

# The controlled choice design and private paternalism in pawnshop borrowing

CRAIG MCINTOSH

Department of Economics University of California San Diego

ISAAC MEZA

Department of Economics Harvard University

JOYCE SADKA

Department of Economics Instituto Tecnológico Autónomo de México

ENRIQUE SEIRA

Department of Economics Michigan State University

FRANCIS J. DiTRAGLIA

Department of Economics University of Oxford

We use a novel three-armed RCT, including both forced and voluntary treatment arms, to provide a unique window into choice, heterogeneous treatment effects and paternalism in the context of pawnbroker lending. Forcing borrowers into commitment contracts with a regular repayment structure decreases the financial cost of pawn loans by 22% inclusive of fees, increases the likelihood of recovering their pawn by 15%, and increases the likelihood of repeat business by 19%. Leveraging the special features of our experimental design, we go on to point-identify the effects of commitment on both the treated and the untreated simultaneously, along with the average selection on gains. We find large and significant effects of commitment even for borrowers who would not voluntarily choose it, and no evidence of selection on gains: borrowers who would freely choose commitment do not appear to benefit more than borrowers who would not. A detailed analysis of treatment effect heterogeneity suggests that the potential gains from targeting commitment based on observable characteristics are extremely small and that most borrowers stand to gain from a policy of universal forced commitment.

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Craig McIntosh: [ctmcintosh@ucsd.edu](mailto:ctmcintosh@ucsd.edu)

Isaac Meza: [isaacmezalopez@g.harvard.edu](mailto:isaacmezalopez@g.harvard.edu)

Joyce Sadka: [jsadka@itam.mx](mailto:jsadka@itam.mx)

Enrique Seira: [enrique.seira@gmail.com](mailto:enrique.seira@gmail.com)

Francis J. DiTraglia: [francis.ditraglia@economics.ox.ac.uk](mailto:francis.ditraglia@economics.ox.ac.uk)

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## 1. INTRODUCTION

The behavioral finance literature has established an important role for commitment devices in helping consumers to achieve their own financial goals. While most academic studies on commitment focus on the role of voluntary self-commitment (Thaler and Benartzi, 2004, Prina, 2015, Brune et al., 2016, Callen et al., 2019, Dupas and Robinson, 2013, Ashraf et al., 2006), in reality the predominant use of rigid structure in financial services is involuntary; firms only offer a product with these features embedded. Laibson (2018) has referred to this implicit bundling of commitment devices as “private paternalism”, and its logic is that individuals may benefit from commitment and yet not explicitly demand it. Comparing voluntary versus paternalistic programs requires that we form counterfactuals for two different groups of people: those who would freely choose commitment, and those who would not (since the latter group is treated only under paternalist policies). In this paper we present an experimental design and econometric analysis that point-identifies and provides estimates for both the effect of treatment on the treated (TOT) and on the untreated (TUT). This permits a clear window into the case for paternalistic (forced) rather than voluntary commitment in financial services.

The relationship between treatment effects and treatment take-up is a core concern in the econometrics literature. In principle, the Marginal Treatment Effect (MTE) approach allows researchers to use observational data and a single excluded instrument to study this relationship. In practice, however, unless the instrument has a rich support set, MTEs can only be point identified by using additional modeling assumptions (Mogstad et al., 2018). An alternative research strategy to study the relation between treatment effects and treatment take-up uses the Becker-DeGroot-Marschak (BDM) mechanism, incentivizing choice prior to treatment assignment to elicit willingness to pay (WTP) for treatment (Becker et al., 1964). A number of studies, however, find that the WTP elicited under the BDM mechanism changes substantially with the distribution of prices used in the elicitation exercise (Bohm et al., 1997, Banerji and Gupta, 2014). This falsifies the assumption of standard preferences that is required for BDM to be incentive-compatible Mamadehussene and Sguera (2023), suggesting that the mechanism may not provide a reliable measure of actual compliance in practice. Our study avoids the drawbacks of the MTE and BDM approaches by combining a novel three-armed randomized controlled experiment, including forced treatment and treatment choice arms, with two transparent exclusion restrictions. Together, these allow us to point identify the relevant TOT and TUT effects in a real-world, high-stakes setting and to study the case for paternalistic commitment head-on, with minimal assumptions.

We apply this approach to an important and understudied context: pawnshop lending. Pawn loans constitute one of the oldest and most prevalent forms of borrowing (Carter and Skiba, 2012). Our partner lender, for example, made over 4 million loans to more than a million clients during the past three years.<sup>1</sup> The question of choice versus

<sup>1</sup>For comparison, there were 2.3 million microfinance clients in Mexico in 2009 (Pedroza, 2010).

paternalism is particularly salient in this context, as choice mistakes could arise from borrowers' low education and the fact that they typically borrow for emergencies under significant stress.<sup>2</sup> Our experiment covers just under 5,000 pawnshop clients in 6 of our partner lender's Mexico City branches. Our control arm illustrates the costs of the status quo contract: fully 44% of borrowers default, losing lose their pawn along with any payments made towards principal.<sup>3</sup> Our "commitment choice" arm gives borrowers the chance to opt into a structured repayment contract when taking a loan. The structured contract requires borrowers to make three monthly payments rather than one balloon payment at the end, with each monthly payment including the accrued interest at that time as well as a nominal fee of 2% of that month's payment if the payment is delinquent. The fee serves as a reminder and a means of reinforcing the importance of these interim payments. In our "forced commitment" arm all borrowers are *required* to repay using the same structured monthly contract offered on an opt-in basis in the commitment choice arm.

We address three key questions. First, do structured repayment contracts lower financial costs for pawnshop borrowers? Second, do borrowers recognize this benefit, demanding commitment in sufficient numbers? Finally, and most uniquely, do the *right* borrowers voluntarily demand commitment? Our ability to answer the last question comes from our unique three-armed experimental design, which we call the "controlled choice design" for short. This design can be viewed as a juxtaposition of *two* randomized encouragement designs, each with one-sided non-compliance. One of them point identifies the effect of commitment for borrowers who would *voluntarily* choose commitment (TOT), while the other point identifies the effect of commitment for borrowers who would *not* (TUT). By identifying both the TOT and TUT effects in the same experiment, the controlled choice design allows us to examine the empirical relevance of "selection on gains" also known as Roy-type selection into treatment by estimating the "average selection on gains"  $ASG = TOT - TUT$ . This enables us to test whether borrowers who voluntarily choose commitment have higher average treatment effects than those who do not, rather than assuming it. The controlled choice design also point identifies the average selection bias (ASB)—the average difference in untreated (status quo) potential outcomes for those who choose commitment relative to those who do not—along with the average selection on levels (ASL)—the analogous comparison for treated (commitment) potential outcomes. Taken together, these causal effects allow us to "go under the hood" of our baseline ATE results, and paint a more complete and economically relevant picture of the effects of commitment. We are unaware of any other paper that simultaneously identifies all of these causal effects without recourse to additional structural modeling assumptions.

We find that commitment is strongly effective in lowering financial costs and preventing default in pawnshop lending: the average individual in the forced arm pays financing costs inclusive of fees that are 22% lower than the control, and faces a probability of default that is 6.6 percentage points lower (15% of the mean). In terms of

<sup>2</sup>A large literature shows stress impairs cognitive function, e.g. [Starcke and Brand \(2012\)](#).

<sup>3</sup>High pawn default rates are common, in the US they oscillate around 15% (see [here](#)).

Annual Percentage Rates, the financial cost of borrowing falls by 11 percentage points (19% of the mean). In short, structured commitment saves borrowers money by charging them fees! Our results are qualitatively robust to deducting transport costs of visiting the branch to make interim payments along with a day of lost wages for each visit. They are also robust to using borrowers' *subjective* values of their pawns rather than the appraised value of the gold, and to adjusting for lost liquidity from requiring monthly payments. The monthly payment contract seems to achieve these cost savings by speeding up payments and by generating an early bifurcation of borrowers into those that will recover the pawn and those that will not. The former are induced to pay faster, saving on interest; the latter pay less towards loans that ultimately default, hence losing less money when they do.

Despite these large financial cost savings, only 11% of borrowers in the choice arm choose commitment. Can the borrowers who did not choose commitment be those who simply don't need it? To answer this question, we carry out a detailed analysis of treatment effect heterogeneity. We begin by bounding the distribution of individual treatment effects using the marginal outcome distributions from the forced commitment and control arms, following [Fan and Park \(2010\)](#). This approach imposes no assumptions beyond the experimental randomization. We find that *at least* 23% of borrowers benefit from commitment. This implies that there must be many borrowers in the choice arm who did not demand commitment despite their *individual treatment effect* from (forced) commitment being positive. Next, we impose an exclusion restriction positing that the effect of a given contract does not depend on *how* borrowers obtain it. In other words, we assume that choosing a contract results in the same potential outcome as being assigned that contract. This is a relatively common if often implicit assumption in causal inference.<sup>4</sup> It also has testable implications that we fail to reject in our empirical context.<sup>5</sup> Under this restriction, the controlled choice design point identifies the TOT, TUT, ASG, ASB, and ASL effects described above. Our estimated TUT effect on financial cost savings is large: \$192 pesos, equivalent to a 10.6 percentage point savings in APR. On average, the borrowers who would *not* choose commitment, would have faced substantially lower financial costs if they had. Finally, we combine our experimental treatment and outcome data with survey responses collected for a subset of borrowers to estimate conditional average treatment effects, both TUTs and ATE, using the Causal Random Forest algorithm of [Athey et al. \(2019\)](#). We estimate positive conditional average TUT effects for 93% of the borrowers who did *not* choose the commitment contract. In short, it is extremely difficult to find identifiable groups of borrowers who are *harmed* by commitment, even when restricting attention to those who would not choose it voluntarily. While targeting commitment products to those that benefit the most is a policy that appears attractive, in this context we find that the usable targeting variables have relatively weak predictive power and hence even our best random forest targeting only reduces

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<sup>4</sup>Papers that use variation in compulsory schooling laws to identify the returns to schooling, for example, typically interpret their results as the causal effect of additional *education* rather than additional *forced education*. [Chamberlain \(2011\)](#) uses a closely related assumption to develop a theory of optimal treatment choice.

<sup>5</sup>See [Appendix G](#) for details.

the overall mis-targeting rate from 9.7% (all to Forcing) to 9.5% (our best-case feasible targeting mechanism).

What explains the persistence of no-commitment contracts so contrary to borrowers' interests in the real world? From the demand side, while a simple measure of time inconsistency does not explain the large and positive TUT effect, we show substantial levels of over-optimism among borrowers. Among borrowers who do not choose commitment, those with the largest estimated benefits from commitment are the individuals who most systematically over-estimate their own probability of repayment without the need to commit, potentially decreasing their demand from commitment. From the supply side, because borrowers' financial savings come directly from the pockets of lenders, pawnshops have an interest in retaining the no-commitment contract. Indeed, pawnshop lending presents an inverted lending case: since these loans are over-collateralized, the lender in the contract stands to gain the most when borrowers default. Our partner's *status quo* pawn contract gave 70% of the value of gold collateral in credit, and charged a monthly interest rate of 7% for loans of a three-month duration, with a flexible no-reminders contract, that could be paid back anytime before the loan comes due at no penalty. This contract is standard contract in the industry. This combination of features, and the fact that the gold pawn is highly liquid, means that the lender makes 90% more profit over three months from a borrower who defaults than one who repays (30% of collateral value recovered under default, 15.8% of collateral value paid in interest if loan fully repaid). While an older literature considers the exploitative potential of over-collateralization and underpriced collateral (Basu, 1984), the implication of such contracts has not been analyzed in the behavioral literature.

Our paper makes a number of contributions to the literature. First, we propose a way to use this three-armed experimental structure to recover treatment effects for choosers and non-choosers under minimal assumptions.<sup>6</sup> A companion STATA package provides simple, regression-based estimators for each of these causal parameters, along with appropriate cluster-robust standard errors.<sup>7</sup> The controlled choice design could be useful in other experimental settings where the question of interest centers on the merits of paternalism, public or private, or the relationship between choice and treatment effects. One obvious example is the design of other financial contracts beyond pawn loans. Another is education, where teachers typically mandate quizzes, homework, and other commitment mechanisms to mitigate student procrastination (Ariely and Wertenbroch, 2002). We further contribute to a relatively small existing literature that sheds light on private paternalism. In the context of food choice, Sadoff et al. (2019) show that individuals with the most time-inconsistent preferences are actually least likely to demand commitment. In contrast to their paper, we directly identify the TUT, obviating the need to first elicit preferences before testing for negative selection. In the context of school choice, Walters (2018) combines a distance instrument with additional structural assumptions, obtaining model-based TUT and ATE estimates. He finds that students who

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<sup>6</sup>While Fowlie et al. (2021) likewise employ a three-armed experimental design in their study of the effect of electricity pricing, they identified *two* TOT effects for different groups of "treated" households, whereas we simultaneously identify the TOT and TUT effects defined with respect to a single "treated" group of borrowers. This difference is what allows our design to point identify the ASG and related quantities.

<sup>7</sup>See Appendix H for details.

select into more effective schools have smaller treatment effects from attending than those who do not select in. In contrast, our approach point identifies TUT and TOT and a range of other interesting causal parameters without the need for a structural model, relying instead on relatively weak exclusion restrictions whose testable implications we fail to reject.

Our study also speaks to recent research on the effects of payment frequency. While experiments in microfinance markets have *not* shown the same benefits from providing a more regularized repayment environment as we find here (Field and Pande, 2008, Barboni and Agarwal, 2023), these experiments differ from ours in two important ways: they are performed on top of already highly structured micro-finance contracts, and they involve borrower pools who may have selected into that type of lending precisely because it provides structure Bauer et al. (2012). These differences may explain why (Field and Pande, 2008) finds almost no default in the control group, in stark contrast to our setting of high default. Second, we provide a deeper analysis of *both* take-up and the efficacy of voluntary commitment mechanisms. A number of papers have found low demand for commitment as we do.<sup>8</sup> Unlike all of these papers, however, we separately point-identify and estimate the effects of commitment for borrowers who would and *would not* choose it. This allows us to conduct a more rigorous and nuanced analysis of private paternalism.

The remainder of the paper is structured as follows: Section 2 provides context and defines our main outcome variables. Section 3 describes the experiment and data sources, and shows pre-treatment balance across arms. Section 4 provides the standard ITT analysis of the experiment, while Section 5 shows how to identify, estimate and carry out inference for the TOT, TUT, ASG, ASB and ASL effects under the controlled choice design. Section 6 investigates why paternalism functions so well in this context and whether it can be more finely targeted and Section 7 concludes.

## 2. CONTEXT

### 2.1 Pawnshop borrowing

Pawn loans involve individuals leaving valuable liquid assets, typically jewelry, as collateral in exchange for an immediate cash loan. Collateral is typically more valuable than the loan amount, allowing lenders to give the loan immediately without checking a borrower's credit history. This makes pawn loans a popular way to get cash to pay for emergencies. In fact, they are one of the most prevalent forms of borrowing. There are more than 11,000 pawn shops across the US, with 30 million clients and \$14 billion in yearly revenues.<sup>9</sup> Our partner pawn lender alone served more than 1 million clients in the last 3 years with more than 4 million contracts. For comparison there were 2.3 million micro-finance clients across all lenders in Mexico in 2009 (Pedroza, 2010).

<sup>8</sup>Ashraf et al. (2006), Giné et al. (2010), Bai et al. (2020), Royer et al. (2015), Sadoff et al. (2019). Others have found more robust demand for commitment (Kaur et al. (2015), Casaburi and Macchiavello (2019), Schilbach (2019), Tarozzi et al. (2009), Dupas and Robinson (2013)).

<sup>9</sup>See [here](#), [here](#), and [here](#).



Pawning is also one of the oldest forms of borrowing. Pawn lending existed in antiquity at least since the Roman Empire, and there are records of it in China about 1,500 years ago (Gregg, 2016). In spite of the high prevalence and long history, pawnshop borrowing has not received much attention in the economics literature. The closest widely studied product is payday lending. In developing countries, however, payday lending is likely small compared to pawnshop lending; the latter is faster and requires less documentation, making it more accessible to informal sector workers who receive their salaries in cash.

As with payday lending, pawnshop lending is controversial. Regulators have concerns with the sophistication of borrowers using it, speculating they may suffer from behavioral and cognitive deficiencies that lead to making sub-optimal choices, biases that are exacerbated by contract design.<sup>10</sup> There is some evidence in support of this view for payday borrowers<sup>11</sup> but none for the large pawn-lending industry. Our study reinforces the idea that a lack of sophistication may be an integral part of the way that standard pawn contracts are designed and structured by lenders.

## 2.2 *Pawning Logistics and Contracts*

To study this market, we partnered with one of the largest pawn shops in Mexico, an institution with more than one hundred branches spanning multiple states in Mexico. This lender (whom we refer to as ‘Lender P’) has a simple and typical business model.

*Appraising and Lending* Lender P takes gold jewelry as collateral in exchange for a fraction of the value of the piece, in cash. No other collateral and no credit history checks are needed. The transaction takes less than 10 minutes and is conducted at the branch in person between the client and the appraiser (i.e. a teller). The appraiser weighs the gold piece and runs tests on its purity. Based on these she assigns a gold value to the piece, stores it as collateral, and gives 70% of the gold value of the piece in cash to the client. The borrower signs a 2-page contract with the conditions of the loan and leaves with the cash.

*Contract* Lender P had only one type of contract, henceforth the *status quo* contract. It stipulated that the interest rate was 7% *per month* compounded daily on the outstanding amount of the loan. The loan had a 90 days term with 15 days’ grace period. The client could make payments at the branch at any time with no penalty for pre-payment. Under this status quo contract, there are no payment reminders or any other kind of interim contact between the lender and the borrower. If the client returns to pay the principal plus the accumulated interest within 105 days, she recovers her pawn, otherwise

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<sup>10</sup>The US congress has actually banned the payday lending industry from serving active military personnel, and some States in the US have imposed zoning restrictions, interest caps, and restrictions on serial borrowing as consumer protection measures against payday lending (Stegman, 2007).

<sup>11</sup>Bertrand and Morse (2011) write that “Under the view that the people borrowing from payday lenders are making an informed, utility-maximizing choice given the constraints that they face, one would not expect additional information disclosure about the payday product to alter their borrowing behavior”, but to the contrary, they find that simply disclosing how financing costs add up reduced demand by 11%. Melzer (2011) finds that payday loan access leads to increased difficulty paying mortgage, rent and utilities bills.

the pawnbroker keeps the piece *and* any payments already made. Before the contract expires, the client had the right to renew for another 3 months by going to the pawnshop, paying the accumulated interest, and signing a new contract with exactly the same terms and the same piece as the original contract (38% of borrowers renew at least once with a given pawn). This contract is standard in the industry. Pawnshops make money in three ways: by reselling the jewelry left as collateral on defaulted loans, by charging interest on non-defaulted loans, and by keeping the payments made on defaulted loans.

*Borrowers* The clients that pawned understood these terms well (as we verified in interviews).<sup>12</sup> These clients have little or no access to other types of loans and they value the convenience of pawn borrowing. This population of pawn borrowers is economically vulnerable: 30% of them could not pay either water, electricity & gas or rent in the past 6 months; 89% said they are pawning because of an emergency, and only 12% stated it was to use in a “non-urgent expense”. When asked why they are pawning this piece, 5% responded “lost a family member”, “a medical emergency” (11%), or “an urgent expense” (72%).

*Many borrowers lose their pawn.* Our context is also one with high borrower default: 43% of clients lose their pawn in a time span of 230 days from the date of pawning. One potential explanation for high default is that clients are really just knowingly selling their gold piece through a pawn contract on which they intend to default. This appears unlikely for several reasons: (a) clients can easily sell the gold and obtain a higher amount of instant cash at gold-buying stores located close to almost all our pawnshop branches, (b) the reported subjective value of the pawn is larger than the appraised value for 83% of clients, (c) among those that lose their pawn, 29% paid a positive amount towards its recovery and on average paid 42% of the value of their loan (see Figure A.1 in Appendix) — this can only be rationalized if they expected to recover their pawn, and (d) 72% of borrowers report a 100% probability of repaying their loan (and 98% at least a 50% chance of repaying) in our baseline survey at the time they take the loan.

### 2.3 Measuring Borrowers Financial Costs

Borrowers’ financial costs are composed of two main categories: the cost of losing their collateral, and the interest and fees incurred during the life of the loan. For each given loan we observe if the client lost her pawn ( $1(\text{Default}_i)$ ). If a loan has been rolled over and is still outstanding, we consider it to be non-defaulted. This approach is conservative in our context (biases treatment effects towards zero), as we show in detail in Section 4. In our data 13% of experimental loans are ongoing (i.e. censored) when the data period ends. Regarding interest, our administrative data classifies payments made in three types according to their payment allocation rules: payments to principal  $P^C$ , payments on generated interests  $P^I$ , and payments on penalty fees  $P^F$ . We observe each and every payment made under each category, its amount and date.

We define a borrower’s financial cost as the total monetary outflow—in cash or pawn value—from the borrower to the lender. This includes all payments the borrower made

<sup>12</sup>87% of clients report in our survey that they have pawned before.



toward interest and fees, but also the net difference between the appraised value of the pawn and the loan amount (Value-Loan) in the event of default. When there is no default the borrower gets her pawn back and there is no loss of value for the borrower. Payments towards capital are considered a cost only when the borrower defaults, as she does not get reimbursed for these. Note however that when she does not default payments to capital are not an actual outflow, as they sum up to the value of the loan the lender disbursed in the first place. The formula for financial cost for person  $i$  is thus as follows:

$$\text{Financial Cost}_i = \sum_t P_{it}^I + \sum_t P_{it}^F + 1(\text{Default}_i) \times \left( \text{Value}_i - \text{Loan}_i + \sum_t P_{it}^c \right)$$

where  $t$  indexes days, and  $1(\text{Default}_i)$  is an indicator function for defaulting. Because the period of the loan is only 90 days we do not apply discounting in calculating costs. In robustness checks reported below we show that our results are virtually unchanged when applying a wide range of time discounting factors.

We consider the above to be an accurate measure of financial cost in pesos. However, we also report results incorporating two non-financial costs: (i) using the subjective value of the pawn reported by the borrower in place of its appraised gold value, and (ii) adding a measure of travel expenses and the opportunity cost of time, as clients have to go to the branch in order to make payments.

As a second measure of cost we calculate the Annual Percentage Rate (APR) in order to express the cost as a percentage of the loan, per year, inclusive of default costs. The standard definition is given in the following formula:  $(\text{APR})_i = \left( 1 + \frac{\frac{\text{Financial Cost}_i}{\text{Loan}_i}}{\text{loan term}_i} \right)^{\text{loan term}_i} - 1$

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### 3. EXPERIMENTAL DESIGN

#### 3.1 *Treatment arms and randomization*

*The Commitment Contract* For the purpose of the experiment we designed a new contract that is identical to the status quo contract except that, informed by the design of micro-lending contracts, it enhances the regularity and salience of payments as a way to encourage repayment (Morduch, 1999, Bauer et al., 2012). It has the same interest rate (7% *per month*) which accumulates daily on outstanding debt, the same loan size/collateral ratio (70%), and the same loan term (90 days, and a grace period of 15 days). Borrowers' gold pawns are appraised in the same way by the same appraisers under both the new and status quo contracts. The commitment contract however requires the client to make regular monthly payments for the duration of the contract, with the principal and interest payments split evenly across the three months of the contract (day 30, 60 and 90 after loan disbursement). The importance of this monthly payment was made salient in the contract and payment receipts, and by the levying of a nominal fee (2% of minimum due) on individuals who fell behind in their payments. The fee was modest and intended to make the payment deadlines salient. As a benchmark, the transportation cost to visit the branch to make a payment is comparable to the fee, on average.

To elicit demand for the monthly payment contract, we include an arm that allows borrowers to opt into this contract if they choose. The existence of both a non-optional “forcing” arm, and a choice arm in our design is key to estimating a battery of treatment effects above an average treatment effect under fairly mild assumptions. We next describe the three experimental arms in more detail.

*Treatment Arms* Treatments were randomized at the branch-day level. Each day a computer randomly assigned which types of contracts were on offer that day in the branch, and the IT system would only offer these. We have 3 different experimental arms<sup>13</sup>

- 1. *Control arm*: consisted of branch-days offering the status quo contract described in Section 2, and only this contract.
- 2. *Forced Commitment arm*: consisted of branch-days requiring all borrowers to use the Commitment contract described above.
- 3. *Commitment Choice arm*: consisted of branch-days offering the client a choice between the Commitment contract, and the status quo contract.

We did not allocate an equal number of days across arms, since we were interested in having more power in some of them. The number of branch days allocated to each were 84 to control, 80 to forced commitment, and 93 to choice. See Figure 1 for a CONSORT-style diagram of the study design and recruitment.

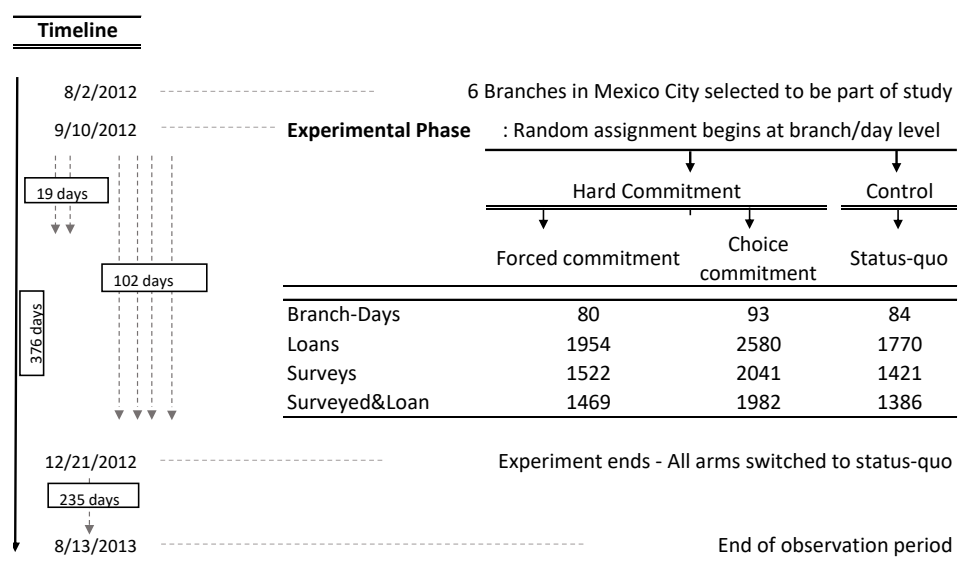


FIGURE 1. Experiment description

<sup>13</sup>The experiment included other independent arms that involved no fee penalties and did not emphasize the structure of payments. These are being analyzed in a separate paper.

*Randomization* We implemented the experiment in 6 branches of Lender *P* beginning on September 6, 2012. The branches were selected by Lender *P* to be dispersed across Mexico City and have varying sizes. In four of them the experiment ran for 102 days, and in 2 of them we ran it for a shorter time to economize on data collection costs once we realized we would not be constrained by sample size. Branches are more than 5 km apart from each other, and there is no substitution among them; none of the consumers appear in more than one of our branches.

Branch personnel did not know which treatment would be assigned to each day and were blind to the objective of the intervention. They were told that there were 3 different “types of contract-days”, that the system chose randomly for any given date, and that it could happen for instance that two or more consecutive dates had the same contract. They were also told that this way of operating was in place in several of Lender *P*’s branches (they did not know which ones), and that it would be in place for several months. Randomizing at the day level limits the problem of contamination arising from clients realizing that other clients get different contracts than theirs. It also limits potential manipulation by appraisers, who in the presence of individual-level randomization could potentially pick their preferred customer from the line or tell them to wait until their desired contract shows up on the screen. Intra-branch day correlation on the probability of default (ICC) is small, at 0.05, so we lose little power vis-a-vis individual-level randomization.

Some clients pawned more than one time during the duration of the experiment, with 14% pawning 2 times and 8% more than 2 times. To have a clean comparison we are considering only the first pawn conducted during the experimental window. It is also the case that 30% of those first pawns involve more than 1 loan, as 2 or more pieces of gold were submitted. We treat each of them as separate loans. In the appendix we show that our results are robust to this analysis choice.

*Timeline* Figure 1 shows the experimental timeline along with the length of time for which we observe payments. For loans made in the first week of the experiment, we observe up to 338 subsequent days of loan information; for loans made in the last week we observe up to 235 days. Figure 1 also illustrates the number branch-days per arm, the number of loans, and the number of surveys.

*Explaining the Contracts* We made sure clients understood the contract terms. First, we had full-time enumerators explaining contract terms to clients. The explanation took about 3-5 minutes and continued until the client said she understood the contract terms. Enumerators then asked clients to explain the contract back to them before correcting any misunderstandings. Second, the appraiser gave clients the “Contract Terms Summary” and read it out loud to them before after their piece had been appraised but before they signed the contract. We are confident the overwhelming majority of clients understood the contracts and that those in the choice arm made informed choices.

### 3.2 Data

*Administrative Data* The study exploits two types of data: administrative data from the lender, and a short survey that we implemented. The administrative data contains

a unique identifier for each client, an identifier for the piece she is pawning, and the transactions relating to that piece. These transactions include the value of the item as assessed by the appraiser, the amount of money loaned (70% of the item's value), the date of the pawn transaction, and the type of contract for that pawn: commitment or status quo. Within the period of the loan, we followed each transaction related to that piece in the administrative data: when payments were made and for what amounts, whether there was default (i.e. the client lost her pawn), and whether any late-payment fees were imposed. After the experimental loan, we are able to track subsequent behavior and to see whether that borrower took a subsequent loan. We have this information for all the pawns that occurred in the experiment's 6 branches between August 2, 2012 and August 13, 2013. This includes all the pawns that took place during our experiment along with those that one month before and eight months after our experiment. Figure 1 shows the design and timing of the experiment, along with the sample sizes in each arm. The experiment comprises 8,519 pawns while our administrative data covers a total of 26,180 pawns.

*Survey Data* An additional team of enumerators stationed in each branch asked clients to complete a 5-minute survey *before* going to the teller window to appraise their piece and before they learned which contracts would be available on that day. The survey was intentionally short to avoid discouraging the potential clients from pawning. It measured demographics, proxies for income/wealth, education, present-biased preferences, experience pawning, if family or friends commonly asked for money, how time-consuming and costly it was to come to the branch, the subjective probability of recovering the piece that they intended to pawn, the subjective value of their piece in money terms (how much money they would sell it for), among others. We surveyed 7,210 clients, and our survey response rate was 78% among clients who took loans. We only use the survey in Section 6 of the paper.

### 3.3 *Experimental Integrity*

*Attrition* There are two main channels through which attrition could complicate the interpretation of our results. The first, and more serious, is the possibility that clients might change their pawning decisions in response to the treatment they encounter in a given branch on the day they enter the branch. If this occurred it would introduce a self-selection dimension which would still reflect the overall impact of a treatment for the lender's portfolio but would no longer deliver *ceteris paribus* effects of treatments on individual borrowers. Narrative reports and the way the treatment was implemented make us believe that selection into treatment is unlikely.<sup>14</sup>

If the treatments had induced demand-side selection, we would expect to see that the number of pawns successfully conducted differ in a systematic way across arms.

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<sup>14</sup>Potential clients did not know that different days could have different contracts. If they asked, appraisers said that whatever was offered on that day was the only available contract for an undetermined length of time. Anecdotally, appraisers told us that they did not think refusals differed across arms, and our enumerators informed us that potential clients rarely left the branch without pawning. Lender P also never complained to us that our different treatments were hurting sales.

That is, if potential borrowers disliked being forced into a commitment contract, we would expect a lower number of pawns on branch days where only the commitment contract is available compared to control days. Table B.2 shows that this is not the case. There is no difference at all between the Control and Forced Commitment arm in terms of the number of pawns per branch-day. This is consistent with the findings of Table B.1.

A more subtle form of sample selection could arise if the treatments induce borrowers to re-pawn in different ways, especially given that their treatment status on subsequent loan/days may not be the same as that initially assigned. To address this issue our analysis uses only the first loan taken by each borrower during the experimental window.

*Balance* Table B.1 presents summary statistics for the sample of actual borrowers across arms, showing that our randomization succeeded in achieving balance across the experimental arms. Panel A uses administrative data for the universe of borrowers in each arm, and shows that loan balances and the days on the week on which individuals pawned are comparable across arms. The average loan size is \$2267 MXN (\$130 USD). Panel B of Table B.1 reports summary statistics across arms from our survey data. Among the 78% of borrowers who completed our survey, 73% of clients are women, the average age is 43 years, 66% have completed at least a high school education, and 87% have pawned before, suggesting that our sample largely consists of experienced borrowers. Finally, borrowers' subjective probability of recovering their pawn is close to 92% on average, in stark contrast to the *actual* recovery rate of 43%; borrowers are highly overconfident on average. The average subjective value they report for the items they pawn is 4084 MXN, much larger than the average appraised gold value of 3238 MXN. While this could arise either from overconfidence in valuation or from undervaluation by the lender, in any case it is *prima facie* evidence that loss of the pawn should be undesirable relative to the quantity of liquidity leveraged by the asset.

#### 4. AVERAGE TREATMENT EFFECTS

We begin by estimating average treatment effects of assignment to the Forced commitment and the Choice arms, relative to those assigned to the Control arm. As we explain below, only about 11% of those in the choice arm chose the monthly payment option (Figure C.1 shows coefficient plots for the characteristics that determine choosing commitment in the Choice arm).

*Specification* Table 1 presents estimates and standard errors from a standard pooled experimental regression

$$y_{ij} = \alpha + \beta^F T_i^F + \beta^C T_i^C + \gamma X_{ij} + \epsilon_{ij} \quad (1)$$

where  $i$  indexes client,  $j$  indexes branch,  $T_i^F$  and  $T_i^C$  are indicator variables for receiving the Forced or Choice arms,  $X_{ij}$  are branch and day-of-week fixed effects. Standard errors are clustered at the branch-day level, the unit of treatment assignment<sup>15</sup>. Given

<sup>15</sup>A minority of clients pawned on more than one day during the experiment: 14% pawned on two distinct days, and 8% on three or more days. To avoid contamination from earlier treatments to which these

TABLE 1. Effects on Financial Cost

	FC	Components of FC					APR
		Interest pymnt	Fee pymnt	Principal pymnt	Lost pawn value	Default	
		$\sum_t P_{it}^I$	$\sum_t P_{it}^F$	$\mathbf{1}(\text{Def}_i) \times \sum_t P_{it}^C$	$\mathbf{1}(\text{Def}_i) \times \text{Value-Loan}_i$	$\mathbf{1}(\text{Def}_i)$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Forced commitment	-204.0*** (48.1)	-157.3*** (34.9)	32.1*** (1.43)	-1.27 (3.10)	-78.8** (31.6)	-0.066*** (0.023)	-0.11*** (0.019)
Choice commitment	-38.9 (49.8)	-24.9 (38.4)	1.34** (0.54)	-0.93 (3.02)	-15.4 (33.1)	-0.023 (0.021)	-0.0086 (0.019)
Observations	6304	6304	6304	6304	6304	6304	6304
R-squared	0.013	0.022	0.151	0.003	0.007	0.013	0.031
Control Mean	942.4	545.9	0	5.96	396.5	0.44	0.57

*Note:* This table shows the treatment effects for our core pecuniary outcomes. Each column is a different regression for different outcomes on an indicator for the forced and choice arms, following specification in equation 1. Columns (1) & (7) analyze our core financial cost measures, while the rest of the columns decompose these into finer components. A few borrowers take more than one loan on the first day they appear in an experimental branch. These are treated as different observations. Additional results, available upon request, show that our results are robust to different ways of handling multiple loans for each borrower. Each regression includes branch and day-of-week FE. Standard errors are clustered at the branch-day level.

full compliance in the Forced arm, the coefficient  $\beta^F$  is the ATE of forced commitment while  $\beta^C$  is the ITT of commitment in the Choice arm on the outcome variable  $y_{ij}$ . Our two primary outcome variables are financial cost in pesos and Annual Percentage Rate (APR), as defined in Section 2.3. Results for these outcomes appear in columns (1) and (7) of Table 1. The remaining columns decompose the financial cost and APR outcomes into their components: interest payments (col 2), payment towards any fees incurred (col 3), payments toward the principal (col 4). Column 5 shows the value of lost pawn conditional on losing it. In column 6 the dependent variable is a dummy indicating default. Finally, column 7 rescales financial cost as a function of loan size to estimate causal effects on incurred APR.<sup>16</sup>

**Results** The results are stark. The Forced Commitment arm yields large and significant decreases in the cost of loans to clients, as measured either by financial cost or APR. Despite causing an increase in fees, the Forced arm leads to a decrease of 204 pesos in the costs of borrowing (out of a group mean of 942 in the status quo), equivalent to 22% reduction as a fraction of mean cost. These cost savings arise from a 6.6 percentage point (pp) decrease in the probability of default (out of a baseline mean of 44pp, implying cost savings of 79 pesos), and also from lower interest payments since, as we will document below, the commitment contract speeds up payments so the interest rate applies to a smaller principal. This translates into a large reduction in APR. A credit product that has an effective average APR of 57% in the status quo arm (inclusive of default) is reduced to a cost of 46% through the imposition of a more regularized payment structure. This is in stark contrast to Field and Pande (2008) for instance.

individuals were exposed, we restrict our sample to each client's *first visit*. Note that a client may pawn multiple items her first visit. We include these as separate observations. Because our standard errors are clustered at the branch-day level, they automatically account for any dependence in error terms arising from multiple pawns by the same client on her first visit.

<sup>16</sup>As we explained above, loans can be extended for an additional 3 months by paying the interest owed and restarting the loan under the same treatment conditions. This means that some loans extend for more than 3 months. We consider the entire flow of cost for the duration of our sample.



In contrast, to the impressive effectiveness of the Forced commitment arm, the Choice arm fails to deliver significant changes in any measure, with the exception of an increase in fees, for which we are highly powered, since this outcome is zero for every control observation. Giving borrowers the choice of contract did not decrease financial cost, whereas forcing them into a structured payment contract dramatically reduced it. As we explore later in the paper, the null effect of the Choice arm arises because few borrowers demanded commitment (consistent with [Ashraf et al. \(2006\)](#), [Giné et al. \(2010\)](#), [Bai et al. \(2020\)](#), [Royer et al. \(2015\)](#), [Sadoff et al. \(2019\)](#)), with 89% choosing the less effective status-quo contract.

*Intermediate Outcomes* To shed light on the mechanisms behind the ATEs discussed above, Table C.1 shows how commitment affects a number of intermediate outcomes. One can group the types of intermediate outcomes into two categories: measures of the speed of pawn recovery, and measures of the decision of when to default. While the first payment for borrowers in the status-quo contract occurs on average only on day 82 (on a 90 days contract), borrowers in the forced commitment arm start paying 13.8 days earlier on average (col 1). Not only do they start paying earlier, the first payment is also 7.9% larger (col 2), and a larger fraction of 9.7% actually pay in full and recover their pawn in the first payment, compared to 30% in the status quo contract (col 3). The resolution of the loan (either by payment or default) is shortened by 27.9 days (col 4), and conditional on recovery (an endogenous control) by 17.9 days (col 5).

A very undesirable outcome from the borrower's perspective is to pay towards the loan without paying in full, i.e. defaulting on the loan while still making some payments. In this case, they lose both the collateral and any payments made toward recovery. One could be concerned that by encouraging them to pay monthly, more borrowers might end up in this dire scenario in the Forced commitment contract. Column 6 shows this is not the case. On the contrary, 7 percentage points fewer borrowers end up in this situation, compared to 12 percent in the status quo contract. Conditional on defaulting those assigned to the Forced commitment contract have paid 4.1% less of their loan (col 7), and a 14 percent higher proportion of borrowers pay zero conditional on defaulting, an outcome analogous to "selling their pawn" (col 8). One interpretation of this bifurcation is that the Forced commitment contract forces borrowers to think earlier about whether they will indeed be able to eventually recover their pawn, and separates borrowers into those "selling" their pawn and those recovering it, reducing the share of undecided borrowers that end up paying interest and end up losing the pawn anyway. This mechanism may also help to explain why the Forced commitment contract does not increase the number of visits to the branch to pay (col 10): those recovering their pawn visit more, but those defaulting have 0.20 fewer visits (col 11). Finally, Column 9 shows that treatment effects are concentrated in the intensive margin as treatment does not affect the fraction of clients who pay a positive amount towards pawn recovery.

*Other Costs* We have shown that forcing borrowers to take the monthly payment contract significantly reduces their financial costs. Although the paper focuses on financial costs, we consider three additional costs here. First, we include the cost of visiting the branch to make a payment. This includes the self-reported transport cost (most use

TABLE 2. Effects on Repeat Pawning

	Ever pawns again (ITT)				
	After 90 days		Within 90 days	Different collateral	Cond. on rec
	(1)	(2)	(3)	(4)	(5)
Forced commitment	0.063 (0.043)	0.041*** (0.013)	0.024 (0.033)	0.042 (0.038)	0.11** (0.055)
Choice commitment	0.050 (0.036)	0.022* (0.011)	0.028 (0.031)	0.043 (0.033)	0.088* (0.045)
Observations	6302	6302	6302	6302	3032
R-squared	0.003	0.007	0.001	0.002	0.008
Control Mean	0.34	0.018	0.32	0.32	0.36

*Note:* This table estimates the specification of equation 1 but at the level of the borrower (not the loan). Each column represents a regression with a different outcome variable. Each regression includes branch and day-of-week FE. Standard errors are clustered at the branch-day level.

public transport), as well as the opportunity cost of time. To err on the conservative side, we subtract a whole day’s minimum wage the day of the visit, instead of just the wage corresponding to a couple of hours. Second, we consider a rough proxy of the value of liquidity that borrowers lose by paying sooner. To do this we add the interest costs on to the payments in the forced commitment arm and recompute treatment effects with these payments compounding daily (as if they had to borrow in order to make the more rapid payments). Thirdly, so far we have valued the collateral at the gold value appraised by the lender, but the piece may be worth more to the borrower than its gold value. For many of them the pawned jewelry has sentimental value. This is reflected in the subjective valuation they reported in the survey which is 87% higher on average. Our third extra cost considers this higher value. Table C.3 shows results are robust to all these changes.

*Repeat Pawns* Table 2 explores the effects of treatment on future pawning behavior. Column (1) shows that participants assigned to the forced arm are 6.3% more likely to be repeat clients later. While this appears to be *prima facie* evidence of greater satisfaction among borrowers in the forced arm, the interpretation is complicated by the fact that monthly payments may themselves trigger more borrowing to pay them. This is unlikely to be the case given that the effect on re-pawning comes after 90 days (during the period of contract demanded payments) and not before (see columns 2 and 3). Column 4 only considers new loans which use different collateral from that of the initial one. We do this to foreclose the explanation that those in the Forced arm, being more liquidity-constrained, return to pawn a second pawn to be able to pay the monthly payments of their first loan. However, we cannot reject a zero effect on pawning a different collateral. Column 5 focuses on the (endogenous) subsample of those recovering their pawn in both arms of the experiment. This means that *both* arms have recovered their pawn and could re-pawn if they so wish, and also that the liquidity demands from the monthly contract are *no longer* there as the contract has been closed. We find that the difference between the Forcing contract and the status quo is even larger in this subsample, with the former having 11pp higher likelihood of being a repeat client during our sample period.

*Censoring of Loan Completion* The window of time during which we were able to observe borrower behavior was limited in each branch, meaning that there were loans that we do not see completed (particularly those pawns that were rolled over for one or two further 90-day spells). Overall, 13% of all experimental loans are censored, meaning that they neither default nor repay within the observation window. In the prior analysis we handle this issue in a conservative way by using outcomes such as “did not default” which are well-defined even when we do not observe the completion of the loan, resulting in an estimate biased toward zero by the more rapid loan repayment observed in the Forcing arm. In the Online Appendix we consider this issue in more detail. Most importantly, Table C.2 conducts a bounding exercise that examines how large the effects of this censoring could possibly be by making bracketing assumptions about repayment on censored loans in the treatment and control, respectively. Comfortingly, Panel B of this table shows that even the most muted possible effect in the bounding exercise still recovers impacts of Forcing on financial costs and APR that are negative and significant at the 99% level. Using a lasso-logit model to predict the outcome of censored loans, Panel E shows the APR impact of Forcing increases from a 14 pp reduction (headline results) to a 17 pp reduction. Hence there appears to be no scope for this censoring issue to overturn our results, and our core results (implicitly assuming censored loans are paid off) is almost certainly an under-estimate of the true impacts.

## 5. CHOICE AND HETEROGENEOUS TREATMENT EFFECTS

The results from Section 4 show that commitment *works*: clients who were assigned to the forced commitment arm experienced substantially lower financial costs on average. In spite of this, given the opportunity, only 11% of borrowers chose commitment. If the effect of commitment were homogeneous, this would be enough to conclude that the 89% who did not choose it would have been financially better-off if they had. In a world of heterogeneous treatment effects, however, low demand for commitment could still be consistent with borrowers adhering to a standard model of rational choice. The borrowers who did not choose commitment could simply be those who don't need it. Indeed, we find strong evidence that the effect of forced commitment varies substantially across individuals in our experiment: we test and the null hypothesis of homogeneous treatment effects, following the methodology of Chernozhukov et al. (2018). (Details available upon request.) So the question remains: do the 89% who do not choose commitment know something about their personal situations that we as researchers do not, or are most people in the choice arm making a costly financial mistake? In this section we present a series of econometric exercises that sheds light on this question, leveraging unique features of our experimental design. Among other results, we show that commitment would lower average financial costs even for the subset of borrowers who choose *not* to commit voluntarily. To simplify the exposition in this and all sections that follow, we re-define all outcome variables so that *beneficial* treatment effects are *positive*. Using this convention, a positive treatment effect of commitment on financial cost, for example, reflects the average cost *savings* caused by commitment.

### 5.1 Bounding the Distribution of Individual Treatment Effects

Calculating the number of individuals who benefit from universal forced commitment requires the distribution of individual treatment effects. While this distribution cannot be point identified, it can be bounded. Let  $(Y_{i0}, Y_{i1})$  be  $i$ 's potential outcomes under the control and forced commitment conditions and define  $\Delta_i \equiv Y_{i0} - Y_{i1}$ . Let  $(F_0, F_1, F_\Delta)$  be the marginal distributions of  $(Y_{i0}, Y_{i1}, \Delta)$  and define

$$\underline{F}(\delta) \equiv \max \left\{ 0, \sup_y F_1(y) - F_0(y - \delta) \right\}, \quad \overline{F}(\delta) \equiv 1 + \min \left\{ 0, \inf_y F_1(y) - F_0(y - \delta) \right\}.$$

Since  $F_0$  and  $F_1$  are point identified under random assignment, so are  $\underline{F}$  and  $\overline{F}$ , and the pointwise sharp bounds for  $F_\Delta$  are  $\underline{F}(\delta) \leq F_\Delta(\delta) \leq \overline{F}(\delta)$  (Fan and Park, 2010). Figure E.1 plots these bounds for the APR outcome in our experiment. To bound the share of borrowers who benefit from forced commitment, we merely substitute  $\delta = 0$  into the preceding, since  $\mathbb{P}(\Delta_i > 0) = 1 - F_\Delta(0)$ . Our point estimates of  $\underline{F}(0)$  and  $\overline{F}(0)$  are 0.03 and 0.77 respectively, with 95% confidence intervals of [0.025, 0.050] and [0.75, 0.80]. Since we are interested in  $\mathbb{P}(\Delta_i > 0) = 1 - F_\Delta(0)$ , it follows that *at least* 23%, and at most 97%, of individuals borrowers benefit from forced commitment.<sup>17</sup> These bounds allow all possible joint distributions for  $(Y_{i0}, Y_{i1})$  that are compatible with the observed marginals  $F_0$  and  $F_1$ .

By adding assumptions it is possible to say more. For example, under rank invariance—i.e. if  $i$ 's rank in the distribution of  $Y_0$  equals her rank in the distribution of  $Y_1$ —the distribution of treatment effects is point identified and given by  $F_\Delta(\delta) = \int_0^1 \mathbb{1}\{F_1^{-1}(u) - F_0^{-1}(u) \leq \delta\} du$  where  $F_1^{-1}$  and  $F_0^{-1}$  are the quantile functions of  $Y_1$  and  $Y_0$ . Figure E.2 in Appendix E plots our estimates of  $F_\Delta$  under rank invariance. For the financial benefit outcome,  $Y_1$  first-order stochastically dominates  $Y_0$ . Since this implies that  $F_1^{-1}(u) - F_0^{-1}(u)$  is always positive, all borrowers have  $\Delta_i > 0$  for this outcome under rank invariance. The results are slightly less stark for the APR outcome: just under 90% of borrowers have  $\Delta_i > 0$  for this outcome under rank invariance.

### 5.2 Potential Outcomes and Exclusion

The assumption-free Fan and Park (2010) bounds from the preceding section show that more than 23% of borrowers would benefit from a policy of forced commitment. This suggests that some of the 89% of borrowers in the choice arm who did *not* choose commitment would have faced lower borrowing costs if they had. Making this intuition precise, however, requires a careful consideration of the relationship between choice and heterogeneous treatment effects. To this end, we now provide a full definition of the potential outcomes in our empirical setting, and introduce a pair of assumptions that will allow us to go beyond these bounds.

Let  $Z_i \in \{0, 1, 2\}$  denote the treatment arm to which to participant  $i$  was assigned:  $Z_i = 0$  denotes the forced no-commitment arm,  $Z_i = 1$  denotes the forced commitment

<sup>17</sup>Confidence intervals are constructed using the asymptotic distribution of  $(\underline{F}, \overline{F})$ . See Fan and Park (2010).

arm, and  $Z_i = 2$  denotes the choice arm. Now let  $D_i$  be the treatment that participant  $i$  actually *received*, where  $D_i = 0$  denotes no-commitment and  $D_i = 1$  denotes commitment. We assume perfect compliance in the  $Z_i = 0$  and  $Z_i = 1$  arms.<sup>18</sup> It is only in the  $Z_i = 2$  arm that participants are free to choose between alternative contracts. Let  $C_i \in \{0, 1\}$  denote a participant's "choice type." If  $C_i = 1$  then participant  $i$  *would choose commitment*, given the option; if  $C_i = 0$  she would not. As shorthand, we call borrowers with  $C_i = 1$  "choosers" and those with  $C_i = 0$  "non-choosers." Whereas a participant's choice type  $C_i$  is only observed if she is allocated to the choice arm ( $Z_i = 2$ ), her treatment  $D_i$  and experimental arm  $Z_i$  are always observed. Given the design of our experiment, these quantities are related by

$$D_i = \mathbb{1}(Z_i \neq 2)Z_i + \mathbb{1}(Z_i = 2)C_i. \quad (2)$$

We maintain the stable unit treatment value assumption (SUTVA) throughout. This means that borrower  $i$ 's outcomes depend only on her *own* values of  $Z_i$  and  $D_i$ , not those of any other person in the experiment. Under this assumption, a fully general model for the potential outcomes in our experiment would take the form  $Y_i(d, z)$  for  $d \in \{0, 1\}$  and  $z \in \{0, 1, 2\}$ , allowing participant  $i$ 's potential outcome to depend *both* on the treatment she actually receives,  $D_i$ , and the experimental arm to which she is assigned,  $Z_i$ . This model is too general, however, to point identify meaningful causal effects. For this reason, we consider two exclusion restrictions.

Before stating these restrictions, we first define some additional notation. Because our experimental design implies that any borrower with  $Z_i = 0$  has  $D_i = 0$ , we abbreviate the potential outcome  $Y_i(d = 0, z = 0)$  as  $Y_{i0}$ . Similarly, since any borrower with  $Z_i = 1$  has  $D_i = 1$ , we abbreviate  $Y_i(d = 1, z = 1)$  as  $Y_{i1}$ . This is in keeping with our notation from section 5.1 above. Using this notation, our first exclusion restriction is given by

$$Y_i(d = 0, z = 2) = Y_i(d = 0, z = 0) \equiv Y_{i0}. \quad (3)$$

Equation 3 only restricts the potential outcomes of non-choosers, individuals with  $C_i = 0$ , because they are the only borrowers for whom  $D_i = 0$  when  $Z_i = 2$ . In words, this condition assumes that every non-chooser experiences the same potential outcome regardless of whether she is assigned to the choice arm or the control arm. Similarly, our second exclusion restriction is given by

$$Y_i(d = 1, z = 2) = Y_i(d = 1, z = 1) \equiv Y_{i1}. \quad (4)$$

Equation 4 only restricts the potential outcomes of choosers, individuals with  $C_i = 1$ , because they are the only borrowers for whom  $D_i = 1$  when  $Z_i = 2$ . In words, this condition assumes that every chooser experiences the same potential outcome regardless of whether she is assigned to the treatment arm or the choice arm.

Mathematically (3) and (4) have the same structure as the standard LATE exclusion restriction that  $Y_i(d, z)$  depends only on  $d$ , not on  $z$ . Substantively, however, they are

<sup>18</sup>For more discussion on this point, see Section 3.3 above.

slightly different, given that there is no explicit reference to the “chosen” versus “forced” treatment distinction in the usual LATE setup. In essence, (3) and (4) assume that being assigned a particular treatment has the same result as choosing it for yourself, provided that you are assigned the same treatment that you *would have chosen*. If the mere fact of having been given a choice has a direct effect on outcomes, one or both of our exclusion restrictions will be violated. One can imagine situations in which this might be the case. For example, even someone who would have voluntarily chosen to undergo drug rehabilitation, given the choice, might respond differently when coerced. In our empirical setting, however, both (3) and (4) are plausible.<sup>19</sup> Moreover, each has testable implications that we fail to reject: see Appendix G. Under (3) and (4), the observed outcome  $Y_i$  is related to  $(Y_{i0}, Y_{i1})$  by

$$Y_i = \mathbb{1}(Z_i = 0)Y_{i0} + \mathbb{1}(Z_i = 1)Y_{i1} + \mathbb{1}(Z_i = 2)[(1 - C_i)Y_{i0} + C_iY_{i1}]. \quad (5)$$

Equation 5 is the key to understanding the results that follows. Random assignment of  $Z_i = 0$  and  $Z_i = 1$  identifies the marginal distributions of  $Y_{i0}$  and  $Y_{i1}$  for the population as a whole. Random assignment of  $Z_i = 2$  likewise point identifies the share of choosers ( $C_i = 1$ ), the distribution of  $Y_{i1}$  for choosers, and the distribution of  $Y_{i0}$  for non-choosers ( $C_i = 0$ ). Because  $Z_i$  is assigned independently of pre-treatment covariates  $X_i$ , we also identify the *conditional* distributions of  $Y_{i0}$  and  $Y_{i1}$  given  $X_i$ .

### 5.3 The “Controlled Choice” Design

As a direct consequence of (5), our experimental design—henceforth the “controlled choice design”—point identifies a number of interesting and economically-relevant causal quantities. First it identifies the treatment on the treated (TOT) and untreated (TUT) effects:

$$\text{TOT} \equiv \mathbb{E}(Y_{i1} - Y_{i0} | C_i = 1), \quad \text{TUT} \equiv \mathbb{E}(Y_{i1} - Y_{i0} | C_i = 0).$$

The TOT is the causal effect of commitment on borrowers who would voluntarily choose it, while the TUT is the causal effect on borrowers who would not. If the TUT is positive, then borrowers who did not choose commitment would have experienced better outcomes, *on average*, if they had. In a canonical Roy model, the TOT should exceed both the TUT and average treatment effect (ATE). If the TOT is statistically distinguishable from and larger than the TUT, this provides empirical support for the relevance of selection-on-gains in real-world decision-making. Because our design identifies all three quantities, it allows us to test this implication directly and to calculate the average selection on gains (ASG), namely the difference between the TOT and TUT effects:

$$\text{ASG} \equiv \mathbb{E}[Y_{i1} - Y_{i0} | C_i = 1] - \mathbb{E}[Y_{i1} - Y_{i0} | C_i = 0] = \text{TOT} - \text{TUT}.$$

<sup>19</sup>Closely related assumptions are common, if often tacit. Chamberlain (2011) explicitly assumes that choosing and being assigned a treatment have the same effect. A significant literature using compulsory schooling laws to estimate the returns to education tacitly assumes that schooling *in general* has the same returns. Similarly, fertilizer yields measured by development economists in experimental plots are tacitly assumed to generate the same returns regardless of whether the fertilizer was chosen by farmers or provided by the government.



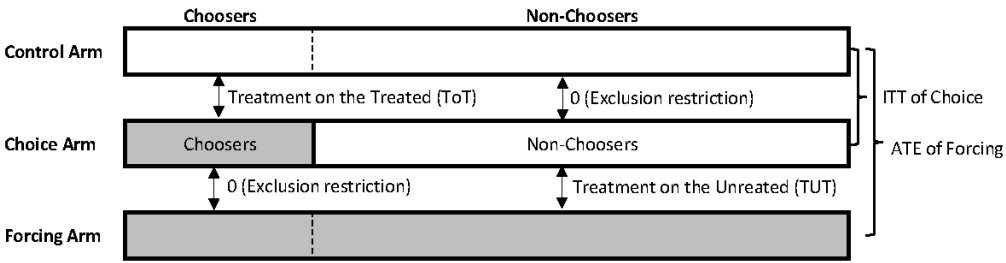


FIGURE 2. Graphical Intuition for the Controlled Choice Design. Gray rectangles denote borrowers with a commitment contract; white rectangles denote borrowers with a status quo contract. A comparison of means across control and forcing arms identifies the ATE of forcing commitment. The difference of mean outcomes across the choice and control “nets out” the non-choosers, and hence equals the TOT multiplied by the share of choosers. Similarly, the difference of means across the forcing and choice arms “nets out” the choosers and equals the TUT multiplied by the share of non-choosers. The share of choosers, illustrated using dashed vertical lines, is equal on average across arms under random assignment.

A number of recent papers compare estimates of the TOT and TUT to better understand who selects into treatment and why, e.g. [Cornelissen et al. \(2018\)](#) and [Walters \(2018\)](#). This line of work relies, at least to some extent, upon structural modeling assumptions to extrapolate from the reduced-form quantities that are identified by the data alone to more interesting, and economically relevant, causal parameters.<sup>20</sup> An alternative approach aims to avoid structural assumptions by calculating conditional local average treatment effects (LATE) given observed covariates  $X$  and re-weighting them according to the distribution of covariates in some population of interest to yield, for example, an average treatment effect ([Aronow and Carnegie, 2013](#), [Angrist and Fernandez-Val, 2013](#)). But there is no free lunch: this “LATE-and-reweight” approach relies upon assumptions of its own, most crucially the assumption that there is *no selection-on-gains* conditional on  $X$ , i.e. that the conditional LATE equals the conditional ATE. In contrast to both approaches, the controlled choice design uses exogenous experimental variation to point identify the ATE, TOT, and TUT without ruling out unobserved selection-on-gains or relying on additional structural modeling assumptions.

The key insight can be read directly from (2) and (5); Figure 2 provides graphical intuition. Viewing  $Z_i$  as an instrumental variable, the controlled choice design can be interpreted as a *pair* of RCTs, each subject to one-sided non-compliance. The first of these compares  $Z_i = 0$  to  $Z_i = 2$ . For each individual with  $Z_i = 0$  we have  $D_i = 0$  and observe  $Y_{i0}$ . For those with  $Z_i = 2$  we have  $D_i = C_i$  and observe  $(1 - C_i)Y_{i0} + C_iY_{i1}$ . This is identical to a “randomized encouragement” design in which treatment is only available to those who are encouraged:  $Z_i = 2$ . Under this interpretation, those with  $C_i =$

<sup>20</sup>While the marginal treatment effects (MTE) approach ([Heckman and Vytlacil, 2007](#)) can in principle be used to identify the TOT and TUT without parametric restrictions, doing so requires an instrumental variable  $Z$  with sufficiently rich support that the probability of treatment take-up given  $Z$  varies continuously between zero and one. In practice, instrumental variables are usually discrete and, even when continuous, typically have a more modest effect on take-up.

1 are “the compliers” and it follows by a standard argument (see Section 8) that

$$\frac{\mathbb{E}(Y_i|Z_i = 2) - \mathbb{E}(Y_i|Z_i = 0)}{\mathbb{E}(D_i|Z_i = 2) - \mathbb{E}(D_i|Z_i = 0)} = \frac{\mathbb{E}(Y_i|Z_i = 2) - \mathbb{E}(Y_i|Z_i = 0)}{\mathbb{E}(D_i|Z_i = 2)} = \text{TOT} \quad (6)$$

since  $\mathbb{E}(D_i|Z_i = 0) = 0$  by (2). A closely related argument can be used to construct a Wald estimand that identifies the TUT. Here we consider  $Z_i = 1$  to be the “encouragement” and compare the outcomes for these individuals to those with  $Z_i = 2$ . If  $Z_i = 1$  then  $D_i = 1$  and we observe  $Y_{i1}$ . If instead  $Z_i = 2$  then  $D_i = C_i$  and we observe  $(1 - C_i)Y_{i0} + C_iY_{i1}$ . Again, we can view this as an experiment with one-sided non-compliance, but now the situation is reversed. Everyone with  $Z_i = 1$  is treated, but some people with  $Z_i = 2$  are “always-takers” who obtain the treatment ( $D_i = 1$ ) despite having been allocated to the “control” arm  $Z_i = 2$ . Under this interpretation, the “compliers” are those with  $C_i = 0$ : when  $Z_i = 1$  they take the treatment, and when  $Z_i = 2$ , they do not. Thus,

$$\frac{\mathbb{E}(Y_i|Z_i = 1) - \mathbb{E}(Y_i|Z_i = 2)}{\mathbb{E}(D_i|Z_i = 1) - \mathbb{E}(D_i|Z_i = 2)} = \frac{\mathbb{E}(Y_i|Z_i = 1) - \mathbb{E}(Y_i|Z_i = 2)}{1 - \mathbb{E}(D_i|Z_i = 2)} = \text{TUT} \quad (7)$$

since  $\mathbb{E}(D_i|Z_i = 1) = 1$  by (2). Because they identify both the TOT and TUT, (6) and (7) also identify the average selection on gains:  $\text{ASG} = \text{TOT} - \text{TUT}$ .

The controlled choice design also identifies the average selection bias (ASB) and average selection on levels (ASL). In particular,

$$\text{ASB} \equiv \mathbb{E}(Y_{i0}|C_i = 1) - \mathbb{E}(Y_{i0}|C_i = 0) = \frac{\mathbb{E}(Y|Z = 0) - \mathbb{E}(Y|Z = 2, D = 0)}{\mathbb{E}(D|Z = 2)} \quad (8)$$

$$\text{ASL} \equiv \mathbb{E}(Y_{i1}|C_i = 1) - \mathbb{E}(Y_{i1}|C_i = 0) = \frac{\mathbb{E}(Y|Z = 2, D = 1) - \mathbb{E}(Y|Z = 1)}{1 - \mathbb{E}(D|Z = 2)} \quad (9)$$

as shown in Section 8. The ASB tells us whether borrowers who voluntarily choose commitment are those who are worse off, on average, under the status quo. Similarly, the ASL tells us whether borrowers who voluntarily choose commitment are those who are better off, on average, under the commitment contract.

Equations (6)–(9) are useful for understanding why the controlled choice design identifies the TOT, TUT, ASG, ASB, and ASL but they are less convenient for estimation and inference. Appendix H explains how to compute each of these quantities from a small number of just-identified, linear instrumental variables regressions, along with appropriate cluster-robust standard errors. These estimators and standard errors are implemented in our companion STATA package.

Table 3 calculates the causal quantities described above—TOT, TUT, ASG, ASB, and ASL—for our experimental data, along with robust standard errors for each. For purposes of comparison, the table also presents the ATE results from Section 4 above (row 1),<sup>21</sup> along with the corresponding average potential outcomes  $\mathbb{E}[Y_0]$  and  $\mathbb{E}[Y_1]$  (rows 4–5). The columns of the table correspond to different outcome variables defined above. For all four outcome definitions, the TUT effect is positive, statistically and economically significant, and comparable in magnitude to the ATE. In other words: commitment is

<sup>21</sup>Coefficients are not exactly the same since Table 4 includes branch and day-of-week FE.

*beneficial*, on average, to the people who *would not choose it*, and these benefits are large. Due to the low take-up rate of commitment in the choice arm, the corresponding TOT effects are imprecisely estimated. Only one of them, % (1-Default) from column (3), is statistically significant. This imprecision carries over into our estimates of the average selection on gains, TOT-TUT. Our point estimates are *negative* for all but the (1 - Default) outcome, but none is statistically distinguishable from zero. For the (1 - Default) outcome, we have sufficient precision to conclude that the average selection bias (ASB) is large and *negative*. This means that borrowers who choose commitment would have faced a *higher* probability of default under the status quo contract than borrowers who do not choose commitment. We may not want to read too much into TOT vs TUT comparisons as they are imprecise. But taken at face value, the result that TOT>TUT for default, while the opposite is true for financial cost, suggests that, while voluntary commitment has a slightly stronger effect on allowing borrowers to avoid default, forcing *reduces* payments in a strong enough manner as to overcome the default-benefits of choice.

TABLE 3. Treatment on the Treated (TOT), Treatment on the Untreated (TUT), Selection-on-gains (TOT - TUT), Average Selection Bias (ASB), and Average Selection Bias.

	APR % benefit	FC benefit	% (1-Default)	% (1-Refinance)
	(1)	(2)	(3)	(4)
ATE	9.41*** (2.06)	183.0*** (50.8)	7.74*** (2.50)	6.34** (2.90)
ToT	-0.59 (21.4)	111.9 (528.3)	37.4* (21.6)	-25.9 (29.1)
TuT	10.6*** (2.47)	191.5*** (50.8)	4.20* (2.41)	10.2*** (2.90)
E[Y <sub>1</sub> ]	-47.4*** (1.42)	-759.4*** (27.3)	64.2*** (1.69)	67.2*** (1.70)
E[Y <sub>0</sub> ]	-56.8*** (1.49)	-942.4*** (42.9)	56.4*** (1.84)	60.9*** (2.35)
ToT-TuT	-11.2 (22.9)	-79.6 (556.2)	33.2 (22.6)	-36.1 (30.6)
ASB	15.8 (22.3)	291.5 (551.2)	-39.1* (22.3)	22.7 (30.1)
ASL	4.58 (3.55)	211.9*** (59.5)	-5.90 (4.29)	-13.4*** (4.20)
Observations	6304	6304	6304	6304
H <sub>0</sub> : ATE-TuT=0	0.62	0.89	0.14	0.23
H <sub>0</sub> : ATE-ToT=0	0.63	0.89	0.14	0.24
H <sub>0</sub> : ToT-TuT=0	0.63	0.89	0.14	0.24
H <sub>0</sub> : ToT-TuT ≥ 0	0.69	0.56	0.071	0.88

*Note:* This table presents results computed using the derivations from Section 5.3. The APR and financial cost outcomes have been multiplied by  $-1$  so that a positive causal effect *benefits* the borrower in each of the four columns. The bottom panel present p-values for a number of null hypothesis tests of treatment effect heterogeneity.

Overall, Table 3 suggests that commitment works but that *not enough* people choose to commit: the commitment contract is beneficial on average even to those who would *not* choose it voluntarily. We believe this result is new in the household finance literature. It also illustrates how the “Controlled-Choice” design can be used to study more

generally whether low take-up of an intervention is problematic, based on the impacts among non-choosers.<sup>22</sup>

## 6. THE CASE FOR PATERNALISM

### 6.1 *Why does paternalism work in this context?*

A behavioral literature has highlighted voluntary commitment as an attractive way of allowing the “right” people to self-select. The argument for compulsory treatment, centers on the surprising result that  $TUT > 0$ . We now investigate four potential explanations for the positive TUT: the need to learn, time discounting, present bias, and overconfidence.

Our experiment introduced a new contract into an environment that had not previously featured commitment; perhaps clients required experience to understand its benefits. Are clients who experienced the commitment contract more likely to choose commitment subsequently than those assigned to the status-quo contract? To test this we look to the subset of 22% clients from our experimental sample who returned to pawn again on another day before the end of the experiment. We have already shown above that those who experienced the Forcing contract were more likely to borrow again. We now ask whether the subset who were assigned to the Choice arm when they returned were more likely to choose commitment. We do this in Appendix Table D.1.<sup>23</sup> We find no statistically discernible difference in commitment take-up rates for those assigned to forced commitment versus those assigned to status quo control. The implication is that while those who have experienced commitment feel more positively towards the pawn contract, the experience does not lead them to conclude that they need commitment on the subsequent loan. While these exercises cannot completely exclude the possibility that learning plays a role, they provide no indication that the lack of voluntary compliance is simply a matter of inexperience with commitment.

Highly impatient individuals might rationally choose the *status quo* contract, despite the benefits that commitment yields returns, since monthly payments are front-loaded while pawn recovery is back-loaded, even if by only a few days. To investigate this explanation we calculate the net present value (NPV) of the financial cost TUT effect under different hypothetical discount rates, given the actual timing of repayment and pawn recovery. Figure D.1 presents the results of this exercise. The solid line gives the TUT effect adjusted for a specified annual discount rate, while the shaded regions give the associated 95% & 90% confidence interval. We see that non-choosers continue to experience significant decreases in NPV financial costs up to annual discount rates of 1,000%, and the NPV remains positive, although not significant, at 5,000% discount rates. As such,

<sup>22</sup>For instance, in the debate on financial commitment take-up, some papers argue it is low (Ashraf et al. (2006), Giné et al. (2010), Bai et al. (2020), Royer et al. (2015)) while others argue it is high (Kaur et al. (2015), Casaburi and Macchiavello (2019), Schilbach (2019), Tarozzi et al. (2009), Dupas and Robinson (2013)), but none estimates the benefits for non-takers. In a different domain, doctors claim that medical treatment abandonment is too high in a broad range of diseases (McDonald et al., 2002), without knowing the treatment benefits for those that abandon.

<sup>23</sup>Table D.1 presents information about participants' *immediate subsequent* pawning behavior. For borrowers who returned to pawn again more than once, this analysis considers only their first repeat pawn.

discounting is unlikely to explain why those who benefit, on average, from commitment fail to choose it.

If the benefits of commitment among non-choosers cannot be explained by standard models of rational choice, the canonical behavioral story would center on time inconsistency. While commitment is useful to anyone with hyperbolic time preferences, only those who are sophisticated—i.e. aware that they are hyperbolic discounters—will demand it. A large share of “naïve” hyperbolics in the population—borrowers who are unaware that they are hyperbolic discounters—could therefore drive a large and positive TUT. Our baseline survey included standard questions about discount rates between today and a month in the future versus discount rates between three and four months in the future. This allows us to classify borrowers who display more impatience over immediate delays as present biased. This measure of financial hyperbolicity is widely used in survey research, although it is not without problems.<sup>24</sup>

If we could perfectly measure present bias and sophistication, we could divide the sample into three groups: sophisticated hyperbolics (who chose commitment), time-consistent non-choosers (for whom forcing will not be effective), and naïve hyperbolic non-choosers (who will benefit from forced commitment). If present bias fully explains the low take-up rate of voluntary commitment, we should find that the TUT for present-biased borrowers is positive. This is because among the group of non-takers, a comparison of present-biased borrowers against everyone else is a comparison of naïve hyperbolics against time-consistent non-choosers. The left panel of Figure D.2 in the Appendix carries out a feasible version of this exercise using our survey measure of present bias. We find no indication that present-bias explains our positive estimated TUT.

While 72% of survey respondents believe they have a 100% chance of recovering their pawn, in reality only 43% will go on to do so. This suggests a borrower pool characterized by over-optimism. Incorrect expectations about recovery probabilities could explain low take-up if individuals who *believe* that they are certain to repay choose, rationally given their incorrect beliefs, to forgo the costs associated with commitment that are designed to induce repayment. We now explore whether over-optimistic expectations of recovery probability can explain our positive overall TUT estimate. To do this, we carry out an analysis that is analogous to our present bias exercise from the preceding paragraph, comparing the overall TUT estimate to estimates computed for two sub-groups. Here, however, the groups are defined by a binary variable that we call “sure-confidence.” This measure equals one for any individuals who say at the time of borrowing that they have a 100% probability of recovering their pawn, zero otherwise. In contrast to our results for present bias, we find (Figure D.2) that the TUT is almost entirely confined to the sure confident individuals, with the effect among those saying they have some chance of defaulting at baseline being very close to zero.

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<sup>24</sup>Our measure is dichotomous, and it is not incentivized. Recent empirical work has shown the superiority of more elaborate measures such as “convex time budgets” (Andreoni et al., 2015) while questioning the interpretation of measures of hyperbolicity that are not based on consumption (Andreoni and Sprenger, 2012, Cohen et al., 2020), suggesting that real effort tasks provide a better measure (Augenblick et al., 2015). Given that we had only a few minutes to interview real pawnshop clients prior to a commercial transaction, our simple measure was a necessary compromise.

As discussed above, our measure of hyperbolicity is based on un-incentivized responses in a short survey and so is likely to be noisy; nonetheless we see no evidence here that it drives the forcing effect. Rather, it seems that the effectiveness of paternalism in our experiment may be driven by *overconfident* borrowers who, heedless of the risk of default, fail to choose commitment despite benefiting substantially when they are forced to commit.<sup>25</sup>

## 6.2 Analyzing Choice versus Paternalism Using Causal Forests

On average, the commitment contract benefits both those who would choose it and those who would not. In Section 5.1, we briefly went *beyond* average effects by presenting bounds on the distribution of individual treatment effects. Because they made no assumptions beyond random assignment, these bounds were relatively wide. Adding the assumption of rank invariance yielded a distribution of treatment effects in approximately 90% of borrowers had positive individual treatment effects for the APR outcome, implying that *practically everyone* would benefit from treatment and hence that there would be hardly any no losers from paternalism. Rank invariance, however, is an extremely strong assumption.<sup>26</sup> In this section, we explore a middle way between the two extremes, using a causal forest analysis to consider conditional average treatment effects and conditional TOT and TUT effects.<sup>27</sup> This exercise provides more fine-grained information about treatment effect heterogeneity. Among other things, it will potentially allow us to identify groups of borrowers who are on average *harmed* by commitment. Under the stronger assumption that our observed survey measures capture the main sources of treatment effect heterogeneity, this exercise will allow us to approximate individual-level counterfactuals, to consider whether particular borrowers made “mistakes” in their choice of contract. To estimate the conditional average treatment effects we use the “generalized random forest” methods of [Athey et al. \(2019\)](#) (see Appendix F for details).

Figure E1 plots densities of the estimated conditional ATE, TOT, and TUT effects from the generalized random forest models described above. In each case, the outcome variable is APR benefit, i.e. the reduction in APR from a commitment contract. As we see from the figure, the conditional average effects are overwhelmingly positive. The TUT density is particularly interesting for the question of paternalism since, as emphasized above, it presents conditional average effects for borrowers who would not voluntarily

<sup>25</sup>In Figure D.3 we plot the coefficient estimates from a regression that predicts sure confidence with a battery of individual-level characteristics. Older males are more likely to be sure-confident, as are those with more education. Taken at face value, the sure-confident also report less financial stress, less trouble paying bills, and to be more frequently relied upon financially by family members. Viewed through a behavioral lens, however, it is also possible that the type of person who is over-confident in their ability to repay a loan also exaggerate their degree of economic security in their response to survey questions. In any case, it appears that sure confidence may be difficult to predict with easily-observed and objective demographic criteria, a point to which we return below.

<sup>26</sup>For the financial cost outcome, rank invariance implies that all individual treatment effects are positive.

<sup>27</sup>Note that this approach imposes our exclusion restriction from 5.3 conditional on observed covariates: administrative data and survey responses.



choose commitment. Only 7% of our estimated conditional TUTs are negative, with a 95% confidence interval of [4%, 9%].<sup>28</sup> To be clear, this is a probability statement about conditional average effects over the distribution of *covariates*. In particular, we estimate that  $\int \mathbb{1}\{\mathbb{E}[Y_1 - Y_0|X = \mathbf{x}, C = 0]\} f(\mathbf{x}|C = 0) d\mathbf{x}$  is approximately 0.07.<sup>29</sup> Figure E1 strengthens our argument, introduced in Section 5, that not enough borrowers choose commitment.

Under the assumption that our instrumental forest estimates capture the main sources of treatment effect heterogeneity, we can use them to assess whether particular borrowers in the choice arm made “mistakes” in their decision to accept or refuse the commitment contract, in terms of predicted financial costs of the loan. To do this we use the same information that is depicted in Figure E1, but present it in a different way. For each non-chooser in the choice arm, we use our instrumental forest from above to estimate the conditional TUT effect, given her observed covariates. Of course conditional average effects need not equal individual treatment effects, and our APR outcome may not capture all of the costs and benefits that are relevant for individual borrowers’ decisions. To account for this, we define a “mistake” for a non-chooser to be a conditional TUT estimate that significantly exceeds some large and positive APR threshold, e.g. 10%. At any such threshold, we can calculate the percentage of non-choosers in the choice arm who have benefited by more than that threshold from having chosen commitment.

The results of this exercise can be read off from the green curve in Figure 3. Defining  $F_{\text{TUT}}(\delta)$  to be the CDF corresponding to the density of conditional TUT estimates from Figure E1, the green curve in Figure 3 is merely  $[1 - F_{\text{TUT}}(\delta)] \times 100\%$ . In other words, the green curve gives the percentage of non-choosers who made a “mistake” when mistakes are defined increasing APR by a given threshold. The green shaded region gives associated 95% pointwise confidence bounds.

For choosers we follow an analogous approach, defining a “mistake” as a *negative* conditional TOT effect that exceeds a particular APR threshold. The results for choosers can be read from the red curve in Figure 3. If  $F_{\text{TOT}}(\delta)$  denotes the CDF corresponding to the density of conditional TOT estimates from Figure E1, then the red curve is merely  $F_{\text{TOT}}(-\delta) \times 100\%$ . In other words, the red curve gives the percentage of choosers who made a “mistake” when mistakes are defined at a particular APR threshold. The red shaded region gives associated 95% pointwise confidence bounds. Note that we use a *positive* APR threshold to denote a mistake for both choosers and non-choosers. This ensures that bigger mistakes are always to the *right* of smaller mistakes for both the green and red curves. The blue curve in Figure 3 gives the *overall* percentage of borrowers in the choice arm who made a “mistake” at a particular APR threshold. This total is

<sup>28</sup>The generalized random forest approach of Athey et al. (2019) produces conditional average effect estimators that are asymptotically normal, and includes methods for computing correct standard errors. Our inferences in this section are carried out by “bootstrapping the limit experiment,” i.e. simulating from the normal limit distributions using the estimated standard errors.

<sup>29</sup>The share of non-choosers with negative conditional *average* treatment effects need not equal the share with a negative *individual* effects, i.e.  $\mathbb{P}(Y_1 < Y_0|C = 0)$ . But the more treatment effect heterogeneity that  $X_i$  explains, the closer these two values become.

computed by taking a weighted average of the green (non-choosers) and red (choosers) curves, with weights equal to their shares in the choice arm.<sup>30</sup>

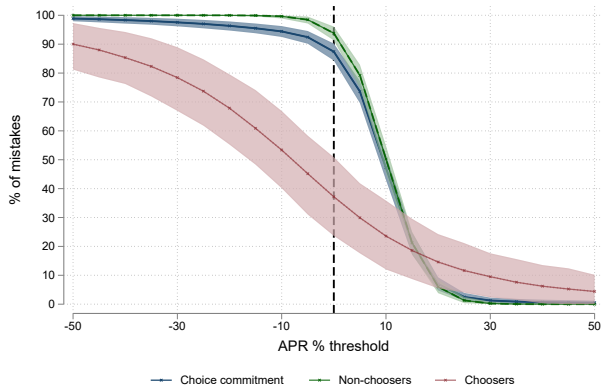


FIGURE 3. “Mistakes” in the choice arm. This figure presents conditional average TUT and TOT effects for the APR outcome from Figure E1 in an alternative manner, to consider the fraction of borrowers in the choice arm who made “mistakes” in their decision to accept or refuse the commitment contract. A “mistake” for a non-chooser is defined as a positive conditional TUT effect that significantly exceeds a specified threshold APR value.

The results in Figure 3 suggest that a large fraction of non-choosers made mistakes by not choosing commitment. Even at an APR threshold as large as 10%, we estimate that more than half of them should have chosen commitment in order to lower financial costs. In contrast, relatively few choosers appear to have made mistakes by choosing commitment. This now allows us to make a stronger statement in favor of paternalism in our context; not only does forced commitment generate large benefits on average, but it also benefits the vast majority of borrowers *who would be coerced* under a policy of forced commitment.

6.3 Can we target paternalism?

Is it possible to target paternalism only towards those who would benefit from it? A financial firm engaging in this type of targeting under real-world circumstances would be unable to use the subjective and unverifiable questions from the survey, and would not use the commitment choice since it will be unattractive for a company to ask consumer preferences and then force a product on those who had refused it. This leaves us with only a few objective covariates that could be used to target: age, gender, high school education or above, desired loan size, and whether that individual has ever pawned before. We call these the “narrow” covariate set, to contrast with the full set of survey variables, which we call the “wide” covariate set. We take the estimated conditional average

<sup>30</sup>The blue curve is very similar to the green curve because 89% of borrowers in the choice arm are non-choosers.

TABLE 4. Type I & II errors using targeting narrow rules

Rule	% incorrectly assigned to control	% incorrectly assigned to treatment	Overall Error Rate
All to control	90.22	0	90.22
All to forcing	0	9.78	9.78
Optimal	0	0	0
Narrow rule (RF)	4.38	5.21	9.59
Narrow rule (Logit)	6.9	7.76	14.66
Allow choice	93.81	37.18	87.4

*Note:* This table reports error rates for six different rules for allocating individuals to commitment. Row 1 assigns all borrowers to control, Row 2 all to the Forcing arm. Row 3 uses 'optimal targeting according to the CATE from the wide covariate set. Row 4 uses a random forest classification, and Row 5 a logit model, both with only the narrow covariate set. Row 6 uses the choices made by borrowers to assign to commitment.

treatment effects (CATEs)  $\widehat{ATE}(X_i)$  using the wide covariates as our ground truth, and now ask how well we can predict this benefit using the narrow covariate set. Figure F2 shows the relationship between these two different CATEs. We generate substantially less heterogeneity when using the narrow covariate set, although we still reject the null hypothesis of no treatment effect heterogeneity with the test from Chernozhukov et al. (2018). To investigate targeting, we can assign a dummy variable equal to one for the 90% of borrowers who have a positive CATE using the wide covariate set, and then implement both a logistic regression and a random forest classification model using only the narrow covariate set to predict positive benefits at the individual level.

We compare the in-sample performance of targeting rules against a policy of universal forced commitment. Table 4 shows error rates for six possible assignment rules: assigning all borrowers to control, all to treatment (Forcing), the optimal (infeasible) assignment, narrow RF targeting, Logit targeting, and the actual choice made by borrowers. All models taking the wide RF as the ground truth. While the narrow RF correctly assigns roughly half of those who do not benefit from commitment to control, it also incorrectly allocates 4.38% of the sample that would have gained from treatment to control. As such, it only improves the overall correct targeting rate by about half of a percentage point relative to universal Forcing. The Logit assignment rule is less successful at predicting benefits and harms, with a higher share of borrowers incorrectly assigned to both treatment and control, meaning that the overall correct targeting rate for the Logit is 5 pp lower than universal Forcing. Self-targeting through choice proves to be little better than assigning everyone to the control condition, given the low take-up rate and the presence of both Type I and Type II errors in the choice arm. The takeaway is that given low take-up, the large fraction of the sample benefiting from commitment, and the weak predictive power of the narrow covariates, in this case universal paternalism assigning all borrowers to the commitment contract appears to be an attractive targeting method.

7. CONCLUSION

This paper makes several contributions. First, it analyzes the large but understudied industry of pawn loans, and shows that a simple change to contract terms results in substantial financial savings for pawn borrowers: forced commitment lowers the APR from 57% to 46%, and reduces the fraction of borrowers who default by 6.6 pp, or 15%. That this new contract generated large benefits for borrowers and yet was not offered,

and that a contract that generated default was the industry standard instead, is related to the idea of “veiled paternalism” Laibson (2018), put on its head. In “veiled paternalism” principals embed forms of commitment into their products but mask this fact from consumers who may need but do not desire commitment, in pawn lending, over-collateralization means that lenders stand to make more money from defaulting borrowers, generating incentives for “veiled *non*-paternalism,” embedding features that lead to high borrower costs in non-obvious ways.

Second, our novel “controlled choice” design allows us to go beyond ATE results and draw an important set of conclusions about the relationship between take-up and heterogeneous treatment effects. In particular, we simultaneously point-identify the impact of commitment on those who would naturally choose it *and* those who only experience commitment when forced. Estimating this later quantity is critical in thinking about paternalism. We find substantial benefits of treatment for non-choosers and no evidence of selection on gains by borrowers who choose commitment. Given that the rate of voluntary commitment in our sample is only 11%, in order to achieve widespread benefits in this context compulsory commitment appears to be necessary.

Why do borrowers leave such substantial returns on the table? Our results suggest that over-optimism is the characteristic most strongly associated with benefiting from the commitment despite not having chosen it. Our borrower pool overestimates their probability of repayment by more than 50%, and our positive TUT estimate is largely confined to borrowers who incorrectly believe that they have little chance of defaulting.

Using machine learning methods, we find the benefits of commitment close to universal. The benefits of targeting commitment based on characteristics that lenders can observe and participants would truthfully reveal are extremely limited, suggesting that universal commitment is an attractive policy in our empirical setting.

Where lenders have no incentive to engage in veiled paternalism and customers display inefficiently low demand for it, financial policy regulation may prove an attractive option. Pawnshops, along with other over-collateralized credit products such as payday lending, exist in an environment where the lender may desire customers to lose their collateral on the loan, hurting especially low-income populations who are its main users. With a now well-established toolkit of regular small payments and incentives delivering small default rates in microfinance, regulators may fruitfully investigate the possibility of requiring pawnbrokers to embed features of commitment and regularity into their repayment structures in more consistent ways.<sup>31</sup> An important question for future research will be the extent to which borrowers are able to learn about the benefits of commitment over time, making it so that temporary, lighter-touch policies could achieve lasting benefits for borrowers. Pawning with commitment may provide an important mechanism to preserve flexible credit access while allowing more poor borrowers to retain their assets.

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<sup>31</sup>If employed at scale in a competitive lending sector this would redistribute welfare from those who would have repaid (whose interest rates must now rise to cover lower returns from collateral seizure) towards those who would only repay in the presence of commitment. In a setting of lender market power however, redistribution from lenders to borrowers could occur.

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## 8. PROOFS

This section gives a formal derivation of the identification results presented in Equations (6)–(9) of subsection 5.3. To simplify the presentation, we omit  $i$  subscripts throughout.

ASSUMPTION 1 (Randomized Choice Design and Exclusion Restriction).

- (i)  $Z$  is independent of  $(Y_0, Y_1, C)$
- (ii)  $D = \mathbb{1}(Z \neq 2)Z + \mathbb{1}(Z = 2)C$
- (iii)  $Y = \mathbb{1}(Z = 0)Y_0 + \mathbb{1}(Z = 1)Y_1 + \mathbb{1}(Z = 2)[(1 - C)Y_0 + CY_1]$

LEMMA 1. Under Assumption 1,

- (i)  $\mathbb{E}(D|Z = 2) = \mathbb{P}(C = 1)$
- (ii)  $\mathbb{E}(Y|Z = 0) = \mathbb{E}(Y_0)$
- (iii)  $\mathbb{E}(Y|Z = 1) = \mathbb{E}(Y_1)$
- (iv)  $\mathbb{E}(Y|D = 0, Z = 2) = \mathbb{E}(Y_0|C = 0)$
- (v)  $\mathbb{E}(Y|D = 1, Z = 2) = \mathbb{E}(Y_1|C = 1)$ .

PROOF. Part (i) follows because  $Z = 2$  implies  $D = C$  and  $Z$  is independent of  $C$ . Parts (ii) and (iii) follow similarly: given  $Z = 0$  we have  $Y = Y_0$ , given  $Z = 1$  we have  $Y = Y_1$ , and  $Z$  is independent of  $(Y_0, Y_1)$ . For parts (iv) and (v), first note that Assumption 1 (iii) implies that  $Z$  is conditionally independent of  $(Y_0, Y_1)$  given  $C$ . Now,  $Z = 2$  implies that  $D = 0$  if and only if  $C = 0$ . Hence,  $\mathbb{E}(Y|D = 0, Z = 2) = \mathbb{E}(Y_0|C = 0)$  establishing part (iv). For part (v)  $Z = 2$  implies that  $D = 1$  if and only if  $C = 1$  from which it follows that  $\mathbb{E}(Y|D = 1, Z = 2) = \mathbb{E}(Y_1|C = 1)$ .  $\square$

PROPOSITION 1. *Under Assumption 1,*

$$\begin{aligned}
 (i) \quad TOT &\equiv \mathbb{E}(Y_1 - Y_0|C = 1) = \frac{\mathbb{E}(Y|Z = 2) - \mathbb{E}(Y|Z = 0)}{\mathbb{E}(D|Z = 2)} \\
 (ii) \quad TUT &\equiv \mathbb{E}(Y_1 - Y_0|C = 0) = \frac{\mathbb{E}(Y|Z = 1) - \mathbb{E}(Y|Z = 2)}{1 - \mathbb{E}(D|Z = 2)} \\
 (iii) \quad ASB &\equiv \mathbb{E}(Y_0|C = 1) - \mathbb{E}(Y_0|C = 0) = \frac{\mathbb{E}(Y|Z = 0) - \mathbb{E}(Y|Z = 2, D = 0)}{\mathbb{E}(D|Z = 2)} \\
 (iv) \quad ASL &\equiv \mathbb{E}(Y_1|C = 1) - \mathbb{E}(Y_1|C = 0) = \frac{\mathbb{E}(Y|Z = 2, D = 1) - \mathbb{E}(Y|Z = 1)}{1 - \mathbb{E}(D|Z = 2)}.
 \end{aligned}$$

PROOF. Parts (i) and (iii) we require an expression for  $\mathbb{E}(Y_0|C = 1)$  in terms of  $(Y, D, Z)$ . By Lemma 1(ii) and iterated expectations

$$\mathbb{E}(Y|Z = 0) = \mathbb{E}(Y_0) = \mathbb{E}(Y_0|C = 0)\mathbb{P}(C = 0) + \mathbb{E}(Y_0|C = 1)\mathbb{P}(C = 1).$$

Re-arranging and substituting Lemma 1(i) and (iv),

$$\mathbb{E}(Y_0|C = 1) = \frac{\mathbb{E}(Y|Z = 0) - \mathbb{E}(Y|Z = 2, D = 0)\mathbb{E}(1 - D|Z = 2)}{\mathbb{E}(D|Z = 2)}. \quad (10)$$

Part (i) follows by combining (10) with Lemma 1(v) and simplifying; part (iii) follows by combining (10) with Lemma 1(iv) and simplifying. Similarly, for parts (ii) and (iv) we require an expression for  $\mathbb{E}(Y_1|C = 0)$  in terms of observables. By Lemma 1(iii) and iterated expectations,

$$\mathbb{E}(Y|Z = 1) = \mathbb{E}(Y_1) = \mathbb{E}(Y_1|C = 0)\mathbb{P}(C = 0) + \mathbb{E}(Y_1|C = 1)\mathbb{P}(C = 1).$$

Re-arranging and substituting Lemma 1(i) and (v),

$$\mathbb{E}(Y_1|C = 0) = \frac{\mathbb{E}(Y|Z = 1) - \mathbb{E}(Y|Z = 2, D = 1)\mathbb{E}(D|Z = 2)}{\mathbb{E}(1 - D|Z = 2)}. \quad (11)$$

Part (ii) follows by combining (11) with Lemma 1(iv) and simplifying; part (iv) follows by combining (11) with Lemma 1(v) and simplifying.  $\square$

## **ONLINE APPENDIX: The controlled choice design and private paternalism in pawnshop borrowing**

CRAIG MCINTOSH

Department of Economics University of California San Diego

ISAAC MEZA

Department of Economics Harvard University

JOYCE SADKA

Department of Economics Instituto Tecnológico Autónomo de México

ENRIQUE SEIRA

Department of Economics Michigan State University

FRANCIS J. DITRAGLIA

Department of Economics University of Oxford

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Craig McIntosh: [ctmcintosh@ucsd.edu](mailto:ctmcintosh@ucsd.edu)

Isaac Meza: [isaacmezalopez@g.harvard.edu](mailto:isaacmezalopez@g.harvard.edu)

Joyce Sadka: [jsadka@itam.mx](mailto:jsadka@itam.mx)

Enrique Seira: [enrique.seira@gmail.com](mailto:enrique.seira@gmail.com)

Francis J. DiTraglia: [francis.ditraglia@economics.ox.ac.uk](mailto:francis.ditraglia@economics.ox.ac.uk)

APPENDIX A: ADDITIONAL MATERIALS

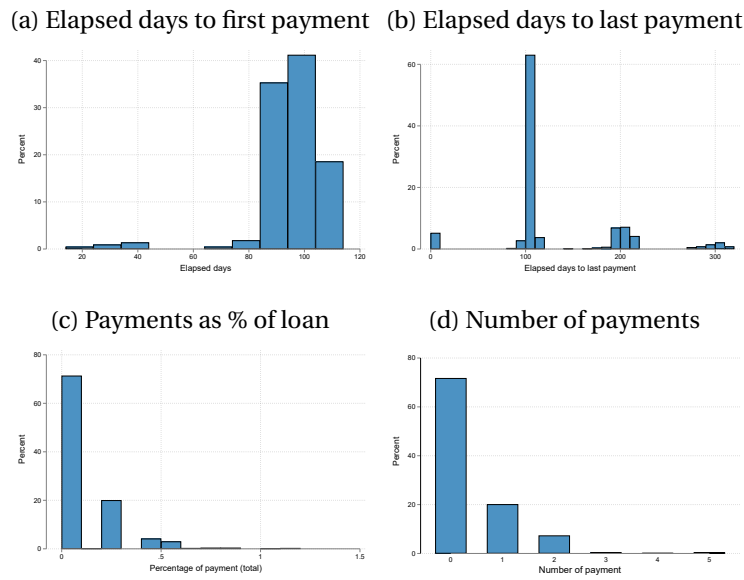


FIGURE A.1. Behavior of borrowers who lost their pawn. This figure provides more details on the behavior of clients who were assigned to the control group and did not recover their pawn. Panel (a) shows a histogram of days elapsed from the pawn to the first payment, while panel (b) displays a histogram of days elapsed until the last payment. Some borrowers make payments after day 105, the end of the grace period: if they pay all interest owed, they can “restart” the loan. This amounts to starting a new loan with the same conditions and same pawn. Panel (c) shows a histogram of the fraction of the loan paid, while panel (d) presents a barplot of the number of times that borrowers went to the branch to make payments.

APPENDIX B: INTERNAL VALIDITY

If potential borrowers disliked being forced into a commitment contract, we would expect a lower number of pawns on branch days where only the commitment contract is available compared to control days. Table B.2 shows that this is not the case. There is no difference at all between the Control and Forced Commitment arm in terms of the number of pawns per branch-day. Although we cannot reject equality across the three arms ( $p$ -value=0.21), the Choice arm appears to have a somewhat larger number of borrowers than the Control and Forced arms.<sup>1</sup> This seems to be due to sampling variability. Differences across arms are smaller when for medians, or when we compare the number of borrowers pawning (some borrowers pawn more than one piece).

The bottom panel of Table B.2 shows balance in a more focused way, given that the surveys were conducted prior to the revelation of treatment status. We find that in no

<sup>1</sup> $p$ -value=0.23 and 0.12 respectively.

TABLE B.1. Summary statistics and Balance

	Control	Commitment arms		
		Forced	Choice	p-value
Panel A : Administrative Data				
Loan amount	2267 (76)	2162 (83)	2223 (66)	0.65
Weekday	0.88 (0.044)	0.89 (0.035)	0.83 (0.048)	0.56
Obs	1770	1954	2580	
Panel B : Survey Data				
Subjective value	4084 (186)	3877 (193)	4173 (172)	0.51
Trouble paying bills	0.19 (0.024)	0.21 (0.023)	0.18 (0.02)	0.67
Present bias	0.14 (0.02)	0.13 (0.01)	0.13 (0.01)	0.89
Makes budget	0.62 (0.028)	0.59 (0.036)	0.65 (0.021)	0.29
Subj. pr. of recovery	91.89 (0.721)	91.65 (1.031)	93.61 (0.582)	0.09
Pawn before	0.87 (0.02)	0.89 (0.013)	0.9 (0.011)	0.25
Age	43.32 (0.688)	42.85 (0.949)	43.82 (0.792)	0.73
Female	0.73 (0.023)	0.72 (0.019)	0.71 (0.02)	0.88
+ High-school	0.66 (0.027)	0.67 (0.022)	0.65 (0.018)	0.84
Obs	1386	1469	1982	

*Note:* This table has two panels. Panel A uses administrative data at the loan level, while Panel B uses survey data. Each row in this table corresponds to a regression, where the level of observation is the individual loan originated. The dependent variables of these regressions are displayed in the first column. Each dependent variable is regressed in a multivariate OLS regression against the experimental arms indicators (control, forced commitment, choice). The table reports the coefficients on each of these indicators, as well as the p-value an F-test of the null hypothesis of equality of the three coefficients. The admin data was a very limited set of pre-determined variables. The dependent variables in Panel A are the loan amount in pesos, and an indicator for whether the day of the loan origination was a weekday (as opposed to weekend). The dependent variables in Panel B from the survey. Subjective value of the pawn (how much would the client be willing to sell it for (Q3), an indicator for having trouble paying bills in the last 6-months (Q28), present bias (constructed from questions Q10 and Q29 in the standard way as in [Ashraf et al. \(2006\)](#)), an indicator for whether they make expenses budget for the month ahead of time. The subjective probability of recovery was elicited a la Manski (from 0 to 100 what is the probability that you will recoup your pawn), pawned before is a dummy=1 if the client declares to have pawned before (although not necessarily with Lender P) age is in year, +High-school is a dummy that indicates if the client has completed high school.

arm did more than three percent of individuals who responded to the survey go on to refuse loans. That is, the overwhelming majority of potential borrowers did not leave the branch after learning which contract was on offer. Moreover, the extremely small fraction that did leave is balanced across arms. Therefore it appears that the treatments have not induced any endogenous shifts in the composition of borrowers.

A less critical form of attrition is differential refusal to answer the survey questions. The survey was conducted before treatment status was revealed, and we observe loan outcomes regardless of whether the survey was conducted. Our core experimental estimates do not use the survey data as covariates, but the analysis in Section 6 is restricted to the subset of borrowers who answered at least some survey questions. The

TABLE B.2. Limited and balanced attrition				
	Control	Commitment arms		
		Forced	Choice	p-value
Number of branch-day pawns	31 (5.5)	31 (5.4)	36 (6.4)	0.21
median	27	28	31	0.15
Number of branch-day borrowers	21 (3.3)	23 (3.8)	25 (4.3)	0.24
median	19	21	21	0.46
Obs	101	97	129	
Ended up pawning	0.98 (0.01)	0.97 (0.01)	0.97 (0.01)	0.62
Survey response rate	0.79 (0.02)	0.76 (0.02)	0.77 (0.02)	0.62
Obs	1770	1954	2580	

*Note:* Each row in this table corresponds to a regression. The dependent variables are: the number of pawns-loans originated per day per branch, the number of borrowers per day-branch, a variable indicating whether a person who answered the baseline survey (before knowing contract terms) ended up pawning, and an indicator of whether the person that obtained the loan answered the baseline survey. Each dependent variable is regressed in a multivariate OLS regression against the experimental arms indicators (control, forced commitment, choice). The table reports the coefficients on each of these indicators, as well as the p-value of an F-test of the null hypothesis of equality of the three coefficients.

bottom row of Table B.2 shows that the survey response rate is broadly similar across arms (about 78 percent).



APPENDIX C: MAIN TREATMENT EFFECTS: ADDITIONAL MATERIAL

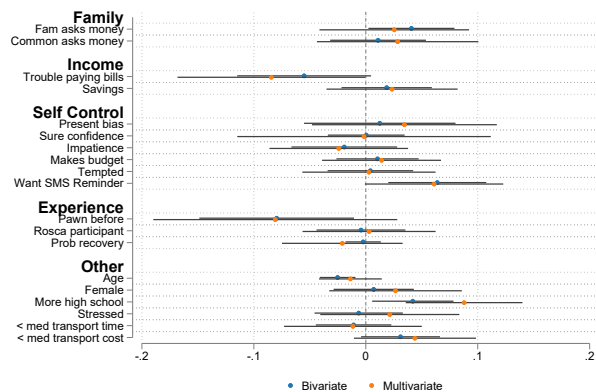


FIGURE C.1. Determinants of choice. The above figure shows the determinants in a bivariate and multivariate OLS regression of choosing commitment. Choice commitment is a binary variable equal to one, whenever subjects choose the forced commitment contract in the choice arm.

C.1 Intermediate Outcomes

TABLE C.1. Effects on intermediate outcomes

Panel A : Speed of payment					
Days to 1st payment		% of payment in 1st visit	Pr(Recovery in 1st visit)	Loan duration (days)	Loan duration   recovery
(1)	(2)	(3)	(4)	(5)	
Forced commitment	-13.8*** (1.61)	9.76*** (2.74)	0.079*** (0.026)	-27.9*** (4.35)	-17.9*** (3.88)
Choice commitment	-3.51** (1.57)	-0.58 (2.23)	-0.010 (0.022)	-0.18 (4.33)	-1.35 (4.19)
Observations	4412	6304	6304	6304	3031
R-squared	0.055	0.017	0.016	0.054	0.041
Control Mean	82.8	45.8	0.30	136.6	103.9
Panel B : Variables related to default					
Pr(+ payment & default)		% of pay   def	Pr(Selling pawn   def)	Pr(Selling pawn)	# of visits   def
(6)	(7)	(8)	(9)	(10)	(11)
Forced commitment	-0.071*** (0.015)	-4.12*** (1.28)	0.14*** (0.034)	0.0050 (0.021)	-0.031 (0.049)
Choice commitment	-0.027** (0.014)	-1.82* (1.05)	0.050* (0.029)	0.0035 (0.019)	0.085 (0.053)
Observations	6304	2492	2492	6304	2492
R-squared	0.011	0.024	0.034	0.016	0.028
Control Mean	0.12	9.68	0.71	0.31	1.14
Panel C : Visit variables					
Note: This table explores treatment effects in “intermediate variables”. Each column represents regression output for different dependent variables following equation (1). Panel A focuses on variables related to the speed of payment. While Panel B focuses on variables related to default, and Panel C related to visits. Each regression includes branch and day-of-week FE. Standard errors are clustered at the branch-day level.					

## C.2 Censoring

Some loans in our sample are “censored” in that they continue beyond our observation period. For these loans, we do not know whether the borrower ultimately defaulted or recovered her pawn. We have also shown that one effect of the forcing arm is to accelerate repayment, meaning that it is less likely for loans in this arm to be censored. This issue is illustrated in Figure C.2, which shows the CDF of loan completion (either default or recovery in Panel (a)) and loan recovery (Panel (b)) by the number of days since first pawn. Two features of these graphs are salient for our analysis. The first is the extent to which loan outcomes are observed more quickly in the forced commitment arm. This is primarily due to the substantially higher rate of repayment of Forced Commitment loans at 120 days (15 pp higher than the other arms). The second is the very low rate at which loans are recovered in any arm after 120 days. In the 180-320 day window loans are largely dormant, suggesting that many of the censored loans will in fact end in default.

The confluence of censoring and a treatment effect on censoring is potentially problematic from an experimental point of view. The approach taken in the headline results is a conservative one in that it inherently assumes that all of the loans outstanding at the end of the observation window will be repaid, making it so that the acceleration of payment observed in the Forced arm does not translate mechanically into the that treatment decreasing default. Nonetheless, to be certain that this issue is not driving our results we conduct a bounding exercise to understand how large the effects of this problem can possibly be.

One way of considering the effect that this issue could have on our results is to make extreme assumptions about the outcome of these loans in the treatment and control so as to bound the possible influence of censoring. In Table C.2 we compare the Forced and Control arms, bounding the censoring issue by reversing assumptions about the outcome of censored loans in the treatment versus the control. Panel B provides the lower bound for the treatment effect (closest to zero) by assuming censored control loans are always repaid and treatment loans never are; even in this lower-bound case the treatment effect is cost-reducing and significant at the 1% significance level and indeed the magnitude of this lower bound estimate is only 6% closer to zero than our headline result. Panel C estimates the upper bound by making the reverse assumption. Comfortingly, even with these extreme assumptions the significance on the main treatment effects never flips and treatment effects on financial cost and interests payments remain negative and significant in all scenarios. So there appears to be no scope for the censoring issue to overturn our main results.

Finally, Panel E of this table conducts a logit prediction model that uses all of the available information on loans that were completed to predict the outcome of loans that were not. This is a “best guess” of the outcome on censored loans. Using this prediction, we replicate the main experimental results and find that the treatment effect on financial cost increases from -204 (main results) to -264 (censored loans predicted), and the APR from -11% to -17%. Hence, while the censoring issue does have an effect on the magnitude of our estimated treatment effects, these checks confirm that (a) the core results are fully robust to censoring, and (b) the headline approach that we take to the issue is conservative and likely understates the true magnitude of impacts.

TABLE C.2. Bounding censoring

	FC	Interest pymnt	Principal pymnt	Lost pawn value	Default	APR
Panel A :    Control = 0    Forced Commitment = 0						
	(1)	(2)	(3)	(4)	(5)	(6)
Forced commitment	-236.0*** (48.1)	-191.7*** (37.6)	-0.63 (3.01)	-75.9** (30.5)	-0.064*** (0.023)	-0.14*** (0.022)
Observations	3724	3724	3724	3724	3724	3724
R-sq	0.016	0.025	0.004	0.012	0.019	0.043
Control Mean	989.9	593.4	5.96	396.5	0.44	0.61
Panel B :    Control = 0    Forced Commitment = 1						
	(7)	(8)	(9)	(10)	(11)	(12)
Forced commitment	-191.2*** (49.7)	-207.7*** (37.4)	1.17 (3.45)	-15.1 (31.2)	0.0083 (0.024)	-0.076*** (0.026)
Observations	3724	3724	3724	3724	3724	3724
R-sq	0.013	0.026	0.004	0.009	0.014	0.023
Control Mean	989.9	593.4	5.96	396.5	0.44	0.61
Panel C :    Control = 1    Forced Commitment = 0						
	(13)	(14)	(15)	(16)	(17)	(18)
Forced commitment	-319.0*** (50.9)	-140.4*** (34.1)	-2.33 (3.16)	-210.3*** (30.3)	-0.21*** (0.023)	-0.24*** (0.027)
Observations	3724	3724	3724	3724	3724	3724
R-sq	0.021	0.020	0.004	0.021	0.053	0.061
Control Mean	1069.2	545.9	7.69	523.3	0.57	0.70
Panel D :    Control = 1    Forced Commitment = 1						
	(19)	(20)	(21)	(22)	(23)	(24)
Forced commitment	-274.2*** (52.5)	-156.3*** (33.8)	-0.53 (3.58)	-149.6*** (31.1)	-0.13*** (0.024)	-0.17*** (0.030)
Observations	3724	3724	3724	3724	3724	3724
R-sq	0.017	0.021	0.003	0.013	0.028	0.032
Control Mean	1069.2	545.9	7.69	523.3	0.57	0.70
Panel E :    Prediction with lasso-logit model						
	(25)	(26)	(27)	(28)	(29)	(30)
Forced commitment	-264.9*** (53.8)	-169.6*** (37.2)	-1.43 (3.52)	-127.4*** (33.1)	-0.12*** (0.025)	-0.17*** (0.028)
Choice commitment	-42.4 (56.9)	-29.1 (41.8)	-2.66 (3.24)	-14.6 (34.9)	-0.017 (0.024)	0.0026 (0.029)
Observations	6304	6304	6304	6304	6304	6304
R-sq	0.018	0.022	0.002	0.010	0.016	0.042
Control Mean	1034.5	563.4	7.69	471.2	0.52	0.66

*Note:* Given the censored loans, i.e. loans that have not finished by the end of the observation period, we estimate ‘a la Manski’ bounds for these loans, meaning that we impute all loans to either *default*= 1 or *recovery*= 0 depending on the treatment arm. Different panels perform different imputations for the censored loans for all possible combinations for the imputation, and computes the ATE for the same outcomes of Table 1. Panel A, for instance, assumes that all outstanding loans are fully paid. Panel B is the most conservative imputation since it assumes all outstanding loans in the control arm are paid, while all the outstanding loans in the forced commitment arm default. Panel C, on the other hand, is the most optimistic scenario opposite to that of Panel B. Panel D assumes all remaining loans default. The last panel makes the imputation to the censored loans according to the best prediction using a piecewise lasso logit model for default. In concrete, we build two logit models with lasso regularization, depending whether the loan duration is less than 220 days (two cycles) or more than 220 days. For prediction we use the former whenever the last recorded payment was done within 220 days, and the latter otherwise. Both models includes loan characteristics (loan size, branch), and payment behavior (loan duration so far, days to first payment, % of first payment, % of payments at 30, 60, 90, and 105 days, and % of interest payed at 105 days), but the latter model also includes % of payments at 150, 180, and 210 days. This predictive model achieves an accuracy rate of 92% both in-sample and out-of-sample. Note that in all panels we maintain significant results for Financial Cost as dependent variable, while only in the most conservative scenario (Panel B) we lose significance for the APR outcome.

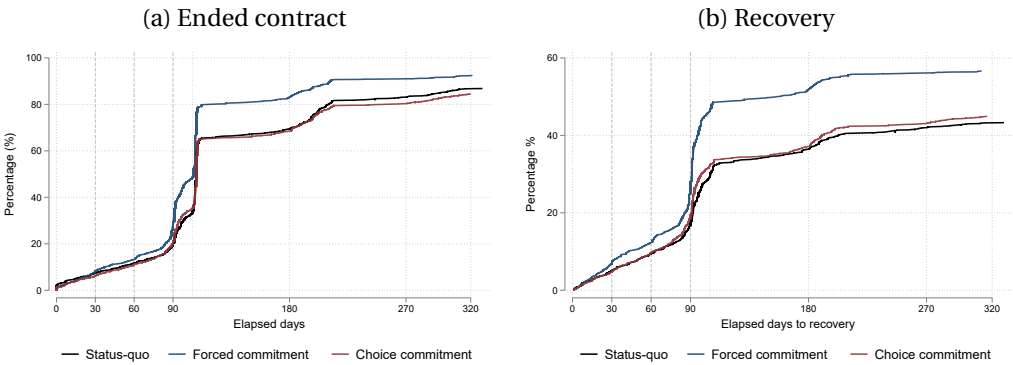


FIGURE C.2. Survival graph. This Figure shows the CDF of loan completion either default or recovery in Panel (a), or loan recovery in Panel (b), by the number of days since first pawn.

C.3 Robustness accounting for other costs

TABLE C.3. Effects on more comprehensive cost measures

	FC	FC (subj.value)	FC + tc	FC - interest	FC (subj.value) + tc - int
	(1)	(2)	(3)	(4)	(5)
Forced commitment	-204.0*** (48.1)	-299.9*** (83.3)	-207.7*** (49.0)	-98.5*** (36.7)	-146.3** (72.8)
Choice comitment	-38.9 (49.8)	-56.4 (83.5)	-32.6 (50.9)	-30.7 (39.2)	-25.3 (74.4)
Observations	6304	6304	6304	6304	6304
R-squared	0.013	0.009	0.014	0.005	0.006
Control Mean	942.4	1389.9	1026.1	480.7	927.7

	APR	APR (subj.value)	APR + tc	APR - interest	APR (subj.value) + tc - int
	(6)	(7)	(8)	(9)	(10)
Forced commitment	-0.11*** (0.019)	-0.22*** (0.051)	-0.13*** (0.028)	-0.062*** (0.019)	-0.097** (0.044)
Choice comitment	-0.0086 (0.019)	-0.053 (0.045)	-0.0035 (0.028)	-0.031* (0.018)	-0.043 (0.040)
Observations	6304	6304	6304	6304	6304
R-squared	0.031	0.011	0.027	0.008	0.007
Control Mean	0.57	1.12	0.72	0.31	0.84

*Note:* This table augments the measure of financial cost presented in Table 1 with measures of transaction costs, subjective costs, and adjustments for liquidity costs. Panel A reports financial cost in pesos, while Panel B shows APR. Columns (1) and (6) replicate our previous results for comparability. Columns (2) and (7) of Table C.3 use the subjective value of the pawn reported by the borrower rather than its appraised value. Columns (3) and (8) adjust for self-reported transport costs per visit plus an entire day's wage, both multiplied by the number of visits that each individual made.<sup>2</sup> Columns (4) and (9) adjust to consider the liquidity cost. Finally, columns (5) and (10) include all three changes together. The main takeaway from the table is that results are quite robust to including a much expanded measure of costs. Each regression includes branch and day-of-week FE. Standard errors are clustered at the branch-day level.

APPENDIX D: ALTERNATIVE EXPLANATIONS

D.1 *Learning*

Table D.1 presents information about borrowers' *future* pawning behavior as a function of treatment assignment. Column (1) considers the 228 clients who returned only a second time to pawn again at a day/branch that was randomly assigned to the choice arm. Each of the two rows in this column presents a difference of mean commitment take-up rates, and associated standard error. The first row compares those who were *initially* assigned to forced commitment against those where were assigned to control; the second row compares those who were initially assigned to the choice commitment arm to those who were assigned to the other two arms. In each case, there is no statistically discernible difference in the rates of commitment take-up. Granted, this is a selected sample because the decision to pawn again is potentially endogenous to the initial treatment allocation. For this reason, Column (2) considers the full sample of 4441 borrowers by re-defining the outcome variable to be an indicator for returning to pawn again at a branch/day when commitment was offered *and* choosing commitment. This composite outcome variable is not subject to the sample selection problem (although it is directly driven by the decision to repeat borrow). The comparison in the two rows remains the same: forced commitment versus control in row one and choice commitment versus

forced arms in row two. Again, there is no statistically discernible difference in commitment take-up rates in either row. While these exercises cannot completely exclude the possibility that learning plays a role, they provide no indication that the lack of voluntary compliance is simply a matter of inexperience with commitment.

TABLE D.1. Effect of Prior Assignment on Subsequent Choice

	Choose commitment in $t + 1$	Ever choose commitment in $t + 1$
$t$	(1)	(2)
Forced commitment (ATE)	-0.0047 (0.048)	0.00014 (0.0027)
Choice commitment (ITT)	0.034 (0.057)	0.0015 (0.0030)
Observations	228	4441
R-sq	0.004	0.000
DepVarMean	0.092	0.0047

*Note:* Column (1) reports results for the 228 borrowers who returned to pawn again at a day/branch that was randomly assigned to the choice arm, enabling us to observe whether they chose commitment or the status quo contract. Each row presents a difference in mean commitment take-up rates and associated standard errors. The first row (ATE) compares borrowers who were initially assigned to forced commitment against those who were assigned to the control condition. The second row (ITT) compares borrowers who were initially assigned to the choice commitment condition to those who were not. Whereas column (1) conditions on the (endogenously) selected sample of borrowers who return to pawn again, column (2) considers the full sample by re-defining the “outcome” to be an indicator for whether a borrower pawned again on a day when choice was offered and chose commitment.

D.2 Discount rates

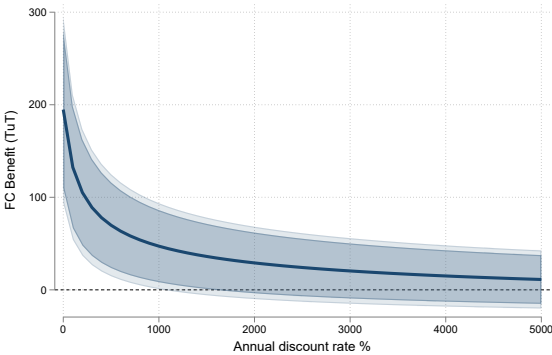


FIGURE D.1. Financial benefit TUT effect for different discount rates. This Figure re-estimates the treatment on the untreated (TUT) effect from Table 3, introducing a daily discount factor in the definition of financial benefit. At a given annual discount rate in percentage points (x-axis) the solid line gives the adjusted TUT and the shaded regions 90% & 95% confidence bands. A discount factor of one corresponds to the estimate from Table 3. As seen from the figure, borrowers would need to face unrealistically large discount rates to reverse our headline result of a large, positive, and statistically significant TUT effect.



### D.3 Present Bias

**Present bias.** If the benefits of commitment among non-choosers cannot be explained by standard models of rational choice, the canonical behavioral story would center on time inconsistency. While commitment is useful to anyone with hyperbolic time preferences, only those who are sophisticated—i.e. aware that they are hyperbolic discounters—will demand it. A large share of “naïve” hyperbolics in the population—borrowers who are unaware that they are hyperbolic discounters—could therefore drive a large and positive TUT. Our baseline survey included standard questions about discount rates between today and a month in the future versus discount rates between three and four months in the future. This allows us to classify borrowers who display more impatience over immediate delays as present biased. This measure of financial hyperbolicity is widely used in survey research, although it is not without problems.<sup>3</sup>

If we could perfectly measure present bias and sophistication, we could divide the sample into three groups: sophisticated hyperbolics (who chose commitment), time-consistent non-choosers (for whom forcing will not be effective), and naïve hyperbolic non-choosers (who will benefit from forced commitment). If present bias fully explains the low take-up rate of voluntary commitment, we should find that the TUT for present-biased borrowers is positive. This is because among the group of non-takers, a comparison of present-biased borrowers against everyone else is a comparison of naïve hyperbolics against time-consistent non-choosers.

The left panel of Figure D.2 carries out a feasible version of this exercise using our survey measure of present bias. The overall TUT estimate along with a 95% confidence interval is given in blue.<sup>4</sup> The corresponding TUT estimate and confidence interval for present-biased borrowers identified with the survey question is given in green; results for all other borrowers are shown in red. The overall TUT is a weighted average of the impact in these two sub-groups. The TUT among the present biased is insignificant and less than half the size of the strongly significant TUT among those who are *not* present biased. Therefore, taking our survey measure of hyperbolicity at face value, we find no indication that present-bias explains our positive estimated TUT.

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<sup>3</sup>Our measure is dichotomous, and it is not incentivized. Recent empirical work has shown the superiority of more elaborate measures such as “convex time budgets” (Andreoni et al., 2015) while questioning the interpretation of measures of hyperbolicity that are not based on consumption (Andreoni and Sprenger, 2012, Cohen et al., 2020), suggesting that real effort tasks provide a better measure (Augenblick et al., 2015). Given that we had only a few minutes to interview real pawnshop clients prior to a commercial transaction, our simple measure was a necessary compromise.

<sup>4</sup>For all borrowers who answered our present-bias survey questions.

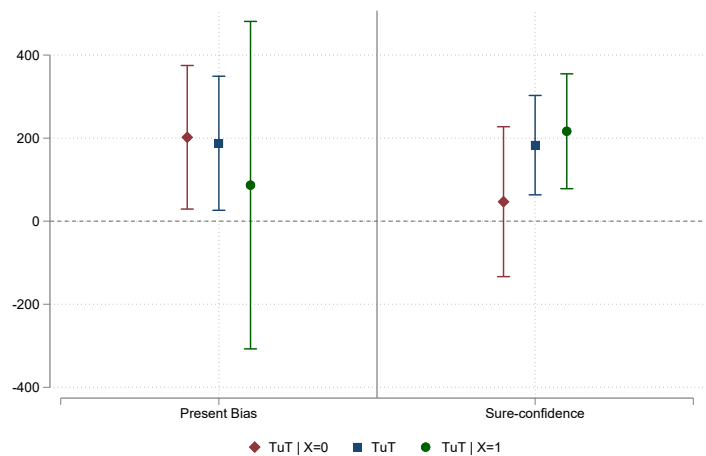


FIGURE D.2. Heterogeneity of the TUT by behavioral variables. Each panel in this figure shows how the estimated treatment on the untreated (TUT) effect varies with a binary survey variable  $X_i$ . In the left panel (P.B.),  $X_i = 1$  if borrower  $i$  is “present-biased” based on her responses to the time preference questions from our survey. In the right panel (Sure-confidence)  $X_i = 1$  if borrower  $i$  reported that she was certain to recover her pawn, zero otherwise.

D.4 *Sure Confidence*

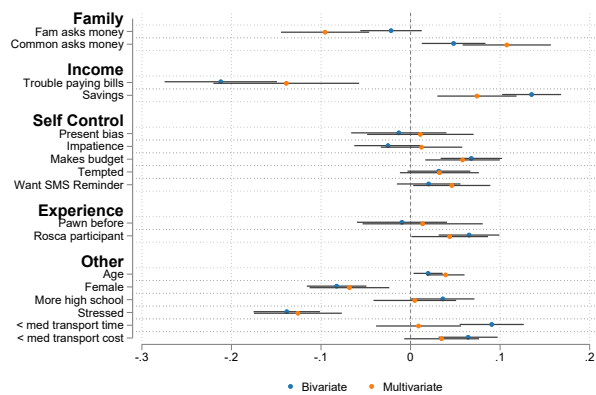


FIGURE D.3. Determinants sure confidence. The above figure shows the determinants in a bi-variate and multivariate OLS regression of sure confidence among the non-choosers. Sure confidence is a binary variable defined to be one when people report a 100% probability of recovery.

APPENDIX E: BOUNDS, FOSD AND RANK INVARIANCE

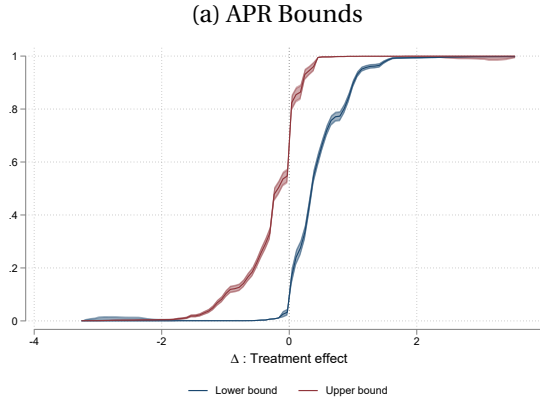


FIGURE E.1. Fan & Park bounds for benefit in APR%. This figure depicts the [Fan and Park \(2010\)](#) bounds on the distribution  $F_{\Delta}$  of individual treatment effects  $\Delta \equiv (Y_1 - Y_0)$ , described in Section 5.1, for the APR outcome. The dark red curve and light red shaded region give the estimated upper bound function  $\bar{F}$  for  $F_{\Delta}$  and associated (pointwise) 95% confidence interval. The dark blue curve and light blue shaded region give the estimated lower bound function  $\underline{F}$  for  $F_{\Delta}$  and associated (pointwise) 95% confidence interval. Confidence intervals are computed using the asymptotic distribution for the bounds. Evaluating the bounds at  $\delta = 0$ , we see that between 23% and 97% of borrowers have a positive individual treatment effect.

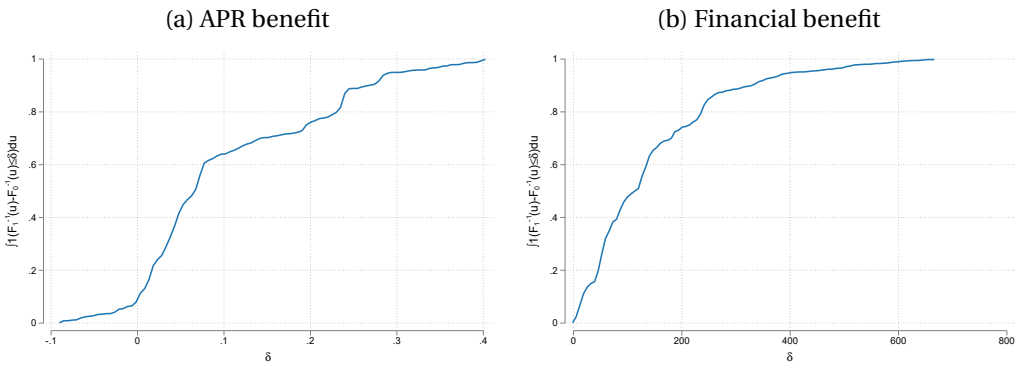


FIGURE E.2. Distribution of treatment effects under rank invariance. This figure shows the CDF of individual treatment effects under the assumption of rank invariance, computed from  $F_{\Delta}(\delta) = \int_0^1 \mathbb{1}\{F_1^{-1}(u) - F_0^{-1}(u) \leq \delta\} du$  where  $F_1^{-1}$  and  $F_0^{-1}$  are the quantile functions of  $Y_1$  and  $Y_0$ .

## APPENDIX F: CAUSAL RANDOM FOREST, CATE, AND ‘MISTAKES’

To estimate conditional average treatment effects given administrative and survey data, we use the function `causal_forest()` of the `grf` R package; to estimate conditional TOT and TUT effects we use the `instrumental_forest()` function from the same package. In each case, we use the default parameter values from the `grf` package with

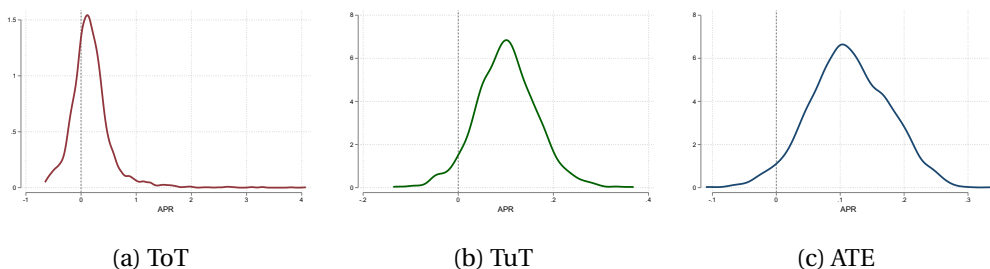


FIGURE F.1. Heterogeneous Treatment Effects.

one exception: we increase the number of trees from the default value of 2000 to 5000. The functions `causal_forest()` and `instrumental_forest()` implement special cases of the “generalized random forest” methods of [Athey et al. \(2019\)](#). In broad strokes, these functions combine a large number of regression trees that recursively partition the covariate space to estimate conditional average effects. The trees are “honest” in that observations used to determine the optimal partition are not used to estimate effects, and vice-versa. While closely related to more familiar “regression-tree” random forests, the generalized random forest approach explicitly targets the parameter of interest—a conditional ATE or IV estimand—when choosing the optimal covariate partition.<sup>5</sup>

<sup>5</sup>For more details, see [Athey et al. \(2019\)](#) and the `grf` documentation: <https://grf-labs.github.io/grf/>. When constructing our random forest estimates of heterogeneous treatment effects, we use observations for all borrowers who answered at least *part* of the intake survey. We impute the median response for the missing values, while also including an indicator whether the variable originally had a missing value. Results are similar if we manually include interactions between the original/imputed variable and an indicator for missingness. This is as expected, given that tree-based methods by their nature “automatically” consider interactions of arbitrary orders.

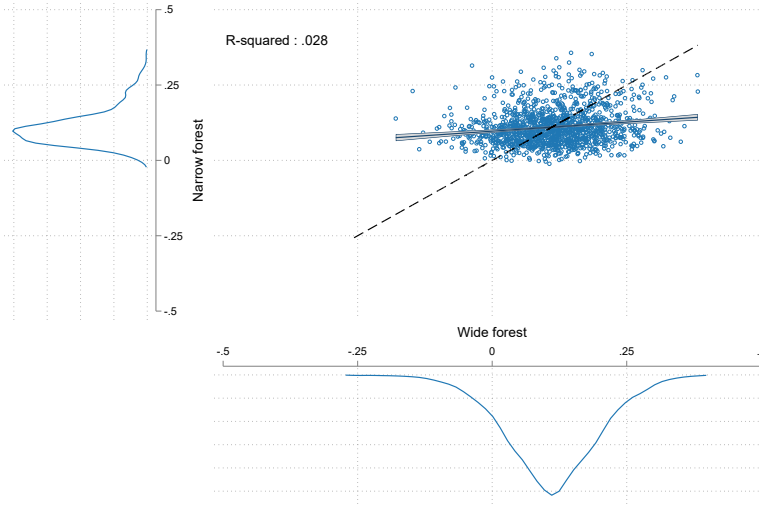


FIGURE F.2. Conditional ATEs from “wide” and “narrow” covariate sets. This figure plots the relationship between the causal forest conditional ATE estimates from Section 6.2 that use the “wide” set of covariates (all intake survey responses) and those based on a restricted “narrow” set of covariates (age, gender, HS education, and previous borrowing). The scatterplot graphs one estimate versus the other, with the “wide” covariate set on the horizontal axis and the “narrow” set on the vertical axis. The density plots on each axis show the estimated marginal distribution of conditional ATEs under each covariate set. The density for the “wide” covariate set is considerably more dispersed, as the causal forest based on this set of covariates captures considerably more treatment effect heterogeneity.

#### APPENDIX G: TESTABLE IMPLICATIONS OF THE EXCLUSION RESTRICTION

As above, let  $Y_0 \equiv Y(d=0, z=0)$  and  $Y_1 \equiv Y(d=1, z=1)$  denote the potential outcomes under *forced treatment*:  $Y_0$  is the potential outcome when forced into the status quo contract and  $Y_1$  when forced into the commitment contract. Further let  $Y_{0,2} \equiv Y(d=0, z=2)$  and  $Y_{1,2} \equiv Y(d=1, z=2)$  denote the potential outcomes under *free choice of treatment*:  $Y_{0,2}$  is the potential outcome when choosing the status quo contract and  $Y_{1,2}$  when choosing the commitment contract. Using this notation, (3) becomes  $Y_0 = Y_{0,2}$  and while (4) becomes  $Y_1 = Y_{1,2}$ . Without imposing these, Assumption 1(iii) becomes

$$Y = \mathbb{1}(Z=0)Y_0 + \mathbb{1}(Z=1)Y_1 + \mathbb{1}(Z=2)[(1-C)Y_{0,2} + CY_{1,2}]$$

but parts (i) and (ii) continue to hold. Accordingly, parts (i)–(iii) of Lemma 1 are unchanged, while parts (iv) and (v) become

$$\mathbb{E}(Y|D=0, Z=2) = \mathbb{E}(Y_{0,2}|C=0), \quad \mathbb{E}(Y|D=1, Z=2) = \mathbb{E}(Y_{1,2}|C=1).$$

Using these expressions, the testable restrictions we consider here are as follows:

$$\mathbb{E}(Y_0|C=0) = \mathbb{E}(Y_{0,2}|C=0) \tag{12}$$

$$\mathbb{E}(Y_1|C=1) = \mathbb{E}(Y_{1,2}|C=1). \tag{13}$$

Because they refer to different groups of people—choosers versus non-choosers—either of (12) and (13) could hold when the other is violated. For this reason we consider each in turn. Our approach is closely related to arguments from [Huber and Mellace \(2015\)](#) and [DiTraglia and Garcia-Jimeno \(2019\)](#), among others.

Consider first (12). Let  $p \equiv \mathbb{P}(C = 1) = \mathbb{P}(D = 1|Z = 2)$  denote the share of choosers in the population. This value is point identified regardless of whether the exclusion restriction holds. Because  $Z$  was randomly assigned, a fraction  $p$  of borrowers with  $Z = 0$  are choosers while the remaining  $(1 - p)$  are non-choosers. It follows that, regardless of whether the exclusion restriction holds, the observed distribution of  $Y|Z = 0$  is a mixture of  $Y_0|C = 0$  and  $Y_0|C = 1$  with mixing weights  $(1 - p)$  and  $p$ . This allows us to construct a pair of bounds for  $\mathbb{E}(Y_0|C = 0)$  as follows. The non-choosers must lie *somewhere* in the distribution of  $Y|Z = 0$ . Consider the two most extreme possibilities: they could occupy the bottom  $(1 - p) \times 100\%$  of the distribution or the top  $(1 - p) \times 100\%$  of the distribution. For this reason, computing the average of the *truncated* distribution of  $Y|Z = 0$ , cutting out the top  $p \times 100\%$ , provides a lower bound for the average of  $Y_0$  among non-choosers. Similarly, cutting out the bottom  $p \times 100\%$  provides an upper bound. Let  $y_{1-p}^0$  denote the  $(1 - p)$  quantile of  $Y|Z = 0$  and  $y_p^0$  denote the  $p$  quantile of the same distribution. Using this notation, the bounds are given by

$$\mathbb{E}(Y|Z = 0, Y \leq y_{1-p}^0) \leq \mathbb{E}(Y_0|C = 0) \leq \mathbb{E}(Y|Z = 0, Y \geq y_p^0)$$

These bounds do not rely on the exclusion restriction. Under Equation 12, however, we know that  $\mathbb{E}(Y_0|C = 0) = \mathbb{E}(Y|D = 0, Z = 2)$ . Therefore, if the exclusion restriction for non-choosers holds, we must have

$$\mathbb{E}(Y|Z = 0, Y \leq y_{1-p}^0) \leq \mathbb{E}(Y|D = 0, Z = 2) \leq \mathbb{E}(Y|Z = 0, Y \geq y_p^0). \quad (14)$$

Equation 14 provides a pair of testable implications of (12). If either inequality is violated, then the exclusion restriction for non-choosers fails. In our experiment,  $\hat{p} = \hat{\mathbb{P}}(D = 1|Z = 2) = 0.11$ . For the APR outcome we estimate

$$\hat{\mathbb{E}}(Y_{\text{APR}}|Z = 0, Y_{\text{APR}} \leq y_{0.89}^0) = 0.48, \quad \hat{\mathbb{E}}(Y_{\text{APR}}|Z = 0, Y_{\text{APR}} \geq y_{0.11}^0) = 0.62.$$

Since  $\hat{\mathbb{E}}(Y_{\text{APR}}|D = 0, Z = 2) = 0.58$  falls between these bounds, we find no evidence against the exclusion restriction for non-choosers. The same result holds for the financial cost outcome: results available upon request.

We can use an analogous approach to construct testable implications for 13, yielding

$$\mathbb{E}(Y|Z = 1, Y \leq y_p^1) \leq \mathbb{E}(Y|D = 1, Z = 2) \leq \mathbb{E}(Y|Z = 1, Y \geq y_{1-p}^1). \quad (15)$$

where  $y_p^1$  and  $y_{1-p}^1$  are the  $p$  and  $1 - p$  quantiles of the distribution of  $Y|Z = 1$ . If either inequality is violated, then the exclusion restriction from Equation 13 fails. Again, in our experiment  $\hat{p} = 0.11$ . For the APR outcome we estimate

$$\hat{\mathbb{E}}(Y|Z = 1, Y \leq y_{0.11}^1) = 0.06, \quad \hat{\mathbb{E}}(Y|Z = 1, Y \geq y_{0.89}^1) = 1.28$$

Since  $\hat{\mathbb{E}}(Y_{\text{APR}}|D = 1, Z = 2) = 0.43$  falls between these bounds, we find no evidence against the exclusion restriction for the choosers. The same holds for the financial cost outcome: results available upon request.

## APPENDIX H: ESTIMATION AND INFERENCE

## H.1 Regression-based Estimation of TOT, TUT, ASG, ASL, and ASB

Let  $Z_0 \equiv \mathbb{1}\{Z = 0\}$ ,  $Z_1 \equiv \mathbb{1}\{Z = 1\}$ , and  $Z_2 \equiv \mathbb{1}\{Z = 2\}$ . Under standard regularity conditions, the following proposition shows that an IV regression of  $Y$  on an intercept,  $Z_1$  and  $Z_2D$  with instruments  $(1, Z_0, Z_1)$  provides consistent estimates the ATE and TOT, while an IV regression of  $Y$  on an intercept,  $-Z_0$  and  $-Z_2(1 - D)$  with the same instrument set consistently estimates the ATE and TUT effects.

PROPOSITION 2. *Under Assumption 1,*

$$(i) Y = \mathbb{E}(Y_0) + ATE \times Z_1 + TOT \times Z_2D + U$$

$$(ii) Y = \mathbb{E}(Y_1) + ATE \times -Z_0 + TUT \times -Z_2(1 - D) + V$$

where  $\mathbb{E}(U|Z) = \mathbb{E}(V|Z) = 0$ .

PROOF OF PROPOSITION 2. For part (i), since  $Z_2D = Z_2C$  and  $(Z_0 + Z_1 + Z_2) = 1$ , Assumption 1 (iii) implies  $Y = Y_0 + Z_1(Y_1 - Y_0) + Z_2D(Y_1 - Y_0)$ . Now define

$$U \equiv [Y_0 - \mathbb{E}(Y_0)] + Z_1[(Y_1 - Y_0) - ATE] + Z_2D[(Y_1 - Y_0) - TOT].$$

Since  $Z_2D = Z_2C$  and  $Z$  is independent of  $(Y_1, Y_0)$  by Assumption 1 (i), it follows that  $\mathbb{E}(U|Z) = Z_2\mathbb{E}[C\{(Y_1 - Y_0) - TOT\}|Z]$ . Thus, by iterated expectations,

$$\mathbb{E}[C\{(Y_1 - Y_0) - TOT\}|Z] = \mathbb{P}(C = 1) [\mathbb{E}(Y_1 - Y_0|C = 1) - TOT] = 0$$

since  $Z$  is independent of  $(Y_0, Y_1)$  given  $C$ , an implication of Assumption 1 (i).

For part (ii), since  $Z_2(1 - C) = Z_2(1 - D)$  and  $(Z_1 + Z_2) = 1 - Z_0$ , Assumption 1 (iii) implies  $Y = Y_1 - Z_0(Y_1 - Y_0) - Z_2(1 - D)(Y_1 - Y_0)$ . Define

$$V \equiv [Y_1 - \mathbb{E}(Y_1)] - Z_0[(Y_1 - Y_0) - ATE] - Z_2(1 - D)[(Y_1 - Y_0) - TUT].$$

Since  $Z_2(1 - D) = Z_2(1 - C)$  and  $Z$  is independent of  $(Y_0, Y_1)$  by Assumption 1 (i),  $\mathbb{E}(V|Z) = -Z_2\mathbb{E}[(1 - C)\{(Y_1 - Y_0) - TUT\}|Z]$ . Thus, by iterated expectations,

$$\mathbb{E}[(1 - C)\{(Y_1 - Y_0) - TUT\}|Z] = \mathbb{P}(C = 0|Z) [\mathbb{E}(Y_1 - Y_0|C = 0) - TUT] = 0$$

since  $Z$  is independent of  $(Y_0, Y_1)$  given  $C$ , an implication of Assumption 1 (i). □

Since  $ASG = TOT - TUT$ , the preceding proposition provides a consistent estimate of the ASG effect. The ASB effect,  $\mathbb{E}(Y_0|C = 1) - \mathbb{E}(Y_0|C = 0)$ , can likewise be estimated by taking the difference of coefficients across two linear IV regressions with *no intercept* and instrument sets  $(Z_0, Z_2)$ , as shown in the following proposition.

PROPOSITION 3. *Under Assumption 1*

$$(i) (1 - D)Y = \mathbb{E}(Y_0) \times Z_0 + \mathbb{E}(Y_0|C = 0) \times (1 - D)Z_2 + U_0$$

$$(ii) (1 - D)Y = \mathbb{E}(Y_0) \times (Z_0 + Z_2) + \mathbb{E}(Y_0|C = 1) \times -DZ_2 + U_1$$

where  $\mathbb{E}(U_0|Z) = \mathbb{E}(U_1|Z) = 0$ .



PROOF. Assumption 1 (ii) implies  $(1 - D) = Z_0 + Z_2(1 - C)$ . Hence,

$$(1 - D)Y = Z_0Y_0 + Z_2(1 - C)[(1 - C)Y_0 + CY_1] = Z_0Y_0 + Z_2(1 - C)Y_0$$

by Assumption 1 (iii), since  $Z_j^2 = Z_j$  for any  $j$  and  $Z_jZ_k = 0$  for any  $j \neq k$  and, similarly,  $(1 - C)^2 = (1 - C)$  and  $C(1 - C) = 0$ . Therefore, since  $Z_2(1 - C) = Z_2(1 - D)$ ,

$$(1 - D)Y = Z_0Y_0 + Z_2(1 - D)Y_0, \quad (1 - D)Y = (Z_0 + Z_2)Y_0 + (-DZ_2)Y_0.$$

Now, define

$$U_0 \equiv Z_0[Y_0 - \mathbb{E}(Y_0)] + Z_2(1 - D)[Y_0 - \mathbb{E}(Y_0|C = 0)]$$

$$U_1 \equiv (Z_0 + Z_2)[Y_0 - \mathbb{E}(Y_0)] + (-Z_2D)[Y_0 - \mathbb{E}(Y_0|C = 1)].$$

Since  $Z_2(1 - D) = Z_2(1 - C)$ , and  $Z$  is independent of  $Y_0$ ,

$$\mathbb{E}(U_0|Z) = Z_2\mathbb{E}[Y_0 - \mathbb{E}(Y_0|C = 0)|C = 0, Z] = 0$$

by iterated expectations and the fact that  $Z$  is conditionally independent of  $Y_0$  given  $C$ . Since  $Z_2D = Z_2C$ , a nearly identical argument gives

$$\mathbb{E}(U_1|Z) = -Z_2\mathbb{E}[Y_0 - \mathbb{E}(Y_0|C = 0)|C = 1, Z] = 0. \quad \square$$

The final result in this section implies that the ASL effect,  $\mathbb{E}(Y_1|C = 1) - \mathbb{E}(Y_1|C = 0)$ , can be estimated as the difference of coefficients across two linear IV regressions with *no intercept* and instrument set  $(Z_1, Z_2)$ .

PROPOSITION 4. *Under Assumption 1,*

$$(i) \quad DY = \mathbb{E}(Y_1) \times (Z_1 + Z_2) + \mathbb{E}(Y_1|C = 0) \times (D - 1)Z_2 + V_0$$

$$(ii) \quad DY = \mathbb{E}(Y_1) \times Z_1 + \mathbb{E}(Y_1|C = 1) \times DZ_2 + V_1$$

where  $\mathbb{E}(V_0|Z) = \mathbb{E}(V_1|Z) = 0$ .

PROOF. By Assumption 1,  $D = Z_1 + Z_2C$ . Hence, by Assumption 1 (iii),

$$DY = Z_1Y_1 + Z_2C[(1 - C)Y_0 + CY_1] = Z_1Y_1 + Z_2CY_1$$

because  $Z_j^2 = Z_j$  for any  $j$  and  $Z_jZ_k = 0$  for any  $j \neq k$  and, similarly,  $(1 - C)^2 = (1 - C)$  and  $C(1 - C) = 0$ . Therefore, since  $Z_2(1 - C) = Z_2(1 - D)$ ,

$$DY = (Z_1 + Z_2)Y_1 + Z_2(D - 1)Y_1, \quad DY = Z_1Y_1 + Z_2DY_1.$$

Now, define

$$V_0 = (Z_1 + Z_2)[Y_1 - \mathbb{E}(Y_1)] + Z_2(D - 1)[Y_1 - \mathbb{E}(Y_1|C = 0)]$$

$$V_1 = Z_1[Y_1 - \mathbb{E}(Y_1)] + Z_2D[Y_1 - \mathbb{E}(Y_1|C = 1)].$$

Since  $Z_2(1 - D) = Z_2(1 - C)$  and  $Z$  is independent of  $Y_1$ ,

$$\mathbb{E}(V_0|Z) = -Z_2\mathbb{E}[Y_1 - \mathbb{E}(Y_1|C=0)|C=0, Z] = 0$$

by iterated expectations and the fact that  $Z$  is conditionally independent of  $Y_1$  given  $C$ . Since  $Z_2D = Z_2C$ , a similar argument gives

$$\mathbb{E}(V_1|Z) = Z_2\mathbb{E}[Y_1 - \mathbb{E}(Y_1|C=1)|C=1, Z] = 0. \quad \square$$

## H.2 Inference for ASG, ASB, and ASL

We now explain how to carry out cluster-robust inference for the ASG, ASB, and ASL effects, as implemented in our companion STATA package. Each of these effects can be expressed as a difference of coefficients from two just-identified linear IV regressions. The ASG effect is the difference of the TOT and TUT effects from Proposition 2. Similarly, the ASB effect is the difference of  $\mathbb{E}(Y_0|C=1)$  and  $\mathbb{E}(Y_0|C=0)$  from Proposition 3 while the ASL effect is the difference of  $\mathbb{E}(Y_1|C=1)$  and  $\mathbb{E}(Y_1|C=0)$  from Proposition 4. Within each pair of IV regressions the outcome variable and instrument set is identical; only the regressors differ. Since our estimators of all three effects share the same structure, our discussion abstracts from the specific regressors and instruments used in each case.

Let  $g = 1, \dots, G$  index clusters and  $i = 1, \dots, N_g$  index individuals within a particular cluster  $g$ . In our experiment, a cluster is a branch-day combination and the experimentally-assigned treatment (control, forced, or choice arm) is assigned at the cluster level. We assume that observations are iid across clusters but potentially correlated within cluster. Now consider a pair of just-identified linear IV regressions given by  $Y_{ig} = \mathbf{X}'_{1,ig}\boldsymbol{\theta}_0 + U_{ig}$  and  $Y_{ig} = \mathbf{X}'_{0,ig}\boldsymbol{\theta}_1 + V_{ig}$  with common instrument vector  $\mathbf{W}_{ig}$ . Stacking observations in the usual manner, e.g.  $\mathbf{W}'_g \equiv [\mathbf{W}_{1g} \cdots \mathbf{W}_{N_gg}]$  and  $\mathbf{W}' = [\mathbf{W}'_1 \cdots \mathbf{W}'_G]$  we can write the preceding equations in matrix form as  $\mathbf{Y} = \mathbf{X}_1\boldsymbol{\theta}_1 + \mathbf{U}$  and  $\mathbf{Y} = \mathbf{X}_0\boldsymbol{\theta}_0 + \mathbf{V}$  with instrument matrix  $\mathbf{W}$ . Now, the IV estimators for  $\boldsymbol{\theta}_1$  and  $\boldsymbol{\theta}_0$  can be expressed as

$$\hat{\boldsymbol{\theta}}_1 = (\mathbf{W}'\mathbf{X}_1)^{-1} \mathbf{W}'\mathbf{Y} = \boldsymbol{\theta}_1 + (\mathbf{W}'\mathbf{X}_1)^{-1} \mathbf{W}'\mathbf{U}$$

$$\hat{\boldsymbol{\theta}}_0 = (\mathbf{W}'\mathbf{X}_0)^{-1} \mathbf{W}'\mathbf{Y} = \boldsymbol{\theta}_0 + (\mathbf{W}'\mathbf{X}_0)^{-1} \mathbf{W}'\mathbf{V}.$$

By our experimental design and exclusion restriction,  $\mathbf{W}_{ig}$  is independent of  $U_{ig}$  both unconditionally and conditional on cluster size. Hence, by a standard argument and under mild regularity conditions, the following expression provides a consistent, cluster robust estimator of  $\widehat{\text{Avar}}(\hat{\boldsymbol{\theta}}_1 - \hat{\boldsymbol{\theta}}_0)$

$$\widehat{\text{Avar}}(\hat{\boldsymbol{\theta}}_1 - \hat{\boldsymbol{\theta}}_0) = [(\mathbf{W}'\mathbf{X}_1)^{-1} - (\mathbf{W}'\mathbf{X}_0)^{-1}] \begin{bmatrix} \mathbf{S}_{UU} & \mathbf{S}_{UV} \\ \mathbf{S}'_{UV} & \mathbf{S}_{VV} \end{bmatrix} \begin{bmatrix} (\mathbf{X}'_1\mathbf{W})^{-1} \\ -(\mathbf{X}'_0\mathbf{W})^{-1} \end{bmatrix}$$

where we define the IV residuals  $\hat{\mathbf{U}}_g \equiv \mathbf{Y}_g - \mathbf{X}_{1,g}\hat{\boldsymbol{\theta}}_1$  and  $\hat{\mathbf{V}}_g \equiv \mathbf{Y}_g - \mathbf{X}_{0,g}\hat{\boldsymbol{\theta}}_0$  along with the matrices  $\mathbf{S}_{UU} \equiv \sum_{g=1}^G \mathbf{W}'_g \hat{\mathbf{U}}_g \hat{\mathbf{U}}'_g \mathbf{W}_g$ ,  $\mathbf{S}_{UV} \equiv \sum_{g=1}^G \mathbf{W}'_g \hat{\mathbf{U}}_g \hat{\mathbf{V}}'_g \mathbf{W}_g$ , and finally  $\mathbf{S}_{VV} \equiv$

$\sum_{g=1}^G \mathbf{W}_g' \hat{\mathbf{V}}_g \hat{\mathbf{V}}_g' \mathbf{W}_g$ . In our application the number of clusters,  $G$ , is large. If desired, an *ad hoc* degrees of freedom correction can be applied by multiplying the associated standard errors by  $\sqrt{G/(G-1)}$ .

Co-editor [Name Surname; will be inserted later] handled this manuscript.