

# Liquidity and Insurance in Student-Loan Contracts: The Effects of Income-Driven Repayment on Borrower Outcomes

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## Abstract

Traditional student loan payments fall on borrowers early in their careers and provide no insurance against earnings shocks. By contrast, Income-Driven Repayment (IDR) lowers monthly minimums to a share of borrower income until debt is repaid or some forgiveness period has been reached, increasing short-run liquidity at the potential cost of long-run debt forgiveness or distorted labor supply. In this paper, I use an administrative panel of student loans to estimate IDR's effect on short- and long-run borrower outcomes and predict its fiscal costs. Exploiting variation in loan-servicing calls, I find that enrolling in IDR results in 22pp fewer delinquencies and \$368 lower balances within eight months of take-up. Three years later, IDR enrollees are 2.0pp more likely to hold mortgages, 1.8pp more likely to move to a higher-income zip code, and hold 0.2 more credit cards than non-enrollees. By contrast, I find no effects on unemployment deferments, a proxy for borrower employment status. I also find that most enrollees exit IDR and return to standard repayment after just one year, meaning the predicted incidence of debt forgiveness under IDR is close to zero. Taken together, my results suggest IDR provides short-term liquidity benefits but limited lifetime insurance value, carrying minimal long-run fiscal costs or labor supply distortions.

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# 1 Introduction

Over one million student borrowers default each year, and millions more suffer from low homeownership and poor financial health. The culprit, according to many, is the poor structure of student loan contracts (Barr et al., 2017). Traditional student loans carry fixed, fully amortized payments that average over \$350 per month, fall on borrowers early in their careers, and provide no insurance against income shocks. The policy response has been Income-Driven Repayment (IDR), which sets monthly minimum payments to a fixed portion of borrowers' income until debt is repaid or some forgiveness period has been reached (see Figure 1). Enrollment in IDR has tripled since 2014 and one-half trillion dollars in debt is currently repaid through the program (Department of Education, 2020b). Similar programs have been adopted in the UK and Australia, where over eighty-five percent of students finance their higher education through income-contingent repayment schemes.

Even as IDR enrollment continues to rise, its effects on social welfare are largely unknown. By aligning the repayment burden of student debt with the wage returns to college, IDR may help credit-constrained borrowers smooth their incomes over time and insure against earnings shocks, allowing them to avoid default, increase consumption, or invest in homes during periods of temporarily low income. However, if borrowers' incomes remain *permanently* low, these benefits could be outweighed by the long-term costs of accumulated debt. If borrowers remain enrolled in IDR with persistently high balances, the program might reduce social welfare through costly debt forgiveness and moral hazard.

Assessing the costs and benefits of IDR requires estimates of its causal impacts on borrowers, but two obstacles have prevented researchers from identifying these effects. First, high-frequency repayment data for large samples of student borrowers have been unavailable, preventing even descriptive statistics like IDR attrition rates from being calculated. Second, causal identification is difficult because IDR selects for borrowers with high debt and low expected income. Low-income individuals typically have worse financial outcomes, but borrowers with high student debt balances are often highly educated and positively selected (Yannelis, 2016), so estimates that rely on cross-sectional comparisons of IDR enrollees to non-enrollees could suffer from selection bias in either direction.

In this paper, I use administrative data from a large student loan servicing company to

estimate IDR’s causal effects on borrower outcomes and forecast its long-term fiscal costs. The data I use link monthly loan records from a large loan servicer (“LLS”) to credit bureau information from TransUnion, allowing me to investigate both short-term repayment behavior and long-term proxies for homeownership, consumption, employment, and income. The data include monthly records of student loan balances, payments, delinquencies, and repayment plan enrollment; annual records of bankruptcies, credit scores, mortgages and credit cards; and borrower-level information on demographics, college attendance, and contact histories. These data are, to the best of my knowledge, the first panel of U.S. federal student loan payments used in public research.

To identify the treatment effects of IDR on both short- and long-term borrower outcomes, I use two complementary empirical strategies exploiting variation in loan-servicing calls. To estimate effects on short-term repayment outcomes, I use an instrumental variables (IV) design exploiting the quasi-random assignment of these calls to debt-servicing agents via an automatic dialing system implemented in 2016. Using both leave-one-out efficacy scores and agent-specific electronic signature (“e-sign”) capabilities as instruments, my IV strategy captures variability in agents’ tendencies to induce IDR take-up, identifying local average treatment effects (LATEs) of IDR among individuals whose repayment plans depend upon their assigned agent.

To estimate effects on long-term credit and employment outcomes, I must rely on borrowers who enrolled in IDR before LLS randomized all of its outgoing calls among agents. Thus, for these outcomes, I employ a difference-in-differences design comparing IDR enrollees (the “treatment group”) to non-enrollees (the “control group”) before and after receiving delinquency calls. I show that pre-call trends in treatment group outcomes are nearly identical to those of the control group, as are post-call responses to “placebo” calls placed years prior to enrollment.

My findings suggest IDR provides large and persistent liquidity benefits to borrowers with no apparent distortions to labor supply. Estimated treatment effects imply IDR reduces delinquency rates (i.e., late payments) by 22.2 percentage points within eight months of take-up. This effect on repayment likelihood dominates IDR’s mechanical effect on payment size, so that IDR borrowers pay down \$35 more debt each month than standard borrowers, despite facing \$170 lower monthly minimum payments. Long-term analysis suggests IDR

has lasting effects on financial outcomes and proxies for consumption and homeownership. Enrollees have credit scores which are 5.6 points higher, hold 0.2 more credit cards, and carry \$360 higher credit card balances than non-enrollees three years after the servicing call. Relative to non-enrollees, IDR enrollees are 2 percentage points more likely to move to a higher-income zip code and 2 percentage points more likely to hold a mortgage after three years, an increase of 9 percent off of the pre-call mean. By contrast, unemployment deferments are not significantly different between IDR and standard borrowers within three years of the servicing call, suggesting the program carries minimal distortions to labor supply.

Despite persistent effects on long-term outcomes, borrowers' time spent enrolled in IDR is remarkably short. Likely due to the burdensome income re-certification process, most borrowers fail to re-enroll after one year and quickly return to their pre-call repayment rates. Extrapolating these rates into the future implies that few, if any, borrowers should expect to qualify for debt forgiveness, even under conservative projections of income growth. These projections suggest IDR's treatment effects are driven by transfers *within* borrowers over time, providing further evidence of minimal moral hazard risk. They also suggest the fiscal costs of IDR may be significantly lower than previous simulations have implied (Lucas and Moore, 2010; Di and Edmiston, 2017), highlighting the importance of considering counterfactual repayment behavior when evaluating the budgetary implications of student loan reforms.

My results carry important implications for higher education policy. IDR was designed to provide comprehensive, "equity-like" restructuring of student-debt contracts, but in practice it serves only as a short-term cash infusion to financially distressed borrowers. This increased cash-on-hand provides large and lasting benefits at minimal cost to social welfare, but if policymakers want IDR to facilitate more persistent income smoothing or insure against lifetime-earnings risk, they must reform program features like recertification requirements and loan forgiveness rules.

This paper complements a small but growing literature on student loan contracts and IDR. Beginning with Friedman (1962), many researchers have documented the theoretical benefits of income-contingent student debt, citing both incomplete credit markets and the need for more "equity-like" instruments for human capital investments (Chapman, 2006; Barr et al., 2017). A related stream of literature documents the revenue implications of

various loan contracts by simulating repayment paths and loan forgiveness-incidence across different populations (Lucas and Moore, 2010; Johnston and Barr, 2013; Britton et al., 2019). Chapman and Leigh (2009) and concurrent work by Britton and Gruber (2019) both use bunching designs to estimate labor supply responses to marginal changes in the income-share rates charged by Australian and UK student loan systems, respectively. Both find small or null effects of increased rates on earned income. Two studies, Abraham et al. (2018) and Field (2009), investigate the *ex-ante* effect of student loan contract offerings on students’ decisions. Finally, indicative of IDR’s policy importance, several studies have aimed to evaluate measures which might improve take-up of IDR among eligible borrowers. Abraham et al. (2018) and Cox et al. (2018) document students’ hypothetical plan choices under alternative framing, information, and income scenarios. In ongoing work, Mueller and Yannelis (2019) investigate the effects of electronic signature technology on IDR take-up using call-agent assignment at Navient Corporation. Consistent with my first-stage results, they find that agents with e-sign capabilities induce significantly higher take-up.

This paper makes four contributions to the existing literature. First, I provide the first causal estimates of IDR’s treatment effects on loan repayment, credit card balances, home-ownership, and employment proxies. Second, I provide new evidence on IDR’s fiscal costs and potential moral hazard effects, as my budgetary simulations are the first to incorporate IDR attrition and repayment responses. Third, my first-stage results demonstrate the importance of psychological frictions or “hassle-costs” not only in the initial take-up of IDR, but also in the retention of existing enrollees. Fourth, my findings provide evidence of liquidity constraints among student borrowers. While liquidity constraints and incomplete credit markets are well-studied phenomena, documenting them in the student-borrowing context carries added importance given the contribution of education to economic growth. Indeed, these findings suggest trillions of dollars in human capital investments are not efficiently financed by existing credit markets, potentially carrying significant macroeconomic consequences.

The remainder of this paper is organized as follows. Section 2 provides a brief overview of federal student loans, IDR, and student loan servicing in the US. Section 3 describes the student loan and credit bureau data used in this study. Section 4 describes my empirical strategy. Section 5 presents results and interpretation. Section 6 provides predictions of IDR’s fiscal costs and discusses its social welfare implications. Section 7 concludes.

## 2 Background

### 2.1 Federal Student Loans and Repayment Plans

Over 90% of student loans in the United States are federally subsidized and guaranteed.<sup>1</sup> The government holds the liability on student loans, and interest rates are set by Congress.<sup>2</sup> Student loans are not secured by collateral or subject to any credit check. While the amount one can borrow from federal sources is capped by semester, virtually anyone attending an accredited institution is eligible to borrow at the same subsidized rate.<sup>3</sup>

The Department of Education sets repayment terms for student loans through repayment plans. Repayment plans specify the monthly minimum payments borrowers must make, though borrowers can pay more than the minimum without penalty if they wish to pay down their debt early. The default repayment plan into which all borrowers are automatically enrolled is known as “standard repayment.” Under standard repayment, minimum monthly payments follow a flat repayment schedule over ten years. Until 2010, the vast majority of borrowers in repayment were enrolled in standard repayment plans, with only a small fraction of borrowers choosing alternative financing options.

Income-driven repayment (IDR) plans were first offered in 2009 as an alternative to standard repayment. Since then, several versions of IDR have become available, including Income-Based Repayment (IBR), Pay-As-You-Earn (PAYE), and Revised-Pay-As-You-Earn (REPAYE). Eligibility criteria and repayment terms can vary across these plans, though they

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<sup>1</sup>A small private student loans market constitutes around ten percent of total student debt, mostly for creditworthy graduate students or borrowers who have exhausted their federal loan limits. In most cases, however, private lenders cannot compete with the subsidized rates offered by the government under the Federal Family Education Loan (FFEL) and Federal Direct Loan programs. Unless stated otherwise, I will use “student loans” to refer to loans originating from these federal programs.

<sup>2</sup>Congress has set rates on student loans since 1965, though it automated the process in 2013 with the Bipartisan Student Loan Certainty Act, which sets interest rates equal to the 10-year Treasury bond rate plus 205 basis points (360 bps for graduate students). Interest rates are fixed throughout the life of a loan and accrue as simple daily interest on principal only.

<sup>3</sup>A small portion of borrowers who exceed their borrowing caps supplement their federal student loans with private loans or parent-cosigned PLUS loans, both of which are excluded from my analysis. While all borrowers are subject to the same federal borrowing caps, short-term borrowing costs can vary by financial need, as the Subsidized Stafford Loan program forgives interest accrued while the borrower is still in school, up to a means-tested limit.

share the same general structure. For the purposes of this study, I focus on the largest IDR plan, Income-Based Repayment (IBR), as borrowers in my sample are ineligible for newer IDR plans, though the discussion generalizes to the broader concept of IDR.

Minimum payments under IDR are pegged to fifteen percent of borrowers' discretionary income, defined as the difference between adjusted gross income (AGI) and 150% of the federal poverty line (FPL). Specifically,

$$\text{Monthly IDR Payment} = 15\% * \left( \frac{\text{AGI} - 1.5 * \text{FPL}}{12} \right) \quad (1)$$

Payments for a married borrower who files jointly are prorated to her share of combined household student debt. Monthly payments are capped at the standard minimum payment amount, and payments continue until the borrower's balance reaches zero. If a borrower successfully makes three-hundred payments under IDR, any remaining balance is forgiven, though any forgiven debt is treated as taxable income. Figure 1 provides a graphical comparison of IDR versus standard repayment plans under alternative income scenarios.

Borrowers can switch to IDR at any point in the repayment process. Opting-in requires completing an online form through the Department of Education, which verifies income and family size using information from a borrower's most recent federal tax return. Borrowers must recertify their income on a yearly basis, though they can adjust their payments more frequently with proof of income. If a borrower on IDR goes more than one year without recertifying income and family size, her payments automatically return to the standard payment amount, though her repayment plan is still classified as IDR (Department of Education, 2020c).

Borrowers who fail to meet their monthly payments (i.e., "fall delinquent") under any repayment plan face penalties that increase in severity with the number of days past due. Between one and ten days past due, borrowers receive delinquency notices by email and post. Between ten and ninety days past due, borrowers are charged late fees and contacted by phone at increasing frequency to encourage repayment and discuss repayment options. At 91, 181, and 271 days past due, borrowers are reported to credit bureaus, damaging their credit scores. Loans more the 270 days past due are considered eligible for default. Once in default, all remaining balance on student debt becomes due, and the Department

of Education can garnish up to fifteen percent of borrowers’ wages or withhold their tax returns to collect on defaulted debt. In twenty states, the federal government can block the renewal of professional licenses for defaulted borrowers working in health care, education, and/or other licensed fields. Unlike other forms of consumer debt, student loans cannot be discharged in bankruptcy, except in rare circumstances. Defaulted borrowers are ineligible for any future federal student aid (Department of Education, 2020a).

## **2.2 Study Setting: Student Loan Servicers and LLS**

As one of ten federal student loan servicing companies, LLS manages disbursement, billing, and processing of over \$300 billion in federal student loans. Debt servicing is provided on behalf of the Department of Education. As a part of its servicing operations, LLS makes frequent contact with delinquent borrowers to encourage repayment. When borrowers become fifteen or more days past due on their payments, their phone numbers are placed in a dialing queue. An automatic dialer then places calls to queued numbers in rapid succession. If a call is unanswered, the dialer places it back at the bottom of the queue. Each answered call is immediately connected to a debt-servicing agent randomly selected from the pool of available agents. If no agents are available, the dialer places the borrower on hold until one becomes available. Such instances are extremely rare, however, as the dialer places calls at a rate to match agent availability, which is highly predictable over large numbers of agents.

LLS employs over three-hundred servicing agents across four call centers. Agents are tasked with informing borrowers of their delinquent status, inquiring about their ability to repay, and informing them of repayment options. During a call session, the questions and responses of the agent are guided by a decision tree. The agent first asks if a borrower can make payments under their current plan. If not, the agent asks if the borrower is unemployed or a full-time student, as such borrowers can typically qualify for interest-free unemployment deferments. Finally, the agent “models-out” IDR payments for the borrower, eliciting information on annual income, marital status, and family size. Borrower responses are entered into the agent’s computer, which provides an estimate of monthly IDR payments according to Equation 1. If these “modeled-out” payments are lower than what the borrower is paying under the standard plan, the agent provides instructions for online IDR enrollment with the



Department of Education. Agents are incentivized to bring delinquent accounts current, but face penalties if they fail to present borrowers with their best available options. Supervisors periodically monitor agents' calls to ensure they meet federal compliance standards. If an agent does not offer IDR to a borrower deemed suitable for the option during a monitored call, the agent's pay is reduced that month.

### 3 Student Loan Servicing Data

The data I use in this paper link administrative student loan repayment and contact data to credit bureau records for over one million borrowers. Data are drawn from LLS's FFEL loan portfolio, which includes over \$90 billion in loans. The LLS loan data contain detailed repayment records for each borrower, including principal borrowing amounts, loan balances, minimum payments due, and dates of delinquency at a monthly frequency. They also include indicators for type of loan (e.g., Subsidized Stafford, PLUS), current repayment plan, and current loan status (e.g., deferment, grace period, default). In addition to loan information, the LLS data contain borrower characteristics, including year of birth, 9-digit zip code, OPE ID for attended institutions, college attendance dates, and graduation status. Gender is inferred using first names.<sup>4</sup>

I merge demographic and loan information with LLS contact histories from 2011 onward. Contact history data provide a single observation for each point of contact and include all incoming and outgoing calls in which the line was connected to a borrower in the sample. For each call in the data, I observe the date, time of day, incoming/outgoing status, and servicing agent identifier associated with the call. Agent identifiers are linked to a small set of agent characteristics, including work site location and work group (e.g., "claims aversion," "skip tracing," etc.).

Finally, borrowers in the LLS data are linked to yearly TransUnion credit bureau records from 2010 through 2018. The TransUnion data provide yearly balances, credit limits, delinquencies, and number of accounts for several categories of consumer debt, including mort-

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<sup>4</sup>The online appendix to Tang et al. (2011) provides a public-use list of common first names paired with the male-female proportions of New York City Facebook profiles with each name. LLS merged this list to first names in their borrower records at my request.

gages, credit cards, and auto loans. They also include broader measures of financial health, like credit scores and bankruptcies.<sup>5</sup> TransUnion data are merged to borrowers in the LLS data by SSN. 92 percent of borrowers are successfully matched to TransUnion records.

### 3.1 Sample Selection

The analysis sample used in this study consists of 133,688 individuals selected to best represent the general population of IDR-eligible borrowers. To construct this sample, I begin with the universe of LLS’s FFEL borrowers with positive balances as of December 2011, excluding those who hold any private or Direct loans.<sup>6</sup> From this population of 5.8 million borrowers, I remove anyone whose loans were canceled, discharged, or paid-in-full by December 2013, leaving 3.8 million borrowers. I then select those borrowers who answered a delinquency call between 2014 and 2018, limiting the sample to 631,273 borrowers. I then remove borrowers who cannot be matched by zip code or first name to inferred measures of gender or income, or whose credit card or mortgage balances exceed the ninety-ninth percentile in any year, leaving 539,456 borrowers. Next, I limit the sample to English speakers who answered at least one call within 140 days of falling delinquent, leaving 443,321 borrowers. Then, I remove borrowers who were already enrolled in IDR prior to their delinquency call, as they would not be eligible for call-induced IDR take-up. I also remove anyone with a previous IDR spell from the sample so that estimates can be interpreted as the effect of *initial* enrollment. From the remaining group of 402,219 borrowers, I keep only those reaching the stage at which borrowers were provided information concerning their potential IDR payments (i.e., “modeled-out”), leaving 133,688 borrowers.<sup>7</sup>

To facilitate my empirical strategy, I use the sample of borrowers described above to

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<sup>5</sup>Additional details concerning TransUnion data can be found in Dobbie et al. (2017), Avery et al. (2003), and Finkelstein et al. (2012).

<sup>6</sup>While borrowers can hold loans from a mixture of FFEL, Direct, and private sources, the database I use only includes repayment information for FFEL borrowers. The analysis sample excludes borrowers with mixture of loans, so I can observe their complete repayment profile. Roughly fifteen percent of LLS’s 2012 FFEL borrowers also hold Direct loans, and fewer than ten percent hold private student loans.

<sup>7</sup>I focus on modeled-out borrowers because they are more representative of the IDR-eligible population than non-modeled borrowers, whose debt-to-income ratios are often too low to qualify for reduced payments. Estimates for the pooled population of modeled and non-modeled borrowers, reported in Table A11, are qualitatively similar to those for the modeled-out sample.

create three balanced panels at the borrower-by-call level, centered around call dates. For instrumental variables analysis of short-term repayment outcomes, I select all calls made from 2017 onward by agents with at least 100 total calls.<sup>8</sup> From the resulting sample of 78,072 calls, I create a balanced monthly panel of 50,120 calls with 20 leads and 10 lags. For difference-in-differences analysis of longer-term outcomes, I broaden the selection criteria to include calls from 2013 to 2016 and those made by small-cell agents. From this sample of 187,987 calls, I create two additional balanced panels corresponding to the frequencies of outcome data: a *yearly* panel of 22,904 calls with 4 leads and 3 lags, and a *monthly* panel of 47,724 calls with 42 leads and 10 lags.

Table 1 provides summary statistics for samples of interest. The “full sample” (column 1) is a random sample of 608,195 drawn from the population of LLS FFEL borrowers as of December 2012. The “analysis sample” (column 2) is the entire subpopulation of borrowers selected according to the criteria described above. In the full sample, IDR has low take-up, with only 14 percent of borrowers enrolled in a plan. That share rises to 34 percent in the analysis sample, as it is constructed to include only borrowers who might benefit from the plan. Unsurprisingly, these borrowers have lower credit card limits, higher rates of bankruptcy and live in lower-income zip codes. Columns 3 and 4 of Table 1 break the analysis sample into control and treatment groups, where treatment is defined as IDR enrollment within four months of answering an LLS delinquency call.<sup>9</sup> Baseline variables for treated borrowers are largely comparable to those for the control group.

## 3.2 External Validity

The external validity of my analysis depends on how well my estimates would generalize to policy-relevant populations of student borrowers. Ideally, the full sample would be representative of the student borrowing population, and the analysis sample would be representative

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<sup>8</sup>Removing agents with few calls reduces measurement error in the agent-score instrument because estimates of the mean taken over a small number of calls are highly imprecise. Restricting the sample to the post-2016 period removes any non-randomly assigned calls placed by older auto-dialing systems.

<sup>9</sup>Note that treatment is defined at the call-level, not the borrower-level. For the borrower-level statistics reported in Table 1, the treatment group consists of all borrowers with *any* treated calls. Also note that 23 percent of the control group does eventually enroll in IDR, though never within four months of a delinquency call included in the balanced panels.

of borrowers who might benefit from IDR. To assess the comparability of my study samples to these respective populations, I make use of a separate, nationally representative dataset from the 2008/2012 Baccalaureate and Beyond Longitudinal Study (B&B).<sup>10</sup> Table A1 provides summary statistics for the full and analysis samples in the LLS data, restricted to include only 2008 graduates, alongside the corresponding statistics for two comparable subsamples of the B&B data. The first sample includes all B&B borrowers who took out federal loans. The second sample includes all B&B borrowers whose reported 2012 incomes and loan balances would have qualified them for reduced payments under IDR. Mean values for variables common to the two data sources are very similar in both comparison samples, suggesting my study sample is largely representative of the policy-relevant population.

While the B&B comparison suggests my study sample is largely representative of comparable cohorts in the larger borrowing population, there are two important caveats concerning external validity. First, individuals in my analysis sample are restricted to those with loans originating prior to 2010. This selection criterion removes many borrowers for whom we would expect IDR to be most effective, as younger borrowers typically have higher debt-to-income ratios. Second, I estimate effects of a specific variant of IDR known as Income-Based Repayment (IBR). While IBR is the largest IDR plan in the U.S. and shares most features with alternatives like Pay-As-You-Earn (PAYE), results may not extend to international IDR plans or hypothetical repayment schemes of policy relevance. In particular, my estimates incorporate the effects of unattractive institutional features like staggered payment adjustments and complicated sign-up procedures that would likely be absent from an ideal system of income-contingent loan repayment (Dynarski and Kreisman, 2013; Barr et al., 2017).

## 4 Empirical Strategy

Consider the following empirical model of borrower  $i$ 's outcomes,  $t$  periods after receiving delinquency call  $c$ :

$$Y_{ict} = \beta_0 + \beta_1 IDR_{ic} + \beta_2 \mathbf{X}_{it} + \epsilon_{ict}, \quad (2)$$

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<sup>10</sup>Provided by the National Center for Education Statistics (NCES), the B&B data include restricted-use administrative loan and financial aid records linked to survey responses for a representative sample of four-year U.S. college graduates in the spring of 2008, followed up in 2011-2012.

where  $Y_{ict}$  denotes the outcome of interest,  $\mathbf{X}_{ict}$  is a vector of borrower control variables,  $IDR_{ic}$  is an indicator for IDR enrollment within four months of the call, and  $\epsilon_{ict}$  is an error term. OLS Estimation of  $\beta_1$  would likely yield biased estimates because preferences over repayment plan choices are correlated with unobserved borrower attributes. To overcome these biases, I employ two complementary empirical strategies. First, I use an instrumental variables (IV) design that exploits the quasi-random assignment of servicing agents to calls. Second, I estimate the difference-in-differences between IDR enrollees and non-enrollees before and after receiving delinquency calls.

## Instrumental Variables

Using a sample of randomized delinquency calls made after 2016, my instrumental variables (IV) design estimates IDR’s effect on monthly repayment outcomes within twenty months of enrollment. I use two instrument instruments in this design, both of which exploit the varying tendencies of quasi-randomly assigned servicing agents to induce IDR take-up among the borrowers they call.

*Agent-Score Instrument.* My first instrument, which I call the “agent-score instrument,” is a leave-one-out measure of agents’ ability to induce IDR enrollment, where post-call enrollment is residualized to account for the timing and ordering of delinquency calls. Similar to measures of residualized judge leniency used by Dahl et al. (2014) and Dobbie et al. (2018), the agent-score instrument removes potential sources of endogeneity arising from agent shift assignment or hiring dates. Specifically,

$$IDR_{ict}^* = IDR_{ict} - \gamma \mathbf{W}_{ict} \quad (3)$$

$$= Z_{icj}^A + \epsilon_{ict}, \quad (4)$$

where  $\mathbf{W}_{ict}$  is a vector of year-by-month, day-of-week, and hour-of-day dummies and  $Z_{icj}^A$  is agent score. I calculate the residualized rate of IDR take-up,  $IDR_{ict}^*$ , using OLS estimates of  $\gamma$  in Equation 3. I then construct agent score  $Z_{icj}^A$  using the leave-one-out mean of this

residualized rate,

$$Z_{icj}^A = \left( \frac{1}{n_j - 1} \right) \left( \sum_{k=0}^{n_j} IDR_{kcj}^* - IDR_{icj}^* \right), \quad (5)$$

where  $n_j$  denotes the number of calls made by agent  $j$ .

The residualized agent-score distribution can be seen in Figure 2. Note that while the two-stage least-squares analysis is conducted on a balanced monthly panel of post-2016 calls, the agent-score instrument is calculated using the larger unbalanced panel of calls satisfying all other sample selection criteria in Section 3. This sample includes calls from 204 different agents in four different call centers. Agents place 246 calls on average to borrowers in the sample, with a median of 157 calls.

Variation in agent score can be driven by several potential sources. Agents can vary in IDR conversion through subtle variations in demeanor or tone, and borrowers often hang up or stop listening depending on the interaction. Conversations with agent supervisors suggest that factors like speech patterns and accents play a large role in keeping borrowers' attention. Agent score may also be influenced by agents' ability to provide clear details regarding plan payments and sign-up instructions. Borrowers must log into the Department of Education website using their social security number, authorize the IRS to transfer their tax return, correctly identify their loan program, and consent to change their payment plan. If an agent fails to properly explain these steps, a borrower may fail to enroll in IDR even if the agent convinces her to do so.

*E-sign Instrument.* Agent score might also be driven by gradual adoption of loan servicing practices or technologies affecting take-up. For example, in 2017 LLS received federal approval to use electronic signature or “e-sign” technology, allowing servicing agents to email pre-populated IDR applications to qualifying borrowers without the need for a separate, physical application through the Department of Education. This technology was rolled out to a subset of call agents as a “pilot experiment” before it was adopted company-wide, creating between-agent variability in IDR sign-up costs for an interim period of five months. Unlike other sources of agent variation, effects through e-sign adoption can be estimated, as I observe which call agents elicited an electronic IDR application. I capture these effects using my “e-sign instrument,”  $Z_{ict}^E$ , which is simply an indicator for whether a call was placed

using e-sign technology.

#### 4.0.1 Identifying Assumptions

In order for two-stage least squares estimates to identify local average treatment effects (LATEs) of IDR take-up, the instruments must satisfy three conditions. First, IDR take-up must vary with agent assignment. Second, agent assignment must correlate with borrower outcomes only through its effect IDR take-up. Third, agents’ tendency to induce IDR take-up must be monotonic across borrowers.

To test the first identifying assumption, I estimate the first-stage relationship between the agent-score instrument and observed IDR enrollment. Specifically I estimate the following model using OLS:

$$IDR_{ict} = \alpha_0 + \alpha_1 Z_{ict} + \alpha_2 \mathbf{X}_{ict} + \epsilon_{ict}. \quad (6)$$

First-stage estimates of  $\alpha_1$  for agent-score and e-sign instruments are equal to 0.98 and 0.11, respectively, with or without borrower controls, and F-statistics on tests of instrument significance equal 168.51 and 46.53 (Tables A2 and A3). Graphical evidence of first-stage effects is provided by Figures 2 and 3, which plot a local linear regression of IDR take-up against the agent-score instrument and monthly IDR enrollment by agents’ e-sign status, respectively.

The second identifying assumption requires that agent assignment be predictive of borrower outcomes only through its impact on repayment plan choice. One way this assumption could be violated would be if different types of borrowers were systematically assigned particular agents. Such violations are effectively ruled out by the automatic dialing mechanism—calls are mechanically assigned at random to the available agents working during a particular shift.<sup>11</sup> However, a more plausible threat to validity concerns the selection of calls into the study sample, which only includes “modeled-out” calls during which agents discussed IDR. If agents experience differential rates of borrower hangup before reaching this stage of the call,

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<sup>11</sup>Note that random assignment does not imply equal probability of assignment—an agent who makes shorter and more frequent phone calls will have a higher rate of availability during her shift. Any given delinquency call will therefore have a higher probability of being assigned to these “quicker” agents. The average call to which such agents are assigned, however, will nonetheless be no different from those calls assigned to relatively “slower” agents who make fewer calls per hour.

the sample would be selected based on agent-specific criteria that could potentially correlate with the instrument and bias my estimates.

To address this concern, I employ a strategy which combines the agent-score instrument construction described above with the selection correction techniques pioneered by Heckman (1979). Specifically, I construct a measure of agent-induced sample-selection propensity by calculating the leave-one-out mean “modeled-out” rate,  $Z_{ict}^M$ , among all calls assigned to the agent on a given call. I perform this calculation on the larger, unconditional sample of 892,529 calls and follow the same procedure as Equations 3 through 5, replacing the treatment variable  $IDR_{ict}$  with  $Modeled_{ict}$ , an indicator for whether borrower  $i$  was “modeled-out” during phone call  $c$ . I then include the sample selection measure  $Z_{ict}^M$  in my instrumental-variables regressions to ensure that assignment of  $Z_{ict}$  is conditionally random.<sup>12</sup>

Table A4 provides empirical evidence that, after correcting for agent modeling propensity and call timing, borrowers do not vary systematically by agent-score or agents’ e-sign status. Column 1 reports results from an OLS regression of realized IDR enrollment against several borrower characteristics and pre-call outcome variables, as well as call-date-and-time fixed effects and modeling propensity  $Z_{ict}^M$ . Not surprisingly, estimates demonstrate non-random selection into IDR; holding date and time of call fixed, IDR enrollees are significantly more likely to be young, female, low-income, and hold lower balances across several types of debt. Columns 2 and 3 report results from OLS regressions of the agent-score instrument and e-sign identifier against the same right-hand side variables. Estimated coefficients on borrower variables in these specifications are statistically indistinguishable from zero, and the F-statistic on tests for whether all borrower variables can jointly predict the instrument is 1.05 for agent score and 1.04 for e-sign.

Even if agents are randomly assigned to borrowers, the exclusion restriction may still be violated if agents can influence borrower outcomes through channels other than repayment plan choice. If, for example, agents who induce high IDR take-up also convince borrowers to make timely payments, two-stage least squares estimates of IDR’s effects on repayment would be biased upwards. While it is impossible to rule out agent effects through non-IDR channels, loan servicing practices suggest that such threats to validity are unlikely. LLS’s delinquency

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<sup>12</sup>I also conduct the IV analysis on the *unconditional* sample with no sample-selection correction, yielding qualitatively similar results to my main specification. See Table A11.



calls are designed solely to provide borrowers with information on their repayment options. Agents provide no advice or counseling to borrowers, and follow a decision tree to present repayment alternatives.

The third identifying assumption requires monotonic agent effects across borrowers. To satisfy this assumption, there can be no borrower for whom a higher-score or e-sign capable agent decreases the likelihood of IDR take-up. Monotonicity would be violated if certain agents “match” well with certain borrowers. For example, if some borrowers respond more favorably to female agents, their take-up may be higher under low-score female agents compared to high-score male agents. The presence of such “defiers” would generate a bias in my LATE estimation, the magnitude of which would increase with the number of defiers and the difference in the marginal treatment effects between defiers and non-defiers (Angrist et al., 1996; Heckman and Vytlačil, 2005).

I implement two partial tests of the monotonicity assumption. First, I estimate the first-stage relationship between my agent-score instrument and IDR take-up within subgroups of my monthly analysis sample. If the monotonicity assumption is satisfied, these estimates should be non-negative for all subsamples. As Table A5 shows, estimated coefficients are positive across a variety of subgroups. Second, I calculate a variety of *group-specific* agent-score instruments, capturing agents’ average IDR inducement rates within observably different subsamples.<sup>13</sup> Monotonicity requires a non-negative relationship between any of these subgroup-specific propensities. Figure A9 reports binned scatter plots and correlation coefficients for several pairwise comparisons of these group-specific instruments computed across the entire analysis sample. I find strongly positive correlations for each pair, suggesting agent inducement is similar across borrower characteristics.

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<sup>13</sup>Group-specific agent-score instruments are calculated as

$$Z_{icj}^g = \left( \frac{1}{n_j^g - 1} \right) \left( \sum_{k=0}^{n_j^g} IDR_{kj}^* - IDR_{ij}^* \mathbf{1}_{\{i \in g\}} \right).$$

For example,  $Z_{icj}^{men}$  is the residualized, leave-one-out propensity of agent  $j$  to induce men into IDR.

## 4.1 Difference-in-Differences

I complement the instrumental variables design described above with a difference-in-differences design that compares pre-/post-call differences in outcomes between borrowers who take up IDR (the “treatment” group) and borrowers who remain in standard repayment plans (the “control” group). In addition to providing a second source of identification for short-term repayment effects, this difference-in-differences design allows me to investigate effects on long-term credit and employment outcomes for delinquency calls placed in 2014 and 2015, a period when some calls may not have been randomized.<sup>14</sup>

Formally, the difference-in-differences specification takes the following form:

$$Y_{ict} = \gamma_i + \gamma_t + \left[ \sum_{\tau \neq -1} \delta_\tau \cdot IDR_{ic} \cdot \mathbf{1}\{t = \tau\} \right] + \beta_1 IDR_{ic} + \beta_2 \mathbf{X}_{ict} + \epsilon_{it}, \quad (7)$$

where  $Y_{ict}$  denotes the outcome of interest,  $\gamma_i$  are individual fixed effects,  $\gamma_t$  are event-time fixed effects,  $IDR_{ic}$  is an indicator for IDR enrollment within four months of the call,  $\mathbf{X}_{ict}$  is a vector of borrower control variables (including call date and time fixed effects),  $\epsilon_{ict}$  is an error term, and  $\delta_\tau$ , the parameters of interest, are coefficients on IDR enrollment status which vary by event time. The specification omits  $\gamma_t$  and  $\delta_\tau$  terms at  $t = -1$ , so estimates can be interpreted relative to the baseline period of one month or year prior to the delinquency call.

Identification in the difference-in-differences specification comes from variation in the propensity to take up IDR following a delinquency call. The identifying assumption is that, holding borrower-specific differences fixed, post-call trends in outcomes would be the same for treatment and control borrowers had neither group taken up IDR. Figures A2 through A6 provide graphical evidence in support of the common-trends assumption. The figures plot mean outcomes for IDR enrollees and non-enrollees relative to call date and normalized by pre-call mean. Trends in pre-call outcomes appear similar between IDR and standard enrollees for several periods, diverging only after receiving the delinquency call. I

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<sup>14</sup>Prior to 2016, LLS used a different auto-dialer to reach customers. While the frequency, timing, and content of calls during this period were unchanged, the details of how that system allocated calls between agents is not available.

also estimate IDR effects in an alternative differences-in-differences specification that controls for group-specific linear trends in months or years prior to call.<sup>15</sup>

Even if IDR and standard borrowers exhibit observably similar pre-trends, difference-in-differences estimates could be biased if treatment and control groups would have responded to delinquency calls differently in the absence of IDR. To address this concern, I develop a placebo test meant to simulate this hypothetical scenario. Many treated borrowers receive one or more “non-converting” calls before their “treatment call” (i.e., the call preceding their IDR enrollment). If, in the absence of IDR, treatment and control borrowers would have responded differently to their  $n$ th delinquency call, they would likely have had different responses to calls 1 through  $n - 1$  as well. Figure A8 plots raw pre- and post-call repayment outcomes for non-IDR control borrowers versus *eventual* IDR borrowers following these earlier “placebo calls” that did *not* induce IDR take-up within the following twelve months. Compared to treatment calls in the main estimation sample, responses to control calls track closely with the control group, suggesting my main difference-in-difference specification captures a pure IDR effect, as opposed to a “call effect.”

Estimates could also be biased if IDR enrollees experienced a shock at the time of a delinquency call that induced them into IDR take-up and influenced outcome variables. I argue that such instances are unlikely. Delinquency calls are *outgoing*, so their incidence is determined by LLS and does not vary systematically between borrowers with observably similar characteristics. If IDR borrowers were enrolling as a response to sudden shocks, outcomes should vary from non-IDR borrowers in the months immediately preceding the call. It is possible that some borrowers make IDR enrollment decisions based on expected *future* shocks to their financial well-being, though it seems unlikely such forward-looking borrowers would take these precautionary measures in response to a delinquency call as opposed to proactive self-enrollment. In any case, IDR benefits are strictly decreasing in income and available credit, so any potential bias created by forward-looking borrowers

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<sup>15</sup>Estimates from the specification including linear pre-trends can be interpreted as IDR’s impact on outcomes relative trend-predicted differences between groups. Formally, the model is given by

$$Y_{ict} = \gamma_i + \delta t \cdot IDR_{ic} \cdot \mathbf{1}\{t < 0\} + \left[ \sum_{\tau \geq 0} \delta_{\tau} \cdot IDR_{ic} \cdot \mathbf{1}\{t = \tau\} \right] + \beta_1 IDR_{ic} + \beta_2 \mathbf{X}_{ict} + \epsilon_{it} \quad (8)$$

should be negative, attenuating any positive treatment effects of IDR.

## 5 Results and Interpretation

### 5.1 Short-Term Outcomes: Repayment and Balances

Figures 4 through 6 plot difference-in-differences and agent-score IV coefficients on minimum payments, loan balances, and indicators for more than 10, more than 90, and more than 270 days delinquent. Left-column graphs plot estimated coefficients on IDR take-up from separate two-stage least-squares regressions in each month using the agent-score instrument.<sup>16</sup> Right-column graphs plot estimated coefficients on the interaction between IDR take-up and months-since-call from the pooled difference-in-differences specification given by Equation 7. IV point estimates for both agent-score and e-sign specifications are reported separately by three-month period in Table A6, and corresponding difference-in-difference estimates are reported in Table A7. All specifications include controls for call date and time, as well as amount borrowed, number of previous calls, inferred gender, and zip-code median income.

*Minimum Payments and Re-enrollment.* The immediate effect of IDR enrollment on minimum payments is mechanical.<sup>17</sup> Nonetheless, estimating the IDR treatment effect on minimum payments can provide useful insight into the “first-stage” effects driving more downstream results.

Both instrumental variables and difference-in-differences estimates of minimum payments effects suggest IDR provides borrowers with large but short-term increases to cash-on-hand. Agent-score IV estimates imply a 86 percent decline in monthly minimums immediately after enrollment, followed by a sharp rise twelve months later. E-sign IV and difference-in-differences strategies find very similar results. As the bottom panel of Figure A1 illustrates,

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<sup>16</sup>Agent-score and e-sign instruments yield similar results, so I focus my attention on the agent-score IV. Monthly e-sign coefficients are plotted in Figure A12.

<sup>17</sup>Given adjusted gross income, family size, and debt balance, one could directly calculate IDR’s effect on payment size using a standard loan amortization formula and Equation 1. For the treatment group in my sample, this effect is approximated by observed IDR payments minus payments in the month prior to receiving the delinquency call. Figure A10 provides a graphical illustration of this measured payment effect across the distribution of IDR enrollees in my analysis sample.

this pattern appears to be driven by a lack of re-enrollment. After one year on IDR, more than sixty percent of enrollees in my sample do not fulfill their income recertification requirements, resulting in a return of minimum payments to their pre-call levels.<sup>18</sup> While a small group of enrollees do eventually recertify, expanding the panel shows that roughly half of initial enrollees have still not recertified by month forty-two (see Figure A11). While some of this attrition may be driven by incomes rising above the reduced-payment-eligibility threshold, the more likely explanation is a behavioral response to the burdensome recertification process required under IDR (Cox et al., 2018).

*Delinquencies.* I measure IDR’s impact on delinquencies using the likelihood of falling more than 10 days delinquent, the likelihood of falling more than 90 days delinquent, and the likelihood of falling more than 270 days delinquent. These three benchmarks reflect points of increased delinquency penalties: At eleven days past due, borrowers begin to accrue late fees for delinquent loans. At 91 days past due, borrowers are reported to credit bureaus. At 271 days past due, a borrower becomes eligible for default. Defaulted loans can result in garnished wages, withheld tax returns, and revocation of professional licenses.

Monthly difference-in-differences and IV estimates, shown in Figure 5, indicate a large negative effect of IDR enrollment for all three delinquency measures in the short term, but attenuate or reverse direction after the twelve-month recertification period. In the difference-in-differences analysis, IDR borrowers are 19 percentage points less likely to fall more than ten days delinquent relative to standard borrowers in the six to eight month period following the delinquency call, with a pre-call mean of 66pp. Estimates remain highly negative at a statistically significant 23pp in months nine through eleven, but begin to attenuate in the subsequent three three-month periods. Corresponding estimates for 90- and 270-day delinquencies exhibit similar patterns, albeit mechanically staggered and smaller in magnitude.

IV estimates for the effect of IDR on delinquencies are broadly consistent with difference-in-differences estimates, though less precise. In months six to eight and nine to eleven, estimates are highly negative and statistically significant for ten-day (−22pp, −22pp) and ninety-day (−5pp, −5pp) delinquencies, while estimates for 270-day delinquencies are sta-

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<sup>18</sup>Technically, standard payments might be higher after a year on IDR because unpaid interest is, under some circumstances, recapitalized into the principal amount.

tistically indistinguishable from zero. In later months, estimates for all three delinquency measures attenuate towards zero or turn positive, indicating an *increase* in non-repayment shortly following the recertification period.

Note that over half of treated borrowers in my sample face IDR payments of zero dollars and thus cannot fall delinquent on their loans. While this mechanical result could still be characterized as a liquidity effect under a neoclassical model, it may be driven in part by psychological frictions or “hassle costs” if borrowers facing payments of  $\epsilon > 0$  dollars would face higher delinquency rates than zero-payment borrowers. To investigate the importance of this channel, I conduct my difference-in-differences analysis among a subsample of individuals with predicted-nonzero IDR payments. Realized IDR payments are nonzero for more than eighty percent of treated individuals in this subsample, yet the repayment effects of IDR persist. Table A12 reports delinquency results for this subsample, and continues to find a large and significant effect on repayment rates.

*Balances.* In theory, IDR could affect balances on student loans in either direction. IDR borrowers face lower monthly minimums payments than those on standard plans, increasing relative balances among those who stay current on their loans. However, IDR borrowers are also more likely to actually *make* their monthly payments, a consideration that is often ignored in fiscal projections of IDR. Figure 6 reports estimated coefficients for student loan balances and monthly changes in balance.<sup>19</sup> In months six through eight, IDR borrowers pay down more debt each month (\$46 for both difference-in-differences and \$35 for IV), but much of those gains are lost by months twelve through fifteen, when their balances begin to *increase* relative to non-IDR borrowers by a monthly average of \$68 for difference-in-differences and \$112 for IV.

My results suggest the effect of reduced minimums on loan balances is dominated by more timely repayment, at least in the short term. While the cumulative effect on balance levels remains negative throughout the panel window, the sharp reversal in effects on changes in balances at the twelve month mark points once again to the negative influence

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<sup>19</sup>Note that, depending on the specific plan and minimum payment amount, IDR borrowers can sometimes receive partial forgiveness on accumulated interest. While effects on balance levels partially reflect these forgiveness provisions, my measure of change in balances removes any interest forgiveness.

of the recertification process on repayment likelihood. However, it is important to note that estimated effects on balances are relative, not absolute. On average, neither standard nor IDR borrowers are decreasing their total balances over the entire period (See Figure A3).

## 5.2 Medium-Term Outcomes: Employment and Zip-Median Income

My data do not include direct measures of employment or income. I can however, construct proxy measures for both variables using the LLS data. For employment, I use the incidence of unemployment deferment. Unemployment deferments provide a six-month pause to student loan payments and most interest accumulation for borrowers working less than thirty hours per week. Qualifying borrowers should prefer unemployment deferment to IDR or standard repayment, as it offers the same or better benefits at a considerably lower take-up cost. For income, I use the median income among households in each borrower’s reported zip code, taken from the 2006-2010 American Community Survey (US Census Bureau, 2010).

Both of these variables carry potential reporting bias. Once a borrower is on standard or IDR payment plans following the phone call, both deferment and zip code are influenced by subsequent contact with LLS. Since such contact is endogenous to initial plan choice and repayment behavior, treatment effects estimated for these outcomes might be biased. For example, standard-plan borrowers receive more follow-up delinquency calls than IDR borrowers for fifteen months following the initial call, giving them more opportunity to update their zip-codes or inquire about unemployment deferment during this period. Such borrowers may have higher *reported* incomes and rates of unemployment, biasing income effects downward and employment effects upward.

To address this concern, I restrict attention to effects in months eighteen and onward, when recertification periods have passed and treatment and control borrowers are equally likely to have had recent contact with LLS.<sup>20</sup> By this time, unemployment effects should be purged of reporting bias, as initial IDR borrowers have reverted to standard payments

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<sup>20</sup>Figure A7 plots average number of additional points of contact for each month relative to the reference call. As expected, rates of contact for IDR borrowers spike during the initial enrollment and re-enrollment periods, differing considerably from non-IDR borrowers during that time period. In later periods, however, contact rates converge, suggesting both groups are equally likely to provide updated employment or zip-code information to LLS during these months.

and earlier deferments have expired, so any lingering effects of disparate rates of contact will have dissipated. For zip-median income, however, the timing of potential biases are more difficult to determine, as outdated zip codes can remain on the books for many months. A higher-income zip code in month twenty may reflect a change in zip code from month ten, so effects in early months should be interpreted with caution. Nonetheless, more than ninety-five percent of borrowers have already recorded at least one change in zip code as of month forty, so late-month estimates likely to reflect effects beyond the potential bias period.

Results for employment and income proxies are reported in Figures 7 and 8. For unemployment deferments, I report both agent-score IV results from the short-term 20-lead panel used in Section 5.1, as well as difference-in-differences results from an expanded panel with 42 leads. For zip-median income, I report only difference-in-differences from the expanded panel, as reporting bias from staggered zip-code updates likely contaminates the entire IV-panel window.

I find no evidence of employment effects. Both IV and difference-in-differences estimates of IDR’s effect on unemployment deferments are statistically indistinguishable from zero for many months following the period of potential reporting bias. By contrast, results suggest IDR borrowers move to marginally higher-income zip codes in the years following enrollment. In month forty-two, zip-median income shows a small increase of 0.6 percent off a pre-call mean of 3.9, and borrowers are 1.8 percentage points more likely to move to a higher-income zip code. These estimates suggest the positive effects of IDR overcome any zip-code-reporting bias, which should be negative if zip-median incomes are rising in general, though it should be emphasized that results for both outcomes should be interpreted with caution given the measurement concerns outlined above.

### 5.3 Long-Term Outcomes: Credit, Homeownership, and Consumption

To investigate effects of IDR on long-term homeownership, and consumption proxies, I shift my focus to calls made in 2014 and 2015, a period when some calls may not have been randomized. I therefore rely solely on the difference-in-difference strategy to estimate effects on these outcomes, as the instrumental variables approach is infeasible.



Figures 9 and 10 plot difference-in-differences estimates of the effect of IDR on credit scores, bankruptcies, mortgages, and auto loans. Plotted points represent the estimated coefficients on IDR in consecutive years from the pooled regression specified in Equation 7, beginning with the year of the delinquency call (“Year 0”), while dashed lines represent corresponding ninety-five percent confidence intervals. Table A8 provides these estimates alongside estimates from a regression which omits pre-call month dummies and includes a linear time trend. All specifications include controls for call date and time, as well as amount borrowed, number of previous calls, inferred gender, and zip-code median income.

Relative to those who remained in standard repayment, borrowers who enrolled in IDR experienced a statistically significant 6.65-point increase in credit scores within one year of the delinquency call off of a pre-call mean of 596.5 points, an increase that persisted for the following four years. Estimates of IDR’s effect on bankruptcy filings and auto loans are statistically indistinguishable from zero for all five years following the delinquency call. IDR’s effects on the likelihood of holding a mortgage are also effectively zero in the year of the call, but rise to 1.9 percentage points by year four, an increase of 9 percent off of the pre-call mean.

Figure 11 and Table A9 provide difference-in-difference estimates for the effect of IDR on credit card balances, number of credit cards, and credit card limits. While I find no significant effects in the year of the delinquency call, IDR is associated with statistically significant increases in all three credit card measures one and two years following the call. Compared to standard borrowers, total balances on credit cards held by IDR enrollees increase by \$238 and \$366 one and two years after the delinquency call, corresponding to an increase of 23 percent off of a pre-call mean of \$1,622. Similarly, by the second year of enrollment, IDR borrowers hold 0.16 more credit cards (pre-call mean of 3.23) and have \$1,105 higher credit limits (pre-call mean of \$5,128) compared to those who remained in standard repayment plans. The positive effects estimated across all three measures are sustained or increased in years three and four.

Credit card results carry two important caveats. First, I have loosely referred to credit card balances as “proxies for consumption,” but balances reported in the credit bureau data capture both flows in credit card spending as well as the stock of unpaid debt. Increased credit card balances arising from more rolled-over debt, as opposed to higher transaction

volumes, might not reflect a consumption response and could decrease borrower welfare. I argue that such increases in accumulated debt are unlikely to drive my credit card results, as accumulated credit-card debt is unlikely to coincide with higher credit scores. However, borrowers who were carrying credit card debt before IDR enrollment may use their increased cash-on-hand to *decrease* those balances. Such a deleveraging response would attenuate the estimated effect on credit card balances, understating the true consumption effect of IDR.

Second, even credit card responses driven entirely by changes in transaction volumes may not reflect changes in consumption. If credit card transactions reflect changes in short-term spending on durable goods, such responses would be more accurately interpreted as “expenditure effects.” For example, if IDR borrowers used their cash-on-hand to expedite purchases on automobiles, furniture, or appliances that they would have eventually bought anyway, their true consumption responses would depend upon the depreciation of these goods between counterfactual purchase dates. While this potential difference between expenditure and consumption effects could be large for results measured within short time horizons, the persistence of estimated credit card responses over three years suggests an true increase in combined durable/non-durable consumption relative to control borrowers.

## 5.4 Interpretation and Alternative Mechanisms

Results for short-term repayment outcomes suggest IDR has a large and immediate increase to cash-on-hand, improving the balance sheets of liquidity-constrained borrowers. In addition to the budgetary implications discussed in the following section, increased repayment rates provide one channel for welfare improvements under IDR, as non-repayment can severely impact borrowers’ credit and employment prospects. These results also speak to the long-standing debate over the determinants of default. Increased repayment following a reduction in minimum payments is suggestive of a liquidity motive for default rather than a strategic motive, as lower monthly payments should not influence strategic default decisions.

Repayment results also highlight potential importance of behavioral barriers and recertification rules in IDR design. The steep increase in payments twelve to fourteen months following the delinquency call corresponds to the one-year recertification period when borrowers are required to provide updated proof of income or revert to standard payment levels.

High attrition from IDR during this period implies many borrowers have failed to recertify. Either their incomes have increased above the level which would make them eligible for reduced payments, the “hassle-costs” of recertification exceed the expected benefit of continuing IDR, or behavioral phenomena like inattention or myopia prevented them providing proof of income. In any case, as I discuss in the following section, IDR’s debt forgiveness provisions imply this apparent lack of re-enrollment carries important implications concerning IDR’s fiscal costs, insurance benefits, and moral-hazard effects.

While monthly payments results show that the increase to cash-on-hand through IDR is short-lived, results for long-term outcomes imply its impacts on consumption, homeownership, and financial health are remarkably persistent. The positive estimated effects of IDR on credit cards, mortgage-holding rates, and zip-median income are suggestive of long-lasting welfare improvements to liquidity-constrained borrowers through two channels—a direct response to the immediate increase in cash-on-hand, and an indirect effect through the increased credit access associated with higher credit scores. This indirect “credit effects” channel may be an important one, as prior research finds a ten-point increase in credit scores can increase credit card balances by more than \$500 one year later (Dobbie et al., 2016). Indeed, credit effects can help explain the rise in zip-median income and reconcile credit card results with student-loan-balance results.<sup>21</sup>

Finally, the discussion above interprets IDR treatment effects as operating through liquidity effects, transferring cash-on-hand *within* borrowers, but debt forgiveness provisions under IDR provide a potential alternative mechanism. If borrowers expect their loans to be forgiven, they may increase repayment to try and qualify for forgiveness or raise short-term consumption out of increases to their expected lifetime wealth. However, as I argue in the following section, the expected future incidence of future loan forgiveness is close to zero. Therefore, the circumstances under which a borrower should expect their loans to be forgiven are exceedingly rare and unlikely to play a significant role in borrowers’ behavior, allowing me to rule out wealth effects as a potential mechanism driving estimated treatment effects.

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<sup>21</sup> Absent credit effects, individuals paying down more student debt should experience a *decrease* in cash-on-hand, driving consumption downward. Credit effects might therefore explain why estimates are positive for both repayment and credit cards, especially since newly-solvent borrowers would experience the largest gains in credit access, though the result could also be explained by the averaging of heterogeneous treatment effects.

## 6 Repayment Predictions and Social Welfare Costs

In the previous section, I argue that treatment effects operate through a liquidity or credit channel, not through wealth effects, because borrowers should not expect their debts to be forgiven. In this section, I formalize that argument by predicting forgiveness likelihood under reasonable projections of borrowers' future incomes, payments, and re-enrollment into IDR. In addition to aiding in the interpretation of Section 5 results, these predictions also provide valuable insight into IDR's fiscal implications and potential moral hazard costs. Indeed, my "back-of-the-envelope" simulations demonstrate how existing estimates of IDR's budgetary impact (Lucas and Moore, 2010; Di and Edmiston, 2017) might be overstated, as they generally assume perfect repayment and zero attrition from the program.

### 6.1 IDR Debt Forgiveness: Fiscal Costs and Moral Hazard

Unlike payment reductions, the forgiveness provisions of IDR are designed to provide "wealth insurance" against lifetime-earnings risk and therefore carry potential costs to social welfare. First, because IDR borrowers pay no "premium" for wealth insurance, the government bears the expected cost of forgiven debts plus some risk premium.<sup>22</sup> Such a transfer would impose a deadweight loss through taxation while carrying ambiguous redistributive consequences. Second, insuring lifetime income for student borrowers can distort labor supply, occupation choice, or college attendance decisions through moral hazard, thereby reducing social welfare. For example, borrowers on IDR may enter riskier or lower-paying professions as a result of being partially insured against income losses.<sup>23</sup> Weighing the costs of these behavioral distortions against the welfare benefits of risk abatement is an important consideration in the design of social insurance programs like unemployment insurance. However, the analysis in this section suggests existing IDR plans do not carry these lifetime insurance benefits or their associated distortionary costs, as few borrowers on IDR should expect their loans to

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<sup>22</sup>While insuring *idiosyncratic* risk between borrowers would be diversified away in large numbers, *systemic* risk could be costly to the government if, for instance, uncertainty over the business cycle makes it difficult to predict the amount of forgiven debt.

<sup>23</sup>Note that increased liquidity may itself affect labor supply, even in the absence of forgiveness or moral hazard. See Chetty (2008) and Shimer and Werning (2008) for discussions of liquidity versus moral hazard in the context of unemployment insurance.

be forgiven.

In order for her loans to be forgiven, a borrower must make three-hundred complete monthly loan payments and still hold an outstanding balance. Standard repayment plans pay off balances after just one-hundred-twenty payments, so a borrower must remain on IDR *and* qualify for substantially reduced payments for twenty-five years before she can have any of her loan forgiven. Neither scenario appears plausible for my sample. Observed and projected re-enrollment rates predict less than ten percent of borrowers in my sample would have spent enough time enrolled in IDR to reach their forgiveness eligibility threshold by age seventy-five.<sup>24</sup> Even if re-enrollment increased or was made automatic, borrowers would have to earn implausibly low incomes for twenty or more years in order to have any remaining balance forgiven. If every treated borrower in my analysis sample earned their current zip code’s median income from month forty-two onward with zero earnings growth, only 18.9 percent of them would have IDR payments low enough to leave a positive forgiveness-eligible balance after twenty-five years (See Figure 13).

These findings suggest the long-term fiscal costs of loan forgiveness through IDR are low. Consistent with the null employment results from Section 5.3, they also imply minimal risk of moral hazard, as borrowers should know will ultimately bear the costs of their own labor supply decisions. Consequently, IDR operates more like “unemployment insurance savings accounts” (UISA’s) (Feldstein and Altman, 1998) than traditional unemployment insurance, smoothing temporary income shocks but offering little insurance to total lifetime earnings.

## 6.2 Potential Cost Savings: Accounting for Repayment Effects in Short-Term Cash Flows

Note that, despite their potential costs to social welfare, policymakers might *want* more generous forgiveness provisions to better insure borrowers against lifetime-earnings risk. Moreover, the subsidized interest rate faced by all student borrowers means that, even in the absence of forgiveness, the government’s accumulated costs of such subsidies might be higher for

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<sup>24</sup>Predictions are formed using estimates from a probit model where IDR enrollment is as a function of inferred gender, age, existing balances, and past recertification behavior. Note that this method overestimates the likelihood of forgiveness, as it assumes IDR payments would never pay down balances. See Figure 12 notes for details.

IDR borrowers if they extend their repayment period. But even if IDR were reformed to promote re-enrollment and forgive more debt, its long-term budgetary consequences relative to standard repayment could be zero or positive, depending on the government’s cost-savings from fewer delinquencies and defaults. As Section 5 illustrates, even though borrowers’ monthly minimums are small while enrolled in IDR, total cash flows through completed payments may *increase* because so many borrowers would not have made timely payments under standard repayment.

In Figure 14, I use IDR’s estimated effect on balances to predict total cash flows for the full representative sample under the counterfactual scenario in which all student borrowers were enrolled in IDR starting January 2013. While extrapolating balance effect estimates to different populations and hypothetical IDR plans carries a number of strong assumptions, the figure demonstrates how increased repayment likelihoods might mitigate many of the budgetary concerns of IDR, at least in the short term. Even in the long term, IDR’s repayment effects may promote cost savings, though they are harder to quantify given the high one-year attrition rate. While defaulted student loans can only be discharged under rare circumstances, the Department of Education still reports a lifetime recovery rate of only eighty percent after accounting for collection costs (Department of Education, 2019). My results suggest a more generous IDR plan might avoid these costs by reducing the number of defaulted loans that are never repaid and avoiding the administrative costs of servicing serially delinquent borrowers.

In short, reasonable predictions of borrowers’ incomes, payments, balances, and re-enrollments suggest IDR’s long-term social welfare costs are low. Barriers to income recertification precludes forgiveness eligibility for most borrowers, and even if these barriers were removed, borrowers would likely pay down their balances before they qualified for debt forgiveness. In the unlikely scenario a borrower consistently re-enrolls in IDR, earns low income for twenty-five years, makes one-hundred-twenty complete monthly payments, and still carries a positive balance, the cost of forgiving that balance could still be outweighed by the savings associated with fewer defaulted loans.

## 7 Conclusion

In this paper, I use administrative student loan servicing data to estimate the causal effect of IDR enrollment on borrower outcomes and its predicted long-run social costs. Exploiting quasi-random assignment of loan-servicing agents to delinquency calls, I find that IDR lowers monthly minimum payments by \$170 within eight months of take-up and reduces delinquencies by 22 percentage points. Despite facing lower monthly minimums, IDR borrowers pay down \$35 *more* student debt each month during this period. Difference-in-differences estimates of long-run effects find that IDR enrollees are 2.0 percentage points more likely to hold mortgages, 1.8 percentage points more likely to move to a higher-income zip code, and hold 0.2 more credit cards than non-enrollees three years after enrollment. By contrast, I find no effects on unemployment deferments, a proxy for borrower employment status.

These results do not appear driven by borrower responses to expected loan forgiveness. Instead, they suggest IDR improves borrower welfare principally through a liquidity channel, providing short-term increases to cash-on-hand during periods of financial distress. Indeed, despite its persistent effects on long-run outcomes, the period of reduced payments under IDR is remarkably short, largely because most IDR borrowers fail to recertify their incomes after one year. The resulting path of payments suggests few loans will ultimately be forgiven under IDR, implying minimal insurance to lifetime earnings and little risk of moral hazard. The low expected incidence of debt forgiveness, combined with IDR's positive effect on repayment likelihood and reduced risk of default, also suggests existing IDR plans impose negligible long-term fiscal costs to the government.

This study carries several lessons for policymakers. First, it demonstrates the benefits of flexible student loan contracts. Relative to standard, flat repayment plans, IDR helps borrowers smooth consumption, invest in homes, and avoid default during periods of financial distress. For many borrowers, these liquidity benefits appear inaccessible through private lending markets, leaving considerable scope for other policies that improve contracts for financing college, particularly those that implicitly extend credit or insurance to the student borrowing population.

Second, my findings demonstrate the importance of considering behavioral phenomena in the design of such contracts. My first-stage estimates of agent-score and e-sign effects on

initial IDR enrollment add to existing evidence on the importance of psychological frictions in student loan borrowing and repayment decisions (Cox et al., 2018; Abraham et al., 2018; Dynarski et al., 2018; Marx and Turner, 2017). More importantly, however, my re-enrollment findings highlight how the *persistence* of such frictions can compound these behavioral effects, as even those who are able to overcome initial IDR enrollment barriers fail to successfully complete the one-year recertification process. If policymakers want IDR to provide more than just short-term increases to cash-on-hand, IDR take-up and re-enrollment must be streamlined or automated.

Third, this study highlights the importance of considering counterfactual repayment behavior when evaluating the budgetary implications of student loan reforms. Programs that offer lower monthly payments and potential debt forgiveness seem expensive, but may be budget neutral or even generate revenue depending on their repayment effects. While the government would eventually incur the cost of any forgiven loans, it would also avoid the costs of defaulted loans that are never repaid and the administrative costs of servicing serially delinquent borrowers. My results suggest that, in the case of IDR, the costs from the former may be small, and savings from the latter may be large.

My findings also raise several questions for future research. First, while this paper documents the ex-post liquidity benefits of IDR on several borrower outcomes, a full accounting of IDR's impact on social welfare must incorporate its effects on ex-ante decisions like college attendance, institution and major, occupation, and principal borrowing amount. Indeed, Abraham et al. (2018) find survey evidence that IDR may influence borrowers' career paths, and a large literature on financial aid has shown that the amount and type of support students receive can affect their decisions both before and during college (Marx and Turner, 2015; Dynarski, 2003). Likewise, the proliferation of IDR may also affect the incentives of post-secondary institutions, which have been shown to strategically respond to students' financing options and labor market conditions (Armona et al., 2018; Turner, 2013).

Second, while the apparent lack of forgiveness-eligible borrowers in my sample provides clear evidence of liquidity benefits, it also leaves unanswered questions regarding the trade-off between insurance value and moral hazard inherent to income-contingent forgiveness policies. These questions hold significant policy importance, as a number of existing and proposed student loan programs offer more generous forgiveness provisions than those under the IDR



plan studied in this paper. For instance, teachers and public service employees can apply for debt forgiveness after only ten years of IDR payments (Department of Education, 2020c), and President Trump has proposed a version of IDR with a fifteen-year forgiveness period (Douglas-Gabriel, 2015). Further empirical work is needed to assess the welfare benefits and potential distortionary effects of these debt-forgiveness policies.

Finally, my analysis focuses on the partial-equilibrium effects of IDR, treating loan terms as policy parameters rather than equilibrium objects. In a private market, one might expect adverse selection into IDR to have general-equilibrium effects on plan-specific interest rates and repayment terms. While such effects are unlikely in the current environment, where loan terms are held fixed by the federal government, the benefits offered by IDR and apparent lack of private alternatives raises the potential for unraveled markets in human capital financing. Further research is needed to determine whether adverse selection can explain the dearth of private contracts offering IDR-like repayment terms.

IDR represents the largest change to higher education financing in more than fifty years. Measuring its impact requires many considerations—the positive externalities of college, the redistributive impact of subsidies, the welfare gains from insuring earnings, and the distortionary costs of income-contingent benefits. While many of these questions remain unanswered, this study provides a crucial first step. These findings speak not only concerns of existing student loan policy, but also to the larger question of how society can best finance investments in human capital.

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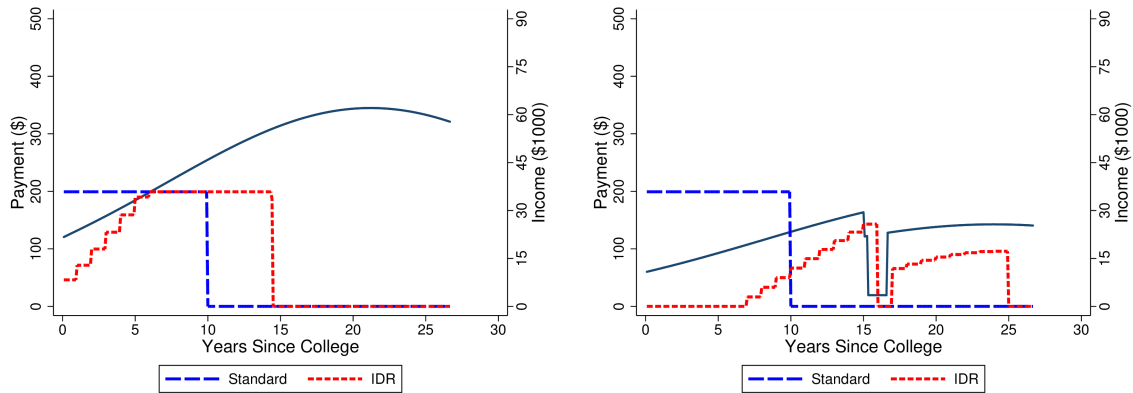
# Tables and Figures

Table 1: Summary Statistics

	Full Sample	Analysis Sample		
	(1)	(2)	(3)	(4)
	Pooled	Pooled	Control	Treatment
<i>Panel A: LLS Data</i>				
IDR	0.135	0.337	0.225	1
Female	0.592	0.693	0.686	0.737
Zip Median Income	61.36	53.21	53.45	51.76
Age	38.17	40.51	40.58	40.15
Amount Borrowed	25.66	22.62	22.42	23.82
10+ Days Delinquent	0.375	0.796	0.803	0.756
90+ Days Delinquent	0.149	0.343	0.351	0.294
Days Delinquent	43.40	89.59	91.56	78.04
<i>Panel B: Credit Data</i>				
Credit Score	679.8	594.8	594.5	596.6
Bankruptcy	0.0832	0.156	0.153	0.176
Derogatory Rating	0.260	0.613	0.610	0.632
Number of Credit Cards	5.376	3.410	3.423	3.333
Credit Card Balances	4.231	1.588	1.617	1.417
Number of Mortgages	1.242	0.789	0.809	0.674
Mortgage Balances	68.96	29.29	30.70	21.13
Credit Card Limits	19.82	5.081	5.179	4.516
Number of Auto Trades	1.972	1.656	1.670	1.576
<i>N</i>	608195	133688	114478	19210

*Note:* This table reports summary statistics at the borrower level. The full sample is a random sample of the population of borrowers in LLS's FFEL portfolio who carried a positive loan balance as of December 31, 2011 and hold no private or Direct loans. The analysis sample is a subsample from the same population, selected according to the following criteria outlined in Section 3. Treated borrowers are those who enroll in IDR within four months of a delinquency call. IDR is an indicator for whether the borrower ever enrolled in IDR. Female is a measure of likelihood-female inferred from first name following Tang et al. (2011). Zip median income is the median 2010 income for the borrower's recorded 5-digit zip code. Days delinquent is the maximum number of days the borrower was ever past due on payments in the past year, and ever delinquent is an indicator for whether days delinquent is greater than 10. Number of calls is the total number of outgoing calls made to the borrower in the past year. IDR and treatment status reflect IDR enrollment histories through September 2019. All other LLS variables are taken from administrative records as of December 31, 2012. Credit scores, bankruptcies, derogatory ratings, credit card, mortgage, and auto loan information are taken from TransUnion credit bureau data collected in August 2012.

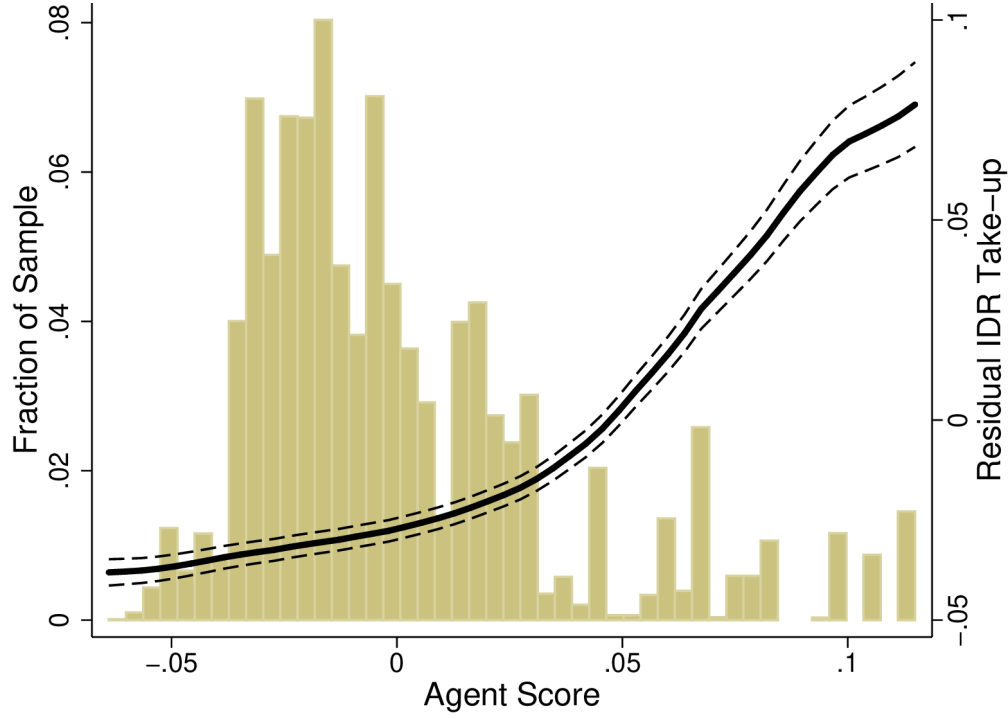
Figure 1: Hypothetical Repayment Scenarios: IDR versus Standard Repayment



*Note:* This figure plots standard and IDR minimum payments under hypothetical income scenarios for a borrower holding \$18,000 of student debt at the time she leaves college. The solid black line, plotted against the right axis, represents annual post-college income. The dashed blue and dotted red lines, plotted against the left axis, represent monthly minimum payments under standard and IDR plans, respectively. The x-axis denotes years since leaving college. Repayment paths assume a 6.0 percent interest rate, no late payments, and no switching between plans.

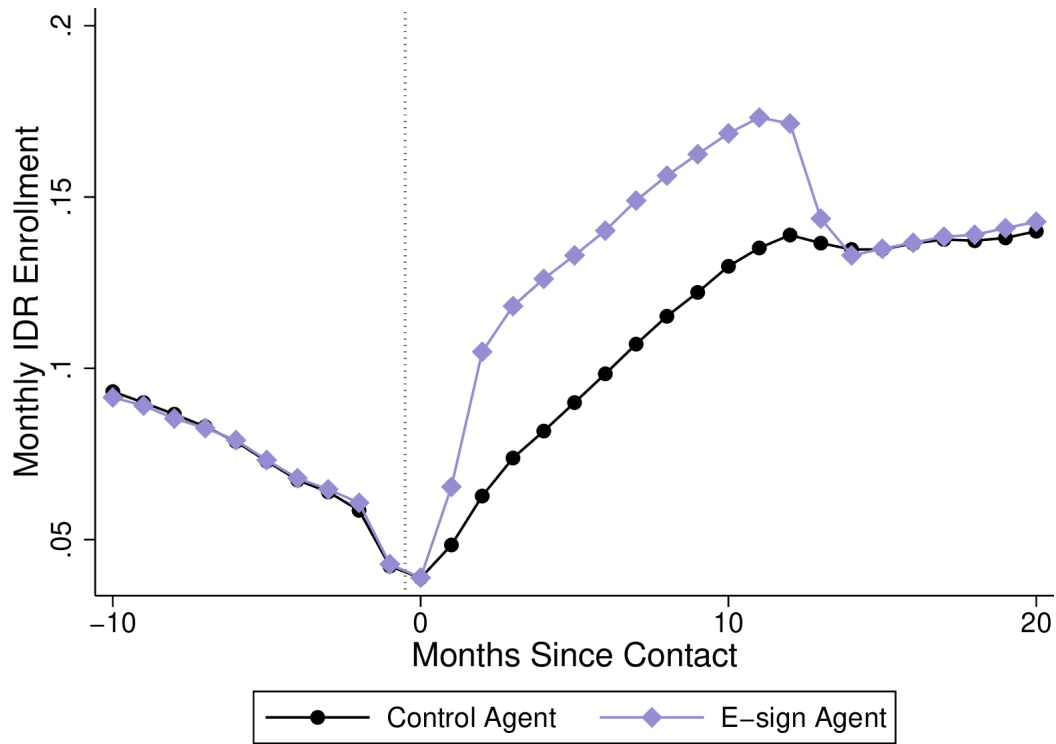


Figure 2: Agent-Score Instrument and IDR Enrollment



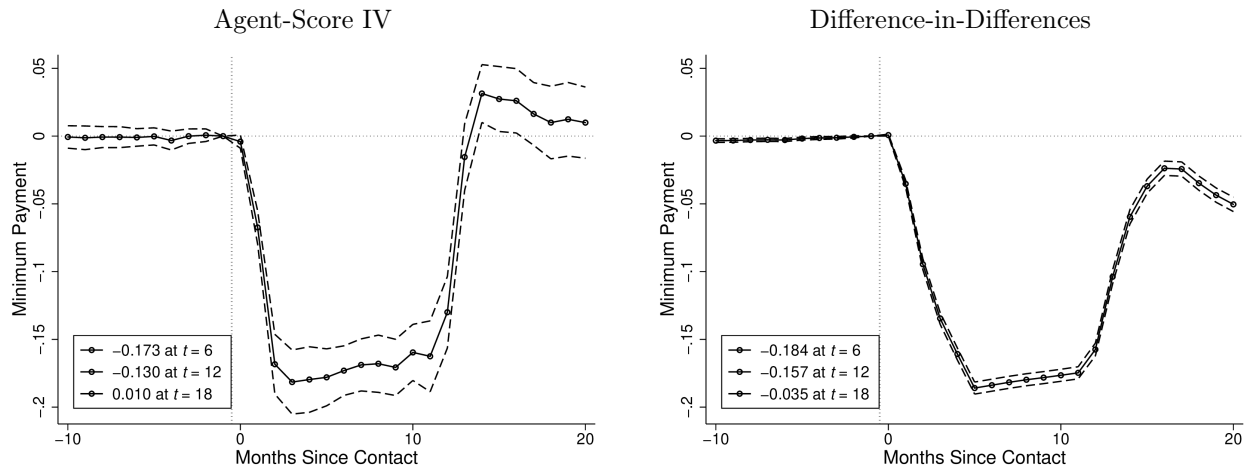
*Note:* This figure reports first-stage effects and distribution of agent scores across delinquency calls, where agent score is the leave-out mean IDR take-up calculated using data from other calls made by the agent following the procedure described in Section 4. The solid and dashed lines, plotted against the right axis, represent predicted means with 95% confidence intervals from a local linear regression of residualized IDR take-up on agent score. The histogram, plotted against the left axis, provides the distribution of agent scores across all delinquency calls in my analysis sample. All regressions include the full set of call date and time fixed effects.

Figure 3: Pre/Post-Call Trends in IDR Enrollment by E-sign status



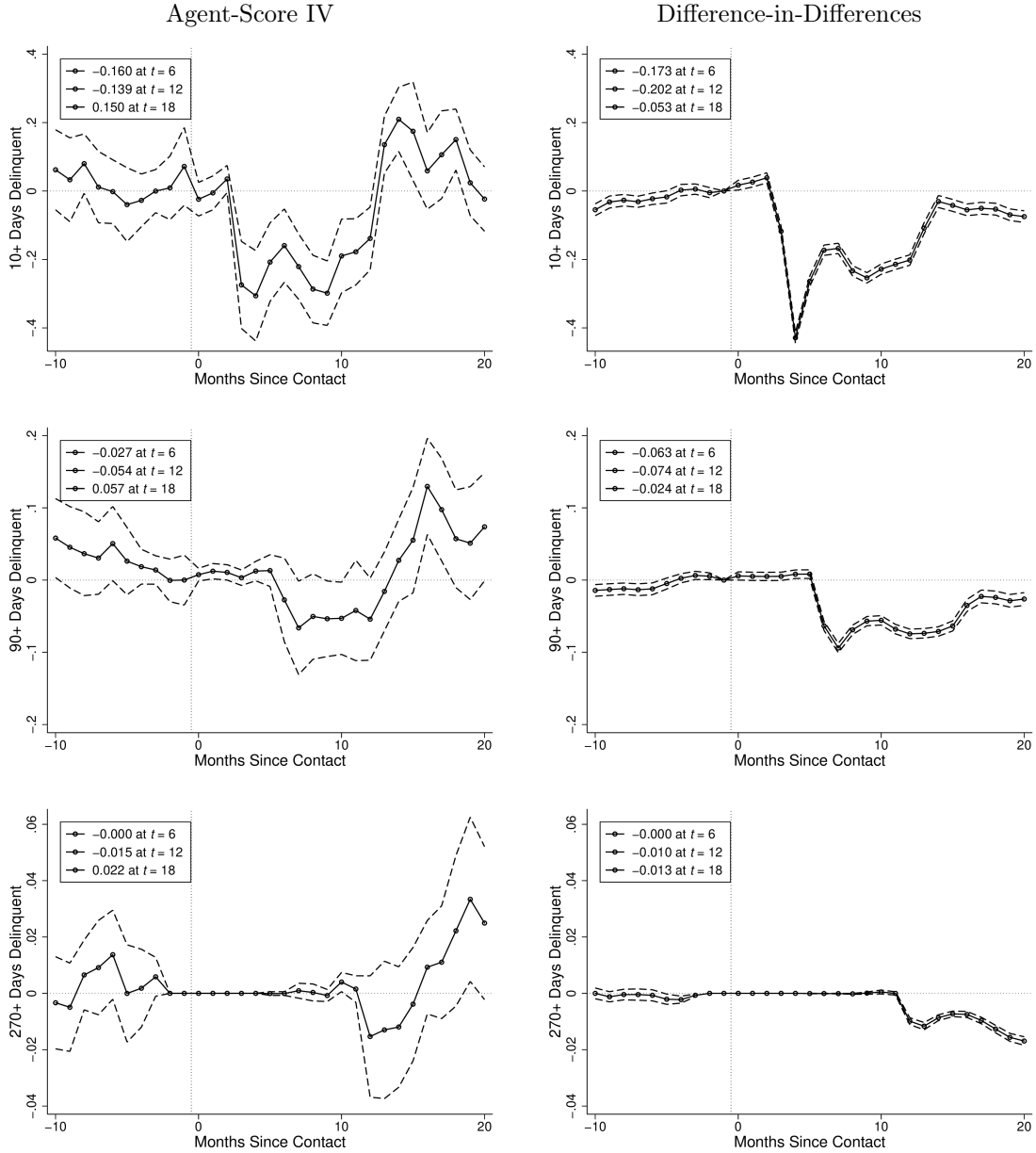
*Note:* This figure plots IDR enrollment status separately by agent e-sign status for all calls placed within the post-2016 twenty-month panel window. The horizontal axis denotes time, in months, relative to the month of the loan servicing call. See Table 1 notes for additional details on the outcome measures and sample.

Figure 4: Estimates of the Effect of IDR Enrollment on Minimum Payments



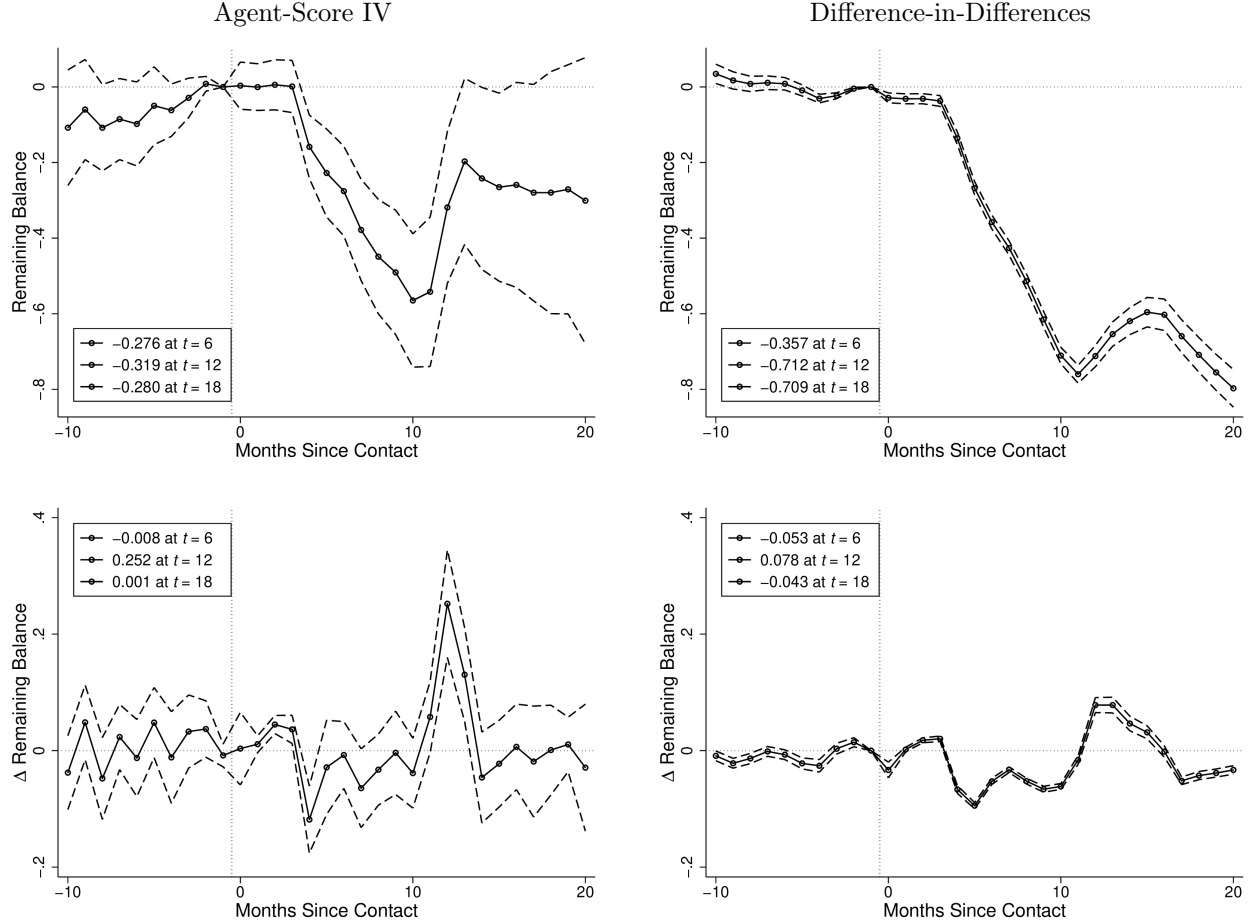
*Note:* This figure reports monthly agent-score two-stage least-squares and difference-in-differences estimates for minimum monthly payments. Each point represents the estimated effect of post-call IDR status on minimum monthly payment at a given time period relative to the date of delinquency call. Relative months are plotted along the x-axis. Dashed lines represent 95% confidence intervals. Boxes list point estimates at selected months. Robust standard errors are two-way clustered at the borrower and agent levels for IV estimates and one-way clustered at the borrower level for difference-in-differences estimates. All regressions include individual and call-date/time fixed effects.

Figure 5: Estimates of the Effect of IDR Enrollment on Delinquencies



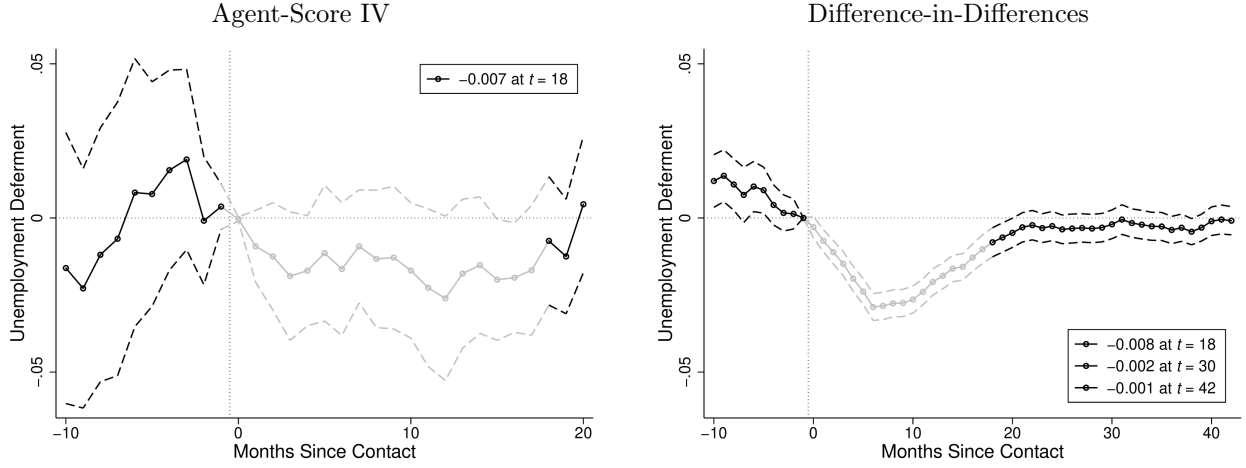
*Note:* This figure reports monthly agent-score two-stage least-squares and difference-in-differences estimates for borrower delinquencies. Each point represents the estimated effect of post-call IDR status on the likelihood of being more than 10, more than 90, and more than 270 days delinquent at a given time period relative to the date of delinquency call. Relative months are plotted along the x-axis. Dashed lines represent 95% confidence intervals. Boxes list point estimates at selected months. Robust standard errors are two-way clustered at the borrower and agent levels for IV estimates and one-way clustered at the borrower level for difference-in-differences estimates. All regressions include individual and call-date/time fixed effects.

Figure 6: Estimates of the Effect of IDR Enrollment on Balances



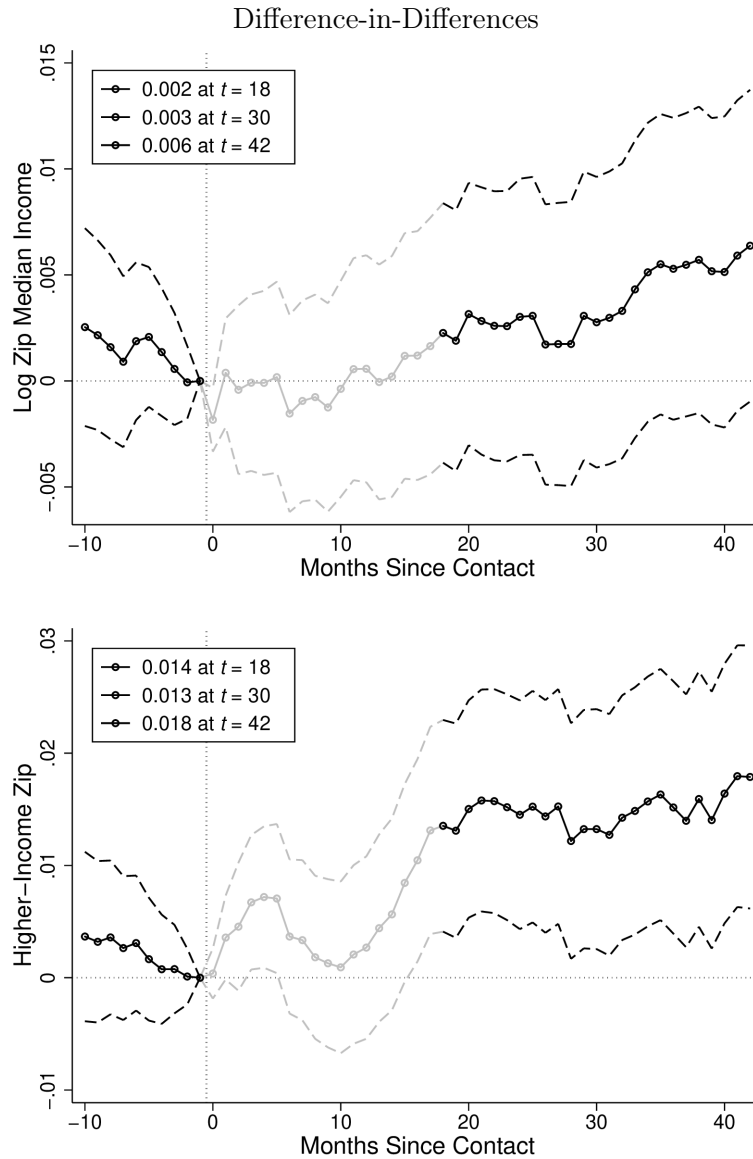
*Note:* This figure reports monthly agent-score two-stage least-squares and difference-in-differences estimates for borrower balances. Each point represents the estimated effect of post-call IDR status on borrowers' month-to-month balance and change in debt balances, respectively, at a given time period relative to the date of delinquency call. Relative months are plotted along the x-axis. Dashed lines represent 95% confidence intervals. Boxes list point estimates at selected months. Robust standard errors are two-way clustered at the borrower and agent levels for IV estimates and one-way clustered at the borrower level for difference-in-differences estimates. All regressions include individual and call-date/time fixed effects.

Figure 7: Estimates of the Effect of IDR Enrollment on Unemployment Deferments



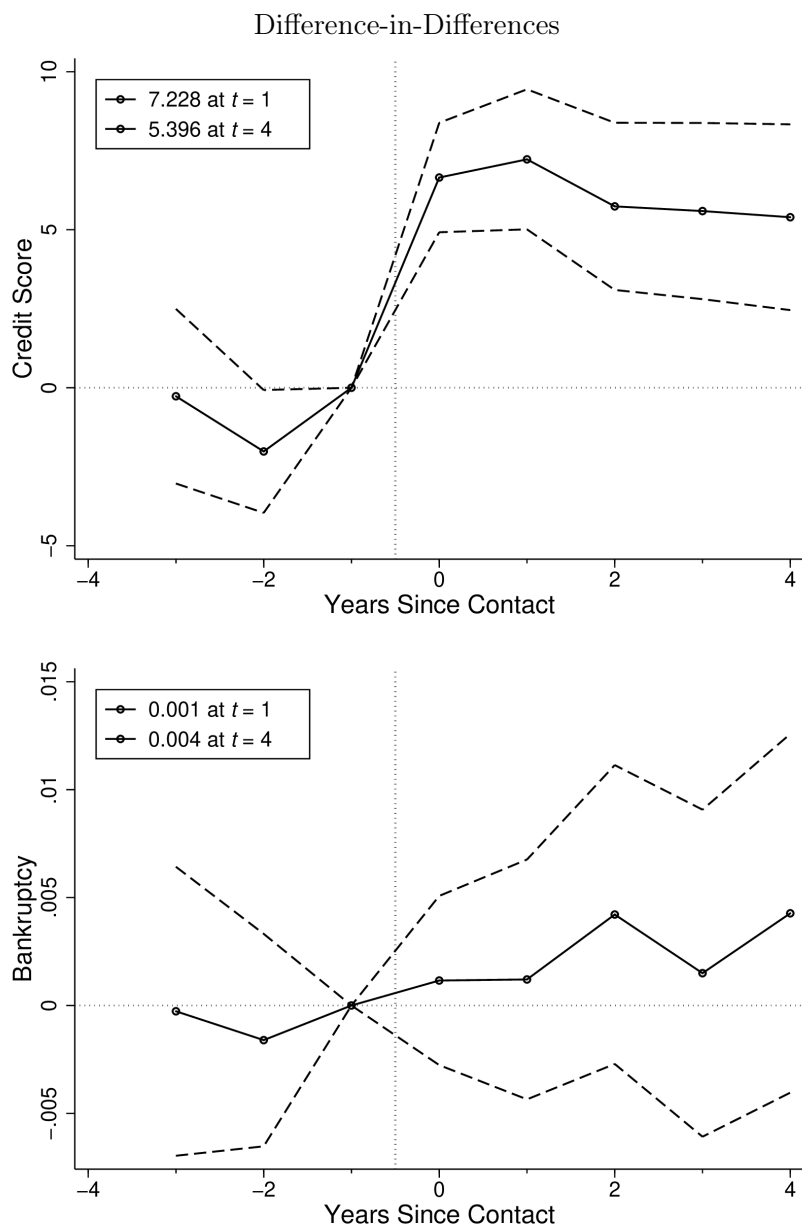
*Note:* This figure reports monthly agent-score two-stage least-squares and difference-in-differences estimates for unemployment deferments. Each point represents the estimated effect of post-call IDR status on take-up of unemployment deferments at a given time period relative to the date of delinquency call. Relative months are plotted along the x-axis. IV results are estimated using a monthly panel with 20 leads and 10 lags, while difference-in-differences results are expanded to a monthly panel of 42 leads and 10 lags. Dashed lines represent 95% confidence intervals. Grey portions of the plot represent periods during which uneven rates of contact with LLS may bias estimates (see discussion in Section 5.2). Boxes list point estimates at selected months. Robust standard errors are two-way clustered at the borrower and agent levels for IV estimates and one-way clustered at the borrower level for difference-in-differences estimates. All regressions include individual and call-date/time fixed effects.

Figure 8: Estimates of the Effect of IDR Enrollment on Zip-Median Income



*Note:* This figure reports monthly difference-in-differences estimates for zip-median income. Each point represents the estimated effect of post-call IDR status on log median income in a borrower's zip-code and a dummy for whether zip-median income exceeds its pre-call level at a given month relative to the date of delinquency call. Relative months are plotted along the x-axis. Results are estimated using an expanded monthly panel of 42 leads and 10 lags. Dashed lines represent 95% confidence intervals. Grey portions of the plot represent periods during which uneven rates of contact with LLS may bias estimates (see discussion in Section 5.2). Boxes list point estimates at selected months. Robust standard errors are clustered at the borrower level. All regressions include individual and call-date/time fixed effects.

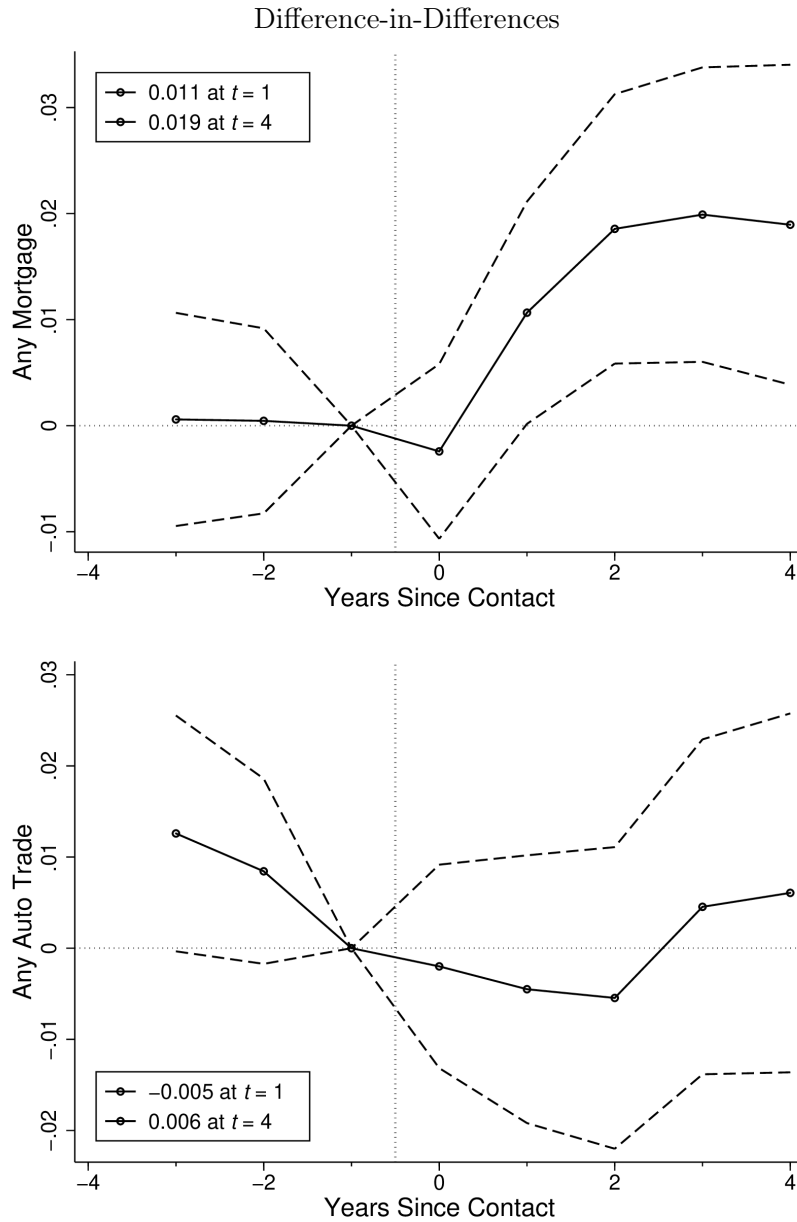
Figure 9: Estimates of the Effect of IDR Enrollment on Credit Scores and Bankruptcies



*Note:* This figure reports annual difference-in-differences estimates for credit scores and bankruptcies. Each point represents the estimated effect of post-call IDR status on credit score or bankruptcy status at a given time period relative to the date of delinquency call. Relative years are plotted along the x-axis. Dashed lines represent 95% confidence intervals using robust standard errors clustered at the borrower level. Boxes list point estimates at selected years. All regressions include individual and call-date/time fixed effects.

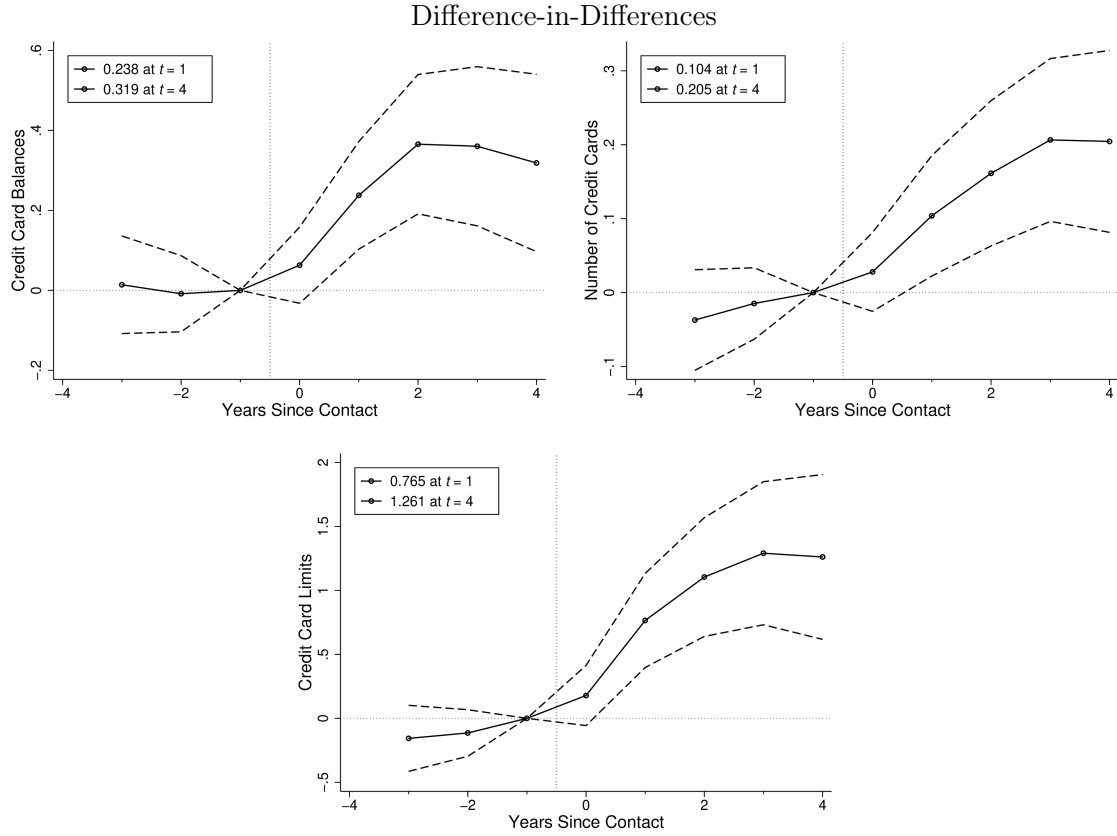


Figure 10: Estimates of the Effect of IDR Enrollment on Mortgages and Auto Loans



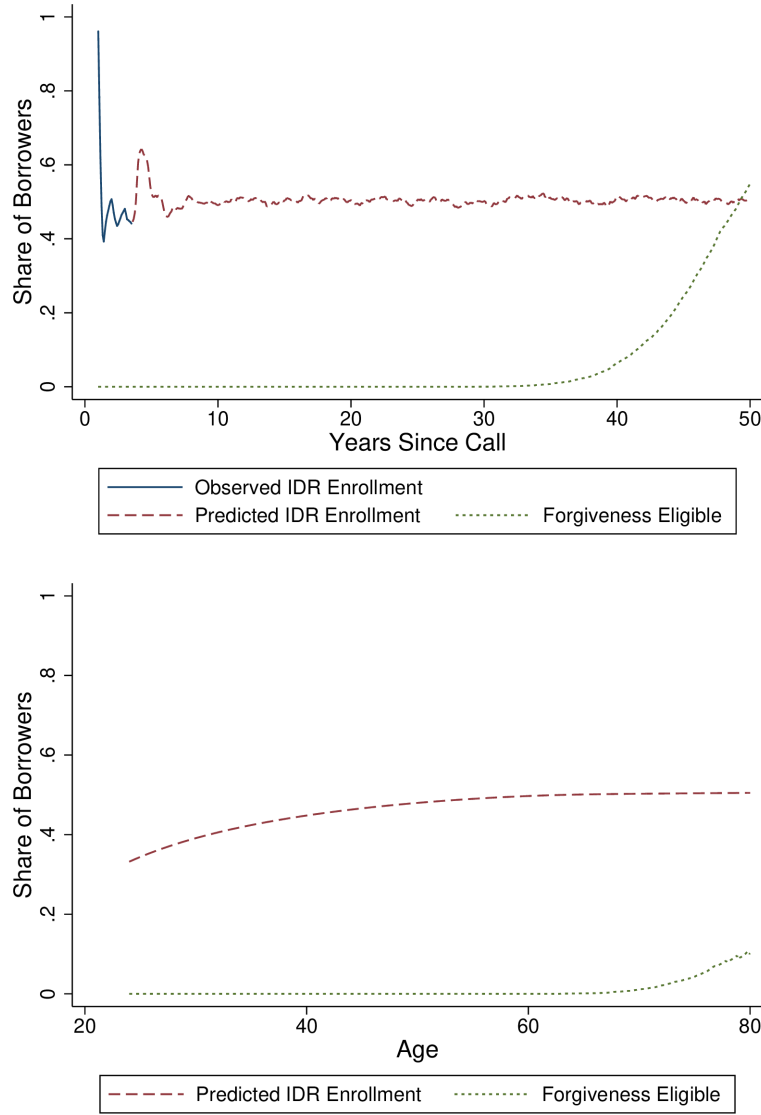
*Note:* This figure reports annual difference-in-differences estimates for borrowers' mortgage- and auto-loan-holding rates. Each point represents the estimated effect of post-call IDR status on the propensity to hold a mortgage or auto loan at a given time period relative to the date of delinquency call. Relative years are plotted along the x-axis. Dashed lines represent 95% confidence intervals using robust standard errors clustered at the borrower level. Boxes list point estimates at selected years. All regressions include individual and call-date/time fixed effects.

Figure 11: Estimates of the Effect of IDR Enrollment on Credit Cards



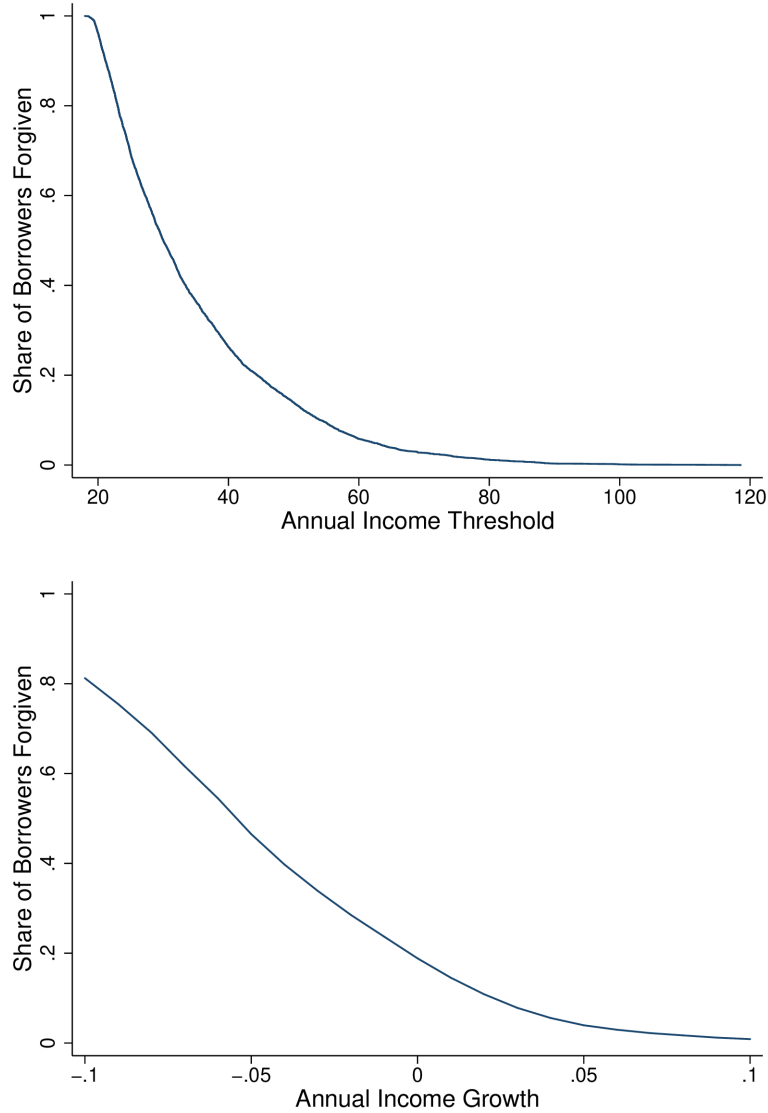
*Note:* This figure reports annual difference-in-differences estimates for credit card balances, number of credit cards, and total credit card limits. Each point represents the estimated effect of post-call IDR status on the total balance, number, and credit limit of all credit cards held by a borrower at a given time period relative to the date of delinquency call. Relative years are plotted along the x-axis. Dashed lines represent 95% confidence intervals using robust standard errors clustered at the borrower level. Boxes list point estimates at selected years. All regressions include individual and call-date/time fixed effects.

Figure 12: Predicted Forgiveness Eligibility



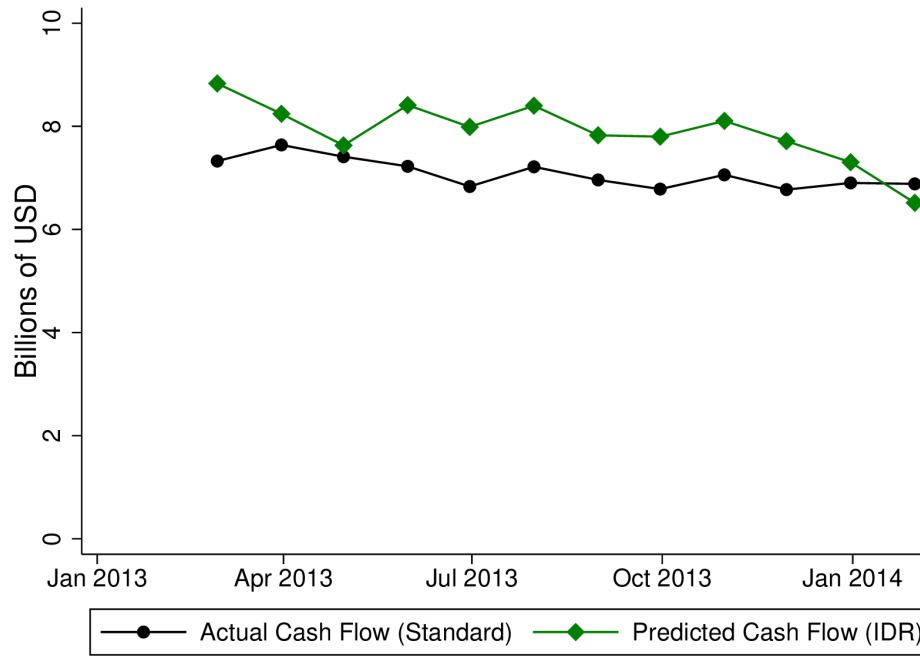
*Note:* This figure plots predicted forgiveness eligibility for my analysis sample, assuming balances are never completely paid off. The blue and red lines plot the true and predicted share of borrowers enrolled in IDR. Predictions are formed from a probit model regressing recertification status against amount borrowed, call-year fixed effects, and a quartic in months since last recertification for those borrowers who have not recertified for at least twelve months. The dotted green line plots the implied share of forgiveness eligible (i.e., the share of borrowers who make at least twenty-five qualifying payments), assuming borrowers recertified at their predicted rate, made all their IDR payments, and never completely paid off their balances. The x-axis denotes years since delinquency call and age of borrower in the top and bottom panels, respectively.

Figure 13: Maximum Qualifying Income



*Note:* This figure plots the share of borrowers in my analysis sample who would have their loans forgiven under different income scenarios, assuming perfect recertification. In the top panel, the y-axis plots the forgiveness rate if everyone in the sample earned the annual income denoted by the corresponding point on the x-axis in every year following month 42. In the bottom panel, the y-axis plots the forgiveness rate if everyone's income started at their current zip code's median in month 42 and grew at the rate denoted by the corresponding point on the x-axis.

Figure 14: Predicted Cash Flows to Government Under IDR



*Note:* This figure plots actual total cash flows versus predicted total cash flows for the counterfactual scenario in which all student borrowers enrolled in IDR in January 2013. Predictions are generated using monthly difference-in-difference estimates for the analysis sample re-weighted so that the joint distribution of pre-call observables matches that of the full representative sample from Table 1. Values are scaled to reflect total national student loan balances as of December 2012.

## Appendix A Additional Tables and Figures

Table A1: Summary Statistics: LLS & Nationally Representative Sample

	All Borrowers		IDR Eligible	
	(1) LLS	(2) B&B	(3) LLS	(4) B&B
Female	0.597	0.602	0.700	0.607
Zip Median Income	60.63	60.47	52.27	59.12
Age	31.97	29.45	34.40	29.40
Amount Borrowed	19.27	19.42	18.63	22.96
Minimum Payment	0.171	0.184	0.180	0.199
Any Mortgage	0.258	0.331	0.156	0.205
Mortgage Balances	48.31	49.70	23.51	25.90
<i>N</i>	271850	8760	43506	2100

*Note:* This table reports summary statistics at the borrower level. The LLS sample (Column 1) is a random sample of the population of borrowers in LLS's FFEL portfolio who graduated in 2008 and made any loan payments from 2010 onward. The B&B sample (Column 2) consists of all student borrowers in the 2008/2012 Baccalaureate and Beyond Longitudinal Study—a separate, nationally representative dataset of four-year college graduates in 2008. B&B data are derived from FAFSA records, the National Student Loan Database System (NSLDS), and survey responses. Variable definitions follow those from Table 1. Values for mortgage, payments, and age variables are taken as of December 2012. Number of observations for the B&B sample are rounded to the nearest ten.

Table A2: First Stage: Agent Score

	(1) IDR	(2) IDR
Agent Score	0.9789864*** (0.0754155)	0.9761268*** (0.0756114)
Female		0.0216765*** (0.0033299)
Amount Borrowed		0.0000862 (0.0001679)
Age		−0.0007326*** (0.0001445)
Lag Log Zip Median Income		−0.0115444*** (0.0039873)
Lag Days Delinquent		−0.0003151*** (0.0000568)
Lag Minimum Payment		−0.0121778 (0.0128935)
Lag Remaining Balance		0.0002079 (0.0001367)
Lag Credit Score		0.0002920*** (0.0000322)
Lag Credit Card Balances		0.0000222 (0.0004448)
Lag Any Auto Trade		−0.0020473** (0.0008739)
Lag Any Mortgage		−0.0119075** (0.0053993)
Lag Mortgage Balances		−0.0001034*** (0.0000262)
Lag Number of Credit Cards		0.0023993*** (0.0005993)
Lag Credit Card Limits		−0.0009289*** (0.0001874)
Mean Dep.	0.101	0.101
F-stat	168.51	166.66
P-value	0.0000	0.0000
R-squared	0.029	0.035
N	50120	50120

*Note:* This table reports first-stage results. The regressions are estimated on the analysis sample described in the notes to Table 1. Columns 1 and 2 report estimated coefficients from an OLS regression of IDR take-up within four months of a delinquency calls against the variables listed, as well as agent modeling propensity and call year, month, and hour fixed effects. Agent score and modeling propensity are estimated using data from other phone calls placed by the same agent following the procedure described in Section 4. Robust standard errors two-way clustered at the borrower and agent level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A3: First Stage: E-Sign

	(1)	(2)
	IDR	IDR
E-sign Agent	0.1068100*** (0.0156584)	0.1066441*** (0.0156585)
Female		0.0219258*** (0.0033921)
Amount Borrowed		0.0000849 (0.0001701)
Age		-0.0007324*** (0.0001475)
Lag Log Zip Median Income		-0.0117649*** (0.0040453)
Lag Days Delinquent		-0.0003163*** (0.0000568)
Lag Minimum Payment		-0.0100604 (0.0130358)
Lag Remaining Balance		0.0001954 (0.0001376)
Lag Credit Score		0.0002878*** (0.0000323)
Lag Credit Card Balances		-0.0000252 (0.0004360)
Lag Any Auto Trade		-0.0021418** (0.0008726)
Lag Any Mortgage		-0.0123738** (0.0054556)
Lag Mortgage Balances		-0.0001004*** (0.0000259)
Lag Number of Credit Cards		0.0023141*** (0.0005965)
Lag Credit Card Limits		-0.0008964*** (0.0001846)
Mean Dep.	0.101	0.101
F-stat	46.53	46.38
P-value	0.0000	0.0000
R-squared	0.028	0.035
N	50120	50120

*Note:* This table reports first-stage results for the instrument defined by call agents' e-sign status. The regressions are estimated on the analysis sample described in the notes to Table 1. Columns 1 and 2 report estimated coefficients from an OLS regression of IDR take-up within four months of a delinquency calls against the variables listed, as well as agent modeling propensity and call year, month, and hour fixed effects. Modeling propensity are estimated using data from other phone calls placed by the same agent following the procedure described in Section 4. Robust standard errors two-way clustered at the borrower and agent level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.



Table A4: Balance Test

	(1)	(2)	(3)
	IDR*100	Agent Score*100	E-sign Agent*100
Female	2.205238*** (0.336733)	0.038508 (0.042312)	0.118663 (0.419120)
Amount Borrowed	0.008472 (0.016898)	-0.000155 (0.001953)	-0.000176 (0.016923)
Age	-0.073510*** (0.014774)	-0.000253 (0.001801)	-0.002483 (0.011765)
Lag Log Zip Median Income	-1.160835*** (0.395556)	-0.006551 (0.033826)	0.146759 (0.405226)
Lag Days Delinquent	-0.030455*** (0.005677)	0.001082 (0.001025)	0.011019 (0.010297)
Lag Minimum Payment	-1.094039 (1.290625)	0.126769 (0.159380)	-0.825126 (1.659435)
Lag Remaining Balance	0.019219 (0.013713)	-0.001612 (0.001338)	-0.003027 (0.017032)
Lag Credit Score	0.029242*** (0.003235)	0.000039 (0.000263)	0.004310 (0.003031)
Lag Credit Card Balances	0.002870 (0.044178)	0.000666 (0.005648)	0.050531 (0.041742)
Lag Any Auto Trade	-0.219860** (0.088141)	-0.015498** (0.007287)	-0.053229 (0.084784)
Lag Any Mortgage	-1.144652** (0.548870)	0.047223 (0.070413)	0.869551 (0.763908)
Lag Mortgage Balances	-0.010703*** (0.002612)	-0.000370 (0.000389)	-0.006245 (0.003788)
Lag Number of Credit Cards	0.232699*** (0.059739)	-0.007409 (0.007921)	0.012128 (0.068323)
Lag Credit Card Limits	-0.094270*** (0.018753)	-0.001414 (0.002104)	-0.043450*** (0.016363)
Mean Dep.	10.106	0.114	12.245
F-stat	22.63	1.05	1.04
P-value	0.0000	0.4025	0.4112
R-squared	0.022	0.016	0.066
N	50120	50120	50120

*Note:* This table reports balance test results. The regressions are estimated on the analysis sample described in the notes to Table 1. Column 1 reports the estimated coefficients from an OLS regression of agent score multiplied by 100 against the variables listed, as well as agent modeling propensity and call year, month, and hour fixed effects. Agent score and modeling propensity are estimated using data from other phone calls placed by the same agent following the procedure described in Section 4. Column 2 reports estimates from an identical regression, except with the dependent variable equal to realized IDR take-up as of six months after the call, multiplied by 100. Robust standard errors two-way clustered at the borrower and agent level are reported in parentheses. The p-value reported at the bottom of columns 1-2 is for an F-test of the joint significance of the variables listed on the left. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A5: First Stage by Subgroup

	Gender		Age		Amount Borrowed		Credit Score	
	(1) IDR	(2) IDR	(3) IDR	(4) IDR	(5) IDR	(6) IDR	(7) IDR	(8) IDR
Agent Score	1.087*** (0.085)	0.711*** (0.103)	0.902*** (0.079)	1.072*** (0.091)	1.127*** (0.155)	0.967*** (0.079)	0.951*** (0.086)	0.996*** (0.084)
Subsample	Women	Men	> 40	≤ 40	> 50K	≤ 50K	> 600	≤ 600
Mean Dep.	0.107	0.086	0.092	0.113	0.103	0.101	0.112	0.093
Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	162.99	47.57	129.69	140.04	52.86	148.80	123.01	140.23
P-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
R-squared	0.037	0.031	0.032	0.039	0.067	0.034	0.039	0.032
N	35562	14558	27826	22294	2974	47146	20993	29127

*Note:* This table reports first-stage results by subgroup. The regressions are estimated on subsamples defined by applying the criteria in the “Subsample” row to the analysis sample described in the notes to Table 1. Agent score is estimated using data from all other phone calls placed by the same agent following the procedure described in Section 4. IDR is an indicator for IDR take-up as of six months after the call. Robust standard errors two-way clustered at the borrower and agent level are reported in parentheses. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A6: Agent and E-sign IV Estimates of the Effect of IDR Enrollment on Repayment Outcomes

<i>Dependent Variable</i>	(1) Mean $t = -1$	Agent Score IV					E-Sign IV				
		(2) Mo. 6-8	(3) Mo. 9-11	(4) Mo. 12-14	(5) Mo. 15-17	(6) Mo. 18-20	(7) Mo. 6-8	(8) Mo. 9-11	(9) Mo. 12-14	(10) Mo. 15-17	(11) Mo. 18-20
Minimum Payment	0.212	-0.170*** (0.010)	-0.164*** (0.011)	-0.038*** (0.011)	0.023** (0.011)	0.011 (0.013)	-0.167*** (0.009)	-0.156*** (0.010)	-0.016 (0.014)	0.048*** (0.015)	0.043*** (0.014)
Remaining Balance	23.819	-0.368*** (0.066)	-0.533*** (0.090)	-0.253** (0.109)	-0.268** (0.135)	-0.284 (0.173)	-0.525*** (0.071)	-0.687*** (0.104)	-0.392*** (0.129)	-0.375** (0.151)	-0.420** (0.179)
$\Delta$ Remaining Balance	0.004	-0.035** (0.015)	0.005 (0.019)	0.112*** (0.020)	-0.012 (0.018)	-0.006 (0.025)	-0.075*** (0.015)	0.017 (0.024)	0.121*** (0.020)	-0.014 (0.019)	-0.017 (0.020)
10+ Days Delinquent	0.659	-0.222*** (0.035)	-0.222*** (0.041)	0.069** (0.032)	0.113** (0.049)	0.050 (0.035)	-0.228*** (0.036)	-0.295*** (0.042)	0.015 (0.036)	0.020 (0.041)	0.034 (0.041)
90+ Days Delinquent	0.046	-0.048* (0.026)	-0.050** (0.023)	-0.014 (0.025)	0.094*** (0.033)	0.061* (0.033)	-0.042 (0.027)	-0.064*** (0.024)	-0.058** (0.025)	0.016 (0.033)	0.001 (0.028)
270+ Days Delinquent	0.000	0.000 (0.001)	0.002 (0.001)	-0.013 (0.010)	0.005 (0.006)	0.027** (0.012)	0.000 (0.001)	0.001 (0.002)	-0.002 (0.011)	-0.010 (0.007)	0.011 (0.012)
Call Time FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$N$	50120	150360	150360	150360	150360	150360	150360	150360	150360	150360	150360

*Note:* This table reports two-stage least squares estimates of the effect of IDR enrollment on monthly loan repayment outcomes. Column 1 reports the dependent variable mean in the month prior to receiving a delinquency call. Each of columns 2-11 reports estimates from a separate 2SLS regression on outcomes in the indicated three-month period following the delinquency call. To instrument for IDR enrollment, columns 2-6 use agent score, as defined in Section 4, while columns 7 - 11 use an indicator for whether the assigned agent was able to facilitate electronic IDR sign-up (“e-sign”). All regressions are estimated on the analysis sample described in the notes to Table 1, limited to a monthly panel with 20 leads and 10 lags. All specifications control for agent modeling propensity following the procedure described in Section 4, as well as call date and time, amount borrowed, number of previous calls, inferred gender, and zip-code median income. Robust standard errors, in parentheses, are clustered at the individual level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A7: Difference-in-Differences and Instrumental Variables Estimates of the Effect of IDR Enrollment on Repayment Outcomes

<i>Dependent Variable</i>	(1) Mean $t = -1$	Diff-in-Diff					Diff-in-Diff w/Trend				
		(2) Mo. 6-8	(3) Mo. 9-11	(4) Mo. 12-14	(5) Mo. 15-17	(6) Mo. 18-20	(7) Mo. 6-8	(8) Mo. 9-11	(9) Mo. 12-14	(10) Mo. 15-17	(11) Mo. 18-20
Minimum Payment	0.212	-0.182*** (0.002)	-0.176*** (0.002)	-0.107*** (0.002)	-0.028*** (0.002)	-0.043*** (0.003)	-0.182*** (0.002)	-0.177*** (0.002)	-0.107*** (0.002)	-0.028*** (0.003)	-0.043*** (0.003)
Remaining Balance	23.819	-0.433*** (0.010)	-0.695*** (0.011)	-0.662*** (0.016)	-0.619*** (0.021)	-0.754*** (0.024)	-0.408*** (0.008)	-0.671*** (0.009)	-0.637*** (0.014)	-0.595*** (0.019)	-0.730*** (0.022)
$\Delta$ Remaining Balance	0.004	-0.046*** (0.002)	-0.048*** (0.002)	0.068*** (0.004)	-0.007** (0.003)	-0.038*** (0.003)	-0.049*** (0.002)	-0.051*** (0.002)	0.065*** (0.004)	-0.010*** (0.003)	-0.041*** (0.002)
10+ Days Delinquent	0.659	-0.191*** (0.007)	-0.232*** (0.007)	-0.114*** (0.007)	-0.049*** (0.008)	-0.066*** (0.008)	-0.205*** (0.005)	-0.246*** (0.005)	-0.127*** (0.006)	-0.063*** (0.006)	-0.079*** (0.006)
90+ Days Delinquent	0.046	-0.075*** (0.003)	-0.060*** (0.003)	-0.073*** (0.003)	-0.040*** (0.004)	-0.026*** (0.004)	-0.083*** (0.002)	-0.068*** (0.002)	-0.081*** (0.002)	-0.048*** (0.003)	-0.034*** (0.004)
270+ Days Delinquent	0.000	-0.000*** (0.000)	0.000 (0.000)	-0.010*** (0.001)	-0.008*** (0.000)	-0.015*** (0.001)	0.001 (0.000)	0.001** (0.000)	-0.009*** (0.001)	-0.007*** (0.001)	-0.014*** (0.001)

*Note:* This table reports difference-in-differences estimates of the effect of IDR enrollment on monthly loan repayment outcomes. Column 1 reports the dependent variable mean in the month prior to receiving a delinquency call. Columns 2-6 report coefficients on the effect of IDR enrollment in consecutive three-month periods following the delinquency call from the pooled OLS regression specified in Equation 7. Regressions are estimated on post-2016 calls from the analysis sample as described in the notes to Table 1, limited to a yearly panel with 20 leads and 10 lags. All specifications include controls for call date and time, as well as amount borrowed, number of previous calls, inferred gender, and zip-code median income. Sample size is 1,553,720 observations from 50,120 calls. Robust standard errors, in parentheses, are clustered at the individual level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A8: Difference-in-Differences Estimates of the Effect of IDR Enrollment on Financial Outcomes

<i>Dependent Variable</i>	(1) Mean $t = -1$	Diff-in-Diff					Diff-in-Diff w/Trend				
		(2) Year 0	(3) Year 1	(4) Year 2	(5) Year 3	(6) Year 4	(7) Year 0	(8) Year 1	(9) Year 2	(10) Year 3	(11) Year 4
Credit Score	596.524	6.652*** (0.883)	7.228*** (1.131)	5.740*** (1.350)	5.591*** (1.422)	5.396*** (1.500)	7.144*** (1.297)	7.720*** (1.494)	6.232*** (1.675)	6.083*** (1.737)	5.888*** (1.813)
Bankruptcy	0.181	0.001 (0.002)	0.001 (0.003)	0.004 (0.004)	0.001 (0.004)	0.004 (0.004)	0.002 (0.003)	0.002 (0.003)	0.005 (0.004)	0.002 (0.004)	0.005 (0.005)
Any Mortgage	0.223	-0.002 (0.004)	0.011** (0.005)	0.019*** (0.006)	0.020*** (0.007)	0.019** (0.008)	-0.002 (0.005)	0.011* (0.006)	0.019** (0.007)	0.020** (0.008)	0.019** (0.008)
Any Auto Trade	0.710	-0.002 (0.006)	-0.005 (0.007)	-0.005 (0.008)	0.005 (0.009)	0.006 (0.010)	0.004 (0.007)	0.001 (0.008)	0.000 (0.009)	0.010 (0.010)	0.012 (0.011)

*Note:* This table reports difference-in-differences estimates of the effect of IDR enrollment on yearly financial outcomes. Column 1 reports the dependent variable mean in the year prior to receiving a delinquency call. Columns 2-6 report coefficients on the effect of IDR enrollment in consecutive years following the delinquency call from the pooled OLS regression specified in Equation 7. Columns 7 - 11 report coefficients on the same yearly effect for a regression which omits pre-call year dummies and includes a linear time trend. The regressions are estimated on the analysis sample as described in the notes to Table 1, limited to a yearly panel with 4 leads and 3 lags. Both specifications include controls for call date and time, as well as amount borrowed, number of previous calls, inferred gender, and zip-code median income. Sample size is 183,232 observations from 22,904 calls. Robust standard errors, in parentheses, are clustered at the individual level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A9: Difference-in-Differences Estimates of the Effect of IDR Enrollment on Credit Cards

<i>Dependent Variable</i>	(1) Mean $t = -1$	Diff-in-Diff					Diff-in-Diff w/Trend				
		(2) Year 0	(3) Year 1	(4) Year 2	(5) Year 3	(6) Year 4	(7) Year 0	(8) Year 1	(9) Year 2	(10) Year 3	(11) Year 4
Credit Card Balances	1.622	0.063 (0.049)	0.238*** (0.069)	0.366*** (0.089)	0.360*** (0.102)	0.319*** (0.113)	0.075 (0.060)	0.250*** (0.078)	0.378*** (0.097)	0.373*** (0.110)	0.331*** (0.120)
Log Credit Card Balances	-2.302	0.034 (0.044)	0.261*** (0.056)	0.264*** (0.064)	0.239*** (0.067)	0.176** (0.070)	-0.004 (0.055)	0.223*** (0.067)	0.226*** (0.075)	0.201*** (0.078)	0.137* (0.080)
Number of Credit Cards	3.230	0.028 (0.027)	0.104** (0.042)	0.161*** (0.050)	0.207*** (0.056)	0.205*** (0.063)	0.008 (0.030)	0.084* (0.043)	0.142*** (0.051)	0.187*** (0.057)	0.185*** (0.063)
Credit Card Limits	5.128	0.179 (0.120)	0.765*** (0.188)	1.105*** (0.237)	1.290*** (0.285)	1.261*** (0.329)	0.112 (0.140)	0.699*** (0.203)	1.038*** (0.250)	1.224*** (0.296)	1.195*** (0.336)

*Note:* This table reports difference-in-differences estimates of the effect of IDR enrollment on yearly credit card outcomes. Column 1 reports the dependent variable mean in the year prior to receiving a delinquency call. Columns 2-6 report coefficients on the effect of IDR enrollment in consecutive years following the delinquency call from the pooled OLS regression specified in Equation 7. Columns 7 - 11 report coefficients on the same yearly effect for a regression which omits pre-call year dummies and includes a linear time trend. The regressions are estimated on the analysis sample as described in the notes to Table 1, limited to a yearly panel with 4 leads and 3 lags. Both specifications include controls for call date and time, as well as amount borrowed, number of previous calls, inferred gender, and zip-code median income. Sample size is 183,232 observations from 22,904 calls. Robust standard errors, in parentheses, are clustered at the individual level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A10: Difference-in-Differences Estimates of the Effect of IDR Enrollment on Employment Outcomes

<i>Dependent Variable</i>	(1) Mean $t = -1$	Diff-in-Diff			Diff-in-Diff w/Trend		
		(2) Year 1	(3) Year 2	(4) Year 3	(5) Year 1	(6) Year 2	(7) Year 3
Unemployment Deferment	0.013	-0.008*** (0.002)	-0.002 (0.002)	-0.001 (0.002)	-0.007* (0.004)	-0.001 (0.004)	0.000 (0.004)
Higher-Income Zip	0.000	0.014*** (0.005)	0.013** (0.005)	0.018*** (0.006)	0.014*** (0.005)	0.014** (0.006)	0.018*** (0.006)
Log Zip Median Income	3.904	0.002 (0.003)	0.003 (0.003)	0.006* (0.004)	0.002 (0.003)	0.003 (0.004)	0.006 (0.004)

*Note:* This table reports difference-in-differences and two-stage least squares estimates of the effect of IDR enrollment on unemployment deferments and median zip-code income. Column 1 reports the dependent variable mean in the month prior to receiving a delinquency call. Columns 2-8 report coefficients on the effect of IDR enrollment in month 18 (“Year 1”), month 30 (“Year 2”), and month 42 (“Year 3”) from the pooled OLS regression specified in Equation 7. Regressions are estimated on the analysis sample as described in the notes to Table 1, limited to a monthly panel with 42 leads and 10 lags. Sample size is 620,208 observations from 47,724 calls. Both specifications include controls for call date and time, as well as amount borrowed, number of previous calls, and inferred gender. Robust standard errors, in parentheses, are clustered at the individual level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A11: Difference-in-Differences and Instrumental Variables Estimates of the Effect of IDR Enrollment on Repayment Outcomes: Including Non-Modeled Borrowers

<i>Dependent Variable</i>	Difference-in-Differences						Instrumental Variables				
	(1) Mean $t = -1$	(2) Mo. 6-8	(3) Mo. 9-11	(4) Mo. 12-14	(5) Mo. 15-17	(6) Mo. 18-20	(7) Mo. 6-8	(8) Mo. 9-11	(9) Mo. 12-14	(10) Mo. 15-17	(11) Mo. 18-20
Minimum Payment	0.182	-0.171*** (0.002)	-0.170*** (0.002)	-0.111*** (0.002)	-0.038*** (0.002)	-0.049*** (0.002)	-0.140*** (0.012)	-0.130*** (0.011)	-0.004 (0.013)	0.045*** (0.015)	0.028* (0.016)
Remaining Balance	20.768	-0.029*** (0.008)	-0.153*** (0.009)	-0.016 (0.013)	0.141*** (0.017)	0.136*** (0.020)	-0.248 (0.188)	-0.429* (0.249)	-0.103 (0.298)	-0.205 (0.336)	-0.256 (0.404)
$\Delta$ Remaining Balance	-0.003	-0.014*** (0.002)	-0.010*** (0.002)	0.094*** (0.003)	0.030*** (0.003)	-0.001 (0.002)	-0.042 (0.026)	0.006 (0.031)	0.106*** (0.025)	-0.049* (0.030)	-0.021 (0.035)
10+ Days Delinquent	0.647	-0.191*** (0.006)	-0.211*** (0.006)	-0.113*** (0.006)	-0.044*** (0.007)	-0.057*** (0.007)	-0.220*** (0.068)	-0.273*** (0.082)	0.023 (0.054)	0.196** (0.088)	0.100* (0.055)
90+ Days Delinquent	0.037	-0.058*** (0.003)	-0.055*** (0.003)	-0.064*** (0.003)	-0.037*** (0.003)	-0.019*** (0.004)	-0.014 (0.041)	-0.071 (0.048)	-0.017 (0.052)	0.110** (0.049)	0.080 (0.053)
270+ Days Delinquent	0.000	-0.001*** (0.000)	-0.001*** (0.000)	-0.005*** (0.000)	-0.006*** (0.000)	-0.011*** (0.000)	0.001 (0.005)	-0.001 (0.006)	-0.015 (0.014)	-0.003 (0.014)	0.002 (0.017)
Call Time FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$N$	160915	4988365	4988365	4988365	4988365	4988365	474147	474147	474147	474147	474147

*Note:* This table reports difference-in-differences and two-stage least squares estimates of the effect of IDR enrollment on monthly loan repayment outcomes following both modeled and non-modeled delinquency calls. Column 1 reports the dependent variable mean in the month prior to receiving a delinquency call. Columns 2-6 report coefficients on the effect of IDR enrollment in consecutive three-month periods following the delinquency call from the pooled OLS regression specified in Equation 7. Each of Columns 7 - 11 report estimates from separate two-stage least squares regressions on outcomes in the same months. Regressions are estimated on the sample of both modeled and non-modeled calls satisfying all other selection criteria outlined in Section 3, limited to a monthly panel with 20 leads and 10 lags. Two-stage least squares models instrument for IDR enrollment with the agent score calculated using the procedure described in Section 4. All specifications include controls for call date and time, as well as amount borrowed, number of previous calls, inferred gender, and zip-code median income. Robust standard errors, in parentheses, are clustered at the individual level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.



Table A12: Difference-in-Differences Estimates of the Effect of IDR Enrollment on Repayment Outcomes: Predicted Non-Zero Payments

<i>Dependent Variable</i>	Difference-in-Differences						Instrumental Variables				
	(1) Mean $t = -1$	(2) Mo. 6-8	(3) Mo. 9-11	(4) Mo. 12-14	(5) Mo. 15-17	(6) Mo. 18-20	(7) Mo. 6-8	(8) Mo. 9-11	(9) Mo. 12-14	(10) Mo. 15-17	(11) Mo. 18-20
Minimum Payment	0.579	-0.386*** (0.014)	-0.374*** (0.014)	-0.252*** (0.014)	-0.105*** (0.016)	-0.133*** (0.016)	-0.405*** (0.076)	-0.404*** (0.079)	-0.171* (0.103)	0.040 (0.101)	-0.048 (0.111)
Remaining Balance	49.672	-0.916*** (0.047)	-1.448*** (0.055)	-1.443*** (0.080)	-1.380*** (0.106)	-1.648*** (0.123)	-0.679* (0.404)	-0.923 (0.577)	-0.053 (0.664)	-0.413 (0.795)	-0.176 (0.929)
$\Delta$ Remaining Balance	0.002	-0.118*** (0.009)	-0.131*** (0.011)	0.099*** (0.021)	-0.018 (0.015)	-0.078*** (0.012)	-0.088 (0.118)	-0.070 (0.130)	0.333** (0.132)	-0.192 (0.119)	0.113 (0.113)
10+ Days Delinquent	0.713	-0.156*** (0.025)	-0.186*** (0.025)	-0.087*** (0.026)	-0.027 (0.028)	-0.040 (0.028)	-0.068 (0.164)	-0.286 (0.175)	0.157 (0.133)	0.128 (0.158)	-0.058 (0.160)
90+ Days Delinquent	0.063	-0.058*** (0.013)	-0.050*** (0.013)	-0.054*** (0.013)	-0.022 (0.014)	-0.022 (0.016)	0.107 (0.112)	0.113 (0.104)	0.062 (0.131)	0.051 (0.123)	0.036 (0.132)
270+ Days Delinquent	0.000	-0.000 (.)	-0.000 (.)	-0.005* (0.003)	-0.002 (0.003)	-0.007** (0.003)	0.000 (.)	0.000 (.)	0.030 (0.046)	0.011 (0.028)	0.027 (0.057)
Call Time FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$N$	4095	126945	126945	126945	126945	126945	12285	12285	12285	12285	12285

*Note:* This table reports difference-in-differences estimates of the effect of IDR enrollment on monthly loan repayment outcomes for those predicted to have non-zero IDR payments. Column 1 reports the dependent variable mean in the month prior to receiving a delinquency call. Columns 2-6 report coefficients on the effect of IDR enrollment in consecutive three-month periods following the delinquency call from the pooled OLS regression specified in Equation 7. Each of Columns 7 - 11 report coefficients on the same monthly effect for a regression which omits pre-call month dummies and includes a linear time trend. The regressions are estimated a subsample of the analysis sample defined by the following procedure: First, I regress an indicator for positive IDR payments among enrollees on a full set of demographic controls and pre-call student loan and credit variables. Second, I use these estimates to predict the likelihood of having positive IDR payments among *all* borrowers in the analysis sample. I then restrict the subsample to those individuals with greater than fifty percent predicted likelihood of positive IDR payments. Both specifications include controls for call date and time, as well as amount borrowed, number of previous calls, inferred gender, and zip-code median income. Sample size is 126,945 observations from 4,095 calls. Robust standard errors, in parentheses, are clustered at the individual level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A13: Placebo Difference-in-Differences Estimates: Financial Outcomes

<i>Dependent Variable</i>	(1) Mean $t = -1$	Diff-in-Diff	
		(2) Year 0	(3) Year 1
Credit Score	594.829	-0.062 (1.520)	1.517 (1.864)
Bankruptcy	0.174	-0.001 (0.003)	0.003 (0.004)
Any Mortgage	0.206	-0.004 (0.008)	-0.007 (0.009)
Any Auto Trade	0.709	-0.009 (0.011)	-0.006 (0.013)

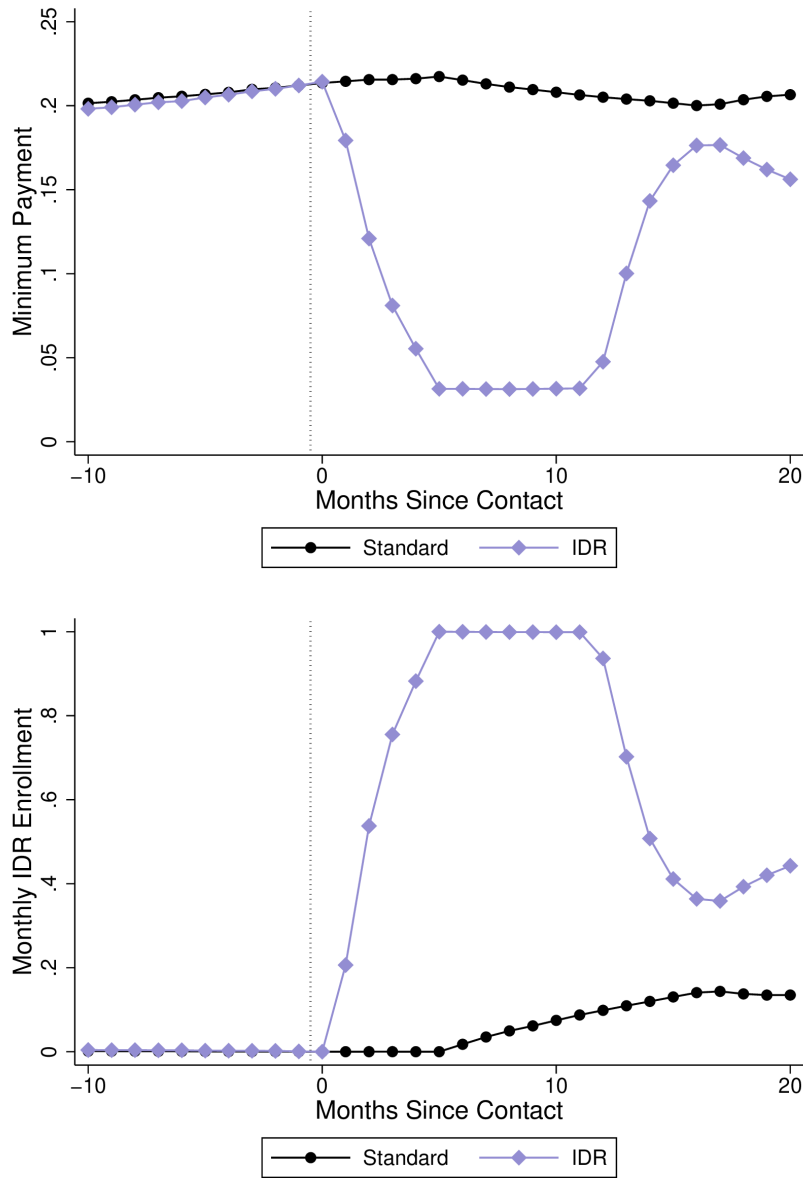
*Note:* This table reports difference-in-differences estimates of the effect of IDR enrollment on yearly financial outcomes for only non-IDR calls. Column 1 reports the dependent variable mean in the year prior to receiving a delinquency call. Column 2 reports coefficients on the effect of IDR enrollment the year of the delinquency call from the pooled OLS regression specified in Equation 7. The regressions are estimated on the subsample of calls for which no borrowers enrolled in IDR during the call-panel window, but some borrowers eventually enrolled following later delinquency calls. Regression includes controls from call date and time, as well as amount borrowed, number of previous calls, inferred gender, and zip-code median income. Sample size is 183,232 observations from 22,904 calls. Robust standard errors, in parentheses, are clustered at the individual level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Table A14: Placebo Difference-in-Differences Estimates: Credit Cards

<i>Dependent Variable</i>	(1) Mean $t = -1$	Diff-in-Diff	
		(2) Year 0	(3) Year 1
Credit Card Balances	1.771	0.063 (0.065)	0.102 (0.105)
Log Credit Card Balances	-2.210	0.130** (0.061)	0.147 (0.091)
Number of Credit Cards	2.923	-0.064* (0.038)	-0.091 (0.061)
Credit Card Limits	5.361	0.100 (0.152)	0.077 (0.244)

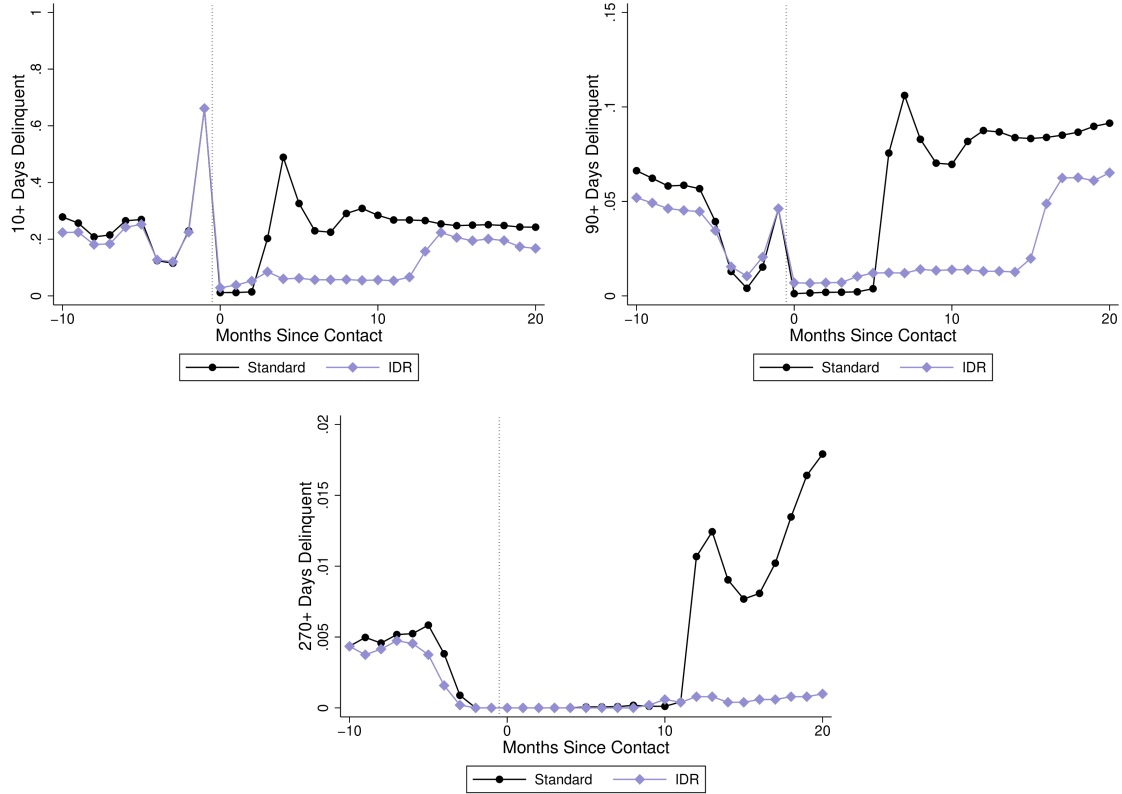
*Note:* This table reports difference-in-differences estimates of the effect of IDR enrollment on yearly financial outcomes for only non-IDR calls. Column 1 reports the dependent variable mean in the year prior to receiving a delinquency call. Column 2 reports coefficients on the effect of IDR enrollment the year of the delinquency call from the pooled OLS regression specified in Equation 7. The regressions are estimated on the subsample of calls for which no borrowers enrolled in IDR during the call-panel window, but some borrowers eventually enrolled following later delinquency calls. Regression includes controls from call date and time, as well as amount borrowed, number of previous calls, inferred gender, and zip-code median income. Sample size is 183,232 observations from 22,904 calls. Robust standard errors, in parentheses, are clustered at the individual level. \*\*\* = significant at 1 percent level, \*\* = significant at 5 percent level, \* = significant at 10 percent level.

Figure A1: Pre/Post-Call Trends in Minimum Payments and IDR Enrollment



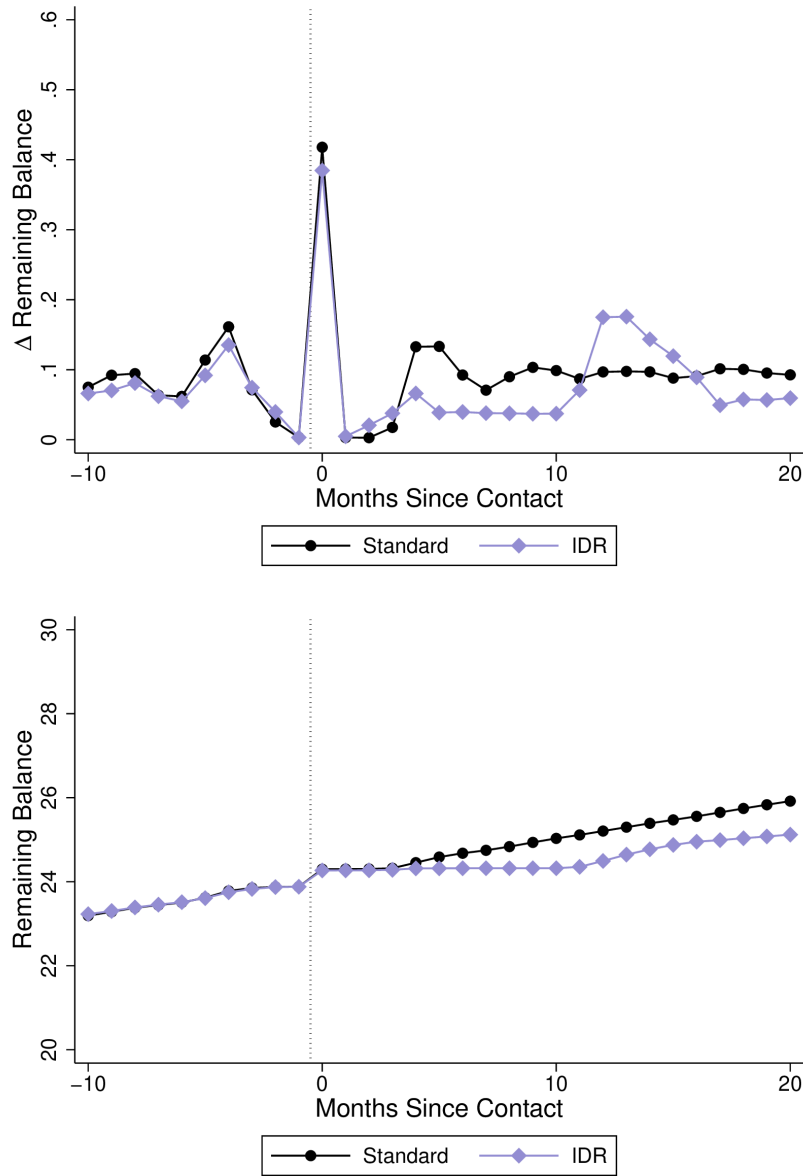
*Note:* This figure plots the average monthly minimum payments and monthly IDR enrollment status for treatment and control borrowers in the analysis sample. The horizontal axis denotes time, in months, relative to the month of the loan servicing call. Outcomes are normalized to the average value for control borrowers in the month prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

Figure A2: Pre/Post-Call Trends in Delinquencies



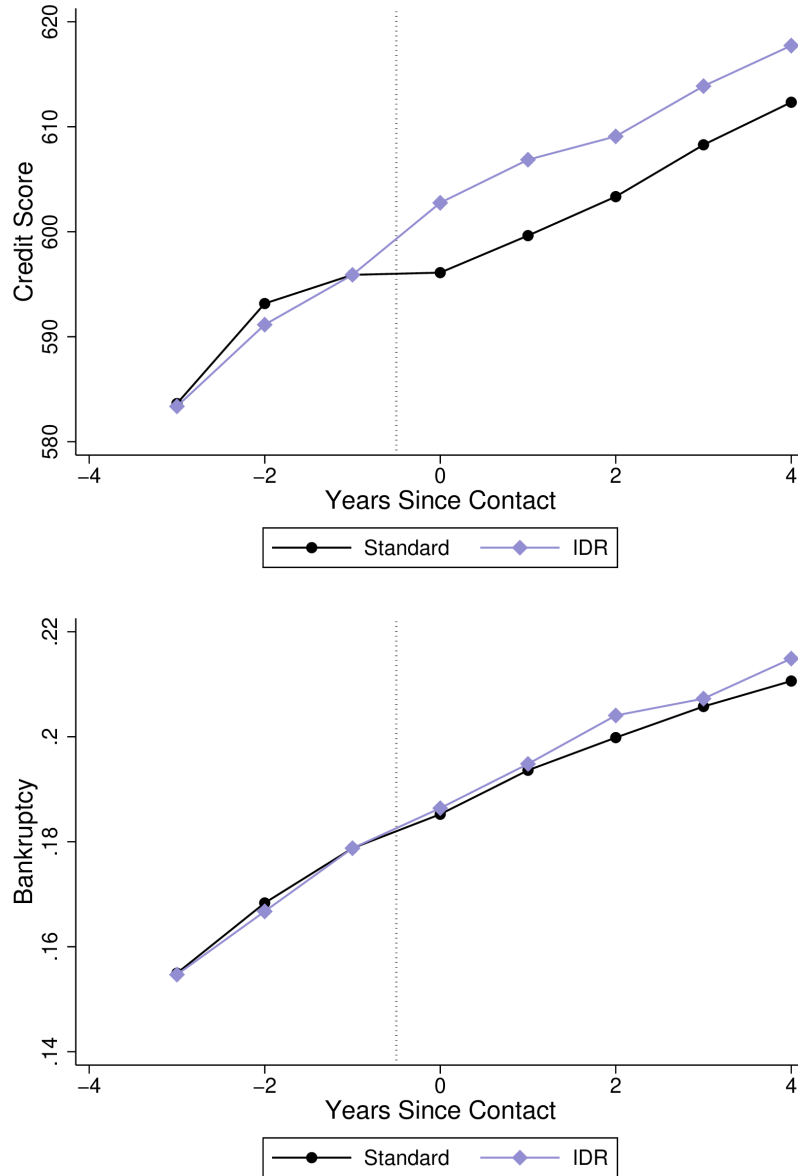
*Note:* This figure plots the shares of treatment and control borrowers more than 10, more than 90, and more than 270 days delinquent in the analysis sample. The horizontal axis denotes time, in months, relative to the month of the loan servicing call. Outcomes are normalized to the share of delinquent control borrowers in the month prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

Figure A3: Pre/Post-Call Trends in Balances



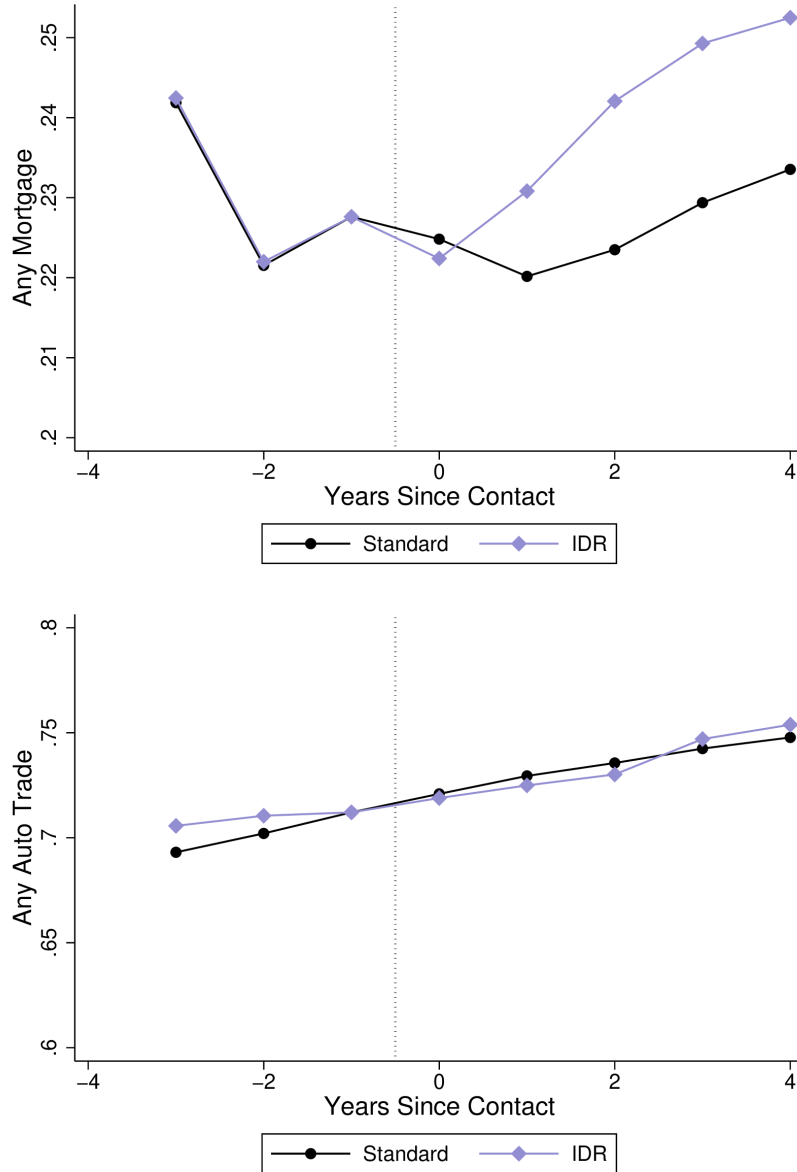
*Note:* This figure plots the average total student loan balances and monthly changes in student loan balances, in thousands of dollars, for treatment and control borrowers in the analysis sample. The horizontal axis denotes time, in months, relative to the month of the loan servicing call. Outcomes are normalized to the average value for control borrowers in the month prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

Figure A4: Pre/Post-Call Trends in Credit Scores and Bankruptcies



*Note:* This figure plots the average credit scores and bankruptcies for treatment and control borrowers in the analysis sample. The horizontal axis denotes time, in years, relative to the year of the loan servicing call. Outcomes are normalized to the average value for control borrowers in the year prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

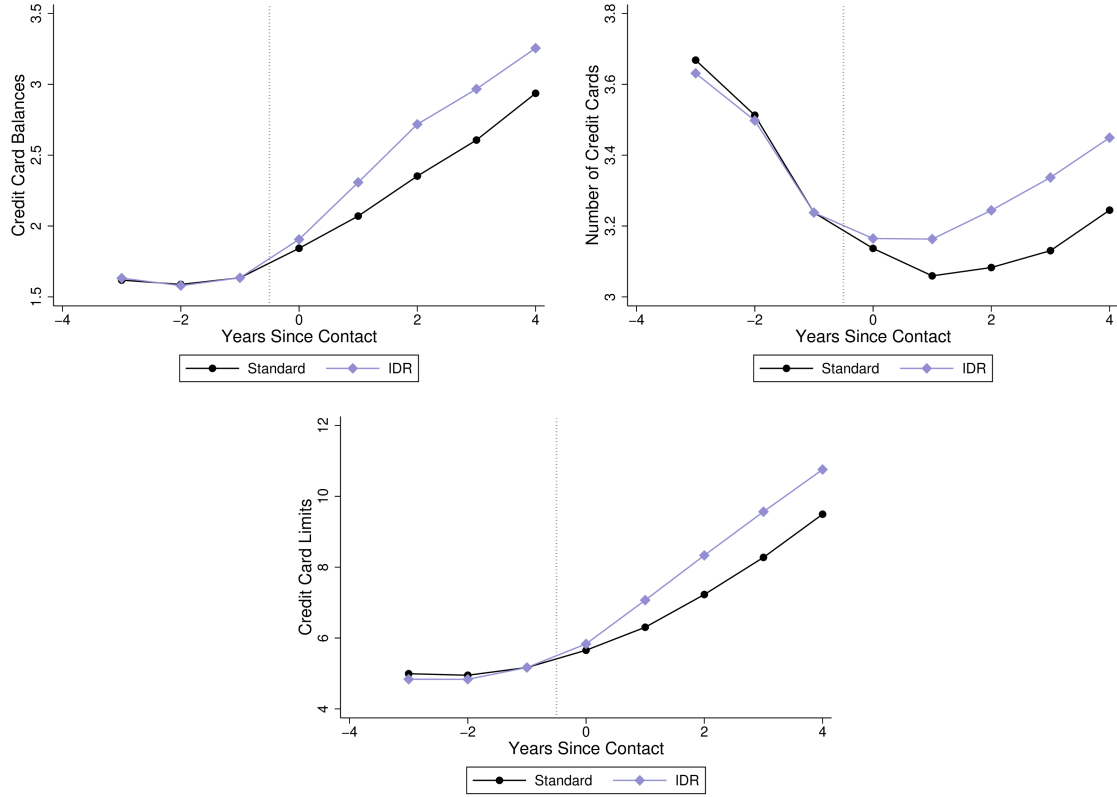
Figure A5: Pre/Post-Call Trends in Mortgages and Auto Loans



*Note:* This figure plots the shares of treatment and control borrowers holding mortgages and auto loans in the analysis sample. The horizontal axis denotes time, in years, relative to the year of the loan servicing call. Outcomes are normalized to the average value for control borrowers in the year prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

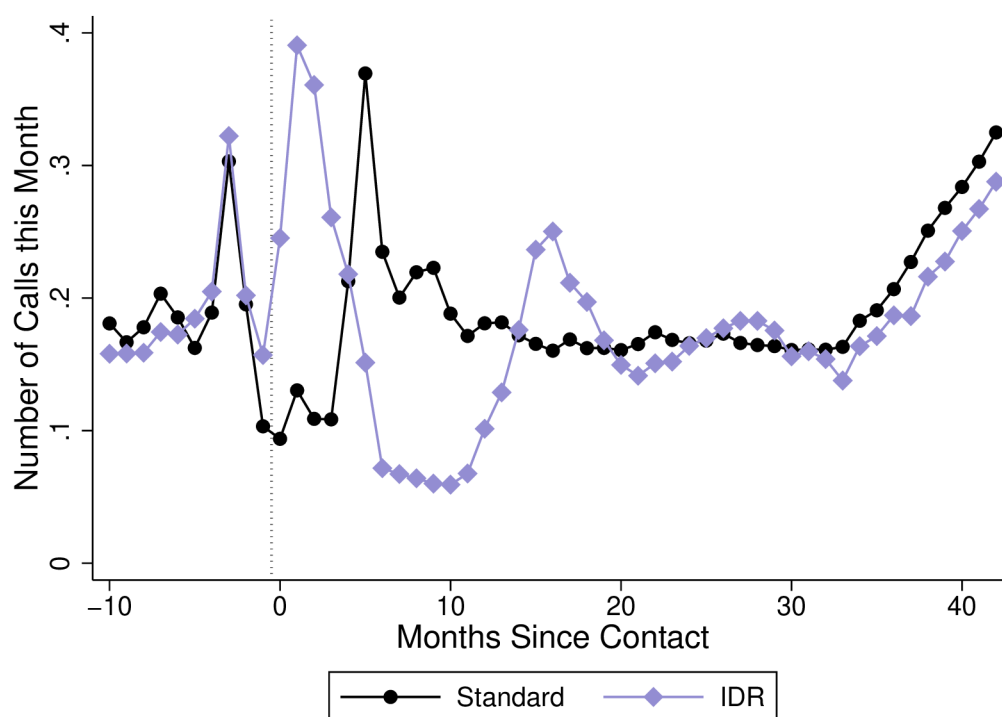


Figure A6: Pre/Post-Call Trends in Credit Cards



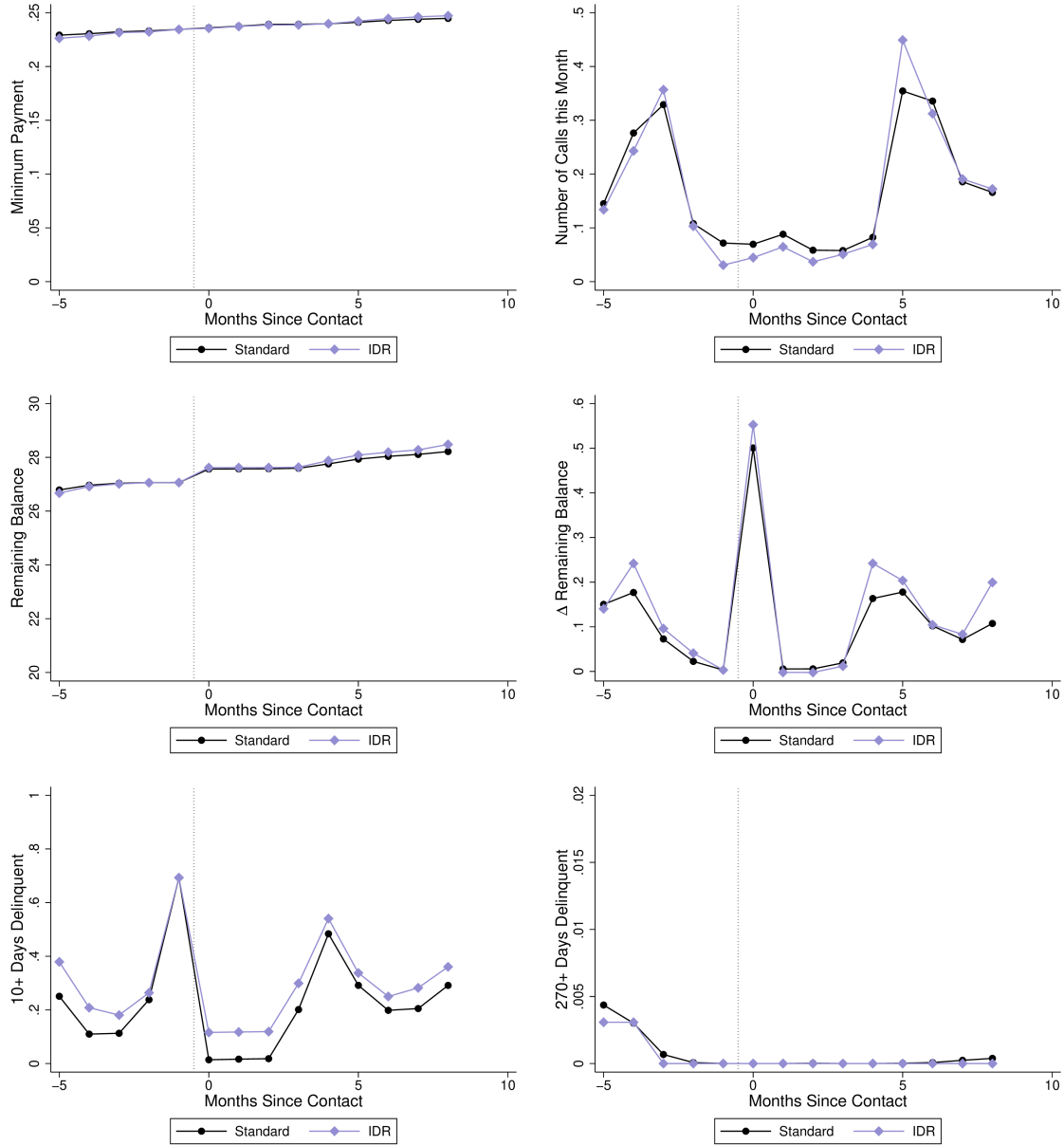
*Note:* This figure plots the average credit card balances, number of credit cards, and total credit card limits for treatment and control borrowers in the analysis sample. The horizontal axis denotes time, in years, relative to the year of the loan servicing call. Outcomes are normalized to the average value for control borrowers in the year prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

Figure A7: Pre/Post-Call Points of Contact



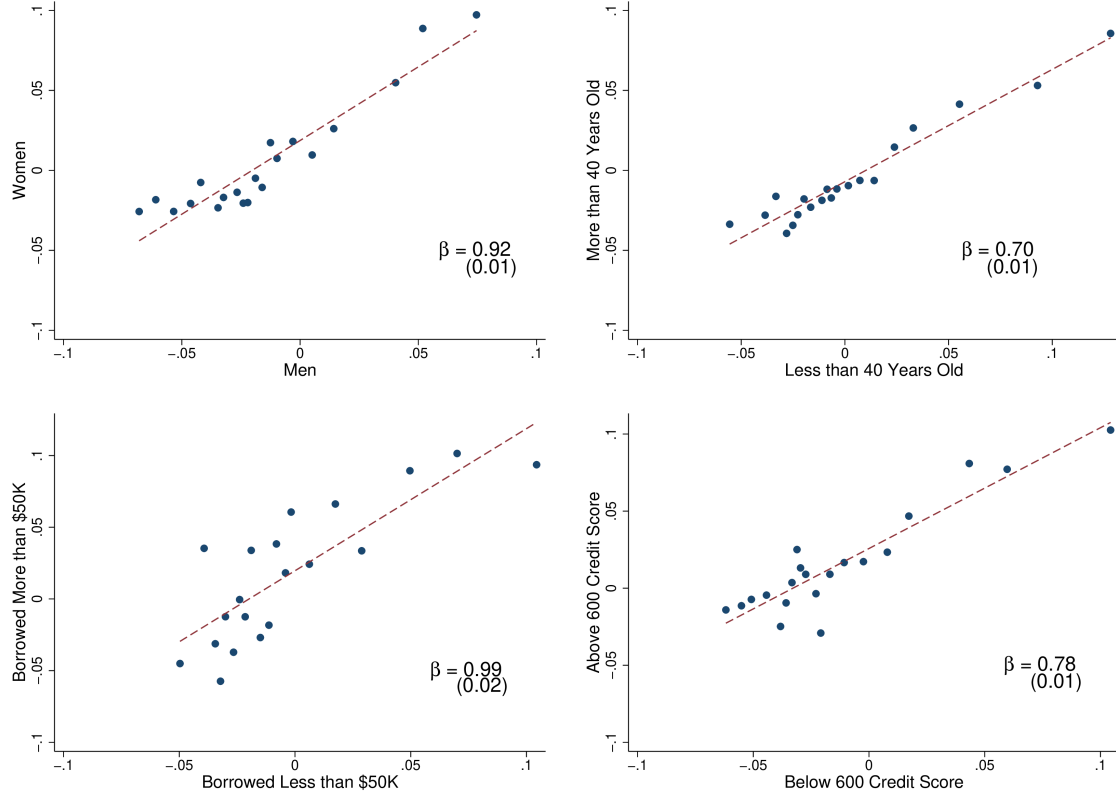
*Note:* This figure plots the average monthly points of contact (incoming calls, outgoing calls, and web chats) between borrowers and LLS for IDR enrollees and non-enrollees in the analysis sample. The horizontal axis denotes time, in months, relative to the month of the loan servicing call. See Table 1 notes for additional details on the outcome measures and sample.

Figure A8: Pre/Post-Call Trends in Repayment Outcomes: Placebo Test



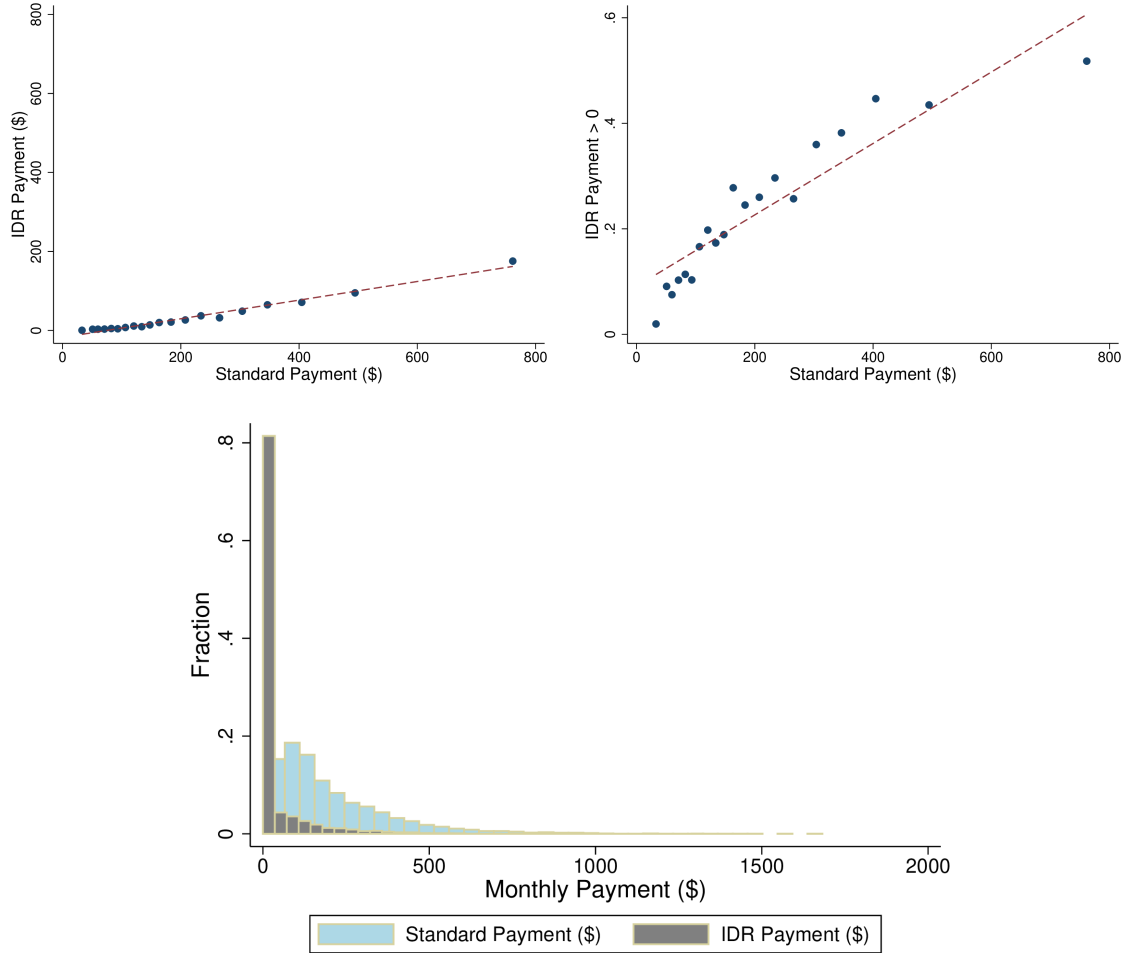
*Note:* This figure plots selected monthly LLS variables for *eventual* IDR enrollees following previous delinquency calls that did not end in enrollment versus non-enrollees in the analysis sample. The horizontal axis denotes time, in months, relative to the month of the loan servicing call. Outcomes are normalized to the average value of the outcome for non-enrollees in the month prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

Figure A9: Group-Specific Instrument Correlations



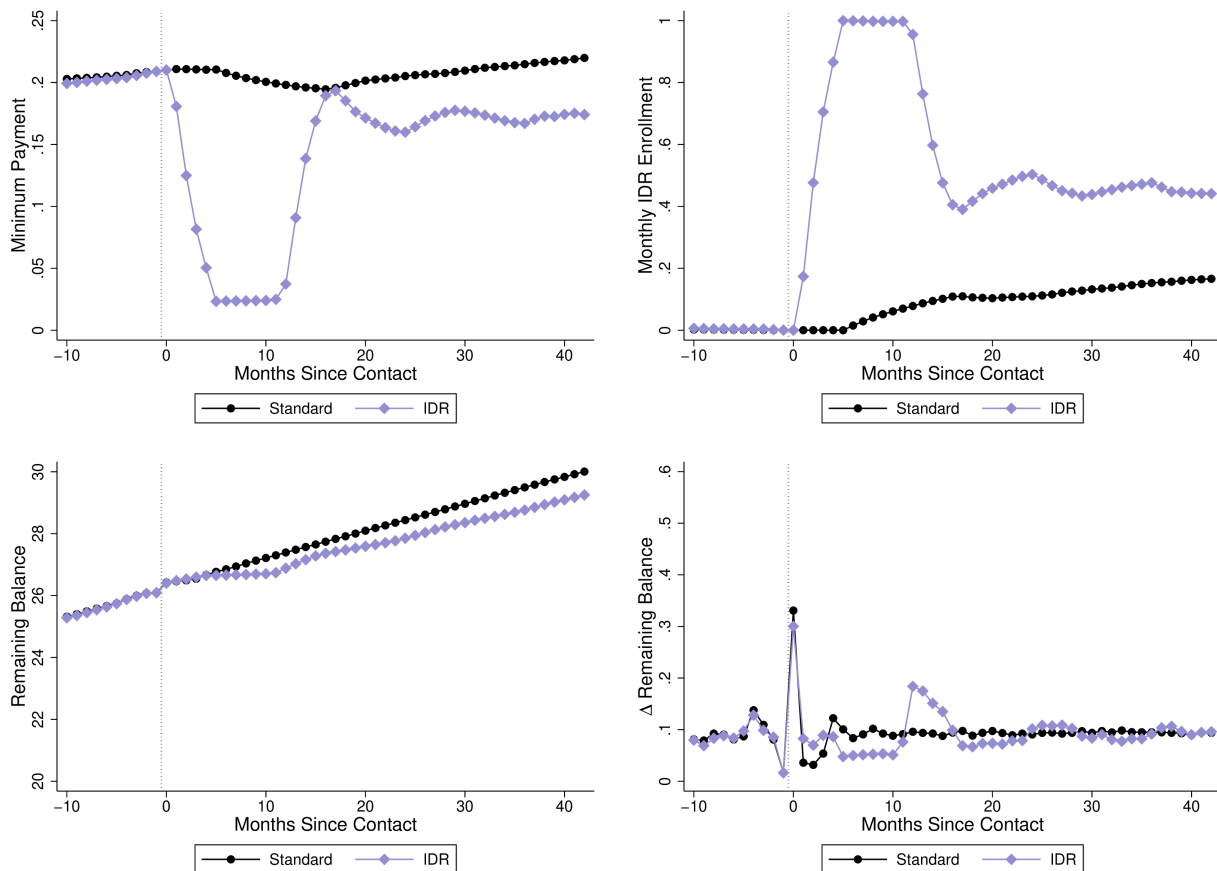
*Note:* This figure plots binned correlations between group-specific instruments  $Z_{icj}^g$ . Each axis measures the residualized, leave-one-out propensity of every call's assigned agent to induce IDR take-up among individuals in the group specified by the axis label. I also plot the linear best fit line estimated using OLS and report the associated coefficients and standard errors in the upper left corner of each panel.

Figure A10: Standard versus IDR Payments among IDR Enrollees



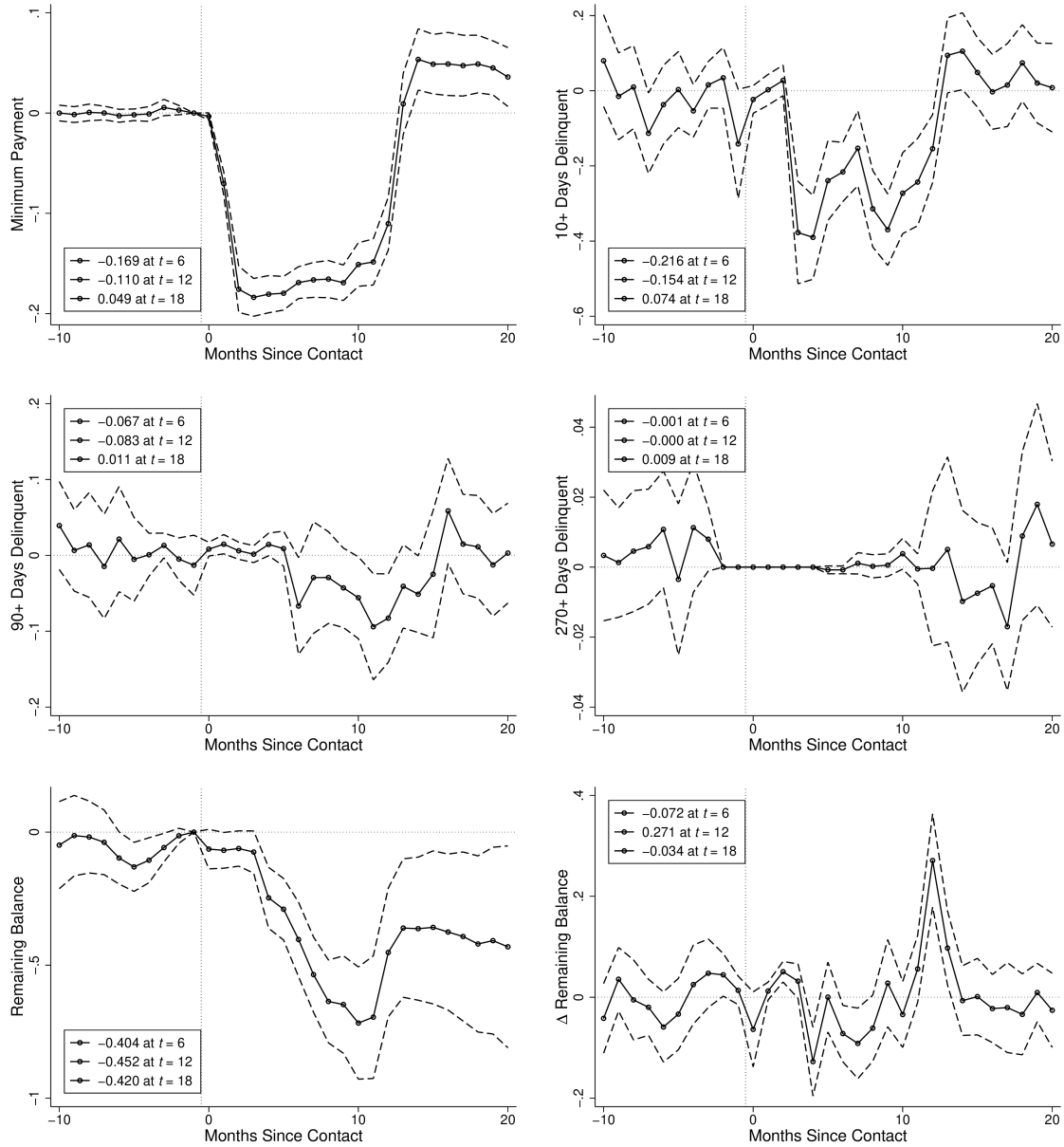
*Note:* This figure plots the relationship between pre-call standard payments and post-call IDR payments. The binned scatter plot is constructed using payment amounts one month before and six months following the delinquency call for borrowers in the analysis sample who take up IDR. The top-left panel plots average standard payment size against average IDR payment size. The top-right panel plots average standard payment size against the share of individuals with IDR payments greater than zero. The bottom panel plots histograms for standard and IDR payments. See Table 1 notes for additional details on the sample.

Figure A11: Pre/Post-Call Trends in Minimum Payments, IDR Enrollment, and Balances: Expanded Panel



*Note:* This figure plots the average monthly minimum payments, IDR enrollment status, balances, and changes in balances for treatment and control borrowers in an expanded monthly panel of 47,724 calls. The horizontal axis denotes time, in months, relative to the month of the loan-servicing call. Outcomes are normalized to the average value for control borrowers in the month prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

Figure A12: E-sign IV Estimates of the Effect of IDR Enrollment on Monthly Outcomes



*Note:* This figure reports monthly e-sign IV estimates for monthly repayment outcomes. Each point represents the estimated effect of post-call IDR status on the outcome variable at a given time period relative to the date of delinquency call. Relative months are plotted along the x-axis. Dashed lines represent 95% confidence intervals. Lower-right box lists point estimates at selected months. Robust standard errors are two-way clustered at the borrower and agent levels. All regressions include individual and call-date/time fixed effects.