

Liquidity and Insurance in Student Loan Contracts: Estimating the Effects of Income-Driven Repayment on Default and Consumption

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Abstract

Enrollment in income-driven repayment (IDR) plans for student debt has tripled in the past five years, yet little is known of its effects on borrower welfare. IDR reduces monthly loan payments to a fixed portion of earnings until debt is repaid or some forgiveness period has been reached. By aligning payments with income, IDR may promote repayment and improve financial well-being among credit-constrained borrowers but carries a potential cost to social welfare through moral hazard. In this paper, I estimate the causal impact of IDR on repayment rates, household balance sheets, homeownership, and consumption proxies using a novel dataset that links the first monthly panel of federal student loan payments to credit bureau records to for over one million student borrowers. My research design uses two complementary identification strategies: a difference-in-differences design comparing borrowers with differential IDR take-up following delinquency calls from their loan servicer, and an instrumental variables design exploiting variation in the tendency of randomly-assigned servicing agents to enroll borrowers in IDR. I find evidence of increased liquidity on both default and consumption margins. Relative to standard repayment plans, IDR decreases delinquency rates by 21 percentage points within seven months of take up. IDR enrollees have credit scores that are 7.53 points higher, hold 0.1 more credit cards, and carry \$240 higher credit card balances than non-enrollees one year after the servicing call, implying increased short-term consumption out of liquidity. IDR enrollees are also 2 percentage points more likely to hold a mortgage, an increase of 10 percent off of the pre-call mean, suggesting a positive effect of IDR on homeownership. Minimum monthly payments decrease by an average of \$140 following IDR take up, but return to standard levels within one year of enrollment, minimizing the potential impact of moral hazard through loan forgiveness. My results suggest IDR improves borrower welfare principally through a liquidity channel, providing short-term increases to cash-on-hand during periods of financial distress.

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1 Introduction

Each year, roughly one million borrowers default on their student loans, and millions more struggle to buy homes (Mezza et al. [2016]), accumulate wealth (Bleemer et al. [2017]), or choose their preferred career (Rothstein and Rouse [2011]).¹ A potential cause may be the financial burden of high student loan payments, which average over \$350 per month (Cortés et al. [2017]). By far, the largest and most far-reaching policy response to distressed student borrowers has been “Income-Driven Repayment” (IDR). First introduced in 2009, IDR allows students to repay a fixed portion of their yearly earnings until either their debt is paid off or some forgiveness period has been reached, offering an alternative to the flat, ten-year repayment schedule offered under traditional repayment plans. Enrollment in IDR has tripled since 2014, and today more than five million borrowers repay their student loans through IDR.

Even as IDR take-up continues to rise, its effects on borrowers are largely unknown. In theory, IDR may provide the liquidity needed to help credit-constrained borrowers smooth consumption over the life-cycle and insure against transitory income shocks. Evidence of high minimum payments (Looney and Yannelis [2015]) and limited credit (Gross and Souleles [2001]) among young borrowers suggests such liquidity may increase borrower welfare, allowing them to pay down their student debt while avoiding financial distress or sharp drops in consumption. However, these short-term liquidity benefits may be outweighed by the long-term costs of IDR’s forgiveness provisions, which cancel any unpaid debt after twenty-five years of payments. If enrollees’ incomes are *permanently* low, the repayment burden ultimately falls on the government, reducing net social welfare through redistributive inefficiencies, administrative costs, and distorted incentives (i.e., moral hazard).

Assessing the costs and benefits of IDR requires causal estimates of its effects on repayment and consumption, but two obstacles have prevented researchers from identifying these effects. First, estimating the impact of IDR requires high-frequency repayment data for a large number of borrowers, but as of yet these data have been unavailable.² With few excep-

¹“Default” is a legal distinction, defined as “Failure to repay a loan according to the terms agreed to in the promissory note.” A federal student loan is classified as defaulted after 270 days of non-repayment (Department of Education [2017a]).

²Economists have long bemoaned the dearth of available student loan data. In a 2015 meeting, William

tions, existing studies of the U.S. student loan market have relied on small cross-sectional samples or survey data.³ Second, IDR enrollees are by design a very selected group, as only borrowers with a sufficiently high debt-to-earnings ratio would benefit from enrolling in IDR. Estimates which rely on cross-sectional comparisons of IDR enrollees to non-enrollees could therefore be biased in either direction, as low-income individuals typically have worse financial outcomes, but borrowers with high student debt balances are often positively selected (Yannelis [2016]).

In this paper, I use administrative data from a large student loan servicing company to estimate the causal effect of IDR enrollment on defaults, credit scores, bankruptcies, and consumption proxies. The data I use link monthly loan records from a large loan servicer (“LLS”) to administrative credit bureau information from TransUnion for over one million student borrowers. LLS manages disbursements and payments for over \$300 billion in federal student loans. The LLS data include detailed loan information (e.g., balances, payments, delinquency status, repayment plan) at a monthly frequency, as well as contact histories, zip code, age, institution attended, and college enrollment dates. These data are, to the best of my knowledge, the first panel of U.S. federal student loan payments used in public research.

I use two complementary research designs to identify the effect of IDR on student borrowers, both of which exploit plausibly exogenous variation from loan servicing phone calls. First, I estimate the difference-in-differences between IDR-enrollees (the “treatment group”) and non-enrollees (the “control group”), before and after receiving delinquency calls from LLS informing them of IDR options. The identifying assumption for this design is that, in the absence of IDR, post-call outcomes for treatment and control groups would have exhibited parallel trends. To support this assumption, I show that trends in borrower outcomes are nearly identical across the two groups for several periods before the delinquency call.

Dudley, President of the Federal Reserve Bank of New York, remarked, “Do borrowers who use programs like income-based repayment eventually succeed in paying off their debts? How do income-based repayment programs affect important decisions such as labor supply, consumption and household formation? These are important questions for the nation,...but it is very hard to answer these questions with existing data.” (Dudley [2015]). In a column for the New York Times, Susan Dynarski echoes these sentiments, writing, “We are remarkably ignorant about student debt...But at the moment, the federal student loan data remains locked within the walls of the Education Department, with limited metrics trickling out.” (Dynarski [2015]).

³Looney and Yannelis [2015], Yannelis [2016], Cox [2017], and Brown et al. [2015] each use student loans data from administrative sources, though none observe borrower payments or repayment plans.

My second empirical strategy is an instrumental variables (IV) design exploiting the quasi-random assignment of delinquency calls to debt-servicing agents via an automatic dialing system. I use variability in agents’ tendencies to induce IDR take-up as a means of identifying the local average treatment effect (LATE) of IDR on individuals whose repayment plan decisions depend on the servicing agent to whom their delinquency calls are connected. I measure an agent’s IDR inducement rate using the leave-one-out, mean IDR status among the borrowers who receive a call from that particular agent. The agent effectiveness measure is predictive of IDR take-up, but uncorrelated with borrower characteristics and pre-call outcomes. This instrumental variables (IV) research strategy resembles Kling [2006], which uses random judge assignment to estimate the effect of incarceration length on earnings, as well as subsequent research focusing on consumer bankruptcy (Dobbie and Song [2015]), juvenile incarceration (Aizer and Doyle [2015]), pre-trial detention (Dobbie et al. [2016]), family welfare culture (Dahl et al. [2014]), disability insurance (Kostol et al. [2017]), and foster care (Autor and Houseman [2010]). While the quasi-experimental nature of the instrumental variables design holds obvious advantages, the increased statistical precision of difference-in-differences estimates makes it my preferred design. Nonetheless, IV estimates provide complementary evidence to the difference-in-differences design, as results are generally consistent between the two strategies.

Results suggest IDR increases loan repayment, credit scores, homeownership, and consumption among student borrowers. Relative to borrowers who remain on standard repayment plans, delinquency rates among IDR-enrollees fall by 21.0 percentage points within seven months of take up. IDR enrollees have credit scores which are 7.53 points higher, hold 0.1 more credit cards, and carry \$240 higher credit card balances one year after the servicing call. While I find no effects on bankruptcies or auto loans, IDR enrollees are 2 percentage points more likely than non-enrollees to hold a mortgage after two years, an increase of 10 percent off of the pre-call mean. Estimated effects on credit and consumption outlast the “first-stage” effect of IDR on payment size: monthly minimum payments drop by \$140 four to seven months following the delinquency call but return to pre-call levels by month twelve. These results suggest that short-term increases in cash-on-hand through IDR have long-term positive impacts on financial health of credit-constrained borrowers.

In theory, my estimates combine three effects of IDR: (1) a liquidity effect through lower

minimum payments, (2) a wealth effect through expected debt forgiveness, and (3) a moral hazard effect, as income-contingent forgiveness can distort incentives.⁴ For two reasons, I argue my estimates capture a pure liquidity effect: First, minimum monthly payments decrease by an average of \$140 following IDR take up, but return to standard levels within one year of enrollment, a rate which makes future loan forgiveness unlikely under the terms of the IDR plan I study. Second, because borrowers can opt-in to IDR at any time, any moral hazard from income-contingent loan forgiveness should apply to both treatment and control groups in my sample, as both groups are eligible for IDR and, by construction, aware of its existence. Estimates should therefore net out any potential labor supply effects, leaving a pure effect of liquidity.⁵

My analysis carries three important caveats. My analysis carries two important caveats. First, the effects I estimate hold interest rates and plan-specific repayment terms fixed. In a private market, one would expect the availability of IDR to have general-equilibrium effects on these parameters. For instance, adverse selection of individuals with low expected earnings into IDR could have an unraveling effect on a hypothetical “repayment-plan market.”⁶ In the existing student loans environment, however, interest rates and repayment terms are set by the government.⁷ My analysis therefore treats loan terms as policy parameters rather than equilibrium objects. Nonetheless, IDR may have equilibrium effects through less market-driven mechanisms, like budgetary constraints or political pressure, all of which are excluded from the partial-equilibrium effects I estimate in this paper.

Second, because my sample includes only IDR-eligible borrowers while they are already in repayment, the treatment effect I identify excludes any ex-ante effects on decisions per-

⁴Alternative effects are also possible, particularly through behavioral channels like myopia or inattention.

⁵Note that increased liquidity may itself affect labor supply, even in the absence of forgiveness. I view such effects as conceptually distinct from moral hazard. See Chetty [2008] and Shimer and Werning [2008] for a discussion of liquidity versus moral hazard in the context of unemployment insurance.

⁶Paradoxically, the existence of adverse selection may explain why there exists virtually no private market in which to study the phenomenon. In concurrent work, I use private elicitations of expected future income among student borrowers to provide evidence in support of this hypothesis.

⁷A small private student loans market exists for fewer than ten percent of loans, 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 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.

taining to college attendance, occupation choice, or principal borrowing amounts. In many ways, removing these effects is a theoretically attractive feature of my research design, as it allows me to isolate a single mechanism for observed effects—ex-post increases in liquidity. Nonetheless, removing ex-ante responses to expected payment flexibility or forgiveness leaves several unanswered questions important for the social welfare implications of IDR.

The remainder of this paper is organized as follows. Section 2 provides a brief overview of federal student loans and student loan servicing in the United States. Section 3 describes the student loan and credit bureau data and provides summary statistics. Section 4 motivates my empirical analysis with a model of consumption under alternative student loan repayment plans and discusses the potential welfare implications of IDR. Section 5 describes my empirical strategy. Section 6 presents results, and Section 7 provides interpretation. Section 8 concludes.

2 Background

2.1 Overview

Over 90% of student loans in the United States are federally subsidized and guaranteed. The government holds the liability on student loans, and interest rates are set by Congress.⁸ Student loans are not secured by collateral or subject to any credit check.⁹ While the federal government holds the liability on student loans, the Department of Education contracts private debt servicing companies to handle disbursements, billing, and processing. Debt servicers disburse loans to colleges' financial aid offices, which apply disbursed funds directly to students' accounts. 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

⁸Congress has set rates on student loans since 1965, though automated the process in 2013 with the Bipartisan Student Loan Certainty Act. The legislation mandates a rate-setting rule for student loans, setting 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.

⁹The exception is PLUS loans, for which parents can serve as cosigners, subject to a credit check.

same subsidized rate.¹⁰

The Department of Education sets repayment terms for student loans through several repayment plans, each requiring borrowers make monthly payments to their loan servicer. The default repayment option into which all borrowers are automatically enrolled is known as “standard repayment.” Under standard repayment, the minimum amount a borrower is required to pay follows a flat repayment schedule over ten years, so that minimum monthly payments are calculated as:

$$\text{Monthly Standard Payment} = \frac{i * \text{Principal}}{1 - (1 + i)^{-(10*12)}} \quad (1)$$

where i denotes the monthly interest rate. 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 first became available in 2009 as an alternative to standard repayment.¹¹ 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).¹² Specifically,

$$\text{Monthly IDR Payment} = 15\% * \left(\frac{\text{AGI} - 1.5 * \text{FPL}}{12} \right) \quad (2)$$

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 twenty-five years of payments under IDR, any remaining balance is forgiven.¹³

¹⁰In the 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.

¹¹Since 2009, several IDR plans have become available, including Income-Based Repayment (IBR), Pay-As-You-Earn (PAYE), and Revised-Pay-As-You-Earn (REPAYE). The eligibility requirements and repayment terms can vary across these IDR plans, though they share the same general structure. For the purposes of this study, I focus on Income-Based Repayment (IBR), as it is the only IDR plan for which the Federal Family Education Loan (FFEL) borrowers in my sample are eligible, though the discussion and conclusion generalize to the broader concept of IDR.

¹²Adjusted gross income (AGI) is an individual’s total annual income minus specific tax deductions. The federal poverty line (FPL) is a government-specified income threshold determined by household size and state of residence. For 2017, the FPL for a family of four is \$24,600 in every state but Alaska and Hawaii.

¹³Forgiveness periods can vary. While the specific IDR plan I study forgives loans only after twenty-five years, loans for more recent IDR borrowers are typically forgiven after twenty years, and non-profit

A graphical comparison of repayment plans can be seen in Figure 1. The figure plots IDR and standard repayment paths under alternative income scenarios for a borrower leaving college with \$18,000 in student loans. In panel A, the borrower’s income is too high to qualify for reduced payments: her payments are identical under standard (dashed blue line) and IDR (dotted red line) plans. In panel B, the borrower’s income is low enough to reduce IDR payments in the first five years of repayment, but rises to the standard amount thereafter, extending the repayment period beyond the standard length of ten years but not long enough to qualify for loan forgiveness at twenty-five years. Panels C and D, by contrast, depict scenarios in which the borrower is granted partial loan forgiveness under IDR.

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, although they can adjust their payments more frequently by providing 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.

Borrowers who fail to meet their monthly payments (i.e., “fall delinquent”) under any repayment plan face penalties which increase in severity with the number of days past due. Between one and fifteen days past due, student loan servicers will contact delinquent borrowers through email or post. Between fifteen 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, and 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 15 percent of borrowers’ wages or withhold their tax returns to collect on defaulted debt. Unlike other forms of consumer debt, student loans cannot be discharged by declaring Chapter 7 or Chapter 13 bankruptcy.¹⁴ Defaulted borrowers are

or government employees can often qualify for a ten-year forgiveness period under the Public Service Loan Forgiveness program. Regardless of the period, any forgiven balance is taxed as income in the year of forgiveness.

¹⁴In rare circumstances, borrowers who demonstrate “undue financial hardship” can discharge their student debt in bankruptcy. Student loans can also be discharged if borrowers are disabled, deceased, or attended

ineligible for any future federal student aid.

2.2 Setting: Student Loan Servicing

LLS manages disbursements and payments for over \$300 billion in federal student loans. Debt servicing is provided on behalf of the federal government, which hires the servicer through a series of contracts.

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 each of the numbers in this queue in rapid succession. If a call is unanswered, the dialer places it back at the bottom of the queue. If the dialer detects a connection has been made, it immediately transfers the call to a debt servicing agent. If multiple agents are available, it randomly chooses among them. If no agents are available, it 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 300 servicing agents across four call centers. Agents are tasked with informing borrowers of their delinquent status, inquiring about their intention 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 “models-out” IDR payments for the borrower, asking about their annual income, family size, and employment status. Borrower responses are entered into the agent’s computer, which provides an estimate of monthly IDR payments according to Equation 1. The agent then provides the borrower with 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, the agent’s pay is reduced that month.

an institution which has since closed (Department of Education [2017a]).

3 Data

The data I use to estimate IDR link administrative student loan repayment data to credit bureau records for over one million borrowers. Borrowers in my samples are drawn from LLS’s FFEL loan portfolio, which includes over \$90 billion in loans. The LLS data contain detailed repayment records for each borrower, including principal borrowing amounts, loan balances, minimum payments due, and dates of delinquency at monthly frequency. They also include indicators for type of loan (e.g., Subsidized Stafford, PLUS), current repayment plan (e.g., Standard, IBR), and current loan status (e.g., forbearance, grace period, default). In addition to loan information, the LLS data contain some 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.¹⁵

LLS data are linked to yearly TransUnion credit bureau records from 2010 through 2017. The TransUnion data provide yearly balances, credit limits, delinquencies, and number of accounts for several categories of consumer debt, including mortgages, credit cards, and auto loans. They also include broader measures of financial health, like credit scores and bankruptcies. Additional details concerning TransUnion data can be found in Dobbie et al. [2015], Avery et al. [2003], and Finkelstein et al. [2012]. TransUnion data are merged to borrowers in the LLS data by SSN.

The loan data provided for this study consist of two samples of borrowers, each drawn from LLS’s FFEL portfolio. The first, which I call the “full sample” ($N = 287,456$), is a random sample of FFEL borrowers with loans disbursed after 1995. While the random sample is not used in my main analysis, descriptive statistics from this sample are representative of the FFEL-borrowing population, and thus provide a useful benchmark to compare against my analysis sample. The second sample, which I call the “call sample” ($N = 899,771$), consists of the universe of LLS’s FFEL borrowers who ever received a delinquency call from 2012 onward, excluding those who hold any private or Direct loans.¹⁶

¹⁵The 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.

¹⁶While 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 Call sample excludes borrowers with mixture of loans so I can observe their complete repayment profile. Roughly 25% of the servicer’s FFEL borrowers

Borrowers in the call sample are linked to loan servicer contact histories from 2009 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 (“claims aversion”, “skip tracing”, etc.).

I construct the instrument and analysis samples on the borrower-by-call level, applying the following selection criteria to all calls placed to borrowers in the call sample: First, I remove borrowers who cannot be matched by zip-code or first name to inferred measures of gender or income, leaving 581,185 calls. To remove calls which may be non-randomly assigned, I remove borrowers marked as non-native English speakers, as well as those more than 140 days delinquent at the time of the call, which leaves 401,100 calls.¹⁷ 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. From the remaining group of 362,764 calls, I keep only those which reached the stage at which borrowers were provided information concerning IDR enrollment (i.e., “modeled-out”), leaving 66,228 calls.

While both estimation strategies use balanced panels, I use the larger unbalanced sample to calculate the instrument, excluding those calls made by agents with fewer than 40 total calls. Applying these criteria to the instrument sample reduce measurement error in my instrument because estimates of the mean taken over small cells have high variance. The instrument sample consists of 59,379 calls.

I use monthly and yearly balanced panels centered around delinquency call dates as my analysis samples for repayment and credit outcomes, respectively.¹⁸ For monthly repayment outcomes, I create a balanced panel of 31,109 calls with 15 leads and 10 lags. For yearly credit outcomes, I create a balanced panel of 16,025 calls with 2 leads and 3 lags. The pooled

also hold Direct loans, and fewer than ten percent hold private student loans.

¹⁷LLS handles “Late Stage” delinquencies of 150 days or more through another division with different agent-assignment procedures.

¹⁸Note that the calendar-correspondence between monthly and yearly variables can vary dramatically depending on call dates and data collection dates. If a borrower receives a call during the month in which yearly TransUnion data are collected, $t_{year} = 0$ corresponds to $t_{month} \in [-11, 0]$. If she receives the call one month later, $t_{year} = 0$ corresponds to $t_{month} \in [1, 12]$.

sample of calls from either panel constitutes my analysis sample.

Table 1 provides summary statistics for the random sample, call sample, and analysis sample. In the full sample, IDR has low take up, with only 4 percent of borrowers enrolled in a plan. That share rises to 13 percent in the analysis sample, as it is constructed to include only borrowers who might benefit from the plan. Unsurprisingly, these borrowers are negatively selected: they have lower credit card limits, higher rates of bankruptcy and live in lower-income zip codes. While negative selection into the analysis sample is partly because borrowers must be delinquent in order to receive a phone call, it also reflects negative selection into IDR; borrowers with high income or low debt balances would not benefit from the plan, and hence would not reach the “modeled out” stage of the delinquency call. Looking within the analysis sample, we see that those who ultimately enroll in IDR are largely comparable to those who do not. IDR enrollees have slightly higher principal borrowing amounts and come from slightly lower-income zip codes, again reflecting characteristics of those who might gain more benefit from IDR.

While the analysis sample is broadly representative of the IDR-eligible population, there are two important caveats concerning external validity. First, while my dataset is nationally representative of borrowers attending college between 1995 and 2010, individuals in my sample are selected along two dimensions: (1) they have at least one instance of late payments, and (2) their loans originated prior to 2010. While the first restriction limits the sample to likely beneficiaries of IDR, the second restriction removes many borrowers for whom we would expect IDR to be most effective, as younger borrowers typically have higher debt-to-income ratios. Estimated effects may therefore be small relative to population-wide effects.

Second, my sample is restricted to individuals eligible for one particular form of IDR. In the U.S., IDR exists through several repayment plans. While all IDR plans follow the same general structure, particulars of the payment calculation formula and forgiveness provisions under each plan can vary.¹⁹ Furthermore, IDR plans include unattractive institutional

¹⁹Individuals in my analysis sample hold FFEL loans and not Direct loans, which means they are eligible for “Income-Based Repayment” (IBR), and not “Pay-As-You-Earn” (PAYE) or “Revised-Pay-As-You-Earn” (REPAYE) plans. Approximately twenty percent of the five million borrowers in IDR plans are enrolled in IBR (Department of Education [2017b]). For a detailed description of each plan, including eligibility criteria and repayment terms, see www.studentaid.gov.

features like staggered payment adjustments and complicated sign-up procedures. To the extent real-world IDR deviates from an “ideal” system of income-contingent loan repayment, estimates from this paper apply to the former and thus include any effects from these institutional features.²⁰ Generalizability of my results to alternative IDR plans, existing or hypothetical, are therefore limited.

4 Model

To motivate my empirical analysis, in this section I develop a dynamic model of student loan repayment. The model illustrates the various channels through which IDR may affect borrowers’ consumption and default decisions.

4.1 Setup

Infinitely-lived borrowers are endowed with student debt D_0 and non-student debt assets $A_0 = 0$. Each period, borrowers draw income $y_t \sim N(\mu_t, \sigma)$ and make student debt payments $x_t = D_{t+1} - D_t$. Individuals can save ($A_{t+1} > 0$) or borrow ($A_{t+1} < 0$) to finance consumption, but face a liquidity constraint L , imposing an upper bound on the amount they can borrow.²¹ To simplify exposition, assume no discounting or interest ($\beta = (1 + r) = 1$). The borrower’s problem is given by:

$$V(y_t, D_t, A_t) = \max_{A_{t+1}} \{u(c_t) + E[V(y_{t+1}, D_{t+1}, A_{t+1})]\} \quad (3)$$

$$A_{t+1} \geq L \quad (4)$$

$$c_t = A_t - A_{t+1} + y_t - x_t \quad (5)$$

$$D_{t+1} = D_t - x_t \quad (6)$$

²⁰For a more detailed discussion of the limitations imposed by, and potential improvements upon, the current structure of IDR in the U.S., see Dynarski and Kreisman [2013].

²¹While I do not explicitly model default in this model, a simple extension would be to add a second liquidity threshold $L' > L$, where holding assets $A_{t+1} \in (L, L')$ carries some additional utility cost. The incidence of binding liquidity constraints in this baseline model can therefore be thought to capture delinquency or default in some loose sense.

Where minimum payments x_t are determined by a payment scheme modeled as a function of current-period income and remaining student debt:

$$x_t = x(y_t, D_t) \quad (7)$$

First order conditions for the borrower's problem yield an Euler inequality:

$$u'(c_t) \geq E \left[\frac{\partial V}{\partial A}(y_{t+1}, A_{t+1}, D_{t+1}) \right] \quad (8)$$

which binds in period t if and only if $A_{t+1} > L$. The solution to the borrower's problem is characterized by Equations 4 through 8. Unless prevented by liquidity constraints, individuals will save or borrow so that the marginal utility of current-period consumption is equal to the expected marginal value of assets next period.

In the following sections, I analyze how a change from Standard ("S") to IDR ("I") repayment schemes affects optimal consumption in the model above, where

$$x^S(y, D) = \min \left\{ \frac{D_0}{N}, D \right\} \quad (9)$$

$$x^I(y, D) = \min \left\{ \theta y, \frac{D_0}{N}, D \right\} \quad (10)$$

For borrowers in repayment, the IDR plan is modeled as the minimum of some share θ of per-period income and the standard payment amount D_0/N . Borrowers under both plans never pay more than their remaining balance D .

4.2 Liquidity Effects through Reduced Payments

To isolate the potential channels through which IDR might influence consumption, I first consider alternative specifications for the payment scheme in Equation 7 and defer discussion of loan-forgiveness provisions to the following subsection. Let m_t denote income net of loan payments, $m_t \equiv y_t - x_t$. The reduction in minimum payments offered by IDR offers two distinct benefits to borrowers, both of which increase consumption through liquidity. First,

IDR weakly increases net income in the short-term relative to the long-term:

$$E[m_t^I] \geq E[m_t^S] \quad \forall t \leq N \quad (11)$$

$$E[m_t^I] \leq E[m_t^S] \quad \forall t > N \quad (12)$$

The result is an intertemporal *smoothing effect*: pushing payments farther into the future flattens borrowers' expected net income profiles. As a result, financing consumption in periods $t \leq N$ requires less borrowing, decreasing the likelihood of binding liquidity constraints. With less fear of binding constraints, borrowers place less value on precautionary savings: the marginal utility of assets in Equation 8 decreases and consumption increases. Intuitively, individuals face less pressure to borrow because their reduced current-period payments are effectively “borrowed” through higher payments in future periods.

Second, in addition to smoothing income over time, IDR also smooths each period's income over states of the world, providing income insurance for student borrowers.²² To see how, note that IDR reduces *ex-post* variance in net income:

$$\text{Var}(m_t^I) \leq \text{Var}(m_t^S) = \sigma^2 \quad \forall t \quad (13)$$

Reducing income uncertainty increases consumption through an *insurance effect*. This insight comes from the literature on consumption dynamics: individuals facing higher income uncertainty are more likely to face binding liquidity constraints in all periods. A mean-preserving spread in the future income distribution will therefore increase the marginal value of assets and decrease current-period consumption out of choice (precautionary savings) or necessity (the liquidity constraint binds).

The complementary smoothing and insurance benefits of lower minimum payments are summarized in the following expression:

$$\Delta c_t = \underbrace{\sum_{k=0}^{\infty} -\frac{dc_t}{d\mu_k} \Delta E[x_k]}_{\text{smoothing}} + \underbrace{\sum_{k=0}^{\infty} \frac{dc_t}{d\sigma_k} [\sigma_k - \sigma]}_{\text{insurance}} \quad (14)$$

²²I use the term “insurance” loosely, referring to state-contingent liquidity benefits under IDR. Note that, ignoring loan forgiveness, income-contingent payments provide no insurance against *lifetime*-earnings risk.

Note that assuming quadratic utility ($u''' = 0$) and no debt forgiveness ($\sum_{k=0}^{\infty} \Delta E[x_k] = 0$), removing liquidity constraints would reduce both terms above to zero, as borrowers would simply borrow and save as needed to achieve constant consumption over all periods. In this sense, IDR serves more as a market-correcting measure than a transfer program: Under standard repayment, borrowers who expect future earnings increases would ideally finance current period consumption through borrowing. Market failures may prevent such borrowing, however, as recent college graduates often lack credit or collateral. IDR alleviates this problem by pushing debt obligations further into the future during periods of low income. If borrowers are liquidity constrained under standard repayment, IDR should therefore decrease delinquency rates, increase consumption, or both. Conversely, if borrowers have alternative credit options, we would expect student loan repayment and consumption to remain unchanged under IDR. For these unconstrained borrowers, IDR may instead have a deleveraging or savings effect, causing them to spend down higher interest credit cards or increase their savings.

Assessing IDR's potential liquidity effects is important for evaluating its welfare implications. If IDR provides liquidity to borrowers who would otherwise default or sharply reduce consumption, it corrects for an incomplete credit market and may increase total welfare: borrowers benefit from smoother consumption, improved credit, and fewer late fees, and the government saves on the considerable cost of collecting delinquent payments.²³ If, however, borrowers simply use their increased cash-on-hand to pay down existing debt or increase savings, flexible repayment amounts to little more than a redistributive policy transferring interest-savings to student borrowers through taxpayer-subsidized loans.

Observations of the student debt market provide support for potential liquidity benefits. Median monthly payments exceed 14% of median earnings for borrowers beginning standard repayment (Looney and Yannelis [2015]), and raising limits on credit card balances has twice the expenditure response for those under 44 as those over (Gross and Souleles [2001]). Together, these facts suggest college graduates might benefit from borrowing against future earnings to finance consumption and avoid delinquency in low-income periods.

²³The Congressional Budget Office estimates annual administrative costs of the FFEL student loan program equal 0.67 percent of outstanding balances, or approximately \$10 billion per year (Lucas and Moore [2010]).

4.3 Wealth Effects through Debt Forgiveness

Note that while the liquidity benefits described above increase borrower welfare, they do not increase lifetime income: reduced payments facilitate wealth transfers *within-borrower* over time, and therefore carry no cost to social welfare. By contrast, the forgiveness provisions of IDR, which forgive any outstanding debt after twenty-five years of successful payments, amount to a means-tested government transfer.²⁴

To capture the loan forgiveness provisions of IDR, I make a simple modification to the model above. Let T denote the number of periods after which remaining balances are forgiven. In period $t = T + 1$, instead of Equation 6, we have:

$$D_{T+1} = 0 \quad (15)$$

Note that if borrowers expect loan forgiveness, $\sum_{k=0}^{\infty} \Delta E[x_k] < 0$ and the first term in Equation 14 includes a *wealth effect*. In this case, the expression can be decomposed to differentiate between liquidity and wealth effects:

$$\Delta c_t = \underbrace{-\frac{dc_t}{dD_{T+1}}E[D_T^I - x_T^I]}_{\text{wealth effect (+)}} + \underbrace{\frac{dc_t}{dD_{T+1}}E[D_T^I - x_T^I] + \sum_{k=0}^T \left(-\frac{dc_t}{d\mu_k} \Delta E[x_k] + \frac{dc_t}{d\sigma_k} [\sigma_k - \sigma] \right)}_{\text{liquidity effects (+)}} \quad (16)$$

4.4 Social Welfare Implications

While reduced payments and debt forgiveness provisions both increase consumption among IDR enrollees, they hold different implications for social welfare. By providing a ceiling on debt's share of lifetime income, debt forgiveness provides "wealth insurance" against lifetime-earnings risk. As with any social insurance program, however, providing state-contingent transfers carries costs. First, because IDR borrowers pay no "premium" for wealth insurance,

²⁴Note that IDR may provide income transfers through other channels not included in this model. For instance, the subsidized interest rates on student loans provide a net-present-value benefit to borrowers whose minimum payments drop due to low income.

the government bears the expected cost of forgiven debts plus some risk premium.²⁵ Such a transfer would impose a deadweight loss to social welfare through taxation while carrying ambiguous redistributive consequences: forgiveness is progressive conditional on student debt, but college graduates, especially those with high debt balances, have higher lifetime income than the general population. 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 take jobs with higher risk of low pay or unemployment as a result of being partially insured against income losses. By contrast, providing liquidity through reduced IDR payments is revenue neutral and non-distortionary. Income-contingent payments may affect labor supply, but such responses represent a corrective response to incomplete credit markets rather than moral hazard.²⁶

A full accounting of IDR's social welfare effects would distinguish between liquidity and wealth effects and measure labor supply, college attendance, and principal borrowing responses through each channel. For several reasons, such an analysis is beyond the scope of this paper. First, IDR has not been available long enough for the twenty-five year forgiveness period to bind, so assessing the incidence of forgiveness benefits necessarily would necessarily involve some degree of forecasting. Second, labor supply effects, like wages or hours worked, can only be detected insofar as they influence consumption decisions, and the post-graduate timing of IDR enrollment for individuals in my sample means I cannot estimate moral hazard effects on decisions like principal borrowing amounts or institution and college major choice. Third, some degree of wealth insurance is theoretically afforded to all IDR-*eligible* borrowers, not just current enrollees, as individuals on standard repayment plans can usually opt-in to IDR at any time. My research design cannot identify moral hazard through these market-wide effects, as all individuals in my sample are eligible for IDR.

Nevertheless, the repayment path of IDR borrowers in my sample should provide some insight into the relative effects of liquidity versus expected debt forgiveness. The incidence of debt forgiveness depends on the likelihood of positive balances after twenty-five years

²⁵While 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.

²⁶See Chetty [2008] for a more complete description of the distinction between moral hazard and liquidity in the context of unemployment insurance.

of IDR payments. While I cannot directly test for moral hazard or measure realized loan forgiveness, the evolution of payment-to-debt ratios in the two years following IDR enrollment can inform forgiveness likelihood. If IDR payments return to the standard amount within one or two years, it suggests IDR holds little insurance benefit and moral hazard is unlikely to play a large role in borrower behavior. If, instead, payments remain low for a prolonged period following enrollment, the likelihood of loan forgiveness is much higher and IDR could potential carry a high social welfare cost through moral hazard or redistributive inefficiencies.

5 Empirical Strategy

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

$$Y_{ict} = \beta_0 + \beta_1 IDR_{ic} + \beta_2 \mathbf{X}_{it} + \epsilon_{ict} \quad (17)$$

where Y_{ict} denotes the outcome of interest, IDR_{ic} is an indicator for IDR enrollment within three months of the call, \mathbf{X}_{it} is a vector of borrower control variables, and ϵ_{ict} is an error term. Estimating β_1 in Equation 17 using OLS would likely yield biased estimates, because preferences over repayment plan choices are correlated with unobserved borrower attributes. For example, borrowers expecting an increase in earnings would likely have a weaker preference for *IDR* than those with lower earnings expectations. In this case, OLS estimates of IDR's effect on measures of financial well-being would be biased *downwards*, reflecting the selection of individuals low earnings potential into the plan.

To overcome the biases associated with OLS and identify the causal effect of IDR on financial outcomes, I employ two complementary empirical strategies. First, I use an instrumental variables (IV) design which captures exogenous variation in IDR take-up by exploiting the quasi-random assignment of servicing agents to delinquency calls. Second, I estimate the difference-in-differences between IDR-enrollees and non-enrollees, before and after receiving delinquency calls.

5.1 Instrumental Variables

The instrument used in my IV design is a measure of a quasi-randomly-assigned servicing agent’s tendency to induce IDR take-up among the borrowers she calls. I interpret observed differences in outcomes between borrowers assigned to different agents as the causal impact of the change in IDR take-up associated with those agents. The parameter I estimate in this specification is a local average treatment effect (LATE), which identifies the impact of enrolling in IDR among borrowers whose plan choice depends on agent-assignment.

Instrument Calculation I construct my instrument using a residualized, leave-one-out measure of agents’ ability to induce IDR take-up that accounts for the timing and ordering of delinquency calls. This “agent score” is analogous to the measures of judge leniency used by Dahl et al. [2014] and Dobbie et al. [2016] to instrument for trial outcomes in court case settings. The approach solves two problems one might see in a non-residualized instrument. First, the start and end dates of an agent’s employment with LLS might coincide with time trends in IDR take-up unrelated to the agent’s ability. For instance, an agent employed before IDR became a popular repayment option might incorrectly be measured as “low-score”. Second, the shift choices of agents are non-random and may correlate with borrowers’ IDR take-up if, for example, borrowers are more likely to hang up on calls received in the evening or on weekends.

I account for these potential selection effects by removing year, month, weekend, and hour-of-day fixed effects as well as a linear time trend from observed IDR enrollments before calculating the leave-out agent score. Specifically,

$$IDR_{ict}^* = IDR_{ict} - \gamma \mathbf{W}_{ict} \quad (18)$$

$$= Z_{ijc} + \epsilon_{ict} \quad (19)$$

where \mathbf{W}_{ict} is a vector of call date and time controls and Z_{ijc} is agent score. I calculate residualized rate of IDR take-up, IDR_{ict}^* , using OLS estimates of γ in Equation 18. I then

construct agent score Z_{ijc} using the leave-one-out mean of this residualized rate:

$$Z_{icj} = \left(\frac{1}{n_j - 1} \right) \left(\sum_{k=0}^{n_j} IDR_{kj}^* - IDR_{ij}^* \right) \quad (20)$$

where n_j denotes the number of calls made by agent j . Removing borrower i 's outcomes from the calculated mean is important to avoid reflection bias: including one's own outcome in her assigned agent score would introduce the same estimation errors on both left- and right-hand side of the regression, incorrectly attributing unobserved between-borrower differences in IDR take-up to agent ability. My two-stage least-squares estimation avoids this problem by using the leave-one-out agent score, calculated as in Equation 20, as an instrumental variable for whether a borrower enrolls in IDR.

The residualized agent score distribution can be seen in Figure 6. The analysis sample includes calls from 144 different agents in four different call centers. Agents place 216.03 calls on average to borrowers in the analysis sample, with a median of 183 calls.

Identifying Assumptions In order for two-stage least squares estimates to identify a local average treatment effect (LATE) for the causal impact of IDR take-up, the instrument must satisfy three conditions: (1) IDR take-up varies with agent assignment, (2) agent assignment predicts borrower outcomes only through its affect on IDR take-up, and (3) agents' tendency to induce IDR take-up is monotonic across borrowers.

To test the first identifying assumption, I empirically evaluate the first-stage relationship between the agent score instrument and observed IDR take-up. Figure 6, which plots a local linear regression of IDR take-up against the agent score instrument, provides graphical evidence for this relationship. IDR take-up is monotonically increasing in agent score throughout the distribution of borrowers.

The first-stage relationship seen in Figure 6 can be represented through following linear probability model:

$$IDR_{ict} = \alpha_0 + \alpha_1 Z_{ict} + \alpha_2 \mathbf{X}_{ict} + \epsilon_{ict} \quad (21)$$

In Table 3, I provide first-stage estimates from an OLS regression of Equation 21. Results imply a positive and highly significant relationship between agent score and IDR take-up.

Point estimates range from 0.62 to 0.60, depending on the inclusion of borrower controls, and the F-statistic on a test of instrument significance equals 21.14.

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. Table 2 provides empirical evidence that, conditional on call date and time, borrowers do not vary systematically by agent score. Column 1 reports results from an OLS regression of realized IDR take-up against several borrower characteristics and pre-call outcome variables, as well as call date and time fixed effects. Not surprisingly, estimates demonstrate considerable 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. Column 2 reports results from an OLS regression of the agent score instrument on the same right-hand side variables. Estimated coefficients on borrower variables in this specification are statistically indistinguishable from zero, and the F-statistic on a test for whether all borrower variables can jointly predict agent score is 1.41.

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, the institutional details in my research setting provide suggestive evidence that such threats to validity are minimal. LLS’s delinquency 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.

Monotonicity The third identifying assumption requires monotonic agent effects across borrowers. To satisfy the monotonicity assumption, there can be no borrower for whom a higher (lower) score agent decreases (increases) the likelihood of IDR take-up; any borrower who declines the IDR option under a high agent must not be induced into IDR under a comparatively low score agent. This assumption 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.

Agent Variation Variation in agent score can be driven by several potential sources. First, while agents ask questions and provide responses according to a standardized decision tree, several nodes of this tree require agent judgment. For example, before informing borrowers of alternative repayment options, an agent must first ask if a borrower is “capable of making payments under her current plan.” This question seldom elicits an unambiguous “yes” or “no” response, so agents must make this determination based on their own judgment. To the extent agents are afforded discretion in their responses to borrowers, their decisions are undoubtedly influenced by the incentive scheme discussed in Section 2.2. For instance, more risk averse agents may present the IDR option to a broader swath of borrowers to avoid potential compliance infractions. Second, variation may exist in agents’ perceived geniality. Even agents reciting the same words can vary in IDR-conversion through subtle variations in demeanor or tone, and borrowers often hang up or stop listening depending on the interaction. Similarly, while call data are not detailed enough to verify, conversations with agent supervisors suggest that factors like speech patterns and accents play a large role in keeping borrowers on the phone long enough to advise them about IDR. Third, agent score may 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.

5.2 Difference-in-Differences Specification

I complement the instrumental variables design described above with a difference-in-differences estimation comparing 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). Formally, the difference-in-differences specification takes the following form:

$$Y_{ict} = \gamma_i + \gamma_t + \left[\sum_{\tau \neq -1} \delta_\tau \cdot IDR_i \cdot \mathbf{1}\{t = \tau\} \right] + \gamma_1 IDR_{ic} + \gamma_2 \mathbf{X}_{ict} + \epsilon_{it} \quad (22)$$

where Y_{ict} denotes the outcome of interest, γ_i are individual fixed effects, γ_t are event-time fixed effects (months or years relative to call date), IDR_{ic} is an indicator for IDR enrollment within three months of the call, \mathbf{X}_{ict} is a vector of borrower control variables (including call date and time fixed effects), ϵ_{ict} is an error term, and δ_t are coefficients on IDR enrollment status which vary non-parametrically by event time. The specification omits α_t and δ_t terms at $t = -1$ to prevent perfect multicollinearity. Estimates can therefore 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. Such variation includes the between-agent differences captured by the instrumental variables strategy described in Section 5.1, as well as *within*-agent variation over calls. While the previous section argues that between-agent variation is as-good-as-random, within-agent variation is not. Internal validity of this specification therefore rests upon the assumption that, holding borrower-specific differences fixed, such within-agent variation is exogenous with respect to outcomes. Stated differently, unbiased estimation in my difference-in-differences design assumes that trends in outcomes would be the same in both groups of borrowers had neither taken up IDR.

Figures 2 - 5 provide graphical evidence in support of the common-trends assumption. The figures plot raw means for monthly repayment and credit bureau outcomes, respectively, for IDR enrollees and non-enrollees, normalized by month- or year-of-call and 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 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.²⁷

Even if IDR and standard borrowers exhibit observably similar pre-trends, other potential violations to the exclusion restriction exist. For example, IDR-enrollees may have

²⁷Estimates from the specification including linear pre-trends can be interpreted as IDR’s impact on

experienced a shock at the time of their delinquency call that both induced them into IDR take-up while also influencing outcome variables. I argue that such instances are unlikely. Delinquency calls are *outgoing*, so their incidence is determined by LLS, not the borrower. While the timing of these calls are mechanically non-random *within-borrower*, they do not vary systematically *between* borrowers with observably similar debt characteristics. If IDR borrowers were enrolling as a response to sudden shocks, outcomes should therefore vary from non-IDR borrowers in the months immediately preceding the call.

It is possible that borrowers exhibit the same or similar trends before delinquency calls but make IDR enrollment decisions based on expected *future* shocks to their financial well-being. While my difference-in-differences design cannot rule out this scenario, any bias created by forward-looking borrowers should be negative: the benefit of IDR is strictly decreasing in income and available credit, so borrowers who select into IDR based on expected shocks to their financial well-being should, all else equal, exhibit *lower* repayment, consumption, and credit scores relative to standard borrowers, attenuating any positive treatment effects of IDR.

Finally, the quasi-experimental nature of my instrumental variables design provides an additional robustness check to my differences-in-differences design. While differences between the two approaches can be partly attributable to heterogeneous treatment effects between their respective complier populations, similar IV and difference-in-differences estimates would suggest that the variation driving the latter is plausibly exogenous. I make this comparison for monthly repayment outcomes, where IV estimates are sufficiently precise, to lend further credibility to difference-in-differences estimates of IDR's effect on noisier credit outcomes.

outcomes relative trend-predicted differences between groups. Formally, the model is given by

$$Y_{ict} = \gamma_i + \delta t \cdot IDR_i \cdot \mathbf{1}\{t < 0\} + \left[\sum_{\tau \geq 0} \delta_\tau \cdot IDR_i \cdot \mathbf{1}\{t = \tau\} \right] + \gamma_1 IDR_{ic} + \gamma_2 \mathbf{X}_{ict} + \epsilon_{it} \quad (23)$$

6 Results

6.1 Repayment Outcomes

Table 4 reports difference-in-differences and IV estimates of IDR’s effect on minimum payments, changes in loan balances, and indicators for 10 or more and 90 or more days delinquent. For each outcome listed on the left-hand side, Columns 2-4 provide estimated coefficients on the interaction between IDR take-up and consecutive three-month periods from the pooled difference-in-differences specification given by Equation 22. Columns 5-7 provide estimated coefficients on IDR take-up from separate two-stage least squares regressions in each three-month period.²⁸ Figure 8 plots coefficients on these outcomes using the same specification, but separated by each month following the date of the delinquency call.

Minimum Payments IDR’s effect on minimum payment is mechanical: given income, family size, and debt balance, one could directly calculate a borrower’s IDR payments using Equation 1. Estimating the change in payments for compliers in my sample, however, can provide insight into the mechanisms behind effects on other outcomes. If we saw no effect on payments, any observed effects on delinquencies or consumption would be entirely attributable to the behavioral effects of expected loan forgiveness.²⁹

In the instrumental variables analysis, IDR lowers minimum payments by an average of \$200 four to seven months following IDR take-up, off of a pre-call mean of \$220. In the top-left panel of Figure 8, payments remain relatively stable in the twelve months following the delinquency call, then sharply increase to their pre-call levels, so that IDR and standard borrowers face similar minimum payments fourteen to eighteen months following their delinquency calls. The convergence in payments corresponds to the one-year recertification period for IDR, suggesting that IDR-enrollees in my sample do not extend benefits beyond

²⁸Results from the first three months following the call are omitted from Table 4 because it typically takes one or two months following contact to process and enroll borrowers in IDR, and the relative timing of successful enrollment, next payment due date, and data collection date at the end of the calendar month adds further lag time before IDR effects can be realized.

²⁹The expectation of future liquidity benefits could technically have a behavioral response as well, though upward-sloping wage profiles, and the ability to opt-into IDR at any point in the future make “expected-liquidity” channels unlikely.

one year. Difference-in-difference estimates of IDR's effect on payments are similar to IV estimates, though slightly smaller in magnitude. Required payments for those on IDR fall by \$140 relative to those who remain on standard repayment, and exhibit the same rapid return to standard levels one year following the delinquency call, as seen in the top left panel of Figure 7.

Delinquency I measure IDR's impact on two measures of delinquency: the likelihood of falling ten or more days delinquent, and the likelihood of falling ninety or more days delinquent. The ten and ninety day benchmarks are points of increased penalties: at ten days past due, borrowers begin to accrue late fees for delinquent loans, and at ninety days past due, borrowers are reported to credit bureaus. IDR borrowers are 36 percentage points less likely to fall ten or more days delinquent relative to standard borrowers in the four to seven months following the delinquency call, off of pre-call mean of 22. Corresponding estimates for months seven through nine and ten through twelve are -30pp and -8pp, respectively. While statistically insignificant, estimates for ninety or more days delinquent exhibit a similar pattern: the likelihood of falling ninety or more days delinquent falls by 13, 8, and 8 percentage points in months four through six, seven through nine, and ten through twelve after the delinquency call, respectively, off of a pre-call mean of 2 percentage points.

Change in Balances While minimum monthly payments measure the amount borrowers are required to pay to avoid late penalties, the high incidence of delinquency means that payments *due* provides a poor measure of payments *made*. I estimate effects on monthly changes in remaining balances to provide a sense of how IDR might impact borrowers' propensity to pay down their debts. Both IV and difference-in-differences estimates of IDR's effect on changes in debt balances suggest that, despite lowering required payments, IDR significantly increases the amount repaid. While not statistically significant, point estimates for the instrumental variables analysis suggest IDR decreases monthly balances by \$150 in months four through six and \$120 in months seven through nine after the delinquency call, off of a pre-call mean of \$130. Difference-in-difference estimates are qualitatively similar and statistically significant: relative to standard borrowers, IDR enrollees pay down \$70 to \$100 more debt each month than they did before their delinquency calls.

6.2 Credit and Consumption Outcomes

For evaluating IDR’s impact on credit and consumption outcomes in the TransUnion data, I rely principally on difference-in-difference estimates, as instrumental variables estimates are too low-powered to draw meaningful inference.³⁰

Table 5 provides estimates of the effect of IDR on credit score, mortgage, and auto loan from the difference-in-differences analysis described in Section 5.2. For each outcome listed on the left-hand side, Columns 2-4 report coefficients on IDR in consecutive years from the pooled regression specified in Equation 22, beginning with the year of the delinquency call. Columns 5-7 report IDR coefficients from a regression which omits pre-call month dummies and includes a linear time trend.

Relative to those who remained in standard repayment, borrowers who enrolled in IDR experienced a statistically significant 5.01-point increase in credit scores within one year of the delinquency call, off of a pre-call mean of 593.26, followed by increases of 7.53 and 5.92 points one and two years after the call. Estimates of IDR’s effect on bankruptcy filings and auto loans are statistically indistinguishable from zero for all three years following the delinquency call. IDR effects on the likelihood of holding a mortgage are also effectively zero in the year of the call, but increase gradually throughout the sample period, rising to 2 percentage points by year two, off of a pre-call mean of 20pp, an increase of 10 percent.

Table 6 provides 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 \$240 (0.33 log points) and \$400 (0.38 log points) one and two years after the delinquency call off of a pre-call mean of \$1420, corresponding to an increase of 28 percent. Similarly, by the second year of enrollment, IDR borrowers hold 0.14 more credit cards (pre-call mean of 3.00) and have \$340 higher credit limits (pre-call mean of \$2930) compared to those who remained in standard repayment plans.

³⁰For completeness, instrumental variables estimates for IDR’s effect on credit outcomes are reported in Appendix A. While imprecise, IV results are broadly consistent with the difference-in-difference estimates reported in this section.

7 Interpretation

In this section, I interpret my results by placing them in the context of the model laid out in Section 4. In short, results are consistent with a pure liquidity effect of IDR: reducing short-term payments increases borrowers' cash on hand, allowing them to increase consumption and avoid default in periods of low income.

Ignoring the potential effects of debt forgiveness, estimates of IDR's effect on minimum payments are analogous to a "first stage" in an estimation of the effects of increased liquidity: a necessary condition for IDR to generate observable liquidity effects is that it measurably reduces payments during my sample period. While the alternative scenario seems unlikely, it is possible for high-income borrowers to sign up for IDR solely in anticipation of future income shocks. My results run counter to this hypothetical: the large and significant observed effect on minimum payments suggests increased liquidity drives at least part of results on other outcomes. Having established that IDR borrowers receive an increase in short-term cash-on-hand, results from delinquency and credit outcomes provide evidence for how borrowers respond to this cash infusion. Recall that borrowers with reduced payments have three options: (1) keep consumption constant and add to their savings, (2) repay their student debt in a more timely manner, or (3) increase short-term consumption. While these options are not mutually exclusive, a repayment and/or consumption response is consistent with liquidity constraints and suggestive of social welfare improvement under IDR. My results provide evidence of both responses: the negative effect on delinquencies and positive effect on credit cards and mortgages suggest borrowers use the liquidity provided by IDR to prevent default, increase non-durable consumption, and buy homes.

By increasing expected lifetime income, debt forgiveness provisions under IDR can also affect borrower behavior. If borrowers expect their loans to be forgiven, they may increase short-term consumption regardless of liquidity constraints. While expected loan forgiveness could, in theory, drive effects of IDR on repayment or consumption, my results are more consistent with a pure liquidity effect. In order for the forgiveness period to bind, a borrower must make 300 complete monthly loan payments on IDR and still hold an outstanding balance on her student loans. Minimum payments under standard plans would pay off balances after just 120 payments. The rapid return of minimum payments to standard levels

therefore suggests forgiveness is very unlikely for borrowers in my sample. Importantly, this rapid rise in payments does not necessarily imply an increase in income; borrowers may qualify for continued reduced payments but not recertify their income after twelve months of IDR. Even so, such borrowers should not expect loan forgiveness. It is possible that payments may drop again in the future, making debt forgiveness more likely. Forgiveness effects on consumption under these circumstances would require one of two scenarios, both of which I argue are unlikely. First, if rising payments reflect increased incomes, payments might fall again in the future if income declines more permanently. While I cannot rule out this possibility, it seems unlikely, as income profiles do not generally exhibit sharp, transitory *upward* shocks followed by permanent decreases. Second, if rising payments reflect a failure to recertify income, borrowers may have permanently low incomes through the end of the sample period and choose to reenroll in IDR at a later date. These borrowers may qualify for forgiveness if they successfully make enough reduced payments following their reenrollment. However, if consumption were effected through the expectation of future forgiveness via potential future reenrollment, one would expect those effects to exist for both treatment and control borrowers in my sample, as those who remain in standard repayment plans following the delinquency call are aware of the forgiveness benefit they would receive from IDR should they decide to enroll at a later date.

While my results suggest that IDR improves welfare by providing liquidity to credit-constrained borrowers, I cannot rule out all alternative mechanisms driving the positive effect on repayment and consumption. Behavioral mechanisms, in particular, may be consistent with my results while holding different welfare implications. For example, myopia can drive effects on consumption upwards while reducing borrower welfare if increased cash-on-hand induces overspending in the short-run. Likewise, enrolling in a new repayment plan may make a borrower's debt more psychologically salient, prompting her to keep current on payments even if the plan provides no real benefit.

8 Conclusion

In this paper, I use new administrative data from a large student loan servicing company and exploit two sources of plausibly exogenous variation from servicing phone calls to isolate

the causal effect of IDR enrollment on default, bankruptcy, consumption proxies, and household balance sheets. I find evidence of increased liquidity on both default and consumption margins. IDR decreases delinquency rates by 16 percentage points within seven months of take up. Relative to borrowers who remain on standard repayment plans, IDR enrollees have credit scores which are 7.53 points higher, hold 0.1 more credit cards, and carry \$240 higher credit card balances one year after the servicing call, implying increased short-term consumption out of liquidity. Minimum monthly payments decrease by an average of \$140 following IDR take up, but return to standard levels within one year of enrollment, suggesting limited forgiveness incidence and minimal moral hazard effects. My results suggest IDR improves borrower welfare principally through a liquidity channel, providing short-term increases to cash-on-hand in periods of financial distress.

The results of my analysis raise several questions for future research. First, while this paper establishes the benefits of IDR on consumption, the same benefits may carry over into other outcomes. Income, employment, and occupation choice might all be affected by reduced loan payments, especially since liquidity benefits are concentrated in the years after college when labor market decisions are most consequential. Likewise, IDR may have ex-ante effects on borrowers' college attendance decisions, major choices, or principal borrowing amounts. In both cases, the relative effects of liquidity and forgiveness hold important implications for social welfare: a borrower may make "riskier" career decisions because she can borrow against future income, or because her future income is insured.

Second, while the apparent lack of forgiveness-eligible borrowers in my sample provides clear evidence of liquidity benefits, it also leaves unanswered questions regarding moral hazard and student debt forgiveness more generally. 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 payments (Department of Education [2017a]), and President Trump has proposed a version of IDR with a fifteen year forgiveness period (Douglas-Gabriel [2015]). Further empirical work on loan forgiveness is needed to assess the welfare benefits and potential distortionary effects of these policies.

Finally, evidence for IDR's benefits raises the natural question of why so few students enroll in the plan. According to classic economic theory, IDR should be weakly preferred

to standard repayment for any borrower. There are no pecuniary costs to enrolling, yet my results suggest there are individuals who would benefit from IDR but do not take it up. There are a number of potential behavioral mechanisms which might explain this low take-up, like “hassle-costs”, inattention, or limited information. Further research into these psychological frictions may reveal policy interventions to help borrowers take advantage of the benefits offered by IDR.

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Table 1: Summary Statistics

	Full Sample	Analysis Sample		
	(1) Pooled	(2) Pooled	(3) non-IDR	(4) IDR
<i>Panel A: LLS Data</i>				
IDR	0.0405	0.132	0	1
Female	0.570	0.708	0.702	0.747
Zip Median Income	59.23	52.22	52.36	51.24
Age	32.92	43.38	43.42	43.12
Amount Borrowed	19.11	27.10	26.71	29.64
10+ Days Delinquent	0.500	0.999	0.999	0.996
90+ Days Delinquent	0.287	0.733	0.748	0.638
Days Delinquent	93.99	165.7	169.5	141.0
Number of Calls		5.373	5.501	4.525
<i>Panel B: Credit Data</i>				
Credit Score	676.3	588.8	587.7	595.8
Bankruptcy	0.127	0.197	0.195	0.208
Derogatory Rating	0.331	0.553	0.550	0.572
Number of Credit Cards	5.020	3.088	3.080	3.143
Credit Card Balances	4.104	1.573	1.572	1.580
Number of Mortgages	1.156	0.698	0.709	0.626
Mortgage Balances	67.25	26.80	27.59	21.58
Credit Card Limits	20.03	4.820	4.823	4.804
Number of Auto Trades	1.923	1.629	1.636	1.587
<i>N</i>	148666	32901	28561	4340

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 made any loan payments from 2010 onward. The analysis sample is a subsample from the same population, selected according to the following criteria: borrowers received an IDR-modeled delinquency call from 2009 onward, have observable repayment histories three years prior and two years following the phone call, hold no private or Direct loans, are not recorded as non-english speakers, and were assigned to agents with at least observed phone calls. 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]. Initial zip income is the median 2010 income for the borrower's 5-digit zip code as of her first payment. Days delinquent is the maximum number of days the borrower was ever past due on payments as of the end of the month, and ever delinquent is an indicator for whether days delinquent is greater than 10. Number of calls is the total number of outgoing calls recorded for the borrower. Credit scores, bankruptcies, derogatory ratings, credit card, mortgage, and auto loan information are taken from TransUnion credit bureau data.

Table 2: Balance Test

	(1)	(2)
	Agent Score*100	IDR*100
Female	0.0217030 (0.0369377)	1.9004466*** (0.4073570)
Amount Borrowed	-0.0003252 (0.0019342)	-0.0728663*** (0.0216098)
Age	0.0001319 (0.0014456)	-0.0354522 (0.0236082)
Lag Zip Median Income	-0.0020782*** (0.0007449)	-0.0259604*** (0.0094822)
Lag Minimum Payment	0.2572103 (0.1746194)	6.2505180*** (1.9446248)
Lag Remaining Balance	0.0006372 (0.0011428)	0.0861494*** (0.0161065)
Lag Credit Score	-0.0006053** (0.0002760)	0.0362596*** (0.0039963)
Lag Credit Card Balances	-0.0084397 (0.0054398)	-0.0209796 (0.0726702)
Lag Number of Auto Trades	-0.0036527 (0.0079196)	-0.2603596** (0.1173033)
Lag Any Mortgage	0.0470706 (0.0548669)	-0.8896503 (0.8258514)
Lag Any Credit Card	0.0462374* (0.0263422)	-0.3862059 (0.4607368)
Lag Mortgage Balances	-0.0005568 (0.0004253)	-0.0111640** (0.0047710)
Lag Number of Credit Cards	-0.0106738** (0.0046604)	0.0825533 (0.0733199)
Lag Credit Card Limits	0.0033605 (0.0021739)	-0.0429108 (0.0385680)
Mean Dep.	-0.095	10.859
F-stat	1.41	16.26
P-value	0.1551	0.0000
R-squared	0.014	0.016
N	31109	31109

Note: This table reports balance test results. The regressions are estimated on the call sample described in the notes to Table 1. Agent score is estimated using data from other phone calls placed by the same agent following the procedure described in Section 5.1. Column 1 reports the estimated coefficients from an OLS regression of agent score multiplied by 100 against the variables listed and call year, month, and hour fixed effects. 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 in the rows. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3: First Stage

	(1) IDR	(2) IDR
Agent Score	0.6157086*** (0.1339117)	0.6020454*** (0.1325485)
Female		0.0188738*** (0.0040515)
Amount Borrowed		−0.0007267*** (0.0002165)
Age		−0.0003553 (0.0002363)
Lag Zip Median Income		−0.0002471** (0.0000949)
Lag Minimum Payment		0.0609567*** (0.0194803)
Lag Remaining Balance		0.0008577*** (0.0001610)
Lag Credit Score		0.0003662*** (0.0000396)
Lag Credit Card Balances		−0.0001590 (0.0007257)
Lag Number of Auto Trades		−0.0025816** (0.0011708)
Lag Any Mortgage		−0.0091799 (0.0082805)
Lag Any Credit Card		−0.0041404 (0.0046053)
Lag Mortgage Balances		−0.0001083** (0.0000480)
Lag Number of Credit Cards		0.0008898 (0.0007373)
Lag Credit Card Limits		−0.0004493 (0.0003881)
Mean Dep.	0.109	0.109
F-stat	21.14	20.63
P-value	0.0000	0.0000
R-squared	0.010	0.018
<i>N</i>	31109	31109

Note: This table reports first stage results. The regressions are estimated on the call sample described in the notes to Table 1. Agent score is estimated using data from other phone calls placed by the same agent following the procedure described in Section 5.1. 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 4: IDR and Loan Repayment Outcomes: Difference-in-Differences and Instrumental Variables Estimates

<i>Dependent Variable</i>	(1) Mean $t = -1$	Difference-in-Differences			Instrumental Variables		
		(2) Mos. 4 to 7	(3) Mos. 8 to 11	(4) Mos. 12 to 15	(5) Mos. 4 to 7	(6) Mos. 8 to 11	(7) Mos. 12 to 15
Minimum Payment	0.22	−0.14*** (0.00)	−0.14*** (0.00)	−0.05*** (0.00)	−0.20*** (0.03)	−0.22*** (0.03)	−0.14** (0.05)
Δ Remaining Balance	0.13	−0.10*** (0.01)	−0.09*** (0.01)	0.07*** (0.01)	−0.15 (0.16)	−0.12 (0.15)	0.04 (0.13)
10+ Days Delinquent	0.22	−0.21*** (0.00)	−0.16*** (0.00)	−0.04*** (0.00)	−0.36** (0.15)	−0.30*** (0.11)	−0.08 (0.13)
90+ Days Delinquent	0.02	−0.08*** (0.00)	−0.08*** (0.00)	−0.07*** (0.00)	−0.13 (0.09)	−0.08 (0.09)	0.07 (0.08)
Call Time FE		Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes	Yes	Yes
N	124431	124431	124431	124431	124431	124431	124431

Note: This table reports difference-in-differences and 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. Columns 2-4 report coefficients on the effect of IDR in consecutive three-month periods, beginning with four months following the delinquency call from the pooled OLS regression specified in Equation 22. Each of Columns 5-7 report estimates from separate two-stage least squares regressions on outcomes in the same months. Regressions are estimated on the analysis sample as described in the notes to Table 1. Two-stage least squares models instrument for IDR with the agent score calculated using data from other calls made by the agent following the procedure described in Section 5.1. All specifications include controls from call date and time, as well as amount borrowed, 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 5: IDR and Credit Outcomes: Difference-in-Differences Estimates

<i>Dependent Variable</i>	(1) Mean $t = -1$	Diff-in-Diff			Diff-in-Diff w Linear Trend		
		(2) Year 0	(3) Year 1	(4) Year 2	(5) Year 0	(6) Year 1	(7) Year 2
Credit Score	593.26	5.01*** (1.06)	7.53*** (1.40)	5.92*** (1.63)	4.88*** (1.53)	7.40*** (1.82)	5.79*** (1.98)
Bankruptcy	0.19	-0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Any Mortgage	0.20	0.00 (0.00)	0.02** (0.01)	0.02*** (0.01)	0.00 (0.01)	0.02** (0.01)	0.02*** (0.01)
Any Auto Trade	0.71	-0.00 (0.01)	-0.00 (0.01)	0.01 (0.01)	-0.00 (0.01)	-0.00 (0.01)	0.01 (0.01)

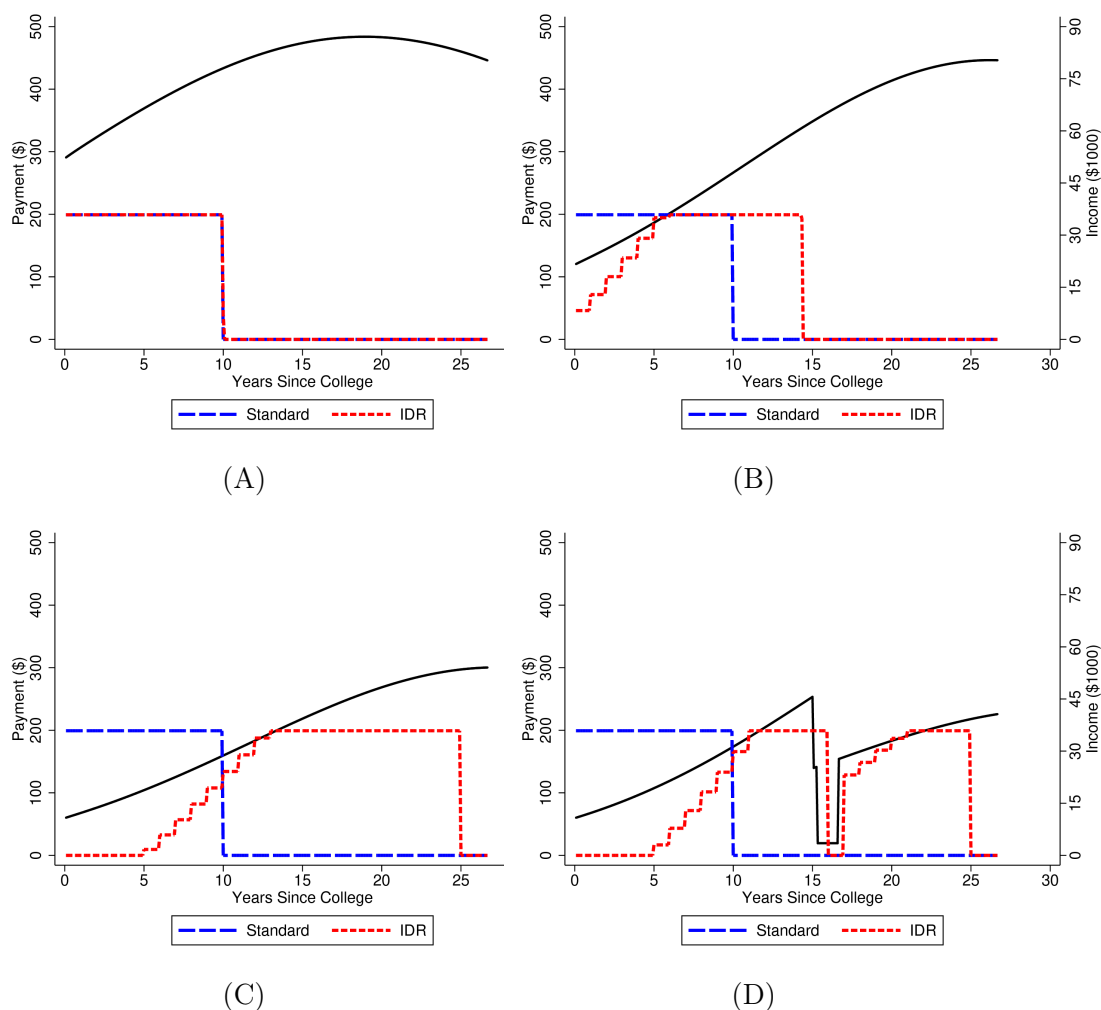
Note: This table reports difference-in-differences estimates of the effect of IDR enrollment on yearly credit outcomes. Column 1 reports the dependent variable mean in the year prior to receiving a delinquency call. Columns 2-4 report coefficients on the effect of IDR in consecutive years, beginning with the year of the delinquency call from the pooled OLS regression specified in Equation 22. Columns 5-7 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. Both specifications include controls from call date and time, as well as amount borrowed, inferred gender, and zip-code median income. Sample size is 16,025. 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 6: IDR and Credit Cards: Difference-in-Differences Estimates

<i>Dependent Variable</i>	(1) Mean $t = -1$	Diff-in-Diff			Diff-in-Diff w Linear Trend		
		(2) Year 0	(3) Year 1	(4) Year 2	(5) Year 0	(6) Year 1	(7) Year 2
Credit Card Balances	1.42	0.05 (0.05)	0.24*** (0.08)	0.40*** (0.11)	0.02 (0.07)	0.20** (0.09)	0.36*** (0.12)
Log Credit Card Balances	-2.51	0.07 (0.05)	0.33*** (0.07)	0.38*** (0.07)	0.06 (0.06)	0.32*** (0.08)	0.37*** (0.09)
Number of Credit Cards	3.00	-0.01 (0.03)	0.07 (0.04)	0.14** (0.06)	-0.03 (0.03)	0.06 (0.05)	0.12** (0.06)
Credit Card Limits	2.93	-0.13 (0.12)	0.14 (0.16)	0.34 (0.21)	-0.23 (0.14)	0.04 (0.18)	0.23 (0.23)

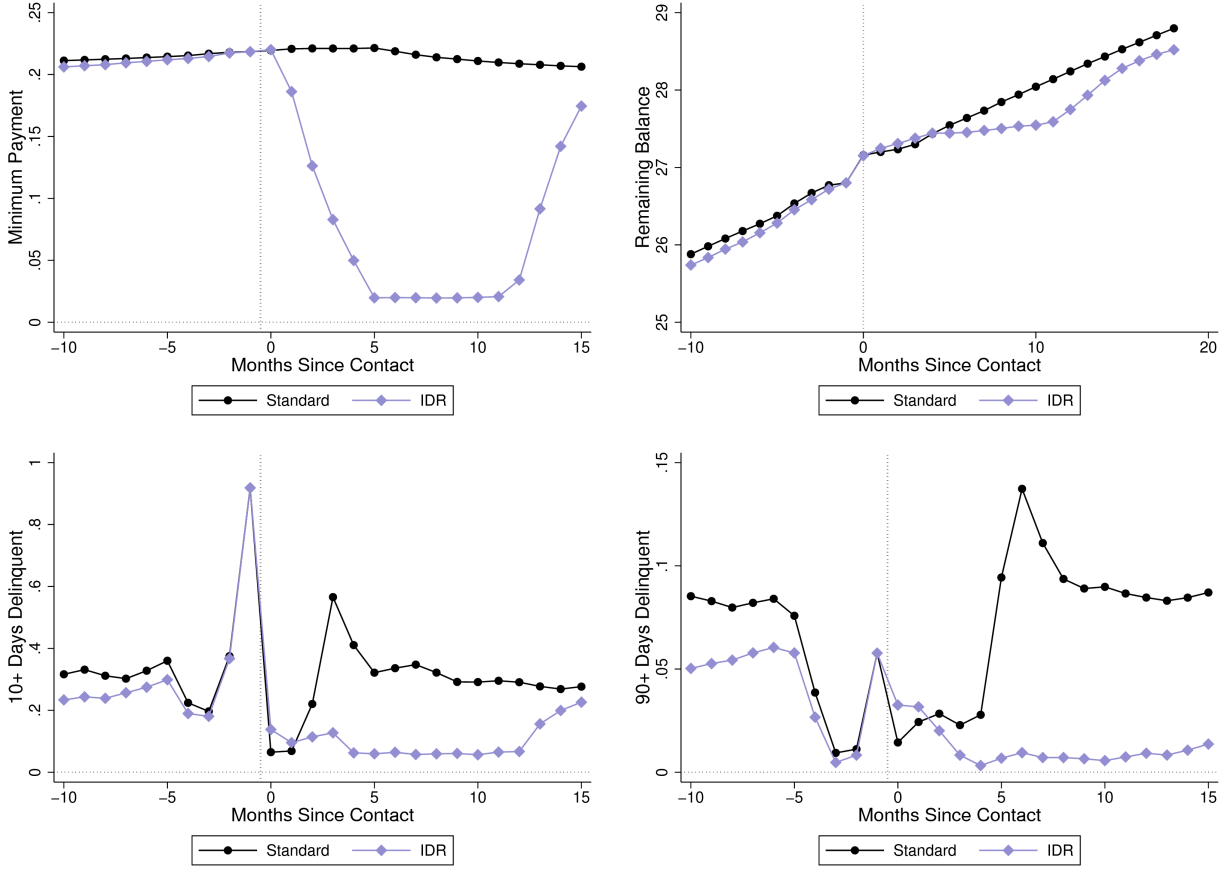
Note: This table reports difference-in-differences estimates of the effect of IDR enrollment on yearly credit cards. Column 1 reports the dependent variable mean in the year prior to receiving a delinquency call. Columns 2-4 report coefficients on the effect of IDR in consecutive years, beginning with the year of the delinquency call from the pooled OLS regression specified in Equation 22. Columns 5-7 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. Both specifications include controls from call date and time, as well as amount borrowed, inferred gender, and zip-code median income. Sample size is 16,025 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 1: Hypothetical Repayment Scenarios: IDR versus Standard Repayment



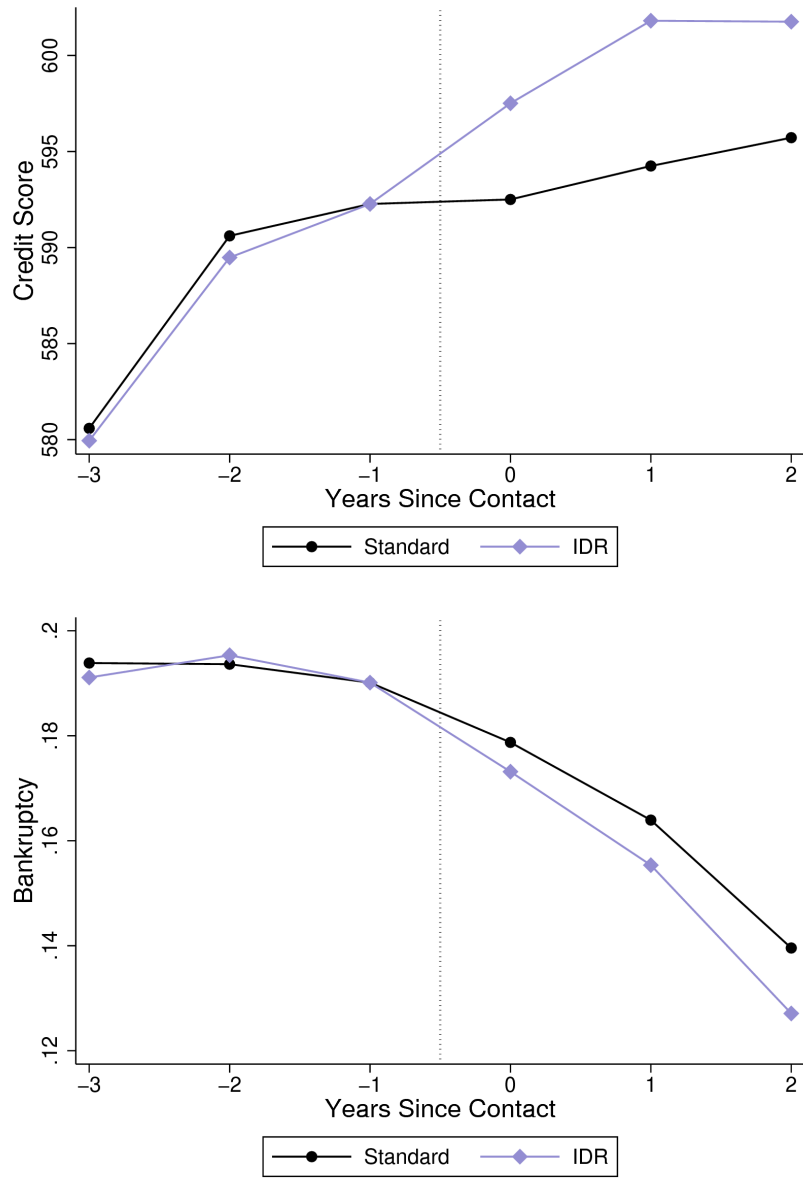
Note: This figure plots hypothetical repayment paths for standard and IDR plans under various income scenarios. Each panel represents an alternative income/repayment scenario for a borrower holding \$18,000 of student debt at the time they leave college. The solid black line, plotted against the right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

Figure 2: Pre/Post-Call Trends in Repayment Outcomes



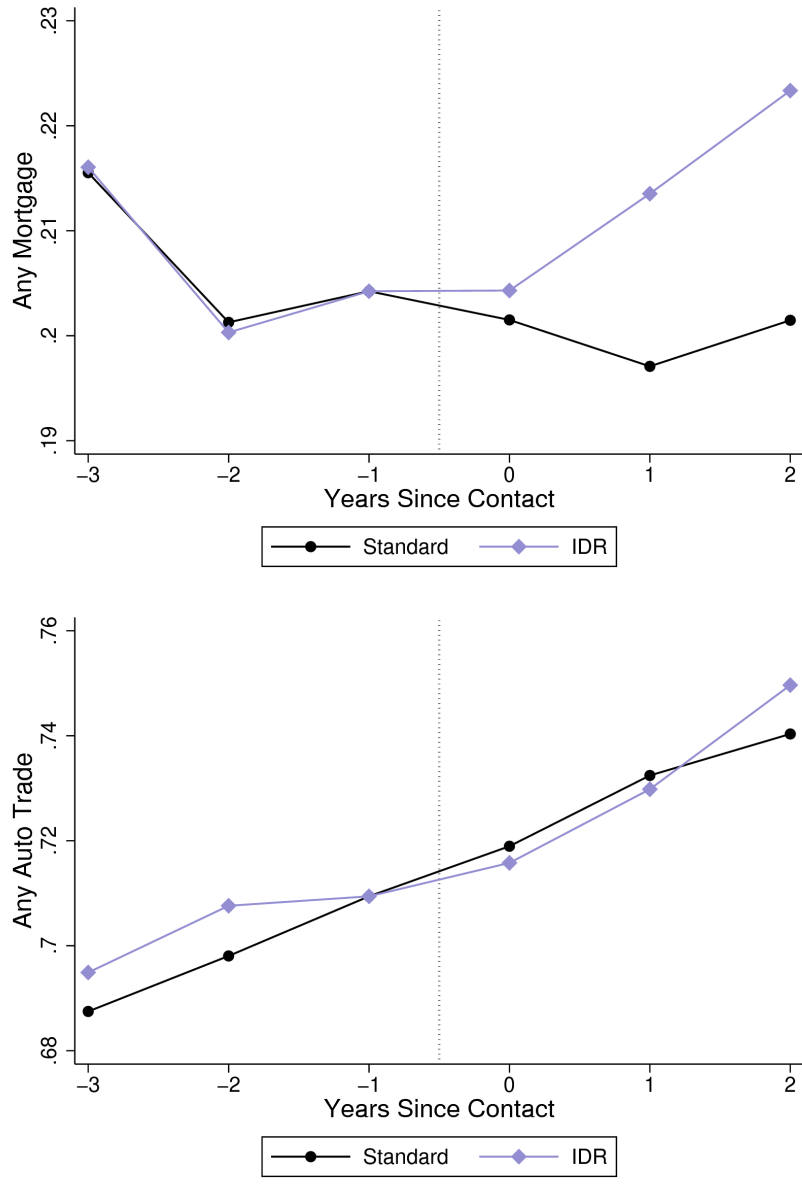
Note: This figure plots the average loan repayment outcomes for IDR enrolees and non-enrolees in the analysis sample. The horizontal axis denotes time, in months, relative to the month of the loan servicing call. Outcomes are normalized to the average value of the outcome for non-enrolees in the month prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

Figure 3: Pre/Post-Call Trends in Credit Scores and Bankruptcies



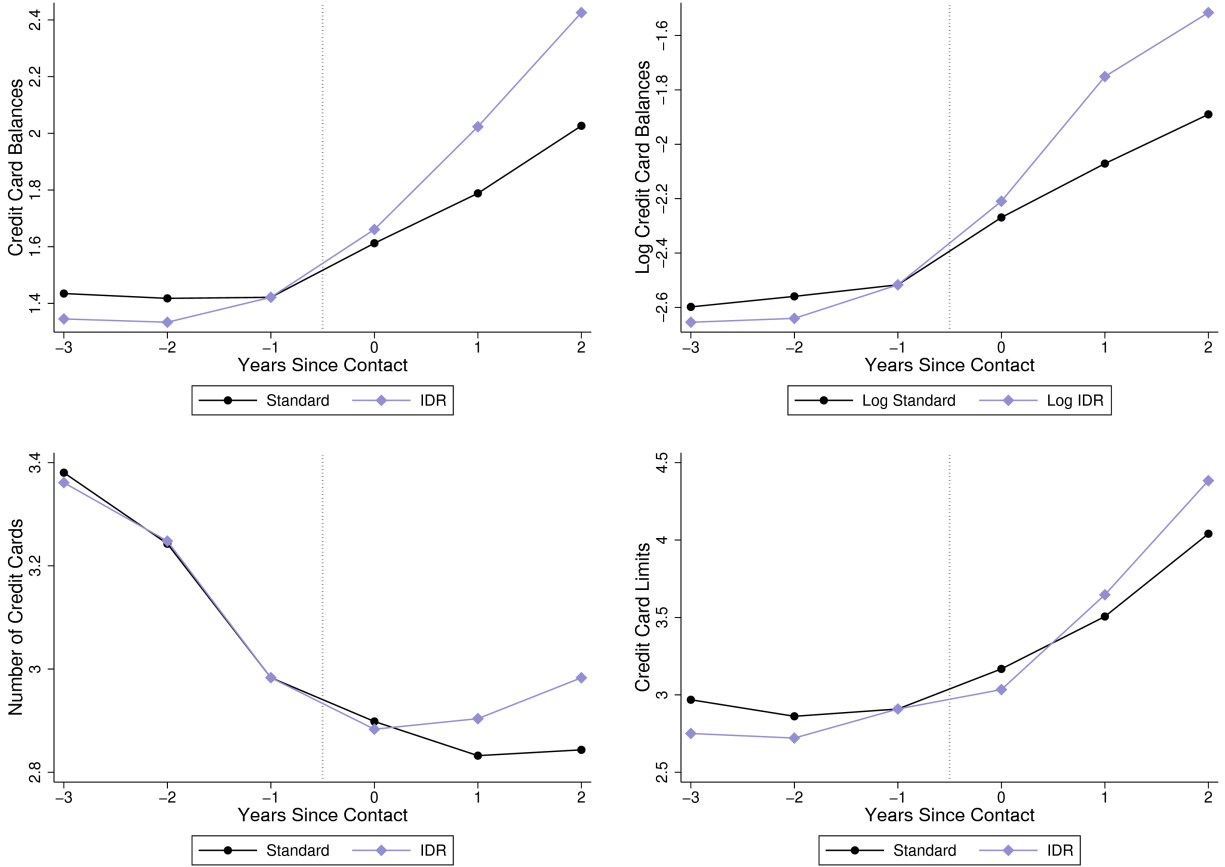
Note: This figure plots the average credit scores and bankruptcies for IDR enrollees and non-enrollees in the analysis sample. The horizontal axis denotes time, in years, relative to the year of the loan servicing call. Outcomes are normalized to the average value of the outcome for non-enrollees in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

Figure 4: Pre/Post-Call Trends in Mortgages and Auto Loans



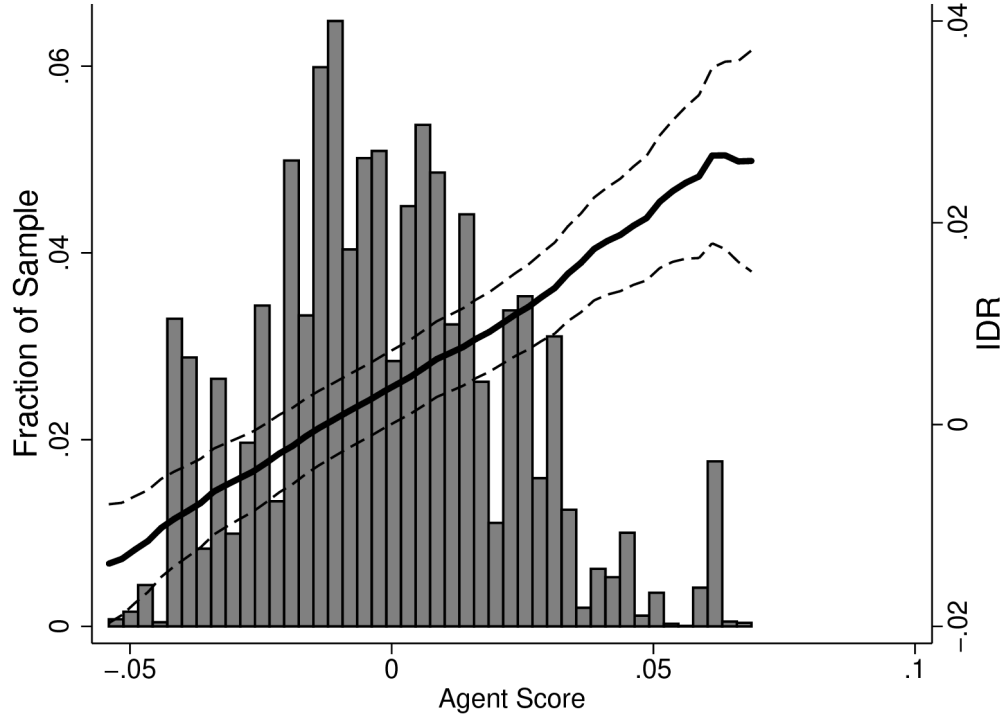
Note: This figure plots the share of IDR enrollees and non-enrollees holding mortgages or auto loans in the analysis sample. The horizontal axis denotes time, in years, relative to the year of the loan servicing call. Outcomes are normalized to the average value of the outcome for non-enrollees in the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

Figure 5: Pre/Post-Call Trends in Credit Cards



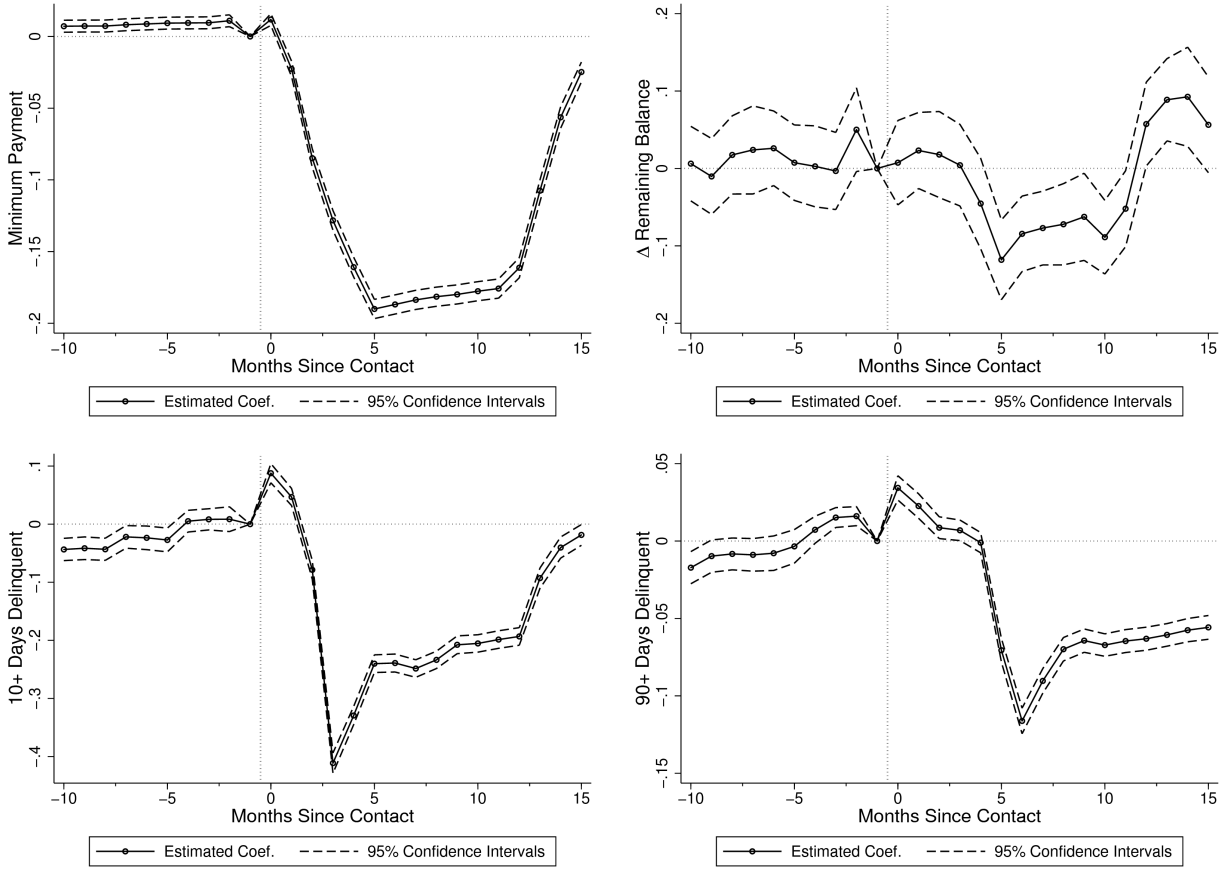
Note: This figure plots the average credit card outcomes for IDR enrollees and non-enrollees in the analysis sample. The horizontal axis denotes time, in years, relative to the year of the loan servicing call. Outcomes are normalized to the average value of the outcome for non-enrollees in the year prior to the call. See Table 1 notes for additional details on the outcome measures and sample.

Figure 6: First Stage



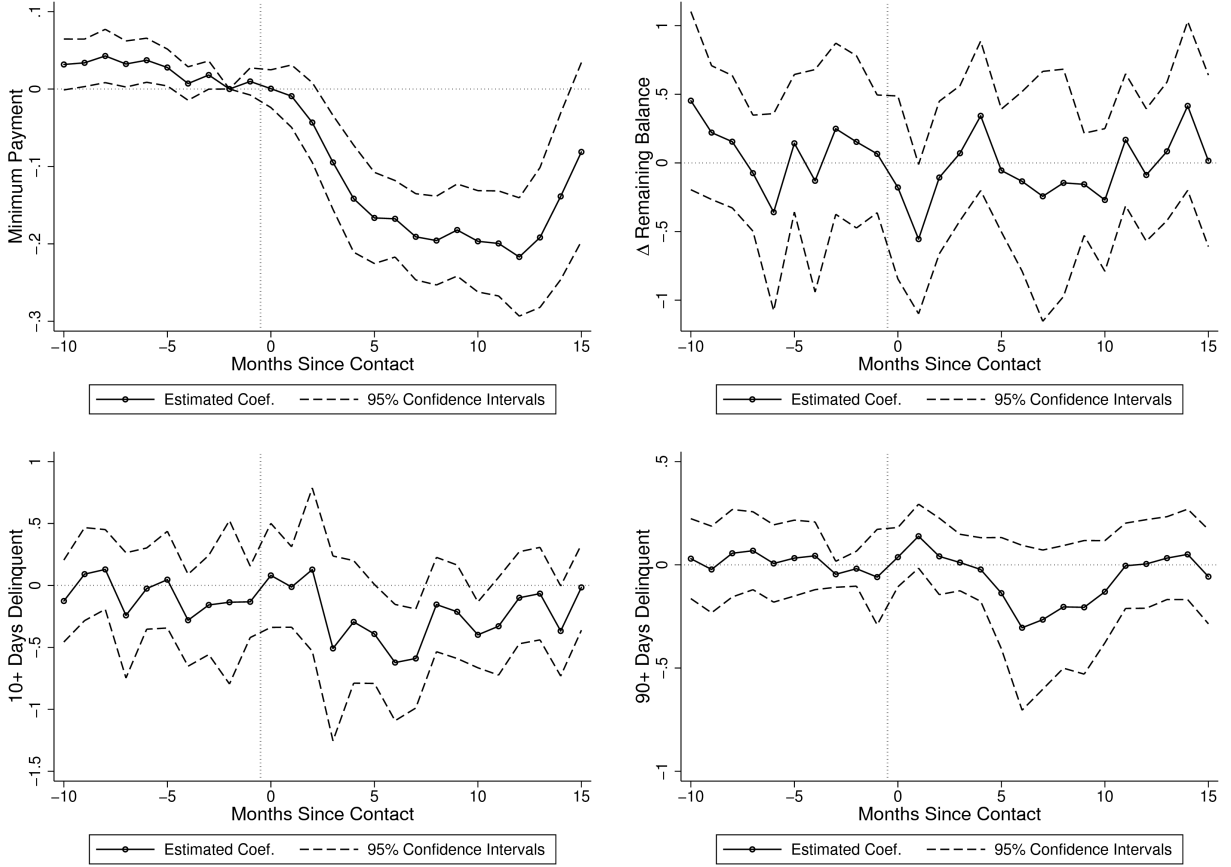
Note: This figure reports first-stage effects and distribution of agent scores across delinquency calls, where agent score is the leave-out mean IDR take-up calculated using data from other calls made by the agent following the procedure described in Section 5.1. The solid and dashed lines, plotted against 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 7: Difference-in-Differences: Repayment Outcomes



Note: This figure reports estimated coefficients from a series of two-stage least squares regressions. 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. All regressions include the full set of call date and time fixed effects. Robust standard errors are two-way clustered at the borrower and agent level.

Figure 8: Two-Stage Least Squares: Repayment Outcomes



Note: This figure reports estimated coefficients from a series of two-stage least squares regressions. 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. All regressions include the full set of call date and time fixed effects. Robust standard errors are two-way clustered at the borrower and agent level.

A Additional Tables and Figures

Table A1: IDR and Credit Outcomes: Instrumental Variables Estimates

<i>Dependent Variable</i>	(1) Mean $t = -1$	OLS			2SLS		
		(2) Year 0	(3) Year 1	(4) Year 2	(5) Year 0	(6) Year 1	(7) Year 2
Credit Score	593.26	8.14*** (0.86)	11.79*** (1.11)	11.08*** (1.39)	-9.36 (18.78)	-22.17 (18.89)	-29.81 (29.03)
Bankruptcy	0.19	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)	0.11 (0.12)	0.17 (0.13)	0.05 (0.11)
Any Mortgage	0.20	-0.00 (0.00)	0.01 (0.01)	0.01 (0.01)	0.03 (0.09)	-0.01 (0.12)	-0.07 (0.13)
Any Auto Trade	0.71	-0.01** (0.00)	-0.01 (0.01)	-0.01 (0.01)	0.06 (0.10)	-0.09 (0.13)	-0.13 (0.13)
Call Time FE		Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes	Yes	Yes
N	15957	16023	15993	15937	16023	15993	15937

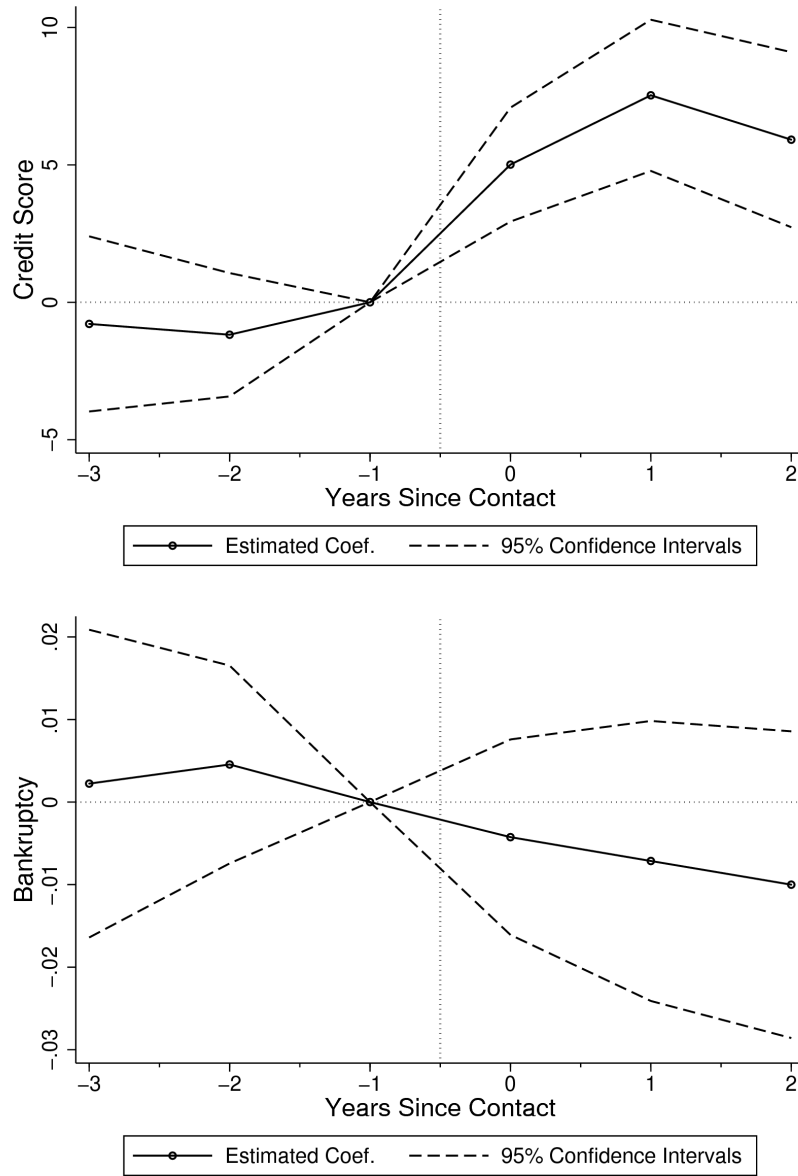
Note: This table reports OLS and two-stage least squares results of the impact of IDR on credit outcomes. Dependent variables are listed in each row. Column 1 reports the dependent variable mean in the year prior to receiving a delinquency call. Each of Columns 2-7 contain estimates from separate regressions on outcomes in years 0,1, and 2, relative to the delinquency call. Regressions are estimated on the analysis sample as described in the notes to Table 1. Two-stage least squares models instrument for IDR with the agent score calculated using data from other calls made by the agent following the procedure described in Section 5.1. All specifications include controls from call date and time, as well as amount borrowed, inferred gender, and zip-code median income. 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 A2: IDR and Credit Cards: Instrumental Variables Estimates

<i>Dependent Variable</i>	OLS				2SLS		
	(1) Mean $t = -1$	(2) Year 0	(3) Year 1	(4) Year 2	(5) Year 0	(6) Year 1	(7) Year 2
Credit Card Balances	1.37	-0.04 (0.04)	0.13** (0.06)	0.26*** (0.08)	-1.60* (0.96)	-2.14 (1.37)	-0.41 (1.49)
Log Credit Card Balances	-2.55	0.00 (0.04)	0.26*** (0.06)	0.32*** (0.06)	-0.57 (0.79)	-0.49 (1.08)	-0.05 (1.20)
Number of Credit Cards	2.94	-0.01 (0.03)	0.09** (0.04)	0.17*** (0.04)	-0.26 (0.47)	-0.62 (0.67)	-0.50 (0.89)
Credit Card Limits	2.78	-0.09 (0.08)	0.13 (0.12)	0.29* (0.16)	-1.84 (1.82)	-0.85 (2.41)	-0.35 (3.11)
Call Time FE		Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes	Yes	Yes	Yes
N	19628	19260	19260	19260	19260	19260	19260

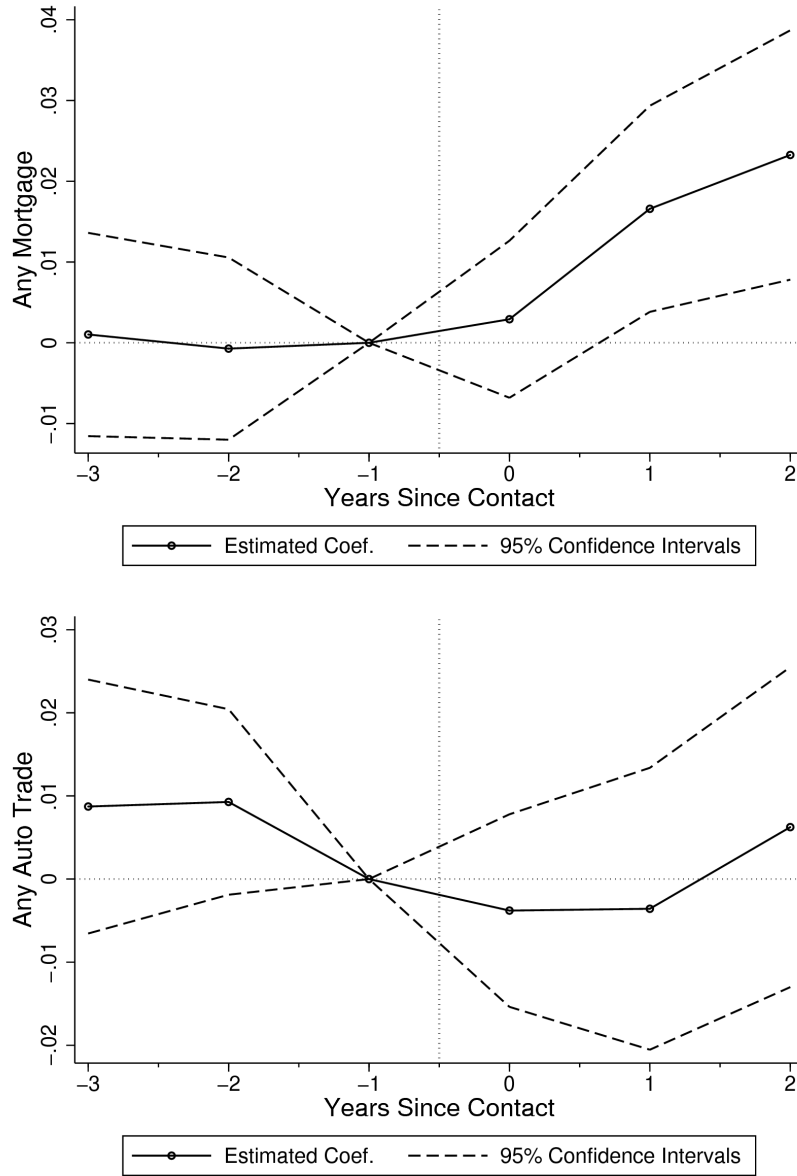
Note: This table reports OLS and two-stage least squares results of the impact of IDR on credit outcomes. Dependent variables are listed in each row. Column 1 reports the dependent variable mean in the year prior to receiving a delinquency call. Each of Columns 2-7 contain estimates from separate regressions on outcomes in years 0,1, and 2, relative to the delinquency call. Regressions are estimated on the analysis sample as described in the notes to Table 1. Two-stage least squares models instrument for IDR with the agent score calculated using data from other calls made by the agent following the procedure described in Section 5.1. All specifications include controls from call date and time, as well as amount borrowed, inferred gender, and zip-code median income. 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.

Figure A1: Difference-in-Differences: Credit Scores and Bankruptcies



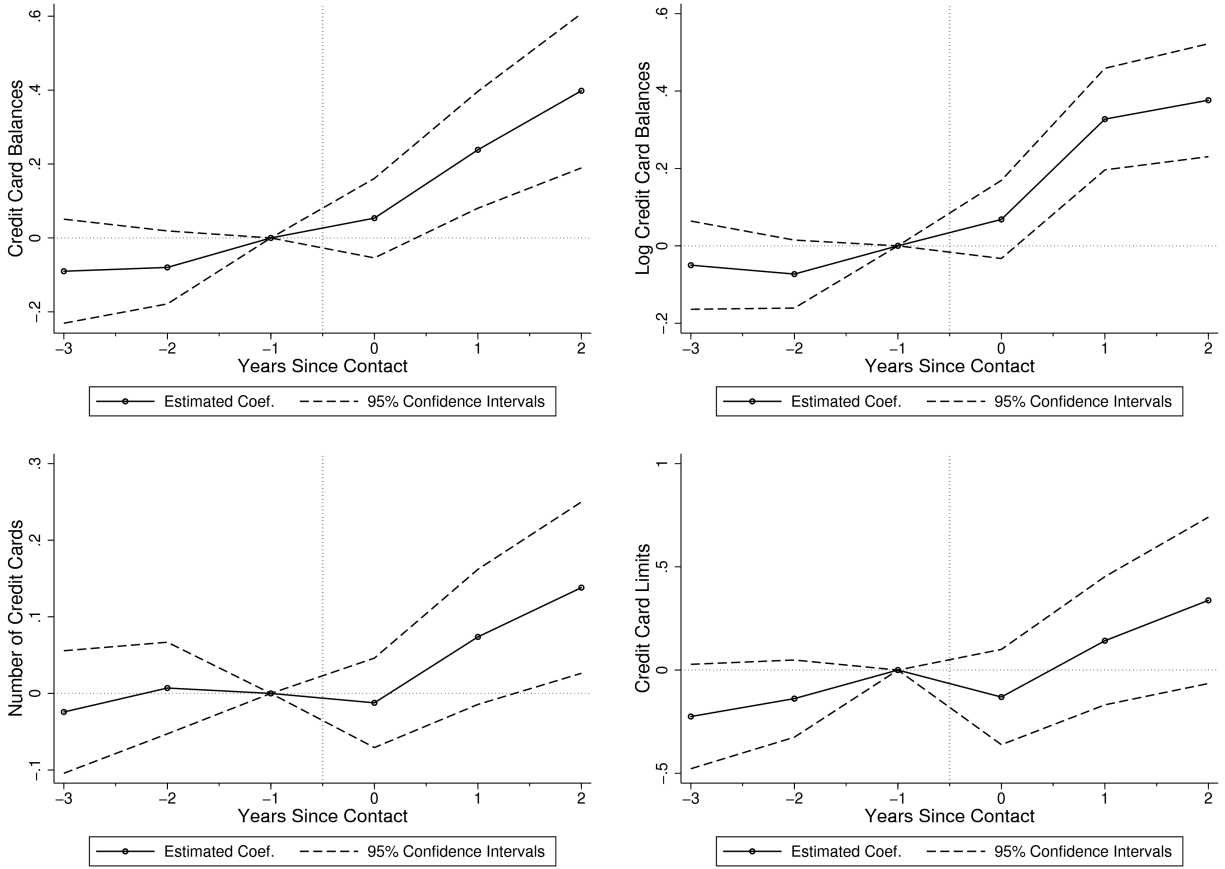
Note: This figure reports estimated coefficients from a series of two-stage least squares regressions. Each point represents the estimated effect of post-call IDR status on the outcome variable at a given time period relative to the date of delinquency call. Relative years are plotted along the x-axis. Dashed lines represent 95% confidence intervals. All regressions include the full set of call date and time fixed effects. Robust standard errors are two-way clustered at the borrower and agent level.

Figure A2: Difference-in-Differences: Mortgages and Auto Loans



Note: This figure reports estimated coefficients from a series of two-stage least squares regressions. 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 years are plotted along the x-axis. Dashed lines represent 95% confidence intervals. All regressions include the full set of call date and time fixed effects. Robust standard errors are two-way clustered at the borrower and agent level.

Figure A3: Difference-in-Differences: Credit Cards



Note: This figure reports estimated coefficients from a series of two-stage least squares regressions. 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 years are plotted along the x-axis. Dashed lines represent 95% confidence intervals. All regressions include the full set of call date and time fixed effects. Robust standard errors are two-way clustered at the borrower and agent level.