Do Cash Transfers Improve Credit Performance? Evidence from Low-Income Borrowers *

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Abstract

This paper examines the impact of government cash-transfer programs on low-income borrowers. To causally identify the effect of government assistance, we examine the duration of debt repayment compliance and the time spent in default, exploiting a discontinuity in eligibility for Uruguayan assistance programs. For proper causal identification, we extend the fuzzy regression discontinuity design to a duration analysis framework and propose a novel estimand that addresses the issue of censoring. Our findings indicate that recipients of government assistance spend less time in the up-to-date state, while the program has a negligible impact on the time spent in default.

Keywords: treatment effects, duration models, fuzzy regression discontinuity, credits, poverty

JEL Codes: C31, C36, I38.

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1 Introduction

Consumer credit can significantly enhance individuals' well-being by providing access to funds that help smooth consumption, facilitate the purchase of durable goods, and cushion against unexpected adverse shocks. Over the past decade, the global consumer credit market has experienced notable growth. This expansion has included increased lending to low-income individuals. However, these borrowers often face higher interest rates, which can lead to a cycle of debt. Payday loans, for example, are commonly used by low-income borrowers but come with high fees and interest rates, making repayment challenging and potentially trapping borrowers in long-term debt

In this work, we study the causal effect of a government cash-transfer program on the behavior of poor individuals in the consumer credit market. Utilizing data of the Uruguayan Central Bank combined with the Uruguayan public ministry in charge of social development we track the credit behavior of vulnerable individuals who can receive social assistance and examine the causal impact of this assistance on the credit performance in the consumer loan market. Specifically, we focus on the duration in two post-borrowing states: Up to Date (UTD) and Default (DEF). Taking advantage of the fact that transfers are assigned based on a continuous index measuring household vulnerability, with a predetermined cutoff, we apply a Regression Discontinuity Design (RDD). We propose a novel parametric approach that integrates duration analysis into the RDD framework while addressing the issue of censoring, which naturally arises in this context.

We derive an estimand that identifies the parameter of interest under standard RDD assumptions aided by independence of the censoring variable at the cutoff; although the identification strategy is fully non-parametric, the parametric implementation requires the continuity of the censoring variable around the cutoff. Furthermore, we show that the full set of required assumptions is empirically testable in the same manner as Arai et al. (2022).

Our contribution is twofold. Firstly, we contribute to the literature on credit default risk (Durkin and Elliehausen, 2014; Elliehausen, 2009; Gardner, 2022; Kaplan et al., 2014; Becher et al., 2018; Camara, 2022; Dobridge, 2016) and particularly close to Bertoletti et al. (2024)

by providing information on the role government assistance can have on credits default for low resources populations. Secondly, in the econometric front, our work is related to the Canonical regression discontinuity (Angrist and Pischke, 2009), the fuzzy regression discontinuity (Arai et al., 2022) and the literature on extensions such as discrete running variables and multi-dimensional designs (Cattaneo et al., 2024) by providing an extension of the design to duration outcomes building on insights from Frandsen (2015) and Acerenza et al. (2023). In a broader sense, our econometric contribution in the paper is related to duration models such as Bover et al. (2002) and Card and Hyslop (2005). Additionally, although our identification results are fully non-parametric our estimation procedure for simplicity is parametric, and in this sense our work is related to previous papers studying the risk factors and borrower behavior using logistic regressions (Agarwal et al., 2011; Campbell and Dietrich, 1983; Cunningham and Hendershott, 1984; Elul et al., 2010).

The rest of the paper is organized as follows. Section 2 presents the data and the selected sample. Section 3 presents the econometric framework. Section 4 reports the results. Section 5 provides a discussion and conclusion. Additional results are collected in the appendices A, B and C.

2 Data and Institutional Background

This study utilizes data from two sources: the Uruguayan Central Bank (BCU) office of Credit Bureau and the Ministry of Social Development (MIDES), the central government agency responsible for public social assistance in Uruguay. Merging these datasets allows us to track the credit behavior of individuals who receive social assistance. These datasets are merged at the individual level using a unique personal identifier.

The Uruguayan Central Bank Credit Bureau provides a comprehensive dataset on loans granted to individuals within the country's financial system. This publicly available information, reported monthly by regulated financial institutions as a mandatory requirement, allows for a granular analysis at the loan product level. The dataset is increasingly used to study borrower behavior; however, previous studies have primarily focused on corporate

rather than personal loans (Dassatti et al., 2025). Our dataset spans from January 2014 to December 2023. Although data are available prior to January 2014 for banks, information on other lending firms, which are key providers of consumer loans, is not included before that date. We examine the causal impact of social assistance on the credit performance of low-income individuals in the consumer loan market. Accordingly, we restrict our analysis to consumer loans.

The dataset includes monthly information on each debtor's credit rating category across all financial institutions with which they hold loans. Using this data, we construct two variables to measure the duration in two key categories: UTD and in DEF. These two key duration variables are at the core of this work. First, we focus on the duration of the first spell in UTD state because it is crucial to determine how long an individual continues repaying their debt without delinquency. Accurately measuring this duration is essential for assessing the lender's financial losses due to default. In particular, analyzing the probability of no repayment at all (i.e., when UTD duration equals 1) is of great interest because it may signal the presence of moral hazard, adverse selection, or severe liquidity constraints that prevent borrowers from making even a single payment. Second, we analyze the duration of DEF, which is more closely related to the costs borne by individuals who default. These costs affect access to credit markets, but in Uruguay, defaulters also face restrictions on entering other contracts, such as accessing certain public services or obtaining guarantees for renting a house.

The MIDES dataset offers detailed information on individuals who have applied for social assistance in Uruguay, covering a significant period from 2011 to 2023. This extensive data collection effort involved visits to 292,580 households (942,423 persons). This dataset has been extensively used to study the impact of government assistance programs on various aspects, such as labor market responses and marital dissolution (Bergolo and Galván, 2018; Bergolo and Cruces, 2021). Significant and relevant variables within this dataset include the Poverty Score (PS) index; indicators of household receipt of public assistance; and data on education, age, gender, race, employment status, and region of residence. Developed in 2008 through a collaboration between MIDES and the University of the Republic, the PS index serves as

a tool to identify households with greater socioeconomic vulnerability and to target non-contributory transfer programs. This index evaluates structural factors related to household well-being, encompassing multiple dimensions to approximate a household's socioeconomic status.

Uruguay implements two primary cash-transfer programs through MIDES. Firstly, the Tarjeta Uruguay Social (TUS) program which aims to provide food support to individuals in extreme poverty. It targets households experiencing significant socioeconomic vulnerability, as determined by the PS index, which must exceed a specified threshold. The second program, Asignaciones Familiares - Plan de Equidad (AFAM), is designed for low-income families with children under 18 and pregnant women. Eligibility for AFAM also requires the household's PS to surpass a certain threshold, which is lower than that of TUS. The cash transfer amount varies non-linearly based on the number of children in each household, up to a maximum of four.

Our study focuses on assessing the causal impact of TUS program. To achieve this, we employ a RDD approach, concentrating on the behavior of our variables of duration UTD or DEF state—within a neighborhood around the eligibility cutoff for the TUS program. Due to imperfect compliance with the eligibility criteria, we adopt a FRDD but adjusted for the existence of potential endogenous censoring which is part of the nature of our outcome variables of interest.

To compute the duration variables, we first identify the initiation of a relationship between a debtor and an institution. We define a "spell" as a continuous period during which a debtor maintains a relationship with a specific institution and analyze the debtor's behavior throughout this period. One of the key variables we derive is the duration in the UTD state, which measures the number of months an individual remains in the initial credit rating category, indicating the time without any payment delays. Subsequently, we calculate the duration in DEF for individuals with delinquencies of 180 days or more. In both cases, we consider only the first sub-spell of each state.

We exclude spells that began before April 2014 because our dataset starts in January

2014. Consequently, for observations in January, we cannot determine whether the debtor-institution relationship had already begun. To adopt a more conservative approach, we also exclude spells that start in February or March of that year. The sample period ends in December 2023, resulting in some observations being right-censored. Addressing this issue within the context of estimating the FRDD is a contribution of this study. A fuzzy design is justified in this context since as noted in Table A.2, there exists elegible and non-elegible individuals in both arms of treatment.

Our working sample is summarized in tables 1 and 2. Table 1 presents summary demographic statistics for individuals in our sample and differentiating between the treated individuals (the ones with TUS) and the untreated ones. We can observe a clear heterogeneity between these two populations, with the treated individuals being more female composed, younger, less educated, with bigger household sizes and in worse employment conditions. Untreated individuals have more debts and are less likely to default on their debts. Table 2 shows the dynamic of the composition of debt and other financial choices of individuals in the sample. We can observe the percentage of individuals with bank debts and credit cards has increased over time. Concomitantly default has also increased over time.

Table 1: Demographic overview: summary statistics

	Full Sample	Untreated	Treated
Individuals	463,460	312,464	150,996
Female (%)	58.0	57.2	59.6
Youth (%)	32.1	29.1	38.4
Young Adult (%)	29.8	26.7	36.3
Middle-aged (%)	25.8	28.6	19.9
Senior (%)	12.2	15.6	5.4
Incomplete elementary school (%)	14.6	13.6	16.6
Completed elementary school (%)	50.7	46.0	60.6
Incomplete secondary school (%)	19.3	22.4	13.0
Completed secondary school (%)	15.4	18.0	9.9
Salaried employee (%)	24.4	28.1	16.7
Self-employed (%)	21.9	20.0	25.6
Unemployed (%)	13.9	11.6	18.6
Retired (%)	10.5	12.9	5.4
Not in the labor force (%)	26.8	25.2	30.1
Capital city (%)	34.9	32.7	39.7
Household size (avg.)	3.9	3.5	4.8
Debtors per household (avg.)	1.1	1.1	1.1
Debts per household (avg.)	3.4	3.6	3.1
Defaulted debts per household (avg.)	1.7	1.6	1.9

Source: Authors' elaboration based on data from BCU and MIDES.

Table 2: Consumer indebtedness summary statistics

	April 2014	April 2017	December 2020	December 2023
Individuals	231,916	276,747	350,564	380,639
Has bank debt (%)	42.3	40.6	36.1	46.9
Has lending firm debt (%)	89.4	89.0	90.6	86.9
Has credit card (%)	45.1	43.8	36.0	53.1
In Default (%)	44.1	50.0	60.1	61.4

Source: Authors' elaboration based on data from BCU and MIDES. Data includes only individuals in the MIDES dataset.

3 Analytical Framework

Our framework extends Arai et al. (2022) to account for censoring, as follows:

$$Y^* = Y^*(1, X)D + Y^*(0, X)(1 - D) \tag{1}$$

$$Y = \min\{Y^*, C\} \tag{2}$$

$$D = D(X) \tag{3}$$

Let X be an observed continuous random variable with support $\mathcal{X} \subset \mathbb{R}$. We call X the running variable. In our context, this will be the poverty score PS. Let $D(.,.) : \mathcal{X} \times \Omega \to \{0, 1\}$ and $D(x, \omega)$ be the potential treatment that individual ω would have received, had her running variable been set to x. For $d \in \{0, 1\}$, we define mappings $Y^*(d, ., .) : \mathcal{X} \times \Omega \to \mathcal{Y} \subset \mathbb{R}$ and let $Y^*(d, x, \omega)$ denote the potential outcome of individual ω had her treatment and running variable been set to d and x, respectively.

For simplicity, we ommit ω. Then we denote $Y^*(d, x)$ is the latent duration outcome under treatment status d and x, D is the treatment indicator, which in our case takes the value 1 if the individual recieves government assistance. Similarly Y is the observed outcome, which in our case is the duration in two particular types of repayment categories: UTD and DEF.

Note that by definition, $D(X)$ is the observed treatment (which we abbreviate it as D) which is a function of the running variable X as well as of other unobserved elements omitted in the notation. C is a continuous censoring variable which in our case is related to the end period of the observed sample. We assume throughout that the conditional distribution of (Y, D, C) given $X = x$ is well-defined for all x in some neighborhood of x_0, and that $\lim_{x \downarrow x_0} D(x)$ and $lim_{x \uparrow x_0} D(x)$ are well defined. Note that by letting the potential outcomes be indexed by x, we allow the running variable to have a direct causal effect on outcomes. The cutoff point, x_0, is zero in our case because we recenter the running variable using the threshold that determines eligibility for the program.

The econometritian observes Y, D, X, C and knows x_0. Since we are in a fuzzy design, we introduce $T_\epsilon(\omega) \equiv T_\epsilon$ which is a variable that indicates the compliance type on a neighbourhood of the cutoff ϵ. The types are always takers A, never takers N, compliers Co and defiers Df. Thus:

$$T_\epsilon = \begin{cases} A \quad \text{if} \quad D(x) = 1 \quad \text{for} \quad x \in (x_0 - \epsilon, x_0 + \epsilon) \\ N \quad \text{if} \quad D(x) = 0 \quad \text{for} \quad x \in (x_0 - \epsilon, x_0 + \epsilon) \\ Co \quad \text{if} \quad D(x) = 1\{x \geq x_0\} \quad \text{for} \quad x \in (x_0 - \epsilon, x_0 + \epsilon) \\ Df \quad \text{if} \quad D(x) = 1\{x < x_0\} \quad \text{for} \quad x \in (x_0 - \epsilon, x_0 + \epsilon) \end{cases}$$

Let us define $I_y = 1(Y \leq y)$ which measure duration for alternative values of y. In the fuzzy context we care about the treatment effect for compliers, more concretely:

$$LATE_{I_y, x_0, Co} = \lim_{x \to x_0} [P(Y^*(1) < y | X = x_0, T_{|x - x_0|} = Co) - P(Y^*(0) < y | X = x_0, T_{|x - x_0|} = Co)]$$

In words, this is the change in the probability of having a duration of y or less in a particular category that government assistance has at the threshold of the PS for compliers of the treatment assignment status.

To aid identification, we impose the following identifying assumptions.

Assumption 1 (Local monotonicity)

$$\lim_{\epsilon \to 0} P(T_\epsilon = Df | X = x_0 + \epsilon, C > y) = \lim_{\epsilon \to 0} P(T_\epsilon = Df | X = x_0 - \epsilon, C > y) = 0$$

Assumption 1 says that as the neighborhood of x_0 shrinks, the conditional proportion of defiers converges to zero, implying that only "always takers," "compliers," and "never takers" may exist at the limit. In our context, this implies that at the cutoff there are no individuals that if they are assigned government assitance would reject it and if the are not assigned they would get it.

Assumption 2 (Local Continuity) *For $t = \{A, N, Co\}$ and any y,*

$$\lim_{\epsilon \to 0} P(Y^*(d, x_0 + \epsilon) \leq y, T_\epsilon = t | X = x_0 + \epsilon, C > y) = \lim_{\epsilon \to 0} P(Y^*(d, x_0 - \epsilon) \leq y, T_\epsilon = t | X = x_0 - \epsilon, C > y),$$

$$\lim_{\epsilon \to 0} P(T_\epsilon = t | X = x_0 + \epsilon, C > y) = \lim_{\epsilon \to 0} P(T_\epsilon = t | X = x_0 - \epsilon, C > y).$$

Assumption 2 states that the conditional joint distributions of potential outcomes and compliance types are continuous at the cutoff. This assumption implies that, the behavior of the duration and the types are sufficiently smooth. In other words, there is no systematic manipulation or anticipation that would lead individuals to alter their durations or their types in response to changes in the running variable, except through the effect of the treatment variable.

Assumption 3 (Discontinuity)

$$\lim_{x \downarrow x_0} P(D = 1 | X = x, C > y) > \lim_{x \uparrow x_0} P(D = 1 | X = x, C > y)$$

We specify in Assumption 3 the main feature of fuzzy RD designs. Namely, that the probability of receiving treatment is discontinuous at the cut-off. To be consistent with the local monotonicity assumption, we specify the discontinuity so that the propensity score jumps up as x goes above the cutoff. This assumption holds by construction in our case since government

assistance programs use the PS as a running variable to determine eligibility.

Assumption 4 (Positive mass at the cutoff) *For* $t = \{A, N, Co\}$ *and any* y, $\lim_{\epsilon \to 0} P(C > y, T_\epsilon = t | X = x_0 \pm \epsilon) > 0$ *for any* y.

Assumption 4 is a regularity condition that assures that at the cutoff there will be individuals for all the relevant latent types. it is standard in treatment effects literature.

With the previous assumptions we can identify an even more local version of the local effect for compliers, in fact we can identify the effect for individuals with different levels of the censoring variable C. If we want to extend this, we need to find a way to pindown the dependence between the outcome and the censoring. For that we can impose:

Assumption 5 (Independence at the cutoff) *For sufficiently small* ϵ, *let* $Y^*(d) \perp\!\!\!\perp C | X = x_0 \pm \epsilon, T_\epsilon = Co$ *for* $d = 0, 1$

Here we are imposing that when we are really close to the cutoff, the censoring variable does not affect the outcome variable once we control for the fact that individuals are compliers. In our context, this implies that duration is independent of censoring once we control for the PS at the cut-off and the compliance type.

The previous assumptions are sufficient for identification of $LATE_{I_y, x_0, Co}$. Denoting $I_{y,d} = 1[(Y \leq y)(D = d)]$ and $\Omega_{XCy} = \{X = x \cap C < y\}$ we demonstrate that

Lemma 1 *under Assumptions 1-5,*

$$\begin{aligned} LATE_{I_y, x_0, Co} &= \frac{\lim_{x \downarrow x_0} P(I_{y,1} | \Omega_{XCy}) - \lim_{x \uparrow x_0} P(I_{y,1} | \Omega_{XCy})}{\lim_{x \downarrow x_0} P(D = 1 | \Omega_{XCy}) - \lim_{x \uparrow x_0} P(D = 1 | \Omega_{XCy})} \\ &+ \frac{\lim_{x \downarrow x_0} P(I_{y,0} | \Omega_{XCy}) - \lim_{x \uparrow x_0} P(I_{y,0} | \Omega_{XCy})}{\lim_{x \downarrow x_0} P(D = 1 | \Omega_{XCy}) - \lim_{x \uparrow x_0} P(D = 1 | \Omega_{XCy}} \\ &= \frac{\lim_{x \downarrow x_0} P(I_y | \Omega_{XCy}) - \lim_{x \uparrow x_0} P(I_y | \Omega_{XCy})}{\lim_{x \downarrow x_0} P(D = 1 | \Omega_{XCy}) - \lim_{x \uparrow x_0} P(D = 1 | \Omega_{XCy})} \end{aligned}$$

We show in Appendix A.1 the details of this results. Furthermore, the previous model has testable implications in a similar manner as Arai et al. (2022). The testable implications are collected in the Appendix A.3.

3.1 Implementation

Although identification and the implementation can be non-parametric , to estimate the previous parameter of interest, we impose a simple parametric structure and we aid ourselves with an extra continuity assumption. We also add some notation, by defining $I_{WC,y} = 1(W \leq y)1(C > y)$ with $W = Y, D$; $I_x = 1(X \geq x_0))$ and $\pi_y = 1(C > y)$.

Assumption 6 (Sampling and Parametric Structure) *Let*

- $\{Y_i, \pi_{yi}, X_i, D_i\}_{i=1}^N$ *be an iid sample.*
- $I_{YC,y} = \Phi(\gamma_{0,y} + \gamma_{x,y}X + \gamma_{x_0,y}I_x) + e_y$, $I_{DC,y} = \Phi(\beta_{0,y} + \beta_{x,y}X + \beta_{x_0,y}I_x + u$ *where* $\Phi()$ *is a known cummulative probabiltiy function such as the normal* CDF *and* e_y, u *are mean independent of functions of* X*.*
- $P(\pi_y|X = x)$ *is continuous at the cutoff.*

Under the previous assumption, we can construct an estimate for $LATE_{I_y,x_0,Co}$ since adding the previous assumption, we have:

$$\begin{aligned} LATE_{I_y,x_0,Co} &= \frac{\lim_{x\downarrow x_0} P(I_y|\Omega_{XCy}) - \lim_{x\uparrow x_0} P(I_y|\Omega_{XCy})}{\lim_{x\downarrow x_0} P(D=1|\Omega_{XCy}) - \lim_{x\uparrow x_0} P(D=1|\Omega_{XCy})} \\ &= \frac{\lim_{x\downarrow x_0} P(I_{YC,y}|X=x) - \lim_{x\uparrow x_0} P(I_{YC,y}|X=x)}{\lim_{x\downarrow x_0} P(I_{DC,y}|X=x) - \lim_{x\uparrow x_0} P(I_{DC,y}|X=x)} \\ &= \frac{\Phi(\gamma_{0,y} + \gamma_{x,y}x_o + \gamma_{x_0,y}) - \Phi(\gamma_{0,y} + \gamma_{x,y}x_0)}{\Phi(\beta_{0,y} + \beta_{x,y}x_0 + \beta_{x_0,y}) - \Phi(\beta_{0,y} + \beta_{x,y}x_0)} \end{aligned}$$

Where the first equality is due to properties of probability and continuity at the cutoff of $P(\pi_y|X = x)$ and the last equality is due to the parametrization.

Note that, the parameters of interest can be estimated by the sample analogs of the following set of moment equations:

$$E\left[\begin{pmatrix} I_{YC,y} - \Phi(Z'\gamma_y) \\ I_{DC,y} - \Phi(Z'\beta_y) \end{pmatrix} \bigotimes Z\right] = 0$$

where $Z = (1, X, I_x)'$ and $\rho = (\rho_{0,y}, \rho_{x,y}, \rho_{x_0,y})$ for $\rho = \gamma, \beta$.

Based on the previous moment equations we implement a multiplier bootstrap (Wellner et al., 2013) based on the influence function derived in the Appendix B.

Furthermore, we conduct a Monte Carlo simulation exercise, which shows that our estimate performs well in terms of mean squared error and coverage in comparison to other alternatives in the FRDD context that do not account in our level of detail for censoring (see Appendix A.2).

4 Results

In this section we present our results. We start by presenting simple comparisons of the outcomes of interest (which are only indicative due to endogeneity) as well as some suggestive graphical evidence. Subsequently we focus on the results of our main identification strategy.

4.1 Descriptive analysis

Table 3 presents descriptive statistics on duration in the UTD and DEF states, separately for untreated and treated individuals. We also test for significant differences between untreated and treated individuals across all reported statistics. The results show that the average and median UTD duration are lower for treated individuals, whereas the opposite holds for DEF duration. Moreover, DEF duration is substantially longer than UTD duration for both groups.

Additionally, we report the probability that duration falls below different threshold values. The proportion of UTD individuals below all these thresholds is higher among treated individuals, whereas default duration tends to be longer for treated individuals. These findings indicate that treated individuals experience shorter UTD spells compared to the untreated

but tend to have longer spells in the DEF state.

The probability of UTD duration being less than or equal to one is particularly relevant, as it indicates that the individual never makes a repayment, leading to a complete loss for the lender. Our results show that the proportion of individuals who never repay is as high as 21.5% for the untreated group and 23.9% for the treated group. While it is not possible to fully disentangle whether these cases arise from strategic behavior, moral hazard, or severe liquidity constraints, we can speculate that both strategic behavior and moral hazard play a role in this setting. Additionally, severe liquidity constraints are commonly present among low-income individuals receiving cash transfers, which may also contribute to repayment failures.

Regarding censoring, as expected, it becomes more relevant at longer durations. We also observe differences in censoring between treated and untreated groups, with treated individuals being more likely to be censored in both outcome variables. This suggests that censoring may be endogenous and correlated with individual economic conditions, particularly the poverty score that determines eligibility. Recall that for identification, we require censoring to be independent—not generally, but specifically within the neighborhood of the cutoff. Therefore, the dependence of censoring on the poverty score does not contradict the assumption that this dependence is negligible within a narrow interval of the poverty score around the cutoff.

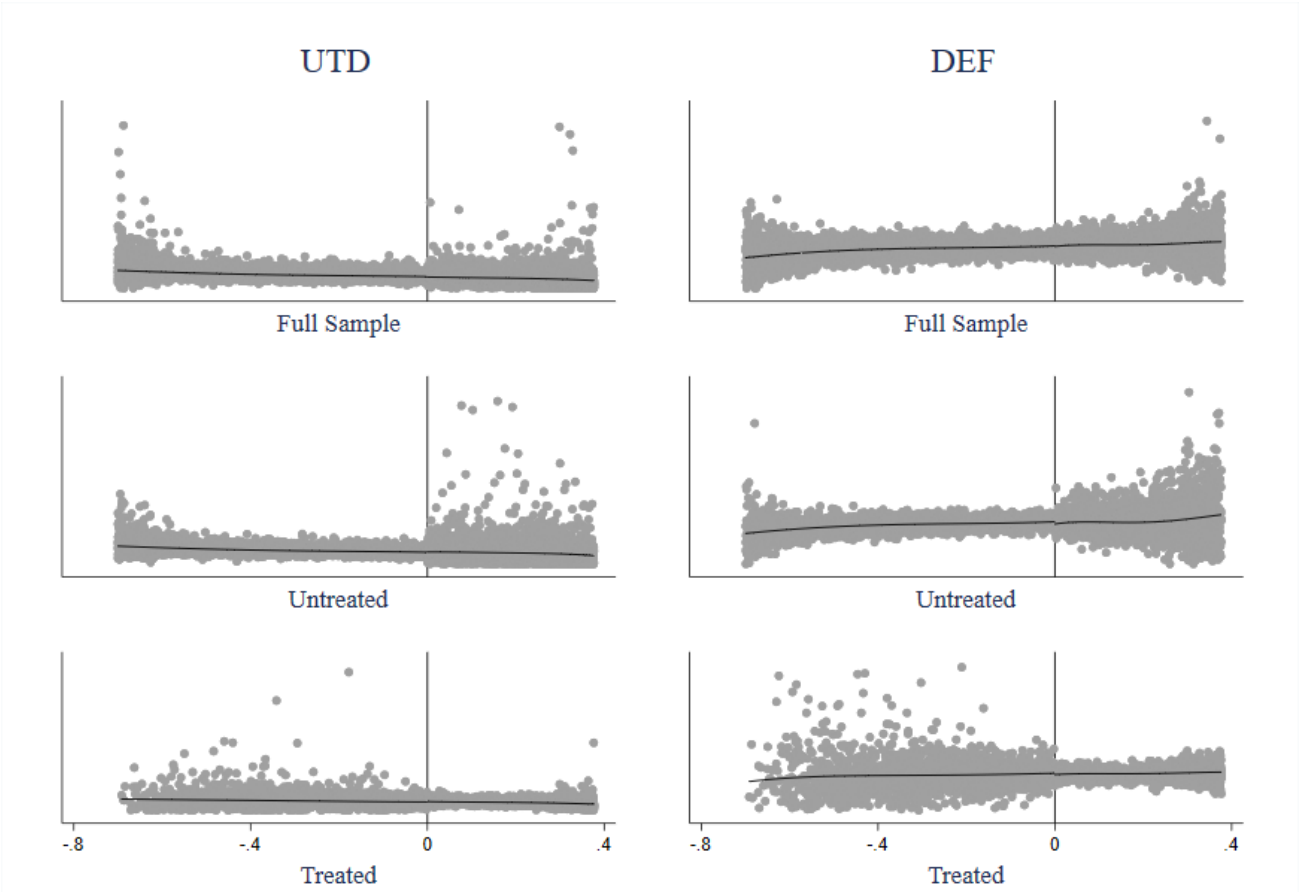
Table 3: Sub-spell duration and censoring by treatment status

	UTD			DEF		
	Untreated	Treated	Mean diff.	Untreated	Treated	Mean diff.
# Sub-spells	1,833,195	565,672		527,214	286,970	
Median duration	5.0	4.0		17.0	20.0	
Mean duration	10.0	7.7	-2.4***	28.0	30.8	2.8***
Changed rating (%)	66.4	73.7	7.4***	18.4	11.1	-7.3***
Went OOS (%)	21.2	15.0	-6.2***	42.9	43.1	0.2*
Censored (%)	12.4	11.2	-1.2***	38.7	45.8	7.1***
Duration $\leq$ 1 (%)	21.5	23.9	2.4***	5.4	3.7	-1.7***
Censored among Duration $\leq$ 1 (%)	1.0	1.1	0.1***	0.9	0.9	0.0*
Duration $\leq$ 12 (%)	76.3	82.7	6.4***	42.0	37.2	-4.8***
Censored among Duration $\leq$ 12 (%)	11.4	13.6	2.2***	13.2	13.6	0.4***
Duration $\leq$ 24 (%)	89.8	93.8	4.0***	59.8	55.6	-4.2***
Censored among Duration $\leq$ 24 (%)	24.3	29.1	4.8***	26.5	27.7	1.2***
Duration $\leq$ 48 (%)	96.7	98.3	1.7***	80.4	76.9	-3.5***
Censored among Duration $\leq$ 48 (%)	42.2	46.3	4.2***	43.4	43.6	0.3**

Source: Authors' elaboration based on data from BCU and MIDES.

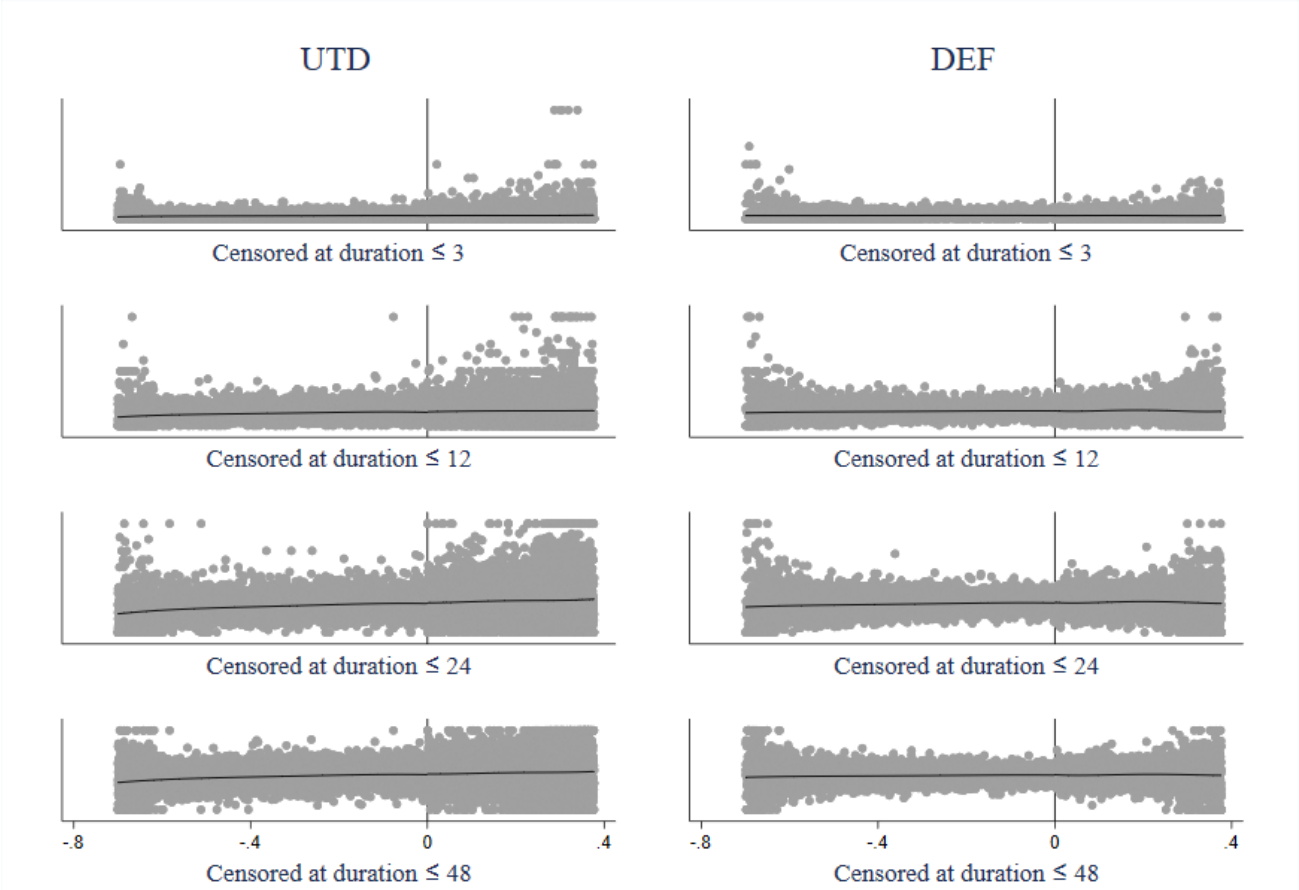
We can observe from Figures 1 and 2 that the the outcome variables for the full sample as well as for treated and untreated individuals do not seem to present a discontinuity at the cut-off. At the same time, we can see that the distribution of the censoring variable associated with the two outcome variables of interest also seems to be continuous at the selected values. These two set of figures are suggestive of fulfillment of the first part of Assumption 2 as well as for the last part of Assumption 6.

Figure 1: Duration variable continuity across the Poverty Score



Notes: The cutoff has been set to zero. The duration variable is defined as the length of the sub-spell, measured in months. The graphs were plotted using Stata's *rdplot*(Calonico et al., 2017).

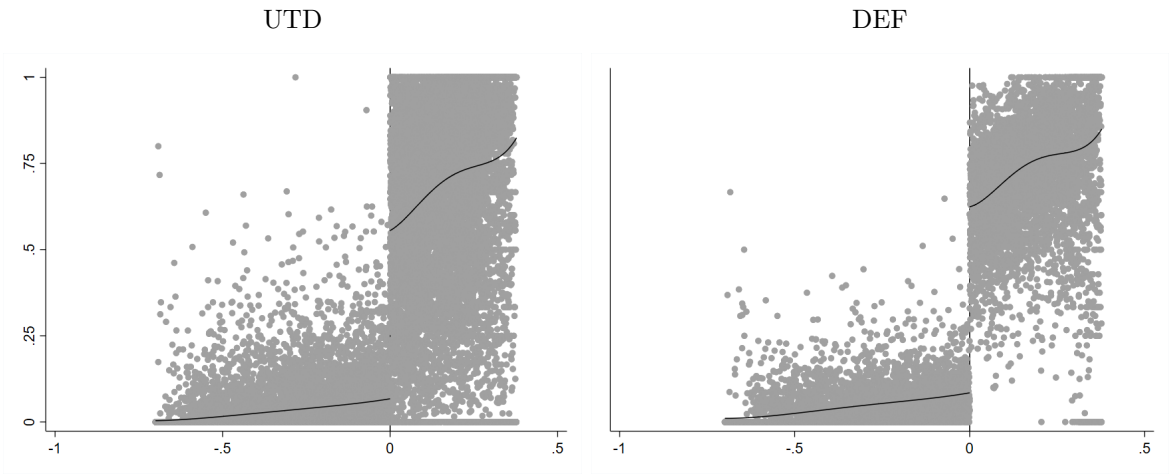
Figure 2: Censoring variable continuity across the Poverty Score



Notes: The cutoff has been set to zero by centering the poverty score. The censoring variable is defined as a dummy indicating whether the sub-spell is censored, given a specified duration window. The graphs were plotted using Stata's *rdplot*(Calonico et al., 2017).

Figures 3a and 3b suggest that Assumption 3 is fulfilled in our context. It is worth to note, that two different set of graphs are presented to check if there is a discontinuity, since the analysis for DEF is conditional on individuals entering in default while there are individuals in UTD that never entered in default.

Figure 3: Discontinuity in treatment assignment at the cutoff



Notes: The cutoff has been set to zero.

4.2 Preliminary Results

In this subsection, we present the results of our proposed implementation. To ensure robustness, we complement our findings with the estimators proposed by Arai et al. (2022), as well as the procedures compiled in Calonico et al. (2017). These estimators do not accommodate censoring; therefore, we compute them both for the full sample and for the subsample of observations that are not censored at each value of y.

Results are collected in Tables 4 and 5. The rows labeled ASLate correspond to the estimates obtained from our parametric procedure. We also estimate the LATE using the method proposed by Arai et al. (2022), where Arai refers to the nonparametric estimator applied to the full sample, and Arai_nc refers to the same estimator applied only to the subsample of non-censored observations. Similarly, CCT and CCT_nc refer to the estimates from Calonico et al. (2017) for the full sample and the subsample of non-censored observations, respectively.

We observe that the treatment has a negative impact on the probability of being UTD for all considered values of duration. This is because the likelihood that the duration in that state falls below each corresponding threshold is higher for the treated group (see Table 4 and Figure 4a). At the same time, the magnitude of this effect appears to increase initially and

Table 4: LATE estimates of the probability of remaining Up To Date below different duration thresholds

Threshold:	1	6	12	24
ASLate	0.0286***	0.0460***	0.0282***	0.0113***
	[0.002]	[0.003]	[0.003]	[0.004]
Arai (All)	0.0110**	0.0247***	0.0207***	0.0133***
	[0.005]	[0.009]	[0.007]	[0.005]
Arai (Non-censored)	0.0084	0.0217**	0.0222***	0.0168**
	[0.005]	[0.010]	[0.008]	[0.007]
CCT (All)	0.0108*	0.0223***	0.0230***	0.0143***
	[0.006]	[0.008]	[0.007]	[0.004]
CCT (Non-censored)	0.0077	0.0202***	0.0236***	0.0196***
	[0.006]	[0.007]	[0.007]	[0.006]
Obs. (All)	1,498,403	1,498,403	1,498,403	1,498,403
Obs. (Non-censored)	1,485,631	1,425,986	1,341,246	1,136,691

Standard errors in brackets: *** p<0.01, ** p<0.05, * p<0.1

then slightly decrease as y grows for the UTD duration variable.

Regarding the variety of estimators, while they differ in magnitude, they consistently point in the same direction concerning the UTD outcome. The estimated impact of cash transfers on the probability of having a duration of one month or less—an outcome of particular interest, as discussed above—is approximately 2.9 percentage points according to the ASLate estimator. However, this effect is not statistically significant at the five percent level for the remaining estimators. For medium-term durations (6 and 12 months), all estimates are significant and positive, with the ASLate estimator yielding the largest effect. For longer durations (24 months), all estimates remain significant, with magnitudes ranging between one and two percentage points.

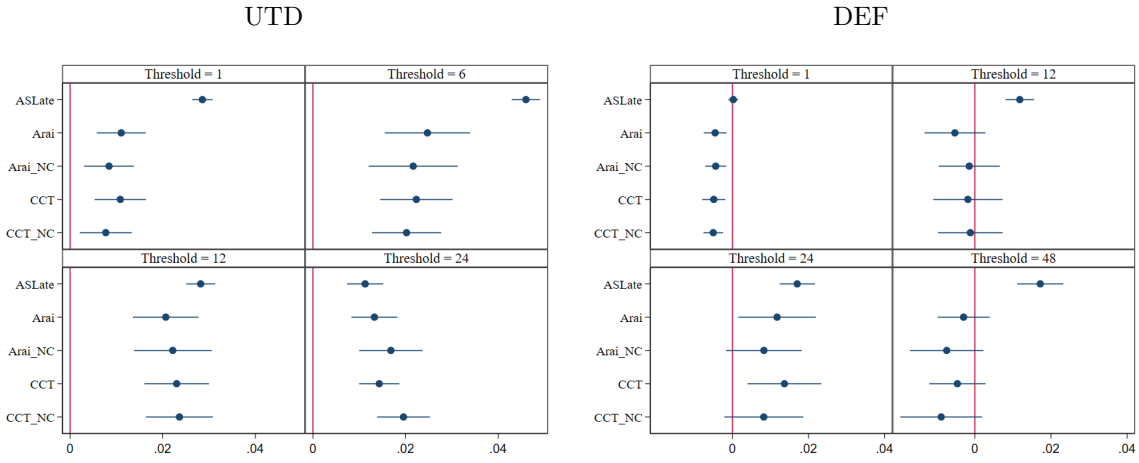
By contrast, cash transfers do not appear to have a significant effect on the duration in DEF according to any estimator except ASLate, which suggests a positive but limited impact on duration. Similar to the UTD outcome, the positive sign indicates a shorter duration in the default state.

Table 5: LATE estimates of the probability of remaining in Default below different duration thresholds

Threshold:	1	12	24	48
ASLate	0.0002	0.0118***	0.0170***	0.0172***
	[0.001]	[0.004]	[0.005]	[0.006]
Arai (All)	-0.0045	-0.0052	0.0117	-0.0029
	[0.003]	[0.008]	[0.010]	[0.007]
Arai (Non-censored)	-0.0044	-0.0015	0.0083	-0.0074
	[0.003]	[0.008]	[0.010]	[0.010]
CCT (All)	-0.0049	-0.0018	0.0137	-0.0046
	[0.003]	[0.009]	[0.010]	[0.007]
CCT (Non-censored)	-0.0050*	-0.0012	0.0082	-0.0088
	[0.003]	[0.008]	[0.010]	[0.011]
Obs. (All)	755,886	755,886	755,886	755,886
Obs. (Non-censored)	749,033	656,736	555,417	434,687

Standard errors in brackets: *** p<0.01, ** p<0.05, * p<0.1

Figure 4: Estimates for the causal effect of cash-transfers over the duration in Up to Date and Default



Notes: Authors' elaboration based on the estimates in Table 4 and Table 5.

5 Preliminary conclusion

In this paper we study the effect government assistance has on low-income individuals in the consumer credit market. We focus on duration in two states: up-to-date (UTD) and default (DEF).

To this end, we extend the FRDD design to duration outcomes. We rely on natural adaptations of existing FRDD assumptions such as local continuity of the latent outcome and types, aided by our main identifying assumption -censoring independence at the cut-off. Additionally, although our identification results are fully non-parametric we provide a simple to implement parametric procedure that, by standard results, is asymptotically valid. Inference can be easily performed via the multiplier bootstrap. Furthermore, we conduct a Monte Carlo simulation exercise that shows that our estimator performs properly in terms of MSE and Coverage.

Our preliminary findings indicate that the treatment has a positive impact on the probability of being UTD. Thus, the effect of the treatment is to shorten the duration in that state. Conversely, the estimated impact on the duration in the default (DEF) state is either not significant or very small in magnitude. Regarding the variety of estimators, although they differ in magnitude they point towards the same direction.

To sum up, our ongoing work derives an identification strategy to address censoring in the FRDD framework, and provides evidence of its performance. We present evidence indicating the suitability and relevance of our approach for the case under study. However, much additional work is needed: first, to assess the probability of entering the default state; second, to extend and enrich the Monte Carlo simulations to evaluate the estimator's performance under diverse circumstances; third implement a non-parametric estimator based on the sample analog of the estimand that identifies the local effect of interest and fourth, to implement the test about assumptions restrictiveness on the data and conduct simulations to assess its power in detecting assumption violations.

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Supporting Information

(Online Appendix)

A Additional results

A.1 Identification details: Proof of Lemma 1

For any two x such that $x \in (x_0 - \epsilon, x_0 + \epsilon)$ for some ϵ. $P(Y < y, D = d | X = x, C > y)$ is such that

$$
\begin{aligned}
&= P(\min\{Y^*, C\} < y, D = d | X = x, C > y) \\
&= P(Y^*(d) < y, D = d | X = x, C > y) \\
&= P(Y^*(d) < y, D(x) = d | X = x, C > y) \\
&= P(Y^*(d) < y, D(x) = d, T_{|x-x_0|} = A | X = x, C > y) \\
&+ P(Y^*(d) < y, D(x) = d, T_{|x-x_0|} = N | X = x, C > y) \\
&+ P(Y^*(d) < y, D(x) = d, T_{|x-x_0|} = Co | X = x, C > y) \\
&+ P(Y^*(d) < y, D(x) = d, T_{|x-x_0|} = Df | X = x, C > y) \\
&= P(Y^*(d) < y | D(x) = d, T_{|x-x_0|} = A, X = x, C > y) P(D(x) = d, T_{|x-x_0|} = A | X = x, C > y) \\
&+ P(Y^*(d) < y | D(x) = d, T_{|x-x_0|} = N, X = x, C > y) P(D(x) = d, T_{|x-x_0|} = N | X = x, C > y) \\
&+ P(Y^*(d) < y | D(x) = d, T_{|x-x_0|} = Co, X = x, C > y) P(D(x) = d, T_{|x-x_0|} = Co | X = x, C > y) \\
&+ P(Y^*(d) < y | D(x) = d, T_{|x-x_0|} = Df, X = x, C > y) P(D(x) = d, T_{|x-x_0|} = Df | X = x, C > y)
\end{aligned}
\tag{A.1}
$$

Where the first equality is due to the structure of Y. The second equality due to the fact that $C > y$. The third equality is due to the structure of the model. The fourth and fifth one due to the properties of probabilities.

Note that once we condition on the type combined with the value of x treatment becomes a degenerate random variable. Then:

$$
\begin{aligned}
P(Y<y, D=1|X=x, C>y) &= P(Y^*(1)<y|D(x)=1, T_{|x-x_0|}=A, X=x, C>y) \\
&+ 1\{x \geq x_0\}\Big[P(Y^*(1)<y|T_{|x-x_0|}=Co, X=x, C>y)\Big] \\
&\times P(T_{|x-x_0|}=Co|X=x, C>y) \\
&+ 1\{x<x_0\}\Big[P(Y^*(1)<y|T_{|x-x_0|}=Df, X=x, C>y)\Big] \\
&\times P(T_{|x-x_0|}=Df|X=x, C>y) \qquad \text{(A.2)}
\end{aligned}
$$

Then if we aproximate from x above x_0:

$$
\begin{aligned}
P(Y<y, D=1|X=x, C>y) &= P(Y^*(1)<y|D(x)=1, T_{|x-x_0|}=A, X=x, C>y) \\
&+ \Big[P(Y^*(1)<y|T_{|x-x_0|}=Co, X=x, C>y)\Big] \\
&\times P(T_{|x-x_0|}=Co|X=x, C>y)
\end{aligned}
$$

While from below:

$$
\begin{aligned}
P(Y<y, D=1|X=x, C>y) &= P(Y^*(1)<y|D(x)=1, T_{|x-x_0|}=A, X=x, C>y) \\
&+ \Big[P(Y^*(1)<y|T_{|x-x_0|}=Df, X=x, C>y)\Big] \\
&\times P(T_{|x-x_0|}=Df|X=x, C>y)
\end{aligned}
$$

Which in turn implies that for $x > x_0$ and $x' < x_0$, $P(Y<y, D=1|X=x, C>y) - P(Y<y, D=1|X=x', C>y)$

$$
\begin{aligned}
&= P(Y^*(1)<y|D(x)=1, T_{|x-x_0|}=A, X=x, C>y) \\
&+ \Big[P(Y^*(1)<y|T_{|x-x_0|}=Co, X=x, C>y)\Big] \\
&\times P(T_{|x-x_0|}=Co|X=x, C>y) \\
&- P(Y^*(1)<y|D(x')=1, T_{|x'-x_0|}=A, X=x', C>y) \\
&- \Big[P(Y^*(1)<y|T_{|x'-x_0|}=Df, X=x', C>y)\Big]
\end{aligned}
$$

$$
\begin{aligned}
\times \quad & P(T_{|x'-x_0|} = Df | X = x', C > y) \\
= \quad & \Big[P(Y^*(1) < y | D(x) = 1, T_{|x-x_0|} = A, X = x, C > y) \\
- \quad & P(Y^*(1) < y | D(x') = 1, T_{|x'-x_0|} = A, X = x', C > y) \Big] \\
+ \quad & \Big[P(Y^*(1) < y | T_{|x-x_0|} = Co, X = x, C > y) \Big] \times P(T_{|x-x_0|} = Co | X = x, C > y) \\
- \quad & \Big[P(Y^*(1) < y | T_{|x'-x_0|} = Df, X = x', C > y) \Big] \times P(T_{|x'-x_0|} = Df | X = x', C > y) \\
= \quad & \Big[P(Y^*(1) < y | D(x) = 1, T_{|x-x_0|} = A, X = x, C > y) \\
- \quad & P(Y^*(1) < y | D(x') = 1, T_{|x'-x_0|} = A, X = x', C > y) \Big] \\
+ \quad & \Big[\int_y^{C_u} P(Y^*(1) < y | T_{|x-x_0|} = Co, X = x, C = c) \frac{f(c)_{c|T_{|x-x_0|}=Co, X=x}}{P(C > y | T_{|x-x_0|} = Co, X = x)} dc \Big] \\
\times \quad & P(T_{|x-x_0|} = Co | X = x, C > y) \\
- \quad & \Big[P(Y^*(1) < y | T_{|x'-x_0|} = Df, X = x', C > y) \Big] \times P(T_{|x'-x_0|} = Df | X = x', C > y)
\end{aligned}
\tag{A.3}
$$

Now if we take the limit as x, x' go to x_0, due to assumptions 1 and the second part of 2 $\lim_{x' \to x_0} P(T_{|x'-x_0|} = Df | X = x', C > y) = 0$ and since by the first part of Assumption 2, $\lim_{x \to x_0} P(Y^*(1) < y | D(x) = 1, T_{|x-x_0|} = A, X = x, C > y)$ is equal to $\lim_{x' \to x_0} P(Y^*(1) < y | D(x') = 1, T_{|x'-x_0|} = A, X = x', C > y)$ it turns that Which in turn implies then that:

$$
\begin{aligned}
& \lim_{x \downarrow x_0} P(Y < y, D = 1 | X = x, C > y) - \lim_{x \uparrow x_0} P(Y < y, D = 1 | X = x, C > y) = \\
& \lim_{x \downarrow x_0} \Big[\int_y^{C_u} P(Y^*(1) < y | T_{|x-x_0|} = Co, X = x, C = c) \frac{f(c)_{c|T_{|x-x_0|}=Co, X=x}}{P(C > y | T_{|x-x_0|} = Co, X = x)} dc \Big] \\
& \times P(T_{|x-x_0|} = Co | X = x, C > y) \\
& = \lim_{x \downarrow x_0} P(Y^*(1) < y | T_{|x-x_0|} = Co, X = x) \lim_{x \downarrow x_0} P(T_{|x-x_0|} = Co | X = x, C > y)
\end{aligned}
$$

Where the second equality is due to Assumption 5 By a similar logic we can show that:

$$
\lim_{x \downarrow x_0} P(T_{|x-x_0|} = Co | X = x, C > y) \quad = \quad \lim_{x \downarrow x_0} P(D = 1 | X = x, C > y) - \lim_{x \uparrow x_0} P(D = 1 | X = x, C > y)
$$

Which in turn implies that:

$$\begin{aligned}
&\frac{\lim_{x\downarrow x_0} P(Y<y, D=1|X=x, C>y) - \lim_{x\uparrow x_0} P(Y<y, D=1|X=x, C>y)}{\lim_{x\downarrow x_0} P(D=1|X=x, C>y) - \lim_{x\uparrow x_0} P(D=1|X=x, C>y)} \\
&= \lim_{x\downarrow x_0} P(Y^*(1)<y|T_{|x-x_0|}=Co, X=x) \\
&= \lim_{x\to x_0} P(Y^*(1)<y|T_{|x-x_0|}=Co, X=x) \qquad \text{(A.4)}
\end{aligned}$$

Where the last equality is due to Assumption 2.

And similarly,

$$\begin{aligned}
&-\frac{\lim_{x\downarrow x_0} P(Y<y, D=0|X=x, C>y) - \lim_{x\uparrow x_0} P(Y<y, D=0|X=x, C>y)}{\lim_{x\downarrow x_0} P(D=1|X=x, C>y) - \lim_{x\uparrow x_0} P(D=1|X=x, C>y)} \\
&= \lim_{x\to x_0} P(Y^*(0)<y|T_{|x-x_0|}=Co, X=x) \qquad \text{(A.5)}
\end{aligned}$$

Thus,

$$\begin{aligned}
&LATE_{1(Y<y)x_0,Co} = \frac{\lim_{x\downarrow x_0} P(Y<y, D=1|X=x, C>y) - \lim_{x\uparrow x_0} P(Y<y, D=1|X=x, C>y)}{\lim_{x\downarrow x_0} P(D=1|X=x, C>y) - \lim_{x\uparrow x_0} P(D=1|X=x, C>y)} \\
&+\frac{\lim_{x\downarrow x_0} P(Y<y, D=0|X=x, C>y) - \lim_{x\uparrow x_0} P(Y<y, D=0|X=x, C>y)}{\lim_{x\downarrow x_0} P(D=1|X=x, C>y) - \lim_{x\uparrow x_0} P(D=1|X=x, C>y)} \\
&= \frac{\lim_{x\downarrow x_0} P(Y<y|X=x, C>y) - \lim_{x\uparrow x_0} P(Y<y|X=x, C>y)}{\lim_{x\downarrow x_0} P(D=1|X=x, C>y) - \lim_{x\uparrow x_0} P(D=1|X=x, C>y)} \qquad \text{(A.6)}
\end{aligned}$$

A.2 Simulations

A.2.1 Simulation of Agents and Their Payment Behavior

We generate a continuous variable *icc* representing the Poverty Score Index for a population of N agents icc $\sim \mathcal{N}(\mu_{icc}, \sigma_{icc})$

There are two types of agents: Never Repay (NR) and Repay Conditional on Economic Situation (RC). The probability of an agent being NR increases with *icc*. We use a logistic model $P(\text{NR}) = \frac{\exp(b_0+b_1\cdot icc)}{1+\exp(b_0+b_1\cdot icc)}$ where b_0 and b_1 are parameters obtained from an estimation where the dependent variable is an indicator that the individual defaults in the first installment. NR status is assigned based on $P(NR)$ relative to a random number $(0.1 + 0.5 * u_{NR}$

where u_{NR} is drawn from an $U(0,1)$.

On the other hand, RC agents experience an idiosyncratic income shock each month, that is drawn from an i.i.d. unit uniform distribution. Depending on that shock, they decide whether or not to repay their debt. In particular, if the shock is below a threshold, they do not repay. This repayment threshold depends on the treatment status. Once in default, the agent remains in this state unless her income shock exceeds a threshold that also depends on the treatment status.

The thresholds that determines delinquency are: $\pi_{L,0} = 0.05$ for the untreated and $\pi_{L,1} = 0.10$ for the treated. Additionally, those for exiting the default state are: $\pi_{H,0} = 0.90$ for the untreated and $\pi_{H,1} = 0.95$ for the treated.

For $m = 1, 2, ..M$ periods, the potential payment (exit the default state) at each period is $\texttt{pay}_{t,m}$ ($\texttt{exit}_{t,m}$) where $t \in 0, 1$ denotes non-treated and treated potential status of the agent are simulated as follows ($u_m \sim U(0,1)$):

$$\texttt{pay}_{t,m} = \begin{cases} 1 & \text{if } u_m > \pi_{L,t} \\ 0 & \text{otherwise} \end{cases} \qquad \texttt{exit}_{t,m} = \begin{cases} 1 & \text{if } u_m > \pi_{H,t} \\ 0 & \text{otherwise} \end{cases}$$

The duration in the states of Up to Date (UTD) and Default (DEF) is computed separately for treated and untreated groups.

$$\texttt{dur}_{t,m,\text{UTD}} = \begin{cases} 0 & \text{if NR} \\ 1 & \text{if } \texttt{pay}_{t,1} = 1 \text{ or } (exit_{t,m} = 1 \text{ and } dur_{t,m-1,DEF} \geq 1) \\ \texttt{dur}_{t,m-1,\text{UTD}} + 1 & \text{if } \texttt{pay}_{t,m} = 1 \text{ and } dur_{t,m-1,UTD} \neq 0 \text{ and } m > 1 \end{cases}$$

$$\texttt{dur}_{t,m,\text{DEF}} = \begin{cases} M & \text{if NR} \\ 1 & \text{if } \texttt{pago}_{t,m} = 0 \text{ and } dur_{t,m-1,DEF} = 0 \\ \texttt{dur}_{t,m-1,\text{DEF}} + 1 & \text{if } dur_{t,m-1,DEF} \neq 0 \text{ and } exit_{t,m} = 0 \end{cases}$$

Treatment is defined by using the *icc*, those above a fixed cutoff are elegible. But there is not perfect compliance. Agents are categorized into four types based on compliance probabilities of being "Complairs" (Co), "Always takers" (A), "Never takers" (N) or "Defiers" (Df):

The compliance type is assigned using random draws $T_\epsilon \sim U(0,1)$, and:

$$\text{Type} = \begin{cases} \text{Co} & \text{if } T_\epsilon \leq 0.85 \\ \text{A} & \text{if } T_\epsilon > 0.85 \\ \text{N} & \text{if } T_\epsilon > 0.95 \\ \text{Df} & \text{if } T_\epsilon > 1 \end{cases}$$

The treatment variable T is defined as follows:

$$T = \begin{cases} 1, & \text{if icc} > \text{cutoff and Te} = \text{“Co”} \\ 0, & \text{if icc} \leq \text{cutoff and Te} = \text{“Co”} \\ 1, & \text{if Te} = \text{“A”} \\ 0, & \text{if Te} = \text{“N”} \\ 1, & \text{if icc} \leq \text{cutoff and Te} = \text{“Df”} \\ 0, & \text{if icc} > \text{cutoff and Te} = \text{“Df”} \end{cases}$$

The observable duration variables are defined as:

$$\begin{aligned} \texttt{dur}_{\text{UTD}} &= \texttt{dur}_{1,\text{UTD}} \cdot T + \texttt{dur}_{0,\text{UTD}} \cdot (1-T) \\ \texttt{dur}_{\text{DEF}} &= \texttt{dur}_{1,\text{DEF}} \cdot T + \texttt{dur}_{0,\text{DEF}} \cdot (1-T) \end{aligned}$$

Censoring depends on agent type. NR agents are never/ever censored in the UTD/DEF state (Cen_UTD= 0 and Cen_DEF=1). For RC agents:

$$\texttt{Cen}_{\text{UTD/DEF}} = \begin{cases} 1 & \text{if } u_2 > 0.95 \\ 0 & \text{otherwise} \end{cases}$$

where $u_2 \sim U(0,1)$. For those that are censored, the final period in which we observe them is determined according a random number that is drawn from a Poisson distribution.

A.2.2 Performance of the estimators

We simulate a population of one million individuals and apply our LATE estimator, as well as those of Arai et al. (2022) and Calonico et al. (2017), to samples of size 10,000 drawn from the simulated data. The following table presents the performance of these estimators for the UTD duration using standard statistics. Results correspond to seven hundred replicates.[1]

[1]Results for the DEF state, as well as those for 1,000 replicates and alternative sample sizes, will be added.

The results are mixed when considering the mean and median bias, but the estimand proposed in this work outperforms others in terms of Mean Squared Error and coverage.

Table A.1: Estimators performance for simulation of UTD duration

(a) Duracion less or equal 3

	MeanBias	MedianBias	MSE	Coverage
ASLate	-0.0027	-0.0088	0.0004	0.9943
Arai (All)	0.0016	-0.0091	0.0019	0.9672
Arai (Non-censored)	0.0016	-0.0067	0.0019	0.9672
CCT (All)	0.0021	-0.0092	0.0016	0.9715
CCT (Non-censored)	0.0021	-0.0063	0.0016	0.9715

(b) Duracion less or equal 9

	MeanBias	MedianBias	MSE	Coverage
ASLate	-0.0095	-0.0088	0.0004	0.9858
Arai (All)	-0.0096	-0.0091	0.0018	0.9601
Arai (Non-censored)	-0.0082	-0.0067	0.0018	0.9630
CCT (All)	-0.0086	-0.0092	0.0015	0.9786
CCT (Non-censored)	-0.0074	-0.0063	0.0015	0.9786

(c) Duracion less or equal 13

	MeanBias	MedianBias	MSE	Coverage
ASLate	-0.0145	-0.0088	0.0005	0.9701
Arai (All)	-0.0195	-0.0091	0.0016	0.9558
Arai (Non-censored)	-0.0121	-0.0067	0.0015	0.9658
CCT (All)	-0.0203	-0.0092	0.0015	0.9558
CCT (Non-censored)	-0.0121	-0.0063	0.0013	0.9715

A.3 Testability of our Model

Note from the results in Appendix A.1

$$\lim_{x \downarrow x_0} P(Y < y, D = 1 | X = x, C > y) - \lim_{x \uparrow x_0} P(Y < y, D = 1 | X = x, C > y) =$$
$$= \lim_{x \to x_0} P(Y^*(1) < y, T_{|x - x_0|} = Co | X = x)$$
$$\lim_{x \downarrow x_0} P(Y < y, D = 0 | X = x, C > y) - \lim_{x \uparrow x_0} P(Y < y, D = 0 | X = x, C > y) =$$
$$= - \lim_{x \to x_0} P(Y^*(0) < y, T_{|x - x_0|} = Co | X = x)$$

Where both of them are probabilities, thus, a testable condition of our model is:

$$
\begin{aligned}
0 < [\lim_{x \uparrow x_0} E(1(Y < y)(1-D)|X = x, C > y) - \lim_{x \downarrow x_0} E(1(Y < y)(1-D)|X = x, C > y)] \leq 1 \\
0 < [\lim_{x \downarrow x_0} E(1(Y < y)D|X = x, C > y) - \lim_{x \uparrow x_0} E(1(Y < y)D|X = x, C > y)] \leq 1
\end{aligned} \tag{A.7}
$$

Note that $E(1(Y < y)(1-D)|X = x, C > y) \in [0,1]$ and $E(1(Y < y)(1-D)|X = x, C > y) \in [0,1]$ thus trivially $E(1(Y < y)(1-D)|X = x, C > y) - E(1(Y < y)(1-D)|X = x, C > y) \leq 1$. Furthermore, the previous inequalities need to hold for every value of y, thus we need to test:

$$
\begin{aligned}
0 < \inf_y [\lim_{x \uparrow x_0} E(1(Y < y)(1-D)|X = x, C > y) - \lim_{x \downarrow x_0} E(1(Y < y)(1-D)|X = x, C > y)] \\
0 < \inf_y [\lim_{x \downarrow x_0} E(1(Y < y)D|X = x, C > y) - \lim_{x \uparrow x_0} E(1(Y < y)D|X = x, C > y)]
\end{aligned} \tag{A.8}
$$

If we reject these conditions, our model is rejected. These can be tested in a manner similar to Arai et al. (2022).

B The influence function of the parametric estimator

The parametric version of $LATE$ under assumptions 1-6 is:

$$
LATE_{I_y, x_0, Co} = \frac{\Phi(\gamma_{0,y} + \gamma_{x,y}X + \gamma_{x_0,y}) - \Phi(\gamma_{0,y} + \gamma_{x,y}X)}{\Phi(\beta_{0,y} + \beta_{x,y}X + \beta_{x_0,y}) - \Phi(\beta_{0,y} + \beta_{x,y}X)}
$$

By the chain rule of influence functions (Jann, 2019), the influence function of our parameter of interest is:

$$
\begin{aligned}
IF_{LATE} = & \frac{[\phi(\gamma_{0,y} + \gamma_{x,y}X + \gamma_{x_0,y}) - \phi(\gamma_{0,y} + \gamma_{x,y}X)][IF_{\gamma_{0,y}} + XIF_{\gamma_{x,y}}]}{\Phi(\beta_{0,y} + \beta_{x,y}X + \beta_{x_0,y}) - \Phi(\beta_{0,y} + \beta_{x,y}X)} \\
& + \frac{[\phi(\gamma_{0,y} + \gamma_{x,y}X + \gamma_{x_0,y})][IF_{\gamma_{x_0,y}}]}{\Phi(\beta_{0,y} + \beta_{x,y}X + \beta_{x_0,y}) - \Phi(\beta_{0,y} + \beta_{x,y}X)} \\
& - \frac{\Phi(\gamma_{0,y} + \gamma_{x,y}X + \gamma_{x_0,y}) - \Phi(\gamma_{0,y} + \gamma_{x,y}X)}{[\Phi(\beta_{0,y} + \beta_{x,y}X + \beta_{x_0,y}) - \Phi(\beta_{0,y} + \beta_{x,y}X)]^2} \\
& \times \Bigg[[\phi(\beta_{0,y} + \beta_{x,y}X + \beta_{x_0,y}) - \phi(\beta_{0,y} + \beta_{x,y}X)][IF_{\beta_{0,y}} + XIF_{\beta_{x,y}}] \\
& + [\phi(\beta_{0,y} + \beta_{x,y}X + \beta_{x_0,y})][IF_{\beta_{x_0,y}}] \Bigg]
\end{aligned}
$$

Thus, we need to obtain the influence function for each of the respective parameters of that define the influence function of our parameter of interest. Since both the numerator and denominator parameters can be expressed in a similar way, we focus on the denominator ones and a symmetric argument holds for the numerator ones. Note that the following moment equations define the parameters of interest:

$$\begin{pmatrix} E[(1(D=1,C>y)-\Phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}1(X\geq x_0)))]=0 \\ E[(1(D=1,C>y)-\Phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}1(X\geq x_0)))X]=0 \\ E[(1(D=1,C>y)-\Phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}1(X\geq x_0)))1(X\geq x_0)]=0 \end{pmatrix}$$

Rename for the sake of notation compactness $(1(D=1,C>y)=W, 1(X\geq x_0)=Z$. Following Kahn (2015) we can perturb the probability distribution towards a point mass distribution with weight ϵ. Thus, this contaminated distribution solves:

$$\begin{pmatrix} (1-\epsilon)E[(W-\Phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))]+\epsilon[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))]=0 \\ (1-\epsilon)E[(W-\Phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))X]+\epsilon[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))x]=0 \\ (1-\epsilon)E[(W-\Phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))Z]+\epsilon[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))z]=0 \end{pmatrix}$$

Take for example the third element, then differentiating with respect to ϵ (recalling the parameters are also functions of the distribution) we obtain:

$$\begin{aligned} &-E[(W-\Phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))Z]+(1-\epsilon)E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))Z]IF_{\beta_{0,y}} \\ &+(1-\epsilon)E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))ZX]IF_{\beta_{x,y}}+(1-\epsilon)E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))ZZ]IF_{\beta_{x_0,y}} \\ &=-[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))z]-\epsilon[(\phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))z]IF_{\beta_{0,y}} \\ &-\epsilon[(\phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))zx]IF_{\beta_{x,y}}-\epsilon[(\phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))zz]IF_{\beta_{x_0,y}} \end{aligned}$$

Now taking ϵ to 0 and recalling that by construction $E[(W-\Phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))Z]=0$:

$$-[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))z] \quad = \quad E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))Z]IF_{\beta_{0,y}}$$

$$
\begin{aligned}
&+ \quad E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))ZX]IF_{\beta_{x,y}} \\
&+ \quad E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))ZZ]IF_{\beta_{x_0,y}}
\end{aligned}
$$

By a similar display we also get:

$$
\begin{aligned}
-[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))x] &= E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))X]IF_{\beta_{0,y}} \\
&+ E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))XX]IF_{\beta_{x,y}} \\
&+ E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))XZ]IF_{\beta_{x_0,y}}
\end{aligned}
$$

$$
\begin{aligned}
-[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))] &= E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))]IF_{\beta_{0,y}} \\
&+ E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))X]IF_{\beta_{x,y}} \\
&+ E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))Z]IF_{\beta_{x_0,y}}
\end{aligned}
$$

Which forms a 3 by 3 system of equations which solutions give the influence functions for the respective parameters. A similar 3 by 3 system can be elaborated for the γ parameters replacing the βs by γs. In matrix form, let A be defined as:

$$
\begin{pmatrix}
E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))] & E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))X] & E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))Z] \\
E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))X] & E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))XX] & E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))ZX] \\
E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))Z] & E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))ZX] & E[(\phi(\beta_{0,y}+\beta_{x,y}X+\beta_{x_0,y}Z))ZZ]
\end{pmatrix}
$$

Then,

$$
A \times \begin{pmatrix} IF_{\beta_{0,y}} \\ IF_{\beta_{x,y}} \\ IF_{\beta_{x_0,y}} \end{pmatrix} = \begin{pmatrix} -[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))] \\ -[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))x] \\ -[(w-\Phi(\beta_{0,y}+\beta_{x,y}x+\beta_{x_0,y}z))z] \end{pmatrix}
$$

Thus replacing the expectations with sample analogs, the coefficients with their estimates we can obtain by matrix inversion the estimated influence function for a generic individual, then replacing w, z, x with the respective W_i, Z_i, X_i we can get the influence function for each

individual. Then we can replace these objects in the influence function for $LATE$ to obtain their estimated influence function. In a more concise notation, let

$\psi_{\rho_i} = \psi(\rho_{0,y} + \rho_{x,y}X_i + \rho_{x_0,y}Z_i)$ and $\psi_{\rho_+} = \psi(\rho_{0,y} + \rho_{x,y}x_0 + \rho_{x_0,y})$ and $\psi_{\rho_-} = \psi(\rho_{0,y} + \rho_{x,y}x_0)$

with $\psi = \{\Phi, \phi\}$ and $\rho = \{\gamma, \beta\}$. Omitting the subscript y then,

$$\begin{aligned} IF_{LATE} =& \frac{(\phi_{\gamma+} - \phi_{\gamma-})(IF_{\gamma_0,i} + IF_{\gamma_x,i}X_i) + \phi_{\gamma+}IF_{\gamma_{x0},i}}{\Phi_{\beta+} - \Phi_{\beta-}} \\ & - \frac{\Phi_{\gamma+} - \Phi_{\gamma-}}{(\Phi_{\beta+} - \Phi_{\beta-})^2}\left[(\phi_{\beta+} - \phi_{\beta-})(IF_{\beta_0,i} + IF_{\beta_x,i}X_i) + \phi_{\beta+}IF_{\beta_{x0},i}\right] \end{aligned}$$

With influence functions solving:

$$\begin{bmatrix} IF_{\rho_0,i} \\ IF_{\rho_x,i} \\ IF_{\rho_{x0},i} \end{bmatrix} = A_n^{-1} \begin{bmatrix} -(w_i - \Phi_{\rho,i}) \\ -(w_i - \Phi_{\rho,i})X_i \\ -(w_i - \Phi_{\rho,i})Z_i \end{bmatrix}$$

Where ($\rho = \{\gamma, \beta\}$). And the sample analog of 3×3 moment matrix is:

$$A_n = \frac{1}{N}\begin{bmatrix} \sum \phi_{\rho,i} & \sum \phi_{\rho,i}X_i & \sum \phi_{\rho,i}Z_i \\ \sum \phi_{\rho,i}X_i & \sum \phi_{\rho,i}X_iX_i & \sum \phi_{\rho,i}Z_iX_i \\ \sum \phi_{\rho,i}Z_i & \sum \phi_{\rho,i}X_iZ_i & \sum \phi_{\rho,i}Z_iZ_i \end{bmatrix}$$

Where inside each ϕ, Φ the parameters are replaced with their estimates.

C Additional Tables and Figures

Table A.2: Summary statistics: Fuzzy Design

	Untreated (%)	Treated (%)
Non-Eligible (%)	82.62	8.17
Eligible (%)	17.38	91.83