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Debt relief for the financially vulnerable: impact on employment, welfare receipt, and mental health

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ABSTRACT

Debt Relief for the Financially Vulnerable: Impact on Employment, Welfare Receipt, and Mental Health*

We study the labor market and mental health impacts of debt relief among financially vulnerable individuals. We exploit a cutoff rule used by a Dutch welfare office to determine eligibility to debt relief of welfare debts. We use this cutoff as an instrument in both a fuzzy regression discontinuity and instrumented difference-in-difference design. With administrative data, we estimate economically small and insignificant effects of debt relief on employment, earnings, welfare receipt, and medication use for mental health problems. Subgroup analyses suggest that debt relief increases employment among debtors with larger welfare debts. The larger amount of debt relief for this group has probably a stronger potential to improve their overall debt position.

JEL Classification: G51, I38, J22, J64, J68

Keywords: debt relief, welfare debts, welfare recipients, fuzzy regression discontinuity design, instrumented difference-in-difference

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1 Introduction

A growing concern in developed countries is the significant number of households facing problematic debts. In the United States, more than a quarter of households have debts in collections (Martinchek et al., 2022). In the United Kingdom and the Netherlands, 16% and 8% of households, respectively, are grappling with (registered) problematic debts (Dias, 2023; Statistics Netherlands, 2022). These figures highlight the magnitude of the issue and raise concerns, given that problematic debts can have adverse effects on both labor market participation and mental health (Dobbie and Song, 2015; Gathergood, 2012). In practice, many households facing problematic debts seek for debt relief at some point in time.[1] In the US, for instance, bankruptcy alone enables about a million households to discharge over $100 billion in debt annually (Indarte, 2023).[2] Moreover, approximately 10% of US households has filed for bankruptcy at some point in their lives (Keys, 2018). Given the prevalence of debt relief, an increasing number of studies have investigated its impact on debtor outcomes (Dobbie and Song, 2015, 2020; Ganong and Noel, 2020).

In this paper, we investigate the impact of debt relief on labor market outcomes, welfare receipt, and mental health among financially vulnerable Dutch debtors. The relevant debt relief intervention was implemented by a major Dutch welfare office in December 2019. It provided debt relief for welfare debts that had not been adequately interrupted during the five-year prescription period. The debt relief of welfare debts not only directly affected debtors' wealth position and potentially reduced monthly repayments, but also largely eliminated implicit marginal repayment taxes on earnings. From a theoretical perspective, the debt relief intervention generates ambiguous effects on employment and welfare receipt. After debt relief, people can retain a greater amount of money for every hour they work and this may stimulate them to resume work (substitution effect). However, improved wealth and net

[1] Debt relief can take several forms, such as defaults (bankruptcy or delinquency) or modifications (e.g., through credit counseling, debt settlement, or government debt relief) (Indarte, 2022).

[2] In Germany, around 575,000 households sought assistance from debt counseling centers in 2021, with 100,000 households filing for private insolvency (Destatis, 2023a,b). Similarly, in the Netherlands, approximately 75,000 households seek help at debt counseling centers each year, while around 20,000 households receive debt relief through government organizations (NVVK, 2023).

disposable income may decrease employment and increase welfare receipt through income effects (Cesarini et al., 2017; Imbens et al., 2001; Chetty, 2008). Alongside this, recent literature suggests that debt relief reduces financial stress, leading to improved mental health and more effective job search efforts (Dobbie and Song, 2015, 2020; Ong et al., 2019; Sergeyev et al., 2023).

This paper presents empirical evidence on the impact of a modest debt relief intervention. The debt relief intervention directly reduced the balance of welfare debt by almost 4,000 Euros, compared to ex-ante average balances of welfare debts of about 6,000 Euros. The amount of debt relief corresponds to approximately four months of welfare benefits for a single-person household or about eight years of repayments (based on an average monthly repayment of 40 Euros). This treatment intensity is similar to the debt relief intervention studied by Dobbie and Song (2020).[3] At the same time, our study context differs from previous work in this field of research. First, we investigate effects on employment and health outcomes, while most debt relief studies have focused on financial outcomes (Gross et al., 2020, 2021; Han and Li, 2011; Ganong and Noel, 2020; Cespedes et al., 2021; Agarwal et al., 2017, 2023). Only a few studies have investigated the effects of debt relief on other life domains, such as labor market outcomes and mortality (Dobbie and Song, 2015, 2020). Second, our study focuses on financially vulnerable individuals of whom the majority is non-employed (83%) and receives welfare benefits (75%). These individuals have typically been receiving welfare benefits for many years and are unlikely to pay off their welfare debts in the long run. This contrasts with the existing literature, which has predominantly focused on samples where the majority was employed.[4] Therefore, our study offers a novel perspective on the use and usefulness of debt relief among financially vulnerable individuals, where reductions in benefit payments may offset the costs of debt relief.

We use detailed individual-level administrative data on the debt relief intervention, along with monthly data on welfare debts, welfare receipt, employment, and

[3] Dobbie and Song (2020) have examined the effects of delayed interest write-downs among financially distressed credit card borrowers. The maximum interest write-down was for borrowers in the treatment group about $ 4,300 higher than for those in the control group.

[4] In the studies of Dobbie and Song (2015) and Dobbie and Song (2020), more than 80% already had paid work pre-treatment.

earnings. We also collect annual data on medication use for mental health problems to estimate treatment effects on (yearly) mental health. In order to estimate causal effects, we exploit an unannounced cutoff rule that relies on the date of origin of welfare debts and which is used by the welfare office to determine eligibility for debt relief. We do so in a fuzzy regression discontinuity (fuzzy RD) design, as well as with an instrumented difference-in-differences (DDIV) design. Both methods use the same sample and yield similar estimates, with the DDIV estimator being more efficient.

Our study provides two key findings. First, debt relief has a significant and long-lasting impact on the balance of welfare debts and the amount of monthly repayments. Debt relief reduces the balance of welfare debt mechanically by almost 4,000 Euros, and this effect remains stable over the following two years. The debt relief leads to a reduction in monthly repayments by about 17 Euros (41.7%) in the first year and 12 Euros (30.1%) in the first two years. Second, debt relief does not have a significant impact on employment status, earnings, welfare receipt, and mental health medication use among the entire sample of debtors. Given these null-results, the debt relief intervention is not beneficial from a public finance perspective. We argue that this lack of effects can most likely be attributed to the ongoing presence of debt-related issues with other creditors. A considerable proportion of the treated debtors continue to face one or more debt-related problems – e.g., benefit garnishment or registered problematic debts – after receiving debt relief. Additional subgroup analyses suggest that debt relief increases employment among debtors initially facing a severe debt position. Specifically, debt relief increases employment with 4.1 percentage points (22.0%) among debtors facing large welfare debts (>4,000 Euros), materializing in the second year after treatment. Similarly, debt relief increases employment by 2.7 percentage points (13.8%) among debtors with registered problematic debts. However, the positive effect on employment for these subgroups does not translate into changes in earnings or welfare receipt. This suggests that these positive effects on employment primarily stems from increases in part-time employment in addition to receipt of welfare benefits.

This paper contributes to the existing literature on the impact of debt relief on

non-financial debtor outcomes in several ways. First, there is a scarcity of research on the effect of debt relief on employment, with only two studies identified (Dobbie and Song, 2015, 2020). The findings of Dobbie and Song (2020) align remarkably with our findings: debt relief has a significantly positive effect on employment only for individuals with relatively large debts (compared to income), and a relief of around 4,000 euros increases the probability of being employed by approximately 4 percentage points. This is striking as our study focuses on individuals in a more financially vulnerable position as compared to the study of Dobbie and Song (2020). Second, our study is the first to examine the causal impact of debt relief on benefit receipt and mental health. While prior evidence from a small-scale 'pre-post' study with 196 participants, based on survey data, suggests that debt relief improves mental health (Ong et al., 2019), our results do not provide causal evidence of the effect of debt relief on mental health medication use.

Our study also contributes to a broader strand of literature on the impact of wealth (shocks) on labor market outcomes, welfare receipt, and mental health. Consistent with standard economic theory, previous studies have shown that positive wealth shocks reduce employment and earnings (Cesarini et al., 2017; Imbens et al., 2001), increase the duration of benefit receipt (Chetty, 2008), and improve mental health outcomes (Adams et al., 2022). Our study distinguishes from these studies by studying debt relief instead of a more general positive wealth shock. Regarding negative wealth shocks and (problematic) debts, existing literature shows positive effects on mental health problems and mental health medication use (Gathergood, 2012; Schwandt, 2018; Fichera and Gathergood, 2016; McInerney et al., 2013). However, our study does not find evidence that debt relief reduces medication use for mental health problems.

This paper proceeds as follows. Section 2 provides an overview of the Dutch system of welfare benefits and debts and discusses the debt relief intervention. In Section 3, we describe the data and summary statistics. In Section 4, we discuss the empirical strategy. Section 5 presents the estimates of the impact of debt relief. Section 6 concludes.

2 Institutional context

2.1 The Dutch system of welfare benefits and debts

In the Netherlands, welfare benefits are provided as a non-contributory transfer scheme for unemployed individuals who have exhausted all other forms of support. Welfare benefits are granted based on eligibility criteria that consider household income and assets. As ongoing benefit conditions, individuals have to comply with administrative requirements and job search requirements. 25% of net earnings of welfare recipients – up to a maximum of 215 Euros per month – are exempted from benefit reductions.[5] For additional earnings, benefits are reduced on a one-to-one basis. Welfare recipients may also be eligible for housing, healthcare, and child allowances. In cases of large unexpected expenses, welfare recipients (and individuals with slightly higher income levels) may be eligible for supplementary welfare, which can be provided as a one-time payment or a loan. Decisions on the provision of these payments and loans are at the discretion of local caseworkers. The national government sets the modalities for the provision of (additional) welfare, while municipalities are responsible for processing applications, disbursing benefits, monitoring compliance, reclaiming unjustified payments, and recovering welfare debts.

Welfare debts typically arise from benefit overpayments and welfare loans. Benefit overpayments occur when welfare recipients receive benefits to which they are not entitled. This may be caused by violations of administrative requirements, such as not reporting paid work, assets, or changes in living situation.[6] Administrative errors and delays in processing information that affect benefit entitlement, level, or duration can also lead to overpayments. Welfare loans include loans for consumer durables, revolving mortgages, and loans for working capital or subsistence for self-employed.[7]

[5] Since April 2021, the city of Rotterdam provides a work bonus for welfare recipients with additional income from work. The work bonus is 12.5% of their net earnings with a maximum of 215 Euros per month and is paid twice a year.

[6] Violations of administrative requirements can range from deliberate benefit fraud to unintentional errors made by welfare recipients. In cases of erroneous payment of benefits, welfare recipients are obligated to repay the amount received in error and may also be subject to a fine imposed on top of the repayment. Fines range between 25% and 150% of the overpayment depending on the level of imputability and the recipient's repayment capacity.

[7] In addition to benefit overpayments and welfare loans, a small part of welfare debts may arise

We collect data from the welfare office of Rotterdam. Rotterdam is the second-largest city in the Netherlands. As of December 2019, the city of Rotterdam provided welfare to 40,474 welfare recipients and recovered welfare debts from 31,217 debtors, which includes both current and former welfare recipients. In comparison, all Dutch municipalities combined had 453,565 welfare recipients and 221,272 welfare debtors. Table 1 provides an overview of the main characteristics of welfare debts among debtors in both the Netherlands and the city of Rotterdam. Welfare debts are substantial in the Netherlands, with an average debt of about 6,000 Euros, while monthly repayments are typically small, on average about 55 Euros. More than half of all debtors did not make monthly repayments. As a result, welfare debts are often prolonged, with 77.6% of debtors having an outstanding debt for more than one year and 47.5% of debtors having debts outstanding for more than three years.

The municipality sets the procedure for recovery and repayment of welfare debts, which depends on the type of debt and the financial situation of the debtor. Here, we discuss the procedure followed by the city of Rotterdam.[8] Benefit overpayments are repaid, if possible, at once or by settlement with current benefit payments. If this is not feasible, the welfare office proposes a repayment plan. The monthly repayment amount is set at 10% of the applicable welfare benefit norm plus 35% of any additional income. Accordingly, debtors face a (short-term) implicit tax rate of 35% on any additional income. Monthly repayments for welfare loans follow a similar procedure. Following from national regulations, the amount of repayment cannot exceed the difference between the debtor's income and the necessary available funds, which is calculated as 95% of the welfare norm. Hence, monthly repayments for debtors receiving welfare are typically around 50 Euros (5% of their benefit level) or equal to zero (if the available funds are inadequate). Their monthly repayment is deducted from the debtor's welfare benefit payment. If debtors face debts at multiple creditors, repaying debts at other government organizations has priority over paying welfare debts which has subsequently priority over paying private debts. This also

from claims due to recourse against maintenance debtors on behalf of a child or ex-partner and recovery from inheritance or gifts.

[8]See Article 9, Beleidsregels opschorting, intrekking en terug- en invordering Participatiewet, IOAW en IOAZ Rotterdam 2017, https://lokaleregelgeving.overheid.nl/CVDR432303/1.

Table 1: Key characteristics of welfare debts among debtors at 1-12-2019: The Netherlands vs. Rotterdam

	Rotterdam	The Netherlands
	(1)	(2)
Balance of welfare debts (in Euros)	4,576	6,103
<250 Euros	0.119	0.122
250-1,000 Euros	0.250	0.232
1,000-2,500 Euros	0.247	0.227
2,500-10,000 Euros	0.272	0.279
>10,000 Euros	0.112	0.140
Duration oldest debt (in months)	58	53
>3 months	0.930	0.919
>1 year	0.794	0.776
>3 years	0.512	0.475
Repayment (in Euros)	47	55
0 Euros	0.607	0.577
0-25 Euros	0.029	0.043
25-100 Euros	0.302	0.288
>100 Euros	0.061	0.091
Number of individuals	31,217	221,272

explains why more than half of all debtors did not make monthly repayments (see Table 1).

Welfare debt relief is possible under certain conditions. First, the creditor is required to interrupt the five-year period of prescription (time-barring) to retain the right of collection for a new five-year period. This can be done with an act of interruption, such as sending a detailed overview of the outstanding debts. If a welfare debt has not been adequately interrupted, the debt should be written-off and can no longer be collected. The evaluated intervention in this paper pertains to this type of debt relief. Second, welfare debts can be relieved as part of a multilateral debt settlement or restructuring, in which all outstanding debts of the debtor at other creditors are included.[9] These debt services are typically provided by local governments.

[9]Benefit overpayments arising from violations of administrative requirements are excluded from these debt services (see Article 60c of the Dutch Participation Act). Since 2021, this exclusion rule has been mitigated.

2.2 Debt relief intervention

This paper evaluates the effects of a debt relief intervention that was implemented by the welfare offices of the city of Rotterdam. The intervention provides debt relief for welfare debts in accordance with a decision made by the Central Appeals Court for Public Service and Social Security Matters (CRvB) in a specific case. Below, we discuss the specifics of the CRvB's ruling, the implications for a sizable group of debtors with outstanding welfare debts, the eligibility criteria for debt relief, and the communication of the debt relief decision to the affected debtors.

On April 29^{th} of 2019, the Central Appeals Court for Public Service and Social Security Matters (CRvB) concluded that the welfare office of Rotterdam had not adequately interrupted the prescription period of a welfare debt owed by an individual debtor. Consequently, the debt was time-barred, had to be written off, and could no longer be collected. The ruling also applied to similar cases for which the municipality of Rotterdam did not adequately interrupt welfare debts. Between April and December 2019, the welfare office assessed whether the welfare debts of other debtors were time-barred and therefore eligible for full debt relief.

Eligibility for debt relief was based on the date of origin of the debt, with debts that originated before January 1, 2013, being time-barred.[10] In our regression discontinuity design, we exploit this cutoff rule (see Section 4.1). However, some types of debt were excluded from the relief, including fines, revolving mortgages, revolving claims, single outstanding debts with repayments in the past five years, and debts under the authority of a specific debt recovery department.[11] The welfare debt was written-off in December 2019 and the welfare office communicated the debt relief decision to the eligible debtors on December 19, 2019, via a letter.[12] Given the limited amount of monthly repayments, the relieved welfare debts were conms, sin-

[10]Debts arisen after this date were not time-barred as the welfare office adequately interrupted these debts from the beginning of 2018.

[11]These types of debt were not time-barred. Note that debts from debtors who were no longer registered at the Dutch civic registry were also written off under slightly different conditions. Since their home addresses were unknown, the welfare office could not inform them about the debt relief. For this reason, this group was out of interest for this study.

[12]This letter contained the following passage: "You owe a debt of €<residual amount>to the municipality. This debt has expired." And: "You no longer have to repay the residual amount of €<residual amount>to us. We will write off this residual amount."

gle outstanding debts with repayments in the past five years, and debts under the authority sidered to be not fully collectible over time. As a consequence, the welfare office already incorporated a substantial loss on these welfare debts. The expected final average repayment ratio was estimated to be about 10%.

A potential threat to the validity of our research design concerns the presence of anticipation effects during the period between the court judgment in April 2019 and the communication of debt relief in December 2019. To address this concern, we examine the timing of public communication of the debt relief to the Rotterdam City Council. The City Council was informed of the broad intentions for debt relief on May 19, 2019, but no details on the scope and parameters were disclosed at that time. Full information on the decision rules was communicated in public documents on December 10, 2019, just before the date of communication to the debtors. We therefore conclude that debtors could not have received public information about the decision rules prior to December 2019. This is also confirmed with information from Nexis Uni, an academic research tool that allows us to investigate the timing and content of Dutch news items on the decision of the CRvB.[13] Although some local and national news providers reported on the judgment in the days following its announcement, the precise decision rules were not communicated.[14] Therefore, we consider anticipation effects to be unlikely for debtors around the cutoff.

2.3 Theoretical predictions

Debt relief has two (potential) direct effects on the debtor. First, it improves the wealth position of debtors and may alleviate liquidity constraints when the amount of monthly repayments is reduced. The extent to which liquidity constraints are alleviated depends on factors such as whether welfare debts are partially or fully relieved, whether the debtor made monthly repayments before the debt relief, and whether the debtor still owes debts to other creditors that need to be repaid. Second,

[13] We used the Dutch equivalents of the search terms "Debt, welfare, Rotterdam" and "Repayment, welfare, Rotterdam" for the period between April 29, 2019, and December 12, 2019.

[14] Note that a local newspaper did include a concrete decision rule, namely that welfare debts imposed before July 2009 would be time-barred and written off. However, this rule was incorrect and far too strict, falling outside the bandwidth of our analyses.

debt relief immediately eliminates the implicit marginal repayment tax of 35% on additional earnings (see Section 2.1) when debts are fully relieved, or gradually eliminates it over time when debts are partially relieved.

The impact of debt relief on behavioral outcomes may vary depending on whether debtors exhibit present-focused behavior. Research by DellaVigna and Paserman (2005) suggests that long-term unemployed individuals are more likely to exhibit present-focused behavior. Present-focused debtors may only change their behavior when debt relief directly affects their liquidity position or eliminates the marginal repayment tax on earnings. They may disregard the future implications of debt relief in their decision-making process.

From a theoretical perspective, the overall behavioral effect of debt relief on employment and welfare receipt is ambiguous. On the one hand, debt relief may increase employment and decrease welfare receipt due to substitution effects. As discussed above, debt relief eliminates the marginal repayment tax immediately or in the longer run. This may increase the effort to find paid work among unemployed debtors or the number of working hours among employed debtors. For present-focused individuals, the substitution effect only becomes effective at the moment the welfare debts are fully relieved or repaid.

On the other hand, debt relief may decrease employment and increase welfare receipt due to income effects. As debtors' liquidity constraints are alleviated and their wealth position improves, debt relief may decrease employment and increase welfare receipt. The existing literature supports this explanation by demonstrating that unemployed individuals facing liquidity constraints tend to find work more quickly (Basten et al., 2014; Card et al., 2007). At the same time, however, Dobbie and Song (2020) do not find evidence for employment effects of debt relief targeting the liquidity position of debtors. For present-focused debtors, income and liquidity effects on employment and welfare receipt are only at work when debt relief immediately alleviates liquidity constraints.

Next to the conventional substitution and income effects of debt relief, a recent literature extends the analysis to its consequences for the financial stress of individ-

uals (Dobbie and Song, 2015, 2020; Sergeyev et al., 2023).[15] The rationale is that debt relief can reduce financial stress, which can lead to more effective job search efforts and higher employment. A recent study among Dutch welfare applicants provides empirical evidence for this rationale (Vethaak et al., 2023). Overall, the substitution and financial stress effects are likely to outperform the income effects of debt relief, as found in previous studies (Dobbie and Song, 2015, 2020). As a consequence, we expect to find a positive effect of debt relief on employment and a negative effect on welfare receipt.

We expect a positive effect of debt relief on the mental health of debtors. Empirical studies have shown that problematic debts have a negative effect on mental health (Gathergood, 2012; Bridges and Disney, 2010; Roos et al., 2021). Concurrent with this, debt relief could alleviate or prevent financial stress by increasing disposable income, decreasing the mental accounting costs due to less outstanding debt accounts, or reducing long-term debt obligations (Ong et al., 2019). While studies that provide insight into the causal effects of debt relief on debtor's mental health are virtually absent, correlational studies have shown that debt relief is associated with reduced symptoms of depression (Hojman et al., 2016) and anxiety (Ong et al., 2019).

For all possible behavioral effects of debt relief, it is important to stress that financially vulnerable debtors may have limited opportunities to adapt their labor market behavior. Therefore, employment and welfare receipt may remain unresponsive to debt relief. In particular, debtors may face structural labor market barriers, such as long periods without work or health problems, which are not solved by debt relief.

[15] We note that debt relief is unlikely to affect labor market outcomes via credit flag removals, as previously shown by Bos et al. (2018). While credit reports are widely used in the US and other developed countries to screen potential hires, this is not the case in the Netherlands. Furthermore, welfare debts are not flagged in the Dutch credit flag system.

3 Data

3.1 Data sources and sample selection

We use individual-level data from the welfare office of the city of Rotterdam. These data concern the debt relief intervention, benefit garnishment, and debt services. We link these data to administrative data from Statistics Netherlands. Specifically, we use monthly data from the welfare debt registry that includes detailed information on the type, amount, origin, and repayments of welfare debts. Additionally, we collect monthly data from the benefit registry (welfare benefit payments), social security records (employment and earnings), municipal population register (sociodemographics), and debt registry (problematic debts). Finally, we obtain yearly data about dispensed medicines provided by the National Health Care Institute.

With these data, we construct a longitudinal dataset with monthly observations for the city of Rotterdam in the period between January 2018 and December 2021. This period includes the month of treatment itself, 23 pre-treatment and 24 post-treatment months. For medical consumption, we construct a dataset with annual observations for the pre-treatment year, the year of treatment itself, and two post-treatment years (period: 2018-2021).

We select our sample using the administrative data of the welfare debts registry, as the welfare office data only included treated subjects. Our aim is to include all subjects who met the all-but-one eligibility criterion for debt relief, which was the cutoff rule. We obtain all debtors with outstanding welfare debts at the welfare office just before the month of treatment and who met all debt relief eligibility rules (see Section 2.2) except for the cutoff rule. Since we cannot adequately exclude debtors with single outstanding debts with repayments in the past five years, our sample contains individuals who were eligible according to the cutoff rule but did not receive debt relief. This non-compliance in our design calls for a fuzzy RD-design. Subjects who were under 18 or over 65.3 years old were excluded. The final sample consists of 15,416 subjects, of which 1,690 were treated and 13,726 subjects were not treated.

3.2 Data description and sample characteristics

To evaluate the impact of debt relief, we distinguish three categories of outcome variables: (*i*) employment-related outcomes, (*ii*) welfare receipt, and (*iii*) mental health. To investigate employment-related outcomes, we construct a binary variable for general employment, taking a value of one if the subject has positive wage earnings, and is zero otherwise. Additionally, we construct a continuous variable for labor market earnings, including those with zero values. We measure welfare receipt using a binary variable. Finally, we construct a binary variable for mental health medication use following the ATC-4 medication classification, provided by Huber et al. (2013) and adapted to the Dutch context by Yildiz et al. (2020).[16]

To gain a better understanding of the financial consequences of the debt relief intervention, we investigate treatment effects on welfare debts and other debt-related variables. For this, we include variables that measure the balance and monthly repayments of welfare debts and construct binary variables that indicate whether a subject faces benefit garnishment, participates in a municipal debt settlement trajectory, or is part of a household with registered problematic debts. Given the narrow scope of and different definitions among debt sources, we regard this latter measure as a proxy for registered problematic debts.[17] We note that welfare debts were not included in the definition of registered problematic debts. Data for benefit garnishment and debt trajectory were not available for all post-treatment months. Finally, our dataset includes covariates for gender, migration background, household types (single, single parent, cohabit, other), age categories, and work history (i.e. being employed 12 months before the start of treatment).

Table 2 presents descriptive statistics for the full sample (Column 1), the sub-

[16]Specifically, the mental health medication use variable takes a value of one if a subject used medication classified as N05B (anxiolytics), N05C (hypnotics and sedatives), or N06A (antidepressants) within a given year, and is zero otherwise.

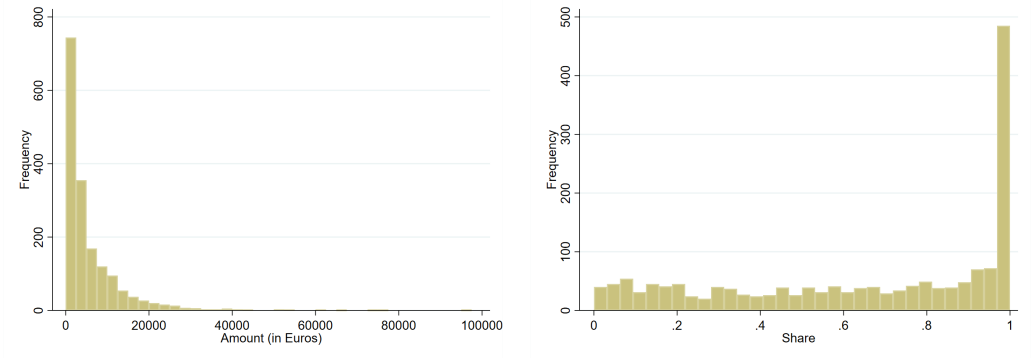
[17]We note that registered problematic debts were measured only at six specific moments between January 2018 and December 2021, and include only a subset of debt sources, mainly debts at public institutions. A subject is considered to be part of a household with problematic debts if any household member meets at least one of the following criteria: (1) participating in a statutory debt trajectory, (2) not paying the healthcare premium for at least six months, (3) having arrears in payment at the Central Judicial Collection Agency of at least 50 Euros and open for at least two months, (4) having tax or benefit debts at the Tax and Customs Administration of at least 50 Euros and open for at least 12 and 27 months, respectively, and (5) having arrears in payment at the Institute for Study Financing of at least 270 Euros and open for at least three months.

sample utilized in our main analyses (Column 2), and different subsamples of this latter sample (Columns 3–6). We discuss here the characteristics of the sample used in our main analyses (Column 2). Slightly more than half of this sample is female, and 82% has a migration background. The sample is divided nearly equally between individuals younger than 45 years of age and those who are older. The majority is single (41%) or single parent (34%), while smaller fractions cohabit (19%) or belong to another household type (6%). Approximately one in five individuals in the sample uses mental health medication. Further insights into the health condition of the sample is provided in Panel A of Table A.1. Around 63% of the sample uses medication for chronic diseases, of which the large majority (57% of the sample) uses medication for two or more chronic diseases.

Additional descriptive statistics provided in Panel B of Table 2 and Panels A and B of Table A.1 give insight into the financially vulnerable position of individuals in the sample. In the month prior to treatment, about three quarters of the sample receives welfare benefits. More than half of the sample receives welfare for more than three years, while 36% receives welfare for more than five years. Only 17% is employed in the month prior to treatment. As a result, the average monthly earnings (205 Euros) are low. The average welfare debt balance prior to the debt relief is roughly 6,000 Euros, which equals about six months of welfare benefit payments for a single-person household. As the average monthly repayment is small (40 Euros), the average remaining period needed to fully repay these welfare debts is long and exceeds 12 years. In the pre-treatment month, only 45% of the sample made a repayment. In addition, a substantial part faces debt-related problems stemming from large welfare debts (39%), registered problematic debts at other creditors (60%), garnishment of welfare benefits (26%), or participation in a debt settlement trajectory (10%) (see Panel C of Table A.1). Almost four in five individuals face at least one of these debt-related problems.

Figure 1 provides graphical insight into the debt relief intervention. The debt relief concerns 1,727 welfare debtors with an average amount of 5,847.63 Euros. As illustrated in Panel (a) of Figure 1, the distribution of debt remission amounts is skewed to the right. On average, the debt relief amount constitutes 65.2% of all

Figure 1: Frequencies of debt relief per amount and per share



(a) Distribution of amount of debt relief **(b)** Share of share of debts relieved

Note: The share of debts relieved (Panel (b)) is calculated by dividing the amount of debt relief by the total balance of outstanding welfare debts. $N = 1,690$.

welfare debts owed by the debtors. Moreover, as reflected in Panel (b) of Figure 1, approximately half of the treated subjects received debt relief covering 75 to 100% of their welfare debts.

The binned scatterplots in Panels (a) and (b) of Figure 2 show the relationship between the balance of welfare debts and the monthly repayments. The figure reveals a clearly positive association: individuals with larger amounts of welfare debts tend to make larger monthly repayments. Panels (c) and (d) illustrate the changes in welfare debts before and after debt relief, focusing on the treated (Panel (c)) and non-treated (Panel (d)) individuals. Panel (c) clearly demonstrates a noticeable shift in the balance of welfare debts for the treated individuals, indicating the impact of debt relief. In contrast, Panel (d) shows that such a shift is not observed among the non-treated individuals. In Panel (c), we observe that on average the proportion of debts relieved was approximately equal for groups with smaller and larger welfare debts.[18]

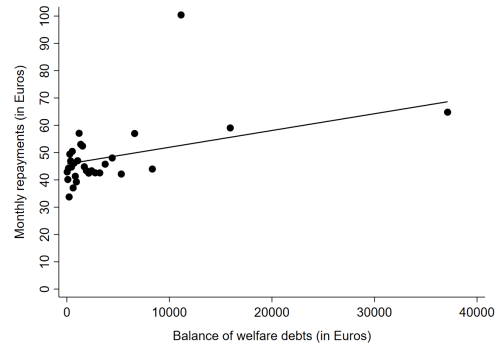
[18] As shown in Figure 1, the shares could substantially differ between individuals.

Table 2: Descriptive statistics of debtors by debtor outcome

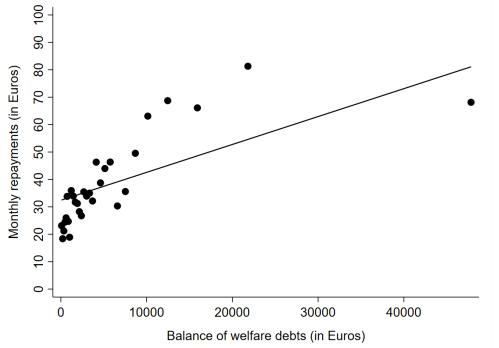
	Full sample	**First-stage sample**					**p-value balance test**	
	All	All	Not treated	Treated	Below cutoff	Above cutoff	Treated vs. not treated	Below vs. above cutoff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Covariates								
Female	0.51	0.54	0.55	0.51	0.55	0.52	0.13	0.29
Migration background	0.82	0.82	0.82	0.83	0.82	0.83	0.22	0.33
Age <35 years	0.31	0.24	0.24	0.23	0.25	0.23	-	-
Age 35-44 years	0.25	0.28	0.29	0.28	0.29	0.28	0.92	0.59
Age 45-54 years	0.26	0.27	0.27	0.27	0.28	0.26	0.82	0.86
Age >54 years	0.19	0.21	0.20	0.23	0.19	0.24	0.22	0.02
Single	0.40	0.41	0.38	0.46	0.40	0.42	-	-
Single parent	0.30	0.34	0.34	0.34	0.35	0.33	0.45	0.92
Cohabit	0.23	0.19	0.22	0.13	0.20	0.18	0.00	0.20
Other household type	0.08	0.06	0.06	0.07	0.06	0.06	0.66	0.76
Work history	0.17	0.14	0.14	0.11	0.14	0.13	0.10	0.82
B: Welfare debts (baseline)								
Balance of welfare debts	3,901.06	5,943.69	5,180.71	7,912.66	4,989.92	7,610.42	-	-
Monthly repayment indicator	0.49	0.45	0.43	0.48	0.44	0.46	-	-
Monthly repayment amount (€)	53.64	40.00	40.87	37.76	41.20	37.91	-	-
C: Outcomes (baseline)								
Employed	0.23	0.17	0.18	0.16	0.17	0.17	-	-
Earnings	278.54	205.22	220.84	164.92	204.58	206.34	-	-
Welfare benefit	0.67	0.75	0.72	0.84	0.75	0.76	-	-
Mental health medication	0.17	0.18	0.17	0.20	0.17	0.19	-	-
F-statistic							4.52	1.42
p-value							0.00	0.18
Individuals	15,416	2,775	2,000	775	1,765	1,010	2,775	2,775

Note: Descriptive statistics of debtors at the month before treatment (for mental health medication use: the year before treatment).

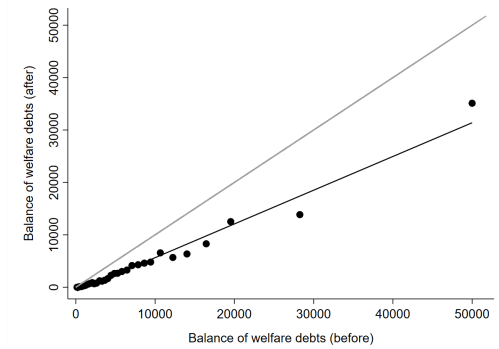
Figure 2: Binscatter plots



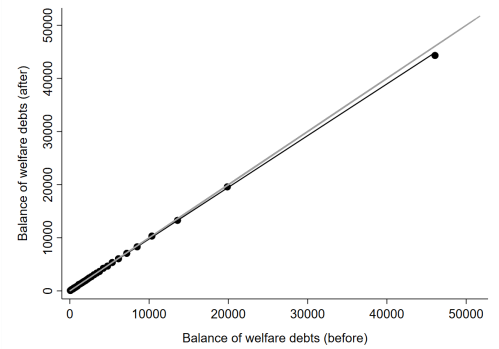
(a) Balance of welfare debts versus amount of repayment (full sample)



(b) Balance of welfare debts versus amount of repayment (first-stage sample)



(c) Debts before versus after treatment: Treated individuals (first-stage sample)



(d) Debts before versus after treatment: Not treated individuals (first-stage sample)

Note: This figure reflect binned scatter plots with 30 equal-sized bins. The dots represent the mean of the x- and y-variables within each bin, while the line reflects a linear fit using OLS. Panels (a) and (b) reflect the relationship between the balance of welfare debts versus the amount of monthly repayment for the full sample (Panel (a)) and the first-stage sample (Panel (b)) (see Table 2). Panels (c) and (d) reflect the welfare debts before versus after debt relief for treated (Panel (c)) and non-treated (Panel (d)) individuals. The 45-degrees line reflect the situation in which debts did not change before versus after treatment.

4 Empirical strategy

A major challenge in the inference of the impact of debt relief is the endogeneity of receiving such relief. As shown in Column (6) of Table 2, individuals who receive debt relief differ significantly in observed characteristics from those who do not (F-statistic = 4.58; p-value = 0.00). To address this endogeneity, we exploit the debt relief intervention that took place in December 2019, when welfare debts that originated before January 1, 2013 became time-barred. Similar to Ganong and Noel (2020), we use two empirical approaches. First, we exploit the cutoff variation in treatment using a fuzzy regression discontinuity (RD) design, which relies on

minimal and testable assumptions. Second, we use an instrumented difference-in-differences design (DDIV) that requires more assumptions but yields more precise estimates that turn out to be similar in magnitude to those from the fuzzy RD approach. Below we discuss the details and validity of both methods.

4.1 The fuzzy RD model: specification

First, we exploit the discontinuity in receiving debt relief created by the date-of-origin cutoff rule (see Section 2.2) using a fuzzy RD design. Debts originating before January 1^{st} of 2013 were mostly relieved, while debts originating after this date were not. Our RD design is considered fuzzy, as the probability of receiving treatment did not deterministically change at the cutoff, as discussed in Section 2.2.

To operationalize the fuzzy RD design, we create a score variable S_i that reflects the duration of the oldest outstanding welfare debt of each debtor relative to this cutoff. For each subject in our sample, we calculate the number of working days between the cutoff (1 January 2013) and the date of origin of their oldest outstanding debt. Subjects with debts that originated after (before) 1 January 2013 received a score value smaller (larger) than zero. So, as the debt is older, the higher the value of S. We count the score variable in number of working days to eliminate zero values corresponding to weekends and holidays.[19]

To estimate the effects of debt relief, we adopt the local polynomial approach proposed by Calonico et al. (2014) to obtain a point estimator with optimal properties (Cattaneo et al., 2020b; Cattaneo and Titiunik, 2022). We allow for local linear regressions on both sides of the cutoff. In determining optimal data-driven bandwidths, we minimize the mean squared error on each side of the cutoff. Since the distribution of the sample over the score value was right-skewed (as shown in Figure 3), we allow for different bandwidths on each side of the cutoff. The optimal bandwidth may differ across outcome variables and time periods. For the sake of comparability of outcomes, however, we use the same first-stage bandwidth for all

[19] In rare cases, the date of origin falls on a weekend or national holiday, which we adjust to the nearest working day. Note that the score variable is approximately continuous as it can take only discrete values.

outcomes and time periods.[20] As is common, we include triangular kernel weights in our regressions.

Inherent with the fuzzy RD approach, we specify the following two-stage least squares model:

$$DR_i = \alpha_{0t} + \alpha_{1t} D_i + \alpha_{2t} S_i + \alpha_{3t} D_i S_i + \alpha_{4t} X_i' + \nu_{it} \tag{1}$$

$$Y_{it} = \beta_{0t} + \beta_{1t} \widehat{DR}_i + \beta_{2t} S_i + \beta_{3t} D_i S_i + \beta_{4t} X_i' + \varepsilon_{it} \tag{2}$$

Where Y_{it} is an outcome of interest measured for individual i observed in month t. DR_i reflects the treatment variable that equals one if the subject received debt relief, and equals zero otherwise. The binary instrument D_i is zero when the score is below the cutoff and one if it is above the cutoff. S_i and $D_i S_i$ reflect the included first-order polynomials below and above the cutoff. X_i reflects the included covariates. In our main specification, we control only for predetermined baseline values of the outcome variable six months before the intervention (t=–6). The parameter of interest (β_{1t}) reflects the local average treatment effect of providing debt relief to compliant debtors with scores at the cutoff. In line with Lee and Card (2008), we cluster the standard errors at the score value.

For each outcome variable, we first estimate an effects-per-month model as reflected in Equation (1) and Equation (2). In this model, we estimate separate regressions for each month $t \in [-5, 24]$.[21] Second, we pool all monthly outcome variables in a model that assumes equal cutoff effects in the first and second year. This model assumes that the parameter estimates are the same for the included months.

4.2 Validity of the fuzzy RD model

The key identifying RD assumption is that, in the absence of the discontinuity in receiving debt relief, outcomes would trend continually through the cutoff. We provide both qualitative and quantitative evidence in support of this assumption.

[20] To test for bandwidth sensitivity, we perform a robustness analysis using outcome-specific optimal bandwidths (see Section 5.1).

[21] As mental health was measured at the yearly level, we estimate treatment effects on this outcome for each year $t \in [0, 2]$.

First, we argue that self-selection of debtors into the treatment was not possible (e.g., by setting or changing the score value). The score value, based on the date of origin of the oldest debt, was already established and could not be adjusted at the time of the ruling or the debt relief. Moreover, the cutoff and treatment assignment were entirely determined by professionals of the welfare office.

Second, we examine potential discontinuities in the number of observations at the cutoff. Panel (a) of Figure 3 shows the distribution of the sample over the score values, indicating no visual evidence of bunching on either side of the cutoff. With the binomial test advised by Cattaneo et al. (2020a), we cannot reject the hypothesis of no change in the density for the windows [-75, 75] and [-25, 25] around the cutoff (p=0.550 and 0.549 respectively). We also implement the density test proposed by Cattaneo et al. (2020b).[22] This test is visualized in Panel (b) of Figure 3 and provides weak evidence for a discontinuity at the cutoff (p=0.095). A potential explanation for this is that this test does not adequately incorporate the skewedness in the distribution of the score.

Third, we test for discontinuities at the cutoff in predetermined debtor characteristics. Table 3 reflects the intention-to-treat effects of debt relief on each predetermined covariate separately and shows that most covariates run smoothly through the cutoff. We only note (marginally) significant effects for the household types singles and cohabits.

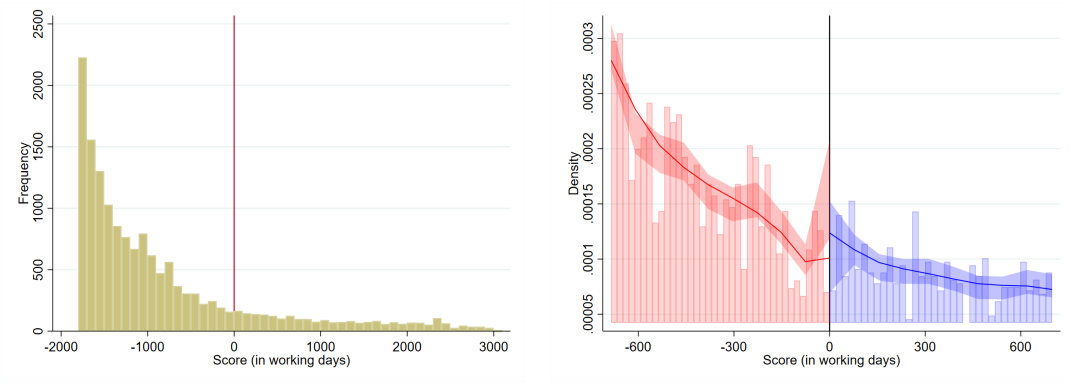
Finally, we inspect for discontinuities in the neighborhood of the cutoff. Figure B.1 in Appendix B shows standard RD plots for each of the outcomes for the full sample (left-handed panels) and first-stage sample (right-handed panels). This figure does not show any discontinuities in the relationships between the score variable and the outcomes. We conduct formal placebo tests using placebo cutoffs at 400 working days before and after the cutoff. As shown in Panel A and B of Table C.1, we find no significant RD effects on any of the outcomes.

The fuzzy RD design also relies on the usual assumptions inherent with the IV design. First, the exclusion restriction assumes that the cutoff only affects the

[22]This density test requires fewer parametric choices and fits better for local polynomial regressions than the original McCrary's density test.

outcome through the provision of debt relief. From interviews with welfare staff involved in the implementation of debt relief and reviewing the communication materials related to debt relief, we conclude that the debt relief was not accompanied by any additional policies. Moreover, the cutoff date (1-1-2013) was used to determine debt relief eligibility approximately seven years after this date, and no other policy intervention using this cutoff was implemented at that time. Second, the instrument D_i needs to be relevant, which means that is has sufficient explanatory power on the debt relief. Figure 4 provides RD plots reflecting the discontinuities of treatment characteristics around the cutoff. Panels (a) and (b) of Figure 4 show a clear discontinuity at the cutoff in the probability of receiving debt relief. For negative values of the running variable S_{it} there is (almost) no debt relief, while for positive scores we find a high probability of receiving debt relief. Using a local polynomial estimation as visualized in Panel (b), the first-stage estimate is 0.763 (F-statistic = 2,839). This implies that the probability of receiving debt relief is 76 percentage points higher for individuals with debts originating just before the cutoff compared to those with debts originating just after the cutoff. Panel (c) and (d) show that, without addressing the non-compliance, the debt relief is about 2,200 Euros at the cutoff, while Panel (e) and (f) indicate that the debt relief reduces the balances of welfare debts with about 40% for individuals at the cutoff. Third, to interpret our estimates as local average treatment effects (LATEs), the instrument D_i must satisfy the monotonicity assumption (Hahn et al., 2001). This assumption states that there are no defiers inside the bandwidth. This means that the treatment status of compliant debtors below or above the cutoff should change if they were on the opposite side of the cutoff. Similarly, the treatment status of non-compliant debtors should remain unchanged upon crossing the cutoff. The monotonicity assumption is violated if treatment intensity increases for some subgroup (e.g., men) while decreases for another subgroup (e.g., women). Although this assumption is not directly testable, Table 4 shows that the first-stage estimates are large, positively significant, and of similar magnitude across all subgroups. This is commonly considered as support for the monotonicity assumption (Imbens and Angrist, 1994).

Figure 3: Density of the score around the cutoff



(a) Distribution of the sample **(b)** Formal RD density test

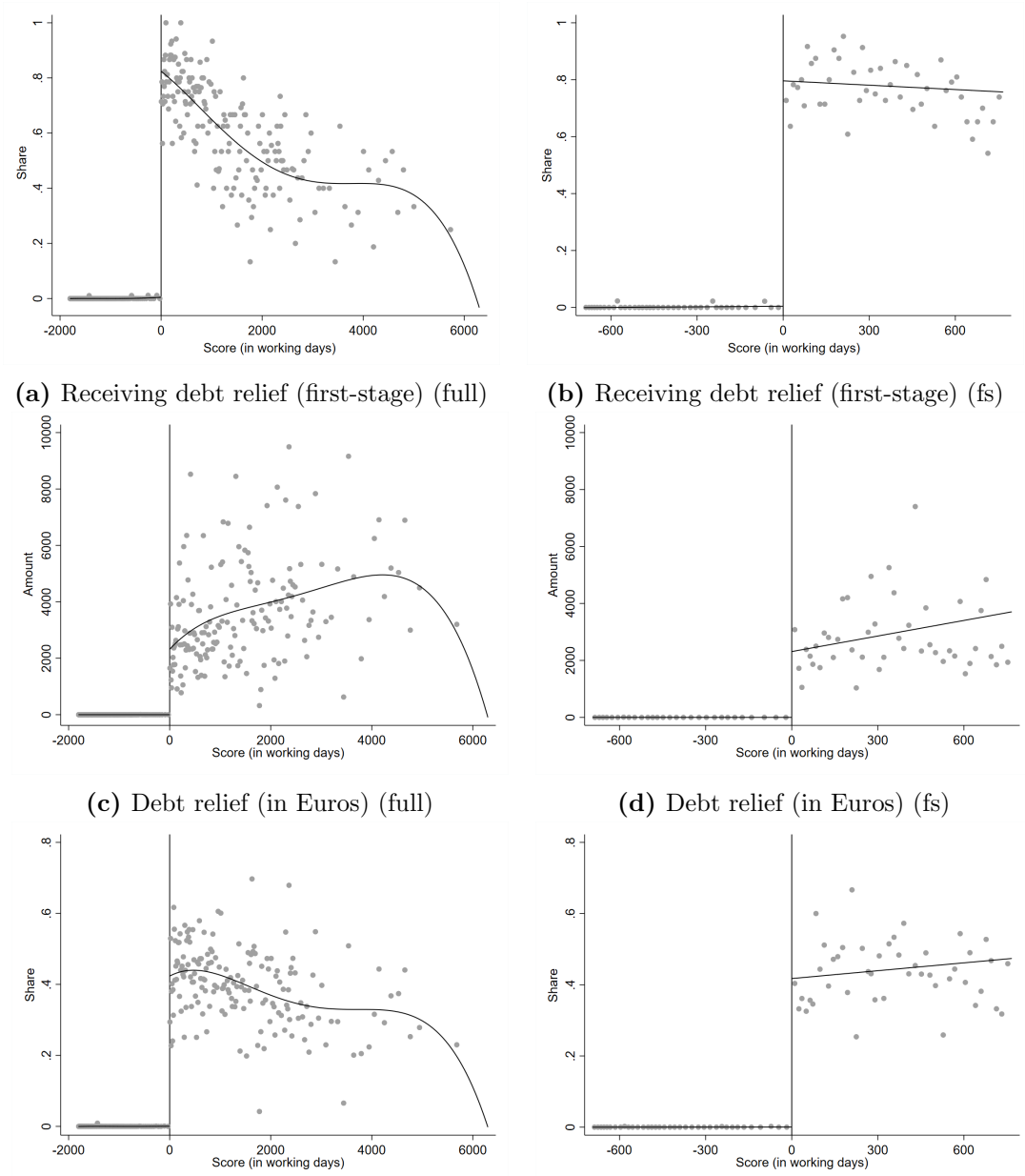
Note: Panel (a) shows the distribution of the sample over the score values. The score is defined as the number of working days between the date of origin of the oldest debt and the cutoff (1-1-2013). The vertical solid lines represent the cutoff. Some outliers with a score above 3,000 were excluded. Panel (b) visualizes the formal density test as proposed by Cattaneo et al. (2020b) and focuses on the distribution of the sample over the score in the direct neighborhood around the cutoff.

Table 3: Intention-to-treat RD effects of debt relief on predetermined covariates

	Coefficient	S.e.	p-value
	(1)	(2)	(3)
Female	-0.056	0.053	0.288
Migration background	0.047	0.044	0.286
Age: below 35 years	0.024	0.049	0.620
Age: 35-44 years	0.007	0.050	0.885
Age: 45-54 years	-0.006	0.046	0.891
Age: Above 54 years	-0.025	0.045	0.575
Single	0.095*	0.052	0.069
Single parent	-0.012	0.051	0.813
Cohabit	-0.088**	0.042	0.038
Other household type	0.005	0.027	0.853
Work history	0.027	0.037	0.465

Note: Each row in this table reports the intention-to-treat regression discontinuity (RD) effect of debt relief on a given predetermined covariate. The analysis is based on local linear estimation (bandwidth below cutoff = 699.7; bandwidth above cutoff = 764.6). Columns (1) and (2) show, respectively, the sharp RD effect and corresponding standard error. Column (3) reflects the p-value using robust bias correction inference. Number of observations: 2,775. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure 4: First-stage and additional treatment characteristics around the cutoff



(a) Receiving debt relief (first-stage) (full)

(b) Receiving debt relief (first-stage) (fs)

(c) Debt relief (in Euros) (full)

(d) Debt relief (in Euros) (fs)

(e) Share of debt amount relieved (full)

(f) Share of debt amount relieved (fs)

Note: This figure shows treatment characteristics around the cutoff for the full sample (left-hand panels) and the first-stage sample (right-hand panels). The horizontal axis shows the score values which is the number of working days between the date of origin of the oldest outstanding debt and the cutoff (1-1-2013). The vertical axis shows the share receiving debt relief (Panel (a) and (b)), the amount of debt relief (Panel (c) and (d)), and the share of welfare debt amount relieved (Panel (e) and (f)). The dots represent conditional means for the quantile-spaced bins. The number of bins is chosen using the mimicking variance method recommended by Cattaneo et al. (2020a). The solid lines show the fourth-ordered global (left-hand panels) or first-ordered local (right-hand panels) polynomial plot on either side of the cutoff. The jump in the predicted values at the cutoff of Panel (b) reflects the coefficient of the first-stage.

Table 4: First-stage estimates by subgroups

	Coefficient	S.e.	F-stat	N	Dependent Mean
	(1)	(2)	(3)	(4)	(5)
Full sample					
Full sample	0.763***	(0.014)	2,895	2,775	0.279
Gender					
Female	0.744***	(0.019)	1,517	1,490	0.266
Male	0.783***	(0.020)	1,592	1,285	0.295
Age					
Below 45 years	0.757***	(0.020)	1,451	1,453	0.270
Above 44 years	0.769***	(0.019)	1,658	1,322	0.290
Migration background					
Migration background	0.768***	(0.016)	2,385	2,276	0.283
No migration background	0.738***	(0.033)	510	499	0.261
Household type					
Single	0.811***	(0.015)	3,116	2,074	0.301
Other	0.612***	(0.033)	345	701	0.214
Balance of welfare debts					
Above 4,000 Euros	0.860***	(0.017)	2,461	1,087	0.392
Below 4,000 Euros	0.669***	(0.021)	1,051	1,688	0.207
Any repayment in previous three months					
Repayment	0.793***	(0.018)	2,044	1,437	0.302
No repayment	0.727***	(0.021)	1,166	1,338	0.255
Welfare receipt					
>3 years	0.833***	(0.016)	2,857	1,470	0.307
<3 years	0.662***	(0.022)	930	1,305	0.248
Health condition					
Two or more chronic diseases	0.783***	(0.018)	1,909	1,563	0.289
One or two chronic diseases	0.736***	(0.022)	1,146	1,212	0.267

Note: Each row in this table reports the first-stage effect on the probability of receiving debt relief for a specific (sub)sample with scores within the bandwidth. Bandwidth below cutoff = 694.2; bandwidth above cutoff = 766.9. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

4.3 The DDIV model: specification

Given that debt relief follows from the discrete implementation rule, the RD design is a natural choice for estimating the effects of debt relief. Zooming into the cutoff itself creates exogenous variation in debt relief, but this comes at the cost of the efficiency of the estimates. Therefore, we also estimate an instrumented difference-in-differences model (DDIV) (Duflo, 2001; Hudson et al., 2017), that relies on the assumption that the treatment and control groups exhibit parallel trends in outcomes prior to the intervention. That is, in the dif-in-dif part of the model we compare the development of debt relief for those individuals with the oldest debt originating before 1-1-2013 with those with the oldest debt originating on or after 1-1-2013. The DDIV model scales a difference-in-difference (DiD) effect of the instrument on an outcome by a DiD effect on a mediating treatment variable. In doing so, we utilize the same set of individuals as in the RD estimation, namely those with the oldest debt originating between 765 days before and 700 days after the cutoff date of 1-1-2013..

For each month t, we specify the following DDIV model:

$$DR_{it} = \gamma_{0t} + \gamma_{1t}G_i + \gamma_{2t}M_t + \gamma_{3t}G_iM_t + \gamma_{4t}X_i' + \upsilon_{it} \tag{3}$$

$$Y_{it} = \delta_{0t} + \delta_{1t}G_i + \delta_{2t}M_t + \delta_{3t}\widehat{DR}_{it} + \delta_{4t}X_i' + \omega_{it} \tag{4}$$

where Y_{it} denotes the time-varying outcome for a set of individuals at time t. DR_{it} is the treatment dummy, indicating whether the welfare debt of individual i was relieved at time t. G_i is a dummy indicating whether individual i has a score below or above the cutoff. M_t is a dummy for calendar month t with month $t = -6$ as the reference month. G_iM_t is the instrumental variable. X_i reflects a full set of covariates including dummy variables for gender, migration background, age categories, household types, and work history. The coefficient of interest (δ_{3t}) reflects the effect of debt relief on the outcome among compliant debtors with scores around the cutoff.

Similar to the fuzzy RD model, we estimate DDIV models in two ways. First, DDIV treatment effects are estimated as effects-per-month model for each pre- and

post-treatment month (t) separately, where $[-5 \leq t \leq 24]$. Second, we estimate a pooled model in which we pool monthly data together to estimate the 1-year and 2-years treatment effects.

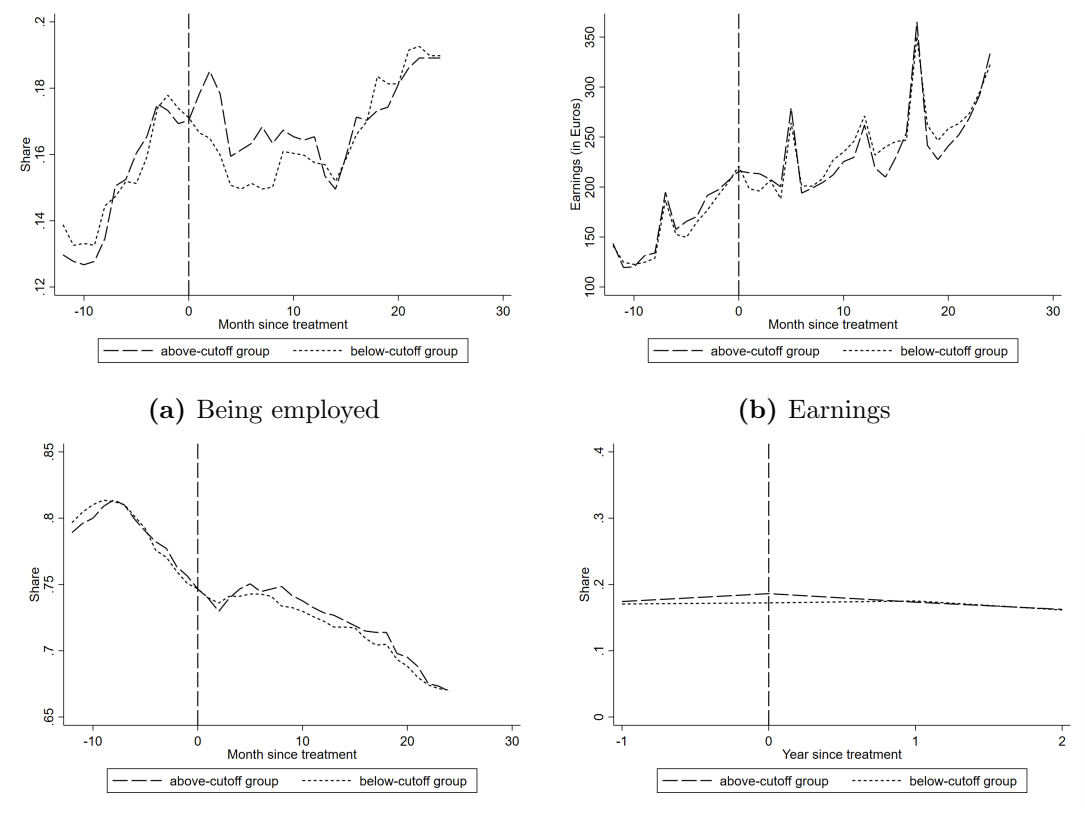
4.4 Validity of the DDIV model

The DDIV model comes with several assumptions. This section discusses the rationale and evidence for these assumptions. The identifying assumption is that of parallel trends, which is similar to a standard DiD design (Hudson et al., 2017). To eyeball the potential presence of non-parallel trends, Figure 5 shows the pre- and post-treatment growth paths of both the below and above cutoff groups. This figure demonstrates that all outcomes had a similar trend during the twelve months prior to the start of the treatment, supporting the parallel trends assumption. Accordingly, we observe no significant DDIV effects on the outcomes during the pre-treatment period (see Figure 7 and Figure 8).

Similar to tests conducted on the independence assumption in standard IV models, we also investigate whether the instrument $G_i M_t$ can be predicted by debtor characteristics. We conduct a balance test by regressing the instrument on the debtor characteristics. Column (8) of Table 2 presents the results, indicating that debtor characteristics do not predict the instrument (p-value = 0.18).[23] Finally, the DDIV model relies on the instrument relevance, exclusion, and monotonicity assumptions familiar from the IV-literature. As these assumptions apply to the fuzzy RD design in a similar way, we refer to Section 4.2 for a discussion on their validity.

[23]For individual covariates, we find a significant effect for the age category >54 years (p<.05). This finding is not surprising, considering that the above cutoff group has longer outstanding debts and is likely to be older than the below cutoff group. In our DDIV regressions, we control for age, along with a complete set of covariates.

Figure 5: Trends for outcome variables: Below-cutoff versus above-cutoff groups



(a) Being employed

(b) Earnings

(c) Receiving welfare

(d) Mental health medication

Note: Trends for outcome variables for below-cutoff versus above-cufoff groups. $N = 5,586$, based on first-stage RD bandwidth. $t = 0$ indicates the month of treatment

5 Results

5.1 Effects on main outcomes

We examine the effects of debt relief using the fuzzy RD and DDIV designs. In the fuzzy RD design, we exploit the discontinuity around the cutoff date of 1-1-2013. In December 2019, debts originating just before 1-1-2013 were relieved, while debts originating just after that date were not. In the DDIV design, we compare the treatment group (individuals whose debt was relieved) with the control group (individuals whose debt was not relieved) both before and after debt relief. Convincingly, the fuzzy RD and DDIV designs show rather similar coefficients. Standard errors are smallest in the pooled DDIV model, and are larger for the fuzzy RD design and the monthly effects. Below, we discuss the results for each of the main outcomes.

In line with expectations, the direct effect of debt relief on the balance of welfare debts is substantial and persistent. Panels (a) and (b) of Figure 6 reflect the effects of debt relief on the balance of outstanding welfare debts under the DDIV and Fuzzy RD specification, respectively. The relief of debt leads to an immediate reduction in the balance of outstanding welfare debts by approximately 3,900 Euros (DDIV) and 2,700 Euros (RD) on average. As shown in Figure 6 and Table 5, the effect on the balance of outstanding debts remains stable during the subsequent two years.

Table 5 shows that debt relief reduced the number of individuals with a montly repayment substantially by about 10 percentage points (compared to a pre-treatment average of 44%). The effects-per-month models presented in Panels (c) and (d) of Figure 6 show similar effects, although the RD estimates become statistically insignificant in the longer run. Furthermore, the debt relief decreased the monthly payment amount by 17 Euros (42%) for one year and 12 Euros (30%) for two years under the DDIV specification (see Table 5). The fuzzy RD and the monthly DDIV effects in Table 5 and Panels (e) and (f) of Figure 6 are not statistically significant.

Figure 7 shows the effects of debt relief on labor market outcomes.[24] Upon comparing the fuzzy RD and DDIV results, we do not find statistically significant effects of debt relief on the probability of being employed (Panels (a) and (b)) and earnings (Panels (c) and (d)) for any of the post-treatment months. Similarly, the pooled model does not reveal any significant effects on these outcomes (see Table 5). Note that the DDIV estimates exhibit smaller standard errors compared to the fuzzy RD effects. The pooled DDIV employment effects 2 years after treatment are close to zero (0.9 percentage points) and precisely estimated (s.e. = 1.2 percentage points).

Figure 8 and Table 5 present the effects of debt relief on welfare receipt and mental health medication use. We do not find any significant DDIV or RD effects for welfare receipt. Similarly, debt relief does not significantly affect mental health medication in any of the post-treatment years. Again, the effect sizes in the pooled DDIV model are close to zero.

[24] Figure B.1 in Appendix B shows standard RD plots for each of the outcomes at month $t = 12$ (for mental health medication: year one) for the full sample (left-handed panels) and the first-stage sample (right-handed panels). These RD plots provide insights in (1) the relationship between the score variable and the outcomes, (2) the local behavior of the regression function at the cutoff, and (3) a first impression of the reduced form effects.

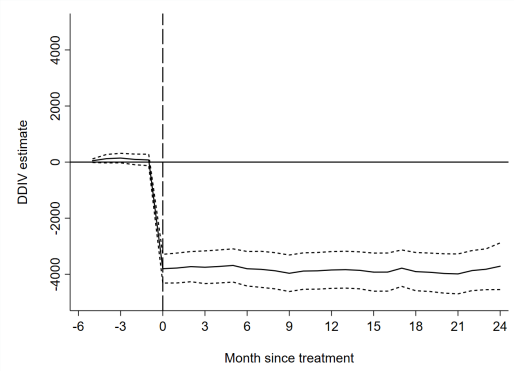
Table 5: Effects of debt relief, pooled model - DDIV and Fuzzy RD estimates

		DDIV		Fuzzy RD	
	Dep. mean	Coef.	S.e.	Coef.	S.e.
	(1)	(2)	(3)	(4)	(5)
Panel A: 1 year after treatment					
Balance of welfare debts (€)	5,944	-3,847***	285	-2,698***	710
Monthly repayment indicator	0.442	-0.086***	0.019	-0.133***	0.047
Monthly repayment amount (€)	40.00	-16.68***	5.47	-9.32	9.62
Being employed	0.172	0.017	0.012	0.012	0.036
Monthly earnings (€)	205	-7	19	-69	55
Receiving welfare	0.753	0.007	0.013	0.015	0.038
Mental health medication†	0.172	0.003	0.014	-0.039	0.034
Number of observations		66,600		33,300	
Number of individuals		2,775		2,775	
Panel B: 2 years after treatment					
Balance of welfare debts (€)	5,944	-3,887***	303	-2,720***	755
Monthly repayment indicator	0.442	-0.091***	0.020	-0.106**	0.043
Monthly repayment amount (€)	40.00	-12.03***	4.15	-3.81	6.26
Being employed	0.172	0.009	0.012	-0.003	0.034
Monthly earnings (€)	205	-13	21	-65	59
Receiving welfare	0.753	0.008	0.014	0.028	0.038
Mental health medication†	0.172	0.001	0.014	-0.029	0.033
Number of observations		99,900		66,600	
Number of individuals		2,775		2,775	

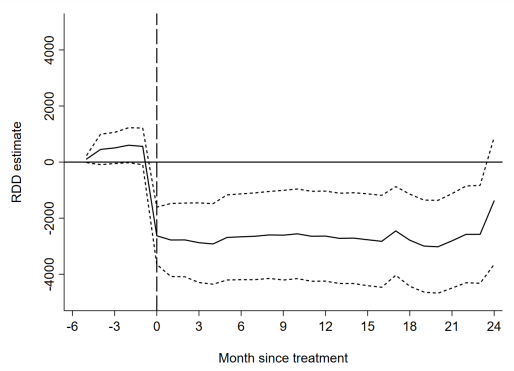
Note: Each row shows the results of the pooled DDIV (Columns 2-3) and fuzzy RD (Columns 4-5) models for a 1-year (Panel A) and a 2-year (Panel B) period after treatment. The means in the first column are from the pre-treatment period. The instrumented difference-in-difference (DDIV) estimates are based on $t = -1$ to $t = -12$ as baseline and include controls for gender, migration background, age categories, household types, and work history. The fuzzy RD estimates include the outcome values at $t = -6$ as control variable. † The pooled model for mental health medication is based on yearly data (instead of monthly data) and includes the pre-treatment year as baseline (DDIV) or control variable (fuzzy RD). The corresponding number of observations is 8,325 (Panel A) and 11,100 (Panel B) for the DDIV model and 5,550 (Panel A) and 8,325 (Panel B) for the fuzzy RD model. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

To test the robustness of the results, we perform several sensitivity checks commonly used in RD designs. First, we examine the sensitivity of our RD estimates to bandwidth selection choices. In our main RD model, we use uniform bandwidths across outcomes (based on the first-stage). Panel B of Table D.1 in Appendix D presents the results for RD estimates using outcome- and time-specific bandwidths.

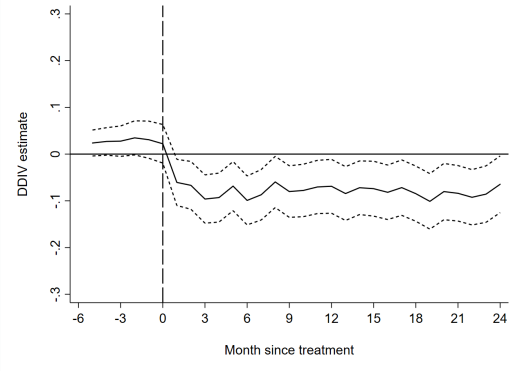
Figure 6: Effects of debt relief on welfare debts and repayment



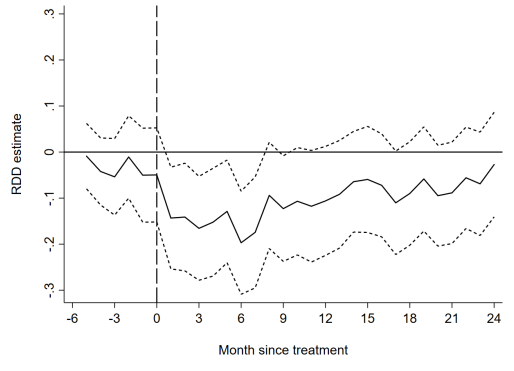
(a) Balance of welfare debts (€) (DDIV)



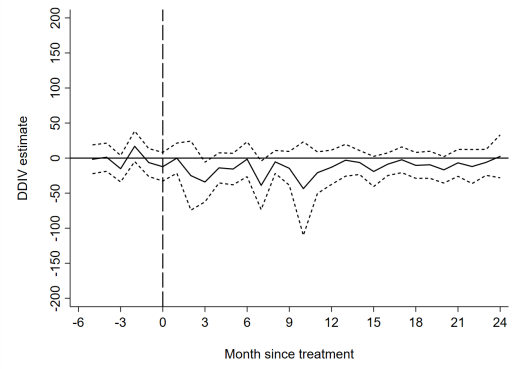
(b) Balance of welfare debts (€) (Fuzzy RD)



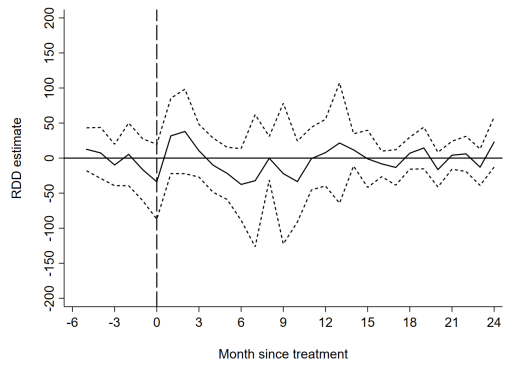
(c) Monthly repayment ind. (DDIV)



(d) Monthly repayment ind. (Fuzzy RD)



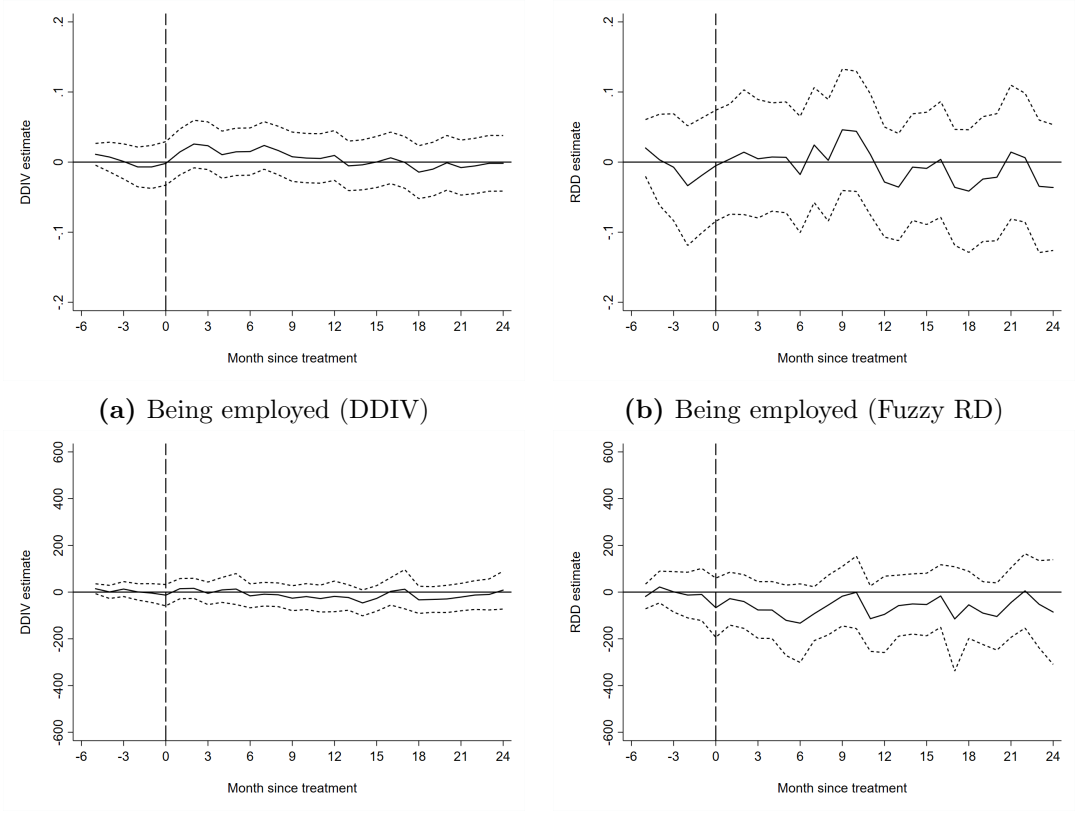
(e) Monthly repayment am. (€) (DDIV)



(f) Monthly repayment am. (€) (Fuzzy RD)

Note: The figures show DDIV (left-sided panels) and fuzzy RD (right-sided panels) treatment effects with corresponding 95%-confidence intervals on balance of welfare debts (Panels (a) and (b)), a monthly repayment dummy (Panels (c) and (d)), and the amount of monthly repayment (Panels (e) and (f)). The fuzzy RD estimates include the outcome values at $t = -6$ as control variable. The instrumented difference-in-difference (DDIV) estimates use t=-6 as baseline and include controls for gender, migration background, age categories, household types, and work history. All treatment effects are estimated for each month separately. The 95% confidence interval (dashed lines) are based on cluster-robust standard errors for the DDIV model or bias-corrected standard erros clustered at the score value for the fuzzy RD model. $t = 0$ indicates the month of treatment. Number of individuals: 2,775.

Figure 7: Effects of debt relief on labor market outcomes



(a) Being employed (DDIV)

(b) Being employed (Fuzzy RD)

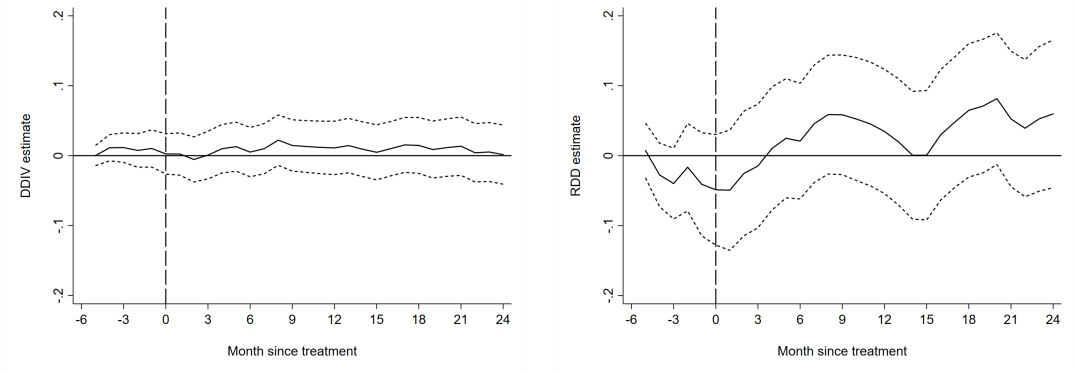
(c) Earnings (€) (DDIV)

(d) Earnings (€) (Fuzzy RD)

Note: The figures show DDIV (left-sided panels) and fuzzy RD (right-sided panels) treatment effects with corresponding 95%-confidence intervals on being employed (Panels (a) and (b)) and earnings (Panels (c) and (d)). The fuzzy RD estimates include the outcome values at t=-6 as control variable. The instrumented difference-in-difference (DDIV) estimates use t=-6 as baseline and include controls for gender, migration background, age categories, household types, and work history. All treatment effects are estimated for each month separately. The 95% confidence interval (dashed lines) are based on cluster-robust standard errors for the DDIV model or bias-corrected standard erros clustered at the score value for the fuzzy RD model. $t = 0$ indicates the month of treatment. Number of individuals: 2,775.

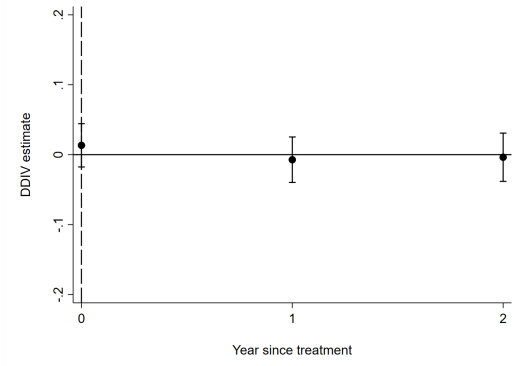
These estimates are highly similar to those of the main specification (Panel A). Second, we assess the sensitivity of our findings to the inclusion of covariates. The inclusion of covariates should not significantly alter the magnitude of the RD estimates. Indeed, Panel C of Table D.1 demonstrates that a fuzzy RD model without including any controls produces similar estimates to the main specification. Third, we conduct donut hole analyses to assess the sensitivity to subjects closest to the cutoff, who received larger weights in the RD estimation due to the triangular kernel weights. Panel D1 and D2 of Table D.1 demonstrate that the RD estimates remain similar when using a donut hole radius around the cutoff of 20 or 40 working days.

Figure 8: Effects of debt relief on receiving welfare and using mental health medication

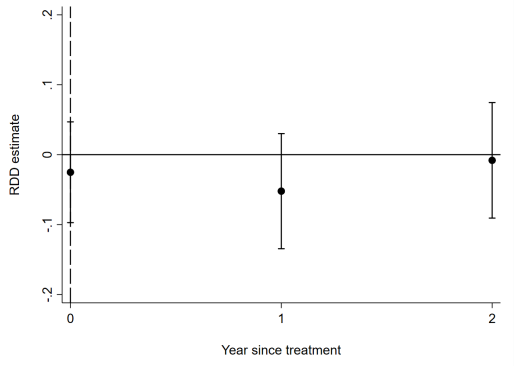


(a) Receiving welfare (DDIV)

(b) Receiving welfare (Fuzzy RD)



(c) Mental health medication (DDIV)



(d) Mental health medication (Fuzzy RD)

Note: The figures show DDIV (left-sided panels) and fuzzy RD (right-sided panels) treatment effects with corresponding 95%-confidence intervals on receiving welfare (Panels (a) and (b)) and using mental health medication (Panels (c) and (d)). The fuzzy RD estimates include the outcome values at t=-6 as control variable. The instrumented difference-in-difference (DDIV) estimates use t=-6 as baseline and include controls for gender, migration background, age categories, household types, and work history. All treatment effects are estimated for each month separately. The 95% confidence interval (dashed lines) are based on cluster-robust standard errors for the DDIV model or bias-corrected standard erros clustered at the score value for the fuzzy RD model. $t = 0$ indicates the month of treatment. Number of individuals: 2,775.

5.2 Mechanisms

Despite a significant and substantial long-lasting impact of debt relief on the balance of welfare debts and the amount of monthly repayments, the emerging picture is that there are no substantial effects on employment, earnings, welfare receipt, and mental health medication use. A possible explanation for this is that the debt relief provided was insufficient to significantly improve individuals' overall debt position. Another explanation could be that the (null-)results regarding the main outcomes reflect heterogeneity in treatment effects, depending on the debtors' likelihood of

responding to the debt relief.

To shed light on the mechanisms, we begin by examining the extent to which debt relief impacted the overall debt position. Since many debtors in our sample have multiple creditors, it is possible that debt relief provided by the welfare office as a preferred creditor may have prompted other creditors to intensify their debt collection efforts. Table 6 shows the effects of debt relief on the probability of facing four types of debt-related problems: registered problematic debts (excluding welfare debts), benefit garnishment, debt settlement trajectory, and large welfare debts (>4,000 Euros). Debt relief substantially decreased the probability of having a large welfare debt by 26.1 percentage points (66.6%). However, the welfare debt relief did not have a significant impact on the likelihood of registered problematic debts, benefit garnishment, and participating in a debt settlement trajectory. More strikingly, debt relief only leads to a modest 3.5 percentage points (4.4%) reduction in the probability of experiencing any of these debt-related problems. Overall, about 3 out of 4 treated individuals still face some form of debt-related problems about 2 years after treatment. So, even though welfare debt relief induces a significant reduction in the share of individuals facing large welfare debts, it only marginally improves the overall debt position. Therefore, it is likely that the debt relief had a limited impact on the liquidity position, as the benefits of reduced welfare debt repayments may have been counterbalanced by increased repayments to other private creditors.

Next, we investigate whether debt relief affects outcomes among subgroups that are more likely to respond to it. First, we distinguish between treatment effects among debtors with larger and smaller welfare debts. Panels A1 and A2 of Table E.1 present the DDIV estimates for individuals with larger (>4,000 Euros) and smaller (<4,000 Euros) welfare debts separately.[25] Indeed, debt relief has a substantially larger effect on the balance of welfare debts ($-6,065$ Euros versus $-1{,}067$ Euros) and monthly repayments (-17.20 versus -8.54 Euros) among debtors with larger welfare debts compared to debtors with smaller welfare debts. Additionally, debt relief has a statistically significant and substantial positive effect on employment

[25] As the DDIV model provides smaller standard errors than the fuzzy RD model, we only present the results of the pooled DDIV model (two-year estimates).

Table 6: Effects of debt relief on debt-related outcomes – pooled DDIV estimates

Dependent variable:	Registered problematic debts	Benefit garnishm.	Debt settlement trajectory	Large welfare debts	Any debt-related problem
	(1)	(2)	(3)	(4)	(5)
Coefficient	0.017	0.015	0.018	-0.261***	-0.035**
(s.e.)	(0.013)	(0.012)	(0.013)	(0.019)	(0.013)
Dependent mean	0.604	0.258	0.103	0.392	0.790
Individual observations			2,775		

Note: Each column shows the results of pooled DDIV treatment effects on debt-related outcomes for the 18-months period after treatment. The instrumented difference-in-difference (DDIV) estimates include controls for gender, migration background, age categories, household types, and work history. For registered problematic debts, we imputed the value of the nearest observed month for missings to align the included time period among all debt-related outcomes. Standard errors are robust and clustered at the individual level. The dependent mean reflects the mean of the outcome at t=-1. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

for debtors with large welfare debts, corresponding to an increase of 4.1 percentage points (compared to a pre-treatment average of 18.6%). Table E.2 shows that the positive effect of debt relief on employment among debtors with large welfare debts is robust to setting alternative bounds. Furthermore, the employment effect increases both in absolute and relative terms for larger amounts of welfare debts. This suggests that the positive employment effects are proportional to the amount of debt reduction.[26] Moreover, Panel (a) of Figure E.1 in Appendix E indicates that the effect on employment primarily manifests as a longer-term effect that accumulates over time.

Interestingly, Panel (c) of Figure E.1 shows that the increased employment effects for large debt reductions are not accompanied by a significant increase in earnings or decrease in welfare receipt. Presumably, debt relief leads to an increase in part-time work in addition to receiving welfare benefits. Concurrently, Panel A2 of Table E.1 and Panels (b) and (d) of Figure E.1 provide suggestive evidence that debt relief decreases employment and earnings among debtors with smaller welfare debts. However, the pooled treatment effects are not robust when alternative bounds

[26]Note that the effect may operate via an improved wealth and/or liquidity position, as both the reduction in the balance of welfare debts and the amount of monthly repayment become larger for higher bounds of large welfare debts.

are set for smaller welfare debts (see Table E.2).

As a second stratification variable, we consider the presence of registered problematic debts. Similar to the subgroup with large welfare debts, we find a positive significant effect of 2.7 percentage points (13.8%) on employment among debtors belonging to a household with registered problematic debts (see Panels B1 and B2 of Table E.1). Again, the effects on earnings and welfare receipt are economically small and statistically insignificant. We do not observe statistically significant treatment effects among debtors without registered problematic debts. These results suggest that, consistent with the findings for the subgroup with larger welfare debts, the labor market response to debt relief is stronger for debtors in a severe debt position.

Third, we examine the potential differential treatment effects among subjects who experience an improvement in their liquidity position as a result of debt relief, compared to those without debt relief. This analysis is particularly relevant when debtors have high discount rates or exhibit present-biased behavior. Consistent with the theoretical predictions discussed in Section 2.3, debt relief might have a stronger impact on labor market outcomes for debtors with an improved liquidity position due to reduced monthly repayments. To test this, we stratify between subjects with and without any repayment in the three months before treatment. We assume that debt relief is more likely to improve the liquidity position of debtors who made repayments before treatment compared to debtors who did not. The results for this subgroup analysis are presented in Panel C1 and C2 of Table E.1. Indeed, we find that debt relief significantly decreases the probability of making a monthly repayment (-13.6 pp) and the amount of monthly repayments (-17.63 Euros) in the two-year post-treatment period among debtors who did repayments and not among debtors who did not make repayments. However, we do not find differential treatment effects for any of the outcomes under investigation. This suggests that variation in repayment obligations or post-treatment liquidity position do not primarily explain our results.[27]

[27] We also examine the effects of three alternative specifications of the treatment variable by replacing the treatment dummy DR_{it} in Equation (3) and Equation (4) for (*i*) a dummy indicating full versus partial or no debt relief, (*ii*) a dummy indicating large versus smaller or no debt relief, and (*iii*) a variable indicating the amount of debt relief. The results for these alternative specifications are presented in Table F.1. We do not find significant effects on any of the outcomes

Finally, one may argue that the ability of the relevant population of debtors to respond to the debt relief was limited. Debt relief may only have affected the behavior of a subgroup of debtors facing smaller labor market and health barriers. To investigate this, we examine treatment effect heterogeneity among debtors facing longer versus smaller elapsed welfare durations, as well as debtors with better versus worse health conditions. Panels A1 and A2 of Table E.3 present the results for subgroups with a welfare duration longer and shorter than three years in the month before treatment. The latter group also includes individuals who did not receive welfare in the month before treatment. We observe substantial pre-treatment differences in the dependent means for the relevant outcomes, but we do not find any differential treatment effects. Similarly, Panels B1 and B2 of Table E.3 show that treatment effects for subjects with two or more chronic diseases (worse health condition) are similar to those with less than two chronic diseases (better health condition).

From these additional analyses, the emerging picture is that the welfare debt relief only marginally improves the overall debt position of debtors. Our results indicate that significant positive employment effects are relevant only for the subsample of debtors who were initially in a worse debt position and (thus) experienced a larger reduction in welfare debt. However, when considering the limited impact on earnings and welfare receipt, these employment effects are primarily limited to part-time work. Additionally, we do not observe differential treatment effects among debtors with longer or shorter welfare durations, or between those with worse or better health. Consequently, we conclude that the welfare debt relief amount should be substantial enough to generate significant employment effects.

To gain insights into the effectiveness and cost-benefits of debt relief as reintegration policy, we compare its potential to a common active labor market policy designed for welfare recipients. For this latter policy, we refer to the evaluation results of a Work First Program implemented by the welfare office of Rotterdam (De Koning et al., 2018). For debt relief, we focus on debt relief for debtors with large welfare debts ($\geq$4,000 Euros). In terms of effectiveness, debt relief has a larger

for any of these alternative specifications.

impact on employment (+4.1 pp) compared to the Work First Program (+2.0 pp).[28] However, debt relief yields worse cost-benefit results for the welfare office than the Work First program. As we found no significant effect of debt relief on welfare receipt, reductions in benefit payments are likely to be absent. Consequently, the benefits of debt relief will not exceed its costs. For the Work First Program, in contrast, the savings in benefit payments exceeds the costs of this program by about 400 Euros per participant (De Koning et al., 2018).[29] Thus, from a public finance perspective, the debt relief offers a smaller marginal value of public funds compared to the Work First Program.

6 Conclusion

This study investigates the causal impact of debt relief on several outcomes among financially vulnerable individuals. We employ data from the welfare office of Rotterdam, the second-largest city in the Netherlands. The welfare office was required to write off time-barred welfare debts of debtors due to a court ruling. We exploit the cutoff that determined debt relief eligibility in a fuzzy regression discontinuity (RD) and instrumented difference-in-difference (DDIV) design to examine the causal effects of debt relief on labor market outcomes, welfare receipt, and mental health medication use. The expected effect of debt relief on employment and welfare receipt is ambiguous: while substitution effects and reduced financial stress have the potential to increase work resumption, income and liquidity effects may decrease incentives to resume work. Additionally, we anticipate a positive effect on mental health, as debt relief may alleviate or prevent financial stress.

[28] This point also holds when comparing the costs of the welfare office to increase employment with one person using a back-of-the-envelope analysis. For debt relief of large welfare debts, the average amount relieved was about 6,000 Euros, while the expected final repayment ratio is 10% (as discussed in Section 2.2). Consequently, the (net) cost of debt relief amounts to 600 Euros per debtor. As a result, the costs for the welfare office to increase employment with one person are about 15,000 Euros (=600/0.041). For the Work First program, the costs per participant are about 1,150 Euros, while the positive employment effect is 2.0 percentage points after 2 years (De Koning et al., 2018). Consequently, the costs to increase employment with one person in the Work First Program are about 57,500 Euros (=1,150/0.02).

[29] Note that the target groups of debt relief and the Work First program are somewhat different. Additionally, a comprehensive comparison of societal benefits for both intervention types should consider other potential benefits, such as improved well-being and increased societal participation.

Our results show that debt relief has significant and long-lasting effects on the amount and repayment of welfare debts in the two years following treatment. However, debt relief did not have a significant impact on employment, earnings, welfare receipt, or the use of mental health medication. A potential explanation for these null-findings is that, despite the substantial amount of debt relief, debt relief only had a small effect on the overall debt position of debtors. A substantial portion of the treated debtors continue to face debt problems even after receiving debt relief. Consequently, creditors other than the welfare office may intensify their efforts to recover debts.

Additional subgroup analyses indicate that debt relief had a positive impact on long-term employment among debtors who faced more severe debt situations due to large welfare debts or problematic debts at other creditors. It is likely that debt relief significantly altered their financial situation – i.e. liquidity and wealth position – of these debtors, while its impact was smaller among debtors with lower debt levels. This result supports the findings of Dobbie and Song (2020), who observed positive labor market effects when debt relief specifically targeted long-term debt constraints among borrowers with high levels of debt. At the same time, these results also suggests that debt constraints may hinder debtors to participate in the labor market.

This study contributes to the ongoing debate on the use and usefulness of debt relief as a policy instrument. Our findings indicate that the cross-domain effects of debt relief by a single creditor are limited when debtors also face debt problems at other creditors. In this setting, a collective household debt settlement in collaboration with other creditors may be a more appropriate intervention than providing debt relief by a single creditor. At this point, it is important to stress that the targeting efficiency of the debt relief intervention in our study was limited due to its reliance on an arbitrary debt characteristic (date of origin). In line with Dobbie and Song (2020), our findings suggest that targeting debt relief policies to debtors in a more severe debt position may lead to positive effects on employment.

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A Additional descriptive statistics

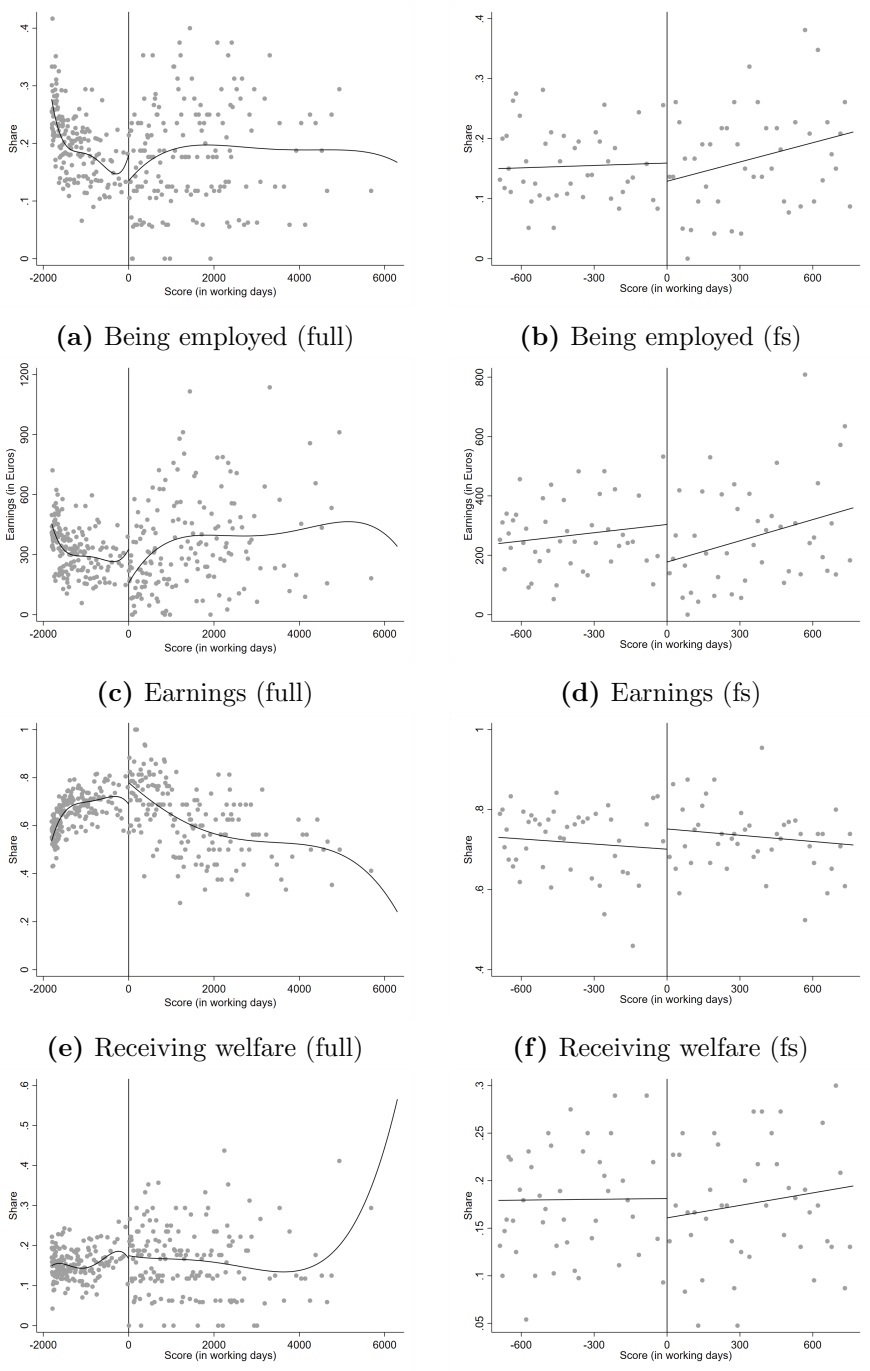
Table A.1: Additional descriptive statistics of debtors by debtor outcome

	Full sample	**First-stage sample**				
	All	All	Not treated	Treated	Below cutoff	Above cutoff
	(1)	(2)	(3)	(4)	(5)	(6)
A: Health condition						
Number of chronic diseases	2.34	2.53	2.47	2.68	2.49	2.61
No chronic disease	0.39	0.37	0.38	0.35	0.38	0.36
1 chronic disease	0.06	0.06	0.06	0.05	0.05	0.06
2 chronic diseases	0.18	0.17	0.17	0.18	0.18	0.17
3 or more chronic diseases	0.36	0.40	0.39	0.42	0.40	0.41
B: Welfare duration						
>5 years	0.25	0.36	0.34	0.42	0.35	0.39
>3 years	0.38	0.53	0.51	0.58	0.53	0.53
>1 years	0.56	0.68	0.65	0.75	0.68	0.68
C: Debt-related problems						
Any debt-related problem	0.64	0.79	0.77	0.85	0.77	0.83
Registered problematic debts	0.51	0.60	0.59	0.64	0.59	0.63
Large welfare debts (>4,000 Euros)	0.24	0.39	0.33	0.55	0.34	0.49
Benefit garnishment	0.16	0.26	0.23	0.33	0.25	0.28
Debt settlement trajectory	0.06	0.10	0.10	0.12	0.10	0.11
Number of individuals	15,416	2,775	2,000	775	1,765	1,010

Note: Descriptive statistics of debtors at the month before treatment (for health condition, the year before treatment). The number of chronic diseases has been measured following the ATC-4 medication classification to 21 common chronic diseases, provided by Huber et al. (2013) and adapted to the Dutch context by Yildiz et al. (2020).

B RD-plots

Figure B.1: Outcome characteristics around the cutoff (at t=12)



(a) Being employed (full)

(b) Being employed (fs)

(c) Earnings (full)

(d) Earnings (fs)

(e) Receiving welfare (full)

(f) Receiving welfare (fs)

(g) Mental health medication (full)

(h) Mental health medication (fs)

Note: This figure shows outcome characteristics around the cutoff at month t=12 (Year 1 for mental health medication) for the full sample (left-hand panels) and the first-stage sample (right-hand panels). The dots represent conditional means for the quantile-spaced bins. The number of bins is chosen using the mimicking variance method (Cattaneo et al., 2020a). The line shows fourth-ordered (left-hand panels) or first-ordered (right-hand panels) polynomial fits on either side of the cutoff.

C Placebo cutoffs

Table C.1: Results for the placebo cutoffs

Dependent variable:	Being employed	Earnings	Receiving welfare	Mental health medication
	(1)	(2)	(3)	(4)
A. Placebo cutoff: Cutoff = +400				
Coefficient	-0.018	-28	-0.001	-0.011
(s.e.)	0.038	56	0.047	0.042
Number of individuals	1,693	1,598	1,451	1,614
B. Placebo cutoff: Cutoff = -400				
Coefficient	0.026	35	-0.024	0.029
(s.e.)	0.038	58	0.044	0.036
Number of individuals	2,321	2,129	1,803	2,582

Note: The table reflects the results of placebo tests using placebo cutoffs 400 working days before (Panel A) and after (Panel B) the original cutoff (1-1-2013). The coefficients reflect RD estimates with outcome-specific, two-sided mean-squared-error optimal bandwidths. Standard errors are bias-corrected and clustered at the score value. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

D Robustness of the results

Table D.1: Sensitivity of the pooled RD estimates

Dependent variable:	Being employed	Earnings (Euros)	Receiving welfare	Mental health medication
	(1)	(2)	(3)	(4)
A. Main specification				
Coefficient	-0.003	-65	0.028	-0.029
(s.e.)	0.034	59	0.038	0.033
Number of individuals	2,775	2,775	2,775	2,775
B. Outcome-specific bandwidths				
Coefficient	-0.003	-43	0.025	-0.020
(s.e.)	0.032	52	0.031	0.030
Number of individuals	3,503	2,683	3,572	3,351
C. No covariate adjustment				
Coefficient	-0.023	-140*	0.041	0.011
(s.e.)	0.040	83	0.053	0.049
Number of individuals	2,775	2,775	2,775	2,775
D1. Donut hole: Radius = 20				
Coefficient	-0.006	-26	0.029	-0.043
(s.e.)	0.035	50	0.040	0.029
Number of individuals	2,731	2,731	2,731	2,731
D2. Donut hole: Radius = 40				
Coefficient	-0.020	-35	0.037	-0.068**
(s.e.)	0.044	55	0.044	0.034
Number of individuals	2,651	2,651	2,651	2,651

Note: The table reflects the results of different sensitivity checks for the pooled RD estimates. Panel A shows the results of the main RD specification, based on the first-stage bandwidth and including the outcome value at t=-6 as control. Panel B reflects the estimates based on outcome-specific bandwidths. Panel C shows the same specification without any controls. Panel D1 and D2 show the same specification as Panel A but uses a donut hole radius around the cutoff of 20 and 40 working days, respectively. Standard errors are bias-corrected and clustered at the score value. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

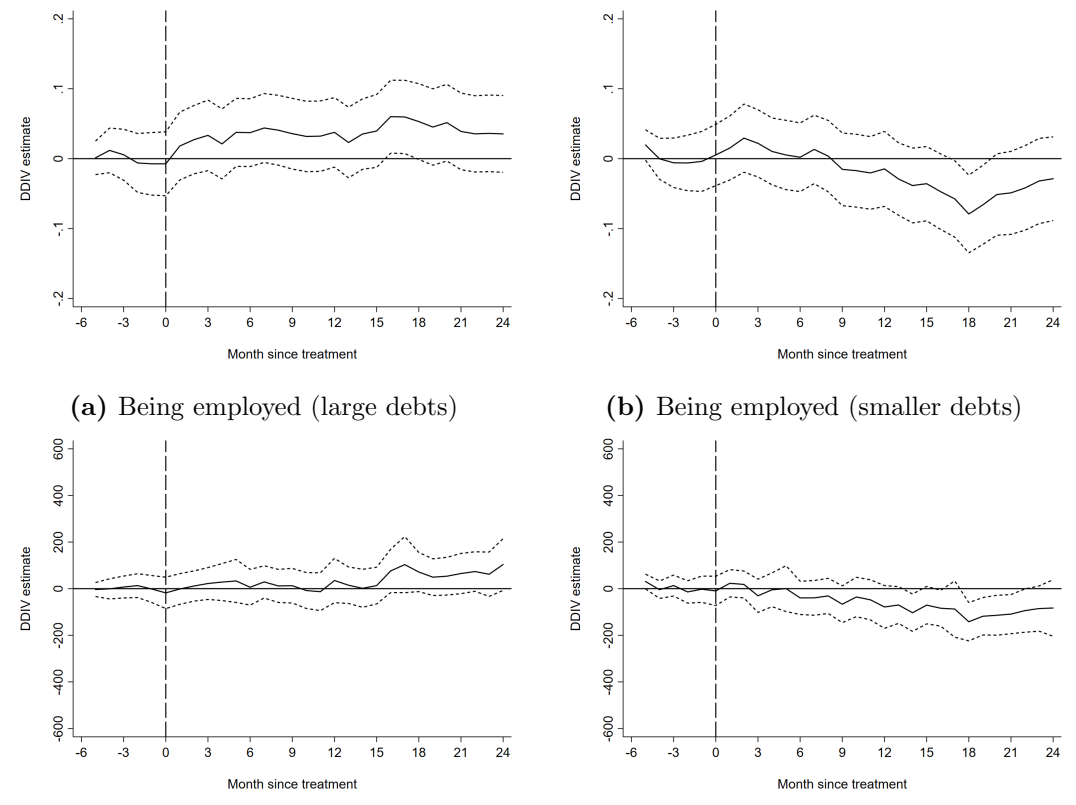
E Heterogeneous treatment effects

Table E.1: Effects of debt relief for subgroups – Financial situation

Dependent variable:	Balance of welfare debts	Monthly repayment indicator	Monthly repayment amount (€)	Being employed	Monthly earnings	Receiving welfare	Mental health medication
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Balance of welfare debts (at t=-1)							
1. >4,000 Euros							
Coefficient	-6,065***	-0.084***	-17.20**	0.041**	25	-0.009	-0.007
(s.e.)	525	0.027	7.52	0.017	29	0.020	0.019
Dependent mean	12,718	0.525	54.61	0.186	209	0.750	0.154
Number of observations	39,132	39,132	39,132	39,132	39,132	39,132	4,348
Number of individuals				1,087			
2. <4,000 Euros							
Coefficient	-1,067***	-0.120***	-8.54*	-0.019	-55*	0.029	-0.002
(s.e.)	150	0.030	4.70	0.018	31	0.021	0.020
Dependent mean	1,581	0.388	30.60	0.164	203	0.755	0.184
Number of observations	60,768	6,0768	60,768	60,768	60,768	60,768	6,752
Number of individuals				1,688			
B. Registered problematic debts (at 1-1-2018)							
1. Yes							
Coefficient	-3,939***	-0.081***	-12.41**	0.027*	4	0.000	-0.006
(s.e.)	358	0.024	4.95	0.016	27	0.019	0.017
Dependent mean	5,954	0.404	41.90	0.195	227	0.730	0.153
Number of observations	63,396	63,396	63,396	63,396	63,396	63,396	7,044
Number of individuals				1,761			
2. No							
Coefficient	-3,799***	-0.113***	-11.25	-0.026	-48	0.024	0.012
(s.e.)	560	0.036	7.41	0.018	30	0.021	0.025
Dependent mean	5,926	0.508	36.71	0.132	168	0.793	0.204
Number of observations	36,504	36,504	36,504	36,504	36,504	36,504	4,056
Number of individuals				1,014			
C. Did repayment (-3 $\leq t \leq -1$)							
1. Yes							
Coefficient	-4,669***	-0.136***	-17.63***	0.017	-7	0.009	-0.002
(s.e.)	443	0.027	6.46	0.016	28	0.018	0.019
Dependent mean	7,146	0.853	77.25	0.173	196	0.816	0.177
Number of observations	51,732	51,732	51,732	51,732	51,732	51,732	5,748
Number of individuals				1,437			
2. No							
Coefficient	-2,854***	-0.005	-2.47	-0.001	-22	0.010	0.004
(s.e.)	385	0.021	4.52	0.018	31	0.022	0.020
Dependent mean	4,653	0.000	0.00	0.171	215	0.685	0.167
Number of observations	48,168	48,168	48,168	48,168	48,168	48,168	5,352
Number of individuals				1,338			

Note: Each column shows the results of pooled DDIV treatment effects on the outcome for a 2-year period after treatment for a specific subgroup. Panels A1 and A2 distinguish between subjects with large (>5,000 Euros) and smaller (<5,000 Euros) welfare debts at the month before treatment. Panels B1 and B2 distinguish between subjects belonging to an household with or without registered problematic debts (at 1-1-2018). Panels C1 and C2 reflect estimates for subjects that did or did not any repayment in the three months before treatment. The instrumented difference-in-difference (DDIV) estimates include t=-1 to t=-12 as baseline and controls for gender, migration background, age categories, household types, and work history. Standard errors are robust and clustered at the individual level. The dependent mean reflects the mean of the outcome at t=-1. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure E.1: Heterogeneous DDIV treatment effects of debt relief on labor market outcomes – large versus smaller welfare debts



(a) Being employed (large debts)

(b) Being employed (smaller debts)

(c) Earnings (€) (large debts)

(d) Earnings (€) (smaller debts)

Note: The figures show heterogeneous DDIV treatment effects for subgroups with large (>4,000 Euros) and smaller (<4,000 Euros) welfare debts on being employed (Panels (a) and (b)) and earnings (Panels (c) and (d)). The instrumented difference-in-difference (DDIV) estimates use t=-6 as baseline and include controls for gender, migration background, age categories, household types, and work history. Treatment effects are estimated for each month separately. The 95% confidence interval (dashed lines) are based on cluster-robust standard errors. $t = 0$ indicates the month of treatment.

Table E.2: Effects of debt relief for debtors with larger versus smaller welfare debts – robustness analyses

Dependent variable:	Welfare debts and repayments			Labor market outcomes	
	Balance of welfare debts	Monthly repayment indicator	Monthly repayment amount (€)	Being employed	Monthly earnings
	(1)	(2)	(3)	(4)	(5)
A. Balance of welfare debts (2,000 euros)					
1. >2,000 Euros					
Coefficient	-4,886***	-0.107***	-14.56**	0.018	1
(s.e.)	395	0.023	5.74	0.014	23
Dependent mean	9,282	0.504	46.51	0.181	204
Number of observations (individuals)			60,228 (1,673)		
2. <2,000 Euros					
Coefficient	-534***	-0.095**	-8.82	-0.017	-59
(s.e.)	133	0.043	5.88	0.025	47
Dependent mean	876	0.348	30.13	0.159	207
Number of observations (individuals)			39,672 (1,102)		
B. Balance of welfare debts (3,000 Euros)					
1. >3,000 Euros					
Coefficient	-5,320***	-0.099***	-15.48**	0.026*	3
(s.e.)	447	0.025	6.91	0.016	26
Dependent mean	10,951	0.515	50.06	0.190	210
Number of observations (individuals)			48,384 (1,344)		
2. <3,000 Euros					
Coefficient	-934***	-0.114***	-9.07*	-0.015	-45
(s.e.)	154	0.035	4.69	0.021	36
Dependent mean	1,241	0.373	30.56	0.156	200
Number of observations (individuals)			51,516 (1,431)		
C. Balance of welfare debts (5,000 Euros)					
1. >5,000 Euros					
Coefficient	-6,712***	-0.087***	-23.80***	0.041**	20
(s.e.)	608	0.029	8.59	0.019	31
Dependent mean	14,416	0.535	57.50	0.188	217
Number of observations (individuals)			32,436 (901)		
2. <5,000 Euros					
Coefficient	-1,246***	-0.107***	-4.97	-0.011	-37
(s.e.)	143	0.028	4.39	0.017	29
Dependent mean	1,870	0.397	31.59	0.165	200
Number of observations (individuals)			67,464 (1,874)		
D. Balance of welfare debts (6,000 Euros)					
1. >6,000 Euros					
Coefficient	-7,444***	-0.080***	-25.57***	0.042**	25
(s.e.)	695	0.032	10.06	0.021	33
Dependent mean	16,121	0.548	60.06	0.178	195
Number of observations (individuals)			27,253 (757)		
2. <6,000 Euros					
Coefficient	-1,320***	-0.102***	-5.43	-0.006	-31
(s.e.)	137	0.026	4.19	0.016	28
Dependent mean	2,126	0.402	32.48	0.170	209
Number of observations (individuals)			72,648 (2,018)		

Note: Each column shows the results of pooled DDIV treatment effects on the outcome for a 2-year period after treatment for a specific subgroup. Each subgroup distinguishes between subjects with larger and smaller welfare debts, based on different frontiers. The instrumented difference-in-difference (DDIV) estimates include t=-1 to t=-12 as baseline and controls for gender, migration background, age categories, household types, and work history. Standard errors are robust and clustered at the individual level. The dependent mean reflects the mean of the outcome at t=-1 $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table E.3: Effects of debt relief for subgroups – Welfare duration and health condition

Dependent variable:	Balance of welfare debts	Monthly repayment indicator	Monthly repayment amount (€)	Being employed	Monthly earnings	Receiving welfare	Mental health medication
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Welfare duration							
1. >3 years							
Coefficient	-3,987***	-0.095***	-15.87***	0.007	11	-0.005	0.006
(s.e.)	394	0.026	4.22	0.012	18	0.013	0.017
Dependent mean	5,875	0.479	31.23	0.064	45	1.000	0.212
Number of observations	52,920	52,920	52,920	52,920	52,920	52,920	5,880
Number of individuals	1,470	1,470	1,470	1,470	1,470	1,470	1,470
2. <3 years							
Coefficient	-3,746***	-0.086***	-7.11	0.010	-47	0.026	-0.006
(s.e.)	471	0.031	7.92	0.024	42	0.028	0.024
Dependent mean	6,021	0.400	49.89	0.294	385	0.474	0.127
Number of observations	46,980	46,980	46,980	46,980	46,980	46,980	5,220
Number of individuals	1,305	1,305	1,305	1,305	1,305	1,305	1,305
B. Health condition							
1. Two or more chronic diseases							
Coefficient	-4,498***	-0.085***	-14.30***	0.019	5	0.010	0.011
(s.e.)	470	0.026	5.32	0.014	24	0.017	0.022
Dependent mean	6,530	0.456	38.75	0.136	160	0.793	0.305
Number of observations	56,268	56,268	56,268	56,268	56,268	56,268	6,252
Number of individuals	1,563	1,563	1,563	1,563	1,563	1,563	1,563
2. Null or one chronic disease							
Coefficient	-3,032***	-0.100***	-8.96	-0.006	-38	0.004	-0.012
(s.e.)	299	0.031	6.63	0.021	37	0.024	0.012
Dependent mean	5,187	0.423	41.61	0.219	264	0.701	0.000
Number of observations	43,632	43,632	43,632	43,632	43,632	43,632	4,848
Number of individuals	1,212	1,212	1,212	1,212	1,212	1,212	1,212

Note: Each column shows the results of pooled DDIV treatment effects on the outcome for a 2-year period after treatment for a specific subgroup. Panels A1 and A2 distinguish between subjects with a welfare duration longer and shorter than 3 years at the month before treatment. This latter group includes subjects that did not receive welfare. Panels B1 and B2 reflect estimates for subjects with two or more (B1) or less than two (B2) chronic diseases in the year before treatment. The instrumented difference-in-difference (DDIV) estimates include t=-1 to t=-12 as baseline and controls for gender, migration background, age categories, household types, and work history. Standard errors are robust and clustered at the individual level. The dependent mean reflects the mean of the outcome at t=-1 *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$

F Effects of alternative specifications of the treatment variable

Table F.1: Effects of alternative specifications of the treatment variable

Dependent variable:	Balance of welfare debts	Monthly repayment indicator	Monthly repayment amount (€)	Being employed	Monthly earnings	Receiving welfare	Mental health medication
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Full debt relief							
Coefficient	-17,611***	-0.415***	-54.49***	0.039	-61	0.035	0.004
(s.e.)	(1,843)	(0.091)	(19.07)	(0.055)	(93)	(0.064)	(0.063)
Dependent mean	5,944	0.442	40.00	0.172	205	0.753	0.172
Number of observations	99,900	99,900	99,900	99,900	99,900	99,900	11,100
Number of individuals	2,775	2,775	2,775	2,775	2,775	2,775	2,775
B. Debt relief of large welfare debts							
Coefficient	-19,959***	-0.470***	-61.75***	0.045	-69	0.040	0.004
(s.e.)	(1,475)	(0.107)	(21.35)	(0.062)	(106)	(0.073)	(0.071)
Dependent mean	5,944	0.442	40.00	0.172	205	0.753	0.172
Number of observations	99,900	99,900	99,900	99,900	99,900	99,900	11,100
Number of individuals	2,775	2,775	2,775	2,775	2,775	2,775	2,775
C. Amount of debt relief (per 1,000 Euros)							
Coefficient	-1,061***	-0.025***	-3.28***	0.002	-4	0.002	0.000
(s.e.)	(57)	(0.006)	(1.14)	(0.003)	(6)	(0.004)	(0.004)
Dependent mean	5,944	0.442	40.00	0.172	205	0.753	0.172
Number of observations	99,900	99,900	99,900	99,900	99,900	99,900	11,100
Number of individuals	2,775	2,775	2,775	2,775	2,775	2,775	2,775

Note: Each column shows the results of pooled DDIV treatment effects on the outcome for a 2-year period after treatment for an alternative specification of the treatment variable. Panel A presents the effects of full debt relief (compared to no or partial debt relief). Panel B shows the effects of large debt relief (>4,000 Euros) (compared to no or smaller debt relief). Panel C presents the effects of the amount of debt relief (per 1,000 Euros). The instrumented difference-in-difference (DDIV) estimates include t=-1 to t=-12 as baseline and controls for gender, migration background, age categories, household types, and work history. Standard errors are robust and clustered at the individual level. The dependent mean reflects the mean of the outcome at t=-1. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$