control for unit-by-year interactions by using a leave-examiner-out residualization. Standard errors are clustered at the applicant level.

Table A13: Weekly Working Hours

	Weekly working hours	
	(1) Initially employed	(2) Initially unemployed
Accepted	3.962**	-3.703
	(1.444)	(4.092)
Constant	26.53***	27.91***
	(0.825)	(2.647)
Relative to control mean	0.141	-0.149
Control for Year x Unit	Yes	Yes
Observations	4008	1085

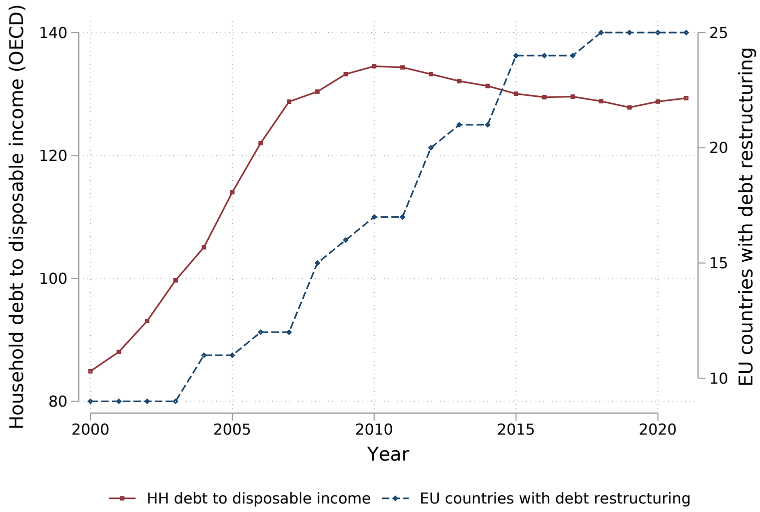
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports coefficients for 2SLS regressions using the past-cases-only instrument. The outcome is the seven-year average of weekly hours worked over the years following the start of the debt restructuring program. The sample is split into two groups: applicants with initial employment and annual labor income above 19,000 USD, and those without initial employment. I control for unit-by-year interactions by using a leave-examiner-out residualization. Standard errors are clustered at the applicant level.

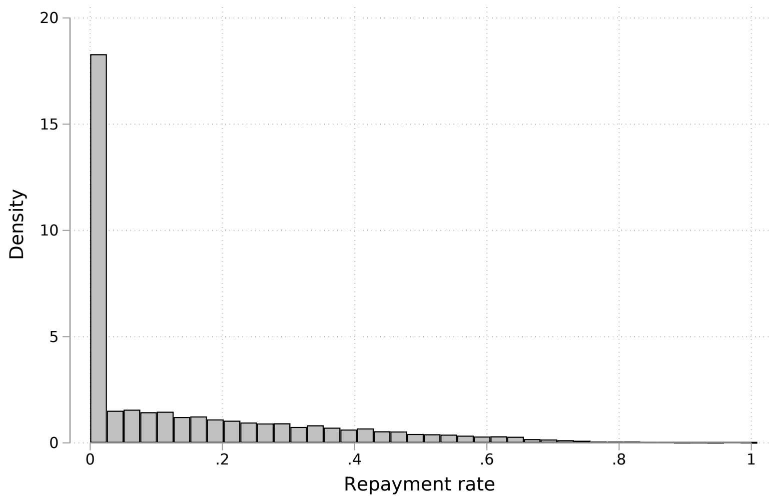
A2 Figures

Figure A1: Household debt and debt restructuring programs



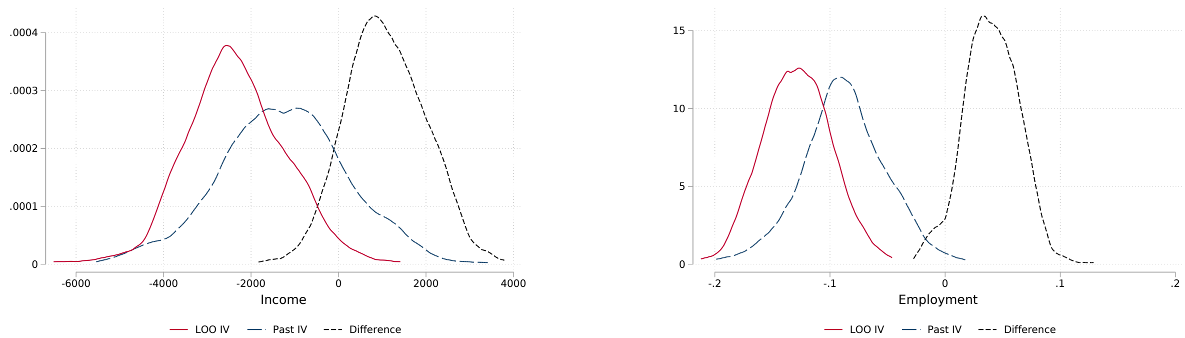
Notes: This figure plots the average ratio of household debt to net disposable income among OECD countries and the number of EU member states which have a household debt restructuring program. Data on household debt are retrieved from `https://www.oecd.org/en/data.html`, and information on the introduction dates of EU countries' debt restructuring programs is sourced from Walter and Krenchel (2021).

Figure A2: Repayment rate of accepted applicants



Notes: This figure presents a histogram of accepted applicants' repayment rates according to their initial repayment plan. The repayment rate is defined as the total debt to be repaid over the program's duration divided by the total debt to be discharged upon program completion.

Figure A3: Bootstrap distributions of 2SLS estimations

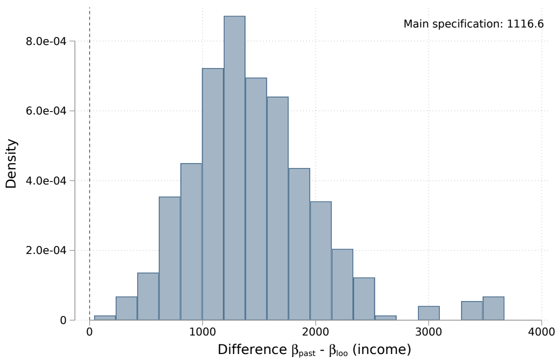


(a) Income

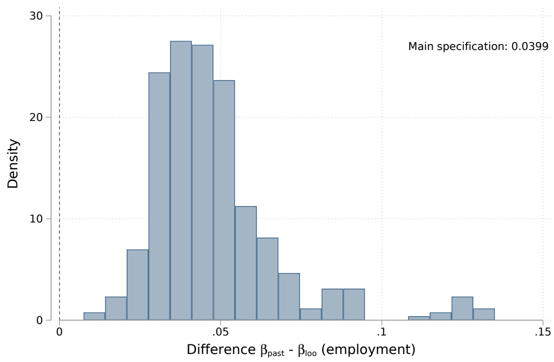
(b) Employment

Notes: This figures present bootstrap distributions of the 2SLS estimates using the leave-one-out IV and the past-cases-only IV, as well as the difference between the two instruments ($\hat{\beta}_{\text{loo}} - \hat{\beta}_{\text{past}}$). The bootstrap uses the exchangeably weighted bootstrap (Praestgaard and Wellner, 1993) clustered at the applicant level with 500 repetitions each. **Panel (a)** shows estimates for annual labor income (in 2019 USD), and **panel (b)** for employment.

Figure A4: Difference in 2SLS Estimates Across Different Specifications



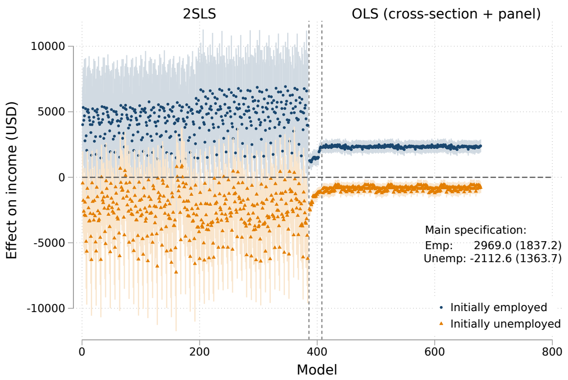
(a) Income



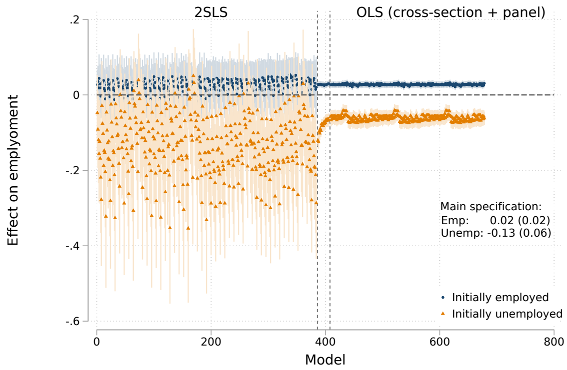
(b) Employment

Notes: This figure reports the difference between estimates obtained using the past-cases-only instrument and the leave-one-out instrument across 384 different specifications. The specifications vary in both the set of controls (initial employment, age, household composition interacted with gender, region of residence, and propensity score; see Section 5.1.2 for details on how I control for these factors) both by year and week of application and in the definition of the instruments (balanced vs. unbalanced samples, imposing age restrictions before vs. after constructing instruments, restricting instruments to be based on at least 10, 25, 50, or 100 observations, and using decisions made 30 or 60 days ago). The differences in the main specification correspond to Table 1. **Panel (a)** reports results for the seven-year average of annual labor income (in 2019 USD), and **Panel (b)** for employment.

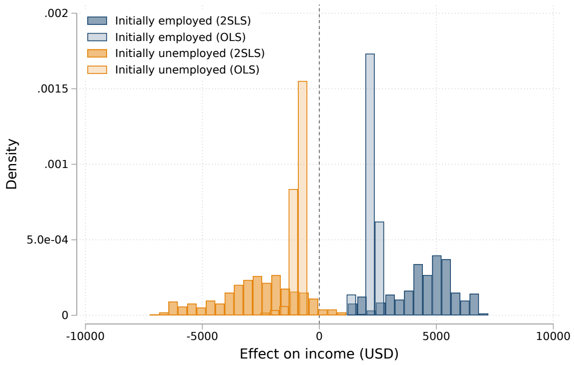
Figure A5: Estimates Across Different Specifications By Initial Employment Status



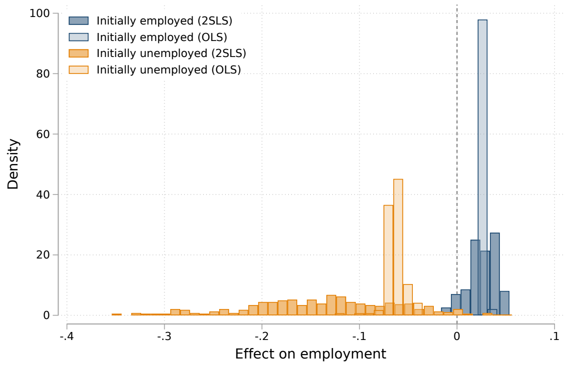
(a) Estimates Across Specifications: Income



(b) Estimates Across Specifications: Employment



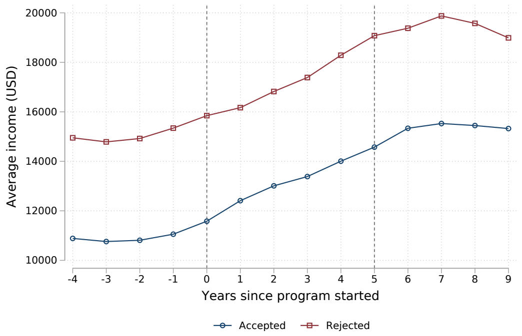
(c) Distribution of Estimates: Income



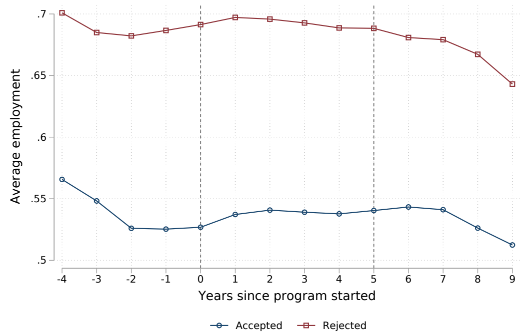
(d) Distribution of Estimates: Employment

Notes: This figure reports the effects of being accepted to the restructuring program across 384 specifications using the past-cases-only instrument, 20 specifications based on a cross-sectional OLS comparison of accepted and rejected applicants, and 270 specifications using a static DiD (panel) estimation comparing accepted applicants to a matched control group of rejected applicants. The sample is split into two groups: applicants with initial employment and annual labor income above 19,000 USD, and those without initial employment. The 2SLS specifications vary in both the set of controls (initial employment, age, household composition interacted with gender, region of residence, and propensity score; see Section 5.1.2 for details) and in the definition of the instruments (balanced vs. unbalanced samples, imposing age restrictions before vs. after constructing instruments, requiring instruments to be based on at least 10, 25, 50, or 100 observations, and using decisions made 30 or 60 days ago). The cross-sectional comparisons control for applicant characteristics based on either a balanced or an unbalanced sample. In the DiD specifications, I estimate a regression of the outcome on a post $\times$ acceptance dummy, conditional on applicant and event-time fixed effects. The matched group is constructed by estimating a logit propensity-score model using varying controls (some specifications include income in $t = -2$, others do not). Propensity scores are partitioned into 2, 10, 30, 50, or 70 bins; within each bin and application year, I retain equal numbers of accepted and rejected applicants The estimates refer to a balanced or unbalanced sample, ranging from 4 years before the application to seven years after. **Panel (a)** reports estimates on the seven-year average of annual labor income (in 2019 USD), and **panel (b)** for employment. Whiskers are 95% confidence intervals. Standard errors are clustered at the applicant level. The first vertical line separates 2SLS estimates from cross-sectional OLS estimates, and the second separates cross-sectional from DiD/panel estimates. **Panel (c)** shows the distribution of all 2SLS and OLS estimates on income and **panel (d)** on employment

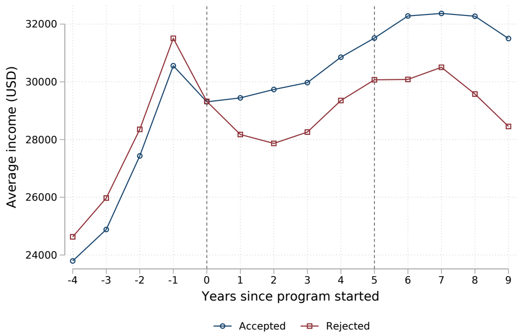
Figure A6: Average income and employment



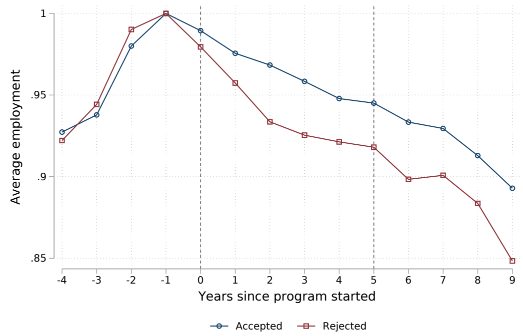
(a) All Applicants: Income



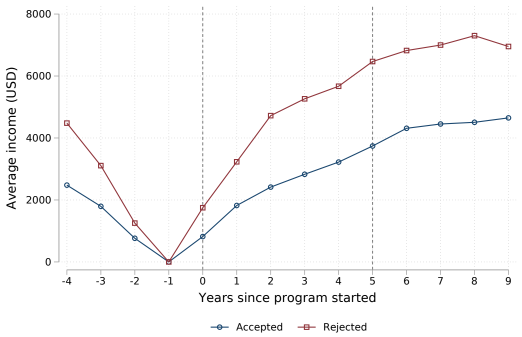
(b) All Applicants: Employment



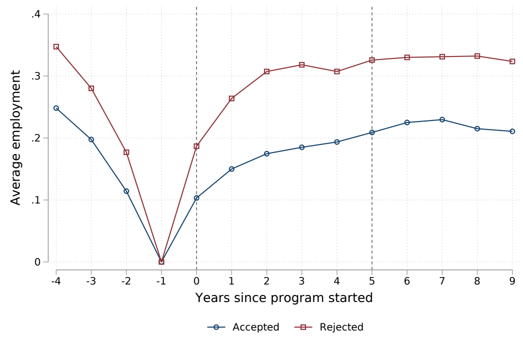
(c) Initially Employed Applicants: Income



(d) Initially Employed Applicants: Employment



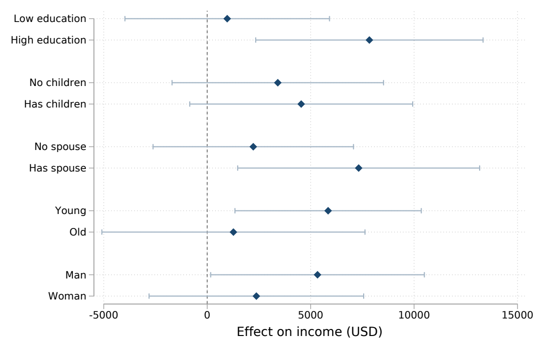
(e) Initially Unemployed Applicants: Income



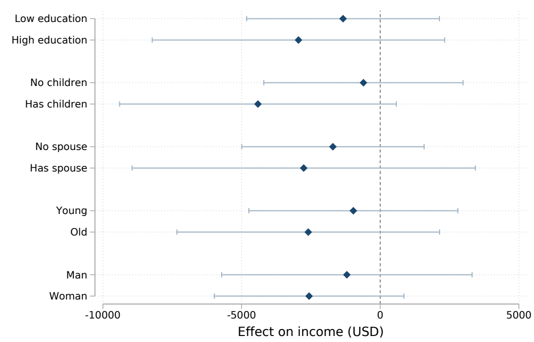
(f) Initially Unemployed Applicants: Employment

Notes: These figures presents average income and employment for accepted and rejected applicants. **Panels (a)** and **(b)** show averages for all applicants, **panels (c)** and **(d)** for applicants with initial employment and annual labor income above 19,000 USD, and **panels (e)** and **(f)** for those without initial employment. The outcomes are annual labor income (in 2019 USD) and an employment indicator for each year.

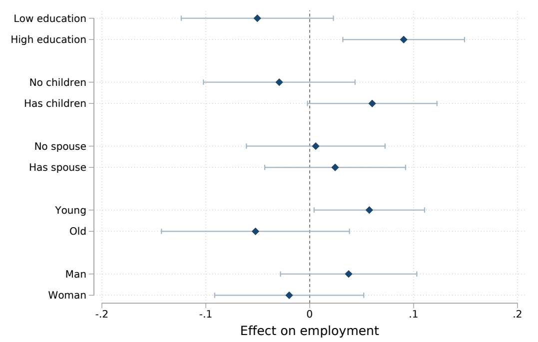
Figure A7: Heterogeneity by Initial Employment Status



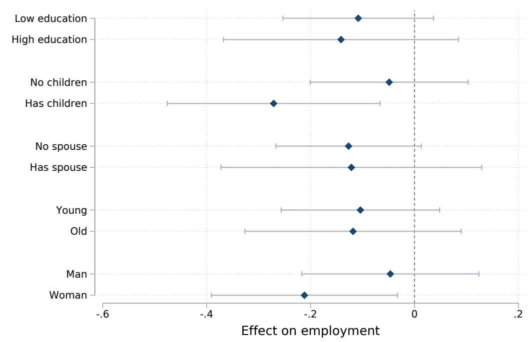
(a) Initially Employed Applicants: Income



(b) Initially Unemployed Applicants: Income



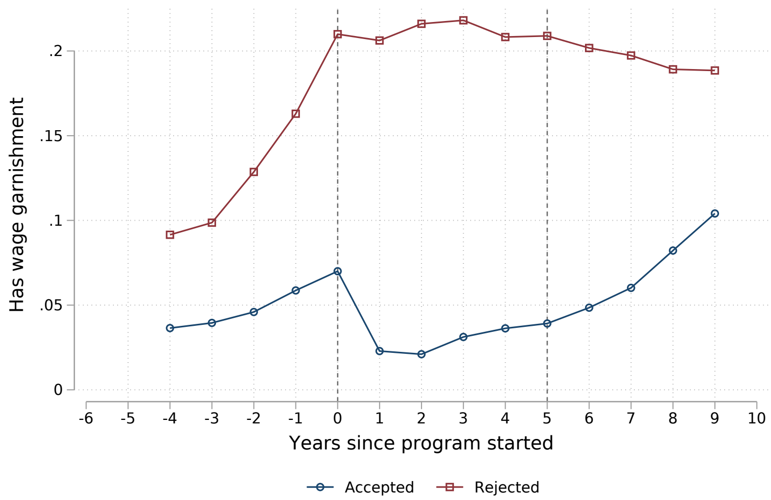
(c) Initially Employed Applicants: Employment



(d) Initially Unemployed Applicants: Employment

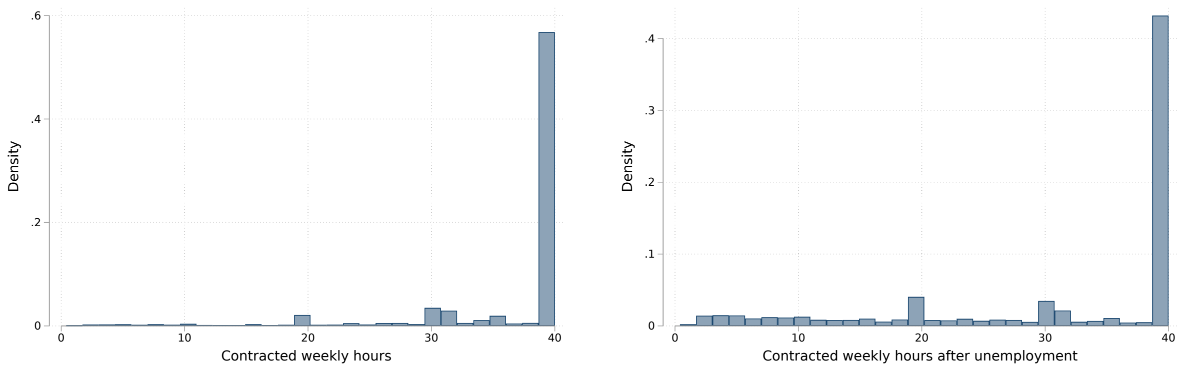
Notes: This figure shows heterogeneity in the effects of acceptance into the debt restructuring program using the past-cases-only instrument. Outcomes are seven-year averages of annual labor income (in 2019 USD) and an indicator for employment in the years following program entry. I split the sample by applicant characteristics measured in the year prior to application: years of schooling (below/above median), presence of children below the age of 18, cohabitation with a spouse, age (below/above median), and gender. **Panel (a)** reports effects on income for initially employed applicants, and **panel (b)** the same for employment. **Panel (c)** plots the effects of income for initially unemployed applicants, and **panel (d)** the same for employment. Whiskers indicate 95% confidence intervals. Standard errors are clustered at the applicant level.

Figure A8: Share of Applicants Facing Wage Garnishment



Notes: This figures plots share of accepted and rejected applicants who are subject to wage garnishment relative to the year of the decision.

Figure A9: Distribution of Weekly Contracted Working Hours

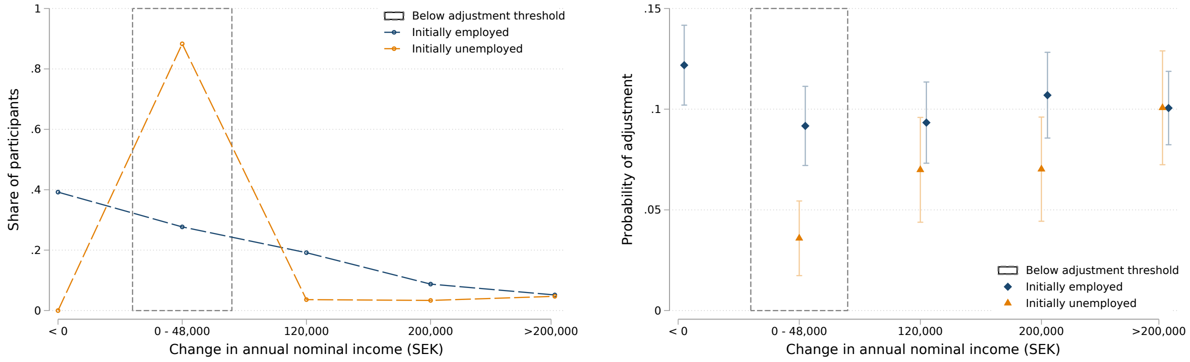


(a) Working Hours

(b) Working Hours After Unemployment

Notes: Panel (a) plots the distribution of contracted weekly working hours for the entire working aged population population between 25 and 65 years captured in the Structure of Earnings Survey. Panel (b) restricts the sample to employees who did not have any employment in the previous year.

Figure A10: Adjustments by Changes in Nominal Annual Income

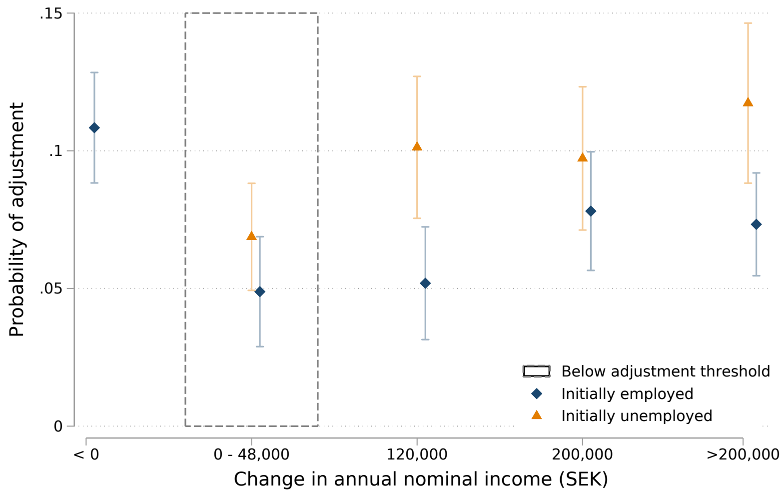


(a) Share of participants

(b) Adjustment probability

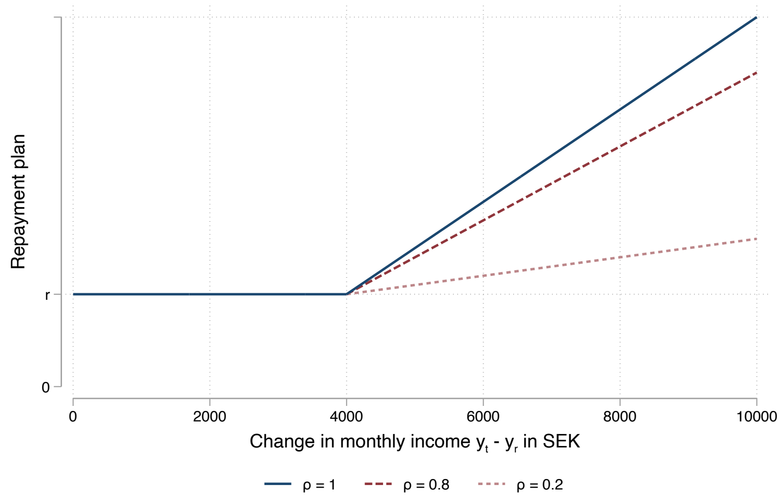
Notes: Panel (a) plots the share of initially employed and unemployed participants within bins of changes in annual nominal (not CPI-adjusted) income relative to initial income. Panel (b) plots the average probability of repayment plan adjustments in a given year for initially employed and unemployed participants against the respective income change bins. The first bin includes all participants with negative income growth relative to initial income. The share of initially unemployed participants is mechanically zero in the first bin. I exclude the respective probability of adjustment for this group. I split the sample into those with initial employment and annual labor income above 19,000 USD and those without initial employment. 95% confidence intervals in panel (a) are based on standard errors clustered at the participant-level. The gray dotted line indicates the threshold below which upward adjustments should typically not occur.

Figure A11: Adjustment Probability Controlling for Initial Repayment Plan



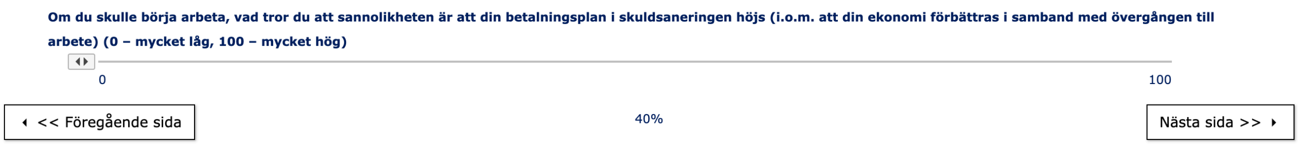
Notes: This figure plots the average probability of repayment plan adjustments in a given year for initially employed and unemployed participants controlling for 10 bins of the level of repayment according to the initial repayment plan against changes in nominal (not CPI-adjusted) annual income changes relative to the income in the year before application similar to Panel (b) in Figure A10. The first bin includes all participants with negative income growth relative to initial income. Since initially unemployed participants do not have any income, I exclude the respective probability for this group. 95% confidence intervals are based on standard errors clustered at the participant-level.

Figure A12: Repayment for Changes in Income



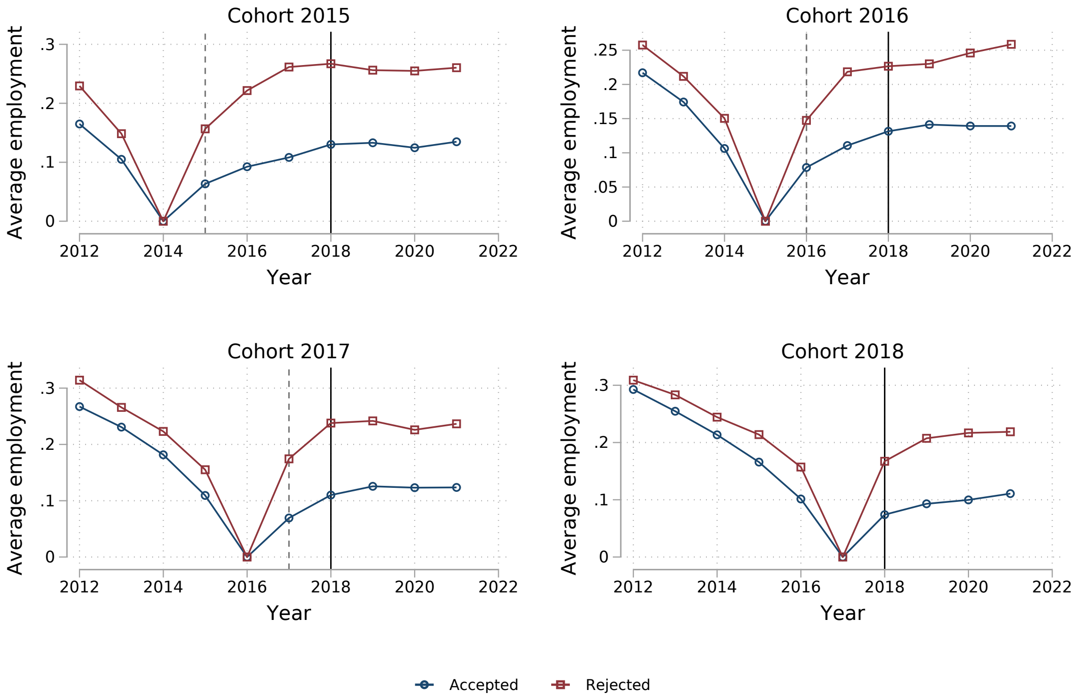
Notes: The graphs plots the repayment amount for different changes in income (in SEK) relative to the reference income and different marginal adjustment rates ρ as in equation (17).

Figure A13: Survey Question on Perceived Probability of Repayment Plan Adjustments



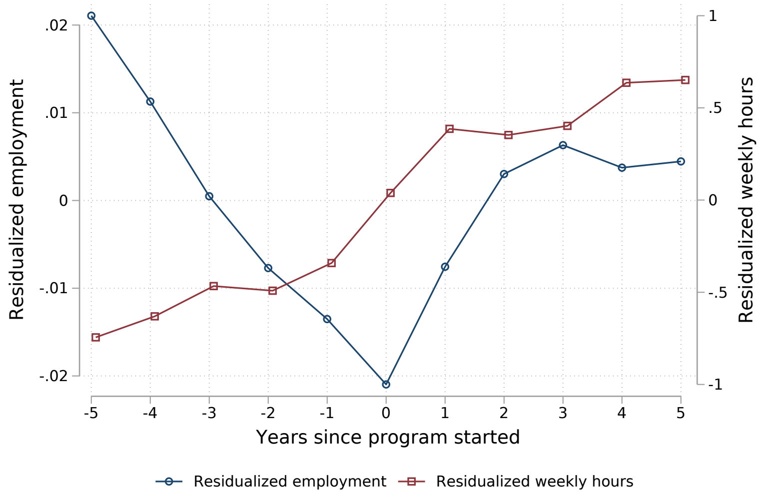
Notes: The figure presents the survey question on the perceived probability of upward adjustments following substantial increases in income in the original Swedish.

Figure A14: Average Employment for Initially Unemployed and Different Cohorts



Notes: This figure plots average employment for accepted and rejected initially unemployed applicants to the debt restructuring program. Each panel corresponds to an application cohort from 2015 to 2018. The dashed gray vertical line represents the respective year of application, while the solid black vertical line marks 2018, the year the General Data Protection Regulation (GDPR) was introduced, limiting employer access to credit default information in recruitment..

Figure A15: Average Intensive and Extensive Margin Labor Supply



Notes: This graphs plots average residualized employment and weekly contracted hours for all applicants in the five years before and after the year of application. I residualize both outcomes using applicant fixed effects. Since hours are only observed for applicants who work and who are covered by the Structure of Earnings Survey, I only include applicants who appear at least once before the year of application and once after the year of application.

B Bias in examiner IV designs - theory and evidence

B.1 Theoretical extensions

B.1.1 Asymptotic bias with many controls

Many empirical applications of examiner IV designs require the inclusion of some controls for identification. In the following section, I will show how, similar to Kolesár (2013), this can reintroduce the correlation between unobserved case characteristics U_{jt} and future decisions D_{jk} even when using only past cases as instrument. Kolesár (2013) shows that including many controls can lead to inconsistent estimates in standard jackknife IV estimations. Intuitively, although we exclude endogenous cases from our instrument, they may still be reflected in different values of the instrument elsewhere in the data. Control variables can capture these endogenous cases, thereby contaminating the instrument.

Let $W_{jt} \in \mathcal{W}$ be some arbitrary discrete group to which we assign observation (j, t). For simplicity, I assume that W_{jt} is orthogonal to all decisions and therefore irrelevant from an identification point of view

$$\mathbf{E}[W_{ik} D_{jt}] = 0 \text{ for all } k \in \mathcal{T} \text{ and all } i \in \mathcal{J} . \tag{A.6}$$

In empirical applications W_{jt} could, for example, refer to court-by-year fixed effects. Let N_W be the number of groups, N_J the number of examiners in each group, and N_T the number of cases for a given group and examiner. To simplify notation I assume that N_J and N_T are equal across all groups and examiners. Let $\tilde{Z}_{jt} = Z_{jt} - \frac{1}{N_J \times N_T} \sum_i \sum_s Z_{is}$ denote the instrument residualized by group fixed effects.[3] The past-cases-only IV estimator can then be written as

$$\tilde{\beta}_{\text{past}} = \frac{\sum_j \sum_t \tilde{Z}_{jt}^{\text{past}} Y_{jt}}{\sum_j \sum_t \tilde{Z}_{jt}^{\text{past}} D_{jt}}. \tag{19}$$

To derive the asymptotic bias, I follow Kolesár (2013) and let only the number of groups N_W grow to infinity while keeping the number of examiners within each group N_J and the number of cases within each examiner-group N_T fixed. Then as $N_W \to \infty$

$$\tilde{\beta}_{\text{past}} \xrightarrow{p} \beta - \frac{\alpha}{N_J} \frac{\sum_t \frac{1}{N_T} \sum_s \frac{1}{s-1} \sum_{l \in \mathcal{P}_s} \mathbf{E}\left[D_{jl} U_{jt}\right]}{\sum_t \mathbf{E}\left[\tilde{Z}_{jt}^{\text{past}} D_{jt}\right]}. \tag{20}$$

By averaging over all other instruments within a fixed effects group in the residualization step, we may capture other observations whose instrument is constructed from current or future

[3] For brevity the index i in the first sum refers to all examiners within the same groups such that $W_{ik} = W_{jt}$ for at least one k, including examiner j. The second sum sums over all N_T cases s for which a given examiner falls into this group.

decisions of the same examiner. These decisions are exactly those that we want to omit because of their correlation with the regression error. A simple solution to this issue is to use a leave-examiner-out residualization instead $\bar{Z}_{jt} = Z_{jt} - \frac{1}{(N_J-1)\times N_T}\sum_{i\neq j}\sum_s Z_{is}$. This ensures that the instrument is residualized using only observations exogenous to the respective examiner's decisions. Then, the corresponding estimator will be able to recover the true causal effect

$$\bar{\beta}_{\text{past}} = \frac{\sum_j \sum_t \bar{Z}^{\text{past}}_{jt} Y_{jt}}{\sum_j \sum_t \bar{Z}^{\text{past}}_{jt} D_{jt}} \xrightarrow{p} \beta. \tag{21}$$

B.1.2 Heterogeneous treatment effects

It is a well-known result that, with treatment effect heterogeneity, 2SLS regressions do not necessarily identify average treatment effects of the underlying population, but weighted averages of compliers and, potentially, defiers (Imbens and Angrist, 1994). Differences in estimates between the leave-one-out IV and the past-cases-only IV can in such settings therefore arise both due of biases in the estimate as well as different weights attached to each observation. To model treatment effect heterogeneity, I allow β to be a function of unobserved case characteristics and impose the assumption that conditional on case characteristics, treatment effects are independent from all decisions and therefore the instruments.

$$\beta(U_{jt}) = \mathbf{E}[\beta \mid U_{jt}] \text{ such that } \beta(U_{jt}) \perp\!\!\!\perp D_{jk} \mid U_{jt} \text{ for all } k \in \mathcal{T}. \tag{A.4}$$

The respective probability limits of the estimators are given by

$$\hat{\beta}_{\text{loo}} \xrightarrow{p} \mathbf{E}[\omega_l(U_{jt})\beta(U_{jt})] + \alpha \frac{\sum_t \sum_{k\in\mathcal{P}_t} \mathbf{E}\left[D_{jk}U_{jt}\right]}{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt}\right]} \tag{22}$$

and

$$\hat{\beta}_{\text{past}} \xrightarrow{p} \mathbf{E}[\omega_p(U_{jt})\beta(U_{jt})]. \tag{23}$$

If we let U_{jt} follow a discrete support $\mathcal{U}$ the weights are

$$\omega_l(u) = \frac{\sum_t \frac{1}{T-1}\sum_{k\neq t} Pr(U_{jt}=u)\mathbf{E}[D_{jk}D_{jt} \mid U_{jt}=u]}{\sum_{w\in\mathcal{U}}\sum_t \frac{1}{T-1}\sum_{k\neq t} Pr(U_{jt}=w)\mathbf{E}[D_{jk}D_{jt} \mid U_{jt}=w]} \tag{24}$$

and

$$\omega_p(u) = \frac{\sum_t \frac{1}{t-1}\sum_{k<t} Pr(U_{jt}=u)\mathbf{E}[D_{jk}D_{jt} \mid U_{jt}=u]}{\sum_{w\in\mathcal{U}}\sum_t \frac{1}{t-1}\sum_{k<t} Pr(U_{jt}=w)\mathbf{E}[D_{jk}D_{jt} \mid U_{jt}=w]}. \tag{25}$$

The main difference is that the weight in the leave-one-out estimation sums over all past and future decisions while the weight in the past-cases-only regressions naturally only sums over past cases. Systematic differences between the two weights therefore depend on how the conditional expectation $\mathbf{E}[D_{jt}D_{jk} \mid U_{jt}]$ evolves over time within examiner. The average decision used in ω_l will by construction capture later cases than the average decision in ω_p. In an examiner decision model such as (2) where both examiner leniency and the mapping between leniency and case characteristics to decisions are constant over time, both weights will yield identical results. If, for example, examiner leniency grows over time such that $\mathbf{E}[D_{jt}D_{jt-p} \mid U_{jt}] \neq \mathbf{E}[D_{jt}D_{jt+p} \mid U_{jt}]$ and these differences are systematically related to $\beta(U_{jt})$, then comparisons between the two estimators can capture both the bias and differences in weighting.

A minimal requirement that is often stated for weighted treatment effects to be interpreted causally is that the estimate should represent a convex combination of underlying treatment effects (see e.g. de Chaisemartain and D'Haultfœuille, 2020). To ensure non-negative weights for every observation, we can impose the following monotonicity assumption (see also Frandesen et al., 2023).

$$\mathbf{E}[Z_{jt}^{\text{past}} D_{jt} \mid U_{jt}] \geq 0. \tag{A.5}$$

This condition implies that, for any level of characteristics U_{jt}, uptake of the program increases in leniency. The regression weights will then be zero for all always-takers and never-takers for who treatment status does not vary with leniency. Compliers will be weighted proportional to how strongly their treatment status reacts to changes in leniency of assigned examiners.

B.2 Derivation of of technical results

B.2.1 Bias leave-one-out IV without controls

The standard leave-one-out 2SLS estimator is given by

$$\hat{\beta}_{\text{loo}} = \frac{\sum_j \sum_t Z_{jt}^{\text{loo}} Y_{jt}}{\sum_j \sum_t Z_{jt}^{\text{loo}} D_{jt}} = \frac{\sum_j \sum_t \frac{1}{T-1} \sum_{k \neq t} D_{jk} Y_{jt}}{\sum_j \sum_t \frac{1}{T-1} \sum_{k \neq t} D_{jk} D_{jt}} \tag{26}$$

where the second step follows from the definition of the leave-one-out instrument (6). Then if $A_j \equiv \frac{1}{T} \sum_t \frac{1}{T-1} \sum_{k \neq t} D_{jk} Y_{jt}$ and $B_j \equiv \frac{1}{T} \sum_t \frac{1}{T-1} \sum_{k \neq t} D_{jk} D_{jt}$ are iid and we let the number of examiners grow to infinity $J \to \infty$

$$\begin{aligned}
\hat{\beta}_{\text{loo}} \xrightarrow{p} \frac{\mathbf{E}\left[\sum_t \frac{1}{T-1} \sum_{k \neq t} D_{jk} Y_{jt}\right]}{\mathbf{E}\left[\sum_t \frac{1}{T-1} \sum_{k \neq t} D_{jk} D_{jt}\right]} &= \frac{\mathbf{E}\left[\sum_t \sum_{k \neq t} D_{jk} Y_{jt}\right]}{\mathbf{E}\left[\sum_t \sum_{k \neq t} D_{jk} D_{jt}\right]} \\
= \beta + \alpha \frac{\sum_t \sum_{k \neq t} \mathbf{E}\left[D_{jk} U_{jt}\right]}{\sum_t \sum_{k \neq t} \mathbf{E}\left[D_{jk} D_{jt}\right]} &= \beta + \alpha \frac{\sum_t \sum_{k \in \mathcal{P}_t} \mathbf{E}\left[D_{jk} U_{jt}\right]}{\sum_t \sum_{k \neq t} \mathbf{E}\left[D_{jk} D_{jt}\right]}
\end{aligned} \tag{27}$$

The third step follows from the structural model (3) and assumption (A.1) the last step from assumption (A.2).

B.2.2 Regression weights with heterogeneous treatment effects

If we allow β to vary with case characteristics U_{jt}, then similar to (27)

$$
\begin{aligned}
\hat{\beta}_{\text{loo}} &\xrightarrow{p} \frac{\mathbf{E}\left[\sum_t \frac{1}{T-1}\sum_{k\neq t} D_{jk}Y_{jt}\right]}{\mathbf{E}\left[\sum_t \frac{1}{T-1}\sum_{k\neq t} D_{jk}D_{jt}\right]} \\
&= \frac{\mathbf{E}\left[\sum_t \sum_{k\neq t} D_{jk}Y_{jt}\right]}{\mathbf{E}\left[\sum_t \sum_{k\neq t} D_{jk}D_{jt}\right]} \\
&= \frac{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt}\beta\right]}{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt}\right]} + \alpha\frac{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}U_{jt}\right]}{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt}\right]} \\
&= \frac{\sum_t \sum_{k\neq t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt}\beta \mid U_{jt}\right]\right]}{\sum_t \sum_{k\neq t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\right]} + \alpha\frac{\sum_t \sum_{k\in\mathcal{P}_t} \mathbf{E}\left[D_{jk}U_{jt}\right]}{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt}\right]} \\
&= \frac{\sum_t \sum_{k\neq t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\beta(U_{jt})\right]}{\sum_t \sum_{k\neq t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\right]} + \alpha\frac{\sum_t \sum_{k\in\mathcal{P}_t} \mathbf{E}\left[D_{jk}U_{jt}\right]}{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt}\right]} \\
&= \mathbf{E}\left[\frac{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\beta(U_{jt})}{\sum_t \sum_{k\neq t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\right]}\right] + \alpha\frac{\sum_t \sum_{k\in\mathcal{P}_t} \mathbf{E}\left[D_{jk}U_{jt}\right]}{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt}\right]} \\
&= \sum_{u\in\mathcal{U}} \frac{\sum_t \sum_{k\neq t} Pr(U_{jt}=u)\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}=u\right]}{\sum_{w\in\mathcal{U}}\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}=w\right]}\beta(u) + \alpha\frac{\sum_t \sum_{k\in\mathcal{P}_t} \mathbf{E}\left[D_{jk}U_{jt}\right]}{\sum_t \sum_{k\neq t} \mathbf{E}\left[D_{jk}D_{jt}\right]}
\end{aligned}
\tag{28}
$$

where the third step follows from the law of iterated expectations and the fourth step follows from assumption (A.4). We then assume that U_{jt} has discrete support $\mathcal{U}$ in the sixth step. Similarly, we can derive the weights for the past-cases-only estimator

$$
\begin{aligned}
\hat{\beta}_{\text{past}} &\xrightarrow{p} \frac{\mathbf{E}\left[\sum_t \frac{1}{t-1}\sum_{k<t} D_{jk}Y_{jt}\right]}{\mathbf{E}\left[\sum_t \frac{1}{t-1}\sum_{k<tt} D_{jk}D_{jt}\right]} \\
&= \frac{\mathbf{E}\left[\sum_t \frac{1}{t-1}\sum_{k<t} D_{jk}Y_{jt}\right]}{\mathbf{E}\left[\sum_t \frac{1}{t-1}\frac{1}{t-1}\sum_{k<t} D_{jk}D_{jt}\right]} \\
&= \frac{\sum_t \frac{1}{t-1}\sum_{k<t} \mathbf{E}\left[D_{jk}D_{jt}\beta\right]}{\sum_t \frac{1}{t-1}\sum_{k<t} \mathbf{E}\left[D_{jk}D_{jt}\right]} \\
&= \frac{\sum_t \frac{1}{t-1}\sum_{k<t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt}\beta \mid U_{jt}\right]\right]}{\sum_t \frac{1}{t-1}\sum_{k<t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\right]} \\
&= \frac{\sum_t \frac{1}{t-1}\sum_{k<t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\beta(U_{jt})\right]}{\sum_t \frac{1}{t-1}\sum_{k<t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\right]} \\
&= \mathbf{E}\left[\frac{\sum_t \frac{1}{t-1}\sum_{k<t} \mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\beta(U_{jt})}{\sum_t \frac{1}{t-1}\sum_{k<t} \mathbf{E}\left[\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}\right]\right]}\right] \\
&= \sum_{u\in\mathcal{U}} \frac{\sum_t \frac{1}{t-1}\sum_{k<t} Pr(U_{jt}=u)\mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}=u\right]}{\sum_{w\in\mathcal{U}}\sum_t \frac{1}{t-1}\sum_{k<t} \mathbf{E}\left[D_{jk}D_{jt} \mid U_{jt}=w\right]}\beta(u)
\end{aligned}
\tag{29}
$$

B.2.3 Bias past IV with controls

When we use the instrument derived from past-cases-only (9) with fixed effects W, then the estimator can be written as

$$
\begin{aligned}
\tilde{\beta}_{\text{past}} &= \frac{\sum_j \sum_t \tilde{Z}_{jt}^{\text{past}} Y_{jt}}{\sum_j \sum_t \tilde{Z}_{jt}^{\text{past}} D_{jt}} \\
&= \frac{\sum_j \sum_t \left(\frac{1}{t-1}\sum_{k<t} D_{jk} - \frac{1}{N_J \times N_T}\sum_i \sum_s \frac{1}{s-1}\sum_{l<s} D_{il}\right) Y_{jt}}{\sum_j \sum_t \tilde{Z}_{jt}^{\text{past}} D_{jt}}.
\end{aligned}
\tag{30}
$$

Then as the number of controls increases keeping the number of examiners within fixed effects group and the number of cases within examiner - fixed effect group fixed we can derive the bias.

$$\tilde{\beta}_{\text{past}} \xrightarrow{p} \beta + \alpha \frac{\mathbf{E}\left[\sum_t \left(\frac{1}{t-1}\sum_{k<t} D_{jk} - \frac{1}{N_J \times N_T}\sum_i \sum_s \frac{1}{s-1}\sum_{l<s} D_{il}\right) U_{jt}\right]}{\mathbf{E}\left[\sum_t \tilde{Z}^{\text{past}}_{jt} D_{jt}\right]} \tag{31}$$
$$= \beta + \alpha \frac{\sum_t \left[\sum_{k<t} \mathbf{E}\left[D_{jk}U_{jt}\right] - \frac{1}{N_J \times N_T}\left(\sum_{i \neq j}\sum_s \frac{1}{s-1}\sum_{l<s}\mathbf{E}\left[D_{il}U_{jt}\right] + \sum_s \frac{1}{s-1}\sum_{l<s}\mathbf{E}\left[D_{jl}U_{jt}\right]\right)\right]}{\sum_t \mathbf{E}\left[\tilde{Z}^{\text{past}}_{jt} D_{jt}\right]}$$
$$= \beta - \frac{\alpha}{N_J \times N_T} \frac{\sum_t \sum_s \frac{1}{s-1}\sum_{l \in \mathcal{P}_s}\mathbf{E}\left[D_{jl}U_{jt}\right]}{\sum_t \mathbf{E}\left[\tilde{Z}^{\text{past}}_{jt} D_{jt}\right]}$$

where the last step follows from assumptions (A.2) and (A.6).

B.2.4 Asymptotic bias with exogenous controls for leave-one-out estimator

Similarly to (31) we can derive the bias in the classical leave-one-out 2SLS estimator when using controls as above

$$\bar{\beta}_{\text{loo}} \xrightarrow{p} \beta + \alpha \frac{\mathbf{E}\left[\sum_t \left(\frac{1}{T-1}\sum_{k \neq t} D_{jk} - \frac{1}{N_J \times N_T}\sum_i \sum_s \frac{1}{T-1}\sum_{l \neq s} D_{il}\right) U_{jt}\right]}{\mathbf{E}\left[\sum_t \tilde{Z}^{\text{loo}}_{jt} D_{jt}\right]} \tag{32}$$
$$= \beta + \alpha \frac{\mathbf{E}\left[\sum_t \left(\frac{1}{T-1}\sum_{k \in \mathcal{P}_t} D_{jk} - \frac{1}{N_J \times N_T}\sum_s \frac{1}{T-1}\sum_{l \in \mathcal{P}_s} D_{il}\right) U_{jt}\right]}{\mathbf{E}\left[\sum_t \tilde{Z}^{\text{loo}}_{jt} D_{jt}\right]}.$$

B.3 Monte Carlo simulation

In this section, I simulate an examiner decision model and evaluate how the bias in the 2SLS estimation using the leave-one-out leniency instrument Z^{loo}_{jt} varies with different parameters of the underlying model. I assume that examiners make binary decisions based on the case characteristics U_{jt}, the examiner's leniency, L_{jt}, and six lags of past case characteristics.

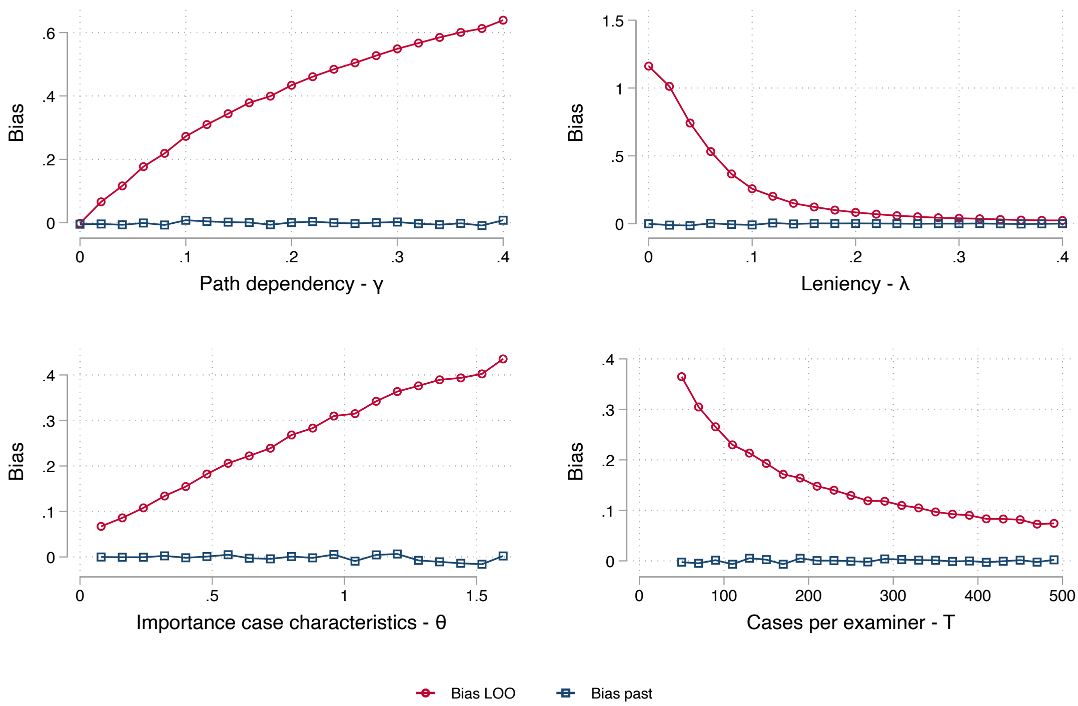
$$D_{jt} = \mathbf{1}\{\theta U_{jt} + \lambda L_j + \gamma \sum_{k=1}^{6} 0.5^k \times U_{jt-k} \geq 0\}. \tag{33}$$

The outcome is determined by the structural outcome model (3) such that

$$Y_{jt} = \beta D_{jt} + \alpha U_{jt} + \nu_{jt}. \tag{34}$$

All random variables L_j, U_{jt}, and ν_{jt} follow an iid $\mathcal{N}(0,1)$ distribution. I set the true causal parameter of interest β and the influence of case characteristics on the outcome α to 1. The initial decision parameters are given by $\theta = 0.8$, $\lambda = 0.1$, and $\gamma = 0.1$ reflecting that case characteristics are likely to play a substantially more important role in decision formation than either examiner leniency or past cases. I set the number of examiners J to 5,000 and the number of cases per examiner T to 110. I then estimate the leave-one-out and past-cases-only instruments and restrict the sample to observations for which the instrument is based on at least 10 observations. That is, I drop the first 10 cases and keep 100 cases per examiner. Figure B1 shows the bias for both instruments, varying each one of the parameters $\{\theta,\ \lambda,\ \gamma,\ T\}$ while keeping the others fixed. Unsurprisingly, the bias induced by the leave-one-out instrument increases in the importance of lagged case characteristics γ and - given a fixed number of lags - decreases in the number of cases each examiner decides over. Perhaps surprisingly, however, the bias increases in the importance of current case characteristics θ and decreases in the importance of examiners' leniency λ. The reason for this is that the ratio of θ to λ governs the strength of the first stage. Since the bias is determined by the size of violations of the exclusion restriction scaled by the first stage, decreasing the latter while keeping the former fixed or letting it decrease at a slower rate, will exacerbate the bias in the leave-one-out estimation. In the past-cases-only estimation the exclusion restriction is not violated such that the size of the first stage does not matter for the bias in expectations, given that the relevance condition holds.

Figure B1: Results from Monte Carlo simulation



Notes: This figure shows the bias for the leave-one-out 2SLS regression and the 2SLS regression using past-cases-only from on a Monte Carlo simulation based on the decision model (33). Each panel varies one of the three decision parameters in (33) or the number of cases per examiner and keeps the other three parameters fixed. Each point is the average estimate for 150 regressions.

B.4 Empirical evidence - debt restructuring

In the following section, I provide empirical evidence that the difference in estimates between the leave-one-out and the past-cases-only estimates in Table 1 are driven by path dependency in decision-making among examiners at the Swedish Enforcement Agency (SEA).

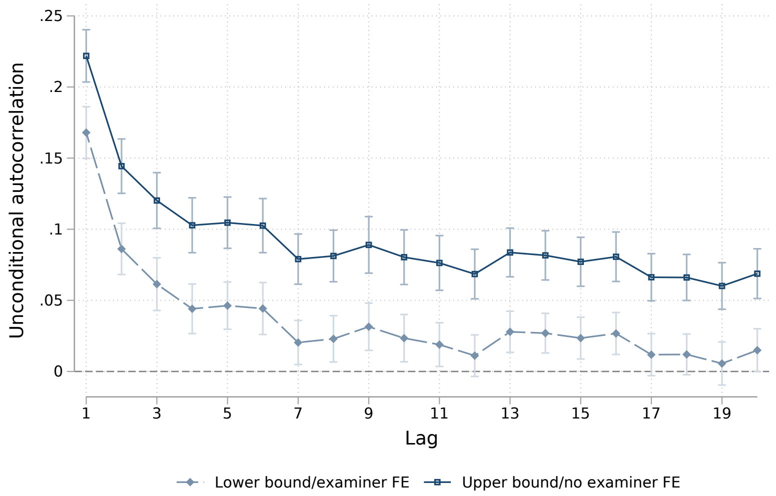
B.4.1 Unconditional autocorrelation in examiner decisions

An examiner decision-making model with path dependency as in model (2) implies autocorrelation among decisions within an examiner. I test for this by regressing an examiner's current decision on its n^{th} lag, conditional on date-of-application fixed effects for up to 20 lags. Since the bias is driven by the *unconditional* correlation between current decisions and past case characteristics, I estimate each regression individually. Decisions will be serially correlated due to time-invariant differences in examiner leniency. This will result in an upward bias in my estimates. Therefore, in a second step, I de-mean each decision by the average of the respective examiner's decisions and estimate autocorrelations between the residuals. This will result in a downward bias of the estimates (Nickell, 1981). Figure B2 plots the respective upper and lower bounds of unconditional autocorrelations in examiners' decisions and reveals substantial autocorrelation for up to 20 lags. Note that this can be a result of path-dependent decision-making in which current cases depend on past case characteristics, but might also be due to exogenous changes in examiner leniency that are innocuous from an identification perspective. The positive autocorrelation is furthermore consistent with the downward bias that we observe empirically.

B.4.2 Difference in first-stage estimates

Table B1 presents the first stage regression of decisions on the two instruments. Both instruments are strongly correlated with the case decision, with Kleibergen and Paap (2006) F-statistics of 991 for the leave-one-out instrument and 641 for the past-cases-only instrument. The leave-one-out leniency IV appears to be the better predictor. The reason for this could be higher precision, since leniency here is estimated from more cases than the past-cases-only instrument. An alternative or complementary explanation is that the leave-one-out instrument predicts more accurately by including potentially endogenous information. To evaluate these two channels, I repeatedly construct leave-one-out instruments using the same number of cases as the respective past-cases-only instrument for each observation, and then re-estimate the first stage using the leave-one-out instrument in a bootstrap procedure with 300 iterations. The average coefficient is 0.685 and the average F-statistic is 795.2, which implies that 44 percent of the gap in explanatory power between the two instruments can be explained by the endogeneity of the leave-one-out instrument.

Figure B2: Unconditional autocorrelation in examiner decisions



Notes: This figure presents upper and lower bounds with their respective 95% confidence intervals for unconditional autocorrelations in examiners decisions conditional on the date of application. To estimate the upper bound, I regress the current decision on each of its lags individually conditional on date-of-application fixed effects. Due to serial correlation from time-constant differences in examiner leniency, this approach introduces an upward bias relative to the true effect. To estimate the lower bound, I first de-mean each decision by subtracting the examiner-specific average, using examiner fixed effects, and then apply the same procedure. This will lead to a downward bias relative to the true effect (Nickell, 1981). Standard errors are clustered at the examiner level.

Table B1: First stage

	(1) Accepted	(2) Accepted
Leniency leave-one-out	0.797***	
	(0.0253)	
Leniency past-cases-only		0.504***
		(0.0199)
Control for Year x Unit	Yes	Yes
F-Stat.	991	641
Observations	16642	16642

Standard errors in parentheses

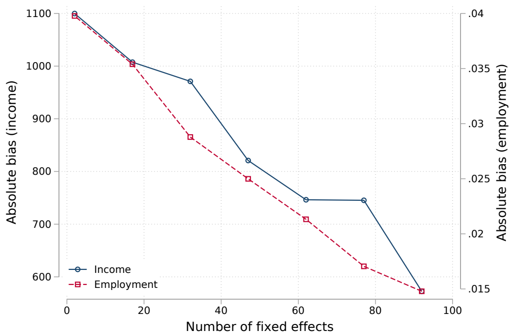
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table reports coefficients and F-Statistics from regressions of the case decision on the leave-one-out instrument and the past-cases-only instrument. I control for unit-by-year interactions by using a leave-examiner-out residualization. Standard errors are clustered at the applicant level.

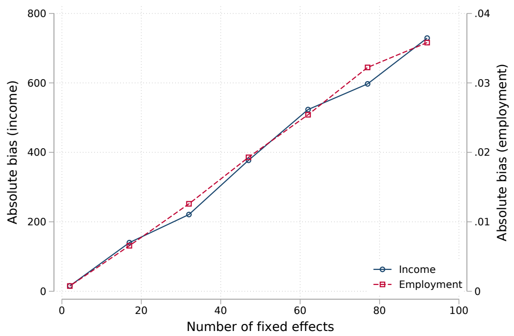
B.4.3 Many controls bias

Next, I evaluate the bias caused by adding fixed effects. I randomly assign examiners to artificial fixed-effect groups. I then estimate both the 2SLS regression using the leave-one-out leniency IV and the past-cases-only leniency IV, including fixed effects, and benchmark these against estimates of the past-cases-only IV using leave-examiner-out residualization for each fixed effect group. I successively increase the number of fixed-effect groups from 2 to 92, so that each group contains around 2 examiners in the final estimation. I repeat this procedure 500 times for each fixed effect group size and report the absolute average difference between the respective estimator and the residualized past-cases-only instrument. Panel (a) of Appendix Figure B3 plots the differences between the leave-one-out estimator with fixed effects and the leave-examiner-out residualized estimator using past-cases-only $|\hat{\beta}_{\text{loo}} - \bar{\beta}_{\text{past}}|$ for the two main outcomes, income and employment. We can interpret this as the absolute bias of the leave-one-out estimation. The two biases from path dependency and many controls go in opposing directions.[4] Increasing the many controls bias reduces the overall bias for both outcomes. When we compare the two estimators based on past cases only, including many fixed effects unambiguously increases the overall bias. Corresponding results are plotted in panel (b) of Figure B3.

Figure B3: Simulate many controls bias



(a) $|\hat{\beta}_{\text{loo}} - \bar{\beta}_{\text{past}}|$



(b) $|\hat{\beta}_{\text{past}} - \bar{\beta}_{\text{past}}|$

Notes: These figures presents the average absolute distances between the leave-one-out estimator with group fixed effects and the past-cases-only estimator using a leave-examiner-out residualization $\hat{\beta}_{\text{loo}} - \bar{\beta}_{\text{past}}$ and the past-cases-only estimator using fixed effects and the residualized past-cases-only estimator $\hat{\beta}_{\text{past}} - \bar{\beta}_{\text{past}}$. For each number of fixed effects, I randomly assign examiners into fixed effects groups 500 times and report the average estimate for those draws. The outcomes are average income over the next seven years in SEK in 2019 prices and average probability of having any employment over the next seven years after the debt restructuring program begins.

[4]This can be seen from evaluating both biases individually, as in (23) and (31). Appendix B.2 equation (32) also explicitly derives the leave-one-out bias with many controls.

B.5 Bias in examiner IV - application to inventor mobility

A growing body of literature uses the quasi-random allocation of patent examiners to patent applications at the US Patent and Trademark Office (USPTO) to estimate the treatment effects of patent approvals (Galasso and Schankerman, 2015; Gaulé, 2018; Farre-Mensa et al., 2019; Sampat and Williams, 2019; Melero et al., 2020). In the following section, I show that using the past-cases-only instrument instead of a leave-one-out instrument can substantially affect estimates of inventors' likelihood of remaining at their current firm. Melero et al. (2020) provide a theoretical framework in which gaining a monopoly through a patent approval increases firms' incentives to retain inventors. This, in turn, can limit inventor mobility and harm the diffusion of knowledge. Empirically, they instrument the number of patents an inventor holds with the average leave-one-out measure of patent examiner leniency over all patent applications of the given inventor. In this setting, an additional patent lowers the probability of changing employers by 2.3 percentage points. I use a specification with a binary indicator for patent approval as the endogenous variable which is identical to the specification in the econometric set-up in section 3. Using the leave-one-out instrument, I find that receiving a patent decreases the probability of working at the previous firm when applying for the next patent by 3 percentage points. This estimate more than doubles to a decrease of 6.6 percentage points when using the past-cases-only instrument. Relative to the OLS bias, this corresponds to a difference of 113%.

B.6 Data

I obtain data on all patent applications from 2001 - 2019 from the USPTO.[5] These data contain information on the application date, if granted the date of filing, the respective examiner, the examiner's art unit group, and the applicant's name. I infer employer names from the assignment data keeping only those assignments that are marked as an "employer assign" by the USPTO. My main outcome is a dummy indicating that the inventor works at the same company in their next application as they do in their current application. This automatically drops all inventors with only one application. I harmonize employer names to some extent.[6] Nevertheless, the mobility measure is likely to overestimate mobility due to differences in company names' spelling e.g. because of typos or changes in their legal structure. This should not affect estimates if the measurement error in mobility is unrelated to examiner leniency. I follow the literature and assume that cases are randomly assigned to examiners within art unit. I therefore residualize the acceptance variable with the leave-examiner-out average acceptance rate

[5] Data on patent examiners are only available from 2001 onward. I drop all applications after 2019 since the patent application process can take several years. See https://www.uspto.gov/ip-policy/economic-research/research-datasets/patent-assignment-dataset

[6] E.g. I drop special characters or terms that refer to the legal structure such as "INC" from firm company names. These can contribute to small differences in spelling.

within the same art unit and year

$$D_{jt} = D_{jt} - \frac{1}{N_{A(j)} - N_j} \sum_{i:A(i)=A(j);\ i \neq j} D_{jt}. \tag{35}$$

The instruments are then constructed as averages over the residualized decisions. It can take several years until a patent is granted. This makes the issue of contemporaneity of cases more severe than in the debt restructuring setting. The average time between application and approval in my sample is 2.89 years. Around 99% of all patents are approved within 8 years. I therefore define the past-cases-only instrument as the average acceptance rate for all applications which were filed until the 8th calendar year before the respective application. I further ensure that all instruments are estimated using more than 10 cases. This leaves me with a sample of around 2.5 million patent applications and 8,884 examiners. The average acceptance rate is 84.9%. The average (median) leave-one-out instrument is estimated using 644 (488) other cases and the average (median) past cases instrument is estimated using 153 (182) cases.

B.7 Results

Table B2 shows the OLS and 2SLS estimates for the two instruments. The OLS and 2SLS estimate based on the leave-one-out instrument are very similar, with -0.034 and -0.030 respectively. The past-cases-only estimator is substantially lower with -0.0657. Taking the past-cases-only estimator as ground truth, the bias of the leave-one-out estimator relative to the OLS bias corresponds to 113%.

	(1) OLS	(2) Leave-one-out	(3) Past-cases-only
Granted	-0.0342***	-0.0302*	-0.0657***
	(0.00123)	(0.0126)	(0.0184)
Constant	0.257***	0.253***	0.284***
	(0.00117)	(0.0108)	(0.0156)
Control Art Unit x Year		Yes	Yes
FS F-stat.		1398	632
Observations	2548909	2548909	2548909

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B2: OLS and 2SLS estimations

Notes: This table shows coefficients for the OLS regressions and 2SLS regressions for each of the two instruments based on leave-one-out estimates of examiner leniency and past-cases-only. The outcome is the probability of working for the same employer at the time of the next patent application. I control for art unit times year by using fixed effects in the OLS regression and a leave-examiner out residualization of decision variables before aggregating them up to the instruments. I report the first stage F-statistics for the two 2SLS regressions. Standard errors are clustered at the applicant level.

C Complier characteristics

I follow the approach outlined in Dahl et al. (2014). Under strict monotonicity, compilers are defined as those applicants who would have received treatment if assigned to an examiner with leniency $\bar{z}$, but would have been rejected if assigned to an examiner with leniency $\underline{z}$. The respective share of compliers corresponds to

$$\pi_C = \Pr(D_{jt} = 1 \mid \tilde{Z}_{jt}^{\text{past}} = \bar{z}) - \Pr(D_{jt} = 0 \mid \tilde{Z}_{jt}^{\text{past}} = \underline{z}) = \Pr(D_{jt}(\bar{z}) > D_{jt}(\underline{z})) \tag{36}$$

where $\tilde{Z}_{jt}^{\text{past}}$ is the residualized past-cases-only instrument and $D_{jt}(z)$ refers to the potential outcome for treatment at instrument value z. The shares of always-takers and never-takers who are accepted or rejected, respectively, regardless of the leniency of their examiner are given by

$$\pi_A = \Pr(D_{jt} = 1 \mid \tilde{Z}_{jt}^{\text{past}} = \underline{z}) = \Pr(D_{jt}(\bar{z}) = D_{jt}(\underline{z}) = 1) \tag{37}$$

and

$$\pi_N = \Pr(D_{jt} = 0 \mid \tilde{Z}_{jt}^{\text{past}} = \bar{z}) = \Pr(D_{jt}(\bar{z}) = D_{jt}(\underline{z}) = 0). \tag{38}$$

For a linear probability model of the first stage, these shares can be estimated as

$$\hat{\pi}_C = \hat{\eta}_1(\bar{z} - \underline{z}), \tag{39}$$

$$\hat{\pi}_A = \hat{\eta}_0 + \hat{\eta}_1\underline{z}, \tag{40}$$

and

$$\hat{\pi}_N = 1 - \hat{\eta}_0 - \hat{\eta}_1\bar{z} \tag{41}$$

where $\hat{\eta}_0$ is the estimate of the first-stage regression's constant and $\hat{\eta}_1$ the estimated coefficient of the instrument on treatment status. I define $\bar{z}$ as the 99th percentile and $\underline{z}$ as the 1st percentile of the leniency distribution. The resulting shares of compliers, always-takers, and never-takers are $\hat{\pi}_C = 0.467$, $\hat{\pi}_A = 0.353$, and $\hat{\pi}_N = 0.18$, respectively. Following Abadie (2003) and Bruze et al. (2024), the distribution of a binary characteristic X_{jt} among compliers can be expressed as

$$\Pr(X_{jt} \mid D_{jt}(\bar{z}) > D_{jt}(\underline{z})) = \frac{E\left[D_{jt} \mid \tilde{Z}_{jt}^{\text{past}} = \bar{z}, X_{jt} = 1\right] - E\left[D_{jt} \mid \tilde{Z}_{jt}^{\text{past}} = \underline{z}, X_{jt} = 1\right]}{E\left[D_{jt} \mid \tilde{Z}_{jt}^{\text{past}} = \bar{z}\right] - E\left[D_{jt} \mid \tilde{Z}_{jt}^{\text{past}} = \underline{z}\right]} \times \Pr(X_{jt}). \tag{42}$$

I estimate the nominator, by estimating the first stage coefficients η_0^X and η_1^X separately by characteristics. The resulting shares are reported in Table A2.

D Labor supply moral hazard and probabilistic adjustments

D.1 Labor supply moral hazard

Changes in the adjustment rate may not only result in different incentives for the initially unemployed, but can also lead to moral hazard for employed applicants. If the repayment plan is based on initial income and is either not adjusted or only minimally adjusted in response to increases in income, then this can create incentives for applicants to lower their debt repayment by working less or not at all before applying. I benchmark the expected utility of working fewer than 35 hours per week against the expected utility of working 35 hours[7] and define expected utility as

$$\mathbf{E}[u\left(h_{-1}; D, \rho\right)] = p_D \times u\left(h_{-1}; D = 1, \rho\right) + (1 - p_D) \times u\left(h_{-1}; D = 0, \rho\right) \tag{43}$$

where $p_D = \Pr(D = 1)$ is the probability of being accepted to the debt restructuring program. The relationship between initial income and probability of acceptance is not straightforward. On the one side, applicants are more likely to be accepted if the ability to repay their debt is smaller and hence lower initial incomes can increase p. On the other side, applicants are expected to have made their best effort to repay. Voluntarily decreasing labor supply might harm an applicant's chances of being accepted. Figure D1 therefore plots expected utility for different values of initial hours and adjustment rates ρ for four values of acceptance probability ranging from 0.2 to 0.8.[8] Only relatively low adjustment rates generate incentives for applicants to strategically withdraw from employment to reduce future repayment obligations. The results therefore suggest that lowering adjustment rates is unlikely to induce additional labor-supply–based moral hazard among applicants.[9]

[7] Observed hours can of course also be driven by moral hazard given the existing program. Appendix Figure A15 plots average residualised extensive and intensive labor supply of all applicants in the years leading up to the application. Extensive labor supply falls by 4 percentage points in the five years before application which could be both the reason why debtors apply to the program as well as moral hazard. However, weekly hours increase by around 0.15 suggesting that moral hazard on the intensive margin is unlikely to matter under the given system.

[8] Note that while the scale of expected utility is irrelevant, the relative differences between different initial hours indicate how easily an applicant is shifted between states.

[9] The calibration does not target the pre-application labor supply margin. As a result, the model predicts an optimal choice of 30 rather than 35 hours when $\rho = 1$. The utility difference between these two choices is small relative to the difference between working full time and not working at all.

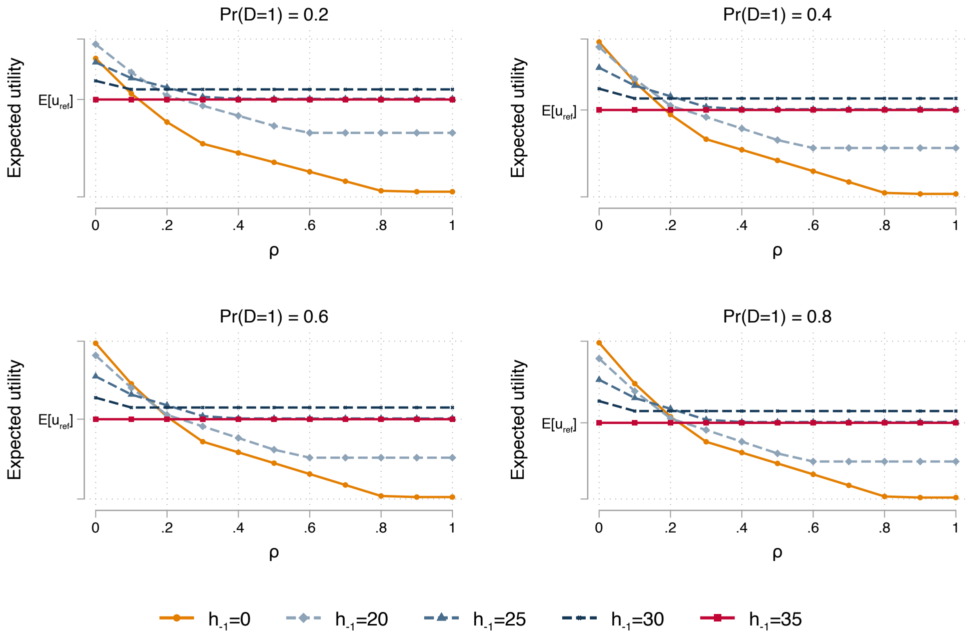


Figure D1: Expected utility by adjustment rates and probabilities of acceptance

Notes: Each of the four panels plot expected utility of different values of initial weekly hours and different adjustment rates ρ as in equation (17). Expected utility is defined over the two states of being accepted to the debt restructuring program and being rejected. The four panels differ in the assumed probability of acceptances which is given by 20% in the upper left panel, 40% in the upper right panel, 60% in the lower left panel, and 80% in the lower right panel.

D.2 Probabilistic adjustments of repayment plan

In the baseline model, I assume that the participant's repayment plan will be adjusted deterministically to sufficiently large increases in income. In an extension to the model, I allow for probabilistic adjustments after large increases in labor supply in each period, with probability p_A. Participants correctly perceive this probability and maximize their expected utility. The lowest adjustment probability p_A for which the unemployed participant will choose to remain unemployed is 81 percent, which is close to the average perceived adjustment probability reported in Figure 6. Figure D2 replicates Figure 8c from the main text by plotting the additional debtor consumption, repayments to creditors, and government revenue over 7 years by different adjustment probabilities. Lower probabilities of adjustment result in lower effective values of adjustment rates ρ and therefore increase debtors labor supply for higher values of ρ compared to the baseline.

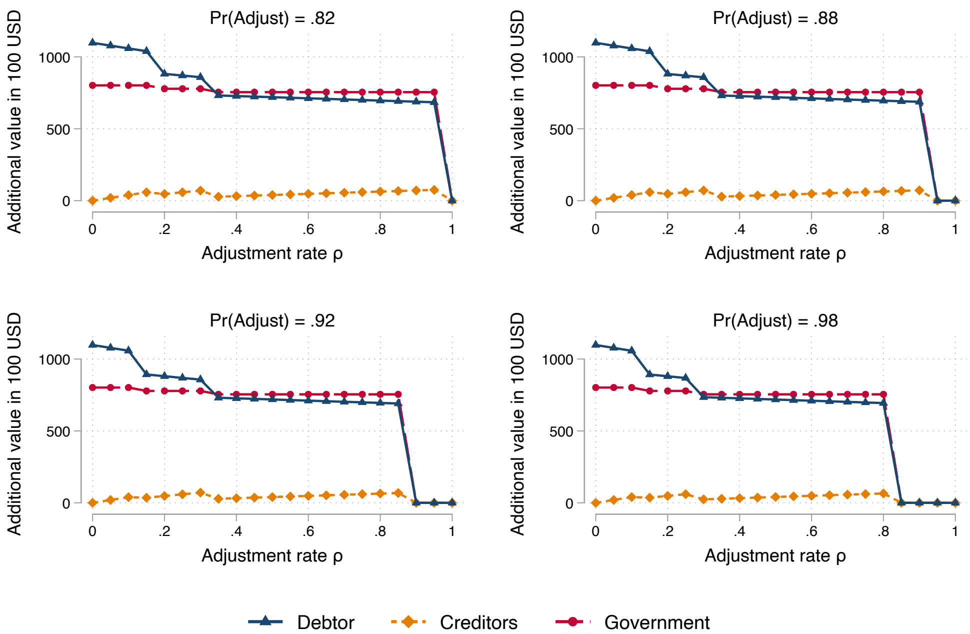


Figure D2: Additional value by adjustment rates and probabilities of adjustment

Notes: This figure shows the additional value in 100 USD over seven years for different marginal adjustment rates, ρ, as specified in equation (17). The blue solid line plots additional consumption for debtors, the dashed orange line plots additional repayment to creditors, and the dashed red line additional government revenue from lower social benefits payed out and higher labor taxes. All outcomes are for accepted applicants who are initially unemployed.

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