

Household Debt Relief and the Debt Laffer Curve*

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Abstract

Debt relief programs are often implemented in debt crises to alleviate debt overhang. If debt overhang is severe, debt relief can even benefit creditors by increasing repayment rates. This paper studies the impact of a large-scale household debt relief program in Hungary that reduced outstanding debt burdens by an average of 14% for over 850,000 housing loans. We find that debt relief leads to a sustained increase in both repayment rates and borrower income. The effects are hump-shaped in initial indebtedness, with the strongest responses among borrowers in the fourth quintile of indebtedness. We construct the Debt Laffer Curve, which relates the net present value of debt to its face value, and find that it flattens and can invert for highly indebted borrowers. A model of household debt overhang and labor supply can account for these findings.

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1 Introduction

Household debt crises are often associated with severe economic downturns. Policy-makers frequently respond to these crises with debt relief measures aimed at reducing the financial strain on borrowers and mitigating overall economic distress (Agarwal et al., 2017; Ganong and Noel, 2020). In theory, one important benefit of reducing debt burdens is to alleviate debt overhang (Myers, 1977; Donaldson et al., 2019). High levels of debt can distort borrowers' incentives to generate cash flows, as these efforts disproportionately benefit creditors. By addressing these distortions, debt relief can enhance repayment rates and stimulate economic activity.¹

Debt relief can have contrasting effects on the net present value of debt. On the one hand, a reduction in the face value of debt entails a direct loss for the lender. However, this loss can be offset by the indirect benefit of improved repayment rates. This benefit is likely to vary significantly across borrowers. In extreme cases, when borrowers are highly responsive to debt relief and the potential loss from default is large, debt relief can even increase the value of debt for creditors. As a result, the Debt Laffer Curve, which relates the net present value of repayment to the face value of debt, can be hump-shaped and invert for highly indebted borrowers (Krugman, 1988; Sachs, 1989). For borrowers on the downward-sloping part of the Debt Laffer Curve, debt relief can thus benefit both the borrower and the lender.

This paper examines the impact of a large-scale debt relief program for mortgage borrowers. We pose the following questions: What is the impact of debt relief on repayment and borrower income? How does this impact vary across the distribution of debt burdens? Are there circumstances under which debt relief increases the net present value of repayment, thereby benefitting both the borrower and the lender? That is, can the Debt Laffer Curve be inverted for the most indebted borrowers?

We study these questions in the context of a large-scale debt relief program for foreign currency (FC) loans implemented in Hungary in 2015. This setting offers two appealing features for studying the impact of debt relief. First, the program was large. Following a major legal settlement regarding loan servicing practices, the program reduced the face value of debt by an average of 13.7% for over 850,000 FC housing loans, leading to a 3.1 percentage point reduction in the household debt-to-GDP ratio. Second, there is substantial *ex ante* variation in the degree of indebtedness. The policy occurred in the aftermath of a severe FC household debt crisis, when

¹There can be other important benefits of household debt relief, including boosting aggregate demand and mitigating costly fire sales (e.g., Mian and Sufi, 2015; Korinek and Simsek, 2016).

FC borrowers saw large debt revaluations due to the depreciation of the exchange rate. At the time of the policy, the default rate on FC loans stood at 20%, and more than half of borrowers had higher debt balances in local currency terms than they originally borrowed nearly a decade earlier. These two features allow us to estimate heterogeneous effects of debt relief across the full distribution of indebtedness. This contrasts with most other debt relief policies, which are typically targeted at the most distressed debtors.

The paper proceeds in four steps. First, we show that debt relief leads to a persistent reduction in default rates, and this effect is highly heterogeneous across borrowers. We use administrative credit registry data with detailed information on loan characteristics and default. Our analysis relies on several identification strategies. We exploit a discrete threshold for program eligibility in regression discontinuity and difference-in-differences designs. To boost power for the estimation of heterogeneous treatment effects, we also exploit variation in treatment intensity in the full population of housing loans. The results are consistent across the three research designs.

We estimate that the debt relief program reduces the probability of default by 5 to 6.5 percentage points. This implies a reduction in default rates of one-third of the pre-policy mean, a large effect. The semi-elasticity of default with respect to a one percent reduction in debt is -0.2 to -0.4 for the average eligible borrower.

Behind this average effect lies substantial heterogeneity in the responsiveness of default to debt relief. We find that the strength of the effect is hump-shaped in *ex ante* indebtedness. The sensitivity of default with respect to debt relief initially increases with borrower indebtedness, peaking around the 80th percentile, beyond which it declines sharply. Moreover, the effect is larger for borrowers that are initially in default, particularly those with moderately high indebtedness. These findings are consistent with the idea that borrowers with moderate to high levels of debt are closest to the default threshold and therefore exhibit the highest sensitivity to debt relief.

In the second part of the paper, we document that debt relief leads to a modest but persistent increase in borrower income. To do this, we merge the credit registry with administrative individual-level income data. We estimate the effect of debt relief on income using a difference-in-differences design that exploits variation in treatment intensity. To support the identifying assumption of parallel trends, we show that debt relief is uncorrelated with income growth before the policy.

Our analysis reveals that a 20% reduction in debt leads to a 0.8% increase in income after two years. The large sample implies that the effect is precisely estimated and

highly statistically significant. Using detailed information on income sources, we find that the rise in income is driven by an increase in earned income. We also find that debt relief leads to a reduction in the likelihood of unemployment.

There is substantial heterogeneity in the effect of debt relief on income. We find the strongest income responses for borrowers in the third and fourth quintiles of indebtedness, similar to the heterogeneous effects observed for default. Hungary is a full recourse environment with wage garnishment rates of up to 33%. For these borrowers, debt reduction is thus likely to alleviate debt overhang. In contrast, borrowers in the bottom and top quintiles of indebtedness display essentially no response. Borrowers with low debt are unlikely to suffer from debt overhang, while borrowers with the highest levels of indebtedness are unlikely to receive sufficient relief from the program to alter their behavior.

In the third part of the paper, we construct an estimate of the household Debt Laffer Curve. The Debt Laffer Curve captures the relation between the net present value of repayment and the face value of debt. We estimate the slope of the Debt Laffer Curve using a simple loan valuation framework. Our estimation relies on two key inputs: the heterogeneous effects of debt on default and the loss-given-default (LGD). For the LGD, we draw on lenders' own loan-level estimates, which are reported to the central bank as inputs to bank capital regulation.

We find substantial heterogeneity in the slope of the Debt Laffer Curve. For most borrowers, the Debt Laffer Curve is upward sloping with a slope close to unity, implying that the net present value of repayment declines one-for-one with debt relief. However, the slope of the Debt Laffer Curve has a long left tail, and it is negative for 5-8% of borrowers. For these loans, debt relief improves welfare for both the borrower and the lender. For 8-13% of borrowers, the slope is less than 0.2, implying that actual cost of debt relief is only one-fifth of its nominal cost.

Our framework highlights that the Debt Laffer Curve is most likely to invert for borrowers with the highest sensitivity of default with respect to debt and the highest loss-given-default. In the data, we show that these two moments are positively correlated. Moreover, they are generally higher for borrowers with higher indebtedness and for borrowers in default. Our results thus pinpoint the borrowers for whom debt relief provides the largest benefit at the lowest cost to lenders.

In the final part of the paper, we interpret the empirical findings through the lens of a quantitative model of household debt and default. In the model, households have long-term mortgage debt denominated in foreign currency. Labor supply is

endogenous. Households can choose to default, subject to a utility cost and wage garnishment by lenders. The presence of wage garnishment in default generates a debt overhang channel, whereby highly indebted borrowers curtail labor supply.

We simulate the debt relief policy in the model. The model quantitatively matches the default and labor supply responses to debt relief in the data. To do so, the model calibration implies a utility cost of default of 16% in consumption equivalent terms and a Frisch elasticity of 0.16.

The model generates a hump-shaped Debt Laffer Curve that inverts for highly indebted borrowers. Cash flows to the lender fall sharply at default for two reasons. First, default leads borrowers to contract labor supply due to debt overhang, reducing creditors' ability to collect in default. Second, households face a cost of default, so default only arises once the cash flow benefit is sufficiently large, implying a large decline in cash flows to the lender. Thus, on the margin, debt relief that moves a household out of default entails a large benefit for the lender. This benefit is strongest for households with high, but not extremely high, debt burdens, similar to empirical findings.

Our paper focuses on the impact of a one-time unexpected debt relief policy in the aftermath of a debt crisis. Other studies, focusing on developing countries, find smaller benefits of debt relief on repayment and income (Kanz, 2016; Giné and Kanz, 2018; Karlan et al., 2019; Indarte and Kanz, 2024), in part because debt relief changes borrowers expectations about future bail outs, increasing moral hazard. In our context, we find that borrowers receiving debt relief were not more likely to take on debt after the policy, which suggests that moral hazard concerns may have been limited. We conjecture that this was because the policy, based on a legal ruling, was believed to be exceptional and was unlikely to be repeated. This contrasts with environments where *ad hoc* debt relief policies occur regularly.

Related Literature. This paper is related to three strands of literature.

First, this paper is closely related to research examining the effect of household debt relief on default rates and borrower outcomes. Several studies find that debt relief reduces default rates and boosts consumption (Agarwal et al., 2017; Ganong and Noel, 2020; Aydin, 2021; Cespedes et al., 2021; Agarwal et al., 2023; Dinerstein et al., 2024).² There is also evidence that debt relief for distressed borrowers can

²A related strand of literature finds that debt relief has sizable effects on aggregate demand and output (Auclert et al., 2019; Auclert and Mitman, 2022).

increase income and reduce mortality (Dobbie and Song, 2015; Di Maggio et al., 2020; Bruze et al., 2024). Ganong and Noel (2020) document that debt relief for distressed mortgage borrowers has the largest effect when it increases households' short-term liquidity, whereas reducing long-term debt burdens without increasing liquidity is not effective.³ A recent study by Kluender et al. (2024) finds that writing off non-performing medical debt valued at cents on the dollar has no impact on borrower outcomes.

We make several contributions relative to this literature. We study the impact of debt relief on default and income across the full distribution of indebted households, rather than focusing solely on distressed borrowers. The broad scope of the policy allows us to document the heterogeneous effect of debt relief, with effect sizes that are non-monotonic in initial indebtedness. These non-monotonic responses help understand why debt relief can be less effective for extremely indebted borrowers (e.g., Kluender et al., 2024). Our finding that an increase in income is driven partly by reduced unemployment is also new. Moreover, combining our heterogeneous effects with lenders' loss-given-default estimates, we estimate the slope of the Debt Laffer Curve across the distribution of affected borrowers.⁴ Our results on the positive relation between banks' loss-given-default estimates, the effect of debt on default, and indebtedness are also new to the literature. Finally, we show that the default and income responses are consistent with a dynamic model of mortgage debt overhang and labor supply that builds on the framework of Campbell and Cocco (2015).⁵

Second, our paper contributes to the literature on the Debt Laffer Curve. This literature primarily focused on debt overhang for sovereign borrowers (Krugman, 1988; Sachs, 1989), and empirical estimates of the Debt Laffer Curve have typically relied on macro-level data (e.g., Claessens, 1990; Cohen, 1993). To our knowledge, no studies have estimated the Debt Laffer Curve using micro data and quasi-experimental variation. We believe this paper is the first to show empirically that Debt Laffer Curve can invert for highly indebted borrowers. Our findings thus provide empirical support for the theoretical analysis by Eberly and Krishnamurthy (2014), who argue that reducing debt for borrowers with high default risk can be in the interest of lenders, as it can

³At the same time, Dobbie and Song (2020) and Aydin (2021) show that reducing long-term debt obligations can be effective in boosting repayment rates for unsecured loans.

⁴In their conclusion, Dobbie and Song (2020) have a back-of-the-envelope calculation showing that debt relief does not increase loan repayments, but they do not consider heterogeneity in the effect, recovery rates, or discounting.

⁵Ganong and Noel (2020) also study the impact of debt relief within a structural model of consumption and default, but they do not model labor supply.

prevent greater loan losses due to default.

Third, this paper contributes to research on the impact of household debt and balance sheet shocks. A large literature finds that household net worth and debt shocks have substantial effects on household consumption, default, and real activity (Mian et al., 2013; Mian and Sufi, 2014; Mian et al., 2017; Fuster and Willen, 2017; Di Maggio et al., 2017; Verner and Gyöngyösi, 2020). High household debt can also affect labor supply through wealth effects and debt overhang (Bernstein, 2021; Gyöngyösi et al., 2023). This paper contributes to this literature by providing estimates of the effect of an exogenous change in debt on repayment and income. We find a negligible wealth effect of debt on labor supply but a role for debt overhang.

Roadmap. The paper is organized as follows. Section 2 describes the data. Section 3 discusses the institutional context. Sections 4 and 5 present the empirical results of the effect of debt relief on default and labor market outcomes. Section 6 provides our estimate of the Debt Laffer Curve. Section 7 presents the model, and Section 8 concludes.

2 Data

Our analysis exploits several administrative data sources at the borrower and loan level with information on loan characteristics, loan performance, borrower labor market outcomes, and demographics.

2.1 Household Credit Registry

Our main data source for loan-level information is the household credit registry (KHR-L10) from the Central Bank of Hungary. The household credit registry provides information on all household loans outstanding as of May 2012 or later. The data contain information on loan characteristics, including the date of origination, loan type, amount borrowed, currency denomination, maturity, and the identity of the creditor institution. Moreover, it provides monthly updates on the current outstanding debt and debt service cost. The combination of information on debt, debt service, maturity, and loan type allows us to back out every loan's interest rate each period. The credit registry also contains borrower-level information on borrower age and zip code of residence.

To study loan performance, we use the credit registry’s information on default status, which is recorded for all loans starting in 2008. A loan is defined as entering default when unpaid installments exceed the minimum wage for at least 90 days. We also rely on a supplement to the household credit registry (KHR-L11) that contains information on banks’ estimates of the loss-given-default (LGD) for each loan starting in June 2015. These estimates are provided by banks to the Central Bank of Hungary and are used as inputs to bank capital regulation. The estimates are based on banks’ internal credit risk models. The LGD is defined based on a threshold of default of 90 days, consistent with the definition of default in the credit registry data.⁶

2.2 Income Data from Pension Contributions

To study labor market outcomes at the borrower level, we draw on income data recorded in individual-level pension contributions data (ONYF). These data are provided by the Hungarian State Treasury. We use the data from 2013 to 2017. The data contain information on all income sources that require a pension contribution payment. This includes all types of employment contracts and various types of social transfers, including unemployment benefits and child-related transfers. There is no cap on pension contribution payments, so the data measure the total amount of income and are not subject to top-coding. These data also allow us to observe the start and end date of an employment or social transfer spell, as well as the amount of income received. For employment contracts, we also observe the tax identifier of the employer, the occupation code, and the reason for spell breaks in employment, such as workplace accidents and medical leave.

We rely on a matching of the credit registry and pension contribution data performed by the Central Bank of Hungary in 2019. The matching was based on personal information that is only available in the original data, including name, date of birth, and mother’s maiden name. Section C.1 in the data appendix provides further details on the matching. Overall, matched borrowers are broadly similar to the full population of borrowers.

⁶Banks that do not follow an Internal Ratings-Based (IRB) approach to modeling credit risk do not report LGD. We therefore use the LGD estimates of IRB banks to impute missing LGD for non-IRB banks using loan type, loan currency, month of origination, maturity, default status, loan size, outstanding debt over originated amount, and borrower age. The results are similar when dropping loans for which LGD is imputed.

3 Context for the 2015 Debt Relief Policy

In this section, we provide background on the Settlement Act, which provided debt relief for most FC debtor households. The program was implemented in the aftermath of a severe household foreign currency debt crisis. We first briefly outline the crisis that preceded the debt relief program. We then describe the institutional details of the debt relief program.

3.1 Foreign Currency Credit Boom and Crisis

Hungary experienced a large credit boom prior to the 2008 Global Financial Crisis. A large fraction of household loans originated during the boom was denominated in foreign currency. Foreign currency lending was widespread for several reasons. First, there was a large spread between Hungarian forint and Swiss franc interest rates.⁷ A stable exchange rate implied a persistent deviation from uncovered interest parity. This meant that FC loans had lower installments, holding fixed the exchange rate. Second, foreign banks expanded credit supply in order to gain market share in the newly contestable Central and Eastern European markets. Third, Hungary joined the EU in 2004. Many expected that Hungary would join the euro several years later, making currency mismatch with respect to the euro temporary (Fidrmuc et al., 2013). As a result, before the outbreak of the Global Financial Crisis in September 2008, 66 percent of household debt was denominated in foreign currency, primarily Swiss franc.

The 2008 Global Financial Crisis led to a large and unexpected depreciation of the Hungarian forint. The crisis also resulted in a strong appreciation of the Swiss franc. By 2014, the forint depreciated by about 64% against the Swiss franc relative to the pre-crisis exchange rate. This led to a substantial revaluation in outstanding FC debt burdens and monthly debt service payments. In aggregate, the household debt-to-GDP ratio was revalued by about 10 percentage points.

Figure 1 plots the distribution of the ratio of outstanding debt in 2014m12 to the originated amount. We refer to this ratio as $\tilde{D}$. The sample is all FC housing loans

⁷See Verner and Gyöngyösi (2020) and Gyöngyösi and Verner (2022) for more details on the causes and consequences of the foreign currency household lending boom. The spread on rates faced by borrowers opened after the unexpected removal of government subsidies for LC loans in late 2003. The removal of interest subsidies for domestic currency loans quickly resulted in a switch to foreign currency lending. From 2004 to 2008, the majority (79.3%) of new housing loans were denominated in foreign currency.

originated between 2000 and 2010 that were still outstanding in 2014.⁸ Variation in $\tilde{D}$ is driven mainly by the time of origination, loan maturity, and loan currency. For example, loans originated closer to the crisis, with longer maturities, at a stronger forint exchange rate, and in Swiss franc have the highest value of $\tilde{D}$, as these loans had the least amortization before the crisis and were exposed to the largest revaluation. We use $\tilde{D}$ as our measure of household indebtedness, as it captures the extent to which households are highly indebted relative to their own borrowing plans. Figure 1 shows that, because of the strong depreciation and limited amortization before the depreciation, the average FC borrower owed 30% more in 2014 than they originally borrowed before the crisis, and there is wide dispersion in indebtedness $\tilde{D}$.

The FC debt revaluation led to a large rise in household default rates. The vast majority of households were not hedged against the depreciation, so the depreciation represented a substantial negative shock to FC borrowers' liquidity and net worth.⁹ Figure 2a plots the evolution of the default rate on housing loans in the aftermath of the crisis. Following the exchange rate depreciation that started in late 2008, the default rate on housing loans gradually increased, reaching almost 20 percent for FC housing loans in 2014. In contrast, the default rate for LC loans rose more gradually, peaking at 8%.¹⁰

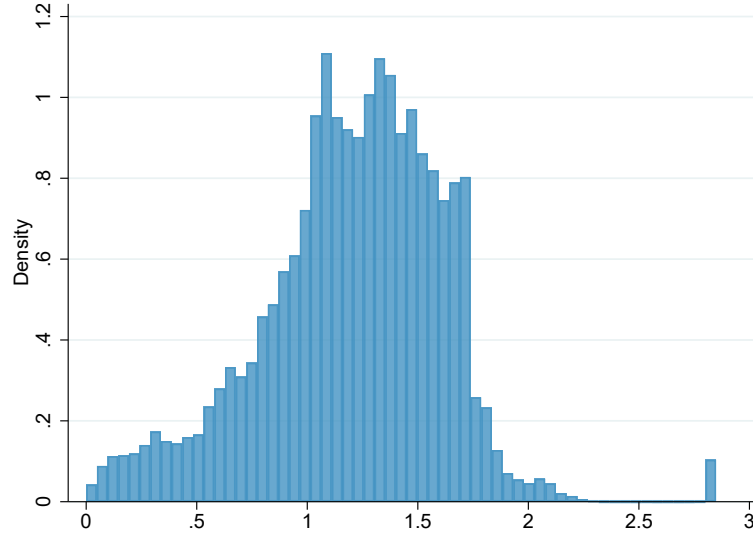
Indebtedness correlates strongly with default rates. Figure 2b relates default in December 2014 against outstanding debt relative to the originated amount, $\tilde{D}$. Borrowers with a higher outstanding debt balance relative to the originated amount were much more likely to default. Note that Hungary is a full recourse legal environment, and wage garnishment rates are typically 33% and can in some cases be as high as 50%. Moreover, households in financial distress could generally not receive relief through personal bankruptcy. There was no personal bankruptcy code in Hungary until 2015. The bankruptcy law introduced in 2015 is considered to be one of the least lenient in Europe and has had very low take-up (Walter and Krenchel, 2021).

⁸We do not include loans originated after 2010, as FC loans were effectively banned in 2010.

⁹Gyöngyösi et al. (2023) provide survey evidence showing that most households had negligible income and assets in foreign currency. As a result, consumption of foreign currency debtors was highly sensitive to exchange rate shocks.

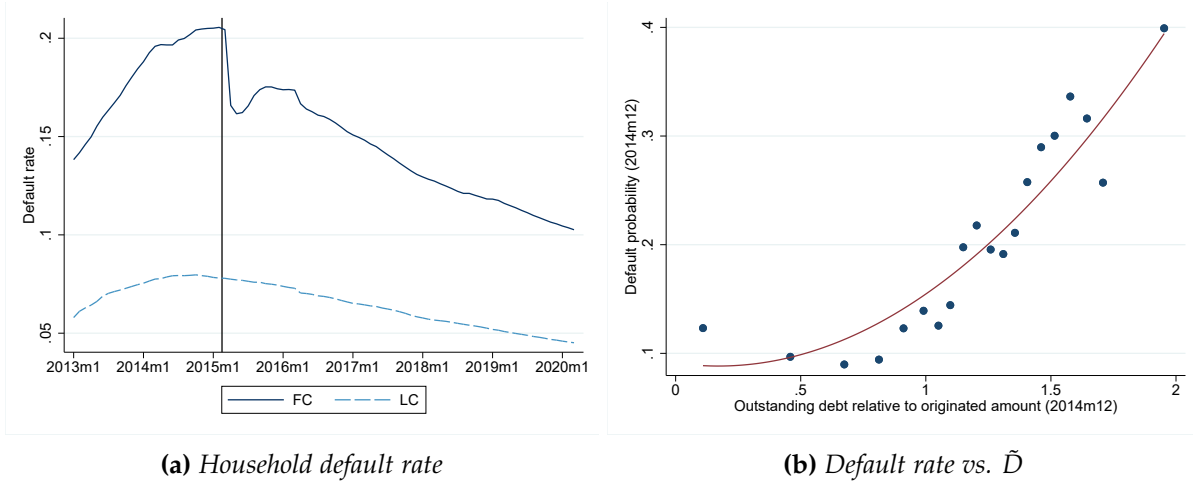
¹⁰In addition to the large increase in non-performing loans, the revaluation of household debt depressed household consumption and significantly contributed to the severity of the recession. Verner and Gyöngyösi (2020) and Gyöngyösi et al. (2023) provide a detailed overview of the credit boom and explore the consequences of the debt revaluation shock on the real economy and household consumption at the household and regional level.

Figure 1: *Distribution of Outstanding Debt in December 2014 as a Fraction of the Borrowed Amount*



Notes: This figure plots $\tilde{D}$, the distribution of outstanding debt in 2014m12 relative to the originated amount for FC mortgage and home equity loans. The sample is all housing loans originated between 2000 and 2010 that were outstanding at the end of 2014. The histogram is censored at the top 0.1 percentile for visual clarity.

Figure 2: *Household Default Rate and Indebtedness*



Notes: Panel (a) plots the time series of the default rate on housing loans around the Settlement Act. The figure is based on a balance sample of housing (mortgage and home equity) loans that were outstanding at the end of 2014. Panel (b) presents a binned scatterplot of the default rate in 2014 against $\tilde{D}_{2014}$, the outstanding debt balance in 2014m12 relative to the originated amount. The sample is all housing loans originated between 2000 and 2010.

3.2 Settlement Act

The large increase in household financial distress and the ensuing economic cost of the FC debt crisis led to a series of policies to address the high debt burdens of FC debtors. The Settlement Act, the focus of this paper, was the largest policy in terms of scope and scale.¹¹

Timeline of the policy. In June 2014, following inquiries by the government on the constitutionality of features of FC loans, the Supreme Court (Curia) ruled that two specific features of FC loan servicing practices were unfair. First, FC lenders usually charged an exchange rate spread when converting loan installments on FC loans into domestic currency. Second, banks unilaterally changed loan contract terms by increasing the interest rate spread and introducing other fees. Housing loans in Hungary were variable rate loan, but did not have a pre-specified basis rate. Instead, banks adjusted rates unilaterally based on changes in their cost of capital. These unilateral contract term changes were deemed unlawful if the loan contract did not specify the reason for passing through various bank costs to loan terms.

The Supreme Court decision provided a legal basis for FC borrowers to file lawsuits against their lenders for these practices. To prevent a large number of individual lawsuits from paralyzing the Hungarian legal system, the government passed the Settlement Act in November 2014 (Act LXXVII/2014) to regulate the compensation of foreign currency debtors.

Design and eligibility. The Settlement Act required banks to compensate eligible FC borrowers for excess payments from the application of the exchange rate spread and unilateral loan term modifications. These excess payments were treated as prepayments, so the compensation would take the form of debt relief. The debt relief was first used to repay penalties and other fees. The remaining amount was used for principal reduction. Households who had already repaid their loans received cash

¹¹Beyond the Settlement Act, there were several other smaller programs targeting foreign currency debtors. The Exchange Rate Cap, introduced in 2012, allowed debtors to pay their debt at a preferential exchange rate for a grace period of 5 years. The difference between the fixed, preferential rate and the actual market rate was collected in a special account as an HUF loan. The Early Repayment Program in 2011 also provided debt relief to FC debtor households in the form of a prepayment option at a preferential exchange rate. However, because the policy required full prepayment, it mostly benefited wealthier households who could afford to participate. FC loans that participated in the 2011 ERP are not in the sample for this paper. Gyöngyösi and Verner (2022) provide an overview of these other policies.

compensation. The average compensation for FC borrowers was 13.7%, and many borrowers saw a reduction of over 20% in their outstanding debt balances (Dancsik et al., 2015). Overall, the program led to a 3.1 percentage point reduction in household debt-to-GDP (IMF, 2015). The Settlement Act was implemented between March and April of 2015, and the cost was borne by the banking sector.

Although the policy was designed to help FC debtors, not all loans were eligible for the program. The Supreme Court decision was based on the EU's Unfair Contract Terms Directive, which only applied to contracts originated after Hungary joined the EU. Therefore, FC loans originated before May 1, 2004 were not eligible for debt relief. Moreover, the program only applied to loans originated before July 26, 2014. The program also only applied to mortgage and home equity loans.

The second element of the Settlement Act was the Conversion Program. It converted eligible loans to local currency at an exchange rate fixed in November 2014. This policy effectively ended the exchange rate exposure of the household sector as almost the entire stock of foreign currency household debt was converted to domestic currency.

Macroeconomic context. The Settlement Act was implemented during a relatively stable macroeconomic environment. Hungary's economy was recovering from the severe recession that started in 2008. Real GDP surpassed to its pre-crisis peak in 2014. Unemployment declined to 7.7% in 2014, from a peak of 11.2% in 2010. The housing market was also gradually recovering. Real house prices declined by about 35% from the start of the 2008 crisis and reached a trough at the end of 2013.

4 The Effect of Debt Relief on Default

This section presents evidence on the impact of debt relief on default. Estimating the causal effect of debt relief requires exogenous variation in exposure to the policy. We present results from three identification strategies. The first identification strategy is a regression discontinuity design based on the May 1, 2004 program eligibility cutoff. The second identification strategy is a difference-in-differences research design based on the same eligibility cutoff for the sample of loans originated in a tight window around the cutoff. To reinforce this analysis, our third strategy is a difference-in-differences design based on treatment intensity in the full sample of housing loans. The first strategy provides casual estimates under the weakest identifying assumption,

while the third strategy leverages a much larger sample and thus provides much more precise estimates, allowing for the estimation of detailed heterogeneous treatment effects.

4.1 Regression Discontinuity Design Evidence

Regression discontinuity. We first present evidence from a regression discontinuity (RD) design. The RD approach compares borrowers who are just above or below the date of origination eligibility cutoff for debt relief. These households borrowed at similar points in time, but only households above the cutoff received debt relief. Therefore, treatment assignment around the cutoff is essentially random. Formally, for an outcome Y such as default, we would like to estimate:

$$\tau_y = \lim_{z \rightarrow 0^+} \mathbb{E}(Y|Z_i = z) - \lim_{z \rightarrow 0^-} \mathbb{E}(Y|Z_i = z), \quad (1)$$

where z is the difference between the date of origination and May 1, 2004, the cutoff in eligibility for the Settlement Act.

We estimate (1) using local linear regressions. We use a triangle kernel and select the MSE-optimal common bandwidth based on Imbens and Kalyanaraman (2012) and Calonico et al. (2014). We use the same bandwidth for estimation across all outcomes. We thus focus on the sample of borrowers with loans originated 103 days before or after the cutoff. This sample contains 649 ineligible borrowers (origination dates before May 1), and 2886 eligible borrowers (origination dates on or after May 1). We report robust bias-corrected RD estimates and standard errors following Calonico et al. (2014, 2018). Although the running variable is discrete, we do not cluster by this variable, as Kolesár and Rothe (2018) show this can lead to worse coverage properties.

Manipulation and covariate balance tests. We start by providing two pieces of evidence to support for the validity of the RD design. First, there is no evidence of manipulation in the date of origination around the eligibility cutoff. In general, the most important threat to identification in an RD design is that agents can manipulate the running variable to receive a more favorable treatment (Cattaneo and Titiunik, 2022). This threat is unlikely to be a concern in this context. At the time of borrowing, households could not plausibly have foreseen the currency crisis in 2008, let alone the debt relief program in 2015. As a result, it is inconceivable that households borrowing in early 2004 would have postponed their borrowing to be eligible for the debt relief

program.

Households and lenders could also not retroactively manipulate the date of origination to become eligible for the program. In the credit registry we can observe data revisions, so we examined whether the date of the origination of individual loans changed from before the announcement of the policy to after it was implemented. All the dates of origination were unchanged. Furthermore, households could not become eligible by refinancing into a new loan after the policy announcement. As discussed above, the program only applied to FC loans originated before July 26, 2014, which was five months before the policy was announced. Therefore, ineligible borrowers could not obtain an eligible loan by refinancing into a new FC loan with a later date of origination.

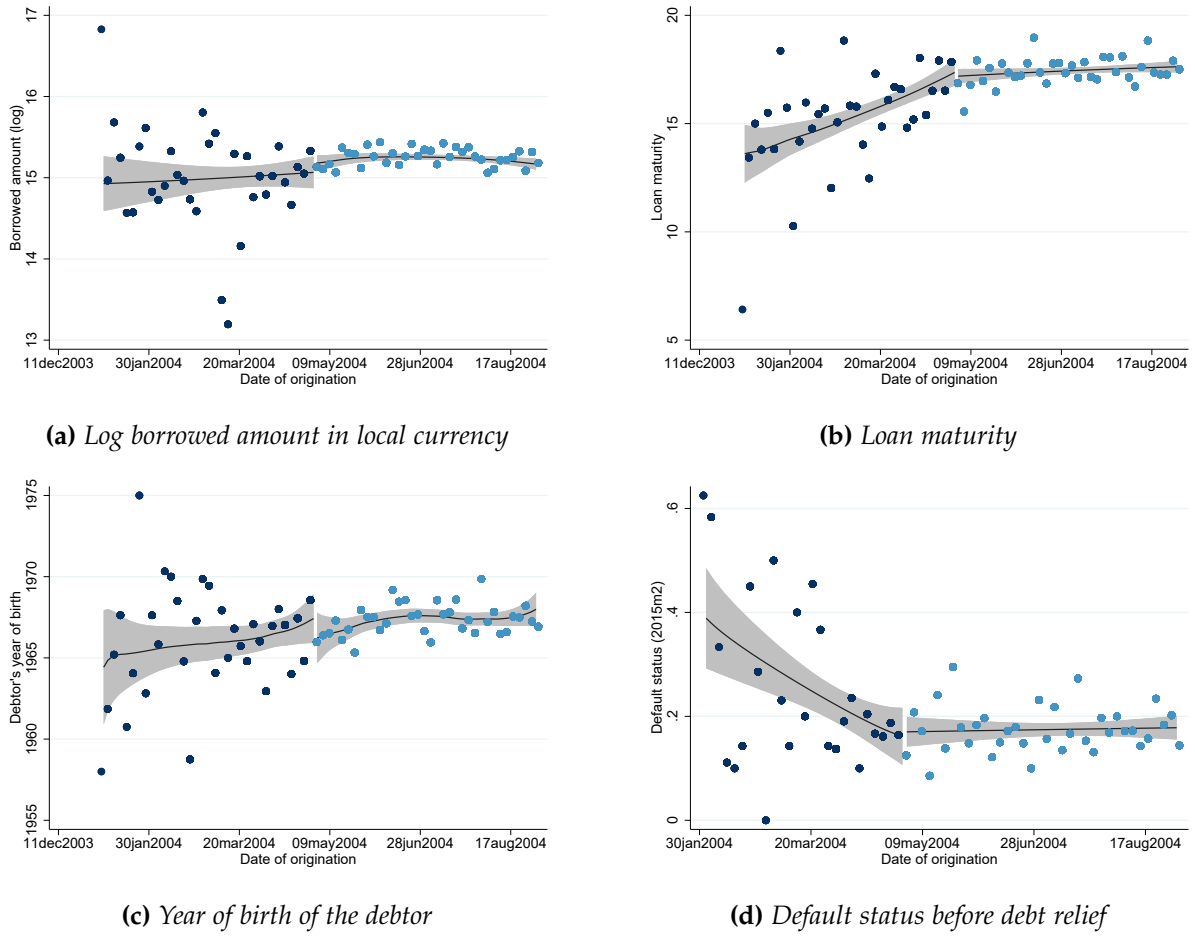
Consistent with no manipulation, Figure A.1 shows that the distribution of loan origination dates is smooth around the May 1, 2004 cutoff for program eligibility. There is no evidence of bunching in the running variable—the date of origination—around the cutoff. More formally, we cannot reject the null (p -value=0.21) of no systematic manipulation in the running variable applying the test for density discontinuity proposed by Cattaneo et al. (2018), which builds on the idea of testing for manipulation in McCrary (2008).

Second, loans and borrowers have similar observable characteristics around the eligibility cutoff. Figure 3 plots several loan and borrower characteristics as a function of the date of origination. We plot both binned means and a local first-order polynomial fit. Characteristics are measured right before the policy was implemented. Borrowers to the left and right of the cutoff have similar principal, loan maturity, and age before the program was implemented. These results confirm that there are no discrete jumps in loan or borrower characteristics around the May 1, 2004 cutoff. Importantly, Figure 3 also shows that borrowers to the left and right of the cutoff have similar default rates right before the policy was implemented. Hence, borrowers that do not receive debt relief are plausibly comparable to borrowers receiving debt relief around the eligibility cutoff. This supports the assumption that treatment assignment around the cutoff is as good as random.¹²

Effect of debt relief on default. Figure 4 presents a graphic visualization of the RD estimates of the effect of debt relief on debt, debt service, and default. Table 1 presents

¹²The higher variance of the sample means of in Figure 3 for early dates of origination occurs because we use equally-spaced bins, and there is a lower number of borrowers at earlier dates of origination.

Figure 3: RD Balance Tests: Loan and Borrower Characteristics by Date of Origination



Notes: These figures plot loan and borrower characteristics against the date of origination around the debt relief eligibility threshold. Loans originated before May 1, 2004 were not eligible for the Settlement Act. Dots are based on sample means constructed using equally spaced bins. Lines are based on local polynomial smoothed fits with a polynomial of order 1.

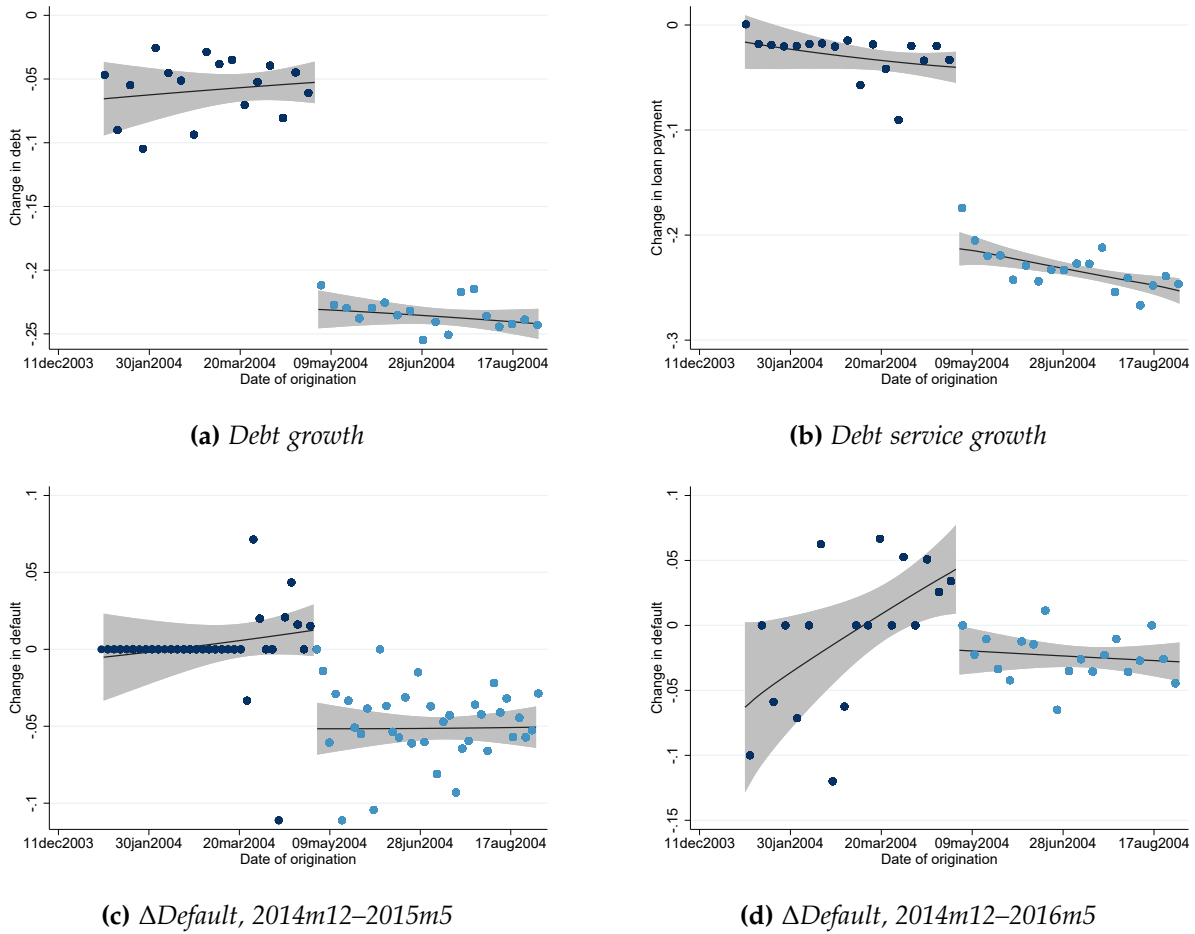
the corresponding RD estimates and standard errors. We report specifications both with and without control variables.

Figure 4a plots the growth in debt from February 2015 to April 2015 as a function of the date of origination. The growth in debt for loan i is calculated as

$$\Delta D_i = \frac{D_{i,HUF,2015m4} - D_{i,c,2015m2} \times \mathcal{E}_c}{D_{i,c,2015m2} \times e_c}, \quad (2)$$

where $D_{i,c,t}$ is outstanding debt for loan i in currency c at time t and $\mathcal{E}_c$ is the conversion exchange rate set by the Settlement Act. We calculate the growth in debt using the

Figure 4: *Effect of Debt Relief on Debt, Debt Service, and Default: Regression Discontinuity Evidence*



Notes: These figures present estimates of the impact of the debt relief on outstanding debt, debt service, and default using a regression discontinuity design around the Settlement Program eligibility cutoff. The horizontal axis shows the date of origination and the vertical axes show various outcomes measured in percent or percentage points, divided by 100. Loans originated before May 1, 2004 were not eligible for debt relief through the Settlement Act.

change in debt from February to April, as banks could implement the policy either in March or April 2015. Figure 4a shows that eligible borrowers receive a 19% reduction in debt, and there is a clear jump around the eligibility threshold. Figure 4b shows the program led to a similar decline in monthly debt service, defined as monthly interest and amortization, for eligible borrowers.

Figure 4c shows that default rates fell by about 6.5 percentage points from before the policy in 2014m12 to shortly after the implementation of the policy in 2015m5. Table 1 shows that the effect is highly statistically significant and almost identical when we account for loan and borrower controls. Part of the immediate effect on default

rates comes from a mechanical curing of loans in default with arrears smaller than the debt relief. Figure 4d therefore considers the change in default from 2014m12 to 2016m5, over one year after the policy. The effect on default is similar, with an estimate of 5.8%. This suggests that the debt relief policy persistently improved repayment rates.

Table 1: *Effect of Debt Relief on Debt, Debt Service, and Default: RD Estimates*

	$\Delta \ln(D)$		$\Delta \ln(\text{Debt service})$		$\Delta \text{Default}$ 2015m2-2015m5		$\Delta \text{Default}$ 2015m2-2016m5	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible	-0.190** (0.0169)	-0.191** (0.0169)	-0.175** (0.0137)	-0.175** (0.0139)	-0.0649** (0.0153)	-0.0646** (0.0153)	-0.0582* (0.0285)	-0.0578* (0.0282)
Bandwidth	103	103	103	103	103	103	103	103
Controls		Yes		Yes		Yes		Yes
Observations	3535	3535	3438	3438	3510	3510	3170	3170

Notes: This table shows the effect of being eligible for debt relief in the Settlement Act by exploiting the May 1, 2004 eligibility cutoff. It reports MSE-optimal common bandwidth, bias-corrected regression discontinuity estimates with robust variance estimator. Controls are year of birth, log originated amount, loan currency (Swiss franc or euro), and loan type (mortgage or home equity). All outcomes are in percent or percentage points, divided by 100. +, *, and ** indicate significance at the 10%, 5%, and 1% level, respectively.

4.2 Difference-in-Differences Evidence Based on Eligibility

The RD design provides compelling evidence that debt relief lowered default rates. We now turn to a difference-in-differences (DID) research design. The DID design allows for more precise estimates of the effect of default relief on default. The increased precision is valuable for estimating dynamic and heterogeneous treatment effects. The benefit of increased precision comes at the expense of making a stronger functional form assumption about the counterfactual evolution of outcomes in the absence of treatment based on the control group of ineligible borrowers, as is standard in the DID framework.

We analyze the dynamic impact of debt relief eligibility on debt and default by estimating the following specification:

$$y_{it} = \alpha_i + \delta_t + \sum_{k \neq 2015m2} (\beta_k \text{Eligible}_i \times \mathbf{1}_{t=k} + \Gamma_k X_i \times \mathbf{1}_{t=k}) + \varepsilon_{it}, \quad (3)$$

where y_{it} is an outcome variable, such as the default status of loan i at time t . α_i and δ_t are a set of loan and time fixed effects. $Eligible_i$ is the eligibility status of the loan based on the time of origination; it is equal to 1 for loans originated after May 1, 2004 and zero otherwise. $1_{t=k}$ is an indicator variable that equals one when $t = k$ and zero otherwise. X_i is a set of loan and borrower characteristics, which we interact with time fixed effects.

For this analysis we restrict our sample to a relatively tight window around the May 1, 2004 eligibility threshold. In particular, we focus on loans originated four months before to four months after May 1, 2004. We refer to this as the *2004-DID Sample*. Non-eligible loans were originated in January through April, while eligible loans originated in May through August.¹³ This provides a sample of treatment and control borrowers with similar observables. We estimate (3) by OLS and cluster standard errors at the loan level. Since most (85%) borrowers in the sample have only one housing loan, the results are almost identical with borrower fixed effects and when clustering standard errors at the borrower level. The identifying assumption for consistent estimation of $\{\beta_k\}$ is that, in the absence of treatment, treated borrowers' outcomes would have evolved in parallel to control borrowers' outcomes.

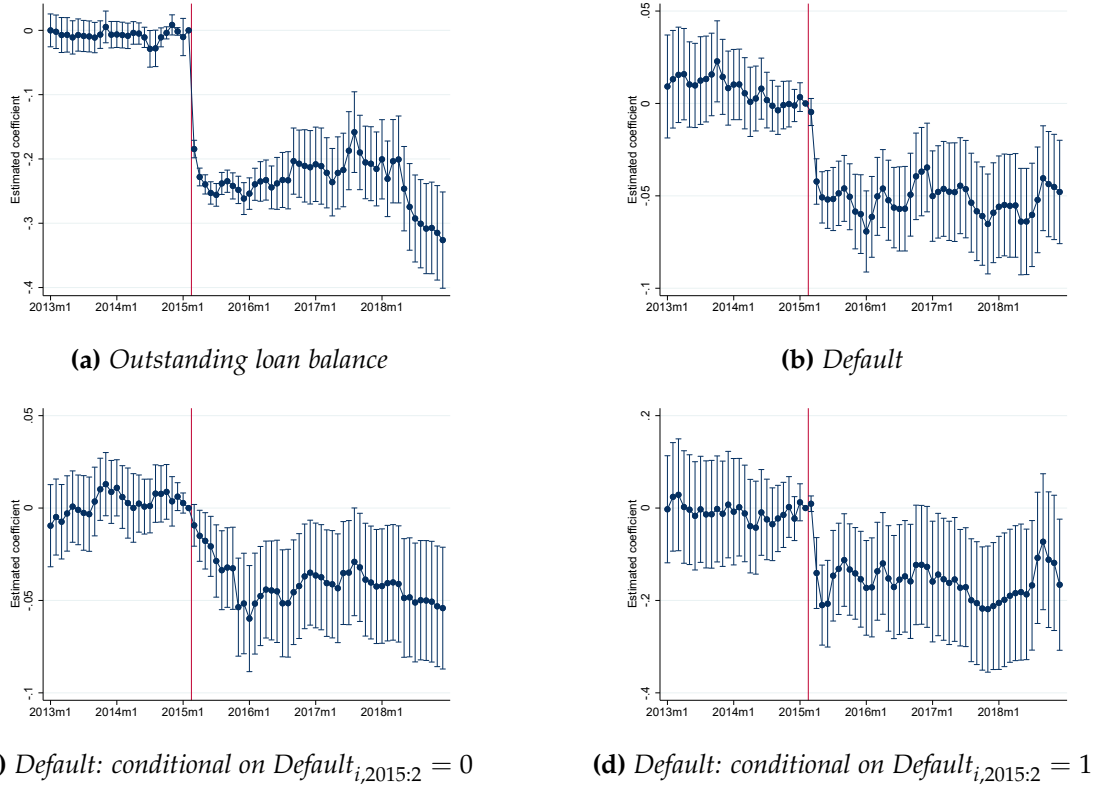
Figure 5 plots the estimates of $\{\beta_k\}$ from equation (3) for debt and default. The estimation period is 2013m1 to 2018m12, and the omitted base period is 2015m2. Panel (a) presents the estimates for outstanding debt. There is no differential pre-trend in the evolution of debt for eligible and ineligible borrowers in the two years leading up to the policy. Eligible borrowers then see an approximately 20% decline in outstanding debt after the policy, consistent with the RD estimate.

Panel (b) of Figure 5 presents the estimates of (3) with default as the outcome variable. There is no difference in the evolution of default status between eligible and non-eligible loans before the policy, consistent with parallel trends. Following the implementation of debt relief, there is a significant decline in the default rate of about 6 percentage points. The effect is persistent, lasting for at least four years until the end of our sample.

Columns 1-3 of Table 2 confirm these estimates based on a difference-in-differences model where we estimate the average effect of debt relief on default over the entire

¹³Figure A.3 shows that the estimated effect of debt relief eligibility is slightly larger when allowing for a wider bandwidth and including all loans originated in 2004, adding many more treated loans to the sample.

Figure 5: Effect of Debt Relief on Debt and Default: DID Evidence Based on Program Eligibility



Notes: This figure plots the dynamic impact of eligibility for debt relief from the Settlement Act on debt and default from estimating equation (3). Panels (c) and (d) split the sample by pre-policy default status. Error bars represent 95% confidence intervals based on standard errors clustered at the loan level.

post-policy period:

$$Default_{it} = \alpha_i + \delta_t + \beta Eligible_i \times Post_t + \Gamma X_i \times Post_t + \varepsilon_{it}, \quad (4)$$

In this specification, $Post_t$ is an indicator variable that equals one after 2015m2 and zero otherwise. Being eligible for debt relief reduces default rates by 6.5 percentage points in our preferred specification that includes loan controls and subregion fixed effects, both interacted with $Post_t$. This effect is almost identical to the RD estimate. Given that the pre-policy default rate was 18% for this sample loans, the 6.5 percentage point reduction represents a 36% reduction in the default rate, a large effect.

Heterogeneity by initial default status. Given the high initial default rates, a natural question is whether the reduction in default is driven by loans in default that are cured

Table 2: Effect of Debt Relief on Default: DID Evidence Based on Program Eligibility

	Reduced form			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible $\times$ Post	-0.0391** (0.00970)	-0.0693** (0.0115)	-0.0658** (0.0113)			
Debt Relief _{<i>i</i>} $\times$ Post				-0.240** (0.0583)	-0.417** (0.0703)	-0.408** (0.0707)
Individual & month FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan controls $\times$ Post		Yes	Yes		Yes	Yes
Subregion $\times$ Post			Yes			Yes
F-statistic				2099.4	915.1	829.1
R ²	0.780	0.782	0.786			
Observations	225962	225962	225962	221674	221674	221674

Notes: Columns 1-3 in this table present estimates of equation (4). Columns 4-6 instrument the percentage reduction in debt from debt relief with program eligibility. Loan controls are loan type, borrower year of birth, log originated amount, loan maturity, and indicators for loan currency, all interacted with $Post_t$. Subregion fixed effects are fixed effects at the NUTS-4 level, interacted with $Post_t$. Standard errors are clustered at the loan level. +, *, and ** indicate significance at the 10%, 5%, and 1% level, respectively.

by the policy or by reduced entry into default by loans that were not in default at the time of the program. We next present estimates of equation (3) separately for loans that were not in default. Default status is measured immediately before the debt relief was implemented in 2015m2.

Panel (c) in Figure 5 presents the estimates for loans that were not in default. For these borrowers, debt relief leads to a gradual decline in the likelihood of default to about 5 percentage points. Since all loans are not in default at the time of the policy, this effect is driven by lower rate of entry into default in the treatment relative to the control group.

Panel (d) in Figure 5 shows the effect for loans that were in default right before the policy. There is a large initial decline in default of nearly 20 percentage points. This is driven by debt relief curing arrears for about one-fifth of all borrowers in default. While this initial effect is mechanical, the reduction in default is persistent. There is a slight reversal in the effect from three to six months after the policy, as some borrowers re-enter default. However, most loans that are cured by debt relief are able to continue making payments to remain current. Thus providing debt relief to borrowers in default can persistently improve repayment rates. Given a pre-policy

default rate of 18% in the *2004-DID Sample*, this implies that about 46% of the overall decline in default in Figure 5b is explained by loans that were initially in default, while 54% is explained by reduced entry into default from loans that were current before the program.

Default semi-elasticity. Is the decline in the default rate large relative to the treatment? As we discuss further below, a key input to evaluating the costs and benefits of debt relief for lenders is the semi-elasticity of default with respect to debt relief. This measures the percentage point change in default for a one percent reduction in debt. We estimate this parameter by two-stage least squares, instrumenting the percentage reduction in debt from debt relief with program eligibility:

$$\begin{aligned} \text{Debt Relief}_i \times \text{Post}_t &= \alpha_i^{FS} + \delta_t^{FS} + \pi^{FS} \text{Eligible}_i \times \text{Post}_t + \Gamma^{FS} X_i \times \text{Post}_t + \varepsilon_{it}^{FS} \\ \text{Default}_{it} &= \alpha_i^{IV} + \delta_t^{IV} + \beta^{IV} \widehat{\text{Debt Relief}_i} \times \text{Post}_t + \Gamma^{IV} X_i \times \text{Post}_t + \varepsilon_{it}^{IV}. \end{aligned} \quad (5)$$

Debt Relief_i is defined as minus the change in debt due to debt relief from equation (2), i.e.,

$$\text{Debt Relief}_i = -\Delta D_i. \quad (6)$$

Note that a larger reduction in debt implies a larger value of Debt Relief_i , so a negative estimate of β^{IV} means that debt relief reduces the default rate.

Columns (4)-(6) of Table 2 present the estimates of β^{IV} from this two-stage least-squares specification. The estimates imply that a one-percentage point reduction in debt reduces default rates by 0.24 to 0.42 percentage points.

Heterogeneity in default semi-elasticity based on indebtedness. Highly indebted borrowers may be more likely to respond to debt relief by improving repayment rates, as these borrowers may be closest to the default threshold and face the most severe debt overhang problem. Table 3 presents estimates of the semi-elasticity of default with respect to debt relief from estimating equation (5) separately across five quintiles of indebtedness. Our measure of the degree of borrower indebtedness before the debt relief program is the ratio of outstanding debt to the originated amount, $\tilde{D}$. Recall that borrowers with higher $\tilde{D}$ are those that borrowed closer to the crisis and with a longer maturity and thus had amortized less before the depreciation, as well as those who borrowed in Swiss franc when the forint exchange rate was strongest.

Table 3: *Effect of Debt Relief on Default: Heterogeneity by Indebtedness*

Quintile of $\tilde{D}$	β^{IV}	S.E.	F-statistic	N
(1)	-0.128	(0.0809)	148.5	36044
(2)	-0.221 ⁺	(0.116)	233.4	39682
(3)	-0.162	(0.161)	153.4	39718
(4)	-0.897**	(0.233)	188.2	39656
(5)	-1.209**	(0.336)	184.0	39557

Notes: This table presents estimates of the semi-elasticity of default with respect to debt relief from estimation of (5) separately by quintile of $\tilde{D}$. $\tilde{D}$ is defined as the outstanding loan amount in 2014m12 relative to the originated amount. Standard errors are clustered at the loan level. +, *, and ** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 3 reveals that the effect of debt relief on default is substantially stronger for high debt borrowers. The semi-elasticity of default with respect to debt relief is between -0.13 and -0.22 for borrowers in the first three quintiles of indebtedness. However, for borrowers in the fourth quintile, the estimate rises in magnitude to -0.897, and it is even larger for borrowers in the fifth quintile of indebtedness. Below, we further explore heterogeneity in the sensitivity of default to debt across the distribution of indebtedness for the full population of loans, and we find that the effect is non-monotonic at the top end of the distribution of indebtedness.

4.3 Difference-in-Differences Evidence from the Full Sample

The evidence from the RD and DID designs based on debt relief eligibility have the advantage of offering sharp identification based on a credible source of variation. At the same time, they only use a small share of the full population of housing loans, leaving out many borrowers that were affected by debt relief. We next show that a difference-in-differences analysis exploiting variation in treatment exposure and intensity on the full sample of all housing loan borrowers yields similar findings. This much larger sample also allows us to explore even more detailed heterogeneity in the elasticity of default with respect to debt.

We focus on the sample of all borrowers with housing loans originated between 2000 and 2010. We include both FC and LC loans. The treatment exposure is the reduction in debt from debt relief defined in equation (6). Variation in treatment Debt Relief_{*i*} comes from several sources. The first is between FC and LC borrow-

ers. For LC borrowers, Debt Relief_{*i*} is approximately zero, while for FC borrowers Debt Relief_{*i*} is large (average = 13.7%). Note that this is effectively the comparison in Figure 2a, which suggests that debt relief led to a 6 percentage points larger decline in default rates for FC borrowers relative to LC borrowers. Second, within FC borrowers there is variation in Debt Relief_{*i*} driven by the identity of the lender. Banks differed in the extent to which they applied unilateral interest rate changes and exchange rate spread charges, so the magnitude of debt relief varies across borrowers with similar loans but from different banks. The third source of variation in Debt Relief_{*i*} is driven by borrower repayment history. Borrowers who remained current on their loans made large excess payments and hence qualified for larger debt relief. This third source of variation will likely bias the estimates down, as we would expect borrowers with the best repayment history to have the smallest default elasticity. We therefore control for the number of months a loan was in default before the policy to alleviate this potential bias.

Figure 6a plots estimates from a difference-in-differences model of the form

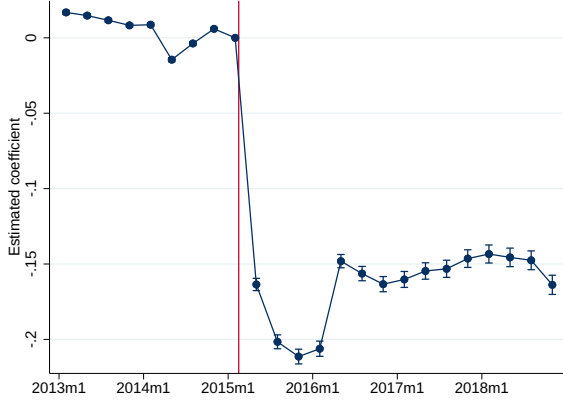
$$\text{Default}_{it} = \alpha_i + \gamma_t + \sum_{k \neq 2015q1} (\beta_k \text{Debt Relief}_i \times \mathbf{1}_{t=k} + \Gamma_k X_i \times \mathbf{1}_{t=k}) + u_{it}. \quad (7)$$

In this specification, β_k represents the semi-elasticity of default with respect to debt relief. The estimates in Figure 6a reveal that borrowers receiving larger debt relief have a similar evolution in default rates in the two years before the policy. Debt relief then leads to a persistent reduction in the default rate. The estimates are extremely precise in this sample. The full-sample semi-elasticity of default with respect to debt relief is about -0.16. Thus, a 20% debt relief lowers the default rate by about 3.2 percentage points. This estimate is about half the size of the estimate based on program eligibility. The smaller estimate in the full sample could be driven by compositional differences. It may also be driven by a potential downward bias from the fact that the size of the debt relief is correlated with repayment history, which our repayment history control only imperfectly controls for.

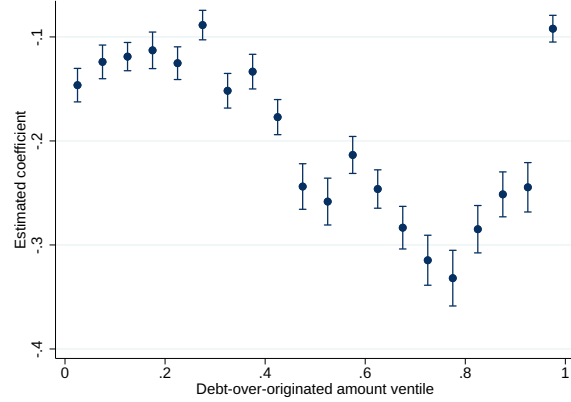
Heterogeneity. We next estimate heterogeneity in the effect of debt relief on default across the distribution of indebtedness $\tilde{D}$ and by default status.¹⁴ We partition the

¹⁴ $\tilde{D}$ ranges from 0.28 in the lowest decile to 1.79 in the highest decile of the full sample (see Figure A.4).

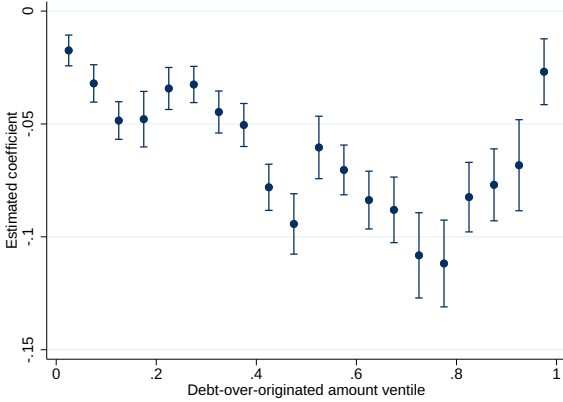
Figure 6: Effect of Debt Relief on Default: DID Evidence from the Full Sample



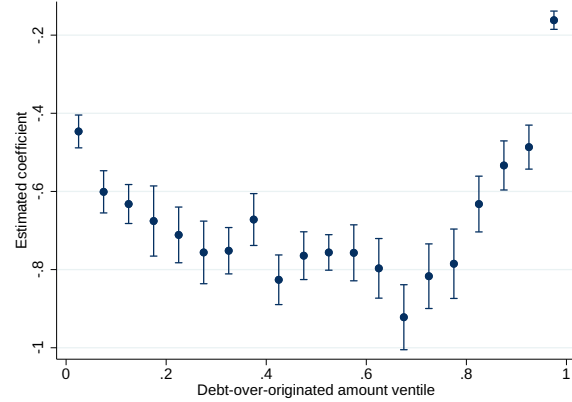
(a) Results from estimating (7) on the full sample



(b) Heterogeneity by $\tilde{D}$



(c) Heterogeneity by $\tilde{D}$; $\text{Default}_{i,2015:2} = 0$



(d) Heterogeneity by $\tilde{D}$; $\text{Default}_{i,2015:2} = 1$

Notes: Panel (a) plots the dynamic impact of a one percent reduction in debt from debt relief on the default rate. The figure is based on the estimation of equation (7) in the full sample of FC and LC housing loans. Panels (b), (c), and (d) present the heterogeneous effects of debt relief on default across the distribution of indebtedness. These figures are based on estimation of β from equation (8) across ventiles of indebtedness ($\tilde{D}$) and by default status. Error bars represent 95% confidence intervals based on standard errors clustered at the loan level.

sample into 20 groups of equal size (ventiles) by $\tilde{D}$ and estimate:

$$\text{Default}_{it} = \alpha_i + \gamma_t + \beta \text{Debt Relief}_i \times \text{Post}_t + \Gamma X_i \times \text{Post}_t + u_{it}. \quad (8)$$

Figure 6b plots the estimates of β across the distribution of indebtedness.¹⁵ The effect of debt relief on repayment rates is hump-shaped across the indebtedness dis-

¹⁵Table A.1 provides the full sample estimates of equation (8).

tribution.¹⁶ The magnitude of the effect rises with indebtedness across most of the distribution, with the largest effects in absolute value around the 80th percentile of indebtedness. The effect then becomes weaker at very high levels of indebtedness. The non-monotonic effect size is consistent with the idea that borrowers with moderately high debt are those that are closest to the default threshold and thus most sensitive to a change in debt. In contrast, extremely indebted borrowers are far above the default threshold. Therefore, on the margin a reduction in debt for these borrowers may not be sufficient for them to exit default.¹⁷

Panels (c) and (d) in Figure 6 present the heterogeneous estimates of β by default status. For both loans in and outside of default before the policy, we again see a non-monotonic relation. The size of the effect, however, is substantially larger in absolute value for borrowers in default. For example, for borrowers in default in the 70th percentile of the indebtedness distribution, a one percent reduction in debt implies a nearly one percentage point reduction in the default rate. This evidence suggests that debt relief is most likely to improve repayment rates for borrowers in default with a moderately high—but not the highest—level of indebtedness.

4.4 Moral Hazard and New Borrowing

We conclude this section by briefly discussing the potential moral hazard effects of the debt relief program. A common argument against *ex post* debt relief is that it may create moral hazard by changing households' expectations about future government interventions in credit markets. As a result, households may take on more debt than they otherwise would, anticipating that they will not bear the full cost of default. This concern is particularly pronounced in developing and emerging market countries, where discretionary debt relief policies are more common and where legal bankruptcy procedures are often absent or ineffective (Indarte and Kanz, 2024). Although Hungary's debt relief program was not, strictly speaking, discretionary, as it was based on a legal ruling, it was not the first time the government intervened in household credit markets.¹⁸

¹⁶Equivalently, the semi-elasticity of default with respect to debt relief is U-shaped.

¹⁷Note that the non-monotonic pattern in Figure 6b is not inconsistent with the pattern in the Table 3 discussed above. The reason is that the distribution of $\tilde{D}$ is shifted to the left for the *2004-DID Sample*, as these borrowers had more time to amortize their loans before the depreciation. Therefore, the *2004-DID Sample* sample has a much lower share of borrowers with $\tilde{D}$ above 1.5.

¹⁸For example the Early Repayment Program in 2011 allowed foreign currency households to prepay their FC loans at a preferential exchange rate.

To examine whether debt relief altered borrower behavior in credit markets, we test whether eligible borrowers were more likely to take on new debt after the policy. Taking on new debt could reflect better financial health or, potentially, overborrowing due to moral hazard. Figure A.6 shows no discernible differences in the propensity to take on new debt in the four years after the policy for eligible and non-eligible borrowers borrowing around the May 1, 2004 eligibility cutoff. This suggests that the policy did not differentially increase expectations about future debt relief among treated borrowers. This result is consistent with survey evidence from other European countries where governments have implemented or discussed debt relief measures. According to this evidence, households' awareness of such measures has no effect on their overall loan demand (Beckmann, 2017).

5 Effect of Debt Relief on Borrower Income

How does debt relief affect borrower income? In theory, debt relief can boost income, as lower debt burdens alleviate debt overhang and increase the incentive to supply labor (Myers, 1977; Donaldson et al., 2019). This can occur through several related channels. One is a reduction in the likelihood of wage garnishment, which acts as a tax on labor market effort (e.g., Obstfeld and Rogoff, 1996). Another is reduced financial distress, which could boost productivity through either rational or behavioral channels (Sergeyev et al., 2023, e.g.). At the same time, debt relief can reduce labor supply through a negative wealth effect when utility features complementarity between consumption and leisure (Chari et al., 2005; Lorenzoni, 2014). The impact of debt relief on income is therefore an empirical question.

For the analysis on income, we do not have the statistical power to credibly estimate moderate-sized effects of debt relief using the RD design or the DID based on eligibility in the *2004-DID Sample*.¹⁹ We therefore rely on the identification strategy based on debt relief intensity in the full sample. This analysis includes LC borrowers, who provide a natural control group. Since income is at the individual level, we aggregate debt relief to the borrower level. We estimate the effect of debt relief on

¹⁹Figure A.5 presents the difference-in-differences estimates of the effect of debt relief eligibility on income using the *2004-DID Sample*. The figure shows that borrowers eligible for debt relief in this sample see a gradual relative increase in income of 2-4%. This evidence is suggestive of a positive effect of debt relief on income. However, the wide error bands indicate that the difference-in-differences based on eligibility in this sample is underpowered to estimate modest positive effects of debt relief on labor income.

income using a difference-in-differences specification:

$$\ln(\text{Income}_{it}) = \alpha_i + \gamma_t + \sum_{k \neq 2015q1} (\beta_k \text{Debt Relief}_i \times \mathbf{1}_{t=k} + \Gamma_k X_i \times \mathbf{1}_{t=k}) + u_{it}. \quad (9)$$

We include the following control variables in X_i : borrower age, sex, one-digit occupation fixed effects, subregion fixed effects, a fixed effect for the firm identifier, default status before the policy, and the cumulative time spent in default before the policy. All controls are time-invariant and defined as of 2014, before the debt relief policy. Note that since Debt Relief is defined as the percentage reduction in debt from the debt relief program, a positive estimate of β_k implies that debt relief leads to an increase in income. We estimate (9) by pseudo-Poisson maximum likelihood to accommodate zeros in the dependent variable.

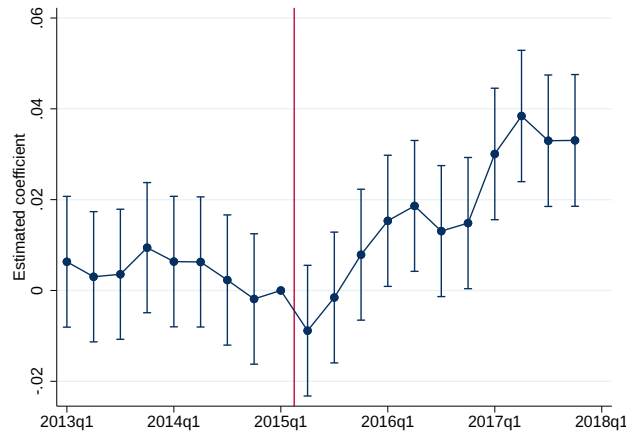
Panel (a) in Figure 7 plots the dynamic effect of debt relief on total borrower income. Total income includes both earned income and income from social transfers. The figure shows that the 2015 debt relief program has no relation with the evolution of income in the two years leading up to the implementation of the program, consistent with parallel pre-trends. After the debt relief policy, borrowers receiving larger debt relief see a gradual rise in income over the next two years. The estimates imply that a debt relief that reduces debt by 20% leads to a 0.76%(= 0.2×0.038) increase in income after 2 years.

Source of increase in income. Our granular administrative income data allows to us analyze the sources of the increase in borrower income from debt relief. Table 4 estimates the following specification for various components of income separately:

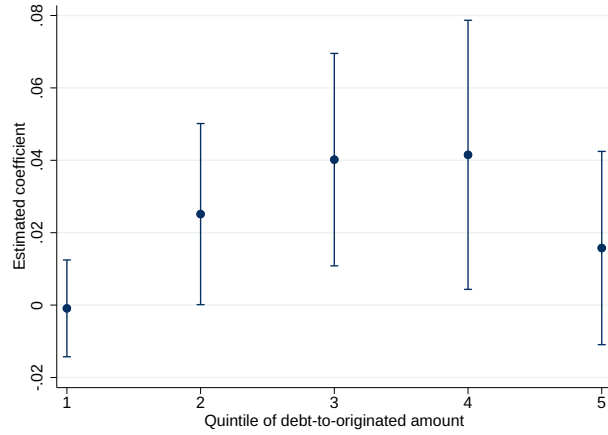
$$\ln(\text{Income}_{it}) = \alpha_i + \gamma_t + \beta \text{Debt Relief}_i \times \text{Post}_t + \Gamma_k X_i \times \text{Post}_t + u_{it} \quad (10)$$

Column 1 in Table 4 shows that debt relief has a positive and significant effect on total borrower income in post-policy period. Columns 2 and 3 in Table 4 decompose total income into earned income and income from government transfers. The increase in total income is mainly driven by an increase in earned income. Transfer income also rises in response to a fall in debt, but the estimate is noisy and not statistically significant. Moreover, transfer income accounts for a modest share of total income. Column 4 shows that debt relief reduces the likelihood of unemployment. The positive effect of debt relief on income is consistent with theories that debt reduction can boost labor

Figure 7: Effect of Debt Relief on Borrower Income



(a) Dynamic effect of debt relief on borrower income



(b) Heterogeneous effect of debt relief on income by quintiles of $\tilde{D}$

Notes: Panel (a) presents the dynamic effect of debt relief on borrower income. The figure is based on the estimation of equation (9). Panel (b) presents the heterogeneous effects of debt relief on income across the distribution of indebtedness. It is based on estimation of equation (10) by quintiles $\tilde{D}$. Note that positive estimates in both panels imply that debt relief leads to an increase in income. All specifications are estimated by pseudo-Poisson Maximum Likelihood to accommodate zeros in the dependent variable. Error bands represent 95% confidence intervals based on standard errors are clustered at the borrower level.

supply and earnings by alleviating debt overhang.²⁰

²⁰In addition to the direct effect of debt overhang on job search effort, wage garnishment can increase the incentive to leave one's job, as a creditor may not pursue a borrower at their next job, thereby increasing job turnover. For example, DeFusco et al. (2024) find that wage garnishment is associated with increased job separation rates. Increased job turnover may expose workers to higher unemployment risk.

Table 4: *Effect of Debt Relief on Labor Market Outcomes*

	Total income	Work income	Transfer income	Unemployment
	(1)	(2)	(3)	(4)
Debt relief $\times$ Post	0.0109** (0.00412)	0.0102* (0.00418)	0.0633 (0.0466)	-0.00189* (0.000927)
Individual & quarter FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2				0.134
N	12304299	12267370	1287700	12346353

Notes: This table shows the effect of debt relief on labor market outcomes. The regression specification is given by equation (10). The income regressions in columns 1-3 are estimated using pseudo-Poisson Maximum Likelihood. Controls include gender, age, one-digit occupation code fixed effects, subregion fixed effects (175 units), firm fixed effects, default status before the policy, and cumulative time spent in default before the policy. All controls are interacted with $Post_t$, and all controls are defined as of the pre-policy period in 2014. Standard errors are clustered at the individual level. +, *, and ** indicate significance at the 10%, 5%, and 1% level, respectively.

Heterogeneity by indebtedness. In theory, the effect of debt relief on income should vary with indebtedness, potentially in a non-monotonic fashion, as we saw for default in the previous section. For borrowers with limited debt, debt relief should have a limited effect on income, as these borrowers do not have a debt overhang problem. These borrowers might even reduce labor supply through a wealth effect on labor supply. For borrowers with moderate to high indebtedness, debt relief can relax the debt overhang problem, leading to an increase in labor supply and income. However, for borrowers with extremely high indebtedness, a marginal reduction in debt may have limited effect, as it does not meaningfully reduce borrowers' debt overhang problem.

Figure 7b confirms this intuition. It presents estimates of equation (10) with the log of income as the dependent variable across five quintiles of the indebtedness measure, $\tilde{D}$. The effect size is hump-shaped in indebtedness. For borrowers in the lowest quintile of indebtedness, debt relief has essentially no effect on income. For borrowers in the third and fourth quintiles of indebtedness, debt relief meaningfully boosts income. For these borrowers, a 20% debt relief implies a 0.8% increase in labor income in the post-policy period. This effect is highly statistically significant and four times larger than the average effect in Table 4. For borrowers in the top quintile of indebtedness, the effect becomes weaker. For these borrowers, 20% debt relief implies a 0.3% increase in income, and this effect is not statistically significant at the 5% level.

6 The Empirical Debt Laffer Curve

The Debt Laffer Curve relates the expected net present value (NPV) of debt to its face value. The slope of the Debt Laffer Curve thus measures the cost of face value debt relief to creditors. In this section, we combine the estimated effect of debt relief on default with lenders' loss-given-default estimates to evaluate the effect of debt relief on the NPV of debt across the distribution of indebtedness.

6.1 Simple Static Valuation

We first consider a simple static valuation framework to build intuition. Below we present results from a more general discounted cash flow valuation that relaxes some of the assumptions from the static valuation and explores more detailed heterogeneity. Suppose that the expected net present value of debt can be represented with a simple credit risk framework:

$$V(D) = D(1 - p(D)) + p(D)D(1 - L(D)), \quad (11)$$

where $p(D)$ is the probability of default and $L(D)$ is the loss-given-default (LGD), both of which are assumed to be functions of the face value of debt D . The NPV is thus the sum of two components. The first component is the face value of debt in repayment times the probability of repayment. The second component is the value of debt in default times the probability of default. The relationship between D and $V(D)$ is the Debt Laffer Curve.

How does $V(D)$ change with a change in the face value of debt? Taking the derivative of (11) yields the slope of the Debt Laffer Curve:

$$\frac{dV(D)}{dD} = 1 - p(D)L(D) - \frac{dp(D)}{dD/D}L(D) - \frac{dL(D)}{dD/D}p(D). \quad (12)$$

If there is no default or if the loss-given-default is zero, then the slope of the Debt Laffer Curve is one, as indicated by the first term. This implies a one-for-one relation between debt relief and the NPV. However, in the presence of costly default, this direct effect is diminished by three effects.

First, if the lender faces a loss from default, then the actual cost of debt relief is mitigated by the fact that some loans will default, $p(D)L(D)$. Second, reducing the face value of debt can reduce the probability of default, $\frac{dp(D)}{dD/D}L(D)$. The strength of

this term is influenced by the semi-elasticity of default with respect to debt, $\frac{dp(D)}{dD/D}$, and the LGD. If debt relief improves repayment rates, then this compensates for the face value reduction, especially when the LGD is high and default is costly. Third, reducing the face value of debt can reduce the loss given default, $\frac{dL(D)}{dD/D}p(D)$.

The representation in equation (12) reveals that the Debt Laffer Curve can flatten and potentially invert for borrowers with a high probability of default, a high semi-elasticity of default with respect to debt, and a high loss-given-default. Debt relief is thus most likely to benefit the lender when borrower repayment is highly responsive to debt relief and the cost of default to the lender is high.

Estimating the slope of the Debt Laffer Curve at a given level of debt D based on this simple framework requires the semi-elasticity of default $\frac{dp(D)}{dD/D}$, the probability of default $p(D)$, the loss-given-default $L(D)$, and the semi-elasticity of the LGD $\frac{dL(D)}{dD/D}$. To obtain estimates of these objects, we focus on the *2004-DID Sample*.²¹ We sort borrowers into five bins (quintiles) by pre-policy indebtedness, measured as the outstanding debt relative to the originated amount, $\tilde{D}$.²² For the probability of default, we compute the average pre-policy default rate in each quintile. For the semi-elasticity of default, we use the estimates of the heterogeneous effect of debt relief on default from Table 3. For the loss-given-default (LGD), we use lenders' estimates, taking the average in each quintile.

The final input to the slope of the Debt Laffer Curve in equation (12) is the semi-elasticity of the LGD with respect to debt, $\frac{dL(D)}{dD/D}$. From an empirical perspective, we would expect this object to be positive. In the data, LGD is strongly positively correlated with indebtedness (see Figure A.8). However, because the LGD data is only available starting in 2015Q2, we cannot estimate $\frac{dL(D)}{dD/D}$ using our difference-in-differences design, as the debt relief program was implemented in 2015Q1. We therefore estimate the semi-elasticity of LGD based on the RD design and assume that it is the same in all bins. This assumption is supported by the linear relation in Figure A.8.²³

With these estimates of $p(D)$, $\frac{dp(D)}{dD/D}$, $L(D)$, and $\frac{dL(D)}{dD/D}$, we obtain the slope of the

²¹In ongoing work, we are estimating the Debt Laffer Curve slope for the full population of loans. We expect that the Debt Laffer Curve is more likely to invert for highly indebted borrowers in the full sample, especially those in default, as indebtedness $\tilde{D}$ and the loss-given-default for loans originated later in the credit boom is generally higher than for the *2004-DID Sample*.

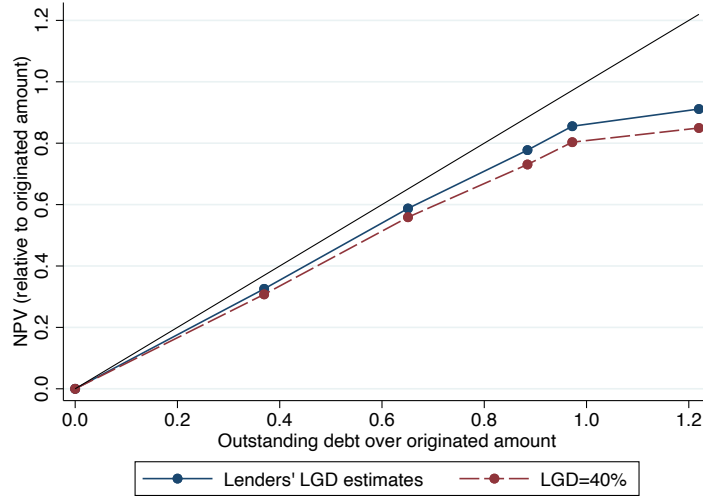
²²Note that we can perform a change of variable from D to $\tilde{D}$ as long as we scale by the originated amount D^0 . That is, defining $\tilde{D} \equiv \frac{D}{D^0}$, then it follows that $\frac{d(V(D)/D^0)}{d\tilde{D}}$ is equal to the right-hand-side of equation (12).

²³Specifically, Figure A.7 presents evidence from the RD design that debt relief lowered the LGD by about 2.5 percentage points. This implies a semi-elasticity of LGD of $0.13 = 0.025/0.19$.

Debt Laffer Curve for each quintile of indebtedness using (12). To visualize the Debt Laffer Curve in $\tilde{D}$ -space, we integrate the slope over indebtedness by computing

$$\widehat{V(\tilde{D}_k)} = \sum_{j=1}^k \left. \frac{d\widehat{V(\tilde{D})}}{d\tilde{D}} \right|_{\tilde{D}=\tilde{D}_j} (\tilde{D}_j - \tilde{D}_{j-1}), \quad \tilde{D}_0 = 0, \quad k = 1, \dots, 5.$$

Figure 8: *Debt Laffer Curve: Intuition from a Simple Static Valuation*



Notes: This figure plots the Debt Laffer Curve estimate based on the static valuation. The x-axis at each point is the mean of $\tilde{D}$ within the quintile. See text for details on the construction of the figure.

Figure 8 plots the estimate of the Debt Laffer Curve using the simple static valuation. There are three takeaways. First, for most borrowers, the Debt Laffer Curve is upward-sloping with a slope of close to one. Second, the Debt Laffer Curve is concave in indebtedness $\tilde{D}$. This implies that, on the margin, increasing outstanding debt balances has a diminishing effect on the net present value of expected repayment. Third, the Debt Laffer Curve flattens for highly indebted borrowers. The estimates imply that, at high levels of indebtedness, face value debt relief has a substantially smaller cost to the lender. Improved repayment rates combined with a high loss-given-default nearly offset the direct effect of face value reduction. The key intuition for the flattening of the Debt Laffer Curve is that, in the data, the probability of default, the semi-elasticity of default, and the loss-given-default are all rising in indebtedness.

Appendix A.1 reports results from an alternative parametric approach to estimating heterogeneity in the probability of default and the semi-elasticity of default. Specifically, we estimate these objects as a function of a higher-order polynomial in $\tilde{D}$.

That approach also finds that the Debt Laffer Curve becomes flat, and even slightly inverted, at high levels of indebtedness. Figure A.9 also illustrates how the probability of default, the semi-elasticity of default, and the loss-given-default are all rising in indebtedness.

6.2 General Discounted Cash Flow Valuation

Framework. The estimated Debt Laffer Curve based on the simple static valuation is useful for building intuition. However, it has several shortcomings. For example, it ignores discounting and dynamics in the probability of default and loss-given-default. It also only considers only one form of heterogeneity, namely by $\tilde{D}$, whereas the semi-elasticity of default and loss-given-default could vary based on many other characteristics such as default status or credit risk.

We now estimate the impact of debt relief on the NPV of each loan using a more general valuation framework. For each loan, let the face value of debt at the time of the policy be $D_i(T_i)$, where i indexes the loan and $T_i \in \{0, 1\}$ is the debt relief status. The change in the face value of debt for loan i due to the debt relief policy at the time of the policy is thus $\Delta D_i = D_i(1) - D_i(0)$.

We represent the net present value of loan i at the time of the policy by

$$V_i(T_i) = \sum_{j=1}^{M_i} \frac{(1 - p_{i,j}(T_i))pmt_{i,j}(T_i) + p_{i,j}(T_i)pmt_{i,j}(T_i)(1 - L_{i,j}(T_i))}{(1 + r^L)^j}, \quad (13)$$

where M_i is the remaining maturity, $p_{i,j}(T_i)$ is the probability of default, $pmt_{i,j}(T_i)$ is the loan payment, $L_{i,j}(T_i)$ is the loss-given-default, and r^L is the lender's discount rate. The slope of the Debt Laffer Curve for loan i is thus:

$$\Delta v_i \equiv \frac{\Delta V_i}{\Delta D_i} = \frac{V_i(1) - V_i(0)}{D_i(1) - D_i(0)}. \quad (14)$$

As in any potential outcomes setting, the challenge is that for a given loan we only observe one of the two treatment statuses. We therefore use the difference-in-differences design based on debt relief eligibility from Section 4 to impute the probability of default with and without debt relief for each loan $\{\hat{p}_{i,j}(0), \hat{p}_{i,j}(1)\}$. We then use banks' own estimated loss-given-default values for each loan. We again assume a fixed semi-elasticity of LGD with respect to debt; the results are very similar if we assume LGD is independent of T_i .

The intuition from equation (12) highlights that the debt relief can be provided at the lowest cost to the lender for borrowers with the highest elasticity of default and loss-given-default. We therefore allow for treatment effect heterogeneity when estimating the effect of debt relief on default using two approaches. First, we simply estimate a dynamic difference-in-differences model that interacts treatment status with loan characteristics and quintiles indebtedness ($\tilde{D}$). Second, to avoid overfitting, we utilize the counterfactual imputation procedure from Borusyak et al. (2024) to obtain heterogeneous counterfactuals. We impose the intuitive restriction that debt relief cannot increase the probability of default. We then estimate the difference in NPV with and without debt relief, normalized by debt relief, Δv_i , for each treated borrower using the counterfactual default rate implied by the model.

Before presenting the results, we make two remarks. First, note that the general valuation in equation (13) collapses to the simple static valuation in (11) under the following simplifying assumptions: the probability of default and loss-given-default are independent of the horizon j , the loan type is an annuity, and the lender's discount rate is equal to the loan interest rate $r_i = r^L$. With these assumptions we can write (13) as:

$$V_i(T_i) = (1 - p_i(T_i))D_i(T_i) + p_i(T_i)D_i(T_i)(1 - L_i(T_i))$$

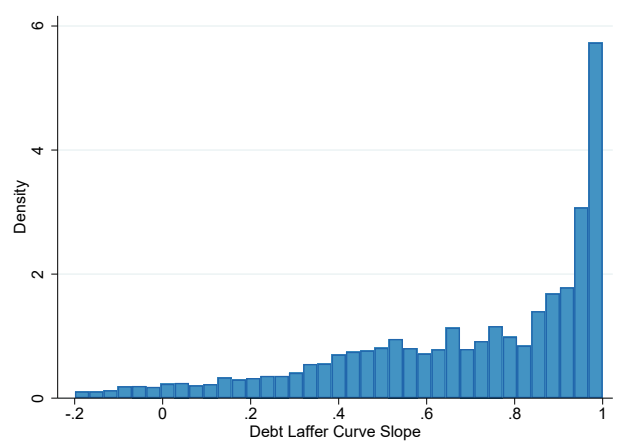
Further, if we assume that p_i , and L_i are functions of the face value of debt, then we recover the simple static valuation in (11).

Second, our calculation of the NPV in equation (13) ignores the effect of debt relief on the discount rate r^L . This approach likely overstates the cost of debt relief for the lender. By reducing default, debt relief would make cash flows safer, thus requiring a lower discount rate. Given that estimating the effect of debt relief on the discount rate requires taking a strong stand on the risk premium, here we focus on the cash flow effect of debt relief.

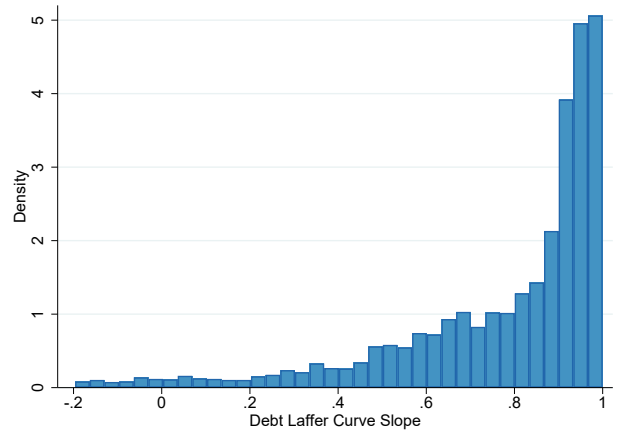
Results. Figure 9 plots the distribution of the estimates of the slope of the Debt Laffer Curve, Δv_i , for each loan. Panel (a) is based on the interacted difference-in-differences model. Panel (b) is based on the Borusyak et al. (2024) counterfactual imputation procedure. The results are consistent across both panels.

There is substantial mass around a slope of one, implying that a unit of face value reduction reduces the value of debt by one for most loans. However, there is also a thick left tail. About 5% (panel a) to 8% (panel b) of borrowers have a negative slope

Figure 9: Debt Laffer Curve Slope: DCF Valuation



(a) *Heterogeneity from interacted difference-in-differences model*



(b) *Heterogeneity based on Borusyak et al. (2024)*

Notes: This figure plots the distribution of the slope of the Debt Laffer Curve based on the DCF valuation. See text for details on the construction of these figures.

and are thus on the “wrong” side of the Debt Laffer Curve. For these borrowers, debt relief would come at no cost to the lender. About 8-13% of borrowers have Δv_i less than 20%. These loans are on a relatively flat portion of the Debt Laffer Curve. These estimates imply that debt relief can be provided at one-fifth of the face value cost to lenders for these borrowers.

Finally, the fact that the majority of borrowers are on the “right” (i.e., upward-sloping) side of the Debt Laffer Curve implies that the debt relief imposed significant losses on banks with loans affected by the policy. In Appendix B, we document that exposed banks saw substantial reductions in their capitalization. This, in turn,

reduced credit supply in the aftermath of the policy, pointing to a cost of the policy.

7 Model

This section presents a quantitative model of mortgage debt, default, and labor supply to interpret the empirical findings. The model can match the causal estimates of the effect of debt relief on default and income. It also generates a hump-shaped Debt Laffer Curve.

7.1 Set-up

We consider an economy with a continuum of *ex ante* identical homeowners with mortgage debt. Homeowners begin life with initial mortgage debt d_0 , which we calibrate to match the distribution of initial debt-to-income in the data.²⁴ Time runs from $t = 1, \dots, T$.

Preferences. Households have preferences over consumption, c_t , labor, n_t , and default, $\varphi_t \in \{0, 1\}$. We suppress the household subscript to lighten the notation. If the household chooses to default on its mortgage ($\varphi_t = 1$), it incurs a utility cost κ . Preferences are given by

$$\mathbb{E}_0 \sum_{t=1}^T \beta^t u(c_t, n_t, \varphi_t).$$

Mortgage contract. The mortgage is assumed to have maturity $m = T$ and interest rate r_d . Repayment begins in period $t = 1$ and continues until period T . Mortgages are denominated in foreign currency. Following our empirical setting, we assume the mortgage payment in the absence of default ($\varphi_t = 0$) follows an annuity:

$$pmt_t^0 = d_t \frac{r_d}{1 - (1 + r_d)^{-T+t-1}}.$$

²⁴We do not explicitly model housing or mortgage choice. This follows Campbell and Cocco (2015), who also do not model housing choice.

If a household does not default, the evolution of the household's mortgage debt in FC follows:

$$d_{t+1} = (1 + r_d)d_t - d_t \frac{r_d}{1 - (1 + r_d)^{-T+t-1}}. \quad (15)$$

Flow of funds constraints and default. A household's flow of funds constraint in local currency without default ($\varphi_t = 0$) is given by

$$c_t + a_{t+1} + \mathcal{E}_t pmt_t^0 = w_t n_t + (1 + r_a)a_t, \quad (16)$$

where a_t is a liquid asset that earns return r_a , pmt_t^0 is the mortgage payment if the household does not default, $\mathcal{E}_t$ is the exchange rate, and w_t is the wage. The wage follows an exogenous process

$$\begin{aligned} w_t &= \alpha_t + y_t, \\ y_t &= \rho y_{t-1} + u_t, \end{aligned}$$

where α_t an exogenous age component and y_t is a persistent shock. Moreover, we assume that there is a borrowing constraint on the liquid asset:

$$a_{t+1} \geq \underline{a}.$$

Default occurs when the household does not make the required payment $\mathcal{E}_t pmt_t^0$. In addition to the utility cost κ , we assume that if the borrower defaults, the lender can garnish ξ fraction of the borrowers' labor income. This assumption is motivated by the fact that wage garnishment was common by banks on FC debtors in default (Berlinger et al., 2021). The payment in default in local currency terms is

$$pmt_t^1 = \xi w_t n_t.$$

The flow of funds constraint for a household that is in default ($\varphi_t = 1$) is thus

$$c_t + a_{t+1} = (1 - \xi)w_t n_t + (1 + r_a)a_t. \quad (17)$$

When the household defaults, the outstanding debt balance (in FC) next period fol-

lows

$$d_{t+1} = d_t - \underbrace{(\mathcal{E}^{-1} \zeta w_t n_t - r d_t)}_{\text{Partial amortization}}. \quad (18)$$

Bellman equations. The exogenous household state variables are $(\mathcal{E}_t, w_t)$, and the endogenous state variables are a_t and d_t . We summarize the state as: $\mathbf{s}_t = (\mathcal{E}_t, w_t, a_t, d_t)$. The Bellman equation conditional on not defaulting ($\varphi_t = 0$) is

$$V_{0,t}(\mathbf{s}_t) = \max_{c_t, n_t, a_{t+1}} u(c_t, n_t, 0) + \beta \mathbb{E} [V_{t+1}(\mathbf{s}_{t+1}) | \mathbf{s}_t]$$

s.t. (15) and (16).

The Bellman equation conditional on defaulting ($\varphi_t = 1$) is

$$V_{1,t}(\mathbf{s}_t) = \max_{c_t, n_t, a_{t+1}} u(c_t, n_t, 1) + \beta \mathbb{E} [V_{t+1}(\mathbf{s}_{t+1}) | \mathbf{s}_t]$$

s.t. (17) and (18).

The period t value function is the maximum of the value functions without and with default

$$V_t(\mathbf{s}_t) = \max_{\varphi_t \in \{0,1\}} \{V_{0,t}(\mathbf{s}_t); V_{1,t}(\mathbf{s}_t)\}.$$

Given the household's optimal policy functions, the value of the loan for the lender (in LC) can be written recursively as

$$V_t^L(\mathbf{s}_t) = (1 - \varphi_t(\mathbf{s}_t)) \mathcal{E}_t p m_t^0 + \varphi_t(\mathbf{s}_t) \zeta w_t n_t(\mathbf{s}_t) + \frac{1}{1 + r^L} \mathbb{E} [V_{t+1}^L(\mathbf{s}_{t+1}) | \mathbf{s}_t]$$

where r^L is the lender's discount rate.

Model set-up comparison to the literature. Our model builds on the model of Campbell and Cocco (2015) (CC2015). However, we enrich the model to better match our empirical setting. In contrast to CC2015, we endogenize labor supply and allow for wage garnishment in default. This allows for a debt overhang effect on labor supply. It also makes the evolution of debt endogenous, adding an endogenous state variable to the household's problem. CC2015 instead assume that default requires the

household to exit homeownership and enter the rental market. The assumptions we make here are better suited to our empirical setting. Foreclosure was limited following a foreclosure moratorium imposed in 2010. Instead, banks pursued repayment by garnishing wages in a full recourse legal environment. We also assume different mortgage contracts and exogenous shocks. For example, we allow for FC mortgages and exchange rate shocks. Unlike CC2015, we also don't allow for pre-payment, which is rare in our empirical setting.

7.2 Model Solution and Calibration

We solve the model backward using the method of endogenous grid points. Because we have two endogenous state variables, we use bilinear interpolation between the endogenous and exogenous grid points.

Preferences. We solve the model with both non-separable and separable preferences. Our preferred specification is non-separable preferences as in Greenwood et al. (1988).²⁵ These are given by:

$$u(c_t, n_t, \varphi_t) = \left(c_t - \phi \frac{n_t^{1+\frac{1}{\eta}}}{1+\frac{1}{\eta}} \right)^{1-\frac{1}{\sigma}} \bigg/ \left(1 - \frac{1}{\sigma} \right) - \varphi_t \kappa,$$

where η is the Frisch elasticity of labor supply and σ is the elasticity of intertemporal substitution.

Externally calibrated parameters. We calibrate the initial mortgage loan size to match a pre-crisis payment-to-income ratio of 15%, with a standard deviation of 9 percentage points. This is consistent with the payment-to-income ratio for FC borrowers before the crisis in the data (see Gyöngyösi et al., 2023). The mortgage maturity is set to 20 years, the modal maturity in the data. We set the wage garnishment share

²⁵We also considered separable preferences, given by

$$u(c_t, n_t, \varphi_t) = \frac{c_t^{1-\frac{1}{\sigma}}}{1-\frac{1}{\sigma}} - \phi \frac{n_t^{1+\frac{1}{\eta}}}{1+\frac{1}{\eta}} - \varphi_t \kappa.$$

The key difference is that non-separable preferences remove the wealth effect on labor supply. We find that only the model with non-separable preferences can jointly match the default and labor supply responses we estimate in the data.

at $\xi = 33\%$, as wage garnishment in Hungary is typically set at 33% of income net of taxes and other deductions. We assume the exchange rate $\mathcal{E}_t$ follows a Markov process specified to match the exchange rate process expected before the 2008 crisis. In particular, we assume a positive but low probability of a large devaluation, based on Consensus Forecasts which the exchange rate was expected to remain stable.²⁶ We approximate the wage process using the Rouwenhorst (1995) method, which works well for discretizing persistent processes. We follow Campbell and Cocco (2015) set the borrowing constraint for the liquid asset to zero, $\underline{a} = 0$.

Internally calibrated parameters. We calibrate the utility cost of default κ and the elasticity of labor supply η to match the identified moments we estimated in Sections 4 and 5. Specifically, we target three moments. First, we target the pre-policy level of the default rate (18%). Second, we target the response of default to a 20% debt relief (-6.58 percentage points, based on the estimates in Table 2). Third, we target the estimated response of labor supply to 20% debt relief implied by the estimates in Figure 7.

To do this, we minimize the distance between the model generated moment and the moment in the data, giving equal weight to each moment. This yields a cost of default $\kappa = 0.21$, which implies that default is as costly as a 16.4% reduction in consumption for the marginal defaulter. The large utility cost of default is consistent with the evidence of limited strategic default from Ganong and Noel (2023). The labor supply elasticity is $\eta = 0.16$, which is similar to labor supply elasticity estimates in microeconomic studies of labor supply (Chetty et al., 2011; de Silva, 2023).

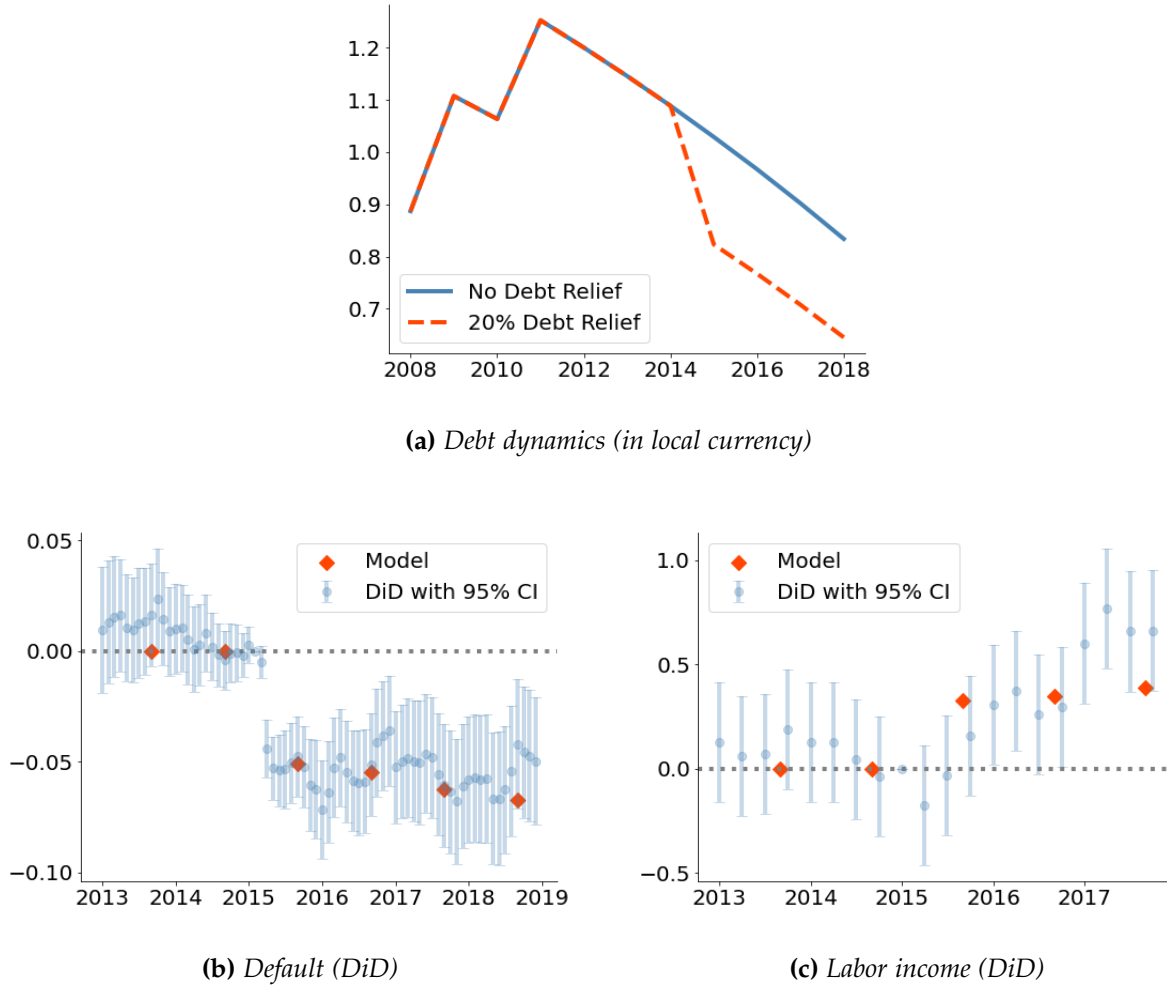
7.3 Debt Relief and the Debt Laffer Curve

Simulation. To match the empirical experiment based on eligibility for debt relief from Section 4, we assume households borrow in 2004. We then simulate an exchange rate path to match the forint depreciation from 2008 to 2014 of 60%. Note that this exchange rate path was unlikely, but not a zero-probability-event from the perspective of households in the model.

In 2015, we consider a policy that exogenously reduces outstanding debt balances by 20%. This magnitude is similar to the average debt relief based on eligibility for the policy (see Figure 5a). The top panel in Figure 10 shows the evolution of the face

²⁶Appendix D.1 provides additional details on the calibration.

Figure 10: Effect of Debt Relief on Default and Labor Supply in the Model and the Data



Notes: This figure plots the model simulation. Panel (a) is the average value of debt (in local currency terms) with and without debt relief. The debt relief policy reduces debt by 20% in 2015. Panel (b) plots the impact of debt relief on the default rate in the model and the data. Panel (c) plots the impact of debt relief on labor income in the model and the data.

value of debt (in local currency terms) for households with and without the debt relief policy.

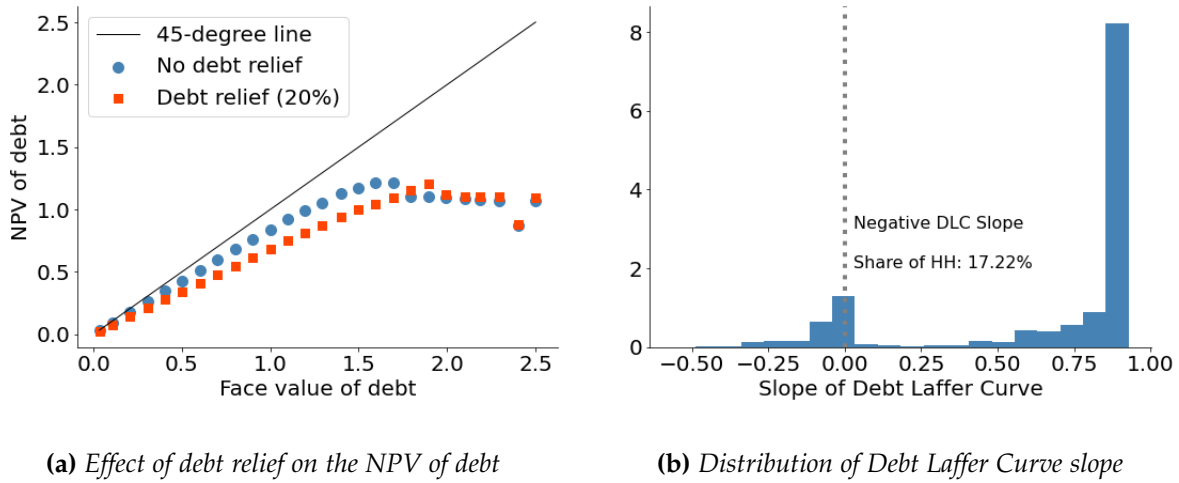
The bottom left panel shows the effect of debt relief on default in our model by comparing the average default rate with and without debt relief. Debt relief reduces the default rate by 6.5%, which is very similar to the effect we estimate in the data. Debt relief also leads to an increase in labor supply (bottom-right panel). Figure 10 shows that with a labor supply elasticity of $\eta = 0.16$, we obtain an increase in labor

supply that is similar to the data.

Debt Laffer Curve. Can debt relief increase the net present value of debt for highly indebted borrowers in the model? That is, are there instances where it can benefit both the borrower and lender to forgive rather than require repayment of debt? Figure 11 shows the answer is yes. We plot the Debt Laffer Curve implied by the model with and without debt relief. The x-axis is the face value of outstanding debt for each borrower. The y-axis is the expected NPV of debt service.

Figure 11 shows that the Debt Laffer Curve is hump-shaped, as in the data. It inverts for highly indebted borrowers. For borrowers with low debt, debt relief reduces the value of the loan. However, for highly indebted borrowers, debt relief has no effect or even a positive effect on the value of the loan. The largest increase in the NPV of the loan from debt relief is for borrowers with moderately high indebtedness. In the model, debt relief increases the value of debt for 17% of households.

Figure 11: Debt Laffer Curve in the Model



Notes: Panel (a) figure plots the expected net present value of debt across across the distribution of the face value of debt in the model with and without debt relief. Panel (b) plots the distribution of the slope of the Debt Laffer Curve.

7.4 Understanding the Mechanism

How does relief generate a reduction in default, an increase in labor supply, and an inverted Debt Laffer Curve in the model? Figure 12 summarizes the intuition. The figure plots the payment to the creditor without and with default as a function of debt,

as well as labor supply as a function of debt. The vertical dashed line is the default threshold. Debt relief moves a borrower to the left in the debt distribution, which can lead the borrower to cross the default threshold and resume repayment. Therefore, debt relief leads to a fall in the default rate.

Figure 12 shows that there is a drop in labor supply at the default threshold. Outside of default, labor supply with GHH preferences is governed by the first-order condition:

$$n_t = \left(\frac{1}{\phi} w_t \right)^\eta.$$

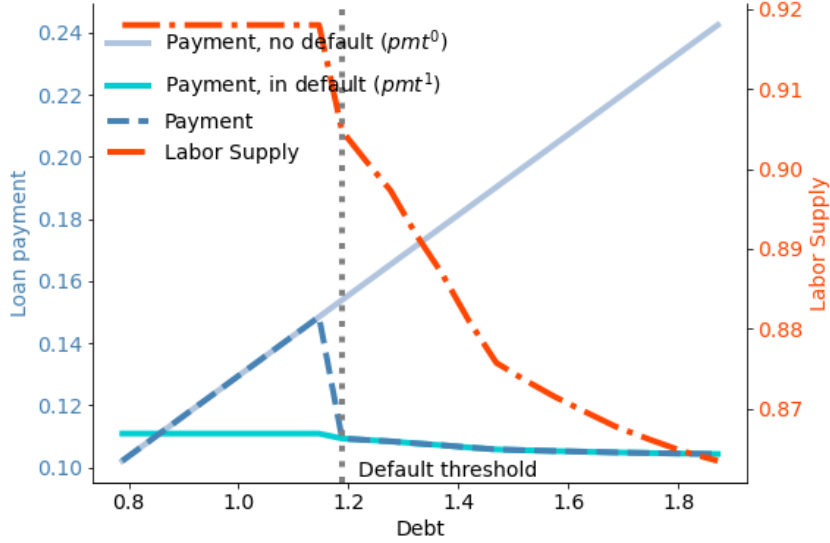
Therefore, outside of default, labor supply is independent of debt. However, in default, labor supply is given by the solution to

$$n_t = \left(\frac{1}{\phi} (1 - \xi) w_t - \frac{1}{\phi} \left(c_t - \phi \frac{n_t^{1+\frac{1}{\eta}}}{1+\frac{1}{\eta}} \right)^{\frac{1}{\sigma}} \beta \mathbb{E}_t[V_{d,t+1}(\mathbf{s}_{t+1})] \xi \mathcal{E}_t^{-1} w_t \right)^\eta$$

The first term within parentheses on the right-hand-side captures that default leads to wage garnishment, which reduces labor supply on the margin. Thus moving from in default to repayment alleviates the debt overhang problem and boosts the incentive to supply labor. The second term captures that labor supply affects the payment to the lender and thus debt in the next period. This term is also decreasing with debt, as the marginal value of debt $V_{d,t+1}$ is negative but becomes smaller in absolute value as debt increases. Therefore, reducing debt increases labor supply for households above the default threshold, even without crossing into repayment. These two forces imply that debt relief boosts labor supply.

Figure 12 also illustrates why the Debt Laffer Curve can invert. The payment received by the lender falls sharply at default for two reasons. First, labor supply drops, leading to a fall in payment collected through wage garnishment, $\xi w_t n_t$, as illustrated by the drop in the payment in default at the default threshold. Second, the utility cost of default drives a wedge between the cash flow received by the lender inside and outside of default. The utility cost of default leads the borrower to try to avoid default. Hence, the borrower continues repaying, even when the payment the lender could extract in default is lower than the required payment. Therefore, once the borrower defaults, the lender sees a large loss from default. Moving borrowers right above the default threshold into repayment thus benefits both the borrower and the lender.

Figure 12: Model Intuition



Notes: This figure illustrates the payment to the lender in the absence of default (solid light blue line), the payment to the lender in default (solid turquoise line), and the actual payment to the lender (dashed blue line) against the outstanding debt balance. The vertical dashed line in the default threshold. The figure also shows labor supply as a function of debt on the right axis.

8 Conclusion

This paper studies the impact of a large-scale debt relief program on debt repayment and borrower income implemented in the aftermath of a severe household debt crisis in Hungary. Debt relief leads to a persistent decline in default rates and a persistent increase in borrower income. The magnitude of the default and income responses are non-monotonic in *ex ante* indebtedness. Borrowers in the fourth quintile of indebtedness exhibit the strongest responses, while borrowers with the lowest and highest levels of indebtedness display the weakest responses. We combine the heterogeneous default responses with banks' estimates of loss-given-default to compute the slope of the Debt Laffer Curve, which summarizes the impact of a change in the face value of debt on the net present value of repayment. The Debt Laffer Curve is upward-sloping for most borrowers. However, we find that for highly indebted borrowers it flattens and can even invert. We show that a model of mortgage debt, default, and endogenous labor supply can reproduce these empirical patterns.

Our findings raise the question of why banks do not unilaterally provide debt relief to households on the wrong side of the Debt Laffer Curve. In debt crises, banks

are often reluctant to write down principal. There could be several reasons for this. Banks may be concerned that raising the possibility of debt forgiveness will reduce repayment rates (e.g., Mayer et al., 2014), especially since our findings suggest that borrowers in default with moderately high debt are those for whom the Debt Laffer Curve is most likely to invert. Banks may also face administrative costs, regulatory constraints, or reputation concerns that prevent them from writing down principal. Further, it is important to note that our estimated Debt Laffer Curve is relatively flat where it is inverted, so the gains to the bank may be small relative to these costs. Other forms of restructuring such as maturity extension may provide more cost-effective ways of providing debt relief.

Finally, there can be social benefits of debt relief that are not internalized by lenders. One such benefit is the expansion in labor supply we document at the micro-level. Debt relief can also increase income and repayment rates for all households through positive general equilibrium effects (e.g., Mian and Sufi, 2015; Korinek and Simsek, 2016; Auclert and Mitman, 2022). Therefore, the aggregate benefits of debt relief may exceed the micro-level benefits, leading to a flatter Debt Laffer Curve in the aggregate. These benefits must be weighed against adverse effects on credit supply and potential moral hazard effects of discretionary debt relief policies.

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Household Debt Relief and the Debt Laffer Curve

Online Appendix

Győző Gyöngyösi[†] Emil Verner[‡]

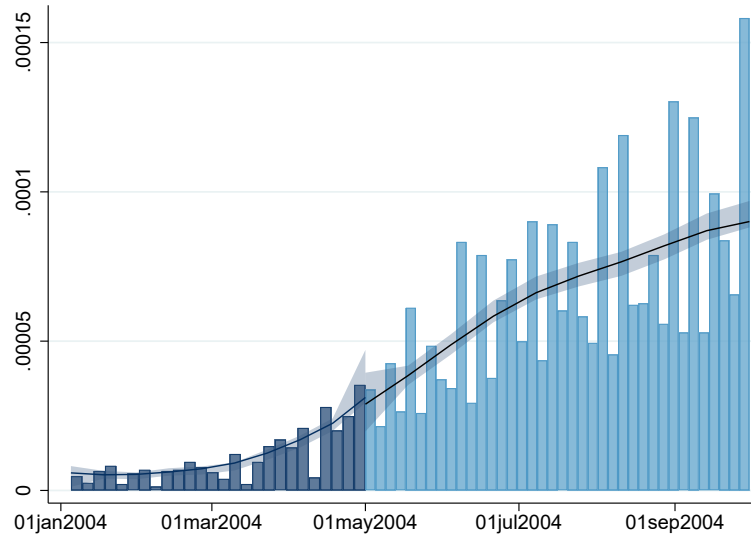
- Appendix A presents additional results.
- Appendix B discusses the impact of debt relief on banks and credit supply.
- Appendix C provides additional data description
- Appendix D provides additional details on the model.

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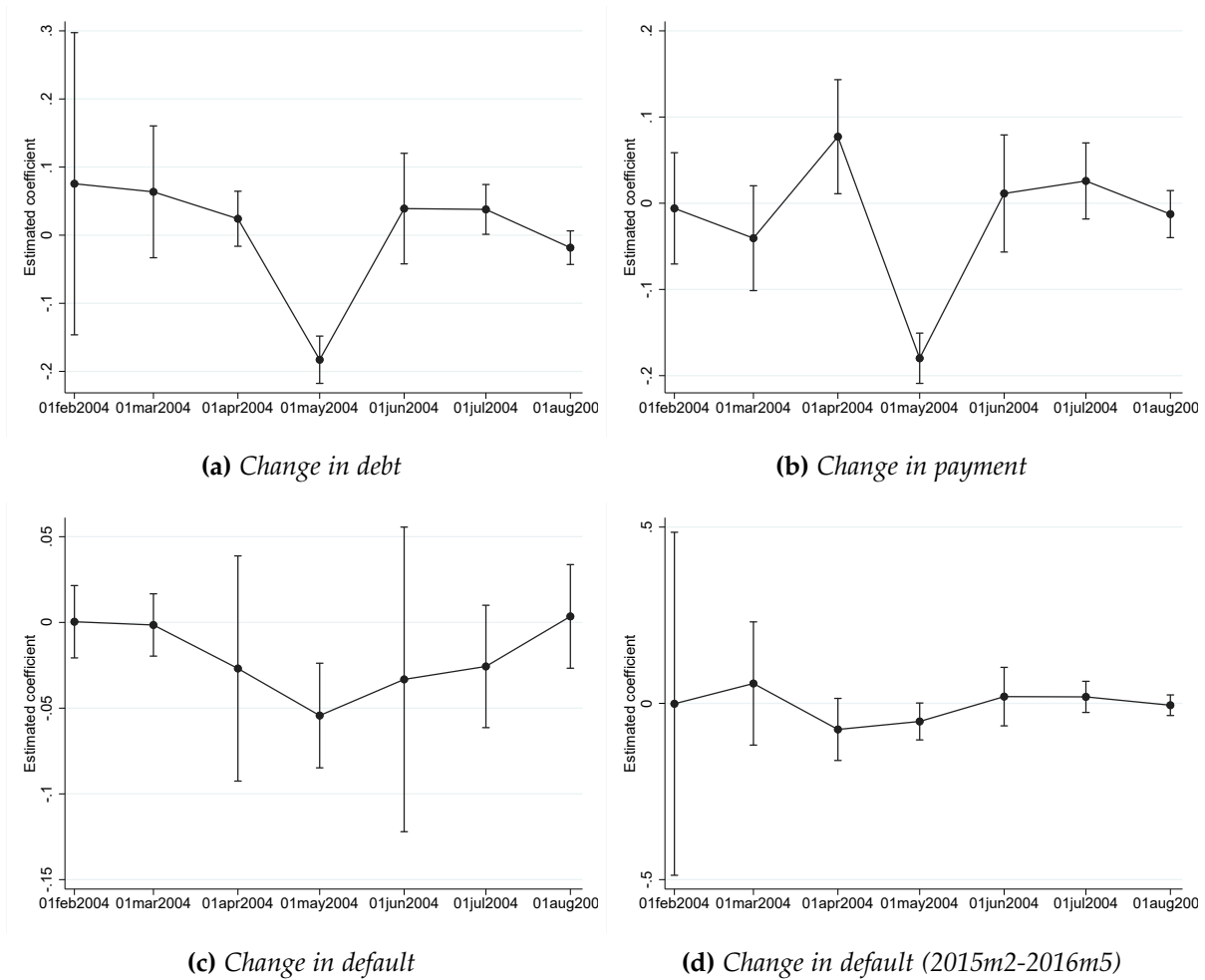
A Additional Results

Figure A.1: *RD Manipulation Test: Distribution of Loan Origination Dates around Debt Relief Program Eligibility*



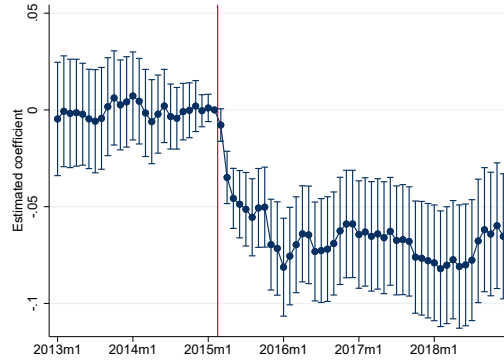
Notes: This figure plots a histogram of the number of foreign currency housing loan originations by calendar time in 2004. Loans originated before May 1, 2004 were not eligible for debt relief program, while loans originated after May 1, 2004 were eligible. The figure shows that the distribution of mortgage originations is smooth around this cutoff.

Figure A.2: RDD: Placebo Tests with Alternative Cutoff Dates



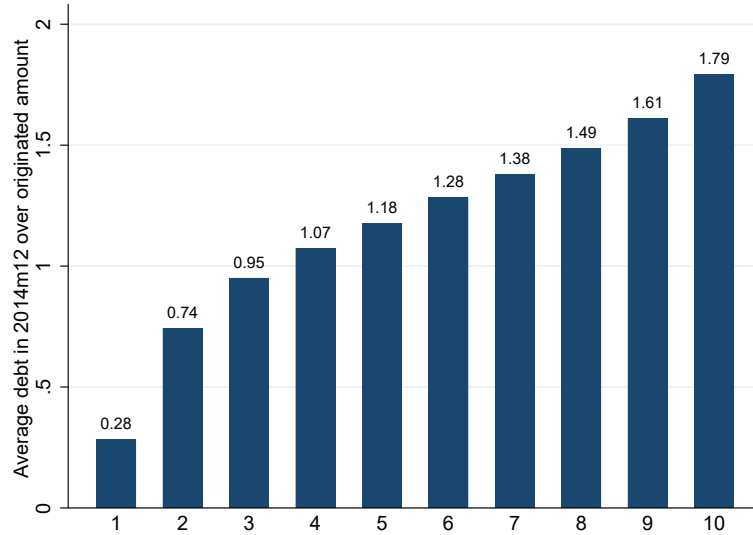
Notes: This figure plots RD estimates using the actual eligibility threshold (May 1, 2004) and using alternative placebo thresholds (February 1, 2004, March 1, 2004, April 1, 2004, June 1, 2004, July 1, 2004, and August 1, 2004). To avoid contamination from the actual treatment effect, we drop treated observations when placebo cutoff is below the true cutoff and drop control observations when placebo cutoff is above the true cutoff. The figure shows that there is no effect on outstanding debt, loan payments, or default at the placebo thresholds.

Figure A.3: *Effect of Debt Relief on Default: Robustness using a Wider Bandwidth around Program Eligibility*



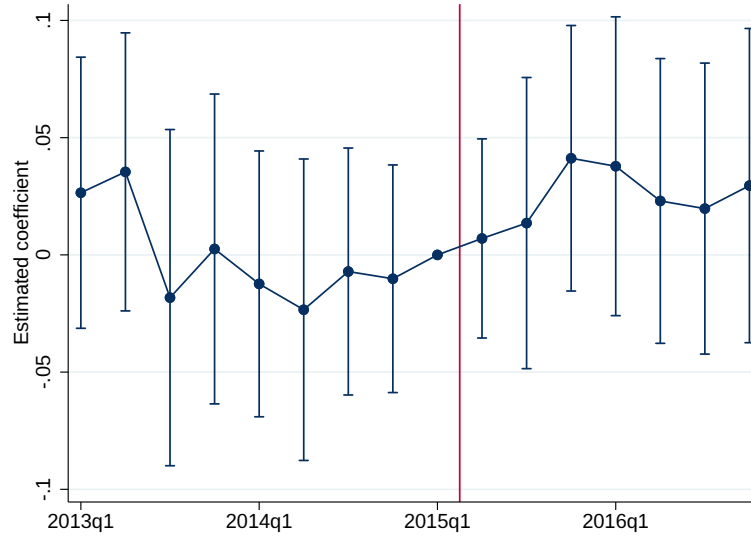
Notes: This figure plots the effect of debt relief program eligibility on default from estimation of (3), as in Figure 5b. However, instead of restricting to loans originated between March 1, 2004 and June 30, 2004, we include all loans originated in 2004. This provides a larger treatment and control group.

Figure A.4: *Distribution of Indebtedness $\tilde{D}$ in the Full Sample*



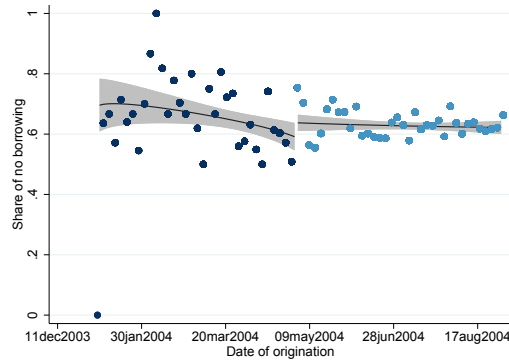
Notes: This figure plots the average value of $\tilde{D}$, the outstanding debt balance in 2014m12 relative to the originated amount, across deciles of $\tilde{D}$ in the full sample of FC housing loans.

Figure A.5: *Effect of Debt Relief on Income: DID based on Program Eligibility*



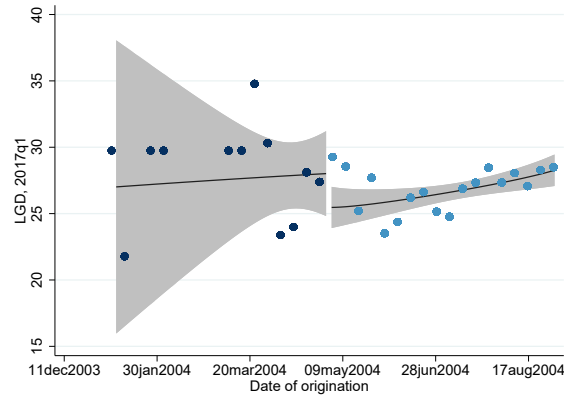
Notes: This figure presents the effect of debt relief eligibility on total borrower income. The figure is based on estimating equation (3) with log income as the dependent variable by pseudo-Poisson maximum likelihood. The sample is the 2004-DID Sample. Error bands represent 95% confidence intervals based on standard errors are clustered at the borrower level.

Figure A.6: *RD Estimates of the Effect of Debt Relief on Future Borrowing Behavior*



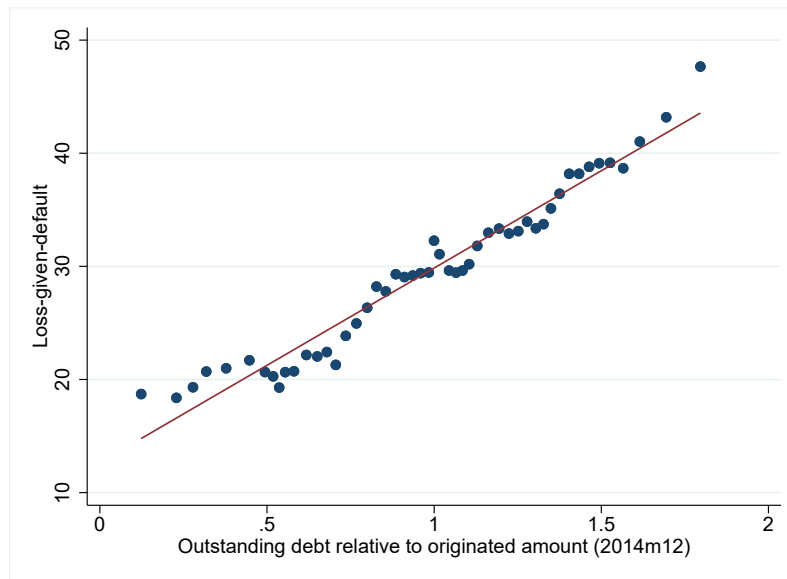
Notes: This figure presents the RD estimate of the effect of debt relief eligibility on future borrowing between May 2015 and December 2019.

Figure A.7: RD Estimates of the Effect of Debt Relief on Banks' Estimated Loss-Given-Default



Notes: This figure presents the RD estimate of the effect of debt relief eligibility on the loss-given-default.

Figure A.8: Loss-Given-Default and Indebtedness $\tilde{D}$



Notes: This figure shows that banks' estimated loss-given-default on FC loans is strongly positively correlated with our measure of indebtedness, $\tilde{D}$. The figure is based on a binned scatterplot on the full sample of FC loans.

Table A.1: *Effect of Debt Relief on Default: Full Sample DID Evidence*

	Default				
	(1)	(2)	(3)	(4)	(5)
Debt relief $\times$ Post	-0.0537** (0.00134)	-0.0537** (0.00187)	-0.0529** (0.00188)	-0.105** (0.00195)	-0.157** (0.00177)
Individual & quarter FE	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes
Municipality-Post FE			Yes	Yes	Yes
Default status (2014m12)				Yes	Yes
Time spent in default					Yes
R^2	0.802	0.802	0.802	0.803	0.813
N	84254730	84245441	83357310	83357310	83357310

Notes: This table shows the effect of a relative reduction in debt induced by debt relief on default in the full sample of housing loan borrowers. The regression specification is:

$$\text{Default}_{it} = \alpha_i + \gamma_t + \beta \text{Debt Relief}_i \times \text{Post}_t + \Gamma_k X_i \times \text{Post}_t + u_{it}.$$

Debt Relief_{*i*} is defined in equation (6). Standard errors are clustered at the loan level. +, *, and ** indicate significance at the 10%, 5%, and 1% level, respectively.

A.1 Debt Laffer Curve: Static Valuation using Parametric Estimation of Heterogeneity

This appendix presents an estimate of the Debt Laffer Curve based on the static valuation using a parametric approach to estimating heterogeneity. In this approach, we first estimate the heterogeneous impact of debt relief on default $p'(\tilde{D})$ across the distribution of $\tilde{D}$ by interacting the growth in debt due to the debt relief program, ΔD_i , with a polynomial in $\tilde{D}_i$, in our instrumental variables difference-in-differences framework:

$$\text{Default}_{it} = \alpha_i + \delta_t + \sum_{j=0}^J \beta_j \Delta D_i \times \tilde{D}_i^j \times \text{Post}_t + \Gamma X_i \times \text{Post}_t + \varepsilon_{it}. \quad (\text{A.19})$$

We set $J = 5$ to flexibly capture heterogeneity in $\frac{dp}{d\tilde{D}}$. Consistent with the difference-in-differences approach in section 4, we instrument $\{\Delta D_i \times \tilde{D}_i^j \times \text{Post}_t\}_{j=0}^J$ with eligibility for debt relief $\{\text{Eligible}_i \times \tilde{D}_i^j \times \text{Post}_t\}_{j=0}^J$. This yields an estimate of the semi-elasticity of default for each loan i .

Next, we estimate the pre-policy probability of default $p(\tilde{D})$ using a probit model with pre-policy default rate (measured in 2014m12) as the outcome variable. As the predictors, we again use a polynomial in $\tilde{D}$ and pre-determined loan and borrower characteristics. The probability-of-default model is:

$$p(\tilde{D})_i = \Phi \left(\sum_{j=0}^J \delta_j \tilde{D}_i^j + \phi X_i \right).$$

From this model, we obtain an estimate of the probability of default for each loan $\widehat{p(\tilde{D})}_i$.

Figure A.9a plots a binned scatterplot of the predicted probability of default, $\widehat{p(\tilde{D})}_i$, against indebtedness, $\tilde{D}$. The probability of default is rising in indebtedness, especially for high levels of indebtedness. The pre-policy predicted default rate for the most indebted borrowers is nearly 60%.

Figure A.9b presents a binned scatterplot of the semi-elasticity of default against $\tilde{D}_i$. The relation is upward-sloping across most of the distribution. Therefore, the effect of debt on default is strongest for moderate to highly indebted borrowers.²⁷ The semi-elasticity of default rises to above one for highly indebted borrowers.

Figure A.9c plots the average loss-given-default reported by lenders across deciles of $\tilde{D}$. The average loss-given-default ranges from 25% to 48%. It is highest for loans with the highest indebtedness. Intuitively, lenders see lower recovery rates from high

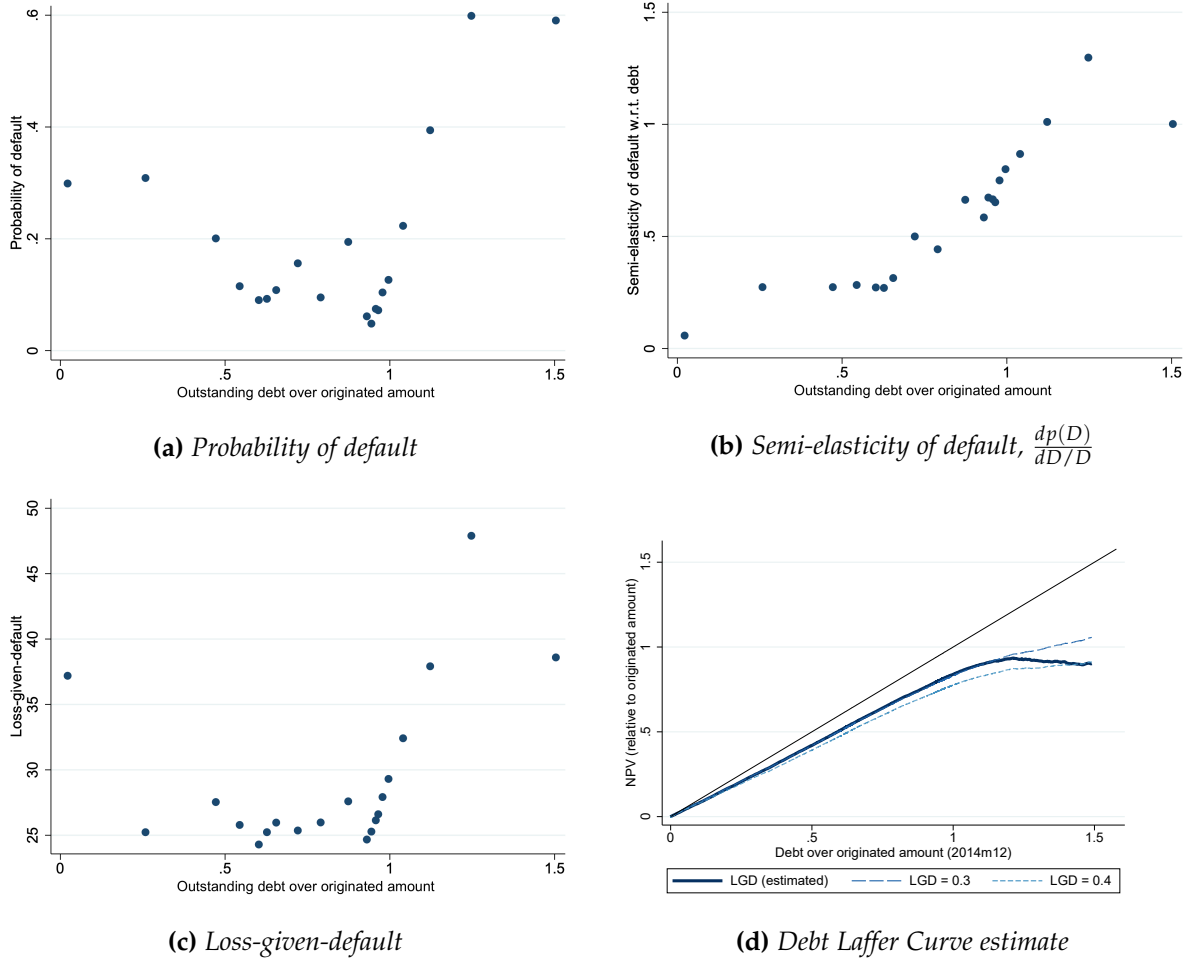
²⁷Note that the distribution of $\tilde{D}$ in the 2004-DID Sample is shifted to the left compared to the full sample considered in section 4.3, as these borrowers had more time to amortize their loans before the exchange rate depreciation in the crisis. Therefore, the semi-elasticity of default is not as hump-shaped here, as this sample contains fewer borrowers with an extremely high $\tilde{D}$.

debt borrowers. The final input to the slope of the Debt Laffer Curve is the elasticity of LGD with respect to debt. For this object, we use the same estimate as in Section 6.1. We then trace out the Debt Laffer Curve in $\tilde{D}$ -space by computing

$$\widehat{V(\tilde{D}_k)} = \sum_{j=1}^k \frac{\widehat{dV(\tilde{D})}}{d\tilde{D}} \Big|_{\tilde{D}=\tilde{D}_j} (\tilde{D}_j - \tilde{D}_{j-1}), \quad \tilde{D}_0 = 0, \quad k = 1, \dots, K. \quad (\text{A.20})$$

Figure A.9d plots the estimate of the Debt Laffer Curve using static valuation with parametric heterogeneity. The Debt Laffer Curve is concave and even inverts slightly for highly indebted borrowers. This inversion happens roughly once $\tilde{D}$ exceeds 1.2, i.e., when borrowers owe 20% more than they initially borrowed a decade earlier. The estimates imply that, at high levels of indebtedness, face value debt relief does not reduce the expected net present value of the loan to the lender. As discussed in the paper, this is driven by a combination of a higher default rate, a higher responsiveness of default to debt relief, and higher loss-given-default for highly indebted borrowers.

Figure A.9: Debt Laffer Curve: Static Valuation using Parametric Approach to Estimate Heterogeneity



Notes: This figure presents the inputs and the estimate of the Debt Laffer Curve based on a static valuation and a parametric estimation of the probability of default and the elasticity of default. See text in Appendix A.1 for details on the construction of each panel. In panel (d), the Debt Laffer Curve based on the estimated LGD relies on banks' loan-level estimates of the LGD.

B Bank Lending Responses to the Debt Relief Program

Section 6 showed that the majority of households are on the upward-sloping part of the Debt Laffer Curve. The debt relief therefore decreases the net present value of their loans, imposing losses on banks holding these loans. In this appendix, we briefly discuss how the debt relief program affected banks' capitalization and lending.

We start by examining average bank capitalization over time. Figure B.1 shows that bank capitalization started increasing in late 2013, when discussions at the government level started about a debt relief program to foreign currency borrowers. The decision of the Supreme Court (Curia) in June 2014 implied that banks had to compensate their borrowers for the exchange rate spread and other unilateral changes in the contract terms. Although the exact amount was determined only later, the decision immediately led banks to provision for losses and bank capital declined by about 1.5 percentage points in the second half 2014.

We measure banks' exposure to the debt relief policy by the change in housing assets relative to total assets before the announcement of the policy:

$$BankExposure_b = \frac{HousingAssets_{b,2015m2} - HousingAssets_{b,2015m3}}{TotalAssets_{b,2014m5}},$$

where $HousingAssets_{b,t}$ is the value of housing assets of bank b at time t and $TotalAssets_{b,t}$ is the value of total assets. To measure the effect of the debt relief policy on bank outcomes, we estimate the following specification at the bank-time level:

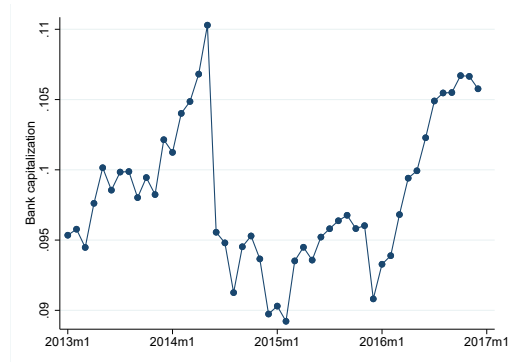
$$Y_{bt} = \alpha_b + \delta_t + \sum_{k \neq 2014m6} (\beta_k BankExposure_b \times \mathbf{1}_{t=k}) + \Gamma X_{bt} + \varepsilon_{bt}, \quad (B.1)$$

where Y_{bt} is a bank level outcome, such as the capital ratio, total assets, and total assets without housing assets. Controls include log total assets and capital ratio from before the June 2014 Supreme Court (Curia) decision, interacted with time fixed effects.

Figure B.2 summarizes the results for various outcomes. Figure B.2a shows that the capitalization of more exposed and less exposed banks evolved similarly before the announcement of the policy. The policy did not significantly affect capitalization, suggesting that more affected banks could adjust their equity ratios reasonably quickly.

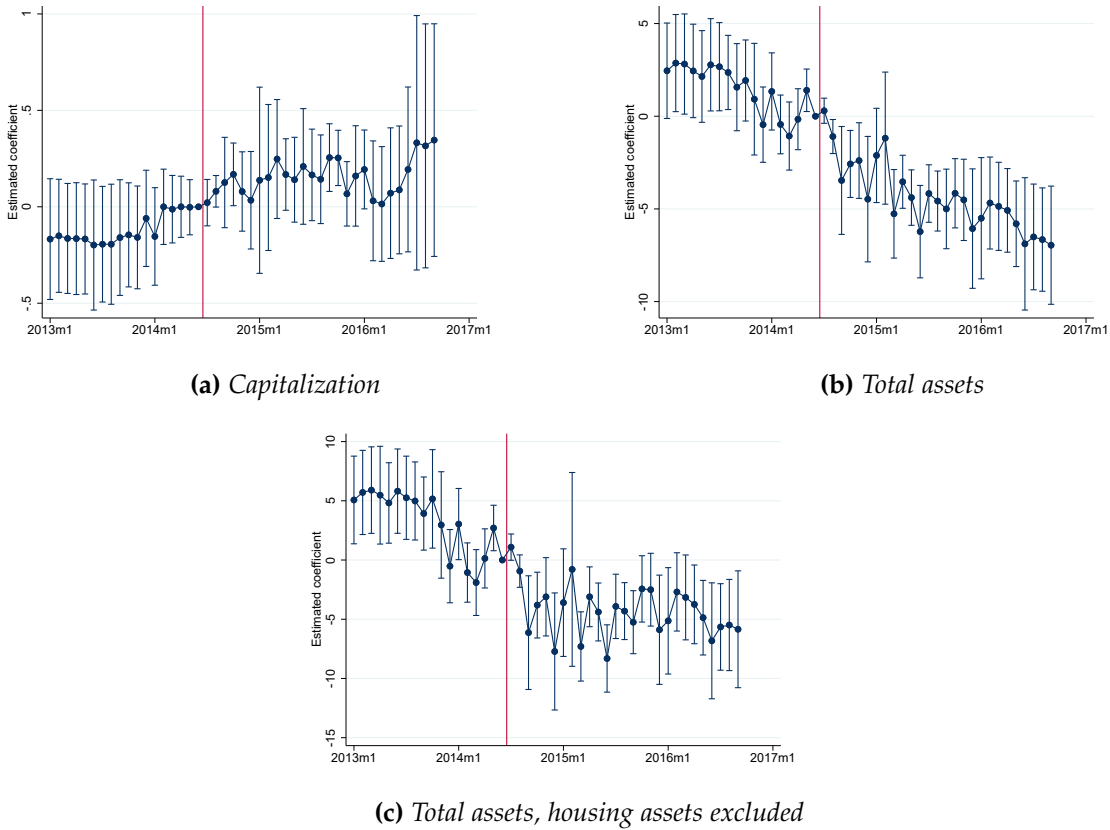
Although bank capitalization was not affected much by the policy, the banking sector reduced its assets significantly. Figure B.2b shows that more exposed banks significantly reduced their total assets after the announcement of the policy. As this result can partly be driven by housing assets, which were directly affected by the policy, we also examine the evolution of total assets excluding housing assets. Figure B.2c shows a similar negative effect on total assets without housing assets. These results suggest that the policy had a negative impact on banks, consistent with the fact that the majority of foreign currency borrowers are on the upward-sloping part of the Debt Laffer Curve.

Figure B.1: Average Bank Capitalization over Time



Notes: This figure plots the total assets weighted average capitalization around the debt relief policy.

Figure B.2: Effect of Debt Relief Exposure on Bank Capitalization and Lending



Notes: This figure estimates of equation (B.1). Error bars represent 95% confidence intervals based on standard errors clustered at the bank level.

C Data Appendix

C.1 Matching Credit Registry with Pension Contribution Data

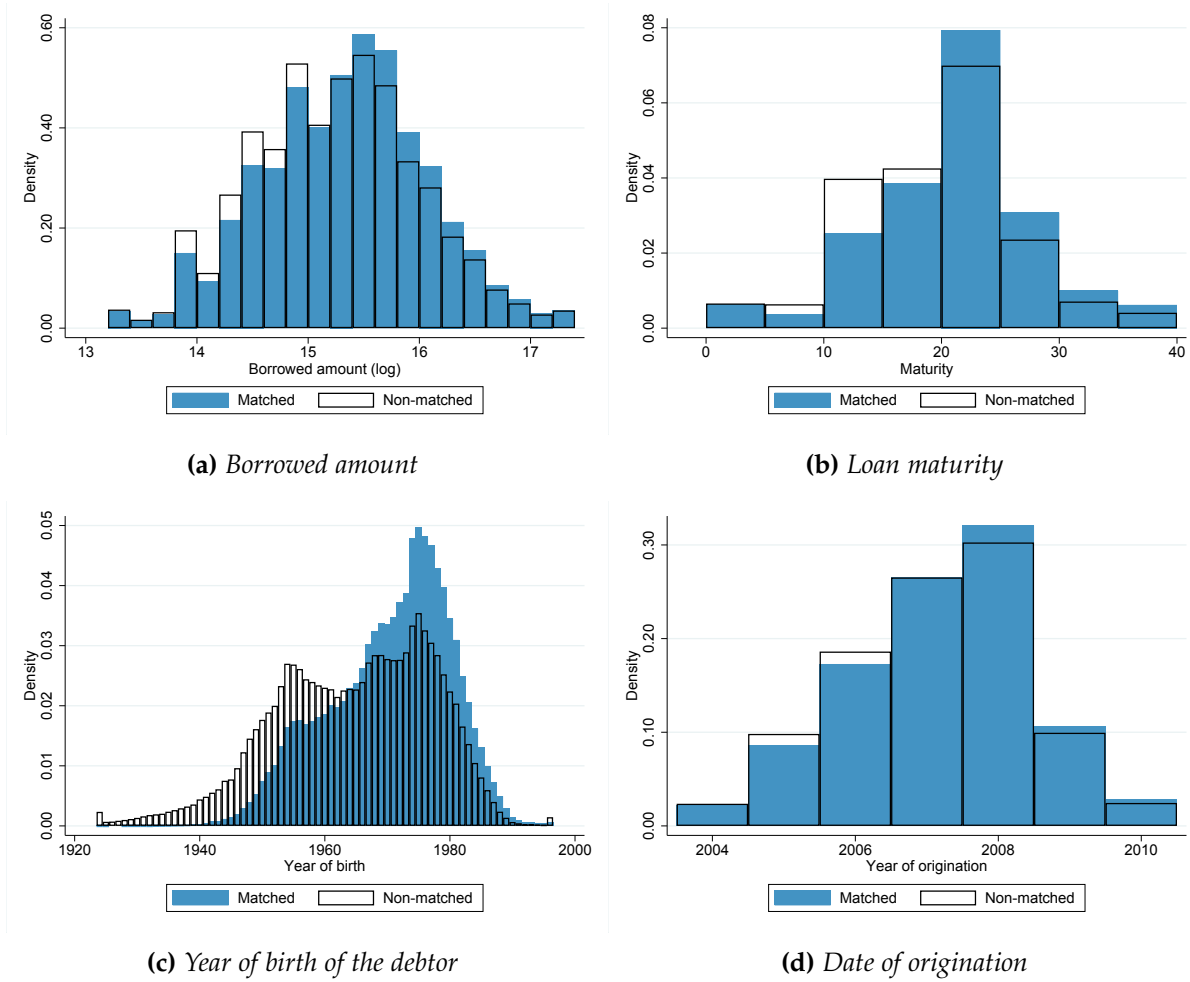
The pension contribution data (ONYF) and household credit registry (KHR) were matched by the Central Bank of Hungary in 2019 using several pieces of personal information, including name, date of birth, and mother's maiden name. BISZ, a subsidiary of the Central Bank of Hungary that maintains the household credit registry, has the legal obligation to delete all information on loans one year after the termination of loan contracts for loans that did not default and five years after the termination of loans in default. Given that the match was conducted in 2019, this implies that the match between borrowers exposed to the Settlement Act in 2015 with their income records in ONYF can potentially be incomplete. In particular, information on terminated loans may not be available in BISZ.

To address this issue, we improve the matching quality by linking individuals across *all* their loan products using the anonymized identifier in the credit registry at the Central Bank of Hungary. In particular, we exploit that even if a mortgage loan affected by the Settlement Act is no longer present in the credit registry in 2019 and cannot be matched with ONYF, we can still match that loan to the borrower's income data through other loans the borrower might have, including loans not affected by the Settlement Act.

The quality of the match is reasonably high. Moreover, matched borrowers are broadly similar to unmatched borrowers. As we observe all loans in the credit registry, we can compare the average characteristics of loans that were matched to ONYF to those that were not matched. This helps assess any potential sample selection arising from the matching. We use the whole period of ONYF for this exercise, that is the period between 1998-2017. This means that if an individual has an income or transfer that results in a pension contribution payment during this period, we consider that as a match if it is identified with a loan in the credit register. However, some of these individuals may not be in the ONYF sample around the implementation of the Settlement Act. This would be the case if, for example, they switch to self-employment.

Figure C.3 plots distributions of loan and individual characteristics of matched and unmatched loans. This analysis reveals that matched and unmatched loans are similar in terms of borrowed amount, loan maturity, and date of origination. The main difference is in the distribution of the year of birth. Debtors in the matched sample are younger compared to the non-matched sample.

Figure C.3: *Credit Registry and Income Data Match Quality: Characteristics of Matched and Un-matched Loans*



Notes: These figures plot the distribution of loan and borrower characteristics for loans that were successfully matched to the pension contribution data and loans that were not matched.

D Model: Additional Details

D.1 Calibration

Table D.1 summarizes the calibration of the model.

Table D.1: *Calibration*

Parameter	Symbol	Value	Target/Source
Default cost	κ	0.21	Response in Table 2 & pre-policy default rate of 18%
Frisch elasticity	η	0.16	Response in Figure 7
Wage garnishment	ξ	0.33	Hungarian Association of Judicial Officers
Scale of labor disutility	ϕ	1.00	Normalization
Loan interest rate	r_d	0.055	Median interest rate on FC loan
Maturity	T	20	Modal maturity on FC loans in credit registry
Discount factor	β	0.92	
EIS	σ	1.00	Kaplan et al. (2018)

Notes: This table summarizes the model calibration.

To allow for a small probability of a large depreciation, we assume that the exchange rate follows a Markov process with states $\{1.0, 1.3, 1.6\}$ and Markov matrix

$$\Pi_E = \begin{bmatrix} 0.92 & 0.06 & 0.02 \\ 0.0 & 0.9 & 0.1 \\ 0.0 & 0.0 & 1.0 \end{bmatrix}.$$